The Aggregate Costs of Political Connections

Jonas Gathen, Toulouse School of Economics (TSE)*

20 May 2022

Abstract

This paper quantifies the costs of political connections in Indonesia under the authoritarian rule of Suharto at the end of the 1990s. I build a general equilibrium model in which politically connected firms benefit from output subsidies and endogenously spend resources on rent-seeking activities. The model is structurally estimated using rich firm-level data for the manufacturing sector and a firm-level measure of political connectedness based on a natural experiment. A major innovation is to non-parametrically identify the output subsidy from differences in distributions of revenue-based total factor productivity (TFP) across connected and non-connected firms. In general equilibrium, both the distribution and the level of subsidies to connected firms matter. I find that subsidies to connected firms are too high and dispersed, costing the economy between 1.0-4.7% of aggregate output. At most, 45% of these output costs can be explained by the misallocation of factors of production towards connected firms. The large remainder is explained by the costs of subsidizing connected firms instead of putting saved subsidies to more productive use.

*Email: jonas.gathen@tse-fr.eu. I am indebted to my advisors Fabrice Collard, Christian Hellwig and Stéphane Straub for their support throughout this project. I further thank, in no particular order, Ben Moll, Beatriz Simon Yarza, Felipe Brugués, Tim Lee, Nicolas Werquin, Mathias Reynaert, Matthias Meier, Patrick Fève, Ferenc Szucs, Mounu Prem and all participants at the TSE Macro workshop, the EEA/ESEM conference 2021 and the Mannheim Internal Seminar for helpful comments. I want to thank Bob Rijkers for sharing data, feedback and helpful information on using the measure of political connections for Indonesia used in this paper and Mushfiq Mobarak for kindly sharing their measure of political connections. I want to thank the World Bank Jakarta team, especially Massimiliano Calí, for gratefully sharing the manufacturing firm-level data for Indonesia including much-improved capital series and providing helpful feedback and suggestions on the project. I am also especially grateful to Alexandre Gaillard and Oscar Fentanes for many discussions. All remaining errors are my own.

1 Introduction

What are the economy-wide costs of corruption and favoritism among the political and economic elite? There is strong anecdotal and quantitative evidence that autocrats and their inner circles obtain special economic privileges to amass large fortunes. For example, wealth in excess of one-quarter of GDP was attributed to Putin's inner circle in Russia (Aslund 2019) and Tunisia's former dictator Ben Ali (Rijkers, Freund, and Nucifora 2017). This accumulation of wealth in the hands of a politically connected elite comes, among others, from corruption, unfair competition and systematic property rights violations and therefore is the sign of larger distortions that matter for the aggregate economy. This paper quantifies the extent of economy-wide costs of political connections by combining theory and detailed firm-level data from Indonesia under dictator Suharto.

A motivating example best illustrates the costs of political connections that this paper quantifies. In 1996, the Indonesian government decided to promote its national car industry by offering a generous combination of various tax and tariff exemptions to selected firms. Seemingly by coincidence, one day before the policy announcement, Suharto's son created a local car manufacturing company that ended up becoming the sole beneficiary of the government tax exemptions. These tax exemptions were awarded despite the company not operating a single auto assembly line. Eventually, another presidential decree by Suharto allowed his son's company to import cars instead and sell these at an effective tax rate that was about 90% lower than that faced by competitors (for details, see Hale 2001). Additionally, the government further supported the company by directly buying its cars. This example illustrates two main economy-wide costs of political connections. First are *misallocation* costs: direct and indirect subsidies led the connected car manufacturer to increase its operations and demand more inputs, pushing up input prices and crowding out productive capital and labor from other firms in the economy. These *misallocation* costs depend crucially on (1) how the government selects connected firms, (2) the extent of the subsidies and (3) whether the subsidies alleviate other distortions in the economy. The second main costs of political connections are *opportunity costs of public funds*: direct and indirect subsidies to connected firms are costly because these resources could be spent on alternative development objectives.

Few firms are connected, and the firms that are connected are large. I show this by drawing on detailed annual firm-level manufacturing census data and previous micro-empirical work by Mobarak and Purbasari (2006), who identify connected firms in Indonesia via a natural experiment.[1] In the data, connected firms

[1]The natural experiment follows Fisman (2001) and identifies all stock-listed firms that benefit from connections by looking at stock-price fluctuations in response to plausibly exogenous shocks to the health of dictator Suharto. Mobarak and Purbasari (2006) then find the remaining connected firms by exploiting a highly concentrated ownership network and link all connected firms to the micro-data.

make up only 1% of firms but 15% of total (value-added) revenue. In terms of revenue, the average connected firm is around twelve times larger than the average non-connected firm, which also holds within narrowly defined industries.

A key question to quantify economy-wide distortions from political connections is how much of this size difference is due to political connections and how much is due to other firm fundamentals that we may simply call *productivity*. I use a structural model to disentangle the role of selection from the benefits of political connections and quantify the costs of favors to connected firms. In the model, firms flexibly spend resources on rent-seeking activities that buy government favors, which directly affects their revenues. The benefits of political connections enter as an output subsidy that can be seen as a reduced-form net transfer from the government. This subsidy captures many of the channels through which political connections matter, such as lower taxes due to tax avoidance and evasion (Johnson and Mitton 2003; Do, Nguyen, and Tran 2017), output and input subsidies, preferential access to government contracts, state-owned land and natural resources (Brugués, Brugués, and Giambra 2018; Chen and Kung 2018; Schoenherr 2019; Straub 2014; Szucs 2017) as well as preferential access to institutions and infrastructure (Fisman and Wang 2015).[2] Ideally, we would want to directly observe or estimate these subsidies before studying how connected firms endogenously obtain them.

The identification of subsidies is difficult because they do not just enter as "wedges" that distort model-based first-order conditions as usually studied in the misallocation literature (Hsieh and Klenow 2009), but they also directly distort observed revenue. A key contribution of this paper is to show how to identify subsidies non-parametrically, that is, without making a functional form assumption on how rent-seeking activities buy government benefits. Identification only draws on a revenue-based measure of total factor productivity (TFP) for connected and non-connected firms. This measure of TFP captures a combination of subsidies and actual productivity. To separately identify them, I crucially rely on two main assumptions. The first is a monotonicity restriction that ensures that firms with the highest measured TFP also have the highest productivity. This restriction does not mean that subsidies need to increase in productivity, only that subsidies cannot decline too fast with firm productivity. The assumption can also be partly tested. The second main assumption is on the selection of politically connected firms, and is more restrictive. The assumption makes estimated subsidies dependent on a selection parameter that captures the degree of selection. The benefit of this assumption is that it gives intuitive bounds on estimated subsidies. The lower bound of estimated subsidies gives the lowest possible subsidies that are still in line with rational decision-

[2]While the identification of benefits allows for any combination of these factors, subsequent welfare estimates rely on the government paying for the benefits and them entering through revenue, as is the case for tax evasion, government subsidies and government demand.

making. This lower bound captures the case where most TFP is actual productivity so that connected firms are maximally selected. The upper bound of estimated subsidies instead gives the highest possible subsidies, whose limit is the case of no selection where connected firms are a representative sample of all firms.[3]

The structural estimation is based on a matching procedure that exploits observing non-connected firms that do not receive subsidies. The idea is to selectively sample connected firms from the population of non-connected firms and use the monotonicity restriction to order and then match them to the observed sample of connected firms to back out their productivities. Estimated subsidies reveal a high degree of selection, especially at the bottom of the productivity distribution. Based on both bounds, the least productive politically connected firm is still more productive than the bottom 40% of non-connected firms. However, despite connected firms being selected, estimated subsidies are sizable. For the average connected firm, the government subsidizes at least 40% of output or, equivalently, pays a price markup of at least 65%.

With the estimated subsidies in hand, I show that a structural model of endogenous rent-seeking can almost perfectly explain them quantitatively. The structural model is needed to infer unobserved rent-seeking activities, which affect input prices in general equilibrium and hence the aggregate costs of political connections. In the model, rent-seeking activities of a firm are organized within a department that is in charge of lobbying, tax evasion, legal affairs and bribery. Connected firms then endogenously choose the size of this rent-seeking department. The estimated model shows decreasing returns to rent-seeking activity and convex costs that increase both in rent-seeking activity and firm size. Model-implied subsidies explain more than 95% of the variation in estimated subsidies for both bounds. The economic intuition is that firms optimally trade-off investing in rent-seeking activities that they use to buy subsidies with trying to stay below the radar of opposing interest groups and public scrutiny. In the data, connected firms with intermediate levels of productivity receive the largest subsidies. Through the lens of the model, these firms are at a sweet spot where they are productive enough at rent-seeking while being small enough to receive little public scrutiny. Based on the estimates, connected firms differ widely in how much they spend on rent-seeking activities. The largest connected firms spend less than 2% of their input costs on rent-seeking, while smaller and less productive firms benefit from receiving less attention and spend up to 30% of their input costs on rent-seeking. As a validation of these estimates of unobserved rent-seeking activities, I show that they align with recent quantitative evidence on high-level rent-seeking activities, as evidenced in the Odebrecht case (see Campos et al. 2021).

The last step to quantify the aggregate costs of political connections is to consider firms' decisions in a simple

[3]There is good evidence that connected firms are negatively selected within subsets of large firms, such as listed firms or firms eligible for government contracts (e.g. Gonzalez and Prem 2019; Schoenherr 2019; Szucs 2017). My results are in line with this since, conditional on firm size, politically connected firms are less productive.

general equilibrium model with competitive capital and labor markets. Even though connected firms are more productive than the average firm, and there is some rationale to subsidize them, estimated subsidies are far too large and dispersed to be efficient. According to the baseline estimates, the total output costs of political connections are 1.0-4.7%. At most, 45% of this increase in output is driven by capital and labor being *misallocated*. Connected firms end up much larger than is socially optimal, crowding out resources from other, non-connected firms in the economy. Almost all misallocation of resources happens across and not within firms because rent-seeking activities are concentrated in a few connected firms. The large remainder of the costs of political connections is driven by *opportunity costs of public funds* as subsidies would be more efficiently spent on reducing distortionary taxes for all firms in the economy. I find even narrower welfare bounds and higher output costs ranging from 4-6.5% when considering the detrimental effects of political connections on the provision of public goods and increasing market power. These results are robust to further heterogeneity in industry- and connections-type and different forms of measurement error.

Given that a large part of the costs come from inefficiently high subsidies to connected firms that do not receive enough public scrutiny, I find high returns to increase monitoring of rent-seeking and corruption in the economy: lower bounds suggest policies in the range of doubling existing monitoring activities or spending an additional 0.1% of GDP on them. Such extra spending would amount to a ten-fold increase in Transparency International's global annual budget.[4] In summary, a few connected firms can pose high societal costs, and curbing their influence can have large returns.

Literature

The key contribution of this paper is to provide quantitative estimates of the costs of political connections in general equilibrium. A growing micro-empirical literature has documented how favors to connected firms drain government resources[5] and lead to large allocative inefficiencies.[6] However, quantifying the aggregate costs of political connections has remained an elusive quest.[7] Notable recent exceptions are Akcigit,

[4]This is at Indonesia's 1997 GDP. See: https://www.transparency.org/en/the-organisation/our-operating-budget. Accessed on May 12th, 2022.

[5]For example, Chen and Kung (2018) find that connected firms in China pay between 55-60% less for state-owned land.

[6]Haselmann, Schoenherr, and Vig (2018) show extensive misallocation of bank credit between connected firms and banks in Germany and Schoenherr (2019) finds that politically connected firms in Korea win a larger number of government contracts and that they execute these contracts systematically worse and at higher costs than non-connected firms. Schoenherr (2019) estimates that three quarters of the costs of contract misallocation are due to selecting the wrong firms to give contracts to. Similarly, Brugués, Brugués, and Giambra (2018) find that connected firms are more likely to win discretionary government procurement contracts in Ecuador and that these firms charge higher prices and are less efficient. Szucs (2017) shows that connected firms in Hungary sort into government procurement contracts that are allocated with higher bureaucratic discretion and finds evidence that these connected firms are of lower productivity. In contrast, Bertrand et al. (2018) does not find evidence that connected firms receive higher benefits from the state in France.

[7]Few papers looked at welfare, e.g. Faccio (2006); Fisman (2001); Gonzalez and Prem (2019); Martinez-Bravo, Mukherjee, and Stegmann (2017); Straub (2014); Gonzalez, Prem, and Urzúa (2018); Chen and Kung (2018); Fisman and Wang (2015); Haselmann, Schoenherr, and Vig (2018); Schoenherr (2019).

Baslandze, and Lotti (2018), Bai, Hsieh, and Song (2020), Brugués, Brugués, and Giambra (2018), Garcia-Santana et al. (2020), Huneeus and Kim (2021), Szucs (2017) and work-in-progress by Koren et al. (2015).

While Akcigit, Baslandze, and Lotti (2018) show evidence for dynamic losses from political connections through a lack of innovation, they do not provide quantitative estimates of the costs of connections. On the other hand, I abstract from such dynamic losses, so my estimates are rather lower bounds on the costs of political connections. Garcia-Santana et al. (2020) consider costs of political connections in general equilibrium but do not have firm-level evidence of political connections, forcing them to draw on sectoral estimates of corruption. The firm-level data allows estimating firm-level subsidies directly, leading to the rejection of the rent-seeking technology in their paper and Akcigit, Baslandze, and Lotti (2018).[8] Bai, Hsieh, and Song (2020) show how bureaucrats in China favor firms to help them avoid bad institutions and growth distorting regulation. While they do not provide quantitative estimates on the costs and benefits of political connections, I also allow for connections to be beneficial but find that costs greatly outweigh benefits on aggregate. The concurrent work by Huneeus and Kim (2021) might be the closest paper to mine. They look at the aggregate costs of lobbying in the US, which they infer from firm-level lobbying expenditures and size distortions. Besides the different focus on corruption and rent-seeking in a development context, the main difference between the papers is methodological: the novelty of Huneeus and Kim (2021) is that they observe lobbying activity. In contrast, this paper shows how to estimate benefits from connections non-parametrically and then infer rent-seeking activities without observing them. Since rent-seeking is rarely observed, my methodological contribution is likely helpful in many other applications. The other papers do not consider general equilibrium effects.

The paper also strongly relates to the misallocation literature, which can be separated into direct and indirect approaches (e.g. Restuccia and Rogerson 2017). An interesting feature of this paper is that it combines the direct and indirect approaches by flexibly capturing various different distortions when estimating subsidies with minimal structural assumptions and only later linking them to a full structural model. The paper also contributes separately to both strands of this literature. The paper also contributes separately to both strands of this literature. On the direct side, it adds to the literature by focussing on political connections as one particular friction.[9] Among papers that follow a direct approach, Guner, Ventura, and Xu (2008) is similar in that it also considers a distortion that firms internalize and that directly affects the measured productivity distribution.

[8]The rent-seeking technology is similar to the idea of a "concealment technology" (Cremer and Gahvari 1994) or evasion technology (e.g. Slemrod and Yitzhaki 2002) used in the tax evasion literature. It is closer to the idea of tax avoidance (see Slemrod and Yitzhaki 2002; Slemrod 2001) in that I model political connections without risk, firms know how much taxes they have to pay this period and are only uncertain about future tax payments as political connections may change. This seems to be more in line with how connections work in developing countries (e.g. see Hoang 2018; Chen and Kung 2018).

[9]For other papers following this direct approach, see the literature cited in Restuccia and Rogerson (2017).

On the indirect side, this paper is like Restuccia and Rogerson (2008) with endogenous subsidies identified using microdata. Since the seminal contributions of Restuccia and Rogerson (2008) and Hsieh and Klenow (2009), most quantitative empirical work has followed Hsieh and Klenow (2009) in inferring general distortions from wedges in first-order conditions that lead to observed variation in factor shares (this also includes Garcia-Santana et al. (2020) and Huneeus and Kim (2021)). By assumption, this captures distortions that only indirectly affect output and inputs via sub-optimal decisions. This paper takes the opposite and neglected approach of identifying "direct" distortions from differences in TFP distributions across connected and non-connected firms.[10] Given that subsidies are identified from fundamentally different variation in the data, I show in a key extension in Section 5 how one can combine subsidies and wedges as distortions. A further benefit of the estimation approach, in this case, is that there are matched non-connected firms from which one can directly infer counterfactual wedges. I show how these counterfactual wedges can quantify additional costs of political connections that stem from market power. Unfortunately, given the different identification, the approach cannot distinguish between an output subsidy and input subsidies, and I capture all subsidies under a single output subsidy throughout.

The structure of the paper is as follows: The next section discusses the measure of political connections in the Indonesian data and how connected firms differ from non-connected firms. In section 3, I present and structurally estimate a model that can explain size differences between connected and non-connected firms and endogenizes subsidies. Section 4 quantifies the economy-wide costs of political connections. Key extensions of the baseline model and various robustness results are in section 5, while the last section concludes.

2 Political Connections in Indonesia

The starting point is a good measure of political connections for which I draw entirely on Mobarak and Purbasari (2006). I introduce their measure and the firm data in the first subsection. The second subsection briefly highlights key empirical regularities that will inform subsequent modelling choices.

[10] TFP in this paper is TFPQ in the setup by Hsieh and Klenow (2009), and they treat this as a fundamental, while I show that this includes a combination of subsidies and fundamental *productivity*. Hsieh and Klenow (2009) and "wedge approaches" in their spirit assume that one observes undistorted (pre-wedge) output, while I instead assume that we can only observe distorted (post-subsidy) output.

2.1 Identifying connected firms in Indonesia

Indonesia under the rule of dictator Suharto at the end of the 1990s was characterized by a vast patronage network that extended from the capital city of Jakarta down to the village level (Fisman 2001; Martinez-Bravo, Mukherjee, and Stegmann 2017). By allocating public contracts, concessions, credit, and extra-budgetary revenues, a network of elites closely connected to the state administration was able to amass large amounts of wealth (see Hadiz and Robison 2013; Robison and Hadiz 2004). There is also strong evidence that these elites held onto power after the fall of the Suharto regime in the aftermath of the Asian Financial Crisis in 1997 (see Robison and Hadiz 2004; Martinez-Bravo, Mukherjee, and Stegmann 2017). Today, based on comparative statistics such as Transparency International's Corruption Index, Indonesia is similarly corrupt as countries such as Russia, Vietnam, Mexico and Brazil. Corruption in Indonesia has also received important scholarly attention in its own right (e.g. Olken 2007).

At the same time, Indonesia is exceptional for providing several rich data sources that have allowed scholars to identify politically connected firms and link these to detailed annual firm-level panel data. Specifically, this paper draws on the Annual Manufacturing Survey (Survei Tahunan Perusahaam Industri Pengolahan) collected by Indonesia's Central Bureau of Statistics (Badan Pusat Statistik), which covers all formal manufacturing establishments with more than 20 employees. Based on the GGDC 10-sector database, these account for about 30% of all value-added manufacturing output in Indonesia (see: Fentanes & Gathen 2022). The survey contains detailed industry information (up to 5-digit), employment, production, and other firm characteristics and has been used extensively in the Economics literature (e.g. Amiti and Konings 2007). I combine this with the measure of political connections from Mobarak and Purbasari (2006), who identified politically connected firms and already linked these to firms in the survey.

