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 employment among new mothers, particularly among those previously unemployed. Earlier childcare access increases new mothers' reallocation of careers into more demanding jobs in male-dominated firms, leading to higher earnings and higher productivity. Firms traditionally unattractive to women with children benefit from such reallocation, experiencing higher growth and 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. A large literature has sought to understand the drivers of these disparities, with a consensus pointing to women's child-rearing responsibilities as an explanation (Kimmel, 1998; Goldin, 2014; Goldin and Katz, 2016; Kleven et al., 2019). Such a "childcare penalty" is at the center of the recent debate on the impact of the Covid-19 pandemic on female workers (Furman et al., 2021; Couch et al., 2021; Garcia and Cowan, 2022). At the same time, there is increasing demand by firms, investors, and policymakers for a gender balanced workforce and greater equality in pay. One potential solution is to subsidize early age childcare. Indeed, childcare subsidies is a commonly debated policy item around the world, and was a center piece of the recent "Build Back Better Act" introduced by President Biden in 2021.[1]

Existing evidence on the effect of childcare subsidies on maternal labor supply and family outcomes has been mixed.[2] 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 started to introduce highly subsidized, universally accessible childcare to parents, regardless of their income and employment. The program provided childcare for young children at a subsidized rate of $5 CAD per day. 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

[1] In March 2022, the government of Canada reached agreement with all provinces and territories to provide $10-a-day childcare. Leading up to the 2022 Australian federal election, the Australian Greens Party released a proposal to make early childhood education universal and free to all Australian children.

[2] See, for example, Baker et al. (2008), Lefebvre and Merrigan (2008), Fitzpatrick (2010), Havnes and Mogstad (2011a), Brodeur and Connolly (2013), and Kleven et al. (2020).

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' financials. Such 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, confirming prior studies (Baker et al., 2008; Goux and Maurin, 2010; Bauernschuster and Schlotter, 2015). Such employment effect is concentrated 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 is strongly effective in drawing those who were previously unemployed into the labor force.

The insignificant employment effect on new mothers previously employed masks, however, important effects on the intensive margins of employment in terms of worker-firm sorting and earnings. 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-pay 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. Interestingly, childcare access reduces new mothers' subsequent fertility, consistent with them pursuing more intense career trajectories.

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 cohorts. 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. 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 prior to the reform.

We then examine the effect of the reform on female workers' sorting into firm types and its impact on firm outcomes. We show that access to subsidized childcare increases the likelihood that new mothers sort into "mom-unfriendly" firms where the fraction of new mothers employed at the firm is less than the fraction of new fathers. It also increases their sorting into industries with "greedier" jobs, i.e, jobs with higher earnings-hours elasticity as defined by (Goldin, 2014). 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 dominated by males. As a result of this sorting, mom-unfriendly firms in Quebec gained employment after the reform relative to other firms, an increase driven by more female employees. These firms experienced better performance, measured by growths in sales and assets, higher return on assets, better asset utilization, and higher labor productivity.

Overall, our findings depart from the prior literature by documenting important intensive margin effects of childcare frictions conditional on employment: childcare frictions hamper working women's career progression and impact worker-firm matching. 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 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 existing employer or employment status. Prior literature has documented an either 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., 2008; Cascio, 2009; Havnes and Mogstad, 2011b).[3] 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

[3]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).

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 focus on individuals' career trajectories and their sorting into firm types. We also examine the heterogeneous impact of the reform on firm outcomes.

Our paper also relates to the literature on gender and firms. Several papers highlight higher costs associated with working in certain sectors or occupations for women. Lagaras et al. (2022) show that female talent does not sort into the financial sector due to inflexible working schedules that make it hard to balance family and work. Ellul et al. (2020) show that female employees are under represented in more demanding careers in finance such as in asset management. Hebert (2020) shows that fundraising is harder for founders in gender-incongruent sectors. 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 are 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").[4] 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 is $11 per day (Lefebvre and Merrigan,

[4]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.

2008).[5] 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 remain unchanged in the rest of Canada, and universal childcare was never offered in other provinces.

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. These 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).

The 1997 Quebec family policy was announced in January of 1997 (Tougas, 2002). The

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

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.

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 subsidy.[6] Although parents had to pay unsubsidized prices for early enrollment, they still benefited from 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.

[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 and children's birth-year 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, 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. 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. We further limit the sample to parents that are at least 18 years older than their first child to limit the number of cases where an individual did not join a family until after the child was born.[8]

[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 affect that individual's career trajectory), we exclude all instances where the gap between parent age and child age is less than 18.

We further restrict our sample to individuals who gave birth between 1993 and 1999. 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 financials, utilities, and government sectors (NAICS 52, 22, 91, respectively). We also require the firm to have at least 5 employees and $25,000 in total assets in 1996.

