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DO ENTRY REGULATIONS DETER ENTREPRENEURSHIP AND JOB CREATION? EVIDENCE FROM RECENT REFORMS IN PORTUGAL*

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We evaluate the consequences of a recent regulatory reform in Portugal, which substantially reduced the cost of firm entry. Our analysis uses matched employer–employee data, which provide unusually rich information on the characteristics of founders and employees associated with new firms before and after the reform. We find that the short-term consequences of the reform were as one would predict with a standard economic model of entrepreneurship: the reform resulted in increased firm formation and employment, but mostly among 'marginal firms' that would have been most readily deterred by existing heavy entry regulations. These marginal firms were typically small, owned by relatively poorly educated entrepreneurs, and operating in low-technology sectors (agriculture, construction and retail trade). In comparison to firms that entered in the absence of the reform, these marginal firms were less likely to survive their first two years.

Government regulation of firm entry can serve to protect insider interests and hold back entrepreneurship. Many scholars have thus argued that burdensome firm entry regulation can be a key deterrent to economic growth (for instance, see the well known work of De Soto, 1989). Two important strands of empirical work provide empirical evidence on the issue.

One set of studies entails cross-country comparison. These studies compare countries in terms of the stringency of entry regulation, and ask if that variation is correlated with measures of economic performance. For instance, Djankov *et al.* (2002) show that onerous entry regulation is associated with higher corruption and a higher concentration of activity in the informal sector. These scholars also find no evidence to suggest that high entry costs create value through improved quality of either publicly or privately provided goods. Many subsequent papers have used similar empirical strategies to evaluate the impact of business entry regulation, suggesting that such regulation is associated with reduced job creation (Ciccone and Papaioannou, 2007), higher industry concentration (Klapper *et al.*, 2006; Fisman and Sarria-Allende, 2010) and reduced entry of new firms (Klapper *et al.*,

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2006; Ciccone and Papaioannou, 2007; Bjørnskov and Foss, 2008; Dreher and Gassebner, 2013).[1]

Empirical work that relies on cross-national correlations leaves researchers with a difficult inference problem – an issue that receives an extended and insightful discussion in the original work of Djankov *et al.* (2002). While it is important to know that heavy entry regulation is associated with poor economic performance within countries, that correlation alone is not sufficient to assess the likely consequence of regulation reform in practice. After all, as Djankov *et al.* (2002) demonstrate, countries with heavy entry regulation also are less likely to have 'good government' along a number of dimensions. One might be concerned, for instance, that entry regulation reform, on its own, would have limited success in a country with other deep structural economic and political problems.[2]

As an alternative to cross-country analyses, a second complementary literature seeks to directly assess the consequences of policies that reduce firm entry costs using time, region and/or industry-specific variation in entry costs created by policies within particular countries. Bertrand and Kramarz (2002), for example, carefully evaluate entry barriers in France's retail industry. That paper presents evidence that stronger entry deterrence increased industry concentration and reduced employment growth. A second prominent example is the work by Aghion *et al.* (2008), which shows that the dismantling of the Licence Raj in India (a system of central controls that regulated entry and output expansion) resulted in industry growth, and did so in ways that were tied to other economic institutions of the states in which the reforms were occurring.[3] Other studies of particular relevance to our own research include analyses of entry regulation reform in Mexico over the 2002–6 period. Both Bruhn (2011) and Kaplan *et al.* (2011) suggest that the reform increased firm formation and employment. While these two papers differ in their estimates of the impact of the Mexican reform, they collectively represent an interesting and important advance in the literature upon which our own study builds. Like these earlier studies, we investigate the impact of entry regulation reform on firm formation and employment creation. Unlike them, we take the additional step of exploring the characteristics of firms this reform creates.[4]

Our study seeks to contribute to this second stream of research – by evaluating the impact of entry deregulation in Portugal. Prior to 2005 the barriers facing entrepreneurs in Portugal were among the highest in Western Europe, according to World Bank measures. In 2005 Portugal implemented the 'On the Spot Firm' programme (*Empresa na Hora*) which established 'one-stop shops' that offered prospective entrepreneurs significantly reduced administrative fees and simplified incorporation

[1] More generally, to date, nearly 200 academic articles have utilised either the original Djankov *et al.* (2002) data set or updated versions published by the World Bank, as part of the World Bank's Doing Business Project, which tracks regulatory reforms in 181 countries. The World Bank indicators are widely used. For example, the Millennium Challenge Corporation, set up by the US to channel aid to developing countries, uses these and similar indicators to screen applicants for US development aid. Djankov (2009) provides more examples and details.

[2] Recent empirical work by Commander and Svejnar (2011) highlights the perils of using cross-national analysis to draw inferences about the impact of 'business environment' on economic performance.

[3] See also Chari (2007) on the Licence Raj.

[4] See also research on Russian reform Yakovlev and Zhuravskaya (forthcoming) and entry regulation reform in Brazil (Monteiro and Assunção, 2006).

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procedures. The reform reduced the time delay of legal incorporation from several months to as little as one hour, and also reduced monetary fees that initially were on the order of 2,000 euro to less than 400 euro. Over the next several years, these one-stop offices were opened across the country. As a consequence of this reform, Portugal's ranking in the World Bank's 'Doing Business Index' moved from 113rd (out of 155 countries) to 33rd, and Portugal was cited by the World Bank as the top reformer in business entry regulation in 2005/2006. Portugal thus provides an excellent context in which to evaluate the impact of substantial entry regulation reform. What effect did this reform have on firm and job creation? What types of entrepreneurs benefited from this reform? How well did any new marginal firm entrants perform over time?

To answer our key questions, we use micro-level data and examine the implementation of the Portuguese business registration reform in different counties at different time periods. Our data provide detailed information on the new firms established in each county between 2000 and 2008. For each firm, we were able to gather information on size, founder characteristics and firm performance (firm productivity and survival). The fact that adoption of the reform varied across county and time is helpful for our identification strategy. The richness of our data allows us to compare the characteristics of new firms created by the reform to those that emerged prior to its implementation, providing a dimension of analysis that has been missing in the previous literature.

We find that in the short run, the Portuguese reform increased the number of business start-ups by approximately 17% and created approximately seven new jobs per 100,000 county inhabitants on a monthly basis. There is evidence to suggest that the 'marginal firms' – firms that entered as a consequence of reduced entry costs – were operated by proprietors who were disproportionately older, more likely to be female, and less educated than proprietors of infra-marginal firms. The marginal firms were typically low-tech, for example, in the agricultural, retail trade, and construction industries. We also find that start-ups established after the programme are smaller and are less likely to survive in the first two years than are firms founded in the absence of the programme. There is some evidence to suggest that these firms are also less productive and pay less to their employees.

Back-of-the-envelope calculations suggest that the overall impact of the programme was likely modest, though not negligible: over the first two years of the programme the total impact at a national level was in the order of 4,500 new, mostly small, firms and, over this same time span, job creation due to the reform was perhaps in the neighbourhood of 17,500.

In short, our results are just as one would predict with a standard economic model of entrepreneurship. Entry deregulation had a positive impact on firm and job creation in Portugal and that impact was observed among entrepreneurs who were plausibly near the margin in terms of the firm formation decision. Barriers that existed prior to reform were an impediment for smaller lower productivity entrepreneurs but less of an impediment to the larger, high-quality firms that are most likely to survive and to create substantial economic growth. These lessons are likely to extend beyond the Portuguese context of this study.

The remainder of this article is structured as follows. In Section 1 we provide a brief overview of the literature on entry regulation. Section 2 develops a simple model for

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the purpose of highlighting the expected effects of entry deregulation on entrepreneurial outcomes. Section 3 describes the Portuguese business simplification reform in further detail. A description of the data follows in Section 4. In Section 5 we present the empirical strategy, results and robustness checks. Section 6 concludes.

1. Entry Regulation: Theory and Existing Evidence

The literature often references two views of entry regulation, dubbed 'public choice' and 'public interest'. The first perspective regards regulation as a socially inefficient mechanism to create and extract rents. This view holds that regulation benefits bureaucrats and politicians by collecting bribes from entrants (De Soto, 1989) or benefits incumbent firms by deterring the competition of potential entrants (Stigler, 1971; Posner, 1975; Peltzman, 1976). In contrast, public interest theory provides a potential rationale for entry barriers: regulation might help correct market failures and achieve socially superior outcomes (Pigou, 1938), for example, screening new firms so as to reduce the prevalence of low-quality products or damaging externalities.

Recent empirical evidence does not appear to square with public interest theory. Many studies, some of which we have cited above, suggest that entry regulation indeed has the expected first-order effect of reducing firm entry and affecting the market structure.[5] Djankov (2008), among others, argues that the main beneficiaries of complex entry regulation are often incumbent firms, which see their rents and competitive position protected by burdensome entry requirements. Considerable effort has also been devoted to studying the link between regulation and macroeconomic outcomes such as employment, productivity and growth.[6] Stricter regulation is linked to slower growth, lessened productivity, inhibited investment and decreased employment (particularly in high-skilled jobs, according to Barseghyan (2008)). Some evidence suggests that entry regulation induces business to operate in the informal sector (Djankov *et al.*, 2002; Monteiro and Assunção, 2006).

