NBER WORKING PAPER SERIES

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Working Paper 33835
http://www.nber.org/papers/w33835

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2025

We thank Sarah H. Bana, Jennifer Brown, Irem Demirci, Isil Erel, Marina Gertsberg, Andrey Golubov, Will Gornall, Kai Li, Menaka Hampole, Alex He, Spyridon Lagaras, Adrien Matray, Hernan Ortiz-Molina, Elena Pikulina, Maximilian Rohrer, Annalisa Scognamiglio, Janis Skrastins, Ben Zhang, and seminar and conference participants at the ASSA, FOM Conference, Olin Corporate Finance Conference, Labor and Finance Group Conference, UBC Summer Finance Conference, Colorado Finance Summit, CSEF-RCFS Conference on Finance, Labor and Inequality, EFA, CICF, FIRS, NFA, MFA, Telfer Conference on Corporate Finance, Boston College, Texas A\&M University, University of Oklahoma, University of Iowa, University of Georgia, Laval University, UVA Darden, Bilkent University, Fordham University, Renmin University, Peking University, and Innovation, Science and Economic Development Canada (ISED) for helpful comments. We also thank the Social Sciences and Humanities Research Council of Canada (SSHRC) Productivity Partnership Grant for funding this project. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 33835
May 2025
JEL No. G30, G38, J13, J16, J2, J6, M5

ABSTRACT

We study the effects of government-subsidized childcare on women's careers and firm outcomes using linked tax filing data. Exploiting cohort-level variation in childcare access based on a Quebec universal childcare reform, we show that earlier access to childcare not only increases new mothers' employment and earnings, but also prompts them to reallocate careers to firms previously unattractive to new mothers. These firms subsequently benefited from the reform, drawing more young, productive female workers and experiencing better performance. Our results suggest that childcare frictions hamper women's career progression and the allocation of human capital in the labor market.

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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, 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). A commonly suggested 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 impacts 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). Importantly, there is little evidence about how such policies affect female employees' career progression and, ultimately, firm outcomes. This paper fills the 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 labor allocation constraints and reducing frictions that generate gender-segmented labor markets.

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

[1] Canada and Australia both passed major childcare subsidies in recent years, with the Canadian federal government reaching agreements 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.

timing of childbirth relative to the reform across different cohorts of mothers 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 labor outcomes before and after childbirth across different cohorts. Our baseline specification includes individual fixed effects, calendar year fixed effects, and event year (i.e., year relative to childbirth) fixed effects.

We use administrative tax filing data from Statistics Canada for our analysis. The data contain information on individuals' employment, earnings, family structure, reasons for job separations, as well as firms' financial information. Such family-employee-employer linked administrative data is important in tracing out the career impact of childcare access, 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, as documented in Baker et al. (2008), Goux and Maurin (2010), and Bauernschuster and Schlotter (2015). We further differentiate the employment response based on employment status prior to childbirth. We show that the positive employment effect is stronger among mothers who were not employed before childbirth, and is weaker among those employed before childbirth. This result suggests that childcare subsidies have limited employment effects on women who were already attached to the labor force, and are effective in drawing those who were previously non-employed into the labor force.

The insignificant employment response by previously-employed new mothers, however, potentially masks important intensive margin effects in terms of earnings, job switching, and worker-firm sorting. Studying these effects forms 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 induced by childbirth may 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 new mothers 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 more pronounced 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 as proxied by sick leave (Bennedsen et al., 2019), suggesting that subsidized childcare increases women's productivity at work. The reform also led new mothers to delay the births of their subsequent children after the first child, consistent with childcare access setting new mothers 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 difference-in-differences analysis, we find no evidence of pre-birth trends in these outcomes, supporting the parallel trends assumption 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 that 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-event year fixed effects to absorb secular trends that impact women's career choices surrounding childbirth. Last, to rule out the concern of pregnancy or birth timing, we show that our results remain similar when focusing on the cohorts who made fertility decisions before the reform.

We then examine the effect of the reform on female workers' sorting into firms with different characteristics. We show that access to subsidized childcare increased the likelihood that new mothers sort into firms they traditionally find unattractive. We proxy for firms where new mothers might find it costlier to work using the fraction of mothers with young children employed at a firm before the reform. This measure broadly captures firm-level characteristics that make certain firms more appealing to new mothers than others, such as family-friendly HR policies and flexible work schedules. We show that childcare access increased new mothers' sorting into firms that previously employed fewer mothers before the reform. We find similar results using two alternative firm type measures. First, we identify "greedier" jobs, i.e., jobs with higher earnings-hours elasticity (Goldin, 2014), that are less attractive to new mothers who value flexible hours or time with their young children. Second, we exploit heterogeneity in firm size. Smaller firms are known to offer lower quality maternity benefits (Liu et al., 2023) and should thus be less appealing to new mothers. Using these measures, we find that childcare subsidies also increased new mothers' sorting into firms with higher pay convexity and smaller firms. These findings are consistent with the career upgrade channel revealed by our individual-level results: lowering childcare frictions allowed new mothers to pursue more demanding careers that they previously found unattractive.

Finally, we examine the impact of the childcare reform on firm outcomes. We document heterogeneity in the impact of the reform on Quebec firms. Consistent with the individual-level sorting result, we show that firms that had lower shares of new mothers before the reform gained employment post reform relative to other firms, an increase driven mostly by young female employees. These firms also experienced better performance, measured by higher sales growth, ROA, and labor productivity. Such outperformance can be explained by these firms drawing more productive female workers post reform, while at the same time keeping their labor costs low relative to other firms, as evidenced in their lower wage growth. Together, these firm-level results suggest that childcare subsidies increased female labor supply to firms traditionally unattractive to new mothers, benefiting such firms.

Overall, our results suggest that government policies that support childcare can promote women's career progression beyond increasing employment. These policies can also reduce gender gaps in the labor market by allowing talent to flow more freely between gender-segmented firms and sectors. A back-of-the-envelope calculation comparing program costs with incremental tax revenues suggests that the Quebec universal childcare program had a positive fiscal net present value. Nevertheless, the program may have other costs and benefits (e.g., social or health effects) we cannot quantify.

Our paper contributes to the literature studying the effect of providing non-wage amenities on workers' career and firm outcomes. Studies have shown that maternity leave increases job continuity (Baker and Milligan, 2008) and female entrepreneurship (Gottlieb et al., 2022), and can be used by firms to attract and retain female talent (Liu et al., 2023). Bennett et al. (2020) show that paid family leave improves firm productivity by reducing employee turnover. We differ by showing that childcare subsidies *increase* 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. As such, childcare subsidies may be more effective in reducing cross-firm gender segmentation than leave policies.

Specific to the topic of childcare, prior studies have 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; Brodeur and Connolly, 2013; Havnes and Mogstad, 2011b).[2] Several papers also document childcare as a driver of gender disparity in the 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

[2]Other papers include Lefebvre and Merrigan (2008), Lefebvre et al. (2009), Cascio (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.

earnings and are more likely to be promoted after childbirth. We differ from these papers in three 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. Third, we document the heterogeneous impact of childcare frictions on firm performance.

Our paper also relates to the literature on women's career and firms more generally. Several papers highlight gender disparities in certain sectors or occupations. Lagaras et al. (2022) show that female talent sort relatively less into the financial sector, driving larger gender pay gaps in that sector. Reuben et al. (2014) and Sapienza et al. (2009) document how gender stereotypes and differences in risk-aversion affect women's career choices. Baghai et al. (2023) find that women are less protected than men against idiosyncratic firm shocks. Several papers also provide evidence of potential solutions to gender gaps in the labor market. Bennedsen et al. (2022) show that 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 childcare access improve women's career outcomes and can benefit firms.

2 The Quebec 1997 Reform

Our empirical strategy exploits the introduction of universal childcare in Quebec in 1997, which is the centerpiece of the 1997 Quebec Family Policy ("Politique Familiale").[3] The

[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 Saskatchewan on agriculture), Quebec's economy is well diversified across a large number of sectors.

policy provided childcare for children aged zero to four at a subsidized rate of $5 CAD per day, at a time when the median childcare cost was $11 per day (Lefebvre and Merrigan, 2008).[4] 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 limited childcare subsidies prior to 1997. 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.

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 (Appendix Figure IA.1 Panel A), while program funding increased from $288 million to $1.6 billion over the same period. As a result of this reform, the percentage of Quebec children aged 0-5 in childcare centers increased from 11% in 1996 to 31% in 2002 (Appendix Figure IA.1 Panel B) (Baker et al., 2008; Lefebvre and Merrigan, 2008). 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

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

childcare providers who were left out of the subsidy program (Philp, 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's stated objectives were to fight family poverty and enhance child development and equality of opportunity (Lefebvre and Merrigan, 2008), not to promote parental employment or firm productivity.

