

# REFORMS IN BUSINESS REGULATION: EVIDENCE FROM RUSSIA

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## Abstract

What determines the enforcement of reform of business regulation? What are the outcomes of such reform? We address these questions using an episode of a drastic reform in Russia between 2001 and 2004 which liberalized registration, licensing, and inspections. Based on the analysis of micro-level panel data on regulatory burden, we find that: 1) On average, the reform reduced the administrative costs of firms; but, the progress of reform had a substantial geographical variation. 2) The enforcement of deregulation reform was better in regions with a transparent government, low corruption, better access of the public to independent media sources, a powerful industrial lobby, and stronger fiscal autonomy. 3) Using the exogenous variation in regulation generated by the interaction of reform and its institutional determinants, we find a substantial positive effect of regulatory reform on net entry into the official sector and small business employment and no effect on pollution and public health. The results support public choice theory of the nature of regulation and are inconsistent with the predictions of public interest theory.

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In the recent years, reforms of business regulations have become very popular among policymakers all over the world. For example, in 2005-2007, sixty two countries undertook reforms to cut administrative costs of starting a business and getting a license (World Bank, 2006, p. 4; 2007, p. 4). How are the reforms of business regulation implemented and what are their effects? Theoretical answer to these questions has been sought after in the longstanding debate on the nature of regulation (Pigou, 1938; Buchanan and Tullock, 1962; Coase, 1960; Stigler, 1971; de Soto, 1990). Only recently, however, motivated by new data sources and different experiences throughout the world, the debate has come to the attention of empirical literature (see Djankov, 2008, for a survey). So far, the literature focused exclusively on the effects of changes in *de jure* regulations. Since the reforms may not be perfectly enforced, the level of enforcement becomes an important missing link between a reform on the books and its outcomes. Despite its relevance both for policy and theory of regulation, previous research had little to say about the obstacles and driving forces behind the enforcement of reforms of business regulation. In this paper we utilize this (previously missing) link. As the first step, we study the determinants of enforcement of a federal regulatory reform in regions of a large federation Russia, and then, use the predicted variation in enforcement to identify the outcomes of reform.

We use a regulatory reform policy experiment undertaken in Russia in the early 2000s. There are several important advantages to using Russian data. First, there is a unique micro-level *panel* data on the *actual* regulatory burden on firms, which allows measuring the enforcement of reforms. Second, the data spans a selection of 20 regions, which allows observing different pace of reforms in different regions. Third, Russia's regions are relatively homogenous in culture, but very diverse in governance; which allows us to study the regional determinants of reform progress.

Between 2001 and 2004, Russia undertook a drastic reform of business regulation. A series of federal laws that drastically simplified procedures and reduced the red tape associated with the entry regulation (registration and licensing) and with the regulation of existing

business (inspections). The laws introduced clear measurable limits to the regulatory burden in several specific regulatory areas. In particular, the new laws require that registering a business involves a visit to just one government agency (“one-stop shop”) and takes no more than a week; each inspecting agency (e.g., fire, sanitary, labor, or certification inspection) comes to inspect a business no more than once in two years; licenses are valid for five years. In addition, a substantial delicensing took place, i.e., a number of business activities which previously had required licenses were exempt from it.

Prior to the reform, many scholars pointed to the excessive regulatory burden on Russian firms and argued that over-regulation was among the most important reasons for Russia’s poor economic performance during the first eight years of transition.<sup>1</sup> The proclaimed goal of the reform was to increase entry and growth of small business.

This paper addresses three questions about this reform. First, did the reform succeed in bringing down administrative costs of firms? Second, which institutional factors affected the enforcement of reform in different regions? Finally, what is the causal effect of *de facto* regulation on outcomes, i.e., entry, small business employment, public health, and pollution?

We use a unique data set entitled “Monitoring of Administrative Barriers on Small Business” (MABS). The data come from regularly-repeated surveys of 2,000 firms in 20 Russian regions with questions about firms’ actual levels of regulatory burden in each area of regulation affected by the reform. Firm-level panel data are collected to measure the dynamics of regulatory burden on existing firms; a repeated cross-section of newly-registered firms is collected to measure changes in the regulation of entry. The data allow observing directly the level of enforcement of each measurable target in the deregulation laws.

First, we investigate whether the *de jure* reform had an effect on *de facto* regulations using the difference in timing of enactment of laws on registration, licensing, and inspections. We estimate the average impact of the enactment of a deregulation law on the regulatory burden in the specific area of regulation affected by this law with difference-in-differences

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<sup>1</sup>See, for instance, Frye and Shleifer (1997); Shleifer (1997); Johnson, Kaufmann and Shleifer (1998); Shleifer and Vishny (1998); Frye and Zhuravskaya (2000).

methodology. The assumption underlying this empirical methodology is that in the absence of reform, a specific trend in regulatory burden followed by each of the different areas of regulations would have continued without any change at the time when the reform actually took place. We control for all time-invariant regional and firm characteristics, linear trends in the level of regulatory burden specific to each dimension of regulation, and macro-economic shocks. We find that, on average, the enactment of a deregulation law leads to a significant reduction in regulatory burden. Figure 1 presents the timing of the reform and of survey rounds. Figure 2 illustrates the level of regulatory burden and the compliance with the targets set by the reform before and after the reform for all regions on average and for the Samara region, which had relatively successful reform implementation. The Figure 2 demonstrates that some of the regulations exhibit diminishing trends, and therefore, it is essential to control for these trends in order to estimate the average impact of reform.

As a second step, we study determinants of the enforcement of the reform. We explore the fact that the dynamics of regulatory burden in each area of regulation exhibits a vast geographical variation, as shown in Figure A.1 in appendix. The Figure presents regional dynamics of the regulatory burden in five specific regulatory areas covered by the reform. We link the variation in reform progress with the variation in regional political accountability, the strength of local industrial lobbies, and local fiscal incentives. We estimate the differential impact of the federal laws on regulatory burden depending on the pre-reform regional institutional environment using difference-in-differences methodology. This identification strategy is valid under the assumption that, in the absence of institutional variation, the average change in regulatory burden induced by a specific deregulation law would have been the same across regions. The following measures of regional institutional environment are associated with significantly better enforcement of deregulation laws (holding everything else constant): 1) government transparency; 2) control over corruption; 3) internet penetration and other measures of the access of the public to independent media sources; 4) industrial concentration; and 5) the share of own revenues in the regional budget. We find

that these institutional characteristics affect the liberalization of entry and the liberalization of regulations of established (incumbent) firms in the same way.

Finally, we use the interaction between reform timing and the institutional determinants of reform enforcement as an exogenous source of variation in the level of regulation to test for a causal link from *de facto* regulation to such outcomes as entry to the official sector, official small business employment, pollution, and public health. Instrumenting regulation is important because of reverse causality going from outcomes to regulation. Using 2SLS, we find a significant negative effect of licensing and inspections on the official number of small businesses (a proxy for net entry) and of the number of registration agencies and inspections on the official employment in small businesses. The changes in the number of small businesses and small business employment reflect the true business formation as well as the shift of business activity between the official and unofficial sectors; both of these processes have an important first order effects on the economy (Johnson, Kaufmann and Shleifer, 1998; Johnson et al., 2000). We also find that there is no robust effect of regulation on either pollution (measured by contaminants' emission from stationary sources into the atmosphere) or public health (measured by morbidity from injuries and poisoning per 1,000 people).

We use this evidence to evaluate the two competing theories of the nature of regulation: the public interest theory (Pigou, 1938) and the public choice theory (Tullock, 1967; Stigler, 1971; Peltzman, 1976). Our results are inconsistent with the public interest theory and fully consistent with the public choice theory as regions with transparent and accountable authorities and with the independent sources of information for the public, such as internet and independent newspapers, achieve better reform progress. Furthermore, liberalization of business regulation does not have an adverse effect on pollution or public health despite the increase in the number of small businesses and their employment.<sup>2</sup>

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<sup>2</sup>Our results also shed light on the theory of institutional change in transition economies. Consistent with the "demand for reform" theory of the determinants of reform progress (Boycko, Shleifer and Vishny, 1995) which implies that privatization creates a political force in favor of continuation of structural reforms, we find that strong industrial lobbies in the Russian regions facilitated progress in the reform implementation.

Our main contribution is to the burgeoning literature on the effects of regulation (see Djankov, 2008, for a recent survey). A distinguishing feature of our work compared to all previous studies is that we compare the changes of the legislation to the changes in the actual regulatory burden and demonstrate that the official regulations are poorly enforced and grossly understate the actual regulatory burden. A large body of literature starting with the pioneering work of Djankov et al. (2002) estimates the effects of various regulations across countries (e.g., Djankov et al., 2003; Botero et al., 2004; Mulligan and Shleifer, 2004, 2005; Klapper, Laeven and Rajan, 2006; Djankov, McLiesh and Ramalho, 2006). Empirical problems with interpreting cross-country correlations as causal lead to the development of a smaller but fast-growing collection of papers on the analysis of within-country policy changes (e.g., Aghion et al., 2005, 2006; Monteiro and Assuncao, 2006; Bruhn, 2007; Kaplan, Piedra and Seira, 2007; Chari, 2007). Our contribution is to this strand of the literature. Our paper is most closely related to Aghion et al. (2006); the two papers study complementary channels through which local institutions affect the outcomes of a nationwide regulatory reform. Our results confirm that there is a vast variation in regulatory burden within a country and looking only at the largest city (as in Djankov et al., 2002, and related work) may give a misleading picture about the state of regulation in the country as a whole. Furthermore, panel data allow us to control for unobserved regional and firm-level variation as well as time trends and, therefore, substantially improve on the cross-sectional analysis of many previous studies.

The paper is organized as follows. In Section 1, we describe the reform and the regulations data. In Section 2, we present hypotheses about the institutional determinants of reform progress and describe institutional measures. Section 3 focuses on the estimation of the effect of reform on the actual regulatory burden and the institutional determinants of reform progress. Section 4 reports the estimates of the effect of regulations on outcomes. Section 5 discusses robustness. Section 6 concludes.

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Using the logic of Grossman and Helpman (1994) we use the variation in regional industrial concentration as a proxy for the variation in the strength of regional industrial lobbies.

# 1 Background and the measures of regulation

## 1.1 The reform

The level of regulatory burden prior to the Russia's reform was extremely high. The goal of the reform was to speed up and simplify administrative procedures, reduce red tape, and, ultimately, to cut costs of firms associated with inspections, licensing, registration, and certification. The reform consisted of a package of laws passed during 2001-2004. Five different laws have come into force at different points in time: the law on inspections – on August 8, 2001; the law on licensing – on February 11, 2002; the first version of the law on registration – on July 1, 2002; the law on certification (“technical regulation”) – on July 1, 2003; and the second version of the law on registration – on January 1, 2004.