4 Empirical Strategy

4.1 Individual-level Analysis

We employ two empirical strategies to identify the impact of childcare access on worker and firm outcomes. At the worker-level, we exploit 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

[9] On December 31, 2001, the Canadian federal government extended the income replacement period associated with parental leave (i.e. paid leave) for all provinces.

the labor market due to ability or ambition. We use calendar fixed effects to control for macro shocks. Event-year fixed effects, absorbing kid's age fixed effect, control for changing 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 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 $PostBirth_{i,t}$ is an indicator equal to one for years after an individual gives birth. $CCYears_i$ is a cohort-level treatment intensity variable that indicates years of post-reform childcare access in the first five years after the first child's birth.[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 government childcare on 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 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 to intertemporally smooth the expected future subsidy (Ding et al., 2020). Later cohorts enjoy longer eligible years and/or longer anticipatory

[10] In a robustness test, we also include age bin fixed effects. See Panel A of Table **??**.

[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.

years, hence receiving higher treatment intensity.

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. 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 $MomEmpGap96$, a measure of how "unfriendly" a firm is to new mothers, and include it as the dependent variable in our regression. Specifically, we define this measure as the fraction of new fathers (with first child aged 0-5) among all employees minus the fraction of new mothers among all employees, measured as of 1996, the year before the reform. A higher value means that the firm's jobs are less mother-friendly and more male dominated before the reform.

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 follow 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 cohorts within each of these pairs became eligible for the subsidy at the same age (see Table 1), the patterns indicate that the difference comes from the anticipatory earlier

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

response by the later cohort. 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.[13]

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.[14] We focus on Quebec children of age 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 significantly increased following the 1997 reform. Column 2 shows that this can be attributed to both an anticipatory effect (reflecting a 3.1 pp increase in take-up) and an eligibility effect (reflecting a 9.5 pp increase in take-up).

4.2 Firm-level Analysis

In our second set of empirical analyses, we use firm-level data to estimate the impact of universal childcare on firm employment and performance. We employ a DID model comparing Quebec firms with different gender employment gaps among new parents before the reform. Such a comparison is motivated by the expected sorting of new mothers into more demanding careers traditionally dominated by males, due to better access to childcare. Our individual-level analysis explicitly tests for such firm-female worker matches, as explained in the previous subsection. Focusing on gender gap among new parents instead of all employees also allows us to zoom in on the effect of childcare subsidy and make sure that our results do not pick up a general increase in gender equality over time. Specifically, we estimate the

[13] Additionally, any secular trend should shift employment parallely across cohorts instead of inducing divergence at a specific child age.

[14] 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.

following regression:

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \theta \times HighMomEmpGap96_j \times Post97_t + \epsilon_{j,t}, \tag{2}$$

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, $HighMomEmpGap96_j$ is an indicator equal to one if $MomEmpGap96$ for firm j is above the sample median, $Post97_t$ indicates years since 1997. Our main variable of interest is the interaction term $HighMomEmpGap96_j \times Post97_t$, which measures the impact of the reform on the performance of "mom-unfriendly" firms relative to the performance of "mom-friendly" firms. Note that $HighMomEmpGap96_j$ is absorbed by firm fixed effect and $Post97_t$ is absorbed by year fixed effect. We cluster standard errors at the firm level.

We first use this specification to examine the effect of the reform on firm-level employment outcomes—in particular, total employment, the fraction of female employees, and the number of male and female employees. We then examine several firm performance measures such as asset growth, sales growth, ROA (pre-tax income/total assets), asset turnover (sales/total assets) and labor productivity (i.e., sales per employee).

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 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 female employment rate by 1.4 percentage points, which is a 2.7% increase relative to post-childbirth mean before the reform. Note that our result represents the effect of childcare access, not the effect of childcare takeup, which should

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. 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.

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 define employer changes based on T4 filings. We find that earlier access to childcare significantly increases the likelihood that new mothers voluntarily switch employers from one year to the next.[15] Based on the coefficient in column 1, a five-year earlier access to childcare increases a new mother's job switching rate by 1.2pp, which is a 39% 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-childbrith employer and join a new firm (columns 1-2). These findings suggest that access to childcare increases labor market mobility for female workers. Childcare support can facilitate job reallocation by freeing up time and resources for new mothers to look for a new job and to take up more demanding careers.

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

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 only 7.5%. This suggests that a large part of the earnings increase is realized through switching employers.[16]

Columns 3-6 of Table 5 further examine the likelihood of promotions and demotions, which we proxy using large earnings' changes. Following McCue (1996), We define promotions as earning increases higher than 10% and demotions as earning decreases higher than 10% relative to pre-childbirth earnings.[17] We find that accessing childcare earlier by five years increases the likelihood of promotions by 8.2pp (41% relative to the mean) (column 3), and increases the likelihood of within-firm promotions by 6.9pp (column 4). Columns 5 and 6 shows 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 and a 3.8pp decrease in the likelihood of within-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.

5.2 Robustness

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

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

[17] McCue (1996) documents that wage growth associated with promotions centers around 10% across different demographic groups.

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

$$Y_{i,t,y} = \alpha_i + \beta_t + \gamma_y + \sum_{n=-4}^{5} \theta_n \times CCYears_i \times YearsToBirth_{i,n} + \epsilon_{i,t,y}, \tag{3}$$

where $YearToBirth_{i,n}$ is a dummy indicating event year relative to childbirth. The childbirth year ($n = 0$) is omitted as the base year. Appendix Table **??** shows the results. We find that the effects of earlier access to childcare are insignificant and economically small in the years before childbirth, and are significantly positive in the years after childbirth. This indicates that childcare access does not affect individuals' career trajectories prior to the birth of their first child. We present plots of the dynamic DID coefficients in Figure 3, which visually illustrate the lack of pre-treatment trends in all three outcomes.