In an effort to take the concerns raised by public interest theory seriously, some work focuses on the impact of regulation on potential market failures such as sub-standard product quality, pollution or public health (Djankov *et al.*, 2002; Bruhn, 2011; Yakovlev and Zhuravskaya, forthcoming). In general there seems to be little evidence of beneficial effects of entry regulation.

As we have mentioned, much of the existing work relies on cross-national variation. Causal inference here is difficult. For example, governments might regulate more heavily in countries where there are more market failures, or regulators might focus effort on rent extraction in countries where such extraction is made possible by other structural deficits in governance. Omitted variables can jointly drive economic outcomes and regulation. One potential path is to search for instruments that drive

[5] See, for example, Bertrand and Kramarz (2002), Djankov *et al.* (2002), Klapper *et al.* (2006), Ciccone and Papaioannou (2007) and Fisman and Sarria-Allende (2010).

[6] See, Bertrand and Kramarz (2002), Alesina *et al.* (2005), Djankov *et al.* (2006), Chari (2007), Aghion *et al.* (2008), Barseghyan (2008), Bruhn (2011), Kaplan *et al.* (2011) and Yakovlev and Zhuravskaya (forthcoming).

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regulation decisions,[7] or to draw lessons from such specific quasi-experiments as German reunification (Prantl and Spitz-Oener, 2009) or reform in Russia (Yakovlev and Zhuravskaya, forthcoming).

While useful, these approaches are not a substitute for empirical evaluations of actual policy shifts that change entry regulation. There are two reasons why our examination of the Portuguese policy shift holds particular promise. First, the policy shift was quite simple and, as we discuss below, dramatic. Second, we have access to extraordinary panel data on firms, their employees and their founders. Thus, in comparison to the extant literature, we are able to evaluate the impact on firm entry and job growth and also the *kinds* of start-ups and entrepreneurs that appeared to benefit from deregulation. Previous empirical work has suggested that entry regulation results in increased firm size (Desai *et al.*, 2003; Klapper *et al.*, 2006; Fisman and Sarria-Allende, 2010). Beyond that, little is known about the nature and quality of the firms that can be expected to enter when governments adopt deregulation.[8] This is a significant gap in the literature because the social impact of deregulation will be a function not only of the number of new firms brought into the market, but also of their quality, size, performance and longevity.

2. A Simple Model

To fix ideas and set the stage for the empirical analysis that follows, we develop a simple model here that draws on the logic given in the seminal work of Lucas (1978).

We evaluate an economy in which agents can choose between three options in each period: home production (or leisure), which provides monetised utility w; supplying one unit of labour in a competitive labour market, or becoming an entrepreneur and hiring other agents. In equilibrium, agents are indifferent between the first two possibilities, and we assume that there are some agents in each of the two possible categories. So to induce agents to supply labour, the labour market must pay wage w.[9] Agents pursue entrepreneurship when the expected rewards equal or exceed w.

We assume, as does Lucas (1978), that our economy is populated by individuals who have identical abilities as workers but who are endowed with different levels of entrepreneurial ability, θ, drawn from a continuous probability distribution $g(\theta)$, with support $[\theta_l, \theta_u]$. Entrepreneurial ability is a unique scarce resource, as in Schumpeter (1934), which allows individuals to create and manage firms effectively.

To make matters interesting, we introduce a dynamic element by allowing agents to live for two periods. In the first period, an agent with endowment θ can choose home production or work, in which case the payoff is w. Alternatively, he can become an

[7] See, Djankov *et al.* (2006), Dulleck *et al.* (2006), Barseghyan (2008) and Fisman and Sarria-Allende (2010).

[8] For example, in their studies on the consequences of the Mexican reform, Bruhn (2011) and Kaplan *et al.* (2011) do not address these issues.

[9] Our assumptions imply that labour supply is perfectly elastic. This is a particularly transparent case for analysis. We might alternatively have chosen to analyse the equally transparent case of perfectly inelastic labour supply, as in Lucas (1978). However, that case rules out an interesting question we want to examine: Does entry regulation affect equilibrium employment? The intermediate case, with upward-sloping supply, adds complication, so we do not pursue it here.

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entrepreneur and receive a payoff $\pi_1(\theta)$ that is a known function of his entrepreneurial ability plus a mean-zero term, say ε (drawn from a known distribution), that reflects uncertainty that cannot be resolved until a potential entrepreneur actually opens her firm. To keep analysis simple, we let the value created by a firm take a constant-returns-to-scale Cobb–Douglas form in entrepreneurial ability and labour employed.[10] The first-period payoff is

$$\pi_1(\theta) = \theta^{\frac{1}{2}} L^{\frac{1}{2}} - (wL + F) + \epsilon, \tag{1}$$

where L is the quantity of labour employed and F is a fixed cost to opening the firm. In the second period, the agent retains her (now revealed) idiosyncratic term ϵ but faces no additional fixed cost. So

$$\pi_2(\theta) = \theta^{\frac{1}{2}} L^{\frac{1}{2}} - wL + \epsilon, \tag{2}$$

if he continues to operate. If, instead, our entrepreneur closes his firm (as he might if he has a particularly poor ε draw), she earns w.

Given this set-up, it is easy to verify that an optimising entrepreneur with ability θ and idiosyncratic draw ϵ earns

$$\pi_1(\theta, \epsilon) = \frac{\theta}{4w} - F + \epsilon, \tag{3}$$

and

$$\pi_2(\theta, \epsilon) = \begin{cases} \dfrac{\theta}{4w} + \epsilon & \text{if } \epsilon \geq w - \dfrac{\theta}{4w}, \quad \text{and} \\ w & \text{if } \epsilon \leq w - \dfrac{\theta}{4w}. \end{cases} \tag{4}$$

Clearly, the value of operating as an entrepreneur depends on one's ability θ, relative to the market wage w. Importantly for our purposes, the value of entrepreneurship depends also on the level of the fixed cost F, and so too, therefore, does the decision to become an entrepreneur. To illustrate, let ϵ be drawn from a uniform distribution with support $[-\epsilon_u, \epsilon_u]$. Then with a bit of algebra it is easy to confirm that a risk-neutral agent who correctly forecasts expected profit will choose to open a firm if, and only if

$$\left(\frac{\theta}{4w} - w\right) + \frac{1}{4\epsilon_u}\left[\left(\frac{\theta}{4w} - w\right) + \epsilon_u\right]^2 \geq F, \tag{5}$$

(assuming a zero rate of time discounting).[11] Expression (5) makes sense. The first term in parenthesis on the left-hand side is expected pure profit from operating in the first period. In a one-period model, the entrepreneurship decision would hinge solely on whether this term was as large as the fixed entry cost. There is an option value,

[10] As part of our effort to keep things clear, we have no capital here and do not consider the issue of liquidity constraints (Evans and Jovanovic, 1989). Clearly, liquidity constraints can be important; for example, Kerr and Nanda (2009) show that banking regulation affected entry in the US, Nanda (2008) finds that an increase in the cost capital in Denmark reduces entrepreneurship and Hvide and Møen (2010) suggest that liquidity constraints might be relevant to Norwegian entrepreneurs. For liquidity-constrained potential entrepreneurs, business registration fees can only make matters worse, because capital must be raised to run the business and to pay the fee.

[11] The assumption that agents use Bayes rule correctly is standard. We thereby implicitly rule out overconfidence, which could lead to excess entry (Camerer and Lovallo, 1999).

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though, associated with continued operation in the second period, and this is represented by the second, strictly positive, term on the left-hand side of (5).

Let $\hat{\theta}$ be the value of θ that solves (5) with equality. Then the fraction of agents who become entrepreneurs is $1 - G(\hat{\theta})$, where $G(\ )$ is the c.d.f. (cumulative density function) for $g(\ )$. Inspection of (5) gives the intuitive result that $\hat{\theta}$ is strictly increasing in F; the lower the entry cost, the lower is the threshold that induces agents to open firms. In turn, the lower the entry cost, the higher will be the fraction of agents who become entrepreneurs.

Labour market employment in our economy of course depends on the level of entrepreneurship. In particular, an entrepreneur with ability θ employs $\theta/4w^2$ workers in period 1, and employs that same number in period 2 if she continues operation. So in our economy, a decrease in F increases employment (while decreasing the number of agents in home production). Notice that the firms that form as a consequence of a reduction in F will be operated by proprietors with relatively low levels of θ; these firms will tend to be small, since labour demand is proportional here to entrepreneurial ability θ.