These laws (with the exception of the law on certification) introduced clear measurable targets for the maximum level of regulatory burden associated with these areas of regulation (e.g., Buev, Makarova and Shehovtsov, 2005; Shehovtsov et al., 2005). The law on inspections postulated that each inspecting agency is allowed to conduct a maximum of one regular (or “planned”) inspection in each particular firm in two years. If during the inspection no violation is found, the next visit can take place no earlier than in two years. If violations are found, they need to be officially recorded by the inspectors, official fine should be levied on the firm, and inspectors can come once again to record the correction of the violation. The previous legislation did not put a limit to the number of “planned” visits by inspectors. Thus, before the new law took force, inspectors came to visit firms very often, they rarely officially recorded violations, but instead extracted unofficial payments from businessmen not requiring them to correct violations. The law on licensing reduced the list of business activities which require licenses from 250 to 103 activities. For example, the following business activities were exempt from licensing in 2002: realtors, pawn shops, publishing houses, audio studios, private certification firms, antique shops, construction firms, bread making, wholesale and retail of bread, drilling and drill manufacturing, service work in sea ports. In

addition, the law on licenses increased the minimum length of license validity from three to five years. The first version of the law on registration introduced the maximum of five working days during which any firm with all necessary documents should receive registration from the authorities (previously, the length of registration procedure was not restricted by law). The second version of the law introduced a “one-stop shop” rule for registration and formalized the list of required documents for registration. Previously, any start-up had to register with several different agencies. In addition to registration in the local branch of tax ministry, entrepreneur had to make a visit to and receive permissive documents from the pension fund, the social security, the statistical and fire department, local administration, and the rules for registration differed across localities. According to the new (2004) version of the law, all of the registration is done at a local branch of tax ministry. And a firm can just notify the rest of the agencies of its existence. We focus on these measurable targets of reform in registration, licensing, and inspections.<sup>3</sup>

## 1.2 MABS survey

Jointly with a team of experts from an independent Moscow think tank – the Center for Economic and Financial Research (CEFIR, [www.cefir.org](http://www.cefir.org)) – we conducted a long-term project of the Monitoring of Administrative Barriers to Small business (MABS). The project collected data on regulatory burden on Russian firms allowing evaluation of the progress of reforms in a selection of Russia’s regions. The MABS is based on regularly repeated surveys

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<sup>3</sup>In addition to the laws described above, the law on simplified tax system for small businesses was passed on January 1, 2003. This law introduced two changes into Russian taxation system: (1) it significantly increased the scope of application of the existing system of simplified tax administration which allows small firms to pay a single “unified” tax with a flat rate on either profit or revenue instead of many taxes, i.e., VAT, profit, sales, and property taxes; and (2) the new law reduced the tax rate for the “unified” tax. For the vast majority of small firms—and, thus, for firms in our data set—the law on simplified tax system changed the tax rate but did not affect tax administration because they already were eligible to use the “unified” tax. We abstract from laws on certification and on simplified tax system because the law on certification did not introduce clear measurable benchmarks, and therefore, one cannot directly observe whether it is enforced; and the law on simplified tax system did not affect tax administration for the vast majority of firms in our sample. It is important to note that the timing of the laws on certification and on simplified tax system is such that they are not a confounding factor to the deregulation laws that we consider. Both of them were enacted between rounds 3 and 4 of the MABS survey; none of the considered deregulation laws were enacted during this period.



of top managers in 2,000 small firms in 20 regions of Russia. During face-to-face interviews, top managers of firms are asked questions about firms' actual quantifiable costs, associated with inspections, licensing, registration, certification, and tax administration, as well as their subjective perceptions of the business climate.<sup>4</sup> Two primary survey instruments are used: one inquires about the regulatory burden on firms established more than a year ago and the other is designed for the newly registered start-ups in order to monitor the administrative costs of entry. Panel data are collected to monitor administrative burden on existing firms which comes from inspections and continuation licenses and a repeated cross-section is collected to monitor costs of registration and start-up licenses. New start-ups constitute about 20% of the total sample in each MABS round. The samples were constructed separately in each region: the sample of established firms was drawn at random from the census of regional small and medium-size businesses and the sample of startups was drawn at random from the official list of firms registered in the region the last half a year.

The data set includes the results of all six rounds of the MABS survey conducted in the spring and the fall of 2002, the spring of 2003, 2004 and 2005, and the fall of 2006.<sup>5</sup> Each round collected information about all aspects of the regulatory burden on firms for the immediately preceding six months and, in addition, about the inspections for the six-month period before that (e.g., the fifth round took place in the spring of 2005 and collected all variables for the second half of 2004 and a few variables on inspections for the first half of 2004).

Figure 1 presents the timing of different stages of reform and the periods covered by the MABS data. The first round of the MABS survey collected the baseline information from the time before any of the deregulation laws came into force. The data from the second round onwards allow evaluation of the reform progress after the enactment of the law on inspections; the data from the third round onwards enable an assessment of the effect of

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<sup>4</sup>In this paper, we focus exclusively on the *objective* data on the regulatory burden because, apart from being affected by reform, the subjective perceptions are influenced by many unobserved factors.

<sup>5</sup>See CEFIR reports on MABS results at [www.cefir.org/index.php?l=eng&id=25](http://www.cefir.org/index.php?l=eng&id=25).

the licensing law and the first version of the law on registration. The last two rounds allow evaluation of the impact of the second version of the registration law.<sup>6</sup>

### 1.2.1 The measures of regulation

Table 1 lists all the regulatory measures used in this paper. For every firm in the sample at each point in time, we measure the level of regulatory burden in each specific regulatory area affected by the reform and record whether it meets the target set by the reform.

In particular, for registration, we look at the log number of agencies a startup firm visited in order to register, the log number of days the registration took, a dummy for more than one “window” for registration (i.e., visits to several agencies as opposed to a one-stop-shop registration), and a dummy for more than a week for registration. The two dummies measure the failure to meet the respective reform targets.

For inspections, we look at the log number of sanitary inspections over six months and the respective violation of the deregulation target: a dummy indicating whether there was more than one sanitary inspection in six-month period.<sup>7</sup> We focus on sanitary inspection because it is one of the most frequent inspectors of firms in our sample.

To describe the measures of regulatory burden in the area of licensing, let us first define the terms. We call a license “*legitimate*” if it is issued for a business activity that is supposed

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<sup>6</sup>All of the MABS data are in half-year increments. The enactment of the two laws on registration fell exactly between the MABS rounds: the first version of the registration law was enacted between rounds 2 and 3; and the second version – between rounds 4 and 5. This is not the case for the laws on licensing and inspections. In the empirical analysis, we assume that the law on inspections took force after the round 1 and before round 2, even though in reality the law took force in the *middle* of round 1. Similarly, we assume that the law on licenses took force between rounds 2 and 3 (rather than in the middle of round 2). This is done for two reasons: first, one should expect at least a few months lag between the enactment of the law and its implementation; and second, during the half a year period when each of these laws were enacted, inspectors and license authorities may have deliberately shifted their activities earlier in the respective half-year periods in order to avoid the need to comply with the new laws. The results are robust to making an alternative assumption about the timing; it, however, requires the use of retrospective data, which are subject to recall bias.

<sup>7</sup>The dummy picks out only the extreme violations of the deregulation target, because the law limits the number of inspections to one in *two years*, whereas we look at the situations with two or more inspections in a firm during *six months* in order to avoid residual autocorrelation. These extreme violations are not rare: in 2001, 12% of all firms had more than one sanitary inspection in six months; the situation improved by 2006 (five years after the law took force), but the rate of violations of this deregulation target remained non-trivial: 6.4% of firms.

to be licensed according to the 2002 deregulation law on licenses. In turn, we call a license “*illegitimate*” if it is granted for an activity that is not supposed to be licensed according to this law.<sup>8</sup> We consider the following measures of licensing regulations for each firm: the log number of illegitimate licenses; minus log term of validity of legitimate licenses; a dummy for the presence of an illegitimate license; and a dummy for less than 5-year-term of license validity. Again, the dummies indicate the failure to meet reform targets.

Summary statistics for the measures of regulation are reported in the Panel A of Table A.1 in the appendix. The means of variables measuring regulation level for each region (without taking logarithms) are presented in Figure A.1 in appendix.<sup>9</sup>

## 2 Hypotheses about the enforcement of deregulation

In this section, we formulate hypotheses of the two alternative theories of the nature of regulations, the public choice and public interest, about the institutional determinants of progress in liberalizing regulation.

We consider three institutional factors: accountability of local governments to the public (measured by government transparency, control over corruption, and access to independent media sources), the strength of local industrial lobby (measured by industrial concentration and state capture indices), and the strength of fiscal incentives of regional governments (measured by the share of own tax revenue in the regional budget). We proceed by formulating predictions of the two theories about the effects of political accountability and the strength

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<sup>8</sup>For example, if a realtor firm applied for and was granted a licence to operate after 2002, we record a violation of the law and call this licence illegitimate. The data show that many firms applied for and were granted licenses for the activities that do not require licenses according to the new licensing law after it took force. In focus group interviews, firm managers said that it is cheaper for them to pay for the illegitimate licenses than go to defend their right to operate without a license in court. Most illegitimate licenses have been granted by regional authorities.

<sup>9</sup>Not all the data points are available for all regions and rounds. In particular, there are no data on newly-registered firms in the round 4 for 11 out of 20 regions. The reason was the resignation of the Russia’s cabinet of ministers leading to the situation in which nobody in the government knew where the data on the registration of firms were located; these data were needed for sampling of new firms in the round 4 of MABS. In addition, there are no data for Altai Krai in the 3rd round due to a reorganization of the regional survey agency, which was supposed to conduct the survey.

of industrial lobbies on the progress in liberalizing regulation; the predictions of the two theories about the effect of these institutions differ. Then, we discuss the same prediction of both theories about the effect of fiscal incentives.

### **Predictions of the public choice theory**

The public choice theory states that opportunistic bureaucrats create welfare-reducing regulations (e.g., Tullock, 1967). The bureaucrats' motivation is twofold. First, the "tollbooth" view of regulations implies that excessive regulations allow bureaucrats to collect rents for themselves by collecting bribes in exchange for avoiding regulations (McChesney, 1987; de Soto, 1990; Shleifer and Vishny, 1993). Second, the regulatory capture view implies that bureaucrats act as agents of industry incumbents who use regulation as protection from competition of potential entrants (Stigler, 1971; Posner, 1974; Peltzman, 1976).

The public choice theory of regulation unambiguously predicts that more accountable (i.e., more transparent, less corrupt, and better monitored by independent media) regional governments should achieve better progress in liberalizing regulation.

The public choice theory, however, has different predictions for the effect of the strength of industrial lobbies on the reform progress of depending on whether bureaucrats collect rents for themselves or are captured by industry incumbents; and in the latter case, the prediction depends on the nature of competition and whether one considers entry regulations or regulations of existing firms. Under the "tollbooth" view, all businesses including the politically powerful and organized suffer from regulations. Since the politically-powerful and better organized businesses are better positioned to lobby for better reform implementation, one should expect better progress in liberalization of regulation in regions with strong industrial lobby.

