Political Connections and Misallocation of Procurement Contracts: Evidence from Ecuador^{*}

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Abstract

We use new administrative data from Ecuador to study the welfare effects of the misallocation of procurement contracts caused by political connections. We show that firms that form links with the bureaucracy through their shareholders experience an increased probability of being awarded a government contract. We develop a novel sufficient statistic—the average gap in revenue productivity and capital share of revenue—to measure the efficiency effects, in terms of input utilization, of political connections. Our framework allows for heterogeneity in quality, productivity, and non-constant marginal costs. We estimate political connections create welfare losses between 2 to 6% of the procurement budget.

JEL codes: D61, D73, H57, P16.

Keywords: Allocative efficiency; political connections; public procurement; bureaucracy; production function estimation.

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1 Introduction

The private benefits of political connections for firms are well documented in the literature.¹ Yet, little is still known about the welfare effects of political connections for society. On the one hand, while previous evidence (Schoenherr, 2019; Brogaard et al., 2021; Ryan, 2020) has shown that connections adversely impact the execution of contracts —in terms of cost overruns, delays, and price increases through renegotiations—these could be considered transfers if connected firms are able to deliver real efficiency gains, by requiring fewer real resources in production and offering higher quality output. Indeed, political connections may help channel resources to more efficient firms by reducing asymmetric information, fostering better informational flow between the private sector and the government.² On the other hand, political connections may simply allow firms to receive contracts despite being inefficient. That is, connections may incentivize rent-seeking behavior that could have long-lasting negative consequences on welfare (Shleifer and Vishny, 2002), adding to the adverse effects on contract performance documented in the literature.³ As a result, the net effect of political connections on welfare is theoretically ambiguous, depending on which force dominates.

This paper examines the welfare effects of political connections in public procurement, a sector accounting for 12% of the global GDP (Bosio et al., 2022). We propose a flexible framework to measure the welfare implications of political connections, specifically in terms of *costs of production per utility unit* for the final consumer, which arise from assigning contracts to connected firms instead of non-connected ones. We show that the gap in costs between any two comparison groups (e.g., connected and non-connected firms) is proportional to the gaps in *revenue* productivity (efficiency in generating revenue from given inputs) and the capital intensity of the firms. Reflecting the theoretical ambiguity, our approach accommodates potential positive, neutral, or negative welfare effects. Furthermore, our framework diverges from traditional analyses that focus on allocative efficiency relative to a first-best output scenario (e.g., as in Hsieh and Klenow, 2009), by evaluating allocations between two arbitrary groups, both of which may exhibit misallocation.⁴

¹See, for example, Fisman (2001), Khwaja and Mian (2005), Johnson and Mitton (2003), Fan et al. (2007), Amore and Bennedsen (2013), Cingano and Pinotti (2013), Rijkers et al. (2017), Acemoglu et al. (2016), Baltrunaite et al. (2020), and Nian and Wang (2023).

²Efficiency increasing effects of (social) connections have been documented in the financial sector (Braggion, 2011; Engelberg et al., 2012).

³In other contexts, e.g., land transactions (Nian and Wang, 2023) or credit (Moon and Schoenherr, 2022), connections have been shown to generate inefficiencies.

⁴An additional application of our framework is to assess the efficiency impacts of awarding contracts to small versus large firms, echoing preferential policies enacted in public procurement across numerous countries.

We apply our methodology to examine public procurement misallocation in Ecuador by combining several administrative databases from 2007 to 2017. Our dataset integrates detailed micro-level data on procurement contracts, firms' balance sheet statements, and firms' political connections. To overcome the challenge of identifying firm-level political connections, we use data that encompasses the entire population of private business shareholders and bureaucratic employees. Specifically, we consider the political connections of private firms through the *ownership channel*, defining a firm as politically connected if any of its shareholders or their siblings start working for the government as bureaucrats. Our main empirical contribution is then measuring the welfare consequences of these political connections in public procurement. Our findings indicate that political connections negatively impact welfare, suggesting that the forces for rent-seeking incentives may outweigh informational gains.

We begin our analysis by providing evidence that political connections play a significant role in the allocation of government contracts. By exploiting the time dimension of the data, we implement the event-study methodology proposed by Callaway and Sant'Anna (2021) to estimate the dynamic effects of political connections on contract allocation. In the extensive margin, we find that when firms establish their first political connection, they benefit from a 2.6 percentage point increase in the probability of being awarded a contract in a given year (from a 20% basis), with an effect that is sustained for several years.⁵ In the intensive margin, we find the total volume of contracts increases by 35%. These effects are robust to various methodologies recently proposed in the eventstudy literature (Sun and Abraham, 2021; de Chaisemartin and D'Haultfoeuille, 2020).

