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Can Unemployment Insurance Spur Entrepreneurial Activity? Evidence from France

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ABSTRACT

We evaluate the effect of downside insurance on self-employment. We exploit a large-scale reform of French unemployment benefits that insured unemployed workers starting businesses. The reform significantly increased firm creation without decreasing the quality of new entrants. Firms started postreform were initially smaller, but their employment growth, productivity, and survival rates are similar to those prereform. New entrepreneurs' characteristics and expectations are also similar. Finally, jobs created by new entrants crowd out employment in incumbent firms almost one-for-one, but have a higher productivity than incumbents. These results highlight the benefits of encouraging experimentation by lowering barriers to entry.

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

OVER THE LAST TWO DECADES, policymakers and academics alike have embraced the idea that reducing barriers to self-employment and entrepreneurship is important for promoting job creation.[1] The primary focus of empirical research has been on understanding how such barriers affect the *level* of entrepreneurial activity (i.e., the number of new firms created). Many recent studies find evidence of significant heterogeneity among potential entrepreneurs'

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

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ability to grow, risk tolerance, ambition, and even 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 *quality* of new entrepreneurs. On the one hand, if individuals have private information about their entrepreneurial abilities, lower barriers to entry may decrease the quality of the average entrepreneur. On the other hand, reducing start-up costs can lead skilled but risk-averse individuals to enter self-employment (Kihlstrom and Laffont (1979)) by allowing them to experiment and learn about their ability (as in Jovanovic (1982), Caves (1998), Manso (2011)).

In this paper, we empirically investigate how large reductions in the cost of entry affect selection into entrepreneurship and allocative efficiency more generally. To do so, we exploit a large-scale reform of the French unemployment insurance (UI) system that provides downside insurance to unemployed workers starting a business. To derive testable predictions, we first develop an equilibrium model of occupational choice that features risk-averse individuals with heterogeneous entrepreneurial skills. Individuals decide between wage employment or self-employment. The success of the firm is determined by a combination of skill (known in advance) and chance. In the model, providing downside insurance to entrepreneurs increases entry, but the number and quality of new businesses depend on the dispersion of talent. When talent is relatively heterogeneous, decreasing downside risk has a modest effect on new firm creation, reduces entrepreneurial quality, and leads to a smaller reduction in the size of incumbent firms competing for the same resources. We refer to this explanation as the selection view. In contrast, when the talent distribution is more homogeneous, providing downside insurance has a pronounced effect on new firm creation, a modest effect on entrepreneurial quality, and a significant crowding-out effect on incumbents. In the latter case, facilitating entry increases allocative efficiency by allowing relatively skilled but risk-averse individuals to become entrepreneurs. Importantly, the model requires that workers cannot engage in risk-sharing contracts with outside financiers (e.g., through revolving credit or outside equity), so that the provision of downside insurance by the UI system generates real benefits for unemployed workers. In this sense, our model, and hence our interpretation of the data, is consistent with the reform completing a missing insurance market (Rampini and Viswanathan (2010)). While the provision of downside insurance may also relax new firms' financial constraints, for example, by allowing entrepreneurs to pledge future unemployment benefits, we reject this alternative interpretation in the data.

We empirically evaluate these trade-offs by exploiting a reform that facilitated the transition of unemployed individuals into self-employment—*Plan d'Aide au Retour à l'Emploi* (PARE). Prior to the PARE, unemployed workers starting a business would lose all access to their UI benefits. After the reform,

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

the UI agency filled the gap between unemployment benefits and realized entrepreneurial income, thereby offering temporary protection against potential losses from entrepreneurship. 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. Our identification strategy relies on the heterogeneity across industries in "exposure" to the reform. Unemployed individuals are empirically more likely to start zero-employee firms and/or register as sole proprietorships. Accordingly, we define exposure to the reform as the fraction of sole proprietors among all newly created firms in an industry, where we measure this "treatment intensity" in the years preceding the reform.[3] Our empirical strategy is akin to a difference-in-difference analysis that compares changes in the number and characteristics of newly created firms following the adoption of PARE across industries with different treatment intensity. Our identifying assumption is that absent the reform, the dynamics of business creation would not have been systematically related to industry treatment intensity.[4]

Our first empirical analysis establishes that the reform significantly boosted new business creation by unemployed workers. Relative to the prereform period, the registration of new firms in the postreform period is 14 percentage points higher in industries belonging to the top quartile of treatment intensity, relative to industries in the bottom quartile. This estimate is robust to a variety of robustness checks, in particular to alternative definitions of treatment intensity or event window and to controls for industries exposure to business cycle shocks. We also find support for our causal interpretation of this estimate by showing that, in the cross section of industries, the postreform increase in new firm creation strongly correlates with the entry of *unemployed* entrepreneurs.

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

[3] In Internet Appendix III, we show that our analysis is robust to defining treatment intensity as the fraction of firms with zero employees among newly created firms in an industry. The Internet Appendix is available in the online version of the article on *The Journal of Finance* website.

[4] We explore the validity of this assumption by conducting numerous robustness checks. In particular, our relatively high-frequency data allow us to include industry-specific trends in the regression analysis. Including these trends does not affect our main estimates, which provides support for the parallel trends assumption.

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 point decline in employment among small incumbents, which are more likely to compete in the product and labor markets with these new entrants. This crowding-out effect among small incumbents is economically large, as it mostly offsets the direct effects of the reform on employment creation by start-ups.[5] 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 shrinking incumbent firms. This productivity differential does not decrease after the reform. Overall, while the reform led to small employment gains in aggregate—as jobs created by new firms mostly crowded out existing jobs at incumbent firms—it led to a reallocation of resources from less productive incumbents to more productive young firms, contributing positively to aggregate productivity (in a similar vein, see also Adelino, Ma, and Robinson (2017)).

Related Literature

Our results make two novel contributions to the existing literature on barriers to entry into entrepreneurship: (i) we provide detailed microevidence on the composition of entrepreneurs who enter self-employment when barriers to enter are relaxed and (ii) we document how removing barriers to entry affects incumbent firms. Previous literature looks at cross-country differences in barriers to entry and their aggregate implications for entry rates (Djankov et al. (2002), Desai, Gompers, and Lerner (2003), Klapper, Laeven, and Rajan (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 (Mullainathan and Schnabl (2010), Bruhn (2011), Branstetter et al. (2014)).

Our paper also contributes to the literature on selection into entrepreneurship (Kihlstrom and Laffont (1979), Blanchflower and Oswald (1998), Hamilton (2000), Moskowitz and Vissing-Jørgensen (2002), Hurst and Pugsley (2011)). These papers document 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

[5] These results bear some similarity to the literature on financial reforms, which also shows that increased entry is detrimental to incumbent firms. See Cetorelli and Strahan (2006), Bertrand, Schoar, and Thesmar (2007), and Kerr and Nanda (2009a).

entrepreneurship.[6] Closest to our paper is Gottlieb, Townsend, and Xu (2016), who study the effect of an extension of protected employment leave to one year for female employees who give birth. Like us, they find that downside protection promotes transition into self-employment and that these newly created firms are not less likely to create jobs.

Finally, our paper is related to the vast literature that examines how unemployment benefits distort labor supply, and in particular unemployment duration (Moffitt (1985), Solon (1985), Katz and Meyer (1990), Card and Levine (2000), among many others). Relative to these papers, our contribution highlights an often-ignored distortionary effect of UI on the transition into self-employment. In the same way that UI can reduce the incentives of unemployed workers to find a new job, the risk of losing unemployment benefits can reduce the incentives of unemployed individuals to start a new firm and create their own job. Our results show that this margin is quantitatively large.

The rest of the paper is organized as follows. We describe the reform in Section I. We develop our simple economic framework in Section II, and we discuss the data and the empirical strategy in Sections III and IV. We report results on the direct effect of the reform on the number and quality of new firms in Section V and on the aggregate effect of the reform on employment and productivity in Section VI. Finally, in Section VII, we conclude.

I. The Reform and Institutional Details

A. *Description of the PARE Reform*

Before describing the reform, it is important to define what we refer to as entrepreneurs in the paper. An entrepreneur is an individual who registers a legal structure (sole proprietorship or LLC) to receive entrepreneurial income. Many of these entrepreneurs are "self-employed," in the sense that they have no employee beyond themselves. In France, *all* self-employed individuals *must* be "entrepreneurs" according to this definition—it is illegal to receive payments without a formal legal structure, even for low-skill/low-scale activities. We therefore measure entrepreneurship using the number of legal structures created. Note that entrepreneurs (whether self-employed or not) do *not* have access to the public UI scheme that we study here.[7] Thus, the reform described below did *not* mechanically affect (i) the propensity to create a firm conditional on being self-employed (it is illegal not to) or (ii) the eligibility of self-employed/entrepreneurs to enroll in the UI system (such individuals are not eligible).