An interesting story emerges when we examine the post-childbirth period. We see that the effect of childcare access 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 soak up 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 equation:

$$\begin{aligned} Y_{i,t,y} = \alpha_i + \beta_{s,t} + \gamma_{s,y} + \theta_1 \times CCYears_i \times PostBirth_{i,t} \\ + \theta_2 \times CCYears_i \times Female_i \times PostBirth_{i,t} + \epsilon_{i,t,y}, \end{aligned} \tag{4}$$

where $Y_{i,t,y}$, α_i, $CCYears_i$, and $PostBirth_{i,t}$ are the same as in equation 1, $\beta_{t,s}$ indicates calendar year-gender fixed effects, $\gamma_{y,s}$ indicates event year-gender fixed effects.[18] Table 6 presents the triple-difference results. We find that our baseline results indeed concentrate among mothers, and are largely absent among fathers, 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.

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. Panel B of Table A.2 shows that the results remain similar.

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.[19] 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,

[18]Note that $Female_i \times PostBirth_{i,t}$ is absorbed by event year-gender fixed effects and $CCYears_i \times Female_i$ is absorbed by individual fixed effects.

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

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 low-income women, consistent with these individuals facing greater constraints in private childcare provision.

We further find that earlier childcare access reduces the likelihood of working mothers taking sick leaves (column1 of Table 8), a common proxy for productivity used in the literature (Bennedsen et al., 2019). This 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.[20]

We then examine whether better access to childcare leads new mothers to invest more in education. Columns 3-4 Table 8 show that this is not the case. We failed to find a significant increase in the likelihood of taking a leave for schooling or further education. This result suggests that childcare-induced earnings growth is not due to new mothers investing in more education. Finally, we examine how childcare subsidies impact female's subsequent fertility after the first child. A lower childcare cost could encourage fertility. However, childcare access may also set new mothers on a more engaging career, discouraging further fertility. We examine this in columns 5-6, where the dependent variable is an indicator equal to one if an individual had a subsequent child in that year. We find that childcare subsidies discourage subsequent fertility, suggesting that the career upgrade effect dominates the cost effect.

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

5.4 Further Discussion

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. For mothers who would have been unemployed due to the need to provide childcare at home (which is free), the reform actually reduced their wealth, as they now have to pay for government childcare to free up time for employment. For mothers who would have been employed and paid for unsubsidized childcare, the subsidies increased their wealth. However, a wealth increase would only make them less willing to upgrade their careers as subsidies substitute labor income.[21]

One may also wonder why some new mothers, especially wealthier ones, did not take up unsubsidized childcare before the reform given the large earnings benefit we document. It is worth emphasizing that the effect of the reform comes not just from childcare costs subsidy, but also from the expansion in the number of childcare spaces (see Figure 1). Anecdotal evidence abounds that many parents, even 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.

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.

[21] A large literature documents that positive liquidity shocks reduce individuals' labor supply or job search efforts. See, for example, Lentz and Tranaes (2005), Card et al. (2007), Chetty (2008), Cesarini et al. (2017), and Li et al. (2020).

Our results on turnover, earnings, and productivity suggest potential career upgrades by new mothers due to better childcare access. Becker (1985) argues that childcare causes women to seek less demanding jobs. Therefore, we should expect childcare access to lead new mothers to pursue more ambitious or demanding jobs that were traditionally dominated by men. Motivated by this, we examine how "mom-unfriendly" a firm is before the reform. Specifically, we construct a firm-level measure, $MomEmpGap96$, defined as

$$MomEmpGap96_j = \frac{NewDads_{j,1996} - NewMoms_{j,1996}}{TotEmp_{j,1996}} \tag{5}$$

where $NewDads_{j,1996}$ ($NewMoms_{j,1996}$) denotes the number of male (female) workers employed by firm j with their first child aged between 0 and 5 as of 1996, and $TotEmps_{j,1996}$ denotes the total number of workers employed by firm j as of 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.

By measuring the imbalance between new fathers and mothers, $MomEmpGap96_j$ is intended to capture the barriers preventing women from pursuing employment at a particular firm due to childcare responsibilities.[22] 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. Since we expect later cohorts 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 higher-$MomEmpGap96$ firms post-birth relative to earlier cohorts.

Columns 1-2 of Table 9 test sorting along this dimension. We find that earlier access to childcare increases new mothers' sorting into firms that are traditionally less appealing

[22]In the data, higher $MomEmpGap96$ firms tend to be larger, more productive and higher-paying firms. Top $MomEmpGap96$ industries include resource extraction, manufacturing and construction and bottom $MomEmpGap96$ industries include service, hospitality, and childcare.

to new mothers (column 1). This sorting continues to hold with a similar magnitude within an industry across firms (column 2). Specifically, a five-year earlier access to childcare shifts new mothers towards a career that is 0.04 standard deviations more "mother-unfriendly".[23]

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.[24] Column 3 of Table 9 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 shifts new mothers toward industries with 0.02 standard deviations higher pay convexity.

