

Does Career Risk Inhibit Potential Entrepreneurs?*

Joshua D. Gottlieb
University of Chicago
and NBER

Richard R. Townsend
University of California San Diego

Ting Xu
University of Virginia

November 12, 2019

Abstract

The career risk associated with taking time away from wage employment may prevent individuals from pursuing entrepreneurship, or even from realizing that they would like to pursue it. We use a Canadian reform that extended job-protected leave to one year for women giving birth after a cutoff date to ask whether having the ability to take time away from wage employment—while insulated from adverse career consequences—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.

JEL Classification: L26, J13, J16, J65, J88, H50

Keywords: Entrepreneurship, Risk-Aversion, Job-Protected Leave, Career, Real Option, Experimentation

*Gottlieb: jgottlieb@uchicago.edu. Townsend: rrtownsend@ucsd.edu. Xu: xut@darden.virginia.edu. We are grateful to Francesca Cornelli (the editor), two anonymous referees, Paul Beaudry, Shai Bernstein, Ing-Haw Cheng, Gordon Dahl, Thomas Hellmann, Kai Li, Ben Lockwood, Gustavo Manso, Kevin Milligan, Will Mullins, Ramana Nanda, Carolin Pflueger, Matt Rhodes-Kropf, David Robinson, Elena Simintzi, conference and seminar participants at AEA, AFA, Caltech/USC, NBER Corporate Finance, NBER Entrepreneurship, MFA, Stanford, UBC, UC Chile, HKU, HKUST, CityU of HK, and Tsinghua SEM, and especially our discussants Jeanne Lafortune, Adrien Matray, John Mondragon, Paige Ouimet, Joshua Rauh, Antoinette Schoar, and Christopher Stanton for valuable comments. Gottlieb thanks the Stanford Institute for Economic Policy Research and Federal Reserve Bank of San Francisco for their hospitality while working on this project, and the Social Sciences and Humanities Research Council of Canada for support. We are indebted to Statistics Canada, SSHRC, and the staff at the British Columbia Interuniversity Research Data Centre at UBC for facilitating data access. The research and analysis are based on data from Statistics Canada and the opinions expressed do not represent the views of Statistics Canada.

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 another important impediment may be the career risk associated with taking time away from wage employment. Indeed, a growing literature suggests that individuals who leave wage employment and then return, end up on a worse career trajectory.² By making it difficult to take time away from wage employment for any reason, this kind of career risk may prevent individuals from pursuing entrepreneurship, or even from realizing that they would like to pursue it. In other words, career risk considerations may create a wage employment “hamster wheel” that is difficult to get off of. In this paper, we use a natural experiment to investigate whether such a hamster wheel exists and inhibits entrepreneurship.

Specifically, we study whether an exogenous reduction in the career risk associated with time away from wage employment causes an increase in entrepreneurial entry. The reduction in career risk that we examine comes from wage-employed individuals who are granted the ability to take prolonged leaves of absence from their jobs during which they are guaranteed the ability to return. Our empirical setting is a reform to Canadian maternity leave laws that took place in 2000, which roughly doubled workers’ entitlements to job-protected leave to 12 months. In contrast, the U.S.

¹See, 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 (2009a); Adelino, Schoar, and Severino (2015); Schmalz, Sraer, and Thesmar (2017a); Mullainathan and Schnabl (2010); Bruhn (2011); Branstetter et al. (2014)

²For example, Mincer and Ofek (1982), Light and Ureta (1995), and Albrecht et al. (1999) document that time away from wage employment leads to an earnings discount upon returning to wage employment. Such a discount can be attributed to disruption in human capital accumulation (Mincer and Polachek, 1974) or a signaling effect (Gibbons and Katz, 1991). The literature on self-employment also 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).

only requires employers to offer 3 months of job-protected leave surrounding the birth of a child. Given that U.S. law expects employees to return to wage employment after 3 months, it is possible that the substantial additional time that workers in Canada are allowed may enable them to consider entrepreneurial career paths, even with a new child in the household. While maternity leave may not be the first thing that comes to mind when considering sparks for entrepreneurship, our setting provides stark variation in the career risk associated with time away from wage employment.

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

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

To ensure that our estimates are not confounded by contemporaneous macroeconomic shocks or

other policies that change discontinuously at year’s end, our main empirical strategy compares the regression discontinuity estimate for mothers, who availed themselves of this extended leave, with that for fathers, who did not. That is, we implement a “difference-in-discontinuities” design (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 discontinuous jump in the birth rate after the cutoff date. Moreover, those who gave birth just before and after the cutoff have similar ages, education levels, and ethnicities. These findings suggest that 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. We find that the increase in job-protected leave entitlements leads to approximately a 1.9 percentage point increase in entrepreneurship rates among mothers relative to fathers. This baseline result is robust to adding various controls, examining different windows around the cutoff date, using different methods of fitting the pre- and post-policy time trends, and defining entrepreneurship in different ways. As one would expect, we find larger effects in provinces that had larger increases in job-protected leave. Moreover, we find no effects around various false, placebo cutoff dates. Finally, we also find similar effects at shorter time horizons using a separate data set.

Several pieces of evidence suggest that the incremental entrepreneurs we observe are engaging in meaningful businesses. First, we measure entrepreneurship continuing 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. In addition, we also find that the reform leads to an increase in entrepreneurs that incorporate their firms, hire

paid employees, work above the median number of hours, and earn above the median income. We also find that 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).

There are two potential mechanisms that may underly our results. First, individuals who already have entrepreneurial ideas and preferences may take longer leaves, when allowed, to explore their ideas without exposure to career risk. This would be a *direct* mechanism through which job-protected leave availability increases entrepreneurship. Second, individuals who do not have entrepreneurial ideas or preferences may instead take longer leaves, when allowed, to pursue non-entrepreneurial objectives (e.g., parental objectives) without exposure to career risk. However, such individuals may also develop entrepreneurial ideas and/or preferences as a result of their additional time away from wage employment. This would be an *indirect* mechanism through which job-protected leave availability increases entrepreneurship.

These two mechanisms are closely related, with the main difference between them having to do with individuals' initial motivations for making use of additional job-protected leave time. Thus, they are difficult to separate empirically. But both mechanisms represent ways in which reducing the career risk associated with time away from wage employment causes an increase in entrepreneurship. Thus, both suggest that the presence of such career risk inhibits entrepreneurship by creating a wage employment hamster wheel. The main contribution of this paper is to establish this fact.

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 only new mothers (e.g., Hurst and Pugsley, 2011).

Indeed, 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—not associated with having children—such a policy might lead to the creation of technology startups.³ Sweden, in fact, requires that workers be allowed to take up to 6 months of job-protected leave if they want to try to start a business. Our results are also related to a commonly-held view that part of the value of business school for students is to provide them with time away from wage employment that is not penalized by the labor market. Much like job-protected leave, such time may then directly or indirectly foster entrepreneurship.⁴

Our paper relates to a growing literature that views entrepreneurship as a series of experiments (see Kerr, Nanda, and Rhodes-Kropf, 2014, for an overview). While many entrepreneurial projects may be negative 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 related work, Manso (2016), Dillon and Stanton (2016), and Catherine (2019) model the dynamics of experimentation in self-employment and quantify this option value.⁵ 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.

³Of 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 career risk inhibits entrepreneurship.

⁴For example, Warby Parker was founded by four students at Wharton, who continued to participate in MBA recruiting while simultaneously launching their business. All four had accepted jobs to start after graduation in case Warby Parker did not look promising at that point (Grant, 2017).

⁵Catherine (2019) also shows theoretically that reducing the wage penalty faced by entrepreneurs returning to wage employment should increase entry into entrepreneurship.

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, 2009a; Adelino, Schoar, and Severino, 2015; Schmalz, Sraer, and Thesmar, 2017a); whether entrepreneurship training programs or exposure to entrepreneurial peers increases entrepreneurship (Karlan and Valdivia, 2011; Lerner and Malmendier, 2013a; 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).⁶ Our paper differs in its focus on the career risk associated with taking time away from wage employment and its potential to create a wage employment hamster wheel. We are not aware of any other work examining the effect of career risk on entrepreneurship. Our findings are consistent with Manso (2011), who shows that the optimal contract to motivate innovation (or experimentation more generally) involves a commitment by the principal not to fire the agent.

2 Data

Our primary dataset comes 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,

⁶Our paper is related to Hombert et al. (2014) in that we study a form of employment insurance. However, the reform that Hombert et al. (2014) study only applies to unemployed individuals and is thus unrelated to career risk or the notion that there may be a wage employment hamster wheel. 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 existing entrepreneurs.

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.⁷

One key variable for this study is the date on which parents had a child. While the census does not directly record this information, it can be inferred fairly well. In particular, the census records family relationships within a household and the date of birth for all members of the household. So we assume that parents had children on the birthdates of the children residing in the same household as themselves.⁸ 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

⁷Due 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.

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

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.⁹ Separately, respondents must also report whether they consider themselves self-employed based on their primary job. We show in robustness tests that our results are similar when using self-reported entrepreneurship.

Table 1 shows basic summary statistics for mothers and for fathers who had their first child within 60 days of the December 31, 2000 reform date. While the sample is selected based on the inferred birth of a child around December 31, 2000, the summary statistics reflect information as of the 2006 census. In our sample, 4.2 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.¹⁰ The last two lines of the table show income and work hours. These are about twice as large for fathers as for mothers, with the differences largely driven by the extensive margin: 39 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

⁹Canadian 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.

¹⁰Note 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.

their U.S. counterparts.

3 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).¹¹ First, employees are protected from dismissal due to pregnancy. Second, a maximum period for the leave is always prescribed, and the provinces do not mandate any paid leave. Initially, the laws of several provinces provided guidance on how the period of leave should be split pre- and post-birth, but current practice is to leave this to the discretion of the mother and employer. Third, the laws specify a minimum period of employment for eligibility. This varies widely: initially, 52 weeks of employment was common, although the recent trend is toward shorter qualification periods. Fourth, most laws specify which terms of employment are preserved during the leave and any responsibility of the employer to maintain benefits. Finally, the laws of some provinces establish rules for extending leaves because of medical complications or pregnancies that continue after term.

