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Regulation and Trade in Development
Explaining Productivity at the Firm Level

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General Introduction

The convergence of median incomes to OECD levels is, for developing countries, imperative in order to reduce poverty and achieve steadily rising living standards. Globalization's advancement in recent decades has facilitated high rates of growth through trade and investment, enabling millions to escape poverty, and large sub-regions of some developing countries to approach OECD living standards. Nevertheless, for many developing countries and sub-regions, institutions and associated regulatory settings are not appropriate to mediate rapid catch-up, and more structural reforms are needed.

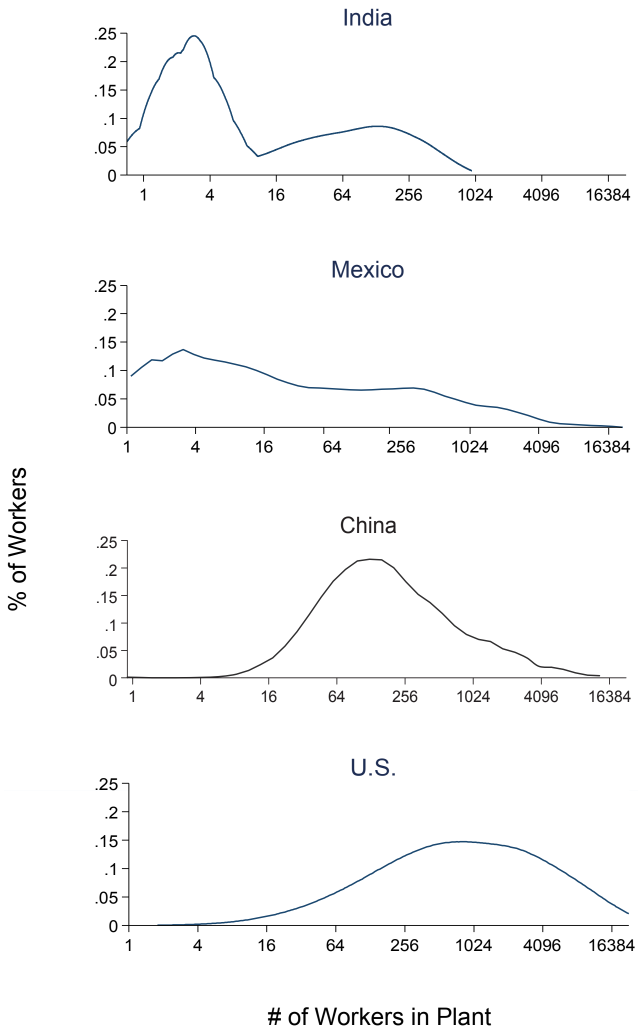
Research on trade and development has sought to shed light on the sources of economic growth, by seeking to understand its fundamental driver: total factor productivity. Considerable insight into some of the main issues has been achieved, yet many mysteries remain, from the impact of various regulatory distortions, to the causal relationship between trade and productivity. At the same time, firm and plant-level data have revealed new stylized facts about the mechanics of trade, plus development outcomes such as the dispersion of productivity within markets, the distribution of business units by size, the extent of concentration, and market power.

New theoretical work has begun to address some of these mysteries and new stylized facts, by incorporating heterogeneous firms into representative agent trade models, many recently inspired by Melitz (2003).[1] This work has helped to fill an important gap in our understanding about the high degree of creation and

[1] Melitz (2003) adapts Hopenhayn's (1992) dynamic industry model to monopolistic competition in a general equilibrium setting, and in so doing, provides an extension of Krugman's (1980) inter-industry trade model incorporating firm level productivity differences.

destruction of business units and jobs that occurs in economies at all levels of development. Less well understood, though, is the impact of various types of market imperfections that exist in developing economies as a result of inappropriate labor and product market regulations, on both the dynamics of firm exit and entry as well as within-firm employment adjustment.

Figure 1: Distributions of employment by firm size



Source: Montage of Hsieh and Klenow (2009, 2012).

Aside from productivity-enhancing dynamics, market access, size and power play a central role in many of the newest trade models (i.e. Melitz and Ottaviano, 2008). To take a tangible example, Figure 1 shows the distribution of employment by firm size in a number of major economies. Here we can see that enormous differences exist across countries, with employment in India concentrated primarily in the microenterprise sector, while Mexico and China fall somewhere in-between India and the United States. Though these firm-wise distributions of employment could be partly driven by market size effects, within-sector differences are so large that it would appear policy and institutional distortions play a major role in blocking adjustment and reallocation (Restuccia and Rogerson, 2013).

Most studies in the trade literature have taken a relatively narrow view of these policy barriers, focusing on market entry ("beachhead") and geographic ("border barrier") trading costs, with little examination of behind-the-border costs or those fixed or variable costs that may impinge directly on underlying firm and job dynamics, such as product market regulation (PMR) and employment protection legislation (EPL). There is a need to make a link between these broader regulatory costs and gains through reallocation in the trading and non-trading sectors.[2]

This dissertation seeks to understand the influence of these different types of behind-the-border costs on firm-level productivity outcomes. It does this by examining different types of regulatory rules and institutional settings in multiple country contexts, with a various types of data, along with relevant econometric methodologies:

- The **first chapter** examines how import penetration affects firms' productivity growth taking into account the heterogeneity in firms' distance to the efficiency frontier and country differences in product market regulation. Using firm-level data for a substantial number of OECD countries from the late 1990s to late 2000s, the analysis reveals non-linear effects of both sectoral import penetration and *de jure* product market regulation measures depending on firms' positions along the global distribution of productivity levels. A magnifying effect is found between import penetration and domestic

[2] One particularly promising study by Koeniger and Prat (2007) examines intermediate outcomes of PMR on firm entry and exit, and EPL on job turnover; however, the model does not feature a trade sector or look at productivity outcomes.

barriers to entry, conditional on a firm's distance to the technological frontier. The heterogeneous effects of international competition and domestic product market regulation on firm-level productivity growth are consistent with a neo-Schumpeterian view of trade and regulation. Close to the technology frontier, import competition has a strongly positive effect on firm-level productivity growth, with stringent domestic regulation reducing this effect substantially. However, far from the frontier, neither import competition nor its interaction with domestic regulation has a statistically significant effect on firm-level productivity growth. The results also suggest that insufficient attention has been made in the trade literature to within-firm productivity growth.

- The **second chapter** examines the effects of labor market reform on plants in different Indian states. Using plant-level data for a period from the late 1990s to late 2000s, the study provides plant-level cross-state/time-series evidence of the impact of reforms of employment protection legislation (EPL) and related labor market policies on productivity in India. Identification of the effect of EPL follows from a difference-in-differences estimator inspired by Rajan and Zingales (1998) that takes advantage of the state-level variation in labor regulation and heterogeneous industry characteristics. The fundamental identification assumption is that EPL is more likely to restrict firms operating in industries with higher labor intensity and/or higher sales volatility. The results show that firms in labor intensive or more volatile industries benefited the most from labor reforms in their states. Point estimates indicate that, on average, firms in labor intensive industries and in flexible labor markets have TFP residuals 14% higher than those registered for their counterparts in states with more stringent labor laws. However, no important differences are identified among plants in industries with low labor intensity when comparing states with high and low levels of EPL reform. Similarly, the TFP of plants in volatile industries and in states that experienced more pro-employer reforms is 11% higher than that of firms in volatile industries and in more restrictive states. In sum, the evidence presented here suggests that the high labor costs and rigidities imposed through Indian federal labor laws have been eased through labor market reforms at the state level.

- The **third chapter** looks at Indian exporters who took advantage of capital account liberalization to invest abroad, and explores whether they gained through learning-by-doing from the mid-1990s and to late 2000s. The magnitude of outbound foreign direct investment (OFDI) flows from countries such as India have raised important questions about whether firms benefit in terms of improved efficiency or whether such ventures primarily seek new markets for their products. In order to shed light on these questions, we go beyond the self-selection issues that are well known in the internationalization literature (Helpman *et al.*, 2004), and examine the causal impact of OFDI on firm-level productivity outcomes, using firm matching techniques combined with a differences-in-differences counterfactual estimator. The results are unambiguous, and imply that there is no learning-by-doing among OFDI firms, compared to similar firms that are already exporting, suggesting that there are alternative motivations aside from efficiency for Indian firms that invest abroad. However, strong evidence is found of rapid gains in scale, in terms of sales revenue.

- The **fourth chapter** explores how legal system quality in different Mexican states has impacted the size of firms over the 2000s. Legal systems provide the basic institutions for firms and markets to operate, and their quality can have important consequences on the size distribution of firms, who rely on them for contract enforcement. This paper uses the variation in legal system quality across states in Mexico to examine the relationship between judicial quality and firm size. Although the country has a single legal system, its implementation and procedures vary widely, while development outcomes there are more imbalanced and unequal than in any other country of the OECD. The effect of the legal system on inter-state firm efficiency is therefore examined. Building on Laeven and Woodruff (2007), this study uses economic census microdata and contract enforcement ratings to examine the impact of state-level legal institutions on firm and industry-level outcomes. A robust effect of judicial quality is observed on the firm size distribution and efficiency, instrumenting for underlying historical determinants of institutions. Indicative evidence is found that the effect is strongest in more capital-intensive industries. Market size and distance-to-market are also found to matter for firm-size outcomes, consistent with the new trade literature.

The chapters each address particular research questions, using data and econometric approaches that aim to robustly identify the empirical impact of various policies and institutions. Each chapter uses a slightly different research design, given the policy and outcome data available for the questions at hand, the administrative structure of the countries or jurisdictions that are analyzed, and the econometric challenges that are faced.

The basic structure of the datasets and their main features are summarized in the **Data Appendix** to this dissertation. Beyond the merging of multiple types of data at various levels of aggregation, and important decisions about price deflation, a key feature of the analysis regards whether productivity outcomes are measured at the establishment or the firm level. In the former case, the investment data series is more reliable, and plant exit can be determined with more certainty. This is the case with the second chapter, and here we use the Olley and Pakes (1996) methodology for productivity estimation, which takes advantage of this information to address concerns about simultaneity biases in the production function estimation. However, when only firm-level data are available — as in the case of the first and third chapters — we favor the Levinsohn and Petrin (2003) approach, which uses materials to address concerns about simultaneity biases and reduce measurement error. In the fourth chapter, the focus is on weighted average firm size, though a semi-aggregate production function is also estimated using an instrumental variables approach.

When examining questions related to the impact of policy and institutional settings, establishing causality is a fundamental challenge. The approach taken in the chapters is to take advantage of the heterogeneity across countries, sub-national jurisdictions, industries and time. Difference-in-differences estimation is used repeatedly, and fixed effects panel data estimators are used where feasible. Firm matching approaches and instrumental variable approaches are used when they are not. In the end, the results appear to be robust to most of the "stress-tests" that are employed — some of which are included in Annexes to each chapter — helping to validate the headline findings.

While addressing the questions that are raised inevitably involves some difficult tradeoffs, we hope that the approaches employed and the results obtained offer useful practical examples of how to tackle complex empirical questions with important policy implications, and may also stimulate new ideas for future theoretical development.

Chapter I

Import competition, domestic regulation and firm-level productivity growth in the OECD[1]

1 Introduction

Globalization has dramatically reduced explicit barriers to international trade in OECD as well as non-OECD countries over recent decades. These tariff-type barriers have fallen far enough in manufacturing that they likely no longer represent a major obstacle to goods exporting and importing (Bouët *et al.*, 2008). Institutional limits on protection that prevent countries from raising tariffs even in times of economic crisis have so far proven effective in preventing a bout of defensive, or retaliatory, anti-trade measures, even in the context of the panic-inducing Great Recession that we have just experienced (OECD, 2011).

[1] This chapter is a revised version of FREIT working paper No. 307 (2011), "Trade, Regulation and Firm-Level Productivity in the OECD," and OECD Economics Department working paper No. 980 (2012), "Import Competition, Domestic Regulation and Firm-Level Productivity Growth in the OECD," jointly written with Sarra Ben Yahmed (Université de la Méditerranée–GREQAM and Institut d'études politiques de Paris).

Nevertheless, behind-the-border regulation still remains quite stringent in many economies (Wölfl *et al.*, 2009; Conway *et al.*, 2010). Stringent regulation of product markets obstructs firm entry, operation and exit, thereby limiting competition, which can reduce firms' ability and incentives to improve their productivity. However, the mechanisms that cause weak competition to hamper productivity are not fully understood. In their recent review of endogenous growth theory, Aghion and Howitt (2009) argue that there is a U-shaped relationship between the degree of competition and productivity, where firms closer to the global technological frontier face stronger incentives to innovate in order to overcome the potential threat of new entrants. Near the frontier, stringent regulation reduces neck-to-neck competition and innovation, harming firm productivity. In contrast, farther from the frontier, Schumpeter (Mark II)-type effects dominate and firms face discouragement, making innovation and productivity growth less likely, regardless of regulation.

New trade theory also incorporates heterogeneity in firm technological efficiency, though with a different perspective, as most theoretical papers take firms' productivity levels as given and investigate how sectoral productivity changes in the aftermath of trade liberalization. Various models featuring heterogeneous firms, notably Melitz's (2003), posit that trade liberalization yields entry and exit dynamics that reallocate market shares from low-productivity firms to higher productivity firms that compete in international markets. Bernard *et al.* (2007) show how this process can help strengthen comparative advantage through creative destruction, though in neither case do the dynamics come about through *intra*-firm productivity dynamics. Melitz and Ottaviano (2008) highlight the pro-competitive effect of trade taking into account market size. They show that sectoral productivity can be enhanced through increasing toughness of import competition, implying the potential for dynamic gains from policy reform.

This paper builds on the intuition of new trade models on the pro-competitive effect of trade along with the prediction of endogenous growth models where the effect of competition and regulation on firm productivity depends on firms' efficiency levels. It takes a difference-in-differences approach that uses the insights from the new trade literature to identify the empirical effects of import competition and anti-competitive domestic regulation on productivity at the firm level, also incorporating distance-to-technological frontier effects. In so doing, it develops new evidence in support of both sets of theories, suggesting that (*i*) trade models could be enriched by incorporating a distance-to-frontier and intra-firm productivity dimension, and (*ii*) distance-to-frontier ideas could be further enriched by examining their interactions with trade, helping to better explain the underlying mechanisms.

Beyond these general insights, several important findings stand out:

- Stronger competition, in the form of higher import penetration, is associated with higher firm-level productivity growth close to the technological (measured in terms of productivity levels) frontier, an effect that remains robust even when estimated in lags, though it varies when the smallest firms are over-sampled in the dataset. The main result is consistent with the predictions of the Aghion endogenous growth model as well as the Melitz and Ottaviano framework, though the latter would not have predicted a differential firm-level effect vis-à-vis the technology frontier.

- Close to the technology frontier, anti-competitive product market regulation substantially reduces the scope for TFP improvements spurred by import competition; far from the frontier, the interaction between regulation and foreign competition is not statistically significant. The effect of product market regulation depends on the sectoral trade orientation; more precisely, we find that product market regulation damages the scope for productivity growth at least in part by reducing the competition-enhancing effect of import competition on top firms.

- The productivity-enhancing effect of import competition and the mitigating effect of product market regulation are robust to the inclusion of a Herfindahl index that captures the market shares concentration across firms, controls for the stringency of upstream regulation, as well as country-time fixed effects and industry fixed effects that capture respectively country specific policies or macroeconomic shocks and time-invariant industry-specific characteristics such as the intensity of ICT use.

In order to examine these questions, a large-scale firm database (Amadeus) is examined that covers half of the OECD member countries, which is then re-weighted to be representative of the actual size distribution of firms in the whole population, and matched with regulation and trade datasets. This firm data is sufficient to allow for the measurement of robust productivity measures that take account of potential simultaneity biases. Unique OECD indexes of product market regulation are used to measure *de jure* regulatory settings, at the country level and across time. International trade data are matched with production data, to generate measures of import penetration at the detailed industry level.

Previous evidence on the effect of domestic regulation on productivity has examined various channels, though these studies have generally not examined their interaction with trade. A number of empirical studies, particularly those of the OECD (2003, 2006, 2011), have found distortionary effects of indicators of product and labor market regulation on overall productivity outcomes. For instance, Arnold *et al.* (2010) look at the effect of product market regulation on firm-level productivity – through the ICT channel – and find supportive evidence of distance-to-frontier effects. At the industry level, Bourlès *et al.* look at the effect of upstream product market regulation on sector-level productivity, and they also find distance-to-frontier effects. Conway *et al.* found similar sectoral effects for broader market regulation, while Nicoletti and Scarpetta (2003) found related, yet inverted, effects with respect to the distance-to-frontier.

More aggregate empirical work has used less detailed indicators of institutional and policy settings to examine the role of institutions in mediating the role of trade in affecting overall growth and productivity outcomes. Cross-country studies include Dollar and Kraay (2003), Rodrick *et al.* (2004), Alcalá and Ciccone (2004), and Freund and Bolaky (2008), who have tried to disentangle the respective roles of institutions and trade for growth at the country level. On balance, the evidence appears to suggest that institutions have a more fundamental role, as they complement trade liberalization, and strengthen the long term effects of trade on growth, by enhancing the role of comparative advantage. However, the types of policies and reforms that may drive productivity in this context are still not clear from this literature.[2]

Research at the level of the firm seems more promising to reveal the underlying mechanics of how policies may work through trade to affect productivity and growth outcomes. Firm-level analysis has revealed a substantial role for product market regulation in affecting the margins of firm exit and entry as well as reallocation of productivity across firms (e.g., Bartelsman *et al.* (2009)). However, this work does not explicitly consider how international trade may drive and/or reinforce these margins.

There have been a series of country-specific firm-level studies that have identified substantial roles for international trade regulation specifically in affecting firm entry/exit and reallocative margins, for Chile (Pavcnik, 2002; Bas and Ledezma, 2010), Columbia (Fernandez, 2007), France (Bas and Strauss-Kahn, 2011), India (Topalova, 2004; Goldberg *et al.*, 2010), Indonesia (Amiti and Konings, 2007) and the UK (Aghion *et al.*, 2009). Several of these studies show that reductions in import barriers can help to boost within-firm productivity (Amiti and Konings, 2007; Bas and Ledezma, 2010; Goldberg *et al.*, 2010). However, these single-country studies do not address behind-the-border regulation, which varies principally *across* countries.[3]

We contribute to the literature by attempting to answer the questions raised

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[2] One promising approach from a related literature uses incomplete contract theory to examine the effect of overall institutional quality on the organization of trade. Studies following this approach include Acemoglu *et al.* (2007), who find an important role of contracting institutions leading to strengthened comparative advantage.

[3] Although not focused on productivity, Crozet *et al.* (2012) take an innovative approach to addressing the effect of different countries' domestic regulations on services trade, using bilateral export data from French firms. The study finds strong detrimental effects of purely domestic regulations on both the extensive and intensive export margins of the firms – with domestic regulations being even more damaging for trade than explicit international trade barriers.

above by estimating productivity growth equations at the firm level where exposure to international markets and to domestic regulation both interact. We find that their effect can be non-linear and depends on the characteristics of heterogeneous firms – especially their distance to the global technological frontier.

The paper proceeds as follows. The second section describes the data and sampling frame, the construction of productivity, import penetration and domestic regulation measures. The third section motivates the empirical approach, and examines the effects of import penetration and domestic regulation on firm-level productivity growth. The fourth section concludes.

2 Data and measurement

In order to investigate the questions raised above, firm-level data are used to compute productivity measures, sectoral trade data are used to measure foreign competition, and restrictive regulation is measured using the OECD's economy-wide indexes of product market regulation.

2.1 Firm-level data: Amadeus

Firm level data are used based on company reports included in the Amadeus database compiled by the Bureau van Dijk. This database covers European OECD countries over the time period 1995–2005. The countries with sufficient numbers of firms for our use are Belgium, the Czech Republic, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Poland, Portugal, Spain, Sweden and the United Kingdom. The data for Greece are not used since they lack wage and materials data. While all the countries included are OECD members, the former transition economies of Central and Eastern Europe are likely to have a wider dispersion of productivity across firms than the other countries as a result of their one-time structural transitions.

Data are cleaned for potential outliers that we identify by several criteria. First, firms with negative values for any variable entering the production function – operating revenue or value added, wages, capital stock, material inputs – or with depreciation higher than net capital stock are eliminated from the sample. Firms that report extreme year-to-year variation in ratios between production function variables and extreme reversals in one of these variables are not retained, either. Finally, outliers have been removed by eliminating the top and bottom one percent of the productivity distribution and subsequently re-estimating productivity without these extreme observations. The productivity estimation is described in more detail below.

Sectoral coverage includes all tradable goods and services, including mining, all of manufacturing (ISIC 15 to 37), electricity, utilities (ISIC 40, 51, 52), transport and communications (ISIC 60 to 64), business activities as R&D, advertising (ISIC 71 to 74) and recreational and cultural activities (ISIC 92). Consolidated accounts in the Amadeus dataset are dropped, which avoids problems of double-counting.

2.2 Sampling frame

The Amadeus data are broadly representative of the business sectors of OECD countries, since they include virtually all public companies, and as such are a fair representation of larger companies. However, smaller firms are underrepresented, since they typically do not report balance sheet information publicly. In addition, not all firms in the Amadeus data report information on all production function variables. The remaining sample used in this study includes only firms for which TFP estimates could be obtained.

In order to ensure that the sample of firms is as representative as possible of the population distribution of firms across size classes, sectors and countries, a re-sampling procedure was applied (see Schwellnus and Arnold, 2008). First, population weights for every size-sector-country strata were calculated from the OECD Structural Demographic Business Statistics (SDBS) database for the year

2000. Second, random draws with replacement from each size-sector-country strata in the TFP sample were taken until the weight of each strata corresponds to its population weight.[4]

This method resulted in a sample that is representative of the population distribution along the dimensions of employment size, sector and country. The sample size is then set to 139,065 firms (drawn from a set of 79,513 real firms) which results in 831,187 firm-year observations. While this method yields a more representative sample in the year 2000, it may also increase measurement error since 'successful' smaller firms are over-sampled. As a result, the resampled dataset may be less representative as the time period shifts away from the year 2000 since normally such firms have high rates of entry and exit. Thus, both the non-resampled and the resampled data are considered in the basic specifications in order to ensure robustness.

2.3 Estimation of Total Factor Productivity

Our productivity variable, total factor productivity (TFP), measures the firm-level efficiency in the use of all inputs. We calculate TFP as the residual from the estimation of a logarithmic Cobb-Douglas production function of the form:

$$\ln y_{isct} = \alpha_{sc} \ln l_{isct} + \beta_{sc} \ln k_{isct} + \epsilon_{isct} \tag{I.1}$$

where the subscripts stand for the firm i from country c operating in sector s at time t. The dependent variable of the production function is the firm's value-added (y). The production factors are labor (l) and capital (k). When value-added was not available, it was imputed as the residual between operating revenue and material inputs. Labor inputs are measured using the total wage bill, while net capital stocks were used to measure capital input. Nominal values are deflated using sector-specific

[4] The re-sampling procedure is restricted to firms with at least 20 employees since the coverage below this threshold is unsatisfactory. The firm size classes used for resampling (from SDBS) are: 20-49; 50-99; 100-499; 500 or more employees.

price indexes, with the exception of capital stocks that have been deflated using deflators for gross fixed capital formation. The production function is estimated at the sector-country level sc, in order to avoid strong assumptions on the homogeneity of production technologies across sectors and OECD countries. The residuals ϵ_{isct} represent plant-specific efficiency in the year t.

The ideal measure of TFP would be in volume terms, "physical TFP". However, given the available data, we use a "revenue-based TFP". The pluses and minuses of using various measures are discussed in Foster *et al.* (2008). In most business micro data sets like Amadeus, establishment-level prices are unobserved. Thus, establishment output is measured as revenue divided by a common industry-level deflator. This method embodies within-industry price differences in output and productivity measures. Difficulties arise when prices reflect idiosyncratic demand shifts, demographic characteristics or market power variation rather than differences in quality or production efficiency.[5] For instance, a firm sheltered from competition because of some regulatory barriers may set high prices and according to a "revenue-based TFP" it may look more efficient than a firm in a more deregulated environment even if their efficiency levels are similar. Since we cannot implement the Foster *et al.* treatment, firm fixed effects are considered as controls for time-invariant characteristics that may determine firm-level prices.[6]

We now turn to the endogeneity issue. Estimation of Equation (I.1) by OLS can lead to biased estimates as inputs in the production function are likely to be related to the residuals. Let us decompose the residuals as follows:

$$\epsilon_{isct} = \omega_{isct} + u_{isct} \tag{I.2}$$

[5] Note that an important advantage of using a revenue-based TFP measures is that if we observe positive effects of competition-related measures on TFP growth, the result is not subject to concerns about markups being conflated, since markups would reduce TFP growth, thus implicitly the efficiency effects must be dominating.

[6] Estimates of the main analytical results using firm-level fixed effects are shown in the Annex, Table I.11. These estimates use the balanced panel dataset, where there are sufficient repeated observations to carry them out, and show that the baseline results are robust to firm fixed effects. This estimate also addresses concerns about the use of a Cobb-Douglas production function, if the underlying production function departs from constant returns to scale. In addition, (insignificant) firm size dummies were used in alternative specifications, and these did not affect the results.

Equation (I.2) decomposes firm efficiency into a part that is predictable by the firm ω_{isct}, though not observable in the data, and a part due to a productivity shock that can be forecast neither by the firm nor by the econometrician.

Firms choose their input on the basis of their knowledge of their environment and own efficiency ω_{isct}. Hence, if firms that anticipate high efficiency level hire more workers and invest more, OLS estimates will be biased upward. The endogeneity of input choices is well known in the literature. Consistent productivity estimates are obtained using the semi-parametric estimation techniques of Olley and Pakes (1996) or Levinsohn and Petrin (2003). These methods correct for simultaneity biases. To carry out such estimations, we need data on investment for the former and intermediate inputs for the latter in order to proxy firm's private knowledge of its efficiency.

Our preferred TFP estimates are those from the Levinsohn and Petrin (LP) method, which uses information on materials to correct for simultaneity biases. We do not use the Olley and Pakes technique, as their method requires primary information on investment to proxy for unobserved productivity shocks, while prior information on investment is not provided in Amadeus. Although we could create an investment measure using the perpetual inventory equation, we do not follow this path because of a high probability of measurement errors in capital depreciation.

Hence, we compute firm-level TFP by using intermediate inputs m to capture variation in firms' prediction of their efficiency ω:

$$\omega_{isct} = f(m_{isct}, k_{isct})$$

Introducing this function into Equation equation:prodfn, we now have:

$$\ln Y_{isct} = \alpha_{sc} \ln l_{isct} + \beta_{sc} \ln k_{isct} + f(m_{isct}, k_{isct}) + u_{isct} \tag{I.3}$$

The variation in inputs is now not related with the error term u_{isct} so that we have consistent estimates of the parameters. We compute each firm's TFP as the residual from an estimate of Equation (I.3). At this stage, firms' TFP values are not yet comparable across sectors and countries.

Following Pavcnik (2002) and Fernandez (2007), we construct a TFP index to deal with the comparability issue. The TFP index is based on the LP estimates and is constructed in two steps. First, for each 4-digit sector s and country c, we construct a reference hypothetical plant that has mean output and input levels calculated over the whole period. We compute the TFP of this reference plant as:

$$\widehat{A}_{sc}^{ref} = \overline{Y}_{sc} - \widehat{\alpha}_{sc}\overline{L}_{sc} - \widehat{\beta}_{sc}\overline{K}_{sc} \tag{I.4}$$

where $\widehat{\alpha}_{sc}$ and $\widehat{\beta}_{sc}$ are the estimates obtained from the regression estimate of Equation (I.3).

Second, we obtain plant i's *productivity index* at time t by subtracting the reference plant productivity A^{ref} from plant i's productivity as estimated in Equation (I.4):

$$A_{isct} = Y_{isct} - \widehat{\alpha}_{sc}L_{isct} - \widehat{\beta}_{sc}K_{isct} - \widehat{A}_{sc}^{ref} \tag{I.5}$$

This index number methodology follows Aw *et al.* (2001) and Caves and Tretheway (1980). The relative TFP measure obtained ensures comparability across industries and countries.

We then compute firms' TFP growth rates as the log difference: $\Delta A_{isct} = lnA_{isct} - lnA_{isct-1}$. Summary statistics for firm's TFP growth are shown in Table I.1. It displays the standard variation, the mean, median, the 10th and 90th percentiles of firm's TFP growth for each country. It shows that there is a wide variation in ΔA_{isct} both within and across countries.

2.4 Trade openness

To capture the pro-competitive impact of trade we construct a proxy for foreign competition which is import penetration. Trade data come from the Comtrade database. By combining it with detailed production data from OECD Structural

Table I.1: Summary statistics – Firm TFP growth

Country	Standard deviation	10th percentile	mean	median	90th percentile
All	2.87	-1.24	-.01	.01	1.28
BEL	4.09	-1.76	0	0	1.79
CZE	1.78	-.84	.09	.01	1.03
DEU	10.83	-1.7	.41	0	2.5
DNK	6.72	-.72	.14	.01	1.17
ESP	2.01	-1.01	.01	0	1.04
FIN	2.1	-1.3	.04	.01	1.46
FRA	1.2	-.63	.06	.03	.76
GBR	4.17	-1.64	-.05	-.02	1.53
ITA	2.3	-1.55	.01	.01	1.56
NLD	3.51	-1.83	.14	0	2.54
NOR	2.01	-1.12	.06	.04	1.35
POL	4.32	-1.75	.47	.05	3.13
PRT	2.15	-1.02	.07	.01	1.36
SWE	6.43	-4.16	-.45	-.03	3.4

Source: Authors' calculations based on Amadeus database. Not resampled dataset.

