

# Product Market Monopolies and Labor Market Monopsonies

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## Abstract

This paper unveils a novel externality of product market regulation in the labor market. It shows theoretically and empirically that higher barriers to entry in product markets translate into higher employers' labor market power, which we measure by the wage markdown—the ratio between the marginal product of labor and the wage. The literature suggests that this wedge can distort factor allocation resulting in lower aggregate output and employment, but also in higher inequality through a reduction in the labor share of national output. Using variation in investment restrictions across 346 manufacturing product markets in Indonesia, we find that wage markdowns increase by 25% in product markets that become subject to investment restrictions. The result is rationalised using a simple oligopsony model in which higher entry costs reduce the equilibrium number of firms, thereby limiting employment options for workers and—hence—their labor market power. Instrumental variable estimates support the predictions of the model that lower entry is the main driver of the positive relation between investment restrictions and wage markdowns.

**Keywords:** product market regulation; labor market power; markdown; firm entry; minimum wage

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# 1 Introduction

Labor market power is an increasingly important source of market distortions in modern economies (Yeh et al., 2022; Benmelech et al., 2022; Mertens, 2022), as it typically allows firms to pay a wage below the marginal product of labor.<sup>1</sup> The wedge between wages and the marginal product of labor has important economic implications. By departing from allocative efficiency, labor market power reduces the economy’s overall output and employment.<sup>2</sup> By depressing employment and wages, market power can also reduce the labor share of national output (Naidu et al., 2018; Brooks et al., 2021; Mertens, 2022), a key measure of inequality that has been declining in most of the world (e.g. Autor et al., 2020; Brooks et al., 2021; Gutiérrez and Philippon, 2017; Karabarbounis and Neiman, 2014; De Loecker and Eeckhout, 2018).

Understanding the determinants of labor market power is thus crucial to address its potentially distortionary effects. The literature has posited a positive relation between labor market concentration and employers’ market power, which is consistent with wages being lower in more concentrated labor markets (Amodio et al., 2022; Benmelech et al., 2022). On the basis of this intuitive relationship, some authors have proposed to extend antitrust approaches used to regulate product markets to regulate labor markets (Naidu et al., 2018; Marinescu et al., 2021). However systematic evidence of a causal relation between market concentration and labor market power remains elusive.

This paper starts to fill this gap by studying how changes in regulatory barriers to entry affect firms’ labor market power. To guide the empirical analysis, we build a simple model in which a finite number of employers compete strategically to attract workers. In the model, de-regulating product markets lowers entry costs and increases the equilibrium number of firms. This in turn raises the number of alternative employment opportunities for workers. The resulting loss of employers’ labor market power reduces their ability to pay wages below the marginal product of labor, i.e. to impose a positive wage markdown.

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<sup>1</sup>While in principle also workers could enjoy labor market power, in practice the evidence of wages above marginal product of labor is limited.

<sup>2</sup> Naidu et al. (2016) estimate that labor market power by U.S. firms reduces overall output and employment by 13 percent.

We test our theory empirically focusing on the case of Indonesian manufacturing, where our estimates suggest that the vast majority of plants exert some degree of labor market power. Specifically, we take advantage of quasi-exogenous variation in investment restrictions across 346 narrowly-defined manufacturing product markets in Indonesia. Such restrictions are implemented by the government through the publication of the Negative Investment List (NIL), a Presidential regulation which details the conditions that new investors have to fulfill to register a company in any Indonesian sector. After addressing concerns of potential endogeneity of changes to the NIL and product market trends, we exploit changes in restrictions across product markets in 2011 to estimate the causal impact of product market regulation on wage markdowns.

To estimate markdowns, we use granular information on prices and quantities of 9-digit products and intermediate inputs from a highly representative sample of Indonesian manufacturing plants.<sup>3</sup> This constitutes an advantage over previous studies, which often rely on industry-level price indices to deflate nominal quantities, incurring in several sources of bias (Bond et al., 2021; De Loecker et al., 2016; Foster et al., 2008). Importantly, we also disentangle wage markdowns from price markups, which are embedded in the “naive” comparison of marginal product of labor and the wage paid to employees.

Using this approach, we provide reduced-form estimates of the elasticity of wage markdowns to investment restrictions, which is around 0.25. This implies that NIL-related entry barriers in the product market have increased markdowns by around 4.3% in our sample. The results are robust to a battery of tests and are also consistent with an event study regression based on a change of the NIL as the “event”. The absence of pre-trends in the event study further relieves endogeneity concerns for the instrument. In addition, we probe the robustness of our results using an alternative identification strategy based on a Bartik instrument exploiting the differential exposure to regulated product markets across 274 commuting zones. Such design minimises the potential endogeneity of changes to the NIL and labor market power in a give product market, thus providing further reassurance against possible endogeneity biasing our results.

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<sup>3</sup> Indonesian manufacturing data have been extensively used in previous work (e.g. Amiti and Konings, 2007; Javorcik, 2004; Arnold and Javorcik, 2009; Hallward-Driemeier and Rijkers, 2013).

We then test empirically to what extent firm entry can explain this effect, as postulated by the model. To that end, we first show that investment restrictions are indeed powerful predictors of subsequent entry by manufacturing firms at the product market-level. We then build on this finding and instrument the entry in a product market with investment restrictions using the NIL. The resulting Two-Stage Least-Squares (2SLS) estimates imply an elasticity of markdown with respect to product market regulation that is almost identical to the reduced form elasticity. This supports the channel identified in the model as driving the relation between product market regulation and labor market power.

The simple, yet understudied relationship between product market regulation and labor market power is highly policy-relevant. If product market regulation affects labor market power, as our evidence suggests, it can also be used as a policy tool to mitigate labor exploitation and rising inequality (Marinescu and Hovenkamp, 2019; Naidu et al., 2018). This seems particularly relevant in light of the growing evidence of oligopsony power in labor markets around the world (e.g. Yeh et al., 2022; Dube et al., 2020; Brooks et al., 2021; Naidu et al., 2016; Mertens, 2022).

The paper is related to several streams of literature. First, we contribute to the literature looking at concentration in the labor market (Azar et al., 2020; Dube et al., 2020; Benmelech et al., 2022; Yeh et al., 2022; Marinescu et al., 2021; Arnold, 2019; Schubert et al., 2021). Since these papers are based on the United States, our focus on Indonesia is one of the features that sets our study apart from the literature. In fact, labor market concentration is especially likely to harm workers' welfare in a developing country, where the geographic mobility of labor is more limited and the levels of skills and compliance with labor regulation are lower.<sup>4</sup> Indeed, our estimates suggest that between 95 and 97 percent of the plants in our sample has some labor market power, as captured by a markdown larger than one. Moreover, none of the above-mentioned studies examine the link between product market regulation and labor market power, as we do in this paper.

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<sup>4</sup> Two exceptions are Naidu et al. (2016) and Brooks et al. (2021), which base their analysis on the United Arab Emirates, China and India.

More specifically, our paper is related to the literature on monopsony and oligopsony power in the labor market (Berger et al., 2022; Burdett and Mortensen, 1998; Card et al., 2018; Manning, 2013, 2003).<sup>5</sup> Our contribution to this literature is linking product market regulation to labor market oligopsony power. In this context, a related paper is Blanchard and Giavazzi (2003), which examines the link between product market regulation and the labor market. However it does not explicitly model strategic interaction among employers, nor does it test empirically the relationship between regulation and labor market power.

Second, we join a growing literature on market power. Among these papers, Autor et al. (2020) and De Loecker et al. (2020) are agnostic about the sources of increasing product market concentration. De Loecker et al. (2021) examines the role of market structure for labor market outcomes using a general equilibrium model. However, they abstract from oligopsony power in the labor market. Gutiérrez and Philippon (2017) and Gutiérrez et al. (2018) point to the regulation as the main driver of increasing concentration. We add to these papers by providing evidence of a causal relationship between product market and labor market power using highly granular information on product market reforms and plant-level markdowns.

Finally, we contribute to the literature measuring firm-level market power using the production approach (Hall et al., 1986; Hall, 1988; De Loecker and Warzynski, 2012; De Loecker et al., 2016). Like in Yeh et al. (2022), Dobbelaere and Mairesse (2013), Morlacco (2019) and Mertens (2022), we apply such methodology to estimate wage markdowns. However, unlike most of the literature, we observe quantity and values of 9-digit-level products and inputs used by plants. This allows us to compute plant-specific output and inputs price deflators that help mitigating the bias arising from using revenue-based measures for the computation of markdowns (e.g. Bond et al., 2021).

The rest of the paper is organised as follows: Section 2 presents a simple model linking wage markdowns and product market regulation; Section 3 describes the data used to test empirically the predictions of the the model; Section 4 describes our procedure to estimate markdowns; Section 5 illustrates the econometric approach and addresses the

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<sup>5</sup> See Bhaskar et al. (2002) for a survey.

potential selection of product markets into reforming activity; Section 6 presents the results relating product market regulation to wage markdowns, and Section 7 concludes.

## 2 The Link Between Product Market Regulation and Labor Market Power

We present a simple model to elucidate how changes in product market regulation that affect firms' entry could affect firms' labor market power. Consider a commuting zone  $m$  populated by a finite number of firms, indexed by  $f$ . Firms produce horizontally differentiated products, indexed by  $i$ .

We assume that it is infinitely costly for workers to commute across different commuting zones—an assumption consistent with our data, as discussed further below, as well as with evidence from advanced economies (Kennan and Walker, 2011).<sup>6</sup>

As a benchmark case, we assume that labor supply is commuting zone and product-specific, for instance due to the skill requirements for producing a certain good.<sup>7</sup> This assumption simplifies the analysis because it allows us to consider firms in a product market-commuting zone pair  $\{m, i\}$  independently of other firms in  $m$ . We relax this assumption in a model extension in Online Appendix E, and in Section 6 we present the empirical results of modifying our estimator accordingly.

Wages are determined by the inverse labor supply function  $W_m(\cdot)$ , which we assume to be an increasing, continuous and differentiable function of aggregate product market-commuting zone-specific labor demand,  $\mathbf{L}_{mi}$ . We index the function by  $m$  to capture the idea that the labor supply elasticity is likely location-specific, for instance due to the quality of transportation infrastructure that affects the disutility of labor.

We assume that the number of firms in a local labor market, a product market-commuting zone pair, is finite and given by  $N_{mi}$ . Hence, the labor market is not perfectly competitive, which implies that employers have labor market power:  $W_m(\cdot)$  is upward-

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<sup>6</sup> One would expect commuting costs to be on average higher in developing countries, where the transportation infrastructure is usually less efficient than in advanced economies.

<sup>7</sup> Berger et al. (2022) uses a similar definition of labor market in the United States, a commuting zone and three-digit industry.

sloping but less than perfectly elastic.<sup>8</sup>

To simplify the notation, we omit the  $f$  index from all firm-level variables. Let  $L$  denoting firm-level employment. Firms' output is given by  $Q = F(L)$ , where  $F(\cdot)$  is an increasing and concave function. We use the output price as the numeraire.

The profit function is given by:

$$\Pi(L, \mathbf{L}_{mi}) \equiv F(L) - W_m(\mathbf{L}_{mi})L \quad (1)$$

Firms' optimal labor demand is obtained from profit maximization, which implies differentiating (1) with respect to labor:

$$F'(L) = W'_m(\mathbf{L}_{mi}) \cdot L + W_m(\mathbf{L}_{mi}) \quad (2)$$

where  $F'(L)$  denotes the partial derivative of  $F$  with respect to  $L$ .

Our focus is to study firms' labor market power, which as in the literature we capture through the wage markdown, i.e. the wedge between the marginal product of labor and the wage paid to the workers. We divide Equation (2) by  $W_m(\mathbf{L}_{mi})$  to derive the wage markdown  $\nu$ :

$$\nu \equiv \frac{F'(L)}{W_m(\mathbf{L}_{mi})} = 1 + \varepsilon_m \cdot \frac{L}{\mathbf{L}_{mi}} \quad (3)$$

where  $\varepsilon_m \equiv \frac{W'_m(\mathbf{L}_{mi})\mathbf{L}_{mi}}{W_m(\mathbf{L}_{mi})}$  denotes the inverse labor supply elasticity. The last term is the firm employment share within the labor market.