Mobarak and Purbasari (2006) identify connected firms in two different ways. In this paper, I use the union of the two sets of firms as my main measure of whether a firm is politically connected. The first set of firms is identified by tracing firms that were directly owned and founded by blood relatives of Suharto. This set excludes firms whose owners might have strategically married into the Suharto family. For the second and more comprehensive set of firms, Mobarak and Purbasari (2006) draw on the natural experiment in Fisman (2001). Fisman (2001) uses news about plausibly exogenous health issues of dictator Suharto in various periods between 1995-1996 and looks at responses to firms' stock prices on the Jakarta Stock Exchange around these events. The idea is that news about the deteriorating health conditions of the dictator should negatively affect the stock price of firms that benefit from being politically connected to the dictator. The added benefit of the Indonesian context is that the Indonesian regime was highly centralized around the dictator, so shocks to the dictator's health should affect any listed connected firm.

To systematically identify connected firms, Mobarak and Purbasari (2006) first run firm-specific time-series regressions using daily stock price data for the universe of firms traded on the Jakarta Stock Exchange (JSX) for the 985 market trading days between 1994 and 1997. A listed firm is defined as "politically connected" if its stock price dropped significantly in periods in which news about Suharto's hospitalizations emerged. They choose a threshold at the 95% confidence level of the indicator variable for Suharto's health shocks, controlling for aggregate movements in the JSX, the average return for the industry category in which that firm belongs, and movements in the exchange and interest rate. After validating their measure of politically connected listed firms, they trace all other firms that share ownership and management with these connected listed firms through conglomerate structures. As Claessens, Djankov, and Lang (2000) and Carney and Child (2013) show, most firms belong to larger conglomerate structures owned by specific families and ownership and control is rarely separated in Southeast Asian firms, including Indonesia. This concentration in ownership allows them to link stock-listed firms to a larger network of other firms of the same conglomerates owned by the same group of people. This linkage identifies more than 2,000 firms as being connected. Taking the union of both measures of politically connected firms together, only about 20% or roughly 400 of these firms are manufacturing firms. To link these firms to the deidentified establishment-level manufacturing panel data, Mobarak and Purbasari (2006) matches firms by province location, industry code and the approximate number of employees. This procedure allows them to match about 60% of firms, giving a total of 241 connected firms, which is the final measure used throughout this paper. Of the 241 firms, 89 firms are identified as being owned and founded by blood connections of Suharto; the remainder is identified via the stock market approach.[11]

I provide more detailed information on each of the steps in Appendix A.1 and discuss the role of measurement error in Section 5. However, it is important to highlight three key features of this data. First, this definition of political connections captures "high-level" political connections and does not capture more local connections of firms to local authorities in the bureaucracy or police. The reason is that the approach only captures firms linked to conglomerate structures that either belong to Suharto's blood family or include at least one listed firm that is identified via the natural experiment. Second, the measure of political connections is different from state-owned enterprises, but there is some overlap. About 15% of connected firms in the data can be classified as at least partly state-owned, while the remaining 85% of connected firms see no state ownership. I further discuss the role of state ownership in Section 5. At last, the approach identifies a snapshot of the connected manufacturing firms in 1994-1997, shortly before the Asian Financial crisis in 1997/8.

[11] 34 of the 89 blood connected firms are similarly identified as being connected by the stock market identification approach.

2.2 Differences between connected and non-connected firms

Figure 1 shows the firm-size differences in value-added output between connected and non-connected firms for the cross-section of Indonesian manufacturing firms in 1997. The average connected firm is about twelve times larger than the average non-connected firm, but there is also considerable overlap in output across the two distributions. In fact, there exist non-connected firms that are smaller than the smallest connected firm and non-connected firms that are larger than the largest connected firm. The size distribution of non-connected firms is visibly more skewed and more dispersed. One way to see the dispersion is that the coefficient of variation is more than four times larger for the size distribution of non-connected than for connected firms (13 vs 3).

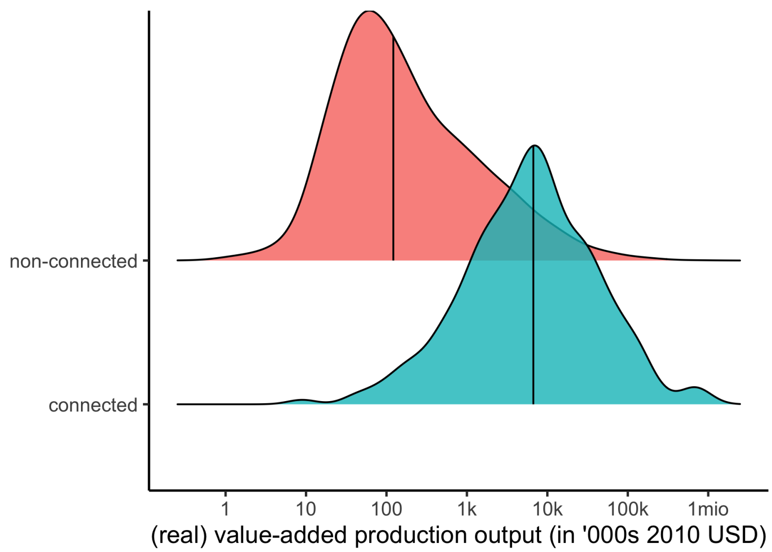

Figure 1: Distributions of firm-specific real value-added output (in 2010 USD in 000's) for cross-section of Indonesian firms in 1997 based on Statistik Industri, the Indonesian manufacturing firm census. Connected vs. non-connected firms are identified as in Mobarak & Purbasari (2006). Non-connected firms: N = 18,317. Connected firms: N = 241.

Table 1 documents the average size differences between connected and non-connected firms within industries. I separately compute the average output of all connected and non-connected firms and compute their ratios by industry. I then take these size ratios and average over them to derive an economy-wide size ratio, using as weights the number of connected firms in each industry. Column 1 reports the average size ratio without industry heterogeneity, and Columns 2-5 report ratios looking respectively within 2-, 3-, 4- and 5-digit industries. I find that connected firms are not only considerably larger on average than non-connected firms, but these size differences are just as large or even larger within industries. Even within narrowly defined industries, the average connected firm is more than 12 times and up to 20 times larger than the average non-connected firm. Outliers do not drive this pattern. As for the distribution of connected firms across

Table 1: Within-industry size ratios of average connected over average non-connected firms

	unconditional	Within industry 2-digit	3-digit	4-digit	5-digit
Ratio	11.96	14.19	13.51	17.74	19.93
# industries	1	9	31	115	302
# industries w/ connected firm	1	9	26	62	103

Details: Data is real value-added output data for the cross-section of Indonesian manufacturing firms in 1997 based on Statistik Industri. Size ratios are computed based on the ratio of the average size for connected vs. the average size of non-connected firms within each considered industry and then averaged across industries using the number of connected firms in each industry as weight. Non-connected firms: N = 18,317. Connected firms: N = 241.

industries, connected firms are widely dispersed. Only about 1.3% of all firms are connected, but connected firms still show up in all nine 2-digit industries, 26 out of 31 3-digit industries and about one-third of all roughly 300 5-digit industries. This dispersion across industries suggests that size differences are not driven by selection into specific industries.[12]

3 Quantifying the role of connections: A structural approach

This section develops a simple model of heterogeneous firms similar to Restuccia and Rogerson (2008) without entry and exit, where firms make static choices of production inputs. Size differences between connected and non-connected firms in the model are driven by fundamental differences in idiosyncratic productivity and differences in benefits from political connections. Benefits from political connections are modelled as idiosyncratic output subsidies, which in contrast to Restuccia and Rogerson (2008), are endogenized via strategic spending on rent-seeking activities similar to Garcia-Santana et al. (2020) and Akcigit, Baslandze, and Lotti (2018). Based on this model, I show how to identify and estimate benefits from political connections flexibly. The last part shows how these estimates align with a rich model of endogenous rent-seeking behavior, which is subsequently used for partial and general equilibrium counterfactuals.

[12]Further empirical results and robustness exercises are reported in Appendix A.2.

3.1 Modeling political connections

Household & government

The household side of the model is kept as simple as possible, featuring a representative household maximizing life-time discounted utility:

$$\sum_{t=0}^{\infty} \beta^t U(C_t)$$

subject to a CRRA intertemporal utility function $U(C_t)$ and a per period budget constraint:

$$A_{t+1} + C_t = (1 + r_t - \delta)A_t + w_t L_t + \Pi_t + T_t$$

where households face no risk, provide labor supply L_t inelastically at potentially time-varying wage w_t, rent capital to firms at the potentially time-varying interest rate r_t, face depreciation of capital at rate δ and demand consumption goods fully elastically. Households further receive net profits by firms and net revenue T_t from the government. The household's optimal consumption-savings choice is then characterized by the standard Euler Equation:

$$1 = \beta(1 + r_{t+1} - \delta)\frac{U'(C_{t+1})}{U'(C_t)}$$

In the baseline model, the government levies taxes, subsidizes connected firms and balances its budget each period by paying any net revenues T_t lump sum to households.

Firms

The economy is populated by a fixed and discrete number of heterogeneous firms indexed by i whose after-tax value-added revenues R_i are given by:

$$R_i = (1 - \tau_i) z_i k_i^{\alpha} l_i^{\beta} \quad \text{with} \quad \alpha + \beta \leq 1 \tag{1}$$

z_i captures firm-specific productivity, k_i and l_i denote the firm's input choices of capital and labor and α and β give the revenue elasticities of capital and labor respectively.[13] Crucially, as in Restuccia and Rogerson (2008), firms face idiosyncratic taxes τ_i that are directly paid to the government. Specifically, all non-connected firms face a constant *de jure* revenue tax $\tau_i = \bar{\tau}$ that is set to 25% to mimic Indonesia's

[13] One can think of this setup as heterogeneous firms producing a single output or as I show in Appendix A.3, isomorphically as an economy with CES demand and differentiated inputs. In the latter case, z_i flexibly captures both productivity and demand. I do not separately identify the role of demand vs productivity. As is standard in models of firm-size dynamics, both factors influence firm dynamics in the same way. For ease of exposition, I simply refer to them as productivity in the following.

flat corporate income tax rate.[14] Political connections in the model solely enter by distorting these baseline taxes. As the key object of interest, define the differential subsidy $(1+\tilde{\tau}_i) \equiv \frac{1-\tau_i}{1-\bar{\tau}}$, so that connected firms for whom $\tau_i < \bar{\tau}$ are subsidized and non-connected firms face $\tilde{\tau}_i = 0$. The differential subsidy $\tilde{\tau}_i$ captures in a reduced-form way many of the channels mentioned in the introduction through which connected firms benefit from interactions with the government. The direct identification and estimation of $\tilde{\tau}_i$ in this paper allow for any of these channels. However, to be consistent with the subsequent quantification of the costs of political connections, I interpret distortions throughout as either direct tax distortions (tax cuts or evasion) or as direct government demand. In this setup, taxes are like firm-specific output prices, such that firms earn revenue $(1-\bar{\tau})p_i y_i$ and $p_i = (1+\tilde{\tau}_i)$ so that subsidized firms benefit from higher output prices that the government pays for - a public mark-up.

To allow the firm size of connected firms to flexibly depend on their benefits from political connections, I endogenize the differential subsidy $\tilde{\tau}_i()$. To fix ideas and in line with the little existing systematic evidence on high-level rent-seeking (Campos et al. 2021), think of rent-seeking activities of a firm as being organized within a department in charge of lobbying, tax evasion, legal affairs and bribery. The endogenous subsidy can then be thought of as the output of the rent-seeking department. To make this clear, call $\tilde{\tau}$ the *Political Connections Technology*, which depends on four inputs: $\tilde{\tau}(l_P, k_P, \varepsilon_i, z_i)$. First, connected firms endogenously choose the size of their rent-seeking department by choosing the amount of capital, k_P, and labor, l_P, engaged in rent-seeking activities. Labor employed in rent-seeking activities captures lawyers who renegotiate contracts and find tax loops, lobbyists who push for favorable legislation and preferential contracts, management and other workers who are involved in managing rent-seeking projects, meeting and cultivating political contacts and labor used by third parties who specialize in facilitating rent-seeking and corruption (Hoang 2018). Rent-seeking capital captures direct bribes as well as any other capital that is used for rent-seeking activities. These endogenous rent-seeking activities are similar in spirit but more general than spending on political connections as considered in Garcia-Santana et al. (2020) or the fixed cost of political connections as in Akcigit, Baslandze, and Lotti (2018).[15] Furthermore, the output of rent-seeking activities can directly depend on firm productivity z_i to capture the idea that more productive firms are also more productive at rent-seeking or that the government may interact differently with more productive firms.

At last, whether the firm takes part in rent-seeking activities depends on an exogenous binary variable ε_i

[14] There are reduced tax rates for small enterprises as well as public enterprises. The former do not play a role in my model and do not show up in the Manufacturing census data. I ignore the latter or implicitly capture them if they are counted as connected firms.

[15] Specifically, the fixed cost in Akcigit, Baslandze, and Lotti (2018) simply captures the remuneration of one politician and is thus nested by rent-seeking activities in labor l_P in the model above. Spending on political connections in Garcia-Santana et al. (2020) is not strictly nested since it is modelled in units of the final consumption good and as if it were fully consumed in production. This captures direct in-kind bribes that are only indirectly captured by rent-seeking activities in capital k_P in my model. Still, having both rent-seeking labor and capital in the model seems more plausible and general.

that captures access to political connections. With $\varepsilon = 0$, individual firms are not currently matched to a politician in power, do not have the ear of the political elite or have a distaste for political connections, so subsidies are zero ($\tilde{\tau}_i = 0$). Similar to Akcigit, Baslandze, and Lotti (2018), this captures the idea that political connections depend partly on luck, evidenced by the fact that despite political connections being profitable, most firms - including some of the largest Indonesian firms - are not connected.[16]

Given the *Political Connections Technology* $\tilde{\tau}(l_P, k_P, \varepsilon_i, z_i)$, how do connected and non-connected firms choose inputs? All firm choices are static.[17] Firms buy workers on the spot market at a common wage w and rent assets at a rental rate r from households. Both assets and labor can be used instantaneously either as productive inputs or for rent-seeking activities. ε_i may vary over time (following a first-order Markov process). However, the realization of ε_i is known to the firm at the beginning of a period before it makes any other production decisions. A firm is thus fully characterized by productivity z_i as well as the realization of ε_i and solves the following static problem each period:

$$\max_{k,l,l_P,k_P} \left\{ \pi(z_{it}, \varepsilon_{it}) \equiv \big(1 + \tilde{\tau}(l_P, k_P, \varepsilon_{it}, z_{it})\big)(1-\bar{\tau}) z_{it} k^{\alpha} l^{\beta} - w_t(l + l_P) - r_t(k + k_P) \right\} \tag{2}$$

This problem gives simple and intuitive static FOCs that say that firms should equalize the marginal benefits and the marginal costs (as captured by the rental prices of capital and labor) for both production and rent-seeking activities:

$$\begin{aligned} r_t &= \alpha \frac{R_{it}(k^*, l^*, l_P^*, k_P^*)}{k^*} = \frac{\partial \tilde{\tau}(l_P^*, k_P^*, \varepsilon_{it}, z_{it})}{\partial k_P}(1-\bar{\tau}) z_{it} k^{*\alpha} l^{*\beta} \\ w_t &= \beta \frac{R_{it}(k^*, l^*, l_P^*, k_P^*)}{l^*} = \frac{\partial \tilde{\tau}(l_P^*, k_P^*, \varepsilon_{it}, z_{it})}{\partial l_P}(1-\bar{\tau}) z_{it} k^{*\alpha} l^{*\beta} \end{aligned} \tag{3}$$

Based on a revealed-preference argument, firms show up as non-connected if they optimally choose rent-seeking labor and capital such that $\tilde{\tau} = 0$.

Equilibrium

The aggregate resource constraint is given by: $Y_t = \sum_i z_{it} k_{it}^{\alpha} l_{it}^{\beta} = C_t + I_t$. The focus in this paper is on a *steady state competitive equilibrium* that is described by competitive prices (r^*, w^*) that households and firms take as given and competitive allocations such that:

[16] Later on, I show how to generalize ε to allow for a finite number of different types of connections. Types could then be industries or different groups of connected firms, such as connected firms that are blood-connected to the dictator Suharto versus connected firms who do not have this special link, nesting corruption-specific productivity as in Garcia-Santana et al. (2020).

[17] In the conclusion, I briefly discuss how the identification approach may also work in a dynamic setup.

- the exogenously given set of firms all produce by optimally choosing capital, labor and rent-seeking activities based on their realizations of $(z_{it}, \varepsilon_{it})$
- the household optimally chooses consumption and savings based on the Euler Equation given above and consumption and savings are constant over time
- the government levies taxes and subsidizes connected firms and balances its budget each period by transferring net revenue lump-sum to the household
- prices adjust such that capital demand and supply and labor demand and supply equalize each period and these aggregates are constant over time
- the distribution of firms over $(z_{it}, \varepsilon_{it})$ is at its stationary distribution

3.2 Identification of political connections

Taking a step back, it is important to highlight that the welfare implications of subsidies to connected firms are ex-ante unclear in this setup. Given the baseline distortion of revenue taxes that all firms face, there is a welfare argument for subsidizing connected firms. As I show formally in Appendix A.4, in a setup with heterogeneous firms, decreasing returns to scale in production and distortive output taxes, it is optimal to subsidize firms at constant rates (up to small general equilibrium corrections) and distribute subsidies as widely as possible. In the end, whether subsidies to connected firms are harmful in comparison to no subsidies to connected firms depends on at least three key margins: (i) How many firms become connected, (ii) the distribution of subsidies as governed by the shape of $\tilde{\tau}(.)$ and (iii) the extent of socially wasteful spending on rent-seeking activities that directly depends on the selection of connected firms as governed by ε.

To estimate the costs of political connections, it is important to capture all three margins flexibly. The first margin is directly pinned down by observing the number of connected firms in the data. In contrast, the other two margins must allow for considerable flexibility in how connected firms are selected and how rent-seeking activities by connected firms translate into firm-specific subsidies. The approach in this paper, as formally stated in Proposition 3.1, is to impose functional form restrictions on the selection of connected firms, pose a weak assumption on the shape of $\tilde{\tau}(.)$ and then back out $\tilde{\tau}(.)$ non-parametrically:

Proposition 3.1 (Main identification result)**.** *Given the previous setup and conditional on having identified total factor productivity (TFP) defined as $TFP_i \equiv (1 - \tau_i)z_i$ one can separately identify τ_i and z_i based on the following two assumptions:*

1. *(**Selection**). Firms with access to political connections have been drawn from a known population of*

productivities z_i according to:

$$\mathbb{P}(\varepsilon \neq 0) = \begin{cases} cz_i^{\rho}, & \text{if } z_i \geq \bar{z} \\ 0, & \text{otherwise} \end{cases}$$

where c is a normalizing constant to ensure well-defined sampling.