Finally, we note that the probability of firm survival in period 2 is also related to θ. In particular, a bit of algebra can be used to confirm that a firm's survival probability is

$$s(\theta) = \frac{\theta}{8w\epsilon_u} + \frac{1}{2} - \frac{w}{2\epsilon_u}. \tag{6}$$

Firms with high-ability proprietors (which are also firms with relatively more employees here) have a higher survival rate. Given that the distribution of talent among operating firms is $g(\theta)/[1 - G(\hat{\theta})]$, average firm survival is

$$\bar{s} = \int_{\hat{\theta}}^{\theta_u} \left(\frac{\theta}{8w\epsilon_u} + \frac{1}{2} - \frac{w}{2\epsilon_u} \right) \frac{g(\theta)}{1 - G(\hat{\theta})} \mathrm{d}\theta, \tag{7}$$

which can be written

$$\bar{s} = \frac{1}{8w\epsilon_u} \mathrm{E}(\theta | \theta \geq \hat{\theta}) + \frac{1}{2} - \frac{w}{2\epsilon_u}. \tag{8}$$

A reduction in F results in a reduction in the cut-off $\hat{\theta}$, and this in turn reduces the mean ability level of entrepreneurs. The consequence is to reduce average firm survival.

To summarise, our model provides an intuitive set of predictions about the consequence of a reduction in the cost of firm entry, that is, a reform of the sort enacted in Portugal:

(*i*) Increased business formation: a higher proportion of agents become entrepreneurs.
(*ii*) Increased employment: a higher proportion of agents are employed; fewer are in home production.
(*iii*) All shifts occur at the margin:[12] new firms will generally have entrepreneurs with relatively lower talent. These firms will generally be smaller and less

[12] Amaral and Quintin (2006) also make this point.

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productive and will have lower survival probabilities than infra-marginal firms. Nanda (2008) and McKenzie and Sakho (2010) provide empirical evidence that business registration taxes negatively affect marginal firms.

It is worth noting that some predictions differ in the short run and long run. For example, in our model only young agents form firms, because only they can take advantage of the option value of continued operation if the firm proves to be particularly successful. A short-run consequence of reduced entry cost would be to cause some older entrepreneurs to form new firms. These would be agents who were deterred by high entry costs when they were young, but now find entrepreneurship to be more attractive. So in the short run, the average age of new entrepreneurs rises. In the long run, though, once again only young agents form firms.[13]

Additionally, we note that in our model artificial barriers that increase entry costs introduce inefficiency; these regulations induce some potential entrepreneurs to decide unnecessarily and inefficiently to work for others or remain out of the labour force. Empirical support of the model's key predictions thus constitutes useful *prima facie* evidence in favour of the proposition that reduced entry regulation is socially useful. If, to the contrary, one were to show little impact of entry deregulation, there would be less reason to be concerned about entry regulation as a practical concern. Conceivably, one might even find that reduced entry regulation reduces employment if markets are non-competitive.[14]

3. The 'On the Spot Firm' Programme

In this Section, we describe the Portuguese business registration reform and the setting in which it was implemented.

Prior to 2005, to meet the government requirements, an entrepreneur had to visit several different public agencies, complete 11 procedures, fill in 20 forms and documents, wait between 54 and 78 days, and pay almost 2,000 euros (approximately 13.5% of per capita gross national income). These numbers were high by international standards, making Portugal one of the least attractive countries in which to start a business.[15] Pressure to reform this system increased as the country's economic performance deteriorated after 2000. Between 1996 and 2000, the economy had experienced a period of reasonably good growth (GDP increased by approximately 4.0% annually) but from 2001 to 2005, growth fell to less than 1.0%.

[13] More generally, of course, it might be reasonable to suppose that experience can increase ability (and also ease liquidity constraints), in which case some older agents would form new firms. The central observation remains, though: agents face the need to recoup the fixed costs of starting a firm over a sustained period of operation, making entrepreneurship less attractive as workers come close to the age of retirement.

[14] See, for example, Mankiw and Whinston (1986) for a general discussion of free entry and social efficiency.

[15] For example, in 2005 an OECD member country required on average 6.5 procedures and 19.5 days, costing approximately 6.8% of the GNI per capita (World Bank, 2006). Djankov *et al.* (2002) present similar figures. In 2005 Portugal ranked 113 out of 155 countries in the Doing Business Ranking (World Bank, 2006) and if we consider only the waiting time to complete the registration procedures, it ranked 74 out of 85 countries studied by Djankov *et al.* (2002).

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Given this environment, in February 2005, the newly elected government decided to rethink the regulatory regime. In May 2005, a cross-departmental task force, the Office of Public Services and Reform, or *Unidade de Coordenação da Modernazição Administrativa* (UCMA), was created to oversee modernisation and simplification of public services. One of the first issues tackled by this office was the simplification of the process of starting a business. As a result, the UCMA implemented the 'on the spot firm' programme, or *Empresa na Hora* (ENH), with the goal of decreasing the time, cost and complexity of starting a business.

The programme allows the registration of single-shareholder companies, private limited companies, partnerships or public limited companies in a single office, the one-stop shop. However, it does not apply to governmental firms or firms involved in industries which require special authorisation, permits or industry-specific requirements.[16] Within one hour, an entrepreneur receives a corporate taxpayer number, social security number, commercial registration and declaration of business initiation. In order to increase the efficiency of the process, the UCMA developed standardised pre-approved articles of association, created lists of pre-defined firm names and eliminated outdated start-up formalities such as the registration of company books and the legal obligation to provide public deeds.

The law for this administrative model was approved 6 July 2005 (with *Decreto-Lei* 111/2005) and, at the same time, six one-stop shops were piloted in four different counties, Coimbra, Aveiro, Moita and Barreiro. The shops generally took advantage of pre-existing Trade Registry Offices and Business Formalities Centres by locating the shops in these facilities. Due to resource constraints, the programme was not implemented simultaneously in all counties. Rather, over time the programme expanded to other locations across the country. Table 1 presents the statistics on the timing and geographical variation of programme adoption from 2005 to 2009.

There was some resistance against the adoption of the programme, particularly from the Association of Notaries, which in 2007 threatened to take the 'on the spot firm' programme to court, but these efforts did not slow the programme's expansion. By the end of 2009, there were 162 one-stop shops located in 144 different counties (47% of total counties).[17] Although the shop locations are quite dispersed throughout

Table 1

One-stop Shop Programme Adoption

	2005	2006	2007	2008	2009	Total
Number of shops	19	27	31	36	49	162
Number of counties implementing	13	23	28	34	46	144
Number of counties >1 shop	6	3	1	2	0	12

Notes. The counties with more than one one-stop shop are: Aveiro, Braga, Castelo Branco, Coimbra, Guarda, Leiria, Lisbon, Loulé, Odivelas, Porto, Setúbal and Viseu.

[16] Table A1 (in online Appendix A) lists all the industries that have to be registered by traditional procedures. The non-eligible industries are mainly in the finance, insurance and transportation sectors.

[17] Portugal is subdivided into 308 counties, which are approximately one quarter of the size of US counties.

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Portugal, the programme targeted larger and economically important counties. At the same time that the programme was implemented, Portugal had local elections for county chief executives. Political affiliation does not seem to play a significant role in programme adoption.[18]

Because of the 'On the Spot Firm' programme, Portugal was considered the top reformer in business entry in 2005–6 and was rewarded with the European Enterprise Award in the Red Tape Reducing category by the European Commission.[19] Portugal's position on the international competitive rankings improved markedly, for example improving from the 74th to 40th percentile in the World Bank Red Tape Percentile Index, and rising to 33rd out of 155 countries in the annual Doing Business Ranking. Figure 1 compares the number and time of the procedures needed to register a firm before and after the introduction of the one-stop shop. After the reform, an entrepreneur could easily finish the registration process in one day, at a cost of 360 euro (or 300 euro if the company's focus is information technology or research and development).

Two features of the programme and institutional environment deserve additional discussion. First, at the end of 2008 there were three concurrent procedures to register a firm: traditional procedures, the 'On the Spot Firm' programme, and an online company incorporation (*Empresa Online*).[20] However, the online company registration was initially only accessible to lawyers, solicitors and notaries with a digital certification and subsequently it only became available to individuals with the

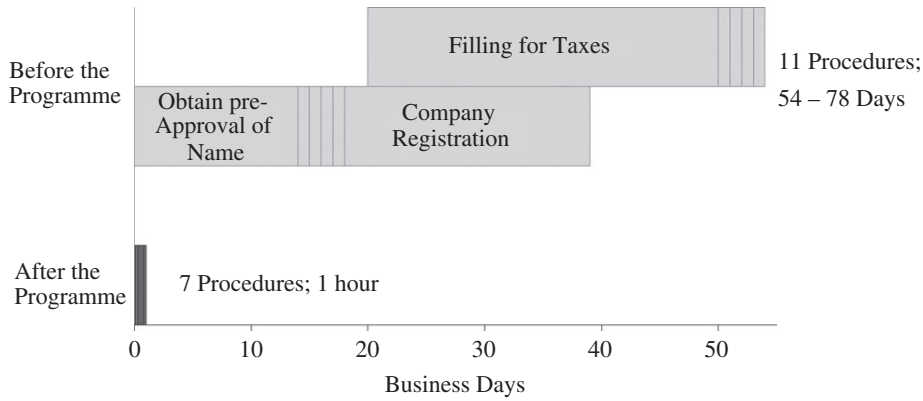

Fig. 1. *Start-up Procedures Before and After the 'On the Spot Firm' Programme*
Notes. The registration procedures are lined up sequentially. Business days required to complete each procedure are measured against the horizontal scale.

