

Labor Regulation and Enterprise Employment in China

Albert Park, HKUST

John Giles, The World Bank

Yang Du, Chinese Academy of Social Sciences

June 2012

Abstract

Using data from a national survey of Chinese manufacturing firms conducted in 2009, we analyze the impact of implementation of China's 2008 Labor Contract Law on the employment of production workers. We find that cities with lax prior enforcement of labor regulations experienced a greater increase in enforcement after 2008 and slower employment growth, and that this finding is robust to inclusion of a rich set of city-level controls and the use of alternative measures of enforcement effort. Although firms affected by the global economic crisis did not report less strict enforcement of the new Law, there is evidence that their employment adjustment was less sensitive to enforcement of labor regulations than firms not affected by the crisis.

JEL codes: J23, J30, J41, O17, O53

Labor Regulation and Enterprise Employment in China

1. Introduction

On January 1, 2008, China implemented a new Labor Contract Law with provisions considered to be highly protective of workers when viewed in international comparative perspective. Prior to passage of the Law, the Chinese labor market was regarded as one of the most flexible labor markets in the world (Forteza and Rama, 2000). Before the Law was enacted, business leaders and many commentators inside and outside of China expressed concern that the Law would increase labor costs of enterprises, reduce employment, and undermine international competitiveness. If the measure of strictness of Employment Protection Legislation (EPL) defined by the OECD for developed countries (OECD, 2004) is applied to China's new Labor Contract Law, China would rank the third among OECD countries in terms of overall strictness. Using a similar methodology, Chen and Funke (2008) find that the Labor Contract Law increases the firing costs of employers in China compared to many other developing countries. The onset of the global economic crisis, which hit China in force in October of 2008, exacerbated this concern, leading to speculation that China would relax enforcement of the new Law in order to support firms in a time of crisis. After four years of implementation, disagreement continues over the role of Labor Contract Law in the Chinese labor market, as evidenced by the National People's Congress plans to revise the Labor Contract Law in 2012 (Wu, 2012).

Despite the large potential impact of the new labor regulations on Chinese workers and the overall economy, to date there exists little empirical evidence on how well the new Labor Law was implemented and how it has affected the employment decisions of manufacturing enterprises. This paper attempts to fill that gap. We also examine the extent to which the economic crisis mediated the implementation and impact of the new labor

regulations. Given China's important role in global trade, the impact of the new law on labor costs and employment in China has direct implications for structural adjustment policies in China as well as the competitive position of exporters in China and in other countries.

Economists have attributed persistently higher unemployment in Europe compared to the United States to stronger labor market institutions and a weaker role of markets (see, for example, Nickell, 1997). However, the empirical robustness of such a relationship in cross-country evidence has been questioned (Baker et al., 2005), and Freeman (2007) points out that the world has a great diversity of labor market institutions that can influence economic outcomes in both positive and negative ways. Stronger labor market institutions can carry important benefits for firm performance by increasing communication of information within firms, improving resolution of worker grievances and reducing turnover costs, and even strengthen market outcomes when markets are not functioning well. Even if there are negative impacts of protective labor regulations on employment, these must be weighed against the positive impacts on the welfare of employees who enjoy greater security.

The majority of previous studies on the employment impact of labor regulations in developing countries find a negative (positive) relationship between inflexibility of labor regulations and employment (unemployment). Most of these studies analyze regional or national aggregate employment data (Besley and Burgess, 2004; Ahsan and Pages, 2009; Feldmann, 2009; Djankov and Ramalho, 2009; Kaplan, 2009). Only a few analyze firm-level data. Almeida and Carneiro (2005) find that stricter enforcement (as measured by higher fines) has no effect on employment, but leads to increases in informal employment in Brazil. Amin (2007) finds a negative effect of stricter regulation (mean perception by state) on employment in Indian retail outlets. After aggregating firm data to the industry level in India and Zimbabwe, Fallon and Lucas (1993) find a negative effect on employment of new legislation to increase job security. Theirs is the only study to use panel data.

There are advantages and disadvantages of using firm-level panel data to study the employment impacts of labor regulation. On the positive side, questions asked directly of firm managers can be used to construct aggregate measures of enforcement and implementation directly relevant for the sample of firms being studied. Using micro- rather than aggregate data also enables examination of heterogeneity in the impacts of labor regulations on firms with different characteristics. The main disadvantages are that bias can arise if firms which exit from the panel are not surveyed, and the results may provide only a partial picture of employment outcomes if the sampling frame excludes certain types of firms. For example, the sample used in this study is likely to under-sample small firms (more detail below) and excludes non-manufacturing firms.

In this paper, we analyze retrospective panel data from a nationally representative sample of manufacturing firms in China to study two main research questions. First, what are the determinants of Labor Law enforcement? Second, how did city-level variation in enforcement of the Labor Law affect employment in manufacturing firms? For each of these questions we also examine the influence of the global economic crisis. We are specifically interested in whether the onset is associated with reduced enforcement of the Law, and whether the crisis mediated the impact of enforcement on employment.

To answer these questions, we estimate static and dynamic models of the determinants of the strictness of enforcement of labor regulations, as well as a model of the determinants of employment changes in Chinese enterprises. We find that cities with lax prior enforcement of labor regulations experienced a greater increase in enforcement after 2008 and slower employment growth, and that this finding is robust to inclusion of a rich set of city-level controls, the use of alternative measures of enforcement effort, and the use of methods to correct for biased standard errors caused by the small number of clusters. Enforcement strictness was not significantly affected by the global economic crisis, and

employment in exporting firms exposed to adverse export demand shocks was less sensitive to enforcement of labor regulations.

The rest of the paper is organized as follows. Section 2 provides background information on the Law's provisions and its enforcement, as well as the impact of the global economic crisis on employment in China. Section 3 provides a theoretical framework for assessing the impact of the new Law on employment, Section 4 introduces the data, Section 5 describes the methodology, Section 6 presents the empirical results, and Section 7 concludes.

2. An Eventful 2008: Labor Law Implementation and Global Economic Crisis

As noted earlier, China's 2008 Labor Contract Law, which took effect on January 1, 2008, included provisions that were highly protective of workers' interests. Two important aspects of the new Law were new regulations on the nature of contracts that employers were obligated to provide workers and the severance conditions for firing workers. Under the new Law, after a worker completes two fixed-term contracts, or ten years of employment, employment contracts must be made open-ended, or permanent. The probationary period for new contracts is limited to one to three months depending on the contract length. New regulations were passed to prevent the use of temporary work agencies, or labor service companies, to circumvent obligations to workers. With respect to severance conditions, the new Law requires 30-day written notice when firing workers, severance pay equal to one month's pay for each year of service (a half month's pay if less than 6 months), and double severance pay for unfair dismissal.

Less than a year after the new Labor Law was enacted, China was buffeted by the global economic crisis, which affected the Chinese economy mainly through a large negative shock to the global demand for China's exports. Figure 1 plots data on quarterly export value calculated by the authors using data from China's Customs Service for the period covered by

the 2009 firm survey. The top panel is for the whole country, while the bottom panel plots data for the 25 cities where the firm survey was carried out. As one can see clearly from either panel, the plots for the whole country and the 25 cities is nearly identical; in both cases there was a dramatic decline in exports by nearly 40% starting from the third quarter of 2008 to the first quarter of 2009. As a result of the crisis, over 20 million migrant workers were estimated to have lost their jobs before spring festival 2009. It was speculated that given the severity of the crisis, leaders in some regions might relax enforcement of the new labor regulations (Giles et al., 2012a). It is thus of interest to better understand how the global economic crisis interacted with the enforcement of labor regulations to affect employment outcomes.