Important for our identification, Quebec did not implement a welfare-to-work program around 1997 like other major Canadian provinces did. Such programs, which require welfare recipients to work as a condition of receiving benefits, increased single mothers' financial incentives to enter the workforce in other provinces (Agostinelli et al., 2020).[5] Due to such confounding policy shifts in other provinces, we do not use non-Quebec provinces as a control group in our empirical analysis, but rather exploit more granular cohort-level 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 of the reform, 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 were not yet eligible for the subsidy (i.e., they would enroll at the unsubsidized price). This strategic response from parents to "claim a spot" in the system may also reflect intertemporal smoothing of expected future subsidies.[6] Although parents of ineligible

[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 individuals becoming eligible is prevalent. See Attanasio and Rohwedder (2003), Card and Hyslop (2005), and Card et al. (2007) for examples.

children paid 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 across birth cohorts in the number of years that parents *knew about* the subsidy program rather than years of subsidy eligibility.

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 all Canadian employees and their matched employers from 1989 to 2013. The dataset is built from four tax files: the T1 personal income tax file, the T2 corporate income tax file, the T4 employee remuneration file, and the 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 the timing of childbirth. 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 labor and financial outcomes. This allows us to trace the career and productivity impact of the policy from the individual level to the firm level. Our data represents one of the most comprehensive data sets assembled to study childcare and employment.

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

[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.

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] Prior research on the child penalty has also focused mainly on the first child as the point of reference in designing event studies (Kleven et al., 2019, 2020, 2022).

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 still requires childcare (i.e., not old enough to enter kindergarten) in 1997. We end with the 1999 cohort to ensure our results are not affected by the federal paid parental leave extension at the end of 2000.[9] We track each cohort from four years before to five years after childbirth, resulting in calendar years from 1989 to 2004 in our sample .

Our firm-level sample covers the period from 1994 to 2000. 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 as of 1996 (the year preceding the reform).

Table 1 provides summary statistics for our sample of mothers at the individual-year level. All earnings are denominated in Canadian dollars (CAD). Over the window of [-4, 5] years surrounding first childbirth, the average employment rate is 65%. In comparison, the average employment in the year before childbirth is 71%, consistent with a decline in employment post childbirth. The average quitting rate is 5.3%, while the employment switching rate is 23%, suggesting many turnovers are involuntary. The median earnings is $15K CAD ($26K in 2024 CAD), and the median ratio between post-birth and pre-birth earnings is 0.995. In the year before childbirth, 44% of the mothers were married, with a

[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.

[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.

median age of 27 and median earnings of $13.8K CAD. On average, mothers in our sample had 0.9 additional children over the 10 years since their first birth. Firms in our sample on average employ, as a proportion of their total workforce, 39% female workers and 24% female workers below age 35. Their median ROA is 4.1% and median log sales growth is 2.1%. Detailed variable definitions are provided in Appendix A .

4 Empirical Strategy

4.1 Individual-level Specification

Our empirical strategy at the individual level exploits the differential effects of the 1997 subsidy 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 child age fixed effect and control for the changing need for childcare that varies with child age, as well as women's changing life patterns and preferences around childbirth. In our more stringent specifications, we also include industry (or industry-year) fixed effects as well as firm fixed effects (wherever appropriate) to identify within-industry and within-firm variation.[10] We cluster standard errors at the

[10]We define industry using 4-digit NAICS codes. In a robustness test, we also include fixed effects for individuals' age bins. We present these results in Panel A of Appendix Table IA.3.

individual level.

Our key treatment variable is $CCYears_i \times PostBirth_{i,t}$, where $PostBirth_{i,t}$ is a dummy indicating years after an individual gives birth. $CCYears_i$ is a cohort-level treatment intensity variable that indicates the number of years of childcare access post reform 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 $CCYears_i \times PostBirth_{i,t}$ 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 the 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 IA.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.

Table 2 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

[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.

parents both qualified for childcare subsidy when their child turned three, but the 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 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.

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., 2024). 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 focuses on 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 (described in Section 5.2). We expect to find limited effects among male individuals if childcare responsibilities are the main driver behind our results.

We examine three sets of individual-level outcomes: employment, turnover, and earnings. We define employment as an indicator 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 and their record of employment (ROE) indicated "quit" as 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 firms with different characteristics. To this end, we construct several measures to capture firms that were traditionally unappealing to new mothers. 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. This measure captures firms that were traditionally unattractive to mothers of young children. For robustness, we also 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). We additionally use a firm's relative size within the industry as a proxy for the quality of maternity benefits offered, as smaller firms tend to provide fewer maternity benefits (Liu et al., 2023). We define dependent variables as working in a firm with these characteristics to examine sorting by new mothers across firms.

4.1.1 Validating Sources of Identification

We first present evidence consistent with the fact that childcare take-up increased in response to the reform. Since we do not observe childcare take-up within our sample, we use the National Longitudinal Survey of Children and Youth (NLSCY) to validate the anticipatory and the eligibility effects of the reform on childcare take-up.[12] We focus on Quebec children aged 0-4 and create two treatment indicators indicating anticipatory years and eligible years as in Table 2, with pre-reform years as the control years. Appendix Table IA.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. Thus, both margins

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

are important for our analysis.

To further validate our cross-cohort comparison, Figure 1(a) plots out the mean employment rates for Quebec women by cohort from one year before to five years after childbirth.[13] We see a clear difference across cohorts in the 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. These inflection points seem to precisely match the different ages at which adjacent cohorts are subject to the subsidy program (Table 2).

Importantly, this diverging pattern is driven by variations in both anticipatory years and eligibility years. To see this more clearly, consider the 1993 and 1994 cohorts highlighted in Appendix Figure IA.2(a). Since both cohorts became eligible for the subsidy at the same age (Table 2), the pattern indicates that the difference comes from the anticipatory earlier response by the 1994 cohort. Similarly, when we compare the 1994 and the 1995 cohorts (Appendix Figure IA.2(b)), we find a stronger response for the 1995 cohort, who had the the same number of anticipatory years as the 1994 cohort but one additional eligible year. Similar patterns can be observed for the 1995/1996 and 1996/1997 cohort pairs. These results are consistent with Appendix Table IA.1 showing that childcare take-up increased for both the anticipatory years (due to the higher supply of childcare seats) and the eligibility years (due to the higher supply of childcare seats and the subsidy).

It is worth mentioning that Figure 1(a) does not control for secular trends in female employment. However, this does not seem to be explaining the differential responses across cohorts. In particular, Figure 1(b) shows no divergence patterns among the 2000 to 2004 cohorts, whose children were always eligible for the subsidy (i.e., no variation in treatment). This placebo result suggests that the employment effect in Figure 1(a) is unlikely to be driven by secular trends in female employment. Additionally, any secular trend should shift

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

employment in parallel across cohorts instead of inducing divergence at a specific child age.

5 Individual-Level Results

5.1 Baseline Results

We first examine the effect of childcare access 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, accessing childcare earlier by five years—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 the 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, which is sizable given that a large part of child penalty is driven by gendered preferences rather than gendered incentives (Kleven, 2022; Kleven et al., 2022). Nevertheless, our estimate is smaller than those 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 effect, that is, the effect of childcare *access*, which should be smaller than the treatment effect of childcare take-up.

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 non-employed 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 sufficiently attached to the labor force that they do not drop out for

[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).

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 labor outcomes conditional on being employed the year before childbirth. We first examine turnover in Table 4. In columns 1-3, we define voluntary turnover based on "quits" in the records of employment (ROE), and in columns 4-6, we examine employer changes based on T4 filings. We find that earlier access to childcare significantly increases the likelihood that new mothers voluntarily switch employers.[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 turnover identified from T4 employer changes (columns 4-6), with similar magnitudes. In Panel A of Appendix Table IA.2, we also show that new mothers with better childcare access 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 pre-childbirth. 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

[15]Note that non-employed in the year before childbirth does not include individuals on maternity leave. We define non-employment 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).

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

childcare significantly increases a woman's earnings relative to her pre-childbirth earnings. In particular, the coefficient in column 1 indicates 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] Increases in work hours may also explain some of the earnings increase, but we do not observe hours worked 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 as proportional earnings increases above 10% and demotions as proportional earning decreases below -10% relative to pre-childbirth earnings.[18] We find in columns 3-5 that accessing childcare earlier by five years increases the likelihood of promotions by 8.2pp (41% of mean), the likelihood of within-firm promotions by 6.9pp (28% of mean), and the likelihood of between-firm promotions by 7.9pp (57% of mean). Columns 6 to 8 show the opposite effect on demotions: a five-year earlier access leads to a 5.3pp (19% of mean) decrease in the likelihood of demotions, a 3.8pp (15% of mean) decrease in the likelihood of within-firm demotions, and a 1.4pp (14% of mean) decrease in the likelihood of between-firm demotions. 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 are unlikely to 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 only represents a positive wealth shock for those who would have paid for the more expensive private childcare. For such individuals, a positive wealth shock would likely

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

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

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

5.2 Robustness

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

First, we provide evidence in support of parallel trends in our main outcomes using a dynamic DID specification:

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

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 2 shows the results. We find that the cohorts did not exhibit significantly different trends in all three outcomes (employment, turnover, and earnings) in the years before childbirth, but diverged significantly 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

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

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. However, 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 access on women's careers.

