

# Does Career Risk Deter Potential Entrepreneurs?

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Do potential entrepreneurs remain in wage employment because of concerns that they will face worse job opportunities should their entrepreneurial ventures fail? Using a Canadian reform that extends job-protected leave to one year for women giving birth after a cutoff date, we study whether the option to return to a previous job increases entrepreneurship. A regression discontinuity design reveals that a longer job-protected leave increases entrepreneurship by 1.9 percentage points. These entrepreneurs start incorporated businesses that hire employees, in industries in which experimentation before entry has low costs and high benefits. (*JEL* L26, J13, J16, J65, J88, H50)

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Entrepreneurship has long been thought to play a critical role in innovation, job creation, and economic growth (Schumpeter 1911). A large body of evidence

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supports this view (e.g., King and Levine 1993; Levine 1997; Beck, Levine, and Loayza 2000; Levine, Loayza, and Beck 2000; Guiso, Sapienza, and Zingales 2004). Yet only a small fraction of the population undertakes entrepreneurial endeavors. For example, in the United States, only 6.6% of the labor force is self-employed (World Bank 2015).

While regulation and capital access are well-known impediments to starting a business,<sup>1</sup> perhaps the most fundamental reason people might avoid setting out on their own is the risk involved in doing so. Entrepreneurs must invest time and capital in businesses that may ultimately fail. Perhaps equally important, entrepreneurs may also face career risk. Indeed, a growing body of evidence suggests that individuals who enter entrepreneurship and then reenter wage employment, find themselves on a worse career trajectory.<sup>2</sup> This implicitly lowers the returns to entrepreneurship, as the value of the real option to return to wage employment is an important component of those returns (Dillon and Stanton 2016; Manso 2016).

This idea of career risk motivates the widely held belief that entrepreneurship increases during recessions. Workers who have already lost their job face a lower opportunity cost of trying to start a new business, though opinions vary as to whether entrepreneurship increased during the Great Recession (Fairlie 2010; Shane 2011). In this paper, we use a natural experiment to investigate the relationship between entrepreneurship and career risk. In particular, we examine whether granting employees the ability to take prolonged leaves of absence, with guaranteed options to return to their jobs, increases entry into entrepreneurship.

While employees do not often have the ability to take job-protected leaves for the purpose of starting a business, governments often require that such leaves be permitted surrounding the birth of a child. Such leaves, if sufficiently long, could in principle be used to work on a business idea, while retaining the option to return to one's previous job. Essentially, job-protected leaves may reduce the frictions to entrepreneurial experimentation (Manso 2011; Kerr, Nanda, and Rhodes-Kropf 2014). We think of this early-stage experimentation very broadly: "working on a business idea" includes any part of the process of transitioning into entrepreneurship that would not be possible while simultaneously working full-time in wage employment. In particular, it could include serious brainstorming and research or preliminary tests of an idea's viability.

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<sup>1</sup> See, for example, Evans and Jovanovic (1989), Holtz-Eakin, Joulfaian, and Rosen (1994a,b), Hurst and Lusardi (2004), Bertrand, Schroar, and Thesmar (2007), Mel, McKenzie and Woodruff (2008), Kerr and Nanda (2009a), Adelino, Schoar, and Severino (2015), Schmalz, Sraer, and Thesmar (2017); Mullainathan and Schnabl (2010), Bruhn (2011), Branstetter et al. (2014).

<sup>2</sup> The literature generally finds that the effect of past entrepreneurship experience on wages is smaller than the effect of past wage employment experience (e.g., Ferber and Waldfogel 1998; Williams 2002; Bruce and Schuetze 2004; Kaiser and Malchow-Møller 2011; Baptista, Lima, and Torres 2012). Many argue that the stigma of entrepreneurial failure discourages entry and explains much of the variation in entrepreneurship across regions (e.g., Gromb and Scharfstein 2002; Landier 2006; Nanda and Rhodes-Kropf 2013a). In addition to the potential wage penalty, search costs and other frictions are likely involved in leaving wage employment and reentering.

We focus on a reform to Canadian maternity leave laws that took place in the year 2000. The reform extended job-protected leave entitlements to 12 months, approximately a 5.5-month increase. In contrast, the U.S. mandates only 3 months of leave in total. Given that U.S. law expects employees to return to work after 3 months, workers in Canada may be able to use their substantial additional time to work on a business idea without career risk, even with a new child in the household. Although maternity leave is not the first thing that comes to mind when considering sparks for entrepreneurship, our setting provides stark variation in guaranteed outside options.

Anecdotal evidence suggests that entry into entrepreneurship among Canadian women did increase following the reform. According to the *Vancouver Sun*, “a growing number [of mothers] are using their maternity leave—now a full year in Canada—to either plan or start a new professional direction in life...longer maternity leaves are making it easier for women to try their hand at starting a business” (Morton 2006). Danielle Botterell, author of the Canadian book *Moms Inc.*, said in an interview with the *Globe and Mail*, “We think the trend of mompreneurship, particularly in this country, really took off when the government extended maternity leave to a year” (Pearce 2011). According to the *Financial Post*, a Canadian business newspaper, “there is a new breed of female entrepreneurs using their maternity leaves to incubate real businesses” (Mazurkewich 2010). One entrepreneur interviewed used her maternity leave to start amassing clients, explaining that “my maternity leave was my security blanket.” In her interpretation, job-protected leave time allowed her to explore Entrepreneurship, while minimizing the risk to her career (Karol 2012).

Our empirical strategy exploits the fact that eligibility for extended maternity leave was tied to the precise date a woman gave birth. In particular, mothers who gave birth on or after December 31, 2000, were eligible for the extended job-protected leave. Those who gave birth even one day before were not. This type of reform lends itself to examination with a regression discontinuity design. In particular, we examine whether mothers who gave birth just after the cutoff date are discontinuously more likely to be entrepreneurs as of the next census 5 years later than those who gave birth just before the cutoff date.

To ensure that our estimates are not confounded by contemporaneous macroeconomic shocks or other policies that change discontinuously at year’s end, our main empirical strategy compares the regression discontinuity estimate for mothers, who availed themselves of this extended leave, with that for fathers, who did not. That is, we implement a “difference-in-discontinuities” design (e.g., Grembi, Nannicini, and Troiano 2016; Lalive 2008; Leonardi and Pica 2013), which uses the time trend of fathers to control for any other factors influencing entrepreneurship rates across the cutoff date.

Given the limitations on the extent to which the timing of births can be controlled, “gaming” around the cutoff date is likely to be limited. Consistent with the difficulty of gaming, we find no evidence of a discontinuous jump in

the birth rate after the cutoff date. Moreover, those who gave birth just before and after the cutoff have similar ages, education levels, and ethnicities. These findings suggest that no obvious confounds would invalidate our strategy. The ability to control for unobservable trends using fathers further strengthens the validity of our difference-in-discontinuities approach.

Our main outcome variable is self-employment status as of the next census 5 years later. We find that the increase in job-protected leave entitlements leads to approximately a 1.9-percentage-point increase in entrepreneurship rates among mothers relative to fathers. This baseline result is robust to adding various controls, examining different windows around the cutoff date, using different methods of fitting the pre- and post-policy time trends, and defining entrepreneurship in different ways. As one would expect, we find larger effects in provinces that had larger increases in job-protected leave. Moreover, we find no effects around various false, placebo cutoff dates. Finally, we also find similar effects at shorter time horizons using a separate data set.

Several pieces of evidence suggest that the incremental entrepreneurs we observe are engaging in meaningful businesses. First, we measure entrepreneurship continuing 5 years after the reform. If the reform only increased transitory entrepreneurship, we would not expect to see long-run effects because the marginal businesses spurred by the reform would cease operating within that time frame. We also find that the reform leads to an increase in entrepreneurs that incorporate their firms, hire paid employees, work above the median number of hours, and earn above the median income.

We consider two mechanisms that could generate these results. First, individuals with entrepreneurial ideas and preferences could use longer leaves to explore their ideas without exposure to career risk. This would be a *direct* mechanism through which job-protected leave availability increases entrepreneurship. Second, individuals who do not have entrepreneurial ideas or preferences may instead use longer leaves to pursue nonentrepreneurial objectives (e.g., parental objectives) without exposure to career risk. However, such individuals may then develop entrepreneurial ideas and/or preferences as a result of their additional time away from wage employment. This would be an *indirect* mechanism through which job-protected leave availability increases entrepreneurship. These two mechanisms are closely related, with the main difference between them having to do with individuals' initial motivations for making use of additional job-protected leave time. Moreover, both would represent a causal effect of reduced career risk on entrepreneurship.

We conduct a number of tests to distinguish empirically between the direct and indirect mechanisms and find greater support for the direct mechanism. Under the indirect mechanism, job-protected leave time should lead to other types of career switches, not just switches into entrepreneurship. But among those who remained in wage employment after childbirth, we find that the ones who had access to extended leave were no more likely to switch from their prechildbirth industry, to experience a large absolute change in earnings

from prechildbirth levels, or to move from their prechildbirth location. Under the direct mechanism, one would expect stronger effects when exploring an idea prior to fully committing has low costs or high benefits (Kerr, Nanda, and Rhodes-Kropf 2014; Ewens, Nanda, and Rhodes-Kropf 2018). Consistent with this, we find that the new businesses tend to be in industries with low startup capital requirements (i.e., low cost of experimentation) or high failure rates (i.e., high real option value of experimentation). Under the direct mechanism, one would also expect stronger effects among those facing greater career risk associated with entrepreneurial failure. Consistent with this idea, we find that the effect of job-protected leave on entrepreneurship is concentrated among those who had been wage-employed in industries where the wage penalty associated with leaving for entrepreneurship and then returning is high. We also find the effect is stronger among those with a college degree. Together, these results point strongly toward the direct mechanism and away from the indirect mechanism.<sup>3</sup>

We think it is plausible that our results may generalize beyond the particular group we study in this paper. For example, one could imagine that if large technology companies in Silicon Valley give their employees a similar ability to take long job-protected leaves—not associated with having children—such a policy might lead to the creation of technology startups.<sup>4</sup> Sweden, in fact, requires that workers be allowed to take up to 6 months of job-protected leave if they want to try to start a business. Our results are also related to a commonly held view that part of the value of business school for students is to provide them with time away from wage employment that is not penalized by the labor market. Much like job-protected leave, such time may allow for experimentation with entrepreneurship.<sup>5</sup>

Our paper relates to a growing literature that views entrepreneurship as a series of experiments (see Kerr, Nanda, and Rhodes-Kropf, 2014, for an overview). While many entrepreneurial projects may be negative net present value (NPV) in a static sense, entrepreneurs can engage in cheap experiments that reveal information about a project's prospects. Conditional on that information being favorable, the project may become positive NPV; thus, there is value in the real option to continue. In related work,

<sup>3</sup> Separate from the mechanism, it is possible that new mothers have a stronger desire for job flexibility than other workers and this generates a preference for entrepreneurship. However, our empirical strategy compares new mothers who were (quasi-randomly) eligible for a longer period of job-protected leave to new mothers who were not. So the desire for job flexibility does not confound our analysis. Rather, job flexibility represents an underlying reason that entrepreneurship may be desirable. Past work has suggested that such nonpecuniary benefits may be an important motivation for many potential entrepreneurs, not only new mothers (Hurst and Pugsley 2011).

<sup>4</sup> Of course, such a policy would also have significant costs associated with it. Conducting a full welfare analysis is beyond the scope of this paper. Our purpose is not to advocate for the use of job-protected leave as a policy tool to promote entrepreneurship. Rather, we use job-protected leaves to shed light on the extent to which career risk inhibits entrepreneurship.

<sup>5</sup> For example, Warby Parker was founded by four students at Wharton who continued to participate in MBA recruiting, while simultaneously launching their business. All four had accepted jobs to start after graduation in case Warby Parker did not look promising at that point (Grant 2017).

Manso (2016), Dillon and Stanton (2016), and Catherine (2019) model the dynamics of experimentation in self-employment and quantify this option value.<sup>6</sup> According to the experimentation view, frictions to experimenting are the chief impediment to entrepreneurship. Such frictions can be due to regulation (Klapper, Laeven, and Rajan 2006), technology (Ewens, Nanda, and Rhodes-Kropf 2018), organizational constraints (Gompers 1996), or financing risk (Nanda and Rhodes-Kropf 2013b; Nanda and Rhodes-Kropf 2017). In our setting, job-protected leaves could reduce the cost of experimentation by giving entrepreneurs the ability to work on an idea, while being shielded from long-term negative career consequences.

