Does Career Risk Deter Potential Entrepreneurs?[*]

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Abstract

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 extended 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 longer job-protected leave increases entrepreneurship by 1.9 percentage points. These entrepreneurs start incorporated businesses that hire employees—in industries where experimentation before entry has low costs and high benefits. The effects are concentrated among those with more human and financial capital.

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1 Introduction

Entrepreneurship has long been thought to play a critical role in innovation, job creation and economic growth (Schumpeter, 1911). There is now a large body of empirical evidence in support of 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 percent of the labor force is self-employed (World Bank, 2015).

While regulation and capital access are well-known impediments to starting a business,[1] perhaps the most fundamental reason people might avoid setting out on their own is the risk involved. 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 re-enter wage employment, end up on a worse career trajectory.[2] 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

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

[2] 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-Moller, 2011; Baptista, Lima, and Preto, 2012). Many have also argued that the stigma of entrepreneurial failure discourages entry and explains much of 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, there are also likely search costs and other frictions involved in leaving wage employment and re-entering.

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 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.

We focus on a reform to Canadian maternity leave laws that took place in 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.

Indeed, anecdotal evidence suggests that entry into entrepreneurship among Canadian women increased 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 five years later.

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 (see, 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 that there are 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 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 there are no obvious confounds that 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 five years later. The 2001 Census is too close to the reform's cutoff date, which prevents us from observing the short-run dynamics induced by the reform. But the benefit of looking at long-term outcomes is that the results cannot merely reflect transitory entry into entrepreneurship. 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 placebo cutoff dates in other years. The results are concentrated among individuals with more human and financial capital, who may be more willing or able to start a business. Also, consistent with the mechanism we have in mind, the new businesses that are spurred by the reform tend to be in industries where exploring an idea prior to fully committing has low costs (i.e., low startup capital requirements) or high benefits (i.e., high failure rates, hence high real option value).

Several pieces of evidence suggest that the incremental entrepreneurs we observe are engaging in meaningful businesses. First, we measure entrepreneurship continuing five years after the reform. If the reform only increased transitory entrepreneurship, we would not expect to see long-run effects because the marginal businesses would cease operating within that time frame. Further, we find that the effect of the reform on entrepreneurship is significantly stronger for those with *ex ante* characteristics that predict higher-quality businesses. In particular, those with more education, work experience, and access to capital respond more strongly to the reform. Finally, 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.

As a general matter, 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 non-pecuniary benefits may be an important motivation for many potential entrepreneurs, not just new mothers (e.g., Hurst and Pugsley, 2011). Non-pecuniary motivations likely coexist with concerns about career risk and other factors influencing the entrepreneurship decision. Just as there is uncertainty about whether one can create a business that generates desired financial payoffs, there is also uncertainty about whether one can create a business that generates desired non-pecuniary benefits. Thus, the ability to work toward creating such a business in the absence of career risk may encourage entrepreneurship.

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 gave their employees a similar ability to take long job-protected leaves—for the explicit purpose of working on a business idea—such a policy might lead to the creation of technology startups. 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 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 potential entrepreneurs are deterred by worries about career risk.

Our paper contributes 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 NPV in a static sense, entrepreneurs can engage in cheap experiments that reveal information about a project's prospects. Conditional upon that information being favorable, the project may become positive NPV; thus, there is value in the real option to continue. In closely related work, Manso (2016) and Dillon and Stanton (2016) model the dynamics of experimentation in self-employment and quantify this option value. 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, 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 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 has also examined whether relaxing financial constraints increases entrepreneurship (Evans and Jovanovic, 1989; Holtz-Eakin, Joulfaian, and Rosen, 1994a,b; Hurst and Lusardi, 2004; Bertrand, Schoar, and Thesmar, 2007; Mel, McKenzie, and Woodruff, 2008; Kerr and Nanda, 2009; Adelino, Schoar, and Severino, 2015; Schmalz, Sraer, and Thesmar, 2017); whether entrepreneurship training programs or exposure to entrepreneurial peers increases entrepreneurship (Karlan and Valdivia, 2011; Lerner and Malmendier, 2013; Drexler, Fischer, and Schoar, 2014; Fairlie, Karlan, and Zinman, 2015); and whether allowing unemployed individuals to continue receiving payments from unemployment insurance after starting a business increases entrepreneurship (Hombert et al., 2014).[3] Our paper differs in its focus on career risk. We are not aware of any other work examining whether potential entrepreneurs hesitate to enter because they are afraid to worsen their fallback option. 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.

2 Conceptual Framework

2.1 Model Setup

In order to fix ideas, we present a stylized model of the entrepreneurial entry decision in our context. The model describes how the choice to explore entrepreneurship can respond to changes in job-protected leave policy when workers worry about career risk.

[3] While our paper is superficially like Hombert et al. (2014) in that we study a form of employment insurance, we differ in our focus on career risk. The reform that Hombert et al. (2014) study only applies to unemployed individuals who do not face career risk in starting a business. Their focus is instead on learning about the quality of the entrepreneurs who are spurred to enter by that reform relative to the quality of other entrepreneurs.

At time 0, an employed worker chooses the length of leave she will take, t. The worker may use this time to experiment with entrepreneurship. The maximum length of available job-protected leave guaranteed by law is denoted by T_L. The benefit of taking a longer leave is that the potential entrepreneur receives a more precise signal about X, the payoff to entrepreneurship. This payoff is the present value of future earnings conditional on becoming an entrepreneur after the leave period ends, plus any non-pecuniary benefits from entrepreneurship. We assume this payoff follows a normal distribution, $X \sim N\left(\mu, \frac{1}{\eta_0}\right)$. At the end of the leave, the worker receives an unbiased signal $s(t)$ about X, with signal precision increasing in t. That is, $s(t)|X \sim N\left(X, \frac{1}{h(t)}\right)$, where $h(0) = 0$, $h^{'}(t) > 0$, and $h^{''}(t) < 0$. This captures the idea that experimentation reduces uncertainty about entrepreneurial payoffs but has decreasing marginal benefits. In what follows, we assume the specific functional form $h(t) = \sqrt{t}$. Of course, if the job-protected leave is associated with the birth of a child, it may be unrealistic to assume that the marginal benefits of leave time for increasing signal precision are highest at the start of the leave. To account for this, simply think of a non-discretionary amount of medical/childcare leave time where no entrepreneurial experimentation occurs prior to time 0 in the model.

When the leave ends at time t, the worker updates her beliefs about the distribution of entrepreneurship payoffs, X, based on the realization of her signal $s\,(t)$. She then chooses between entering entrepreneurship and returning to wage employment. If she returns to wage employment, her payoff is $y\,(1 - \delta(t))$. This is comprised of two parts: first, y represents the present value of future earnings from wage employment; unlike X, it is not stochastic. Second, y is multiplied by $1 - \delta(t) \leq 1$ to capture the idea that taking a longer leave may lead to a wage penalty. Note that, although the employer cannot legally penalize workers for taking a longer leave, it is not clear how strictly this can be interpreted. Even if there is no nominal wage decline when t is high, subsequent promotions, wage growth, and job-security may be negatively affected by longer leaves (Rossin-Slater, 2017).

We assume the following functional form for the wage penalty:

$$\delta(t) = \begin{cases} \theta dt & \text{if } t \leq T_L \\ dt & \text{if } t > T_L \end{cases}.$$

The parameter $d \geq 0$ represents the rate at which wages would decline with time out of the labor force, absent job-protection.[4] Higher values of d mean that the payoff from subsequent wage employment decreases more quickly as a function of leave time. If the worker chooses to take a leave that is longer than the legally-guaranteed period of T_L, she bears the full brunt of the wage penalty. This is equivalent to quitting her job, and then re-entering wage employment subsequently after a period of time $t > T_L$. The parameter $\theta \leq 1$ captures the degree of shielding provided by job-protected leave. At the extremes, job-protected leave offers full protection from any earnings impact when $\theta = 0$, and no protection at all when $\theta = 1$. We are only interested in cases where $\theta < 1$; otherwise job-protected leave is ineffective.

Recall that the worker chooses the length of her leave, t, at time 0. Then, at the end of the leave, she decides whether to enter entrepreneurship or return to wage employment based on the signal $s(t)$ that she draws. When she chooses the optimal t at the beginning of her leave, she takes into account the fact that at the end of the leave she will select the career path that provides the highest expected utility given her updated beliefs. Thus her time-0 expected utility maximization problem is:

$$\max_{t \geq 0} EU(t) = \max_{t \geq 0} E\left\{ \max\left[EU(X|t),\ EU(y(1-\delta(t))|t) \right] \right\}.$$

We assume that individuals are risk-averse and have exponential utility given by $U(x) = -e^{-\lambda x}$, where λ is the coefficient of absolute risk aversion.

[4] One could think of d as capturing skill depreciation. It also captures other labor market frictions. For example, since workers may start out in jobs where they earn a premium from being matched with a good firm, they risk losing that premium if forced to find a new job after taking leave without job protection (Song, Price, Guvenen, Bloom, and Von Wachter, 2015; Card, Cardoso, Heining, and Kline, 2016).

2.2 Model Implications

To understand the tradeoff an individual faces when choosing the length of leave at time 0, first consider the case when $\theta = 0$. In this case, there is no wage penalty. The payoff from returning to wage employment is simply y during this period, which is a constant. Further assume that she is risk neutral. Such an individual knows that, on average, she will expect the same payoff from entrepreneurship after a leave of length t as she does at time 0. That is, since taking a longer leave only increases the precision of her signal about X, but does not change the underlying distribution of X, she cannot expect to become more optimistic or pessimistic about X based on her choice of t.

However, while the mean of her future expectations is invariant to t, the variance of her future expectations is increasing in t. Since she can always return to wage employment and receive y when her future expectations about X turn out to be low, her overall payoff at date 0 is convex in her future expectations about X. Given this convexity, taking a longer leave is valuable since it increases the variance of her future expectations. Intuitively, the ability to enter entrepreneurship in the future is a real option. The value of this option increases, the more the individual will learn about the payoffs from entrepreneurship before having to decide whether or not to pursue it. Absent other forces, this pushes people toward taking as much leave as they can.

But there is also a potential cost of taking a longer leave, which comes from the wage penalty. This is the key tradeoff in the model. With full shielding, the wage penalty is only felt when a worker takes more than the maximum amount of leave—*i.e.*, quits her job and subsequently returns to wage employment. With partial shielding, it is also felt to a lesser extent when a worker takes a leave that is shorter than the maximum.

