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CHILDCARE ACCESS AND THE ALLOCATION OF FEMALE HUMAN CAPITAL ACROSS FIRMS

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ABSTRACT

We study how relaxing childcare-related constraints affects the allocation of female human capital across firms. Using linked family-employee-employer tax filing data and cohort-level variation in exposure to Quebec's universal childcare reform, we show that earlier access to childcare induces new mothers to reallocate toward firms where they were previously underrepresented—firms with more demanding hours, higher risk, and higher pay. The effect is mainly driven by the relaxation of childcare-related time constraints. This reallocation raises mothers' earnings and improves the performance of the firms they join. Our results suggest that expanding childcare access improves worker-firm matching and firms' access to productive talent.

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It is well understood that frictions in the labor market can affect the allocation of female talent across firms, with implications for aggregate productivity and growth (Hsieh et al., 2019). Such frictions often stem from women's child-rearing responsibilities. These responsibilities have been shown to impact women's labor force participation (Kimmel, 1998; Goldin and Katz, 2016; Kleven et al., 2019; Cortés and Pan, 2023), but less is known about how they affect the allocation of women's human capital across different types of firms in the economy. Examining such reallocation effects is important because they may impact firms' ability to tap a larger and more diverse pool of workers and ultimately affect their growth and productivity.

Policy makers have often considered subsidizing early-age childcare—including increasing its supply and lowering its costs—as a way to boost mothers' employment. Such policies have been at the forefront of policy debates in several major developed countries in recent years, including the U.S., albeit with mixed legislative success.[1] In this paper, we study a universal childcare reform in Quebec, Canada. Our objective is to understand how improving access to early-age childcare reallocates new mothers' careers across firms by relaxing childcare-specific constraints, and to examine its implications for firm outcomes.

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 (or $3.6 USD) per day, at a time when the median childcare cost was $11 CAD per day. The program also tripled the number of regulated childcare spaces available. Our empirical analysis uses cohort-level variation in exposure to the reform, exploiting the timing of childbirth relative to the reform across different cohorts of mothers. To address concerns about pregnancy timing, we focus on

[1]Canada and Australia both introduced major childcare subsidies in recent years, while France is implementing a multi-year childcare reform as of 2026. 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. Free universal childcare has also been at the center of recent mayoral campaigns in New York City and Washington D.C (Times, 2025; Flynn, 2026). See Table IA.1 for a summary of recent childcare reforms across the world.

mothers who made fertility decisions before the reform. Using detailed administrative tax filing data from Statistics Canada with family-employee-employer linkages, we trace the impact of childcare access on worker-firm sorting and its implications for worker and firm outcomes.

We use the birth year of the first child as the reference point to estimate a stacked difference-in-differences (DID) model (Cengiz et al., 2019) at the individual level, comparing women's career outcomes before and after childbirth across cohorts of mothers with differential access to government childcare. First, consistent with earlier work, we find that earlier access to childcare significantly increases employment among new mothers (Baker et al., 2008; Goux and Maurin, 2010; Bauernschuster and Schlotter, 2015). A five-year earlier access to government childcare—i.e., full access pre-kindergarten—increases new mothers' employment by 4.3% relative to the mean. This effect is concentrated among those employed before childbirth, suggesting that relaxing childcare constraints reduces career interruptions by increasing the likelihood of returning to work after childbirth. Interestingly, the increase in employment post childbirth is paired with a large 15.6% increase in the probability of *voluntary* job turnover.

These two results raise the question of whether new mothers' increased labor mobility changes their allocation of human capital across firm types. On the one hand, by alleviating time constraints, childcare access could allow new mothers to pursue more demanding careers with firms that were previously out of reach (Becker, 1985; Goldin, 2014). On the other hand, financial subsidies may shift women toward lower-paying, less demanding jobs (Vattø and Østbakken, 2024; Core and Karpati, 2024). We thus examine *what types of firms* new mothers reallocate their careers to.

To test this, we define *%Mom*—the fraction of a firm's employees who are mothers of children under five. This measure captures the realized matching between mothers and firms in the presence of childcare constraints, which affect both the costs mothers face when

working at particular firms and the costs firms incur when hiring mothers.[2] We show that access to subsidized childcare increases the likelihood that new mothers join firms where they were previously underrepresented, i.e., low-%mom firms. This suggests that the reform reduced gender segmentation across firms. Such sorting occurs both through job changes among those already employed (3.1% effect) and through labor-force entry into low-%mom firms among those who would otherwise have been non-employed (6.2% effect). Decomposing job turnover, we show that the sorting effect is driven by greater switching from high-%mom firms to low-%mom firms (a 19.2% increase) rather than reallocation among low-%mom firms. We do not find that the reform led mothers to sort into firms with lower *%Dad*, suggesting that the sorting is triggered by relaxation of gender-specific constraints.

To better understand why new mothers sort into low-%mom firms after the reform, we examine the specific characteristics of these firms. First, we find that low-%mom firms tend to be in more demanding industries—those with longer hours and fewer part-time opportunities. Indeed, we find that the reform shifted new mothers toward industries with 4% longer hours and 3.4% fewer part-time jobs relative to their respective standard deviations. These results suggest that childcare responsibilities had previously confined mothers to jobs with shorter and more flexible hours (Becker, 1985), and that the reform relaxed these constraints.

We also find that low-%mom firms are riskier on average, as measured by higher within-job earnings volatility and higher financial leverage relative to industry peers. Accordingly, we find that the reform shifted new mothers to firms with 4.3% higher earnings volatility and 2.5% standard deviations higher excess leverage. These results suggest that childcare constraints previously limited mothers to safer careers. Indeed, Baghai et al. (2023) show that mothers face higher income and layoff risk than women without children or men due to their inability to expand hours precisely when firms need additional commitment. Ex ante, mothers would therefore match with less risky firms in the absence of childcare support.

[2]This revealed-preference approach is similar to Sorkin (2018), who infer unobserved job quality through individuals' job choices.

Childcare responsibilities could also reduce the time available for job search upon layoff, again leading mothers to avoid riskier firms.

Finally, we find that low-%mom firms are higher-paying, as measured by higher firm-specific wages from an AKM decomposition or higher median worker wages. As such, following the reform, new mothers sort into firms with 8.2% standard deviations higher AKM wages and 6.2% higher median wages. These results are consistent with the literature documenting that women select into lower-paying occupations, due to either less demanding career paths or compensating differentials for non-wage amenities disproportionately valued by women (Sorkin, 2018; Goldin, 2014; Wiswall and Zafar, 2018; Liu et al., 2023).

Overall, these results suggest that removing childcare constraints enables new mothers to reallocate their human capital toward firms where they were traditionally underrepresented—firms with more demanding hours, higher risk, and higher pay. As such, the reform reduced segmentation in the allocation of female talent across firm types. This reallocation seems to have benefited new mothers. Using the same stacked DID, we find that the reform increased mothers' earnings by 8.3% post childbirth, while it reduced within-household gender pay gaps by 6.8%.

Our baseline results are robust to several alternative specifications. First, in a dynamic stacked DID analysis, we find no evidence of pre-childbirth trends, supporting the parallel trends assumption across different cohorts of mothers. Second, using fathers as the benchmark group in a triple-difference analysis, we show that our main results hold under a tighter specification that includes cohort $\times$ year-to-birth fixed effects, which absorb secular trends across cohorts. This finding is also consistent with mothers bearing most of the childcare responsibilities, and that it is the relief from such responsibilities that drives our results. Third, to allow for potential non-linearity and identify the treatment effect of accessing childcare at each child's age, we conduct a cohort-by-cohort DID. We find a monotonically increasing employment effect as we move from the 1993 cohort (whose child was four at the time of reform) to the 1997 cohort (whose child was just born at the time of reform). This suggests

that having *earlier* access to government childcare matters more for mothers' careers, since childcare responsibilities are greater when the child is younger.

Our findings are consistent with the reform mainly alleviating the time constraints rather than the financial constraints associated with childcare for new mothers. The finding that mothers shifted into industries with longer and less flexible hours directly supports the time constraints channel. We also find that mothers more exposed to the reform had fewer subsequent children, consistent with relaxed time constraints setting mothers on more intensive career paths, while a financial-constraint story would instead predict *more* subsequent children as childcare becomes cheaper. Additionally, the reform raised mothers' earnings, partly by enabling them to move into higher-paying jobs. This speaks against the financial constraint channel, since cheaper childcare should induce sorting to lower-paying jobs via income substitution (Vattø and Østbakken, 2024; Core and Karpati, 2024). Finally, a margin decomposition exploiting variation across adjacent cohorts shows that the childcare-spot-supply effect—which operates through time constraints by making childcare easier to access—dominates, while the effects of receiving financial subsidies or expected future subsidies are smaller.

We next examine the impact of the reform on Quebec firms. Motivated by the sorting result, we use a firm-level DID comparing low-%mom firms—firms employing a lower fraction of new mothers before the reform—with other Quebec firms. Although the two groups trended similarly before the reform, low-%mom firms significantly outperformed afterward: ROA increased by 0.8 percentage points, sales grew by 13.1%, and labor productivity rose by 6.7%.

Three channels explain these firm-level gains. First, low-%mom firms gained access to a larger and higher-quality female talent pool. Consistent with the individual-level sorting result, the share of new mothers in these firms nearly doubled, and the number of young female workers grew by 9.5%.[3] The quality of female workers also improved: the ability of

[3]Consistent with low-%mom firms being riskier, higher-paying, and in industries with more demand-

female joiners and female stayers (measured by an AKM model pre-reform) both increased, by 5.5% and 1.8%, respectively. It is important to note that while our individual-level sorting results focus on specific cohorts of first-time mothers, our firm-level analyses capture the broader impact of the reform on *all* women's career decisions, including those who do not (yet) have children. Second, the relative increase in female labor supply exerted downward pressure on wages. Female wages fell by 3.8% despite the higher quality of new hires. This compressed labor costs and boosted profitability. Third, worker-firm matching improved overall: quit rates declined for both female and male workers in low-%mom firms.

Together, these findings suggest that childcare constraints not only limited women's career choices but also prevented firms from accessing a broader talent pool; removing those frictions generated measurable gains on both sides of the labor market.

Our paper contributes to the literature on worker-firm matching and firm performance. A growing body of work documents that frictions in labor markets distort the allocation of workers across firms, with consequences for firm productivity and aggregate efficiency (Hsieh et al., 2019; Bilal et al., 2022; Coraggio et al., 2025). Within this literature, several papers highlight how gender-specific frictions generate misallocation. Lagaras et al. (2022) show that female talent sorts relatively less into the financial sector, driving larger gender pay gaps. Baghai et al. (2023) find that women are less protected against idiosyncratic firm shocks, distorting their firm choices ex ante. Sorkin (2018) shows that compensating differentials for non-wage amenities lead workers to sort into lower-paying firms, a mechanism particularly relevant for mothers facing childcare constraints. We contribute to this literature by providing causal evidence that policy-induced relaxation of childcare constraints reduces gender-based labor misallocation across firms, generating firm performance gains.

Our paper also contributes to the literature on the effects of non-wage amenities on worker careers and firm outcomes. Studies have shown that maternity leave increases job

ing hours, we confirm that (young) female employment increased in firms characterized by each of these characteristics following the reform.

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, which ties a worker to her firm, improves firm productivity by reducing employee turnover. We differ by showing that childcare subsidies *increase* female workers' voluntary turnover to pursue more ambitious careers. Specific to childcare, prior studies document positive or insignificant effects of subsidies on maternal labor supply (Baker et al., 2008; Lefebvre and Merrigan, 2008; Fitzpatrick, 2010; Havnes and Mogstad, 2011; Kleven et al., 2020).[4] We extend this literature by using finer cohort-based identification and focusing on the allocative effects of childcare-related frictions and their impact on firm performance.

Our paper also relates to the broader literature on women's careers and firms. Reuben et al. (2014) and Sapienza et al. (2009) document how gender stereotypes and differences in risk aversion affect women's career choices. Benson et al. (2026) show that firms systematically underestimate women's potential relative to men, contributing to a gender promotion gap. Several papers provide evidence of potential solutions to gender gaps in the labor market: Bennedsen et al. (2022) show that mandated gender wage disclosure narrows the pay gap by lowering male wage growth, and Tate and Yang (2015) show that female leadership promotes a more female-friendly firm culture. We add to this literature by showing that policies that improve childcare access advance women's career outcomes and generate positive spillovers for the firms that hire them.

1 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"). Quebec

[4]Other papers include Lefebvre and Merrigan (2008), Lefebvre et al. (2009), Cascio (2009), Bauernschuster and Schlotter (2015), Kottelenberg and Lehrer (2017), Cornelissen et al. (2018), and Chhaochharia et al. (2021). See Olivetti and Petrongolo (2017) for a review.

is the second largest province in Canada, representing 25% of Canada's population and 22% of its GDP in 1997.[5] Interestingly, Quebec in the late 1990s resembled the present-day U.S. in terms of female labor force participation and the gender pay gap. Today, however, Quebec outperforms the U.S. on both metrics—consistent with its earlier adoption of more generous family policies.[6]

The policy provided childcare for children aged zero to four at a subsidized rate of $5 CAD per day ($8.95 CAD today), at a time when the median childcare cost was $11 CAD per day (Lefebvre and Merrigan, 2008).[7] 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 in September 1998, two-year-olds in September 1999, and children aged zero to one in September 2000.

The implementation of the program involved the creation of new childcare spaces and the conversion of existing non-profit childcare centers into *Centres de la petite enfance* (centers for young children, known by the acronym CPE) under enhanced quality standards. Each CPE offered childcare services and served a network of regulated home-based childcare providers that emerged during policy implementation. Home-based providers were preferred by parents of younger children, whereas 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 num-

[5]Unlike other large provinces in Canada, which 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.

[6]In 1997, the labor force participation rate for Quebec women aged 25-44 was 77%, similar to the 78% rate for U.S. women in the same age group in 2024. The female-to-male earnings ratio in Quebec was 83.4% in 1998, similar to the U.S. ratio of 83.6% in 2023 (by 2021, Quebec's ratio has risen to 90.8%). More broadly, the participation rate for Quebec women aged 15 and above in 1996 (54.2%) is comparable to that of many developed countries today: the UK (58%); the USA, Germany, Korea (all 57%); Japan (56%); Hungary, Portugal (both 55%); Spain, France (both 53%); Hong Kong and Poland (both 52%). Sources: Statistics Canada (2019), U.S. Department of Labor (2024), Institut de la statistique du Québec (2022), Bureau of Labor Statistics (2024), World Bank (2025).

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

ber of home-based care centers quadrupled. Together, the total number of regulated spaces increased from about 74,000 in 1996 to 200,000 in 2006 (Figure IA.1(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 (Figure IA.1(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). 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 to the provincial legislature in the spring of 1997 and faced opposition from unlicensed childcare providers who were excluded from 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).[8] Due to such

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

confounding policy shifts in other provinces, we do not use non-Quebec provinces as a control group in our main 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, strong 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.[9] Although parents of ineligible children paid unsubsidized prices for early enrollment, they still benefited from the increased supply of childcare spaces. These institutional features inform our empirical design, in which we exploit variation across birth cohorts in the number of years parents *knew about* the program rather than years of subsidy eligibility.

Despite Quebec introducing universal childcare as early as 1997, similar policies have been adopted more recently across many countries and regions. As summarized in Table IA.1, Canada introduced its federal version of universal childcare in 2021. Australia, Denmark, Germany, Japan, New Zealand, Norway, South Korea, and Wales (UK) have all introduced similar programs in the past two decades. Even now, universal childcare policies remain at the center of public debate, as evidenced by recent mayoral elections in New York City and Washington, D.C. (Times, 2025; Flynn, 2026). San Francisco and the state of New Mexico also expanded their free childcare programs in 2026 (Lee, 2025; Nowell, 2026). Given the similarity between Quebec's labor market in the late 1990s and those of many developed economies today, our setting and results offer relevant insights for childcare policy in these broader contexts.

[9] Anticipatory response to social programs before individuals become eligible is prevalent. See, for example, Attanasio and Rohwedder (2003), Card and Hyslop (2005), and Card et al. (2007).

2 Data and Sample

We use a custom administrative dataset 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.[10] 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 can observe, longitudinally, worker-level employment, turnover, earnings, and family structure, as well as firm-level labor and financial outcomes. This allows us to trace the career and productivity impacts of the policy at the individual and firm levels. Our data represent one of the most comprehensive datasets assembled to study childcare and employment.

We assign each parent 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 a particular child allows us to clearly define pre-birth and post-birth outcomes for each individual, and using the first child as the reference point is motivated by the greater career disruption associated with first-time parents.[11] 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).

[10] 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.

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

We further restrict our sample to individuals who gave birth between 1989 and 1997. We use the 1989-1992 cohorts as control cohorts who were unaffected by the reform, as their children would have been too old to qualify for the childcare program post reform. We use the 1993-1997 cohorts as treated cohorts who were affected by the reform to various extents. We stop at the 1997 cohort to avoid potential endogenous response, since the fertility decisions of post-1997 cohorts may have been influenced by the reform.[12] Our calendar year runs from 1989 to 2004.