Equation (3) shows that the markdown is decreasing in the labor supply elasticity and increasing in the employment share. The intuition behind Equation (3) is that workers have few alternative employment opportunities when the labor market is concentrated, i.e. a few firms employ most workers, which allows employers to extract rents. This implies that any factor increasing firms' employment shares within a labor market tends to increase wage markdowns.

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<sup>8</sup> Since labor market power arises because employers are "scarce", our model can be considered as one of classical oligopsony, as opposed to alternative theories based on search frictions or heterogeneous workers' preferences over jobs (Manning, 2003; Bhaskar et al., 2002).

We focus on the symmetric Cournot equilibrium, for which existence and uniqueness are discussed in [Amir and Lambson \(2000\)](#) for the oligopoly case. We adapt these sufficient conditions for the oligopsony case in [Online Appendix B.1](#). In a symmetric equilibrium,  $\mathbf{L}_{mi} = L \times N_{mi}$ .

The number of active firms is determined endogenously by the free-entry condition:

$$\Pi(N_{mi}) = \chi_i \tag{4}$$

where  $\chi_i$  represents a fixed entry cost that entrant firms need to pay to produce and sell their product in the respective product market.

[Online Appendix B.2](#) shows that a sufficient (but not necessary) condition for the profit function to be monotonically decreasing in  $N_{mi}$  is  $W_m''(\mathbf{L}_{mi}) \geq 0$ . Assuming that condition to hold and denoting by  $\tilde{\Pi}(\cdot)$  the inverse profit function, [Equation \(3\)](#) can be written as:

$$\nu = 1 + \frac{\varepsilon_m}{\tilde{\Pi}(\chi_i)} \tag{5}$$

If  $W_m(\cdot)$  is a constant elasticity of substitution function (CES), then  $\varepsilon_m$  is constant and the impact of product market regulation on markdowns is unambiguously positive.<sup>9</sup>

### 3 Data

We test empirically the insights of the model on Indonesian manufacturing plants. To that end we exploit two main sources of data: an extensive panel of manufacturing plants and data on product market regulation over time, which we manually code.

#### 3.1 Manufacturing Data

Plant-level data are taken from the Indonesian survey of manufacturing plants with at least 20 employees (*Statistik Industri, SI*) administered by the *Indonesian Statistical Office*

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<sup>9</sup> The impact of product market regulation on markdown is ambiguous in the model extension with homogeneous labor within a commuting zone, as shown in [Online Appendix E](#).



(BPS). The coverage of the survey is extensive; in fact it becomes an actual census in 1996 and 2006 and it is very close to a census in the remaining years, hence ensuring high representatives even for narrowly-defined industries and geographic areas. Plants are grouped into 5-digits industries following the definition *Klasifikasi Baku Lapangan Usaha Indonesia* (KBLI), a classification mostly compatible with ISIC coding. One challenge of the *Statistik Industri* data is the lack of complete series of capital stock. To address the issue, we develop an algorithm described in Online Appendix F.1. Once we obtain clean capital series, we deflate them using price indexes from BPS, distinguishing between machinery and equipment, vehicles, buildings, and land.

To deflate nominal quantities, we construct plant-specific output and materials price indexes, as in Eslava et al. (2004). To do so, we exploit the fact that our data include information on quantities and values of the products produced and materials used in production. These are both defined at a highly granular level, namely 9-digits *Klasifikasi Komoditi Indonesia* (KKI), a more detailed classification based on KBLI.

In our sample, each plant produces on average 2 products, 25% of the plants produce more than one product, and each plant uses four different varieties of raw inputs. Online Appendix F.2 describes in detail the methodology we use to clean product- and input-level data. After computing unit prices by dividing value with quantities, we use them to construct plant-level output and input price deflators (see Online Appendix F.3).

As discussed in Section 4 below, we use energy consumption to proxy for unobserved productivity to estimate plant-level production functions. To do so, we take advantage of the unusual feature of our data, which provide information on the *quantity* of energy used, by energy type.<sup>10</sup>

In our empirical examination, we use Indonesian regencies to identify commuting zones. Regencies are the second level of sub-national administrative divisions (the first being the province). A number of features make it a reasonable proxy for a commuting zone in Indonesia. First, the mobility of labor is limited across regencies. In 2010 for

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<sup>10</sup> Specifically, we consider electricity, gasoline, diesel, lubricants. These constitute roughly ninety percent of energy consumption in our sample and are reported separately in all years of the sample. To convert liters of fossil fuels into KWh equivalents, we used the following standard conversion factors: 1 litre of Diesel corresponds to 10 kWh; gasoline: 9.1 kWh; lubricants: 11 kWh.

example, only five percent of the workforce worked in a different regency than their residence.<sup>11</sup> Second, regencies hold significant administrative powers following the 1999 decentralization reform in Indonesia. Those include also the minimum wage setting. We use the pre-decentralization reform division in 292 regencies to ensure the consistency of the analysis over time.

In our sample, 34% of plants change product market over the sample and 5% of plants change commuting zone. To ensure consistency over the years, we drop all such plants. However, our results are robust to just dropping observations after plants change product or location, or to not dropping them at all.

After cleaning the data and keeping observations with non-missing values for all the dependent variables considered, we end up with an unbalanced panel of 14,142 plants between 2009 and 2014—including 346 product markets (5-digit industries) and 274 commuting zones (regencies). We take the census year 2006 as our base year to maximise representativeness. Online Appendix Table A1 presents summary statistics of all variables used in the empirical analysis.

### 3.2 Measuring Product Market Regulation

Bringing the insights of the model in Section 2 to the data requires identifying a suitable measure of entry costs and an exogenous source of variation. To that end, we collect granular data on investment restrictions across Indonesian product-markets and exploit two consecutive Indonesian Presidential Decrees—*Daftar Negatif Investasi*, or Negative Investment List (NIL).

The concept of a NIL was introduced in Indonesia in 2000 through the Presidential Decree 96/2000, which aimed to create a single repository of the many existing sectoral restrictions on investments at the central government-level. In fact, it was not until 2008 that the NIL approach was properly enforced, as the Investment Law No.25/2007 replaced the old Investment Law (No. 1/1967), which used a *positive* list approach to restrictions, i.e. whatever market is not included in the regulations is to be considered closed to

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<sup>11</sup>Indonesia’s island geography and often underdeveloped transportation infrastructures make the hypothesis of limited mobility likely to hold.

investments. This was followed by the Presidential Decree 77/2007, which provided the first official NIL in Indonesia.<sup>12</sup>

In addition to having a more coherent legal basis for enforcing the NIL, this Decree was the first to explicitly list the conditions on investments across product markets.<sup>13</sup> In addition, for the first time the Decree provided a consistent definition of product market based on the 5-digit KBLI classification.<sup>14</sup> To ensure the consistency of these regulatory changes, we center our analysis around the NIL revision in 2011.<sup>15</sup>

There are four main types of restriction coded in the NIL. The first concerns markets that are fully closed to private investment, both domestic and foreign. These included products and services that are considered sensitive from political, security or moral point of views, such as chemical weapons, alcoholic beverages, radio and television public broadcasting services, and casinos. The second includes limitations to foreign equity ownership, which vary between 0 percent (i.e. open only to domestic investors) and 95 percent. In 2008, this list included hundreds of business fields and 13.3 percent of the universe of KBLI 5-digit product markets. The third type of restrictions consists of reserving certain business activities to Micro-Small and Medium Enterprises (MSMEs) in full, or of requiring investors to partner with MSMEs. The reservation to MSMEs *de jure* excludes foreign firms from the activity, as foreign investors cannot operate an MSME in Indonesia. Finally, special licenses, usually from a Ministry, are required to produce certain products or services. For example, in 2008 investors in plywood industries needed a permit that ensured the country had a sufficient supply of raw materials, and producers of narcotics needed a special license/permit from the Minister of Health.

We obtain a combined indicator of investment restrictiveness, our measure of product market regulation, in each 5-digits KBLI market-year pairs. Specifically, we construct a

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<sup>12</sup> All Presidential Decrees are announced at the end of the calendar year, which implies that they become effective at the beginning of the next year. We follow this convention in constructing our dataset.

<sup>13</sup> Only a few sectoral laws included investment restrictions which were not included in the Presidential Decree. These restrictions were still legally valid as laws have higher legal standings than decrees in Indonesia. As discussed below, we complement the investment restrictions data in the Presidential Decrees with these sectoral restrictions.

<sup>14</sup> This differs from the definition in the previous Presidential Decision, which used only general industry descriptions, thus making it difficult to identify variation across product markets.

<sup>15</sup> Further revisions were enacted since 2015, but these extend beyond the coverage of the our manufacturing plants' survey.

dummy variable  $nil = 1$  if in a given year the product market is on the NIL for at least one of the following reasons: i) foreign equity limit is below 100 percent; ii) it is subject to special license requirements; iii) it is reserved to MSMEs; iv) it is closed to investments altogether, and  $nil = 0$  otherwise. This variable broadly mirrors existing indicators of product market regulation involving explicit barriers to investment, administrative burdens and state involvement (e.g. the PMR indicator of the OECD). However,  $nil$  is coded at a much more granular level, allowing us to identify the impacts at the product market-level.

The Government changed the NIL in January 2011 (with the Presidential Decree No.36/2010), which we exploit for our identification. Changes were made both in terms of product coverage and types of restrictions. Such changes did not apply to incumbents, but only to new investors.<sup>16</sup> While it is hard to suppose that changes to the NIL are random, we show below that they do not appear related to the pre-reform evolution of our key variables of interest, particularly wage markdowns. We also include a full set of sector-year fixed effects and a wide-array of product-market characteristics (interacted by time effects) plausibly related to the NIL determination to address further endogeneity concerns.

In our sample, roughly 15 percent of the 346 manufacturing product markets are subject to investment restrictions through the period. On average, product market regulation has been tightening. The share of plants subject to the NIL increased from 9 percent in 2009—the first year of our sample—to 17 percent in 2011, when the NIL has changed. Online Appendix Figure A1 shows the percentage of regulated plants within 2-digit industries in 2009 and 2011. Even aggregating products into 2-digit industries, there is a considerable amount of cross-sectional and time variation, which is what we exploit for our identification.

While it is not clear what are the factors underlying the government’ reforming activity, it is safe to assume that restrictions are not randomly allocated across product markets. Section 5.1 below presents several endogeneity tests and discusses how the empirical

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<sup>16</sup> Hence they represent pure changes to entry costs, making this regulation particularly suitable to proxy for  $\chi_i$  in Equation (5).

models address the potential endogeneity of changes to the NIL and wage markdowns.

## 4 Estimating Employers' Labor Market Power

Our key dependent variable is labor market power, which in line with the literature we measure by the wage markdown. Our baseline strategy is to compute it on the basis of Equation (3), by estimating the parameters of  $F(\cdot)$ , computing  $F'(\cdot)$ , and then dividing it by the average plant-level wage, which we directly observe in the data.

