The Impact of Business Environment Reforms on New Registrations of Limited Liability Companies

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Panel data for 91 countries are used to study how the ease of registering a business and the magnitude of registration reforms affect new registrations of limited liability companies (LLCs). The costs, days, and procedures required to start a business are found to be important predictors of new LLC registration. Panel regressions also show important synergies in multiple reforms of two or more business environment indicators. These results are consistent with the intuition that to be effective, reforms should be sufficiently large so that the costs of registration are lower than the expected benefits. In addition, countries with relatively weaker business environments prior to reforms require relatively larger reforms to impact the number of newly registered LLCs. Entrepreneurship, Business Environment, Government Reforms. JEL classification codes: G18, G38, L51, M13

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 in an attempt 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 et al. (2002). Since 2003, the World Bank's annual Doing Business report has used this methodology to quantify the registration process in more than 170 countries in its "Ease of Starting a Business" section.[1] An outstanding question, however, is whether and to what

degree there exists an economically meaningful relationship among 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,[2] 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 newly registered limited liability companies (LLCs).

An understanding of 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 LLC registrations. We compare countries that have reformed the registration process to countries without reforms both before and after reform. Thus, our main analysis uses a difference-in-difference approach. We further examine the effect of multiple reforms across several measures of entry regulation. Finally, we examine the extent to which the impact of reform depends on the initial 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 LLCs. We supplement this dataset with information from Doing Business reports on the cost, days, and number of procedures required to register new companies. Importantly, both datasets focus on only limited liability companies.

We start with a discussion of the intuition for our empirical strategy. We presume that the total costs of registering, which include a monetary value of time and effort, need to fall below the expected benefits of being a registered firm to induce an entrepreneur to register. Our results are consistent with this intuition and suggest that reforms should be sufficiently large that the costs of registration fall below the expected benefits to induce entrepreneurs to register their firms. We also find that the impact of reforms is non-linear. In addition, multiple reforms have a larger impact on business registration at smaller cutoff levels. Thus, there is a synergistic effect of multiple 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 the number of newly registered LLCs. This implies that in countries with relatively high initial registration costs, the expected 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

2. For instance, a 2008 report of the Multilateral Donor Committee for Enterprise Development states that "[r]eforming 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 2008, 3).

et al. (2002), who show that high registration costs do not serve public interest but only benefit politicians and bureaucrats.[3]

The results of our paper suggest non-trivial economic magnitudes of the number of newly registered limited liability 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 consistent with previous studies of registration reforms in two different OECD countries. For example, a reform in Mexico that decreased the number of procedures by approximately 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 2011). A reform in Portugal that reduced costs and days by 50 percent or more resulted in an increase in firm startups of approximately 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).

There are several potential caveats to our analysis. The first is that business registration reforms may take place along with other political or regulatory changes, such as the transition to a new government, capital market liberalization, or a broader set of private sector reforms. To alleviate the concern that other reforms are driving our results, we include in our regressions four additional Doing Business indicators - Registering Property, Getting Credit, Enforcing Contracts, and Closing a Business – and find them to be insignificant predictors of new firm registrations. In addition, we control for lagged GDP growth to capture any macroeconomic and business cycle effects. Nevertheless, there might be other simultaneous reforms or a general shift toward greater support of the private sector that we do not measure. The concern is that the ease of registration reforms could be part of a broader private sector reform package. However, such packages are likely to vary across countries that undertake ease of registration reforms. That is, the ease of the registration reforms that we consider are not always accompanied by the same types of other "omitted" reforms. This is highly plausible given the heterogeneity of reforming countries in our sample. Therefore, it is unlikely that the same omitted reforms across all the reforming countries in our sample will drive our results. Nevertheless, the causal interpretation of our results should be considered with caution.

