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Firm dynamics and regional development in Indonesia

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Acknowledgments

I would like to prepend some personal words to the main body of this thesis. Given that English is not my mother tongue, I will switch to German just for this one page. I think that this makes it easier to say exactly what I would like to say.

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Contents

List of figures VIII

List of tables XI

1 Introduction 1

1.1 Motivation . . . 1

1.2 Research agenda . . . 3

2 Heat and firm productivity: Evidence from Indonesia's manufacturing sector 9

2.1 Introduction . . . 10

2.2 Literature . . . 12

2.3 Climate change in Indonesia . . . 14

2.3.1 Measurement of temperature . . . 14

2.3.2 Descriptive trends . . . 15

2.4 Theoretical framework . . . 17

2.5 Data . . . 18

2.5.1 Weather data . . . 18

2.5.2 Firm data . . . 19

2.5.3 Additional data . . . 23

2.6 Empirical strategy . . . 23

2.7 Results . . . 25

2.7.1 Main results . . . 25

2.7.2 Electricity as a channel of adaptation . . . 31

2.7.3 Further heterogeneities . . . 36

2.7.4 Effect stability over time . . . 40

2.8 Conclusion . . . 40

3 What happens to FDI spillovers when input-output tables go granular? 42

3.1 Introduction . . . 43

3.2 Measuring spillovers . . . 44
3.3 Results . . . 46
3.4 Conclusion . . . 48

4 Foreign investment regulation and firm productivity: Granular evidence from Indonesia 50

4.1 Introduction . . . 51
4.2 Regulatory background . . . 55
4.2.1 Foreign investment regulation in Indonesia . . . 55
4.2.2 The political economy of the NIL . . . 57
4.3 Data . . . 60
4.3.1 Firm data . . . 60
4.3.2 Combined dataset and descriptive trends . . . 61
4.4 Estimation strategy . . . 64
4.5 Results . . . 67
4.5.1 Pre-trends and anticipation effects . . . 67
4.5.2 Baseline results . . . 69
4.5.3 Potential reasons for productivity losses . . . 72
4.5.4 Types of regulation and de-regulation . . . 76
4.5.5 Further robustness checks . . . 79
4.6 Conclusion . . . 83

5 Regulating FDI in Indonesia's manufacturing sector: Local labor market responses to a protectionist policy 85

5.1 Introduction . . . 86
5.2 Institutional context . . . 90
5.3 Data . . . 92
5.3.1 Labor market data . . . 92
5.3.2 Measuring local regulatory penetration . . . 94
5.4 Structural change . . . 97
5.4.1 Long-difference strategy . . . 97
5.4.2 Results . . . 98
5.5 Labor market dynamics . . . 103

5.6 Robustness . . . 105
5.6.1 Possible confounders . . . 105
5.6.2 Effect timing, pre-trends and spillovers . . . 107
5.6.3 Validity of the shift-share approach . . . 109
5.7 Further results . . . 110
5.7.1 Possible channels . . . 110
5.7.2 Heterogeneities and alternative outcomes . . . 112
5.8 Conclusion . . . 114

6 Concluding remarks **116**

Bibliography **119**

A Appendix: Heat and firm productivity: Evidence from Indonesia's manufacturing sector **131**
A.1 Sample size reduction . . . 131
A.2 Additional figures . . . 133
A.3 Additional tables . . . 140

B Appendix: Foreign investment regulation and firm productivity: Granular evidence from Indonesia **144**
B.1 Product-level determinants of regulation . . . 144
B.2 Cleaning the firm data . . . 145
B.3 Merging the NIL conditions to product codes . . . 146
B.4 Further variable definitions . . . 150
B.5 Wooldridge approach for productivity estimation . . . 151
B.6 Additional figures . . . 154
B.7 Additional tables . . . 155

C Appendix: Regulating FDI in Indonesia's manufacturing sector: Local labor market responses to a protectionist policy **164**
C.1 Economy-wide regulation . . . 164
C.2 Instrumental variable approach . . . 165
C.3 Robustness: Possible confounders . . . 166

C.3.1 Political economy factors 166
C.3.2 Exposure to global dynamics 168
C.3.3 Agglomeration and further labor market dynamics 169
C.3.4 Migration 171
C.4 Additional figures 172
C.5 Additional tables 176

D Declaration for admission to the doctoral examination **186**

E Author contributions **187**

List of figures

2.1 Average temperature, relative humidity and precipitation over time . . . 15

2.2 Change of average temperature, relative humidity and precipitation between 1993 and 2015 . . . 16

2.3 Number of days in temperature bins in 1993 and 2015 (by population density) . . . 20

2.4 Firm outcomes over time (by population density) . . . 22

2.5 Average and location-specific effects of temperature on output, productivity and input factors . . . 26

2.6 Effect of temperature on electricity use and spending . . . 33

3.1 Spillover effects using different aggregation levels of IO tables . . . 45

4.1 Trends in capital ownership structure over time . . . 58

4.2 FDI and productivity in regulated vs. non-regulated firms . . . 63

4.3 Time profiles of regulation on FDI and TFP . . . 67

5.1 Sectoral composition of local regulatory penetration (LRP) over time . . . 92

5.2 Change in LRP between 2006 and 2010 . . . 96

5.3 Sectoral decomposition of the impact of LRP on the employment rate (by sector) . . . 102

5.4 Impact of LRP (full change) on total employment rate . . . 108

A1 Number of firms in each sample year by different samples . . . 132

A2 Number of days in temperature bins over time . . . 133

A3 Average electricity quantity and spending . . . 134

A4 Rural firms: Effect of temperature on output, productivity and input factors by sector . . . 135

A5 Urban firms: Effect of temperature on output, productivity and input factors by sector . . . 136

A6 Robustness check: Pre and post 2006 . . . 137

B1 Trends in FDI shares: continuously regulated vs. newly regulated in 2007 154
B2 Average state-owned and private capital over time 154

C1 Working-age population and sectoral employment rates 172
C2 Active population and sectoral employment rates (*Sakernas*) 173
C3 LRP levels in 2006, 2007 and 2010 174
C4 Density distribution of local regulatory penetration (LRP) 175

List of tables

2.1 Effect of temperature on output, productivity and input factors 30

2.2 Electricity as additional control and outcome 32

2.3 Effect of temperature on output, productivity and input factors by electricity intensity 35

2.4 Effect of temperature on output, productivity and input factors by labor intensity 38

2.5 Effect of temperature on output, productivity and input factors by technology intensity 39

3.1 FDI spillover effects on total factor productivity 47

4.1 Predictors of product-level regulatory penetration 59

4.2 Summary statistics in 2001, 2007 and 2015 62

4.3 Regulatory effects on FDI and productivity 70

4.4 Transmission channels 72

4.5 Robustness: Pseudo-Poisson Maximum Likelihood estimations 73

4.6 Heterogeneity by external financial dependence and technology (binding regulation) 75

4.7 Distinguishing between types of regulation 77

4.8 Transmission channels of de-regulation 78

4.9 Robustness: Exit and entry of firms 80

4.10 Robustness: Product switching behavior 82

5.1 Impact of regulatory tightening between 2006 and 2010 on the change in employment rates (Economic Census) 99

5.2 Impact of regulatory tightening between 2006 and 2010 on firm size and number of firms (Economic Census) 100

5.3 Impact of local regulatory penetration on employment rates (*Susenas*) . . 104

5.4 Impact of LRP on private and public expenditure as well as wage premia 111

5.5 Impact of local regulatory penetration on employment by worker and job characteristics . . . 113

A1 Summary statistics of main input and output variables in 1993, 2005 and 2015 . . . 140
A2 Alternative measures of urbanization . . . 141
A3 Effect of temperature on output, productivity and input factors by generator use . . . 142
A4 Effect of temperature on output, productivity and input factors by major islands . . . 143

B1 Conditions of the negative investment list in 2000, 2007, 2010 and 2014 . 149
B2 Production function coefficients by two-digit sector . . . 153
B3 Summary statistics of the main variables . . . 155
B4 Summary statistics by sectors in 2001, 2007 and 2015 . . . 156
B5 Baseline results including control coefficients: Regulation, FDI and productivity . . . 157
B6 Impact on productivity while controlling for FDI . . . 158
B7 Robustness: More restrictive fixed effects and error clustering . . . 159
B8 Heterogeneity by external financial dependence and technology (regulated product) . . . 160
B9 Heterogeneity by firm size and trading (binding regulation) . . . 161
B10 Heterogeneity by firm size and trading (regulated product) . . . 162
B11 Robustness: Levels of TFP estimation and sector-specific deflators . . . 163

C1 Predictors of product-level regulatory penetration (Genthner and Kis-Katos 2019) . . . 176
C2 Summary statistics of long-difference sample . . . 176
C3 Summary statistics of district-level panel . . . 177
C4 Robustness: Economy-wide regulatory penetration (Economic Census) . 177
C5 Sectoral composition of LRP in selected years . . . 178
C6 Summary statistics of districts per product and products per district in our samples . . . 178
C7 Alternative specifications: Impact of regulatory tightening between 2006 and 2010 on the change in employment (Economic Census) . . . 179

C8 Robustness checks according to shift-share literature . . . 179
C9 IV results: Impact of local regulatory penetration on FDI stocks and employment (*Susenas*) . . . 180
C10 Robustness: Political economy . . . 181
C11 Robustness: Global financial crisis, trade, automation and high-technology firms . . . 182
C12 Robustness: Agglomeration and labor market reform . . . 183
C13 Impact of local regulatory penetration on migration . . . 184
C14 Robustness: Pre-trends and spillovers . . . 184
C15 Standard errors and rejection rate of the hypothesis $H_0 : \beta = 0$ at 5% significance level (Adão et al. 2019) . . . 185
C16 Impact of local regulatory penetration on other labor outcomes . . . 185

CHAPTER 1

Introduction

1.1 Motivation

Like all economic actors, firms continuously need to adjust to changes of the environment they are operating in. These factors include not only global phenomena like internationalization of trade (Amiti and Konings 2007, Conconi et al. 2016, Bonfiglioli et al. 2019) or increasing foreign direct investment (FDI) (Javorcik and Poelhekke 2017, Lu et al. 2017), growing importance of automation (Acemoglu and Restrepo 2019) and climate change (Zhang et al. 2018, Somanathan et al. 2021), but also more localized factors like changes in national policy (Harrison and Rodríguez-Clare 2010, Martin et al. 2017) or natural disasters (de Mel et al. 2012, Zhou and Botzen 2021). Theoretical models of heterogeneous firms (e.g., Melitz 2003) predict that the least productive firms fail to meet these challenges of adjustment and exit markets due to competitive pressure. Only sufficiently productive companies adapt themselves to new conditions and manage to stay in business.

Firms produce goods or provide services that are consumed within the economy. At the same time, they generate demand for input factors. By that, they create employment that constitutes an essential source of income to the local population, and provide investment opportunities for capital owners. Over the past decades, some corporations have even become global players with branches all over the world. These multinational enterprises (MNEs) use direct investments in foreign countries to open up new markets and save the trade costs of exporting. Oftentimes, MNEs generate revenues that exceed the gross domestic product (GDP) of the countries they are operating in (Zingales 2017). But also small and medium-sized enterprises (SMEs) are highly relevant in national economies, as they still account for every second job in formal labor markets of developing countries on average (Ayyagari et al. 2011). At the same time, small firms also employ a large majority of informal workers in developing countries (McCaig and Pavcnik 2018).

Given the pivotal role and immense importance of firms in the world economy, any policy change or external shock may not only affect firms directly (Amiti and Konings 2007, Hanna 2010, Zhang et al. 2018), but also has an indirect impact on input factor markets (Autor et al. 2013, Gopinath et al. 2017, McCaig and Pavcnik 2018), economic development (Kline and Moretti 2014, OECD 2017) and innovation (Amiti and Khandelwal 2013, Lin et al. 2021) of a country. Therefore, a better understanding of the underlying dynamics of the firms' adjustment to policy reforms or environmental changes is crucial not only for firms and their shareholders, but also for policy makers.

This thesis looks at manufacturing firm dynamics in three particular settings. First, it presents evidence for productivity drops and input factor adjustments due to rising temperatures as a result of climate change. With global warming being among the "most pressing issues of our time" (United Nations 2017), learning about its impact on firms is important to get a more complete picture of its economic consequences. Second, this thesis adds to the understanding of FDI productivity spillovers among manufacturing firms by splitting vertical spillovers across industries depending on their sectoral distance. The results indicate that the size of FDI spillovers hinges on whether foreign and domestic firms are direct competitors, and the ability of local enterprises to absorb new technology from MNEs. Third, this thesis shows that the regulation and restriction of FDI flows directly result in productivity drops among firms through a substitution of more productive foreign capital with inferior domestic capital. Even though firms are able to avoid general capital shortages, the findings highlight the tremendous importance of FDI for the international competitiveness of the local economy. Forth, the effects of the latter FDI regulation are also tested in the context of regional dynamics on local labor markets, finding that a higher regulatory penetration leads to sizable positive employment spillovers in both the manufacturing and the service sector.

All chapters of the thesis are based on empirical analyses using micro data from Indonesia. As Southeast Asia's largest economy and the fourth most populated country in the world, Indonesia offers great opportunities to look into the effects of temperature, foreign investment or FDI regulation on firm dynamics. On the one hand, there is a very diverse industrial landscape that draws on an abundance of both human and natural resources (Blalock and Gertler 2008). On the other hand, data availability is exceptionally good compared to other developing countries. This allows for relatively disaggregated analyses of firm dynamics (cf. Amiti and Konings 2007, Xie 2019) and regional development (cf. Kis-Katos and Sparrow 2015, Cisneros et al. 2021).

The empirical analysis of this thesis relies on three main data sources. First, it uses an annual firm census of medium-sized and large plants operating in the manufacturing sector (*Survei Industri*).[1] The data feature a wide range of firm information

[1] Throughout the thesis, the concept of plant and firm will be used interchangeably, as the data do not allow for identification of multi-plant corporations.

on sales, employment or capital stocks, but also include granular information on its main product and location.[2] The analysis is complemented with yearly labor market and household surveys on individuals (*Susenas* and *Sakernas*). Most importantly, individual-level data allow to consistently retrieve yearly population and employment figures among socio-economic groups on the second level of administrative subdivisions (*kabupaten/kotamadya*, hereafter called districts). The analysis of this thesis also draws from two waves of the national Economic Census (*Sensus Ekonomi*) which is conducted every ten years and consistently reports employment and district information for the universe of all firms in the manufacturing and service sector.

1.2 Research agenda

Chapters 2 to 5 of this thesis are based on four individual empirical studies. Each study's title as well as a complete list of co-authors are provided in footnotes. An overview of each co-author's contribution to the respective study can be found in chapter E.

Heat and firm outcomes

One of the key challenges of the twenty-first century are the consequences of climate change. There is a very rich and rapidly growing body of literature in economics investigating the ramifications of heat on health (e.g., Kjellstrom et al. 2009, Barreca et al. 2016), individual behavior (e.g., Graff Zivin and Neidell 2014), energy demand (e.g. Wolfram et al. 2012, Gertler et al. 2016), agricultural yields (e.g., Schlenker and Roberts 2009, Colmer 2021), and firm outcomes (e.g., Zhang et al. 2018, Somanathan et al. 2021). However, there is little evidence on the effects of heat waves on firm outcomes for Indonesia, with the notable exception of Xie (2019) who looks at firm exit in response to increasing average temperature. A better understanding of the heat effects on Indonesian firms is particularly important because most existing studies focus on countries that expand over several climate zones. Indonesia offers a great opportunity to check whether previous results hold when looking at firms in the tropical climate zone only.

In the second chapter of this thesis, we therefore aim to fill this gap by looking into the effects of rising temperature on firm output, inputs and productivity.[3] Heat may affect firms through various channels. First, high temperatures can result in lower productivity of workers due to a reduced thermal comfort, impaired cognitive function

[2] See Márquez-Ramos (2021) for an overview of the data and related studies.
[3] The study "Heat and firm productivity: Evidence from Indonesia's manufacturing sector" is co-authored by Sebastian Renner and Enrica de Cian.

and fatigue (Graff Zivin and Neidell 2014). Similarly, extreme temperature may also affect the proper functioning of machinery once cooling devices are stretched to their limits (Day et al. 2019). At the same time, firms can offset potential negative productivity effects by accommodating their factor inputs. For instance, a firm can hire more workers to counteract declining labor productivity, or investments can be put into new machinery or air conditioning (Zhang et al. 2018).

Our estimation strategy makes use of a bin regression design (Deschênes and Greenstone 2011), thereby exploiting year-to-year variation in the number of heat days to estimate the impact of high temperature on firm outcomes. As more urbanized regions heat up more rapidly (urban heat islands, Oke 1973), the empirical strategy further allows for differential effects within rural and urban districts. Our results confirm the existence of urban heat islands and show stronger effects for firms in more densely populated districts. Heat predominantly reduces capital productivity, but firms increase both capital stocks and employment to compensate for productivity losses. In consequence, output does not react to rising temperatures. A heterogeneity analysis reveals that especially under-electrified firms suffer from the consequences of global warming. Electricity usage for cooling devices, however, is positively associated with temperature only in rural areas, whereas no clear pattern is found for urban firms.

FDI and productivity spillovers

Chapter 3 looks into the firm-level effects of FDI spillovers.[4] In the developing country context, FDI is considered to be a main factor to realize economic development and to increase productivity through various channels (Newman et al. 2015). First, foreign capital directly alleviates potential financial restrictions in the receiving economy. Second, MNEs are expected to introduce new technologies and non-tangible assets like managerial skills which directly benefit the receiving domestic firms (Aitken and Harrison 1999). Partly foreign-owned enterprises thus are typically more productive and also more capital intensive compared to purely domestic firms (Harrison and Rodríguez-Clare 2010). Third, this new knowledge may also generate productivity spillovers to other firms either horizontally within the same industry or vertically across industries (Javorcik 2004, Fons-Rosen et al. 2017). FDI spillovers may either occur due to technology leakage from MNEs to domestic firms or voluntary sharing of knowledge with domestic suppliers. At the same time, the presence of MNEs may lead to increased competition on the domestic market, thereby forcing less productive firms to either become more productive or exit the market (Blalock and Gertler 2008).

In the chapter, I show that the nature of these productivity spillovers depends on two

[4] The note "What happens to FDI spillovers when input-output tables go granular?" is single-authored and forthcoming in *Economics Bulletin*.

opposing mechanisms related to the sectoral distance between firms. On the one hand, MNEs are more willing to share technology with non-direct competitors in more distant supplying industries which should lead to stronger positive spillovers to firms in different industries and sectors. On the other hand, a successful adoption of new technology is easier for firms that operate in closer industries already using similar production processes (Fons-Rosen et al. 2017). According to the latter mechanism, the positive productivity spillover thus might be declining with industrial distance. By using disaggregated input-output (IO) tables on three-digit industry level to proxy for FDI spillovers, I am able to disentangle these two differential channels, which cannot be otherwise achieved using aggregated IO tables. My findings lean support to both more technology sharing among firms which are not directly competing with one another and lower costs of adaption for firms with closer industrial ties to the MNE. This highlights the importance of granular measurement of industrial linkages, as more aggregated IO tables would have masked this effect heterogeneity.

FDI regulation and firm productivity

National governments often have incentives to protect their economies from both international trade and FDI (Gawande and Krishna 2003). Among the protectionist arguments are the guarding of infant industries or the creation of national champions. Protectionist policies, however, are also often driven by lobbyist motives of industries and other interest groups (cf. Grossman and Helpman 1994). At the same time, protectionism disturbs free trade and capital flows and may result is substantial economic costs as well as welfare reductions, not only for trade partners but also the domestic economy (Feenstra 1992, Kee et al. 2013, Fajgelbaum et al. 2020).

The remaining two chapters of this thesis present evidence from the introduction of an FDI regulating policy in Indonesia. The negative investment list (NIL) was first introduced in 2000 and has been repeatedly revised over the last two decades. It includes FDI inflow restrictions that range from licensing requirements to complete FDI bans, and is issued at the granular five-digit product level.

Chapter 4 examines the response of firms to FDI regulation using the Indonesian manufacturing firm census of medium-sized and large enterprises.[5] There are only a few studies investigating the direct effects of regulation on firm outcomes. These studies show a negative link between aggregate measures of policy restrictiveness and productivity at either sectoral (Bourlès et al. 2013) or firm level (Duggan et al. 2013). Closest related to our study, Eppinger and Ma (2019) find a positive effect of FDI de-regulation

[5] The study “Foreign investment regulation and firm productivity: Granular evidence from Indonesia” is co-authored by Krisztina Kis-Katos and currently in the revision process at the *Journal of Comparative Economics*.

on Chinese firms' productivity. It remains unclear, however, whether the effect of regulatory tightening in Indonesia is symmetric to de-regulation in the Chinese context. Given the evidence of positive productivity spillovers from FDI in chapter 3, however, it intuitively makes sense to expect a negative impact of FDI regulation on firm productivity.

To fill this gap in the literature, our empirical strategy starts with looking into the political economy of the NIL. Anecdotal evidence suggests that the Indonesian government was motivated by national interests to protect industries from international competition and merge-and-acquisition activity. We find that sectoral exposure to the presence of state-owned firms or recent privatization are the strongest predictors of subsequent increases of regulation. In the main analysis, we then control for the identified drivers of regulation to alleviate endogeneity concerns. Additionally, we also include a set of fixed effects and flexibly allow for differential trends with respect to initial product and firm characteristics to come closer to a causal identification of the regulatory impact.

The results document a robust negative effect of FDI regulation on firm productivity. At the same time, foreign capital shares also decline with regulation, but are fully compensated by increases in domestic capital. This suggests either a less efficient allocation or a lower technological content of domestic capital. Meanwhile, we do not find evidence of symmetric effects of regulation and de-regulation since there is no immediate positive productivity response to de-regulation. By that, this study is among the first to exploit fine grained variation in FDI regulation within a developing country. In contrast to previous studies which mainly focused on de-regulation (Eppinger and Ma 2019), we are able to measure direct firm exposure to a tightening of FDI regulation and link it to declines in firm productivity, most likely driven by changing capital composition.

FDI regulation and local labor markets

Despite the strong effect of FDI regulation on the firms' capital structure and productivity, the analysis in chapter 4 does not find a statistically meaningful impact on firm employment and wages per worker. Especially the latter result seems to be at odds with the consistent finding in the literature that MNEs pay higher wages compared to their local competitors (Harrison and Rodríguez-Clare 2010, Amiti and Davis 2012), and shows that FDI regulation does not just symmetrically invert the effect of foreign ownership. However, the NIL may still have an impact on labor market outcomes on a more aggregate level. While the manufacturing firm census only allows to look at employment in medium-sized and large manufacturing enterprises, it is possible that a more protectionist environment generates additional labor demand in smaller, potentially informal, manufacturing enterprises due to reduced foreign competition. At the same time, contrary to MNEs, domestic regulated firms may not be able to provide all required services in-house. This may result in an increased demand for domestic services and potentially creates employment spillovers in other parts of the economy as well.

Chapter 5 therefore exploits employment data from both the Economic Census and household surveys to look into the effect of the NIL on local labor markets.[6] A series of studies has documented overall negative employment and wage effects from import competition (Autor et al. 2013, Dix-Carneiro and Kovak 2017). However, there are not many studies linking FDI presence to labor markets at the region level (e.g., Feenstra and Hanson 1997, Axarloglou and Pournarakis 2007, McLaren and Yoo 2017). These approaches face the challenge that the location decision of foreign investors is endogenous therefore making it hard to establish a causal interpretation. Our paper exploits the reverse angle by relating FDI de-liberalization to local labor markets.

Thereby, a local labor market is defined at the level of Indonesian districts. First, we compute employment rates on district level by aggregating firm-level number of workers from the Economic Census for the years 2006 and 2016 and dividing them by the total working age population. Second, we calculate yearly employment rates based on individual labor market information from household surveys. For each of the districts, the paper further develops a shift-share measure which accounts for the local penetration by FDI regulation in a particular year. We then relate the regulatory penetration to the employment rate in either long-difference specifications or fixed effects panel regressions. To make sure that our measure of regulation does not spuriously capture employment dynamics related to other factors such as structural change, we further allow for differential trends in the initial level of regulatory penetration prior to the first revision of the NIL, as well as the initial shares of agricultural, manufacturing and service sector employment.

The results show a strong and positive employment effect of regulatory penetration that is equally distributed among manufacturing and services. While manufacturing employment shares increase due to market entry of small firms, the service sector employment gains are exclusively driven by new hiring in existing, large corporations. This is indicative for strong employment spillovers driven by increased domestic demand for services and can only be rationalized by a high degree of sectoral integration in the Indonesian economy. FDI regulation thus seems to be successful in directly protecting small manufacturing businesses from international competition.

The thesis is structured as follows: Chapter 2 presents the study on the effect of heat

[6] The study "Regulating manufacturing FDI: Local labor market responses to a protectionist policy in Indonesia" is co-authored by Krisztina Kis-Katos.

days on firm outcomes. Next, chapter 3 introduces the note on disaggregated FDI spillovers on firm productivity. In chapters 4 and 5, the thesis outlines the studies on the firm-level and local labor market effects of FDI regulation. Chapter 6 provides some concluding remarks.

CHAPTER 2

Heat and firm productivity: Evidence from Indonesia's manufacturing sector

Robert Genthner, Sebastian Renner and Enrica de Cian[7]

Abstract

The economic effects of extreme temperatures are complex and still not sufficiently well understood, particularly not in low- and middle-income countries. This paper aims at providing new evidence concerning the impact of rising temperatures on manufacturing firm outcomes in Indonesia. Using a panel of manufacturing firms and controlling for a wide range of fixed effects, we estimate the marginal effect of additional heat days. Our findings suggest a negative relationship between high temperatures and firm productivity. The effect is especially pronounced in urban areas. However, firms are able to offset the negative productivity effect by adjusting factor inputs, thereby keeping output unaffected.

[7] We would like to thank conference participants at the Annual Conference of the European Association of Environmental and Resource Economists 2021 in Berlin, and seminar participants at the University of Göttingen and Freiburg for helpful comments and discussions. Neither the European Commission nor ECMWF is responsible for use of the Copernicus Climate Change Service Information data. All remaining errors are our own.

2.1 Introduction

Rising temperatures can have significant economic and social impacts, especially in developing countries. Temperature changes have been shown to have major impacts on economic output (Burke et al. 2015, Dell et al. 2014, Somanathan et al. 2021), agricultural output (Schlenker and Roberts 2009, Fisher et al. 2012, Colmer 2021), labor productivity (Graff Zivin and Neidell 2013, Somanathan et al. 2021), health and mortality (Deschênes and Greenstone 2011, Barreca et al. 2016), energy use (Auffhammer and Mansur 2014) as well as conflicts and political stability (Hsiang et al. 2013).[8]

A recently emerged body of literature addresses economic output and productivity directly at the firm level, with the works of Somanathan et al. (2021) and Colmer (2021) for India as well as Zhang et al. (2018) and Chen and Yang (2019) for China. However, beyond India and China, nothing is known yet about the relationship between temperature changes and firm performance. Indonesia, as the fourth most populous country in the world, with a large manufacturing sector and an expected, significant temperature increase due to climate change, is of particular importance in this regard. Given its exclusive location in the tropical climate zone, it is essential to quantify the impacts on the production sector and to understand potential heterogeneous effects as they may differ from previous results for countries that extend over more than one climate zone.

In this paper we are primarily interested in the question of how extreme temperatures, closely associated with the recent change in climate conditions, affect the production of goods. In the context of climate change, the main focus for tropical countries is on exceptional heat waves, which can affect firms in the manufacturing sector in different ways. Besides immediate negative effects of weather anomalies on productivity and output (e.g., due to limited physical work capacity, Kjellstrom et al. 2009), firms also face transition risks from technological adaptation and change towards a low-carbon economy that are associated with higher business costs and lower profitability of established products (Goldstein et al. 2019, Semieniuk et al. 2021).

The existing literature emphasizes that the consequences of global warming can lead to substantial productivity losses, not only in agriculture but also the rest of the economy. While it is straightforward that global warming can directly impact crop yields of farmers (Hsiang 2010, Colmer 2021), the link between higher temperatures and changes in manufacturing production is less obvious. Early studies argue that workers are less

[8] Before the emergence of this new weather and climate impact literature, calculations for damage functions used in Integrated Assessment Models (IAMs) have often been non-empirical in nature and have been heavily criticized (Pindyck 2013, Stern 2016). Despite the recent advances in obtaining credible estimates of climate damage estimates (Carleton and Hsiang 2016, Auffhammer 2018), the evidence base is still thin, particularly for low- and middle-income countries.

productive once the temperature at the working site is above (or below) the zone of thermal comfort (Kjellstrom et al. 2009). This was repeatedly confirmed by more recent studies on the individual level (Graff Zivin and Neidell 2014, Adhvaryu et al. 2020, Somanathan et al. 2021, for India) and the firm level (Zhang et al. 2018, Chen and Yang 2019, for China). These papers thereby also stress the effect of heat on capital productivity because the smooth operation of machinery may be impeded on hotter days. The latter studies also argue that firms can counteract potential negative effects of climate change by factor input adjustments, for instance additional investments into cooling devices or heat-proof machinery.

Our paper adds to this literature by presenting evidence of firm-level responses to increasing temperatures, which are closely associated with climate change, over a time span of more than 20 years. This constitutes the longest time span analyzed with respect to firm-level impacts of temperature changes in the literature. To our knowledge this is also one of the first studies to investigate the response of firms to climate change in Indonesia (with the notable exception of Xie (2019) who looks at exit and entry decisions of firms). Our study thereby is among the first to investigate firm responses to extreme temperatures in a country that is exclusively located in the tropical climate zone. Our findings thus also test the validity of previous papers that exploit large temperature variations across different climate zones (e.g., Zhang et al. 2018, on China).

Throughout our analysis, we allow for differential effects with respect to the degree of urbanization. This incorporates the established fact that metropolitan areas and cities are on average exposed to higher temperatures compared to their surroundings – also known as the urban heat island effect (Bornstein 1968, Oke 1973, Tan et al. 2010). It thus seems plausible to expect differential effects of extreme temperature events among firms in rural and urban areas that go beyond mere differences in the degree of electrification or access to technology. We are not aware of any study that investigates firm responses to global warming with the particular focus on differences across rural and urban areas. This paper investigates the effect of changes in temperature on manufacturing firm outcomes such as output, productivity and factor inputs. To shed more light on the exact mechanism, we further exploit rich information on electricity consumption to see whether firms react to higher temperatures by using more energy to power air conditioning.

Our analysis builds upon firm panel data of the manufacturing sector in Indonesia.[9] The data spans over the period from 1993 to 2015 which enables us to control for time-invariant but otherwise unobservable firm characteristics. We merge climate data to firms on the district level, a second tier administrative division which enables a geographically detailed representation of climate variables over time. Our main explana-

[9] The units of observation are manufacturing plants. We use the terms firm and plants interchangeably.

tory variables count the number of days with average temperature within a 1°C range for each year in each district. This bin approach (Deschênes and Greenstone 2011) preserves more nuanced temperature dynamics as compared to simply using the average annual temperature. Our preferred specification further includes annual measures of precipitation and relative humidity on district level, and controls for year-specific dynamics within macro-regions and five-digit products.

Our results do not show clear evidence of an inverted U-shaped temperature-output relationship across all Indonesian districts and thus cannot reproduce the distinct findings by related studies on China (cf. Zhang et al. 2018). As argued above, this may be explained by very small temperature variation across time and space in absolute terms in comparison to other countries that stretch over more than one climate zone. In contrast, we find evidence for the inverted U-shaped relationship between productivity and temperature among firms in more urbanized districts. One additional day above 27°C in city districts results in a 0.15% reduction in capital productivity, relative to an additional day between 21 and 22°C. These effects are entirely driven by firms on the island of Java which also hosts the vast majority of manufacturing activity in Indonesia. The negative productivity effects are particularly pronounced among under-electrified firms in agglomeration areas. When looking into the effect of temperature on electricity use, we see that higher temperature increases demand for electricity in rural areas, while there is no such effect among urban firms.

With Indonesia being one of largest emerging economies in the world with huge potential for development and growth in the future, our findings are relevant not only nationally to a large number of individuals, but also from a global perspective. A significant number of low- and middle-income countries are located in the tropical climate zone, where empirical knowledge about the impact of rising temperatures on the economy is particularly small. In this group of countries, the measures taken to adapt to climate change will be essential. Here, empirical findings on the influence of weather can be an important basis for decision-making.

This paper proceeds as follows. Section 2.2 reviews the existing literature. Section 2.3 introduces some descriptive facts on climate change in Indonesia and section 2.4 presents a basic theoretical framework. We introduce our data in section 2.5, while section 2.6 discusses the empirical strategy. Eventually, in section 2.7 we discuss our results and section 2.8 concludes.

2.2 Literature

There is a rapidly growing body of literature looking into economic and agro-economic effects of climate change. Most of the early studies thereby focus on the aggregate effects of rising temperature across countries, states or industries. Using sector-level data for several countries in the Caribbean and Central America, Hsiang (2010) shows that increasing temperatures do not only affect agricultural output through reduced crop yields, but also non-agricultural production. He explains his finding by reduced labor productivity due to thermal stress on workers.[10] In a similar vein, Dell et al. (2012) find that higher temperatures reduce growth rates in a cross-country setting for the time period 1950 to 2003. However, this relationship only holds for poor countries. Like Hsiang (2010), the authors identify declining output both in agriculture and manufacturing as main economic channels, but also show that increasing temperatures correlate with political instability. More recently, Somanathan et al. (2021) confirm that manufacturing output suffers in response to higher temperatures in India. Based on evidence from daily worker-level data of several manufacturing plants over two calendar years, they conclude that, on average, worker productivity declines on extremely hot days. At the same time, the authors also document increases in absenteeism of workers.

Using detailed worker-level data for the US, Graff Zivin and Neidell (2014) also find that temperature affects daily decisions by individuals. In particular, they show that workers spent less time outdoors on extremely hot days. This effect is especially pronounced among workers in heat-exposed industries. Adhvaryu et al. (2020) identify negative productivity effects of high temperature among workers. Using daily data, they exploit the rollout of low-heat LED in Indian garment plants and find that diminished heat radiation of LED lightning attenuates the negative relationship between temperature and labor productivity. They further show that neither absenteeism of workers nor working hours can explain this effect, and conclude that output adjustment takes place at the intensive margin.

On the firm level, Kelly et al. (2005) show in their model that simulated weather shocks cause profit losses for agricultural firms in the US. More closely related to our study, Somanathan et al. (2021) use firm-level data from the Indian manufacturing sector for the years 1998 to 2013. In line with their results on the individual level, they find that firms exposed to a higher number of extremely hot days also produce less output. They further decompose the temperature effect on output by input factors and find that massive declines in labor productivity explain most of the output reduction. Zhang et al. (2018) use Chinese panel data of manufacturing firms from 1998 to 2007 and find an inverted U-shape relationship between temperature (measured as number of days in temperature bins for each year) and output. Like for Indian firms, input factors such as labor and capital do not react to changes in temperature, while reductions in output are mainly driven by lower productivity in both labor- and capital-intensive industries.

[10] This relationship is also well known in the medical literature (cf. Axelson 1974).

This can be explained by fatigue and cognitive impairment of workers when temperatures are above the zone of thermal comfort, as well as the lowered efficiency and performance of machines. Using the same firm dataset like Zhang et al. (2018), Chen and Yang (2019) augment the set of results by looking into more nuanced dynamics. Their findings indicate that productivity effects are stronger in initially cooler regions and explain this by human adaptation already being undertaken in warmer provinces. The study further disentangles the impact of seasonal temperature variation and finds that output actually increases with warmer spring temperature, while hotter summers affect production negatively. In contrast, Colmer (2021) shows for India that higher daily average temperature leads to a reduction of agricultural production and employment. In flexible labor markets, these unemployed workers act as positive labor supply shock in manufacturing, thereby resulting in a net increase in manufacturing output. He emphasizes the importance of labor mobility and reallocation of workers across sectors as coping mechanisms for climate change. In a recent working paper based on the same yearly data for Indonesian manufacturing as our study, Xie (2019) examines the relationship of firm entry and exit, and temperature increases over the period 2001 to 2012. She finds that firms are more likely to exit markets with increasing average yearly district temperatures and that this effect is most pronounced among the least productive enterprises.

2.3 Climate change in Indonesia

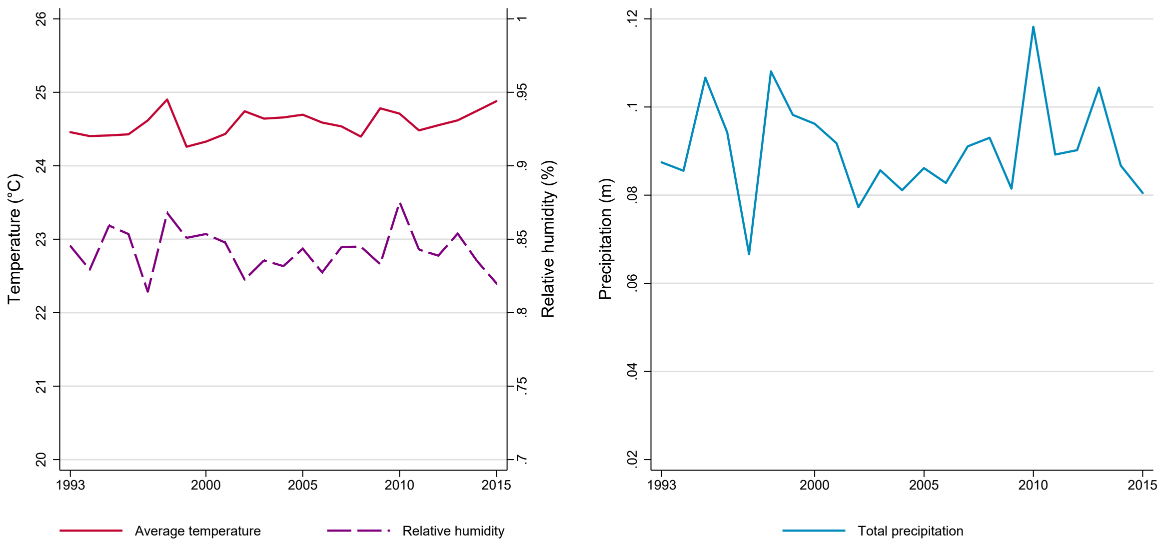
2.3.1 Measurement of temperature

The question of how to measure temperature anomalies is closely linked to the estimation strategy. Both the spatial and temporal aggregation level of meteorological variables thereby impacts the empirical specification and modeling. Additionally, a whole battery of potential climate indicators may be included in the analysis.

The most intuitive strategy is to include average values of temperature for a particular time and spatial unit. Dell et al. (2012) use yearly average temperature for each country as the main explanatory variable, while further controlling for precipitation. Similarly, Colmer (2021) constructs a district-level measure of daily average temperature and further controls for rainfall in India, while Xie (2019) uses yearly temperature plus relative humidity and total precipitation calculated from daily observations.

Using average temperature, however, may hide important weather dynamics. Calculating the mean for a time period implies loss of information about the distribution of weather events. In their pioneering work, Deschênes and Greenstone (2011) introduce an idea to overcome this problem by conserving the daily distribution of temperature

Figure 2.1: Average temperature, relative humidity and precipitation over time



Note: Temperature (in °C), relative humidity (in %) and total precipitation (in m) as average across all non-missing districts by year. Source: authors' visualization based on ERA5-Land.

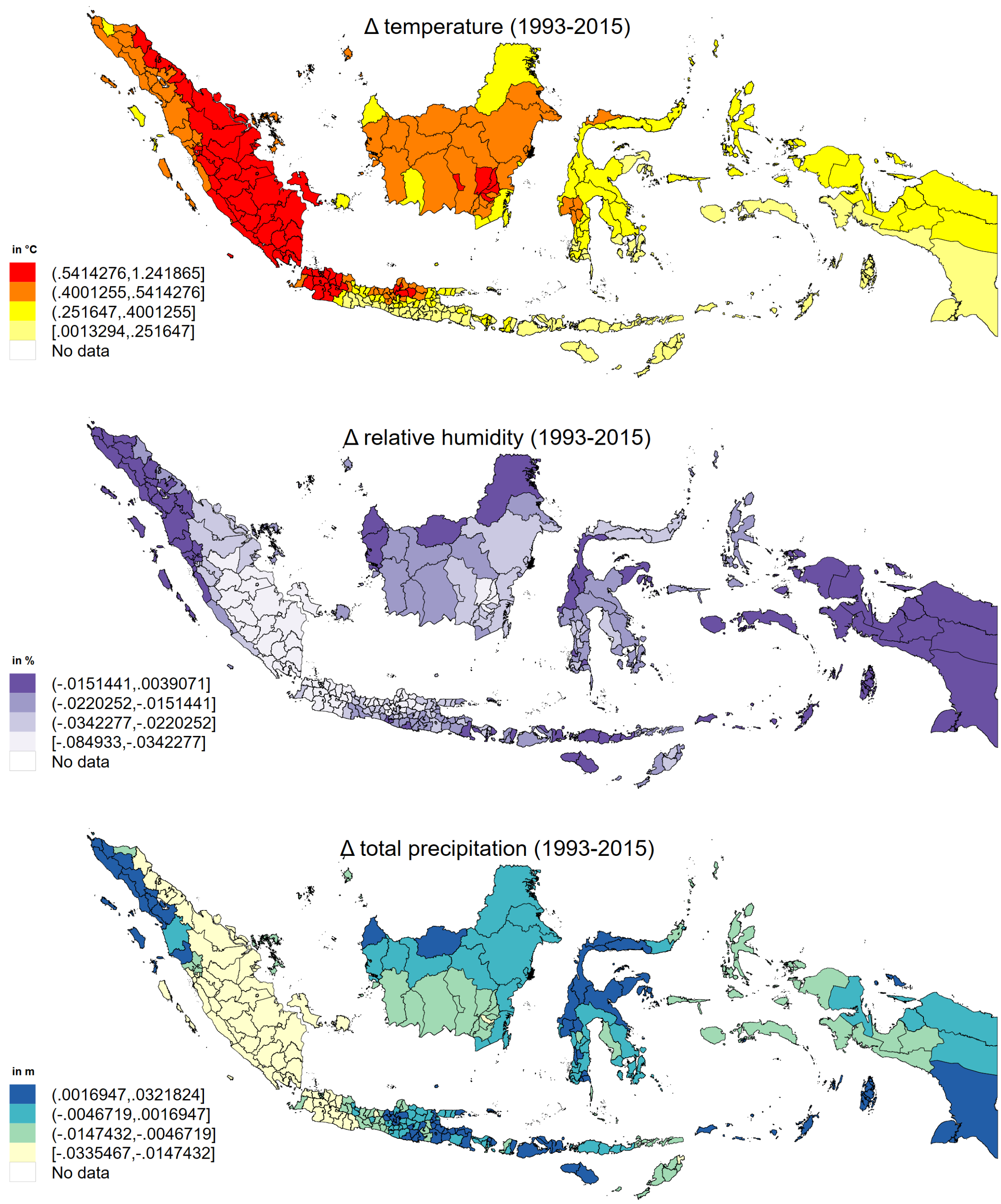
over a year in the data. More specifically, they count the number of days with average temperature within 10-degree Fahrenheit bins for each year. Looking at the time period from 1968 to 2002 in US counties, they find that mortality and residential energy consumption peak in years with an exceptionally high number of very hot or cold days, leading to a U-shaped relationship. The approach of temperature bins was used in various contexts by other studies (cf. Zhang et al. 2018, Chen and Yang 2019, Somanathan et al. 2021). Following this literature, our estimation strategy also exploits changes in the temperature distribution over time.

2.3.2 Descriptive trends

According to the World Bank Climate Change Knowledge Portal, Indonesia experienced a 0.3°C increase in the average annual temperature between 1990 and 2016. Over the same period, annual rainfall has declined by 2 to 3 percent (World Bank 2016). Our data show similarly small changes over time as depicted in figure 2.1.

When looking at the spatial distribution of climate change between 1993 and 2015 in our data, we also find small increases in average annual temperature across all Indonesian districts in figure 2.2. However, temperature shifts were a lot more pronounced on the island of Sumatra and the western parts of Java (around the national capital Jakarta), whereas temperature changes are a smaller concern in eastern Indonesia. At the same time, relative humidity actually declined in all districts on average. Strongest declines can be observed at the coast near Aceh in the west and on Papua. Figure 2.2

Figure 2.2: Change of average temperature, relative humidity and precipitation between 1993 and 2015



Note: Spatial representation of change in average district temperature, relative humidity and precipitation between 1993 and 2015. Source: authors' visualization based on ERA5-Land.

further shows how total precipitation has changed over time. Most districts experienced a decline in rainfall, spatially mirroring the patterns for relative humidity.

Annual aggregation of weather data, however, blurs more nuanced meteorological changes in Indonesia. For example, precipitation patterns have changed towards more extreme events. Southern regions like the island of Java have experienced an overall decline in average annual rainfall, while precipitation in the wet season (November to April) has actually increased at the same time (resulting in massive floodings in Greater Jakarta, The Jakarta Post 2020). In contrast, the northern provinces on Kalimantan and Sulawesi have faced average increases in yearly rainfall, but less rainfall in the dry season (World Bank 2016).

2.4 Theoretical framework

It is helpful to think about the mechanisms through which rising temperatures may influence firm output within the framework of a standard Cobb-Douglas production function on the firm level:

$$Y = \quad (A_L L)^{\alpha_L} (A_K K)^{\alpha_K}, \tag{2.1}$$

where Y is total firm output, L is labor input and K is capital input. A_L and A_K capture labor and capital productivity. Taking logs yields:

$$\ln Y = \quad \alpha_L \ln A_L + \alpha_K \ln A_K + \alpha_L \ln L + \alpha_K \ln K, \tag{2.2}$$

where the sum of log labor and capital productivity, weighted by the output elasticity of each input, can be summarized as total factor productivity (TFP).

Climate change may affect all components of equation (2.2). First, high temperatures can reduce labor productivity by reducing thermal comfort and cognitive functions, as well as increasing fatigue (Graff Zivin and Neidell 2014, Adhvaryu et al. 2020). Kjellstrom et al. (2009) highlight that the human body is in general "designed to maintain a core body temperature of 37°C" (p.2). The core body temperature is thereby dependent on external factors like temperature, humidity and air movement, but also clothing and physical activity. In a hot working environment, however, standard cooling mechanisms like sweating and convection may be insufficient. If the core body temperature exceeds the zone of thermal comfort, this results in a limited physical work capacity, a lower mental task ability, as well as an increased risk of accidents or heat exhaustion. At the same time, a low temperature may also cause discomfort. Second, temperature can also affect capital productivity since it impacts the underlying nature of physical laws and chemical reactions. For example, cooling devices for computers may reach their limit when temperature is too high. As a third channel, firms can counteract potential negative productivity effects indirectly by adjusting their factor inputs. For instance, the length of working shifts may be adjusted, or more workers can be hired. This latter adjustment may be especially relevant if additional heat days lead to layoffs in agriculture and thus increase labor supply in the manufacturing sector (Colmer 2021). Finally, firm management can also change capital input by investing into air conditioning, or new machinery which is better adapted to more challenging climate conditions.[11]

In line with existing studies (e.g., Zhang et al. 2018, Chen and Yang 2019), this paper disentangles these direct and indirect channels to see whether and how output in Indonesian manufacturing firms is affected by climate change. More precisely, we estimate the effect of changes in the temperature distribution on firm outcomes like output, productivity and factor inputs.

2.5 Data

2.5.1 Weather data

We compute our weather variables based on the ERA5-Land database from the Copernicus Climate Change Service (C3S) (Muñoz-Sabater 2019a). The ERA5-Land data is a reanalysis dataset based on climate models that use historical local observations and satellite data to generate a consistent record of weather variables on 0.1x0.1 degrees grid level. As purely station-based weather data is frequently not available (especially not when analyzing a long time period), reanalysis data offer a consistent best estimate of climate parameters over time and space (Auffhammer et al. 2013). Since we can locally identify firms only at the district level, we merge grid cells to the respective district borders.[12]

We use hourly data from 1993 to 2015 to construct temperature bins for each year. As argued above, this approach preserves more nuanced information of the data as compared to simple yearly averages (cf. Deschênes and Greenstone 2011, Zhang et al. 2018). We hereby use dry bulb average temperature to get the daily temperature distribution.[13] We count the total number of days T^r_{dt} with an average temperature (measured in °C) within a particular temperature range r for each district d in year t. For dry bulb temperature, we have $r = 1...9$ bins, where T^1_{dt} is the number of days below 20°C for district d in year t, and each interior bin covers a range of 1°C. At the upper end of the distribution, T^9_{dt} is the number of days above 27°C. Figure A2 in the appendix shows that the frequency distribution of daily temperature shifts to the right

[11] Day et al. (2019) provide an extensive discussion of potential adjustment strategies to counteract productivity losses from climate change.

[12] Indonesia underwent a significant decentralization process from the late 1990s to the end of the 2000s. In this process, many districts split and formed new administrative entities. To keep district borders consistent over time, we fix borders before decentralization started (in 1993).

[13] Dry bulb temperature refers to standard "air temperature" at two meter above ground. It disregards moisture of the air and can be measured with a standard thermometer.

over time, with cold days being more and more replaced by warmer days. There is a massive increase in days per year in the 26-27°C bin (T^8_{dt}), which seems to originate mainly from less days in the lower temperature bins. To simplify the visualization, figure 2.3 only shows the temperature distribution of the first (1993) and last (2015) year in our sample. Additionally, we separately depict the distribution within districts with low (rural) and high (urban) population density. The graph illustrates the urban heat island effect. Rural areas exhibit more days with moderate average temperature and fewer days with extreme heat, resulting in the rural distribution being left of the urban distribution. At the same time, the temperature distribution shifts to the right over time for all districts. The increase in days with an average temperature of more than 26°C is the identifying variation that our empirical strategy exploits.

We also include further weather controls beyond temperature (Zhang et al. 2017). This may be of particular importance in the context of the tropical climate of Indonesia. Similar high temperature obviously feels different depending on relative humidity of the surrounding air and, thus, affects the zone of thermal comfort. Disregarding humidity or precipitation may induce severe omitted variable bias into our regressions. We therefore add average yearly relative humidity and total precipitation at the district level to our set of control variables, based on monthly data from ERA5-Land (Muñoz-Sabater 2019b).

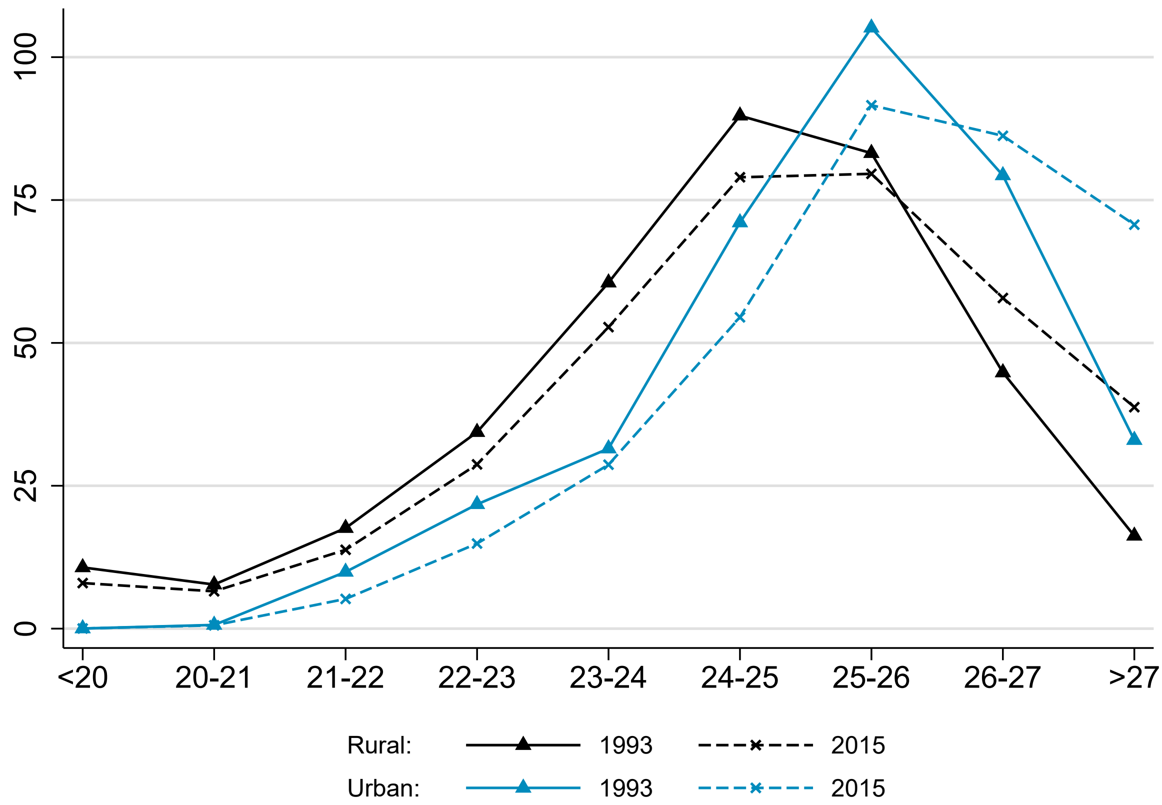
2.5.2 Firm data

Our firm data stems from the annual manufacturing census of Indonesia (*Survei Industri*, SI). It covers the universe of all registered medium-sized and large manufacturing firms in Indonesia.[14] Firms are surveyed on an annual basis and can be tracked over time. Our empirical strategy thus allows for firm fixed effects. The survey collects information on the district of a firm's location, as well as balance sheet data. These inputs include labor and fixed capital, as well as aggregate intermediate materials and energy consumption such as electricity. We clean the data to account for missing values and trim our sample to get rid of extreme outliers. As pointed out by Márquez-Ramos (2021), the capital variable is particularly critical since it is not available in 1996 and 2006. We follow the literature and interpolate those missing values with information from the previous and next year (cf. Amiti and Konings 2007, Genthner and Kis-Katos 2019). Remaining missing observations are excluded. Monetary values are transferred to 2008 prices and all input and output variables are transformed into their natural log.