3. Theory

We present a simple theoretical framework for thinking about how implementation of the new Labor Law may have affected employment decisions. Consider employment (E_t) to be a simple ratio of a firm's optimal employment assuming perfect enforcement of labor regulations (L_t) and the strictness of enforcement (S_t). Thus,

$$E_t = L_t/S_t. \quad (1)$$

Here, $0 < S_t \leq 1$, so that lower S_t leads to higher E_t . When there is perfect enforcement, $S_t = 1$, employment equals optimal employment based on the substance of the law, or $E_t = L_t$. When enforcement is less than perfect, or $S_t < 1$, then employment E_t is greater than the optimal level of employment assuming perfect enforcement. This reflects the fact that with looser enforcement firms can reduce labor costs by evading regulations that, for example, require labor contracts with workers and require that payroll contributions be made to provide workers with social insurance coverage (i.e., pensions, health insurance, unemployment insurance, work injury insurance).

From equation (1) changes in employment can be expressed in log form as: $\Delta \ln E = \ln E_2 - \ln E_1 = \Delta \ln L - \Delta \ln S$. In theory, new labor regulations can influence employment in two ways. First, changes in the substance of the law could lead to a reduction in the optimal number of workers hired assuming perfect enforcement ($L_2 < L_1$). Second, implementation of the new law may be accompanied by greater strictness in the enforcement of labor regulations ($S_2 > S_1$). One limitation we face is that, abstracting from firm heterogeneity in the treatment effects of the new law, changes in L_t are national in scope and thus affect all firms, making it impossible to distinguish the impacts of the Law from the effects of other time-varying factors such as macroeconomic shocks or other national policy changes.

For this reason, we concentrate on variation across cities in changes in enforcement of labor regulations associated with the implementation of the new Law. Consider two possible ways in which the new Law could influence the strictness of enforcement. First, introduction of the new Law may have been accompanied by a determined effort to raise the degree of strictness of labor regulation enforcement across cities to a higher and more uniform level. To take an extreme case for illustrative purposes, assume that after implementation of the new Law, labor regulations were perfectly enforced in all cities. In that case, $S_2 = 1$ and thus $\Delta \ln E = \Delta \ln L + \ln S_1$. Here, differences in changes in enforcement are entirely determined by the initial strictness of enforcement, so that strictness increases less and employment falls by less in cities with high levels of strictness prior to implementation of the new Law (high S_1). This is analogous to tariff reductions being greater in sectors with high initial tariff rates during a full trade liberalization. A second way to think about changes in enforcement is to assume that changes in the substance of labor regulations are not necessarily accompanied by changes in enforcement strictness. Again taking an extreme example, assume there is no

change in enforcement at all and thus no impacts on employment through this channel.¹In this case, we do not expect to see any differences across cities in the impact of the new Law. Whether such differences actually exist thus is an empirical question.

4. Data

Our data source is a nationally representative survey of 1644 manufacturing firms in China conducted by the Research Department of the People's Bank of China in the fall of 2009. The authors contributed an employment module that included questions on employment before and after implementation of the new Labor Law. The surveys were conducted in 25 cities located in eight provinces, including 4 coastal provinces (Shandong, Jiangsu, Zhejiang, and Guangdong), one northeast province (Jilin), one central province (Hubei), one northwest province (Shaanxi), and one southwest province (Sichuan). The sampling frame for the PBC national firm survey includes all firms who have ever had credit relationship with any financial institution, which is likely to under-sample very small firms. The average firm employs 499 production workers.

The firm survey collected information on the number of employees at four points in time: December 2007, June 2008, December 2008, and June 2009. The initial employment measure predates implementation of the new Labor Law, the second comes six months after the new Law was implemented but before the onset of the global economic crisis, the third is at the height of the economic crisis, and the fourth is at a point after the crisis when China's overall employment situation had substantially recovered. Our main interest is on the total

¹One caveat to this result is that it is sensitive to assumptions about the complementarity between enforcement strictness and the substance of labor regulations. Our simple model rules out such complementarities, but one might expect that if enforcement strictness did not change, a change in the substance of regulations would have a greater impact in cities with stricter enforcement. This would lead to a prediction that employment growth would be slower in cities with higher initial enforcement. However it is also theoretically possible for the impact of substantive law change to be greater in places with weaker enforcement.

employment of production workers, who account for the vast majority of employees in manufacturing firms.

The firm survey asked a number of questions related to the implementation of the new Labor Law. Our primary measure of the strictness of the enforcement of labor regulations is the firm manager's response to the following question: "How strictly have labor regulations been enforced?" Possible responses are 1=very strict, 2=strict, and 3=not strict. We reverse the coding for choices 1 and 3 so that 3=very strict, and 1=not strict. Thus, higher values correspond to greater strictness. The same question is asked retrospectively about the same four points in time for which we have employment data. The question does not refer specifically to the new Labor Law, so is intended to capture S_t in the theoretical framework. To construct city-level enforcement measures (S_{ct}), we calculate the mean value of all firm responses in each city in each time period. To reduce firm-level endogeneity, we exclude the firm's own report in calculating the city-level enforcement measurement for each firm.²

The firm survey also asked direct questions about the impact of the new Labor Law on labor costs and hiring and firing decisions. We summarize the responses to these questions to provide a descriptive picture of perceptions of the Law's impact. In order to collect more direct evidence on efforts to enforce the law, we also asked questions about how many days were spent training managers about the new Law, and the amount of money spent on training activities. Since these investments were made prior to the implementation of the Law, they can also be considered to be measures of enforcement strictness prior to the Law's implementation.

² There may be concern about reporting biases of managers responding to a survey conducted by the research department of the PBC; in this regard it is worth pointing out that managers were told that the data was for research purposes only and also that the PBC has no direct interest in how well labor regulations are enforced, which are the responsibility of Labor Bureaus. Another concern is that retrospective reporting bias may be correlated with current or past experiences in the local economy or labor market; here we emphasize that when we control for a rich set of city control variables that include growth and current city conditions, the main results are even stronger.

To accurately measure export demand shocks associated with the global economic crisis, we link the firm data to Chinese quarterly customs data on export value from each sector in each city. Using these data, we are able to construct multiple export shock measures (and their lags) based on different timing assumptions. For example, we can define an export shock variable E_{cst} affecting firms in city c and sector s as the log of export value in the past three months ($\ln(\text{exports}_{cst})$) minus the log of export value in the previous three months ($\ln(\text{exports}_{cst-1})$), reflecting the most recent change in city-sector exports. This can be expressed as follows: $E_{cst} = \Delta \ln(\text{exports}_{cst}) = \ln(\text{exports}_{cst}) - \ln(\text{exports}_{cst-1})$,³ where t refers to the past three months and $t-1$ refers to the period 4 to 6 months ago. The lag of this value equals the log value of exports 4-6 months ago minus the log value of exports just prior to the previous employment measure (7-9 months ago). We can also define changes in biannual export totals, or look at the difference between the recent three month export value and the value of exports in the three month period prior to the last employment measure.⁴

Because our main measure of enforcement strictness is a city-level measure, it is important to control for other city-level economic variables that are likely to influence employment. To do so, we linked the firm data to city-level data for 2007 that are published in the 2008 China Urban Statistical Yearbook. The city-level variables include GDP per capita, GDP growth rate, population, government budgetary expenditures per capita, mean wage level, and share of employment in the secondary sector (industry, construction and mining).

5. Methodology

³ In order to be able to take logs without creating missing values, missing or very small city-sector values were set to equal 10000 yuan, which is the value at the 1st percentile of such values.

⁴ We experimented with different definitions of export change based on different durations of time, and with different lag periods. None of these different specifications yielded significant results.