Demographic Business Statistics (SDBS) database, we compute different openness measures at the 4-digit sectoral level. Import penetration is constructed in the following way for each sector, country and year:

$$IP_{sct} = \frac{M_{sct}}{Q_{sct} + M_{sct} - X_{sct}}$$

where M_{sct} is total imports of good s to country c in year t. Q_{sct} is the production of good s while X_{sct} is the exports of good s from country c to its trade partners in year t.

Summary statistics for the import penetration measure across countries are shown in Table I.2. This table displays the median, the 25th and 75th percentiles of import penetration. There is considerable variation in import penetration across country and time, and these differences persist even within narrowly defined sectors.

2.5 Regulation and market structure measures

The primary measure of regulation is the OECD product market regulation indicators of *de jure* anti-competitive regulations, focusing on the vintages which

Table I.2: Summary statistics – Import penetration

	1996			2005		
Country	25th percentile	median	75th percentile	25th percentile	median	75th percentile
All	.17	.43	.75	.23	.55	.87
BEL	.36	.7	1.26	.42	.88	1.57
CZE	.05	.35	.61	.25	.61	1.02
DEU	.02	.2	.64	.2	.41	.87
DNK	.32	.59	.86	.4	.76	1.22
ESP	.13	.29	.55	.17	.46	.68
FIN	.16	.47	.67	.18	.49	.82
FRA	.17	.37	.54	.23	.48	.7
GBR	.17	.4	.61	.24	.54	.78
GRC	.06	.26	.63	.3	.58	.82
ITA	.12	.22	.37	.14	.31	.5
NLD	.42	.96	1.41	.4	.84	1.73
NOR	.36	.62	.82	.3	.62	.91
POL	.02	.25	.44	.16	.55	.75
PRT	.15	.41	.72	.23	.49	.76
SWE	.21	.51	.84	.27	.55	.93

Source: Authors' calculations based on Comtrade and OECD SDBS databases.

coincide with the coverage of the Amadeus data. These include the 1998 and 2003 data updates, the settings for which are assumed to be unchanged for the immediately following years, preceeding the most recent 2008 data update. These indicators include both domestic as well as international barriers; only the domestic barriers are used here, specifically the grouping 'barriers to entrepreneurship', which covers sub-indicators for administrative burdens on startups, regulatory and administrative opacity and sectoral barriers to competition. Each of the low-level indicators are based on a scoring of regulatory data on a 0 to 6 scale reflecting the extent to which the regulations inhibit competition (see Wölfl *et al.*, 2009).

A Herfindahl index of firm concentration at the four-digit level using the Amadeus firm database is used to control for the extent of *de facto* competition from domestic firms. It is calculated in the standard way, based on the sum of the square revenue market shares of each firm in an industry, so that it ranges between $1/n$ and 1 where n is the number of firms. The OECD 'Regimpact' measure, which assesses the industry-specific knock-on effects of anti-competitive regulation in seven network sectors is also used in robustness checks to control for the extent of upstream regulation.[7]

Table I.3 displays some summary statistics for the main measures of domestic competition. Though there has been convergence in these measures over time, a wide variation is still observed across countries.

Table I.3: Summary statistics – Market structure and domestic regulation

		'Barriers to entrepreneurship' Index			
Country	Standard deviation	10th percentile	mean	median	90th percentile
All	.6	1.45	2.23	2.39	3.05
BEL	.22	1.88	2.16	2.33	2.33
CZE	.08	2.09	2.13	2.09	2.27
DEU	.24	1.83	2.05	1.83	2.31
DNK	.17	1.42	1.52	1.42	1.82
ESP	.35	1.63	2.17	2.39	2.39
FIN	.49	1.42	2.01	2.41	2.41
FRA	.62	1.79	2.55	3.05	3.05
GBR	.23	.95	1.29	1.45	1.45
ITA	.54	1.58	2.38	2.74	2.74
NLD	.13	1.78	1.93	2.05	2.05
NOR	.21	1.33	1.45	1.33	1.83
POL	.28	3.15	3.42	3.15	3.72
PRT	.25	1.57	2.02	2.16	2.16
SWE	.48	1.15	1.69	2.11	2.11
		Herfindahl Index			
Country	Standard deviation	10th percentile	mean	median	90th percentile
All	.08	0	.05	.02	.12
BEL	.12	.01	.09	.04	.23
CZE	.11	.01	.09	.06	.22
DEU	.2	.04	.22	.16	.45
DNK	.11	.02	.11	.08	.21
ESP	.07	0	.03	.01	.07
FIN	.13	.02	.11	.06	.25
FRA	.07	0	.04	.02	.1
GBR	.09	.01	.08	.04	.18
ITA	.06	0	.03	.01	.08
NLD	.21	.05	.23	.15	.53
NOR	.09	0	.05	.03	.09
POL	.13	.02	.1	.05	.25
PRT	.21	.06	.22	.15	.51
SWE	.09	.01	.07	.03	.17

Source: 'Barriers to entrepreneurship' is sourced from the OECD Regulatory database. The Herfindahl Index is based on author's calculations using the Amadeus database.

[7] These indicators are calculated using a bottom-up approach in which regulatory data are quantified and aggregated to into summary indicators by sector using weights from I/O tables.

3 Empirical analysis of firm-level productivity

3.1 The effect of competition

Competition may stem from both foreign as well as domestic sources, which we take into account by differentiating the two. Our methodology assumes that increased import shares are equivalent to an increase in competition within a narrowly defined industry and that this increase is exogenous to the productivity growth of an individual firm. Several studies document that increased imports amount to tougher competition: for instance, Katics and Petersen (1994) find that it is associated with reduced price-cost margins using industry-level data for the United States. Recent empirical studies, including Aghion *et al.* (2009), Bas and Strauss-Kahn (2011), Fernandez (2007) and Pavcnik (2002), use import shares as measures of competition from trade, while Kletzer (2002) discusses assumptions necessary for this approach to be valid. Using a more structural approach, Chen *et al.* (2009) find that import penetration has a boosting effect on industry average productivity, supporting the pro-competitive effect of trade predicted by the theoretical model of Melitz and Ottaviano (2008).

To capture domestic competition, different measures have been proposed in the literature, such as price-cost margins and concentration indexes. Both measures have substantial flaws. First, they do not allow the effect of foreign competition to be distinguished from the effect of domestic competition. Secondly, while both sources of competition are supposed to put a downward pressure on price-cost margins, it is not clear that higher concentration indexes indicate lower competitive forces. Indeed, pressures from abroad may lead to exit of domestic firms, resulting in a small number of national firms operating, and a more concentrated domestic sector. While we control for concentration, we believe that the two sub-indexes of product market regulation that we use, namely barriers to entrepreneurship and burdens on startups, capture more accurately domestic competitive pressures, as they are direct measures of barriers to market entry.

Aghion *et al.* (2009) exploit several policy reforms that influenced the competitive environment in Europe, namely the European Single Market Program and industry specific reforms imposed by the Monopolies and Mergers Commission. They claim that those experiments enable them to identify the causal impact of competition on innovation. The perspective of this paper is similar; it makes the most of a country-specific product market regulation (PMR) index that captures various product market reforms that took place in OECD countries between 1998 and 2008. The product market regulation index captures various policies with different treatment intensity across countries and time.

Our empirical analysis highlights that the effect of foreign competition varies with the local stringency of product market regulation. Theoretical predictions on the interaction between trade and product market regulation are ambiguous though. On one hand, PMR and openness can go in the same direction and have a positive additive effect by demanding further productivity improvements. While foreign exposure reduces rents and demand stronger competitiveness to survive, this pro-competitive effect can be higher in countries with stringent regulation protecting incumbents as it creates new incentives to upgrade the production technology. On the other hand, rigidities can impede reallocation, innovation and firm adjustments, reducing the ability to react quickly to new competitive pressures.

3.2 Empirical specification: difference-in-differences

We relate firm-level TFP growth to domestic and foreign competition as well as domestic regulation in the following way:

$$\Delta A_{isct} = \beta_0 + \beta_1 IP_{sct} + \beta_2 IP_{sct} \times PMR_{ct} + \beta_3 X_{isct} + \gamma_s + D_{ct} + \epsilon_{isct} \qquad \text{(I.6)}$$

where ΔA_{isct} is the productivity growth of firm i that belongs to sector s and country c, IP_{sct} is the level of import penetration in sector s for country c in year t, PMR_{ct} is the level of product market regulation in country c and year t. One issue is that productivity growth can vary across firms because of sectoral features that have nothing to do with competitive pressures. To avoid any spurious correlation due to industry characteristics, sector fixed effects γ_s are included. They capture time-invariant characteristics that, for example, shape the potential for technological upgrading. It is also very likely that TFP growth is influenced by other institutional determinants or policies that do not affect competition. Country-time fixed effects D_{ct} are added to deal with this type of correlation. The country-time fixed effects also address country macroeconomic shock common to all sectors. X_{isct} is a set of control variables that vary across firms and time such as the size of the firm or across sectors s, country c and time t such as the level of concentration or the impact of regulation in services sectors on the manufacturing sector under study.

Equation (I.6) enables us to understand first how firm-level TFP growth depends on foreign competition (β_1), and second, how the effect of foreign competition varies with the regulation of the product market (β_2). Since we control for industry and country-time fixed effects, this specification identifies the effect of foreign competition through differential evolution of the import penetration across industries (industry-time variation).

Models of endogenous growth, considering the existence of technological flows between firms across all countries, dwell on the role played by the pool of highly innovative firms in driving productivity growth of incumbent firms. Productivity growth of followers depends on the productivity growth of the global technological frontier. Adding productivity growth of the frontier firms (top 1 percent in levels), we estimate:

$$\Delta A_{isct} = \beta_0 + \alpha \Delta A_{st}^{front} + \beta_1 IP_{sct} + \beta_2 IP_{sct} \times PMR_{ct} + \beta_3 X_{isct} + \gamma_s + D_{ct} + \epsilon_{isct} \quad \text{(I.7)}$$

where ΔA_{st}^{front} is the frontier's productivity growth. We compute the productivity level of the industry-year specific frontier A_{st}^{front} by taking the average productivity level of the top 1 percent of firms across all countries: it is thus a global frontier which is consistent with our cross-country empirical strategy.[8]

3.3 The importance of the firm's distance to the frontier

We allow for a non-monotonic effect of competition according to the heterogeneity of firms. The position on the firm in the productivity distribution is determined specific to its industry, with the right tail of the distribution representing the technological or productivity frontier. Is the positive escape-competition effect conditional on the distance of the firm to its industry frontier? The rationale behind this question is the following: the closer firms are to the frontier, the stronger the escape-competition effect on TFP growth tends to be. In other words, the pro-competitive effect of trade displays a boosting effect for firms with relatively high level of productivity. On the other hand, for laggard firms, an increase of competition due to the entry of foreign products on their market has a depressing effect because they are too far from the frontier to cope with it.

To capture the size of the technology gap among firms in an open-economy setting, we compare each firm's productivity to the median productivity of the the same sector and year. We then divide firms into two groups: a group of firms that are above the median level of TFP – those closer to the global TFP frontier – and a group of firms that have a TFP level below the median of their industry – who have a larger technological gap. To evaluate the differential impact of foreign competition and product market regulation according to firm heterogeneity in technology gap, we estimate Equations (I.6) and (I.7) separately for the two sub-samples.

3.4 The issue of reverse causality

Foreign competition is proxied by import penetration. It is possible that a bias exists because of reverse causality between firm productivity and trade orientation of the firm's sector. Foreign firms are able to enter more heavily a market if domestic firms

[8] As a robustness check, we also compute the productivity frontier using the average of the top 5% of firms.

Table I.4: Impact of import penetration and PMR on firms' TFP growth
Not resampled data set

PMR variable	**Barriers to entrepreneurship**					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP	0.173**	0.542**	0.019	0.175**	0.445**	0.011
	(0.084)	(0.215)	(0.031)	(0.076)	(0.201)	(0.028)
IP×PMR	-0.094**	-0.292**	-0.008	-0.128***	-0.303***	0.032
	(0.043)	(0.114)	(0.016)	(0.046)	(0.102)	(0.029)
Herf				0.021	0.132	0.317***
				(0.221)	(0.377)	(0.098)
IP×Herf				0.134**	0.275*	-0.130**
				(0.066)	(0.165)	(0.060)
Constant	0.373	1.004	-0.381***	0.384	0.988	-0.454***
	(0.758)	(1.012)	(0.060)	(0.752)	(0.996)	(0.068)
Observations	455,491	234,361	221,130	455,491	234,361	221,130
R-squared	0.024	0.033	0.033	0.024	0.033	0.033
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

are not efficient, leaving the competitive advantage to trade partners. This implies a negative correlation between productivity and import shares. However, this relation should be weak in our specification as we regress firm level productivity on sectoral import shares. We also consider that the reverse causality issue is less acute when we look at TFP growth compared to productivity levels. Finally, this could bias us away from finding a productivity-enhancing effect of import competition. In spite of this, our results indicate a positive relationship between productivity growth of the top firms and import penetration, which strengthens our confidence in the findings.

3.5 Interpretation of results

The first set of results of the estimation of Equation equation:estbase are shown in Tables I.4 and I.5, while Tables I.6, I.7 and I.8 provide robustness checks of the same equation. These results are based on the regression of firm-level productivity growth on import penetration (IP) and the interaction between import penetration and

Table I.5: Lagged impact of import penetration and PMR on firms' TFP growth
Not resampled data set

PMR variable	**Barriers to entrepreneurship**					
	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	Close to the frontier	Far from the frontier	All firms	Close to the frontier	Far from the frontier
IP_{t-1}	0.166*	0.518**	-0.039	0.135	0.419*	-0.012
	(0.100)	(0.222)	(0.071)	(0.096)	(0.249)	(0.071)
$IP \times PMR_{t-1}$	-0.088*	-0.281**	0.025	-0.108**	-0.268**	0.036
	(0.051)	(0.118)	(0.034)	(0.052)	(0.120)	(0.037)
$Herf_{t-1}$				-0.046	0.042	0.314***
				(0.230)	(0.408)	(0.088)
$IP \times Herf_{t-1}$				0.142**	0.168	-0.089
				(0.057)	(0.142)	(0.068)
Constant	0.380	0.908	-0.428***	0.408	0.907	-0.489***
	(0.731)	(0.942)	(0.088)	(0.731)	(0.940)	(0.093)
Observations	454,375	233,529	220,846	454,375	233,529	220,846
R-squared	0.022	0.030	0.033	0.022	0.030	0.033
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

domestic regulation ($IP \times PMR$). Import penetration at the sectoral level (IP) is used to proxy foreign competition pressures, while the 'barriers to entrepreneurship' index is used to measure the stringency of domestic regulation (PMR). The same equations are also estimated with the control variables. The first set of results, Tables I.4 through I.7, use the 'barriers to entrepreneurship' index (PMR) contemporaneously and with lags, both with the default dataset (Tables I.4 and I.5) and the resampled dataset (Tables I.6 and I.7).

Overall, the results, which split the sample by distance to frontier, are highly consistent with our hypotheses, and are robust across specifications, including those that account for potential reverse causality (using lagged values of IP) and potential sampling bias (on the resampled dataset).

Changes in firm productivity are impacted by both the domestic institutional environment and the extent of openness to foreign markets. However, firms' responses to foreign competition are heterogeneous, even within narrowly defined

Table I.6: Impact of import penetration and PMR on firms' TFP growth
Resampled data set

PMR variable	**Barriers to entrepreneurship**					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP	-0.020	0.427**	-0.023	-0.005	0.409*	-0.002
	(0.204)	(0.213)	(0.075)	(0.207)	(0.213)	(0.080)
IP×PMR	0.007	-0.232**	0.008	0.002	-0.234**	0.009
	(0.105)	(0.114)	(0.037)	(0.105)	(0.115)	(0.039)
Herf				0.371	0.303	0.117
				(0.252)	(0.304)	(0.097)
IP×Herf				-0.010	0.036	-0.049
				(0.043)	(0.079)	(0.046)
Constant	-0.179*	-5.055***	0.073**	-0.295**	-5.174***	0.042
	(0.100)	(0.960)	(0.029)	(0.150)	(0.991)	(0.039)
Observations	348,007	162,479	164,429	348,007	162,479	164,429
R-squared	0.037	0.043	0.025	0.037	0.043	0.025
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

Table I.7: Lagged impact of import penetration and PMR on firms' TFP growth
Resampled data set

PMR variable	**Barriers to entrepreneurship**					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP_{t-1}	0.422**	0.718**	0.273*	0.400*	0.645**	0.339**
	(0.209)	(0.282)	(0.160)	(0.228)	(0.305)	(0.159)
$IP \times PMR_{t-1}$	-0.193*	-0.357***	-0.128*	-0.193*	-0.348**	-0.127*
	(0.099)	(0.134)	(0.076)	(0.099)	(0.135)	(0.076)
$Herf_{t-1}$				-0.024	-0.249	0.067
				(0.288)	(0.351)	(0.110)
$IP \times Herf_{t-1}$				0.051	0.302	-0.160
				(0.208)	(0.304)	(0.105)
Constant	-0.250***	-5.269***	-0.089***	-0.241**	-5.189***	-0.117***
	(0.062)	(0.832)	(0.018)	(0.115)	(0.854)	(0.035)
Observations	338,137	158,549	159,645	338,137	158,549	159,645
R-squared	0.039	0.043	0.028	0.039	0.043	0.028
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

Table I.8: Impact of import penetration and PMR on firms' TFP growth
Not resampled data set

PMR variable	Burdens on startups					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP	0.075	0.189***	-0.039*	0.181**	0.122	0.071
	(0.050)	(0.073)	(0.023)	(0.072)	(0.113)	(0.070)
IP×PMR	-0.055*	-0.117***	0.032*	-0.079***	-0.113**	0.012
	(0.029)	(0.044)	(0.019)	(0.029)	(0.044)	(0.025)
Herf				-0.121	-0.035	0.422***
				(0.260)	(0.277)	(0.101)
IP×Herf				-0.141	0.130	-0.145*
				(0.086)	(0.159)	(0.078)
Constant	-0.042	-0.614	-0.481***	-0.017	-0.597	-0.561***
	(0.309)	(0.470)	(0.109)	(0.323)	(0.475)	(0.104)
Observations	417,389	237,355	160,651	417,389	237,355	160,651
R-squared	0.025	0.046	0.035	0.025	0.046	0.035
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

sectors. The evolution of firm TFP growth depends remarkably on its position in the distribution of firm efficiency. Firms that are technologically advanced benefit from competitive pressure of foreign firms' entry into their domestic markets. This "escape competition effect" is only present for the most competitive firms, with foreign competition generally having no significant impact on firms that are at the bottom of the efficiency distribution.

The positive pro-competitive effect of trade on advanced firms has a different magnitude according to the extent of product market regulation in the country. The negative coefficient on the interaction term indicates that trade becomes more beneficial as market regulation becomes less stringent. The 'barriers to entrepreneurship' PMR index is used in the estimates shown in Tables I.4 through I.7, which reflects anti-competitive measures such as entry barriers and administrative burdens that inhibit competition across sectors.

To more clearly delineate the effects of the measures, the 'burdens on startups' sub-indicator is used in Table I.8. This indicator focuses more clearly on administrative burdens for new firms, including sector-specific burdens. Using this index, these results yield coefficient estimates that are qualitatively very similar to the estimates with the broader PMR 'barriers to entrepreneurship' index shown in the previous tables. The other two PMR sub-indicators of this index, 'regulatory and administrative opacity' and 'barriers to competition', show less significance.

Domestic competition may also vary within a country, across sectors. This may have an effect on firms' incentives to upgrade their technology. The level of competition within a sector can be proxied by the concentration level within a sector.[9] In concentrated sectors, firms are not forced to reduce prices and can make positive profits more easily. Hence low productivity firms can survive. Our analysis suggests that the concentration level has a different impact on more advanced versus laggard firms, based on the raw dataset (Tables I.4 and I.5). While high concentration seems to allow less efficient firms to perform well, it is not a condition for high productivity firms whose TFP growth rates are not significantly affected by the concentration level. Such a concentration index is however an imperfect measure of competition as it does not capture the existence of entry threats. Moreover it focuses on a geographically limited definition of competition while European manufacturing sectors are open and some firms operate in international markets. Our favored measure of competition is the product market regulation index, as it can proxy unobservable entry threats as well as the existing regulatory scope that can be used to adjust to changes in market structure.

These results are robust to a number of alternative specifications, such as inclusion of the growth of the productivity frontier (Table I.9, using Equation (I.7)) or the restriction of the sample to only surviving firms (Table I.10). While these changes in specification have a slight impact on the results, they remain the same

[9] We also use the *Regimpact* regulatory impact index to help control for pressures that may affect costs. Regimpact can control for the cost structure of intermediate inputs coming from upstream sectors. Robustness checks were run with all of the estimated equations, and the inclusion of *Regimpact* in the equations does not affect the interpretation of the estimates. Firms that are closer to the frontier are found to cope more easily with high regulation in upstream services sectors, and it has a damping effect on firms farther from the frontier.

Table I.9: The impact of IP and PMR on firms' TFP growth, with frontier TFP growth

Not resampled data set

PMR variable	Barriers to entrepreneurship					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
ΔA^{front}	0.003***	0.005**	0.001*	0.003***	0.005**	0.001*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
IP	0.172**	0.494**	-0.040	0.162*	0.382*	0.002
	(0.087)	(0.197)	(0.063)	(0.086)	(0.202)	(0.055)
IP×PMR	-0.092**	-0.265**	0.020	-0.102**	-0.252**	0.046
	(0.045)	(0.105)	(0.032)	(0.050)	(0.104)	(0.035)
Herf				0.160	0.239	0.346***
				(0.191)	(0.374)	(0.097)
IP×Herf				0.058	0.204	-0.164***
				(0.055)	(0.143)	(0.061)
Constant	-0.612***	-0.770**	-0.425***	-0.637***	-0.817**	-0.503***
	(0.191)	(0.350)	(0.092)	(0.206)	(0.393)	(0.102)
Observations	414,890	211,820	203,070	414,890	211,820	203,070
R-squared	0.032	0.042	0.031	0.032	0.043	0.031
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

in sign, significance and roughly the same in magnitude, in these contemporaneous results. We have also checked for the inclusion of other controls at the firm level, such as an indicator of exit during the period, the size of the firm, which has no discernible effect on the main results.

Inclusion of the direct effect of product market regulation has a somewhat larger effect on the results, which was expected as we include country fixed effect and year fixed effect separately to estimate the impact of country-wide PMR. Yet the results on our variables of principal interest, import penetration and its interaction with PMR remain qualitatively similar.

Table I.10: The impact of IP and PMR on surviving firms' TFP growth
Not resampled, balanced data set

PMR variable	**Barriers to entrepreneurship**					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP	0.212**	0.602***	-0.024	0.199*	0.571***	0.032
	(0.106)	(0.218)	(0.075)	(0.101)	(0.216)	(0.069)
IP×PMR	-0.114**	-0.323***	0.014	-0.119**	-0.319***	0.036
	(0.056)	(0.116)	(0.038)	(0.057)	(0.115)	(0.041)
Herf				-0.021	0.019	0.384***
				(0.268)	(0.441)	(0.120)
IP×Herf				0.048	0.056	-0.187**
				(0.065)	(0.111)	(0.077)
Constant	-0.688*	-0.945	-0.429***	-0.677*	-0.944	-0.525***
	(0.380)	(0.649)	(0.043)	(0.382)	(0.657)	(0.050)
Observations	230,267	125,647	104,620	230,267	125,647	104,620
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

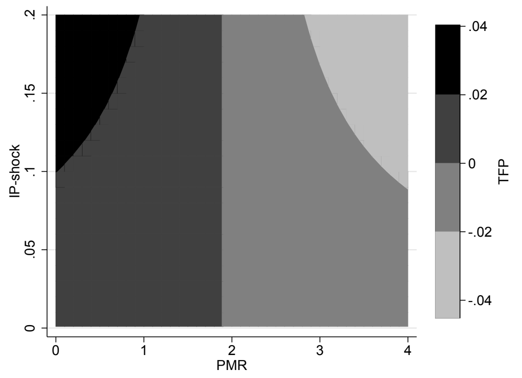
Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

3.6 Effects on productivity

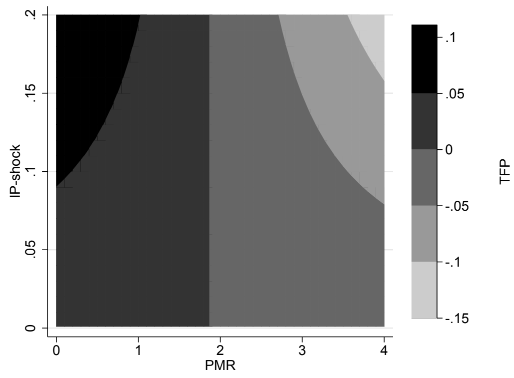
What is the economic significance of the results just discussed? Taking our preferred equation estimates from Table I.4, the effects of changes in import penetration and product market reform can be simulated for within-firm productivity growth, among the relatively large firms in our dataset. Given the difference-in-differences specification of the estimation equation, we therefore focus on conditional shocks. A qualitative visualization of these simulations is shown in Figure I.1.

Increases in import penetration (*IP*) only boost firm TFP growth if PMR is sufficiently low, below a certain threshold (Figure I.1, Panel A) equivalent to the median PMR setting at the end of the period studied. If PMR is higher than this threshold, an increase in IP (*i.e.* international competition) has a perverse impact on TFP, leading to negative TFP growth through discouragement. This effect arises from an even larger-magnitude effect on the firms in the upper half of the productivity distribution (Panel B). To take a particular example, for firms

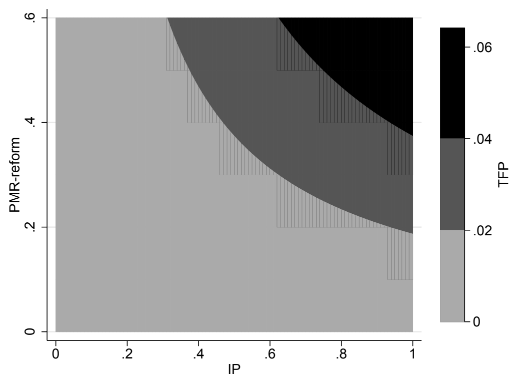
Figure I.1: Estimated within-firm TFP growth effects under conditional IP and PMR shocks



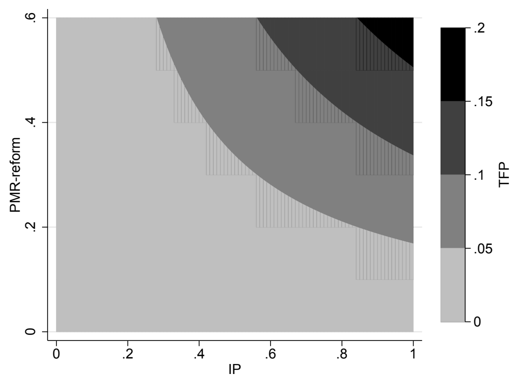
(a) Import penetration shocks: All firms



(b) IP shocks: Top firms



(c) Product market reform shocks: All firms



(d) PMR shocks: Top firms

Source: Simulations based on equation estimates from Table I.4, columns 2 and 3.

in the United Kingdom, the country with the lowest PMR, an increase in import penetration of 10 percentage points would raise firm TFP growth by approximately 1.0% per year on average, or 2.7% for the firms in the upper half of the productivity distribution. Yet for countries (primarily in earlier time periods) with higher PMR settings, the effect is essentially reversed.

A similar simulation can be carried out for a range of PMR reforms taking varying levels of import penetration as given (Figure I.1, Panels C and D). Product market regulatory reforms unambiguously boost productivity growth; however, their effects are magnified considerably when import penetration is higher. For instance, a PMR reform of 10% of the median setting would boost within-firm productivity growth by 0.5% in a sector at the 25th percentile of import penetration, and by 2.3% in a sector at the 75th percentile. Again, the impact is driven by firms in the upper half of the productivity distribution, where productivity growth is boosted by 1.4% and 6.3%, respectively. For firms in the lower half of the productivity distribution, the impact of PMR reform through this channel is negligible. Countries with a large share of high-productivity firms will thus benefit much more from PMR reforms.

4 Conclusion

This paper offers a new assessment of the effect of import penetration on firm-level productivity growth, taking into account heterogeneity in distance to the technological frontier and country differences in product market regulation. Our results show that firms in sectors with higher import penetration have higher TFP growth only if the firms are close to their sectoral technology frontier. Only the most productive firms enjoy an increase in productivity when foreign competitors' pressure is high. This result illustrates that in order to understand firms' TFP growth, it is important to combine explanations based on the pro-competitive effect of trade with a "Schumpeterian" distance-to-the-frontier mechanism, an area that theoretical trade models have overlooked to date.

