

The Impact of Business Environment Reforms on New Firm Registration

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Abstract

The authors use panel data on the number of new firm registrations in 92 countries to study how the magnitude of reforms affects new firm registrations. They find that small reforms, in general less than 40 percent reduction in costs, days, or procedures required for business registration, do not have a significant effect on new firm creation. This suggests that small reforms do not have

the intended effect on private sector development. 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 require relatively larger reforms in order to impact new firm growth. These results can be helpful to motivate policymakers to make larger, broader reforms.

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.

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1. Introduction

In response to the 2008 global financial crisis, governments around the world are interested in policies and reforms to promote new firm creation. For instance, policymakers in the U.S. have called for more government support of entrepreneurs in order to help the US to regain a competitive lead in the world economy (Schramm, 2004, and Baumol, 2007). Moreover, in the EU, the Lisbon Declaration of March 2000 explicitly identifies entrepreneurship as the key to the EU becoming the most competitive world region by 2010 (Naudé, 2008).

To promote new firm registrations, many countries have been undertaking reforms of the registration process, by reducing the costs, days or procedures required to register a business. For example, the World Bank's Doing Business reports name countries as "10 Top Reformers" (based on all subsections) and 30-65 "Starting a Business Reformers", classified as top reformers of the business registration process based on the quantitative measures they collect.¹ Yet, regulatory reform is more widespread: In 2008/09, 131 economies around the globe reformed business regulation, making it easier to start and operate businesses, strengthening property rights, and improving commercial dispute resolution and bankruptcy procedures. Two-thirds of the reforms recorded in the report were in low- and lower-middle-income economies, and Rwanda was listed as the world's top reformer of business regulation.

An outstanding question is whether some reforms are more important than others. For instance, a significant reform affecting firms starting a business is the introduction of "one-stop-shop business registration centers", which allow entrepreneurs to quickly register their businesses in one step, without requiring visits to courts and tax offices; other reforms might include a reduction in registration fees or minimum capital requirements, without simplification of the registration process.

In this paper, we examine the impact of institutional reforms on the number of new firm registrations. Understanding the regulatory environment that promotes entrepreneurship is necessary to successfully identify appropriate policies to foster new firm growth in local economies. We attempt to quantify and measure the real effect of business environment 'reformers'.

¹ Reports are available on-line at: www.doingbusiness.org.

Specifically, we empirically investigate the magnitude of reform required for significant impact on the number of new registrations. A priori, it is not clear what magnitude of reduction in costs (or other parameters such as days or procedures) is necessary to create a significant impact on firm registration. In other words, what exactly constitutes a reform? Is a 20% reduction in the costs of registration sufficient or is a 50% reduction necessary to induce a statistically significant number of firms to register? This paper is an empirical attempt to address this question.

We use a new dataset that is uniquely suited for this purpose: The *World Bank Group Entrepreneurship Snapshots* (WBGES) – a cross-country, time-series panel dataset on the number of newly registered companies. We supplement this dataset with information from Doing Business reports on the cost, time and number of procedures required for registration of new companies. Importantly, both datasets focus comparably on only limited liability companies.

Our results show that small reforms, in general less than 40%, do not have a significant effect on new firm registration. In addition, reforms in multiple indicators (e.g. cost and time in setting up a business) have a larger impact on business registration and simultaneous reforms have a larger impact than sequential reforms. In addition, we find that a country's initial conditions matter: countries that start out with high registration costs need larger reforms to induce a significant number of new registrations. 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 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.

Our study is motivated by recent literature which shows that entrepreneurship is essential for the continued dynamism of the modern economy and economic growth (Hause and Du Rietz, 1984; Black and Strahan, 2002). Many studies have concluded 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). Moreover, the robust entry rate of new businesses can foster competition and economic growth (Klapper, et al., 2006; Djankov, 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 U.S. is driven by new formal business entry rather than by the growth of existing firms (Brander, et al., 1998; Haltiwanger, 2009). In many developing countries, where there was no significant private sector to start out with, new firms often strengthened reforms by improving economic conditions, as for instance in China (McMillan and Woodruff, 2002).