This tradeoff leads to the following predictions, which we state formally in Proposition 1 below. More available leave time makes people more likely to enter entrepreneurship when either (1) there is full shielding and the rate of the wage penalty is sufficiently high or (2) there is partial shielding and the rate of the wage penalty is neither too low nor too high. This is because, if the rate of the wage penalty is very low (with either partial or full shielding), people take more than the maximum amount of leave—*i.e.* quit their jobs to explore entrepreneurship. So increasing the maximum has no effect. If the rate of the wage penalty is very high (and shielding is only partial), people take less than the maximum amount of leave. Increasing the maximum again has no effect.[5]

Outside of these extremes, job-protected leave provides a valuable backstop for entrepreneurship. In the intermediate cases, workers value the increased precision of $s(t)$ they get from taking a longer leave, but not enough to accept the wage penalty at the full rate d. Thus they take the longest job-protected leave guaranteed by law, setting $t^* = T_L$. For these workers, an increase in T_L increases the length of time during which they face the wage penalty at the lower rate θd. This makes longer experimentation worthwhile, as they can now obtain a more precise signal at a lower cost in terms of the wage penalty.

More formally, we can prove the following proposition.

Proposition 1. *Assume that* $\mu - \frac{\lambda}{2\eta_0} < y$. *This means that prior beliefs make entrepreneurship less attractive than wage employment absent any signal.*

(i) Full Shielding $(\theta = 0)$:

There exists a $\underline{d}$ *such that:*

For $d \geq \underline{d}$, $t^* = T_L$, $\frac{\partial t^*}{\partial T_L} > 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} > 0$.

For $d < \underline{d}$, $t^* > T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

(ii) Partial Shielding $(0 < \theta < 1)$:

There exists a $\underline{d}$ *and* $\bar{d}$ *such that:*

For $\underline{d} \leq d \leq \bar{d}$, $t^* = T_L$, $\frac{\partial t^*}{\partial T_L} > 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} > 0$.

For $d < \underline{d}$, $t^* > T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

For $d > \bar{d}$, $t^* < T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

[5] It is not immediately obvious that a higher t^* increases the probability of entrepreneurial entry. In particular, if the *ex ante* payoff from entrepreneurship is sufficiently high relative to wage employment, obtaining more information will actually tend to push individuals away from entering entrepreneurship. However, we argue that for the individuals we are interested in, the *ex ante* payoff from entrepreneurship (after risk-adjustment) is unlikely to exceed the payoff from wage employment by so much. If it did, these individuals would not have started out in wage employment in the first place. It turns out that $\mu - \frac{\lambda}{2\eta_0} < y$ is a sufficient condition for the probability of entrepreneurial entry to increase with t^*. This is equivalent to the requirement that individuals would not wish to enter entrepreneurship absent any signal.

Proof. See Appendix B. □

3 Data

Our data come primarily from the Canadian Census of the Population, which is administered every five 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 mother tongue. 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. Just as with the U.S. Census, individuals and households cannot be linked across census years. So although the census is administered to the whole population every five years, it is not possible to form a panel and we rely on exclusively cross-sectional data. 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.[6]

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

[6]Due to 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 percent of households selected for the long form survey is random, the weights are generally very close to 5 for all respondents. That is, one observation in the sample data is representative of approximately 5 observations in the population data. Because the weights are so uniform, our results change little when they are unweighted.

assume that parents had children on the birth dates of the children residing in the same household as themselves.[7] Of course, non-biological family relationships within a household will lead to some degree of measurement error (e.g., step-children 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 self-employment income. In most cases, this information is obtained directly from their tax filings. Our primary definition of self-employment is someone who receives at least 50 percent of total income from self-employment.[8] Separately, respondents must also report whether they consider themselves self-employed based on their primary job. We favor the income-based measure as it comes from administrative data. However, we show in robustness tests that our results are similar when using self-reported entrepreneurship. Note that both measures of entrepreneurship include individuals who have incorporated their businesses or hired paid employees. Thus, we are not limited to studying sole proprietorships.

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 percent of mothers are self-employed as of 2006 when using the definition based on self-employment income. Based on self-reports, 7.1 percent 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 28 percent are college graduates. The rate of self-employment for fathers is higher, as is their age.[9] The last two lines of the table show income and work hours.

[7]We use children reported in the 2006 census to infer child birth dates in a window around December 31, 2000; therefore the relevant children would be around five years old as of the 2006 census date.

[8]Canadian taxes are assessed based on individual income, not combined spousal income as in the U.S. Thus, our data record self-employment and wage employment income for each individual.

[9]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.

These are about twice as large for fathers as for mothers, with the differences largely driven by the extensive margin: 39 percent 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. Appendix Table A.1 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 five-year-old child and apply the same variable definitions as 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.

4 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 pre-birth job after a specified period of absence—has several common features across the provinces (Baker and Milligan, 2008b).[10] 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

[10] In addition to the ten provinces, whose combined population is 34 million, Canada has three territories with a combined population of 100,000, located north of 60 degrees latitude.

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 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 percent 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 $275 USD). Of course, temporary income replacement is less useful if one's pre-birth 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 prefer. 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 pre-dated 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.[11] 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.

5 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

[11] 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.

data from the 1996 and 2006 censuses, which are five years before and after the reform, respectively.

Figure 1 Panel A shows that in all weeks following birth, the probability of employed mothers being on leave is indeed greater in 2006 than in 1996. Of course, these two samples are ten 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 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, Løken, and Mogstad (2014) find evidence that the reason fathers are reluctant to take parental leave is due to fears about how employers and co-workers 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.[12] Also, Appendix Table A.2 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. In our context, this would mean estimating equations of the form:

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

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 $f_1(.)$ captures a smooth trend on the left side of the cutoff, and the sum, $f_1(.) + f_2(.)$, captures a (potentially different) smooth trend on the right side of the cutoff. The coefficient τ captures any discontinuity between these trends at the cutoff.

[12]This calculation is based on annual entrepreneurship rates from the Canadian Labor Force Survey from 1995–2010.

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, 2017).[13] This means limiting the sample to mothers who gave birth within a narrow bandwidth, h, around the cutoff date and imposing linearity on the control functions. Within a narrow bandwidth, it is reasonable to assume that the control functions are approximately linear. One could also allow the control functions to follow higher order polynomials, but doing so has been shown to be problematic (Gelman and Imbens, 2017). Observations within the specified bandwidth around the cutoff are also 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).[14] For a given bandwidth and kernel, one can then estimate local linear regressions of the form:

$$y_i = \alpha + \tau Post_i + \delta_1 DayChild_i + \delta_2 Post_i \times DayChild_i + \epsilon_i. \tag{2}$$

In order 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:

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

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 coefficient τ now represents the difference between the discontinuity for mothers $(t + \tau)$ and that for fathers (just t). This difference, τ, is our estimated treatment effect and will be labeled $Treatment$ in the tables.

[13]For recent examples in the finance literature of RDD estimation using local linear regression, see, e.g. Berg (2015); Malenko and Shen (2016); Campello et al. (2018); Li, Liu, and Wu (2018).

[14]In practice, the choice of kernel generally makes little difference (Imbens and Lemieux, 2008)

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 3. Thus, we are examining the effect of extended job-protected leave on entrepreneurship status approximately five years later. We do not examine entrepreneurship status in the 2001 census because the census date falls too close to the reform date. 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. Even those who began working on a business while on leave would probably report "leave" rather than "self-employment" as their labor force status during the leave period, in order to remain eligible for the benefits of leave. As a result, we cannot observe whether these individuals entered entrepreneurship during or immediately after their leave. However, looking at long-term outcomes has the benefit that our results cannot reflect merely transitory short-term entry into entrepreneurship.

To interpret the estimates, 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 following sense: a worker could always quit her job, leave the labor force, and then re-enter whenever she wanted. Following the cutoff date, employees retained the option to take a short job-protected leave, but now could instead take a long job-protected leave, if desired. So τ measures the causal effect of giving employees the option to take a long job-protected leave relative to a long non-job-protected leave. If job protection (and career risk) were not specifically important in the entrepreneurial entry decision, we would not expect to see any effect, as unlimited non-job-protected leave was always available.[15]

[15] Note that the reform extended the income replacement period as well. We will discuss the potential role of income replacement in Section 6.7.

6 Results

6.1 Validity of 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.[16] Baker and Milligan (2014) find no evidence of gaming in birth timing around the reform we study in this paper. Similarly, Dahl, Løken, and Mogstad (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 U.S. to take advantage of tax benefits.[17] To minimize gaming concerns, we focus on 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 birth date. Still, some gaming remains possible even for these births. Such gaming may even be related to the mechanism we have in mind—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 towards 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

[16] Caesarean sections are also much less common in Canada, where the overall rate is 20 percent lower than in the U.S. (OECD, 2015).

[17] More recent work suggests that the magnitude of birth timing in the U.S. is small and largely due to misreporting rather than actual shifting of births (LaLumia, Sallee, and Turner, 2015).

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. Thus, 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

$$NumBirths_t = \alpha + \tau Post_t + \delta_1 EventDay_t + \delta_2 Post_t \times EventDay_t + \epsilon_t, \tag{4}$$

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 is gaming, we expect τ to be positive—that is, there should be a discontinuous jump in the birth rate around the cutoff date.

The results of this exercise are shown in Panel A of Table 3. We estimate equation (4) using bandwidths ranging from 60 days to 30 days and a triangular kernel. We find no significant discontinuity in the birth rate at the reform date for all bandwidths. 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 pre-reform period to the post-reform period. Appendix Table A.3 shows that these point estimates are even smaller when one accounts for the fact that there is a small, statistically insignificant jump in the birth rate around January 1 in non-reform 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 on $Post_t$. We see a smooth evolution of birth density across the cutoff date. 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, it is plausible that those who gave birth just before the cutoff date are similar to those who gave birth just after, both in terms of their observable and 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 equation (2), with parents' observable characteristics 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. The results are shown in Panel B of Table 3. We find no discontinuity in terms of age, education, or ethnicity for parents who have a child around the cutoff date.

Finally, our focus in this section has been on gaming in the timing of births within a small window around the cutoff date. However, one may also be concerned about the timing of pregnancies if the cutoff date were known far enough in advance. As discussed in Section 4, job-protected leave is regulated at the provincial level and provinces did not announce that they were extending job-protected leave until November at the earliest—meaning that those who gave birth within a narrow bandwidth around the December 31 cutoff date conceived long before they knew that job-protected leave would be extended or what the cutoff date for the extension would be. Moreover, even if some people correctly anticipated the cutoff date and timed their pregnancy accordingly, as long as births were not timed within a narrow bandwidth around the cutoff, it would remain fairly random exactly which side of the cutoff people ended up on. Thus, 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 people who had 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 the 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 may be largest among this group. In that case, our estimates would represent a lower bound of what we would find were the reform completely unanticipated.

