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Local labor market effects of FDI regulation in Indonesia*

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Abstract

Using yearly Indonesian labor market data for 2000 to 2015, we investigate the impact of a protectionist foreign direct investment (FDI) policy reform on employment and wages. The so-called negative investment list regulates FDI at the highly granular product level and has been repeatedly revised throughout time. We construct spatial measures of regulatory penetration based on firm-level data and thereby exploit the exposure of local manufacturing industry employment to the negative investment list. Controlling for time and locality fixed effects as well as trends in initial district conditions, our findings suggest an overall positive effect of local regulatory penetration on employment, which is especially pronounced among young, females and low-skilled workers and mostly driven by job creation in the manufacturing sector. We also present evidence in support of positive wage effects.

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

Keywords: FDI regulation, Indonesia, local labor markets

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1 Introduction

Over the past few years, economic protectionism has been globally on the rise, partially reversing earlier achievements of bringing down trade barriers that often required long and cumbersome negotiations. This protectionist backlash did not only result in partial tariff increases (like in the case of the US-Chinese trade war) but also in the proliferation of various non-tariff barriers. For instance, in the 2000s, a so-called negative investment list has been used to restrict foreign direct investment (FDI) inflows into selected national champion sectors in Indonesia (Genthner and Kis-Katos 2019). Such protectionist actions have often been based on economic arguments, like the need to safeguard domestic labor markets. Whether they really achieve this goal remains an open question.

The distributional effects of liberalizing trade have been in the focus of a very rich strand of literature that links tariff reductions to firm-level outcomes in developing and industrialized countries (cf. Amiti and Konings 2007, Lileeva and Treffer 2010, Topalova and Khandelwal 2011, Amiti and Cameron 2012, Arnold et al. 2016). The recurring result of these studies is that lower tariffs on either input or output products stimulate firm productivity, also leading to reduced wage skill premia in developing countries. In a similar way, studies also link tariff reductions to labor market outcomes on industry or locality level and find overall negative labor market effects from output trade liberalization.¹ A second extensive strand of literature documents a positive relationship between the presence of FDI and firm productivity (Blalock and Gertler 2008, Fons-Rosen et al. 2014, Eppinger and Ma 2017, Javorcik and Poelhekke 2017). Productivity increases are not exclusive to firms directly receiving FDI, but also transmit indirectly to domestic firms through spillover effects along the value chain. A third, substantially smaller, strand of the literature has focused on the opposite direction, investigating the impact of protectionism on firms (Bourlès et al. 2013, Duggan et al. 2013, Genthner and Kis-Katos 2019). Up to date, we are not aware of any study that relates FDI de-liberalization to labor market outcomes.

In this paper we aim to link a particular protectionist FDI regulation policy to local labor market outcomes in Indonesia. The negative investment list is released in the form of Presidential Decrees and contains information on five-digit products that are subject to FDI inflow restrictions. Its conditions vary in intensity and range from soft licensing requirements to hard investment bans for some products. Some of the restrictions are conditional on firm characteristics such as location, size and legal status. The list was first released in 2000 and then revised several times over the later years. Most importantly

¹See Goldberg and Pavcnik (2005) or Hakobyan and McLaren (2016) for industry-level evidence, and Autor et al. (2013) and Dix-Carneiro and Kovak (2017) for locality-level evidence on output trade liberalization. By contrast, Kis-Katos and Sparrow (2015) and Kis-Katos et al. (2018) show positive labor market consequences of input trade liberalization in Indonesian regions.

for our analysis, the regulatory environment has been strongly tightened in 2007, when the list was massively extended and plenty of new products were added. A second, revision in 2010 changed the range of products, also altering some of the restrictions, while later revisions (in 2014 and 2016) have de-regulated the investment regime to some extent. This has induced substantial spatial variation in the strictness of the locally relevant FDI policy environment over time, which our empirical analysis exploits.

For our analysis, we assess the local regulatory penetration (LRP) of this policy by combining policy information from the Presidential Decrees with firm-level and labor market data. We use a shift-share approach, interacting the initial share of the potentially directly affected local labor force with regulatory shifts over time. We then regress local labor market outcomes on the time-variant district-level LRP measure while controlling for a rich set of fixed effects and time trends in initial district conditions.

One potential reason behind the lack of studies on the link between protectionist policies and labor market outcomes lies in the tremendous difficulties of a clean and convincing identification strategy. In terms of trade liberalization, many studies argue that the latter is dictated by international organizations such as the International Monetary Fund (IMF) for India (Topalova 2010) or accession to the World Trade Organization (WTO) for Indonesia (Kis-Katos and Sparrow 2015). Similarly, Autor et al. (2013) exploit the rapid rise of China after its WTO accession to estimate its impact on US labor markets. These studies can convincingly argue in favor of causal effects since they are exploiting sudden trade policy changes that are determined by the initial levels of sectoral protection. When dealing with variations in FDI policy or other non-tariff barriers in general, the line of argumentation is substantially less straightforward. Policy makers react to changes in the economic environment and, thus, estimated coefficients are not only driven by the policy response but may also reflect other underlying location-specific economic dynamics that may have triggered the policy intervention in the first place.

In the case of the negative investment list in Indonesia, Genthner and Kis-Katos (2019) indeed show that there is a whole range of political economy factors that potentially explain the choice of protected sectors. However, their results also indicate that past labor market dynamics barely figure among the factors explaining the product-level targeting of regulation. Instead, there is plenty of evidence that the sectoral presence and productivity of public enterprises has shaped policy decisions. In order to alleviate concerns that endogenous policy formation and omitted variables are driving our results, we carefully check for pre-trends in the main outcomes using a long-difference approach first. We then control for the initial levels in regulatory penetration and the dependent variable in our baseline panel specification and test for the robustness of our results by including a rich set of political economy factors.

Our results indicate that employment in more strongly protected districts increases on

average. These findings are in line with regulation acting as a positive labor demand shock. Regulated firms deal with a reduced supply of foreign capital by increasing their demand for labor. This results in substantial aggregate local labor market effects of regulation. While formal manufacturing employment in medium and large scale enterprises comprises only a very minor share of the local labor force, regulation also results in substantial local labor demand multipliers. We also present some evidence for wage gains among manufacturing and service sector workers in districts with higher regulatory penetration which highlights the importance of sectoral spillovers. The earnings effect is especially pronounced among female and high-skilled workers. Our results are in line with previous studies on developing and transition economies, which focus on negative demand shocks due to trade liberalization. These studies show that workers in industries or regions highly affected by trade liberalization often bear the adjustment shocks through facing diminishing earnings or job losses in the short run (see for example Arbache et al. (2004) and Kovak (2013) on Brazil), but even in the long run (Dix-Carneiro and Kovak 2017).

The paper proceeds as follows. Section 2 introduces the institutional background of FDI regulation and the NIL in Indonesia. Next, section 3 discusses the data in use and defines our measure of local regulatory penetration. In section 4, we describe the estimation strategy, point out potential threats to identification and discuss how the paper deals with them. Section 5 then presents our results, including a set of robustness checks and alternative labor market outcomes. We finally conclude in section 6.

2 Institutional background

Early steps towards opening the Indonesian economy to FDI already started in the first years after the end of the Sukarno regime. Both the Foreign Investment Law in 1967 and the constitution of the investment coordination board (*Badan Koordinasi Penanaman Modal*, BKPM) in 1973 were landmark reforms as they promoted more FDI and enabled potential investors to apply for investment projects at a central agency (Gammeltoft and Tarmidi 2013). The relaxations of the previously tight investment environment, however, were partially withdrawn at the beginning of the 1970s when the Indonesian government finally succumbed to violent protests against foreign presence in particular industries (van Zanden and Marks 2012). Despite the resulting drop in FDI inflows after the implementation of further restrictions, the Indonesian economy as a whole did not suffer much as the ongoing oil boom ensured sufficient revenues to compensate for the lost FDI inflows. When oil prices collapsed in the 1980s, the government was forced to react and started to open the economy to FDI again (van Zanden and Marks 2012). Major reforms in the 1990s converted Indonesia into “one of the most promising countries [for investment]” (Lindblad 2015, p. 225).

Increasing inflows of FDI came to a sudden halt during the Asian financial crisis in 1997 that destroyed much of the investors' confidence (WTO 1998). To restore its status as 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 (WTO 2013). In contrast to these efforts of promoting FDI, however, the president also introduced a so-called negative investment list (*Daftar negatif investasi*, NIL) in 2000, which listed products that are entirely closed or only conditionally open to FDI.² In the NIL 2000, conditions include licensing requirements or the prerequisite to form a joint venture with a domestic enterprise. While the release of such a blacklist improved the transparency of previously unclear procedures (WTO 2013), it also constitutes a protectionist policy. Thus, Indonesian FDI policy remained "blurred by contradictory signals" (Lindblad 2015, p. 229).

The list was repeatedly updated over the years. The first revision took place in 2007 (with the Presidential Decree 77/2007) that extended its products coverage substantially, leading to a more restrictive regulatory environment. The NIL 2007 also widened the scope of potential conditions to investment. FDI may still be entirely prohibited in some products, while others allow FDI only in small- and medium-sized enterprises, in partnerships, below a certain threshold of foreign capital participation, in particular provinces, or only with licensing permission by the ministry in charge.

Figure 1 shows the evolution of our measure for LRP (as described below in section 3) over time. While regulation levels were low between 2000 and 2006, regulatory penetration rose steeply in 2007. After some minor adjustments at the beginning of 2008 (by the amendment 111/2007), the next major revision took place in 2010 (within the Presidential Decree 36/2010). Changes in product composition and the applied conditions has lead to another strong increase in the LRP measure. Further revisions (by Presidential Decrees 39/2014 and 44/2016) turned out relatively minor in comparison.³ A more detailed description of the NIL and its conditions and coverage is given in Genthner and Kis-Katos (2019).

²This first version of the NIL (released with the Presidential Decree 96/2000) describes regulated products only verbally. Nonetheless, they can be easily linked to the Indonesian sector classification KBLI. The KBLI (*Klasifikasi Baku Lapangan Usaha*) sector classification is published by BPS (Indonesian Statistical Office, *Badan Pusat Statistik*). It is equivalent to the United Nation's International Standard Industrial Classification of All Economic Activities (ISIC) at the four-digit level, but it is adjusted to five-digit level in order to distinguish between additional Indonesian sectors of local importance. Throughout this paper, we will refer to five-digit KBLI coding level as products, while two-digit (three-digit) will be called sectors (industries).

³One 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. For instance, see article 8 in Presidential Decree 36/2010.

3 Data

3.1 Labor market data

We derive local labor market outcomes from two surveys, the national household survey (*Survei Sosial Ekonomi Nasional, Susenas*) and the national labor force survey (*Survei Angkatan Kerja Nasional, Sakernas*), both collected by BPS on an annual basis as repeated cross-sections. *Susenas* is our primary data source as it is representative for the Indonesian population on district level over the full analyzed time period (from 2001 to 2015), which also allows us to analyze labor market dynamics in the years before the regulatory change. Since *Susenas* only records employment status and sector affiliation consistently over time, we complement it with information from the labor force survey. *Sakernas* includes, besides employment status and affiliation, also a rich set of individual labor market information such as labor market participation, working hours and earnings. Unfortunately, the labor market survey is not necessarily representative at the district level before 2007. However, our baseline results for employment show similar results irrespective of using *Susenas* or *Sakernas*.

In both samples, we restrict our attention to the working-age population (between the age of 15 and 64) and eliminate observations with missing values in important characteristics such as gender, educational attainment or age. Table A1 reports summary statistics based on the two sources, also contrasting two samples of *Sakernas* that either include all individuals or only full-time workers (working at least 30 hours per week). *Susenas* includes about 9.7 million observations, *Sakernas* about 5.8 million observations, of which 1.7 million full-time workers with non-missing earnings information are used for estimating regional wage premia. Overall, about half of the sample is female and lives in urban areas and the majority can be considered as low-skilled (having completed junior high school or less). About 65% of the working-age population are employed, out of which every second worker is employed in the service sector and every eight worker in manufacturing. By contrast, the sample of full-time workers in *Sakernas* is predominantly male, more urban, and considerably better educated. Full-time workers earn on average 8267 Rupiah per hour (deflated to 2008), which amounts to about 0.6 USD.

We measure local labor market outcomes by aggregating both the *Susenas* and the *Sakernas* sample to district-year level and deriving total employment and working-age population numbers.⁴ We also separately aggregate those figures by gender and skill level. For the *Sakernas* sample, we additionally calculate the total active population, as well as total weekly working hours and working hours per worker.