More broadly, we contribute to a large literature on factors that discourage entrepreneurship. Entry regulations have been shown to limit entrepreneurship both across (Djankov et al. 2002; Desai, Gompers, and Lerner 2003; Klapper, Laeven, and Rajan 2006) and within countries (Mullainathan and Schnabl 2010; Bruhn 2011; Branstetter et al. 2014). Much work examines whether relaxing financial constraints increases entrepreneurship (Evans and Jovanovic 1989; Holtz-Eakin, Joulfaian, and Rosen 1994a; Holtz-Eakin, Joulfaian, and Rosen 1994b; Hurst and Lusardi 2004; Bertrand, Schhaar, and Thesmar 2007; Mel, McKenzie and Woodruff 2008; Kerr and Nanda 2009b; Adelino, Schoar, and Severino 2015; Schmalz, Sraer, and Thesmar 2017) or whether entrepreneurship training programs or exposure to entrepreneurial peers increases entrepreneurship (Karlan and Valdivia 2011; Lerner and Malmendier 2013; Drexler et al. 2014; Fairlie, Karlan, and Zinman 2015). Our paper differs in its focus on career risk. We are not aware of any other work examining whether career risk discourages entry into entrepreneurship. Our findings are consistent with Manso (2011), who shows that the optimal contract to motivate innovation (or experimentation more generally) involves a commitment by the principal not to fire the agent.

In recent related work, Hombert et al. (2020) examine a French reform to unemployment insurance (UI). Prior to the reform, unemployed workers would stop receiving UI payments if they started a business. Following the reform, starting a business no longer required giving up these benefits. Our paper is related in that we also study a form of employment insurance. However, since Hombert et al. (2020) only study unemployed workers, their paper is necessarily silent on whether worries about the career consequences of leaving one's job affect the decision to enter entrepreneurship. The core finding of our paper is that such worries are indeed an important factor for employed workers. In contrast, the focus of Hombert et al. (2020) is on studying the quality of the entrepreneurs who are spurred to enter by that reform and the reform's welfare implications.

Finally, our paper also contributes to a large literature analyzing the impacts of paid maternity leave on female labor supply (Ejrnaes and Kunze 2006;

<sup>6</sup> Catherine (2019) also shows theoretically that reducing the wage penalty faced by entrepreneurs returning to wage employment should increase entry into entrepreneurship.

Gregg et al. 2007; Baker and Milligan 2008a; Lalive and Zweimüller 2009; Hegewisch and Gornick 2011; Lalive et al. 2014; Schönberg and Ludsteck 2014). Most of that literature studies the extent to which maternity leave causes some women who would have dropped out of the labor force to instead remain wage-employed. In contrast, our paper studies the extent to which maternity leave causes some women who would have remained wage-employed to instead become entrepreneurs. Interestingly, we find that the effect of extended leave time on entrepreneurship is comparable in magnitude to its effect on wage employment.

## 1. Data

Our primary data set comes from the Canadian Census of the Population, which is administered every 5 years by Statistics Canada. The census enumerates the entire population of Canada. Eighty percent of households receive a short census questionnaire, which asks about basic topics, such as age, sex, marital status, and native language. Twenty percent of households receive the long-form questionnaire, which adds many additional questions on topics such as education, ethnicity, mobility, income, employment, and dwelling characteristics. Respondents to the long-form survey typically give Statistics Canada permission to directly access tax records to answer the income questions. Participation in the census is mandatory for all Canadian residents. Aggregated data from the census are available to the public. Individual-level data are only made publicly available 92 years after each census and, in some cases, only with the permission of the respondent. However, for approved projects, Statistics Canada makes the micro-data from the long-form survey available for academic use. We use these confidential micro-data in our study. While the data are at the individual level, they are anonymized. Individuals and households cannot be linked across census years. So although the census is administered to the whole population every 5 years, it is not possible to form a panel, and we rely on cross-sectional data exclusively. Our primary sample consists of parents from the 2006 census who (we infer) had their first child within 60 days of the December 31, 2000, reform date. There are 86,565 such parents in the census.<sup>7</sup>

One key variable for this study is the date on which parents had a child. While the census does not directly record this information, it can be inferred fairly well. In particular, the census records family relationships within a household and the date of birth for all members of the household. So we assume that parents had children on the birthdates of the children residing in the same household as

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<sup>7</sup> Because of restrictions from Statistics Canada, all of our results (including observation counts) are reported using census weights. Because participation in the census is mandatory and the 20% of households selected for the long-form survey is random, the weights are generally very close to five for all respondents. That is, one observation in the sample data is representative of approximately five observations in the population data. Because the weights are so uniform, our results change little when they are unweighted.

**Table 1**  
Summary statistics

Sample:	Mothers				Fathers			
	Observations	Mean	Median	SD	Observations	Mean	Median	SD
Number of children	46,545	1.760	2	0.662	40,020	1.813	2	0.653
Entrepreneur (income-based)	46,545	0.042	0	0.200	40,020	0.080	0	0.272
Entrepreneur (self-reported)	46,545	0.071	0	0.256	40,020	0.137	0	0.344
Age	46,545	32.78	33	5.787	40,020	35.98	35	6.467
Bachelor's degree	46,545	0.278	0	0.448	40,020	0.272	0	0.445
Minority	46,545	0.258	0	0.438	40,020	0.248	0	0.432
Total income	46,545	24,003.3	18,859	22,365.4	40,020	48,378.5	38,416	71,292.0
Work hours	46,545	20.58	20	19.71	40,020	39.94	40	19.76

This table presents summary statistics for mothers and fathers who had their first child (excluding multiple births) within 60 days of the December 31, 2000, reform date. All variables reflect information as of the 2006 census date (May 16, 2006). *Number of children* is the total number of children the parent has had as of the census date. *Entrepreneur* (income-based) is an indicator equal one if the parent receives at least 50% of his/her total income from self-employment. *Entrepreneur* (self-reported) is an indicator equal to one if the parent self-reports as self-employed. *Age* is the parent's age as of the census date. *Bachelor's degree* indicates having a Bachelor's degree or higher. *Minority* indicates being in a nonwhite ethnic group. Total income indicates total annual personal income (in US\$(2006)). Work hours indicate the average number of work hours per week. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada.

themselves.<sup>8</sup> Of course, non-biological-family relationships within a household will lead to some degree of measurement error (e.g., stepchildren and children adopted as non-newborns). But this measurement error is likely small in magnitude and, if anything, it would bias us against finding any effect.

The other key variable for our study is entrepreneurship, which we proxy with self-employment, as is common in the literature. Respondents to the long-form census must provide information on both their total income and their self-employment income. In most cases, this information is directly obtained from their tax filings. Our primary definition of self-employment is someone who receives at least 50% of total income from self-employment.<sup>9</sup> Separately, respondents must also report whether they consider themselves self-employed based on their primary job. We show in robustness tests that our results are similar when using self-reported entrepreneurship.

Table 1 shows basic summary statistics for mothers and for fathers who had their first child within 60 days of the December 31, 2000, reform date. While the sample is selected based on the inferred birth of a child around December 31, 2000, the summary statistics reflect information as of the 2006 census. In our sample, 4.2% of mothers are self-employed as of 2006 when using the definition based on self-employment income. Based on self-reports, 7.1% identify themselves as being self-employed. The average mother in the sample is approximately 33 years old and has 1.76 children as of 2006. About

<sup>8</sup> We use children reported in the 2006 census to infer child birthdates in a window around December 31, 2000; therefore, the relevant children would be around 5 years old as of the 2006 census date.

<sup>9</sup> Canadian taxes are assessed based on individual income, not combined spousal income as in the United States. Thus, our data record self-employment and wage employment income for each individual.

28% are college graduates. The rate of self-employment for fathers is higher, as is their age.<sup>10</sup> The last two lines of the table show income and work hours. These are about twice as large for fathers as for mothers, with the differences largely driven by the extensive margin: 39% of mothers report zero work hours and mean work hours increase to 34.5 when those reporting zero are excluded. Although dramatic, these gender differences do not influence our estimation, which focuses on discontinuous *changes* for mothers (as compared to changes for fathers) around December 31, 2000.

Since our study relies on policy variation in Canada, the results will naturally be specific to that setting. Nevertheless, to determine whether our sample is substantially different from a comparable sample in the United States, we compute analogous summary statistics with U.S. data. Table A.1 in the Internet Appendix uses data from the 2006 American Community Survey to produce a U.S. version of Table 1. We restrict the sample to parents with a 5-year-old child and apply the same variable definitions used in Table 1. The two samples show very similar characteristics, and in particular, similar self-employment rates. Canadian parents in our sample thus appear quite comparable to their U.S. counterparts.

## 2. Maternity Leave Policy in Canada

Canada's ten provinces have significant legal and fiscal autonomy, and in particular, have primary responsibility for labor legislation. Despite this autonomy, legislatively guaranteed maternity leave—the right to return to a prechildbirth job after a specified period of absence—has several common features across the provinces (Baker and Milligan 2008b).<sup>11</sup> First, employees are protected from dismissal due to pregnancy. Second, a maximum period for the leave is always prescribed, and the provinces do not mandate any paid leave. Initially, the laws of several provinces provided guidance on how the period of leave should be split pre- and post-birth, but current practice is to leave this to the discretion of the mother and employer. Third, the laws specify a minimum period of employment for eligibility. This varies widely: initially, 52 weeks of employment was common, although the recent trend is toward shorter qualification periods. Fourth, most laws specify which terms of employment are preserved during the leave and any responsibility of the employer to maintain benefits. Finally, the laws of some provinces establish rules for extending leaves because of medical complications or pregnancies that continue after term.

While provinces only mandate a period of unpaid leave, the federal Employment Insurance system provides partial income replacement. Prior

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<sup>10</sup> Note that there are fewer fathers than mothers in the sample because there are more households with only a mother present than households with only a father present.

<sup>11</sup> In addition to the 10 provinces, whose combined population is 34 million, Canada has three territories with a combined population of 100,000, located north of 60 degrees latitude.

**Table 2**  
**Maternity leave reform**

Province	Weeks leave Prereform	Weeks leave Post-reform	Cutoff date
Alberta	18	52	December 31, 2000
British Columbia	30	52	December 31, 2000
Manitoba	34	54	December 31, 2000
New Brunswick	29	54	December 31, 2000
Newfoundland and Labrador	29	52	December 31, 2000
Nova Scotia	34	52	December 31, 2000
Ontario	35	52	December 31, 2000
Prince Edward Island	34	52	December 31, 2000
Saskatchewan	30	52	December 31, 2000
Mean	30.3	52.4	

This table shows the maximum length of job-protected leave by province before and after the 2001 reform.  
*Sources:* Baker and Milligan (2008b), provincial statutes, and Employment Standards.

to 2001, Employment Insurance provided partial income replacement for 25 weeks surrounding the birth of a child (a 2-week unpaid waiting period followed by a 25-week paid leave period). In 2001, the Employment Insurance Act was reformed to allow for up to 50 weeks of partial income replacement (a 2-week unpaid waiting period followed by a 50-week paid leave period). Those on leave receive 55% of their normal income up to a maximum determined each year based on mean income levels (at the time, \$413 CAD per week, or about US\$275). Of course, temporary income replacement is less useful if one's prechildbirth employer does not approve of the leave, and the absence were to cost the new mother her job. Prior to the 2001 reform to the Employment Insurance Act, provinces required that employers grant anywhere from 18 to 35 weeks of job-protected leave surrounding the birth of a child. Following the reform, all provinces increased the mandated guarantee to at least 52 weeks to match the new income replacement period set by Employment Insurance (including the 2-week waiting period). Following the reform, 35 out of the 52 weeks of job-protected leave could technically be split between the two parents however they preferred. In practice, very few families allocated job-protected leave time to fathers instead of mothers (Baker and Milligan 2008a).