[6] See, among others, Evans and Jovanovic (1989), Holtz-Eakin, Joulfaian, and Rosen (1994a, b), Hurst and Lusardi (2004), de Mel, McKenzie, and Woodruff (2008), Kerr and Nanda (2009b), Bianchi and Bobba (2013), Adelino, Schoar, and Severino (2015), and Schmalz, Sraer, and Thesmar (2017).

[7] A private, optional UI system exists for entrepreneurs, but the participation rate is extremely low and it was not affected by the reform we study.

The PARE reform (*Plan d'Aide au Retour à l'Emploi*) consisted of a new agreement between labor unions and employer organizations.[8] The main objective of this agreement was to make unemployment benefits more generous in exchange for tighter supervision of unemployed individuals and lower contributions of firms to the UI body (Crépon, Dejemeppe, and Gurgand (2005)). In addition to changes in unemployment benefits, an important provision of this new agreement was to reduce the implicit disincentives for unemployed workers to start a new business. The agreement was signed by unions in January 2001 and became effective on July 1 (for the text, see UNEDIC (2001)). However, its provisions, as described below, were not advertised before the beginning of 2002.

Turning to its provisions, the PARE reform allows unemployed entrepreneurs to claim unemployment benefits in the 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 allows formerly unemployed entrepreneurs to retain their rights to the remaining unemployment benefits for up to three years if their business fails.[9] Second, the reform also stipulates that unemployed entrepreneurs can supplement their income with unemployment benefits. However, the income derived from their entrepreneurial activity crowds out unemployment benefits one-for-one: each additional euro of income reduces unemployment benefits by exactly 1 euro, until unemployment benefits are zero (Rieg (2004)). As a result, an unemployed entrepreneur has unearned benefits if she generates positive entrepreneurial income. Finally, these unearned benefits are not voided but remain actionable for up to three years after the beginning of the unemployment spell.[10]

In sum, following the PARE reform, unemployed individuals who decide to start a business are guaranteed to receive an amount equal to their unemployment benefits for at least two years and up to three years. Because entrepreneurial income crowds out unemployment benefits one-for-one, the implicit subsidy under this new regime comes only through downside insurance. For an unemployed entrepreneur certain to generate more income than its unemployment benefits, the reform does not change anything: before the reform, such an entrepreneur would not be eligible to unemployment benefits, while after the reform, she would be eligible but would not earn any additional income since her entrepreneurial income exceeds her unemployment benefits. Similarly, an unemployed individual certain to generate entrepreneurial income lower than her unemployment benefits would have no incentive to start

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

[9] See Articles 1-5 in UNEDIC (2001).

[10] Each month, the unemployment agency uses the daily preunemployment wage, w, as a benchmark. It then divides monthly entrepreneurial income by the daily wage, w, to obtain the number of days, d, in the months in which the jobless person receives the equivalent of her former salary. The agency then pays unemployment benefits based on $28 - d$ days of unemployment. However, the individual retains the "rights" to unpaid unemployment benefits corresponding to d worked days, which she can claim for up to three years.

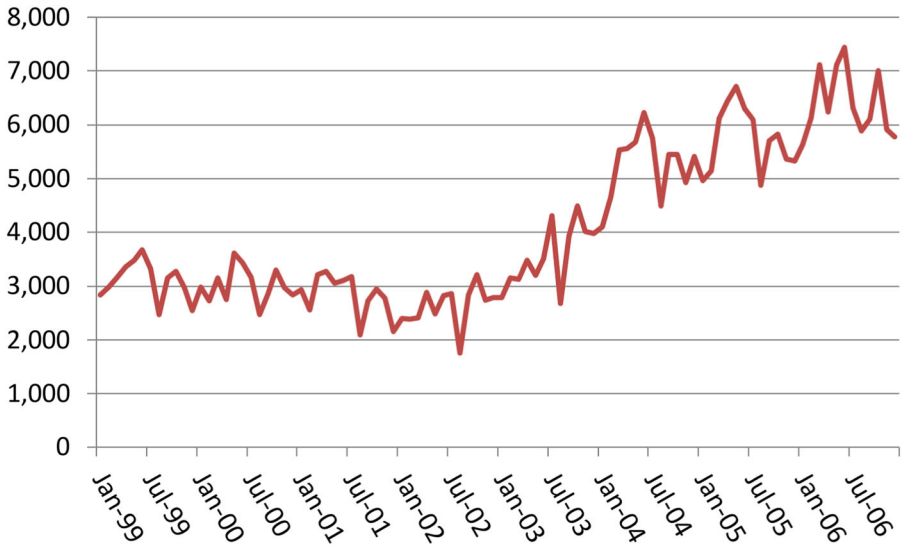

Figure 1. Monthly number of new firms started with the ACCRE subsidy. This figure shows the monthly number of individuals receiving the ACCRE subsidy, which is granted to unemployed individuals creating a new firm. The sample period covers 1999 to 2006. Source: French Ministry of Labor. (Color figure can be viewed at wileyonlinelibrary.com)

a firm before or after the reform: before the reform, such an individual would lose eligibility to higher unemployment benefits, while after the reform, she would earn exactly the same amount (her unemployment benefit), whether or not she starts her own firm. Thus, the reform only helps insofar as the prospective unemployed entrepreneur is uncertain about success and risk averse (see Section II for a formalization). We also note that the crowding out of unemployment benefits with entrepreneurial income makes it difficult to abuse the system, unless, of course, the unemployed person does not declare business income (which is illegal; the penalty for such evasion is constant over our sample period).

The timing of the reform's implementation is fuzzy. The reform officially came into effect on July 1, 2001. In February 2002, the agency explicitly allowed the new regime to be combined with preexisting subsidies targeting unemployed entrepreneurs. An intensive information awareness campaign started in September 2002 (Rieg (2004)). Overall, we believe that the reform probably started to diffuse to local unemployment agencies throughout the first semester of 2002. Unfortunately, no data on the number of firms registered by unemployed individuals were available to confirm the precise timing. However, we can use data on the aggregate take-up of the ACCRE program to proxy for firm creation by unemployed entrepreneurs. ACCRE is a subsidy allocated *only* to unemployed workers who start firms. This subsidy existed before the reform and was not affected by its introduction. Figure 1 shows aggregate ACCRE take-up at the monthly frequency. As can be clearly seen, a large and steady

increase in the number of ACCRE recipients started sometime in 2002 and ended in 2005.

The evidence in Figure 1 is insufficient to draw a clear inference, however, as 2002 corresponds to a sharp downturn in the French economy following the burst of the tech bubble.[11] Given that firm creation tends to be procyclical, this may have delayed the diffusion of the reform to the French economy, and hence may explain why the increase in ACCRE take-up postreform cannot be precisely identified. Accordingly, in the rest of the paper, we focus on cross-sectional evidence by *comparing* industries by treatment intensity. In particular, we use separate data on the evolution of the ACCRE take-up rate on the cross section of industries. We note, however, that these alternative data are available at a much lower frequency.

B. External Validity

External validity is an important concern for our analysis. Certain characteristics of the French labor market might help explain how entry and average firm quality responded to the reform that we evaluate. In this section, we compare the relevant aspects of the French labor market with those of other OECD countries' labor markets.

First, our results on the relatively high quality of new entrepreneurs may be driven in part by the fact that France has a particularly large pool of highly skilled unemployed individuals. For comparison, in 2002, France's unemployment rate of 8.3% is higher than the OECD average of 7.3%, and about 32% of the unemployed in France had been unemployed for less than three months versus more than 50% in the United States and Canada and 45% for the United Kingdom.

Second, abnormally low ex ante entrepreneurial rates could help explain why the 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) but 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 United Kingdom). Thus, continental Europe faces stronger barriers to entry than Anglo-Saxon countries, so reforms like PARE may have a weaker effect on firm creation in these countries.

Third, the reform might help explain entry because unemployment benefits in France are particularly generous, creating strong disincentives 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%. Thus, while the French UI system is slightly more generous than that in the typical developed economy, the difference remains marginal. In terms of employment protection in 2002, France is also average, ranking 15

[11] Figure IA.I shows year-over-year GDP growth in France. As can be clearly seen, 2002 marks the beginning of a slow growth period after the very strong economy of the late 1990s to early 2000s.

out of 28 according to the OECD Employment Protection Index. Finally, at the time of the reform, France does not have an experience rating scheme, which is common for most E.U. countries.

II. Economic Framework

In this section, we lay out the theoretical framework that will guide our empirical strategy. Starting from the model of entrepreneurship in Lucas (1978), we 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, to capture the intuition behind our empirical strategy that some industries are naturally more exposed to UI reforms.