In addition, we use the non-zero wage sample of full-time employees from *Sakernas* to es-

⁴Due to an ongoing decentralization process, Indonesian districts repeatedly split over our sample period. To deal with changing district borders, we focus on initial district borders in 2000.

timate district-specific log wage premia, by running the following Mincer wage regressions separately by year:

$$\text{asinh}(Wage)_{ijdt} = \sum_d (\alpha_{1,d} \times D_{dt}) + \mathbf{W}'_{ijdt} \alpha_2 + \theta_j + \epsilon_{ijdt}, \quad (1)$$

where $\text{asinh}(Wage)_{ijdt}$ is the inverse hyperbolic sine of the hourly wage earned by individual i working in three-digit industry j in district d in year t . \mathbf{W}_{ijdt} includes individual level controls like education, gender, age as proxy for work experience as well as marital status, and θ_j adds industry fixed effects. We additionally include 341 indicator variables, one for each district d and extract the local wage premia estimates as the predicted values \hat{D}_{dt} (see e.g. Dix-Carneiro and Kovak 2017).

Figure 2 presents the spatial variation in the estimated log wage premia for selected years, showing substantial variation both across locations and time. As the log wage premia are estimates, we will weight any regression using them as dependent variable by the inverse of the squared standard error to correct for potentially imprecisely estimated district coefficients in equation 1.

3.2 Measuring local regulatory penetration

In our empirical models, we link changes in local labor market outcomes to regional level measures of the strictness of the regulatory environment by combining policy data from the Presidential Decrees with data from the annual manufacturing census (*Survei Industri*, SI) and *Sakernas*.⁵ In order to proxy for the extent of regulatory penetration within each Indonesian district, we construct locality-year-specific measures of local regulatory penetration LRP_{dt} in district d and year t :

$$LRP_{dt} = \sum_p \sum_k \frac{L_{kpd0}^f}{L_{d0}} REG_{kpd t}. \quad (2)$$

To isolate changes in local regulatory penetration, we apply a Bartik-style shift-share approach (Bartik 1991), interacting the initial share of potentially directly exposed labor force with the regulatory shifts over time. The initial shares divide firm employment L_{kpd0}^f by firms of type k operating within five-digit product group p and region d (derived from the firm census) by the initial size of the local labor force L_{d0} (estimated from labor market surveys). As regulation is specific to a selected list of firm characteristics, we calculate

⁵The *Survei Industri* comprises the whole universe of manufacturing firms 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). For exact details on data cleaning and the sample used see Genthner and Kis-Katos (2019).

initial employment shares not only by product p , but also by firm characteristics k .⁶

The initial time period $t = 0$ is based on the years 2000 to 2005, during which no regulatory changes occurred. Most importantly, the shares should not be affected by endogenous employment adjustment dynamics due to later reforms. By calculating the median number of employees for each firm for the whole period from 2000 to 2005, we increase the precision of our share estimates.⁷

The time-varying policy shifts are derived from the policy instrument of the NIL. The indicator variable $REG_{kpd,t}$ turns one if firms of type k that produce the primary product p and operate within region d are included on the blacklist in year t , and takes zero otherwise. All time variation in LRP_{dt} thus originates from revisions of the NIL. Revisions may extend (or shorten) the list by adding new products p (or removing existing ones). Additionally, $REG_{kpd,t}$ may also turn to one if regulation of product p is extended to include hitherto unregulated firms of type k . For instance, manufactured food products from soy and other beans were added to the list in 2007, making it only conditionally open to investment within partnerships. A further condition was imposed in 2010, additionally prohibiting investment into large enterprises (and only allowing it within small and medium-sized firms).

The average development of LRP over time is depicted in figure 1 and shows a step-wise increase in the overall regulatory penetration after each of the two major revisions (in 2007 and 2010). For our estimation strategy, we exploit the yearly change in LRP for each district while also using the total change between 2006 and 2010 in a long-difference pre-check. To ease interpretation, we multiply LRP by 100 to represent the percentage of local workers potentially directly exposed to FDI regulation. On average, LRP increases by 0.75 percentage points between 2006 and 2010. The rise in penetration is more or less equally split between the first (2006-2007) and second (2007-2010) revision (see table 1). Figure 3 further disentangles the contribution of each industry to total LRP over time. We therefore split up LRP into its sectoral components based on 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 drive LRP on a nation-wide scale (e.g. food and beverage industry, tobacco products or wearing apparel). A complete list of all industrial components can be found in table A2 of the appendix. Noteworthy, there are several industries which are not affected by the NIL at all, such as basic metals or motor vehicles.

Figure 4 presents the spatial distribution of the LRP levels for the years most relevant to

⁶These characteristics are firm size (regulation often only applies to big companies) and legal status (partnerships are often excluded from regulation). The range of locations d is only relevant for the regulatory restriction in very few cases where regulation only applies to particular provinces.

⁷This does not only increase the underlying number of firms but also makes our firm employment measures more robust against outliers.

our estimation strategy.⁸ Initial LRP levels have varied in 2006 substantially. In 2007, a major revision tightened the regulatory environment which is also visible in the graph by higher prevalence of darker shades. Looking at the change in LRP over time, figure 5 shows a tightening of the regulatory environment in most of the districts from 2006 to 2010. Declines in the LRP in the first round of revisions between 2006 and 2007 mainly occur on the islands of Sumatra and Kalimantan, while regulation tightens especially on Java. Between 2007 and 2010, LRP further increases in about half of all districts, while other districts experience declines in regulation at the same time. It is exactly this spatial and temporal variation that we use in our identification.

3.3 Employment dynamics and summary statistics

Figures 6 and 7 depict descriptive trends in labor market outcomes over time. Figure 6 shows aggregate trends in working-age population and active labor force, both of which have been increasing over time. To the extent that population dynamics are heterogeneous across districts, this may also drive employment effects. We will check for this in section 5 and also control for population growth in all our specifications. Additionally, figure 7 plots total employment over time, as well as employment in the three major sectors of agriculture, manufacturing and services. The left (right) panel is based on *Susenas* (*Sakernas*) data. Both panels reveal an overall increase in total employment over time. Most new jobs are created in services while agricultural employment is mostly stagnating (and hence declining in relative terms). The share of manufacturing workers in total workforce remains relatively small compared to the other two sectors despite the fact that manufacturing employment grows at a similar rate to that of services. Although the two data sources do not yield exactly the same estimates of sectoral composition, the overall trends mirror each other fairly well.⁹

We transform the individual-level data of repeated cross-sections into a district-year level dataset. Table 1 shows descriptive statistics. The number of observations is smaller within the *Sakernas* sample (especially for wage premia estimates). Note, however, that the mean of working-age population or total employment are fairly similar across both data source. Log wage premia are highest in the service sector while lowest in agriculture. The number of observations varies for sectoral wage premia as some smaller districts do not report individuals affiliated to each sector in each year.

⁸Figure A1 in the appendix shows the density distribution of LRP.

⁹Figure A2 in the appendix also replicates the same graph for employment rates in total working-age population. The overall message remains the same.

4 Estimation strategy

For our main results, we link the constructed measure of LRP to local labor market outcomes y_{dt} such as total or sectoral district employment. We thereby exploit yearly variation in regulatory penetration LRP_{dt} on district level. In particular, our panel regressions take the form:

$$y_{dt} = \beta_1 \times LRP_{dt} + \mathbf{X}'_{dt}\beta_2 + \mathbf{Z}'_{d0} \times t + \gamma_d + \phi_{rt} + \varepsilon_{dt}, \quad (3)$$

where \mathbf{X}_{dt} are further time-varying local-level controls. In particular, we control for contemporaneous working-age population to alleviate concerns that employment results are driven by population dynamics.¹⁰ Moreover, γ_d are district fixed effects and ϕ_{rt} are island-year fixed effects while ε_{dt} is the error term that is clustered at the province level. We weight regressions using wage premia as the dependent variable by the inverse of the squared standard error of wage premia estimates to correct for heteroskedasticity.

We control for a wide range of initial local conditions within the vector \mathbf{Z}_{d0} and interact it with a linear time trend. These initial district-level characteristics may both drive differences in regulatory exposure and labor market dynamics. Thereby, we allow districts to be on different trajectories depending on their initial situation. For our baseline specification, we include linear trends in the initial levels of regulatory penetration and the dependent variable, both measured in 2005. Additionally, as the LRP variable relates the number of regulated manufacturing workers to the total active population, the calculated shares do not add up to one but reflect the relative importance of manufacturing in local labor markets. We thus also allow for linear trends of the initial share of manufacturing employment in the total working-age population (cf. Borusyak et al. 2018).

Our robustness checks in section 5 complement \mathbf{Z}_{d0} with additional factors that potentially explain changes in LRP and labor market outcomes at the same time. In particular, we alleviate concerns that our results are driven by the global financial crisis or trade, political economy factors such as lobbyism or privatization, automation as well as labor market reforms. We further show that our results remain robust when controlling for urbanization rate and measures of agglomeration. A more detailed discussion of the rationale behind the robustness checks is given in the respective subsections.

Our identification strategy further requires the absence of pre-trends in employment conditional on our baseline controls. To check for pre-trends, we start the results part with long-difference estimations showing the time profile of the regulatory effect.

¹⁰Section 5 shows that changes in LRP are indeed positively correlated with population dynamics. In the baseline panel result, the LRP coefficient remains robust after controlling for district population but it also strongly declines in magnitude.

5 Results

5.1 Long-difference estimations

Before we continue with our baseline specification from equation 3, we present two results from long-difference estimations in graphical form in figures 8 and 9. We thereby run yearly regression of changes in district population and employment relative to the base year 2006 on the change in each district’s regulatory penetration between 2006 and 2010 and report the yearly coefficients together with their 90% confidence interval. The time period 2006 to 2010 covers both the initial revision of the NIL in 2007 and the subsequent adjustment of products on the list in 2010.¹¹ To test for pre-trends, we run an additional set of regressions relative to the baseline year 2001 while keeping the change in LRP constant at the period 2006 to 2010. Each regression further controls for the initial values of the dependent variable, LRP and the manufacturing share as well as island fixed effects.

As mentioned above, one concern is that our results are driven by underlying general population dynamics. In a first test, we therefore relate our LRP measure to total working-age population. Figure 6 already indicates that the size of working-age population is steadily increasing over time. In fact, figure 8 also shows that districts with increasing regulatory penetration experience stronger population growth at the same time. Though the coefficients do not reach conventional significance levels in most years, we still interpret this as strong indication to control for population dynamics.

In a next step, we anticipate our main result using the long-difference approach. Figure 9 presents the relationship between the change in LRP from 2006 to 2010 and total employment growth. We observe an immediate response of district employment due to the first revision of the NIL in 2007, even though the coefficients are not significant. The impact of regulation on employment starts to rise during the second revision of the NIL in 2010 and eventually turns significant. The positive effect of regulatory penetration on economy-wide employment levels off at about 0.007 and remains statistically significant until the end of the sample period. Furthermore, we do not detect any evidence of pre-trends before 2006 as none of the coefficients is significantly positive. If anything, districts with ex-post higher regulatory penetration exhibit employment losses between 2002 and 2005. As past privatization experiences are a strong predictor of product-level regulation (Genthner and Kis-Katos 2019), these early employment losses in more heavily regulated districts could partly reflect the privatization experience.

¹¹See figure 5 for a spatial distribution of the main explanatory variable.

5.2 Employment

Table 2 shows the main result of this paper. Further controls are added step-wise in each column, starting with the correlation between total employment growth and LRP in column 1 conditional on district and year fixed effects. Column 2 allows for island-year fixed effects, while column 3 further includes working-age population. Columns 4 adds linear time trends in initial district conditions in 2005 (LRP, total employment and the share of manufacturing employment) whereas column 5 even allows districts to be on different nonlinear trajectories by interacting initial conditions with year dummies. As a consequence, however, clustering is no longer feasible on province level as we lack a sufficient degrees of freedom. In the latter specification standard errors are therefore clustered on district level.

Like our long-run graphical results of figure 9, the coefficient of interest is positive and highly significant. It is heavily reduced in magnitude when we control for population dynamics in column 3, reflecting a high correlation between population and employment growth on district level. Allowing for linear or nonlinear time trends in initial conditions reduces the coefficient only slightly to 0.005 which also confirms the previous long-difference result. In terms of magnitude, a one standard deviation increase in LRP is associated with a 0.7 percent increase in overall employment. The size of the effect is economically meaningful. As manufacturing employment amounts to about 8% on average, and employment in large regulated firms relative to the initial local labor force is only 0.6% on average, employment increases of this magnitude are only feasible if protection has employment generating effects that go far beyond the regulated formal manufacturing firms.

5.3 Robustness checks

In this section, we perform various robustness checks that address endogeneity concerns driving our main result. Many of our controls come from aggregation of the SI firm census data. For initial district conditions of industrial characteristics, we follow the same procedure as for LRP and calculate the firm-level median values over the period 2000 to 2005. This takes care of potential outliers in the firm data and can be considered a conservative measurement. We then aggregate the initial firm-level values to district-level totals (or averages). As proxy for automation, we further add the average time-varying stock of industrial robots to our set of controls. Data on robot stocks comes from the International Federation of Robotics (IFR).¹² We weight yearly stocks in an industry by

¹²The International Federation of Robotics provides comprehensive data on the operational stock of robots by country, year and industry (International Federation of Robotics 2016). Note that the database reports zero stocks of operational robots until 2006. Thus, initial stocks of robots are redundant.

firm employment in the respective year to account for the labor force which is potentially affected by mechanization.