Under the regulatory capture view, the prediction is less straightforward. The Russian reform affected entry regulations which can plausibly deter entry only of small firms, such as excessive red tape in registration and licensing. If small entrants are in competition with

politically powerful firms in product or labor markets, one would expect the presence of strong lobbies to have an adverse effect on the implementation of reform in the area of entry regulations, i.e., registration and startup licensing and a beneficial effect on the implementation of reform of regulations that affect incumbents, such as inspections and continuation licenses. Alternatively, small potential entrants and members of powerful industrial lobbies may not compete because they produce different goods and services; and because large firms are interested in shedding excess labor which is less politically costly with growing small enterprize sector. In this case, strong industrial lobbies should facilitate reform in all areas including the regulation of entry. The conditions of this latter case are more plausible for Russia.

### **Predictions of the public interest theory**

The public interest theory's basic premise is benevolent government which sets regulation to correct market failures (Pigou, 1938). The mere presence of reform of business regulation is hard to reconcile with the public interest theory. The reason is that the reform puts constraints on bureaucrats so that they cannot increase regulatory burden (e.g., to inspect a firm more than twice in two years). If bureaucrats are publicly-motivated, there is no need to place constraints on them. If market failures go down, benevolent local bureaucrats lower the level of regulation accordingly without a need for a federal law. Thus, the federal deregulation reform may arise in two cases. It can happen when local governments are benevolent while the federal government serves some special interest. Or, alternatively, it could happen when the changes in the federal legislation are only a reflection of the reduction in market failure that would have lead to a reduction in local regulatory burden irrespective of the legal change. In either case, the public interest theory predicts that the progress of reform should not depend on government transparency or the access of the public to independent sources of information. Since publicly-motivated bureaucrats do not care for special interests, the presence of strong industrial lobby also should not have an effect on

reform progress.<sup>10</sup>

### **Prediction of both theories about fiscal incentives**

Both theories of the nature of regulation predict that bureaucrats respond to fiscal incentives. In particular, if budgets of local politicians primarily rely on own revenues (i.e., local taxes) rather than on discretionary transfers from the federal budget, politicians have stronger incentives to enforce deregulation laws in order to maximize tax base by fostering business growth irrespective of whether they want to divert revenue or use it according to public interests (Zhuravskaya, 2000; Jin, Qian and Weingast, 2005).

## **3 Effect of reform on de facto regulation and its institutional determinants**

The Russian reform of business regulation gives us a good opportunity to test hypotheses outlined in the previous section because it allows observing the effect of the pre-determined (i.e., pre-reform) institutional characteristics on the local enforcement of exogenously-given from the point of view of the regions change in federal regulation laws.

First, we focus on the average reform progress across all areas of regulation and estimate how it is affected by the institutional characteristics (Section 3.1). Second, as institutions may differently affect the enforcement of reforms in different regulatory areas, we study the effect of institutional measures on reform progress separately in each specific area of regulation (Section 3.2).

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<sup>10</sup>The last point is true insofar as the strength of industrial lobby is uncorrelated with market failure. One could argue, however, that concentrated industries with strong lobbies may be subject to market failures (e.g., monopolization); then, one would expect higher regulation levels for existing firms to cure market failures in regions with higher industrial concentration.

## 3.1 Effect of reform on average across all regulatory areas

### 3.1.1 Methodology: average across all regulations

We estimate the average impact of adoption of a law from deregulation package on the actual level of regulatory burden using the difference-in-differences (DD) estimators by relying on the variation in the timing of enactment of different deregulation laws. Our main focus is on analyzing the institutional determinants of reform progress: We explore the differential impact of an average deregulation law on regional regulatory burden depending on the regional institutional environment using the difference-in-differences-in-differences (DDD) estimators.

We construct two alternative measures of regulatory burden comparable across types of regulations, firms and over time: (1) a proxy for the overall level of regulation and (2) a proxy for the overall level of violation of targets set in the deregulation laws. First, we select variables from the MABS survey that measure the regulatory burden along the five dimensions targeted by the reform. The measures of the level of regulatory burden on firms at each point in time are: 1) the number of illegitimate licenses; 2) minus the term of license validity; 3) the number of sanitary inspections; 4) the number of days needed for registration, and 5) the number of agencies needed for registration. For comparability across these series, for each of these five variables we construct Z-scores by subtracting the sample mean and dividing by standard deviation. To measure the extent of violations of the deregulation targets along the five dimensions of reform, we take dummies indicating whether firm had 1) an illegitimate license; 2) a legitimate license with too short term of validity; 3) more than one sanitary inspection in half a year; 4) more than one week for registration; and 5) more than one window for registration. Then, we pool the five series within the two groups together. This yields two variables which vary across firms, five dimensions of regulations, and six points in time: 1) z-scores measuring the level of regulation in a particular firm for different types of regulations at different points in time and 2) dummies measuring the violations of reform targets in a particular firm at a particular point in time.<sup>11</sup>

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<sup>11</sup>The two measures are theoretically distinct because the level of regulation can differ even in situations

We denote these measures by  $V_{ift}$ , where  $i$  indexes the five dimensions of regulatory reform,  $f$  indexes firms, and  $t$  indexes rounds of the MABS survey (i.e., our measure of time). For each of the two measures of the overall regulatory burden, we run OLS regressions with fixed effects for each dimension of regulation in each region and linear trends for each dimension of regulation:

$$V_{ift} = \alpha(I_r - \bar{I})A_{it} + \beta(V_{ift_0} - \bar{V}_{t_0})A_{it} + \gamma A_{it} + \delta' \mathbf{X}_{ft} + \mu' \mathbf{Z}_{rt} + \phi_{ir} + \rho_t + \sum_i \eta_i t d_i + \varepsilon_{ift}. \quad (1)$$

Here,  $r$  refers to the region where firm  $f$  is located. The variable  $A_{it}$  denotes a dummy indicating whether the respective deregulation law responsible for the regulatory measure  $i$  is in force at time  $t$  or not. As different deregulation laws took force at different points in time, “after reform” dummy ( $A_{it}$ ) varies both over time and across regulations  $i$ . The coefficient  $\gamma$  on the “after reform” dummy is a DD estimate of the average effect of adoption of a deregulation law on the overall regulatory burden.  $I_r$  denotes a particular institutional characteristic of a region  $r$  which can potentially affect the reform progress (i.e., measures of government transparency, control over corruption, availability of independent media sources, the strength of local industrial lobbies, and the share of own revenue in local budget). All institutional measures are described in the Data Appendix and summarized in the Panel B of Table A.1. It is important to note that our institutional determinants do not vary over time and were measured in 2000, i.e., before the reform had started.<sup>12</sup> Our main coefficient of interest,  $\alpha$ , is a DDD estimate of the impact of institutional characteristics ( $I_r$ ) on the progress of the reform. To be precise, it estimates the differential effect of the regulatory reform (i.e., the enactment of the deregulation laws) on the level of actual regional regulatory burden in an average region depending on the level of regional institutional characteristic ( $I_r$ ).

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when all reform targets are met or when all of them are violated. In the latter case, the level of regulation would measure the distance to targets set in the deregulation laws.

<sup>12</sup>In order to interpret the coefficient  $\gamma$  as the full effect of reform at the mean level of institutional environment, we subtract the sample means ( $\bar{I}$  and  $\bar{V}_{t_0}$ ) from  $I_r$  and  $V_{ift_0}$  before taking their cross-terms with  $A_{it}$ .



We include the following covariates into the regression equation.  $\tau_t$  are the fixed effects for time and  $\phi_{ir}$  are fixed effects for each regulation  $i$  in each region  $r$ . Regulation\*region fixed effects control for all time invariant characteristics of regions and of types of regulations in each region, including the initial level of regulatory burden. Time fixed effects control for all global trends and macro-economic events that uniformly affect regulations during the sample period. To control for diminishing trends in some of the regulations, we also include linear time trends specific to each regulation ( $t * d_i$ , where  $t$  is linear trend and  $d_i$  is a dummy for regulation  $i$ ).

An important control variable is the interaction of the initial level of regulatory burden ( $V_{ift_0}$ ) and the “after reform” dummy ( $A_{it}$ ). The coefficient on this interaction measures the extent to which the progress in reform depends on the initial level of regulation. Since the institutional environment is often correlated with the initial level of regulation, without this covariate one could have found spurious correlation between the progress of reforms and institutions. Indeed, in our data industrial concentration is positively and significantly correlated with the initial level of regulation, whereas government transparency has a negative and significant correlation with the initial regulation level. The initial time period ( $t_0$ ) refers to the first round of the MABS survey that measures the benchmark level of regulatory burden before any of the deregulation laws took effect, i.e., the second half of 2001.

$\mathbf{X}_{ft}$  is a vector of controls for basic firm characteristics, i.e., age, size allowing for a quadratic term, legal firm, state vs. private ownership, and industry.<sup>13</sup>  $\mathbf{Z}_{rt}$  is a vector of additional regional covariates; it includes the logarithm of regional population to control for the regional size and the mean individual income to control for prosperity of the region. It is important to note that we correct standard errors to allow for clustering of error terms ( $\varepsilon_{ift}$ ) for all observations within each region that are related to registration, licensing, and inspections, yielding  $3 * 20 = 60$  clusters. Clusters take care of two potential concerns:

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<sup>13</sup>Previous literature documented a large variation in regulatory environment faced by firms within localities and showed that variables in  $\mathbf{X}_{ft}$  explain much of this variation. See, for instance, Frye and Zhuravskaya (2000).

autocorrelation in residuals and cross-sectional correlation among the observations within areas of regulations in each region (Bertrand, Duflo and Mullainathan, 2004).<sup>14</sup>

As with any DD estimation, our empirical strategy is valid only if the following two assumptions hold (subject to holding all covariates constant): 1) in the absence of reform, different regulatory measures would have followed their own overtime trends ( $t * d_i$ ) and would not have had a discrete shift at the time when the reform actually took place; and 2) in the absence of the institutional variation among regions, reform impact on *de facto* regulations would have been uniform across regions.

### 3.1.2 Results: average across all regulations

Table 2 presents the results. The first column of the table (regressions 1 and 8) presents the DD estimates of the effect of reform on *de facto* regulations without controlling for regional or firm characteristics but controlling for fixed effects for each regulation in each regions, time dummies, and regulation-specific linear trends. We find that the reform caused a statistically significant albeit not terribly large improvement in the average regulatory burden. The coefficients on  $A_{it}$  are negative and statistically significant. The enactment of a new deregulation law on average lead to a decrease in the rate of violation of a specific deregulation target set by this law of 10 percentage points, equal to a quarter of its standard deviation (SD); and it lead to a decrease in the level of regulatory burden in an average region in the specific regulatory area covered by this law of 8 percent of its SD.

The reform progress is not uniform across regions. What drives the differences? The rest of the Table 2 reports results for the effect of five institutional variables – transparency of authorities, internet penetration, control over corruption, industrial concentration of employment, and fiscal incentives (i.e., the share of own revenues). Each of these institutional

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<sup>14</sup>As a baseline, we report results where we simply pool the samples of new and old firms together; thus, there are four times as many old firms as new firms by construction of the MABS samples. To check robustness, we also applied equal weights for new and old firms and equal weights for each regulation type. We also re-estimated the model taking averages of firm-level observations for each of the five dimensions of regulation in each region and round. In all cases, the results are robust: Both the direction and statistical significance of the effects of institutions are preserved; while their magnitude increases.

measures facilitated enforcement of reform. The coefficients  $\alpha$ —the estimates of the effect of institutions on the reform progress—in all of these regressions are negative and, with one exception, statistically significant. Thus, government transparency, the presence of independent sources of news, i.e., internet and independent radio and newspapers (the latter are not reported), control over corruption, the presence of strong industrial lobby, and strong fiscal incentives have a significant effect on the overall progress in the implementation of reform.<sup>15</sup> These results are fully consistent with the predictions of the public choice theory of the nature of regulations discussed in Section 2. In contrast, the results about the effect of independent media, government transparency, and industrial concentration are inconsistent with the predictions of public interest theory.