Furthermore, these effects are also robust to focusing the analysis on the set of connections that are coming from large government reshuffles or indirectly through a sibling, and therefore, less likely to be subject to anticipatory behavior by the firm. The reallocative effects are concentrated in discretionary contracts and auctions (which can be manipulated by restricting the number of participants), rather than in contracts allocated through a lottery system. These results are consistent both with contract manipulation ex-ante (e.g., the public official screens or preselects competing firms) and with information (e.g., the firm is now aware of the existence of the contracts), but inconsistent with ex-post rule breaking (i.e., the allocation system is rigged in favor of the politically connected firms). Interestingly, we observe that the effects of political connections are more pronounced in industries characterized by a higher degree of product heterogeneity, such as non-tradable goods and services, as opposed to the wholesale and retail trade sectors. Additionally, we find that the reallocation of contracts is concentrated in competitive

⁵This supports recent empirical evidence from several countries. See, for example, the recent study by Goldman et al. (2013) in the context of the US, the paper by Schoenherr (2019) for Korea, and the one by Baltrunaite (2020) for Lithuania.

industries, as indicated by their Herfindahl-Hirschman Index (HHI). These sectors represent areas where political connections may offer significant advantages, but they are also sectors where allocative inefficiencies could be more substantial if connected firms are inefficient or of lower quality.

We study whether political connections have effects on government prices using a subset of contracts with prices for standardized goods and services. We find that politically connected firms charge higher unit prices *only after* the connection becomes active. Furthermore, we investigate whether firms experience additional benefits beyond increased demand and prices as a result of their political connections. We find positive yet statistically insignificant outcomes in terms of spillover effects on the private market, and precisely estimated zero effects on profitability (profits over sales), changes in aggregate markups, revenue productivity, and the revenue-assets ratio. These results indicate that political connections act primarily as government revenue shocks, resulting both from increased quantities and higher prices in the government market.

As a reduced-form test of the efficiency of reallocation of contracts *within* the set of connected firms, we explore the variance in contract allocation across these firms before and after the connection. In the spirit of Rajan et al. (2015), if the government is using improved informational flow to distinguish between connected contractors, one would expect the dispersion of contracts to increase after the connection. Instead, we find that, across multiple measures of contract allocation, dispersion remains unchanged. If anything, for a measure of dispersion in the total volume of contracts by firm, we find that the dispersion *decreases* significantly for firms with active connections. Hence, it does not appear to be the case that the government is improving allocative efficiency, at least within the set of firms for which they possess better information. Moreover, this is also indicative that all connected firms benefit from the connections, and most reallocation of contracts is from non-connected to connected firms.

At face value, these results suggest a reallocation of contracts but do not address the aggregate efficiency implications of shifting contracts from non-connected to connected firms. It is possible that appointing bureaucrats aims to reduce informational asymmetries, leveraging their sector-specific expertise. Consequently, while the efficiency among the set of connected contractors may not change, the overall efficiency in procurement could improve by allocating contracts to connected firms that offer higher levels of quality and efficiency. Thus, despite the additional rents accrued by connected firms due to higher prices and a larger volume of contracts, these rents could represent transfers that result in efficiency gains. Conversely, such appointments might foster rent-seeking opportunities, where bureaucrats divert resources to their relatives, even if they are not the most suitable contractors for the task.

To deal with this ambiguity, we introduce a flexible theoretical framework to recover the (quality-efficiency related) average welfare effect to society of procuring from a politically connected firm, as opposed to a non-connected firm. The framework relaxes several assumptions criticized in the literature (Haltiwanger et al., 2018) by allowing for unobserved quality heterogeneity, productivity differences, and non-constant cost functions.

Starting from the firm's cost minimization problem and constant elasticity of substitution (CES) preferences of the final consumer, we show that the quality-adjusted efficiency gains or losses—costs of production per utility unit—are proportional to differences in *revenue* productivity and capital intensity between the two types of firms. Intuitively, revenue productivity captures both quantity productivity (how much each firm needs to spend in resources to achieve a certain level of output) and quality differences (how much utility each unit generates), while the capital intensity of the firm indicates the location of the firm's output on a non-constant marginal cost function. Thus, accounting for the curvature of the marginal cost function, the comparison of revenue productivities of the two sets of firms is indicative of the number of resources that will be used to achieve the same level of utility, and, therefore, indicative of the welfare effects of the allocation of contracts. We recover the required parameters through a simple modification of standard production function estimation tools, where firms produce for both the private and government sectors.