The two industries, T (Treatment) and C (Control), produce differentiated goods. Let x_s be the consumption of the good produced in industry $s \in \{T, C\}$. All agents maximize a constant elasticity of substitution (CES) utility function, $U(x_T, x_C) = \log((x_T^{\frac{\sigma-1}{\sigma}} + x_C^{\frac{\sigma-1}{\sigma}})^{\frac{\sigma}{\sigma-1}})$, where $\sigma > 0$. Let p_s be the price of each good s and y be the income of an agent. Indirect utility is then 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 one without loss of generality. All agents in the economy are potential entrepreneurs. A measure one of potential entrepreneurs is 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 determine 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, 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 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 that $g(\theta) \equiv \frac{\theta^{1-\beta}}{(1-\beta)^{1-\beta}\beta^{\beta}}$ to simplify notation. In each industry, entrepreneurial ability is 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, that is, 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 threshold 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 examine 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 Internet Appendix I. Proposition 1 summarizes the results.

PROPOSITION 1: *Assume that 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 firms' average quality, $q_s = E(\log(\theta)|\theta \geq \theta_s)$, *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*

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

The expectation E *is positive and increasing, function* Q *is positive, decreasing, and tends to zero when* $\phi \to \infty$, *and function* S *is positive and increasing, where* $S(1) = 0$. *Neither* E, Q, *nor* S *depends 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. In addition, the distribution of ability has a decreasing hazard rate $F'(\theta)/(1 - F(\theta))$ (similar to Pareto). As a result, 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, more crowding out in industry T, and a larger decline in entrepreneurial quality.

Proposition 1 also shows that the reform's effect on the economy depends on the heterogeneity in skill distribution. When the shape parameter ϕ is close to 1, entrepreneurial skill is highly 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, however, does show a significant response to an increase in insurance, since marginal entrepreneurs are much worse than inframarginal 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 described in the Introduction. Conversely, when entrepreneurial skill is 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 described above.

III. Data

We employ 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.

A. Registry

The firm registry contains the universe of firms registered in France each month from 1993 to 2008. For each newly created firm, the registry includes the industry the firm operates based on a four-digit classification system similar to the four-digit NAICS. It also provides the firm's legal status (Sole Proprietorship, Limited Liability Corporation, or Corporation). In addition, the registry data contain an exhaustive list of French firms at the end of each year, which we use to construct an exit dummy.

Figure 2 reports the 12-month moving average of the number of monthly firm creations over different categories of firms and sample periods. Panel A examines 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 substantial compared to previous fluctuations (1995 and 2000). After reaching a plateau in 2005, firm creation starts increasing again, which is often attributed to a series of subsequent reforms that are not related to PARE.[12] To avoid any contamination in the postperiod, we focus our analysis on the 1999 to 2005 time frame. Panel B narrows in on this period. Panel C separately examines the number of new firms that have zero employees at creation (dotted line) and the number of firms that have zero employees two years after creation (solid line). In aggregate, we see that the reform is accompanied by a surge in the creation of firms that start 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 (dotted line) and the number of firms with at least one employee two years after creation (solid line). While the reform is not associated with an increase in the number of firms created with more than one employee, it is clearly associated with a massive increase in new firms starting with no employees. However, as the solid line indicates, a significant fraction of 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 triggered by the reform, the dramatic surge in firm creation observed in Figure 2 consists mostly of unemployed entrepreneurs, that is, individuals targeted by

[12] These reforms allowed entrepreneurs to register a company online (June 2006).

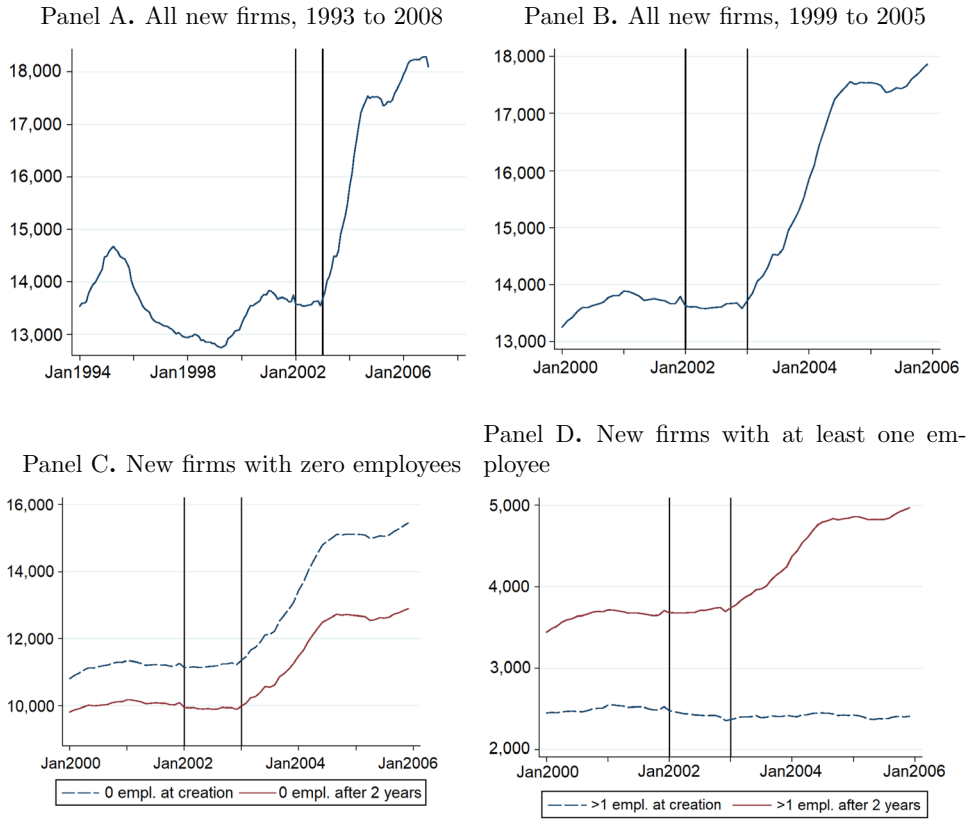

Figure 2. Monthly number of new firms. Panel A plots the 12-month moving average of the number of firms created between January 1993 and January 2008 (1993 does not appear on the graph as we compute a 12-month moving average). Panel B zooms in on our 1999 to 2005 sample period (1999 does not appear on the graph as we compute a 12-month moving average). Panel C plots the number of new firms started with zero employees (dotted line) and the number of new firms with zero employees two years after creation including firms that have exited (plain line). Panel D plots the number of new firms started with at least one employee (dotted line) and the number of new firms with at least one employee two years after creation (plain). Source: Firm registry from the French Statistical Office. (Color figure can be viewed at wileyonlinelibrary.com)

the reform. While we do not observe the overall number of new firms created each month by unemployed individuals, we can estimate this number as follows. As shown in Figure 1, the number of new firms that receive the ACCRE subsidy (a subsidy only available to unemployed entrepreneurs that was not itself affected by the reform we consider) increased progressively from 3,000 per month in 2002 to about 6,000 per month in 2006. We can also use the SINE survey (described in detail in Section III.C below) 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.

Table I
Industry Composition: Annual Data

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

Industry	Prereform # Entries (1)	% of Pre # Entries (2)	Postreform # Entries (3)	% of Post # Entries (4)	Pre-Post Growth (5)
Transport/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%

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 to 2005 surge in firm creations to unemployed entrepreneurs.

Table I provides annual data on firm creation for eight broad industries from 1999 to 2005. Both pre- and postreform, newly created firms concentrate largely in services, construction, and retail trade. These three industries comprise about 70% of all firm creations in the prereform years. We also show that the industries with the largest growth of new entrants postreform are services, retail trade, construction, and finance, insurance, and real estate (FIRE), which are labor-intensive, low-fixed-cost industries.[13]

Table II, Panel A, aggregates creation data at the four-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, 2001 (our prereform period). The results show that, prereform, the average industry observes approximately 43.6 creations per month, which leads to about 152,000 newly created firms per year.

B. Accounting Data

To analyze the long-term performance of new ventures, we complement the registry data with accounting information from tax files (see Bertrand, Schoar, and Thesmar (2007) for a detailed description). Tax files provide us with the

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

Table II
Summary Statistics

Panels A and B report summary statistics on all new firms started during the prereform period (1999 to 2001). Statistics are computed at the four-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 1998 wave of the SINE survey. Panel D reports summary statistics for incumbent firms over the 1999 to 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 or fewer employees and those 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 three columns provide summary statistics by splitting the sample into four quartiles of treatment intensity. We report the average of the statistics for quartiles 2 and 3 in the interest of space. 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 prereform period). Source: Firm registry and tax files from the French Statistical Office and 1998 SINE survey.

