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# The Impact of Business Environment Reforms on New Firm Registration

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## Abstract

This paper uses panel data on the number of new firm registrations in 91 countries to study how the ease of registering a business and the magnitude of registration reforms affect new firm registrations. The authors find that the costs, days and procedures required to start a business are important predictors of the number of new firm registrations. However, they find that small reforms, in general less than a 40 percent reduction in

procedures or 50 to 60 percent reduction in costs and days, do not have a significant effect on new registrations. They also find important synergies in multiple reforms of two or more business environment indicators. Finally, they show that countries with relatively weaker business environments prior to reforms require relatively larger reforms in order to impact the number of newly registered firms.

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This paper—a product of the Finance and Private Sector Development Team, Development Research Group—is part of a larger effort in the department to study entrepreneurship and growth. Policy Research Working Papers are also posted on the Web at <http://econ.worldbank.org>. The author may be contacted at [ilove@worldbank.org](mailto:ilove@worldbank.org).

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# **The Impact of Business Environment Reforms on New Firm Registration**

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## 1. Introduction

Entrepreneurship is essential for the continued dynamism of the modern market economy and a higher entry rate of new businesses can foster competition and innovation (Klapper, et al., 2006; Ciccone and Papaioannou, 2007; Aghion, et al., 2009). To promote private sector growth, many countries have focused on simplifying the registration process, seeking to reduce the costs, days and/or procedures required to formally register a business.

A methodology for measuring the effectiveness of the regulatory framework for firm registration was developed by Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2002). Since 2003, the World Bank's annual Doing Business report has used this methodology to quantify the registration process in over 170 countries in its "Ease of Starting a Business" section.<sup>1</sup> An outstanding question, however, is whether, and to what degree, there exists an economically meaningful relationship between the costs, days and procedures to start a business and the actual number of new firms that register each year. Given the widespread recognition of the importance of business environment reforms<sup>2</sup>, it is surprising that there is little research, and no cross-country panel analyses, on the real effect of these regulatory reforms. Our paper aims to address this gap in the literature by examining the impact of institutional reforms on the number of new firms registered.

Understanding the regulatory environment that promotes entrepreneurship is necessary to successfully identify appropriate policies to foster entrepreneurship in local economies. In this paper we empirically investigate the relationship between the regulatory ease of registering a business and actual new business registrations. Next, we explore the magnitude of reform in

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<sup>1</sup> Reports are available on-line at: [www.doingbusiness.org](http://www.doingbusiness.org).

<sup>2</sup> For instance, a 2008 report of the Multilateral Donor Committee for Enterprise Development states, "Reforming the business environment is a priority for development agencies and governments because of the significant influence the business environment has on the development of the private sector and therefore on economic growth and the generation of livelihoods and jobs." (DCED, 2002, p.3)

entry regulation required for a significant impact on new firm registration. A priori, it is not clear what magnitude of reduction in costs (or other parameters such as days or procedures) is necessary to cause a significant impact on firm registration. In other words, what exactly constitutes a reform? Is a 20 percent reduction in the costs of registration sufficient or is a 50 percent reduction necessary to encourage a significant number of firms to register? We further examine the effect of simultaneous and sequential reforms across measures of entry regulation. Finally, we examine how the impact of reform depends on the level of regulation prior to the reform.

We use a new dataset that is uniquely suited for this purpose: a cross-country, time-series panel dataset on the number of newly registered limited liability firms. We supplement this dataset with information from Doing Business reports on the cost, days and number of procedures required for the registration of new companies. Importantly, both datasets focus comparably on only limited liability firms.

Our results show that the costs, days and procedures required for business registration are important predictors of new firm registration over time. However, on average, small reforms - in general less than 40 percent for procedures and 50 to 60 percent for costs and days - do not have a significant effect on new firm registration. In addition, reforms in multiple indicators (e.g. the cost and number of procedures required to register a business) have a larger impact on business registration and simultaneous reforms have a larger impact than sequential reforms. Furthermore, we find that a country's initial conditions matter: Countries that start out with relatively higher initial costs need larger reforms to result in a significant increase in number of new registered firms.

We offer a simple model to motivate our empirical strategy. The model demonstrates how some reforms could be classified as small or large depending on the relative magnitude of the costs and benefits of registration. Our results imply that in countries with high initial registration costs, the benefits of registration are significantly below the costs of registration, likely because of limited access to finance or rigid labor markets. This is consistent with Djankov et al. (2002) who show that high registration costs do not serve public interest, but only benefit politicians and bureaucrats.<sup>3</sup>

The results in our paper suggest non-trivial economic magnitudes on the number of newly registered businesses. For example, we find that across OECD countries in our sample, on average, a reduction of 50 percent or more in the number of procedures, days or costs leads to an average increase in the number of new registrations of 14, 19 and 30 percent, respectively. These results are remarkably consistent with previous studies of registration reforms in two different OECD countries. A reform in Mexico that decreased the number of procedures by about 60 percent resulted in a 5 percent increase in the total number of firms, which translates to a 24 percent increase in new registrations in the municipality of Guadalajara (Bruhn, 2008). A reform in Portugal that reduced costs and days by 50 percent or more resulted in an increase in firm startups of about 17 percent, mostly among “marginal” firms that would have been most deterred by burdensome regulations, such as small firms in low-tech sectors (Branstetter, et al., 2010).

Our analysis is motivated by earlier studies that find that new firms are the ones most likely to grow (Lingelbach, et al., 2005; Johnson, et al., 2000) and to create new jobs (Audretsch, et al., 2006; McMillan and Woodruff. 2002). For example, studies using longitudinal data sets on

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<sup>3</sup> Our results are also consistent with de Mel, et al. (2012) who find that firms in Sri Lanka rationally refrain from formalizing, since they see few benefits from doing so – while a few firms seem to be suboptimally informal. Furthermore, their results suggest that a relatively modest increase in the net benefits to firms of formalizing could dramatically increase the rate of formalization.

the evolution of firm formation document that economic growth in both Canada and the U.S. is driven by new formal business entry rather than by the growth of existing firms (Brander, et al., 1998; Haltiwanger, 2009).

Earlier studies also show that entrepreneurship can foster competition and economic growth (Barseghyan, 2008; Klapper, et al., 2006; Djankov, et al., 2006; Black and Strahan, 2002; Hause and Du Rietz, 1984) and a reduction in informality (Antunes and Cavalcanti, 2009; Dabian-Norris, et al., 2008). Previous cross-country studies have found that new firm creation is significantly related to country-level indicators of economic development and growth, the quality of the legal and regulatory environment, ease of access to finance, and prevalence of informality (Wennekers, et al., 2005; Klapper, et al., 2010; Ardagna and Lusardi, 2010).

Our paper is most closely related to the literature showing that costly entry regulations may impede the setting up of businesses and stand in the way of economic growth (De Soto, 1990; Djankov et al., 2002, Klapper, et al., 2006; Kaplan, et al., 2006; Bruhn, 2011). For example, a study of entry regulations across 34 European countries shows that onerous entry regulations are related to lower firm entry, specifically in industries with higher average entry rates (Klapper, et al., 2006).

Some studies, however, have cast doubt on the relationship between Doing Business measures and the real world business environment as perceived by actual business owners (Hallward-Driemeier and Pritchett, 2010). This paper helps to reconcile this recent skepticism with an analysis of the relevance of Doing Business measures using a real-world output: new firm registrations.

Importantly, this paper offers policymakers empirically-based insight into the impact of the reform process on new firm creation. For instance, insufficiently large reforms may not have

the intended impact on firm registrations, resulting in a potential misallocation of money and political capital. These results can help policymakers to design interventions with the biggest impact on private sector growth.

The paper proceeds as follows. Section 2 presents our model and empirical methodology. Section 3 discusses our data and summary statistics, Section 4 shows our results and Section 5 concludes.

## **2. Methodology**

### ***2.1. Theoretical intuition***

We begin with a simple model of reforms to formal business registration that guides our empirical strategy. Assume there are benefits for an entrepreneur to operate a formally registered firm (as compared to not starting a business or operating informally). This benefit might be increased access to finance, a sales tax ID to attract larger or foreign customers, better contract terms with suppliers or customers or a reduced risk of government sanctions. Suppose the total sum of these benefits can be represented by monetary amount  $b$ .

On the other side, there are costs to registering a formal business, such as official and non-official payments to start and operate the business, personnel and managerial time spent dealing with required procedures and minimum capital requirements that need to be met. Suppose the total cost is represented by  $c$ , which also includes the monetary value of employee time and the time value of any delays. Clearly, individuals will only chose to register their firms when the total benefits of registration exceed total costs, i.e.  $c < b$ .



Since benefits can vary from firm to firm, it follows then, that there should be a negative relationship between the costs of registering a business and the number of new businesses registered. This negative relationship is our first testable hypotheses.

Suppose there is a reform that reduces the costs of registering a business. This could be a reduction in required direct fees, or a reduction in the number of procedures, that can be translated to a reduction in costs because of personnel time savings. Assume that before the reform the registration costs are equal to  $c_0$  and after the reform the total costs are  $c_1$ , which is lower than  $c_0$  (i.e.,  $c_1 < c_0$ ). The reform will only lead to new formal sector registrations when post-reform costs fall below the benefits to registration, i.e.  $c_1 < b$ .

Put another way, the reform might be effective in inducing new business registrations or ineffective, depending on the relative magnitude of parameters  $b$ ,  $c_0$  and  $c_1$ . Specifically, there are two possible cases, which we refer to as a “small reform” or a “large reform”:

Small reform:  $b < c_1 < c_0$

Large reform:  $c_1 < b < c_0$

In other words, if the reform is “small” then the reduction in costs is insufficient to fall below the benefits, and therefore we should observe no significant increase in new firm registrations. In the case of a “large” reform, the costs fall below the benefits and we should observe a significant increase in new registrations. An important assumption in our model is that only the registration costs change, while other costs or the benefits remain unchanged. For example, we assume that the government will not simultaneously raise taxes on formally registered businesses as it reduces the costs of registrations.