[18] Approximately 40% of the counties with one-stop shops had a chief executive that was from the government's party (PS) or from the main opposition party (PSD).

[19] World Bank (2006, p. 9), for example, noted the changes: 'In Portugal, now one of the fastest economies for start-up, an entrepreneur using the new fast-track service simply chooses a pre-approved name from the registry's website then goes to the one-stop shop to register the company'.

[20] This latter programme was the result of an initiative from June 2006, in which the government launched a special regime for setting up companies via the Internet (*Decreto-Lei* 125/2006), with similar steps as the 'On the Spot Firm' programme.

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citizens card (*cartão do cidadão*) and electronic certification in the last trimester of 2009.[21] Our data do not include information on the type of procedure used by entrepreneurs to register their firms. However, according to official information, more than 70% of new firms were established through the 'On the Spot Firm' programme until 2008. Traditional procedures and on-line registration were used much less frequently because of the cost and time of the former and the unavailability of the latter. Second, any individual or firm, anywhere in Portugal or abroad, can establish a firm in any one-stop shop, regardless of the location of the company's headquarters. Although firms are allowed to register in one county and operate in another, this is not the usual procedure; discussions with government officials strongly suggested that the fraction of firms registering outside their county of operation was trivially small. In short, over the time period we study, 2000–8, there was considerable variation across regions and time in business registration costs and procedures. We will exploit this variation in policy using the rich data described below.

4. Data and Descriptive Statistics

Data for our empirical analysis come from an extraordinary matched employer–employee data set (*Quadros de Pessoal or SISED – Sistema de Informação de Salários, Emprego e Duração do Trabalho*), built using a mandatory survey submitted annually by firms with at least one employee to the Portuguese Ministry of Employment and Social Security. These data include information on an average of 227,000 firms and two million individuals per year, covering virtually all employees and firms in the Portuguese private sector. As individuals and firms are cross-referenced by a unique identifier, the data make it possible to match founders with their firms characteristics. Each year, firms report their year of constitution, location, industry classification, number of employees, number of establishments, initial capital and ownership structure. At the individual level, the data provide information on gender, age, date of hire, education, occupation, working hours and earnings.[22]

We supplement these data with information from other sources. Information on the opening date of each one-stop shop was obtained from the Institute of Registration and Notarisation (*Instituto dos Registos e do Notariado*) at the Ministry of Justice.[23] County-level data on inhabitants, GDP, and an industrial production index are from the National Statistics Office.

Our matched employer–employee data include 177,595 start-ups established in eligible industries for which we have a precise founding date that lies between 2000 and 2008. We use these firms and their characteristics to study the impact of the 'on the spot firm' programme on firm entry and job creation. For these new firms, we identify the founders and their background history. We exclude firms for which we

[21] The citizens card is a non-mandatory document, introduced in February 2007 (*Lei* 7/2007) with the goal of allowing individuals to identify themselves when dealing with computerised services and to authenticate electronic documents.

[22] The online Appendix B provides further details on the database and variable construction.

[23] The complete list of counties with a one-stop shop by December 2009 and their respective opening date is provided in Table C1 (in online Appendix C).

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could not identify at least one owner or the background history of the founder.[24] We also restrict the sample to founders with age between 20 and 60. In total, we ended up with 139,868 founders of 94,586 new firms. This entrepreneurs sample is used to evaluate the impact of the programme on firm survival.

In addition, we draw a 30% random sample of all individuals who were employees in eligible firms between the period of 2000 and 2008 (excluding 2001, because worker data are unavailable for that year), within the age range 20–60, and with known labour market histories. Then we merge the latter sample with the entrepreneurs sample. This allows us to compare characteristics of firm founders and non-founders in specific years. In total, we then have a sample of 5,071,627 individuals, of whom 33,958 are entrepreneurs. These data allow us to evaluate the impact of the programme on the decision to become an entrepreneur.

Our basic empirical strategy, discussed below, is to compare county-level economic outcomes before and after the introduction of the one-stop shops. Table 2 provides various descriptive statistics for such counties.

In the first column we give statistics for the 12 months prior to the opening of the one-stop shop, in the second column we provide statistics for the 12 months after the opening and, in the third column, we record the difference. In general, the opening of a one-stop shop is associated with an increase in number of firms and jobs created (and this increase is statistically significant only in the former case). Firms established after the programme introduction have fewer employees, lower subsequent probability of surviving in the first two years and lower productivity as measured by the logarithm of initial sales divided by the initial number of employees. In terms of the founder characteristics, after the programme is introduced entrepreneurs are less likely to be male and are slightly older (differences that are not statistically significant) and are less experienced. To make the latter point, we focused on two categories of entrepreneurs: 'novice entrepreneurs', who have not previously established a firm but who do have previous labour experience, and 'habitual entrepreneurs', who have both previous entrepreneurial and labour experience. We notice that after the opening of one-stop shops, there is a 3% increase in the proportion of novice entrepreneurs and a 2% decline in habitual entrepreneurs.

5. Empirical Methodology and Results

We turn now to estimation of the impact of the Portuguese reform on entrepreneurial outcomes – entry, job creation and performance – using a simple difference-in-differences approach. The basic idea is compare outcomes for eligible firms and for individuals in counties in which a one-stop shop is introduced to counterfactuals which are formed by observed outcomes in counties prior to the reform and in counties that continue to have no one-stop shop. More precisely, we estimate regressions in which

[24] For the employees, the data include some cases in which the record changes in gender and year of birth. We consider observations with multiple changes in the gender or year of birth to be errors, corresponding to individuals whose identification number was not recorded, or wrongly identified by the respondent. We drop individuals whose gender and year birth change in more than 70% of the total number of observations.

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Table 2

Descriptive Statistics

	Before	After	Difference
(*a*) *Firm entry and job creation*			
Number of firms	191.0	214.7	23.69*
	(8.916)	(10.89)	(14.07)
	57	57	114
Job creation	600.3	630.8	30.45
	(39.33)	(40.66)	(56.57)
	57	57	114
(*b*) *Firm characteristics*			
Size	3.735	3.493	−0.241**
	(0.0816)	(0.0600)	(0.101)
	6,186	6,981	13,167
2 Year survival	0.833	0.801	−0.0329**
	(0.0109)	(0.0108)	(0.0153)
	1,165	1,379	2,544
Ln(sales/worker)	10.130	10.064	−0.066***
	(0.014)	(0.014)	(0.020)
	5,830	5,763	11,593
Ln(average wage)	6.239	6.253	0.013
	(0.006)	(0.006)	(0.009)
	4,762	4,485	9,247
(*c*) *Entrepreneur characteristics*			
Proportion male	0.667	0.659	−0.00714
	(0.00599)	(0.00567)	(0.00825)
	6,186	6,981	13,167
Average age	35.82	35.86	0.0375
	(0.114)	(0.109)	(0.158)
	6,186	6,981	13,167
Proportion novice	0.548	0.581	0.0336***
	(0.00646)	(0.00604)	(0.00884)
	5,943	6,668	12,611
Proportion habitual	0.168	0.152	−0.0158**
	(0.00485)	(0.00439)	(0.00654)
	5,943	6,668	12,611

Notes. The Table evaluates several characteristics of the new firms (number of new firms, jobs created by new firms, initial size, survival, sales per worker and average wage) and characteristics of the entrepreneurs (gender, age and type) in counties that opened one-stop shops in a 12-month window before the shop opened and a 12-month window after the shop opened. Robust standard errors are in parentheses and the number of observations is presented below. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

the outcome variable of interest is estimated as a function of month effects (to pick up seasonal variation), county-year fixed effects and our key indicator variable, which equals one when the reform takes place within a county. Our identification strategy relies, of course, on an assumption that the programme is not being rolled out in a way that correlates with unobserved pre-existing trends in the dependent variables of interest. For example, our strategy would give misleading inferences if the one-stop shops were purposely introduced in locations that initially were experiencing rapid job growth (not picked up by county × year fixed effects).

Below we conduct several analyses intended to establish the credibility of our identifying assumption. As an initial check of this issue, though, we investigated whether 'early adopting' counties (which established one-stop shops between July 2005

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and February 2008) differed from 'late adopting' counties (which established one-stop shops between March 2008 and December 2009), in terms of initial growth trends (2000–4) of total sales, sales of new firms, number of firms, number of new firms and purchasing power. We find no significant difference between these sets of counties.