				Mean by Quartile % of Sole Prop. New Firms		
	N	Mean	*SD*	Q1	Q2/Q3	Q4
Panel A: New Firms, Industry Level						
# new firms (monthly)	290	43.62	84	22	31	87
# new jobs after two years	290	32.49	62	22	32	43
# new job incl. entrepreneurs	290	69.30	123	39	58	118
Panel B: New Firms, Firm Level						
Employment at creation	381,683	0.49	1.9	0.86	0.64	0.32
At least one job at creation	381,683	0.20	0.4	0.26	0.24	0.15
Employment after two years	381,683	0.87	2.5	1.06	1.23	0.60
At least one job after two years	381,683	0.29	0.45	0.36	0.39	0.23
Hire in first two years	381,683	0.25	0.43	0.31	0.34	0.20
Exit in first two years	381,683	0.16	0.36	0.21	0.15	0.14
Panel C: New Firms, Survey, Firm Level						
High school graduate	17,449	0.46	0.49	0.48		0.42
Plan to hire	17,449	0.23	0.31	0.30		0.18
Panel D: Incumbents, Industry Level						
# small incumbents	290	2,779	5,289	1,039	2,532	4,747
# jobs in small incumbents	290	3,647	7,667	1,497	3,790	5,196
# large incumbents	290	804	1,243	705	892	715
# jobs in large incumbents	290	21,967	38,740	27,527	24,468	11,948

number of employees both at creation and two years after creation. These files 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 microbusiness tax regime, firms that choose this regime are likely to have zero employees. For this reason, in the empirical analysis, we assume that firms that do not appear in the tax files do not have employees.

Table II, 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. However, there is 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 prereform sample, 25% of new firms hire at least one employee in the first two years and 16% of firms exit the sample before the end of the second fiscal year.

C. 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 that contains questions about the entrepreneur and the firm she creates.[14] We obtain three cross sections of the survey in the relevant time period: 1998, 2002, and 2006. The first of these years, 1998, clearly belongs to the prereform period. The second, 2002, belongs to the very beginning of the postreform period—the survey is conducted during the first semester of 2002, while the reform is progressively affecting firms throughout 2002.[15] Year 2006 corresponds to the postreform period. SINE covers approximately one-third of newly created firms in the first six months of a survey year (some 20,000 observations in 1998, 26,000 in 2002, and 30,000 in 2006) and has a response rate of about 85%.[16]

We first use the SINE survey to measure entrepreneurs' highest level of educational attainment. We also use the response to the survey question "Do you plan to hire in the next twelve months?" as a measure of subjective growth expectations or "ambition." Table II, Panel C, reports descriptive statistics for the survey variables. As can be seen, 46% of the entrepreneurs surveyed in SINE are at least high school graduates, and 23% of surveyed entrepreneurs plan to hire in the year following creation. For robustness purposes, we also construct two additional variables: a dummy variable that indicates when the entrepreneur declares being "a supplier or client of his former employer," and a dummy variable that indicates when the entrepreneur responds that her firm "has at most 2 different customers."

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

[15] In unreported regressions, we use only firms created in January and February of 2002 as our prereform observations. We obtain similar results to those reported in the paper.

[16] The response rate is high because the survey is administered by the statistical office INSEE.

IV. Empirical Strategy

A. Identification Strategy

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 classify as sole proprietorships.[17] 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 election. We expect industries with a larger fraction of sole proprietorships to be more affected by the reform, a prediction of our model in Section II. Following this intuition, we define treatment intensity as the fraction of sole proprietorships among newly created firms at the industry level and measure it at the four-digit industry level in the prereform period. We then rank industries in ascending order of treatment intensity and construct four quartiles (Q1 to Q4) that should be increasingly affected by the PARE reform. Our identification strategy then simply involves comparing how the number and characteristics of newly created firms changed from the pre- to the postreform periods depending on the treatment intensity quartile to which the industry belongs.[18] 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 our measure of treatment intensity.

Internet Appendix Table IA.I lists industries that belong to the least (i.e., bottom quartile Q1) versus most (i.e., top quartile Q4) treated industries. Highly exposed industries consist of, for example, 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 II, we present summary statistics for firms and industries in each of these four quartiles of treatment intensity. In industries belonging to Q4, firms have 0.54 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. On average, entrepreneurs in Q4 industries are also less educated (seven percentage points less likely to have a high school degree) and less ambitious (16 percentage points less likely to hire in the next 12 months) than those in Q1 industries.

To illustrate our empirical strategy, in Internet Appendix Table IA.II, we report the top 20 four-digit industries in terms of their contribution to the postreform surge in new firm creation. For each industry s over the 2002 to 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 concentrates among Q4

[17] We also repeat our analysis using an alternative definition of treatment intensity—the fraction of firms created with zero employees within a four-digit industry. Tables IA.IX to IA.XV in Internet Appendix III report regression results using this alternative treatment definition. The results are qualitatively similar to our main results.

[18] In robustness checks, we also split industries using deciles and vigintiles of treatment intensity and obtain similar results.

industries: (i) the top 20 four-digit industries contribute to more than half of the aggregate surge in new firm creations and (ii) of these 20 industries, 13 belong to the fourth quartile of treatment intensity (Q4) and 18 belong to either Q4 or Q3. The top contributing industries are the usual suspects for such a reform: masonry and electricity contractors, business consulting services, plumbers, retail trade, and real estate intermediaries.

B. Empirical Specification

Our main specification for industry-level outcomes is[19]

$$Y_{st} = \sum_{k=1}^{4} \alpha_k \cdot Q_s^k \times post_t + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times t + \mu_s + MONTH_t + \epsilon_{st}, \tag{1}$$

where Q_s^k is 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 $MONTH_t$ denotes 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:

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

When using the SINE survey, where cross sections of data are only available in 2002 and 2006, our main specification becomes

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

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.

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.

Figure 3 shows that firm creation in treated industries surged, relative to less treated sectors, during the first half of 2002. Recall that 2002 corresponds to an economic downturn in the French economy.[20] Because firm creation tends to be procyclical, this aggregate shock weighs negatively on firm creation in

[19] Since our sample of industries is balanced, the inclusion of time fixed effects in this difference-in-difference model does not affect the estimated treatment effects.

[20] See Figure IA.I for a plot of quarterly year-over-year GDP growth.

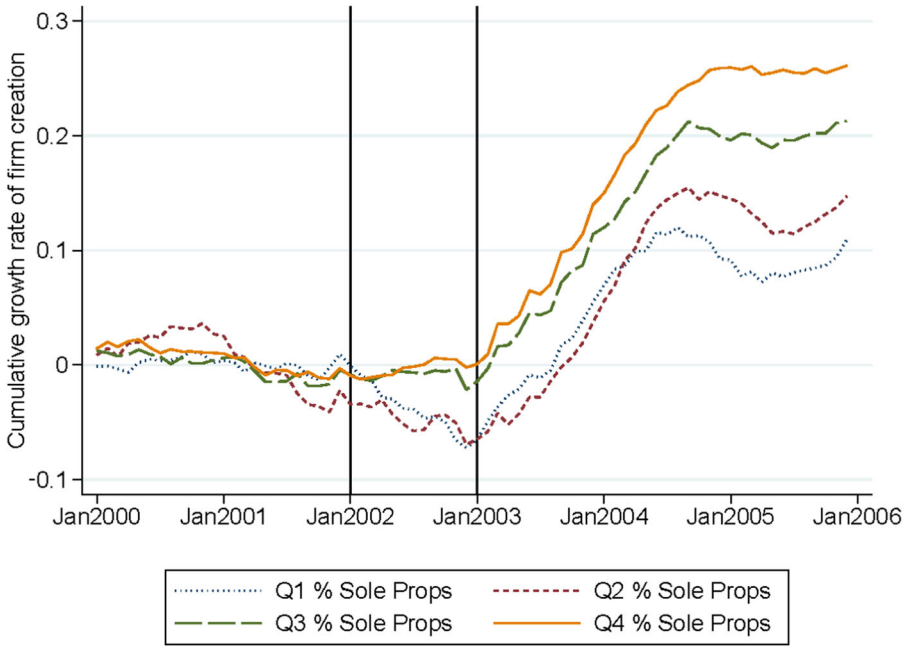

Figure 3. Growth rate in firm creation: Treated versus control. Qk% is the k^{th} quartile of our treatment intensity variable (the fraction of sole proprietorships among newly created firms in the industry, measured in the prereform period). Each month t and for each quartile Q_k ($k = 1, 2, 3, 4$) 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 to 2000) to month t: $g_t^k = \frac{1}{\#\,industries\ in Q_k} \sum_{s \in Q_k} (\log(\#\,firms\ created_{st}) - \frac{1}{24} \sum_{\tau \in 1999,2000} \log(\#\,firms\ created_{s\tau}))$. The graph plots the 12-month moving average of g_t^k. Source: Firm registry from the French Statistical Office. (Color figure can be viewed at wileyonlinelibrary.com)

all industries. This pattern explains why, in 2002, firm creation in less treated industries—industries in Q1 and Q2—actually decreased. However, for industries most exposed to the reform—industries in Q3 and Q4—this negative shock seems to be entirely offset by the positive effect of the reform. After 2002, all industries experience a steady increase in the number of new firms created, but creations in industries most exposed to the reform increase at a faster pace than in less exposed industries. These effects are persistent throughout the sample period. Overall, the number of newly created firms increases by about 10% in Q1 industries and 25% in Q4 industries.