Clearly, different firms will receive different benefits from formal registration. In other words, instead of a single benefit parameter there is a distribution of firms with various benefits.

In this case  $b$  can denote the average benefit. If the distribution of benefits is non-uniform - i.e. if there is some mass around the average benefit, such as in a normal distribution, the relationship between costs reduction and new registration will be non-linear. In this case the reform will be large if the costs fall below the average benefit. Thus, there would be some threshold before which reforms have little or no impact, and after which reforms have a significant impact on new firm registrations.

So far we have shown that the impact of a reform will depend on both the benefits of registration and the relative reduction in costs. In addition, the impact of a reform is likely to depend on the initial conditions in a country. Suppose there are two groups of countries – one with high initial costs and the other one with low initial costs. If in a country with high registration costs the benefits are also very high, then even a small change in costs may induce a significant number of new registrations. In this case, the benefit is just below the pre-reform costs. This case is illustrated in Figure 1, Panel A. Here an identical level drop in costs in both groups of countries results in similar increases in new registrations. In terms of percent changes, countries with higher initial costs will need smaller percent changes to induce entrepreneurs to register than countries with lower initial costs. In other words, in such an environment even a small change in costs will be welcomed by severely constrained local firms.

Alternatively, higher registration costs might be symptomatic of overall weaker business environments and fewer benefits to formal firms. For instance, in countries with less developed financial systems - which often have high registration costs - the benefit of formal sector financing might be less accessible. In this case, the average benefits are significantly below the costs. This hypothesis is represented in Figure 1, Panel B. In this case, the benefits in both groups of countries are pictured at about the same level, while the initial costs are significantly

higher in the high-costs group. Therefore a much larger level drop in costs is needed for costs to fall below benefit in a high initial cost environment. Note that if the change is measured as a percent change, the drop may be of similar magnitude (i.e. a 50 percent change in the high-cost environment translates into a much larger level change than it is in a low-cost environment). Thus, measuring reforms as a percent change will serve as a “normalizing” function with respect to the initial business environment levels. Our paper offers a way to empirically test these two alternative hypotheses.

To summarize, our model demonstrates that when registration costs are significantly reduced, the cost of registrations for some firms will move below the expected benefits of formal sector registration, and more entrepreneurs will chose to register under the new reformed regime. The empirical tests that follow will identify the magnitude of reforms necessary for a significant increase in the number of newly registered firms.

## **2.2. *Empirical Strategy***

First, we test the predicted negative relationship between regulatory barriers in starting a business and registrations. To do that we run a simple model of new firm registrations on our regulatory indicators, while capturing any country-specific unobservable factors with country fixed effects.

Second, we investigate what magnitude of reforms produce the most significant changes in newly registered businesses. Specifically, we measure reforms (the move from  $c0$  to  $c1$ ) as the percent change over time in the Doing Business “Starting a Business” parameters. We construct various indicators of “reform” along the percent change continuum, using discrete cutoffs; for instance, countries that reduced costs by 20 percent or more (“Reform\_20%”), countries that

reduced costs by 30 percent or more (“Reform\_30%”) and so on. For each of the cutoffs, we classify countries as treated (reformers, defined as change below the cutoff) or controls (non-reformers, change above the cutoff).

Our empirical strategy here is to use a difference-in-difference estimation to test the effect of one indicator of reform (e.g. “Reform\_20%”) on new firm registration. We then compare results for different cutoff points: If the cutoff point is correctly classified (i.e. it is close to the “true” magnitude of reform seen in the data), the difference-in-difference estimation will produce the most significant and largest in magnitude estimates of the impact. However, we should observe a lower impact of misclassified reforms.<sup>4</sup> In other words, this methodology allows us to identify which cutoff points lead to the largest and most significant increases in formally registered businesses.

### **3. Data and Descriptive Statistics**

#### **3.1 Data on New Firm Registrations**

For our dependent variable, we use data on the number of new firms registered in the calendar year. The data is collected directly from the local Registrar of Companies, which is the entry point for entrepreneurs joining or transitioning to the formal sector in most countries, or other statistical offices, and are not based on surveys or estimations.<sup>5</sup> The data includes only new corporations, which are defined here as *private companies with limited liability*. Notably, this is

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<sup>4</sup> Thus, if a true reform, i.e. a change that makes cost fall below benefits is a reduction in 20% of costs, but we classify a country as a reformer only if a reduction of 40% or more is observed, several countries with actual reforms, i.e. those in the range of 20-40%, will be classified as non-reforms and hence the difference between reformers and non-reformers will be muted. This is an example of Type I error, when true reformers are misclassified as non-reformers. On the other hand, if a true reform is 40%, but we set our reform dummy for 20%, a number of countries in the range of 20-40% will be classified as reformers, when in reality they are not. This is an example of Type II error. In either case, we should observe the most significant results the closer our definition of the reform dummy is to the magnitude of the actual reform.

<sup>5</sup> For additional information on the methodology, see Klapper and Love, 2010.

the same definition used by the World Bank's Doing Business report. It is also the most prevalent formal business form in most countries around the world (Doing Business, 2010).<sup>6</sup>

The main variable of interest is new business "entry density", calculated as the ratio of newly registered limited liability firms per 1,000 working age population (those ages 15-64).<sup>7</sup> The final dataset includes an unbalanced panel of 494 observations from 91 countries over the six-year period 2004 to 2009. Figure 2, left panel, shows the distribution of entry density across countries; it has a minimum value of 0.0021 (in Niger) and a maximum value of about 10.<sup>8</sup>

Entry density varies significantly by income groups: it ranges from 4.21 in high-income countries to less than one in lower-middle and lower income countries (Figure 2, right panel). In other words, there are on average about four limited-liability firms registered annually per 1,000 working age individuals in industrialized countries, as compared to about one firm per 1,000 individuals in developing countries.<sup>9</sup>

### **3.2 Business Environment Indicators**

Our indicators of business environment regulations come from the *Starting a Business* section of the World Bank's *Doing Business* database, which provides country-level indicators to measure the efficiency of the business registration process. The first indicator, Starting Costs, captures all official fees and additional fees for legal and professional services involved in incorporating a business, and is measured as a percentage of the economy's income per capita.

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<sup>6</sup> We exclude countries identified by the IMF (Zorome, 2011) as off-shore financial centers, since information provided by these countries likely reflects a nontrivial number of shell companies, defined as companies that are registered for tax purposes, but are not active businesses. For example, in 2007 the British Virgin Islands had 77,022 newly registered corporations, which translates to approximately four new firms for each working age individual.

<sup>7</sup> The complete database and list of sources is available at: <http://econ.worldbank.org/research/entrepreneurship>.

<sup>8</sup> We restricted entry density to be less than 10. Effectively this excludes two countries that are outliers on the number of new registrations (Iceland and New Zealand). In addition, we exclude the top and bottom 1 percent of the distribution of new density in all regressions.

<sup>9</sup> Summary Statistics, by country, are shown in Appendix 1.

The second indicator is the Number of Procedures necessary to incorporate a business. Third, Starting Days, measures the time required to start a business, which is defined as the number of days that incorporation lawyers indicate is necessary to complete all required procedures with minimum follow-up with government agencies and no extra payments. Fourth, paid-in Minimum Capital Requirement captures the amount that an entrepreneur needs to deposit in a bank or with a notary before or shortly after registration and is recorded as a percentage of income per capita. Figure 3 shows the relationship between the explanatory variables and entry density. Figure 4 shows the distributions of the annual percent change in these four explanatory variables.

Complete variable definitions and summary statistics are shown in Tables 1 and 2. For all four indicators of Starting a Business, the mean of the annual percent change is negative, suggesting that, over time, most countries have been successful in lowering registration costs, reducing days, procedures and capital requirements.

### **3.3 *Classifying Reforms***

Table 3 shows the number of reformers we obtain with various reform classifications described above. For example, for a 20 percent cutoff for cost reform we count in our dataset 56 reformers. Clearly, as we increase the cutoff, we ask for a more significant change and the number of reformers declines to 41 with at least a 30 percent drop in cost, 28 with a 40 percent cutoff, 16 with a 50 percent cutoff, and only 7 with a 60 percent cutoff. We do not explore larger (i.e. more negative) cutoffs because they produce insufficient numbers of reformers. We observe similar trends across the different business environment variables; increasing the cutoff result in a smaller number of reforming countries. For the purpose of our regression analysis, we look at

only the impact of the first reform for each of our definitions, i.e. we consider a country a ‘reformer’ for all subsequent years following the first reform.<sup>10</sup>

Often a country that is reforming the business registration process will make changes in the process along several dimensions. For example, as the number of procedures goes down, the length of the process will also go down, and often the costs will be reduced as well. It is possible that when several changes occur simultaneously, smaller changes along each dimension will produce significant results, while a single parameter change will not. Using the intuition of our model, the aggregate cost reduction as a result of two reforms may fall below the benefit and the joint reform will be identified as “large,” while each individual reform is independently too “small.” We investigate this hypothesis by defining simultaneous reforms. In this case a country is classified as a reformer if at least two reforms occur simultaneously. We also look at cases in which three reforms occur simultaneously, although these are very rare.

In addition, reforms may occur in subsequent years. For example, the costs may be reduced in one year and in the next year the number of procedures or days will be reduced. Similarly, a single change by itself may be insufficient to induce significant number of new registrations, but when the second change occurs, there will be cumulative effect that will make a difference. To test this assumption we define sequential reforms. In other words, for each cutoff we only classify a country as a reformer when two or more reforms with a given cutoff occur sequentially. For example, at a 20 percent cutoff, a country is not classified as a reformer when the first reform occurs. However, when a second 20 percent reform occurs, the country is classified as a reformer under our “two sequential reforms\_20%” definition. Similarly, for three

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<sup>10</sup> Note that since our data is a panel, a country can be identified as a reformer in more than one year - but the number of countries with repeated reforms is rather small. For example, for a 50 percent cutoff we only have one country with a repeated reform for costs, one country for procedures, six for days and nine for minimum capital.

sequential reforms a country is only classified as a reformer only after three reforms occur within our sample frame.

