

Can Unemployment Insurance Spur Entrepreneurial Activity?

Evidence From France*

Johan Hombert[†]

Antoinette Schoar[‡]

David Sraer[§]

David Thesmar[¶]

July 9, 2013

Abstract

We investigate how a large-scale French reform to reduce the risk from small business creation for unemployed workers, affects the composition of people who are drawn into entrepreneurship. New firms started in response to the reform are, on average, smaller, but have similar growth expectations and education levels compared to start-ups before the reform. They are also as likely to survive or to hire. However, there are large crowd-out effects: Employment in incumbent firms decreases by a similar magnitude as the number of new jobs created in start-ups. These results point to the importance of Schumpeterian dynamics when facilitating entry.

*This is the substantially revised version of a paper previously titled "Should the Government Make it Safer to Start a Business? Evidence From French Reforms". We thank participants at the INSEE, the AEA meetings in San Diego, and London Business School for their input. The data used in this paper is confidential but not the authors' exclusive access.

[†]HEC Paris

[‡]MIT-Sloan, NBER, and CEPR

[§]Princeton University and NBER

[¶]HEC Paris and CEPR

“*The problem with the French is that they have no word for entrepreneur*”, attributed to George W. Bush.

1 Introduction

Over the last decade, policy makers and academics alike have embraced entrepreneurship as a panacea for many economic challenges. Reducing barriers to entrepreneurship, such as financing or regulatory constraints, has consequently become a major policy objective and an object of intense academic evaluation.¹ The primary focus of this literature has been on how reduction in barriers to entrepreneurship affects the *level* of entrepreneurship. However, this focus may be misleading. Entrepreneurial rates conceal a substantial amount of heterogeneity, as entrepreneurs vary in their ability to manage their firm or to grow and create jobs (Nanda (2008)), from self-employed individuals – looking for subsistence opportunities – to transformational entrepreneurs who aim at building large firms (Schoar (2010) or Haltiwanger, Jarmin, and Miranda (2013)). Beyond ability, entrepreneurs have different degrees of risk tolerance, ambition, or optimism (Hurst and Pugsley (2011), Landier and Thesmar (2009), or Holtz-Eakin, Joulfaian, and Rosen (1994a)). This heterogeneity will affect the welfare impact of policy reforms to the extent that post-reform entry decisions are driven by these entrepreneurial characteristics. For instance, policies that draw less qualified individuals into entrepreneurship may increase the idiosyncratic risk and failure rate of newly created firms, exerting substantial externalities on firms’ stakeholders, such as workers or suppliers.

The welfare implications of barriers to entrepreneurship thus crucially depend on *how* individuals select into this activity. Two views can be contrasted. On the one hand, the “self-selection view” asserts that individuals know their types and self-select into entrepreneurship only if the return they perceive overcomes the cost of entry. Reducing the cost of entry leads to a worsening in the pool of entrepreneurs, since the marginal

¹See, for example, the fast growing literature on the impact of financial market reforms on entrepreneurship, e.g., Bertrand, Schoar, and Thesmar (2007) or Cole (2009). For papers on regulatory constraints, see Djankov et al. (2002) or Klapper, Laeven, and Rajan (2006).

entrepreneurs have worse characteristics. Consistent with this view, Nanda (2008) reports an increase in the percentage of poorly performing firms after a reduction of entry cost in Denmark. The “experimentation view”, on the other hand, assumes that individuals ignore their ability as entrepreneurs and only learn their type by trying to start a business (Jovanovic (1982) or Caves (1998)). Barriers to entry fail to screen out low ability entrepreneurs and only keep out the most constrained individuals. If constrained individuals have similar or even better entrepreneurial characteristics than unconstrained individuals (Holtz-Eakin, Joulfaian, and Rosen (1993)), a relaxation of barriers to entry draws in new entrants that are of similar or even better quality than existing founders. The empirical evaluation of policy reforms changing the cost of entry must go beyond “level” effects – how many new firms were created through the reform? – to document the selection process at work in the data – how did firms created through the reform behave ex post?

This paper provides such an evaluation using a large-scale French reform implemented in 2002 and aimed at facilitating (small) business creation for unemployed workers, called PARE – *Plan d’Aide au Retour a l’Emploi*. This reform allowed unemployed entrepreneurs to retain their rights to unemployment benefits for three years in case their venture fails. Additionally, the reform mandated the unemployment insurance fund to pay out to unemployed entrepreneurs any gap between their entrepreneurial revenues and their unemployment benefits, providing insurance against cash flow shortfalls. These changes all facilitated entry into entrepreneurship by removing the strong financial disincentives that were previously associated with entry into entrepreneurship by unemployed workers.

Our first set of empirical findings evaluate the impact of the reform on firm creation. To evaluate if the reform led to an increase in the number of newly created firms, our empirical strategy is a standard difference-in-difference estimation. The treatment group consists of industries where newly created firms tend to be small before the reform. By contrast, the control group contains industries where small firms are not prevalent at creation. The idea underlying this choice is that since the reform removed financial disincentives for unemployed workers, the reform should have mostly affected small-scale entrepreneurs. Therefore, our treatment group should have a larger exposure to the reform than our control group. The identifying assumption is that absent the policy reform, both types of

industries would have experienced similar changes in entrepreneurial activity relative to pre-reform levels. Under this assumption, which we make more explicit in Section 4, the difference-in-difference estimator allows us to reject the null hypothesis that the reform did not increase entrepreneurial activity. Empirically, we find a very large effect of the reform on business creation across industries, with a 25% increase in monthly creation rates after the reform is implemented. Importantly, this effect is much stronger for our treated industries: post-reform entry growth is larger by more than 12 percentage points in industries where small firms are prevalent at creation. This corresponds to more than 750 new firms created per months in those treated industries relative to the control industries.

Using the same identification strategy, we further document that the firms created in response to the reform are not of (observably) worse quality ex post. We do not observe a significant change in the failure rate, hiring rate, or growth rate of young firms in treated industries following the reform relative to control industries. We also find that the reform did not significantly affect the composition of educational backgrounds of founders. Somewhat surprisingly, the reform seem to have led to a significant entry of “ambitious” founders, where ambition is measured from a survey that asks founders of new firms about their growth expectations and intention to hire workers in the next year. Firms created in the treatment industries are 3.5 percentage points more likely to intend to hire relative to firms in control industries. This is a sizable effect since the sample average is 18% for these industries. Overall, it appears that the pool of entrepreneurs which emerged thanks to the reform, is not of observably worse quality relative to earlier cohorts. This evidence is more consistent with the “experimentation view” – barriers to entry prevent unemployed workers from trying to set up a new business – than with the self-selection view – barriers to entry prevent low-quality entrepreneurs from starting-up a venture. This evidence also suggests that the increased insurance benefiting unemployed entrepreneurs did not foster risk-taking.

The final part of the paper focuses on equilibrium effects and looks at potential spillovers the reform might have had on incumbent firms in the treated industries. The significant entry of new firms post-reform may have eroded incumbent firms’ market shares, which is the key idea underlying the concept of creative destruction. Our difference-in-

difference analysis reveals that employment growth among small incumbents is lower by 2.6 percentage points after the reform in treated industries relative to control industries. However, we do not find any evidence for spillover effects on *large* incumbent firms. This is consistent with the idea that the competitive pressure from increased entrepreneurial activity is stronger for small firms than for large ones, at least in the short-run. The crowding-out effects on small incumbents are large economically. They offset most of the direct effects of lowering entry barriers on employment creation by start-ups. This result echoes the literature on financial reforms, which also documents that increased entry is detrimental to incumbent firms (Cetorelli and Strahan (2006), Bertrand, Schoar, and Thesmar (2006), Kerr and Nanda (2009)). We also document that wages and productivity (measured as value added or sales per worker) are larger in newly created firms, both in treated and control industries as well as before and after the reform, when compared with “shrinking” incumbents, i.e., incumbents whose employment has recently decreased. Two years after creation, value added per worker is 7,000 euros per year higher in newly created firms relative to these incumbents. This suggests that, even if the jobs created by newly created firms after the reform are fully offset by jobs destroyed in small incumbent firms, this labor reallocation process from incumbents to start-ups can have a positive impact on aggregate productivity, since newly created firms in the data are, on average, more productive.

Related Literature

Our results make two novel contributions to the existing literature on barriers to entry into entrepreneurship: (1) we provide detailed micro-evidence on the composition of entrepreneurs who get drawn into self-employment when entry barriers are relaxed; (2) we document how removing barriers to entry affect incumbent firms. The earlier literature has looked at cross-country differences in barriers to entry and the aggregate implications for entry rates (Djankov et al. (2002), Desai et al. (2003), Klapper et al. (2006)). Because of its focus on cross-country outcomes, this literature has mostly overlooked how barriers to entry affect the composition of the pool of actual entrepreneurs. There are few

country-level studies on the impact of entry regulations that use micro-data. Most of these papers focus on the effect of simplifications in the registration process and/or reduction in the transaction costs associated with entry (Branstetter et al. (2010), Mullainathan and Schnabl (2010), or Bruhn (2011)). These reforms affect not only the incentives for individuals to create new firms but also the willingness to formalize existing activities. Additionally, these papers typically examine entry rates and do not consider how these de-regulations affect entrepreneurial quality or labor reallocation across entire industries, which is the focus of our paper.

Our paper is also related to a large literature on the role of financing constraints on entrepreneurship. Many papers have shown that limited access to finance affects business creation and growth (Evans and Jovanovic (1989), Holtz-Eakin et al. (1994a, 1994b), Hurst and Lusardi (2004), de Mel et al. (2008), Kerr and Nanda (2010)). The policy experiment in this paper can be viewed as a monetary transfer to entrepreneurs, but in the form of increased insurance in case of failure. Our results demonstrate that these types of subsidies also increase entrepreneurial activity, thereby fostering creative destruction in affected industries.

Our paper also contributes to the literature on selection into entrepreneurship (Kihlstrom and Laffont (1979), Oswald and Blanchflower (1998), Hamilton (2000), Moskowitz and Vissing-Jorgensen (2002), Hurst and Pugsley (2011)). These papers have documented a large heterogeneity in the talent, ambition, and risk-preferences of entrepreneurs, which translates into different investment and effort choices following entry. Our results show that at the time of entry, potential entrepreneurs seem to ignore this heterogeneity: in our sample, the marginal entrepreneurs that enter post-reform share characteristics similar to infra-marginal ones. Consequently, the distribution of entrepreneurial talent improves following a reduction in entry costs as individuals are able to experiment with setting-up a firm and learn about their type.

Finally, our paper is also related to the vast literature that examines how unemployment benefits distort labor supply, and in particular unemployment duration (Solon (1985), Moffitt (1985), Katz and Meyer (1990), Card and Levine (2000) among others). Relative to these papers, our contribution highlights a new, important distortive margin

of unemployment insurance by looking specifically at the supply of self-employed workers. In the same way that unemployment insurance can reduce the incentives of unemployed workers to find a new job, the risk of losing unemployment benefits can reduce the incentive of unemployed individuals to start a new firm/create their own job. Our results show that this margin is quantitatively large. When the risk of losing UI benefits are reduced, which is precisely the experiment we are looking at, we observe a very large increase in the supply of self-employment.

The rest of the paper is organized as follows. We present the reform and its economic consequences in Section 2, the data in Section 3, the empirical strategy in Section 4, and the results in Section 5.

2 The Reform and Its Likely Consequences

2.1 Institutional Details

We focus on a reform passed by France in 2001 that aimed to reduce the implicit disincentives for unemployed workers to start a business. These changes were decided in mid-2001, as part of a larger negotiation on unemployment benefits, and became fully effective in mid-2002.

In July 2001, a new agreement between labor unions and employer organizations was signed (PARE, *Plan d'Aide au Retour a l'Emploi*), which established new rules on unemployment benefits. The overall goal was to provide more generous benefits for unemployed workers who engage in an active employment search.² An important part of this reform included the provision of insurance to unemployed workers who start a new firm. First, the new system allowed unemployed entrepreneurs to claim unemployment benefits in case of business failure. Before the reform, an unemployed worker would lose eligibility to all future unemployment benefits when starting a business: If the business failed, the entrepreneur could not claim the benefits accumulated under her pre-entrepreneurial job

²In France, labor and employer unions jointly run the unemployment benefit agency. Every third year, a new agreement is signed to adapt unemployment insurance to changing labor market conditions. In 2001, against a backdrop of strong economic recovery in the late 1990s, the unemployment insurance regime was running a large surplus, and was expected to do so over the next few years.

spell. The new agreement allowed an unemployed entrepreneur to retain her rights to the remaining unemployment benefits for up to three years.³ Besides, the reform also stipulated that unemployed entrepreneurs could keep their unemployment benefits *while* starting their own firm (Rieg (2004)). This provision applied as long as the income derived from the entrepreneurial activity remained below 70% of the pre-unemployment income. Unemployment benefits were in this case calculated so as to complement entrepreneurial income up to 70% of the pre-unemployment income level. Finally, unearned benefits were not voided, but could be paid in the future if entrepreneurial income would ever fall back below 70% of the pre-unemployment threshold.⁴ Therefore, unemployed workers who decided to start a business were guaranteed to receive at least their unemployment benefits, and more if entrepreneurial income were to exceed these benefits.