A second caveat is that increased firm registrations may precede rather than follow the ease of registration reforms. It is plausible that governments may want to capitalize on a new registration boom and appease the business community by easing the registration process. Alternatively, the government might be forced to

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3. Our results are also consistent with de Mel et al. (2012), who find that firms in Sri Lanka rationally refrain from formalizing because they see few benefits from doing so, whereas 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.

streamline the registration process in response to increased demand for new registrations. To alleviate this concern, we perform a parallel trend analysis and find that we cannot reject the hypothesis that prior to their first reform, reforming countries had the same trend as non-reforming countries. Nevertheless, the assumption of parallel trends (i.e., that the trends would have remained parallel in the absence of reform) is inherently untestable.

A third caveat is that we can measure the increase in new LLC registrations following a reform, but we cannot determine if there is a net increase in total new firm registrations because there may also be an unobserved decrease in the number of non-LLC registrations (e.g., sole proprietors or partnerships).[4] In other words, easing registration of LLCs might incentivize entrepreneurs to choose to register their firms as limited-liability companies rather than as an alternative registration form. Finally, we cannot identify whether new registrations represent newly created businesses or existing businesses that previously operated informally. This is an important avenue for future research.

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; Johnson, et al. 2002). For example, studies using longitudinal data sets on the evolution of firm formation document that economic growth in both Canada and the United States is driven by new formal business entry rather than the growth of existing firms (Brander et al. 1998; Haltiwanger et al. 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; Dabla-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 the prevalence of informality (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 establishment of businesses, lead to greater misallocation of productive resources, and obstruct economic growth (De Soto 1990; Djankov et al. 2002, Klapper et al. 2006; Kaplan et al. 2011; Bruhn 2011; Barseghyan and DiCecio 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 perceived by actual business owners (Hallward-Driemeier and Pritchett 2011). This paper helps

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4. Some studies show that formal sole proprietorships are more numerous than limited liability companies in most developed and developing countries; e.g., Giovanni (2008) and Fabozzi and Peterson (2003).

reconcile this recent skepticism with an analysis of the relevance of Doing Business measures using a real-world output: new LLC registrations.

Importantly, this paper offers policymakers empirically based insight into the potential 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 design interventions with the greatest impact on private sector growth.

The paper proceeds as follows. We first present our intuition and empirical strategy. We then discuss the data and summary statistics. Subsequently, the regression results are presented, and we then offer some conclusions.

Intuition

We begin with a simple intuition of how the reforms to formal business registration affect entrepreneurs' decisions to formally register their business. We assume there are potential benefits for an entrepreneur to operate a formally registered firm (compared to not starting a business or operating informally). The benefits might include 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. The costs of registration include 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. For a reform to be effective, the cost of registration must be less than the benefits of registration. This simple intuition suggests that "small" reforms, in which costs are reduced insufficiently and do not fall below the benefits, will not have a significant impact on business decisions.

First, we test whether there is a negative relationship between regulatory barriers in starting a business and actual registrations. To do so, we run a simple model of new LLC registrations on our regulatory indicators while capturing any country-specific unobservable factors with country fixed effects.

Second, we investigate what magnitude of reforms produces the most significant changes in newly registered businesses. Specifically, we measure reforms as the percent change from year to year 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 is to use a difference-in-difference estimation to test the effect of one indicator of reform (e.g., "Reform_20%") on new LLC registration. We then compare the 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. In other words, this methodology allows us to identify which cutoff points lead to the largest and most significant increases in formally registered businesses.

DATA AND DESCRIPTIVE STATISTICS

Data on New LLC Registrations

For our dependent variable, we use data on the number of new limited liability firms registered in the calendar year. The data are 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 from other statistical offices. The data are not based on surveys or estimations[5] and include only new corporations, which are defined here as *private companies with limited liability*. Notably, this is the same definition used by the World Bank's Doing Business report.

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 (aged 15–64).[6] The final dataset includes an unbalanced panel of 494 observations from 91 countries over the six-year period 2004–9. Figure A1 in the supplemental appendix shows the distribution of entry density across countries, with a minimum value of 0.0021 (in Niger) and a maximum value of approximately 10.[7]

Entry density varies significantly by income groups, ranging from 4.21 in high-income countries to less than one in lower-middle and lower income countries. In other words, on average, there are approximately four limited liability firms registered annually per 1,000 working age individuals in industrialized countries compared with approximately one LLC per 1,000 individuals in developing countries.[8]

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 that can be used to measure the efficiency of the business

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5. For additional information on the methodology, see Klapper and Love (2010). We also exclude countries identified by the IMF (Zorome 2007) as off-shore financial centers.