Second, one may be concerned that our results are driven by secular trends across different cohorts of mothers. While the placebo graph in Figure 1b, discussed in Section 4.1.1, should largely alleviate this concern, we further address it by saturating our benchmark 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 presented in Panel B of Appendix Table IA.3 and are similar to our baseline results.

Third, to further sharpen identification, we conduct a triple-difference analysis using men as a benchmark group. This analysis also helps address concerns about unobserved differences across cohorts of mothers. 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. $\beta_{s,t}$ and $\gamma_{s,y}$ indicate calendar year $\times$ gender fixed effects and event year $\times$ 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 $\times$ event year fixed effects ($\delta_{c,y}$) to absorb secular trends in labor market across cohorts of par-

ents. Exploiting the availability of spousal identifiers in the T1FF data, we also include family-year fixed effects ($\sigma_{f,t}$) to compare mothers and fathers within the same household, and to absorb unobserved household-level shocks.[20]

Table 6 presents the 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 the inclusion of family-year fixed effects restrict our sample to married mothers who have a much weaker employment response than single mothers as we show in the next section. Using a family-year level panel, we further show that childcare access reduced the gender wage gap between husband and wife within the same household (Appendix Table IA.5).

Finally, to address concerns about potential pregnancy or birth timing in response to the reform, we restrict our sample to the 1993-1997 cohorts. These cohorts already made fertility decisions before the announcement of the reform. Appendix Table IA.4 shows that the results remain similar. We also show in Appendix Table IA.6 that our results are robust to excluding employees in the public sectors or excluding part-time employees.

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-

[20]Note that in both equations, $PostBirth_{i,t} \times Female_i$ is absorbed by gender $\times$ 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 $\times$ event year fixed effects.

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.[22] Furthermore, the increased supply of childcare spaces should benefit all parents regardless of their socioeconomic 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. Importantly, our specification allows us to include cohort $\times$ event-year fixed effects to absorb unobserved trends across cohorts of mothers.[23] Consistent with our finding, Kimmel (1998), Cascio (2009), Goux and Maurin (2010) also document stronger employment response to childcare access by single mothers, though using different settings.

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 implies that childcare access might increase productivity. Such an effect persists after we control for industry fixed effects (column 2), suggesting it is not driven by mothers switching to industries with lower work intensity.[24] 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 do not find a significant increase in the likelihood of taking a leave for schooling or further education. These results

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

[22] Prior to 1997, Quebec already provided some childcare subsidies for low-income families, including child tax credits that decrease with income.

[23] Table IA.7 shows that we find similar heterogeneity effects without cohort $\times$ event-year fixed effects.

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

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 may encourage fertility by decreasing the cost of childbearing, but childcare access may also set new mothers on a more demanding career path, discouraging further births. We examine this in Panel B of Table 8, where the dependent variable is the total number of subsequent kids a mother had over the 5 years (or 10 years) after the birth of the first child. We estimate this regression using a Poisson pseudo maximum likelihood (PPML) model on a cross-section of mothers. We find that childcare subsidies discourage subsequent fertility in the short run, but have no effect in the long run. This suggests that new mothers delayed subsequent childbirths, likely due to their more demanding careers.

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 how firms with different characteristics are affected by the reallocation of female labor supply induced by childcare subsidies, as indicated by the turnover results discussed above.

Our results on turnover, earnings, and productivity suggest potential career upgrades by new mothers due to increased childcare access. Becker (1985) argues that childcare responsibilities result in women seeking less demanding careers. Therefore, we should expect new mothers with better childcare access to take on more ambitious or demanding careers that they previously lacked the time or flexibility to pursue. Motivated by this, we construct proxies for firms that were previously unattractive to mothers of young children before the

reform, and check whether mothers with greater childcare access were more likely to sort into these firms.

Our primary measure, *Low%Mom*, is a dummy variable equal to one if a firm had a below-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 *Low%Dad* to capture firms with a below-industry-median percentage of fathers of young children in 1996. The *Low%Mom* measure is meant 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.[25] 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 at a higher rate than earlier cohorts. Further, if these barriers are gender-specific, we should see limited sorting into low-%dad firms. It is important to note that such firms are not necessarily gender-biased or averse to hiring women. Instead, our measures capture an equilibrium outcome in which new mothers are less likely to be employed at such firms, due to either supply or demand side factors.

Panel A of Table 9 tests sorting along this dimension. We find that earlier access to childcare increases new mothers' sorting into firms traditionally less appealing to new mothers (columns 1-2). Specifically, accessing childcare earlier by five years shifts new mothers towards *Low%Mom* firms 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

[25] Appendix Table IA.8 shows that *Low%Mom* firms tend to be smaller, more productive, more capital intensive, and similarly profitable compared with *High%Mom* firms.

across firms. In contrast, we do not see a similar sorting into *Low%Dad* firms. This suggests that the childcare barriers mitigated by the reform are gender-specific. Figure 3a shows the dynamics for sorting into *Low%Mom* firms. We find evidence largely consistent with parallel trends across cohorts before childbirth.

For robustness, we examine two alternative proxies for firms that are unattractive to mothers. We first examine the convexity of pay relative to work hours, a job characteristic previous research has shown to be undesirable by 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 those in the legal and banking professions. 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 *High Pay Convexity*, a dummy indicating firms in 2-digit-NAICS industries with above-median pay convexity. Industries with the highest pay convexity include retail and finance, while those with the lowest pay convexity include healthcare and agriculture. Column 1 of Appendix Table IA.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, accessing childcare earlier by five years increases the probability of a new mother working in a high-pay-convexity industry post-birth by 1.4%.

Finally, we examine the sorting of new mothers into firms of different sizes. Prior literature shows that firm size correlates with the provision of maternity benefits by firms, with smaller firms generally offering lower quality benefits (Liu et al., 2023). Larger firms may also be able to offer more flexible work arrangements to their employees by having employees share job responsibilities (Kotey and Koomson, 2021). We therefore use *Large Firm*, a dummy indicating firms with above-industry-median sales in 1996, as a dependent variable in our benchmark specification. Columns 2-3 of Appendix Table IA.9 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 work flexibility. 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 high-%mom and low-%mom firms. We then examine how these effects differ by the attractiveness of the pre-birth employer to young mothers (*High%Mom_PreBirth*). Panel B of Table 9 shows the results. We first find that childcare access increased job switching mainly among mothers previously employed at high-%mom firms pre-birth, while those working at low-%mom firms pre-birth had a limited response (column 1). Further, neither group of mothers switched to high-%mom firms in response to childcare access (column 2). Instead, switching largely occurred from high-%mom firms to low-%mom firms (column 3). These results suggest that our sorting effect mainly reflects increased switching to low-%mom firms rather than decreased switching to high-%mom firms).

Overall, childcare subsidies appear to reallocate female labor supply from firms traditionally more appealing to mothers of young children to those traditionally less appealing. 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, as employers raise their expectations of new mothers' productivity. Regardless, these reallocation effects are likely to reduce gender employment gaps across firms. We next examine how such reallocation impacts firms traditionally unattractive to mothers by giving them access to a larger supply of female workers.

6.2 Impact of the Reform on Firm Performance

Motivated by the individual-level sorting results, we examine the differential impact of the 1997 childcare reform on firms with different levels of attractiveness to new mothers. We

present evidence consistent with the notion that firms that were less attractive to mothers before the reform benefited more from the reduction in workplace gender barriers resulting from the reform.

Specifically, we employ a firm-level difference-in-differences model comparing Quebec firms with a higher fraction of new mothers in the workforce in 1996 (the year before the reform) with those with a lower fraction. 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 (industry is 4-digit NAICS), $Low\%Mom96_j$ is an indicator equal to one if firm j had a below-industry-median fraction of women with pre-kindergarten children in 1996, and $Post97_t$ is a dummy indicating years after 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 low-%mom firms relative to high-%mom 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 use this specification to examine the impact of the reform on firm outcomes. We first consider three measures of firm performance: sales growth, ROA (pre-tax income/total assets), and labor productivity (i.e., sales per employee). Table 10 shows the results. We find that low-%mom firms experienced improved performance relative to high-%mom firms after the reform. Specifically, the reform led to a 4.2% increase in sales growth for low-%mom firms relative to high-%mom firms (column 1). Low-%mom firms also experienced a 3.3 pp increase in ROA (though statistically insignificant), as well as a 2.3% increase in labor productivity relative to other firms (columns 2 and 3, respectively). Figure 4 shows the dynamics of these effects, which are largely consistent with parallel trends between the two groups of firms before the reform. Overall, it appears that the reform benefited firms traditionally

unattractive to new mothers relative to other firms. We next investigate potential channels.