5.3.1 Global financial crisis and trade

Our baseline results cannot exclude the possibility that particular districts have been more severely affected by the global financial crisis in 2009 in relative terms. Despite the fact that Indonesia turned out to be relatively immune to the downturn of global trade and financing, it is still possible that this singular event is driving our result. For instance, districts which are more involved in global trade may experience smaller employment growth because firms in these regions had to downscale their employment in response to declining foreign demand. If policy makers refrained from regulating FDI in especially these districts, our results will suffer from omitted variable bias. The first two columns of table 3 therefore add the initial level of import and export volume by district and allow for either linear or nonlinear time effects. The effect of LRP on district employment growth is barely reduced and still highly significant. The result also remains robust if we control for initial average output and input tariffs as well as the share of industrial employment which is affected by non-tariff measures (NTMs).¹³ Thus, there is no evidence that our results are driven by the initial exposure to foreign competition or potential trade liberalization in ex-ante highly protected industries.

5.3.2 Automation and technological upgrading

Another concern may be that FDI regulation is especially pronounced within districts with relatively little potential of automation and technological upgrading. If this was the case, our measure of regulatory penetration would pick up the positive effect on employment relative to districts where industrial robots are introduced and technological upgrading leads to job losses. The remaining columns 5 to 7 of table 3 exclude this channels by controlling for the time-variant weighted average stock of industrial robots or the initial share of employment in high-technology enterprises.¹⁴ Irrespective of the specification, the coefficient of interest does not change. This makes us confident that mechanization does not spuriously drive our result.

¹³Output tariffs and NTM indicators are retrieved from the UNCTAD-TRAINS database (United Nations 2019). We construct input tariffs using IO tables as it is standard in literature and then merge tariff and NTM information to the firm data. Our measures of initial output and input tariff are simple average aggregated to district level whereas the NTM measure is weighted by initial firm employment.

¹⁴We define high-technology industries according to the OECD definition (OECD 2003). We then allocate firms (and their employment numbers) to either low- or high-technology industries.

5.3.3 Political economy

A huge literature discusses the political economy of trade policy (c.f. Grossman and Helpman 1994, Goldberg and Pavcnik 2005, Asher and Novosad 2017). The main argument thereby is that trade policy is not exogenous but endogenously determined within the political process. Particular industries and firms may lobby for policy changes that favor their own business while, at the same time, political incumbents face re-election motives that may make them sensitive to concerns of specific interest groups. Even though the Indonesian government did not explicitly state reasons that explain the selection of products which enter the NIL, Genthner and Kis-Katos (2019) show that there are certain factors on industry level that predict changes in the regulatory environment. Table 4 therefore tests the robustness of our results with respect to various political factors. Odd columns thereby use our preferred specification with linear trends and extend the set of initial conditions by the respective controls. Even columns flexibly allow for non-linear trends in political factors.

Columns 1 to 4 check whether our results are driven by market power as proxied by the initial Herfindahl sales or employment concentration indices within districts. If sales are concentrated among a few firms, these companies may have more power to lobby for (or against) FDI protection as they face lower costs of coordination and can thus pursue their interest more easily and effectively (Grossman and Helpman 1994). In a similar vein, firms which employ a high share of total district workforce can more easily push for regulation in policy.

In columns 5 to 8, we include controls for presence of state-owned enterprises. Genthner and Kis-Katos (2019) showed that public enterprise status and privatization are among the most important factors to describe later regulation. We therefore add the initial share of employment in both public and recently privatized enterprises to test whether the increase in district employment is driven by differential trends among those firms which were most likely affected by the NIL. Next, we include the share of firm employment that works in medium-sized enterprises in columns 9 and 10. One important condition within the NIL is to exclude small and medium-sized (SME) firms from regulation while still applying it to large firms. If the development of SME employment was on a different trajectory compared to large firms, we will mechanically pick up this trend in our LRP measure.

Columns 11 to 14 further control for presence of vulnerable employment in a district. If decisions on product coverage of the NIL take social justice into consideration, our employment results may be originating from particular trends in those dimensions and not regulation itself. We therefore control for the initial share of low-skilled employment, which can be considered especially vulnerable in the context of foreign competition, or the average wage per worker in manufacturing, as industries with lower wages need more

social protection (Gawande and Krishna 2003, Topalova and Khandelwal 2011).¹⁵

Alternatively, firms which are highly dependent on external financing may lobby against FDI regulation. On the other hand, policy makers also refrain from hurting the local economy by restricting their access to capital. We therefore test for the initial share of employment in highly credit-dependent industries in columns 15 and 16 (Rajan and Zingales 1998).¹⁶ Finally, columns 17 and 18 use the distance from a district’s centroid to the national capital Jakarta as proxy for political connectivity. There is a huge debate in Indonesia about Java-centered politics and people in the periphery (such as Papua or Sulawesi) often complain about how public money is distributed in favor of Java and parts of Sumatera (The Economist 2019).

Throughout all our robustness checks, the coefficient of interest does not vary in magnitude and remains statistically significant. This also holds when including all the above mentioned initial controls at the same time in columns 19 and 20. We view this as support for a regulation-driven increase in district employment and do not find strong evidence of a political economy story.

5.3.4 Agglomeration and urbanization

In this section, we address another threat to our identification strategy. The LRP measure is constructed by summing up each firm’s regulatory status over districts and years and then weighting it by the initial share of firm employment in total labor force. One concern thereby is that we do not only capture changes in regulation over time but that our main result reflects the relative importance of manufacturing in a particular location. If industrial areas were on different trajectories than the remaining regions, we would just pick up an agglomeration effect.

To test whether the effect of LRP really originates from changes in regulatory penetration, we therefore decompose initial firm employment shares into three time-invariant parts. The first component is the employment share within products which are and will never be regulated in future. For our identification to be valid, this part of firm employment must not affect the LRP coefficient as it would be a clear indication of agglomeration driving the result. Secondly, we define the employment share within firms which operate in regulated product markets but are and will never be subject to regulation (as they do not fulfill the firm-specific requirements such as firm size or legal status). The final component includes firm employment which becomes regulated at some point in time.¹⁷

¹⁵In particular, the share of unskilled employment is also one of the main determinants found to drive regulatory decisions (Genthner and Kis-Katos 2019).

¹⁶We use 0.2 as cut-off value of the sectoral share of external funding to define highly dependent industries.

¹⁷Note that the three components add up to the total firm employment share in district labor force, i.e. they are mutually exclusive and overlapping is not possible. Never regulated product employment accounts for 48%, never regulated firm employment 10% and ever regulated firm employment 42% of the total firm

The measures of never regulated employment (both product and firms) are positively but not highly correlated with LRP (with a correlation coefficient of about 0.15). In contrast, the initial share of firm employment which will be regulated at some point in time is strongly associated with time-varying regulatory penetration (about 0.82).

We start with a description of linear trends of the three initial employment components without further controls. Table A3 step-wise adds trends in never regulated product employment, never regulated firm employment and ever regulated firm employment in columns 2 to 4. All trends exhibit a positive correlation with total district employment with never regulated firm employment showing the largest coefficient. However, when including both trends in never regulated initial employment shares in column 5 only product employment remains statistically significant. The same also holds in column 6 where we control for all three trends at the same time. Across all columns, the LRP estimate drops in magnitude relative to the uncontrolled coefficient in column 1. Most importantly, however, the coefficient of interest remains robust to including linear trends in employment which is never directly affected by regulation.

The previous result also holds when including the linear time trend in never regulated employment in the fully specified baseline model. Columns 1 and 2 of table 5 show a robust LRP coefficient which is similar in size compared to our main result. The rest of table 5 uses an alternative proxy for agglomeration. The *SI* surveys of 2004 and 2005 include an item asking for whether the plant is located within an industrial area. Based on this survey question, we compute initial district employment in industrial areas as an alternative measure of agglomeration.¹⁸ The LRP coefficients is unchanged when including either linear or non-linear trends of the alternative agglomeration proxy. Finally, columns 5 and 6 introduce an interaction term of LRP with a district's agglomeration status.¹⁹ Despite the fact that the regulatory impact is not spuriously picking up agglomeration effects, we still see that our results are driven by regulation within districts with at least some agglomerated industry.

While agglomeration effects are demand-driven, another potential confounding factor may come from labor supply. If employment increased in large cities over time and LRP just correlated with this upward trend (as firms tend to be located next to metropolitan areas), our identification strategy was not valid. We thus use information from *Susen*as to construct the share of individuals living in urban areas and allow for differential trends in its initial level in columns 1 and 2 of table 6.²⁰ The coefficient of interest declines but remains robust even when allowing for more flexible time patterns. We confirm this result

employment share.

¹⁸The correlation between our two proxies for agglomeration is 0.83, suggesting that both measures capture similar dynamics.

¹⁹We define districts as agglomeration areas if the share of employment in industrial areas is different from zero. 42% of the districts are hereby considered as agglomerated.

²⁰This control is frequently used in the literature (c.f. Asher and Novosad 2017).

with an alternative proxy for urbanization by adding the initial population density of a district in columns 3 and 4. The last two columns split the sample into Java and the remaining islands. Most economic activity is concentrated on Java and it also hosts the majority of the Indonesian population as well as most of the big cities.²¹ While we still find an effect of regulatory penetration on district employment in Sumatera, Kalimantan, Sulawesi and Papua (plus outer islands), the coefficient is much larger in magnitude when restricting the sample to Java.

5.3.5 Labor market reforms

As a last robustness check, we test whether our result are affected by labor market reforms. In particular, we control for minimum wage legislation. The classical labor market model without any frictions predicts that the introduction of binding minimum wages should result in unemployment. However, alternative models which allow for market imperfections even feature positive employment effects from a lower bound on wages (c.f. Shapiro and Stiglitz 1984, Dickens et al. 1999).

As part of the decentralization efforts in Indonesia, minimum wage legislation was delegated to the provincial governments in 2001 (Widarti 2006). In case that localized minimum wage setting correlated with the introduction of the NIL in particular regions, LRP might pick up some of the effect of wage regulation. To exclude this possibility, columns 1 and 2 of table 7 add yearly minimum wages at the province-level to our baseline specification. We cannot detect any significant effect from minimum wages on district employment, while our main estimate is barely affected both in magnitude and significance. To account for any other province-level change in labor market regulation (or other policy reforms that indirectly affect job creation), we finally replace island-year dummies with province-year fixed effects. This specification more flexibly allows for trends and shocks on province level, including minimum wage legislation. Columns 3 and 4 show that the LRP estimate only marginally changes in terms of magnitude and remains significant at the 10 percent level. Given the robustness of our main result in this section, we are confident that our results capture the impact of regulatory penetration on employment and are not driven by other confounding factors.

5.4 Employment heterogeneity

This section further disentangles the effect on total employment by sector and type of worker, and also looks at the job quality. We thereby focus on our preferred specification with linear trends in the initial controls as it allows for error clustering on province level.

²¹According to the latest census of 2010, Java inhabits 60% of Indonesia's population. At the same time, its contribution to national GDP amounts to about 61% (BPS 2014).

Note that all results also hold when flexibly controlling for year interactions.

Table 8 presents employment effects in manufacturing, agriculture and the service sector. For ease of comparison we again show the coefficient for total employment in column 1. We find that none of the estimates of interest in the sectoral decomposition turns significant due to large standard errors. The latter can be explained by a lot of noise in the sectoral data due to potential misreporting or sampling errors on individual level. Notably, the effect of regulation within manufacturing employment is the largest in magnitude while agriculture and services are smaller (or even negative). Despite the lack of significance we still interpret this as evidence that most of the job creation originates from the manufacturing sector.

In a next step, table 9 documents heterogeneous adjustment across different groups of workers. The largest effects can be found among females. A one standard deviation increase in LRP is associated with a 1.7 percent increase in female employment in column 1. Male employment also increases with regulatory penetration, but the effect is much smaller and less significant (column 2). However, the labor market response of males is also lower because they are ex-ante already more likely to be employed. Columns 3 to 7 show the employment effect across age cohorts. Especially younger cohorts (and in particular young adults between 15 and 24) benefit from protection while individuals above the age of 45 do not experience a significant increase in employment. In that sense, most of the created jobs are filled with workers who recently entered the labor force and, therefore, potentially lack practical work experience. The strong bias towards young employment points towards a potential abundance of lower quality jobs which would compromise the overall positive employment effects to some extent. We thus look deeper into the overall quality of newly generated positions.

We start by splitting up the employment effects into skill groups, thereby referring to low-skilled if an individual has a degree of primary education at most. Columns 1 and 2 in table 10 show that the positive total labor market effect is mostly driven by low-skilled individuals taking up employment. The effect of LRP on less educated workers is positive and highly significant while individuals with at least secondary education do not experience a statistically significant employment boost. This is perfectly in line with our results that especially the youngest cohort of workers gains in districts with stronger regulatory penetration.