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 5 employees in 1996 (the year before the reform) and to have operated both before and after the reform.

We examine four sets of individual-level outcomes: employment, turnover, sorting, and earnings. We define employment as an indicator for an individual filing a T4 tax slip in a given year with at least $1K CAD earnings.[13] We define quit as an indicator for an individual being separated from their employer in a given year and their ROE indicating "quit" as the reason for separation, which captures voluntary rather than forced turnover such as layoffs. Our primary sorting outcome is a firm's fraction of mothers of pre-kindergarten children (age<5) among all employees. This measure captures firms that were traditionally unattractive to mothers of young children. We also consider several other measures that capture hours flexibility, firm risk, and firm-specific pay. We detail these sorting measures in Section 4.2. Finally, we define earnings as the logarithm of an individual's T4 earnings in a year.

Table 1 provides summary statistics for the individual-year sample of mothers and firm-year sample of firms used in our analyses.[14] The average employment rate is 62% and the

[12]The reform was implemented in September 1997. Hence, all children born in 1997 should be conceived before the reform.

[13]Restricting to minimum $1K CAD annual earnings ensures we capture meaningful employment instead of "empty" filings.

[14]The individual-level sample corresponds to the sample used in our stacked DID specification. As a result, control cohorts appear multiple times.

average quit rate conditional on employment in a year is 4.8%. The median earnings is $17K CAD ($31.5K in 2026 CAD). Regarding the types of firms these mothers work at, 8.1% of these firms' workforce are mothers of young children. On average, these firms operate in industries with 33 weekly work hours and 22% part-time jobs. Their average financial leverage is 16 p.p. lower than industry peers and the average median employee salary is $16K.

The average firm in our firm-level sample employs, as a fraction of their total workforce, 39% female workers and 22% young (below age 40) female workers. In the three years before the reform, 3.9% of these firms' workforce were mothers of young children. The median firm has 9 employees, 3.9% ROA, $0.56M CAD sales, and $68.2K CAD sales per employee. Detailed variable definitions are in Appendix IA.1.

3 Empirical Strategy

Our individual-level analysis exploits the differential effects of the 1997 childcare program on mothers of children born in different years. Specifically, we implement a stacked difference-in-differences (DID) approach (Cengiz et al., 2019), comparing each treated individual (i.e., cohorts with some exposure to the 1997 reform) to never-treated individuals (i.e., cohorts never exposed to the 1997 reform) before and after childbirth. In this way, we avoid using already-treated units as controls, which can introduce bias in the presence of heterogeneous treatment effects or dynamic responses (Goodman-Bacon, 2021; Sun and Abraham, 2021).[15]

To implement our stacked DID, we construct a separate "stack" g for each treated cohort, defined by childbirth year g, where $g \in [1993, 1997]$. In each stack, we combine the treated cohort with a common set of never-treated cohorts who gave birth between 1989

[15]See Baker et al. (2022) for a discussion of using stacked DID to correct for biases in traditional two-way fixed-effect staggered DID. Another advantage of stacked DID is that it flexibly allows for continuous treatment, whereas most alternative methods allow only for binary treatment (e.g., Callaway and Sant'Anna (2021), Borusyak et al. (2024)).

(the first year of our data) and 1992 (the last untreated cohort) to form a panel balanced in calendar year from 1989 to 2004. This process yields a collection of stacked datasets, each containing one treated cohort and the same control cohorts. We then append these stacks to form a single estimation sample. Table IA.2 provides a breakdown of the sample size by cohort.

To exploit finer variation in treatment intensity, we use a generalized treatment variable $CCYears_{is} \in \{0, 1, 2, 3, 4, 5\}$ that captures the level of treatment individual i in stack s receives. Specifically, $CCYears_{is}$ is defined at the cohort level as the number of post-reform years before the child turns five. It equals 0 for never-treated cohorts (1989-1992), 1 for the 1993 cohort, 2 for the 1994 cohort, and so on, up to 5 for the 1997 cohort. We then estimate our baseline stacked DID by interacting *CCYears* with the post-birth indicator *PostBirth*:

$$Y_{ist} = \beta \cdot CCYears_{is} \cdot PostBirth_{ist} + \gamma_{is} + \delta_{st} + \lambda_{sy} + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the labor outcome for individual i in stack s and year t, γ_{is} are stack-by-individual fixed effects, δ_{st} are stack-by-calendar-year fixed effects, and λ_{sy} are stack-by-year-to-birth fixed effects (we define year-to-childbirth as $y = t - b$, where b is the child's birth year).[16] This specification forms a generalized difference-in-differences estimation in which the intensity of the treatment varies with how old the child was when the 1997 program was introduced. Our coefficient of interest β 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. Because never-treated cohorts serve as common controls in all stacks, they appear repeatedly in the stacked dataset, which may induce correlation in the error term across stacks. To account for this, we follow Cengiz et al. (2019) and cluster standard errors at the individual level.

In the above specification, stack-by-individual fixed effects remove fixed personal char-

[16]Note that $CCYears_{is}$ is absorbed by γ_{is} and $PostBirth_{ist}$ is absorbed by λ_{sy}.

acteristics, such as education, demographics, or innate tendency to participate in the labor market due to ability or ambition. We use stack-by-calendar-year fixed effects to control for macro trends. Stack-by-year-to-birth fixed effects absorb life-cycle patterns surrounding childbirth, including changes in women's preferences or constraints around childbirth. In our analysis, we are interested in changes around childbirth. Childbirth provides a well-defined reference point, allowing us to track outcomes relative to a consistent event regardless of when treatment occurs in calendar time. Because we focus on parents, we can assign event-time (i.e., years-to-childbirth) indicators to both treated and control cohorts. In our more stringent specifications, we also include stack-interacted industry (4-digit NAICS) or firm fixed effects to identify within-industry and within-firm variation.

We also estimate an event-study version of the stacked DID by interacting *CCYears* with a set of year-to-birth indicators $\mathbf{1}\{\text{Year-To-Birth}_{it} = y\}$:

$$Y_{ist} = \sum_{y \neq -1} \beta_y \cdot CCYears_{is} \cdot \mathbf{1}\{\text{Year-To-Birth}_{ist} = y\} + \gamma_{is} + \delta_{st} + \lambda_{sy} + \varepsilon_{ist} \tag{2}$$

where β_k captures the marginal effect of a one-unit increase in treatment intensity in event year y.

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 do not yet qualify for the subsidized rate (anticipatory years). During the anticipatory years, parents pay the unsubsidized price for childcare but benefit from access to more 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 and/or 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*. 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 1996-cohort 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 their child's life (at age one versus age two). If parents responded to the subsidy program by enrolling their child early to secure a spot, we should expect childcare-induced career interruption to be shorter and less severe for 1996-cohort parents than for 1995-cohort parents. In later analysis, we exploit these adjacent-cohort comparisons to decompose the different margins of response.

3.1 Validating Sources of Identification

We first present "first-stage" evidence that childcare take-up increased in response to the reform. Since we do not observe childcare take-up in our sample, we use the National Longitudinal Survey of Children and Youth (NLSCY) to validate the reform's anticipatory and eligibility effects on childcare take-up.[17] We focus on Quebec children aged 0-4 and create two treatment indicators for anticipatory years and eligible years as in Table 2, with pre-reform years as the control years. Appendix Table IA.3 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 p.p. increase in take-up) and an eligibility effect (a 9.5 p.p. increase in take-up), representing 36% and 110% increases relative to the respective pre-reform mean, respectively. Thus, both margins are important for our analysis.

To further validate our cross-cohort comparison, Figure 1(a) plots the mean employment rates for Quebec women by cohort from one year before to five years after childbirth.[18] We

[17] We use the 94-95, 96-97, and 98-99 cycles of the survey. The sample is a repeated cross-section because children are de-identified in the publicly available data.

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

observe 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 a kink point at which the later cohort diverges upward. These kinks appear 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.2a. Since both cohorts became eligible for the subsidy at the same age (Table 2), the pattern indicates that the difference arises from the 1994 cohort's earlier anticipatory response. Similarly, when we compare the 1994 and the 1995 cohorts (Appendix Figure IA.2b), we find a stronger response for the 1995 cohort, who had 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.

To address the potential concern that secular trends in female employment drive the patterns in Figure 1a, we plot the employment rates for placebo cohorts in Figure 1b. We find no diverging patterns for 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 patterns in Figure 1a are unlikely to be driven by secular increases in female labor market participation. Additionally, any secular trend should shift employment in parallel across cohorts rather than diverging at a specific child age.

Finally, Figure 1c shows the average employment rates collapsed within treated (1993-1997) and control cohorts (1989-1992). We find that the two groups trended similarly in employment over the three years before childbirth but started to diverge after childbirth. This pattern supports our identifying assumption of parallel trends between treated and control cohorts.

4 Mothers' Reallocation Across Firms

4.1 Employment and Turnover

Panel A of Table 3 formalizes the employment results in Figure 1(a), estimating Equation 1. We find that earlier access to government childcare significantly increases the likelihood of women being employed post-childbirth relative to pre-childbirth (column 1). In particular, accessing childcare five years earlier—i.e., full access for all years before kindergarten—increases mothers' employment rate by 2.65 p.p., a 4.3% increase relative to the mean. This effect amounts to 18.2% of the child penalty in employment estimated in Kleven et al. (2022) for Canada, which is sizable given that a large part of the child penalty is driven by gendered preferences rather than gendered incentives (Kleven, 2022; Kleven et al., 2022). Nevertheless, our estimate is smaller than those reported 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 and 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.

Columns 2 and 3 of Table 3, Panel A further partitions 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 employed before childbirth, whereas mothers non-employed pre-childbirth do not react significantly to the reform.[19] These results could be explained by the fact that those not working before childbirth did not have a preference to participate in the labor force, while relaxing childcare constraints reduced career interruptions for women already working before childbirth by increasing their probability of *returning to work* afterward.

[19]Note that non-employed excludes maternity leave. We define non-employment as the absence of a T4 wage slip. Individuals on leave from their employer would still be issued a T4 and classified as employed (on leave).

Table 3 Panel B examines voluntary turnover conditional on employment. Ex-ante, lowering childcare constraints could allow women to reallocate their human capital more freely, or increase their job continuity by increasing retention by employers. To test this, we define voluntary turnover based on "quits" in the records of employment (ROE).[20] We find a large positive effect on quit rate: a five-year earlier access to childcare increases a new mother's quit rate by 0.75 p.p., a 16% effect relative to the mean. These effects are similar when including stack-by-industry or stack-by-firm fixed effects, which control for average turnover rates at the industry or firm level.

Panels A and B of Figure 2 show the dynamics for employment and quits, following the specification in Equation 2. We find largely parallel trends in these outcomes across cohorts before childbirth, supporting our DID identification assumption. The treatment effects start immediately in the year after childbirth and grow in magnitude over time.

Overall, these findings suggest that access to childcare increases women's attachment to the labor market after childbirth and enhances their labor mobility. These results are consistent with childcare support freeing up time and resources for new mothers, enabling them to continue pursuing their careers and in more active ways.

4.2 Worker-Firm Matching

The increased labor mobility of new mothers raises the question whether earlier access to childcare affects the *type of firms* they now work for. Becker (1985) argues that childcare responsibilities lead women to seek less demanding careers. Accordingly, we might expect new mothers with better childcare access to pursue more ambitious or demanding careers that they would previously avoid. On the other hand, childcare cost subsidies may substitute for mothers' employment income, shifting women toward lower-paying jobs and less demanding

[20]Quits from ROE identify voluntary rather than forced separations. Our results cannot be driven by quitting to become non-employed, as we show that the employment rate actually increased for those employed before childbirth.

careers.[21] Finally, new mothers may simply have more time to work or search for new jobs, without necessarily changing the direction of their careers or the types of firms they work for.

To examine how childcare access shapes the matching of new mothers to firms, we consider a revealed-preference measure based on where mothers tend to work. To this end, we define *%Mom*—the fraction of a firm's employees who are mothers of children under five—as a measure that captures both mothers' labor supply and firms' labor demand for mothers. This measure summarizes various factors that affect both the costs mothers face in working at a given firm and the costs firms incur in employing them. We use *%Mom* as the dependent variable in the same individual-level specification as in Table 3, conditional on employment. Section 4.2.1 examines the economic factors underlying *%Mom* to better understand its relationship with childcare-related constraints.

Table 4 Panel A shows that earlier access to childcare increases the likelihood that new mothers are employed at firms where they were traditionally underrepresented (column 1). Specifically, accessing government childcare five years earlier shifts new mothers to work at firms with a *%Mom* that is 3.1% lower than the mean. This effect is similar when we include industry fixed effects (column 2), suggesting that it also happens within an industry across firms. Panel C of Figure 2 shows the dynamics. We see largely parallel trends in employers' *%Mom* across cohorts before childbirth, with treatment effect becoming evident after childbirth and growing steadily thereafter. As a placebo, we define *%Dad* to capture firms where fathers typically work. We do not see a similar sorting effect for that measure (Table IA.5). These results suggest that childcare creates gender-specific barriers that prevent mothers from pursuing employment at particular types of firms.

To establish patterns of labor reallocation, we decompose employer switches into moves

[21] Vattø and Østbakken (2024) show that childcare subsidies for low-income parents in Norway reduced their labor supply, while Core and Karpati (2024) show that maternity cash benefits substitute the income of self-employment mothers in the Netherlands. The literature also documents that positive wealth or income shocks reduce individuals' labor supply or job-search efforts (e.g., Lentz and Tranaes (2005), Card et al. (2007), Chetty (2008), Cesarini et al. (2017), and Li et al. (2020)).

into high-%mom versus low-%mom firms. We then examine, through an interaction, how these switching patterns differ by mother's pre-birth employer type—in particular, whether her pre-birth employer had a higher share of new mothers (i.e., *High%Mom_PreBirth*). We find that childcare access increased job switching mainly among mothers employed at high-%mom firms pre-birth, while those working at low-%mom firms pre-birth had little response (columns 3-4). In particular, women at high-%mom firms pre-birth now switch away from such firms after childbirth toward low-%mom firms. In other words, switching largely occurred from high-%mom firms to low-%mom firms. These results suggest that childcare subsidies induce new mothers to sort away from firms where they were previously overrepresented toward firms where they are underrepresented, thereby reducing gender segmentation across firms in the labor market.

Panel B of Table 4 further shows that similar sorting happens at the extensive margin, when mothers who would otherwise have left employment join the labor force. We establish this by decomposing the employment indicator in Panel A of Table 3 by whether the employer is a high- or low-%mom firm, so that the dependent variable captures both labor force entry and the type of firm one enters. We find that mothers who would have left employment are now 2.7% more likely to be employed at a high-%mom firm (column 1) but are 6.2% more likely to be employed at a low-%mom firm (column 2). This much larger effect of entering employment through low-%mom firms than through high-%mom firms is consistent with the earlier finding of sorting into low-%mom firms conditional on employment.

Panel C of Table 4 examines the heterogeneity in sorting, using a triple-difference specification. One benefit of this specification is that we can include cohort×year-to-birth fixed effects to absorb any unobserved secular trends across cohorts, further tightening identification. We find that married, older, higher-earning, and higher-ability (all measured pre-childbirth) mothers are more likely to sort to low-%mom firms.[22] This heterogeneity may

[22]Individual-level ability is measured with individual fixed effects extracted from an AKM regression model estimated on pre-childbirth job histories.

reflect the fact that these individuals tend to be mid-career workers with greater ability or incentives (e.g., due to a career bottleneck) to switch to careers in which they were traditionally underrepresented. Indeed, Table IA.6 shows that these individuals are less likely to be entry-level workers (i.e., bottom 10% earners) within their firms and tend to have longer tenures. Additionally, higher income, as well as income insurance from the spouse, may enable these individuals to take greater career risks.

4.2.1 Other Sorting Dimensions

To better understand why new mothers sort to low-%mom firms after the reform, we next unpack the economic dimensions underlying the *%Mom* measure. As discussed in the previous section, *%Mom* captures the advantage that certain firms have in providing amenities and work environments that attract mothers, and we explore the sources of this advantage.

First, an implication of childcare constraint is that new mothers are limited to jobs with shorter, more flexible hours (Becker, 1985). Although we do not directly observe job hours or occupations in our data, we can observe a firm's industry and thus measure the average hours and flexibility of that industry from alternative data sources. Using the Canadian IPUMS (Integrated Public Use Microdata Series) data, we measure the average weekly hours worked and the fraction of part-time jobs in an industry (NAICS 2-digit) in Quebec in the 1990s. We show that, in the cross-section, low-%mom firms indeed tend to be in industries with longer hours and fewer part-time opportunities—industries that are less able to offer flexible work schedules (Tables IA.7 and IA.8). Accordingly, we find in Table 5 that better access to childcare motivated new mothers to move into such industries with more demanding work schedules (columns 1-2). In particular, five-year-earlier access to childcare shifts new mothers into industries with 3.9% standard deviations more hours and 3.4% standard deviations lower fraction of part-time jobs.