6. The complete database and list of sources is available at http://doingbusiness.org/entrepreneurship. Summary statistics, by country, are shown in Table A1 in the supplemental appendix.

7. We restrict entry density to be less than 10. This effectively 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.

8. Summary statistics, by country, are shown in figure A1 in the supplemental appendix, available at http://wber.oxfordjournals.org.

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. 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, the 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 A2 in the supplemental appendix shows the relationship between the explanatory variables and entry density, and figure A3 in the supplemental appendix shows the distributions of the annual percent change in these four explanatory variables.

Additional tables available in the supplemental appendix contain the following: complete variable definitions (table A2), basic summary statistics (table A3) and a correlation matrix (table A4). For all four indicators of Starting a Business, the mean of the annual percentage change is negative, suggesting that, on average, countries have been lowering registration costs and reducing days, procedures, and capital requirements.

Classifying Reforms

Table 1A shows the number of reformers that we obtain with the 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 seven with a 60 percent cutoff. Because the number of reforms becomes smaller with increasingly high cutoffs, 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: increased cutoffs result in a smaller number of reforming countries. For the purpose of our regression analysis, we consider only the impact of the first

TABLE 1A. Number of Reforms with Different Cutoff Points by Country

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

Source: Doing Business (2011).

TABLE 1B. Average Reduction for Each Reform

Reform	Procedures	Days	Cost	Min. Capital
20%–30%	2.1	13	12	58
30%–40%	3.7	19	16	38
40%–50%	4.0	21	51	124
50%–60%	4.7	33	33	368
> 60%	5.0	30	125	421

Source: Doing Business (2011).

reform for each of our definitions; that is, we consider a country a "reformer" for all subsequent years following the first reform.[9]

Table 1B shows the average reductions in the actual number of procedures, days, cost and minimum capital associated with different degrees of reform.[10] The data show that a 20 percent reform (i.e., the averages in the 20–30 percent bucket) is, on average, associated with a reduction of two procedures, 13 days, 12 percent cost (as a fraction of income per capita), and 58 percent reduction in minimum capital (as a percent of income per capita). A 40 percent reform, on average, is associated with a reduction of approximately four procedures, 21 days, a 51 percent reduction in cost, and a 124 percent reduction in minimum capital.

Because our reforms are defined as a percent reduction in costs, days, and procedures, the same reform will imply different level changes. Although larger reforms in terms of percent change generally imply larger nominal cuts in procedures, days, and costs, the relationship is not necessarily monotonic because a country with a larger initial number of procedures will have a larger reduction in procedures for the same magnitude of reform. For example, Singapore changed the number of procedures from four to three in 2009, which is a 25 percent change and is classified as a 20 percent reform (i.e., it fits in the 20–30 percent bracket), whereas Mozambique changed the number of procedures from 13 to 10 in 2007, which is a 23 percent change that falls within the same 20–30 percent reform category.

9. Note that because our data are 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.

10. In this table, reforms are defined as mutually exclusive – that is, in the row identifying 20 percent reforms, the averages are calculated over the country-years with reforms between 20–30 percent, in the row identifying 30 percent reforms, the averages are calculated over the country-years with reforms between 30–40 percent, and so on. The last row shows all reforms 60 percent or larger (although it is important to remember that there is only a small number of reforms in this final category).