Table 4 presents the number of countries classified as reformers according to each of these different definitions. Column one shows the number of “reformers” classified for each cutoff if any one of the four business environment measures previously discussed fell below the cutoff in any given year. We get 78 “reformers” by this definition at a 20 percent cutoff and 45 at a 50 percent cutoff.

In Column 2 we classify a country as a reformer if two or more reforms occur sequentially in a country. These two reforms could be any of the four measures discussed above, i.e., it could be reform of days and procedures, or reform of cost and days, or any other combination. We get fewer countries classified as reformers in the case of two reforms: 66 with cutoff of 20 percent and only 24 with cutoff of 50 percent. Clearly, this is stricter definition than the one used in column 1 and fewer countries fit this new criterion. In column 3 we tighten the definition even further and require that two or more reforms occur simultaneously, in the same year. In other words, column 3 is a subset of reformers classified in column 2. We get 50 countries at a 20 percent cutoff and only 14 countries at a 50 percent cutoff.

Column 4 shows the number of reformers when we change our definition to require that at least three reforms occur in a country sequentially, i.e. the first reform in year one, the second in year two and the third reform in year three (or the first reform in year one and the second and third reform simultaneously in a future year). We only classify a country as a reformer when we observe the third change happening. This definition is stricter than the one used in column two, but is not necessarily stricter than the one used in column 3. We get about the same number of reformers in columns 3 and 4.



In the last column we present the number of countries with three or more reforms occurring simultaneously. The numbers are significantly smaller. At a 20 percent cutoff only 26 countries are classified as reformers (relative to 50 countries with two simultaneous reforms). The numbers quickly drop as the cutoffs increase; only 14 countries are identified as reformers using a 30 percent cutoff and 8 or less using a 40 percent cutoff. These numbers suggest that despite the fact that changes are correlated (i.e. a change in procedures is more likely to be accompanied by a change in days and a change in costs), it is relatively rare to observe three of these changes occurring simultaneously.

## 4. Results

### 4.1 *Continuous Regression Model*

Based on our model, we first perform a simple, but important empirical test to investigate whether, on average, a country's registration costs are negatively related to the number of newly registered businesses. To do this we investigate within country variation, exploiting the unique panel nature of our data. In other words, we estimate the following model:

$$\text{Entry Density}_{it} = \alpha_i + \beta_1 \text{ESB}_{it} + \beta_2 \text{GDPgrowth}_{it-1} + \beta_3 X_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

Here, ESB stands for a given individual measure of "Ease of Starting a Business", in this case the costs, days, procedures and minimum capital required. We allow each country to have its own country-specific error term,  $\alpha_i$ , (i.e. country fixed effect) to capture any unobserved differences between countries that would affect the level of registration and registration costs. In addition, we control for lagged GDP growth which captures business cycle effects, which may affect both the ease of starting business regulations and new registrations (although our results

are not sensitive to this control). We also include time dummies to control for any global changes in the macroeconomic environment that may affect registrations in all countries. This is important because our data covers the period of the global financial crisis. For example, Klapper and Love (2010) show that number of new registrations dropped significantly during 2009 in most countries. The time dummies will capture the average drop in registration in the year of the crisis, relative to previous years, and eliminate the confounding impact of the crisis.<sup>11</sup> In addition, the time dummies will also capture global changes in registration trends. Thus, Klapper and Love (2010) show that number of registered businesses is increasing overtime; in other words there is a trend toward more formalization. If firm registrations are increasing in all countries because of this trend, we may erroneously attribute the impact to reforms (which capture the later years of the sample) to the increasing registration trend.

An important consideration is to isolate the impact of business registration reforms from other reforms that may be happening at the same time. While reforms to business registration should have a first order, direct impact on the number of registered businesses, other reforms, such as improved access to finance or better contract enforcement, may also affect the number of newly registered businesses. Therefore, in our model we control for a vector X, which represents other features of the business environment. Specifically, we add indices measuring Doing Business indicators for Registering Property, Getting Credit, Enforcing Contracts and Closing a Business (described in Table 1).<sup>12</sup> We estimate this model with errors clustered on the country level to capture any serial correlation of errors within country.<sup>13</sup>

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<sup>11</sup> In addition, we test robustness of our results to eliminating the year 2009 from our regressions all together. This significantly reduces our sample (by about 1/6) and some of the results become statistically weaker, but the general patterns remain the same.

<sup>12</sup> In separate regressions (available upon request) we also control for taxation by including the Doing Business index of Tax regime; it is important to control for changes in tax policy given that firms view taxes (including tax-compliance costs) as the major obstacle to registering (La Porta and Shleifer, 2008). However, our sample is

Results are presented in Table 5. Columns 1-4 present our results without controls, and columns 5-8 present results including controls for other business environment indicators. The three main variables - costs, days and procedures - are all significant predictors of entry density in both specifications. Minimum capital requirement has a predicted negative sign, but is not significant at conventional levels. Including indices for other features of the Doing Business has no material impact on our results, and the indices themselves are not significant in any of the specifications. These results suggest that unlike business registration regulations, other characteristics of the business environment do not have a first order impact on business registration. Of course our results do not suggest that these reforms are not important – they are likely important for other outcomes besides firm registration.

To summarize, our results so far show that there is a negative and significant relationship between the costs of registering a business and the number of new firm registrations.

#### **4.2 Reform Regression Model**

In this section we investigate the impact of reforms to the registration process on business registrations. Specifically, we test which magnitude of reforms produces the largest change in business registrations. We run the following regressions:

$$\text{Entry Density}_{it} = \alpha_i + \beta_1 \text{Reform}_{it} + \beta_2 \text{GDPgrowth}_{it-1} + \beta_3 \mathbf{X}_{it} + \delta_t + \varepsilon_{it} \quad (2)$$

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reduced by one year when the tax index is included (from 491 to 418 observations). The results for procedures and days remain significant at the same levels as reported, while the result for costs drops to about 15% significance, likely as a result of the smaller sample.

<sup>13</sup> Our results are robust to using one year lags of business environment variables to allow for delayed effects of costs on new firm registrations (available upon request).

Here, “Reform” is equal to one for all countries classified as reformers using each of the cutoffs discussed above. This dummy is equal to one for the year in which reform occurred and all years after the reform. In essence this is a difference-in-difference approach, in which “treated” countries are the countries that have been classified as reformers versus controls (non-reformers), and the time is defined as before and after the reform.

The main difference with the continuous framework discussed in the last section is that each reform is treated as a discrete event, which is allowed to have a lasting impact during the years subsequent to the reform. In contrast, in the previous section we only considered the contemporaneous relationship between the level of business registration and the level of new firm registrations.

Again, we estimate this model including lagged GDP growth, which ensures that changes in new registrations are due to the specific reforms rather than business cycle effects, country and year fixed effects and errors clustered on the country level. In addition, to capture the impact of other features of the business environment, we add the same vector of control variables as in the previous model: Registering Property, Getting Credit, Enforcing Contracts and Closing a Business.<sup>14</sup>

### **4.3 *Single Reform Results***

Table 6 contains our main regression results. Each cell in the table reports the coefficient of a separate regression; to save space, we only report the coefficient for the reform variable (i.e. coefficient  $\beta_1$  from model 2). We observe that for a reform to costs or days to be effective, they need to be reduced by at least 50 percent relative to the pre-reform level. The largest impact for

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<sup>14</sup> In separate results, not reported, we add taxes as an additional control; although this reduces our sample by one-year, our results remain virtually unchanged.

costs and number of days is observed for a 60 percent reduction, but in this case only 7 countries are classified as reformers in the case of costs and 23 for number of days (Table 3).

However, even a small change in the number of procedures, such as a 20 percent reduction, produces significant results. The magnitude and significance of the procedures reform coefficients peak at 40 percent. However, with higher cutoffs, such as a 50 or 60 percent, we misclassify actual reforms of lower magnitude, and hence the size of the coefficient drops (see the discussion of Type I errors in footnote 3). This does not imply that larger reforms are ineffective. It simply suggests that in the case of procedures even smaller reforms are significant, with an optimal reduction of about 40 to 50 percent. For minimum capital requirements we find that a 20 percent reform, as well as 40-60 percent reforms, yield significant coefficients.<sup>15</sup>

To gauge the economic magnitude of these results in a way that is comparable with previous literature we convert the increase in new density (our main measure, which is the ratio of new firms to total population) to the percent increase in new registrations.<sup>16</sup> The increase in new registrations varies from country to country, depending on actual pre-reform number of new registrations and the local population. We focus on OECD averages for comparability with previous studies. We find that on average across OECD countries in our sample, a reduction of 50 percent or more in the number of procedures leads to an average increase of 14 percent in the number of new registrations, while similar reductions in the number of days and costs lead to an average 19 percent and 30 percent increase in new registrations of limited liability firms,

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<sup>15</sup> The results on minimum capital are somewhat weaker throughout the paper, perhaps because many countries do not impose minimum capital requirements or because these requirements are not binding. In addition, the changes to minimum capital requirements are often discrete in which they are completely eliminated, rather than reduced. This is visible on the distributions of changes to minimum capital, which has significant density in the left tail of the distribution.

