Can Unemployment Insurance Spur Entrepreneurial Activity? Evidence From France*

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Abstract

We study a large-scale French reform that provided generous downside insurance for unemployed individuals starting a business. We study whether this reform 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. In aggregate, the effect of the reform on employment is largely offset by large crowd-out effects. However, because new firms are more productive, the reform has the impact of raising aggregate productivity. These results suggest that the dispersion of entrepreneurial abilities is small in the data, so that the facilitation of entry leads to sizable Schumpeterian dynamics at the firm-level.

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"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 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 et al. (2013)). Beyond ability, entrepreneurs have different degrees of risk tolerance, ambition, or optimism (Hurst and Pugsley (2011), Landier and Thesmar (2009), or Holtz-Eakin et al. (1994a)). The welfare implications of barriers to entrepreneurship thus crucially depend on *how* individuals select into this activity. For instance, policies that draw less qualified individuals into entrepreneurship may deteriorate allocative efficiency, as scarce resources are diverted to less productive firms.

Beyond ability, an important dimension of selection into entrepreneurship is risk-aversion, since entrepreneurs have to bear significant idiosyncratic and possibly fundamental risk (Kihlstrom and Laffont (1979)). The academic literature has been struggling to evaluate conclusively whether this inherent risk associated with entrepreneurship and self-employment is an undesirable constraint or a necessary selection criterion. When designing interventions that defray some of the downside risk of entrepreneurship, policy-makers face an inherent tension. On the one hand, entrepreneurial risk might dissuade many able individuals from starting a business, if these individuals are risk averse and can only learn about their ability as entrepreneurs by starting a firm (see Jovanovic (1982) or Caves (1998)). As a result, many promising businesses might not get set up. In such a world, providing a form of downside insurance to would-be entrepreneurs could lead to increased efficient entry into self-employment. On the other hand, this downside insurance could distort the pool of entrepreneurs who start businesses. If people have ex ante private information about their entrepreneurial abilities, being forced to bear downside risk might serve as an effective way

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

to screen out entrepreneurs who have low expectations about the success of their venture.

To investigate this trade-off, we first develop a general equilibrium model of selection into self-employment that features risk-averse individuals and heterogeneity in the distribution of talent. We show that the effect of downside insurance provision on the pool of entrepreneurs depends on the size of the wedge that is created by risk aversion and the heterogeneity of talents. If talents are relatively heterogeneous, the "self-selection" effect dominates: an intervention that lowers downside risk draws in low-quality entrepreneurs. In contrast, if the talent distribution is more homogeneous, such an intervention draws in entrepreneurs of similar ex ante quality and thus allows risk averse individuals to learn about their success as entrepreneurs. We refer to this as the "experimentation channel".[2]

We then evaluate the above trade-off in the context of a large-scale reform implemented in France in 2002, which aimed at facilitating (small) business creation for unemployed workers, called PARE—*Plan d'Aide au Retour à l'Emploi*. The reform provided downside insurance to unemployed individuals starting businesses, mostly by allowing them to retain their rights to unemployment benefits for three years in case of failure of their venture, which previously they would lose by becoming entrepreneurs. Additionally, the reform gave "unemployed entrepreneurs" the possibility to fill any gap between their entrepreneurial revenues and their unemployment benefits by using their accrued unemployment benefits, providing insurance against cash flow shortfalls in the first three years.

As soon as the reform is implemented, monthly firm creation immediately increases by 25%. This is strongly suggestive of the large impact of the reform. To filter out the effect of other macroeconomic shocks, we employ 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 provided downside insurance for unemployed workers, it should have mostly affected industries that are most likely to attract small-scale entrepreneurs. Therefore, our treatment industries should have a larger exposure to the reform than our control industries. 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 5, 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 indus-

[2]One can also extend this model to allow for heterogeneous talent *and* heterogeneity in risk aversion. The net effect of greater downside insurance then depends on the sign of the correlation between talent and risk aversion. In the limit, if very talented individuals are the most (resp. least) risk averse, providing downside insurance leads to an increase (resp. a decrease) in average entrepreneurial talent.

tries: Post-reform entry growth is larger by more than 12 percentage points in industries where small firms are prevalent at creation.

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 seems 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, the evidence is more consistent with the "experimentation view", whereby providing downside insurance allowed unemployed workers to enter into self-employment without significantly lowering the average quality of the pool. This suggests that the ex ante selection into self-employment was not based on steep differences in the talent distribution of would-be entrepreneurs.

The final part of the paper focuses on the equilibrium effects of the reform and looks at potential spillovers on incumbent firms in 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 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 competition from increased entrepreneurial activity is stronger for small incumbents than for large ones, at least in the short-run. The crowding-out effects on small incumbents are economically large. According to our estimates, they offset most of the direct effects of the reform on employment creation by start-ups. These results bear some similarity to the literature on financial reforms, which also documents that increased entry is detrimental to incumbent firms (Cetorelli and Strahan (2006), Bertrand et al. (2007), Kerr and Nanda (2009a)).

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 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 than the firms

they displace.

In the final part of the paper we offer an assessment of the aggregate cost-benefit analysis of these reforms. We calculate that the reform had a positive impact on the French economy in the order of magnitude of about €350 million per year. The analysis weighs the benefits of the reform due to shorter unemployment spells and labor reallocation to more productive and higher-paying jobs against the costs of subsidizing the move of marginal and infra-marginal unemployed into self-employment. We also find that the cost to the unemployment agency is about €100 million per year.

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. (2014), 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), Holtz-Eakin et al. (1994b), Hurst and Lusardi (2004), de Mel et al. (2008), Kerr and Nanda (2009b), Adelino et al. (2013), Schmalz et al. (2013)). 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), Blanchflower and Oswald (1998), Hamilton (2000), Moskowitz and Vissing-

Jørgensen (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 does not worsen following a reduction in entry costs as individuals are able to experiment with setting-up a firm and learn about their type.[3]

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 unemployment benefits are reduced, which is precisely the experiment we are looking at, we observe a large and significant increase in the supply of self-employment.

The rest of the paper is organized as follows. We present the reform in Section 2, a simple economic framework in Section 3, the data in Section 4, the empirical strategy in Section 5, the results on the direct effect of the reform on the number and quality of new firms in Section 6, the aggregate impact effect of the reform on employment and productivity in Section 7. Section 8 is an attempt to provide a cost-benefit analysis.

2 The Reform and Institutional Details

2.1 Describing the Reform

We focus on a reform passed by the French Ministry of Labor in 2001 that aimed at reducing 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 à l'Emploi*), which established new rules on unemployment benefits. The overall goal was to provide more generous benefits for unemployed workers

[3]By emphasizing the role of experimentation for entrepreneurs, our paper is also related to Manso (2011), who shows that the combination of tolerance for early failure and reward for long-term success is optimal to motivate innovation when entrepreneurs need to experiment.

who engage in an active employment search.[4] An important part of this reform included the provision of insurance to unemployed workers starting 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 its accumulated unemployment benefits when starting a business, even if the business subsequently failed. The new agreement allowed unemployed individuals starting a firm to retain their rights to the remaining unemployment benefits for up to three years.[5] Second, the reform also stipulated that unemployed could keep their unemployment benefits *while* starting their own firm (Rieg (2004)) if 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.[6] Therefore, unemployed workers who decided to start a business were guaranteed to receive at least their unemployment benefits for at least two and up to three years.

INSERT FIGURE 1 ABOUT HERE

The unemployment agency began massively advertising the reform to unemployed individuals 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 monthly data on the take-up of the ACCRE program, a subsidy allocated to unemployed entrepreneurs only.[7] Figure 1 shows the monthly number of new firms receiving the ACCRE subsidy, which is therefore a lower bound of the number unemployed entrepreneurs. As is clear from Figure 1, the number of new firms created by unemployed entrepreneurs increases sharply between 2002 and 2005. We discuss the aggregate magnitude in Section 4.

[4] In 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.

[5] Article 1-5 of the PARE agreement.

[6] Each 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.

[7] This program significantly reduces payroll taxes paid by entrepreneurs up to three years after starting their firm.

2.2 External Validity

Since the reform we analyze takes place in France, a valid concern is that certain characteristics of the French labor market might explain the effect the reform had on entry and average firm quality. This section provides a comparison of the relevant aspects of the French labor market with other OECD countries' labor markets.

First, our results on the relatively high quality of new entrepreneurs may in part be driven by the fact that France has a particularly large pool of highly-skilled unemployed. France is a high unemployment economy, with an unemployment rate of 8.3% in 2002 vs. 7.3% on average in the OECD. Long-term unemployment is more prevalent than in Anglo-Saxon countries.[8] This points toward France having, if anything, a lower-skilled pool of individuals in unemployment. So we would expect the reform to draw in relatively less-skilled individuals.

Second, the reform could have a large effect on entry because large unemployment benefits created, prior to the reform, a strong disincentive to start a company. The Net Replacement Rate computed by OECD for the average wage in France is 62%, compared to an OECD average of 56%. It is thus the case that the French unemployment insurance system is slightly more generous than the typical developed economy, but the difference is marginal.

Third, the strength of the response to the reform may be related to the fact that France had too few entrepreneurs to start with. Using World Bank data from 2004, we see that the French firm creation rate (2.8 new corporations per 1,000 inhabitants) is slightly above the Eurozone average, which is at 2.6, and slightly below the OECD median of 3.3. However, the firm creation rate in France in the 2004 World Bank data shows a very large gap with the rate observed in Anglo-Saxon countries (e.g., 9.8 for the UK). Clearly, continental Europe faces stronger barriers to entry than Anglo-Saxon countries, so that reforms like the one we analyze in the paper may have a weaker effect on firm creation in the latter countries.

3 Economic Framework

This section lays out the theoretical framework that will guide our empirical strategy. We first describe the model, and then use it to derive predictions. The key insight is that these predictions allow to indirectly measure the degree of heterogeneity in entrepreneurial ability.

[8] About 32% of the unemployed in France have been unemployed for less than 3 months vs. more than 50% in the US or Canada, and 45% for the UK.

3.1 The Model

We start from the model of entrepreneurship in equilibrium in Lucas (1978). We only make two modifications. First, we make entrepreneurship risky, and introduce some level of government insurance. Second, we introduce two separate sectors, which differ by their scale of production, in order to fit our difference-in-difference empirical strategy.

There are two industries, T (Treatment) and C (Control), which produce differentiated goods. Let x_s be the consumption of the good produced in industry $s \in \{T, C\}$. All agents maximize a CES utility function $U(x_T, x_C) = \log\left(\left(x_T^{\frac{\sigma-1}{\sigma}} + x_C^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}\right)$, where $\sigma > 0$. Let p_s be the price of each good s and y be the income of an agent, its indirect utility is given by:

$$U(y, p_T, p_C) = \log(y) + \frac{1}{\sigma - 1}\log\left(p_T^{1-\sigma} + p_C^{1-\sigma}\right).$$

The model has two periods. First, agents choose between starting a firm or supplying labor. Second, production takes place, entrepreneurs in each industry receive profits and workers salaries—which we normalize without loss of generality to 1. All agents consume.

All agents in the economy are potential entrepreneurs. There is a measure 1 of potential entrepreneurs tied to each industry s. Industry knowledge is crucial for entrepreneurs, but irrelevant for workers. An agent tied to s can work in any industry, but only start a firm in s. Starting a firm is risky: When an individual decides to become an entrepreneur, he first needs to find out whether there is a market for his idea. If there is no such market (with probability $1-q$), it is too late to become a worker and he gets b. b is a government subsidy given to failed entrepreneurs. This subsidy is financed through a proportional income tax, which creates no distortion since we have assumed log utility.

With probability q, the business lives but the profit depends on ability. The entrepreneur then hires l workers and produces $g(\theta)A^{1-\beta}l^{\beta}$, where A is an aggregate productivity parameter, θ is entrepreneurial ability, and $\beta \in (0, 1)$. We posit $g(\theta) \equiv \frac{\theta^{1-\beta}}{(1-\beta)^{1-\beta}\beta^{\beta}}$ to simplify expressions. In each industry, entrepreneurial abilities are distributed according to a Pareto distribution of c.d.f. $F(\theta) = 1 - (\theta_0/\theta)^{\phi}$, $\phi \geq 1$. Total costs consist of the wage bill l and a fixed cost c_s that depends on the industry. Industry T has a lower scale of production, i.e., a lower fixed cost: $c_T < c_C$. Entrepreneurial profit is thus given by $\pi_s(\theta, l, p_s) = p_s A^{1-\beta} g(\theta) l^{\beta} - l - c_s$.

3.2 Predictions

Similarly to Lucas (1978), the equilibrium is characterized by an ability cutoff θ_s in each industry above which all agents become entrepreneurs and below which all agents become workers.

We then model the reform as an increase in the downside protection for failed entrepreneurs

b. We look at how this impacts entry, firm quality, and incumbent size. These quantities react differently in sectors T and C, and the model leads to closed-form solutions for all three differential reactions. We solve the model in Appendix A and gather the results in proposition 1.