In our main specification, we allow for politically connected firms to charge an additional premium to the government, in line with our findings and the previous empirical literature showing that connected firms charge higher unit prices to the government (Szucs, 2023; Baranek and Titl, 2020). The counterfactual exercise studies the welfare effects of procuring from the average politically connected firm relative to the average non-connected one in a given 2-digit industry. Our results imply that politically connected firms are, on average, less revenue efficient than non-connected contractors. This efficiency gap translates into quality-adjusted excess costs of provision of 3.8%, which map into welfare costs of 3% of the procurement budget allocated to politically connected firms. The interpretation of this estimate is that the government could keep the utility of the final consumer obtained through government goods fixed and make a transfer of 3 cents per every dollar spent to the final consumer if the contract were allocated to a non-connected firm instead of a politically connected firm.

The estimated effects are robust across various specifications that address different potential biases. The results persist when estimating the production functions using only observations prior to the establishment of political connections. This approach not only addresses the possible endogeneity of input intensity and political connection status but also indicates that political connections do not stem from anticipated efficiency gains; otherwise, positive efficiency effects would be expected. Furthermore, the welfare effects are not contingent upon the assumption of a political connection price premium. The robustness of the estimates to direct imputation of these premiums (rather than adjusting for them in the estimation process), or even after requiring that prices offered to the government be uniform across all firm types in a specific sector, reinforces this point. Additionally, the results are not driven by the cost curvature assumption, as negative welfare effects persist even when forcing constant marginal costs by treating capital as a flexible input. Throughout the robustness checks, we identify welfare losses of up to 6% of the government budget allocated to politically connected firms.

To address concerns regarding the definition of comparison groups, we undertake several welfare calculations, restricting our analysis to firms within the same province, asset quartiles, and levels of government demand (specialization). In every scenario, we continue to observe significant welfare losses attributable to connections. Moreover, employing the most stringent tests, we derive welfare estimates after adjusting for contract-level characteristics such as contract type, province, or agency, or by limiting the comparison to firms vying for the same contract, utilizing contract fixed effects. Through contractlevel comparisons, we identify losses ranging between 5 to 6% of the government budget.⁶ Examining the influence of the connection's nature on the outcome, welfare losses persist even when considering plausibly incidental connections, such as those resulting from large office reshuffles or indirectly by connections to siblings. These findings imply that it is not merely a subset of inefficient firms seeking to establish connections; rather, given the chance, firms will leverage these connections for private gain at society's expense.

Finally, we examine the heterogeneity of effects across sectors in the economy that differ in their level of standardization and find results consistent with our expectations: the wholesaling and retailing sectors exhibit small or negligible efficiency losses, whereas sectors such as engineering, telecommunications, and consultancy services show large and significant welfare losses. We also observe that the effects persist in competitive industries, as measured by their HHI. However, when restricting our analysis to firms participating exclusively in more competitive contracts, like auctions and lotteries, the misallocation effects are smaller and not significant. Yet, for firms involved in discretionary contracts or a mix of contract types, the effects are large and significant. Overall, our findings suggest that political connections result in rent transfer at the expense of efficiency rather than resolving informational asymmetries.

⁶As previously discussed, our estimator enables the comparison of any arbitrary groups, for instance, non-connected winners to non-connected losers. Such a comparison at the contract level reveals that procuring from winning firms results, on average, in efficiency gains of approximately 2 cents per dollar spent. These findings underscore our framework's utility for policymakers in evaluating the efficiency of various procurement methods, whether currently employed or under consideration for future implementation.

Our analysis has important limitations. First, despite our efforts to narrowly define counterfactual allocations by examining sectoral competitors within the same contract or restricting our focus to firms in the same province, our data does not allow us to further obtain quality-efficiency estimates for each product a firm sells. Second, our measure of welfare effects reflects expected gains or losses, given efficiencies primarily estimated in the private sector. If connections help improve ex-post performance relative to the private sector by reducing moral hazard through lower renegotiation rates, delays, and cost overruns, our estimates would serve as an upper bound. Conversely, if, in line with previous evidence in the US (Brogaard et al., 2021), India (Ryan, 2020), and Korea (Schoenherr, 2019), connections exacerbate those issues, then our estimates would serve as a lower bound. Third, as we lack information on quantities, we are unable to disaggregate the welfare losses into components of productivity inefficiency and lower quality. Consequently, our analysis provides an aggregate measure that encompasses both aspects. Nevertheless, we conduct a limited validation exercise using audit data concerning the quality of infrastructure in government schools. This exercise reveals that firms with political connections exhibit poorer performance.