The pro-competitive effect of international trade depends on domestic product market regulation as measured by the OECD's Product Market Regulation (PMR) index. Our results indicate that, at the top of the productivity distribution, the positive effect of foreign competition is inhibited for firms operating in a country with stringent regulation such as higher barriers to entry. Domestic and foreign competitive pressures are found to be complementarity: firms' incentives or abilities to improve their productivity to cope with foreign competition are stronger in countries with lower levels of PMR. As for firms at the bottom of the productivity distribution, foreign competition does not have a significant within-firm benefit on their efficiency – irrespective of the regulatory environment – though it may faciliate their demise, whereby they relinquish their market share to more productive firms.

Future work in this area could go beyond this paper in a number of respects. First, if firm-level trade information were available in a multi-country dataset, both the extensive and intensive margins could be examined, since their impact on competition likely differs. Second, instrumentation of import penetration would make the results for the measure more robust. Third, once a longer time series of domestic regulation indicators is available, further analysis would be worthwhile.

5 Annex

Table I.11: The impact of IP and PMR on surviving firms' TFP growth
With firm fixed effects – Not resampled, balanced data set

PMR variable	**Barriers to entrepreneurship**					
	(1) All firms	(2) Close to the frontier	(3) Far from the frontier	(4) All firms	(5) Close to the frontier	(6) Far from the frontier
IP	0.310**	0.567*	0.094	0.306**	0.513*	0.100*
	(0.131)	(0.312)	(0.063)	(0.130)	(0.311)	(0.059)
IP×PMR	-0.168**	-0.307*	-0.052	-0.190**	-0.312*	-0.021
	(0.069)	(0.166)	(0.033)	(0.080)	(0.168)	(0.047)
Herf				-0.070	0.165	0.050
				(0.443)	(0.835)	(0.112)
IP×Herf				0.096	0.149	-0.121*
				(0.086)	(0.153)	(0.070)
Constant	0.102***	0.353***	-0.139***	0.120***	0.358***	-0.162***
	(0.023)	(0.046)	(0.012)	(0.036)	(0.062)	(0.019)
Observations	230,708	125,978	104,730	230,708	125,978	104,730
R-squared	0.036	0.046	0.006	0.036	0.046	0.006
Number of IDs	34,071	25,210	22,054	34,071	25,210	22,054
Firm FE	YES	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses, clustered standard errors by country and sector.
*** p<0.01, ** p<0.05, * p<0.1

Chapter II

Labor market regulation and plant-level productivity in India[1]

1 Introduction

It is well known that India's formal Employment Protection Legislation (EPL) is among the most stringent in the world. Many believe that this is one of the main reasons behind the stagnant share of manufacturing output in India's GDP during the last 40 years (OECD, 2007). Although the country has recorded impressive output growth rates since the 1970s, the share of manufactures in total output has remained between 14% and 18%. Though infrastructure and product market regulation have been major challenges, strict labor laws have been blamed in particular for the poor performance of large-scale labor intensive manufactures despite India's labor abundance (Panagariya, 2003; Conway and Herd, 2009; Dougherty *et al.*, 2009). According to the MCI (2011), the top five goods exported during 2010-11 represented almost 50% of the country's total exports and they were all relatively capital intensive goods such as petroleum products, gems and jewelry, transport equipment, machinery and instruments, and pharmaceutical products. In contrast, ready-made garments, traditionally an unskilled-labor intensive export, has seen its share in total Indian exports decline from 12.5% to 6% between 2000

[1] This chapter is a revised version of NBER working paper No. 17693 and OECD Economics Department working paper No. 917 (2011), "Employment Protection Legislation and Plant-Level Productivity in India," jointly written with Verónica Frisancho Robles (IDB) and Kala Krishna (Penn State).

and 2010. In 2010, India was the fifth largest exporter of apparel, with 3.2% of the world's exports (WTO, 2011).

Industrial relations in India fall under the joint jurisdiction of central and state governments, an arrangement that has generated a degree of variation in labor regulations across states. Although all states had essentially the same starting point under the License Raj, each state has independently amended labor regulations, rules and practices during the post-Independence period. In the last decade, this "natural experiment" setting has been exploited by several empirical studies that have tried to assess the effects of labor regulation on output, employment, and productivity. However, and despite increasing interest in the topic, the evidence for India is still inconclusive and mostly limited to industry-level analysis.

One of the most influential studies of India is Besley and Burgess (2004), which constructs an index summarizing state-level amendments to the Industrial Disputes Act (IDA) between 1949 and 1992. The index, henceforth referred to as BB, is used along with several control variables to explain state-level outcomes corresponding to the organized manufacturing sector using industry-level panel data for 1958-92. The authors identify a negative impact of pro-worker regulation on output, investment, employment, and labor productivity among registered manufacturing firms. Several papers that also rely on the BB index reach similar conclusions.[2]

Nonetheless, the validity of the BB index and the econometric methodology used to identify the effect of excessive pro-worker regulation have been extensively criticized. The main concerns with the use of this index are related to problems in the coding of labor laws and its exclusive focus on formal reforms to the IDA. This study tries to overcome the shortcomings of the previous empirical evidence in the tradition of Besley and Burgess to evaluate the effect of labor regulation on the Indian organized manufacturing sector. We make use of a more comprehensive measure of labor market regulations proposed in OECD (2007) and elaborated in Dougherty (2009). We argue that this index is superior to the BB index as it includes

[2] See Aghion *et al.* (2008) and Ahsan and Pagés (2009) as examples.

information on formal and informal labor market reforms, not only to the IDA but in seven additional areas: the Factories Act, the State Shops and Commercial Establishments Acts, the Contract labor Act, the role of inspectors, the maintenance of registers, the filing of returns and union representation.

Using this comprehensive EPL measure and plant-level data from the Annual Survey of Industries (ASI) for all the fiscal years between 1998-99 and 2007-08, we evaluate whether labor market regulation differences across Indian states led to a differential response in industrial performance.[3] However, differences across states in terms of labor regulation may be endogenous. A higher number of pro-employer reforms in a given state may be driven by the characteristics of the firms located in that state.

Following Rajan and Zingales (1998), we focus on the details of the theoretical mechanisms at play. As we will show below, unit labor costs increase with more stringent EPL, and more so for firms operating in industries with higher labor intensity. This implies that firms in industries with higher labor shares will suffer the most from the additional costs of hiring and firing workers. Thus, we implement a difference-in-difference estimator that exploits both the variation in EPL by state, as well as the variation in industry-specific characteristics related to labor intensity and volatility. In addition, to the extent that such costs act as adjustment costs, they will have more of an effect in more volatile industries so that the productivity of firms in more volatile sectors should be more affected by strict labor laws. By focusing on a specific mechanism through which EPL reform operates (labor intensity or volatility), this approach provides stronger evidence of causality.

Previous studies have also exploited the variation in state and industry characteristics[4] but their focus was at the industry level. To our knowledge, this is the first study of India to evaluate the effect of labor regulation on plant-level productivity

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[3] In this paper, EPL is used as a shorthand to refer to a customized measure of state-level labor regulation reforms in India as elaborated in Dougherty (2009). The official OECD measure is country-specific and has a longstanding standardized definition, as most recently elaborated in Venn (2009).

[4] See Gupta *et al.* (2008) and Bassanini *et al.* (2009).

using a longitudinal sample,[5] and is one of only a few studies on any country to examine labor regulation effects at the plant level.

The evidence presented here shows that firms in industries with higher labor intensity or higher sales volatility benefited the most from labor market reforms in their states. The positive effect of relaxed EPL on organized manufacturing firms in labor intensive industries is experienced through higher total factor productivity (TFP) although there is no consistent effect on labor productivity measured as value added per worker. Similarly, firms in more volatile industries that experience pro-employer labor reforms tend to have higher levels of TFP. We also identify a heterogeneous effect of EPL in labor intensive industries by plant size and ownership type. In particular, we find that smaller firms and private firms with a high usage of labor inputs tend to benefit the most from relaxation of state labor laws. In general, our results suggest that state-level reforms can help to mitigate the detrimental effects that strict federal labor laws have on industrial outcomes in the organized Indian manufacturing sector.

Our paper contributes to two strands of literature. First, its adds to the literature that focuses on the effect of labor and product regulation on industrial outcomes and economic performance, of which Besley and Burgess (2004) has been one of the most influential studies. It also contributes to some recent studies on the potential links between labor markets and comparative advantage that have received special attention in the trade literature. Within this literature, our study is particularly related to Cuñat and Melitz (2007) and Krishna and Levchenko (2009), who highlight the role of firm-level volatility in determining the pattern of comparative advantage.

The rest of the paper proceeds as follows. Section 2 sketches out the major findings in the literature. Section 3 describes the data as well as some basic stylized facts. The empirical strategy is described in Section 4 while Section 5 displays the results. Some robustness checks are presented in Section 6. Section 7 concludes and describes the limitations of the study, as well as directions for future research.

[5] Harrison *et al.* (2011) use a similar dataset also based on the Annual Survey of Industries (ASI) to examine market share reallocations; however they focus on trade policy reforms.

2 Previous Literature

Despite increasing interest in the effect of institutions and regulation in industrial performance, the theoretical and empirical evidence to support or negate the beneficial effect of EPL relaxation is still limited. Although labor market equilibrium models such as Garibaldi's (1998) and Mortensen and Pissarides's (1999) predict a negative effect of stricter EPL on job mobility, its effects on productivity are not that straightforward. There is even a branch of the literature which suggests that the net effects of EPL on productivity may be positive. Workers could be more willing to invest in human capital specific to the firm if their jobs are better protected. Firms may also be willing to invest more to increase labor productivity as an alternative to downsizing. Bassanini *et al.* (2009) provide an extensive discussion of these theoretical results, suggesting that there might be an "optimal" level of EPL.

Stricter labor regulation increases the costs of hiring and firing workers, making it more difficult for the firm to react to demand or supply shocks that require labor reallocation or staff reduction. The restriction of labor movement even in more productive firms or sectors can thus result in lower productivity levels. Poschke (2009) develops a model that takes into account firm dynamics and where firms receive idiosyncratic productivity shocks. He shows that selection eliminates the active firms with the lowest productivity, and entrants imitate more productive survivors. In this setting, strict EPL ends up reducing firm value, discouraging not only entry but also the exit of less productive firms. Product or technology innovation can also be discouraged if the firm has to face high labor costs and high layoff costs in case of failure Samaniego (2006). Moreover, growth losses tend to be larger when productivity is more volatile. This latter result is in line with previous findings of worse effects of strict EPL for firms operating in more turbulent sectors (see Bentolila and Bertola, 1990).[6]

[6] Under a general equilibrium framework, Hopenhayn and Rogerson (1993) show how the distortion induced by firing restrictions pushes firms to use resources less efficiently. EPL is likely to make it more difficult for firms to react quickly to rapid changes in technology or product demand that requires reallocation of staff or downsizing. As a result, employment levels adjust at a lower speed and productivity is reduced.

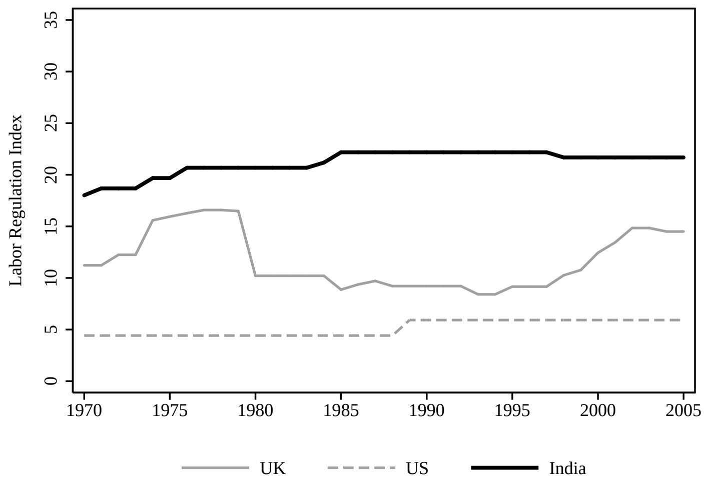
A paper by Cuñat and Melitz (2012, 2007) studies the link between volatility, labor market flexibility, and international trade. They develop a model and test it using country-industry data and find that countries with more flexible labor markets fare better in more volatile industries, where their ability to adjust to unexpected shocks is more important. This implies that labor market reforms might have differential effects across industries and that their effects might be more beneficial among sectors with a higher dispersion of within-industry shocks.

More broadly, the empirical literature is quite inconclusive and has tried to measure the effects of EPL on industrial outcomes using cross-country studies with industry-level data or industry-state-level data. Among the first group of papers, Micco and Pagés (2007) implement a difference-in-differences estimator in a cross-section of industry-level data for a sample of developed and developing countries. They are able to identify the effect of EPL by arguing that sector differences in the intrinsic volatility of demand and supply shocks can lead to differential responses to labor regulation. Their results show that EPL reduces turnover, employment, and value added in more volatile industries but they only find weak evidence of a negative relationship between labor regulation stringency and labor productivity. Similarly, Bassanini *et al.* (2009) use aggregate cross-country/time-series data on OECD countries to measure the differential effects of country-level EPL on industry-level productivity. They find that dismissal regulations tend to generate larger TFP growth loses among industries with a high layoff propensity relative to industries where firms rely less on layoffs to adjust labor-inputs' usage.

A recent strand in the empirical literature focuses on India, one of the countries with the strictest labor regulation in the world. Although Indian labor laws were strongly influenced by the British model inherited on independence, it is clear that Indian labor regulation is substantially more protective than the UK's present system, as shown in Figure II.1. The gap between these countries broadens after 1979, which is when a conservative government committed to labor market deregulation was elected in the UK. India fares even worse when compared to the US.

However, the Indian case is particularly interesting and a nice setting for empirical studies given the ability of state governments to introduce formal and informal amendments to the labor laws. Consequently, changes in the application of the law at the state-level have resulted in important variations in the stringency of EPL within the same country.

Figure II.1: Evolution of Labor Law in India, UK, and the US



Source: Deakin *et al.* (2007).
Notes: The laws reported for India are mostly federal laws. The authors also report some state-level variations in case law, especially for the most heavily industrialized states. Their Labor Regulation Index is a score obtained out of 40 possible points, where higher values indicate more stringent regulation.

First promoted by Besley and Burgess (2004), most studies focusing on India tend to use cross-state and intertemporal variation in labor legislation as measured by state IDA amendments. These studies find that changes towards more flexible labor regulation are correlated with higher levels of manufacturing output, employment, and labor productivity in the organized industrial sector. For example, Aghion *et al.* (2008) find that, following delicensing in the 1980s and early 1990s, industries located in states with pro-employer labor regulations grew more quickly than those in pro-worker environments. Ahsan and Pagés (2009) also use the BB index over a similar period, but decompose it into amendments that reduce transaction costs of initiating and sustaining industrial disputes and those that increase job security and reduce labor flexibility. Their results suggest that regulations that increase the cost of settling disputes are more costly for employment than the restrictions directly imposed by the IDA.

Focusing on rural India in the same time period, Adhvaryu *et al.* (2012) develop a partial equilibrium model where agriculture exists alongside industry. They use rainfall fluctuations to measure exogenous unobserved demand and cost shocks, and analyze the response of states with different labor regulations as measured by the BB index. Their results show that the change in employment is significantly greater in states with laxer labor laws. However, shocks do not generate a differential response in output or profits. This is explained by a greater adjustment of the use of capital and materials in pro-worker states.

Despite its extended use in the empirical literature, the BB index has been heavily criticized. Bhattacharjea (2006, 2009) claims that the Besley and Burgess (2004) scoring system can erroneously classify a state as pro-employer or pro-worker with just one or two amendments to the IDA in the 50 years covered by the index. Nagaraj (2004) points out that the BB index focuses only on the IDA, abstracting from several other labor laws that affect industrial performance. Another important critique is its exclusive focus on *formal* amendments, which ignores changes in the actual practices and enforcement of the labor laws. In fact, most recent changes in state-level practices have resulted from judicial interpretations of the laws by the Supreme Court. It is thus not surprising that updates of the BB index, including our own, using the most recent edition of Malik, show very few changes in labor regulation after 1992. In addition, Bhattacharjea (2009) emphasizes the fragility of Besley and Burgess's (2004) econometric results. In particular, Bhattacharjea criticizes the use of irrelevant state-level control variables and inadequate tests for robustness, as well as the fragility of their results once state-specific time trends are introduced in their model.

A recent study by Gupta *et al.* (2008) tries to overcome some of the BB index's measurement problems by using a simple majority rule across three EPL measures available in the empirical literature, including the BB index. They argue that this approach has the advantage of weeding out any measurement error, unless there are systematic mistakes in coding the states across different indicators. Using this state-level composite measure of EPL, they exploit industry-level variation in labor usage to test the differential impact of product and labor market regulations. They find that labor intensive industries in states with flexible labor regulation have higher levels of value added.

Bhattacharjea (2009) departs from Besley and Burgess's (2004) work by focusing on the legislative content of the state-level amendments as well as on the judicial interpretations to Chapter V of the IDA.[7] Although his proposed index is better in the sense that it includes information on practices at the ground level, he still focuses on only one labor law. His results on the effect of state-level labor regulation reform on the number of factories, value added, and share of contract labor are mixed but he highlights that his main contribution lies on his critique of the earlier literature.

All in all, the evidence on the effects of EPL on TFP and/or TFP growth in India is still scarce. This gap in the literature is even larger when we focus on the evidence available at the plant or firm level. Besides the well-known difficulties involved in TFP estimation at the plant level, the fact that state-level changes in labor regulation may be endogenously determined requires additional sources of variation in the data to identify the effect of EPL on plant-level productivity.

In particular, based on our reading of the literature, we expect labor regulation differences to have heterogenous effects on productivity across industries with different levels of labor intensity and volatility. We assume that there is a Cobb-Douglas production function specific to each manufacturing industry, $Y = AL^{\alpha}K^{1-\alpha}$, and thus the unit cost function (which is inversely related to A, multifactor productivity) will be given by:

$$c = \left(\frac{w}{\alpha}\right)^{\alpha} \left(\frac{r}{1-\alpha}\right)^{1-\alpha} \frac{(R_s)^{\alpha}}{A}$$

[7] This chapter relates to firms' requirements to obtain government permission for layoffs, retrenchments, and closures.

where w and r are the labor and capital input prices. Employment protection legislation is captured through the constant R_s, which multiplies wages in state s to capture the effective cost of labor, consistent with our view of employment protection in India as being roughly proportional to the number of workers in a firm. Whenever labor legislation imposes additional costs through layoff regulation or hiring restrictions, R_s will be above one.[8]

The percentage change in the unit cost with respect to R_s will be given by:

$$\frac{\partial \log c}{\partial R_s} = \frac{\alpha}{R_s} \tag{II.1}$$

which is positive and increasing in α. In other words, the percentage change in the unit cost is higher as EPL becomes stricter and more so for labor-intensive industries. Our study will then identify the effect of EPL by taking advantage of the state-level variation in labor regulation as well as the industry-level variation in labor intensity as measured by an estimate of α.

3 Data

The data used in this study come from the Indian Annual Survey of Industries (ASI), conducted by the Indian Ministry of Statistics (MOSPI). We use ASI data from the 1998-99 through 2007-08 fiscal years to obtain an unbalanced panel of registered manufacturing plants. Previous studies using the same data source have been unable to build a plant-level panel due to the lack of factory identifiers that have only been made available recently.[9] We differ from virtually all of them in that we make use of a subsample of plants that constitute a longitudinal panel.[10]

[8] While we see the cost of EPL in India to be primarily a variable cost, the unit cost function above implies that any increase in R_s will also directly reduce multifactor productivity.

[9] We thank India's Central Statistical Organization (CSO) for providing us the data we use for this study. The confidentiality of the unit level data was maintained and adequate precautions have been taken to avoid disclosing the identity of the units directly or indirectly.

[10] A notable exception is Harrison *et al.* (2011), which uses the ASI panel to examine the role of market-share reallocations in aggregate productivity growth in India's organized manufacturing sector between 1985 and 2004.

The ASI sampling frame includes all factories employing 10 or more workers using power, or 20 or more workers without using power. In general, the ASI's basic strategy over the years has been to divide the survey frame into census and sample sectors, where the census sector includes larger plants. Although this strategy has remained intact, the definition of census and sample sectors has undergone some changes over the years. Between the 1998-1999 and 2007-2008 rounds, the size threshold for the census sector fluctuated between 50 and 200 workers, so that only plants employing 200 or more workers are *always* surveyed during the years analyzed.[11] The remaining plants are randomly sampled. For more details about the sampling design changes as well as a detailed description of the data problems present in ASI see Bollard *et al.* (2013); Harrison *et al.* (2011) discuss the new longitudinal sample specifically.

ASI data provides factory reports on output, value added, fixed capital, investment, materials, fuel, labor, and labor expenditures. It also provides information on the type of ownership, the type of organization, as well as the start-up year of each plant. The ASI reports the book value of fixed capital both at the beginning and at the end of the fiscal year, net of depreciation. Our measure of fixed capital will be the average of the net book value of fixed capital at the beginning and at the end of the fiscal year, while all other variables are measured at the end. The data collected from the ASI are at current prices and must be corrected for price changes over time. Details on the specific deflators used for each variable can be found in the Annex to Dougherty *et al.* (2011).

The raw data consist of about 384,000 observations over 10 years, with an average of about 38,000 plants surveyed each year. We remove observations corresponding to non-operative plants (26,553) and plants with non-positive values of output and negative values of fixed capital stock (499). Table II.1 shows that following this, on average, 26% of the observations in each round have missing values for output, value added, materials, fuels, fixed capital, or labor. After removing these observations,

[11] All industrial units belonging to the five least industrially developed states (Manipur, Meghalaya, Nagaland, Tripura and Andaman & Nicobar Islands) were also included in the census sector.

Table II.1: Percentage of missing observations in each ASI round

Year	Total Obs.[a]	Missing Obs.[b]	% Missing
1998-1999	23,620	4,290	18.2
1999-2000	24,684	6,944	28.1
2000-2001	31,053	8,349	26.9
2001-2002	33,387	8,579	25.7
2002-2003	33,800	8,625	25.5
2003-2004	45,429	12,483	27.5
2004-2005	39,714	11,503	29.0
2005-2006	43,675	10,039	23.0
2006-2007	43,304	12,812	29.6
2007-2008	38,439	10,777	28.0
Total	357,105	94,401	26.4

[a/] After removal of non-operative plants and plants with non-positive values of output and fixed capital stock. Only 7% of all observations are dropped for these reasons.
[b/] Observations are coded as missing when the factory does not have data on output, value added, materials, fuels, fixed capital, labor, or labor expenditures.

we also drop three manufacturing industries (2-digit NIC) with too few observations: other mining and quarrying, recycling, and office, accounting, and communication equipment. Following Aghion *et al.* (2008) and Gupta *et al.* (2008), we also drop "other" manufacturing industries. This category groups different activities which are likely to vary across states, making it incomparable across states. Finally, we also drop the states and union territories of Jammu & Kashmir, Chandigarh, Nagaland, Manipur, Tripura, Meghalaya, Daman & Diu, Dadra & Nagar Haveli, Pondicherry, and Andaman & Nicobar Islands due to lack of information on employment legislation. We also exclude Lakshadweep due to lack of data in the ASI and Goa given its economy's dependence on tourism.

The final sample consists of 239,921 plant-year observations with data on 103,478 plants in 20 states. Almost 60% of the observations and 74% of the plants in our data come from the sample sector. Moreover, almost 50% of the plants appear in only one round of the survey. As expected, these are smaller plants, with an average of 48 workers. This is an important limitation of the ASI; since plants in the sample sector are not deliberately followed over time, entry and exit for smaller plants is missed. Due to changes in the census threshold size, exit and entry is only consistently observed for census plants with at least 200 workers. We call this sample the *restricted* census sample which contains 49,895 plant-year observations on 11,343 plants. Basic statistics on the final sample are presented in the Annex.

We rely on the restricted census sample to obtain TFP estimates but use information on all the plants surveyed to measure the effect of EPL on productivity. To take into account simultaneity and selection biases, we obtain production function estimates using the Olley-Pakes estimator. Since this approach uses information on plants' exits and lagged values of some variables, we only apply it to the restricted census sample. We then apply estimates of the production function's parameters to the full sample of plants and obtain TFP residuals for all plants in ASI's census and sample sectors.

An additional problem posed by ASI data is the substantial number of outliers. To reduce their influence in our estimates, we "winsorized" the data, following Bollard *et al.* (2013). This procedure basically implies top-coding and bottom-coding the 1% tails for each plant-level variable. In other words, for each year and each variable we replace outliers in the top 1% tail (bottom 1% tail) with the value of the 99$^{\text{th}}$ (1$^{\text{st}}$) percentile of that variable. This procedure was applied separately to each 2-digit industry.[12]

Our measure of labor reform comes from the OECD index which summarizes state-level indicators of procedural changes to the implementation of labor laws either through formal amendments or through *de facto* practices (Dougherty, 2009).[13] The OECD, with the support of the All-India Association of Employers (AIOE), surveyed 21 Indian states in 2007. The EPL index reflects the extent to which procedural or administrative changes have reduced transaction costs in relation to labor issues. It is constructed using data from a survey instrument

[12] We do not remove these outliers because we would have generated an additional loss of 59,896 observations, about 25% of the complete sample.

[13] Unfortunately, while it would have been desirable to separate the *de facto* from the *de jure* procedural changes, as Davies and Vadlamannati (2013) do in a different context, it is not possible to do so given the questionaire design.

developed to identify areas in which Indian states have experienced specific changes to the implementation and administration of labor laws over the 1990s and 2000s. The survey covered 50 specific subjects of possible reform in seven major areas of labor regulation in addition to the IDA: the Factories Act, the State Shops and Commercial Establishments Acts, the Contract Labor Act, the role of inspectors, the maintenance of registers, the filing of returns and union representation. We use the ordinal EPL count index, rebased and rescaled from zero to one, which is essentially the percentage of areas in which pro-employer labor reform occurred. It is worth emphasizing that, although the OECD index can be separated by its subcomponents, we rely on the aggregate measure of labor reform since the index was designed to capture a state's general stance towards labor regulations, more than the character of specific reforms.

To add state-level controls to our estimates, we gathered time series data on population, telephone availability, installed electric capacity, and paved road length. State population comes from census population data for 1991, 2001, and 2011, and it is linearly interpolated for other years. Time series data on fixed and mobile phones per 100 population comes from the Ministry of Statistics and Programme Implementation's (MOSPI) website. Installed electric capacity, measured as kilowatts per million people on the state, is obtained from the Annual Report of the Indian Ministry of Power for the years 1997-98, 2000-01, 2001-02, 2002-03, 2003-04, 2004-05, 2005-06, and 2007-08. State-wise surfaced road length is obtained from two sources: i) the Basic Road Statistics of India report from the Ministry of Road Transport and Highways for the years 2004-05, 2005-06, 2006-07, and 2007-08, and ii) the Planning Commission's 9th and 10th Five Year Plans. Road density is measured as paved kilometers per thousand people in the state.

We also include an OECD measure of state-level product market regulation as a time-invariant control to take into account the potential role of product regulation as a complement (or substitute) of labor market laws. The product market regulation index is taken from OECD (2007) and it contains information on state intervention and legal or administrative barriers to entrepreneurship (see Conway and Herd, 2009).

In our robustness checks, we will also make use of the BB index that we update through 2008 using Malik (2011) as well as Gupta *et al.*'s (2008) labor market regulation composite index. The latter is based on a simple majority rule across the EPL indicators proposed in Besley and Burgess (2004), Bhattacharjea (2006), and Dougherty (2009). States are coded as pro-labor, pro-business, or neutral if the majority of the studies considered classified them as such. Additionally, we check the robustness of our results using industry-level layoff propensity instead of the measure of labor intensity captured by the estimated αs. Layoff propensities are measured for the US between 2002 and 2003 with data from the 2004 CPS Displaced Workers Supplement (see Table A.3 in Bassanini *et al.* (2009)).[14] Using these propensities, we construct a dummy variable for above and below the median industry.

We must emphasize that the ASI only provides data on organized manufacturing plants. In a country where the informal sector constitutes a majority of the labor force and the unorganized sector produces a third of total manufacturing value added, there is also a need to understand how EPL reforms have affected unorganized plants. A source of data on these plants is the National Sample Survey Organization's (NSSO) survey, but it is only carried out every five years. This lack of data comparable to the ASI forces most researchers to focus exclusively on the registered, or organized sector. However, this focus is also appropriate since labor market rigidities in the organized sector constrain the absorption of formal workers, who tend to be more productive, receive higher wages, and face better working conditions than workers in the informal sector (see Gupta *et al.*, 2008). Moreover, Goldar and Aggarwal (2010) provide some evidence on the effects of labor market reforms in the unorganized manufacturing sector. Using the OECD labor market reform index for Indian states, they find a negative and significant relationship

[14] The industry classification in this data (ISIC Rev. 3) does not exactly match the 2-digit industry classification of the ASI, so in some cases we had to merge Indian industries to make them comparable to those in the United States.

between labor laws' flexibility and the probability of being a casual worker both in the formal and informal manufacturing sector, although the effect in the organized sector is far stronger.