Previous 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 (Klapper and Love, 2010; Klapper, Amit, and Guillen, 2009). Costly regulations may also impede the setting up of businesses and stand in the way of economic growth (De Soto, 1990; Djankov et al., 2002). Furthermore, countries with higher entry costs have more corruption and larger unofficial economy (Djankov et al., 2002).

It is important for policymakers to understand the impact of the reform process on new firm creation and this paper can also offer empirically based advice to policymakers designing reforms. For instance, insufficiently large reforms may not have the intended impact on firm registrations and money and effort might be poorly spent on the reform. These results can help policymakers to design interventions with the biggest impact on private sector growth.

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

2. Methodology

We begin by suggesting ways to classify reforms to starting a business, based on observed changes in cost, days, procedures or minimal capital requirements for new formally registered businesses (Doing Business, 2010). Next, we investigate empirically the varying impact of various classifications of reforms on new firm registrations. This section presents theoretical intuition to guides our empirical strategy.

Assume there is a benefit to the firm of registering as a formal business (versus continuing to operate 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 governmental sanctions. Suppose the total sum of 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, personnel and managerial time spent dealing with required procedures, and minimum capital requirements that need to be met.² 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, the firms will only chose to register when the total benefits of registration exceed total costs, i.e. $c < b$.

Suppose there is a reform that affects 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 costs are equal to $c0$, and after the reform the total cost is $c1$, which is lower than $c0$ (i.e., $c1 < c0$). The reform will only lead to new formal sector registration when post-reform costs fall below the benefits to registration, i.e. $c1 < b$. The reform might be effective in inducing new business registrations or not effective, depending on the relative magnitude of parameters b , $c0$ and $c1$. Specifically, there are two possible cases, which we refer to as a “small reform” or a “large reform”:

Small reform: $b < c1 < c0$

Large reform: $c1 < b < c0$

In other words, if the reform is “small” the reduction in costs is insufficient to fall below the benefits, and therefore no significant increase in new registrations will be observed. In the case of a “large” reform, the costs fall below the benefits and there will be a significant increase in new registrations. An important assumption in our model is that only the costs change, while 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.

² We ignore the costs of tax payments that firms may incur after registration since we do not have data on tax payments.

Clearly, different firms will have different benefits from 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. Our model will still hold under this continuous case as long as there is a non-uniform distribution of benefits (i.e. if there is some mass around the average benefit, for example as in a normal distribution). In this case the reform will be large if the costs fall below the average benefit. Under this scenario some firms chose to register even under an old regime with high costs, but the increase in registrations will be significantly more pronounced in a large reform than in a small reform.

The impact of a reform will depend on both the relative pre-reform benefits in the reforming country and the relative reduction in costs. For instance, in countries with less developed financial systems, the benefit of access to formal sector financing might be less accessible. In addition, some countries might only reduce registration fees by 20% while other countries might reduce fees by 50%. To summarize, we expect that when starting costs are significantly reduced, the cost of registrations for some firms will move below the expected benefits of formal sector registration, and these firms will chose to register under the new reformed regime. Our empirical tests will identify the magnitude of reforms necessary for a significant number of newly registered firms.

In our empirical tests, we are unable to observe b , but we can measure the change in costs (or other parameters). Because we cannot perform such experiments within a single country, we use cross-country and over time variation in costs to determine what changes in costs result in most significant increase in the number of newly registered firms. Specifically, we measure reforms (the move from $c0$ to $c1$) as the percent change over time in the Doing Business ‘Starting a Business’ parameters. Next, we construct various indicators of ‘reform’ along the percent change continuum, using discrete cutoffs; for instance, countries that reduced costs by more than 20% (‘Reform_20’), countries that reduced costs by more than 30% (‘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 firm registration. We then compare results for

different cutoff points and examine the level of reform that results in the most significant and largest in magnitude estimate of the impact. If the cutoff point is correctly classified (i.e. it is close to the “true” definition of reform in the data), the difference in difference estimation will produce the most significant and largest in magnitude estimates of the impact. However, misclassifying reforms will result in reduced observed impact. Appendix 1 presents an example of how misclassification can happen due to Type I or Type II errors.

In other words, this methodology allows us to identify which cutoff points lead to the largest and most significant increases in formally registered businesses. The closer our chosen cutoff point is to true reform point, the more significant the regression results should be. Thus, we can use the significance and magnitude of the coefficients to point out the true definition of ‘reformer’ as a specific percent change in the underlying variable.

