Enforcement Capacity and the Impact of Labor Regulation

Evidence from the Russian Federation

Alvaro Gonzalez
Siddharth Sharma
Hari Subhash

WORLD BANK GROUP

Jobs Cross Cutting Solution Area

&

Trade and Competitiveness Global Practice Group

November 2016

Abstract

The impact of business regulations on firms could depend on how the regulations are enforced in practice. Exploiting variation in enforcement capacity across the Russian Federation's administrative regions, this paper examines whether the enforcement of restrictive regulations on hiring and firing workers affects how firms adjust employment during industry upswings and downswings. The analysis finds that the extent to which firms adjust employment upward during industry upswings and downward during

downswings is smaller in regions with stronger enforcement capacity (or stricter de facto employment protection). The effect of enforcement is sizable: for example, increasing enforcement capacity from the 25th to the 75th percentile dampens employment adjustment in a downswing by 34 percent. Thus, although restrictive regulation on hiring and firing reduces the ability of firms to adjust employment, the extent to which it does so depends on enforcement.

This paper is a product of the Jobs Cross Cutting Solution Area and the Trade and Competitiveness Global Practice Group. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at <http://econ.worldbank.org>. The authors may be contacted at ssharma1@worldbank.org.

The Policy Research Working Paper Series disseminates the findings of work in progress to encourage the exchange of ideas about development issues. An objective of the series is to get the findings out quickly, even if the presentations are less than fully polished. The papers carry the names of the authors and should be cited accordingly. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

Enforcement Capacity and the Impact of Labor Regulation: Evidence from the Russian Federation

Alvaro Gonzalez,¹ Siddharth Sharma² and Hari Subhash^{3,4}

JEL: O12, D22, K31

Keywords: Firms; Labor regulation; Enforcement; Employment adjustment

¹ The World Bank

² The World Bank. Corresponding author. Email- ssharma1@worldbank.org

³ The World Bank

⁴ The authors wish to thank John T. Giles, Francesca de Nicola, Leonardo Iacovone and Luis-Felipe Lopez-Calva for their suggestions.

1. Introduction

On paper, many countries have stringent regulations governing the hiring and firing of workers. For instance, there are almost 90 countries in which a firm is required to pay ten weeks or more of salary as severance pay when dismissing an employee for redundancy (World Bank, 2016). Such regulations are the subject of a growing body of research which, like research on other business regulation, is based largely on *de jure* measures. However, survey data suggest that the enforcement of business regulations in low and middle income countries is often weak and uncertain (Hallward-Driemeier and Pritchett, 2011). Is this poor enforcement related to weak enforcement capacity, and does it matter to how firms respond to regulations?

The Russian Federation's labor code provides a good setting for examining this question. Its provisions governing hiring and layoffs are strict by international standards. While the labor code is the same countrywide, there is evidence that government capacity to enforce it varies across the regions of the Federation. For example, the number of labor inspectors per hundred thousand covered employees varies from a minimum of 11 to a maximum of 50 across regions. Similarly, the capacity of courts to handle labor disputes also varies across regions (Gimpelson et al., 2010; Gimpelson and Kapeliushnikov, 2008). In general, regulatory enforcement is weak in Russia; for example, its score in the World Justice Project's "Effective Regulatory Enforcement Index" is 0.5, while most high income countries have a score near 0.8.⁵

In order to test if enforcement capacity matters to firm behavior, we draw on a fundamental theoretical prediction about the effect of regulations that raise the cost of hiring and firing workers. The prediction is that such rules should reduce employment adjustment in response to shocks, not only decreasing the number of layoffs during upturns, but also dampening hiring in upturns because of direct hiring costs and the potential cost of having to lay off newly-hired workers in the future (Oi, 1962; Nickell, 1986; Hamermesh, 1993). The enforcement of such rules too matters here because the expected cost of labor adjustment depends on the firm's assessment of the probability that the law will be enforced.

Our empirical strategy for testing this prediction is the following. We first identify episodes of upturns (surges) and downturns (slumps) in different manufacturing sectors (relative to their trend growth rates) from time series data on industry output. Using a large firm-level panel data set that includes firms from 79 regions of the Russian Federation, we then test if firms' employment adjustment during these episodes is smaller in regions with better capacity to enforce labor laws.

As expected, the regression results show that compared to normal years, the annual growth in firm revenue and employment is significantly higher during surges, and lower during slumps. But unlike revenue, the degree to which employment adjusts upwards during upswings and downwards during downswings is significantly smaller in regions with stronger enforcement capacity. This suggests that the extent to which employment protection laws dampen labor adjustment by firms depends on enforcement capacity.

⁵ World Justice Project data can be found at <http://data.worldjusticeproject.org/#groups/AUT>. The indices are based on surveys of households and experts across countries.

The results hold up to a series of robustness checks. Since the incidence of surges and slumps varies across industries and over time, we can control for aggregate shocks and firm fixed effects (FEs) when estimating how firms adjust employment in surges and slumps. We also show that the results are robust to using alternative measures of enforcement. Another concern is that differences in how firms respond to shocks reflect systematic regional differences in the attributes of firms. The results, however, are robust to allowing firm responses to vary by observable attributes like sector, size, age and capital intensity.

One feature of the study's institutional setting, which is good for our empirical strategy, is that the variation in enforcement capacity across Russian regions is arguably exogenous to regional labor market conditions. This is because labor inspectors are allocated to regions by the center and on the basis of uniform rules based on regional population size (Gimpelson and others, 2010). Yet, the concern remains that regional enforcement capacity somehow happens to be correlated with unobserved regional attributes that influence employment adjustment. We address this concern by exploiting within-region variation in the enforcement of labor laws. In Russia, authorities in charge of monitoring compliance focus their efforts on firms above a certain size threshold ('Medium and Large Firms'). Consistent with this, our analysis suggests that regional enforcement capacity has a bigger impact on firms above this threshold. Correlated unobserved regional attributes are thus unlikely to be driving our results, unless they too have a disproportionate effect on firms above this size threshold.

Empirical research into the impact of labor regulations has examined their impact on a range of outcomes: labor adjustment, job flows, unemployment, total employment, wages and firm performance.⁶ But with most existing studies focusing on regulations as they are written, enforcement is a largely overlooked issue. The few studies which do look at enforcement suggest that it is unequal within countries, and that it matters. Almeida and Carneiro (2007 and 2012) exploit variable enforcement of labor law in Brazil to show how stronger labor law enforcement is associated with firm performance and a range of labor market outcomes such as unemployment and informality.⁷ Gimpelson et al. (2010) similarly document regional variation in the enforcement of Russian labor laws, and link it to labor market outcomes. While not on labor laws *per se*, Hallward-Driemeier and Pritchett (2011) is also notable as it documents a gap between business licensing procedures and their application using firm-level data from a set of low and middle income countries. Yakolev and Zhuravskaya (2013) present evidence that discretionary amendments by regional authorities in Russia diluted the enforcement of reforms intended to liberalize business regulation. Our paper thus adds to growing evidence on the significance of regulatory enforcement, and in particular on the role of institutional capacity therein.

Another contribution of our study is to use sub-national variation in enforcement to better identify the impact of labor regulations. Labor laws rarely vary within country, with the result that much of the

⁶ For example, Bentolila and Saint-Paul, 1992; Blanchard and Wolfers, 2000; Botero et al., 2004; Autor et al., 2007; Krishna, Robles, and, Dougherty 2011; Aghion and others; 2008; Haltiwanger et al., 2010.