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

¹¹In 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.

2001 reform to the Employment Insurance Act, provinces required that employers grant anywhere from 18 to 35 weeks of job-protected leave surrounding the birth of a child. Following the reform, all provinces increased the mandated guarantee to at least 52 weeks to match the new income replacement period set by Employment Insurance (including the 2-week waiting period). Following the reform, 35 out of the 52 weeks of job-protected leave could technically be split between the two parents however they preferred. In practice, very few families allocated job-protected leave time to fathers instead of mothers (Baker and Milligan, 2008a).

Table 2 shows the maximum leave period by province, before and after the 2001 reform. Quebec is excluded from the table as it is the one province that did not change in 2001, mandating 70 weeks of job-protected leave throughout. The average province went from approximately 30 weeks to 52 weeks, an increase of approximately 5.5 months. Given that maternity leave entitlements usually increase gradually over time, this reform represents one of the largest year-over-year increases in any country.

A key aspect of the reform's implementation for our purposes is that it was tied to the date a woman gave birth. Those who gave birth on or after December 31, 2000, were entitled to an extended leave. Those who gave birth even a day before were not. Despite unhappiness among those who just missed the cutoff, this policy admitted no exceptions (Muhlig, 2001).

In terms of the timing of the reform's announcement, the federal budget was announced on February 29, 2000, with the December 31, 2000, cutoff date for extended income replacement eligibility. In principle, this announcement 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.¹² Thus,

¹²Two 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.

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.

4 Empirical Strategy

While there was a discontinuous jump in the maximum length of leave available to parents who had children around the reform cutoff, this does not necessarily mean that there was a change in actual leave-taking, as individuals may not have made use of the additional time available. If the reform had no effect on actual leave-taking, we would not expect to find an effect on entrepreneurship. While respondents do not retrospectively report the length of previous leaves taken, which would allow us to look for a discontinuous jump in leave-taking around the cutoff date, they do report whether they are currently on leave as of the census date. We, therefore, trace out the probability of a respondent being on leave on the census date as a function of the number of weeks between the date of birth of the respondent's youngest child and the census date. We do this separately using data from the 1996 and 2006 censuses, which are 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 0.95.¹³ 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.

¹³This calculation is based on annual entrepreneurship rates from the Canadian Labor Force Survey from 1995–2010.

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:

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

where y_i is an outcome of interest for mother i ; $DayChild_i = DateChild_i - C$, which is the date mother i had a child ($DateChild_i$) relative to the reform cutoff date (C); and $Post_i = \mathbb{1}\{DayChild_i \geq 0\}$, which is an indicator equal to one if mother i gave birth on or after the cutoff date. The control functions capture time trends around the cutoff. In particular, the function $f_1(\cdot)$ captures a smooth trend on the left side of the cutoff, and the sum, $f_1(\cdot) + f_2(\cdot)$, captures a (potentially different) smooth trend on the right side of the cutoff. The estimated coefficient $\hat{\tau}$ captures any discontinuity between these trends at the cutoff.

The RDD literature recommends estimating equation (1) using local linear regression (see e.g., Fan et al., 1996; Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Gelman and Imbens, 2017).¹⁴ This means limiting the sample to mothers who gave birth within a narrow bandwidth, h , around the cutoff date and assuming linear control functions. Within a narrow bandwidth, it is reasonable to assume that the control functions are approximately linear. One could also allow the control functions to follow higher order polynomials. Our baseline estimates use local linear control functions since Gelman and Imbens (2017) show higher order polynomials to be problematic, but we test our results’ robustness to both methods. Observations within the specified bandwidth around the cutoff are weighted according to some kernel function. A triangular kernel, which puts more weight on observations near the cutoff, has been shown to be optimal in the RDD literature (Fan et al., 1996).¹⁵

¹⁴For 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).

¹⁵In practice, the choice of kernel generally makes little difference (Imbens and Lemieux, 2008)

For a given bandwidth and kernel, one can then estimate local linear regressions of the form:

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

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:

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

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

Our primary outcome of interest is an indicator equal to one if individual i is an entrepreneur as of the 2006 census date, as defined in Section 2. 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. We do examine shorter-term outcomes, however, using supplemental data from the Longitudinal Administrative Databank in Section 6.8 below.

5 Interpretation

5.1 Causal Effect of Reduced Career Risk

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

An important caveat to the above discussion is that the reform we study extended not only the job-protection period but also the income replacement period. This means that we are actually estimating a joint effect of these two aspects of the reform. In Section 6.7, we show evidence that an increase in the income replacement period alone has little impact in our setting. This evidence is also consistent with Dahl et al. (2016) and Stearns (2016), who find that increases in income replacement without changes in job protection have little effect on a wide variety of outcomes. Thus, it does seem that the job-protection aspect of the reform is critical. However, we acknowledge that we cannot say conclusively that an extension in job-protection alone would be enough to spur additional entrepreneurship. It is possible that decreasing career risk does spur entrepreneurship, but only when coupled with at least some passive income. In other words, the wage employment hamster wheel may be driven by the career risk associated with taking time away from wage employment together with a need for at least a low level of uninterrupted income.

5.2 Potential Mechanisms

Before presenting our results, we first consider potential mechanisms through which reducing the career risk associated with time away from wage employment could cause an increase in entrepreneurship. First, there may be individuals who have entrepreneurial ideas and know that they have a preference for an entrepreneurial lifestyle. When faced with a reduction in the career risk associated with a gap in wage employment, such individuals might seize the opportunity to take time away from wage employment to explore their entrepreneurial ideas. This would represent a *direct* mechanism. Appendix B provides a model to illustrate formally how such a mechanism might operate.

Alternatively, there may also be individuals who do not have entrepreneurial ideas or who do not know that they have a preference for an entrepreneurial lifestyle. When faced with a reduction in the career risk associated with a gap in wage employment, such individuals might take time away from wage employment for non-entrepreneurial reasons. However, during that time away from wage employment, they may develop entrepreneurial ideas or preferences. In the context of our empirical setting, it could be that the maternity leave extension reduces career risk, leading workers to take longer leaves primarily for parental reasons. Nonetheless, these longer leaves may lead these workers to have business ideas unexpectedly, or to realize that they would prefer an entrepreneurial lifestyle (e.g., job flexibility). This would represent an *indirect* mechanism.

The difference between these two mechanisms is subtle, primarily having to do with the initial motivation for taking time away from wage employment in response to a reduction in the associated career risk from doing so. Therefore, differentiating between the two empirically is very difficult. Moreover, it is possible that both mechanisms operate simultaneously.

Both mechanisms represent ways in which reducing the career risk associated with time away from wage employment causes an increase in entrepreneurship. As far as we are aware, this is the first paper to examine whether the career risk associated with time away from wage employment inhibits individuals from becoming entrepreneurs. Thus, rather than distinguishing between the direct and indirect mechanisms, our primary contribution is to establish whether there is a wage-employment

hamster wheel driven by career risk, which prevents individuals from pursuing entrepreneurship, or even from realizing that they would like to pursue it.

5.3 The Counterfactual

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

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

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

job-protected leave availability, such individuals should simply quit their job, try entrepreneurship (their first choice), and then be out of the labor force (their second choice) if their business fails.

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 birthdate. 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

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

¹⁷More 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).

on their ability to become an entrepreneur.

To test for gaming in an RDD framework, McCrary (2008) suggests checking for a discontinuity in sample density at the cutoff. Indeed, in our context, gaming would imply that births that would otherwise have occurred prior to December 31 instead occur after. Moreover, it is likely easier to delay a birth that would have otherwise occurred close to the cutoff date than one that would have occurred far in advance. 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

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

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

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. For all bandwidths, we find no significant discontinuity in the birth rate at the reform cutoff date. The point estimates are positive, but insignificant both statistically and economically. If we take the point estimates seriously, despite being statistically indistinguishable from zero, they imply that 12.55 to 17.5 births in total across Canada may have been shifted from the 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 December 31 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.

As shown in Appendix Figure A.1, the graphical results look similar with a 60 day bandwidth. Appendix Figure A.2 also shows that there is no discontinuity in birth rates around the cutoff date in the previous year, when there was no reform. The absence of gaming is consistent with Baker and Milligan (2014), who find that the reform had no effect on the spacing of births.

Given that there is no evidence of gaming, 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 a version of equation (2), but 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. These results are also shown graphically in Appendix Figure A.3.

Why do individuals not try harder to end up on the right side of the cutoff? There are two ways in which they could potentially do so. First, they could time their pregnancies so as to be assured of giving birth after the cutoff date (i.e., “pregnancy timing”). Second, those who were pregnant around the cutoff date could potentially influence the timing of when they gave birth (i.e., “birth timing”). Clearly, birth timing is more difficult than pregnancy timing. The tests above are primarily designed to check for birth timing, which would result in a discontinuity in births at the cutoff date. It is less likely that pregnancy timing would lead to such a discontinuity, given variation in the lengths of pregnancies as well as in the amount of time it takes to conceive. Even if the cutoff date were perfectly anticipated and many individuals delayed pregnancies as a result, such delays would likely lead to a downward trend in birth rates leading up to the cutoff and an upward trend following the cutoff, with no discontinuity at the cutoff. We do see a subtle pattern along these lines in Appendix

Figure A.1. However, pregnancy timing may have been limited since, as discussed in Section 3, the cutoff date for extended job-protected leave was not perfectly anticipated. In addition, pregnancy timing can only occur for planned pregnancies, while survey evidence suggests that approximately 27% of pregnancies in Canada that are carried to term are not planned (Oulman et al., 2015).

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

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.¹⁸

The results are shown in Panel A of Table 4. The row labeled *Treatment* corresponds to the estimated coefficient $\hat{\tau}$ on $Mother_i \times Post_i$ in equation (3), which is our estimated average treatment effect. As Panel A shows, we find a positive and statistically significant average treatment effect. This means that, relative to fathers, mothers giving birth right after the cutoff date are discontinuously more likely to be entrepreneurs than those giving birth right before. The estimated magnitudes are quite stable using different bandwidths. Across the different specifications, we find a 1.9 to 2.1 percentage point increase in entrepreneurship following the policy change.