3. Data and Descriptive Statistics

3.1 Data on New Firm Registrations

For our dependent variable, we use data from the *World Bank Entrepreneurship Snapshots* (WBGES) – a cross-country, time-series panel dataset on new firm registrations.³ Data on the number of newly registered firms in 92 countries is collected annually from 2004 to 2009 directly from business registries and other government statistical offices worldwide. The main variable of interest is new business ‘entry density’, defined as the ratio of newly registered limited liability firms per 1,000 working age population (those ages 15-64). The final dataset includes 494 observations from 92 countries over the six-year period 2004 to 2009.

Entry density ranges from 4.21 in high-income countries to less than one in lower-middle and lower income countries. 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. There are also significant disparities across regions, ranging from an entry density of 0.58 in Sub-Saharan African (SSA) countries to 2.26 in Europe and Central Asia (ECA). This translates roughly to an average of 55,000 newly registered limited-liability firms per year in industrialized countries,

³ The complete database and related reports is available at: <http://econ.worldbank.org/research/entrepreneurship>.

relative to about 35,000 in Latin America, 14,000 in South Asia, and 9,000 in Sub-Saharan Africa. Figure 1 shows the distribution of entry density across countries.

3.2 *Identifying Reforms to the Business Environment*

Our measures of reform come from changes in 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 environment. 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, 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 2 shows the distributions of the annual percent change in these four explanatory variables.

We use the annual percent change in these variables to construct dummy variables (0/1) identifying different reform levels (e.g. 20%, 30%, etc.). 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 or capital requirements.

3.3 *Classifying Reforms*

First, we empirically study the impact of different cutoff points that we use for reform classification. For each of the variables of interest we classify a country as a reformer if the percent change in the variable is below the cutoff point. For example, for the variable 'Cost Reform_20', we classify as reformers all countries that reduced costs at least by 20%; all other countries are classified as non-reformers. We get 57 reformers using this definition (Table 3). Clearly, as we increase the cutoff, we ask for a more significant change and the number of

reformers declines to 42 with at least a 30% drop in cost, 28 with a 40% cutoff, 16 with a 50% cutoff, and only 7 with a 60% cutoff. We do not explore larger (i.e. more negative) cutoffs because they produce very insufficient number of reformers.

Table 3 shows similar trends across the different business environment variables; increasing cutoffs result in a lower number of reforming countries. For example, a 50% reduction in the number of days required to start a business occurs in 32 countries in our data; however, when we examine a 50% reduction in the number of procedures, only five countries are classified as reformers. This suggests that countries are more likely to implement large reforms in the number of days, relative to costs and procedures.

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 20% cutoff we have 30 repeated reforms for cost, 31 for days, 11 for procedures and 20 for minimum capital. Clearly as the cutoff becomes more stringent, the number of repeated reforms goes down significantly. Thus, for 50% cutoff we only have one repeated reform for costs, six for days, none for procedures and six for min capital. For the purposes of our regression analysis, we look at the effects of the first reform for each of our definitions, i.e. we consider a country a 'reformer' for all subsequent years following a first reform.

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.

For example, a 20% change in costs or a 20% change in the number of procedures that occur independently (i.e. only one change happens in a country) may not produce a significant impact on number of registered businesses because the changes are too small for new overall costs to fall below the benefits. However, if two changes are combined, in other words, if both costs and procedures are reduced at the same time, the effect of these two simultaneous changes may be significant. Using the intuition of our model, the decline in procedures can be translated into monetary value because of time savings of personnel that will be involved in the registration

process. Thus, the aggregate costs of both 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, it may happen that reforms 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, for a 20% cutoff, a country is not classified as a reformer when the first reform occurs. However, when a second reform occurs, the country is classified as a reformer under our ‘two sequential reforms’ definition. Similarly, for three 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 gives a number of “reformers” classified for each cutoff if any one of the four business environment measures previously discussed has fallen below the cutoff in any given year. We get 79 “reformers” by this definition and a 20% cutoff and only 47 for a 50% cutoff.