Overall, childcare subsidies appear to reallocate female labor supply from female-friendly firms to male-friendly firms that offer "greedier" jobs that reward long hours. Such a reallocation is 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 above, we examine the differential impact of the 1997 childcare reform on firms with different gender employment gaps among new parents and estimate a firm-level DID introduced in Equation 2. We present results consistent with

[23]In Appendix Table A.4, we show that better childcare access also increases the likelihood of women who were unemployed before childbirth sorting into "mother-unfriendly" firms when they became employed.

[24]Industries with the highest pay convexity are retail and finance/insurance; industries with the lowest pay convexity are healthcare and agriculture.

the notion that firms that were less attractive to mothers before the reform benefit more from the reduction of workplace barriers brought on by the reform.

Table 10 presents the results. Our main variable of interest is the interaction term $HighMomEmpGap96_j \times Post97_t$, which captures the differential impact of the reform on Quebec firms that were "mom-unfriendly" vs "mom-friendly" before the reform. Panel A first examines firm employment outcomes. We find that, post reform, mom-unfriendly firms (i.e. with above median $MomEmpGap96$) experienced a 4% employment growth (column 1) and a 1.4pp increase in the share of female employees (column 2). These effects are driven by a 6% increase in the number female employees (column 3), while the number of male employees remained largely unchanged (column 4). We also find an insignificant 0.6% decrease in the average earnings of employees in mom-unfriendly firms relative to mom-friendly firms (column 5). This could reflect some downward pressure on wages due to the additional supply of female labor. However, such an interpretation can be confounded by compositional shifts in jobs. For example, if the new female workers seek more ambitious positions, this can offset the downward pressure on wages from the supply shock.

Panel B examines the effect on firm performance. We find that the inflow of female workers is associated with better performance for mom-unfriendly firms. Specifically, the reform led to a 8.6% increase in assets and a 10.3% increase in sales for mom-unfriendly firms relative to mom-friendly firms (columns 1-2). In columns 3-4, we examine the effect on ROA and asset turnover and continue to find a positive and significant effect. Finally, labor productivity (i.e., sales per employee) increased by 6.3% (column 5). This result could suggest that the newly hired female employees are more productive than existing employees, or that workforce gender diversity itself increases productivity of all workers . In Appendix Table A.5, we show that the reform also led to higher growths in employment, assets, and sales, as well as higher income per employee in mom-unfriendly firms relative to mom-friendly firms. Appendix Figure A.1 shows the dynamic DID effects for several key performance variables. Overall, these results suggest that childcare subsidies reduced labor

market segmentation across genders and benefited firms that were traditionally unattractive to mothers.

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, if the reform increases female workers' likelihood of becoming a mother (or accelerates it), it could also benefit mom-unfriendly relative to mom-friendly firms, since motherhood disrupts productivity. Hence, the magnitude of our firm-level results should not be interpreted as all driven by new mothers' sorting.

7 Conclusion

Much of the attempt to reduce gender gaps in 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. 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 in the labor market and benefits firms that traditionally attract mainly male workers.

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. That said, we are not able to fully evaluate the allocative efficiency of the

Quebec childcare subsidies, as these subsidies generate other general equilibrium/welfare effects that are hard to quantify, and need to be weighted against the costs of such subsidies.

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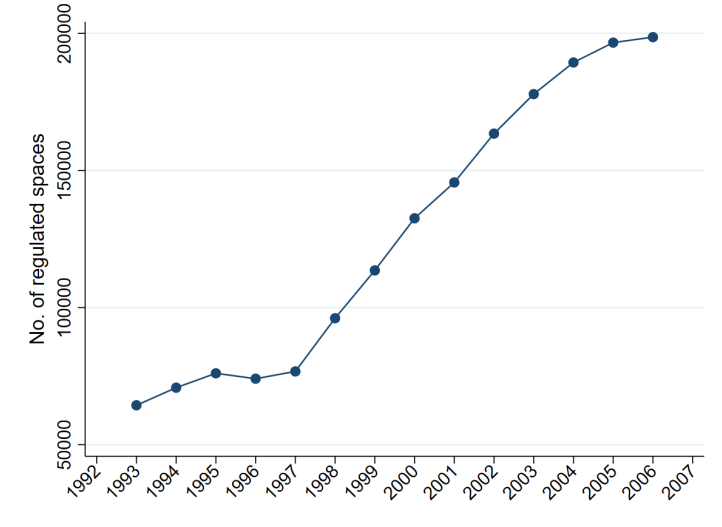
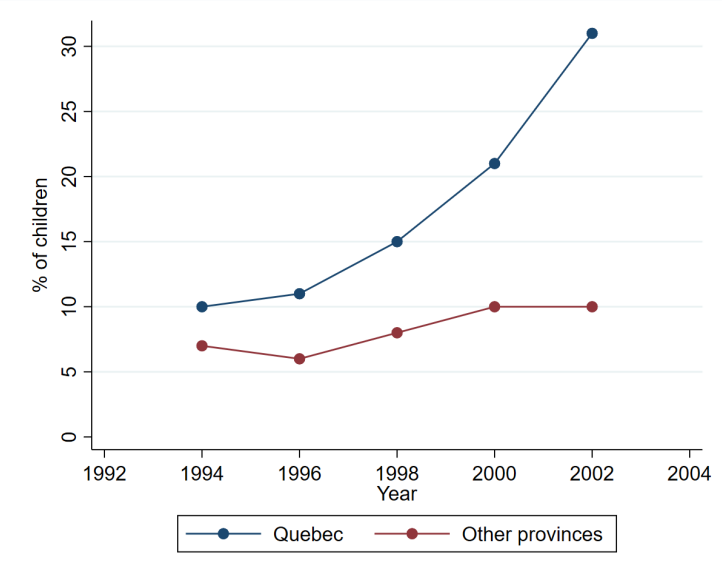
Figure 1: Childcare Provision and Usage in Quebec Around 1997