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.¹⁹

The full set of coefficient estimates that generate this figure are shown in column (1) of Appendix Table A.5. 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

¹⁸Appendix Table A.4 shows results for narrower bandwidths (20 days and 10 days) as well.

¹⁹It 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.

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 4, our difference-in-discontinuities strategy should largely address concerns that our results are driven by year-end effects. Nonetheless, to further rule out such concerns, we perform several placebo tests. First, we examine whether there is a discontinuous jump in 2006 entrepreneurship rates for mothers who had a child around December 31 of 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 non-reform years from 1991 to 2005. We then estimate a variant of equation (3), in which we interact all of our covariates with a $Reform Year_i$ 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 estimate is around December 31 in the actual reform year. Again as we would expect, we find a significantly larger treatment effect in the reform year.²⁰

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.6, 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

²⁰To 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 $Reform Year_i \times Mother_i \times Post_i$.

effects. In addition, to help rule out other events occurring during December 2000 or January 2001, we also consider placebo tests that shift the cutoff date backward 60 days and forward 60 days. As shown in Appendix Table A.7, we do not find any discontinuity around these placebo cutoff dates.

The magnitudes of our point estimates are also economically significant. For example, the coefficient estimated in Table 4, column (4), implies a 45% increase in the probability of entering entrepreneurship relative to the base rate among mothers in our sample. If anything, this may seem too large in percentage terms. However, we note that there is a fairly wide confidence interval around this point estimate, implying anywhere from an 11% increase in entrepreneurship to an 80% increase in entrepreneurship. The literature typically finds estimates in this range.

For example, one of the papers most closely related to ours is Hombert et al. (2014). They find that the reform to French unemployment insurance that they study increases the number of new firms started by unemployed individuals from 5,660 per month to 8,966 per month. This corresponds to a 58% increase in the probability of an unemployed individual in France entering entrepreneurship following the reform.

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

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

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. Finally, in Panel D, we also expand the sample to include births of non-first children. We again find similar results among that sample.

In a valid RDD or difference-in-discontinuities setting, controlling for additional covariates should not be necessary, as such covariates should not change discontinuously across the cutoff. Only the treatment should change discontinuously across the cutoff. Therefore, any discontinuity in the outcome variable can be attributed to the treatment rather than other confounding factors. Nonetheless, in Table 7, we test whether our results are robust to the inclusion of controls. Column (1) repeats the baseline regression from Table 4 column (4). Columns (2) through (8) sequentially add ethnic origin fixed effects (225 categories), education level fixed effects (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). 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 Across Provinces

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

In Table 8, we estimate a variant of equation (3), in which we interact all variables with the change in available weeks of leave in an individual's province. To make the estimates easier to interpret, we standardize the change in available leave time by subtracting its mean and dividing by its standard deviation. Thus, the coefficient 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).²¹ 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.

6.5 Business Quality

We next consider the types of firms that these individuals are starting: are they small, transitory ventures or do they engage in substantive economic activities? The first evidence on this point is that we measure businesses that still exist 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

²¹To 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 $\Delta Weeks Leave_i \times Mother_i \times Post_i$.

businesses would cease operating within that time frame.

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

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

It 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). Related to this, we do not find the *difference* between the effect of job-protected leave on high-quality and low-quality entrepreneurship to be statistically significant in most cases (as indicated by the p-values reported at the bottom of each panel). Thus, the main finding from Table 9 is that the reform does appear to increase high-quality entrepreneurship—not that it increases high-quality entrepreneurship more than low-quality entrepreneurship.

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—such a policy might lead to the creation of technology startups.

6.6 Industry Characteristics

Next, we examine the type of industries that entrepreneurs enter. To do so, 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 Industries for failure rates are based on 3-digit NAICS codes and industries for startup capital are based on 2-digit NAICS codes, the finest level available in the Survey of Business Owners. We then decompose our dependent variable according to each of these measures. For startup capital requirements, we define an indicator variable equal to one if an individual is an entrepreneur working in a high-startup-capital industry and another if the individual is an entrepreneur working in a low startup capital industry. These two variables add up to our original dependent variable. We categorize industries based on the median among the entrepreneurs in our sample. Thus, we categorize roughly half of the entrepreneurs in our sample as

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

high-startup-capital entrepreneurs and half as low-startup-capital entrepreneurs. This helps ensure that there is no mechanical tendency for the reform to increase one type of entrepreneurship more than the other based on the general prevalence of that type of entrepreneurship.²³ We decompose entrepreneurship based on industry failure rates analogously.²⁴

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. The difference is significant at $p < 0.1$. In columns (3) and (4), we find that extending job-protected leave increases high-risk entrepreneurship, but has no effect on low-risk entrepreneurship. In this case, the difference is significant at $p < 0.01$. As one would expect, the coefficients in each pair of columns add up to our baseline estimate.

This analysis sheds light on the characteristics of the industries entrepreneurs choose when the reform spurs them to enter self-employment. In particular, the entrepreneurs that we study enter industries where the cost of experimentation is low (i.e., low startup capital requirements) or the benefits of experimentation are high (i.e., high exit rates). While these results are interesting, they do not necessarily distinguish between the direct and indirect mechanisms. Regardless of whether an individual is interested in entrepreneurship prior to taking a job-protected leave (as under the direct mechanism) or develops such an interest during a job-protected leave (as under the indirect mechanism), she may subsequently tend to pursue ideas in industries where experimentation has low costs or high benefits.

6.7 Job-Protection vs. Income Replacement

As discussed in Section 3, the reform that we study simultaneously increased the job-protection period and the income-replacement period. However, despite the presence of income replacement,

²³In Appendix Table A.8, we also show that results remain very similar when we exclude the five most prevalent industries among entrepreneurs from the analysis. Appendix Table A.9 shows the distribution of entrepreneurs across industries.

²⁴Examples of low startup capital industries in our sample include professional services, construction, retail trade, and educational services; examples of high failure rate industries include professional services, transportation and warehousing, performing arts, and environmental services.

it is important to note that our results are distinct from the literature on financial constraints and entrepreneurship. 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, as people earn significantly *lower* income while on leave. If someone had an idea but insufficient capital to pursue it, she would still have insufficient capital while on leave.

Nonetheless, we consider the possibility that the increase in the income-replacement period drives our results, rather than the increase in the job-protection period. First, we note that we find stronger effects in provinces that had larger increases in the job-protection period. If changes in the income-replacement period were driving our results, we would not expect this, as all provinces had the same increase in the income replacement period. Second, to further disentangle the effects of job-protection and income replacement, we repeat our baseline analysis using the sample of parents who gave birth in Quebec. Quebec increased job-protected leave to 70 weeks many years earlier and did not change it along with the other provinces. Thus, a mother who gave birth just after December 31, 2000, in Quebec would be eligible for a longer income replacement period than one who gave birth before, but not a longer job-protection period. In Appendix Table A.10, 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, these results suggest that an income replacement extension alone is not enough to spur additional entrepreneurship. They are also consistent with Dahl et al. (2016) and Stearns (2016), who find that increases in income replacement without changes in job protection have little effect on a wide variety of outcomes. Importantly, however, we acknowledge that we do not know if an extension in job-protection alone would be enough to spur entrepreneurship either. This is difficult to test, as we do not have a situation in which there is an extension in job-protected leave that is not paired with an extension in income replacement. Thus, it is possible that decreasing career risk does spur entrepreneurship, but only when coupled with at least some passive income. In other words, the wage employment hamster wheel could be driven by the career risk associated with taking time

away from wage employment—or by that risk combined with a need to maintain a minimal income.

6.8 Short-Run Effects

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

To examine short-run effects, we use the Longitudinal Administrative Databank (LAD). The LAD is another confidential data set provided by Statistics Canada. It is based on a random sample of individual tax filings and has an annual frequency.²⁵ Once an individual is included in the LAD, the individual remains in the sample and can be consistently identified over time. The standard version of the LAD does not provide information on children’s birthdates. However, for this project, Statistics Canada created a customized version of the data set that includes children’s birthdates.²⁶ The primary weakness of the LAD relative to the census is that it only provides tax-related data. Therefore, it lacks demographic information about workers. It also lacks self-reported information about the extent and nature of their entrepreneurial activities. We thus use the census for our primary analysis but use the LAD to explore short-run effects.

One additional complication in looking at short-run effects is that new businesses may not generate substantial income early on and likely even generate losses in many cases. Therefore, in order to detect short-run entry as well as possible in the LAD, we broaden our definition of entrepreneurship. Specifically, we consider an individual to be an entrepreneur if she reports any net self-employment income, positive or negative, on her tax return.²⁷ Using this definition of

²⁵The random sampling for the LAD is completely independent of the random sampling for the long-form census.

²⁶Specifically, Statistics Canada was able to add child birthdates to the data set for a large subset of individuals, starting in 2006. We used this information to backfill previous years.

²⁷Negative net self-employment income can be deducted from other positive income on an individual’s tax return from the same year. It can also be carried back for up to 3 years and carried forward for up to 7 years. In order to

entrepreneurship, we then repeat our baseline analysis in 2002, the first full calendar year following our birth window. Because the LAD has a panel dimension, in this case we can also narrow our focus to the most relevant group for our hypothesis—those who were exclusively wage employed prior to childbirth. Accordingly, we limit the sample to individuals who had wage employment income in the year prior to the cutoff date and who also did not have any net self-employment income (positive or negative) in that year. As can be seen in Table 11, Panel A, we continue to find a significant effect in the short run. Among individuals who were exclusively wage employed prior to childbirth, those who qualified for the extended job-protected leave were more likely to enter entrepreneurship immediately after.