Table 2 shows the maximum leave period by province, before and after the 2001 reform. Quebec is excluded from the table as it is the one province that did not change in 2001, mandating 70 weeks of job-protected leave throughout. The average province went from approximately 30 weeks to 52 weeks, an increase of approximately 5.5 months. Given that maternity leave entitlements usually increase gradually over time, this reform represents one of the largest year-over-year increases in any country. A key aspect of the reform's implementation for our purposes is that it was tied to the date a woman gave birth. Those who gave birth on or after December 31, 2000, were entitled to an extended leave. Those who gave birth even a day before were not. Despite unhappiness among those who just missed the cutoff, this policy admitted no exceptions (Muhlig 2001).

In terms of the timing of the reform's announcement, the federal budget was announced on February 29, 2000, with the December 31, 2000, cutoff date for extended income replacement eligibility. In principle, this announcement predated the cutoff sufficiently so that parents could delay pregnancy until a point where they would be sure to give birth under the new rules. But recall that the federal announcement only concerns income replacement, not job protection. The provinces, which determine rules surrounding job protection, did not announce that they would extend job-protected leave until November 2000 at the earliest. In several cases, they claimed that they would not be extending job-protected leave, even though they later ended up doing so.<sup>12</sup> Thus, all of the mothers who gave birth around what turned out to be the cutoff date for the extension in job-protected leave, conceived long before they knew whether job-protected leave would be extended in their province and, if so, what the cutoff date would be.

### **3. Empirical Strategy**

While there was a discontinuous jump in the maximum length of leave available to parents who had children around the reform cutoff, this does not necessarily mean that there was a change in actual leave-taking, as individuals may not have made use of the additional time available. If the reform had no effect on actual leave-taking, we would not expect to find an effect on entrepreneurship. While respondents do not retrospectively report the length of previous leaves taken, which would allow us to look for a discontinuous jump in leave-taking around the cutoff date, they do report whether they are currently on leave as of the census date. We, therefore, trace out the probability of a respondent being on leave on the census date as a function of the number of weeks between the date of birth of the respondent's youngest child and the census date. We do this separately using data from the 1996 and 2006 censuses, which are 5 years before and after the reform, respectively.

Figure 1, panel A, shows that in all weeks following giving birth, the probability of employed mothers being on leave is indeed greater in 2006 than in 1996. Of course, these two samples are 10 years apart, and leave-taking behavior may have changed over these years for reasons other than the reform. To address this possibility, we also look at the 2001 census. Mothers who gave birth 0–19 weeks before the 2001 census date were eligible for the extended leave, whereas those who gave birth 20 or more weeks before the census date were not. We find that for those in the 2001 census who had access to the additional leave time, their leaving-taking behavior was nearly identical to that of those in the 2006 census. For those in the 2001 census who did not have access to the additional leave time, their leave-taking behavior was nearly identical to

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<sup>12</sup> Two provinces (Alberta and Saskatchewan) waited until the first half of 2001 to announce the extension and retroactively extended job-protected leave for those who gave birth after the December 31, 2000, cutoff date.

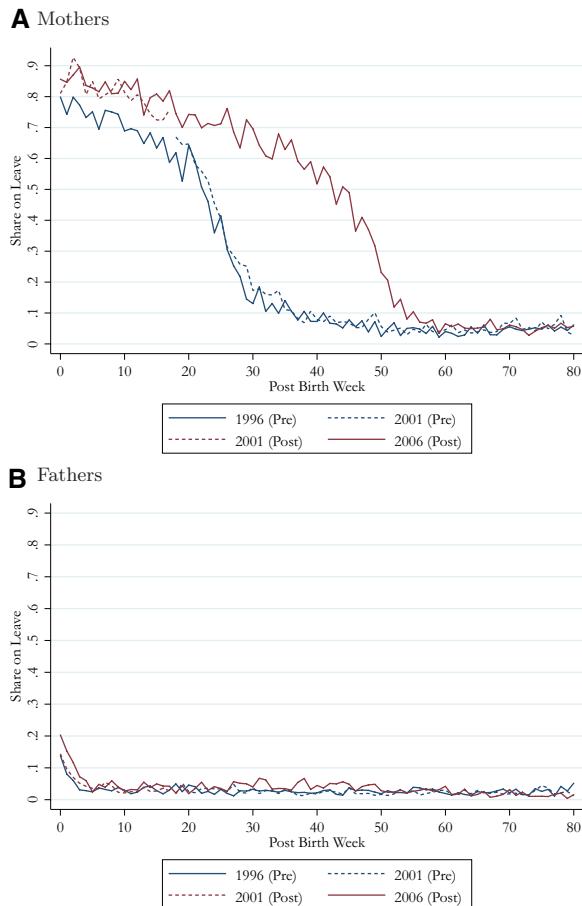
those in the 1996 census. This strongly suggests that the reform was indeed the main driver of the change from 1996 to 2006. We repeat the same exercise for fathers in panel B and find little change in leave-taking behavior over the same years. The reluctance of fathers to use parental leave is consistent with prior research (Lalive and Zweimüller 2009; Schönberg and Ludsteck 2014; Dahl et al. 2016). Dahl et al. (2014) find evidence that the reason fathers are reluctant to take parental leave is due to fears about how employers and coworkers would react. In other words, the perceived stigma costs for fathers to take leave is high enough that they do not do so, even when given the legal right.

Thus, it appears that there was a discontinuous increase in the amount of leave available to and taken by mothers who gave birth just after the December 31, 2000, cutoff date, while fathers did not change their leave-taking behavior. Aside from leave-taking, mothers on each side of the cutoff are likely to be similar in terms of other characteristics. The reform thus lends itself naturally to analysis with a sharp regression discontinuity design (RDD). However, some potential concerns remain with such an empirical strategy. First, a simple RDD relies on the identifying assumption that other factors influencing the dependent variable evolve smoothly across the cutoff. However, given that our running variable is calendar time, this may not be the case. For example, macroeconomic conditions do not necessarily evolve smoothly over time. Thus, the reform cutoff date might happen to coincide with a macroeconomic shock that affects entrepreneurial entry. Second, since our cutoff date is the last day of the calendar year, one may worry that other things may change discontinuously around that date. For example, other policies (e.g., policies related to taxes or school eligibility) may differ discretely across the cutoff. To address the possibility of such confounds, we use fathers as a control group. As Baker and Milligan (2008a) suggest, fathers are a natural control group in the context of this reform; the reform did not affect their leave-taking behavior, but contemporaneous macroeconomic shocks, as well as other policies, are likely to affect them and mothers similarly. Consistent with this idea, we find that the time-series correlation between the entrepreneurship rates among mothers and fathers in Canada is 0.95.<sup>13</sup> Also, Table A.2 in the Internet Appendix shows that the cross-sectional correlation between individual characteristics (e.g., age, education, and ethnicity) and entrepreneurship status is statistically indistinguishable for mothers and fathers in our sample.

Thus, rather than using simple RDD, we implement a “difference-in-discontinuities” design (see, e.g., Grembi, Nannicini, and Troiano 2016; Lalive 2008, Leonardi and Pica 2013). This empirical strategy differences out any discontinuous change in entrepreneurship rates among fathers from that among mothers. To fix ideas on how this is implemented, first consider a simple RDD.

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<sup>13</sup> This calculation is based on annual entrepreneurship rates from the Canadian Labor Force Survey from 1995 to 2010.



**Figure 1**  
**Leave-taking before and after reform**

Panel A shows the share of employed mothers on leave in the census reference week (the week ending the Sunday before the census date) as a function of the number of weeks between their most recent child's birth and the reference week. Panel B shows the share of fathers. We calculate these shares separately using data from the 1996 census (before the reform), the 2001 census (before or after the reform, depending on the child's exact birthdate), and the 2006 census (after the reform).

In our context, this would mean estimating equations of the form:

$$y_i = \alpha + \tau Post_i + f_1(DayChild_i) + Post_i \times f_2(DayChild_i) + \epsilon_i, \quad (1)$$

where  $y_i$  is an outcome of interest for mother  $i$ ;  $DayChild_i = DateChild_i - C$ , which is the date mother  $i$  had a child ( $DateChild_i$ ) relative to the reform cutoff date ( $C$ ); and  $Post_i = \mathbb{1}\{DayChild_i \geq 0\}$ , which is an indicator equal to one if mother  $i$  gave birth on or after the cutoff date. The control functions capture time trends around the cutoff. In particular, the function  $f_1(\cdot)$  captures a smooth trend

on the left side of the cutoff, and the sum,  $f_1(\cdot) + f_2(\cdot)$ , captures a (potentially different) smooth trend on the right side of the cutoff. The estimated coefficient  $\hat{\tau}$  captures any discontinuity between these trends at the cutoff.

The RDD literature recommends estimating Equation (1) using local linear regression (see e.g., Fan et al. 1996; Imbens and Lemieux 2008; Lee and Lemieux 2010; Gelman and Imbens 2019).<sup>14</sup> This means limiting the sample to mothers who gave birth within a narrow bandwidth,  $h$ , around the cutoff date and assuming linear control functions. Within a narrow bandwidth, one can reasonably assume that the control functions are approximately linear. One could also allow the control functions to follow higher-order polynomials. Our baseline estimates use local linear control functions since Gelman and Imbens (2019) show higher-order polynomials to be problematic, but we test our results' robustness to both methods. Observations within the specified bandwidth around the cutoff are weighted according to some kernel function. A triangular kernel, which puts more weight on observations near the cutoff, has been shown to be optimal in the RDD literature (Fan et al. 1996).<sup>15</sup> For a given bandwidth and kernel, one can then estimate local linear regressions of the form:

$$y_i = \alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i + \epsilon_i. \quad (2)$$

To use fathers as a control group, we convert this model into a difference-in-discontinuities design. Specifically, we augment the sample to also include fathers who had children within the same bandwidth around the cutoff date. We then augment Equation (2) by fully interacting all terms with an indicator variable for mothers:

$$\begin{aligned} y_i = & Mother_i \times [\alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i] \\ & + [a + t Post_i + d_1 DayChild_i + d_2 Post_i \times DayChild_i] + \epsilon_i \end{aligned} \quad (3)$$

Thus, we allow mothers and fathers to have their own smooth control functions on each side of the cutoff and their own potential discontinuities at the cutoff. The estimated coefficient  $\hat{\tau}$  now represents the difference between the discontinuity for mothers ( $\hat{\tau} + \hat{\tau}$ ) and that for fathers (just  $\hat{\tau}$ ). This difference,  $\hat{\tau}$ , is our estimated treatment effect and will be labeled *Treatment* in the tables.

Our primary outcome of interest is an indicator equal to one if individual  $i$  is an entrepreneur as of the 2006 census date, as defined in Section 1. Thus, we are examining the effect of extended job-protected leave on entrepreneurship status approximately 5 years later. We do not examine entrepreneurship status in the 2001 census because the census date falls too close to the reform date.

<sup>14</sup> For recent examples in the finance literature of RDD estimation using local linear regression, (see, for example, Berg 2015; Malenko and Shen 2016; Campello et al. 2018, Li, Liu, and Wu 2018).

<sup>15</sup> In practice, the choice of kernel generally makes little difference (Imbens and Lemieux 2008).

The 2001 census was administered on May 15, only about 4.5 months from the reform date. This means that individuals who just qualified for extended leave by giving birth shortly after December 31, 2000, would still likely be on leave by the census date, as they would be eligible for 12 months of leave. We do examine shorter-term outcomes, however, using supplemental data from the Longitudinal Administrative Databank in Section 5.7.

## 4. Interpretation

### 4.1 Causal effect of reduced career risk

To interpret the treatment effect obtained from estimating equation (3), it is important to note that employees always had the option to take a short job-protected leave or a long non-job-protected leave, even prior to the reform. The latter was possible in the sense that a worker could always quit her job, leave wage employment, and then reenter whenever she wanted. Following the cutoff date, employees retained the option to take a short job-protected leave, but now could also take a long job-protected leave, if desired. Thus, the primary thing that the reform changed was the career risk associated with a long absence from wage employment. Our empirical strategy, therefore, measures whether an exogenous reduction in the career risk associated with a long absence from wage employment causes an increase in entrepreneurial entry.