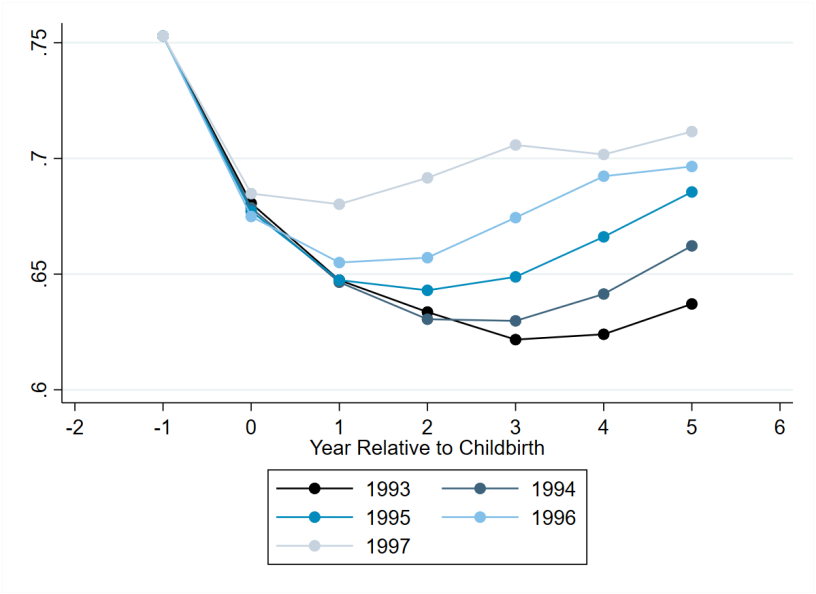
(a) Number of regulated childcare spaces



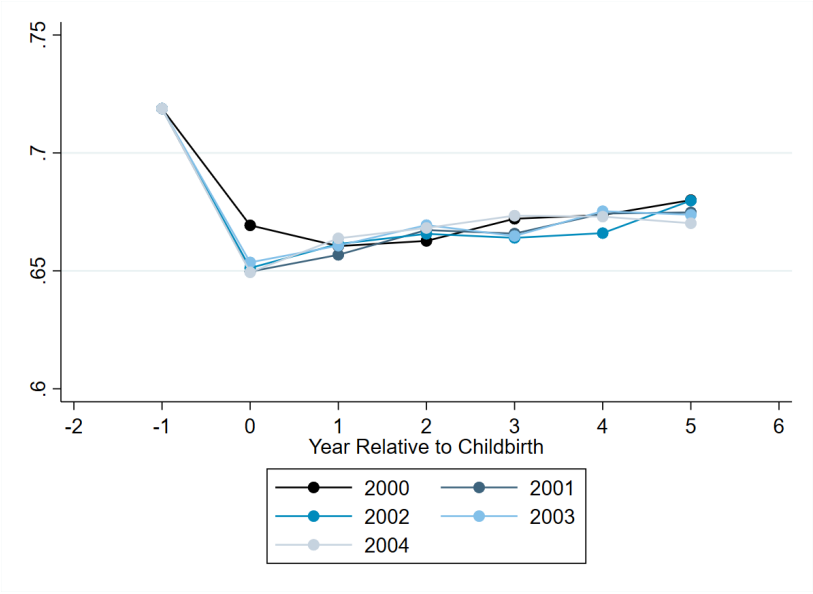
(b) Percentage of 1-5-year-olds in childcare centers

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 and non-Quebec provinces. The data come from biennial National Longitudinal Survey of Children and Youth (NLSCY).