We first estimate models of the determinants of enforcement strictness. We consider both static and dynamic specifications. The static specification is descriptive and captures cross-sectional differences in perceived strictness of enforcement. The estimating equation is the following:

$$S_{ijc,t} = \alpha_1 X_{c,2007} + \alpha_2 X_{ijc} + \alpha_3 \text{Exporter}_{ijc,2007} * \lambda_t + \lambda_t + \varepsilon_{ijc,t} \quad (2)$$

The perceived strictness of enforcement of labor regulations by the manager of firm i in sector j in city c in time period t (S_{ijct}) is posited to be a linear function of initial period city-level characteristics ($X_{c,2007}$), a vector of firm time-invariant characteristics (X_{ijc}), including initial period (2007) characteristics such as firm employment and exporter status; whether the firm was an exporter in 2007 ($\text{Exporter}_{ijc,2007}$) interacted with time period dummy variables (λ_t),⁵ which may capture the impact of periods associated with export shocks (the time dummies are also included separately to capture time-varying factors), and unobserved time-varying shocks or firm characteristics and random measurement error ($\varepsilon_{ijc,t}$).

We also estimate a dynamic model of enforcement strictness, in which we control for enforcement strictness in December 2007 ($S_{c,2007}$). The other right-hand side regressors are similar to before:

$$S_{ijc,t} = \delta_1 S_{c,2007} + \delta_2 X_{c,2007} + \delta_3 X_{ijc} + \delta_4 \text{Exporter}_{ijc,2007} * E_{jc,t} + \lambda_t + \varepsilon_{ijc,t} \quad (3)$$

The only difference is that instead of interacting Exporter_{ijc} with time period dummies, we interact it with our export shock measure(s) described earlier. This is appropriate because by controlling for enforcement strictness prior to the reform, the regression identifies factors affecting changes in enforcement, which could be explained by changes in export opportunities. We also estimate a version of (3) in which enforcement strictness (both currently and prior to the Labor Law) are measured at the city level, with standard errors clustered by city.

⁵All interacted terms also are included independently as regressors.

Next, we turn to our main question of interest—whether implementation of the Labor Law had a negative impact on employment. To do so, a model is estimated of the determinants of the change in the number of production workers employed, specified as follows:

$$\Delta l_{ijc,t} = \beta_1 S_{c,2007} + \beta_2 X_{c,2007} + \beta_3 X_{ijc} + \beta_5 \text{Exporter}_{ijc,2007} * E_{jc,t} * S_{c,2007} + \lambda_t + u_{ijc,t} \quad (4)$$

The dependent variable is change in the log of production workers employed by firm i in sector j in city c in time period t compared to period $t-1$. We are mainly interested in the coefficients β_1 and β_5 , which tell us how changes in employment are related to city-level differences in enforcement strictness prior to the new Labor Law ($S_{c,2007}$). The coefficient β_5 measures the extent to which enforcement had a different effect on firms that were adversely affected by the economic crisis. We first estimate a version of (4) that includes enforcement strictness without interacting it with any other variables in order to get the average effects of enforcement strictness. The other right-hand side variables are the same as in the models of enforcement strictness.

Equation (4) is specified in a way designed to minimize endogeneity bias. At first glance, it might seem more natural to examine how changes in employment are affected by *changes* in enforcement strictness rather than the level of enforcement strictness prior to implementation of the Law. However, changes in the strictness of enforcement are likely to respond to changes in local economic conditions or could even respond directly to changes in employment, leading to omitted variable or simultaneity bias. For example, if strictness increased less in cities with lower economic growth, this would lead us to underestimate the negative impacts of changes in enforcement strictness.

By a similar logic, all firm- and city-level variables are time-invariant or measured in 2007 prior to implementation of the new Law out of concern that they could be affected by

the implementation of the Law or changes in economic conditions correlated with employment changes.

Despite these efforts to reduce endogeneity bias, the concern remains that 2007 city-level enforcement differences are correlated with other city-level differences that affect employment growth. One plausible story is that enforcement strictness is worse in China's interior regions, and that such regions produce fewer new jobs than in China's dynamic coastal areas. If this were the case, then initially strict enforcement would be associated with higher employment growth for reasons unrelated to the strictness of enforcement of labor regulations. In this case, controlling for city-level economic indicators would reduce the estimated positive relationship between enforcement strictness and employment growth. The best that we can do to address this concern is to control for a rich array of city-level characteristics and examine how inclusion of such controls affects the estimation results.

Because our main variable of interest—city-level enforcement strictness in 2007 is defined at the city level, in estimating (4) we cluster standard errors at the city level. However, because we have a relatively small number of clusters (23), the clustered standard errors are likely to be underestimated. To address this issue, we estimate standard errors using the wild cluster bootstrap as recommended by Cameron, Gelbach, and Miller (2008). We also test the robustness of our results to alternative measures of enforcement effort prior to the implementation of the new Labor Law. Finally, we aggregate all of the data to the city level and despite the small sample sizes report the results of city-level regressions as another robustness test to address concerns about underestimating standard errors. All of our robustness checks confirm the main findings.

6. Results

Descriptive Evidence. We begin by presenting some descriptive evidence on the strictness of enforcement of new labor regulations and their impact on firm employment decisions based on the subjective assessments of firm managers. Table 1 describes assessments of whether enforcement of labor regulations was very strict, strict, or not strict at different points in time. The vast majority of respondents found enforcement to be strict (24.6%) or very strict (71.3%) during the most recent period (January to June, 2009), with only 4.0% reporting “not strict”. Managers reported that strictness has increased steadily over time; with 21.6% reporting very strict enforcement in 2007 and 7.3% finding enforcement to be “not strict”. There is no evidence of less strict enforcement in late 2008 or early 2009 when China was hard by the global financial crisis. This is corroborated by household surveys conducted in 6 Chinese cities in 2010 which found that most workers felt satisfied that the new Labor Contract Law had been implemented, and that enforcement had increased steadily over time (Gallagher et al., 2012). Table 1 also breaks down strictness by ownership, province, and firm size. There are large differences in the percentage of firm managers reporting very strict enforcement; 30% for state-owned firms compared to 21% for private and foreign firms, a range of 17% to 35% across provinces, and 18% in the smallest firms compared to 26% in the largest firms.

The firm survey questionnaire also asked whether the new Labor Law had increased labor costs, and whether the Law had affected hiring and firing decisions. Only 20.6% said there was no increase in labor costs, while 68.2% said there had been some increase and 11.2% said that there had been a significant increase. Table 2 summarizes the responses to the questions about actual hiring and firing decisions. Just over one third (34.5%) of managers reported that labor regulations had made it more difficult for their firms to hire and fire workers. Interestingly, more managers reported that the Law had reduced firing (30.8%) than hiring (15.8%). As seen in Table 2, state and collective firms reported somewhat smaller impacts. Foreign firms are more likely to report that the Law made it more difficult to hire

and fire workers, but are less likely to report that the Law actually led them to hire and fire workers. There are significant differences in the reported impacts on employment reported by firms in different provinces, with coastal provinces (Zhejiang, Guangdong, Jiangsu) generally reported larger impacts (Table 2). There is not a significant difference between impacts reported by exporting and nonexporting firms.

In Table 3, we report more objective data on employment changes from the end of 2007 to mid-2009, based on firm accounting data on the number of production workers employed at each point in time. Among all firms, employment of production workers increased by 3.0% from end-2007 to mid-2008, fell by 0.5% from mid-2008 to end-2008, then increased by 2.9% from end-2008 to mid-2009. These firm averages are weighted by initial employment levels, so reflect aggregate employment changes during this period. As seen in Table 3, state-owned firms were already shedding workers in early 2008, and foreign firms and large firms were hit hardest by the global financial crisis. The severe impact on foreign firms makes sense given that many foreign firms produced for export.