REGRESSION RESULTS

Continuous Regression 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 LLCs. To do so, we investigate within-country variation by 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, α_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 that 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 cover 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 capture the average drop in registration in the year of the crisis relative to previous years and eliminate the confounding impact of the crisis. In addition, the time dummies capture global changes in registration trends. Thus, Klapper and Love (2010) show that the number of registered businesses is increasing over time; 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 the need to isolate the impact of business registration reforms from other reforms that may be happening at the same time. Although 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.[11] Specifically, we add indices measuring Doing Business indicators for Registering Property, Getting Credit, Enforcing Contracts, and Closing a Business (described in tables 1A and 1B). We

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11. We recognize that there might be other factors that encourage new firm registration, such as elections and changes in political regimes.

estimate this model with errors clustered at the country level to capture any serial correlation of errors within country.[12]

The results are presented in table 2. Columns 1–4 present our results without controls, and columns 5–8 present the 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 it is not significant at conventional levels.[13]

Including indices for other features of Doing Business has no material impact on our results (see columns 5–8), and the indices themselves are not significant in any of the specifications. These results suggest that, unlike business registration

T A B L E 2 .   Continuous Regressions

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

Notes: This table uses an unbalanced panel dataset of 497 observations from 91 countries for the six-year period 2004–9. 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, as described in table 1. P-values are in squared brackets. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Source: World Bank Entrepreneurship Database (2010), Doing Business (2011), and data sources discussed in the text.

12. 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).

13. In addition, we have attempted a specification with all four variables included simultaneously with and without controls. We find that individual variables are not significant. This is not surprising

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. However, the fact that these additional Doing Business reforms are not significant mitigates the concern that some omitted reforms might be driving our results.

In addition, the relationship between registration and costs, days and procedures appears to be non-linear (figure A2). Therefore, we perform additional regressions with the quadratic specification reported in table A5 in the supplemental appendix.[14] We find some evidence of non-linear effects. The results suggest that when the procedures/days/costs are high, a small reduction is not associated with a significant change in the number of LLC registrations.

Reform Regression Model

In the previous section, we showed that higher costs (days and procedures) are associated with lower new LLC registrations. Although these continuous results suggest that a reduction in regulatory barriers will increase new registrations, they do not directly imply that "reforms", which are discrete events, will have cumulative and significant effects. To evaluate the impact of "reforms", one needs to compare what happens to registration after the reform with what occurs before the reform both in the "treatment" group (i.e., the group of reforming countries) and in a "control" group without reforms. Our main methodology implements this idea using a difference in difference set-up. However, our data come in continuous form, and we need to define what constitutes a reform and what does not. We accomplish this by investigating various cutoff points, as discussed earlier. Specifically, we test which magnitude of reforms produces the largest change in business registrations. We run the following regression:

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

Here, Reform is equal to 0 using each of the cutoffs discussed above and for years in which the 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 from the continuous framework discussed in the last section is that each reform is treated as a discrete event and is allowed to have a cumulative impact during the years subsequent to the reform. In contrast, in the

because they are highly correlated. Nevertheless, an F-test for joint significant rejects the null hypothesis that all four coefficients are equal to zero.

14. We have also experimented with cubic specification and found it to be not significant for days, costs, and procedures.

TABLE 3. Regression Results for Single Reform

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

Notes: This table uses an unbalanced panel dataset of 487 observations from 91 countries for the six-year period 2004–9. 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 the 1%, 5%, and 10% levels, respectively.

Source: World Bank Entrepreneurship Database (2010), Doing Business (2011), and data sources discussed in the text.

previous section, we only considered the contemporaneous relationship between the level of business regulations and the level of new LLC 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.

Table 3 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 β_1 from model 2). We observe that for a reform to costs or days to be statistically effective, the barrier needs to be reduced by at least 50 percent relative to the pre-reform level. The largest impact for costs and number of days is observed for a 60 percent reduction, but in this case, only 7 countries are classified as reformers for costs and 23 for the number of days (table 1).

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. 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 approximately

40 to 50 percent. For minimum capital requirements, we find that a 20 percent reform, as well as 40–60 percent reforms, yields significant coefficients.[15]

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.[16] 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 the 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, whereas similar reductions in the number of days and costs lead to average 19 and 30 percent increases, respectively, in new registrations of limited liability firms. These results are remarkably consistent with previous studies of registration reforms in two different OECD countries, as discussed in the introduction (Branstetter et al. 2010; Bruhn 2011; Bruhn 2013).[17]

Next, we construct a graphical demonstration of our main results, shown in figure 1. These graphs show average new density around the reform window. Based on the coefficients reported in table 3, we report the graphs for 20 percent reform for procedures and 40–50 percent for costs and days. We line up all countries around the reform time, denoting the year of reform as zero. The vertical line is drawn at the year of reform. We remove the country and year fixed effects for easier comparison across countries and time.[18] To make the results comparable across time, we create two samples of countries that contain the same set of countries for all the years around the reform window (the sample construction is described in the notes to figure 1).