The magnitude of these effects is as follows. The adoption of a deregulation law lead to a 9 percentage point larger compliance with reform targets and a 27% of SD lower level of regulatory burden in the Russian regions with the most transparent administrations compared to the regions with the least transparent administrations. The difference in reform progress between the best and the worst regions in terms of public access to independent media, control over corruption, and fiscal incentives and the regions with the largest and the smallest industrial concentration is between 6 to 7 percentage points of the share of violations of reform targets and 16 to 19 percent of the regulation level’s standard deviation.<sup>16</sup>

The initial severity of regulatory burden itself is also a very important determinant of the reform progress. The coefficients on the interaction of the initial level of regulatory burden and “after reform” dummy are negative, statistically significant and large in magnitude. The reform partially equalized the level of regulatory burden across firms: a one SD higher initial level of regulation leads on average to a 0.6 SD larger decrease in the level of regulation;

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<sup>15</sup>The last column of Table 2 reports regression results in which we simultaneously include interaction terms between a measure of regional accountability, namely, transparency, industrial concentration, and fiscal incentives, on the one hand, and after-reform dummy, on the other hand. The results are robust. One, however, can not simultaneously include cross-terms with alternative measures of accountability, i.e., transparency, internet, and control over corruption because the measures are highly correlated.

<sup>16</sup>These extreme differences in the institutional characteristics of the regions (i.e., the differences between the worst and the best regions) are equal to 3 to 4 SDs in the respective variables.

and a 10 percentage point higher rate of violation of a particular reform target leads to a 7 percentage point larger decrease in the rate of violation of this target as a result of reform.<sup>17</sup>

## 3.2 Effect of reform in specific regulatory areas

Section 3.1 established the average effect of institutions on reform progress across regulatory areas. A priori it is not clear, however, whether reform progress in different regulatory areas is affected by institutions in the same way. We address this question in this section.

### 3.2.1 Methodology: specific regulations

Henceforth, we treat each measure of the actual regulatory burden and of the violation of each reform target as a separate dependent variable. The methodology is, again, the difference-in-differences. We regress each of these variables on the interaction between the “after reform” dummy and a potential institutional determinant of deregulation ( $I_r$ ). We control for time fixed effects and region- or firm-fixed effects depending on whether we look at new startups for which we have repeated cross-section or established firms for which we have panel data. Firm-level panel data on established firms contains information on licensing and inspections; repeated cross-section of new firms contains information on licensing and registration.

Thus, for licensing and inspections in established firms, we estimate equation with firm fixed effects ( $\phi_f$ ):

$$R_{ft} = \alpha I_r * A_t + \beta R_{ft_0} * A_t + \delta' \mathbf{X}_{ft} + \mu' \mathbf{Z}_{rt} + \phi_f + \rho_t + \varepsilon_{ft}; \quad (2)$$

whereas for licensing and registration of new firms, the estimated equation has region fixed

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<sup>17</sup>Positive correlation between the speed of reform and the initial level of regulation potentially could be generated by a mean reversion process due to a measurement error in the level of regulation. First, for both of the registration laws, we checked directly that there was no mean reversion before the reform; we could not do it for laws of licensing and inspections as there are not enough data. Second, we verified that the exclusion of the cross-term of after reform dummy and the initial level of regulation from the list of regressors does not substantially affect the results about the effect of institutions, i.e., estimates of  $\alpha$ . They are robust in terms of the direction and statistical significance of the effects.

effects ( $\phi_r$ ):

$$R_{ft} = \alpha I_r * A_t + \beta R_{rt_0} * A_t + \delta' \mathbf{X}_{ft} + \mu' \mathbf{Z}_{rt} + \phi_r + \rho_t + \varepsilon_{ft}. \quad (3)$$

$R_{ft}$  stands for one of the specific measures of regulatory burden (listed in Panels A to C of Table 1 and summarized in Table A.1). The rest of the notation is the same as in Equation 1. As above,  $I_r$  denotes a particular institutional characteristic of a region  $r$  which can potentially affect reform progress;  $A_t$  denotes a dummy indicating whether the respective deregulation law is in force or not yet. In contrast to Equation 1, in Equations 2 and 3 “after reform” dummy varies only over time because in each regression we consider a specific regulation affected by reform only once. “After reform” dummy is, therefore, collinear with time dummies and omitted from the list of regressors. Note that time dummies control for any trends in the specific regulation measure  $R$ . Our primary parameter of interest ( $\alpha$ ) estimates the differential effect of the enactment of a specific deregulation law on the level of actual regulatory burden in the specific regulatory area covered by this law in an average firm depending on the level of institutional characteristic  $I_r$ . Again, we control for the interaction of the “after reform” dummy with the initial (before reform) level of regulatory burden ( $R_{ft_0}$ ). In Equation 2, we correct standard errors to allow for clustering of error terms ( $\varepsilon_{ft}$ ) within each firm to account for residual autocorrelation. In Equation 3, we correct standard errors to allow for clustering of error terms ( $\varepsilon_{ft}$ ) within each round and region to account for residual correlation among firms within region. The results are robust to making alternative assumptions about the variance-covariance structure of the error term.

The main assumption necessary for the validity of this DD methodology is that in the absence of institutional variation the average change in regulatory burden as a result of reform would have been the same across regions for a given level of  $\mathbf{X}$  and  $\mathbf{Z}$ .

### 3.2.2 Results: specific regulations

Results are presented in Tables 3 and 4. First, let us discuss the results for the sample of established firms where we control for firm fixed effects. Table 3 reports regressions with firm fixed effects for two selected regulatory measures: “at least one illegitimate license” and “more than one sanitary inspection.” Column 1 of Table A.2 provides abbreviated results (i.e., the point estimates of  $\alpha$ ) for all other regulatory measures. All institutional measures (with the exception of fiscal incentives), i.e., government transparency, internet penetration, control over corruption, industrial concentration, and the share of own revenues, significantly improve the local enforcement of delicensing reform; and all the institutional measures (with the exception of control over corruption) significantly improve the enforcement of reform in limiting the number of sanitary inspections.

These results are powerful because they account for all the variation across firms; they, however, are limited to established firms only. Since we are interested in comparing the effect of institutions on the reform progress for incumbent firms and for new entrants, we also report results of estimation of Equation 3. Table 4 reports full regression output for selected regulatory measures and Column 2 of Table A.2 reports abbreviated results for all regulatory measures. Panel A of Table 4 replicates the results for the established firms with regional instead of firm fixed effects: the results are qualitatively the same. Panel B of Table 4 presents results for newly-registered startups. Industrial concentration, government transparency, and internet penetration significantly reduce the probability that a startup firm has to use more than one agency for registration and apply for an illegitimate license as a result of the reform. The effect of control over corruption has the same sign but is statistically insignificant. Fiscal incentives significantly affect reform progress only in reducing the number of windows for registration.

Interestingly, there is no difference in the direction of the effect of institutional measures, and particularly, industrial concentration, for entry regulations and the regulations of existing businesses. Thus, industry incumbents do not lobby for an increase of the entry regulations.

On the contrary, they lobby for lower entry regulations as well as lower regulations of their own activities. This result contrasts with the prediction of the regulatory capture theory under the assumption that large business lobbies compete with small potential entrants, which—as we discussed in Section 2—may not be a reasonable assumption. If there is no competition between small business entrants and large industrial lobbies, regulatory capture theory cannot be tested using data on Russia’s regulatory reform.

We find no effect of any of the institutional measures on reform progress in reducing the number of days for registration or lengthening the term of license validity for both old and new firms (see Table A.2).<sup>18</sup> Thus, the average effect of the enactment of a deregulation law, estimated in Section 3.1, averaged between the large effect of the institutional characteristics on the reform progress in delicensing, reducing the number of windows for registration, and limiting number of inspections, and no effect of these institutions on the reform progress in reducing the length of registration and increasing the length of license validity.<sup>19</sup>

To illustrate the magnitude of these effects, consider government transparency. The most transparent regions had 12 and 15 percentage point lower share of established and new firms with illegitimate licenses, respectively, a 4 percentage point lower share of established firms inspected more than once by sanitary agency in six months, and a 35 percentage point higher share of start-ups with “one-stop-shop” registration as a result of reform.

To summarize, our main finding in this section is that industrial concentration, govern-

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<sup>18</sup>It is plausible, however, that the length of license validity changes only with a lag; in particular, this would be the case if the starting and ending times of licenses are correlated across firms.

<sup>19</sup>Overall, the results are consistent for the regressions with region and firm fixed effects and for the samples of old firms and startups. To ensure robustness of our results, we use several additional institutional measures: the industrial concentration of output, state capture index, non-zero subscription to the only two independent (at that time) federal business newspapers *Vedomosti* and *Kommersant*, the presence of a signal of the largest independent radio station *Echo Moscow* in the area, and the media freedom index. State capture index and media freedom index do not have a significant effect on the progress of reforms; whereas all other measures produce results very similar to the baseline. The media freedom and the state capture indices, however, may be poorly measured because they are constructed on the basis of subjective perceptions of experts, unlike all other institutional measures; as a result, the coefficient estimates for regressions with these two measures may have an attenuation bias. In addition, our results do not depend on the inclusion of the regional control variables, i.e., population and income. We also tries to control for the obedience of local governments to the federal center by including the interaction of  $A$  with the dummy indicating whether the regional governor belongs to the governing “United Russia” party as an additional covariate. The results are robust. The progress of reform itself is unaffected by whether the governor belongs to the governing party.

ment transparency, and internet penetration consistently significantly and robustly affected implementation of reform in limiting the number of inspections, delicensing, and establishing one-stop-shop registration. These results are inconsistent with the public interest theory of the nature of regulations and fully consistent with the tollbooth theory.

## 4 The outcomes of de facto regulation

An important question for theory and policy is whether regulation is beneficial or detrimental for social welfare, growth, and development. A common problem with figuring out the effect of regulation on any of the outcomes is endogeneity of regulation. On the one hand, under the public interest theory, benevolent regulators should regulate more in places where there are higher market failures. This could lead to a reverse causality from poor outcomes (e.g., poor quality of goods or pollution) to higher levels of regulation. On the other hand, under the public choice theory, predatory regulators may be disproportionately attracted to places where there is a thriving business growth because they can generate more rents by preying on successful and profitable firms. This could lead to a reverse causality from business growth to higher regulation levels. Without finding an exogenous source of variation in regulation, causal claims based on correlation between regulatory burden and economic outcomes are problematic.