In Column 2 we classify country as a reformer if two or more reforms occur in a country. These two reforms could be any of the four measures discussed above and for now we do not make a distinction. In other words it could be reform of days and procedures, or reform of cost and days, or any other combination. We classify a country as a reformer if two reforms occur simultaneously (in the same year) or if they occur sequentially. We get fewer countries classified as reformers in the case of two reforms: 69 with cutoff of 20% and only 22 with cutoff of 50%. 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 51 countries for 20% cutoff and only 14 countries for 50% cutoff.

Column four shows the number of reformers when we change our definition to require that at least three reforms occur in a country, but they can occur simultaneously or sequentially – i.e. the first reform in one year, the second in the next year, and the third reform a year or two later. 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. Thus, for cutoff of 20% we observe more reformers in this column than in column 3. In other words, more countries make reforms in stages than simultaneously. However, as the cutoff decreases we get about the same number of reformers as in the previous column.

In the last column we present the number of countries with three or more reforms occurring simultaneously. The numbers are significantly smaller. With 20% reforms only 28 countries are classified as reformers (relative to 51 countries with two simultaneous reforms). The numbers quickly drop as the cutoffs increase, only 14 countries are identified as reformers with a 30% cutoff and 8 or less with a 40% cutoff. These numbers suggest that despite the fact that changes are correlated (i.e. change in procedures is more likely to be accompanied by change in days and change in costs), it is relatively rare to observe three of these changes occurring simultaneously.

4. Results

4.1 Regression Model

To test which definition of reforms produces the largest change in business registrations, we run the following regressions:

$$\text{Entry Density}_{it} = \alpha_i + \beta \text{Reform}_{it} + \gamma \text{GDPPC}_{it-1} + d_t + \varepsilon_{it} \quad (1)$$

Here, Reform is equal to one for all countries classified as reformer using each of the cutoffs discussed above. This dummy equals to one for the year in which reform has occurred and all years after the reform. In other words, we allow the reform to have a lasting impact on a country, not only in the year of 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. Thus, the ‘Reform_{it}’ term can be written as a combination of two dummy (0/1) variables, ‘Reform country’, which is equal to one for countries identified as reformers, and ‘After reform period’, which is a dummy equal to one for the year of the reform and all subsequent years:

$$\text{Reform}_{it} = \text{Reform Country}_i * \text{After}_t$$

In other words, this framework allows us to use reformer countries as controls in years before they implemented a reform. We allow each country to have its own country-specific error term, α_i , (i.e. country fixed effect) because the level of new registrations varies significantly around the world (see Klapper and Love, 2010). In addition, to make sure the changes in new registrations are due to the specific reforms rather than the business cycle effects, we control for lagged GDP per capita (although our results are not sensitive to this control).

Finally, we include time dummies to control for any global changes in macroeconomic environment that may affect all countries registrations. 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 the crisis year in most of the 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.⁴ 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. We estimate these regressions by OLS with standard errors clustered on a country level to allow for any serial correlation within countries.

⁴ 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.

4.2 *Single Reform Results*

Table 5 contains our regression results. Each cell in the table reports the coefficient of a separate regression; to save space, we only report coefficient on the reform variable (i.e. coefficient β from model 1). We observe that for cost reform to be effective, the costs have to go down at least by 50% relative to the pre-reform level. However, even a 40% drop in the number of days produces significant outcome in terms of registered businesses. The largest impact for costs and number of days is observed for 60% reduction, but in this case only 7 countries are classified as reformers in the case of costs and 23 for number of days (Table 1).

For number of procedures even a small change of 20% reduction produces significant results. However, larger changes produce more significant results, which peak at 40%. In light of our model, for number of procedures the reforms appear to be sufficiently large at a 20%-40% reduction. Therefore, setting a stricter cutoffs (such as 60%), results in more misclassification due to Type I error such as discussed in Appendix 1. 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%.

For minimal capital requirements we find the impact is weaker, perhaps because many countries do not impose minimum capital requirements or because these requirements are not binding. The results are marginally significant (at about 11-12% levels) for about all levels of reductions.

4.3 *Simultaneous and Sequential Reform Results*

Next we investigate the impact of several reforms at the same time, whether occurring simultaneously or sequentially. Table 6 presents our results. The first column presents the single reform case for comparison. In this 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 4, 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 50% or 60% cutoff. Thus, a country must change one of the four measures by at least 50% to result in a statistically significant increase in registrations; such a large change can be safely classified as a reform.