In Panel B, we also take advantage of the annual frequency of the LAD to examine the dynamics of our estimated effects. We repeat the analysis of Table 11 in each year from 2001 to 2005 (the latter of which corresponds to the 2006 census data).²⁸ In 2001, we find a smaller, marginally significant effect. This makes sense, as many individuals in our sample were likely on job-protected leave during most, or all, of that year. To the extent that they explored a business idea, it may not have shown up on their tax returns. As Panel A showed, we find a much larger effect in 2002. The effect continues to increase in 2003, as additional businesses become detectable. In 2004 and 2005, the effect diminishes somewhat, presumably reflecting business failures. In terms of magnitudes, the coefficients that we estimate here are larger than in our baseline analysis. However, under the broader definition of entrepreneurship that we use in this analysis to detect short-run entry, the base rate of entrepreneurship among parents is roughly double that in our baseline analysis (8.7% among mothers, 13.5% among fathers). Thus, in percentage terms, the magnitudes converge to a similar range by 2005.

The LAD also allows us to observe income from employment insurance payments. As a “sanity check,” in Appendix Table A.11, we repeat the analysis of Table 11, Panel B, using employment

claim negative net self-employment income, one’s business must have a reasonable expectation of becoming profitable and must clearly be commercial in nature.

²⁸The 2006 census was conducted on May 16, 2006, and the relevant questions for us (e.g., self-employment income) refer to the previous calendar year.

insurance income (which includes parental leave payments) as the outcome variable. As we would expect, we find a positive effect of the reform on employment insurance income in 2000–2001, but no effect in the subsequent years.²⁹ In terms of magnitudes, the coefficients suggest a 67% increase in employment insurance income in 2000–2001, which in turn implies a 67% increase in the length of leave that individuals have taken.³⁰ These results further help to validate our empirical strategy.

7 Conclusion

When workers are immersed in a job, it may be difficult to consider alternative career paths like entrepreneurship. A prolonged absence from regular wage employment provides an opportunity to consider alternative career paths, but taking such an absence normally generates substantial career risk: the labor market reacts poorly to gaps in employment history. We find evidence that this friction inhibits wage-employed workers from entering entrepreneurship. When workers are given the ability to take prolonged job-protected leaves—time away from work that is not penalized by the labor market—they are more likely to become entrepreneurs. These leaves may give workers a chance to act on a pre-existing interest in entrepreneurship. They may also enable workers who were not previously interested in entrepreneurship to discover a new interest in it.

We find that economically meaningful businesses emerge from prolonged job-protected leaves. The introduction of such leaves leads to an increase in businesses that survive at least five years, incorporate, and hire paid employees. These businesses 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).

The forces that we document may also be relevant in other settings. The career risk associated with gaps in employment could also inhibit workers from changing jobs, industries, or geographic locations. Job-protected leaves need not be the only way of mitigating such risk. For example,

²⁹For this analysis, we combine insurance payments from 2000 and 2001 since workers in our sample who had a child before the cutoff date would have taken paid leave spanning both years.

³⁰The reform did not change the income replacement percentage (or cap), it only changed the maximum income replacement period.

business schools may provide students with the opportunity to explore potential career changes while preserving their ability to return to their original career. More broadly, flexible and well-functioning labor markets can play the same role, and may therefore be important for facilitating entrepreneurship.

REFERENCES

Adelino, Manuel, Antoinette Schoar, and Felipe Severino, 2015, House prices, collateral, and self-employment, *Journal of Financial Economics*, 117(2), 288–306.

Albrecht, James W, Per-Anders Edin, Marianne Sundström, and Susan B Vroman, 1999, Career interruptions and subsequent earnings: A reexamination using swedish data, *Journal of Human Resources*, 294–311.

Baker, Michael and Kevin Milligan, 2008a, How does job-protected maternity leave affect mothers' employment?, *Journal of Labor Economics*, 26(4), 655–91.

Baker, Michael and Kevin Milligan, 2008b, Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates, *Journal of Health Economics*, 27(4), 871–887.

Baker, Michael and Kevin Milligan, 2014, Maternity leave and children's cognitive and behavioral development, *Journal of Population Economics*, 28(2), 373–391.

Baptista, Rui, Francisco Lima, and Miguel Torres Preto, 2012, How former business owners fare in the labor market? Job assignment and earnings, *European Economic Review*, 56(2), 263–276.

Beck, Thorsten, Ross Levine, and Norman Loayza, 2000, Finance and the sources of growth, *Journal of Financial Economics*, 58(1-2), 261–300.

Berg, Tobias, 2015, Playing the devil's advocate: The causal effect of risk management on loan quality, *Review of Financial Studies*, 28(12), 3367–3406.

Bertrand, Marianne, Antoinette Schoar, and David Thesmar, 2007, Banking deregulation and industry structure: Evidence from the french banking reforms of 1985, *Journal of Finance*, 62(2), 597–628.

Branstetter, Lee, Francisco Lima, Lowell J. Taylor, and Ana Venâncio, 2014, Do entry regulations deter entrepreneurship and job creation? Evidence from recent reforms in portugal, *Economic Journal*, 124(577), 805–832.

Bruhn, Miriam, 2011, License to sell: The effect of business registration reform on entrepreneurial activity in Mexico, *Review of Economics and Statistics*, 93(1), 382–386.

Campello, Murillo, Janet Gao, Jiaping Qiu, and Yue Zhang, 2018, Bankruptcy and the cost of organized labor: Evidence from union elections, *Review of Financial Studies*, 31(3), 980–1013.

Catherine, Sylvain, 2019, Keeping options open: What motivates entrepreneurs?, *Available at SSRN 3274879*.

Dahl, Gordon B., Katrine V. Løken, and Magne Mogstad, 2014, Peer effects in program participation, *American Economic Review*, 104(7), 2049–2074.

Dahl, Gordon B., Katrine V. Løken, Magne Mogstad, and Kari Vea Salvanes, 2016, What is the case for paid maternity leave?, *Review of Economics and Statistics*, 98(4), 655–670.

Desai, Mihir, Paul Gompers, and Josh Lerner, 2003, Institutions, capital constraints and entrepreneurial firm dynamics: Evidence from Europe, *Working Paper*.

Dickert-Conlin, Stacy and Amitabh Chandra, 1999, Taxes and the timing of births, *Journal of Political Economy*, 107(1), 161–177.

Dillon, Eleanor W. and Christopher T. Stanton, 2016, Self-employment dynamics and the returns to entrepreneurship, *Working Paper*.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-de Silanes, and Andrei Shleifer, 2002, The regulation of entry, *Quarterly Journal of Economics*, 117(1), 1–37.

Drexler, Alejandro, Greg Fischer, and Antoinette Schoar, 2014, Keeping it simple: Financial literacy and rules of thumb, *American Economic Journal: Applied Economics*, 6(2), 1–31.

Evans, David S. and Boyan Jovanovic, 1989, An estimated model of entrepreneurial choice under liquidity constraints, *Journal of Political Economy*, 97(4), 808–827.

Ewens, Michael, Ramana Nanda, and Matthew Rhodes-Kropf, 2018, Cost of experimentation and the evolution of venture capital, *Journal of Financial Economics*, 128(3), 422–442.

Fairlie, Robert W., Dean Karlan, and Jonathan Zinman, 2015, Behind the GATE experiment: Evidence on effects of and rationales for subsidized entrepreneurship training, *American Economic Journal: Economic Policy*, 7(2), 125–161.

Fan, Jianqing, Irène Gijbels, Tien-Chung Hu, and Li-Shan Huang, 1996, A study of variable bandwidth selection for local polynomial regression, *Statistica Sinica*, 113–127.

Ferber, Marianne A. and Jane Waldfogel, 1998, The long-term consequences of nontraditional employment, *Monthly Labor Review*, 3–12.

Gelman, Andrew and Guido Imbens, 2017, Why high-order polynomials should not be used in regression discontinuity designs, *Journal of Business and Economic Statistics*, (Forthcoming).

Gibbons, Robert and Lawrence F Katz, 1991, Layoffs and lemons, *Journal of Labor Economics*, 9(4), 351–380.

Gompers, Paul A., 1996, Grandstanding in the venture capital industry, *Journal of Financial Economics*, 42(1), 133–156.

Grant, Adam M, 2017, *Originals: How non-conformists move the world* (Penguin).

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano, 2016, Do fiscal rules matter?, *American Economic Journal: Applied Economics*, 8(3), 1–30.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2004, Does local financial development matter?, *Quarterly Journal of Economics*, 119(3), 929–969.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen, 1994a, Entrepreneurial decisions and liquidity constraints, *RAND Journal of Economics*, 25(2), 334–347.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen, 1994b, Sticking it out: Entrepreneurial survival and liquidity constraints, *Journal of Political Economy*, 102(1), 53–75.

Hombert, Johan, Antoinette Schoar, David Sraer, and David Thesmar, 2014, Can unemployment insurance spur entrepreneurial activity?, *Working Paper*.

Hurst, Erik and Annamaria Lusardi, 2004, Liquidity constraints, household wealth, and entrepreneurship, *Journal of Political Economy*, 112(2), 319–347.

Hurst, Erik and Benjamin Wild Pugsley, 2011, What do small businesses do?, *Brookings Papers on Economic Activity*, 73–142.

Imbens, Guido W and Thomas Lemieux, 2008, Regression discontinuity designs: A guide to practice, *Journal of Econometrics*, 142(2), 615–635.

Jukic, A. M., D. D. Baird, C. R. Weinberg, D. R. McConaughay, and A. J. Wilcox, 2013, Length of human pregnancy and contributors to its natural variation, *Human Reproduction*, 28(10), 2848–2855.

Kaiser, Ulrich and Nikolaj Malchow-Moller, 2011, Is self-employment really a bad experience? The effects of previous self-employment on subsequent wage-employment wages, *Journal of Business Venturing*, (26), 572–588.

Karlan, Dean and Martin Valdivia, 2011, Teaching entrepreneurship: Impact of business training on microfinance clients and institutions, *Review of Economics and Statistics*, 93(2), 510–527.

Karol, Gabrielle, 2012, Is the power maternity leave trend good or bad for women?, *Forbes*.