Our main specification exploits alternative measures of productivity. We use log value added per worker as a proxy for labor productivity (Amiti and Konings 2007, Zhang

[14] Medium-sized refers to plants with 20 employees or more.

Figure 2.3: Number of days in temperature bins in 1993 and 2015 (by population density)



Note: Number of days in discrete temperature bins across all non-missing rural (black) or urban (blue) districts. Solid lines depict the distribution in 1993 while dashed lines represent the distribution in 2015. Source: authors' visualization based on ERA5-Land.

et al. 2018), and similarly log value added per capital as a proxy for capital productivity. However, we also complement the analysis with a measure of total factor productivity. We therefore use a GMM estimation procedure by Wooldridge (2009) to account for simultaneity bias in the input factors, and estimate TFP according to equation (2.2) separately for each two-digit sector. Our baseline sample is an unbalanced panel of 39,097 firms for which we have 318,675 observations.[15]

The SI data have some limitations. First, one may question whether the census really includes the full universe of manufacturing firms with more than 20 employees. The enormous size of the Indonesian economy and the related obstacles in the surveying process cast at least some doubt on the completeness of the survey. In particular, non-responding firms or smaller enterprises may remain unobserved, potentially leading to non-random selection. However, there are financial incentives for field agents to register new firms and follow up on pending replies (Blalock and Gertler 2008, Arnold and Javorcik 2009).

[15] The sample size further shrinks depending on the specifications and additional missing values in some of the variables. A more detailed discussion of the sample reduction due to missings is provided in appendix A.1.

Second, firms may report wrong information, either accidentally or on purpose. If answers included false information by accident, we would not expect systematic misreporting and our estimates should be unbiased. However, noise in the data may be substantial and increase our standard errors. Intentional misreporting is a more serious issue for our identification. Even though national law guarantees that all information collected in the survey is only used for statistical purposes and will not be passed on to third parties, firms may still be concerned about information leakage to tax authorities or direct competitors. This could be an incentive to report false data on purpose (Blalock and Gertler 2008). However, we exploit exogenous weather shocks which should not be systematically correlated to firm accounting and, thus, we think that intentional misreporting again only inflates our standard errors.

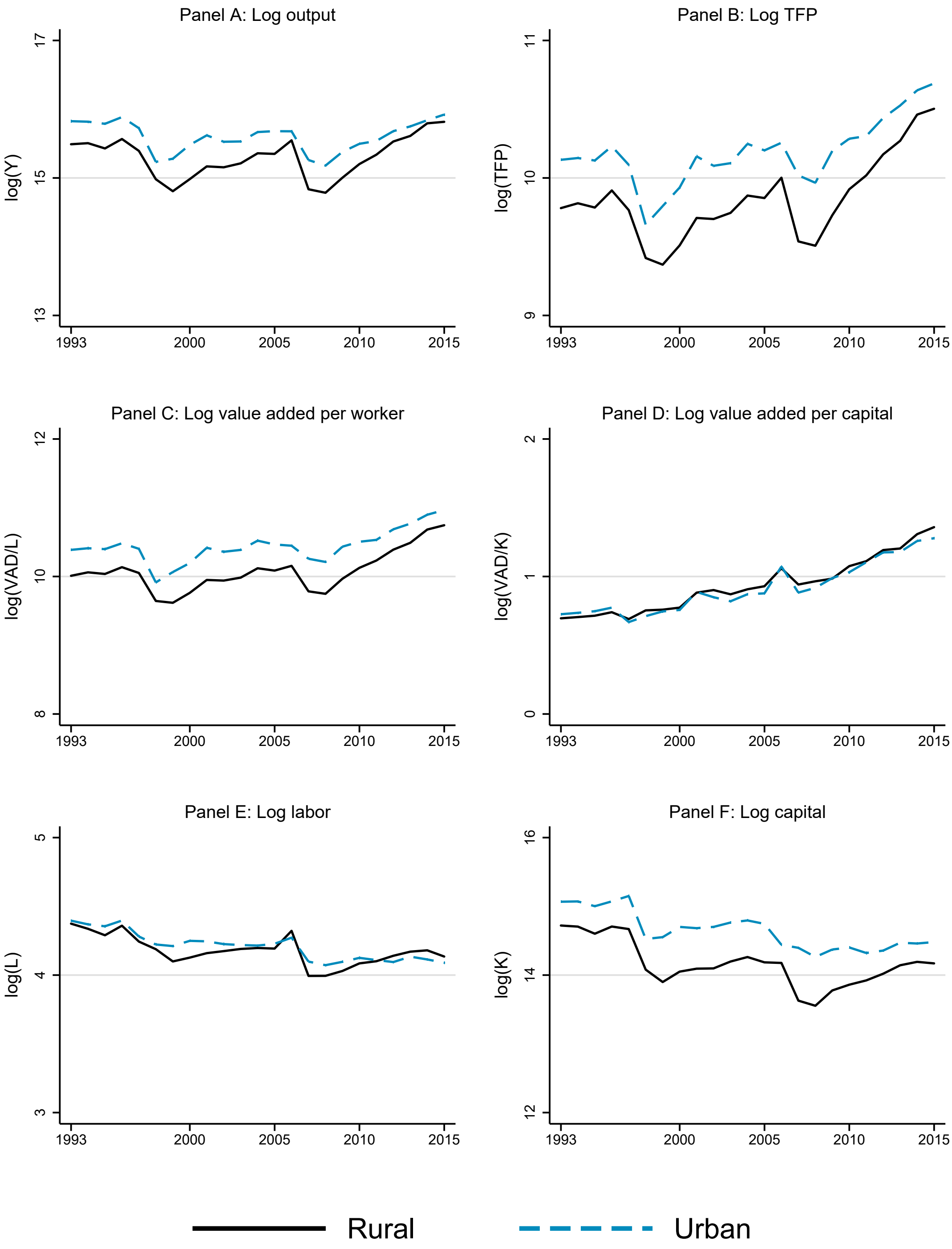
Third, the SI data do not reveal information about multi-product firms, and we therefore assign each firm to the industrial sector of its main product on 5-digit KBLI classification.[16]

Figure 2.4 shows the development of key input and output variables as well as productivity of an average firm. Trajectories are shown separately for firms located in districts with low (rural) or high (urban) population density. Firms in urban districts on average have higher output and capital, as well as higher total factor and labor productivity. Over the last years of the sample period, there is some evidence that this gap is closing. There is no such pronounced difference with respect to capital productivity and labor input. As an important robustness check, we will allow for differential trends among rural and urban districts to make sure that the temperature variation does not spuriously pick up underlying catch-up dynamics. Moreover, the graphs show two remarkable kinks in trends over time for all key variables. The first occurs at the end of the 1990s and can be associated to the Asian financial crisis. A second structural break in the data can be observed around 2006 (and 2007). The latter is related to the survey coverage in the census year 2006 and a selective over-reporting of smaller enterprises as argued above. These breaks, however, have had a similar effect on firms in rural and urban districts.

Similarly, figure A3 in the appendix depicts trends of electricity consumption by Indonesian manufacturing firms over time. On average, urban firms have higher electricity consumption, even though the used quantity is stagnating in the last ten years of the sample period. The trend is also interrupted by a steep drop in 2006. Like before, this can be explained by a large increase in the number of firms in this year, thereby reducing the sample average. In section 2.7.4, we split the sample into pre and post 2006

[16] The Indonesian Statistical Office (BPS, *Badan Pusat Statistik*) classifies sectors according to KBLI (*Klasifikasi Baku Lapangan Usaha*), which is equivalent to the United Nation's International Standard Industrial Classification of All Economic Activities (ISIC) at the four-digit level. There are minor differences on the five-digit level to account for particular sectors of local importance.

Figure 2.4: Firm outcomes over time (by population density)



Note: Graphs depict average firm outcomes in logs over time, based on the baseline sample of 39,097 firms.

to see whether the structural break due to the changed survey coverage can explain our main results. In general, however, this structural break is only a concern on the aggregate level. Our identification strategy relies on within firm variation over time where the structural break is not visible anymore. We present further descriptive summary statistics on the main input and output variables from the SI in appendix table A1.

2.5.3 Additional data

We complement our data with information on the degree of urbanization of districts. More urbanized and densely populated districts are likely to be heat islands and thus firms are subject to differential dynamics. Our preferred measure is the median population density, calculated as the median district population relative to the district size. We extract population numbers from the Indonesian household surveys (*Susenas*). We define districts in the upper 10 percent of the population density distribution as urban. As a robustness check, we test two alternative indicators of urbanization. First, we use the *Susenas*-based share of population living in an urban area and again split districts at the 90th percentile. Second, we exploit the naming of districts and separately look at *kabupaten* and *kotamadya*, with the latter being city districts by law.

The correlation between the three alternative measures is fairly high (between 0.6 and 0.75), but also shows that the three capture different dimensions of urban agglomerations. The measures based on the distribution of population density (urbanization rate) identify 29 (28) districts as urban, while there are 55 districts classified as *kotamadya*.

2.6 Empirical strategy

Since the SI data is only available annually, our analysis relies on yearly changes in temperature. To preserve more nuanced dynamics in climate change on a daily basis, we split the annual distribution of daily average temperature into a fixed set of temperature bins (Deschênes and Greenstone 2011). Using this semi-parametric approach provides a flexible estimation of non-linear effects without requiring a predefined assumption on the nature of these non-linearities (like in higher degree polynomial regressions).

Our empirical specification estimates the effect of temperature on firm-level outcomes. We follow the literature and first re-estimate results by Zhang et al. (2018) for each term of the production function (in equation (2.2)). The baseline regression equation is:

$$y_{idt} = \sum_r \beta^r T^r_{dt} + \boldsymbol{W}'_{dt}\gamma + \delta_i + \phi_{mt} + \lambda_{jt} + \varepsilon_{idt}, \tag{2.3}$$

where y_{idt} denotes the log firm outcome (such as productivity or factor inputs) of firm i located in district d in year t. $\boldsymbol{W}_{dt}$ includes further climate controls like total precipitation and relative humidity, as well as their squared terms to allow for non-linear relationships. δ_i are firm fixed effects to account for time-invariant firm traits. ϕ_{mt} are macro-region-year fixed effects to control for yearly shocks common to all firms located on the same major island m.[17] This also takes care of island-specific climate trends. λ_{jt} are five-digit product-year fixed effects that address common shocks to all firms operating in the same manufacturing product j, such as price dynamics, import competition or industrial regulation. To account for serial correlation in the error term ε_{idt}, we cluster standard errors on firm and district-year level to permit for both serial and spatial correlation. For our main results, we intentionally exclude any non-weather variables on firm level to obtain the total marginal effects of temperature on output (Chen and Yang 2019).

The coefficients of interest are the semi-elasticities β^r. They capture the marginal effect of one additional day with average temperature in bin r relative to a day in the reference temperature bin, which is omitted from equation (2.3).[18] We choose T^3_{dt} (21-22°C) as the reference bin, as this temperature guarantees the highest thermal comfort for workers and thus should be the relative benchmark for labor productivity effects.

Our analysis extends equation (2.3) by allowing for differential effects among firms in rural or urban districts:

$$\begin{aligned} y_{idt} = \; & \sum_r \text{Rural}_d \times \beta^r T^r_{dt} + \sum_r \text{Urban}_d \times \beta^r T^r_{dt} \\ & + \theta\, \text{Urban}_d \times t + \boldsymbol{W}'_{dt}\, \gamma + \delta_i + \phi_{mt} + \lambda_{jt} + \varepsilon_{idt}. \end{aligned} \tag{2.4}$$

This allows us to test the urban heat island hypothesis and check if firms in urban areas are differentially affected by extreme temperatures and the outcomes of global warming. By estimating the total marginal effect among rural or urban firms of one additional day in a particular temperature bin relative to the reference bin, we are able to keep both the number of observations and the composition of our control groups constant. Equation (2.4) additionally controls for urban-specific trends. This allows firms in more densely populated areas to be on different trajectories than rural firms. For instance, rural firms may catch up in terms of productivity over time, or input factor supply could be different depending on the location. Table 2.1 shows that controlling for these dynamics is particularly important for output and factor inputs.

For a causal interpretation of our results, temperature variation from year to year needs to be exogenous to firms. We consider this assumption to hold, since the impact of Indonesian firms on global climate change is rather negligible, at least in the short-run. At the same time, we do not find evidence in the data for any efforts of plant re-location

[17] We distinguish between five islands: Sumatra, Java, Kalimantan, Sulawesi, as well as Papua and Outer islands.

[18] $\sum_r T^r_{dt} = 365$, so one bin has to be dropped to avoid perfect multicollinearity. The choice of the reference bin is arbitrary and does not affect our results in relative terms.

to actively select into particular climate conditions. Our heterogeneity results also indicate that the effects are entirely driven by firms in Java. This alleviates concerns that extreme temperature events may be spuriously correlated with deforestation dynamics, as most deforestation in Indonesia takes place on Sumatra and Kalimantan (Cisneros et al. 2021) and any effect of declining forest area on climate change can be considered fairly exogenous to localities on Java.

Complementing our main results, we exploit the rich information on inputs and energy consumption in our firm data to show further potential channels of adjustment by introducing further interactions. We thereby allow for effect heterogeneity by the firm's electricity intensity, as well as the industry's labor intensity or technological content.

2.7 Results

2.7.1 Main results

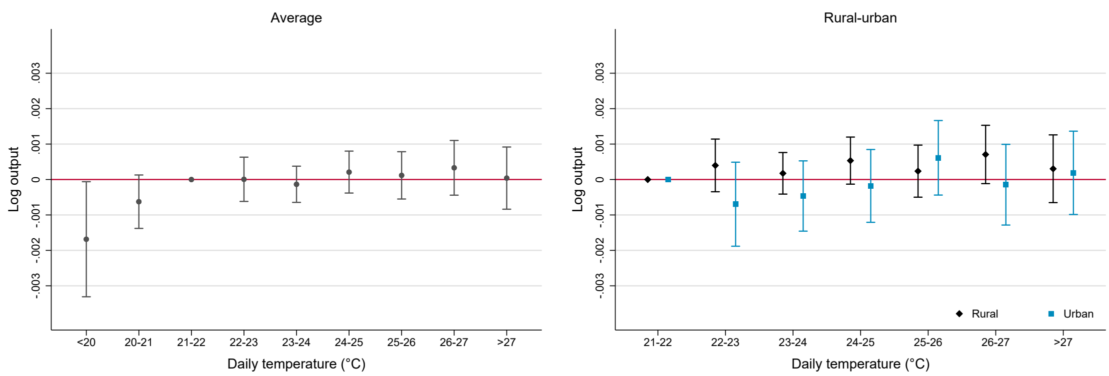
Figure 2.5 visualizes our main results, controlling for firm, island-year and product-year fixed effects, as well as second order polynomials of relative humidity and total precipitation. We first report average effects of temperature on firm outcomes in the left graphs (based on equation (2.3)), but then also check for the urban heat island hypothesis by allowing for differential effects among rural and urban firms (based on equation (2.4)). These regressions additionally allow for differential trends among rural or urban firms. The point estimates of the temperature bins are depicted together with their 90% confidence intervals. In all panels, T^3_{dt} (21-22°C) is omitted as reference category, and estimates show effects relative to the reference bin.

For the average effects, we do not find a clear pattern for the temperature-output relationship in panel A. Our findings thus do not feature the striking inverted U-shape known from studies on Chinese manufacturing firms (Zhang et al. 2018, Chen and Yang 2019). One potential reason may be the relatively small temperature changes in Indonesia and the associated narrow temperature bins in our analysis (compare 1°C bins versus approx. 5°C bins in Zhang et al. (2018)). However, it is also possible that Indonesian manufacturers manage to counteract the negative effects of higher temperatures. Interestingly, the lowest temperature bin is most clearly associated with output declines. This may hint at a stronger response by firms to colder days. However, we do not want to over-interpret these findings given that there is a rather small number of firms affected by very few days in the coldest temperature bin (compare figure 2.3). This is in line with a relatively large confidence interval around the point estimate.

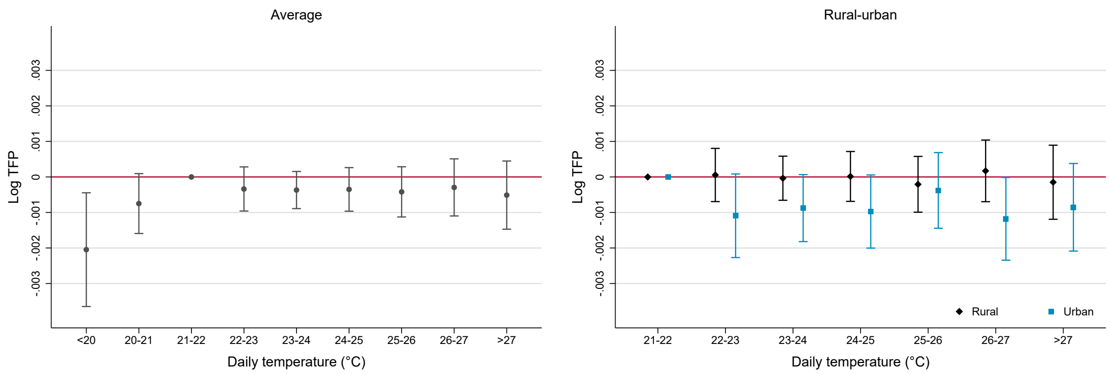
The presented average results mask more pronounced dynamics with respect to the degree of urbanization of districts. When allowing for differential dynamics in rural

Figure 2.5: Average and location-specific effects of temperature on output, productivity and input factors

Panel A: Log output



Panel B: Log TFP



Panel C: Log value added per worker

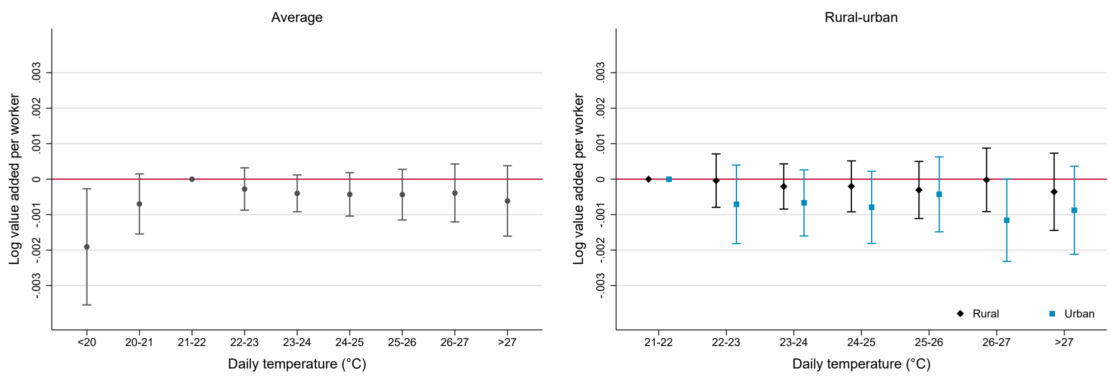
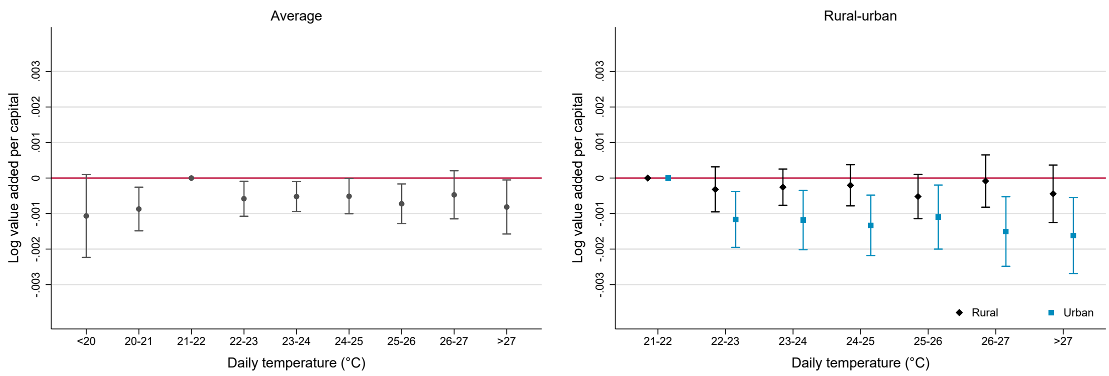
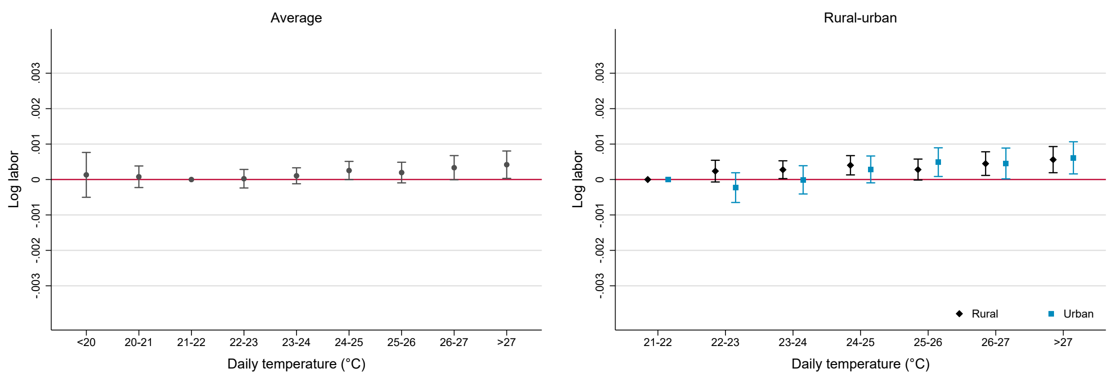


Figure 2.5: Average and location-specific effects of temperature on output, productivity and input factors (continued)

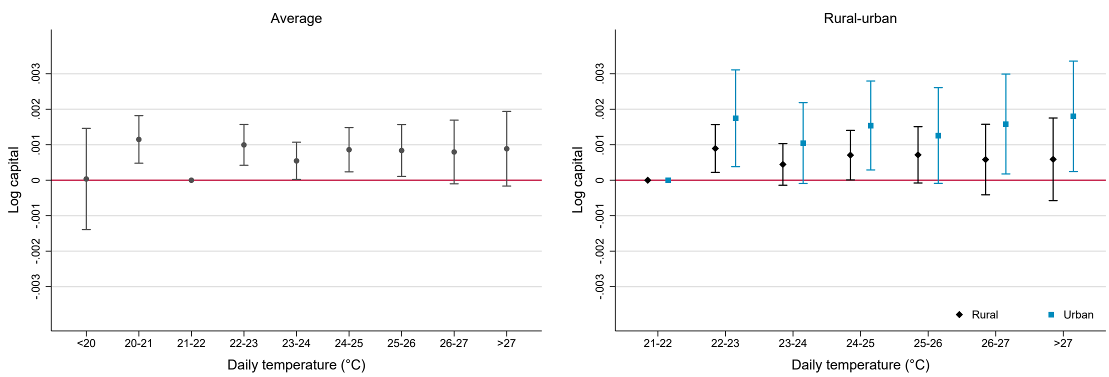
Panel D: Log value added per capital



Panel E: Log labor



Panel F: Log capital



Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Figures show point estimates and the associated 90% confidence intervals as bars. Regressions are specified according to equation (2.4) and control for firm, island-year and product-year fixed effects. Standard errors are clustered on firm and district-year level. 21-22°C is omitted.

and urban areas, the right graph of panel A shows zero effects of high temperature on output within urban firms. In contrast, firms in rural areas even experience small output gains. As our identification strategy restrictively controls for underlying differential trends among rural and urban firms we are unlikely to pick up ongoing location-specific dynamics. For the right graphs in figure 2.5 we always exclude the lowest two temperature bins from the left graphs for the benefit of a visually more appealing axis scaling.[19]

Next, panels B to D depict the effect of temperature on various measures of productivity. In all panels, the average results suggest a weak inverted U-shape relationship. The negative effects of heat are driven by productivity losses within urban firms, even though most coefficients do not reach conventional significance levels. This is in line with the idea of urban heat islands which exaggerate the impact of rising temperatures. The increasing number of hotter days does not affect the productivity of firms in less densely populated districts.

Panels C and D further decompose the TFP effect into labor and capital productivity (proxied by value added per worker or capital). Like for TFP, the effects of additional warm days is only present within urban firms. The effect on capital productivity is larger and also more precisely estimated as compared to the impact on TFP or labor productivity. This suggests that TFP losses are driven less by reduced worker performance, and to a larger extent by the diminished capacity of machinery.

The estimate for urban firms in Panel D suggests that one additional day with more than 27°C results in a drop in capital productivity by about 0.16%, relative to an additional day within the reference bin of 21-22°C. To put our result into perspective, Zhang et al. (2018) find a 0.56% decline in TFP from one extra day in the hottest bin (above 32°C) relative to their omitted group (10-16°C). Thus, their finding identifies the effect of an additional day with more than 16°C above the reference group. This difference is much smaller in our case (about 5°C). Using a naive approximation to ease comparison yields a 0.19% productivity decline for an equivalent temperature increase in the Chinese data, which is only slightly larger than our estimate.

Panels E and F depict potential adjustment mechanisms of firms. On average, we find a statistically significant increase in employment in the years with extremely hot days in panel E. This time, however, the effect is equally driven by rural and urban firms. The effect is economically meaningful. For instance, rural districts have experienced an increase in days with an average temperature above 27°C of about 25 days over the sample period. This translates into about 4 additional employees per firm due to

[19] In particular, the coefficients for urban firms are very imprecisely estimated due to a very low number of observations. This leads to very large confidence bands which inflate the axis scaling and make a clear visualization of the remaining coefficients impossible. However, the coefficients are shown in table 2.1.

climate change. Even though we cannot trace back the origin of these newly hired workers, our finding is in line with Colmer (2021) who shows that heat reduces agricultural employment and thus can act as a positive labor supply shock in the manufacturing sector. At the same time, there is evidence that capital investments are a relevant coping mechanism for all firms, but especially urban enterprises. The positive impact of higher temperatures on the firm's capital stock is suggestive of investments into cooling devices or new heat-proof machinery to keep operations going.

Our findings indicate that firms manage to keep output levels unaffected despite ongoing climate change. Firms increase both employment and capital stocks in response to additional heat days. The latter effect is more pronounced among urban enterprises. At the same time, there are remarkable differences with respect to productivity levels. While rural firms are not negatively affected in terms of productivity, urban enterprises experience productivity drops due to heat. The effect is most pronounced for capital productivity.

Table 2.1 presents the full set of main results using additional specifications. To keep results readable and clear, we only report the point estimate of the lowest and upper two temperature bins. Each regression result is shown in two sub-columns (for rural or urban interactions). Specifications 1 to 3 add fixed effects step-by-step, while 4 is our preferred and most conservative specification including urban-specific trends (see equation (2.4)). Most of our results only become visible after controlling for both island- and product-specific time shocks. This emphasizes the importance of controlling for unobservable shocks in order to cleanly identify the effects of temperature on firm outcomes. Throughout all specifications, none of the estimates changes its sign. Results for productivity also remain qualitatively the same when additionally allowing for differential dynamics of rural and urban firms. In contrast, panels A, E and F show that the estimated effects on output, as well as labor and capital inputs substantially increase among urban firms. This suggests that firms in urban areas have been on declining trajectories relative to rural areas with respect to output and factor inputs over time.

Our results are robust to alternative definitions of the degree of urbanization. Table A2 in the appendix presents two further proxies to measure urban and rural districts. On top of our preferred measure based on median population density, we further use the median urbanization rate based on the household-level survey *Susenas*, which administratively assigns rural or urban status to residential localities. Like for population density, we split districts at the 90th percentile. As a third measure, we simply split districts by name, where *kotamadya* are assumed to be urban areas. The two alternative measures reproduce our main results consistently. Output effects remain statistically insignificant, while urban firms suffer from productivity declines (mainly capital pro-

Table 2.1: Effect of temperature on output, productivity and input factors

	(1)		(2)		(3)		(4)	
Split by pop. density:	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban
Panel A: Log output								
< 20°C	−0.0020	0.0023	−0.0020*	−0.0007	−0.0017*	−0.0045	−0.0013	0.0031
	(0.0012)	(0.0155)	(0.0012)	(0.0152)	(0.0010)	(0.0115)	(0.0009)	(0.0115)
26-27°C	0.0013**	−0.0006	0.0017***	−0.0002	0.0011**	−0.0012	0.0007	−0.0001
	(0.0006)	(0.0008)	(0.0006)	(0.0009)	(0.0005)	(0.0008)	(0.0005)	(0.0007)
> 27°C	0.0006	−0.0003	0.0011	−0.0000	0.0007	−0.0010	0.0003	0.0002
	(0.0006)	(0.0009)	(0.0007)	(0.0009)	(0.0006)	(0.0008)	(0.0006)	(0.0007)
Panel B: Log TFP								
< 20°C	−0.0029**	−0.0026	−0.0028**	−0.0056	−0.0020**	−0.0098	−0.0018*	−0.0059
	(0.0014)	(0.0128)	(0.0013)	(0.0128)	(0.0010)	(0.0104)	(0.0009)	(0.0104)
26-27°C	0.0004	−0.0010	0.0006	−0.0009	0.0003	−0.0017**	0.0002	−0.0012*
	(0.0006)	(0.0008)	(0.0007)	(0.0008)	(0.0005)	(0.0007)	(0.0005)	(0.0007)
> 27°C	0.0000	−0.0007	0.0001	−0.0006	0.0001	−0.0015*	−0.0001	−0.0009
	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0006)	(0.0008)	(0.0006)	(0.0007)
Panel C: Log value added per worker								
< 20°C	−0.0026*	0.0019	−0.0025*	−0.0010	−0.0018*	−0.0098	−0.0017*	−0.0077
	(0.0014)	(0.0121)	(0.0013)	(0.0122)	(0.0010)	(0.0096)	(0.0010)	(0.0095)
26-27°C	0.0004	−0.0007	0.0006	−0.0005	0.0001	−0.0015**	−0.0000	−0.0012
	(0.0006)	(0.0008)	(0.0007)	(0.0008)	(0.0005)	(0.0007)	(0.0005)	(0.0007)
> 27°C	−0.0000	−0.0004	0.0001	−0.0003	−0.0002	−0.0012	−0.0004	−0.0009
	(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0008)	(0.0007)	(0.0008)
Panel D: Log value added per capital								
< 20°C	−0.0016**	0.0014	−0.0015**	−0.0004	−0.0011	−0.0105	−0.0010	−0.0087
	(0.0008)	(0.0100)	(0.0007)	(0.0100)	(0.0007)	(0.0078)	(0.0007)	(0.0079)
26-27°C	0.0006	−0.0004	0.0004	−0.0006	0.0000	−0.0018***	−0.0001	−0.0015**
	(0.0005)	(0.0006)	(0.0005)	(0.0007)	(0.0005)	(0.0006)	(0.0004)	(0.0006)
> 27°C	0.0004	−0.0004	−0.0000	−0.0009	−0.0003	−0.0019***	−0.0004	−0.0016**
	(0.0005)	(0.0006)	(0.0006)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0006)
Panel E: Log labor								
< 20°C	0.0006	0.0012	0.0006	0.0006	0.0002	−0.0001	0.0004	0.0049
	(0.0005)	(0.0040)	(0.0005)	(0.0042)	(0.0004)	(0.0040)	(0.0004)	(0.0031)
26-27°C	0.0006***	−0.0003	0.0007***	−0.0001	0.0007***	−0.0003	0.0004**	0.0005*
	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0004)	(0.0002)	(0.0003)
> 27°C	0.0008***	−0.0001	0.0009***	0.0000	0.0008***	−0.0002	0.0006**	0.0006**
	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0003)
Panel F: Log capital								
< 20°C	0.0011	−0.0071	0.0011	−0.0079	0.0001	−0.0024	0.0003	0.0012
	(0.0009)	(0.0103)	(0.0009)	(0.0105)	(0.0009)	(0.0092)	(0.0009)	(0.0084)
26-27°C	−0.0001	−0.0002	0.0004	0.0003	0.0008	0.0011	0.0006	0.0016*
	(0.0006)	(0.0009)	(0.0007)	(0.0010)	(0.0006)	(0.0009)	(0.0006)	(0.0009)
> 27°C	−0.0002	−0.0000	0.0004	0.0005	0.0008	0.0012	0.0006	0.0018*
	(0.0006)	(0.0010)	(0.0008)	(0.0011)	(0.0007)	(0.0010)	(0.0007)	(0.0009)
Weather controls	Yes		Yes		Yes		Yes	
Firm FE	Yes		Yes		Yes		Yes	
Year FE	Yes							
Island-year FE			Yes		Yes		Yes	
Product-year FE					Yes		Yes	
$\mathbf{Urban}_d$-specific trends							Yes	
Observations	318,675		318,675		318,675		318,675	

Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

ductivity). At the same time, all firms adjust to climate change by increasing labor and capital input. We are thus confident that our results do not hinge on the definition of urbanized localities.

2.7.2 Electricity as a channel of adaptation

Electricity usage

Electricity input is a potential key factor in the adjustment process to climate change and was repeatedly investigated in the literature (cf. Fisher-Vanden et al. 2015, on Chinese firms). In particular, firms may be able to avoid negative effects of higher temperatures by an increased demand for electricity to power air conditioners for their labor force, or cooling devices for the machinery. Our firm data includes rich information on the energy use patterns of firms. We exploit this information to additionally control for a firm's quantity of total used electricity (in kWh) in equation (2.4). If electricity was an important mechanism to alleviate climate effects on firm outcomes, we would expect that including electricity as control would change the impact patterns of our main results.

We report our findings in table 2.2. Panel A shows results for the baseline sample and is directly comparable to results in table 2.1. Estimations in panel B are based on a reduced sample which includes only observations with positive (non-zero) electricity input. This restriction has a stronger focus on the estimation of the intensive margin, whereas panel A also includes the extensive margin.[20] For both samples, controlling for electricity input in columns 1 to 6 of table 2.2 returns qualitatively similar point estimates of temperature bins like in our preferred specification in table 2.1. Quantitatively, the productivity effects are slightly larger in the non-zero electricity sample in panel B, while the effect on labor input is not statistically significant anymore in urban areas. In general, these findings show that firms neither use electricity to alleviate negative productivity effects in response to more heat days, nor substitute labor or capital inputs with electricity. At the same time, electricity is positively associated with output, productivity and other factor inputs. Interestingly, labor productivity reacts more strongly to electricity as compared to capital productivity. Labor and capital inputs both seem to be complements to electricity use. In the baseline sample in panel A, urban firms seem to be more sensitive to electricity input since the coefficients are twice as large compared to rural areas. In the restricted sample in panel B, however, the marginal effect of additional electricity use on all firm outcomes is slightly higher and very similar across locations. This suggests that the adjustment takes primarily place at the intensive margin.

In a next step, we look at the direct electricity input response of firms to additional heat days by using either a firm's electricity quantity or spending as the dependent

[20] At the same time, zero electricity input may also be an indicator for misreporting, since it is highly unlikely for medium-sized (and large) manufacturing firms to be off the grid. By that, we also test the robustness of our main results.

Table 2.2: Electricity as additional control and outcome

Dependent variable:	log Y	log TFP	log VAD/L	log VAD/K	log L	log K	log E_q	log E_{value}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline sample								
Rural								
× < 20°C	−0.0014	−0.0019**	−0.0018*	−0.0010	0.0003	0.0002	0.0026	0.0018
	(0.0009)	(0.0009)	(0.0010)	(0.0007)	(0.0004)	(0.0009)	(0.0020)	(0.0019)
× 26-27°C	0.0007	0.0002	−0.0000	−0.0001	0.0004**	0.0006	−0.0009	−0.0007
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0002)	(0.0006)	(0.0015)	(0.0013)
× > 27°C	0.0002	−0.0002	−0.0004	−0.0005	0.0005**	0.0006	0.0004	0.0006
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0002)	(0.0007)	(0.0018)	(0.0015)
× log Electricity	0.0842***	0.0469***	0.0437***	0.0191***	0.0246***	0.0374***		
	(0.0024)	(0.0017)	(0.0017)	(0.0016)	(0.0010)	(0.0020)		
Urban								
× < 20°C	0.0081	−0.0031	−0.0056	−0.0072	0.0062**	0.0022	−0.0179	−0.0108
	(0.0108)	(0.0101)	(0.0092)	(0.0078)	(0.0030)	(0.0084)	(0.0153)	(0.0136)
× 26-27°C	0.0001	−0.0011	−0.0011	−0.0014**	0.0005*	0.0016*	−0.0005	−0.0006
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0003)	(0.0008)	(0.0016)	(0.0014)
× > 27°C	0.0003	−0.0008	−0.0008	−0.0016**	0.0006**	0.0018*	0.0002	−0.0001
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0003)	(0.0009)	(0.0017)	(0.0015)
× log Electricity	0.1514***	0.0803***	0.0701***	0.0403***	0.0421***	0.0451***		
	(0.0064)	(0.0045)	(0.0042)	(0.0043)	(0.0027)	(0.0049)		
Observations	318,675	318,675	318,675	318,675	318,675	318,675	318,675	318,675
Panel B: Non-zero electricity sample								
Rural								
× < 20°C	−0.0011	−0.0014*	−0.0013	−0.0009	0.0004	0.0006	−0.0023*	−0.0022*
	(0.0008)	(0.0009)	(0.0009)	(0.0007)	(0.0004)	(0.0008)	(0.0013)	(0.0012)
× 26-27°C	0.0002	−0.0001	−0.0003	−0.0001	0.0003	0.0002	0.0022**	0.0016**
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0002)	(0.0006)	(0.0009)	(0.0007)
× > 27°C	−0.0001	−0.0006	−0.0008	−0.0005	0.0005*	0.0002	0.0013	0.0012
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0002)	(0.0007)	(0.0012)	(0.0008)
× log Electricity	0.2207***	0.1288***	0.1147***	0.0679***	0.0598***	0.0664***		
	(0.0047)	(0.0033)	(0.0032)	(0.0029)	(0.0022)	(0.0035)		
Urban								
× < 20°C	0.0067	−0.0038	−0.0062	−0.0068	0.0055*	0.0008	−0.0102	−0.0070
	(0.0099)	(0.0092)	(0.0085)	(0.0072)	(0.0030)	(0.0085)	(0.0167)	(0.0154)
× 26-27°C	−0.0002	−0.0014**	−0.0013**	−0.0015**	0.0003	0.0014*	0.0006	−0.0003
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0003)	(0.0008)	(0.0011)	(0.0010)
× > 27°C	−0.0000	−0.0012*	−0.0012*	−0.0017**	0.0005	0.0015	0.0014	0.0001
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0003)	(0.0009)	(0.0012)	(0.0011)
× log Electricity	0.2526***	0.1372***	0.1196***	0.0716***	0.0670***	0.0678***		
	(0.0076)	(0.0057)	(0.0054)	(0.0055)	(0.0038)	(0.0065)		
Observations	293,362	293,362	293,362	293,362	293,362	293,362	293,362	293,362
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban$_d$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variables are log output, log TFP, log value added per worker, log value added per capital, log labor, log capital and log electricity quantity or spending. Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). Columns 1 to 6 add log electricity quantity as control variable. For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

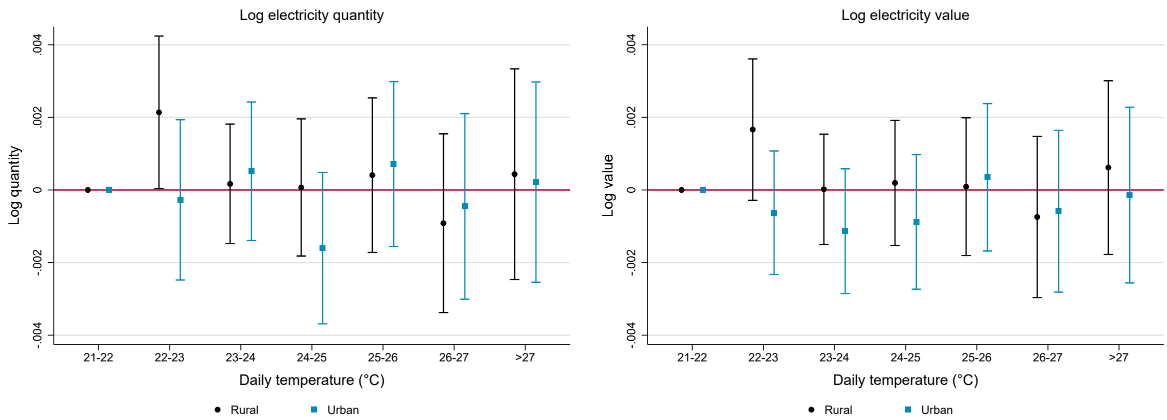
variables. Figure 2.6 presents the full set of coefficients, where panel A (panel B) shows point estimates and confidence intervals of the temperature-electricity relationship for the baseline (non-zero electricity) sample.[21]

However, results for the baseline sample in panel A do not yield any conclusive insight. Neither rural nor urban firms show clear patterns of adjustment of their electricity use

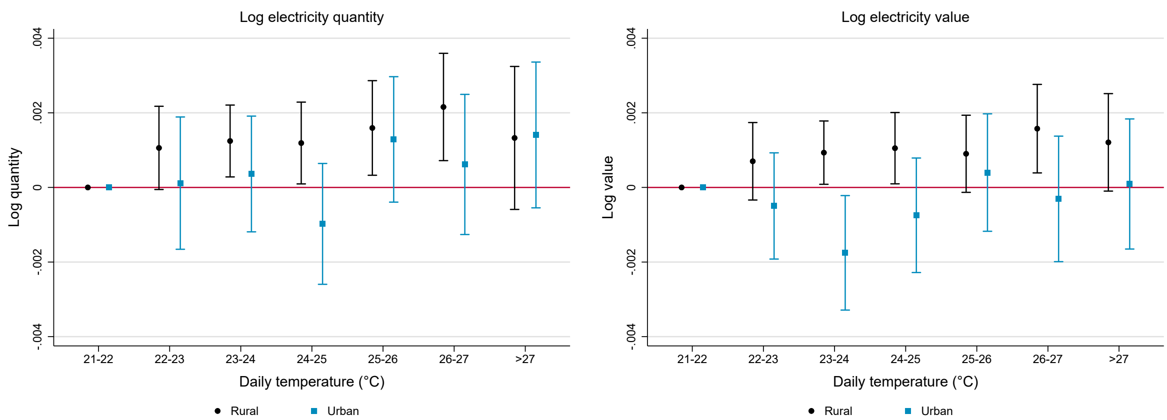
[21] For completeness, columns 7 and 8 of table 2.2 show the same coefficients in table format, where panel A (panel B) again show results for the baseline (non-zero electricity) sample.

Figure 2.6: Effect of temperature on electricity use and spending

Panel A: Baseline sample



Panel B: Non-zero electricity sample



Note: Panels show the estimated impact of temperature on log electricity use (in kWh) or log electricity spending in the baseline sample (panel A), or the non-zero electricity sample (panel B). Figures show point estimates and the associated 90% confidence intervals as bars. Regressions are specified according to equation (2.4) and control for firm, island-year and product-year fixed effects. Standard errors are clustered on firm and district-year level. 21-22°C is omitted.

in reaction to additional heat days. When excluding firms that report zero electricity consumption in panel B, our results change substantially. We find evidence for an increase in electricity quantity and spending among rural firms. For urban firms, the pattern is less obvious. At extreme temperature we find similar coefficients compared to rural areas for electricity quantity. This pattern, however, does not persist for more moderate temperature bins and also vanishes for the value of electricity. Figure 2.6 thus suggests that the positive effects among rural firms in panel B are driven by the intensive margin. For these firms, electricity input adjustment seems to be a relevant coping mechanism to deal with the consequences of climate change. The fact that we cannot detect an electricity channel in urban areas may hint at excess electricity demand in these localities. This reveals the potential problem of electricity shortages in Indonesia. However, this is only suggestive and a more careful analysis would be needed to uncover more details on this mechanism.

Heterogeneity by electricity intensity and generator use

We further check for potential heterogeneities with respect to electricity intensity and generator use of manufacturing firms. Fisher-Vanden et al. (2015) show that electricity shortages in China can have substantial productivity effects and may lead to a reallocation of factor inputs in the production process. Our measure for the intensity of electricity use is the ratio of average spending on electricity and the firm's value added (Roy and Yasar 2015). We define firms as low (high) intensity users if they are below (above) median of the yearly electricity intensity distribution in each year. Firms that switch between low and high electricity intensity are sorted into the medium category.[22] As air conditioning is highly dependent on electricity, firms that have relatively higher expenses for electrical power may also have better opportunities to react to particularly hot days. Results are shown in table 2.3.

We find that the negative output reaction to hot days is indeed the strongest among low-intensity users of electricity in urban areas. Even though the coefficients are not statistically significant, they are in stark contrast to the positive interaction estimates for high-intensity users. This pattern is confirmed in columns 2 to 4 for firm productivity. Again, under-electrified firms in urban areas are most exposed to negative productivity shocks due to heat. However, the effect is mostly driven by declines in labor productivity which is in line with missing air conditioning at workplaces with low electricity consumption. In contrast, highly-electrified firms in rural areas even experience a strong productivity boost. This finding is in line with our results on electricity usage in figure 2.6 which shows that for rural firms electricity input adjustment is an important margin of adaptation to a higher number of heat days (while the same does not hold for urban firms). There are also quite large negative effects among urban medium-electrified firms. Since this category also captures firms in transition (potentially from low to high electricity intensity), this may mirror the short-run costs of investment into the capital stock (and thus machinery). We find supporting evidence for the latter result in column 6. Firms in transition in fact exhibit the largest estimates on capital stock adjustment. Unfortunately, our data does not allow for a deeper analysis to further pin down this channel.

An alternative way to assess the extent of electricity use within a firm is to distinguish between plants with and without own power generator. Firms that report to have a

[22] On average, about 60% of firms in our sample are *medium E*, while about 20% are sorted into *low/high E* each. The share of *high E* is slightly larger among urban firms.

Table 2.3: Effect of temperature on output, productivity and input factors by electricity intensity

Dependent variable:	Log Y	Log TFP	Log VAD/L	Log VAD/K	Log L	Log K
	(1)	(2)	(3)	(4)	(5)	(6)
Rural × low E						
× < 20°C	0.0030	0.0016	0.0026	0.0010	−0.0006	−0.0000
	(0.0021)	(0.0019)	(0.0021)	(0.0019)	(0.0008)	(0.0016)
× 26-27°C	0.0006	−0.0001	−0.0001	−0.0004	0.0003	0.0009
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0003)	(0.0008)
× > 27°C	0.0003	−0.0001	−0.0002	−0.0005	0.0004	0.0008
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0009)
Rural × medium E						
× < 20°C	−0.0005	−0.0011	−0.0010	−0.0010	0.0004	0.0009
	(0.0010)	(0.0008)	(0.0008)	(0.0006)	(0.0006)	(0.0010)
× 26-27°C	0.0004	−0.0002	−0.0004	−0.0003	0.0005**	0.0008
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0002)	(0.0006)
× > 27°C	−0.0002	−0.0007	−0.0009	−0.0008*	0.0006**	0.0010
	(0.0006)	(0.0007)	(0.0007)	(0.0005)	(0.0003)	(0.0007)
Rural × high E						
× < 20°C	−0.0035**	−0.0038***	−0.0039***	−0.0014	0.0005	−0.0005
	(0.0015)	(0.0015)	(0.0015)	(0.0010)	(0.0004)	(0.0012)
× 26-27°C	0.0026***	0.0028***	0.0025**	0.0015**	0.0003	−0.0004
	(0.0009)	(0.0010)	(0.0010)	(0.0007)	(0.0003)	(0.0009)
× > 27°C	0.0026***	0.0024**	0.0021**	0.0012	0.0003	−0.0006
	(0.0009)	(0.0010)	(0.0010)	(0.0007)	(0.0003)	(0.0010)
Urban × low E						
× < 20°C	0.0030	−0.0171	−0.0164	−0.0060	0.0061	−0.0057
	(0.0204)	(0.0195)	(0.0184)	(0.0184)	(0.0104)	(0.0182)
× 26-27°C	−0.0010	−0.0029*	−0.0033**	−0.0021	0.0007	0.0005
	(0.0017)	(0.0015)	(0.0014)	(0.0022)	(0.0008)	(0.0024)
× > 27°C	−0.0015	−0.0033**	−0.0036**	−0.0023	0.0007	−0.0002
	(0.0017)	(0.0016)	(0.0015)	(0.0023)	(0.0008)	(0.0025)
Urban × medium E						
× < 20°C	−0.0018	−0.0066	−0.0063	−0.0149*	0.0020	0.0101
	(0.0108)	(0.0108)	(0.0103)	(0.0076)	(0.0024)	(0.0085)
× 26-27°C	−0.0001	−0.0011	−0.0010	−0.0020***	0.0005*	0.0027***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0003)	(0.0009)
× > 27°C	0.0001	−0.0007	−0.0007	−0.0022***	0.0006**	0.0030***
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0009)
Urban × high E						
× < 20°C	0.0249	−0.0001	−0.0110	0.0166	0.0166	−0.0349**
	(0.0275)	(0.0176)	(0.0135)	(0.0181)	(0.0140)	(0.0165)
× 26-27°C	0.0003	−0.0007	−0.0009	0.0004	0.0003	−0.0020
	(0.0011)	(0.0010)	(0.0009)	(0.0008)	(0.0006)	(0.0014)
× > 27°C	0.0011	−0.0004	−0.0008	0.0005	0.0007	−0.0015
	(0.0011)	(0.0010)	(0.0010)	(0.0009)	(0.0006)	(0.0015)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban$_d$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,675	318,675	318,675	318,675	318,675	318,675

Note: The table splits the sample by low, medium and high electricity intensity. The dependent variables are log output, log TFP, log value added per worker, log value added per capital, log labor and log capital. Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

generator in at least one sample year are sorted in the category *has generator*.[23] Our expectation would be that having an in-house power generating unit makes a firm more independent from external power supply and thus may enable a better and more reliable operation of cooling devices. This may be especially relevant in areas of Indonesia with weak electrical grid. However, table A3 in the appendix shows no evidence for a higher capability to cope with the consequences of climate change among firms with generators. On the contrary, firms with no generator even exhibit a positive output response to heat while firms with generators are negatively (but insignificantly) affected. At the same time, urban firms with generators suffer from larger capital productivity losses. This can be mechanically explained by relatively larger capital input in these firms (and no labor response) due to heat. Interestingly, urban firms without generator do not only increase capital stocks, but additionally adjust by increasing employment. Not having an own generator may provide these firms with the flexibility to use all margins of factor adjustment, while plants with generators seem to purely rely on additional investments.

2.7.3 Further heterogeneities

Geography

Appendix table A4 splits the sample by the five major islands of Indonesia. Note that the number of observations varies substantially across the regional sub-samples with Java inhabiting the vast majority of manufacturing firms. It is not surprising, that results for Java in specification 2 more or less reproduce our main results for whole Indonesia. On the other hand, this delivers another piece of support of our identification strategy. Extreme weather events due to climate change may also result from accelerating deforestation in Indonesia. Sumatra and Kalimantan experienced massive deforestation over the same time period, in particular to boost local palm oil production (Cisneros et al. 2021). If local manufacturing firms adapt to palm oil expansion, the temperature measure may endogenously pick up this variation. However, deforestation is less of an issue on Java. We thus consider any deforestation-related effects on climate change to be quasi exogenous to firms in Java. Therefore, our firm-level results are unlikely to be driven by endogenous adaptation to palm oil related deforestation. On the contrary, most coefficients for the remaining regions are insignificant, most likely due to a small sample size. Some islands also do not have any urban (or in one case rural) districts with average daily temperature in particular bins.