The remainder of table 10 distinguishes between part-time and full-time jobs where part-time is defined as less than 30 hours per week. Unfortunately, we do not have information on working hours in the *Susen* data for two years (2007 and 2008). We thus may not be able to quantify the short-run labor market response and therefore re-estimate our baseline result for the restricted time period in column 3. Despite excluding two important years directly around the first revision of the NIL, the LRP coefficient does

not change much. Columns 4 and 5 then contrast part-time and full-time job creation. We find strong evidence supporting the hypothesis from above that most of the positive employment effect is driven by lower quality jobs. The estimate in column 4 suggests that employment positions with less than 30 hours increase by roughly 3 percent once LRP is increased by one standard deviation. In contrast, full-time jobs are not affected at all. Table 10 therefore puts the positive impact of FDI protection on aggregate employment into perspective.

5.5 Alternative labor market outcomes

Until now we have focused exclusively on employment based on the household survey *Susen**as*. One drawback of the data is, however, that it is impossible to retrieve consistent district aggregates of further labor market outcomes like working hours or wage income.²² As a consequence we complement our data with information from the labor market survey *Sakernas*. As mentioned above, we do not use *Sakernas* in our main results because it is only representative at the district level since 2007. We will lose much of the identifying variation in regulatory penetration if we only use this shorter time frame. To be able to still present some additional labor market outcomes, panel A of table 11 is ignorant about districts being not representative before 2007 and estimates equation 3 over the full time period. This will not cause harm as long as sampling errors are randomly distributed across districts. We check for validity of this assumption by restricting the sample to the years 2007 to 2015 in panel B.

Column 1 again reports the baseline results using *Susen**as* employment for both samples. The effect remains surprisingly robust when we restrict the sample to the years after 2007 and thereby fully exclude the first revision of the NIL. When we switch to the *Sakernas*-based data, we can still confirm our results for the full sample period in column 2. However, we do not find a robust result for the shorter time period. A potential explanation could be that *Susen**as* also partially covers the informal sector while *Sakernas* mainly surveys formal employment. This in turn means that employment adjustments to the second revision of the NIL in 2010 are mostly driven by the rise of the informal sector whereas the more formal jobs did not react much. Given that informal employment on average tends to be of lower quality relative to formal jobs, this is in line with earlier results.

Column 3 shows for the full time period that LRP also increases labor force participation by pulling inactive individuals to the labor market. Similar to the employment result, however, this finding does not hold in the shorter time frame of panel B. Columns 4 and 5 look at the intensive margin of labor adjustment. Like before, coefficients do not reach

²²Many items in the employment modules within *Susen**as* unfortunately exhibit many missing observations or are not available at all for some years, making it infeasible to use them for analysis.

conventional significance levels for the sample between 2007 and 2015. We though find strong positive effects both for total working hours in the economy and weekly hours per worker. This provides some indication for an adjustment, which does not purely take place at the extensive margin via job creation but also at the intensive margin.²³

In our final set of results, we look into the impact on wages of workers. Given the classical framework of competitive labor markets without frictions, one would expect wages to rise due to increases in aggregate labor demand. As wage information is not consistently available within the household surveys, we again rely on *Sakernas* data. Table 12 reports the effect of LRP on wage premia in the total economy in column 1 and then splits the effects by sector in columns 2 to 4. When looking at the full time period in panel A, none of the coefficient reaches conventional significance levels. For the shorter (district-level representative) sample period, however, some more nuanced patterns emerge. We detect an overall positive effect on wage premia where a one standard deviation increase in regulatory penetration is associated with a wage increase of about 1 percent. The effect seems to be strongest within manufacturing even though the coefficient does not come out significant. In contrast, we estimate a significant coefficient on service sector wage premia which points towards substantial spillover effects within the economy. The more precisely estimated effects in the 2007-2015 sample may be due to noise in the data before 2007 when sampling was not representative on district level. However, it could also be evidence for a stronger effect on wages from the second revision of the NIL in 2010.

Finally, table 13 disentangles the wage premia effects by gender and skill groups. While female wages react strongly to regulation both in the full and the restricted sample, we do not observe a similar pattern for male workers. This heterogeneity is puzzling as we cannot think of a theoretical explanation for female wages benefiting more from protection. Wage premia of high-skilled workers rise more as compared to low-skilled in both samples. In a frictionless labor market, the last finding would suggest that high-skilled labor supply is way more inelastic as compared to low-skilled. At the same time, the stronger responsiveness of low-skilled workers in terms of employment is in line with a more elastic labor supply.

²³We provide additional tables of the impact on active population and working hours by sector, gender and skill-group in Appendix tables A4 and A5. Like above, we do not find strong effects in the shorter sample period between 2007 and 2015. However, it does not seem to be the case that females enter the labor market. In contrast, the positive impact on active population is mainly driven by males. Like for employment, we predominantly find positive effects on working hours per worker in manufacturing, while there is no differential effect across gender or skill group.

6 Conclusion

Most policy interventions have a particular target in mind. However, there may be additional side effects on other actors of the economy which can affect the overall evaluation of a political reform. In this paper, we showed that the introduction of a protectionist FDI policy in Indonesia lead to employment creation and wage increases, at least for certain parts of the working population. In particular, we presented evidence featuring a 0.7 percent increase in total employment due to a one standard deviation larger increase in regulatory penetration across districts. The effect immediately emerged after the massive regulatory reform in 2007 but also persisted in the longer run. We further showed that our estimates survive various robustness checks where we control for alternative factors like minimum wage regulation, mechanization or agglomeration. Most of the job creation, however, seem to be of lower quality as most employment growth is found in low-skilled or part-time jobs. Finally, we presented some evidence of positive wage effects, especially among females and high-skilled workers. Our findings suggest that the labor market effects of trade protection behave symmetrically to trade liberalization. While tariff reductions have been shown to depress both wages and employment (cf. Autor et al. 2013, Dix-Carneiro and Kovak 2017), we find the opposite effects from an FDI hostile policy reform. However, this should not be understood as encouraging evidence for protectionist policies. In contrast, our results hint at an increase in subsistence work positions. Therefore, we prefer to put our results in the context of a trade-off between immediate employment gains and long-run economic development. Closing down the economy for foreign capital investments and the inflow of new technology and know-how may be tempting in the short-run but potentially comes with a long-run cost. We do not want to make any statement regarding broader local economic development and its effect on the standard of living as it is beyond the scope of this paper. On a final note, our results are subject to some limitations. We are only able to measure regulatory penetration precisely in the manufacturing sector but not in agriculture and services. Hence, economy-wide employment effects may both arise from spillovers from regulated manufacturing firms or a spatial correlation among the regulated primary/tertiary and secondary products. Moreover, we still lack sufficient information on the quality of employment to end up with reliable welfare implications. More information on work contracts would be needed to further investigate the nature of employment creation.

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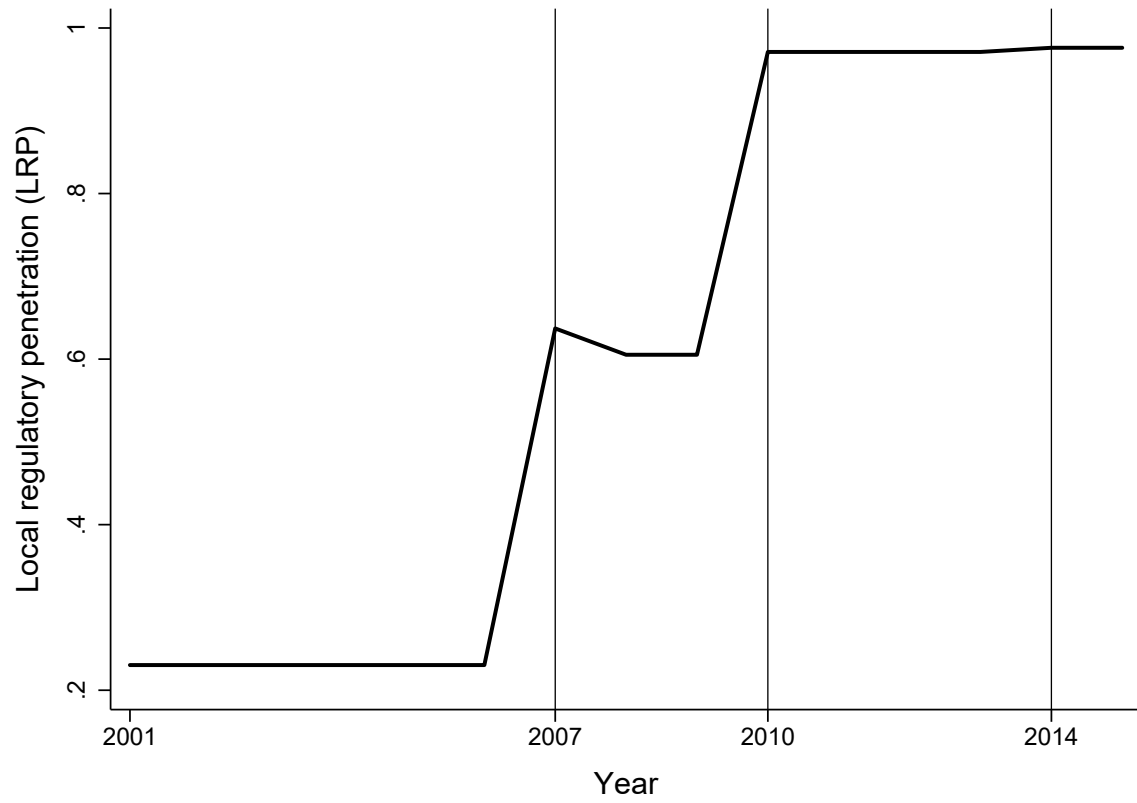
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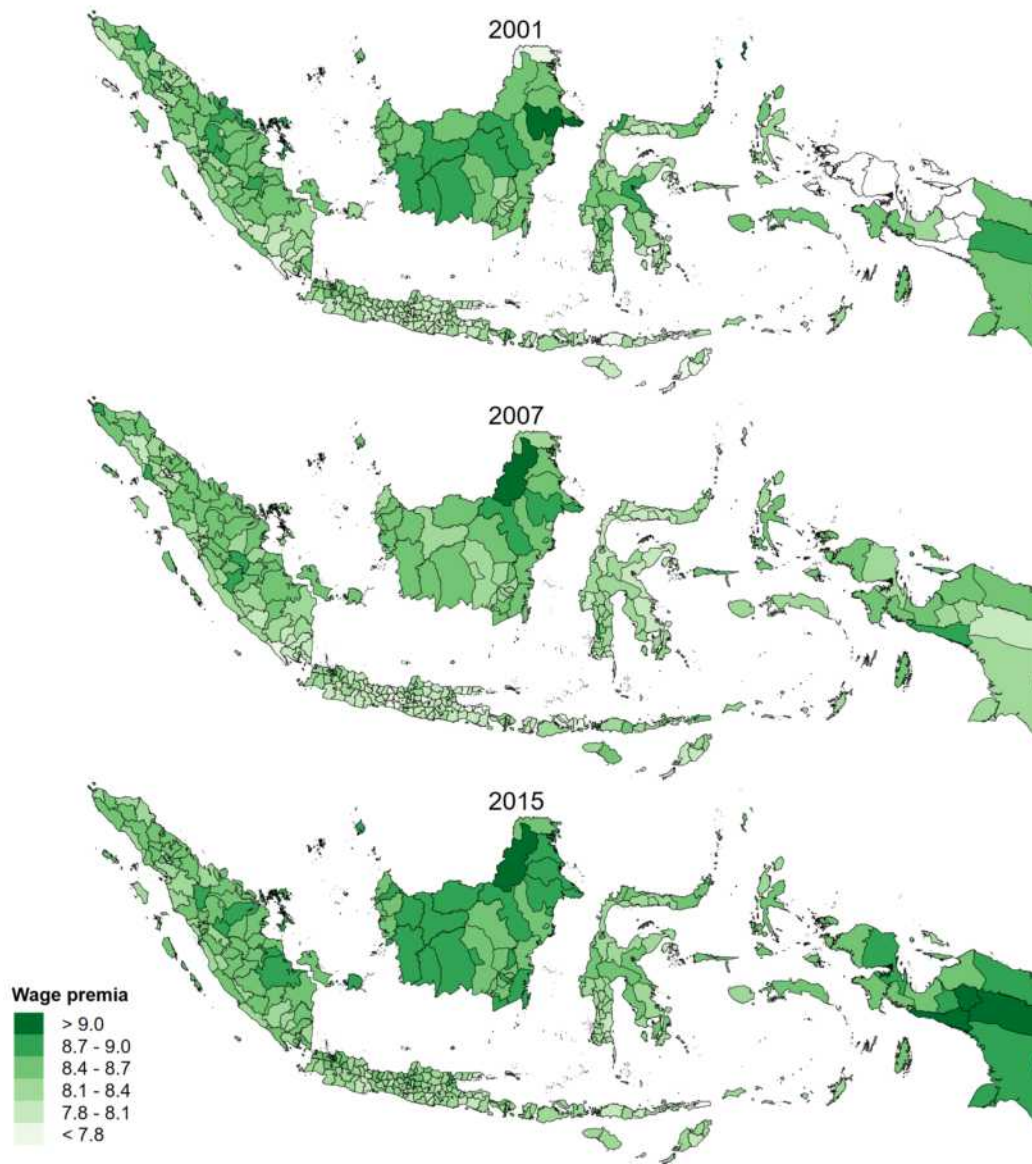
Figures