⁷ While not focusing on enforcement, Tella and McCullough (2005) use measures of labor regulations based on firm perceptions, which are likely to reflect firms' perceptions of the written law as well as its enforcement. Also, Pierre, Gaelle, and Stefano Scarpetta (2006) show that looking across countries, firms' perceptions about labor laws match their *de jure* stringency. Their result is not inconsistent with variable enforcement of labor laws within each country. It also suggests that enforcement is not systematically weaker in countries with more stringent laws.

literature is based on cross-country analysis, where it is difficult to control for confounding factors. Only a few studies have been able to exploit within-country variation in the stringency of labor regulations, such as those arising from variation across sub-national entities (Aghion et al., 2008; and Adhvaryu et al. 2014), across firm categories (Boeri and Jimeno, 2005) or over time (Bentolila and Saint-Paul, 1992; Aghion et al., 2008). In this respect, our study is closest to Adhvaryu et al. (2013), which shows that Indian states with stricter employment protection experience lower employment adjustment in manufacturing firms in response to weather shocks. We differ from that study in that our source of variation is not *de jure* but rather, *de facto*.

Our study is also relevant to the new literature on firm dynamics and misallocation in low and middle income countries.⁸ An emerging finding in this literature is that compared to firms in developed economies, firms in low and middle income countries do not increase their productivity and grow as they age (Hsieh and Klenow 2014). It is hypothesized that this is due to misallocations that harm large firms, and discourage investments in raising plant productivity. Our finding that the enforcement of stringent labor laws which constrain labor adjustment is focused on large firms is consistent with this hypothesis, and thus identifies a particular channel to explain firm dynamics in low and middle income countries.

The rest of the paper is organized as follows. Section 2 gives the institutional background for this paper, describing labor regulations and how they are enforced in Russia. These details are important to understand how we exploit regional variation to identify the empirical results of the paper. Section 3 describes the data, followed by an explanation of our empirical strategy in Section 4. Section 5 presents the results, and Section 6 concludes, encapsulating what we have learned and providing possible directions for future research.

2. Institutional setting: Labor regulations and their enforcement in Russia

The Russian Federation has comprehensive employment protection laws to regulate the labor market. The current version, enacted in 2002, is mainly adapted from Code of Laws for Labor from Soviet times, though multiple revisions have been made. Whereas the stated purpose of the revisions is to render more flexibility in the labor market, the actual law is still relatively restrictive by international standards. The severance pay requirements and regulation of fixed-term contracts in Russia are much more rigid than those in the OECD or many middle-income countries.

The OECD index of employment protection represents a composite indicator of the strictness of labor regulations that primarily takes into account employers' firing costs. The index is calculated by assigning a score varying from 0 (no regulations) to 6 (most rigid regulations) to specific provisions in national labor law and averaging them using a system of weights (OECD, 2014).⁹ The OECD score puts Russia almost twice as high as average OECD countries (3.6 versus 2.0).

⁸ See, for example, Hsieh and Klenow 2009 and 2014; Hsieh and Olken, 2014.

⁹ Details of the methodology can be found at <http://www.oecd.org/els/emp/EPL-Methodology.pdf>.

In addition, according to the ‘Employing Workers’ indicator of the *Doing Business* report, during 2006-2009, Russia was approximately in the bottom third of countries in terms of costs and other restrictions imposed on firms making changes to their labor force (World Bank, 2010).¹⁰ For instance, in the specific case study considered by the *Doing Business* report, the law would imply a redundancy cost of 13-14 weeks’ worth of a worker’s salary during this period. It is also difficult to hire workers on fixed term or flexible contracts. Fixed term contracts are prohibited for “permanent tasks”, and the maximum length of fixed term contracts is 60 months, while other flexible forms of labor contracting are restricted or not allowed (World Bank, 2014).

However, unlike Western Europe or the United States where labor laws are strictly enforced, Russia has much more variance and flexibility in the enforcement of labor regulations across regions and firms. As described in Gimpelson et al. (2010), there are three dimensions of variation in the enforcement; namely, the institutional capacity of law enforcement agencies, the demand for enforcement, and the coverage of employed population.

The institutional capacity to enforce labor laws depends, among other factors, on the capacity of the regional labor inspectorates (such as the number of inspectors relative to the employment covered). It also depends on the capacity of courts to deal with labor disputes filed to the judiciary system, indicated by the number of judges available for trying labor disputes or the costs to plaintiffs.

Our main measure of enforcement capacity is the staff size of the State ‘Labor Inspectorate’ (LI), a federal agency with regional offices. The inspectorate is charged with monitoring the enforcement of all labor regulations concerning hiring, firing, pay, and safety. The Russian Labor Code gives the inspectorate significant discretion in monitoring, and also some executive authority to take legal action in case of noncompliance. Another feature of LIs that is critical for the purpose of our research design is that the size of the regional LI office is determined on the basis of population, and is therefore not always proportionate to the number of firms in the region. This causes regional variation in the ratio of labor inspectors to the number of firms monitored. This mismatch in the staffing of regional LIs is partly reflective of institutional weaknesses inherited from the Soviet past which have not been reformed.

The demand for enforcement of employment protection laws depends on the capacity and propensity of workers and trade unions to raise their voice for better enforcement, which could vary across regions due to a range of cultural and historical factors.

Though the Labor Code generally applies to all firms, and the formal jurisdiction of the LIs extends across firms of all sizes, there is much qualitative evidence to suggest that in practice, the probability of inspection depends on the size of the firm. In particular, regulatory authorities focus inspections on firms categorized as ‘Large and Medium Firms’. Unlike such firms, ‘Small’ firms and individual entrepreneurs are not required to submit monthly statistical reports to authorities. The strict definition of what constitutes a ‘Large and Medium Firm’ is complex, but as a general rule the size cut off is 50 workers.

¹⁰ This cross-country ranking of labor laws was discontinued by the *Doing Business* report 2010 onwards.

The demand for enforcement also might be higher for large and medium firms due to higher labor union representation. While the trade union density or coverage of workers has reduced from close to 100% in the Soviet era, it still remains high for large and medium firms. For instance, the largest trade union in Russia, FNPR, claims to represent 70 percent of the workers in the large and medium firms. Small firms, on the other hand, have few workers who are represented by the union (Gimpelson 2010; Ashwin and Clarke, 2002).

The Labor Code itself has specific restrictions that apply to larger firms. Article 217 of the code stipulates that all organizations with over 100 employees need to either setup a labor protection service or employ a labor protection expert. In addition, these organizations need to provide an elective trade union body and a “room and appliances” for its functioning, all free of charge (Labor Code, 2002).

3. Data

3.1 Labor regulation enforcement

The measures of labor law enforcement used in this paper were originally published in Vishnevskaya and Kapelyushnikov (2007), and subsequently discussed and analyzed in more detail in Gimpelson et al. (2010). The measures reflect the capacity of labor inspectorates and courts to enforce the regulation or judge disputes, and the demand/voice for enforcement as expressed by the number of cases filed. The authors collected these data for the years 2000-2005 from the records of the Federal Labor Inspectorate and the Judicial Department of the Supreme Court. The measures are averaged over 2000-2005, and as suggested by the authors, expressed per hundred thousand employees in large and medium firms, the population effectively covered by the regulation. Table 1 summarizes these measures, which cover 79 regions.

Our preferred measure of enforcement capacity is the number of labor inspectors per hundred thousand employees in large and medium firms. This captures the capacity of the labor inspectorate. Another such measure, the density of inspections, is highly correlated with the density of inspectors and does not add much information (Table 2).

We also present regressions in which enforcement is measured by the number of dismissal dispute cases filed (by workers) per hundred thousand employees in large and medium firms. This a broader measure as case filing by workers should, among other factors, depend on their perceptions of how efficiently the legal system will deal with a dismissal related dispute. It could also reflect a perceived pro-worker bias of local courts. Higher worker demand for legal redress is also expected to raise firms’ assessment of the probability of enforcement of severance payments.

Using this alternative measure does not change our main results. This is not surprising because it is highly correlated with the density of inspectors. Though it adds another dimension to the measurement of enforcement and as such, serves as a robustness check, it is not our preferred measure. Its interpretation is less clear than that of LI capacity; moreover, the number of cases filed could mechanically depend on

the incidence of dismissals, which is endogenous to labor regulation. The same concerns hold for another available indicator, the number of detected violations per thousand employees. We do not use the other available measures (“all cases filed” and “pay cases filed”), because they are not specifically about hiring and firing disputes.