[23] On average, 57% of all firms report to have *no generator*. The share is slightly higher (60%) among urban firms.

Sector

Figures A4 and A5 in the appendix additionally depict the main results by two-digit sectors. We only present results for the two highest temperature bins since the number of observations in the lowest bin (<20°C) is very small in most sectors (especially in urban areas) which leads to extremely large confidence bands.[24]

We detect some effect heterogeneity across sectors. For rural firms (figure A4), especially tobacco firms, experience strong output losses driven by both productivity and labor input reductions. In contrast, some sectors also manage to raise their output. For most sectors, these gains manifest due to higher factor inputs (either labor or capital), with the exception of chemicals where we observe pronounced productivity increases. Among urban firms (figure A5), there are also winners and losers in terms of output. Food, wood, transportation and furniture (and n.e.c.) experience the largest reductions in output due to heat days. In all of these industries, the effect is driven by productivity declines despite increases in factor inputs (e.g., capital investments by transportation firms). Contrary to this, chemicals and electronics seem to benefit from higher temperatures. However, the adjustment mechanism is quite distinct: while chemical firms realize their output gains by boosting productivity, electronic enterprises mainly increase labor to cope with the challenges of climate change.

Labor intensity

We continue our heterogeneity analysis by distinguishing between capital- and labor-intensive firms. We define labor intensity (according to input costs) as the ratio of the average industrial wage bill and capital stock. Labor-intensive industries exhibit above-median labor intensity. Table 2.4 displays the results separately for urban and rural districts. Among capital-intensive firms, the effect of heat on output in column 1 is positive and larger compared to labor-intensive firms (and at least in one case also statistically significant). In terms of TFP and labor productivity, columns 2 and 3 indicate that the negative effect of temperatures below 20°C entirely originates in labor-intensive industries. Capital productivity, however, does not significantly change with one additional cold day. The negative productivity effects of hot days is more pronounced in labor-intensive firms. In particular, the marginal effect of one additional heat day on capital productivity is large and highly significant among labor-intensive firms in urban areas. At the same time, urban firms still suffer more in terms of productivity as compared to their rural counterparts. There are no striking differences between capital- and labor-intensive firms with respect to the effect on factor inputs.

[24] Due to the low total number of firms in some sectors, we also completely omit the sectors coke and refined petroleum (567 obs in 128 firms), basic metals (2,830 obs in 478 firms) radio and television (2,007 obs in 447 firms), as well as medical and optical instruments (711 obs in 129 firms).

Table 2.4: Effect of temperature on output, productivity and input factors by labor intensity

Dependent variable:	Log Y	Log TFP	Log VAD/L	Log VAD/K	Log L	Log K
	(1)	(2)	(3)	(4)	(5)	(6)
Rural × capital intensive						
× < 20°C	−0.0002	−0.0005	−0.0004	−0.0003	0.0003	0.0005
	(0.0010)	(0.0010)	(0.0011)	(0.0008)	(0.0007)	(0.0014)
× 26-27°C	0.0012**	0.0006	0.0004	0.0003	0.0006**	0.0003
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0002)	(0.0006)
× > 27°C	0.0005	0.0002	0.0000	−0.0003	0.0006**	0.0007
	(0.0006)	(0.0007)	(0.0007)	(0.0005)	(0.0003)	(0.0007)
Rural × labor intensive						
× < 20°C	−0.0017	−0.0022**	−0.0022**	−0.0012	0.0005	0.0002
	(0.0011)	(0.0010)	(0.0011)	(0.0008)	(0.0004)	(0.0009)
× 26-27°C	0.0002	−0.0002	−0.0004	−0.0004	0.0003	0.0006
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0002)	(0.0007)
× > 27°C	0.0001	−0.0004	−0.0007	−0.0006	0.0005**	0.0003
	(0.0006)	(0.0007)	(0.0007)	(0.0006)	(0.0002)	(0.0008)
Urban × capital intensive						
× < 20°C	0.0155	0.0116	0.0092	−0.0072	0.0048*	0.0108
	(0.0139)	(0.0102)	(0.0098)	(0.0089)	(0.0029)	(0.0111)
× 26-27°C	0.0004	−0.0008	−0.0008	−0.0012*	0.0008***	0.0018*
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0010)
× > 27°C	0.0007	−0.0004	−0.0003	−0.0010	0.0006*	0.0015
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0010)
Urban × labor intensive						
× < 20°C	−0.0067	−0.0186	−0.0207*	−0.0105	0.0046	−0.0058
	(0.0111)	(0.0114)	(0.0107)	(0.0089)	(0.0041)	(0.0094)
× 26-27°C	−0.0003	−0.0012*	−0.0012	−0.0015**	0.0003	0.0015*
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0003)	(0.0009)
× > 27°C	0.0001	−0.0010	−0.0011	−0.0019***	0.0007**	0.0020**
	(0.0007)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0010)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban$_d$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,675	318,675	318,675	318,675	318,675	318,675

Note: The table splits the sample by labor and capital intensive industries. The dependent variables are log output, log TFP, log value added per worker, log value added per capital, log labor and log capital. Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

Labor adjustment remains an important channel for all firms, while capital increases are more strongly observed in urban areas.

Technology

We further split the sample according to the technological content and requirements of industries in table 2.5. For that purpose, we use the sectoral distinctions by the OECD (2003). Low-tech sectors are for example food or textiles, while high-tech industries comprise machinery or chemicals. There is some evidence that high-tech firms are able to realize little output gains in column 1, while low-tech firms do not show any response to temperature. At the same time, low-tech firms in urban areas experience

Table 2.5: Effect of temperature on output, productivity and input factors by technology intensity

Dependent variable:	Log Y	Log TFP	Log VAD/L	Log VAD/K	Log L	Log K
	(1)	(2)	(3)	(4)	(5)	(6)
Rural × low-tech						
× < 20°C	−0.0010	−0.0016*	−0.0016	−0.0008	0.0006	0.0002
	(0.0010)	(0.0009)	(0.0010)	(0.0006)	(0.0005)	(0.0010)
× 26-27°C	0.0005	−0.0000	−0.0001	−0.0002	0.0003	0.0007
	(0.0005)	(0.0005)	(0.0006)	(0.0005)	(0.0002)	(0.0006)
× > 27°C	0.0001	−0.0002	−0.0004	−0.0005	0.0005*	0.0006
	(0.0006)	(0.0006)	(0.0007)	(0.0005)	(0.0002)	(0.0007)
Rural × high-tech						
× < 20°C	−0.0022	−0.0026*	−0.0020	−0.0015	−0.0005	0.0010
	(0.0016)	(0.0015)	(0.0016)	(0.0012)	(0.0005)	(0.0012)
× 26-27°C	0.0012*	0.0006	0.0001	0.0003	0.0007***	0.0001
	(0.0006)	(0.0007)	(0.0007)	(0.0006)	(0.0002)	(0.0008)
× > 27°C	0.0006	−0.0001	−0.0005	−0.0003	0.0008***	0.0003
	(0.0007)	(0.0008)	(0.0008)	(0.0006)	(0.0003)	(0.0008)
Urban × low-tech						
× < 20°C	−0.0053	−0.0144	−0.0153	−0.0127	0.0010	−0.0030
	(0.0124)	(0.0111)	(0.0104)	(0.0089)	(0.0032)	(0.0080)
× 26-27°C	−0.0003	−0.0013*	−0.0013*	−0.0016**	0.0004	0.0015*
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0003)	(0.0009)
× > 27°C	0.0001	−0.0008	−0.0007	−0.0017**	0.0005*	0.0019**
	(0.0007)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0010)
Urban × high-tech						
× < 20°C	0.0284**	0.0245**	0.0154	0.0035	0.0161***	0.0136
	(0.0134)	(0.0122)	(0.0123)	(0.0122)	(0.0061)	(0.0166)
× 26-27°C	0.0005	−0.0007	−0.0008	−0.0013*	0.0007**	0.0015
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0010)
× > 27°C	0.0006	−0.0010	−0.0012	−0.0014*	0.0008**	0.0014
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0003)	(0.0011)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban$_d$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,675	318,675	318,675	318,675	318,675	318,675

Note: The table splits the sample by low and high technology industries. The dependent variables are log output, log TFP, log value added per worker, log value added per capital, log labor and log capital. Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

larger productivity drops compared to high-tech firms due to extremely low or high temperatures in columns 2 to 4. Beyond the effect on capital productivity, we also find significant effects on TFP and labor productivity among this sub-group. In both urban and rural firms, we observe differential factor input adjustments. Surprisingly, high-tech firms more strongly react to heat by hiring additional labor, while the marginal effect on capital investment is slightly larger (and more precisely estimated) among low-tech firms. Even though the difference between coefficients is not statistically significant, this can be interpreted as suggestive evidence for differential coping strategies with respect to technology intensity.

2.7.4 Effect stability over time

As a robustness check, we finally test whether our main results are driven by the structural break in the data due to changes in survey coverage in 2006. Figures 2.4 and A1 depict a visible break for the main outcomes and the sample size in this year. We therefore split our sample into a pre and post 2006 period (omitting 2006 completely) and report the graphical results in appendix figure A6. The right graphs show results for the pre-2006 period whereas left graphs depict the post-2006 years.

All panels of figure A6 show that results in the pre-2006 period are more precisely estimated. In panel A, we see small output gains due to additional heat days in the first half of our sample period among all firms. In contrast, losses in output due to extreme temperatures are mainly driven by the post-2006 period. Similarly, panels B and C depict negative productivity effects only for the later period and barely any response for the time between 1993 and 2005. We observe a significantly negative response of capital productivity (panel D) already in the years before 2006 which then intensifies in the later period. Panels E and F further show that the full input factor adjustment takes place in the first half of the sample period, while neither labor nor capital seems to play any role in the post-2006 firms' coping strategies to manage the consequences of climate change. Electricity use also adjusts only between 1993 and 2005, with some evidence for increased quantity even among urban firms. The effect vanishes after 2006.

Importantly, figure A6 shows that our main results do not hinge at the structural break in 2006 as we completely omit observations from 2006 in these samples. However, it also shows that our results are mostly driven by the pre-2006 period. Second, this suggests that the absence of strong productivity effects for the early years may be explained by firms successfully compensating for higher temperatures by increasing factor inputs as well as a higher demand for electricity to power air conditioning. Output and productivity drops are only realized when firms do not adjust their input factor to heat in the post-2006 period (except for capital productivity). Even though we cannot causally link these two findings and coefficients are imprecisely estimated, the narrative makes sense intuitively.

2.8 Conclusion

This paper estimates the impacts of temperature changes on manufacturing outcomes in Indonesia. Using firm-level data for over more than 20 years, we find that extremely cold and warm temperatures lead to productivity declines, mainly manifesting in capital productivity drops. This effect is most pronounced in urban locations, which is in line with the stylized fact of urban heat islands. In terms of our theoretical expectations, we are able to confirm the channel of impeded operation of machinery at elevated temperature but, on average, do not find strong evidence of productivity drops among workers at temperatures outside the zone of thermal comfort. At the same time, our results suggest that factor re-allocation is an important channel to attenuate any negative output effects. Our heterogeneity analysis further reveals that negative effects are mostly concentrated among firms with low or medium electricity intensity. Interestingly, productivity drops in under-electrified firms are mainly driven by labor productivity reductions. This points towards these firms being incapable of providing adequate cooling at the workplace.

Given the finding that firm output is on average unaffected by heat, our study's key insight is that manufacturing enterprises in Indonesia successfully cope with the consequences of global warming by increasing their labor and capital inputs. They are thus able to compensate for temperature induced productivity drops. Similarly, a high electricity intensity alleviates the negative productivity effects of heat days. This result is in contrast to the existing literature that finds an overall negative impact of high temperature on firm output in China (Zhang et al. 2018) or India (Somanathan et al. 2021). One reason may be that our study only exploits rather small temperature variation due to Indonesia's geographic location in one single climate zone. At the same time, the evidence raises hope for other countries in the tropical zone that the direct impact of heat due to climate change may not hit firms as severely as predicted by previous studies.

However, the question of how firms' coping strategies and input factor adjustment affect global warming in the long-run remains open. If adaption to higher temperatures requires a higher degree of electrification in a country, the resulting increased energy demand itself may accelerate climate change. More research is needed in this field for a better understanding of the ongoing firm dynamics and their consequences for the environment.

CHAPTER 3

What happens to FDI spillovers when input-output tables go granular?

Robert Genthner[25]

Abstract

Multinational enterprises affect the productivity of domestic firms through FDI spillovers, especially when these firms use similar technology. The impact of spillovers varies with the technological distance between industries. More granular measurement of trade linkages across industries allows for the estimation of an additional intra-sectoral vertical component within two-digit sectors, which was part of the aggregated horizontal spillover effect before. Using Indonesian firm data reveals substantial effect heterogeneity. Horizontal spillovers within the same three-digit industry are negative, while intra-sectoral vertical spillovers across industries are positive and large in magnitude.

[25] The note is forthcoming in *Economics Bulletin*. I would like to thank John P. Conley, Anna Gasten, Krisztina Kis-Katos, Kerstin Unfried, Feicheng Wang and two anonymous referees for helpful comments and suggestions. All remaining error are my own.

3.1 Introduction

Foreign direct investment (FDI) is an essential element in the expansion of international market coverage among multinational enterprises (MNEs). However, only the most productive firms decide to engage in FDI, while less productive companies still rely on exports or focus solely on the domestic market (Melitz 2003). The positive selection of the most successful enterprises also affects the target economy by importing advanced technologies and other entrepreneurial skills, which can in turn be adopted by domestic firms. In terms of productivity, the literature finds positive direct effects of FDI among manufacturing plants (Javorcik and Poelhekke 2017). Earlier studies have identified only insignificant horizontal spillovers on firms in the same sector (Javorcik 2004, Blalock and Gertler 2008), while more recent studies have found a negative impact (Lu et al. 2017). Looking for vertical spillovers across industries, there is evidence of a sizable positive influence of FDI on upstream industries (backward spillovers), whereas the impact on downstream industries (forward spillovers) is smaller in size and negative (Javorcik 2004, Davies et al. 2016).[26]

Industrial linkages are typically measured using input-output (IO) tables as a proxy for trade between sectors within a country (Javorcik 2004). For a long time, IO tables have been available only on the two-digit sector level, especially for developing countries.[27] However, such aggregated horizontal spillover measures do not distinguish between firms within the same two-digit sector and identify an average effect irrespective of the industrial distance. With increasing granularity of IO tables, it has become feasible to proxy for more complex value chain relationships. In this note, I show that using more disaggregated IO linkages reveals important heterogeneities, especially within aggregated horizontal spillover effects.

The magnitude of productivity spillover effects from technology adjustment are determined by two opposing mechanisms: the MNEs' willingness to share their technology with local firms and the domestic firms' cost of adapting new technology. First, firms in the same three-digit industry are more likely to be direct competitors and thus have a strong incentive to inhibit the diffusion of knowledge within their industry. More productive MNEs may even take away market share from domestic firms in the same industry, resulting in a negative competition effect. In contrast, firms are more willing to share technology with potential suppliers in other three-digit industries, thereby en-

[26] There is a parallel strand of the literature looking at spillovers from aggregate supply and demand shocks along the value chain from a macro perspective (cf. Acemoglu et al. 2015). These studies do not only consider first order effects (directly from upstream/downstream industries) but also higher order effects which manifest through aggregate reallocation and demand effects. As first order effects generally dominate the higher order effects, this note follows the micro-based literature and only considers first order vertical spillovers.

[27] See for instance the world input-output tables (WIOD) (Timmer et al. 2015).

abling domestic firms to improve their productivity. Second, a successful adoption of technology will be facilitated if domestic firms and MNEs operate in the same industry and are more likely to use similar production processes (Fons-Rosen et al. 2017).[28] This results in a stronger impact on firms within the same two-digit sector whereas the effect diminishes with rising costs of technological adaptation. These costs depend on a firm's relative position in the value chain and, thus, are different from the concept of absorptive capacity, which refers to the overall ability to innovate.

Splitting vertical spillovers into groups depending on technological distance will account for potential heterogeneities of cross-industry linkages. Testing this decomposition with Indonesian firm-level data shows that horizontal spillovers within the same industry exhibit the expected negative sign, while spillovers across two-digit sectors turn positive (negative) for backward (forward) linkages. At the same time, backward and forward spillovers across industries within the same two-digit sector are positive and large in magnitude. This supports both more technology sharing among firms which are not in direct competition with one another, and lower adaptation costs for firms with close industrial ties to the MNE. Studies based on aggregated IO tables (like WIOD) mask this heterogeneity and capture intra-sectoral vertical linkages in the aggregated horizontal variable.

3.2 Measuring spillovers

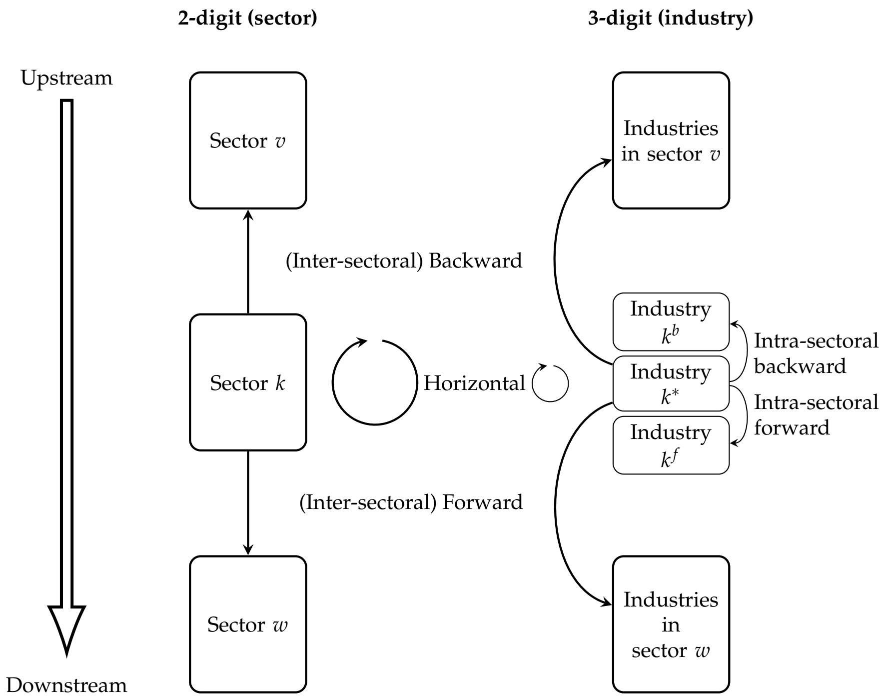
Horizontal spillovers are captured by constructing a measure of FDI presence in an industry k in year t (cf. Javorcik 2004). They are defined as the share of foreign capital within an industry weighted by each firm i's initial sales ($Horizontal_{kt} = \sum_{i \in k} FDI_{it} \times Sales_{i0} \;/\; \sum_{i \in k} Sales_{i0}$). All variation within the spillover variables stems from changes in FDI over time. Vertical FDI spillovers are defined based on the horizontal spillover variable, but use IO tables to account for each sector's input purchases from and output sales to other sectors.[29] FDI spillovers from MNEs in downstream industries v to their domestic suppliers k are $Backward_{kt} = \sum_{v \neq k} \alpha_{kv} \times Horizontal_{vt}$, where α_{kv} is the proportion of industry k's output supplied to industry v. Similarly, FDI spillovers from MNEs in upstream industries w to their domestic customer industries k are defined as $Forward_{kt} = \sum_{w \neq k} \sigma_{wk} \times Horizontal_{wt}$, where σ_{wk} is the proportion of industry k's intermediate inputs purchased from industry w.

Earlier literature used IO tables on the two-digit sector level (cf. Javorcik 2004), while

[28] Fons-Rosen et al. (2017) use global firm data from Orbis and construct a novel measure of "technology closeness" based on US patent data to account for effect heterogeneity in vertical spillovers. Since data quality in developing countries often does not allow for such a comprehensive analysis, my approach offers a similar and more feasible alternative.

[29] See the left side of figure 3.1 for visualization.

Figure 3.1: Spillover effects using different aggregation levels of IO tables



Note: The figure depicts supply chain relationships relative to sector k or industry k^*. Sector v (w) is a representative upstream supplier (downstream consumer) of k. Industry k^b (k^f) is an upstream (downstream) industry of industry k^* within the same two-digit sector k.

more recent studies exploited the increasing granularity of IO tables (cf. Davies et al. 2016). This note distinguishes between 175 domestic industries (comparable to the three-digit industry level), allowing for a new potential layer of heterogeneous spillovers. Figure 3.1 illustrates that switching from two-digit to three-digit IO tables splits up the aggregated horizontal spillover variable into three components. The first component captures linkages within three-digit industry k^* and forms a new horizontal spillover variable. The second component includes backward linkages from industry k^* to industry k^b within the same two-digit sector and is referred to as intra-sectoral backward spillover. Likewise, the third component comprises intra-sectoral forward linkages to industry k^f. For example, manufacture of cement (26411) is in the same three-digit industry like manufacture of lime (26412) and linkages between both will be captured by the horizontal variable.[30] At the same time, manufacture of clay bricks (26322) is still in the same two-digit sector but any spillover will be measured by the intra-sectoral vertical spillover variables. Finally, vertical spillovers across two-digit sectors (e.g. between manufacture of natural fertilizer (24121) and manufacture of wheat flour (15321)) remain identical to the two-digit methodology and will be referred to as inter-sectoral backward or forward linkages in the following.

[30] Product codes refer to KBLI (*Klasifikasi Baku Lapangan Usaha*) sector classification as published by BPS (Indonesian Statistical Office, *Badan Pusat Statistik*).

Using this method of decomposition allows us to explore an additional layer of heterogeneous spillover effects. Two firms within the same three-digit industry may be competitors, trying to prevent technology transfers while potentially even stealing market shares from each other. This will yield an insignificant or even negative productivity spillover effect. For firms operating in distinct industries with larger technological distance, competition becomes less relevant and MNEs have the incentive to share their technology with domestic suppliers to improve the quality of their locally produced intermediate inputs. In this case, technology sharing outweighs the competition effect. Similarly, local downstream firms may benefit from a higher quality of intermediate inputs which are produced and sold to them by MNEs.

However, technology transfers across two-digit sectors may be more difficult since the adoption of new procedures requires certain overlapping in terms of the production process (Fons-Rosen et al. 2017). Such knowledge transfers may be easier between two firms within the same two-digit sector. The costs of adapting new technology increase with industrial distance since the production technology differs more. This mechanism counteracts the positive effect from reduced competition between both firms.

Positive horizontal spillovers within the same three-digit industry underline the importance of low adaptation costs, whereas positive inter-sectoral vertical spillovers (across two-digit sectors) suggest that technology sharing outweighs difficulties of adaptation. Positive intra-sectoral vertical spillovers are in line with both effects since technology adaptation is still feasible at a relatively low cost and MNEs are more willing to share technology along their value chain.

3.3 Results

Using a panel of medium-sized and large Indonesian manufacturing firms over the period 2000-2015, the empirical specification estimates the effect of FDI and its spillovers on firm productivity in first differences:

$$\begin{aligned} \Delta \mathit{ln(TFP)}_{it} = \quad & \beta_1 \times \Delta \mathit{FDI}_{it} + \ \beta_2 \times \Delta \mathit{Horizontal}_{kt} \\ & + \beta_3 \times \Delta \mathit{Intra\text{-}sectoral\ vertical}_{kt} + \ \beta_4 \times \Delta \mathit{(Inter\text{-}sectoral)\ vertical}_{kt} \\ & + \Delta \boldsymbol{X}'_{it}\beta_5 + \ \gamma_{rt} + \psi_s + \varepsilon_{it}. \end{aligned} \tag{3.1}$$

To account for simultaneity bias from a firm's endogenous input choice, I apply an approach suggested by Wooldridge (2009). Total factor productivity (TFP) is separately estimated for each three-digit industry, which allows for varying importance of

Table 3.1: FDI spillover effects on total factor productivity

Dependent variable: Δ ln(TFP)	WIOD	BPS	
	(1)	(2)	(3)
Δ Foreign capital share	−0.015	−0.014	−0.016
	(0.028)	(0.028)	(0.028)
Δ Horizontal	0.247***	0.218***	−0.088***
	(0.048)	(0.049)	(0.022)
Δ Intra-sectoral backward			1.670***
			(0.065)
Δ Intra-sectoral forward			1.176***
			(0.101)
Δ (Inter-sectoral) Backward	0.723***	0.756***	1.314***
	(0.100)	(0.114)	(0.124)
Δ (Inter-sectoral) Forward	0.151	−0.348***	−0.140*
	(0.106)	(0.072)	(0.073)
Basic controls	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Level of aggregation	2-digit	2-digit	3-digit
Observations	172,149	172,149	172,149
Firms	25,535	25,535	25,535
R-squared	0.016	0.016	0.032

Note: The dependent variable is change in ln(total factor productivity) as estimated by Wooldridge (2009). Column 1 uses spillovers based on two-digit IO coefficients from WIOD while columns 2 and 3 use spillovers based on three-digit IO coefficients from BPS. Basic controls include categories of firm age and a public enterprise indicator. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1 percent (***), 5 percent (**) and 10 percent (*).

input factors.[31] $\Delta ln(TFP)_{it}$ then is the growth rate of productivity of firm i in year t. $Horizontal_{kt}$ and $Intra\text{-}sectoral\ vertical_{kt}$ are spillover effects in year t within the same two-digit sector. Those may work either horizontally within the same three-digit industry k, or vertically (backward and forward) along the value chain. $Inter\text{-}sectoral\ vertical_{kt}$ are vertical spillovers across two-digit sectors. $\boldsymbol{X}_{it}$ are additional time-variant controls (firm age and an indicator for state-owned enterprises). γ_{rt} and ψ_s are island-year and two-digit sector fixed effects. Column 1 of table 3.1 replicates findings from previous studies by using vertical spillover variables based on two-digit IO coefficients in 2005 taken from the world input-output database (WIOD) (Timmer et al. 2015). The remaining columns use more granular IO coefficients from the Indonesian statistical office (BPS). Column 2 still aggregates cross-industrial linkages to the sector level for comparison with the WIOD-based estimates, while column 3 splits the horizontal spillover variable into within and across three-digit industry effects to account for heterogeneity.

[31] See Genthner and Kis-Katos (2019) for a detailed description of the data cleaning process and variable generation.

There is no evidence of a direct effect of FDI on firm productivity across all specifications, and the horizontal spillover estimates in columns 1 and 2 show a positive sign. The latter effect is in contrast with studies finding an insignificant or even negative coefficient (Lu et al. 2017). Narrowing the scope of horizontal spillovers to the same three-digit industry in columns 3 reverses the sign of the coefficient, which is in line with the hypothesis that MNEs disproportionately inhibit knowledge transfers to their direct competitors. At the same time, intra-sectoral vertical spillovers are positive and highly significant in column 3. This can be explained by an increase in technology sharing by MNEs and relatively low costs of technology adaptation. The stronger intra-sectoral backward effect suggests that the spillover is slightly more beneficial for domestic suppliers. This is in line with the dominant backward FDI spillover often found in the literature (cf. Fons-Rosen et al. 2017). However, downstream firms also experience productivity gains as they learn from high quality inputs supplied by MNEs which are still within close industrial distance.

Inter-sectoral backward spillovers exhibit the well-known significantly positive impact on firm productivity, irrespective of the level of aggregation (cf. Davies et al. 2016). Column 3 further shows that the inter-sectoral backward estimate is significantly smaller in magnitude compared to its intra-sectoral counterpart. This highlights that knowledge transfer across sectors entails higher cost relative to transfers within the same sector. Finally, inter-sectoral forward FDI spillovers are insignificant when using WIOD-based coefficients, and turn negative in columns 2 and 3. Downstream domestic firms with larger industrial distance to the MNE are not able to realize productivity gains by using its advanced intermediate inputs because of higher adoption costs.[32] Contrary to the backward spillover, the MNE has no incentive to help its customer firms with the technology adoption process (by reducing adoption costs), but may rather prefer to produce the downstream product in-house.

3.4 Conclusion

More granular IO tables allow for a more nuanced estimation of FDI spillovers effects on productivity. This note decomposes aggregated horizontal spillovers into a more narrowly defined horizontal component within three-digit industries and two further intra-sectoral vertical elements. The decomposition reveals an important layer of heterogeneous indirect effects of FDI: while three-digit horizontal spillovers are negative and small, intra-sectoral spillovers are positive and large in magnitude. A potential explanation for this is the interplay of low technology adaptation costs within the same

[32] Upstream MNEs may even have market power which allows them to charge higher prices, thereby increasing costs for domestic customer firms and lowering their productivity.

two-digit sector and the MNEs' increasing willingness to share advanced knowledge with domestic firms when they are not direct competitors. Studies relying on more aggregated IO tables fail to identify the positive intra-sectoral spillover which may be masked by a composition effect. The latter may be insignificant or negative because negative price effects outweigh benefits from lower costs due to technological closeness.

CHAPTER 4

Foreign investment regulation and firm productivity: Granular evidence from Indonesia

Robert Genthner and Krisztina Kis-Katos[33]

Abstract

When regulating foreign direct investment (FDI), countries often face a trade-off between pursuing national policy interests and suffering efficiency losses due to FDI restrictions. We demonstrate the presence of this trade-off in the case of a protectionist FDI policy in Indonesia. Using a yearly census of Indonesian manufacturing firms from 2000 to 2015, we link product-level changes in binding FDI regulation due to major regulatory tightening to changes in firm-level productivity. Controlling for an extensive set of fixed effects as well as potential political economy drivers of regulation, we show that a tightening of the regulatory environment was successful in reducing foreign capital reliance among regulated firms, and led to increases in FDI among non-regulated firms producing the same product. Despite compensating increases in domestic capital, regulated firms experienced relative productivity losses. This points towards either a less efficient allocation of domestic capital or a general inferiority of domestic capital as compared to foreign investments.

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4.1 Introduction

In the course of the last two decades, developing and emerging economies liberalized their markets substantially, dismantling trade barriers and welcoming larger inflows of foreign direct investment (FDI). This global process was accompanied by numerous regulatory shifts and reversals (Harding and Javorcik 2011, Bourlès et al. 2013). The recent global trend of tightening investment regimes has been mostly motivated by concerns for national security and the need of protecting strategic national assets (UNCTAD 2020). When FDI regulation is used to shield strategic domestic industries, this may not only change the firms' access to foreign capital but also the overall competitive environment (Aitken and Harrison 1999, Helpman et al. 2004). Our study adds to the understanding of this phenomenon by investigating the effects of a protectionist policy reform on firm productivity in Indonesia that introduced substantial product- and firm-specific limitations on FDI.

While the positive link between foreign participation and firm productivity has been widely documented in the literature (Aitken and Harrison 1999, Arnold and Javorcik 2009, Javorcik and Poelhekke 2017), the direct effects of FDI regulation on firm outcomes have been less extensively explored. Existing panel studies show a negative link between more aggregate measures of policy restrictiveness and sectoral (Bourlès et al. 2013) or firm productivity (Duggan et al. 2013). Analysing the aftermath of China's accession to the World Trade Organization (WTO), Eppinger and Ma (2019) document positive effects of FDI de-regulation on Chinese firms' productivity. This still leaves the empirical question open, whether the effects of a regulatory tightening are symmetric to those of de-regulation. Our empirical analysis addresses this research gap by analysing the effect of a substantial increase in restrictions on foreign ownership in 2007 on the productivity of Indonesian manufacturing firms. Based on a rich firm panel that spans 16 years, our empirical strategy explores the aftermath of five-digit product-specific[34] regulatory reforms at a high level of granularity.

Foreign capital is expected to affect firm productivity through several channels. It can substitute for domestic capital and relieve liquidity constraints if access to domestic capital is limited. Foreign investors have been shown to introduce non-tangible productive assets such as technological advancements, managerial abilities, marketing skills, trading contacts and improved reputation (Aitken and Harrison 1999, Arnold and Javorcik 2009, Javorcik and Poelhekke 2017). As a result, firms with foreign par-

[34] The Indonesian Statistical Office (BPS, *Badan Pusat Statistik*) classifies sectors according to KBLI (*Klasifikasi Baku Lapangan Usaha*). It is equivalent to the United Nation's International Standard Industrial Classification of All Economic Activities (ISIC) at the four-digit level, but it is adjusted to five-digit level in order to distinguish between additional Indonesian sectors of local importance. Throughout the paper, we refer to codes at five-digit level as products, three-digit level as industries and two-digit level as sectors.

ticipation are typically more productive, more capital intensive, and pay higher wages (Harrison and Rodríguez-Clare 2010). At the same time, the mere threat of foreign competition has been shown to increase the productivity of domestic enterprises (Bao and Chen 2018). Similarly, FDI are usually also found to spill over along the value chain to other firms in the economy (Javorcik 2004, Gorodnichenko et al. 2014, Genthner 2021). Since precise data on FDI regulation is frequently unavailable, most studies rely on FDI flows to proxy for reforms in FDI regulation. But as investment flows themselves are influenced by a large number of different factors, this raises the fundamental problem of unobserved heterogeneity (Harrison and Rodríguez-Clare 2010). Alternatively, newer studies rely on aggregated indices of FDI openness (Topalova and Khandelwal 2011, Duggan et al. 2013), which by construction cannot be used to capture differential effects of regulation across more disaggregated sectors. The use of disaggregated regulation data should help us to trace the effects of FDI regulation at a finer grained product scale.

Indonesia offers a great case to not only study the effects of FDI on firms (e.g., Blalock and Gertler 2008), but also to investigate the productivity effects of FDI regulation itself. As one of the largest economies in the world, with a wide variety of industries that rely on an abundance of both human and natural resources, Indonesia has emerged as an attractive FDI recipient. In order to increase transparency of the regulatory environment, the Indonesian government introduced a negative investment list (NIL) in 2000, which listed all sectors to be closed or only conditionally open to FDI (WTO 2013, Lindblad 2015). The FDI regime tightened regulations in 2007 with a substantial extension of the sectoral coverage of the NIL, followed by minor adjustments in 2010 and a partial deregulation in 2014.

Like in many other cases of recent regulatory tightening world-wide, national strategic interest was the stated reason for the regulatory reforms in 2007 as the government pledged to protect national industries from international competition and takeovers. Our regressions investigating the political economy of these reforms find that sectoral exposure to privatization at the beginning of the decade is among the strongest predictors of increased regulatory penetration at the product level, suggesting that protection of both current and former state-owned enterprises may have played a central role in decisions regarding the NIL.

The effects of this policy instrument have been hitherto unexplored, and offer a particularly interesting opportunity to investigate the effects of FDI regulation on firm performance at a highly disaggregated level. This paper exploits policy variation due to three revisions to the NIL that regulated various sectors at the five-digit product code level, listing each product that will be either fully or partially closed to FDI in the future and also specifying whether all firms or only certain types of firms are to be affected. We link regulatory changes in the NIL to a 16 year firm-level panel dataset, from 2000 to 2015, derived from the Indonesian yearly census of manufacturing plants. This census includes the full universe of manufacturing firms with at least 20 employees and reports a wide range of firm-level outcomes.[35] Our main outcome variables are the share of foreign ownership of each firm and two measures of firm productivity: estimated total factor productivity (TFP) and value added per worker. Our empirical strategy contrasts firms exposed to binding firm-specific regulation to firms which operate in regulated product markets but have not been subject to FDI restrictions and are only affected due to regulatory spillovers (Bourlès et al. 2013). Regulation indicators are firm-specific and vary by year, linking information from the NIL to the firm's main product (at five-digit level) while also utilizing individual firm characteristics (firm size, legal status, and prior foreign investment) to identify direct exposure to regulation. To get a better understanding of what drives our main results, we focus on changes in capital composition within affected firms and product markets and contrast the effects of the initial regulatory tightening to those of later de-regulation. We also consider several forms of heterogeneity, comparing especially effects between firms differing in their dependence on external finance (Rajan and Zingales 1998) and advanced technologies.

All our results are conditional on firm fixed effects, and hence only consider within-firm variation in the main economic outcomes over time. Additionally, our specifications include island-year and three-digit industry-year effects, which capture all average time variation due to global and national industry-specific shocks, as well as variation in average input prices or regional economic conditions. The panel structure of 16 yearly waves allows us to investigate the time profile of regulation in a more flexible way, by also including lags and leads of regulatory change.

The identification of causal linkages between FDI regulation and firm-level changes in foreign capital shares and productivity requires two main conditions to be fulfilled: no further interventions should be spuriously correlated with the FDI regulation, and regulation should be exogenous to all factors driving changes in firm productivity. The inclusion of three-digit industry-year effects deals with average effects of broad regulatory trends, at least to some extent. Additionally, we control for changes in output and input tariffs, as well as a combined proxy for other non-tariff measures, to make sure that our results are not driven by adjustments of tariff rates or fine-grained non-tariff trade regulations. As most of Indonesia's trade liberalization reforms were already completed by the mid-2000s (Kis-Katos and Sparrow 2015), we do not expect major effects from tariff changes.

[35] In what follows, we use the concepts of firm and plant interchangeably as we have no further information on the structure of multi-plant firms.

The second requirement poses a larger challenge, especially since the government may have faced incentives to restrict foreign entry, particularly among the least productive industries. Such a lobbying process (in the spirit of Grossman and Helpman 1994) would likely yield a negative correlation between regulation and productivity. Indeed, by testing a wide range of product market characteristics, we demonstrate that several political economy factors serve as good predictors of changes in regulation at the product level. Product markets were more likely to be regulated by the NIL if a larger share of the firms producing those products were either recently privatized or remained state-owned, or if existing state-owned producers were less productive, on average. Moreover, the NIL generally regulated products which were produced in larger firms, within more concentrated industries, and in firms which experienced larger capital accumulation before becoming regulated.

We address such reasons for endogenous product-specific regulation by allowing for differential outcome dynamics by including a set of interactions of initial product characteristics with a full set of year effects as well as a large set of time-variant product-level traits. Finally, since the NIL does not equally affect all firms within the same five-digit product market (regulation of some products is conditional on firm size, legal status, or foreign ownership share), we also have to make sure that the coefficients on binding regulation do not simply reflect different trajectories of firm growth across selected traits. Therefore, in our preferred specifications, we also allow for differential initial firm-trait-specific trends. Conditional on our controls, we see no evidence of pre-reform trends in productivity differences across regulated and non-regulated firms, which supports a causal interpretation of our findings.

The results document a robust negative relationship between binding regulation and foreign capital shares as well as firm productivity. Foreign capital already declined in regulated firms one year before the regulation came into effect, reflecting the presence of anticipation effects. Declines in productivity in regulated firms followed with a lag and were concentrated among industries with strong dependency on external financing (Rajan and Zingales 1998), as well as high technology sectors. Notably, relative as well as absolute declines in foreign capital within regulated firms were fully compensated by increases in domestic capital, suggesting either less efficient allocation or a lower technological content of domestic capital.

Our study is among the first to exploit fine grained variation in the regulatory framework of FDI in an emerging economy. While previous studies mainly focused on FDI liberalization (Girma et al. 2015, Eppinger and Ma 2019), we identify direct firm exposure to a tightening of FDI regulation and link it to declines in firm productivity. We further show that subsequent de-regulation does not trigger an immediate productivity response by undoing earlier productivity losses. This highlights the importance of rolling out FDI policies over the long-term and provides evidence of longer-lasting effects of regulatory tightening.

The paper is structured as follows. Section 4.2 describes the regulatory framework of the NIL in Indonesia and discusses political economy determinants of the regulation. Section 4.3 describes the data sources and presents descriptive trends. Section 4.4 introduces the estimation strategy and the identification approach. Section 4.5 presents results on the effects of the investment reform on foreign capital share and firm productivity and investigates potential channels behind these effects. Section 4.6 concludes.

4.2 Regulatory background

4.2.1 Foreign investment regulation in Indonesia

Indonesia began to remove barriers to FDI already under the "New Order" regime of President Suharto. The investment coordination board (BKPM, *Badan Koordinasi Penanaman Modal*) was installed in 1973 to oversee the process of approving or denying foreign investments (Gammeltoft and Tarmidi 2013). However, due to a strong dependence on natural resources, the Indonesian manufacturing sector was poorly developed until the early 1980s (Lindblad 2015). Successful subsequent efforts towards industrialization increased the importance of the manufacturing sector and made it the driving force behind the country's accelerating growth (Blalock and Gertler 2008). Beginning in the 1990s, the Indonesian government started to restructure its previously investment-hostile regime by opening up the economy to investments from abroad. The country quickly became a highly attractive host for foreign investment, offering access to natural resources as well as a large and quickly developing domestic market (Lindblad 2015).

The Asian financial crisis of 1997 substantially stunted Indonesia's economic development. Despite immediate intervention by the International Monetary Fund, the consequences of a rapidly depreciating Rupiah spread to the real economy. This was accompanied by social and political instability that destroyed much of the confidence in Indonesia as a host for investment (WTO 1998). In order to regain the confidence of foreign investors, substantial steps were taken towards administrative reform, privatization and further trade liberalization (Duggan et al. 2013). However, subsequent economic growth was not immediate, and foreign investors remained cautious since the business and legal environment remained rather precarious. Major reforms after 2004 introduced fiscal incentives to foreign investors, streamlined bureaucratic procedures, and ensured non-discriminatory treatment of foreign and domestic investors alike (WTO 2013). In the aftermath of these reforms, FDI inflows again experienced massive increases and economic growth recovered strongly. Nonetheless, despite the ongoing liberalization, trade and investment policy in Indonesia remains "blurred by contradictory signals" (Lindblad 2015, p. 229).

Immediately following the Asian financial crisis in 2000, the president released Presidential Decree 96/2000, of which the negative investment list was a key component (NIL 2000). The NIL created a list of sectors to be either closed or only conditionally open to FDI; conditions included among others the need to form joint ventures between domestic and foreign entities, and licensing requirements. Before 2000, no explicit list of sectors closed to foreign investment was publicly available. Approval procedures lacked transparency and were completely in the hands of the BKPM. The NIL 2000 was the first document of its kind to publish regulatory information in a transparent way. It included a fairly limited range of products and resulted in direct regulation of about 3% of the firms in our dataset.

The NIL was revised for the first time in 2007, and the new list was released with Presidential Decree 77/2007. In its trade policy review, the WTO emphasized that a detailed NIL improved transparency regarding regulations for investments and was therefore beneficial (WTO 2013). However, closing or conditionally opening certain sectors to foreign investment is likely to be associated with wasted gains from FDI (cf. Javorcik and Poelhekke 2017, Bao and Chen 2018). In this sense, the revised version can be interpreted as a protectionist measure, since it added substantially more sectors and involved more types of conditions compared to the NIL 2000. The NIL 2007 comprised manufacturing as well as agriculture and services and introduced five new standardized categories of conditions for the first time. Some five-digit products were fully closed to foreign investment; in others, FDI was only allowed under certain restrictions, limited to small and medium-sized firms, to the legal form of partnerships, restricted by upper limits on foreign capital ownership, or subject to new requirements for a licensing approval by the ministry in charge. The list has been revised by further Presidential Decrees in 2010, 2014 and 2016 (36/2010, 39/2014 and 44/2016), which removed some products while adding others, converted bans into licensing requirements, and slowly decreased the overall extent of regulation.

A comprehensive overview of all revisions of the NIL is provided in table B1 in the appendix, including its representation in our sample and the shares of regulated firms in sectoral and total manufacturing output. The share of firms subject to binding regulation in our dataset increased from the initial 3% to 22% due to the first revision in 2007. While the composition of regulation across conditions and sectors changed due to a revision in 2010, the share of affected firms remained fairly stable. The NIL 2014 finally reduced overall coverage of regulation to 15% of firms. The fact that still one quarter of manufacturing output recorded in our data remained subject to regulation suggests, however, a stronger protectionist focus on firms with larger sales over time.

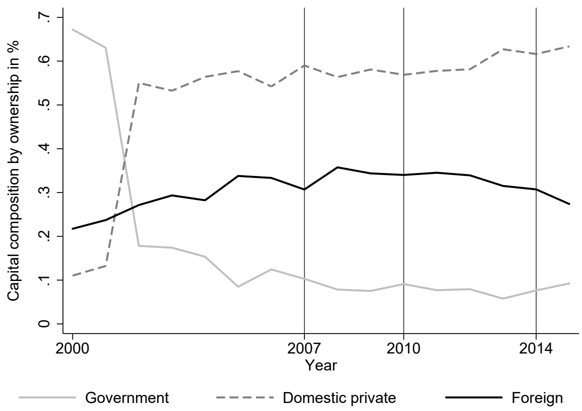
The various conditions used by the NIL to regulate FDI can be broadly grouped into licensing requirements, firm-specific and product-specific bans and limitations, or some combinations thereof (see again table B1 or appendix B.3 for more detail). *Licensing requirements* are administered by ministries in charge of regulating the product in general and may also be combined with upper limits or outright FDI bans. *Specific bans and FDI limitations* target specific firms either by their size, their legal status, or by some combinations thereof (or, in the case of a few selected wood products, their location). Firm size is determined according to Indonesian law and depends on cutoff values in a firm's sales value or net assets stock, while for legal status only partnerships are permitted to receive FDI. Last, FDI limitations provide forward-looking upper limits to foreign capital shares, only directly affecting those firms that have not yet surpassed the limit. The most restrictive *Product-wide bans* prohibit FDI within entire five-digit products irrespective of firm characteristics. The introduction of licensing can be expected to increase the costs of receiving FDI. Firm-specific bans and limitations will stop some firms from receiving additional foreign capital and potentially divert FDI to non-affected firms producing the same (or similar) products. If enforced, product-wide bans can be expected to stop new FDI going into some products altogether.

4.2.2 The political economy of the NIL

Although the Indonesian government did not explicitly announce their reasoning behind the choice of strategic products to be included in the NIL, a whole range of political economy factors could have influenced this decision (Gawande and Krishna 2003). The government may have chosen to particularly protect relatively less competitive state-owned enterprises (Chari and Gupta 2008) or those industries that rely more on unskilled and more vulnerable workers (Topalova and Khandelwal 2011). Moreover, from a theoretical perspective, market concentration should have eased coordination problems across firms and improved firms' ability to influence the passing of certain regulatory instruments through lobbying the government (Grossman and Helpman 1994, Chari and Gupta 2008). We explicitly control for these three dimensions in our empirical specifications by including variables for initial conditions at the five-digit product level (concentration of sales, share of blue-collar workers, share of public enterprises) interacted with a complete set of year fixed effects.

In order to better understand the driving factors behind FDI regulation, we systematically tested a large set of further factors which could possibly impact the product-specific regulatory penetration. We applied the procedure by Sala-i-Martin (1997) to search for robust predictors of changes in the regulatory environment by running re-

Figure 4.1: Trends in capital ownership structure over time



Note: The graph plots the average firm shares of total assets in government, domestic private and foreign ownership across all firms included in our sample in each year.

gressions at the product level across a wide range of model specifications. We investigated five groups of potential political economy factors at the product level, capturing (1) state ownership and prior privatization, (2) productivity dynamics, (3) firm size and concentration, (4) internationalization and (5) labor market characteristics, resulting in a total of 36 variables. Of these variables, we measure lagged levels as well as long differences over the previous five years.[36]

The Indonesian fiscal and administrative decentralization of 2001 (see e.g., Kis-Katos and Sjahrir 2017) was accompanied by a major wave of privatization, shifting a large share of firm assets formerly owned by local governments into the hands of private owners. Among the firms in our sample, the average share of firm capital stock owned by the government declined rapidly, from about 67% in 2000 to about 15% in 2004, and stabilized at this lower level (see figure 4.1).

This wave of privatization turns out to be the most important predictor of regulation through the NIL in product-level regressions. Table 4.1 lists the twelve product-level characteristics with the largest explanatory power for regulatory penetration across firms, together with their average estimated coefficients. Among the top five strongest predictors of product-level regulation, three are related to state ownership and the recent privatization of Indonesian firms. Products are more likely to have been subject to new FDI regulation if the firms producing them have experienced increased privatization over the past five years, and if the remaining share of state-owned enterprises operating within the product category was relatively higher and the productivity of these state-owned enterprises was relatively lower in the previous year.

Additionally, scale and productivity dynamics seem to have had an effect on which

[36] We tested these variables against each other in triplets, by running 6,545 regressions in total. See B.1 for more details on the estimation procedure and the full list of tested variables.

Table 4.1: Predictors of product-level regulatory penetration

Variable	Change in share of regulated firms ($t-1$ to t, sales weighted) Coefficient	CDF (non-normal distribution)	Cluster
Change in share of state-owned firms ($t-6$ to $t-1$)	-0.046	0.96	State ownership/privatization
Growth rate of capital-labor ratio ($t-6$ to $t-1$)	0.003	0.96	Productivity dynamics
Share of medium-sized firms ($t-1$)	-0.020	0.94	Firm size/concentration
Share of state-owned firms ($t-1$)	0.019	0.88	State ownership/privatization
Average productivity of state-owned firms ($t-1$)	-0.003	0.87	State ownership/privatization
Log of average firm sales ($t-1$)	0.001	0.84	Firm size/concentration
Change in share of exports in total sales ($t-6$ to $t-1$)	-0.012	0.83	Internationalization
Growth rate of average firm sales ($t-6$ to $t-1$)	0.002	0.82	Productivity dynamics
Growth rate of capital intensity ($t-6$ to $t-1$)	0.004	0.82	Productivity dynamics
Herfindahl concentration index of sales ($t-1$)	0.006	0.79	Firm size/concentration
Growth rate of average wage per worker ($t-6$ to $t-1$)	-0.004	0.79	Labor markets
Change in import penetration ($t-6$ to $t-1$)	0.006	0.78	Internationalization

Note: The table includes the 12 product-level characteristics with the highest predictive power of regulation, together with their estimated coefficient, the value of the CDF under the non-normality assumption (see Sala-i-Martin, 1997) and their respective thematic cluster. Factors are selected based on five-digit product-level regressions of the change in the average regulation share on triplets of explanatory variables.

products were included on the list. Prior to regulation, regulated product categories experienced capital accumulation (a larger growth in the capital-labor ratio) as well as larger sales growth. By contrast, the share of medium-sized firms within the industry is almost mechanically (negatively) linked to regulation, as several rules and limitations targeted large firms only. This is also why sales concentration or past average sales are positively correlated with regulation. From the large number of labor market characteristics that we evaluated, only average wage growth is linked to regulation, showing a negative correlation. When analyzing the relationship between regulation and internationalization, we find that regulatory penetration is negatively linked to past export growth, whereas past growth in import penetration turns insignificant in more extensive regressions.

Taken together, these results indicate that one of the motivations behind the NIL must have been to maintain domestic ownership among recently privatized firms and shield them from direct foreign competitors, as well as to cushion the remaining state-owned enterprises from competitive pressures. Moreover, these results suggest that regulation focused on product groups with larger market potential, as measured by past sales growth whereas the protection of domestic employment does not seem to have played a crucial role. Throughout our empirical analyses, we will include the twelve most important product-level drivers of regulation from table 4.1 as time-variant controls.

4.3 Data

4.3.1 Firm data

Our source of firm data is the annual manufacturing census of Indonesia (*Survei Industri*, SI), which surveys the universe of all registered Indonesian manufacturing firms with at least 20 employees. The census has been conducted by the BPS on a yearly basis since 1975 and contains a rich set of information at the level of manufacturing firms. Key variables include the values of inputs and output, foreign ownership, the value of imports and exports as well as employment, and capital stocks. We follow the literature by using the share of foreign capital as a proxy for FDI (see e.g., Takii 2005, Amiti and Konings 2007, Arnold and Javorcik 2009, all based on the same SI data).

The data is cleaned for missing values and extreme outliers. As is common in the literature, data points are interpolated between the previous and the next year to avoid the loss of too many observations, while further missing observations are dropped from the sample (Amiti and Konings 2007).[37] Our final dataset consists of an unbalanced panel of 24,725 firms with a total of 180,783 observations. The sample size decreases further in some regressions due to missing values contained in some of the control variables. We transform all input and output variables to their natural logarithms. In those cases where this would result in loss of many observations due to zero values (like foreign capital stock or imports), we use a Box-Cox-transformation. To make sure that this transformation does not distort our results, we replicate our main results using fixed effects Pseudo-Poisson Maximum Likelihood (PPML) estimators (see e.g., Chung et al. 2016). We deflate all monetary values to the base year 2008 by using the yearly wholesale price index from BPS.