2. *(**Monotonicity of TFP**) The connections technology $\tilde{\tau}(.)$ is such that there is a monotonic mapping between TFP_i and productivity z_i for firms with access to political connections. Formally, $\frac{\partial TFP(z,\tilde{\tau})}{\partial z} = (1+\tilde{\tau})(1-\bar{\tau}) + \frac{\partial \tilde{\tau}}{\partial z}(1-\bar{\tau})z > 0$ for $\bar{\tau} \in (0,1)$ given and all $\tilde{\tau} \in supp(\tilde{\tau})$.*

The proof of this proposition is straightforward. The assumption on selection guarantees that we can link an identified distribution of productivities to an observed distribution of TFP for connected firms. In contrast, the monotonicity assumption allows linking moments of this identified productivity distribution to moments of the observed distribution of TFP of connected firms. We can then identify the entire subsidy distribution using $\tau(q) = TFP(q)/z(q) - 1$ for any quantile q.

There are several components in the proposition that are important to unpack. Beginning with the monotonicity assumption on TFP, this assumption states that the ranking of connected firms by TFP is identical to the underlying ranking of their productivities. This restriction on the underlying *Political Connections Technology* $\tilde{\tau}(q)$, namely that $(1+\tilde{\tau}) + \frac{\partial \tilde{\tau}}{\partial z} z > 0$, is naturally given for functions that are strictly increasing in rent-seeking activities, but exhibit any form of decreasing, constant or increasing returns to scale. More generally, it also allows $\tilde{\tau}(q)$ to be decreasing, only that any decline in $\tilde{\tau}(q)$ is not faster than the corresponding increase in productivity. For example, this allows a *Political Connections Technology* where benefits from political connections decline with firm size as this puts the firm into the public eye, making any corrupt practices more difficult or where the *Political Connections Technology* is understood as a reduced-form tax evasion technology where the probability of getting caught increases with firm size. Importantly, the monotonicity assumption nests decreasing returns to scale functions that have been considered in the literature (Akcigit, Baslandze, and Lotti 2018; Garcia-Santana et al. 2020) and thus allows for testing their functional form assumptions formally.

The selection assumption is more restrictive and can be divided into two parts. First, identification requires observing a known population of productivities from which firms with access to political connections are drawn. The setup in this paper makes this particularly suitable as subsidies to connected firms are considered differential subsidies compared to non-connected firms. Hence, the TFP of non-connected firms gives their underlying productivities up to a known baseline revenue tax $\bar{\tau}$. Furthermore, we need to assume that the productivities of non-connected firms can be treated as the underlying population from which access to political connections is drawn. Again, the setup in this paper is such that the sample of non-connected firms is roughly 100 times larger than the sample of connected firms, making this population assumption a natural choice. The last component for the first part of the selection assumption is that the support of underlying productivities for connected firms is entirely contained in the support of productivities of non-connected firms. This common support assumption is similar to standard matching estimators and, as I show in the estimation part below, can be readily verified in the data.

The second part of the selection assumption puts structure on the selection rule with which firms receive access to political connections. The idea for identification is that given a selection rule with which access to connections is drawn, one can selectively sample the productivities of connected firms using the population of productivities of non-connected firms. An estimator can then simply be an average across many independently but selectively drawn productivities. The functional form restriction is required to identify the selective sampling process. To see this, note that without this assumption, one could draw arbitrary samples of non-connected firms rationalizing any productivity distribution of connected firms within the productivity support of non-connected firms. The functional form assumption allows for considerable selection. One way to see this is to think of it as a setup in which connections are formed at selective meetings where a minimal firm size is needed to access the meetings and larger, more productive firms are more likely to meet or be approached by politicians at that meeting. The pre-selection might be done directly by politicians or may capture fixed or high variable costs to rent-seeking activity that are not worthwhile for firms below a certain productivity/size threshold. Identification results for the parameters of the selection rule are stated in Proposition 3.2:

Proposition 3.2 (Identification of the selection of connected firms and conservative subsidy bounds)**.** *Given the assumptions in Proposition 1, the parameters of the selection rule that govern access to political connections are set-identified under the following additional restrictions:*

1. *(**conservative normalization**): The subsidy of the connected firm with the lowest possible observable TFP is zero ($\tilde{\tau}_{q=0} = 0$ for q giving the quantile of the underlying productivity distribution of connected firms).*

2. *(**rational rent-seeking**): Connected firms will never choose $\tilde{\tau} < 0$.*

3. *(**independence as minimum selection**): There is a lower bound for selection that is given by $\rho = 0$ and $\bar{z} = 0$, the case of independence between ε and z.*

Specifically, $\bar{z}$ is point-identified under ***conservative normalization*** *and independent of ρ. And ρ is set-identified with $\rho \in \{0, \bar{\rho}\}$, where $\bar{\rho}$ is identified from the maximum ρ for which* ***rational rent-seeking*** *still holds and another firm's $\tilde{\tau}_{q>0} = 0$. The model is rejected in case $\bar{\rho} < 0$. We can call identified subsidy distributions $\tilde{\tau}(q)$ based on the sets of parameters $\Big\{\{\bar{z}, 0\}, \{\bar{z}, \bar{\rho}\}\Big\}$ conservative bounds for actual subsidy distributions.*

This proposition makes clear how the parameters $\bar{z}$ and ρ allow for considerable selection of connected firms. $\bar{z}$ truncates the productivity distribution of non-connected firms from which access to political connections is drawn and thus shifts the entire productivity distribution of connected firms. ρ allows for additional correlation between access to political connections and underlying productivity within this truncated productivity distribution. The distribution of subsidies is then identified from residual dispersion in TFP that is not explained by selective sampling-implied variation in underlying productivities.

3.3 Estimation of political connections

Estimation of subsidies to connected firms proceeds in two steps. The first step estimates TFP for all firms in the economy. In the second step, I use a bootstrap-based matching estimator for subsidies that builds on Propositions 1 and 2. To estimate TFP, I follow a strictly model-consistent approach. In principle, any estimator for TFP as defined previously can be used in the first step. However, model consistency ensures that partial and general equilibrium counterfactuals are sensical and gives a cleaner identification and estimation of subsidies. In Section 5, I consider how results are affected by alternative TFP estimation that allows for further production function heterogeneity across industries and wedges.

The model-consistent TFP estimator consists of first estimating revenue function elasticities α and β exploiting static first-order conditions of firms. These first-order conditions state that revenue spending shares on productive labor and capital equal their respective revenue elasticities. By assumption, revenue elasticities are identical across connected and non-connected firms, so it suffices to use observed revenue spending shares for non-connected firms. This has the benefit of not having to take a stand on whether reported input spending by connected firms is misreported or partly includes spending on non-productive, rent-seeking activities. Static first-order conditions of non-connected firms imply that observed revenue factor spending shares should be constant across firms. In the data, as shown among others in Hsieh and Klenow (2009), there is strong variation in revenue factor shares even within narrowly defined industries. In the baseline results, I treat the observed variation in factor shares as stemming solely from measurement error in reported labor and capital spending centred around 0. Specifically, I estimate α and β using median factor revenue shares across all non-connected firms. Given estimates for α and β, I use observed firm revenue R_{it} and the model-implied spendings on productive capital k_{it}^* and labor l_{it}^* to identify:

$$TFP_i = (1-\tau_i)z_i = \frac{R_{it}(k_{it}^*, l_{it}^*, l_P^*, k_P^*)}{k_{it}^{*\alpha_s} l_{it}^{*\beta_s}} \tag{4}$$

Using model-implied spendings on productive capital and labor is crucial here. It cleans the data from measurement error in labor and capital spendings, which shuts down all the variation used in Hsieh and Klenow (2009) and abstracts from any variation in factor shares due to dynamic input choices (e.g. Asker, Collard-Wexler, and De Loecker 2014). It ensures that - conditional on α and β - all variation in TFP is estimated from variation in observed revenue. The implicit assumption here is that reported revenue is reported without measurement error. In Section 5, I consider both measurement error in reported revenue and generalize the approach to allow for both wedges and subsidies at the same time.

Figure 2 shows the estimated TFP distributions of connected and non-connected firms for the cross-section of Indonesian manufacturing firms in 1997. The data shows that the average connected firm has slightly less than 3.5 times higher TFP than the average non-connected firm, and there is a large overlap in the two distributions. These size differences are considerably smaller than the value-added output differences reported in Figure 1. Based on the baseline model, TFP for non-connected firms is exactly equal to their productivity z_i, so Figure 2 captures the entire productivity distribution of non-connected firms.

Given TFP, the second step of the estimation approach constructs a matching estimator that matches each connected firm with a comparable non-connected firm for which: $TFP_{i,NC} = \tilde{z}_{i,NC}$. For a given selection rule, Propositions 1 and 2 can be used to draw a set of connected firms from the population of non-connected firms and match them according to their ordering of productivities and TFP. In the case of sampling independent bootstrap samples, the approach matches the nth highest productivity firm in the bootstrap sample to the nth highest TFP connected firm. The productivity estimate for each connected firm is the average over all matched productivities for this specific connected firm. This non-standard, distributional matching estimator is necessary as standard matching based on observables does not work here. Standard matching approaches would require matching on observables that explain productivity but are not directly affected by political connections. It is unclear which observables fulfil this condition, and given that other determinants of connections are unobservable, conditional matching is also impossible. Treating non-connected firms as the underlying population and selectively sampling from their productivities works well in this context as there are many more non-connected to connected firms in the data. To verify the common support assumption for productivities, we can first note that common support is fulfilled for

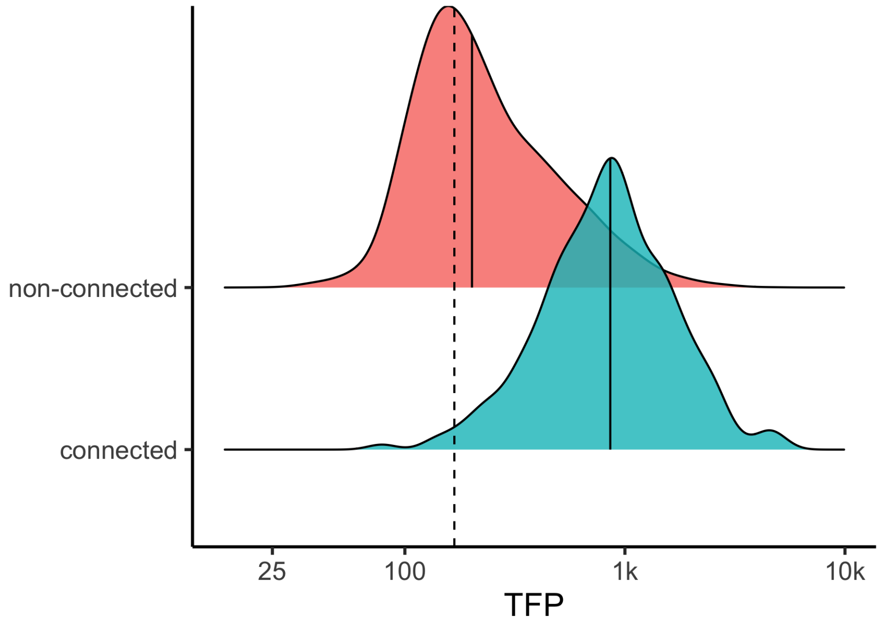

Figure 2: Distributions of firm-specific TFP for cross-section of Indonesian firms in 1997 based on Statistik Industri, the Indonesian manufacturing firm census. Connected vs. non-connected firms are identified as in Mobarak & Purbasari (2006). TFP is identified as residual from production function estimation at the 1-digit industry level (single production function across all firms) as explained in the text. The x-axis is on a log-scale. The dotted line indicates the minimum TFP of connected firms after dropping the 3 lowest TFP connected firms. Non-connected firms: N = 18,317. Connected firms: N = 241.

TFP. The highest productivity of a non-connected firm is about 70% higher than the highest TFP among all connected firms. Furthermore, we know that due to **rational rent-seeking**, $\tilde{\tau}_i \geq 0$, so that $\text{TFP}_i \geq \tilde{z}_i$, which establishes that there is no connected firm with productivity higher than that of all non-connected firms in the data. For the lower bound, I assume that no connected firms have lower productivity than all other non-connected firms in the data. This assumption is very weak since connected firms are unlikely to be that unproductive, and a violation of this assumption would imply unrealistically high subsidies.

The parameters of the selection process are estimated as follows. Call the number of connected firms N_C and the sample of connected firms C_i, which is ordered by TFP. Under the assumption of **conservative normalization**, the subsidy is zero for the connected firm with the lowest possible productivity. $C_i = 1$ refers to the connected firm with the lowest productivity in the data. Setting the subsidy to zero for this connected firm gives the most conservative estimate of the lower productivity bound that is still in line with the data and the assumption of **conservative normalization** without extrapolating beyond the lowest TFP connected firm observed. It is conservative because it raises the productivity estimates for all connected firms and, in turn, lowers their subsidy estimates. Since $\bar{z}$ entirely depends on the lowest observed TFP of connected firms, this estimator is susceptible to low-TFP outliers among connected firms, which would drive up estimates of subsidies. Again, I take a conservative approach to bias my estimates against finding high subsidies by dropping the three connected firms with the lowest observed TFP. The dotted line in Figure 2 reports the baseline estimate for $\bar{z}$. To estimate $\bar{\rho}$, first define the truncated productivity distribution of non-connected firms $\tilde{Z}(q)_{\bar{z}}$ for any quantile q. For the case of $\rho = 0$, sampling from the truncated productivity distribution is uniform so that the subsidy distribution is given by $\tau(q) = TFP(q)/\tilde{Z}(q)_{\bar{z}} - 1$ for all uniformly spaced $(N_C - 1)$ quantiles. The estimator may also be referred to as a *quantile matching estimator*. For the lower subsidy bound, one draws bootstrap samples from the truncated productivity distribution according to the additional correlation ρ. The productivity estimate is the average productivity across bootstrap samples for each connected firm. $\bar{\rho}$ is the maximum possible correlation ρ for which another subsidy estimate than for $C_i = 1$ becomes zero.

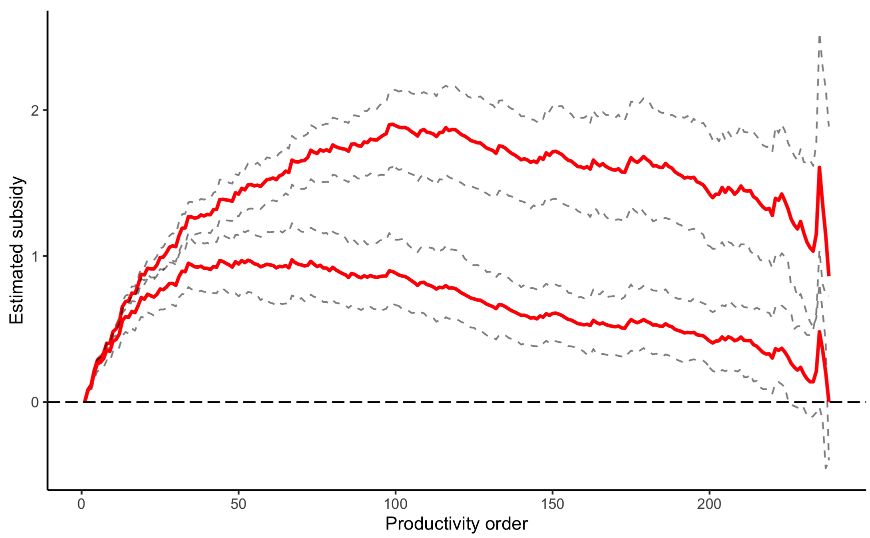

Figure 3: Baseline non-parametric estimates of conservative subsidy bounds. Red lines give point estimates formed by average estimated productivities across bootstrap samples respectively for the lower and upper bound. Grey, dashed lines give point-wise 95% bootstrap confidence bands using 10,000 bootstrap samples. Lower bound estimated to be given by $p = 0.93$. Estimates based on assumptions explained in the text and estimated using data on cross-section of Indonesian manufacturing firms in 1997 based on Statistik Industri, the Indonesian manufacturing firm census. Connected (N = 238, dropped 3 outliers) vs. non-connected (N = 18,317) firms are identified as in Mobarak & Purbasari (2006).

The resulting non-parametric estimates of the *Political Connections Technology* τ_i are shown in Figure 3, plotted over the ordering of productivities. Both the estimated upper and the lower bound subsidy schedules follow a hump shape over productivity; the subsidy first increases and then decreases in absolute terms for highly productive firms. The shape is precisely estimated based on the 95% pointwise bootstrap confidence bands given by the grey dotted lines. The estimated shape follows from the TFP distribution of connected firms being less dispersed and less skewed than the truncated productivity distribution and is not enforced by the estimator. As shown in Section 5, the concave or hump shape also shows up when considering wedges and industry- or type-specific subsidy schedules.

The estimate for $\bar{\rho}$ is 0.93. This leads a firm with productivity similar to the largest connected firm in the sample to be more than 23x as likely to be connected as a firm close to the estimated productivity threshold. Importantly, point-estimated subsidies stay positive over the entire distribution, which is not enforced by the estimation approach except for the connected firm with the lowest TFP. We can thus use the bootstrap confidence bands as an overidentification test for the assumption that $\tilde{\tau}_i \geq 0$. The upper bound clearly passes this test except for the largest connected firm whose subsidy is imprecisely estimated. And even though the lower bound estimate shrinks all subsidies towards zero, confidence bands in this case also clearly rule out negative subsidy estimates.

Through the lens of the model, the estimated subsidy can be directly interpreted as the total price premium paid by the government - a public mark-up. For the average subsidized connected firm, this estimated public mark-up varies from around 65% for the lower bound to about 150% for the upper bound. While it is difficult to directly compare these estimates to other estimates in the literature, Schoenherr (2019) estimates average direct cost increases for politically connected firms of around 30% for Korea. The above estimates are reasonable if indirect subsidies such as tax evasion or direct input subsidies are of a similar magnitude to direct output subsidies.[18]

3.4 Estimating endogenous rent-seeking activities

What do estimated subsidies imply for how firms invest in rent-seeking activities? In the following, I estimate a *Political Connections Technology* that flexibly captures both benefits from investing in rent-seeking activities and costs and explains most variation in non-parametrically estimated subsidies. This functional form is crucial for two reasons: First, it can be used to improve productivity and subsidy estimates by reducing the variance in estimates. Second and more importantly, the functional form is needed to consider partial and general equilibrium counterfactuals where connections are shut down. Especially for general equilibrium counterfactuals, we need to know how political connections distort aggregate capital and labor demand through rent-seeking activities.