Proposition 1. *Assume the reform leads to a marginal increase in b by Δb. Then:*

1. *The differential increase in the number of firms $N_s = 1 - F(\theta_s)$ is given by:*

$$\Delta \log(N_T) - \Delta \log(N_C) = E(\phi)$$

2. *The differential increase in average quality of firms $q_s = E(\log(\theta)|\theta \geq \theta_s)$ is given by:*

$$\Delta q_T - \Delta q_C = -Q(\phi)$$

3. *The average size of "incumbents" firms $\log(L_s) = E(\log(l(\theta))|\theta > \theta_s)$ is given by:*

$$\Delta \log(L_T) - \Delta \log(L_C) = -S(\phi)$$

where E is positive and increasing. Q is positive, decreasing and tends to 0 when $\phi \to \infty$. S is positive, increasing and $S(1) = 0$. Neither E, Q nor S depend on aggregate productivity A.

The first output of the model is the difference-in-difference specification. Proposition 1 shows that comparing outcomes between T and S allows to filter out the (unobserved) aggregate productivity shock A. It also shows the intuitive result that the reform has a stronger impact on the low-scale industry T. To see why, notice first that the minimum ability to start a business is lower in industry T, since the fixed cost is lower. A key assumption is that the distribution of abilities has a decreasing hazard rate $F'(\theta)/(1 - F(\theta))$ (as is the case with Pareto). Thus, in industry T, the number of "marginal entrepreneurs" right below the threshold is larger, and the reform brings in a heavier mass of entrepreneurs into that industry. This induces more entry and more crowding-out in sector T. This also induces a bigger deterioration of quality.

Second, the effect of the reform depends on the degree of entrepreneurial skill homogeneity. When the shape parameter ϕ is close to 1, entrepreneurial skills are very heterogeneous. This setting would be consistent with the "self-selection view" described in introduction, whereby entry is determined by large differences in talents. In this case, Proposition 1 establishes that the reform only has a small (positive) effect on entry. When b goes up, the ability threshold above which agents become entrepreneurs goes down: There is more entry, but the effect is small because agents are more "spread out" on the ability spectrum. Average quality does, however, respond a lot, since the new entrepreneurs are much worse than the inframarginal ones. Because entry

is limited, however, there is very little crowding-out of incumbents (in the limit, not at all since $S(1) = 0$).

The picture changes drastically when entrepreneurial skills are very homogeneous, a setting more consistent with the "experimentation view" discussed in introduction, whereby entrepreneurs are similar ex ante, and entry is mostly determined by business risk. When the shape parameter ϕ is large, the model predicts a big positive effect on entry, strong crowding-out, and a small deterioration in firm quality. Intuitively, an increase in b reduces the ability threshold by the same amount as before, but since the c.d.f. is a steeper function of ability, the amount of entry is much larger. As a result, new entrants exert a stronger competitive pressure on incumbent firms, which in turn shrink more. Since skills are more homogeneous however, the quality of new entrants is not much lower than the quality of infra-marginal entrepreneurs. Our empirical findings are more consistent with this second case.

4 Data

We use three sources of data, which we obtain from the French Statistical Office (INSEE): the firm registry, accounting data on firm performance and employment, and a survey that is conducted every four years on a sixth of all French entrepreneurs who start in that year.

4.1 Registry

The firm registry contains the universe of firms that are registered 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 4-digit classification system similar to the 4-digit NAICS. It also provides the firm's legal status (Sole Proprietorship, Limited Liability Corporation, or Corporation). 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 the number of monthly creations over different categories of firms and sample periods. Panel A looks at monthly firm creation for all types of firms between 1993 and 2008. It shows that, starting in 2003, the number of firms created each month increases from 14,000 in early 2003 to about 18,000 at the end of 2004. This 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 self-employment (for individuals billing less than €30,000 per year). The study of these reforms is beyond the scope of this paper. To avoid any contamination in the post-period, 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 (red, solid line). It shows that, in the aggregate, the reform is accompanied by a surge in the creation of firms that are started very small and remain so after two years. Panel D presents, for the 1999–2005 period, 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). As we see in Panel D, 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 is thus to increase the creation of firms created with no employees, as we see in Panel C. However, as we again see in Panel D, the reform is also followed by a large increase in the creation of firms that will have at least one employee two years after creation—from about 4,000 to 5,000 monthly creations. Taken together, Panels C and D suggest that, while the reform fostered the creation of firms with zero employees at creation, these firms grow and eventually hire employees after two years.

Consistent with the idea that the increase in entrepreneurial activity observed in Figure 2 is in fact triggered by the reform, we find that the dramatic surge in firm creation mostly consists of unemployed entrepreneurs targeted by the reform. 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.[9] 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). Table 1 shows that both pre- and post-reform, newly-created firms are mostly in Services, Construction, and Retail Trade. These three industries constitute about 70% of all firm creations in the pre-reform years. Table 1 also shows that the industries where the growth in the number of newly-created firms following the

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

reform is the largest are Services, Retail Trade, Construction and FIRE, which could be characterized as labor-intensive, low fixed-cost industries.[10]

INSERT TABLE 2 ABOUT HERE

Table 2, Panel A aggregates creation data at the 4-digit industry level (290 industries), and then averages the monthly number of newly-created firms across all months from January 1, 1999 to December 31, 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.

4.2 Accounting Data

To analyze the long-term performance of new ventures, we complement the registry data with accounting information from tax files (see Bertrand et al. (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 (*Bénéfice Réel Normal*) or to the simplified corporate tax regime (*Régime Simplifié d'Imposition*), which together represent 55% of newly created firms during our sample period. Small firms with annual sales below €32,600 (€81,500 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 from the tax files. 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. 25% of the new firms in the pre-reform sample hire in the first two years in the sense that the number of employees they report after two years is strictly larger than the number of employees at creation. 16% of the firms in the pre-reform sample exit the sample before the end of the second fiscal year.

4.3 SINE Survey

To obtain 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

[10] A finer exploration of the data shows that, within the FIRE industry, most of the increase in the number of newly-created firms occurs for real estate agencies.

Office every four years called the SINE survey (see Landier and Thesmar (2009) for an extensive description of this survey). The SINE survey is a detailed questionnaire sent out to individuals registering new firms, which contains questions about both the entrepreneur as well as the firm she creates.[11] We only have two cross-sections of the survey 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, with a response rate typically around 85%. 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 characteristics of entrepreneurs and their firm. We construct two dummy variables related to the entrepreneur's education—a dummy variable for high school graduate and a dummy variable for college graduate. The survey also contains information on entrepreneurial ambition, which we measure by the answer to the survey question, "Do you plan to hire in the next twelve months?". Table 2, Panel C reports descriptive statistics on these three variables. 50% of the entrepreneurs surveyed in SINE 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. Finally, we construct two more variables in order to check that entrepreneurs are not just past employees re-hired as contractors. The first variable is a dummy equal to 1 if the entrepreneur declares to be "a supplier or client of his former employer". The second variable is a dummy equal to 1 if the entrepreneur responds that her firm "has at most 2 different customers".

5 Empirical Strategy

5.1 Identifying Assumption

We seek to evaluate the reform's impact on various firm- and industry-level outcomes, 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 shock to macroeconomic fundamentals that could have affected these outcomes. To this effect, we use a standard difference-in-difference analysis with differing treatment intensity.

In the spirit of our model, we use "small scale" industries as the treatment group. Concretely, the intensity of treatment is measured, in the pre-reform period, as the fraction of sole proprietorships among newly created firms at the industry level. In the model, the effect of the reform is

[11] The survey uses stratified sampling, where the strata are the headquarter's region and the 2-digit industry of the firm.

expected to be stronger in industries where starting a business is comparatively less costly. In practice, the reform was aimed at unemployed individuals with limited capital, and thus, much more likely to start as a sole proprietorship (we also use size at creation in alternative specifications—see below). The 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 previously employed entrepreneurs make this choice.[12] In our empirical specification, we split industries into quartiles of our treatment intensity variable. Our treatment (resp. control) group is thus composed of industries in the top (resp. bottom) quartile of the distribution of the industry-level fraction of sole proprietorships among newly created firms.[13] Appendix Table B.1 reports the name of the industries that belong to the least treated industries and the most treated ones. Unsurprisingly, in the most treated group, we find industries such as street vendors (which operate on farmers' markets), taxi drivers, healthcare specialists, and personal services. In the least treated group, we find real estate developers and operators, movie and TV producers, wholesale trades, supermarkets, and publishers. The identifying assumption is that absent the reform, these most and least treated industries would have experienced similar evolutions in entry rates and other outcomes of interest. In particular, this assumes that the industry characteristic used in defining treatment intensity (i.e., the 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 5.3.

For robustness, we also repeat our analysis using an alternative definition of treatment intensity: 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. Tables B.2-B.10, in Appendix B, report regression results using this alternative treatment definition and show that our results hold.

In Table 2, we split our pre-reform sample into four quartiles of treatment intensity (i.e., the fraction of sole proprietorships among newly created firms) and present summary statistics for firms/industries in each of these quartiles. In industries where newly created firms are predom-

[12]The 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.

[13]An alternative treatment intensity 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 in the pre-period. 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 therefore believe that the treatment variable is estimated more precisely this way.

inantly sole proprietors, the average firm is smaller at creation and is less likely to hire at least one employee in the two years following creation. Comparing firms in the fourth and first quartile of treatment intensity, we see that firms in the most treated industries have 0.54 less employees at creation and are 11 percentage points less likely to hire at least one employee in their first two years relative to firms in the least treated industries. Entrepreneurs in the most treated industries are also, on average, less educated (7 percentage point less likely to have a high school degree than firms in the least treated industries) and less ambitious (16 percentage point less likely to declare a plan to hire an employee in the next twelve months).

As an illustration of our empirical strategy, we report, in Appendix Table B.11, the top 20 4-digit industries in terms of their contribution to the post-reform surge in new firm creation, as well as the quartile of treatment intensity these industries belong to. More precisely, we simply compute for each industry s over the 2002-2005 period, $\frac{\Delta N_s}{\Delta N}$ where ΔN_s is the increase in the average monthly number of creations and $\Delta N = \sum_s \Delta N_s$. As can be seen in Appendix Table B.11, the increase in new firm creation is concentrated and mostly occurs in the most treated industries: (1) the top 20 4-digit industries contribute to more than half of the aggregate surge in new firm creations; (2) out of these 20 industries, 13 belong to the fourth quartile (Q4) of treatment, and 18 belong to either Q4 or Q3.

5.2 Empirical Specification

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

$$Y_{st} = \sum_{k=1}^{4} \alpha_k \cdot Q_s^k \times \text{post}_t + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times t + \mu_s + \text{MONTH}_t + \epsilon_{st}, \tag{1}$$

For firm-level outcomes (e.g., the probability that a firm exits in the two years following creation), our main specification is as follows:

$$Y_{ist} = \sum_{k=1}^{4} \alpha_k \cdot Q_s^k \times \text{post}_t + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times t + \mu_s + \text{MONTH}_t + \epsilon_{ist}, \tag{2}$$

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

For specifications using dependent variables from the SINE survey, where only two cross-

sections of data are available in 2002 and 2006, our main specification becomes:

$$Y_{ist} = \sum_{k=1}^{4} \alpha_k \cdot Q_s^k \times \text{post}_t + \mu_s + \epsilon_{ist}, \tag{3}$$

where the post dummy is equal to 1 for outcomes measured in the 2006 wave of the SINE survey and 0 when measured in the 2002 wave.

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 from January 1, 1999 to December 31, 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 intensity variable (i.e., the fraction of sole proprietorships among newly created firms by industry). We then plot a 12-month backward moving-average of this average growth rate for the four quartiles.

Figure 3 first illustrates an important aggregate surge in entrepreneurial activity following the reform, 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 treatment intensity, while it increased by 25% in industries in the top quartile. More generally, growth in entrepreneurial activity increases monotonically with treatment intensity.

5.3 Discussion of Identifying Assumption

Two types of omitted variable concerns arise from the way we design our treatment intensity.

INSERT TABLE 3 ABOUT HERE

A first concern, already stated in Section 5.1, is our assumption that the treatment intensity variable (i.e., the industry-level fraction of sole proprietors) is not correlated with industry exposure 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. To invalidate this hypothesis, we estimate equation (1) using the log of industry sales as our left-hand side variable. If economic recovery is stronger in treated sectors, we should expect aggregate industry sales to pick up more sharply in treated sectors. 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 $post_t$ captures total industry 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% around the reform: This is the 2003 recovery, which can be seen at the macroeconomic level. However, columns (2) and (3) show that this recovery is uncorrelated with treatment intensity. In column (3), none of the estimates of α_k—the interaction of the post dummy and the quartiles of treatment intensity—are statistically significant. Columns (4)-(6) reach the same conclusion using value added (which is easier to aggregate at the sectoral level) instead of sales to measure industry growth.[14]

Relatedly, the treatment may correlate 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 more labor-intensive industries. At the same time, entry in growing or labor-intensive sectors may be more responsive to aggregate shocks. 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, neither quantitatively nor qualitatively.