5.1. *Firm Entry and Job Creation*

We start by estimating the effect of business registration reform on firm entry. Let Y_{cmy} be the number of newly formed firms per 100,000 inhabitants in county c, month m and year y. We estimate, for the 308 counties in Portugal and over the time period 2000–8,

$$Y_{cmy} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} v_{cy} + \delta I_{my} + \theta Z_{cmy} + \epsilon_{cmy}, \tag{9}$$

where α_m controls for seasonal effects in firm entry,[25] v_{cy} are county/year fixed effects, I_{my} is a monthly index of industrial production for the country, which further controls for economic activity, and, Z_{cmy} is the key variable of interest – an indicator variable that equals one at the opening month of the one-stop shop and all subsequent months (and zero otherwise). Notice that because of our inclusions of county-year fixed effects, identification here comes from comparison of firm entry rates within a particular county in months immediately before the introduction of the one-stop shop to the firm entry rates in the months immediately thereafter. Standard errors for this and all subsequent regressions are clustered at the county level.

Column (1) of panel (*a*) in Table 3 presents our first estimate. Our theory leads us to expect that the opening of a one-stop shop leads to an increase in firm entry, that is, the estimate of θ should be positive. In fact, we find that the introduction of the one-stop shop is associated with an increase in the number of new firms per 100,000 inhabitants of approximately 2.1, which is an approximately 17% increase.

Column (1) of panel (*b*) in Table 3 uses the same specifications to examine the impact of the one-stop shop reform on initial employment in the newly formed firms. In this case, the dependent variable is the initial number of employees of start-ups at the county level per 100,000 inhabitants. As expected, the coefficient is positive; we estimate that the reform is associated with an increase in initial employment in new firms of 7 per 100,000, corresponding to an increase of initial employment in new firms of approximately 22%.

An alternative, somewhat less flexible difference-in-differences specification for evaluating the impact of the one-stop shops is:

$$Y_{cmy} = \sum_{m=1}^{12} \alpha_m + \gamma(t) + \gamma(t)D_c + \delta I_{my} + \theta Z_{cmy} + \epsilon_{cmy}, \tag{10}$$

where we replace county-year fixed effects with $\gamma(t)$, a polynomial time trend and $\gamma(t)D_c$, which is the polynomial of time trend interacted with an indicator variable

[25] As it turns out, entry is generally stronger in the first three months of a year, slower in the summer, stronger in early fall and slower again in November and December.

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Table 3

Impact of 'On the Spot Firm' Programme on Entry and Job Creation

	County-year fixed effects (1)	Quadratic trend (2)
Panel (a) Firm entry		
One-stop shop ($Z = 1$)	2.081***	1.834***
	(0.643)	(0.509)
Economic activity index	0.016	0.292***
	(0.033)	(0.030)
Observations	32,648	32,648
Adjusted R^2	0.300	0.265
Panel (b) Job creation		
One-stop shop (Z = 1)	6.704**	4.975**
	(3.365)	(2.466)
Economic activity index	−0.030	1.420***
	(0.172)	(0.138)
Observations	32,648	32,648
Adjusted R^2	0.258	0.214

Notes. Column (1) reports the coefficients in (9) using county-year fixed effects and column (2) reports the coefficients in (10) using a quadratic time trend. The dependent variable in panel (*a*) is the number of new firms established per 100,000 inhabitants. In panel (*b*), the dependent variable is the number of employees at start-up firms per 100,000 inhabitants. Month fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

equal to 1 if the county has a one-stop shop (0 otherwise).[26] As mentioned before, Z_{cmy} is an indicator variable equal to 1 if there is a one-stop shop in county c in year y and month m. Column (2) in Table 3 reports results. We see that inferences with this latter specification are very similar to those made with specification (9).[27]

To examine our identification strategy – that administrative decisions to the open one-stop shops in particular counties and time periods are not correlated with existing trends – we estimate the following variant of our baseline regression:

$$Y_{cmy} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} v_{cy} + \delta I_{my} + \sum_{l=-10}^{11} \theta_l z_{lc} + \epsilon_{cmy}, \tag{11}$$

where z_{lc} is a set of indicator variables for the 10 months prior to the opening of a one-stop shop and the 10 months after the opening of the one-stop shop. Thus, for

[26] The polynomial time trend is quadratic and it assumes the following form: $\gamma(t) = \beta_1 \times (y - 1999) + \beta_2 \times (y - 1999)^2$, where y = 2000,…2008. A specification with a cubic trend gives similar results. Notice that by interacting the time trend with D_c we are allowing time trends to differ in counties with and without the one-stop shops.

[27] Given that specification (10) seems to work reasonably well for our inferential problem, we can proceed with a comparable specification that estimates the effect of the programme on the number of employees working in established firms. Given our identifying assumption – that counties with one-stop shops are not experiencing secular improved business conditions unrelated to policy changes – we expect there to be no impact on employment. The reason we must use a specification like (10) here is that we have data for employment in established firms only at the annual level, so we cannot rely on the within-year identification strategy of (9). Importantly, we find that the coefficient on the one-stop shop indicator variable is not statistically significant (and is, indeed, slightly negative); our exercise provides no evidence that the positive estimated impact of reform on new firm formation and new employment is due to a spurious correlation with unobserved county-level employment trends.

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instance, z_{-1} is equal to one in the month prior to the opening of the one-stop shop (otherwise 0), while z_1 is equal to one for the month after the opening of the one-stop shop (otherwise 0). The only exception is z_{11}, which is equal to one for month 11 and for all months that follow. We set θ_{-1} to 0 (i.e. let that be the 'omitted' indicator variable). Figures 2 and 3 plots the coefficients θ_l, for firm entry and employment regressions, respectively.

Consider Figure 2. The coefficients on the 'lags' are generally close to 0; using an F test, we cannot reject the hypotheses that $\theta_{-10}, \ldots, \theta_{-1} = 0$ or that $\theta_{-5}, \ldots, \theta_{-1} = 0$. This suggests that on average, in the months leading up to the establishment of the one-stop shop there was *no* unusual trend in the establishment of new firms. This, in turn, gives us increased confidence in our identifying strategy. In contrast, in the months following the establishment of the one-stop shop, we generally have positive and statistically significant coefficients, which we expect, given the estimates from our baseline regression (9). Our F-test easily rejects the null hypothesis that these coefficients jointly equal 0. Figure 3 shows comparable results for our analysis of employment at newly established firms.

It is possible, of course, that over a short horizon, say three months, some entrepreneurs might have postponed the registration of their new firms in anticipation of the policy change. If we drop information on the three months before the opening of the one-stop shops and repeat our analyses, this makes virtually no difference in our inferences.

Our research design is well suited for evaluating the short-term impact of the regulatory reform but of course we have less to say about long-run effects. The reason, of course, is that the reform began in 2005 and we have data only through 2008. Even when additional data become available, though, inference is likely to become less precise as we look at outcomes that are further removed from the identifying event (i.e.

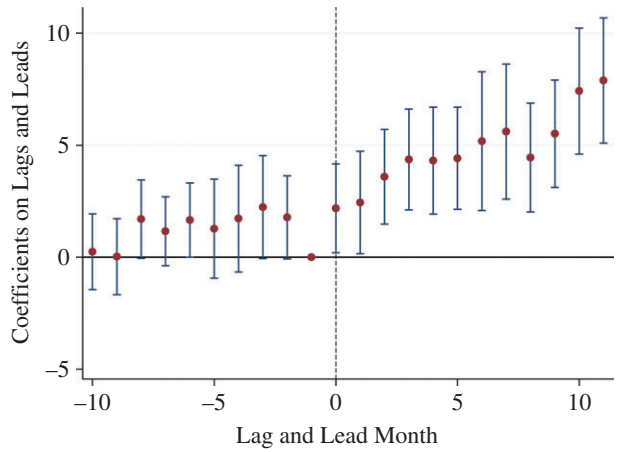

Fig. 2. *Coefficients on the Programme Month Dummies for Firm Entry*

Notes. The Figure plots the coefficients θ_1 from (11), with a dependent variable, number of firms established in a country per 100,000 inhabitants. Vertical lines are the 95% confidence intervals for the coefficients. The *F*-statistic for pre period for the last 10 lags is 1.20 ($p = 0.296$) and for the last five lags is 1.58 ($p = 0.179$); *F*-test statistic for post period is 3.10 ($p < 0.001$).

