Can Unemployment Insurance Spur Entrepreneurial Activity? Evidence From France*

Johan Hombert† Antoinette Schoar‡ David Sraer§ David Thesmar¶

August 12, 2017

Abstract

We analyze how a large-scale French reform that provided insurance to unemployed workers who start businesses affects the pool of entrants into self-employment. The reform significantly increased firm creation without worsening the quality of new entrants. Firms started post-reform are initially smaller, but employment growth, productivity, and survival rates are similar to entrants pre-reform. New entrepreneurs, post-reform, are also similar in characteristics and expectations. Finally, jobs created by new entrants crowd-out employment in incumbent firms almost one-for-one, but have higher productivity and value-added than the displaced incumbents. This highlights the benefits of allowing experimentation by lowering the barriers to self-employment.

*This is the substantially revised version of a paper previously titled "Should the Government Make it Safer to Start a Business? Evidence From a French Reform". We thank the editor, Ali Hortacsu, and referees of this journal for their comments. We thank participants at many conferences and seminars for comments and suggestions. In particular, we are indebted to Ashwini Agrawal, Steve Davis, Guy Laroque, David Matsa, Toby Moskowitz, Marina Niessner, and Elena Simintzi for their valuable insights. The data used in this paper is confidential but not the authors' exclusive access.

†HEC Paris

‡MIT-Sloan, NBER, and CEPR

§UC Berkeley, NBER and CEPR

¶MIT-Sloan and CEPR

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

1 Introduction

Over the last two decades, policy makers and academics alike have embraced the idea that reducing barriers to entrepreneurship and self-employment is important for promoting economic growth and job creation.[1] The primary focus, however, has been on understanding how such barriers affect the *level* of entrepreneurial activity. Yet, many recent studies highlight the significant heterogeneity that exists among potential entrepreneurs in their ability to grow, their risk-tolerance, their ambition, or even their optimism.[2] In light of such heterogeneity, the welfare implications of reducing barriers to entrepreneurship become unclear, as they depend crucially on how such policies affect the *selection* of individuals into entrepreneurship.

One of the key barriers to entry for would-be entrepreneurs is the ability to bear significant idiosyncratic and fundamental risk (Kihlstrom and Laffont (1979)). But the empirical literature has struggled to evaluate conclusively whether this inherent risk is an undesirable barrier to new entry or a necessary selection criterion. The risk associated with starting a firm might dissuade talented but risk averse individuals from this activity if they can only learn about their ability by starting a firm (see Jovanovic (1982) and Caves (1998)). Providing some form of downside insurance to would-be entrepreneurs could thus lead to more efficient entry into self-employment. In contrast, if individuals have ex ante private information about their entrepreneurial abilities, decreasing the cost of entry via downside insurance, it might lead less qualified individuals to start new firms. As a result, allocative efficiency may be impaired if scarce resources are diverted to less productive firms.

To develop testable predictions, we first analyze these trade-offs through the lens of an equilibrium model of occupational choice that features risk-averse individuals and heterogeneity in the distribution of talent. In the model, providing downside insurance to entrepreneurs always fosters entry, but the level of entry and how it affects entrepreneurial quality and incumbents depends on the dispersion of the talent distribution. When talent is relatively heterogeneous, which we refer to as the "self-selection" view, lowering downside risk has a modest effect on new firm creation, draws in lower quality entrepreneurs, and leads to a smaller reduction in the size of incumbent firms. In contrast, when the talent distribution is more homogeneous, the reform has a larger

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

[2] See, among others, Haltiwanger et al. (2013), Nanda (2008), Hurst and Pugsley (2011), Landier and Thesmar (2009), Holtz-Eakin et al. (1994a), and Schoar (2010)

effect on new firm creation, a modest effect on entrepreneurial quality and leads to a significant reduction in incumbent size; in this case, fostering entry increases allocative efficiency by allowing relatively high-quality individuals to become entrepreneurs, consistent with the "experimentation" view of entrepreneurship (Manso (2011)).

We then exploit a reform of the French unemployment insurance system to investigate empirically how large reductions in the cost of entry affect selection into entrepreneurship and allocative efficiency more generally. The reform we consider, called PARE (*Plan d'Aide au Retour à l'Emploi*), was implemented starting in 2002 to facilitate (small) business creation by unemployed individuals. Prior to this reform, unemployed workers starting a business would lose all access to their unemployment insurance benefits. After the reform, such "unemployed entrepreneurs" were allowed to retain the rights to their unemployment benefits for up to three years in case their business failed.[3] By extending such downside insurance, this reform led to a large reduction in the cost of starting a new business when unemployed. We find a steep and persistent increase in new firm creation (of about 25%) following the implementation of the reform (see Section 4.1). We leverage firm- and individual-level administrative data to evaluate how this large-scale reform affected not only firm creation, but also the characteristics of newly-created firms and industry-wide employment.

To evaluate the PARE reform in the data, we exploit the heterogeneity across industries in "exposure" to the reform. Unemployed individuals are empirically more likely to start smaller firms and register as sole proprietorships. We thus define exposure to the reform as the fraction of sole proprietors among all newly-created firms in an industry, and measure this "treatment intensity" in the years preceding the reform.[4] Our identification strategy is akin to a difference-in-difference: We compare how the number and characteristics of newly-created firms evolve around the time of the reform for industries with different levels of treatment intensity. The identifying assumption is that absent the reform, changes in the number and characteristics of newly-created firms around 2002 would not have been systematically related to industry treatment intensity.[5]

Using this empirical strategy, we first find that the reform significantly fostered new business creation: Relative to the pre-reform period, firm creation post-reform is 12 percentage points higher in industries belonging to the top quartile of treatment intensity than those in the bottom

[3]Under certain circumstances, the reform also allowed entrepreneurs 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. The reform did not, however, generate any additional transfers to unemployed people registering a business.

[4]We show in Appendix B that our analysis is robust to defining treatment intensity as the fraction of firms with zero employees among newly created firms in an industry.

[5]We explore the validity of this assumption through numerous robustness checks in the paper. In particular, our relatively high frequency data allow us to check the parallel trend assumption through the inclusion of treatment-specific trends in our regressions.

quartile. Using ancillary data on the take-up of a subsidy program for unemployed entrepreneurs, we show that most of the newly-registered firms in the most exposed industries created after the reform are, in fact, started by unemployed individuals. This result provides further evidence for the causal link between the reform and the increase in firm creation post-reform observed in the data.

We then document that firms created in response to the reform are not of (observably) worse quality. We first measure quality using ex post outcomes and show that, relative to the pre-reform period, there are no significant changes in failure rates, hiring rates, or growth rates of newly-created firms started after the reform in the most vs. least treated industries. Using administrative survey data, we also measure quality using ex ante characteristics of entrepreneurs such as education and self-reported, subjective, growth expectations. We find no significant effect of the reform on the composition of entrepreneurs' educational backgrounds and a small, *positive* effect on growth expectation. Overall, the evidence supports the "experimentation view", whereby providing downside insurance fosters new firm creation without significantly lowering the average quality of the new entrepreneurs.

Since our sample covers the universe of private and public firms in France, we can also evaluate how the entry of a large number of new firms due to the reform affected industry-wide employment, and in particular the growth of incumbent firms. While we find no evidence of spillovers on *large* incumbent firms, the reform did lead to a 2.6 percentage points decline in employment among small incumbents, which are more likely to compete on the product and labor market with these new entrants. This "crowding-out" effect on small incumbents is economically large, as it mostly offsets the direct effects of the reform on employment creation by start-ups.[6] We also document that wages and productivity (measured as value added or sales per worker) are significantly larger in newly-created firms relative to the incumbents they crowd out: Value added per worker is €7,000 per year higher in recently created firms relative to incumbent firms. This productivity differential does not decrease after the reform, consistent with the other measures of new firm quality investigated above. Overall, while the reform led to small employment gains in the aggregate—as jobs created by new firms mostly crowded-out existing jobs at incumbent firms—it led to a large reallocation of resources from less productive incumbents to more productive young firms, likely leading to an increase in aggregate productivity.

We conclude the paper by providing a tentative aggregate cost-benefit analysis of the PARE reform. We calculate that the PARE had a positive impact on the French economy on the order of about €350 million per year, while the cost to the unemployment agency was about €100 million

[6]These results bear some similarity to the literature on financial reforms, which also documents that increased entry is detrimental to incumbent firms, see (Cetorelli and Strahan (2006), Bertrand et al. (2007), and Kerr and Nanda (2009a)).

per year. This 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 transition of marginal and infra-marginal unemployed individuals into self-employment.

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; and (2) we document how removing barriers to entry affects incumbent firms. The earlier literature has looked at cross-country differences in barriers to entry and their aggregate implications for entry rates (Djankov et al. (2002), Desai et al. (2003), and Klapper et al. (2006)). Because of its focus on cross-country outcomes, this literature has not been able to test how barriers to entry affect the composition of the pool of entrepreneurs (Branstetter et al. (2014), Mullainathan and Schnabl (2010), and Bruhn (2011)).