Before turning to the regression results, we report summary statistics for all of the variables used in the regression analysis for the regression sample (Table 4). Most of the firms are joint stock or limited liability corporations (52.5%), followed in importance by private firms (29.0%), foreign firms (14.7%), and state or collective firms (3.8%). The numbers of firms sampled in the eight provinces reflect the actual number of firms in each province: 29.6% in Zhejiang, 18.3% in Shandong, 14.0% in Guangdong, 16.1% in Jiangsu, and 13.6% in Guangdong. Jilin, Hubei, Sichuan, and Shaanxi all account for less than 10% each.⁶ Only 25.9% of the sample firms are exporters. The sampled firms are all manufacturers and are categorized into five sectors: food products (11.4%), consumer

⁶Data from 2 of the 25 sampled cities were dropped because of lack of reporting on enforcement strictness in 2007.

products (27.7%), basic materials (31.8%), capital and equipment (22.8%), and other (6.2%). The average age of firms in the sample is 10.9 years.

Determinants of Labor Regulation Enforcement. Next, we report the results from the regression analysis. We first examine the determinants of enforcement strictness (3=very strict, 2=strict, 1=not strict) modeled as linear function.⁷ We estimate equations (2) and (3) and report the results in columns 1 and 2 of Table 5. Column 3 reports the results when we estimate equation (3) using city-level rather than firm-level enforcement strictness. In these and subsequent regressions, the reference categories are the food and beverage sector, state ownership, Zhejiang Province, the smallest size quartile, and the time period from the end of 2007 to mid-2008. In estimating equation (2), data for 2007 are dropped to make the results comparable to the dynamic models.

The results from estimating the static model of enforcement strictness (equation (2), column 1 of Table 5) reveal that enforcement is stricter in “other” sectors, for state-owned enterprises (compared to private and especially foreign enterprises), for firms in Sichuan, Shaanxi, and Jiangsu (in declining order, relative to Zhejiang), and for larger firms. As seen from the coefficients on the time period dummies, strictness increases in each subsequent period, but these differences are statistically insignificant. There is no evidence that strictness increased less for exporters in the post-crisis periods. Finally, strictness was greater in cities with higher GDP per capita, lower 2007 economic growth rates, lower wages, smaller industrial sectors, and smaller populations. These relationships make sense, if one considers that aspirations and resources for enforcing regulations may be higher in richer cities, the cost of regulation may be greater in high-growth, high-wage economies, and monitoring may be easier in smaller cities with smaller industrial sectors.

⁷Estimates using an ordered probit model produced nearly identical results.

Next, we report the results from estimating the dynamic model of enforcement strictness (equation (3), reported in column 2 of Table 5). We add pre-Law enforcement strictness as a regressor, and examine whether changes in strictness are related to city-sector export demand shocks hurting exporters. The export shock variables are changes in export value between the past two quarters and between the previous two quarters. The specification can be interpreted as the determinants of changes in strictness relative to the level of enforcement strictness prior to implementation of the Labor Law. It is possible for the important determinants to differ from those that were important in the static model.

An important finding from estimating the dynamic model is that 2007 enforcement strictness has a coefficient of 0.81 which is highly statistically significant. Because this coefficient is less than 1, it implies that more strict enforcement in 2007 is associated with lower expected enforcement in subsequent years. This could be driven by the fact that firms reporting very strict enforcement initially cannot increase the strictness of enforcement, creating an upper truncation of the distribution. We also are unable to control for bias associated with serial correlation and inclusion of a lagged dependent variable as a regressor, which is likely to create upward bias in the coefficient estimate (and so understate the extent to which higher initial strictness is associated with lower future strictness). The coefficient 0.81 suggests that if a firm experiences one unit (or level) greater initial strictness than another firm (in 2007), the increase in strictness in subsequent time periods is expected to be 0.19 units less. The standard error of the coefficient is sufficiently small that the 0.81 coefficient is statistically significantly different than 1 (or the 0.19 difference in change in strictness is statistically significantly different than 0).

We also find that export shocks associated with the financial crisis are not associated with differences in the strictness of enforcement. This finding is consistent with the lack of descriptive evidence of reduced strictness during the post-crisis period. As for other

covariates, we find that in the dynamic model, sector does not matter, and foreign firms experienced less increase in enforcement than SOEs. Sichuan and Shaanxi show greater increases in strictness, just as they showed greater levels of strictness in the static model. Size continues to be positively associated with strictness. Greater strictness in the most recent period (mid-2009) is now statistically significant. And the signs of the coefficients on city control variables are the same as for the static model and for three of the variables are still statistically significant.

Finally, we estimate the same dynamic model, but replace firm-level strictness measures (both dependent variable and 2007 strictness level) with city-level strictness variables. Results are reported in column 3 of Table 5. The coefficient on 2007 city enforcement is 0.86, somewhat greater than for the firm-level measure of strictness in the previous regression, but still statistically significant from 0, and from 1. This means that at the city level, greater initial strictness of enforcement also is associated with less increase in strictness in the future. When we use the city-level measure, we cluster standard errors at the city level, but recognize that these standard errors could be downward biased due to the small number of clusters (23). We return to this point when discussing the city-level regressions which should not be subject to such bias. As before, we find all of the export shock variables to be of insignificant importance. We also find that cities that increase enforcement strictness more have fewer firms in the raw materials and capital and equipment sectors, have more foreign firms, and are in Sichuan and Shaanxi. The coefficients on the time dummies are statistically significant, reflecting increasing strictness over time, and the coefficients on city control variables have similar signs as magnitudes as for the regression for firm-level strictness, but they are no longer statistically significant. This could suggest that using city-level strictness as the key independent variable to explain changes in employment will not be

confounded with omitted city variables, since observable city variables are only weakly associated with changes in enforcement strictness.

Determinants of Employment Growth. Next, we turn to the estimation results of central policy interest, those from estimating the model of the determinants of changes in firm employment specified in equation (4). The results are presented in Table 6. The dependent variable in all of the regressions is the change in log of employment of production workers during each 6-month period covered by the data (three changes per firm). As noted earlier, our main measure of the strictness of labor regulation enforcement is the 2007 mean strictness level of other firms in the same city, which we argue is predetermined as it dates from before the Labor Law took effect, and so is unlikely to be influenced by changes in the economic environment after the Law was implemented. We have shown that higher initial strictness is associated with smaller increases in enforcement; for this reason we expect higher initial strictness to lead to greater employment growth.

As seen in the first row of results in Table 6, we find strong evidence that stricter 2007 city-level enforcement has a positive effect on employment growth. Looking at the first two columns which report the treatment effect of a change in initial strictness, we find that if the mean city-level enforcement increases by one unit (e.g., from not strict to strict, or from strict to very strict), employment grows 7.3-7.4% faster and this effect is statistically significant at the 1% level. The only difference in the two columns is that the second column includes the 3-month lag of change in log city-sector export value; these extra terms are not statistically significant. In columns 3 and 4, we test whether the impact of labor regulation strictness is intermediated by export shocks by including a triple interaction term including 2007 city enforcement, being an exporter, and city-sector export shock. We do find a positive and significant coefficient (at the 5% level) on the interaction with the most recent export shock in the specification that also includes the 3-month lagged export shock (column 4 of

Table 6). This coefficient on the same term is significant at the 10% level and slightly smaller in magnitude when the lagged export shock is excluded. The coefficient of 0.098 on the interaction term suggests that a 50% fall in exports leads to an impact of a unit increase in 2007 enforcement level on employment that is 4.9% less than without such a shock (recall that the mean effect on employment is 7.4%).

Turning to the other covariates, we find that employment of exporting firms grew 2% slower than non-exporting firms (columns 1 and 2); larger firms grew faster than smaller firms; older firms grew slower; the second half of 2008 had employment growth 3% lower than the first half; firms grew faster in cities with larger population, lower GDP per capita, faster growth in 2007, less government expenditures per capita, higher wages, and a larger industrial share of GDP; finally, firms in the West (Sichuan and Shaanxi) grew slower than those in other provinces while those in Hubei grew relatively faster. Worth noting is that when city controls are not included in the model, the coefficient on prior enforcement falls in magnitude, suggesting that omitted city variables bias our estimate of employment impacts downward.