Second, childcare constraints could lead mothers to avoid risky careers. Baghai et al. (2023) show that employed mothers face higher labor risk than women without children

or men when firms are hit by idiosyncratic shocks: they are more likely to be laid off or experience wage cuts. The authors argue that home-production demands—especially childcare constraints—help explain this pattern, as employed mothers are less able to expand hours precisely when firms require greater employee commitment. Thus, mothers would avoid risky firms ex ante in the absence of childcare support. Alternatively, childcare constraints may reduce the time available for job search if one is laid off (e.g., difficulty attending interviews with a newborn), again leading mothers to avoid risky firms ex-ante. Consistent with this, we find that the reform allowed mothers to sort to riskier firms, measured by 4.3% higher within-worker earnings volatility and 2.5% standard deviations higher financial leverage relative to industry peers (columns 3-4 of Table 5). In the cross-section, we also find that low-%mom firms exhibit higher earnings volatility and higher leverage relative to peers (Tables IA.7 and IA.8).[23]

Finally, it is well documented that women sort into lower-paying firms due to their higher willingness to pay for non-wage amenities or their limited ability to take on more demanding or riskier jobs that are typical of higher-paying firms (Goldin, 2014; Sorkin, 2018; Mas and Pallais, 2017; Wiswall and Zafar, 2018; Le Barbanchon et al., 2021). Indeed, we show that in the cross-section, low-%mom firms tend to be higher-paying, measured by higher firm-specific wages extracted from the AKM model, higher median employee salary, and lower gender pay gap for women (Tables IA.7 and IA.8). As such, after the reform, new mothers sort into higher-paying firms: firms with 8.2% standard deviations higher AKM wages or 6.2% higher median worker wages (columns 5-6 of Table 5).

Taken together, the above results suggest that easing childcare constraints enables mothers to pursue more demanding, higher-risk, and higher-return careers that they would have otherwise avoided. In Figure 3, we show that these sorting effects increase over the five years after childbirth, and were absent before childbirth. Note that our *%Mom* measure is an all-encompassing measure of different firm attributes that determine mothers' human

[23]We also find that low-%mom firms tend to be smaller firms, which are typically riskier.

capital allocation across firms, including dimensions we cannot observe, such as the provision of non-wage amenities.[24] Nevertheless, the three dimensions we focus on—risk, return, and flexible jobs—capture most of the variation in *%Mom* related to childcare frictions, since we find no remaining sorting effect after residualizing *%Mom* on those three dimensions (Table IA.9).

4.3 Earnings

Our results thus far indicate that childcare subsidies reallocate female human capital toward higher-paying firms and more demanding careers. Such reallocations could translate into earnings gains for new mothers. Equally plausible, however, is that they enter at the bottom of these firms' hierarchies, earning no more than in their prior jobs. Whether these reallocations benefit new mothers by raising their earnings is therefore an empirical question, which we examine next.

Table 6, Panel A presents the earnings result, using the same specification in Equation 1, conditional on employed individual-years. The dependent variable is the logarithm of an individual's annual employment earnings from T4. For this analysis, we drop the year of childbirth from our sample since mothers are usually on extended leave in that year, and thus their T4 income is incomplete.[25] We find that accessing government childcare five years earlier increases a new mother's income by 8.3% post childbirth relative to pre-birth (column 1). This effect is 6.8% within an industry and 3.3% within a firm (columns 2-3), suggesting that a large part of the earnings increase is realized through switching firms or industries. Indeed, our prior sorting results showed that mothers with greater access to childcare tend to

[24]Firm-level frictions likely explain the variation in amenities provision across firms, such as differences in operational constraints, financial constraints, and economies of scale. Firms with little operational slack or higher productivity face a higher cost of worker absences and therefore are less able to offer flexible work schedules (Labanca and Pozzoli, 2026). Smaller and more financially constrained firms are less likely to provide benefits that involve high fixed costs and economies of scale (e.g., onsite daycare) (Mas, 2025; Lamadon et al., 2022; Liu et al., 2023; Ouimet and Tate, 2023). Indeed, in our data, low-%mom firms are smaller and more levered relative to industry peers.

[25]Income replacement from Employment Insurance maternity benefits is not included in T4 income.

move to higher-paying firms. Increases in work hours may also explain some of the earnings increase, but we do not observe hours worked at the job level. The dynamic plot in Figure 2(d) shows increasing earnings effects over the five years after childbirth.

The earnings increase for new mothers translates into a reduction in within-household gender pay gaps (Table 6 Panel B). Using a family-year level panel, we find that the reform reduced the ratio between the husband's and the wife's pay by 6.8% (column 1). This effect is similar even when we include fixed effects for the industries or firms of both of the partners (columns 2-3).

Consistent with mothers shifting into more demanding careers, we also show that the reform affected mothers' subsequent fertility decisions. Table IA.10 examines the 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 on a cross-section of mothers using a Poisson pseudo maximum likelihood (PPML) model. After controlling for mother's age, we find that five-year earlier access to government childcare for the first child reduces mother's subsequent fertility by 27% over the next 5 years and by 8.7% over the next 10 years.

4.4 Robustness and Further Analysis

Alternative Measures and Samples. Table IA.4 reports robustness to alternative definitions of turnover and earnings. Panel A presents two alternative turnover measures: 1) employer switch, defined as an individual filing T4 with a different employer than the one they filed with in the previous year (though such a switch may not necessarily be voluntary); 2) an indicator for employment with a different employer than the employer the year before childbirth. We continue to find a positive effect of childcare access on these turnover measures. Panel B examines alternative earnings measures: year-to-year earnings growth rate (columns 1-2), ratio of current earnings to pre-childbirth earnings (columns 3-4), and a "promotion" indicator for more than 10% year-to-year earnings increase (columns 5-6). We find

a positive effect of childcare access on all these alternative earnings measures. Finally, Table IA.11 shows that our individual-level results are similar when we exclude part-time employees (defined as workers in the bottom 10% earnings distribution of a firm) or public-sector employees.

Alternative Specifications. Table IA.12 uses fathers as a benchmark group in a triple-difference analysis. In this specification, we can further include cohort × year-to-birth fixed effects to tighten identification, absorbing unobserved secular trends that drive differences across cohorts. We continue to find similar effects using fathers as a benchmark group. This finding is consistent with mothers bearing most of the childcare responsibilities, and it is the relief from these responsibilities that drives our results.

Our baseline specification uses *CCYears* as the continuous treatment variable, which imposes a linear relationship between years of childcare access and individual outcomes. To enhance transparency and allow for any non-linearity, we also conduct a cohort-by-cohort DID, in which we interact the indicator for each treated cohort with *PostBirth* in separate, stack-by-stack regressions. Table IA.13 presents the results. We find a monotonically increasing employment effect from the 1993 cohort (whose child was 4 years old at the time of reform) to the 1997 cohort (whose child was just born at the time of reform). In particular, the employment effect of accessing childcare when one's child is 0 years old is 69% larger than that of accessing childcare when the child is 4 years old. This suggests that having *earlier* access to government childcare matters more for mothers' careers, since childcare responsibilities are greater when the child is younger.

Table IA.14 presents an alternative specification using non-Quebec provinces as the comparison group, following Baker et al. (2008). The treatment variable is an indicator of age eligibility for childcare subsidies, thereby capturing both the QC-non-QC difference post-1997 and the program's age-based rollout. To mitigate the effect of other confounding programs mentioned in Section 1, we focus on married mothers. Across both cross-sectional and panel

samples, we find that childcare subsidies raise post-childbirth employment. However, as discussed in Section 1, this specification does not account for the anticipatory effect.

Decomposing Margins of Response. The reform can affect mothers' employment through three margins: 1) anticipation of childcare cost subsidies (i.e., intertemporal smoothing of future subsidies), 2) eligibility for the actual subsidy, and 3) increased supply of childcare spots. The first and third margins constitute the anticipatory effect described in Section 3.1. Because adjacent birth cohorts in our sample face different combinations of these three forces, we can triangulate each margin's contribution by comparing pairs of adjacent cohorts. A simple decomposition (see Appendix IA.2 and Table IA.15 for details) shows that the supply effect is the largest margin, followed by the eligibility effect (17% smaller) and anticipation of future subsidies (37% smaller). Further confirming the supply margin, a cross-sectional test shows that mothers in CMAs (Census Metropolitan Areas) with higher pre-reform childcare-worker density exhibit smaller employment responses (Table IA.16),[26] consistent with these areas experiencing less relaxation of childcare supply constraint after the reform.

Individual-Level Magnitude. Given that not all eligible mothers take up government childcare, our individual-level estimates capture intent-to-treat (ITT) effects and understate the impact on mothers who actually used the program. To gauge the implied treatment effect on the treated (TOT), we combine our ITT estimates with the first-stage take-up results in Table IA.3. Table IA.3 shows that childcare take-up rose by 4.9 p.p. within 1.5 years of the reform. Rescaling our baseline estimates to the same 1.5-year horizon yields ITT effects of 1.29% for employment and 0.93% for sorting. Dividing them by the take-up effect yields TOT effects of 26% and 19%, respectively. The modest ITT magnitudes thus

[26]Following Rutigliano (2025), we construct our measure of childcare-worker density, *Local Childcare Supply*, as the fraction of childcare workers among all workers in a Census Metropolitan Area (CMA) in 1996.

mask the sizable effects on mothers who actually utilized subsidized childcare.

4.5 Frictions: Time vs Financial Constraint

We next discuss what underlying frictions the reform mitigated to explain our results. We consider two main frictions: financial constraints and time constraints imposed on new mothers by childcare responsibilities. The reform may have alleviated mothers' financial constraints by reducing out-of-pocket childcare expenses for individuals who would have paid for childcare services regardless of the reform. Alternatively, the reform may have alleviated mothers' time constraints by enabling individuals to outsource childcare responsibilities, both by expanding the supply of childcare spots and by making childcare affordable for those who would not have paid for childcare services.

Several pieces of evidence presented above suggest that the alleviation of time constraints, rather than financial constraints, is the primary mechanism driving our results. First, if the policy relieved time constraints, a direct implication is that mothers will be more likely to work in firms and industries with longer and less flexible hours. This is exactly what we found in Table 5. Additionally, the reform led new mothers to have fewer subsequent children after their first child (Table IA.10)—consistent with relaxed time constraints setting them on more intense career paths, while a financial-constraint story would instead predict *more* subsequent children as childcare becomes cheaper.

Second, our margin decomposition above highlights that the spot supply effect is the largest margin, followed by the financial subsidy effect, while the intertemporal smoothing effect from expected future subsidies is the smallest. Given that increased childcare spot supply directly alleviates time constraints (i.e., making it easier for mothers to place their children in childcare centers) while childcare cost subsidies mainly alleviate financial constraints, this result again supports alleviated time constraints as the primary mechanism explaining our findings.

Lastly, we found that the reform increased mothers' earnings, partly because they switched to higher-paying jobs. This runs counter to the financial constraints channel, which would predict sorting into lower-paying jobs in response to cheaper childcare via income substitution (Vattø and Østbakken, 2024; Core and Karpati, 2024).

5 Firm-Level Impact

We next examine the effect of the 1997 childcare reform on firms, focusing on whether firms with a low pre-reform share of mothers benefited from the labor reallocation following the relaxation of childcare constraints.

5.1 Firm-Level Specification

We employ a firm-level difference-in-differences model comparing firms with a higher fraction of new mothers employed in the three years before the reform (1994-1996) with those with a lower fraction. We estimate the following specification:

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \theta \times Low\%Mom_Pre_j \times Post97_t + \epsilon_{j,t}, \tag{3}$$

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), $Low\%Mom_Pre_j$ is an indicator equal to one if the fraction of women with pre-kindergarten children averaged over 1994-1996 for firm j is less or equal to the sample median, and $Post97_t$ is a dummy indicating years after 1997. Our main variable of interest is the interaction term $Low\%Mom_Pre_j \times Post97_t$, which measures the differential impact of the reform on low-%mom firms relative to high-%mom firms. Note that $Low\%Mom_Pre_j$ is absorbed by firm fixed effect and $Post97_t$ is absorbed by year fixed effect. Our sample period is 1994 to 2000. We cluster standard errors at the firm level.

We first examine the impact of the reform on firm performance: ROA (net income/lagged

total assets), sales, and labor productivity (i.e., sales per employee). To shed light on the mechanisms underlying the performance results, we also examine changes in employment, wages, labor composition, and employee retention.

5.2 Firm-Level Results

Table 7 shows the results on firm performance. We find that firms employing a lower fraction of new mothers before the reform (i.e., low-%mom firms) experienced improvements in performance relative to other firms after the reform. Specifically, the reform led to a 0.8 p.p. increase in ROA for low-%mom firms relative to high-%mom firms (column 1). This effect is 11.3% of the outcome mean and 1.6% of its standard deviation, reflecting the large dispersion of ROA among private firms.[27] Low-%mom firms also experienced a 13.1% increase in sales relative to other firms after the reform (column 2), explained by a 6.7% increase in labor productivity (i.e., sales per employee) (column 3) and a 5.6% increase in total employment (column 1 of Table 8 Panel B). Figure 4 shows the dynamics of the performance effects, which are consistent with parallel trends between the two groups of firms before the reform.

We next investigate potential channels that explain the outperformance of low-%mom firms after the reform. First, we find that low-%mom firms experienced an increase in female employment, consistent with our individual-level results that mothers reallocate to these firms. Panel A of Table 8 shows that low-%mom firms experienced a 3% increase in female employment share after the reform relative to other firms (column 1). This effect is larger at 7.4% when we focus on the share of young (age<40) female employees (column 2), and is much larger at 93.5% when we restrict to mothers of children below 5 (column 3). These increasing effects suggest that it is the individuals mostly affected by the reform

[27]The ROA effect we find is comparable in magnitude to the effects of CEO hospitalization or turnover documented in the literature. Bennedsen et al. (2020) find that a 10-day hospitalization of the CEO reduces firm operating profitability by 0.5 p.p. or 5.8% from its mean. Bennedsen et al. (2007) find that family successions are associated with a 1.41 p.p. lower profitability (21.7% of mean) relative to unrelated successions. This effect increases to 6 p.p. (92% of the mean) once family succession is instrumented with the gender of the first child.

that drive most of the changes in female employment. When focusing on employment levels, Panel B of Table 8 shows a 6% increase in female employment and a 9.5% increase in young female employment in low-%mom firms (columns 2 and 3). We also observe a 3.2% increase in male employment (column 4), likely reflecting positive spillovers from female employment growth as low-%mom firms expand.

The inflow of female workers to low-%mom firms could explain their sales growth, but not necessarily improved ROA and productivity. Panel A of Table 9 examines workers' ex-ante ability, measured by individual fixed effects extracted from an AKM regression estimated on pre-reform job histories. We find that low-%mom firms hired higher-ability female workers after the reform, as evidenced by a 5.5% standard-deviation increase in the average AKM of female joiners to these firms (column 1). At the same time, there is a 3.7% standard-deviation decrease in the average ability of male joiners (column 3), suggesting that firms now hire high-ability women but not high-ability men when filling new positions. The average ability of both female stayers and male stayers also increased in these firms, by 1.8% and 1.9% standard deviations respectively (columns 2 and 4). This result suggests the departure of lower-ability employees and better retention of higher-ability employees. Overall, the reform improved not just the quantity but also the quality of female labor supply to low-%mom firms, with positive spillovers to male workers who were retained.

Second, the increase in female labor supply exerted downward pressure on wages, thereby explaining the improved ROA in low-%mom firms. As shown in Panel B of Table 9, the average wages decreased by 1.9% in low-%mom firms (column 1), driven by a 3.8% decrease in female wages (column 2) and an insignificant 0.4% increase in male wages (column 3). The lower wages for female workers—despite female joiners' higher quality—is consistent with a supply-driven decrease in wages. This result is also consistent with a compression of wage differentials between low-%mom and high-%mom firms following the reform.[28]

[28]A plausible explanation is that pre-reform, low-%mom firms paid higher wages to compensate for the lack of family-friendly amenities. Post-reform, government-provided childcare substituted for firm-provided amenities, leading to greater pooling across firms and compressed wage differentials for female workers,

Third, better worker-firm matching can also explain the improved performance of low-%mom firms. Several pieces of evidence support this. We find that the quit rate went down for female workers in these firms, suggesting better retention and better matching (column 5 of Table 8 Panel B). Quit rate also decreased for male workers (column 6, Panel B Table 8), suggesting better matching for male workers who were retained. Further, our sorting heterogeneity results above (Panel A of Table 5) point toward assortative matching: higher-AKM mothers sort to low-%mom firms, which also tend to be higher-AKM firms, as shown in Tables IA.7 and IA.8. Lastly, note that our ex-ante worker AKM measure captures pre-reform ability before relaxation of childcare constraints. The larger increase in realized labor productivity (6.7%) relative to the increase in female workers' ex-ante AKM (1.8%-5.5%) suggests that the reallocation may have increased worker productivity ex post, potentially through better matching.

In sum, three channels explain the improved performance of low-%mom firms relative to other firms after the reform: 1) hiring of productive female workers, 2) compressed female wages due to increased female labor supply, and 3) better worker-firm matching.