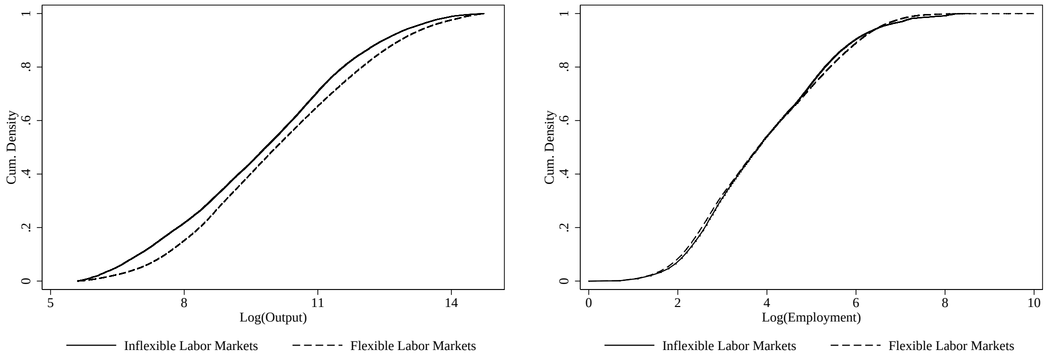
3.1 Basic Patterns

Using the OECD index, we classified states as having flexible labor markets when they were above the median state according to the degree of labor regulation reforms carried out. Figure II.2 plots the cumulative distribution of output and employment by labor laws' rigidity. Panel (a) suggests that the variation in labor standards across states may have allowed some states to fare better than others; the distribution of output in states with flexible labor laws first order dominates that of states with more stringent regulation. However, panel (b) of Figure II.2 suggests that EPL does not seem to influence formal employment. Although these patterns are suggestive, we need to control for the states' total population to get a better idea of the general picture.

Figure II.3 plots output and employment per capita at the state level in 2000 against our EPL reform indicator.[15] Each observation in the scatter plot represents a state. Even after controlling for the state's population, Panel (a) in Figure II.3 shows that there is a modest positive relationship between output per capita and the preponderance of labor law reforms in the state. However, this pattern is much weaker for formal employment per capita, as shown in panel (b).

[15] The OECD labor reform index has been re-scaled so that 0 corresponds to the lowest level of reform and 1 indicates the highest level of reform at the state level.

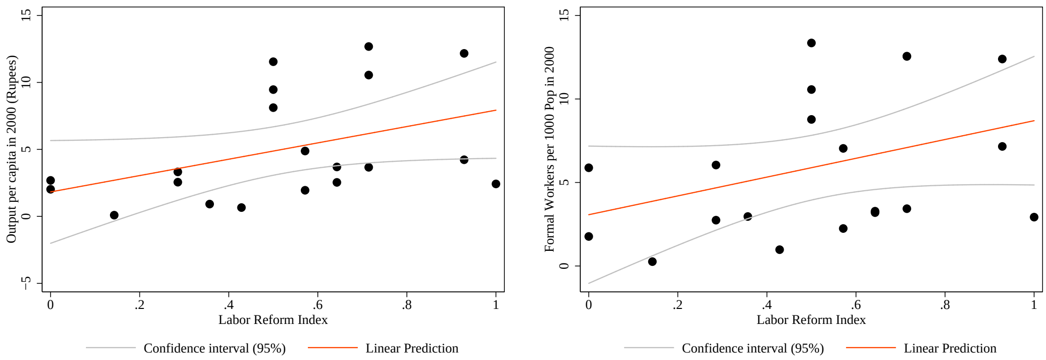
Figure II.2: Output, employment, and EPL in 2000



(a) Output (b) Employment

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08 rounds.

Figure II.3: Output and employment per capita and EPL in 2000

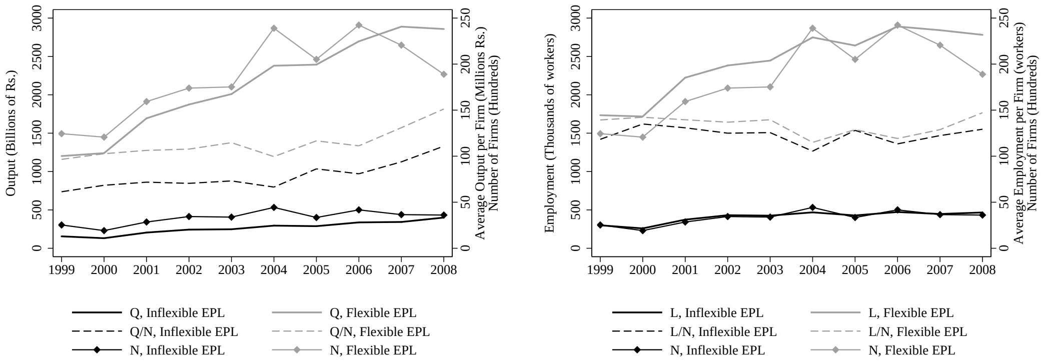


(a) Output (b) Employment

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08 rounds.

However, differences in the number of plants in each state may be driving these patterns. To deal with this, Figure II.4 decomposes total output and employment by EPL flexibility into their *extensive* and *intensive* margins. While the extensive margin is captured by the number of plants (N), the intensive margin is measured by the average output or average employment per plant (Q/N or L/N). Both in terms of output and employment, states with more flexible regulation fare better than plants operating in more restrictive labor markets. However, most of this "advantage" seems to be explained by the evolution of the extensive margin. On average, intensive margin differences explain about 36% of the output gap and 9% of the employment differences between flexible and inflexible states.[16]

Figure II.4: Labor market regulations and manufacturing production and employment



(a) Total output (b) Total employment

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.

Figure II.5 plots the distribution of TFP and labor productivity by EPL and labor intensity. We obtain TFP estimates separately for each industry (so that scaling is not an issue) using the Olley-Pakes approach in the subsample of ongoing plants in ASI's panel. Sub-section 4.1 below describes the details of the estimation of TFP residuals, which yields unbiased estimates of the production function coefficients. In particular, we rely on the output elasticity with respect to labor, α, estimated in the panel and identify labor intensive industries as those with an $\hat{\alpha}$ above the median industry. Following Besley and Burgess (2004), we also show

[16] Let the subscripts 0 and 1 correspond to outcomes in inflexible and flexible labor markets, respectively. Output differences can be decomposed in the following way:

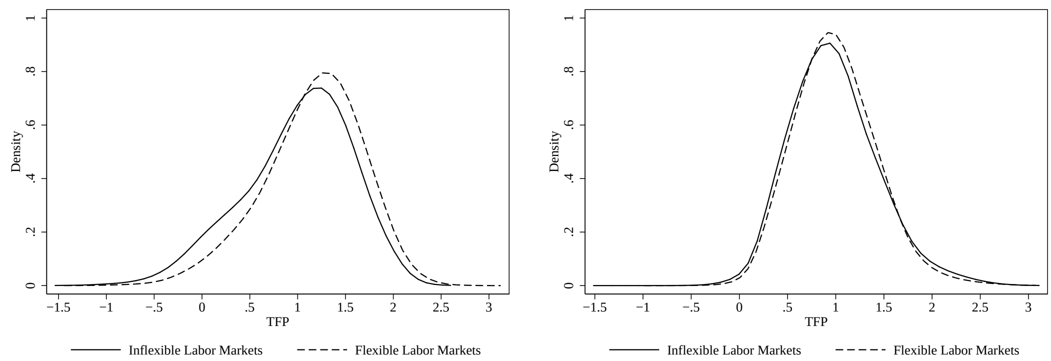
$$\left(\frac{Q}{N}\right)_1 N_1 - \left(\frac{Q}{N}\right)_0 N_0 = \left[\left(\frac{Q}{N}\right)_1 - \left(\frac{Q}{N}\right)_0\right] N_1 + \left(\frac{Q}{N}\right)_0 [N_1 - N_0]$$

where the first term in the right hand side captures output differences coming from the intensive margin for a fixed number of plants. The second term fixes output per plant to capture extensive margin differences.

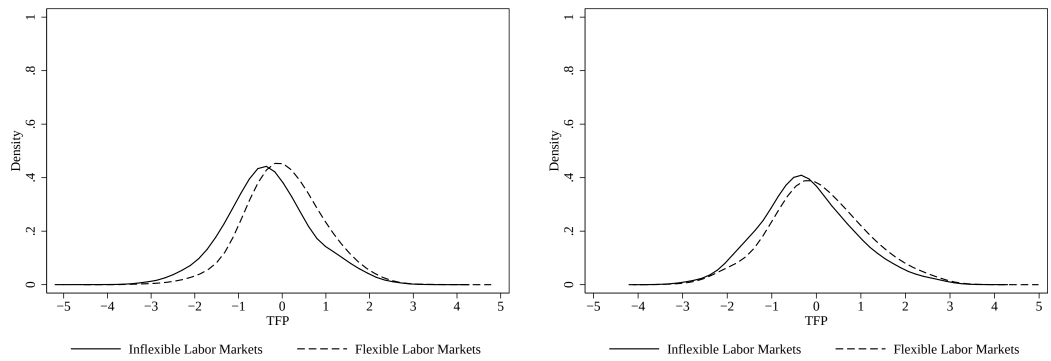
labor productivity measured as value added per employee, net of industry fixed effects. Panels (a) and (b) show that industries with high labor intensity experience a greater improvement in their TFP distribution from the relaxation of labor laws' enforcement when compared to less labor intensive industries. Additionally, panels (c) and (d) show that, irrespective of the industry's labor usage, the distribution of labor productivity in flexible states is always to the right of that of states with stricter EPL but the distance between distributions is larger in labor-intensive industries.

So far, this preliminary evidence suggests that labor intensive industries benefit the most from EPL relaxation in Indian states. Section 5 below will test if the patterns identified for productivity remain relevant after a more rigorous analysis.

Figure II.5: Labor market regulation, labor intensity, and productivity



(a) TFP: High labor intensity (b) TFP: Low labor intensity



(c) Labor productivity: High labor intensity (d) Labor productivity: Low labor intensity

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.

4 Empirical Strategy

The main objective of this study is to assess the effect of employment regulation reform in India on TFP and labor productivity between 1998-99 and 2007-08. The basic specification proposed to evaluate productivity performance is similar to the one used by Aghion *et al.* (2008), in the sense that we take advantage of state-level variation in labor regulation, but we extend it to incorporate industry-level variation. Our fundamental assumption is that EPL reform is more likely to restrict plants operating in industries with higher labor intensity, or alternatively higher volatility.

Consider the partial equilibrium effect of a change in EPL derived in equation (II.1). The impact on productivity is expected to be larger in industries where plants rely more on labor than in industries in which this input is relatively less important. We can also think of more volatile industries having a harder time adjusting their labor input usage when strict labor regulations are in place. To capture the effect of labor regulation reform, we use a difference-in-differences estimator inspired by Rajan and Zingales (1998). By comparing cross-industry differences in states with different levels of labor reform we can evaluate the effect of EPL changes towards pro-employer legislation on productivity levels. Labor-intensive industries will be more constrained by labor regulation so the impact of EPL reform is identified using industries with a lower output elasticity with respect to output as a control group. Relaxation in labor regulation may also interact with industry-level differences in the dispersion of plant-level shocks to generate larger TFP gains among sectors with a higher dispersion of these shocks.

Below, we briefly describe the TFP estimates used in this study. Next, we proceed to describe the econometric model used to measure the impact of labor reform on manufacturing plants.

4.1 TFP Measures

When trying to estimate a production function using observed plant-level variables, obtaining TFP measures from the residuals encompasses several measurement and econometric problems. One issue is that measurement of outputs and inputs generates an aggregation problem, especially in multiproduct plants. Another measurement issue relates to capital usage; since it is very tough to obtain data on capital consumption as an input in the production process, the researcher has to settle for the book value of total capital and machinery involved in the production process.

Although the previous problems are complex enough, there is not much the empirical researcher can do about them but try to collect better quality and more detailed micro data. In addition to these problems, several econometric difficulties arise when estimating production functions at the plant level. Two of the most prominent and serious problems are simultaneity and selection biases.

Assume a Cobb-Douglas production function like the one described below:

$$Y_{it} = A_{it} L_{it}^{\alpha} K_{it}^{\beta} M_{it}^{\gamma} F_{it}^{\lambda}$$

where Y_{it} are physical units of output and L_{it}, K_{it}, M_{it}, and F_{it} measure labor, fixed capital, materials, and fuels, respectively. Since A_{it} enters the right hand side in a multiplicative way, affecting all the other factors' marginal product simultaneously, it represents the TFP. Taking logarithms allows us to use a linear estimation model described by:

$$y_{it} = \alpha l_{it} + \beta k_{it} + \gamma m_{it} + \lambda f_{it} + u_{it} \tag{II.2}$$

where small letters are used for logs.

From the estimation of equation (II.2), we can retrieve the error term u_{it}, which is the log of plant-specific A_{it}, provided that the coefficients on the inputs are consistently estimated. OLS estimation does not yield consistent estimates if plants' choices on exit and on factor demands (when they continue operating) depend on their productivity. This fact generates both a selection and a simultaneity problem in the estimation of production functions.

Olley and Pakes (1996) deals with the simultaneity problem by using the firm's investment decision to proxy for unobserved productivity shocks. It is assumed that a higher value of the productivity shock observed by the firm (but unobserved by us) will induce higher investment today. The Olley-Pakes approach also offers a correction for selection bias due to exit. In the first stage, a probit of survival is estimated as a function of a polynomial of capital and investment, and the fitted values from this regression are used in the second stage to consistently estimate the production function parameters.[17]

Since this technique requires information on exit and lagged values of some variables, we estimate the parameters in equation (II.2) using Olley-Pakes in the restricted census sample, for which panel data is available. We estimate the coefficients for capital, labor, materials, and fuels separately for each industry and assume that these estimates are applicable to plants in the census as well as in the sample sector. We can then obtain TFP as a residual for all the plants using the industry-specific coefficient estimates. Estimating TFP using industry-specific regressions allows for differences in the production function's coefficients, including a constant term, which yields unit-free productivity residuals that are comparable across industries. In the end, TFP residuals are obtained as the exponential of the residual in equation (II.2).[18]

To estimate TFP at the plant level, we use real gross output instead of value added as the dependent variable. According to Basu and Fernald (1997) and

[17] See Olley and Pakes (1996). Their approach assumes a strictly monotonic relationship between output and investment so that all observations with zero investment are dropped. An alternative approach to deal with the simultaneity bias is offered by Levinsohn and Petrin (2003), who use intermediate inputs as a proxy for investment to avoid losing observations. However, only 4% of the plant-year observations in the restricted census sample used to estimate TFP have zero investment. Moreover, unlike Olley-Pakes, Levinsohn-Petrin methodology does not offer a correction for selection bias. For more details on the problems faced when estimating productivity as well as available solutions, see Arnold (2005).

[18] Notice that since the error is mean zero, this explains why the mean of the TFP distribution in Figure II.5 is so close to one.

Carlsson and Skans (2011), the use of value added is only valid for TFP estimation under perfect competition and constant returns to scale.[19] Labor is measured in number of workers and fixed capital is measured as the average of the net book real value of fixed capital at the beginning and at the end of the fiscal year. The amount of fuels and materials consumed is used to measure the usage of these inputs. Investment is measured by the gross value of additions to fixed capital. All the variables are measured in rupees at the end of the period and in 1993-94 constant prices, unless otherwise noted.

4.2 Econometric Model

Our analysis of the impact of labor reform on manufacturing outcomes relies on this basic model:

$$\log(W_{fist}) = \theta_0 + \theta_1 LI_i + \theta_2 R_s + \theta_3 (LI_i \times R_s) + \eta_t + \varepsilon_{fist} \qquad \text{(II.3)}$$

In equation (II.3), W_{fist} is some performance outcome for plant f, in industry i and state s, at year t. We analyze TFP and labor productivity (measured as value added per worker), but the Annex also provides some evidence on total gross output and total value added. LI_i denotes industry's i labor intensity measure while state labor reform is captured by R_s.

Our indicator of R_s is a dummy variable based on the normalized count of EPL reforms in each state. We label states as having flexible regulation when their labor reform index is at or above the median state in terms of the proportion of state-level reforms (using the count index). We adopt this dummy specification because the OECD measure of labor reform cannot be considered a continuous variable but is closer to an ordinal or categorical variable. However, there are too many categories to use it as such and the dummy specification eases presentation of the results.

[19] See Appendix C in Carlsson and Skans. They show that a residual measure of TFP that comes from value added is not independent of the use of intermediate inputs and factor input growth when there are increasing or decreasing returns to scale.

To measure LI_i, we construct a dummy variable for above and below the median labor-intensive industry based on the $\hat{\alpha}$s obtained from the estimation of equation (II.2).[20] We believe that the use of $\hat{\alpha}$ to measure the intrinsic labor intensity in each industry is superior to the use of the share of labor expenditures in total output. The use of the estimated output elasticity with respect to labor overcomes the potential biases that the ratio of labor expenditures to output may have due to the endogeneity of the plant's input choices. Moreover, since our TFP estimation using the Olley-Pakes methodology takes into account year fixed effects, $\hat{\alpha}$ provides a clean estimate of the underlying labor intensity of each industry that is not biased by exogenous demand or supply shocks in the inputs markets.

An alternative specification of equation (II.3) uses industry volatility measures instead of labor intensity. In that case, we follow Krishna and Levchenko (2009) and measure industry volatility by the standard deviation of the annual growth rate of plants' output. We then construct a dummy variable for above and below the median volatile industry.

Since our measure of EPL reform is time-invariant and measured at the state level, we cannot include state fixed effects. Similarly, our labor intensity indicator is fixed at the industry level, so it restrains us from including industry fixed effects.[21] We control for year fixed effects, denoted by η_t in equation (II.3), and add a plant-specific trend.[22] Robust variance estimates are used to adjust standard deviations for within-state correlation. We also incorporate additional controls in our estimates to make sure we take into account the effect of state-level characteristics.

The coefficient θ_3 on the interaction between LI_i and R_s will capture the heterogeneous effect of EPL reform on industries with different labor intensity. Given that R_s is higher when state labor reforms make EPL more flexible, a positive coefficient on the interaction implies that plants in industries that use labor

[20] Again, this specification follows the one of R_s and facilitates the exposition of the results.

[21] Full collinearity restrains us from including industry-year, state-year, or industry-state fixed effects.

[22] Of course, this trend is only relevant for plants present in multiple years and its removal does not quantitatively or qualitatively affect the results.

more intensively fare better in states with pro-employer labor regulation. In the alternative specification, which uses industry volatility in place of labor intensity, the interaction term should also have a positive coefficient since more volatile plants are expected to benefit the most from laxer labor regulations.

5 Results

The results presented in Table II.2 provide initial evidence of a beneficial effect on multifactor and labor productivity for labor intensive industries in states with higher levels of pro-employer labor reform. The positive and significant interaction of LI_i and R_s in column 1 shows that manufacturing plants with high labor requirements that operate in states moving towards more flexible regulation exhibit larger TFP gains than plants in less labor intensive industries. The interaction in the value added per worker equation is also positive but it is not significant.

The point estimates from Table II.2 imply that there are important multifactor productivity gains from conducting more labor reforms, particularly for plants in labor intensive industries. In 2008, the ratio of the geometric mean of TFP for plants in states with flexible labor markets over the same mean of TFP for plants

Table II.2: Effect of EPL reforms on TFP and labor productivity by labor intensity

	log(TFP)	log(VA/L)
Constant	0.943***	-0.463***
	(0.031)	(0.064)
High labor intensity	0.016	-0.115*
	(0.051)	(0.060)
Pro-employer EPL reform	0.013	0.260**
	(0.035)	(0.109)
High labor intensity x Pro-employer EPL reform	0.145**	0.119
	(0.061)	(0.079)
Observations	224,634	213,147
R-squared	0.043	0.043
Firm trend	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

in states with inflexible labor markets is 1.17 in labor intensive industries, but it is close to one in industries with lower $\hat{\alpha}s$.[23] In other words, a plant in a labor intensive industry that moves from an inflexible to a flexible state would get an average TFP improvement of about 17% while TFP gains are close to zero in industries with lower labor intensity.

To check the robustness of our findings, we add a number of control variables to take into account state characteristics. These include both time-variant as well as time-invariant controls at the state level. Among the first group, we use the log of fixed and mobile phones's availability per 100 population, log of the installed electric capacity per million people, and the log of road density. Information on telephones, installed electric capacity, and road density are reasonable proxies for the general conditions of infrastructure, which are expected to be positively related to manufacturing output. We also include the OECD product market regulation index that measures how much regulations restrict competition.

Table II.3 shows that the positive effect identified for labor intensive plants in flexible labor markets is still present for TFP once we control for state characteristics. The interaction between EPL reform and high labor intensity is positive and significant. Once state-level controls are introduced, our point estimates indicate that, on average, plants in labor intensive industries and operating in flexible labor markets have a TFP residual that is 14% higher than it is among plants in states with low levels of EPL reform and high $\hat{\alpha}$s. Among plants in industries with low $\hat{\alpha}$s, TFP gains from EPL reform are negligible. Although the interaction of EPL reform and labor intensity is not significant in the value added per worker equation, there are slightly larger gains among plants in labor intensive industries. While plants in industries with low $\hat{\alpha}$s see their labor productivity increase by 28% where EPL reforms are more extensive, the effect of EPL reform in labor intensive industries

[23] Using the parameter estimates from Table II.2, the mean values of the trend, and the year dummy corresponding to 2008, we predict log(TFP) for 4 groups: i) plants in states with high levels of EPL reform and high $\hat{\alpha}$s, ii) plants in states with low levels of EPL reform and high $\hat{\alpha}$s, iii) plants with high levels of EPL reform and low $\hat{\alpha}$s, and iv) plants with low levels of EPL reform and low $\hat{\alpha}$s. To obtain 1.17, for example, we get the difference between the predictions of log(TFP) for group i) and ii) and exponentiate it to get the ratio of their TFP in levels.

Table II.3: Effect of EPL reforms on TFP and labor productivity by labor intensity, with state-level controls

	log(TFP)	log(VA/L)
Constant	1.274***	-1.026
	(0.278)	(1.012)
High labor intensity	0.004	-0.118*
	(0.054)	(0.062)
Pro-employer EPL reform	-0.023	0.248**
	(0.044)	(0.092)
High labor intensity x Pro-employer EPL reform	0.153**	0.124
	(0.063)	(0.075)
Time-variant state controls		
Log(Telephones/100 pop)	0.043**	0.031
	(0.019)	(0.044)
Log(Installed electricity capacity/million pop)	-0.018	0.019
	(0.021)	(0.115)
Log(Paved roads/1000 pop)	0.014	-0.027
	(0.014)	(0.065)
Time-invariant state controls		
Product Market Regulation	-0.032	0.060
	(0.050)	(0.292)
Observations	224,634	213,147
R-squared	0.048	0.044
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

translates into VA/L increases of 45%.

Next, we try to identify differential effects by plant size and type of ownership. Let X_{fist} denote a specific plant characteristic, such as size or ownership type. We extend the model in equation (II.3) in the following way:

$$\begin{aligned}\log(W_{fist}) &= \theta_0 + \theta_1 LI_i + \theta_2 R_s + \theta_3(LI_i \times R_s) + \\ &\quad \theta_4 X_{fist} + \theta_5(LI_i \times X_{fist}) + \theta_6(R_s \times X_{fist}) + \theta_7(LI_i \times R_s \times X_{fist}) + \eta_t + \varepsilon_{fist}\end{aligned}$$

Although θ_3 will still give us the average effect of the interaction of labor intensity and labor reform on productivity, the coefficient θ_7 becomes particularly important since it will capture any heterogeneous effects due to differences in X_{fist}.

In the case of plant size, X_{fist} will be a matrix of 4 size dummies. These are constructed using the number of workers with cutoffs at 50, 100, and 250. The first cutoff corresponds to the presence of a few labor laws that are enforced starting at this establishment size. The second cutoff is consistent with IDA's national threshold set in 1982. The last cutoff is in line with empirical evidence for India, above which plant TFP was observed to be substantially higher (see Dougherty *et al.*, 2009). This check is particularly important since larger plants are subject to stricter labor regulation but are also more likely to subcontract workers to evade labor laws.

Let the share of contract labor in total expenditures for each plant be given by:

$$h^*_{fist} = \delta X_{fist} + \nu_i + \nu_s + \nu_t - \mu_{fist}$$

where ν_i, ν_s, and ν_t denote industry, state and year fixed effects. From this latent variable, we construct a categorical variable, h_{fist}, such that $h_{fist} = 1$ if the plant hires no contract labor, $h_{fist} = 2$ when the plant spends 20% or less of their labor costs on indirect labor, and $h_{fist} = 3$ when the plant spends more than 20% of total labor expenditures on hiring labor through contractors. Let the cutoffs for h^*_{fist} be given by $\xi_0 = -\infty$, $\xi_1 = 0$, $\xi_2 = 0.2$, and $\xi_3 = \infty$. The probability of $h_{fist} = H$ is given by:

$$\begin{aligned}\Pr(h_{fist} = H|X_{fist}) &= \Pr(\xi_{H-1} < h^*_{fist} < \xi_H|X_{fist}) \\ &= \Phi(\delta X_{fist} + \nu_i + \nu_s + \nu_t - \xi_{H-1}) - \Phi(\delta X_{fist} + \nu_i + \nu_s + \nu_t - \xi_H)\end{aligned}$$

where Φ is the normal cumulative distribution with mean zero and variance σ^2.

Table II.4 reports δ estimates from an interval regression model like the one above. We find that larger plants are more likely to hire labor indirectly: the share of contracted labor increases by a factor of 0.317 when we compare plants with 250 or more workers to plants with less than 50 workers. Similarly, relative to the smallest plants, medium size plants with 50 to 99 workers and 100 to 249 workers see their share of contract labor expenditures increased by a factor of 0.268 and 0.3,

Table II.4: Interval regression results for the share of contract labor in total labor expenditures

Plant size (base: $<$ 50 workers)	δ	S.E.
[50 – 100[	0.268***	0.004
[100 – 250[	0.300***	0.003
250 or more	0.317***	0.003
Observations	229693	
Log likelihood	-165507.27	
σ	0.384***	
Year FE	yes	
Industry FE	yes	
State FE	yes	

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

respectively. Clearly, the tendency of larger plants to hire more workers through contractors helps them partially bypass labor legislation. Consequently, we expect them to benefit less from the state-labor reforms.

Estimates with the size dummies shown in Table II.5 confirm our initial prediction. The coefficient on the interaction between pro-employer EPL reform and labor intensity is now positive and significant both for TFP and labor productivity (θ_3). Moreover, the coefficient on triple interaction between EPL, labor intensity, and plant size (θ_7) is not significant for medium size plants but it is negative and significant for larger plants in both columns. Both in terms of TFP and labor productivity, plants with more than 250 workers in industries with high labor intensity earn much less than their smaller counterparts from pro-employer labor reforms. This result is consistent with the fact that larger plants face higher restrictions in inflexible labor regulation settings. Since many norms and regulations apply only to them, it looks like they have found a way out by reducing their dependence on a permanent workforce and relying more on temporary labor hired through contractors as suggested by Table II.4. It has been well documented that casual or contract labor in India provides unskilled labor at wages below the minimum wage and without benefits, so the substitution of regular labor for casual labor can help larger plants reduce the labor costs imposed by more stringent EPL.

We also estimated the effects of pro-employer EPL reform separately for publicly

Table II.5: Effect of EPL reforms on TFP and labor productivity by labor intensity and plant size, with state-level controls

	log(TFP)	log(VA/L)
Constant	1.371***	-0.757
	(0.261)	(0.995)
High labor intensity	-0.049	-0.125**
	(0.066)	(0.047)
Pro-employer EPL reform	-0.032	0.202**
	(0.034)	(0.096)
High labor intensity x Pro-employer EPL reform	0.161**	0.187***
	(0.068)	(0.054)
Plant Size (Base: <= 50 workers)		
]50 – 100]	0.127	0.069
	(0.074)	(0.139)
]100 – 250]	-0.023	0.290**
	(0.054)	(0.105)
> 250	0.049	0.604***
	(0.059)	(0.174)
High labor intensity x]50-100]	-0.075	0.257
	(0.096)	(0.178)
High labor intensity x]100-250]	0.094	0.118
	(0.130)	(0.125)
High labor intensity x >250	0.278***	-0.133
	(0.072)	(0.221)
Pro-employer EPL reform x]50-100]	-0.063	0.042
	(0.074)	(0.148)
Pro-employer EPL reform x]100-250]	0.077	-0.038
	(0.059)	(0.156)
Pro-employer EPL reform x >250	0.020	0.269
	(0.064)	(0.175)
High labor intensity x Pro-employer EPL reform x]50-100]	0.105	-0.115
	(0.099)	(0.187)
High labor intensity x Pro-employer EPL reform x]100-250]	-0.034	-0.130
	(0.138)	(0.160)
High labor intensity x Pro-employer EPL reform x >250	-0.154*	-0.398*
	(0.085)	(0.229)
Time-variant state controls		
Log(Telephones/100 pop)	0.044**	0.033
	(0.018)	(0.043)
Log(Installed electricity capacity/million pop)	-0.028	-0.018
	(0.021)	(0.112)
Log(Paved roads/1000 pop)	0.020	-0.007
	(0.014)	(0.063)
Time-invariant state controls		
Product Market Regulation	-0.026	0.089
	(0.048)	(0.279)
Observations	224,634	213,147
R-squared	0.065	0.090
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

and privately owned plants, where X_{fist} is a dummy that is equal to one when the plant is publicly owned. In the sample periods analyzed, publicly owned plants tend to have lower rates of job destruction and creation than privately owned plants. Although public plants tend to have a lower turnover rate than privately owned plants, their net contribution to employment is highly negative in half of the rounds analyzed. A proposed explanation for this lies in voluntary retirement schemes (VRS), which are used as a mutually agreeable mechanism for downsizing. Since VRS has allowed public plants to bypass labor regulation and adjust their labor usage it may be possible that the effect of EPL within them is smaller than among private plants.