The graphs for all three main variables of interest show an increase during the year of reform shortly after the reform, in line with our regression results. Figure A4 in the supplemental appendix replicates these graphs with added error

15. 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 case they are completely eliminated rather than reduced. This is visible in the distributions of changes to minimum capital, which has significant density in the left tail of the distribution.

16. We calculate the increase in new registrations by taking actual new density in a country and adding 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. We multiply these new densities by population to obtain new registrations after the reform and calculate a percent change in new registrations.

17. Kaplan et al. (2011) 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 do not include firms without employees or firms that fail to formally register their employees for Social Security benefits.

18. In other words, we plot the residuals from the regression of density on country and year fixed effects. This removes the variation in density introduced by cross-country differences in the levels of new registration and the variation across time that is the same for all countries.

FIGURE 1. Pre- and Post-Reform Event StudiesThese graphs show the average entry density around the reform window. The average entry density is stripped from country and year fixed effects. The reform time is zero, as indicated by the vertical line. Sample 1 consists of all countries that experienced the stated reform of given magnitude and have at least three years of data surrounding the reform (i.e., one year prior to the reform, the year of reform and one year post reform). Sample 2 consists of all countries that experienced the stated reform of given magnitude and have at least five years of data surrounding the reform (i.e., two years prior to the reform, the year of reform and two years post reform). The averages are calculated only for the countries in the respective samples. The number of countries in each sample is given in parenthesis.

Panel A: Procedures 20% reform,
Sample 1 (n = 33)
Panel C: Days 40% reform,
Sample 1 (n = 25)
Panel E: Cost 50% reform,
Sample 1 (n = 10)

Panel B: Procedures 20% reform,
Sample 2 (n = 14)
Panel D: Days 40% reform,
Sample 2 (n = 11)
Panel F: Cost 40% reform,
Sample 2 (n = 7)
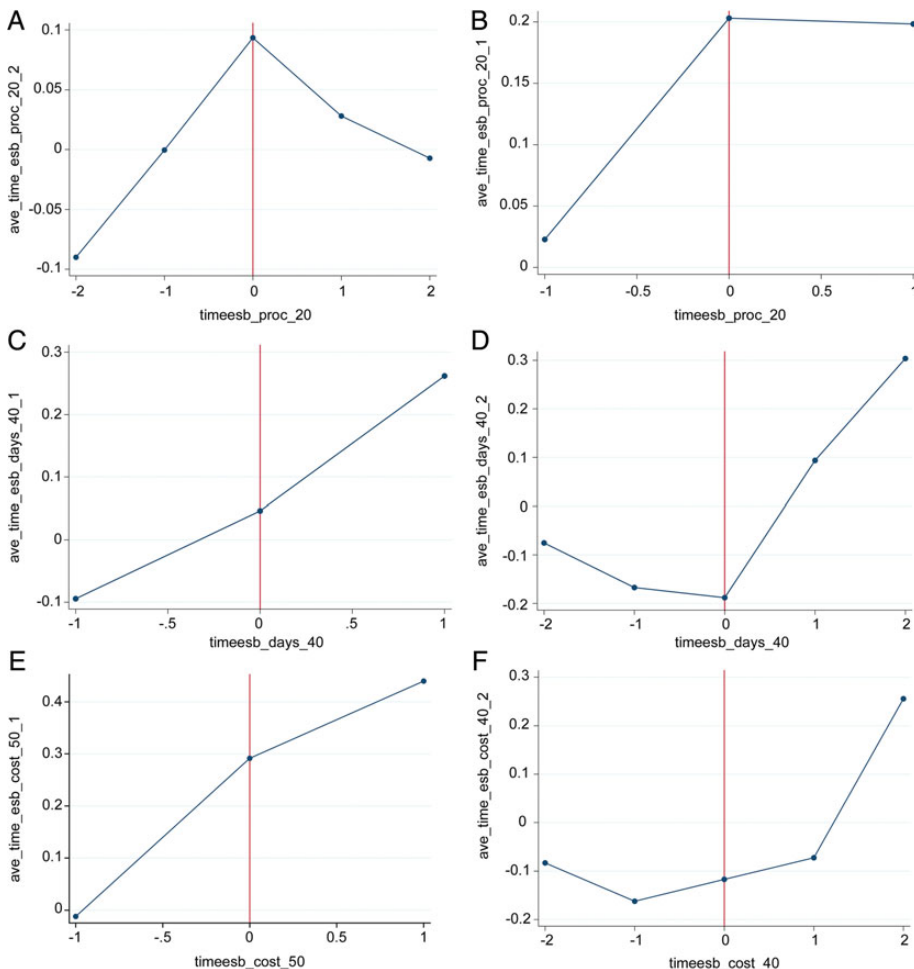
Source: World Bank Entrepreneurship Survey (2010), Doing Business (2011).

bands (one standard deviation above and below the average estimate). The errors are fairly large; hence, the impact appears muted on the graph with error bands. Nevertheless, our regressions show statistical significance because they use all available data in the sample and aggregate the total post-reform impacts. In other words, the regression coefficients show a cumulative impact of the reform in all post-reform years available in our sample. Thus, our regression methodology is a better test of significance than the graphs; however, the graphs (without the bands) provide visually compelling evidence.

Testing for Parallel Trends

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 controls for any factors 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 because we do not observe the counterfactual (i.e., the treated group in the absence of the treatment). One way to validate this assumption is to compare the pre-reform trends in two groups. We do this using the following model:

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

Here, Reform is equal to one for all countries classified as reformers using each of the cutoffs discussed above. Unlike model (2), the Reform dummy is not time varying. In other words, it equals 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, whereas for the reformer countries, we only use observations *prior to the first reform* of 20 percent 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 and year dummies.[19]

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. Although this is not direct proof of the identification assumption (which is not testable, as noted above), these results offer some degree of reassurance.

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19. Our results are similar if year dummies are not included.

TABLE 4. Testing for Parallel Trends

Variables	Procedures	Days	Cost	Min Capital
Trend	−0.311	−0.447	−0.274	−0.345
	[0.022]**	[0.004]***	[0.044]**	[0.010]***
Trend*Reform 20%	**0.077**	**0.053**	**0.161**	**0.107**
	[0.385]	**[0.738]**	**[0.153]**	**[0.284]**
Trend	−0.309	−0.441	−0.280	−0.354
	[0.022]**	[0.004]***	[0.033]**	[0.008]***
Trend*Reform 30%	**0.132**	**0.120**	**0.263**	**0.087**
	[0.250]	**[0.548]**	**[0.093]***	**[0.444]**
Trend	−0.314	−0.437	−0.309	−0.353
	[0.019]**	[0.005]***	[0.019]**	[0.008]***
Trend*Reform 40%	**0.192**	**0.148**	**0.479**	**0.091**
	[0.334]	**[0.491]**	**[0.153]**	**[0.456]**
Trend	−0.317	−0.430	−0.309	−0.361
	[0.017]**	[0.005]***	[0.019]**	[0.006]***
Trend*Reform 50%	**0.256**	**0.224**	**0.404**	**0.066**
	[0.349]	**[0.356]**	**[0.356]**	**[0.619]**
Trend	−0.332	−0.427	−0.309	−0.390
	[0.012]**	[0.005]***	[0.019]**	[0.004]***
Trend*Reform 60%	**−0.120**	**0.252**	**0.644**	**−0.077**
	[0.008]*	**[0.344]**	**[0.223]**	**[0.132]**
Adjusted R2	0.95	0.95	0.95	0.95
N	372	274	287	365

Notes: The dependent variable is annual entry density. Trend is a linear Trend, and Trend * Reform is the 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 and year dummies. All models are estimated on a sample of observation before the 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 the 1%, 5%, and 10% levels, respectively.