6.3 Firm-Level Mechanisms

We first examine the effect of the reform on firms' labor composition, using the same specification as above. Panel A of Table 11 shows that, post-reform, low-%mom firms experienced a 1.3pp increase in the fraction of female workers (column 1), or a 3.3% increase relative to the mean. This increase is even bigger when we zoom in on the fraction of *young* (age<35) female workers, which increased by 7.2% relative to the mean (column 2). Columns 3 and 4 show that the increase in female share is driven both by more female workers joining (8% relative to mean) and, to a lesser extent, fewer female workers leaving (-2.2% relative to mean). These findings are consistent with the sorting effect documented at the individual level, where mothers switch from high-%mom firms to low-%mom firms due to better childcare access.[26] They also show that childcare access leads to better retention of female workers at firms traditionally unattractive to mothers. We find the opposite compositional changes for male workers, where fewer male workers are joining and more male workers are leaving low-%mom firms (column 2-3 of Appendix Table IA.10) . This suggests that female workers substituted male workers in firms traditionally unattractive to mothers, leading to higher gender diversity in these firms post reform.

We find that the above results reflect expansions in employment in low-%mom firms rather than a simple gender reallocation within those firms. Panel B of Table 11 shows that female employment growth in low-%mom firms increased by 4% relative to other firms (column 1), while male employment growth did not significantly change (column 2), resulting in a 1.4% increase in overall employment growth in these firms (column 1 of Appendix Table IA.10). These results suggest that childcare subsidies reduced gender-based labor segmentation across firms, as low-%mom firms gained more female workers. Such a positive

[26]The increase in female worker share in low-%mom firms could also be driven by changes in women's career choices before they become mothers, due to changes in expectations.

female labor supply shock exerted downward pressure on wages in low-%mom firms. In particular, female wage growth in these firms decreased by 0.24%, while male wage growth decreased by 0.13%. These wage effects may explain the improvement in low-%mom firms' ROA post reform.[27]

We next examine how the reform affected the quality composition of workers. We use the AKM model of wage determination (Abowd et al., 1999) to estimate each worker's ex-ante productivity (hereinafter, "ability") based on their job histories before the reform.[28] We then average workers' ability to the firm level for different groups of workers. Because our worker-level ability measure is time-invariant, any effect of the reform on firm-level average worker ability reflects changes in the quality composition of workers and not within-worker productivity changes. Panel C of Table 11 reports the results. We find that the reform increased the average female worker ability by 1.1% s.d. in low-%mom firms relative to high-%mom firms, while it had no significant impact on the average male worker ability in these firms (columns 1 and 2, respectively). This increase in female worker ability is driven by low-%mom firms hiring higher-ability women (2.1% s.d. increase) and losing lower-ability women (1.2% s.d. decrease), though the latter effect is not statistically significant (columns 3 and 4, respectively). Because we do not observe job title or seniority of the position, the increase in the average ability of newly hired women could reflect either higher worker quality for a given position, or hiring women for higher-ranked positions. In columns 4-5 of Table IA.10, we further show that the reform reduced the average ability of male workers hired by low-%mom firms, suggesting that these firms potentially substituted women for men in filling higher-ranked jobs. These results could explain the increase in labor productivity of low-%mom firms in Table 10.

Taken together, the above results suggest that the outperformance by low-%mom firms

[27]Note that this wage compression happens within a firm, and does not imply that wages are reduced within a given individual. In fact, we find that childcare access increased new mothers' post-birth earnings relative to pre-birth.

[28]This requires us to restrict to workers that had switched employers before the reform. See Appendix B for details of our AKM estimation.

post reform can be explained by an influx of productive female workers into these firms and their better retention of female workers. At the same time, these firms were able to pay lower wages to workers due to the positive labor supply shock. Of course, other explanations may be at play too. For example, low-%mom firms may have been labor-constrained before the reform. Workplace gender diversity itself could also increase the productivity of all workers. Regardless, our firm-level results demonstrate that childcare subsidies reduced labor market segmentation across genders, and benefited firms that were traditionally unattractive to mothers.

One may question why firms previously unattractive to mothers did not voluntarily 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 paid by the government and financed by general tax revenue (not through payroll taxes). As such, privately funded childcare may not necessarily benefit firms if they fully internalize the costs. Our results only suggest that firms can benefit from government-funded childcare.

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 a 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 government funds used to pay for the subsidy.

That said, we attempt one quantification of the Quebec childcare program from a purely

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 incremental tax revenue generated by the program from higher personal income and higher corporate income. Table IA.11 provides details on the calculations. From 1997 to 2002, the average annual program cost was $593m/year. The Quebec government gained an additional $353.5m tax revenue per year from the program, while the Quebec and federal governments together gained an 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 per year on net. The bottom two panels of the table compute the NPV of the program. We use a 10-year horizon to discount annual tax benefits and a 5-year horizon (i.e., all years pre-kindergarten) to discount annual program costs.[29] We find that the NPV of the program is -$465m to $51m for Quebec, and $2,320m to $3,570 for Canada, depending on how much the effects in years 6-10 dissipate relative to our estimated effects for years 1-5. As such, the program appears to be profitable from a fiscal perspective. Of course, as discussed, we caveat that the program also 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

[29]We apply a 7% discount rate as recommended by the U.S. Office of Management and Budget (OMB) for federal agencies' cost-benefit analysis.

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 female human capital towards more demanding and male-dominated careers. This results in higher earnings and productivity for new mothers. Such a reallocation 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 female labor supply, but also constrain the types of firms at which women are willing to work. Removing frictions in childcare can therefore advance women's careers and help narrow gender gaps across firms and sectors.

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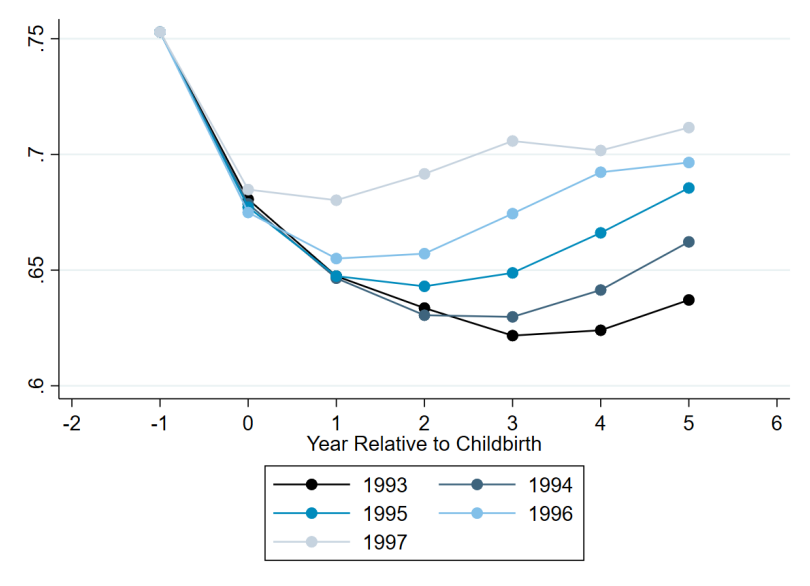
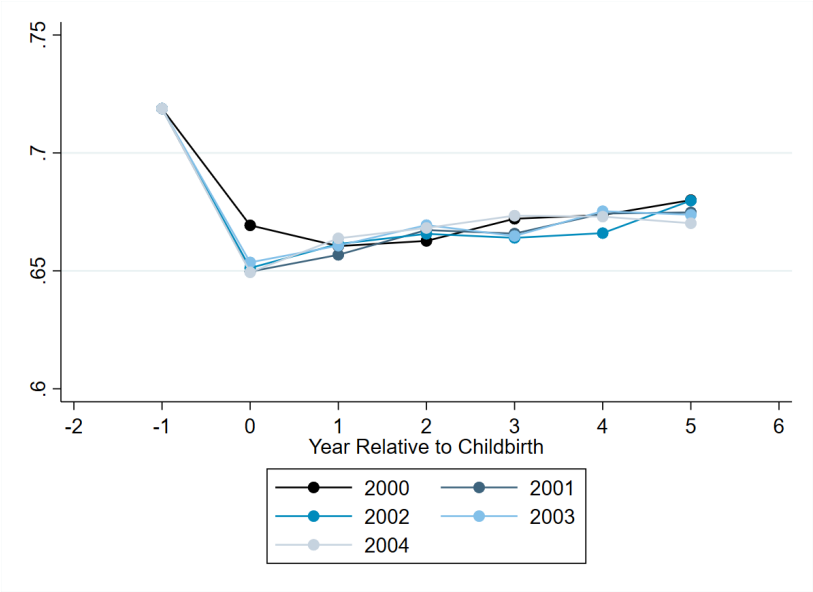
Figure 1: Mean Employment Rate by Cohort Around Childbirth