Figure 1: Measure of local regulatory penetration (LRP) over time



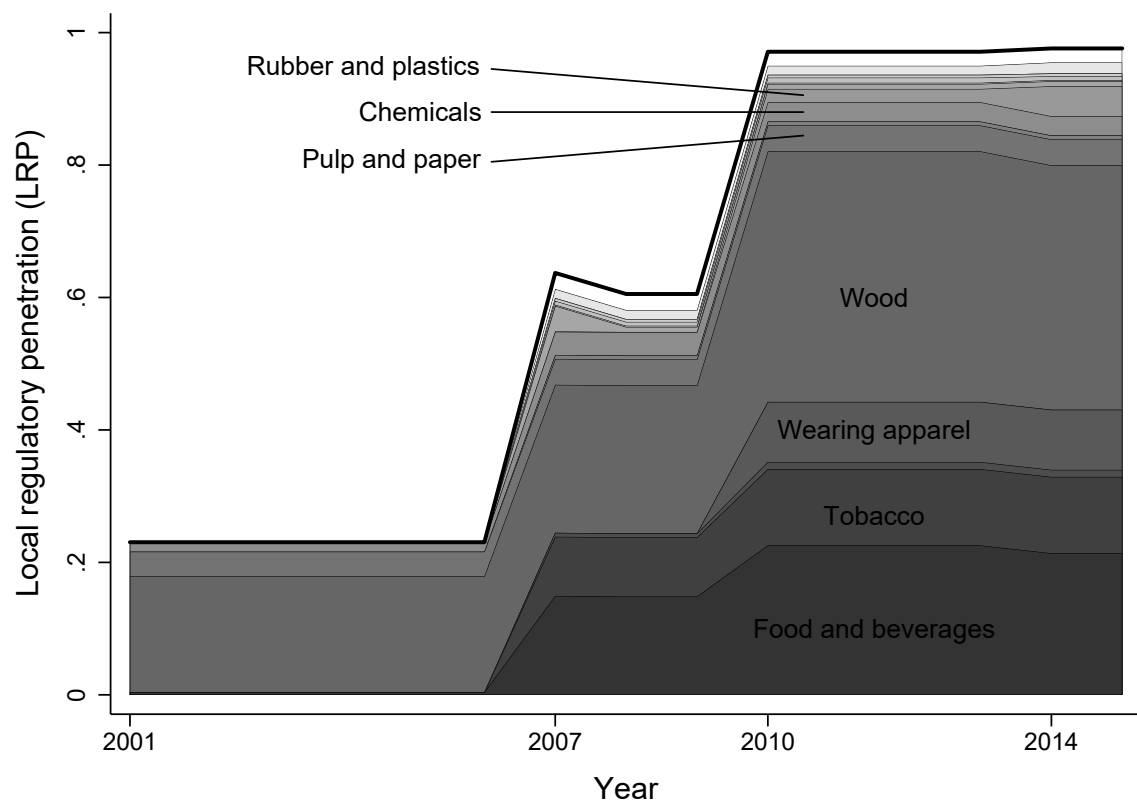
Note: The graph depicts average local regulatory penetration (LRP) from 2001 to 2015 based on equation 2. Values are re-scaled by factor 100.

Figure 2: Log wage premia



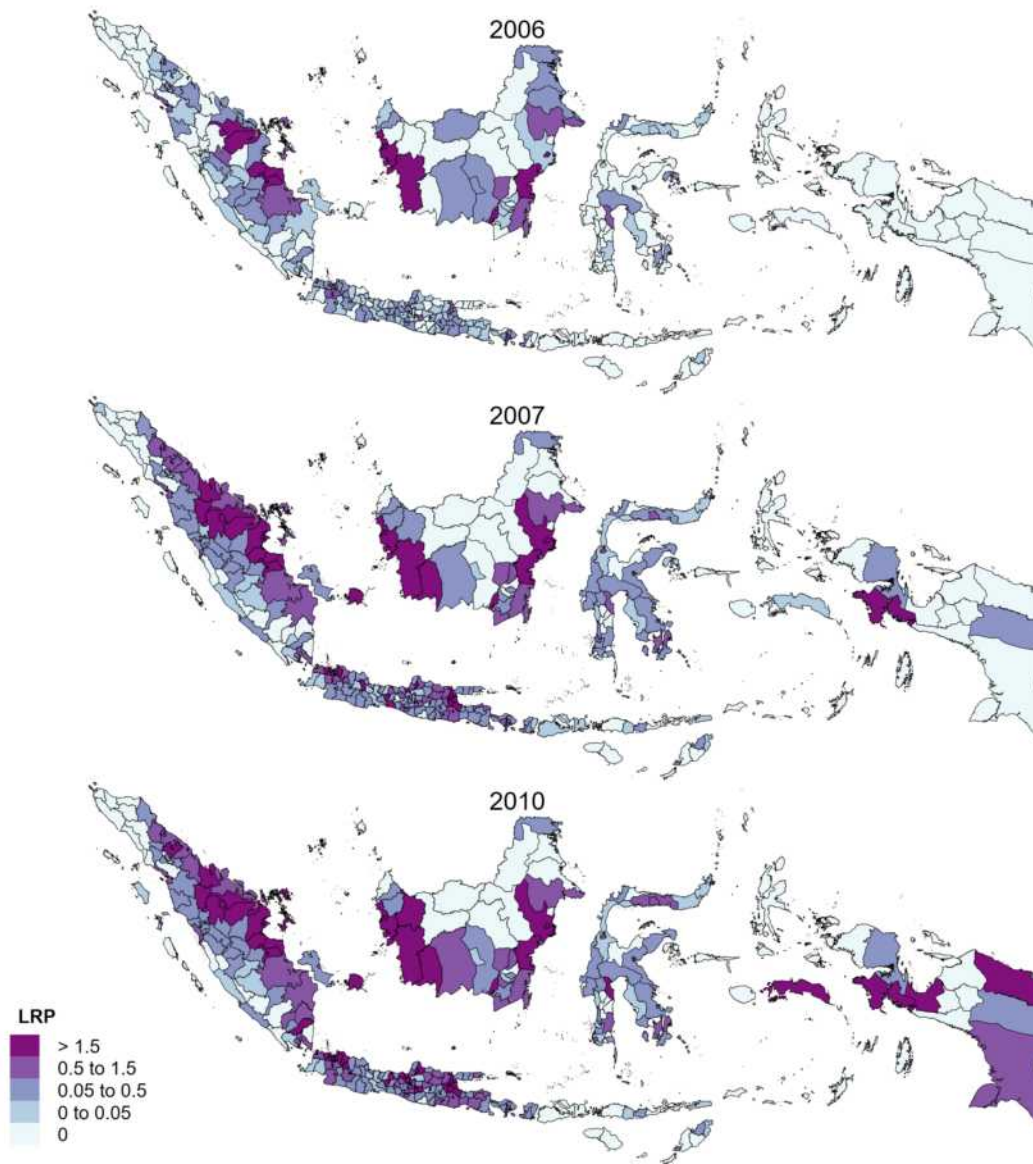
Note: Note: District borders are from 2000. Wage premia are estimated based on equation 1.

Figure 3: Sectoral composition of local regulatory penetration (LRP) over time



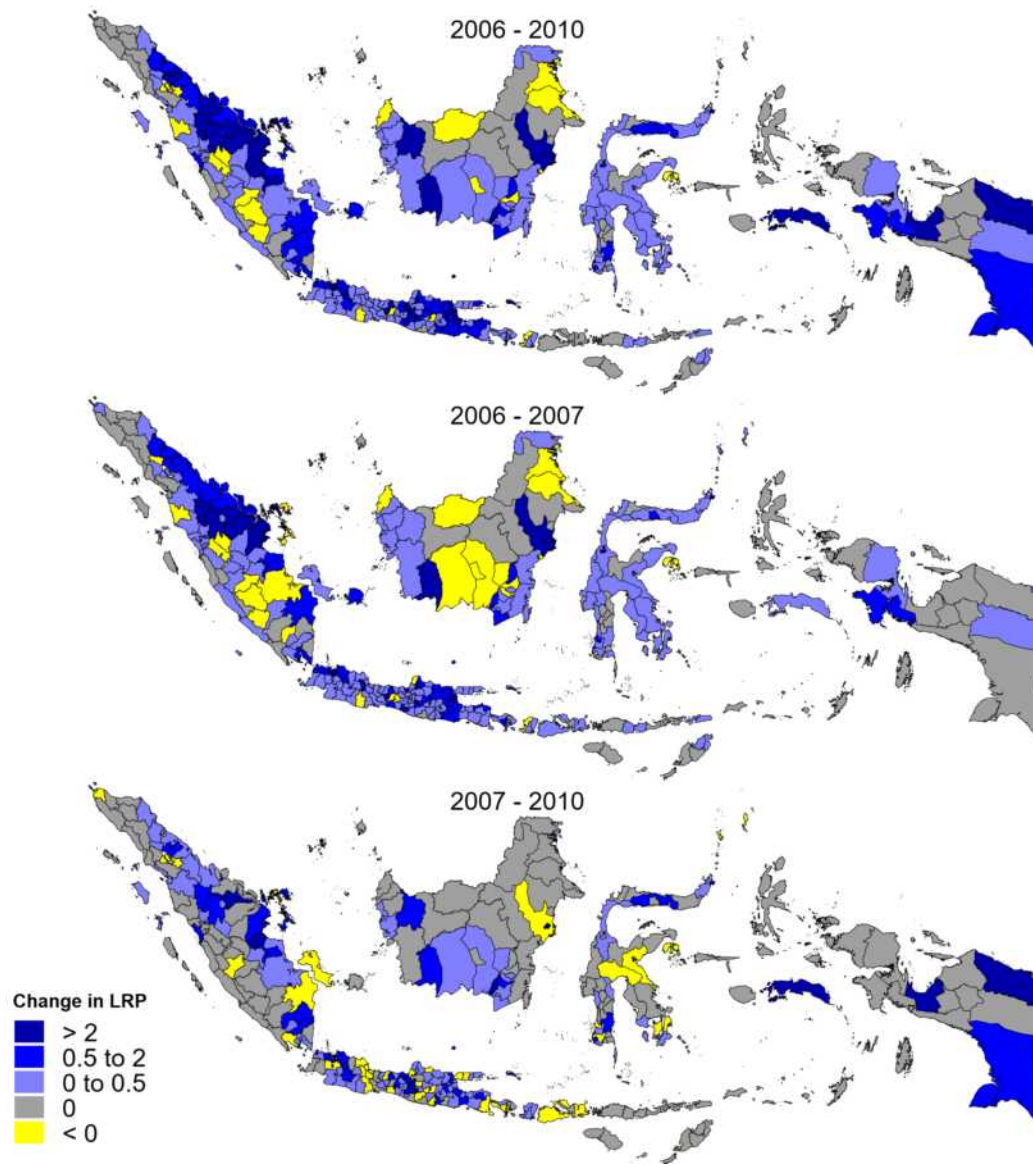
Note: The solid black line depicts average local regulatory penetration (LRP) from 2001 to 2015 based on equation 2. Shaded areas show the sectoral contribution to LRP based on regulated share of initial employment composition. Values are re-scaled by factor 100.

Figure 4: LRP levels in 2006, 2007 and 2010



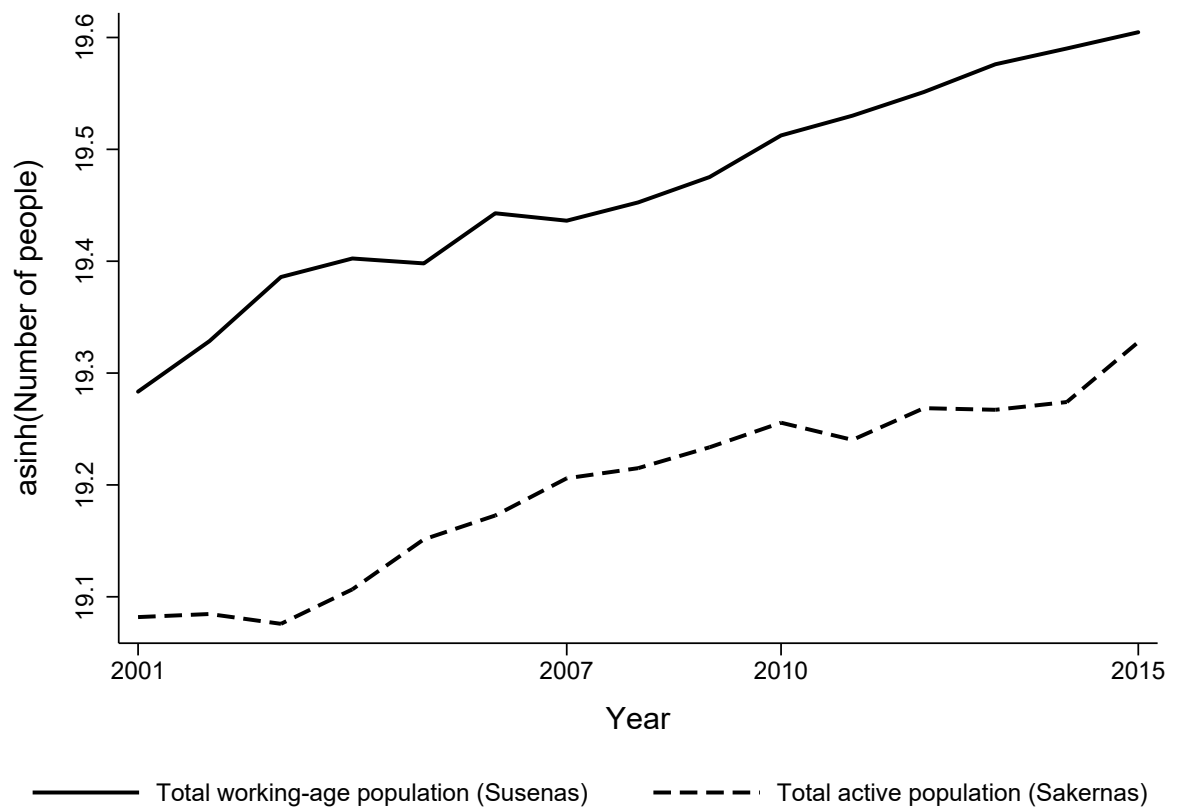
Note: District borders are from 2000. Values are re-scaled by factor 100.