Kerr, William and Ramana Nanda, 2009a, Financing constraints and entrepreneurship, *Working Paper*.

Kerr, William R., Ramana Nanda, and Matthew Rhodes-Kropf, 2014, Entrepreneurship as experimentation, *Journal of Economic Perspectives*, 28(3), 25–48.

King, Robert G. and Ross Levine, 1993, Finance and growth: Schumpeter might be right, *Quarterly Journal of Economics*, 108(3), 717–737.

Klapper, Leora, Luc Laeven, and Raghuram Rajan, 2006, Entry regulation as a barrier to entrepreneurship, *Journal of Financial Economics*, 82(3), 591–629.

Knezevic, Andrea, 2008, Overlapping confidence intervals and statistical significance, *Cornell Statistical Consulting Unit*, <https://cscu.cornell.edu/news/statnews/stnews73.pdf>.

Lalive, Rafael, 2008, How do extended benefits affect unemployment duration? A regression discontinuity approach, *Journal of Econometrics*, 142(2), 785–806.

Lalive, Rafael and Josef Zweimüller, 2009, How does parental leave affect fertility and return to work? Evidence from two natural experiments, *Quarterly Journal of Economics*, 124(3), 1363–1402.

LaLumia, Sara, James M. Sallee, and Nicholas Turner, 2015, New evidence on taxes and the timing of birth, *American Economic Journal: Economic Policy*, 7(2), 258–293.

Lee, David S and Thomas Lemieux, 2010, Regression discontinuity designs in economics, *Journal of Economic Literature*, 48(2), 281–355.

Leonardi, Marco and Giovanni Pica, 2013, Who pays for it? The heterogeneous wage effects of employment protection legislation, *Economic Journal*, 123(573), 1236–1278.

Lerner, Josh and Ulrike Malmendier, 2013a, With a little help from my (random) friends: Success and failure in post-business school entrepreneurship, *Review of Financial Studies*, 26(10), 2411–2452.

Levine, Ross, 1997, Financial development and economic growth: Views and agenda, *Journal of Economic Literature*, 35(2), 688–726.

Levine, Ross, Norman Loayza, and Thorsten Beck, 2000, Financial intermediation and growth: Causality and causes, *Journal of Monetary Economics*, 46(1), 31–77.

Li, Kai, Tingting Liu, and Juan (Julie) Wu, 2018, Vote avoidance and shareholder voting in mergers and acquisitions, *Review of Financial Studies*, 31(8), 3176–3211.

Light, Audrey and Manuelita Ureta, 1995, Early-career work experience and gender wage differentials, *Journal of Labor Economics*, 13(1), 121–154.

Malenko, Nadya and Yao Shen, 2016, The role of proxy advisory firms: Evidence from a regression-discontinuity design, *Review of Financial Studies*, 29(12), 3394–3427.

Manso, Gustavo, 2011, Motivating innovation, *Journal of Finance*, 66(5), 1823–1860.

Manso, Gustavo, 2016, Experimentation and the returns to entrepreneurship, *Review of Financial Studies*, 29(9), 2319–2340.

Mazurkewich, Karen, 2010, Baby biz grows up; Women expand scope of businesses, *Financial Post*.

McCrary, Justin, 2008, Manipulation of the running variable in the regression discontinuity design: A density test, *Journal of Econometrics*, 142(2), 698–714.

Mel, Suresh de, David McKenzie, and Christopher Woodruff, 2008, Returns to capital in microenterprises: Evidence from a field experiment, *Quarterly Journal of Economics*, 123(4), 1329–1372.

Mincer, Jacob and Haim Ofek, 1982, Interrupted work careers: Depreciation and restoration of human capital, *Journal of Human Resources*, 3–24.

Mincer, Jacob and Solomon Polachek, 1974, Family investments in human capital: Earnings of women, *Journal of Political Economy*, 82(2, Part 2), S76–S108.

Morton, Brian, 2006, New working moms are busier than ever: Many use maternity leave to start new businesses, *Vancouver Sun*.

Muhlig, Ingrid, 2001, Leave policy should be flexible, *Ottawa Citizen*.

Mullainathan, Sendhil and Philipp Schnabl, 2010, Does less market entry regulation generate more entrepreneurs? Evidence from a regulatory reform in Peru, *Working Paper*.

Nanda, Ramana and Matthew Rhodes-Kropf, 2013b, Investment cycles and startup innovation, *Journal of Financial Economics*, 110(2), 403–418.

Nanda, Ramana and Matthew Rhodes-Kropf, 2017, Financing risk and innovation, *Management Science*, 63(4), 901–918.

OECD, 2015, *Health at a glance 2015: OECD Indicators* (OECD Publishing, Paris).

Oulman, Elizaveta, Theresa HM Kim, Khalid Yunis, and Hala Tamim, 2015, Prevalence and predictors of unintended pregnancy among women: an analysis of the canadian maternity experiences survey, *BMC Pregnancy and Childbirth*, 15(1), 260.

Pearce, Tralee, 2011, How to be a mompreneur; The women behind a successful baby-blanket company write a manual to offer business and family advice, *Globe and Mail*.

Schmalz, Martin C., David A. Sraer, and David Thesmar, 2017a, Housing collateral and entrepreneurship, *Journal of Finance*, 72(1), 99–132.

Schoar, Antoinette, 2010, The divide between subsistence and transformational entrepreneurship, *Innovation Policy and the Economy*, 10(1), 57–81.

Schönberg, Uta and Johannes Ludsteck, 2014, Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth, *Journal of Labor Economics*, 32(3), 469–505.

Schumpeter, Joseph A, 1911, *The Theory of Economic Development* (Harvard University Press, Cambridge, MA).

Stearns, Jenna, 2016, The long-run effects of wage replacement and job protection: Evidence from two maternity leave reforms in Great Britain, *Working Paper*.

Williams, Donald R., 2002, Returns to education and experience in self-employment: Evidence from Germany, *Working Paper*.

World Bank, 2015, *World development indicators 2015* (World Bank, Washington, D.C.).

Figure 1
Leave Taking Before and After Reform

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

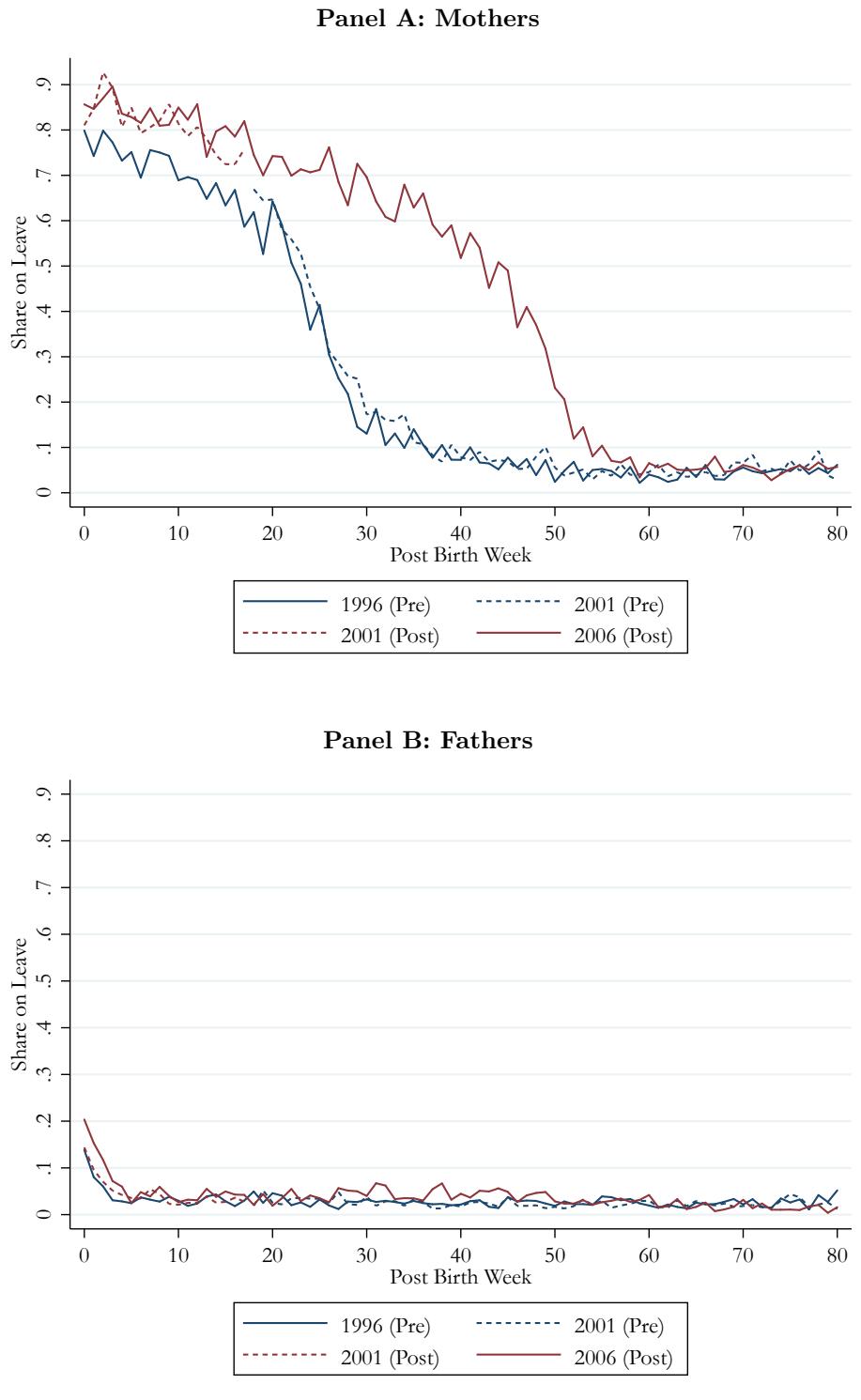


Figure 2
RDD Validity Check

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.