This paper contributes to several strands of literature. First, it addresses the literature examining the relationship between public procurement and political connections. Recent empirical studies have demonstrated that politically connected firms secure more contracts than their non-connected counterparts (Goldman et al., 2013; Tahoun, 2014; Do et al., 2015). However, these connected firms tend to execute contracts with greater delays and at higher costs (Schoenherr, 2019), achieve more favorable renegotiation terms (Brogaard et al., 2021; Ryan, 2020), charge higher prices (Szucs, 2023; Baranek and Titl, 2020), exhibit lower efficiency (Szucs, 2023), and suffer from declines in sales following anti-corruption crackdowns on public spending (Colonnelli and Prem, 2022). Our findings augment these insights by verifying that politically connected firms are awarded more procurement contracts, are less productive, and charge higher prices than non-connected firms in a novel setting within the developing world. Furthermore, our paper zeroes in on ownership as a channel for connections, a relatively unexplored aspect.⁷

⁷More broadly, our paper enriches the literature establishing a positive correlation between political connections and firm performance. This link has been recently documented across various developed and developing nations such as the US (Acemoglu et al., 2016), Italy (Cingano and Pinotti, 2013; Baltrunaite et al., 2020), Tunisia (Rijkers et al., 2017), Denmark (Amore and Bennedsen, 2013), China (Fan et al., 2007), Malaysia (Johnson and Mitton, 2003), Indonesia (Fisman, 2001), and Pakistan (Khwaja and Mian, 2005). The paper most closely related to ours is Baltrunaite et al. (2020). Alongside them, we introduce two innovations compared to previous work by focusing on private firms, which are more prevalent in the developing world, and by identifying a firm as connected through ownership information. Two additional studies also classify firms as politically connected via ownership and concentrate on private firms, albeit with a smaller sample size than ours. Rijkers et al. (2017) defines a firm as connected if owned by President Ben Ali or his family, resulting in a sample of 220 firms. Fisman (2001) identifies 14 firms owned by President Suharto's family. In contrast, our study tracks 6,030 politically connected

Our main contribution relates to the literature on the welfare consequences of political connections and corruption. Our paper adds to this literature by providing empirical estimates of the sign and magnitude of the welfare effects of political connections in the context of public procurement. To the best of our knowledge, only Schoenherr (2019), Baranek and Titl (2020), and Szucs (2023) investigate the allocative efficiency of procurement contracts. Schoenherr (2019) approach this by quantifying the social costs of delays and estimating the additional government expenditures due to ex-post cost increases caused by political connections. In contemporaneous work, Baranek and Titl (2020) quantifies the total transfers from the government to connected firms due to overpricing. Conversely, Szucs (2023) examines the welfare effects of different entry thresholds into high-discretion procurement procedures on production and administrative costs. In contrast, our paper develops a framework that assesses the social losses in terms of inefficient use of production inputs resulting from awarding contracts to less quality-efficient firms.⁸

This paper is closely aligned with existing literature that examines the impact of political connections on bureaucracy (e.g., Xu, 2018; Colonnelli et al., 2020; Riaño, 2021; Callen et al., 2023). Xu (2018) and Colonnelli et al. (2020) demonstrate the influence of political connections on bureaucratic recruitment, revealing negative consequences for the performance, competence, and attendance of appointed officials (Callen et al., 2023). Our contribution lies in uncovering the association between connections to the bureaucracy and detrimental quality-adjusted efficiency in government contracts. On the other hand, Riaño (2021) shows the pervasiveness of family connections in bureaucracy, and how favoritism in salary and promotions might help explain selection into bureaucracy. Similar to Riaño (2021), we emphasize the significance of family connections within the bureaucracy, deviating from the traditional emphasis on elected politicians. Our descriptive evidence reveals that individuals in appointed and career positions, rather than elected officials, are the primary recipients of procurement contracts. Furthermore, we introduce procurement contracts as an additional incentive that contributes to the understanding of selection into the public sector.

Finally, our paper is also related to the literature that studies misallocation, pioneered by Restuccia and Rogerson (2008) and Hsieh and Klenow (2009). Several papers have

government contractors.