There are some concerns regarding the data quality of the SI. First, doubts arise with respect to its completeness since it claims to include all medium-sized and large manufacturing firms in Indonesia. Due to the large number of firms, it is at least possible that the SI missed certain firms in some years, or failed to gather data from non-respondents, leading to non-random selection and an undercounting of smaller firms. The inclusion of financial incentives for the field agents to register new firms and identify firms which do not reply immediately reduces this problem considerably, since budgets are linked to the number of reported establishments (Blalock and Gertler 2008, Arnold and Javorcik 2009). However, this may also adversely incentivize field agents to forge values for non-reporting firms themselves. A second possible issue is the potential misreporting of information by firms. National law guarantees that information

[37] Appendix B.2 and B.3 provide more detailed information on data cleaning and merging procedures, whereas appendix B.4 introduces additional control variables.

gathered by the SI will be exclusively and anonymously used for statistical purposes. Firms may still be concerned that reported information might be leaked to tax authorities or competitors, and may in turn intentionally report incorrect data (Blalock and Gertler 2008). However, as the SI dataset is not explicitly used for monitoring purposes, we do not expect firms to report wrong values just in order to avoid being subject to FDI regulation. Finally, if firms do not put adequate effort into the accurate completion of the questionnaires, numbers may be falsely reported by accident. Hence, noise within the data is likely to be a considerable issue. However, as long as firm selection and response behavior is not directly linked to FDI regulation, firm and three-digit industry-year fixed effects are likely to lead to unbiased within-estimates. We investigate one specific dimension of the response behavior more explicitly in section 4.5.5 by assessing whether regulation causes firms to switch their reported main product, and whether switching firms show different levels of productivity in response to regulation.

4.3.2 Combined dataset and descriptive trends

In order to identify exposure to firm- or product-level regulation, we combine data from the firm census with self-collected information from five revisions of the NIL at the five-digit product level. We determine whether a particular firm faces binding regulations on its main product by combining data on its previous sales, net assets, legal status, foreign ownership shares and location with the detailed conditions of the NIL. We define the indicator variable *Binding regulation* as being equal to one if a firm is restricted by FDI regulation in any given year, taking firm characteristics relevant for the applicability of the regulation into account. For example, *Binding regulation* will equal zero for a medium-sized firm operating in a product market where large firms need a license to receive FDI, whereas it will be equal to one for a large firm producing the same product. The indicator will be equal to one for all firms producing a product with a strict product-wide ban, irrespective of firm characteristics. If regulation instead establishes an upper limit on FDI shares, we then consider firms which have already surpassed this threshold of foreign capital as exempt from binding regulation in the short run.[38] As a second measure of regulatory exposure, we record whether a firm is operating in a regulated five-digit product market with the indicator variable *Regulated product*. This allows us to capture potential product-level spillovers and to measure the differential impact of regulation by comparing regulated and non-regulated firms producing the same product.

[38] In order to alleviate concerns of endogenous adjustment of firm characteristics (such as firm size or legal status), we use each firm's time-invariant median value of these characteristics when determining its binding regulatory status.

Table 4.2: Summary statistics in 2001, 2007 and 2015

	2000		2007		2015	
	Mean	SD	Mean	SD	Mean	SD
Regulation variables:						
Binding regulation	0.03	0.18	0.23	0.42	0.18	0.39
Licensing requirements	0.01	0.10	0.03	0.16	0.06	0.25
Specific bans and FDI limitations	0.00	0.05	0.22	0.41	0.14	0.35
Sector-wide bans	0.02	0.14	0.01	0.11	0.01	0.11
Regulated product	0.04	0.19	0.36	0.48	0.42	0.49
Licensing requirements in product	0.01	0.10	0.04	0.21	0.13	0.34
Specific bans and FDI limitations in product	0.00	0.05	0.35	0.48	0.38	0.48
Binding de-regulation	0.00	0.00	0.03	0.16	0.19	0.39
De-regulated product	0.00	0.00	0.00	0.04	0.06	0.23
Main dependent variables:						
FDI share	0.05	0.20	0.05	0.21	0.07	0.25
ln(TFP)	10.17	1.51	10.18	1.50	11.13	1.47
ln(VAD/L)	9.87	1.24	9.88	1.23	10.79	1.19
ln(K)	14.23	2.04	13.79	2.07	14.30	2.12
ln(Value of foreign capital)	1.00	3.97	1.00	3.92	1.40	4.60
ln(Value of domestic capital (private + state-owned))	13.81	2.83	13.18	3.17	13.43	3.65
ln(Value of domestic private capital)	0.90	3.57	12.97	3.47	13.18	3.97
ln(Value of state-owned capital)	12.95	4.25	0.30	2.15	0.31	2.22
Weak dep. on ext. finance	0.58	0.49	0.61	0.49	0.61	0.49
Low technology	0.92	0.27	0.93	0.25	0.93	0.25
Medium-sized firm	0.94	0.24	0.94	0.24	0.92	0.26
Trading firm	0.41	0.49	0.41	0.49	0.44	0.50
Further firm variables:						
Government share > 50%	0.89	0.31	0.01	0.12	0.02	0.13
(Limited) partnership	0.09	0.28	0.09	0.29	0.09	0.29

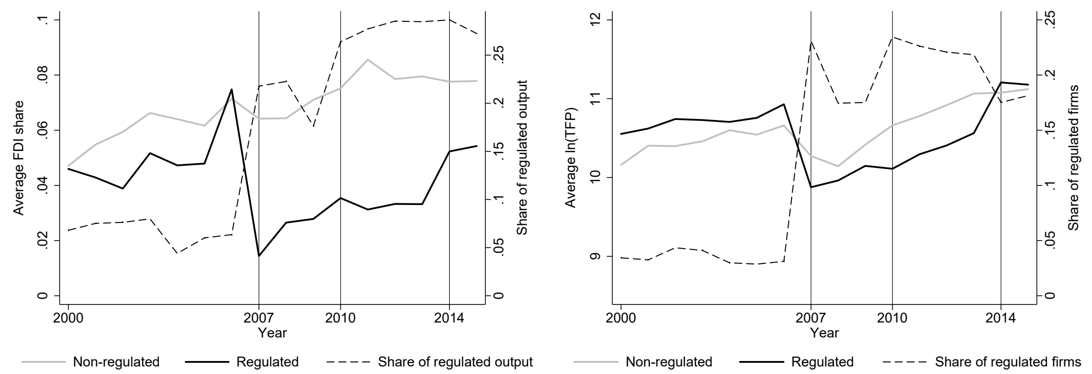
Note: Number of observations in 2001: 11,968; 2007: 13,347; 2015: 9,791.

Table 4.2 presents summary statistics for the main variables in the years 2000, 2007 and 2015.[39] The average share of foreign capital increases over time, although most domestic firms receive zero FDI throughout the whole time period. At the same time, binding regulatory penetration peaks in 2007. The same pattern is also reflected in figure 4.2. Dashed lines show a marked increase in both the share of total firm output subject to binding regulation and the share of the regulated firms in 2007, as well as some smaller adjustments in regulatory exposure afterwards. Table 4.2 also shows a more recent shift in the mix of the regulatory instruments from firm-specific bans towards licensing requirements.

While unregulated firms experienced a steady increase in average FDI shares over time (denoted by the gray solid line in the left panel of figure 4.2), average FDI shares among regulated firms dropped sharply in 2007 and only reached their previous trend by the end of our observed time period. However, this reflects the changing composition of the sample of regulated firms as regulation was extended towards firms with lower FDI shares. Figure B1 in the appendix splits firms according to their regulatory status in 2007, thereby distinguishing between firms that were continuously subject to binding regulation and those that faced binding regulation in 2007 for the first time. Newly regulated firms exhibit lower levels of foreign capital shares already prior to regula-

39 Summary statistics for the full estimation sample can be found in table B3 in the appendix.

Figure 4.2: FDI and productivity in regulated vs. non-regulated firms



Note: The graph plots the share of regulated output or regulated firms over the sample period (right scale) together with the average FDI share or the average log of TFP among regulated and non-regulated firms in the respective year (left scale).

tion, driving the massive drop of FDI shares among regulated firms. Figure B2 in the appendix additionally shows trends in average capital stock of firms, distinguishing between state-owned versus private capital. Average capital stayed relatively constant over time, with a drop in private capital among regulated firms. The structural shifts in FDI shares and capital composition arose from a combination of within-firm shifts in these outcomes and a composition effect as regulation was extended in 2007 to smaller and less internationalized firms. Our empirical strategy will not rely on the composition effect but only on within-firm variation.

The trends in estimated total factor productivity (TFP) in figure 4.2 show that firms facing binding regulation were somewhat more productive than other firms before the reform. In 2007, both regulated and non-regulated firms faced a negative productivity shock on average, but the average drop in productivity was substantially larger among regulated firms. In the aftermath, productivity in both groups recovered slowly and by 2015, average TFP was again close to equal in regulated and non-regulated firms. These trends are clearly descriptive, and again reflect composition effects, but they foreshadow our regression results. The observed negative productivity shock precedes any potential effects of the global financial crisis, the macro-economic effects of which did not reach the emerging markets for another two years. It was only in 2009 that Indonesian GDP experienced a short stagnating period, followed by a quick recovery. Hence, it is less likely that the 2007 productivity drop was driven by this common global market shock. Moreover, common market shocks that affected entire industries will be factored out in our empirical analysis by our use of three-digit industry-year effects.

Table B4 in the appendix displays the share of regulated firms, the average foreign capital share, and TFP by two-digit sector and year. Regulation across sectors shows a very heterogeneous picture. Wood and wood products was the only sector that was already strictly regulated in 2000 and remained regulated over time. By contrast, certain technology-intensive sectors were never affected by the NIL, including electrical and communication equipment, motor vehicle, and medical and optical instrument industries. Other sectors experienced regulatory tightening in 2007 and substantial deregulation afterwards, like food and beverages, publishing and printing, non-metallic mineral products, and transportation equipment. Finally, one last group of sectors experienced either continuous regulatory tightening, in the case of tobacco products and basic metals, or a stricter regulation in 2007 that was only marginally changed afterwards (like textiles, machinery and equipment, or furniture and the remaining category). Our analysis will only rely on within-industry variation in regulation and productivity of the three-digit industries over time, while controlling for industry-year effects. Thus, we do not explain changes in FDI penetration or industry-wide changes in productivity, but rather focus on the within-firm and within-industry-year differential relationship between FDI regulation and firm outcomes.

4.4 Estimation strategy

We investigate the effect of the foreign investment regulation on firm outcomes by estimating the impact of *Binding regulation* relative to firms operating in a *Regulated product* environment, within the same three-digit industry and macro-region, for which regulation is non-binding. The corresponding estimation equation is

$$\begin{aligned} y_{ijsrt} = \quad & \alpha\, \textit{Regulated product}_{jsr,t-1} + \beta\, \textit{Binding regulation}_{ijsr,t-1} + \lambda_i + \eta_{rt} + \psi_{st} \\ & + \boldsymbol{X}'_{ijsr,t-1}\, \gamma + \boldsymbol{Z}'_{1j,2005} \times \phi_t + \boldsymbol{Z}'_{2jt}\, \varphi + \boldsymbol{W}'_{i0}\, \theta \times t + \varepsilon_{ijsrt}, \end{aligned} \tag{4.1}$$

where y_{ijsrt} measures the relevant outcomes of firm i operating in the five-digit product market j within the three-digit industry s in macro-region r and year t.[40] Our main outcomes measure foreign equity as a percentage of total firm equity (FDI share) and two productivity proxies: the estimated log of TFP and the log of value added per worker in each firm.

Essentially, equation (4.1) follows a difference-in-difference approach, with *Regulated product*$_{jsr,t-1}$ being a product-level treatment variable indicating whether product market j is subject to any kind of regulation in year $t-1$, irrespective of firm i's characteristics. *Binding regulation*$_{ijsr,t-1}$ can be interpreted as an interaction term of the treatment with an applicability condition on firm level. It takes a value of one if an investment restriction in product market j in year $t-1$ is de facto binding, which is conditional on the characteristics of firm i (and, in a very few exceptional cases, on firm location).

[40] We distinguish between five island groups as macro-regions: Sumatra, Java, Kalimantan, Sulawesi and the rest of smaller islands.

Our main coefficient of interest is β, which measures the differential effect on firms facing binding regulation as compared to non-regulated enterprises operating within the same regulated product environment, whereas α captures the horizontal spillover of regulation within five-digit products among indirectly affected firms. Our specifications focus on the lagged effects of regulation occurring in $t-1$, while further tests also include up to three lags and leads of regulation at the same time. Adding further lags and leads for regulation helps us to better understand the timing patterns of regulatory effects and to look for anticipatory effects or pre-trends. We condition our results on firm fixed effects λ_i, a set of year effects that vary by macro-region η_{rt}, and three-digit industry-year fixed effects ψ_{st}.

All regressions include a vector of controls $\boldsymbol{X}_{ijsr,t-1}$ to capture time-variant firm characteristics, such as a set of indicators of firm age categories and a public enterprise indicator (if more than half of a firm's capital is owned by the state). We additionally control for changes in other dimensions of trade policy (see B.4 for more detailed variable descriptions). Although the extensive wave of tariff liberalization in Indonesia has ended by the mid-2000s, we include output and input tariffs in all regressions to alleviate concerns that late adjustments of tariff rates may drive our results. We also generate a proxy for non-tariff barriers based on data from the UNCTAD's Non-tariff Measures (NTM) Programme. Our NTM indicator takes a value of one if there is at least one non-tariff trade regulation within a four-digit product in a particular year. The residuals ε_{ijsrt} are robustly estimated and clustered at the firm level in our main specifications (and at the product-year level as a robustness check).

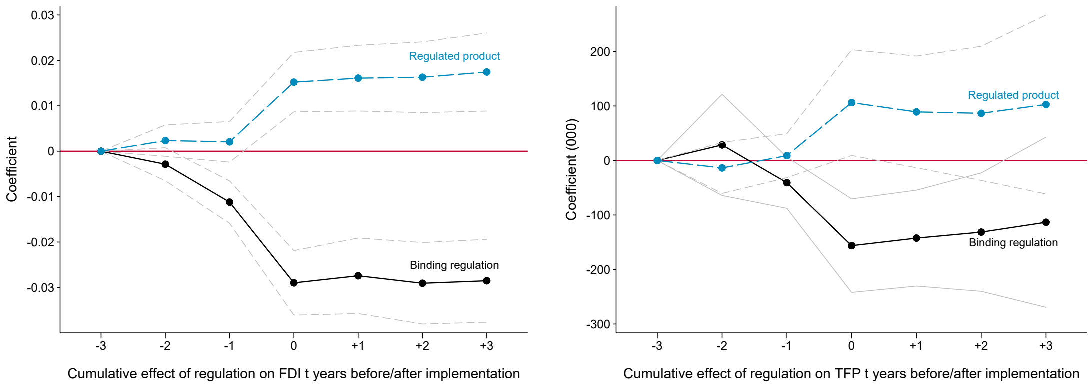
Our extensive fixed effects mitigate issues with unobserved heterogeneity and endogenous regulation. Firm fixed effects absorb all time invariant unobservable firm characteristics, including the firms' average propensity to enjoy protection or be subject to regulation (Goldberg and Pavcnik 2005). Island-year fixed effects flexibly control for all regional factors that may correlate with both regional exposure to regulation and shifts in foreign capital shares. The industry-year fixed effects control for time-variant incentives to lobby for protection at the three-digit industry level (Blalock and Gertler 2008). These controls also reflect industry-specific variation in the price of intermediates, which will affect TFP estimates substantially, and thus are considered particularly crucial for productivity estimates (Goldberg and Pavcnik 2005). Moreover, they also implicitly cancel out common time trends and common macroeconomic or regulatory shocks to FDI and productivity.

In our preferred specifications, we capture three further sets of determinants of regulatory action both at the detailed five-digit product level and at the firm level. First, we interact $\boldsymbol{Z}_{1j,2005}$, average characteristics of product j measured two years before the major regulatory reform (in year 2005), with a full set of year effects ϕ_t. By choosing a pre-reform year, we avoid product characteristics to be affected by anticipation effects of later regulation.[41] These characteristics include the share of state-owned firms, a Herfindahl index of sales concentration and the share of blue-collar (production) workers within the five-digit product, all of which have been hypothesized to explain the success of firm lobbying for or against regulation (Grossman and Helpman 1994, Chari and Gupta 2008, Topalova and Khandelwal 2011). Interactions with a full set of time effects control flexibly for all further (as well as previous) product-specific dynamics that may be related to these product market characteristics. In a second approach, we control for $\boldsymbol{Z}_{2jt}$, a vector of twelve time-variant product market traits that were determined to be the most robust predictors of product-level regulatory penetration. These traits are listed in table 4.1 and include past privatization and productivity dynamics, measures of firm size and concentration, growth in wages and exports, as well as import penetration (also see section 4.2.2 and B.1 in the appendix). The third set of factors, $\boldsymbol{W}_{i0}$, allows for differential time dynamics by a set of firm-specific characteristics. Certain traits make exposure to binding regulation less likely as, for example, medium-sized firms were exempt from certain forms of regulation. Further firm characteristics like legal status or state ownership may also put firms on different growth trajectories. We control for these differences in firm growth by interacting the first observation of these variables for each firm with a time trend. These three sets of controls help us to isolate the causal effect of regulation on productivity.

Throughout the paper, we report results on two productivity estimates, contrasting an estimated TFP residual with the reported value added per worker. We estimate firm-specific TFP by simultaneously accounting for the correlation of the firm's input choices with the error term (cf. Javorcik 2004, Amiti and Konings 2007, Newman et al. 2015, Fons-Rosen et al. 2017). We apply the approach suggested by Wooldridge (2009), estimating the log of TFP separately for each two-digit sector, taking into account the varying importance of input factors across industries (see appendix B.5 for a detailed description). A more disaggregated estimation, yielding separate input coefficients on the three-digit industry level, is also feasible but results in less stable input coefficients (see also section 4.5.5). When estimating TFP over time, results may also be sensitive to the choice of price deflators. We comment on the robustness of our results to using more detailed sectoral input and output deflators in section 4.5.5. To account for potential imprecision in TFP estimates, we weight all regressions that use TFP as a dependent variable by the inverse of the estimated standard error of the residual. Finally, we check the robustness of our results to the way productivity is estimated by always contrasting TFP results with a substantially simpler but frequently used proxy of productivity (see e.g., Amiti and Konings 2007), the log of value added per worker.

[41] Our results remain robust when substituting characteristics in 2005 with the median firm observation between 2000 and 2005.

Figure 4.3: Time profiles of regulation on FDI and TFP



Note: Plotted coefficients are estimated controlling for categories of firm age, a public enterprise indicator, output and input tariffs, an indicator of non-tariff measures, interactions of five-digit product traits with year fixed effects, time-variant political economy factors and firm trait specific trends as well as firm, island-year and industry-year FEs. The left (right) graph shows cumulative effects of leading and lagged binding (product) regulation on FDI (TFP) over time where the effect of regulation three periods ahead is normalized to zero. Grey-shaded lines indicate 90% confidence bands.

4.5 Results

4.5.1 Pre-trends and anticipation effects

Time patterns of FDI and productivity before and after the regulatory change indicate some anticipatory effects for FDI, but no pre-trends in productivity. This latter finding supports a causal interpretation of the effects of regulation on productivity. Figure 4.3 plots estimated accumulated coefficients over time of both product and binding regulation from fully specified firm-level regressions according to equation (4.1), in which both the *Binding regulation* and *Regulated product* indicator include three further lags and leads. To plot the time pattern of productivity, we transform log TFP into levels as the horizontal accumulation of semi-elasticities over time is not feasible. The results of this exercise thus have to be interpreted in terms of productivity units. The sample size shrinks with the inclusion of three lags of regulation (as we have to omit the years before 2003 from the regression), but leads of regulation can be calculated based on newer revisions of the NIL.

Foreign capital shares and productivity follow different time patterns near the time of the regulatory intervention (figure 4.3). FDI shares (in the left graph) decline in firms facing binding regulation already one year before the regulatory tightening, followed by a further decrease in the year of regulation and an increase within firms that produce the same product but are not subject to regulation. The spillover effect of product market regulation is thereby somewhat smaller in magnitude than the differential effect of binding regulation, resulting in a significant aggregate decrease in FDI shares among regulated firms. FDI shares stabilize in the aftermath of binding regulation. As existing FDI stocks are not directly affected by regulation, this highlights how anticipated future changes to the NIL increase uncertainty about the investment environment and lead to a reallocation of foreign capital. By contrast, the time effects of binding regulation and spillovers on TFP show no significant pre-trends in the three years prior to implementation. This indicates that, conditional on our main controls, regulated and non-regulated firms were similar in terms of productivity in the period immediately preceding regulatory change. Beginning in the year the regulation was implemented, productivity tends to move upward in regulated product markets without reaching consistent significance, with a marked differential productivity decline among regulated firms, and the cumulative differential effect of binding regulation over time becomes significantly negative one year after the regulatory change.

These time patterns indicate firm responses in accordance with the original intent of the NIL, which had the primary goal to shift the sectoral presence of FDI. In our baseline models, we follow the literature by linking both FDI and productivity to regulatory intervention occurring within the past year. Note, however, that figure 4.3 indicates an anticipatory effect of FDI, and hence our lagged results will understate the full impact on foreign capital shares and capital adjustment in general (but not on productivity). The missing pre-trends in productivity suggest that policy makers were not implementing protectionist measures in product markets with declining productivity and thus reverse causality is unlikely to drive our results. Nonetheless, the significant FDI reaction in the year preceding regulation highlights the potential importance of anticipatory effects.

Anecdotal evidence suggests that firms may have anticipated changes to the regulatory framework before the release of the new Presidential Decrees. For instance, the largest Indonesian newspaper, Kompas, had already begun covering the topic in 2005 (June 30), two years before the actual NIL 2007 revision, reporting that the wheat industry would not be included on the new list. News coverage of the NIL intensified at the beginning of 2007. On February 8, Kompas announced that the Ministry of Industry intended to include sugar refineries on the list. When the revision finally took place, Kompas reported some concerns from businesses that were critical about the list, citing that “existing investment is difficult to develop even though the NIL is not retroactive” (July 16, 2007, p. 18). Similarly, Kompas had already begun reporting on plans to revise the NIL at the beginning of 2013, while the Presidential Decree was only released in April of 2014. In February of 2013, Kompas quoted the head of the investment coordination board, M. Chatib Basri, who said that “the main goal is to improve national competitiveness and to be more investor friendly” while “there are still sectors that must be protected” (February 18, 2013, p. 20). Another article reported on government plans for a relaxation of investment regulations in the alcoholic beverage industry on July 12, 2013. By the end of 2013, news coverage of the topic had increased substantially. For example, Reuters reported on the "ease of regulation to allow foreign companies [...] to manage and operate airports" (November 20, 2013). Around the same time, Kompas published a letter to the editor in which a concerned reader named further sectors where access to foreign investment is planned, including, among others, the pharmaceutical industry (November 28, 2013).

Even though this evidence is purely anecdotal, the fact that newspapers openly discussed revisions to the NIL over one year ahead of its implementation shows its relevance to the Indonesian economy. We further believe that industries and firms had become aware of more detailed plans of the revisions even before the issue was covered by the media, which would explain why the observed anticipatory effects and changes in foreign investment shares were already occurring one year before the regulation was in place.

4.5.2 Baseline results

Results in table 4.3 show a significant differential decline both in FDI and productivity among firms facing binding regulation. We report the effects of regulation on FDI shares (panel A) and our two measures of productivity (panel B and C) using equation (4.1). All regressions identify within firm variation by including firm fixed effects. We also factor out all regional macroeconomic and industry-wise policy shocks by including island-year and three-digit industry-year effects. Moreover, we control for basic time-variant firm characteristics as well as tariff measures, as described in section 4.4 and reported in table B5 in the appendix.[42] Further columns extend this basic specification, flexibly controlling for a range of potential determinants of endogenous FDI policies. Column 2 includes interactions of selected product characteristics in a pre-reform year (2005) with a full set of year effects in order to capture the firms' ability to lobby regulators (proxied by the share of state-owned firms, sales concentration, and the share of blue-collar workers). Column 3 further includes controls for twelve time-variant product-level characteristics determined to be the most important drivers of product-level regulation (see table 4.1). Column 4 adds firm-trait-specific time trends, allowing for differential growth trajectories by initial firm characteristics that were related to regulatory exposure (using FDI shares and indicators for state ownership, legal status, and firm size in the initial period). Overall, these additional variables control for a rich list of political economy factors that could have potentially explained the exposure to binding product-level regulation. The coefficients stay remarkably stable when further time dynamics of product and firm characteristics are controlled for, indicating

[42] The coefficients of these controls point into the expected direction (see table B5 in the appendix).

Table 4.3: Regulatory effects on FDI and productivity

	(1)	(2)	(3)	(4)
Panel A: Dependent: FDI share				
Regulated product	0.002	0.003	0.003	0.005***
	(0.002)	(0.002)	(0.002)	(0.002)
Binding regulation	−0.004*	−0.004**	−0.005**	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.871	0.871	0.871	0.872
Panel B: Dependent: ln(*TFP*)				
Regulated product	0.012	0.012	0.011	0.004
	(0.014)	(0.014)	(0.014)	(0.014)
Binding regulation	−0.044***	−0.040**	−0.044***	−0.037**
	(0.016)	(0.016)	(0.016)	(0.016)
R-squared	0.811	0.811	0.812	0.812
Panel C: Dependent: ln(VAD/L)				
Regulated product	0.014	0.010	0.009	0.001
	(0.015)	(0.015)	(0.015)	(0.015)
Binding regulation	−0.041**	−0.035**	−0.038**	−0.030*
	(0.016)	(0.017)	(0.017)	(0.016)
R-squared	0.736	0.736	0.737	0.737
Basic controls	Yes	Yes	Yes	Yes
Industry-year interactions	Yes	Yes	Yes	Yes
Island-year interactions	Yes	Yes	Yes	Yes
Product traits in 2005 × Year		Yes	Yes	Yes
Time-variant product traits			Yes	Yes
Firm traits specific trend				Yes
Observations	180,783	180,783	180,783	180,783
Firms	24,725	24,725	24,725	24,725

Note: The dependent variable is the share of foreign capital, log of total factor productivity or log of value added per worker within each firm. Regulated product is set to one if the firm's main product (five digit) has been regulated in the given year. Binding regulation is set to one if the main product (five digit) has been regulated in the given year and the firm itself has been subject this regulation. Basic controls include firm fixed effects, categories of firm age, a public enterprise indicator, output and input tariffs as well as an indicator of non-tariff measures. Five-digit product traits in 2005 include sector concentration of sales, the share of blue-collar workers and the share of public enterprises. For full list of time-variant product traits see table 4.1. Initial firm-level traits include foreign capital share as well as firm size, legal status and public enterprise indicators and allow for trait-specific linear trends. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

that endogenous regulation is unlikely to be driving these results.

In our preferred specification (column 4), the estimated lagged impact of the binding regulation indicator on foreign capital share is highly significant, and implies that regulation is associated with up to a 0.8 percentage points reduction in the foreign equity ownership share on average (see table 4.3, panel A). This effect may not appear very substantial at first glance, but it still amounts to more than 10% of the mean foreign ownership within the sample, which is about 7%. Moreover, as FDI shares have adjusted more quickly to regulation due to anticipation effects (see figure 4.3), coefficients for lagged regulation will not capture the full effect. Firms that operate in a regulated product market experienced a comparable although somewhat smaller increase in their foreign capital shares, which becomes highly significant in column 4. This provides evidence for horizontal spillovers from regulation as foreign investors re-allocate their new investments to non-regulated firms producing the same product.

Binding regulation is also linked to a statistically significant decline in TFP and value added per worker (see table 4.3, panels B and C). Our preferred specification in column 4 shows that firms which faced binding regulation experienced a 3.7% reduction in TFP, or a 3% reduction in the value added per worker relative to non-regulated firms producing the same product. These productivity adjustments are not proportionately linked to the relative decline in FDI shares. Alternative specifications that include the foreign capital share directly as an additional (albeit endogenous) control result in virtually the same regulatory coefficient (see table B6 in the appendix). While TFP is insignificantly positively related to foreign ownership shares, the regulatory effects cannot be mechanically explained by the decline in the firms' FDI shares. Hence, we believe that productivity losses reflect changing patterns of technological upgrading as well as changing expectations with respect to competitive pressure that lead to adjustments in factor use. Moreover, we do not detect statistically significant productivity increases among non-affected firms operating within the same product markets. This lack of significant regulatory spillovers reinforces the notion that productivity effects do not mechanically reflect shifts in foreign capital across firms but result from a changing product-specific investment climate and competition. Among non-regulated firms, the beneficial effects of a new foreign capital influx may have been counteracted by an increasing regulatory uncertainty and declining competitive pressures at the product level.

Our main results remain robust when controlling more flexibly for fixed effects and allowing for alternative standard error clusters. Table B7 in the appendix first substitutes three-digit industry-year fixed effects with more aggregate two-digit sector-year interactions in column 1. Column 2 repeats our preferred specification, which is then re-estimated with clustering standard errors at the product-year level (instead of firm level). Columns 4 and 5 control for more flexible three-digit industry-island-year interactions, thereby controlling for industry-specific shocks within macro-regions and allowing for error clustering on either firm or product-year level. Both coefficients of interest are remarkably stable and stay mostly significant across all specifications. In particular, the regulatory impact on foreign capital shares in panel A barely changes. At the same time, the effect of binding regulation on TFP is slightly decreasing when introducing additional fixed effects or clustering up, but remains statistically significant at the 10% level. Results for labor productivity in panel C also remain qualitatively the same, even though they do not always reach standard significance levels.

Table 4.4: Transmission channels

	Regulated product		Binding regulation		Observations	Firms
	Coeff	SE	Coeff	SE		
FDI share	0.005***	(0.002)	−0.008***	(0.002)	180,783	24,725
ln(*TFP*)	0.004	(0.014)	−0.037**	(0.016)	180,783	24,725
ln(VAD/L)	0.001	(0.015)	−0.030*	(0.016)	180,783	24,725
ln(K)	0.001	(0.019)	0.024	(0.021)	180,783	24,725
ln(Foreign K)	0.129***	(0.038)	−0.197***	(0.044)	180,783	24,725
ln(Domestic K)	−0.011	(0.036)	0.077**	(0.038)	180,783	24,725
ln(Private K)	−0.077*	(0.044)	0.082*	(0.048)	180,783	24,725
ln(Gov.t K)	0.058*	(0.030)	0.023	(0.034)	180,783	24,725
ln(L)	−0.001	(0.009)	0.000	(0.010)	180,783	24,725
ln(w/L)	−0.010	(0.012)	0.034**	(0.013)	142,211	21,564
ln(Sales)	0.015	(0.016)	−0.029	(0.018)	180,783	24,725
ln(Exports)	−0.021	(0.072)	0.107	(0.077)	142,333	21,857
ln(Imports)	0.128**	(0.060)	−0.192***	(0.064)	180,783	24,725

Note: The dependent variables are listed in the first column. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

4.5.3 Potential reasons for productivity losses

Adjustments of other firm outcomes

In order to shed more light on the adjustment dynamics behind the observed productivity losses, table 4.4 shows changes in input use and other balance sheet outcomes in response to binding and product regulation. The first two columns show coefficients and standard errors for the spillover effects from product regulation on unregulated firms, the next two columns present differential coefficients (and their standard errors) on binding regulation. The first three lines repeat results for FDI and productivity from the fully specified baseline model (table 4.3, column 4). Further results focus on capital use and composition, labor use and remuneration, sales, exports, and imports.

Contrary to our expectations, limiting access to foreign capital did not lead to an overall shortage of capital, neither within regulated product markets on average, nor among firms facing binding regulation. The value of foreign assets increased substantially among unregulated firms producing regulated products (by 12.9%), reflecting horizontal spillovers, but there was a larger differential decrease in foreign capital among firms facing binding regulation (by 19.7%). This resulted in a total decline of foreign capital within directly regulated firms (with the sum of the two coefficients being significantly negative). These differential dynamics were fully compensated by a relative increase in domestic private and state capital. The value of state-owned capital shows marginally significant increases within the entire regulated product group, irrespective of binding regulation. Together with our previous finding that prior privatization experiences were among the strongest predictors of product-level exposure

Table 4.5: Robustness: Pseudo-Poisson Maximum Likelihood estimations

	Regulated product		Binding regulation		Observations	Firms
	Coeff	SE	Coeff	SE		
K	0.022	(0.152)	0.250	(0.155)	180,783	24,725
Foreign K	0.181	(0.162)	−0.317*	(0.190)	18,559	2,521
Domestic K	−0.006	(0.184)	0.457**	(0.180)	177,344	24,070
Private K	0.094	(0.123)	0.312**	(0.124)	176,074	23,838
Gov.t K	−0.423	(0.441)	0.590	(0.485)	98,436	11,835
L	0.054**	(0.023)	−0.026	(0.025)	180,783	24,725
w/L	−0.009	(0.020)	0.019	(0.021)	142,212	21,564
Exports	0.003	(0.105)	−0.128	(0.105)	39,968	5,817
Imports	−0.222*	(0.127)	−0.030	(0.182)	42,733	5,185

Note: The dependent variables are listed in the first column. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

to the NIL (cf. table 4.1), this provides suggestive evidence that plans to strengthen "national champions" may have contributed to the targeting of FDI regulation. For domestic private capital, we see opposite dynamics as for foreign capital, namely negative spillover effects and differential increases among directly regulated firms. The results on capital composition broadly hold when using PPML estimation, as shown in table 4.5, but only the differential effects of binding regulation remain significant.[43] Overall, the NIL seems to have been successful in channeling more domestic private capital towards regulated firms, but this has been accompanied by productivity losses in the aftermath of its introduction.

Although labor market factors had low explanatory power for product-wise penetration of the NIL, the policy seems to have benefited local workers within protected firms through differential wage increases. However, given the substantially restricted sample size due to missing wage information, we do not want to over-interpret this result. Additionally, the positive wage effect does not persist in PPML estimation (shown in table 4.5). We also see statistically significant changes in total employment in regulated products in the PPML model but not in our baseline log specification.

We do not see average changes in sales or exports, but the use of intermediate imports moves in the same direction as foreign assets, both for the baseline and the interaction effects. Positive regulatory spillovers at the product level may have resulted in higher access to intermediate imports, whereas import use is the one thing that substantially declined in firms facing binding regulation. This is in line with the literature showing that the reliance on intermediate imports moves hand-in-hand with foreign ownership (Amiti and Konings 2007, Arnold and Javorcik 2009). Like for the labor market effects, table 4.5 reveals that trade results are not robust to PPML estimation. The positive

[43] We only perform PPML estimation for variables that have at least some zeros or low values as for the others we do not expect any distortions from a log-linear model.

coefficient at the product level even turns negative, although estimated over a very substantially reduced sample. For our further results, we will therefore focus on the most robust channel of shifting capital composition.

Heterogeneities by sector and firm types

Our average results mask differential adjustment dynamics across firms of different types. In order to investigate the role of heterogeneities in our baseline results as well as in the shifts of capital composition, we test for heterogeneous effects across different groups of firms. We thereby focus on two potential channels, while referring to additional two heterogeneities in the appendix (see appendix B.4 for a precise definition of the sub-groups). First, following Rajan and Zingales (1998), we assess the importance of access to financing in terms of productivity. Alquist et al. (2019) point out that foreign investors more frequently target sectors which are more reliant on external financing, especially in the context of full acquisitions. Any policy that restricts foreign capital inflows may also complicate the external financing of domestic enterprises, resulting in mis-allocation of capital, inefficiencies and productivity losses. For instance, Fauceglia (2015) shows that an improved investor protection in Brazil is particularly beneficial for firms with high dependence on external finance as those firms become more likely to adopt new technology. Second, following the large body of literature advocating the importance of FDI in technology transfer to domestic enterprises (e.g., Blalock and Gertler 2008, Lin et al. 2021), we assess the role of technology intensity (using sectoral distinctions as defined by OECD 2003). Descriptively, neither high finance dependency, nor high-tech sectors seem to have been specifically targeted by the NIL, but still, decreased access to either financing or technology could have contributed to the observed productivity declines.

Table 4.6 repeats our main FDI and productivity results by splitting firms into two groups based on sectoral characteristics, and interacting the indicators for binding and product regulation with the group indicators. In panel A, we separate the effects of regulation by sectoral dependency on external finance (Rajan and Zingales 1998), whereas panel B is separated by sectoral technology intensity (OECD 2003). The table only reports the differential interaction coefficients with binding regulation, although spillover effects (interactions with product regulation) are also controlled for (and results are reported in table B8 in the appendix).

When compared to other firms within the same regulated product market, FDI shares declined upon binding regulation to a statistically similar extent between sectors with high and low finance dependency, whereas the relative losses in FDI were larger among high-technology sectors. The resulting relative productivity declines were most pronounced among firms operating in high-technology and high-finance-dependency sec-

Table 4.6: Heterogeneity by external financial dependence and technology (binding regulation)

	Coeff	SE	Coeff	SE	p-value: 1=2
Panel A					
Binding regulation ×	Weak dep. on ext. finance		Strong dep. on ext. finance		
Dependent:					
FDI share	−0.009***	(0.002)	−0.007*	(0.004)	[0.742]
ln(*TFP*)	−0.017	(0.020)	−0.058**	(0.026)	[0.212]
ln(VAD/*L*)	0.002	(0.021)	−0.072***	(0.026)	[0.025]
ln(*K*)	0.058**	(0.026)	−0.027	(0.033)	[0.044]
ln(Foreign *K*)	−0.183***	(0.046)	−0.222***	(0.084)	[0.678]
ln(Private *K*)	0.127**	(0.057)	0.014	(0.082)	[0.241]
ln(Gov.t *K*)	0.020	(0.043)	0.027	(0.050)	[0.916]
Panel B					
Binding regulation ×	Low tech. sector		High tech. sector		
Dependent:					
FDI share	−0.007***	(0.002)	−0.036***	(0.014)	[0.035]
ln(*TFP*)	−0.032**	(0.016)	−0.141*	(0.078)	[0.176]
ln(VAD/*L*)	−0.021	(0.017)	−0.157**	(0.074)	[0.072]
ln(*K*)	0.035	(0.021)	−0.144	(0.111)	[0.113]
ln(Foreign *K*)	−0.144***	(0.042)	−1.177***	(0.333)	[0.002]
ln(Private *K*)	0.089*	(0.047)	0.130	(0.286)	[0.887]
ln(Gov.t *K*)	0.007	(0.035)	0.181	(0.124)	[0.174]

Note: The dependent variables are listed in the first column, indicator variables interacted with Binding regulation on the top of each panel. All regressions are specified according to column 4 of table 4.3 and also include interactions of the reported indicator variables with Regulated product (reported in table B8). The last column tests whether the reported interaction terms are statistically different from each other. For number of observations see table 4.4. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

tors (with losses of up to 16%), and in fact turn insignificant in sectors with low external finance dependency. Both high finance dependency and high technology may reflect differences in the need for foreign capital, either for means of financing, or for accessing foreign technologies. These results support the potential importance of both possible explanations for productivity declines.

As previously noted, productivity losses were not driven by a mere shortage of capital. While binding regulation is positively but insignificantly related to capital stocks on average (see table 4.4), total capital stocks even rose in relative terms upon binding regulation in sectors less reliant on external finance. However, the patterns connecting relative productivity losses to capital stock changes across these sub-groups are not entirely clear-cut (cf. table 4.6). Firms in high-tech and high-finance-dependent sectors experienced relative decreases in both foreign capital and productivity but no relative change in total capital. Firms in low-tech and low-finance-dependent sectors were also facing declines in foreign capital, no or positive relative adjustments in total capital and generally smaller relative productivity losses. The coefficients for the types of domestic capital (private or government capital) do not statistically significantly differ across the different groups of firms. However, although very imprecisely estimated, the point estimate on government capital is substantially higher in high-technology sectors, providing at least some suggestive evidence that the government may have channeled new investments into regulated high-tech sector products.

Although we cannot disentangle the relative importance of each of the above factors and consider the heterogeneity results as merely descriptive, some common patterns emerge. Productivity declines are always observed in tandem with substantial declines in foreign capital among the affected groups of firms. It is important to note though that reduced productivity can not be directly explained by changes in foreign capital shares (see appendix table B6). Instead, we suspect that changes in the general competitive environment, technology access and factor reallocation all may have played a role. As total capital availability in many sub-groups increased significantly in relative terms, the observed relative productivity losses are more likely to reflect a lower technological content and less efficient use of domestic capital. Firms experiencing relative productivity declines upon being subject to binding regulation are more likely to use advanced technology and operate in external finance-dependent sectors.

Table B9 in the appendix splits firms into groups with respect to their size (panel A) or trading status (panel B). Like before, interactions with product regulation are also controlled for (and results are reported in table B10). Large firms have been more specifically targeted by the NIL and as a result could be expected to respond more strongly to declines in foreign capital. At the same time, larger firms are more productive (Melitz 2003, Blalock and Gertler 2008, Mrázová and Neary 2019) and may be better able to accommodate (or even circumvent) sudden regulatory changes. Furthermore, firms may differ in their involvement in global value chains. We therefore allow for differential effects among firms that engage in export or import activities. In our sample, these firms are also more likely to be foreign owned and, thus, may suffer larger productivity losses when access to foreign capital is limited. In contrast, if foreign technologies are acquired through importing, trading firms may suffer less from missing foreign capital. These differences, however, yield less conclusive results. Relative losses in FDI were larger among larger firms and among firms engaging in international trade. At the same time, firms of different size or trading behavior exhibit similar relative productivity losses. Most of the other comparisons of relative effects of binding regulation lack statistical precision.

4.5.4 Types of regulation and de-regulation

Testing different regulatory instruments

Our regulation indicators cover a range of different policy provisions of varying restrictiveness. Table 4.7 differentiates between the effects of various major types of reg-

Table 4.7: Distinguishing between types of regulation

Dependent variable:	FDI share		ln(*TFP*)		ln(VAD/*L*)	
	(1)	(2)	(3)	(4)	(5)	(6)
Licensing requirements in product	0.007**	0.007**	0.025	0.022	0.020	0.014
	(0.003)	(0.003)	(0.029)	(0.030)	(0.031)	(0.032)
Binding licensing requirements	−0.001	−0.002	−0.073**	−0.081**	−0.084**	−0.083**
	(0.004)	(0.004)	(0.037)	(0.037)	(0.039)	(0.040)
Specific bans and FDI limitations in product	0.002	0.002	0.002	−0.001	−0.006	−0.002
	(0.002)	(0.002)	(0.015)	(0.015)	(0.015)	(0.015)
Binding specific bans and FDI limitations	−0.008***	−0.008***	−0.014	−0.013	0.001	0.001
	(0.002)	(0.002)	(0.018)	(0.018)	(0.018)	(0.019)
Product-wide bans	−0.007*	−0.008**	−0.062**	−0.045	−0.067**	−0.052
	(0.004)	(0.004)	(0.030)	(0.031)	(0.031)	(0.032)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year interactions	Yes		Yes		Yes	
Industry-year interactions		Yes		Yes		Yes
Island-year interactions	Yes	Yes	Yes	Yes	Yes	Yes
Product traits in 2005 × Year	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant product traits	Yes	Yes	Yes	Yes	Yes	Yes
Firm traits specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	180,797	180,783	180,797	180,783	180,797	180,783
Firms	24,726	24,725	24,726	24,725	24,726	24,725
R-squared	0.871	0.872	0.810	0.812	0.735	0.738

Note: The dependent variable is the foreign capital share within each firm, log TFP or log value added per worker. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

ulatory instruments by contrasting licensing requirements with other more direct bans and limitations, separated into binding and product regulation. Licensing requirements leave the affected sectors open to FDI but increase the burden of compliance by introducing costly and time-consuming procedures. In comparison, the various direct bans and limitations aim at restricting FDI flows more generally, either by closing off FDI for entire product categories or limiting FDI conditional on further firm-specific characteristics. We lose statistical precision of some estimates due to the fragmentation of our measure into different regulatory instrument types Hence, table 4.7 additionally reports coefficients controlling for less strict two-digit sector-year fixed effects in columns 1, 3 and 5, in order to keep more identifying variation in the model.

The results show that while binding licensing requirements did not curb foreign investment, all other types of binding bans and limitations did reduce FDI (columns 1 and 2 of table 4.7). Both outcomes seem plausible. While bans and limitations were used from the beginning, some limitations have been substituted by licensing requirements only in later years. As licenses played a role in the partial de-regulation process, they may have also been seen as successful in restoring openness to these sectors. However, this does not imply that licensing was costless in productivity terms. Estimates of the productivity effects of these different regulatory instruments show similar relative productivity losses both due to the introduction of binding licensing requirements and due to general bans (of about 5 to 8%, in columns 4 to 6 of table 4.7). By contrast,

Table 4.8: Transmission channels of de-regulation

	Regulated product		Binding regulation		Deregulated product		Binding de-regulation	
	Coeff	SE	Coeff	SE	Coeff	SE	Coeff	SE
FDI share	0.005***	(0.002)	−0.008***	(0.003)	−0.001	(0.005)	−0.000	(0.003)
ln(*TFP*)	0.002	(0.015)	−0.034*	(0.019)	−0.032	(0.043)	0.009	(0.022)
ln(VAD/*L*)	−0.003	(0.015)	−0.028	(0.019)	−0.086**	(0.043)	0.008	(0.021)
ln(*K*)	0.004	(0.019)	0.037	(0.025)	0.067	(0.070)	0.034	(0.031)
ln(Foreign *K*)	0.130***	(0.038)	−0.203***	(0.054)	0.017	(0.104)	−0.017	(0.065)
ln(Domestic *K*)	−0.008	(0.036)	0.100**	(0.045)	0.069	(0.109)	0.057	(0.056)
ln(Private *K*)	−0.074*	(0.045)	0.078	(0.056)	0.069	(0.152)	−0.015	(0.066)
ln(Gov.t *K*)	0.062**	(0.031)	0.052	(0.041)	0.099	(0.098)	0.073*	(0.043)

Note: The dependent variables are listed in the first column. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

firms that were subject to binding regulation conditional on firm-specific characteristics show substantially smaller and always insignificant reductions in productivity. This may reflect a larger flexibility in the enforcement of firm-specific regulations. In the case of firm size, we do not find any evidence for firms endogenously adjusting their sales or net assets to affect their firm size status. As mentioned before, we also take median firm size for each firm to further alleviate concerns of endogenous selection into regulation. Firm-specific regulatory effects could also be attenuated due to measurement error in the firm-specific characteristics (firm size, legal status, and previous ownership shares) that we use to define exposure to regulation.

Testing deregulation

In our main specifications, the regulation indicator turns to zero upon de-regulation, which implicitly assumes symmetric effects of regulation and de-regulation. However, it is ex-ante unclear why the effect should be equal in both directions. For instance, Davies et al. (2016) show that employment and capital growth within Jordanian firms react asymmetrically depending on whether foreign investors increase or decrease their shares. Table 4.8 therefore simultaneously tests for the impact of being regulated and de-regulated after a period of protection in order to contrast it with our baseline results.[44] We do not find evidence for an immediate relative impact of binding de-regulation, whereas binding regulation still affects foreign capital and productivity negatively. Notably, state-owned capital significantly increases in firms facing binding de-regulation as compared to other firms in de-regulated products. In general, it seems that the effects of binding de-regulation, if any, may need an even longer time to materialize. This finding is evidence against the assumption of symmetrical effects of closing and re-opening the economy for FDI.

[44] 3,308 firms experience de-regulation in our sample whereas 6,455 firms shift into being newly subject to binding regulation.

By contrast, the spillover effect of product de-regulation on productivity is negative. While puzzling at first sight, this result fits into the narrative of changing capital composition. Domestic capital, particularly state-owned capital, keeps on flowing into "strategic" products even after de-regulation while foreign investors seem to hesitate. Even though our coefficients are imprecisely estimated, this supports the idea of the inferiority of domestic capital in terms of productivity.

4.5.5 Further robustness checks

Firm entry and exit

Although our results are identified within the same firms and hence are less likely to be driven by shifts in firm composition, it is still important to understand whether firm composition endogenously adjusted in response to the revision of the NIL. Protection of a sector may keep out new entrants or reduce the exit rate of firms. For instance, Bonfiglioli et al. (2019) show that financial frictions increase entry costs, thereby allowing non-competitive firms to stay in the market. Conversely, regulations may also negatively impact firms by forcing them to leave the market or increasing the incentives for new firms to enter the market.

It is not clear in which direction the effect will go ex-ante, but the resulting shifts in firm composition may affect average firm productivity. Column 1 of table 4.9 documents that binding regulation indeed reduced the probability of market entry by new firms in the same period, whereas product regulation itself is not linked to changes in market entry. By contrast, all firms producing a regulated product were more likely to exit the market in the next period, irrespective of whether they were facing binding regulation themselves or not. Since the exact year of entry or exit may be mis-measured (as only firms with at least 20 employees are included in the census), we cannot provide a more detailed analysis of true market entry and exit dynamics. Instead, we test for a differential response to regulation among those firms that either entered or exited the sample. Columns 3 and 4 show that FDI adjustments of binding and product regulation are attenuated among newly entering firms but not among exiting firms. As firm exit and entry are endogenous, this may reflect selection effects. With respect to productivity, we see in general no differential changes in entry or exit of firms after regulation (except for the value added per worker increasing with product regulation among exiting firms in column 8). Most importantly, the substantial declines in productivity upon binding regulation persist and are fairly stable compared to our main findings. Overall, these results show that entering and exiting firms did not react more negatively (or potentially experienced less short-run adjustments) upon regulation. This makes it unlikely that the average results would be driven by entry or exit dynamics.

Table 4.9: Robustness: Exit and entry of firms

Dependent variable:	Entry	Exit	FDI share		ln(*TFP*)		ln(VAD/*L*)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regulated product	−0.002	0.008***	0.008***	0.006***	0.005	−0.003	0.001	−0.008
	(0.001)	(0.003)	(0.002)	(0.002)	(0.019)	(0.015)	(0.019)	(0.015)
Entry firm × regulated product			−0.005*		−0.001		0.002	
			(0.003)		(0.023)		(0.023)	
Exit firm × regulated product				−0.004		0.046		0.048
				(0.003)		(0.030)		(0.030)
Binding regulation	−0.003	0.001	−0.012***	−0.008***	−0.041*	−0.035**	−0.041*	−0.036**
	(0.002)	(0.004)	(0.003)	(0.002)	(0.022)	(0.017)	(0.022)	(0.018)
Entry firm × binding regulation			0.008**		0.009		0.027	
			(0.003)		(0.028)		(0.029)	
Exit firm × binding regulation				0.002		−0.016		0.032
				(0.004)		(0.035)		(0.036)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product traits in 2005 × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant product traits	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm traits specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	180,783	180,783	180,783	180,783	180,783	180,783	180,783	180,783
Firms	24,725	24,725	24,725	24,725	24,725	24,725	24,725	24,725
R-squared	0.255	0.385	0.872	0.872	0.812	0.812	0.737	0.738

Note: The dependent variable is an entry (exit) indicator turning 1 if a firm enters (leaves) the sample in t, the foreign capital share within each firm, log TFP or log value added per worker. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Product switching behavior

A different, and potentially more serious, concern is that firms endogenously decide on whether they want to operate in a regulated sector or switch to a non-regulated product. For instance, Utar (2014) shows that a firm's product mix is endogenous to rising import competition. In our context, this could lead to changes in the main reported product. Such product switching could bias our estimates, but the direction of the bias is a priori unclear. It is equally possible that firms either select into newly protected sectors or choose to operate in non-regulated sectors. Moreover, as SI firms may produce multiple products but only report their main product in our dataset, product switches may simply reflect the changing importance of a product that still is retained in the product portfolio. Finally, direct avoidance behavior due to misreporting products is unlikely to play a role in our setting as the firm census is not used by the authorities to explicitly monitor firms (Blalock and Gertler 2008).

Table 4.10 addresses the product switching behavior. Our dependent variable in the first two columns is an indicator for a product switch that takes one if a firm changes its reported five-digit product code in year t. Column 1 shows no evidence for a product switch occurring in year t as a response to contemporaneous regulation, hence firms did not switch *into* protected sectors. Column 2 looks at the response to regulation in year $t-1$ instead, testing for whether firms actively selected *out of* regulated sectors. Indeed, we see some evidence that binding regulation induced movement out of a product market whereas more firms entered markets for regulated products if regulation for them was not binding. However, beyond the robust negative effect of binding regulation on FDI and the positive spillover effects of product regulation, FDI shares did not change upon sector switches (column 3). When distinguishing between sector switches into or out of regulated sectors, and within currently regulated or non-regulated sectors, neither type of sector switching behavior was linked to changing FDI shares (column 4). Columns 5 and 7 show more pronounced differences for TFP and value added per worker: firms that had recently switched sectors experienced a productivity decline by 2.4–2.6% in the next period. This seems plausible as switches may require changes in the production process at the cost of initial productivity losses. As before, the direction of the switch matters (see columns 6 and 8). While the average effect of binding regulation is still negative and significant, firms switching into or out of regulated sectors did not experience changes in productivity. Only firms switching within non-regulated sectors saw significant productivity declines. This shows that it is very unlikely that switching behavior across sectors would drive our findings.