Table II.6 presents the results obtained by ownership type. Public plants in labor intensive industries tend to have higher multifactor productivity but lower labor productivity as shown by the interaction of the ownership dummy and the labor intensity dummy. Moreover, the interaction between pro-worker EPL reform and labor intensity is positive and significant for both TFP and VA/L, which shows that the average beneficial effect of labor reform on labor intensive industries is higher. As we expected, the triple interaction for EPL reform, labor intensity, and public ownership is negative and significant for both TFP and labor productivity, though only significant for the former. This implies that labor intensive public plants in flexible markets exhibit lower TFP gains from EPL reform, which is in line with the use of VRS among public plants as a strategy to circumvent labor regulation. Through this strategy, constrained public plants have been able to ameliorate the negative effects of inflexible regulation on productivity so that pro-employer labor reforms have smaller relative effects among them.

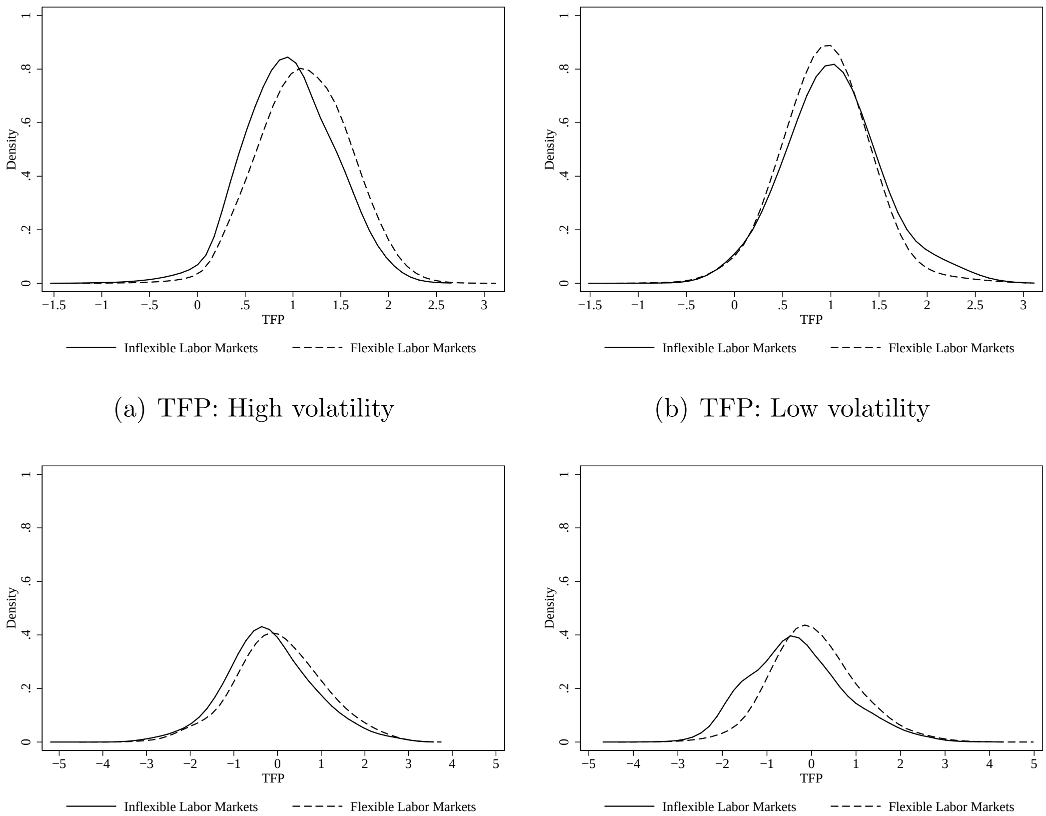
In general, the results show that there are important TFP and some labor productivity gains for labor intensive plants that operate in states with laxer EPL. Moreover, the different strategies used by plants to overcome the constraints imposed by labor regulation generate differential effects of state-level labor reform both by plant size and type of ownership.

Table II.6: Effect of EPL reforms on TFP and labor productivity by labor intensity and ownership type, with state-level controls

	log(TFP)	log(VA/L)
Constant	1.339***	-0.568
	(0.279)	(0.910)
High labor intensity	-0.048	-0.056
	(0.051)	(0.064)
Pro-employer EPL reform	-0.042	0.184*
	(0.049)	(0.098)
High labor intensity x Pro-employer EPL reform	0.213***	0.162*
	(0.060)	(0.082)
Public plant	0.007	0.735***
	(0.047)	(0.120)
High labor intensity x Public plant	0.208**	-0.274**
	(0.088)	(0.101)
Pro-employer EPL reform x Public plant	0.069	0.203
	(0.051)	(0.135)
High labor intensity x Pro-employer EPL reform x Public plant	-0.243**	-0.179
	(0.090)	(0.122)
Time-variant state controls		
Log(Telephones/100 pop)	0.044**	0.040
	(0.019)	(0.041)
Log(Installed electricity capacity/million pop)	-0.022	-0.019
	(0.022)	(0.104)
Log(Paved roads/1000 pop)	0.016	-0.004
	(0.014)	(0.059)
Time-invariant state controls		
Product Market Regulation	-0.038	0.005
	(0.051)	(0.256)
Observations	224,535	213,018
R-squared	0.053	0.130
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure II.6: Labor market regulation, volatility, and productivity



(a) TFP: High volatility

(b) TFP: Low volatility

(c) Labor productivity: High volatility

(d) Labor productivity: Low volatility

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.

5.1 Volatility

We now test if laxer labor regulation benefits volatile industries relatively more as suggested by Poschke (2009) and others. Our measure of volatility is similar to the one used by Krishna and Levchenko (2009): the standard deviation of the annual growth rate of plants' output in a given industry. Notice that we need a plant-level growth measure to quantify volatility, so we are will obtain a proxy for each industry from the restricted census sample, average it over all the ASI rounds we use, and apply it to the complete sample of plants. We then construct a dummy variable which classifies industries as highly volatile when they are at or above the median industry in terms of the average standard deviation of annual growth rate of output.

Panels (a) and (b) in Figure II.6 present preliminary evidence on the existence of a comparative advantage among more volatile plants in flexible markets. State-level labor reforms seem to shift the TFP distribution to the right only in more turbulent industries, which is in line with Cuñat and Melitz (2007) findings. However, as panels (c) and (d) show, the comparative advantage identified in terms of TFP among plants in more volatile sectors is not present for labor productivity. The difference between the distributions of value added per worker (VA/L) across states with different levels of labor reform does not seem to vary by industry-level volatility, although plants in more flexible states always have better VA/L distributions.

Table II.7 confirms these patterns. The interaction between EPL and volatility is positive and significant only in the TFP equation, which implies that plants in

Table II.7: Effect of EPL reforms on TFP and labor productivity by volatility, with state-level controls

	log(TFP)	log(VA/L)
Constant	1.411***	-1.078
	(0.324)	(1.039)
High volatility	-0.052	0.097
	(0.108)	(0.097)
Pro-employer EPL reform	-0.116	0.379***
	(0.078)	(0.125)
High volatility x Pro-employer EPL reform	0.225*	-0.151
	(0.116)	(0.101)
Time-variant state controls		
Log(Telephones/100 pop)	0.042**	0.030
	(0.019)	(0.044)
Log(Installed electricity capacity/million pop)	-0.020	0.018
	(0.022)	(0.114)
Log(Paved roads/1000 pop)	0.016	-0.027
	(0.015)	(0.065)
Time-invariant state controls		
Product Market Regulation	-0.058	0.041
	(0.057)	(0.283)
Observations	224,634	213,147
R-squared	0.051	0.044
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

more volatile industries that operate in flexible labor markets have a comparative advantage in terms of multifactor productivity. The larger costs of hiring and firing people imposed by strict EPL seem to be particularly restrictive in sectors with higher volatility, generating an unequal distribution of the productivity gains that come from labor market deregulation.

6 Robustness checks

In the previous section, we showed that plants in more labor intensive and/or more volatile industries are the big winners of pro-worker labor reforms in India. The interactions between higher levels of EPL reform and labor intensity as well as between pro-worker EPL reform and volatility were positive and significant even after the introduction of state-level controls. Moreover, the Annex shows that our results are not sensitive to a different specification of the labor intensity measure. Including labor intensity in the model either as the value of $\hat{\alpha}$ or the relative ranking of each industry implied by $\hat{\alpha}$ does not affect the results presented above.

This section provides additional robustness tests of the impact of labor regulation on organized manufacturing plants. First, we try out two alternative measures of EPL available in the literature. We use Gupta *et al.*'s (2008) EPL index as well as the BB index updated through 2008 using Malik (2011). The former uses the BB index, Bhattacharjea's (2006) indicator – which takes into account legislative and judicial interventions affecting Chapter V-B of the IDA – and Dougherty's (2009) index to construct a composite measure of labor regulation. This composite measure, which we call EPL-G, classifies states into inflexible, neutral, and flexible in terms of their EPL strictness.

We also check if our results hold when we use industry layoff propensity instead of labor intensity. According to Bassanini *et al.* (2009), the firm's natural propensity to adjust through layoffs will influence the size of the costs imposed by EPL so we would expect that plants that operate in industries that are more likely to adjust through layoffs will benefit the most from more flexible labor laws, especially those pertaining to retrenchment and firing of workers.

Table II.8 shows the estimates using Gupta *et al.*'s (2008) EPL indicator.[24] If we focus on the interaction effect identified for states classified as flexible by EPL-G, the estimates are very similar to those obtained with our measure of EPL reform. In terms of TFP gains, Table II.3 reported an interaction effect of 0.153 while this effect amounts to 0.143 when EPL-G is used. Although still not significant, the interaction effect of EPL-G and labor intensity in the labor productivity equation 0.120 is very close to the effect identified in Table II.3 using our EPL measure 0.124.

When the BB index is used, the positive effects of labor regulation previously identified among plants in labor intensive industries go away. Table II.9 shows that when the cumulative BB index is used, the interaction between EPL reform and labor intensity is negative and significant in the case of TFP, though it remains insignificant for value added per worker. These results are not too surprising if we consider that the BB index only captures formal amendments to the IDA, which have been scarce in recent years. In fact, there were only four pro-worker reforms registered in Gujarat (in 2004) and two pro-employer reforms in Madhya Pradesh (in 2003) after 1999. Moreover, the correlation between BB and Dougherty's (2009) proportional index is -0.25, which could be indicating that the lack of reforms to the IDA post-1990 were compensated by formal or informal state-level changes in industrial practices on the ground.

We conclude by testing if plants in industries with a higher layoff propensity benefit the most from labor reforms as suggested by Bassanini *et al.* (2009).[25] The evidence provided in Table II.10 shows that, indeed, plants in industries with higher $\hat{\alpha}$s are the ones who experience the largest TFP improvements from state-level labor reforms. The magnitude of the interaction effect of EPL reforms and layoff propensities implies that, on average, plants in industries with a high layoff

[24] Compared to our final sample of states, Gupta *et al.* omits two states/union territories, Delhi and Himachal Pradesh, which represent 6.2% of the plant-year observations in our complete sample.

[25] Due to lack of adequate US data, tobacco industries were dropped from our original sample. This generates a loss of 1.35% of the plant-year observations.

propensity are 20% more productive in flexible states than in inflexible states.

Table II.8: Effect of EPL-G on productivity and output by labor intensity, all plants

	log(TFP)	log(VA/L)
Constant	1.059**	-0.085
	(0.380)	(1.657)
High labor intensity	0.055***	-0.104***
	(0.007)	(0.026)
Neutral EPL-G	0.006	-0.293
	(0.025)	(0.177)
Flexible EPL-G	-0.027	-0.269
	(0.025)	(0.166)
High LI x Neutral EPL-G	0.052	0.144
	(0.036)	(0.089)
High LI x Flexible EPL-G	0.143***	0.120
	(0.042)	(0.086)
Time-variant state controls		
Log(Telephones/100 pop)	0.038	0.082
	(0.024)	(0.078)
Log(Installed electricity capacity/million pop)	0.005	0.003
	(0.030)	(0.119)
Log(Paved roads/1000 pop)	0.001	-0.034
	(0.018)	(0.064)
Time-invariant state controls		
Product Market Regulation	-0.065	-0.215
	(0.050)	(0.329)
Observations	215,208	204,129
R-squared	0.047	0.045
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

7 Conclusions and Extensions

This paper studies the extent to which the effects of EPL on productivity among registered manufacturing plants change by labor intensity. To do this, we rely on a difference-in-differences strategy that includes state-level EPL reforms and industry-level labor intensity interactions. Our main finding is that there are important positive gains in terms of multifactor productivity for labor intensive plants that

Table II.9: Effect of EPL measured by BB index on productivity and output by labor intensity, all plants

	log(TFP)	log(VA/L)
Constant	1.173***	0.100
	(0.326)	(1.320)
High labor intensity	0.193***	-0.005
	(0.050)	(0.115)
Neutral EPL (BB)	0.011	0.080
	(0.031)	(0.152)
Flexible EPL (BB)	0.022	0.338*
	(0.029)	(0.170)
High labor intensity x Neutral EPL (BB)	-0.063	0.059
	(0.055)	(0.130)
High labor intensity x Flexible EPL (BB)	-0.137**	-0.098
	(0.051)	(0.116)
Time-variant state controls		
Log(Telephones/100 pop)	0.040**	0.093
	(0.018)	(0.067)
Log(Installed electricity capacity/million pop)	-0.008	-0.037
	(0.028)	(0.101)
Log(Paved roads/1000 pop)	0.007	-0.010
	(0.017)	(0.050)
Time-invariant state controls		
Product Market Regulation	-0.061	-0.226
	(0.047)	(0.282)
Observations	224,634	213,147
R-squared	0.048	0.046
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

operate in states with laxer labor regulation. This effect remains after the addition of state-level controls as well as various sensitivity checks. Our point estimates indicate that, on average, plants in labor intensive industries and in flexible labor markets have TFP residuals 14% higher than those registered for their counterparts in states with more stringent labor laws. However, EPL reform does not seem to have any important effect on plants with lower levels of labor intensity. Similarly, the TFP of plants in more volatile industries and in states that experienced pro-employer reforms is 11% higher than that of plants in volatile industries and in

Table II.10: Effect of EPL on productivity and output by layoff propensity, all plants

	log(TFP)	log(VA/L)
Constant	1.169***	-1.023
	(0.259)	(1.001)
High layoff propensity	0.080	-0.179**
	(0.065)	(0.082)
Pro-employer EPL reform	-0.027	0.251***
	(0.042)	(0.087)
High layoff propensity x Pro-employer EPL reform	0.213***	0.179*
	(0.071)	(0.096)
Time-variant state controls		
Log(Telephones/100 pop)	0.041*	0.033
	(0.020)	(0.044)
Log(Installed electricity capacity/million pop)	-0.013	0.018
	(0.019)	(0.114)
Log(Paved roads/1000 pop)	0.009	-0.026
	(0.013)	(0.065)
Time-invariant state controls		
Product Market Regulation	-0.028	0.065
	(0.045)	(0.289)
Observations	224,634	213,147
R-squared	0.104	0.044
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

more restrictive states. Among plants in less volatile industries, EPL reform seems to drive a 11% reduction in TFP residuals. In the case of labor productivity, we fail to find robust evidence in favor of a differential effect of EPL reform by either labor intensity or volatility.

We also find that the different strategies used by plants to overcome the constraints imposed by labor regulations generate heterogeneous effects of state-level labor reform both by plant size and type of ownership. Given the extensive use of contract labor among large plants and voluntary retirement schemes among public plants, smaller plants and private plants tend to accrue the largest productivity gains from state-level labor reforms.

Our study is particularly important for three reasons. This is the first study that makes use of plant-level information from the ASI to evaluate the effect of EPL in India. Second, we take advantage of the recently available ASI panel data to obtain plant-level TFP measures that control for simultaneity and selection bias using the Olley-Pakes approach. This feature is unique to our study since previous papers on the topic have only measured the effects of EPL on labor productivity measured as value added per worker or on aggregate measures of TFP at the industry-level. Finally, our measure of labor regulation is much more comprehensive and appropriate for the years analyzed than the BB index, popular in the EPL literature in India. In particular, our EPL reform index takes into account both formal and informal amendments to the labor laws at the state level.

Although the coverage of our EPL reform indicator is a plus, we acknowledge the important data limitations posed by the OECD index. Our analysis could greatly benefit from a time series version of the labor reform indicator that could allow us to evaluate short versus long-term effects as well as to include fixed effects at the state level. However, our attempts to collect a time-varying state-level EPL indicator have not yet been successful. Since the index goes beyond formal amendments to cover informal changes to labor rules and practices – many of which are not systematically notified in a consolidated publication – it is very difficult to track the exact dates in which these practices actually changed at the state level.

Although we are able to take advantage of the longitudinal data available in the ASI, we are aware that taking the industry-wise production function estimates from the restricted census sample as applicable to the complete sample is a strong assumption. Unfortunately, this is the only way in which we can implement the Olley-Pakes methodology to obtain clean estimates of plant-level TFP residuals. We believe that relying on OLS estimates of multifactor productivity in the complete sample, or trying to impute the sample firm's prior observations would be even more problematic than the approach we take here.

Preliminary evidence also shows that the effect of labor regulation reforms may be non-linear, which could potentially be explained by endogenous relocation of plants from states with more stringent regulation to states with more flexible EPL. Our future agenda includes the development of a partial or general equilibrium model that can help us to explain this pattern.

8 Annex

Table II.11: Descriptive Statistics: All years

(a) All plants

Variable	Obs	Mean	S.D	Min	Max
Output	239,921	330.24	3,075.47	0.01	320,327.70
Value added	239,921	38.47	213.83	-157.29	26,969.15
Fixed capital	239,921	111.22	722.22	0.00	56,809.98
Number of workers	239,921	175.76	420.85	0.00	21,637.00
Investment	239,921	14.87	128.99	0.00	17,713.72
Fuel expenditures	239,921	7.32	39.16	0.00	2,639.63
Intermediate inputs	239,921	136.33	878.74	0.00	66,449.92
Share of contract labor	239,726	0.09	0.20	0.00	1.00
Age of the plant	239,088	20.92	19.61	0.00	208.00
Plant size dummies (based on ♯ workers)					
< 50	239,921	0.52		0.00	1.00
$[50 - 100[$	239,921	0.13		0.00	1.00
$[100 - 250[$	239,921	0.16		0.00	1.00
≥ 250	239,921	0.18		0.00	1.00
Public ownership (dummy)	239,785	0.23		0.00	1.00
TFP (Olley-Pakes residuals)	238,961	1.05	0.47	-6.96	5.29
Labor productivity (VA/L)	222,363	0.00	1.02	-5.00	4.79
Volatility (S.D. of annual growth rate of output)	239,921	0.71	0.20	0.31	0.98

(b) Restricted Census sample

Variable	Obs	Mean	S.D	Min	Max
Output	49,895	1,290.73	6,642.43	0.02	320,327.70
Value added	49,895	154.63	446.22	-157.29	26,969.15
Fixed capital	49,895	455.01	1,518.57	0.00	56,809.98
Number of workers	49,895	646.61	745.19	200.00	21,637.00
Investment	49,895	58.33	267.54	0.00	17,713.72
Fuel expenditures	49,895	29.34	81.24	0.00	2,639.63
Intermediate inputs	49,895	513.08	1,868.66	0.14	66,449.92
Share of contract labor	49,873	0.10	0.18	0.00	1.00
Age of the plant	49,880	28.88	25.34	0.00	208.00
Plant size dummies (based on ♯ workers)					
< 50	49,895	0.00		0.00	0.00
$[50 - 100[$	49,895	0.00		0.00	0.00
$[100 - 250[$	49,895	0.15		0.00	1.00
≥ 250	49,895	0.85		0.00	1.00
Public ownership dummy	49,864	0.59		0.00	1.00
TFP (Olley-Pakes residuals)	49,879	1.10	0.49	-6.96	4.04
Labor productivity (VA/L)	46,204	0.44	1.10	-4.14	4.79
Volatility (S.D. of annual growth rate of output)	49,895	0.72	0.19	0.31	0.98

Table II.12: Effect of EPL reforms on total output and total value added by labor intensity, adding state-level controls

	log(Q)	log(VA)
Constant	-3.564**	-4.325***
	(1.614)	(1.470)
High labor intensity	-0.149	-0.152
	(0.161)	(0.115)
Pro-employer EPL reform	0.253*	0.390***
	(0.139)	(0.128)
High labor intensity x Pro-employer EPL reform	0.184	0.190
	(0.165)	(0.120)
Time-variant state controls		
Log(Telephones/100 pop)	-0.007	-0.018
	(0.101)	(0.081)
Log(Installed electricity capacity/million pop)	0.228	0.237
	(0.170)	(0.145)
Log(Paved roads/1000 pop)	-0.149	-0.147*
	(0.098)	(0.084)
Time-invariant state controls		
Product Market Regulation	-0.149	0.136
	(0.433)	(0.419)
Observations	217,379	229,863
R-squared	0.196	0.179
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
Note: Output (Q) and value added (VA) are net of industry fixed effects.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table II.13: Effect of EPL reforms on TFP and VA/L by labor intensity, adding state-level controls (LI_i as the value of $\hat{\alpha}$)

	log(TFP)	log(VA/L)
Constant	1.432***	-0.998
	(0.332)	(1.011)
Labor intensity ($\hat{\alpha}$)	-0.067	0.242**
	(0.047)	(0.092)
Pro-employer EPL reform	-1.681***	-0.499*
	(0.345)	(0.278)
Labor intensity ($\hat{\alpha}$) x Pro-employer EPL reform	1.485***	0.638
	(0.425)	(0.431)
Time-variant state controls		
Log(Telephones/100 pop)	0.055***	0.029
	(0.017)	(0.044)
Log(Installed electricity capacity/million pop)	-0.014	0.016
	(0.025)	(0.115)
Log(Paved roads/1000 pop)	0.015	-0.026
	(0.015)	(0.065)
Time-invariant state controls		
Product Market Regulation	-0.052	0.055
	(0.060)	(0.289)
Observations	224,634	213,147
R-squared	0.031	0.043
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table II.14: Effect of EPL reforms on TFP and VA/L by labor intensity, adding state-level controls (LI_i as a ranking based on $\hat{\alpha}$)

	log(TFP)	log(VA/L)
Constant	1.282***	-0.992
	(0.283)	(1.010)
Labor intensity (ranking)	-0.114	0.235**
	(0.068)	(0.111)
Pro-employer EPL reform	-0.003	-0.006
	(0.006)	(0.006)
Labor intensity (ranking) x Pro-employer EPL reform	0.016**	0.006
	(0.007)	(0.007)
Time-variant state controls		
Log(Telephones/100 pop)	0.043**	0.030
	(0.019)	(0.045)
Log(Installed electricity capacity/million pop)	-0.017	0.018
	(0.022)	(0.116)
Log(Paved roads/1000 pop)	0.014	-0.026
	(0.014)	(0.065)
Time-invariant state controls		
Product Market Regulation	-0.030	0.056
	(0.050)	(0.290)
Observations	224,634	213,147
R-squared	0.045	0.043
Firm trend	yes	yes
State-level controls	yes	yes
Year FE	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Robust clustered standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table II.15: Effect of EPL on productivity, total output and value added by labor intensity, restricted census sample

	log(TFP)	log(Q)	log(VA)
Constant	2.121***	12.067***	10.651***
	(0.022)	(0.027)	(0.032)
High Labor intensity	-0.211***	-0.158***	0.091**
	(0.033)	(0.037)	(0.044)
Pro-employer EPL reform	-0.164***	0.411***	0.374***
	(0.017)	(0.019)	(0.022)
High labor intensity x Pro-employer EPL reform	0.236***	0.055	0.060
	(0.035)	(0.040)	(0.047)
Observations	43,501	47,458	44,839
R-squared	0.029	0.028	0.017
Year FE	yes	yes	yes

Source: Annual Survey of Industries (ASI) 1998-99 to 2007-08.
Standard errors in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Chapter III

Outward foreign direct investment and productivity in Indian manufacturing firms[1]

1 Introduction

The wave of outbound foreign direct investment ("OFDI") by companies from the developing world, into both emerging and developed markets, is an endeavour that Indian companies have engaged in with particular gusto. Such flows have already become substantial relative to India's overall capital formation. The enormous size of these flows raises important questions about the impact of such investments on firm-level performance. Are these firms benefiting in terms of improved efficiency, through absorption of technology and intangible knowledge, requiring proximity and head-to-head competition, or are they simply seeking out new markets for their products when faced with barriers to expansion at home?

In order to address this question, one needs to go beyond the self-selection issues that are well known in the internationalization literature (see Helpman *et al.*, 2004; Greenaway and Kneller, 2007; Wagner, 2007; ISGEP, 2008), and examine the causal impact of OFDI on firm-level productivity outcomes, also referred to as *learning-by-doing*. We do this by using firm matching techniques in order to

[1] This chapter is based on the NIPFP working paper (2011), "Overseas FDI from a developing economy: is there learning-by-doing among Indian exporters?," jointly written with Rudrani Bhattacharya and Ila Patnaik (NIPFP).

address endogeneity concerns, and build on earlier work by Demirbas *et al.* (2010), who find larger and more innovative Indian firms invest overseas using an ordered probit analysis, and Bhattacharya *et al.* (2012), who examine productivity outcomes without matching, and show that while the most productive firms invest overseas in a traditional industry such as chemicals, the pheonomenon is reversed in software, where there is uncertainty about product quality and transport costs are low.

In the literature to date, the *causal* impact on firm-level productivity growth of OFDI originating from a developing country has not been rigorously examined (i.e. using matching techniques or instrumentation), an important omission since the motivation for this investment should differ substantially from that of OFDI originating from more advanced countries. So far in the more advanced country context, the evidence of a learning effect from OFDI is still inconclusive (see Hayakawa *et al.*, 2012). For instance, while studies of Italian and Japanese firms have found little evidence of learning effects, evidence for France (Hijzen *et al.*, 2011) suggests that there may be learning effects when OFDI is targeted at similarly advanced economies.[2]

In the related case of learning-by-doing through exporting, though positive evidence for developed countries is scant, a meta-analysis of studies of exporting by Martins and Yang (2009) finds a positive causal effect on productivity when developing country firms export to more advanced economies. And for India, positive effects were found by Pattnayak and Thangavelu (2009) and Mukim (2011), though Tabrizy and Trofimenko (2010) found none. Such results beg the question of whether Indian firms' engagement in OFDI could boost their productivity through further learning.

This paper focuses on the decision to engage in OFDI by firms that are already exporting, and assesses whether there is a static or dynamic productivity gain in terms of levels or growth. It does this using balance sheet data from the 2,500

[2] In the inverse case of acquisition-oriented FDI of exporting firms, there is evidence of UK manufacturing firms experiencing productivity gains through technology transfer (Girma *et al.*, 2007).

most liquid Indian firms, from the CMIE Prowess database, covering all of the liberalization period. Only exporting firms are included, in order to ensure that all firms are facing a similarly competitive international environment. The Mahalnobis distance is then used to match non-OFDI and OFDI firms which have similar sales, assets, and asset tangibility. Levinsohn and Petrin (2003) measures of productivity are computed, which account for problems of simulteneity in input choices. Panel estimators are used to assess the differences at the firm level, and a difference-in-differences estimator is deployed to consider the unobserved counterfactual.

Results of the analysis, which controls for endogeneity, show very little productivity difference between the OFDI and non-OFDI firms. No statistically significant productivity differences are observed between the closely matched firms, in either levels or growth rates. This holds true in all four productivity quartiles, with a standard set of control variables. The results are unambiguous, and imply that there is no learning-by-doing among OFDI firms, compared to similar firms that are already exporting, suggesting that there must be alternative motivations from efficiency for Indian firms that invest abroad.

The remainder of this paper is divided into four sections. Section 2 describes the dataset used, and its evolution over time. Section 3 lays out our methodology, including the productivity measurement and matching approach. Section 4 presents the results, and Section 5 concludes.