Our approach relies on the common assumption of producers' optimising behavior and it does not require *a priori* assumptions about the presence of market power. Calculating the markdown in this way is equivalent to adopting the production approach introduced by Hall et al. (1986); Hall (1988) and later popularised by De Loecker and Warzynski (2012) and De Loecker et al. (2016).<sup>17</sup> Among others, Dobbelaere and Mairesse (2013), Morlacco (2019), Mertens (2022) and Yeh et al. (2022) apply this methodology to compute wage markdowns.<sup>18</sup>

To estimate the plant-level markdown, we adopt a structural value added specification with capital, labor and materials having some degree of cross-substitution, and energy being a complement to the combination of the other inputs (see the formal markdown derivation in Online Appendix C. This has three advantages in our framework. First, it allows us to avoid estimating the output elasticity of energy. In our data, within-industry variation of energy consumption is low and therefore it is hard to identify the energy parameters in the production function. Second, by treating labor and materials as flexible inputs in the production function, this approach delivers two first order conditions that allow us to separate labor from product market power, as discussed in Online Appendix D. Third, this specification overcomes the identification issues of gross output specifications

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<sup>17</sup> To see this, let  $\theta^L \equiv \frac{F'(L)}{F(L)}L$  be the output elasticity of labor. Then,  $\frac{F'(L)}{W_m(\mathbf{L}_{mi})} = \theta^L \frac{F(L)}{W_m(\mathbf{L}_{mi})L}$ , where the right-hand-side is the familiar expression for market power. A detailed derivation is provided in Online Appendix D.

<sup>18</sup> An alternative approach would be estimating the inverse labor supply elasticity  $\varepsilon_m$ , as in Dube et al. (2020), Card et al. (2018) or (Berger et al., 2022). However, that would require either making strong assumptions on the labor supply curves  $W_m(\cdot)$ , or having a plausibly exogenous source of variation shifting labor supply, which is not available to us.

discussed in [Akerberg et al. \(2015\)](#) and [Gandhi et al. \(2020\)](#) among others.

While our estimation choices to estimate plant-level markdowns appear the most suitable given the context, there are alternative specifications used in the literature. In [Section 6](#), we probe the robustness of our estimates to using some of these alternatives, including a specification based on gross output instead of value added, and using materials rather than energy to proxy for unobserved productivity.

As in most of the literature estimating market power with the production approach, we treat capital as a dynamic input, and labor and materials as static inputs. Production functions are separately estimated for the 24 2-digits industries in our data.. We apply the two-steps estimator of [Akerberg et al. \(2015\)](#) in order to address the transmission bias due to the endogeneity of static inputs and unobserved productivity, which we proxy using energy consumption.

Our data has the unusual feature of including both quantity and values of both products produced and inputs used by plants at a highly disaggregated level (9-digit ISIC type classification). The richness of the data allows us to estimate markdowns more precisely than in most of the existing literature. First, we can compute plant-specific output and inputs price deflators.<sup>19</sup> This is a strong advantage over existing studies using aggregate inputs variables or industry-level price deflators (e.g. [Yeh et al., 2022](#)). Such aggregate approaches essentially assume that prices are identical across plants within an industry ([Klette and Griliches, 1996](#); [Foster et al., 2008](#); [Bond et al., 2021](#)), and thus they are subject to input price-bias ([De Loecker et al., 2016](#)). Second, our data enables us to rely on output elasticities, which is considered more appropriate than revenue elasticities for the estimation of wage markdowns ([Bond et al., 2021](#)). Finally, using KWh equivalents rather than energy expenditures to proxy for unobserved productivity—as it is done in most of the literature—mitigates the bias introduced by energy price fluctuations that are not necessarily related to changes in productivity.

We employ three additional corrections to our estimator. First, a growing literature

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<sup>19</sup> While in principle we could use actual quantities to estimate the production function parameters, we prefer to use plant-specific deflators in order to avoid issues related to multi-product plants and inputs being expressed in different units.

emphasise that the wedge between the marginal product of labor and the wage paid to workers reflects both labor and product market power (e.g. [Yeh et al., 2022](#); [Mertens, 2022](#); [Dube et al., 2020](#)). Online Appendix D shows the detail of our approach to purge the impact of product market power from the markdowns. Second, similarly to [De Loecker and Warzynski \(2012\)](#), [De Loecker et al. \(2016\)](#) and [Doraszelki and Jaumandreu \(2013\)](#) among others, we allow product market regulation to affect expected productivity when estimating the production function parameters. Third, as in [De Loecker and Warzynski \(2012\)](#), we purge cross-plant and year variation in factor expenditures using the first stage residuals, which mitigates the concern that the variation observed in the data is biased by measurement errors. All details are presented in Online Appendix C

Based on this approach, we find that the vast majority of manufacturing plants exert some degree of labor market power. Depending on the specification used, between 95 and 97 percent of plants have markdown larger than unity.<sup>20</sup> Consistently, all the industries in our dataset have average markdown larger than one.<sup>21</sup>

## 5 Econometric Approach

We use the 2011 reform of the NIL to test the prediction of Equation (5): entry costs in a product market increase the markdown of the plants producing in that market. Specifically, we regress the log-markdown on product regulation through the following baseline specification:

$$\ln \nu_{fijmt} = \gamma_0 + \gamma_1 \cdot nil_{it-1} + u_f + u_{jt} + u_{mt} + \varepsilon_{fijmt} \quad (6)$$

The dependent variable is the log of the estimated wage markdown of plant  $f$ , producing the 5-digit product  $i$  within the 2-digit industry  $j$ , located in commuting zone  $m$

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<sup>20</sup> As in [Yeh et al. \(2022\)](#), the level of our estimated markdown is sensitive to whether we use energy or materials as the flexible input. As we explain in the text, we prefer using energy over material, but the share of plants with markdown larger than one is similar between the two cases. However, this is not a relevant issue in our context, since the analysis leverages log-changes over time rather than levels for the identification.

<sup>21</sup> The extent of monopsony power in our sample is higher than in the United States, where [Yeh et al. \(2022\)](#) estimate that 89 percent of industries have average markdown above unity.

in year  $t$ .

We lag  $nil$  by one year to allow plants to adjust to the changes in regulation. Equation (6) includes plant fixed effects  $u_f$ , which absorbs all time-invariant factors specific to the plant. This is especially important in light of the fact that the panel is unbalanced, and so the plant fixed effect accounts for potential selection of plants entering after the reforming year.

We include 2-digit industry-year fixed effect  $u_{jt}$  to absorb the confounding impact of demand and supply shocks, as well as to address potential endogeneity concerns for changes in  $nil$ , as discussed in Section 5.1 below.

The addition of commuting zone-year fixed effects  $u_{mt}$  absorbs the impact of potentially location-specific labor supply elasticity (e.g.  $\varepsilon_m$  in Equation (5)), allowing us to compare changes in markdowns across product markets within a commuting zone. This enables us to interpret each product market as a distinct labor market. Further below, we experiment with alternative definitions of labor market where allowing for greater homogeneity of labor supply across products.<sup>22</sup>

Since  $nil$  varies at the product market-level, we cluster errors at the same level. In order to avoid outliers driving the results, we drop the top and bottom one percent of estimated markdowns.<sup>23</sup> We estimate Equation (6) with OLS.

## 5.1 Endogeneity Concerns

Ideally, regulatory changes to the NIL should be randomly or as good as randomly allocated across products to ensure an unbiased estimation of  $\gamma_1$  in equation 6.<sup>24</sup> However this is unlikely to be the case in our setting. For example, some aspects of product market regulations—notably foreign equity limits—shelter both foreign and domestic firms from competition with foreign producers. So it is plausible that lobbying activity from industry associations may affect regulatory changes. To the extent that these factors are also related to changes in labor market power, the estimates of  $\gamma_1$  would be biased.

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<sup>22</sup> This comes at the expenses of having to aggregate the 5-digit dummies  $nil$  at the 4-digit level.

<sup>23</sup> This procedure does not affect our main results in any meaningful way.

<sup>24</sup> Due to the inclusion of year fixed effects in 6, we are less concerned about the potential endogeneity of the timing of the restrictions.



Similarly, reserving a market partly or fully to MSMEs may reflect the intention by the government to favor these types of firms.

In order to address these endogeneity concerns, we proceed in three steps. First, we rule out that the changes to the NIL are systematically related to pre-reform trends in observables that may also affect wage markdowns. To that end, we regress the product market average growth rate of key economic variables in the pre-reform period (2008-2010) on a dummy taking value 1 if the product market was reformed in 2011. We focus on variables with a plausible impact on the markdown, including output, capital, employment, wages, along with the markdown itself and the share of foreign capital, which is an important determinant of the exposure to NIL. If product market trends within a broader 2-digits industry were related to subsequent reforming activity, we would expect a probit model to estimate a significant coefficient. The results are shown in Online Appendix Table A2, which rules out evidence of pre-trends in these variables. Even if the regulatory changes are not random, this check reassures of the absence of a clear selection of regulatory changes on key observable variables.

The second way in which we deal with the endogeneity concerns is by including a series of controls that should capture many of the factors generating the alleged endogeneity bias. Specifically, the inclusion of the 2-digit industry-year effects in Equation (6) should absorb the impact of a substantial part of the lobbying activity that may be behind the regulatory changes. That is because lobbying is typically organised around sectoral business associations, which tend to be defined according to a similar classification (e.g. textile, garments, processed food, etc.). In addition, we include year effects interacted with base-year product market characteristics that may influence regulatory changes. In particular we use six such characteristics: i) the product average import tariff rate; ii) a dummy equal to one if a product market is subject to at least one non-tariff measure;<sup>25</sup> iii) the market share of politically connected plants.<sup>26</sup> In addition, we include variables

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<sup>25</sup>To control for the impact of non-tariff measures, we aggregate HS ten digit product-level information at the 5-digit industry-level using concordance tables provided by Indonesia Bureau of Statistics. We construct a dummy variable equal to 1 if a 5-digit industry has at least one non-tariff measure, and zero otherwise. The NTM and tariff data comes from Cali et al. (2021).

<sup>26</sup> Politically connected plants are those being identified as having connections with the Suharto regime by Mobarak and Purbasari (2006). There are 246 such plants in our dataset.

that [Genthner and Kis-Katos \(2022\)](#) identify as important correlates to NIL reforming activity in Indonesia. These variables are: iv) a Herfindahl–Hirschman index based on plants’ sales, to account for product market concentration; v) the share of blue collar workers to control for government’ s reluctance in reforming an industry, and vi) the share of output produced by state-owned enterprises (SOE).<sup>27</sup>

The third strategy to address endogeneity of product regulation is to identify the impact of *nil* at the commuting zone- rather than the product market-level. Specifically, for each regency we compute a Bartik-style variable weighting *nil* by the base-year industry-specific employment share in that regency. With this design, the identification is based on the regencies’ differential exposure to reformed industries. The results in [Goldsmith-Pinkham et al. \(2020\)](#) imply that even if reforming activity were endogenous to wage markdowns at the product level, our alternative estimator would be valid as far as local industry composition is uncorrelated to *changes* in markdowns conditionally on the control variables.<sup>28</sup> Thus, by breaking the direct relation between markdown and *nil*, the Bartik design minimises potential endogeneity problem at the product-level. Since we no longer rely on product market-level variation, we can also substitute the 2-digit industry-year fixed effects with 5-digit product market-year fixed effects and control for any time-varying shocks at the product-level, further relieving concerns of endogeneity of the reforming activity.

In order to implement the regency-level estimator, we need to substitute regency-year with province-year effects in Equation (6), since the Bartik variable varies by regency and year. This generates the concern that local time-varying differential shocks across regencies affecting markdowns might be also correlated to local industry specialisation, which would invalidate our identification based on the Bartik instrument.<sup>29</sup> Therefore, we include year effects interacted with regency-level base-year controls that might be related

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<sup>27</sup> We define plants as SOE if they have more than fifty percent of their capital owned by local or central government.

<sup>28</sup> The inclusion of the plant fixed effects absorb the potential correlation between the shares and the level of markdowns.

<sup>29</sup> For instance, the model in Section 2 suggests that any local factor affecting labor supply would affect markdowns. These factors might affect industrial composition too. For instance, economically developed regencies might be more densely populated by specific industries and have better transportation infrastructures, which would result low labor supply elasticity and lower markdowns.

to industrial structure. Specifically, we control for labor market size differences with log-population. As a measure of overall economic development we include log-real output per capita and the share of population with tertiary education. To capture productivity differences, we include log-real manufacturing output per worker. Finally, as an inverse proxy of employment opportunities in non-manufacturing industries, we include the share of manufacturing output.<sup>30</sup> We cluster errors at the regency-level to match the variation of the Bartik instrument.