In February 2002, the unemployment agency ruled that these provisions could be cumulated with two already existing popular subsidies targeted at unemployed entrepreneurs. The first subsidy (ACCRE) significantly reduces payroll taxes paid by entrepreneurs up to three years after starting their firm. The second subsidy (EDEN) consists of small soft loans (up to 6,100 euros). These loans, initially restricted to young unemployed, became available to all unemployed after February 2002. The unemployment agency jointly promoted to potential entrepreneurs the new provisions of the PARE agreement, as well as the EDEN and ACCRE extended subsidies.

INSERT FIGURE 1 ABOUT HERE

While the PARE agreement was passed in mid-2001, it only became fully effective in the middle of 2002, as the unemployment agency finalized the rules under which it would operate. The agency began massively advertising the benefits of the reform to unemployed workers in the fall of 2002 (Rieg (2004)). While it is not possible to directly observe the timing of this advertisement effort, the Ministry of Labor provides us with

³Article 1-5 of the PARE agreement.

⁴Each month, the unemployment agency uses the daily pre-unemployment wage w as a benchmark. It then divides monthly entrepreneurial income by daily wage w to obtain the number d of days in the months in which the jobless person has received the equivalent of her former salary. The agency then pays unemployment benefits based on $28-d$ days of unemployment. The person does, however, retain the “rights” over unpaid unemployment benefits corresponding to d worked days, which she can claim for up to three years.

monthly data on the take-up of the ACCRE program. Figure 1 shows the monthly number of new firms receiving the ACCRE subsidy. Consistent with the slow implementation of the reform, we see that the take-up really starts in the second half of 2002. As we will see below, the reported increase in the take-up of the ACCRE program is consistent with the acceleration of aggregate business creation in the country.

2.2 Economic Consequences

This reform makes it easier for unemployed entrepreneurs to start new businesses via two distinct channels. The first one is a decrease in the risk associated with a failing venture. The reform provides *de facto* unemployed entrepreneurs with full insurance against cash-flow risk for up to three years. This could potentially draw into entrepreneurship unemployed workers who are more risk averse (Kihlstrom and Laffont (1978)). The second channel is a relaxation of credit constraints. The reform provides the entrepreneur with small amounts of start-up capital, since she continues to receive her benefits if the venture makes little or no profit. The importance of access to credit for entrepreneurship has been discussed in many papers (Evans and Jovanovic (1989), Holtz-Eakin et al. (1994a, 1994b), or de Mel et al. (2008)).

This reform is designed and expected to increase entrepreneurial rates. Its effect on the *quality* of newly created firms is, however, ambiguous. It depends on how the reform changed the composition of the pool of individuals who decide to start-up a company. Consider the case where entrepreneurs self-select only on entrepreneurial ability. In this case, removing barriers to entry, as in our policy experiment, would draw in entrepreneurs with lower ability, dampening the expected benefits from increased entrepreneurial activity (Nanda (2008)). Similarly, by making it easier to get financial support to start a firm, the reform might attract potential entrepreneurs who are potentially less driven or lack a financial motive (Moskowitz and Vissing-Jorgensen (2002), Hurst and Pugsley (2011)). Generally, if entrepreneurs self-select on characteristics that correlate with expected gains from entrepreneurial activity, lowering barriers to entry will lead to marginal entrepreneurs of lower quality (the “self-selection” view mentioned in the introduction).

If conversely, potential entrepreneurs ignore these entrepreneurial characteristics and self-select only on characteristics that are orthogonal to expected gains from entrepreneurial activity (e.g., risk aversion), then the reduction in barriers to entrepreneurship will not affect the quality of individuals who decide to create a new firm.

Finally, newly created firms may crowd out existing incumbents, an effect reminiscent of Schumpeter’s creative destruction. This point is potentially important in a large scale reform such as the one we evaluate here. By “democratizing entry”, the reform could increase the competitive pressure on incumbents, both on the product market and in the input market, such as the market for employees. If new entrants are productive, existing firms may be forced to lose market shares, downsize, and potentially exit. However, if new entrants have essentially low productivity, the reform might simply lead to increased turnover among young firms but no significant spillover effect on well-established firms.

3 Data

We use three sources of data, which we obtain from the French statistical institute (INSEE): the firm registry, accounting data on firm performance and employment, and a survey that is conducted every four years on a third of all French entrepreneurs for that year.

3.1 Registry

The firm registry contains the universe of firms that are started each month in France. This is a monthly data set. It is available from 1993 to 2008. For each newly created firm, it includes the industry the firm operates in, using a detailed, 4-digit, classification system (similar to the 4-digit NAICS). It also provides the firm legal status (Sole Proprietorship or LLC). The registry dataset also contains the exhaustive list of French firms at the end of each year, which we use to construct an exit dummy.

INSERT FIGURE 2 ABOUT HERE

As Figure 2 shows, the reform was followed by a steep increase in firm creation. This figure reports the 12-month moving-average of number of firms created per month for various categories of firms or various sample periods. Panel A looks at monthly firm creation for all types of firms and between 1994 and 2009. It shows that, starting in 2003, the number of firms created each month increases from a relatively stable 14,000 in early 2003 to about 18,000 at the end of 2004. The 2003–2004 increase in firm creation is very large compared to previous fluctuations (1995 and 2000). After reaching a plateau in 2005, firm creation starts increasing again. This increase is often linked to a series of later reforms, including the possibility to declare a company online (June 2006) and a reform (“auto-entrepreneur”, in August 2008) designed to facilitate and reduce the cost of small firm creation (with turnover below 30,000 euros). The study of these reforms is beyond the scope of this paper. To avoid any contamination in the post-period results, we focus our analysis on the 1999–2005 time frame. Panel B narrows in on this period. Panel C looks separately at the number of firms created each month that have zero employees at creation (blue, dotted line) or that have zero employees two years after creation (blue, solid line). It shows that in the aggregate, the reform coincides with a large increase in the creation of zero-employee firms, and also in the creation of firms with zero employees two years after creation. Panel D presents the number of firms created each month with at least one employee at creation (blue, dotted line) and with at least one employee two years after creation (red, solid line). Interestingly, the reform is not associated with an increase in the number of firms created with more than one employee. The bulk of the aggregate effect of the reform seems thus to be on firms created with no employee. However, the reform is followed by a large increase in the number of firms that will have at least one employee two years after creation – from 4,000 to 5,000 monthly creations. Taken together, Panels C and D suggest that, while newly created firms are small – they are more likely to have zero employees at creation – they have a non-zero probability to grow and eventually hire employees. We go back to this question in greater detail below.

This dramatic surge in firm creation mostly consists of unemployed entrepreneurs targeted by the PARE agreement. As shown in Figure 1, the number of new firms that receive the ACCRE subsidy (a subsidy only accessible for unemployed entrepreneurs)

progressively increased from 3,000 per month in 2002, to about 6,000 per month in 2005. Hence, between 2002 and 2005, the number of firms created by unemployed people rose by at least 3,000 per month.⁵ This is to be compared to the aggregate increase in firm creation reported in Figure 2, which is somewhere between 3,500 and 4,000. Hence, in the aggregate, the increase in firm creation by unemployed entrepreneurs is enough to explain most of the rise in aggregate firm creation.

INSERT TABLE 1 ABOUT HERE

Table 1 provides annual data on firm creation for 8 broad industries, for both the pre-reform period (1999–2001) and the post-reform period (2003–2005). Labor intensive industries are over-represented. Newly created firms are mostly in Services, Construction, and Retail Trade. However, Table 1 does not show large changes in industry composition of newly created firms after the reform.

INSERT TABLE 2 ABOUT HERE

Table 2, Panel A aggregates creation data at the 4-digit industry level (290 industries), and then averages across all months prior to January 1, 2002 (our pre-reform period). It shows that the average industry experiences, pre-reform, approximately 43.6 creations per month, which leads to an annual number of newly created firms of about 152,000 per year.

3.2 Accounting Data

To monitor the long-term performance of new ventures, we complement the registry data with accounting information from tax files (see Bertrand, Schoar, and Thesmar (2007) for a more detailed description). Tax files provide us with the number of employees at creation as well as two years after creation. They cover all firms subject to the regular corporate tax regime (*Benefice Reel Normal*) or to the simplified corporate tax regime (*Regime Simplifie d'Imposition*), which together represent 55% of newly created firms

⁵It could be higher as some unemployed entrepreneurs may not take the ACCRE subsidy when starting their business.

during our sample period. Small firms with annual sales below 32,600 euros (81,500 euros in retail and wholesale trade) can opt-out and choose a special micro-business tax regime (*Micro-Entreprise*), in which case they do not appear in the tax files. Since expenses, and in particular, wages cannot be deducted from taxable profits under the micro-business tax regime, firms opting for this regime are likely to have zero employees. For this reason, in the empirical analysis we will assume that firms that do not appear in the tax files do not have employees.

Table 2, Panel B presents descriptive statistics for firms with non-missing accounting data. The average firm has 0.49 employees at creation. This number includes the entrepreneur if she pays herself a salary. There is, however, considerable skewness. Only 20% of firms have at least one employee at birth. Two years after creation, firms have, on average, 0.87 employees. On average, 25% of the firms hire in the first two years, while 16% exit.

3.3 SINE Survey

To measure additional demographic and personal information on the entrepreneurs (such as education, age, growth expectations, etc.), we use a large-scale survey run by the French statistical office every four years, the SINE survey (see Landier and Thesmar (2009) for an extensive description of this survey). We only have two cross-sections in the relevant time period: 2002 and 2006. 2002 corresponds to the pre-reform period – the survey is done during the first semester of 2002, while the unemployment agency advertises the reform only in the second half of 2002. 2006 corresponds to the post-reform period. SINE has a remarkably large coverage. It covers approximately a third of newly created firms in the first six months of a survey year (26,683 observation, in 2002, and 29,538 observations in 2006).

The SINE survey provides information on the education of entrepreneurs as well as their ambition (evidenced in their answer to the question, “Do you plan to hire in the next twelve months?”). Table 2, Panel C reports descriptive statistics on this survey. 50% of the entrepreneurs are at least high school graduates and 14% have at least a five-

year college degree (which is equivalent to having a graduate degree in the US). 23% of surveyed entrepreneurs plan to hire in the year following creation.

4 Empirical Strategy

4.1 Identifying Assumption

We seek to evaluate the reform’s impact on various firm- and industry-level outcomes, and, in particular, whether it led to an increase in entrepreneurial activity. The main identification challenge is to separate the effect of the reform from any other contemporaneous change in fundamentals that could have simultaneously affected these outcomes. To this effect, we use a standard difference-in-difference analysis. To create our treatment and control groups, we split the cross-section of industries based on the fraction of sole proprietorships among newly created firms in each industry before the reform. The reform was aimed at unemployed individuals with limited capital, and thus, much more likely to become sole proprietors. This relationship between employment status and the legal form of newly created firms is obvious from the 2002 SINE survey: In the 2002 wave of the SINE survey, 70% of unemployed entrepreneurs choose to create their firm as a sole proprietorship, while only 45% of “employed” entrepreneurs make this choice. We thus expect the reform to have a large effect in those industries, where newly created firms are mostly sole proprietorships, while having a much smaller effect on industries where most newly created firms are limited-liability corporations. Accordingly, our control (treatment) group is composed of industries in the top (bottom) quartile of the fraction of sole proprietorships among newly created firms.^{6 7} Our identifying assumptions are thus that absent the reform, treated and control industries would have experienced similar

⁶The fraction of sole proprietorships among newly created firms is measured using the monthly creation file from the registry at the 4-digit industry level. It is computed in the pre-reform period.

⁷An alternative treatment variable could be the fraction of newly created firms started by unemployed workers in each industry, using the 2002 wave of the SINE survey. We chose our treatment variable for two reasons. First, the SINE survey is available only in 2002, while we can compute the fraction of sole proprietorships among newly created firms every year. Second, we use a fine definition of industries (290 industries), so that a sizeable fraction of industries have a scarce representation in the SINE survey, while we can compute our treatment variable over the exhaustive sample of newly created firms every year. We thus believe that the treatment variable is much more precisely estimated this way.

evolutions in entry rates and other outcomes of interest. In particular, this assumes that the industry characteristic used in defining the treatment and control group (fraction of sole proprietors among entrepreneurs) is unrelated to how industry-level entry rates are exposed to the business cycle. We discuss this assumption and the sources of its potential violation in Section 4.3.

For robustness, we also repeat our analysis using an alternative treatment variable: the fraction of firms created with zero employees at the 4-digit industry level. Again, we expect the reform to have a negligible impact on those industries where newly created firms are large, so that industries with a small fraction of firms created with zero employees provide a valid control group. Appendix A reports regression results using this alternative treatment definition and shows that our results hold.

In Table 2, we describe our *pre-reform* (1999–2001) sample for each quartile of the treatment variable (i.e., fraction of sole proprietorships among newly created firms). As expected, in industries where newly created firms are predominantly sole proprietors, the average firm is smaller at creation and is less likely to hire in the two years following creation. Entrepreneurs in these industries are also, on average, less educated and less likely to plan on hiring.