6.2 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.

The results are shown in Panel A of Table 4. The row labeled *Treatment* corresponds to the estimated coefficient τ 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.

Figure 3 shows our results graphically. The grey dots represent the raw data—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—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.[18]

The full set of coefficient estimates that generate this figure are shown in column (1) of Appendix Table A.4. As can be seen, the estimated coefficient on the $Post_i$ variable is statistically insignificant, meaning that there is no statistically significant discontinuity at the cutoff for fathers. The coefficient on $Mother_i \times Post_i$ (i.e., our estimated treatment effect) is positive and statistically significant, meaning that the discontinuity for mothers is significantly larger than that for fathers. The estimated coefficient on $Mother_i$ 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 5, 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 two 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 non-reform 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 previous years, going back to 1991. We then estimate a variant of equation (3), in which we interact all of our covariates with a $Reform\ Year_i$ 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. Results are shown in Panel A of Table 5. The coefficient on $Treatment_i$, without an interaction, now represents the size of the difference-in-discontinuities estimate around December 31 in non-reform years (i.e., $Reform\ Year_i = 0$). As we would expect, we find no evidence of a "treatment effect" in non-reform years. The coefficient on the interaction term, $Reform\ Year_i \times Treatment_i$, represents how much larger the difference-in-discontinuities

[18] 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 are simply showing independent 95% confidence intervals on each side of the cutoff.

estimate is around December 31 in actual the reform year. Again as we would expect, we find a significantly larger treatment effect in the reform year.[19]

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 Appendix Table A.5, 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.

To interpret the magnitude of our result, we would ideally like to compare it to other estimates of how much wages influence labor supply choices. Since our variation is not in wages, we must first determine the wage increase that would be equally valuable as the increased job protection. Then we can ask, for a given percentage change in the value of one sector relative to another, what share of the workforce would switch sectors? We express this as a semi-elasticity and compare the magnitude to other estimates of how wages influence discrete choices related to labor supply.

To compute our implied semi-elasticity, we need an estimate of the monetary value of job protection. Dillon and Stanton (2016) estimate that the option value of being an entrepreneur for one year, with the opportunity to return to paid work subsequently, is worth 7 percent of lifetime earnings. We estimate that providing a very similar sort of option through job protection generates a 1.9 percentage point increase in entrepreneurship. The implied semi-elasticity of sectoral choice with respect to wage equivalent is thus $0.019/0.07 = 0.27$.

We are not aware of existing direct estimates of this same parameter. However, the empirical literature has estimated analogous parameters for other types of discrete labor supply choices. Chetty (2012) finds an extensive margin labor supply elasticity of 0.25. Nicholson and Souleles (2001) find

[19]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 $Reform\ Year_i \times Mother_i \times Post_i$.

that physicians' specialty choice exhibits a semi-elasticity of 0.25 with respect to income. Lockwood, Nathanson, and Weyl (2017) estimate a wage elasticity of 1.42 for the share of workers choosing careers in finance. In an economy with formal and informal sectors, Becerra (2015) estimates an elasticity of formal sector labor supply with respect to the net-of-tax income share above 1. Of course, these existing estimates are considering outcomes very different from choosing a spell of self-employment. But together they demonstrate that discrete choices of whether, how, and where to work can be quite responsive to income differences. When quantifying our result in terms of responsiveness to an equally valuable wage increase, it is in the lower range of these existing estimates.

6.3 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 this has been shown in the RDD literature 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 (2017) 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.

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 variable can be attributable 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 (3 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 birth). 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.

6.4 Heterogeneity

In this section, we explore how the effect of job-protected leave documented above varies across provinces, which experienced differential increases in leave availability, and across individuals, who may differ in their propensity to start a business.

6.4.1 Heterogeneity Across Provinces

In Section 2, we derived conditions under which extending job-protected leave can increase entrepreneurship. When these conditions hold, a longer increase in available leave time should lead to more entrepreneurship than a shorter increase. 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. The first type of heterogeneity we examine is 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 on $Treatment_i$, 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 $\Delta Weeks\ Leave_i = 0$). As we would expect, this coefficient matches the effect estimated in the baseline regression from Table 4. The coefficient on the interaction term, $\Delta Weeks\ Leave_i \times Treatment_i$, represents how the difference-in-discontinuities estimate varies as the change in available leave time deviates from its mean (in units of standard deviations).[20] 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.

6.4.2 Heterogeneity Based on Individual Characteristics

We also explore whether the effect of the leave extension varies across individuals with different characteristics. It is plausible that some people will be more sensitive to job-protected leave availability than others because they are more willing or able to start a business. For example, individuals with more education or work experience may have human capital that better positions them to work on a business idea during a job-protected leave. In addition, individuals with higher human capital may face a higher rate of skill depreciation. For such individuals, leaving their job to experiment with entrepreneurship absent job-protection may be costlier in terms of the wage penalty they would face should they return to wage employment. These individuals should therefore have stronger incentives to take advantage of the job-protection. In terms of financial capital, individuals with high-income spouses may be less financially constrained and thus better able to start a business given the opportunity.

To test these predictions, we examine whether the effect of job-protected leave differs for those with and without a college degree, those above and below the median age at childbirth (since age is a proxy for work experience), and those with a high-income and low-income spouse. Table 9 shows the results. Consistent with the discussion above, in columns (1) and (2) we find that there is a positive effect of job-protected leave on entrepreneurship for those with a college degree, but no effect for those without one. The p-value of the difference in coefficients is shown below the

[20] 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 the standardized change in weeks of leave available in an individual's province; $\Delta Weeks\ Leave_i \times Treatment_i$ corresponds to $\Delta Weeks\ Leave_i \times Mother_i \times Post_i$.

estimates. The difference between columns (1) and (2) is significant at $p < 0.01$. In columns (3) and (4) we find a positive effect among individuals above the median age at childbirth, and no effect among those below the median age. The difference is significant at $p < 0.05$. Finally, in columns (5) and (6) we find a positive effect among individuals with a spouse making above the median income and no effect among individuals with a spouse making below the median income. In this case, the difference is significant at $p < 0.1$.[21] Overall, the results suggest that the effect of job-protected leave on entry into entrepreneurship is higher for those with more human and financial capital and thus a greater ability to enter.

6.5 Mechanism

Our results thus far show that offering employees extended job-protected leaves makes them more likely to pursue entrepreneurship. Our posited mechanism is that job-protected leaves allow entrepreneurs to explore business ideas without putting their non-entrepreneurial career trajectories at risk. If that is indeed the channel through which the effect operates, we should expect stronger results when experimentation prior to entry has low costs or high benefits (Kerr, Nanda, and Rhodes-Kropf, 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. 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.[22] In both cases,

[21] One caveat regarding the spousal income results is that we can only measure spousal income as of 2006. Ideally, we would observe spousal income prior to childbirth and split the sample based on that. Nonetheless, since income is persistent, 2006 spousal income may be a reasonable proxy for 2001 spousal income.

[22] For each industry in Orbis, we compute failure rates for private firms during their first five years of existence.

industries are defined based on 2-digit NAICS codes. 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 (startup capital requirements above the median) and another if the individual is an entrepreneur working in a low startup capital industry (startup capital requirements below the median). These two variables add up to our original dependent variable. We decompose entrepreneurship based on industry failure rates analogously.[23]

Table 10 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. In columns (3) and (4) we find that extending job-protected leave increases high-risk entrepreneurship, but has no effect on entrepreneurship in low-risk industries. As one would expect, the coefficients in each pair of columns add up to our baseline estimate. Overall, these results are consistent with the view that job-protected leave drives entrepreneurial entry by insulating entrepreneurs from downside risk and enabling them to experiment.

6.6 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 observation to note is that we measure businesses that still exist five 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. Further, the effect of the reform on entrepreneurship is significantly stronger for individuals with *ex ante* characteristics that predict higher-quality businesses. In particular, those with more education and more work experience respond more strongly to the reform.

Nonetheless, to further explore the quality of these businesses we use self-reported information

[23]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, retail trade, and environmental services.

about them. Our primary measure of entrepreneurship thus far has been based on self-employment income. However, 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 making sure that our results are robust to using self-reported entrepreneurship as the dependent variable. In the first column of Table 11, Panel A, we re-estimate equation (3), with self-reported entrepreneurship. The results are similar to our main estimates in 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 should also 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). Thus, the main finding from Table 11 is that the reform does appear to increase high-quality entry. We cannot determine whether it increases low-quality entry as well, since those entrepreneurs may exit before the 2006 census.

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 ability to take long job-protected leaves—for the explicit purpose of working on a business idea—such a policy might lead to the creation of technology startups.

6.7 Alternative Explanations

6.7.1 Job Flexibility

One alternative explanation for our results is that new mothers have a stronger desire for job flexibility than other workers. However, our estimates should not be influenced by changes in preferences that result from having a child, as everyone in our sample has a child. Conditional on having a child, we compare those who (quasi-randomly) were eligible for a longer period of job-protected leave to those who were not. Therefore, we do not think of job flexibility as providing an alternative explanation for our results. Rather, job flexibility is an underlying reason that entrepreneurship may be desirable.

In particular, even if those in our sample were only interested in entrepreneurship for job flexibility reasons, our results would still suggest that career risk deters potential entrepreneurs. Prior to the reform, employees always had the option to quit wage employment and create a flexible job without the security of job-protection. The primary thing the reform changed was to allow them to experiment while maintaining the option to return to their previous job. Put differently, our conceptual framework allows for uncertain payoffs from entrepreneurship, both pecuniary and non-pecuniary (Schoar, 2010; Hurst and Pugsley, 2011). Job-protection allows employees to reduce this uncertainty before making a major career change.

6.7.2 Skill Depreciation

Another possibility is that longer leaves cause employees' skills to depreciate. In this case, workers might lack the skills to return to their previous jobs, essentially forcing them into entrepreneurship. Again, recall that the "treatment" in this natural experiment is not a longer *forced* leave. If taking a short leave is optimal for employees from a skill-depreciation standpoint, they could still take a short leave after the reform. If the benefits of additional time away outweigh the costs of skill depreciation, they could always have taken additional time away before the reform (without job-protection) and eventually have entered self-employment. In other words, long leaves that forced workers into self-employment were available before the reform as well. Therefore, there is no reason to expect the reform to lead to an increased tendency for employees to take such leaves.

Moreover, one would expect "skill-depreciation entrepreneurs" to be lower quality entrepreneurs. But we find an increase in high-quality entrepreneurship. In addition, if employees were forced out of their jobs due to skill depreciation, we would expect job continuity to decrease. However, using panel data, Baker and Milligan (2008a) find that the reform we study in this paper actually increased job continuity with pre-birth employers.[24] Finally, even if an employee's skills did depreciate, she would have grounds to bring legal action were she forced out of her job as a result of having taken job-protected leave.