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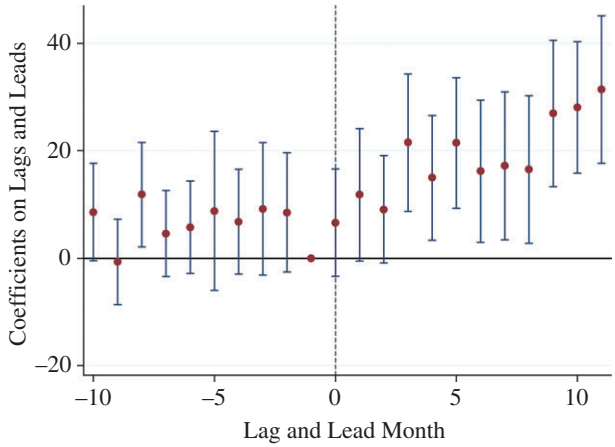

Fig. 3. *Coefficients on the Programme Month Dummies for Job Creation*
Notes. The Figure plots the coefficients θ_1 from (11), with a dependent variable, number of employees at newly established firms in a country per 100,000 inhabitants. Vertical lines are the 95% confidence intervals for the coefficients. The *F*-statistic pre period for the last 10 lags is 1.54 ($p = 0.135$) and for the last five lags is 1.23 ($p = 0.299$); *F*-statistic for post period is 3.07 ($p < 0.001$).

the establishment of a one-stop shop). Having said that, recall that the coefficient on 'month 11' is for that month and subsequent months in our data. Figures 2 and 3 shows that coefficient if anything is larger than the other coefficients, giving little indication that the impact is dying out over our period of analysis.[28]

Some counties have more than one one-stop shop by the end of the sample period. We find that our results (here and in other specifications) do not qualitatively change when we exclude these counties from the 'treatment' group. Additionally, we also study the effect of opening a second office within a county.[29] We find that the opening of the second office created approximately 3 new firms and 14 new jobs per 100,000 county inhabitants per month in eligible industries.

Another concern is that entrepreneurs in counties that border another county with a one-stop shop might cross borders to register in the other county and then operate in their county of residence. This would lead us to underestimate the causal effect of the one-stop shop on observed effects, because some one-stop shops would 'treat' individuals in the 'control' counties. Conversations with officials in the one-stop shop programme indicated that it was very rare for entrepreneurs to register in a county

[28] Following up on that observation, we tried estimating our baseline regression (9) but using dependent variables 'Employment after 1 Year' and 'Employment after 2 Years.' To estimate these regressions, though, we could only use data for firms established in 2006 or earlier. Given that the policy change only began in late 2005 this gives us little power. In any event, the point estimates for the coefficient on the one-stop shop indicator variables are 7.31 (SE = 7.87) when we use the dependent variable 'Employment after 1 Year' and 5.96 (SE = 8.43) when we use the dependent variable 'Employment after 2 Years'. In short, the estimated impact one to two years out is essentially the same as estimated in our baseline specification in panel (*b*) of Table 3, which is 6.70 (SE = 3.37), but inference is very imprecise.

[29] To draw this inference, we estimate regression (9) using the sub-sample of counties in which there was already a one-stop shop and now Z_{cmy} is an indicator variable for the introduction of a second one-stop shop.

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other than their county of operation. Nonetheless we cannot rule out this possibility, and we note that if the practise were widespread this would cause us to under-estimate the impact of the one-stop shops. We did experiment with aggregating counties where this border-crossing was more likely and these alternative specifications produce similar results to those reported here.

5.2. *New Firm Characteristics*

Our theory leads us to believe that any impact of the reform – the establishment of the one-stop shops – should be found in 'marginal firms', which in our theory are also smaller less-productive firms. So we estimate our key regression (9) but for firms categorised by the firm's initial size. Columns (1)–(4) of Table 4 report the coefficient when we consider the number of newly established firms with one, two, three to five and more than five employees, respectively. Our estimates indicate statistically significant increases in the number of firms with two employees and three to five employees, but no statistically significant increase in larger firms.

Next, the number of new firms is broken down into 10 sectors: agriculture, construction, high-technology industries, low-technology industries, utilities, wholesale trade, retail trade, real estate, services and communities.[30] Table 5 presents the results for this analysis, again using (9). The estimated positive impact of the reform is found to operate in three sectors: agriculture, construction and retail trade.

Table 4

Impact of 'On the Spot Firm' Programme on Firm Size

	One employee (1)	Two employees (2)	Three to five employees (3)	More than five employees (4)
One-stop shop ($Z=1$)	0.568	0.592**	0.630**	0.276
	(0.424)	(0.240)	(0.261)	(0.211)
Economic activity index	−0.018	0.039**	0.015	−0.020*
	(0.020)	(0.015)	(0.018)	(0.011)
Observations	32,648	32,648	32,648	32,648
Adjusted R^2	0.122	0.109	0.173	0.180

Notes. Regressions include county-year and month fixed effects. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

[30] We use the definition of OECD (2002) to divide firms into high-technology and low-technology industries. 'High-technology industries' include the following sectors: office and computing machinery, radio, TV and communication equipment, medical, precision and optical equipment, aircraft and spacecraft, chemicals, machinery and equipment, electrical machinery and apparatus, motor vehicles and trailers, railroad and transport equipment. 'Low-technology industries' include coke, refined petroleum products and nuclear fuel, rubber and plastic products, other non-metallic mineral products, basic metals, fabricated metal products except machinery and equipment, building and repairing ships and boats, food products, beverage and tobacco, textile and textile products, leather and footwear, wood, pulp and paper products, printing and publishing, and recycling. We acknowledge that this is a rough taxonomy; the low-technology category could include some technologically progressive firms and *vice versa*.

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Table 5

Impact of 'On the Spot Firm' Programme on Firm Industry

	Agriculture (1)	High-technology industries (2)	Low-technology industries (3)	Construction (4)	Utilities (5)
One-stop shop ($Z = 1$)	0.741***	0.0553	0.230	0.753***	−0.0109
	(0.200)	(0.0463)	(0.179)	(0.250)	(0.0210)
Economic activity index	−0.00610	0.00539***	−0.00733	0.0201	5.33e−05
	(0.0102)	(0.00202)	(0.00958)	(0.0163)	(0.00130)
Observations	32,648	32,648	32,648	32,648	32,648
Adjusted R^2	0.067	0.068	0.205	0.194	0.004

	Wholesale trade (6)	Retail trade (7)	Real estate (8)	Services (9)	Communities (10)
One-stop shop ($Z = 1$)	0.201	0.503*	−0.0280	−0.408	−0.117
	(0.130)	(0.272)	(0.0930)	(0.290)	(0.122)
Economic activity index	−0.00228	−0.00457	−0.00327	0.0110	0.00383
	(0.00896)	(0.0140)	(0.00305)	(0.0135)	(0.00568)
Observations	32,648	32,648	32,648	32,648	32,648
Adjusted R^2	0.077	0.095	0.060	0.111	0.037

Notes. The Table reports estimated coefficients for (9). The dependent variable is the number of new firms established per 100,000 inhabitants divided in 10 industries: agriculture, construction, high-technology industries, low-technology industries, utilities, wholesale trade, retail trade, real estate, services and communities. We use the definition from OECD (2002) to classify high and low-technology industries. Month and county-year fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

5.3. *New Firm Performance*

As mentioned in our article's introduction, a distinctive feature of our data is the ability to examine the impact of the reform on the performance of newly formed firms.

We begin by examining the two-year survival rate, that is, the variable S_{ifcmy}, firm survival for founder i in firm f, county c, month m and year y. This dependent variable is 1 if the start-up is still operating after two years, and 0 if not. It is taken to be a function of observables, as follows:

$$S_{ifcmy} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} v_{cy} + \boldsymbol{\tau} \boldsymbol{X}_i + \theta Z_{cmy} + \delta I_{my} + \epsilon_{ifcmy}, \tag{12}$$

where again we have indicators for month (α) and county-year (v) fixed effects. In some specifications we also include a vector of founder characteristics $\boldsymbol{X}_i$: gender, which equals 1 for men, 0 for women; four indicator variables for the founder's age, partitioned at 20, 30, 40 and 50; an 'industry experience' (Ind) variable which equals 1 for founders that previously work on the same four-digit industry digit code, 0 otherwise; and education, which is taken to be 'very low' for those never completing elementary school, 'low' for those that attended junior high school, 'medium' for those with a high school diploma or equivalent, and 'high' for those reporting bachelor's degree or more advanced degree. In reporting the estimated coefficients, our omitted categories are founders aged 20–29 and with 'very low' education. Finally,

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we include an economic activity index and, of course, we have an indicator variable for the one-stop shop. Our primary interest is in the coefficient (θ) on that variable.

Columns (1) and (2) of Table 6 give estimates. In the first column we include as covariates only the month and year-county fixed effects and the economic activity index. The negative coefficient on the one-stop shop variable indicates that firms formed after the policy was in place are approximately four percentage points less likely to survive two years later than firms formed in the absence of the reform. We then add the demographic covariates. In general, start-up firm survival is higher for firms founded by experienced individuals, by men (in comparison to women), by relatively older individuals and by relatively well-educated individuals. As we show below, there is some evidence that the reform changes the demographic composition of firm founders. Still, inclusion of the covariates has little effect on our key inference about the coefficient on the one-stop shop. Similarly, as noted above, the policy change appears to have had an impact on industry composition of new firms. Nonetheless, if we add industry fixed effects to the regression reported in column (2), the coefficient on the one-stop shop is still estimated to be −0.044 (SE = 0.021).