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), and Hurst and Pugsley (2011)). These papers have documented large heterogeneity in the talent, ambition, and risk-preferences of entrepreneurs, which translates into different investment and effort choices following entry. We extend this literature by showing how an increase in downside insurance affects self-selection into entrepreneurship. We also complement a large literature on the role of financing constraints on entrepreneurship.[7].

Finally, our paper is 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), and Card and Levine (2000) among many others). Relative to these papers, our contribution highlights an often-ignored distortive effect of unemployment insurance on the transition into self-employment. 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.

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, and

[7]See, among others, 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), Bianchi and Bobba (2013), Adelino et al. (2015), and Schmalz et al. (2015)

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 of the PARE reform.

2 The Reform and Institutional Details

2.1 Description of the PARE Reform

The PARE reform (*Plan d'Aide au Retour à l'Emploi*) consisted of a new agreement between labor unions and employer organizations, which aimed to provide more generous benefits for unemployed workers engaging in an active employment search.[8] The agreement was signed in July 2001 and came into full effect in mid-2002. An important aspect of this reform was to reduce the implicit disincentives for unemployed workers to start a new business by extending the unemployment insurance system to unemployed entrepreneurs in several dimensions.

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 the accumulated unemployment benefits when starting a business, even if the business subsequently failed. The new agreement allowed these individuals to retain their rights to the remaining unemployment benefits for up to three years if their business failed.[9] Second, the reform also stipulated that unemployed workers could supplement their income with unemployment benefits *while* starting their own firm (Rieg (2004)) if the income derived from their entrepreneurial activity remained below 70% of their pre-unemployment income. 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.[10] Therefore, unemployed workers who decided to start a business were guaranteed to receive their unemployment benefits for at least two years and up to three years in case their business failed.

INSERT FIGURE 1 ABOUT HERE

The unemployment agency began advertising the reform to unemployed individuals in the fall of 2002 (Rieg (2004)). While we do not 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

[8] In France, labor and employer unions jointly run the unemployment benefit agency.

[9] Articles 1-5 of the PARE agreement.

[10] 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 of days, d, 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" to unpaid unemployment benefits corresponding to d worked days, which she can claim for up to three years.

subsidy allocated *only* to unemployed workers who start firms. The ACCRE program was not itself affected by the reform but gives us a lower bound for the number of unemployed entrepreneurs. Figure 1 shows these data at the monthly frequency. Clearly, the number of new firms created by unemployed workers increases sharply between 2002 and 2005, suggesting the reform had a large effect on new firm creation. We discuss these magnitudes in Section 4.

2.2 External Validity

External validity is an important concern for our analysis; certain characteristics of the French labor market might explain how entry and average firm quality responded to the reform we evaluate. 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 individuals. France's unemployment rate is high (8.3% vs. 7.3% on average in the OECD in 2002). Long-term unemployment is more prevalent than in Anglo-Saxon countries: in 2002, about 32% of the unemployed in France had been unemployed for less than three months vs. more than 50% in the US and Canada, and 45% for the UK.

Second, the reform could have a large effect on entry because unemployment benefits in France are particularly generous, creating a strong disincentive to start a company prior to the reform. However, the Net Replacement Rate computed by the OECD for the average wage in France is only 62%, compared to an OECD average of 56%. While the French unemployment insurance system is slightly more generous than the typical developed economy, the difference remains marginal.

Third, abnormally low ex ante entrepreneurial rates could explain why this reform led to a massive entry of new businesses. World Bank data from 2004 show that firm creation rates in France (2.8 new corporations per 1,000 inhabitants) are slightly above the Eurozone average (2.6), and somewhat below the OECD median (3.3). Importantly, firm creation rates in France are significantly smaller than in Anglo-Saxon countries (e.g., 9.8 in the UK). Clearly, continental Europe faces stronger barriers to entry than Anglo-Saxon countries, so reforms like the PARE may have a weaker effect on firm creation in these countries.

3 Economic Framework

This section lays out the theoretical framework that will guide our empirical strategy. We start from the model of entrepreneurship in Lucas (1978) and introduce two modifications. First, we allow entrepreneurship to be risky, which provides a role for entrepreneurial insurance. Second,

we introduce two distinct industries, which differ by their scale of production, in order to capture the intuition behind our empirical strategy that some industries are naturally more exposed to unemployment insurance reforms.

The two industries are T (Treatment) and C (Control) and they 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. 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. In the first, agents choose between starting a firm or supplying labor. In the second, production takes place, entrepreneurs in each industry receive profits, and workers receive salaries, which we normalize to 1 without loss of generality. 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 can only start a firm in s. Starting a firm is risky: When an individual decides to become an entrepreneur, she first needs to find out whether there is a market for her idea. If there is no such market (with probability $1 - q$), it is too late to become a worker and she gets b, which 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 survives, but its 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 with 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 $\pi_s(\theta, l, p_s) = p_s A^{1-\beta} g(\theta) l^{\beta} - l - c_s$.

As in 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 model the reform as an increase in the downside protection for failed entrepreneurs b. We look at how this change in b affects entry, firm quality, and incumbent size across the two industries T and C. We solve the model in closed-form in Appendix A and gather the results in Proposition 1:

Proposition 1. *Assume the reform consists of a marginal increase in b of 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 "incumbent" firm* $\log(L_s) = E(\log(l(\theta))|\theta > \theta_s)$*, is given by:*

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

.

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.

Proposition 1 first shows that the reform has a stronger effect on the low-scale industry T. This is the underlying principle behind our identification strategy. Quite intuitively, the minimum ability necessary to start a business is lower in the low-scale sector. Add to this the fact that the distribution of abilities has a decreasing hazard rate $F'(\theta)/(1 - F(\theta))$ (as is the case with Pareto). Thus, the number of "marginal entrepreneurs" right below the threshold is larger in the treated sector T, and thus the reform brings in a heavier mass of entrepreneurs to that industry. This induces more entry and more crowding-out in industry T, and a larger decline in entrepreneurial quality.

Proposition 1 also illustrates that how the reform affects the economy depends on the heterogeneity in skill distribution. When the shape parameter ϕ is close to 1, entrepreneurial skills are very heterogeneous and the reform only has a small (positive) effect on entry: An increase in insurance b decreases the ability threshold above which agents become entrepreneurs, but this effect is limited since agents are more "spread out" on the ability spectrum. Average quality does, however, respond a lot to an increase in insurance, since marginal entrepreneurs are much worse than infra-marginal ones. Because entry is limited, there is very little crowding-out of incumbents (in the limit, none since $S(1) = 0$). These predictions are consistent with the "selection view" mentioned above in the introduction. Conversely, when entrepreneurial skills are homogeneous (large ϕ), an increase in insurance leads to a large effect on entry, significant crowding-out of

existing firms, and a small decline in entrepreneurial quality, consistent with the "experimentation view" defined above.

4 Data

We use three sources of data, which we obtain from the French Statistical Office (INSEE): the exhaustive 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 register that year.

4.1 Registry

The firm registry contains the universe of registered firms each month in France, 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

Figure 2 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. Starting in 2003, the year after the reform, 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, which is often linked to a series of later reforms that are not related to PARE.[11] 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 new firms that have zero employees at creation (blue, dotted line) and how many firms have zero employees two years after creation (red, solid line). In the aggregate, we see that the reform is accompanied by a surge in the creation of firms that are started with zero employees and remain small after two years. Panel D plots the number of firms created each month that start with at least one employee at creation (blue, dotted line) and the number of firms with at least one employee two years after creation (red solid line). While the reform is not associated with an increase in the number of firms created with more than one employee, it clearly coincides with a massive increase in new firms starting with no employees. However, as the red

[11] These reforms allowed entrepreneurs to register a company on-line (June 2006).

line indicates, these zero-employee firms eventually grow and hire some employees two years after creation.

Consistent with the idea that this increase in entrepreneurial activity is, in fact, triggered by the reform, the dramatic surge in firm creation observed in Figure 2 mostly consists of unemployed entrepreneurs, i.e., those individuals targeted by the reform. While we do not observe the overall number of new firms created each month by unemployed individuals, we can estimate this number in the following way. As shown in Figure 1, the number of new firms that receive the ACCRE subsidy (a subsidy only accessible for unemployed entrepreneurs, which was not itself affected by the reform we study) progressively increased from 3,000 per month in 2002, to about 6,000 per month in 2006. Simultaneously, we can use the SINE survey (described in detail below in Section 4.3) to compute the change in the take-up rate of this subsidy during this period: 53% in 2002 and 67% in 2006. Hence, monthly firm creation by unemployed individuals increased from $3,000/53\% = 5,660$ to $6,000/67\% = 8,955$, a monthly increase of 3,300. This number corresponds *almost exactly* to the increase in total firm creation observed at the aggregate level, which goes from 14,000 to 17,500 (Figure 2). Hence, a detailed examination of the data allows us to trace the *entirety* of the 2003–2005 surge in firm creations to unemployed entrepreneurs.