4.2 Empirical Specification

Our main specification for industry-level outcomes is as follows:

$$Y_{st} = \sum_{k=1}^4 \alpha_k \cdot T_s^k \times \text{post}_t + \sum_{k=1}^4 \beta_k \cdot T_s^k \times t + \mu_s + \text{MONTH}_t + \epsilon_{st} \quad (1)$$

where Y_{st} is an outcome variable in industry s in month t . $\text{post} = 0$ from January 1999 to December 2001, $\text{post} = 1$ from January 2002 through December 2005. μ_s and MONTH_t are industry fixed effects and month-of-the-year fixed effects. T_s^k is a dummy equal to 1 if the fraction of sole proprietorships among newly created firms in industry s belongs to the k^{th} quartile. If the reform causally impacts outcome Y_{st} , we expect the impact to be larger for industries more likely to have been affected by the reform – so that we should

expect $\alpha_4 > \alpha_3 > \alpha_2 > \alpha_1$. $T_s^k \times t$ are treatment-specific trends. We cluster standard errors at the industry level.

For firm-level outcomes (e.g., the probability that a firm is still active two years after creation), our main specification is as follows

$$Y_{ist} = \sum_{k=1}^4 \alpha_k \cdot T_s^k \times \text{post}_t + \sum_{k=1}^4 \beta_k \cdot T_s^k \times t + \mu_s + \text{MONTH}_t + \epsilon_{ist} \quad (2)$$

where Y_{ist} is the outcome for firm i created in month t in industry s . We also cluster standard errors at the industry level in this specification.

INSERT FIGURE 3 ABOUT HERE

Figure 3 provides a graphical illustration of our identification strategy and our main regression analysis. For each industry, we compute the log number of firms created each month from 1999 to 2005 normalized by the average monthly log number of firms created in the same industry in 1999 and 2000. This corresponds to an industry log-growth of firm creation in month t and industry j relative to 1999–2000, our benchmark years. We then average these growth rates across industries for each quartile of our treatment variable (i.e., the fraction of sole proprietorships among newly created firms by industry). We then plot a 12-month moving average of this average growth rate for all four quartiles.

Figure 3 first illustrates an important aggregate surge in entrepreneurial activity following the reform relative to the 1999–2000 period, which is consistent with Figure 2. However, this surge was much more pronounced in industries with a larger fraction of sole proprietorships among newly created firms. Entrepreneurial activity increased by about 10% in industries in the bottom quartile of our treatment variable, while it increased by 25% in industries in the top quartile. More generally, growth in entrepreneurial activity increases monotonically with the treatment “intensity”, with industries in larger quartiles exhibiting stronger growth in firm creation.

4.3 Discussion

INSERT TABLE 3 ABOUT HERE

Two endogeneity concerns arise from the creation of our control group.

As stated in Section 4, our identification strategy assumes that the industry characteristic used in defining the treatment and control groups (i.e., the fraction of sole proprietors among entrepreneurs) is not correlated with how industries are exposed to macroeconomic fluctuations. If this is not the case, then industries in the control and treatment groups could experience different evolutions, even in the absence of a reform – a violation of the parallel trend assumption. One might worry, however, that industries with a larger fraction of sole proprietors tend to have a higher exposure to the business cycle, in which case the 2003 economic recovery would cause more growth in firm creation in treated industries for reasons unrelated to the reform we analyze. To invalidate this hypothesis, we estimate equation (1) using the log of industry sales as our left-hand side variable. Industry sales are computed by aggregating firm-level sales at the industry level, and since financial statements are only observed annually, they are annual. Table 3 reports the results. Because we include industry fixed effects, the coefficient on post captures sales growth between the pre- and the post-period. Column (1) includes only the post variable and shows that industry sales increased overall by 8.2% in real terms around the reform. The reform coincided with a period of accelerated growth in France. However, columns (2) and (3) show that this increase in industry sales is not related to our treatment variable, i.e., the fraction of sole proprietorships among newly created firms. In column (3), none of the α_k coefficients (the interaction terms) are statistically significant. Columns (4)-(6) reach the same conclusion using value added instead of sales to measure industry growth. These results invalidate the idea that our treatment industries are simply more exposed to the business cycle and thus experience higher growth in the post-2003 period because of the economic recovery in France at that time.

A second concern is that the industry characteristic we use to define our treatment (the fraction of sole proprietorships among newly created firms) is correlated with other industry characteristics that could explain how entry in these industries reacted to the 2003 aggregate recovery. For instance, industries where firms start on a small scale could be industries with better growth opportunities or industries with little upfront investment. At the same time, it is plausible that entry in industries with large growth opportunities

or small capital intensity could benefit more from an aggregate recovery. Table 3 has already shown that overall, treated industries did not grow faster post-reform. To further account for this concern, we augment our main specification, equation (1), by interacting both the post dummy and a trend variable with a measure of industry capital intensity (the average assets-to-labor ratio of firms in the industry from 1999 to 2001) and industry growth (the average growth rate of sales for firms in the industry from 1999 to 2001). In most regressions, the inclusion of these additional controls does not affect our results, both quantitatively and qualitatively.

5 Results

5.1 Impact of the Reform on the Creation of New Firms

INSERT TABLE 4 ABOUT HERE

We first analyze the growth in firm creation induced by the reform. We estimate equation (1) using the log number of firms created in industry s in month t as our dependent variable.⁸ The regressions use a balanced sample of 290 industries over 84 months – from January 1999 to December 2001 for the pre-period and January 2002 to December 2005 for the post-period – and thus a total of 24,360 industry-month observations. Column (1) is a simple time-series difference estimate of the reform. It only includes the post dummy. The estimated coefficient on the post dummy is 0.1 and significant at the 1% confidence level. Following the reform, the monthly number of newly created firms increased, on average, by 10% across all industries. Given that there 290 industries and that 44 firms are created every month in the average industry prior to the reform (see Table 2), this amounts to 1,300 additional firms per month being created after the reform. This result differs in magnitude from the aggregate growth of new creation observed in Figure 2, which reports an increase by about 3,500 per month between 2001 and 2004. This dis-

⁸More precisely, our dependent variable is $\log(1 + \# \text{ firms created})$. Some smaller industries experience months without any creation. Using $\log(\# \text{ firms created})$ as our dependent variable would lead us to drop these industries. To keep a balanced sample of industries, we thus use instead $\log(1 + \# \text{ firms created})$. The results are similar same when using $\log(\# \text{ firms created})$.

crepancy comes from the fact that we conservatively define the post dummy as equal to 1 starting in January 2002, while the effect of the reform only starts to materialize in late 2002 and is progressive.⁹

Column (2) estimates our main specification (equation (1)) without including the treatment-specific trends. Column (3) adds these treatment-specific trends. Column (4) estimates the augmented version of equation (1), where industry capital intensity and growth are interacted with the post dummy and a trend. As seen in Figure 3, post-reform growth in firm creation increases monotonically with treatment “intensity”. For industries in the top quartile of the treatment variable, firm creation grows by 12 percentage points more following the reform than for industries in the bottom quartile (i.e., industries with the lowest fraction of sole proprietorships at creation). Given that there are 72 industries in the top quartile of sole proprietorships, and that these industries create 87 firms per month prior to the reform (see Table 2), this corresponds to an increase of 750 newly created firms each month. This number underestimates the overall impact of the reform, since firm creation also grows significantly more in the third quartile relative to the bottom quartile (approximately 250 new creations per month). Taking the third and fourth quartile of treatment together (the treatment group), we find that treated industries experience an increase in firm creation following the reform of about 1,000 newly created firms per month. This effect is slightly larger once we control for industry-specific trends (column (3)) and industry characteristics interacted with post and time trends (column (4)).

5.2 The Quality of Post-Reform Start-Ups

In this section, we explore whether the reform led to a significant change in the characteristics of newly created firms. The first characteristic we look at is job creation. As mentioned before, the reform could lead individuals of lower ability, have less ambition, or higher risk-aversion into self-employment. In this case, the number of jobs created by newly created firms in treated industries should drop after the reform relative to control

⁹We have checked that our results hold and are actually stronger if we exclude 2002 from the sample.

industries. Alternatively, individuals may have only noisy signal of their ability as entrepreneurs. The entrepreneurs drawn in by the reform are then comparable to those who created a firm prior to the reform. The reduction in the cost of entry triggered by the reform then allows for a larger pool of equally talented people to enter self-employment. In this case, the number of jobs created by newly created firms in treated industries should not change after the reform relative to control industries.

INSERT TABLE 5 ABOUT HERE

Table 5 explores the effect of the reform on job creation by newly created firms, both at the time of firm creation and two years after firm creation. Columns (1)-(4) break down the results from Table 4 by cutting the sample into start-ups that create at least one job when they are created and start-ups that don't. More precisely, we estimate equation (1) using as a dependent variable the number of firms created in industry s in month t with zero employees at creation (columns (1) and (2)) and the number of firms created in industry s in month t with at least one employee. We find that newly created firms tend to be smaller at birth following the reform. While the reform led to a large increase in the number of firms created with zero employees (columns (1) and (2)), it had no effect on the number of firms created with more than one employee (columns (3) and (4)). Quantitatively, the creation of firms with zero employees increased by a significant 16 percentage points following the reform in treated industries relative to control industries (Q4 vs. Q1 of our treatment variable). In contrast, the creation of firms with at least one employee increased by an insignificant 0.8 percentage points following the reform in treated industries relative to control industries. In other words, the bulk of the effect of the reform on new firm creation is concentrated on firms with zero employees at creation. This is not surprising given that the reform was targeted to unemployed workers, with mostly little financial and human capital.

That the reform mostly had an impact on the creation of zero-employee firms does not mean that it had little or no effect on job creation overall. As long as zero-employee firms create jobs in the future with some probability, the reform still has the potential to stimulate employment in the aggregate. If zero-employee firms create one job after

two years with probability π and the reform leads to the creation of ΔN additional zero-job firms, the reform generates $\pi\Delta N$ new jobs. Hence, even if the reform only creates zero-employee firms, as long as π does not go down to zero after the reform, its effect on job creation is large provided the number of induced firm creation ΔN is large. We already know from Table 5, column (2) that the number of induced creations is indeed large (+16% in the top treatment quartile). We now look for evidence on long-term job creation by newly created firms after the reform.

Table 5, columns (5)-(8) show that the reform did lead to a significant increase in the creation of firms that do not hire any employee after two years (column (6), increase of 9.3%). However, the number of firms that eventually have at least one employee two years after creation also increased significantly, and by a larger amount (column (8), increase of 14%). Overall, the reform thus led to increased entry by both job-creating entrepreneurs and zero-employee entrepreneurs. This is not consistent with the view according to which the reform worsened the pool of entrepreneurs, at least in the job-creation dimension.

INSERT TABLE 6 ABOUT HERE

We also directly check that firms created in treated industries are as likely to hire as other firms, i.e., that π is the same for firms created before or after the reform. To this end, we estimate equation (2) using as a dependent variable a firm-level dummy equal to 1 if the firm hires at least one employee between its creation date and the second calendar year after creation. We do not find any differential change in the propensity to hire between firms in treated vs. control industries (Table 6, columns (1)-(3)): firms created in treated industries after the reform were not *less likely* to hire. Hence, an increase in entrepreneurial activity, even from zero-employee firms, mechanically leads to a proportionate increase in the number of firms with eventually at least one employee.

Another dimension of quality is the probability of exit. In our sample, 16% of newly-created firms exit the sample in the two years following creation. This high attrition rate is consistent with existing cross-country evidence. Our data does not indicate why entrepreneurs leave the sample. However, it is very likely that a firm that drops out of

the sample is closed down, so that we interpret exit as a measure of failure.¹⁰ In Table 6, columns (4)-(6), we estimate equation (2) using a dummy of exit within two years as our dependent variable. The reform has the same impact on the probability of exit in treated and non-treated industries. The probability of exiting the sample increases by 1.1 percentage point in the aggregate, which is small when compared to the large surge in firm creation observed in Table 1. However, this increase is similar across industries (column (6)). Overall, firms created in industries more exposed to the reform have the same probability of survival and the same probability of hiring as firms created in “control” industries. Even though they are smaller at creation, these newly created firms do not seem to have a lower potential to create jobs.

INSERT TABLE 7 ABOUT HERE

We provide further evidence that firm quality does not decline after the reform in Table 7. We again estimate equation (2), but we now use observable characteristics of the entrepreneurs as our dependent variable. More precisely, we look at education and initial “ambition to grow”, which have both been shown to correlate with entrepreneurial success. These characteristics are available from the SINE survey (see Section 3.3 for a description), for a third of newly created firms in the first semester of 2002 and 2006. We have 26,783 observations in 2002, and 29,538 observations in 2006. Our ex ante measure of entrepreneurial quality are the following: a dummy variable equal to one if the entrepreneur holds a high-school degree, another indicating that she holds a college degree, and one that indicates if she declares that she “plans to hire” in the next twelve months. The difference with the previous firm-level regressions is that we only have two cross-sections of data available, one before and one following the reform.

Table 7, panel A starts by showing that, in the pre-reform period (2002), these ex-ante measures of quality correlate well with ex-post entrepreneurial success. Entrepreneurial success is measured as the firm’s employment four years after creation, i.e., in 2006 (columns (1)-(3)), the probability that the firm has more than one employee in 2006

¹⁰The 1998 wave of the SINE survey shows that only 5% of newly created firms that no longer exist two years after creation have been purchased or transmitted, i.e., 95% correspond to firms that have closed down permanently.

(columns (4)-(6)), and the probability that the firm has more than five employees (columns (7)-(9)). More educated and ambitious entrepreneurs are more likely to head large firms. For instance, entrepreneurs who “plan to hire” at creation end up with a larger probability of having at least one employee (column (5), increase of 17 percentage points) as well as a higher probability of having at least five employees (column (8), increase of 7.9 percentage points).