5.3 Robustness and Further Discussions

Robustness. Our main treatment indicator is based on a below-median indicator for firms' pre-reform %mom. In the data, the median firm has zero %mom before the reform. In Table IA.17, we show robustness to using other percentiles (66.7th, 75th, 80th) as the cutoff to define low-%mom—our main results are quantitatively similar.

Our low-%mom measure captures three distinctive job or firm characteristics: long and inflexible hours, income risk, and high compensating differential. In Section 4.2.1, we established that mothers sort toward firms with these characteristics once childcare constraints are relaxed (Table 5). Table IA.18 further confirms the reallocation of female workers along

consistent with the wage decline at low-%mom firms.

these three dimensions at the firm level, with firms in longer-hours industries, higher earnings volatility, and higher firm-specific wages gaining young female workers relative to other firms after the reform (columns 1-3 and 5-7). Such reallocation also happens when we define low-%mom based on %mom projected onto these three job dimensions (columns 4 and 8).

Dynamics. Comparing the dynamics of firm performance and employment outcomes in Figures 4 and IA.3, we see that the increase in ROA, sales, and labor productivity for low-%mom firms did not significantly take off until 1999—two years after the reform. In contrast, employment changes happened immediately after the reform. As such, changes in labor composition preceded the effects on firm performance.

Connection to Individual-Level Magnitudes. Our individual-level sorting result documents a 3.1% shift toward lower-%mom firms. This magnitude should not be used to benchmark firm-level results, which often have larger magnitudes. First, firm-level results reflect not only low-%mom firms gaining female employment, but also compositional changes in workers' ability and potential increases in ex-post productivity due to better matching. More importantly, our individual-level results are identified from a specific set of first-time mothers around childbirth, while the firm-level results reflect the impact of the reform on a much broader set of workers, including all mothers and women without children, who may decide to change their careers before they have a child in anticipation of better future childcare.[29] Finally, as firms become more productive and scale up, their demand for male workers may also increase, further expanding firm employment. Consistent with this, we also find a 3.2% increase in male employment at low-%mom firms.

Efficiency and Equilibrium Implications. An important, yet unanswered, question is whether the Quebec universal childcare program generated net benefits. Theoretically,

[29] Women who do not plan to have a child may also find low-%mom firms more appealing after the reform, due to a more diverse workplace culture or female peer effects (Kanter, 1977; Carrell et al., 2010; Tate and Yang, 2015; Schwartz-Ziv, 2017).

the reallocation of mothers across firm types should improve efficiency, as it results from a relaxation of childcare constraints that previously limited women's matching with firms. However, fully quantifying the equilibrium effect of the childcare reform is difficult and is beyond the scope of our paper. This is because the program likely generated other general equilibrium or welfare effects that are hard to quantify.

Nevertheless, we perform a quantification of the Quebec childcare program from a purely fiscal perspective. Specifically, we conduct a back-of-the-envelope calculation of the program's net fiscal value. We obtain fiscal costs from realized annual program spending from Lefebvre and Merrigan (2008). The fiscal benefits stem from the program's incremental tax revenue generated by higher personal and corporate income. Table IA.19 provides details on the calculations. From 1997 to 2006, the annual program cost increased significantly from $288m in 1997 to $1,493m in 2006, with an average of $869m per year. The Quebec government gained $207m in annual personal and corporate income tax revenue from the program, while the federal government gained an additional $309m in annual tax revenue. Using a 10-year horizon and a 7% discount rate,[30] the program has an NPV (net present value) of $ -4.37 billion for Quebec, assuming it did not use federal funding to pay for the program. At the national level, the program has an NPV of $ -2.20 billion. As such, the program appears unprofitable from a purely fiscal perspective, consistent with the findings in Baker et al. (2008); Geloso and Eisen (2017).

6 Conclusion

This paper shows that relaxing childcare constraints on new mothers—through subsidized childcare—reallocates their human capital across firms with implications for both mothers' careers and firms' performance. Using linked Canadian administrative tax data and

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

cohort-level variation in access to Quebec's 1997 universal childcare program, we show that relaxing childcare constraints does more than increasing mothers' labor force participation: it fundamentally reallocates their human capital across the firm distribution.

Our central finding is that earlier access to government childcare shifts new mothers toward firms where they were traditionally underrepresented—firms with more demanding schedules, higher risk, and higher pay—leading to higher earnings for these individuals. These individual-level reallocations have meaningful firm-level impacts: firms previously with fewer mothers drew in more productive female workers and, after the reform, better retained existing female workers, resulting in gains in ROA, sales, and labor productivity.

Our findings reframe the case for childcare subsidies. Beyond the labor supply and family well-being arguments that dominate policy debates, universal childcare can reduce gender-based segmentation across firms and improve worker-firm matching—with downstream effects on firm performance. These results suggest that childcare constraints do not merely reduce female labor supply in aggregate, but distort its *allocation*—steering female human capital away from productive firms, at a cost that constrains both individuals' careers and firms' access to broader talent pools.

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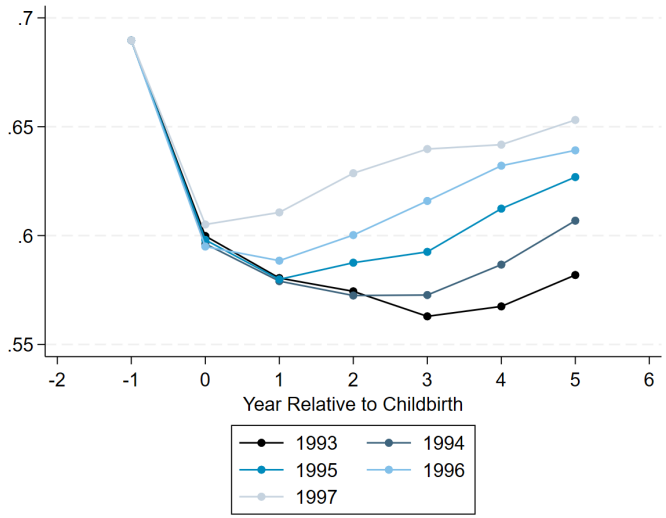
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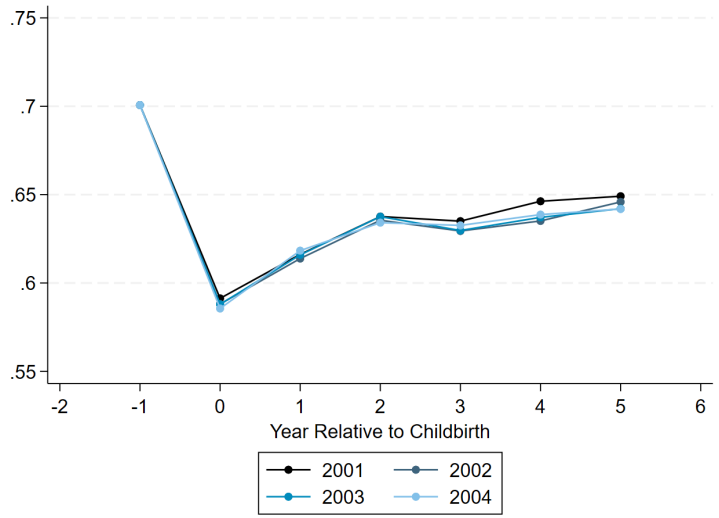
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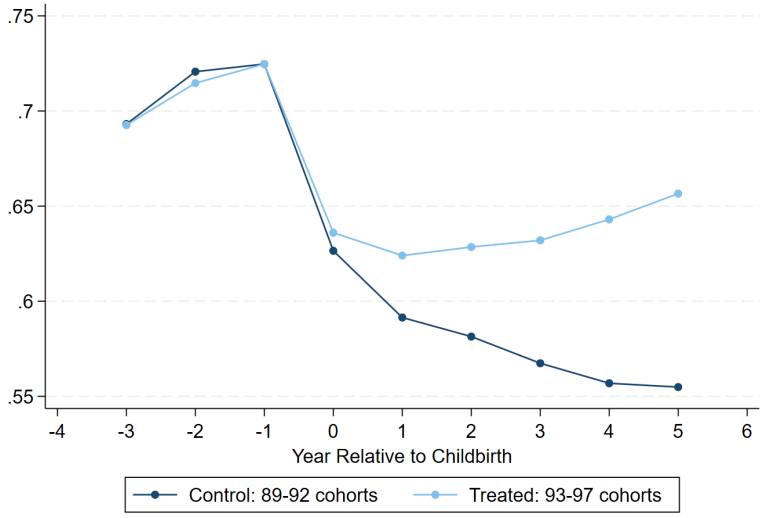
Figure 1: Mean Employment Rates Around Childbirth



(a) Treated Cohorts



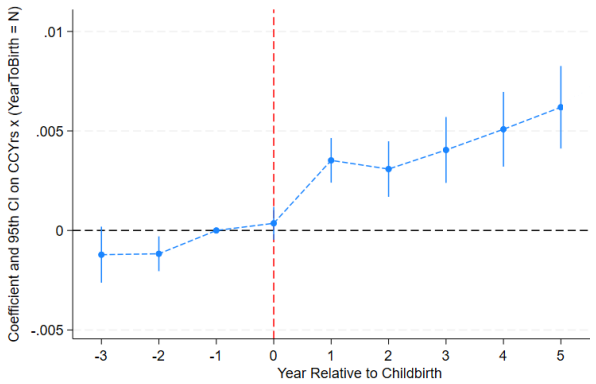
(b) Placebo cohorts



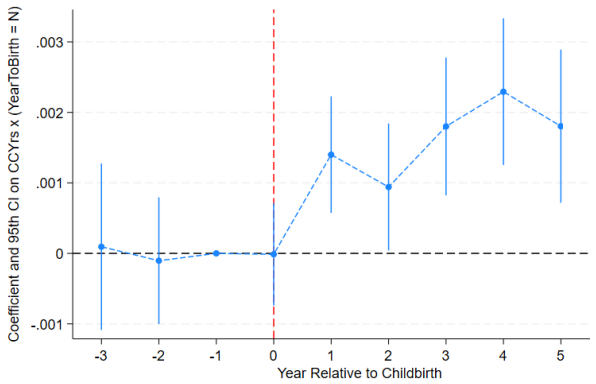
(c) Average of Treated vs Control Cohorts

Panels A and B show the adjusted mean employment rate for different cohorts of mothers from 1 year before to 5 years after childbirth. Panel A shows the treated cohorts from 1993 to 1997, each with a different level of exposure to the reform. Panel B shows the 2001-2004 placebo cohorts whose children were always eligible for the subsidy. Darker colors represent earlier cohorts. In both panels, the cohorts are shifted to align with the pre-childbirth employment rate of the earliest cohort. Panel C shows the collapsed average employment rates for treated and control cohorts over a window of -3 to 5 years relative to childbirth. The treated cohorts are mothers who gave birth from 1993 to 1997, and the control cohorts are mothers who gave birth from 1989 to 1992. For ease of comparison, the treated line is shifted to align with the control line in the year before childbirth.

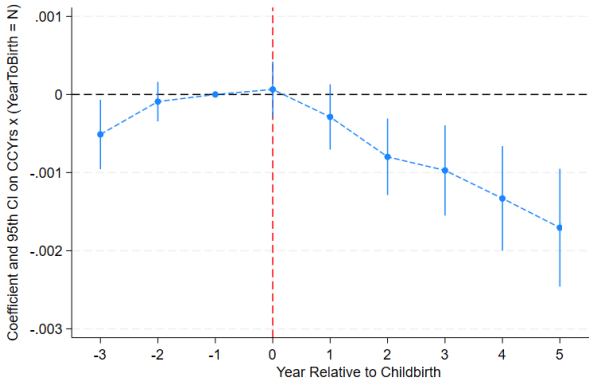
Figure 2: Dynamics Treatment Effects: Employment, Quit, Earnings, and Sorting



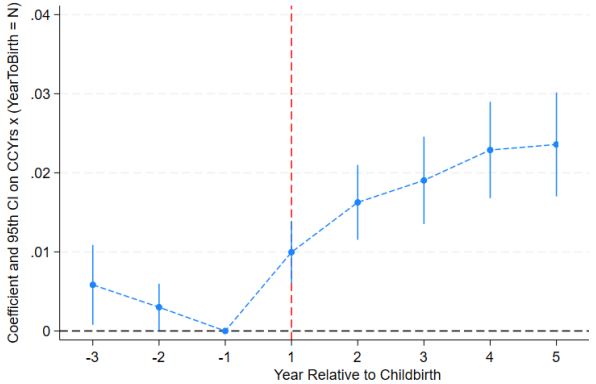
(a) Employed



(b) Quit



(c) %Mom of Employer



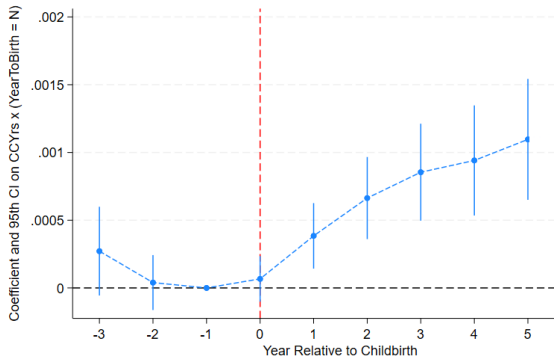
(d) Ln(Earnings)

These figures show the dynamic DID effects estimated from the equation

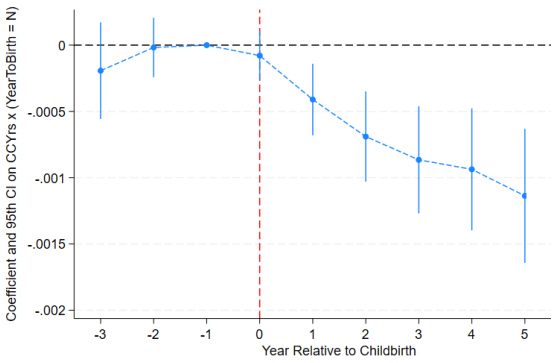
$$Y_{ist} = \sum_{y \neq -1} \beta_y \cdot CCYears_{is} \cdot \mathbf{1}\{\text{Year-To-Birth}_{ist} = y\} + \gamma_{is} + \delta_{st} + \lambda_{sy} + \varepsilon_{ist},$$

where the year before childbirth is omitted as the base year. Each dot (bar) represents the point estimate (95th confidence interval) of the coefficient on $CCYears_{is} \cdot \mathbf{1}\{\text{Year-To-Birth}_{ist} = y\}$ in Equation 2. *Employed* is an indicator equal to one if the individual is employed and earns a minimum of $1K CAD in a year. *Quit* is an indicator equal to one if the individual voluntarily leaves their employer. *%Mom of Employer* is the percent of mothers of children aged 0-5 among all employees of a firm in a year. *Ln(Earnings)* is the natural log of total T4 earnings.