INSERT TABLE 1 ABOUT HERE

Table 1 provides annual data on firm creation for eight broad industries from 1999 to 2005. 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. We also show that the industries with the largest growth of new entrants after the reform are Services, Retail Trade, Construction, and Finance, Insurance and Real Estate (FIRE), which are labor-intensive, low fixed-cost industries.[12]

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.

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

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 detailed description). Tax files provide us with the number of employees at creation and 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 often 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 creation. Two years after creation, firms have, on average, 0.87 employees. In the pre-reform sample, 25% of the new firms hire at least one employee in the first two years and 16% of the firms exit the sample before the end of the second fiscal year.

4.3 SINE Survey

To obtain additional demographic and personal information on entrepreneurs, we use the SINE survey, a large-scale survey run by the French Statistical Office every four years (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 the entrepreneur and the firm she creates.[13] We only have two cross-sections of the survey in the relevant time period: 2002 and 2006. 2002 belongs to the pre-reform period—the survey is done during the first semester of 2002, while the unemployment agency only started to advertise the reform in the second half of 2002. 2006 corresponds to the post-reform period. SINE covers approximately a third of newly created firms in the first six months of a survey year (26,683 observations in 2002 and 29,538 observations in 2006) and has a response rate typically around 85%.

We first use the SINE survey to measure entrepreneurs' highest educational attainment. We also use the response to the survey question "Do you plan to hire in the next twelve months?" as

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

a measure of subjective growth expectations or "ambition". Table 2, Panel C reports descriptive statistics on the survey 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, for robustness purposes, we construct two additional variables: an indicator variable when the entrepreneur declares to be "a supplier or client of his former employer"; another indicator when the entrepreneur responds that her firm "has at most 2 different customers".

5 Empirical Strategy

5.1 Identification Strategy

The aggregate response of new firm creation around 2002 observed in Figure 2 may not necessarily be caused by the PARE reform; aggregate shocks unrelated to this reform could instead be responsible for the rise in new firm creation. To address this issue, we use a simple identification strategy akin to a standard difference-in-difference but with heterogeneous treatment intensity.

The PARE reform was aimed at unemployed individuals who have limited start-up capital and are more likely to start low-scale firms, which we measure as sole proprietorships.[14] In the 2002 wave of the SINE survey, 70% of unemployed workers who started a firm chose to register as a sole proprietorship, while only 45% of previously employed entrepreneurs made this choice. We expect industries with a larger fraction of sole proprietorships to be more affected by the reform, a prediction of our model in Section 3. Following this intuition, we therefore define treatment intensity as the fraction of sole proprietorships among newly created firms at the industry level, and measure it at the 4-digit industry level in the pre-reform period. We then rank industries in ascending order of treatment intensity and construct four quartiles (Q1–Q4), which should be increasingly affected by the PARE reform. Our identification strategy then simply compares how the number and characteristics of newly-created firms changed from the pre- to the post-reform period depending on the treatment intensity quartile to which the industry belongs.[15] The identifying assumption is that absent the reform, changes in the number and characteristics of newly-created firms around 2002 would not have been systematically related to industry treatment intensity.[16]

[14] We also repeat our analysis using an alternative definition of treatment intensity: the fraction of firms created with zero employees within a 4-digit industry. Tables B.2–B.10 in Appendix B report regression results using this alternative treatment definition that are qualitatively similar to our main results.

[15] In robustness checks, we also split industries using deciles and vigintiles of treatment intensity and obtain consistent findings.

[16] We show below that in fact, in the pre-reform period, the time-series of new-firm creation is very similar across the four quartiles of treatment intensity.

Appendix Table B.1 lists industries that belong to the least (i.e., bottom quartile Q1) vs. most (i.e., top quartile Q4) treated industries. Highly exposed industries consist of, e.g., taxi drivers, health care specialists, and personal services. Low exposure industries consist instead of real estate developers, movie and TV producers, and wholesale trades. In Table 2, we present summary statistics for firms and industries in each of these four quartiles of treatment intensity. In industries belonging to Q4, firms have .54 fewer 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 Q1 industries. Entrepreneurs in Q4 industries are also, on average, less educated (7 percentage points less likely to have a high school degree) and less ambitious (16 percentage points less likely to want to hire in the next twelve months) than those in Q1 industries.

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. For each industry s over the 2002–2005 period, we compute $\frac{\Delta N_s}{\Delta N}$, where ΔN_s is the increase in the average monthly number of creations and $\Delta N = \sum_s \Delta N_s$. Consistent with our identification strategy, the increase in new firm creation is concentrated among Q4 industries: (1) the top 20 4-digit industries contribute to more than half of the aggregate surge in new firm creations; and (2) out of these 20 industries, 13 belong to the fourth quartile of treatment intensity (Q4) and 18 belong to either Q4 or Q3.

5.2 Empirical Specification

Our main specification for industry-level outcomes is as follows:[17]

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

where Q_s^k refers to the quartile of treatment intensity to which industry s belongs, post_t is a dummy equal to 1 for outcomes measured after January 2002, and MONTH_t is month-of-creation fixed effects.

For firm-level outcomes, we use a similar specification where i refers to a firm in industry s created at date t:

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

[17] Since our sample of industries is balanced, it is not necessary to include time fixed-effects in this difference-in-difference model.

When using 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.

In all specifications, we cluster standard errors at the industry level.

INSERT FIGURE 3 ABOUT HERE

Figure 3 provides a graphical illustration of the identification strategy. For each industry, we compute the log number of firms created each month from 1999 to 2005 *minus* the average monthly log number of firms created in the same industry from January 1, 1999, to December 31, 2000. We then average these log changes across industries within each quartile of our treatment intensity variable, and plot the 12-month moving-average of these four growth rates. Consistent with Figure 2, Figure 3 shows a significant increase in firms started right after the reform is implemented. However, this surge is clearly more pronounced in industries with larger treatment intensity: The number of newly-created firms increases by about 10% in Q1 industries and by 25% in Q4 industries. Additionally, growth in entrepreneurial activity increases monotonically with treatment intensity.

5.3 Discussion of the Identifying Assumption

Two types of omitted variable concerns can arise in our empirical setting. First, the measure of treatment intensity could be correlated with industry exposure to macroeconomic fluctuations. If this is the case, the post-2003 increase in new firm creations observed in industries with high exposure to the reform would, in fact, be caused by the economic recovery and not by the reform itself. We can alleviate this concern by estimating how *total* industry sales respond to the reform. If our estimates simply capture industries' heterogeneous exposures to the business cycle, we should then expect *aggregate* industry sales to increase significantly more post-reform in industries with the highest treatment intensity. We aggregate firm-level sales from our annual accounting data and estimate equation 1 using the log of annual industry sales as our dependent variable. The results, presented on Table 3, show that while there is a significant increase in aggregate industry sales in the post-reform period (of 8.2% in column (1)), this increase is not significantly different across the 4 quartiles of treatment intensity (columns (2) and (3). columns (4)–(6) lead to a

similar conclusion when using aggregate industry value added).[18]

INSERT TABLE 3 ABOUT HERE

Relatedly, we can directly control for industry characteristics that might correlate with treatment intensity and make industries more sensitive to business cycle fluctuations. We highlight two particular characteristics: growth and labor intensity. Industries in which firms start on a small scale could have better growth opportunities or be more labor-intensive. At the same time, entry in growing or labor-intensive sectors may be more sensitive to aggregate shocks. To account for these effects, we re-estimate equation (1), but add interactions of 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). With these added controls, the estimated effect of the reform on new firm creation is not significantly different from the previous estimates.

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 the post-reform period coincides with an increase in the fraction of skilled individuals in the unemployment pool, then industries with high treatment intensity could experience increased entry for reasons unrelated to the PARE reform. To test this hypothesis, we use the 2002 wave of the SINE survey to show that the fraction of educated entrepreneurs does not differ significantly across industries (Appendix Table B.13). This result shows that changes in the skill composition of the pool of unemployed individuals cannot be driving the post-reform increase in new firm creation observed in industries with high treatment intensity.[19]

6 Effect of the Reform on Entrepreneurial Activity

6.1 Creation of New Firms

We first analyze the growth in firm creation induced by the reform. We estimate equation (1) using the log number of firms created in an industry s and month t as our dependent variable.[20]

[18] In Appendix Table B.12, we run an additional robustness test that directly controls for industries' exposure to the business cycle. We compute industry "βs" with respect to GDP in the pre-reform period (1993 to 1999). We re-estimate equation (1), including a control for the interaction of industry β and the post dummy. Our estimates are not affected by these controls.