Figure 2: Mean Employment Rate by Cohort Around Childbirth



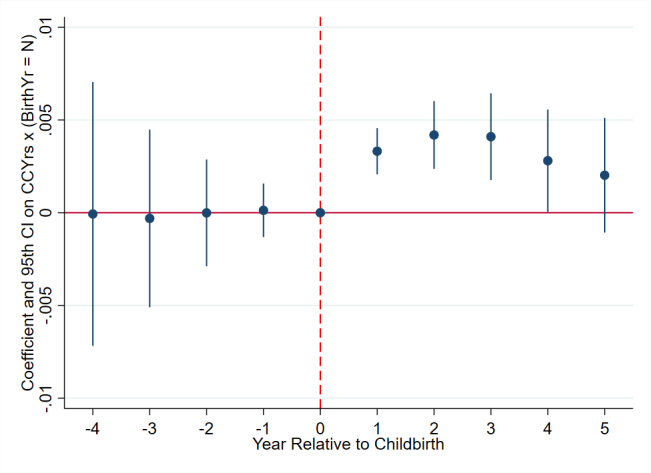
(a) Cohorts in our sample



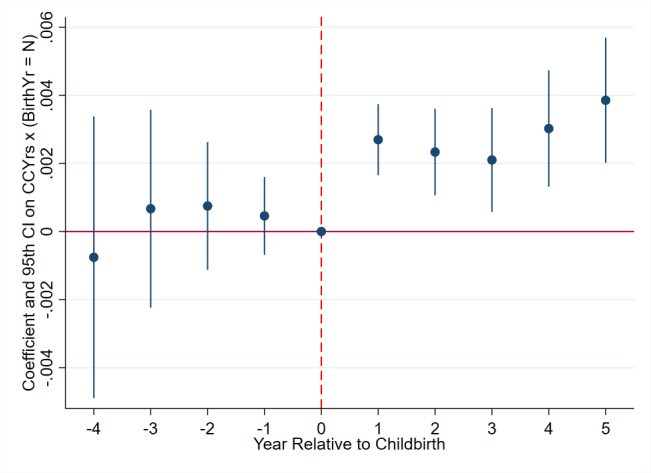
(b) Placebo cohorts

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.

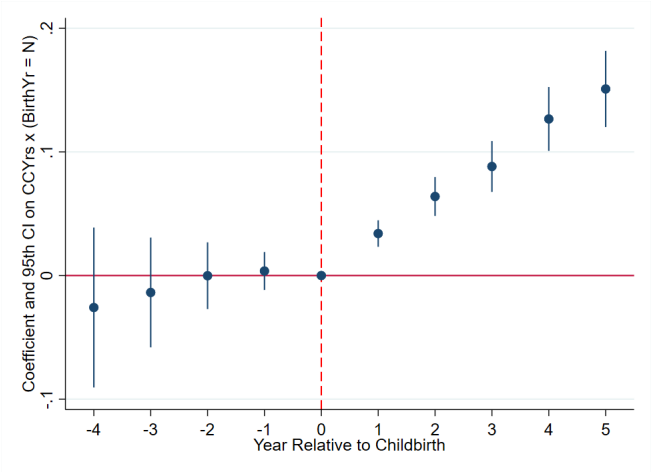
Figure 3: Dynamics Treatment Effects



(a) Employment



(b) Turnover



(c) Earnings

These figures show the dynamic DID effects estimated from Equation 3, 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 3. *Employment* is an indicator equal to one if the individual is employed. *Turnover* 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.

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

Variable	N	Mean	P5	P50	P95	Std. Dev.
			Individual-level:			
Employed						
Turnover						
Turnover_T4						
Raw earnings						
Earnings (scaled)						
Promotion						
Demotion						
Married			Pending Disclosure Review			
Age						
Leave pre-birth employer						
Sick leave						
Earnings growth						
			Firm-level:			
GenderEmpGap96						
Quebec						
Ln(emp)						
Female ratio						
Ln(female emp)			Pending Disclosure Review			
Ln(male emp)						
Ln(sales)						
Sales growth						
ROA						
Asset turnover						
Labor productivity						

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)	(3)
		Employed	
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)
	Turnover			Turnover_T4		
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. *Turnover* (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). *Turnover_T4* (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) Demotion	(6) Demotion
CCYears × PostBirth	0.0432***	0.0150***	0.0163***	0.0138***	-0.0106***	-0.0075***
	[0.0054]	[0.0039]	[0.0008]	[0.0009]	[0.0009]	[0.0010]
Individual FE	X		X		X	
Year FE	X	X	X	X	X	X
Event year FE	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	1,437,530
Ad. R-squared	0.603	0.781	0.454	0.430	0.425	0.351

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. *Demotion* is an indicator equal to one if the individual current earnings is less than 90% of her pre-childbirth earnings. 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) Employed	(2) Turnover	(3) Earnings
CCYears × PostBirth	0.0003	-0.0004	0.0108*
	[0.0006]	[0.0005]	[0.0057]
CCYears × Female × PostBirth	0.0032***	0.0027***	0.0325***
	[0.0008]	[0.0006]	[0.0079]
Individual FE	X	X	X
Year-Sex FE	X	X	X
Event year-Sex FE	X	X	X
Observations	5,459,330	3,564,880	3,554,900
Ad. R-squared	0.552	0.059	0.624

This table reports the triple-difference results comparing male and female for our three main outcomes. The specification is based on Equation 4. 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			Turnover			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

	(1)	(2)	(3)	(4)	(5)	(6)
	Sick leave		School leave		New child	
CCYears × PostBirth	-0.0010***	-0.0011***	0.0003	0.0002	-0.0030***	-0.0030***
	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0004]	[0.0004]
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				
Observations	1,692,000	1,692,000	1,686,920	1,686,920	1,686,920	1,686,920
Ad. R-squared	0.025	0.025	0.079	0.09	0.067	0.067