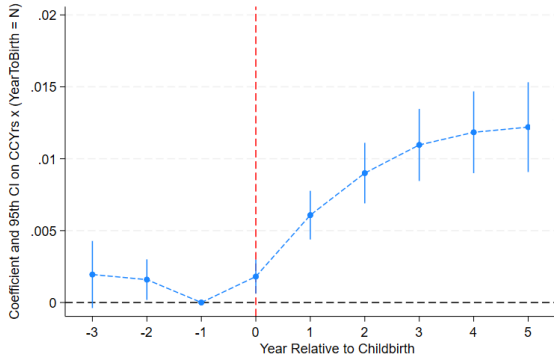
Figure 3: Dynamics Treatment Effects: Other Sorting Outcomes



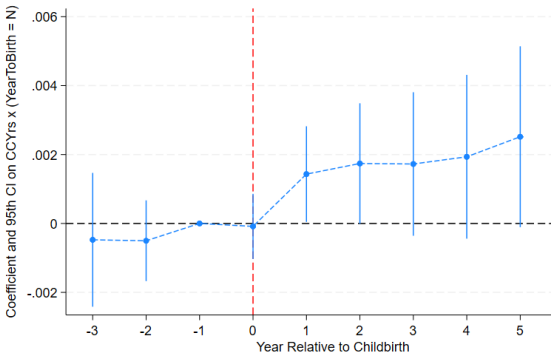
(a) Ln(Industry Hours)



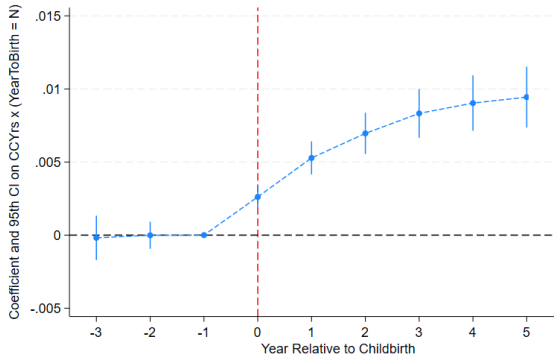
(b) Fraction Part Time



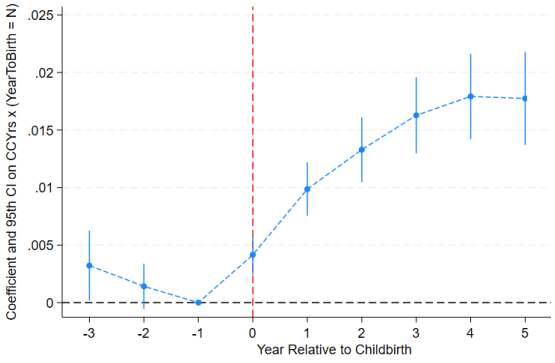
(c) Ln(Earning Vol)



(d) Excess Leverage



(e) Firm AKM



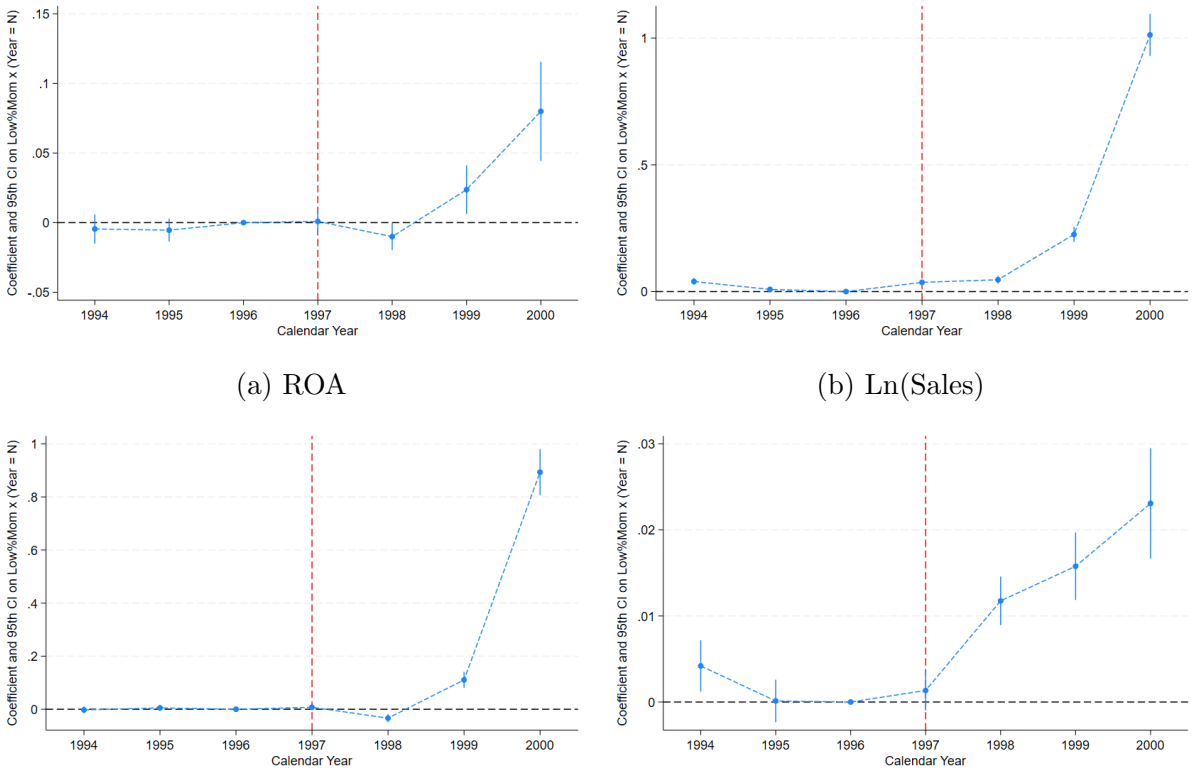
(f) Ln(Median Earning)

This figure shows the dynamic DID effects estimated from the equation

$$Y_{ist} = \sum_{y \neq -1} \beta_y \cdot CCYears_{is} \cdot \mathbf{1}\{\text{Year-To-Birth}_{ist} = y\} + \gamma_{is} + \delta_{st} + \lambda_{sy} + \varepsilon_{ist},$$

where the year before childbirth is omitted as the base year. Each dot (bar) represents the point estimate (95th confidence interval) of the coefficient on $CCYears_{is} \cdot \mathbf{1}\{\text{Year-To-Birth}_{ist} = y\}$ in Equation 2. The dependent variable for each plot represents a firm-level or industry-level characteristic and is indicated in the panel caption. Definitions for all dependent variables are provided in Appendix IA.1.

Figure 4: Impact on Firm Outcomes: Dynamics



(a) ROA

(b) Ln(Sales)

(c) Ln(Sales/Employment)

(d) %Female

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

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

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%Mom_Pre* is an indicator equal to one if a firm had a below-median percentage of mothers of pre-kindergarten children (age<5) among all employees in the three years before reform. Each dot (bar) represents the point estimate (95th confidence interval) for the coefficient on $Low\%Mom_Pre_j \times Year_t$. The four panels correspond to dependent variables *ROA*, *Ln(Sales)*, *Ln(Sales/Emp)* (i.e., labor productivity), and *%Female*, which are all defined in Appendix IA.1. Standard errors are clustered by firm.

Table 1: Summary Statistics

	N	Mean	Std. Dev.	P25	Median	P75
			Individual-level:			
Employed	19,038,350	0.616	0.486	0.000	1.000	1.000
Quit	12,494,360	0.048	0.213	0.000	0.000	0.000
Ln(Earnings)	11,675,590	9.373	1.369	8.827	9.736	10.290
Ln(Husband Pay/Wife Pay)	6,255,820	0.696	1.192	0.000	0.530	1.240
%Mom of Employer	12,494,360	0.081	0.107	0.033	0.058	0.091
Switch to High%Mom Firm	9,039,340	0.099	0.298	0.000	0.000	0.000
Switch to Low%Mom Firm	9,039,340	0.074	0.262	0.000	0.000	0.000
Married	12,494,360	0.325	0.468	0.000	0.000	1.000
Older	12,494,360	0.514	0.500	0.000	1.000	1.000
High Earnings	9,039,340	0.462	0.499	0.000	0.000	1.000
High AKM	11,932,850	0.433	0.496	0.000	0.000	1.000
Ln(Industry Hours)	12,366,390	3.504	0.089	3.462	3.488	3.583
Fraction Part Time	12,366,390	0.223	0.102	0.117	0.283	0.296
Ln(Earnings Vol)	12,394,460	8.807	0.645	8.384	8.888	9.228
Excess Leverage	8,796,460	-0.163	0.399	-0.399	-0.248	-0.016
Firm AKM	12,476,720	0.005	0.423	-0.261	0.050	0.334
Ln(Median Earnings)	12,486,910	9.705	0.855	9.206	9.976	10.336
			Firm-level:			
%Mom_Pre	356,820	0.039	0.071	0.000	0.000	0.055
Low%Mom_Pre	356,820	0.522	0.500	0.000	1.000	1.000
ROA	338,540	0.070	0.491	-0.019	0.039	0.136
Ln(Sales)	350,520	13.293	1.487	12.388	13.235	14.199
Ln(Sales/Employment)	347,330	11.073	1.238	10.442	11.129	11.769
%Female	353,210	0.385	0.305	0.128	0.333	0.600
%Young Female	353,210	0.224	0.240	0.000	0.158	0.364
%Mom	353,210	0.038	0.083	0.000	0.000	0.047
Ln(Employment)	353,210	2.237	1.146	1.386	2.197	2.944
Ln(Female)	298,610	1.403	1.150	0.693	1.386	2.197
Ln(Young Female)	239,080	1.248	1.080	0.000	1.099	1.946
Ln(Male)	329,430	1.757	1.160	1.099	1.609	2.485
Female Quit Rate	353,210	0.016	0.079	0.000	0.000	0.000
Male Quit Rate	353,210	0.020	0.076	0.000	0.000	0.000
Avg AKM of Female Joiners	156,920	-0.549	0.715	-0.950	-0.484	-0.076
Avg AKM of Female Stayers	270,950	-0.218	0.639	-0.568	-0.179	0.167
Avg AKM of Male Joiners	204,980	-0.270	0.655	-0.627	-0.222	0.143
Avg AKM of Male Stayers	305,300	0.178	0.601	-0.132	0.216	0.517
Avg Ln(Earnings)	353,140	9.321	0.763	8.859	9.390	9.852
Avg Ln(Female Earnings)	298,500	9.154	0.831	8.666	9.230	9.745
Avg Ln(Male Earnings)	329,340	9.455	0.845	8.995	9.554	10.007

This table presents the summary statistics for our individual-level and firm-level samples. All variables are defined in Appendix IA.1. 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 child's age 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 after the 1997 reform. Only the 1993 to 1997 cohorts are used as the treated cohorts in our main analysis.

Table 3: Employment and Turnover Effects

Panel A: Employment

	(1)	(2)	(3)
		Employed	
CCYears x Postbirth	0.0053***	0.0020**	0.0007
	(0.0007)	(0.0010)	(0.0015)
Sample	All	Prebirth Employed	Prebirth Non-Employed
Stack x Individual FE	X	X	X
Stack x Calendar Year FE	X	X	X
Stack x Year-To-Birth FE	X	X	X
Observations	19,038,350	11,464,620	3,679,250
Adj. R-sq.	0.521	0.425	0.370

Panel B: Turnover

	(1)	(2)	(3)
		Quit	
CCYears x PostBirth	0.0015***	0.0017***	0.0015***
	(0.0003)	(0.0003)	(0.0003)
Stack x Individual FE	X	X	X
Stack x Calendar Year FE	X	X	X
Stack x Year-To-Birth FE	X	X	X
Stack x Industry FE		X	
Stack x Firm FE			X
Observations	12,494,360	12,494,360	12,266,730
Adj. R-sq.	0.076	0.085	0.145

This table examines the effect of childcare access on females' employment status (Panel A) and job turnover (Panel B). The specification is a stacked DID as indicated in Equation 1. In Panel A, *Employed* is an indicator equal to one if the individual is employed in that year with a minimum $1,000 CAD ($720 USD equivalent) income. 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. In Panel B, *Quit* 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). All columns are conditioned on mothers' years of employment. Standard errors are reported in parentheses 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: Sorting on %Mom

Panel A: Sorting on %Mom

	(1) %Mom of Employer	(2) %Mom of Employer	(3) Switch to High%Mom	(4) Switch to Low%Mom
CCYears x PostBirth	-0.0005**	-0.0005**	-0.001	0.0003
	(0.0002)	(0.0002)	(0.0008)	(0.0005)
High%Mom_PreBirth x CCYears x PostBirth			-0.0059***	0.0038***
			(0.0005)	(0.0004)
Stack x Individual FE	X	X	X	X
Stack x Calendar Year FE	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X
Stack x Industry FE		X		
Observations	12,494,360	12,494,360	9,039,340	9,039,340
Adj. R-sq.	0.306	0.340	0.105	0.078

Panel B: Sorting at Entry Into Employment

	(1) Employed in High%Mom Firm	(2) Employed in Low%Mom Firm
CCYears x PostBirth	0.0018***	0.0035***
	(0.0007)	(0.0007)
Stack x Individual FE	X	X
Stack x Calendar Year FE	X	X
Stack x Year-To-Birth FE	X	X
Observations	19,038,350	19,038,350
Adj. R-sq.	0.405	0.392

Table 4: Sorting on %Mom (Continued)

Panel C: Heterogeneity in Sorting

	(1)	(2)	(3)	(4)
	%Mom of Employer			
CCYears x PostBirth x Married	-0.0022***			
	(0.0002)			
CCYears x PostBirth x Older		-0.0035***		
		(0.0002)		
CCYears x PostBirth x High Earnings			-0.0054***	
			(0.0002)	
CCYears x PostBirth x High AKM				-0.0045***
				(0.0002)
Stack x Individual FE	X	X	X	X
Stack x Calendar Year FE	X	X	X	X
Stack x Cohort x Year-To-Birth FE	X	X	X	X
Observations	12,494,360	12,494,360	9,039,340	11,932,850
Adj. R-sq.	0.306	0.307	0.302	0.308

This table examines how childcare access reallocates mothers to firms where they were underrepresented. The specification is a stacked DiD, following Equation 1, and the sample is conditioned on employed individual-years. In Panel A, the dependent variable *%Mom of Employer* is the employer's percentage of mothers with pre-kindergarten children among all employees. The dependent variables in columns 2 and 3 represent indicator variables for an individual switching employers to firms with above-median and below-median values of *%Mom of Employer*, respectively. We also interact treatment with $High\%Mom_PreBirth$, an indicator for whether the pre-birth employer had an above-median *%Mom of Employer* in the year before childbirth. Coefficients on $High\%Mom_PreBirth \times PostBirth$ are omitted from the table for brevity. Panel B examines sorting upon entry into employment. It decomposes the dependent variable, the employment indicator, in column 1 of Table 3 by whether the employer is above- or below-median in *%Mom of Employer*. Panel C examines the heterogeneity of our baseline sorting results by individual characteristics. 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. $High\ Earnings$ indicates that the individual had above-median earnings in the year before childbirth. $High\ AKM$ indicates individuals with above-median ex-ante ability, measured from individual fixed effects extracted from an AKM model estimated on pre-reform job histories. Standard errors are reported in parentheses 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: Sorting on Other Firm Characteristics

	(1) Ln(Industry Hours)	(2) Fraction PartTime	(3) Ln(Earnings Vol)	(4) Excess Leverage	(5) Firm AKM	(6) Ln(Median Earnings)
CCYears x PostBirth	0.0007***	-0.0007***	0.0085***	0.0020**	0.0069***	0.0124***
	(0.0001)	(0.0002)	(0.0010)	(0.0008)	(0.0007)	(0.0013)
Stack x Individual FE	X	X	X	X	X	X
Stack x Calendar Year FE	X	X	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X	X	X
Observations	12,366,390	12,366,390	12,394,460	8,796,460	12,476,720	12,486,910
Adj. R-sq.	0.613	0.628	0.637	0.619	0.608	0.586

This table examines how childcare access reallocates mothers across other firm types. The specification is a stacked DID following Equation 1 and the sample conditions on employed individual-years. *Ln(Industry Hours)* is the natural logarithm of average weekly hours worked at the industry level. *Fraction Part Time* is the fraction of jobs that are part-time. *Ln(Earnings Vol)* is the natural logarithm of firm-level average within-job earnings volatility. *Excess Leverage* is the difference between the liability-to-assets ratio of a given firm and the average liability-to-assets ratio across firms in the same industry. *Firm AKM* is the firm fixed effect as estimated from an AKM model as described in Section IA.3 of the Internet Appendix. *Ln(Median Earnings)* is the natural logarithm of the median worker's earnings within a firm in a given year. More detailed definitions for all variables are provided in Appendix IA.1. Standard errors are reported in parentheses 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: Effect on Earnings

Panel A: Individual Earnings

	(1)	(2)	(3)
	Ln(Earnings)		
CCYears × PostBirth	0.0166***	0.0135***	0.0066***
	(0.0025)	(0.0024)	(0.0023)
Stack x Individual FE	X	X	X
Stack x Calendar Year FE	X	X	X
Stack x Year-To-Birth FE	X	X	X
Stack x Industry FE		X	
Stack x Firm FE			X
Observations	11,675,590	11,675,590	11,446,170
Adjusted R-squared	0.558	0.571	0.625

Panel B: Within Household Gender Pay Gap

	(1)	(2)	(3)
	Ln(Husband Pay/Wife Pay)		
CCYears × PostBirth	-0.0136***	-0.0132***	-0.0118***
	(0.0027)	(0.0026)	(0.0025)
Stack x Family FE	X	X	X
Stack x Calendar Year FE	X	X	X
Stack x Year-To-Birth FE	X	X	X
Stack x Wife industry FE		X	
Stack x Husband industry FE		X	
Stack x Wife Firm FE			X
Stack x Husband Firm FE			X
Observations	6,255,820	6,255,820	5,962,930
Adj. R-sq.	0.523	0.535	0.621

This table examines the effect of childcare access on women's earnings. The specification is a stacked DID as defined in Equation 1. In Panel A, *Ln(Earnings)* is the logarithm of total T4 earnings in a year. The sample excludes the year of childbirth, when mothers are on leave, and their T4 (i.e., labor) income is only partial. Panel B examines the effect of childcare access on within-family gender pay gap, where the analysis sample is at the family-year level. *Ln(Husband Pay/Wife Pay)* is the natural logarithm of the ratio between the husband's pay and the wife's pay. Same-sex couples or family-years where the wife is not employed are excluded. All columns condition on mothers' years of employment. Standard errors are reported in parentheses 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: Impact on Firm Performance

	(1) ROA	(2) Ln(Sales)	(3) Ln(Sales/ Employment)
Low%Mom_Pre × Post97	0.0079*	0.1308***	0.0665***
	(0.0042)	(0.0085)	(0.0086)
Firm FE	X	X	X
Industry x Year FE	X	X	X
Observations	338,540	350,520	347,330
Adj. R-sq.	0.249	0.775	0.675