3.2 Firm-level data

Our regressions use panel data from the RUSLANA database (from Bureau van Dijk, BvD). RUSLANA is an extensive data set that provides up to 10 years of financial, administrative, locational and managerial information on about 65,000 registered firms in Russia. The data are based on firms’ reporting to various administrative databases such as the tax directorate and social security.

According to BvD, every effort is made to ensure comprehensive coverage of these official databases, so that RUSLANA may include all registered and active firms. The fact remains that RUSLANA coverage is dependent on the extent to which firms report to such official databases. As we understand, a specific issue with coverage is that some firms ‘enter’ and ‘exit’ RUSLANA with a lag relative to their actual dates of market entry and exit, possibly due to lagged updating of official databases. The lag in entry into the database, in particular, indicates that RUSLANA has more selective coverage of newly established firms. The selection process is unobserved, in the sense that we lack information on why some new firms enter the database with a lag, while others do not. It could be a concern for our estimation if it varies systematically across provinces, and if the employment adjustment behavior of such selectively excluded firms is systematically different from that of other firms.¹¹

The data set we use is the 2012 ‘version’ of RUSLANA, meaning that it was last updated by BvD in 2012, and spans the 1999-2011 period. Due to relatively poorer availability of employment data in years prior to 2003 and in 2011,¹² we limit our analysis to 2004-2010. In addition, all observations with negative or missing values for any of the following variables used on our analysis—age, operating revenue, tangible fixed assets and number of employees—were dropped; observations with missing profit value were also dropped. Outlier values (outside the 1-99 percentile range) of key variables (such as the level and change in employment and revenue) were Winsorized (top-coding and bottom-coding the 1 percentile tails).

3.3 Measuring shocks to industrial sectors

We classify large deviations from the trend annual growth rate of a manufacturing sector as a shock episode. For each 4-digit level sector, a global trend growth rate was measured using long-term, cross-country data on industrial output.¹³ A sector is defined as being in ‘surge’ (resp. ‘slump’) when the difference between its actual sales growth rate and the trend growth rate is larger (resp., smaller) than a pre-specified cutoff value (resp., the 75th and 25th percentile values of the distribution of the trend growth

¹¹ Concerns around exit and panel definition are addressed in Section 4.1.

¹² The gaps in year 2011 data are likely due to updating lags in the official databases which BvD uses to compile RUSLANA.

¹³ Only countries in the same per capita income quartile as Russia were included, on the assumption that they would share similar technological levels and global markets with Russia.

rate). A sector can be in three states: surge, slump or 'normal' rate of growth. We conduct this analysis for approximately 80 4-digit level manufacturing sectors. Details are provided in Annex 1.

3.4 Summary statistics

Table 3 presents correlation coefficients between our regional enforcement capacity measure (inspectorate density) and key regional characteristics such as number of firms and gross regional product (GRP). The relatively high positive correlation (0.6) between enforcement and the regional unemployment rate stands out. The fact that regions with stronger enforcement capacity have higher rates of unemployment is consistent with theoretical predictions about the effect of stringent labor laws; however this is not a hypothesis we can convincingly test given the data and setting.

Regions with stronger enforcement capacity are somewhat less ‘industrial’: they have fewer firms, a smaller share of manufacturing and a larger share of services in total output. This is consistent with our claim that labor inspectorate density is determined by an ‘exogenous’ population based rule, as that rule would automatically leave more industrialized regions with lower inspectorate density (unless regional population size is correlated with industrialization).

Regions with stronger enforcement capacity are (weakly) less urbanized. Other attributes like regional GDP per capita and nominal wages are uncorrelated with enforcement capacity. Thus, with the exception of unemployment, urbanization and industrialization, regions with high and low inspectorate density look broadly comparable.

Table 4 summarizes the cleaned RUSLANA panel data by year, showing the number of firms, how many of those are “entering” RUSLANA that year, and the breakdown of firms by sectors in surges and slumps. We note, again, that “entry” into RUSLANA is not an accurate measure of firm entry into the market. Regarding surges and slumps, the data show that 2006-2009 were volatile years. A large share of firms were in surging sectors in 2006 and 2007, and then slumping in 2008 and 2009; the latter reflects the onset of the global recession in those years.

Table 5 summarizes the panel of incumbent firms that we use for most of our analysis; the statistics for each year t include only those firms that existed in the panel and have data in both year $t-1$ and t . In a given year, the mean employment level in a firm is around 100, and about 2-5 jobs are added per firm, on average. But there is significant cross-sectional variance in these variables. The mean age of firms ranges between 8-10 years across the years.¹⁴

¹⁴ It is notable that the mean age rises between 2004 and 2010. We suspect it is because RUSLANA has a procedure of dropping firms that stopped operating in early years from the panel altogether. That is, if a firm exited in 2005, it may not exist in the panel at all. This rule is not clearly documented, but we suspect that it results in systematic underrepresentation of older firms in earlier years.

4. Empirical Strategy

4.1 Main specification

We use firm-level panel regressions to examine the association between sector level shocks and employment growth in firms. The base regression specification estimates the average effect of shocks using OLS:

$$\Delta Employment_{ist} = \alpha Surge_{st} + \beta Slump_{st} + Year_t + \Phi_{isrt} + e_{ist} \quad (1)$$

The regression is estimated on an unbalanced panel of incumbent firms (that is, in a year t , it includes all firms that exist in RUSLANA in year t as well as $t-1$). $\Delta Employment_{ist}$ is the change in employment in firm i (in sector s , region r) between year t and $t-1$ (that is, growth in absolute terms). $Surge_{st}$ (resp., $Slump_{st}$) are dummies indicated whether sector s was in surge (resp., slump) in year t . The dummy for a ‘normal’ year for a sector is omitted. The regression include year fixed effects to account for macro shocks that are common across sectors, and firm fixed effects (Φ_{isrt}) to control for time-invariant unobserved differences in firms. Hence, the effect of a shock is identified from the correlation between shocks and within-firm variation in employment growth. The coefficient on *surge* (resp., *slump*) captures the association between sectoral surge (resp., slump) and firm employment growth relative to a ‘normal’ year when its sector is in neither surge nor slump.

The standard errors are clustered by sector to account for cross-sectional and serial correlation in errors within sectors, a conservative clustering scheme.

A positive coefficient on *Surge* would indicate that relative to a normal year, employment growth slows down during a surge. Analogously, a positive coefficient on *Slump* would indicate that relative to a normal year, employment growth slows down during a slump.

Building on the base specification, our subsequent specification examines how the association between shocks and employment increase varies by regional labor regulation enforcement:

$$\begin{aligned} \Delta Employment_{ist} = & \alpha Surge_{st} + \beta Slump_{st} + \lambda Surge_{st} * Enforcement_r + \mu Slump_{st} * Enforcement_r \\ & + Year_t + \Phi_{isrt} + e_{ist} \end{aligned} \quad (2)$$

$Enforcement_r$ is the (time-invariant) labor regulation enforcement measure for region r . The coefficient on $Surge * Enforcement$ (resp., $Slump * Enforcement$) measures how the association between a surge (resp., slump) and firm employment growth varies by the regional enforcement level.

Suppose that the average effect of a surge (as measured by α in Equation (1)) is estimated to be positive. Then, the interpretation of a negative coefficient on $Surge * Enforcement$ is that the positive association between surges and employment growth is weaker in provinces with stronger labor regulation enforcement. Analogously, suppose that the average effect of a slump (as measured by β in Equation (1)) is estimated to be negative. Then, a positive coefficient on $Slump * Enforcement$ can be interpreted to indicate that the negative association between slumps and employment growth is weaker in provinces

with stronger labor regulation enforcement. In other words, a negative coefficient on *Surge*Enforcement* and a positive one on *Slump*Enforcement* are consistent with the hypotheses that stronger enforcement attenuates the employment response to shocks.