Column two presents results for two reforms that can occur either in the same years or in subsequent years (i.e. sequentially). We observe that in this case even a 40% 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 three), although the magnitudes of the coefficients are slightly larger for simultaneous reforms. For example, for a 40% cutoff the coefficient on simultaneous reforms is 0.613, while the coefficient on sequential reforms is 0.525. Even larger differences are observed for 50% cutoff: 0.617 with two sequential reforms and 0.78 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% 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, for 30% cutoffs the three sequential reforms produce a change in 0.346 in entry density, while three simultaneous reforms produce a change of about 0.6, almost twice as large. The results for 60% change are ignored since there are only 2 countries classified as reformers using this definition.

An important caveat to these results is that multiple reforms – either sequentially or simultaneously – might suggest that these reforms are part of a larger private sector reform package. In other words, we cannot assess with absolute certainty that the increase in new firm registrations is exclusively the result of these specific reforms and not a more general improvement in the business environment. However, even in such circumstances, our results

suggest that very small changes (less than 30%) are unlikely to increase formal sector participation.

4.4 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 environment. There are two alternative hypotheses: First hypothesis is that in weak business environments firms need larger changes to motivate them to register. For example, in countries with very high costs or requiring many procedures, small reforms (such as 20% or 30% reductions) will be insufficient to induce significant business registrations. The alternative hypothesis might also hold if in weak countries even a small change will be welcomed by the severely constrained firms. More formally, the outcome of the reform depends on the distributions of costs and benefits, as discussed earlier. If in a country with very 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 terms of our model, in this case, the benefit is just below the pre-reform costs.

Alternatively, countries with higher registration costs might also offer fewer benefits to formal firms, so that only a large change in costs will induce a significant number of new registrations. In terms of our model, this hypothesis suggests that in countries with high initial costs, the average benefits are significantly below the costs.

To investigate these alternative hypotheses we add the interaction of the pre-reform level of the business environment to our model.

$$\text{Entry Density}_{it} = \alpha_i + \beta_1 \text{Reform}_{it} + \beta_2 \text{Reform}_{it} * \text{Pre-reform_level}_i + \gamma \text{GDPPC}_{it} + d_t + \varepsilon_{it} \quad (2)$$

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 reform has more or less significant impact with different pre-reform levels.

Table 7 presents the results. 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 all the measures smaller reforms are less significant in countries with high pre-reform levels. For example, for the cost variable, a 50% change in costs is less significant in countries with high pre-reform cost levels. However, a reform of 60% is almost equally effective in countries with high pre-reform levels (i.e. the interaction is not significant at conventional level, but is still negative).

For a reform reducing the number of days required to register a business we observe a similar pattern at 40% change – the higher the number of pre-reform days, the smaller the benefit of the 40% change. However, the interaction becomes not significant at conventional level for 50% reform, again suggesting that larger reforms are more effective in countries with high pre-reform levels. For procedures this impact is even clearer: at a 20% change, the impact is significantly reduced with higher pre-reform number of procedures – the interaction coefficient is -0.145 and significant at 10%. However, with a 30% change in procedures, the interaction is much smaller, -0.084, and not significant at conventional levels (p-value of 0.145). The 40% change in procedures is effective no matter the initial conditions as the interaction is not significant. The results for minimum capital suggest that any reform is less effective in countries with higher minimum capital.

Overall, these results suggest that in countries with very high costs, long delays or a large number of procedures necessary to register a business, the benefits of registrations are significantly lower than the existing costs for a large number of firms. Therefore, large changes (40% or more) are required in order for firms to choose formal registration. Simply speaking, in countries with very weak business environments, small reforms are not sufficient to motivate significant changes in the number of new firm registrations.

5. Conclusion

In this paper we offer an empirically based quantitative way to identify business environment ‘reformers’. Our results show that small reforms, in general less than a 40% reduction in costs, days or number of procedures, 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 might be 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 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 private sector development. Our results can also be helpful to motivate policymakers to make larger, and more difficult, reforms.