This table examines the effect childcare access on other individual-level labor outcomes. 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. *New child* is an indicator equal to one if the individual had a new child in that year after 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

	(1) MomEmpGap96	(2) MomEmpGap96	(3) PayConvexity
CCYears × PostBirth	0.0009***	0.0010***	0.0006**
	[0.0002]	[0.0002]	[0.0003]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Industry FE		X	
Observations	1,515,440	1,515,440	1,498,990
Ad. R-squared	0.614	0.666	0.757

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 columns 1-2, the dependent variable *MomEmpGap96* is a firm's employment gap between new mothers and new fathers as a fraction of all employees, measured in 1996, the year before the reform. We define new mothers/fathers as those with kids of age 0 to 5. In column 3, the dependent variable *PayConvexity* is an industry-level (NAICS 2-digit) measure of elasticity of annual earnings to weekly hours. The measure is aggregated from occupation-level estimates from Goldin (2016) using occupation-industry crosswalk, based on occupation weights within each industry. 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

Panel A: Employment Outcomes

	(1) Ln(emp)	(2) Female ratio	(3) Ln(female emp)	(4) Ln(male emp)	(5) Ln(avg earnings
HighMomEmpGap96	0.0406***	0.0139***	0.0597***	0.0069	-0.0062
× Post97	[0.0059]	[0.0015]	[0.0059]	[0.0064]	[0.0040]
Firm FE	X	X	X	X	X
Industry-Year FE	X	X	X	X	X
Observations	504,580	545,660	459,000	503,140	504,580
Ad. R-squared	0.871	0.870	0.896	0.887	0.794

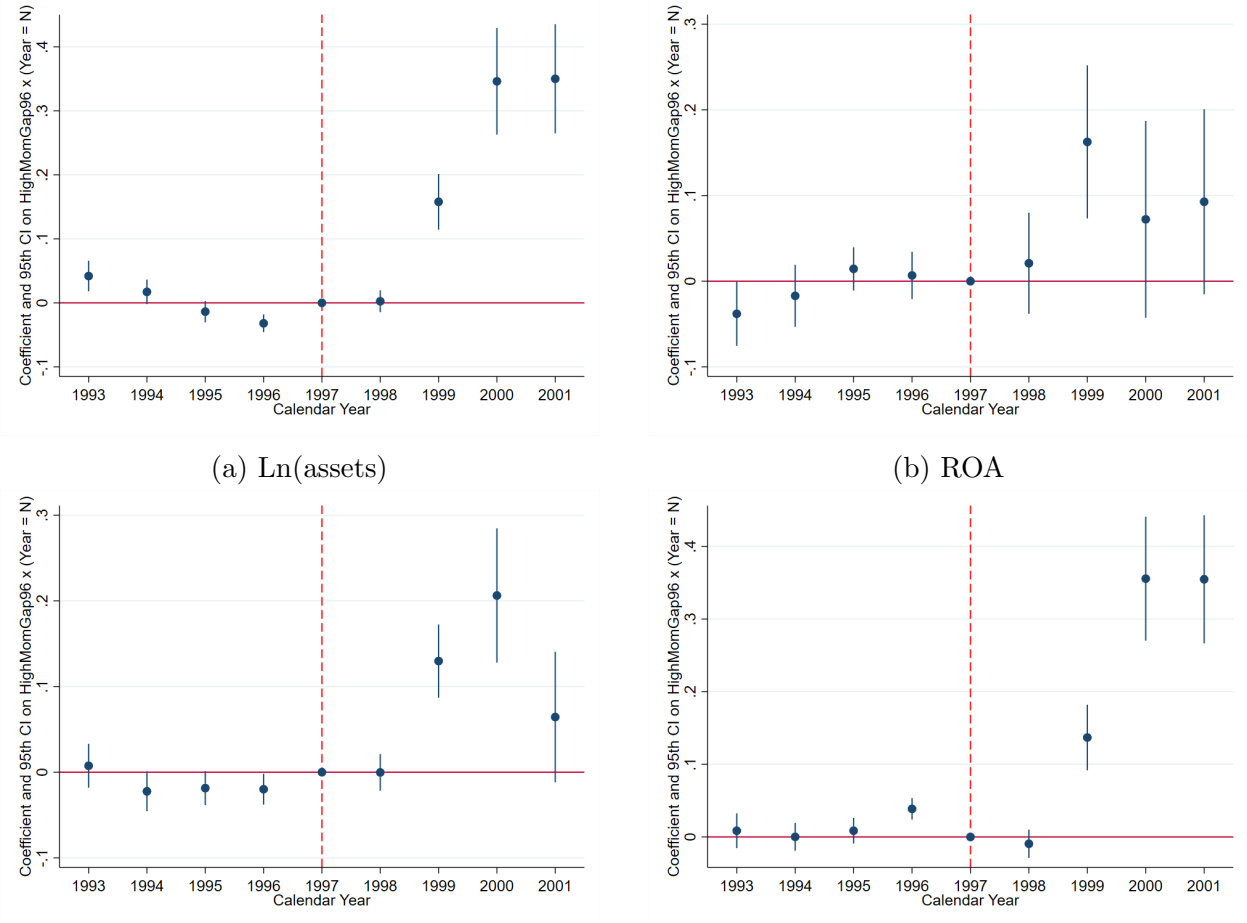
Panel B: Firm Performance

	(1) Ln(assets)	(2) Ln(sales)	(3) ROA	(4) Ln(asset turnover)	(5) Ln(labor prod)
HighMomEmpGap96	0.0864***	0.1030***	0.0879***	0.0601***	0.0628***
× Post97	[0.0125]	[0.0128]	[0.0294]	[0.0112]	[0.0134]
Firm FE	X	X	X	X	X
Industry-Year FE	X	X	X	X	X
Observations	535,060	533,890	442,010	504,180	490,220
Ad. R-squared	0.814	0.773	0.21	0.541	0.742