C. Discussion of the Identifying Assumption

At least two omitted variable concerns represent a threat to identification in our context. First, our measure of treatment intensity could be correlated with industry exposure to some aggregate "shock" happening at the time of the reform. For instance, the 2002 recession may lead to a relative decline in new firm

Table III
Aggregate Growth Rate: Treated versus Control

In columns (1) to (3), the dependent variable is the log of total industry sales. In columns (4) to (6), the dependent variable is the log of total industry value-added. POST is a dummy variable equal to 0 for observations in the 1999 to 2001 period and to 1 for the 2002 to 2005 period. 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 prereform period). Treatment-specific trends are the interactions of Q2, Q3, and Q4 with linear time trends, labeled as *Treatment trend*. All regressions include industry fixed effects. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Tax files from the French Statistical Office. Sample: 290 industries from 1999 to 2005, annual observations.

	Sales			Value-Added		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.082***	0.095***	−0.003	0.10***	0.130***	0.013
	(0.013)	(0.021)	(0.013)	(0.013)	(0.020)	(0.011)
Q2 % Sole Props		−0.063*	−0.031		−0.086**	−0.020
× POST		(0.034)	(0.022)		(0.036)	(0.024)
Q3 % Sole Props		−0.008	−0.011		−0.041	−0.029*
× POST		(0.028)	(0.021)		(0.029)	(0.016)
Q4 % Sole Props		0.017	−0.002		0.006	−0.003
× POST		(0.039)	(0.018)		(0.038)	(0.016)
Treatment 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^2	0.99	0.99	0.99	0.98	0.98	0.98

creation in Q1/Q2 relative to Q3/Q4, which would lead to a spurious estimated treatment effect. Note, however, that if this aggregate shock is temporary, we should expect its effect on the relative rate of new firm creation across industries to vanish over time: in the long run, new firm creation should not differ systematically across industries with different treatment intensity. This is not what we see in the data. Figure IA.2 depicts the cumulative growth rate of firm creation in Q2, Q3, and Q4 *relative to* Q1, the control group. Figure IA.2 shows that the relative spread in new firm creation in Q3 and Q4 (the two treatment groups for which the effect of the reform is significant) increases *steadily* from 2002 (the beginning of the postperiod) to 2006. In other words, the data provide clear evidence of a *widening* gap in new firm creation between Q3/Q4 and Q1 over time.

Another approach consists of directly measuring industry exposure to the business cycle. If our effect comes from heterogeneous exposure to aggregate shocks, then *aggregate* industry sales should increase significantly more postreform 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 in Table III, show that while there is a significant increase in aggregate industry

sales in the postreform period (8.2% in column (1)), this increase is not significantly different across the four quartiles of treatment intensity (columns (2) and (3)). Columns (4) to (6) yield a similar conclusion when using aggregate industry value-added.[21]

Our identifying assumption may fail if (i) the shock is persistent and (ii) exposure to this persistent shock is correlated with the treatment. We address this concern in several ways in the next section. First, we can directly control for industry characteristics that might correlate with treatment intensity and make industries more sensitive to a permanent shock. We augment equation (1) to include 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 sales growth rate for firms in the industry from 1999 to 2001). As we show in Table IV, these added controls do not affect our main estimates. However, there may remain unobserved characteristics that we cannot control for and that create a bias in our estimation. We therefore also consider a second approach whereby we tie the increase in new firm creation directly to the increase in unemployed entrepreneurs. We explain this approach in detail in Section V.B. The results of this analysis imply that a confounding shock would have to be persistent, occur at the time the PARE reform is adopted, affect industries that are more exposed to this reform disproportionately, and change the propensity to start new firms of unemployed workers *only*. We cannot think of a plausible candidate for such a shock.

A second threat to identification arises from potential changes in the pool of unemployed individuals. For instance, if skilled individuals tend to create firms in small-scale industries and the postreform period coincides with an increase in the fraction of skilled individuals in the pool of unemployed workers, 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 (Internet Appendix Table IA.III). In addition, we do not find that, on average, entrepreneurs become more educated or more "ambitious" after the reform, as shown in Table VII. Taken together, these results imply that changes in the skill composition of the pool of unemployed individuals cannot be driving the postreform increase in new firm creation observed in industries with high treatment intensity.[22] Of course, we

[21] In Internet Appendix Table IA.XVI, 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 prereform period (1993 to 1999). We reestimate equation (1) after including a control for the interaction of industry β and the post dummy. Our estimates are not affected by the inclusion of these controls.

[22] 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.

Table IV
Firm Creation: Treated versus Control

The dependent variable is the log of one plus the number of new firms created in an industry-month. POST is a dummy equal to 0 for observations in the 1999 to 2001 period and to 1 for the 2002 to 2005 period. 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 prereform 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 *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry from the French Statistical Office. Sample: 290 industries, 1999 to 2005, monthly observations.

	Number of Firms Created			
	(1)	(2)	(3)	(4)
POST	0.10^{***}	0.046^{*}	-0.16^{***}	-0.25^{***}
	(0.014)	(0.027)	(0.031)	(0.072)
Q2 % Sole Props		0.019	0.035	0.027
× POST		(0.043)	(0.044)	(0.043)
Q3 % Sole Props		0.08^{**}	0.11^{***}	0.11^{***}
× POST		(0.038)	(0.037)	(0.036)
Q4 % Sole Props		0.12^{***}	0.13^{***}	0.14^{***}
× POST		(0.038)	(0.039)	(0.039)
Industry capital intensity				0.041^{*}
× POST				(0.025)
Industry growth				−0.048
× POST				(0.038)
Industry capital intensity				−0.014
× Trend				(0.008)
Industry growth				0.054^{***}
× Trend				(0.017)
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^2	0.92	0.92	0.92	0.92

cannot rule out the possibility that a change in *unobservable* characteristics in the pool of unemployed workers contributed to the change we observe in the creation of new firms across industries.

V. Effect of the Reform on Entrepreneurial Activity

A. 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 industry *s*

and month t as our dependent variable.[23] The regressions use a balanced sample of 290 industries from January 1999 to December 2005. The results are reported in Table IV. Column (1) only includes the post dummy, along with industry and month-of-creation fixed effects. The results show 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 find in Figure 2. The difference can be attributed to the fact that we start the postreform period in January 2002, while the reform was massively advertised by local unemployment agencies starting only in mid-2002.

Column (2) adds interactions 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) to (4) are not significantly different. Q4 (Q3) industries experience a significant increase in new firm creation of 12 to 14 (8 to 11) percentage points in the postreform 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), we 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 reform was implemented gradually over time with no clear starting date, in the Internet Appendix, we check that our results are robust to alternative definitions of the event window. We show that: (i) when we exclude 2002 from the sample, the estimated effects are actually larger, as expected since the reform is massively advertised by local unemployment agencies starting only in mid-2002 (Internet Appendix Table IA.IV); (ii) when we assign 2002 to the prereform period, the estimated effects are smaller, which is not surprising since part of the "treatment" period is now classified as the "control" period (Internet Appendix Table IA.V); and (iii) when we exclude 2005 from the postreform sample, the results are virtually unchanged (Internet Appendix Table IA.VI).

B. Additional Evidence: Unemployed Entrepreneurs and Firm Creation

In this section, we provide further evidence that ties down the dynamics of firm creation postreform to the population of unemployed workers. This is an important step in our analysis because it helps confirm the causal interpretation of the results in Section V.A and invalidates the hypothesis that the results are driven by confounding factors such as heterogeneous exposures to

[23] A low number of small industries experience months without any creation. To keep a balanced panel, we use as our dependent variable log(1 + # firms created). The results are similar when using log(# firms created).

business cycle fluctuations. First, recall from Section III.A that we use data on the take-up of the ACCRE subsidy (a subsidy for unemployed entrepreneurs that is unrelated to the PARE reform) to show that more than 90% of the *aggregate* increase in new firm creation observed postreform could be attributed to unemployed individuals. Such time-series evidence strongly points toward the reform.

Here, we complement time-series evidence with cross-sectional 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, for example, capturing heterogeneous exposures to business cycle fluctuations, then within-industry shifts in the share of unemployed entrepreneurs should not correlate with firm creation rates. To test this hypothesis, we estimate the equation

$$\frac{n_s^{2006} - n_s^{2002}}{n_s^{2002}} = \beta\, \Delta Unemp_s + \gamma\, AggGrowth_s + \epsilon_s, \tag{4}$$

where n_s^t is the total number of creations in industry s in the first half of year t, $\Delta Unemp_s$ is the change in the fraction of unemployed entrepreneurs measured in the SINE survey in industry s between 2002 and 2006,[24] and $AggGrowth_s$ is the growth rate over the 2002 to 2006 period of industry s total value-added, which we add to equation (4) as a natural control.[25]

Table V reports results of this estimation. Column (1) includes all unemployed entrepreneurs in the calculation of $\Delta Unemp_s$ and shows that the increase in firm creation observed between 2002 and 2006 is significantly more pronounced in industries in which the fraction of unemployed entrepreneurs increased the most from 2002 to 2006.[26] Columns (2) and (3) extend the analysis in column (1) by decomposing the industry-level change in unemployed entrepreneurs ($\Delta Unemp_s$) based on whether these entrepreneurs take up the ACCRE subsidy. Since ACCRE is a pure subsidy for unemployed entrepreneurs that entails no constraint, those failing to claim the ACCRE subsidy are presumably ill-informed and therefore less likely to be aware of the PARE reform.[27] Thus, if the increase in industry-level entrepreneurship post-2002 is, in fact, due to the PARE, this increase should consist of informed entrepreneurs, that

[24] We focus on the first semester of 2002 and 2006 because the SINE survey, which we use to compute $\Delta Unemp_s$, only surveys firms created in the first half of the survey year.