Table 4.10: Robustness: Product switching behavior

Dependent variable:	Switch in	Switch out	FDI share		ln(*TFP*)		ln(VAD/*L*)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regulated product	0.007	−0.015**	0.005***	0.005***	0.004	0.004	0.001	0.001
	(0.008)	(0.007)	(0.002)	(0.002)	(0.014)	(0.014)	(0.015)	(0.015)
Binding regulation	−0.002	0.018**	−0.008***	−0.009***	−0.037**	−0.042**	−0.030*	−0.033*
	(0.007)	(0.007)	(0.002)	(0.002)	(0.016)	(0.017)	(0.016)	(0.018)
Sector switch			−0.000		−0.024***		−0.025***	
			(0.001)		(0.006)		(0.006)	
Switch into regulated sector				0.002		−0.004		−0.011
				(0.002)		(0.020)		(0.020)
Switch into nonregulated sector				−0.000		−0.020		−0.021
				(0.003)		(0.021)		(0.021)
Switch within regulated sectors				0.004		0.013		−0.010
				(0.006)		(0.041)		(0.041)
Switch within nonregulated sectors				−0.000		−0.027***		−0.028***
				(0.001)		(0.007)		(0.007)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product traits in 2005 × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant product traits	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm traits specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	180,783	180,783	180,783	180,783	180,783	180,783	180,783	180,783
Firms	24,725	24,725	24,725	24,725	24,725	24,725	24,725	24,725
R-squared	0.377	0.378	0.872	0.872	0.812	0.812	0.738	0.738

Note: The dependent variable is a product switch indicator turning 1 if a firm switches its operating sector in t, the foreign capital share within each firm, log TFP or log value added per worker. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Alternative TFP estimates

As a last robustness check, table B11 in the appendix assesses the sensitivity of our baseline results to our TFP estimation procedures. The first two columns of the table repeat the baseline TFP estimates, calculated at the two-digit level, with and without further time-variant controls. Two further columns test the sensitivity of the two-digit TFP estimates by exchanging the common wholesale price deflators used for the baseline results with five-digit product-specific price deflators. These include a five-digit wholesale price index used to deflate firm sales, a five-digit input price index used to deflate intermediate inputs, and a machinery price index, used to deflate the capital stock and net assets (for identifying large firms). Columns 3 to 4 of table B11 fully replicate our main results. Columns 5 to 6 of table B11 are based on a more disaggregated TFP estimation instead. Here the TFP regressions are separately estimated for each three-digit industry, even though a few sectors have to be combined because of insufficient number of observations. The results show that our preferred specification yields almost the same regulatory effects on TFP irrespective of sectoral detail in price deflators and the sectoral aggregation for TFP estimation. All in all, we prefer to use the two-digit TFP estimates (together with the aggregated wholesale price deflator) in our main models as the higher level of detail in sectors and price deflators comes at the cost of a loss in precision. Due to the relatively lower number of firms operating in some three-digit industries, input coefficients estimated at the three-digit level tend to be more unstable and some of them even turn negative, which does not happen at the two-digit level (cf. table B2). Moreover, five-digit sector-specific output and input price indices, as well as the machinery price index are only available to us until 2012 and must be imputed for the following years by assuming proportionate sectoral price variation. As our fully specified results do not change when using more detailed TFP estimates or more detailed price deflators, we interpret this as supportive of our more aggregated TFP estimation approach.

4.6 Conclusion

This paper contributes to the literature on the effects of product-specific regulation of foreign investment. Despite its relatively open FDI regime, the Indonesian government uses the instrument of a negative investment list to restrict future foreign investment in certain product markets. In a clear instance of regulatory tightening, it has increased the number of regulated products very substantially in 2007. Empirical results show that the government particularly targeted not only products that were previously more exposed to privatization, but also products with a larger share of state-owned firms that were less productive on average, and products where firms accumulated more capital in the past.

We measure the effects of this product-level regulation, exploiting the revisions of the NIL in 2007, 2010 and 2014. Our identification strategy relies on fixed effects at the firm, region-year, and three-digit industry-year level and controls for tariff and non-tariff measures. Moreover, we allow yearly FDI dynamics and productivity changes to be proportionate to an extensive set of five-digit product and firm characteristics in order to control for classical political economy factors that could drive product-level variation in regulatory action. An examination of the time pattern of productivity changes helps to exclude the possibility that the effects of regulation merely reflect differences in pre-trends.

We find robust evidence showing a substantial effect of FDI restrictions on foreign ownership shares both within directly affected firms as well as their competitors. FDI shares increased among non-regulated firms operating in regulated product markets due to spillover effects, which was accompanied by somewhat larger differential decreases in FDI shares among directly regulated firms. Analyzing the relationship between regulation by the NIL and firm-level productivity, we find relative declines in TFP and value added per worker upon binding investment regulation. Regulated firms experienced productivity decreases of about 3% as compared to non-regulated firms within the same product market, starting in the year following the regulatory change. The productivity declines cannot be mechanically explained by a drop in foreign capital shares. Instead, we see that productivity declines were concentrated within industries that rely more strongly on external financing and those more technology intensive. As the drop in foreign capital was fully compensated by an increase in the value of domestically owned firm assets, the average results are unlikely to be driven by a simple shortage of capital. Instead, the domestically supplied capital may have been an imperfect substitute for foreign capital and may have contributed less to firm productivity.

Our results indicate that the Indonesian NIL has been very successful in shifting domestic investment towards the newly identified strategic industries and especially towards regulated firms, while domestic private investment has been shifted away from non-regulated firms producing regulated products. Moreover, regulated products generally experienced increases in state-owned capital. However, our empirical results also emphasize that restricting foreign ownership in sectors deemed to be important domestically is likely to come at the cost of efficiency losses in the form of productivity declines among affected firms.

CHAPTER 5

Regulating FDI in Indonesia's manufacturing sector: Local labor market responses to a protectionist policy

Robert Genthner and Krisztina Kis-Katos[45]

Abstract

Using labor market data from the Indonesian firm census as well as yearly household-level surveys, we investigate the effects of a protectionist foreign direct investment (FDI) policy reform on employment rates. The so-called negative investment list regulates FDI at the highly granular product level and has been repeatedly revised throughout time. We construct spatial measures of regulatory penetration of FDI restrictions within the manufacturing sector based on firm-level data. Our findings suggest that regions that were more exposed to this protectionist policy, experienced increases in employment that were most likely due to reduced competitive pressure. The employment gains concentrate among small manufacturing firms and also show substantial spillovers from the manufacturing to the service sector.

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5.1 Introduction

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

The distributional effects of trade liberalization have been in the focus of a very rich literature that investigates the impact of tariff reductions on firms, manufacturing industries or local labor markets in both developing and industrialized countries.[46] A common finding of these studies is that lower tariffs in general stimulate firm productivity, whereas particularly output tariff liberalization results in overall negative labor market effects and increasing competition.[47] A second strand of literature investigates the relationship between FDI (or its regulation) and firm outcomes, as well as domestic employment. Studies find that firm productivity is positively affected by both direct FDI (Aitken and Harrison 1999, Eppinger and Ma 2019, Javorcik and Poelhekke 2017) and indirect FDI spillovers along the value chain (Javorcik 2004, Genthner 2021). A substantially smaller number of studies documents an overall negative firm productivity effect of anti-competition reforms (Bourlès et al. 2013) or FDI regulation (Duggan et al. 2013, Genthner and Kis-Katos 2019). At the same time, the literature emphasizes a positive link between FDI and domestic employment since multinational enterprises (MNEs) own larger plants on average and employ more workers (Arnold and Javorcik 2009). Foreign firms also tend to formalize employment and pay higher wages to their employees (e.g., Harrison and Rodríguez-Clare 2010, Lipsey et al. 2010, Steenbergen and Tran 2020). By introducing advanced technologies, FDI increases the demand for high-skilled workers, resulting in a larger skill wage gap (Feenstra and Hanson 1997, Figini and Görg 2011, Lee and Wie 2015). However, foreign acquisitions are also likely to introduce a more efficient use of labor, which could result in a lower demand for employment (Girma 2005). Moreover, FDI may destroy jobs in domestic firms by in-

[46] See among others Amiti and Konings (2007), Lileeva and Trefler (2010), Topalova and Khandelwal (2011), Arnold et al. (2016) on firms, Goldberg and Pavcnik (2005), Hakobyan and McLaren (2016) on manufacturing industries or Autor et al. (2013), Dix-Carneiro and Kovak (2017, 2019) on local labor markets.

[47] For Indonesia, Kis-Katos and Sparrow (2015) and Kis-Katos et al. (2018) document positive labor market consequences of input tariff liberalization due to increasing labor demand.

creasing the overall competition in the local market and crowding out less productive companies (Melitz 2003, Jenkins 2006). The net employment effect thus depends on which of the above-mentioned channels dominates.

We expect FDI de-liberalization to revert the above outlined effects of FDI on labor market outcomes. At the extensive margin, shielding local markets from foreign investors could result in market entry of domestic firms due to reduced competition. Employment generation by new local firms may outweigh the negative employment effect due to reduced foreign investment. At the same time, already existing domestic firms may expand production at the intensive margin to take over market shares from regulated foreign firms. Similarly to the effects of place-based interventions, such targeted anti-competitive policies could spill-over to non-targeted firms as well (Neumark and Simpson 2015), generating broader employment effects. Despite the potential employment gains, FDI regulation may still involve a higher degree of informality as well as more low-payed jobs under the assumption of effect symmetry (Harrison and Rodríguez-Clare 2010).

In this paper, we link an FDI regulation policy protecting manufacturing national champion industries to local labor market outcomes in Indonesia, measured at the level of regencies (*kabupaten*) and cities (*kotamadya*), which we jointly refer to as districts. The negative investment list (NIL) is released in the form of Presidential Decrees and contains information on five-digit products that are subject to FDI inflow restrictions. Its conditions vary in intensity and range from soft licensing requirements to hard investment bans for some products. Some of the restrictions are conditional on firm characteristics such as size, legal status, and prior FDI shares. The list was first released in 2000 and then revised several times over the later years. Most importantly for our analysis, the regulatory environment was strongly tightened in 2007, when the list was massively extended and plenty of new products were added. A second revision in 2010 changed the range of products, also altering some of the restrictions, while a later revision in 2014 has de-regulated the investment regime to some extent. This has induced substantial spatial variation in the strictness of the locally relevant investment environment in manufacturing firms over time, which our empirical analysis exploits. For our analysis, we assess the local regulatory penetration (LRP) of this policy by combining policy information from the Presidential Decrees with firm-level and labor market data. We use a shift-share approach, interacting the initial share of the potentially directly affected local labor force with regulatory shifts over time.

We measure labor market dynamics in two ways. For our main analysis, we use the Indonesian Economic Census that covers all enterprises in manufacturing and services and consistently reports their number of workers. This allows for a detailed analysis of sectoral dynamics with respect to total employment, firm size and number of firms.

Based on this census data, we regress changes in labor market outcomes over ten years on the change in LRP between 2006 and 2010, which captures the major regulatory tightening. We complement these results by a long district-level panel derived from the annual household surveys *Susenas* and the labor market surveys *Sakernas*. The yearly structure of the data enables us to run panel regressions of local labor market outcomes on the time-variant district-level LRP measure, while controlling for a rich set of fixed effects and time trends in initial district conditions.

One potential reason behind the lack of studies on the link between protectionist policies and labor market outcomes lies in the difficulty of building a convincing identification strategy. Many studies argue that the extent of trade liberalization is dictated by international organizations such as the International Monetary Fund (IMF) for India (Topalova 2010) or the accession to the World Trade Organization (WTO) for Indonesia (Kis-Katos and Sparrow 2011). Similarly, Autor et al. (2013) exploit the rapid rise of China after its WTO accession to estimate its impact on US labor markets. Such sudden trade policy changes that are determined by the initial levels of sectoral protection allow for a convincing identification of causal effects. However, when dealing with variations in FDI policy or other non-tariff barriers in general, the line of argumentation is substantially less straightforward. Policy makers react to changes in the economic environment and, thus, estimated coefficients are not only driven by the policy response but may also reflect other underlying location-specific economic dynamics that may have triggered the policy intervention in the first place.

In the case of the negative investment list in Indonesia, Genthner and Kis-Katos (2019) indeed show that there is a whole range of political economy factors that potentially explain the choice of protected manufacturing industries. However, their results also indicate that past labor market dynamics barely figure among the factors explaining the product-level targeting of regulation. Instead, the sectoral presence and past productivity of public enterprises has shaped the decision to include selected products in the negative investment list. In order to alleviate concerns that endogenous policy formation and omitted variables are driving our results, we check for pre-trends in the main outcomes using repeated yearly long-difference regressions. Our baseline specifications further allow districts to be on different trajectories depending on the initial levels in regulatory penetration and by employment shares in manufacturing, agriculture and services. We also test for the robustness of our results by including a rich set of further controls, both in form of initial conditions and by introducing time-variant control variables. In particular, we control for political economy factors like lobbying (driven by industrial concentration) or privatization (captured by the share of and change in state-owned employment), exposure to changing trade flows (based on import and export flow data), trade liberalization (in the form of average input and output tariffs and non-tariff measures), trends in automation (measured by the stock of industrial robots), agglomeration effects (measured by initial population density or its pre-reform change) and labor market reforms (reflected in minimum wage legislation).

A recent strand of the literature discusses validity concerns in shift-share instrument designs. Our robustness checks address the most common arguments, even though our main specifications only exploit a shift-share measure in reduced form regressions. In particular, we control for unobserved shocks common to local labor markets that started off with similar initial employment composition by including the initial employment share of agriculture, manufacturing and services (interacted with time trends in the panel setting) in our baseline specifications (Borusyak et al. 2021). We further check if serial correlation across districts with similar employment structure leads to excessively small standard errors and, thus, over-rejection of the null hypothesis (Adão et al. 2019). Finally, we investigate if regulation in particular sectors drives our findings and whether results change once we exclude those sectors from the analysis (in the spirit of Goldsmith-Pinkham et al. 2020).

Our results indicate that firm employment (based on the Economic Census) increased in those districts that were most affected by the new restrictions on manufacturing FDI. On average, a one standard deviation increase in regulatory penetration increases the overall district employment rate by 1.3 percentage points in the long-run. Employment increases are found not only in manufacturing but also in services, highlighting the importance of cross-sectoral spillovers (Neumark and Simpson 2015, Dix-Carneiro and Kovak 2019). Gains in manufacturing employment are entirely driven by market entry of new small firms, leading to a shrinking average firm size in the affected districts. In contrast, increases in service employment originate at the intensive margin where existing larger firms hire new workers. These findings are in line with the expectation that tightening FDI regulation will invert the labor market effects of FDI presence. We see that, on average, more directly affected manufacturing firms get smaller as reduced competition enables market entry of less productive small-scale enterprises. The overall positive employment effect is confirmed within the yearly district panel setting based on *Susenas*. Here, a one standard deviation larger exposure to FDI restrictions results in a yearly 0.3 percentage point increase in the employment share. At the same time, we do not find any evidence for wage or income effects. These findings can only be rationalized by a strongly increased demand for services by the regulated manufacturing firms. Small manufacturing firms are not able to produce particular services themselves and thus have to outsource them to domestic firms in the tertiary sector, thereby generating new job opportunities in services.

Our results are in line with previous studies on developing and transition economies, which focus on negative demand shocks due to trade liberalization. These studies show that workers in manufacturing industries or regions highly affected by trade liberalization often bear the adjustment costs by facing diminishing earnings or job losses in the short run (see, for example, Arbache et al. (2004) and Kovak (2013) on Brazil) but also in the long run (Dix-Carneiro and Kovak 2017). We show that a protectionist measure like FDI regulation can have the opposite effect and contribute to employment gains among the local population. Our results also contribute to the regional economic literature on labor market effects of place-based policies and demand shocks. Studies in this field highlight the importance of spillover effects on the local employment structure (see, for instance, Kline and Moretti (2014) for the US and Lu et al. (2019) for China). Shielding the economy against foreign investment enforces cross-sectoral demand linkages and results in substantial employment spillovers within districts.

The paper proceeds as follows. Section 5.2 introduces the institutional background of FDI regulation and the NIL in Indonesia. Section 5.3 presents the data and develops our measure of regulatory penetration. Section 5.4 describes our long-difference estimation strategy and main results, while section 5.5 confirms these findings using a panel regression approach. Section 5.6 discusses potential threats to identification as well as how the paper deals with them and also includes a set of robustness checks. Section 5.7 then presents an analysis of potential channels, heterogeneities and alternative labor market outcomes. Section 5.8 concludes.

5.2 Institutional context

Early steps towards opening the Indonesian economy to FDI already started in the first years after the end of the Sukarno regime. Both the Foreign Investment Law in 1967 and the constitution of the investment coordination board (*Badan Koordinasi Penanaman Modal*, BKPM) in 1973 were landmark reforms as they promoted more FDI and enabled potential investors to apply for investment projects at a central agency (Gammeltoft and Tarmidi 2013). The relaxations of the previously tight investment environment, however, were partially withdrawn at the beginning of the 1970s, when the Indonesian government finally succumbed to violent protests against foreign presence in particular industries (van Zanden and Marks 2012). Despite the resulting drop in FDI inflows, the ongoing oil boom ensured sufficient revenues to compensate for the lost FDI inflows. When oil prices collapsed in the 1980s, the government was forced to re-open the economy to FDI (van Zanden and Marks 2012). Major reforms in the 1990s converted Indonesia into "one of the most promising countries [for investment]" (Lindblad 2015, p. 225).

Increasing FDI inflows came to a sudden halt during the Asian financial crisis in 1997 that destroyed much confidence among the investors (WTO 1998). To restore its status as an attractive host for FDI, the government introduced fiscal incentives and established an anti-discrimination rule between foreign and domestic investors while also streamlining application procedures in the years after the crisis (WTO 2013). In contrast to these efforts of promoting FDI, however, the president also introduced a so-called negative investment list (*Daftar negatif investasi*, NIL) in 2000, which listed products that are entirely closed or only conditionally open to FDI, requiring licensing or the formation of joint ventures.[48] While the release of such a blacklist improved the transparency of previously unclear procedures (WTO 2013), it also constitutes a protectionist policy. Thus, Indonesian FDI policy remained "blurred by contradictory signals" (Lindblad 2015, p. 229).

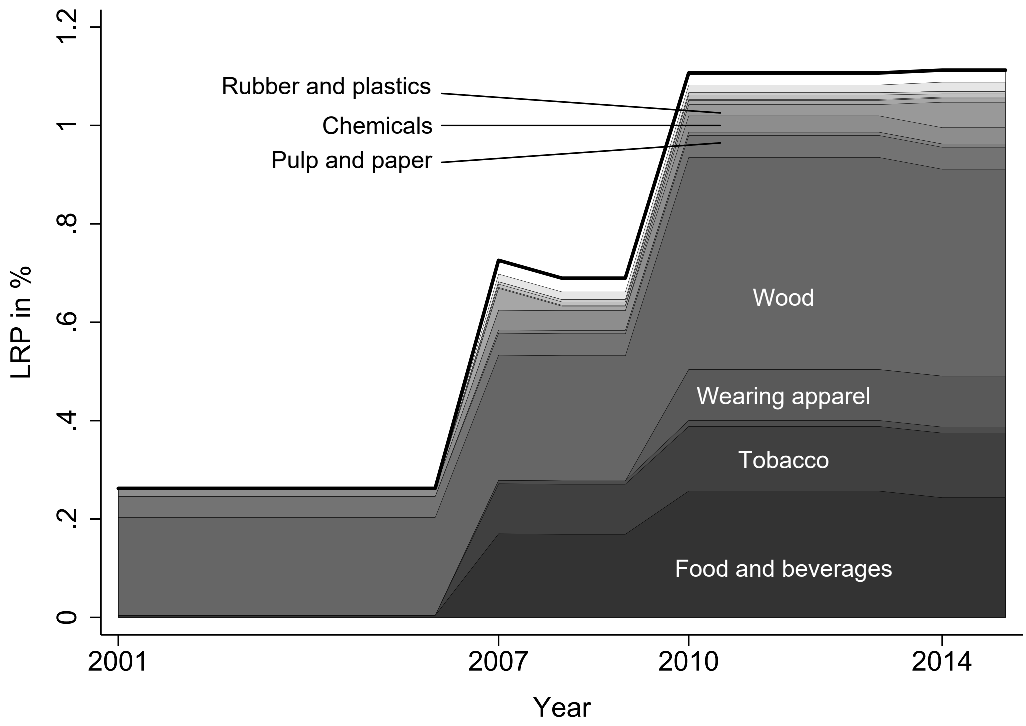
The list was repeatedly updated over the years. The first revision took place in 2007 (with Presidential Decree 77/2007) and extended its product coverage substantially, leading to a more restrictive regulatory environment. While the first NIL in 2000 only included conditions on licensing requirements and the prerequisite to form a joint venture with a domestic enterprise, the NIL 2007 also widened the scope of potential conditions to investment. FDI may still be entirely prohibited in some products, while in others it is restricted to small and medium-sized enterprises, to partnerships, limited to a certain threshold of foreign capital participation, to particular provinces, or it requires a licensing permission by the ministry in charge.

Figure 5.1 shows the changes in average stringency of FDI regulation in the manufacturing sector across Indonesian regions (measured by LRP, as described in section 5.3) over time. While regulation levels were low between 2000 and 2006, regulatory penetration rose steeply in 2007. After some minor adjustments at the beginning of 2008 (due to amendment 111/2007), the next major revision took place in 2010 (with Presidential Decree 36/2010), which extended the list of regulated products and changed some of the conditions. Overall, this resulted in a second strong increase in the LRP measure in 2010. The next revision (by Presidential Decree 39/2014) turned out relatively minor in comparison.[49] In a systematic analysis of the determinants of product-level regulation, Genthner and Kis-Katos (2019) show that FDI regulation is closely

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

[49] One important characteristic of all revisions of the NIL is that they only apply to future investments while existing foreign capital is untouched. Firms are not forced to divest but the regulation only interferes with future plans of investment and the product-specific investment environment. For instance, see article 8 in Presidential Decree 36/2010. See also Genthner and Kis-Katos (2019) for a more detailed description of the NIL and its conditions and coverage.

Figure 5.1: Sectoral composition of local regulatory penetration (LRP) over time



Note: The solid black line depicts average local regulatory penetration (LRP) from 2001 to 2015 based on equation (5.1). Shaded areas show the sectoral contribution to LRP based on regulated shares in the initial employment composition. Values are re-scaled by factor 100.

related to the presence of public enterprises in a market and prior privatization experiences (see table C1 in the appendix). For instance, products that experienced larger decreases in the share of state-owned firms at the beginning of the 2000s were more likely to become part of the NIL in 2007 and prior privatization turned out the most frequently significant predictor of product regulation. By contrast, none of the top ten predictors refer to prior labor market outcomes or dynamics within the product market. Hence, we consider it unlikely that labor market considerations contributed to the use of this regulatory instrument.

5.3 Data

5.3.1 Labor market data

We derive local labor market outcomes from three datasets provided by BPS: the Economic Census (*Sensus Ekonomi*), the national household survey (*Survei Sosial Ekonomi Nasional, Susenas*) and the national labor force survey (*Survei Angkatan Kerja Nasional, Sakernas*). While the Economic Census is only available once every ten years, the other two are collected annually as repeated cross-sections. The Economic Census covers the universe of all firms in the economy except for agriculture for the years 2006 and 2016. It therefore provides a full picture of economic activity across sectors and districts. Both census waves consistently collect firm-level information on the total number of workers. For our main results, we use this information to compute aggregate district employment in total and by firm size, as well as average firm employment and the number of firms in a district.[50] Table C2 in the appendix shows descriptive statistics. Between 2006 and 2016, we observe a rise in employment rates, mainly driven by services. The average firm size also increased by about 12%. However, this masks heterogeneous trends across sectors as manufacturing firms became substantially smaller, while the number of manufacturing firms strongly increased.

Our second data source, *Susenas*, provides annual representative population information on district level over the full analyzed time period (from 2001 to 2015), which allows us to also analyze local labor market dynamics in the years before and directly after the regulatory change. We rely on information on individuals' age and employment status, but also utilize information on individuals' gender, skill level, type of employment and migration status as well as household expenditures for further results. However, the household surveys are not designed to perfectly resemble sectoral employment composition on the district level, in particular in more remote and less densely populated areas. We thus only consider total employment rates when relying on household surveys. We complement this data with selected information from the labor force survey, *Sakernas*, including a more precise measure of the activity status, unemployment, working hours and hourly wages to estimate wage premia. Though more detailed, the labor market survey is only fully representative at the district level starting in 2007 and hence lacks a reliable measure of pre-reform dynamics. We rely on *Sakernas* to calculate the size of the initial labor force for our local regulatory penetration measure,[51] but also present some alternative, less precisely measured, labor market outcomes.

We restrict our attention to the working-age population (between the age of 15 and 64) and eliminate observations with missing values in crucial characteristics such as gender, educational attainment or age. We measure local labor market outcomes by aggregating all surveys to the district-year level. We also compute employment rates separately by gender, age and skill level. Table C3 in the appendix presents descriptive statistics.

[50] Due to an ongoing decentralization process, Indonesian districts repeatedly split over our sample period. To deal with changing district borders, we aggregate all data to the initial district boundaries of 2000. Note that our results are not driven by job creation due to decentralization (see appendix C.3).

[51] This allows for a more precise measurement of the working population potentially exposed to regulation as, unlike *Susenas*, it allows for identifying those who are active in the labor market. To improve data quality, we calculate time-invariant initial conditions by combining information over several pre-reform years and measuring median employment and population.

Figure C1 in the appendix shows a relatively steady increase in the working age population over time. To control for spatial heterogeneities in population dynamics, we focus on employment rates instead of employment numbers in our main specifications. Employment rates also increased over time, accompanied by substantial structural change and a relative expansion of the services sector at the costs of agricultural employment. The share of manufacturing employment remained relatively small compared to the other two sectors, but it stayed stable over time.[52]

5.3.2 Measuring local regulatory penetration

In our empirical models, we link changes in local labor market outcomes to regional level measures of the strictness of the regulatory environment in the manufacturing sector by combining policy data from the Presidential Decrees with data from the annual manufacturing census (*Survei Industri*, SI)[53] and the initial labor force based on *Sakernas* (as this information cannot be extracted from *Susenas*). In order to proxy for the extent of regulatory penetration within each Indonesian district, we construct locality-year-specific measures of local regulatory penetration LRP_{dt} in district d and year t:

$$LRP_{dt} = \sum_{kp} \frac{L^f_{kpd,0}}{L_{d,0}} REG_{kpdt}. \tag{5.1}$$

To isolate changes in local regulatory penetration, we apply a Bartik-style shift-share approach (Bartik 1991), interacting the initial share of the potentially directly exposed labor force with the regulatory shifts over time. The initial shares divide firm employment $L^f_{kpd,0}$ by firms of type k operating within the five-digit product group p and region d (derived from the *SI*) by the initial size of the local labor force $L_{d,0}$ (estimated based on the median value between 2000 and 2005 in the labor market surveys). As regulation is specific to a selected list of firm characteristics, we calculate initial employment shares not only by product p, but also by firm characteristics k. These characteristics include firm size (regulation often only applies to big companies), legal status (partnerships are often excluded from regulation) and shares of prior FDI ownership. The range of locations d is only relevant for the regulatory restriction in very few products in the wood sector, where regulation only applies to particular provinces. The initial time period $t = 0$ is based on the years 2000 to 2005, during which no

52 Additional labor market trends (based on the active population) derived from *Sakernas* in figure C2 in the appendix show generally similar dynamics.

53 The *Survei Industri* comprises the whole universe of manufacturing firms with more than 20 employees in Indonesia. The survey is conducted by BPS on an annual basis and was frequently used in other empirical studies (e.g., Amiti and Konings (2007), Blalock and Gertler (2008), see Márquez-Ramos (2021) for a survey). For exact details on data cleaning and the sample used, see Genthner and Kis-Katos (2019).

regulatory changes occurred. Most importantly, the shares should not be affected by endogenous employment adjustment dynamics due to later reforms. By calculating the median number of employees for each firm for the whole period from 2000 to 2005, we increase the precision of our share estimates. This increases the underlying number of firms and makes our firm employment measures more robust against outliers. We exclude districts for which the *SI* does not report any operating firms. To further reduce noise in the shift-share measure, we drop districts in the lowest five percent of the firm employment (L^f) distribution.

The time-varying policy shifts are derived from the policy instrument of the NIL. The indicator variable REG_{kpdt} takes the value of one if firms of type k that produce the primary product p and operate within region d are included on the investment blacklist in year t and zero otherwise. All time variation in LRP_{dt} thus originates from revisions of the NIL. Revisions may extend (or shorten) the list by adding new products p (or removing existing ones). Additionally, REG_{kpdt} may also turn one if regulation of product p is extended to include hitherto unregulated firms of type k. For instance, products of coloring yarns using natural or man-made fibers (17115) were added to the list in 2007, making it only conditionally open to investment within small and medium-sized firms. Revisions in 2010 and 2014 did not change this condition.[54]

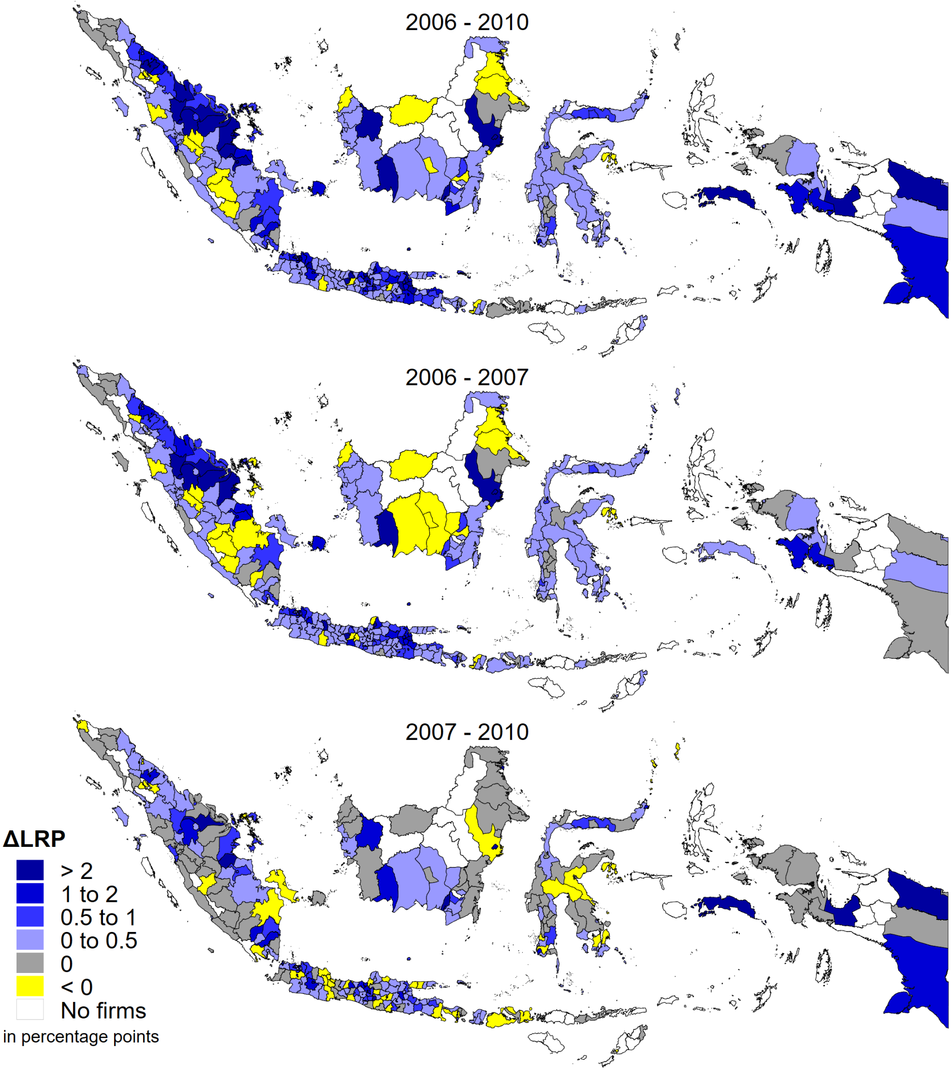
The average development of LRP over time is depicted in figure 5.1. The upper thick line in the graph shows a step-wise increase in the overall regulatory penetration after each of the two major revisions (in 2007 and 2010). For our estimation strategy, we exploit the yearly change in LRP for each district while also using the total change between 2006 and 2010 in the long-difference specifications with data from the Economic Census. To ease interpretation, we multiply LRP by 100 to represent the percentage of local workers potentially directly exposed to FDI regulation. On average, LRP increased by 0.84 percentage points between 2006 and 2010 (see table C2).

Figure 5.1 also shows the contribution of each industry to total manufacturing LRP over time. It splits LRP into its sectoral components, reflecting the initial share of industrial employment in total labor force and the shifts in regulation over time. Wood and wood products make up a substantial part of regulatory penetration, but there are also other sectors that contribute to LRP on a nation-wide scale (e.g., the food and beverage industry, tobacco products or wearing apparel). A complete detailed list of sectors can be found in table C5 in the appendix. There are several industries that are not affected by the NIL at all, such as leather products or motor vehicles.

Figure 5.2 maps the spatial distribution of changes in LRP for the period from 2006 to 2010, as well as separately by the two major revisions (2006 to 2007 and 2007 to 2010).

[54] We focus on regulation in manufacturing in our baseline specifications, but also show results for service sector regulation in table C4 of the appendix.

Figure 5.2: Change in LRP between 2006 and 2010



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

In most districts, we observe a tightening of the regulatory environment from 2006 to 2010. Declines in the LRP in the first round of revisions concentrated on the islands of Sumatra and Kalimantan, while regulation tightened especially on Java. Between 2007 and 2010, LRP further increased in about half of all districts, while other districts experienced declines at the same time. This is the spatial and temporal variation that underlies our identification strategy.[55]

Beyond its spatial distribution, regulatory policy may still be clustered within particular districts due to the spatial concentration of products (cf. Neumark and Simpson 2015). However, descriptive statistics in table C6 in the appendix do not show any evidence of such a regulatory clustering. The average number of five-digit products

[55] We also report the spatial distribution of the LRP levels for the years most relevant to our estimation strategy in figure C3 of the Appendix. Figure C4 further shows the density distribution of LRP.

produced within one district is 20.8, while 6.5 of those are regulated. Despite its right-skewed distribution, there are still only a few districts hosting very few products. This means that variations in LRP are generally driven by many different products. Moreover, each five-digit product is produced in about 20 different districts on average. Again, this shows that most products are manufactured in several places and district-specific economic concerns should play a minor role in the selection of products that enter the list.

5.4 Structural change

5.4.1 Long-difference strategy

To estimate the effect of regulatory penetration on employment rates from aggregated Economic census data, our long-difference specification regresses changes in labor market outcomes in district d between 2006 and 2016 (Δy_d^{06-16}) on changes in the constructed LRP measure between 2006 and 2010 (ΔLRP_d^{06-10}). By that, we get a measure of regulatory tightening during the first two major NIL revisions (compare figure 5.1). We estimate the following regression:

$$\Delta y_d^{06-16} = \alpha_1 \, \Delta LRP_d^{06-10} + \boldsymbol{X}'_{d,0} \, \alpha_2 + \Delta \boldsymbol{Z}_d'^{06-16} \, \alpha_3 + \lambda_r + \varepsilon_d, \tag{5.2}$$

where island-group fixed effects λ_r rule out common trends by macro-regions. Standard errors are robustly estimated.

We additionally control for a set of initial local conditions within the vector $\mathbf{X}_{d,0}$. These initial district-level characteristics may both drive differences in regulatory exposure and labor market dynamics. Thereby, we allow districts to experience different changes in the employment structure depending on their initial situation. For our baseline specification, we include the initial level of regulatory penetration in 2006. More protected districts may be less responsive to a tightening of regulation and thus may react less strongly in terms of employment dynamics. As the LRP variable relates the number of regulated manufacturing workers to the total active population, the calculated shares do not add up to one but reflect the relative importance of manufacturing in local labor markets. If there were unobserved shocks over the years 2006 to 2010 that systematically differed across manufacturing and other sectors, this would bias the ΔLRP estimate. We thus also control for the initial share of manufacturing employment in the total working-age population to ensure that the estimate is purely driven by changes in the regulatory framework between 2006 and 2010 and not by the relative size of the manufacturing sector (cf. Borusyak et al. 2021). Descriptive trends in figure C1 reveal an ongoing structural change within the Indonesian labor market. We further include the initial employment shares of the agricultural and service sector to rule out that our results depend on the initial employment structure of a district.

Our identification strategy requires the absence of pre-trends in employment conditional on our baseline controls. To check for pre-trends, we present a full set of long-difference estimates based on the yearly *Susenas* data in section 5.6, showing the full time profile of the regulatory effect. Our robustness checks in section 5.6 further extend the set of initial conditions $\mathbf{X}_{d,0}$, and also add a list of time-varying controls for which we calculate the change between 2006 and 2016 ($\Delta \boldsymbol{Z}_d^{06-16}$). In particular, we address concerns that our results are driven by the global financial crisis or international trade, trends in automation or high-tech sectors, political economy factors such as lobbying, privatization or protection of vulnerable groups, as well as labor market reform. We further show that our results remain robust when controlling for measures of agglomeration or regulatory spillovers across districts.

5.4.2 Results

Table 5.1 shows the main results based on the long-difference estimations using the Economic Census data. Panel A reports the overall effect of the change in LRP from 2006 to 2010 on the change of the employment rate. The time period 2006 to 2010 covers both the initial revision of the NIL in 2007 and the subsequent adjustment of products on the list in 2010 (see also figure 5.2 of the appendix). Information on the number of employees additionally allows us to split up the impact of the regulatory change on self-employed (Panel B), small firms with 2-19 employees (Panel C) and medium/large firms with more than 20 employees (Panel D). Column 1 shows the effect size across all sectors while columns 2 and 3 report the effect on the change of the employment rate in manufacturing or services. All regressions rely on the long-difference baseline specification from equation (5.2) and thus include island indicators, as well as initial district characteristics in the pre-reform year 2006 (LRP and the share of manufacturing, agricultural and service employment). By that, we rule out that our results are driven by differential developments due to the initial level of protection in a district. Similarly, the initial share of manufacturing employment controls for unobserved heterogeneity across districts with respect to the relative importance of manufacturing (Borusyak et al. 2021). Finally, controlling for initial sector shares of agriculture and services ensures that our long-difference results are not driven by dynamics of structural change in the economy.[56]

[56] Table C7 in the appendix reports alternative specifications of the total employment rate result in Panel A, showing that our results do not depend on the inclusion of island indicators or initial conditions. Our results still hold when using total employment growth instead of employment rates in Panel B.

Table 5.1: Impact of regulatory tightening between 2006 and 2010 on the change in employment rates (Economic Census)

Dependent variable: ΔEmployment rate	Total	Manufacturing	Services
	(1)	(2)	(3)
Panel A: All			
ΔLRP 2006-2010	0.0095***	0.0042**	0.0054**
	(0.0035)	(0.0016)	(0.0025)
Panel B: micro firms (1 employee)			
ΔLRP 2006-2010	0.0007	0.0003	0.0003
	(0.0006)	(0.0002)	(0.0006)
Panel C: Small firms (2-19 employees)			
ΔLRP 2006-2010	0.0014	0.0020***	−0.0010
	(0.0014)	(0.0008)	(0.0013)
Panel D: Medium/large firms (20+ employees)			
ΔLRP 2006-2010	0.0074**	0.0017	0.0061***
	(0.0033)	(0.0014)	(0.0024)
Observations	298	298	298
Island FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes

Note: The dependent variable is the change in employment rates. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2006. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Results in Panel A indicate that a tightening of the regulatory environment between 2006 and 2010 leads to an overall employment increase. A one standard deviation increase in LRP results in a 1.3 percentage point increase in the employment rate. The effect partly originates from the direct protectionist effect of manufacturing regulation on employment generation in manufacturing. At the same time, however, column 3 shows that employment spillovers to services are of broadly comparable magnitude. Panel B does not show any significant effect on self-employment, whereas Panel C reveals that most of the employment increases in manufacturing are realized among small firms. There is no such reaction in small service firms and the full employment effect in services comes from medium or large enterprises (Panel D).

Table 5.2 provides further evidence of where and how employment creation takes place, contrasting the growth rate of the average firm size with the growth rate of the number of firms (of various sizes). All regressions control for population growth to make sure that results are not spuriously driven by population dynamics. Overall, we do not find significant results when focusing on total employment in column 1. However, there are clear heterogeneous effects across sectors. Columns 2 and 3 of Panel A show that employment generation in services is fully driven by the intensive margin, while manufacturing firms even reduce their number of employees due to an increase in local regulatory penetration. At the extensive margin, we find that FDI protection results in massive firm entry in manufacturing which is mostly concentrated among the self-employed and small firms. There are no effects on large manufacturing firms

Table 5.2: Impact of regulatory tightening between 2006 and 2010 on firm size and number of firms (Economic Census)

	Total	Manufacturing	Services
	(1)	(2)	(3)
Panel A: Dependent variable: Δasinh L per firm			
ΔLRP 2006-2010	0.0115	−0.0629**	0.0133**
	(0.0073)	(0.0247)	(0.0052)
Panel B: Dependent variable: Δasinh number of (all) firms			
ΔLRP 2006-2010	0.0084	0.1307***	0.0019
	(0.0053)	(0.0410)	(0.0055)
Panel C: Dependent variable: Δasinh number of micro firms			
ΔLRP 2006-2010	0.0108	0.0968***	0.0066
	(0.0085)	(0.0225)	(0.0086)
Panel D: Dependent variable: Δasinh number of small firms			
ΔLRP 2006-2010	0.0010	0.0430***	−0.0068
	(0.0085)	(0.0135)	(0.0091)
Panel E: Dependent variable: Δasinh number of medium/large firms			
ΔLRP 2006-2010	0.0076	−0.0025	0.0095
	(0.0164)	(0.0255)	(0.0171)
Observations	298	298	298
Island FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes
Δasinh population	Yes	Yes	Yes

Note: The dependent variable is the growth rate in average firm employment (Panel A) or the growth rate in the number of firms (Panels B to E). $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2006. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

and also service firms do not seem to react to regulation by neither market entry nor exit.

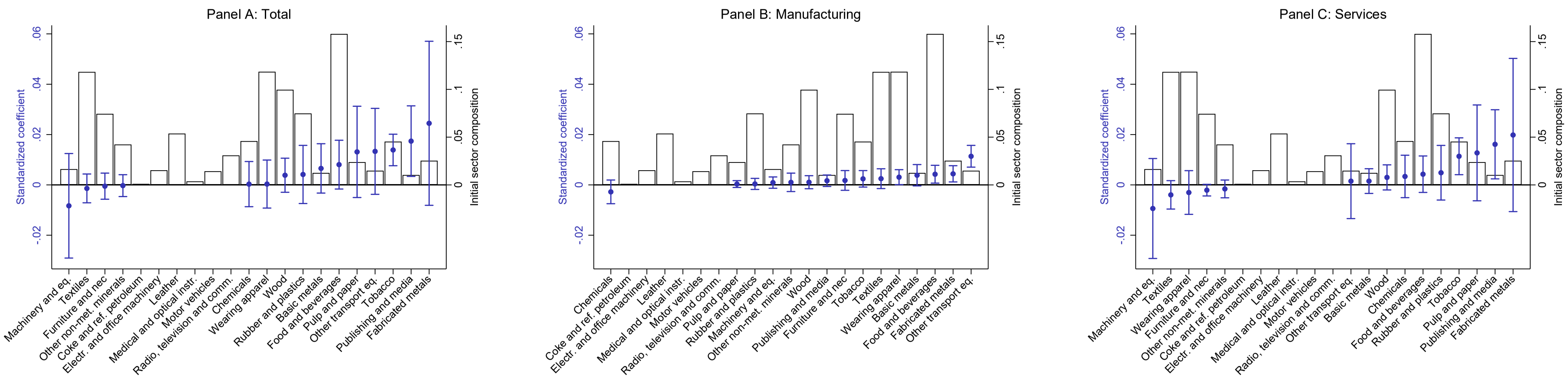
The results based on the Economic Census indicate that FDI protection of manufacturing firms directly increases manufacturing employment through market entry of small enterprises. The NIL thus successfully protects the manufacturing sector from foreign competitors and enables domestic entrepreneurs to start a new business. In contrast, self-employment does not substantially contribute to the overall increase in employment. Although the increase in the number of 1-person firms is statistically significant, they do not contribute a substantial employment mass. At the same time, our results detect a large employment spillover effect to the service sector. Shielding the manufacturing sector from foreign investors seems to increase domestic demand for services which in turn leads to additional hiring within medium and large firms. Our results support a high degree of sectoral integration and are in line with evidence by Dix-Carneiro and Kovak (2019) who find strong effects of tariff liberalization on the nontradable sector in Brazil. This suggests that FDI regulation increases outsourcing activities from manufacturing to the service sector. This is in contrast to evidence from developed countries showing that especially foreign-owned firms create forward demand spillovers to services (Girma and Görg 2004, Ascani and Iammarino 2018). On the contrary, Abraham and Taylor (1996) argue that firms start outsourcing if they are not capable to provide specialized services themselves. Small domestic manufacturing enterprises thus may be forced to rely more strongly on local services that cannot be provided in-house. Regulatory penetration then leads to a stronger demand for local services which can explain the observed employment increases.

The estimated increase in local employment reflects an average effect from tightening the regulatory environment for manufacturing FDI in medium and large enterprises. In the spirit of Goldsmith-Pinkham et al. (2020), this effect can also be decomposed by two-digit sectors to identify which manufacturing sectors drive this result. Bars in figure 5.3 show the initial employment distribution (also used for our shift-share measure) by two-digit sector. About 40% of manufacturing employment is concentrated in the production of food and beverages, textiles, and wearing apparel. The coefficients depict each two-digit sector's contribution in a standardized form to the main results of Panel A in table 5.1. They show that regulatory tightening in the majority of all sectors contributes to increases in district-level total employment rates, including among others fabricated metals, tobacco, as well as food and beverages. No coefficients are reported for the six sectors without any regulation (compare table C5), whereas the effects turn negative only in four sectors. To check whether our estimates are exclusively driven by the sectors that contribute most to the overall effect, we exclude the top three sectors (fabricated metals, publishing and media, tobacco) from the sample altogether (see Panel A of column 3 in appendix table C8), where the coefficient estimate is slightly reduced but remains statistically significant.

We further disentangle the two-digit industry contribution to the sectoral employment effects in Panels B and C of figure 5.3. We see a strong contribution of regulation in transport equipment, fabricated metals, as well as food and beverages to employment gains in manufacturing. Given the high share of initial employment in food and beverages (about 16%), employment rates in manufacturing particularly increase in districts with a dominant food industry. The overall gains are only reduced by employment losses due to regulation in the chemical industry. The main contributors to employment gains in services are again fabricated metals, publishing and media, and tobacco (in Panel C).

For our main analyses, we base our LRP measure solely on the regulation of medium-sized and large manufacturing firms as this can be measured more precisely and this group of firms is much more likely to receive FDI. However, as the NIL affects the whole economy, we also construct alternative, albeit somewhat less precise, measures of LRP for the full manufacturing sector (including micro and small enterprises) and including the service sector. Unfortunately, data limitations do not allow for a mean-

Figure 5.3: Sectoral decomposition of the impact of LRP on the employment rate (by sector)



Note: Panel A shows the effect on total employment rates by two-digit sector, while Panels B and C further disentangle the effect on manufacturing and service employment. Bars depict the share of initial employment in each two-digit sector. Plotted coefficients estimate the standardized sectoral contribution to the overall effect of ΔLRP on the change in employment rate. No coefficients are estimated for 6 unregulated sectors (compare table C5). The regression controls for the initial level of LRP before the first revision and the initial share of manufacturing, agricultural and service employment, as well as island indicators. Bars around the point estimates denote 90% confidence intervals for robustly estimated standard errors.

ingful measurement of LRP in agriculture. More details on the construction of this alternative measure are provided in the Appendix C.1. Panel A of Table C4 shows that the effect of manufacturing LRP based on economy-wide exposure of manufacturing firms is barely different from our main results. This is to be expected, since micro and small firms are very unlikely to be directly affected by FDI (and its regulation). When further adding regulation in the service sector to the LRP measure in Panel B, the total employment rate effect of FDI regulation in manufacturing and services is positive and larger in magnitude. It is predominantly employment in services that is driving employment gains, now also partly due to service sector regulation. However, increases in manufacturing employment rates are still directly contributing to the overall positive labor market effect. The alternative LRP measures, however, are very sensitive to outliers and ambiguity in the sector code conversion. We thus prefer to rely on our substantially more robust main measure of manufacturing LRP.

5.5 Labor market dynamics

For our complementary results based on household-level data, we link variation in LRP to total employment rates in district d and year t, y_{dt}. We thereby exploit yearly changes in regulatory penetration LRP_{dt} on district level. Our panel regressions take the form:

$$y_{dt} = \beta_1 \, LRP_{dt} + \mathbf{X}'_{d,0} \, \beta_2 \times t + \mathbf{Z}'_{dt} \, \beta_3 + \gamma_d + \phi_{rt} + \varepsilon_{dt}. \tag{5.3}$$

All regressions are conditional on district fixed effects, γ_d, and island-year fixed effects ϕ_{rt}. The error term ε_{dt} is clustered at the district level. To mirror our long-difference specification, we control for the same set of initial district conditions $\mathbf{X}_{d,0}$ and interact them either with a linear time trend or a full set of year fixed effects. By that, we make sure that dynamics in the initial level of regulatory penetration, the relative importance of the manufacturing sector, or the overall sectoral composition in a district do not spuriously affect our estimates.

Likewise, we also check for the robustness of our panel results in section 5.6 by allowing for linear trends in an extended set of initial conditions $\mathbf{X}_{d,0}$, or adding time-varying controls $\mathbf{Z}_{dt}$.

We use employment data from annual household surveys in order to verify and complement the long-difference evidence based on the Economic Census. This enables us to exploit yearly variation in regulation and employment rates within a district panel. Our preferred specification based on equation (5.3) includes district and island-year fixed effects, as well as interactions of linear time trends with the same set of initial

Table 5.3: Impact of local regulatory penetration on employment rates (*Susenas*)

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable: Employment rate					
LRP	0.0212***	0.0043***	0.0042***	0.0020**	0.0020**
	(0.0027)	(0.0011)	(0.0011)	(0.0009)	(0.0010)
Panel B: Dependent variable: asinh(Employment)					
LRP	0.0239***	0.0082***	0.0081***	0.0038**	0.0035**
	(0.0038)	(0.0019)	(0.0019)	(0.0015)	(0.0015)
asinh(Population)	1.1846***	1.0090***	1.0089***	0.9943***	0.9966***
	(0.0209)	(0.0202)	(0.0203)	(0.0170)	(0.0170)
Observations	4,339	4,339	4,339	4,339	4,339
District FE	Yes	Yes	Yes	Yes	Yes
Island-year FE		Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends			Yes	Yes	
$\mathbf{Sector}_{d,0}$-specific trends				Yes	
$\mathbf{LRP}, \mathbf{Sector}_{d,0} \times$ Year					Yes

Note: The dependent variable is the total employment rate. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

district characteristics like in the long-difference regressions. This allows districts to be on different trajectories depending on the pre-reform regulatory penetration and sectoral employment shares.

Table 5.3 presents our main results using *Susenas* as data source for total employment. The first two columns display the correlation between total employment growth and LRP, conditional on district and island-year fixed effects. Further controls are added step-wise in each column, starting with an interaction between a linear time trend and regulatory penetration in the pre-reform year 2005. Column 4 presents our preferred specification which additionally controls for trends in the initial sectoral employment shares in manufacturing, agriculture and services to control for dynamics of structural change in the economy. Column 5 finally allows districts to be on different nonlinear trajectories by interacting the above four initial conditions with a full set of year fixed effects.

Our results confirm the significant positive relationship between LRP and total employment shares based on the Economic Census. The estimated coefficient is substantially reduced in magnitude when we control for potentially different structural change dynamics in column 4. By contrast, flexibly allowing for nonlinear time trends in initial conditions does not alter the coefficient. Our preferred specification in column 4 yields an estimate of 0.002.[57] In terms of magnitude, a one standard deviation increase in LRP is associated with a 0.3 percentage point increase of the total employment rate. The size of the effect is economically meaningful. As the manufacturing employment

57 Results look similar for the level of total employment as dependent variable. Note that when regressing total employment on LRP, we also add the size of the working-age population as a time-variant control variable to account for population dynamics.

rate amounts to about 8% on average, and employment in large regulated firms relative to the initial local labor force is only 0.7% on average, employment increases of this magnitude are only feasible if protection has employment generating effects that go beyond the regulated manufacturing firms.

Alternatively, we also provide estimation results using a Bartik-style instrument in table C9 that generally point in the same direction, linking regulation-induced decreases in FDI to higher employment levels. Details on the IV strategy are provided in Appendix C.2. In particular, the IV strategy relies on the assumption that the regulatory effect runs through adjustments in FDI stocks only and that the effect is stronger in districts with a higher past likelihood to receive FDI (Nunn and Qian 2014). Since we are not able to provide strong evidence in support of the exclusion restriction and our instrument is relatively weak in the first stage, we only present these findings as further suggestive evidence.