Source: World Bank Entrepreneurship Database (2010), Doing Business (2011), and data sources discussed in the text.

Table 4 presents our results. Each box presents results for a separate regression for each of the cutoff points. The number of observations is the same in each column because we use all observations prior to observing the first reform of 20 percent or more.

Again, the key focus is on the interaction of Trend and Reform. We estimate a total of 20 interaction coefficients (five cutoff points for four indicators), and only two of the 20 coefficients are statically significant: only one is positive at 10 percent significance (for cost at 30 percent reform) and one is negative (for 60 percent procedures reform). A negative interaction does not invalidate our strategy (because it suggests that the reformers would have had lower levels of registrations if the reform did not occur). Thus, our results do not show systematic differences in pre-reform trends in two groups of countries.

Importantly, these results suggest that there is no reason to expect that the reforming countries had different trends relative to non-reformers prior to their first reform. We perform this test to rule out the possibility that the increase in

registrations that we observe would have happened anyway because of the pre-existing trend differences. This also alleviates concerns with endogeneity (i.e., that an increase in new LLC registrations preceded rather than followed reforms).

Additional Results

In this section, we briefly describe several additional tests. First, we investigate whether reforms have synergistic impact. For example, one 30 percent reform might be insufficient, but two or three 30 percent reforms may be enough to induce firms to register. Table A6 in the supplemental appendix contains the number of countries with multiple reforms, whereas table A7 contains the regression results. Indeed, we find that when multiple reforms occur, each individual reform can be smaller in magnitude than in the case of a single or double reform. Thus, as part of the aggregate package, even 20 percent or 30 percent reforms can produce a significant outcome. However, at 50 percent, one reform is already enough to produce a significant difference, and two reforms offer a further significant reduction in aggregate costs. These results are in line with our main conclusion, which states that regulatory reforms need to be large enough to bring the costs of registrations below the expected benefits of formalization.

Second, we investigate whether the impact of reforms varies with the pre-reform level of the business registration environment. In other words, we study how initial regulatory conditions affect the impact of reforms. There are two alternative hypotheses regarding the impact of initial costs. The first hypothesis holds if the expected benefits in a high initial cost environment are also relatively high. In this case, even a small change will be welcomed by severely constrained entrepreneurs. The second hypothesis is that in high initial cost environments, expected benefits are significantly below costs; hence, entrepreneurs need larger changes to motivate them to register. Although we cannot observe the expected benefits directly, we can test these alternative hypotheses with the interaction of our reform measures with initial conditions. Specifically, we interact our reform dummies with the country-specific level of the regulatory conditions in the year before the reform (again, we only consider first reforms). This specification allows us to investigate whether the same magnitude of reform has a more or less significant impact with different pre-reform levels.[20]

Our results are given in table A8 in the supplemental appendix. We observe that for almost all cutoff points and all four measures, the interactions with pre-reform levels are significantly positive. This finding implies that a better pre-reform regulatory environment for starting a business is associated with a larger impact of the reform. Thus, a country with weak pre-existing business environment levels

20. 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 the "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 the mean about zero and the standard deviation about one.

needs a larger reform to achieve the same impact on the number of registered businesses as a country with a strong business environment.

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, and the interaction is positive. We interpret this to mean that small reforms are only statistically meaningful in countries with above-average pre-reform business environments (i.e., with an ESB index above zero). Larger reforms (of a 50 percent decline in days or costs) are effective even in countries with below-average business environments (because the coefficients and interaction are both significant), but their effectiveness is higher in countries with stronger starting positions (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 a higher pre-reform ESB index (i.e., better initial conditions).

To summarize, these results suggest that countries that begin 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 than countries that have relatively better ex-ante business environments. These results suggest that in countries with high registration costs (or long delays or a large number of procedures), the expected 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 expected benefits of registrations. Instead, these results support the "public choice" theory, which suggests that high registration costs exist to benefit bureaucrats and politicians (Djankov et al. 2002).