<sup>16</sup> We calculate the increase in new registrations by taking actual new density in a country, add the reform coefficient, which shows an increase in new density as a result of a reform, for example at 50 percent reform we obtain 0.23 for procedures or 0.35 for days and 0.57 for costs (Table 6). Then we multiply these new densities by population to obtain new registrations after the reform and calculate a percent change in new registrations.

respectively.<sup>17</sup> These results are remarkably consistent with previous studies of registration reforms in two different OECD countries, as discussed in the introduction (see Branstetter, et al., 2010, Bruhn, 2008, Bruhn 2010).<sup>18</sup>

We perform several robustness tests to ensure that our main results are not driven by omitted variables. First, previous research noted that the rate of new firm formation is related to industry structure (e.g. Reynolds, et al., 1994 and Baptista, 2008). For example, an economy consisting of mainly self-employed microenterprises has a different structure than one where large-scale manufacturing enterprises account for a large share of employment. Clearly, our sample includes countries with very different industrial composition. However, all level differences between countries are already accounted for in our model with country-specific fixed effects. Nevertheless, we also verify that time-varying differences in industry composition are not affecting our results. In separate regressions (available upon request) we control for industry composition by including annual data on the value added of the manufacturing sector (as a percentage of GDP) and the service sector (as a percentage of GDP) (World Development Indicators, 2012). The results remain robust to these additional controls.<sup>19</sup>

Second, differences in the level of economic development have also been found to affect the rate of new firm entry (eg. Wennekers, 2005). To test whether the level of economic development is driving our results we add lagged GDP per capita to our model. We find our

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<sup>17</sup> As an additional robustness check, we also evaluate the impact of “negative” reforms, which refers to instances in which starting a business measures *increase*. Though the sample size for these reforms is very small (generally less than 10, even at 20 percent), we do observe a significant relationship between increases in procedures and decreases in entry density (results available upon request).

<sup>18</sup> Kaplan, et al. (2006) also study the impact of the same Mexican reform and find a 4 percent increase in the number of newly registered firms. However, Bruhn (2011) argues that they underestimate the true impact because the authors use data that does not include firms without employees or firms that fail to formally register their employees for Social Security benefits.

<sup>19</sup> Interestingly, the share of services is mostly significantly negative; in other words, in countries with a greater concentration of service firms (which generally have lower leverage rates, relative to manufacturing firms), we find a lower registration rate of limited-liability companies.

results to be robust (available on request). Finally, since previous literature has pointed out a non-linear U-shaped relationship between the level of economic development and new firm entry (eg. Wennekers, 2005), we also add the squared term of GDP per capita in addition to the linear term. Again, our results remain robust.<sup>20</sup>

#### **4.4 Testing for Parallel Trends**

Before proceeding with our additional tests, in this section we discuss the key identification assumption that is required to credibly attribute our results to the observed reforms rather than other unobserved changes. First, we note that our model already controls for any level differences between countries that we classify as reformers and non-reformers because we include country fixed effects. In addition, our methodology also controls for any impacts that could simultaneously affect reforming and non-reforming countries with time dummies. The main assumption of the difference-in-difference methodology is that the reformer countries (i.e. the treated group) would have had the same trend as non-reformer countries (i.e. the control group) in the absence of reform. Clearly, this assumption is not testable as we don't observe the counterfactual (i.e. the treated group in the absence of the treatment). However, one way to validate this assumption is to compare the pre-reform trends in two groups. We do this using the following model:

$$\text{Entry Density}_{it} = \alpha_i + \beta_1 \text{Trend}_t + \beta_2 \text{Trend}_t * \text{Reform}_i + \delta_t + \varepsilon_{it} \quad (3)$$

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<sup>20</sup> We thank an anonymous referee for bringing these points to our attention.

Here, “Reform” is equal to one for all countries classified as reformers using each of the cutoffs discussed above. Unlike model (2), here the Reform dummy is not time-varying. In other words, it equals to one for any country that has had a reform during our sample period. The Trend is a linear trend variable. We estimate this model using all available observations for non-reforming countries, while for the reformer countries we only use observations prior to the first reform of 20% or more (measured separately for each of the relevant variables, i.e. costs, procedures, time and minimum capital). As before, we include country fixed effects, but we no longer include time dummies since our focus is on the Trend.

The variable of interest is the interaction of the Trend and the Reform dummy. If this interaction is not significantly different from zero this would suggest that we cannot reject the hypothesis of equal (i.e. parallel) trends in two groups of countries prior to reforms. While not a direct test of the identification assumption, which is not testable as noted above, such results would offer some degree of reassurance.

Table 7 presents our results. Each box presents results for a separate regression for each of the cutoff points. The number of observation is the same in each column since we use all observation prior to observing the first reform of 20% or more.

We see that for all “large” reforms, e.g. 40%, 50% and 60%, the interactions of Trend and Reform are not significantly different from zero. In other words, for larger reforms (40% or more) we cannot reject the hypothesis of parallel trends. However, for smaller reforms, such as 20% and 30% for all variables except days we observe significant differences in trends. The interaction is positive, suggesting that for small reforms there is a visible increase in new registrations before the first small reform. Thus, small reforms follow increases in new registration, rather than precede it. It is plausible that the governments institute smaller reforms



in response to observed increase in registrations as a way of supporting whatever positive changes are already happening. Since instituting a small reform does not require a large commitment or expense, they are easy to use to “piggy-back” on positive trends.

These results suggest that any positive increase in registrations after a 20% or 30% reform (which was only found significant for procedures) cannot be attributed solely to the reforms, and is likely to be the result of higher trends pre-reform. In other words, small reforms cannot be considered exogenous to the number of new registrations. Thus, the positive results that we observed for 20% and 30% changes for procedures should not be considered to suggest a casual impact of reforms. The good news is that for larger reforms (40% or more) we cannot reject the hypothesis of parallel trends and thus, the increase in new registrations in response to larger reforms can be attributed to the reforms, rather than pre-reform trends.

#### **4.5 *Simultaneous and Sequential Reform Results***

Next we investigate the impact of several reforms occurring simultaneously or sequentially. Table 8 presents our results. In the first column a country is defined as a reformer if any of the four business environment measures change by the percent specified in each cutoff. In essence it parallels results in Table 6, but now lumps all reforms together in one measure. We observe that if the reform is defined as a change in any of the four variables, the only significant results are obtained with a 50 percent or 60 percent cutoff. Thus, a country must change one of the four measures by at least 50 percent to result in a statistically significant increase in registrations.

Column 2 presents results for two reforms that can occur either in the same year or in subsequent years (i.e. sequentially). We observe that in this case even a 40 percent reduction in

the underlying measures is sufficient to produce a significant impact. Thus, if more than one reform is occurring in a country, the magnitude of each of the reforms can be smaller because of the synergistic effect.

We observe similar results for two reforms occurring simultaneously (column 3), although the magnitudes of the coefficients are slightly larger for simultaneous reforms. For example, for a 50 percent cutoff the coefficient on simultaneous reforms is 0.81, while the coefficient on sequential reforms is 0.60. Even larger differences are observed for 60 percent cutoff: 0.64 with two sequential reforms and 1.04 with two simultaneous reforms. This suggests that there is some advantage in two reforms occurring simultaneously rather than sequentially.

Next, we investigate the impact of three reforms occurring sequentially in column 4. We find that even a 30 percent reduction in three out of four measures results in a significant impact on number of registered businesses. This further confirms the synergistic impact of reforms as when three changes are occurring, each individual change can be smaller in magnitude than in the case of single or double reforms. Thus, in combination even smaller reforms produce a significant outcome.

The last column presents our results for three simultaneous reforms. The results are similar to three sequential reforms, but the coefficients again are larger for the comparable cutoffs. For example, at 30 percent cutoffs the three sequential reforms produce a change in 0.36 in entry density, while three simultaneous reforms produce a change of about 0.58.<sup>21</sup>

The coefficients in Table 7 also suggest that simultaneous and sequential reforms have an economically significant impact on new firm registration. For example, for our sample of OECD countries, two simultaneous reforms of at least 50 percent result in a 38 percent average or 22

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<sup>21</sup> The results for 50 percent and 60 percent changes for three simultaneous reforms are not reliable, since there are only 5 and 2 countries, respectively, classified as reformers using this definition.

percent median, which is consistent with Branstetter, et al. (2010) who study a reduction in both procedures and time.

An important consideration is that multiple reforms – either sequentially or simultaneously – might suggest that these reforms are part of a larger private sector reform package. However, our results in Table 7 include controls for the four measures of the Doing Business Environment discussed above, which mitigates this concern.

#### **4.6 Sensitivity to Pre-reform Business Environment Levels**

In this section we investigate whether the impact of reforms varies with the pre-reform level of the business registration environment. In other words, we address the question of how initial regulatory conditions affect the impact of reforms. As we discussed in section 2.1, our model predicts two alternative scenarios in regard to the impact of initial costs. The first scenario holds if in high initial costs environment the benefits are also relatively high. In this case even a small change will be welcomed by severely constrained entrepreneurs. The second scenario is that in high initial cost environments benefits are significantly below the costs and hence entrepreneurs need larger changes to motivate them to register. While we cannot observe the benefits directly, we can test these alternative hypotheses with the interaction of our reform measures with initial conditions. Specifically, we use the following model:

$$\begin{aligned} \text{Entry Density}_i = & \alpha_i + \beta_1 \text{Reform}_{it} + \beta_2 (\text{Reform}_{it} * \text{Pre-reform\_level}_i) + \beta_3 \text{GDPgrowth}_{it-1} \\ & + X_{it} + d_t + \varepsilon_{it} \end{aligned} \quad (4)$$

The pre-reform level is defined as the level of business environment just a year before the reform (again, we only consider first reforms). Thus, it is not time-varying (hence the subscript i)

and in the regression the level is subsumed in the fixed effects. In other words, this specification allows us to investigate whether the same magnitude reform has more or less significant impact with different pre-reform levels. We measure the pre-reform level of the business environment as a combination of the four indicators of starting a business (cost, days, procedures and minimum capital) and construct an average index of these indicators (standardized before averaging), which we refer to as “ESB Index”. This index is constructed such that higher values indicate a better business environment (i.e. lower costs, days, procedures and minimum capital). This variable is approximately normally distributed with mean about zero and standard deviation about one.