Based on the previous model of rent-seeking, connected firms employ workers and capital in a rent-seeking department to oversee all rent-seeking activities within the company. Since rent-seeking activities are not directly observed, I aggregate total rent-seeking in labor and capital using a Cobb-Douglas aggregator: $p \equiv k_p^{\eta} l_p^{1-\eta}$. I assume that $\eta = \frac{\alpha}{\alpha+\beta}$, which keeps the relative importance of capital and labor fixed and gives

[18] It is important to note that while input subsidies are likely quantitatively important, they will be picked up through the output subsidy but they are not isomorphic in this setup.

a realistic relative role for capital and labor in rent-seeking in the absence of better data. Next, I assume that the *Political Connections Technology* takes the form:

$$\tau_i(\varepsilon, z, k_p, l_p) = \varepsilon z p^{\theta_p} - c p^{\theta_c} z^{\theta_z}$$

In Appendix A.5, I provide two different micro-foundations for this functional form. One is where firms bribe and lobby politicians who need to push for regulatory changes, preferential policies and access to government contracts, and an alternative micro-foundation where firms bribe tax collectors to avoid taxes. In both cases, the first part captures benefits from connections. θ_p captures the output elasticity with which bribes are funnelled to politicians via lobbying and obfuscatory exchanges. It also captures the degree of returns with which either politicians or tax collectors can allow for subsidies. Intuitively, this will be identified from the increasing part of estimated subsidies. ε captures the level of benefits so that not having access to political connections shuts down their benefits. Including z allows more productive firms to also be more efficient at rent-seeking activities. The second part of the technology captures costs of political connections. In both micro-foundations, one can think of these costs as capturing the risk of being detected or having some benefits overturned by other politicians who oppose policies in the political process, public scrutiny or by lawsuits. The elasticity θ_c then captures the convexity or concavity of these costs and is identified from the curvature at the top of estimated τ where subsidies change from increasing to decreasing. c captures the level of these costs. Additionally, the term z^{θ_z} captures in a simple way the mechanism that costs of rent-seeking activities may be increasing in firm size, making it harder for larger firms that are in the public eye to obtain subsidies and explaining why estimated subsidy rates τ are decreasing for larger firms. θ_z is identified by how fast subsidies decrease with productivity. Note that there would be no absolute decrease in subsidies without these additional costs.

Figure 4 shows the estimates for the lower and upper bound of the *Political Connections Technology.*[19] The economic model fits estimated subsidies almost perfectly, giving an R^2 of around 95% using a constant subsidy as the baseline comparison. Estimated parameters for the upper and lower bound are very similar and differ mostly in the "level" parameters. Both bounds exhibit strong decreasing returns to scale in benefits from rent-seeking activities ($\theta_p = 0.57 - 0.59$) and slightly convex costs ($\theta_c = 1.15 - 1.2$) in combination with sizable additional costs of rent-seeking activities in firm size ($\theta_z = 2.33 - 2.51$). These additional costs of size are larger for the lower bound and are important to match the faster decline in subsidies. The estimated model generates an interesting trade-off due to the dependence of rent-seeking activities on

[19]Parameters are estimated using non-linear least squares (NLS), minimizing the sum of squared residuals between the non-parametric subsidies and subsidies implied by the model. I use R's "L-BFGS-B" solver, which is a box constrained quasi-Newton method, to solve for optimal parameter values.

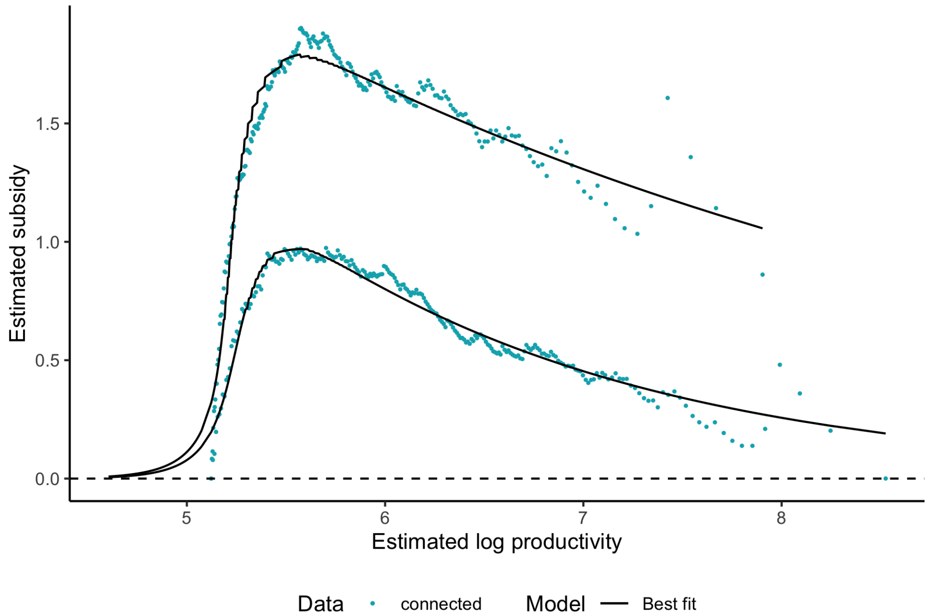

Figure 4: Fitting non-parametrically identified subsidies against estimates of subsidy based on functional form for the Political Connections Technology. Estimates are at the 1-digit industry level, considering a single production function across all firms. Parameters of the Political Connections Technology are chosen to minimize the sum of squared residuals between the non-parametric subsidies from the data and the implied, optimal subsidies from the model. The R^2 is 94.7 percent for the upper bound and 95.1 percent for the lower bound compared to a constant subsidy.

underlying productivity: Highly productive firms are also highly productive at rent-seeking activities, but they are large and visible in the public eye, making any rent-seeking activities riskier. On the other hand, less productive firms are less productive at rent-seeking activities, but they are also less prominent in the public eye, making it easier to avoid detection. Based on the model results, observed connected firms have positive subsidies because they are in the sweet spot where they are productive enough to generate subsidies in the presence of detection costs and not large enough yet to avoid too much public scrutiny. As Figure 4 shows, by extrapolating model-implied subsidies for lower productivities, these subsidies quickly go to zero as costs are too high compared to benefits from rent-seeking activities. Seen through the lens of the model, low productive connected firms are not observed in the data because their subsidies are so low that they are not picked up as politically connected firms by the measure of connected firms used in this paper.

Based on the estimated model of rent-seeking, we can also look at implied spendings on rent-seeking activities. The average connected firm spends about 11% of total labor and capital costs on rent-seeking activities for the upper bound and around 5-6% for the lower bound estimates. However, there is a large variation in these shares across connected firms. Connected firms with the highest subsidies spend as much as 22-27% of input spending on rent-seeking activities while spending on rent-seeking declines quickly with underlying productivity. They are below 2% for the 75th productivity percentile for both bounds, and the most productive firms spend negligible shares on rent-seeking activities. How do these numbers compare to micro evidence on rent-seeking and lobbying activities by firms? Campos et al. (2021) look at judicial documents from the Odebrecht case, the anti-corruption case against a Brazilian engineering and construction conglomerate that bribed hundreds of politicians and political parties across Latin America. In this case, bribe payments alone were estimated to be around 1% of final project costs. Adding additional costs of rent-seeking going to lawyers and workers employed in rent-seeking activities, this observed magnitude is well in line with the economic model as long as we think of the Odebrecht conglomerate and its companies as being above average in size compared to other connected firms.

4 Quantifying the costs of political connections

This section quantifies the costs of political connections using the estimated subsidies and estimated *Political Connections Technology*. The main cost estimates are measured in output and welfare losses compared to the counterfactual economy where political connections are absent. In the last part of this section, I also quantify the benefits of public oversight to limit the role of political connections by studying counterfactual increases in auditing. Throughout, I use estimates of productivities and subsidies based on the estimated *Political Connections Technology*, which reduces estimation variance compared to the non-parametric estimates. The main results are almost indistinguishable when using original estimates, while the robust estimates give slightly more sensible counterfactual results for the largest connected firms. At last, I always separately estimate effects for the conservative upper and lower bounds (denoted LB and UB) and report bounds for all estimates throughout.

4.1 Baseline output and welfare losses from political connections

Table 2 reports the baseline estimates of the aggregate costs of political connections. The presence of political connections costs the economy between 1.0-4.7% of aggregate output and lowers aggregate wages between 1.3-5.6%. The baseline estimates come from comparing the distorted economy with political connections in 1997 to a counterfactual economy where subsidies to connected firms are entirely shut down and where the government lowers output taxes to the extent that total government revenue without subsidies stays constant. One might think of this setting as an economy where the government needs to finance a number of public goods that require a fixed amount of spending and can only do so via distortive corporate taxes. This counterfactual does not require to take a stand on how and why the government spends resources as net tax revenue (apart from resources to connected firms) is kept constant. It is important to note that the baseline counterfactual does not abolish all distortions in the economy; it only reduces size distortions of connected firms to the benchmark of non-connected firms. I further assume that the 1997 distorted economy is in steady state and compare it to the steady-state of the counterfactual economy.[20] To solve for this counterfactual, I jointly solve for the wage that clears the labor market and the tax rate that keeps tax revenue equal to the baseline distorted economy. It turns out that abolishing subsidies to connected firms allows the government to reduce distortive output taxes from 25% to at least 22.2% and up to 17.8% while keeping total government expenditures constant.

Before decomposing the aggregate costs of political connections, it makes sense to briefly discuss the baseline distorted economy as reported in Table 2. In the baseline distorted economy, the average connected firm is estimated to be between 5.2-9.2 times larger than the average non-connected firm. This number is different from the ratio of 12 reported in Table 1, because we are now looking at firm gross output Y_{it} instead of net output $(1-\tau_i)Y_{it}$ reported as value-added revenue in the data. Total output by connected firms makes up between 6.3-10.7% of total output in the economy. At last, the government effectively subsidizes connected firms. Based on the upper bound estimates, the government spends about 25% of tax revenue on subsidizing connected firms. According to the lower bound estimates, on net the government subsidizes connected firms to the extent that their contribution to the government budget is zero.

The output and labor income costs of political connections split up into two main costs. First, is a *misallocation cost* where capital and labor is captured by highly subsidized firms instead of reallocating these resources to more productive firms in the economy. Once subsidies are eliminated, reallocation of resources happens because connected firms downsize and prices in the economy adapt, leading more productive non-connected firms to demand more inputs. The second main cost is the *shadow cost of public funds*. There are costs to raising public funds for government expenditures captured by the distortion that output taxes bring and by the opportunity costs of public funds. In the baseline counterfactual, these costs are only captured by the distortion of the output tax.

To quantify the pure *misallocation cost*, I consider a counterfactual economy where political connections are abolished, but where the *shadow cost of public funds*, hence the baseline distortive output tax, is kept constant. Any additional revenue gains in this counterfactual are then redistributed lump-sum to households.

[20]Transition dynamics between steady states in the static setup are not particularly interesting and relatively fast. Household savings slowly adapt to changes in capital demand. In the case where steady state capital demand rises, interest rates will first spike up and then converge back to the steady state interest rate. While labor supply is fixed, capital labor complementarity will also lead wages to slowly adapt. Relative consumption and output gains depend on household preferences, while steady-state comparisons allow to abstract from specifying them. In the case where the government productively invests tax revenue (considered further below), the stock of public capital increases only slowly, leading to slower transition dynamics and a more muted price response.

Table 2: Main results: Aggregate costs of political connections

Outcomes:	Output		Welfare		Wages		Govt Revenue	
	LB	UB	LB	UB	LB	UB	LB	UB
(Naive) Partial eq. costs	-4.11%	-4.92%	-5.4%	-5.73%	0.0%	0.0%	7.43%	26.61%
Baseline general eq. costs	**1.05%**	**4.67%**	**1.02%**	**4.56%**	**1.31%**	**5.59%**	**0%**	**0%**
Contribution (in %):								
Misallocation	-2.43%	43.94%	40.61%	68.38%	-268.94%	-105.78%	12.01pp	35.9pp
Shadow cost of public funds	102.43%	56.06%	59.39%	31.62%	368.94%	205.78%	-12.01pp	-35.9pp
Costs of worse institutions	**1.65%**	**6.76%**	**0.96%**	**5.11%**	**-1.9%**	**-1.57%**	**13.89%**	**42.17%**

Details: Costs using baseline subsidy estimates. Baseline general eq. (GE) costs are computed by comparing the observed distorted economy with a counterfactual economy where connections are shut down and distortive taxes are reduced such that govt revenue stays constant. Partial eq. results abolish subsidies to connected firms but keep prices fixed. The contribution of misallocation is quantified via a GE counterfactual where taxes stay constant and any additional tax revenue is redistributed lump-sum. Costs of worse institutions is based on an economy where govt resources are invested productively (see Section 4.2). All GE counterfactuals compare steady states. In steady state, the interest rate is pinned down by HH preferences and only the wage may change. LB and UB refer respectively to lower and upper bound estimates. Output refers to net production (without subsidies), Welfare costs are based on the percentage of consumption that households are willing to forego to keep welfare constant (and is equivalent to consumption changes here). Government revenue refers to revenue net of subsidies.

As shown in Table 2, the *misallocation cost* in terms of total output makes up between zero to 45% of the costs of political connections according to the baseline estimates. The lower bound of a zero *misallocation cost* is an interesting limit case in which the benefits from political connections - lowering output tax distortions for connected firms - exactly balance out with the negative effects from misallocating resources.

To better understand the mechanisms that are driving the *misallocation cost*, consider first the partial equilibrium setting in which prices stay fixed (as reported in the first row of Table 2). The partial equilibrium counterfactual already reveals the extent of misallocation in the distorted economy as firm size differences are now only driven by differences in fundamental productivity. In partial equilibrium, abolishing political connections leaves choices of non-connected firms entirely unchanged but leads to a drastic reduction in subsidies to connected firms, leading them in turn to downsize. The average connected firm is between 40-80% smaller when differential subsidies are shut down. This leads to a strong increase in government revenue, but will also lead to a reduction in total output of the economy. In general equilibrium, this clearly does not hold as prices adjust. Here, the reduction in firm size by connected firms means they will lower their demand for capital and labor. This will put downward pressure on prices. Lower interest rates and lower wages incentivize all other firms in the economy to increase their capital and labor demand and this leads to an increase in firm sizes for non-connected firms and pushes up prices again. In a stationary equilibrium, the interest rate stays unchanged because it is pinned down by household preferences: the fall in the interest rate leads households to dissave until the stationary interest rate is reached.

As seen in Table 2 when reporting the relative contribution of the pure *misallocation cost*, these forces on net leave wages lower than they were in the distorted economy with political connections (by 3-6% in levels). The drop in labor and capital demand from connected firms is only partly offset by non-connected firms. In principle, this could also be driven by freeing labor and capital from rent-seeking activities that can now be used in productive activities. However, according to the structural estimates, rent-seeking capital and labor comprise less than 0.2% of aggregate capital and labor and their effects on prices are correspondingly small. Of course, lower wages in general equilibrium do not mean that households are worse off here as seen by the positive welfare effects reported in Table 2 that are driven by lump-sum transfers and redistributing higher firm profits. At last, given that tax revenue is saved from spending it on connected firms and output is increasing, I find that total tax revenue from corporate taxes increases by 12-36% in this general equilibrium counterfactual. About 2/3 of this increase is driven by eliminating transfers to connected firms as seen in partial equilibrium, while the remainder is driven by the general equilibrium response of output, leading all other firms to pay more taxes as they increase their output.

Given that the *misallocation cost* for the baseline estimates are between zero to 45% of the total output costs of political connections, the remaining 55-100% are explained by the *shadow cost of public funds*. These opportunity costs turn out to dominate the total costs of political connections as it would allow the government to additionally reduce distortive tax rates on all firms in the economy. According to the baseline estimates, this effect is strong enough to entirely reverse the negative wage effects when shutting down the *misallocation costs*, as all firms in the economy - especially the most productive firms - demand more labor and capital, driving up final wages by between 1.3 to 5.6%.

At last, based on the model estimates, a large part of the misallocation of resources happens across industries. That is, political connections also distort the relative size of industries, not just the allocation of resources within industries. Based on both bounds, about 10% of industries at the 4-digit level are larger due to political connections. These "connected industries" account for roughly 20% of total output and are between 14-24% larger due to political connections. However, the subsidization of connected firms within these industries comes at the detriment of the remaining 90% of industries that pay average output costs that range from 4-8%.

4.2 The costs of political connections from weakening institutions

The baseline estimates of the costs of political connections are based on a *shadow cost of public funds* that comes solely from distortive taxes. However, in developing countries, the costs of political connections likely also go through the quality of public goods and institutions more generally. Political connections deteriorate institutions and weaken the rule of law since subsidies crowd out resources that could otherwise be spent on public goods or improving public institutions. To capture this effect, I consider an alternative cost estimate that is based on a counterfactual where the government uses tax revenue productively to invest in productive public goods such as infrastructure, legal institutions, security and the enforcement of property rights. This counterfactual clearly requires more assumptions. Specifically, one needs to take a stand on how the government spends resources, how efficient it is at spending these resources on productive public goods and how public goods enter firms' production functions.

I assume the following standard setup for government investment (e.g. Ramey 2020). The productivity of a firm is now also directly determined by spending on public goods G. I assume that we can rewrite TFP as $\text{TFP}_i = (1+\tilde{\tau}_i)(1-\bar{\tau})\tilde{z}_i G^{\chi}$, where χ captures the private output elasticity of public goods. While there is considerable debate in the literature on the value of this elasticity, I am not separately estimating this parameter here and simply assume a conservative long-run value of 0.1 taken from the literature (see Bom and Ligthart 2014). Call total tax revenue T and suppose that the government invests a constant fraction of this tax revenue productively (I^G) in public goods according to the following law of motion: $G' = (1-\delta_G)G + \kappa I^G$. Public goods G capture public capital such as roads and other infrastructure as well as any other resources the government spends that affect firm production. Here, δ_G gives the depreciation rate of public goods[21] and $\kappa \in (0,1]$ gives a measure of public funds that are misused (see: Pritchett 2000). I take the estimate of κ directly from the randomized controlled trial in Olken (2007) who studies corruption in infrastructure spending in Indonesia finding that on average about 24% of funds are lost in infrastructure projects financed by the central state.[22] I also assume that the government spends a constant 35% of tax revenue on productive investments and returns the remainder as lump-sum transfers to households.[23] In a stationary economy, G and T are constant so that the stationary level of public goods is $G^* = \frac{\kappa I^G}{\delta_G}$. To solve for this counterfactual, I jointly solve for the wage that clears the labor market and G^* that is consistent with tax revenue. I report results in the last row of Table 2.

It turns out that empirically, introducing public goods exacerbates the aggregate costs of political connections. Since higher tax revenue leads to better institutions which in turn incentivizes firms to produce even more, this creates a positive feedback loop that drives up benefits from higher tax revenue. In the Indonesian data, the aggregate costs of political connections taking into account productive public goods are about 1.65-6.75% of total output, as captured by the comparison with the distorted economy with public goods. The positive feedback loop eventually leads to tax revenue and hence public good spending that is between

[21] This is taken to be 3.25% following Arslanalp et al. (2010).

[22] This means that $\kappa = 0.76$ in the following.

[23] I do not have a direct estimate of this fraction, but this number is 20% in the US (Ramey 2020). I vary this parameter and find that welfare benefits are higher, the lower this share. Hence, I take 35% as a conservative estimate.

14-42% higher than in the distorted economy. For the conservative lower bound estimates, all of these output costs are driven by the *shadow costs of public funds*. For the upper bound estimates, about 30% of the total costs of political connections are *misallocation costs* while the remaining 70% are *shadow costs of public funds*. These results indicate that investing the increased tax revenue in public goods is far more effective in this context than lowering tax rates, which turns out to be robust to alternative parameterizations of the benefits of public goods. For example, more than doubling the depreciation rate of public goods to $\delta_G = 7.5\%$ per annum and halving the private output elasticity of public capital to $\chi = 0.05$, still gives general equilibrium output increases that are comparable to the constant tax revenue counterfactual. Given the robustness of the public goods results and the more interesting and arguably more relevant economic mechanisms, I take the public goods case as the preferred estimate of the costs of political connections.