Table 7, Panel B then looks at the impact of the reform on the share of “good” newly created firms, using these ex ante measures of quality. Our empirical strategy is similar to that of Table 6, except we use our ex ante measures of quality as dependent variables and work on the restricted sample of newly created firms surveyed in SINE in the first six months of 2002 (pre-period) and in the first six months of 2006 (post-period). We do not find a significant change in the composition of entrepreneurs by education. Although the average level of education of entrepreneurs increases across all industries, reflecting perhaps the positive trend in educational attainment in France, treated industries do not differ from control industries in this dimension. The proportion of entrepreneurs with a high-school or a college degree is not significantly different in treated industries relative to control industries (columns (1)-(4)). While the lack of statistical significance could come from the reduced size of the sample, the coefficient estimates are very close to 0. These results are not consistent with the hypothesis that the reform lowered the education of the marginal entrepreneurs, at least when using the coarse measures available in the SINE survey.

Surprisingly, the entrepreneurs drawn in by the reform appear more ambitious. In table 7, Panel B, columns (5)-(6), we find that the fraction of entrepreneurs in treated industries that plan to hire in the next twelve months increases by 3.8 percentage points relative to entrepreneurs in control industries. This is not a trivial effect given that only 23% of entrepreneurs surveyed in SINE plan to grow, on average. While we interpret this results in terms of entrepreneurial ambition, it is also consistent with new entrepreneurs being more optimistic. As long as ambition and optimism are important ex ante determinants of entrepreneurial success, this result remains inconsistent with the hypothesis that the reform led to a worsening of the pool of entrepreneurs.

5.3 Job Creation

We now estimate the effect of the reform on total job creation. To do this, we estimate equation (1) on industry-level employment data. Precisely, we use the log of 1 plus L_{st} as our dependent variable, where L_{st} is the total number of jobs reported after two years of existence by all firms created at date t , as reported by the tax files. L_{st} measures the overall number of jobs that firms created today will create in two years. Importantly, this measure takes exit into account: firms that will exit before two years will simply not contribute to L_{st} , whatever their total employment at creation.

INSERT TABLE 8 ABOUT HERE

One issue in the measure of L_{st} is that we might be missing the entrepreneur's job itself. The tax files do not specify whether the entrepreneur is one of the firm's employees or whether or not she receives any wages. We thus make two alternative assumptions to bound this potential measurement error. In columns (1)-(2), we make the aggressive assumption that the entrepreneur is never a wage earner, and add one to each surviving firm's future employment. Accordingly, we use $\log(1 + L_{st} + NS_{st})$ as our left-hand side variable, where NS_{st} is the number of firms born at t that will still be active in two years. In columns (3)-(4), we make the conservative assumption that all entrepreneurs are already counted as employees of their own firm. We then simply use $\log(1 + L_{st})$ as the dependent variable, thereby assuming that L_{st} contains all entrepreneurial jobs created. If the firm reports zero employees after two years, we implicitly assume that the entrepreneur's main job is not with the firm. This is a conservative assumption designed to provide a lower bound on the employment effects of the reform. Table 8 reports the regression estimates. We find that the reform had a large impact on aggregate job creation. After the reform, the number of jobs created by entrepreneurial firms two years after creation increases by 21 percentage points more in treated industries relative to control industries (column (2)). We know from Table 2, in the last line of Panel A, that treated industries (i.e., in the top quartile of the fraction of sole proprietorships among newly created firms) create, on average, 118 jobs per month pre-reform. Thus, the reform leads to at least $118 \times (e^{.21} - 1) = 28$ additional jobs created monthly. Since the top

quartile of our treatment variable has 72 industries, 2,000 jobs per month are created in these industries through the reform. Under the more conservative assumption that the tax files' employment figure always includes the entrepreneur herself, we obtain a smaller estimate of 750 new jobs created monthly in the treated industries (Table 8, column (4)). Naturally, this difference comes from the difference in the base rate of jobs created by entrepreneurial firms under the two assumptions: under the conservative assumption, newly created firms in treated industries generate on average 43 jobs, while the aggressive assumption lead to 118 jobs created monthly.

These results suggest that the reform led to the creation of between 9,000 and 25,000 new jobs every year. These numbers are of course lower bounds on the true quantitative effect of the reform on job creation since even our control group is affected by the reform. Additionally, these figures do not take general equilibrium effects into account. For instance, the additional firms created following the reform may be gaining market shares and employees at the expense of existing incumbent firms. These general equilibrium effects are the focus of the next section.

5.4 General Equilibrium Effects: Crowding Out

To investigate possible crowding out of existing jobs, we re-run our industry-level regressions (equation (1)) and look at the impact of the reform on employment growth for incumbent firms. We report the results in Table 9. More precisely, we define incumbents as firms present in the data in year t but created before $t - 4$. This long lag ensures that all incumbents were started before the reform we are studying. We then define our dependent variable as the growth rate of total employment in all incumbent firms by industry. In columns (1)-(2), we focus on small incumbents only (i.e., with less than five employees), which are more likely to be exposed to the competition of these new entrants. In columns (3)-(4), we look at large incumbents only (i.e., with more than five employees). Since we use industry-level annual data (these tax files are only available annually), there are only 2,320 observations in these regressions, corresponding to a balanced panel of 290 industries followed over 8 years (1999–2006).

INSERT TABLE 9 ABOUT HERE

Table 9 shows how the reform led to lower employment growth for small incumbents. Following the reform, annual employment growth fell by a significant 2.6 percentage points in treated industries relative to control industries (columns (1) and (2)). This effect is robust to controlling for industry capital intensity and industry growth. This result is consistent with competitive dynamics where newly created firms partially crowd out existing small firms. To strengthen our intuition, we also analyze the effect of the reform on large incumbents. Large incumbents (here, firms with 5 employees or more) are less likely to directly compete with new entrants, who tend to be very small (median employment is zero). Indeed, Table 9, columns (3)-(4) show that large incumbents' employment does not differentially change following the reform in treated vs. control industries (insignificant 0.4 percentage points estimate in column (4)).

Since the average industry in our treatment group (i.e., in the top quartile of the fraction of sole proprietorships among newly created firms) has, on average, 5,196 employees working for small incumbents (Table 2, Panel D), the industry-level impact of the reform on small incumbents is estimated to be $5,196 \times 0.026 = 135$ jobs destroyed per year and per industry. Aggregating over all the industries in the treatment group, this amounts to 10,000 jobs per year. This has to be compared to the approximate 9,000 to 25,000 jobs created per year that we estimated in Section 5.3 using the same treatment variable. Of course, these numbers are imprecisely estimated. Nevertheless, this approach suggests that crowding out effects are of the same order of magnitude as the direct effect of the reform.

In Table 9, columns (5)-(6), we look directly at the overall effect of the reform on industry employment. To this end, we use the growth rate in the total number of jobs in small incumbents and in firms created over the last two years as our dependent variable. This variable cumulates the direct job creation from the reform with the job destruction induced by crowding out. We estimate the net effect by again comparing industries in the treatment vs. control group. Since columns (3)-(4) of Table 9 have shown that the reform has no effect on large incumbents' employment, we exclude the contribution of the large

incumbents to industry employment. Columns (5)-(6) show that treated industries do experience a two percentage point larger growth in employment coming from entrepreneurial firms and small incumbents following the reform and relative to control industries. While this interaction coefficient (α_4) is large, it is also not statistically significant (0.028 standard error). Overall, we conclude that, while the reform allowed unemployed workers to start firms on a large scale, crowding-out effects also appear significant. These general equilibrium effects make it hard to quantify the aggregate employment gains from such a reform.

5.5 Efficiency

The previous section has demonstrated that the reform led to a significant reallocation of labor from small incumbent firms to newly created firms. A natural question in this context is whether or not such a reallocation is desirable. It is of course very difficult to make any welfare statements in our context. On the one hand, newly created firms may generate large growth opportunities, which cannot be observed in our limited time frame. On the other hand, the reform is costly for public finances since it amounts to a subsidy for unemployed entrepreneurs. Nevertheless, we can still empirically explore the relative productivity of firms created following the reform with that of small incumbent firms that are negatively affected by the reform as a first pass to a welfare analysis.

Table 10 provides regression evidence to this effect. It estimates equation (2) using measures of productivity at the firm-level as our dependent variable. Productivity is measured through wages or value added, and sales divided by the number of employees. Wage is measured as total wage bill normalized by employment. In principle, value added per worker is a better measure of productivity, as it excludes intermediate input purchases, but for small firms, total sales may be better reported. We also restrict the sample to two categories of firms: (1) entrepreneurial firms created in year t and (2), the small incumbent firms that are the most likely affected by the crowding-out effects. We construct category (2) as “shrinking incumbents”, i.e., firms whose labor force decreases by at least one body count between t and $t + 1$ — including those incumbents who exit the sample in $t + 1$.

For each newly-created firm in year t , productivity is measured as of year $t + 2$.

INSERT TABLE 10 ABOUT HERE

Columns (1), (3), and (5) of Table 10 show that, prior to the reform, wages and productivity in newly created firms are much larger than those of shrinking incumbents. Annual wages are larger by about 5,200 euros; value added per worker is higher by about 7,000 euros per year. This difference is sizable, considering that the average wage (including payroll taxes, as in our data) in France is about 50,000 euros per year. However, these regressions also show that this productivity advantage of newly created firms does not change following the reform. The interaction of the new firm dummy with the post dummy is quantitatively small (about 14 euros) and statistically insignificant. This shows that the productivity distribution of newly created firms did not change very much following the reform. However, this result could mask a relative drop in the productivity of newly created firms in treated industries, and a relative increase in the productivity of newly created firms in control industries. Columns (2), (4), and (6) show that this productivity advantage of newly created firms remained constant following the reform, irrespective of the industries where firms operate. For instance, the larger average wage observed for newly created firms, on average, does not increase differentially following the reform for firms in the control industries relative to firms in the treated industries.

As pointed out in Section 5.4, the aggregate employment effects of this reform are blurred by the reallocation of labor across firms. Yet, the reform can still be efficient: since newly created firms are more productive, both before and after the reform, and in all industries, this reallocation has the potential to lead to significant productivity gains.

5.6 Dissecting the Firm Creation Effect

In this section, we seek to differentiate three potential channels through which the reform could have affected entrepreneurial activity. First, the reform puts a floor on entrepreneurial income by guaranteeing that the entrepreneur gets at least her unemployment benefits. This effect acts like a direct subsidy that shifts the distribution of

entrepreneurial income to the right. Second, by making entrepreneurial income safer, the reform creates positive Net Present Value (NPV) projects in risky industries and thus leads to increased entry in those industries. Third, the reform relaxes credit constraints. The NPV of future payment of unemployment benefits create some additional pledgeable income that increases the entrepreneur’s financing capacity. In this section, we investigate the explanatory power of the last two hypotheses.

We first start by testing out the “risk-taking” hypothesis, whereby unemployed entrepreneurs enter predominantly risky industries following the reform, as these industries were unattractive to them prior to the reform. Tests reported in Table 11 show no support for this hypothesis. In this table, we ask if the effects of the reform are more pronounced in “risky” industries. Because the measurement of risk is difficult, we propose four different measures. The first measure of industry risk (Total volatility) is the sample standard deviation of industry aggregate sales growth. The second measure (Systematic volatility) is the regression coefficient of industry aggregate sales growth on aggregate (economy-wide) sales growth. The third measure (Idiosyncratic volatility) is the average, across all years, of the cross-sectional standard deviation of firm sales growth in the industry. The last measure (Exit rate) is simply the five-year exit rate of new firms. We then estimate the following triple-interaction equation:

$$\begin{aligned}
 Y_{st} = & \sum_{k=1}^4 \alpha_k \cdot T_s^k \times \text{post}_t + \sum_{k=1}^4 \beta_k \cdot T_s^k \times t \\
 & + \sum_{k=1}^4 \gamma_k \cdot T_s^k \times \text{post}_t \times \text{Risk}_s + \sum_{k=1}^4 \delta_k \cdot \text{post}_t \times \text{Risk}_s + \mu_s + \text{MONTH}_t + \epsilon_{st} \quad (3)
 \end{aligned}$$

where Risk_s is one of the industry-risk measures defined above. Under the “risk-taking” hypothesis, we would expect γ_4 to be positive: the effect of the reform on firm creation should be larger in treated industries, but even more so in “risky” treated industries. Estimates reported in Table 11 show no robust, consistent pattern across all four risk measures. Entry is indeed more concentrated in risky treated industries when risk is measured with total volatility. However, entry is less concentrated in treated industries with higher exit rates. The other two measures are not statistically significant.

We now look for evidence on the “financing constraint” hypothesis. By providing a stream of safe cash flows to the entrepreneur, the reform increases the entrepreneur’s pledgeable income and hence her debt capacity. Under this hypothesis, we should observe newly created firms borrowing more in treated industries after the reform. To test this hypothesis, we estimate equation (2) using various proxies of firm debt as our dependent variables and report the results in Table 12. A positive α_4 coefficient would vindicate the “credit constraint” hypothesis. In columns (1)-(3), we focus on entrepreneurs present in the 2002 and 2006 waves of the SINE survey. The SINE survey provides us with information on whether or not the newly-created firm has some debt (column (1)), whether or not the entrepreneur took on personal debt (column (2)), or whether or not the entrepreneur took on personal debt, conditionally on the company taking on some (column (3)). In columns (4) and (5), we use the leverage ratio and the log of bank debt at the end of the first year of the firm’s existence as our dependent variable. Data on debt are only available for firms present in the tax files. We set debt to zero if the newly-created firm is not in the tax files. In the last two columns, we use the same variables, but restrict ourselves to firms reporting in the tax files. Table 12 shows no evidence that the debt policy of newly created firms is affected in a significant way by the reform.