This table examines the impact of the reform on Quebec firms' financial performance. The sample period is 1994 to 2000. The specification follows Equation 3. All columns include firm fixed effects and NAICS 4-digit-year fixed effects. *Low%Mom_Pre* is an indicator equal to one if a firm had a below-median average percentage of mothers of pre-kindergarten children (age<5) among all employees in the three years before the reform. *ROA* is net income divided by lagged total assets. *Ln(Sales)* is the natural logarithm of sales. *Ln(Sales/Employment)* is the natural logarithm of labor productivity, i.e., the natural logarithm of sales divided by employment. Standard errors are reported in parentheses and are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 8: Impact on Firm Employment

Panel A: Worker Composition

	(1) %Female	(2) %Young Female	(3) %Mom
Low%Mom_Pre × Post97	0.0114***	0.0166***	0.0354***
	(0.0012)	(0.0012)	(0.0006)
Firm FE	X	X	X
Industry x Year FE	X	X	X
Observations	353,210	353,210	353,210
Adj. R-sq.	0.875	0.804	0.483

Panel B: Employment and Retention

	(1) Ln(Employment)	(2) Ln(Female)	(3) Ln(Young Female)	(4) Ln(Male)	(5) Female Quit Rate	(6) Male Quit Rate
Low%Mom_Pre × Post97	0.0563***	0.0603***	0.0947***	0.0316***	-0.0029***	-0.0025***
	(0.0044)	(0.0044)	(0.0054)	(0.0045)	(0.0004)	(0.0004)
Firm FE	X	X	X	X	X	X
Industry x Year FE	X	X	X	X	X	X
Observations	353,210	298,610	239,080	329,430	353,210	353,210
Adj. R-sq.	0.885	0.896	0.859	0.887	0.709	0.606

This table examines the impact of the reform on Quebec firms' worker composition, employment, and employee retention. The sample period is 1994 to 2000. The specification follows Equation 3. All columns include firm fixed effects and NAICS 4-digit-year fixed effects. *Low%Mom_Pre* is an indicator equal to one if a firm had a below-median average percentage of mothers of pre-kindergarten children (age<5) among all employees in the three years before the reform. Panel A examines the composition of employees. *%Female* (*%Young Female*) is the percent of (young—i.e., age<40) female employees. *%Mom* is the percent of mothers with pre-kindergarten children in a year. Panel B examines employment counts and quit rates for different groups of employees. *Ln(Employment)*, *Ln(Female)*, *Ln(Young Female)*, and *Ln(Male)* denote the natural logarithm of the total number of workers, the number of female workers, the number of female workers under the age of 40, and the number of male workers, respectively, in a given firm-year. *Female Quit Rate* (*Male Quit Rate*) denotes the number of female (male) workers in a given firm who voluntarily leaves a firm divided by total employment at that firm. Standard errors are reported in parentheses and are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 9: Workers' Ex-Ante Ability and Earnings

Panel A: Average Ex-Ante Ability by Group

	(1)	(2)	(3)	(4)
	Average AKM of			
	Female Joiners	Female Stayers	Male Joiners	Male Stayers
Low%Mom_Pre x Post97	0.0392***	0.0113***	-0.0243***	0.0115***
	(0.0106)	(0.0036)	(0.0071)	(0.0029)
Firm FE	X	X	X	X
Industry x Year FE	X	X	X	X
Observations	156,920	270,950	204,980	305,300
Adj. R-sq.	0.212	0.809	0.21	0.823

Panel B: Average Earnings

	(1)	(2)	(3)
	Avg	Avg	Avg
	Ln(Earnings)	Ln(Female Earnings)	Ln(Male Earnings)
Low%Mom_Pre x Post97	-0.0190***	-0.0380***	0.0044
	(0.0039)	(0.0053)	(0.0047)
Firm FE	X	X	X
Industry x Year FE	X	X	X
Observations	353,140	298,500	329,340
Adj. R-sq.	0.719	0.637	0.678

This table examines the impact of the reform on Quebec firms' worker ex-ante quality (Panel A) and average worker earnings (Panel B). The specification and sample follow Table 8. *Low%Mom_Pre* is an indicator equal to one if a firm had a below-median average percentage of mothers of pre-kindergarten children (age<5) among all employees in the three years before the reform. In Panel A, the dependent variables are the average ex-ante (pre-1997) worker ability across worker categories. Worker-level ability is individual-level fixed effect extracted from AKM regressions estimated on pre-reform job histories (see Appendix IA.3 for details). *Female Joiners* (*Male Joiners*) denote the group of female (male) workers who did not work at their current employer in the previous year. *Female Stayers* (*Male Stayers*) denote the group of female (male) workers who worked at their current employer in the previous year. In Panel B, the dependent variables are the average log earnings for all employees (*Avg Ln(Earnings)*), female employees (*Avg Ln(Female Earnings)*), and male employees (*Avg Ln(Male Earnings)*), respectively. Standard errors are reported in parentheses and are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

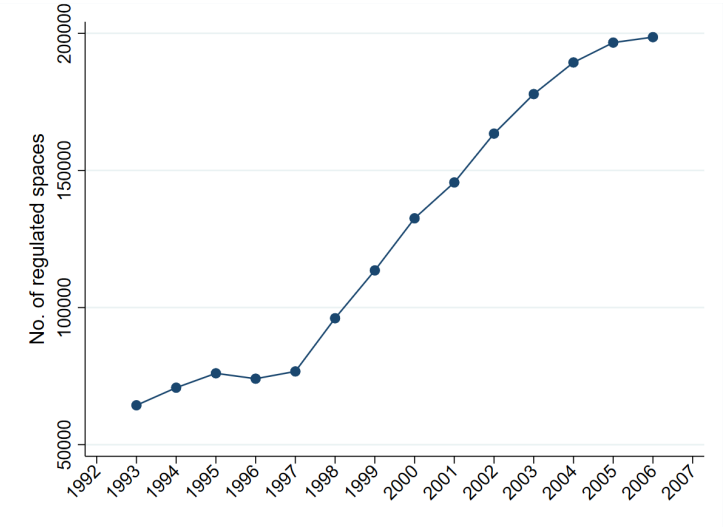
Internet Appendix to "Childcare Access and the Allocation of Female Human Capital Across Firms"

Elena Simintzi (UNC-Chapel Hill, CEPR and ECGI)

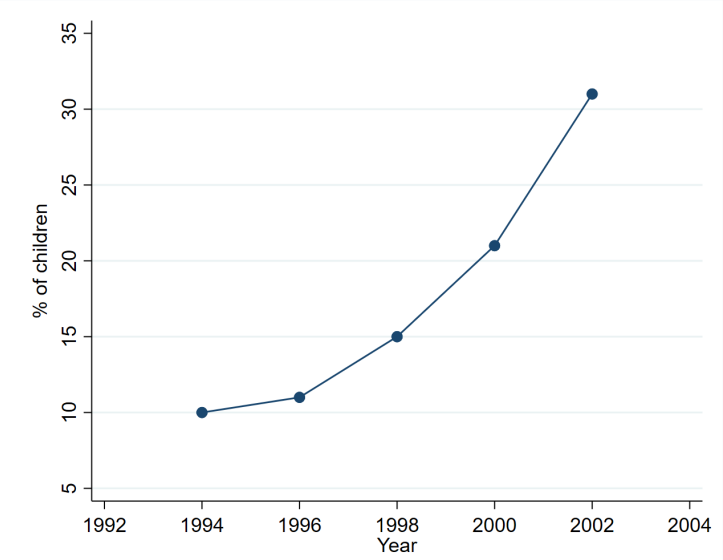
Sheng-Jun Xu (University of Alberta)

Ting Xu (University of Toronto and NBER)

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



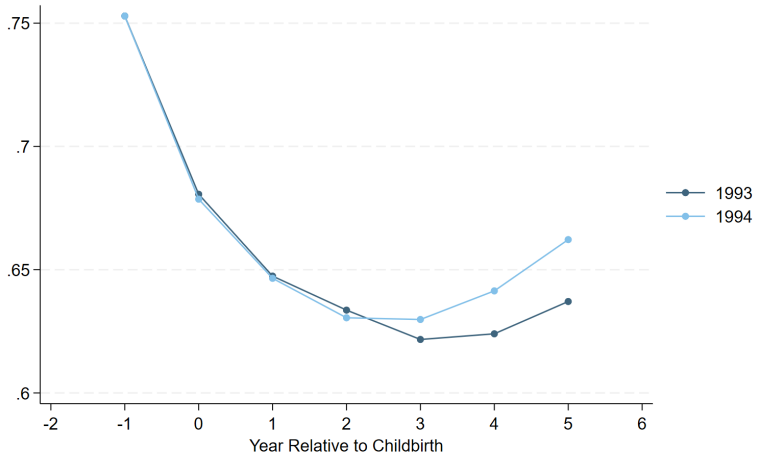
(a) Number of regulated childcare spaces



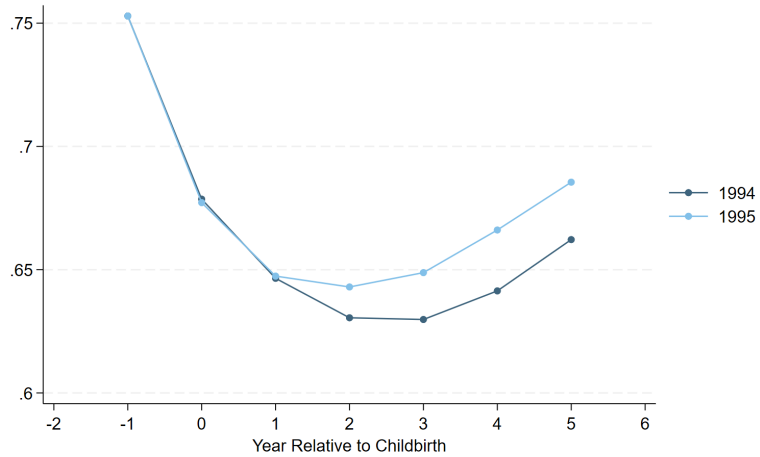
(b) Percentage of 1-5-year-olds in childcare centers

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 a childcare center. The data come from the biennial National Longitudinal Survey of Children and Youth (NLSCY).

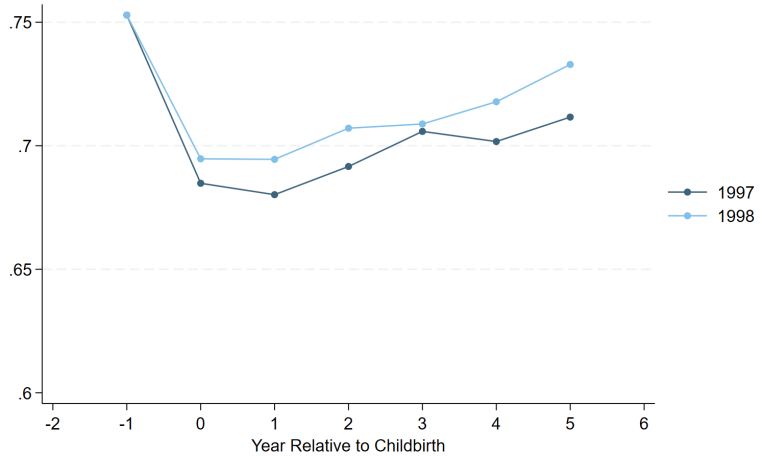
Figure IA.2: Employment Rates Around Childbirth by Cohort Pairs



(a) 1993 and 1994 Cohorts



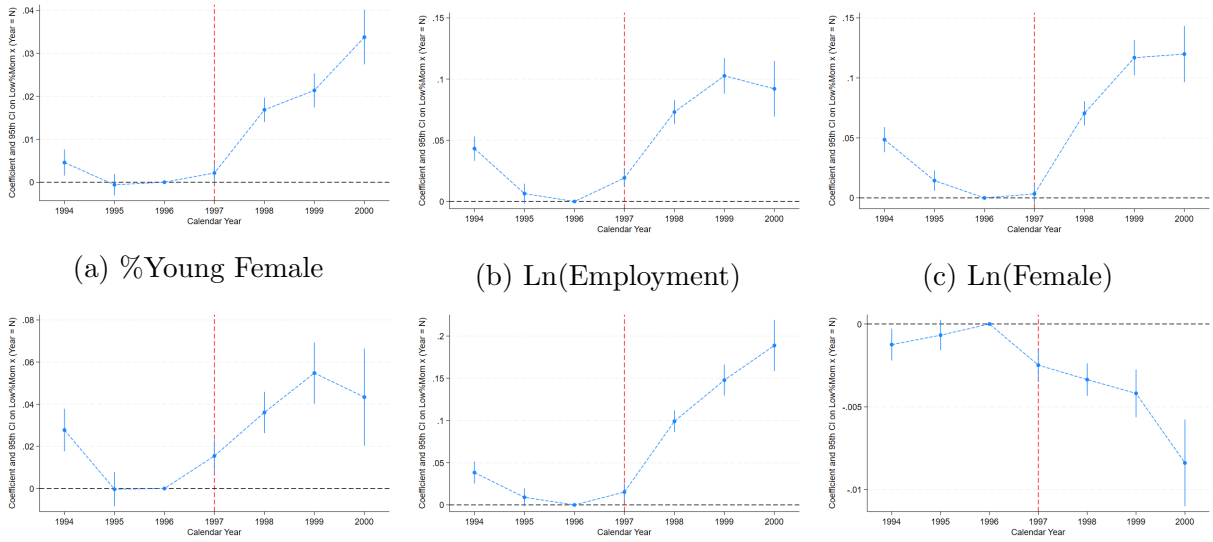
(b) 1994 and 1995 Cohorts



(c) 1997 and 1998 Cohorts

This figure shows the adjusted mean employment rates for pairs of adjacent cohorts of mothers (1993/1994 for Panel (a), 1994/1995 for Panel (b), and 1997/1998 for Panel (c)) 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.

Figure IA.3: Impact on Firm Employment: Dynamics



(a) %Young Female (b) Ln(Employment) (c) Ln(Female)

(d) Ln(Male) (e) Ln(Young Female) (f) Female Quit Rate

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

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

where 1996 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 $Low\%Mom_Pre$ 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 $Low\%Mom_Pre_j \times Year_t$. The dependent variable for each regression is indicated in the panel caption, representing: (a) the percentage of female workers under the age of 40, (b) the natural logarithm of total employees, (c) the natural logarithm of total female employees, (d) the natural logarithm of total male employees, (e) the natural logarithm of female employees under age 40, and (f) the percentage of female employees who leave their current employer voluntarily. Standard errors are clustered by firm.

Table IA.1: Countries or Regions that Have Introduced Universal Childcare

Jurisdiction	**Introduced**	**Key Features**
Australia	2022	Australia implemented a major expansion of its Child Care Subsidy program, increasing subsidy rates for most families and extending eligibility to additional households. The reform was designed to lower families' out-of-pocket childcare expenses and encourage greater labor-force participation among women.
Canada	2021	The federal government signed agreements with all provinces and territories to establish a nationwide subsidized childcare system, with the goal of reducing average childcare fees to $10 per day by 2025–2026. The initiative constitutes one of the most significant expansions of social policy in recent Canadian history.
Denmark	1990s & 2004	Children are guaranteed access to a childcare spot starting at 6 months of age. Childcare costs are heavily subsidized by municipalities, which cover 75% of expenses, while parents pay the remaining 25%. Universal access to childcare has been guaranteed since 2004.
Finland	1996	Families are guaranteed access to municipally provided daycare upon the end of parental leave. Childcare fees are income-based and heavily subsidized, resulting in relatively low out-of-pocket costs for most families and high take-up rates.
France	1989 & 2019	Free *école maternelle* is available for children aged 3–6 and is administered by the Ministry of Education. Eligibility was expanded in 2019 to include children under the age of 3.
Germany	2013	Children are legally entitled to a daycare spot starting at age 1. Childcare fees vary across states (*Länder*), although many regions have moved toward free provision. The reform also led to a substantial expansion in childcare coverage for children under the age of 3.
Iceland	1990s	Children are guaranteed universal access to childcare by age 2. Care is provided primarily through heavily subsidized municipal programs, with enrollment rates exceeding 90%. The expansion occurred through a gradual municipal rollout of childcare services.
New York City	2025	Free universal childcare was a central issue in the 2025 New York City mayoral campaign, with candidate Zohran Mamdani winning in part on a pledge to make childcare free. In January 2026, Mayor Mamdani and Governor Hochul launched the 2-Care program to provide free childcare for two-year-olds citywide, beginning with 2,000 seats in four high-need communities in fall 2026 and expanding universally by year four. The state committed $1.2 billion to fund the first two years of implementation, building on the city's existing universal Pre-K and 3-K programs.