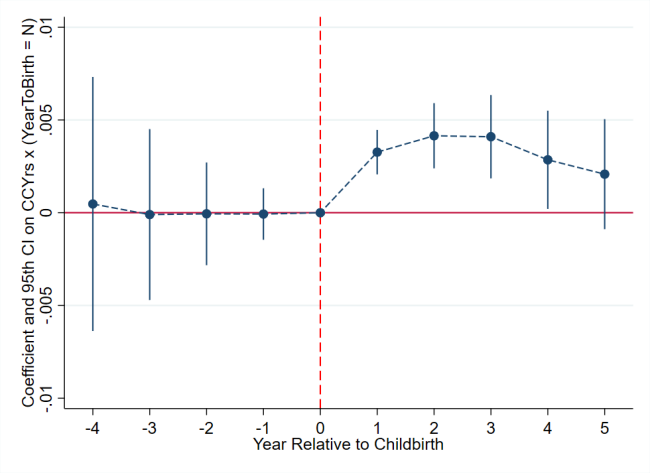
(a) Cohorts in our sample



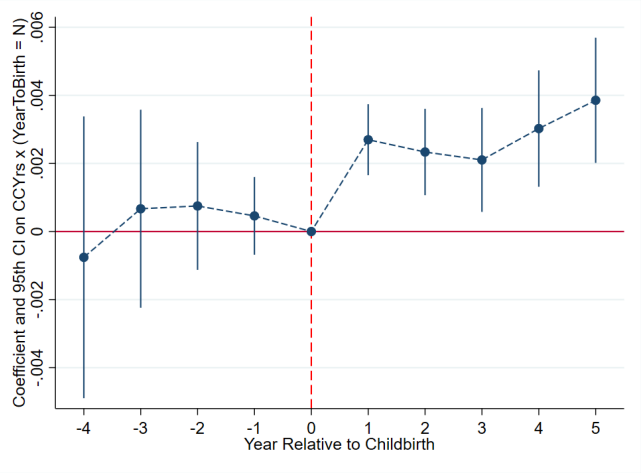
(b) Placebo cohorts

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.

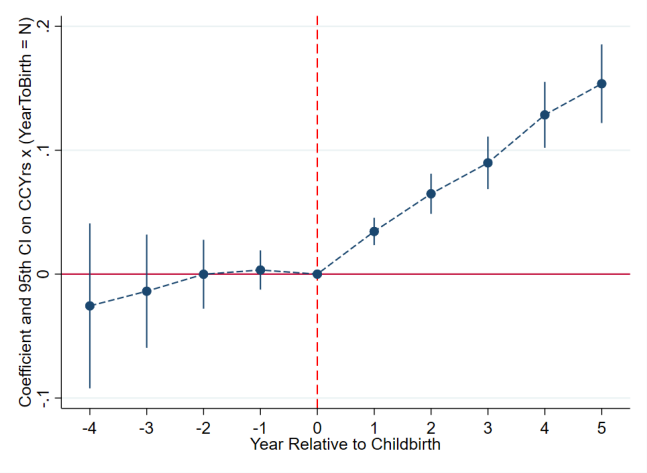
Figure 2: Dynamics Treatment Effects: Employment, Turnover, and Earnings



(a) Employment



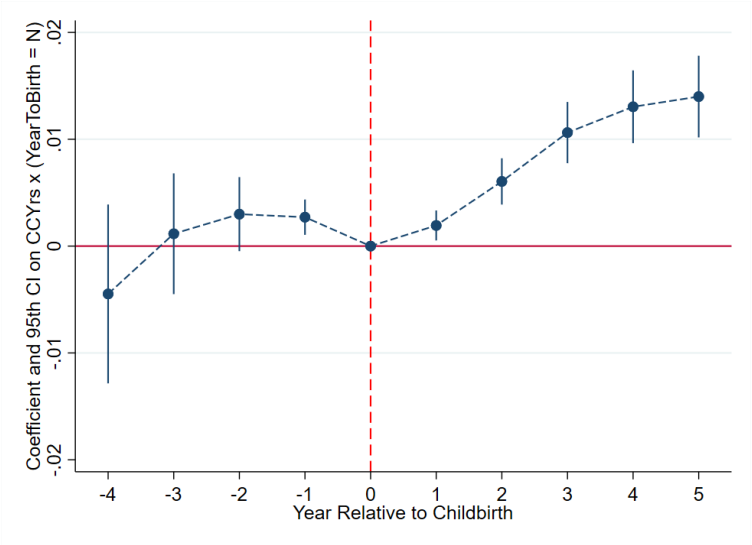
(b) Quit



(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.

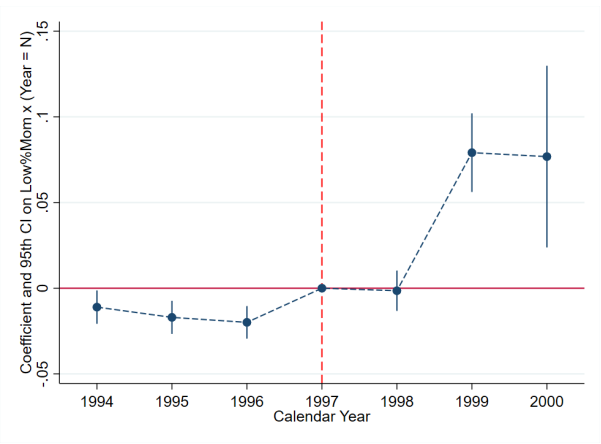
Figure 3: Dynamics Treatment Effects: Sorting



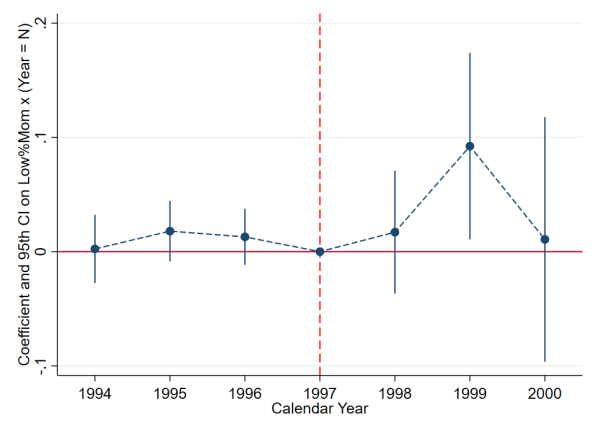
(a) Work in Low-%Mom Firm

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. *Work in Low-%Mom Firm* is a dummy equal to one if an individual's current employer had a below-industry-median percentage of mothers of pre-kindergarten children among all employees in 1996, the year before the reform.

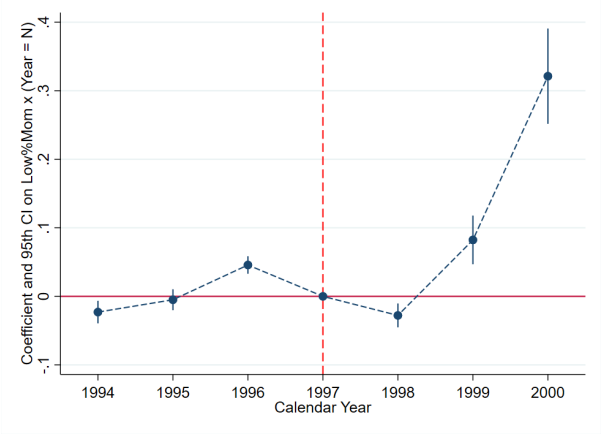
Figure 4: Impact on Firm Performance: Dynamics



(a) Ln(Sales Growth)



(b) ROA



(c) Ln(Sales/Emp)

These figures show the firm-level dynamic DID effects of the reform on firm performance estimated from the equation:

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \sum_{t=1994}^{2000} \theta_t \times Low\%Mom96_j \times Year_t + \epsilon_{j,t},$$

where 1997 is the omitted base year, α_j is firm fixed effect, $\beta_{k,t}$ is industry-year fixed effect (industry is 4-digit NAICS), and *Low%Mom96* is an indicator equal to one if a firm had a below-industry-median percentage of mothers of pre-kindergarten children (age<5) among all employees in 1996. 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 ln(sales growth), ROA, and labor productivity, respectively. Standard errors are clustered by firm.