As discussed above, although all of the employment regressions calculated standard errors clustered at the city level, the relatively small number of clusters (23) leads estimated standard errors to be biased downward (Cameron, Gelbach, and Miller, 2008). To address this problem, we estimate standard errors using a wild bootstrap method developed by Cameron, Gelbach, and Miller (2008). In Table 7, we report the p-value for the coefficient on 2007 city enforcement based on the wild bootstrap for the same regression specification reported in column 1 of Table 7, alongside the p-value calculated without bootstrapping. We are also concerned about whether our results are robust to alternative measures of enforcement strictness prior to the implementation of the Labor Law. To check this, we run the same regression as column 1 of Table 7 using two different measures: the city mean log

of Labor Law training expenditures per firm, and the city mean Labor Law training days for firm managers, where means exclude the firms own data. For these measures, we also report standard errors clustered at the city level, with and without bootstrapping. As seen in Table 7, all of the coefficients are statistically significant, whether we use the wild bootstrap or not. In particular, the effects of 2007 city enforcement and city mean log of Labor Law training costs are statistically significant at the 1% level for all estimated standard errors, and city mean Labor Law training days is significant at the 5% level, with or without clustering. Thus the main result of a positive association between pre-Labor Law enforcement strictness and subsequent employment growth appears to be quite robust.

As a final robustness check, we collapse the data to 69 city-year observations, and also to 23 city observations, to see if the strong statistical association between 2007 city enforcement and both future enforcement strictness and employment growth persist. Although the number of observations is quite small which limits our ability to include a large number of covariates and maintain statistical power, in this case there is no concern that use of regressors measured at the city level with firm-level data is leading to overestimation of the strength of the empirical relationship between labor regulation enforcement and employment growth. Results of this exercise are presented in Table 8. To keep the specification parsimonious given the lack of degrees of freedom, we focus on specifications that include only city-level variables and provincial and time-period fixed effects. For clarity, we suppress reporting of coefficients on the provincial dummy variables and the city control variables. In the first column, we regress city-level enforcement strictness in the periods after implementation of the Labor Law on 2007 city enforcement, and find a coefficient of 0.86, the same as when we used firm-level data (reported in column 3 of Table 5). The coefficient is statistically significant at the 1% level. In the next 6 columns, we report the results of regressions that estimate the determinants of mean changes in log of employment. With the

parsimonious specification, we find a coefficient of 0.088 on 2007 city enforcement, a bit higher than the 0.074 estimate from the firm-level regression and still highly statistically significant. We then check robustness to adding aggregate characteristics of firms in each city, by controlling for the distribution of different firm characteristics one at a time. Except for having exporters and having very large firms, these controls are all statistically insignificant. Their inclusion leads to variation in the size of the coefficient on 2007 city enforcement, ranging from 0.055 to 0.095, but with one exception the coefficient remains statistically significant at either the 1% or 5% significance levels. The one exception is when we control for mean firm age, but the coefficient is statistically insignificant, suggesting that this non-result is likely to be spurious.

Finally, in the last 3 columns of Table 8, we report regressions using 23 city-level observations where the dependent variable is the city mean change in $\log(\text{employment})$ from end-2007 to mid-2009. The coefficient on 2007 city enforcement is still statistically significant (note that we continue to include all city control variables), with a magnitude of 0.264 for a 3-period (18-month) effect which is roughly in line with the one-period coefficient of 0.088. In the last two columns, we investigate whether the 3-period change in city employment is strongly related to the 3-period change in city enforcement strictness, although we remain cognizant that there could be a feedback effect from changes in employment to changes in strictness that is likely to create upward bias in our coefficient estimate assuming that authorities increase strictness less in slow-growing economies. We find the expected negative relationship (coefficient of -0.804, significant at the 10% level). One strategy for addressing the endogeneity problem just described is to use the 2007 enforcement variable as an instrument for the change in enforcement strictness. The validity of this instrument requires that the 2007 city enforcement is not correlated with unobserved factors that affect future employment growth. We find as expected that using an instrument

makes the effect of increased strictness on employment growth even more negative and highly statistically significant. The magnitude of the coefficient more than doubles, suggesting that an increase in strictness of just 0.1 units would reduce employment growth by 20%. This magnitude may be implausibly large, but does highlight the strength of the underlying relationship between city level enforcement strictness and employment growth that exists in the data.

7. Conclusions

In this paper, we have provided the first rigorous assessment of which we are aware of the employment impacts of greater strictness of labor regulation enforcement following the implementation of China's Labor Contract Law at the beginning of 2008. Most managers feel that the new Labor Law is being enforced strictly and that it has increased labor costs. Many report that the Law has influenced hiring and firing decisions.

Our main finding is that cities with lax enforcement of labor regulations before the passage of the Labor Law saw greater increases in enforcement after the Law was enacted, and that these increases were associated with slower employment growth, even after controlling for a host of city economic performance measures. This finding is consistent with a number of studies of the impact of restrictive labor regulations in other countries. The impacts of enforcement of labor regulations in China were mitigated for firms exposed to adverse export demand shocks during the global economic crisis. The positive association between initial enforcement level and subsequent employment growth could also reflect the ability of firms in cities with stricter enforcement to adapt to the regulatory environment to meet their employment goals. This interpretation would imply that negative employment effects of stricter regulation could fall over time.

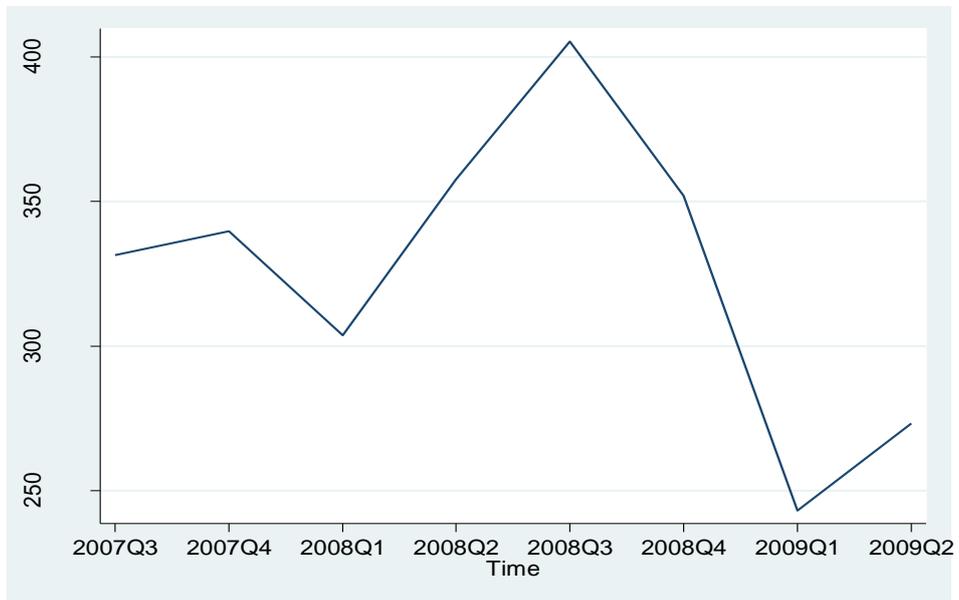
These findings apply only to the sample of industrial firms studied. In the aggregate, employment in China recovered rapidly following the negative shock of the global economic crisis, with wages rising rapidly again by 2010 and reports of labor shortages widespread (Giles et al., 2012). The service sectors absorbed many workers dislocated by the negative export shocks that hit many manufacturing firms. Thus, overall it appears that thus far China has been able to successfully put in place highly protective labor regulations without suffering from aggregate unemployment or even an increase in informalization. Nonetheless, our results suggest that differences in the strictness of enforcement of new labor regulations have affected the relative employment levels of different cities. It remains to be seen whether some of the Labor Contract Law's more protective provisions, such as the granting of permanent labor contracts after the completion of two fixed-term contracts, will eventually have more significant consequences for employment in China in the future.