4.3 Quantifying the benefits of public oversight

Numerous societal actors constrain the influence of politically connected firms by enforcing taxes and regulation, voicing concerns over legislature and executive orders that benefit connected firms and uncovering tax evasion and corruption. This subsection looks at the benefits of all these activities which I simply call public oversight. To quantify how important public oversight is in limiting the aggregate costs of political connections, I consider the following setup. Suppose we could increase the extent of public oversight in the Indonesian economy, which one can think of as increasing the amount of tax audits, increasing efforts of congressional oversight and increasing the amount of investigative reports by watchdogs such as Global Witness and Transparency International by some common factor x. Suppose further that there is a fixed cost of such audits F (total costs of increasing audits is then $x * F$). Note that the audit effectiveness is still an endogenous outcome of the model, depending on how much connected firms invest in rent-seeking activities. More specifically, since $\theta_c > 1$ in the estimated Political Connections Technology, the marginal detection probability is decreasing in the level of audits, making any additional audit less effective, which connected firms also take into account.

While the costs of increasing audits is unobserved and hence a full cost-benefit analysis is beyond the scope of this paper, one can still quantify the output gains over the increase in audits x. To do so, I consider counterfactuals in which I increase the level of public oversight as given by c in the Political Connections Technology by a factor x.[24] Furthermore, I only consider counterfactuals where the government returns any additional revenue lump-sum to households and does not use it in a productive way (e.g. via lowering

[24] In Appendix A.5, I discuss in more detail the microfoundation of the Political Connections Technology as well as the exact interpretation of all parameters, including the level of public oversight c.

distortive taxes or investing extra revenue in public goods). This comes closest to a policy in which only the intensity of audits is changed. Note that there is a trade-off even in the case of zero audit costs, because subsidizing connected firms can be beneficial up to the point where subsidies relax distortive taxes. This means that the maximum output gain over x gives an upper bound for the optimal amount of auditing. I find that while maxima differ widely across the lower and upper bound, both bounds suggest large output gains from increasing auditing. Based on the conservative lower bound estimates, maximal output gains for zero costs of audits are reached slightly below doubling auditing economy-wide and a social planner that cares about maximizing output should be willing to spend as much as 0.1% of GDP on this. To get a rough idea of this magnitude, taking Indonesia's GDP in 1997, 0.01% of GDP amounts to about ten times the annual global budget of Transparency International in 2019.[25] For the upper bound, output gains are as large as 2.1% of GDP, which are realized for increasing auditing threefold. Through the lens of the model, audits are effective despite not targeting heavily subsidized connected firms that pose especially large aggregate costs. Based on the model parameters, connected firms endogenously respond to a general increase in audits in such a way that audits also end up affecting the entire distribution of subsidies in a uniform way. There is an even stronger case for increasing audits in case they can be targeted at more heavily subsidized firms.

5 Extensions & Robustness results

In this section, I consider a variety of different robustness exercises and extensions. I start out by considering the role of variation in marginal revenue products, wedges and market power. I then turn to re-estimating non-parametric subsidies allowing for further industry- and connection-type variation and at last I consider how sensitive results are to measurement error and misreporting.

5.1 Wedges and the costs of market power

The setup so far has abstracted from any variation in factor revenue shares. A large part of the misallocation literature following Hsieh and Klenow (2009) have used this variation to estimate wedges in static first-order conditions that pose aggregate costs for the economy. To discuss how this additional variation might affect results, Table 5 in Appendix A.2 reports evidence on observed labor and capital spending shares across and within industries. Connected firms have systematically lower observed factor revenue shares than non-connected firms. While factor revenue shares between the two groups of firms are only slightly lower for capital, there are large differences for labor. Differences in labor shares decline considerably when comparing

[25] See: https://www.transparency.org/en/the-organisation/our-operating-budget. Accessed on 12th May 2022.

shares within industries, but stay large. One more important feature of the data is that factor shares are up to 60% more dispersed for connected than for non-connected firms. In Appendix A.2, I describe in more detail how these results are derived and show their robustness.

Through the lens of the model and in the spirit of Hsieh and Klenow (2009), systematic dispersion in factor revenue shares can be explained by reduced-form wedges that prevent firms from optimally choosing inputs. Specifically, we can define idiosyncratic labor and capital wedges based on the following distorted first-order conditions:

$$\alpha R_i^* = (1+\tau_{iK}) r k_i^* \tag{5}$$

$$\beta R_i^* = (1+\tau_{iL}) w l_i^* \tag{6}$$

where R_i^* is observed optimal firm revenue, τ_{iK} and τ_{iL} are firm-specific wedges for capital and labor choices and k_i^* and l_i^* are productive capital and labor inputs. Assume that all firms report their productive inputs plus some potentially firm-specific fraction of their rent-seeking activities (that is: $\tilde{k}_i \in [k_i^*, k_i^* + k_{ip}^*]$ and similarly for labor). Then it follows directly that lower observed factor revenue shares translate into connected firms facing higher wedges, indicating that they face higher implicit input costs, which Hsieh and Klenow (2009) interpret as size restrictions.[26]

To quantify how wedges affect the costs of political connections, I reestimate subsidies, the *Political Connections Technology* and general equilibrium counterfactuals allowing for wedges. For simplicity, I assume that connected firms only report productive capital and labor inputs. Further, normalizing wedges by assuming that median reported factor shares for non-connected firms identify output elasticities, both idiosyncratic wedges and TFP can be directly estimated in the data for all firms. The estimation approach for subsidies of connected firms then remains unchanged assuming that the assumptions in Propositions 3.1 and 3.2 continue to hold. Most importantly, this means that connected firms are still only selected based on productivity (and not directly on wedges) and that wedges do not break the monotonicity of TFP, which naturally holds as long as wedges do not directly enter the *Political Connections Technology.* This rules out quid-pro-quo benefits where subsidies are offered conditional on how connected firms choose inputs.

Figure 5 shows conservative upper and lower bound estimates of subsidies. In comparison to the baseline estimates in Figure 3, estimated subsidies allowing for additional idiosyncratic wedges leads to roughly 40% higher average subsidy estimates. The reason for this directly follows from observing higher and more dis-

[26] The interpretation of positive wedges as size restrictions also holds for a dynamic setting: in the case where within industry variation in revenue factor shares is driven by dynamic input choices such as with time to build capital and labor or factor adjustment costs, observing lower factor shares would mean connected firms face higher or more binding adjustment costs (e.g. see Asker, Collard-Wexler, and De Loecker 2014).

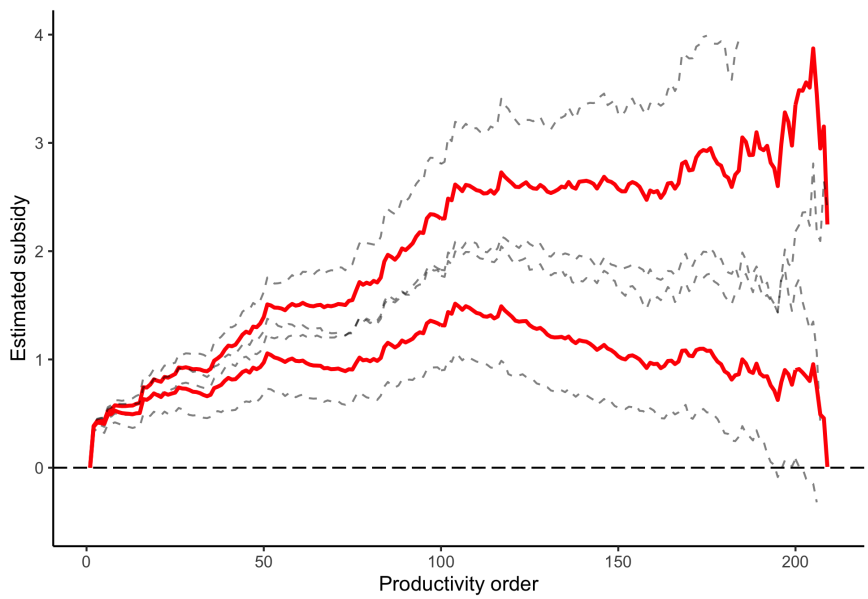

Figure 5: Non-parametric estimates of conservative subsidy bounds additionally allowing for firm-specific wedges in capital and labor input costs. Red lines give point estimates formed by average estimated productivities across bootstrap samples respectively for the lower and upper bound. Grey, dashed lines give point-wise 95% bootstrap confidence bands using 10,000 bootstrap samples. Lower bound estimated to be given by $p = 0.39$, implying that the most productive connected firm was about 10 times more likely to become connected than the least productive connected firm. Estimates based on assumptions explained in the text. Connected (N = 209, after dropping same outliers as for baseline) vs. non-connected (N = 14,713) firms with observed inputs are identified as in Mobarak & Purbasari (2006).

persed wedges for connected firms as they put downward pressure on size differences between connected and non-connected firms, requiring higher subsidies to explain large observed differences in size distributions.[27] While wedges do lead to more heterogeneity, non-parametric subsidy estimates can be well explained by model-implied subsidies based on optimally choosing rent-seeking activities taking into account firm's own productivity, the costs of rent-seeking activity as well as idiosyncratic wedges. Specifically, model-implied subsidies explain about 70% of the variation in subsidies for both bounds based on the R^2 with the average subsidy as the comparison. The key economic mechanisms as captured by the estimated elasticities stay unchanged: benefits of political connections continue to exhibit decreasing returns to scale ($\theta_p \approx 0.52-0.56$) and costs of political connections are convex in rent-seeking activities ($\theta_c \approx 1.25 - 1.28$) as well as firm size ($\theta_z \approx 2.08 - 2.26$).

To consider the aggregate costs of political connections, one needs to take a stand on what estimated wedges capture and how these wedges may change in a counterfactual economy where political connections are abolished. Building on good evidence for Indonesia that connected firms are in less competitive industries (Hallward-Driemeier, Kochanova, and Rijkers 2020), that connected firms are much more likely to receive licenses that buy them market power (Mobarak and Purbasari 2006) and that there is a positive correlation between within-industry firm size and profit shares (shown in Appendix A.2), I interpret higher wedges of connected firms (which capture higher profit shares) as being primarily driven by market power. The benefit of the previous estimation approach is that each connected firm automatically has a matched sample of comparable non-connected firms who have not benefited from political connections. I can then directly use this firm-specific matched set of comparable firms to infer counterfactual wedges. To maintain realistic variation in wedges in the counterfactual, I bootstrap 10,000 counterfactuals in which I randomly sample a single wedge for each connected firm among the set of wedges of matched firms. Abstracting from aggregate changes in market power due to abolishing political connections, connected firms in this counterfactual will only lose the market power that is associated to their connections and not the market power that they would have in either case because of their high productivity. Table 4 shows that aggregate costs of political connections additionally taking into account wedges and market power are at least 30% higher than the benchmark costs. Output costs are more precisely estimated and lie between 4.5-6%. Furthermore, we can quantify the contribution of the market power channel by estimating costs without re-drawing wedges. I find that market power of connected firms as measured by the counterfactual reduction in the dispersion of wedges drives between 10-35% of the total costs of political connections.

[27]Note that the productivity threshold $\bar{z}$ is re-estimated so that most of the changes in the subsidy estimates are driven by the increased dispersion in wedges for connected firms rather than the level difference.

5.2 Industry heterogeneity

In this subsection, I consider subsidy estimates and costs of political connections under more industry heterogeneity. The non-parametric within-industry estimator separately draws productivities from non-connected firms within the same industry and matches firms accordingly. This introduces a trade-off as within industry matching matches firms that are more similar while at the same time reduces both the population from where productivities can be drawn and the sample with which one can match. Relatedly, there is a bias-variance trade-off in estimating industry-specific cutoffs $\bar{z}_s$ and industry-specific selection parameters ρ_s. As a solution to this bias-variance trade-off, I enforce a single $\bar{z}$ and ρ across industries. I discuss these points and the estimation in more detail in Appendix A.6.

Subsidy estimates for four different levels of production function heterogeneity are reported in Figure 6 showing only upper bound estimates for expositional clarity. Within each panel, each line marks one different industry at the respective digit. Three patterns are noteworthy. First, comparing the 1-digit to the 2-digit results, we can see that the concave or even hump-shaped *Political Connections Technology* does show up in most industries (6 out of 8), indicating that it is an important feature of the data. The two 2-digit industries where this pattern is less clear have only few observations, leading to very noisy estimates. While results are slightly harder to interpret at the 3-digit and 4-digit level, we can still see hump-shape relationships between the subsidy and productivity within many industries. Secondly, the level of the subsidies increases slightly when allowing for more production function heterogeneity, reflecting slightly larger size differences within industries as reported in Table 1. At last, productivity estimates change considerably across the different specifications since production function elasticities now vary across industries.

For the costs of political connections, I re-estimate the *Political Connections Technology* taking into account further heterogeneity. We generally expect the parameters of the *Political Connections Technology* to vary across industries as industries differ in how closely related they are to the political system, affecting the difficulty of lobbying for preferential policies or receiving government contracts, and they differ in visibility, affecting oversight and the chance of preferential deals being detected and publicly reported. However, this variation is not summarized in a single parameter in the proposed *Political Connections Technology.* To explain estimated subsidies with further industry heterogeneity, while keeping estimation parsimonious, I keep the same functional form and estimated parameters, but allow the parameters that govern levels (ε and c) and parameters that govern elasticities to differ by a common factor across 2-digit industries. This adds two additional parameters per industry and I found this a good compromise between not overfitting, while allowing reasonable variation in the *Political Connections Technology* across industries that mimics

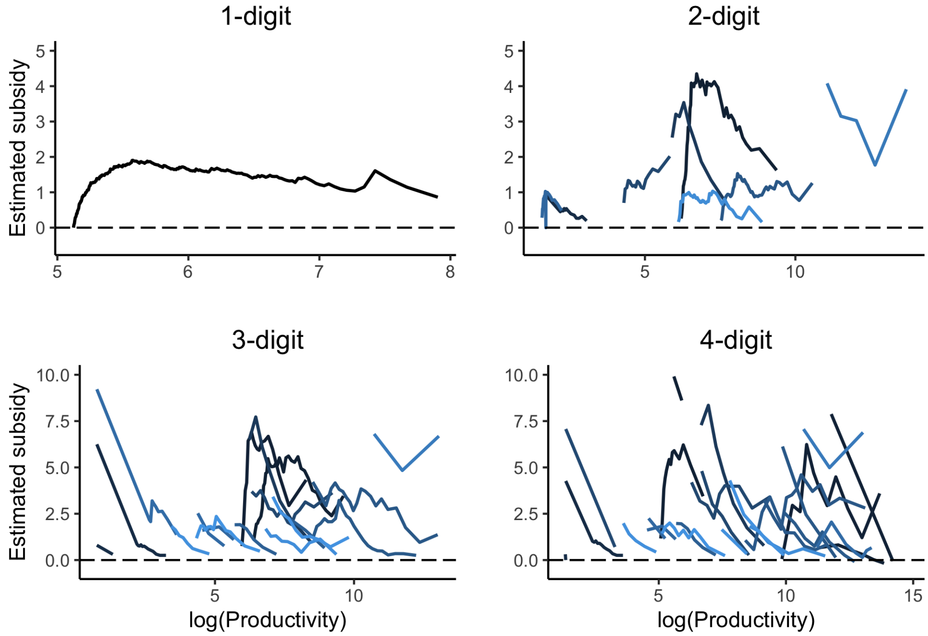

Figure 6: Non-parametric identification of subsidies to connected firms as function of estimated firm-level productivities with industry heterogeneity. Panel 1-4 give estimates at 1-, 2-, 3- and 4-digit industry levels respectively using the upper bound estimator for productivities. Estimation is explained in the text. Connected (N = 238, dropped 3 outliers) vs. non-connected (N = 18,317) firms are identified as in Mobarak & Purbasari (2006).

the same hump shape pattern of subsidies and captures the same fundamental drivers of observed subsidies. Estimated parameters at the 2-digit level maintain decreasing returns to scale in benefits from rent-seeking activities and convex costs both in rent-seeking activities and firm size. The R^2 of the noisier estimates at the 2-digit industry level still exceeds 95%. However, as for the baseline results, rent-seeking activities have only a very limited effect on aggregate effects because they only raise aggregate capital and labor by a small amount. The key effects go through extensive subsidies and firm-size distortions which misallocate productive capital and labor at marginally unproductive firms. Estimated costs of political connections turn out to be similar at the 2-digit level and much higher at the 3-digit level, which should be interpreted with care given noisier subsidy and rent-seeking activity estimates.

5.3 Testing for further types of connected firms

The second exercise to judge the robustness of the results is to consider more heterogeneity in the types of connected firms ε. I consider two sets of type heterogeneity. For the estimation, I enforce the same productivity cutoff and the same conservative selection bounds as for the main estimates, in line with the null hypothesis of identical *Political Connections Technologies*. For each set of type heterogeneity, I then separately draw and match bootstrap samples for the differently-connected firms to obtain productivity estimates.

Panel A of Figure 7 reports separately estimated subsidies and productivities for "normal" connected firms and for firms directly owned, founded and run by blood relatives of dictator Suharto. One can think of many reasons why *Political Connections Technologies* should look differently for the two sets of connected firms and why the latter set of firms should receive larger subsidies. Perhaps surprisingly, estimated *Political Connections Technologies* look almost indistinguishable, with blood connected firms receiving slightly higher subsidies. I also formally test equality of *Political Connections Technologies* by considering bootstrapped confidence bands and cannot reject equality.[28] Panel B shows separately estimated subsidies and productivities for connected firms that are at least partly state-owned and connected firms that are not. One concern with the estimates might be that state ownership changes the relationship between connected firms and politicians and thus leads to very different *Political Connections Technologies*. Again, I find no evidence for this in the data. While estimated subsidies are slightly larger for connected firms that are state-owned (in line with economic intuition), the distribution of subsidies looks very similar and as for the previous results I cannot reject equality from a statistical point-of-view. These are encouraging findings for the paper, because it alleviates concerns that unobserved type-heterogeneity or a few connected firms are biasing the results, lending credence to the baseline results. Similarity in the subsidy estimates also shows up when estimating the aggregate costs of political connections: I find largely similar, though slightly higher estimated costs as reported in Table 4.

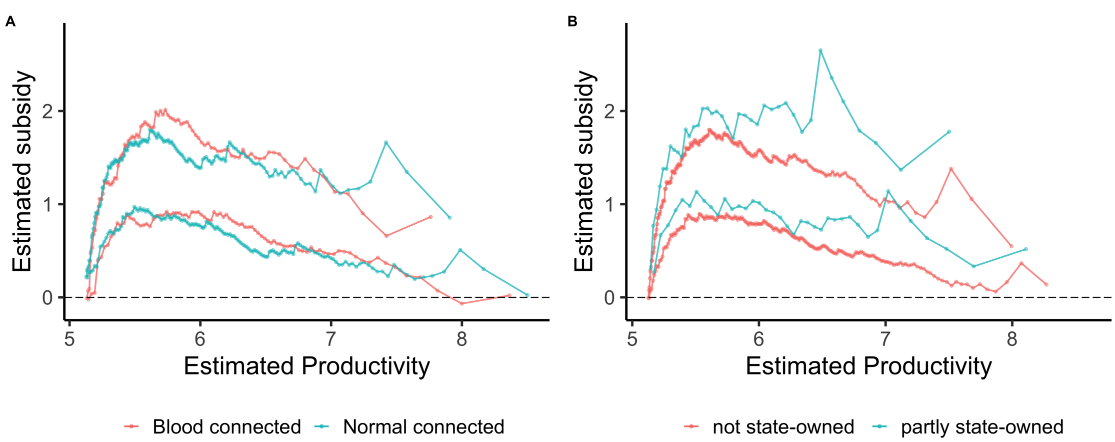

Figure 7: Non-parametric estimation of output subsidies and productivity allowing for different Political Connections Technologies for different types of connections. Panel A: Connection types distinguish blood connected (N = 89) and normal connected (N = 152) firms (details in Section 2). Panel B: Distinguishes connected firms that are partly state-owned (N = 39) and connected firms that are not state-owned (N = 199).