5.6 Robustness

5.6.1 Possible confounders

Since FDI regulation is an outcome of the political process, our main results may reflect alternative economic dynamics that affected the scope of regulatory intervention and may even fully drive the estimated employment effects. A series of robustness checks helps to address these endogeneity concerns. We briefly summarize these concerns in the main text, whereas a detailed discussion of the robustness tests is provided in appendix C.3.

A vast literature discusses the political economy of trade policy (cf. Grossman and Helpman 1994, Goldberg and Pavcnik 2005, Asher and Novosad 2017). The main argument therein is that trade policy is endogenously determined within the political process. Particular industries and firms may lobby for policy changes that favor their own business while, at the same time, political incumbents face re-election motives that could make them sensitive to concerns of specific interest groups. Even though the Indonesian government did not explicitly state reasons that explain the selection of products which enter the NIL, Genthner and Kis-Katos (2019) show that there are certain product-level factors that predict changes in the regulatory environment. Table C10 in the appendix shows that our results are robust to controlling for a wide range of political factors such as market power, presence of state-owned companies and decentralization. For instance, market concentration reduces costs of coordination among incumbent firms and makes lobbying for their individual interests easier (Grossman and Helpman 1994). At the same time, national champion firms may be specially treated by policy makers due to their pivotal importance to the domestic economy. The historical presence of state-owned firms and the subsequent privatization process has been shown to be an important determinant of later FDI regulation (Genthner and Kis-Katos 2019). Finally, political decentralization may also drive employment dynamics as the creation of new governmental structures also provides new job opportunities (Bazzi and Gudgeon 2021). If the timing of district splits overlapped with changes in regulatory penetration, our results would spuriously capture the employment effects of decentralization.

Our baseline results do not correct for ongoing global dynamics that may also influence domestic employment in Indonesia. For example, our sample period covers the global financial crisis from 2009 which potentially affected trade-oriented districts more severely. Similarly, changes in tariff rates have been shown to impact domestic labor markets (Hakobyan and McLaren 2016, Dix-Carneiro and Kovak 2017). The increasing importance of automation in the industrial production process and the regional potential for technological upgrading are additional factors that may affect local employment, for instance through a reduction of routine-task jobs (Acemoglu and Restrepo 2019). Appendix table C11 tests for these concerns and finds that our main results do not change when controlling for the local exposure to global dynamics.

Our results are also not driven by underlying agglomeration or labor market dynamics (see table C12). Given that our LRP measure uses the initial presence of manufacturing employment as weighting factor, our results are particularly vulnerable to concerns that LRP only picks up the relative importance of agglomeration and not purely the changes in regulation over time. We thus control for a set of proxies that capture the regional agglomeration, measuring the size of the manufacturing work force that was never regulated within our sample period as well as population density and its growth. Given the rich literature on the employment effects of minimum wages (cf. Neumark and Munguía Corella 2021), table C12 further adds Indonesian minimum wages to our baseline specification. We also substitute island with province fixed effects because provincial governments are in charge of labor policy (and in particular minimum wage legislation, Widarti 2006). These very granular indicators absorb a lot of the variation of the regulation measures. Our long-difference estimate thus is strongly reduced in magnitude while the *Susenas*-based panel estimate remains robust and stable.

Internal migration within Indonesia is mostly driven by movements towards densely populated areas on Java and the province Lampung on South Sumatra, as these regions promise better living conditions and offer labor market opportunities (van Lottum and Marks 2012). The creation of new jobs in more strongly regulated districts may thus pull internal migrants away from locations that experience less protectionism. At the same time, incoming migrants also increase the labor supply in a district and may therefore help to satisfy excess labor demand.[58] Table C13, however, shows that internal migration is barely affected by LRP and, thus, allows us to discard migration as potential driving force behind the regulatory effects of employment increase.

5.6.2 Effect timing, pre-trends and spillovers

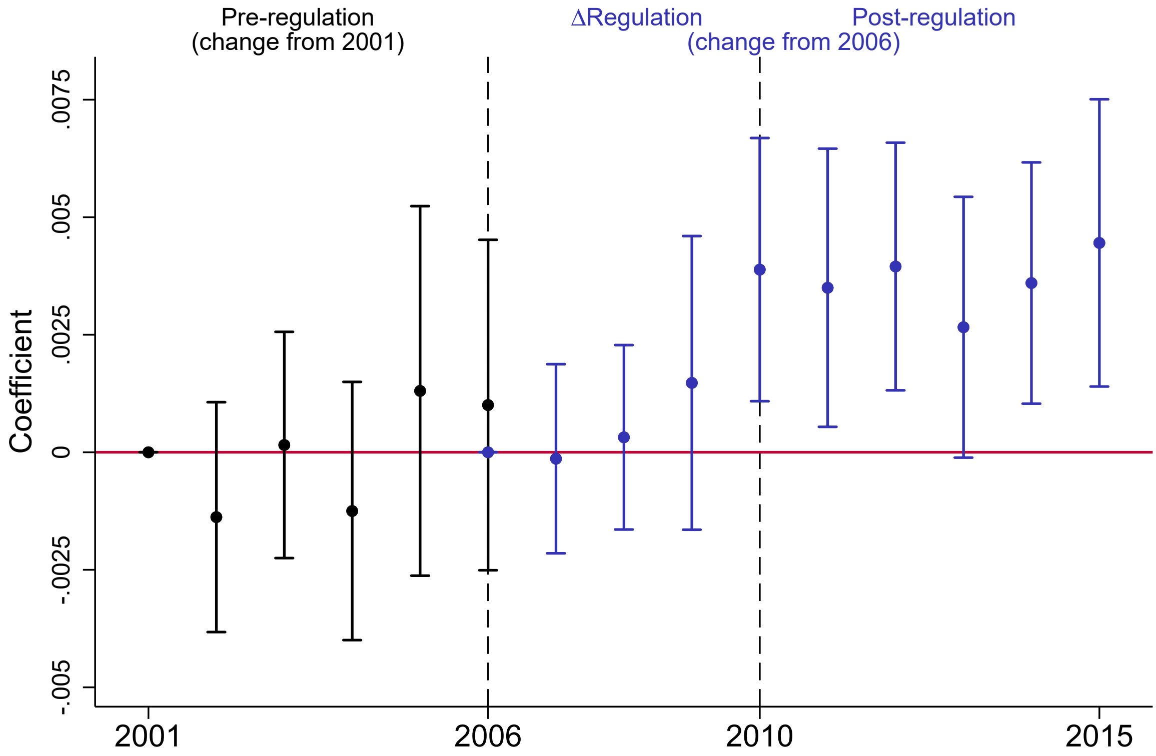
Long-difference estimates based on *Susenas* show the full time profile of the regulatory effect in the form of a series of regressions over different time frames. They specifically enable us to check the parallel trend assumption prior to 2006, which is a necessary condition for our identification strategy. We present the results of the repeated long-difference estimations in graphical form in figure 5.4. For the estimations, we run yearly regressions of the change in district employment rates relative to the base year 2006 on the change in each district's regulatory penetration between 2006 and 2010 (similar to equation (5.2)) and report yearly coefficients along with their 90% confidence intervals. To test for pre-trends, we run an additional set of regressions with the change in district employment rates relative to the base year 2001 as the dependent variable, while still calculating the change in LRP as the main explanatory factor over the period of 2006 to 2010. For the parallel trend assumption to hold, the change in the employment rate before 2006 needs to be unaffected by later changes in regulatory penetration. Each regression further controls for the initial values of LRP and manufacturing employment share as well as island dummies.

Figure 5.4 documents larger total employment growth in places that faced a larger change in LRP from 2006 to 2010. We observe only a small initial increase in the district employment rate after the first revision of the NIL in 2007. The impact of regulation on employment rates turns significant in 2010. This could reflect that the effect of LRP only materializes after the full change in regulation between 2006 and 2010 has been implemented. The positive effect of regulatory penetration on total employment rates levels off at about 0.0035 and remains statistically significant until the end of the sample period. Importantly, we do not detect any evidence of pre-trends before 2006 as none of the coefficients are significantly positive. Districts that experience a regulatory tightening after the two major revisions of the NIL do not systematically differ from non-affected districts before 2006 in terms of their employment rates. This alleviates concerns that our LRP measure may spuriously pick up ongoing employment trends.

We also confirm this result within the panel estimation by allowing for linear trends in the district's change of the employment rate between 2001 and 2005 in column 1 of table C14 in the appendix. While the coefficient of interest is slightly reduced in magnitude, it remains statistically significant. Column 2 goes even further back in

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

Figure 5.4: Impact of LRP (full change) on total employment rate



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

time and allows for different trajectories with respect to the change of the employment rate between 1997 and 2000 – a period during which Indonesia went through the Asian financial crisis. Note that the number of observations is slightly reduced due to missing employment information in 1997. The impact of LRP on the employment rate is not affected by adding pre-trends. Finally, column 3 of table C14 additionally controls for spatial regulatory spillovers. We construct the spillover variable by summing up all districts' LRP and weighting them by the inverse squared distance to a particular district's centroid. There is no evidence for the existence of spatial spillovers across district borders, which also supports our definition of districts as local labor markets. Given the robustness of our main result to a large set of additional controls, we are confident that our results capture the impact of regulatory penetration on employment and are not driven by other confounding factors.

5.6.3 Validity of the shift-share approach

One concern raised by the recent literature on the validity of shift-share instruments is that serial correlation of the error terms across districts with similar initial employment structure may lead to severe downward bias of the estimated standard errors (Adão et al. 2019). This in turn results in an over-rejection of the null hypothesis. To address this concern in our case, we follow Adão et al. (2019) and run placebo regressions in which we randomly assign regulatory status to groups of firms. The regulation dummy in these regressions is drawn from a Bernoulli distribution with mean 0.139 (the true average of regulation in the data). The regression design is identical to our preferred specifications in the long-difference or the fixed effects panel setting. Table C15 in the appendix shows the results of 10,000 placebo samples. The mean coefficient across all placebo samples in column 1 of Panel A and B is very close to zero. This is not surprising as we do not expect any systematic result from randomly assigning regulatory status to groups of firms. Column 2 reports the standard deviation of all estimated coefficients, while column 3 shows the median of all estimated standard errors. Theoretically, these two figures should be identical. Our test shows that the median standard error is always smaller, but the difference between the two is only marginal compared to the very large discrepancies shown by Adão et al. (2019). Accordingly, the rejection rates of the null hypothesis at the 5% significance level are relatively close to their expected value (note that Adão et al. (2019) find extremely large rejection rates between 30-50%). We thus do not consider serial correlation with respect to initial sector composition to be a severe concern in our two approaches.

As a second check, we allow standard errors to be correlated within percentiles of the initial distribution of LRP. This results in a clustering up (to 55 clusters), since districts are nested in the initial LRP percentiles. Appendix table C8 shows our main results in column 1 as benchmark. Column 2 then clusters standard errors based on the initial LRP distribution. The coefficient of interest remains statistically highly significant.

Third, as discussed in section 5.4, we assess whether regulation of singular two-digit sectors entirely drives our result (Goldsmith-Pinkham et al. 2020). We therefore exclude the three most contributing sectors from the computation of LRP in column 3. Despite a slight reduction in magnitude, the coefficient of interest remains statistically significant in both panels.

5.7 Further results

5.7.1 Possible channels

Our results imply that the regulation-induced reduction of FDI inflows to the district (see also the first stage results in table C9) did not only change aggregate manufacturing labor demand in the local labor market, but also resulted in a large spillover effect to services. This could reflect either further underlying dynamics, demand for local services along the value chain, or be the result of classical income and multiplier effects. Throughout the whole analyzed time period, manufacturing employment stayed very stable whereas services expanded, reflecting an ongoing structural change from agriculture to services but also a moderate increase in total employment (compare figure C1). Our specifications control for such different time dynamics at the district level by including time trends by initial employment in agriculture, manufacturing, and services. In section 5.6, we also presented a whole range of possible differential dynamics that districts may be exposed to. We showed that our results are robust against the concern that exposure to regulation is spuriously related to other factors explaining structural change. We now ask whether the presence of such a spillover effect could be rationalized and supported by channels that go beyond sectoral integration and increased demand for services (Dix-Carneiro and Kovak 2019). In particular, we check if FDI protection also generates income effects among households or may correlate with public spending.

Protectionism may have also generated aggregate income and wealth effects, which could also explain the increased demand for services. To check for wealth effects, we exploit information on monthly household expenditures in *Susenas* to construct a measure of per capita consumption (in adult-equivalence units) as proxy for demand. Second, we also estimate wage premia based on the labor market survey *Sakernas*, which should more directly measure potential gains in earnings.[59] Table 5.4 shows the effects of LRP on household expenditures per capita (in adult-equivalence units) in column 1. The estimation is defined according to our preferred specification (including trends in initial LRP and sectoral shares). We find a positive but insignificant effect on per capita spending. Similarly, we find an insignificant and even negative estimate of the impact

[59] For that purpose, we run yearly Mincer wage regressions of the form on a prior stage:

$$\mathit{ln(Wage)}_{dijt} = \sum_{d=1}^{341} (\beta_{1,d} \times \mathit{District}_{dt}) + \boldsymbol{X}'_{dijt}\beta_2 + \phi_j + \epsilon_{dijt}, \tag{5.4}$$

where $\mathit{ln(Wage)}_{dijt}$ denotes the log(hourly wage) of individual i in industry j within district d and $\boldsymbol{X}_{dijt}$ includes individual characteristics. We then take the estimated coefficient $\beta_{1,d}$ as our measure for the log wage premia in district d in year t (cf. Dix-Carneiro and Kovak 2017). We weight the regression of log wage premia on LRP by the inverse of the squared standard error of equation (5.4).

Table 5.4: Impact of LRP on private and public expenditure as well as wage premia

Dependent variable:	asinh(Household expenditure pc)	Log wage premia	asinh(pub. expenditure pc)	asinh(personnel expenditure pc)	Poverty card	Health card
	(1)	(2)	(3)	(4)	(5)	(6)
LRP	0.0025	−0.0019	−0.0125	0.0004	−0.0007	0.0026
	(0.0037)	(0.0042)	(0.0086)	(0.0075)	(0.0006)	(0.0048)
Observations	4,339	4,324	4,260	4,260	4,339	4,339
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the inverse hyperbolic sine of monthly household expenditure per capita (in adult-equivalence units). $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

of LRP on wage premia in column 2. Our results, thus, do not support the narrative of regulatory employment spillovers to the service sector due to household income effects.

Local or national governments may have also reacted to increased regulatory penetration and its potentially disruptive effects on local firms by increasing public spending, thereby creating additional labor demand, especially in the service sector. Our data does not allow us to cleanly distinguish whether the employment effects are due to private demand or public investment. However, we can try to proxy for different dimensions of public demand. First, we exploit district-level expenditure data from *Dapoer* (World Bank 2019) to see whether regulatory tightening was associated with increased local public investment. We distinguish between total district expenditure and expenses for personnel (both per capita) that may directly translate into employment gains. Second, we use information from the village census *Podes* (*Potensial Desa*) to control for remedial policies within districts. In particular, *Podes* records the number of poverty and health cards distributed among the local population, and we construct measures of social security penetration by using the share of people receiving social assistance within the total population.[60] One straightforward policy intervention to react to adverse effects of regulatory penetration would be to selectively increase the number of people entitled to social benefits. Columns 3 to 6 of table 5.4 show the correlation between LRP and our proxies of public investment. We do not find statistically significant support for the hypothesis that increased public spending creates demand for services within districts.

[60] Social security variables are interpolated since *Podes* is only available for the years 2000, 2003, 2006, 2008, 2011 and 2014.

5.7.2 Heterogeneities and alternative outcomes

For the upcoming heterogeneity analysis, we rely on results based on panel regressions using data from *Susenas*. In a last step, we complement this data with further outcomes surveyed within *Sakernas*, since *Susenas* does not consistently record labor market outcomes like working hours or the size of the active population. As *Sakernas* turns out to be much noisier, we only allow for linear trends in initial LRP and manufacturing share in regressions using data from the labor market survey as outcome variable.

Table 5.5 documents heterogeneous employment adjustment across different groups of workers. The largest effects can be found among females in column 1. Even though the marginal effect on female total employment rates is insignificant, the size of the effect is 60% larger compared to the male counterparts. Note that the labor market response of males shows a lower scope for adjustment partly due to generally higher male employment rates. Columns 3 and 4 show the employment effect across age cohorts. Especially younger cohorts benefit from protection induced job creation. The effect size is smaller and statistically insignificant for workers above the age of 30.

When we split up the employment effects by skill groups, the positive total labor market effect is slightly larger among low-skilled (with at most primary education), even though none of the coefficients turns significant (columns 5 and 6 of table 5.5). Our findings by type of employment do not support the growth of precarious labor. The increase in employment shares is entirely driven by additional wage jobs (column 8), while the effect on self-employment is insignificant and even negative (column 7). These findings are confirmed by the last two columns of table 5.5 based on data from the labor market survey *Sakernas*. We even find a slight reduction in the share of employees who report receiving a wage below the minimum wage in column 9, and an insignificantly negative impact on the share of employees working in several jobs (column 10).

Results relying on further labor market outcomes from *Sakernas* show that FDI regulation also increases labor force participation by pulling inactive individuals into the labor market (column 1 of table C16). While the positive effect of FDI protection on the employment rate can be confirmed in this dataset as well (column 2), there is no significant effect on unemployment (column 3). Finally, column 4 shows a positive coefficient on the working hours per worker, even though it does not reach conventional levels of significance. Thus, although the individual labor supply adjustment might have not only take place at the extensive margin via job creation but also at the intensive margin, we do not find strong conclusive evidence for this margin of adjustment.

Table 5.5: Impact of local regulatory penetration on employment by worker and job characteristics

Dependent variable:	Female	Male	15-29	30-64	Low-skilled	High-skilled	Self-employed	Wage employed	Below min. wage	Add. job
Employment rate among:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LRP	0.0025	0.0015*	0.0032***	0.0014	0.0014	0.0008	−0.0009	0.0029**	−0.0001*	−0.0006
	(0.0016)	(0.0008)	(0.0012)	(0.0010)	(0.0011)	(0.0012)	(0.0011)	(0.0012)	(0.0001)	(0.0011)
Observations	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,325	4,325
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the employment rate by gender, age groups, skill level, or the share in working age population who work as self- or wage employed, below minimum wage or have an additional job (the latter two are from *Sakernas*). $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

5.8 Conclusion

Policy interventions often generate substantial side effects, which can affect their overall evaluation. In this paper, we show that the introduction of a protectionist FDI policy in Indonesia leads to employment creation. In particular, we present evidence featuring a small but precisely estimated increase in the total employment rate due to a regulatory tightening across districts. Using data from two waves of a firm census, we show that the effect is driven by both entry of small manufacturing firms, and employment generation at the intensive margin among medium and large service firms. Shielding the economy against foreign competitors thus generates not only a surge in less competitive manufacturing firms but also increases the demand for domestic services. Panel data based on household and labor market surveys corroborate the employment increases but do not yield evidence for either wage or income effects. Similarly, our findings also do not originate from increases in public investment. Our estimates are robust to controlling for a wide range of alternative drivers of employment dynamics, including among others globalization, automation, or agglomeration dynamics.

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

Nonetheless, this should not be understood as encouraging evidence for protectionist policies. We believe that our results highlight the trade-off between immediate employment gains and long-run economic development. Shielding the economy from foreign capital investments and the inflow of new technology and know-how may be tempting in the short-run but also means that countries forfeit the positive productivity effects of FDI (Blalock and Gertler 2008, Javorcik and Poelhekke 2017), like it has been shown by Genthner and Kis-Katos (2019) for the case of the negative investment list. The evidence at hand, however, does not allow for wider-ranging conclusions with respect to the effects of FDI protectionism on broader local economic development and living standards.

Our results are subject to some limitations. We are only able to construct meaningful measures of regulatory penetration in manufacturing and services, but still lack a similar measure for FDI into agriculture, the study of which could also provide valuable insights. Moreover, we lack sufficient information on the quality of employment that would provide us with reliable welfare implications. More precise information on work contracts or linked employer-employee data would be needed to further investigate the nature of employment creation and its spillovers.

CHAPTER 6

Concluding remarks

Firms continuously have to adjust to alterations in their business environment. These changes may originate from new policies implemented by national governments, but are also brought along by forces like globalization, automation or climate change. This thesis has investigated the firm dynamics related to environmental and institutional changes in the context of Indonesia using three examples: rising temperatures due to global warming, productivity spillovers due to the entry of MNEs, and a tightening of the regulatory framework of FDI. In the case of FDI regulation, the thesis has extended the scope of analysis by examining its impact on regional development using the example of local labor market adjustment.

One common finding in all chapters of this thesis is that firms make reasonable adjustments to deal with their altering business environment. For instance, chapter 2 showed that manufacturing enterprises managed to avoid output losses due to more heat days by increasing labor and capital input. They thereby successfully compensated for the productivity losses they were experiencing. As shown in chapter 3, firms took advantage of the presence of MNEs and were able to realize productivity gains through spillovers. The ability to learn from foreign enterprises, for instance by adapting new technology or managerial skills, is an important mechanism for developing countries to foster economic development. Chapter 4 also showed that domestic enterprises overcame the capital shortages due to FDI regulation by substituting foreign with domestic capital, even though the replacement resulted in productivity losses.

This thesis helps to improve the understanding of ongoing firm dynamics in response to business environment changes. The findings could provide insightful recommendations to policy makers that assist them to make efficient and welfare enhancing decisions and eventually contribute to the successful implementation of evidence-based policies. Especially the results on the impact of FDI regulation on manufacturing firms should call into question the protectionist motives behind the investment restrictions given the long-term consequences of productivity losses. At the same time, one has to reconsider whether FDI regulation is the right instrument to foster domestic employment or whether more adequate and direct policy measures would be more appropriate to obtain positive labor markets outcomes.

Moreover, three chapters of this thesis improve the understanding of the impact of FDI in general. While chapter 3 added to a large body of literature evidencing the productivity improving effects of FDI (cf. Javorcik 2004, Lu et al. 2017), the last two chapters addressed the relatively understudied field of FDI regulation. With the rise of national protectionism over the past decades, the question of how domestic economies were impacted by restricting or fully prohibiting foreign capital inflow has gained both economic and political relevance.

The results at hand in chapter 4 suggested that policy makers predominantly used the instrument of capital regulation to shield previously privatized firms and sectors from foreign competition. Even though this form of protectionism might benefit individual interests, there was strong evidence for an overall negative effect of FDI regulation on firm productivity. In that sense, the FDI restricting regulation seemed to invert the well established positive effect of FDI on domestic firms. Chapter 5 detected evidence for a direct effect of regulatory penetration on employment in the manufacturing sector, originating from entry of small-scale enterprises. In that sense, the protectionist policy seemed to reduce foreign competition and allows local businesses to enter the market. FDI restrictions further resulted in strong employment share increases within the service sector, supporting the narrative of increased domestic demand for services.

The findings of chapter 4 and 5 thus indicate that national policy makers face a trade-off between positive domestic employment effects and the negative impact on firm productivity. Further research is needed, however, to provide a final answer regarding potential welfare implications. Similarly, future research will be able to cover a time period sufficiently long to draw final long-run conclusions.

As a final point, the thesis adds to a better general understanding of the economic environment within Indonesia. As a very large emerging country, economic shocks directly affect a great number of people. Insights from empirical analyses can provide the necessary information to deal with the economic and social consequences and help to increase welfare of the local population.

It is unclear, however, whether the results for Indonesia are also externally valid in the context of other countries. With respect to the impact of heat on firm outcomes, chapter 2 showed that findings for Indonesia were different compared to existing studies. This provides valuable evidence for other countries that similarly lie in the tropical climatic zone only. Even though the underlying infrastructural conditions in Indonesia may not be fully representative for other countries, results from chapter 2 can be considered to be externally valid at least to a certain extent. More research is needed though to determine the role of electrification and other types of infrastructure on the relationship between rising temperature and firm outcomes. Given the high degree of specificity of FDI regulation in Indonesia, it is unclear whether the results in chapters

4 and 5 are transferable to alternative contexts. Nevertheless, these findings may still serve as a good starting point, especially in cases where the quality of available data is insufficient and similar analyses are not feasible.

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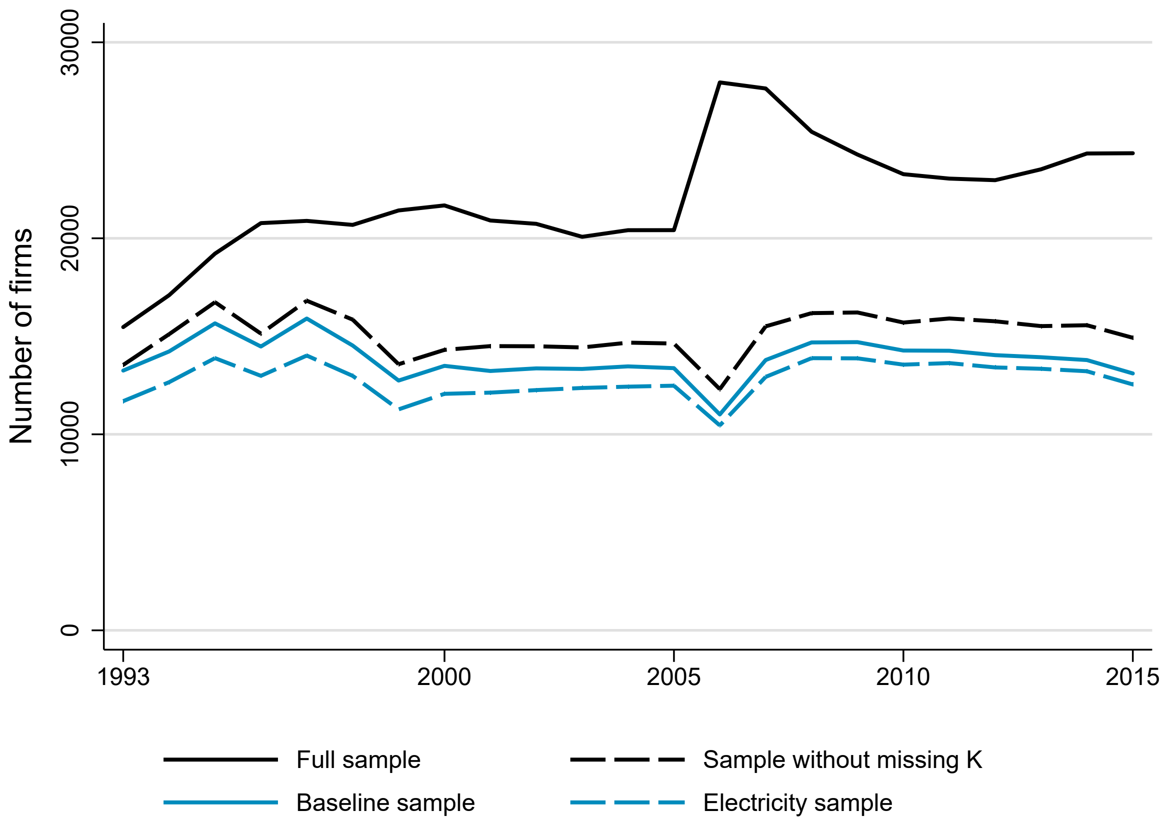
APPENDIX A

Heat and firm productivity: Evidence from Indonesia's manufacturing sector

A.1 Sample size reduction

Figure A1 shows the development of our sample size over time. Without taking care of the missing capital variable, we observe a massive increase in the number of firms in 2006 for the full sample. This can be explained by the BPS conducting an economic census in that year and, thus, more attention was given to surveying the full universe of enterprises. As mentioned above, capital stocks are unfortunately not reported in 2006. After interpolating and dropping observations with missing capital information, we end up with a smaller number of firms per year (around 15,000), and a distinct drop in 2006. Our preferred sample size is further reduced in the TFP sample, where we clean the data for unrealistic spikes and strong outliers in the main input and output variables required in the TFP estimation. Finally, the most restrictive sample additionally excludes all observations with zero electricity consumption. We restrict the analysis to this sample when looking at potential adjustment patterns in electricity usage (cf. Petrick et al. 2011).

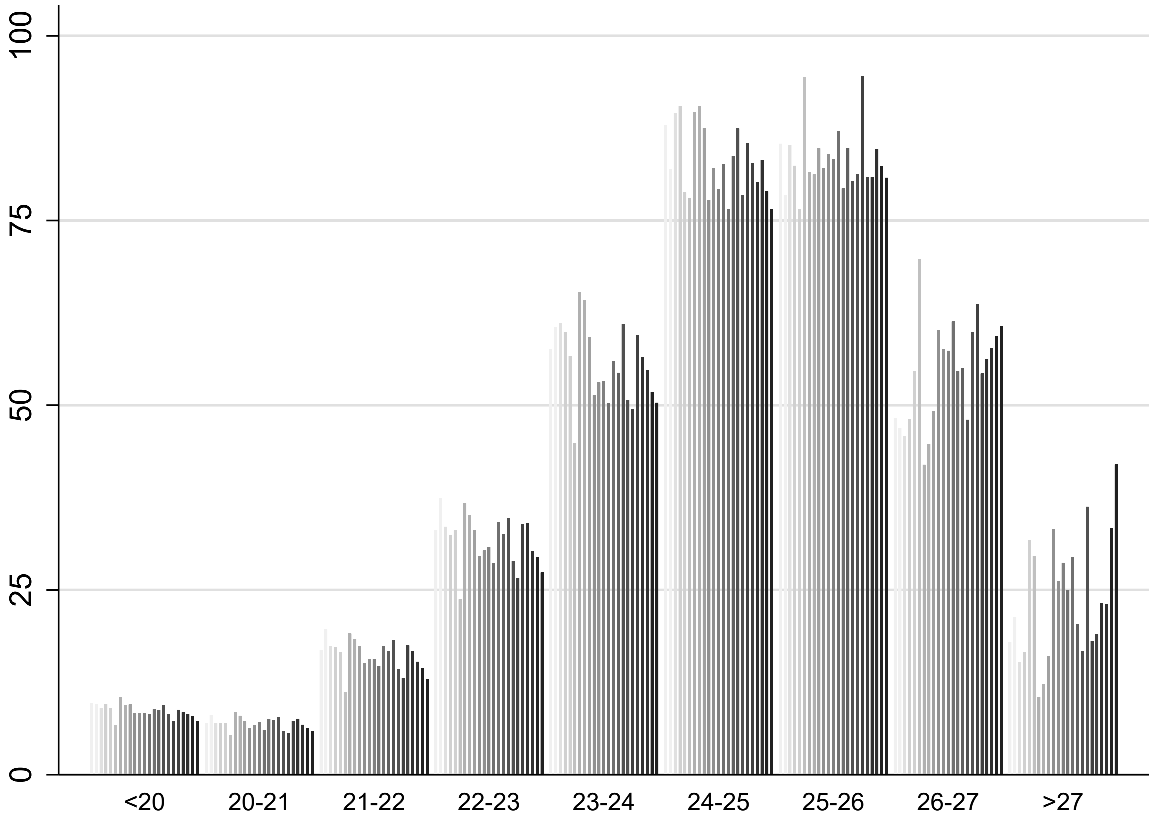
Figure A1: Number of firms in each sample year by different samples



Note: The graphs depicts the number of firms in various samples over time.

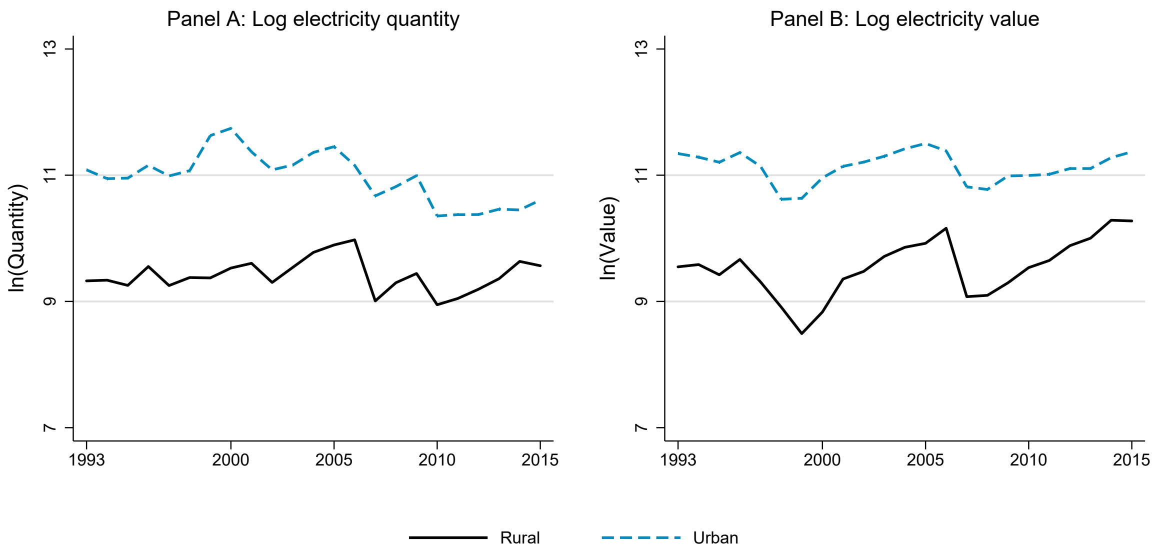
A.2 Additional figures

Figure A2: Number of days in temperature bins over time



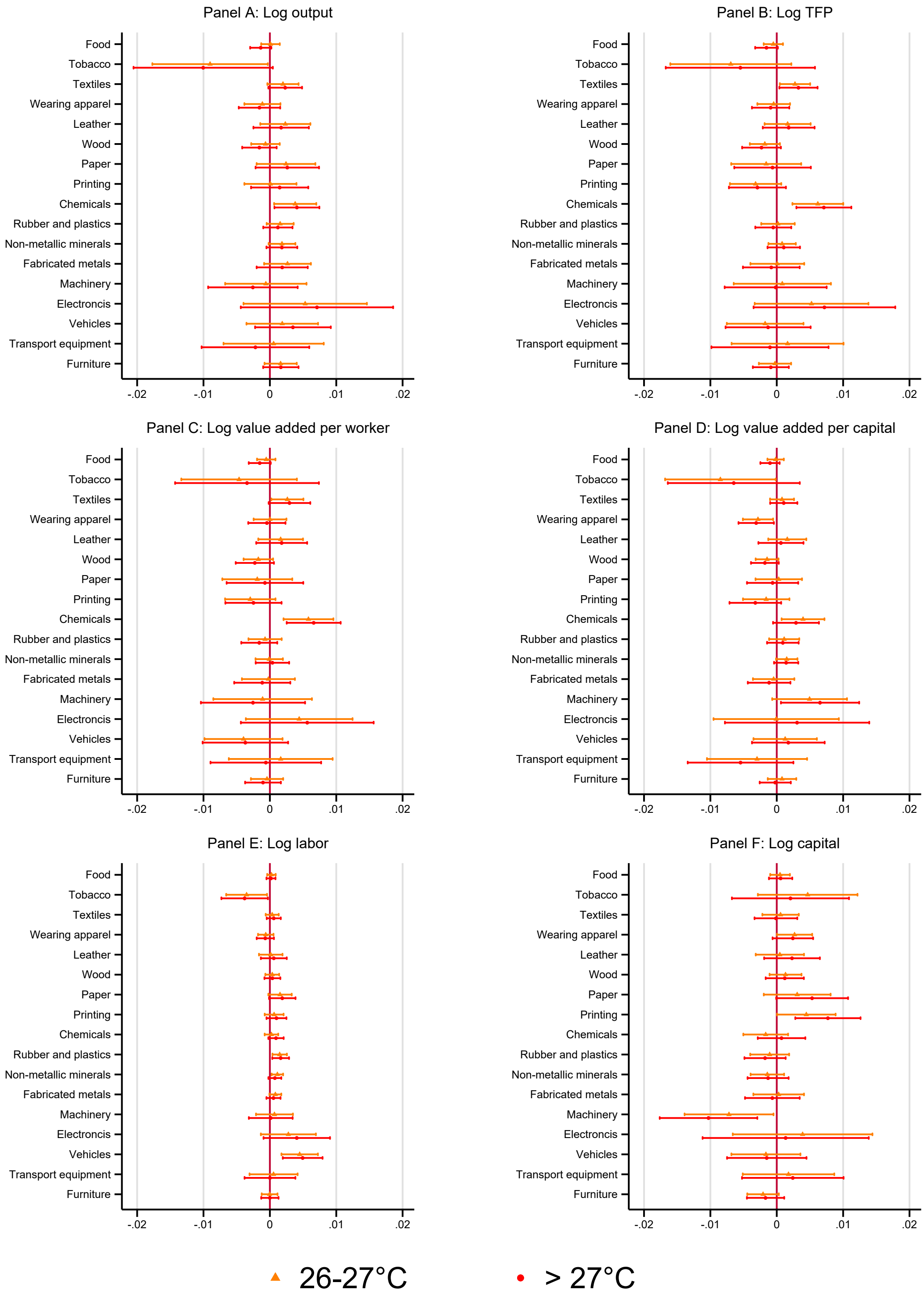
Note: Number of days in temperature bins across all non-missing districts. Each bar within a bin represents one year (1993-2015), and darker bars are more recent years. Source: authors' visualization based on ERA5-Land.

Figure A3: Average electricity quantity and spending



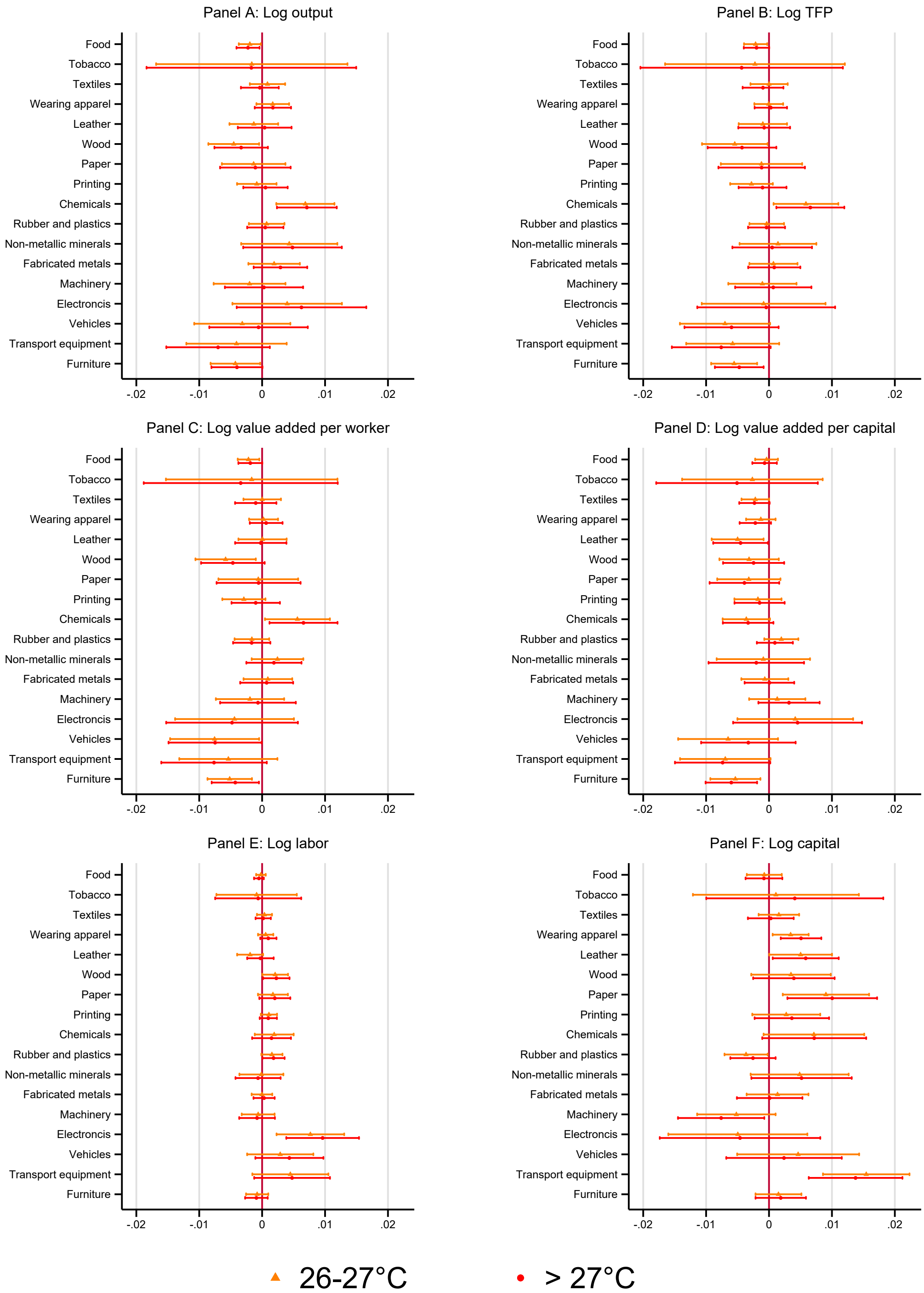
Note: The graph shows the average log electricity quantity or value among firms in our baseline sample (including zero electricity values).

Figure A4: Rural firms: Effect of temperature on output, productivity and input factors by sector



Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Figures show point estimates for the temperature bins 26-27°C (orange) and >27°C (red), and the associated 90% confidence intervals as bars. Regressions are specified according to equation (2.4) and control for firm, island-year and product-year fixed effects. Standard errors are clustered on firm and district-year level. 21-22°C is omitted.

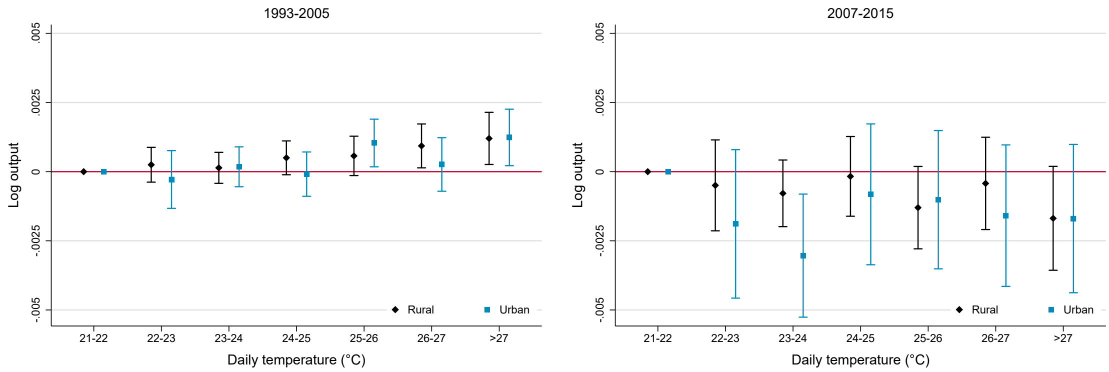
Figure A5: Urban firms: Effect of temperature on output, productivity and input factors by sector



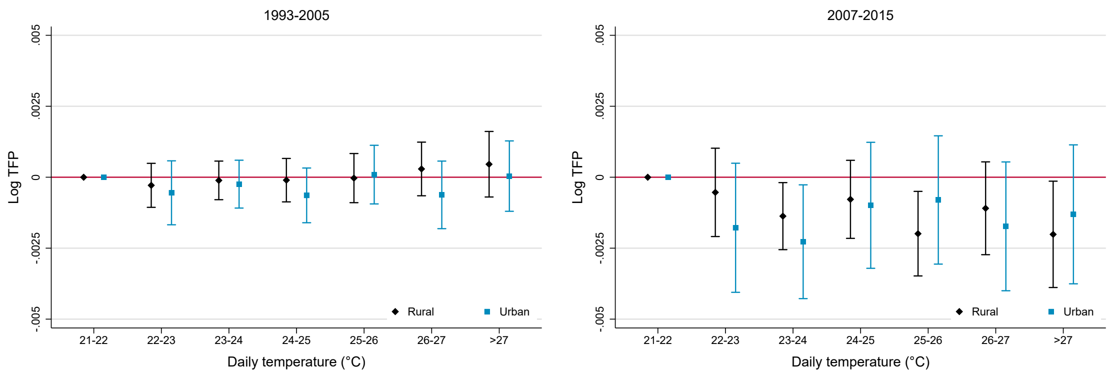
Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Figures show point estimates for the temperature bins 26-27°C (orange) and >27°C (red), and the associated 90% confidence intervals as bars. Regressions are specified according to equation (2.4) and control for firm, island-year and product-year fixed effects. Standard errors are clustered on firm and district-year level. 21-22°C is omitted.

Figure A6: Robustness check: Pre and post 2006

Panel A: Log output



Panel B: Log TFP



Panel C: Log value added per worker

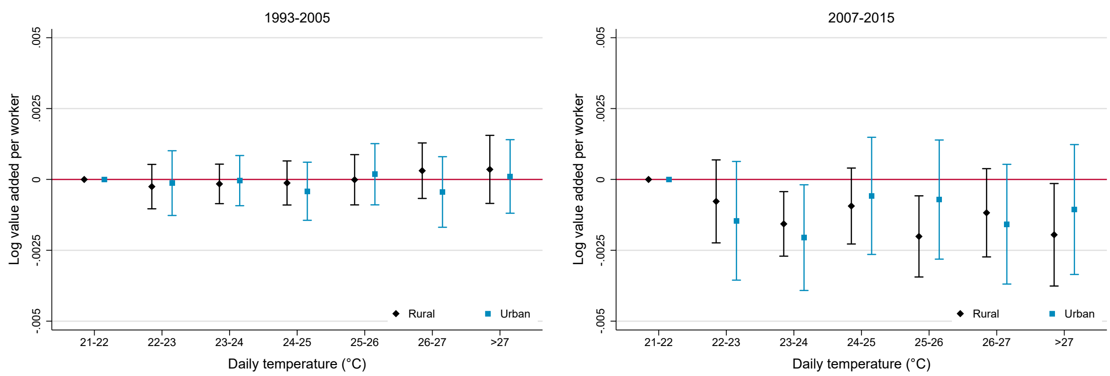
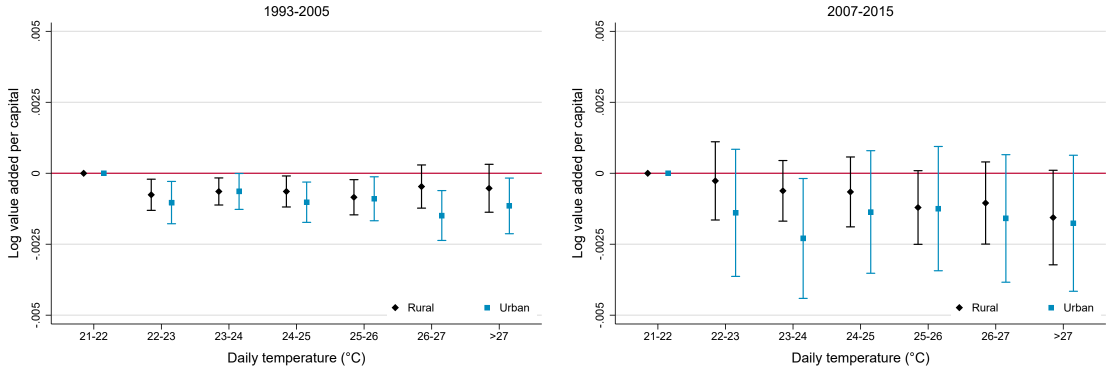
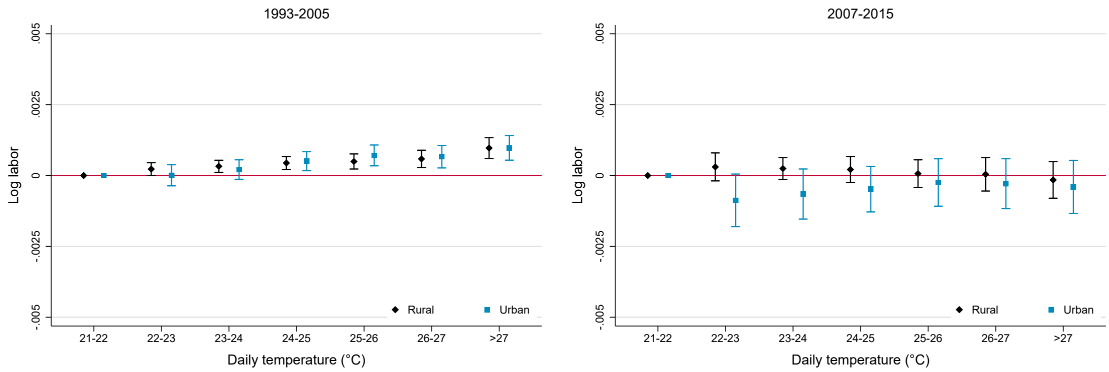


Figure A6: Robustness check: Pre and post 2006 (continued)

Panel D: Log value added per capital



Panel E: Log labor



Panel F: Log capital

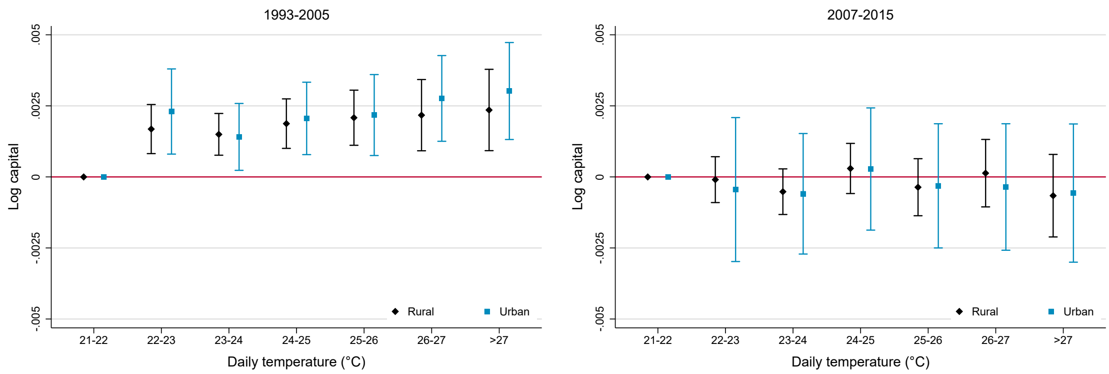
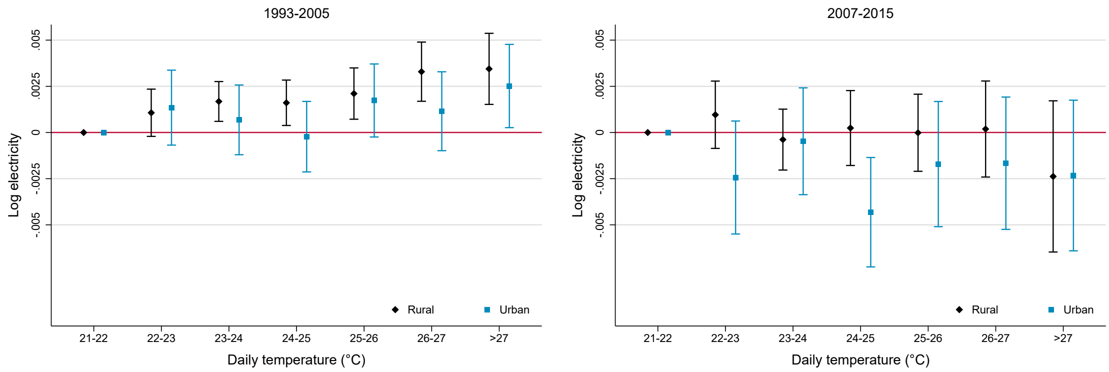
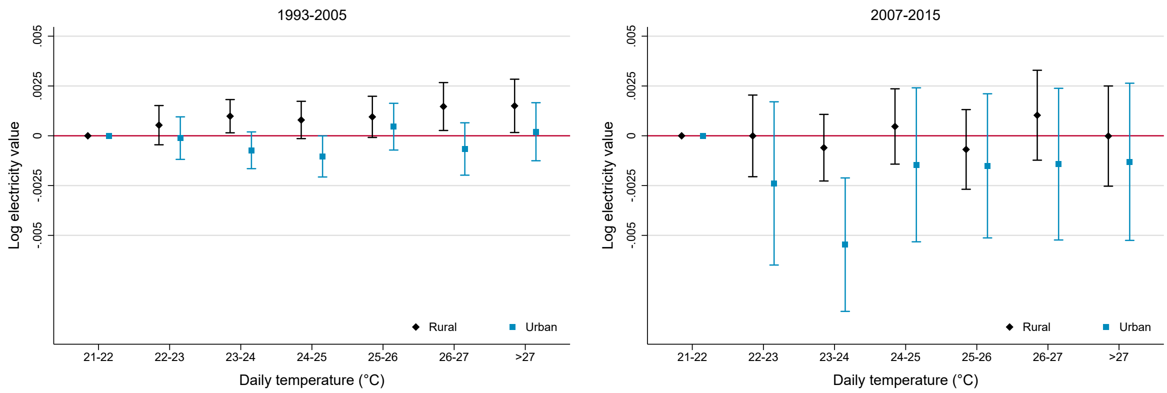


Figure A6: Robustness check: Pre and post 2006 (continued)

Panel G: Log electricity quantity



Panel H: Log electricity value



Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E), log capital (F), log electricity quantity (G) and log electricity spending (H). Figures show point estimates and the associated 90% confidence intervals as bars separately for rural and urban firms. Regressions are specified according to equation (2.4) and control for firm, island-year and product-year fixed effects. Standard errors are clustered on firm and district-year level. 21-22°C is omitted.