2 The data set

2.1 Growth of Indian MNCs

A wave of Indian companies began carrying out outbound foreign direct investment in 2000, when a change in capital controls formally allowed Indian firms to invest abroad. This was initially subject to a ceiling of USD 50 million. In subsequent years, this ceiling has been raised. In 2004 the rules were amended to allow firms to invest abroad up to twice their net worth. Table III.1 shows that from 2001

Table III.1: Rising number of multinationals

	New MNCs	Total MNCs	Domestic firms	Total firms
1999	5	38	2110	2148
2000	22	76	2143	2219
2001	73	230	2040	2270
2002	62	323	2010	2333
2003	41	349	2051	2400
2004	26	366	2079	2445
2005	37	401	2097	2498
2006	50	476	2075	2551
2007	49	555	2033	2588
2008	72	629	1968	2597
2009	26	650	1928	2578

onwards, a number of companies have invested overseas and become multinational corporations (MNCs). As a consequence, among the most actively traded companies, the number of Indian companies with investment abroad grew from 76 in 2000 to 650 in 2009.

We draw firm-level data from the CMIE Prowess database, using data for firms in the CMIE COSPI index, which is a set of roughly 2,500 companies with high stock market liquidity and good disclosure.[3] We focus here only on manufacturing firms, since the motivations affecting services firms are often different than for goods (Bhattacharya *et al.*, 2012). This leaves us with about 1,000 firms in recent years. Transformation into a MNC is defined as when a firm's ratio of its foreign investment to total assets exceeds 1%. If as a result of growth in domestic assets or decline in foreign assests, this percentage subsequently falls below 1%, we assume it remains an MNC. However, if its foreign investment is below this small threshold, it is treated as a domestic firm. Similarly, a firm is considered to have export status if the ratio of exports to sales crosses the threshold of 1%.

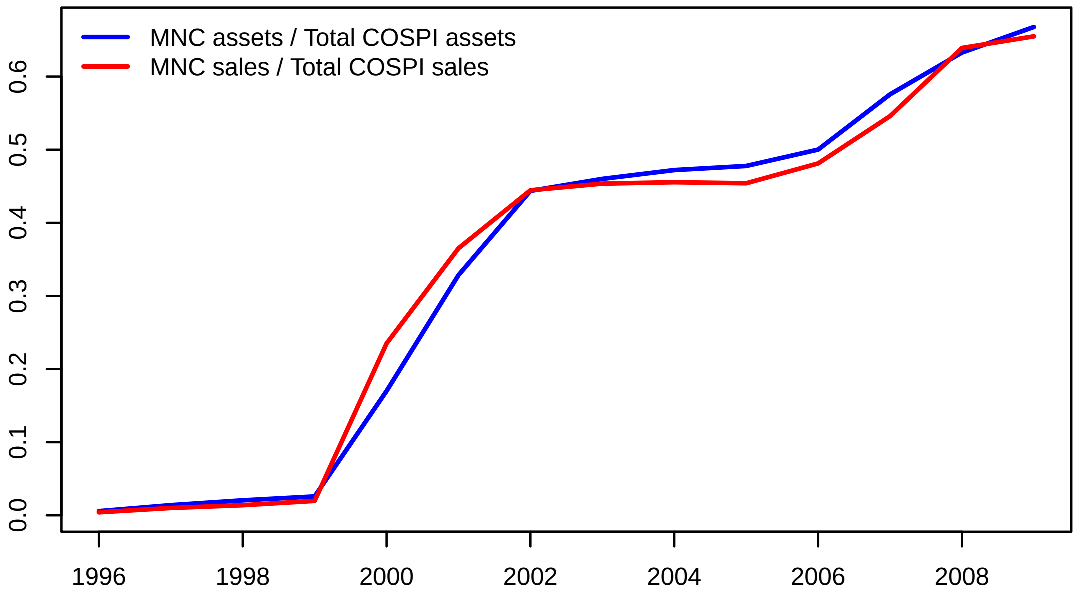
Not surprisingly, MNCs in India tend to be much larger in size than typical listed companies. As Table III.2 shows, in 2009 amongst the largest quartile of

[3] The Centre for Monitoring the Indian Economy indexes listed firms in India that trade on at least 66% of trading days. These firms constitute the universe of Indian listed firms, for all practical purposes.

Table III.2: Top quartile of Indian firms

	NonMNC	MNC
	Top quartile	
2003	187	128
2004	218	137
2005	270	166
2006	322	217
2007	376	303
2008	409	351
2009	392	347

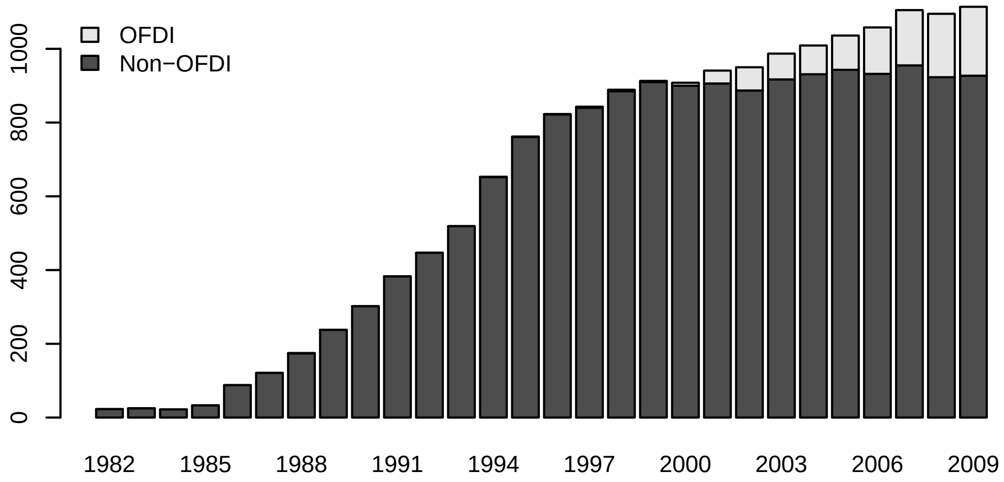
Figure III.1: Sales and assets share of MNCs



Indian firms, there were 347 MNCs and 392 domestic firms. Among these largest firms, the aggregate share of MNCs assets and sales constitute more than half of the aggregate asset and sales of all listed firms in India (Figure III.1).

The rise of manufacturing MNCs started in the mid-1990s, and significant growth

Figure III.2: Rise of manufacturing MNCs



in their number is visible following the initial relaxation of capital controls in 2000. Figure III.2 shows the growing number of these firms by year. For the purposes of this paper, we focus only on the post-liberalization period.

We seek to compare productivity levels and growth for domestic non-OFDI and OFDI firms. Productivity measurement relies on robust production function estimation. Hence, we consider the subset of firms for which positive values for output and inputs are observed. We observe 25,598 firm-years from 1,804 distinct firms over the period 1991 to 2009.

3 Methodology

3.1 Productivity measurement: Levinsohn-Petrin methodology

We now turn to the measurement of firm-level productivity. Consider a basic two-factor production function:

$$Y_{it} = A_{it} L_{it}^{\beta} K_{it}^{\alpha}$$

where $\beta + \alpha = 1$ would imply constant returns to scale. Here Y_{it} is a measure of output like value added, while L_{it} and K_{it} represent the usage of labor and capital, respectively. A_{it} is the total factor productivity (TFP) of the ith firm at period t because it increases all factor's marginal product simultaneously. Transforming the above production function into logarithms allows linear estimation in the following form:

$$y_{it} = \beta l_{it} + \alpha k_{it} + u_{it}$$

The small letters indicate variables in the equation above expressed in logs. The estimated residual $\hat{u}_{it}$ of this equation is the logarithm of firm-specific total factor productivity A_{it}.

However, a simultaneity problem arises when at least a part of the TFP is observed by the firm at a point in time early enough to allow the firm to change its factor input decision. In this case, profit maximization of the firm and its the realization of the error term in the production function is expected to affect the choice of factor inputs. This means that the regressors and the error term are correlated, which makes ordinary least squared estimates biased.

Olley and Pakes (1996) and Levinsohn and Petrin (2003) suggest use of instruments in a non-linear semiparametric estimation technique to deal with this endogeneity problem. While Olley and Pakes use the investment of the firm as an instrument, Levinsohn and Petrin rely on the use of raw materials, since the relationship between investment and output may not be strictly monotonic. We estimate productivity of manufacturing firms using the Levinsohn-Petrin method.

To measure productivity by this method, we first need to obtain real values of the variables to be used. The output measure, sales, is deflated by the wholesale price index (WPI) for manufacturing, since we do not have firm-level prices, and

Table III.3: Summary statistics of manufacturing companies: 1991-2009

	Non-OFDI	OFDI
Sales (INR Billions)	7.49	15.64
Total assets (INR Billions)	6.55	21.12
Gross fixed assets (INR Billions)	3.82	10.37
Exports to sales (Percent)	36.00	33.00
OFDI to total assets (Percent)	0.00	8.00
Productivity (Index)	55.15	55.78

revenue-based TFP is our preferred productivity measure. Wages are deflated by the CPI for industrial workers, and this is used as a proxy for weighted real labor input by the firm. Gross fixed assets are deflated by the national accounts capital stock deflator, and used to measure capital input. Expenses on raw materials are deflated by the WPI for manufacturing and are used to measure real raw material inputs.

3.2 Panel regression

Our aim is to compare the productivity levels and growth rates of exporting versus outbound FDI firms.[4] Hence we consider the sample of all firms who serve foreign customers, whether through exporting, OFDI, or both. We exclude firms who serve the domestic market exclusively. For this sub-sample, we observe 16,148 firm-years from 1,519 distinct firms over the period 1991 to 2009.

We obtain productivity of a firm relative to the frontier productivity in this sector using:

$$\hat{a}_{it} = \hat{u}_{it} - \hat{u}_{it}^{\max}$$

where $\hat{a}_{it}$ denotes productivity of firm i in period t relative to the maximum productivity in this sector over the entire period, $\hat{u}_{it}^{\max}$.

[4] Since we are examing the largest Indian firms, virtually all of them are exporters. Thus we do not compare against purely domestic-oriented firms.

Table III.3 shows summary statistics for these firms. On average, MNCs have bigger values for total sales, total assets and gross fixed assets. The exports to sales ratio measuring export intensity is higher for the exporting firms, as expected, since exporting may substitute for OFDI. In the class of OFDI firms, on average, foreign assets are 8% of total assets. However there is no significant difference in the average relative productivity level across these two categories of firms.

We estimate two models. In one, we explore how relative productivity depends on whether the firm exports or is engaged in OFDI and also whether the growth in relative productivity varies significantly between exporting and OFDI firms. In our second specification, we explore whether growth in relative productivity depends on whether the firm exports or does OFDI.

Other firm-specific characteristics which may affect technical efficiency, drawn from the productivity literature, are age, size, the investment rate, and market power. Age is proxied by the difference between the year in which a firm is observed and the year of incorporation. The investment rate is measured by the ratio of change in gross fixed assets over a year to the stock of fixed assets. Firms with high investment rates are expected to be more efficient. We proxy market power by market share, the ratio of the sales of an individual firm over the sectoral sales by year. Size is potentially associated with productivity. The log of total assets, or the value of the balance sheet, and the log of total sales are used to measure firm size. We also include industry dummies, based on the CMIE classification of industries.

We divide our sample into four quartiles, by relative productivity level. We estimate the above-described models on the top and bottom (1 and 4) separately, and on the middle quartiles (2 and 3) clubbed together.

3.3 Matching

3.3.1 Treating the data

Since the typical domestic or even exporting firm may be quite different than a firm that engages in OFDI, we create a matched control group of firms for the "treated firms" – those that self-selected into becoming MNCs. We first examine the statistics for the matched firms as a whole. Next we estimate the average treatment effect for the treated firms using a difference-in-difference approach to assess the impact of becoming an MNC on the growth rate of productivity.

Indian macroeconomic conditions as well as the policy framework for investing abroad have changed frequently since the mid-nineties. The matching framework needs to take into account the consideration that two firms should be matched if one invested abroad in year t, while the other did not. The unique advantage of Indian data is that because companies became MNCs in recent years, we observe each company both before and after it did so. To the extent that a firm treated in calendar time t_1 is matched against a control observed at calendar time t_2 with $t_1 \neq t_2$, there is a problem that macroeconomic changes in policy, administration and business cycle conditions are not lined up correctly between treatment and control firms. We realign the data to the event time frame. The event under consideration is a firm becoming an MNC. We obtain a matched domestic firm in the year the firm became an MNC.

In matching the firms, we focus on the firms which became MNCs in the years from 2003 to 2007, where we have adequate pre and post-internationalization observations. For each firm-year, a matched control is chosen, subject to the condition that it was always domestic and is an exporting firm. Matching is without replacement: once a firm is used as a control, it is not an eligible control for later years. In event time, the treated firm and its control are identified in an identical set of years. Matching is based on the multivariate distance between firms within each year, using the citeauthormahalanobis1936generalized forumla, to reduce the chance of accepting biased matches (see Caliendo and Kopeinig, 2008).[5] A 0.25 standard deviation in the weights is the tolerance level for the cutoff. Firms were matched based on the following covariates: log(assets), log(sales), and asset tangibility. Asset tangibility is defined as the ratio of gross fixed assets to total assets. We are able to find 133 MNC firms with suitable matching partners.

3.3.2 Match balance

We now show that the matching done by the above method results in good matches. Figure III.3 provides empirical cumulative distribution functions of the treated and control groups before and after matching. The empirical cumulative distribution functions of all the variables should become closer for the treated and control groups after matching, if the matching is good. The graphs show that the matching procedure provides a match that approximates the cumulative distribution of the treatment group among the matched covariates.

3.3.3 Constructing the counterfactual

OFDI firms may engage in outbound foreign direct investment through acquision, joint venture, or greenfield investment (though we cannot distinguish these objectives in the dataset). They then may or may not enjoy growth in productivity. Other firms that do not invest abroad may face a different degree of growth in productivity. We do not directly observe the hypothetical of what would have been the growth in productivity of the OFDI firms had they not invested abroad. We partly address the potential impact of international competition by including in the dataset only firms that are exporters. This means that the going-abroad 'treatment' that we apply is to consider when exporting firms engage in OFDI.

In the microeconometric evaluation literature, this question has been viewed as a missing-data problem. Following Heckman *et al.* (1998), Dehejia and Wahba (2002) and Blundell (2000), we define the average effect of the 'treatment', in this case,

[5] The analysis was also performed using the propensity score matching method, and the results for productivity were comparable.

Figure III.3: Match balance: empirical cumulative distribution functions MNC and non-MNC firms after matching (AM) and before matching (BM)

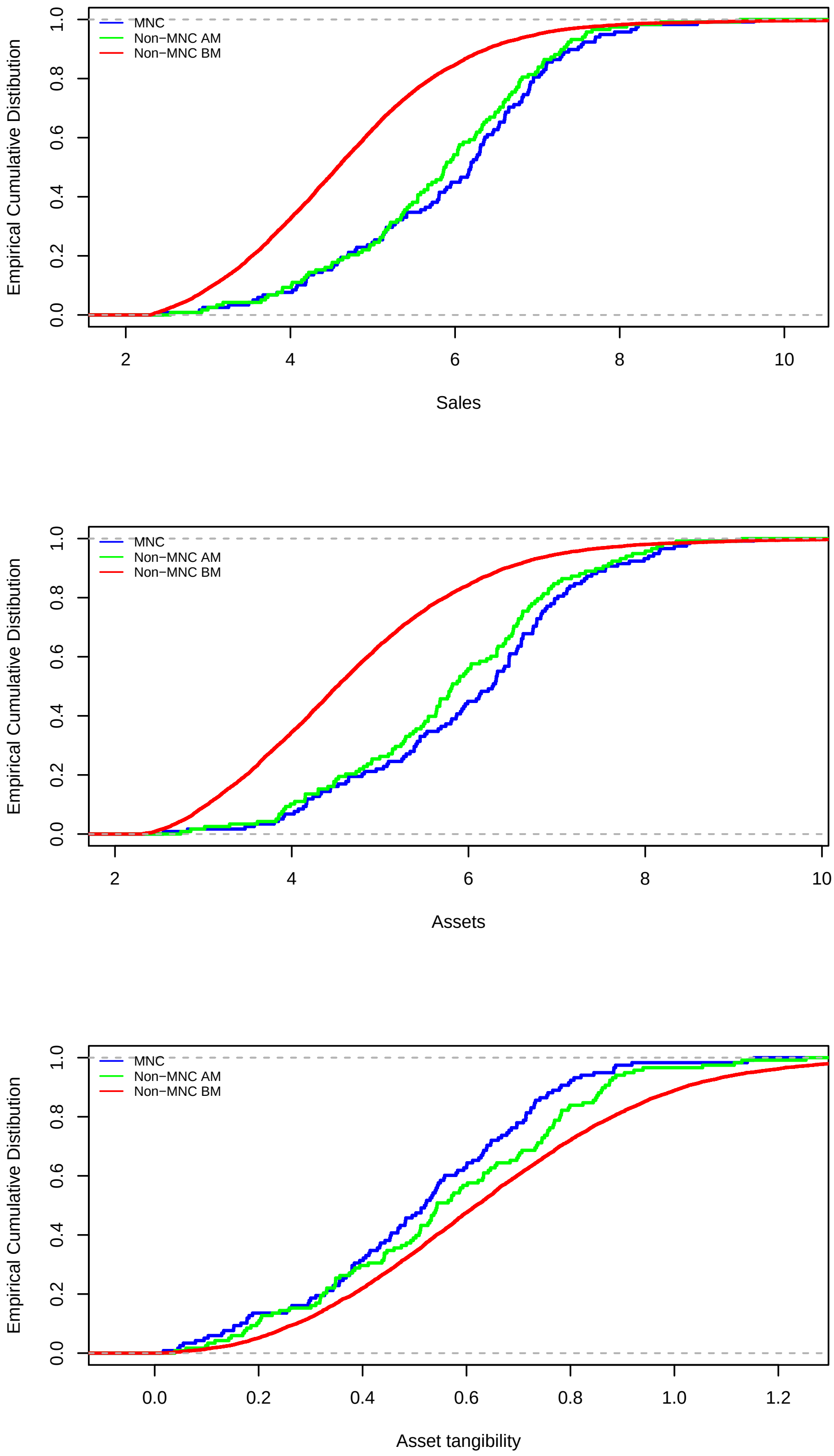


Table III.4: Levinsohn-Petrin productivity estimation results

	Coefficient	Std.Error	p-value
Log(Wages)	0.22	0.01	0.00
Log(Capital)	0.14	0.11	0.21
Log(Raw materials)	0.52	0.14	0.00

investment abroad, on the OFDI firms as the difference between the counterfactual and the observed outcome. The counterfactual is constructed by choosing a set of firms with similar characteristics.

The challenge here is an accurate construction of the counterfactual. This is done through the selection of a well-chosen control group. We have employed matching techniques to do this. The purpose of matching is to pair each firm that invests abroad with one or more that remain domestic based on observable pre-treatment characteristics such as age, size, and wages.

We can then use the difference in year-on-year productivity growth between the two groups, treated and control, to assess the causal impact of investment abroad on this productivity. The limitation of this approach is that it ignores the unobserved time-invariant differences between the firms who self-select themselves into investing abroad and those who do not.

4 Results

4.1 Productivity measures

Table III.4 shows the elasticities of the inputs following the Levinsohn-Petrin productivity estimation. Raw materials have an elasticity of about one-half, reflecting a substantial value-added share, with labor and capital having smaller elasticities. The estimates are well within the norm for firm-level gross output production function estimates, and though the coefficient on capital is not significant, the coefficient has a standard ratio to labor, 1:2. The sum of elasticities is just under one, suggesting near-constant returns to scale.

Table III.5: Productivity level differences for the first quartile

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	54.55	0.02	2807.40	0.00
OFDI dummy	0.15	0.10	1.50	0.13
Age	-0.00	0.00	-3.32	0.00
Size	0.03	0.01	3.82	0.00
Trend	-0.00	0.00	-0.30	0.77
Trend*OFDI dummy	-0.01	0.01	-1.90	0.06
Market share	0.04	0.02	1.98	0.05
Investment rate	0.01	0.02	0.31	0.75
Major industry: Diversified	0.01	0.06	0.23	0.82
Major industry: Food	-0.04	0.03	-1.24	0.21
Major industry: Machinery	-0.04	0.02	-1.68	0.09
Major industry: Metals	-0.03	0.03	-1.05	0.29
Major industry: MiscManuf	-0.01	0.04	-0.16	0.87
Major industry: NonMetalMin	0.05	0.03	1.56	0.12
Major industry: Textiles	-0.07	0.02	-3.32	0.00
Major industry: TransportEq	-0.06	0.03	-2.33	0.02
No. of obeservations	3987			
Adj. R-Squared	0.99			

4.2 Panel regression

4.2.1 Productivity differences in level

The results of the panel regressions show no significant differences in productivity levels between exporting and OFDI firms. This result also holds within each quartile, and although there are some differences in the control variables, market share is positive and highly significant in all specifications. The effect of size is also positive in the first quartile, though not significant for the other quartiles. The results are shown in Tables III.5, III.6 and III.7. The overall results are consistent with the aggregate summary statistics, given in Table III.3.

4.2.2 Productivity differences in growth rates

We find no significant difference in productivity growth between exporting and OFDI firms, in any of the four quarties. However, again, the effect of market share is positive and significant in all specifications. These results are shown in Tables III.8, III.9 and III.10.

Table III.6: Productivity level differences for the fourth quartile

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	57.57	0.15	375.34	0.00
OFDI dummy	0.22	0.32	0.68	0.50
Age	-0.00	0.00	-1.51	0.13
Size	-0.23	0.06	-3.53	0.00
Trend	0.03	0.01	3.32	0.00
Trend*OFDI dummy	-0.02	0.02	-0.89	0.38
Market share	0.03	0.00	6.95	0.00
Investment rate	-0.02	0.01	-2.88	0.00
Major industry: Diversified	0.55	0.19	2.84	0.00
Major industry: Food	1.54	0.33	4.62	0.00
Major industry: Machinery	0.12	0.18	0.67	0.50
Major industry: Metals	0.65	0.24	2.77	0.01
Major industry: MiscManuf	0.27	0.26	1.02	0.31
Major industry: NonMetalMin	1.76	0.40	4.42	0.00
Major industry: Textiles	0.57	0.26	2.17	0.03
Major industry: TransportEq	0.04	0.23	0.17	0.86
No. of observations	3986			
Adj. R-Squared	0.91			

Table III.7: Productivity level differences for quartiles two and three

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	56.30	0.12	454.86	0.00
OFDI dummy	0.04	0.20	0.18	0.86
Age	-0.01	0.00	-2.95	0.00
Size	-0.07	0.04	-1.62	0.11
Trend	0.03	0.01	5.17	0.00
Trend*OFDI dummy	-0.01	0.02	-0.50	0.62
Market share	0.03	0.00	8.28	0.00
Investment rate	-0.02	0.00	-5.34	0.00
Major industry: Diversified	0.84	0.17	4.89	0.00
Major industry: Food	1.62	0.37	4.36	0.00
Major industry: Machinery	0.23	0.15	1.54	0.12
Major industry: Metals	0.60	0.18	3.32	0.00
Major industry: MiscManuf	0.21	0.18	1.17	0.24
Major industry: NonMetalMin	1.75	0.31	5.63	0.00
Major industry: Textiles	0.27	0.17	1.57	0.12
Major industry: TransportEq	-0.32	0.15	-2.08	0.04
No. of observations	7972			
Adj. R-Squared	0.92			

Table III.8: Difference in productivity growth for the first quartile

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	-0.01	0.00	-3.50	0.00
OFDI dummy	-0.00	0.00	-0.86	0.39
Age	-0.00	0.00	-1.58	0.11
Size	-0.00	0.00	-2.34	0.02
Market share	0.01	0.01	2.10	0.04
Investment rate	-0.00	0.00	-1.82	0.07
Major industry: Diversified	-0.00	0.00	-0.53	0.59
Major industry: Food	-0.00	0.00	-1.09	0.27
Major industry: Machinery	0.00	0.00	0.25	0.80
Major industry: Metals	-0.00	0.00	-0.97	0.33
Major industry: MiscManuf	-0.00	0.00	-0.22	0.82
Major industry: NonMetalMin	0.00	0.00	0.88	0.38
Major industry: Textiles	0.00	0.00	1.14	0.25
Major industry: TransportEq	0.00	0.00	1.90	0.06
No. of obeservations	3987			
Adj. R-Squared	0.01			

Table III.9: Difference in productivity growth for the fourth quartile

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	57.71	0.14	398.63	0.00
OFDI dummy	0.04	0.09	0.42	0.67
Age	0.00	0.00	0.20	0.84
Size	-0.21	0.06	-3.29	0.00
Market share	0.03	0.00	7.13	0.00
Investment rate	-0.02	0.01	-2.86	0.00
Major industry: Diversified	0.39	0.18	2.09	0.04
Major industry: Food	1.54	0.33	4.64	0.00
Major industry: Machinery	0.14	0.18	0.74	0.46
Major industry: Metals	0.71	0.23	3.01	0.00
Major industry: MiscManuf	0.26	0.26	0.98	0.33
Major industry: NonMetalMin	1.74	0.40	4.36	0.00
Major industry: Textiles	0.62	0.26	2.38	0.02
Major industry: TransportEq	0.00	0.23	0.01	0.99
No. of obeservations	3986			
Adj. R-Squared	0.03			

Table III.10: Difference in productivity growth for quartiles two and three

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	56.38	0.12	468.95	0.00
OFDI dummy	0.04	0.06	0.66	0.51
Age	0.00	0.00	0.09	0.93
Size	-0.06	0.04	-1.37	0.17
Market share	0.03	0.00	8.50	0.00
Investment rate	-0.02	0.00	-5.28	0.00
Major industry: Diversified	0.71	0.17	4.22	0.00
Major industry: Food	1.59	0.37	4.29	0.00
Major industry: Machinery	0.21	0.15	1.41	0.16
Major industry: Metals	0.65	0.18	3.56	0.00
Major industry: MiscManuf	0.19	0.18	1.04	0.30
Major industry: NonMetalMin	1.74	0.31	5.57	0.00
Major industry: Textiles	0.27	0.17	1.58	0.11
Major industry: TransportEq	-0.37	0.15	-2.49	0.01
No. of observations	7972			
Adj. R-Squared	0.02			

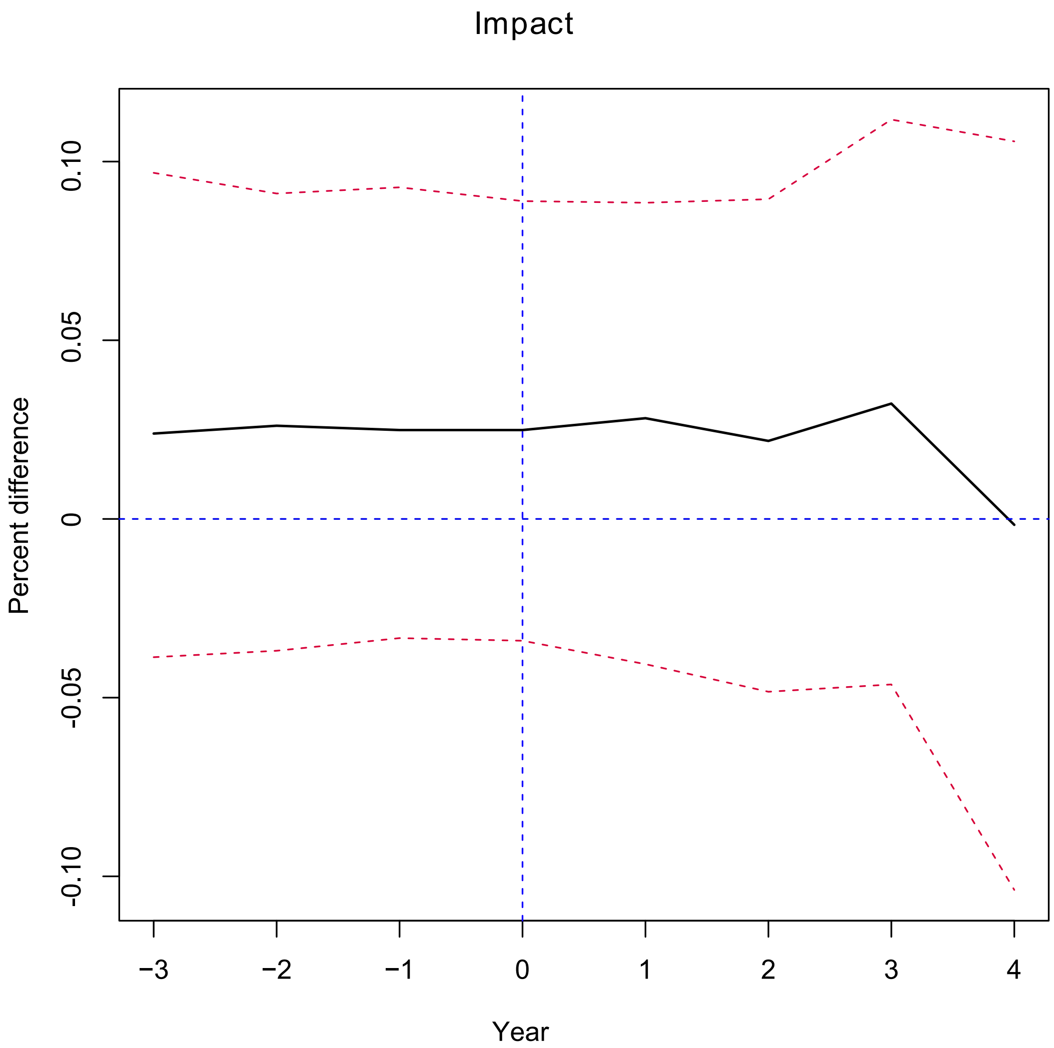
4.3 Average productivity growth

We follow the microeconometric evaluation literature and use a difference-in-differences approach to evaluate the average treatment effect (ATE) on the treated on the firms that invested abroad. This requires longitudinal data, which we have. To measure the ATE, we estimate the counterfactual following Blundell (2000); Girma and Gorg (2007) and using the Match-It and Zelig packages in *R* (Ho *et al.*, 2007; Sekhon, 2007).