Figure 5: Change in LRP between 2006 and 2010



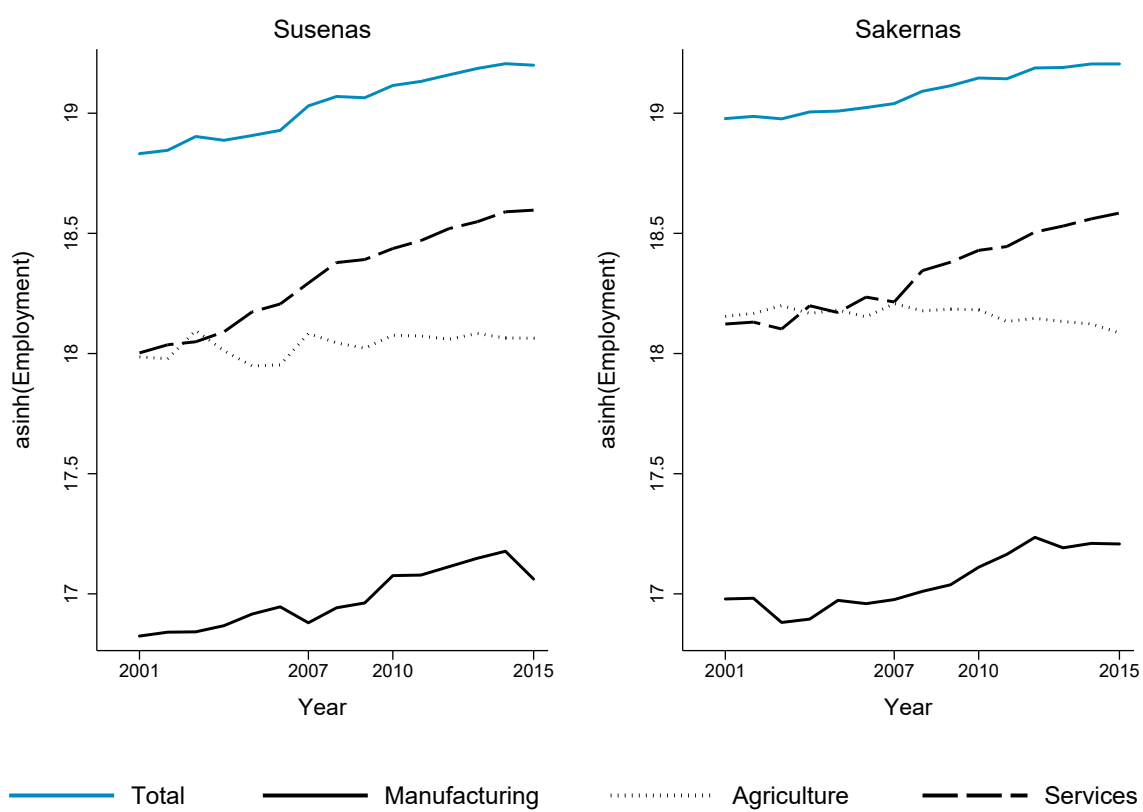
Note: Note: District borders are from 2000. Values are re-scaled by factor 100.

Figure 6: Working-age population and active labor force



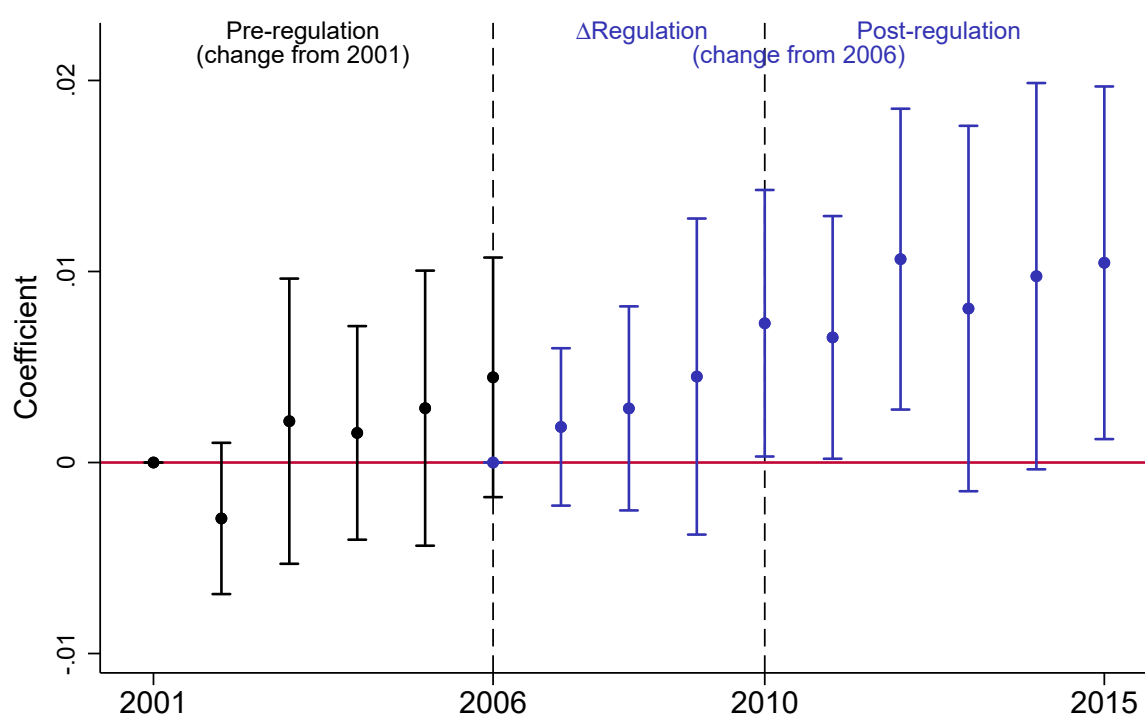
Note: Based on own calculations using the *Susenias* sample for calculating working-age population and the *Sakernas* sample for total active population (see table A1)

Figure 7: Sectoral employment in *Susenas* and *Sakernas* over time



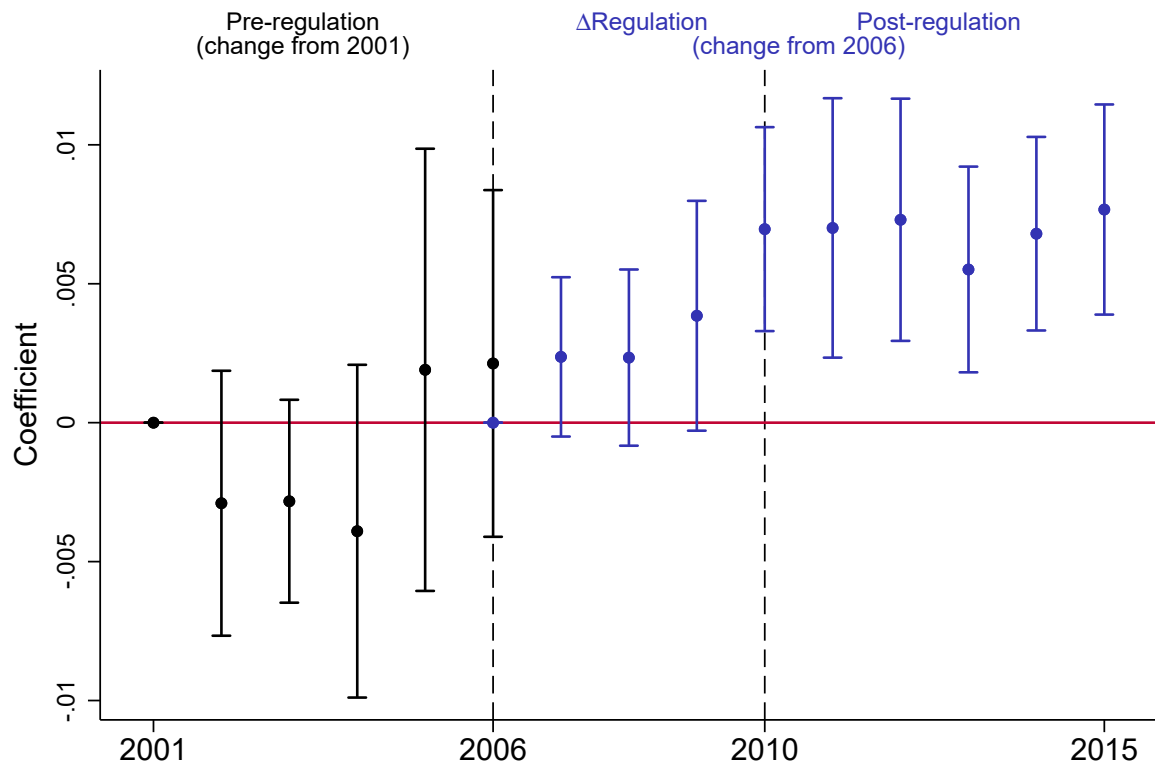
Note: Based on own calculations. See table A1 for summary statistics.

Figure 8: Impact of LRP (full change) on working-age population



Note: The dependent variable is the inverse hyperbolic sine of total working-age population. Each plotted coefficients is estimated in a separate regression. Coefficients after 2006 are long-difference effects relative to 2006. In each regression, we control for the initial level of LRP and population before the first revision, the initial share of manufacturing employment and island indicators. Coefficients between 2001 and 2006 are pre-trend estimates with 2001 as base year. Bars around the point estimates denote 90% confidence intervals.

Figure 9: Impact of LRP (full change) on total employment



Note: The dependent variable is the inverse hyperbolic sine of total employment. Each plotted coefficients is estimated in a separate regression. Coefficients after 2006 are long-difference effects relative to 2006. In each regression, we control for the change in district population relative to the base year, the initial level of LRP and population before the first revision, the initial share of manufacturing employment and island indicators. Coefficients between 2001 and 2006 are pre-trend estimates with 2001 as base year. Bars around the point estimates denote 90% confidence intervals.

Tables

Table 1: Summary statistics of district-level panel

	Mean	SD	Minimum	Maximum	Observations
<i>Local regulatory penetration:</i>					
LRP	0.64	1.46	0	16	4,779
Δ LRP 2006-2010	0.78	1.37	−.43	9.8	4,779
Δ LRP 2006-2007	0.43	0.82	−.52	6.9	4,779
Δ LRP 2007-2010	0.35	1.00	−1.9	8.8	4,779
<i>Susenas variables:</i>					
asinh(Working-age population)	13.29	0.89	9.5	16	4,779
asinh(Total employment)	12.87	0.90	9.1	15	4,779
asinh(Manufacturing employment)	10.04	1.75	0	14	4,779
asinh(Agricultural employment)	11.66	1.32	6.5	14	4,779
asinh(Services employment)	12.00	1.09	0	15	4,779
<i>Sakernas variables:</i>					
asinh(Working-age population)	13.37	0.89	11	16	4,779
asinh(Active population)	13.03	0.90	9.9	15	4,779
asinh(Total employment)	12.93	0.90	9.8	15	4,779
asinh(Total number of working hours)	16.56	0.92	13	19	4,779
Log total wage premia	8.32	0.28	7.1	10	4,775
Log manufacturing wage premia	8.59	0.56	6	12	4,467
Log agricultural wage premia	8.53	0.41	6	11	4,651
Log services wage premia	8.77	0.24	7.8	10	4,763

Note: LRP is re-scaled by factor 100. Working-age population is defined as all individuals at the age between 15 and 64. Log wage premia are estimated by equation 1.