Table 1: Summary Statistics

Variable	N	Mean	Std. Dev.	P25	P50	P75
			Individual-level:			
CCYears	2,838,510	3.447	1.523	2	4	5
Employed	2,838,510	0.652	0.476	0	1	1
Pre-Birth Employed	2,838,510	0.707	0.455	0	1	1
Quits	1,692,000	0.053	0.223	0	0	0
Switch Employer	1,692,000	0.233	0.423	0	0	0
Earnings	1,686,920	17,322	14,813	5,698	14,048	25,354
Earnings (scaled)	1,686,920	1.654	4.068	0.604	0.995	1.244
Promotion	1,692,000	0.243	0.429	0	0	0
Demotion	1,692,000	0.253	0.435	0	0	1
Promotion_Moved	1,692,000	0.137	0.344	0	0	0
Demotion_Moved	1,692,000	0.102	0.303	0	0	0
Married	2,838,510	0.435	0.496	0	0	1
Age at Childbirth	2,838,510	26.818	4.909	23	27	30
Pre-Birth Earnings	2,007,890	16,845	14,358	5,576	13,767	25,109
Sick Leave	1,692,000	0.010	0.100	0	0	0
School Leave	1,692,000	0.008	0.090	0	0	0
Num of Kids in 5yrs	283,850	0.678	0.654	0	1	1
Num of Kids in 10yrs	283,850	0.901	0.804	0	1	1
Work in Low-%Mom Firm	1,388,510	0.295	0.544	1	0	0
Work in Low-%Dad Firm	1,388,510	0.417	0.507	1	0	0
Switch Employer	1,454,430	0.216	0.412	0	0	0
Switch to High-%Mom Firm	1,454,430	0.125	0.330	0	0	0
Switch to Low-%Mom Firm	1,454,430	0.058	0.234	0	0	0
			Firm-level:			
Low%Mom96	325,880	0.650	0.477	0	1	1
Ln(Sales Growth)	314,420	-0.049	0.549	-0.049	0.021	0.099
ROA	257,720	0.124	1.246	0	0.041	0.136
Ln(Sales/Emp)	325,880	10.984	1.335	10.337	11.072	11.742
%Female	334,490	0.393	0.299	0.143	0.333	0.615
%YoungFemale	334,490	0.244	0.238	0.023	0.185	0.391
%Female Joiners	334,490	0.128	0.178	0	0.061	0.200
%Female Leavers	334,490	0.122	0.172	0	0.054	0.190
Ln(Female Emp Gr)	270,350	0.027	0.380	-0.105	0	0.182
Ln(Male Emp Gr)	295,840	0.018	0.396	-0.150	0	0.182
Ln(Female Wage Gr)	253,310	0.008	0.068	-0.023	0.004	0.035
Ln(Male Wage Gr)	277,920	0.007	0.063	-0.022	0.004	0.032
Avg. Emp. Ability: Female	273,740	-0.293	0.584	-0.616	-0.257	0.060
Avg. Emp. Ability: Male	296,210	0.060	0.546	-0.234	0.091	0.379
Avg. Emp. Ability: Female Joiners	159,350	-0.545	0.703	-0.938	-0.480	-0.080
Avg. Emp. Ability: Female Leavers	159,150	-0.506	0.695	-0.897	-0.443	-0.050

This table presents the summary statistics for our individual-level and firm-level samples. All variables are defined in Appendix A. Observation counts are rounded to the nearest 10.

Table 2: 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 indicate 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 in childcare early to claim a spot, albeit at an 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 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	Non-employed	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 are 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. *Quit* (columns 1-3) is an indicator equal to one if the individual voluntarily leaves the employer from the previous year as identified from the 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 years of mothers who were 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 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 are 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 employed years of mothers who were 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 Analysis: 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 × PostBirth × Female	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 × 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 analysis results comparing males and females 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			Quit			Earnings	
CCYears × PostBirth × Married	-0.0213***			-0.0023***			-0.1415***		
	[0.0008]			[0.0005]			[0.0098]		
CCYears × PostBirth × Older		-0.0185***			-0.0030***			-0.1176***	
		[0.0008]			[0.0005]			[0.0090]	
CCYears × PostBirth × HighEarn			-0.0261***			-0.0035***			-0.1634***
			[0.0009]			[0.0005]			[0.0080]
Individual FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Cohort-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	0.567	0.563	0.322	0.062	0.062	0.062	0.599	0.602	0.604

This table examines the cross-sectional heterogeneity in our baseline results for employment, quit, and earnings. All columns include cohort-event-year fixed effects, which absorb $CCYears \times PostBirth$. For brevity, we only report the coefficient on the triple interaction terms. *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 childbirth. $HighEarn$ 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		X
Observations	1,692,000	1,654,540	1,692,000	1,654,540
Ad. R-squared	0.025	0.025	0.079	0.090

Panel B: Fertility

	(1)	(2)	(3)	(4)
	Num of Kids in 5yrs	Num of Kids in 10yrs	Num of Kids in 5yrs	Num of Kids in 10yrs
CCYears	-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 of 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 who recently had their first child. The outcome *Num of Kids in 5yrs* (*Num of Kids in 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 in Panel A. * 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)
	Work in Low-%Mom Firm		Work in Low-%Dad Firm	
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	Switch to High-%Mom Firm	Switch to Low-%Mom Firm
CCYears × PostBirth	-0.0015	-0.0002	-0.0003
	[0.0014]	[0.0011]	[0.0009]
High%Mom_PreBirth × 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 *Work in Low-%Mom Firm* (*Work in Low-%Dad Firm*) is a dummy equal to one if an individual's current employer had a below-industry-median percentage of mothers (fathers) of pre-kindergarten children among all employees in 1996, the year before the reform. In Panel B, the dependent variable *Switch Employer* indicates that an individual's current employer is different from her employer in the previous year. The dependent variables in columns 2 and 3 decompose *Switch Employer* into switches to an employer that was $High\%Mom$ ($Low\%Mom$) in 1996. On the right hand side, we interact with $High\%Mom\_PreBirth$, an indicator for whether the pre-birth employer had an above-industry-median percentage of mothers of pre-kindergarten children among all employees in 1996. Coefficients on *High%Mom_PreBirth* × *PostBirth* are omitted from the table for brevity. 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' financial performance. The sample period is 1994 to 2000. The specification follows Equation 5. All columns include firm fixed effects and industry-year fixed effects, where the industry is defined at NAICS 4-digit. *Low%Mom96* is an indicator equal to one if a firm had a below-industry-median percentage of mothers of pre-kindergarten children (age<5) among all employees in 1996. *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.

Table 11: Firm-Level Mechanisms

Panel A: Worker Composition

	(1) %Female	(2) %Young Female	(3) %Female Joiners	(4) %Female Leavers
Low%Mom96 × Post97	0.0130*** [0.0012]	0.0180*** [0.0012]	0.0102*** [0.0012]	-0.0027** [0.0013]
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Observations	334,490	334,490	334,490	334,490
Ad. R-squared	0.887	0.820	0.529	0.520

Panel B: Employment and Wage Growth

	(1) Ln(Female Emp Gr)	(2) Ln(Male Emp Gr)	(3) Ln(Female Wage Gr)	(4) Ln(Male Wage Gr)
Low%Mom96 × Post97	0.0401*** [0.0036]	0.0015 [0.0038]	-0.0024*** [0.0006]	-0.0013** [0.0006]
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Observations	270,350	295,840	253,310	277,920
Ad. R-squared	0.050	0.024	-0.122	-0.120

Panel C: Worker Ability

	(1)	(2)	(3)	(4)
	Average Worker Ability:			
	Female	Male	Female Joiners	Female Leavers
Low%Mom96 × Post97	0.0065* [0.0036]	-0.0021 [0.0033]	0.0145* [0.0085]	-0.0085 [0.0081]
Firm FE	X	X	X	X
Industry-year FE	X	X	X	X
Observations	273,740	296,210	159,350	159,150
Ad. R-squared	0.745	0.748	0.208	0.232

This table examines the impact of the reform on Quebec firms' worker composition (Panel A), employment and wage growth (Panel B), and worker ex-ante quality (Panel C). The specification and sample follow Table 10. *Low%Mom96* is an indicator equal to one if a firm had a below-industry-median percentage of mothers of pre-kindergarten children (age<5) among all employees in 1996. In Panel A, *%Female* (*%Young Female*) is the percent of (young (i.e., age<35)) female employees, *%Female Joiners* (*%Female Leavers*) is the percent of female employees this year who were not employed with the same firm in the previous (next) year. In Panel B, the dependent variables are the log growth of a firm's female and male employment, as well as the log growth of average female and male wages. In Panel C, the dependent variables are the average ex-ante worker ability of all female workers, all male workers, female leavers, and female joiners, respectively. Worker-level ability is estimated from AKM regressions using pre-reform job histories (see Appendix B for details). 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.