Using this approach, we first fit a linear model to the treatment group. We then conduct a simulation procedure in order to impute the counterfactual outcome for the control group using the model parameters of the treated group. These are a proxy for the missing data, that is, what would have been the outcome for the treated group had they not invested abroad. We then compute the difference between observed and the counterfactual: expected values for the treated group. The estimated treatment effect for the control units is the effect of control which is the observed control outcome minus the imputed outcome under treatment from the model.

Figure III.4 shows us the average productivity growth rate of the control group minus the average productivity growth rate of the treated group. The event is defined

Figure III.4: Difference-in-differences: productivity growth



as the year a company invests abroad. We see that even four years after firms invest abroad, there is no significant difference between the average productivity growth rate and that of their matched control firm.

5 Conclusion

This paper examines the impact of outward foreign direct investment on the productivity of Indian firms. The impact of the decision of a firm to invest abroad is examined using matching techniques that result in a treatment and control group of firms. Panel data analysis of the differences in productivity growth and levels finds no significant impact of the internationalization decision of these firms, as compared with the control group of exporting firms that do not invest abroad.

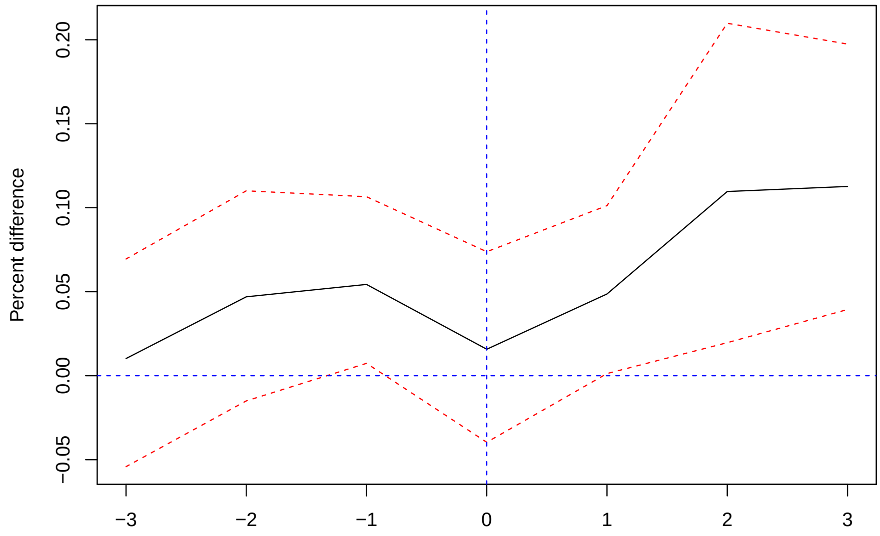
The observation of no productivity gain from investing abroad is an important finding in and of itself, given that India is a developing country, and almost half of aggregate OFDI is targeted towards advanced economies in the form of M&As. In such cases, one would expect that there would be some forms of technology transfer. The lack of productivity gains suggests that the decision to invest abroad by Indian companies is being driven strongly by non-efficiency considerations.[6] Some evidence supports the view that market size and increasing scale is likely to be a key driver (Mold, 2003), and an application of the difference-in-differences method to total sales and wages leads considerable support for this hypothesis (Annex Figure III.5). While the wagebill increases slightly two years before internationalization, it does not increase afterwards, while total sales begin to grow significantly.

The strongly positive coefficient on market share in all regressions also suggests that access to markets may be a driving factor in the results, implying that the productivity differences that we observe may be magnified at a macroeconomic level, through reallocation. In addition, there may be considerable industry-level variation that would be worth further inqury, given the relatively large industry fixed effects.

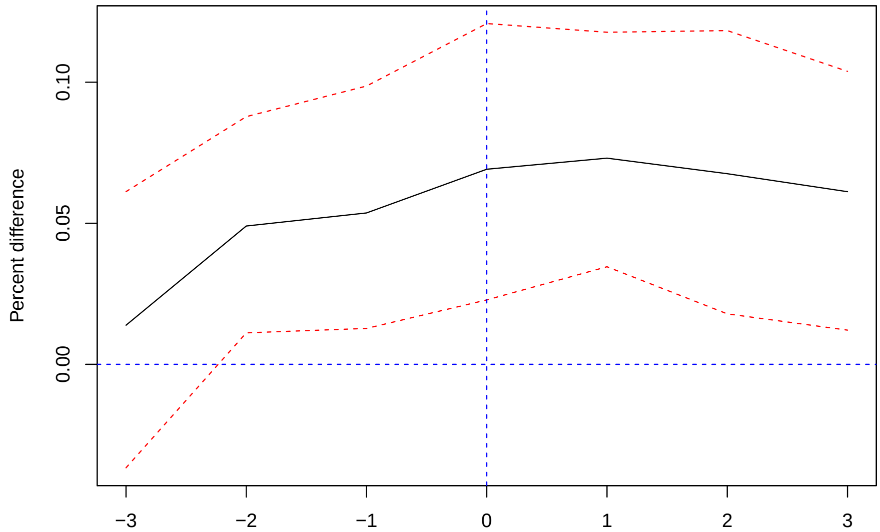
[6] Multinationals in India may well be driven by a range of incentives, as has been observed in case studies of OFDI from other developing countries (Goldstein, 2007).

6 Annex

Figure III.5: Difference-in-differences: alternative outcome indicators



(a) Growth in total sales



(b) Growth in total wages

Chapter IV

Contract enforcement, market access and firm size in Mexico

1 Introduction

Contract enforcement is essential to the efficient functioning of decentralised markets, and legal systems provide necessary institutions to support such enforcement. The quality of the judicary is based not only on *de jure* laws and regulations, but also on their *de facto* implementation, which can often differ considerably from statutes in countries that are still developing and have relatively weak state capacity. In theory, without a high-quality judiciary, transaction costs may be prohibitive, deterring market transactions and firm entry, inhibiting competition and trade. The literature on growth and development has long argued for a fundamental role of "deep" institutions such as the judiciary in determining long-run economic outcomes (Acemoglu *et al.*, 2005), yet identification of the mechanisms at play is often difficult (see OECD, 2012).

Increasing firm scale or size is the main channel through which the most efficient firms can expand their production, by taking on capital and labor as they grow. This up-scaling may be motivated by competition with less efficient firms, who give up market share, particularly when they exit the market. Such dynamics are thought to be a main driver of aggregate productivity growth in open economies (Melitz, 2003; Melitz and Ottaviano, 2008), though there is also evidence of substantial within-firm productivitygains induced by foreign and domestic competition (Harrison *et al.*, 2011; Ben Yahmed and Dougherty, 2012).

This paper examines the link between legal systems and firm size in a developing economy – Mexico – where legal system quality and enforcement varies across states and is also in the process of being substantially reformed. We proceed by extending the study of Laeven and Woodruff (2007), that focused on Mexico in the year 1998, to look at a more recent time period – the five years to 2009 – using new insights from the trade literature to interpret the mechanisms at work. Our study also relates to Kumar *et al.* (2002), who carried out a similar analysis of the effect of court efficiency on average firm size across 15 European jurisdictions, as well as Bürker and Minerva (2012), who looked at (among other things) trial length and firm size across provinces in Italy.

We find that firms in states with higher judicial quality tend to be substantially larger than those in remaining states, and this result is robust to a variety of alternative measures of firm size, as well as to instrumentation for the potential endogeneity of judicial quality. Additionally, we find that firms in more capital intensive industries are more likely to benefit from higher quality judicial systems, consistent with insights from the incomplete contracting literature, suggesting that hold-up problems may be limiting the scaling up of firms.

The paper proceeds by considering the theoretical linkage between firm size and judicial quality in the next section. In the third section, we discuss the data used in the analysis. In the fourth section, the estimation strategy and results are discussed. The fifth section concludes.

2 Firm size and legal systems

The industrial organization and new institutional economics literatures both give support for the idea that average firm size should be positively related to legal system characteritics (see Kumar *et al.*, 2002). We focus on a model that cuts across these literatures, based on Laeven and Woodruff's (2007) adaptation of Lucas's (1978) model of firm size, which views the legal system as reducing the investment risk faced by entrepreneurs who invest an increasing share of their wealth in an enterprise. The model predicts that improvements in the legal system will cause an increase in the demand for capital and labor from all entrepreneurs. This in turn puts upward pressure on wage and rental rates, inducing entrepreneurs with low ability to leave self-employment for wage work in incorporated firms. As a result, average firm size increases.

A related adaptation of the Lucas (1978) model by Quintin (2008) sees the contractual framework as imperfect, with a variable degree of enforcement across jurisdictions, and proxies the quality of the legal system as an exogenous probability that agents will default. They calibrate their model to the firm size distribution in the United States, Mexico and Argentina, and show that differences in enforcement in the model explain a sizable part of the observed differences in economy-wide firm size distributions.

Delving more into the mechanisms that may be mediating the link between the legal system and firm size, the role of capital intensity appears to be critical. The Grossman-Hart-Moore property rights theory of the firm emphasizes the importance of hold-up costs in contracting, which can make capital-intensive and input-dependent investments especially risky. However, the evidence for this idea in the context of legal systems is mixed.[1] Laeven and Woodruff (2007) find no significant effect of increasing capital intensity or decreasing vertical integration, but do find a role for risk diversification through incorporation in affecting the incentives of entrepreneurs, increasing average firm size. Kumar *et al.* (2002) even find some

[1] Evidence from Nunn (2007) and Ma *et al.* (2010) also supports the idea that hold-up costs from a weak judiciary may distort the comparative advantage of some industries through an influence on their input structure, though outcomes are measured in terms of exporting rather than increasing firm size. Such findings may suggest that improving legal institititions allows firms to better specialize by reducing transactions costs (see Williamson, 2005).

evidence in the opposite direction: that firms in more capital intensive industries are less affected by judicial quality, which they speculate to be attributable to physical capital needing less protection than do intangible assets. In contrast, our study re-examines these questions, finding new evidence supporting the idea that hold-up costs may be more substantial in capital intensive manufacturing industries, making the quality of the legal system even more important for these sectors to support higher average firm size.

Limited guidance is available on the shape of the firm size distribution. The model of Guner *et al.* (2008) suggests that the firm size distribution should be more dispersed when there are fewer restrictions on capital use. However, Bürker and Minerva (2012) find that in Italian provinces with shorter civil trials – one narrow measure of judicial quality – the firm size distribution is more compact. We, however, find evidence that the shape of the distribution tends to be more dispersed when firms are located in Mexican states with better judicial quality, using our fairly broad measure. Next we turn to the data at hand.

3 Data

The key data used in this study are economic census microdata for measuring firm size and characteristics, and survey-based data, that measure judicial quality for contract enforcement, along with state-level demographic, distance and gravity-type data that are included as control variables.

3.1 Bin-level economic census data

Disaggregated data from the Mexican economic census were sourced from the *Instituto Nacional de Estadística y Geografía* (INEGI) in Aguacalientes, for the census years 2003 and 2008. The economic census enumerates all fixed establishments in Mexico every five years, and we sourced the information it collects from firms on output, employment, fixed assets, and intermediate inputs. While we gained access to the unit-level microdata, due to the complexity of confidentiality proceedures, we chose to use the data at the firm-size bin level. This data is available for all 31 Mexican states and the *Distrito Federal* (Mexico City) from the level of "sub-sector", or three-digit industry. Within manufacturing, where we focus most of our analysis, there are up to 21 such industries in each state. These industries are then stratified by firm size, in the following size "bins": 0 to 2; 3 to 5; 6 to 10; 11 to 15; 16 to 20; 21 to 30; 31 to 50; 51 to 100; 101 to 250; 251 to 500; 501 to 1000; and over 1000. The bin-level data allow for computation of a weighted-average firm size at the industry level by state.

An employee-weighted firm size, following the approach of Kumar *et al.* (2002) and Laeven and Woodruff (2007), is specified as follows, for state s, industry i and time t:

$$EWFS_{s,i,t} = \sum_{b=1}^{n} \left(\frac{N^{emp}_{b,s,i,t}}{N^{emp}_{s,i,t}} \right) \left(\frac{N^{emp}_{b,s,i,t}}{N^{firm}_{b,s,i,t}} \right) \quad \text{(IV.1)}$$

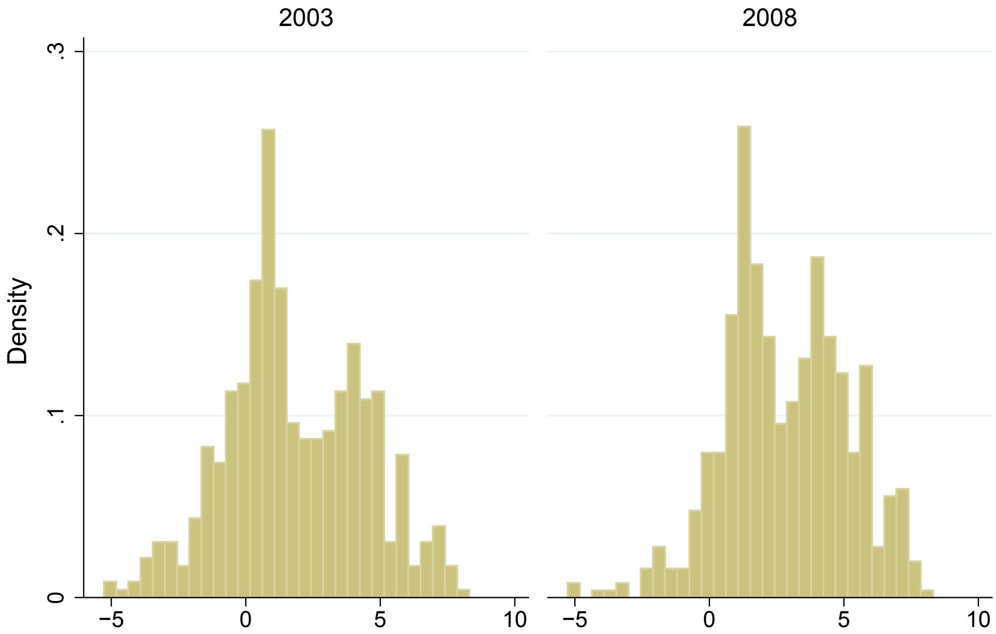
where b is a firm-size bin, and $N^{emp}_{b,s,i,t}$ captures the employment in a single bin, for all bins with more than 3 firms,[2] and up to 12 bins. The formula weights average firm size by the share of employment in each firm size bin. We use the natural log of EWFS, and the distribution of this variable in manufacturing is shown for 2003 and 2008 in Figure IV.1. The distribution is normal according to standard tests, although qualitatively there appears to be some indication that it could be bi-modal.[3]

Equation (IV.1) gives greater weight than a simple average to those bins that contain larger firms. Alternative measures, including a simple average of firm size across bins and the average size of firms in the bin with the median worker, are also computed.

[2] When three or fewer units are available in a firm bin (true for 15% of establishments), the firm count data are suppressed, which we exclude these bins from our firm size analysis.

[3] As a result, when we use $\log(EWFS)$ as a dependent variable below, we also carry out quantile regressions, though there does not appear to be any significant difference across quartiles in the main coefficients of interest.

Figure IV.1: Distribution of log employee-weighted firm size



Source: Calculations from INEGI bin-level economic census data.

3.2 Measures of judicial quality

The Intituto Technológico Autónomo de México (ITAM) and the law firm Gaxiola, Moraila and Associates (GMA) have cooperated since 2001 with Moody's Investors Service to measure the efficiency of state institutions involved in the administration of justice every 2-3 years, for 2001, 2003, 2006, 2008 and 2011 (see Annex, Table IV.7). These studies focus on the adequacy of legal administration, as it relates to the enforceability of commercial contracts and mortgages disputed in state court in Mexico. The sources of information for the measures comprise expert opinion surveys completed by litigation attorneys in each of the federal entities, information supplied by each state tribunal, data collected by the researchers and on-site visits by ITAM in each state.

The four key factors considered in the measures are:

1. *Institutional quality – 50%*: factors within and outside the control of the judicial branch in a state that affect its ability to carry out its functions. These include the perceived quality of tribunals' judges and magistrates, their

expertise in commercial cases, the criteria required for the promotion of judges and the nomination of magistrates and the impartiality of persons in both of these positions.

2. *Duration of cases – 40%*: the average time and backlog involved in processing a typical case related to contract enforcement.

3. *Quantity and efficiency in the use of resources – 10%*: human and physical resources devoted to the judicial branch, including an assessment of their qualilty by legal practitioners.

4. *Enforcement of resolutions – adjustment to the criteria*: an evaluation of the support provided by the executive branch in obtaining final enforcement of verdicts.

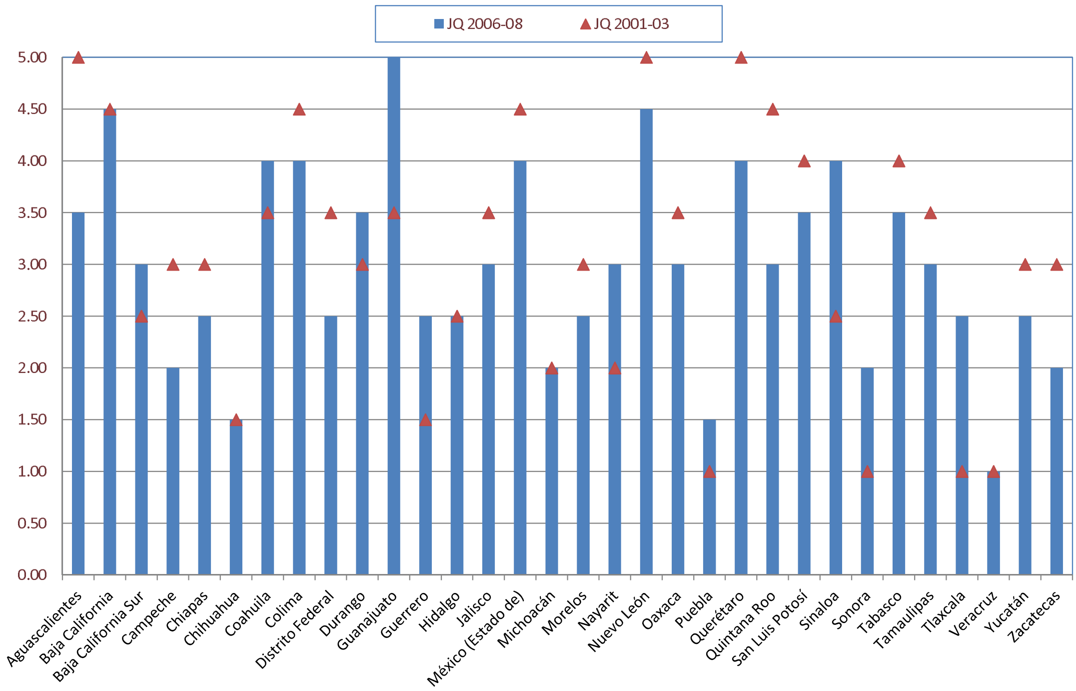
These factors are combined into a five-level score by Moody's's (2011), from which we code judicial quality from worst to best as follows:

- *Lowest quality (EC5)*: scored 1
- *Below-average quality (EC4)*: scored 2
- *Average quality (EC3)*: scored 3
- *Above-average quality (EC2)*: scored 4
- *Highest quality (EC1)*: scored 5

The scores we use are based on an inversion of the digit associated with the Moody's EC rating, and we disregard the "+" that some states receive for being at the top end of a given rating. Using our scoring of the Moody's measure of contract enforcement, a higher score for judicial quality JQ represents "better" state-level institutions.

Comparison across states of the scores for judicial quality in each of the state-level entities, in Figure IV.2, shows considerable heterogeneity for the period immediately preceding 2003 and 2008. The measures for 2006 and 2008 are averaged by state and shown in the columns, and the measures for 2001 and 2003 are averaged and shown with diamonds. There is no clear pattern of evolution of these indicators, though

Figure IV.2: Evolution of judicial quality measure by state
Rated on a scale of lowest (1) to highest (5) quality



Source: Moody's based on ITAM and Gaxiola, Moraila and Associates survey.

there is clear evidence of a deterioration in the state-level scores, with two-thirds of states having a worse score in the period preceding 2008 than in 2003. The reasons for this evolution varies, but is likely related to the pace of legal reform at the state level, which is being explored in a companion paper to this one.

A map of the most recent vintage of the judicial quality scores for 2011, shown in Figure IV.3, reveals few obvious patterns in the spatial distribution of judicial quality, although there is some indication that states closer to the border with the United States may tend to score more highly.

As an alternative measure of legal system quality, we use a standard measure of financial market development – the ratio of private credit to GDP, from the Banco de México. This measure is based on more concrete data than our survey-based measure of judicial quality, and gives a useful additional robustness check to our estimates.

Figure IV.3: Map of judicial quality measure by state in 2011
Rated on a scale of lowest (1) to highest (5) quality

Source: Moody's based on ITAM and Gaxiola, Moraila and Associates surveys.

3.3 Instruments

Judicial quality cannot be considered to be exogenous to economic outcomes such as investment and firm size, so we employ an instrumentation strategy taking inspiration from Acemoglu *et al.* (2005), and also used by Laeven and Woodruff (2007) in their earlier study. The key instruments are indigenous state-level population in 1900, and the number of crops with large economies of scale in 1939. The justification for their use is based, first, on the use of *encomienda* system imported from Europe, that treated indigenous labor as a resource to be used by the ruling elite. Hence, the presence of a larger share of indigenous people could be expected to be associated with a worse institutional environment. The second instrument is based on the presence of substantial production of crops that had sufficiently large economies of scale that they led to substantial distortions in the distribution of land and income. Thus, where there was more cultivation of sugar, coffee, rice and cotton as revealed in the 1940 census, we expect that political institutions and thus legal system should be worse. The correlation between both instruments is low, and together they explain a appreciable share of the state-level variation in judicial quality.

3.4 Geographic controls

Mexico's firm size distribution has been found to be distorted, and skewed towards small firms, especially when compared with the United States (see Hsieh and Klenow, 2012). However, this is in part due to its considerably smaller market size, as measured by either GDP or population. Theoretical work in the trade literature has demonstrated that in a monopolistically competitive model with firm heterogeneity, average firm size is larger and dispersion is higher in larger markets Melitz and Ottaviano (2008). Thus, we control for market size using the log of state population, or alternatively the log of total state GDP, though the later is more likely to be endogenous.

The firm geography literature also makes predictions about export market success and consequently firm size (see Redding and Venables, 2004), and we thus use several different variables to proxy distance to market and foreign market potential. These controls include the following: (*i*) distance to the nearest major point of entry into the United States, from Rios and Romo (2008); (*ii*) the average distance to the closest of one of the 10 largest cities in Mexico, weighted by the inverse of the distance, from the same source; and (*iii*) foreign market potential, as estimated by Escobar Gamboa (2010) using the (Head and Mayer, 2004) method. We also use GDP per capita and murders per capita to proxy level of development and the crime rate, from the INEGI and OECD Regional Database, respectively. Next we turn to the estimations.

4 Estimation strategy and results

The empirical analysis starts with a basic estimation of the firm size equation and its distribution, then a series of additional variables are introduced to examine the robustness of the relationship, and alternative measures are explored, as well as interactions. Finally, a production function is estimated that looks at the efficiency implications.

Summary statistics for the variables used in the main analysis are shown in Table IV.1. They show that across industries, the average employment-weighted firm size is 119, while the simple average is 56. Yet the typical size of a firm (in the median size bin) is only 20. The average industry has slightly over 100 firms in total.

Table IV.1: Summary statistics for pooled 2003 and 2008 data

VARIABLE	Obs.	Mean	Std. Dev.	Min.	Max.
Industry/state-level /a					
Employment-weighted firm size	1,062	119.0	328.1	0.005	4,076
Log employment-weighted firm size	1,062	2.4	2.49	-5.27	8.31
Simple average firm size	1,062	56.3	115.4	1.1	1,952
Typical firm size (median bin)	1,062	19.6	72.5	1.1	1,952
Number of employees	1,062	8,172	13,833	14.0	161,347
Number of firms (establishments)	1,062	108.6	1,517	3.0	19,451
Log capital intensity (K/L) /b	538	1.72	1.65	-4.81	5.39
Log vertical integration (GO/VA) /b	550	1.03	.369	.156	3.79
Log value added /b	550	12.9	2.31	4.76	18.0
Log gross output /b	555	13.9	2.36	5.77	18.9
State-level					
Judicial quality (JQ)	64	3.1	1.11	1	5
Log JQ score	64	1.0	0.44	0	1.61
Market size (log population)	64	14.8	0.76	13.1	16.5
Foreign market potential	32	.097	.246	.014	1.35
Log international distance to market	32	6.2	1.90	0	7.74
Log domestic distance to market	32	7.1	1.4	4.1	9.6
Log private credit as a share of GDP	64	-1.9	0.9	-4.6	0.10
Log real GDP in millions of pesos	64	19.0	0.84	17.5	21.1
Log GDP per capita	64	8.8	0.48	8.1	10.8
Log indigenous share in 1900	32	0.1	0.19	0	0.69
Number of large-scale crops in 1939	32	1.7	1.2	0	4

/a For industry and state pairs where firm size data is available. **/b** 2008 only.

4.1 Basic estimates

Following Laeven and Woodruff (2007), the default estimation equation is:

$$firm\_size_{s,i,t} = \alpha_i + \beta B_{s,t} + \gamma \Gamma_{s,i,t} + \epsilon_{s,i,t} \qquad \text{(IV.2)}$$

where α_i is an industry fixed effect, $B_{s,t}$ is a vector of state-level variables, $\Gamma_{s,i,t}$ is a vector of variables that vary by state and industry, when applicable; $\epsilon_{s,i,t}$ is the error term.

In the first set of regressions, shown in Table IV.2 (columns 1 to 3), we set $firm\_size$ as $\log(EWFS)$, and regress it on judicial quality and market size, using the pooled 2003 and 2008 data, with industry fixed effects. Estimates of the equation are shown using both ordinary least squares (OLS) and two-stage least squares (2SLS) instrumental variables methods. All estimates show a significant positive coefficient on judicial quality (JQ), supporting our hypothesis that states with better legal institutions should have larger firms on average. Market size using state population is also found to be significant, with systematically high t-statistics.

The OLS estimates of JQ appear to be highly biased downwards, as the 2SLS estimates show much higher coefficients on JQ, which is well-identified using the standard Hansen overidentification test once both instruments are included. The first stage equation shows the expected signs on both instruments, though only the indigenous population share is significant.

The second set of regressions in Table IV.2 (columns 4 and 5) include our preferred gravity variable, the log of the distance to the nearest point of entry to the United States, which we call distance to international markets, since the US border is the departure point for most of Mexico's exports. This variable is also highly significant (and negative), along with state market size (positive).