An important caveat to the above discussion is that the reform we study extends not only the job protection period but also the income replacement period. This means that we are actually estimating a joint effect of these two aspects of the reform. In Section 5.6, we show evidence that an increase in the income replacement period alone has little impact in our setting. This evidence is also consistent with Dahl et al. (2016) and Stearns (2016), who find that increases in income replacement without changes in job protection have little effect on a wide variety of outcomes. Thus, it does seem that the job protection aspect of the reform is critical. However, we cannot say conclusively that an extension in job protection alone would be enough to spur additional entrepreneurship. It is possible that decreasing career risk does spur entrepreneurship, but only when coupled with at least some passive income.

### 4.2 The counterfactual

Access to extended job-protected leave might cause some people who would have been out of the labor force or unemployed (henceforth, we will simply call this “out of the labor force” for brevity) to instead be wage employed, and some people who would have been wage employed to instead be entrepreneurs. Under this scenario, access to extended job-protected leave should cause a decrease in the probability of an individual being out of the labor force, an increase in the probability of an individual being an entrepreneur, and an ambiguous change in the probability of an individual being wage employed.

However, another possibility is that access to extended job-protected leave causes some people who would have been out of the labor force to instead be entrepreneurs and others who would have been out of the labor force to instead be wage employed. Under this scenario, access to extended job-protected leave should cause a decrease in the probability of an individual being out of the labor force, an increase in the probability of an individual being an entrepreneur, and an increase in the probability of an individual being wage employed.

We argue that the first counterfactual makes more sense theoretically. It is not clear why access to extended job-protected leave would cause some people who would have been out of the labor force to directly enter entrepreneurship instead. Loosely speaking, for those whose first choice is wage employment and whose second choice is being out of the labor force, it makes sense that access to extended job-protected leave allows them to move from their second to their first choice. Likewise, for those whose first choice is entrepreneurship and whose second choice is wage employment, it makes sense that access to extended job-protected leave allows them to move from their second to their first choice. However, for those whose first choice is entrepreneurship and whose second choice is being out of the labor force, extended job-protected leave is irrelevant. Regardless of job-protected leave availability, such individuals should simply quit their job, try entrepreneurship (their first choice), and then be out of the labor force (their second choice) if their business fails.

## 5. Results

### 5.1 Validity of the empirical design

We begin the analysis by examining the validity of our empirical design. To the extent that the timing of births can be controlled, one concern is that different types of individuals might choose to locate themselves on the right side of the cutoff threshold. Conditional on the timing of pregnancy, the timing of births is difficult to control precisely, as the length of pregnancy naturally varies by five weeks (Jukic et al. 2013). Nevertheless, scheduled Caesarean deliveries or induced births could conceivably be shifted within a small window, although it is likely easier to shift births earlier in time rather than later, as would be required in our setting.<sup>16</sup> Baker and Milligan (2014) find no evidence of gaming in birth timing around the reform we study in this paper. Similarly, Dahl et al. (2014) find no evidence of gaming around a similar reform in Norway. However, Dickert-Conlin and Chandra (1999) do find evidence that births are moved from the beginning of January to the end of December in the United States to take advantage of tax benefits.<sup>17</sup> To minimize gaming concerns, we focus on

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<sup>16</sup> Cesarean sections are also much less common in Canada, where the overall rate is 20% lower than in the United States (OECD 2015).

<sup>17</sup> More recent work suggests that the magnitude of birth timing in the United States is small and largely due to misreporting rather than actual shifts in births (LaLumia, Sallee, and Turner 2015).

first-time singleton births (i.e., we exclude twins, second children, and so forth), as these deliveries are considerably less likely to be scheduled in advance. We categorize a birth as a first-time singleton birth if a child residing in the same household as a parent is the oldest child in the household, and no other children in the household share the same birthdate. Still, some gaming remains possible even for these births. Such gaming may be even related to the mechanism we have in mind; namely, individuals who want to explore entrepreneurship select into the longer leave to allow themselves the ability to do so. Alternatively, it may simply be those who are savvier about how to game the reform are also more inclined toward entrepreneurship, but the reform has no effect on their ability to become an entrepreneur.

To test for gaming in an RDD framework, McCrary (2008) suggests checking for a discontinuity in sample density at the cutoff. Indeed, in our context, gaming would imply that births that would otherwise have occurred prior to December 31 instead occur after. Moreover, it is likely easier to delay a birth that would have otherwise occurred close to the cutoff date than one that would have occurred far in advance. In particular, if gaming is present in our sample, we would expect a discontinuous jump in sample density around the cutoff, as mass is shifted from the left of the cutoff to the right. To test whether this is the case, we aggregate our data to the day level and estimate

$$NumBirths_t = \alpha + \tau Post_t + \delta_1 EventDay_t + \delta_2 Post_t \times EventDay_t + \epsilon_t, \quad (4)$$

where  $NumBirths_t$  represents the number of (first-time singleton) births on date  $t$ , and  $EventDay_t$  measures the distance to the reform cutoff date. This is analogous to the RDD specification in Equation (2), but with the outcome being the birth rate rather than an entrepreneurship measure. If there were gaming, we would estimate a positive  $\hat{\tau}$ ; that is, there should be a discontinuous jump in the birth rate around the cutoff date.

Panel A of Table 3 shows the results. We estimate Equation (4) using bandwidths ranging from 60 to 30 days and a triangular kernel. For all bandwidths, we find no significant discontinuity in the birth rate at the reform cutoff date. The point estimates are positive but insignificant, both statistically and economically. If we take the point estimates seriously, despite being statistically indistinguishable from zero, they imply that 12.55 to 17.5 births in total across Canada may have been shifted from the prereform period to the post-reform period. Table A.3 in the Internet Appendix shows that these point estimates are even smaller when one accounts for the small, statistically insignificant jump in the birth rate around December 31 in nonreform years. Figure 2 shows these results graphically. The lines correspond to the estimated linear trends on each side of the cutoff, and the insignificant discontinuity at the cutoff date corresponds to the estimated coefficient for  $Post_t$ . We see a smooth evolution of birth density across the cutoff date. As shown in Figure A.1 in the Internet Appendix, the graphical results look similar with a 60-day bandwidth. Figure A.2 in the Internet Appendix also shows that there is no discontinuity

**Table 3**  
**RDD validity tests**

A. Smoothness of density

Dependent variable:	Number of births			
	(1)	(2)	(3)	(4)
Post	12.55 (23.16)	15.89 (25.54)	17.50 (28.38)	14.40 (32.30)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	121	101	81	61

B. Smoothness of covariates

Sample:	Mothers			Fathers		
	Age (1)	BA degree (2)	Minority (3)	Age (4)	BA degree (5)	Minority (6)
Dependent variable:						
Post	-0.602 (0.370)	-0.025 (0.028)	0.043 (0.030)	-0.682 (0.451)	-0.005 (0.030)	0.030 (0.031)
Bandwidth	30	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st	1st	1st
Observations	22,845	22,845	22,845	19,585	19,585	19,585

Panel A of this table tests for gaming around the cutoff date. It estimates the following local linear regression with a triangular kernel and bandwidths ranging from 60 to 30 days:

$$NumBirths_t = \alpha + \tau Post_t + \delta_1 EventDay_t + \delta_2 Post_t \times EventDay_t + \epsilon_t,$$

where observations are at the day level, and the sample consists of days around December 31, 2000, the reform cutoff date;  $NumBirths_t$  represents the number of (first-time, singleton) births on date  $t$ ;  $EventDay_t$  is the date relative to the cutoff date; and  $Post_t$  is an indicator equal to one if the date is on or after the cutoff date.

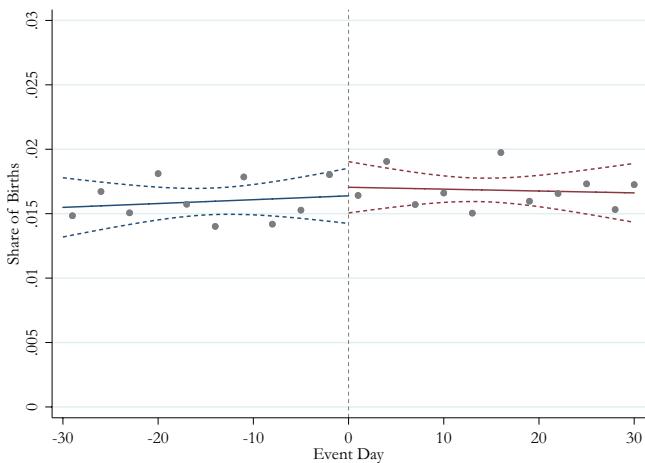
Panel B of this table tests for discontinuity in the observable characteristics of parents who had a child around the cutoff date. It estimates the following local linear regression with a triangular kernel and a 30-day bandwidth:

$$Characteristic_i = \alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i + \epsilon_i,$$

where observations are at the parent level; in columns 1–3 the sample consists of mothers who had a first child (excluding multiples) within 30 days of the cutoff date; in columns 4–6, the sample consists of analogous fathers;  $DayChild_i$  represents the date parent  $i$ 's child was born relative to the reform cutoff date; and  $Post_i$  is an indicator equal to one if parent  $i$  had a child on or after the cutoff date. Other variables are defined in Table 1. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

in birth rates around the cutoff date in the previous year, when there was no reform. The absence of gaming is consistent with Baker and Milligan (2014), who find that the reform had no effect on the spacing of births.

Given that there is no evidence of gaming, those who gave birth just before the cutoff date could be plausibly similar to those who gave birth just after, both in terms of their observable and in terms of their unobservable characteristics. In other words, around the cutoff date, eligibility for extended leave is as good as randomly assigned. While we cannot test whether individuals on each side of the cutoff are similar in terms of unobservable characteristics, we can test whether they are similar in terms of observable characteristics. To do so, we estimate a version of Equation (2), but with parents' observable characteristics



**Figure 2**  
**RDD validity check**

This figure graphically shows the results from Table 3, panel A, with the y-axis showing the share of births. The gray dots represent the raw data on the share of births within 3-day bins around the cutoff date. The solid lines represent the regression estimates, that is, the predicted shares based on the coefficients obtained from estimating the local linear regression in Table 3, panel A, with a 30-day bandwidth and triangular kernel. The dashed lines represent 95% confidence intervals.

as dependent variables. We choose characteristics that are largely fixed at the time of childbirth so they are unlikely to be affected by the treatment. Panel B of Table 3 shows the results. We find no discontinuity in terms of age, education, or ethnicity for parents who have a child around the cutoff date. Figure A.3 in the Internet Appendix illustrates these results.

Why do individuals not try harder to end up on the right side of the cutoff? They could potentially do so in two different ways. First, they could time their pregnancies so as to be assured of giving birth after the cutoff date (i.e., “pregnancy timing”). Second, those who were pregnant around the cutoff date could potentially influence the timing of when they gave birth (i.e., “birth timing”). Clearly, birth timing is more difficult than pregnancy timing. The tests above are primarily designed to check for birth timing, which would result in a discontinuity in births at the cutoff date. Pregnancy timing is less likely to lead to such discontinuity, given the variation in the lengths of pregnancies, as well as in the amount of time it takes to conceive. Even if the cutoff date were perfectly anticipated and many individuals delayed pregnancies as a result, such delays would likely lead to a downward trend in birth rates leading up to the cutoff and an upward trend following the cutoff, with no discontinuity at the cutoff. We do see a subtle pattern along these lines in Figure A.1 in the Internet Appendix. However, pregnancy timing may have been limited since, as discussed in Section 2, the cutoff date for extended job-protected leave was not perfectly anticipated. In addition, pregnancy timing can only occur for intended pregnancies, while survey evidence suggests that approximately 27%

of pregnancies in Canada that are carried to term are unintended (Oulman et al. 2015).

Birth timing and pregnancy timing have different implications for the validity of our empirical strategy. Birth timing would undermine the “internal validity” of our estimates, that is, the validity of a causal interpretation. Pregnancy timing only relates to the “external validity” of our estimates, that is, the generalizability of our causal estimates to other groups. In the presence of pregnancy timing, our empirical strategy would still yield an unbiased estimate of the causal effect of the reform, but we would be estimating the causal effect among the types of individuals who ended up with a due date close to the cutoff date. This group would consist of (1) those who had unplanned pregnancies, (2) those who had planned pregnancies but did not anticipate the cutoff date, and (3) those who had planned pregnancies and anticipated the cutoff date but did not delay their pregnancy for the sake of extended job-protected leave. We could not say anything about the causal effect among the residual group: those who planned their pregnancies, anticipated the cutoff date, and found it worthwhile to delay. However, it seems plausible that the effect actually may be largest among this residual group, given they seem to care the most about job-protected leave. In that case, our estimates would represent a lower bound of what we would have found if the reform were completely unanticipated.