Table IA.1: Countries or Regions that Have Introduced Universal Childcare (Continued)

Jurisdiction	**Introduced**	**Key Features**
Japan	2019	Free early childhood education and care (ECEC) was introduced in 2019 for children aged 3–5, subject to program-specific limits. Subsequent policy discussions and planned reforms have aimed to expand affordability for younger children, including proposals to extend free ECEC to children from infancy onward by 2025, depending on the final program design.
New Zealand	2007	The *20 Hours ECE* scheme introduced 20 hours of free childcare per week for children aged 3–5. Childcare beyond the 20-hour threshold remains subsidized. Services are delivered through a mix of public and private childcare providers.
Norway	2009	Children are legally entitled to childcare starting at age 1. Parental fees are capped at EUR 250 per month, corresponding to approximately 15% of the actual cost of care. Municipalities bear primary responsibility for ensuring the provision of childcare services.
South Korea	2013	Free childcare is available for all children aged 0–5 and is funded by the central government. The policy led to a rapid expansion in access to childcare services, although the quality of care varies across providers and regions.
Sweden	1995	Children are universally entitled to childcare from age 1 until school entry, and municipalities are required to provide a childcare spot upon request. Parental fees are income-based and capped at 3% of household income, with a maximum monthly fee of approximately EUR 150–200.
United States	2021 (not implemented)	The Build Back Better Act included extensive provisions for universal preschool and large federal subsidies for childcare costs. These provisions were ultimately removed from the final legislation, leaving no major new federal childcare program.
Wales (UK)	2018 (ongoing)	Eligible working parents of children aged 3–4 are entitled to up to *30 hours per week* of combined early education and subsidized childcare, available for up to *48 weeks per year*.
Washington, D.C.	2026 (proposed)	In the 2026 D.C. mayoral race, leading candidate Council member Janeese Lewis George proposed universal affordable childcare as a centerpiece of her platform. The plan would expand D.C.'s existing childcare subsidy to eventually cover all families, capping costs at 7% of household income. It also calls for co-locating childcare centers in underutilized school buildings and expanding subsidy eligibility for home-based care providers.

This table summarize the key countries that introduced universal childcare programs similar to that introduced in Quebec in 1997. We exclude reforms that expand existing programs.

Table IA.2: Stacked DID Sample Breakdown

Cohort	CCYears	In number of stacks	Observations in the stack	Observations in DID sample
1989	0	5	778,896	3,894,480
1990	0	5	811,360	4,056,800
1991	0	5	793,968	3,969,840
1992	0	5	750,160	3,750,800
1993	1	1	746,510	746,510
1994	2	1	687,280	687,280
1995	3	1	676,620	676,620
1996	4	1	649,760	649,760
1997	5	1	606,260	606,260
Total				19,038,350

This table breaks down the observation counts of our stacked DID sample by cohort. Each treated cohort from 1993 to 1997 is paired with all never-treated cohorts from 1989 to 1992 to form a stack. As such, each of the never-treated cohorts appears five times in our sample.

Table IA.3: 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
Adj. R-sq.	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.4: Individual-Level Robustness: Alternative Definitions

Panel A: Turnover

	(1) Switch Employer	(2) Switch Employer	(3) Leave Pre-Birth Employer	(4) Leave Pre-Birth Employer
CCYears × PostBirth	0.0014***	0.0013***	0.0022**	0.0022**
	(0.0003)	(0.0003)	(0.0011)	(0.0011)
Stack x Individual FE	X	X	X	X
Stack x Calendar Year FE	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X
Stack x Firm FE		X		
Observations	12,494,360	12,266,730	9,039,340	9,039,340
Adj. R-sq.	0.044	0.113	0.637	0.637

Panel B: Earnings

	(1) Earning Growth	(2) Earning Growth	(3) Earnings/PBEarnings	(4) Earnings/PBEarnings	(5) Promotion	(6) Promotion
CCYears × PostBirth	0.0375***	0.0467***	0.0359***	0.0326***	0.0163***	0.0174***
	(0.0043)	(0.0044)	(0.0135)	(0.0118)	(0.0008)	(0.0009)
Stack x Individual FE	X	X	X	X	X	X
Stack x Calendar Year FE	X	X	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X	X	X
Stack x Firm FE		X		X		X
Observations	9,983,410	9784410	8,400,720	8205750	11,675,590	11,446,170
Adj. R-sq.	0.039	0.078	0.674	0.738	0.040	0.040

This table examines the effect of childcare access on alternative definitions of turnover (Panel A) and earnings (Panel B) of mothers. The specification is based on Equation 1. The sample is conditional on mothers' years of employment. *Switch Employer* 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. *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. *Earnings/PBEarnings* is the ratio between current earnings and pre-birth year earnings. *Promotion* is an indicator equal to one if the individual's current earnings are more than 110% of her earnings in the previous year. Standard errors are reported in parentheses 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: Placebo Sorting Results: Mothers' Sorting on %Dad

	(1)	(2)
	%Dad of Employer	
CCYears x PostBirth	0.0000	0.0000
	(0.0001)	(0.0001)
Stack x Individual FE	X	X
Stack x Calendar Year FE	X	X
Stack x Year-To-Birth FE	X	X
Stack x Industry FE		X
Observations	12,494,360	12,494,360
Adj. R-sq.	0.334	0.399

This table examines the effect of childcare access on mothers' sorting to firms, where the dependent firm-level variable is *%Dad of Employer*, the percentage of fathers with pre-kindergarten children relative to all employees. The specification is based on Equation 1. Standard errors are reported in parentheses 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.6: Heterogeneity in Within-Firm Earnings Rank and Job Tenure

	(1)	(2)	(3)	(4)	(5)	(6)
	Bottom10%Earnings			Ln(Tenure)		
Married	-0.0439***			0.2684***		
	(0.0010)			(0.0024)		
HighEarn		-0.1181***			0.5011***	
		(0.0008)			(0.0024)	
Older			-0.0600***			0.3375***
			(0.0010)			(0.0024)
Observations	263,060	263,060	263,060	263,060	263,060	263,060
Adj. R-sq.	0.007	0.052	0.014	0.045	0.153	0.071

This table shows the estimates from cross-section regressions of pre-birth characteristics on dummy variables for married mothers (columns 1 and 4), high-earning mothers (columns 2 and 5), and older mothers (columns 3 and 6). In columns 1-3, the dependent variable *Bottom10%Earnings* is a dummy variable indicating whether the individual is in the bottom 10% of the earnings distribution at their current employer. In columns 4-6, the dependent variable *Ln(Tenure)* is the natural logarithm of the number of years the individual has worked at their current employer. Standard errors are reported in parentheses and are clustered at the individual level. * indicates the statistical significance of t-test at the 10% level, ** at the 5% level, and *** at the 1% level.

Table IA.7: Comparing High- vs Low-%Mom Firms

	Mean of Low-%Mom Firms	Mean of High-%Mom Firms	p-value of t-test
Ln(Industry Hours)	3.5183	3.4878	0.000
Fraction Part Time	0.217	0.243	0.000
Ln(Earnings Vol)	8.5114	8.5077	0.193
Excess Leverage	-0.0907	-0.152	0.000
Firm AKM	-0.113	-0.1217	0.014
%Female	0.3348	0.5672	0.000
Ln(Avg Earnings)	9.2607	9.0969	0.000
Ln(Avg Female Earnings)	9.0838	8.9669	0.000
Gender Pay Gap	0.276	0.3326	0.000
Ln(Assets)	11.8336	12.8991	0.000

This table presents t-test statistics from comparing the characteristics of firm-years with high and low values of %Mom. *Ln(Industry Hours)* is the natural logarithm of average weekly hours worked at the industry level. *Fraction Part Time* is the fraction of jobs that are part-time. *Ln(Earnings Vol)* is the natural logarithm of firm-level average within-job earnings volatility. *Excess Leverage* is the difference between the liability-to-assets ratio of a given firm and the average liability-to-assets ratio across firms in the same industry. *Firm AKM* is the firm fixed effect as estimated from an AKM model as described in Section IA.3 of the Internet Appendix. *% Female* is the percent of female employees in a given firm-year. *Ln(Average Earnings)* (*Ln(Average Female Earnings)*) is the natural logarithm of the average earnings across (female) workers in a given firm-year. *Gender Pay Gap* is the natural logarithm of the ratio between the average male earnings and average female earnings within a firm-year. *Ln(Assets)* is the natural logarithm of total firm assets.

Table IA.8: Pairwise Cross-Sectional Regressions of %Mom on Firm Characteristics

	(1) %Mom	(2) %Mom	(3) %Mom
Ln(Industry Hours)	-0.0982***		-0.0676***
	(0.0015)		(0.0015)
Fraction Part Time	0.0731***		0.0446***
	(0.0013)		(0.0013)
Ln(Earnings Vol)	-0.0038***	-0.0001	-0.0005*
	(0.0003)	(0.0003)	(0.0003)
Excess Leverage	-0.0016***	-0.0015***	-0.0017***
	(0.0003)	(0.0003)	(0.0003)
Firm AKM	-0.0016***	-0.0010***	-0.0015***
	(0.0003)	(0.0003)	(0.0003)
%Female	0.0935***	0.0911***	0.0916***
	(0.0006)	(0.0007)	(0.0006)
Ln(Avg Earnings)	-0.0089***	-0.0076***	-0.0023***
	(0.0002)	(0.0002)	(0.0002)
Ln(Avg Female Earnings)	-0.0067***	-0.0070***	-0.0010***
	(0.0002)	(0.0003)	(0.0003)
Gender Pay Gap	0.0065***	0.0064***	0.0074***
	(0.0002)	(0.0002)	(0.0002)
Ln(Assets)	0.0003***	0.0008***	0.0000
	(0.0001)	(0.0001)	(0.0001)
Year FE	X		X
Industry-Year FE		X	
Control for avg worker age			X

This table shows the results from pairwise correlation regression of *%Mom* on each of the other firm characteristics. Column 1 includes year fixed effects, column 2 includes NAICS-4digit-year fixed effects, and column 3 controls for average worker age in addition to year fixed effects. The dependent and independent variables are the same as those reported in Table IA.7. Robust standard errors are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table IA.9: Sorting on Residualized %Mom

	(1) %Mom residualized on all other sorting outcomes
CCYears x PostBirth	0.0002
	(0.0002)
Stack x Individual FE	X
Stack x Calendar Year FE	X
Stack x Year-To-Birth FE	X
Observations	8,702,720
Adj. R-sq.	0.345

This table examines the effect of childcare access on *%Mom of Employer* residualized on the other sorting outcomes examined in Table 5—i.e., *Ln(Industry Hours)*, *Fraction Part Time*, *Ln(Earnings Vol)*, *Firm AKM*, *Excess Leverage* and *ln(Median Earnings)*—where *%Mom of Employer* is the percent of mothers with pre-kindergarten children relative to all employees. The specification is based on Equation 1. Standard errors are reported in parentheses 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: Fertility

	(1) Number of Kids within 5Yrs	(2) Number of Kids within 10Yrs	(3) Number of Kids within 5Yrs	(4) Number of Kids within 10Yrs
CCYears	-0.0248***	-0.0170***	-0.0201***	-0.0118***
	(0.0009)	(0.0008)	(0.0009)	(0.0008)
Age-bin FE			X	X
Observations	357,620	357,620	357,620	357,620

This table examines the effect of childcare access on mothers' subsequent fertility after the first child. The specification estimates a Poisson pseudo maximum likelihood (PPML) regression on a cross-section of mothers who recently had their first child. The outcome *Number of Kids within 5Yrs* (*Number of Kids within 10Yrs*) is the total number of subsequent children a mother had 5 years (10 years) after the birth of their first child. Columns 3 and 4 present estimates from regressions that include age-bin fixed effects, where each bin groups parents based on their age, rounded to the nearest 5-year interval. Standard errors are reported in parentheses 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.11: Individual-level Robustness: Dropping Part-Time or Public Sector Employees

Panel A: Drop part-time employees

	Employment	Quit	%Mom	Ln(Earnings)
CCYears x PostBirth	0.0051***	0.0015***	-0.0006***	0.0128***
	(0.0007)	(0.0003)	(0.0002)	(0.0019)
Stack x Individual FE	X	X	X	X
Stack x Calendar Year FE	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X
Observations	17,894,070	11,340,540	11,340,540	10,641,760
Adj. R-sq.	0.549	0.08	0.32	0.622

Panel B: Drop public sector employees

	Employment	Quit	%Mom	Ln(Earnings)
CCYears × PostBirth	0.0052***	0.0016***	-0.0006**	0.0143***
	(0.0007)	(0.0003)	(0.0002)	(0.0026)
Stack x Individual FE	X	X	X	X
Stack x Calendar Year FE	X	X	X	X
Stack x Year-To-Birth FE	X	X	X	X
Observations	18,100,610	11,557,130	11,557,130	10,791,410
Adj. R-sq.	0.525	0.077	0.311	0.546

This table shows the robustness of our individual-level results to dropping part-time employees (Panel A) or dropping public-sector employees (Panel B). The variable definitions used in the analysis in both panels are the same as those reported in Tables 3, 4, and 6. Part-time employees are those in the bottom 10% of wage distribution within a firm. Public-sector employees are classified as those working in an industry with a 2-digit NAICS of 91. 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.12: Triple-Difference Analysis: Female vs Male

	(1) Employed	(2) Quit	(3) %Mom	(4) Ln(Earnings)
CCYears x PostBirth x Mother	0.0030***	0.0009**	-0.0005**	0.0082**
	(0.0009)	(0.0004)	(0.0002)	(0.0032)
Stack x Individual FE	X	X	X	X
Stack x Sex x Calendar Year FE	X	X	X	X
Stack x Sex x Year-To-Birth FE	X	X	X	X
Stack x Cohort x Year-To-Birth FE	X	X	X	X
Observations	38,247,340	27,079,560	27,079,560	25,325,480
Adj. R-sq.	0.542	0.071	0.369	0.597

This table reports the triple-difference analysis results comparing fathers and mothers for our three main outcomes: *Employed*, an indicator equal to one if the individual is employed in that year with a minimum $1,000 CAD ($720 USD equivalent) income, *Quit*, an indicator equal to one if the individual voluntarily leaves the employer from the previous year as identified from the Record of Employment (ROE), *%Mom*, an individual's employer's percentage of mothers with pre-kindergarten children among all employees, and *Ln(Earnings)*, the logarithm of total T4 earnings in a year. The specification is based on Equation 1 with an additional triple interaction term involving *Mother*, an indicator for mothers, and the inclusion of *Stack* $\times$ *Cohort* $\times$ *Year To Birth* fixed effects. Standard errors are reported in parentheses 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.13: Stack-by-Stack Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
			Employed			
Cohort1993 x PostBirth	0.0118***					
	(0.0023)					
Cohort1994 x PostBirth		0.0155***				
		(0.0026)				
Cohort1995 x PostBirth			0.0191***			
			(0.0027)			
Cohort1996 x PostBirth				0.0196***		
				(0.0028)		
Cohort1997 x PostBirth					0.0199***	
					(0.0030)	
Cohort1998 x PostBirth						0.0218***
						(0.0030)
Treated Cohort	1993	1994	1995	1996	1997	1998
Individual FE	X	X	X	X	X	X
Calendar Year FE	X	X	X	X	X	X
Year-To-Birth FE	X	X	X	X	X	X
Observations	3,880,900	3,821,660	3,811,010	3,784,140	3,740,640	3,722,850
Adj. R-sq.	0.522	0.521	0.52	0.52	0.52	0.521

This table reports the results from our stack-by-stack estimations. In each column, we include only one treated cohort and use all never-treated cohorts (1989-1992) as the control group. *Employed* is an indicator equal to one if the individual is employed in that year with a minimum $1,000 CAD ($720 USD equivalent) income. *CohortX* $\times$ *PostBirth* is an interaction term between a dummy variable indicating an individual having their first child in year X and a dummy variable indicating years after childbirth. Standard errors are reported in parentheses 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.14: Employment Effect: Comparing Quebec vs Other Provinces

	(1)	(2)	(3)
Dependent Variable:		Employed	
Sample:	Cross-section	Cross-section	Panel
Eligible	0.0599***	0.0610***	0.0326***
	(0.0026)	(0.0023)	(0.0008)
Province FE	X	X	X
Mother controls		X	
Mother FE			X
Cal. Yr FE			X
Observations	793,260	793,260	3,966,305
Adj. R-sq.	0.009	0.186	0.502

This table uses alternative specifications to test the effect of childcare subsidies on mothers' employment post childbirth. The sample focuses on married mothers who gave birth between 1989 and 1999, during the years when their children were age 0-4. *Eligible* is an indicator for the years a child is eligible for the childcare subsidy, which depends on both the child's age and the calendar year. *Employed* in an indicator for the mother being employed and earning at least $1,000 CAD ($720 USD equivalent) in a year. In columns 1-2, the sample is a cross-section of mothers collapsed over the 5 years after the birth of the first child. In these two columns, both *Eligible* and *Employed* are ratios averaged over the five-year post-birth period. In column 3, the sample is the uncollapsed panel of individual-years for the 5-year post-birth period. Mother controls include a pre-birth married indicator, Ln(Earnings), and pre-birth mother's age. Standard errors are reported in parentheses 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.15: Employment: Comparing Adjacent Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
			Employed			
Cohort1993 x Postbirth	0.0243***					
	(0.0022)					
Cohort1994 x Postbirth		0.0176***				
		(0.0022)				
Cohort1995 x Postbirth			0.0231***			
			(0.0023)			
Cohort1996 x Postbirth				0.0179***		
				(0.0023)		
Cohort1997 x Postbirth					0.0127***	
					(0.0024)	
Cohort1998 x Postbirth						0.0109***
						(0.0025)
Identified Effect	E + S	A + S	E + S	A + S	E + S	S
Benchmark Cohort	1992	1993	1994	1995	1996	1997
Individual FE	X	X	X	X	X	X
Year-To-Birth FE	X	X	X	X	X	X
Observations	841,880	806,510	767,200	746,090	706,510	672,030
Adj. R-sq.	0.580	0.584	0.583	0.581	0.577	0.573