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Table 1. Variable Definitions

Variable	Description
density_new	Entry density, defined as new firms registered per working age population (normalized by 1,000) (<i>World Bank Entrepreneurship Snapshots, 2010</i>)
L.log_gdppc	Lagged log GDP per capita (<i>WB-WD, 2010</i>)
esb_cost_ch	Annual % change in cost of starting a business (% of income) (<i>Doing Business</i>)
esb_days_ch	Annual % change in days to start a business (<i>Doing Business</i>)
esb_proc_ch	Annual % change in procedures to start a business (<i>Doing Business</i>)
esb_mincap_ch	Annual % change in minimum capital requirement to start a business (<i>Doing Business</i>)
topref	Classified as one of ten Top Reformers by <i>Doing Business</i> (<i>Doing Business</i>)
esb_ref	Classified as a Starting a Business Reformer by <i>Doing Business</i> (<i>Doing Business</i>)

Table 2. Summary Statistics

Variable	N	Mean	SD	p50	Min	Max
density_new	494	2.03	2.11	1.16	0.00	9.81
L.log_gdppc	494	7.91	1.51	7.82	4.82	10.65
esb_cost	494	34.12	59.50	12.00	0.10	480.10
esb_cost_ch	494	-11.19	18.54	-8.91	-98.82	73.95
esb_days	494	36.43	64.64	26.00	2.00	694.00
esb_days_ch	494	-9.97	20.72	0.00	-86.21	66.67
esb_proc	494	8.76	3.48	9.00	1.00	18.00
esb_proc_ch	494	-4.86	11.89	0.00	-75.00	25.00
esb_mincap	494	87.71	207.42	15.50	0.10	1821.90
esb_mincap_ch	494	-10.06	27.40	-5.54	-99.97	283.33
topref	494	0.08	0.28	0.00	0.00	1.00
esb_ref	494	0.32	0.47	0.00	0.00	1.00

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

Cutoff	1 Cost reform	2 Days reform	3 Proc reform	4 Min Cap reform
20%	57	58	40	40
30%	42	50	28	29
40%	28	43	16	24
50%	16	32	5	23
60%	7	23	3	22

Table 4. Number of Simultaneous or Sequential Reforms, by Country

Cutoff	1 One or more reform	2 Two or more reforms occurring sequentially	3 Two or more reforms occurring simultaneously	4 Three or more reforms occurring sequentially	5 Three or more reforms occurring simultaneously
20%	79	69	51	53	28
30%	70	51	35	30	14
40%	60	34	22	17	8
50%	47	22	14	12	4
60%	37	17	8	6	2

Table 5. Regression Results for Single Reform Variable

This table uses an unbalanced panel dataset of 494 observations from 92 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 log GDP per capita. Standard errors are clustered at the country-level. P-values are in squared brackets. ***, **, * and ^a represent significance at 1%, 5%, 10% and 15% level respectively.

Cutoff	1 Cost reform	2 Days reform	3 Proc reform	4 Min Cap reform
20%	-0.014 [0.912]	0.006 [0.960]	0.313* [0.073]	0.296 ^a [0.118]
30%	-0.026 [0.856]	0.132 [0.413]	0.406** [0.042]	0.219 [0.260]
40%	0.087 [0.627]	0.300** [0.038]	0.419** [0.016]	0.378 ^a [0.103]
50%	0.600** [0.038]	0.427** [0.021]	0.417* [0.089]	0.378 ^a [0.103]
60%	0.637*** [0.005]	0.581** [0.018]	0.2 [0.266]	0.385 ^a [0.116]

Table 6. Regression Results for Simultaneous or Sequential Reforms

This table uses an unbalanced panel dataset of 494 observations from 92 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 log GDP per capita. Standard errors are clustered at the country-level. P-values are in squared brackets. ***, **, * and ^a represent significance at 1%, 5%, 10% and 15% level respectively.