It is worth noting that the regency-level Bartik design departs from the assumption of product market-specific labor supply underlying our empirical and theoretical approaches used so far. Instead, the assumption here is that labor supply is homogeneous across product markets and hence firms tap into the same labor market—the entire commuting zone  $m$  that we identify with a regency—to employ workers. Under this assumption, regulation in product market  $i$  would create local spillovers through affecting labor market power—and thus the wage markdown—of *all* firms in  $m$ . Such a spillover effect works through the impact on total labor market demand and the unique equilibrium wage, as shown in Online Appendix E.

## 6 Results

The results of estimating Equation (6) are presented in column 1 of Table 1. Imposing entry barriers in a product market increases the wage markdown in that market by 25%. The effect is statistically significant at the 95 percent confidence level. The estimated impact is economically relevant. Given the employment-weighted sample average of *nil* (0.186), the coefficient implies that markdowns are 4.6% higher due to barriers to entry across manufacturing products. This result is consistent with the framework laid out in Section 2 and fits the predictions of Equation (5).

It is useful to complement the baseline specification through an event study design that allows both to visualize the dynamic impact of changes to product regulation and

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<sup>30</sup> Regency-level data comes from the national statistical institute BPS in a dataset assembled by the World Bank.

to check for possible pre-reform trends. The event study estimates in Figure 1 show a significant impact of the NIL, which increases markdowns by nearly 50% relative to the year before the reform and by around 35% relatively to the preceding years. The effect appears to be persistent over two years following the reform.

The event study does not indicate any significant pre-reform trends, which provides further reassurance vis-à-vis endogeneity bias. In our framework, the bias would be particularly problematic if positive, because it would make it more likely to reject the hypothesis that  $\gamma_1 = 0$ . This might happen, for instance, if plants with market power and high markdowns find it easier to obtain protection. However, in the context of trade policy Grossman and Helpman (1994) suggest that the bias is more likely to be negative: protection is more valuable in markets with a large number of entrants and low market power.<sup>31</sup> In this case, the negative bias would result in estimates of  $\gamma_1$  constituting a lower bound for the actual effect.

We test the robustness of the result by using a regency-based identification, which is less exposed to such bias as argued in Section 5.1. The results of the estimation are presented in column 2. Reassuringly, the coefficient remains negative and statistically significant. However, the absolute magnitude of the coefficient is slightly smaller and the coefficient is less precisely estimated than in column 1. This is likely a reflection of the nature of the identification that—unlike the baseline specification—assumes fully homogeneous labor supply across product markets within a commuting zone. To the extent that some skill-specificity exists at the product level, this effectively “dilutes” the impact of the regency-level Bartik instrument relative to the baseline specification.<sup>32</sup>

Given the Bartik-type identifying variable, we follow Goldsmith-Pinkham et al. (2020) and compute the “Rotemberg weights” to assess the relative importance of each product markets in the overall power of the shift-share variable. We find that virtually all the variation exploited by the Bartik variable comes from cross-regency differences in exposure

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<sup>31</sup> For instance, an increase in entry—either actual or perspective—in a given product market, could trigger lobbying efforts to increase entry costs and reduce competition.

<sup>32</sup> This is even more likely if the bias is negative, as suggested by Grossman and Helpman (1994), and so the difference cannot be due to the endogeneity of reforming activity.

to the crude palm oil industry.<sup>33</sup> In our data, roughly ninety percent of plants producing crude palm oil are on the islands of Sumatera and Kalimantan, where oil palm plantations are concentrated. The share of palm oil employment in regencies hosting that industry is generally high, which might explain the prominent role of the product in the Bartik design.

The dependence of the Bartik variable on one industry should not bias the estimates so long as differences in cross-regency exposure to that industry are orthogonal to markdowns conditional on the controls. As variation in exposure to crude palm oil mostly reflects variation in local time-invariant characteristics, such as availability of natural resources (Sloan and Stork, 2010), plant fixed effects would absorb the potential correlation between such characteristics and the level of wage markdown. If any, the remaining time-varying factors correlated to industry exposure and changes in markdowns should be—at least in part—absorbed by the province-year fixed effects and the district-level base-year covariates interacted with year dummies.

In order to further relieve concerns about the reliance of the Bartik instrument on a single product market, we test the robustness of the estimate in column 2 of Table 1 to excluding the palm oil market from the analysis. As shown in Online Appendix Table A3, the coefficient is very similar to the baseline coefficient and still statistically significant. In column 2, we go one step further and drop all product market-regency pairs with employment shares—the weights of the Bartik instrument—larger than 0.5. The coefficient is still significant and similar to the baseline regency-level specification.

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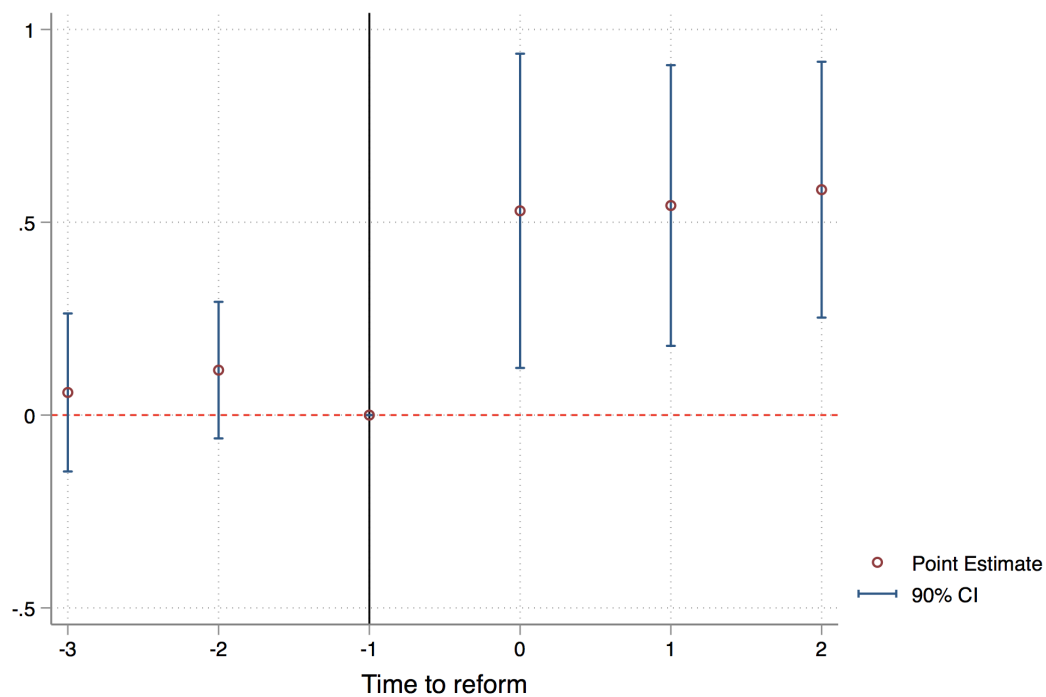
<sup>33</sup> Indonesia is the world’s largest producer of palm oil. The industry accounts for a large share of the country’s export and it employs roughly two percent of Indonesia’s labor force (Gaskell, 2015).

Table 1: Main results: reduced form estimates of the impact of product market regulation on plant-level wage markdowns.

	(1)	(2)
	Markdown	Markdown
NIL = 1 (5-dig product market)	0.256** (0.113)	
Bartik NIL (regency)		0.150* (0.086)
Observations	34,269	38,234
R-squared	0.815	0.812
Base-year covariates-year FE	yes	yes
Plant FE	yes	yes
Industry-year FE	2-dig	5-dig
Commuting zone-year FE	regency	province
SE clustering	5-dig	regency

*Notes:* this table presents OLS estimates of the impact of investment restrictions on plant-level log-wage markdown. In column 1, base-year covariates are: i) a Herfindahl–Hirschman index based on plants’ sales to account for product market concentration; ii) the share of blue collar workers to control for government’s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. In column 2, base-year covariates are: i) log real output per capita; ii) log population; iii) share of population with tertiary education; iv) log real government expenditure on infrastructure; v) log real manufacturing output per manufacturing worker, and vi) the share of manufacturing output. Errors are clustered at the product market-level or commuting zone-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

Figure 1: Event study design: impact of product market regulation on plant-level wage markdowns.



*Notes:* this figure presents OLS estimates of the impact of investment restrictions on plant-level log-wage markdown using an event study design. The event is imposing investment restrictions in 2012. The sample excludes plants in product markets with restrictions already imposed before 2012. Base-year covariates are: i) a Herfindahl–Hirschman index based on plants’ sales to account for product market concentration; ii) the share of blue collar workers to control for government’ s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Errors are clustered at the product market-level or commuting zone-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

## 6.1 Further Robustness Checks

This section presents a battery of tests to support the robustness of the main result in column 1 of Table 1.

First, in Equation (6), the coefficient is identified by comparing markdowns of plants in regulated 5-digits product markets against others within the same 2-digits industry. If labor is mobile across 5-digit markets, then there would be spillovers to the control group and we would underestimate the true impact of product market regulation. This would result in a larger coefficient when using 4-digit labor markets as the identifying units.

To test this hypothesis, we adopt such a broader definition of labor market, i.e. a 4-digit industry-regency pair. This is similar to the definition used by Berger et al. (2022) and Yeh et al. (2022), with the 4-digit classification including 176 manufacturing products in our dataset. To aggregate regulation in 5-digit markets into 4-digit markets, we construct a Bartik-style variable using base-year employment shares as weights. The resulting continuous variable varies between 0 and 1.

The results of using the 4-digit market definition are presented in column 1 of Online Appendix Table A4. The coefficient is positive, statistically significant at 95 percent level, and slightly larger than the baseline coefficient in column 1 of Table 1. This suggests that labor is characterised by some degree of mobility across 5-digit markets within a regency.<sup>34</sup>

Second, technology adoption might be correlated to market power and reforming activity due to winner-takes-all dynamics (e.g. Autor et al., 2020). In this case, not controlling for technology adoption might introduce omitted variable bias. To assess this hypothesis, we construct an index of technological sophistication using base-year plant-level information on R&D units, product and process innovation, use of computers and the Internet.<sup>35</sup> We then interact this index with year effects.

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<sup>34</sup> The result is in line with the significance of the coefficient in column 2 of Table 1. However, the latter is smaller than the 5-digits and 4-digits product market-level coefficients, suggesting that while there is some labor mobility across products, on average labor is not fully homogeneous within a commuting zone.

<sup>35</sup> The index ranges from 0 to 1. It is computed by taking the average of five dummies, each equal to 1 if a plant had any R&D unit, performed product innovation, process innovation, used computers, or the Internet in the base year.



We capture additional aspects of technological adoption by proxying for automation and skill-biased technical change. We interact the base-year shares of mid-skill and high-skilled employment in each plant with year fixed effects.<sup>36</sup> The coefficient in column 2 of Online Appendix Table A4 shows that the *nil* coefficient is robust to controlling for such technological characteristics.

Third, we examine whether the effects are driven by specific product market measures in the NIL. To that end, we replace *nil* with its four individual dummy variable components. The results in Online Appendix Table A5 show that all components but SMEs restrictions exert a positive and significant effect on markdown. On the one hand, this reassures us that different aspects of product market regulation have a consistently similar impact on markdowns. On the other hand, the large size of the coefficients (compared to our main *nil* variable) hints at the presence of multicollinearity, for instance due to some degree of policy complementarity. This finding further justifies our approach of combining them in one single regressor.

Fourth, we check to what extent our results are sensitive to modifying the assumption of structural value added discussed in Section 4. In particular, we follow Yeh et al. (2022) and De Loecker and Warzynski (2012), and use a gross output production function with materials proxying for unobserved productivity, rather than energy consumption.<sup>37</sup> The estimate in column 3 of Online Appendix Table A4 confirms that the impact of product market regulation on labor market power is robust to alternative approaches to estimate the markdown.