[19] A related concern could be that the 2002 recession increased the number of unemployed individuals disproportionately in high treatment intensity industries. This could result in a mechanical increase in the number of unemployed entrepreneurs in these industries. Using the French Labor Force survey, however, we see that, if anything, unemployment rates in Q4 industries increase *less* in 2002.

[20] Our dependent variable is log(1 + # firms created). Some smaller industries experience months without any creation. The results are similar when using log(# firms created).

The regressions use a balanced sample of 290 industries from January 1999 to December 2005. The results are reported in Table 4. Column (1) only includes the post dummy, along with industry and month-of-creation fixed-effects. It shows that following the reform, the monthly number of newly-created firms increased by a significant 10% across all industries. This effect is slightly smaller than what we reported from Figure 2, which can be attributed to the fact that we conservatively start the post-reform period in January 2002, while the reform is only progressively implemented in 2002.

INSERT TABLE 4 ABOUT HERE

Column (2) adds interaction for the post dummy and quartiles of treatment intensity. Column (3) adds interactions for linear trends and quartiles of treatment intensity. Column (4) additionally interacts both the post dummy and linear trends with industry-characteristics (capital intensity and industry growth). The results in columns (2)–(4) are not significantly different. Q4 (resp. Q3) industries experience a significant increase in new firm creation of 12 to 14 (resp. 8 to 11) percentage points in the post-reform period relative to Q1 industries. Grouping Q3 and Q4 industries together, these estimates imply an increase in firm creation following the reform of about 1,000 newly created firms per month. While this number is only one-fourth of the aggregate increase in firm creation (about 3,500 new firms per month in Figure 2), note that these estimates are quite conservative since they assume that any increase in new firm creation observed in Q1 and Q2 industries is unrelated to the reform.

Since the implementation of the reform was progressively phased in between July 2001 and mid-2002, in the Appendix we check that our results are robust to alternative definitions of the event window. We show that: (1) when we exclude 2002 from the sample, the estimated effects are actually larger, consistent with what we expect since the reform was only fully advertised by local unemployment agencies in mid-2002 (Appendix Table B.14); (2) when we assign 2002 to the pre-reform period, the estimated effects are smaller, which is not surprising since part of the "treatment" period is now classified as the "control" period (Appendix Table B.15); and (3) when we exclude 2005 from the post-reform sample, the results are virtually unchanged (Appendix Table B.16).

6.2 Additional Evidence: Unemployed Entrepreneurs and Firm Creation

In this section, we provide further evidence that ties down the dynamics of firm creation post-reform to the population of unemployed workers. This is an important step in our analysis because it helps to confirm the causal interpretation of the results in Section 6.1 and invalidates the

hypothesis that these results are driven by confounding factors such as heterogeneous exposures to business cycle fluctuations. First, recall from Section 4.1 that we have used data on the take-up of the ACCRE subsidy (a subsidy for unemployed entrepreneurs, unrelated to the PARE reform) to show that more than 90% of the *aggregate* increase in new firm creation observed post-reform could be attributed to unemployed individuals. Such time series evidence strongly points towards the reform.

Here, we complement time series evidence with cross-section evidence. We check that firm creation increases more in industries with a larger increase in the fraction of unemployed entrepreneurs. If our results were spurious, e.g., capturing heterogeneous exposures to business cycle fluctuations, within-industry shifts in the share of unemployed entrepreneurs should not correlate with firm creation rates. To test this hypothesis, we estimate the following equation:

$$\frac{n_s^{2006} - n_s^{2002}}{n_s^{2002}} = \beta\, \Delta\text{Unemp}_s + \gamma\, \text{AggGrowth}_s + \epsilon_s, \tag{4}$$

where n_s^t is the total number of creations in industry s in the first half of year t and ΔUnemp_s is the change in the fraction of unemployed entrepreneurs measured in the SINE survey in industry s between 2002 and 2006.[21] AggGrowth_s is the growth rate over the 2002–2006 period of industry s total value added, which we add to Equation (4) as a natural control.[22]

INSERT TABLE 5 ABOUT HERE

Table 5 reports the results of this estimation. Column (1) includes all unemployed entrepreneurs in the calculation of ΔUnemp_s and shows that the rise in firm creation observed between 2002 and 2006 is significantly more pronounced in industries where the fraction of unemployed entrepreneurs increased the most from 2002 to 2006.[23] Columns (2) and (3) extend the analysis in column (1) by decomposing the industry-level change in unemployed entrepreneurs (ΔUnemp_s) based on whether these entrepreneurs take up the ACCRE subsidy or not. Since ACCRE is a pure subsidy for unemployed entrepreneurs entailing no constraint, those failing to claim the ACCRE subsidy are presumably ill-informed and therefore less likely to be aware of the PARE reform.[24]

[21] We focus on the first semester of 2002 and 2006 because the SINE survey, which we use to compute ΔUnemp_s, only surveys firms created in the first-half of the survey year.

[22] Some industries may naturally grow faster, which would lift the growth in new firm creation. At the same time, these industries lay off fewer workers and thus have fewer potential entrepreneurs that are formerly unemployed, creating a spurious negative correlation between ΔUnemp_s and $\frac{n_s^{2006} - n_s^{2002}}{n_s^{2002}}$. This correlation only arises if entrepreneurs tend to start businesses in industries they have been working in before.

[23] Note that in order to obtain precise estimates of ΔUnemp_s, we restrict the sample to industries that have at least 20 firms in both waves of the SINE survey, which leads us to consider only 195 industries, as opposed to the 290 industries included in our main specification.

[24] These policies are mostly advertised by local unemployment agencies, which likely creates a correlation in the propensity to know about the different programs.

A natural hypothesis is thus that, if the increase in industry-level entrepreneurship post-2002 is, in fact, due to the PARE, this increase should consist of informed entrepreneurs, i.e., mostly those claiming the ACCRE subsidy. In column (2), we thus re-estimate Equation (4), but define ΔUnemp_s using *only* unemployed entrepreneurs claiming the ACCRE subsidy. The estimated β is .28***, confirming that industry-level growth in new firm creation is driven by an increase in unemployed entrepreneurs. In column (3), we instead define ΔUnemp_s using only unemployed entrepreneurs *not taking* the ACCRE subsidy and estimate a small and statistically insignificant β, which is consistent with our causal interpretation that entrepreneurs not claiming ACCRE are indeed unlikely to know about PARE.

Finally, in the last three columns of Table 5 we perform a placebo analysis, which exploits the previous wave of the SINE survey in 1998. Since there was no significant reform favoring business creation by unemployed entrepreneurs over the 1998–2002 period, industry-level shifts in the fraction of unemployed entrepreneurs should *not* explain industry-level increases in firm creation during that period. We re-estimate Equation (4) for this period and show the results in columns (4)–(6) of Table 5. In contrast to the estimation performed on the 2002–2006 period, the estimated β are all insignificant, as hypothesized.

6.3 The Quality of Post-Reform Start-Ups

6.3.1 Job Creation and Exit

We have firmly established that our treatment intensity variable is valid, so we can now examine whether the reform led to a significant change in the characteristics of newly created firms (the second prediction of our model and main purpose of the paper). We first use ex post measures of firm quality: job creation and exit probability. If the main effect of the reform was to draw in individuals of lower ability, start-ups created after the reform should be less likely to create jobs and more likely to exit, particularly in industries with high treatment intensity ("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 or exit (the "experimentation channel").

We estimate Equation (2) using as a dependent variable a firm-level indicator equal to 1 when the firm hires at least one employee between its creation date and the end of the second calendar year after creation. We chose two years since, typically, firms who ever hire start within the first two years. The estimation results are reported on Table 6. While there is an increase in a startup's propensity to hire post-reform (column (1)), we find that firms started in Q4 industries (and Q3 and Q2) do not experience a significant change in the propensity to hire in their first two years relative to new firms started in Q1 industries (columns (2) and (3)). We can reject at the 5%

confidence level the null hypothesis that firms started in Q4 industries have a lower propensity to hire in the first two years (by 2.5 percentage points) than firms started in Q1 industries, which is a small effect since the average propensity to hire in the first two years is 25% (Table 2). Additionally, Table 6 shows that the estimated effect of the reform on Q3 industries is *positive* and also insignificant, so grouping Q3 and Q4 industries together would lead to an even smaller effect. The evidence in Table 6 is overall inconsistent with the view that the reform led to the creation of new firms that are significantly less likely to hire.[25]

INSERT TABLE 6 ABOUT HERE

The second measure of ex post quality we use is the probability of exit. In our sample, 16% of newly-created firms exit in the first two years following creation. This attrition rate is consistent with existing cross-country evidence and is typically interpreted as the failure rate of new firms.[26] In Table 6, columns (3)–(4), we estimate equation (2) using a dummy of exit within two years as our dependent variable. The results are similar to the ones described above for hiring patterns: While there is a significant increase in the probability of exit within the first two years in the post-reform period (column (1)), firms started in Q4 industries (and Q3 and Q2) do not become significantly more likely to exit within two years in the post-reform period, relative to new firms started in Q1 industries (columns (2) and (3)). The estimated effects allow us to reject at the 5% confidence level the null hypothesis that in the post-reform period, firms started in Q4 industries are .6 percentage points more likely to exit within two years than firms started in Q1 industries, relative to the pre-reform period. Given a baseline rate of exit within two years of 16%, the magnitude of such an effect is quite small.