References

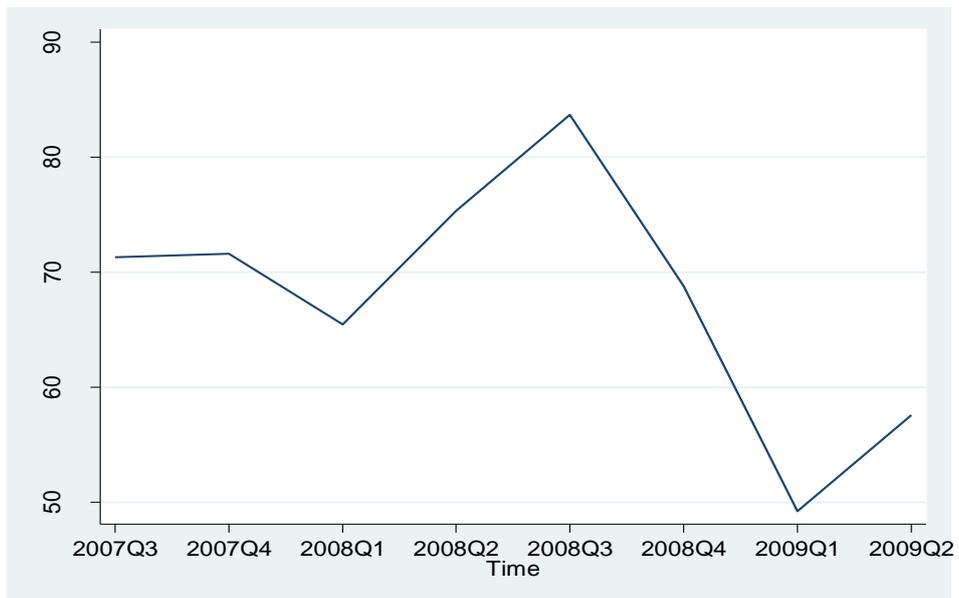
- Ahmad, Ahsan, and Carmen Pages. 2009. "Are all Labor Regulations Equal?: Evidence from Indian Manufacturing", *Journal of Comparative Economics* 37(1): 62-75.
- Almeida, Rita, and Pedro Carneiro. 2005. "Enforcement of Labor Regulation, Informal Labor, and Firm Performance", World Bank Policy Research Working Paper 3756.
- Amin, Mohammad. 2007. "Labor Regulation and Employment in India's Retail Stores", World Bank Policy Research Working Paper 4314.
- Baker, D., A. Glyn, D. Howell, and J. Schmitt. 2005. "Labor market institutions and unemployment : A critical assessment of the cross-country evidence", in David R. Howell. ed., *Fighting Unemployment: The Limits of Free Market Orthodoxy* (Oxford: Oxford University Press).
- Besley, Timothy, and Robin Burgess. 2004. "Can Labor Regulations Hinder Economic Performance?: Evidence from India", *The Quarterly Journal of Economics* 119(1): 91-134.
- Bond, Steven, and J. Van Reenan. 2008. "Microeconomic Models of Investment and Employment," in J.J. Heckman and E.E. Leamer, eds., *Handbook of Econometrics*, Vol. 6A, Elsevier.
- Cameron, A. Colin, Jonah. B. Gelbach, and Douglas Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, Vol 90, No 3, 414-427.
- Chen, Y. and Michael Funke. 2008. *China's New Labour Contract Law: No Harm to Employment?*, BOFIT Discussion Papers 29.2008, Institute for Economics in Transition, Bank of Finland.
- Djankov, Simeon, and Rita Ramalho. 2009. "Employment Laws in Developing Countries", *Journal of Comparative Economics* 37(1): 3-13.
- Dong, Xiao-yuan, and Lixin Colin Xu. 2009. "Labor Restructuring in China: Toward a Functioning Labor Market," *Journal of Comparative Economics* 37(2): 287-305.
- Fallon, Peter, and Robert Lucas. 1993. "Job Security Regulations and the Dynamic Demand for Industrial Labor in India and Zimbabwe", *Journal of Development Economics* 40: 241-275.
- Feldmann, Horst. 2009. "The Unemployment Effects of Labor Regulation around the World", *Journal of Comparative Economics* 37(1): 76-90.
- Fortez, A., and Martin Rama. 2000. *Labor Market Rigidity and the Success of Economic Reforms Across More than One Hundred Countries*, Working Paper, World Bank.
- Freeman, Richard. 2007. *Labor Market Institutions Around the World*, NBER Working Paper 13242.

- Giles, John, Albert Park, Fang Cai, and Yang Du. 2012. "Weathering a Storm: Survey-Based Perspectives on Employment in China in the Aftermath of the Global Financial Crisis", in Arup Banerji, David Newhouse, David Robalino, and Pierella Paci (eds), *Labor Markets in Developing Countries during the Great Recession: Impacts and Policy Responses*, Washington, D.C.: The World Bank.
- Gallagher, Mary, John Giles, Albert Park, and Meiyang Wang. 2012. "China's 2008 Labor Contract Law: Implementation and Implications for Chinese Workers, mimeo.
- Kaplan, David. 2009. "Job Creation and Labor Reform in Latin America." *Journal of Comparative Economics* 37(1): 91-105.
- Nickell, Stephen. 1997. "Unemployment and labor market rigidities: Europe versus North America", *Journal of Economic Perspectives*, 11(3): 55-74.
- OECD. 2004. *OECD Employment Outlook*, OECD, Paris.
- Wu, Bangguo. 2012. *Work Report on the Fifth Session of Eleventh National People's Congress*, <http://www.npc.gov.cn/englishnpc/news/>.

Figure 1: Export Value Nationally and in 25 Sample Cities



National Data



25 Sample Cities

Table 1. Strictness of labor law enforcement 2007-2009

	Very strict	Strict	Not strict
Total	23.0	71.9	5.1
By year:			
2007	21.6	71.1	7.3
2008a	22.5	72.6	4.9
2008b	23.5	72.3	4.2
2009	24.6	71.3	4.0
By ownership:			
State/collective	30.0	69.6	0.5
Private	21.4	71.7	6.9
Joint/Ltd/Other	24.3	71.3	4.4
Foreign	20.7	73.5	5.8
By province:			
Zhejiang	18.4	77.3	4.3
Jiangsu	22.1	75.3	2.6
Guangdong	19.5	78.3	2.2
Shandong	28.8	60.6	10.6
Jilin	31.3	64.6	4.1
Hubei	16.7	76.4	6.9
Shaanxi	22.4	70.7	7.0
Sichuan	35.4	60.6	4.0
By size:			
Smallest quartile	18.3	73.2	8.5
Second quartile	25.0	70.4	4.6
Third quartile	22.0	73.7	4.3
Largest quartile	26.4	70.3	3.3

Table 2: Did Labor Law influence hiring or firing decisions?