[28]Formally, I only consider point-wise overlap in confidence bands, which is not a full statistical test. However, for most points, confidence bands include point estimates, which is sufficient for rejection. Statistical power of this test is obviously limited given the few number of connected firms by type, but differences in point estimates are also economically small.

5.4 Measurement error

The non-parametric subsidy estimation seems to rely crucially on the assumption that value-added output is correctly reported, because conditional on the production function parameters, estimated TFP only relies on variation in reported value-added output. Estimated subsidy schedules are then inferred from the relative dispersion in TFP between connected and non-connected firms. This subsection considers how sensitive subsidy estimates are in the presence of measurement error in value-added output.

I consider four types of measurement error. For each type I independently draw B type-specific realizations of measurement error for each firm. For each iteration $b \in B$, I obtain true output by purging observed output from the measurement error and then reestimate subsidies, the political connections technology and welfare costs for both bounds. I report average results for each type of measurement error.[29] In case not otherwise stated, I choose the variance of measurement error such that a regression of reported output on real output gives an $R^2 = 0.75$.

Figure 9 in Appendix A.6 reports average alternatively estimated subsidies for each type of measurement error. Table 3 reports the corresponding welfare effects. In Panels A-C, I consider measurement error that affects all firms, connected and non-connected, and that has mean zero. Panel A considers multiplicative measurement error of the form: $\tilde{y}_{it} = y_{it} * \text{error}_{it}$ where $\tilde{y}_{it}$ is reported value-added output and $log(\text{error}_{it})$ is normally distributed. Estimated subsidy schedules are almost entirely unaffected, which shows up in almost identical welfare costs. The reason is that observed log output is also close to normally distributed so that adding normally distributed errors leaves the output distribution unaffected. While not shown here, I also consider distributions with heavier or lighter tails by simply scaling normally distributed measurement error and find also no quantitatively meaningful differences in subsidy estimates.[30] To consider error that is differently distributed than log output, I consider non-symmetric measurement error by now letting error_{it} be normally distributed. In this case, subsidy estimates turn out to be higher, which also translate into higher welfare costs of connections. The reason is that this measurement error led to a lower dispersion of the right tail of the observed output distributions, biasing baseline subsidy estimates downward and leading to underestimate true subsidies. Similarly, in case measurement error led to a higher dispersion of the observed output distributions on the right tail, then the baseline subsidy estimates would be overestimated. Still, based on Panel A and B, these two forms of measurement error have little effects on the overall shape of the estimated subsidy schedules nor estimated welfare costs.

To consider a potentially more problematic case of bias, I consider measurement error that correlates directly

[29] I choose B = 50.

[30] Specifically, I take $log(\text{error}_{it})^{\varphi}$ with $\varphi \in \{0.5, 1.5\}$. In case of negative errors, I take $-(-x)^{\varphi}$.

Table 3: Main robustness and extension results: Aggregate costs of political connections

Outcomes:	Output		Welfare		Wages	
	LB	UB	LB	UB	LB	UB
Baseline GE costs	**1.05%**	**4.67%**	**1.02%**	**4.56%**	**1.31%**	**5.59%**
Market Power + Wedges:						
Full costs	4.57%	5.97%	4.89%	6.05%	7.65%	12.66%
Market power contrib. (%)	35.81%	10.22%	35.45%	9.56%	18.56%	8.97%
Industry heterogeneity:						
2-digit	1.09%	3.53%	0.96%	3.3%	2.99%	6.37%
3-digit	9.4%	12.23%	8.81%	12.89%	7.18%	4.91%
Type heterogeneity:						
Blood vs. normal	1.59%	4.66%	1.54%	4.54%	2.01%	5.59%
State-owned vs. not	2.03%	4.82%	1.97%	4.69%	2.58%	5.78%
Measurement Error:						
Classic	1.19%	5.07%	1.15%	4.91%	1.52%	6.15%
Non-symmetric	1.7%	5.37%	1.64%	5.24%	2.13%	6.38%
Correlated	1.17%	4.27%	1.12%	4.13%	1.47%	5.18%
Underreporting C	1.22%	5.42%	1.17%	5.24%	1.54%	6.58%

Details: Aggregate costs of political connections under various robustness exercises and model extensions. Throughout, general eq. costs are computed by comparing the observed distorted economy with a counterfactual economy where connections are shut down and distortive taxes are reduced such that government revenue stays constant. All general equilibrium counterfactuals compare steady states. LB and UB refer respectively to lower and upper bound estimates. Output refers to net production (without subsidies), Welfare costs are based on the percentage of consumption that households are willing to forego to keep welfare constant (and is equivalent to consumption changes here). Government revenue refers to revenue net of subsidies.

with firm size in Panel C. Specifically, I consider $log(\text{error}_{it}) = \beta_0 + \beta_1 log(\tilde{y}_{it}) + \nu_{it}$ where ν_{it} is mean zero normally distributed, β_0 is such that the overall error is mean zero and $\beta_1 > 0$. Perhaps surprisingly, it turns out that this form of correlated measurement error also leaves subsidy estimates and estimated welfare costs basically untouched. The reason for this is that this form of correlated measurement error does not affect the relative dispersion of output distributions across connected and non-connected firms. To also consider the effect of differential output distortions, I introduce measurement error that only affects connected firms in Panel D. Specifically, I assume that all connected firms systematically underreport a fixed 20% of output.[31] The results again are robust to this form of measurement error. The reason is that such selective underreporting is entirely captured by the productivity cutoff. A higher percentage of underreporting simply leads to a lower estimated cutoff, leaving the estimated levels unbiased. If anything, the upper bound welfare cost estimates are now somewhat higher.

[31] I considered underreporting of 5%, 10% and 20% respectively, but results were all quantitatively similar and I report the results with the most measurement error.

6 Conclusion

This paper has provided a structural approach to quantify the general equilibrium costs of political connections. More precisely, using a model where firms endogenously invest in rent-seeking activities to obtain firm-specific subsidies, I showed how to non-parametrically identify conservative bounds for these subsidies and flexibly estimate the technology with which firms invest in rent-seeking activities.

Applying this methodological approach to Indonesia, I find high aggregate costs of political connections between 1.0-4.7% of output. Costs are even higher when accounting for market power and effects on public goods. Given that connected firms in the data make up only around 1% of firms and contribute less than 15% of total output, I show that a few big firms in the economy can have large aggregate costs. In contrast to Restuccia and Rogerson (2008), these are high aggregate costs even in a context where subsidies are (weakly) positively correlated with productivity. At most, 45% of the costs are *misallocation costs*. The majority of costs capture opportunity costs of putting saved subsidies to better use. Given the important role of the opportunity costs of public funds highlighted in this paper, an interesting corollary is that the costs of political connections should be increasing in the efficiency of public spending and decreasing in the level of baseline distortions.

More qualifications of the results are in order. I showed that results are robust to further industry- and type-heterogeneity, wedges and different forms of measurement error, but some issues are harder to assess. For example, due to data constraints, the focus of this paper has been on manufacturing plants. Political connections likely play an even bigger role in other sectors and at the firm-level such that the current results provide conservative lower bounds. Furthermore, political connections will always remain elusive, making measurement of them difficult. This paper's measure of political connections is based on a natural experiment and arguably the most credible estimates we have. One important avenue for future research is to collect more direct evidence on rent-seeking activities (e.g. as in contemporaneous work by Huneeus and Kim (2021) on measuring lobbying). The estimated model in this paper predicts an entire distribution of rent-seeking activities, which seems to be in line with anecdotal quantitative evidence (Campos et al. 2021), but should be validated further. This paper has also shed light on how political connections distort the allocation of talent within and across firms (see Murphy, Shleifer, and Vishny 1991, 1993). Excitingly, future research could use the framework in this paper to quantitatively study how rent-seeking beyond connected firms distorts the allocation of talent, including human capital investments.

Methodologically, the subsidy estimation approach in this paper can be used in other contexts where one is interested in relative distortions across groups that have common support in fundamentals and where one can use static first-order conditions, and cross-sectional revenue data is available. Another avenue of future research is to study the dynamics of political connections in more detail. It is unclear whether dynamic input choices will increase or decrease the costs of political connections.[32] With better data that tracks the status of connections over time and the time firms obtain connections, one could use the proposed structural matching approach to compare connected and non-connected firms over time to infer the dynamic gains of political connections. This methodological approach should also have applications in other fields of research.

7 References

Akcigit, Ufuk, Salomé Baslandze, and Francesca Lotti. 2018. "Connecting to Power: Political Connections, Innovation, and Firm Dynamics." National Bureau of Economic Research.

Amiti, Mary, and Jozef Konings. 2007. "Trade Liberalization, Intermediate Inputs, and Productivity: Evidence from Indonesia." *American Economic Review* 97 (5): 1611–38.

Arslanalp, Serkan, Fabian Bornhorst, Sanjeev Gupta, and Elsa Sze. 2010. "Public Capital and Growth." Citeseer.

Asker, John, Allan Collard-Wexler, and Jan De Loecker. 2014. "Dynamic Inputs and Resource (Mis) Allocation." *Journal of Political Economy* 122 (5). University of Chicago Press Chicago, IL: 1013–63.

Aslund, Anders. 2019. *Russia's Crony Capitalism*. Yale University Press.

Bai, Chong-En, Chang-Tai Hsieh, and Zheng Song. 2020. "Special Deals with Chinese Characteristics." *NBER Macroeconomics Annual* 34 (1). The University of Chicago Press Chicago, IL: 341–79.

Bertrand, Marianne, Francis Kramarz, Antoinette Schoar, and David Thesmar. 2018. "The Cost of Political Connections*." *Review of Finance* 22 (3): 849–76. https://doi.org/10.1093/rof/rfy008.

Bom, Pedro RD, and Jenny E. Ligthart. 2014. "What Have We Learned from Three Decades of Research on the Productivity of Public Capital?" *Journal of Economic Surveys* 28 (5): 889–916.

Brugués, F., J. Brugués, and S. Giambra. 2018. "Political Connections and Misallocation of Procurement Contracts: Evidence from Ecuador." CAF.

Campos, Nicolás, Eduardo Engel, Ronald D. Fischer, and Alexander Galetovic. 2021. "The Ways of Corruption in Infrastructure: Lessons from the Odebrecht Case." *Journal of Economic Perspectives* 35 (2):

[32]Given that the baseline results abstract from variation in factor shares as used in Hsieh and Klenow (2009), this means that dynamic input choices do not necessarily bias the estimates of the costs of political connections (see Asker, Collard-Wexler, and De Loecker 2014).

171–90.

Carney, Richard W., and Travers Barclay Child. 2013. "Changes to the Ownership and Control of East Asian Corporations Between 1996 and 2008: The Primacy of Politics." *Journal of Financial Economics* 107 (2): 494–513. https://doi.org/10.1016/j.jfineco.2012.08.013.

Chen, Ting, and James Kai-sing Kung. 2018. "Busting the 'Princelings': The Campaign Against Corruption in China's Primary Land Market." *The Quarterly Journal of Economics* 134 (1): 185–226.

Claessens, Stijn, Simeon Djankov, and Larry HP Lang. 2000. "The Separation of Ownership and Control in East Asian Corporations." *Journal of Financial Economics* 58 (1-2). Elsevier: 81–112.

Costinot, Arnaud, and Andrés Rodríguez-Clare. 2014. "Trade Theory with Numbers: Quantifying the Consequences of Globalization." In *Handbook of International Economics*, 4:197–261. Elsevier.

Cremer, Helmuth, and Firouz Gahvari. 1994. "Tax Evasion, Concealment and the Optimal Linear Income Tax." *The Scandinavian Journal of Economics*, 219–39.

Do, Quoc-Anh, Kieu-Trang Nguyen, and Anh N. Tran. 2017. "One Mandarin Benefits the Whole Clan: Hometown Favoritism in an Authoritarian Regime." *American Economic Journal: Applied Economics* 9 (4): 1–29. https://doi.org/10.1257/app.20130472.

Faccio, Mara. 2006. "Politically Connected Firms." *American Economic Review* 96 (1): 369–86.

Fisman, Raymond. 2001. "Estimating the Value of Political Connections." *American Economic Review* 91 (4): 1095–1102. https://doi.org/10.1257/aer.91.4.1095.

Fisman, Raymond, and Yongxiang Wang. 2015. "The Mortality Cost of Political Connections." *The Review of Economic Studies* 82 (4): 1346–82.

Garcia-Santana, Manuel, Enrique Moral-Benito, Josep Pijoan-Mas, and Roberto Ramos. 2020. "Growing Like Spain: 1995–2007." *International Economic Review* 61 (1). Wiley Online Library: 383–416.

Gonzalez, Felipe, and Mounu Prem. 2019. "Losing Your Dictator: Firms During Political Transition." *Working Paper*. https://doi.org/10.2139/ssrn.2670869.

Gonzalez, Felipe, Mounu Prem, and Francisco Urzúa. 2018. "The Privatization Origins of Political Corporations." *Working Paper*, 46.

Guner, Nezih, Gustavo Ventura, and Yi Xu. 2008. "Macroeconomic Implications of Size-Dependent Policies." *Review of Economic Dynamics* 11 (4). Elsevier: 721–44.

Hadiz, Vedi R., and Richard Robison. 2013. "The Political Economy of Oligarchy and the Reorganization of Power in Indonesia." *Indonesia* 1 (96): 35–57.

Hale, Christopher D. 2001. "Indonesia's National Car Project Revisited." *Asian Survey* 41 (4). JSTOR: 629–45.

Hallward-Driemeier, Mary, Anna Kochanova, and Bob Rijkers. 2020. *Does Democratization Promote Competition?: Indonesian Manufacturing Pre and Post Suharto*. The World Bank.

Haselmann, Rainer, David Schoenherr, and Vikrant Vig. 2018. "Rent Seeking in Elite Networks." *Journal of Political Economy* 126 (4). University of Chicago Press Chicago, IL: 1638–90.

Hoang, Kimberly Kay. 2018. "Risky Investments: How Local and Foreign Investors Finesse Corruption-Rife Emerging Markets." *American Sociological Review* 83 (4): 657–85.

Hsieh, Chang-Tai, and Peter J. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics* 124 (4): 1403–48. https://doi.org/10.1162/qjec.2009.124.4.1403.

Huneeus, Federico, and In Song Kim. 2021. "The Effects of Firms' Lobbying on Resource Misallocation." *Working Paper*. MIT Political Science Department Research Paper.

Johnson, Simon, and Todd Mitton. 2003. "Cronyism and Capital Controls: Evidence from Malaysia." *Journal of Financial Economics* 67 (2): 351–82.

Koren, Miklos, Adam Szeidl, Ferenc Szucs, and Balazs Vedres. 2015. "Political Favor Exchange in a Democracy." *Unpublished Working Paper*.

Martinez-Bravo, Monica, Priya Mukherjee, and Andreas Stegmann. 2017. "The Non-Democratic Roots of Elite Capture: Evidence from Soeharto Mayors in Indonesia." *Econometrica* 85 (6): 1991–2010.

Mobarak, Ahmed Mushfiq, and Denni Puspa Purbasari. 2006. "Corrupt Protection for Sale to Firms: Evidence from Indonesia." *Unpublished Working Paper, University of Colorado at Boulder*.

Murphy, Kevin M., Andrei Shleifer, and Robert W. Vishny. 1991. "The Allocation of Talent: Implications for Growth." *The Quarterly Journal of Economics* 106 (2). MIT Press: 503–30.

———. 1993. "Why Is Rent-Seeking so Costly to Growth?" *The American Economic Review* 83 (2). JSTOR: 409–14.

Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy* 115 (2): 200–249.

Pritchett, Lant. 2000. "The Tyranny of Concepts: CUDIE (Cumulated, Depreciated, Investment Effort) Is Not Capital." *Journal of Economic Growth* 5 (4): 361–84.

Ramey, Valerie A. 2020. "The Macroeconomic Consequences of Infrastructure Investment." National Bureau of Economic Research.

Restuccia, Diego, and Richard Rogerson. 2008. "Policy Distortions and Aggregate Productivity with Heterogeneous Establishments." *Review of Economic Dynamics* 11 (4). Elsevier: 707–20.

———. 2017. "The Causes and Costs of Misallocation." *Journal of Economic Perspectives* 31 (3): 151–74.

Rijkers, Bob, Caroline Freund, and Antonio Nucifora. 2017. "All in the Family: State Capture in Tunisia." *Journal of Development Economics* 124 (January): 41–59. https://doi.org/10.1016/j.jdeveco.2016.08.002.

Robison, Richard, and Vedi Hadiz. 2004. *Reorganising Power in Indonesia: The Politics of Oligarchy in an Age of Markets*. Routledge.

Schoenherr, David. 2019. "Political Connections and Allocative Distortions." *The Journal of Finance* 74 (2). Wiley Online Library: 543–86.

Slemrod, Joel. 2001. "A General Model of the Behavioral Response to Taxation." *International Tax and Public Finance* 8 (2): 119–28.

Slemrod, Joel, and Shlomo Yitzhaki. 2002. "Chapter 22 - Tax Avoidance, Evasion, and Administration." In *Handbook of Public Economics*, edited by Alan J. Auerbach and Martin Feldstein, 3:1423–70. Handbook of Public Economics. Elsevier. https://doi.org/https://doi.org/10.1016/S1573-4420(02)80026-X.

Straub, Stéphane. 2014. "Political Firms, Public Procurement, and the Democratization Process." *Institut d'Economie Industrielle (IDEI), Working Paper* 1 (817).

Szucs, Ferenc. 2017. "Discretion and Corruption in Public Procurement." *Job Market Paper*.

A Appendix

A.1 Further details on measuring political connections

Mobarak and Purbasari (2006) extend the work by Fisman (2001) by examining how the stock price of the universe of firms traded on the Jakarta Stock Exchange (JSX)[33] responded to adverse news about Suharto's health in various episodes between 1994 and 1997. Using daily stock price data for the 985 market trading days between 1994 and 1997, they run a set of regressions of abnormal stock returns for each firm on aggregate movements in the JSX, the average return for the industry category in which that firm belongs, movements in the exchange rate and interest rate, and an indicator variable for days when the news about Suharto's health was reported by the press. A firm is defined to be "politically connected" if the Suharto health news indicator has a negative coefficient which is significantly different from zero at the 95% confidence level. Using statistical significance as a threshold gives a firm-specific threshold that also takes into account the firm-specific variability of its stock price.[34] This identifies 29 stock listed firms as being politically connected and the authors used newspapers and other media to confirm that these firms were indeed connected.