Russia's reform of business regulation is a policy experiment that provides instruments for solving this endogeneity problem. Our main goal in this section is to establish a causal relationship going from the level of regulation to such outcomes as net entry, small business employment, pollution, and morbidity. We are interested in estimating the following relationship:

$$S_{rt} = \xi \bar{R}_{rt} + \zeta' \mathbf{Z}_{rt} + \phi_r + \rho_t + \varepsilon_{rt}. \quad (4)$$

The dependent variable ( $S_{rt}$ ) stands for one of the following regional outcomes: the net entry into the official (measured by the log number of small businesses), official small business



employment (measured by the number of employees in small business sector per capita), pollution (measured by the log emissions of contaminants into the atmosphere), and public health (measured by morbidity from injuries and poisoning per 1,000 people). The regional outcome variables are summarized in the Panel C of Table A.1. They come from the official Russia’s statistical agency *Rosstat*; and are available for all regions annually up until 2004 (inclusive), i.e., for the period from the first to the fifth round of MABS survey. There are no reliable data on the size of the unofficial sector.

$\bar{R}_{rt}$  stands for a specific *regional-level* regulation measure. We construct regional-level regulation measures by aggregating firm-level regulation measures across firms in the same region and round. The aggregation takes two steps. First, we partial out the effect of basic firm characteristics ( $\mathbf{X}_{ft}$ ) from regulation measures ( $R_{ft}$ ) by taking residuals of the OLS regression:  $R_{ft} = \lambda' \mathbf{X}_{ft} + \varepsilon_{ft}$ . Second, we take simple averages of these residuals by region in each round of the survey:  $\bar{R}_{rt} = \frac{1}{N} \sum_{f=1}^N \hat{R}_{ft}$ , where  $N$  is the number of firms in each region\*round.<sup>20</sup>

The rest of the notation is as above.  $\xi$  is our coefficient of interest. Since it cannot be estimated by OLS because of reverse causality, we estimate it with 2SLS. The analysis presented in the Section 3 of the paper helps to identify the sources of exogenous variation in regulatory burden. We use the interactions of  $A$  with institutional measures  $I$  and with the initial level of regulatory environment as instruments. Inclusion of time and region fixed effects into the list of covariances is crucial for the validity of our instruments because both the regional institutions ( $I_r$ ) and the time trend (collinear with  $A_t$ ) have a direct effect on the outcomes ( $\bar{R}_{rt}$ ) and are correlated with the instruments ( $I * A$ ); time and region fixed

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<sup>20</sup>The use of firm employment to construct regional regulation measures potentially could introduce a simultaneity problem if regional and firm employment co-vary. However, the point estimates of  $\xi$  in the second stage remain unchanged if we construct regional regulation measures as simple averages without controlling for firm characteristics. As a baseline we control for firm characteristics because it increases power of the instruments in the first stage. The results are unchanged if we use region\*round fixed effects rather than averages of residuals to aggregate regulation measures.

effects control for the direct effects of institutions and time. The first stage is as follows:

$$\bar{R}_{rt} = \alpha I_r * A_t + \mu' \mathbf{Z}_{rt} + \phi_r + \rho_t + \varepsilon_{rt}. \quad (5)$$

As regulation measures, we take the average regional values of the frequency of sanitary inspections, number of illegitimate licenses, and the number of agencies needed for registration. We do not consider how the length of license validity and the time needed for registration affect the outcomes because institutions did not affect the reform progress in these areas (as shown in Section 3.2) and, therefore, we do not have instruments for them. As the model is over-identified, we can test for the validity of over-identification restrictions. We find that Hansen’s J-test does not reject the null hypothesis of the validity of restrictions for the regressions explaining proxies for small business entry and employment. In contrast, exogeneity of instruments in regressions explaining pollution and public is questionable, as in three out of six regressions the null is rejected. For each regression, we chose the subset of instruments, which maximizes F-statistics for the excluded instruments in the first stage. Table A.3 in the appendix reports the first stage along with the results of F and J tests. For registration and licensing, the instruments are sufficiently strong; whereas for inspections instruments are weak and, therefore, the second stage results for inspections may be biased due to the weak instruments problem (we use criteria for weak instruments from Stock, Wright and Yogo, 2002).

#### 4.1 Results: outcomes of regulation

First, let us consider the estimates of the effect of regulation on the net entry and employment of small businesses (presented in Panels A and B of Table 5, respectively). The table reports OLS and 2SLS estimates. 2SLS regressions (even columns) yield statistically significant negative effects of illegitimate licenses and the share of firms with frequent sanitary inspections on the net entry measured by the log number of small businesses. In addition, the share of firms that had to visit more than one agency in order to register and the share

of firms with frequent sanitary inspections in a region have a significant negative effect on the small business employment as a share of population. The share of firms with more than one agency for registration in a region does not have a significant effect on net entry and the number of illegitimate licenses does not significantly affect employment.

In order to illustrate the direction and size of the bias in uninstrumented regressions, in addition to the results of the 2SLS estimation, we present OLS results (odd columns). In all regressions uninstrumented OLS estimates are larger than 2SLS estimates. This points to a positive and rather large bias in the OLS estimates, which is consistent with the view that predatory regulators are attracted to the environments with more vibrant and growing business.<sup>21</sup> As we mentioned above, the finding about the effect of inspections may be tenuous due to the weak-instrument problem. Despite the weak instruments, one can be confident of the direction of the effect since one expects a positive bias in the OLS estimate, while both the OLS and 2SLS regressions produce negative and significant coefficients for the effect of inspections on entry and small business employment.

The magnitude of the estimated effects is large. A perfectly-enforced reform, which shifts to a one-stop-shop registration (from an average pre-reform level with 80% of startups registering in more than one agency), leads to a 24% increase in small business employment. Perfect enforcement of delicensing reform (which lifts the pre-reform licensing requirements) leads to a 21% increase in the number of small businesses. A reduction in the number of sanitary inspections to a level of at most one in six months (from the pre-reform level) leads to a 17% increase in the number of small businesses and a 14% increase in the small business employment.<sup>22</sup>

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<sup>21</sup>Such endogeneity of regulation can explain why Klapper, Laeven and Rajan (2006) find that more benign entry regulations are not associated with higher entry in corrupt countries whereas there is a strong relationship in uncorrupt countries.

<sup>22</sup>The instruments used for different regulatory measures (i.e., the interaction terms of institutional determinants of reform progress and “after reform” dummy) are correlated with each other. Thus, the interpretation of the results requires a word of caution: the instruments do not allow us to distinguish between the effects of changes in different dimensions of regulation; instead, we estimate the causal effect of the whole cluster of regulations associated with registration, licensing, and inspections on the outcomes. Thus, the correct interpretation of the results in this section is that liberalizing regulation *in general* is beneficial for entry and small business growth.

Let us now turn to the estimation of the effect of regulation on pollution and morbidity. Under the assumption that registration procedures, licensing, and sanitary inspections aim at correcting such market failures as pollution externalities or provision of toxic goods by neglectful fly-by-night businesses, the public interest theory predicts a negative relationship between the level of these regulations, on the one hand, and pollution and morbidity, on the other. We find no empirical support for this. Panel A of Table 6 presents the regression results for emissions and Panel B—for morbidity. None of the estimated coefficients is statistically significant negative. For pollution, the coefficients of interest are negative but very imprecisely estimated; the same is true for one of the three coefficients in regressions for public health. The other two coefficients in regressions for public health are positive (contrary to the prediction of public interest theory) and one is marginally significant. Contrary to the public interest theory which predicts an upward bias in the OLS estimation, there is no systematic relationship between OLS and 2SLS estimates: in registration and licensing regressions for public health, the OLS estimates are actually lower than 2SLS estimates. It is important to keep in mind, however, that over-identification restriction test failed for three of these regressions. Moreover, the public health and pollution variables may be poorly measured and considered regulations may aim at curing other market failures; thus, one should treat the evidence of no relationship between pollution and morbidity, on the one hand, and regulations, on the other hand, merely as suggestive.

## 5 Robustness

In this section, we describe various robustness checks for our baseline results.

*Placebo experiment.* In order to make sure that standard errors in our dif-in-dif regressions are not biased downwards (Bertrand, Duflo and Mullainathan, 2004), we conduct a placebo experiment. We take all possible combinations of dates for each of the five dimensions of reforms, such that these dates are different from the dates of the real deregulation laws and if these dates happen to be after the dates of the real laws they are at least two rounds

away from the actual laws. We consider these combinations as the dates of the enactment of placebo laws in placebo reform packages. We exclude dates for placebo laws that take place one round after the actual law in each dimension of regulation because of a possible delay in the implementation of the real laws. Altogether there are 81 of such combinations. For these 81 placebo reforms, we re-ran all regressions of table 2 for both the regulation z-scores and the shares of violation of reform targets. The results are as follows. First, consider the average effect of reforms on the regulatory burden (i.e., coefficient  $\gamma$  in Equation 1): we found a significant (at 10% significance level) effect of placebo reforms in lowering the *de jure* regulations in 14% of the 162 regressions. Second, we find a significant effect (at 10% significance level) of our five institutional measures in facilitating the reform progress (i.e., coefficient  $\alpha$  in Equation 1) in 12% of the 810 regressions. Thus, although it does seem to be the case that the standard errors are slightly biased downwards, this cannot explain the strong and robust effects which we find for the real laws.

*Region-specific linear trends.* One could argue that, independently of the reform, different regional institutional environments may be associated with different trends in regulation level. In order to make sure that our results are not driven by this relationship, we re-ran regression equations 1, 2, and 3 with region-specific linear trends as additional regressors. First, the estimates of the average effect of reform on *de jure* regulations are unaffected by the inclusion of region-specific trends. Second, the direction of the estimated effects of institutions on the reform progress remains the same ( $\alpha$  coefficients remain negative), the magnitude of the effects decreases a little bit in some cases, the magnitude of the standard errors increases quite substantially, but in the majority of regressions the coefficients of interest remain statistically significant. To be more precise, in Tables 2-4, we report forty regressions estimating  $\gamma$  coefficients (eight regressions for each institutional measure) in which we find significant effect of considered institutions on the reform progress in 32 (80%) of these regressions. Once we include region-specific linear trends, significance is preserved in 55% of all regressions. In the vast majority of the cases, the statistical significance is

lost because of an increase in the standard errors rather than a decrease in the magnitude of point estimates. This suggests that our baseline specification does not suffer from the omitted variable bias. The most vulnerable to the inclusion of the region-specific trends turn out to be internet penetration which remains significant only in 2 out of 8 regressions. One should note, however, that many of the alternative measures of access to independent media remain significant after controlling for region-specific trends. Thus, the results are qualitatively the same, but become somewhat weaker statistically with the inclusion of region-specific trends. This, however, is to be expected considering that we have only 6 time periods.

Overall, our results prove to be robust.

## 6 Conclusions

We analyze firm-level panel data on the regulatory burden of firms in Russia during a period of a drastic regulatory reform. Our findings are as follows. On average, the reform significantly lowered the actual regulatory burden on Russian firms; the reform progress, however, exhibited a vast regional variation. Five institutional factors had a robust, statistically significant, and economically strong effect on the implementation of the reform in the Russian regions: government transparency, control over corruption, internet penetration and the access to other independent sources of media, the presence of strong industrial lobby, and strong fiscal incentives. These factors are associated with a better reform progress both in the regulations of entry and regulations of businesses already in operation. Using the interaction between the timing of reform and the determinants of its success as exogenous sources of variation in regulatory burden, we show that the reform had a large significant positive causal effect on the official small business entry and employment and had no (adverse) effect on pollution and public health.