The identification assumption behind our strategy is that unobserved determinants of how firms adjust employment in response to shocks are not correlated with the enforcement of labor laws across provinces. One concern with this arises from the fact that the distribution of firms by sector varies across provinces. It could be a ‘surge’ in Sector X is somehow different from one in Sector Y. It is also possible that how a firm in Sector X responds to a ‘surge’ in that sector is inherently different from how a firm in Sector Y responds to a surge in that sector. If so, cross-province variation in how firms respond to shocks could reflect cross-province variation in firms’ sectors. Hence, we would like to focus on estimating variation in the response to shocks among firms belonging to the same sector, experiencing the same sector-wide shock. To do so, our preferred specification includes sector-year FEs rather than just year FEs.

Cross-province variation in how firms respond to shocks could also reflect systematic differences in the types of firms across provinces; specifically, it could reflect inherent differences in the elasticity of employment growth to shocks. Therefore, in a successive robustness check, we add interactions of firm-level baseline (year $t-1$) variables with *Surge* and *Slump* to control for size, age and capital intensity related differences in how a firm responds to shocks.

4.2 Exploiting size-related variation in enforcement

Russian provinces could differ in unobservable attributes that also influence how firms adjust employment in response to shocks. For instance, the response to shocks may depend on access to finance, which could vary across provinces. Our empirical strategy would be in doubt if such unobserved attributes were correlated with labor regulation enforcement.

To address this concern about correlated unobserved province attributes, we exploit the *de facto* variation in labor law enforcement across firms of different sizes. As discussed in Section 2, labor inspectorates focus on firms classified as ‘Medium’ and ‘Large’ firms. Increased enforcement capacity would therefore matter disproportionately to such firms. Hence, the observed association between province-level enforcement capacity and firms’ response to shocks is expected to be significantly stronger among medium and large firms. Unless there are correlated unobservable province attributes that similarly dampen employment adjustment to a greater extent among Medium and Large firms, this pattern must be due to enforcement capacity.

Gimpelson et al. (2010) note that based on the official definition of a Medium firm, a firm size of 50 employees can be considered as a rough threshold below which labor regulations are unenforced in practice. Building on this, we use the following specification to examine if the relationship between province enforcement level and employment response is stronger among firms of size 50 and above:

$$\Delta \text{Employment}_{isrt} = \alpha \text{Surge}_{st} + \beta \text{Slump}_{st} + \lambda \text{Surge}_{st} * \text{Enforcement}_r + \mu \text{Slump}_{st} * \text{Enforcement}_r + \eta_1 \text{Surge}_{st} * \text{Enforcement}_r * \text{Large}_{isrt} + \eta_2 \text{Slump}_{st} * \text{Enforcement}_r * \text{Large}_{isrt} + \text{Year}_t + \Phi_{isrt} + e_{ist} \quad (3)$$

$Large_{isrt}$ is a dummy indicating whether a firm had more than 50 employees in the previous period. The specification includes second-order interactions of *Surge*, *Slump* and *Enforcement* with *Large*, omitted here for brevity.

The variables of interest are the triple interaction terms between shock measures, enforcement and the indicator for being a large firm. A negative coefficient on $Surge*Enforcement*Large$ would indicate that stronger enforcement attenuates the positive effect of surges to a greater extent among firms with employment above 50. Analogously, a positive coefficient on $Slump*Enforcement*Large$ would indicate that stronger enforcement attenuates the negative effect of slumps to a greater extent among firms with employment above 50.

Since the *Large* dummy is by definition correlated with firm size (employment), measured interactions between enforcement and *Large* could reflect a firm size effect, and not just a threshold effect at size 50. Ideally, therefore, a regression discontinuity design (RDD) around size 50 would be a more convincing way to exploit the size-related variation in enforcement. It would demonstrate that the association between enforcement capacity and the response to shocks is significantly stronger in firms *just* above the threshold of 50 employees, compared to those just below it.¹⁵ But there is an issue with applying RDD methods in this setting: we cannot be certain that there is a *discontinuous* jump in the probability of enforcement at the 50 employee threshold, a precondition for applying RDD techniques.

Given this limitation, the best we can do is to add interactions with a continuous measure of firm size (employment) as controls in Equation (3), and examine if there is still a ‘break’ at size 50:

$$\Delta Employment_{isrt} = \alpha Surge_{st} + \beta Slump_{st} + \lambda Surge_{st}*Enforcement_r + \mu Slump_{st}*Enforcement_r + \eta_1 Surge_{st}*Enforcement_r*Large_{isrt} + \eta_2 Slump_{st}*Enforcement_r*Large_{isrt} + v_1 Surge_{st}*Enforcement_r*Size_{isrt} + v_2 Slump_{st}*Enforcement_r*Size_{isrt} + Year_t + \Phi_{isrt} + e_{ist} \quad (4)$$

$Size_{isrt}$ is the lagged employment level of the firm.¹⁶ As in Equation (3), the variables of interest are the triple interaction terms between shocks, enforcement and the binary indicator for being a large firm. But here they measure the break in the relationship between employment size and $Shock*Enforcement$ around the size threshold of 50 employees.

4.3 Testing for differential firm exit

Since our main regressions are estimated on incumbent RUSLANA firms (those which exist in year t as well as $t-1$) they do not capture employment lost due to exiting firms. This could matter to the interpretation of our results if the rate of exit in periods of shock is systematically different across regions with different enforcement levels. Hence, we also examine if the association between shocks and the pattern of ‘exit’ from RUSLANA differs with enforcement capacity:

¹⁵ The identification assumption in an RDD would be that correlated province attributes do not have a discontinuous jump in their effect on firms at this size threshold.

¹⁶ The specification includes interactions of *Surge*, *Slump* and *Enforcement* with *Large* and with *Size*, omitted here for brevity.

$$Exit_{isrt} = \alpha Surge_{st} + \beta Slump_{st} + \lambda Surge_{st} * Enforcement_r + \mu Slump_{st} * Enforcement_r + Year_t + e_{ist} \quad (5)$$

$Exit_{isrt}$ is a dummy indicating if firm i exits the panel between years t and $t-1$.

5. Results

5.1 Main result: How employment adjustment varies by labor law enforcement

Table 6 presents OLS estimates of Equation (1), measuring the baseline effect of surges and slumps on the annual growth (in absolute terms) in firm employment, revenue and profits. The regressions include year fixed effects, which control for macro shocks common to sectors. In addition, Columns 2, 4 and 6 include firm FEs to account for differences in the fixed characteristics of firms across sector-year cells. Standard errors are adjusted for clustering within sectors (across firms and over time).

As expected, the coefficient on *Slump* is uniformly negative, implying that relative to a normal year, a slump is associated with a significant decrease in the growth of employment, revenue and profits. The implied effect is large. Consider employment, for example. Based on the summary statistics shown in Table 5, continuing firms hired 2.3 additional workers on average between 2006 and 2007. In comparison, as the point estimate of the coefficient on *Slump* implies, a firm in a slumping sector would have hired only 1.7 additional workers. Also as expected, the coefficient on *Surge* is sizably positive for all outcomes, though not significant at the 10% level for change in employment and profit.

Table 7 present OLS estimates of the main specification in this paper, Equation (2), measuring how the association between shocks and employment growth varies by regional labor regulation enforcement. The enforcement measure used in this estimation is the number of labor inspectors per thousand employees in large and medium firms.

In Column 1, showing the basic specification with year and firm FEs, the coefficient on the interaction term *Slump*Enforcement* is positive and statistically significant. This implies that the negative relationship between annual employment growth and slumps is significantly weaker in regions with stronger labor regulation enforcement capacity. Symmetrically, the *Surge*Enforcement* is negative and statistically significant, which means that the positive relationship between annual employment change and surges is significantly attenuated in regions with stronger labor regulation enforcement capacity. Thus, stronger enforcement capacity dampens the employment adjustment to surges and slumps.