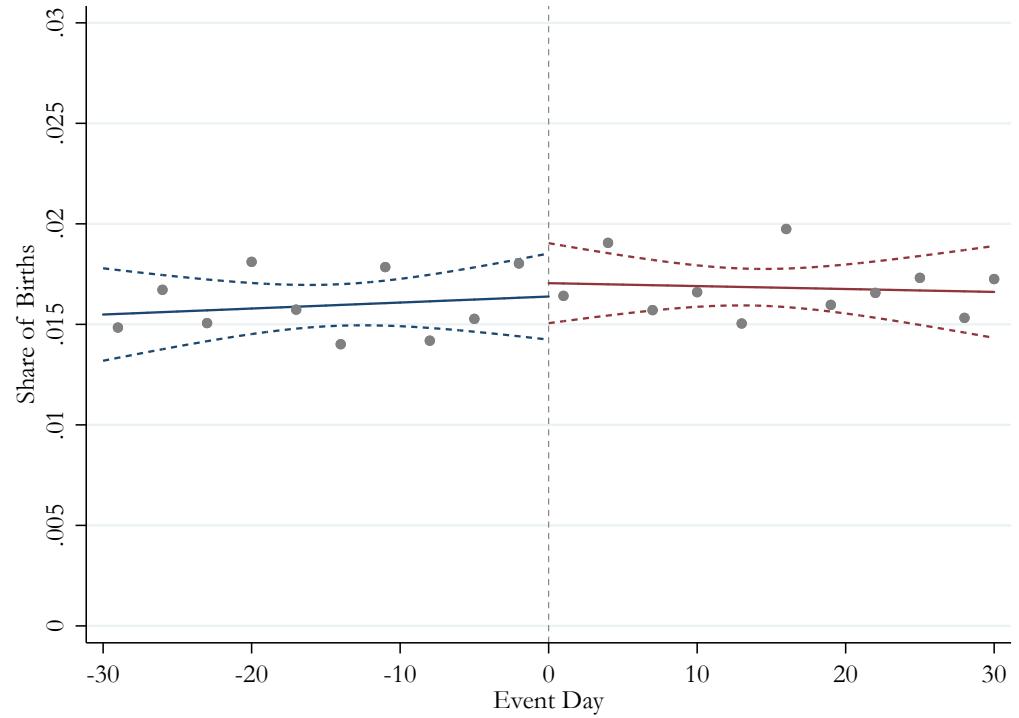


Figure 3
Baseline Results

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.

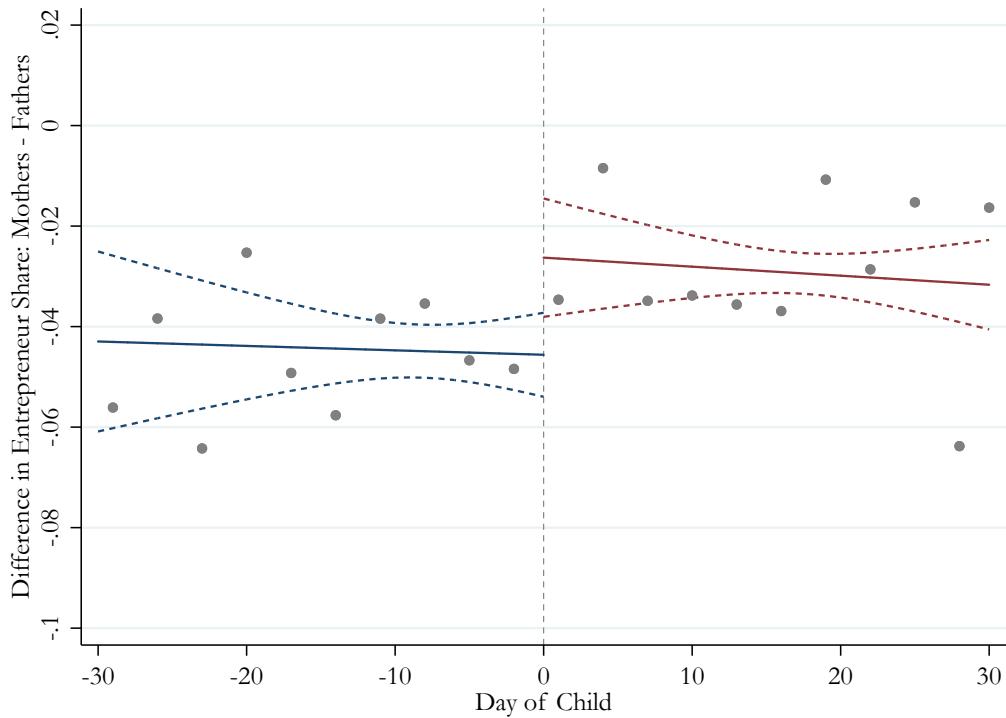


Table 1
Summary Statistics

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 statutes 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 (23.16)	15.89 (25.54)	17.50 (28.38)	14.40 (32.30)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	121	101	81	61

Panel B: Smoothness of Covariates

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

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

where observations are at the parent level; the sample consists of parents who had a first child (excluding multiples) within a specified bandwidth around the December 31, 2000, cutoff date; $Entrepreneur_i$ is an indicator equal to one if over 50% of parent i 's income is from self-employment as of the 2006 census; $DayChild_i$ represents the date parent i 's child was born relative to the reform cutoff date; $Post_i$ 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*** (0.0060)	0.0210*** (0.0061)	0.0199*** (0.0064)	0.0193*** (0.0074)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	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 1991–2005. All variables from the equation in Table 4 are then interacted with a reform year indicator equal to one for parents who had a child around December 31 in the year 2000. All other variables are defined 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	-0.0038 (0.0035)	-0.0043 (0.0038)	-0.0046 (0.0042)	-0.0025 (0.0048)
Reform Year \times Treatment	0.0270*** (0.0084)	0.0300*** (0.0085)	0.0300*** (0.0091)	0.0287*** (0.0101)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	1,414,220	1,174,400	939,745	704,080

Table 6
Robustness to Alternative Specifications/Samples

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. Panel D expands the sample to include all births, rather than first-time, singleton births. 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*** (0.0070)	0.0193*** (0.0071)	0.0216*** (0.0064)	0.0175** (0.0080)
Bandwidth	60	50	40	30
Kernel	Uniform	Uniform	Uniform	Uniform
Polynomial Degree	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*** (0.0067)	0.0202*** (0.0078)	0.0223*** (0.0079)	0.0296*** (0.0062)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	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** (0.0090)	0.0264*** (0.0075)	0.0323*** (0.0060)	0.0362*** (0.0045)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	3	3	3	3
Observations	86,565	71,285	57,405	42,430

Table 6
(Continued)
Panel D: All Births

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0220** (0.0104)	0.0251** (0.0117)	0.0241** (0.0121)	0.0257* (0.0137)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	193,495	159,240	127,915	94,745

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.

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*** (0.0060)	0.0210*** (0.0061)	0.0199*** (0.0064)	0.0193*** (0.0074)
$\Delta Weeks Leave \times Treatment$	0.0116 (0.0071)	0.0171*** (0.0061)	0.0180*** (0.0049)	0.0138*** (0.0021)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	86,565	71,285	57,405	42,430

Table 9

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 (4)–(5) of Panel A decompose self-reported entrepreneurship based on whether individuals hire paid employees. Columns (2)–(3) of Panel B decompose self-reported entrepreneurship based on whether individuals report working above or below the median number of hours. Columns (4)–(5) of Panel B decompose self-reported entrepreneurship based on whether individuals report earning above or below the median income. Sample sizes are weighted and rounded to the nearest multiple of 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** (0.0125)	0.0164*** (0.0040)	0.0110 (0.0105)	0.0151** (0.0070)	0.0123 (0.0105)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1
Difference p-val		0.585		0.827	
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)	(2)	(3)	(4)	(5)
Treatment	0.0274** (0.0125)	0.0310*** (0.0080)	-0.0036 (0.0102)	0.0205*** (0.0059)	0.0069 (0.0141)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1
Difference p-val		0.010		0.439	
Observations	42,430	42,430	42,430	42,430	42,430

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 among the entrepreneurs in our sample). The dependent variable in column (2) is an indicator variable equal to one if the individual is an entrepreneur working in a low-startup-capital industry (startup capital requirements below the median among the entrepreneurs in our sample). Columns (3)–(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 3-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	
	(1)	(2)	(3)	(4)
Treatment	0.0006 (0.0047)	0.0187*** (0.0071)	0.0270*** (0.0045)	-0.0077 (0.0050)
Bandwidth	30	30	30	30
Kernel	Triangular 1	Triangular 1	Triangular 1	Triangular 1
Polynomial Degree				
Difference p-val	0.0558			
Observations	42,430	42,430	42,430	42,430

Table 11
Short-Run Effects

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

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

where observations are at the parent level; the sample consists of parents in the Longitudinal Administrative Databank (LAD) who had a first child (excluding multiples) within a specified bandwidth around the December 31, 2000, cutoff date and who were exclusively wage employed (had wage employment income and did not have any net self-employment income) in the year prior to child birth; $Entrepreneur_i$ is an indicator equal to one if an individual reports any net self-employment income, positive or negative, on her tax return; $DayChild_i$ 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. Panel A estimates the effects for year 2002 using various bandwidths. Panel B estimates the dynamic effects in each year from 2001 to 2005. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Short-Run Effects

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

Panel B: Dynamics

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

Appendix For Online Publication

A Appendix Exhibits

Figure A.1
RDD Validity Check: 60-day Window

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 60-day bandwidth and triangular kernel. The dashed lines represent 95% confidence intervals.