This evidence is inconsistent with the public interest theory and is fully consistent with the public choice theory and, in particular, tollbooth theory of the nature of regulation (de Soto, 1990; Shleifer and Vishny, 1993; Djankov et al., 2002): regions with transparent,

accountable and least corrupt governments as well as more informed populations are the ones that achieve better progress in liberalizing regulation.

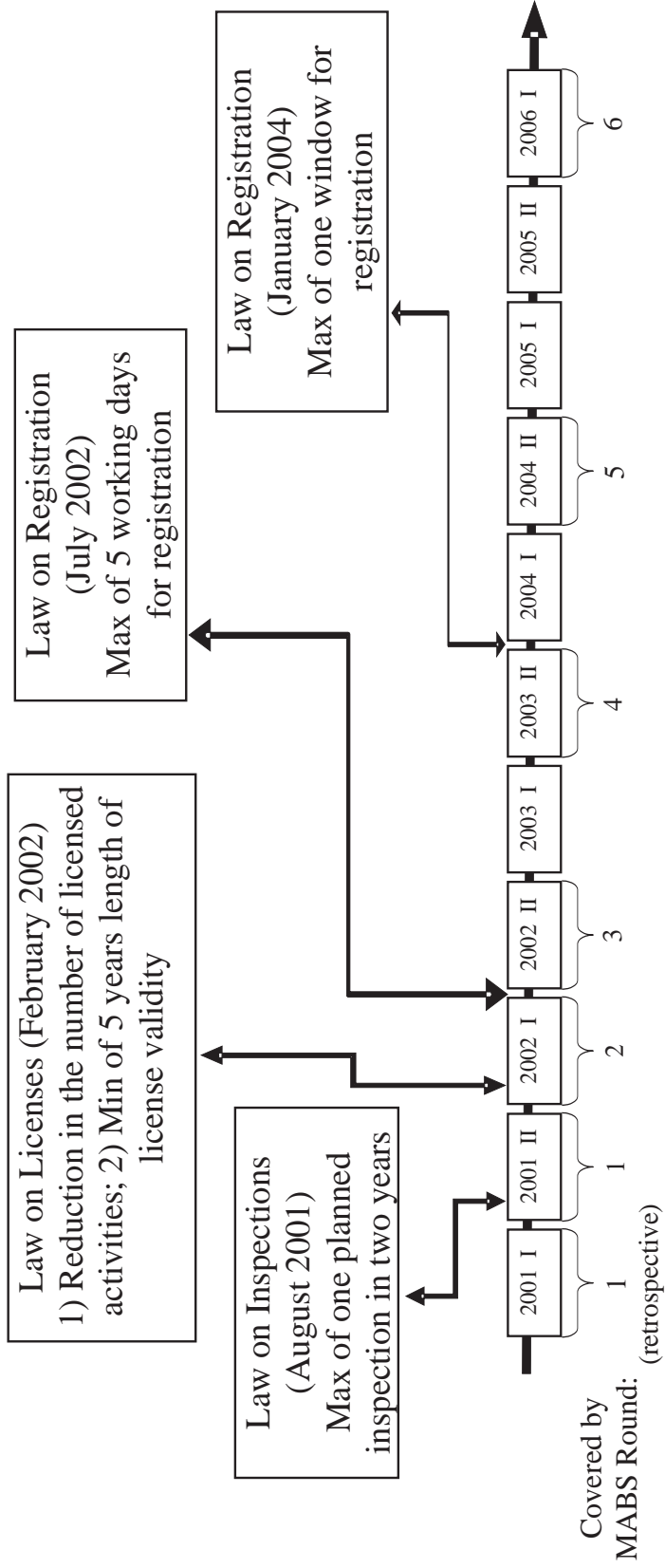
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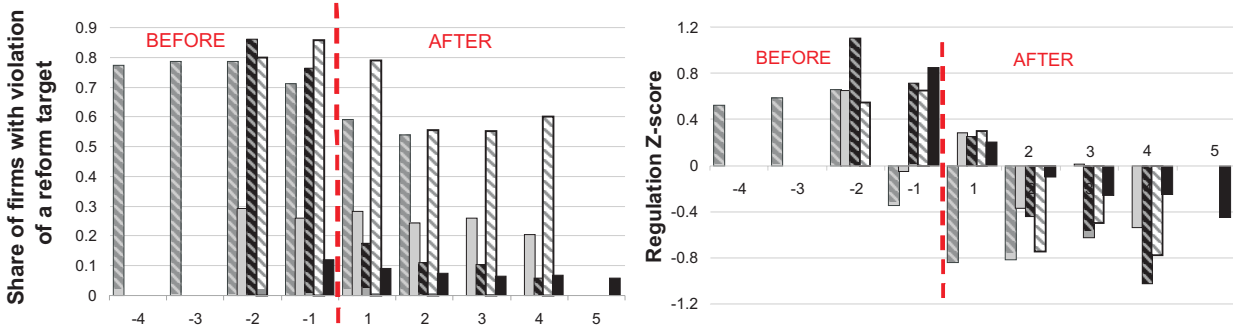


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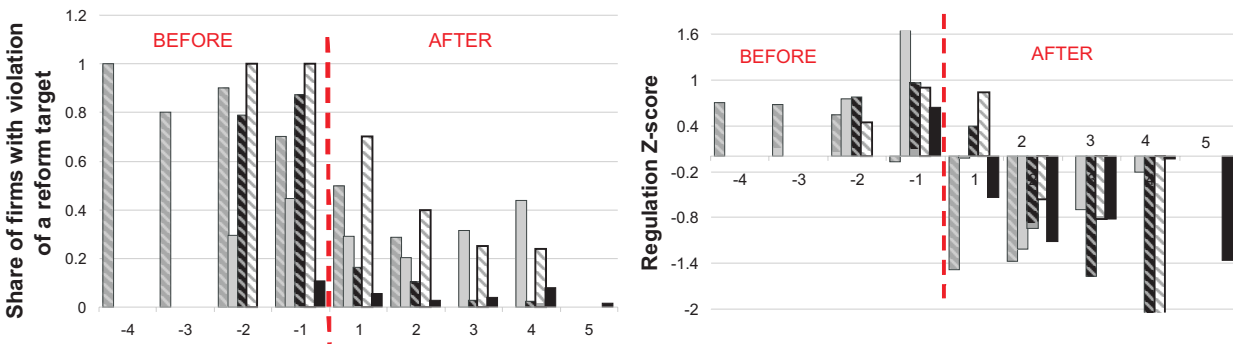


**Figure 1:** The Timing and Content of Regulatory Reform and Rounds of MABS Survey

### Average across all regions:



### Average for Samara region:



#### Legend for share of violations of reform targets:

- Horizontal axis - number of survey rounds before and after reform
- ▨ Windows for registration > 1
  - ▤ Illegitimate licenses > 0
  - ▩ Length of license validity < 5 years
  - ▧ Days for registration > 5
  - Inspections > 1

#### Legend for regulation z-scores:

- Horizontal axis - number of survey rounds before and after reform
- ▨ Windows for registration
  - ▤ Illegitimate licenses
  - ▩ Length of license validity
  - ▧ Days for registration
  - Inspections

**Figure 2:** Regulations before and after reform for all regions (upper panel) and for Samara region (lower panel)

The number of rounds before and after the reform varies for different regulatory areas as the timing reform differs (the details on the timing are presented in Figure 1).

**Table 1: The List of Variables Measuring Regulatory Burden**

	Variable:	Abbreviated name:	
Panel A:	Log (number of sanitary inspections in six months)	log number of inspections	V
Inspections	Dummy for more than one sanitary inspection in six months	more than one sanitary inspection	V
Panel B:	Log (number of legitimate licenses the firm has)	log number of legitimate licenses	
Licensing	Dummy for presence of an illegitimate license	at least one illegitimate license	V
	Negative of Log (term of license validity for the legitimate licenses)	minus log term of license validity	
	Dummy for less than 5-year-term of license validity for a legitimate license	too short term of license validity	V
Panel C:	Log (number of agencies needed for registration)	log number of windows for registration	
Registration	Dummy for more than one agency for registration	more than one window for registration	V
	Log (days for registration)	log days for registration	
	Dummy for more than a week for registration	more than one week to register	V
Panel D:	Overall level of regulation:	regulation level	
Overall	Panel of Z-scores for the number of legitimate licenses, minus term of license validity, the number of sanitary inspections, the number of days for registration, and the number of windows for registration		
regulatory burden	Overall level of violation of deregulation targets:	violation of reform targets	
	Panel of dummies indicating too short license validity, an illegitimate license, more than one sanitary inspection, more than one week to register, and more than one window for registration		

Note: “V” labels dummy variables indicating whether there is a violation of a specific target in one of the deregulation laws. We take the negative of the length of license validity variable in order to have all the effects go in the same direction, i.e., higher values of all regulatory measures mean higher regulatory burden.

**Table 2: Overall effect of reform on de jure regulations**

	1	2	3	4	5	6	7
	Violation of reform targets						
AFTER	-0.103	-0.233	-0.233	-0.237	-0.233	-0.235	-0.258
Transparency * AFTER	[0.02]***	[0.03]***	[0.03]***	[0.02]***	[0.03]***	[0.03]***	[0.022]***
Internet * AFTER		-0.006					-0.004
		[0.002]***					[0.002]**
Corruption cont. * AFTER			-0.005				
			[0.003]*				
Ind. concentr * AFTER				-0.103			
				[0.058]*			
Fiscal incentives * AFTER					-0.222		-0.133
					[0.07]***		[0.069]*
Initial regulation * AFTER						-0.153	-0.086
						[0.06]***	[0.054]
Firm and region controls	No	-0.711	-0.711	-0.725	-0.709	-0.717	-0.714
Region*Regulation FE	Yes	[0.06]***	[0.06]***	[0.06]***	[0.06]***	[0.06]***	[0.06]***
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulation-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25 250	25 026	25 026	21 171	25 026	25 026	25 026
Regions*Regulations	100	99	99	79	99	99	99
R-squared	0.12	0.17	0.17	0.16	0.17	0.17	0.17
	8	9	10	11	12	13	14
	Regulation level						
AFTER	-0.081	-0.082	-0.083	-0.068	-0.083	-0.087	-0.154
Transparency * AFTER	[0.043]*	[0.047]*	[0.048]*	[0.052]	[0.046]*	[0.047]*	[0.050]***
Internet * AFTER		-0.016					-0.01
		[0.003]***					[0.003]***
Corruption cont. * AFTER			-0.013				
			[0.005]***				
Ind. concentr * AFTER				-0.061			
				[0.136]			
Fiscal incentives * AFTER					-0.633		-0.42
					[0.183]***		[0.191]**
Initial regulation * AFTER						-0.435	-0.255
						[0.145]***	[0.146]*
Firm and region controls	No	-0.591	-0.607	-0.644	-0.595	-0.639	-0.633
Region*Regulation FE	Yes	[0.07]***	[0.08]***	[0.14]***	[0.07]***	[0.07]***	[0.07]***
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulation-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24 842	24 640	24 640	20 849	24 640	24 640	24 640
Regions*Regulations	100	99	99	79	99	99	99
R-squared	0.02	0.07	0.07	0.07	0.07	0.07	0.07

Note: Robust standard errors adjusted for clusters at the level of the region \* each of the three regulation types (licensing, inspections, registration) are in brackets; \* significant at 10%; \*\* significant at 5%;\*\*\* significant at 1%.