The magnitude of the differential with respect to enforcement is significant. According to the baseline estimate presented earlier in Table 6, the average effect of a slump is -0.6. The point estimate of the coefficient on *Slump*Enforcement* in Table 7 implies that the slump effect varies from -0.7 in a region at the 25th percentile of enforcement capacity to -0.5 in a region at the 75th percentile of enforcement capacity. This is a variation of plus/minus 16% in the average effect size of -0.6.

Columns 2-4 repeat this estimation with additional controls, and the results stay qualitatively similar. The regression reported in Column 2 includes sector-year fixed effects in order to identify the effect of

enforcement using only the variation within sector-year cells. This adjusts for regional differences in the distribution of firms by sectors. Column 3 includes interactions of the shock dummies with lagged firm employment level. It could be that employment adjustment by firms depends on the baseline firm size. Hence, this inclusion is to control for any systematic regional differences in the distribution of firms by size. Similarly, Column 3 includes interactions of the shock dummies with the initial age and capital-labor ratio of the firm.

Table 8 displays estimates of the same specification as in Table 7, but with absolute growth in revenue and profits as outcome variables. The key result here—as seen in Column 1 (with firm and year FEs) and Column 2 (with firm and sector-year FEs)—is that the relationship between shocks (surges and slumps) and the growth in revenue does not vary significantly with regional enforcement. This increases our confidence in the empirical strategy, for if regional enforcement did have a relationship with revenue change, we would be concerned that somehow that underlying shock itself was not similar across regions. Similarly, the relationship between growth in profits and shocks too does not vary significantly by enforcement (Columns 3 and 4).

Table 9 repeats the estimations reported in Table 7 using a different measure of labor regulation enforcement, the number of labor disputes (regarding pay or dismissal) brought to the courts per thousand employees. As discussed, this measure could reflect workers’ perceptions of the judicial system’s capacity in handling labor disputes, which is another critical dimension of enforcement capacity. The results are substantively similar. Because this measure is highly correlated with the inspectorate measure, we do not attempt to distinguish between their effects by including both measures in the same regression.

The regressions in Tables 6 and 7 were estimated on surviving firms (those which exist in both year t and $t-1$). Thus, employment lost through firm exit is not included in the estimation. An alternative would be to estimate the result on all incumbent firms in year $t-1$, including those which would exit in year t , and treat the latter as having zero employment in year t . While our findings are robust to that approach, we are not convinced that it is a better alternative since the expected relationship between shocks, exits and labor regulation is ambiguous. On the one hand, a firm considering shutting down might be discouraged in the face of heavier labor resistance when employment protection is more stringent. On the other hand, if inflexible labor laws are especially harmful to firm profits, more firms might be forced to shut down. As an alternative to including exit in the main specification, we explicitly estimate how the probability of exit during shocks varies by enforcement. This is discussed in the next section.

5.2 Robustness checks: Exploiting size-related variation in enforcement

One concern with our empirical strategy is that enforcement capacity could be correlated with unobserved regional attributes which influence labor adjustment by firms. As discussed in Section 4.2, our strategy for addressing this concern uses within-region variation in labor regulation enforcement. Given that enforcement is focused on firms with more than 50 employees, we expect the effect of enforcement capacity to be stronger for firms above this threshold.

In Table 10, Column 1, we present OLS estimates of Equation (3), which allows the relationship between enforcement and shocks to vary across firms above and below this threshold. It does so by including interactions of *Large*, an indicator for a firm being above the size threshold of 50, with *Surge*Enforcement* and *Slump*Enforcement*.

Consistent with our hypothesis, the coefficient on *Surge*Enforcement*Large* is negative. It indicates that stronger enforcement attenuates hiring during surges to a greater extent among *Large* firms. The point estimate is sizable: specifically, it implies that the coefficient on *Surge*Enforcement* expands from -0.06 for firms below the size threshold to -0.22 for those above it.

Analogously, the positive coefficient on *Slump*Enforcement*Large* indicates that stronger enforcement attenuates firing during slumps to a greater extent among *Large* firms. Though the coefficient is not significant—it is marginally below the 10% level—the point estimate is large: it implies that the coefficient on *Slump*Enforcement* goes up from 0.007 for firms below the size threshold to 0.150 for those above it. This too suggests that the observed relationship between enforcement and labor adjustment during shocks is driven by the *Large* firms in our sample.

Column 2 includes triple interactions of a continuous measure of firm size (lagged employment level) with *Surge*Enforcement* and *Slump*Enforcement*. This makes it, in effect, a test for a ‘break’ at size 50. This modification does not make a qualitative difference to the estimated coefficients on *Surge*Enforcement*Large* and *Slump*Enforcement*Large*, although their statistical significance drops away. This result is therefore weakly consistent with a threshold effect of enforcement at size 50. The weak result could be because the fuzzy definition of threshold lowers the power of this test. Column 3 adds triple interactions of *Surge*Enforcement* and *Slump*Enforcement* with other firm attributes (age and capital intensity) as there is a concern that the employment measure could be correlated with other firm attributes. The main results are the same. Overall, the results in Table 10 suggest that the relationship between enforcement and employment adjustment is indeed stronger among those firms that are the focus of labor inspectorates.

5.3 Effect on firm exit

Table 11 provides evidence of the impact of shocks and labor regulation enforcement on firm exit, estimating Equation (5) through OLS. The baseline specification is shown in column 1; column 2 adds sectors-year FEs and columns 3 and 4 add interactions of firm employment, age and capital-labor ratio with shocks as additional controls.

The significant negative coefficient on *Slump* indicates that on average, firms are more likely to exit during slumps than during normal and surge years. At -0.01, it is sizable compared to the baseline annual exit rate in the RUSLANA panel, which ranges between 0.04 and 0.07 during the study period. However, the estimated interaction between *Slump* and *Enforcement* is small (-0.001) and statically not significant; and the same holds for the estimated interaction between *Surge* and *Enforcement*. Thus, enforcement is not associated with exit rates during shocks, and ignoring or including employment loss due to firm exit is immaterial to our main results.

6. Conclusion

Laws that impose high firing costs on firms can constrain the efficient reallocation of labor across firms. Indeed, there is a negative relationship between stringency of labor laws and the extent of job reallocation across countries (Haltiwanger, 2010).¹⁷ Our study affirms these concerns, uncovering a specific mechanism through which labor laws could have a dampening effect on job flows.

At the same time, we observe that this mechanism is significantly weaker in places with more compromised enforcement capacity. Thus, a reduction in the stringency of labor regulations might have unexpectedly small impact in places where the existing law is already weakly enforced. Moreover, to the extent that enforcement capacity is weaker in poorer countries, we run the risk of attributing too much blame to their stringently formulated labor laws. We also risk being too complacent about regulations that look good on paper but are not well-enforced.

Our study thus underlines the importance of including regulatory enforcement more comprehensively in the research agenda on productivity and growth in low and middle income countries. Systematic measurement of the enforcement of business regulations would be a good starting point: most of the current evidence on enforcement gaps is qualitative and anecdotal. The role of institutional enforcement capacity in determining the effectiveness of regulations—and its measurement—is another largely untapped field of research.

¹⁷ In a similar vein as Haltiwanger (2010), Brown and Earle (2002, 2006 and 2008) analyze jobs flows in Russia, and relate them to institutional features. Relatedly, labor regulations can also diminish the impact of economic reforms by dampening employment reallocation that should accompany them. For instance, in models by Kambourov (2009) and Helpman et al. (2011), the reallocation of workers following trade liberalization depends on the country's labor market institutions, such as firing costs and search frictions. Aghion et al. (2008) show how the impact of India's industrial entry regulation reforms depended on the stringency of labor laws.