## 6.2 Mechanism: Product Market Regulation and Firm Entry

While the reduced-form estimates suggest that product market regulation has a significant impact on employers' labor market power, they are silent about the underlying

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<sup>36</sup> In Cali and Presidente (2022), we show that mid-skill workers in Indonesia are the most exposed category to the risk of automation i.e. they tend to be employed in routine task-intensive occupations (Autor et al., 2003).

<sup>37</sup> In doing so, we need to address the critique of Gandhi et al. (2020), which argue that as a flexible input, materials might lack the adjustment frictions necessary for the identification of output elasticities. We thus add log-real wages in the control function for the unobserved productivity. This choice seems appropriate as in our data real wages are serially correlated and differ substantially across plants.

mechanism. In the model of Section 2, regulatory barriers to entry reduce the number of firms in the market (cf. Equation 4), increasing the markdown (cf. Equation 5).

To test the relevance of such mechanism in our data, we first examine whether changes in product market regulation indeed affect manufacturing plants' entry at the product market-level. Online Appendix G describes the methodological approach and presents the results of regressing entry of manufacturing plants—measured as the number of new plants in a 5-digit product market—on the *nil* variable. Our preferred estimates in column 2 of Online Appendix Table G1 suggest that imposing restrictions in a product market can reduce entry in that market by as much as forty percent.

We then use *nil* to instrument the number of entrants in a product market in a 2SLS framework so as to test to what extent entry can explain the reduced form results in Table 1. Specifically, we estimate the following system of equations:

$$\begin{cases} \ln E_{i,t} = b_0 + b_1 nil_{i,t-1} + BX_{i,t} + u_f + u_{jt} + u_{mt} + \eta_{fijmt} \\ \ln \nu_{fijmt} = \beta_0 + \beta_1 \ln \hat{E}_{i,t} + BX_{i,t} + u_f + u_{jt} + u_{mt} + \varepsilon_{fijmt} \end{cases} \quad (7)$$

where  $E_{i,t}$  is the log-number of entrants and  $\hat{E}_{i,t}$  is the (exogenous) log-number of entrants predicted on the basis of *nil*.<sup>38</sup> We continue to include all control variables of Equation (6), including base-year product market characteristics interacted with year effects, plant-, industry-year and reGENCY-year fixed effects.

Table 2 presents the results. The coefficient in column 1 suggests that a ten percent increase in the number of entrants lowers the markdown by approximately ten percent. Taking the estimated first stage elasticity of entry with respect to *nil* (-0.26), this coefficient implies that product market regulation decreases the number of entrants by 25 percent.<sup>39</sup> Hence, the product market restrictions in NIL increase markdown on average by 25 percent, which is very similar to the reduced form coefficient in column 1 of Table 1. This suggests that virtually all of the impact of product market regulation on markdown comes from the entry of new plants, in line with the model in Section 2.

As the power of the instrument is low—the first stage F-statistic is equal to 5.68—we

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<sup>38</sup> We apply an inverse hyperbolic sine transformation to the log-number to deal with zero entry.

<sup>39</sup> The full tables of the first stage regressions are available upon request.

test the robustness of the result to a more powerful first stage. As discussed in Section 3.2, many of the restrictions in the NIL apply only to foreign investors, notably the limitations on foreign equity shares. Hence, in column 2 we replace total entry with entry of foreign plants.<sup>40</sup> Using this specification, the value of the F-statistic increases up to 12.2, which is above the standard threshold of 10 (Stock et al., 2005). As in column 1, the coefficient is negative and significant, but double in size. Along with the results of the first stage, this implies a similar -0.25 elasticity of wage markdown with respect to product market liberalisation. This evidence is consistent with foreign entrants driving the impact of product liberalisation on labor market power.

Table 2: 2SLS estimates: Impact of product market entry on plant-level wage markdowns.

	(1)	(2)
	Markdown	Markdown
Entry (log-# of entrants, 5-dig product market)	-0.990** (0.438)	
Entry (log-# of foreign entrants, 5-dig product market)		-2.303** (1.158)
Observations	34,269	34,269
Base-year covariates-year FE	yes	yes
Plant FE	yes	yes
Industry-year FE	2-dig	2-dig
Commuting zone-year FE	regency	regency
SE clustering	5-dig	5-dig
First stage coefficient	-0.259** (0.105)	-0.111*** (0.032)
First stage F	6.068	12.20

*Notes:* this table presents 2SLS estimates of the impact of the log-number of entrants in a product market, defined as a 5-digit industry, on plant-level log-wage markdown. An inverse hyperbolic sine transformation is applied to the independent variable in order to deal with zero entry. The log-number of entrants is instrumented with a dummy variable quantifying investment restrictions. Base-year covariates are: i) a Herfindahl–Hirschman index based on plants’ sales to account for product market concentration; ii) the share of blue collar workers to control for government’s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Errors are clustered at the product market-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

### 6.3 The Role of Minimum Wages

The presence of monopsony and oligopsony power often justifies the use of labor market policies to reduce it, notably minimum wages (Manning, 2021). Thus, we conclude the analysis by examining whether product market regulation affects markdowns less when

<sup>40</sup> Foreign entrants are defined as plants with age zero and a positive share of foreign capital. Results—available upon request—are similar when using shares of foreign capital larger than 50% and 90%.

minimum wage policies are in place. This is a relevant question in the context of our analysis, as Indonesia enacted a monthly minimum wage policy at the local level over the period of analysis.<sup>41</sup>

We test for the hypothesis that a tighter minimum wage policy reduces the impact of product market regulation on markdowns by interacting  $nil_{it-1}$  with the regency log-minimum wage.<sup>42</sup> To estimate both the main impact of regency-level (log) minimum wages and their interaction with  $nil_{it-1}$ , we replace regency-year with province-year fixed effects and control for the base-year local covariates discussed in Section 5.1.<sup>43</sup>

The results are presented in column 1 of Online Appendix Table A6. As expected, the coefficient of minimum wages is negative, but it is not statistically significant (coeff. = -.214, s.e. = .170). However, the interaction term is negative and significant. We interpret this evidence to suggest that minimum wage policy is less relevant in unrestricted product markets, where employers' labor market power is lower. However, in restricted markets where employers have substantial power, minimum wages limit the extent in which labor can be exploited. In column 2, we re-include regency-year fixed effects and estimate the interaction term only. We find a similar but even stronger reduction in the impact of NIL on markdown than in column 1.

Thus, we conclude that minimum wages can significantly limit labor exploitation, even when employers' market power originates from the product market.

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<sup>41</sup> The minimum wage in Indonesia is set by the provincial governors on the basis of the recommendation by the regional wage council (or the regency's regent). The council is a tripartite institution comprising members of government, labor unions, and employers. The members negotiate and set the standards of decent cost of living (*Komponen Hidup Layak* or KHL in Indonesian), which forms the basis of the minimum wage recommendation to the regency governor. This recommendation is a central element in the governor's decision on the minimum wage level, along with political negotiations with employers and trade unions.

<sup>42</sup> While minimum wages are set at provincial level every year, regencies are allowed to have different rate of minimum wage, as long as it is above the provincial minimum wage. The regency-wage data was kindly shared by Chris Manning and Nurina Merdikawati, who collected manually the data from individual Governors' decrees. The data is only available for Java and Bali, which however comprise the bulk of our sample.

<sup>43</sup> The inclusion of province/district-year effects purges the nominal values of the minimum wages from the impact of price growth.

## 7 Conclusions

This paper sheds light on the determinants of labor market power by examining how changes in product market regulation affect wage markdowns in a large sample of Indonesian manufacturing plants.

The empirical analysis leverages a new dataset on investment restrictions to quantify product market regulation, as well as highly granular information on values and quantities of products and inputs used by plants to estimate the markdown.

In line with the predictions of a simple oligopsony model, we document empirically a novel externality of product market regulation in the labor market. Specifically, we find that imposing investment restrictions in a product market through the Negative Investment List, increases the markdown of plants operating in that market by 25%. The evidence suggests that the effect is driven by the negative impact of product market regulation on firm entry—particularly foreign.

These findings can have important policy implications. By creating a wedge between the marginal product of labor and the wage, product market regulation increases employers' labor market power and distorts factor allocation. This can result in lower aggregate output and employment, but also higher inequality through a negative impact on the labor share.

Our analysis suggests that pro-competitive product market policies can mitigate labor market distortions. This provides evidence-based support for the policies advocated in [Naidu et al. \(2018\)](#) and [Marinescu et al. \(2021\)](#), which propose to extend antitrust measures from product to labor markets. Any change in industry concentration, due for instance to mergers and acquisitions, should also be evaluated in terms of its impact on concentration in the labor market.

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# Online Appendix

## (not for publication)

### A Figures and Tables Appendix

Table A1: Summary statistics.

	(1)	(2)	(3)	(4)	(5)
	N	mean	sd	min	max
Plant-level log-wage markdown	43,127	2.320	1.579	-1.748	6.631
Product market-level (5-dig) NIL	45,193	0.141	0.348	0	1
Product market-level (4-dig) NIL	45,193	0.121	0.287	0	1
Commuting zone-level (regency) NIL	45,193	0.138	0.172	0	1
Number of entrants	45,193	4.726	6.846	0	36
Number of foreign entrants	45,193	0.281	0.728	0	4
Share of blue collar workers (base-year, product market-level)	39,218	0.849	0.0782	0.340	1
Share of State-Owned Enterprises (base-year, product market-level)	39,218	0.0394	0.0814	0	1
Tariff rate (base-year, product market-level)	43,792	11.52	10.58	0	150
Non-tariff measure index (base-year, product market-level)	43,792	0.775	0.418	0	1
Plant-level technological sophistication (base-year, index)	45,193	0.190	0.271	0	1
Log-real output per capita (base-year, regency)	45,193	3.003	0.661	1.918	5.518
Log-population (base-year, regency)	45,193	14.03	0.726	10.30	15.57
Share of population with secondary education (base-year, regency)	45,193	0.173	0.0758	0.0391	0.375
Share of population with primary education (base-year, regency)	45,193	0.392	0.0687	0.216	0.546
Share of population with tertiary education (base-year, regency)	45,193	0.0527	0.0320	0.00898	0.206
Log-real public expenditure on infrastructures (base-year, regency)	43,220	21.13	0.766	17.37	23.91
Log-real manufacturing output per worker (base-year, regency)	45,193	4.194	1.052	0.148	7.888
Share of manufacturing output (base-year, regency)	45,193	0.279	0.176	0.00337	0.854
Share of services (base-year, regency)	45,193	39.29	25.37	-182.1	88.58

*Notes:* this table presents summary statistics of all variables used in the empirical analysis.

Table A2: Pre-trends: regression of 2008-2010 growth rates on product market regulation in 2011.

	(1)	(2)	(3)	(4)	(5)	(6)
	Output growth 2008-2010	Capital growth 2008-2010	Employment growth 2008-2010	Foreign capital share growth 2008-2010	Wage growth 2008-2010	Markdown growth 2008-2010
Dummy nil=1	-0.0218 (0.1016)	0.0899 (0.1003)	-0.0279 (0.0446)	0.0276 (0.8941)	-0.0315 (0.0734)	17.5371 (19.8936)
Observations	340	340	340	340	340	340
R-squared	0.0466	0.0818	0.0779	0.0536	0.0944	0.0956
2dig-industry FE	yes	yes	yes	yes	yes	yes

The table shows the results of regressing average growth rates of key variables in the pre-reforming period 2008-2010 on a dummy taking value 1 if a product market is reformed in 2011 using a linear model. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

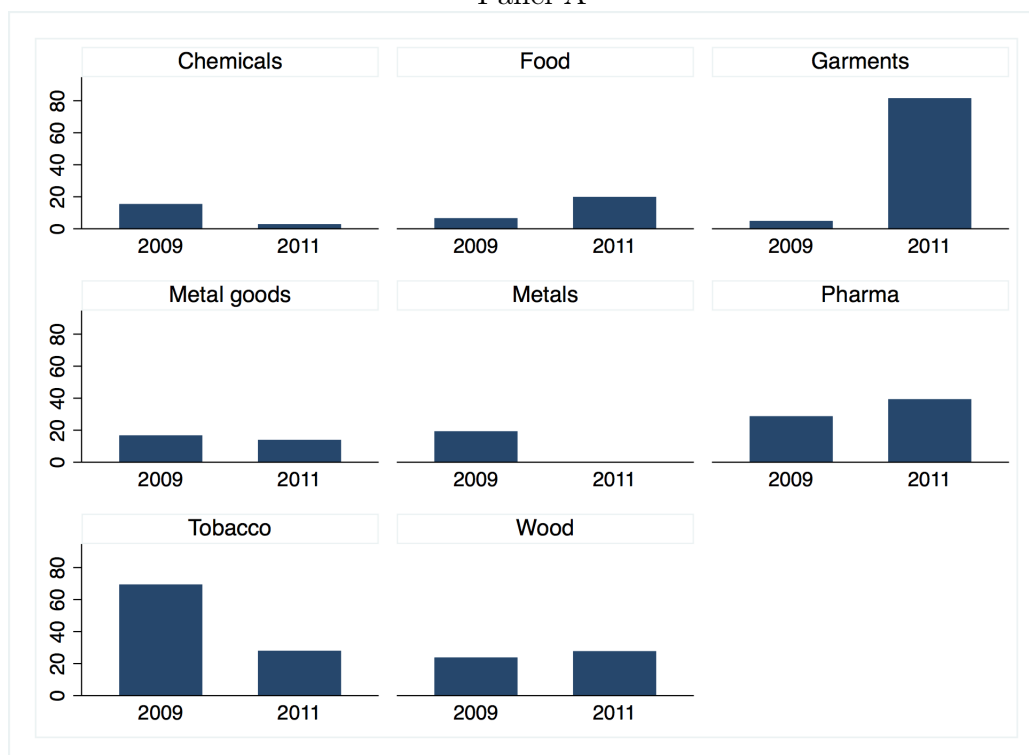
Table A3: Robustness checks: regency-level Bartik instrument.

	(1)	(2)
	Markdown	Markdown
Bartik NIL (regency, excluding crude palm oil)	0.160* (0.096)	
Bartik NIL (regency, excluding product markets with employment shares > 0.5)		0.181* (0.093)
Bartik NIL (regency)		
Observations	36,431	33,431
R-squared	0.807	0.801
Base-year covariates-year FE	yes	yes
Plant FE	yes	yes
Industry-year FE	5-dig	5-dig g
Commuting zone-year FE	province	province
SE clustering	regency	regency

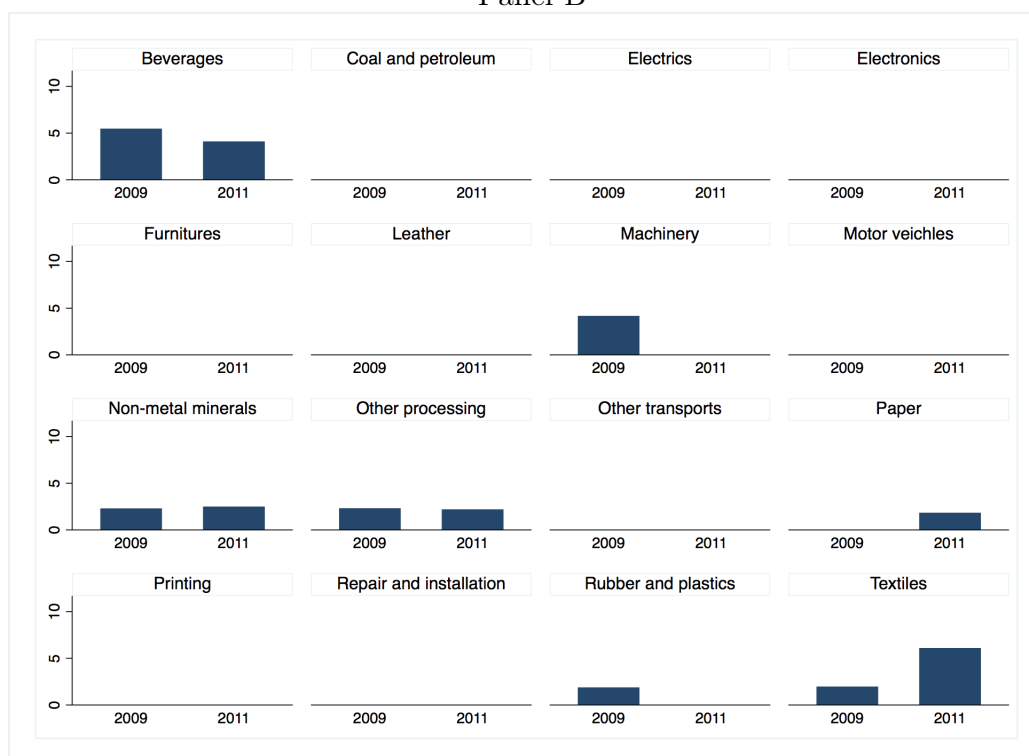
*Notes:* this table presents OLS estimates of the impact of investment restrictions on plant-level log-wage markdown. Base-year covariates are: i) log real output per capita; ii) log population; iii) share of population with tertiary education; iv) log real government expenditure on infrastructure; v) log real manufacturing output per manufacturing worker, and vi) the share of manufacturing output. Errors are clustered at the commuting zone-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

Figure A1: Share of regulated product markets at each revision of the NIL.

Panel A



Panel B



Notes: this figure shows the share of regulated 5-digit industries ( $nil = 1$ ) within 2-digit industries for each reforming year. Panel A includes 2-digit industries with at least ten percent of regulated product markets. Panel B includes industries with less than ten percent of regulated product markets. Source: authors' calculations based on SI.

Table A4: Robustness checks: alternative definition of labor market, technological controls, gross output production function.

	(1) Markdown	(2) Markdown	(3) Markdown (GO)
Bartik NIL (4-dig product market)	0.303** (0.132)		
NIL = 1 (5-dig product market)		0.194* (0.103)	0.252** (0.115)
Observations	34,269	32,355	25,837
R-squared	0.815	0.819	0.801
Base-year technology-year FE	no	yes	no
Base-year covariates-year FE	yes	yes	yes
Plant FE	yes	yes	yes
Industry-year FE	2-dig	2-dig	2-dig
Commuting zone-year FE	regency	regency	regency
SE clustering	5-dig	5-dig	5-dig

*Notes:* this table presents OLS estimates of the impact of investment restrictions on plant-level log-wage markdown. The index of technological sophistication is computed by taking the average of five dummies, each equal to 1 if a plant had any R&D unit, performed product innovation, process innovation, used computers, or the Internet in the 2006 census year. Controls include the base-year shares of mid-skill and high-skilled employment in each plant with year fixed effects to account for automation and skill-biased technical change. Base-year covariates are: i) a Herfindahl–Hirschman index based on plants’ sales to account for product market concentration; ii) the share of blue collar workers to control for government’s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Errors are clustered at the product market-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

Table A5: Reduced form estimates of the impact of product market regulation by type on plant-level wage markdowns.

	(1) Markdown
Special license (5-dig product market)	1.047*** (0.267)
Foreign equity limits (dummy, 5-dig product market)	1.050*** (0.356)
Restricted to SMEs (5-dig product market)	0.014 (0.088)
Closed to investment (5-dig product market)	0.993*** (0.371)
Observations	34,269
R-squared	0.816
Other restrictions	yes
Base-year covariates-year FE	yes
Plant FE	yes
Industry-year FE	2-dig
Commuting zone-year FE	regency
SE clustering	5-dig

*Notes:* this table presents OLS estimates of the impact of investment restrictions by type on plant-level log-wage markdown. Base-year covariates are: i) a Herfindahl–Hirschman index based on plants’ sales to account for product market concentration; ii) the share of blue collar workers to control for government’s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Errors are clustered at the product market-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.

Table A6: Product market regulation and minimum wages.

	(1)	(2)
	Markdown	Markdown
NIL = 1 (5-dig product market)	2.819*** (0.986)	2.721*** (0.945)
Regency minimum wage (log)	-0.214 (0.170)	
NIL $\times$ regency min wage	-0.355*** (0.133)	-0.356*** (0.128)
Observations	26,427	26,420
R-squared	0.794	0.810
Base-year covariates-year FE	yes	yes
Plant FE	yes	yes
Industry-year FE	2-dig	2-dig
Commuting zone-year FE	province	regency
SE clustering	5-dig and regency	5-dig and regency

*Notes:* this table presents OLS estimates of the impact of investment restrictions on plant-level log-wage markdown in regencies enforcing minimum wages. Column 1 includes the main effect of log-minimum wage and its interaction with NIL. Column 2 includes the interaction only because it includes regency-year effects. Product market base-year covariates are: i) a Herfindahl–Hirschman index based on plants' sales to account for product market concentration; ii) the share of blue collar workers to control for government' s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Commuting zone base-year covariates are: i) log real output per capita; ii) log population; iii) share of population with tertiary education; iv) log real government expenditure on infrastructure; v) log real manufacturing output per manufacturing worker, and vi) the share of manufacturing output. Errors are clustered at the product market and commuting zone-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.



## B Model Appendix

### B.1 Existence and Uniqueness

We now apply the well-known existence and uniqueness result developed by [Amir and Lambson \(2000\)](#) for symmetric Cournot oligopoly equilibria to our oligopsony framework.<sup>44</sup>

First, we write the profit function as

$$\Pi(\mathbf{L}_{mi}, \mathbf{L}_{-mi}) \equiv F(\mathbf{L}_{mi} - \mathbf{L}_{-mi}) - W_m(\mathbf{L}_{mi})(\mathbf{L}_{mi} - \mathbf{L}_{-mi}) \quad (\text{B1})$$

where  $\mathbf{L}_{-mi} \equiv \mathbf{L}_{mi} - L$ . Using Theorem 2.1. in [Amir and Lambson \(2000\)](#), a sufficient condition for a symmetric oligopsony equilibrium in which firms compete in employment quantities to exist and being unique, is

$$\frac{\partial^2 \Pi(\mathbf{L}_{mi}, \mathbf{L}_{-mi})}{\partial \mathbf{L}_{mi} \partial \mathbf{L}_{-mi}} = -F''(\mathbf{L}_{mi} - \mathbf{L}_{-mi}) + W'_m(\mathbf{L}_{mi}) > 0 \quad (\text{B2})$$

In our framework, condition (B2) is always satisfied because of the assumptions on  $F$  and  $W_m$ .

### B.2 Monotonicity of The Profit Function

Theorem 2.3. in [Amir and Lambson \(2000\)](#) implies that in a symmetric Cournot equilibrium, the the individual best response functions  $L(N_{mi})$  and the profit function  $\Pi(N_{mi})$  are non-increasing in  $N_{mi}$ .

A necessary and sufficient condition to obtain a monotonically decreasing mapping between product market regulation and firm entry is

$$\frac{\partial \Pi(N_{mi})}{\partial N_{mi}} = -L \cdot \left[ 2(F'(L) - W_m(\mathbf{L}_{mi})) + \frac{W''_m(\mathbf{L}_{mi})}{W'_m(\mathbf{L}_{mi})^2} (F'(L) - W_m(\mathbf{L}_{mi}))^2 \right] < 0 \quad (\text{B3})$$

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<sup>44</sup> While the symmetric case provides a simple way to analyse long-run equilibria, our key results can be extended to the non-symmetric case ([Polo, 2018](#); [Anderson et al., 2020](#)). We leave such task for future research.

All quantities in the squared brackets are unambiguously non-negative, except  $W_m''(\mathbf{L}_{mi})$ . Thus, profits are decreasing in the number of firms as long as  $W_m''(\mathbf{L}_{mi}) \geq 0$  i.e. the inverse labor supply function is not strictly concave.

## C Details of the wage markdown estimation

We assume that in each year  $t$ , producer  $f$  produces gross output  $Q_{ft}$  with the following production function:

$$Q_{ft} = \min \left\{ \gamma_e E_{ft}, F(K_{ft}, L_{ft}, M_{ft}) \cdot \Omega_{jt} \right\} \quad (\text{C1})$$

where  $M_{ft}$  are intermediate inputs,  $K_{ft}$  the capital stock and  $L_{ft}$  labor,  $\gamma_e > 0$  and  $E_{ft}$  is energy consumption. The term  $\Omega_{jt}$  represents Hicks-neutral productivity.

In the production function (C1), we assume capital, labor and materials have some degree of substitution, while energy consumption is a perfect complement to the combination of the other inputs.

Given Equation (C1), a profit maximising producer sets

$$Q_{ft} = \gamma_e E_{ft} = F(K_{ft}, L_{ft}, M_{ft}) \cdot \Omega_{jt} \quad (\text{C2})$$

Our objective is estimating plants' production function parameters based on the production function (C2).

We begin by taking logs:

$$q_{ft} = f(k_{ft}, l_{ft}, m_{ft}; \beta) + \omega_{ft} + \epsilon_{ft} \quad (\text{C3})$$

The term  $\omega_{ft}$  represents the log of Hicks-neutral productivity, which is known by plants' managers but not by us. The variable  $\epsilon_{ft}$  is an i.i.d. error term that captures factors such as measurement errors.

We are interested in estimating the vector of the production function parameters  $\beta$ . To recover unbiased and consistent estimates of firms' production function (C3), we need to address the well-known simultaneity problem deriving from the fact that  $\omega_{ft}$  is correlated to labor and materials but not to capital, which is chosen one period ahead. We build on the methodology of [Akerberg et al. \(2015\)](#). In particular, we make the following timing assumptions concerning inputs' decisions: i) capital  $k_{ft}$  is chosen at  $t - 1$ ; ii)  $l_{ft}$

and  $m_{ft}$  are chosen at  $t - b$  after observing  $\omega_{ft}$ , and iii) energy  $e_{ft}$  is chosen at  $t - a$ , with  $1 < b < a$ .

We then specify the energy demand function,  $e_{ft} = \tilde{h}(\omega_{ft}, k_{ft}, l_{ft}, m_{ft})$ .

Assuming that the materials' demand function of the plant,  $\tilde{h}$  is monotonically increasing and invertible in  $\omega$ , we obtain a control function that proxies for unobserved productivity,

$$\omega_{ft} = h(e_{ft}, k_{ft}, l_{ft}, m_{ft}) \quad (\text{C4})$$

where  $h \equiv \tilde{h}^{-1}$ . Adding  $h(\cdot)$  to Equation (C3), we get

$$q_{ft} = f(k_{ft}, l_{ft}, m_{ft}; \beta) + h(e_{ft}, k_{ft}, l_{ft}, m_{ft}) + \epsilon_{ft} \quad (\text{C5})$$

We follow [Akerberg et al. \(2015\)](#) by approximating the right-hand-side of (C5) with a third-order polynomial in all its elements.

From the first stage, we obtain expected output  $\hat{q}_{ft}$  and the residuals  $\hat{\epsilon}_{ft}$ .<sup>45</sup> The next step is specifying a law of motion for productivity  $\omega_{ft}$ . We assume that  $\omega_{ft}$  follows a Markov process that can be shifted by plant managers' action:

$$\omega_{ft} = g(\omega_{f,t-1}, nil_{i,t-1}) + \xi_{ft} \quad (\text{C6})$$

In Equation (C6), we allow future productivity to be affected by current product market regulation. In a similar way, [De Loecker and Warzynski \(2012\)](#) include export status, [De Loecker et al. \(2016\)](#) include export dummies and import tariffs, [De Loecker \(2007\)](#) include export quotas, [Doraszelski and Jaumandreu \(2013\)](#) include R&D expenditure, and [Konings and Vanormelingen \(2015\)](#) include measures of workforce training. The variable  $\xi_{ft}$  denotes the innovation to productivity.<sup>46</sup> Current expected productivity is then expressed as a function of the data and parameters:

---

<sup>45</sup>It should be noticed that in the first stage, none of the production function parameters are identified, because they enter both  $f(\cdot)$  and  $h(\cdot)$ .

<sup>46</sup>In our application we impose a simple AR(1) form for (C6).

$$\omega(\boldsymbol{\beta})_{ft} = \hat{q}_{ft} - f(k_{ft}, l_{ft}; \boldsymbol{\beta}) \quad (\text{C7})$$

To estimate  $\boldsymbol{\beta}$ , we form moments based on the innovation  $\xi_{ft}$  in the law of motion (C6),

$$\xi(\boldsymbol{\beta})_{ft} = \omega(\boldsymbol{\beta})_{ft} - E[\omega(\boldsymbol{\beta})_{ft} | \omega(\boldsymbol{\beta})_{f,t-1}] \quad (\text{C8})$$

The moments that identify the parameters are:

$$E[\xi(\boldsymbol{\beta})_{ft} \mathbf{M}_{ft}] = 0 \quad (\text{C9})$$

where the vector  $\mathbf{M}_{ft}$  includes current capital, and lagged labor, materials and energy consumption.

In our empirical application, we use a flexible Translog specification to approximate  $f(\cdot)$ .

The parameters of the production functions  $\hat{\boldsymbol{\beta}}$  are estimated separately for 24 2-digit industries with the Generalised Method of Moments (GMM), which we implement bootstrapping errors over hundred repetitions.<sup>47</sup>

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<sup>47</sup> We experimented with different bootstrapping repetitions. The parameter values tend to be very similar.

## D Correcting for Product Market Power

The simple model of Section 2 abstracts from the impact of product market reforms on product market power. However a recent literature emphasises that the wedge between the marginal product of labor and the wage paid to workers reflects both labor and product market power (e.g. [Yeh et al., 2022](#); [Mertens, 2022](#); [Dube et al., 2020](#)).

This can be seen by extending the model of Section 2 to include the inverse product demand function  $P(Q)$ , which we assume to be differentiable and decreasing in plants' supply  $Q$ . Straightforward differentiation of the profit function  $\Pi \equiv P(F(L))F(L) - W_m(\mathbf{L}_{mi})L$  with respect to labor, delivers the following expression for the wage markdown:

$$\nu \equiv \frac{F'(L)}{W_m(\mathbf{L}_{mi})} = \frac{1}{\mu} \left( 1 + \varepsilon_m \cdot \frac{L}{\mathbf{L}_{mi}} \right) \quad (\text{D1})$$

where  $\mu \equiv 1 + \frac{P'(Q)}{P(Q)}Q$  is the plant's markup, a widely used measure of product market power that depends on the elasticity of the inverse product demand function.

To the extent that product market reforms affect markup  $\mu$ , this would have an impact on the wage markdown as well. Specifically, we would expect that product market liberalisation would reduce  $\mu$ . Given the inverse relation between  $\nu$  and  $\mu$ , not purging  $\nu$  of the product market power component  $\mu$  would generate a positive bias in the estimated impact of liberalisation on  $\nu$ . To purge this component we estimate  $\mu$  using the production approach of [De Loecker and Warzynski \(2012\)](#), and then obtain a "corrected" measure of markdown.

Empirically, we proceed by first rewriting the production function of Section 2 as

$$Q = F(K, L, M)$$

where  $M$  denotes the demand for materials, which we assume to be a static input. Consider the Lagrangian function associated to the dual problem of cost minimization:

$$\mathcal{L} \equiv W_m(\mathbf{L}_{mi})L + P^M M + P^K K - \lambda(F(K, L, M) - Q)$$

where  $P^M$  the price of materials,  $P^K$  the price of capital and  $\lambda$  the Lagrangian multiplier.

Cost minimisation with respect to labor and materials implies the following first order conditions:

$$\nu = \frac{\lambda}{W_m(\mathbf{L}_{mi})} F'_L(K, L, M) \quad (\text{D2})$$

where as before,  $\nu$  is defined as the wedge between the marginal product of labor and the wage paid to workers.

Moreover, we have:

$$P^M = \lambda F'_M(K, L, M) \quad (\text{D3})$$

Defining the output elasticity of labor  $\theta^L \equiv \frac{F'_L(K,L,M)}{F(K,L,M)} L$ , we can write Equation (D2) as

$$\nu = \lambda \cdot \theta^L \frac{F(K, L, M)}{W_m(\mathbf{L}_{mi})} \quad (\text{D4})$$

Substituting Equation (D3) into Equation (D4), we obtain

$$\nu = \frac{1}{\mu} \cdot \tilde{\nu} \quad (\text{D5})$$

where  $\mu \equiv \theta^M \frac{F(K,L,M)}{P^M M}$  represents the product markup, which summarises plants' product market power. Finally,  $\tilde{\nu}$  is the “naive” markdown which does not disentangle product and labor market power, as expressed in Equation (3).

## E Model extension: Homogeneous Labor Within A Commuting Zone

Consider the wage markdown equation for a general firm if we assume that labor supply is homogeneous across products within a commuting zone:

$$\nu = 1 + \varepsilon_m \cdot \frac{L}{\mathbf{L}_m} \quad (\text{E1})$$

Crucially, the last term in Equation (E1) is now the firm employment share relative to the whole labor market, rather than its own product market.

A liberalisation of product market  $i$  lowers the entry cost to some level  $\chi'_i < \chi_i$ . As a result, new firms enter market  $i$ , which in turn affect firm  $f$ 's employment share and so its wage markdown. However, through the impact on total labor demand, a reform in  $i$  will also affect the markdown of firms in other product markets  $j \neq i$ , both due to the change in employment share, as well as to changes in the inverse labor supply elasticity.

To illustrate the argument with a simple example, suppose that  $F(\cdot)$  is linear. Combining equations (2) and (4) under the assumption of homogeneous labor yields:

$$\nu = 1 + \phi_m \sqrt{\chi_i}$$

where  $\phi_m \equiv \frac{\sqrt{W'_m(\mathbf{L}_m)}}{W_m(\mathbf{L}_m)}$ . If the term  $\phi_m \sqrt{\chi_i}$  is sufficiently small, the derivative of log-markdown to changes in regulation can be approximated by

$$\frac{\partial \ln \nu_{im}}{\partial \chi_i} \approx \frac{\partial \phi_m}{\partial \chi_i} + \frac{1}{2\sqrt{\chi_i}} \quad (\text{E2})$$

The last term in Equation (E2) represents the direct impact of product market regulation on wage markdowns. Since  $\frac{1}{2\sqrt{\chi_i}} > 0$ , as in the benchmark case a product market liberalisation would tend to reduce firms' labor market power. However, changes in regulation in  $i$  would also have an impact on firms in other product markets, which is captured by the term  $\frac{\partial \phi_m}{\partial \chi_i}$ . Notice that if labor supply is product-specific, then  $W'_m(\mathbf{L}_{mi}) = 0$  for  $j \neq i$  and so only firms producing  $i$  would be affected by regulation.



## F Data Appendix

### F.1 Construction of the Capital Series

Earlier studies tried to re-construct capital stock series applying the perpetual inventory method (PIM) to the first year of capital stock data reported by the plant (Amiti and Konings (2007); Javorcik and Poelhekke (2017)). However this imputation method crucially relies on the capital value self-reported by the plant the first year this data is available, which is not necessarily accurate.<sup>48</sup> One potential advantage of using PIM is that purchase and sales data might be more accurate relative to self-reported value of the stock, requiring an appropriate calculation of market values and depreciations. However, PIM needs to rely on measures of capital depreciation, which are difficult to accurately estimate. To mitigate such tradeoff, we have adopted a hybrid strategy. We first clean the self-reported adopting an algorithm which keeps only observations that fulfil a battery of tests, which are described below. Then, we apply the PIM only to fill the gaps between the missing observations and reapply the same battery of tests to ensure consistency of the series.