A.3 Additional tables

Table A1: Summary statistics of main input and output variables in 1993, 2005 and 2015

	1993		2005		2015	
	Mean	SD	Mean	SD	Mean	SD
Productivity:						
ln(TFP)	9.87	1.42	9.93	1.48	10.53	1.42
ln(VAD/L)	10.11	1.22	10.16	1.28	10.78	1.22
ln(VAD/K)	0.70	0.68	0.92	0.87	1.35	1.20
Inputs and outputs:						
ln(K)	14.81	2.18	14.30	2.12	14.22	2.18
ln(L)	4.38	1.20	4.20	1.17	4.13	1.14
ln(Sales)	15.58	2.09	15.42	2.14	15.83	1.96
ln(Quantity of total electricity use)	9.78	4.39	10.22	3.98	9.73	3.53
ln(Real value of total electricity use)	10.01	4.47	10.25	3.98	10.45	3.67

Note: Note: Number of observations in 1993: 13,252; 2005: 13,376; 2015: 13,105.

Table A2: Alternative measures of urbanization

District split by:	(1) Population density		(2) Urbanization rate		(3) Kabupaten/Kota	
	Rural	Urban	Low	High	Kab.	Kota
Panel A: Log output						
< 20°C	−0.0013	0.0031	−0.0014	0.0000	−0.0014	0.0003
	(0.0009)	(0.0115)	(0.0010)	(0.0030)	(0.0009)	(0.0029)
26-27°C	0.0007	−0.0001	0.0007	−0.0003	0.0007	0.0001
	(0.0005)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0007)
> 27°C	0.0003	0.0002	0.0003	0.0000	0.0003	0.0001
	(0.0006)	(0.0007)	(0.0006)	(0.0007)	(0.0006)	(0.0007)
Panel B: Log TFP						
< 20°C	−0.0018*	−0.0059	−0.0019**	−0.0024	−0.0019**	−0.0023
	(0.0009)	(0.0104)	(0.0009)	(0.0024)	(0.0009)	(0.0023)
26-27°C	0.0002	−0.0012*	0.0001	−0.0012	0.0001	−0.0009
	(0.0005)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0007)
> 27°C	−0.0001	−0.0009	−0.0002	−0.0009	−0.0002	−0.0008
	(0.0006)	(0.0007)	(0.0006)	(0.0008)	(0.0006)	(0.0007)
Panel C: Log value added per worker						
< 20°C	−0.0019*	−0.0082	−0.0020**	−0.0020	−0.0020**	−0.0019
	(0.0010)	(0.0096)	(0.0010)	(0.0023)	(0.0010)	(0.0022)
26-27°C	0.0000	−0.0012	−0.0001	−0.0011	−0.0001	−0.0008
	(0.0005)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0007)
> 27°C	−0.0003	−0.0009	−0.0004	−0.0009	−0.0004	−0.0007
	(0.0007)	(0.0008)	(0.0007)	(0.0008)	(0.0007)	(0.0008)
Panel D: Log value added per capital						
< 20°C	−0.0010	−0.0089	−0.0010	−0.0042*	−0.0011	−0.0040*
	(0.0006)	(0.0075)	(0.0007)	(0.0025)	(0.0007)	(0.0024)
26-27°C	−0.0001	−0.0016***	−0.0001	−0.0017***	−0.0001	−0.0014**
	(0.0004)	(0.0006)	(0.0004)	(0.0006)	(0.0004)	(0.0006)
> 27°C	−0.0005	−0.0018***	−0.0005	−0.0019***	−0.0005	−0.0018***
	(0.0005)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0006)
Panel E: Log labor						
< 20°C	0.0004	0.0047	0.0004	0.0006	0.0004	0.0005
	(0.0004)	(0.0032)	(0.0004)	(0.0013)	(0.0004)	(0.0013)
26-27°C	0.0004**	0.0004	0.0005**	0.0004	0.0005**	0.0003
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
> 27°C	0.0005**	0.0006**	0.0005**	0.0006**	0.0005**	0.0005*
	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Panel F: Log capital						
< 20°C	0.0002	0.0027	0.0002	0.0045	0.0002	0.0040
	(0.0009)	(0.0081)	(0.0009)	(0.0042)	(0.0009)	(0.0041)
26-27°C	0.0006	0.0017**	0.0005	0.0021**	0.0006	0.0018**
	(0.0006)	(0.0009)	(0.0006)	(0.0009)	(0.0006)	(0.0008)
> 27°C	0.0006	0.0021**	0.0006	0.0023**	0.0006	0.0022**
	(0.0007)	(0.0010)	(0.0007)	(0.0010)	(0.0007)	(0.0009)
Weather controls	Yes		Yes		Yes	
Firm FE	Yes		Yes		Yes	
Island-year FE	Yes		Yes		Yes	
Product-year FE	Yes		Yes		Yes	
Urban/High/Kota$_d$-specific trends	Yes		Yes		Yes	
Observations	318,675		318,675		318,675	

Note: Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Specification 1 uses our preferred measure of population density to split districts (at the 90th percentile). Specification 2 uses the urbanization rate based on household level reporting from *Susenas*. Specification 3 divides districts by name (*kabupaten/kotamadya*). Regressions are specified according to equation (2.4). Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A3: Effect of temperature on output, productivity and input factors by generator use

Dependent variable:	Log Y	Log TFP	Log VAD/L	Log VAD/K	Log L	Log K
	(1)	(2)	(3)	(4)	(5)	(6)
Rural × no generator						
× < 20°C	−0.0029*	−0.0030*	−0.0029*	−0.0016*	0.0001	−0.0003
	(0.0016)	(0.0016)	(0.0016)	(0.0009)	(0.0005)	(0.0011)
× 26-27°C	0.0014**	0.0008	0.0008	0.0003	0.0004*	0.0008
	(0.0007)	(0.0007)	(0.0007)	(0.0006)	(0.0002)	(0.0007)
× > 27°C	0.0013*	0.0007	0.0005	0.0001	0.0006**	0.0006
	(0.0007)	(0.0008)	(0.0008)	(0.0006)	(0.0002)	(0.0008)
Rural × has generator						
× < 20°C	−0.0002	−0.0009	−0.0009	−0.0005	0.0006	0.0007
	(0.0008)	(0.0006)	(0.0007)	(0.0006)	(0.0006)	(0.0010)
× 26-27°C	0.0001	−0.0003	−0.0006	−0.0003	0.0005*	0.0004
	(0.0005)	(0.0005)	(0.0006)	(0.0005)	(0.0003)	(0.0007)
× > 27°C	−0.0005	−0.0008	−0.0010	−0.0009*	0.0005*	0.0006
	(0.0006)	(0.0007)	(0.0007)	(0.0005)	(0.0003)	(0.0008)
Urban × no generator						
× < 20°C	0.0141	−0.0012	−0.0053	−0.0068	0.0142**	0.0124
	(0.0145)	(0.0138)	(0.0130)	(0.0116)	(0.0056)	(0.0116)
× 26-27°C	0.0006	−0.0010	−0.0012	−0.0010	0.0009**	0.0016*
	(0.0009)	(0.0008)	(0.0008)	(0.0007)	(0.0004)	(0.0008)
× > 27°C	0.0011	−0.0005	−0.0007	−0.0009	0.0010***	0.0014
	(0.0009)	(0.0008)	(0.0008)	(0.0007)	(0.0004)	(0.0010)
Urban × has generator						
× < 20°C	−0.0060	−0.0103	−0.0103	−0.0106*	−0.0027	−0.0077
	(0.0110)	(0.0089)	(0.0085)	(0.0060)	(0.0049)	(0.0088)
× 26-27°C	−0.0007	−0.0013*	−0.0011	−0.0019***	0.0001	0.0017
	(0.0008)	(0.0008)	(0.0008)	(0.0006)	(0.0004)	(0.0012)
× > 27°C	−0.0004	−0.0011	−0.0010	−0.0022***	0.0003	0.0022*
	(0.0008)	(0.0008)	(0.0009)	(0.0007)	(0.0004)	(0.0012)
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Urban}_d$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,675	318,675	318,675	318,675	318,675	318,675

Note: The table splits the sample by firms with and without own electricity generator. The dependent variables are log output, log TFP, log value added per worker, log value added per capital, log labor and log capital. Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A4: Effect of temperature on output, productivity and input factors by major islands

	(1) Sumatra		(2) Java		(3) Kalimantan		(4) Sulawesi		(5) Papua and islands	
Split by pop. density:	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban
Panel A: Log output										
< 20°C	0.0017		-0.0017	0.0027			0.0012		0.0358**	
	(0.0024)		(0.0010)	(0.0112)			(0.0053)		(0.0167)	
26-27°C	-0.0025	0.0018	0.0007	-0.0005	0.0020	-0.0026	-0.0007	0.0022	-0.0019	-0.0021
	(0.0020)	(0.0018)	(0.0006)	(0.0007)	(0.0071)	(0.0019)	(0.0031)	(0.0019)	(0.0030)	(0.0747)
> 27°C	-0.0036		0.0003	-0.0001	0.0006		-0.0025		-0.0005	0.0172
	(0.0024)		(0.0007)	(0.0008)	(0.0077)		(0.0038)		(0.0035)	(0.0771)
Panel B: Log TFP										
< 20°C	0.0021		-0.0028***	-0.0060			0.0002		0.0491***	
	(0.0020)		(0.0010)	(0.0101)			(0.0049)		(0.0157)	
26-27°C	-0.0028	0.0024	0.0002	-0.0014*	-0.0121	-0.0011	-0.0061*	0.0011	-0.0033	-0.1149
	(0.0023)	(0.0022)	(0.0006)	(0.0008)	(0.0087)	(0.0025)	(0.0037)	(0.0021)	(0.0033)	(0.1030)
> 27°C	-0.0040		-0.0001	-0.0012	-0.0147		-0.0087*		-0.0007	-0.1099
	(0.0029)		(0.0007)	(0.0008)	(0.0098)		(0.0045)		(0.0039)	(0.1068)
Panel C: Log value added per worker										
< 20°C	0.0026		-0.0026**	-0.0082			0.0019		0.0448***	
	(0.0021)		(0.0011)	(0.0092)			(0.0048)		(0.0141)	
26-27°C	-0.0027	0.0023	-0.0000	-0.0014*	-0.0100	-0.0008	-0.0065*	0.0005	-0.0026	-0.1409
	(0.0023)	(0.0022)	(0.0006)	(0.0008)	(0.0083)	(0.0025)	(0.0038)	(0.0023)	(0.0031)	(0.0990)
> 27°C	-0.0042		-0.0003	-0.0012	-0.0125		-0.0085*		-0.0001	-0.1469
	(0.0030)		(0.0007)	(0.0008)	(0.0095)		(0.0046)		(0.0036)	(0.1040)
Panel D: Log value added per capital										
< 20°C	0.0012		-0.0015*	-0.0092			-0.0018		0.0345**	
	(0.0016)		(0.0008)	(0.0078)			(0.0045)		(0.0162)	
26-27°C	-0.0007	-0.0015	0.0000	-0.0016**	-0.0199**	-0.0019	-0.0020	0.0001	-0.0038	-0.0481
	(0.0019)	(0.0018)	(0.0005)	(0.0007)	(0.0086)	(0.0015)	(0.0027)	(0.0015)	(0.0026)	(0.0962)
> 27°C	-0.0005		-0.0005	-0.0018***	-0.0211**		-0.0041		-0.0013	-0.0403
	(0.0024)		(0.0006)	(0.0007)	(0.0090)		(0.0033)		(0.0031)	(0.0959)
Panel E: Log labor										
< 20°C	-0.0008		0.0006	0.0051			-0.0021		0.0087	
	(0.0009)		(0.0005)	(0.0031)			(0.0024)		(0.0110)	
26-27°C	-0.0002	0.0009	0.0005*	0.0004	-0.0023	-0.0014	0.0016	0.0012	-0.0007	-0.0020
	(0.0010)	(0.0008)	(0.0002)	(0.0003)	(0.0033)	(0.0010)	(0.0015)	(0.0011)	(0.0011)	(0.0416)
> 27°C	0.0003		0.0005*	0.0005*	-0.0022		0.0015		-0.0013	0.0171
	(0.0012)		(0.0003)	(0.0003)	(0.0037)		(0.0019)		(0.0013)	(0.0398)
Panel F: Log capital										
< 20°C	-		0.0004	0.0012			0.0074		-0.0102	
	(0.0025)		(0.0010)	(0.0085)			(0.0081)		(0.0191)	
26-27°C	0.0007	0.0025	0.0003	0.0016*	0.0129	-0.0032	0.0012	0.0012	0.0036	0.0562
	(0.0025)	(0.0020)	(0.0007)	(0.0009)	(0.0130)	(0.0032)	(0.0039)	(0.0022)	(0.0033)	(0.1263)
> 27°C	-0.0003		0.0005	0.0019*	0.0154		0.0036		0.0014	0.0327
	(0.0031)		(0.0008)	(0.0010)	(0.0139)		(0.0050)		(0.0041)	(0.1219)
Weather controls	Yes		Yes		Yes		Yes		Yes	
Firm FE	Yes		Yes		Yes		Yes		Yes	
Island-year FE	Yes		Yes		Yes		Yes		Yes	
Product-year FE	Yes		Yes		Yes		Yes		Yes	
$\mathbf{Urban}_d$-specific trends	Yes		Yes		Yes		Yes		Yes	
Observations	31,107		258,729		6,011		8,409		10,833	

Note: The table splits the sample by major Indonesian islands in columns 1 to 5. Panels show the estimated impact of temperature on log output (A), log TFP (B), log value added per worker (C), log value added per capital (D), log labor (E) and log capital (F). Regressions are specified according to equation (2.4) and, thus, include the full set of temperature bins (where 21-22°C is omitted). For reasons of clarity, the table only reports coefficients on the lowest and upper two bins. Standard errors are clustered on firm and district-year level. Significance at or below 1% (***), 5% (**) and 10% (*).

APPENDIX B

Foreign investment regulation and firm productivity: Granular evidence from Indonesia

B.1 Product-level determinants of regulation

We study the drivers of product-level regulation by testing the predictive power of an extensive set of product-level characteristics to identify factors that robustly explain changes in the regulatory environment across a wide range of model specifications (Sala-i-Martin 1997).

Therefore, we run regressions on five-digit product level of the form:

$$\Delta REG_{jt} = \alpha + \beta_z z_{jt} + \beta_1 x_{1,jt} + \beta_2 x_{2,jt} + \beta_3 x_{3,jt} + \delta_t + \psi_s + \varepsilon_{jt} \tag{B1}$$

Our dependent variable is the change in the share of regulated firms in product market j in year t, weighted by sales. z_{jt} denotes the political economy factor to be tested, while $x_{1,jt}$ and $x_{2,jt}$ are two additional controls taken from the pool χ of all available variables, and $x_{3,jt}$ denotes a permanent control. Our permanent control is the share of state-owned firms since we expect the presence of public enterprises to be a major determinant of regulation. The regressions additionally include year fixed effects δ_t and two-digit sector fixed effects ψ_s, which alleviate the most obvious problems of misspecification.[61] We then run regressions with all possible combinations of z_{jt}, $x_{1,jt}$, $x_{2,jt}$ and $x_{3,jt}$ and compute the cumulative distribution function [CDF(0)] under the assumption of non-normality (see Sala-i-Martin (1997) for details).

The 36 investigated product level factors include the lag ($t-1$) and the long difference ($t-1$ to $t-6$) of the following variables:

- *State ownership and privatization:* share of state-owned firms, average TFP of state-owned firms
- *Firm size and concentration:* share of medium-sized firms, Herfindahl index of sales, Herfindahl index of employment concentration

[61] Adding a further random control from the pool alters our results only marginally.

- *Productivity dynamics:* log capital-labor ratio, log capital intensity log average firm sales, log total sales,
- *Internationalization:* share of exports in total sales, average foreign capital share, import penetration
- *Labor market factors:* log average wage per worker, log total wage bill, log blue-collar worker wage bill, log white-collar worker wage bill, share of blue-collar workers, log total employment

In total, we estimate 6,545 regressions. We then select the 12 political economy factors with the highest significance in terms of the non-normal cumulative density function (CDF) and include them as time-variant controls in all our main specifications.

B.2 Cleaning the firm data

Matching the yearly firm panel and the NIL regulatory data relies on the five-digit product level of the KBLI (*Klasifikasi Baku Lapangan Usaha*). The KBLI sector classification is published by BPS (Indonesian Statistical Office, *Badan Pusat Statistik*). It is equivalent to the United Nation's International Standard Industrial Classification of All Economic Activities (ISIC) at the four-digit level, but it is adjusted to five-digit level in order to distinguish between additional Indonesian sectors of local importance.

If product codes are incomplete (e.g., '151' instead of '15111') or missing, we impute, whenever plausible and unambiguously possible, the same code as in the year before or in the next year. We exclude all observations for which product codes are still missing or incomplete after this adjustment. We convert all codes to the common standard of KBLI 2000 based on conversion tables provided by BPS. We drop all observations with ambiguous conversion results. We start with 378,856 observations over the 16 years, of which 1,213 have to be removed due to missing or incomplete coding. 725 additional observations are lost because of ambiguous conversion results between the years, arising from a split or unification of sector codes across different versions.

In order to estimate TFP, we rely on information on the capital stock, employment, the value of intermediate inputs, and value added. Out of these four core variables, capital stock is the one missing most frequently (cf. Márquez-Ramos 2021). As common in the literature (e.g., Amiti and Konings 2007), we interpolate the capital stock if values are missing in one year only. This is especially relevant in the year 2006, where information on the capital stock is missing for a large part of the firm sample. We are able to interpolate 21,656 observations within 17,169 firms. Even after interpolation, we have to drop 130,385 observations within 31,412 firms due to missing information on the capital stock. Thereby we lose 10,711 firms completely. We investigate the sensitivity of our results to these missing observations by also repeating our estimates for a larger sample using value added per worker as a proxy for productivity, the results for which stay comparable.

In a next step, we exclude extreme outliers by dropping all observations for which inputs or output are not within the threefold of the inter-quartile range above and below the 25 and 75 percentiles. We deal with extraordinary spikes in the data by also dropping all observations with firm-level input growth (labor, intermediate inputs and capital) as well as output growth that is outside the first and ninety-ninth percentile range of each variable's distribution. These steps reduce our sample size by further 14,217 observations within 8,652 different firms.

We also drop all firms with only one observation within the sample period, which reduces our dataset to 222,633 observations pertaining to 31,184 firms. Finally, we completely exclude all firms which do not report their legal status in any year. Though we make the rather conservative assumption of no regulation if legal status is only missing in one year, we lose all firms that never report their legal status when controlling for trends in initial firm-specific traits. We further lose some observations due to lag and long difference structure within our time-variant product trait measures. As a result, we end up with a final dataset of 180,783 observations within 24,725 firms.

B.3 Merging the NIL conditions to product codes

Both the Negative Investment List (NIL) and the main products of the firm (KBLI) are encoded at the same five-digit level that we use to determine a product-level match between firms and regulated product groups.

Unlike in later years, NIL 2000 does not yet provide KBLI codes, but only states the names of the included sectors. Thus, in this one year we match the verbally stated sector names to the corresponding KBLI sector codes. Furthermore, as the NIL 2007 slightly changes in 2008 by an amendment to the existing regulation, we use the content of the first draft of the NIL for 2007 and the amendment for the years starting with 2008. We convert the changing KBLI sector codes between the years and adjust the coding of the NIL 2010, NIL 2014 and NIL 2016 to the KBLI 2000 standard. The regulatory and firm data are merged according to the five-digit KBLI 2000 sector codes and the relevant year.

As several of the regulatory instruments are conditional on firm characteristics (see table B1 for a more detailed representation), we encode them conditional on firm attributes:

- *Closed* [closed to new investment in general] applies to all kinds of investment, both domestic and foreign. We set this regulatory measure to zero for firms that have already existing foreign involvement as the regulation is forward looking and cannot restrict foreign participation anymore.[62] The average FDI share among these firms is 85% with the majority of firms reporting full foreign ownership of 100%. Thus, these firms are not limited by forward looking regulation since a further increase is not feasible for them anyway.

- *Condition a* [opened to small and medium-sized firms] is conditional on firm size as regulated in law 20/2008 on micro, small and medium enterprises (see Presidential Decree 36/2010). According to Law 20/2008, firms should be considered as large if they have annual revenues from sales above 50 billion Rupiah and assets (excluding land and buildings) equal or above 10 billion Rupiah. The earlier Presidential Decree 77/2007 refers to the law 9/1995 on small enterprises, which establishes similar thresholds in real terms. When applying the firm size thresholds over time, we adjust for inflation. Accordingly, we generate an indicator variable that encodes large firms based on their annual sales and assets, thereby deviating from the most commonly used definition in literature which relies on the number of workers. Due to high volatility in the data, we use the median sales and median net assets of each firm in order to circumvent wrong coding in cases of outliers. Hence, we consider the classification into large and small enterprises to be time invariant. Regulation turns to one if a firm is operating in a product market regulated by *condition a* and (only if) this firm is a large firm.

- *Condition b* [opened to partnerships] depends on the legal status of a firm. We exploit information on the firm's legal status given by the SI as regulation in *condition b* only applies to firms that do not have the legal status of a partnership. Unfortunately, the SI does not give any useful extra information on neither the exact structure of the partnership nor the partner's identity. Additionally, the variable on legal status suffers from plenty of missing values. In these cases, we assume no regulation as the default. Therefore, we suspect that we may undercount firms subject to *condition b*. We checked the robustness of our results to setting *condition b* to apply product-wide instead: TFP results stay practically the same also if we consider this condition to apply to all firms within a five-digit product, while the results for FDI reduce in size and significance.

- *Condition c* [upper limit to foreign capital] sets a maximum share of capital that can be owned by foreign investors. In nine out of ten cases the upper limit to foreign capital is set to be 95% of total capital. We set this regulatory measure to zero

[62] We use offsets in this and other categories for a total of 799 firms: 755 firms in closed sectors (defined by conditions closed, a, b, d, f, i) and further 44 firms that fall under FDI limitations (c, h).

for firms that have already reached a foreign capital share above the threshold as the regulation is forward looking and cannot restrict their foreign capital shares anymore.

- *Condition d* [limited to certain locations] is easily implemented by matching the regulation with firm location. Regulation is applied if a firm is located outside the authorized province.
- *Condition e* [licensing requirement] and *condition h* [upper limit to foreign capital ownership and license] allow for (limited) FDI under the prerequisite of a valid license issued by the appropriate authorities.
- *Condition f* [investment open to domestic capital] and *condition i* [investment open to domestic capital and license] ban FDI in the affected sectors entirely.
- *Condition g* [upper limits of foreign capital ownership in a certain location] is not listed in table B1 as it does never apply to any manufacturing product.

Although there are a few NIL stipulations that narrow regulation to selected product features, we always assign regulation to the whole five-digit product.

Table B1: Conditions of the NIL over time: affected sectors and regulated firms in the sample

Industry division	closed	a	b	c	d	e	f	h	i	Regulated firms in sample	% share of regulated firms within sector	% share of regulated firms in sector output
PANEL A: NIL 2000												
Food and beverages	3	0	0	0	0	1	0	0	0	25	0.84	6.51
Wood products	0	0	0	0	2	3	0	0	0	323	32.96	45.72
Pulp and paper	0	0	0	0	0	1	0	0	0	2	0.85	12.09
Publishing and printing media	0	0	0	0	0	1	0	0	0	0	0	0
Chemicals	2	0	2	0	0	1	0	0	0	50	9.47	5.93
Machinery and equipment	1	0	0	0	0	0	0	0	0	3	1.78	0.04
Regulated firms in sample	37	0	28	0	214	125	0	0	0	403	3.21	5.00
PANEL B: NIL 2007												
Food and beverages	3	14	7	7	0	0	0	1	0	1018	28.22	35.54
Tobacco products	0	1	3	0	0	3	0	0	0	220	27.23	75.81
Textiles	0	3	1	0	0	0	0	0	0	170	11.43	6.22
Wood products	0	7	5	0	0	4	0	0	0	255	30.50	33.91
Pulp and paper	0	0	0	0	0	2	0	0	0	7	2.76	34.40
Publishing and printing media	0	0	0	0	0	1	2	0	0	64	18.39	44.65
Chemicals	3	1	1	3	0	2	1	2	0	164	35.65	21.47
Rubber and plastic	0	1	0	0	0	0	0	0	0	0	0	0
Other non-metallic mineral prod.	0	1	11	0	0	0	0	0	0	880	71.08	13.49
Basic metals	1	0	0	0	0	1	0	0	0	15	15.15	18.84
Fabricated metal products	0	4	1	0	0	0	0	0	0	24	6.11	6.92
Machinery and equipment	0	0	3	0	0	0	0	0	1	31	19.38	5.01
Other transport equipment	0	0	4	0	0	0	0	0	0	76	64.41	75.52
Furniture	0	1	6	0	0	0	0	0	0	205	13.70	15.64
Regulated firms in sample	59	96	2743	150	0	269	98	94	1	3129	22.09	20.83
PANEL C: NIL 2010												
Food and beverages	3	16	9	0	0	0	0	11	0	1145	30.92	47.76
Tobacco products	0	1	1	0	0	3	0	1	0	588	87.89	94.65
Textiles	0	5	1	0	0	0	0	0	0	248	13.12	11.10
Wearing apparel	0	1	0	0	0	0	0	0	0	19	1.70	13.08
Wood products	0	7	5	0	0	5	0	0	0	289	34.86	64.21
Pulp and paper	0	0	0	0	0	2	0	0	0	2	0.72	27.24
Publishing and printing media	0	0	0	0	0	1	2	0	0	7	2.77	1.21
Chemicals	3	1	1	2	0	3	1	3	0	193	33.11	19.11
Rubber and plastic	0	3	0	0	0	1	0	3	0	34	3.79	29.94
Other non-metallic mineral prod.	0	1	6	0	0	0	0	0	0	138	11.26	1.12
Basic metals	0	0	0	0	0	1	0	0	0	29	20.28	14.66
Fabricated metal products	0	4	2	0	0	0	0	0	0	65	13.43	10.70
Machinery and equipment	0	0	3	0	0	0	0	0	1	31	18.13	20.97
Other transport equipment	0	0	4	0	0	0	0	0	0	84	49.12	39.46
Furniture	0	1	5	0	0	1	0	0	0	176	12.73	10.35
Regulated firms in sample	52	207	2249	80	0	504	87	380	2	3048	20.80	25.93
PANEL D: NIL 2014												
Food and beverages	3	16	7	4	0	0	0	11	0	611	15.82	47.56
Tobacco products	0	1	1	0	0	3	0	1	0	407	76.65	96.80
Textiles	0	5	1	0	0	0	0	0	0	162	10.02	6.35
Wearing apparel	0	1	0	0	0	0	0	0	0	28	2.62	18.58
Wood products	0	7	3	0	0	5	0	0	0	212	30.11	79.34
Pulp and paper	0	0	0	0	0	2	0	0	0	2	0.76	28.77
Publishing and printing media	0	0	0	0	0	1	2	0	0	4	1.38	24.69
Chemicals	3	1	1	2	0	3	1	3	0	182	30.43	11.45
Rubber and plastic	0	2	0	0	0	0	0	2	1	96	10.42	22.43
Other non-metallic mineral prod.	0	1	6	0	0	0	0	0	0	123	10.82	1.14
Basic metals	0	0	0	0	0	1	0	0	0	42	26.58	25.51
Fabricated metal products	0	4	1	0	0	0	0	0	1	26	5.16	7.97
Machinery and equipment	0	0	3	0	0	0	0	0	1	31	14.16	25.31
Other transport equipment	0	0	4	0	0	0	0	0	1	88	45.60	26.80
Furniture	0	1	5	0	0	1	0	0	0	108	9.00	14.84
Regulated firms in sample	37	214	1013	89	0	490	42	393	99	2.122	15.00	24.41

Note: Panels A to D outline the sectoral incidence of various forms of regulation in the NIL 2000, 2007, 2010 and 2014. In each panel, two-digit sectors are displayed in rows and the various conditions of the NIL in columns (closed and a to i). The figures in the central block display the number of five-digit products that are subject to the specific form of regulation in the respective year, whereas the last three columns (and the last row of each panel) display the number of firms that are subject to binding regulation, or the the two-digit sectoral penetration (simple average or weighted by firm output). The specific conditions include: a - Reserved for micro, small and medium enterprises and cooperatives. b - Reserved for partnerships. c - Upper limit to foreign capital ownership. d - Limited to certain locations. e - Special license required. f - 100% local capital. g - Upper limit to foreign capital ownership and limited location. h - Special license and upper limit to foreign capital ownership. i - 100% local capital and special license.

B.4 Further variable definitions

Output and input tariff We retrieve output tariff data from the UNCTAD-TRAINS database using the World Integrated Trade Solutions (WITS) software and construct input tariffs following the literature (cf. Amiti and Konings 2007). During the analyzed time period, average tariff rates increased only slightly (cf. table 4.2), with the product group of alcoholic beverages (and to a lesser extent tobacco products) responsible for by far the largest tariff increases.

Non-tariff measures Non-tariff measures (NTM) are retrieved from the same source (UNCTAD-TRAINS, WITS) and coded using the Harmonized System (HS) on six-digit product level. HS codes in the NTM data are more granular than five-digit KBLI sector codes. We are likely to overestimate the presence of NTMs in our firm data, because some firms are assumed to be affected by NTMs even though they may not produce the regulated product. We make the simplifying assumption to treat all types of NTMs equally and define the NTM indicator to take one if the product is subject to any NTM.

Large firm We define firm size according to Presidential Decree No. 36/2010 that refers to law 20/2008 on small and medium-sized enterprises. A firm is defined as large by this law if its annual sales are higher than 50 billion IDR or its net assets (excluding land and buildings) surpass 10 billion IDR. Presidential Decree No. 77/2007 refers to an earlier law 9/1995 on small enterprises with very similar definitions once the thresholds are adjusted for inflation. We apply this rule yearly, adjusting for inflation.

Weak vs. strong dependency on external finance We distinguish between sectors using the sectoral thresholds provided by Rajan and Zingales (1998). Using a cut-off value of sectoral share of external funding of 0.2, 15,885 firms fall in the weak dependency on external finance category at any point in time (with an average foreign capital share of 4.3%), whereas 12,165 firms are listed in the strong dependency on external finance category (with an average foreign capital share of 9.3%).

Low vs. high technology We group sectors by their technology intensity, based on their global research and development activity. We merge the upper two categories by the OECD (2003) to denote high-technology industries. The low technology group (e.g., food, textiles and metal products) includes 24,005 firms (with an average foreign capital share of 5.2%), and the high technology group (e.g., machinery, chemicals and pharmaceuticals) includes 2,665 firms (with an average foreign capital share of 19.5%).

Trading firm We define a firm as trading if it has ever reported positive export or import values.

B.5 Wooldridge approach for productivity estimation

The estimation of TFP is based on a Cobb-Douglas production function in value added terms on firm level:

$$VA_{it} = Y_{it} - M_{it} = A_{it} L_{it}^{\alpha_L} K_{it}^{\alpha_K}, \tag{B2}$$

where the value added of firm i in year t, VA_{it}, is calculated by subtracting the value of the intermediate inputs M_{it} from total firm output Y_{it}. Value added is a function of productivity A_{it}, the variable input factor labor L_{it} and quasi-fixed capital K_{it}. Taking natural logs results in:

$$va_{it} = \alpha_0 + \alpha_L l_{it} + \alpha_K k_{it} + \omega_{it} + e_{it}, \tag{B3}$$

where small letters denote logs. The error term can be decomposed into two components, an unobserved productivity component ω_{it} and the independently identically distributed error term e_{it}. Simultaneity bias is introduced because a part of the productivity shocks is also correlated with the choice of the variable inputs, namely labor and intermediate goods.

Wooldridge (2009) suggests an alternative and more efficient way of estimating TFP compared to the well-known procedures by Olley and Pakes (1996) or Levinsohn and Petrin (2003). Hereby, estimation of TFP needs to account for potential simultaneity bias due to correlation of input choices with the error term.[63]

The Wooldridge approach decomposes total output as:

$$va_{it} = y_{it} - m_{it} = \alpha_0 + \alpha_l l_{it} + \alpha_k k_{it} + \omega_{it} + e_{it}. \tag{B4}$$

The error term combines an unobserved productivity shock component, ω_{it}, which is correlated with the input choices, and the independently identically distributed error term component, e_{it}. For the i.i.d. component it must hold that

$$E(e_{it} | l_{it}, k_{it}, m_{it}, l_{it-1}, k_{it-1}, m_{it-1}, ..., l_{i1}, k_{i1}, m_{i1}) = 0. \tag{B5}$$

[63] See CompNet Task Force (2014) for a more detailed description of the approach. Our notation follows that of CompNet Task Force (2014).

At the same time, assume that the dynamics of productivity shocks are restricted to

$$\begin{aligned} E(\omega_{it}|k_{it}, l_{it-1}, k_{it-1}, m_{it-1}, ..., l_{i1}, k_{i1}, m_{i1}) &= E(\omega_{it}|\omega_{it-1}) \\ &= j(\omega_{it-1}), \end{aligned} \tag{B6}$$

where $\omega_{it-1} = g(k_{it-1}, m_{it-1})$.

By introducing productivity innovations a_{it}, the error component turns to

$$\omega_{it} = j(\omega_{it-1} + a_{it}), \tag{B7}$$

under the assumption that

$$E(a_{it}|k_{it}, l_{it-1}, k_{it-1}, m_{it-1}, ..., l_{i1}, k_{i1}, m_{i1}) = 0. \tag{B8}$$

Consequently, only the contemporaneous choice variables l_{it} and m_{it} are correlated with innovations a_{it}, while k_{it} and all past values of inputs are uncorrelated with a_{it}. The production function becomes:

$$va_t = \alpha_0 + \alpha_l l_{it} + \alpha_k k_{it} + j(g(k_{it-1}, m_{it-1})) + u_{it}, \tag{B9}$$

where $u_{it} = a_{it} + e_{it}$ and $E(u_{it}|k_{it}, l_{it-1}, k_{it-1}, m_{it-1}, ..., l_{i1}, k_{i1}, m_{i1}) = 0$.

Assuming that the productivity process is a random walk with drift $\omega_{it} = \tau + \omega_{it-1} + a_{it}$ (cf. CompNet Task Force 2014) and the function $g(.)$ takes the polynomial form of order three, we can identify the coefficients of input factors α_K and α_L. Then, equation (B9) becomes:

$$va_{it} = (\alpha_0 + \tau) + \alpha_l l_{it} + \alpha_k k_{it} + g(k_{it-1}, m_{it-1}) + u_{it}. \tag{B10}$$

We estimate equation (B10) using a pooled instrumental variable approach, instrumenting labor by the one period lag of labor input. The estimation relies on a two-step efficient generalized method of moments (GMM) approach. The log of TFP is derived for each two-digit sector s separately, taking into account the varying importance of input factors across industries:

$$\ln(TFP)^s_{it} = va^s_{it} - \hat{\alpha_0}^s - \hat{\alpha_l}^s\, l^s_{it} - \hat{\alpha_k}^s\, k^s_{it}, \tag{B11}$$

where α^s_l and α^s_k are the sector-specific input coefficients (see also table B2).

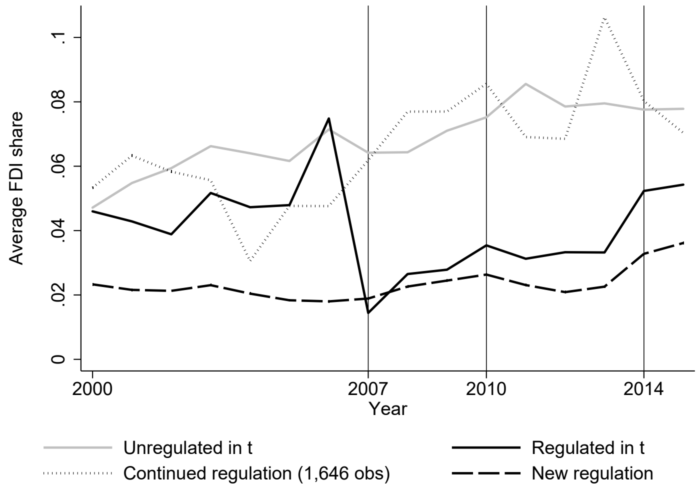
Table B2: Production function coefficients by two-digit sector

		ln(*TFP*)		
	Sector	Labor	Capital	Observations
Food products and beverages	15	0.561	0.139	50,190
Tobacco products	16	0.610	0.105	9,029
Textiles	17	0.535	0.072	20,932
Wearing apparel	18	0.778	0.078	17,112
Leather and leather products	19	0.716	0.017	4,923
Wood and wood products, except furniture	20	0.594	0.108	12,201
Pulp, paper and paper products	21	0.546	0.109	3,574
Publishing, printing and recorded media	22	0.666	0.042	4,710
Coke, refined petroleum products and nuclear fuel	23	0.464	0.173	407
Chemicals and chemical products	24	0.438	0.058	8,279
Rubber and plastics products	25	0.500	0.073	12,293
Other non-metallic mineral products	26	0.438	0.127	17,872
Basic metals	27	0.554	0.125	1,880
Fabricated metal products	28	0.642	0.071	7,243
Machinery and equipment	29	0.629	0.103	2,991
Electrical equipment, office machinery, computers	31	0.642	0.015	1,743
Radio, television and communication equipment	32	0.586	0.039	1,284
Medical, precision and optical instruments	33	0.496	0.089	476
Motor vehicles	34	0.571	0.049	2,188
Other transport equipment	35	0.511	0.120	2,371
Furniture and n.e.c.	36	0.714	0.062	19,340

Note: The production function is estimated by GMM according to Wooldridge (2009).

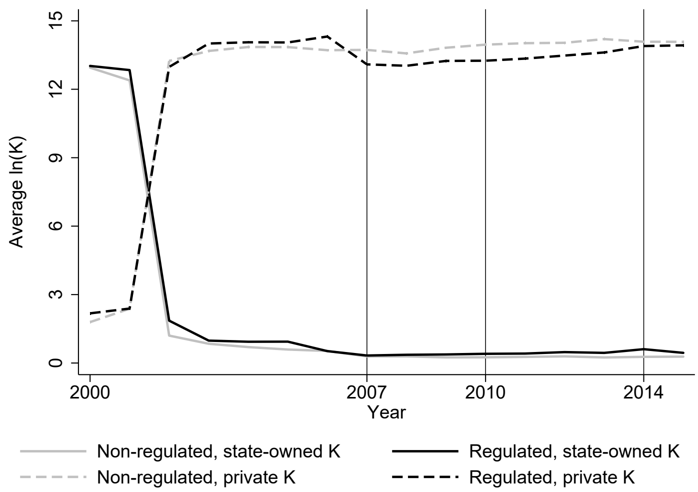
B.6 Additional figures

Figure B1: Trends in FDI shares: continuously regulated vs. newly regulated in 2007



Note: The graph plots the average FDI share among regulated and non-regulated firms in the respective year, as well as the the average FDI share among firms that were regulated already before 2007 and firms that were newly regulated in 2007.

Figure B2: Average state-owned and private capital over time



Note: The graph plots the average of the state-owned (private) log capital among regulated and non-regulated firms in the respective year.

B.7 Additional tables

Table B3: Summary statistics of the main variables

	Mean	SD	Minimum	Maximum	Observations
Binding regulation	0.13	0.33	0.00	1.00	196,809
Licensing requirements	0.03	0.17	0.00	1.00	196,809
Specific bans and FDI limitations	0.10	0.30	0.00	1.00	196,809
Sector-wide bans	0.02	0.12	0.00	1.00	196,809
Regulated product	0.22	0.42	0.00	1.00	196,809
Licensing requirements in product	0.06	0.24	0.00	1.00	196,809
Specific bans and FDI limitations in product	0.19	0.39	0.00	1.00	196,809
Binding de-regulation	0.07	0.25	0.00	1.00	196,809
De-regulated product	0.03	0.16	0.00	1.00	196,809
FDI share	0.06	0.23	0.00	1.00	196,809
ln(TFP)	10.55	1.59	0.71	19.43	196,809
ln(VAD/L)	10.20	1.30	0.63	18.71	196,809
ln(K)	14.16	2.09	4.57	23.52	196,809
ln(Value of foreign capital)	1.24	4.36	0.00	23.52	196,809
ln(Value of domestic capital (private + state-owned))	13.48	3.33	0.00	23.48	196,809
ln(Value of domestic private capital)	11.52	5.60	0.00	23.48	196,809
ln(Value of state-owned capital)	2.02	5.05	0.00	23.45	196,809
ln(L)	4.15	1.16	3.00	9.26	196,809
ln(Blue labor)	3.98	1.15	0.00	9.23	196,809
ln(White labor)	1.99	1.48	0.00	8.56	196,809
ln(Production wage per worker)	8.93	1.01	0.09	16.36	196,532
ln(Non-production wage per worker)	9.40	1.12	0.01	15.99	155,815
ln(Sales)	15.38	2.09	8.48	25.05	196,809
ln(Value of exports)	3.22	6.51	0.00	25.05	157,375
ln(Value of imports)	2.30	5.54	0.00	24.49	196,809
Firm age below 5 years	0.07	0.26	0.00	1.00	196,809
Firm age between 5-15 years	0.39	0.49	0.00	1.00	196,809
Firm age between 15-25 years	0.31	0.46	0.00	1.00	196,809
Firm age above 25 years	0.24	0.43	0.00	1.00	196,809
Government share > 50%	0.13	0.34	0.00	1.00	196,809
(Limited) partnership	0.09	0.29	0.00	1.00	185,921
Medium-sized firm	0.93	0.26	0.00	1.00	196,809
Weak dep. on ext. finance	0.59	0.49	0.00	1.00	196,809
Low technology	0.92	0.27	0.00	1.00	196,809
Trading firm	0.44	0.50	0.00	1.00	196,809
Output tariff	8.92	6.36	0.00	332.45	196,809
Input tariff	2.67	1.67	0.03	7.41	196,809
Non-tariff measure (WITS)	0.70	0.46	0.00	1.00	196,809
Switch in t	0.15	0.35	0.00	1.00	196,809
Switch into binding regulation	0.01	0.11	0.00	1.00	196,809
Switch into non-binding regulation	0.01	0.10	0.00	1.00	196,809
Switch within binding regulation	0.00	0.06	0.00	1.00	196,809
Switch within non-binding regulation	0.12	0.32	0.00	1.00	196,809
Exit in t	0.04	0.20	0.00	1.00	196,809
Entry in t	0.01	0.12	0.00	1.00	196,809

Table B4: Summary statistics by sectors in 2001, 2007 and 2015

	Binding regulation			FDI share			ln(*TFP*)		
	2000	2007	2015	2000	2007	2015	2000	2007	2015
Food products and beverages	0.01	0.28	0.16	0.02	0.03	0.04	9.57	9.64	10.35
Tobacco products	0.00	0.27	0.76	0.00	0.00	0.01	8.66	8.66	10.76
Textiles	0.00	0.11	0.10	0.06	0.04	0.06	10.85	10.65	11.34
Wearing apparel	0.00	0.00	0.03	0.05	0.05	0.09	9.53	9.44	10.27
Leather and leather products	0.00	0.00	0.00	0.06	0.07	0.10	11.08	10.89	11.94
Wood and wood products, except furniture	0.33	0.31	0.32	0.04	0.04	0.06	10.24	9.95	10.66
Pulp, paper and paper products	0.01	0.03	0.01	0.05	0.07	0.11	10.81	10.77	11.54
Publishing, printing and recorded media	0.00	0.18	0.02	0.01	0.01	0.02	10.80	11.16	11.53
Coke, refined petroleum products and nuclear fuel	0.00	0.00	0.00	0.31	0.00	0.07	10.50	10.74	11.17
Chemicals and chemical products	0.09	0.36	0.29	0.14	0.16	0.18	12.51	12.62	13.40
Rubber and plastics products	0.00	0.00	0.09	0.07	0.10	0.12	11.49	11.64	12.44
Other non-metallic mineral products	0.00	0.71	0.11	0.02	0.01	0.03	9.79	9.72	10.66
Basic metals	0.00	0.15	0.26	0.22	0.18	0.20	11.61	11.62	11.87
Fabricated metal products	0.00	0.06	0.04	0.09	0.13	0.13	10.71	10.96	11.88
Machinery and equipment	0.02	0.19	0.16	0.12	0.20	0.22	10.46	10.83	11.54
Electrical equipment, office machinery, computers	0.00	0.00	0.00	0.27	0.32	0.24	12.57	12.52	13.38
Radio, television and communication equipment	0.00	0.00	0.00	0.55	0.61	0.58	12.95	12.54	13.30
Medical, precision and optical instruments	0.00	0.00	0.00	0.20	0.27	0.26	11.82	11.45	11.97
Motor vehicles	0.00	0.00	0.00	0.13	0.20	0.25	12.05	12.14	13.35
Other transport equipment	0.00	0.64	0.47	0.07	0.12	0.20	10.63	11.38	11.85
Furniture and n.e.c.	0.00	0.14	0.10	0.05	0.06	0.11	10.11	10.11	11.12
Total	0.03	0.23	0.18	0.05	0.05	0.07	10.17	10.18	11.13

Note: Average share of regulated firms, average foreign capital share and average log productivity within sectors. Number of observations in 2001: 11,968; 2007: 13,347; 2015: 9,791.

Table B5: Baseline results including control coefficients: Regulation, FDI and productivity

Dependent variable:	FDI share		ln(*TFP*)		ln(VAD/*L*)	
	(1)	(2)	(3)	(4)	(5)	(6)
Regulated product	0.005***	0.005***	0.004	0.004	0.001	0.001
	(0.002)	(0.002)	(0.014)	(0.014)	(0.014)	(0.015)
Binding regulation	−0.008***	−0.008***	−0.036**	−0.037**	−0.029*	−0.030*
	(0.002)	(0.002)	(0.016)	(0.016)	(0.016)	(0.016)
Output tariff		−0.000		0.001		0.001
		(0.000)		(0.001)		(0.001)
Input tariff		−0.001		−0.007		−0.010*
		(0.001)		(0.006)		(0.006)
Non-tariff measure		0.000		0.007		0.009
		(0.002)		(0.014)		(0.014)
Firm age 25-50 years		0.003		0.039***		0.016
		(0.002)		(0.012)		(0.013)
Firm age 50-75 years		0.005**		0.058***		0.028
		(0.002)		(0.017)		(0.018)
Firm age over 75 years		0.001		0.056**		0.031
		(0.003)		(0.024)		(0.024)
Government share > 50%		−0.003		−0.036***		−0.052***
		(0.002)		(0.012)		(0.013)
Industry-year interactions	Yes	Yes	Yes	Yes	Yes	Yes
Island-year interactions	Yes	Yes	Yes	Yes	Yes	Yes
Product traits in 2005 × Year	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant product traits	Yes	Yes	Yes	Yes	Yes	Yes
Firm traits specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	180,783	180,783	180,783	180,783	180,783	180,783
Firms	24,725	24,725	24,725	24,725	24,725	24,725
R-squared	0.872	0.872	0.812	0.812	0.737	0.737

Note: The dependent variable is the share of foreign capital, log of total factor productivity or log of value added per worker within each firm. Five-digit product traits in 2005 include sector concentration of sales, the share of blue-collar workers and the share of public enterprises. For full list of time-variant product traits see table 4.1. Initial firm-level traits include foreign capital share as well as firm size, legal status and public enterprise indicators and allow for trait-specific linear trends. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B6: Impact on productivity while controlling for FDI

Dependent variable:	ln(*TFP*)	ln(VAD/*L*)
	(1)	(2)
Regulated product	0.003	0.000
	(0.014)	(0.015)
Binding regulation	−0.036**	−0.029*
	(0.016)	(0.016)
FDI share	0.030	0.021
	(0.026)	(0.026)
Basic controls	Yes	Yes
Industry-year interactions	Yes	Yes
Island-year interactions	Yes	Yes
Product traits in 2005 × Year	Yes	Yes
Time-variant product traits	Yes	Yes
Firm traits specific trend	Yes	Yes
Observations	180,783	180,783
Firms	24,725	24,725
R-squared	0.812	0.737

Note: The dependent variable is log total factor productivity or log value added per worker. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B7: Robustness: More restrictive fixed effects and error clustering

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent: FDI share					
Regulated product	0.005***	0.005***	0.005***	0.004**	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Binding regulation	−0.007***	−0.008***	−0.008***	−0.008***	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: Dependent: ln(*TFP*)					
Regulated product	0.007	0.004	0.004	0.004	0.004
	(0.014)	(0.014)	(0.020)	(0.015)	(0.020)
Binding regulation	−0.036**	−0.037**	−0.037*	−0.034**	−0.034*
	(0.016)	(0.016)	(0.020)	(0.016)	(0.020)
Panel C: Dependent: ln(VAD/L)					
Regulated product	−0.002	0.001	0.001	−0.004	−0.004
	(0.015)	(0.015)	(0.019)	(0.015)	(0.019)
Binding regulation	−0.029*	−0.030*	−0.030	−0.026	−0.026
	(0.016)	(0.016)	(0.020)	(0.017)	(0.020)
Basic controls	Yes	Yes	Yes	Yes	Yes
Sector-year interactions	Yes				
Industry-year interactions		Yes	Yes		
Island-year interactions	Yes	Yes	Yes		
Industry-island-year interactions				Yes	Yes
Product traits in 2005 × Year	Yes	Yes	Yes	Yes	Yes
Time-variant product traits	Yes	Yes	Yes	Yes	Yes
Firm traits specific trend	Yes	Yes	Yes	Yes	Yes
Firm level cluster	Yes	Yes		Yes	
Product-year level cluster			Yes		Yes
Observations	180,797	180,783	180,783	180,432	180,432
Number of clusters	24,726	24,725	4,219	24,691	4,214

Note: The dependent variable is the foreign capital share within each firm, log total factor productivity or log value added per worker. All regressions are specified according to column 4 of table 4.3 except for the inclusion of fixed effects and the clustering level of standard errors. Sectors are defined at the two two-digit level, industries at the three-digit level, products at the five-digit level. Robust standard errors are clustered on firm or product-year level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B8: Heterogeneity by external financial dependence and technology (regulated product)

	Coeff	SE	Coeff	SE	p-value: 1=2
Panel A					
Regulated product ×	Weak dep. on ext. finance		Strong dep. on ext. finance		
Dependent:					
FDI share	0.005**	(0.002)	0.006*	(0.003)	[0.725]
ln(*TFP*)	0.025	(0.018)	−0.021	(0.023)	[0.103]
ln(VAD/*L*)	0.016	(0.018)	−0.008	(0.023)	[0.403]
ln(*K*)	−0.003	(0.024)	0.017	(0.030)	[0.609]
ln(Foreign *K*)	0.104***	(0.038)	0.167**	(0.074)	[0.437]
ln(Private *K*)	−0.091*	(0.052)	−0.044	(0.070)	[0.572]
ln(Gov.t *K*)	0.060	(0.037)	0.056	(0.046)	[0.942]
Panel B					
Regulated product ×	Low tech. sector		High tech. sector		
Dependent:					
FDI share	0.004**	(0.002)	0.034***	(0.013)	[0.019]
ln(*TFP*)	0.001	(0.015)	0.087	(0.073)	[0.248]
ln(VAD/*L*)	−0.000	(0.015)	0.070	(0.069)	[0.321]
ln(*K*)	−0.002	(0.019)	0.115	(0.108)	[0.282]
ln(Foreign *K*)	0.093***	(0.035)	0.979***	(0.332)	[0.008]
ln(Private *K*)	−0.061	(0.044)	−0.281	(0.254)	[0.392]
ln(Gov.t *K*)	0.053*	(0.031)	0.047	(0.122)	[0.957]

Note: The dependent variables are listed in the first column, indicator variables interacted with Binding regulation on the top of each panel. All regressions are specified according to column 4 of table 4.3 and also include interactions of the reported indicator variables with Binding regulation (reported in table 4.6). The last column tests whether the reported interaction terms are statistically different from each other. For number of observations see table 4.4. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B9: Heterogeneity by firm size and trading (binding regulation)

	Coeff	SE	Coeff	SE	p-value: 1=2
Panel A					
Binding regulation ×	Medium-sized firm		Large firm		
Dependent:					
FDI share	−0.004**	(0.002)	−0.055***	(0.020)	[0.013]
ln(TFP)	−0.042**	(0.016)	−0.047	(0.075)	[0.951]
ln(VAD/L)	−0.034**	(0.017)	−0.041	(0.081)	[0.929]
ln(K)	0.030	(0.021)	−0.147	(0.093)	[0.065]
ln(Foreign K)	−0.105***	(0.036)	−1.661***	(0.470)	[0.001]
ln(Private K)	0.056	(0.046)	0.211	(0.357)	[0.669]
ln(Gov.t K)	0.023	(0.032)	−0.089	(0.151)	[0.460]
Panel B					
Binding regulation ×	Trading firm		Non-trading firm		
Dependent:					
FDI share	−0.013***	(0.004)	−0.003*	(0.002)	[0.013]
ln(TFP)	−0.042*	(0.023)	−0.030	(0.021)	[0.697]
ln(VAD/L)	−0.044*	(0.024)	−0.016	(0.021)	[0.356]
ln(K)	0.045	(0.031)	0.004	(0.025)	[0.284]
ln(Foreign K)	−0.340***	(0.077)	−0.075**	(0.036)	[0.001]
ln(Private K)	0.118	(0.076)	0.053	(0.051)	[0.440]
ln(Gov.t K)	0.053	(0.051)	−0.003	(0.039)	[0.346]

Note: The dependent variables are listed in the first column, indicator variables interacted with Binding regulation on the top of each panel. All regressions are specified according to column 4 of table 4.3 and also include interactions of the reported indicator variables with Regulated product (reported in table B10). The last column tests whether the reported interaction terms are statistically different from each other. For number of observations see table 4.4. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B10: Heterogeneity by firm size and trading (regulated product)

	Coeff	SE	Coeff	SE	p-value: 1=2
Panel A					
Regulated product ×	Medium-sized firm		Large firm		
Dependent:					
FDI share	0.004**	(0.002)	0.036*	(0.019)	[0.103]
ln(*TFP*)	0.001	(0.015)	0.075	(0.068)	[0.283]
ln(VAD/*L*)	−0.002	(0.015)	0.065	(0.073)	[0.358]
ln(*K*)	−0.007	(0.019)	0.187**	(0.088)	[0.032]
ln(Foreign *K*)	0.081**	(0.033)	1.255***	(0.466)	[0.012]
ln(Private *K*)	−0.081*	(0.044)	0.036	(0.341)	[0.734]
ln(Gov.t *K*)	0.051*	(0.031)	0.225*	(0.120)	[0.148]
Panel B					
Regulated product ×	Trading firm		Non-trading firm		
Dependent:					
FDI share	0.012***	(0.003)	−0.001	(0.002)	[0.000]
ln(*TFP*)	−0.015	(0.019)	0.023	(0.018)	[0.109]
ln(VAD/*L*)	−0.013	(0.020)	0.017	(0.018)	[0.200]
ln(*K*)	−0.011	(0.028)	0.011	(0.023)	[0.515]
ln(Foreign *K*)	0.293***	(0.062)	−0.025	(0.034)	[0.000]
ln(Private *K*)	−0.126*	(0.065)	−0.029	(0.045)	[0.160]
ln(Gov.t *K*)	0.035	(0.044)	0.080**	(0.032)	[0.345]

Note: The dependent variables are listed in the first column, indicator variables interacted with Binding regulation on the top of each panel. All regressions are specified according to column 4 of table 4.3 and also include interactions of the reported indicator variables with Binding regulation (reported in table B9). The last column tests whether the reported interaction terms are statistically different from each other. For number of observations see table 4.4. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table B11: Robustness: Levels of TFP estimation and sector-specific deflators

Dependent variable:	ln(*TFP*)				ln(*TFP*), 3-digit	
	(1)	(2)	(3)	(4)	(5)	(6)
Regulated product	0.012	0.004	0.013	0.006	0.004	−0.006
	(0.014)	(0.014)	(0.015)	(0.015)	(0.014)	(0.014)
Binding regulation	−0.044***	−0.037**	−0.045***	−0.039**	−0.039**	−0.032**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year interactions	Yes	Yes	Yes	Yes	Yes	Yes
Island-year interactions	Yes	Yes	Yes	Yes	Yes	Yes
Product traits in 2005 × Year		Yes		Yes		Yes
Time-variant product traits		Yes		Yes		Yes
Firm traits specific trend		Yes		Yes		Yes
5-digit sector-specific deflators			Yes	Yes	Yes	Yes
Observations	180,783	180,783	180,534	180,534	180,534	180,534
Firms	24,725	24,725	24,714	24,714	24,714	24,714
R-squared	0.811	0.812	0.819	0.820	0.832	0.832

Note: The dependent variable is log total factor productivity as estimated on the two digit (columns 1 and 2) and three digit sector level (columns 3 to 6). Columns 5 and 6 additionally use five-digit product-specific input and wholesale price deflators. All regressions are specified according to column 4 of table 4.3. Robust standard errors are clustered on firm level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

APPENDIX C

Regulating FDI in Indonesia's manufacturing sector: Local labor market responses to a protectionist policy

C.1 Economy-wide regulation

Throughout the paper, we only measure regulatory penetration in the manufacturing sector but not in the rest of the economy. However, economy-wide employment effects may both arise from spillovers from regulated manufacturing firms or from unobserved regulation in agriculture or services. To check for economy-wide regulation, we construct alternative measures of LRP based on the *Economic Census* from 2006, which covers the universe of Indonesian service firms plus micro and small firms in manufacturing. Similar to our main LRP measure (see section 5.3), we extract firm employment at five-digit product level for each firm in 2006 and use it for construction of the initial employment composition. Initial product employment then enters equation 5.1 in terms of $L^{f}_{kpd,0}$ and, like before, is interacted with the group-specific regulatory status.