Table 9 presents the results for single variable regressions (i.e., matching those in Table 6). Once again each cell (of 4 rows each) is a single regression but now we report two coefficients – the reform coefficient and the interaction of reform with pre-reform levels. We observe that for almost all cutoff points and all four measures, the interactions with pre-reform levels are significantly positive, meaning that better pre-reform indicators of the regulatory environment for starting a business are associated with a larger magnitude of the reform. Thus, a country with weak pre-existing business environment levels needs a larger reform to achieve the same impact on the number of registered businesses than a country with strong business environment.<sup>22</sup>

Our results show that for small reforms to the number of days or costs of starting a business (i.e. reforms of 20 to 40 percent) the level effect of reforms is not significant, while the interaction is positive. We interpret this to mean that small reforms are only effective in countries with above average pre-reform business environments (i.e. with ESB index above zero). Larger

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<sup>22</sup> Interaction results for multiple reforms are not reported to save space, but they are similar to single reform results and most of the interactions are significantly positive.

reforms (of 50 percent decline in days or costs) are effective even in countries with below average business environment (because the coefficients and interaction are both significant), but their effectiveness is lower in countries with weaker starting position (i.e. the interaction is positive and significant for coefficients indicating at least an improvement of 50 percent).

For reductions in the number of procedures, we find, as before, that even small reforms of 20 or 30 percent are effective in most countries (except for the very worst initial conditions), but the effect is larger in countries with higher pre-reform ESB index (i.e. better initial conditions).

To summarize, these results suggest that countries that start out in a disadvantaged position (higher costs, more procedures and longer time) need larger reforms to achieve the same impact on the number of registered businesses as countries that have relatively better ex-ante business environments. In light of our model, these results suggest that in countries with high registration costs (or long delays or a large number of procedures), the benefits of registrations are significantly lower than the existing costs for a large number of entrepreneurs. These results are inconsistent with the “public interest” theory of regulation and the hypothesis that countries have high registration costs because there are high benefits of registrations, and instead support the “public choice” theory, which suggests that high registration costs exist to benefit bureaucrats and politicians (see Djankov et al., 2002).

#### ***4.7 Comparing the magnitudes-buckets approach.***

All reform results discussed so far are based on defining reforms as cutoff points. For example, a 20 percent reform dummy equals to one if the reform in a given year is at least 20 percent *or more*. In other words, the reforms in each regression start at a specified cutoff point and include all larger reforms. This definition is convenient for empirical analysis and

policymakers, who may need to know the size of the minimum effective reform. However, the drawback of such a definition is the difficulty in comparing statistical magnitudes between various cutoff points – i.e. statistically speaking, is the impact of a 40 percent or more reform significantly different from the impact of a 50 percent or more reform? In this section we address this question by redefining our reform dummies into buckets, rather than using cutoffs as we have done previously.

Specifically, we define multiple reforms buckets, such as reform of 1-19 percent, 20-29 percent, 30-39 percent, 40-49 percent and so on. The advantage of this approach is that we can put all reform dummies in a single regression and run a “horserace” between different buckets. However, the main disadvantage is the loss of degrees of freedom as now different buckets have fewer reforms on which to estimate the coefficients.<sup>23</sup>

Table 10, Panel A shows the number of reforms in each bucket. As expected, the number of reforms is relatively small, on average about 10 reforms per bucket (more for the case of the lowest bucket for 1-19 percent, which were not considered “reforms” in our previous definitions). Some buckets do not even have enough reforms to estimate separately. For example, for minimum capital 40-49 percent and 50-59 percent buckets have zero or one reform per bucket, respectively. We exclude these from the regression to avoid erroneous results based on only one reform.

Table 10, Panel B presents our regression results. Unlike earlier tables, in which each cell represents a single regression, now each column represents a single regression and all dummies enter simultaneously. We find that for procedures, the largest impact is observed in 40-49

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<sup>23</sup> Because our buckets are mutually exclusive, we now consider only the largest reform in each country, while before we have considered the first reform, which may or may not be the largest. Our previous results are robust to considering only the largest reforms.

percent bucket, while the higher bucket results are also significant (but smaller). For days, costs and minimum capital we only observe significant results in the 60 percent or more bucket.

The bottom of Table 10 shows F-tests for pair-wise comparisons of different coefficients. We see that for procedures the coefficient on the 40-49 percent bucket (which has the largest significant coefficient in the regression) is significantly different from the coefficient on the 1-19 percent bucket. Also, the coefficient for the 50-59 percent bucket is marginally different (at 15 percent) from those in the 1-19 percent bucket. These results square well with what we found in the earlier tables – that for procedures the reform of 40 percent or more has the largest and most significant coefficient.

For both days and costs, the coefficients for the 60 percent or more bucket are significantly different from buckets below 50 percent, but are not significantly different from the coefficients for the 50-59 percent bucket. This result closely matches our previous result, as for both costs and days the coefficients on the 50 percent or more and 60 percent or more reforms are significant (see Table 6). Finally, for the minimum capital regressions we find that the coefficients for a reform of 60 percent or more are significantly different from the 30-39 percent bucket and are marginally different from the 1-19 percent bucket.

In summary, these tests provide a robustness test to our previous results that suggest that larger reforms - 40 percent or more for procedures and 50 percent or more for costs, days and minimum capital- are significantly different from smaller reforms. While our results are robust to this alternative approach, our main approach is more consistent with our model, in which a reform is a discrete change that “kicks in” after costs drop below the benefits, which implies a cutoff point. In addition, the cutoff results are easier to interpret for policy makers who may need to know the minimum reduction (i.e. the cutoff) after which the intended results will occur.

## 5. Conclusion

In this paper we test the relationship between the Doing Business “Ease of Starting a Business” measures of the regulatory environment and annual new firm registrations. We offer an empirically based, quantitative approach to identifying business environment “reformers”.

Our results show that the ease of starting a business is a significant predictor of new business registrations but that small reforms, in general less than a 40 percent reduction for procedures, or less than 50 percent reduction for costs and days, do not have a significant effect on new firm registration. This suggests that ‘token’ reforms, perhaps motivated by political or multilateral pressures to reform, do not have the intended effect on private sector activity. Furthermore, we show that countries with relatively weaker business environments require relatively larger reforms in order to impact new firm registration. It is likely the case that countries with weaker business environments also have fewer benefits for formal sector registration (such as access to formal financial and labor markets) and therefore larger reductions in costs are necessary to incentivize firms to incur the costs of formal registration.

We also show that there is a synergistic impact of reforms. In the case of two sequential reforms, each of the reforms can be smaller than if they occurred independently. In addition, two reforms occurring simultaneously have more impact than if they occur over a longer period of time.

Our results highlight the importance of defining reforms in a way that is consistent with expected outcomes, i.e. in this case the number of newly registered businesses. The methodology developed in this paper can offer alternative classifications of reformers, which can be useful to policymakers, researchers and practitioners working to improve the business



environment and promote private sector development. Our results can also be helpful to motivate policymakers to make larger, and more significant, reforms.

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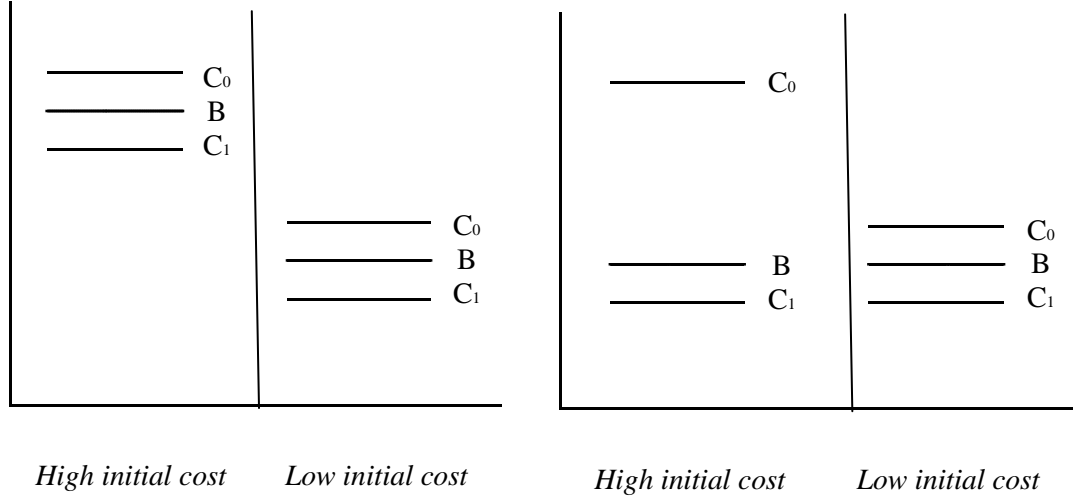
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**Figure 1: Graphical Representation of Relative Costs and Benefits**