6.3.2 Characteristics of Entrepreneurs

We now provide further evidence that firm quality does not decline after the reform using ex ante measures of entrepreneurial quality: education and self-reported expectation to grow (which we also call "ambition"). Since these variables come from the SINE survey, they are 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 for a firm born in 2002 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

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

[26] 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.

more than five employees (columns (7)–(9)). More educated and ambitious entrepreneurs are more likely to start 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 these ex ante quality measures, and finds none. The empirical strategy is similar to Table 6, but we use our ex ante measures of quality as dependent variables: the probability of having a high school diploma (columns (1)–(3)), the probability of having a college diploma (columns (4)–(6), and the probability of plans to hire in the coming year (columns (7)–(9)). Regression results unanimously show that these measures of quality did not deteriorate in the post-reform period in Q4 industries relative to Q1 industries. The interactions of the post dummy and the Q3 and Q4 indicators are all positive, and even significant at the 5% confidence level when looking at entrepreneurial ambition. Again, the estimated effects allow us to safely reject small, negative effects: For instance, we can reject at the 5% confidence level that, in the post-reform period, the probability that entrepreneurs in Q4 industries have a high school diploma dropped by more than 2.8 percentage points relative to entrepreneurs in Q1 industries.

6.3.3 Marginal Versus Average Effect

The results on firm quality documented above are obtained by comparing the *average* quality of newly created firms across industries following the reform. These averages do not 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 new entrants.

To make this calculation, we make two simplifying assumptions: (1) all firms created in Q1 industries in the post-reform period are created by infra-marginal entrepreneurs and (2) marginal entrepreneurs constitute 100% of the differential entry between Q1 and Q4 industries. These assumptions allow us to re-weight the quality of marginal entrepreneurs based on their propensity in the population of new entrants. 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 increased by $\delta = 14\%$ relative to the least treated industries. Thanks to assumption (2) 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 the regressions give us the change in average quality, Δq, and knowing the fraction of marginal firms $\delta = .14$, we can infer the difference in

observable quality between the marginal and infra-marginal entrants.

Consider for instance the probability of hiring as a measure of quality. Column (2) of Table 6 shows that for this measure, $\Delta q = -0.0089$ and is insignificant. Applying the formula derived above yields a difference in average quality between marginal and infra-marginal entrepreneurs of about $q_m - q_i = -7\%$. Using the same methodology, we find that the average two-year exit rate of marginal entrepreneurs is 10% compared to 17% for infra-marginal ones. Overall, these results reject the hypothesis of a significant decline in entrepreneurial quality due to the PARE reform.

6.3.4 Robustness Check: Disguised Employment

A potential issue in our analysis is that the reform allowed employers and employees to engage in regulatory arbitrage by transforming workers into self-employed contractors who receive unemployment benefits while de facto keeping their previous job. To rule out this channel, we extract from the SINE survey information on the number of customers, and on the existence of business relationships with the entrepreneur's former employer. The results in Appendix Table B.17 show that while the propensity to work with a past employer and the propensity to have only one or two clients seem to have slightly increased in the post-reform period, this increase is not more pronounced in treated industries. The results in Appendix Table B.17 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

This section provides an empirical evaluation of the aggregate effect of the PARE reform. We first investigate the effect of the reform on job creation by new firms, and then shift the focus to small incumbent firms in the same sector. Second, we compare the efficiency of newly created firms to small incumbents.

7.1 Job Creation and Crowding Out

We first estimate equation (1) using industry-level employment data and report the results in Table 8. We use the log of one plus L_{st} as our dependent variable, where L_{st} is the total number of jobs reported in the tax files after two years of existence by all firms created in industry s in month t. This measure thus counts the jobs that *will* be created in two years, and excludes firms that exit before $t + 2$. Since entrepreneurs are not always employees of their firm, we account for this potential source of measurement error in two ways. In columns (1) and (2), we make the assumption that the entrepreneur is never a wage earner and add one to reported firm employment.

In columns (3) and (4), we make the conservative assumption that all entrepreneurs are already counted as employees of their own firm.

INSERT TABLE 8 ABOUT HERE

Independent of how we account for the entrepreneur's employment in the firm, we find that the reform had a large impact on aggregate job creation by newly-created firms. In columns (1) and (2) we see that the number of jobs created by new firms within the first two years of existence increased by 21 percentage points in the most treated industries (Q4) relative to the least treated industries (Q1). Focusing on Q4 industries, we find that about 2,000 new jobs per month are created in the post-reform period by these newly-created firms. When we repeat the estimation in columns (3) and (4) with the more conservative assumption that the tax files already include the entrepreneur as an employee, we obtain a smaller but still significant estimate of 750 new jobs created monthly in Q4 industries.[27] These results suggest that the reform led to the direct creation of jobs by newly-created firms ranging from 9,000 to 24,000 new jobs every year—note that this estimate excludes the effect of the reform on Q2 and Q3.

To investigate possible crowding out of existing jobs induced by the PARE reform, we run the same regression using employment growth of *incumbent* firms as a dependent variable. We report the results in columns (5)–(8) of Table 8. We 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. In columns (5) and (6), we first focus on small incumbents with five or fewer employees. These small incumbents are more likely to be competing directly with the new entrants, either in the product or the labor market. In columns (7) and (8), we compute the growth rate of total employment at large incumbents with more than five employees.[28]

Table 8 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 Q4 industries relative to Q1 industries (columns (1) and (2)). This result is consistent with competitive dynamics whereby newly created firms partially crowd out existing small firms, as illustrated in the third prediction of Proposition 1. In contrast, Table 8, columns (3) and (4) show that employment growth at large incumbent firms does not significantly change following the reform in Q4 relative to Q1 industries (insignificant 0.9 percentage points estimate in column (4)).

[27] 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 generated 43 jobs on average, while the aggressive assumption led to 118 jobs created monthly.

[28] 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. Note that the sample in these regressions stops in 2007 while before it stopped in 2005 since we need to observe employment counts two years after a firm's creation and 2007 is the last year in our data.

Based on the estimates in column (3), we can quantify the number of jobs that are being displaced following the large entry of new firms induced by the reform. Since the average industry in Q4 has 5,196 employees working for small incumbents (Table 2, Panel D), the industry-level effect of the reform on small incumbent employment 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 above. While these numbers are somewhat imprecisely estimated, they suggest that crowding out effects are of the same order of magnitude as the jobs created by the reform.

In Table 8, columns (5) and (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 incumbent 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) and (4) of Table 8 have shown that the reform had no effect on large incumbents' employment. Columns (5) and (6) show that in the post-period, Q4 industries do experience larger growth (by 2 percentage points) in employment coming from entrepreneurial firms and small incumbents relative to Q1 industries. While this interaction coefficient is large, it is not statistically significant, so we cannot reject the null hypothesis that the employment decline at small incumbents equals the rise in employment due to newly-created firms.

7.2 Efficiency

Given the significant reallocation of resources highlighted in the previous section, we now turn to allocative efficiency by comparing the productivity of newly created firms to that of small incumbents.

Table 10 estimates the following equation:

$$Y_{ist} = \sum_{k=1}^{4} a_k \cdot Q_s^k \times \text{post}_t \times \text{New firm}_{ist} + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times \text{New firm}_{ist} + \gamma \cdot \text{New firm}_{ist} \times \text{post}_t + \zeta \cdot \text{New firm}_{ist} + \delta_{st} + \epsilon_{ist}, \quad (5)$$

where Y_{ist} is a measure of productivity for firm i created in industry s in month t. 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)).[29] We also restrict the sample to two categories of firms: (1) entrepreneurial firms created in year t and (2) small incumbent firms that are most likely to be 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 (5) 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 10 ABOUT HERE

Columns (1), (3), and (5) of Panel A in Table 10 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; and 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 estimations also show that this productivity advantage of newly-created firms does not change in the post-reform period. The interaction of the new firm dummy with the post dummy is quantitatively small and statistically insignificant. Of course, this result could mask a relative drop in the productivity of newly created firms in Q4 industries, and a relative increase in the productivity of newly created firms in Q1 industries. However, columns (2), (4), and (6) show this is not the case. The larger productivity observed for newly-created firms does not increase differentially in the post-reform period for firms in Q1, Q2, Q3, and Q4 industries.

In Panel B, Table 10, we repeat these tests using total factor productivity (TFP). TFP is more standard but we believe less directly applicable to our set of very small firms. This is why we present this set of regressions as robustness. $TFP1$ is obtained as the residual of the following regression, where i is a firm, s is its industry, and t is the year of observation. 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 1 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 (5) comparing the productivity

[29] 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.

of new entrants vs. incumbent firms using these TFP measures as dependent variables and report the results in Panel B, Table 10. 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 significantly in response to the reform.