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

[26] Note that to obtain precise estimates of $\Delta Unemp_s$, 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.

[27] These policies were publicized largely by local unemployment agencies, which likely leads to a correlation in the propensity to know about the different programs.

Table V
Firm Creation Growth and the Increase in Unemployed Entrepreneurs

The dependent variable in columns (1) to (3) is the industry-level growth rate of the number of new firms created from 2002 to 2006 regressed on the industry change in the fraction of formerly unemployed individuals among all entrepreneurs over the same period. The explanatory variable *% change ACCRE* is the percentage change (at the industry level) in the fraction of formerly unemployed individuals receiving the ACCRE subsidy among all entrepreneurs from 2002 to 2006. *% change non-ACCRE* is the industry change in the fraction of formerly unemployed individuals not receiving the ACCRE subsidy among all entrepreneurs. Columns (4) to (6) repeat the same regressions for the time period 1998 to 2002. All regressions control for the contemporaneous growth rate in aggregate industry value-added. Robust standard errors (in parentheses) are reported. *, **, and *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Creation files and SINE surveys 1998, 2002, and 2006.

	2002 to 2006 Entry Growth			1998 to 2002 Entry Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
% change former unemployed	0.230**			−0.024		
	(0.120)			(0.084)		
% change ACCRE		0.280***			−0.066	
		(0.095)			(0.075)	
% change non-ACCRE			−0.033			0.077
			(0.240)			(0.120)
Aggregate sector growth rate	0.39***	0.42***	0.37***	0.22**	0.22**	0.21**
	(0.110)	(0.110)	(0.110)	(0.091)	(0.091)	(0.092)
Observations	195	195	195	195	195	195
R^2	0.071	0.093	0.053	0.030	0.033	0.032

is, those claiming the ACCRE subsidy. Accordingly, in column (2), we reestimate equation (4) but we define $\Delta Unemp_s$ as unemployed entrepreneurs claiming the ACCRE subsidy. The estimated β is 0.28, significant at the 1% level, consistent with industry-level growth in new firm creation being driven by an increase in unemployed entrepreneurs. In column (3), we instead define $\Delta Unemp_s$ as unemployed entrepreneurs *not taking* the ACCRE subsidy. In this case, the estimated β is small and statistically insignificant, 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 V, we perform a placebo analysis in which we exploit the previous wave of the SINE survey in 1998. Since there was no significant reform that favored business creation by unemployed entrepreneurs over the 1998 to 2002 period, industry-level shifts in the fraction of unemployed entrepreneurs should *not* explain industry-level increases in firm creation during that period. We reestimate equation (4) for this period and report the results in columns (4) to (6) of Table V. In contrast to the estimation performed for the 2002 to 2006 period, the estimated βs are all insignificant as expected.

C. The Quality of Postreform Start-Ups

C.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 the main purpose of the paper). We first use ex post measures of firm quality, namely, job creation, and exit probability. If the main effect of the reform was to attract 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, then after the reform start-ups should be as likely to create jobs or to exit as before the exit (the experimentation channel).

We estimate equation (2) using as the 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 firms that ever hire typically begin to do so within the first two years. The estimation results are reported in Table VI. While there is an increase in a start-up's propensity to hire postreform (column (1)), we find that firms started in Q4 industries (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 (column (2)). 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 II). In addition, Table VI shows that the estimated effect of the reform on Q3 industries is *positive* and insignificant, so grouping Q3 and Q4 industries together would lead to an even smaller effect. Results are similar using log employment two years after creation instead of the hiring dummy as the dependent variable (columns (3) and (4)). Overall, the evidence in Table VI is inconsistent with the view that the reform led to the creation of new firms that are significantly less likely to hire.

The second measure of ex post quality that 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.[28] In Table VI, columns (5) and (6), we estimate equation (2) using a dummy for exit within two years as our dependent variable. The results are similar to those ones above for hiring patterns: while there is a significant increase in the probability of exit within the first two years in the postreform period (column (5)), firms started in Q4 industries (Q3 and Q2) do not become significantly more likely to

[28] 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, that is, 95% correspond to firms that have closed down permanently.

Table VI
Firm Quality: Ex Post Measures

The dependent variable in columns (1) and (2) is a dummy equal to 1 if the firm's employment two years after creation is greater than at creation covering 1,034,674 observations. We estimate a difference-in-difference specification where POST is a dummy equal to 0 for observations in 1999 to 2001 and to 1 for the period after 2001. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of treatment intensity (the fraction of sole proprietorships among newly created firms in the industry, measured prereform). The interactions of Q2, Q3, and Q4 with linear time trends control for treatment-specific trends labeled as *Treatment trend*. 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 industry level sales from 1999 to 2001. In columns (3) and (4), the dependent variable is equal to the log of one plus employment two years after creation covering 824,184 observations; in columns (5) and (6), it is a dummy equal to 1 if the firm exits during the first two years covering 1,034,674 observations. All regressions include industry and month-of-the-year fixed effects. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry and tax files from the French Statistical Office from 1999 to 2005.

	Hire		Log(employment)		Exit	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.010***	−0.002	0.005	0.015	0.011***	0.019
	(0.004)	(0.013)	(0.005)	(0.022)	(0.002)	(0.014)
Q2 % Sole Props		−0.009		0.001		0.004
× POST		(0.008)		(0.016)		(0.01)
Q3 % Sole Props		0.005		0.017		−0.001
× POST		(0.007)		(0.014)		(0.01)
Q4 % Sole Props		−0.009		−0.013		−0.009
× POST		(0.006)		(0.012)		(0.008)
Ind. capital intensity		0.007		0.003		−0.006
× POST		(0.004)		(0.007)		(0.005)
Ind. growth		−0.009*		−0.015*		−0.001
× POST		(0.005)		(0.008)		(0.006)
Ind. capital intensity		−0.003		−0.003		0.003**
× Trend		(0.002)		(0.003)		(0.001)
Ind. growth		0.008*		0.014**		0.002
× Trend		(0.004)		(0.006)		(0.002)
Treatment trend	No	Yes	No	Yes	No	Yes
Month of year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.091	0.091	0.110	0.110	0.037	0.038

exit within two years in the postreform period, relative to new firms started in Q1 industries (column (6)). The estimated effects allow us to reject at the 5% confidence level the null hypothesis that in the postreform period, firms started in Q4 industries are 0.6 percentage points more likely to exit within two years than firms started in Q1 industries, relative to the prereform period. Given a baseline rate of exit within two years of 16%, the magnitude of such an effect is quite small.

Table VII
Firm Quality: Ex Ante Measures

The dependent variable in columns (1) and (2) is the log of one plus the number of employees four years after creation. The dependent variable in columns (3) and (4) is a dummy equal to 1 if the firm has at least one employee four years after creation; and in columns (5) and (6), it is an indicator variable for whether the firm has at least five employees four years after creation. High school is an indicator for whether the entrepreneur has at least a high school 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 *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: 1998 SINE survey.

Panel A: Education and Ambition Predict Firm Size

	Log (employment)		Employment ≥ 1		Employment ≥ 5	
	(1)	(2)	(3)	(4)	(5)	(6)
High school	0.076***	0.055***	0.036***	0.025**	0.023***	0.017***
	(0.016)	(0.015)	(0.011)	(0.010)	(0.006)	(0.005)
Plan to hire		0.290***		0.160***		0.084***
		(0.028)		(0.015)		(0.010)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,449	17,449	17,449	17,449	17,449	17,449
R^2	0.10	0.14	0.11	0.14	0.067	0.089

C.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, namely, education and self-reported expectation to grow (which we also call "ambition"). Since these variables come from the SINE survey, they are available only once before (1998H1) and once after (2002H1) the reform (see Section III.C for more details).

In Table VII, Panel A, we check that ex ante measures of quality correlate well with ex post entrepreneurial success. Entrepreneurial success for a firm born in 1998 is measured as the firm's employment four years after creation, that is, in 2002 (columns (1) and (2)), the probability that the firm has more than one employee in 2002 (columns (3) and (4)), and the probability that the firm has more than five employees (columns (5) and (6)). More educated and ambitious entrepreneurs are more likely to start successful firms. For instance, entrepreneurs who have a high school degree at creation end up with a larger probability of having at least one employee (column (3), increase of 3.6 percentage points) as well as a higher probability of having at least five employees four years after creation (column (5), increase of 2.3 percentage points).