Cutoff	1 One or more reform	2 Two or more reforms occurring sequentially	3 Two or more reforms occurring simultaneously	4 Three or more reforms occurring sequentially	5 Three or more reforms occurring simultaneously
20%	-0.008 [0.947]	0.145 [0.202]	0.195 [0.243]	0.032 [0.814]	0.154 [0.551]
30%	-0.076 [0.510]	0.166 [0.204]	0.237 [0.263]	0.346* [0.054]	0.609* [0.079]
40%	-0.011 [0.935]	0.525*** [0.000]	0.613*** [0.010]	0.624** [0.020]	0.944* [0.052]
50%	0.321** [0.024]	0.617*** [0.003]	0.780** [0.025]	0.617* [0.071]	0.991 [0.204]
60%	0.413** [0.023]	0.615** [0.011]	0.988** [0.034]	0.591*** [0.000]	N/A

p-values are in squared brackets. ***, **, * and ^a represent significance at 1%, 5%, 10% and 15% level respectively.

Table 7. Regression Results for Single Reform Variable

This table uses an unbalanced panel dataset of 494 observations from 92 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 variable level. Each box represents a separate regression. All models include country and year fixed effects and control for lagged log GDP per capita. Standard errors are clustered at the country-level. P-values are in squared brackets. ***, **, * and ^a represent significance at 1%, 5%, 10% and 15% level respectively.

Cutoff	Variable	1 Cost Reform	2 Days Reform	3 Proc Reform	4 Min Cap Reform
20%	Reform	0.029 [0.849]	-0.021 [0.934]	1.668** [0.025]	0.412* [0.065]
	Reform*pre-reform level	-0.001 [0.201]	0.001 [0.878]	-0.145* [0.056]	-0.001** [0.020]
30%	Reform	0.022 [0.906]	0.238 [0.421]	1.179* [0.076]	0.293 [0.244]
	Reform*pre-reform level	-0.001 [0.326]	-0.002 [0.567]	-0.084 ^a [0.145]	0 [0.345]
40%	Reform	0.14 [0.505]	0.596*** [0.009]	0.599* [0.087]	0.586* [0.061]
	Reform*pre-reform level	-0.001 [0.170]	-0.007* [0.098]	-0.022 [0.580]	-0.001* [0.083]
50%	Reform	0.788** [0.015]	0.759*** [0.008]	0.549 [0.424]	0.586* [0.061]
	Reform*pre-reform level	-0.003*** [0.007]	-0.007 ^a [0.124]	-0.016 [0.869]	-0.001* [0.083]
60%	Reform	0.820*** [0.003]	1.022 [0.170]	0.684* [0.078]	0.616* [0.068]
	Reform*pre-reform level	-0.005 [0.162]	-0.013 [0.470]	-0.059 [0.171]	-0.001* [0.085]

p-values are in squared brackets. ***, **, * and ^a represent significance at 1%, 5%, 10% and 15% level respectively.