Overall, despite low aggregate employment gains (Section 7.1), the evidence in this section suggests that the significant reallocation of labor from small incumbent firms to new ventures led to significant productivity gains at the industry level. The next section discusses these estimates quantitatively.

8 Cost-Benefit Analysis of the Reform

This section proposes a cost-benefit analysis of the PARE 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 entrepreneurs, and savings on unemployment benefits.

8.1 Job Reallocation

The first channel is that more productive jobs are created, which leads to €350m 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.[30] We showed in Section 7.2 that because new firms are more productive than incumbents, job reallocation creates additional value added. Our most conservative estimation suggests that about 10,000 jobs are reallocated annually. Value added per worker in these new jobs is higher by about €7,000 (see Table 10). Finally, we assume that, on average, these new firms survive five years.[31] 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. Also, these numbers are obtained by focusing on the effect on Q4 only, thus leaving out half of the industries.

[30] While the overall employment effect uncovered in column (6) of Table 9 is positive, it is not significantly different from 0, so that we use 0 as a conservative estimate.

[31] 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.

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 to which they are entitled. To calculate the corresponding 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.[32] 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 to which the entrepreneur is entitled, 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 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

[32] 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.

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 her 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 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. The reform led to a large increase in firm creation. Surprisingly, the reform did not lead to a significant deterioration in the composition of the pool of entrepreneurs. While most firms start out small at creation, they show no differences in survival rates, growth, or likelihood to hire workers in the 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. Newly created firms are estimated to create between 9,000 and 24,000 jobs annually. These results are in line with the experimentation view; the reform allows talented but potentially more risk averse people to explore their success with self-employment. We do not find that the downside insurance provided by the reform leads to significant adverse selection in who becomes self-employed.

The paper also emphasizes the importance of going beyond a partial equilibrium analysis of

these types of reforms. We document that the large entry of new firms had strong crowding-out effects, especially on small incumbents, which experienced a reduction in employment growth due to the reform. This crowding-out effect is of the same order of magnitude as the direct creation effect, so the overall effect on job creation is quite small. At the same time, we show that newly created firms are significantly 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, while €100 million are transferred from the unemployment agency to unemployed entrepreneurs. Accounting for greater industry dynamism and non-pecuniary benefits from shorter unemployment spells would lead to higher aggregate benefits.

References

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

Bertrand, Marianne, Antoinette Schoar, and David Thesmar, "Banking Deregulation and Industry Structure: Evidence from the French Banking Reforms of 1985," *Journal of Finance*, 04 2007, *62* (2), 597–628.

Bianchi, Milo and Matteo Bobba, "Liquidity, Risk, and Occupational Choices," *The Review of Economic Studies*, 2013, *80* (2), 491–511.

Blanchflower, David G and Andrew J Oswald, "What Makes an Entrepreneur?," *Journal of Labor Economics*, January 1998, *16* (1), 26–60.

Branstetter, Lee, Francisco Lima, Lowell J. Taylor, and Ana Venâncio, "Do Entry Regulations Deter Entrepreneurship and Job Creation? Evidence from Recent Reforms in Portugal," *Economic Journal*, 06 2014, *124* (577), 805–832.

Bruhn, Miriam, "License to Sell: The Effect of Business Registration Reform on Entrepreneurial Activity in Mexico," *The Review of Economics and Statistics*, February 2011, *93* (1), 382–386.

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

Caves, Richard E., "Industrial Organization and New Findings on the Turnover and Mobility of Firms," *Journal of Economic Literature*, December 1998, *36* (4), 1947–1982.

Cetorelli, Nicola and Philip E. Strahan, "Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets," *Journal of Finance*, 02 2006, *61* (1), 437–461.

Cole, Shawn, "Financial Development, Bank Ownership, and Growth: Or, Does Quantity Imply Quality?," *The Review of Economics and Statistics*, February 2009, *91* (1), 33–51.

de Mel, Suresh, David McKenzie, and Christopher Woodruff, "Returns to Capital in Microenterprises: Evidence from a Field Experiment," *The Quarterly Journal of Economics*, November 2008, *123* (4), 1329–1372.

Desai, Mihir, Paul Gompers, and Josh Lerner, "Institutions, Capital Constraints and Entrepreneurial Firm Dynamics: Evidence from Europe," NBER Working Papers 10165, National Bureau of Economic Research, Inc December 2003.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-De-Silanes, and Andrei Shleifer, "The Regulation Of Entry," *The Quarterly Journal of Economics*, February 2002, *117* (1), 1–37.

Evans, David S and Boyan Jovanovic, "An Estimated Model of Entrepreneurial Choice under Liquidity Constraints," *Journal of Political Economy*, August 1989, *97* (4), 808–27.

Goux, Dominique, Eric Maurin, and Barbara Petrongolo, "Worktime Regulations and Spousal Labor Supply," *American Economic Review*, January 2014, *104* (1), 252–76.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda, "Who Creates Jobs? Small versus Large versus Young," *The Review of Economics and Statistics*, May 2013, *95* (2), 347–361.

Hamilton, Barton H., "Does Entrepreneurship Pay? An Empirical Analysis of the Returns to Self-Employment," *Journal of Political Economy*, June 2000, *108* (3), 604–631.

Holtz-Eakin, Douglas, David Joulfaian, and Harvey S. Rosen, "Entrepreneurial Decisions and Liquidity Constraints," *RAND Journal of Economics*, Summer 1994, *25* (2), 334–347.

_ , _ , and Harvey S Rosen, "Sticking It Out: Entrepreneurial Survival and Liquidity Constraints," *Journal of Political Economy*, February 1994, *102* (1), 53–75.

Hurst, Erik and Annamaria Lusardi, "Liquidity Constraints, Household Wealth, and Entrepreneurship," *Journal of Political Economy*, April 2004, *112* (2), 319–347.

_ and Benjamin Wild Pugsley, "What do Small Businesses Do?," *Brookings Papers on Economic Activity*, 2011, *43* (2 (Fall)), 73–142.

Jovanovic, Boyan, "Selection and the Evolution of Industry," *Econometrica*, May 1982, *50* (3), 649–70.

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

Kerr, William and Ramana Nanda, "Financing Constraints and Entrepreneurship," NBER Working Papers 15498, National Bureau of Economic Research, Inc November 2009.

Kerr, William R. and Ramana Nanda, "Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship," *Journal of Financial Economics*, October 2009, *94* (1), 124–149.

Kihlstrom, Richard E and Jean-Jacques Laffont, "A General Equilibrium Entrepreneurial Theory of Firm Formation Based on Risk Aversion," *Journal of Political Economy*, August 1979, *87* (4), 719–48.

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

Landier, Augustin and David Thesmar, "Financial Contracting with Optimistic Entrepreneurs," *Review of Financial Studies*, January 2009, *22* (1), 117–150.

Lucas, Robert E., "On the Size Distribution of Business Firms," *Bell Journal of Economics*, Autumn 1978, *9* (2), 508–523.

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

Moffitt, Robert, "Unemployment insurance and the distribution of unemployment spells," *Journal of Econometrics*, April 1985, *28* (1), 85–101.

Moskowitz, Tobias J. and Annette Vissing-Jørgensen, "The Returns to Entrepreneurial Investment: A Private Equity Premium Puzzle?," *American Economic Review*, September 2002, *92* (4), 745–778.

Mullainathan, Sendhil and Philipp Schnabl, "Does Less Market Entry Regulation Generate More Entrepreneurs? Evidence from a Regulatory Reform in Peru," in "International Differences in Entrepreneurship" NBER Chapters, National Bureau of Economic Research, Inc, 2010, pp. 159–177.

Nanda, Ramana, "Cost of External Finance and Selection into Entrepreneurship," Harvard Business School Working Papers 08-047, Harvard Business School January 2008.

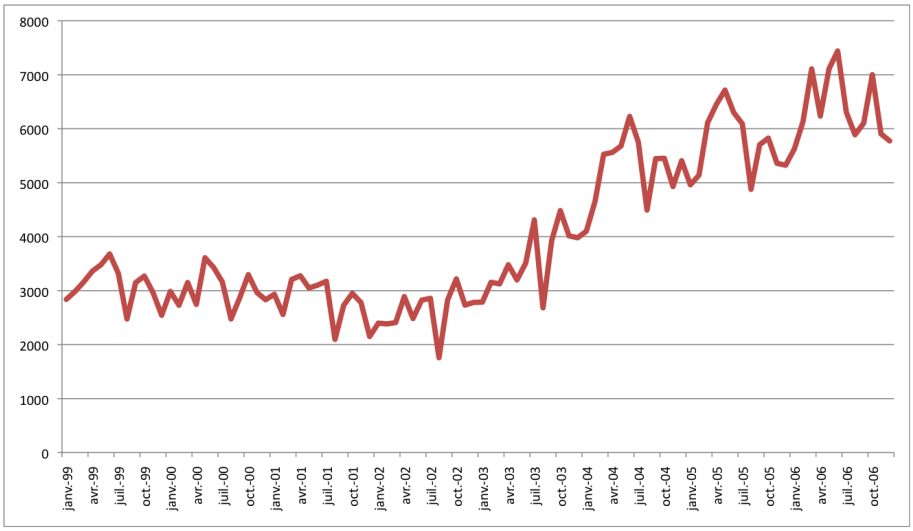
Rieg, Christian, "Forte Hausse des Créations d'Entreprises en 2003," *INSEE Première*, 2004, *994*.