A Appendix: Variable Definitions

Variable Name	Definition
CCYears	The number of years post reform before a child turns 5. It is equal to $min(5, T - 1992)$, where T is the year an individual gives birth.
PostBirth	An indicator equal to one for years after the childbirth year.
Employed	An indicator equal to one if an individual is employed (i.e., filed a T1 income tax package) in a particular year.
Pre-Birth Employed	An indicator equal to one if an individual is employed (i.e., filed a T1 income tax package) in the year before childbirth.
Quits	An indicator equal to one if an individual voluntarily left the employer from the previous year as identified from Record of Employment (ROE).
Switch Employer	An indicator equal to one if an individual is with a different employer this year compared with last year, as identified from T4 filing (a tax slip identifying all of the remuneration paid by an employer to an employee during a calendar year).
Earnings	Total T4 earnings divided by the individual's earnings in the year before childbirth
Promotion	An indicator equal to one if the individual's current earnings is more than 110% of her pre-childbirth earnings
Promotion_Moved	An indicator equal to one if the individual's current earnings is more than 110% of her pre-childbirth earnings and the individual has moved to a different employer relative to the pre-childbirth employer.
Demotion	An indicator equal to one if the individual current earnings is less than 90% of her pre-childbirth earnings.
Demotion_Moved	An indicator equal to one if the individual current earnings is less than 90% of her pre-childbirth earnings and the individual has moved to a different employer relative to the pre-childbirth employer.
Married	An indicator equal to one if the individual was married (including common-law status) in the year before childbirth.
Older	An indicator equal to one if the individual had an above-median age in the year before childbirth
HighEarn	An indicator equal to one if the individual had above-median earnings in the year before childbirth.
Sick Leave	An indicator equal to one if the individual took a temporary sick leave in a year (based on Record of Employment records).

Variable Name	Definition
School Leave	An indicator equal to one if the individual took a leave to pursue schooling or further education (based on Record of Employment records).
Num of Kids in 5yrs (10yrs)	The total number of subsequent children a mother had 5 years (10 years) after the birth of her first child.
Work in Low-%Mom Firm	An indicator equal to one if an individual's current employer had a below-industry-median percentage of mothers of pre-kindergarten-age children among all employees in 1996.
Work in Low-%Dad Firm	An indicator equal to one if an individual's current employer had a below-industry-median percentage of fathers of pre-kindergarten-age children among all employees in 1996.
High%Mom_PreBirth	An indicator equal to one if a mother worked at a high-%mom firm in the year before childbirth, where high-%mom is defined in 1996.
Switch to High-%Mom (Low-%Mom) Firm	An indicator equal to one if a mother switched employer this year into a high-%mom firm (low-%mom firm), where high-%mom (low-%mom) is defined in 1996.
Low%Mom96	A firm-level indicator equal to one if a firm had a below-industry-median percentage of mothers of pre-kindergarten children (age<5) among all employees in 1996.
Post97	An indicator equal to one for years after 1997.
Ln(Sales Growth)	Log sales growth from previous year to this year.
ROA	Pre-tax income divided by total assets.
Ln(Sales/Emp)	Log labor productivity, i.e., log of sales divided by employment.
%Female	The percent of female employees in a given firm-year.
%Young Female	The percent of female employees between the ages of 18 and 35 in a given firm-year.
%Female Joiners	The percent of a firm's female employees this year who were not employed with the firm in the previous year.
%Female Leavers	The percent of a firm's female employees this year who are not employed with the firm in the next year.
Ln(Female Emp Gr)	The year-to-year log growth of a firm's female employment.
Ln(Male Emp Gr)	The year-to-year log growth of a firm's male employment.
Ln(Female Wage Gr)	The year-to-year growth of a firm's log average female wage.
Ln(Male Wage Gr)	The year-to-year growth of a firm's log average male wage.
Avg. Emp. Ability: Female (Male)	The average ex-ante worker productivity of all female (male) workers in a firm. Worker-level productivity is estimated from AKM regressions using pre-reform job histories (see Appendix B for details).

Variable Name	Definition
Avg. Emp. Ability: Female Joiners (Leavers)	The average ex-ante worker productivity of all female joiners (leavers) in a firm. Worker-level productivity is estimated from AKM regressions using pre-reform job histories (see Appendix B for details).

B Appendix: Estimating AKM Person Effects

We estimate AKM person fixed effects following Abowd et al. (1999) using our employer-employee-linked panel. Since our analysis is aimed at detecting compositional changes in the productivity of a firm's workers, we construct person fixed effects using the pre-reform sample panel that spans from 1989 to 1996. We then estimate the following specification:

$$ln(Earn)_{ijt} = \alpha_i + \lambda_j + \gamma_t + \beta_1 age_{it} + \beta_2 age_{it}^2 + \beta_3 age_{it}^3 + \epsilon_{it} \tag{6}$$

where $ln(Earn)_{ijt}$ is the natural log of earnings for individual i working for firm j in year t, age_{it} is an individual i's age at year t, and α_i and λ_j constitute person and firm fixed effects, respectively. Note that we use earnings rather than wages, as is traditional in AKM models. We do not control for education in our AKM model since we do not observe educational attainment in our data. Hence, our ability measure also incorporates human capital accumulated through education.

After obtaining pre-1997 person fixed effects from estimating Equation 6, we construct time-varying firm productivity measures by averaging α_i for all individuals working for a given firm in a given year:

$$Avg\ Ability_{jt} = \frac{\Sigma_{i \in I(j,t)} \alpha_i}{|I(j,t)|} \tag{7}$$

where $I(j,t)$ denotes the set of all individuals working for firm j in year t and $|I(j,t)|$ denotes the number of individuals working for firm j in year t. We define versions of *Avg Ability* for males and females separately, as well as for the subcategories of joiners (those who did not work at the firm in year $t-1$) and leavers (those who did not work at the firm in year $t+1$). Since α_i is time-invariant and defined based on the pre-reform data, any changes in *Avg Ability* capture only compositional shifts in a firm's labor force.

The table below provides information on the number of workers and firms in the estimation of our AKM fixed effects. On average, we observe worker person fixed effects for 90.4%

of the total population of employed workers in our pre-1997 sample. Of the 419,685 firms in the pre-1997 sample, 84.6% form our largest connected set representing firms with multiple workers and connected by worker mobility. Our estimation procedure drops all singletons from the population (9.6% of the population).

Number of workers	Number of firms	Largest connected set	Workers with person effects
4,716,554	419,685	355,082	4,264,110

Internet Appendix to "The Effect of Childcare Access on Women's Careers and Firm Performance"

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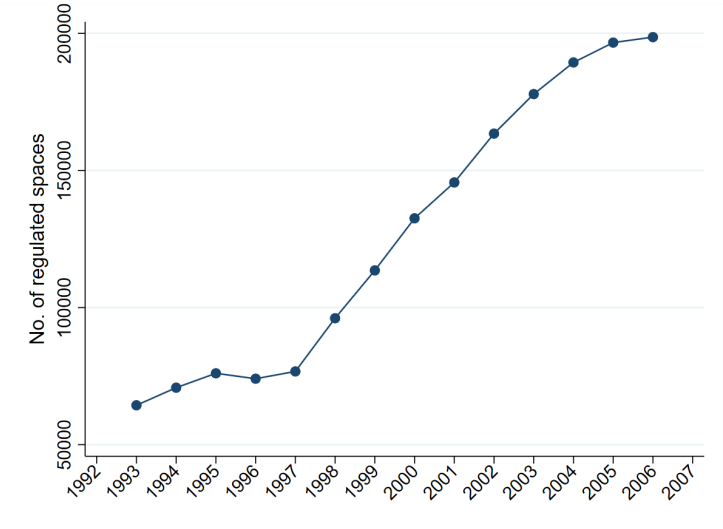
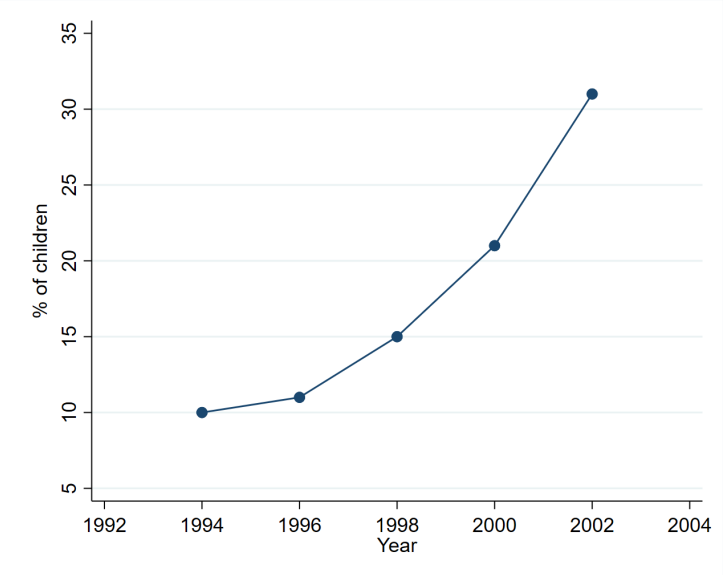
Figure IA.1: Childcare Provision and Usage in Quebec Around 1997