Figure 1. Distribution of Entry Density

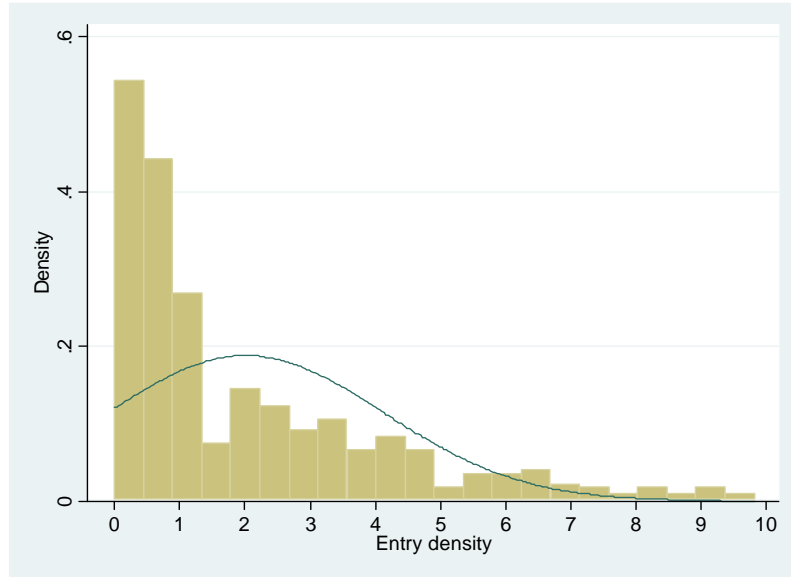
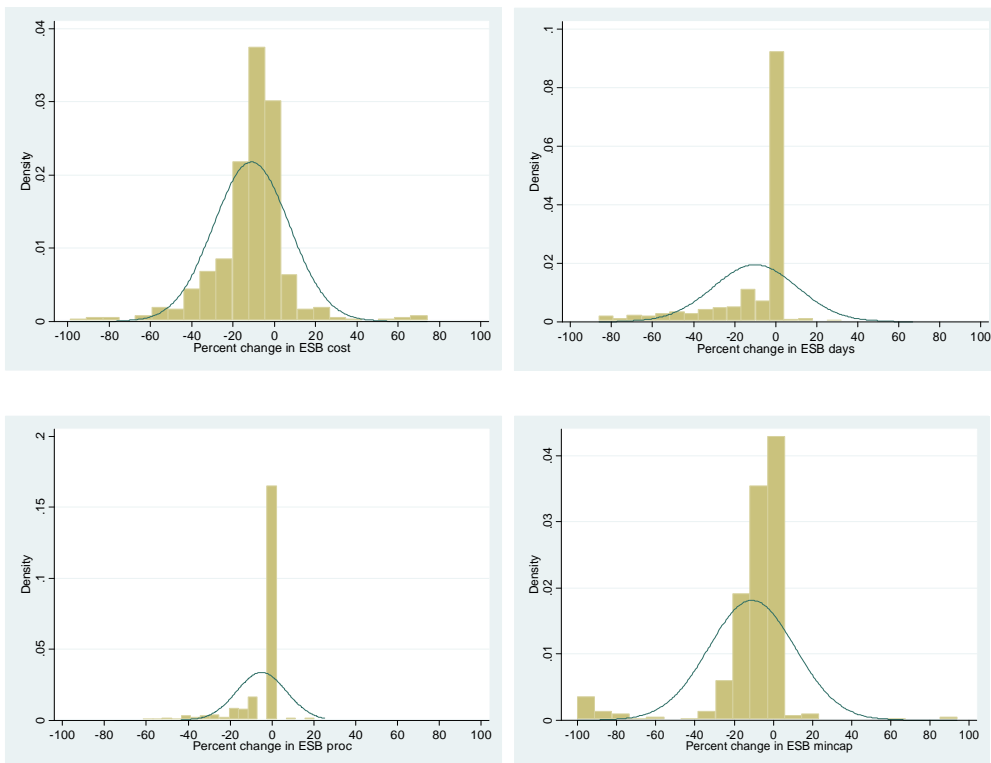


Figure 2. Histograms of Percent Changes in Business Environment Variables



Appendix 1. Numerical Example of Misclassification Errors

Suppose, a true reform is a reduction in the cost of registration of 40%. In other words, this reduction leads to the largest number of new business registration on average, across all countries in our sample. But without knowing what the correct cutoff is, we could erroneously misclassify a reform. Suppose we set a cutoff to 30% and all countries that experienced a change of 30% or more cost reduction are classified as reformers (i.e. treated sample) and all others are not-reformers (controls). However, a change between 30% and 40% is not a true reform in a sense that a significant number of new businesses will not register under new regime, relative to the old regime. This is similar to making a Type II error – we classify a country as a reformer when indeed it is not. This is represented by a shaded portion of the distribution in Figure A1, left panel. Thus, our treated sample is contaminated by a number of countries that have no observable increase in registrations. Therefore, our statistical results will be biased downward, and unlikely to register a significant impact of this change on the outcome.

The opposite situation can happen when we set the cutoff at a 50% reduction, when in reality 40% is sufficient for a reform (i.e. sufficient to induce a significant number of new business registrations). In this case we classify all countries with less than 50% reduction on costs as non-reformers. Therefore, our sample of “non-reformers” (i.e. controls) contains a number of true reformers incorrectly classified (i.e. all countries with reforms in between 40% and 50% reduction are incorrectly classified as non-reformers). This is similar to a Type I error – the true reformers are misclassified as non-reformers. This is represented by the shaded portion of the distribution in Figure A1, right panel. Similarly, this will also bias results downward (because the control sample contains some of the treated firms).

The closer our chosen cutoff point is to the true reform point, the more significant the regression results should be. Thus, we can use the significance and magnitude of the coefficients to point out the true definition of reform as a specific percent change in the underlying variable.

Figure A1. Classification Errors in Reform Definition