In order to avoid relying on depreciation rates, we tried to preserve the self-reported original values by the plant as much as possible and applied the PIM only to fill gaps. In this paper self-reported capital series were object of an extensive cleaning algorithm aimed at mitigating measurement errors. One problem with the reported series is that in some years, there are plants were characterised by implausible large values of capital. Studying the behaviour of the stock within plants reveals that in some circumstances plants reported values in different units. The phenomenon is somewhat more frequent in 1996 and 2006, when the BPS conducted a wider economic census that collected information in units rather than in thousand Rupiah. For instance, in 2006 the number of surveyed firms increased by 40%. The increase in coverage required hiring inexperienced enumerators that were more likely to make mistakes, which contributed to increase measurement errors.

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<sup>48</sup>In particular, there is no a priori reason to believe that the quality of the self-reported capital stock the first year is necessarily better than the value in other years.

Our algorithm consists first in replacing zero or negative values as missing observations and then applying a two-steps procedure based on capital-labor ratios (KL). For each year, we compute the average KL in each 4-digit KBLI sector over the whole sample, but excluding the years in which the average and total values of the capital stock exhibited suspicious jumps, i.e. 1996, 2000, 2003, 2006, 2009 and 2014. An observation is dropped if the ratio of plant-KL to the sector average KL is below 0.02 or larger than 50. We experiment with stricter thresholds which result in too many observations dropped. Then, in a second step we compare a plant KL in a given year with the average value of the KL within the same plant but in the other years of observation. An observation is dropped if the ratio of plant-year-KL to the plant average KL is below 0.2 or larger than 5. Plants are dropped from the sample in case the cleaning procedure results in all missing values of self-reported capital. When a plant has some but not all valid observations for self-reported capital stock, then missing values are replaced by applying a forward/backward perpetual inventory method (PIM). Being only a fraction of the total observations, we rely less on estimates of depreciation rates. We follow Arnold and Javorcik (2009) and assume that the annual depreciation rate for buildings is 3.3 percent, for machinery 10 percent, and for vehicles and other fixed assets 20 percent. For land, we assumed no depreciation.

Previous studies focus on the first year of observation of a plant, without assessing the plausibility of the data point. Since PIM series are very sensitive to the choice of the initial observation, especially with relatively short time series, the resulting capital stock could be severely mis-measured. Moreover, information on purchases and sales of capital equipment, which is subject to the same measurement errors of the reported capital. For such a reason, after filling missing values with the PIM we re-apply the two stages check described above in order to minimise the possibility of mis-measurement. As a final test, we compute plant level growth rates of KL and we check that it is reasonably distributed (Figure F1). Figure F2 compares original and clean capital stock series.

Figure F1: Plants' growth rate distribution of capital-labor ratio.

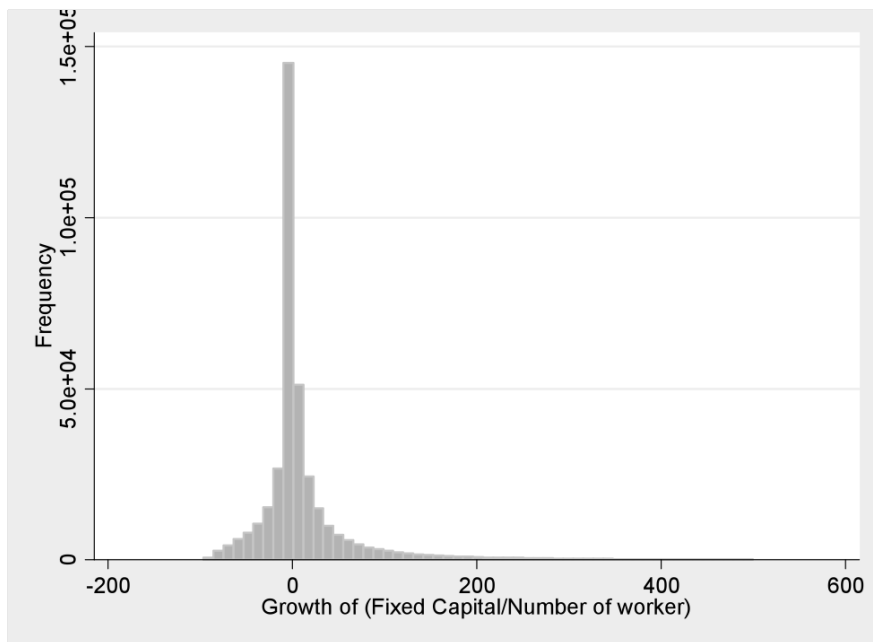
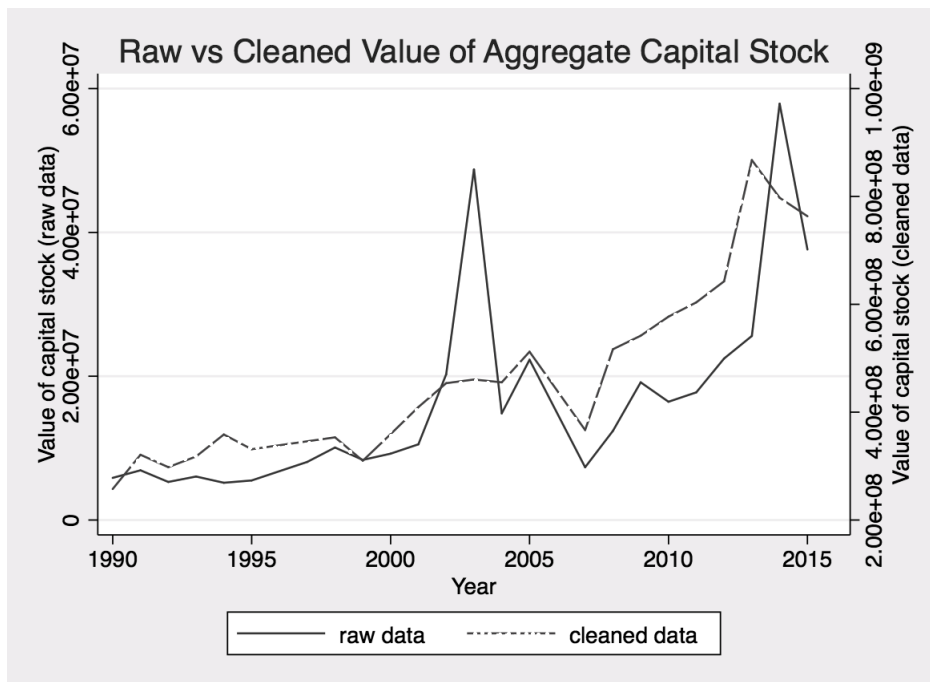


Figure F2: Comparison of Aggregate Nominal Capital Stock Series.



## F.2 Products and Inputs Data Cleaning Algorithm

This section describes the algorithm we use to clean the raw product file.

- We first go through the KKI publications and list coding and descriptions of each products and inputs in all years of the sample. If we find discrepancies for some year, we use coding and description that appears more frequently.
- We correct manually spelling mistakes in the descriptions and coding when there was an obvious coding mistake (e.g. we replace code 0123456789 with 123456789. This resulted in the adjustment of more than 300 entries.
- When products or inputs are expressed in different units across plants or in different years within a plant, we converted units and the corresponding corresponding values using the relevant conversion tables.
- After computing unit prices by dividing value with quantities, we compute yearly price growth. If the price grow by more than a factor of 10 or decreases more than by a factor of 1/10, we drop the observation.

## F.3 Construction of plant level Price indices

The derivation of plant-specific price indices from product-level price data closely follows [Eslava et al. \(2004\)](#) and [Mertens \(2022\)](#).

These are plant level Tornqvist indices exploiting information on 9-digit products produced and inputs used by each plant.

$$\pi_{jt} = \prod_{p=1}^n \left( \frac{P_{pjt}}{P_{pj,t-1}} \right)^{.5(s_{pjt} + s_{pj,t-1})} \pi_{j,t-1}$$

where  $P_{pjt}$  is the price of good  $p$  and  $s_{pjt}$  is the share of this good in total product market sales of plant  $j$  in period  $t$ . Therefore, the growth of  $\pi_{jt}$  is the product of each plant's price growth, each weighted with the average share of sales in  $t$  and  $t - 1$ . We set  $\pi_{jt} = 100$  in 2006. For plants entering after 2006, we follow [Eslava et al. \(2004\)](#) and [Mertens \(2022\)](#) and use the 5-digit industry average of the plant price indices as a starting value. When

price growth data are missing, we replace it with an average of product or inputs price changes within the same 5-digit industry.

## G The impact of NIL on firm entry

We estimate the following specification on data aggregated at the product market-level:

$$E_{i,t} = \exp \left( b_0 + b_1 nil_{i,t} + u_i + u_{j,t} + \eta_{i,j,t} \right) \quad (\text{G1})$$

where  $E_{i,t}$  is the number of entrants in product market  $i$  in year  $t$ . We define a plant as an entrant if it has age equal to zero.<sup>49</sup>

In Equation (G1), we include product market fixed effect,  $u_i$ , as well as 2-digit industry-year fixed effects,  $u_{jt}$ , capturing industry shocks and potential changes in lobbying activity over time.

Roughly seventy percent of product markets have zero entrants in some year. Hence we estimate Equation (G1) with a Poisson pseudo-likelihood estimator, which is more appropriated than a linear model in dealing with a count, zero-inflated dependent variable (Silva and Tenreyro, 2006). We weigh observations by the base-year output share in each product market, which ensures that the results are not driven by small sectors with a marginal impact on labor markets.<sup>50</sup>

Table G1 presents the results of estimating Equation (G1), which confirms the significant impact of the NIL on firm entry in Indonesian manufacturing. The coefficient in column 1, significant at the 95 percent confidence level, implies that the number of entrant plants in regulated product markets is only forty percent the number of entrants in unregulated ones.<sup>51</sup>

The results of estimating Equation (G1) including the control variables described in Section 5.1 are presented in column 2. Adding the full set of these product-market characteristics interacted with year effects makes the coefficient smaller in absolute value, but very precisely estimated (coeff. = -0.495, s.e. = 0.166). The coefficient implies that the average number of entrants in regulated markets is only sixty percent the number in

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<sup>49</sup> In order to accommodate cases of zero entrants, we use an inverse hyperbolic sine transformation.

<sup>50</sup> Results, available from the authors, are similar if we use an OLS estimator and we do not weight the estimates.

<sup>51</sup> The coefficient is equal to -0.855. Therefore, the expected ratio of entrants in regulated and unregulated markets is equal to  $\exp(-0.855) = 0.42$ .

unregulated markets.

Table G1: The impact of product market regulation on firms' entry at the product market-level.

	(1)	(2)
	Entry	Entry
NIL = 0 (5-dig industry)	-0.855** (0.366)	-0.495*** (0.166)
Observations	1,102	965
Base-year covariates-year FE	no	yes
Product market FE	yes	yes
Industry-year FE	yes	yes

*Notes:* this table presents Poisson pseudo-likelihood estimates of the impact of investment restrictions on the number of entrants in a product market, defined as a 5-digit industry. Base-year covariates are: i) a Herfindahl–Hirschman index based on plants' sales to account for product market concentration; ii) the share of blue collar workers to control for government' s reluctance in reforming an industry; iii) the share of industry output produced by state-owned enterprises (SOE); iv) the product average import tariff rate; v) a dummy equal to one if a product market is subject to at least one non-tariff measure, and vi) the market share of politically connected plants. Errors are clustered at the product market-level. The coefficients with \*\*\* are significant at the 1% level, with \*\* are significant at the 5% level, and with \* are significant at the 10% level.