## 5.2 Main findings

We now present our central results, examining whether mothers who had access to longer job-protected leave were subsequently more likely to forgo wage employment and become entrepreneurs. Specifically, we estimate Equation (3) on the sample of parents who had their first child (excluding multiple births) around the December 31, 2000, cutoff date. The main outcome of interest is whether the majority of an individual’s income came from self-employment as of the May 16, 2006, census date. We estimate local linear regressions using a triangular kernel and bandwidths ranging from 60 days around the cutoff date down to 30 days.

Panel A of Table 4 shows the results. The row labeled *Treatment* corresponds to the estimated coefficient  $\hat{\tau}$  on  $Mother_i \times Post_i$  in Equation (3), which is our estimated average treatment effect. As panel A shows, we find a positive and statistically significant average treatment effect. This means that, relative to fathers, mothers giving birth right after the cutoff date are discontinuously more likely to be entrepreneurs than those giving birth right before. The estimated magnitudes are quite stable using different bandwidths. Across the different specifications, we find a 1.9- to 2.1-percentage-point increase in entrepreneurship following the policy change. Table A.4 in the Internet Appendix shows that we also find similar results for narrower bandwidths (20 days and 10 days) as well. This provides additional evidence that individuals do not time the date of pregnancy in anticipation of the policy.

**Table 4**  
**Baseline results**

Dependent variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0194*** (0.0060)	0.0210*** (0.0061)	0.0199*** (0.0064)	0.0193*** (0.0074)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	86,565	71,285	57,405	42,430

This table tests whether increased job-protected leave availability causes an increase in entrepreneurship using a differences-in-discontinuities framework. It estimates the following local linear regression with bandwidths ranging from 60 to 30 days and a triangular kernel:

$$\begin{aligned} Entrepreneur_i &= Mother_i \times [\alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i] \\ &\quad + [a + t Post_i + d_1 DayChild_i + d_2 Post_i \times DayChild_i] + \epsilon_i, \end{aligned}$$

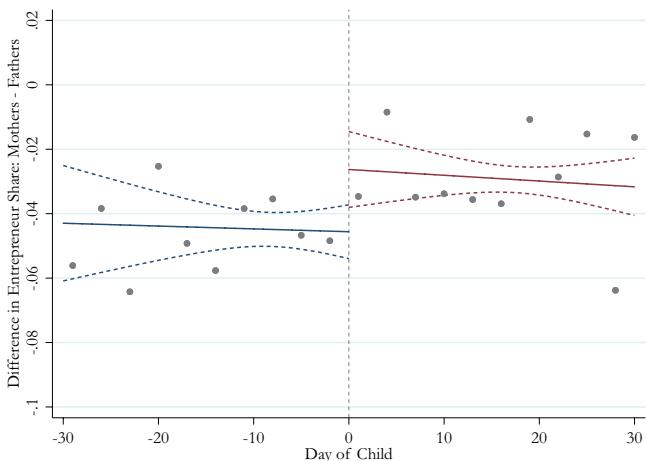
where observations are at the parent level; the sample consists of parents who had a first child (excluding multiples) within a specified bandwidth around the December 31, 2000, cutoff date;  $Entrepreneur_i$  is an indicator equal to one if over 50% of parent  $i$ 's income is from self-employment as of the 2006 census;  $DayChild_i$  represents the date parent  $i$ 's child was born relative to the reform cutoff date; and  $Post_i$  is an indicator equal to one if parent  $i$  had a child on or after the cutoff date. The row labeled *Treatment* in the table corresponds to the coefficient  $\tau$  in the above equation and represents the estimated average treatment effect. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week.  
<sup>\*</sup> $p < .1$ ; <sup>\*\*</sup> $p < .05$ ; <sup>\*\*\*</sup> $p < .01$ .

While it may be possible to time a pregnancy within 60 days, it would be much more difficult to time a pregnancy within 10 days.

Figure 3 shows our results graphically. The gray dots represent the raw data, that is, the share of mothers within 3-day bins around the cutoff date who became entrepreneurs minus the share of fathers within the same bins who became entrepreneurs. The solid lines represent the regression estimates, that is, the predicted values based on the coefficients obtained from estimating Equation (3). The dashed lines represent 95% confidence intervals. The discontinuity in the solid lines at time zero corresponds exactly with the average treatment effect reported in column 4 of Table 4. Note that, although the confidence intervals slightly overlap in the figure, the difference between the two lines (i.e., the discontinuity at time zero) is statistically significant at the 1% level.<sup>18</sup>

Column 1 of Table A.5 in the Internet Appendix shows the full set of coefficient estimates that generate this figure. The estimated coefficient for the  $Post_i$  variable is statistically insignificant, meaning that there is no statistically significant discontinuity at the cutoff for fathers. Thus, while the reform could, in theory, also affect fathers, there is no evidence to suggest that it does. The coefficient for  $Mother_i \times Post_i$  (i.e., our estimated treatment effect) is positive and statistically significant, meaning that the discontinuity for mothers is

<sup>18</sup> It is possible, and in fact, common, for there to be a statistically significant discontinuity at the cutoff, even when the confidence intervals on each side of the cutoff in the corresponding figure overlap (Knezevic 2008). Essentially, the regression tables are testing whether the difference between the two lines is significant, while the figures simply show independent 95% confidence intervals on each side of the cutoff.



**Figure 3**  
**Baseline results**

This figure graphically shows the baseline results from Table 4. The gray dots represent the raw data on the share of mothers within 3-day bins around the cutoff date who became entrepreneurs minus the share of fathers within the same bins who became entrepreneurs. The solid lines represent the regression estimates, that is, predicted values based on the coefficients obtained from estimating the local linear regression in Table 4 with a 30-day bandwidth and triangular kernel. The dashed lines represent 95% confidence intervals. The discontinuity in the solid lines at time zero exactly corresponds with the average treatment effect reported in column 4 of Table 4. The discontinuity is significant at the 1% level.

significantly larger than that for fathers. The estimated coefficient for *Mother*, is negative and statistically significant. This reflects the fact that mothers are generally less likely than fathers to be entrepreneurs. The remaining coefficients are all statistically insignificant. Thus, the slopes of the time trends for fathers and mothers are statistically indistinguishable from zero, both before and after the cutoff date. Column 2 shows that these results remain similar when we use a uniform kernel rather than a triangular one.

As discussed in Section 3, our difference-in-discontinuities strategy should largely address concerns that our results are driven by year-end effects. Nonetheless, to further rule out such concerns, we perform several placebo tests. First, we examine whether there is a discontinuous jump in 2006 entrepreneurship rates for mothers who had a child around December 31 of nonreform years. Specifically, we pool parents who had their first child around December 31 in the reform year with parents who had their first child around December 31 in nonreform years from 1991 to 2005. We then estimate a variant of Equation (3), in which we interact all of our covariates with a *Reform Year<sub>i</sub>* indicator variable. This takes the value of one for births within the narrow bandwidth *h* of December 31, 2000, and zero for all other births. Panel A of Table 5 shows the results. The coefficient on *Treatment<sub>i</sub>*, without an interaction, now represents the size of the difference-in-discontinuities estimate around December 31 in nonreform years (i.e., *Reform Year<sub>i</sub>*=0). As we would

**Table 5**  
Placebo test using nonreform years

Dependent variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	-0.0032 (0.0034)	-0.0036 (0.0037)	-0.0036 (0.0041)	-0.0014 (0.0047)
Reform year $\times$ Treatment	0.0226*** (0.0067)	0.0245*** (0.0070)	0.0235*** (0.0074)	0.0207*** (0.0084)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	1,447,045	1,201,755	961,680	720,840

This table compares the difference-in-discontinuities estimate from placebo years with the estimate in the actual reform year. The sample consists of parents who had their first child (excluding multiples) within a specified bandwidth around December 31 in the years 1991–2005. All variables from the equation in Table 4 are then interacted with a reform year indicator equal to one for parents who had a child around December 31 in the year 2000. All other variables are defined in Table 4. The coefficient for *Treatment*, without an interaction, represents the size of the difference-in-discontinuities estimate around December 31 in nonreform years (i.e., *Reform Year*=0). The coefficient for the interaction term, *Reform Year* $_i \times$  *Treatment* $_i$ , represents how much larger the difference-in-discontinuities estimate is around December 31 in the actual reform year relative to nonreform years (*Reform Year* $_i \times$  *Treatment* $_i$  corresponds to *Reform Year* $_i \times$  *Mother* $_i \times$  *Post* $_i$ ). Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

expect, we find no evidence of a “treatment effect” in nonreform years. The coefficient for the interaction term,  $Reform Year_i \times Treatment_i$ , represents how much larger the difference-in-discontinuities estimate is around December 31 in the actual reform year. Again, as we would expect, we find a significantly larger treatment effect in the reform year.<sup>19</sup>

One limitation of this test is that the length of time between the cutoff dates and the subsequent measurement of entrepreneurship status is different in the placebo reform years than in the actual reform year. In Table A.6 in the Internet Appendix, we instead use data from the 2001 census to examine 2001 entrepreneurship rates among parents of children born around December 31, 1995. In this case, the time between the placebo cutoff date and the measurement of entrepreneurship status is the same as in our baseline regressions. We again find no discontinuity around the placebo cutoff date. Overall, this evidence suggests that our baseline results are indeed driven by the reform rather than year-end effects. In addition, to help rule out other events occurring during December 2000 or January 2001, we also consider placebo tests that shift the cutoff date backward 60 days and forward 60 days. As shown in Table A.7 in the Internet Appendix, we do not find any discontinuity around these placebo cutoff dates.

The magnitudes of our point estimates are also economically significant. For example, the coefficient estimated in Table 4, column 4, implies a 45% increase

<sup>19</sup> To be clear,  $Treatment_i$  corresponds to  $Mother_i \times Post_i$  in a variant of Equation (3) in which all variables are interacted with a reform year indicator;  $Reform Year_i \times Treatment_i$  corresponds to  $Reform Year_i \times Mother_i \times Post_i$ .

in the probability of entering entrepreneurship relative to the base rate among mothers in our sample. If anything, this may seem too large in percentage terms. However, we note that there is a fairly wide confidence interval around this point estimate, implying anywhere from an 11% increase in entrepreneurship to an 80% increase in entrepreneurship. The literature typically finds estimates in this range.

For example, one of the papers most closely related to ours is Homber et al. (2020). They find that the reform to French unemployment insurance that they study increases the number of new firms started by unemployed individuals from 5,660 per month to 8,966 per month. This corresponds to a 58% increase in the probability of an unemployed individual in France entering entrepreneurship following the reform.

To give a bit more context from less related papers, Lerner and Malmendier (2013) find that decreasing the percentage of students with prior entrepreneurial experience in a Harvard Business School section from 7.7% to 4.8% (approximately a 2.3-student decrease) increases the probability of other students in that section entering entrepreneurship by 34% (from a 2.9% probability to a 3.9% probability). Schmalz, Sraer, and Thesmar (2017) find that experiencing a house price increase of 36% (one standard deviation above the mean in their sample) leads to a 24% increase in the probability of an individual entering entrepreneurship.

Thus, although our estimated magnitudes may seem large in percentage terms, they do not appear to be out of line with the existing literature on entrepreneurial entry, which generally finds large percentage magnitudes. This may reflect the fact that the base rate of entrepreneurship tends to be low, so moderate absolute increases correspond to large percentage increases.

### 5.3 Robustness

Table 6 shows that our baseline results are robust to a range of alternative regression specifications. In our baseline specification, we use a triangular kernel, as the RDD literature has shown it to be optimal (Fan et al. 1996). Nonetheless, panel A of Table 6 shows that results remain similar when using a uniform kernel rather than a triangular kernel. In our baseline specification, we also estimate linear control functions, as Gelman and Imbens (2019) show that high-order polynomials are problematic as RDD control functions. Nonetheless, panels B and C of Table 6 show that our results remain similar when we use quadratic and cubic control functions, although the magnitudes are slightly larger in these cases. Finally, in panel D, we also expand the sample to include births of nonfirst children. We again find similar results among that sample.

In a valid RDD or difference-in-discontinuities setting, controlling for additional covariates should not be necessary, as such covariates should not change discontinuously across the cutoff. Only the treatment should change discontinuously across the cutoff. Therefore, any discontinuity in the outcome

**Table 6**  
Robustness to alternative specifications and/or samples

Dependent variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
<i>A. Uniform kernel</i>				
Treatment	0.0198*** (0.0070)	0.0193*** (0.0071)	0.0216*** (0.0064)	0.0175** (0.0080)
Bandwidth	60	50	40	30
Kernel	Uniform	Uniform	Uniform	Uniform
Polynomial degree	1st	1st	1st	1st
Observations	86,565	71,285	57,405	42,430
<i>B. Second-degree polynomial</i>				
Treatment	0.0221*** (0.0067)	0.0202*** (0.0078)	0.0223*** (0.0079)	0.0296*** (0.0062)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	2nd	2nd	2nd	2nd
Observations	86,565	71,285	57,405	42,430
<i>C. Third-degree polynomial</i>				
Treatment	0.0219** (0.0090)	0.0264*** (0.0075)	0.0323*** (0.0060)	0.0362*** (0.0045)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	3rd	3rd	3rd	3rd
Observations	86,565	71,285	57,405	42,430
<i>D. All births</i>				
Treatment	0.0220** (0.0104)	0.0251** (0.0117)	0.0241** (0.0121)	0.0257* (0.0137)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	193,495	159,240	127,915	94,745

This table repeats the analysis reported on in Table 4 using alternative specifications. Panel A shows the results estimated using a uniform kernel. Panel B shows the results estimated using quadratic control functions. Panel C presents the results estimated using cubic control functions. Panel D expands the sample to include all births, rather than first-time singleton births. All variables are defined in Table 4. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

variable can be attributed to the treatment rather than other confounding factors. Nonetheless, in Table 7, we test whether our results are robust to the inclusion of controls. Column 1 repeats the baseline regression from Table 4 column 4. Columns 2 through 8 sequentially add ethnic origin fixed effects (225 categories), education level fixed effects (three categories), urban/rural fixed effects, age controls (age and age squared), province fixed effects, stock market controls (the log value of the Toronto Stock Exchange index on the date of childbirth), and exchange rate controls (the Canadian-U.S. dollar exchange rate on the date of childbirth). Finally, column 9 allows the effects of these control variables to differ between mothers and fathers, by interacting each of them with a mother indicator. Across all specifications, the magnitude of our estimated treatment effect remains quite stable.

**Table 7**  
**Robustness to controls**

Dependent variable:	Entrepreneur								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.0193*** (0.0074)	0.0201*** (0.0077)	0.0213*** (0.0076)	0.0219*** (0.0076)	0.0215*** (0.0078)	0.0215*** (0.0078)	0.0215*** (0.0078)	0.0214*** (0.0077)	0.0202** (0.0082)
Race	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes
Education	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Urban	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Age	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Province	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Stock index	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Exchange rate	No No	No No	No No	No No	No No	No No	No No	No No	No Yes
Mother × Controls	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes	No Yes
Bandwidth	30 Triangular	30 Triangular	30 Triangular	30 Triangular	30 Triangular	30 Triangular	30 Triangular	30 Triangular	30 Triangular
Kernel	1st 1st	1st 1st	1st 1st	1st 1st	1st 1st	1st 1st	1st 1st	1st 1st	1st 1st
Observations	42,430 Polynomial degree	42,430 1st	42,430 1st						

This table reports the results of OLS regressions on the baseline regression from Table 4, except we add a range of different controls. Column 1 repeats the baseline regression from Table 4, except we add a range of different controls. Column 1 adds the same sample sizes as we weighted and rounded to the nearest multiple of five as required by Statistics Canada. Column 9 allows the value of the beta coefficients, standard errors and p-values by increasing each of them with a multiplier. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. This table reports the results of OLS regressions on the baseline regression from Table 4, except we add a range of different controls. Column 1 repeats the baseline regression from Table 4, except we add a range of different controls. Column 1 adds the same sample sizes as we weighted and rounded to the nearest multiple of five as required by Statistics Canada. This table reports the results of OLS regressions on the baseline regression from Table 4, except we add a range of different controls. Column 1 repeats the baseline regression from Table 4, except we add a range of different controls. Column 1 adds the same sample sizes as we weighted and rounded to the nearest multiple of five as required by Statistics Canada.

**Table 8**  
**Heterogeneity across provinces**

Dependent variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0194*** (0.0060)	0.0210*** (0.0061)	0.0199*** (0.0064)	0.0193*** (0.0074)
$\Delta$ Weeks leave $\times$ Treatment	0.0116 (0.0071)	0.0171*** (0.0061)	0.0180*** (0.0049)	0.0138*** (0.0021)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	86,565	71,285	57,405	42,430

This table examines whether our main results differ across provinces with different lengths of leave extension. We repeat the analysis of Table 4, allowing all variables to interact with the change in available weeks of leave in an individual's province. We standardize the change in available weeks of leave by subtracting its mean and dividing by its standard deviation. The coefficient for *Treatment*, without an interaction, now represents the difference-in-discontinuities estimate in a province experiencing the average change in available weeks of leave (i.e., standardized  $\Delta$ Weeks Leave=0). The coefficient for the interaction term,  $\Delta$ Weeks Leave  $\times$  Treatment, represents how the difference-in-discontinuities estimate varies as the change in available leave time deviates from its mean (in units of standard deviations). Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

## 5.4 Heterogeneity Across Provinces

As shown in Table 2, there is some heterogeneity across provinces in how much additional job-protected leave time was available after the cutoff date. For example, in Alberta, available leave time increased by 34 weeks, while in Ontario, it only increased by 17 weeks. Motivated by this, we examine whether the reform had a larger effect in provinces experiencing a bigger change.

In Table 8, we estimate a variant of Equation (3), in which we interact all variables with the change in available weeks of leave in an individual's province. To make the estimates easier to interpret, we standardize the change in available leave time by subtracting its mean and dividing by its standard deviation. Thus, the coefficient for *Treatment<sub>i</sub>*, without an interaction, now represents the difference-in-discontinuities estimate corresponding to a province experiencing the average change in available weeks of leave (i.e., standardized  $\Delta$ Weeks Leave<sub>*i*</sub>=0). As we would expect, this coefficient matches the effect estimated in the baseline regression from Table 4. The coefficient on the interaction term,  $\Delta$ Weeks Leave<sub>*i*</sub>  $\times$  Treatment<sub>*i*</sub>, represents how the difference-in-discontinuities estimate varies as the change in available leave time deviates from its mean (in units of standard deviations).<sup>20</sup> We find that the estimated treatment effect is larger in provinces with larger increases in leave time. This is consistent with the predictions of our conceptual framework. In addition, it lends further support to the idea that our baseline results are indeed driven by changes in job-protected leave, as opposed to some other mechanism.

<sup>20</sup> To be clear, Treatment<sub>*i*</sub> corresponds to Mother<sub>*i*</sub>  $\times$  Post<sub>*i*</sub> in a variant of Equation (3) in which all variables are interacted with the standardized change in weeks of leave available in an individual's province;  $\Delta$ Weeks Leave<sub>*i*</sub>  $\times$  Treatment<sub>*i*</sub> corresponds to  $\Delta$ Weeks Leave<sub>*i*</sub>  $\times$  Mother<sub>*i*</sub>  $\times$  Post<sub>*i*</sub>.

## 5.5 Business quality

We next consider the types of firms that these individuals are starting: are they small, transitory ventures or do they engage in substantive economic activities? The first evidence on this point is that we measure businesses that still exist 5 years after the cutoff date. If the reform only increased transitory, negligible ventures, we might expect to see no long-run effects because these businesses would cease operating within that time frame.

To further explore the quality of these businesses, we use additional information reported in the census. Our primary measure of entrepreneurship thus far has been based on self-employment income. Respondents to the long-form census questionnaire also self-report whether they are self-employed. If they identify themselves as self-employed, they further report additional information about their business.

We begin by ensuring that our results are robust to using self-reported entrepreneurship as the dependent variable. In the first column of Table 9, panel A, we reestimate Equation (3), with self-reported entrepreneurship. The results are similar to our main estimates from Table 4. The magnitudes are larger, but the mean of this dependent variable is also higher than our baseline measure. Next, we decompose this self-reported entrepreneurship variable into two separate variables: an indicator equal to one if the individual reports being self-employed with an incorporated business, and an indicator equal to one if the individual reports being self-employed with an unincorporated business. In columns 2 and 3, we find that the reform increases incorporated entrepreneurship but has no significant effect on unincorporated entrepreneurship. As expected, the coefficients in columns 2 and 3 sum to the coefficient in column 1. Similarly, in columns 4 and 5, we decompose self-reported entrepreneurs into those that hire paid employees and those that do not. We find that the reform increases job-creating entrepreneurship but has no effect on non-job-creating entrepreneurship. In panel B, we examine work hours and income. In this case, we define a high-hour entrepreneur to be one working above the median number of hours, and a low-hour entrepreneur to be one working below the median number of hours. We similarly define a high-earning entrepreneur to be one earning above the median income, and a low-earning entrepreneur as the complement. We find that the reform leads to an increase in high-hour and high-earning entrepreneurs, but leads to no change in low-hour or low-earning entrepreneurs.

It also should be noted that we may be limited in our ability to detect low-quality entry, as low-quality entrants are less likely to still be operating at the point when we can observe them (5 years later). Related to this, we do not find the *difference* between the effect of job-protected leave on high-quality and low-quality entrepreneurship to be statistically significant in most cases (as indicated by the *p*-values reported at the bottom of each panel). Thus, the main finding from Table 9 is that the reform does appear to increase high-quality entrepreneurship, not that it increases high-quality entrepreneurship more than low-quality entrepreneurship.

**Table 9**  
**Characterizing entrants based on self-reported information**

*A. Incorporation and employees*

Dependent variable:	Self-reported entrepreneur				
	All	With incorp.	Without incorp.	With employees	Without employees
(1)	(2)	(3)	(4)	(5)	
Treatment	0.0274** (0.0125)	0.0164*** (0.0040)	0.0110 (0.0105)	0.0151** (0.0070)	0.0123 (0.0105)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st	1st
Difference p-val		.585			.827
Observations	42,430	42,430	42,430	42,430	42,430

*B. Income and hours*

Dependent variable:	Self-reported entrepreneur				
	All	With high hours	With low hours	With high income	With low income
(1)	(2)	(3)	(4)	(5)	
Treatment	0.0274** (0.0125)	0.0310*** (0.0080)	-0.0036 (0.0102)	0.0205*** (0.0059)	0.0069 (0.0141)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1	1	1	1	1
Difference p-val		.010			.439
Observations	42,430	42,430	42,430	42,430	42,430

This table characterizes entrants based on self-reported information. Column 1 of both panels repeats the analysis reported on in column 4 of Table 4 but defines an individual to be an entrepreneur if they report themselves as self-employed based on their primary job. The dependent variable in column 2 of panel A is an indicator variable equal to one if an individual is a self-reported entrepreneur who has incorporated his/her business. The dependent variable in column 3 of panel A is an indicator variable equal to one if the individual is a self-reported entrepreneur who has not incorporated his/her business. Columns 4 and 5 of panel A decompose self-reported entrepreneurship based on whether individuals hire paid employees. Columns 2 and 3 of panel B decompose self-reported entrepreneurship based on whether individuals report working above or below the median number of hours. Columns 4 and 5 of panel B decompose self-reported entrepreneurship based on whether individuals report earning above or below the median income. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \* $p < .1$ , \*\* $p < .05$ ; \*\*\* $p < .01$ .

While these findings do not necessarily indicate that the reform led to so-called “transformational entrepreneurs” (Schoar 2010; Hurst and Pugsley 2011), they do seem to indicate the creation of meaningful businesses. Moreover, even if one were skeptical about the quality of these businesses, understanding the relation between career risk and entrepreneurial entry remains important. The role of career risk may generalize beyond the particular group we study in this paper. For example, one could imagine that if large technology companies in Silicon Valley gave their employees a similar option to take long job-protected leaves, such a policy might lead to the creation of technology startups.

## 5.6 Job protection versus income replacement

As discussed in Section 2, the reform that we study simultaneously increased the job protection period and the income-replacement period. However, despite the presence of income replacement, it is important to note that our results are distinct from the literature on financial constraints and entrepreneurship.

Employment insurance only provided 55% income replacement up to a maximum of \$413 CAD per week (about US\$275). Thus, the reform does not represent a positive wealth shock, as people earn significantly *lower* income while on leave. If someone had an idea but insufficient capital to pursue it, she would still have insufficient capital while on leave.

Nonetheless, we consider the possibility that the increase in the income-replacement period drives our results, rather than the increase in the job protection period. First, we note that we find stronger effects in provinces that had larger increases in the job protection period. If changes in the income-replacement period were driving our results, we would not expect this, as all provinces had the same increase in the income replacement period. Second, to further disentangle the effects of job protection and income replacement, we repeat our baseline analysis using the sample of parents who gave birth in Quebec. Quebec increased job-protected leave to 70 weeks many years earlier and did not change it along with the other provinces. Thus, a mother who gave birth just after December 31, 2000, in Quebec would be eligible for a longer income replacement period than one who gave birth before, but not a longer job protection period. In Table A.8 in the Internet Appendix, we reestimate our baseline specification using the sample of parents who gave birth in Quebec. We find insignificant effects of the reform in this case.

Overall, these results suggest that an income replacement extension alone is not enough to spur additional entrepreneurship. However, we acknowledge that we do not know if an extension in job protection alone would be enough to spur entrepreneurship either. This is difficult to test, as we do not have a situation in which there is an extension in job-protected leave that is not paired with an extension in income replacement. We speculate that reducing career risk may spur entrepreneurship more when individuals have access to enough resources to cover their living expenses without wage income for a period of time. In that case, the effect of job protection alone would depend on how many potential entrepreneurs found the income replacement provided by the program (55% up to a max of US\$275 per week) necessary for covering their living expenses while on leave. Regardless of whether job protection alone would have an effect, our results indicate that the job protection component of the policy is needed to spur entrepreneurship. This suggests that career risk does indeed play a key role in deterring potential entrepreneurs.

## 5.7 Short-run effects

One weakness of the census is that it only allows us to examine long-run effects. It is also interesting to consider whether there are effects in the short run, although this does not necessarily distinguish between the direct and indirect mechanisms. Neither mechanism necessarily implies immediate entry into entrepreneurship following a job-protected leave; that is, individuals could develop or explore entrepreneurial ideas and preferences during a job-protected

leave, but then return to their job for some time before actually launching a business.

To examine short-run effects, we use the Longitudinal Administrative Databank (LAD). The LAD is another confidential data set provided by Statistics Canada. It is based on a random sample of individual tax filings and has an annual frequency.<sup>21</sup> Once an individual is included in the LAD, the individual remains in the sample and can be consistently identified over time. The standard version of the LAD does not provide information on children's birthdates. However, for this project, Statistics Canada created a customized version of the data set that includes children's birthdates.<sup>22</sup> The primary weakness of the LAD relative to the census is that it only provides tax-related data. Therefore, it lacks demographic information about workers. It also lacks self-reported information about the extent and nature of their entrepreneurial activities. We thus use the census for our primary analysis but use the LAD to explore short-run effects.

One additional complication in looking at short-run effects is that new businesses may not generate substantial income early on and likely even generate losses in many cases. Therefore, to detect short-run entry as well as possible in the LAD, we broaden our definition of entrepreneurship. Specifically, we consider an individual to be an entrepreneur if she reports any net self-employment income, positive or negative, on her tax return.<sup>23</sup> Using this definition of entrepreneurship, we then repeat our baseline analysis in 2002, the first full calendar year following our birth window. Because the LAD has a panel dimension, in this case we can also narrow our focus to the most relevant group for our hypothesis, namely, those who were exclusively wage employed prior to childbirth. Accordingly, we limit the sample to individuals who had wage employment income in the year prior to the cutoff date and who also did not have any net self-employment income (positive or negative) in that year. As can be seen in Table 10, panel A, we continue to find a significant effect in the short run. Among individuals who were exclusively wage employed prior to childbirth, those who qualified for the extended job-protected leave were more likely to enter entrepreneurship immediately after.

In panel B, we also take advantage of the annual frequency of the LAD to examine the dynamics of our estimated effects. We repeat the analysis of Table 10 in each year from 2001 to 2005 (the latter of which corresponds to the 2006 census data).<sup>24</sup> In 2001, we find a smaller, marginally significant

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<sup>21</sup> The random sampling for the LAD is completely independent of the random sampling for the long-form census.

<sup>22</sup> Specifically, Statistics Canada was able to add child birthdates to the data set for a large subset of individuals, starting in 2006. We used this information to backfill previous years.

<sup>23</sup> Negative net self-employment income can be deducted from other positive income on an individual's tax return from the same year. It can also be carried back for up to 3 years and carried forward for up to 7 years. To claim negative net self-employment income, one's business must have a reasonable expectation of becoming profitable and must clearly be commercial in nature.

<sup>24</sup> The 2006 census was conducted on May 16, 2006, and the relevant questions for us (e.g., self-employment income) refer to the previous calendar year.

**Table 10**  
**Short-run effects**

A. Short-run effects

Dependent variable:	Entrepreneur in 2002			
	(1)	(2)	(3)	(4)
Treatment	0.0558*** (0.0118)	0.0594*** (0.0136)	0.0500*** (0.0149)	0.0461*** (0.0100)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	56,200	46,305	37,065	27,300

B. Dynamics

Dependent variable:	Entrepreneur in				
	2001	2002	2003	2004	2005
	(1)	(2)	(3)	(4)	(5)
Treatment	0.0107* (0.0059)	0.0461*** (0.0100)	0.0598*** (0.0182)	0.0337*** (0.0073)	0.0276** (0.0122)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st	1st
Observations	27,300	27,300	27,300	27,300	27,300

This table examines the short-run effects of increased job-protected leave availability on entrepreneurship using a differences-in-discontinuities framework. It estimates the following local linear regression with bandwidths ranging from 60 to 30 days and a triangular kernel:

$$\begin{aligned} Entrepreneur_i &= Mother_i \times [\alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i] \\ &\quad + [a + t Post_i + d_1 DayChild_i + d_2 Post_i \times DayChild_i] + \epsilon_i, \end{aligned}$$

where observations are at the parent level; the sample consists of parents in the Longitudinal Administrative Databank (LAD) who had a first child (excluding multiples) within a specified bandwidth around the December 31, 2000, cutoff date and who were exclusively wage-employed (received wage employment income and did not report any net self-employment income) in the year prior to childbirth;  $Entrepreneur_i$  is an indicator equal to one if an individual reports any net self-employment income, positive or negative, on their tax return;  $DayChild_i$  represents the date parent  $i$ 's child was born relative to the reform cutoff date; and  $Post_i$  is an indicator equal to one if parent  $i$  had a child on or after the cutoff date. The row labeled  $Treatment$  in the table corresponds to the coefficient  $\tau$  in the above equation and represents the estimated average treatment effect. Panel A estimates the effects for year 2002 using various bandwidths. Panel B estimates the dynamic effects in each year from 2001 to 2005. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

effect. This makes sense, as many individuals in our sample were likely on job-protected leave during most, or all, of that year. To the extent that they explored a business idea, it may not have shown up on their tax returns. As panel A showed, we find a much larger effect in 2002. The effect continues to increase in 2003, as additional businesses become detectable.<sup>25</sup> In 2004 and 2005, the effect diminishes somewhat, presumably reflecting business failures. In terms of magnitudes, the coefficients that we estimate here are larger than in our baseline

<sup>25</sup> Because we define entrepreneurship status based on income reported in tax filings, there may be a lag in terms of when businesses become detectable in our data. In other words, someone affected by longer job-protected leave may start a business in 2002, but we may only observe the business in 2003 if it did not generate net self-employment income before then. In addition, someone affected by a longer job-protected leave could start a business after some delay. For example, someone affected by longer job-protected leave may return to wage employment in 2002 in order to save money and then formally launch a business in 2003.

analysis. However, under the broader definition of entrepreneurship that we use in this analysis to detect short-run entry, the base rate of entrepreneurship among parents is roughly double that in our baseline analysis (8.7% among mothers, 13.5% among fathers). Thus, in percentage terms, the magnitudes converge to a similar range by 2005.

The LAD also allows us to observe income from employment insurance payments. As a “sanity check,” in Table A.9 in the Internet Appendix, we repeat the analysis reported on in Table 10, panel B, using employment insurance income (which includes parental leave payments) as the outcome variable. As we would expect, we find a positive effect of the reform on employment insurance income in 2000–2001, but no effect in the subsequent years.<sup>26</sup> In terms of magnitudes, the coefficients suggest a 67% increase in employment insurance income in 2000–2001, which in turn implies a 67% increase in the length of leave that individuals have taken.<sup>27</sup> These results further help to validate our empirical strategy.

## 6. Mechanism

We hypothesize two mechanisms that could generate these results. First, individuals with entrepreneurial ideas and preferences could use longer leaves to explore their ideas without exposure to career risk. This would be a *direct* mechanism through which job-protected leave availability increases entrepreneurship. Internet Appendix B provides a model to illustrate formally how such a mechanism might operate. Second, individuals who do not have entrepreneurial ideas or preferences may instead use longer leaves to pursue nonentrepreneurial objectives (e.g., parental objectives) without career risk. However, such individuals may then develop entrepreneurial ideas and/or preferences as a result of their additional time away from wage employment. This would be an *indirect* mechanism through which job-protected leave availability increases entrepreneurship. These two mechanisms are closely related, with the main difference between them having to do with individuals’ initial motivations for making use of additional job-protected leave time. Moreover, both would represent a causal effect of reduced career risk on entrepreneurship. In this section, we conduct a number of tests to distinguish empirically between the direct and indirect mechanisms and find greater support for the direct mechanism.

### 6.1 Ruling out the indirect mechanism

We begin by testing predictions of the indirect mechanism. Under the indirect mechanism, one would likely expect to see increased job-protected leave time

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<sup>26</sup> For this analysis, we combine insurance payments from 2000 and 2001 since workers in our sample who had a child before the cutoff date would have taken paid leave spanning both years.

<sup>27</sup> The reform did not change the income replacement percentage (or cap), it only changed the maximum income replacement period.

**Table 11**  
**Ruling out the indirect mechanism: Changes in industry, earnings, and location**

*A. Industry change*

Dependent variable:	Industry change from 1999 in			
	2002	2003	2004	2005
	(1)	(2)	(3)	(4)
Treatment	-0.0222 (0.0219)	0.0549 (0.0406)	0.0352 (0.0483)	0.0366 (0.0589)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	22,725	20,925	19,500	18,375

*B. Significant wage change*

Dependent variable:	Industry wage change from 1999 in			
	2002	2003	2004	2005
	(1)	(2)	(3)	(4)
Treatment	-0.0355* (0.0207)	-0.0169 (0.0134)	-0.0724*** (0.0160)	0.0072 (0.0070)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	22,730	20,930	19,490	18,380

*C. Location change*

Dependent variable:	Metro area	Census division	Census subdivision	Address
	change	change	change	change
	(1)	(2)	(3)	(4)
Treatment	0.0139 (0.0289)	-0.0086 (0.0248)	-0.0060 (0.0229)	-0.0350 (0.0233)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	22,960	22,960	22,960	22,960

This table examines the effect of increased job-protected leave availability on the likelihood that a parent switches industry (panel A) or experiences significant wage changes (panel B) in years 2002 to 2005 relative to the prereform year 1999. Panel C examines the effect of increased job-protected leave availability on a parent's likelihood of changing residential location in 2006 Census relative to his/her location 5 years ago. Panels A and B are estimated from LAD. The samples consist of individuals who were wage-employed in 1999 and continuously wage-employed from 2002 to the focal year. The specification is the same as that in panel B of Table 10. Industry is defined at the three-digit NAICS level. An individual experiences a significant wage change if the absolute value of his/her percentage wage change from 1999 is above the 75th percentile. Panel C is estimated from the 2006 Census. The sample consists of individuals who were self-employed as of the 2006 Census. The specification is the same as that in Table 4. The outcomes are dummy variables indicating location changes at different geographical levels. Standard errors are clustered by week. \* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

lead to other types of career switches as well, not just career switches into entrepreneurship. While we cannot observe individuals' employers or job titles in our data, we can observe the industry in which they work, their earnings, and their geographic location. It is reasonable to think that those switching careers would experience changes along one or more of these dimensions. However, we do not find evidence of this. Panel A of Table 11 shows that, among those who remained in wage employment after childbirth, the ones who had access to

the extended leave time were no more likely to switch from their prechildbirth industry (as measured by three-digit NAICS code). Panel B also shows that they were no more likely to experience a large (top-quartile) absolute percentage change in earnings from prechildbirth levels. Finally, panel C shows that they were no more likely to move from their prechildbirth location (as measured by metro area, census division, census subdivision, or address).<sup>28</sup> Overall, this evidence points away from the indirect mechanism.

We also note that the results on business quality presented in Section 5.5 point away from the indirect mechanism as well. In particular, we find a significant increase in high-quality entrepreneurship (i.e., incorporated, hire paid employees, high hours, high income) but no significant increase in low-quality entrepreneurship. We may be limited in our ability to detect low-quality entry, as low-quality entrants might be less likely to be still operating at the point when we can observe them (5 years later). However, low-quality entrants in fields where the risk of failure is low (e.g., driving for Uber) still could be operating at the point when we can observe them. Therefore, under the indirect mechanism, we would expect to find some increase in this type of entrepreneurship. The fact that we do not is more consistent with the direct mechanism, as job-protected leaves are not necessary for exploring low-risk entrepreneurship.

## 6.2 Ruling out the indirect mechanism

We also note that the results on business quality presented in Section 5.5 point away from the indirect mechanism as well. In particular, we find a significant increase in high-quality entrepreneurship (i.e., incorporated, hire paid employees, high hours, high income) but no significant increase in low-quality entrepreneurship. We may be limited in our ability to detect low-quality entry, as low-quality entrants might be less likely to be still operating at the point when we can observe them (5 years later). However, low-quality entrants in fields where the risk of failure is low (e.g., driving for Uber) still could be operating at the point when we can observe them. Therefore, under the indirect mechanism, we would expect to find some increase in this type of entrepreneurship. The fact that we do not is more consistent with the direct mechanism, as job-protected leaves are not necessary for exploring low-risk entrepreneurship.

## 6.3 Ruling in the direct mechanism

Next, we conduct tests related to the direct mechanism. Under the direct mechanism, we would expect stronger effects when experimentation prior to entry has low costs or high benefits (Kerr, Nanda, and Rhodes-Kropf

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<sup>28</sup> Panels A and B use panel data from the LAD, which allows us to observe prechildbirth industry and earnings. Panel C uses cross-sectional data from the 2006 Census. We proxy for prechildbirth location from the Census data based on the questions about where respondents lived 5 years prior.

2014; Ewens, Nanda, and Rhodes-Kropf 2018). We expect the costs of experimentation to be low in industries with low startup capital requirements. In such industries, it is relatively inexpensive to obtain information about the prospects of a business idea. As discussed above, we expect the benefits of experimentation to be high in industries with high failure rates. In such industries, the downside protection provided by job-protected leave will be more valuable.

To operationalize these ideas, we obtain data on industry-level startup capital requirements from the Survey of Business Owners, following Adelino, Schoar, and Severino (2015). We also obtain data on industry-level 5-year failure rates from Bureau van Dijk's Orbis database.<sup>29</sup> Industries for failure rates are based on three-digit NAICS codes and industries for startup capital are based on two-digit NAICS codes, the finest level available in the Survey of Business Owners. We then decompose our dependent variable according to each of these measures. For startup capital requirements, we define an indicator variable equal to one if an individual is an entrepreneur working in a high-startup-capital industry and another if the individual is an entrepreneur working in a low startup capital industry. These two variables add to our original dependent variable. We categorize industries based on the median among the entrepreneurs in our sample. Thus, we categorize roughly half of the entrepreneurs in our sample as high-startup-capital entrepreneurs and half as low-startup-capital entrepreneurs. This helps ensure no mechanical tendency for the reform to increase one type of entrepreneurship more than the other based on the general prevalence of that type of entrepreneurship.<sup>30</sup> We decompose entrepreneurship based on industry failure rates analogously.<sup>31</sup>

Table 12 shows the results using these decompositions. In columns 1 and 2, we find that extending job-protected leave increases low-startup-capital entrepreneurship, but has no effect on high-startup-capital entrepreneurship. The difference is significant at  $p < 0.1$ . In columns 3 and 4, we find that extending job-protected leave increases high-risk entrepreneurship, but has no effect on low-risk entrepreneurship. In this case, the difference is significant at  $p < 0.01$ . Overall, this evidence points more toward the direct mechanism.

Finally, if the direct mechanism drives our results, the effects should be stronger among those facing greater career risk associated with entrepreneurial failure. Consider someone who works in an industry where leaving for entrepreneurship and failing is very costly: if she were to try entrepreneurship

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<sup>29</sup> For each industry in Orbis, we compute failure rates for private firms during their first 5 years of existence.

<sup>30</sup> In Table A.10 in the Internet Appendix, we also show that results remain very similar when we exclude the five most prevalent industries among entrepreneurs from the analysis. Table A.11 in the Internet Appendix shows the distribution of marginal entrepreneurs across industries.

<sup>31</sup> Examples of low startup capital industries in our sample include professional services, construction, retail trade, and educational services; examples of high failure rate industries include professional services, transportation and warehousing, performing arts, and environmental services.

**Table 12**  
**Ruling in the direct mechanism: Characterizing entrants based on industry characteristics**

Dependent variable:	Entrepreneur			
	In high startup cap. industry	In low startup cap. industry	In high failure rate industry	In low failure rate industry
	(1)	(2)	(3)	(4)
Treatment	0.0006 (0.0047)	0.0187*** (0.0071)	0.0270*** (0.0045)	-0.0077 (0.0050)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Difference <i>p</i> -val	.0558		.0000	
Observations	42,430	42,430	42,430	42,430

This table characterizes entrants into entrepreneurship based on industry characteristics. It repeats the analysis reported on in column 4 of Table 4, except we decompose the dependent variable. The dependent variable in column 1 is an indicator variable equal to one if an individual is an entrepreneur working in a high-startup-capital industry (startup capital requirements above the median among the entrepreneurs in our sample). The dependent variable in column 2 is an indicator variable equal to one if the individual is an entrepreneur working in a low-startup-capital industry (startup capital requirements below the median among the entrepreneurs in our sample). Columns 3 and 4 decompose entrepreneurship based on industry failure rates analogously. Industry startup capital requirements are defined as the mean of startup requirements for firms in a two-digit NAICS industry from the Survey of Business Owners. Industry failure rates are defined as 5-year failure rates for private firms in a three-digit NAICS industry from Bureau van Dijk's Orbis database. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week.

\**p* <.1; \*\**p* <.05; \*\*\**p* <.01.

and then return to her former industry, she would face a high wage penalty. In contrast, someone else might work in an industry where this career detour would have a low penalty. Someone in the former industry would be more worried about the career consequences of entrepreneurial failure, and—under the direct mechanism—she would benefit more from the opportunity to experiment without penalty. In contrast, under the indirect mechanism, we would not necessarily expect to find stronger effects in these industries, since this mechanism assumes workers are not explicitly choosing to experiment.

This idea provides another empirical test that we employ. We use the panel data from LAD to estimate each industry's wage penalty associated with leaving for entrepreneurship and then returning. For each industry (three-digit NAICS), we compute the median wage change experienced by those who left for self-employment and then returned, relative to the median wage change experienced by those who remained continuously wage-employed over the same time period.<sup>32</sup> We then repeat our baseline LAD analysis, allowing for the possibility of differential effects among those who were wage-employed in high (above-median) and low (below-median) entrepreneurship-penalizing

<sup>32</sup> This wage penalty corresponds to the parameter *d* in our model in Internet Appendix ???. Of course, the decision to leave for entrepreneurship and then return is endogenous. This could lead us, for example, to underestimate the entrepreneurship wage penalty, as those who face a high penalty may choose not to return. However, we are only interested in the relative ranking of industries, which may be less affected by such endogeneity issues.

**Table 13**  
**Ruling in the direct mechanism: Heterogeneity in career risk from entrepreneurial failure**

**A. Industry-level wage penalty for entrepreneurship**

Dependent variable:	Entrepreneur in			
	2002	2003	2004	2005
	(1)	(2)	(3)	(4)
Treatment	0.0242*** (0.0081)	0.0034 (0.0284)	-0.0326** (0.0155)	0.0175 (0.0203)
Treatment × High ent. penalty	0.0518*** (0.0197)	0.1167*** (0.0241)	0.1489*** (0.0221)	0.0463*** (0.0115)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial degree	1st	1st	1st	1st
Observations	27,300	27,300	27,300	27,300

**B. Education**

Dependent variable:	Entrepreneur	
	College degree	No college degree
Subsample:	(1)	(2)
Treatment	0.0751*** (0.0220)	0.0010 (0.0060)
Bandwidth	30	30
Kernel	Triangular	Triangular
Polynomial degree	1st	1st
p-value of difference		.0092
Observations	11,835	30,595

Panel A of this table examines heterogeneity in our LAD results across industries with different wage discounts associated with time away from wage employment in self-employment. For each three-digit NAICS industry, we estimate the median wage discount upon returning to wage employment after 4 years of self-employment, and then subtract the median wage change associated with continuous wage-employment over the same horizon. We define an industry as *High entrepreneurship penalty* if the relative wage discount from self-employment spell is above median across all industries. We then interact *Treatment* with *High entrepreneurship penalty*. The sample and specification are the same as those in panel B of Table 10, except for the interaction. Panel B of this table examines how the effect of increased job-protected leave availability varies across individuals with and without a college degree. It repeats the analysis reported on in column 4 of Table 4 for each subsample. *p*-values indicate the significance of the difference in the estimated treatment effects across complementary subsamples. Sample sizes are weighted and rounded to the nearest multiple of five as required by Statistics Canada. Standard errors are clustered by week. \**p* < .1; \*\**p* < .05; \*\*\**p* < .01.

industries prior to childbirth. Table 13, panel A, shows the results.<sup>33</sup> As can be seen, we find that the effect of job-protected leave on entrepreneurship is indeed concentrated in high entrepreneurship-penalizing industries.

Along similar lines, one might also expect that individuals with a college degree face greater career risk associated with entrepreneurial failure than those without a college degree. Thus, if the direct mechanism drives our results, we would expect our results to be stronger among college-educated workers. To test this idea, we repeat our baseline analysis among subsamples of individuals with and without a college degree. Table 13, panel B, shows that the effect of

<sup>33</sup> We focus on the wage penalty associated with a 4-year self-employment spell. Our results are similar if we use 2- or 3-year windows.

job-protected leave is significantly stronger among those with a college degree, providing further evidence in support of the direct mechanism.<sup>34</sup>

## 7. Conclusion

Choosing to start a business is an inherently risky proposition. In this paper, we highlight the importance of one particular type of risk: the downside risk that an entrepreneur faces when giving up alternative employment. If a potential entrepreneur starts a venture that ultimately fails, it is difficult to obtain as good a job as the one she could have otherwise had. We have adduced evidence that this phenomenon is indeed a relevant consideration for potential entrepreneurs, by showing the effect of job-protected leave availability. When Canadian mothers are granted the ability to take extended leaves of absence, during which they are guaranteed the option to return to their job, their entry into entrepreneurship increases. They enter in industries where the cost of experimentation is low and downside protection appears most valuable. The resultant businesses are economically meaningful, as our results are not driven by new businesses that quickly fail. Instead, the entrepreneurs that are spurred to enter tend to incorporate their business and hire paid employees. We conclude that potential entrepreneurs are indeed concerned about their downside risk in the event they want to return to paid employment.

These results suggest a key role for well-functioning labor markets in facilitating entrepreneurship. Potential entrepreneurs are also potential employees (Grombok and Scharfstein 2002). It is much easier to take a big risk with one's career when a good fallback option is in place. We show that job-protected leave can provide this fallback option in some circumstances. Flexible and well-functioning labor markets can do the same, and may, therefore, play an important role in facilitating entrepreneurship.

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<sup>34</sup> Another potential interpretation of this result could be that those with a college degree are more capable entrepreneurs and thus better able to take advantage of job-protected leave when it is available.

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