Table 2: Impact of local regulatory penetration on total employment

Dependent variable:	asinh(Total employment)				
	(1)	(2)	(3)	(4)	(5)
LRP	0.0135*	0.0213***	0.0072***	0.0052***	0.0055***
	(0.0070)	(0.0057)	(0.0022)	(0.0015)	(0.0018)
asinh(Working-age population)			1.0295***	1.0189***	1.0195***
			(0.0179)	(0.0197)	(0.0220)
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes				
Island-year FE		Yes	Yes	Yes	Yes
Linear trends in initial condition				Yes	
Initial conditions \times Year					Yes
Observations	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment. Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Robust standard errors are clustered on province level (with the exception of column 5 where they are clustered on district level) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 3: Robustness: Global financial crisis, trade, automation and high-technology firms

Dependent variable: asinh(Total L)	Global crisis		Trade		Automation		High tech		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LRP	0.0037** (0.0013)	0.0037** (0.0018)	0.0054*** (0.0014)	0.0055*** (0.0017)	0.0052*** (0.0015)	0.0055*** (0.0018)	0.0054*** (0.0015)	0.0057*** (0.0017)	0.0041*** (0.0014)	0.0041** (0.0017)
asinh(Working-age population)	1.0166*** (0.0183)	1.0177*** (0.0218)	1.0296*** (0.0184)	1.0299*** (0.0218)	1.0189*** (0.0197)	1.0195*** (0.0219)	1.0185*** (0.0185)	1.0194*** (0.0219)	1.0226*** (0.0172)	1.0236*** (0.0218)
asinh(Average stock of robots)					0.0001 (0.0010)	0.0003 (0.0014)			0.0006 (0.0011)	0.0008 (0.0014)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes		Yes		Yes		Yes		Yes	
Initial conditions \times Year		Yes		Yes		Yes		Yes		Yes
Observations	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment (L). Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Columns 1 and 2 extend the set of initial conditions by import and export volume. Columns 3 and 4 add input and output tariffs as well as the share of employment affected by non-tariff measures. Columns 7 and 8 control for the employment share of high-technology firms according to OECD classification. Columns 9 and 10 include all above mentioned initial controls. Robust standard errors are clustered on province (district) level in columns with linear trends (year interactions) and reported in parentheses. Significance at or below 1% (***) , 5% (**) and 10% (*).

Table 4: Robustness: Political economy

Dependent variable: asinh(Total L)	Sales concentration		Labor concentration		State-owned employment		Privatized employment		Employment in SME	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LRP	0.0048*** (0.0017)	0.0050*** (0.0019)	0.0053*** (0.0015)	0.0055*** (0.0018)	0.0051*** (0.0015)	0.0053*** (0.0018)	0.0051*** (0.0015)	0.0053*** (0.0018)	0.0040** (0.0016)	0.0042** (0.0019)
asinh(Working-age population)	1.0194*** (0.0196)	1.0196*** (0.0219)	1.0227*** (0.0201)	1.0225*** (0.0221)	1.0182*** (0.0195)	1.0192*** (0.0220)	1.0181*** (0.0193)	1.0193*** (0.0217)	1.0140*** (0.0188)	1.0160*** (0.0214)
Distance to JKT										
Low-skilled employment			Average wage			Credit dependency			All	
(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	
LRP	0.0051*** (0.0015)	0.0052*** (0.0018)	0.0046*** (0.0015)	0.0048*** (0.0018)	0.0056*** (0.0015)	0.0059*** (0.0018)	0.0057*** (0.0017)	0.0059*** (0.0019)	0.0040* (0.0020)	0.0043** (0.0020)
asinh(Working-age population)	1.0138*** (0.0194)	1.0136*** (0.0221)	1.0118*** (0.0190)	1.0146*** (0.0220)	1.0173*** (0.0195)	1.0181*** (0.0219)	1.0209*** (0.0201)	1.0221*** (0.0222)	1.0062*** (0.0187)	1.0100*** (0.0225)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes		Yes		Yes		Yes		Yes	
Initial conditions \times Year		Yes		Yes		Yes		Yes		Yes
Observations	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment (L). Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Columns 1 to 4 extend the set of initial conditions by a Herfindahl sales or labor concentration index. Columns 5 and 6 add the share of employment in state-owned enterprises. Columns 7 and 8 control for the employment share privatized firms between 2001 and 2005. Columns 9 and 10 extend the set of initial conditions by the employment share in medium-sized firms. Columns 11 and 12 add the share of low-skilled employment. Columns 13 and 14 control for the average wage per worker. Columns 15 and 16 include the share of workers that are employed in external finance dependent firms. Columns 17 and 18 add the distance to Jakarta. Columns 19 and 20 include all above mentioned initial controls. Robust standard errors are clustered on province (district) level in columns with linear trends (year interactions) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 5: Robustness: Agglomeration

Dependent variable: asinh(Total L)	Never reg. L		Industrial areas			
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	0.0038** (0.0017)	0.0038** (0.0017)	0.0043*** (0.0014)	0.0044** (0.0017)	-0.0013 (0.0025)	-0.0013 (0.0030)
LRP \times Agglomeration area					0.0086*** (0.0030)	0.0087** (0.0040)
asinh(Working-age population)	1.0078*** (0.0174)	1.0078*** (0.0223)	1.0176*** (0.0198)	1.0182*** (0.0220)	1.0195*** (0.0199)	1.0200*** (0.0218)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes		Yes		Yes	
Initial conditions \times Year		Yes		Yes		Yes
Observations	4,779	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment (L). Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Columns 1 and 2 extend the set of initial conditions by the initial share of never regulated product employment. Columns 3 and 4 add the share of employment in industrial areas (based on SI). Agglomeration area is a dummy that turns 1 if a district has a non-zero share of employment in industrial areas. Robust standard errors are clustered on province (district) level in columns with linear trends (year interactions) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 6: Robustness: Urbanization

Dependent variable: asinh(Total L)	Urbanization		Population density		Java	Without Java
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	0.0029*** (0.0009)	0.0031** (0.0015)	0.0046*** (0.0014)	0.0048*** (0.0017)	0.0091*** (0.0014)	0.0036** (0.0017)
asinh(Working-age population)	1.0243*** (0.0178)	1.0261*** (0.0218)	1.0244*** (0.0200)	1.0251*** (0.0216)	1.1081*** (0.0806)	1.0020*** (0.0172)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes		Yes		Yes	Yes
Initial conditions \times Year		Yes		Yes		
Observations	4,779	4,779	4,779	4,779	1,649	3,130

Note: The dependent variable is the inverse hyperbolic sine of total employment (L). Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Columns 1 and 2 extend the set of initial conditions by the initial share of urban population. Columns 3 and 4 add the initial population density for each district. Column 5 and 6 split the sample into districts on the main island Java and the remaining islands. Robust standard errors are clustered on province (district) level in columns with linear trends (year interactions) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 7: Robustness: Labor market reform

Dependent variable:	asinh(Total employment)			
	(1)	(2)	(3)	(4)
LRP	0.0052*** (0.0015)	0.0054*** (0.0018)	0.0044* (0.0023)	0.0046** (0.0020)
asinh(Minimum wage)	0.0100 (0.0292)	0.0108 (0.0187)		
asinh(Working-age population)	1.0184*** (0.0195)	1.0189*** (0.0221)	1.0040*** (0.0216)	1.0037*** (0.0255)
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes		
Province-year FE			Yes	Yes
Linear trends in initial condition	Yes		Yes	
Initial conditions \times Year		Yes		Yes
Observations	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment. Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Robust standard errors are clustered on province (district) level in columns with linear trends (year interactions) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 8: Impact of local regulatory penetration on employment by sector

Dependent variable: asinh(Employment)	Total	Manufacturing	Agriculture	Services
	(1)	(2)	(3)	(4)
LRP	0.0052*** (0.0015)	0.0094 (0.0103)	0.0033 (0.0057)	-0.0032 (0.0077)
asinh(Working-age population)	1.0189*** (0.0197)	0.8634*** (0.1225)	0.9704*** (0.0668)	1.5221*** (0.3069)
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes
Observations	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of employment in the respective sector. Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 9: Impact of local regulatory penetration on employment by worker's characteristics

Dependent variable: asinh(Employment)	Females	Males	Age 15-24	Age 25-34	Age 35-44	Age 45-54	Age 55-64
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LRP	0.0123*** (0.0023)	0.0028* (0.0015)	0.0129*** (0.0040)	0.0042*** (0.0012)	0.0046*** (0.0009)	0.0027 (0.0023)	0.0027 (0.0042)
asinh(Working-age population)	1.0324*** (0.0613)	1.0080*** (0.0285)	1.1198*** (0.0456)	1.0136*** (0.0247)	1.0117*** (0.0090)	0.9682*** (0.0078)	0.9624*** (0.0096)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,779	4,779	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of employment of the respective worker group. Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***) , 5% (**) and 10% (*).

Table 10: Impact of local regulatory penetration on employment quality

Dependent variable: asinh(Employment)	Low-skilled	High-skilled	Total	Part-time	Full-time
	(1)	(2)	(3)	(4)	(5)
LRP	0.0039** (0.0018)	0.0026 (0.0018)	0.0059*** (0.0017)	0.0207*** (0.0074)	-0.0004 (0.0030)
asinh(Working-age population)	1.0637*** (0.0170)	0.9613*** (0.0113)	1.0173*** (0.0217)	0.9381*** (0.0953)	1.1752*** (0.1052)
District FE	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes	Yes
Observations	4,779	4,779	4,138	4,138	4,138

Note: The dependent variable is the inverse hyperbolic sine of employment of the respective worker group. Initial district conditions include the level of LRP and employment as well as the share of manufacturing employment in total district population, all measured in 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 11: Impact of local regulatory penetration on other labor outcomes

Dependent variable:	asinh(Total L)		asinh(N)	asinh(Total hrs)	Hrs per worker
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Sample period 2001-2015</i>					
LRP	0.0052*** (0.0015)	0.0055** (0.0022)	0.0058** (0.0022)	0.0097*** (0.0030)	0.1975* (0.0992)
asinh(Working-age population)	1.0189*** (0.0197)	0.9770*** (0.0132)	0.9818*** (0.0105)	1.0056*** (0.0126)	
Observations	4,779	4,779	4,779	4,779	4,779
<i>Panel B: Sample period 2007-2015</i>					
LRP	0.0040** (0.0018)	-0.0001 (0.0026)	-0.0020 (0.0028)	-0.0021 (0.0032)	-0.0300 (0.0826)
asinh(Working-age population)	1.0129*** (0.0154)	0.9924*** (0.0167)	0.9692*** (0.0220)	1.0039*** (0.0168)	
Observations	2,887	2,879	2,879	2,879	2,879
Based on	Susenas	Sakernas	Sakernas	Sakernas	Sakernas
District FE	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the inverse hyperbolic sine of total employment (L), total active population (N), total working hours or working hours per worker. Initial district conditions include the level of LRP and the dependent variable as well as the share of manufacturing employment in total district population, all measured as median of the years 2000 to 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 12: Impact of local regulatory penetration on wage premia by sector

Dependent variable: Log wage premia	Total	Manufacturing	Agriculture	Services
	(1)	(2)	(3)	(4)
<i>Panel A: Sample period 2001-2015</i>				
LRP	0.0031 (0.0039)	0.0079 (0.0107)	0.0076 (0.0048)	0.0012 (0.0045)
asinh(Working-age population)	0.0828 (0.0915)	0.0467 (0.0768)	0.0466 (0.0738)	-0.0128 (0.0340)
Observations	4,775	4,423	4,647	4,747
<i>Panel B: Sample period 2007-2015</i>				
LRP	0.0072* (0.0037)	0.0138 (0.0086)	0.0060 (0.0106)	0.0063** (0.0030)
asinh(Working-age population)	0.0443 (0.0468)	0.2562** (0.1015)	-0.0366 (0.0865)	0.0931** (0.0358)
Observations	2,879	2,652	2,777	2,870
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes

Note: The dependent variable is log wage premia both total and by sector (as estimated by equation 1). Initial district conditions include the level of LRP and the dependent variable as well as the share of manufacturing employment in total district population, all measured as median of the years 2000 to 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table 13: Impact of local regulatory penetration on wage premia by worker type