*Panel A: High Benefits in High Initial Cost Countries.*

*Panel B: Low Benefits in High Initial Cost Countries*



**Figure 2: Distribution of Entry Density**

*Panel A: Distribution of Entry Density*

*Panel B: Entry Density across Income Groups*

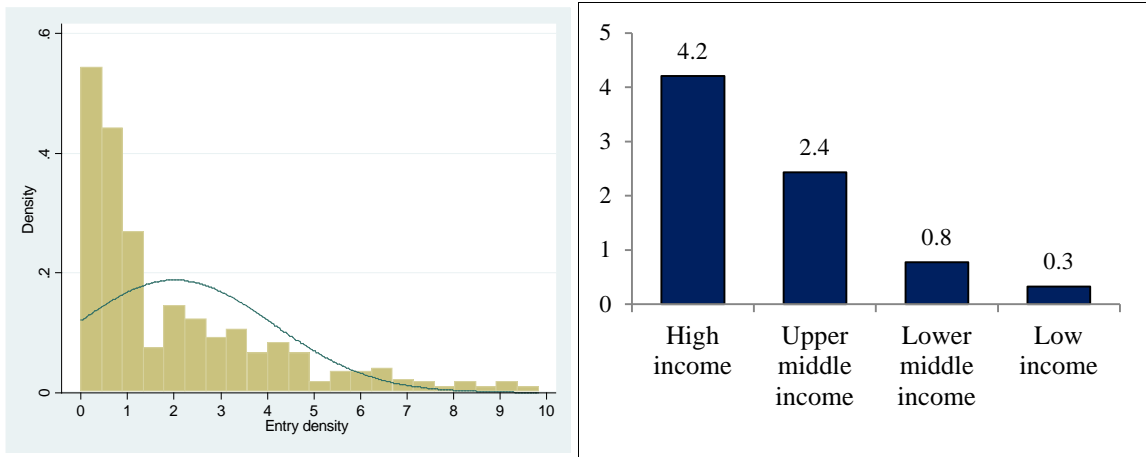
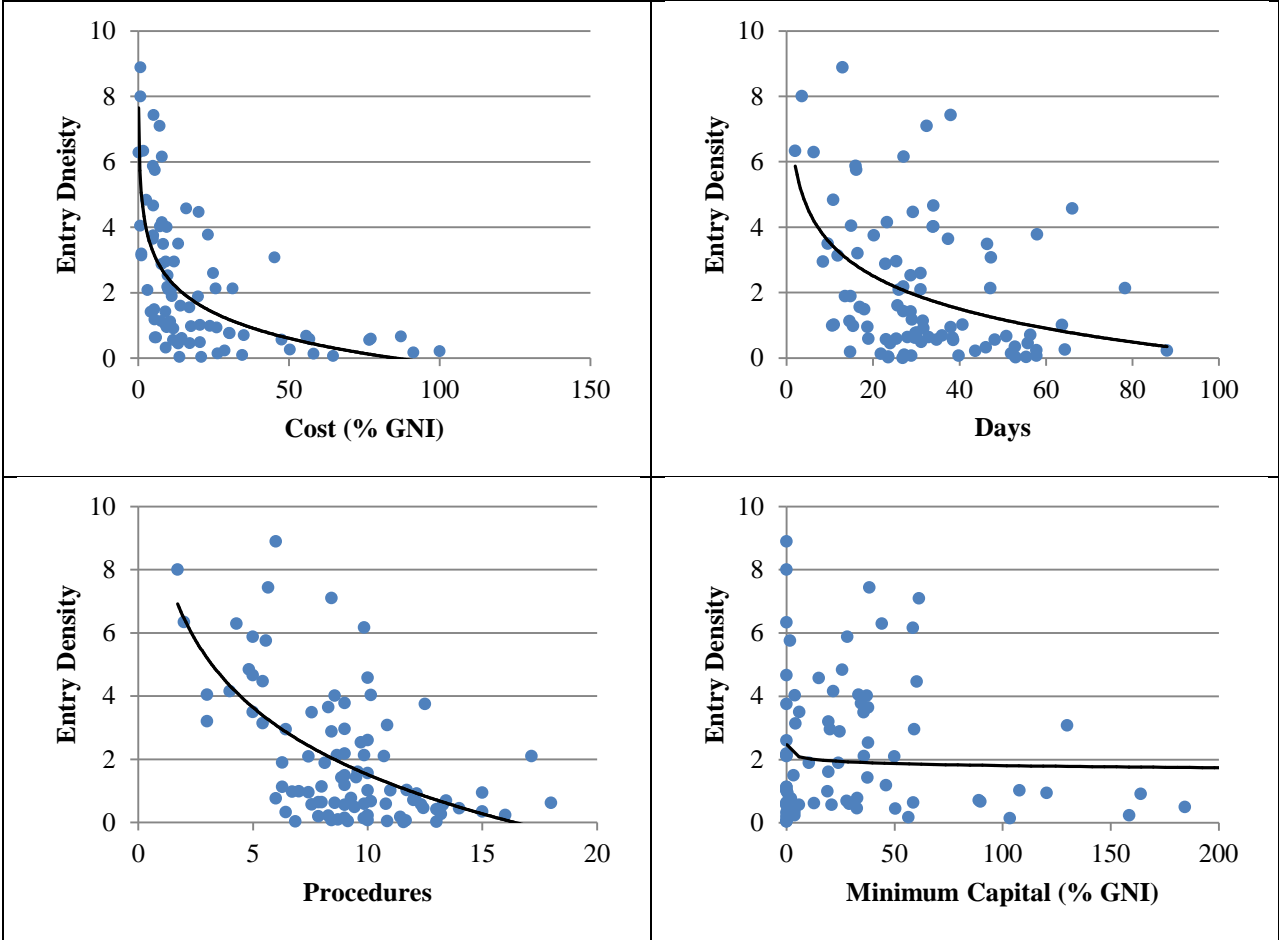
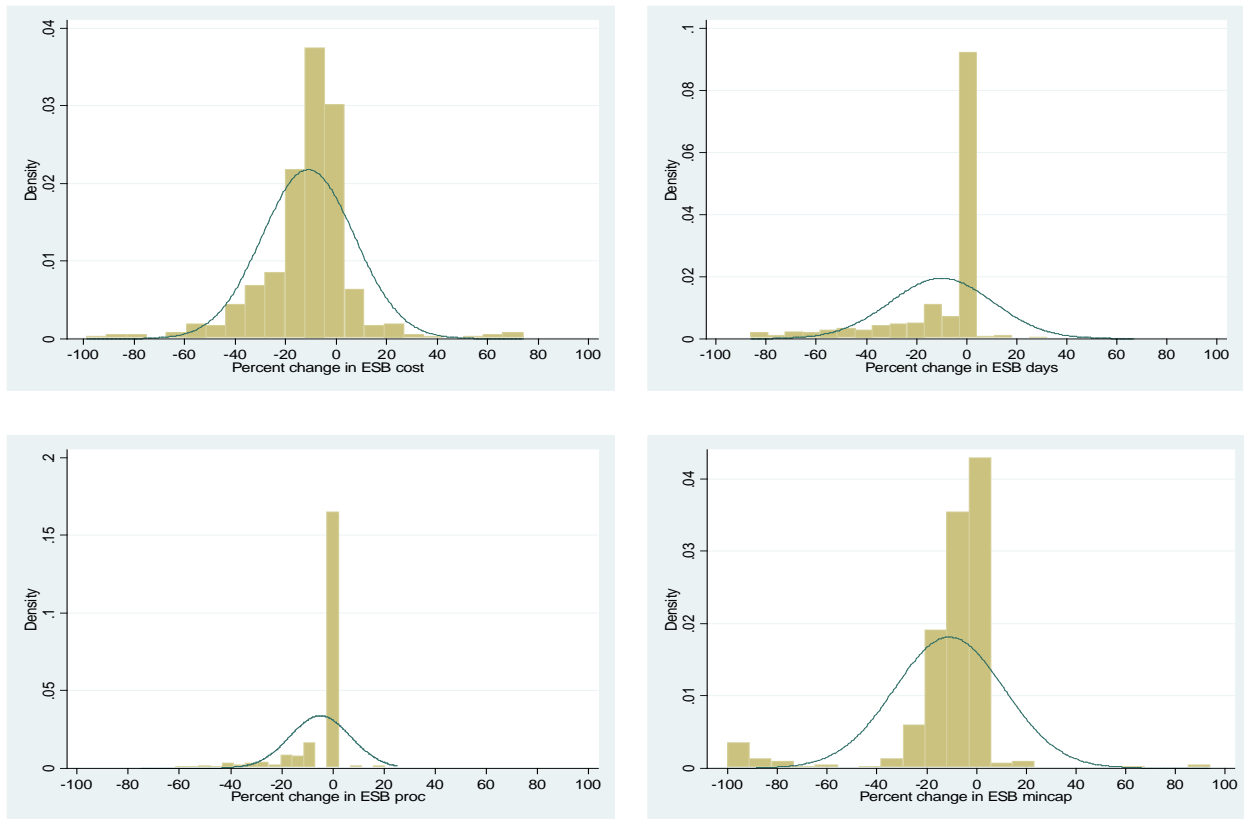


Figure 3: Scatter plots of Business Environment Variables





**Figure 4: Histograms of Percent Changes in Business Environment Variables**



**Table 1: Variable Definitions**

Label	Description
<b>Entry Density</b>	Entry density, defined as new firms registered per working age population (normalized by 1,000) ( <i>Entrepreneurship Database</i> )
<b>GDP growth (lagged)</b>	Lagged GDP growth ( <i>World Bank-World Development Indicators</i> )
<b>Procedures</b>	Procedures to register a business (number). Defined as any interaction of the company founders with external parties (i.e., government agencies, lawyers, auditors or notaries) ( <i>Doing Business</i> )
<b>Days</b>	Time needed to register a business (days). The measure captures the median duration that incorporation lawyers indicate is necessary to complete a procedure. ( <i>Doing Business</i> )
<b>Cost</b>	Cost to register a business. Cost is recorded as a percentage of the economy's income per capita. It includes all official fees and fees for legal or professional services if such services are required by law ( <i>Doing Business</i> )
<b>Minimum Capital</b>	The paid-in minimum capital required deposit in a bank or with a notary before registration (% GNI) ( <i>Doing Business</i> )
<b>Starting a Business: Index</b>	Average of standardized values of procedures (#), time (days), cost (% GNI) and minimum capital (% GNI) to register a business ( <i>Doing Business</i> )
<b>Registering Property: Index</b>	Average of standardized values of procedures (#), time (days) and cost (% GNI) to register a property ( <i>Doing Business</i> )
<b>Getting Credit: Index</b>	Average of standardized values of strength of legal rights index (0-10), depth of credit information index (0-6), public registry coverage (% adult population), private credit bureau coverage (% adult population) ( <i>Doing Business</i> )
<b>Enforcing Contracts: Index</b>	Average of standardized values of procedures (#), time (days) and cost (% GNI) of enforcing contracts ( <i>Doing Business</i> )
<b>Closing a Business: Index</b>	Average of standardized values of time (days), cost (% GNI) of enforcing contracts and recovery rate (cents on dollar recovered by creditors, inverted) ( <i>Doing Business</i> )
<b>Paying Taxes: Index</b>	Average of standardized values of time (hours), payments (number), and total tax rate (% profit before all taxes) for paying taxes ( <i>Doing Business</i> )