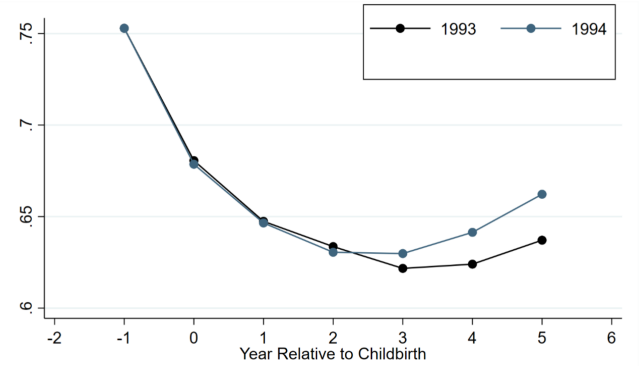
(a) Number of regulated childcare spaces



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

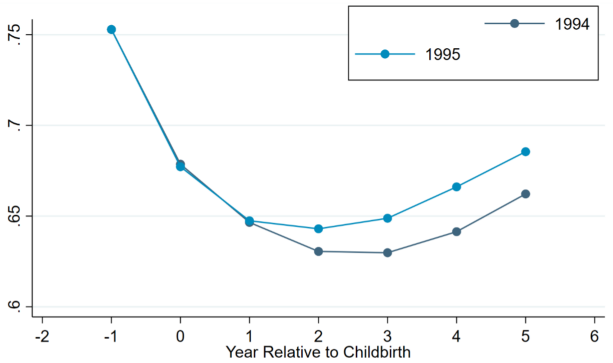
Figure (a) shows the number of regulated childcare spaces in Quebec from 1993 to 2006 based on data in Table 2 of Lefebvre and Merrigan (2008). Figure (b), based on Table 3 of Lefebvre and Merrigan (2008), shows the percentage of children aged 1-5 in Quebec whose primary care arrangement is childcare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

Figure IA.2: Employment Rates Around Childbirth by Cohort Pairs



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

(a) 1993 and 1994 Cohorts



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

(b) 1994 and 1995 Cohorts

This figure shows the adjusted mean employment rates for two pairs of adjacent cohorts of mothers (1993/1994 for Panel (a) and 1994/1995 for Panel (b)) from 1 year before to 5 years after childbirth. Darker colors represent earlier cohorts. All cohorts are shifted to align with the pre-childbirth employment rate of the earliest cohort. The bottom table highlights the corresponding cohorts in Table 2.

Table IA.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 on 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 was in daycare at the time of the survey. *Post97 Years* is a dummy indicating that the survey year was after 1997. *Anticipatory Years* is a dummy indicating that the survey year was post reform but before the child was age-eligible for the subsidy (i.e., the light blue cells in Table 2). *Eligible Years* is a dummy indicating that the child was age-eligible for the subsidy (i.e., the dark blue cells in Table 2) in the survey year. 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 IA.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	956,050	956,050	956,050	956,050
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 shows the robustness of our promotion and demotion results to alternative cutoffs for $> 20\%$ or $> 30\%$ earnings changes. 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 IA.3: Individual-level Robustness: Additional Fixed Effects

Panel A: Age bin fixed effects

	(1) Employed	(2) Quit	(3) Earnings	(4) Work in Low-%Mom Firm
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) Work in Low-%Mom Firm
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. × 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 not employed before childbirth, earnings is set to zero. 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 IA.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, in order 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 IA.5: Within-Family Gender Pay Gap

	(1)	(2)	(3)
	Ln(Husband Pay/Wife Pay)		
CCYears × PostBirth	-0.0097***	-0.0093***	-0.0092***
	[0.0029]	[0.0029]	[0.0030]
Family FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Wife and husband industry FE		X	
Wife and husband firm FE			X
Observations	806,860	806,860	758,000
Ad. R-squared	0.500	0.510	0.569

This table examines the effect of childcare access on within-family gender pay gap. The sample is at the family-year level. The dependent variable is the log ratio of the husband's pay to the wife's pay. Same-sex couples or family-years where the wife is not employed are excluded. Column 2 (3) includes two sets of fixed effects for the wife's and the husband's work industry (employer) respectively. Standard errors are reported in brackets and are clustered at the family level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table IA.6: Individual-level Robustness: Dropping Part-Time or Public Sector Employees

Panel A: Drop part-time employees

	(1) Employed	(2) Quit	(3) Earnings	(4) Work in Low-%Mom Firm
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) Work in Low-%Mom Firm
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). Part-time employees are those in the bottom 10% of wage distribution within a firm. 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 IA.7: Heterogeneity: Without Cohort-Event Year Fixed Effects

	(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 × PostBirth × Married	-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 × PostBirth × Older		-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 × PostBirth × HighEarn			-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 shows the robustness of our heterogeneity tests in Table 7 to excluding cohort-event year fixed effects. *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 childbirth. *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 IA.8: Correlations between *Low-%Mom Firm* and Firm Characteristics

	(1) Low-%Mom Firm
Ln(Assets)	-0.2087***
ROA	0.0063
Ln(Sales/Emp)	0.1442***
Ln(Capital/Emp)	0.0665***

This table reports firm-level correlations between the *Low-%Mom Firm* measure used in Table 9 and various firm characteristics in 1996. *Low-%Mom Firm* is an indicator equal to one if a firm had a below-industry-median percentage of mothers of pre- kindergarten children among all employees in 1996. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table IA.9: Sorting into Firms – Alternative Firm Type Measures

	(1)	(2)	(3)
	Work in High Pay Convexity Industry	Work in Large Firm	
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 using alternative measures of firm types. The specification follows Equation 1 and the sample conditions on employed individual-years. *Work in High Pay Convexity Industry* is a dummy equal to one if an individual works at a firm in a 2-digit NAICS 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) to industry level using occupation-industry crosswalk, based on occupation weights within each industry. *Work in Large Firm* is a dummy equal to one if an individual works at a firm with 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 IA.10: Firm-Level Mechanisms: Additional Results

	(1) Ln(Emp Gr)	(2) %Male Joiners	(3) %Male Leavers	(4) Avg. Male Joiner Ability	(5) Avg. Male Leaver Ability
Low%Mom96	0.0144***	-0.0219***	0.0031**	-0.0156**	0.0007
× Post97	[0.0034]	[0.0013]	[0.0014]	[0.0071]	[0.0068]
Firm FE	X	X	X	X	X
Industry-Year FE	X	X	X	X	X
Observations	317,160	334,490	334,490	204,320	208,150
Ad. R-squared	0.198	0.445	0.434	0.209	0.238

This table examines the impact of the reform on other firm outcomes. The sample period is 1994 to 2000. The specification follows Equation 5. All columns include firm fixed effects and industry-year fixed effects. *Low%Mom96* is an indicator equal to one if a firm had a below-industry-median percentage of mothers of pre-kindergarten children (age<5) among all employees in 1996. *Ln(Emp Gr)* is log year-to-year overall employment growth. *%Male Joiners* (*%Male Leavers*) is the percent of male employees this year that were not employed with the firm in the previous (next) year. *Avg Male Joiner Ability* (*Avg Male Leaver Ability*) is the average ex-ante ability of male joiner (leaver) based on individual-level AKM estimates pre-reform. 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 IA.11: Fiscal Analysis of the Quebec Universal Childcare Program

	Quebec	Quebec + Federal
	Parameters:	
Effective personal tax rate	14.40%	32.30%
Marginal personal tax rate	15.40%	35.40%
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	$716
Marginal corporate tax rate	9%	30%
Δ corporate income ($)	$3,600	$3,600
No. of QC firms	128,197	128,197
Δ corporate income tax revenue ($m)	$42	$138
	Annual Net Benefit:	
Δ total tax revenue/year ($m)	$353.50	$854.40
Program cost/year ($m)	$593.00	$593.00
Net benefits/year ($m)	($239.50)	$261.40
	NPV:	
Assume same effects in years 6-10 relative to years 1-5:		
PV (Δ total tax revenue over 10 years) ($m)	$2,482.84	$6,000.95
PV (program cost over 5 years) ($m)	$2,431.42	$2,431.42
NPV ($m)	$51.42	$3,569.53
Assume half effects in years 6-10 relative to years 1-5:		
PV (Δ total tax revenue over 10 years) ($m)	$1,966.13	$4,752.08
PV (program cost over 5 years) ($m)	$2,431.42	$2,431.42
NPV ($m)	($465.29)	$2,320.66

This table provides back-of-the-envelope calculations of the net fiscal payoff of the program. All numbers in the parameters panel are annual averages 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 incremental tax revenue generated by the program 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. The second panel shows the net annual benefits over 1997 to 2002. The last panel shows the fiscal NPV of the program under two assumptions: 1) same program effects in years 6-10 as those in years 1-5, 2) half program effects in years 6-10 relative to those in years 1-5. For NPV calculation, we discount annual fiscal costs over 5 years and annual tax benefits over 10 years. We use a 7% discount rate, which is recommended by U.S. Office of Management and Budget (OMB) for cost-benefit analysis for all federal agencies.