While the reform provides a sizable subsidy to unemployed entrepreneurs, we find little evidence in the data that the structure of this subsidy led entrepreneurs to enter riskier industries. We also find no evidence that this subsidy affected the financing capacity of newly created firms, at least when looking at debt measures. It thus appears that the reform works mostly through the removal of monetary disincentives for unemployed workers to start businesses.

6 Conclusions

This paper looks at a large-scale policy reform that heavily subsidized entry into entrepreneurship by French unemployed workers. Our identification rests on the idea that some industries – industries where new ventures are, on average, small – were more exposed to the reform. Unsurprisingly, we find that the reform led to a large acceleration in

the creation of new firms, especially in those industries. The new firms created through the reform tend to be mostly zero-employee firms. More surprisingly, while small at creation, these firms are not more likely to exit, and not less likely to grow over the two years following creation. Similarly, measures of entrepreneurial quality (education, ambition) are not lower for these entrepreneurs drawn in by the reform. As a result, newly created firms are estimated to create directly between 9,000 and 25,000 jobs annually.

The paper also emphasizes that this partial equilibrium analysis does not provide a fair evaluation of the reform. Empirically, small incumbents experience a reduction in employment growth because of the reform. This crowding out effect is of the same order of magnitude as the direct creation effect, so that the overall effect on job creation is inconclusive. At the same time, newly created firms are more productive than incumbents. This reform thus provides a textbook example of Schumpeterian creative destruction.

References

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, 597-628.

Branstetter, Lee G., Francisco Lima, Lowell J. Taylor, and Ana Venâncio, 2010, “Does Entry Regulations Deter Entrepreneurship and Job Creation? Evidence From Recent Reforms in Portugal”, NBER Working Paper 16473.

Andrew, J. Oswald, and David J. Blanchflower, 1998, “What Makes an Entrepreneur?”, *Journal of Labor Economics*, 16, 26-60.

Bruhn, Miriam, 2011, “License to Sell: The Effect of Business Registration Reform on Entrepreneurial Activity in Mexico”, *Review of Economic and Statistics* 93, 382-386.

Card, David, and Levine, Phillip B., 2000. “Extended benefits and the duration of UI spells: evidence from the New Jersey extended benefit program,” *Journal of Public Economics*, vol. 78(1-2), pages 107-138, October.

Caves, Richard E., 1998, “Industrial Organization and New Findings on the Turnover and Mobility of Firms”, *Journal of Economic Literature* 36, 1947-1982.

Cetorelli, Nicola, and Philip E. Strahan, 2006, “Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets”, *Journal of Finance* 61, 437-461.

Cole, Shawn, 2009, “Financial Development, Bank Ownership, and Growth. Or, Does Quantity Imply Quality?”, *Review of Economics and Statistics* 91, 33-51.

de Mel, Suresh, David McKenzie, and Christopher Woodruff, 2008, “Returns to Capital in Microenterprises: Evidence From a Field Experiment”, *Quarterly Journal of Economics* 123, 1329-1372.

Desai, Mihir, Paul Gompers, and Josh Lerner, 2003, “Institutions, capital constraints and entrepreneurial firm dynamics: Evidence from Europe”, NBER working paper 10165.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-De-Silanes, and Andrei Shleifer, 2002, “The Regulation of Entry”, *Quarterly Journal of Economics* 117, 1-37.

Evans, David S., and Boyan Jovanovic, 1989, “An Estimated Model of Entrepreneurial

Choice under Liquidity Constraint”, *Journal of Political Economy* 97, 808-827.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda, 2013, “Who Creates Jobs? Small Versus Large Versus Young”, *Review of Economics and Statistics* 95, 347-361.

Hamilton, Barton H., 2000, “Does Entrepreneurship Pay? An Empirical Analysis of the Returns to Self-Employment”, *Journal of Political Economy*, 108, 604-631.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen, 1993, “The Carnegie Conjecture: Some Empirical Evidence”, *Quarterly Journal of Economics* 108, 413-435.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen, 1994a, “Entrepreneurial Decisions and Liquidity Constraints”, *Rand Journal of Economics* 25, 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, 53-75.

Hurst, Erik, and Annamaria Lusardi, 2004, “Liquidity Constraints, Household Wealth and Entrepreneurship”, *Journal of Political Economy* 112, 319-347.

Hurst, Erik, and Benjamin Pugsley, 2011, “What Do Small Businesses Do”, *Brookings Papers on Economic Activity*, Fall 2011, 73-142.

Jovanovic, Boyan, 1982, “Selection and the Evolution of Industry”, *Econometrica* 50, 649-670.

Katz, Lawrence F., and Meyer, Bruce D, 1990. “Unemployment Insurance, Recall Expectations, and Unemployment Outcomes,” *The Quarterly Journal of Economics*, vol. 105(4), pages 973-1002, November.

Kerr, William R., and Ramana Nanda, 2009, “Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship”, *Journal of Financial Economics* 94, 124-149.

Kerr, William, and Ramana Nanda, 2010, “Financing Constraints and Entrepreneurship”, HBS Working Paper 10-013.

Kihlstrom, Richard E., and Jean-Jacques Laffont, 1979, “A General Equilibrium Entrepreneurial Theory of Firm Formation Based on Risk Aversion”, *Journal of Political Economy* 87, 719-748.

Klapper, Leora, Luc Laeven, and Raghuram Rajan, 2006, “Entry Regulation as a

Barrier to Entrepreneurship”, *Journal of Financial Economics* 82, 591-629.

Landier, Augustin and David Thesmar, 2009, “Financial Contracting with Optimistic Entrepreneurs: Theory and Evidence”, *Review of Financial Studies* 22, 117-150.

Moffitt, Robert, 1985. “Unemployment insurance and the distribution of unemployment spells,” *Journal of Econometrics*, vol. 28(1), pages 85-101, April.

Moskowitz, Tobias J., and Annette Vissing-Jorgensen, 2002, “The Returns to Entrepreneurial Investment: A Private Equity Premium Puzzle?”, *American Economic Review*

Mullainathan, Sendhil, and Philipp Schnabl, 2010, “ Does Less Market Entry Regulation Generate More Entrepreneurs? Evidence from a Regulatory Reform in Peru” In: J. Lerner and A. Schoar (Eds.), *International Differences in Entrepreneurship*, University of Chicago Press, 159-179.

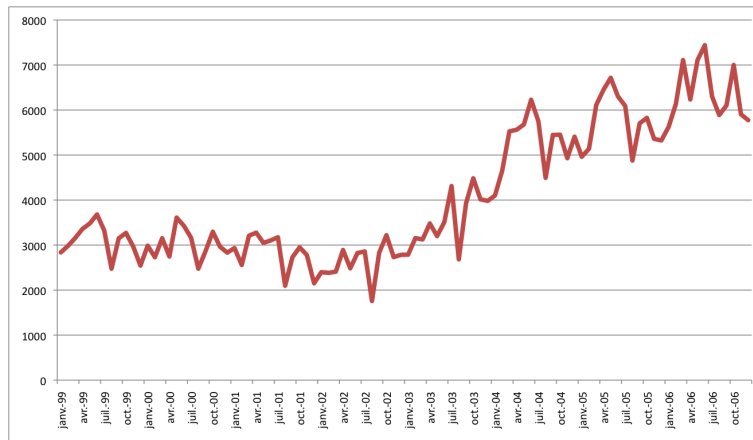
Nanda, Ramana, 2008, “Cost of External Finance and Selection into Entrepreneurship”, HBS Working Paper 08-047.

Rieg, Christian, 2004, “Forte Hausse des Créations d’Entreprises en 2003”, *INSEE Première* 994.

Solon, Gary R, 1985. “Work Incentive Effects of Taxing Unemployment Benefits,” *Econometrica*, vol. 53(2), pages 295-306, March.

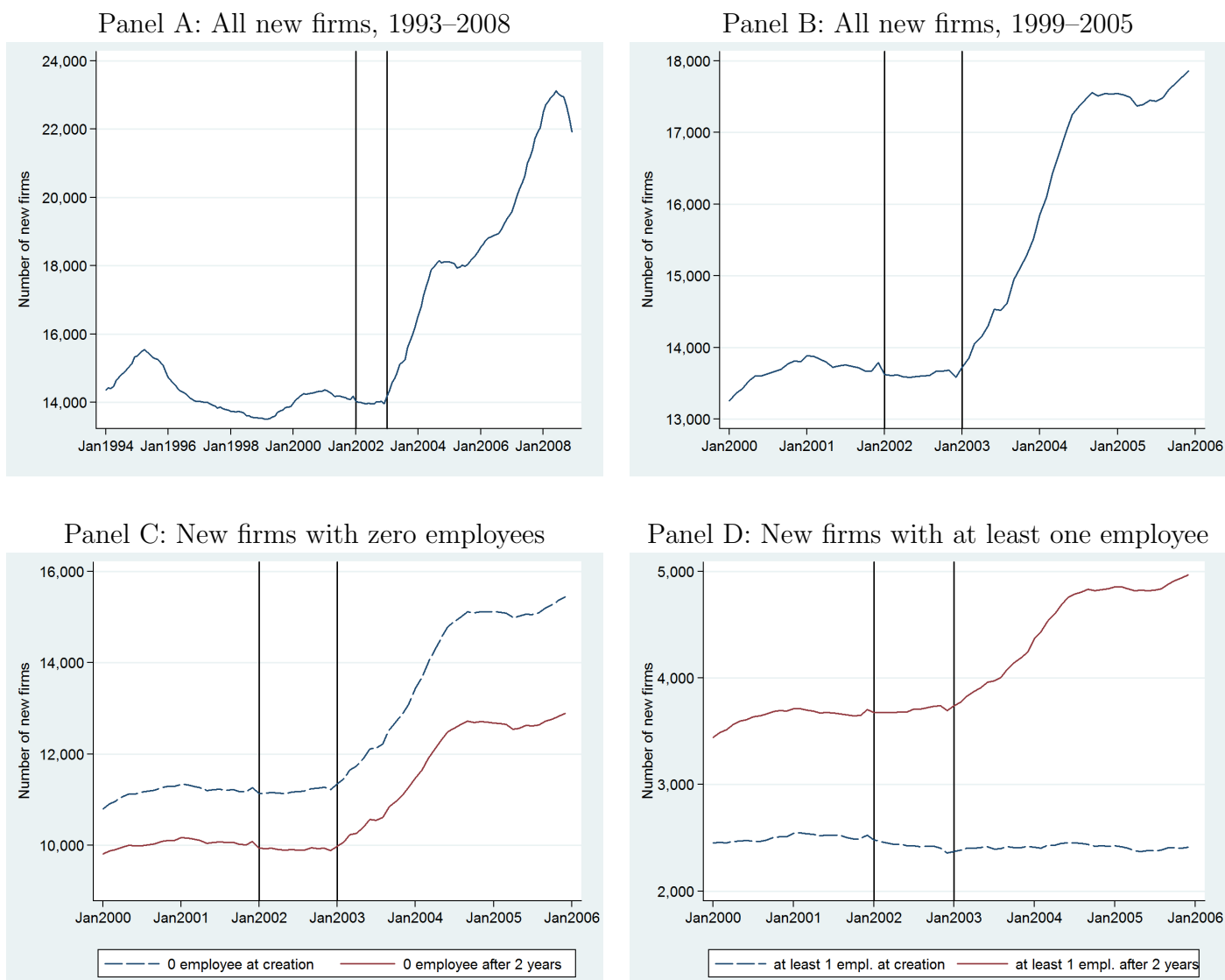
Schoar, Antoinette, 2010, “The Divide between Subsistence and Transformational Entrepreneurship”, in J. Lerner and S. Stern (Eds.), *Innovation Policy and the Economy*, University of Chicago Press, Volume 10, 57-81.

Figure 1: Monthly Number of New Firms Started With the ACCRE Subsidy



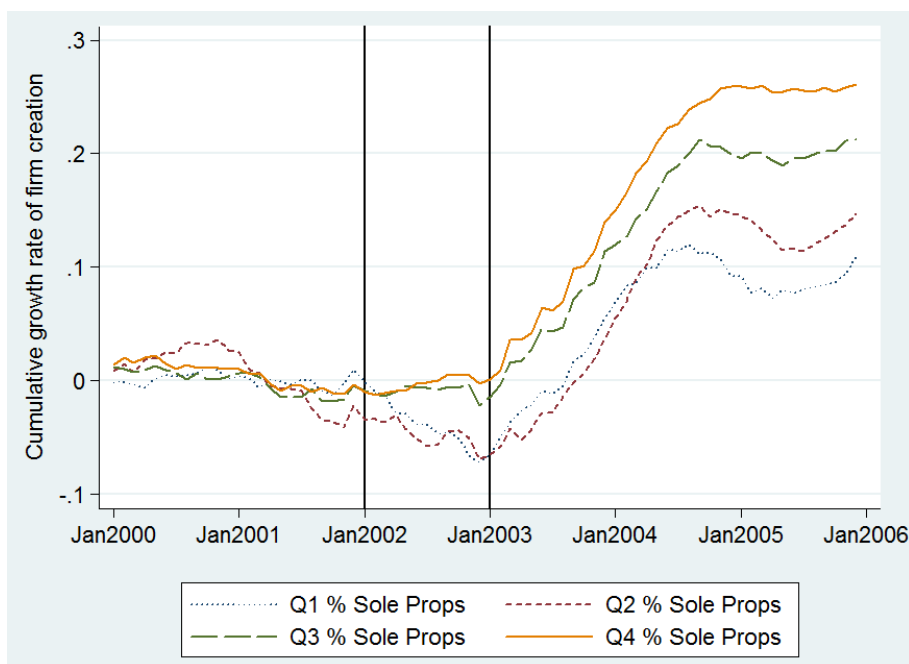
Source: Ministry of labor.