Table 3: Specific Regulations and Institutions: Panel of Old Firms with Firm Fixed Effects

	1	2	3	4	5	6	7	8	9	10
	At least one illegitimate license				More than one sanitary inspection					
Transparency * AFTER	-0.007 [0.002]***					-0.003 [0.002]*				
Internet * AFTER		-0.008 [0.003]**					-0.004 [0.002]**			
Corruption cont. * AFTER			-0.129 [0.066]**					-0.033 [0.046]		
Ind. concentr * AFTER				-0.313 [0.112]***					-0.161 [0.067]**	
Fiscal incentives * AFTER					-0.08 [0.079]					-0.119 [0.049]**
Initial regulation * AFTER	-0.64 [0.03]***	-0.64 [0.03]***	-0.63 [0.03]***	-0.64 [0.03]***	-0.65 [0.03]***	-0.78 [0.03]***	-0.78 [0.03]***	-0.79 [0.03]***	-0.78 [0.03]***	-0.78 [0.03]***
Log (population)	0.25 [0.17]	0.38 [0.20]*	0.10 [0.19]	0.06 [0.16]	0.15 [0.17]	0.17 [0.14]	0.06 [0.13]	0.15 [0.16]	0.11 [0.14]	0.17 [0.14]
Log (mean pc income)	0.082 [0.057]	0.107 [0.060]*	0.070 [0.064]	0.078 [0.057]	0.057 [0.059]	-0.005 [0.038]	0.011 [0.040]	-0.004 [0.046]	-0.007 [0.038]	-0.023 [0.039]
New firm dummy	-0.36 [0.04]***	-0.34 [0.04]***	-0.001 [0.001]	-0.35 [0.04]***	-0.34 [0.04]***					
Firms age	-0.003 [0.013]	-0.002 [0.013]	-0.001 [0.015]	-0.003 [0.013]	-0.003 [0.013]	-0.005 [0.009]	-0.005 [0.009]	-0.002 [0.010]	-0.005 [0.009]	-0.004 [0.009]
Firms size	0.08 [0.04]**	0.08 [0.04]**	0.10 [0.04]**	0.08 [0.04]**	0.08 [0.04]**	0.03 [0.02]	0.02 [0.02]	0.04 [0.03]	0.03 [0.02]	0.03 [0.02]
Firms size squared	-0.009 [0.007]	-0.009 [0.007]	-0.012 [0.008]	-0.009 [0.007]	-0.009 [0.007]	-0.001 [0.005]	-0.001 [0.005]	-0.004 [0.006]	-0.001 [0.005]	-0.001 [0.005]
Firm's FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Legal form	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7 290	7 290	6 503	7 290	7 290	5 305	5 280	4 569	5 305	5 305
R-squared	0.12	0.12	0.11	0.12	0.12	0.29	0.30	0.30	0.29	0.29
Number of firms	2 270	2 270	2 215	2 270	2 270	1 522	1 516	1 518	1 522	1 522

Note: Robust standard errors adjusted for clusters at the firm level in brackets. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. There are no statistically significant results for the effect of institutions on reform progress in increasing the length of license validity.

Table 4: Specific Regulations and Institutions: Repeated Cross-section with Regional Fixed Effects

<b>Panel A</b>		1	2	3	4	5	6	7	8	9	10
		More than one sanitary inspection, old firms					At least one illegitimate license, old firms				
Transparency * AFTER		-0.004 [0.001]***					-0.004 [0.001]***				
Internet * AFTER			-0.002 [0.002]					-0.006 [0.002]***			
Corruption cont. * AFTER				0.029 [0.042]					-0.109 [0.039]***		
Ind. concentr * AFTER					-0.128 [0.050]**					-0.097 [0.054]*	
Fiscal incentives * AFTER						-0.113 [0.046]**					-0.091 [0.049]*
Initial regulation * AFTER		-0.54 [0.100]***	-0.53 [0.108]***	-0.41 [0.161]**	-0.53 [0.110]***	-0.58 [0.112]***	-0.62 [0.125]***	-0.58 [0.131]***	-0.62 [0.154]***	-0.57 [0.126]***	-0.65 [0.127]***
Region and Round FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm and Region controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		8 969	8 969	7 427	8 969	8 969	9 085	9 085	7 826	9 085	9 085
R-squared		0.09	0.09	0.1	0.09	0.09	0.05	0.05	0.05	0.05	0.05
<b>Panel B</b>		11	12	13	14	15	16	17	18	19	20
		More than one window for registration, new firms					At least one illegitimate license, new firms				
Transparency * AFTER		-0.023 [0.011]**					-0.009 [0.004]**				
Internet * AFTER			-0.037 [0.022]*					-0.015 [0.006]**			
Corruption cont. * AFTER				-0.177 [0.222]					-0.011 [0.105]		
Ind. concentr * AFTER					-0.934 [0.314]***					-0.478 [0.190]**	
Fiscal incentives * AFTER						-0.815 [0.314]**					0.108 [0.113]
Initial regulation * AFTER		-0.84 [0.25]***	-0.67 [0.24]***	-0.83 [0.24]***	-0.63 [0.25]**	-0.67 [0.23]***	-0.86 [0.13]***	-0.88 [0.14]***	-0.79 [0.14]***	-0.77 [0.12]***	-0.84 [0.14]***
Region and Round FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm and Region controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		812	812	754	812	812	2 031	2 031	1 718	2 031	2 031
R-squared		0.2	0.2	0.21	0.2	0.2	0.08	0.08	0.08	0.09	0.08

Note: Robust standard errors adjusted for clustered at the level of the region\*round in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. There are no statistically significant results for the effect of institutions on reform progress in shortening the length of registration procedures.

**Table 5: Regulation and Small Business**

Panel A: Regulation and (Net) Entry						
Dependent variable - log of total number of small businesses						
	1	2	3	4	5	6
	OLS	2SLS	OLS	2SLS	OLS	2SLS
More than one window for registration	0.022 [0.081]	-0.182 [0.193]				
Log (number of illegitimate licenses)			0.078 [0.212]	-0.872 [0.480]*		
More than one sanitary inspection					-0.7 [0.413]*	-2.122 [0.918]**
Log (population)	0.812 [0.870]	0.615 [1.009]	0.134 [0.381]	0.275 [0.344]	0.192 [0.374]	0.287 [0.274]
Log (mean pc income)	0.033 [0.166]	-0.05 [0.199]	0.021 [0.150]	-0.067 [0.165]	0.01 [0.146]	0.001 [0.162]
Round and Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84	84	99	99	99	99
R-squared, within	0.13		0.07		0.1	
F-stat for instruments (1st stage)		14.5		9.0		6.5
Panel B: Regulation and Small Business Employment						
Dependent variable - total employment in small business per capita						
	7	8	9	10	11	12
	OLS	2SLS	OLS	2SLS	OLS	2SLS
More than one window for registration	-0.008 [0.003]**	-0.016 [0.008]**				
Log (number of illegitimate licenses)			0.018 [0.011]*	0.001 [0.036]		
More than one sanitary inspection					-0.036 [0.021]*	-0.09 [0.042]**
Log (population)	-0.041 [0.033]	-0.049 [0.034]	-0.097 [0.019]***	-0.094 [0.028]***	-0.092 [0.019]***	-0.088 [0.026]***
Log (mean pc income)	0.001 [0.006]	-0.002 [0.007]	0.005 [0.007]	0.003 [0.007]	0.003 [0.007]	0.002 [0.006]
Round and Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84	84	99	99	99	99
R-squared, within	0.44		0.4		0.4	
F-stat for instruments (1st stage)		14.5		9.0		6.5

Note: Robust standard errors in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.



**Table 6: Regulation, Pollution and Public Health**

Panel A: Regulation and Pollution						
Dependent variable - log emissions of contaminants into the atmosphere						
	1	2	3	4	5	6
	OLS	2SLS	OLS	2SLS	OLS	2SLS
More than one window for registration	0.009 [0.098]	-0.088 [0.222]				
Log (number of illegitimate licenses)			0.217 [0.251]	-1.27 [0.815]		
More than one sanitary inspection					-0.521 [0.499]	-0.924 [1.169]
Log (population)	1.71 [1.048]	1.616 [1.182]	0.417 [0.452]	0.638 [0.440]	0.511 [0.273]*	0.501 [0.266]*
Log (mean pc income)	-0.354 [0.200]*	-0.394 [0.227]*	-0.256 [0.178]	-0.394 [0.290]	-0.282 [0.197]	-0.281 [0.196]
Round and Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84	84	99	99	99	99
R-squared, within	0.99		0.08		0.08	
F-stat for instruments (1st stage)		14.46		9		6.5
Panel B: Regulation and Public Health						
Dependent variable - morbidity from injuries and poisoning per 1,000 people						
	7	8	9	10	11	12
	OLS	2SLS	OLS	2SLS	OLS	2SLS
More than one window for registration	0.20 [2.04]	2.47 [4.15]				
Log (number of illegitimate licenses)			-3.12 [5.34]	22.77 [12.06]*		
More than one sanitary inspection					-8.20 [10.61]	-14.37 [40.11]
Log (population)	-63.3 [21.9]***	-61.2 [20.9]***	-87.9 [9.6]***	-91.7 [7.5]***	-87.8 [9.6]***	-87.4 [7.9]***
Log (mean pc income)	-8.4 [4.2]*	-7.4 [3.8]**	-7.5 [3.8]*	-5.1 [3.4]	-7.2 [3.8]*	-7.3 [3.3]**
Round and Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84	84	99	99	99	99
R-squared, within	0.98		0.61		0.61	
F-stat for instruments (1st stage)		14.46		9		6.5

Note: Robust standard errors in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

## A Appendix: Data on Institutions

Summary statistics for all institutional measures are presented in Panel B of Table A.1.

### The measures of local accountability

*Government transparency:* The regional indices of government transparency come from an independent informational agency “Strana.ru” and an independent association of journalists “Media Soyuz.” In the paper, we report results for the overall transparency of authorities; the results using the other measures of government transparency are very similar. These indices were constructed on the basis of a survey of more than a thousand prominent regional journalists who were asked to evaluate performance of the regions along the following dimensions: accessibility and accuracy of information about decisions of a particular regional authority, impartiality and easiness of journalist accreditation rules, quickness of response on journalist inquiries, presence and quality of internet site, etc. The transparency ratings are available at [www.strana.ru/print/128316.html](http://www.strana.ru/print/128316.html).