Our preferred specification is the final 2SLS estimate (column 5), which shows a coefficient of 2.9 on judicial quality, which represents a median increase of 24%

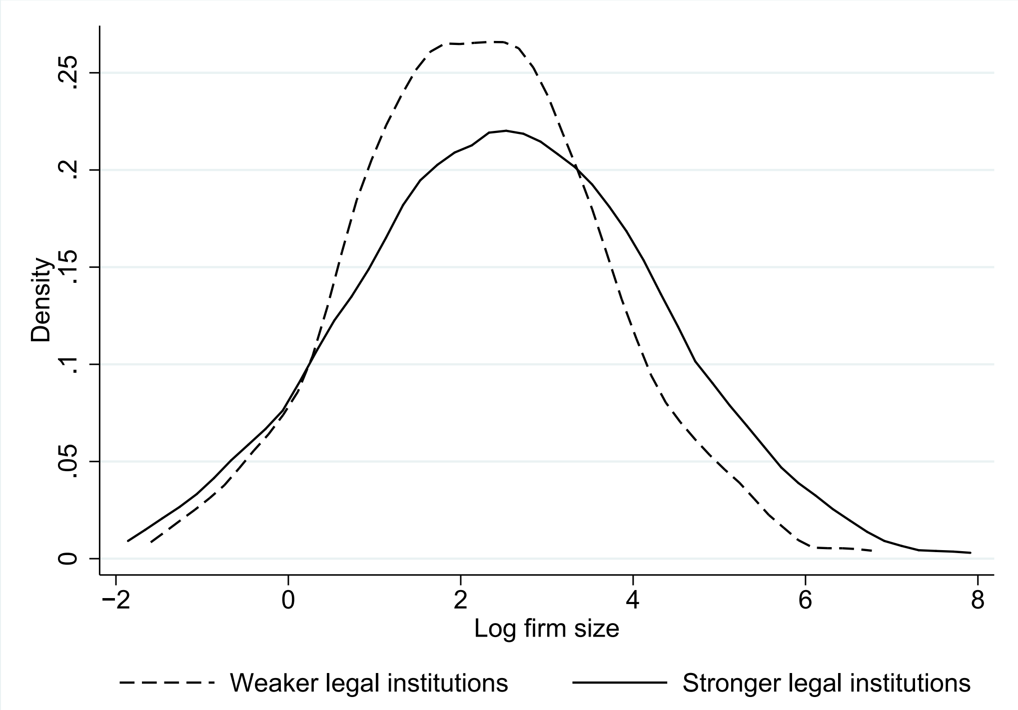
Table IV.2: Baseline estimates of the effect of judicial quality

	Dependent variable: weighted average firm size				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS-1	IV-1a	IV-1b	OLS-2	IV-2
Judicial quality (JQ)	0.698**	4.223*	3.641***	0.740***	2.907***
	(0.306)	(2.098)	(0.623)	(0.227)	(0.507)
Market size	1.321***	1.533***	1.496***	1.273***	1.378***
	(0.124)	(0.185)	(0.080)	(0.144)	(0.107)
Distance to int'l markets				-0.209***	-0.244***
				(0.074)	(0.045)
Constant	-16.651***	-23.402***	-22.257***	-14.658***	-18.220***
	(1.771)	(4.471)	(1.788)	(2.246)	(1.628)
Observations	1,062	1,062	1,062	1,062	1,062
R-squared	0.315	0.320	0.359	0.340	0.367
First Stage: JQ (2008 estimates shown)					
Indigenous		-0.445	-0.509*		-0.637**
		(0.307)	(0.285)		(0.305)
Crops			-0.121**		-0.129**
			(0.054)		(0.054)
Market size		-0.067	-0.011		0.006
		(0.100)	(0.080)		(0.077)
Distance to int'l markets					0.041
					(0.044)
Constant		2.077	1.473		0.999
		(1.458)	(1.187)		(1.223)
Observations (States)		32	32		32
Hansen overidentification test (p-value)			0.711		0.188
Partial R-squared		0.050	0.159		0.185
Instrumented	No	Yes	Yes	No	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1

in terms of weighted average firm size for each one-step increase in judicial quality, or a near-doubling of average firm size if the legal system in the typical state were to improve from the worst to best practice in judicial quality. An illustration of the impact of such a dramatic reform on the predicted firm size distribution is shown in Figure IV.4, plotted on a log scale. This plot shows that the shape of the distribution also shifts with the increase in judicial quality, becoming more dispersed, as hypothesized.[4]

Figure IV.4: Predicted employee-weighted firm size distribution
Density function conditional on presence in best or worst-performing state



Source: Calculations using equation estimates the "preferred" estimate in Table IV.2, column 5.

4.2 Robustness tests

Several sets of robustness checks are carried out: (*i*) to account for the possiblity of additional geographic and gravity-type effects; (*ii*) with alternative measures of firm size and institutions; (*iii*) using various specification with respect to time.

The first set of these regressions are shown in Table IV.3, Panel A, compared with the preferred 2SLS specification above. Using an additional distance variable, for distance to domestic markets (column 2), does reduce the size of the estimated (2SLS) coefficient, and strangely the effect of judicial quality disappears when

[4] A quantile regression was also run using the preferred equation specification, and no significant difference was found in the estimated JQ coefficient for firms at the first and third quartiles, as compared with the median firm. However, the estimated coefficients were slightly larger for firms in the lower half of the distribution, which is consistent the the idea that they face greater effective barriers to up-scaling.

market size is removed from the equation (column 3). This would appear to be a result of multicolinearity, as these two variables are highly correlated (see Rios and Romo, 2008), and partly for this reason, foreign market potential is often preferred (Benassy-Quere *et al.*, 2005). Thus, we also estimate the equation using Escobar Gamboa's (2010) estimate of foreign market potential (FMP) in 2002 for each Mexican state (column 4), and while FMP is not significant, judicial quality remains strongly significant. The effect of JQ remains significant, though the size of the coefficient is diminished, when total GDP (in the year of observation) is used in place of total population (column 5).

Estimates using alternative measures of firm size and institutional quality are shown in Table IV.3, Panel B. The estimates using a simple average firm size (column 6) and typical firm size (column 7) still support significantly positive effects of judicial quality, and the R-squared for these regressions is even higher than for our preferred employment-weighted firm size.

Alternative estimates of the quality of judicial-related institutions show that the effects we are observing are unlikely to be spurious. Private credit as a share of GDP can also be thought of as a proxy measure of the effectiveness of contract enforcement (see Laeven and Woodruff, 2007). When we replace JQ with this variable (column 8), its effect is significant and positive on weighted firm size.

Controlling for the overall level of development using GDP per capita (column 9) only reduces the size of the estimated coefficient on JQ, but it remains large and significant. Similarly, adding the crime rate (column 10) – measured as the annual number of murders per capita – does not substantially diminish the impact of JQ on firm size.

So far the estimates have been based on pooled estimates over 2003 and 2008; while this is an improvement over the earlier results by Laeven and Woodruff (2007) who examined only a single year (1998), in order to more fully take advantage of

Table IV.3: Robustness checks of preferred specification

Panel A

VARIABLES	(1) Preferred specif-ication	(2) with domestic distance	(3) without market size	(4) with market potential	(5) using GDP for market size
Judicial quality	2.907***	1.729***	-0.131	3.563***	1.386**
	(0.507)	(0.629)	(0.518)	(0.626)	(0.539)
Market size	1.378***	0.897***		1.495***	
	(0.107)	(0.229)		(0.105)	
Distance to int'l markets	-0.244***	-0.210***	-0.168***		-0.178***
	(0.045)	(0.032)	(0.039)		(0.058)
Distance to domestic markets		-0.349**	-0.797***		
		(0.149)	(0.085)		
Foreign market potential				0.259	
				(0.293)	
GDP size					1.218***
					(0.152)
Constant	-18.220***	-7.612	10.492***	-22.184***	-19.864***
	(1.628)	(4.834)	(0.914)	(1.666)	(3.095)
Observations	1,062	1,062	1,062	1,062	1,062
R-squared	0.367	0.383	0.363	0.365	0.369
Instrumented	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes

Panel B

VARIABLES	(6) Average firm size	(7) Typical firm size	(8) with private credit	(9) with GDP per capita	(10) with crime rate
Judicial quality	2.021***	1.222***	2.003***	2.152***	2.658***
	(0.372)	(0.235)	(0.582)	(0.600)	(0.457)
Private credit			0.517***		
			(0.178)		
Market size	0.918***	0.557***	1.140***	1.421***	1.287***
	(0.074)	(0.052)	(0.141)	(0.135)	(0.116)
Distance to int'l markets	-0.202***	-0.142***	-0.221***	-0.202***	-0.257***
	(0.029)	(0.020)	(0.041)	(0.052)	(0.051)
GDP per capita				0.706**	
				(0.309)	
Murders per capita					-0.638***
					(0.224)
Constant	-10.485***	-5.772***	-12.909***	-24.584***	-15.200***
	(1.162)	(0.792)	(2.465)	(3.426)	(1.928)
Observations	1,062	1,062	1,062	1,062	1,062
R-squared	0.498	0.490	0.391	0.381	0.370
Instrumented	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered by state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

the time dimension of the data, state-year dummies are introduced, and panel data models are estimated. The results, shown in Table IV.4 (columns 1–3), show that using either random effects (column 1) or fixed effects (column 2), the a strong positive relationship remains between judicial quality and firm size, though the size of the effect is lower when using fixed effects, or when the relationship is estimated in differences (column 3).[5]

Table IV.4: Specifications taking into account time and sample selection

VARIABLES	(1) Levels using random effects	(2) Levels using fixed effects	(3) estimated using first differences	(4) Levels, with all industries /b	(5) Domestic single-plant firms /b
Judicial quality	1.619***	0.677**		1.761*	1.523*
	(0.263)	(0.286)		(0.899)	(0.788)
Change in judicial quality			0.256*		
			(0.141)		
Market size	1.304***	15.455***	-0.455***	0.783***	0.671***
	(0.106)	(1.329)	(0.131)	(0.189)	(0.180)
Distance to int'l markets	-0.236***			-0.114*	-0.039
	(0.042)			(0.068)	(0.068)
Murders per capita	-0.500***	-0.082			
	(0.176)	(0.360)			
Constant	-16.082***		6.051***	-10.744***	0.000
	(1.800)		(2.014)	(3.392)	(0.000)
Observations	1,062	998	407	1,822	1,062
R-squared (overall)	0.328	0.216	0.135	0.431	0.369
Number of groups	563	499			
Instrumented	Yes	Yes /c	No	Yes	No
State-year dummies	Yes	No	No	No	No
Industry controls	Yes	Yes	Yes	Yes	Yes

/a Robust standard errors in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1
/b 2008 only **/c** Sargan overidentification statistic: 97.4 (p<0.001)

In all of the estimates so far, only manufacturing industries have been included. This has been to ensure that the firms included were roughly comparable, since

[5] While the estimates in columns 1-3 use employment-weighted firm size as the dependent variable, these results are robust to the use of average firm size and typical firm size as the dependent variable. (Refer to the Annex, Table IV.8.) While the coefficients are smaller – similar to the results from the pooled specification (Table IV.3, columns 6 and 7) – they are statistically significant.

they are primarily goods-producing sectors. If we include all services, the sample size is about four time larger; however, these firms are not as comparable given the extreme heterogeneity of their activities with respect to "optimal" firm size (since this includes many sole-proprietorship dominated service sectors). Nevertheless, if we carry out an estimate using all industries (Table IV.4, column 4), the coefficient on JQ remains significant and positive. However, distance to international markets is no longer significant, presumably indicating the fact some of these sectors do not engage extensively in international trade.

A final robustness check examines whether there may be selection biases through the channel of multi-plant and foreign-invested firms, who could potentially select the states with better-quality judicial systems as the home of their investments. Moreover, foreign companies may not necessarily be subject to weaknesses of state courts, since some of them rely on arbitration to handle certain types of contractual disputes. At the same time, we also limit the sample to single-plant firms, since there is then less possibility for a firm with plants in multiple states to arbitrage across different state courts. Reassuringly, the results of these estimates (Table IV.4, column 5) re-confirm the findings we found using the full sample.[6]

4.3 Interactions

Next we explore the mechanisms that may explain the effects that we have been observing, using the ratio of fixed assets to employment to measure capital intensity, and the ratio of gross output to value added to measure the degree of intermediate input use, or decreasing vertical integration. Extending the estimation equation to include interactions terms with judicial quality gives us:

$$firm\_size_{s,i} = \alpha_i + \beta B_s + \gamma \Gamma_{s,i} + \xi X_{s,i} \Omega_s + \epsilon_{s,i} \tag{IV.3}$$

[6] Estimates of all the key equations presented in the paper were made using this more limited dataset, and with it, all coefficients on JQ still remained significant, and of comparable magnitudes. Nevertheless, we choose to present the results with the broader dataset since while only 5% of firms are lost from the sample, these firms cover (just) over 50% of employment.

where the variables are as in equation (IV.3), except an extra term $X_{s,i}\Omega_s$ is included, where $X_{s,i}$ is the log of the ratio of fixed assets to employment for each state s and industry i, or alternatively, the ratio of gross output to value added, and Ω_s is judicial quality in state s, that may also be included in the vector B_s. We estimate this equation using only 2008 data, due to data availability.

Table IV.5: Interactions with capital intensity and vertical integration
2008 data only

VARIABLES	(1) Preferred 2008 eqn.	(2) Capital intensity	(3) only interaction	(4) interaction (IV)	(5) Vertical integration
Judicial quality (JQ)	0.951***	0.282			0.853**
	(0.287)	(0.290)			(0.380)
JQ X Capital intensity		0.285***	0.317***	0.725***	
		(0.049)	(0.051)	(0.159)	
JQ X Vertical integration					0.085
					(0.271)
Market size	1.118***	1.039***	1.017***	0.859***	1.089***
	(0.146)	(0.136)	(0.140)	(0.146)	(0.146)
Distance to int'l markets	-0.249***	-0.253***	-0.250***	-0.245***	-0.276***
	(0.084)	(0.078)	(0.073)	(0.053)	(0.074)
Constant	-10.219***	-14.155***	-13.614***	-6.930***	-13.935***
	(2.591)	(2.392)	(2.456)	(2.116)	(2.567)
Observations	555	538	538	555	550
R-squared	0.390	0.426	0.424	0.417	0.394
Instrumented	No	No	No	Yes	No
Industry controls	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1

The estimates including capital intensity imply that the capital intensity of a firms' activities may be a dominant mechanism determining the effect of judicial quality on firm size. For comparisons in Table IV.5, the preferred OLS equation is estimated using only 2008 data (column 1). It is probably a lower-bound estimate, due to OLS bias. In the estimate when JQ and its interaction with capital intensity are both included (column 2), only the interaction term is significant. Moreover, dropping JQ from the equation only increases the estimated effect of the interaction (column 3), that is considerably larger when using instrumental variables estimates (column 4).

Effects of decreasing vertical integration (or heightened input use) in affecting firm size thorugh judicial quality are not observed in our estimates (column 5), when the direct effect of JQ is included. However, there is some weak indication that vertical integration may have an indirect effect on firm size through judical quality.[7] We plan to examine the integration channel further in future work.

4.4 Efficiency effects

At the outset, we argued that removing barriers to firm growth was a "good" in that it facilitates the expansion of more efficient firms, promoting entrepreneurship and productivity. While this link is well-known, we have adequate data in 2008 to perform a straightforward test, using production function estimates at the industry level. Thus, a translog gross output (Y) production function of the following form is estimated on manufacturing firms only:

$$\begin{aligned}\log(Y) &= (\alpha + \gamma\Omega) + \beta_K \log(K) + \beta_L \log(L) + \beta_M \log(M) + \\ &\quad \beta_{KL} \log(K)\log(L) + \beta_{KM} \log(K)\log(M) + \beta_{LM} \log(L)\log(M) + \\ &\quad \beta_{KK} \log(K)^2 + \beta_{LL} \log(L)^2 + \beta_{MM} \log(M)^2 + \epsilon\end{aligned} \tag{IV.4}$$

where the β coefficients are the elasticities on the production factors: capital K, measured as fixed assets; [8] labor L, measured as employment; and intermediate materials M, approximated by the difference between gross output and value added. The constant α is the average level of TFP and γ is the effect of state-level judicial quality Ω on TFP, which we instrument with indigenous population in 1900 and

[7] The coefficient on the interaction is only significant when included without JQ. We are not able to identify this channel in the current dataset due to high co-linearity of vertical integration with judicial quality. If decreasing vertical integration (or heightened input use) was indeed an important mechanism for explaining the effect of judicial quality on firm size, this would be consistent with both the hold-up explanation highlighted above, as well as the transaction cost theory of the firm.

[8] Gross fixed capital formation is presently being used to proxy fixed assets.

large scale crops in 1939, as above. The error term ϵ captures the residual TFP. We similarly estimate a translog value added production function, replacing Y with value added, with intermediate materials M in the equation set to unity, so that only capital and labor inputs remain.

Table IV.6: Production function estimates with judicial quality
2008 data only

VARIABLES	(1) Value added production function	(2) Gross output production function
Judicial quality	0.727***	0.226**
	(0.200)	(0.107)
$\log(K)$	0.062	0.218***
	(0.078)	(0.083)
$\log(L)$	1.383***	0.415***
	(0.118)	(0.094)
$\log(K)\times\log(L)$	-0.019	0.004
	(0.020)	(0.013)
$\log(M)$		0.523***
		(0.113)
$\log(K)\times\log(M)$		-0.045***
		(0.014)
$\log(L)\times\log(M)$		-0.027
		(0.020)
$\log(K)^2$	0.030***	0.021***
	(0.007)	(0.006)
$\log(L)^2$	-0.039**	0.006
	(0.017)	(0.013)
$\log(M)^2$		0.035***
		(0.010)
Constant	1.633***	0.941**
	(0.421)	(0.385)
Observations	594	605
R-squared	0.896	0.975

/a Instrumental variable estimates.
/b Robust standard errors shown in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

The results, shown in Table IV.6, reveal a strongly positive effect on TFP of differences in state-level judicial quality. The estimated size of the effects of judicial quality in value added (column 1) and gross output (column 2) specifications – of about 2% of GDP per step of the judicial quality score – are roughly equivalent since the ratio between the coefficients is similar to the average ratio (in the dataset) of gross output to value added of three (3.12). The gross output specification is usually preferred in industry-level data, and it seems to be more robust here (column 2), both in terms of its R-squared as well as its significant capital coefficients, that are of a similar magnitude to what is commonly found in emerging country microdata.

5 Conclusion

This chapter has found a robust relationship between the increasing quality of the legal system and higher average firm size in Mexico, strongly supporting versions of the Lucas (1978) model of firm size that incorporate contractual uncertainty in investment decisions. These effects are estimated using bin-level census data and state-level measures of judicial quality observed over a period of five years. The findings are strengthened with the inclusion of geographic controls, historical instruments and the use of alternative measures of firm size and judicial quality. Moreover, evidence is found that firms in capital-intensive industries are affected the most by lower judicial quality. This is consistent with hold-up problems in contract enforcement limiting investment in states and regions that lack an adequate quality legal system.

The size of the estimated effects on firm size and efficiency are substantial. Moving from worst to best-practice judicial quality is estimated to be able to nearly double average weighted firm size, widen the dispersion of its distribution, and increase the weakest states' GDP by as much as 8% through higher TFP.

In future work, we would also like to be able to decompose judicial quality further, and identify in more detail the effects of specific channels and ongoing reforms. In particular, it would be useful to understand how recent constitutional amendments that have promted some of the changes in state judicial quality measured in our indicators, even if there are long lags.

6 Annex

Table IV.7: Indicators of judicial quality
for contract enforcement

	Ejecutabilidad Contractual ("EC")				
FEDERAL ENTITY	2001	2003	2006	2008	2011
Aguascalientes	EC1	EC1	EC2+	EC3	EC2
Baja California	EC2+	EC1	EC2+	EC1	EC1
Baja California Sur	EC3	EC4+	EC4	EC2+	EC3
Campeche	EC4+	EC2	EC3	EC5	EC2
Chiapas	EC3	EC3	EC3+	EC4+	EC4
Chihuahua	EC5	EC4+	EC4	EC5	EC4
Coahuila	EC2	EC3+	EC2+	EC2	EC2
Colima	EC2+	EC1	EC2+	EC2	EC2
Distrito Federal	EC3+	EC2+	EC3+	EC4+	EC2
Durango	EC4+	EC2+	EC2+	EC3+	EC2
Guanajuato	EC3+	EC2	EC1	EC1	EC2
Guerrero	EC5	EC4+	EC3	EC4+	EC3
Hidalgo	EC4+	EC3	EC4	EC3+	EC3
Jalisco	EC2	EC3	EC3	EC3+	EC3
México (Estado de)	EC2	EC1	EC1	EC3+	EC2
Michoacán	EC5	EC3+	EC4+	EC4+	EC3
Morelos	EC4+	EC2	EC4	EC3+	EC3
Nayarit	EC4+	EC4	EC2	EC4	EC1
Nuevo León	EC1	EC1	EC1	EC2+	EC3
Oaxaca	EC3	EC2	EC3	EC3+	EC4
Puebla	EC5	EC5	EC4	EC5	EC3
Querétaro	EC1	EC1	EC1	EC3	EC4
Quintana Roo	EC1	EC2	EC4+	EC2	EC5
San Luis Potosí	EC2	EC2	EC3	EC2+	EC3
Sinaloa	EC3	EC4+	EC3+	EC1	EC2
Sonora	EC5	EC5	EC4+	EC4+	EC3
Tabasco	EC2	EC2	EC3+	EC2	EC3
Tamaulipas	EC2+	EC3	EC2+	EC4	EC2
Tlaxcala	EC5	EC5	EC4	EC3	EC5
Veracruz	EC5	EC5	EC5	EC5	EC4
Yucatán	EC4+	EC2	EC3	EC4	EC5
Zacatecas	EC4	EC2+	EC4	EC4	EC5

Source: Moody's (2011) and its surveys with ITAM and GMA.

Table IV.8: Alternative specifications taking into account time

Panel A
Dependent variable: average firm size

VARIABLES	(1) Levels using random effects	(2) Levels using fixed effects	(3) estimated using first differences
Judicial quality	1.102***	0.401***	
	(0.147)	(0.142)	
Change in judicial quality			0.159**
			(0.075)
Market size	0.907***	7.829***	-0.183**
	(0.061)	(0.666)	(0.075)
Distance to international markets	-0.186***		
	(0.024)		
Constant	-10.690***		2.287*
	(0.969)		(1.201)
Observations	1,062	998	407
Number of groups	563	499	
R-squared (overall)	0.436	0.222	0.122
Instrumented	Yes	Yes /b	No
State-year dummies	Yes	No	No
Industry controls	Yes	Yes	Yes

Panel B
Dependent variable: typical firm size

VARIABLES	(4) Levels using random effects	(5) Levels using fixed effects	(6) estimated using first differences
Judicial quality	0.680***	0.222**	
	(0.101)	(0.098)	
Change in judicial quality			0.085*
			(0.046)
Market size	0.548***	4.742***	-0.180***
	(0.042)	(0.462)	(0.055)
Distance to international markets	-0.132***		
	(0.017)		
Constant	-5.963***		2.084**
	(0.666)		(0.941)
Observations	1,062	998	407
Number of groups	563	499	
R-squared (overall)	0.440	0.178	0.200
Instrumented	Yes	Yes /c	No
State-year dummies	Yes	No	No
Industry controls	Yes	Yes	Yes

/a Robust SE in parenthesis, clustered by state. *** p<0.01, ** p<0.05, * p<0.1
/b Sargan stat = 121.593 (p<0.001) **/c** Sargan stat: 137.995 (p<0.001)

Data Appendix

This appendix summarizes the main data sources for each chapter, their coverage, and any restrictions on their accessibility.

1 Chapter I

- *Firm-level input and output data*: Bureau van Dijk's (BvD) Amadeus dataset is used. The extract covers 1995-2005 for key European countries, with balance sheet variables for revenue, value added, wages, capital stock, material inputs and principal industry of production. Levinsohn-Petrin productivity is estimated from the data, using material inputs. Some restrictions exist on accessing the original dataset, as the OECD's licensing arrangements with BvD have changed over time.

- *Firm size distribution and production data*: OECD's Structural Business Statistics database is used for the year 2000 for the benchmark firm size distribution, and also annually to measure total industry-level production.

- *Industry-level trade data*: UN Commodity Trade Statistics (Comtrade) trade data are used to source the value of imports and exports, following an OECD mapping of HS to ISIC four-digit industries.

- *Product market regulation data*: OECD's Product Market Regulation database is used to obtain economy-wide and subsidiary regulatory settings for 1998, 2003 and 2008 (in the most recent update), and also for the industry-level time series of 'regimpact' upstream market regulation indicators.

2 Chapter II

- *Plant-level input and output data*: the Indian Annual Survey of Industries (ASI) longitudinal dataset of all industrial plants (above micro-enterprises) is used. This dataset covers fiscal years from 1998/99 to 2007/08 for gross output, value added, fixed capital, investment, materials, fuel, labour, labour expenditures, and the industry, ownership and location of principal activity. Olley-Pakes productivity (using investment) and the average volatility of firms' growth are derived from the data. Access is available to the original dataset under license.

- *Price deflators*: MOSPI's wholesale price indexes (WPIs) by industry and type of input are used (see Dougherty et al., 2011: Annex A).

- *State-level labour regulation*: a compilation of 21 Indian states' reforms carried out from the mid-1990s to the mid–2000s, based on a survey carried out by OECD (2007) that is documented in Dougherty et al. (2009). Alternative indicators from Besley and Burgess (2004) and Gupta et al. (2009) are also used in tests for robustness.

- *Layoff propensity*: industry-level job turnover propensities use the 2004 CPS dataset, sourced from Bassanini et al. (2009).

- *Control variables*: state-level population, telephone availability, installed electric capacity, and paved road length are sourced from MOSPI, the Indian Ministry of Power, the Ministry of Road Transport and Highways, and the Planning Commission. Indian state-wise product market regulation is sourced from OECD (2007).

3 Chapter III

- *Firm-level input and output data*: the CMIE Prowess database is used, limited to those firms that are in the COSPI index, which ensures a degree of high liquidity and consistent disclosure. Only manufacturing firms are used, covering about 1,000 firms from 1999 to 2009, with sales, wages, gross fixed assets, raw material inputs, year of incorporation, and CMIE industry classifications available in the data. Levinsohn-Petrin productivity estimates are made using information on raw material inputs. Data licensing restrictions limit access to the original dataset.

- *Price deflators*: MOSPI's wholesale price indexes (WPIs) used to deflate output and raw materials, while wages are deflated with the CPI for industrial workers, and gross fixed assets using the national accounts capital stock deflator.

4 Chapter IV

- *Bin-level firm size and production data*: INEGI economic census data is used for the census years 2004 and 2009. The census data are available at the state-industry-size bin level, where size bins are defined as one of a dozen firm size strata, defined by size of employment. Data on total employment, number of production units, gross output, value added, fixed assets and investment are used. Custom extracts from confidential INEGI data were used in some specifications. Access to this dataset at the OECD is ongoing.
- *Judicial quality measure*: the Moody's scoring of the efficiency of state institutions devoted to the administration of justice for contract enforcement are used for the years available: 2001, 2003, 2006, 2008, and the mosts recent vintage, 2011. These data are based on expert and survey-based analysis, and were carried out in cooperation with ITAM and the law firm GMA (see Moody's, 2011).
- *Instruments*: state-level indigenous population in 1900 and number of crops with large economies of scale in 1939 are sourced from Laeven and Woodruff (2007).
- *Distance and market potential*: distance to foreign and domestic markets use the measures by Rios and Romo (2008), while estimates of state-wise foreign

market potential come from Escobar (2010), who uses the Head and Mayer (2004) method.

- *Control variables*: INEGI data are used to proxy market size based on population, as well as to source per capita income and state-level GDP. Banco de México data are used to measure the amount of private credit extended. The OECD Regional Statistics database provide state-level crime rates, based on murders per capita.

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Abstract

Institutions, and their underlying rules, are essential for economic development, in that they provide a framework for markets to operate. However, distinct types of regulatory rules and even institutional settings may have very different effects on outcomes at the aggregate versus business unit levels. This dissertation examines the effect of several types of rules and institutions on firm-level productivity and related measures. The first chapter examines the effect of international competition and domestic competitive barriers on firm-level productivity growth in the OECD. A close interaction is observed between import penetration and domestic barriers to entry, conditional on a firm's distance to the technological frontier. The second chapter examines the effects of labor market reform on plants in different Indian states. A positive effect of labor market reform is found on plant-level productivity growth in labor-intensive and volatile industries. The third chapter looks at Indian exporters who took advantage of capital account liberalization to invest abroad, and explores whether they gained through learning-by-doing. After matching these firms with similar firms that did not invest abroad, the chapter finds that productivity was not boosted, though firms did gain in terms of their overall size through market access. The fourth chapter explores how legal system quality in different Mexican states have impacted the size of firms. States with higher quality legal institutions are found to have larger, more capital intensive and higher-productivity firms.

Area: Economics

Keywords: Development, Trade, Regulation, Productivity, Firms

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Résumé

Les institutions et leurs règles sous-jacentes sont essentielles pour le développement économique car elles fournissent un cadre nécessaire au bon fonctionnement des marchés. Cependant, différents types de règles réglementaires et même différents environnements institutionnels peuvent affecter différemment les entreprises ou les individus. Cette dissertation examine l'effet de plusieurs types de règlementations et de cadres institutionnels sur la productivité des entreprises et sur d'autres mesures de performance. Le premier chapitre analyse l'effet de la concurrence internationale et des règles domestiques définissant la politique de la concurrence sur la croissance de la productivité au niveau de l'entreprise dans les pays de l'OCDE. L'interaction entre la pénétration des importations sur le marché domestique et la réglementation en vigueur, notamment les barrières l'entrée, ont des effets sur la productivité qui dépendent de la distance de l'entreprise à la frontière technologique. Le deuxième chapitre examine les effets d'une réforme du marché du travail sur les usines de différents États indiens. La réforme du marché du travail a un effet positif sur la croissance de la productivité des usines dans les industries intensives en main-d'œuvre et celles où les revenus sont instables. Le troisième chapitre se concentre sur les exportateurs indiens qui ont profité de la libéralisation des comptes de capitaux pour investir à l'étranger et cherche à savoir s'ils ont profité de cette réforme grâce à un "apprentissage par la pratique". La comparaison, au moyen d'un estimateur de matching, des entreprises ayant investi à l'étranger avec des entreprises similaires mais n'ayant pas investi à l'étranger, montre que la productivité n'a pas été stimulée par l'ouverture des comptes de capitaux. En revanche, les entreprises ont considérablement augmenté leur taille du fait d'un accès plus vaste aux marchés. Le quatrième chapitre explore comment la qualité du système juridique dans les différents États du Mexique a impacté la performance des entreprises. Les États pourvus de meilleures institutions juridiques ont des entreprises de plus grande taille, plus intensives en capital et plus productives.

Discipline : Sciences Economiques (05)

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