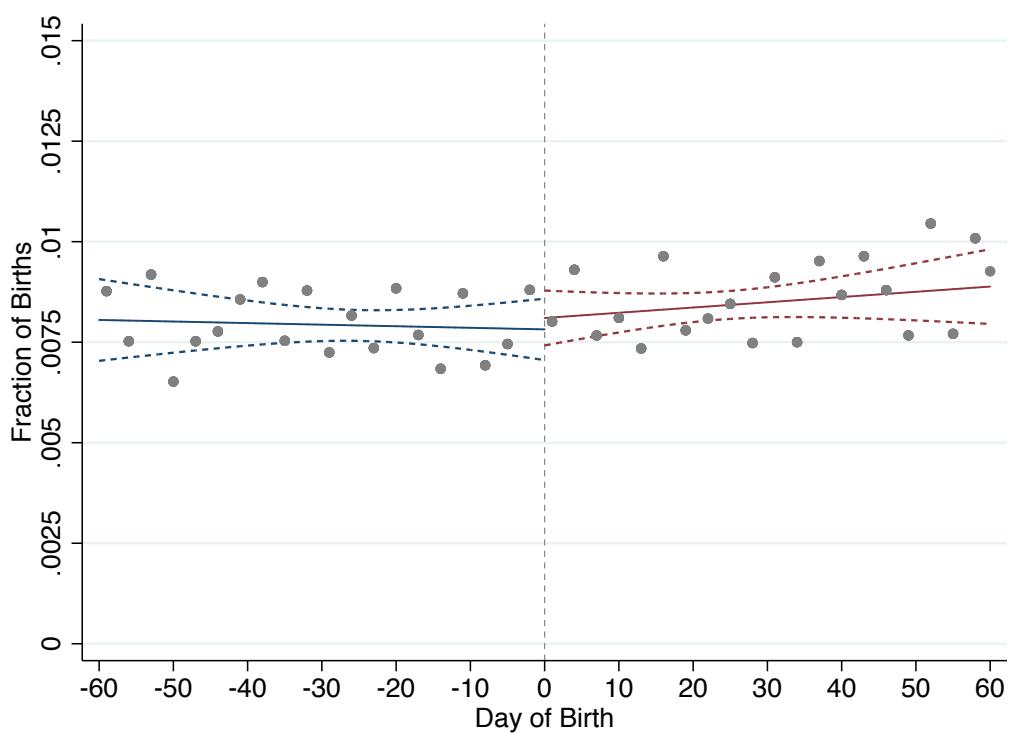


Figure A.2
RDD Validity Check: Placebo Year

This figure repeats the analysis of Figure A.1 around January 1, 1999, when there was no reform.

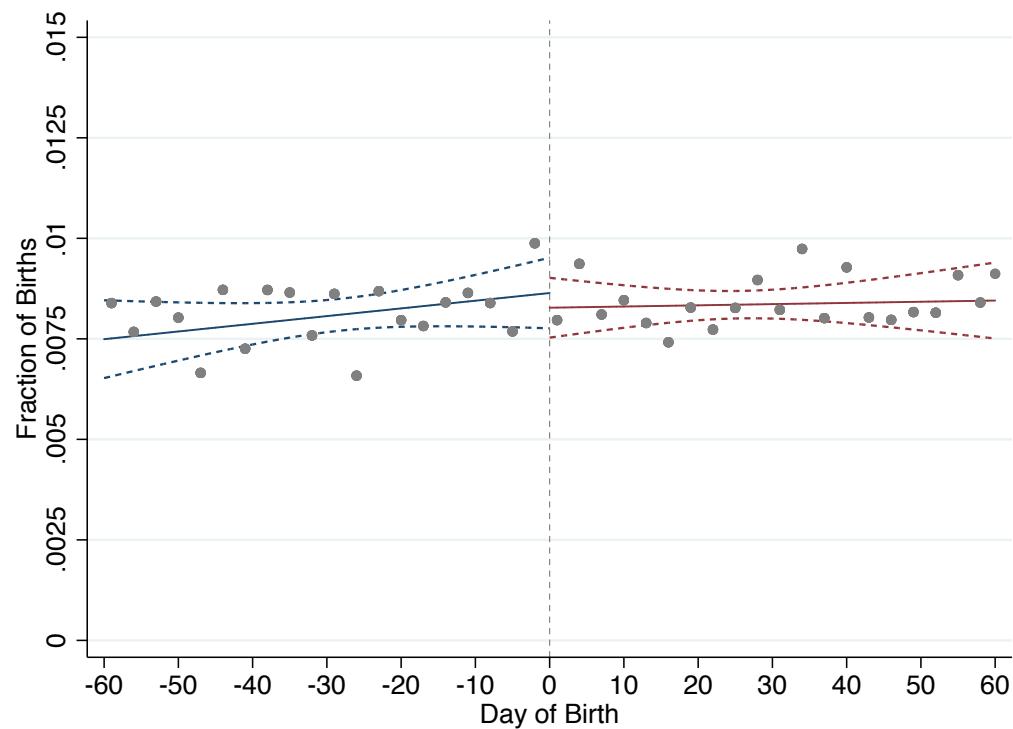


Figure A.3

Smoothness of Covariates

This figure shows the results of Table 3, Panel B, graphically.

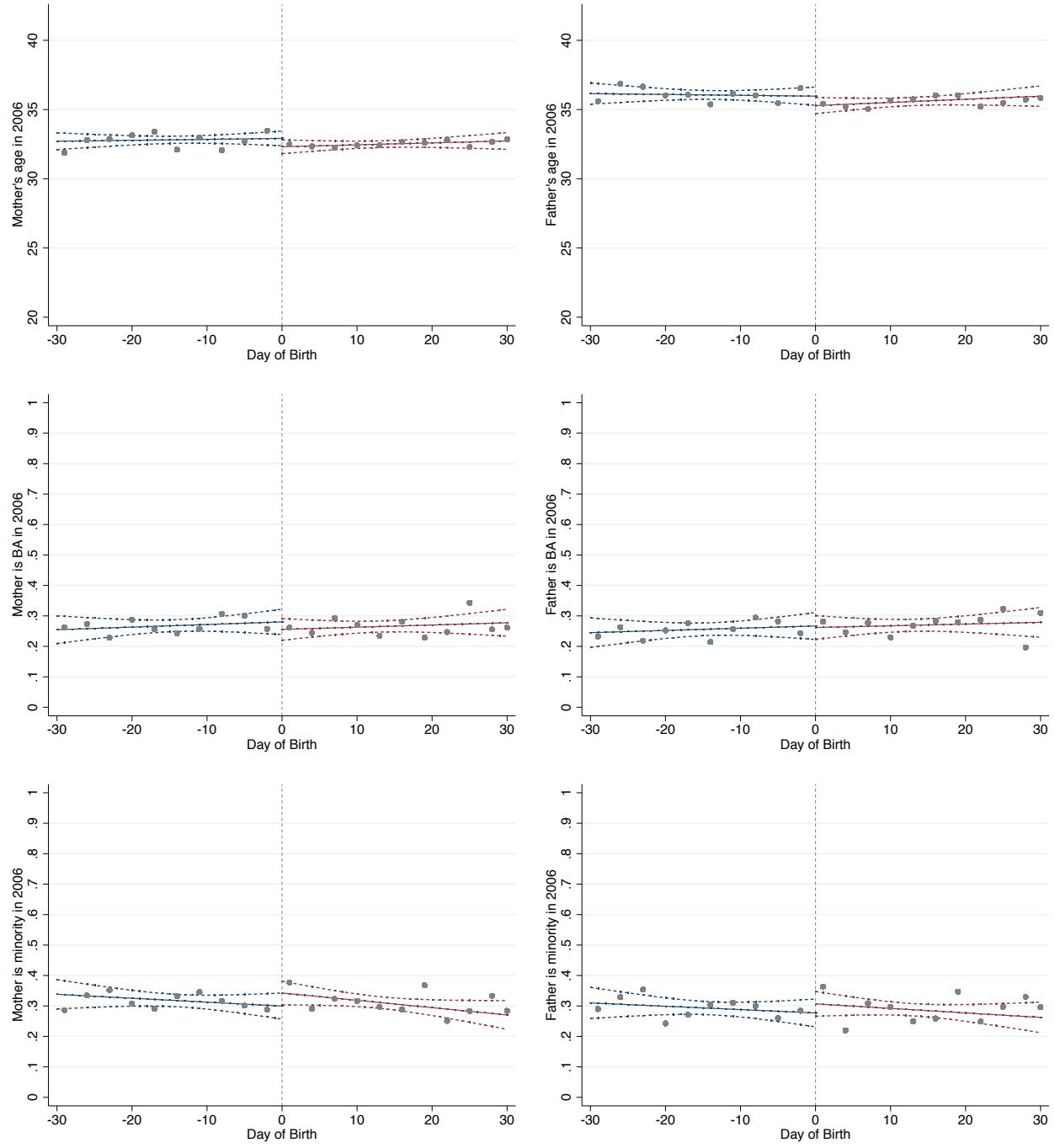


Table A.1**Summary Statistics for Analogous U.S. Parents**

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									
	OLS					Logit				
Estimation Method:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age at Child Birth	0.003*** (0.001)				0.003*** (0.001)	0.039*** (0.008)				0.035*** (0.008)
Bachelor Degree		0.056* (0.022)			0.053** (0.024)	0.625*** (0.190)				0.581*** (0.220)
Minority			-0.012 (0.016)		-0.012 (0.017)	-0.012 (0.017)		-0.173 (0.226)		-0.172 (0.246)
Urban				0.001 (0.013)	-0.009 (0.013)	-0.009 (0.013)	0.0248 (0.0232)		0.009 (0.176)	-0.103 (0.176)
Mother × Age at Child Birth	-0.000 (0.001)				-0.001 (0.001)	-0.001 (0.001)				0.018 (0.022)
Mother × Bachelor Degree		-0.006 (0.033)			-0.011 (0.033)	0.243 (0.360)				0.113 (0.351)
Mother × Minority			-0.011 (0.018)		-0.009 (0.019)	-0.470 (0.304)				-0.411 (0.318)
Mother × Urban				0.007 (0.009)	0.008 (0.011)			0.185 (0.119)		0.125 (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 (8.480)	6.311 (9.155)	8.584 (10.04)	14.26 (11.35)
Reform Year \times Post	8.682 (24.35)	9.581 (26.72)	8.917 (29.52)	0.140 (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
Baseline Results—Lower Bandwidths