Figure 2: Monthly Number of New Firms



Panel A plots the 12-month moving average of the number of firms created over a long time period. The reform period starts in January 2002 and ends in January 2003. Panel B zooms in on our sample period 1999–2005 (1999 does not appear on the graph as we compute a 12-month moving average). Panel C plots the number of new firms started with 0 employees (dotted blue) and the number of new firms with 0 employees two years after creation including firms that have exited (plain red). Panel D plots the number of new firms started with at least 1 employee (dotted blue) and the number of new firms with at least 1 employee two years after creation (plain red).

Figure 3: Growth Rate in Firm Creation: Treated vs. Control



Each month t and for each quartile Q_k ($k = 1, 2, 3, 4$) of the treatment variable, we compute the average growth rate of the number of firms created in industries belonging to quartile Q_k from the beginning of the sample period (1999–2000) to month t :

$$g_t^k = \frac{1}{\#\text{industries in } Q_k} \sum_{s \in Q_k} \left(\log(\# \text{ firms created}_{st}) - \frac{1}{24} \sum_{\tau \in 1999, 2000} \log(\# \text{ firms created}_{s\tau}) \right).$$

The graph plots the 12-month moving average of g_t^k .

Table 1: Industry Composition: Annual Data

Industry	Pre-reform		Post-reform	
	# creations	(%)	# creations	(%)
Construction	25,454	16,8	34,970	18,3
FIRE	6,546	4,3	9,768	5,1
Manufacturing	9,119	6,0	10,006	5,2
Mining	21	0,0	19	0,0
Retail trade	25,498	16,8	34,683	18,1
Services	68,266	45,0	84,317	44,0
Transportation - Utilities	4,937	3,3	5,031	2,6
Wholesale trade	11,942	7,9	12,711	6,6
Total	151,787		191,506	

Number of firms created per year during the pre-reform period (1999–2001) and the post-reform period (2003–2005).

Table 2: Summary Statistics

	N	Mean	SD	Mean by quartile of % of Sole Prop. new firms			
				Q1	Q2	Q3	Q4
Panel A: New firms, industry-level							
Avg # firms created (monthly)	290	43.62	84	22	18	44	87
Avg # jobs created after two years (monthly)	290	32.49	62	22	22	41	43
——— adding entrepreneurs' jobs (monthly)	290	69.30	123	39	37	78	118
Panel B: New firms, firm-level							
Employment at creation	381,683	0.49	1.9	0.86	0.72	0.55	0.32
Dummy at least 1 employee at creation	381,683	0.20	.4	0.26	0.27	0.22	0.15
Employment two years after creation	381,683	0.87	2.5	1.06	1.38	1.08	0.60
Dummy at least 1 employee two years after creation	381,683	0.29	.45	0.36	0.42	0.36	0.23
Hire during first two years	381,683	0.25	.43	0.31	0.36	0.31	0.20
Exit during first two years	381,683	0.16	.36	0.21	0.15	0.15	0.14
Panel C: New firms, survey, firm-level							
High school graduate	26,783	0.50		0.54	0.59	0.53	0.47
College graduate	26,783	0.14		0.12	0.16	0.16	0.12
Plan to hire	26,783	0.23		0.34	0.34	0.27	0.18
Panel D: Incumbents, industry-level							
# small incumbents	290	2,779	5,289	1,039	1,466	3,597	4,747
# jobs in small incumbents	290	3,647	7,667	1,497	2,381	5,200	5,196
# large incumbents	290	804	1,243	705	791	992	715
# jobs in large incumbents	290	21,967	38,740	27,527	24,135	24,802	11,948

Panels A and B report summary statistics on all new firms started during the pre-reform period (1999–2001). Statistics are computed at the 4-digit industry level in Panel A and at the firm level in Panel B. Panel C uses data from the 2002 SINE survey which covers about 1/3 of all firms created during the first semester of 2002. Panel D reports summary statistics on all incumbent firms during 1999–2001: Incumbents are firms that have been in the tax files for the last four years; small incumbents are defined as incumbents with 5 employees or less and which are not reported to be part of a group; large incumbents are incumbents with more than 5 employees and those which belong to a group.

Table 3: Aggregate Growth Rate: Treated vs. Control

	Sales			Value added		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.082*** (.013)	.095*** (.021)	-.0026 (.013)	.1*** (.013)	.13*** (.02)	.013 (.011)
Q2 % Sole Props × POST		-.063* (.034)	-.031 (.022)		-.086** (.036)	-.02 (.024)
Q3 % Sole Props × POST		-.0084 (.028)	-.011 (.021)		-.041 (.029)	-.029* (.016)
Q4 % Sole Props × POST		.017 (.039)	-.0021 (.018)		.0058 (.038)	-.003 (.016)
Constant	15*** (.0074)	15*** (.0073)	-40*** (8.2)	14*** (.0077)	14*** (.0076)	-44*** (9.2)
Treatment-specific trend	No	No	Yes	No	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,030	2,030	2,030	2,030	2,030	2,030
R-squared	.99	.99	.99	.98	.98	.98

290 industries, 1999–2005, annual. In columns (1) to (3) the dependent variable is the log of industry sales (in 1999 euros). In column (1) the explanatory variable is the post dummy which is equal to 0 from 1999 to 2001 and equal to 1 from 2002 to 2005. In column (2) we interact post with the quartiles of the treatment variable. In column (3) we add treatment-specific time trends. In columns (4) to (6) the dependent variable is the log of industry value added. All regressions include industry fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 4: Firm Creation: Treated vs. Control

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.1*** (.014)	.046* (.027)	-.16*** (.031)	-.25*** (.072)
Q2 % Sole Props × POST		.019 (.043)	.035 (.044)	.027 (.043)
Q3 % Sole Props × POST		.08** (.038)	.11*** (.037)	.11*** (.036)
Q4 % Sole Props × POST		.12*** (.038)	.13*** (.039)	.14*** (.039)
Industry capital intensity × POST				.041* (.025)
Industry growth × POST				-.048 (.038)
Industry capital intensity × Trend				-.014 (.0085)
Industry growth × Trend				.054*** (.017)
Constant	3.2*** (.017)	3.2*** (.018)	.98*** (.24)	.98*** (.23)
Treatment-specific trend	No	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360
R-squared	.92	.92	.92	.92

290 industries, 1999–2005, monthly. The dependent variable is the log of one plus the number of new firms. In column (1) the explanatory variable is the post dummy which is equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005. In column (2) we interact post with the quartiles of the treatment variable. In column (3) we add treatment-specific time trends. In column (4) we also interact the post dummy and the time trend with pre-reform industry capital intensity and industry sales growth. All regressions include industry and month-of-the-year fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 5: Job Creation Through New Firms

	Number of firms created							
	0 employees at creation		≥ 1 employee at creation		0 employees after 2 years		≥ 1 employee after 2 years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	-.17*** (.031)	-.31*** (.072)	-.055** (.026)	-.054 (.062)	-.11*** (.028)	-.21*** (.072)	-.15*** (.026)	-.25*** (.063)
Q2 % Sole Props \times POST	.04 (.044)	.031 (.043)	-.016 (.034)	-.019 (.034)	.013 (.043)	.0058 (.043)	.021 (.039)	.016 (.039)
Q3 % Sole Props \times POST	.12*** (.037)	.13*** (.036)	.012 (.033)	.012 (.033)	.048 (.035)	.049 (.034)	.14*** (.034)	.14*** (.034)
Q4 % Sole Props \times POST	.15*** (.039)	.16*** (.038)	.0067 (.035)	.0088 (.035)	.086** (.037)	.093** (.037)	.13*** (.035)	.14*** (.035)
Industry capital intensity \times POST		.057** (.024)		.0038 (.02)		.041* (.024)		.04** (.02)
Industry growth \times POST		-.031 (.037)		-.032 (.027)		-.035 (.041)		-.016 (.037)
Industry capital intensity \times Trend		-.019** (.0087)		.0038 (.0073)		-.0087 (.0091)		-.023*** (.0069)
Industry growth \times Trend		.048*** (.018)		.043*** (.011)		.032 (.02)		.11*** (.015)
Constant	.072 (.24)	.072 (.23)	2.1*** (.18)	2.1*** (.18)	.66*** (.24)	.66*** (.24)	.092 (.22)	.092 (.2)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360	24,360	24,360	24,360	24,360
R-squared	.91	.91	.84	.84	.91	.91	.86	.86

290 industries, 1999–2005, monthly. In columns (1) and (2) the dependent variable is the log of one plus the number of new firms started with 0 employees. In column (1) the explanatory variables are the post dummy equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005, and its interactions with the quartiles of the treatment variable. In column (2) we also interact the post dummy and the time trend variable with pre-reform industry capital intensity and industry sales growth. In columns (3) and (4) the dependent variable is replaced by the log of one plus the number of new firms started with 1 employee or more. In columns (5) and (6) we use the number of new firms with 0 employees two years after creation, including those which have exited. In columns (7) and (8) we count the number of new firms with 1 employee or more two years after creation. All regressions include industry and month-of-the-year fixed effects and treatment-specific time trends. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 6: Firm Quality: Ex Post Measures

	Hire			Exit		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.01*** (.0038)	.0076 (.0046)	-.0021 (.013)	.011*** (.0017)	.0036 (.0058)	.019 (.014)
Q2 % Sole Props × POST		-.0058 (.008)	-.0088 (.0081)		.0032 (.0096)	.0038 (.01)
Q3 % Sole Props × POST		.0053 (.007)	.0052 (.0069)		.000016 (.0074)	-.00077 (.007)
Q4 % Sole Props × POST		-.0064 (.0056)	-.0089 (.0061)		-.0087 (.0083)	-.0086 (.0077)
Industry capital intensity × POST			.0066 (.0044)			-.006 (.0052)
Industry growth × POST			-.0086* (.005)			-.0011 (.0062)
Industry capital intensity × Trend			-.0029 (.002)			.0032** (.0015)
Industry growth × Trend			.0082* (.0043)			.0023 (.0021)
Constant	.26*** (.0043)	.21*** (.049)	.21*** (.05)	.17*** (.0028)	.048 (.034)	.048 (.033)
Treatment-specific trend	No	Yes	Yes	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,034,674	1,034,674	1,034,674	1,034,674	1,034,674	1,034,674
R-squared	.091	.091	.091	.037	.038	.038

1,034,674 new firms started during 1999–2005. All regressions are OLS. In columns (1) to (3) the dependent variable is a dummy equal to 1 if the firm has more employees two years after creation than when it is started. In column (1) the explanatory variable is the post dummy equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005. In column (2) we interact the post dummy with the quartiles of the treatment variable and we add treatment-specific time trends. In column (3) we also interact the post dummy and the time trend variable with pre-reform industry capital intensity and industry sales growth. In columns (4) to (6) the dependent variable is replaced by a dummy equal to 1 if the firm exits during the first two years. All regressions include month-of-the-year fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 7: Firm Quality: Ex Ante Measures

	Log(employment)			Employment \geq 1			Employment \geq 5		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High school	.052*** (.011)		.033*** (.01)	.031*** (.007)		.02*** (.0066)	.017*** (.0038)		.012*** (.0037)
College	.043** (.02)		.043** (.018)	.012 (.011)		.012 (.01)	.017** (.0071)		.017** (.0067)
Plan to hire		.29*** (.022)	.29*** (.022)		.17*** (.013)	.17*** (.013)		.079*** (.0072)	.078*** (.0074)
Constant	.29*** (.0053)	.25*** (.0053)	.22*** (.0067)	.23*** (.0035)	.21*** (.0032)	.2*** (.0046)	.042*** (.002)	.033*** (.0018)	.025*** (.0022)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,783	26,783	26,783	26,783	26,783	26,783	26,783	26,783	26,783
R-squared	.094	.13	.13	.099	.12	.12	.05	.069	.07

Panel A uses a random sample of about 1/3 of all firms started in the first semester of 2002. In columns (1) to (3) the dependent variable is the log of one plus the number of employees four years after creation. In column (1) the explanatory variables are a dummy equal to 1 if the entrepreneur has at least a high school degree and a dummy equal to 1 if the entrepreneur has at least a five-year college degree. In column (2) the explanatory variables is a dummy equal to 1 if the entrepreneur answers “yes” to the question “Do you plan to hire in the next twelve months?” In column (3) we include all explanatory variables. In columns (4) to (6) the dependent variable is a dummy equal to 1 if the firm has at least 1 employee four years after creation. In columns (7) to (9) the dependent variable is a dummy equal to 1 if the firm has at least 5 employees four years after creation. All regressions include industry fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 7 (continued)