The identities of the key personnel running these 29 politically connected firms allow Mobarak and Purbasari (2006) to identify, by proxy, other firms that are connected to Suharto, but not traded on the Jakarta Stock Exchange. The authors do this by locating all other firms that share ownership and management with those 29 firms. As Claessens, Djankov, and Lang (2000) and Carney and Child (2013) show, ownership and control is rarely separated in Southeast Asian firms including Indonesia and most firms belong to larger conglomerate structures that are owned by specific families. This allows to link stock-listed firms to a larger network of other firms of the same conglomerate, who are owned by the same family. Due to the prevalence of political connections being tied to interpersonal links between families, this allows to track connected firms beyond stock-listed firms. Specifically, Mobarak and Purbasari (2006) identify each member of the Board of Directors and Board of Commissioners of each of the 29 firms using the Indonesian Capital Market Directory 1998. They then use the publication *400 Prominent Indonesian Businessmen* to find the names of all conglomerates to which the individuals running the connected firms belong. Finally, they turn to *Conglomeration Indonesia* to identify all subsidiary firms of the 'connected' business groups and trace all other firms and conglomerates that share ownership and management. In total, this gives them 2,126

[33] The authors estimate this for 285 of the 293 firms traded on the Jakarta Stock Exchange at that time.

[34] The authors use three different definitions of firm stock returns, including the actual return, the deviation of the actual return from its average, and the abnormal return net of movements correlated with the aggregate JSX market return. They also variably define the event dates to be the day the illness occurs or the day it is reported in the press. The identities of 'politically connected' firms are roughly invariant to the particular definition of returns or event dates used. Note that using statistical significance as a filter may introduce differential bias by size. If the variability of stock prices is related to fundamentals such as firm size then statistical power will vary by size of firm and then selection will be worse for smaller firms. I have not conducted tests or simulations to assess this concern, but given that T = 985, it seems like power should not be large concern.

connected firms.

The implicit assumption at this point is that all relevant political connections in Indonesia go through larger conglomerates which have at least one publicly traded firm that is identified as being politically connected. Thus, this definition of political connections captures “high-level” political connections and is unlikely to capture more local connections of firms to local authorities in the bureaucracy or police. This should be kept in mind when interpreting the results in this paper. Another key concern of using this measure of connections is that it is likely to capture only larger firms and is more likely to miss small connected firms. In the structural approach used later in the paper, results will explicitly depend on the smallest observed connected firms exactly to be robust to the idea that if all connected firms are large and successful this must not imply that connections are very beneficial, but could also be driven by the fact that we do not capture smaller and less successful connected firms in the data.

The next limitation of the data is that while the approach allows to identify a variety of connected firms, the available firm-level data to link these to is the annual manufacturing census data that captures medium- and large-sized manufacturing firms with more than 20 employees. This considerably restricts the sample: only 16 of the 29 initial stock-listed firms and 408 of the 2,126 identified connected firms are manufacturing firms, which makes up roughly 20% of firms. Based on the GGDC 10-sector database, the manufacturing sector accounted for about 34% of value-added output in 1997, which is squarely between the percentage of manufacturing firms among stock-listed firms and the percentage among all connected firms. It is unclear exactly what biases this sample selection introduces, but it may even lead to more conservative estimates of the costs of political connections given that connections are likely to play a bigger role in a number of non-manufacturing sectors such as utilities (including telecommunications and energy), mining, construction, finance and land-dependent agriculture. Of these manufacturing firms, linking them to the census is further complicated by the fact that firms are generally de-identified in the manufacturing census data. Using three broad identifying variables - province location, 5-digit industry code and (rough) number of employees - Mobarak and Purbasari (2006) can succesfully match 241 firms or 59% of connected firms to the census of manufacturing firms. Mobarak and Purbasari (2006) argue that the attrition involved in this matching step is not related to any fundamentals and should thus not differentially bias the estimates apart from underestimating the number of connected firms.[35]

[35] However, I have not been able to validate this claim and replicate this part of their analysis given that the authors could not share this part of the analysis with me. There could be a number of reasons why the matching step could introduce additional problems. For example, matching by (rough) number of employees may introduce bias against small firms as this set of firms may include more overlap in the number of employees and thus makes it less likely to find unique matches in the data. On the other hand, matching by province location may make it harder to match more successful firms in more economically active parts of the country (e.g. Java). Access to the set of all connected manufacturing firms could allow to control for potential differential misclassification in this step of the analysis.

In the end, this approach allows to identify 241 connected firms in the manufacturing census data. It allows to identify the snapshot of politically connected firms at the highest level for a short time period of around 1-2 years shortly before the Asian Financial crisis in 1997/8. Throughout the paper, I allow the set of connected firms to vary over time with some firms loosing their connections or seeing changes in the extent of their connections, but all results will be based on the set of connected firms in 1997 and I therefore assume that this is a representative picture of connected firms also for other years in the data. Of the 241 firms, 89 firms are identified as being owned and founded by blood connections of Suharto. 34 of these 89 firms are similarly identified as being connected by the stock market identification approach. This imperfect overlap may be due to three different problems. First, it may show that the stock market identification approach is highly imperfect in capturing all connected firms (only about 40% of connected firms are identified). This could be due to the nature of the approach only capturing firms that are linked through conglomerates that have a stock listed firm or the statistical uncertainty in the estimates, but it could also be because the approach only captures connected firms whose connections are deemed sufficiently volatile. These issues only pose a real problem for this paper if they bias the identified size distribution of connected firms, otherwise, this paper will only underestimate the costs of political connections. Second, imperfect overlap may indicate that not all blood connected firms identified in the data truly benefit from their connections. In this case, I could overestimate the costs of political connections. However, if the assumptions for the estimation of subsidies are correct, this should be picked up by the estimation approach.

A.2 Further empirical results

A.2.1 Further differences between connected and non-connected firms

In this subsection of the Appendix, I report further results on differences between connected and non-connected firms that are in part referenced in the text, but not reported. First, Figure 8 reports size distributions for connected and non-connected firms in 1997 using gross firm output instead of value-added output. This looks very similar to the corresponding value-added figure. In fact, the average connected firm is slightly less than 12 times as large as the average non-connected firm for both value-added and gross output measures.

Second, we can look at size differences between connected and non-connected firms within industries looking at (real) gross output figures instead of (real) value-added figures. Similar to Table 1 in the main text, I report differences between connected and non-connected firms in Table 4.

Next, Table 5 reports descriptive evidence on median observed labor and capital spending shares across and

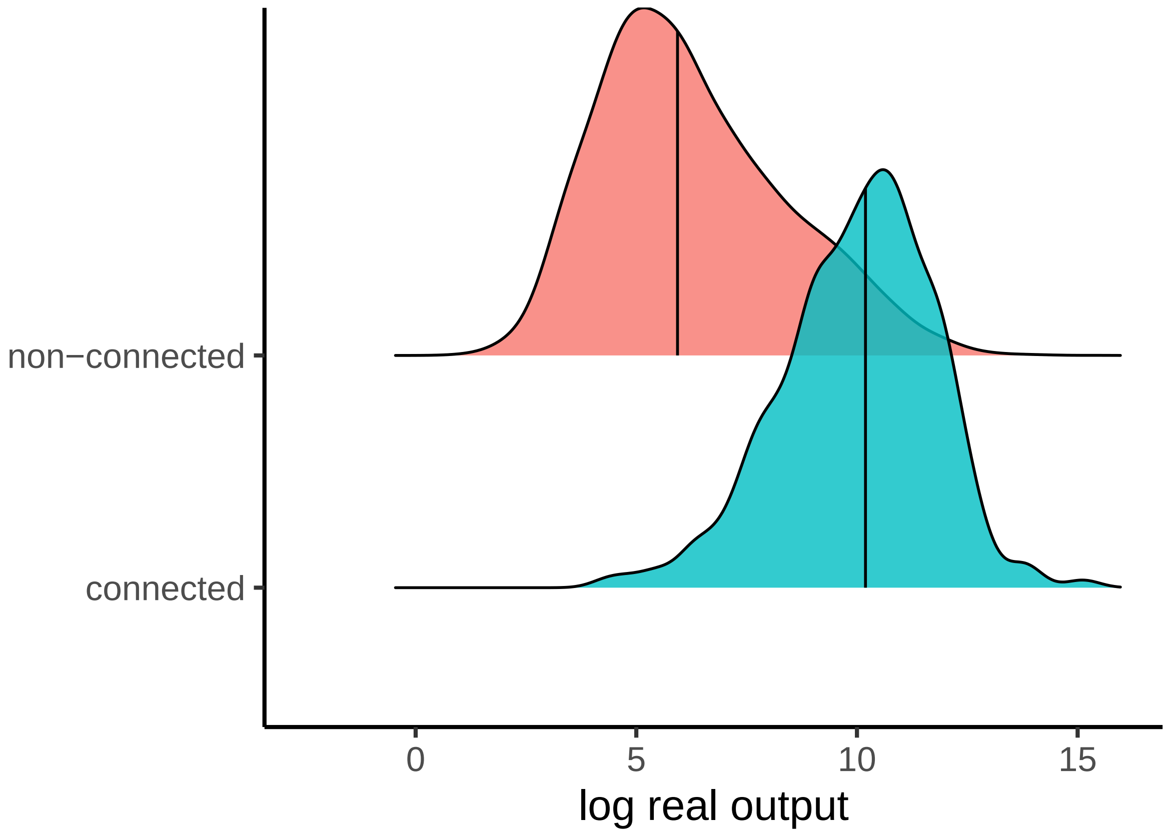

Figure 8: Distributions of firm-specific real gross output (in logs and in 2010 USD) for cross-section of Indonesian firms in 1997 based on Statistik Industri, the Indonesian manufacturing firm census. Connected vs. non-connected firms are identified as in Mobarak & Purbasari (2006). Non-connected firms: N = 18,303. Connected firms: N = 241.

Table 4: Average relative size of connected vs. non-connected firms within industry (for gross output)

		Within industry		
	unconditional	2-digit	3-digit	4-digit
Difference	11.77	12.62	11	9.44
# connected	241	241	241	241
# non-connected	18,317	18,317	18,317	18,317

Table 5: Median observable factor revenue shares for labor and capital for connected (C) and non-connected (NC) firms across and within industries

	labor share (va)		capital share (va)		Total share (va)	
	NC	C	NC	C	NC	C
Unconditional	0.51	0.20	0.26	0.18	0.82	0.45
Within 2-digit	0.48	0.23	0.26	0.20	0.77	0.47
Within 3-digit	0.45	0.23	0.27	0.23	0.75	0.48
Within 4-digit	0.39	0.24	0.23	0.21	0.60	0.42
Within 5-digit	0.37	0.27	0.22	0.26	0.57	0.49

Details: The table reports median factor shares of connected and non-connected firms across the different factor inputs (columns) and across industries (rows). For within-industry estimates, median factor shares in each industry are computed separately for connected and non-connected groups and are aggregated across industries using the number of connected firms within an industry as weights.

within industries. I compute capital and labor shares using the reported wage bill and the capital bill as a ratio over reported value-added output. For the capital bill, I use the estimated capital stock of a firm and multiply it with the effective model-based rental rate of capital. Additionally, I compute the sum of revenue shares for capital and labor. To obtain within industry estimates, I aggregate median factor shares at the industry-level across industries using the number of connected firms within an industry as weights. Table **??** reports averages instead of median factor shares.

Results are very similar: Connected firms have much lower observable labor shares, but very similar capital and materials shares. This is not an issue of selection into specific industries that have lower labor shares, but also holds within industries. Additionally, we can look at the dispersion of factor revenue shares by comparing coefficients of variation, the ratio of the standard deviation over the mean. Looking across all industries, I find that labor revenue shares are 60% more dispersed for connected than for non-connected firms. The coefficient of variation for connected firms is around 0.94 while it is around 0.57 for non-connected firms. Similar results but smaller differences in dispersion hold for capital (0.99 vs. 0.82) and total shares (0.84 vs. 0.56). These results are robust to outliers.

A.2.2 Correlation between firm size, market share and profit share

This subsection reports regression results for how profits correlate with market shares (defined at different industry levels). Results clearly show a positive relationship between market shares and profits, which is in line with theories where the market share is a measure of market power and is thus correlated with profits. Furthermore, the results show that connected firms seem to have even larger profit shares conditional on their market share, indicating that political connections might buy market power beyond what is expected based on firm size.

Table 6: Market Power Regressions: testing the relationship between profits and market share

	Profit share							
	1-digit	1-digit	2-digit	2-digit	3-digit	3-digit	4-digit	4-digit
Market Share (1-digit)	72.174***	60.782*						
	(5.510)	(35.050)						
Market Share (2-digit)			10.172***	9.612***				
			(0.730)	(2.832)				
Market Share (3-digit)					4.466***	4.818***		
					(0.261)	(0.861)		
Market Share (4-digit)							1.890***	2.266***
							(0.096)	(0.199)
Non-connected?	−0.225***	−0.136***	−0.201***	−0.119***	−0.199***	−0.108***	−0.186***	−0.090**
	(0.033)	(0.042)	(0.033)	(0.040)	(0.033)	(0.040)	(0.033)	(0.038)
Constant	0.359***		0.334***		0.329***		0.311***	
	(0.033)		(0.033)		(0.033)		(0.033)	
Industry FE (4-digit)?	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.016	0.203	0.017	0.206	0.023	0.213	0.029	0.222
Adjusted R^2	0.015	0.197	0.017	0.200	0.023	0.207	0.029	0.216

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors are clustered at the fixed effect level.

A.3 Productivity vs. demand and the relation between single good vs. differentiated inputs with CES demand

In this part of the Appendix, I show a standard result in the heterogeneous firm literature, namely that a setup with a single good produced by heterogeneous firms with DRS technology is isomorphic to a setup where firms produce differentiated goods and face CES demand. The latter setup makes it clearer that z_i in the model used in this paper can flexibly capture both productivity and demand processes.

Assume the economy is populated by a mass of identical households of total measure L who each supply labor inelastically and consume a large variety of differentiated goods according to a standard Constant Elasticity of Substitution (CES) demand system.[36] To allow for variation in demand across industries, I consider two different levels of nested preferences such that products within and across industries s can have

[36] E.g. see Costinot and Rodríguez-Clare (2014), or Hsieh and Klenow (2009).

different elasticities of substitution:

$$C = \left(\sum_s \psi_s^{\frac{1}{\sigma}} C_s^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \tag{7}$$

$$C_s = \left(\int_{i \in s} \psi_i^{\frac{1}{\sigma_s}} c_{i,s}^{\frac{\sigma_s-1}{\sigma_s}} di \right)^{\frac{\sigma_s}{\sigma_s-1}} \tag{8}$$

where C, C_s and $c_{i,s}$ are respectively the demands for the composite consumption good, for the sector-specific composite good and for the differentiated goods. $\psi_s \geq 0$ and $\psi_i \geq 0$ are exogenous demand parameters that are sector-specific and firm-sector-specific. $\sigma \geq 1$ and $\sigma_s \geq 1$ capture the elasticities of substitution between composite goods from different sectors and between differentiated goods within a sector. Households are assumed to statically choose consumption that maximizes their utility, leading to a simple and well-known closed-form expression for product demand:

$$c_{i,s} = B_{i,s} p_{i,s}^{-\sigma_s} \tag{9}$$

where $B_{i,s}$ denotes a combination of the exogenous demand parameters.[37] Note that this setup assumes that households do not distinguish between goods of connected and non-connected firms within industries and in the CES setup this means that connected firms have no additional market power within industries.

On the firm side, we have individual heterogeneous firms i in industry s that differ in their firm-specific productivity $A_{i,s}$ and their political connections and that produce differentiated products $q_{i,s}$ with a standard decreasing-returns-to-scale (DRS) Cobb-Douglas production function that is industry-specific (denoted by s):

$$q_{i,s} = A_i k_i^{\tilde{\alpha}_s} l_i^{\tilde{\beta}_s} \tag{12}$$

where $q_{i,s}$ is firm-specific output, k & l are firm-specific capital and labor inputs, and $\tilde{\alpha}_s$ & $\tilde{\beta}_s$ are industry-specific output elasticities. Firm i statically chooses the optimal price $p_{i,s}$ given inputs k & l such that demand and supply equalize. Suppose further that firms are small so that they cannot affect the aggregate price level P nor industry-level price levels P_s and thus take product demand as given.[38] At last, political

[37] Specifically, product-specific demand is given by:

$$C_s = \psi_s C \left(\frac{P_s}{P} \right)^{-\sigma} \tag{10}$$

$$c_{i,s} = \psi_i C_s \left(\frac{p_{i,s}}{P_s} \right)^{-\sigma_s} \tag{11}$$

where P is the aggregate price index, P_s are the price indices for sectoral composite goods and $p_{i,s}$ are prices for final differentiated goods.

[38] This is a standard assumption in models of monopolistic competition, but may be violated in case where we look at large

connections enter through a revenue or output subsidy τ_i such that firm-specific revenue is given by:

$$R_{i,s} = (1+\tau_i)p_{i,s}q_{i,s} = (1+\tau_i)A_i^{\frac{\sigma_s-1}{\sigma_s}} B_i^{\frac{1}{\sigma_s}} k_i^{\alpha_s} l_i^{\beta_s} \equiv (1+\tau_i)z_i k_i^{\alpha_s} l_i^{\beta_s} \tag{13}$$

z_i then measures a combination of demand and supply factors, which I simply call "productivity" throughout the paper. The idea is that the process z_i is seen as a highly flexible, exogenous process that is not directly affected by political connections, but benefits from political connections τ_i can directly depend on z_i and can additionally correlate due to self-selection.

A.4 Optimal subsidies to connected firms in the presence of distortive taxes

In this section, I formally solve for optimal subsidies to connected firms.[39] The problem of optimal subsidies is an optimal taxation problem where the government has a fixed amount of resources $\bar{T}$ it needs to levy from connected firms that are heterogeneous in productivity and tries to set firm-specific output tax rates τ_i to maximize total output for this group of firms. Note that this reduces to net subsidies instead of net taxes if $\bar{T} < 0$ and individual firms are subsidized in case $\tau_i < 0$. This encompasses arbitrary subsets of firms: e.g. the government might only be able to set some of the taxes/subsidies in an idiosyncratic way (for connected firms), while for others (non-connected) taxes could be fixed. I start with the simpler case of a partial equilibrium analysis where input prices are unaffected by the taxes. Given that the focus is on arbitrary taxes for a few firms, this is almost equivalent to the optimal taxes in general equilibrium and I deal with the general case further below.