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 10 days and a triangular kernel:

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

where observations are at the parent level; the sample consists of parents who had a first child (excluding multiples) within a specified bandwidth around the December 31, 2000, cutoff date; $Entrepreneur_i$ is an indicator equal to one if over 50% of parent i 's income is from self-employment as of the 2006 census; $DayChild_i$ represents the date parent i 's child was born relative to the reform cutoff date; $Post_i$ 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)	(5)	(6)
Treatment	0.0194*** (0.0060)	0.0210*** (0.0061)	0.0199*** (0.0064)	0.0193*** (0.0074)	0.0251*** (0.0060)	0.0346*** (0.0055)
Bandwidth	60	50	40	30	20	10
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1	1
Observations	86,565	71,285	57,405	42,430	28,690	14,680

Table A.5
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 (0.0122)	0.0011 (0.0139)
DayChild	0.0001 (0.0006)	-0.0001 (0.0005)
Post \times DayChild	-0.0006 (0.0009)	-0.0010 (0.0007)
Mother	-0.0456*** (0.0043)	-0.0455*** (0.0037)
Mother \times Post (Treatment)	0.0193*** (0.0074)	0.0175** (0.0080)
Mother \times DayChild	-0.0001 (0.0004)	-0.0001 (0.0002)
Mother \times Post \times DayChild	-0.0001 (0.0005)	0.0001 (0.0004)
Constant	0.0864*** (0.0085)	0.0838*** (0.0084)
Bandwidth	30	30
Kernel	Triangular	Uniform
Polynomial Degree	1	1
Observations	42,430	42,430

Table A.6
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 (0.0106)	-0.0100 (0.0115)	-0.0094 (0.0124)	-0.0011 (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.7
Placebo Test Based on Shifting Cutoff Date 60 Days

This table repeats the analysis of Table 4 shifting the cutoff 60 days earlier (Panel A) and 60 days later (Panel B). 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: Shifting Cutoff Date 60 Days Earlier

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	-0.0042 (0.0086)	-0.0036 (0.0092)	-0.0004 (0.0100)	0.0083 (0.0094)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	86,435	72,180	57,660	43,210

Panel B: Shifting Cutoff Date 60 Days Later

Dependent Variable:	Entrepreneur			
	(1)	(2)	(3)	(4)
Treatment	0.0070 (0.0068)	0.0054 (0.0070)	0.0016 (0.0061)	-0.0001 (0.0061)
Bandwidth	60	50	40	30
Kernel	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1
Observations	90,900	76,360	61,240	45,860

Table A.8
Characterizing Entrants Based on Industry Characteristics—Excluding Top-5 Industries

This table repeats the analysis of Table 10 excluding the five most prevalent industries among entrepreneurs from the analysis: health care and social assistance; professional, scientific and technical services; other services; retail trade; administrative and support. 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	0.0025 (0.0102)	0.0188** (0.0075)	0.0273** (0.0126)	-0.0060 (0.0095)
Bandwidth	30	30	30	30
Kernel	Triangular 1	Triangular 1	Triangular 1	Triangular 1
Polynomial Degree				
Difference p-val	0.0246			
Observations	28,560	28,560	28,560	28,560

Table A.9
Distribution of Entrepreneurs Across Industries

This table shows the distribution of entrepreneurs across industries (2-digit NAICS) for the mothers in our sample.

Industry	Percent
Health care and social assistance	25.0%
Professional, scientific and technical services	14.3%
Other services (except public administration)	11.7%
Retail trade	6.7%
Administrative and support	6.6%
Agriculture, forestry, fishing and hunting	6.1%
Educational services	4.7%
Real estate and rental and leasing	4.5%
Manufacturing	3.2%
Construction	3.1%
Accommodation and food services	3.1%
Finance and insurance	2.7%
Arts, entertainment and recreation	2.3%
Wholesale trade	1.8%
Transportation and warehousing	1.8%
Information and cultural industries	1.3%
Public administration	0.7%
Mining and oil and gas extraction	0.3%
Management of companies and enterprises	0.1%
Utilities	0.1%

Table A.10
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 (0.0184)	-0.0218 (0.0192)	-0.0212 (0.0179)	-0.0122 (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

Table A.11
Employment Insurance Income

This table repeats the analysis in Table 11, Panel B, using employment insurance income as the outcome variable. The sample consists of parents in the Longitudinal Administrative Databank (LAD) who had a first child (excluding multiples) within a 30-day window around the December 31, 2000, cutoff date and who were exclusively wage employed (had wage employment income and did not have any net self-employment income) in the year prior to child birth. The dependent variable is the amount of employment insurance income (in CAD) individuals report receiving in various years from 2000 to 2005. Standard errors are clustered by week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Employment Insurance Income in				
	2000–2001	2002	2003	2004	2005
	(1)	(2)	(3)	(4)	(5)
Treatment	4096.8*** (183.2)	178.2 (121.9)	115.0 (136.3)	276.7 (411.4)	-11.12 (185.7)
Bandwidth	30	30	30	30	30
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular
Polynomial Degree	1	1	1	1	1
Observations	27,300	27,300	27,300	27,300	27,300

B Model

B.1 Model Setup

In order to fix ideas, we present a stylized model of the direct mechanism described in Section 5.2.

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.³¹ 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:

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

We assume that individuals are risk-averse and have exponential utility given by $U(x) = -e^{-\lambda x}$,

³¹One 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, Heinrich, and Kline, 2016).

where λ is the coefficient of absolute risk aversion.

B.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.³²

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 $\frac{\partial \text{Prob}(\text{Entrepreneur}|t^*)}{\partial T_L} > 0$.*

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

(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 $\frac{\partial \text{Prob}(\text{Entrepreneur}|t^*)}{\partial T_L} > 0$.*

³²It 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.

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

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

Proof. See Appendix C. □

C Proofs

C.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

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

21

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

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

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

C.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., $EU(X|s_t) > EU(y(1 - \delta_t))$. We assume

individuals have exponential utility: $U(x) = -e^{-\lambda x}$, where λ is the coefficient of absolute risk aversion. For $x \sim N(\mu, \sigma^2)$, 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 :

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

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 $U(X|s_t)$ follows a log-normal distribution. Specifically,

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

It can be shown that

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

and

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

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 $\Phi()$ is the c.d.f. of the standard normal distribution.

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

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

Finally, the probability of entering entrepreneurship is

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

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 $\Pr(Entrepreneurship|t=0) = 0$, i.e., those for whom $\mu - \frac{\lambda}{2\eta_0} < y$.

□

C.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 $\mu - \frac{\lambda}{2\eta_0} < y$, $\Pr(Entrepreneurship|t)$ 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

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

24

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

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 . \square

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

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

Σ

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

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 $d = 0$, $t^* \rightarrow +\infty$.

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,

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

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

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, $t^* \rightarrow +\infty$ when $d = 0$. \square

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 $EU(t) = -\exp\left(\frac{\lambda^2}{2\eta_0} - \lambda\mu\right) \Phi(g(t)) - \exp(-\lambda y(1 - \delta_t)) \Phi(-f(t))$.

It can be shown that

2

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

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

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

Note that $\sigma' = \left(\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0 + h_t}} \right)' > 0$. There exists $\tilde{d} = \max_{t \geq 0} \frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ 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 $EU'(t) = \lambda \exp(-\lambda y(1 - \delta_t)) [\phi(f(t))\sigma' - \Phi(-f(t))yd]$, therefore

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

27

$\sigma' = \left(\sqrt{\frac{1}{\eta_0} - \frac{1}{\eta_0 + h_t}} \right)' = \left(\frac{1}{\eta_0} - \frac{1}{\eta_0 + h_t} \right)^{-\frac{1}{2}} \frac{1}{4h_t(\eta_0 + h_t)^2}$. 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

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

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

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

Proof. From the proof of Lemma 4 we know that there exist $\underline{d} = \min_{t \geq 0} \frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ and $\tilde{d} = \max_{t \geq 0} \frac{\phi(f(t))\sigma'}{\Phi(-f(t))y}$ such that $EU'(t) > 0$ for all $t \geq 0$

when $d \leq \underline{d}$, 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 $\underline{d} \leq d \leq \tilde{d}$, 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., $\frac{y - \frac{\partial G}{\partial f} \frac{\partial f}{\partial d}}{\frac{\partial G}{\partial t^*}} = \frac{y - IMR(IMR - f)\sigma' \frac{y t}{\sigma}}{IMR(IMR - f)f'\sigma' + IMR\sigma''} = \frac{y}{IMR\sigma'} \left[\frac{1 - IMR(IMR - f)\frac{\sigma'}{\sigma}t}{(IMR - f)f' + \frac{\sigma''}{\sigma'}} \right] < 0$. 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 $K'(IMR) = 0$, $K(IMR) = \frac{(IMR(IMR-f)\frac{\sigma'}{\sigma}t-1)^2f'}{(2IMR-f)\frac{\sigma'}{\sigma}t} > 0$ (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 $\underline{d} \leq d \leq \bar{d}$. \square

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 $\frac{\partial \text{Prob}(\text{Entrepreneur}|t^*)}{\partial T_L} > 0$.

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

(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 $\frac{\partial \text{Prob}(\text{Entrepreneur}|t^*)}{\partial T_L} > 0$.

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

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

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 $t_0^*(\bar{d}) = T_L$. 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 $t^*(\bar{d}) = t_0^*(\bar{d}) = T_L$. We know that $\Delta EU^*(\bar{d}) > 0$. Since $\Delta EU^*(d)$ 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 $t^*(d) = T_L$. 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

REFERENCES

Card, David, Ana Rute Cardoso, Jörg Heining, and Patrick Kline, 2016, Firms and labor market inequality: Evidence and some theory, *Working Paper*.

Rossin-Slater, Maya, 2017, Maternity and family leave policy, *Working Paper*.

Song, Jae, David J Price, Fatih Guvenen, Nicholas Bloom, and Till Von Wachter, 2015, Firming up inequality, *Working Paper*.