Schmalz, Martin C, David Alexandre Sraer, and David Thesmar, "Housing Collateral and Entrepreneurship," *Journal of Finance, Forthcoming*, 2015.

Schoar, Antoinette, "The Divide between Subsistence and Transformational Entrepreneurship," in "Innovation Policy and the Economy, Volume 10" NBER Chapters, National Bureau of Economic Research, Inc, 2010, pp. 57–81.

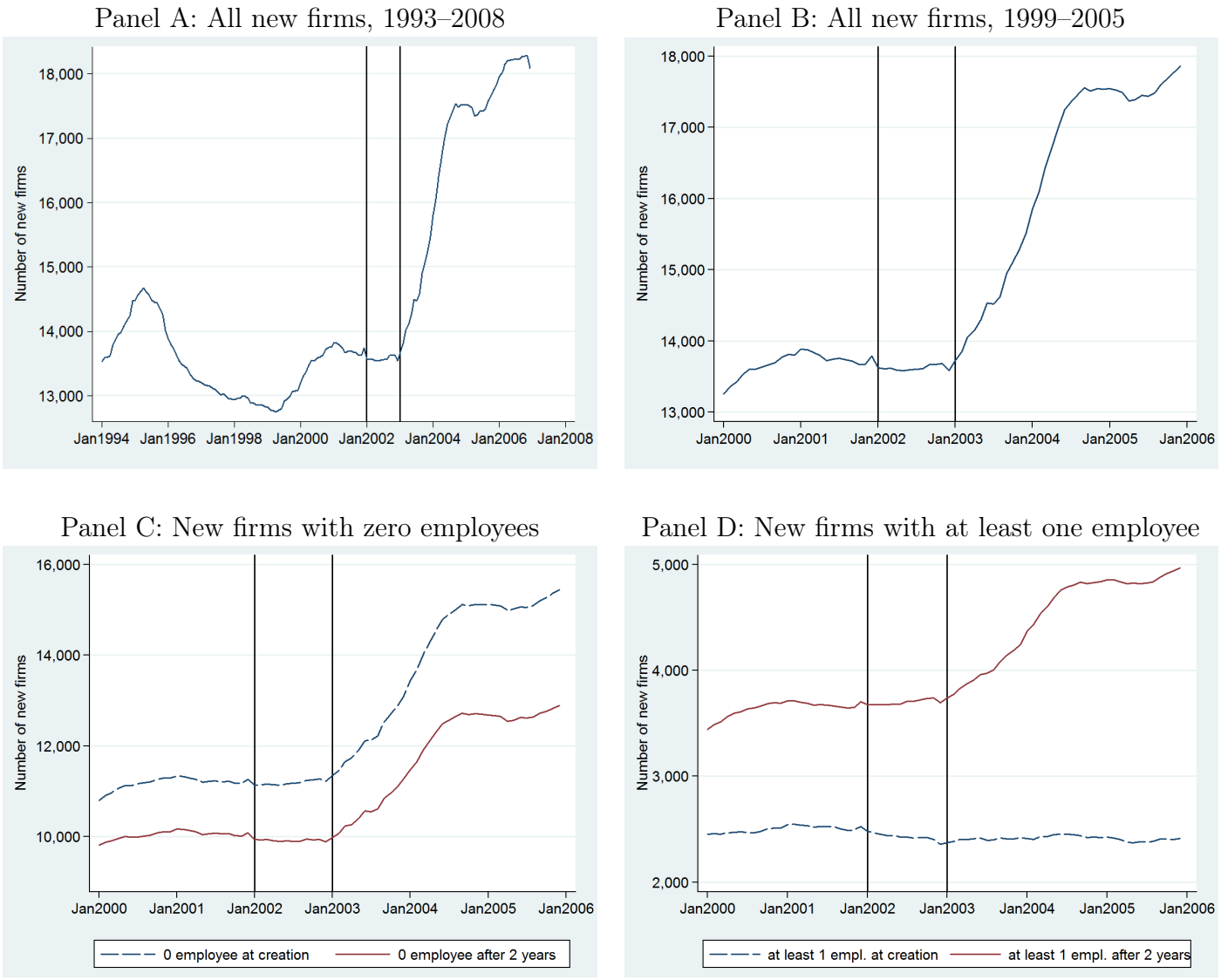
Solon, Gary R, "Work Incentive Effects of Taxing Unemployment Benefits," *Econometrica*, March 1985, *53* (2), 295–306.

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



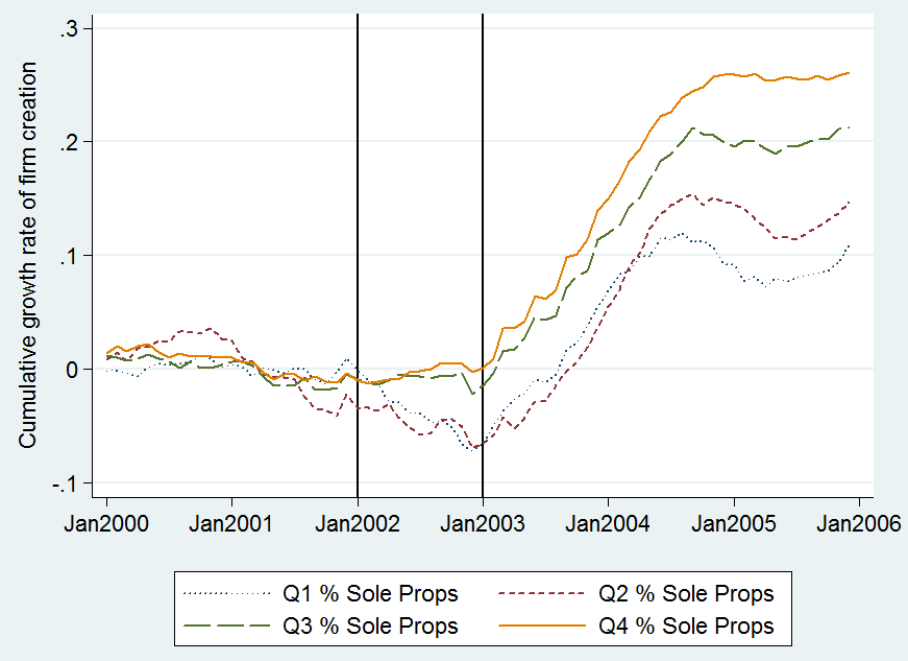
Source: French Ministry of Labor. Note: This figure shows the monthly number of individuals receiving the ACCRE subsidy, which is granted to unemployed individuals creating a new firm. The sample period covers 1999 to 2006.

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 zero employees (dotted blue) and the number of new firms with zero employees two years after creation including firms that have exited (plain red). Panel D plots the number of new firms started with at least one employee (dotted blue) and the number of new firms with at least one employee two years after creation (plain red). The vertical dark lines correspond to 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-reform period.

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 five employees or less and which are not reported to be part of a conglomerate; large incumbents are incumbents with more than five 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)–(3) the dependent variable is the log of total industry sales. In columns (4)–(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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table 5: Firm Creation Growth and the Increase in Unemployed Entrepreneurs

	2002–2006 entry growth			1998–2002 entry growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Change in % former unemployed	.23**			-.024		
	(.12)			(.084)		
Change in % former unemployed ACCRE takers		.28***			-.066	
		(.095)			(.075)	
Change in % former unemployed non-ACCRE takers			-.033			.077
			(.24)			(.12)
Aggregate sector growth rate	.39***	.42***	.37***	.22**	.22**	.21**
	(.11)	(.11)	(.11)	(.091)	(.091)	(.092)
Constant	.073*	.025	.11***	-.014	-.013	-.008
	(.044)	(.049)	(.04)	(.021)	(.021)	(.023)
Observations	195	195	195	195	195	195
R-squared	.071	.093	.053	.03	.033	.032