	Have labor regulations made it more difficult for your firm to hire and fire workers? (% yes)	Has new Law reduced hiring? (% yes)	Has new Law reduced firing? (% yes)
Total	34.5	15.8	30.8
By ownership type:			
State/collective	28.1	18.4	27.3
Private	31.6	19.0	33.8
Joint/Ltd/Other	35.4	15.2	32.8
Foreign	38.3	13.5	25.8
By province:			
Zhejiang	46.5	17.8	29.7
Jiangsu	31.9	20.3	35.0
Guangdong	45.5	15.8	38.9
Shandong	21.5	13.2	28.7
Jilin	25.4	51.5	34.1
Hubei	21.4	5.3	37.2
Shaanxi	26.0	7.1	27.7
Sichuan	20.4	5.1	13.8
By export status:			
Nonexporter	34.9	16.8	27.2
Exporter	33.5	14.8	35.7

Table 3: Mean Firm Employment Changes (%)

	Jun-08	Dec-08	Jun-09
All firms	3.03	-0.53	2.87
Non-exporters	3.27	0.68	3.20
Exporters	2.76	-1.92	2.48
By ownership:			
State/collective	-6.05	-0.83	1.78
Private	2.61	0.99	5.40
Joint/Ltd/Other	3.70	0.65	1.70
Foreign	3.84	-4.55	4.30
By size (#employees)			
Smallest quartile	2.11	0.48	3.41
Second quartile	3.00	0.28	3.20
Third quartile	3.00	0.16	4.16
Largest quartile	3.05	-0.72	2.63

Note: means are weighted by firm starting employment.

Table 4: Descriptive Statistics

			Mean	Std. Dev.
Ownership type (%)				
State/collective	3.81	City enforcement of labor regulations		
Private	28.99	2007 city enforcement	2.14	0.12
Joint/Ltd/Other	52.5	City Labor Law training expenditure	9.33	0.53
Foreign	14.7	City Labor Law training days	6.47	2.13
Province (%)		City control variables		
Zhejiang	29.61	Log(2007 city population)	4.92	0.70
Jiangsu	16.09	Log(2007 city GDP per capita)	10.46	0.68
Guangdong	13.59	2007 city GDP growth rate	15.57	2.45
Shandong	18.31	Log(2007 budgetary expenditures per capita)	8.19	0.68
Jilin	4.58	Log(2007 city mean wage)	10.17	0.24
Hubei	2.77	2007 city secondary sector GDP share	50.17	12.90
Shaanxi	8.6	$\Delta\log(\text{production workers})$		
Sichuan	6.45	end-2007 to mid-2008	0.033	0.230
Exporter (%)	25.87	mid-2008 to end-2008	0.001	0.166
Industrial sector (%)		end-2008 to mid-2009	0.013	0.212
Food Products	11.37	$\Delta\log(\text{city-sector exports})$ for exporters		
Consumer Products	27.74	end-2007 to mid-2008	0.145	0.404
Raw Materials	31.83	mid-2008 to end-2008	-0.185	0.409
Capital & Equipment	22.82	end-2008 to mid-2009	0.090	0.400
Other	6.24	Firm age in 2007	10.86	9.30

Table 5: Determinants of Enforcement Strictness

	Enforcement	Enforcement	City enforcement
2007 enforcement		0.81 (0.008)**	
2007 city enforcement			0.861 (0.040)**
Sector-consumer products	-0.038 (0.03)	-0.022 (0.02)	-0.005 (0.00)
Sector-raw materials	-0.016 (0.03)	0.024 (0.01)	-0.003 (0.002)*
Sector-capital & equipment	0.034 (0.03)	0.02 (0.02)	-0.01 (0.004)**
Sector-other	0.078 (0.039)*	0.002 (0.02)	-0.003 (0.00)
Ownership-private	-0.097 (0.045)*	0.003 (0.02)	0.005 (0.00)
Ownership-Joint/Ltd/Other	-0.075 (0.04)	-0.011 (0.02)	0.002 (0.00)
Ownership-foreign	-0.161 (0.049)**	-0.054 (0.025)*	0.011 (0.005)*
Province-Jiangsu	0.079 (0.028)**	-0.01 (0.01)	-0.017 (0.03)
Guangdong	0.05 (0.04)	-0.04 (0.019)*	-0.037 (0.04)
Shandong	0.083 (0.04)	0.009 (0.02)	0.012 (0.04)
Jilin	0.14 (0.063)*	0.01 (0.03)	-0.06 (0.04)
Hubei	0.021 (0.07)	-0.056 (0.03)	-0.014 (0.03)
Shaanxi	0.167 (0.046)**	0.085 (0.024)**	0.095 (0.025)**
Sichuan	0.389 (0.046)**	0.154 (0.024)**	0.14 (0.030)**
Size quartile 2 share	0.137 (0.023)**	0.045 (0.012)**	0 (0.00)
Size quartile 3 share	0.144 (0.024)**	0.062 (0.012)**	0.002 (0.00)
Size quartile largest share	0.18 (0.026)**	0.08 (0.013)**	0.006 (0.00)

continued on next page

Table 5: Determinants of Enforcement

	Enforcement	Enforcement	City enforcement
2007 enforcement		0.81 (0.008)**	
2007 city enforcement			0.861 (0.040)**
Sector-consumer products	-0.038 (0.03)	-0.022 (0.02)	-0.005 (0.00)
Sector-raw materials	-0.016 (0.03)	0.024 (0.01)	-0.003 (0.002)*
Sector-capital & equipment	0.034 (0.03)	0.02 (0.02)	-0.01 (0.004)**
Sector-other	0.078 (0.039)*	0.002 (0.02)	-0.003 (0.00)
Ownership-private	-0.097 (0.045)*	0.003 (0.02)	0.005 (0.00)
Ownership-Joint/Ltd/Other	-0.075 (0.04)	-0.011 (0.02)	0.002 (0.00)
Ownership-foreign	-0.161 (0.049)**	-0.054 (0.025)*	0.011 (0.005)*
Province-Jiangsu	0.079 (0.028)**	-0.01 (0.01)	-0.017 (0.03)
Guangdong	0.05 (0.04)	-0.04 (0.019)*	-0.037 (0.04)
Shandong	0.083 (0.04)	0.009 (0.02)	0.012 (0.04)
Jilin	0.14 (0.063)*	0.01 (0.03)	-0.06 (0.04)
Hubei	0.021 (0.07)	-0.056 (0.03)	-0.014 (0.03)
Shaanxi	0.167 (0.046)**	0.085 (0.024)**	0.095 (0.025)**
Sichuan	0.389 (0.046)**	0.154 (0.024)**	0.14 (0.030)**
Size quartile 2	0.137 (0.023)**	0.045 (0.012)**	0 (0.00)
Size quartile 3	0.144 (0.024)**	0.062 (0.012)**	0.002 (0.00)
Size quartile largest	0.18 (0.026)**	0.08 (0.013)**	0.006 (0.00)

Table 5: Determinants of Enforcement Strictness, p.2

Firm age	-0.002 (0.00)	0 0.00	0 0.00
end-2008	0.016 (0.02)	0.013 (0.01)	0.018 (0.007)*
mid-2009	0.032 (0.02)	0.03 (0.010)**	0.031 (0.007)**
Log(2007 city population)	-0.04 (0.020)*	-0.019 (0.01)	-0.018 (0.01)
Log(2007 city GDP per capita)	0.235 (0.055)**	0.03 (0.03)	0.059 (0.04)
2007 city GDP growth rate	-0.02 (0.007)**	-0.009 (0.003)**	-0.007 (0.01)
Log(2007 budgetary expenditures p.c.)	0.057 (0.04)	0.097 (0.020)**	0.079 (0.04)
Log(2007 city mean wage)	-0.227 (0.115)*	-0.091 (0.06)	-0.106 (0.10)
2007 city secondary sector GDP share	-0.007 (0.002)**	-0.002 (0.001)*	-0.003 (0.00)
Exporter	0.027 (0.03)	-0.013 (0.01)	0.003 (0.00)
Exporter x end-2008	-0.002 (0.043)		
Exporter x mid-2009	-0.012 (0.043)		
$\Delta\log(\text{exports})$		-0.001 (0.010)	0.001 (0.002)
Exporter x $\Delta\log(\text{exports})$		-0.027 (0.020)	-0.001 (0.003)
Lag of $\Delta\log(\text{exports})$		0.005 (0.008)	-0.001 (0.001)
Exporter x Lag of $\Delta\log(\text{exports})$		0.012 (0.021)	0.007 (0.006)
Constant	2.311 (0.824)**	0.538 (0.42)	0.47 (0.60)
Observations	3904	3887	4326
R-squared	0.05	0.75	0.94
Standard errors in parentheses			
* significant at 5%; ** significant at 1%			