7. References

- Adhvaryu, A., A. Chari and S. Sharma, 2013. "Firing Costs and Flexibility: Evidence from Firms' Employment Responses to Shocks in India." *Review of Economics and Statistics*, 95(3): 725–40.
- Aghion P., R. Burgess, S. Redding, F. Zilibotti, 2008. "The Unequal Effects of Liberalization: Evidence from Dismantling the License Raj in India." *American Economic Review*, 94(4):1397-1412.
- Almeida, R. and P. Carneiro, 2012. "Enforcement of Labor Regulation and Informality." *American Economic Journal: Applied Economics*, 4(3): 64–89.
- Almeida, R. and P. Carneiro, 2007. "Inequality and Employment in a Dual Economy: Enforcement of Labor Regulation in Brazil." IZA DP No. 3094.
- Ashwin S. and S. Clarke, 2002. Russian Trade Unions and Industrial Relations in Transition. Basingstoke and New York: Palgrave.
- Autor, D., W. R. Kerr and A. D. Kugler, 2007. "Does Employment Protection Reduce Productivity? Evidence from U.S. States." *The Economic Journal*, 117(521): 189–217.
- Bentolila, S. and G. Saint-Paul. 1992. "[The Macroeconomic Impact of Flexible Labor Contracts, With an Application to Spain.](#)" *European Economic Review* 36(5): 1013–47.
- Blanchard, O. and J. Wolfers, 2000. "The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence." *Economic Journal*, Royal Economic Society, 110(462): 1–33.
- Boeri, T. and J.F. Jimeno, 2005. "[The Effects of Employment Protection: Learning from Variable Enforcement.](#)" *European Economic Review*, 49(2005): 2057–77. (Last accessed June 15, 2015).
- Botero, J., S. Djankov, R. La Porta, F. López-de-Silanes and A. Shleifer, 2004. "The Regulation of Labor." *The Quarterly Journal of Economics*, 119(4): 1339–1382.
- Brown, J.D. and J.S. Earle, 2002. "[Job Reallocation and Productivity Growth under Alternative Economic Systems and Policies: Evidence from the Soviet Transition.](#)" Institute for the Study of Labor Discussion Paper, IZA DP No. 644. (Last accessed June 4, 2015).
- Brown, J.D. and J.S. Earle, 2006. "[The Microeconomics of Creating Productive Jobs: A Synthesis of Firm-Level Studies in Transition Economies.](#)" World Bank Policy Research Working Paper No. WPS 3886. (Last accessed April 11, 2014)
- Brown, J.D. and J.S. Earle, 2008. "[Understanding the Contributions of Reallocation to Productivity Growth: Lessons from a Comparative Firm-Level Analysis.](#)" Institute for the Study of Labor Discussion Paper, IZA DP No. 3683. (Last accessed June 4, 2014).

- Di Tella, R. and R. MacCulloch, 2005. "The Consequences of Labor Market Flexibility: Panel Evidence Based on Survey Data." *European Economic Review*, 49(5): 1225–1259.
- Gimpelson, V.E., R. Kapeliushnikov and A. Lukiyanova, 2010. "Employment Protection Legislation in Russia: Regional Enforcement and Labor Market Outcomes." *Comparative Economic Studies*, 52(4):611–36.
- Gimpelson V. and R. Kapeliushnikov, 2008. "Applying Labor Law in Russia: Role of Judiciary." CLMS-HSE, (mimeo)
- Hallward-Driemeier, M. and L. Pritchett, 2011. "[How Business is Done and the 'Doing Business' Indicators: The Investment Climate When Firms Have Climate Control.](#)" Policy Research Working Paper Series 5563, The World Bank.
- Haltiwanger, J.C., S. Scarpetta, and H. Schweiger, 2010. "[Cross-county Differences in Job Reallocation: The Role of Industry, Firm Size and Regulations.](#)" EBRD Working Paper No. 116.
- Hamermesh, D. 1993. *Labor Demand*, Princeton University Press, Princeton NJ.
- Helpman, E., O. Itskhoki and S. Redding, 2011. "Trade and Labor Market Outcomes." in D. Acemoğlu, M. Arellano and E. Dekel (eds.), *Advances in Economics and Econometrics: Theory and Applications*, Tenth World Congress of the Econometric Society.
- Hsieh, Chang-Tai, and Benjamin A. Olken 2014. "The Missing 'Missing Middle'." *The Journal of Economic Perspectives*, 28(3): 89-108.
- Hsieh, Chang-Tai, and Peter J. Klenow 2014. "The Life Cycle of Plants in India and Mexico." *The Quarterly Journal of Economics*, 129(3):1035-1084.
- Hsieh, C-T, and P.J. Klenow 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics*, 124(4): 1403-1448.
- Kambourov, G., 2009. "Labor Market Regulations and the Sectoral Reallocation of Workers: The Case of Trade Reforms." *Review of Economic Studies*, 76(4):1321–58.
- Krishna, K., S. Dougherty and V. Frisancho Robles, 2011. "[Employment Protection Legislation and Plant Level Productivity in India.](#)" NBER Working Paper 17693.
- Labor Code of the Russian Federation, 2001. Federal Law No. 197-FZ of 30 December 2001
- Nickell, S. J. 1986. "Dynamic Models of Labor Demand.", in *Handbook of Labor Economics*, Vol 1, edited by O. Ashenfelter and R. Ledyard.
- OECD, 2014. *Employment Outlook 2014*, OECD, Paris.
- Oi, W., 1962. "Labor as a Quasi-Fixed Factor." *Journal of Political Economy*, 70(6): 538–55.

- Pierre, G. and S. Scarpetta 2006. "Employment protection: Do firms' Perceptions Match with Legislation?" *Economics Letters*, 90(3): 328-334.
- Vishnevskaya N. and R. Kapelyushnikov, 2007. "The EPL Enforcement in Russia: Coverage, Dynamics, Interregional Differentiation." Working paper WP3/2007/02. Moscow: State University — Higher School of Economics, 2007. (in Russian).
- World Bank, 2010. *Doing Business 2011*. The World Bank Group, Washington, D.C.
- World Bank, 2014. *Doing Business 2015*. The World Bank Group, Washington, D.C.
- World Bank, 2016. *Doing Business 2017*. The World Bank Group, Washington, D.C.
- Yakovlev, Evgeny, and Ekaterina Zhuravskaya 2013. "The Unequal Enforcement of Liberalization: Evidence from Russia's Reform of Business Regulation." *Journal of the European Economic Association*, 11(4): 808-838.

Table 1: Labor law enforcement measures

<i>(per 100,000 employees)</i>	<i>Mean</i>	<i>Minimum</i>	<i>25th percentile</i>	<i>Median</i>	<i>75th percentile</i>	<i>Maximum</i>
<i>Inspectors</i>	11.5	5.6	8.6	9.8	11.55	50.6
<i>Inspections</i>	7.9	2.9	5.5	6.9	8.9	26
<i>Identified violations</i>	56.5	21	41	54	66	126
<i>All labor dispute cases filed</i>	23.6	3.1	12.35	18.2	24.3	289.2
<i>Pay cases filed</i>	18.8	1.7	9.7	14.6	20.1	264.7
<i>Dismissal cases filed</i>	1.1	0.4	0.7	0.9	1.3	4.8

Table 2: Correlations between regional labor law enforcement measures

	<i>Inspectors</i>	<i>Inspections</i>	<i>Identified violations</i>	<i>All labor dispute cases</i>	<i>Pay cases filed</i>
<i>Inspections</i>	0.8454	1			
<i>Identified violations</i>	0.4507	0.615	1		
<i>All labor dispute cases</i>	0.4923	0.6296	0.3702	1	
<i>Pay cases filed</i>	0.4734	0.6148	0.3786	0.9949	1
<i>Dismissal cases filed</i>	0.8423	0.7567	0.3307	0.534	0.4925