*Corruption:* An index of regional corruption was constructed by Transparency International jointly with the Information for Democracy foundation (INDEM) on the basis of a an opinion survey among regionally-representative samples of managers of small and medium-size firms and of population about their perceptions of corruption. As our measure of control over corruption we take (1-“corruption volume”), with “corruption volume” variable available at [www.anti-corr.ru/rating\\_regions/index.htm](http://www.anti-corr.ru/rating_regions/index.htm).

*Independent media sources:* We use several alternative measures of the access of the public to independent media. First, the internet penetration variable — the number of personal computers connected to internet per 100 employees — comes from the official Russia’s statistical agency (Rosstat). Second, we use a dummy that indicates regions with non-zero subscription to the two main independent (in 2000) daily newspapers — *Kommersant* and *Vedomosti*. These data come from their respective websites, [www.kommersant.ru](http://www.kommersant.ru) and [www.vedomosti.ru](http://www.vedomosti.ru). Third, we use a dummy for availability of the signal in the region of the largest independent radio station — *Echo Moscow*. These data come from the radio’s website, [www.echo.msk.ru](http://www.echo.msk.ru). Fourth, we also take an index of regional media freedom collected and published by the nongovernmental organization “Public Expertise,” which measures restrictions in regional legislation on information dissemination through the media. This rating can be found at [www.freepress.ru/arh\\_e.shtml](http://www.freepress.ru/arh_e.shtml). As a baseline, we report results with internet penetration; the results for the *Vedomosti* and *Kommersant* subscriptions and for the *Echo Moscow* coverage are very similar to the results for internet penetration. We have no significant results for the media freedom index.

### The measures of industrial lobbying

We use three alternative variables to proxy for the political power of industry incumbents. Each of these proxies is imperfect. Yet, even though they are constructed in different ways and from different data sources, they are correlated and produce similar results. Thus, we are reasonably confident that these measures pick up the effect of lobbying by politically-powerful firms. The first two measures are the concentration (Herfindahl-Hirschman) indices of sales and of employment among industrial firms in each region. The logic behind the choice of industrial concentration as proxy for the strength of industrial lobbying is as in Grossman and Helpman (1994). The source of these data is the Russia’s Industrial Registry. The third proxy is a measure of regional regulatory capture constructed by and described in Slinko, Yakovlev and Zhuravskaya (2005). This is the concentration of preferential treatments (i.e., subsidies, tax breaks, etc.) given to large firms in each region by the regional laws and regulations. This variable reflects the extent to which political power is concentrated in the hands of a few large firms. In the paper, we report results using the HHI of employment, but the results using other proxies are similar.

### The measure of fiscal incentives

The share of own budgetary revenues in the total regional budget is used as a simple (and rather crude) proxy for the regional fiscal incentives. The data come from the Treasury of the Russian Federation ([www.roskazna.ru/reports/mb.html](http://www.roskazna.ru/reports/mb.html)).



**Figure A.1:** The level of regulation in each regulatory area by region and survey round. The numbers on the horizontal axis correspond to regions: 1-Komi Republic, 2-Altaysky Krai, 3-Krasnoyarsky Krai, 4-Primorsky Krai, 5-Khabarovskiy Krai, 6-Amurskaya Oblast, 7-Kaluzhskaya Oblast, 8- Kurganskaya Oblast, 9-Moskovskaya Oblast, 10-Nizhegorodskaya Oblast, 11-Novosibirskaya Oblast, 12-Permskaya Oblast, 13-Rostovskaya Oblast, 14-Samarskaya Oblast, 15- Saratovskaya Oblast, 16-Sakhalinskaya Oblast, 17-Smolenskaya Oblast, 18-Chelyabinskaya Oblast, 19-Moscow City, 20- St.Petersburg City. "X" denotes missing data.

**Table A.1: Summary Statistics**

Variable	Obs	Mean	Std. Dev.	Min	Max
<b>Panel A: Regulation measures</b>					
Log number of sanitary inspections	9046	0.222	0.442	0	3.932
More than one sanitary inspection	9046	0.080	0.271	0	1
Log number of illegitimate licenses	11246	0.241	0.554	0	5.889
Presence of illegitimate licenses	11246	0.216	0.412	0	1
Log number of windows for registration	827	1.390	0.558	0	3.045
More than one window for registration	827	0.703	0.457	0	1
Too short term of license validity	5058	0.336	0.472	0	1
Minus log (term of license validity)	5058	-3.606	0.625	-5.889	-0.693
Log days for registration	820	2.711	0.843	0.262	5.903
More than one week to register	850	0.729	0.445	0	1
Overall regulation level (z-scores)	24842	0	1	-2.612	4.863
Overall violations of the law	25250	0.2413	0.4279	0	1
<b>Panel B: Institutional determinants</b>					
Overall transparency of regional authorities	20	7.478	4.014	0.060	15.860
Transparency of executive power	20	4.224	2.248	0.030	8.750
Transparency of legislative power	20	3.254	1.872	0.030	7.110
Transparency of courts	20	2.221	1.615	0.090	6.940
Control over corruption	16	0.358	0.154	0.087	0.669
Concentration of industrial output	20	0.219	0.099	0.122	0.528
Concentration of industrial employment	20	0.178	0.077	0.110	0.385
Concentration of preferential treatments	20	0.535	0.238	0.209	0.907
Internet penetration	20	4.808	3.181	1.800	16.000
Radio Echo Moskvi, signal coverage	20	0.600	0.503	0	1
Vedomosti daily, subscription	20	0.550	0.510	0	1
Kommersant daily, subscription	20	0.450	0.510	0	1
Media freedom index	20	42.040	12.650	18	75
Share of own revenues	20	0.829	0.117	0.592	0.959
Governor from the governing party	20	0.721	0.413	0	1
<b>Panel C: Outcomes</b>					
Small business employment per capita	99	0.053	0.037	0.019	0.200
Log number of small businesses	99	2.560	1.135	0.875	5.282
Log emissions of contaminants	99	5.152	1.172	2.425	7.859
Morbidity from injuries and poisoning per 1000	99	92.085	18.352	54.100	129.000

**Table A.2: Account of results for all specific regulation measures**

Institution:	Variable:	Panel of firms		X-section of firms	
		Coef	SE	Coef	SE
Transparency	more than one sanitary inspection	-0.003	[0.002]*	-0.004	[0.001]***
	log number of sanitary inspection	0.002	[0.002]	-0.004	[0.002]**
	log number of illegitimate licenses	-0.006	[0.002]***	-0.008	[0.003]**
	presence of illegitimate licenses, old firms	-0.007	[0.002]***	-0.004	[0.001]***
	presence of illegitimate licenses, new firms			-0.009	[0.004]**
	minus log term of license validity	0.002	[0.002]	-0.003	[0.004]
	too short length of license validity	0.000	[0.004]	0.001	[0.003]
	log number of windows for registration			-0.014	[0.014]
	more than one window for registration			-0.023	[0.011]**
	log number of days for registration			0.045	[0.023]*
Internet	more than 5 days for registration			0.017	[0.011]
	more than one sanitary inspection	-0.004	[0.002]**	-0.002	[0.002]
	log number of sanitary inspection	0.003	[0.003]	0	[0.002]
	log number of illegitimate licenses	-0.008	[0.003]**	-0.01	[0.005]**
	presence of illegitimate licenses, old firms	-0.008	[0.003]**	-0.006	[0.002]***
	presence of illegitimate licenses, new firms			-0.015	[0.006]**
	minus log term of license validity	0.006	[0.005]	-0.002	[0.008]
	too short length of license validity	-0.003	[0.004]	0.009	[0.005]*
	log number of windows for registration			-0.016	[0.026]
	more than one window for registration			-0.037	[0.022]*
Corruption cont.	log number of days for registration			0.05	[0.056]
	more than 5 days for registration			0.055	[0.017]***
	more than one sanitary inspection	-0.033	[0.046]	0.028	[0.043]
	log number of sanitary inspection	-0.111	[0.059]*	-0.092	[0.046]**
	log number of illegitimate licenses	-0.041	[0.068]	-0.175	[0.085]**
	presence of illegitimate licenses, old firms			-0.109	[0.039]***
	presence of illegitimate licenses, new firms	-0.129	[0.066]**	-0.011	[0.105]
	minus log term of license validity	0.049	[0.106]	0.229	[0.122]*
	too short length of license validity	-0.071	[0.069]	0.072	[0.074]
	log number of windows for registration			-0.129	[0.285]
Ind. concentration	more than one window for registration			-0.177	[0.222]
	log number of days for registration			-0.493	[0.348]
	more than 5 days for registration			0.234	[0.205]
	more than one sanitary inspection	-0.161	[0.067]**	-0.118	[0.050]**
	log number of sanitary inspection	-0.017	[0.088]	-0.204	[0.065]***
	log number of illegitimate licenses	-0.315	[0.113]***	-0.474	[0.209]**
	presence of illegitimate licenses, old firms	-0.313	[0.112]***	-0.097	[0.054]*
	presence of illegitimate licenses, new firms			-0.478	[0.190]**
	minus log term of license validity	-0.071	[0.158]	-0.165	[0.180]
	too short length of license validity	-0.067	[0.085]	-0.192	[0.179]
Fiscal incentives	log number of windows for registration			-0.732	[0.454]
	more than one window for registration			-0.934	[0.314]***
	log number of days for registration			-0.45	[0.722]
	more than 5 days for registration			0.7	[0.497]
	more than one sanitary inspection	-0.119	[0.049]**	-0.104	[0.046]**
	log number of sanitary inspection	-0.082	[0.062]	-0.176	[0.054]***
	log number of illegitimate licenses	-0.067	[0.084]	-0.217	[0.102]**
	presence of illegitimate licenses, old firms	-0.08	[0.079]	-0.091	[0.049]*
	presence of illegitimate licenses, new firms			0.108	[0.113]
	minus log term of license validity	-0.098	[0.124]	-0.107	[0.126]
too short length of license validity	-0.046	[0.073]	-0.03	[0.091]	
log number of windows for registration			-0.572	[0.335]*	
more than one window for registration			-0.815	[0.314]**	
log number of days for registration			0.367	[0.560]	
more than 5 days for registration			-0.162	[0.279]	

**Table A.3: The first stage**

	More than one window for registration	Log(number of illegitimate licenses)	More than one sanitary inspection
Fiscal Incentives * AFTER	-1.479 [0.389]***		
Transparency * AFTER		-0.01 [0.003]***	-0.006 [0.003]*
Initial regulation * AFTER			-0.542 [0.166]***
Log (population)	-0.169 [1.279]	0.373 [0.213]*	0.138 [0.101]
Log (mean pc income)	-0.612 [0.242]**	-0.055 [0.079]	-0.003 [0.039]
Round FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Observations	84	99	99
R-squared	0.62	0.53	0.71
F-stat for excluded instruments	14.5	9.0	6.5
	Test of overidentification restrictions		
Dependent variable:	p-value of Hansen's J-test:		
Log number of small businesses	0.62	0.51	0.49
Small business employment per capita	0.42	0.19	0.43
Log emissions	0.04	0.13	0.75
Morbidity from injuries per 1,000 people	0.11	0.04	0.04

Note: Robust standard errors in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. The choice of a particular set of instruments is guided by maximization of the F-statistic for the excluded instruments.