I show that the partial equilibrium problem has a simple solution that requires setting a constant subsidy rate across connected firms. This means that more productive firms will receive higher total amounts of subsidies, but not at a higher subsidy rate. Take any subset of firms for which the government tries to maximize their output by setting idiosyncratic output tax rates. That is:

$$\max_{\{\tau_i\}_i} \sum_i z_i k_i^*(\tau_i, w, r)^\alpha l_i^*(\tau_i, w, r)^\beta + \lambda \left[\bar{T} - \sum_i \tau_i z_i k_i^*(\tau_i, w, r)^\alpha l_i^*(\tau_i, w, r)^\beta \right]$$

where $k_i^*(\tau_i, w, r)$ and $l_i^*(\tau_i, w, r)$ give optimal input choices by firms that take their idiosyncratic tax rate as given. Technically, the government optimizes over the envelope of optimal firm decisions and this is a

connected firms within industries that contain only few firms in total. In case it holds, the price is given by:

$$p_{i,s}^* = A_i^{-\frac{1}{\sigma_s}} B_i^{\frac{1}{\sigma_s}} \left(k_i^{\alpha_s} l_i^{\beta_s} \right)^{-\frac{1}{\sigma_s}}$$

[39] I want to thank Matthias Meier for suggesting to do this exercise.

perfect information setup where the government can set idiosyncratic taxes based on the revealed size of the firm. Given Cobb-Douglas production functions and constant input prices across firms, optimal input policies take the following well-known closed-form:

$$k_i^*(\tau_i, w, r) = [(1-\tau_i)z_i]^{\frac{1}{1-\alpha-\beta}} \left(\frac{\beta}{w}\right)^{\frac{\beta}{1-\alpha-\beta}} \left(\frac{\alpha}{r}\right)^{\frac{(1-\beta)}{1-\alpha-\beta}} \tag{14}$$

$$l_i^*(\tau_i, w, r) = [(1-\tau_i)z_i]^{\frac{1}{1-\alpha-\beta}} \left(\frac{\beta}{w}\right)^{\frac{(1-\alpha)}{1-\alpha-\beta}} \left(\frac{\alpha}{r}\right)^{\frac{\alpha}{1-\alpha-\beta}} \tag{15}$$

Taking first-order conditions for any τ_i, we get the following optimal tax condition:

$$\frac{\alpha+\beta}{1-\alpha-\beta} + \lambda \left\{(1-\tau_i^*) - \tau_i^* \frac{\alpha+\beta}{1-\alpha-\beta}\right\} = 0$$

which states that the government should equalize the marginal budget benefits from setting a higher tax rate (captured by the shadow cost of public funds λ) with the negative marginal output effects from setting a higher tax rate. The budget benefits scale with the tax rate times the optimal output that the firm chooses based on the tax rate, while the output scales without the tax rate. The optimal tax rate can then be expressed in closed-form as a function of the shadow cost of public funds:

$$\tau_i = \frac{\frac{\alpha+\beta}{1-\alpha-\beta} + \lambda}{\frac{\lambda}{1-\alpha-\beta}}$$

Importantly, idiosyncratic productivity z_i cancels out in this expression such that optimal tax rates end up being uniform across firms and their level is determined by the need of funds.

In general equilibrium, this result changes slightly. The reason is that any tax changes now also have an indirect effect on equilibrium prices. Fortunately, this is still tractable here. Specifically, the interest rate is pinned down in steady state so that we only need to look at the effect on wages. The market clearing wage, on the other hand, can be solved for in closed-form. Given inelastic labor supply L_t, the following holds for the wage:

$$w^* = \beta \left[L_t^{-1} \sum_j^N [(1-\tau_j)z_j]^{\frac{1}{1-\alpha-\beta}}\right]^{\frac{1-\alpha-\beta}{1-\alpha}} \left(\frac{\alpha}{r}\right)^{\frac{\alpha}{1-\alpha}}$$

One can then write the general equilibrium optimal taxation problem as:

$$\max_{\{\tau_i\}_{i\in C}} \sum_i^N (1-\lambda\tau_i) z_i^{\frac{1}{1-\alpha-\beta}} (1-\tau_i)^{\frac{\alpha+\beta}{1-\alpha-\beta}} \left(\sum_j^N [(1-\tau_j)z_j]^{\frac{1}{1-\alpha-\beta}}\right)^{\frac{-\beta}{1-\alpha}} - \lambda\bar{T}$$

First-order conditions now take into account for the effect a tax change has on the entire distribution of output:

$$0 = \frac{(1-\lambda\tau_i^*)}{1-\alpha-\beta}\left[(\alpha+\beta)(1-\tau_i^*) + z_i^{\frac{1}{1-\alpha-\beta}}\frac{\beta}{1-\alpha}(1-\tau_i^*)^{\frac{\alpha+\beta}{1-\alpha-\beta}}\left(\sum_j^N [(1-\tau_j)z_j]^{\frac{1}{1-\alpha-\beta}}\right)^{-1}\right] - \lambda$$

While this does not have a closed-form form for the optimal tax rate τ_i^*, one can still say something about how the optimal tax rate is changing in productivity z_i. Using the implicit function theorem and plugging in estimated productivity values, I show that the optimal tax rate is (perhaps surprisingly) increasing in productivity. The intuition for this is that for more productive firms, charging them lower taxes increases their input demand disproportionately more, which ends up driving up input prices marginally more than other firms. This is a "small is beautiful" result that stems from the decreasing returns to scale. Importantly, these general equilibrium corrections are small because this is about a few distorted tax rates compared to the entire economy (which shows up in the formula through the inverse of the entire productivity distribution). So in the case where you start from flat taxes and relax the budgetary constraint λ (that is decrease $\bar{T}$), you optimally want to marginally lower everyones' taxes but marginally marginally more for low productive firms.

As a final remark, one could alternatively solve the optimal taxation problem in general equilibrium additionally taking into account the endogenous response of connected firms in influencing their effective tax rate. This would fix the estimated *Political Connections Technology* in this paper and solve for the optimal baseline tax rate, knowing that connected firms distort these. This is another interesting question that the setup and estimations in this paper allow to study more rigorously. I leave this for future work.

A.5 Microfoundations of the Political Connections Technology

In the following I provide two possible microfoundations for the *Political Connections Technology* used throughout the paper that are based on two different interpretations of what political connections buy. In the first interpretation, the *Political Connections Technology* buys output subsidies, while in the second interpretation, the *Political Connections Technology* is reinterpreted as the share of taxes that connected firms pay.

A.5.1 The *Political Connections Technology* as an output subsidy

One interpretation of the *Political Connections Technology* is as a net output subsidy. The parametric form chosen for this technology is: $\tau_i = \varepsilon z_i p^{\theta_p} - c p^{\theta_c} z_i^{\theta_z}$. To microfound this choice, suppose the government can use part of the tax revenue to buy products from firms that are then redistributed to households. As was shown before, τ_i only captures demand beyond standard demand for a similar non-connected firm. That is, the government basically offers a contract to a connected firm saying, whatever your total demand from households, we will pay $\tau_i/(1+\tau_i)$ percent of this demand or we subsidize households' demand by this percentage. The assumption here is that most government policies that directly or indirectly subsidize firms can be represented by this menu over τ_i instead of contracts that are fixed to quantities. That is, politicians directly bargain over subsidy rates and not absolute transfers. The microfoundation of the parametric form of τ_i is then linked to the political process that offers subsidy rates.

Specifically, suppose that for each connected firm there exists a continuum of relevant government bills that each may promise a unit of government demand. τ_i gives at the same time the net subsidy rate obtained by a connected firm i as well as the measure of government bills that the connected firm managed to influence in its favor. Given that there are few connected firms in this economy, this model abstracts from competition for government bills across connected firms and simply assumes that all connected firms care about their own set of government bills that they can influence. There are two terms in the *Political Connections Technology*. The first term captures the amount of bills that the firm managed to influence, while the second term captures the amount of influenced bills that are overturned via audits or other public oversight. Given the continuous measure of government bills, these audits give deterministic detection rates. Let us look at each of the terms in turn.

The first part of the technology ($\varepsilon z_i p^{\theta_p}$) captures the measure of bills that the connected firm manages to influence via bribing and lobbying the politician they are connected to. There are two ways to think about this term that lead to very similar parameter interpretations. First, the politician has direct access to government bills and offers the firm a linear bribe schedule ($\tilde{\tau}_i = \text{const.} * b$), but the firm faces costs of concealment or production costs to transform rent-seeking spending p into actual bribes b so that $b = \widetilde{\text{const.}} + z_i * p^{\theta_p}$ where z_i gives the firm's productivity at concealing bribes and θ_p is now the elasticity of this concealment technology. This captures what economic sociologists call costs of obfuscating bribes as meaningful, symbolic interactions (Hoang 2018). Remember that rent-seeking spending p captures a combination of capital and labor and one can think of this as final goods (any form of bribes such as luxuries and money) or as some combination of capital and labor services. Also, the constant in the linear bribe schedule captures the politician's efficiency and one may think of this as also being potentially heterogeneous across the type of connections - an idea I explore in the paper.

Alternatively, political capital does not need to be converted ($b = p$), but the politician may face direct costs of obtaining the subsidy rates through parliamentary approval, bargaining with other politicians or filling out the paper work. For example, increasingly higher benefits to firms might require the approval of more politicians who all need to be bribed as well (in the case of $\theta_p \in (0, 1)$) or costs of bribing decline as there are increasing returns to scale in filling out paper work (in the case of $\theta_p > 1$). In these cases, θ_p captures the elasticity of costs from obtaining output subsidy rates. z_i then captures that politicians are more efficient at influencing bills if the firm is more productive (as they need to argue less). In both cases, counterfactuals have very similar interpretations. For example, one can think of doubling ε as doubling the efficiency of the politician to transform bribes into subsidies.

For the second term, suppose the politician faces risks of audits or opposition from other politicians. Remember that subsidy rates are determined by a continuum of small amendments to laws or policies. In this case, audits can overturn a fraction of subsidies. The second term ($cp^{\theta_c} z_i^{\theta_z}$) then captures the number of subsidy rates that are overruled by audits. p^{θ_c} captures the idea that benefits to politically connected firms are more likely to be contested by other politicians or the public as the number of distortionary policy and regulatory amendments increases. θ_c measures the elasticity of this opposing reaction. $z_i^{\theta_z}$ instead captures the opposition stemming not from bribes, but from extra scrutiny that larger firms in the economy receive. Importantly, c measures the level of audits in the economy.

An alternative interpretation that is not considered here is one where the *Political Connections Technology* is interpreted as a state-funded project such as a private-public partnership (PPP) or a state-owned enterprise. The idea of this interpretation is that output of connected firms is $(1+\tau_i) z_i k_i^{\alpha_s} l_i^{\beta_s}$, which can be separated into standard output $z_i k_i^{\alpha_s} l_i^{\beta_s}$ and a rent-seeking project $\tau_i z_i k_i^{\alpha_s} l_i^{\beta_s}$ that is financed entirely by the government.

A.5.2 The *Political Connections Technology* as a tax evasion technology

Following the redefinition of the *Political Connections Technology* as a tax evasion technology in Section 4, we can write the share of taxes that connected firms pay as: $\phi_i \equiv 1 - \tau_i \left(\frac{1-\bar{\tau}}{\bar{\tau}}\right)$ where $\bar{\tau}$ gives the official corporate tax rate. Plugging in the parametric form chosen for the *Political Connections Technology*, this can be rewritten as:

$$\phi_i = 1 - \varepsilon z_i p^{\theta_p} \left(\frac{1-\bar{\tau}}{\bar{\tau}}\right) + cp^{\theta_c} z_i^{\theta_z} \left(\frac{1-\bar{\tau}}{\bar{\tau}}\right)$$

The share of taxes that a firm pays is then determined by two terms; the first term decreases and the second term increases the share of taxes as political capital spending is increasing. Suppose the following simple setup. A tax collector is in charge of a firm's filing and has discretion over ϕ_i. The tax collector takes bribes b in the form of capital for setting a lower ϕ_i as in the previous narrative. Suppose that total taxes depend on many different rules, different documents or that it depends on a long list of entries in revenue filings to the tax administration. Suppose that the tax collector charges a bribe for reducing the tax in each document, each data entry or each part of the tax. In this case, the share of taxes paid by the firm can be expressed as a linear rule in bribes: $\phi_i = 1 - \text{const.} * b$. Now suppose that the firm needs to "produce" or "conceal" b so that $b = \widetilde{\text{const.}} * z * p^{\theta_p}$ where θ_p is now the elasticity of this production or concealment technology and z the productivity.

For the second term, suppose the tax collector faces oversight from managers or risk of being checked up on. The tax collector conceals or calculates lower rates for each entry and managers may sporadically check up on any entry. As the number of entries becomes large, the probability of being detected equals the number of checks. Suppose for simplicity that for each check that leads to corrections, the tax collector does not face any punishment and only the tax demands are changed. The tax collector offers to reduce taxes, but does not insure the risk of corrections. Then the second term captures the number of distorted tax entries that become corrected and one can rewrite this term as $\text{const.} * f(\text{size}) * b^{\tilde{\theta}}$, where $f(\text{size})$ captures flexibly the idea that check-ups by superiors might depend on the size of the firm where larger firms are also more likely to be checked up and $\tilde{\theta}$ can be thought of as a span-of-control parameter that captures how close tax collectors are being monitored. For a high $\tilde{\theta}$, this control is high, which leaves little room for tax collectors to change tax rates for connected firms. Importantly, c captures the level of auditing.

A.6 Further details on extensions & robustness results

This part of the Appendix provides more details on the extensions and robustness results presented in Section 5.

A.6.1 Wedges and the costs of market power

A.6.2 Industry heterogeneity

This subsection provides further details on estimating subsidies and costs of political connections under more industry heterogeneity. The non-parametric within-industry estimator separately draws productivities from non-connected firms within the same industry and matches firms accordingly. This introduces a trade-off as within industry matching matches firms that are more similar while at the same time reduces both the population from where productivities can be drawn and the sample with which one can match. For example, in the extreme case of only a single connected firm within an industry, the productivity estimate will simply be the average productivity of non-connected firms above the threshold productivity $\bar{z}_s$ within this industry. Precision of non-parametric productivity estimates is driven by being able to order many firms within bootstrap samples, so the precision of the estimates declines with further industry heterogeneity. Relatedly, allowing for unrestricted industry-specific productivity thresholds $\bar{z}_s$ fixes the lowest TFP connected firm within each industry, restricting more and more subsidies as more industry heterogeneity is considered. As a solution of this bias-variance trade-off, I enforce a single quantile cutoff, meaning that the bottom x% based on TFP of non-connected firms in each industry are excluded when matching. Hence, the implied productivity threshold across industries can still vary depending on the industry-specific distributions of productivities. The cutoff x is then conservatively estimated to be the minimum productivity quantile of connected firms across all industries.[40] The lower subsidy bound is estimated similarly, enforcing a single correlation $\bar{\rho}$ across industries, which is given by the minimum $\bar{\rho}$ for which any other subsidy estimate across any industry becomes zero.[41]

A.6.3 Measurement Error

[40] This is well-defined as long as there are multiple connected firms within an industry. There is only a single connected firm for one industry at the 4-digit level, which I exclude when computing the 4-digit cutoff. The estimated cutoff is around 38.6% at the 1-digit level, 36% at the 2-digit level, 10.2% at the 3-digit level and 8.6% at the 4-digit level.

[41] This is well-defined as long as the upper bound estimates are all strictly positive. This is the case for the 2-digit and 3-digit industries. Here, I find $\bar{\rho} = 0.186$ at the 2-digit level and $\bar{\rho} = 0.157$ at the 3-digit level. At the 4-digit level, a few subsidy estimates are already negative for the upper bound. Formally, the model is rejected at the 4-digit level and given the noise at this estimation level, I abstract from it for the subsequent welfare estimates.

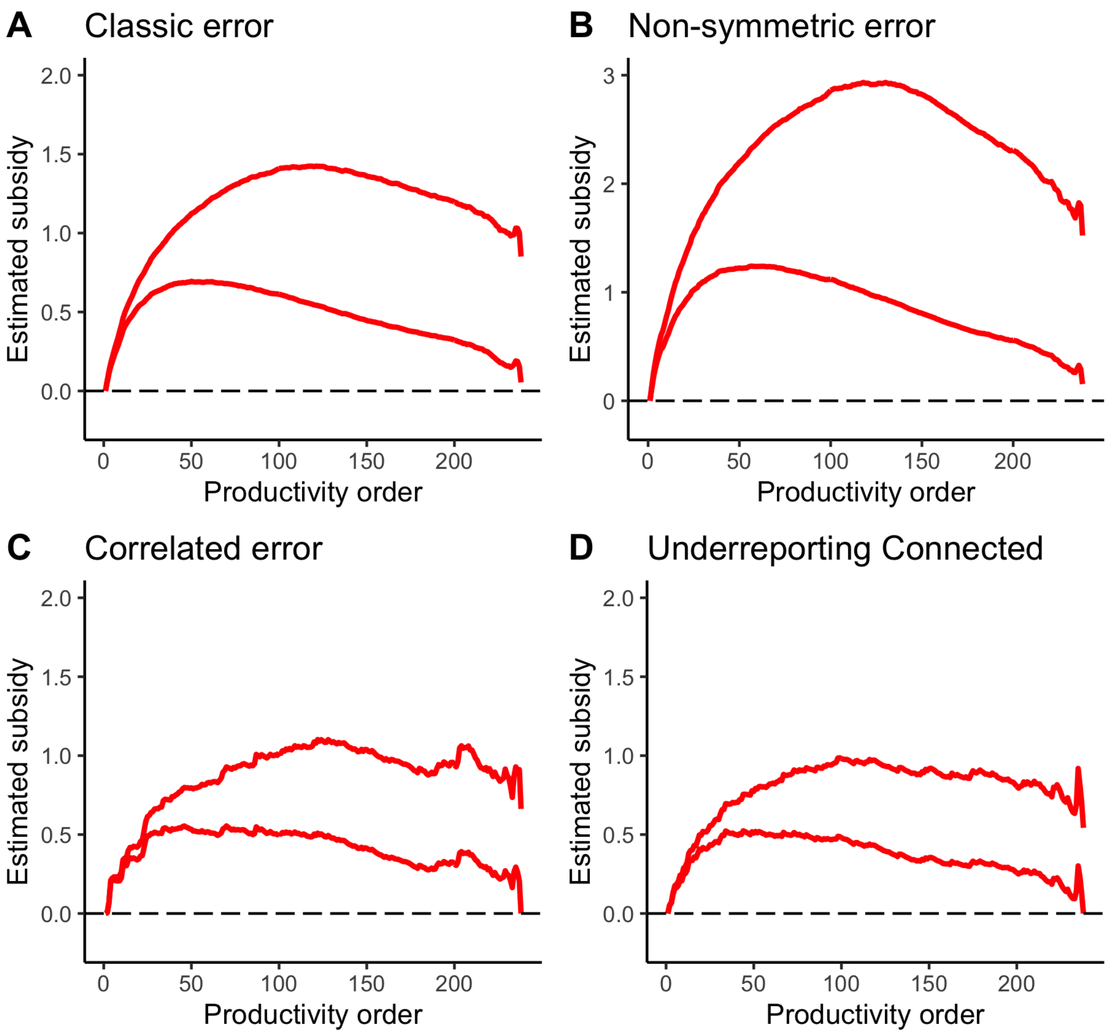

Figure 9: Non-parametric estimation of output subsidies and productivity allowing for different forms of measurement error in reported output (value-added revenue). For Panel A - C, measurement error is mean zero and its variance is chosen such that the R2 of a regression of reported output on real output is 75 percent. Panel A: Classic normal log-additive measurement error for all firms. Panel B: Log-normal log-additive measurement error for all firms (non-symmetric). Panel C: Measurement error that positively correlates with firm size (taking reported output) for all firms. Panel D: Systematic underreporting of connected firms by 20 percent of output. Plot gives average subsidy estimates across 50 independent draws of measurement error for each type.