Table 3: Correlation between labor law enforcement (inspector density) and other regional characteristics

<i>Variable</i>	<i>Correlation coefficient</i>
<i>Unemployment Rate</i>	0.6
<i>No. of Firms</i>	-0.16
<i>Nominal Wage</i>	0.07
<i>Urbanization Rate</i>	-0.28
<i>Gross Regional Product</i>	-0.18
<i>Gross Regional Product per Capita</i>	-0.04
<i>Total Industrial Output</i>	-0.2
<i>Industrial Output from State Enterprises</i>	-0.2
<i>Share of Manufacturing Sector in Output</i>	-0.46
<i>Share of Services Sector in Output</i>	0.34

Table 4: Summary of RUSLANA firm panel data

Year	Total number of firms	Number of “entering” firms	Firms in slumping sectors	Firms in surging sectors	Firms in sectors that are neither surging nor slumping
2004	65942	7618	5464	19688	40790
2005	65136	12683	10357	699	54080
2006	64536	11068	1090	31595	31851
2007	65498	10616	829	40423	24246
2008	64026	8908	60563	13	3450
2009	61035	7075	41876	1416	17743
2010	59179	7874	7630	4589	46960

Note: A firm is classified as an “entering” firm in year t if year t is the earliest year in which that firm exists in the RUSLANA panel, regardless of the year of incorporation. The total number of firms in year t is inclusive of entering firms.

Table 5: Summary statistics on incumbent firms

Year	No. of firms	Medium/Large Firms (>50 workers)	Mean no. of workers	Mean change in workers	Mean age of firms	Mean change in revenue (in 1000s)	Mean change in profit (in 1000s)
2004	58324	12655	86.9 (406.4)	-4.5 (113.7)	7.8 (11.4)	390.5 (14545)	34.7 (2493.1)
2005	52453	12013	102 (425.6)	1.9 (57.5)	8.3 (11.7)	252.2 (21295.6)	17.1 (2475.9)
2006	53468	12831	87.5 (383.8)	1.5 (51.8)	8.6 (12.1)	876.6 (14902.7)	105.1 (3206.9)
2007	54882	12754	97.7 (411.4)	2.8 (58.1)	8.8 (12)	1525.7 (157723.9)	164.5 (15630.5)
2008	55118	14447	101.7 (398.4)	4 (53.8)	9 (12.1)	-1665.9 (143070.6)	-137.4 (13890.5)
2009	53960	15444	105.4 (379)	4.8 (121.5)	9.5 (12.3)	-630.9 (17797.4)	-178.3 (6076.4)
2010	51305	15403	107.8 (383.6)	4.6 (78.2)	9.9 (12.3)	478.6 (11134.5)	88.7 (3617.4)

Note: Standard deviation in parentheses. Excludes 'entering' firms.

Table 6: Average Effect of Surges and Slumps

	<i>Change in Employment</i>		<i>Change in Revenue</i>		<i>Change in Profit</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Slump</i>	-0.5*** (0.2)	-0.6*** (0.2)	-1,492,467.0** (589,743.9)	-1,845,347.0** (794,039.1)	-289,401.3*** (95,788.8)	-383,361.7** (149,452.0)
<i>Surge</i>	0.3 (0.3)	0.3 (0.3)	658,174.7* (348,275.7)	1,100,003.0* (664,394.8)	59,134.1 (45,574.5)	93,041.3 (80,116.4)
<i>Fixed effects</i>	Year	Firm + Year	Year	Firm + Year	Year	Firm + Year
<i>Observations</i>	306,879	306,879	306,879	306,879	306,879	306,879
<i>R²</i>	0.01	0.4	0.001	0.2	0.001	0.1
<i>Adjusted R²</i>	0.01	0.1	0.001	-0.1	0.001	-0.2
<i>Residual Std. Error</i>	10.0 (df = 306870)	9.7 (df = 210464)	30,663,977.0 (df = 306870)	32,438,662.0 (df = 210464)	4,931,107.0 (df = 306870)	5,509,054.0 (df = 210464)

Note: *p**p***p<0.01; Errors clustered by Sector

Table 7: How employment adjustment to shocks varies by regional enforcement capacity

	<i>Change in Employment</i>			
	(1)	(2)	(3)	(4)
<i>Slump</i>	-1.251*** (0.328)			
<i>Surge</i>	1.157*** (0.323)			
<i>Slump*Enforcement</i>	0.070** (0.031)	0.051* (0.030)	0.051* (0.029)	0.042 (0.028)
<i>Surge*Enforcement</i>	-0.098*** (0.031)	-0.098*** (0.032)	-0.092*** (0.030)	-0.093*** (0.029)
<i>Slump*LagEmp</i>			-0.002*** (0.001)	-0.002*** (0.001)
<i>Surge*LagEmp</i>			0.0004 (0.001)	0.001 (0.001)
<i>Slump*Age</i>				0.015 (0.009)
<i>Surge*Age</i>				-0.021* (0.012)
<i>Slump*log(K/L)</i>				0.045** (0.022)
<i>Surge*log(K/L)</i>				0.028 (0.030)
<i>Fixed effects</i>	Firm + Year	Firm + Sector * Year	Firm + Sector * Year	Firm + Sector * Year
<i>Observations</i>	306,879	306,879	306,879	306,879
<i>R2</i>	0.357	0.364	0.391	0.392
<i>Adjusted R2</i>	0.062	0.071	0.111	0.111
<i>Residual Std. Error</i>	9.743 (df = 210462)	9.699 (df = 210050)	9.487 (df = 210047)	9.485 (df = 210043)

Note: *p **p ***p<0.01; Errors clustered by Sector. (3) and (4) include lagged employment, not shown for brevity.

Table 8: How effects of shocks on revenue and profit vary by enforcement

	Change in Revenue		Change in Profit	
	(1)	(2)	(3)	(4)
<i>Slump</i>	-1,858,934.0* (1,093,314.0)		-371,501.2** (181,618.1)	
<i>Surge</i>	1,281,901.0 (814,809.9)		142,899.7 (102,560.0)	
<i>Slump*Enforcement</i>	1,471.9 (53,349.3)	75,477.9 (62,419.0)	-1,368.0 (7,505.7)	14,851.9 (9,648.8)
<i>Surge*Enforcement</i>	-20,905.9 (32,575.1)	-83,943.4 (64,039.2)	-5,730.9 (6,555.3)	-13,355.1 (8,986.3)
<i>Slump*LagEmp</i>		-18,116.5*** (4,452.0)		-4,615.3*** (1,559.1)
<i>Surge*LagEmp</i>		8,887.8** (3,904.0)		1,355.2** (597.5)
<i>Slump*Age</i>		42,150.8** (19,818.3)		9,029.9* (5,171.9)
<i>Surge*Age</i>		-33,975.7*** (12,723.9)		-7,968.9 (6,779.8)
<i>Slump*log(K/L)</i>		-265,468.4*** (98,286.7)		-40,378.6** (20,077.9)
<i>Surge*log(K/L)</i>		215,338.3* (128,116.4)		9,064.4 (18,779.0)
<i>Fixed effects</i>	Firm + Year	Firm + Sector * Year	Firm + Year	Firm + Sector * Year
<i>Observations</i>	306,879	306,879	306,879	306,879
<i>R2</i>	0.2	0.3	0.1	0.2
<i>Adjusted R2</i>	-0.1	-0.1	-0.2	-0.2
<i>Residual Std. Error</i>	32,438,810.0 (df = 210462)	32,026,253.0 (df = 210043)	5,509,078.0 (df = 210462)	5,372,099.0 (df = 210043)

Note: *p**p***p<0.01; Errors clustered by Sector. (2) and (4) include lagged employment, not shown for brevity.