Table 6. Determinants of Changes in Firm Employment

	$\Delta\log(\text{production workers})$			
2007 city enforcement	0.074 (0.015)**	0.073 (0.015)**	0.075 (0.017)**	0.074 (0.016)**
2007 city enforcement x Exporter			-0.007 (0.06)	-0.018 (0.05)
2007 city enforcement x $\Delta\log(\text{exports})$			-0.027 (0.02)	-0.042 (0.03)
2007 city enforcement x Exporter x $\Delta\log(\text{exports})$			0.089 (0.04)	0.098 (0.044)*
2007 city enforcement x Lag of $\Delta\log(\text{exports})$				-0.035 (0.04)
2007 city enforcement x Exporter x Lag of $\Delta\log(\text{exports})$				-0.087 (0.08)
Exporter in 2007	-0.021 (0.009)*	-0.019 (0.009)*	-0.005 (0.12)	0.02 (0.12)
$\Delta\log(\text{exports})$	0.01 (0.01)	0.01 (0.01)	0.071 (0.05)	0.102 (0.07)
Exporter x $\Delta\log(\text{exports})$	-0.009 (0.01)	-0.007 (0.01)	-0.206 (0.10)	-0.224 (0.102)*
Lag of $\Delta\log(\text{exports})$		0.001 (0.01)		0.078 (0.08)
Exporter x Lag of $\Delta\log(\text{exports})$		0.027 (0.02)		0.215 (0.19)
Sector				
Consumer Products	-0.019 (0.02)	-0.019 (0.02)	-0.02 (0.02)	-0.02 (0.02)
Raw Materials	0.001 (0.01)	0.001 (0.01)	0.001 (0.01)	0.001 (0.01)
Capital & Equipment	0.005 (0.01)	0.005 (0.01)	0.005 (0.01)	0.004 (0.01)
Other	0.001 (0.02)	0.001 (0.02)	0.000 (0.02)	0.001 (0.02)
Ownership				
Private	0.017 (0.02)	0.017 (0.02)	0.017 (0.02)	0.018 (0.02)
Joint/Ltd/Other	0.018 (0.02)	0.018 (0.02)	0.018 (0.02)	0.019 (0.02)
Foreign	0.004 (0.02)	0.004 (0.02)	0.004 (0.02)	0.004 (0.02)
Size				
Quartile 2	0.017 (0.01)	0.017 (0.01)	0.017 (0.01)	0.017 (0.01)
Quartile 3	0.021 (0.008)*	0.021 (0.008)*	0.022 (0.008)*	0.022 (0.008)*
Largest quartile	0.024 (0.009)*	0.025 (0.009)*	0.025 (0.009)*	0.025 (0.009)*
Firm age	-0.002 (0.001)**	-0.002 (0.001)**	-0.002 (0.001)**	-0.002 (0.001)**

continued on next page

Table 6. Determinants of Changes in Firm Employment, p.2

end-2008	-0.03	-0.031	-0.03	-0.031
	(0.009)**	(0.009)**	(0.009)**	(0.009)**
mid-2009	-0.017	-0.015	-0.017	-0.014
	(0.008)*	(0.01)	(0.008)*	(0.01)
Log(2007 city population)	0.017	0.017	0.017	0.017
	(0.004)**	(0.004)**	(0.004)**	(0.004)**
Log(2007 city GDP per capita)	-0.03	-0.03	-0.03	-0.031
	(0.011)*	(0.011)*	(0.011)*	(0.011)*
2007 city GDP growth rate	0.005	0.005	0.005	0.005
	(0.001)**	(0.001)**	(0.002)**	(0.001)**
Log(2007 budgetary expenditures p.c.)	-0.042	-0.042	-0.043	-0.042
	(0.009)**	(0.009)**	(0.009)**	(0.009)**
Log(2007 city mean wage)	0.095	0.096	0.098	0.097
	(0.026)**	(0.026)**	(0.029)**	(0.028)**
2007 city secondary sector GDP share	0.001	0.001	0.001	0.001
	(0.000)*	(0.000)*	(0.000)*	0.00
Province				
Jiangsu	0.001	0.001	0.002	0.002
	(0.01)	(0.01)	(0.01)	(0.01)
Guangdong	-0.015	-0.014	-0.014	-0.014
	(0.01)	(0.01)	(0.01)	(0.01)
Shandong	0.009	0.01	0.011	0.011
	(0.01)	(0.01)	(0.01)	(0.01)
Jilin	-0.01	-0.009	-0.008	-0.009
	(0.01)	(0.01)	(0.01)	(0.01)
Hubei	0.103	0.103	0.104	0.103
	(0.011)**	(0.011)**	(0.011)**	(0.011)**
Shaanxi	-0.026	-0.025	-0.026	-0.025
	(0.008)**	(0.008)**	(0.008)**	(0.008)**
Sichuan	-0.033	-0.032	-0.032	-0.031
	(0.015)*	(0.014)*	(0.015)*	(0.015)*
Constant	-0.633	-0.64	-0.66	-0.647
	(0.197)**	(0.196)**	(0.224)**	(0.217)**
Observations	4326	4326	4326	4326
R-squared	0.04	0.04	0.04	0.04
Robust standard errors in parentheses				
* significant at 5%; ** significant at 1%				

Table 7: Robustness Checks: Enforcement Measures and Wild Bootstrap Standard Errors

	Coefficient	Clustered Standard Error p-value	Wild Bootstrap p-value
2007 City enforcement	0.074	0.000	0.000
Log(city mean Labor Law training expenditures)	0.0298	0.000	0.000
City mean Labor Law training days	0.00588	0.013	0.046

Table 8: City-level Regressions

	City enforcement								end-2007 to mid-2009	
	Δlog(production workers)								Δlog(production workers)	
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	IV
2007 city enforcement	0.863 (0.046)**	0.088 (0.025)**	0.095 (0.026)**	0.055 (0.03)	0.085 (0.033)*	0.093 (0.036)*	0.08 (0.027)**	0.264 (0.104)*		
Change in city enforcement									-0.804 (0.38)	-2.037 (0.773)**
end-2008	0.015 (0.01)	-0.034 (0.008)**	-0.034 (0.008)**	-0.034 (0.007)**	-0.034 (0.008)**	-0.034 (0.008)**	-0.034 (0.007)**			
mid-2009	0.032 (0.007)**	-0.02 (0.008)*	-0.02 (0.007)**	-0.016 (0.01)	-0.019 (0.008)*	-0.02 (0.007)**	-0.02 (0.008)*			
Exporter share			-0.062 (0.031)*							
Mean firm age				-0.004 (0.00)						
Size quartile 2 share					-0.146 (0.11)					
Size quartile 3 share					0.003 (0.05)					
Size quartile largest share					-0.151 (0.067)*					
Ownership-private share						-0.323 (0.27)				
Ownership-Joint/Ltd/Other share						-0.393 (0.26)				
Ownership-foreign share						-0.416 (0.26)				
Sector-consumer products share							0.168 (0.14)			
Sector-raw materials share							0.174 (0.14)			
Sector-capital & equipment share							0.114 (0.15)			
Sector-other share							0.271 (0.19)			
Observations	69	69	69	69	69	69	69	23	23	23
R-squared	0.94	0.63	0.65	0.64	0.65	0.66	0.66	0.87	0.85	0.65
Robust standard errors in parentheses, coefficients on provincial dummies and city variables not reported * significant at 5%; ** significant at 1%										