Panel B: Education and ambition after the reform						
	High school		College		Plan to hire	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.03**	.026	-.0047	-.009	-.031**	-.026
	(.015)	(.035)	(.008)	(.023)	(.014)	(.025)
Q2 % Sole Props × POST	.0073	.000073	-.0094	-.014	-.00082	-.0035
	(.022)	(.022)	(.019)	(.02)	(.019)	(.019)
Q3 % Sole Props × POST	.033*	.031*	.0078	.0068	.029*	.028
	(.019)	(.018)	(.011)	(.011)	(.018)	(.017)
Q4 % Sole Props × POST	.012	.0052	.0047	.00076	.038**	.035**
	(.018)	(.017)	(.0092)	(.0097)	(.015)	(.016)
Industry capital intensity × POST		.0088		.0058		.00089
		(.014)		(.0092)		(.0084)
Industry growth × POST		-.023**		-.013**		-.012
		(.012)		(.0063)		(.01)
Constant	.5***	.5***	.14***	.14***	.25***	.25***
	(.0038)	(.0037)	(.0022)	(.0021)	(.0029)	(.0028)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,321	56,321	56,321	56,321	56,321	56,321
R-squared	.25	.25	.29	.29	.07	.07

Panel B uses a random sample of about 1/3 of all firms started in the first semesters of 2002 and 2006. All regressions are OLS. In columns (1) and (2) the dependent variable is the high school dummy degree. In column (1) the explanatory variables are the post dummy, its interactions with the quartiles of the treatment variable, and treatment-specific time trends. In column (2) we also interact the post dummy and the time trend variable with pre-reform industry capital intensity and industry sales growth. In columns (3) and (4) the dependent variable is the five-year college degree dummy. In columns (5) and (6) the dependent variable is a dummy equal to 1 if the entrepreneur answers “yes” to the question “Do you plan to hire in the next twelve months?”. All regressions include month-of-the-year fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 8: Job Creation

	Number of jobs created adding entrepreneurs' jobs		Number of jobs created	
	(1)	(2)	(3)	(4)
POST	-.23*** (.051)	-.48*** (.096)	-.23*** (.049)	-.53*** (.1)
Q2 % Sole Props × POST	.087 (.065)	.075 (.064)	.093 (.066)	.087 (.066)
Q3 % Sole Props × POST	.17*** (.059)	.18*** (.058)	.21*** (.06)	.22*** (.06)
Q4 % Sole Props × POST	.2*** (.059)	.21*** (.058)	.21*** (.061)	.22*** (.061)
Industry capital intensity × POST		.096*** (.033)		.1*** (.033)
Industry growth × POST		-.025 (.044)		.055 (.057)
Industry capital intensity × Trend		-.037*** (.012)		-.042*** (.013)
Industry growth × Trend		.079*** (.014)		.12*** (.018)
Constant	.85*** (.27)	.85*** (.25)	.4 (.3)	.4 (.27)
Treatment-specific trend	Yes	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360
R-squared	.84	.84	.76	.77

290 industries, 1999–2005, monthly. In columns (1) and (2) the dependent variable is the log of one plus the number of employees in new firms two years after creation plus the number of surviving firms after two years (to account for the entrepreneurs' jobs). In column (1) the explanatory variables are the post dummy equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005, and its interactions with the quartiles of the treatment variable. In column (2) we also interact the post dummy and the time trend variable with pre-reform industry capital intensity and industry sales growth. In columns (3) and (4) the dependent variable is replaced by the log of one plus the number of employees in new firms two years after creation. All regressions include industry and month-of-the-year fixed effects and treatment-specific time trends. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 9: Employment Growth per Category of Firm

	Small incumbents		Large incumbents		Small incumbents + New firms	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-.027**	-.032	-.045***	-.068*	-.025	-.22*
	(.011)	(.047)	(.015)	(.039)	(.026)	(.11)
Q2 % Sole Props × POST	-.021	-.022	.02	.015	-.017	-.028
	(.014)	(.013)	(.017)	(.017)	(.037)	(.033)
Q3 % Sole Props × POST	-.018	-.018	.022	.022	.012	.015
	(.012)	(.013)	(.019)	(.019)	(.028)	(.026)
Q4 % Sole Props × POST	-.026**	-.026**	.0064	.004	.015	.02
	(.012)	(.013)	(.018)	(.018)	(.028)	(.028)
Industry capital intensity × POST		.0028		.015		.077**
		(.016)		(.011)		(.037)
Industry growth × POST		-.005		-.038		-.032
		(.0093)		(.024)		(.032)
Industry capital intensity × Trend		-.0023		-.0059*		-.024***
		(.0032)		(.0036)		(.0091)
Industry growth × Trend		.0026		.0039		-.0095
		(.0027)		(.0044)		(.012)
Constant	-.12	-.12	-1.1	-1.1	-2.6	-2.6
	(1.8)	(1.8)	(2.7)	(2.7)	(6)	(5.7)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,320	2,320	2,320	2,320	2,320	2,320
R-squared	.47	.47	.19	.19	.89	.9

290 industries, 1999–2006, annual. In columns (1) and (2) the dependent variable is the growth rate of total employment in small incumbent firms (i.e., firms which have been in the tax files for the last four years, have 5 employees or less in year $t - 1$, and are not reported to be part of a group in either year $t - 1$ or year t). In column (1) the explanatory variables are the post dummy equal to 0 from 1999 to 2001 and equal to 1 from 2002 to 2006, and its interactions with the quartiles of the treatment variable. In column (2) we also interact the post dummy and the time trend variable with pre-reform industry capital intensity and industry sales growth. In columns (3) and (4) the dependent variable is the growth rate of total employment in large incumbent firms (i.e., firms which have been in the tax files for the last four years and are not small according to the above definition). In columns (5) and (6) the dependent variable is the growth rate of total employment in small incumbents and new firms started over the last two years (i.e., firms started in years $t - 2$, $t - 1$ and t). All regressions include industry fixed effects and treatment-specific time trends. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 10: Comparison New Firms vs. Shrinking Incumbents

	Wage		Value added per worker		Sales per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
New firm	5.2*** (.39)	5.7*** (1.6)	7*** (.37)	6.6*** (.78)	9.3*** (.51)	5.4*** (1.9)
New firm \times POST	.014 (.18)	.18 (.39)	.19 (.15)	.62 (.55)	.23 (.29)	1.8 (1.1)
Q2 % Sole Props \times New firm \times POST		-.41 (.54)		-.22 (.65)		-2.2 (1.3)
Q3 % Sole Props \times New firm \times POST		-.72 (.47)		-.94 (.63)		-2.3* (1.2)
Q4 % Sole Props \times New firm \times POST		.56 (.53)		-.25 (.6)		-1.4 (1.2)
Constant	22*** (.11)	22*** (.11)	26*** (.61)	26*** (.61)	43*** (.86)	43*** (.88)
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	265,586	265,586	1,269,812	1,269,812	1,258,595	1,258,595
R-squared	.16	.16	.12	.12	.2	.2

New firms and small “shrinking” incumbents (i.e., incumbents whose employment decreases between year t and year $t + 1$), 1999–2005. For new firms all dependent variables are computed two years after creation. In columns (1) and (2) the dependent variable is total wages divided by number of employees (requires that the firm has at least 1 employee). In column (1) the explanatory variables are a dummy equal to 1 if the firm is a firm and equal to 0 if the firm is a small shrinking incumbent, and the new firm dummy interacted with the post dummy equal to 0 from 1999 to 2001 and equal to 1 from 2002 to 2005. In column (2) we add the interaction term between the new firm dummy and the quartiles of the treatment variable, and the triple interaction term between the new firm dummy, the post dummy and the quartiles of the treatment variable. In columns (3) to (4) the dependent variable is value added divided by one plus number of employees. In columns (5) and (6) the dependent variable is sales divided by one plus number of employees. All regressions include industry \times year fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 11: Risk Aversion

	Number of firms created							
	Total volatility		Systematic volatility		Idiosyncratic volatility		Exit rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	-.29***	-.24***	-.25***	-.25***	-.25*	-.37*	-.26**	-.53***
	(.075)	(.08)	(.072)	(.071)	(.14)	(.19)	(.11)	(.16)
Q2 % Sole Props × POST	.028	.02	.03	.03	.027	.49*	.027	.46
	(.043)	(.071)	(.044)	(.046)	(.045)	(.26)	(.042)	(.29)
Q3 % Sole Props × POST	.11***	.11*	.11***	.11***	.11***	.32	.11***	.58***
	(.036)	(.061)	(.037)	(.039)	(.039)	(.21)	(.036)	(.18)
Q4 % Sole Props × POST	.14***	.028	.14***	.13***	.14***	.088	.14***	.43**
	(.039)	(.062)	(.039)	(.041)	(.042)	(.23)	(.039)	(.18)
Industry risk × POST	.34*	-.1	.056	.042	-.019	.39	.012	.75**
	(.2)	(.48)	(.043)	(.1)	(.3)	(.49)	(.18)	(.32)
Industry risk		.066		-.033		-1.5*		-1.2
× Q2 % Sole Props × POST		(.63)		(.12)		(.85)		(.79)
Industry risk		-.034		-.036		-.7		-1.2***
× Q3 % Sole Props × POST		(.54)		(.13)		(.64)		(.41)
Industry risk		1.3**		.12		.2		-.79*
× Q4 % Sole Props × POST		(.53)		(.12)		(.68)		(.44)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360	24,360	24,360	24,360	24,360
R-squared	.92	.92	.92	.92	.92	.92	.92	.92

290 industries, 1999–2005, monthly. The dependent variable is the log of one plus the number of new firms. In column (1) the explanatory variables are the post dummy which is equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005, the post dummy interacted with the quartiles of the treatment variable, and the post dummy interacted with industry risk measured as the sample standard deviation of annual industry sales growth (Total volatility). In column (2) we add the triple interaction between industry risk, the quartiles of the treatment variable and the post dummy. In columns (3) and (4) industry risk is measured as the regression coefficient of industry sales growth on aggregate sales growth (Systematic volatility). In columns (5) and (6) industry risk is measured as the average, across all years, of the cross-sectional standard deviation of firm sales growth in the industry (Idiosyncratic volatility). In columns (7) and (8) industry risk is measured as the average five-year exit rate of new firms (Exit rate). All regressions include industry and month-of-the-year fixed effects, and treatment-specific time trends. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

Table 12: Debt

	New firms in SINE survey			All new firms		New firms in tax files	
	Dummy firm's bank debt	Dummy entrepreneur's bank debt	Dummy entr.'s debt/dummy any debt	Bank debt/ Assets	Log of bank debt	Bank debt/ Assets	Log of bank debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	.058*	-.0014	.071	-.0038	-.082*	.0078	-.057
	(.032)	(.026)	(.053)	(.0071)	(.047)	(.0096)	(.062)
Q2 % Sole Props \times post	-.00038	.0062	-.0087	.0027	.00044	.0082	.032
	(.022)	(.014)	(.038)	(.0061)	(.038)	(.0085)	(.052)
Q3 % Sole Props \times post	.0064	-.0059	.035	.0032	.044	.0038	.064
	(.021)	(.0097)	(.034)	(.006)	(.037)	(.0086)	(.052)
Q4 % Sole Props \times post	-.0054	-.014	.018	-.0022	-.005	.0028	.042
	(.02)	(.011)	(.031)	(.0059)	(.035)	(.0081)	(.048)
Log(Employment)	.084***	.00039	.065***	.0065	.32***	-.016***	.28***
	(.011)	(.0069)	(.011)	(.0052)	(.051)	(.0034)	(.032)
Constant	.22***	.14***	.67***	.052**	.58***	.044*	.83***
	(.0046)	(.0027)	(.0064)	(.02)	(.15)	(.025)	(.16)
Treatment-specific trend	No	No	No	Yes	Yes	Yes	Yes
Month-of-the-year FE	No	No	No	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,483	56,483	21,723	1,360,846	1,360,846	734,298	734,298
R-squared	.074	.033	.051	.12	.17	.13	.16

In columns (1) to (3) data are from a random sample of about 1/3 of all firms started in the first semesters of 2002 and 2006. All regressions are OLS. In column (1) the dependent variable is a dummy equal to 1 if the firm has bank debt contracted by the firm. The explanatory variables are the post dummy which is equal to 0 from January 1999 to December 2001 and equal to 1 from January 2002 to December 2005, the post dummy interacted with the quartiles of the treatment variable, and the log of firm employment. In column (2) the dependent variable is a dummy equal to 1 if the firm has bank debt contracted by the entrepreneur. In column (3) the dependent variable is a dummy equal to 1 if the firm has bank debt contracted by the firm and we restrict the sample to firms which have bank debt contracted by either the firm or the entrepreneur. In columns (4) and (5) the sample contains all new firms started during 1999–2005 and we add month-of-the-year fixed effects and treatment-specific time trends; for firms which do not report to the tax files we assume bank debt is zero. In column (4) the dependent variable is firm-level bank debt divided by total assets. In column (5) the dependent variable is firm-level log of bank debt. In columns (6) and (7) the sample contains new firms started during 1999–2005 which are reported in the tax files. In column (6) the dependent variable is firm-level bank debt divided by total assets. In column (7) the dependent variable is firm-level log of bank debt. All regressions include industry fixed effects. Standard errors are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.