This table parametrically compares the employment response to the reform between two adjacent cohorts shown in Figure 1, using a fixed event window (from three years before birth to five years after birth). *Employed* is an indicator equal to one if the individual is employed in that year with a minimum $1,000 CAD ($720 USD equivalent) income. *CohortX* $\times$ *PostbBirth* is an interaction term between a dummy variable indicating an individual having their first child in year X and a dummy variable indicating years after childbirth. In each column, the control cohort gave birth one year before the treated cohort. Hence, the interaction term estimates the differential response of the current cohort relative to the previous cohort. These comparisons between adjacent cohorts are then used to decompose the three margins of response: 1) anticipatory effect due to expectation of price subsidy ("A"), 2) eligibility effect due to actual price subsidy ("E"), and 3) childcare supply effect due to greater supply of childcare spots regardless of price ("S"). The row "Identified Effect" indicates which combination of the three margins each column identifies. Standard errors are reported in parentheses 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.16: Heterogeneity by Local Childcare Supply

	(1) Employed	(2) Ln(Earnings)
CCYears x PostBirth x Local Childcare Supply	-0.0051***	-0.0214***
	(0.0007)	(0.0024)
CCYears x PostBirth	0.0068***	0.0293***
	(0.0008)	(0.0029)
PostBirth x Local Childcare Supply	0.0088***	-0.0071
	(0.0017)	(0.0060)
Stack x Individual FE	X	X
Stack x Calendar Year FE	X	X
Stack x Year-To-Birth FE	X	X
Observations	17,372,220	11,269,580
Adj. R-sq.	0.493	0.558

This table shows the heterogeneity of our baseline employment and earnings results with respect to local childcare supply. *Employed* is an indicator equal to one if the individual is employed in that year with a minimum $1,000 CAD ($720 USD equivalent) income. *Ln(Earnings)* is the logarithm of total T4 earnings in a year. The additional interaction term *Local Childcare Supply* indicates an above-median fraction of childcare workers among all workers in a Census Metropolitan Area (CMA) in 1996, which proxies for pre-reform local childcare supply. Standard errors are reported in parentheses 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.17: Alternative Thresholds for Low%Mom

Panel A: Low%Mom is <66.7 pctile

	(1) Ln(Sales)	(2) Ln(Sales/ Employment)	(3) ROA	(4) %Mom	(5) %Young Female	(6) %Female	(7) Ln(Young Females)
Low%Mom_Pre x Post97	0.0660***	0.0375***	0.0031	0.0459***	0.0245***	0.0161***	0.0743***
	(0.0090)	(0.0091)	(0.0046)	(0.0008)	(0.0013)	(0.0012)	(0.0053)
Firm FE	X	X	X	X	X	X	X
Industry x Year FE	X	X	X	X	X	X	X
Observations	351,220	347,600	339,140	342,550	353,520	353,520	239,300
Adj. R-sq.	0.774	0.675	0.25	0.497	0.804	0.875	0.859

Panel B: Low%Mom is <75 pctile

	(1) Ln(Sales)	(2) Ln(Sales/ Employment)	(3) ROA	(4) %Mom	(5) %Young Female	(6) %Female	(7) Ln(Young Females)
Low%Mom_Pre x Post97	0.0488***	0.0321***	0.004	0.0553***	0.0281***	0.0181***	0.0617***
	(0.0097)	(0.0098)	(0.0050)	(0.0010)	(0.0014)	(0.0013)	(0.0055)
Firm FE	X	X	X	X	X	X	X
Industry x Year FE	X	X	X	X	X	X	X
Observations	351,220	347,600	339,140	342,550	353,520	353,520	239,300
Adj. R-sq.	0.774	0.675	0.25	0.501	0.804	0.875	0.859

Panel C: Low%Mom is <80 pctile

	(1) Ln(Sales)	(2) Ln(Sales/ Employment)	(3) ROA	(4) %Mom	(5) %Young Female	(6) %Female	(7) Ln(Young Females)
Low%Mom_Pre x Post97	0.0354***	0.0279***	0.0081	0.0628***	0.0312***	0.0204***	0.0516***
	(0.0102)	(0.0103)	(0.0054)	(0.0011)	(0.0016)	(0.0014)	(0.0058)
Firm FE	X	X	X	X	X	X	X
Industry x Year FE	X	X	X	X	X	X	X
Observations	351,220	347,600	339,140	342,550	353,520	353,520	239,300
Adj. R-sq.	0.774	0.675	0.25	0.503	0.804	0.875	0.859

This table examines the impact of the reform on Quebec firms' financial performance and employment outcomes using alternative thresholds to define *Low%Mom_Pre* firms. *Low%Mom_Pre* is an indicator equal to one if a firm's average percent of mothers of pre-kindergarten children among all employees in the three years before the reform is below 66.7th percentile in Panel A, below 75th percentile in Panel B, and below 80th percentile in Panel C. *Ln(Sales)* is the natural logarithm of sales. *Ln(Sales/Employment)* is the natural logarithm of labor productivity, i.e., the log of sales divided by employment, *ROA* is net income divided by lagged total assets. *%Mom* is the percent of mothers with pre-kindergarten children in a year. *%Female* (*%Young Female*) is the percent of (young—i.e., age<40) female employees, and *Ln(Young Females)* is the log number of young (i.e., age<40) female employees. The sample period is 1994 to 2000. The specification follows Equation 3. All columns include firm fixed effects and NAICS 4-digit-year fixed effects. Standard errors are reported in parentheses 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.18: Alternative Firm-Level Treatments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	%Young Female				Ln(Young Females)			
High Industry Hours x Post97	0.0052***				0.0256***			
	(0.0009)				(0.0051)			
High Earnings Vol x Post97		0.0047***				0.0452***		
		(0.0013)				(0.0057)		
High Firm AKM x Post97			0.0040***				0.0452***	
			(0.0012)				(0.0057)	
Low Projected %Mom x Post97				0.0030***				0.0255***
				(0.0009)				(0.0048)
Firm FE	X	X	X	X	X	X	X	X
Year FE	X			X	X			X
Industry x Year FE		X	X			X	X	
Observations	353,430	353,210	353,250	352,840	239,290	239,090	239,100	238,880
Adj. R-sq.	0.803	0.804	0.804	0.803	0.857	0.859	0.859	0.857

This table examines the impact of the reform on female labor reallocation among Quebec firms using alternative sorting variables: *High Industry Hours*, *High Earnings Vol*, and *High Firm AKM* are dummy variables that indicate whether a firm is above the sample median in *Ln(Industry Hours)*, *Ln(Earnings Vol)*, and *Firm AKM*, respectively, as defined in Table 5. *%Young Female* is the percent of young (i.e., age<40) female employees. *Ln(Young Females)* is the log number of young (i.e., age<40) female employees. Columns 1-3 and 5-7 focus on heterogeneity in industry-level work hours, firm-level earnings volatility, and firm-specific productivity (using estimates from the AKM model described in Appendix IA.3), respectively. Columns 4 and 8 focus on *%Mom* projected on the previous three variables, which captures the joint effects of all three firm characteristics. Specifically, we regress *%Mom* on firm-level earnings volatility, firm-specific wages (extracted from AKM), and industry-level weekly work hours and extract the fitted value. We then define *Low Projected %Mom* as an indicator for the fitted value being below the median. The sample period is 1994 to 2000. The specification follows Equation 3. All columns include firm fixed effects and NAICS 4-digit-year fixed effects, except columns 1, 4, 5, and 8, where the new interaction variable is at the industry level. Standard errors are reported in parentheses 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.19: 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	14,560	14,560
Base earnings ($)	$25,749	$25,749
Base employment	338,595	338,595
Δ earnings ($)	$2,137	$2,137
Δ personal income tax revenue ($m)	$165	$377
Marginal corporate tax rate	9%	30%
Δ corporate income ($)	$3,600	$3,600
No. of QC firms	128,197	128,197
Δ corporate income tax revenue ($m)	$42	$138
Δ total tax revenue/year ($m)	$206.96	$515.71
	NPV Analysis:	
PV (Δ total tax revenue over 10 years) ($m)	$1,453.60	$3,622.13
PV (program cost over 10 years) ($m)	$5,818.64	$5,818.64
NPV ($m)	-$4,365.04	-$2,196.51

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. The fiscal costs are obtained from realized annual program spending from 1997 to 2006, reported in Table 1 of Lefebvre and Merrigan (2008). The fiscal benefits arise from the incremental tax revenue generated by the program from higher personal and 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 fiscal NPV analysis of the program based on a 10-year horizon. We use a 7% discount rate, which is recommended by the U.S. Office of Management and Budget (OMB) for cost-benefit analysis for all federal agencies.

IA.1 Appendix: Variable Definitions

Variable Name	Definition
CCYears	The number of years post reform before a child turns 5. It is equal to $max(0, min(5, T - 1992))$, 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) and earned at least $1,000 CAD per year (based on T4 filings) 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.
Quit	An indicator equal to one if an individual voluntarily leaves his/her employer, as identified from Record of Employment (ROE).
Ln(Earnings)	The natural logarithm of earnings, based on reported earnings on T4 filings.
Ln(Husband Pay/Wife Pay)	The natural logarithm of the ratio of the male spouse's earnings to the female spouse's earnings within a family, where earnings are measured using T4-reported income.
%Mom of Employer	The percentage of an employer's workforce consisting of female employees who have at least one child under age 5.
Employed in High-%Mom (Low-%Mom) Firm	An indicator equal to one if a mother works at a high-%mom firm (low-%mom firm), where high-%mom (low-%mom) is based on being above (below) the sample median for %Mom of Employer.
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 based on being above (below) the sample median for %Mom of 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
High Earnings	An indicator equal to one if the individual had above-median earnings in the year before childbirth.
High AKM	An indicator equal to one if an individualâs ex-ante ability exceeds the sample median, where ex-ante ability is measured by the individual fixed effect from an AKM model estimated using pre-reform job histories.
Ln(Industry Hours)	The natural logarithm of average weekly hours worked at the 2-digit NAICS industry level in Quebec during the 1990s, taken from Canadian IPUMS data.
Fraction Part Time	The fraction of jobs that are part-time within a 2-digit NAICS industry in Quebec during the 1990s, based on Canadian IPUMS data.

Variable Name	Definition
Ln(Earnings Vol)	The natural logarithm of firm-level average within-job earnings volatility. For each worker, within-job earnings volatility is defined as the standard deviation of earnings within an employer; this measure is then averaged across all workers in the firm.
Excess Leverage	The difference between the liability-to-assets ratio of a given firm and the average liability-to-assets ratio across firms in the same industry (4-digit NAICS).
Firm AKM	The firm fixed effect as estimated from an AKM model as described in Section IA.3 of the Internet Appendix.
Ln(Median Earnings)	The natural logarithm of the median worker's earnings within a firm in a given year.
%Mom_Pre	The time-series average of %mom for a given firm in the four years (1994-1996) before the 1997 Quebec childcare reform.
Low%Mom_Pre	An indicator equal to one if %Mom_Pre is below the sample median.
Post97	An indicator equal to one for years after 1997.
Ln(Sales)	The natural logarithm of sales.
ROA	Pre-tax income divided by total lagged assets.
Ln(Sales/Employment)	The natural logarithm of sales divided by employment.
%Female	The percent of female employees in a given firm-year.
%Young Female	The percent of female employees under the age of 40 in a given firm-year.
Ln(Employment)	The natural logarithm of the number of workers at a given firm-year.
Ln(Female)	The natural logarithm of the number of female workers at a given firm-year.
Ln(Young Female)	The natural logarithm of the number of female workers under the age of 40 at a given firm-year.
Ln(Male)	The natural logarithm of the number of male workers at a given firm-year.
Female Quit Rate (Male Quit Rate)	The number of female (male) workers in a given firm who voluntarily leaves his/her employer in a given year.
Avg AKM of Female Joiners (Male Joiners)	The average ex-ante worker productivity of all female (male) joiners in a firm. Worker-level productivity is estimated from AKM regressions using pre-reform job histories (see Appendix IA.3 for details).
Avg AKM of Female Stayers (Male Stayers)	The average ex-ante worker productivity of all female (male) stayers in a firm. Worker-level productivity is estimated from AKM regressions using pre-reform job histories (see Appendix IA.3 for details).
Avg Ln(Earnings)	The average of the logarithm of earnings across all workers in a given firm in a given year.
Avg Ln(Female Earnings)	The average of the logarithm of earnings across all female workers in a given firm in a given year.
Avg Ln(Male Earnings)	The average of the logarithm of earnings across all male workers in a given firm in a given year.

IA.2 Appendix: Decomposing Margins of Response

The reform could affect mother's employment through three possible margins: 1) anticipation of subsidies (i.e., intertemporal smoothing of future subsidies), 2) eligibility for the actual subsidy, and 3) increased supply of childcare spots. Comparing different pairs of adjacent cohorts provide us rich variation that allows us to isolate and back out each of the three margins.

First, comparing the 1997 and 1998 cohorts allows us to isolate the supply effect, since both cohorts face the same number of anticipatory years (2 years) and the same number of eligible years (3 years), while the 1998 cohort enjoys an increased supply of childcare spots as the government expands supply over time (Table 2 and Figure IA.1). As shown in Figure IA.2 Panel C, we see a larger employment response by the 1998 cohort than the 1997 cohort, consistent with the effect of greater childcare spot supply.

More formally, we can quantify the supply effect in Table IA.15, where we parametrically test the difference between the two adjacent cohorts in a simple DID in which the latter cohort is the "treated cohort". Column 6 of Table IA.15 shows a significant 1.09 pp difference in employment, which can be attributed to a one-year supply effect.

Next, we can compare the 1993 and 1994 cohorts to isolate the combined effect of anticipation and increased supply. Both cohorts have the same number of eligible years, but the 1994 cohort had one more anticipatory year and an additional year of increase in childcare spot supply. We again observe a stronger response by the 1994 cohort than the 1993 cohort (Figure IA.2 Panel A). A similar comparison can be drawn between the 1995 and 1996 cohorts. Averaging the differences across these two pairs (columns 2 and 4 of Table IA.15) yields a combined anticipation and supply effect of 1.78 pp.

Lastly, comparing the 1994 and 1995 cohorts helps us isolate the combined effect of subsidy eligibility and increased supply, since both cohorts face the same number of anticipatory years, while the latter cohort faced one more eligible year and an additional year of supply increase. We observe a stronger response by the 1995 cohort relative to the 1994 cohort (Figure IA.2 Panel B). A similar comparison can be drawn between the 1992 & 1993 and the 1996 & 1997 pairs. Averaging the differences across these three pairs (columns 1, 3, 5 of Table IA.15) yields a combined eligibility and supply effect of 2.00 pp.

Given that the supply increase is almost linear over the period of 1997 to 2004 (see Figure IA.1), we can assume a constant supply effect across cohorts. This allows us to triangulate between the effects estimated above to back out each of the three margins. Let E, A, and S denote eligibility,

anticipation, and supply effects, respectively. The comparisons above give us:

$$S = 1.09 \tag{1}$$

$$A + S = 1.78 \tag{2}$$

$$E + S = 2.00 \tag{3}$$

This yields: S=1.09, A=0.69, and E=0.91. As such, quantitatively, the supply effect is the largest, followed by the eligibility effect (20% smaller) and then the anticipatory effect (i.e., intertemporal smoothing of future subsidies) (58% smaller). The majority of the anticipatory response seems to be driven by increases in the supply of childcare spots rather than intertemporal smoothing of expected subsidies.

Finally, we provide one additional piece of evidence that childcare supply matters for our results. In Table IA.16, we conduct a cross-sectional heterogeneity test, where we interact our main treatment with the local supply of childcare workers pre-reform. We follow Rutigliano (2025) and measure local childcare supply as the density of childcare workers in the individuals' location, i.e., the fraction of childcare workers among all workers in a Census Metropolitan Area (CMA) in 1996.[31] We define childcare workers as individuals working in an industry with four-digit NAICS code 6244 ("Child day-care services"). We find a negative interaction effect: individuals living in areas with higher pre-reform local childcare supply experienced a smaller employment and earnings response to the reform. This is consistent with these individuals experiencing a smaller relaxation of childcare supply constraints.

[31] A CMA is defined as one or more adjacent municipalities with a population of at least 100,000 people.

IA.3 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:

$$ln(Earn)_{ijt} = \alpha_i + \lambda_j + \gamma_t + \beta_1 age_{it} + \beta_2 age_{it}^2 + \beta_3 age_{it}^3 + \epsilon_{it} \tag{4}$$

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 4, we construct time-varying firm productivity measures by averaging α_i for all individuals working for a given firm in a given year:

$$Avg\ Ability_{jt} = \frac{\Sigma_{i \in I(j,t)} \alpha_i}{|I(j,t)|} \tag{5}$$

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