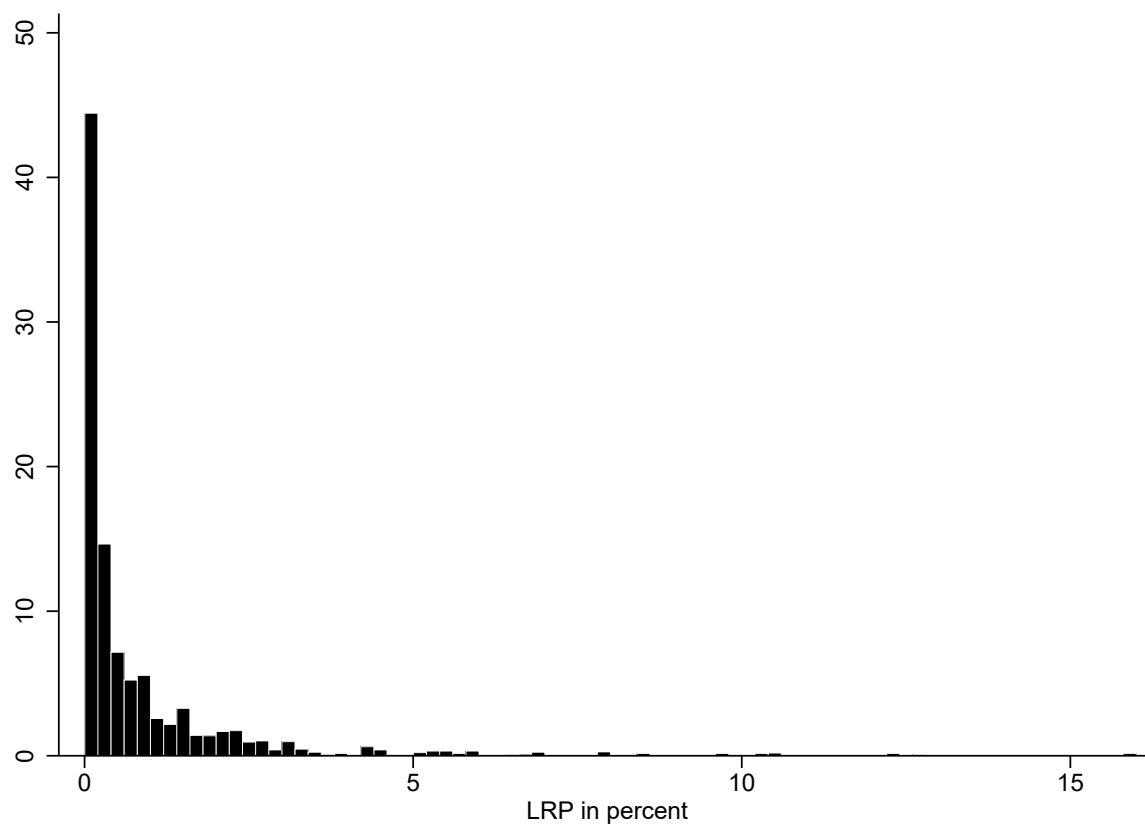
Dependent variable: Log wage premia	Female	Male	Low-skilled	High-skilled
	(1)	(2)	(3)	(4)
<i>Panel A: Sample period 2001-2015</i>				
LRP	0.0078 (0.0077)	0.0025 (0.0049)	0.0020 (0.0049)	0.0048 (0.0051)
asinh(Working-age population)	0.0794 (0.0801)	0.1110 (0.1014)	0.0873 (0.0714)	0.0014 (0.0300)
Observations	4,745	4,774	4,767	4,748
<i>Panel B: Sample period 2007-2015</i>				
LRP	0.0111*** (0.0036)	0.0015 (0.0037)	0.0050 (0.0049)	0.0080** (0.0032)
asinh(Working-age population)	0.1321** (0.0490)	0.0528 (0.0319)	-0.0336 (0.0477)	0.0059 (0.0431)
Observations	2,870	2,879	2,871	2,861
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes

Note: The dependent variable is log wage premia by gender and skill group (as estimated by equation 1). Initial district conditions include the level of LRP and the dependent variable as well as the share of manufacturing employment in total district population, all measured as median of the years 2000 to 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

A Appendix

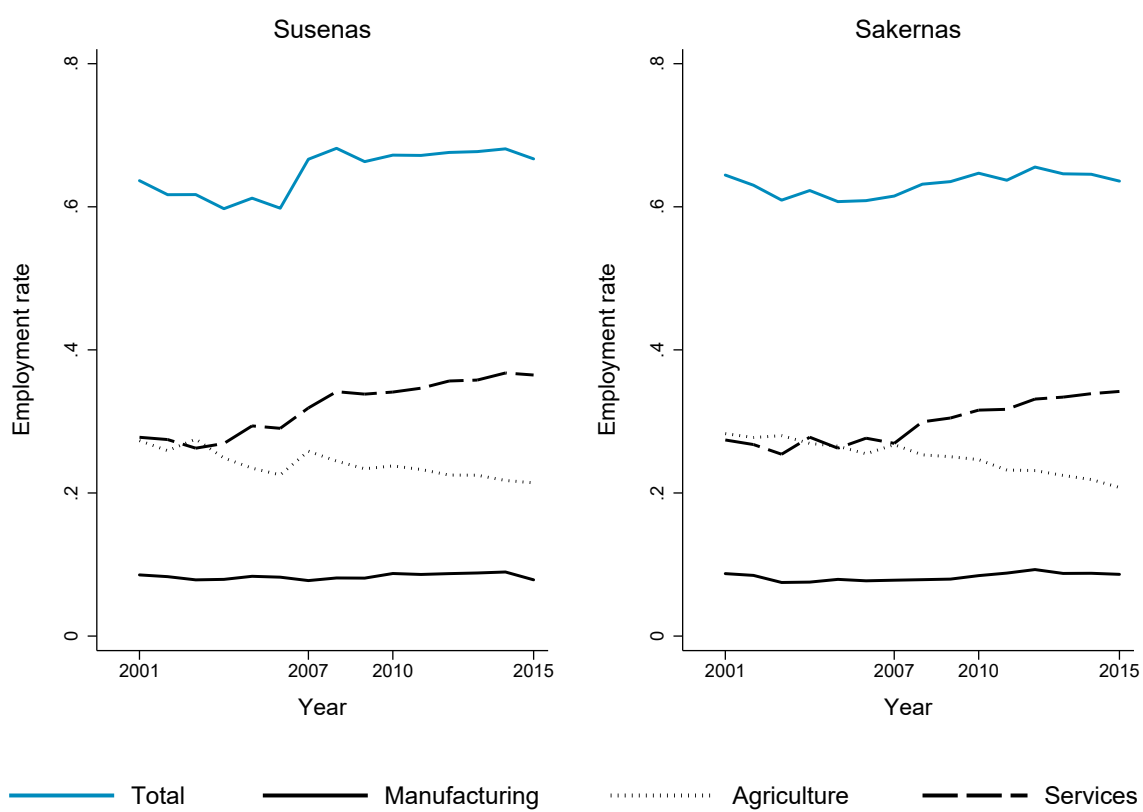
A.1 Additional figures

Figure A1: Density distribution of local regulatory penetration (LRP)



Note: Values are re-scaled by factor 100.

Figure A2: Sectoral employment rate in *Susenas* and *Sakernas* over time



Note: Based on own calculations. See table A1 for summary statistics.

A.2 Additional tables

Table A1: Summary statistics of the *Susenas* and *Sakernas* samples

	Susenas		Sakernas		Sakernas (wage sample)	
	Mean	SD	Mean	SD	Mean	SD
<i>Personal characteristics:</i>						
Age	34.38	12.84	34.78	13.14	35.71	11.13
Female	0.49	0.50	0.50	0.50	0.29	0.45
Marital status: married	0.68	0.47	0.67	0.47	0.71	0.45
Less than primary school	0.14	0.35	0.16	0.37	0.11	0.32
Primary school	0.33	0.47	0.32	0.46	0.27	0.45
Junior high school	0.22	0.42	0.23	0.42	0.20	0.40
Senior high school	0.24	0.43	0.23	0.42	0.30	0.46
Diploma or higher	0.07	0.25	0.06	0.24	0.11	0.32
Urban area	0.50	0.50	0.47	0.50	0.61	0.49
<i>Employment and earnings:</i>						
Employment status	0.66	0.47	0.63	0.48	1.00	0.00
Works in manufacturing	0.08	0.28	0.08	0.28	0.19	0.39
Works in agriculture	0.24	0.43	0.25	0.43	0.17	0.38
Works in services	0.32	0.47	0.30	0.46	0.64	0.48
Total work hours from main job in past week			39.35	17.50	48.15	12.09
asinh total earnings per hour (2008)			9.05	0.82	9.02	0.79

Note: Summary statistics are based on individual level data from *Susenas* and *Sakernas* between 2001 and 2015. The number of observations in *Susenas* is 9,739,221 (representative for 2,141 million individuals in total or approximately 143 million individuals per year). The number of observations in *Sakernas* is 5,816,941 (representative for 2,319 million individuals in total or approximately 724 million individuals per year). The number of observations in the wage sample of *Sakernas* is 1,696,953 (representative for 724 million individuals, or approximately 483 million individuals per year).

Table A2: Sectoral composition of LRP in selected years

Industry	Contribution to LRP		
	2001	2007	2015
Food products and beverages	0.00	0.15	0.21
Tobacco products	0.00	0.09	0.12
Textiles	0.00	0.01	0.01
Wearing apparel	0.00	0.00	0.09
Leather and leather products	0.00	0.00	0.00
Wood and wood products	0.18	0.22	0.37
Pulp, paper and paper products	0.04	0.04	0.04
Publishing, printing and media	0.00	0.01	0.01
Coke, refined petroleum products	0.00	0.00	0.00
Chemicals and chemical products	0.01	0.04	0.03
Rubber and plastics products	0.00	0.00	0.05
Other non-metallic mineral products	0.00	0.04	0.01
Basic metals	0.00	0.00	0.00
Fabricated metal products	0.00	0.01	0.01
Machinery and equipment	0.00	0.00	0.00
Electrical equipment, office machinery	0.00	0.00	0.00
Radio, television and communication equipment	0.00	0.00	0.00
Medical, precision and optical instruments	0.00	0.00	0.00
Motor vehicles	0.00	0.00	0.00
Other transport equipment	0.00	0.01	0.02
Furniture and n.e.c.	0.00	0.02	0.02
Local regulatory penetration	0.23	0.64	0.98

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

Table A3: Robustness: Linear trends in agglomeration measures

Dependent variable:	asinh(Total employment)					
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	0.0213*** (0.0057)	0.0141** (0.0062)	0.0179*** (0.0053)	0.0118** (0.0051)	0.0131** (0.0062)	0.0072* (0.0041)
Never regulated product L \times trend		0.1692*** (0.0583)			0.1565** (0.0580)	0.1520** (0.0599)
Never regulated firm L \times trend			0.3246* (0.1699)		0.1438 (0.1608)	0.1375 (0.1601)
Ever regulated firm L \times trend				0.1013* (0.0524)		0.0655 (0.0575)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,779	4,779	4,779	4,779	4,779	4,779

Note: The dependent variable is the inverse hyperbolic sine of total employment. All regressions include district and island-year fixed effects. Columns 2 to 6 allow for linear trends in initial shares of never (ever) regulated firm or product employment in total district labor force. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A4: Impact of local regulatory penetration on total active population

Dependent variable: $\text{asinh}(N)$	Female	Male	Low-skilled	High-skilled
	(1)	(2)	(3)	(4)
<i>Panel A: Sample period 2001-2015</i>				
LRP	0.0061 (0.0049)	0.0046*** (0.0010)	-0.0028 (0.0051)	0.0027 (0.0026)
$\text{asinh}(\text{Working-age population})$	0.9941*** (0.0280)	0.9855*** (0.0085)	1.2882*** (0.1299)	0.9648*** (0.0045)
Observations	4.779	4.779	4.779	4.779
<i>Panel B: Sample period 2007-2015</i>				
LRP	-0.0091* (0.0048)	0.0014 (0.0018)	0.0061 (0.0080)	0.0136 (0.0080)
$\text{asinh}(\text{Working-age population})$	0.9263*** (0.0400)	0.9796*** (0.0160)	0.0677 (0.0623)	0.3236** (0.1250)
Observations	2.879	2.879	2.879	2.879
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
Linear trends in initial condition	Yes	Yes	Yes	Yes

Note: The dependent variable is the inverse hyperbolic sine of total active population (N) by gender or skill group. Initial district conditions include the level of LRP and the dependent variable as well as the share of manufacturing employment in total district population, all measured as median of the years 2000 to 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A5: Impact of local regulatory penetration on working hours per worker

Dependent variable: Hours per worker	Manufacturing		Agriculture		Services		Female		Male		Low-skilled		High-skilled	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
<i>Panel A: Sample period 2001-2015</i>														
LRP	0.5788*** (0.1048)		0.1571 (0.1841)		0.2805*** (0.0760)		0.2082* (0.1151)		0.2118** (0.0932)		0.2288** (0.1084)		0.2913*** (0.0915)	
Observations	4,589		4,731		4,760		4,778		4,779		4,777		4,775	
<i>Panel B: Sample period 2007-2015</i>														
LRP	0.3156*** (0.1120)		-0.0555 (0.1263)		0.0114 (0.1088)		-0.0734 (0.0963)		0.0059 (0.0853)		-0.0131 (0.0975)		-0.0198 (0.0807)	
Observations	2,771		2,833		2,870		2,879		2,879		2,879		2,879	
District FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Island-year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Linear trends in initial condition	Yes		Yes		Yes		Yes		Yes		Yes		Yes	

Note: The dependent variable is working hours per worker by sector, gender or skill group. Initial district conditions include the level of LRP and the dependent variable as well as the share of manufacturing employment in total district population, all measured as median of the years 2000 to 2005. Robust standard errors are clustered on province level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).