We can take an alternative approach to our preferred specification that uses year-county fixed effects and, instead, use a quadratic time trend interacted with being a one-stop shop county. This analysis exactly parallels our empirical work above (10). Results without individual-level covariates are reported in column (3) of Table 6, and results with those covariates are reported in column (4). Our inferences are quite similar to those with the fixed effects specification. Here we estimate that the reform results in an approximately three percentage point drop in the two-year firm survival rate for newly formed enterprises.

As in the analyses presented above, we also estimate the impact of the one-stop shop programme on our survival variable, using estimates for lag and lead months. Figure 4 plots the coefficients on the lag and lead indicator variables, which were constructed using the same methodology as presented in subsection 5.1. As can be seen in Figure 4, the fraction of surviving start-ups is close to zero before the opening of the one-stop shop and then it becomes jointly significantly negative from months 0 to >10.

We have two additional sets of analysis intended to inform us about the relative quality of 'marginal firms' (i.e. those associated with the introduction of the one-stop shops). The idea is to repeat the analyses reported in Table 6, but now use the following dependent variables: first we use a 'productivity' measure, the logarithm of initial sales divided by the initial number of employees (with sales expressed in 2008 euro). Second, we use the logarithm of employees' monthly average wages (but not the employer's compensation as we do not have those data).

Table 7 gives the results for sales per worker. Unfortunately our results are not definitive. Using our conservative fixed effects specification, the key coefficient on the one-stop indicator variable is close to zero and is not statistically significant. On the other hand, with the difference-in-differences specification with a quadratic time trend, we show that the introduction of one-stop shops is associated with a decline in sales per employee.[31]

[31] If we include industry indicator variables, this coefficient drops to −0.066 (SE = 0.02).

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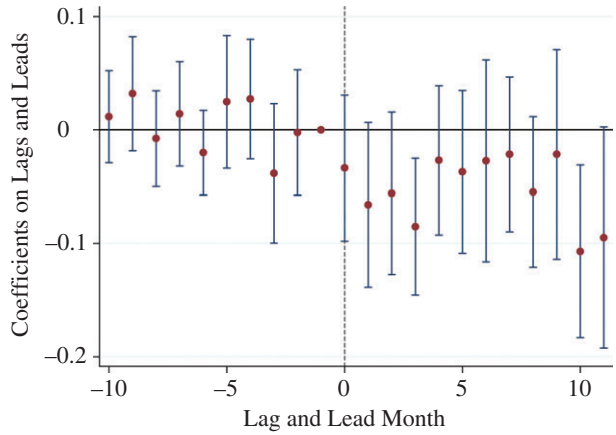

Fig. 4. *Coefficients on the Programme Month Dummies for Two-year Survival*
Notes. The Figure plots the coefficients θ_1 from (11), with a dependent variable equal to 1 if the firm survived two years. Vertical lines are the 95% confidence intervals for the coefficients. The *F*-statistic for pre period for the last 10 lags is 0.65 ($p = 0.756$) and for the last five lags is 1.02 ($p = 0.399$); *F*-test statistic for post period is 3.69 ($p < 0.001$).

Table 6
Impact of 'On the Spot Firm' Programme on Survival

	County-year fixed effects		Quadratic trend	
	(1)	(2)	(3)	(4)
One-stop shop ($Z = 1$)	−0.041**	−0.044**	−0.030*	−0.026*
	(0.020)	(0.021)	(0.015)	(0.015)
Economic activity index	−0.000	0.000	−0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Ind		0.015***		0.016***
		(0.003)		(0.003)
Gender		0.010***		0.009***
		(0.002)		(0.002)
Age 30–39		0.028***		0.029***
		(0.003)		(0.003)
Age 40–49		0.033***		0.033***
		(0.003)		(0.003)
Age 50–60		0.042***		0.042***
		(0.004)		(0.004)
Low education		0.006*		0.005
		(0.003)		(0.003)
Medium education		0.009**		0.007
		(0.005)		(0.004)
High education		0.048***		0.045***
		(0.005)		(0.005)
Observations	118,193	112,123	118,193	112,123
Adjusted R^2	0.035	0.038	0.024	0.028

Notes. In columns (1) and (2), we include county-year fixed effects. In columns (3) and (4) we include a quadratic time trend interacted with an indicator variable for a one-stop shop. The dependent variable is two-year survival (1 if the firm survived, 0 if not). All regressions include month fixed effects. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

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Table 7

Impact of 'On the Spot Firm' Programme on Sales per Worker

	County-year fixed effects		Quadratic trend	
	(1)	(2)	(3)	(4)
One-stop shop ($Z = 1$)	−0.015	−0.019	−0.098***	−0.096***
	(0.049)	(0.045)	(0.022)	(0.022)
Economic activity index	0.003	0.002	−0.008***	−0.007***
	(0.003)	(0.003)	(0.002)	(0.002)
Ind		−0.084***		−0.091***
		(0.016)		(0.017)
Gender		0.174***		0.177***
		(0.015)		(0.016)
Age 30–39		0.117***		0.112***
		(0.010)		(0.010)
Age 40–40		0.191***		0.189***
		(0.013)		(0.012)
Age 50–60		0.254***		0.259***
		(0.015)		(0.014)
Low education		0.206***		0.217***
		(0.011)		(0.011)
Medium education		0.382***		0.397***
		(0.016)		(0.017)
High education		0.362***		0.373***
		(0.034)		(0.033)
Observations	106,862	101,689	106,862	101,689
Adjusted R^2	0.109	0.129	0.085	0.106

Notes. In columns (1) and (2), we include county-year fixed effects. In columns (3) and (4) we include a quadratic time trend interacted with an indicator variable for a one-stop shop. The dependent variable is the logarithm of initial sales divided by the initial number of employees. Sales are expressed in 2008 euro. All regressions include month fixed effects. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5% , *significance at 10%.

Table 8 shows the results for the average wages of newly created firms. The dependent variable is the log of average wage. In our theory, there is no impact on the market wage of reducing fixed entry costs, but this is the consequence of our assumption that the supply of labour is perfectly elastic. In principle, wages might rise, if the policy has a large effect on aggregate labour demand. On the other hand, there are good reasons to think that the marginal lower-productivity firms that are entering might generally offer lower wages than inframarginal firms. In any event, using either specification we infer the new firms established in counties with a one-stop shop have pay that is approximately 2–3% less than other newly established firms. However, the result is statistically significant only in one specification, and in that case is only marginally significant.

We conclude that marginal firms established as a consequence of the entry regulation reform likely had relatively lower survival rates. Evidence suggests that they may have lower sales per worker and slightly lower pay, but we are less certain about these latter claims.

5.4. *Entrepreneur Characteristics*

Our final piece of analysis comes from the individual-level data. We explore the impact of the one-stop shop programme by estimating an equation in which the

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Table 8

Impact of 'On the Spot Firm' Programme on Employee Wages

	County-year fixed effects		Quadratic trend	
	(1)	(2)	(3)	(4)
One-stop shop ($Z = 1$)	−0.025	−0.032	−0.019*	−0.019*
	(0.026)	(0.026)	(0.011)	(0.010)
Economic activity index	0.001	0.001	0.001	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Ind		0.010**		0.009**
		(0.004)		(0.004)
Gender		0.075***		0.075***
		(0.008)		(0.007)
Age 30–39		0.044***		0.044***
		(0.006)		(0.006)
Age 40–49		0.068***		0.069***
		(0.006)		(0.006)
Age 50–60		0.107***		0.107***
		(0.009)		(0.009)
Low education		0.049***		0.049***
		(0.006)		(0.006)
Medium education		0.117***		0.116***
		(0.012)		(0.011)
High education		0.258***		0.257***
		(0.025)		(0.024)
Observations	74,698	70,771	74,698	70,771
Adjusted R^2	0.083	0.123	0.072	0.112

Notes. In columns (1) and (2), we include county-year fixed effects. In columns (3) and (4) we include a quadratic time trend interacted with an indicator variable for a one-stop shop. The dependent variable is the logarithm of the average of employees' monthly wages. Wages are expressed in 2008 euro. All regressions include month fixed effects. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5% , *significance at 10%.

dependent variable is the probability that any individual in our data potentially founds a start-up firm in a given year. Specifically, we start with the simple linear probability model:

$$E_{icy} = \sum_{y=2000}^{2008} \beta_y + \sum_{c=1}^{308} v_c + \delta G_{ny} + \theta Z_{cy} + \tau \boldsymbol{X_i} + \epsilon_{\boldsymbol{icy}}, \tag{13}$$

where E_{icy} is an indicator variable that equals 1 if individual i establishes a firm in county c and year y, and 0 if that individual does not open a firm. We also control for economic activity with a measure of GDP, G_{ny}, for 30 NUTS ('Nomenclature of Unit for Territorial Statistics') regions defined for Portugal. We also include a vector of individual characteristics $\boldsymbol{X_i}$ (measures of age, gender, and education). Finally, Z_{cy} is an indicator variable equal to 1 if there is a one-stop shop in county c in year y for at least part of the year.[32] As noted above, we use a 30% random sample of our data, giving us more than five million individuals but fewer than 34,000 who transition into entrepreneurship. In other words, less than 0.68% of the sample are individuals who start a new firm.