A second concern with our strategy is that results could be driven by changes in the pool of unemployed individuals. For instance, if skilled individuals tend to create firms in small-scale industries and that the post-reform period coincides with an increase in the fraction of skilled individuals in the unemployment pool, then our most treated industries could experience an increase in entrepreneurial activity that would not have anything to do with the reform itself. In the data, however, it is not the case that educated individuals are more likely to start businesses in our treated industries. To obtain this result, we use the SINE survey and simply regress, using the pre-reform wave of the survey (2002), our education dummies (high school graduate, college

[14] As an alternative way to rule out that our main results are not driven by treated sectors being overall more pro-cyclical, we simply compute industry betas with respect to GDP, and then use these betas as controls in our main regression analysis. More precisely, for each industry, we use the 1993-1998 period to regress, in the time-series, aggregate industry value added on GDP, using annual data. We then retrieve the coefficient estimate (i.e., the industry beta), which captures the extent to which each industry is sensitive to aggregate economic conditions in the mid-1990s. We then run our main regression analysis specified in equation (1), but include an additional term that interacts this estimated industry beta and the post dummy or the national GDP growth rate. Controlling in this way for the differential exposure of industries to the business cycle, we find that our main estimates are left unchanged. These results are reported in Appendix Table B.12.

graduate) on the quartiles of treatment intensity. As one can see in the Appendix Table B.13, the fraction of educated entrepreneurs does not significantly differ across industries. We are thus confident that changes in the skill composition of the pool of unemployed individuals are not driving the post-reform increase in new firm creation.

6 Effect on the Number and Quality of New Firms

In this section, we investigate the first two predictions of our model: (1) the reform should increase the number of creations and (2) the quality of new firms should decrease. The model shows that (1) should be stronger, and (2) weaker, if entrepreneurial talent is very heterogeneous.

6.1 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.[15] 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 are 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 discrepancy 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.[16]

Column (2) estimates our main specification (equation (1)) without including the treatment-specific trends. Column (3) and column (4) show that our estimates are robust to treatment-specific trends and other controls (capital intensity and industry growth). As seen in Figure 3, post-reform growth in firm creation increases monotonically with treatment "intensity". For industries

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

[16] Our results are naturally stronger if we exclude 2002 from the sample.

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 is only one fourth of the aggregate increase in firm creation (about 3,500 - 4,000 new firms per month). These aggregate estimates are conservative since they assume that the increase in new firm creation observed in the control industries (i.e., industries in the bottom quartile of our treatment intensity variable) is unrelated to the reform.

A less excessively conservative estimate can be obtained if (1) we assume that the effect of the reform on "tiny firm growth" is the same in all industries and (2) we attribute all of the differential effect across sectors to the differential prevalence of small firms across industries. To fix ideas, assume that the reform increases the number of "tiny" firms by g % in all industries, but has no effect on "serious" firms. Industries vary by the fraction of tiny firms in total creations: Let us call α_s the fraction of tiny firms in total creations in industry s. Then, the post-reform growth in firm creation in industry s is equal to $g \times \alpha_s$: The reform has a bigger impact on industries where a lot of the creations tend to be tiny firms. The differential growth in firm creation between treated and control industries, which is what our difference-in-difference estimate measures, should thus write as $g \times (\alpha_{Q4} - \alpha_{Q1})$. Hence, one could obtain an estimate of g by dividing the DD estimate by $\alpha_{Q4} - \alpha_{Q1}$, the difference in the share of tiny firms in treated and control sectors. Taking the DD estimate of column (4) and "sole proprietorships" as our measure of tiny firms, we find that the reform increases the creation of new sole proprietorships by some $14/(0.72 - 0.11) \approx 23$ %.[17] This is to be compared with the actual aggregate post-reform growth in sole proprietorship creation, or about 44%.[18] Hence, using this alternative method, we estimate a larger aggregate impact of the reform, closer to economy-level numbers. To remain conservative, however, in the rest of the paper we will focus on the difference between the most and the least treated group.

[17] In the pre-period, the fraction of sole proprietorships in the most treated industries is 72%, versus 11% in the least treated ones.

[18] In the pre-period, the French economy creates about 7,500 such firms per month, while in the post-period it creates some 10,800.

6.2 The Quality of Post-Reform Start-Ups

6.2.1 Job Creation

We now explore whether the reform led to a significant change in the characteristics of newly created firms (the second prediction of our model). The first measure of firm success or quality we look at is job creation. If the main effect of the reform was to draw in individuals of lower ability, start-ups should be less likely to create jobs, particularly in treated sectors (the "selection view"). Alternatively, if entrepreneurial talent is homogeneous and entrepreneurial success is hard to predict ex ante, after the reform, start-ups should be as likely as before to create jobs (the "experimentation channel").

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 creation and two years after creation. Columns (1)-(4) break down the results from Table 4 by splitting the sample into businesses that start with at least one employee and businesses 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 at creation (columns (3) and (4)). We find that newly created firms tend to be smaller at birth following the reform. While the reform leads to a large increase in the number of firms created with zero employees (columns (1) and (2)), it has 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 increases by a significant 16 percentage points following the reform in the most treated industries relative to the least treated industries (Q4 vs. Q1 of our treatment intensity variable). In contrast, the creation of firms with at least one employee increases by an insignificant 0.8 percentage points following the reform in the most treated industries relative to the least treated 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 individuals, who are typically more likely to start very small firms.

That the reform mostly had an impact on the creation of zero-employee firms does *not* mean that it had 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. Table 5, columns (5)-(8) show that the reform does 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 percentage points, significant at the 1% confidence level). The reform "created" some tiny firms that do not eventually grow. But more interestingly, the number of firms that eventually have at

least one employee two years after creation also increases significantly, and by a larger amount (column (8), increase of 14 percentage points, also significant at the 1% confidence level). Overall, the reform thus mostly leads to an increase in tiny firms, that eventually grow. This result is not consistent with the reform worsening the pool of entrepreneurs, at least on the job-creation dimension.

INSERT TABLE 6 ABOUT HERE

An alternative way to show the effect of the reform on average firm quality is to directly check that firms created in the most treated industries are as likely to hire as firms in the least treated industries. 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 end of the second calendar year after creation. We do not find any differential change in the propensity to hire between firms in the most vs. least treated industries. As is apparent from Table 6, columns (1)-(3), firms created in the most treated industries after the reform are 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 at least one employee two years after creation.[19]

6.2.2 Exit

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 on young ventures. Our data does not indicate why entrepreneurs leave the sample. However, it is very likely that firms dropping out of the sample are closed down, so that we interpret exit as a measure of failure.[20] In Table 6, columns (4)-(6), we estimate equation (2) using a dummy of exit within two years as our dependent variable. As is apparent from Table 6, the reform has a similar effect on the probability of exit at two years across industries. The probability of exiting the sample in the first two years after creation increases by 1.1 percentage point in the aggregate, which is small relative to the large surge in firm creation observed in Table 1. But this increase in exit probability is unrelated to the treatment (column (6)). Overall, the additional firms created by the reform are not more likely to exit.

INSERT TABLE 7 ABOUT HERE

[19] In unreported regressions, we also run this specification using the actual number of hired employees as a dependent variable and find similar results.

[20] The 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.

6.2.3 Characteristics of Entrepreneurs

In this section, we provide further evidence that firm quality does not decline after the reform using alternative, ex ante, measures of quality: education of the entrepreneur and her self-declared "ambition to grow". These variables come from the SINE survey, only available once before (2002H1) and once after (2006H1) the reform (see Section 4.3 for more details).

Table 7, Panel A checks that 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 (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 start eventually successful 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 four years after creation (column (8), increase of 7.9 percentage points).

Table 7, Panel B looks at the impact of the reform on ex ante 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). Table 7 shows no significant change in the composition of entrepreneurs by education. Although the average level of education of entrepreneurs increases across all industries following the reform, perhaps reflecting the positive trend in educational attainment in France, our most treated industries do not differ from the least treated ones in this dimension. Overall, these results are not consistent with the hypothesis that the reform lowered the education of the average entrepreneur.

6.2.4 Marginal Versus Average Effect

The results on firm quality in Table 6 and Table 7 are obtained by comparing the *average* quality of newly created firms across industries following the reform. As a consequence, these average effects do not directly isolate the effect of the reform on the quality of *marginal* entrants, i.e., those newly-created firms that would not have been created absent the reform. In this section, we attempt to provide a quantification of the effect of the reform on the quality of these marginal entrants.

First note that, by definition, infra-marginal entrepreneurs would be created irrespective of whether the reform takes place or not, so that infra-marginal entrants constitute 100% of the firms created in all industries before the reform. We then make two simplifying assumptions: (1) all firms created in the least treated industries (industries in the first quartile of treatment intensity)

following the reform are created by infra-marginal entrepreneurs and (2) marginal entrepreneurs constitute 100% of the differential entry between treated and control sectors. Let q_i (resp. q_m) be a measure of the average quality of infra-marginal (resp. marginal) entrepreneurs. We know from Table 4 that the number of firms created in the most treated industries increases by $\delta = 14\%$ relative to the least treated industries. Thanks to the second assumption above, all these firms are marginal and thus of expected quality q_m. The average quality in the most treated industries, relative to the least treated ones, thus increases by an amount $\Delta q = \frac{\delta}{1+\delta} \times (q_m - q_i)$. Given that we observe the change in average quality Δq and the fraction of marginal firms $\delta = .14$, we can infer, under our set of simplifying assumptions, the difference in observable quality between the marginal and the infra-marginal entrants.

Consider for instance the probability of hiring as a measure of quality, as we did in Table 6. Column (3) of Table 6 shows that for this measure of quality, $\Delta q = -0.0089$ and is insignificant. Applying the formula derived above yields a difference in average quality between marginal and infra-marginal entrepreneurs of about $q_m - q_i = -7\%$. Before the reform, all entrepreneurs are infra-marginal so that q_i is equal to the average pre-reform hiring rate, which is 25%. Our estimates thus suggest that the hiring rate of marginal entrepreneurs is 18%, on average, while it is 25% for infra-marginal entrepreneurs. The two numbers are not statistically different from one another.

The same calculation can be made for the other measures of observable quality used in the paper. For instance, using the same methodology, we find that the average 2-year exit rate of marginal entrepreneurs is 10%, compared to 17% for infra-marginal ones. According to this estimate, it is thus very unlikely that the exit rate of marginal entrepreneurs is higher.

6.2.5 Disguised Employment

In this section, we check that the new start-ups are not just disguised employment. One could worry that the reform allowed employers and employees to engage in regulatory arbitrage by transforming workers into self-employed contractors who receive unemployment benefits at the same time. To check this, we extract from the SINE survey information on the number of customers, and on the business relationships with the entrepreneur's former employer (see Section 4.3 for more details).

INSERT TABLE 8 ABOUT HERE

Results are reported in Table 8. This table first shows that the propensity to work with a past employer and have only one or two clients seems to have slightly increased after the reform. For instance, column (7) of Table 8 shows that the proportion of entrepreneurs reporting both to work with their former employer and to have one or two customers has increased by about 0.5

percentage points. There is, however, no difference across industries. The results in Table 8 are thus hard to reconcile with the view that many entrepreneurs drawn in by the reform are simply employees "in disguise".

7 Aggregate Effect on Employment and Productivity

7.1 Direct Effect of the Reform on Job Creation

We first estimate the aggregate effect of the reform on total job creation. We defer the discussion on potential crowding-out effects to the next Section 7.2. To estimate this direct effect, we first re-estimate equation (1) on industry-level employment data. We use the log of one 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 in industry s in month 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. 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 a wage or business income. 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 reported firm employment. In columns (3)-(4), we make the conservative assumption that all entrepreneurs are already counted as employees of their own firm.[21]

INSERT TABLE 9 ABOUT HERE

Whatever the convention we take, we find that the reform had a large impact on aggregate job creation. We assume that the least treated industries Q1 are not affected by the reform. Table 9 reports the regression estimates. After the reform, the number of jobs created by entrepreneurial firms two years after creation increases by 21 percentage points more in the most treated industries relative to the least treated industries (column (2)). We know from Table 2, in the last line of Panel A, that the most treated industries (i.e., industries in the top quartile of the fraction of sole proprietorships among newly created firms) create, on average, 118 jobs per month pre-reform. Focusing on the most treated sectors only, we find that the reform leads to at least $118 \times (e^{.21} - 1) = 28$ additional jobs created monthly. Since the top quartile of our treatment intensity variable has 72 industries, this implies that 2,000 jobs per month are created in these industries through the

[21] If the firm reports zero employees after two years, we implicitly assume that the entrepreneur's main job is not with the firm.

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 9, column (4)). [22] These results suggest that the reform led to the direct creation of between 9,000 and 24,000 new jobs every year.

7.2 Accounting for Crowding-Out

The third prediction of our model is that in equilibrium existing firms should shrink in order to allow new firms to hire. This prediction hinges on the assumption that there is little slack on the labor market, an assumption that can be disputed in the case of France, a high-unemployment economy.

To investigate possible crowding out of existing jobs triggered by the reform, we re-run our industry-level regressions (equation (1)) using the employment growth of incumbent firms as a dependent variable. We report the results in Table 10. More precisely, we first define incumbents as firms present in our sample in year t but created before year $t-4$. 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 industry employment of all incumbent firms. In columns (1)-(2) of Table 10, we first focus on small incumbents only (i.e., incumbents with five employees or less). These small incumbents are more likely to be competing directly with these new entrants, either on the product or the labor market. In columns (3)-(4), we compute the growth rate of total employment at large incumbents only (i.e., incumbent firms with more than five employees). Since we use industry-level annual data, there are 2,610 observations in these regressions, corresponding to a balanced panel of 290 industries followed over the 1999–2007 period.[23]

INSERT TABLE 10 ABOUT HERE

Table 10 shows that the reform led to lower employment growth for small incumbent firms. Following the reform, annual employment growth fell by a significant 2.2 percentage points in the most treated industries relative to the least treated ones (columns (1) and (2)). This result is robust to controlling for industry capital intensity and industry growth and is consistent with competitive dynamics whereby newly created firms partially crowd out existing small firms, as illustrated in the third prediction of Proposition 1. As a form of a placebo test, we also analyze

[22] Naturally, this wedge 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 43 jobs, on average, while the aggressive assumption lead to 118 jobs created monthly.

[23] Note that in Table 9, the sample stopped in 2005, while it stops in 2007 in Table 10. The reason is that in Table 9, we need to observe employment counts two years after a firm's creation. Since 2007 is the last year where we have employment data, we need to stop at the 2005 cohort. In Table 10, we compute the overall employment of incumbent firms, i.e., all firms created more than four years ago, which we can do up to 2007.

the effect of the reform on larger incumbents. Large incumbents—incumbent firms with more than five employees—are less likely to directly compete with new entrants, who tend to be very small.[24] Table 10, columns (3)-(4) show that in fact, employment growth at large incumbent firms does not significantly change following the reform in the most treated industries relative to the least treated ones (insignificant 0.9 percentage points estimate in column (4)).

In Table 10, columns (5)-(6), we look directly at the overall effect of the reform on industry employment. To this end, we compute, for each industry, the total number of jobs at small incumbents firms and at firms created over the last two years and use the growth rate of this variable as our dependent variable in equation (1). This variable cumulates the direct effect of the reform on job creation at new firms with the crowding-out effect leading to job destruction at small incumbents. We exclude the contribution of large incumbents to total industry employment since columns (3)-(4) of Table 10 have shown that the reform had no effect on large incumbents' employment. Columns (5)-(6) show that the most 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 the least treated industries. While this interaction coefficient is large, it is not statistically significant. Overall, we conclude that, while the reform allowed unemployed workers to start firms on a large scale, it is important to consider the effect of crowding-out when quantifying the aggregate employment gains from such a reform.

Based on the estimates of column (3), we can quantify how many jobs are being displaced following the massive entry of new firms triggered by the reform. Since the average industry in our group of most treated industries has, on average, 5,196 employees working for small incumbents (Table 2, Panel D), the industry-level effect of the reform on small incumbents is estimated to be $5,196 \times 0.022 = 114$ jobs destroyed per year and per industry. Aggregating over all the industries in the treatment group, this amounts to about 8,000 jobs per year. This aggregate effect has to be compared to the approximate (and admittedly conservative) 9,000 to 24,000 jobs directly created per year that we estimated in Section 7.1 using the same treatment variable. Of course, these numbers are imprecisely estimated. Nevertheless, this analysis suggests that crowding out effects are of the same order of magnitude as the direct effect of the reform.

7.3 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 productivity enhancing. In this section, we test whether new entrants are more productive than incumbents.

[24] In fact, the median employment among newly created firms is zero.

Table 11 provides regression evidence to this effect. It estimates the following equation:

$$Y_{ist} = \sum_{k=1}^{4} a_k \cdot Q_s^k \times \text{post}_t \times \text{New firm}_{ist} + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times \text{New firm}_{ist} + \gamma \cdot \text{New firm}_{ist} \times \text{post}_t + \zeta \cdot \text{New firm}_{ist} + \delta_{st} + \epsilon_{ist} \quad (4)$$

where Y_{ist} is a measure of productivity for firm i created in industry s in month t. We use several simple firm-level measures of productivity as dependent variables in Panel A of Table 11. Productivity is measured through average wage (columns (1) and (2)), value added per worker (columns (3) and (4)), and sales per worker (columns (5) and (6)). In principle, value added per worker is a better measure of productivity than sales per worker, 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) 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 $t+1$. The new firm dummy in equation (4) takes the value of 1 if the observation corresponds to a newly-created firm and 0 otherwise. For each new firm created in year t, productivity is measured as of year $t+2$. We cluster standard errors at the industry level.

INSERT TABLE 11 ABOUT HERE

Columns (1), (3), and (5) of Panel A in Table 11 show that, prior to the reform, wages and productivity in newly created firms are larger than those of shrinking incumbents. Annual wages are larger by about €5,200; value added per worker is higher by about €7,000 per year. This difference is sizable, considering that the average wage (including payroll taxes, as in our data) in France is about €50,000 per year. 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 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 the most treated industries, and a relative increase in the productivity of newly created firms in the least treated 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 are operating. For instance, the larger average wage observed for newly created firms does not increase differentially following the reform for firms in the control industries relative to firms in the most treated industries.

In Panel B, Table 11, we repeat these tests using standard measures of Total Factor Productivity. $TFP1$ is obtained as the residual of the following regression, where i is a firm, s its industry, t is the year of observation and we use the universe of firms present in the tax files:

$$\log(Y_{ist}) = \alpha_{st} + \beta_{st}\log(L_{ist}) + \gamma_{st}\log(K_{ist}) + \epsilon_{ist},$$

where L_{ist} is one plus firm i's total employment (thus setting employment of zero-employee firms to 1), K_{ist} is firm i's fixed assets, and Y_{ist} is firm i's value added. $TFP2$ is obtained directly by computing $TFP2_{ist} = \log(Y_{ist}) - w_s\log(L_{ist}) + (1 - w_s)\log(K_{ist})$, where w_s is the average labor share in value added in industry s. We then re-estimate equation (4) comparing the productivity of new entrants vs. incumbent firms using these TFP measures as dependent variables and report the results in Panel B, Table 11. We find results very similar to Panel A: The TFP of new firms is higher, but the difference between entrants and incumbents does not change after the reform.

As pointed out in Section 7.2, the aggregate employment effects of this reform are blurred by the reallocation of labor across firms. Yet the evidence in this section points out that the reform can still improve efficiency. Since newly-created firms are more productive, both before and after the reform, and in all industries, the reallocation of labor from small incumbent firms to new entrepreneurial firms has the potential to lead to significant productivity gains.

8 Cost-Benefit Analysis of the Reform

In this section, we propose a rough cost-benefit analysis of the reform. Such an analysis inevitably has to be somewhat tentative as it relies on many assumptions. We focus on three main channels: job reallocation, subsidizing unemployed entrepreneur, and savings on unemployment benefits.

8.1 Job Reallocation

The first channel is that more productive jobs are created, which leads to €350 million of additional GDP per year. This, in our view, is the main aggregate benefit of the reform. To get this estimate, we start from the conservative assumption that the reform led to zero *net* new job creation.[25] We showed in Section 7.3 that because new firms are more productive than incumbents, job reallocation creates additional value added. Our baseline estimation suggests that about 10,000 jobs are reallocated annually. Value added per worker in these new jobs is higher by about €7,000

[25] While the overall employment effect uncovered in column (6) of Table 10 is positive, it is not significantly different from 0, so that we use 0 as a conservative estimate.

(see Table 11). Finally, we assume that, on average, these new firms survive five years.[26] With these assumptions, the overall value added created by the reform, in steady state, is $7,000 \times 10,000 \times 5 =$ €350 million every year. We believe that this calculation is a lower bound since we estimated in Section 7.1 that new job creation through the reform was between 9,000 and 24,000, while job destruction in incumbent firms was estimated to be around 8,000 in Section 7.2.

8.2 The Cost of Subsidizing Unemployed Entrepreneurs

Prior to the reform, an unemployed individual starting a business would give up all unemployment benefits. After the reform, all unemployed entrepreneurs (about 70,000 creations per year—see Figure 1) can claim the difference between entrepreneurial income and the benefit they are entitled to. This section estimates this cost.

To calculate the subsidy per entrepreneur, we collect data on unemployed individuals transitioning into entrepreneurship. We use the 2003–2006 waves of the French Labor Force Survey (equivalent to the CPS in the US, see for instance Goux et al. (2014) for a description). The French Labor Force Survey is a quarterly panel with about 280,000 individuals where households are followed during 6 consecutive quarters. In this sample we can isolate 352 unemployed individuals who become entrepreneurs.[27] Since we also need to observe unemployment benefits and entrepreneurial income after starting a firm, the sample size goes down to 38 individuals. For each of these unemployed entrepreneurs, we can then compute:

$$Sub_i = \min\{(36 - T_i) \times \max(0, UB_i - EI_i), (24 - T_i) \times UB_i\},$$

where T_i is the number of months between the beginning of the unemployment spell and the date of firm creation, UB_i is the unemployment benefits the entrepreneur is entitled to, and EI_i is the reported entrepreneurial income. We observe all these numbers for each of the 38 individuals in our sample. The above formula mimics the spirit of the reform: The entrepreneur receives the difference between the unemployment benefit and the entrepreneurial income (if this difference is positive) every month until one of two conditions is met: (1) three years have passed since the beginning of the unemployment spell—in which case the entrepreneur receives the subsidy for $36 - T_i$ months; or (2) the entrepreneur has exhausted her rights to two full years of benefits—in which case she receives a total subsidy of $(24 - T_i) \times UB_i$. On average, this subsidy is small and

[26] This assumption is consistent with the fact that about 50% of firms in our sample are active for more than five years, and that firms created through the reform do not have a differential exit rate, as shown in Table 6.

[27] Our selection criterion is conservative, as we are excluding individuals that experience inactivity between unemployment and entrepreneurship. The quarterly frequency also forces us to miss many employees that lose their job and start a business a few weeks after: In such cases, the Labor Force Survey observes a transition from employment into entrepreneurship.

represents only some €2,000 annually. This number is small because about 70% of the unemployed generate more entrepreneurial income than their benefits. Overall, the cost of the reform for the unemployment insurance fund is thus about $2,000 \times 70,000 = €140$ million annually.

8.3 Savings from Shortening Unemployment Spells

As some unemployed return to work more quickly, the unemployment agency saves on unemployment benefits. Our most conservative estimates suggest that about 12,000 *additional* firms are created every year thanks to the reform. We then use the French Labor Force Survey to compute the corresponding savings for the unemployment insurance fund. For each unemployed individual transitioning to entrepreneurship, we calculate $UB_i \times ([1-p(X_i,T_i)]+[1-p(X_i,T_i)]^2+ ... + [1-p(X_i,T_i)]^{24-T_i})$, where T_i is the length (in months) of the unemployment spell before the unemployed is observed to start his business. $p(X_i,t_i)$ is the conditional probability that an unemployed finds a paid job during in the coming quarter, conditional on fixed observed characteristics X_i (age, education, gender, 1-digit occupation classification), and t_i, which is the number of months since the unemployment spell started. $p(X_i,t_i)$ is estimated with a logit model using the entire sample of unemployed from the Labor Force Survey (i.e., some 50,000 observations in total). UB_i is the average unemployment benefit claimed by the unemployed before the observed transition to entrepreneurship. This formula computes the savings to the unemployment insurance fund coming from the reform as the sum of the benefits that would have been paid had the unemployed remained jobless. An obvious limitation of this approach is that unemployed entrepreneurs may have a higher probability of returning to the workforce, for unobservable reasons. If this is the case, our savings estimation will be upward biased. We finally compute the average of this imputed saving across all 92 transitioning individuals for which we have enough data to make this computation (out of the 352 transitions observed in the sample). The average total savings is equal to €3,600, which leads to aggregate savings of some $12,000 \times 3,600 \approx €45$ million annually.

Overall, savings are about a third of the costs of the reform to the unemployment agency. The intuition is that savings are larger *per individual*, but only apply to *marginal* entrepreneurs. Costs per individual are smaller, but apply to both marginal and infra-marginal entrepreneurs.

9 Conclusion

This paper looks at a large-scale policy reform that provided significant downside insurance to unemployed workers who enter into entrepreneurship. Our identification rests on the idea that the reform had a differential effect on some industries, where new businesses are smaller. Perhaps unsurprisingly, we find that the reform led to a large increase in the level of firm creation, espe-

cially in those treated industries. More surprisingly, our results suggest that the reform did not lead to a significant change (worsening) in the composition of the pool of entrepreneurs. While most firms start out small at creation, they show no differences in their survival rate, firm growth or likelihood to hire workers over the three years following creation. Similarly, personal characteristics of entrepreneurial quality such as educational attainment or ambition are not lower for the entrepreneurs drawn in by the reform. As a result, newly created firms are estimated to create directly between 9,000 and 24,000 jobs annually.

The paper also emphasizes the importance of going beyond a partial equilibrium analysis of these types of reforms. We document that the entry of the new firms had strong crowd-out effects, especially on small incumbents, which experienced 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 neutral. At the same time, we show that the newly created firms are more productive than incumbents. Therefore, on net, we calculate that the reforms had a positive impact on the French economy. We weigh the benefits of the reform due to shorter unemployment spells and labor reallocation to more productive and higher-paying jobs against the costs of subsidizing the move of marginal and infra-marginal unemployed into self-employment. We find that the benefits are roughly 350 million euros while 100 million euros are transferred from the unemployment agency to unemployed entrepreneurs.

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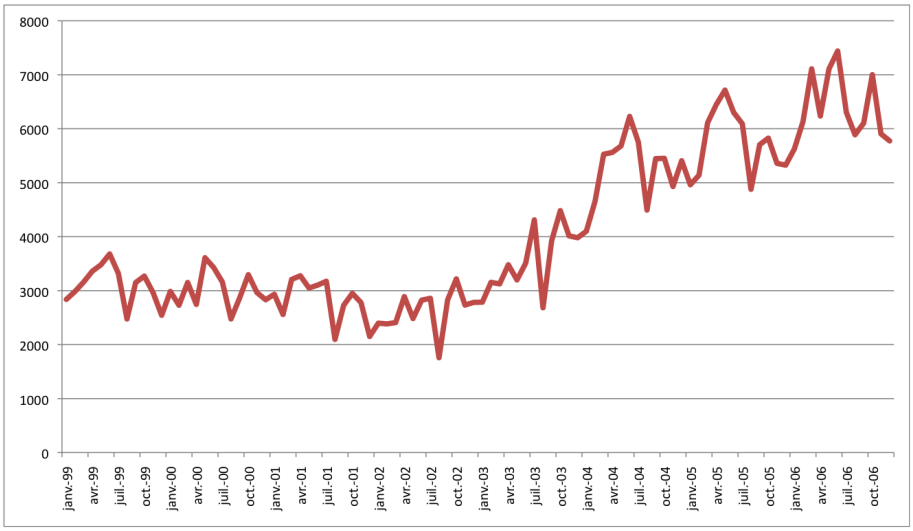
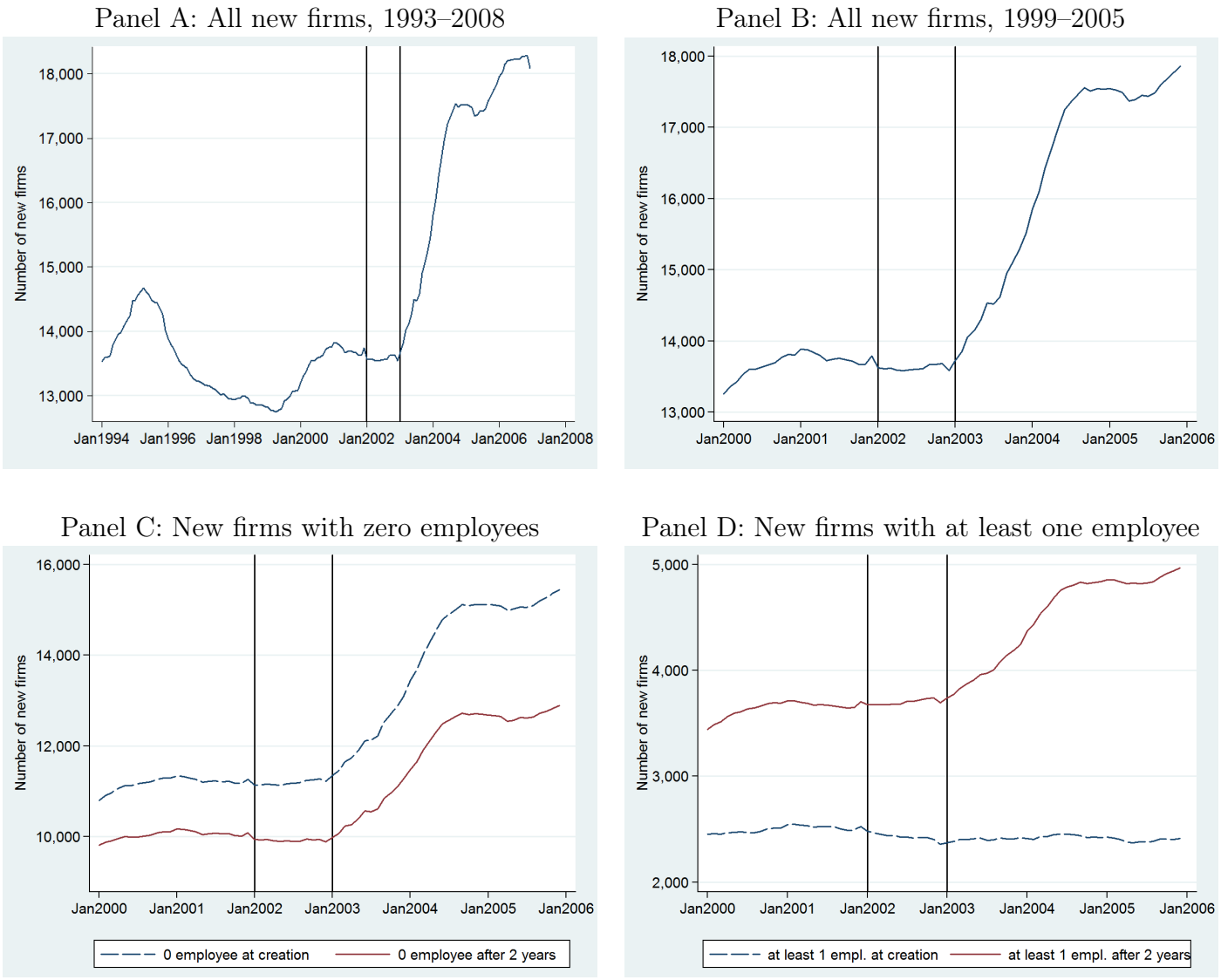
Figure 1: Monthly Number of New Firms Started With the ACCRE Subsidy



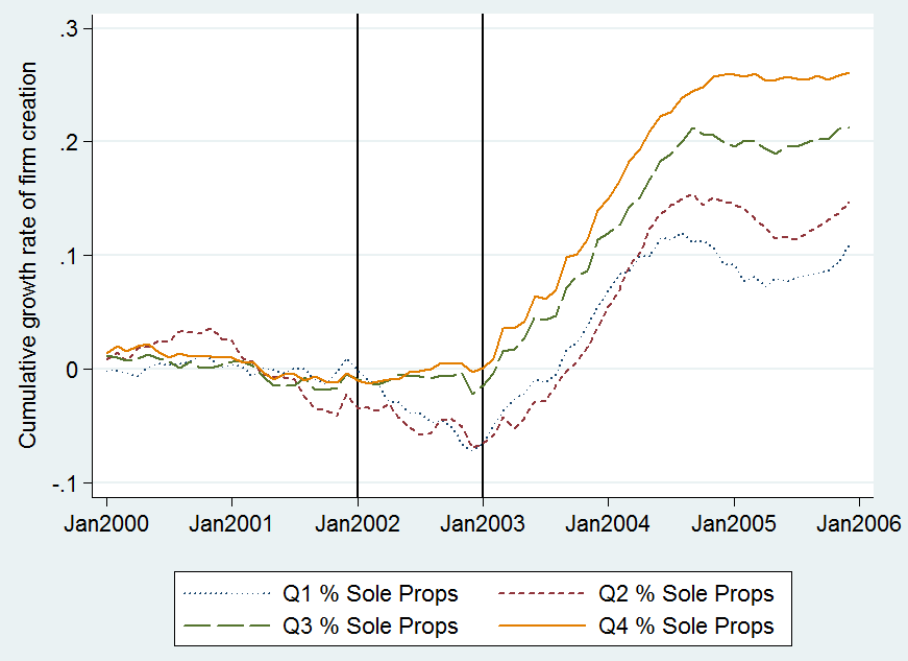
Source: French Ministry of Labor. Note: This figure shows the monthly number of individuals receiving the ACCRE subsidy, which is granted to unemployed individuals creating a new firm. The sample period covers 1999 to 2006.

Figure 2: Monthly Number of New Firms



Source: Firm registry from the French Statistical Office. Panel A plots the 12-month moving average of the number of firms created January 1993 to January 2008 (1993 does not appear on the graph as we compute a 12-month moving average). 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). The vertical dark lines correspond to the the reform period, which starts in January 2002 and ends in January 2003.

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



Source: Firm registry from the French Statistical Office. Note: Qk% is the k$^{\text{th}}$ quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Each month t and for each quartile Q_k ($k = 1, 2, 3, 4$) of treatment intensity, 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 # creations	% of pre # creations	Post-reform # creations	% of post # creations	Pre-post growth in # creations
	(1)	(2)	(3)	(4)	(5)
Transportation - Utilities	4,937	3.3	5,031	2.6	2%
Wholesale trade	11,942	7.9	12,711	6.6	6%
Manufacturing	9,119	6.0	10,006	5.2	10%
Mining	21	0.0	19	0.0	10%
Services	68,266	45.0	84,317	44.0	23%
Retail trade	25,498	16.8	34,683	18.1	36%
Construction	25,454	16.8	34,970	18.3	37%
FIRE	6,546	4.3	9,768	5.1	49%
Total	151,787	100	191,506	100	26%

Source: Firm registry from the French Statistical Office. Note: This table reports the number of firms created per year during the pre-reform period (1999–2001, column 1) and the post-reform period (2003–2005, column 3) at the 1-digit industry level. Columns 2 and 4 normalize these numbers by the total number of firm creation in the pre- and the post-reform period respectively. Column 5 reports the growth in new firm creation in the post-reform period relative to the pre-period reform.

Table 2: Summary Statistics

	N	Mean	SD	Mean by quartile of % of Sole Prop. new firms			
Panel A: New firms, industry-level				Q1	Q2	Q3	Q4
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

Source: Firm registry and tax files from the French Statistical Office and 2002 SINE survey. 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 reports summary statistics on entrepreneurs' education and ambition using the 2002 wave of the SINE survey. Panel D reports summary statistics on incumbent firms in the 1999–2001 period, where incumbents are defined as 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 conglomerate; large incumbents are incumbents with more than 5 employees and those that belong to a conglomerate. The last four columns provide summary statistics by splitting the sample into four quartiles of treatment intensity. Qi is the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period).

Table 3: Aggregate Growth Rate: Treated vs. Control

	Sales			Value added		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.082***	.095***	-.0026	.1***	.13***	.013
	(.013)	(.021)	(.013)	(.013)	(.02)	(.011)
Q2 % Sole Props × POST		-.063*	-.031		-.086**	-.02
		(.034)	(.022)		(.036)	(.024)
Q3 % Sole Props × POST		-.0084	-.011		-.041	-.029*
		(.028)	(.021)		(.029)	(.016)
Q4 % Sole Props × POST		.017	-.0021		.0058	-.003
		(.039)	(.018)		(.038)	(.016)
Constant	15***	15***	-40***	14***	14***	-44***
	(.0074)	(.0073)	(8.2)	(.0077)	(.0076)	(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

Source: Tax files from the French Statistical Office. Sample: 290 industries from 1999–2005, annual observations. Note: In columns (1) to (3) the dependent variable is the log of total industry sales. In columns (4) to (6) the dependent variable is the log of total industry value added. POST is a dummy variable equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. 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***	.046*	-.16***	-.25***
	(.014)	(.027)	(.031)	(.072)
Q2 % Sole Props × POST		.019	.035	.027
		(.043)	(.044)	(.043)
Q3 % Sole Props × POST		.08**	.11***	.11***
		(.038)	(.037)	(.036)
Q4 % Sole Props × POST		.12***	.13***	.14***
		(.038)	(.039)	(.039)
Industry capital intensity				.041*
× POST				(.025)
Industry growth × POST				-.048
				(.038)
Industry capital intensity				-.014
× Trend				(.0085)
Industry growth × Trend				.054***
				(.017)
Constant	3.2***	3.2***	.98***	.98***
	(.017)	(.018)	(.24)	(.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

Source: Firm registry from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly observations. Note: The dependent variable is the log of one plus the number of new firms created in an industry in a month. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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***	-.31***	-.055**	-.054	-.11***	-.21***	-.15***	-.25***
	(.031)	(.072)	(.026)	(.062)	(.028)	(.072)	(.026)	(.063)
Q2 % Sole Props × POST	.04	.031	-.016	-.019	.013	.0058	.021	.016
	(.044)	(.043)	(.034)	(.034)	(.043)	(.043)	(.039)	(.039)
Q3 % Sole Props × POST	.12***	.13***	.012	.012	.048	.049	.14***	.14***
	(.037)	(.036)	(.033)	(.033)	(.035)	(.034)	(.034)	(.034)
Q4 % Sole Props × POST	.15***	.16***	.0067	.0088	.086**	.093**	.13***	.14***
	(.039)	(.038)	(.035)	(.035)	(.037)	(.037)	(.035)	(.035)
Industry capital intensity		.057**		.0038		.041*		.04**
× POST		(.024)		(.02)		(.024)		(.02)
Industry growth × POST		-.031		-.032		-.035		-.016
		(.037)		(.027)		(.041)		(.037)
Industry capital intensity		-.019**		.0038		-.0087		-.023***
× Trend		(.0087)		(.0073)		(.0091)		(.0069)
Industry growth × Trend		.048***		.043***		.032		.11***
		(.018)		(.011)		(.02)		(.015)
Constant	.072	.072	2.1***	2.1***	.66***	.66***	.092	.092
	(.24)	(.23)	(.18)	(.18)	(.24)	(.24)	(.22)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly. Note: In columns (1) and (2) the dependent variable is the log of one plus the number of new firms started with 0 employees. In columns (3) and (4) the dependent variable is the log of one plus the number of new firms started with 1 employee or more. In columns (5) and (6), the dependent variable is the log of one plus the number of new firms with 0 employees two years after creation, including those which have exited. In columns (7) and (8), the dependent variable is the log of one plus the number of new firms with 1 employee or more two years after creation. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 6: Firm Quality: Ex Post Measures

	Hire			Exit		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.01***	.0076	-.0021	.011***	.0036	.019
	(.0038)	(.0046)	(.013)	(.0017)	(.0058)	(.014)
Q2 % Sole Props × POST		-.0058	-.0088		.0032	.0038
		(.008)	(.0081)		(.0096)	(.01)
Q3 % Sole Props × POST		.0053	.0052		.000016	-.00077
		(.007)	(.0069)		(.0074)	(.007)
Q4 % Sole Props × POST		-.0064	-.0089		-.0087	-.0086
		(.0056)	(.0061)		(.0083)	(.0077)
Industry capital intensity			.0066			-.006
× POST			(.0044)			(.0052)
Industry growth × POST			-.0086*			-.0011
			(.005)			(.0062)
Industry capital intensity			-.0029			.0032**
× Trend			(.002)			(.0015)
Industry growth × Trend			.0082*			.0023
			(.0043)			(.0021)
Constant	.26***	.21***	.21***	.17***	.048	.048
	(.0043)	(.049)	(.05)	(.0028)	(.034)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 1,034,674 new firms started in the 1999–2005 period. Note: 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. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 7: Firm Quality: Ex Ante Measures

Panel A: Education and ambition predict firm size									
	Log(employment)			Employment≥1			Employment≥5		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High school	.052***		.033***	.031***		.02***	.017***		.012***
	(.011)		(.01)	(.007)		(.0066)	(.0038)		(.0037)
College	.043**		.043**	.012		.012	.017**		.017**
	(.02)		(.018)	(.011)		(.01)	(.0071)		(.0067)
Plan to hire		.29***	.29***		.17***	.17***		.079***	.078***
		(.022)	(.022)		(.013)	(.013)		(.0072)	(.0074)
Constant	.29***	.25***	.22***	.23***	.21***	.2***	.042***	.033***	.025***
	(.0053)	(.0053)	(.0067)	(.0035)	(.0032)	(.0046)	(.002)	(.0018)	(.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

Source: 2002 SINE survey. Sample: random sample of 26,783 new firms started in the first semester of 2002. Note: In columns (1) to (3) the dependent variable is the log of one plus the number of employees four years after creation. 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. High school is a dummy variable equal to 1 if the entrepreneur has at least a high school degree. College is a dummy variable equal to 1 if the entrepreneur has at least a five-year college degree. Plan to hire is a dummy variable equal to 1 if the entrepreneur answers "yes" to the question "Do you plan to hire in the next twelve months?". 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

Source: 2002 and 2006 SINE survey. Sample: random sample of 56,321 new firms started in the first semester of 2002 and the first semester of 2006. Note: In columns (1) and (2), the dependent variable is a dummy variable equal to 1 if the entrepreneur has at least high school degree. In columns (3) and (4) the dependent variable is a dummy equal to 1 if the entrepreneur has at least a five-year college degree. 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?". POST is a dummy equal to 0 for observations from the 2002 wave of the survey and equal to 1 for observations from the 2006 wave of the survey. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 8: Entrepreneurs' Relation with Former Employer across Industries

	Relation with former employer			One or two clients			Relation former employer & One or two clients		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
POST	.011***	-.00063	.0021	.013***	.015	.046**	.005***	.0051**	.025***
	(.0039)	(.01)	(.02)	(.0037)	(.01)	(.019)	(.0018)	(.0024)	(.0071)
Q2 % Sole Props × POST		.019	.021		.003	.0029		-.0021	.00058
		(.017)	(.017)		(.023)	(.023)		(.0075)	(.0069)
Q3 % Sole Props × POST		.013	.014		-.0016	-.0023		.0019	.0021
		(.012)	(.012)		(.011)	(.011)		(.0036)	(.0032)
Q4 % Sole Props × POST		.011	.013		-.005	-.0059		-.00084	.0012
		(.012)	(.011)		(.011)	(.012)		(.0036)	(.0037)
Industry capital intensity × POST			-.0029			-.0098			-.0091***
			(.0077)			(.0066)			(.0026)
Industry growth × POST			.0056			-.012*			.0018
			(.0077)			(.0073)			(.0036)
Constant	.098***	.098***	.098***	.13***	.13***	.13***	.018***	.018***	.018***
	(.0024)	(.0024)	(.0024)	(.0022)	(.0022)	(.0022)	(.0011)	(.0011)	(.00098)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,264	56,264	56,264	56,264	56,264	56,264	56,264	56,264	56,264
R-squared	.028	.029	.029	.11	.11	.11	.03	.03	.03

Source: 2002 and 2006 SINE survey. Sample: random sample of 56,321 new firms started in the first semester of 2002 and the first semester of 2006. Note: In columns (1) to (3), the dependent variable is a dummy variable equal to 1 if the entrepreneur reports to be "a supplier or client of his former employer". In columns (4) to (6), the dependent variable is a dummy variable equal to 1 if the entrepreneur reports to have one or two customers. In columns (7) to (9), the dependent variable is a dummy variable equal to 1 if the entrepreneur reports to be "a supplier or client of his former employer" and to have one or two customers. POST is a dummy equal to 0 for observations from the 2002 wave of the survey and equal to 1 for observations from the 2006 wave of the survey. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 9: Job Creation

	Number of jobs created adding entrepreneurs' jobs		Number of jobs created	
	(1)	(2)	(3)	(4)
POST	-.23***	-.48***	-.23***	-.53***
	(.051)	(.096)	(.049)	(.1)
Q2 % Sole Props × POST	.087	.075	.093	.087
	(.065)	(.064)	(.066)	(.066)
Q3 % Sole Props × POST	.17***	.18***	.21***	.22***
	(.059)	(.058)	(.06)	(.06)
Q4 % Sole Props × POST	.2***	.21***	.21***	.22***
	(.059)	(.058)	(.061)	(.061)
Industry capital intensity × POST		.096***		.1***
		(.033)		(.033)
Industry growth × POST		-.025		.055
		(.044)		(.057)
Industry capital intensity × Trend		-.037***		-.042***
		(.012)		(.013)
Industry growth × Trend		.079***		.12***
		(.014)		(.018)
Constant	.85***	.85***	.4	.4
	(.27)	(.25)	(.3)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly. Note: 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 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. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 10: Employment Growth per Category of Firm

	Small incumbents		Large incumbents		Small incumbents + New firms	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-.027***	-.027	-.058***	-.093**	.0016	-.14
	(.01)	(.04)	(.016)	(.038)	(.027)	(.13)
Q2 % Sole Props × POST	-.025*	-.024**	.02	.016	-.014	-.019
	(.013)	(.012)	(.019)	(.019)	(.031)	(.026)
Q3 % Sole Props × POST	-.019*	-.019	.03	.031	.0095	.012
	(.011)	(.012)	(.019)	(.019)	(.028)	(.028)
Q4 % Sole Props × POST	-.022**	-.022**	.01	.0099	.018	.024
	(.010)	(.011)	(.018)	(.017)	(.031)	(.033)
Industry capital intensity × POST		-.00031		.017		.053
		(.013)		(.012)		(.043)
Industry growth × POST		.0012		-.02		.00087
		(.0092)		(.022)		(.037)
Industry capital intensity × Trend		-.0013		-.0063***		-.019*
		(.0024)		(.002)		(.01)
Industry growth × Trend		.00073		-.002		.0043
		(.0019)		(.0034)		(.0077)
Constant	-.09	-.09	-7.1***	-7.1***	1.9	1.9
	(1.4)	(1.4)	(2.1)	(2.1)	(4.2)	(3.9)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,610	2,610	2,610	2,610	2,610	2,610
R-squared	.47	.47	.17	.18	.61	.62

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2007, monthly. Note: In columns (1) and (2) the dependent variable is the growth rate of total employment in small incumbent firms (i.e., firms that 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 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). POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 11: Comparison New Firms vs. Shrinking Incumbents

Panel A: Simple measures of productivity

	Wage		Value added per worker		Sales per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
New firm	5.2***	5.7***	7***	6.6***	9.3***	5.4***
	(.39)	(1.6)	(.37)	(.78)	(.51)	(1.9)
New firm × POST	.014	.18	.19	.62	.23	1.8
	(.18)	(.39)	(.15)	(.55)	(.29)	(1.1)
Q2 % Sole Props × New firm × POST		-.41		-.22		-2.2
		(.54)		(.65)		(1.3)
Q3 % Sole Props × New firm × POST		-.72		-.94		-2.3*
		(.47)		(.63)		(1.2)
Q4 % Sole Props × New firm × POST		.56		-.25		-1.4
		(.53)		(.6)		(1.2)
Constant	22***	22***	26***	26***	43***	43***
	(.11)	(.11)	(.61)	(.61)	(.86)	(.88)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quartile treatment × New firm	No	Yes	No	Yes	No	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

Source: Firm registry and tax files from the French Statistical Office. Sample: all new firms and small "shrinking" incumbents in the tax files, 1999-2005. Note: Incumbent firms are defined as firms that have been in the tax files for the last four years. "Shrinking" incumbents are defined as incumbents whose employment decreases from year t to year $t+1$). 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 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. New firm is a dummy variable equal to 0 if the observation corresponds to a "shrinking" incumbent and 1 if it corresponds to a newly-created firm. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Quartile treatment × New firm are the interactions of Q2, Q3 and Q4 with the new firm dummy. All regressions include industry × 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 (continued)

Panel B: Total Factor Productivity

	TFP1		TFP2	
	(1)	(2)	(3)	(4)
New firm	.16***	.13***	.33***	.2***
	(.0061)	(.023)	(.013)	(.057)
New firm × POST	-.0053	-.0025	-.02***	.029
	(.0032)	(.014)	(.0063)	(.038)
Q2 % Sole Props × New firm × POST		.0039		-.042
		(.015)		(.04)
Q3 % Sole Props × New firm × POST		-.0076		-.051
		(.015)		(.039)
Q4 % Sole Props × New firm × POST		-.0019		-.062
		(.014)		(.038)
Constant	-.14***	-.14***	-.27***	-.27***
	(.0036)	(.0035)	(.011)	(.012)
Industry × Year FE	Yes	Yes	Yes	Yes
Quartile treatment × New firm	No	Yes	No	Yes
Observations	966,938	966,938	966,786	966,786
R-squared	.035	.035	.079	.08

Source: Firm registry and tax files from the French Statistical Office. Sample: all new firms and small "shrinking" incumbents in the tax files, 1999-2005. Note: Incumbent firms are defined as firms that have been in the tax files for the last four years. "Shrinking" incumbents are defined as incumbents whose employment decreases from year t to year $t+1$). For new firms, all dependent variables are computed two years after creation. In column (1) and (2), the dependent variable is TFP1, the residual of a Cobb-Douglas production function estimated industry by industry. In column (3) and (4), the dependent variable is TFP2, which uses the industry-level labor share as coefficients in the industry-level Cobb-Douglas production function. New firm is a dummy variable equal to 0 if the observation corresponds to a "shrinking" incumbent and 1 if it corresponds to a newly-created firm. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Quartile treatment × New firm are the interactions of Q2, Q3 and Q4 with the new firm dummy. All regressions include industry × 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.

A Derivation of the Model

A.1 Solving the model

Solving the model is simple. First, start with the entrepreneurial decisions. Maximizing profits w.r.t. l gives labor demand and the expected profit of a successful entrepreneur:

$$l = \frac{\beta}{1-\beta} p_s^{\frac{1}{1-\beta}} A\theta \qquad \text{and} \qquad \pi = p_s^{\frac{1}{1-\beta}} A\theta - c_s.$$

Given the indirect utility written above, an individual becomes entrepreneur if and only if:

$$\begin{aligned} & q \log(p_s^{\frac{1}{1-\beta}} A\theta - c_s) + (1-q)\log(b) \geq \ln(1) \\ \Leftrightarrow \qquad & A\theta \geq \theta_s \equiv p_s^{-\frac{1}{1-\beta}} \left(b^{-\frac{1-q}{q}} + c_s \right), \end{aligned} \tag{5}$$

so that production in industry s is given by:

$$Y_s = \int_{A\theta \geq \theta_s} q \frac{1}{1-\beta} p_s^{\frac{\beta}{1-\beta}} A\theta dF(\theta) = \frac{q}{1-\beta} p_s^{\frac{\beta}{1-\beta}} \frac{\phi}{\phi - 1} \theta_s \left(\frac{A\theta_0}{\theta_s} \right)^{\phi}.$$

We now write the two product market clearing conditions. Aggregating over individual consumption leads to:

$$p_T X_T^{\frac{1}{\sigma}} = p_C X_C^{\frac{1}{\sigma}}.$$

Given that markets clear, we have that $X_s = Y_s$ for $s \in \{T, C\}$. This implies that:

$$\frac{p_T^{\frac{\beta+\sigma(1-\beta)}{\sigma(1-\beta)}}}{\theta_T^{\frac{\phi-1}{\sigma}}} = \frac{p_C^{\frac{\beta+\sigma(1-\beta)}{\sigma(1-\beta)}}}{\theta_C^{\frac{\phi-1}{\sigma}}} \equiv k. \tag{6}$$

A.2 The Reform

Once the equilibrium conditions are written, we can investigate the effect of the reform. We first compute the differential increase in the number of entrepreneurs in industries T and C as a response to the reform. We model the reform as a increase in b. Differentiating (5), we get:

$$\Delta \log \theta_s = -\frac{1}{1-\beta} \Delta \log(p_s) - \frac{\frac{1-q}{q}}{1 + c_s b^{\frac{1-q}{q}}} \Delta \log(b),$$

and differentiating (6), we get:

$$\frac{\beta+\sigma(1-\beta)}{\sigma(1-\beta)}\Delta\log(p_s)-\frac{\phi-1}{\sigma}\Delta\log(\theta_s)=\Delta\log(k).$$

Therefore:

$$\Delta\log\theta_s=-\frac{\sigma}{\phi+(\sigma-1)(1-\beta)}\Delta\log(k)-\frac{1+(\sigma-1)(1-\beta)}{\phi+(\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}}{1+c_s b^{\frac{1-q}{q}}}\Delta\log(b). \tag{7}$$

We can write our first prediction:

Proposition 2. *Assume the reform leads to a marginal increase in b by Δb. Then, the Difference-in-Difference (DD) estimate of the increase in the number of entrepreneurs is given by:*

$$\Delta\log(N_T)-\Delta\log(N_C)=\frac{\phi+\phi(\sigma-1)(1-\beta)}{\phi+(\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}(c_C-c_T)b^{\frac{1-q}{q}}}{(1+c_T b^{\frac{1-q}{q}})(1+c_C b^{\frac{1-q}{q}})}\Delta\log(b).$$

Proof. The log number of entrepreneurs in industry s is given by:

$$\log(N_s)=\log(1-F(\theta_s/A))=\phi\log\theta_0-\phi\log\theta_s+\phi\log A.$$

We then use equation (7) to calculate the DD. □

Firm creation increases more in industry T than in industry C. When ϕ increases, the populations of entrepreneurs become more homogeneous. The differential effect increases, and eventually converges to $1+(\sigma-1)(1-\beta)$ as ϕ goes to infinity. If the "experimentation view" prevails (i.e., when ex post outcomes are the dominant source of heterogeneity—ϕ is very large), the effect of the reform is the largest.

The second prediction is about average quality of entrepreneurs, which we define as:

$$q_s\equiv E[\log(\theta)|A\theta\geq\theta_s]=\frac{1}{\phi}+\log\theta_s-\log A.$$

We directly combine this definition with equation (7) to obtain our second proposition:

Proposition 3. *Assume the reform leads to a marginal increase in b by Δb. Then, the DD estimate of the average quality of entrepreneurs is given by:*

$$\Delta q_T-\Delta q_C=-\frac{1+(\sigma-1)(1-\beta)}{\phi+(\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}(c_C-c_T)b^{\frac{1-q}{q}}}{(1+c_T b^{\frac{1-q}{q}})(1+c_C b^{\frac{1-q}{q}})}\Delta\log b.$$

Quality decreases more in industry T than in industry C. This happens because there is more entry in industry T. When potential entrepreneurs are however more similar, this effect vanishes (the quality threshold θ_s responds less in both industries). The difference goes to zero when $\phi \to +\infty$, i.e., when ex post outcomes are the dominant source of heterogeneity.

Finally, we compute the size of "incumbents". Employment in a firm of given quality is proportional to $p_s^{\frac{1}{1-\beta}}$. So employment change in existing firms is:

$$\begin{aligned}\Delta \log(L_s) &= \frac{1}{1-\beta}\Delta \log p_s + \Delta \log A \\ &= \frac{\sigma}{\phi + (\sigma-1)(1-\beta)}\Delta \log k - \frac{\phi - 1}{\phi + (\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}}{1 + c_s b^{\frac{1-q}{q}}}\Delta \log b + \Delta \log A.\end{aligned}$$

This allows us to write down our third prediction:

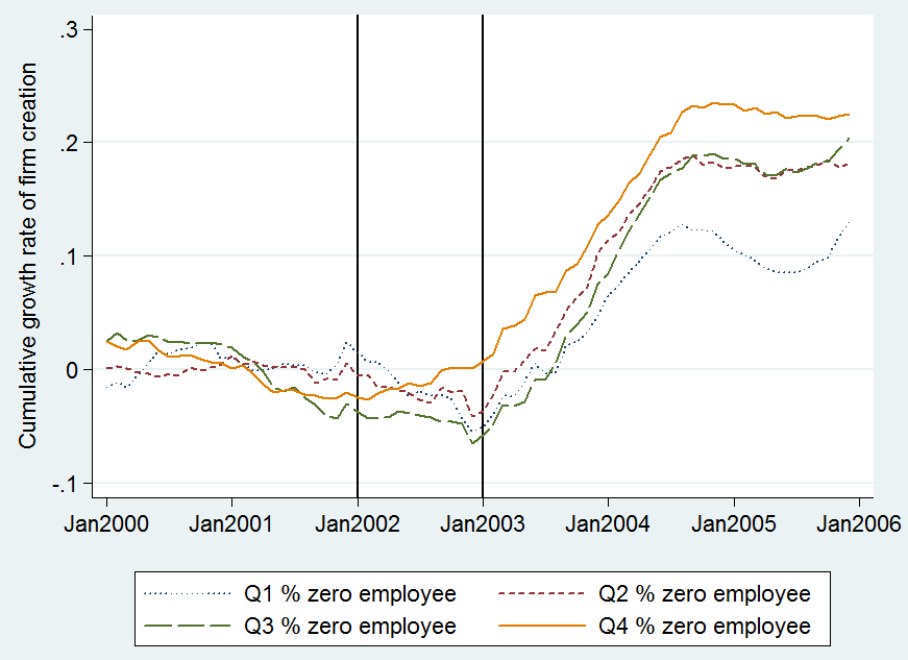
Proposition 4. *Assume the reform leads to a marginal increase in b by Δb. Then, the DD estimate of the average size of "incumbents" is given by:*

$$\Delta \log(L_T) - \Delta \log(L_C) = -\frac{\phi - 1}{\phi + (\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}(c_C - c_T)b^{\frac{1-q}{q}}}{(1 + c_T b^{\frac{1-q}{q}})(1 + c_C b^{\frac{1-q}{q}})}\Delta \log(b).$$

Since there is more entry in industry T, competition is fiercer there. Marginal revenues fall, and entrepreneurs hire less. When ϕ increases, the effect of the reform is even larger, which reinforces the crowding-out.

B Appendix Tables and Figures

Figure B.1: Growth Rate in Firm Creation. Alternative Treatment Intensity Variable.



Source: Firm registry from the French Statistical Office. Note: Qk% is the k$^{\text{th}}$ quartile of the alternative treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). For each month t and for each quartile Q_k ($k = 1, 2, 3, 4$) of treatment intensity, 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 B.1: Industries in Treatment and Control Industries

Industry name	% Sole Proprietorships	Treatment Quartile
Infrastructure development	1.3	Q1
Temporary work agencies	2.1	Q1
Holding Companies	2.5	Q1
Residential real estate development	2.6	Q1
Property operators	2.9	Q1
Television film production	4.9	Q1
Periodical publishing	5.8	Q1
Television non-film production	5.8	Q1
Wholesale trade: Footwear	6.0	Q1
Wholesale trade: Apparel	6.0	Q1
Wholesale trade: Packaged frozen food	6.3	Q1
Motion picture production	6.3	Q1
Arrangement of Transportation of Freight and Cargo	6.7	Q1
Department stores	7.5	Q1
Newspaper publishing	7.6	Q1
Secretaries and translators	83.1	Q4
Miscellaneous trade intermediaries	83.3	Q4
Other sport services	87.2	Q4
Other educational services	87.3	Q4
Fairground attractions	88.0	Q4
Other personal services	89.4	Q4
Taxis	92.0	Q4
Food non-store retailers	92.5	Q4
Independent artists	92.9	Q4
Veterinary offices	93.6	Q4
Dental offices	95.9	Q4
Non-food non-store retailers	96.2	Q4
Medical offices	96.5	Q4
Legal services	96.6	Q4
Medical aides	99.7	Q4

Table B.2: Summary Statistics: Alternative Treatment Intensity Variable.

	N	Mean	SD	Mean by quartile of % of New zero-employee firms			
Panel A: New firms, industry-level				Q1	Q2	Q3	Q4
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

Source: Firm registry and tax files from the French Statistical Office and 2002 SINE survey. 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 reports summary statistics on entrepreneurs' education and ambition using the 2002 wave of the SINE survey. Panel D reports summary statistics on incumbent firms in the 1999–2001 period, where incumbents are defined as 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 conglomerate; large incumbents are incumbents with more than 5 employees and those that belong to a conglomerate. The last four columns provide summary statistics by splitting the sample into four quartiles of treatment intensity. Qi is the i^{th} quartile of our alternative treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)).

Table B.3: Aggregate Growth Rate: Alternative Treatment Intensity Variable.

	Sales			Value added		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.082***	.081***	-.025**	.1***	.092***	-.013
	(.013)	(.014)	(.011)	(.013)	(.014)	(.011)
Q2 % zero employees × POST		-.011	.013		-.0034	.024
		(.03)	(.019)		(.032)	(.021)
Q3 % zero employees × POST		-.031	.012		-.025	.007
		(.025)	(.021)		(.025)	(.017)
Q4 % zero employees × POST		.049	.021		.07*	.02
		(.04)	(.018)		(.04)	(.018)
Constant	15***	15***	-40***	14***	14***	-44***
	(.0074)	(.0073)	(8.2)	(.0077)	(.0076)	(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

Source: Tax files from the French Statistical Office. Sample: 290 industries from 1999–2005, annual observations. Note: In columns (1) to (3) the dependent variable is the log of total industry sales. In columns (4) to (6) the dependent variable is the log of total industry value added. POST is a dummy variable equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of the alternative treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. 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 B.4: Firm Creation: Alternative Treatment Intensity Variable.

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.1***	.059**	-.13***	-.2***
	(.014)	(.023)	(.028)	(.074)
Q2 % zero employees × POST		.046	.045	.046
		(.038)	(.035)	(.035)
Q3 % zero employees × POST		.041	.024	.021
		(.036)	(.038)	(.038)
Q4 % zero employees × POST		.088**	.1***	.11***
		(.04)	(.04)	(.039)
Industry capital intensity				.033
× POST				(.024)
Industry growth × POST				-.051
				(.037)
Industry capital intensity				-.013
× Trend				(.0083)
Industry growth × Trend				.056***
				(.017)
Constant	3.2***	3.2***	.98***	.98***
	(.017)	(.018)	(.23)	(.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

Source: Firm registry from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly observations. Note: The dependent variable is the log of one plus the number of new firms created in an industry in a month. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of the alternative treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (199-2001)). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.5: Job Creation Through New Firms: Alternative Treatment Intensity Variable

	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***	-.24***	-.1***	-.097	-.1***	-.18**	-.13***	-.21***
	(.026)	(.074)	(.025)	(.063)	(.026)	(.072)	(.024)	(.061)
Q2 % zero employees × POST	.034	.037	.076**	.075**	.018	.021	.067*	.07**
	(.035)	(.035)	(.033)	(.033)	(.035)	(.035)	(.035)	(.035)
Q3 % zero employees × POST	.029	.028	.03	.027	.0074	.0052	.032	.032
	(.037)	(.037)	(.033)	(.033)	(.037)	(.037)	(.036)	(.036)
Q4 % zero employees × POST	.11***	.12***	.079**	.083**	.087**	.092**	.1***	.11***
	(.04)	(.039)	(.034)	(.034)	(.039)	(.038)	(.035)	(.035)
Industry capital intensity		.046*		.0037		.034		.031
× POST		(.024)		(.02)		(.023)		(.02)
Industry growth × POST		-.033		-.036		-.04		-.015
		(.036)		(.026)		(.039)		(.038)
Industry capital intensity		-.017**		.0025		-.0076		-.023***
× Trend		(.0084)		(.0068)		(.0088)		(.0067)
Industry growth × Trend		.05***		.044***		.035*		.1***
		(.017)		(.011)		(.019)		(.016)
Constant	.072	.072	2.1***	2.1***	.66***	.66***	.092	.092
	(.24)	(.23)	(.18)	(.18)	(.24)	(.24)	(.23)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly. Note: In columns (1) and (2) the dependent variable is the log of one plus the number of new firms started with 0 employees. In columns (3) and (4) the dependent variable is the log of one plus the number of new firms started with 1 employee or more. In columns (5) and (6), the dependent variable is the log of one plus the number of new firms with 0 employees two years after creation, including those which have exited. In columns (7) and (8), the dependent variable is the log of one plus the number of new firms with 1 employee or more two years after creation. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.6: Firm Quality: Ex Post Measures. Alternative Treatment Intensity Variable.

	Hire			Exit		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	.01***	.0033	-.013	.011***	.019***	.037**
	(.0038)	(.0061)	(.014)	(.0017)	(.0051)	(.017)
Q2 % zero employees × POST		.0084	.011		-.0047	-.0072
		(.0071)	(.0072)		(.0068)	(.0072)
Q3 % zero employees × POST		.0088	.013*		-.016***	-.02***
		(.0075)	(.0069)		(.0061)	(.0068)
Q4 % zero employees × POST		-.008	-.0051		-.034***	-.037***
		(.0067)	(.0069)		(.0072)	(.0072)
Industry capital intensity			.0069			-.0072
× POST			(.0044)			(.006)
Industry growth × POST			-.0058			.0038
			(.0046)			(.0048)
Industry capital intensity			-.0035*			.0036**
× Trend			(.0019)			(.0018)
Industry growth × Trend			.0073*			.00053
			(.004)			(.0019)
Constant	.26***	.21***	.21***	.17***	.049*	.049*
	(.0043)	(.048)	(.048)	(.0028)	(.029)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 1,034,674 new firms started in the 1999–2005 period. Note: 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. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.7: Firm Quality: Ex Ante Measures. Alternative Treatment Intensity Variable.

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 × POST	-.021	-.019	-.025**	-.024**	.025	.026
	(.031)	(.031)	(.011)	(.011)	(.018)	(.018)
Q3 % zero employees × POST	.01	.016	-.006	-.0025	.036**	.04**
	(.03)	(.03)	(.0094)	(.01)	(.017)	(.017)
Q4 % zero employees × POST	.00012	.0049	-.00077	.0019	.052***	.055***
	(.031)	(.03)	(.011)	(.01)	(.016)	(.017)
Industry capital intensity × POST		.0075		.0036		.0035
		(.015)		(.0091)		(.0084)
Industry growth × 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

Source: 2002 and 2006 SINE survey. Sample: random sample of 56,321 new firms started in the first semester of 2002 and the first semester of 2006. Note: In columns (1) and (2), the dependent variable is a dummy variable equal to 1 if the entrepreneur has at least high school degree. In columns (3) and (4) the dependent variable is a dummy equal to 1 if the entrepreneur has at least a five-year college degree. 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?". POST is a dummy equal to 0 for observations from the 2002 wave of the survey and equal to 1 for observations from the 2006 wave of the survey. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.8: Job Creation: Alternative Treatment Intensity Variable.

	Number of jobs created adding entrepreneurs' jobs		Number of jobs created	
	(1)	(2)	(3)	(4)
POST	-.17***	-.39***	-.16***	-.42***
	(.046)	(.099)	(.048)	(.1)
Q2 % zero employees × POST	.058	.067	.071	.082
	(.057)	(.058)	(.062)	(.062)
Q3 % zero employees × POST	.041	.041	.034	.041
	(.059)	(.057)	(.064)	(.062)
Q4 % zero employees × POST	.12**	.12**	.12**	.12*
	(.059)	(.057)	(.063)	(.062)
Industry capital intensity × POST		.085**		.09***
		(.033)		(.035)
Industry growth × POST		-.025		.056
		(.044)		(.057)
Industry capital intensity × Trend		-.037***		-.043***
		(.012)		(.013)
Industry growth × Trend		.078***		.12***
		(.014)		(.019)
Constant	.85***	.85***	.4	.4
	(.27)	(.25)	(.3)	(.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

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly. Note: 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 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. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.9: Employment Growth per Category of Firm: Alternative Treatment Intensity Variable.

	Small incumbents		Large incumbents		Small incumbents + New firms	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-.039***	-.038	-.053***	-.091**	-.012	-.15
	(.0088)	(.04)	(.011)	(.04)	(.023)	(.13)
Q2 % zero employees × POST	-.0064	-.0063	.017	.018	.032	.038
	(.01)	(.011)	(.015)	(.015)	(.026)	(.027)
Q3 % zero employees × POST	-.011	-.011	.023	.021	.00094	.0021
	(.012)	(.012)	(.016)	(.017)	(.028)	(.026)
Q4 % zero employees × POST	-.00025	-.00035	.004	.0052	.036	.037
	(.011)	(.011)	(.016)	(.016)	(.028)	(.028)
Industry capital intensity × POST		-.00049		.017		.051
		(.014)		(.012)		(.042)
Industry growth × POST		.002		-.018		-.0014
		(.0096)		(.023)		(.036)
Industry capital intensity × Trend		-.0013		-.0062***		-.019*
		(.0024)		(.0019)		(.01)
Industry growth × Trend		.00075		-.002		.0037
		(.002)		(.0036)		(.0082)
Constant	-.09	-.09	-7.1***	-7.1***	1.9	1.9
	(1.4)	(1.4)	(2.1)	(2.1)	(4.2)	(3.9)
Treatment-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,610	2,610	2,610	2,610	2,610	2,610
R-squared	.47	.47	.17	.18	.61	.63

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2007, monthly. Note: In columns (1) and (2) the dependent variable is the growth rate of total employment in small incumbent firms (i.e., firms that 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 columns (3) and (4), the dependent variable is the growth rate of total employment in large incumbent firms (i.e., firms that 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). POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. 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 B.10: Comparison New Firms vs. Shrinking Incumbents: Alternative Treatment Intensity Variable

	Wage		Value added per worker		Sales per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
New firm	5.2***	4***	7***	6.6***	9.3***	9.2***
	(.39)	(.31)	(.37)	(1)	(.51)	(1.1)
New firm × POST	.014	.67	.19	.79*	.23	1.1
	(.18)	(.53)	(.15)	(.45)	(.29)	(.69)
Q2 % zero employees × New firm × POST		-.79		-.75		-1
		(.61)		(.5)		(.77)
Q3 % zero employees × New firm × POST		-1.1*		-1*		-1.4
		(.58)		(.53)		(.89)
Q4 % zero employees × New firm × POST		-.0032		.1		-.13
		(.7)		(.58)		(.93)
Constant	22***	22***	26***	26***	43***	43***
	(.11)	(.12)	(.61)	(.61)	(.86)	(.86)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quartile treatment × New firm	No	Yes	No	Yes	No	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

Source: Firm registry and tax files from the French Statistical Office. Sample: all new firms and small "shrinking" incumbents in the tax files, 1999-2005. Note: Incumbent firms are defined as firms that have been in the tax files for the last four years. "Shrinking" incumbents are defined as incumbents whose employment decreases from year t to year $t+1$). 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 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. New firm is a dummy variable equal to 0 if the observation corresponds to a "shrinking" incumbent and 1 if it corresponds to a newly-created firm. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% zero employees is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of zero-employee firms among newly created firms in the industry, measured in the pre-reform period (1999-2001)). Quartile treatment × New firm are the interactions of Q2, Q3 and Q4 with the new firm dummy. All regressions include industry × 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 B.11: 20 Industries with Largest Post-reform Surge in Aggregate Creation

Industry name	% Share of Aggregate Increase in Creation	Quartile of Treatment
Business and management consulting services	7.7	Q3
Non-food non-store retail trade	6.8	Q4
Masonry contractors	4.4	Q4
Real estate agents	3.9	Q4
Electrical contractors	3.5	Q4
Miscellaneous trade intermediaries	3.3	Q4
Other Miscellaneous store retailers	2.8	Q4
Beauty parlors	2.4	Q4
Other business services	2.4	Q3
Real estate brokers	2.4	Q1
Apparel retail trade	2.4	Q3
Painting contractors	2.4	Q4
Plumbing contractors	2.0	Q4
Full-service restaurants	1.9	Q3
Legal services	1.8	Q4
Hairdressers	1.7	Q4
Food non-store retail trade	1.7	Q4
Carpentry contractors	1.7	Q4
Engineering services	1.7	Q2
Computer maintenance services	1.7	Q3
Total	58	

Source: Firm registry data from French Statistical Office. Note: In this Table, we list the 20 4-digit industries that contribute the most to the increase in average monthly firm creation between the pre period (1999-2001) and the post period (2002-2005). Column 1 is the industry's name. Column 2 is the contribution in percentage points to the aggregate surge in creation. For industry s, it is computed as $\frac{\Delta N_s}{\Delta N}$ where ΔN_s is the increase in the average monthly number of creations and $\Delta N = \sum_s \Delta N_s$. Column 3 reports the quartile of treatment (measured through the % of sole proprietorships in industry creations, as in the main text). Overall, the 20 top contributors contribute to 58% of the total surge in business creation. The rise in masonry creation contributes to 4.4% of the total surge.

Table B.12: Firm Creation: Controlling for Industry-level Exposure to the Cycle

	Number of firms created		
	(1)	(2)	(3)
POST	-.28***	-.28***	-.29***
	(.076)	(.075)	(.075)
Q2 % Sole Props × POST	.03	.031	.031
	(.044)	(.044)	(.044)
Q3 % Sole Props × POST	.11***	.11***	.11***
	(.036)	(.036)	(.036)
Q4 % Sole Props × POST	.14***	.14***	.14***
	(.039)	(.038)	(.038)
Industry capital intensity × POST	.042*	.042*	.042*
	(.025)	(.025)	(.025)
Industry growth × POST	-.00018	.00075	.00075
	(.036)	(.035)	(.035)
Industry capital intensity × Trend	-.014*	-.014*	-.014*
	(.0087)	(.0086)	(.0086)
Industry growth × Trend	.006	.0052	.0052
	(.014)	(.014)	(.014)
GDP growth	.062***		.063***
	(.0087)		(.0085)
Beta × GDP growth	-.1		.058
	(.069)		(.065)
Beta × POST		-.14	-.15
		(.14)	(.14)
Beta × Trend		.12***	.12***
		(.042)	(.044)
Constant	.69***	.98***	.69***
	(.25)	(.23)	(.25)
Treatment-specific trend	Yes	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	24,360	24,360	24,360
R-squared	.92	.92	.92

Source: Firm registry and tax files from the French Statistical Office. Sample: 290 industries, 1999–2005, monthly. Note: The dependent variable is the log of one plus the number of new firms created in an industry in a month. POST is a dummy equal to 0 for observations in the 1999-2001 period and equal to 1 from 2002 to 2005. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). Treatment-specific trends are the interactions of Q2, Q3 and Q4 with linear time trends. Trend is a linear time trend. Industry capital intensity is the average assets-to-labor ratio of firms in the industry from 1999 to 2001. Industry growth is the average growth rate of sales for firms in the industry from 1999 to 2001. Beta is computed for each industry by regressing, in the time-series, the aggregate industry value added on national GDP, using annual data. 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 B.13: Entrepreneur's education across Industries

	High school graduate		College graduate	
	All entrepreneurs	Unemployed entrepreneurs	All entrepreneurs	Unemployed entrepreneurs
	(1)	(2)	(3)	(4)
Q2 % Sole Props	.066	.042	.044	.056
	(.079)	(.073)	(.054)	(.056)
Q3 % Sole Props	.023	-.0037	.063	.028
	(.088)	(.075)	(.062)	(.056)
Q4 % Sole Props	-.053	-.13**	.0054	-.029
	(.079)	(.058)	(.042)	(.028)
Constant	.52***	.53***	.12***	.1***
	(.057)	(.042)	(.022)	(.02)
Observations	27,157	9,479	27,157	9,479
R-squared	.0072	.018	.0056	.011

Source: SINE survey. Sample: 27,157 new firms created in 1998, 9,479 new firms created by unemployed entrepreneurs. Note: The dependent variable is a dummy variable equal to 1 if the entrepreneur is a high school graduate (columns (1) and (2)) or a dummy variable equal to 1 if the entrepreneur is a college graduate (columns (3) and (4)). Columns (1) and (3) use the whole sample. Columns (2) and (4) restricts the analysis to the sample of unemployed entrepreneurs. Qi% Sole Props is the i^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the pre-reform period). *, **, and *** mean statistically different from zero at 10, 5 and 1% levels of significance.