6.7.3 Financial Constraints

Another potential explanation is that longer leaves simply relax financial constraints. However, in 2000, employment insurance only provided 55 percent income replacement up to a maximum of $413 CAD per week (about $275 USD). Thus, the reform does not represent a positive wealth shock,

[24]Note that Baker and Milligan's (2008a) results are entirely consistent with ours. Longer job-protected leave entitlements can lead both to greater entry into entrepreneurship and greater job continuity. Longer leaves may cause some people to leave their pre-birth employer to start a business. However, longer leaves may simultaneously cause even more people to return to their pre-birth employer who otherwise would have left the labor force or become unemployed.

as people earn significantly lower income while on leave. If someone had an idea but insufficient capital to pursue it, she would most likely still have insufficient capital while on leave. Moreover, we find stronger effects in provinces that had larger increases in job-protected leave time. If changes in paid leave time were driving our results, we would not expect this, as all provinces had the same increase in paid leave.

Finally, 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 more *paid* leave than one who gave birth before, but no additional *job-protected* leave. In Appendix Table A.6, we re-estimate our baseline specification using the sample of parents who gave birth in Quebec. We find insignificant effects of the reform in this case.

Overall, it seems unlikely that changes in paid leave entitlements drive our results. Instead, the job-protection aspect of the reform appears to be the key factor. These results are also consistent with Dahl et al. (2016) and Stearns (2016), who find that increases in paid leave without changes in job protection have little effect on a wide variety of outcomes.

7 Conclusion

Choosing to start a business is inherently a 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 hard to obtain as good a job as the one she could have otherwise had. We have adduced empirical 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 resulting 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. They also tend to have more human and financial capital. 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 (Gromb and Scharfstein, 2002). It is much easier to take a big risk with one's career when there is a good fallback option 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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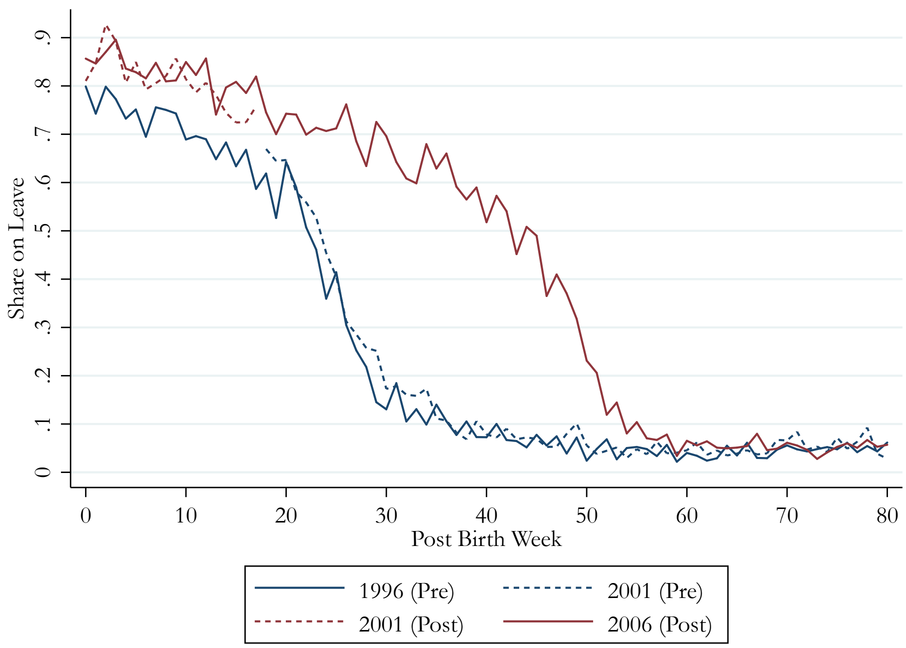
Figure 1
Leave Taking Before and After Reform

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 birth date), and the 2006 census (after the reform).

Panel A: Mothers



Panel B: Fathers

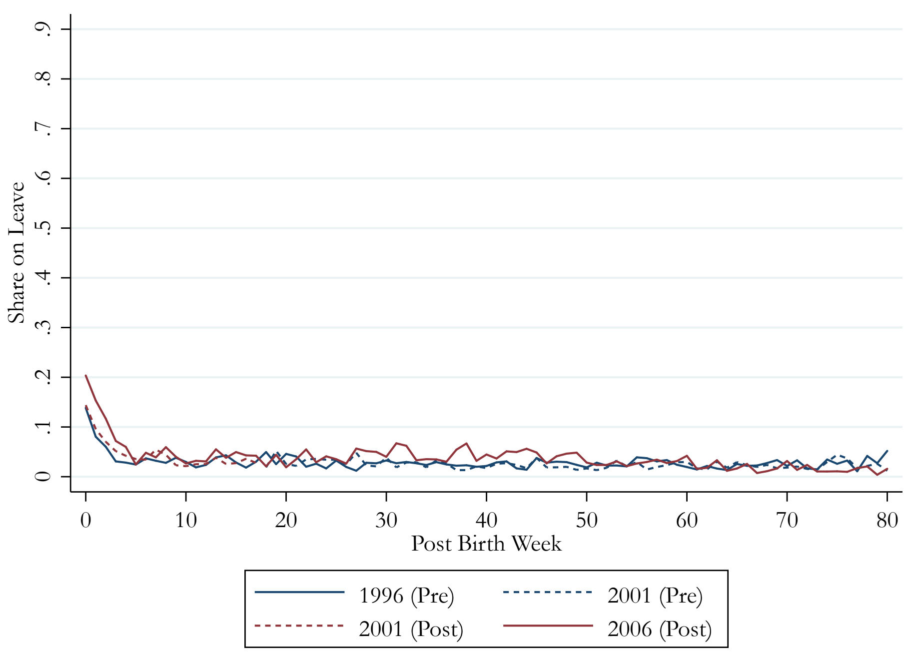


Figure 2
RDD Validity Check

This figure shows the results from Table 3, Panel A, graphically, with the y-axis showing the share of births. The grey dots represent the raw data—the share of births within 3-day bins around the cutoff date. The solid lines represent the regression estimates—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.

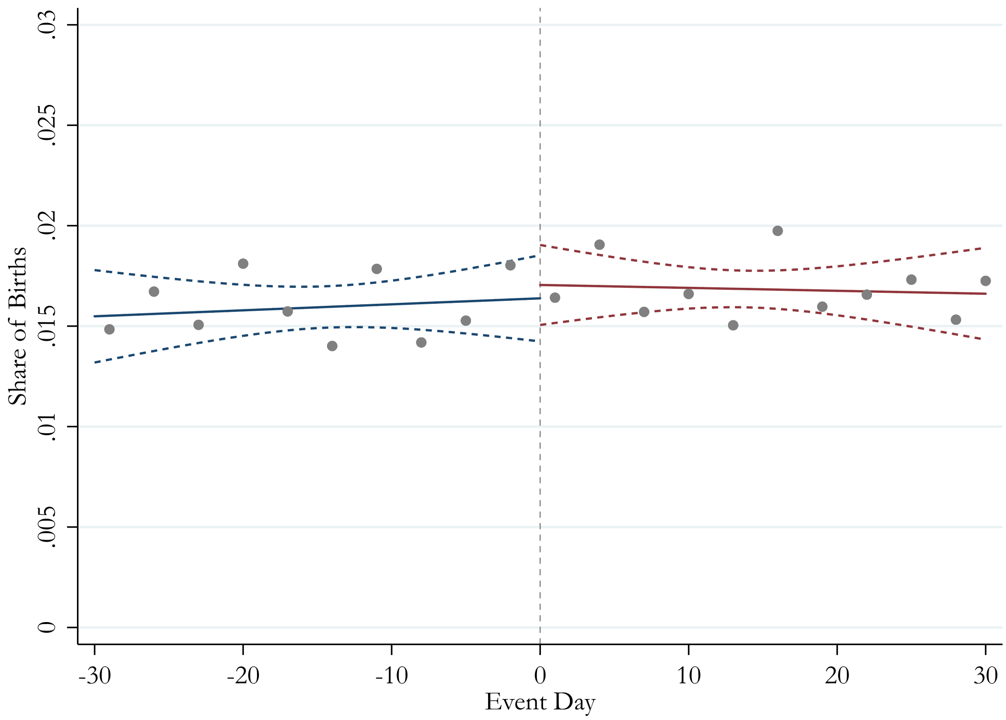


Figure 3
Baseline Results

This figure shows the baseline results from Table 4 graphically. The grey dots represent the raw data—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—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 corresponds exactly with the average treatment effect reported in column (4) of Table 4. The discontinuity is significant at the 1% level.

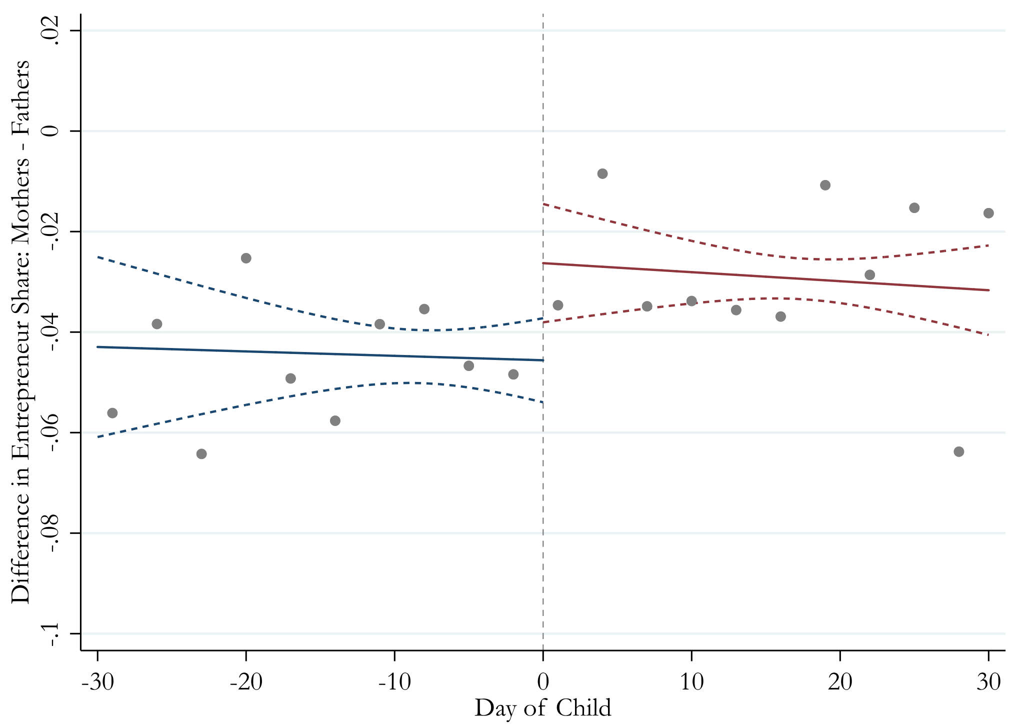


Table 1
Summary Statistics

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 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 or above Bachelor degree. Minority indicates being in a non-white ethnic group. Total income indicates total annual personal income (in 2006 USD). Work hours indicate the average number of work hours per week. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada.