In Table VII, Panel B, we look at the impact of the reform on these ex ante quality measures and find none. The empirical strategy is similar to that in Table VI, but we use our ex ante measures of quality as dependent variables: the

Table VII—*Continued*

In columns (1) and (2), the dependent variable is a dummy variable equal to 1 if the entrepreneur has at least a high school degree. In columns (3) and (4), 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 1998 wave of the survey and to 1 for observations from the 2002 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 prereform 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 *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: 1998 and 2002 SINE surveys. Sample: Random sample of 47,088 new firms started in the first semester of 1998 and the first semester of 2002.

Panel B: Education and Ambition after the Reform

	High school		Plan to hire	
	(1)	(2)	(3)	(4)
POST	0.070***	0.039	−0.016	−0.007
	(0.017)	(0.035)	(0.019)	(0.039)
Q2 % Sole Props	−0.008	−0.021	0.010	0.006
× POST	(0.026)	(0.024)	(0.027)	(0.027)
Q3 % Sole Props	−0.004	−0.006	0.013	0.012
× POST	(0.021)	(0.018)	(0.023)	(0.024)
Q4 % Sole Props	−0.003	−0.014	0.020	0.015
× POST	(0.020)	(0.019)	(0.022)	(0.023)
Industry capital intensity		0.025**		0.0022
× POST		(0.012)		(0.012)
Industry growth		−0.041***		−0.023
× POST		(0.015)		(0.015)
Industry FE	Yes	Yes	Yes	Yes
Observations	47,088	47,088	47,088	47,088
R^2	0.250	0.250	0.059	0.059

probability of having a high school diploma (columns (1) and (2)) and the probability of plans to hire in the coming year (columns (3) and (4)). The regression results consistently show that these measures of quality did not deteriorate in the postreform period in Q4 industries relative to Q1 industries.

C.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, that is, those newly created firms that would not have been created absent the reform. In this section, we attempt to quantify the effect of the reform on the quality of these marginal new entrants.

To do so, we make two simplifying assumptions: (i) all firms created in Q1 industries in the postreform period are created by inframarginal entrepreneurs and (ii) marginal entrepreneurs constitute 100% of the differential entry between Q1 and Q4 industries. These assumptions allow us to reweight the quality of marginal entrepreneurs based on their propensity in the population of new entrants. Let q_i (q_m) be a measure of the average quality of inframarginal (marginal) entrepreneurs. We know from Table IV that the number of firms created in the most treated industries increased by $\delta = 14\%$ relative to the least treated industries. Due to assumption (2) above, all of 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 $\Delta q = \frac{\delta}{1+\delta} \times (q_m - q_i)$. Given that the regressions give us the change in average quality, Δq, and knowing that the fraction of marginal firms is $\delta = 0.14$, we can infer the difference in observable quality between the marginal and inframarginal entrants.

Consider, for instance, the probability of hiring as a measure of quality. Column (2) of Table VI shows that for this measure, Δq is -0.009 and insignificant. Applying the formula derived above yields a difference in average quality between marginal and inframarginal 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 inframarginal ones. Overall, these results reject the hypothesis of a significant decline in entrepreneurial quality due to the PARE reform.

C.4. Robustness Checks

Disguised Employment. A potential concern with 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 possibility, 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 Internet Appendix Table IA.VII show that while the propensity to work with a past employer and the propensity to have only one or two clients seem to have increased slightly in the postreform period, this increase is not more pronounced in treated industries. The results in Table IA.VII are thus hard to reconcile with the view that many entrepreneurs attracted by the reform are simply employees "in disguise."

Insurance versus Financial Constraint. Our preferred interpretation of the results above is that downside insurance facilitates entry into entrepreneurship. A competing interpretation is that the reform simply relaxes financial constraints by providing unemployed entrepreneurs with additional pledgeable income. While insurance and financial constraints are conceptually related forms of market incompleteness (unconstrained entrepreneurs should be able to self-insure), we can reject the hypothesis that the reform expanded new firms' debt capacity. To do so, we estimate our main specification using debt as the dependent variable. The tax files provide us with the amount of

financial (i.e., bank) debt on the balance sheet. We report the results in Internet Appendix Table IA.VIII. In columns (1) and (3), we look at the differential evolution of the ratio of debt to total assets. In columns (2) and (4), we focus on the log of one plus bank debt. Since not all new firms are in the tax files, we first assume that debt is zero for firms not in the tax files (columns (1) and (2)). To test the robustness of this assumption, we also restrict the sample to new firms present in the tax files (columns (3) and (4)). The results show that firms in the most exposed sectors do not issue significantly more debt after the reform. This finding is not consistent with the hypothesis that the reform expanded debt capacity for new firms. Instead, it suggests that future unemployment benefits are difficult to pledge, which is consistent with anecdotal evidence that French banks avoid lending to unemployed workers. In sum, the effect of the reform was to offer insurance, not expand credit.

VI. Aggregate Effect on Employment and Productivity

In this section, we empirically evaluate the aggregate effect of the PARE reform. We first investigate the effect of the reform on job creation by new firms. We then shift focus to small incumbent firms in the same sector. Next, we compare the efficiency of newly created firms to that of small incumbents.

A. Job Creation and Crowding Out

We first estimate equation (1) using industry-level employment data and report the results in Table VIII. 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 assume 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.

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 postreform period by these newly created firms. When we repeat the estimation in columns (3) and (4) using 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

Table VIII
Job Creation

This table contains difference-in-difference specifications of job creation at the industry level. 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 *including* the job created for the founders. In columns (3) and (4), it is the same dependent variable but *without* including the entrepreneurs' own job. POST is a dummy equal to 0 for observations in 1999 to 2001 and to 1 for the period after 2001. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of treatment intensity (the fraction of sole proprietorships among newly created firms in the industry, measured prereform). The interactions of Q2, Q3, and Q4 with the post dummy. 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 industry level sales 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 *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry and tax files from the French Statistical Office.

	Number of Jobs Created Adding Entrepreneurs' Jobs		Number of Jobs Created	
	(1)	(2)	(3)	(4)
POST	−0.23***	−0.48***	−0.23***	−0.53***
	(0.051)	(0.096)	(0.049)	(0.100)
Q2 % Sole Props	0.087	0.075	0.093	0.087
× POST	(0.065)	(0.064)	(0.066)	(0.066)
Q3 % Sole Props	0.17***	0.18***	0.21***	0.22***
× POST	(0.059)	(0.058)	(0.060)	(0.060)
Q4 % Sole Props	0.20***	0.21***	0.21***	0.22***
× POST	(0.059)	(0.058)	(0.061)	(0.061)
Industry capital intensity		0.096***		0.10***
× POST		(0.033)		(0.033)
Industry growth		−0.025		0.055
× POST		(0.044)		(0.057)
Industry capital intensity		−0.037***		−0.042***
× Trend		(0.012)		(0.013)
Industry growth		0.079***		0.120***
× Trend		(0.014)		(0.018)
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^2	0.84	0.84	0.76	0.77

created monthly in Q4 industries.[29] These results suggest that the reform led to the direct creation of 9,000 to 24,000 jobs by newly created firms each year. Note that this estimate excludes the effect of the reform on Q2 and Q3.

[29] This wedge arises naturally 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.

Table IX
Employment Growth Per Category of Firm

This table contains difference-in-difference specifications of job creation for different types of incumbent firms. The dependent variable in columns (1) and (2) is the growth rate of total employment in small incumbent firms. POST is a dummy equal to 0 in 1999 to 2001 and to 1 for the period after 2001. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of treatment intensity (the fraction of sole proprietorships among newly created firms in the industry, measured prereform). Treatment 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 industry level sales from 1999 to 2001. In columns (3) and (4), it is the growth rate of total employment in large incumbent firms; and in columns (5) and (6), it is the growth rate of total employment in small incumbents and new firms started over the last two years. All regressions include industry and month-of-the-year fixed effects. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry and tax files from the French Statistical Office from 1999 to 2005.