Table 9: Alternate Measure of enforcement

		<i>Change in Employment</i>		
	(1)	(2)	(3)	(4)
<i>Slump</i>	-0.870*** (0.238)			
<i>Surge</i>	0.485 (0.315)			
<i>Slump*Enforcement</i>	0.017*** (0.006)	0.014*** (0.005)	0.013** (0.005)	0.012** (0.005)
<i>Surge*Enforcement</i>	-0.014* (0.008)	-0.013 (0.009)	-0.013 (0.008)	-0.013 (0.008)
<i>Slump*LagEmp</i>			-0.002*** (0.001)	-0.002*** (0.001)
<i>Surge*LagEmp</i>			0.0004 (0.001)	0.001 (0.001)
<i>Slump*Age</i>				0.015 (0.009)
<i>Surge*Age</i>				-0.021* (0.012)
<i>Slump*log(K/L)</i>				0.045** (0.021)
<i>Surge*log(K/L)</i>				0.024 (0.031)
<i>Fixed effects</i>	Firm + Year	Firm + Sector * Year	Firm + Sector * Year	Firm + Sector * Year
<i>Observations</i>	306,879	306,879	306,879	306,879
<i>R2</i>	0.357	0.364	0.391	0.392
<i>Adjusted R2</i>	0.062	0.071	0.111	0.111
<i>Residual Std. Error</i>	9.743 (df = 210462)	9.699 (df = 210050)	9.487 (df = 210047)	9.485 (df = 210043)

Note: *p**p***p<0.01; Errors clustered by Sector

(3) and (4) include Lagged Employment, not shown for brevity.

Table 10: How enforcement impacts differ for firms with more than 50 workers

	<i>Change in Employment</i>		
	(1)	(2)	(3)
<i>Slump*Enforcement</i>	0.007 (0.020)	0.009 (0.021)	-0.028 (0.047)
<i>Surge*Enforcement</i>	-0.060*** (0.018)	-0.057*** (0.017)	-0.104 (0.064)
<i>Slump*Enforcement*Large</i>	0.143 (0.096)	0.121 (0.117)	0.125 (0.112)
<i>Surge*Enforcement*Large</i>	-0.166* (0.098)	-0.129 (0.122)	-0.127 (0.124)
<i>Slump*Enforcement*LagEmp</i>		0.0001 (0.0004)	0.0001 (0.0004)
<i>Surge*Enforcement*LagEmp</i>		-0.0001 (0.0003)	-0.0001 (0.0003)
<i>Slump*Enforcement*Age</i>			-0.0003 (0.004)
<i>Surge*Enforcement*Age</i>			0.003 (0.004)
<i>Slump*Enforcement*log(K/L)</i>			-0.001 (0.006)
<i>Surge*Enforcement*log(K/L)</i>			0.004 (0.009)
<i>Fixed effects</i>	Firm + Sector * Year	Firm + Sector * Year	Firm + Sector * Year
<i>Observations</i>	306,879	306,879	306,879
<i>R2</i>	0.410	0.432	0.433
<i>Adjusted R2</i>	0.138	0.171	0.171
<i>Residual Std. Error</i>	9.339 (df = 210044)	9.162 (df = 210038)	9.160 (df = 210029)

Note: *p **p ***p<0.01; Errors clustered by Sector

The specification includes the following variables when necessary: Large, Lagged Employment, Slump*Large, Surge*Large, Enforcement*Large, Slump*LagEmp, Surge*LagEmp, Enforcement*LagEmp, Slump*Age, Surge*Age, Enforcement*Age, Slump*log(K/L), Surge*log(K/L), Enforcement*log(K/L), not shown for brevity.

Table 11: Relationship between firm exits and enforcement

	(1)	(2)	<i>Firm Exits</i>		(5)
			(3)	(4)	
<i>Slump</i>	0.010*** (0.003)	0.017*** (0.006)	(0.000)	(0.000)	(0.000)
<i>Surge</i>	-0.003 (0.003)	0.006 (0.005)	(0.000)	(0.000)	(0.000)
<i>Slump*Enforcement</i>		-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
<i>Surge*Enforcement</i>		-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
<i>Fixed effects</i>	Year	Year	Sector * Year	Sector * Year	Sector * Year
<i>Shock * LagEmp</i>	No	No	No	Yes	Yes
<i>Shock * Age</i>	No	No	No	No	Yes
<i>Observations</i>	328,324	328,324	328,324	328,324	328,324
<i>R2</i>	0.012	0.012	0.019	0.020	0.027
<i>Adjusted R2</i>	0.012	0.012	0.018	0.019	0.025
<i>Residual Std. Error</i>	0.246 (df = 328315)	0.246 (df = 328312)	0.245 (df = 327831)	0.245 (df = 327828)	0.244 (df = 327822)

Note: *p**p***p<0.01; Errors clustered by Sector

The specification includes the following variables: *Slump*log(K/L)*, *Surge*log(K/L)* which were omitted for brevity.

Annex 1: Measurement of ‘Surges’ and ‘Slumps’

Global sector-level trend growth rates that were estimated using the INDSTAT 4 2009 Revision 2¹⁸ and INDSTAT 4 2012 Revision 3¹⁹ data sets from the United Nations Industrial Development Organization (UNIDO) are used. The two UNIDO data sets were combined to create a database representing 84 sectors (4-digit NACE)²⁰ from 134 countries for the time-period 1993 to 2009. Outlier observations – identified as growth greater than 3 standard deviations above or below the mean for each sector in each country – were removed. This results in dropping about 45 percent of the observations in the data set.

In order to identify surges and slumps, first, a trend output growth rate for each sector is estimated. In order to account for life product cycle effects, only countries in the same GDP per capita quartile as the Russian Federation are included, and an average growth trend of each 4-digit NACE sector in this group calculated by OLS regression of log output on time.²¹ To increase the robustness of the results sectors with fewer than 60 observations from the trend regressions are dropped.

Shocks are defined in terms of “extreme” deviations from this global trend, relative to the distribution of the deviations during 1993-2009. Since the distribution of deviations could vary by sector, the next step is to calculate the sector-wise distribution of deviations from trend in the UNIDO data. For each country, sector and year in UNIDO, the percent deviation of actual output from trend output is calculated. For each sector, the 75th and 25th percentiles of the distribution of this deviation (across countries and during 1993-2009) are calculated. Let D^{75}_s and D^{25}_s be the 75th and 25th percentiles of the deviations from trend output in sector s .

Next, the global trend for each sector is used to calculate predicted Russian output for that sector using RUSLANA data. The percent deviation of actual output from this predicted output is calculated for each sector-year. A sector s is defined to be in slump in year t if its deviation from trend in year t is above D^{75}_s , and to be in slump if its deviation from trend is below D^{25}_s . The sector is in a “normal” year if its deviation is between D^{75}_s and D^{25}_s .

¹⁸ <http://unstats.un.org/unsd/cr/registry/regcst.asp?Cl=8&Lg=1>

¹⁹ <http://unstats.un.org/unsd/cr/registry/regcst.asp?Cl=2&Lg=1>

²⁰ NACE is the acronym used to designate the various statistical classifications of economic activities developed since 1970 in the European Union (EU). NACE provides the framework for collecting and presenting a large range of statistical data according to economic activity in the fields of economic statistics (e.g. production, employment, national accounts) and in other statistical domains. This classification was designed to delineate broad economic categories, into large economic classes of commodities, distinguishing food, industrial supplies, capital equipment, consumer durables and consumer non-durables. It is broadly used to stand for sectors. The higher the number of digits for the NACE, the more detailed the sector; from the most aggregate to the least, the classifications are organized by Section, Division, Group and finally Class. The analysis here is at the 4-digit NACE level; namely at the Group level. For more information, see <http://unstats.un.org/unsd/cr/registry/regcst.asp?Cl=27&Lg=1>.

²¹ It is reasonable to assume that sectors that are booming in poorer countries may be shrinking in richer ones. To take this into account, countries with different income levels are allowed to have different sectoral growth trends.