[32] Our data for this regression are annual, so we cannot look at within-year effects as in the analyses above.

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Table 9

Impact of 'On the Spot Firm' Programme on Type of Entrepreneurs

	Decision of becoming an entrepreneur	
	(1)	(2)
One-stop shop ($Z = 1$)	0.000983*	
	(0.000560)	
Gender	0.00363***	0.00386***
	(0.000319)	(0.000288)
Age 30–39	0.00215***	0.00203***
	(0.000194)	(0.000250)
Age 40–49	0.00142***	0.00121***
	(0.000164)	(0.000253)
Age 50–60	0.000133	−0.000316
	(0.000175)	(0.000293)
Low education	0.00241***	0.00229***
	(0.000253)	(0.000326)
Medium education	0.00589***	0.00588***
	(0.000647)	(0.000702)
High education	0.00903***	0.00949***
	(0.000797)	(0.000844)
(Gender) × Z		−0.000855***
		(0.000251)
(Age 30–39) × Z		0.000468
		(0.000311)
(Age 40–49) × Z		0.000768*
		(0.000408)
(Age 50–60) × Z		0.00157**
		(0.000624)
(Low education) × Z		0.000362
		(0.000300)
(Medium education) × Z		−4.23e-07
		(0.000153)
(High education) × Z		−0.000412**
		(0.000160)
ln(GDP)	−0.0118***	−0.0122***
	(0.00337)	(0.00336)
Constant	0.0873***	0.0907***
	(0.0273)	(0.0271)
Observations	5,070,999	5,070,999
Adjusted R^2	0.004	0.004

Notes. The Table reports estimated coefficients for (13). The dependent variable equals 1 if the individual is an entrepreneur, 0 otherwise. Year and county fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. ***Statistical significance at 1%, **significance at 5%, *significance at 10%.

The estimates for this specification are presented in column (1) of Table 9. Given results above, we are not surprised that the presence of a one-stop shop is associated with an increase in the probability an individual becomes an entrepreneur. Still, we view the results as important because it comes from an independent data set. Given the small fraction of the sample that ever becomes an entrepreneur, the coefficient on the one-stop shop indicator variable is associated with an marginal increase of about 14.5%. As for demographic and education variables, we can infer that male, middle aged (age between 30 and 39) and well-educated individuals are relatively more likely to transition into entrepreneurship than are other individuals.

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Our model predicts that entry regulation reform leads to an increase in the entry of 'marginal entrepreneurs' who might differ in terms of demographic characteristics from their infra-marginal counterparts. To evaluate that claim we interact each of the demographic and educational variables with Z_{cy} (our indicator that the reform has occurred in the county in that year) and add these interactions to (13). The coefficient estimates are reported in column (2) of Table 9. These results suggest that the entrepreneurs induced into the market by the establishment of the one-stop shop are from demographic categories that were previously least likely to initially be entrepreneurs. Relative to the omitted category (very low educated individuals), the fraction of highly educated entrepreneurs decreases by 0.4%. Similarly, marginal entrants appear to be older (individuals aged 40–50 and 50–60) and are less likely to be male.

One possible impact of an entry regulation reform is that existing active firms in the unregistered/informal sector might be induced into the formal sector. While there may be advantages to such a shift, if most 'new firms' are merely new registrations, this certainly changes the interpretation of reform's impact. This is a potential concern in our case because Portugal has a large informal sector (accounting for approximately 22% of GDP according to Antunes and Cavalcanti (2007)). An important advantage of our individual-level panel data is that we have a record of the labour market activity for individuals prior to their registering a new firm. In fact, we find that the majority of firms created were established by these 'novice entrepreneurs' who were previously working as paid employees. These do appear to be new establishments.

6. Conclusion

This study uses Portuguese micro-level data to analyse the effects of a programme that substantially reduced entry regulation for business. The reform, which introduced one-stop shops in counties throughout the country, appears to have increased firm entry and employment in new firms. We also find that the start-ups created in response to the reform are relatively smaller, headed by relatively inexperienced and poorly educated entrepreneurs and, in general, are less likely to survive their first two years. There is some evidence that they have lower sales per worker and slightly lower wages than start-up firms prior to the reform. It appears that new jobs were created primarily in such low-technology sectors as agricultural, retail trade and construction.

We view the results as consistent with the theoretical approach to entrepreneurship set out in this study. Using a model inspired by Lucas (1978), we show that artificial entry costs deter entrepreneurship and can be expected to do so at the margin. The reduction of entry costs is predicted to affect entrepreneurship and job formation in just the way we observe. Our results, then, can be seen as broadly consistent with the 'public choice' approach to entry regulation, which emphasises the inefficiencies associated with entry regulation.

In the Portuguese case, the reform was substantial. Portugal undertook one of the most complete and thorough deregulation efforts of any country in Western Europe, moving up 80 places in the World Bank's Doing Business Index and winning international accolades for the government in the process. This reform appears to have

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induced statistically significant, economically meaningful increases in firm formation and job creation. Our key estimates (from Table 3) indicate that the reform increased the number of monthly start-ups in eligible industries by approximately 17%. The effect on employment was an increase of seven new jobs per month per 100,000 inhabitants – a 22% increase in job formation by new firms.

In short, the reform worked as intended. Still, the gains were likely quantitatively modest because of the characteristics of new firms induced into the market by the reforms. To see this, we try the following back-of-the-envelope calculations: suppose, based on our estimates, the reform is responsible for two new firms and seven new jobs per 100,000 inhabitants per month. If all Portuguese counties received the programme, then after two years there would be roughly 4,500 new firms creating approximately 17,500 new jobs.[33]

The estimated impact of the reform is non-trivial but, to put it in context, we note that at the start of the reform (2005) the Portuguese labour force was approximately 5.5 million, with more than 400,000 unemployed. So the reform likely made only a small ripple in a poorly performing labour market. In this regard our analysis of the Portuguese regulatory reform is similar to the assessment of the 2002 Mexican regulatory reform by Kaplan *et al.* (2011). They find that the reform increased firm formation but primarily among small firms (i.e., firms with 10 or fewer employees) and, overall, the impact on the labour market was small.[34]

Nevertheless, the Portuguese regulatory reform was achieved with relatively low costs. According to government officials, the majority of the investment required was spent in training activities. The programme took advantage of pre-existing resources and facilities and the workers were re-allocated from the previous procedures to the new programme. Overall, this reform created few jobs at relatively low costs.

This conclusion comes with some caveats. First, we have adopted a conservative empirical specification that attempts to control as completely as possible for differential trends in firm creation at the county level; the conservative nature of our approach could bias our estimates downward. For example, perhaps some of the marginal firms are credit constrained and rely on cash flow to grow. Longer run effects then might be under-estimated by our relatively short window of analysis. Second, the reform was implemented in a difficult macroeconomic context. Since 2005, growth in the Portuguese economy has been weak, consumption and investment have remained depressed, and unemployment has been high (and increasing). When the macro-economic environment improves, the new administrative regime could have a stronger effect on firm and job creation than is evidenced by our analysis. Finally, in thinking about the generality of the results for other economies, it is important to note that the impact of entry barriers might depend on other factors in a country's economy, for example, imperfections in financial markets (as discussed in De Soto (2000) and Antunes and Cavalcanti (2007)).

[33] Our approximation comes by assuming two new firms are created per 100,000 county residents × 0.34 average population per county (in 100,000s) × 308 counties × 24 months × an average survival rate of 0.9 (which seems about right given that we estimate a 24 month survival rate of 0.8). As for our jobs estimate, we just take seven new jobs per 100,000 × 0.34 × 308 × 24. Our causal estimates are from column (1) of Table 3.

[34] Estimates by Kaplan *et al.* (2011) are substantially smaller than those of Bruhn (2011). For a comparison of data and methods between the two studies, see Kaplan *et al.* (2011), especially subsection 5.2.

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We view our work as being generally supportive of the consensus view in the recent literature – that firm entry regulation can serve to deter entrepreneurship. In the case we study, though, even a fairly dramatic reform seems to have had a modest impact on the economy because of the marginal nature of the new entrepreneurship generated.

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Additional Supporting Information may be found in the online version of this article:

Appendix A. List of Non-eligible Industries.
Appendix B. Data and Construction of Variables.
Appendix C. One-stop Shop Opening Dates.

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