**Table 2A: Summary Statistics**

Variable	N	Mean	SD	p50	Min	Max
Entry Density	497	2.04	2.13	1.16	0.00	9.81
GDP growth (lagged)	497	5.56	3.57	5.34	-4.67	34.50
Procedures	497	8.78	3.47	9.00	1.00	18.00
Procedures (log)	497	2.21	0.40	2.30	0.69	2.94
Procedures (annual % change)	493	-4.74	11.84	0.00	-75.00	25.00
Days	497	37.69	70.86	26.00	2.00	694.00
Days (log)	497	3.23	0.82	3.30	1.10	6.54
Days (annual % change)	493	-9.72	20.48	0.00	-86.21	66.67
Cost	497	34.59	59.74	12.10	0.10	480.10
Cost (log)	497	2.75	1.25	2.57	0.10	6.18
Cost (annual % change)	493	-11.24	18.59	-8.92	-98.82	73.95
Minimum Capital	497	87.62	206.79	15.70	0.10	1821.90
Minimum Capital (log)	497	2.56	2.08	2.82	0.10	7.51
Minimum Capital (annual % change)	493	-9.93	27.14	-5.53	-99.94	283.33
Starting a Business: Index	497	0.00	0.65	0.18	-3.13	0.93
Registering Property: Index	491	0.00	0.67	0.08	-3.86	1.16
Getting Credit: Index	497	0.00	0.65	0.01	-1.30	2.20
Enforcing Contracts: Index	497	0.00	0.64	0.09	-1.83	1.09
Closing a Business: Index	497	0.06	0.51	0.06	-1.15	1.54
Paying Taxes: Index	423	-0.01	0.72	0.13	-3.38	1.40

**Table 2B: Correlation Matrix**

	(1)	(2)	(3)	(4)	(5)
Entry density (1)	1				
Procedures (log) (2)	-0.58*	1			
Days (log) (3)	-0.41*	0.69*	1		
Cost (log) (4)	-0.67*	0.59*	0.50*	1	
Minimum Capital (log) (5)	-0.18*	0.08	0.03	0.24*	1

Note: Correlations performed at the country level using 2005-2009 averages. Asterisk indicates significance at the 10% level

**Table 3: Number of Reforms with Different Cutoff Points, by Country**

<b>Cutoff</b>	<b>1 Procedures Reform</b>	<b>2 Days Reform</b>	<b>3 Cost Reform</b>	<b>4 Min Cap Reform</b>
<b>20%</b>	38	55	56	39
<b>30%</b>	27	47	41	28
<b>40%</b>	16	39	28	23
<b>50%</b>	8	31	16	23
<b>60%</b>	3	21	7	22

**Table 4: Number of Simultaneous or Sequential Reforms, by Country**

<b>Cutoff</b>	<b>1 One or more Reform</b>	<b>2 Two or more reforms occurring sequentially</b>	<b>3 Two or more reforms occurring simultaneously</b>	<b>4 Three or more reforms occurring sequentially</b>	<b>5 Three or more reforms occurring simultaneously</b>
<b>20%</b>	78	66	50	52	26
<b>30%</b>	67	50	34	29	14
<b>40%</b>	56	34	22	16	8
<b>50%</b>	45	24	14	12	5
<b>60%</b>	34	17	7	6	2

**Table 5: The Impact of Business Environment on New Firm Creation**

This table uses an unbalanced panel dataset of 497 observations from 91 countries for the six-year period 2004 to 2009. All variables are defined in Table 1. The dependent variable is annual entry density. All columns include country fixed effects, year fixed effects, and standard errors clustered at the country-level. Columns 5-8 also include four additional controls for other Doing Business reforms, described in Table 1. P-values are in squared brackets. \*\*\*, \*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

Variables	Entry Density							
	1	2	3	4	5	6	7	8
<b>Procedures (log)</b>	-0.674*** [0.003]				-0.708*** [0.005]			
<b>Days (log)</b>		-0.271* [0.060]				-0.263* [0.076]		
<b>Cost (log)</b>			-0.259* [0.057]				-0.259* [0.074]	
<b>Min. Capital (log)</b>				-0.061 [0.294]				-0.073 [0.227]
<b>Registering Property (index)</b>					0.072 [0.485]	0.135 [0.176]	0.137 [0.221]	0.214** [0.040]
<b>Getting Credit (index)</b>					-0.217 [0.498]	-0.174 [0.606]	-0.198 [0.556]	-0.229 [0.511]
<b>Enforcing Contracts (index)</b>					-0.123 [0.592]	-0.053 [0.826]	-0.1 [0.713]	-0.051 [0.841]
<b>Closing a Business (index)</b>					0.594 [0.132]	0.508 [0.141]	0.537 [0.173]	0.52 [0.168]
<b>GDP growth (lagged)</b>	0.008 [0.574]	0.007 [0.671]	0.007 [0.680]	0.011 [0.504]	0.011 [0.499]	0.01 [0.584]	0.01 [0.587]	0.014 [0.407]
<b>Constant</b>	3.253*** [0.000]	2.667*** [0.000]	2.490*** [0.000]	1.873*** [0.000]	3.235*** [0.000]	2.563*** [0.000]	2.405*** [0.000]	1.824*** [0.000]
<b>Observations</b>	497	497	497	497	491	491	491	491
<b>Adjusted R-squared</b>	0.947	0.946	0.946	0.945	0.947	0.946	0.946	0.945

**Table 6: Regression Results for Single Reform**

This table uses an unbalanced panel dataset of 487 observations from 91 countries for the six-year period 2004 to 2009. All variables are defined in Table 1. The dependent variable is annual entry density. The reported independent variable is a reform dummy denoted by the reform variable (columns 1-4) and the cutoff level in each row. Each box represents a separate regression. All models include country and year fixed effects and control for lagged GDP growth and four doing business indices: Registering a Property, Getting Credit, Enforcing Contracts and Closing a Business. Standard errors are clustered at the country-level. P-values are in squared brackets. \*\*\*,\*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

<b>Cutoff</b>	<b>1 Proc Reform</b>	<b>2 Days Reform</b>	<b>3 Cost Reform</b>	<b>4 Min Cap Reform</b>
<b>20%</b>	0.434** [0.012]	-0.008 [0.951]	0.070 [0.610]	0.332* [0.066]
<b>30%</b>	0.454** [0.032]	0.133 [0.432]	0.038 [0.827]	0.264 [0.164]
<b>40%</b>	0.519** [0.019]	0.197 [0.303]	0.142 [0.493]	0.420* [0.062]
<b>50%</b>	0.417 [0.166]	0.380** [0.042]	0.641** [0.030]	0.420* [0.062]
<b>60%</b>	0.085 [0.434]	0.594** [0.016]	0.785** [0.022]	0.402* [0.096]

**Table 7: Testing for Parallel Trends**

The dependent variable is annual entry density. Trend is a linear Trend and Trend \* Reform are interactions of the Trend and each respective reform dummy denoted by the reform variable (columns 1-4) and the cutoff level in each row. Each box represents a separate regression. All models include country fixed effects. All models are estimated on a sample of observation before first reform of 20% is observed for any of the four measures. Standard errors are clustered at the country-level. P-values are in squared brackets. \*\*\*,\*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

<b>Cutoff</b>	<b>1 Proc Reform</b>	<b>2 Days Reform</b>	<b>3 Cost Reform</b>	<b>4 Min Cap Reform</b>
<b>Trend</b>	0.039 [0.135]	0.034 [0.213]	0.032 [0.262]	0.051** [0.040]
<b>Trend * Reform20%</b>	0.105** [0.038]	0.13 [0.194]	0.176** [0.011]	0.168** [0.022]
<b>Trend</b>	0.041 [0.103]	0.035 [0.167]	0.037 [0.165]	0.059** [0.019]
<b>Trend * Reform30%</b>	0.136** [0.028]	0.174 [0.201]	0.233** [0.014]	0.143* [0.079]
<b>Trend</b>	0.048** [0.047]	0.035 [0.159]	0.051* [0.062]	0.061** [0.015]
<b>Trend * Reform40%</b>	0.128 [0.181]	0.193 [0.196]	0.298 [0.193]	0.141 [0.110]
<b>Trend</b>	0.050** [0.036]	0.034 [0.174]	0.055** [0.047]	0.065*** [0.010]
<b>Trend * Reform50%</b>	0.132 [0.321]	0.257 [0.138]	0.222 [0.373]	0.115 [0.221]
<b>Trend</b>	0.055** [0.020]	0.035 [0.155]	0.053* [0.052]	0.075*** [0.006]
<b>Trend * Reform60%</b>	-0.031 [0.204]	0.274 [0.157]	0.411 [0.239]	0.024 [0.501]
<b>Number of Observations</b>	367	269	284	360

**Table 8: Regression Results for Simultaneous or Sequential Reforms**

This table uses an unbalanced panel dataset of 493 observations from 91 countries for the six-year period 2004 to 2009. All variables are defined in Table 1. The dependent variable is annual entry density. The reported independent variable is a reform dummy denoted by the reform combination (columns 1-5) and the cutoff level in each row. Each box represents a separate regression. All models include country and year fixed effects and control for lagged GDP growth and four doing business indices: Registering a Property, Getting Credit, Enforcing Contracts and Closing a Business. P-values are in squared brackets. \*\*\*,\*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