Sample:	Mothers				Fathers			
	Observations	Mean	Median	Std. Dev.	Observations	Mean	Median	Std. Dev.
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

Table 2
Maternity Leave Reform

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

Province	Weeks Leave Pre-Reform	Weeks Leave Post-Reform	Cut-off 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	

Table 3
RDD Validity Tests

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 days 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 the December 31, 2000, 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; $Post_i$ is an indicator equal to one if parent i had a child on or after the cutoff date. Other variables are defined as in Table 1. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Smoothness of Density

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

Panel B: Smoothness of Covariates

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

Table 4
Baseline Results

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 days to 30 days and a triangular kernel:

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

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; $Post_i$ 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 τ in the above equation and represents the estimated average treatment effect. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

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

Table 5
Placebo Test Using Non-Reform Years

This table compares the difference-in-discontinuities estimate from placebo years prior to the reform 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 1991–2000. 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 as in Table 4. The coefficient on $Treatment$, without an interaction, represents the size of the difference-in-discontinuities estimate around December 31 in non-reform years (i.e., $Reform\,Year = 0$). The coefficient on the interaction term, $Reform\,Year \times Treatment$, represents how much larger the difference-in-discontinuities estimate is around December 31 in the actual reform year relative to non-reform years ($Reform\,Year \times Treatment$ corresponds to $Reform\,Year \times Mother \times Post$). Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	-0.0029	-0.0023	-0.0016	0.0017
	(0.0042)	(0.0046)	(0.0051)	(0.0058)
Reform Year × Treatment	0.0261***	0.0281***	0.0269***	0.0245**
	(0.0087)	(0.0089)	(0.0096)	(0.0107)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	1,079,200	896,455	717,400	536,660

Table 6
Robustness to Alternative Specifications

This table repeats the analysis of 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. All variables are defined as in Table 4. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Uniform Kernel

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0198***	0.0193***	0.0216***	0.0175**
	(0.0070)	(0.0071)	(0.0064)	(0.0080)
Bandwidth	60	50	40	30
Kernel	Uniform	Uniform	Uniform	Uniform
Polynomial Degree	1	1	1	1
Observations	86,565	71,285	57,405	42,430

Panel B: Degree 2 Polynomial

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0221***	0.0202***	0.0223***	0.0296***
	(0.0067)	(0.0078)	(0.0079)	(0.0062)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	2	2	2	2
Observations	86,565	71,285	57,405	42,430

Panel C: Degree 3 Polynomial

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0219**	0.0264***	0.0323***	0.0362***
	(0.0090)	(0.0075)	(0.0060)	(0.0045)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	3	3	3	3
Observations	86,565	71,285	57,405	42,430

Table 7
Robustness to Controls

This table repeats the analysis of Table 4 adding a range of different controls. Column (1) repeats the baseline regression from column (4) of Table 4. Columns (2) through (8) sequentially add ethnic origin fixed effects (225 categories), education level fixed effects (3 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). Column (9) allows the effect of these control variables to differ between mothers and fathers, by interacting each of them with a mother indicator. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.0193***	0.0201***	0.0213***	0.0219***	0.0215***	0.0215***	0.0215***	0.0214***	0.0202**
	(0.0074)	(0.0077)	(0.0076)	(0.0076)	(0.0078)	(0.0077)	(0.0078)	(0.0077)	(0.0082)
Race	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Age	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Province	No	No	No	No	No	Yes	Yes	Yes	Yes
Stock Index	No	No	No	No	No	No	Yes	Yes	Yes
Exchange Rate	No	No	No	No	No	No	No	Yes	Yes
Mother $\times$ Controls	No	No	No	No	No	No	No	No	Yes
Bandwidth	30	30	30	30	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1	1	1	1	1
Observations	42,430	42,430	42,430	42,430	42,430	42,430	42,430	42,430	42,430

Table 8
Heterogeneity Across Provinces

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 on $Treatment$, 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 on 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 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0194***	0.0210***	0.0199***	0.0193***
	(0.0060)	(0.0061)	(0.0064)	(0.0074)
ΔWeeks Leave × Treatment	0.0116	0.0171***	0.0180***	0.0138***
	(0.0071)	(0.0061)	(0.0049)	(0.0021)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	86,565	71,285	57,405	42,430

Table 9
Heterogeneity Across Individuals

This table examines how the effect of job-protected leave varies across individuals with different characteristics. It repeats the analysis of column (4) of 4 on subsamples of individuals with and without a college degree (columns 1–2), subsamples of individuals above and below the median age at childbirth (columns 3–4), and subsamples of individuals with a spouse earning above and below the median income (columns 5–6). P-values indicate the significance of the differences in the estimated treatment effect across complementary subsamples. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur					
Subsample:	High Education	Low Education	High Age	Low Age	High Spouse Income	Low Spouse Income
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0751***	0.0010	0.0362**	-0.0010	0.0356***	0.0066
	(0.0220)	(0.0060)	(0.0142)	(0.0047)	(0.0133)	(0.0073)
Bandwidth	30	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1	1
P-value of Difference	0.0092	0.0092	0.0459	0.0459	0.0879	0.0879
Observations	11,835	30,595	24,945	17,485	19,215	23,215

Table 10
Characterizing Entrants Based on Industry Characteristics

This table characterizes entrants into entrepreneurship based on industry characteristics. It repeats the analysis of column (4) of Table 4, decomposing 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). 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). Columns (3)–(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 2-digit NAICS industry from the Survey of Business Owners. Industry failure rates are defined as 5-year failure rates for private firms in a 2-digit NAICS industry from Bureau van Dijk's Orbis database. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

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	0.0057	0.0136***	0.0138***	0.0055
	(0.0040)	(0.0047)	(0.0051)	(0.0070)
Bandwidth	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	42,430	42,430	42,430	42,430

Table 11

Characterizing Entrants Based on Self-Reported Information

This table characterizes entrants based on self-reported information. Column (1) of both panels repeat the analysis of column (4) of Table 4 but defining 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 (3)–(4) of Panel A decompose self-reported entrepreneurship based on whether individuals hire paid employees. Columns (1)–(2) of Panel B decompose self-reported entrepreneurship based on whether individuals report working above or below the median number of hours. Columns (3)–(4) 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 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel 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**	0.0164***	0.0110	0.0151**	0.0123
	(0.0125)	(0.0040)	(0.0105)	(0.0070)	(0.0105)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1
Observations	42,430	42,430	42,430	42,430	42,430

Panel B: Income and Hours

Dependent Variable:	Self-Reported Entrepreneur				
	All	With High Hours	With Low Hours	With High Income	With Low Income
	(1)	(4)	(5)	(2)	(3)
Treatment	0.0274**	0.0310***	-0.0036	0.0205***	0.0069
	(0.0125)	(0.0080)	(0.0102)	(0.0059)	(0.0141)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1
Observations	42,430	42,430	42,430	42,430	42,430

Appendix For Online Publication

A Appendix Tables

Table A.1
Summary Statistics for Analogous U.S. Parents

This table presents summary statistics analogous to Table 1 but using U.S. data from the 2006 American Community Survey. The samples include all mothers and fathers with a first child of age five. All variable are defined just as in Table 1. Sample sizes are weighted.

Sample:	Mothers				Fathers			
	Observations	Mean	Median	Std. Dev.	Observations	Mean	Median	Std. Dev.
Number of Children	1,322,908	1.826	2	0.722	1,141,922	1.861	2	0.705
Entrepreneur (Income-Based)	1,322,908	0.041	0	0.199	1,141,922	0.068	0	0.252
Entrepreneur (Self-Reported)	1,322,908	0.064	0	0.244	1,141,922	0.118	0	0.322
Age	1,322,908	31.99	32	6.462	1,141,922	34.98	35	6.891
Bachelor's Degree	1,322,908	0.343	0	0.475	1,141,922	0.355	0	0.479
Minority	1,322,908	0.271	0	0.444	1,141,922	0.236	0	0.425
Total Income	1,322,908	22,625	13,000	34,678	1,141,922	58,700	42,000	65,142
Work Hours	1,322,908	35.32	40	12.07	1,141,922	45.42	40	10.75

Table A.2
Entrepreneurship and Cross-Sectional Characteristics

This table shows the cross-sectional relationship between various characteristics and the probability of being an entrepreneur as of the 2006 census. Characteristics are interacted with a mother indicator to test whether the association between a characteristic and entrepreneurship is different between mothers and fathers. Columns (1) through (5) are estimated using OLS and columns (6) through (10) are estimated with Logit. The sample is the same as that used in column (4) of Table 4. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week of childbirth. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur									
Estimation Method:	OLS					Logit				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age at Child Birth	0.003***				0.003***	0.039***				0.035***
	(0.001)				(0.001)	(0.008)				(0.008)
Bachelor Degree		0.056**			0.053**		0.625***			0.581***
		(0.022)			(0.024)		(0.190)			(0.220)
Minority			-0.012		-0.012			-0.173		-0.172
			(0.016)		(0.017)			(0.226)		(0.246)
Urban				0.001	-0.009				0.009	-0.103
				(0.013)	(0.013)				(0.176)	(0.176)
Mother × Age at Child Birth	-0.000				-0.001	0.0248				0.018
	(0.001)				(0.001)	(0.0232)				(0.022)
Mother × Bachelor Degree		-0.006			-0.011		0.243			0.113
		(0.033)			(0.033)		(0.360)			(0.351)
Mother × Minority			-0.011		-0.009			-0.470		-0.411
			(0.018)		(0.019)			(0.304)		(0.318)
Mother × Urban				0.007	0.008				0.185	0.125
				(0.009)	(0.011)				(0.119)	(0.150)
P-value of joint F-test					0.574					0.198
Observations	42,430	42,430	42,430	42,430	42,430	42,430	42,430	42,430	42,430	42,430

Table A.3

Smoothness of Density: Reform Year vs. Other Years

This table repeats the analysis of Table 3 on a sample consisting of days within a specified bandwidth around December 31 of any year in the range 1991–2005. All variables are interacted with a *Reform Year* indicator equal to one for days within the specified bandwidth around December 31, 2000. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Number of Births			
	(1)	(2)	(3)	(4)
Post	3.869	6.311	8.584	14.26
	(8.480)	(9.155)	(10.04)	(11.35)
Reform Year × Post	8.682	9.581	8.917	0.140
	(24.35)	(26.72)	(29.52)	(33.35)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	1,815	1,515	1,215	915

Table A.4
Full Coefficient Estimates

This table reports the full coefficient estimates in our baseline difference-in-discontinuities regression. Column (1) uses a triangular kernel and corresponds to column (4) of Table 4. Column (2) uses a uniform kernel and corresponds to column (4) of Table 6, Panel A. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur	
	(1)	(2)
Post	-0.0071	0.0011
	(0.0122)	(0.0139)
DayChild	0.0001	-0.0001
	(0.0006)	(0.0005)
Post × DayChild	-0.0006	-0.0010
	(0.0009)	(0.0007)
Mother	-0.0456***	-0.0455***
	(0.0043)	(0.0037)
Mother × Post (Treatment)	0.0193***	0.0175**
	(0.0074)	(0.0080)
Mother × DayChild	-0.0001	-0.0001
	(0.0004)	(0.0002)
Mother × Post × DayChild	-0.0001	0.0001
	(0.0005)	(0.0004)
Constant	0.0864***	0.0838***
	(0.0085)	(0.0084)
Bandwidth	30	30
Kernel	Triangular	Uniform
Polynomial Degree	1	1
Observations	42,430	42,430

Table A.5
Placebo Test Based on 2001 Census

This table repeats the analysis of Table 4 using data from the 2001 census rather than the 2006 census, and a placebo reform cutoff date of December 31, 1995, rather than the true reform cutoff date of December 31, 2000. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	-0.0098	-0.0100	-0.0094	-0.0011
	(0.0106)	(0.0115)	(0.0124)	(0.0098)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	86,565	71,285	57,405	42,430

Table A.6

Quebec

This table repeats the analysis of Table 4 using the sample of parents who gave birth in Quebec, where job-protected leave remained unchanged for parents of children born around December 31, 2000. Sample sizes are weighted and rounded to the nearest multiple of 5 as required by Statistics Canada. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	-0.0170	-0.0218	-0.0212	-0.0122
	(0.0184)	(0.0192)	(0.0179)	(0.0136)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	17,850	14,385	11,435	8,280

B Proofs

B.1 Bayesian Learning

We begin by characterizing the learning process. At the end of the leave, upon observing the signal, a worker forms a posterior about the payoff from entrepreneurship: $X|s(t)$. It can be shown that her posterior expectation is

$$E[X|s(t)] = \frac{\eta_0}{\eta_0 + h(t)}\mu + \frac{h(t)}{\eta_0 + h(t)}s(t),$$

and the posterior variance is

$$Var[X|s(t)] = \frac{1}{\eta_0 + h(t)}.$$

Therefore, $X|s(t) \sim N\left(\frac{\eta_0}{\eta_0+h(t)}\mu + \frac{h(t)}{\eta_0+h(t)}s(t), \frac{1}{h+h(t)}\right)$.

Using variance decomposition, $Var[E[X|s(t)]] = Var(X) - E[Var[X|s(t)]] = \frac{1}{\eta_0} - \frac{1}{\eta_0+h(t)}$. Note that the posterior variance decreases in t, while the variance of the posterior expectation increases in t. Therefore, we also have

$$E[X|s(t)] \sim N\left(\mu, \frac{1}{\eta_0} - \frac{1}{\eta_0 + h(t)}\right).$$

B.2 Optimization

To simplify notation, let h_t stand in for $h(t)$, s_t for $s(t)$, and δ_t for $\delta(t)$. At $T = t$, the worker chooses entrepreneurship if the expected utility based on her updated belief is higher than the utility from wage employment, i.e., $EU(X|s_t) > \ EU\left(y(1-\delta_t)\right)$. We assume individuals have exponential utility: $U(x) = -e^{-\lambda x}$, where λ is the coefficient of absolute risk aversion. For $x \sim N\left(\mu, \sigma^2\right)$, the expected utility will be $EU(x) = -e^{-\lambda\mu + \lambda^2\frac{\sigma 2}{2}}$.

The worker maximizes expected utility at time 0 by choosing the optimal t:

$$\max_{t\geq 0} EU(t) = \max_{t\geq 0} E\left\{\max\left[EU(X|t),\ EU\left(y\left(1-\delta_t\right)|t\right)\right]\right\}.$$

Note that $X|s_t \sim N\left(\frac{\eta_0}{\eta_0+h_t}\mu + \frac{h_t}{\eta_0+h_t}s_t, \frac{1}{\eta_0+h_t}\right)$, so $U\left(X|s_t\right)$ follows a log-normal distribution. Specifically,

$$EU\left(X|s_t\right) = -\exp\left[\frac{\frac{1}{2}\lambda^2 - \lambda\left(\eta_0\mu + h_t s_t\right)}{\eta_0 + h_t}\right].$$

It can be shown that

$$EU\left(X|s_t\right) \sim -LN\left(\frac{\lambda^2}{2\left(\eta_0+h_t\right)}-\lambda\mu,\ \lambda^2\left(\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}\right)\right)$$

and

$$EU\left(y\left(1-\delta_t\right)\right)=U\left(y\left(1-\delta_t\right)\right)=-\exp\left(-\lambda y\left(1-\delta_t\right)\right).$$

Applying the conditional expectation formula for log-normal distribution, we can show that for any $S \sim LN(\mu,\sigma)$ and a constant K, $E[\max(S,K)] = E[S|S>K]P(S>K)+K[1-P(S>K)] = \exp\left(\mu+\frac{1}{2}\sigma^2\right)\Phi\left(\frac{\mu-\ln(K)}{\sigma}+\sigma\right)+K\Phi\left(\frac{\ln(K)-\mu}{\sigma}\right)$, where $\Phi\left(\right)$ is the c.d.f. of the standard normal distribution.

Let $EU(t)$ stand for $EU\left(X|s_t\right)$. Therefore, we have

$$EU(t)=-\exp\left(\frac{\lambda}{2\eta_0}-\lambda\mu\right)\Phi\left(\frac{\mu-y\left(1-\delta_t\right)-\frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}}-\lambda\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}\right)-\exp\left(-\lambda y\left(1-\delta_t\right)\right)\Phi\left(\frac{-\mu+y\left(1-\delta_t\right)+\frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}}\right).$$

Finally, the probability of entering entrepreneurship is

$$\Pr(Entrepreneurship|t)=\Pr\left[EU\left(X|s_t\right)\geq\ EU\left(y\left(1-\delta_t\right)\right)\right]=\Phi\left(\frac{\mu-y\left(1-\delta_t\right)-\frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}}\right).$$

Since job-protected leave only applies to those who did not quit their job for entrepreneurship before leave starts, we are only interested in individuals for whom $\Pr\left(Entrepreneurship|t=0\right)=0$, i.e., those for whom $\mu-\frac{\lambda}{2\eta_0}<y$.

B.3 The effect of leave extension

Having derived expressions for expected utility and the probability of entrepreneurship as a function of t, we can now prove Proposition 1. To do so, we first prove several lemmas.

Lemma 1. *Assuming* $\mu-\frac{\lambda}{2\eta_0}<y$, $\Pr\left(Entrepreneurship|t\right)$ *increases in* t.

Proof. First, let $f(t)=\frac{\mu-y(1-\delta_t)-\frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}}$ and therefore $\Pr(Entrepreneurship|t)=\Phi(f(t))$. Noting that $h_t=\sqrt{t}$, $h_t^{'}=\frac{1}{2h_t}$, and $\delta_t^{'}=\theta d$ ($\theta=1$ when $t>T_L$), we have

$$f^{'}(t)=\left(\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}\right)\left\{\left[y\theta d+\frac{\lambda}{4h_t\left(\eta_0+h_t\right)^2}\right]\left[\frac{h_t}{\eta_0\left(\eta_0+h_t\right)}\right]^{\frac{1}{2}}-\left[\mu-y\left(1-\theta dt\right)-\frac{\lambda}{2\left(\eta_0+h_t\right)}\right]\left[\frac{h_t}{\eta_0\left(\eta_0+h_t\right)}\right]^{-\frac{1}{2}}\frac{1}{4h_t\left(\eta_0+h_t\right)^2}\right\}$$

$$\begin{aligned}
sign:f(t)=sign:&\left[y\theta d+\frac{\lambda}{4h_t\left(\eta_0+h_t\right)^2}\right]\frac{h_t}{\eta_0\left(\eta_0+h_t\right)}-\left[\mu-y\left(1-\theta dt\right)-\frac{\lambda}{2\left(\eta_0+h_t\right)}\right]\frac{1}{4h_t\left(\eta_0+h_t\right)^2}\\
&=sign:y\theta dh_t^2\left(\eta_0+h_t\right)^2+\frac{1}{4}\lambda h_t-\left[\mu-y\left(1-\theta dt\right)\right]\frac{1}{4}\eta_0\left(\eta_0+h_t\right)+\frac{1}{8}\lambda\eta_0\\
&=sign:y\theta dt\left(\eta_0+\sqrt{t}\right)^2+\frac{1}{4}\lambda\sqrt{t}-\frac{1}{4}\left(\mu-y+y\theta dt\right)\eta_0\left(\eta_0+\sqrt{t}\right)+\frac{1}{8}\lambda\eta_0\\
&=sign:y\theta dt\left[\left(\eta_0+\sqrt{t}\right)^2-\frac{1}{4}\eta_0\left(\eta_0+\sqrt{t}\right)\right]+\frac{1}{4}\lambda\sqrt{t}-\frac{1}{4}\left(\mu-y\right)\eta_0\left(\eta_0+\sqrt{t}\right)+\frac{1}{8}\lambda\eta_0\\
&=sign:y\theta dt\left(\frac{3}{4}\eta_0+\sqrt{t}\right)\left(\eta_0+\sqrt{t}\right)-\frac{1}{4}\left(\mu-y-\frac{\lambda}{2\eta_0}\right)\eta_0\left(\eta_0+\sqrt{t}\right)+\frac{1}{8}\lambda\sqrt{t}
\end{aligned}$$

Because $\mu-y-\frac{\lambda}{2\eta_0}<0$, it can be easily seen that $f^{'}(t)>0$. At the boundary, when t increases from T_L to $T_L+\epsilon$, δ_t increases discretely. Therefore $f(t)$, and hence $\Phi(f(t))$, also increases discretely at the boundary. We therefore proved that $Prob(Entrepreneurship|t)$ always increases in t. □

Lemma 2. $\frac{\partial EU(t)}{\partial d}<0$.

Proof. Let $\sqrt{\frac{1}{\eta_0}-\frac{1}{\eta_0+h_t}}=\sigma$ and $\mu-y\left(1-\delta_t\right)-\frac{\lambda}{2(\eta_0+h_t)}=K$

$$\begin{aligned}
\frac{\partial EU(t)}{\partial \delta} &= -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\phi\left(\frac{K}{\sigma} - \lambda\sigma\right)\frac{y}{\sigma} - \exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) + \exp\left(-\lambda y\left(1-\delta\right)\right)\phi\left(-\frac{K}{\sigma}\right)\frac{y}{\sigma} \\
&= -\exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) + \frac{y}{\sigma}\left[\exp\left(-\lambda y\left(1-\delta\right)\right)\phi\left(\frac{K}{\sigma}\right) - \exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\phi\left(\frac{K}{\sigma} - \lambda\sigma\right)\right] \\
&= -\exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) + \frac{y}{\sigma\sqrt{2\pi}}\left\{\exp\left[-\frac{1}{2}\left(\frac{K}{\sigma}\right)^2 - \lambda y\left(1-\delta\right)\right] - \exp\left[-\frac{1}{2}\left(\frac{K}{\sigma} - \lambda\sigma\right)^2 + \frac{\lambda^2}{2\eta_0} - \lambda\mu\right]\right\} \\
&= -\exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) + \frac{y}{\sigma\sqrt{2\pi}}\left\{\exp\left[-\frac{1}{2}\left(\frac{K}{\sigma}\right)^2 - \lambda y\left(1-\delta\right)\right] - \exp\left[-\frac{1}{2}\left(\frac{K}{\sigma}\right)^2 + \lambda K - \frac{1}{2}\lambda^2\sigma^2 + \frac{\lambda^2}{2\eta_0} - \lambda\mu\right]\right\} \\
&= -\exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) + \frac{y}{\sigma\sqrt{2\pi}}\left\{\exp\left[-\frac{1}{2}\left(\frac{K}{\sigma}\right)^2 - \lambda y\left(1-\delta\right)\right] - \exp\left[-\frac{1}{2}\left(\frac{K}{\sigma}\right)^2 - \lambda y\left(1-\delta\right)\right]\right\} \\
&= -\exp\left(-\lambda y\left(1-\delta\right)\right)\lambda y \Phi\left(-\frac{K}{\sigma}\right) < 0
\end{aligned}$$

Since $\frac{\partial \delta}{\partial d} > 0$, $\frac{\partial EU(t)}{\partial d} = \frac{\partial EU(t)}{\partial \delta}\frac{\partial \delta}{\partial d} < 0$. □

Lemma 3. *When $d = 0$, $t^* \to +\infty$.*

Proof. Let $g(t) = \frac{\mu - y(1-\delta_t) - \frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}} - \lambda\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}} = f(t) - \lambda\sigma_t$. When $d = 0$, $\delta_t = 0$. In this case,

$$\begin{aligned}
EU(t)|_{\delta=0} &= -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\Phi\left(g(t)|_{\delta=0}\right) - \exp\left(-\lambda y\right)\Phi\left(-f(t)|_{\delta=0}\right) \\
&= -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\Phi\left(\frac{\mu - y + \frac{\lambda}{2(\eta_0+h_t)} - \frac{\lambda}{\eta_0}}{\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}}\right) - \exp\left(-\lambda y\right)\Phi\left(\frac{-\mu + y + \frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}}\right)
\end{aligned}$$

$$g^{'}(t)|_{\delta=0} = \left(\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}\right)^{-1}\left\{-\frac{\lambda}{4h_t(\eta_0+h_t)^2}\left[\frac{h_t}{\eta_0(\eta_0+h_t)}\right]^{\frac{1}{2}} + \left(\mu - y + \frac{\lambda}{2\left(\eta_0+h_t\right)} - \frac{\lambda}{\eta_0}\right)\left[\frac{h_t}{\eta_0(\eta_0+h_t)}\right]^{-\frac{1}{2}}\frac{1}{4h_t(\eta_0+h_t)^2}\right\}$$

Since $\mu - y + \frac{\lambda}{2(\eta_0+h_t)} - \frac{\lambda}{\eta_0} = \left(\mu - y - \frac{\lambda}{2\eta_0}\right) - \frac{1}{2}\lambda\left(\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}\right) < 0$, $g^{'}(t)|_{\delta=0} < 0$. We also proved from Lemma 1 that $-f^{'}(t) < 0$. It is therefore immediate that $EU(t)|_{\delta=0}$ increases in t. Hence, $t^* \to +\infty$ when $d = 0$. □

Lemma 4. *Assuming $\delta(t) = dt$ for all t, there exists a value $\tilde{d}$ so that when $d > \tilde{d}$, $t^* = 0$.*

Proof. Recall that $f(t) = \frac{\mu - y(1-\delta_t) - \frac{\lambda}{2(\eta_0+h_t)}}{\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}}$ and $g(t) = f(t) - \lambda\sigma_t$. Rewrite $EU(t)$ as $EU(t) = -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\Phi\left(g(t)\right) - \exp\left(-\lambda y\left(1-\delta_t\right)\right)\Phi\left(-f(t)\right)$. It can be shown that

$$
\begin{aligned}
EU'(t) &= -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\phi(g(t))g'(t) - \exp\left(-\lambda y\left(1-\delta_t\right)\right)\Phi\left(-f(t)\right)\lambda yd + \exp\left(-\lambda y\left(1-\delta_t\right)\right)\phi\left(-f(t)\right)f'(t) \\
&= -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\phi(g(t))\left[f'(t) - \lambda\sigma'\right] - \exp\left(-\lambda y\left(1-\delta_t\right)\right)\Phi(-f(t))\lambda yd + \exp\left(-\lambda y\left(1-\delta_t\right)\right)\phi\left(f(t)\right)f'(t)
\end{aligned}
$$

From the proof of Lemma 2 we know that $\exp\left(-\lambda y\left(1-\delta_t\right)\right)\phi(f(t)) = \exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right)\phi(g(t))$. Therefore we have

$$
\begin{aligned}
EU'(t) &= -\exp\left(-\lambda y\left(1-\delta_t\right)\right)\phi\left(f(t)\right)\left[f'(t) - \lambda\sigma'\right] - \exp\left(-\lambda y\left(1-\delta_t\right)\right)\Phi\left(-f(t)\right)\lambda yd + \exp\left(-\lambda y\left(1-\delta_t\right)\right)\phi\left(f(t)\right)f'(t) \\
&= \lambda\exp\left(-\lambda y\left(1-\delta_t\right)\right)\left[\phi\left(f(t)\right)\sigma' - \Phi\left(-f(t)\right)yd\right]
\end{aligned}
$$

Note that $\sigma' = \left(\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}\right)' > 0$. There exists $\tilde{d} = \underset{t\geq 0}{max}\frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ such that when $d > \tilde{d}$, $EU'(t) < 0$ for all $t \geq 0$, and hence $t^* = 0$. □

Lemma 5. *Assuming $\delta(t) = dt$ for all t, $EU''(t) < 0$.*

Proof. We know $EU'(t) = \lambda\exp\left(-\lambda y\left(1-\delta_t\right)\right)\left[\phi\left(f\left(t\right)\right)\sigma' - \Phi\left(-f\left(t\right)\right)yd\right]$, therefore

$$
\begin{aligned}
EU''(t) &= \lambda\exp\left(-\lambda y\left(1-\delta_t\right)\right)\left\{\lambda yd\phi\left(f(t)\right)\sigma' - \Phi\left(-f(t)\right)\lambda y^2 d^2 - f(t)\phi\left(f(t)\right)\sigma' + \phi(f(t))\sigma'' + \phi\left(f(t)\right)f'(t)yd\right\} \\
&= \lambda\exp\left(-\lambda y\left(1-\delta_t\right)\right)\left\{\phi\left(f(t)\right)\left[\lambda yd\sigma' - f(t)\sigma' + \sigma'' + f'(t)yd\right] - \Phi\left(-f(t)\right)\lambda y^2 d^2\right\}
\end{aligned}
$$

$\sigma' = \left(\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}}\right)' = \left(\frac{1}{\eta_0} - \frac{1}{\eta_0+h_t}\right)^{-\frac{1}{2}}\frac{1}{4h_t(\eta_0+h_t)^2}$. It can be easily seen that σ' decreases in h_t and therefore decreases in t. Hence $\sigma'' < 0$.

Note that $\sigma' = \frac{\eta_0}{4h_t^2(\eta_0+h_t)}\sigma$, $\sigma'' = \frac{-\left(\frac{3}{4}\eta_0+\frac{3}{2}h_t\right)}{h_t^2(\eta_0+h_t)}\sigma' = \frac{-\frac{1}{4}\eta_0\left(\frac{3}{4}\eta_0+\frac{3}{2}h_t\right)}{h_t^4(\eta_0+h_t)^2}\sigma$, and $f' = (yd + \frac{\lambda}{4h_t(\eta_0+h_t)^2})\sigma^{-1} - f\frac{\eta_0}{4h_t^2(\eta_0+h_t)}$, therefore

$$
\begin{aligned}
yd(f'(t)+\lambda\sigma') - f(t)\sigma' + \sigma'' &= yd\left\{\left[yd + \frac{\lambda}{4h_t\left(\eta_0+h_t\right)^2}\right]\sigma^{-1} - f\frac{\sigma'}{\sigma} + \lambda\sigma'\right\} - f\sigma' - \frac{\frac{3}{4}\eta_0 + \frac{3}{2}h_t}{h_t^2\left(\eta_0+h_t\right)}\sigma' \\
sign: yd(f'(t)+\lambda\sigma') - f(t)\sigma' + \sigma" &= sign: yd\left\{\left[yd + \frac{\lambda}{4h_t\left(\eta_0+h_t\right)^2}\right]\sigma^{-1}\sigma'^{-1} - f\sigma^{-1} + \lambda\right\} - f - \frac{\frac{3}{4}\eta_0 + \frac{3}{2}h_t}{h_t^2\left(\eta_0+h_t\right)} \\
&= sign: yd\left\{4ydh_t\left(\eta_0+h_t\right)^2 - f\sigma^{-1} + 2\lambda\right\} - f - \frac{\frac{3}{4}\eta_0 + \frac{3}{2}h_t}{h_t^2\left(\eta_0+h_t\right)} \\
&= sign: 2yd\left\{2ydh_t\left(\eta_0+h_t\right)^2 + \lambda\right\} - f\left(1 + yd\sigma^{-1}\right) - \frac{\frac{3}{4}\eta_0 + \frac{3}{2}h_t}{h_t^2\left(\eta_0+h_t\right)}
\end{aligned}
$$

It can be shown that $2yd\left\{2ydh_t\left(\eta_0+h_t\right)^2 + \lambda\right\} - f\left(1 + yd\sigma^{-1}\right) - \frac{\frac{3}{4}\eta_0+\frac{3}{2}h_t}{h_t^2(\eta_0+h_t)} < 0$. Therefore $EU''(t) < 0$. □

Lemma 6. *Assuming $\delta(t) = dt$ for all t, there exist values $\underset{\sim}{d}$ and $\tilde{d}$ such that $\frac{\partial t^*}{\partial d} < 0$ when $\underset{\sim}{d} \leq d \leq \tilde{d}$, and $\frac{\partial t^*}{\partial d} = 0$ when $d \leq \underset{\sim}{d}$ or $d \geq \tilde{d}$.*

Proof. From the proof of Lemma 4 we know that there exist $\underset{\sim}{d} = \underset{t\geq 0}{min}\frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ and $\tilde{d} = \underset{t\geq 0}{max}\frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ such that $EU'(t) > 0$ for all $t \geq 0$ when $d \leq \underset{\sim}{d}$, and $EU'(t) < 0$ for all $t \geq 0$ when $d > \tilde{d}$. In these cases, we obtain corner solutions for t^* and $\frac{\partial t^*}{\partial d} = 0$.

When $\underset{\sim}{d} \leq d \leq \tilde{d}$, there is an interior solution for t^*, which solves the equation $\frac{\phi(f(t))\sigma'}{1-\Phi(f(t))} = yd$. Let $G(t,d) = \frac{\phi(f(t))\sigma'}{1-\Phi(f(t))} = IMR(f(t))\sigma'$, where IMR is the inverse-Mills ratio function. Taking derivative with respect to d on both sides of $\frac{\phi(f(t))\sigma'}{1-\Phi(f(t))} = yd$ yields $\frac{\partial G(t^*,d)}{\partial t^*}\frac{\partial t^*}{\partial d} + \frac{\partial G(t^*,d)}{\partial d} = y$. Because in $G(t,d)$, d is only contained in $f(t,d)$, we have $\frac{\partial G}{\partial t^*}\frac{\partial t^*}{\partial d} + \frac{\partial G}{\partial f}\frac{\partial f}{\partial d} = y$. To prove $\frac{\partial t^*}{\partial d}<0$, we only need to prove $\frac{\partial t^*}{\partial d} = \frac{y - \frac{\partial G}{\partial f}\frac{\partial f}{\partial d}}{\frac{\partial G}{\partial t^*}} < 0$, i.e., $\frac{y - \frac{\partial G}{\partial f}\frac{\partial f}{\partial d}}{\frac{\partial G}{\partial t^*}} = \frac{y - IMR(IMR-f)\sigma'\frac{yt}{\sigma}}{IMR(IMR-f)f'\sigma' + IMR\sigma''} = \frac{y}{IMR\sigma'}[\frac{1-IMR(IMR-f)\frac{\sigma'}{\sigma}t}{(IMR-f)f' + \frac{\sigma''}{\sigma'}}] < 0$. This is equivalent to proving $(IMR(IMR-f)\frac{\sigma'}{\sigma}t - 1)((IMR-f)f' + \frac{\sigma''}{\sigma'}) > 0$. We know from the properties of inverse-Mills ratio that $IMR(f) > f$. We therefore only need to prove that the minimum of $K(IMR) = (IMR(IMR-f)\frac{\sigma'}{\sigma}t - 1)((IMR-f)f' + \frac{\sigma''}{\sigma'})$ on $IMR > f$ is positive. To find the minimum of $K(IMR)$, set its first-order condition with respect to IMR to zero, i.e., $K'(IMR) = (2IMR-f)((IMR-f)f' + \frac{\sigma''}{\sigma'})\frac{\sigma'}{\sigma}t + (IMR(IMR-f)\frac{\sigma'}{\sigma}t - 1)f' = 0$. At $K'(IMR) = 0$, $K(IMR) = \frac{(IMR(IMR-f)\frac{\sigma'}{\sigma}t-1)^2 f'}{(2IMR-f)\frac{\sigma'}{\sigma}t} > 0$ (since $IMR > f$). Also note that at the boundary when $IMR = f$, $K(f) = -\frac{\sigma''}{\sigma'} > 0$. Therefore $K(IMR)$ is always positive and therefore $\frac{\partial t^*}{\partial d} = \frac{y - \frac{\partial G}{\partial f}\frac{\partial f}{\partial d}}{\frac{\partial G}{\partial t^*}} < 0$. We therefore proved that t^* decreases in d when $\underset{\sim}{d} \leq d \leq \tilde{d}$. □

Proposition 1. *Assume that* $\mu - \frac{\lambda}{2\eta_0} < y$, *so that prior beliefs are such that no one would enter entrepreneurship absent any signal.*
(i) Full Shielding $(\theta = 0)$:

There exists a $\underline{d}$ *such that:*

For $d \geq \underline{d}$, $t^* = T_L$, $\frac{\partial t^*}{\partial T_L} > 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} > 0$.

For $d < \underline{d}$, $t^* > T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

(ii) Partial Shielding $(0 < \theta < 1)$:

There exists a $\underline{d}$ *and* $\bar{d}$ *such that:*

For $\underline{d} \leq d \leq \bar{d}$, $t^* = T_L$, $\frac{\partial t^*}{\partial T_L} > 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} > 0$.

For $d < \underline{d}$, $t^* > T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

For $d > \bar{d}$, $t^* < T_L$, $\frac{\partial t^*}{\partial T_L} = 0$, *and* $\frac{\partial Prob(Entrepreneur|t^*)}{\partial T_L} = 0$.

Proof. First we define some notation. Let, $EU_0(t)$ represent the expected utility function that would prevail if we let $\delta(t) = \theta dt$ for all t, including $t > T_L$. Similarly define t_0^* as the t that maximizes $EU_0(t)$. Next define:

$$EU_0^*(d) = \max_{t \geq 0} EU_0(t; d, \theta)$$

$$t_0^*(d) = \arg\max_{t \geq 0} EU_0(t; d, \theta)$$

$$t_l^*(d) = \arg\max_{0 \leq t \leq T_L} EU(t; d, \theta)$$

$$EU_l^*(d) = \max_{0 \leq t \leq T_L} EU(t; d, \theta)$$

$$t_r^*(d) = \arg\max_{t > T_L} EU(t; d, \theta)$$

$$EU_r^*(d) = \max_{t > T_L} EU(t; d, \theta)$$

$$t^*(d) = \arg\max_{t \geq 0} EU(t; d, \theta)$$

$$EU^*(d) = \max_{t \geq 0} EU(t; d, \theta)$$

$$\Delta EU^*(d) = EU_l^*(d) - EU_r^*(d).$$

We start with the case of full shielding. With $\theta = 0$, we know from the proof of Lemma 3 that regardless of d, $EU(t)$ is strictly increasing for $t \leq T_L$. By Lemma 3, If $d = 0$, $EU(t)$ is smooth around T_L and $E(t)$ is increasing in t for all t. By Lemma 2 we know that for $d > 0$ there is a discrete drop in $EU(t)$ at $t = T_L$. By Lemma 4, we also know that there exists a $\underline{d}$ such that for all $d > \underline{d}$, $EU(t)$ is decreasing for all $t > T_L$. It follows that for all $d > \underline{d}$, $t^* = T_L$. It also follows that for all $d > \underline{d}$, $\frac{\partial t^*}{\partial T_L} > 0$, because as we said before $EU'(T_L) > 0$. Finally, Lemma 1 shows that, assuming $\mu - \frac{\lambda}{2\eta_0} < y$, the probability of entry into entrepreneurship is increasing in t^*.

Next consider the case of partial shielding. We know from Lemma 3 that when $d = 0$, $t_0^* = +\infty$. Similarly, from Lemma 4, when $d > \tilde{d}$, $t_0^* = 0$. Finally, from Lemma 6, we know that, $\frac{\partial t_0^*}{\partial d} < 0$. Therefore, there must exist a unique crossing point $\bar{d}$ such that $t_0^*\left(\bar{d}\right) = T_L$. Note that, $EU(t) = EU_0(t)$ for all $t \leq T_L$; However, by Lemma 2, $EU(t) < EU_0(t)$ for all $t > T_L$. Therefore, with $d = \bar{d}$ it must be that $t^*\left(\bar{d}\right) = t_0^*\left(\bar{d}\right) = T_L$. We know that $\Delta EU^*\left(\bar{d}\right) > 0$. Since $\Delta EU^*\left(d\right)$ is a continuous function, there must exist a d such that for all $d < \bar{d}$ and it is still the case that $\Delta EU^*(d) > 0$ and $t^*\left(d\right) = T_L$. Let $\underline{d}$ denote the lowest such d.

Given the way that $\bar{d}$ and $\underline{d}$ were defined, we know that for $\underline{d} \leq d < \bar{d}$, $EU_0(t)$ is maximized at some $t_0^*(d) > T_L$. Since Lemma 5 shows that $EU_0^{''}(t) < 0$, $EU_0(t)$ must be strictly increasing up until this maximum and strictly decreasing afterward. Therefore, since $EU(t) = EU_0(t)$ for $t \leq T_L$, it must be the case that $EU'(T_L) > 0$ for $\underline{d} \leq d < \bar{d}$. Combining this with the fact that $t^* = T_L$ implies that $\frac{\partial t^*}{\partial T_L} > 0$.

Given the way that $\bar{d}$ was defined, we also know that for $d > \bar{d}$, $EU_0(t)$ is maximized at some $t_0^*(d) < T_L$. Since Lemma 5 shows that $EU_0^{''}(t) < 0$, $EU_0(t)$ must be strictly increasing up until this maximum and strictly decreasing afterward. Note that, $EU(t) = EU_0(t)$ for all $t \leq T_L$; However, by Lemma 2, $EU(t) < EU_0(t)$ for all $t > T_L$. Thus, $EU(t)$ is also maximized at $t_0^*(d) < T_L$. In other words, $t^*(d) < T_L$. Under these conditions, $\frac{\partial t^*}{\partial T_L} = 0$. By a similar argument, we also know that for $d < \underline{d}$, $t^*(d) > T_L$. Under these conditions, $\frac{\partial t^*}{\partial T_L} = 0$.

Finally, Lemma 1 shows that, assuming $\mu - \frac{\lambda}{2\eta_0} < y$, the probability of entry in entrepreneurship is increasing in t^*. $\square$