Comparing the Magnitudes: Buckets Approach

Our results thus far are based on defining reforms as cutoff points. For example, a 20 percent reform dummy equals 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 intuitive for 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; 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 reform buckets, such as reforms 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 because different buckets have fewer reforms on which to estimate the coefficients.[21]

Table A9 in the supplemental appendix shows the number of reforms in each bucket. As expected, the number of reforms is relatively small – on average, approximately 10 reforms per bucket (more for the lowest bucket defined as reforms of 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, the 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 5 presents our regression results. Unlike table 3, in which each cell represents a single regression, each column now represents a single regression, and all dummies enter simultaneously. We find that for procedures, the largest impact is observed in the 40–49 percent bucket, whereas 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.

TABLE 5. Comparing Magnitudes - Buckets Approach

	Procedures Reform	Days Reform	Cost Reform	Min Cap Reform
1–19% (biggest)	0.088	−0.048	−0.087	0.097
	[0.584]	[0.769]	[0.448]	[0.349]
20–29% (biggest)	0.23	−0.171	0.093	0.181
	[0.281]	[0.201]	[0.673]	[0.545]
30–39% (biggest)	0.371	0.011	0.016	−0.12
	[0.295]	[0.944]	[0.895]	[0.596]
40–49% (biggest)	0.704**	−0.282	−0.15	
	[0.019]	[0.461]	[0.642]	
50–59% (biggest)	0.464**	−0.047	0.499	
	[0.023]	[0.854]	[0.225]	
60 + % (biggest)	0.246*	0.485*	0.740***	0.449*
	[0.069]	[0.056]	[0.000]	[0.067]

Notes: This table uses an unbalanced panel dataset of 493 observations from 91 countries for the six-year period 2004–9. The dependent variable is annual entry density. The reported independent variables are mutually exclusive reform dummies reflecting the cutoff bucket (e.g., 1–19%, 20–29%) into which the largest reform in each country falls in 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 the 1%, 5%, and 10% levels, respectively.

Source: World Bank Entrepreneurship Database (2010), Doing Business (2011), and data sources discussed in the text.

21. Because our buckets are mutually exclusive, we now consider only the largest reform in each country, whereas we previously considered the first reform, which may or may not be the largest. Our previous results are robust to considering only the largest reforms.

We use F-tests to obtain pair-wise comparisons of different coefficients (table A10 in the supplemental appendix). 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. Additionally, the coefficient for the 50–59 percent bucket is marginally different (at 15 percent) from the coefficient for the 1–19 percent bucket. These results square well with what we found in the earlier tables: 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 the buckets below 50 percent, but they are not significantly different from the coefficients for the 50–59 percent bucket. This result closely matches our previous result because for both costs and days, the coefficients on the 50 percent or more and 60 percent or more reforms are significant (table 3). 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, this robustness test for our previous results suggests 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.

Conclusion

In this paper, we test the relationship between the Doing Business "Ease of Starting a Business" measures of the regulatory environment and the number of annual new limited liability 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 reforms need to be large enough so that the total costs of registration fall below the expected benefits of registration. Thus, "token" reforms, perhaps motivated by political or multilateral pressures to reform, do not have the intended effect on private sector activity. We also show that there is a synergistic impact of reforms. In the case of two reforms, each of the reforms can be smaller than if they occurred independently. Furthermore, we show that countries with relatively weaker business environments (i.e., relatively more stringent initial registration requirements) require relatively larger reforms to impact new LLC registration. It is likely the case that countries with weaker business environments also have fewer expected benefits for formal sector registration (such as access to formal financial and labor markets); therefore, larger reductions in costs are necessary to incentivize firms to incur the costs of formal registration.

Our results highlight the importance of defining reforms in a way that is consistent with expected outcomes – in this case, the number of newly registered

limited liability companies. The methodology developed in this paper offers 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 may also be helpful to motivate policymakers to make larger and more meaningful reforms.

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