<b>Cutoff</b>	<b>1</b> <b>One or more Reform</b>	<b>2</b> <b>Two or more Reforms occurring sequentially</b>	<b>3</b> <b>Two or more Reforms occurring simultaneously</b>	<b>4</b> <b>Three or more reforms occurring sequentially</b>	<b>5</b> <b>Three or more reforms occurring simultaneously</b>
<b>20%</b>	0.037 [0.758]	0.163 [0.177]	0.255 [0.152]	0.054 [0.686]	0.283 [0.223]
<b>30%</b>	-0.060 [0.642]	0.221 [0.121]	0.311 [0.159]	0.363** [0.032]	0.578* [0.083]
<b>40%</b>	0.024 [0.873]	0.457*** [0.008]	0.513* [0.084]	0.618** [0.020]	0.873* [0.067]
<b>50%</b>	0.360** [0.013]	0.602*** [0.004]	0.816** [0.024]	0.620* [0.061]	0.722 [0.294]
<b>60%</b>	0.452** [0.017]	0.643*** [0.007]	1.038** [0.043]	0.564*** [0.000]	0.348** [0.048]



**Table 9: Regression Results for Single Reform, with Interactions**

This table uses an unbalanced panel dataset of 487 observations from 91 countries for the six-year period 2004 to 2009. All variables are defined in Table 1. The dependent variable is annual entry density. The reported independent variable is a reform dummy denoted by the reform variable in the top rows and the cutoff level in the left columns and an interaction term between the reform dummy and the pre-reform index of the “Starting a Business” (ESB Index) measures. Each box represents a separate regression. All models include country and year fixed effects and control for lagged GDP growth and four doing business indices: Registering a Property, Getting Credit, Enforcing Contracts and Closing a Business. Standard errors are clustered at the country-level. P-values are in squared brackets. \*\*\*, \*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

Cutoff	Variable	1 Procedures Reform	2 Days Reform	3 Cost Reform	4 Min Cap Reform
20%	Reform	0.395*** [0.008]	0.028 [0.836]	0.116 [0.417]	0.384** [0.033]
	Reform*pre-reform ESB Index	0.691*** [0.003]	0.234** [0.035]	0.246*** [0.008]	0.347** [0.022]
30%	Reform	0.440** [0.021]	0.205 [0.241]	0.085 [0.645]	0.259 [0.148]
	Reform*pre-reform ESB Index	0.721** [0.016]	0.391** [0.016]	0.228** [0.038]	0.382* [0.074]
40%	Reform	0.458** [0.029]	0.200 [0.262]	0.174 [0.404]	0.462** [0.027]
	Reform*pre-reform ESB Index	0.428* [0.086]	0.637*** [0.001]	0.234** [0.015]	0.557** [0.042]
50%	Reform	0.369 [0.177]	0.396** [0.015]	0.700** [0.011]	0.462** [0.027]
	Reform*pre-reform ESB Index	0.550** [0.023]	0.753*** [0.001]	0.520** [0.025]	0.557** [0.042]
60%	Reform	0.118 [0.286]	0.459*** [0.008]	0.876*** [0.003]	0.451** [0.046]
	Reform*pre-reform ESB Index	0.167 [0.370]	0.703 [0.140]	1.289* [0.068]	0.542* [0.050]

**Table 10: Comparing Magnitudes - Buckets Approach**

This table uses an unbalanced panel dataset of 493 observations from 91 countries for the six-year period 2004 to 2009. All variables are defined in Table 1. The dependent variable is annual entry density. The reported independent variables are mutually exclusive reform dummies reflecting the cutoff bucket (1-19%, 20-29%, etc) into which the biggest reform in each country falls into a given category. Each **column** represents a separate regression. The excluded category is zero or no reforms for a country in a given category. All models include country and year fixed effects and control for lagged GDP growth and four doing business indices: Registering a Property, Getting Credit, Enforcing Contracts and Closing a Business. Standard errors are clustered at the country-level. P-values are in squared brackets. \*\*\*, \*\*, and \* represent significance at 1%, 5%, and 10% level respectively.

	<b>Panel A. Number of bucket reforms by country</b>			
	<b>Procedures</b>	<b>Days</b>	<b>Cost</b>	<b>Min Cap</b>
<b>1-19%</b>	23	17	32	24
<b>20-29%</b>	11	8	15	11
<b>30-39%</b>	11	8	13	5
<b>40-49%</b>	8	8	12	0
<b>50-59%</b>	5	10	9	1
<b>60+%</b>	3	21	7	22

**Panel B. Regression Results with Buckets**

	<b>Procedures Reform</b>	<b>Days Reform</b>	<b>Cost Reform</b>	<b>Min Cap Reform</b>
<b>1-19% (biggest)</b>	0.088 [0.584]	-0.048 [0.769]	-0.087 [0.448]	0.097 [0.349]
<b>20-29% (biggest)</b>	0.23 [0.281]	-0.171 [0.201]	0.093 [0.673]	0.181 [0.545]
<b>30-39% (biggest)</b>	0.371 [0.295]	0.011 [0.944]	0.016 [0.895]	-0.12 [0.596]
<b>40-49% (biggest)</b>	0.704** [0.019]	-0.282 [0.461]	-0.15 [0.642]	
<b>50-59% (biggest)</b>	0.464** [0.023]	-0.047 [0.854]	0.499 [0.225]	
<b>60+% (biggest)</b>	0.246* [0.069]	0.485* [0.056]	0.740*** [0.000]	0.449* [0.067]
<b>F-Tests</b>				
<b>20/29=1/19</b>	0.54	0.49	0.44	0.77
<b>30/39=1/19</b>	0.44	0.77	0.51	0.33
<b>30/39=20/29</b>	0.72	0.33	0.74	0.38
<b>40/49=1/19</b>	0.07*	0.55	0.85	
<b>40/49=20/29</b>	0.16	0.77	0.53	
<b>40/49=30/39</b>	0.44	0.47	0.62	
<b>50/59=1/19</b>	0.15	0.99	0.17	
<b>50/59=20/29</b>	0.41	0.64	0.37	
<b>50/59=30/39</b>	0.81	0.84	0.26	
<b>50/59=40/49</b>	0.50	0.59	0.21	
<b>60+=1/19</b>	0.37	0.05*	0.00***	0.16
<b>60+=20/29</b>	0.94	0.01**	0.03**	0.46
<b>60+=30/39</b>	0.73	0.09*	0.00***	0.06*
<b>60+=40/49</b>	0.13	0.09*	0.02**	
<b>60+=50/59</b>	0.29	0.12	0.59	

### Appendix 1: Entry Density, Average 2004-2009

Entry Density is defined as new firms registered per working age population (normalized by 1,000). Offshore financial centers identified by the IMF are shown in italics – and omitted from our empirical analysis.

Albania	0.77	Ghana	0.59	New Zealand	22.85
Algeria	0.45	Greece	0.94	Niger	0.00
Argentina	0.56	Guatemala	0.69	Nigeria	0.55
Armenia	1.49	<i>Hong Kong, China</i>	<i>15.25</i>	Norway	4.84
Australia	6.33	Hungary	4.46	Oman	1.42
Austria	0.64	Iceland	14.61	Pakistan	0.04
Azerbaijan	1.01	India	0.07	<i>Panama</i>	<i>3.04</i>
Belarus	0.45	Indonesia	0.17	Peru	2.13
Belgium	4.15	<i>Ireland</i>	<i>5.97</i>	Philippines	0.23
<i>Belize</i>	<i>3.88</i>	Israel	4.66	Poland	0.49
Bhutan	0.04	Italy	1.89	Portugal	4.01
Bolivia	0.34	Jamaica	1.12	Romania	5.75
Bosnia and Herzegovina	0.71	Japan	1.43	Russian Federation	4.03
Brazil	2.10	<i>Jersey</i>	<i>48.67</i>	Rwanda	0.19
Bulgaria	7.09	Jordan	0.59	Senegal	0.21
Burkina Faso	0.07	Kazakhstan	2.88	Serbia	2.10
Cambodia	0.23	Kenya	0.56	<i>Singapore</i>	<i>6.55</i>
Canada	8.00	Korea, Rep.	1.56	Slovak Republic	3.64
Chile	2.18	Kosovo	0.14	Slovenia	3.49
Colombia	1.02	Kyrgyz Republic	0.95	South Africa	1.13
<i>Cook Islands</i>	<i>7.77</i>	Latvia	5.88	Spain	4.57
<i>Costa Rica</i>	<i>11.14</i>	<i>Liechtenstein</i>	<i>34.62</i>	Sri Lanka	0.32
Croatia	2.95	Lithuania	2.09	Suriname	0.41
<i>Cyprus</i>	<i>25.64</i>	<i>Luxembourg</i>	<i>6.59</i>	Sweden	4.04
Czech Republic	2.53	Macedonia, FYR	6.16	<i>Switzerland</i>	<i>3.38</i>
Denmark	6.29	Madagascar	0.10	Tajikistan	0.26
<i>Dominica</i>	<i>3.60</i>	Malawi	0.07	Thailand	0.64
Dominican Republic	2.13	Malaysia	2.60	Togo	0.02
Egypt, Arab Rep.	0.13	Maldives	3.50	Tunisia	1.02
El Salvador	0.67	<i>Malta</i>	<i>9.66</i>	Turkey	0.99
Estonia	7.43	<i>Mauritius</i>	<i>8.20</i>	Uganda	0.62
Ethiopia	0.03	Mexico	0.62	Ukraine	0.91
Finland	3.20	Moldova	1.60	United Kingdom	9.25
France	3.14	Montenegro	3.75	Uruguay	3.07
Gabon	3.77	Morocco	0.98	Uzbekistan	0.56
Georgia	1.89	Netherlands	2.95	<i>Vanuatu</i>	<i>4.21</i>
Germany	1.18	<i>Netherlands Antilles</i>	<i>8.84</i>	Zambia	0.76