	Small Incumbents		Large Incumbents		Small Incumbents + New Firms	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	−0.027***	−0.027	−0.058***	−0.093**	0.002	−0.140
	(0.010)	(0.040)	(0.016)	(0.038)	(0.027)	(0.130)
Q2 % Sole Props	−0.025*	−0.024**	0.020	0.016	−0.014	−0.019
× POST	(0.013)	(0.012)	(0.019)	(0.019)	(0.031)	(0.026)
Q3 % Sole Props	−0.019*	−0.019	0.030	0.031	0.010	0.012
× POST	(0.011)	(0.012)	(0.019)	(0.019)	(0.028)	(0.028)
Q4 % Sole Props	−0.022**	−0.022**	0.010	0.010	0.018	0.024
× POST	(0.010)	(0.011)	(0.018)	(0.017)	(0.031)	(0.033)
Ind. cap. intens.		0.000		0.017		0.053
× POST		(0.013)		(0.012)		(0.043)
Ind. growth		0.001		−0.020		0.001
× POST		(0.009)		(0.022)		(0.037)
Ind. cap. intens.		−0.001		−0.006***		−0.019*
× Trend		(0.002)		(0.002)		(0.010)
Ind. growth		0.001		−0.002		0.004
× Trend		(0.002)		(0.003)		(0.008)
Treatment 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^2	0.47	0.47	0.17	0.18	0.61	0.62

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 the dependent variable. We report the results in columns (1) to (4) of Table IX. 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 that we are studying. In columns (1) and (2), we focus first on small incumbents with five or fewer employees. These small incumbents are more likely to be competing directly with the new entrants in the product

or labor markets. In columns (3) and (4), we compute the growth rate of total employment at large incumbents with more than five employees.[30]

The results show 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, columns (3) and (4) show that employment growth at large incumbent firms does not change significantly following the reform in Q4 relative to Q1 industries (the estimate is an insignificant 1 percentage point in column (4)).

Based on the estimates in column (2), we can quantify the number of jobs that are 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 II, 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 industry-year. Aggregating over all of 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) direct creation of 9,000 to 24,000 jobs per year 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 IX, columns (5) and (6), we directly examine the overall effect of the reform on industry employment. Toward this end, for each industry, we compute the total number of jobs at small incumbent firms and at firms created over the last two years. We then 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 VIII show that the reform had no effect on large incumbents' employment. The results show that in the postperiod, Q4 industries do experience two percentage point larger growth 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 increase in employment due to newly created firms.

[30] Since we use industry-level annual data, there are 2,610 observations in these regressions, which corresponds to a balanced panel of 290 industries followed over the 1999 to 2007 period. Note that in these regressions, we stop the sample in 2007, while in the previous analysis, we stop it in 2005 because we need to observe employment counts two years after a firm's creation and 2007 is the last year in our data.

Table X
New Firms versus Shrinking Incumbents

This table analyzes the crowding out of incumbent firms in response to new entrants. In Panel A, the dependent variable in columns (1) and (2) is total wages divided by number of employees across 265,586 observations; in columns (3) and (4), it is value-added per employee across 1,269,812 observations; and in columns (5) and (6), sales per employee across 1,258,595 observations. Shrinking incumbents are defined as incumbents whose employment decreased over the prior year. New firm is a dummy variable equal to 0 if the observation corresponds to a shrinking incumbent and to 1 if it corresponds to a newly created firm. POST is a dummy equal to 0 in the 1999 to 2001 period and to 1 for the 2002 to 2005 period. Qi% Sole Props is a dummy equal to 1 if the industry belongs to the i^{th} quartile of our treatment intensity variable. Quartile treatment × New firm are the interactions of Q2, Q3, and Q4 with the new firm dummy. In Panel B, the dependent variables are: In columns (1) and (2), TFP1 is the residual of a Cobb-Douglas production function estimated industry by industry. In columns (3) and (4), TFP2 uses the industry-level labor share as a coefficient in the industry-level Cobb-Douglas production function. All regressions include industry × year fixed effects. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry and tax files from the French Statistical Office, 1999 to 2005.

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***	7.0***	6.6***	9.3***	5.4***
	(0.39)	(1.6)	(0.37)	(0.78)	(0.51)	(1.9)
New firm × POST	0.014	0.18	0.19	0.62	0.23	1.8
	(0.18)	(0.39)	(0.15)	(0.55)	(0.29)	(1.1)
Q2 % Sole Props		−0.41		−0.22		−2.2
× New firm × POST		(0.54)		(0.65)		(1.3)
Q3 % Sole Props		−0.72		−0.94		−2.3*
× New firm × POST		(0.47)		(0.63)		(1.2)
Q4 % Sole Props		0.56		−0.25		−1.4
× New firm × POST		(0.53)		(0.6)		(1.2)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quartile treatment × New firm	No	Yes	No	Yes	No	Yes
R^2	0.16	0.16	0.12	0.12	0.20	0.20

B. Efficiency

This section investigates how the PARE reform affected overall allocative efficiency. Section V shows that the reform led to the massive entry of new firms in the most exposed industries. The previous section provides evidence that this entry of new firms led to a significant reallocation of resources across new and old firms. The overall effect of the reform on allocative efficiency depends on the relative productivity of entrants attracted by the reform and incumbents displaced by these entrants. In Table X, Panel A, we define displaced incumbents as (small) incumbents whose employment has been shrinking and

Table X—*Continued*

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 a coefficient 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 to 2001 period and to 1 for the 2002 to 2005 period. 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 prereform 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 *** denote statistically significant at the 10%, 5%, and 1% level, respectively. Source: Firm registry and tax files from the French Statistical Office. Sample: All new firms and small "shrinking" incumbents in the tax files, 1999 to 2005.

Panel B: Total Factor Productivity (TFP)

	TFP1		TFP2	
	(1)	(2)	(3)	(4)
New firm	0.16***	0.13***	0.33***	0.20***
	(0.006)	(0.023)	(0.013)	(0.057)
New firm × POST	−0.005	−0.002	−0.020***	0.029
	(0.003)	(0.014)	(0.006)	(0.038)
Q2 % Sole Props	0.004		−0.042	
× New firm × POST	(0.015)		(0.040)	
Q3 % Sole Props	−0.008		−0.051	
× New firm × POST	(0.015)		(0.039)	
Q4 % Sole Props	−0.002		−0.062	
× New firm × POST	(0.014)		(0.038)	
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^2	0.035	0.035	0.079	0.080

estimate

$$Y_{ist} = \sum_{k=1}^{4} a_k \cdot Q_s^k \times post_t \times New\ firm_{ist} + \sum_{k=1}^{4} \beta_k \cdot Q_s^k \times New\ firm_{ist} \qquad (5)$$
$$+ \gamma \cdot New\ firm_{ist} \times post_t + \zeta \cdot New\ firm_{ist} + \delta_{st} + \epsilon_{ist},$$

where Y_{ist} is a measure of productivity for firm i created in industry s in month t. We compute productivity using average wage (columns (1) and (2)), value-added per worker (columns (3) and (4)), and sales per worker (columns (5) and

(6)).[31] The variable $New\ firm_{ist}$ is a dummy equal to 1 for entrepreneurial firms created in year t and to 0 for "shrinking incumbents," that is, firms whose labor force decreases by at least one person count between t and $t+1$—including those incumbents who exit the sample in $t+1$. For each new firm created in year t, productivity is measured as of year $t+2$. We cluster standard errors at the industry level.

Columns (1), (3), and (5) of Panel A show that, prior to the reform, wages and productivity in newly created firms are larger than those of shrinking incumbents. Annual wages are about €5,200 larger, and value-added per worker is about €7,000 higher per year. These differences are 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 the productivity advantage of newly created firms does not change in the postreform 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 decrease 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 that this is not the case. The larger productivity observed for newly created firms does not increase differentially in the postreform period for firms in Q1, Q2, Q3, and Q4 industries.

In Panel B of Table X, we repeat these tests using total factor productivity (TFP). While TFP is more standard, we believe it less directly applicable to our set of very small firms, and thus we present this set of regressions for robustness. We obtain $TFP1$ as the residual of the following regression, where i is a firm in industry s and year t, and we use the universe of firms present in the tax files:

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

where L_{ist} is one plus firm i's total employment (thus setting employment of zero-employee firms to one), K_{ist} is firm i's fixed assets, and Y_{ist} is firm i's value-added. We obtain $TFP2$ 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 reestimate equation (5) comparing the productivity of new entrants to that of incumbent firms using these TFP measures as dependent variables and report the results in Table X, Panel B. The results are very similar to those in 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.[32]

Overall, despite low aggregate employment gains (Section VI.A), the evidence in this section suggests that the significant reallocation of labor from

[31] 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.

[32] In Internet Appendix Table IA.XVII, we obtain similar results when comparing the profitability of new firms to that of incumbents (Operating profit/Sales and Operating profit/Total assets).

small incumbent firms to new firms led to significant productivity gains at the industry level.

VII. Conclusion

In this paper, we look 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 attracted by the reform. New firms are estimated to create between 9,000 and 24,000 jobs annually. These results are in line with the experimentation view, which argues that the reform allows talented but potentially more risk-averse people to explore self-employment. We do not find that the downside insurance provided by the reform leads to significant adverse selection into self-employment.

The results also emphasize 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 among 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. In a tentative cost-benefit analysis (see Internet Appendix II for details), we show 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 inframarginal 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. Overall, the net cost of the reform is estimated to be about 10,000 euro per job. We note that several factors are missing from this analysis. For instance, accounting for greater industry dynamism and nonpecuniary benefits from shorter unemployment spells would lead to higher aggregate benefits.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication code.