A Appendix Tables

Figure A.1: Growth Rate in Firm Creation: Treated vs. Control

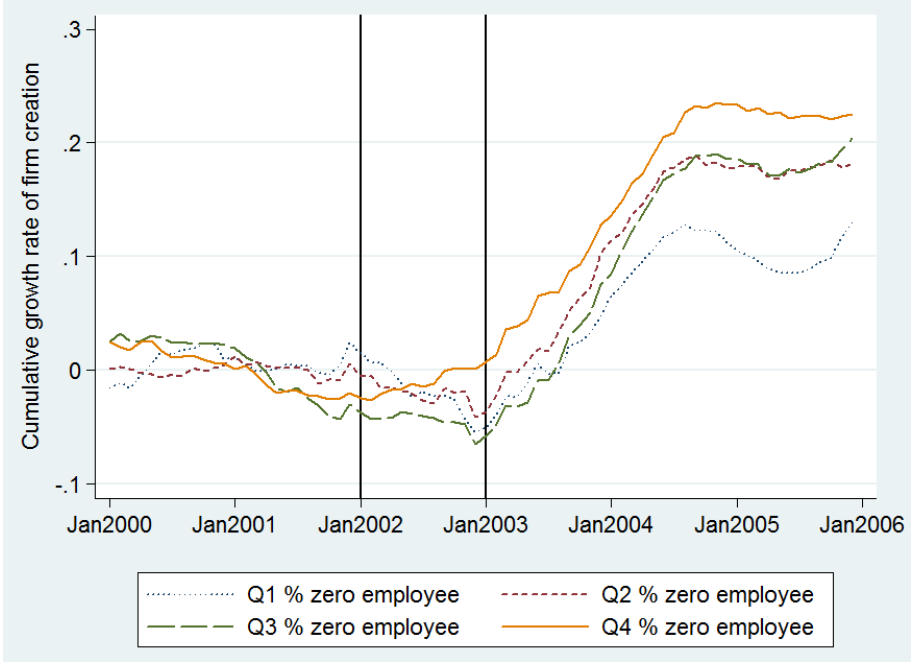


Figure 3 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employee in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Figure 3.

Table A.1: Summary Statistics

	N	Mean	SD	Mean by quartile of % of New zero-employee firms			
				Q1	Q2	Q3	Q4
Panel A: New firms, industry-level							
Avg # firms created (monthly)	290	43.62	84	12	35	59	69
Avg # jobs created after two years (monthly)	290	32.49	62	22	41	47	19
——— adding entrepreneurs' jobs (monthly)	290	69.30	123	33	71	95	77
Panel B: New firms, firm-level							
Employment at creation	381,683	0.49	1.9	1.18	0.82	0.47	0.19
Dummy at least 1 employee at creation	381,683	0.20	.4	0.38	0.31	0.20	0.09
Employment two years after creation	381,683	0.87	2.5	2.03	1.29	0.91	0.36
Dummy at least 1 employee two years after creation	381,683	0.29	.45	0.54	0.43	0.33	0.13
Hire during first two years	381,683	0.25	.43	0.46	0.37	0.29	0.12
Exit during first two years	381,683	0.16	.36	0.12	0.12	0.18	0.16
Panel C: New firms, survey, firm-level							
High school graduate	26,783	0.50		0.42	0.38	0.49	0.60
College graduate	26,783	0.14		0.06	0.10	0.13	0.18
Plan to hire	26,783	0.23		0.39	0.32	0.26	0.14
Panel D: Incumbents, industry-level							
# small incumbents	290	2,779	5,289	1,961	2,798	4,167	2,180
# jobs in small incumbents	290	3,647	7,667	3,752	4,189	4,891	1,739
# large incumbents	290	804	1,243	1,005	891	1,010	305
# jobs in large incumbents	290	21,967	38,740	33,540	21,739	24,991	7,396

Table 2 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 2.

Table A.2: Aggregate Growth Rate: Treated vs. Control

	Sales			Value added		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.082*** (.013)	.081*** (.014)	-.025** (.011)	.1*** (.013)	.092*** (.014)	-.013 (.011)
Q2 % zero employees \times POST		-.011 (.03)	.013 (.019)		-.0034 (.032)	.024 (.021)
Q3 % zero employees \times POST		-.031 (.025)	.012 (.021)		-.025 (.025)	.007 (.017)
Q4 % zero employees \times POST		.049 (.04)	.021 (.018)		.07* (.04)	.02 (.018)
Constant	15*** (.0074)	15*** (.0073)	-40*** (8.2)	14*** (.0077)	14*** (.0076)	-44*** (9.2)
Treatment-specific trend	No	No	Yes	No	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,030	2,030	2,030	2,030	2,030	2,030
R-squared	.99	.99	.99	.98	.98	.98

Table 3 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 3.

Table A.3: Firm Creation: Treated vs. Control

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.1*** (.014)	.059** (.023)	-.13*** (.028)	-.2*** (.074)
Q2 % zero employees × POST		.046 (.038)	.045 (.035)	.046 (.035)
Q3 % zero employees × POST		.041 (.036)	.024 (.038)	.021 (.038)
Q4 % zero employees × POST		.088** (.04)	.1*** (.04)	.11*** (.039)
Industry capital intensity × POST				.033 (.024)
Industry growth × POST				-.051 (.037)
Industry capital intensity × Trend				-.013 (.0083)
Industry growth × Trend				.056*** (.017)
Constant	3.2*** (.017)	3.2*** (.018)	.98*** (.23)	.98*** (.23)
Treatment-specific trend	No	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360
R-squared	.92	.92	.92	.92

Table 4 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 4.

Table A.4: Job Creation Through New Firms

	Number of firms created							
	0 employees at creation		≥ 1 employee at creation		0 employees after 2 years		≥ 1 employee after 2 years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	-.13*** (.026)	-.24*** (.074)	-.1*** (.025)	-.097 (.063)	-.1*** (.026)	-.18** (.072)	-.13*** (.024)	-.21*** (.061)
Q2 % zero employees \times POST	.034 (.035)	.037 (.035)	.076** (.033)	.075** (.033)	.018 (.035)	.021 (.035)	.067* (.035)	.07** (.035)
Q3 % zero employees \times POST	.029 (.037)	.028 (.037)	.03 (.033)	.027 (.033)	.0074 (.037)	.0052 (.037)	.032 (.036)	.032 (.036)
Q4 % zero employees \times POST	.11*** (.04)	.12*** (.039)	.079** (.034)	.083** (.034)	.087** (.039)	.092** (.038)	.1*** (.035)	.11*** (.035)
Industry capital intensity \times POST		.046* (.024)		.0037 (.02)		.034 (.023)		.031 (.02)
Industry growth \times POST		-.033 (.036)		-.036 (.026)		-.04 (.039)		-.015 (.038)
Industry capital intensity \times Trend		-.017** (.0084)		.0025 (.0068)		-.0076 (.0088)		-.023*** (.0067)
Industry growth \times Trend		.05*** (.017)		.044*** (.011)		.035* (.019)		.1*** (.016)
Constant	.072 (.24)	.072 (.23)	2.1*** (.18)	2.1*** (.18)	.66*** (.24)	.66*** (.24)	.092 (.23)	.092 (.2)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360	24,360	24,360	24,360	24,360
R-squared	.91	.91	.84	.84	.91	.91	.86	.86

Table 5 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 5.

Table A.5: Firm Quality: Ex Post Measures

	Hire			Exit		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.01*** (.0038)	.0033 (.0061)	-.013 (.014)	.011*** (.0017)	.019*** (.0051)	.037** (.017)
Q2 % zero employees × POST		.0084 (.0071)	.011 (.0072)		-.0047 (.0068)	-.0072 (.0072)
Q3 % zero employees × POST		.0088 (.0075)	.013* (.0069)		-.016*** (.0061)	-.02*** (.0068)
Q4 % zero employees × POST		-.008 (.0067)	-.0051 (.0069)		-.034*** (.0072)	-.037*** (.0072)
Industry capital intensity × POST			.0069 (.0044)			-.0072 (.006)
Industry growth × POST			-.0058 (.0046)			.0038 (.0048)
Industry capital intensity × Trend			-.0035* (.0019)			.0036** (.0018)
Industry growth × Trend			.0073* (.004)			.00053 (.0019)
Constant	.26*** (.0043)	.21*** (.048)	.21*** (.048)	.17*** (.0028)	.049* (.029)	.049* (.028)
Treatment-specific trend	No	Yes	Yes	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,034,674	1,034,674	1,034,674	1,034,674	1,034,674	1,034,674
R-squared	.091	.091	.092	.037	.038	.038

Table 6 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 6.

Table A.6: Firm Quality: Ex Ante Measures

Panel B: Education and ambition after the reform						
	High school		College		Plan to hire	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.048*	.039	.0073	.0043	-.04***	-.042
	(.029)	(.048)	(.0079)	(.027)	(.015)	(.028)
Q2 % zero employees \times POST	-.021	-.019	-.025**	-.024**	.025	.026
	(.031)	(.031)	(.011)	(.011)	(.018)	(.018)
Q3 % zero employees \times POST	.01	.016	-.006	-.0025	.036**	.04**
	(.03)	(.03)	(.0094)	(.01)	(.017)	(.017)
Q4 % zero employees \times POST	.00012	.0049	-.00077	.0019	.052***	.055***
	(.031)	(.03)	(.011)	(.01)	(.016)	(.017)
Industry capital intensity \times POST		.0075		.0036		.0035
		(.015)		(.0091)		(.0084)
Industry growth \times POST		-.025**		-.015**		-.018*
		(.011)		(.0062)		(.01)
Constant	.5***	.5***	.14***	.14***	.25***	.25***
	(.0036)	(.0036)	(.0022)	(.0021)	(.0029)	(.0028)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,321	56,321	56,321	56,321	56,321	56,321
R-squared	.25	.25	.29	.3	.07	.07

Table 7 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 7.

Table A.7: Job Creation

	Number of jobs created adding entrepreneurs' jobs		Number of jobs created	
	(1)	(2)	(3)	(4)
POST	-.17*** (.046)	-.39*** (.099)	-.16*** (.048)	-.42*** (.1)
Q2 % zero employees \times POST	.058 (.057)	.067 (.058)	.071 (.062)	.082 (.062)
Q3 % zero employees \times POST	.041 (.059)	.041 (.057)	.034 (.064)	.041 (.062)
Q4 % zero employees \times POST	.12** (.059)	.12** (.057)	.12** (.063)	.12* (.062)
Industry capital intensity \times POST		.085** (.033)		.09*** (.035)
Industry growth \times POST		-.025 (.044)		.056 (.057)
Industry capital intensity \times Trend		-.037*** (.012)		-.043*** (.013)
Industry growth \times Trend		.078*** (.014)		.12*** (.019)
Constant	.85*** (.27)	.85*** (.25)	.4 (.3)	.4 (.27)
Treatment-specific trend	Yes	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	24,360	24,360	24,360	24,360
R-squared	.84	.84	.76	.77

Table 8 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 8.

Table A.8: Employment Growth per Category of Firm

	Small incumbents		Large incumbents		Small incumbents + New firms	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-.038*** (.01)	-.045 (.047)	-.041*** (.0088)	-.068* (.037)	-.042* (.022)	-.23** (.11)
Q2 % zero employees × POST	-.0061 (.012)	-.006 (.012)	.0087 (.014)	.0085 (.014)	.039 (.026)	.045* (.025)
Q3 % zero employees × POST	-.014 (.013)	-.014 (.013)	.024 (.015)	.021 (.016)	-.0035 (.033)	-.0047 (.03)
Q4 % zero employees × POST	-.0014 (.012)	-.0012 (.013)	-.0005 (.016)	.0015 (.015)	.043* (.026)	.046* (.025)
Industry capital intensity × POST		.0032 (.016)		.016 (.011)		.074** (.036)
Industry growth × POST		-.0041 (.01)		-.036 (.025)		-.035 (.031)
Industry capital intensity × Trend		-.0025 (.0032)		-.0058* (.0034)		-.022** (.009)
Industry growth × Trend		.0027 (.0029)		.0039 (.0045)		-.0071 (.012)
Constant	-.12 (1.8)	-.12 (1.8)	-1.1 (2.7)	-1.1 (2.7)	-2.6 (5.9)	-2.6 (5.7)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,320	2,320	2,320	2,320	2,320	2,320
R-squared	.47	.47	.19	.19	.89	.89

Table 9 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 9.

Table A.9: Comparison New Firms vs. Shrinking Incumbents

	Wage		Value added per worker		Sales per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
New firm	5.2*** (.39)	4*** (.31)	7*** (.37)	6.6*** (1)	9.3*** (.51)	9.2*** (1.1)
New firm \times POST	.014 (.18)	.67 (.53)	.19 (.15)	.79* (.45)	.23 (.29)	1.1 (.69)
Q2 % zero employees \times New firm \times POST		-.79 (.61)		-.75 (.5)		-1 (.77)
Q3 % zero employees \times New firm \times POST		-1.1* (.58)		-1* (.53)		-1.4 (.89)
Q4 % zero employees \times New firm \times POST		-.0032 (.7)		.1 (.58)		-.13 (.93)
Constant	22*** (.11)	22*** (.12)	26*** (.61)	26*** (.61)	43*** (.86)	43*** (.86)
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	265,586	265,586	1,269,812	1,269,812	1,258,595	1,258,595
R-squared	.16	.16	.12	.12	.2	.2

Table 10 with the alternative treatment variable. To construct the alternative treatment variable, we first compute the fraction of firms with zero employees in each industry in the pre period 1999–2001. We then split industries into four quartiles of this variable. The rest of the methodology is identical to Table 10.