This table examines the impact of the Quebec reform on firm-level outcomes, in particular employment outcomes (Panel A) and financial performance (Panel B). The specification follows Equation 2. All columns include firm fixed effect and industry-year fixed effects. *HighMomEmpGap96* is an indicator equal to one if a firm had above median fraction of new fathers relative to new mothers among all employees in 1996 (new parents are parents whose first child was age 0-5 as of 1996. *Female ratio* is the fraction of female employees. *ROA* is pre-tax income divided by total assets. *Asset turnover* is sales divided by total assets. *Labor prod* is 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(assets)

(b) ROA

(c) Asset turnover

(d) Labor productivity

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

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \sum_{t=1993}^{2001} \theta_t \times HighMomEmpGap96_j \times Year_t + \epsilon_{j,t},$$

Each dot (bar) represents the point estimate (95th confidence interval) for the coefficient on $HighMomEmpGap96_j \times Year_t$. The four panels correspond to the dynamics for assets, ROA, asset turnover 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., tthe 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 and Samples

Panel A: Alternative defintions 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: Rule out pregnancy/birth timing: cohorts 1993-1997

	(1)	(2)	(3)
	Employed	Turnover	Earnings
CCYears × PostBirth	0.0030***	0.0025***	0.0211***
	[0.0008]	[0.0007]	[0.0078]
Birth year cohorts	1993-1997	1993-1997	1993-1997
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

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.

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

Panel A: Age bin fixed effects

	(1) Employed	(2) Turnover	(3) Earnings
CCYears × PostBirth	0.0031***	0.0023***	0.0381***
	[0.0006]	[0.0005]	[0.0053]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Age bin FE	X	X	X
Observations	2,731,040	1,692,000	1,686,920
Ad. R-squared	0.532	0.062	0.603

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

	(1) Employed	(2) Turnover	(3) Earnings
CCYears × PostBirth	0.0030***	0.0023***	0.0315***
	[0.0006]	[0.0005]	[0.0049]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Pre-birth char. × event year FE	X	X	X
Observations	2,731,040	1,692,000	1,686,920
Ad. R-squared	0.542	0.063	0.683

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: Sorting into Firms: Robustness

Panel A: Sorting by Those Unemployed Before Childbirth

	(1)	(2)
	Employed in	
	HighMomGap96	LowMomGap96
CCYears × PostBirth	0.0027***	0.0018*
	[0.0007]	[0.0009]
Individual FE	X	X
Year FE	X	X
Event year FE	X	X
Observations	723,150	723,150
Ad. R-squared	0.198	0.218

Panel B: Sorting Heterogeneity

	(1)	(2)	(3)
		MomGap96	
CCYears × PostBirth	0.0012***	0.0022***	0.0036***
	[0.0003]	[0.0003]	[0.0003]
Married × PostBirth	0.0037***		
	[0.0012]		
CCYears × Married × PostBirth	-0.0006**		
	[0.0003]		
Older × PostBirth		0.0100***	
		[0.0012]	
CCYears × Older × PostBirth		-0.0022***	
		[0.0003]	
HighEarn × PostBirth			0.0223***
			[0.0012]
CCYears × HighEarn × PostBirth			-0.0050***
			[0.0003]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Observations	1,515,440	1,515,440	1,515,440
Ad. R-squared	0.614	0.614	0.614

Panel A examines how childcare access affects the sorting of mothers who were unemployed before childbirth. The dependent variable in column 1 (2) is an indicator equal to one if the individual is employed in a firm with above (below) median mom employment gap in 1996, and is equal to zero if the individual is otherwise employed or unemployed. Panel B examines heterogeneity of our baseline sorting result with respect to marital status, age, and earnings.

Table A.5: Impact on Firm Performance: Other Outcomes

	(1) ln(emp growth)	(2) Ln(asset growth)	(3) Ln(sales growth)	(4) Ln(income/emp)
HighMomEmpGap96	0.0448***	0.0382***	0.0512***	0.1260***
× Post97	[0.0061]	[0.0080]	[0.0083]	[0.0374]
Firm FE	X	X	X	X
Industry-Year FE	X	X	X	X
Observations	503,620	507,170	505,420	292,060
Ad. R-squared	0.186	0.146	0.133	0.667

This table examines the impact of the Quebec reform on alternative firm-level outcomes, in particular the growths of employment, assets, and sales, as well as income per employee. The specification follows Equation 2. All columns include firm fixed effect and industry-year fixed effects. Standard errors are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.