First, this allows us to construct a measure of manufacturing sector regulation that covers not only medium-sized and large firm employment, but further includes micro and small manufacturing enterprises. For this purpose, we add all micro and small firms to our original firm-level dataset of medium-sized and large enterprises in manufacturing and construct LRP accordingly. Second, we use employment numbers from all firms in manufacturing and services to construct an LRP measure that covers economy-wide regulatory penetration.

These alternative measures have some drawbacks. First, since we only observe each firm once (in 2006), we cannot systematically check for outliers in the data over time. In the *SI*, we are able to deal with outliers by calculating the median firm employment over several years. Second, the conversion of sector codes from several KBLI versions induces some ambiguity, which is especially pronounced in the tertiary sector. Even though we cross-checked all classifications, we may still introduce measurement error to our regression. Finally, the *Economic Census* does not include the agricultural sector. We also do not have sufficiently detailed information on agricultural employment in the labor market survey to measure regulatory penetration in a meaningful way.

C.2 Instrumental variable approach

As LRP is a measure of FDI regulation, it is straightforward to expect that the regulatory effect should run through adjustments in FDI stocks. Genthner and Kis-Katos (2019) show that regulation by the NIL is linked to a differential reduction in foreign capital shares within firms that produce regulated products. The same effect should also be visible on the aggregate level. Many districts, however, do not have positive FDI stocks in the data, thereby shifting the effect of LRP on FDI stocks towards zero. We thus borrow from Nunn and Qian (2014) who interact their original instrument with the likelihood to be treated at all. In our case, we construct a variable that measures the share of years with positive FDI stocks between 2001 and 2005 in a given district. We then interact LRP with this propensity to receive FDI and thereby assign a lower weight to those districts that historically did not host FDI. The first stage of the instrumental variable approach in our panel regression setting then is:

$$FDI_{dt} = \alpha_1 \, LRP_{dt} \times \overline{S}_{d0} + \boldsymbol{X}'_{d,0} \alpha_2 \times t + \gamma_d + \phi_{rt} + \varepsilon_{dt}, \tag{C1}$$

where FDI_{dt} is the inverse hyperbolic sine of a district's FDI stock and $\overline{S}_{d,0}$ is the share of years with non-zero FDI stocks per district d between 2001 and 2005. In a second step, we regress the employment rate on the predicted FDI stocks from equation (C1), resulting in

$$y_{dt} = \delta_1 \, F\hat{D}I_{dt} + \boldsymbol{X}'_{d,0} \delta_2 \times t + \gamma_d + \phi_{rt} + \varepsilon_{dt}. \tag{C2}$$

For the exclusion restriction to hold, FDI regulation should affect labor market outcomes only through its effect on FDI stocks.

Table C9 shows the results of the IV approach. On the first stage in Panel B, we find a negative relationship between LRP (weighted by the propensity to host FDI) and actual FDI stocks in a district. According to the F-statistics of the first stage, the instrument turns out to be rather weak, which may also explain the insignificant effect of instrumented FDI stocks on the total employment rate in our preferred specification in column 4. In less strictly specified regressions, however, the coefficient turns significantly negative. This is in line with our main results since a reduction of FDI stocks (due to regulation) is associated with an increase in the employment rate.

Our instrumental variable approach most likely does not yield stronger results because of a weak instrument in the first stage. Additionally, the exclusion restriction may not hold as LRP could also affect employment through alternative channels such as expectations or investment uncertainty. Including FDI stocks as additional control in the reduced form regression only leads to a small reduction of the LRP coefficient, which reinforces doubts about the exclusion restriction.

C.3 Robustness: Possible confounders

This section presents a detailed discussion of a series of robustness tests to alleviate concerns that our main results are driven by confounding factors. This is particularly relevant given that FDI regulation itself is an outcome of the political process and thus may reflect alternative economic dynamics that spuriously affect employment rates. Tables simultaneously include estimates of the long-difference result based on the Economic Census in Panel A and the fixed effects panel coefficient based on the household-level data in Panel B, respectively specified according to equations (5.2) or (5.3).

C.3.1 Political economy factors

In table C10 we test the robustness of our main results to a selected list of political factors. Columns 1 to 3 check whether our results are driven by the concentration of market power within the districts. If sales are concentrated among few firms, these companies may have more power to lobby for (or against) FDI protection as they face lower costs of coordination and can thus pursue their interests more easily and effectively (Grossman and Helpman 1994). In a similar vein, firms that employ a higher share of the total district workforce can more easily push for protectionist regulation. We control for market power by extending the list of initial conditions by a Herfindahl index of sales or employment concentration in each district. Column 3 also controls for the initial presence of national champion firms in a district. For this, we rank firms in each five-digit product market by their total sales and then calculate their employment share within districts. Firms of high relevance to the national economy may get a special treatment.

In columns 4 and 5, we include controls for the historical presence of state-owned enterprises. Genthner and Kis-Katos (2019) showed that public enterprise status at the beginning of the 2000s and the subsequent privatization are among the most important factors that are positively linked to later FDI regulation (see also table C1). We therefore add the initial share of employment in both public and recently privatized enterprises to make sure that our results are not driven by districts with high historical presence of state-owned companies, which are on different employment trajectories. Next, we include the share of district employment within companies considered to be small and medium-sized by the official regulatory legislation in column 6. One important condition within the NIL is to exclude small and medium-sized enterprises (SME) from regulation while still regulating large firms.[64] If the employment dynamics followed a different trend in districts that have many SMEs as compared to districts with large firms, our LRP measure may mechanically pick up this difference.

Columns 7 and 8 introduce proxies for the presence of vulnerable employment in a district. If decisions on product coverage of the NIL take social protection into consideration, our employment results may be originating from particular trends in those dimensions and not regulation itself. We therefore control for the initial share of low-skilled employment, which can be considered especially vulnerable in the context of foreign competition, or the average wage per worker in manufacturing, as industries with lower wages need more social protection (Gawande and Krishna 2003, Topalova and Khandelwal 2011).[65]

Alternatively, firms that are highly dependent on external financing may lobby against FDI regulation. By contrast, policy makers may also refrain from hurting local economies by not restricting their access to capital. Column 9 therefore tests for the initial share of employment in highly credit-dependent industries (Rajan and Zingales 1998).[66] Column 10 uses the distance from a district's centroid to the national capital, Jakarta, as a proxy for political connectivity. Complaints about Java-centered politics abound within Indonesia; especially people living in the periphery (such as Papua or Sulawesi) often voice concerns that public money is distributed in favor of Java and parts of Sumatra (The Economist 2019).

Finally, column 11 controls for district splits during the decentralization process in Indonesia. Bazzi and Gudgeon (2021) show that district splits lead to the creation of jobs in the public administration due to the creation of new governments in the child districts. If the timing of district splits is also correlated with FDI regulation, we may spuriously capture the employment effect of the decentralization process. We therefore include a time-variant dummy variable that indicates if a mother district (in 2000 borders) experienced a split in a particular year. We also include its lagged value to allow for a delayed effect of job creation. For the long-difference regression, we only control for the change in the district split dummy between 2006 and 2016.

Throughout all our robustness checks, the coefficient of interest does not vary in magnitude and remains statistically significant. This also holds when including all the above-mentioned initial or time-variant controls at the same time in column 12. We

[64] Firm size is defined by Presidential Decree No. 36/2010 (which refers to law 20/2008 on small and medium-sized enterprises). According to this decree, a firm is considered large if either its annual sales exceed 50 billion IDR or its net assets are larger than 10 billion IDR. All firms with both sales and net assets below these thresholds are considered small or medium-sized in our sample.

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

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

view this as support for a regulation-driven increase in district employment and do not find strong evidence of region-specific political economy dynamics. This is not to say that political economy considerations did not affect product selection in the NIL. But there is no direct evidence that would point towards a precise regional targeting of this protectionist policy.

C.3.2 Exposure to global dynamics

Our baseline results do not correct for the possibility that particular districts have been more severely affected by the 2009 global financial crisis in relative terms. Despite the fact that Indonesia turned out to be relatively immune to the downturn of global trade and financing, this singular event could be driving our results if the local exposure to protectionism and to the impact of the global financial crisis were correlated. For instance, districts that are more involved in global trade may experience smaller employment growth because firms in these regions had to downscale their employment in response to declining foreign demand. If policy makers refrained from regulating FDI especially in products that are predominantly produced in these districts, our results will suffer from omitted variable bias. The first column of appendix table C11 therefore adds the initial level of import and export volume by district and allows for linear time effects in the fixed effects panel setting. The effect of LRP on district employment growth is barely reduced and still highly significant. Similarly, we still detect a significant positive effect when controlling for contemporaneous shocks in trade flows by including the time-variant figures of each district's imports and exports (or their change between 2006 and 2016) directly in column 2. Even though Indonesia has liberalized its foreign trade during the 1990s, column 3 controls for (changes in) time-variant tariffs and the share of industrial employment which was potentially affected by non-tariff measures (NTMs).[67] FDI regulation could still be used for an immediate response by political actors to balance minor tariff reductions over our sample period. Column 3 shows that our results are not driven by concurrent trade liberalization dynamics.

The increasing importance of automation in the industrial production process calls for a restructuring of employment within firms and potentially leads to layoffs of the routine-task work force (e.g., Acemoglu and Restrepo 2019). If FDI regulation is particularly used to protect districts that show a relatively high potential of automation, our measure of regulatory penetration would pick up some of the relative employment losses due to automation and, thus, will be downward biased. As a proxy for automa-

[67] Output tariffs and NTM indicators are retrieved from the UNCTAD-TRAINS database (United Nations 2019b). We construct input tariffs using input-output tables as it is standard in the literature (cf. Amiti and Konings 2007) and then merge tariff and NTM information to the firm data. Our tariff and non-tariff measures are weighted by initial firm employment.

tion, we add the average time-varying stock of industrial robots (or its change between 2006 and 2016) to our set of controls.[68] We weight yearly stocks in an industry by firm employment in the respective district and year to account for the labor force which is potentially affected by mechanization. Another concern may be that FDI regulation is especially pronounced within districts that exhibit a relatively large potential for technological upgrading. Even though there is no evidence for a particular targeting of high-technology sectors by the NIL (Genthner and Kis-Katos 2019), we still want to exclude this potential source of endogeneity. The direction of the bias is ex-ante ambiguous, since it is not clear whether high-tech firms may increase or decrease their workforce over time. Columns 4 and 5 of table C11 alleviate concerns about highly regulated sectors being more prone to automation or having a larger concentration of high-tech firms, by controlling for industrial robots on the one hand and trends in the initial share of employment in high-technology enterprises on the other hand.[69] Irrespective of the specification, the coefficient of interest does not change. Therefore, we are confident that regional exposure to automation does not spuriously drive our result.

C.3.3 Agglomeration and further labor market dynamics

The LRP measure is constructed by summing up each firm's regulatory status over districts and years and then weighting it by the initial share of firm employment in the total labor force. One concern here is that we do not only capture changes in regulation over time but that our results reflect the relative importance of agglomeration in manufacturing within particular locations. If industrial areas followed different employment dynamics than the remaining regions, our share component within the LRP measure may also be correlated with regional differences in agglomeration dynamics.

To test for agglomeration dynamics, we first construct a time-invariant measure of the employment share within products that are never regulated throughout the whole time period. For our identification strategy to be valid, this part of firm employment must not affect the LRP coefficient, as this would be a clear indication of agglomeration driving the result. When including the initial share of never regulated employment to our controls in column 1 of table C12, the coefficient of interest remains robust and similar in size compared to our main result. Column 2 of table C12 uses an alternative proxy for agglomeration. The *SI* surveys of 2004 and 2005 include an item asking for

[68] Data on robot stocks comes from the International Federation of Robotics (IFR). The International Federation of Robotics provides comprehensive data on the operational stock of robots by country, year and industry (International Federaration of Robotics 2016). Note that the database reports zero stocks of operational robots until 2006. Thus, measuring the initial stocks of robots is redundant.

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

whether the plant is located within an industrial area. Based on this survey question, we compute initial district employment in industrial areas as an alternative measure of agglomeration potential.[70] The LRP coefficient barely changes when including trends of the alternative agglomeration proxy.

While agglomeration effects are demand-driven, another potential confounding factor may come from labor supply. If employment increased in more densely populated areas over time and LRP was correlated with this upward trend (as firms tend to be located next to metropolitan areas), this would invalidate our identification strategy. Similarly, firms may also be attracted to rapidly growing urban areas due to a more abundant labor supply in those regions. We thus use initial population density of a district, as well as its change between 2000 and 2005, as proxies for urbanization dynamics in columns 3 and 4 of table C12. In both panels, the coefficient of interest declines a little, but remains significantly positive. This shows that LRP at least partially picks up different trends across more and less urban regions, but we conclude that agglomeration effects from the supply side do not essentially drive our results.

As a last robustness check, we test whether our results are affected by labor market reforms. In particular, we control for minimum wage legislation. The classical labor market model without any frictions predicts that the introduction of binding minimum wages should result in unemployment. However, alternative models which allow for market imperfections show moderate positive employment effects from minimum wages (cf. Shapiro and Stiglitz 1984, Dickens et al. 1999). The empirical evidence of minimum wages on employment in developing countries is indeed mixed, with more systematic findings of dis-employment among low-skilled and workers in the formal economy (Neumark and Munguía Corella 2021).

As part of the decentralization efforts in Indonesia, minimum wage legislation was delegated to the provincial governments in 2001 (Widarti 2006). If the localized minimum wage setting was correlated with the introduction of the NIL in particular regions, LRP might pick up some of the effect of wage regulation. To exclude this possibility, column 5 of table C12 adds yearly minimum wages at the province level (or their change between 2006 and 2016) to our baseline specification. Our main estimate, however, is barely affected both in magnitude and significance. To account for any other province-level changes in labor market regulation (or other policy reforms that indirectly affect job creation), we finally replace island(-year) fixed effects with province(-year) fixed effects in Panel A (B). This specification more flexibly controls for a wide range of trends and shocks that occur at the level of 30 provinces, including but not limited to province-specific minimum wage legislation. However, this also absorb substantial variation in

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

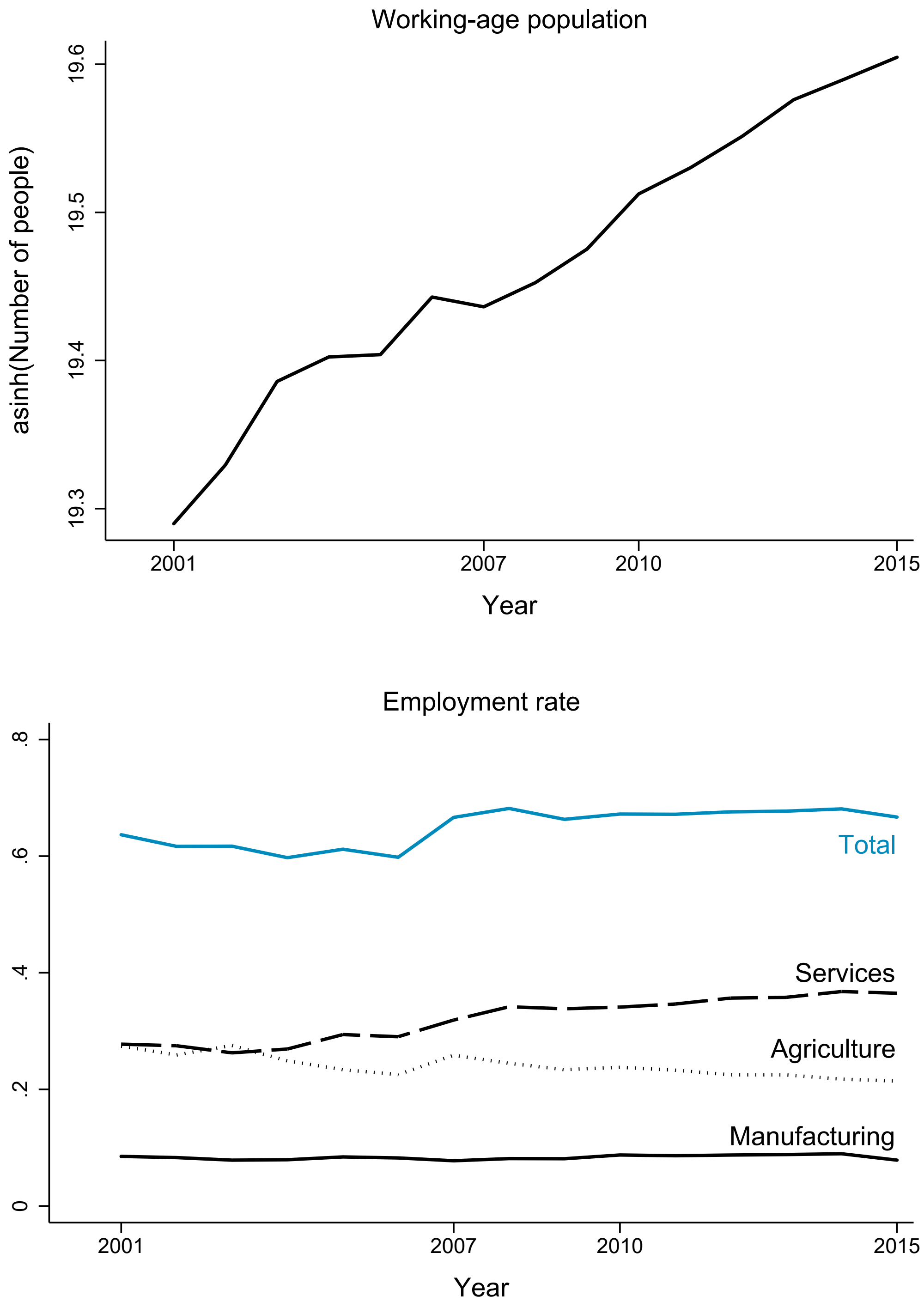
the change of LRP within the long-difference regression. In fact, column 6 shows that our long-difference results are not robust to including the very restrictive province indicators. In contrast, the LRP estimate in the panel regression only marginally changes in terms of magnitude and remains significant at the 5 percent level.

C.3.4 Migration

To check for the relevance of the migration channel, we use contemporaneous and past residency information (referring to five years before the current survey) that were collected within the *Susenas* household surveys. Unfortunately, this information is only available from 2011 onwards, allowing us to trace back migration decisions only until 2006. However, the data allows us to construct measures of not only immigration rates, but also of emigration rates based on the past district of residence. To account for the unknown timing of migration, our specifications are based on lagged values of LRP. In particular, we construct a measure of past LRP as the average of LRP and its lags up to $t-5$. Appendix table C13 shows the relationship between LRP and migrant shares. The dependent variable is either immigration and emigration rates or the share of employed migrants in the total district population. Our results in columns 1 and 2 show that regulatory penetration does not act as a pull factor to foster immigration. The coefficients take a negative sign and are not significantly different from zero. For emigration in columns 3 and 4, however, we find weak indications of a positive effect of regulatory penetration on emigration (though none of the coefficients turns statistically significant). Thus, the protectionist policy does not seem to be acting as a pull factor to attract migrants from other districts. This allows us to discard migration as a potential driving force behind the regulatory effects of employment increase.

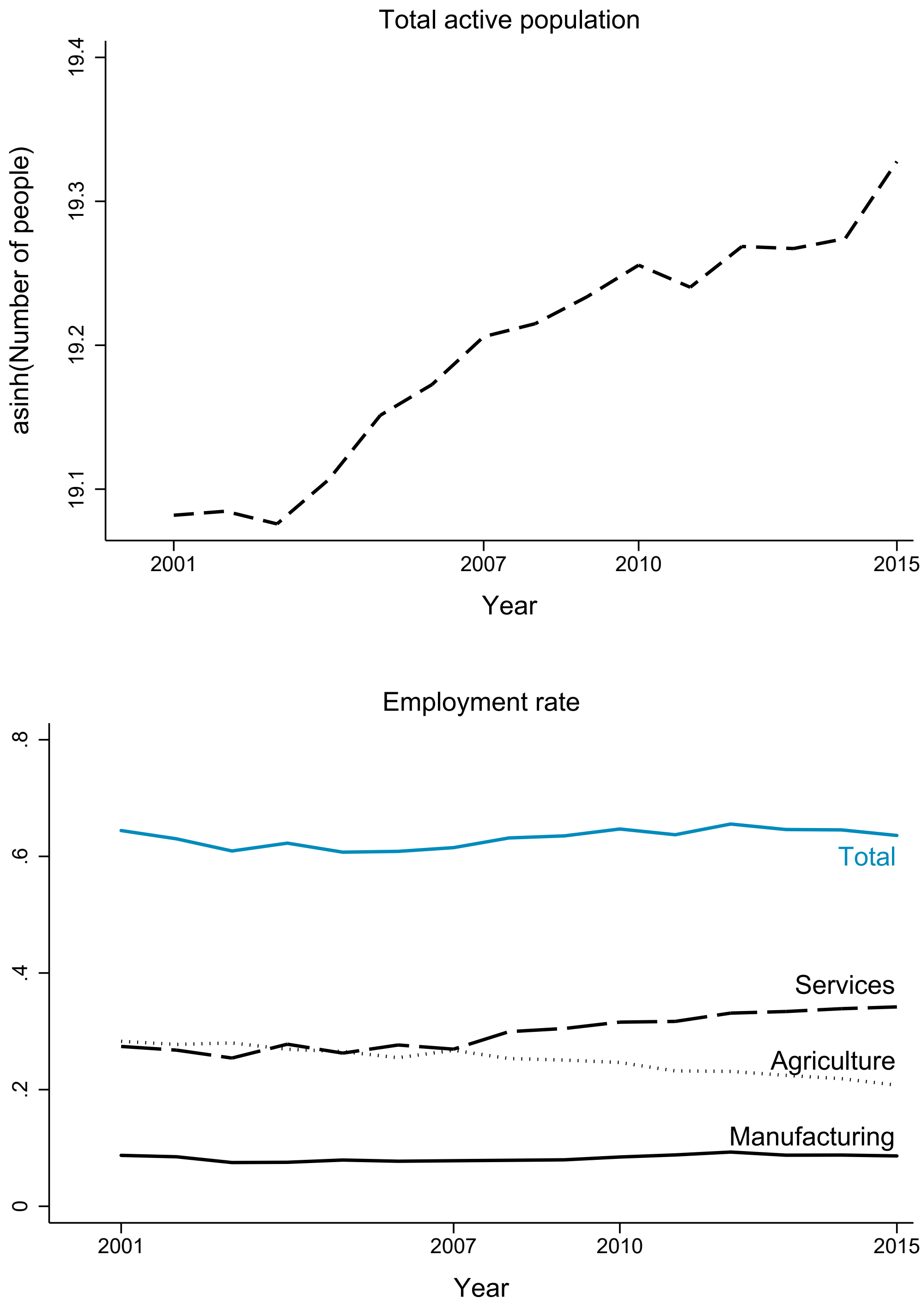
C.4 Additional figures

Figure C1: Working-age population and sectoral employment rates



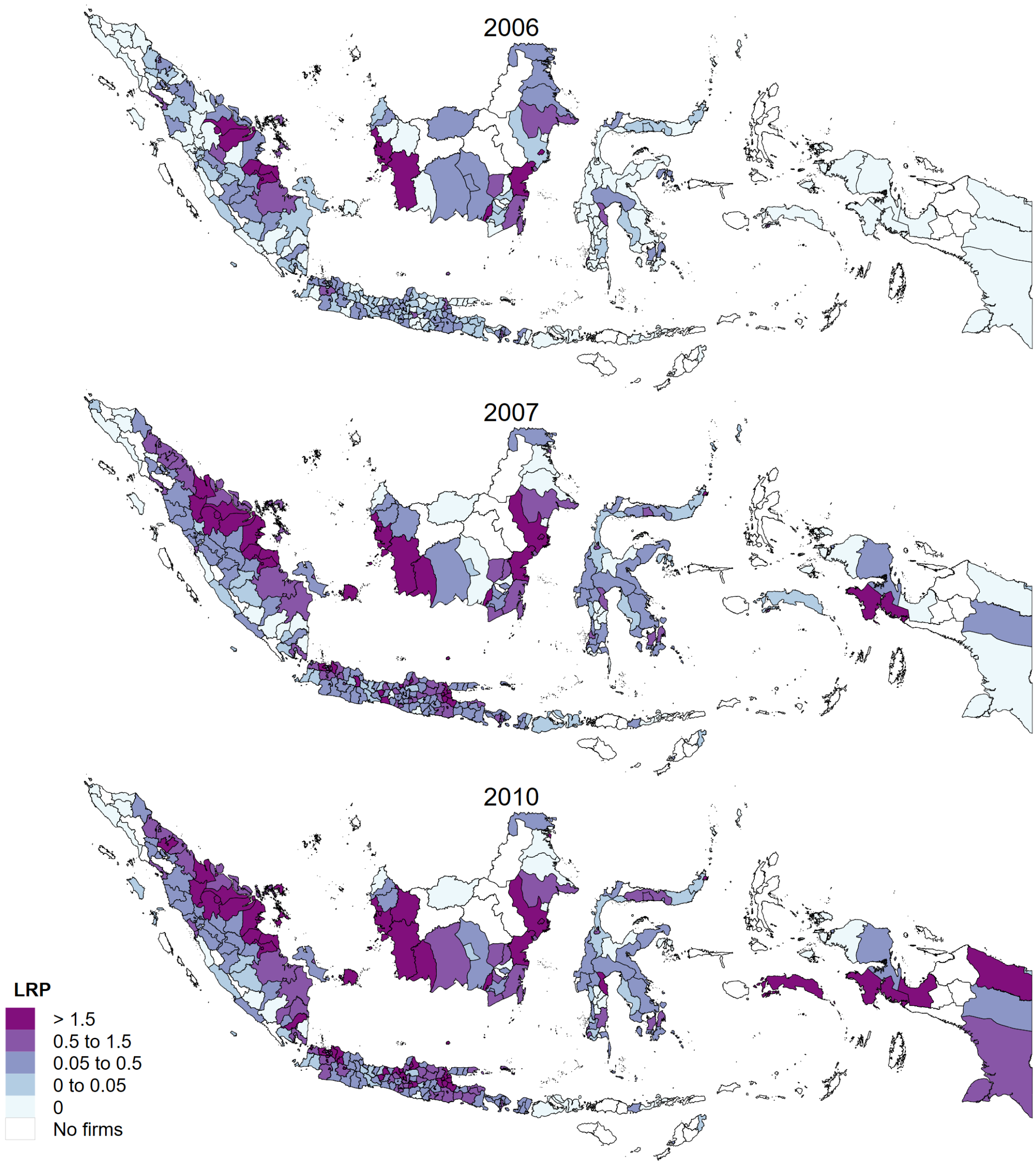
Note: Based on own calculations using the *Susenas* sample.

Figure C2: Active population and sectoral employment rates (*Sakernas*)



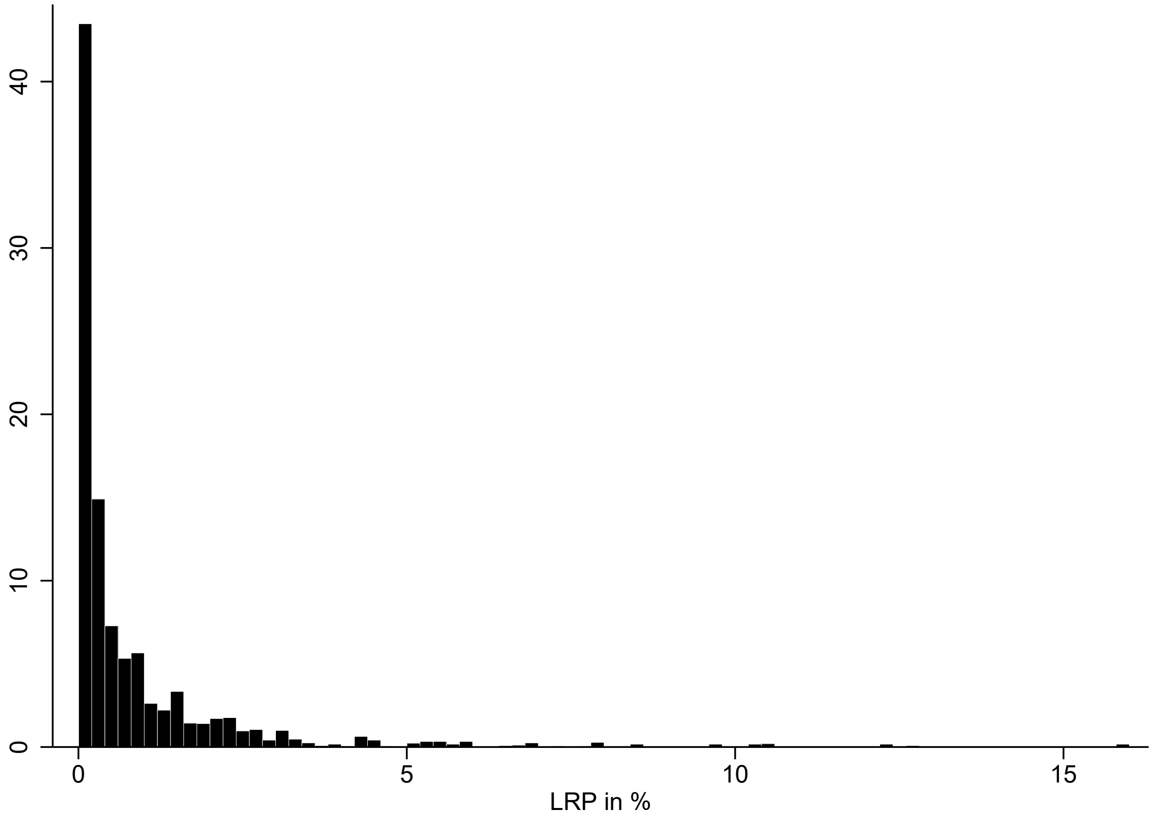
Note: Based on own calculations using the *Sakernas* sample.

Figure C3: LRP levels in 2006, 2007 and 2010



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

Figure C4: Density distribution of local regulatory penetration (LRP)



Note: Values are re-scaled by factor 100.

C.5 Additional tables

Table C1: Predictors of product-level regulatory penetration (Genthner and Kis-Katos 2019)

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

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

Table C2: Summary statistics of long-difference sample

	Long-difference sample				
	Mean	SD	Minimum	Maximum	Observations
Survei Industri variables:					
ΔLRP 2006-2010	0.84	1.40	−0.43	9.77	298
Economic Census variables:					
ΔEmployment rate	0.07	0.10	−0.11	0.71	298
in manufacturing	0.02	0.04	−0.13	0.44	298
in services	0.04	0.08	−0.12	0.63	298
Δasinh(Employment per firm)	0.12	0.17	−0.17	1.09	298
in manufacturing	−0.13	0.52	−5.65	1.21	298
in services	0.12	0.16	−0.26	1.26	298
Δasinh(Number of firms)	0.23	0.18	−0.61	0.94	298
in manufacturing	0.58	0.86	−0.88	7.62	298
in services	0.20	0.17	−0.53	0.77	298

Note: LRP is re-scaled by factor 100.

Table C3: Summary statistics of district-level panel

	Panel sample				
	Mean	SD	Minimum	Maximum	Observations
Survei Industri variables:					
LRP	0.71	1.52	0.00	15.92	4,339
Share of FDI years × LRP	0.42	1.15	0.00	10.62	4,339
asinh(FDI stock)	8.61	9.48	0.00	24.77	4,141
Susenas variables:					
Total employment rate	0.66	0.08	0.40	0.93	4,339
in agriculture	0.27	0.17	0.00	0.85	4,339
in manufacturing	0.07	0.06	0.00	0.43	4,339
in services	0.31	0.12	0.05	0.68	4,339
asinh(Monthly expenditure per capita)	13.60	0.40	12.67	15.07	4,339
Sakernas variables:					
Total employment rate	0.64	0.09	0.35	0.97	4,325
Unemployment rate	0.10	0.06	0.00	0.43	4,325
Total working hours per worker	38.95	5.89	11.86	61.19	4,325

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

Table C4: Robustness: Economy-wide regulatory penetration (Economic Census)

Dependent variable: ΔEmployment rate	Total	Manufacturing	Services
	(1)	(2)	(3)
Panel A: LRP of manufacturing			
ΔLRP 2006-2010	0.0090**	0.0044***	0.0048*
	(0.0035)	(0.0017)	(0.0025)
Panel B: LRP in total			
ΔLRP 2006-2010	0.0160***	0.0049***	0.0109***
	(0.0040)	(0.0014)	(0.0032)
Observations	298	298	298
Island FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes

Note: The dependent variable is the change in employment rates. LRP in full manufacturing (Panel A) and total LRP (Panel B) is generated using the *Economic Census* from 2006. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Robust standard errors are reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C5: Sectoral composition of LRP in selected years

2-digit manufacturing sector	Contribution to LRP in		
	2001	2007	2015
Food products and beverages	0.004	0.170	0.244
Tobacco products	0.000	0.102	0.131
Textiles	0.000	0.007	0.012
Wearing apparel	0.000	0.000	0.104
Leather and leather products	0.000	0.000	0.000
Wood and wood products	0.199	0.255	0.421
Pulp, paper and paper products	0.043	0.045	0.045
Publishing, printing and media	0.000	0.007	0.007
Coke, refined petroleum products	0.000	0.000	0.000
Chemicals and chemical products	0.016	0.040	0.033
Rubber and plastics products	0.000	0.001	0.052
Other non-metallic mineral products	0.000	0.044	0.009
Basic metals	0.000	0.002	0.002
Fabricated metal products	0.000	0.007	0.007
Machinery and equipment	0.000	0.005	0.005
Electrical equipment, office machinery	0.000	0.000	0.000
Radio, television and communication equipment	0.000	0.000	0.000
Medical, precision and optical instruments	0.000	0.000	0.000
Motor vehicles	0.000	0.000	0.000
Other transport equipment	0.000	0.016	0.019
Furniture and n.e.c.	0.000	0.028	0.024
Local regulatory penetration	0.262	0.726	1.113

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

Table C6: Summary statistics of districts per product and products per district in our samples

	Mean	5%	25%	Median	75%	95%
	(1)	(2)	(3)	(4)	(5)	(6)
Number of products per district	20.8	1	4	10	25	97
Number of regulated products per district	6.5	0	2	4	9	23
Number of districts per product	20.0	1	5	12	26	67
Number of districts per regulated product	6.3	0	0	0	1	35

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

Table C7: Alternative specifications: Impact of regulatory tightening between 2006 and 2010 on the change in employment (Economic Census)

	(1)	(2)	(3)	(4)
Panel A: Dependent variable: ΔEmployment rate				
ΔLRP 2006-2010	0.0096**	0.0085**	0.0109***	0.0095***
	(0.0043)	(0.0037)	(0.0041)	(0.0035)
Panel B: Dependent variable: Δasinh(Employment)				
ΔLRP 2006-2010	0.0161**	0.0173**	0.0232***	0.0204**
	(0.0077)	(0.0080)	(0.0086)	(0.0083)
Δasinh(Population)	0.7674***	0.7946***	0.8011***	0.7922***
	(0.1032)	(0.1234)	(0.1238)	(0.1203)
Observations	298	298	298	298
Island FE		Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$			Yes	Yes
$\mathbf{Sector}_{d,0}$				Yes

Note: The dependent variable is the change in employment rates in Panel A and the growth rate of employment in Panel B. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2006. Robust standard errors reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C8: Robustness checks according to shift-share literature

	Baseline	Initial LRP cluster	Exclude 3 sectors
	(1)	(2)	(3)
Panel A: Economic Census (ΔEmployment rate)			
ΔLRP 2006-2010	0.0095***	0.0095**	0.0068**
	(0.0035)	(0.0037)	(0.0034)
Observations	298	298	298
Island FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes
Panel B: Susenas (Employment rate)			
LRP	0.0020**	0.0020***	0.0020**
	(0.0009)	(0.0007)	(0.0010)
Observations	4,339	4,339	4,339
District FE	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. Column 1 reproduces the main results of Panel A in table 5.1 and column 4 in table 5.3. Column 2 groups districts based on percentiles in the initial distribution of LRP (resulting in 55 clusters), and column 3 excludes fabricated metals, publishing and media, as well as tobacco from LRP. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. If not stated otherwise, standard errors are robustly estimated (or clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C9: IV results: Impact of local regulatory penetration on FDI stocks and employment (*Susenas*)

Dependent variable:	Total employment rate				
	(1)	(2)	(3)	(4)	(5)
Panel A: Second stage					
asinh(FDI)	−0.0572**	−0.0036**	−0.0045**	−0.0017	−0.0017
	(0.0231)	(0.0018)	(0.0021)	(0.0017)	(0.0017)
Panel B: First stage					
Share of FDI years × LRP	−0.3612**	−1.0411***	−0.8820***	−0.7771***	−0.8211***
	(0.1587)	(0.2756)	(0.2510)	(0.2833)	(0.3071)
F-stat first stage	5.178	14.272	12.351	7.521	7.148
Observations	4,141	4,141	4,141	4,141	4,141
District FE	Yes	Yes	Yes	Yes	Yes
Island-year FE		Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends			Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends				Yes	
$\mathbf{LRP}$, $\mathbf{Sector}_{d,0}$ × Year					Yes

Note: The dependent variable is the total employment rate for the second stage results in Panel A, or the inverse hyperbolic sine of district FDI stocks in the first stage in Panel B. FDI stocks are instrumented by the interaction of a district's LRP and its share of years with positive FDI stocks between 2001 and 2005 (Nunn and Qian 2014). $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C10: Robustness: Political economy

	HI sales	HI labor	Nat. champs	State-owned	Privatized	SME	Low-skilled	Av. wage	Credit	JKT	Splits	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Economic Census (ΔEmployment rate)												
ΔLRP 2006-2010	0.0102***	0.0093***	0.0090***	0.0101***	0.0090**	0.0074**	0.0094***	0.0084**	0.0100***	0.0078**	0.0097***	0.0091**
	(0.0034)	(0.0035)	(0.0034)	(0.0035)	(0.0035)	(0.0036)	(0.0035)	(0.0034)	(0.0036)	(0.0035)	(0.0036)	(0.0038)
Observations	298	298	298	298	298	298	298	298	298	298	298	298
Island FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Susenas (Employment rate)												
LRP	0.0021**	0.0019**	0.0019**	0.0019**	0.0018*	0.0018*	0.0020**	0.0021**	0.0021**	0.0023**	0.0020**	0.0026***
	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0009)
Observations	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339	4,339
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Z}_{d,0}$(-specific trends)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		Yes
Control for											Yes	Yes

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Columns 1 and 2 extent the set of initial conditions by a Herfindahl sales or labor concentration index within districts. Column 3 adds trends in the initial prevalence of employment in national champion firms. Column 4 adds the share of employment in state-owned enterprises, while column 5 controls for the employment share of privatized firms between 2001 and 2005. Column 6 extends the set of initial conditions by the employment share in medium-sized firms. Column 7 adds the share of low-skilled employment and column 8 controls for the average wage per worker. Column 9 includes the share of workers that are employed in external finance dependent firms. Column 10 adds the distance to Jakarta, while column 11 controls for district splits. Column 12 includes all above-mentioned controls. Standard errors are robustly estimated (and clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C11: Robustness: Global financial crisis, trade, automation and high-technology firms

	Global crisis	Trade flows	Tariffs	Automation	High tech	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Economic Census (ΔEmployment rate)						
ΔLRP 2006-2010	0.0095**	0.0096***	0.0101***	0.0093***	0.0099***	0.0095**
	(0.0037)	(0.0035)	(0.0037)	(0.0035)	(0.0035)	(0.0037)
Observations	298	268	298	298	298	298
Island FE	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Susenas (Employment rate)						
LRP	0.0022**	0.0016*	0.0020**	0.0019**	0.0022**	0.0020**
	(0.0009)	(0.0009)	(0.0010)	(0.0009)	(0.0009)	(0.0010)
Observations	4,339	4,141	4,337	4,339	4,339	4,141
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Z}_{d,0}$(-specific trends)	Yes				Yes	Yes
Control for		Yes	Yes	Yes		Yes

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Column 1 extends the set of initial conditions by import and export volume. Columns 2 controls for trade flows by including time-variant import and export figures. Column 3 includes input and output tariffs as well as the share of employment affected by non-tariff measures. Column 4 controls for the stock of industrial robots in a district. Column 5 adds the employment share of high-technology firms according to OECD classification. Column 6 includes all above-mentioned controls. Standard errors are robustly estimated (and clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C12: Robustness: Agglomeration and labor market reform

	Never reg. L	Industrial area	Pop. density	Chg. pop. density	Min. wage	Province FE
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Economic Census (ΔEmployment rate)						
ΔLRP 2006-2010	0.0075**	0.0083**	0.0073**	0.0093***	0.0078**	0.0008
	(0.0034)	(0.0034)	(0.0033)	(0.0036)	(0.0034)	(0.0037)
Observations	298	298	298	298	298	298
Island FE	Yes	Yes	Yes	Yes	Yes	
$\mathbf{LRP}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Susenas (Employment rate)						
LRP	0.0020**	0.0021**	0.0017*	0.0020**	0.0020**	0.0020**
	(0.0010)	(0.0010)	(0.0009)	(0.0010)	(0.0009)	(0.0010)
Observations	4,339	4,339	4,339	4,339	4,339	4,339
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes	Yes	
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
$\mathbf{Z}_{d,0}$(-specific trends)	Yes	Yes	Yes	Yes		
Control for					Yes	
Province(-year) FE						Yes

Note: The dependent variable is the change in the total employment rate in Panel A, or the total employment rate in Panel B. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Column 1 extends the set of initial conditions by the initial share of never regulated product employment. Column 2 adds the share of employment in industrial areas (based on SI). Column 3 includes the initial population density for each district, while column 4 adds the change in population density between 2000 and 2005. Column 5 controls for minimum wages and column 6 further includes province-year fixed effects. Standard errors are robustly estimated (and clustered on district level in Panel B) and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C13: Impact of local regulatory penetration on migration

Dependent variable:	Immigration rate	of which employed	Emigration rate	of which employed
	(1)	(2)	(3)	(4)
Past LRP	−0.0006	−0.0006	0.0014	0.0017
	(0.0009)	(0.0007)	(0.0014)	(0.0012)
Observations	1,450	1,450	1,450	1,450
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes

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

Table C14: Robustness: Pre-trends and spillovers

Dependent variable:	Total employment rate		
	(1)	(2)	(3)
LRP	0.0015*	0.0023**	0.0018*
	(0.0008)	(0.0011)	(0.0009)
Spatial regulatory spillover			0.0097
			(0.0285)
Observations	4,339	4,249	4,339
$\mathbf{Z}_{d,0}$-specific trends	Pre-trend 01-05	Pre-trend 97-00	
District FE	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes

Note: The dependent variable is the total employment rate. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of agricultural, manufacturing and service employment in a district, all measured in 2005. Column 1 (2) controls for pre-trends in the employment rate between 2001 and 2005 (1997 and 2000). Spatial spillovers are calculated as total sum of LRP, weighted by the squared inverse distance. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table C15: Standard errors and rejection rate of the hypothesis $H_0 : \beta = 0$ at 5% significance level (Adão et al. 2019)

	Estimate		Median std. error	Rejection rate
	Mean (1)	Std. deviation (2)	(3)	(4)
Panel A: Economic Census				
Total employment rate	−0.00116	0.00615	0.00537	14.00%
Manufacturing	−0.00037	0.00307	0.00238	10.00%
Services	−0.00069	0.00435	0.00396	7.00%
Panel B: Susenas				
Total employment rate	0.00004	0.00060	0.00054	7.64%

Note: Panel A and B present results from the long-difference or the panel data setting, respectively. The left column indicates the dependent variable. Columns 1 and 2 show the mean and standard deviation of the OLS estimates of β_1 in equations (5.2) or (5.3) across the placebo samples, while column 3 indicates the median standard error estimates. Column 4 indicates the percentage of placebo samples for which we reject the null hypothesis $H_0 : \beta = 0$ using a 5% significance level test. Standard errors are clustered at the district level. Results are based on 10,000 placebo samples.

Table C16: Impact of local regulatory penetration on other labor outcomes

Dependent variable:	Activity rate	Employment rate	Unemployment rate	Working hrs/L
	(1)	(2)	(3)	(4)
LRP	0.0036**	0.0031**	0.0003	0.1528
	(0.0016)	(0.0015)	(0.0015)	(0.1142)
Observations	4,325	4,325	4,325	4,325
District FE	Yes	Yes	Yes	Yes
Island-year FE	Yes	Yes	Yes	Yes
$\mathbf{LRP}_{d,0}$-specific trends	Yes	Yes	Yes	Yes
$\mathbf{Sector}_{d,0}$-specific trends	Yes	Yes	Yes	Yes

Note: The dependent variable is the activity rate, total employment rate, unemployment rate or number of working hours per worker, all based on *Sakernas*. $\mathbf{LRP}_{d,0}$ controls for the initial level of LRP. $\mathbf{Sector}_{d,0}$ includes the share of manufacturing employment in a district, measured in 2005. Robust standard errors are clustered at district level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

APPENDIX D

Declaration for admission to the doctoral examination

I, Robert Genthner, confirm

- that the dissertation "Firm dynamics and regional development in Indonesia" that I submitted was produced independently without assistance from external parties, and not contrary to high scientific standards and integrity,
- that I have adhered to the examination regulations, including upholding a high degree of scientific integrity, which includes the strict and proper use of citations so that the inclusion of other ideas in the dissertation are clearly distinguished,
- that in the process of completing this doctoral thesis, no intermediaries were compensated to assist me neither with the admissions or preparation processes, and in this process,
  - No remuneration or equivalent compensation were provided
  - No services were engaged that may contradict the purpose of producing a doctoral thesis
- that I have not submitted this dissertation or parts of this dissertation elsewhere for the purpose of obtaining a doctoral degree.

I am aware that false claims (and the discovery of those false claims now, and in the future) with regards to the declaration for admission to the doctoral examination can lead to the invalidation or revoking of the doctoral degree.

Signed:

Date:

APPENDIX E

Author contributions

The main part of the thesis builds on four research papers. The contributions to each have been divided among the respective co-authors as follows:

1. **Heat and firm productivity: Evidence from Indonesia's manufacturing sector**
   The project is co-authored by Sebastian Renner and Enrica de Cian. All authors contributed to the conceptualization of the research idea. I mainly developed the research design and conducted the data preparation and analysis, while Sebastian Renner provided minor contributions. Sebastian Renner and I both contributed to the writing of the manuscript.

2. **What happens to FDI spillovers when input-output tables go granular?**
   The project is single-authored. Research design, data preparation and analysis, as well as the writing of the manuscript are my own work. Krisztina Kis-Katos gave feedback in the writing process.

3. **Foreign investment regulation and firm productivity: Granular evidence from Indonesia**
   The research project is co-authored by Krisztina Kis-Katos. We both contributed to the conceptualization of the research idea and the research design, as well as the writing of the manuscript. I did the data preparation and analysis. Krisztina Kis-Katos supervised the research project.

4. **Regulating FDI in Indonesia's manufacturing sector: Local labor market responses to a protectionist policy**
   The research project is co-authored by Krisztina Kis-Katos. We both contributed to the conceptualization of the research idea and the research design, as well as the writing of the manuscript. I did the data preparation and analysis. Krisztina Kis-Katos supervised the research project.

Signed:

Date: