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ABSTRACT

Regulating Manufacturing FDI: Local Labor Market Responses to a Protectionist Policy in Indonesia*

We analyze the effect of rising protectionism towards foreign direct investment (FDI) on domestic employment, exploiting revisions in Indonesia's highly-granular negative investment list, and spatial variation in the exposure of the manufacturing sector to these investment restrictions. Rising FDI restrictions caused employment gains at the local level, explaining about one-tenth of the aggregate employment increases observed between 2006 and 2016 in Indonesia. These employment gains went along with a reorganization of the local production structure, and new firm entries in the manufacturing sector that are concentrated among micro and small enterprises. While our results are consistent with an increase in the labor-to-capital ratio and reduced productivity among regulated firms (which allowed smaller and less productive firms to enter the market), we also document that at least half of the employment gains are driven by spillover-effects along the local value chain and into the service sector.

JEL Classification: F16, F21, F23, J23, J31, L51

Keywords: FDI regulation, Indonesia, local labor markets

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1 Introduction

After decades of continuous growth, the global volume of foreign direct investment (FDI) has been stagnant over the past decade and even decreased in past years (UNCTAD 2023).[1] While the reasons for such de-globalization dynamics are multifaceted (Razin 2020), they coincided with a general protectionist drive in the United States and beyond (The Economist 2023, Investment Monitor 2021). Starting in 2018, both the US and the European Union strengthened their investment screening regimes with the explicit goal of protecting strategic national interests. Emerging economies followed suit; most notably, China expanded the scope of its national security reviews of FDI in 2020.

As conventional economic wisdom ascribes FDI a host of economic benefits, restricting FDI inflows could come at substantial costs for local firms and workers. The existing literature is fairly conclusive on the positive direct effects of FDI on beneficiary firms and workers.[2] In contrast, much less evidence exists on potential (positive or negative) wider labor-market spillovers. Any trade-offs between national security concerns, employment protection and productivity effects are even more pressing for emerging economies that strongly rely on FDI to access modern technologies.

In this paper, we study the labor-market effects of FDI restrictions in an emerging economy context. Conceptually, these effects could be both positive or negative. On one side, regulation may lead to capital exit, hamper the access to new technologies and make regulated firms less productive, also reducing their labor demand. On the other side, shielding local firms from foreign investors could create jobs if domestic firms substitute technology-intensive capital with local workers, or if reduced productivity (and higher prices) result in the market entry and growth of domestic firms. Employment gains could also arise through spillover effects along the value chain, as domestically-owned firms have been documented to have a higher propensity to source inputs locally rather than internationally.[3] Indirect employment gains could also arise through immigration, and—if employment gains in the manufacturing sector translate into consumption gains—through local multiplier effects.

For our empirical analysis, we explore restrictions to FDI inflows in Indonesia over the time period 2000–2016. Indonesia offers an excellent context for such a study: it has maintained a negative investment list (*daftar investasi negatif*, NIL) already since 2000.

[1] The total volume of global FDI was at 1.3 trillion USD in 2022, which lies substantially below both its value in 2007 before the onset of the global financial crisis (1.9 trillion USD), and below its peak value of 2.1 trillion USD in 2015 (UNCTAD 2023).

[2] FDI has been widely documented to increase firm productivity directly and through spillovers along the value chain (see e.g., Aitken and Harrison 1999, Javorcik 2004, Arnold and Javorcik 2009, Javorcik and Poelhekke 2017, Eppinger and Ma 2019, Genthner 2021, Abebe et al. 2022), and its direct employment effects on target firms are overwhelmingly positive (for a review, see Hale and Xu 2019).

[3] See for Amiti and Konings (2007), Arnold and Javorcik (2009), Genthner and Kis-Katos (2022) recent examples.

The NIL contains five-digit product-specific[4] restrictions regarding FDI inflows.[5] Most importantly for our analysis, Indonesia started to revise its policy towards FDI inflows relatively early by global standards: it substantially expanded the coverage of the NIL in 2007 and once again in 2010; placing restrictions on FDI receipts on roughly 22% of all large manufacturing firms operating in the country (Genthner and Kis-Katos 2022).

To be able to identify aggregate labor-market effects of FDI regulation—and capture direct as well as spillover effects—, we construct a measure of local regulatory penetration (LRP). In this measure, we combine policy information from the Presidential Decrees with firm and labor-market data in a shift-share structure, and interact the initial share of the labor force that is employed in firms of different types operating in different five-digit product categories with regulatory shifts (at the product-code and firm-type level) over time. This measure is constructed at the level of local labor markets: Indonesian regencies (*kabupaten*) and cities (*kotamadya*), which we jointly refer to as districts, and allows us to exploit local spillovers to various economic sectors.

Our empirical strategy relates labor-market outcomes to FDI regulation in two ways. *First*, we use the Indonesian Economic Census data from 1996, 2006 and 2016, that comprises information about the number of employees among all enterprises in mining, manufacturing and services. This allows for a detailed analysis of firm dynamics with respect to employment, entry and exit, by firm size and industry. Based on this census data, we regress changes in employment between 2006 and 2016 on the change in LRP between 2006 and 2016, which encompasses the two major regulatory reforms, while controlling for a rich set of time trends by initial district conditions, most notably the initial employment share in manufacturing (as in Autor et al. 2013). *Second*, we complement these results with estimates from a district-level panel constructed from annual household survey data, the *Susenas*, that covers the time period 2000–2016. The annual structure of the data enables us to estimate the dynamic effects of the time-variant district-level LRP measure on local labor-market outcomes.

Our results based on the Economic Census indicate that non-agricultural employment increased in those districts that were more strongly affected by the new restrictions on manufacturing FDI. On average, a one standard deviation increase in regulatory penetration increased the employment rate by 0.97 percentage points. Employment increases are found not only in manufacturing but also in services, highlighting the

[4] Products in Indonesia are classified according to KBLI (*Klasifikasi Baku Lapangan Usaha*), which is largely equivalent to the United Nations's ISIC classification at the four-digit level. In what follows, we refer to codes at the five-digit level as products, to codes at the four- and three-digit level as industries, and to codes at the two- or one-digit level as sectors.

[5] The negative investment list is released in the form of Presidential Decrees and investment conditions vary in intensity, ranging from soft licensing requirements to hard investment bans in some sectors. Some of the restrictions are conditional on firm characteristics such as size, legal status, and prior FDI shares.

importance of cross-sectoral spillovers (Neumark and Simpson 2015, Dix-Carneiro and Kovak 2019). Gains in manufacturing employment were largely driven by the market entry of new micro and small firms, leading to a reduction in the average firm size in the affected districts. In contrast, increases in service employment originated at the intensive margin, as existing larger firms hired new workers. Positive employment effects are also detected in the *Susenas* data, and the analysis of dynamic effects points toward an increase in effect size over time. The effect peaks about five years after the FDI regulation became more restrictive, suggesting that a one standard deviation increase in the exposure to FDI restrictions results in a 1.1 percentage point increase in the employment rate in the medium-run.

Our results are consistent with an increase in the labor-to-capital ratio among restricted firms, and a fall in restricted firms' productivity, which allowed for the market entry of smaller and less competitive firms, but cannot be fully explained by these mechanisms alone. We document additional spillover effects along the value chain, through an increase in the share of inputs that are sourced locally by regulated firms, as well as spillovers into the service sector, where we find evidence that increased immigration triggered employment gains in the construction sector. These spillovers explain at least 50% of the overall effect. In contrast, local multiplier effects seem to play no role in explaining employment growth.

While previous work has shown that past labor-market dynamics are not predictive of the targeting of FDI regulation in this context (Genthner and Kis-Katos 2022), this does not preclude concerns that endogenous policy formation or omitted variables may be driving our results. We therefore check for pre-trends in the main outcomes both in the Economic Census data as well as in the *Susenas* data, and find no evidence that those would be explaining the positive employment effects. Our baseline specifications also allow districts to be on different trajectories depending on initial levels of regulatory penetration and by employment shares in manufacturing, overall employment rates and degree of urbanization. In addition, we show that our results are robust to including a rich set of additional controls, both in form of time trends that vary by initial conditions as well as in the form of time-variant control variables. In particular, we control for political-economy factors such as lobbying-potential (driven by industrial concentration) or privatization-pressure (captured by the share of and change in state-owned employment), exposure to changing trade flows (based on import and export flow data), trade liberalization (in the form of average input and output tariffs and non-tariff measures), trends in automation (measured by the stock of industrial robots), agglomeration effects (measured by initial population density or its pre-reform change) and labor-market reforms (reflected in minimum wage legislation).

A recent strand of the literature discusses validity concerns in shift-share instrumental variable designs. Our robustness checks address the most common arguments, even though our empirical strategy exploits the shift-share structure only in a reduced form approach. In particular, we test if our standard errors are downward biased because of correlation in error terms between districts that have a similar production structure initially. Our results are robust to clustering standard errors by the initial distribution of LRP (Borusyak et al. 2021). We also run placebo regressions in which we randomly assign regulatory status to groups of firms belonging to the same sector, and find no evidence that our empirical strategy leads to an over-rejection of the null hypothesis (Adão et al. 2019). Finally, we investigate if regulation in particular sectors drives our findings and whether results change once we exclude those sectors from the analysis (in the spirit of Goldsmith-Pinkham et al. 2020).

Our results contribute to four strands of literature. *First*, we contribute to the rich literature that analyzes the effects of FDI flows and foreign acquisitions on firms and workers. Previous studies have shown that multinational enterprises (MNEs) employ more workers on average, are more likely to formalize employment and pay higher wages (e.g., Lipsey and Sjöholm 2004, Harrison and Rodríguez-Clare 2010, Javorcik 2014). Due to its higher technology content, FDI is found to be complementary to high-skilled workers, resulting in a larger skill wage gap (Feenstra and Hanson 1997, Figini and Görg 2011, Lee and Wie 2015). Moreover, foreign acquisitions can also lead to positive spillovers on workers employed in domestic firms (Poole 2013, Alfaro-Urena et al. 2021) and foster local development in the long run (Méndez-Chacón and Van Patten 2022), but they may also destroy jobs within less competitive domestic firms and reduce the demand for low-skilled workers (Jenkins 2006, Girma 2005). We add to this literature by showing that FDI restrictions led to an increase in local labor demand in Indonesia and by demonstrating that a substantial share of the employment gains can be attributed to spillover effects (along the value chain or into the service sector). Our findings can also be seen as complementary to the extensive literature on the negative labor market effects of offshoring in the global North (see e.g., Harrison and McMillan 2011, Ottaviano et al. 2013, Kovak et al. 2021).

Second, we contribute to a relatively smaller literature that focuses on the effects of changes in the regulatory environment of FDI. Related studies either address the effects of FDI de-regulation on firm productivity directly (Bourlès et al. 2013, Duggan et al. 2013, Eppinger and Ma 2019, Genthner and Kis-Katos 2022), or complement their analyses of tariff liberalization by controlling for changes in FDI regulation (Topalova and Khandelwal 2011). More closely related to our paper, Erten et al. (2023) show that FDI liberalization in China increases employment, and accelerates structural change at the local level. We contribute to this literature by providing the first analysis of the local labor market effects of a tightening FDI regime, which has increasing global policy relevance, and by demonstrating that the labor market effects of FDI regulation and deregulation need not be symmetric.

Third, we contribute to the literature on the distributional effects of local labor demand shocks due to trade liberalization in developing and transition economies (Topalova 2010, Kis-Katos and Sparrow 2011, Kovak 2013, Kis-Katos and Sparrow 2015, Dix-Carneiro and Kovak 2017, Gaddis and Pieters 2017, McCaig and Pavcnik 2018, Kis-Katos et al. 2018, Wang et al. 2022). These studies show that workers in regions highly affected by trade liberalization often bear the adjustment costs by facing diminishing earnings or job losses both in the short and longer run, although with substantial heterogeneities. With our study, we show that a protectionist measure, here FDI regulation, can have a comparable effect and contribute to employment gains among the local population.

Finally, we also contribute to the regional economic literature on labor market effects of place-based policies and demand shocks. Studies in this field highlight the importance of spillover effects on the local employment structure (see, for instance, Kline and Moretti (2014) for the US and Lu et al. (2019) for China). We show that shielding the economy against foreign investment enforces cross-sectoral demand linkages and results in substantial employment gains within districts.

The remainder of this paper proceeds as follows. Section 2 introduces the institutional background of FDI regulation and the NIL in Indonesia. Section 3 presents the data and develops our measure of regulatory penetration. Section 4 describes the empirical strategy and presents the results. Section 5 then analyses potential mechanisms, and Section 6 concludes.

2 Institutional Context

Early steps towards opening the Indonesian economy to FDI already started in the first years after the end of the Sukarno regime in 1967 (Gammeltoft and Tarmidi 2013). However, public opposition against foreign presence in particular industries halted the liberalization process (van Zanden and Marks 2012). Only in the 1990s, major reforms converted Indonesia into "one of the most promising countries [for investment]" (Lindblad 2015, p. 225). Increasing FDI inflows came to a sudden halt during the Asian financial crisis in 1997 that destroyed much confidence among investors (WTO 1998). To restore its status as an attractive host for FDI, the government introduced fiscal incentives and established an anti-discrimination rule between foreign and domestic investors while also streamlining application procedures in the years after the crisis (WTO 2013).

The efforts of promoting FDI in the 2000s were, however, accompanied by a protectionist movement that targeted very specific sectors deemed in Indonesia's national interest. Designed to increase transparency of previously unclear procedures (WTO 2013), the so-called negative investment list was first released in 2000 and listed products that were either entirely closed to FDI or required special licensing or the formation of joint ventures for foreign investment.[6] This list was substantially expanded during its first revision in 2007, leading to a more restrictive regulatory environment overall. The NIL 2007 not only added new products to the list, it also widened the scope of potential conditions to investment: While the first NIL in 2000 only included conditions on licensing requirements and the prerequisite to form a joint venture with a domestic enterprise, the NIL 2007 additionally specified that foreign investments could be restricted to small and medium-sized enterprises, to partnerships, or to particular provinces, or limited to a certain threshold of foreign capital shares. Figure 1 shows the changes in the average stringency of FDI regulation in the manufacturing sector across Indonesian regions (measured by LRP, as described in section 3) over time. While regulation levels were low between 2000 and 2006, regulatory penetration rose steeply in 2007.

Additional revisions followed in subsequent years. After some minor adjustments at the beginning of 2008, the next major revision took place in 2010, which extended the list of regulated products further, and changed some of the conditions. Overall, this resulted in a second strong increase in the LRP measure in 2010. The next revision in 2014 turned out relatively minor in comparison.[7] In a systematic analysis of the determinants of product-level regulation, Genthner and Kis-Katos (2022) show that FDI regulation was especially targeting product markets with public enterprises and prior privatization experiences (see table A1 in the appendix). For instance, products that experienced larger decreases in the share of state-owned firms at the beginning of the 2000s were more likely to become part of the NIL in 2007. Determinants related to prior privatization dynamics are the most frequent predictors of product regulation (among the top ten determinants). By contrast, none of the top ten predictors refer to prior employment dynamics within the product-category. Hence, we consider it unlikely that labor market considerations played a primary role in the use of this regulatory instrument.

[6] This first version of the NIL was released within Presidential Decree 96/2000. Later revisions took place in 2007 (by Presidential Decree 77/2007), 2010 (36/2010) and 2014 (39/2014), as well as a minor amendment (111/2007). To ease exposition, we will directly refer to the respective revisions as NIL 2000, NIL 2007, NIL 2010 or NIL 2014.

[7] One important characteristic of all revisions of the NIL is that they only apply to future investments while existing foreign capital is untouched. Firms are not forced to divest but the regulation only interferes with future plans of investment and the product-specific investment environment. For instance, see article 8 in Presidential Decree 36/2010. See also Genthner and Kis-Katos (2022) for a more detailed description of the NIL and its conditions and coverage.

3 Data

3.1 District-Level Outcome Variables

We derive local labor market outcomes from two datasets provided by BPS (Indonesian Statistical Office, *Badan Pusat Statistik*): the Economic Census (*Sensus Ekonomi*), and the national household survey (*Survei Sosial Ekonomi Nasional, Susenas*). While the Economic Census is only available once every ten years, the *Susenas* is collected annually as repeated cross-sections.

The Economic Census covers the universe of all firms in the economy (excluding agriculture), and is available to us for the years 1996, 2006 and 2016. It provides a complete picture of economic activity across all districts in mining, manufacturing, and services. All three census waves consistently collect firm-level information on the total number of workers. For our main results, we use this information to compute aggregate non-agricultural employment, employment by firm size and by broad economic sector, as well as average firm employment and the number of firms at the district level.[8] As Indonesia experienced substantial population growth over the same time period and accordingly a steady increase in the working age population (see Figure A1 in the appendix), we construct employment rates instead of employment numbers as main outcome variables.[9] Table A2 in the appendix shows descriptive statistics. Between 2006 and 2016, we observe a rise in non-agricultural employment rates, mainly driven by the service sector. The average firm size also increased by about 22%. However, this masks heterogeneous trends across sectors, as manufacturing firms became substantially smaller while the number of manufacturing firms strongly increased.

Our second data source, *Susenas*, provides annual representative population information at the district level. We use data from the time period 2000 to 2016, which allows us to also analyze local labor market dynamics in the years before and directly after the regulatory change. We mainly rely on information on individuals' employment status, but also utilize information on individuals' skill level, sector of employment (for initial characteristics), place of residence (urban vs. rural) and migration status as well as household expenditures for additional analyses. When constructing individual-level outcomes, we restrict our attention to the working-age population (individuals between the age of 15 and 64) and eliminate observations with missing values in crucial characteristics such as gender, educational attainment or age, before aggregating to the

[8]Due to an ongoing decentralization process, Indonesian districts repeatedly split over our sample period. To deal with changing district borders, we aggregate all data to the district boundaries of the first year observed in the data (1996 for analyses that use the Economic Census, 2000 for analyses that use *Susenas*). Note that our results are not driven by job creation due to decentralization (see Section 4.5).

[9]This allows abstracting from for spatial heterogeneities in population dynamics. Year and district specific population numbers are obtained from the *Susenas*.

district-year level. Table A3 in the appendix presents descriptive statistics.

We complement this data with a variety of other data sources. We use the labor force survey, *Sakernas*, which is available to us for the years 2000 to 2015, and contains information about individuals' activity status, (un)employment, working hours and hourly wages. Though more detailed in terms of labor market outcomes than the *Susenas*, the labor market survey is only fully representative at the district level starting in 2007 and hence lacks a reliable measure of pre-reform dynamics. We therefore only use the *Sakernas* data to compute wage-premia. We also rely on auxiliary data from the Indonesia Database for Policy and Economic Research (*Indo-Dapoer*) by World Bank (2019), on tariff data and non-tariff trade measures data from the UNCTAD-TRAINS database (United Nations 2019), as well as on stock of industrial robots from the International Federation of Robotics (2018). Price indices, regional poverty lines and minimum wages as well as input-output tables are obtained from BPS.

3.2 Local Regulatory Penetration

To construct a measure of local regulatory penetration in the manufacturing sector, we combine policy data from the NIL with data from the annual manufacturing census (*Survei Industri*, SI)[10] and *Sakernas*. We focus on regulation in the manufacturing sector because this type of FDI regulation is expected to have more immediate implications for employment outcomes as manufacturing firms can more flexibility adjust the labor-to-capital ratio in their production than firms operating in services.

From each NIL, we extract the list of products (at the five-digit level) that are regulated and the type of regulation that firms operating in that product-group are exposed to. Revisions may extend the list by adding new products or by adding to the existing regulations such as to include hitherto unregulated firms. For instance, production of coloring yarns using natural or man-made fibers was added to the list in 2007, but was kept conditionally open to investment in small and medium-sized firms. From the *Survei Industri*, we obtain the number of manufacturing firms operating in each product-group (as identified by the 5-digit code of their main product), their characteristics, and number of employees. Relevant firm characteristics include firm size in terms of assets and sales,[11] legal status (partnerships are often excluded from regu-

[10]The *Survei Industri* covers the universe of manufacturing plants with more than 20 employees in Indonesia. The survey is conducted by BPS on an annual basis and was frequently used in other empirical studies (e.g., Amiti and Konings (2007), Blalock and Gertler (2008), see Márquez-Ramos (2021) for a survey). As the *SI* links plants between survey rounds, we exploit the panel structure of the data to calculate the median number of employees for each plant for the time period 2000 to 2005, before aggregating employment numbers by year, product-group and firm type. For more details on data cleaning and the sample used, see Genthner and Kis-Katos (2022). For the sake of simplicity, in what follows we refer to *firms* instead of *plants* as the survey information does not identify multiplant firms.

[11] The size of Indonesian firms is defined by Presidential Decree No. 36/2010 (which refers to law 20/2008

lation), and shares of prior FDI ownership. We use this information to compute pre-reform employment per product-group, firm type and district. From the *Sakernas*, we calculate the size of the initial labor force, as the *Sakernas*, unlike *Susenas*, identifies individuals who are currently active in the labor market. To improve precision, we combine data from several pre-reform years (2000–2005) and compute the median labor force per district over that time period.

Our measure of local regulatory penetration LRP_{dt} in district d and year t is defined as:

$$LRP_{dt} = \sum_{kp} \frac{L_{kpd,0}}{L_{d,0}} REG_{kpdt}, \tag{1}$$

where REG_{kpdt} is an indicator that takes the value one if firms of type k that produce the product p and operate within district d are on the investment blacklist in year t and zero otherwise. $L_{kpd,0}$ is the total pre-reform employment in all manufacturing firms of firm type k, producing p as their primary product, operating in district d (derived from the *SI*), and $L_{d,0}$ is the initial size of the labor force in district d (derived from *Sakernas*). The initial time period $t = 0$ is represented by the median value observed over the years 2000 to 2005, during which no regulatory changes occurred.

Our measure of local regulatory penetration interacts the initial share of the potentially directly exposed labor force with the regulatory shifts accruing over time, and resembles a Bartik-style shift-share instrument (Bartik 1991). The temporal variation in LRP_{dt} originates from revisions of the NIL, while spatial variation originates from the initial distribution of firms of different types and operating in different product-groups across districts. This measure builds the cornerstone of our identifications strategy, which we discuss below.

The average development of LRP over time is depicted in figure 1. The upper thick line in the graph shows a step-wise increase in the overall regulatory penetration after each of the two major revisions (in 2007 and 2010). To ease interpretation, we multiply LRP by 100 so that it represents the percentage of local workers exposed to FDI regulation. Between 2000 and 2016, LRP increased by 0.6 percentage points in the average district (see tables A2 and A3). Figure 1 also shows the contribution of each industry to total manufacturing LRP over time. It splits LRP into its sectoral components, reflecting the initial share of industrial employment in total labor force and the shifts in regulation over time. Wood and wood products make up a substantial part of regulatory penetration, but there are also other sectors that contribute to LRP (e.g., the food

on small and medium-sized enterprises): Any firm is considered large if its annual sales exceed 50 billion IDR or its net assets are larger than 10 billion IDR (in constant prices). All firms with both sales and net assets below these thresholds are considered small or medium-sized. Regulation often applies only to large companies according to this definition.

and beverage industry, tobacco products, or wearing apparel). A detailed list of the sectors that contribute to the LRP can be found in table A4 in the appendix. There are several sectors that are not affected by the NIL at all, such as leather products or motor vehicles. Figure 2 maps the spatial distribution of changes in LRP for the period from 2000 to 2016. In most districts and across all major islands, the regulatory environment tightened between 2000 and 2016. Only very few districts experienced declines in overall regulation.[12]

For our robustness checks, we also construct a number of control variables from the *Survei Industri*, such as the regional concentration in sales and employment, the share of state-owned enterprises, the share of recently privatized firms, imports and exports, etc.

4 Empirical Strategy and Results

4.1 Empirical Strategy

In our empirical models, we link changes in local labor market outcomes to temporal and spatial variation in LRP. The main unit of observation is the district-by-year cell, and identification relies on two main assumptions: *first*, that regulatory shifts do not respond systematically to labor market trends at the local level, and *second*, that no other unobserved shocks affect our measure of regulatory tightening and employment outcomes simultaneously.

The high spatial as well as sectoral dispersion of the regulatory shifts are cornerstones for the first assumption to hold (Borusyak et al. 2021). In other words, if certain sectors were highly spatially concentrated, then district-specific economic concerns could lead policy makers to restrict FDI to the sectors particularly prevalent in those districts (cf. Neumark and Simpson 2015). However, descriptive statistics in table A5 in the appendix do not show any evidence of such a regulatory clustering. The average number of five-digit products produced within one district is 20.8, with 6.5 of those being regulated. Despite its right-skewed distribution, there are still only a few districts hosting very few products. This means that variations in LRP are generally driven by many different products. The average number of regulatory shifts (i.e., actual changes from unregulated to regulated products) per district is similarly high. Moreover, each five-digit product is produced in about 20 different districts on average. Again, this shows that most products are manufactured in several places and district-specific economic concerns should play a minor role in the selection of products that enter the list. To fur-

[12]We also report the spatial distribution of the LRP levels for the years most relevant to our estimation strategy in figure A2 of the Appendix. Figure A3 further shows the density distribution of LRP.

ther corroborate the validity of the orthogonality assumption, we test for the existence of pre-trends. As discussed further below, we find no evidence of non-parallel trends.

For the second assumption to be valid, we need to be able to separate shocks to FDI restrictions from more general shocks to certain sectors or places. We therefore control for initial employment in the manufacturing sector in all our specifications, so as to restrict identification to variation in FDI restrictions stemming from differences in the product-composition within local manufacturing sectors. We also control for aggregate shocks at the regional-level, and for a set of initial conditions (regulatory penetration, urbanization and employment rates) that may correlate with the product-composition in manufacturing and with trends in employment.

4.2 First-Difference Estimates from the Economic Census

To estimate the effect of regulatory penetration on employment rates from aggregated Economic Census data, we use a first-difference specification, and regress changes in labor market outcomes in district d of region r between 2006 and 2016, Δy_{dr}^{06-16}, on changes in the constructed LRP measure between 2006 and 2016, ΔLRP_{dr}^{06-16}, which captures the regulatory tightening during the first two major NIL revisions as well as some minor adjustments in between (*c.f.* figure 1). We estimate the following regression:

$$\Delta y_{dr}^{06-16} = \alpha_1 \, \Delta LRP_{dr}^{06-16} + \mathbf{X}'_{dr,0} \, \alpha_2 + \lambda_r + \varepsilon_{dr}, \tag{2}$$

where λ_r are island-group fixed effects, and $\mathbf{X}_{dr,0}$ is a vector of initial controls and captures local conditions that may drive differences both in regulatory exposure as well as in labor market dynamics. Thereby, we absorb trends in the employment structure that are driven by initial conditions. For our baseline specification, $\mathbf{X}_{dr,0}$ includes the initial level of regulatory penetration, the share of manufacturing employment in total employment, the share of urban population, and the non-agricultural employment rate (in the working-age population), all observed in 2006. Our robustness checks in section 4.5 further extend the set of initial conditions $\mathbf{X}_{dr,0}$, and also add a list of time-varying controls for which we calculate the change between 2006 and 2016, $\Delta \mathbf{Z}_{dr}^{06-16}$.

Table 1 presents results based on the first-difference specification using Economic Census data. In columns 1 to 5, we report the effect of the local regulatory tightening between 2006 and 2016 on the change in the non-agricultural employment rate between 2006 and 2016. We add controls step-wise to gauge the robustness of our estimates. Our results indicate that a tightening of the regulatory environment leads to an overall increase in non-agricultural employment in the district. The point estimates are largely unchanged across specifications. In column 6 we report results of a placebo-test in which we regress the change in the employment rate in the previous decade (between 1996 and 2006) on changes in the LRP between 2006 and 2016. We find no evidence that pre-trends may be driving the observed effect. In terms of magnitude, the point estimates of column 4 suggest that an increase in LRP by one standard deviation (1.00) raises employment by an additional 0.97 percentage points (pp). Another way to gauge the economic relevance of the NIL is to calculate the share in the employment gain between the two census rounds that can be explained by the change in local regulatory penetration. On average, employment rates increased by 6pp between 2006 and 2016, while the LRP increased by 0.62 on average. This implies that FDI restrictions can explain about 10% of the employment gains observed over the time period.

We disaggregate the employment effect by sector and firm size in table 2. Column 1 reports the effect across all non-agricultural sectors, while columns 2 and 3 report effects on the employment rate in manufacturing (incl. mining) and services, respectively. Panel A reports the effect across all firm sizes, while panels B to D split the sample by the size of firms (in terms of their number of employees). All estimates are obtained from the baseline first-difference specification (corresponding to column 4 of table 1) and control for island fixed effects, as well as initial district characteristics (LRP, the share of manufacturing in overall employment, the employment rate, and urbanization). Given that the outcome variable is the employment rate per subgroup, the sector- and group-level coefficients mechanically add up to the coefficients in the first column (for columns 2 and 3), as well as in the first row (for panels B to D).

We find statistically significant evidence of employment increases in the manufacturing sector whereas coefficients in the service sector are also positive but not statistically significant. Most of the employment gains in manufacturing are realized among (micro and) small firms, and to some extent also among medium to large firms (panels B to D). In the service sector, by contrast, all potential employment gains seem to concentrate in medium or large enterprises, even though imprecisely estimated (panel D).[13]

Table 3 provides additional evidence of where and how employment creation takes place by contrasting changes in the average firm size with changes in the number of firms (of various sizes). We find no evidence of significant increases in average firm size across all non-agricultural sectors (column 1 of Panel A). When disaggregating the effects on firm size by sector, we find negative (but not statistically significant) coefficients in the manufacturing sector, and insignificant positive coefficients in the service sector. At the extensive margin, FDI regulation results in substantial firm entry in manufacturing which is concentrated among the self-employed and small firms. By contrast, we observe no meaningful market entry nor exit among service sector firms.

[13] Table A6 in the appendix shows placebo-tests by firm size and sector, and confirms the absence of pre-trends on aggregate and in the services sector, while documenting pre-trends of the reversed sign among small manufacturing firms.

Our results so far indicate that manufacturing FDI regulation increases manufacturing employment through the market entry of relatively smaller enterprises, and to some extent through growth in the number of employees per firm among medium to large manufacturing firms. At the same time, our results suggest some scope for employment spillover effects to the service sector, even though none of the services results turn out statistically significant. What is not clear from the results so far, is whether the creation of new jobs leads to net employment gains or whether they merely accelerate the ongoing process of structural transformation. We turn to this question in the following.

4.3 Difference-in-Difference Estimates from the *Susenas*

To complement our results with yearly dynamics based on household-level data, we link variation in LRP to total employment rates in district d and year t, y_{drt}. We thereby exploit annual changes in regulatory penetration LRP_{drt} at the district level. Our empirical specification takes the form:

$$y_{drt} = \beta_1 \, LRP_{drt} + \mathbf{X}'_{dr,0} \, \beta_2 \times t + \gamma_d + \phi_{rt} + \varepsilon_{drt}. \tag{3}$$

All regressions are conditional on district fixed effects, γ_d, and island-year fixed effects ϕ_{rt}. The error term ε_{drt} is clustered at the district level. To mirror our long-difference specification, we control for the same set of initial district conditions $\mathbf{X}_{dr,0}$ and interact them with a linear time trend. By that, we make sure that dynamics that are mechanically related to the initial level of regulatory penetration, the relative importance of the manufacturing sector, or the overall employment rate and urbanization in a district do not spuriously affect our estimates. Because the panel starts in 2000, but the *Susenas* does not cover all districts in the earlier years (for example no data were collected in Aceh in 2000 and 2001), all our initial conditions are calculated by taking the median over the years 2000–2005. We also check for the robustness of our results in section 4.5 by allowing for linear trends in an extended set of initial conditions $\mathbf{X}_{dr,0}$, or adding time-varying controls $\mathbf{Z}_{drt}$.

Table 4 presents our main results using *Susenas* data for the total employment rate (panel A) as well as employment numbers (panel B). The first two columns display the correlation between total employment and LRP, conditional on district and island-year fixed effects. Further controls are added step-wise in each column, starting with an interaction between a linear time trend and regulatory penetration in the pre-reform years. Column 4 presents our preferred specification which additionally absorbs differential trends driven by initial conditions as specificed in eq. (3). Column 5, finally, allows districts to be on different nonlinear trajectories by interacting the initial LRP with a full set of year fixed effects.

Our results confirm the significant positive relationship between LRP and total employment. The point estimate drops in magnitude when we absorb differential structural change dynamics in column 4. By contrast, allowing for flexible time trends in initial conditions does not alter the coefficient further. Our preferred specification (column 4 of panel A) yields a point estimate of 0.002. In terms of magnitude, this suggests that a one standard deviation increase in LRP (1.22) is associated with a 0.24 percentage point increase in the total employment rate.[14]

The fact that we have annual data for several years pre-reform as well as post-reform, lends itself to a more a dynamic specification of the estimation equation. While the gradual nature of the LRP expansion (with multiple revisions over the time period we study) does not allow us to estimate a classical dummy variable event-study design, we can nonetheless recover dynamic effects by estimating a distributed-lag model and appropriately reparameterizing the estimated coefficients (Schmidheiny and Siegloch 2023).

We estimate:

$$y_{drt} = \sum_{l=-4}^{5} \gamma_l \, LRP_{dr,t-l} + \boldsymbol{X}'_{dr,0} \, \beta \times t + \gamma_d + \phi_{rt} + \varepsilon_{drt}, \tag{4}$$

and cumulate the post-treatment and pre-treatment coefficients away from zero to recover dynamic treatment effects. To be specific, we construct $\beta_l = -\sum_{k=l+1}^{-1} \gamma_k$ if $l \leq -2$, $\beta_l = 0$ if $l = -1$ and $\beta_l = \sum_{k=0}^{l} \gamma_k$ if $l \geq 0$.[15] As outlined in Schmidheiny and Siegloch (2023), this procedure delivers consistent estimates of dynamic treatment effects as long as the treatment effect is proportional to the observed treatment intensity.

Dynamic treatment effects of local regulatory tightening are depicted in figure 3. Two insights emerge. *First*, we do not detect any evidence of pre-trends associated with local regulatory tightening, as all of the coefficients in the pre-reform years are close to zero, and none are statistically significant. This confirms that districts that experience a regulatory tightening do not systematically differ from non-affected districts in terms of their employment dynamics and alleviates concerns that our LRP measure may spuriously pick up underlying employment trends. *Second*, the treatment effects of LRP seem to be building up over time. Point estimates turn positive and statistically significant about three years after the revision of the NIL, and further increase in mag-

[14] Results are similar for the level of total employment as dependent variable (panel B). Note that when regressing total employment on LRP, we also add the size of the working-age population as a time-variant control variable to account for population dynamics.

[15] Because we include four leads and five lags, the time period covered in this analysis has to be restricted to the years 2005–2012.

nitude in subsequent years, stabilizing at effects sizes of about 0.008. This aligns well with the design of the regulation: As discussed previously, revisions to the NIL do not usually affect existing FDI but merely restrict firms and sectors from attracting future investments.

The fact that treatment effects increase over time also helps understanding the differences in effect sizes between the Economic Census and the *Susenas* sample: A point estimate of 0.009 suggests that a one standard deviation increase in LRP (1.22) raises the employment rate by 1.1pp, which is very close to the estimates reported in section 4.2. We can also calculate the share of the employment gain observed between 2000 and 2016 that can be attributed to changes in LRP. In the *Susenas* data this is 11.3%, which again aligns closely with the estimate obtained from the Economic Census data.

That the estimates from the two data sets align so closely also illustrates that employment gains observed in the non-agricultural sectors did not materialize at the expense of agricultural employment. Instead, we seem to be observing a pull effect that draws more people into the labor force. It also illustrates that the employment gains reported in the Economic Census data are not an artifact of workers starting to work for more that one firm.

4.4 Validity of the Shift-Share Approach

One concern raised by the recent literature on the validity of shift-share designs is that correlation in the error terms arising from districts having a similar initial employment structure may lead to severe downward bias of the estimated standard errors (Adão et al. 2019). This in turn results in an over-rejection of the null hypothesis. To address this concern, we follow Adão et al. (2019) and run placebo regressions in which we randomly assign regulatory status to groups of firms. The regulation indicator in these regressions is drawn from a Bernoulli distribution with mean 0.143 (the true average of regulation in the data). The regression design is identical to our preferred specifications in the long-difference or the fixed effects panel setting. Table A7 in the appendix shows the results of 10,000 placebo samples. The mean coefficient across all placebo samples in column 1 of Panel A and B is very close to zero. This is not surprising as we do not expect any systematic result from randomly assigning regulatory status to groups of firms. Column 2 reports the standard deviation of all estimated coefficients, while column 3 shows the median of all estimated standard errors. Theoretically, these two figures should be identical. Our test shows that the median standard error is always smaller, but the difference between the two is only marginal compared to the very large discrepancies shown by Adão et al. (2019). Accordingly, the rejection rates of the null hypothesis at the 5% significance level are relatively close to their expected value (note that Adão et al. (2019) find extremely large rejection rates between 30-50%). We thus do not consider correlation with respect to initial sector composition to be a severe concern in our empirical approach.

As a second check, we allow standard errors to be correlated within percentiles of the initial distribution of LRP in the spirit of Borusyak et al. (2021). This results in a reduction in the number of clusters (to 55 clusters), since districts are nested in the initial LRP percentiles. Appendix table A8 shows our main results in column 1 as benchmark. Column 2 then clusters standard errors based on the initial LRP distribution. The standard errors remain virtually unchanged.

To assess whether the regulation of singular two-digit sectors drives our result (Goldsmith-Pinkham et al. 2020), we decompose the overall effect from tightening the regulatory environment for manufacturing FDI in medium and large enterprises by two-digit sectors to identify the regulated sectors with the largest statistical influence. Bars in figure A4 show the initial employment distribution (also used for our shift-share structure) by two-digit sector. About 40% of manufacturing employment is concentrated in the production of food and beverages, textiles, and wearing apparel. The coefficients depict each two-digit sector's contribution to the main results (panel A in table 2) in a standardized form. They show that regulatory tightening in the majority of all sectors contributes to increases in district-level employment rates, including among others fabricated metals, tobacco, as well as food and beverages. No coefficients are reported for the six sectors without any regulation (compare table A4), whereas the effects turn negative only in four sectors. To check whether our estimates are exclusively driven by the sectors that contribute most to the overall effect, we exclude the top three sectors (publishing and media, other transport equipment, fabricated metals) from the sample (as reported in columns 3 to 5 in table A8), which reduces the coefficient estimates slightly but does not alter the overall interpretation of our results.

4.5 Possible Confounders

A remaining concern with the results presented so far is that FDI regulation may coincide with other time-variant shocks or that districts that experienced regulatory tightening display differential economic dynamics for a variety of reasons, leading to a spurious association between LRP and employment. In this section, we address a wide range of possible confounders.

The first set of possible confounders relates to the ability of firms to influence FDI regulation.[16] If this ability coincides with firms' economic performance, then this could

[16] A vast literature discusses the political economy of trade policy (cf. Grossman and Helpman 1994, Goldberg and Pavcnik 2005, Asher and Novosad 2017). The main argument therein is that trade policy is

lead to a spurious correlation between FDI restrictiveness and employment creation. To address this concern, we construct a wide range of proxies for the ability of firms to influence the political process from the *Survei Industri*, and test if our results are robust to allowing time-trends to differ by these characteristics. In particular, we construct indices of market concentration as this reduces the cost of coordination between firms belonging to one sector (Herfindahl index in sales or employment), as well as the fraction of initial district-level employment in national champion firms, in state-owned firms, and in recently privatized firms.[17] We also directly control for (time-varying) policies that could have been deliberately implemented by local authorities in response to FDI regulation. Here we are particularly interested in the effects of minimum wage legislation (which could have direct employment effects) and of political decentralization (as the creation of new districts and government structures also provides new job opportunities (Bazzi and Gudgeon 2021)). As reported in Table A9, our results are robust to controlling for these confounders.

A second concern would be that global dynamics influence employment outcomes in Indonesia and that the extent to which this happens coincides with the location and timing of FDI regulation.[18] We address these concerns by allowing districts to be on different time trends by initial trade openness, and also control for changes in firms' imports and exports, as well as for changes in tariffs and in non-tariff trade measures (NTMs).[19] As a proxy for automation, we add the average time-varying stock of industrial robots to our set of controls, as well as the initial share of employment in high-technology enterprises (OECD 2003). As reported in table A10, our results do not change when controlling for the local exposure to global dynamics.

Given that our LRP measure uses the initial presence of manufacturing employment as weighting factor, our results are vulnerable to concerns that LRP picks up dynamics that correspond to the relative importance of agglomeration rather than changes in regulation over time. We thus control for a set of proxies that capture regional agglomeration. First, we construct a time-invariant measure of the employment share

determined by a political process: Industries and firms lobby for policy changes that favor their own business while, at the same time, political incumbents face re-election motives that could make them sensitive to the influence of specific interest groups.

[17]National champion firms are identified by ranking firms by their total sales in each five-digit product market. We then calculate the employment in each districts in first-ranked firms. The definition of firm size is described in footnote 11.

[18]Trade policy, for example, has been shown to impact domestic labor markets directly (Hakobyan and McLaren 2016, Dix-Carneiro and Kovak 2017), while openness to trade could also determine the extent to which the ramifications of the global financial crisis of 2009 were felt by particular industries. Similarly, the increasing importance of automation in the industrial production process could lead to the restructuring of employment within firms, the reduction of routine-task jobs and to layoffs (Acemoglu and Restrepo 2019).

[19]We construct input tariffs using input-output tables as it is standard in the literature (cf. Amiti and Konings 2007) and then merge tariff and NTM information to the firm data. Our tariff and non-tariff measures are weighted by initial firm employment.

within products that are never regulated throughout the sample period. Second, we compute initial district employment in industrial areas (from the *Survei Industri*) as an alternative measure of agglomeration potential. Third, we control for initial population density, its growth between 2000 and 2005, and distance to Jakarta. Our results are robust to any of these controls (see table A11).

Finally, we show that our results are not affected by the presence of spatial regulatory spillovers, and that regulation increases overall employment only if it occurs in the manufacturing sector. In table A12 we additionally control for spatial spillovers, by summing up all other districts' LRP weighted by the inverse of the squared distance to a particular district's centroid. The coefficient on the spillover variable is not statistically significant, and the coefficient on LRP is virtually unchanged. This suggests that the main effect of regulatory tightening occurs locally. In table A13, we show that we get positive employment effects (that closely mimic the main results) if we construct LRP from the firm distribution observed in the Economic Census, but only of we restrict regulation to the manufacturing sector, and not if regulation to the service sector is included as well. It thus seems that service sector regulation generates negative employment effects at the district level.

5 Mechanisms

Our results indicate that manufacturing FDI regulation led to aggregate employment gains, which accrue both in the manufacturing and service sectors, although only statistically significantly in the former. These employment gains are net of any potential negative effects operating through declines in firm productivity, and could be operating through a variety of mechanisms, which we try to uncover in the following.

Direct employment gains could be observed for mainly three reasons. *First*, the shift from foreign to domestic capital may go hand in hand with a reduced technology content and a reduction in the capital-to-labor ratio. This would lead to an increased demand for labor in regulated firms. *Second*, employment gains could be driven by reduced competitive pressure: if firms in regulated sectors largely sell their products on the domestic market and demand is inelastic, then a decline in productivity among large firms would increase output prices and allow less productive firms to enter the market. *Third*, employment gains could arise because of spillovers along the value chain: previous work has documented a higher tendency of domestic firms to source intermediate inputs locally rather than internationally (Amiti and Konings 2007, Arnold and Javorcik 2009, Genthner and Kis-Katos 2022). Indirect employment gains (which would accrue mostly in the service sector) could arise for two reasons: *First*, an increased demand for local services could stem from income gains and resulting local economy effects or from increased public investments to affected areas. *Second*, increased immigration could raise the demand for local amenities.

If the direct employment effects were driven by a shift in the capital-to-labor ratio, these employment gains should mainly arise in regulated firms in the manufacturing sector as FDI regulation typically applies only to medium to large firms. However, this group of firms is responsible for less than a third of the overall employment gain, suggesting that this mechanism can only partly explain our results (c.f. table 2). We can also disaggregate the employment gains observed among large manufacturing firms in the Economic Census data by the regulatory status of their main product.[20] To this end, we code any 5-digit product group as regulated if it was ever subject to restrictions between 2001 and 2016. In table A14, we report the effect of changes in local regulatory penetration between 2006 and 2016 on employment gains among large manufacturing firms over the same time period. Column 1 shows the effect for all firms, columns 2 and 3 show the effect for firms that are ever regulated and firms that are never regulated, respectively.[21] We find that only about half of the employment gains in this group of firms accrue in regulated sectors, and about half (or 13% of the overall effect) in unregulated sectors, again underlining the limited explanatory power of a pure capital substitution effect.

To investigate the latter two direct mechanisms, we interact LRP with the share of output that is exported by firms of a particular industry (aggregated at the 3-digit and 2-digit level, and weighted by the share of each industry in regulated employment at district-level) or district.[22] A similar exercise interacts LRP with the share of intermediate inputs that are sourced internationally rather than domestically (again at the 3-digit, 2-digit and district level). Finally, we interact LRP with the fraction of inputs to each regulated sector that are produced in the same districts (weighted by the share of each sector in regulated employment at district-level). In table 5, columns 1 to 3, we show that employment gains are indeed the strongest in areas where regulation affects sectors and firms that typically export none of their outputs or only a small fraction. The higher the export share among the regulated sectors, the smaller the employment gain. However, the interaction term is only statistically significant in the Economic Census data and when the export share is calculated at the sector level, but not in the

[20]We can only carry out this exercise for large firms as these are the only firms for which a 5-digit product code is reported in the 2016 census, for all other firms only the 1-digit industry is reported for manufacturing firms, and 2-digit for service firms.

[21]The point estimate in column 1 is slightly larger than the one reported in table 2, panel D, as the one reported here is restricted to manufacturing firms (and excludes mining).

[22]This information is obtained from the *SI*. Let exp_p be the fraction of output being exported in each industry d, then the interaction term is defined as $I_{dt} = \frac{\sum_{kp} exp_p \times l_{kpd,0} \times REG_{kpdt}}{\sum_{kp} l_{kpd,0} \times REG_{kpdt}}$. A natural exercise would be to compare employment gains in regulated sectors versus non-regulated sectors. However, the 2016 Economic Census full count data only reports codes at the one-digit level in manufacturing.

Susenas data or when the export share is calculated at the district level. We take this as suggestive, albeit not conclusive, evidence that reduced competitive pressure is driving some of the employment gains. In terms of value chain integration, the results are somewhat more conclusive: employment gains indeed seem to be larger in districts in which regulated firms were initially relying more strongly on imports for their intermediate inputs. This result is unaffected by the level of aggregation or choice of sample (columns 4 to 6). The interaction term on local availability of inputs is also positive (column 7), but not statistically significant in either of the specifications. Taken together these results suggest that employment gains as more pronounced in places in which there is more potential for spillovers along the value chain.

In terms of indirect employment effects, we find no evidence that employment gains are driven by aggregate income effects or an increased level of public spending. An increased LRP does not coincide with increased monthly household expenditures per capita (obtained from the *Susenas* data) on average, or with expenditures at the 5th and 10th percentiles (see table 6). We also use regional poverty lines to calculate the poverty rate, but find no evidence of a decline in poverty. We then show that wage premia (estimated from the labor market survey *Sakernas*) are also unrelated to our measure of FDI regulation.[23] Finally, we show that regulatory tightening is not associated with increased local public investment (as observed from district-level expenditure data in the *Indo-Dapoer* data (World Bank 2019).

By contrast, we do find some evidence that internal migration is positively related to LRP (see table 7) and hence could be a likely mechanism underlying the potential employment gains in the service sector (which account for 38% of the overall gains). The creation of new jobs in more strongly regulated districts seems to have pulled internal migrants away from locations that experience less protectionism, and internal migration may have helped to satisfy increasing labor demand in regulated districts.[24] Unfortunately, information on migration is only available in the *Susenas* in the years 2011–2015 and thus does not coincide with the large revisions of the NIL in 2007 and 2010. However, we find similarly positive effects when we estimate the relationship between population and LRP (column 6 of Table 7). Consistent with the hypothesis that immigration may explain some of the indirect employment gains, we find that a

[23]For that purpose, we run yearly Mincer wage regressions:

$$\mathit{ln(Wage)}_{dijt} = \sum_{d=1}^{341} (\beta_{1,d} \times \mathit{District}_{dt}) + \boldsymbol{X}'_{dijt}\beta_2 + \phi_j + \epsilon_{dijt}, \tag{5}$$

where $\mathit{ln(Wage)}_{dijt}$ denotes the log hourly wage of individual i in industry j within district d and $\boldsymbol{X}_{dijt}$ includes individual characteristics. We then take the estimated coefficient $\beta_{1,d}$ as our measure for the log wage premia in district d in year t (cf. Dix-Carneiro and Kovak 2017). We weight the regression of log wage premia on LRP by the inverse of the squared standard error of equation (5).

[24]In terms of international migration, Cinque et al. (2021) show that relative reductions in FDI inflows due to regulation by the NIL result in an increasing number of emigrants to investor countries.

substantial fraction of the service sector employment gains occur in the construction sector and increases in construction sector employment are also significant statistically (as reported in Table A15).[25]

6 Conclusion

In this paper, we show that increasing protectionism towards manufacturing FDI led to employment gains in Indonesia. Our results suggest that FDI restrictions can explain about 10% of the overall increase in employment observed between 2006 and 2016. In terms of mechanisms, we find that evidence is consistent with an increase in the labor-to-capital ratio in regulated firms, and reduced productivity (which facilitated the entry of new firms). But our results also show that at least half of the overall effect is driven by cross-sectoral spillovers, which can be explained by the integration of firms in local value chains, and by an increase in immigration (which raised the demand for housing and local amenities).

Our results suggest that the labor market effects of investment protection behave symmetrically to those of trade liberalization. While output tariff reductions have been shown to depress employment (cf. Autor et al. 2013, Dix-Carneiro and Kovak 2019), we find the opposite effects from a policy reform that tightens FDI regulation (and potentially reduces the strength of local competition). Our results are also in line with studies that find overall negative employment effects of FDI due to a more efficient use of labor and higher competition (cf. Girma 2005, Jenkins 2006). In fact, we provide novel evidence showing that shielding domestic employment against foreign investment can have substantial spillover effects to other parts of the economy.

Nonetheless, this should not be understood as conclusive evidence in favour of protectionist policies. Our results rather highlight the trade-off between immediate employment gains and long-run economic development. Shielding the manufacturing sector from foreign capital investments and the inflow of new technology and know-how may be tempting in the short-run but also means that countries forfeit the positive productivity effects of FDI (Blalock and Gertler 2008, Javorcik and Poelhekke 2017), as has been shown by Genthner and Kis-Katos (2022) for the case of the negative investment list in Indonesia. The evidence at hand also does not suggest that there are positive effects on broader local economic development or living standards.

Our results are subject to some limitations. We are only able to construct meaningful measures of regulatory penetration in manufacturing and services, but still lack a similar measure for FDI into agriculture, the study of which could also provide valu-

[25] To do this, we disaggregate service-sector employment in the Economic Census data by 2-digit sector.

able insights. Moreover, we lack sufficient information on the quality of employment that would provide us with reliable welfare implications. More precise information on work contracts or linked employer-employee data would be needed to further investigate the nature of employment creation and its spillovers.

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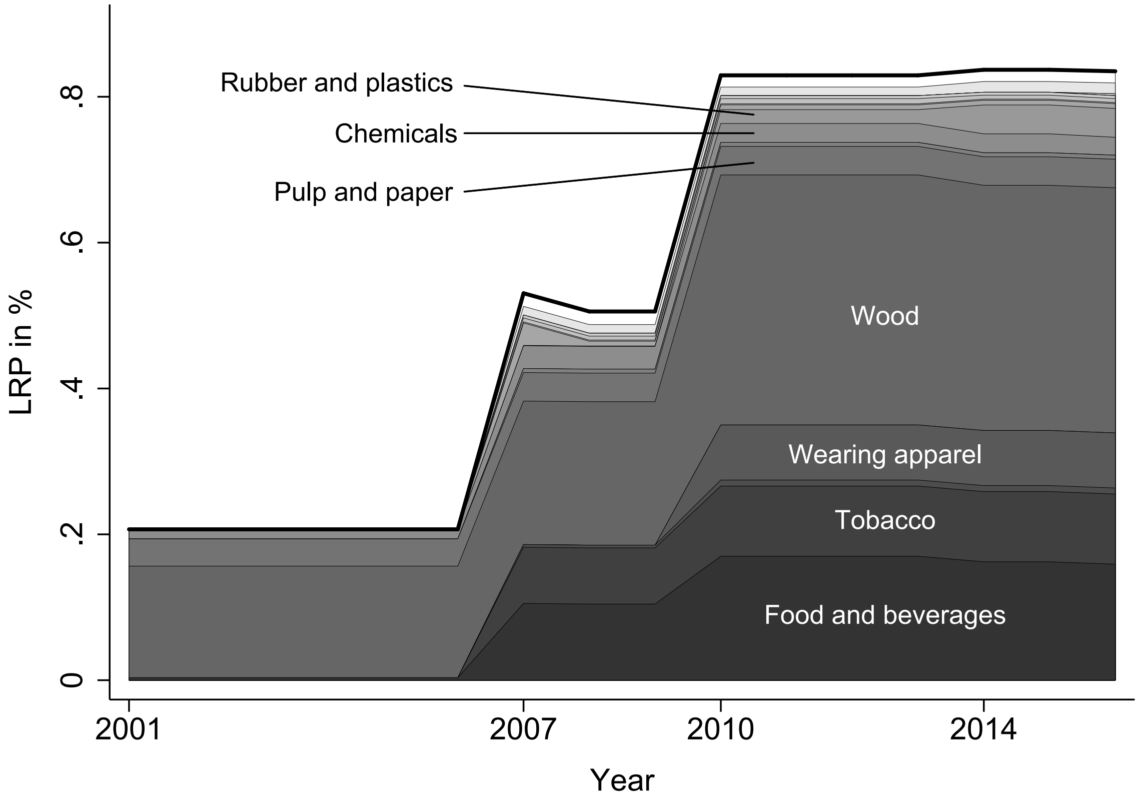
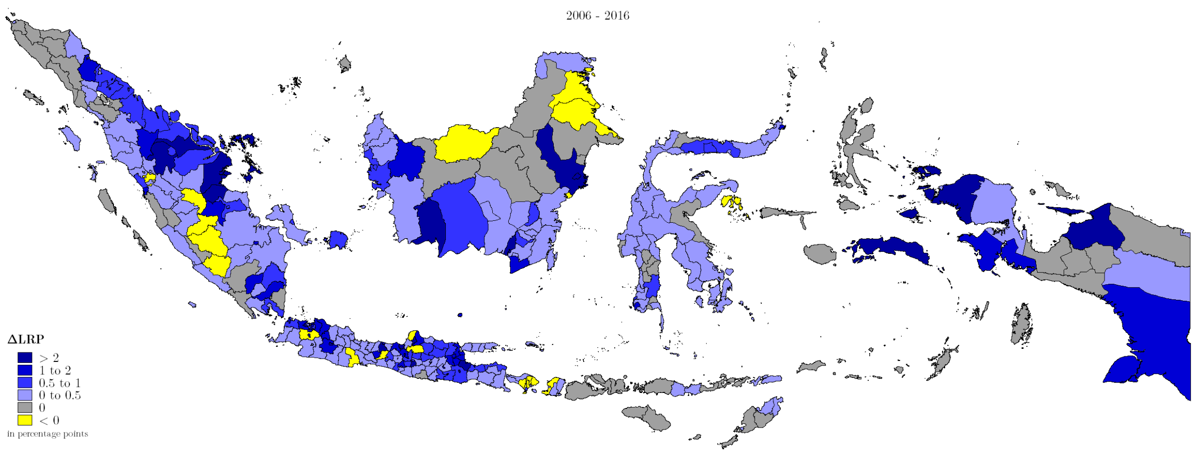
Figures

Figure 1: Sectoral Composition of Local Regulatory Penetration (LRP) over Time



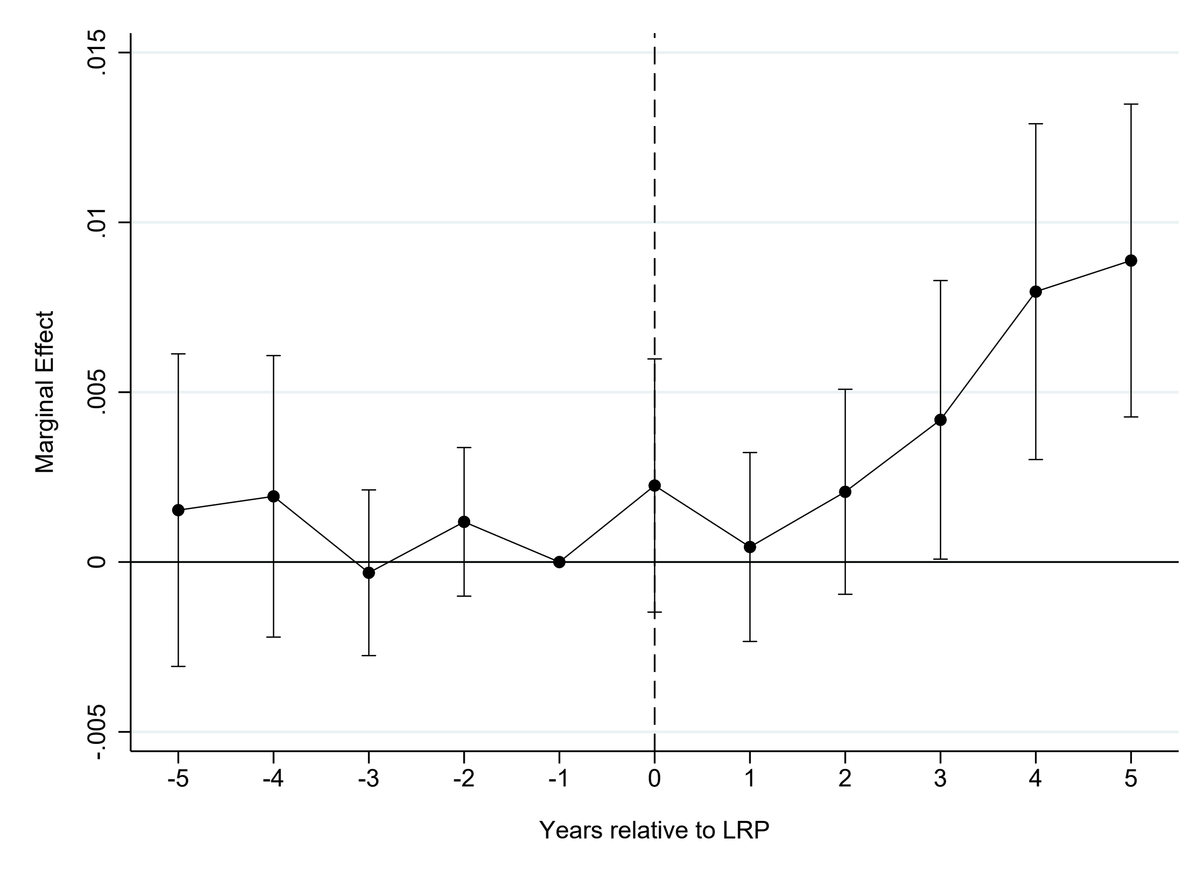
Note: The solid black line depicts average local regulatory penetration (LRP) from 2001 to 2016 based on equation (1). Shaded areas show the sectoral contribution to LRP based on regulated shares in the initial employment composition. Values are multiplied by factor 100.

Figure 2: Change in LRP between 2006 and 2016



Note: District borders are from 2000. Values are multiplied by factor 100.

Figure 3: Effect of LRP on Employment



Note: The dependent variable is the total employment rate. Each point estimate is obtained by recovering dynamic treatment effects from eq. (4) as outlined in section 4.3. Bars are 90% confidence intervals.

Tables

Table 1: Effect of Local Regulatory Penetration on Employment (Economic Census)

Dep. var.: ΔNon-agr. Employment rate	2006-2016					1996-2006
	(1)	(2)	(3)	(4)	(5)	(6)
ΔLRP 2006-2016	0.0185***	0.0155***	0.0179***	0.0097**	0.0106**	-0.0005
	(0.0059)	(0.0056)	(0.0057)	(0.0048)	(0.0048)	(0.0046)
Observations	291	291	291	291	291	291
Island FE		✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$			✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$				✓	✓	✓
ΔEmployment Rate (1996-2006)					✓	

Note: The dependent variable is the change in non-agricultural employment rate. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population within a district), all measured as the median value between 2000 and 2005. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 2: Effect of Local Regulatory Penetration on Employment by Sector, Firm size (Economic Census)

Dep.var.: ΔEmployment rate 2006-2016	Non-agr.	Manufacturing	Services
	(1)	(2)	(3)
Panel A: All			
ΔLRP 2006-2016	0.0097**	0.0060***	0.0037
	(0.0048)	(0.0023)	(0.0049)
Panel B: Microfirms (1 employee)			
ΔLRP 2006-2016	0.0014	0.0006*	0.0007
	(0.0009)	(0.0004)	(0.0008)
Panel C: Small firms (2-19 employees)			
ΔLRP 2006-2016	0.0014	0.0029**	-0.0015
	(0.0031)	(0.0014)	(0.0033)
Panel D: Medium/Large firms (20+ employees)			
ΔLRP 2006-2016	0.0069*	0.0025	0.0044
	(0.0036)	(0.0017)	(0.0034)
Observations	291	291	291
Island FE	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓

Note: The dependent variable is the change in non-agricultural employment rate. Manufacturing employment includes mining. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population within a district), all measured as the median value between 2000 and 2005. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 3: Effect of Local Regulatory Penetration on Firm size and Number of Firms (Economic Census)

	Non-agr.	Manufacturing	Services
	(1)	(2)	(3)
Panel A: Dependent variable: Δasinh L per firm			
ΔLRP 2006-2016	0.0097	-0.0455	0.0087
	(0.0085)	(0.0357)	(0.0074)
Panel B: Dependent variable: Δasinh number of (all) firms			
ΔLRP 2006-2016	0.0171*	0.2163*	0.0077
	(0.0095)	(0.1198)	(0.0109)
Panel C: Dependent variable: Δasinh number of micro firms			
ΔLRP 2006-2016	0.0262**	0.2110*	0.0187
	(0.0121)	(0.1110)	(0.0121)
Panel D: Dependent variable: Δasinh number of small firms			
ΔLRP 2006-2016	0.0027	0.1884*	-0.0084
	(0.0166)	(0.1130)	(0.0202)
Panel E: Dependent variable: Δasinh number of medium/large firms			
ΔLRP 2006-2016	0.0165	-0.0159	0.0184
	(0.0256)	(0.0441)	(0.0262)
Observations	291	291	291
Island FE	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓

Note: The dependent variable is the growth rate in average firm employment (Panel A) or the growth rate in the number of firms (Panels B to E). Manufacturing employment includes mining. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district), all measured as the median value between 2000 and 2005. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 4: Effect of Local Regulatory Penetration on Employment (*Susenas*)

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable: Employment Rate					
LRP	0.0046***	0.0053***	0.0053***	0.0019*	0.0019*
	(0.0015)	(0.0014)	(0.0014)	(0.0011)	(0.0011)
Panel B: Dependent variable: asinh(Employment)					
LRP	0.0088***	0.0105***	0.0105***	0.0039**	0.0038**
	(0.0024)	(0.0023)	(0.0023)	(0.0017)	(0.0017)
asinh(Population)	0.9694***	0.9735***	0.9735***	0.9613***	0.9610***
	(0.0134)	(0.0137)	(0.0137)	(0.0153)	(0.0155)
Observations	5,687	5,687	5,687	5,687	5,687
District, Year FE	✓	✓	✓	✓	✓
Island-by-Year FE		✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends			✓	✓	
$\mathbf{LaborMarket}_{d,0}$-specific trends				✓	✓
$\mathbf{LRP}_{d,0} \times$ Year					✓

Note: The dependent variable is the total employment rate. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district), all measured as the median value between 2000 and 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 5: Effect of Local Regulatory Penetration by Import/Export Shares

LRP interaction:	Export share of output in			Import share in inputs in			Input availability
	3d industry	2d industry	district	3d industry	2d industry	district	in district
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Economic Census (Δ Non-Agr. Employment Rate)							
LRP change 2006-2016	0.0371***	0.0388***	0.0231**	0.0098	0.0074	0.0063	0.0086
	(0.0103)	(0.0097)	(0.0094)	(0.0070)	(0.0073)	(0.0080)	(0.0150)
Interaction term	-0.0940**	-0.1304***	-0.0149	0.1515	0.2038*	0.0755**	0.0152
	(0.0385)	(0.0444)	(0.0152)	(0.1083)	(0.1181)	(0.0362)	(0.0239)
Observations	291	291	291	291	291	291	291
Panel B: Susenas (Employment rate)							
LRP	0.0076**	0.0061**	0.0071***	0.0019	0.0023	0.0031*	0.0017
	(0.0033)	(0.0030)	(0.0023)	(0.0018)	(0.0019)	(0.0017)	(0.0042)
Interaction term	-0.0073	-0.0053	-0.0040	0.0837***	0.0790***	0.0198**	0.0070
	(0.0098)	(0.0114)	(0.0035)	(0.0244)	(0.0261)	(0.0077)	(0.0073)
Observations	5687	5687	5687	5687	5687	5687	5687

Note: The dependent variable is the change in the non-agr. employment rate in Panel A, or the total employment rate in Panel B. Each regression controls for the main controls as specified in column 3 of table 1 (Panel A) and column 3 of table 4 (Panel B). Standard errors are robustly estimated (and clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 6: Effect of Local Regulatory Penetration on Private and Public Expenditures and Wage Premia

Dependent variable:	asinh(Household expenditure pc) Mean	p5	p10	Poverty rate	Log wage premia	asinh(pub. expenditure pc)
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	-0.0040	-0.0030	-0.0018	0.0027	-0.0060	-0.0065
	(0.0067)	(0.0055)	(0.0051)	(0.0029)	(0.0051)	(0.0107)
Observations	5687	5687	5687	5687	5357	5301
District, Year FE	✓	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓

Note: The dependent variable is the inverse hyperbolic sine of monthly household expenditure per capita (in adult-equivalence units). $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district), all measured as the median value between 2000 and 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 7: Effect of Local Regulatory Penetration on Immigration

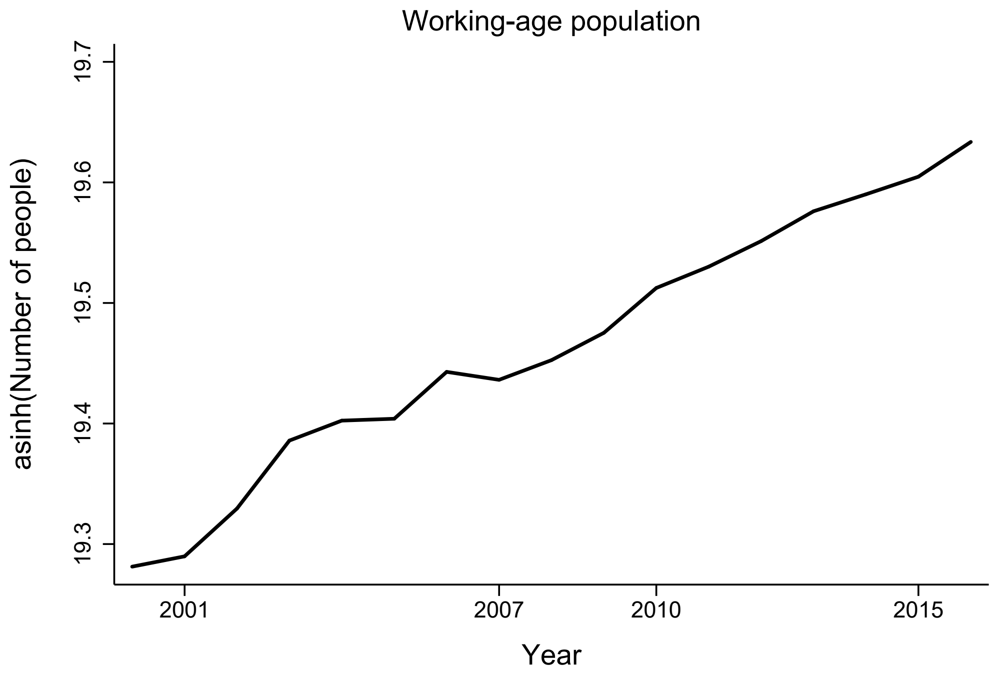
Dep.var.:	Immigration Rate					asinh(Pop.Size)
Subgroup:	All	Educ $\leq$ 9yrs	Educ $>$ 9yrs	Age 15-24	Age $\geq$ 25	All
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	0.0073**	0.0056	0.0110**	0.0171***	0.0044	0.0165**
	(0.0034)	(0.0041)	(0.0044)	(0.0065)	(0.0039)	(0.0076)
Observations	1701	1701	1701	1701	1701	5687
District, Year FE	✓	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓

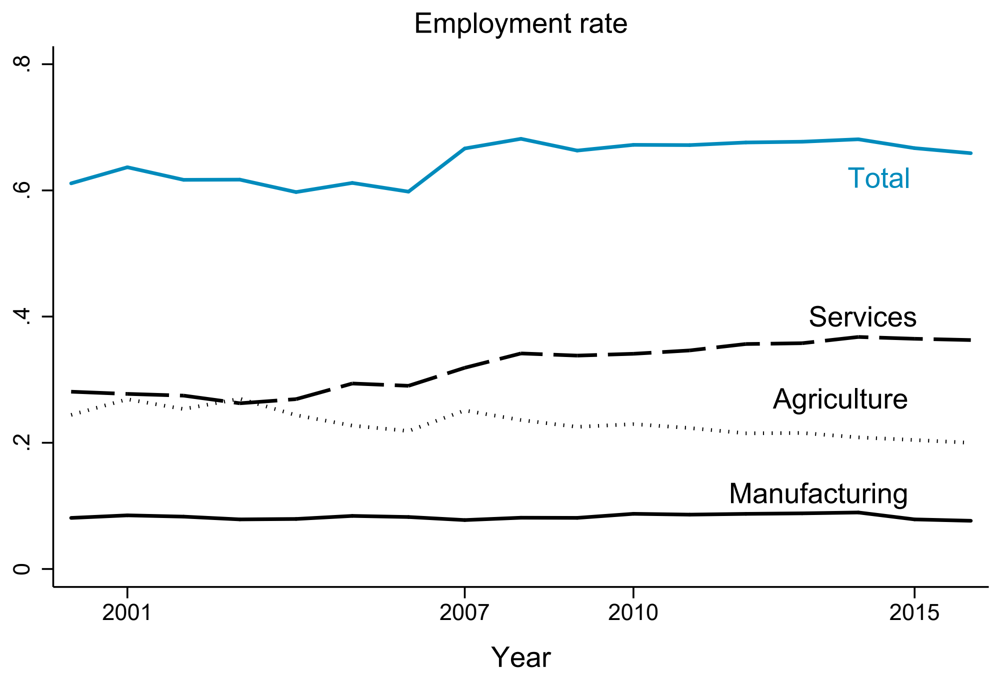
Note: The dependent variable is the immigration/emigration rate or the share of employed immigrants/emigrants in a district's population. Migrants are defined as not living in the same district as five years ago. The sample only covers the years 2011 to 2015 due to unavailable migration data in earlier years. LRP is the average of lagged regulatory penetration (from t to $t-5$) for the five year period over which migration is measured. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district), all measured as the median value between 2000 and 2005. Robust standard errors are clustered at the district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

A Online Appendix

A.1 Additional Figures

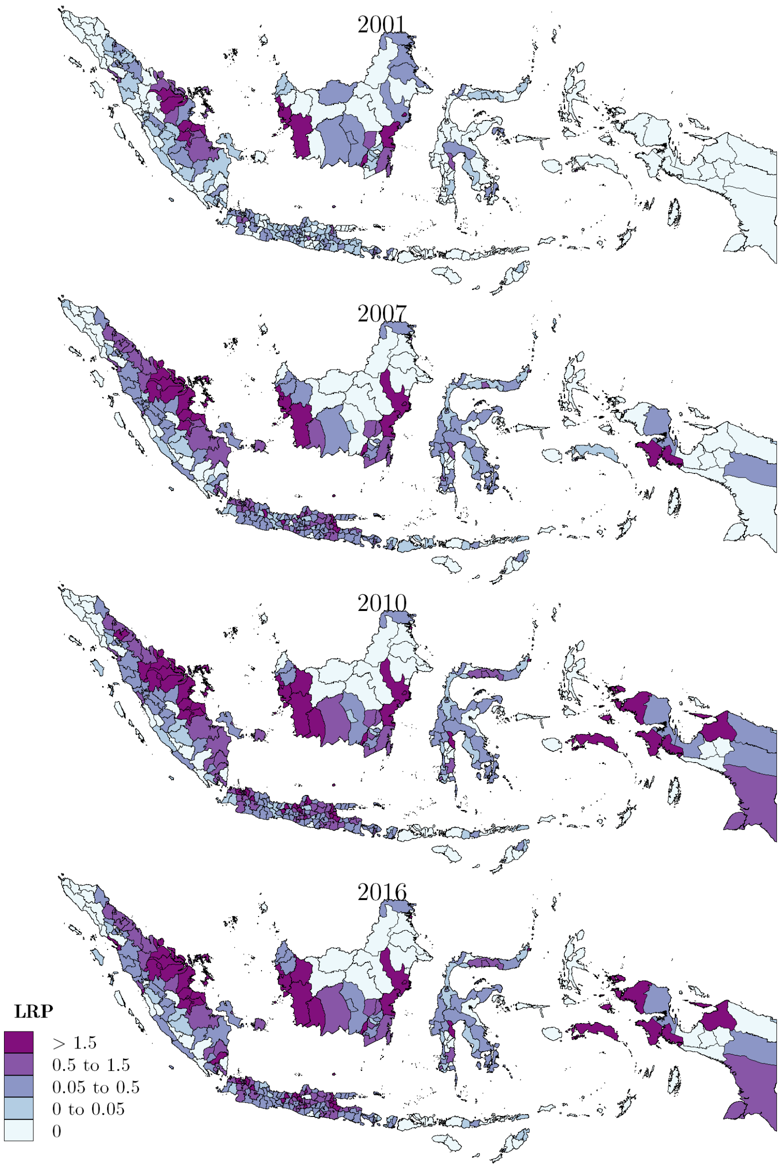
Figure A1: Working-age Population and Employment Rates by Sector (*Susenas*)





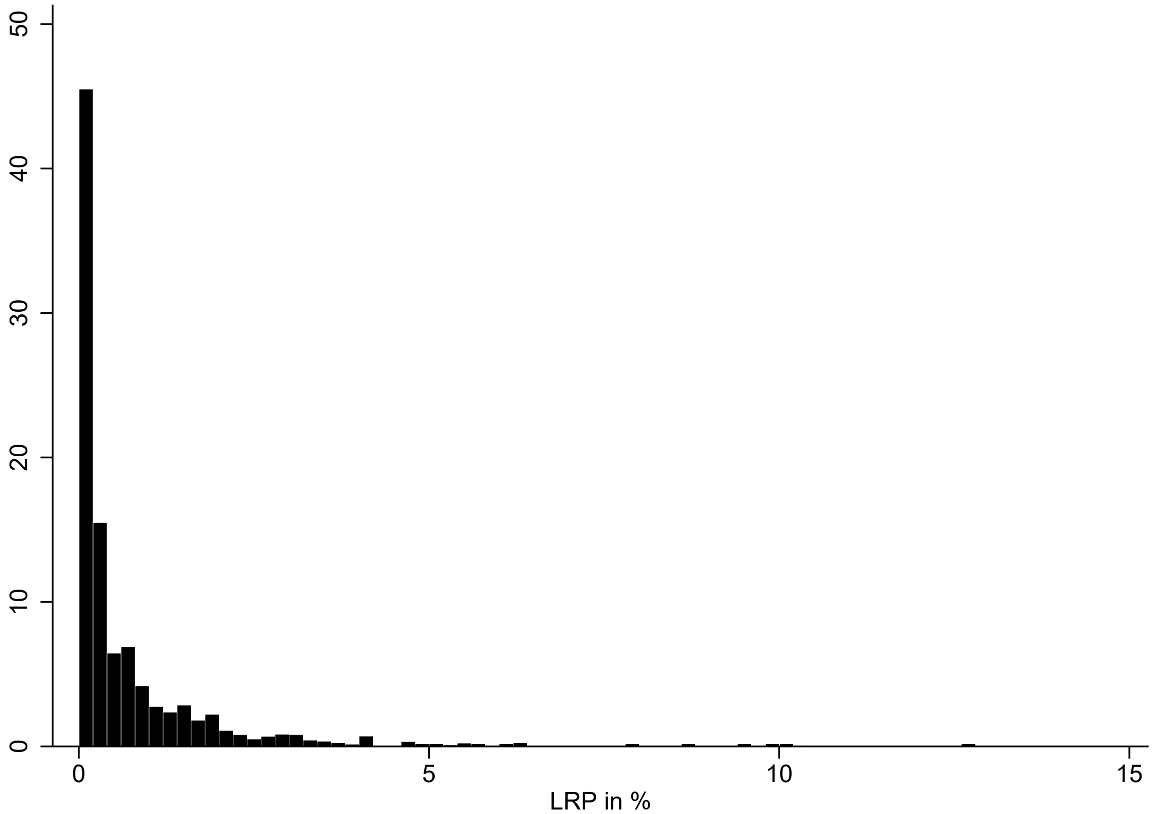
Note: Based on own calculations using the *Susenas* sample.

Figure A2: LRP Levels in 2001, 2007, 2010 and 2016



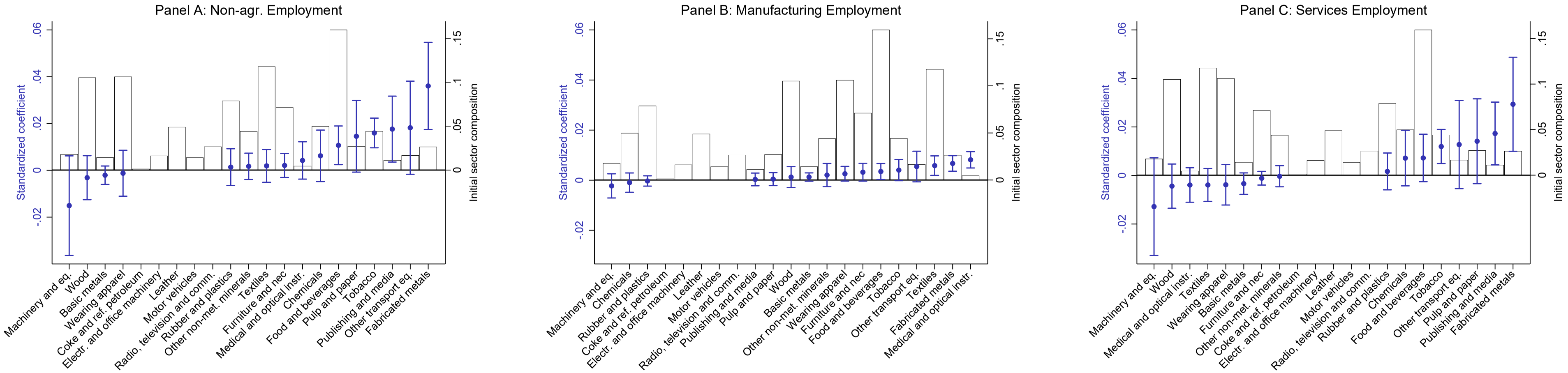
Note: District borders are from 2000. Values are multiplied by factor 100.

Figure A3: Density Distribution of Local Regulatory Penetration (LRP)



Note: Values are multiplied by factor 100.

Figure A4: Sectoral Decomposition of the Impact of LRP on Employment



Note: Panel A shows the effect on total employment rates by two-digit sector, while Panels B and C further disentangle the effect on manufacturing and service employment. Bars depict the share of initial employment in each two-digit sector. Plotted coefficients estimate the standardized sectoral contribution to the overall effect of ΔLRP on the change in employment rate. No coefficients are estimated for 6 unregulated sectors (compare table A4). The regression controls for the initial level of LRP before the first revision and the initial share of manufacturing, agricultural and service employment, as well as island indicators. Bars around the point estimates denote 90% confidence intervals for standard errors robustly estimated.

A.2 Additional tables

Table A1: Predictors of 5-Digit Product-Level Regulatory Penetration (Genthner and Kis-Katos 2019)

Variable	Change in share of regulated firms ($t-1$ to t, sales weighted)		
	Coefficient	CDF (non-normal distribution)	Cluster
Change in share of state-owned firms ($t-6$ to $t-1$)	-0.046	0.96	State ownership/privatization
Growth rate of capital-labor ratio ($t-6$ to $t-1$)	0.003	0.96	Productivity dynamics
Share of medium-sized firms ($t-1$)	-0.020	0.94	Firm size/concentration
Share of state-owned firms ($t-1$)	0.019	0.88	State ownership/privatization
Average productivity of state-owned firms ($t-1$)	-0.003	0.87	State ownership/privatization
Log of average firm sales ($t-1$)	0.001	0.84	Firm size/concentration
Change in share of exports in total sales ($t-6$ to $t-1$)	-0.012	0.83	Internationalization
Growth rate of average firm sales ($t-6$ to $t-1$)	0.002	0.82	Productivity dynamics
Growth rate of capital intensity ($t-6$ to $t-1$)	0.004	0.82	Productivity dynamics
Herfindahl concentration index of sales ($t-1$)	0.006	0.79	Firm size/concentration

Note: The table includes the 10 product-level characteristics with the highest predictive power of regulation, together with their estimated coefficient, the value of the CDF under the non-normality assumption (see Sala-i-Martin 1997) and their respective thematic cluster. Factors are selected based on five-digit product-level regressions of the change in the average regulation share on triplets of explanatory variables.

Table A2: Summary Statistics of First-Difference Sample

	1996			2006			2016			Δ 2006-2016		
	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N
Survei Industri variables:												
LRP	.	.	0	0.18	0.54	291	0.80	1.29	291	0.62	1.00	291
Economic Census variables:												
Non-agr. employment rate	0.25	0.09	291	0.35	0.09	291	0.41	0.14	291	0.06	0.09	291
in manufacturing	0.08	0.05	291	0.07	0.05	291	0.10	0.06	291	0.02	0.05	291
in services	0.17	0.06	291	0.28	0.07	291	0.32	0.12	291	0.04	0.08	291
asinh(Employment per firm)	1.93	0.34	291	2.15	0.28	291	2.50	0.71	291	0.12	0.17	291
in manufacturing	4.42	5.65	291	4.83	15.73	287	3.64	2.96	291	-0.05	0.46	287
in services	1.67	0.21	291	2.02	0.25	291	2.34	0.61	291	0.12	0.16	291
asinh(Number of firms)	11.21	1.02	291	11.56	0.90	291	11.78	0.84	291	0.22	0.17	291
in manufacturing	9.33	1.27	291	9.32	1.77	291	9.96	0.98	291	0.63	1.17	291
in services	10.98	1.04	291	11.39	0.90	291	11.58	0.84	291	0.19	0.17	291
Susenas variables:												
Manuf. employment in total emp.	0.10	0.08	291	0.11	0.08	291						
Urban population rate	0.35	0.31	291	0.42	0.32	291						

Note: LRP is multiplied by factor 100.

Table A3: Summary Statistics of District-Level Panel

	All			2000			2016		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Survei Industri variables:									
LRP	0.53	1.22	5,687	0.21	0.74	334	0.84	1.55	341
Susenas variables:									
Total employment rate	0.66	0.08	5,687	0.63	0.09	334	0.68	0.07	341
asinh(Monthly expenditure per capita)	11.87	0.38	5,687	11.54	0.31	334	12.11	0.34	341
asinh(Monthly expenditure per capita, p5)	10.98	0.29	5,687	10.78	0.30	334	11.05	0.30	341
asinh(Monthly expenditure per capita, p10)	11.12	0.30	5,687	10.91	0.29	334	11.19	0.29	341
Poverty rate	0.16	0.13	5,687	0.30	0.19	334	0.11	0.09	341
asinh(Population size)	13.26	0.91	5,687	13.15	0.96	334	13.42	0.87	341
Immigration rate	0.04	0.03	1,701	.	.	0	.	.	0
education $\leq$ 9 years	0.03	0.03	1,701	.	.	0	.	.	0
education $>$ 9 years	0.06	0.04	1,701	.	.	0	.	.	0
age 15-24 years	0.06	0.06	1,701	.	.	0	.	.	0
age $\geq$ 25 years	0.04	0.02	1,701	.	.	0	.	.	0
Urban population rate (2000-05)	0.40	0.32	5,687	0.40	0.32	334	0.40	0.32	341
Manuf. employment in total emp. (2000-05)	0.10	0.08	5,687	0.10	0.08	334	0.10	0.08	341
Sakernas & DAPOER variables:									
Log wage premia	8.33	0.27	5,249	8.26	0.25	282	.	.	0
asinh(Public expenditure p.c.)	8.48	0.93	5,198	.	.	0	9.36	0.61	336

Note: LRP is multiplied by factor 100. Working-age population is defined as all individuals between the age of 15 and 64.

Table A4: Sectoral Composition of LRP in Selected Years

2-digit manufacturing sector	Contribution to LRP in		
	2001	2007	2016
Food products and beverages	0.004	0.105	0.159
Tobacco products	0.000	0.077	0.096
Textiles	0.000	0.004	0.008
Wearing apparel	0.000	0.000	0.076
Leather and leather products	0.000	0.000	0.000
Wood and wood products	0.153	0.197	0.336
Pulp, paper and paper products	0.037	0.039	0.039
Publishing, printing and media	0.000	0.006	0.006
Coke, refined petroleum products	0.000	0.000	0.000
Chemicals and chemical products	0.013	0.031	0.025
Rubber and plastics products	0.000	0.001	0.040
Other non-metallic mineral products	0.000	0.031	0.007
Basic metals	0.000	0.002	0.002
Fabricated metal products	0.000	0.005	0.005
Machinery and equipment	0.000	0.004	0.004
Electrical equipment, office machinery	0.000	0.000	0.000
Radio, television and communication equipment	0.000	0.000	0.000
Medical, precision and optical instruments	0.000	0.000	0.003
Motor vehicles	0.000	0.000	0.000
Other transport equipment	0.000	0.012	0.015
Furniture and n.e.c.	0.000	0.018	0.016
Local regulatory penetration	0.207	0.531	0.835

Note: Columns show the contribution of sectoral regulation to total LRP in respective years. Values are multiplied by factor 100.

Table A5: Summary Statistics of Districts per Product and Products per District

	Mean	5%	25%	Median	75%	95%
Number of products per district	20.6	1	4	10	26	96
Number of regulated products per district	6.6	0	2	4	9	24
Number of reg. shifts per district	5.6	0	1	4	8	19
Number of districts per product	19.9	1	5	13	26	66

Note: Numbers are based on aggregation of the full sample and show the average number of products per district, as well as the average number of districts hosting the same product.

Table A6: Placebo Effect of Local Regulatory Penetration on Employment by Sector, Firm size (Economic Census)

Dep.var.: ΔEmployment rate 1996-2006	Total	Manufacturing	Services
	(1)	(2)	(3)
Panel A: All			
ΔLRP 2006-2016	-0.0005	-0.0042**	0.0037
	(0.0046)	(0.0020)	(0.0039)
Panel B: Microfirms (1 employee)			
ΔLRP 2006-2016	0.0022	0.0006	0.0017
	(0.0015)	(0.0006)	(0.0014)
Panel C: Small firms (2-19 employees)			
ΔLRP 2006-2016	-0.0001	-0.0027*	0.0026
	(0.0031)	(0.0014)	(0.0028)
Panel D: Medium/Large firms (20+ employees)			
ΔLRP 2006-2016	-0.0026	-0.0021	-0.0005
	(0.0020)	(0.0014)	(0.0009)
Observations	291	291	291
Island FE	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓

Note: The dependent variable is the change in employment rates. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district), all measured in 1996 or 1997. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A7: Standard Errors and Rejection Rate of $H_0 : \beta = 0$ at 5% Significance Level (Adão et al. 2019)

	Estimate		Median std. error	Rejection rate
	Mean (1)	Std. deviation (2)	(3)	(4)
Panel A: Economic Census				
Non-agr. employment rate	-0.00006	0.00817	0.00719	8.91%
Manufacturing	-0.00002	0.00365	0.00283	10.59%
Services	-0.00004	0.00669	0.00606	4.15%
Panel B: Susenas				
Total employment rate	0.00002	0.00069	0.00065	6.84%

Note: Panels A and B present results in the Economic Census and the Susenas data, respectively. The dependent variable is indicated in the row header. Columns 1 and 2 show the mean and standard deviation of the OLS estimates of β_1 in equations (2) or (3) across the placebo samples, while column 3 indicates the median standard error estimates. Column 4 indicates the percentage of placebo samples for which we reject the null hypothesis $H_0 : \beta = 0$ using a 5% significance level test. Standard errors are clustered at the district level. Results are based on 10,000 placebo samples.

Table A8: Robustness Checks According to Shift-Share Literature

	Baseline	Initial LRP cluster	Exclude Media	Exclude Transport Eq	Exclude Metals
	(1)	(2)	(3)	(4)	(5)
Panel A: Economic Census (Δ Employment Rate)					
ΔLRP 2006-2016	0.0097**	0.0097*	0.0091*	0.0090*	0.0087*
	(0.0048)	(0.0051)	(0.0049)	(0.0048)	(0.0050)
Observations	291	291	291	291	291
Island FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓	✓	✓
Panel B: Susenas (Employment Rate)					
LRP	0.0019*	0.0019**	0.0019*	0.0019*	0.0019*
	(0.0011)	(0.0009)	(0.0011)	(0.0011)	(0.0011)
Observations	5687	5687	5687	5687	5687
District, Year FE	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. Column 1 reproduces the main results of Panel A in table 1 and column 4 in table 4. Column 2 groups districts based on percentiles in the initial distribution of LRP (resulting in 55 clusters), and columns 3-5 exclude publishing and media, other transport equipment and fabricated metals from LRP, respectively. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district). Standard errors are clustered on district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A9: Robustness Checks: Political-Economy Factors

	HI sales	HI labor	Nat. champs	State-owned	Privatized	Min. wage	Splits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Economic Census (ΔEmployment rate)							
ΔLRP 2006-2016	0.0094*	0.0095**	0.0088*	0.0096**	0.0086*	0.0082*	0.0096**
	(0.0048)	(0.0048)	(0.0049)	(0.0048)	(0.0050)	(0.0047)	(0.0048)
Observations	291	291	291	291	291	291	291
Island FE	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓	✓	✓	✓	✓
Panel B: Susenas (Employment rate)							
LRP	0.0019*	0.0018	0.0019*	0.0017	0.0019*	0.0019*	0.0020*
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Observations	5673	5673	5673	5673	5673	5668	5687
District, Year FE	✓	✓	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓	✓	✓
$\mathbf{Z}_{d,0}$(-specific trends)	✓	✓	✓	✓	✓		
Control for						✓	✓

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district). Columns 1 and 2 extent the set of initial conditions by a Herfindahl index in sales and employment, respectively. Column 3 adds trends in the initial prevalence of employment in national champion firms. Column 4 adds trends in the initial share of employment in state-owned enterprises, while column 5 adds trends in the employment share of firms (present in 2005) that were privatized between 2001 and 200 all calculated from the *Survei Industri*. Column 6 controls for changes in minimum wage legislation and column 7 controls for district splits. Standard errors are robustly estimated (and clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A10: Robustness checks: Global Dynamics

	Initial trade	Trade flows	Tariffs	Automation	High tech
	(1)	(2)	(3)	(4)	(5)
Panel A: Economic Census (Δ Employment rate)					
ΔLRP 2006-2016	0.0101**	0.0100**	0.0103**	0.0096**	0.0099**
	(0.0049)	(0.0048)	(0.0048)	(0.0048)	(0.0048)
Observations	291	291	291	291	291
Island FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓	✓	✓
Panel B: Susenas (Employment rate)					
LRP	0.0020*	0.0019*	0.0018	0.0016	0.0019*
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Observations	5673	5687	5320	5346	5673
District, Year FE	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓
$\mathbf{Z}_{d,0}$(-specific trends)	✓				✓
Control for		✓	✓	✓	

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of initial labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district). Column 1 extends the set of initial conditions by import and export volume. Columns 2 controls for trade flows by including time-variant import and export figures. Column 3 includes input and output tariffs as well as the share of employment affected by non-tariff measures. Column 4 controls for the stock of industrial robots in a district. Column 5 adds the employment share of high-technology firms according to OECD classification. Standard errors are robustly estimated (clustered at the district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A11: Robustness: Agglomeration Effects

	Never reg. L	Industrial area	Pop. density	Chg. pop. density	Dist JKT
	(1)	(2)	(3)	(4)	(5)
Panel A: Economic Census (Δ Employment rate)					
ΔLRP 2006-2016	0.0069	0.0079	0.0089*	0.0093*	0.0087*
	(0.0050)	(0.0048)	(0.0047)	(0.0048)	(0.0050)
Observations	291	291	291	291	291
Island FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓	✓	✓
Panel B: Susenas (Employment rate)					
LRP	0.0020*	0.0020*	0.0019*	0.0019*	0.0024**
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Observations	5687	5687	5687	5687	5687
District, Year FE	✓	✓	✓	✓	✓
Island-by-Year FE	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$-specific trends	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$-specific trends	✓	✓	✓	✓	✓
$\mathbf{Z}_{d,0}$(-specific trends)	✓	✓	✓	✓	✓

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ is the level of LRP in 2001, and $\mathbf{LaborMarket}_{d,0}$ is a vector of intial labor market characteristics (share of employment in the working-age population, employment share of the manufacturing sector, and share of urban population in a district). Column 1 extends the set of initial conditions by the initial share of never regulated product employment. Column 2 adds the share of employment in industrial areas (based on SI). Column 3 includes the initial population density for each district, while column 4 adds the change in population density between 2000 and 2005. Column 5 controls for trends by distance to Jakarta. Standard errors are robustly estimated (and clustered at the district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A12: Robustness: Spatial Spillovers

Sample:	Economic Census	Susenas
	(1)	(2)
Local regulatory penetration	0.0096**	0.0020*
	(0.0049)	(0.0011)
Spatial regulatory spillover	0.0487	-0.0223
	(0.1368)	(0.0448)
Observations	291	5687

Note: The dependent variable is the first difference in the non-agricultural employment rate (col. 1) and the total employment rate (col. 2). Local regulatory penetration is ΔLRP 2006-2016 in col. 1 and LRP in col. 2. Each regression controls for for district and island-year fixed effects and for trends by initial conditions as specified in cols. 4 of table 1 and of table 4, respectively. Spatial spillovers are calculated as total sum of LRP, weighted by the squared inverse distance. Robust standard errors are clustered at the district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A13: Robustness: Economy-wide Regulatory Penetration

Dep.var.:	Non-agr	Manufacturing	Services	Total (*Sus*)
	(1)	(2)	(3)	(4)
Panel A: Manufacturing LRP				
Local regulatory penetration	0.0097**	0.0065***	0.0032	0.0017
	(0.0047)	(0.0024)	(0.0049)	(0.0011)
Panel B: Non-agricultural LRP				
Local regulatory penetration	-0.0028	0.0041*	-0.0068	0.0007
	(0.0068)	(0.0021)	(0.0065)	(0.0006)
Observations	291	291	291	5,687

Note: The dependent variable is the change in employment rates between 2006 and 2016 in cols. 1-3 and the total employment rate in col. 4. Local regulatory penetration is ΔLRP 2006-2016 in cols. 1-3 and LRP in col. 4. Each regression controls for district and island-year fixed effects and for trends by initial conditions as specified in cols. 4 of table 1 and of table 4, respectively. Manufacturing LRP (Panel A) and Non-agricultural LRP (Panel B) are generated using the *Economic Census* from 2006. Robust standard errors are clustered at the district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A14: Disaggregating Manufacturing Sector Employment Gains (Medium/Large Firms)

Dep.var.: ΔEmployment rate 2006-2016	All Manufacturing	Ever Regulated	Never Regulated
	(1)	(2)	(3)
ΔLRP 2006-2016	0.0029*	0.0016	0.0013
	(0.0015)	(0.0014)	(0.0008)
Observations	291	291	291
Empl. rate (2006)	0.0130	0.0073	0.0057
Island FE	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓

Note: The dependent variable is the change in employment rates between 2006 and 2016 in each of the 2-digit industries. Each regression controls for district and island-year fixed effects and for trends by initial conditions. Robust standard errors are clustered at the district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A15: Disaggregating Service Sector Employment Gains

	Construction	Trade	Transp. & Comm.	Restauration	Finance	Real Estate	Education	Social services	Others
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ΔLRP 2006-2016	0.0035*	-0.0002	0.0003	0.0002	-0.0002	0.0000	-0.0006	-0.0002	0.0008**
	(0.0019)	(0.0026)	(0.0010)	(0.0011)	(0.0004)	(0.0006)	(0.0005)	(0.0003)	(0.0004)
Observations	291	291	291	291	291	291	291	291	291
Empl. rate (2006)	0.0088	0.1424	0.0244	0.0347	0.0031	0.0097	0.0322	0.0063	0.0140
Island FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LRP}_{d,0}$	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\mathbf{LaborMarket}_{d,0}$	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: The dependent variable is the change in employment rates between 2006 and 2016 in each of the 2-digit industries. Each regression controls for district and island-year fixed effects and for trends by initial conditions. Robust standard errors are clustered at the district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).