Source: Creation files and SINE surveys from 1998, 2002, and 2006. In columns (1)–(3) (resp. columns (4)–(6)), the dependent variable is the industry growth rate of the number of new firms created from 2002S1 to 2006S1 (resp. from 1998S1 to 2002S1). In column (1) (column (4)), the explanatory variable is the industry change in the fraction of formerly unemployed individuals among all entrepreneurs from 2002S1 to 2006S1 (from 1998S1 to 2002S1). In column (2) (column (5)), the explanatory variable is the industry change in the fraction of formerly unemployed individuals receiving the ACCRE subsidy among all entrepreneurs from 2002S1 to 2006S1 (from 1998S1 to 2002S1). In column (3) (column (6)), the explanatory variable is the industry change in the fraction of formerly unemployed individuals not receiving the ACCRE subsidy among all entrepreneurs from 2002S1 to 2006S1 (from 1998S1 to 2002S1). All regressions include the contemporaneous growth rate in aggregate industry value added. Robust standard errors (in parentheses) are reported. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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)–(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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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)–(3) the dependent variable is the log of one plus the number of employees four years after creation. In columns (4)–(6) the dependent variable is a dummy equal to 1 if the firm has at least one employee four years after creation. In columns (7)–(9) the dependent variable is a dummy equal to 1 if the firm has at least five 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 surveys. 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table 8: 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table 9: 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 five employees or less in year $t-1$, and are not reported to be part of a conglomerate 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table 10: 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 one employee). In columns (3) to (4), the dependent variable is value added divided by 1 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table 10 (continued)

Panel B: Total Factor Productivity (TFP)

	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 columns (1) and (2), the dependent variable is TFP1, the residual of a Cobb-Douglas production function estimated industry by industry. In columns (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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 an entrepreneur if and only if:

$$\begin{aligned} & q \log(p_s^{\frac{1}{1-\beta}} A\theta - c_s) + (1-q)\log(b) \geq \ln(1) \\ \Leftrightarrow \qquad & A\theta \geq \theta_s \equiv p_s^{-\frac{1}{1-\beta}} \left(b^{-\frac{1-q}{q}} + c_s \right), \end{aligned} \tag{6}$$

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:

$$\frac{p_T^{\frac{\beta+\sigma(1-\beta)}{\sigma(1-\beta)}}}{\theta_T^{\frac{\phi-1}{\sigma}}} = \frac{p_C^{\frac{\beta+\sigma(1-\beta)}{\sigma(1-\beta)}}}{\theta_C^{\frac{\phi-1}{\sigma}}} \equiv k. \tag{7}$$

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 an increase in b. Differentiating (6), 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 (7), 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:

$$\Delta\log\theta_s=-\frac{\sigma}{\phi+(\sigma-1)(1-\beta)}\Delta\log(k)-\frac{1+(\sigma-1)(1-\beta)}{\phi+(\sigma-1)(1-\beta)}\frac{\frac{1-q}{q}}{1+c_s b^{\frac{1-q}{q}}}\Delta\log(b). \tag{8}$$

We can write our second 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:*

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

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 (8) 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 (8) to obtain our third 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. However, when potential entrepreneurs are 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 fourth prediction:

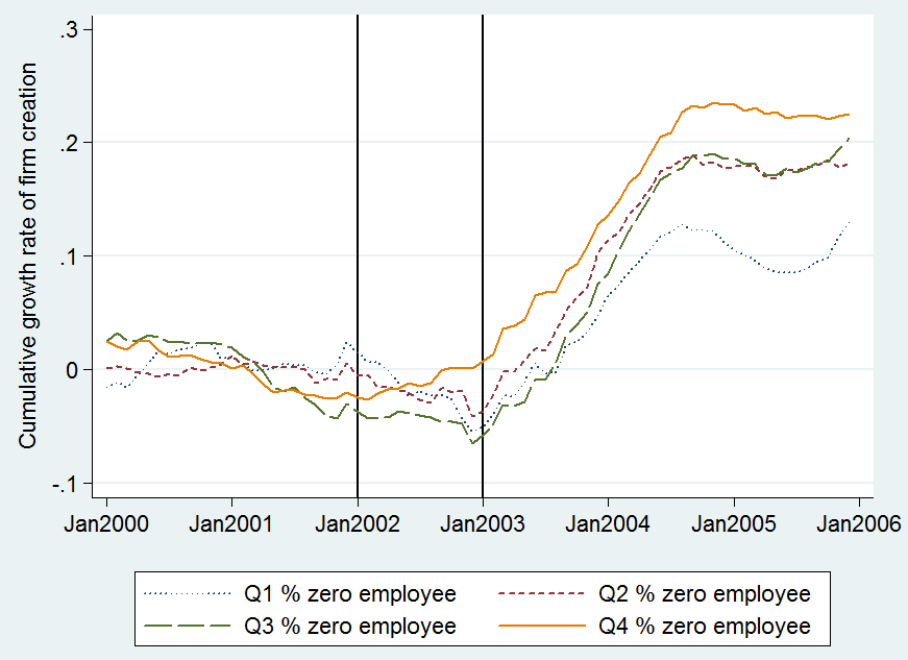
Proposition 4. *Assume the reform leads to a marginal increase in b by Δb. Then, the DD estimate of the average size of "incumbents" is given by:*

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

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 five employees or less and which are not reported to be part of a conglomerate; large incumbents are incumbents with more than five 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)–(3) the dependent variable is the log of total industry sales. In columns (4)–(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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 (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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 one 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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)–(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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 surveys. 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 1 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 1 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 five employees or less in year $t-1$, and are not reported to be part of a conglomerate 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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 one employee). In columns (3) and (4), the dependent variable is value added divided by 1 plus number of employees. In columns (5) and (6), the dependent variable is sales divided by 1 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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-reform period (1999–2001) and the post-reform 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

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: 2002 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) restrict 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, respectively.

Table B.14: Firm Creation: Treated vs. Control, Excluding 2002

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.15***	.086***	-.065	-.16
	(.017)	(.032)	(.041)	(.1)
Q2 % Sole Props × POST		.025	.07	.064
		(.05)	(.057)	(.057)
Q3 % Sole Props × POST		.09**	.17***	.17***
		(.045)	(.05)	(.05)
Q4 % Sole Props × POST		.13***	.22***	.22***
		(.045)	(.049)	(.048)
Industry capital intensity × POST				.039
				(.035)
Industry growth × POST				-.024
				(.042)
Industry capital intensity × Trend				-.014
				(.01)
Industry growth × Trend				.011
				(.014)
Constant	3.2***	3.2***	2.2***	2.2***
	(.018)	(.018)	(.25)	(.24)
Treatment-specific trend	No	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	20,880	20,880	20,880	20,880
R-squared	.92	.92	.92	.92

Source: Firm registry from the French Statistical Office. Sample: 290 industries, 1999–2001 and 2003–2005, 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 2003 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table B.15: Firm Creation: Treated vs. Control, Including 2002 in the pre-reform window

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.16***	.099***	.065**	.086
	(.016)	(.029)	(.025)	(.063)
Q2 % Sole Props × POST		.024	.061*	.058
		(.046)	(.036)	(.036)
Q3 % Sole Props × POST		.079*	.096***	.095***
		(.041)	(.033)	(.033)
Q4 % Sole Props × POST		.12***	.13***	.12***
		(.041)	(.03)	(.029)
Industry capital intensity × POST				-.0019
				(.022)
Industry growth × POST				-.031
				(.026)
Industry capital intensity × Trend				-.000017
				(.000021)
Industry growth × Trend				.000035
				(.000029)
Constant	3.2***	3.2***	3***	3***
	(.016)	(.017)	(.19)	(.19)
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 1999–2002 period and equal to 1 from 2003 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table B.16: Firm Creation: Treated vs. Control, Excluding 2005

	Number of firms created			
	(1)	(2)	(3)	(4)
POST	.075***	.025	-.17***	-.3***
	(.012)	(.026)	(.033)	(.078)
Q2 % Sole Props × POST		.013	.055	.05
		(.037)	(.045)	(.044)
Q3 % Sole Props × POST		.072**	.13***	.13***
		(.034)	(.039)	(.038)
Q4 % Sole Props × POST		.11***	.15***	.16***
		(.034)	(.041)	(.041)
Industry capital intensity × POST				.047*
				(.024)
Industry growth × POST				-.0032
				(.035)
Industry capital intensity × Trend				-.017*
				(.0089)
Industry growth × Trend				.0069
				(.014)
Constant	3.2***	3.2***	.99***	.99***
	(.017)	(.017)	(.24)	(.24)
Treatment-specific trend	No	No	Yes	Yes
Month-of-the-year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	20,880	20,880	20,880	20,880
R-squared	.92	.92	.92	.92

Source: Firm registry from the French Statistical Office. Sample: 290 industries, 1999–2004, 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 2004. 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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.

Table B.17: Entrepreneurs' Relation with Former Employer across Industries

	Relation with former employer			One or two clients			Relation with 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 surveys. 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)–(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)–(6), the dependent variable is a dummy variable equal to 1 if the entrepreneur reports to have one or two customers. In columns (7)–(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 (in parentheses) are clustered at the industry level. *, **, and *** mean statistically different from zero at 10, 5, and 1% levels of significance, respectively.