⁸As Cingano and Pinotti (2013) lack direct measurements of who wins a public procurement contract, they estimate the allocative effects of political connections in the aggregate, relative to a fully efficient scenario, with estimates ranging between 0 and 120%, depending on the calibration parameter. Our framework, however, directly estimates the misallocation (without the need for calibration), with precisely estimated excess costs. Moreover, our framework benchmarks against a (plausibly) inefficient scenario (those in which non-connected contractors win the contract), and explores heterogeneity effects by detailed contractor, location, and contract characteristics.

applied and extended their framework to quantify aggregate productivity losses stemming from misallocation (see, for instance, Blattner et al., 2019; Rotemberg, 2019; Baqaee and Farhi, 2020). Within this literature, the closest papers to ours are Asker et al. (2019) and Boehm and Oberfield (2020). Asker et al. (2019) studies misallocation in the oil production cartel by measuring the gap in cost functions from heterogeneous producers. Boehm and Oberfield (2020) contributes instead to the misallocation literature by studying suboptimal input usage due to weak legal enforcement and exploiting first moments rather than the dispersion in productivities to identify misallocation. Relative to these papers, we show that the average differences in revenue productivity and capitalrevenue share are a sufficient statistic for the difference in production costs per utility unit.

Moreover, our paper differs from Hsieh and Klenow (2009), both methodologically and in focus. The focus of Hsieh and Klenow (2009) is to understand how resources are allocated relative to a frictionless world, whereas we are concerned about the efficiency effects of a specific counterfactual—allocating contracts from politically connected firms to non-connected ones, which may or may not be more efficient. Notice that although we focus on political connections, our framework could be adapted to evaluate the excess cost across firms generated, for example, by other government interventions, such as preferential rules in procurement contracts. Furthermore, our approach relaxes several of their assumptions by allowing for non-constant marginal cost functions and firms to be heterogeneous in quality, addressing concerns raised by Haltiwanger et al. (2018).

The remainder of the paper is organized as follows. Section 2 details the data and main definitions of the paper. Section 3 shows reduced-form evidence of the reallocation of procurement contracts in the presence of political connections. Section 4 develops the model and empirical framework to estimate the welfare losses from political connections. The main results of the welfare analysis are reported in Section 5. Section 6 concludes the paper.

2 Data and Definitions

Our framework for estimating the welfare effects of political connections in public procurement relies on several administrative databases that allow us to i) measure firm-level political connections and allocation of government contracts over time, and ii) obtain firm-level time-varying estimates of revenue productivity measures and capital share of revenue. In this section, we present a detailed description of the data sources used, provide our working definition of a political connection, and offer descriptive statistics of the assembled data.

2.1 Data

2.1.1 Bureaucrats

In Ecuador, all elected or appointed public sector workers are required by law to submit a sworn statement of net worth each time they have a new appointment. This regulation became effective in 2003 for high-ranking positions and was extended to all civil servants in 2008. For each public official, the webpage of the *Contraloría General del Estado del Ecuador* (Comptroller General) makes publicly available information regarding national ID, full name, the agency where the bureaucrat works, starting year, and position held. We scraped this data for all years up to 2018.⁹ For our analysis, we exclude individuals with non-administrative jobs in schools, hospitals, and military institutions.¹⁰

2.1.2 Firms Ownership

We use a database collected by the *Superintendencia de Compañias* (Business Bureau) that tracks any change to the ownership composition of Ecuadorian private companies. The data starts in 2000, and we scraped it for each year up to 2017. Shares can be owned by natural persons or by legal entities, following a pyramidal structure. For shares directly owned by individuals, the records show each owner's national ID, full name, and their respective share in the firm. When another firm owns shares, we walk up the chain of control until we identify the ultimate beneficiaries at the top of the pyramid.¹¹ In combination with the bureaucratic database, we can track firm-level political connections through the ownership channel.

2.1.3 Government Purchases

Starting in 2008, the Ecuadorian government issued new regulations to centralize and modernize the public procurement system. Among these changes, the government created a new web portal with the intent of facilitating the interaction between local agencies and contractors.¹² Agencies use the platform to post calls for tenders and registered suppliers

 $^{^{9}}$ Even if records report a start date as early as 1970, the coverage of the data becomes representative of the public labor force in the early 2000s.

¹⁰Although the data allows us to identify any subsequent inter- or intra-agency moves, it does not keep track of whether an individual stops working for the government. Therefore, it cannot be used to study the effects of *exit* from bureaucracy, and in our data, political connections are considered fully persistent.

¹¹The dataset does not retain information on the individuals or companies investing in mutual funds. Therefore, we cannot establish a complete ownership structure for businesses owned by mutual funds. However, on the aggregate, total shares owned by national firms that cannot be traced to final local ownership amount to 1% of the firms in the data.

¹²The portal is administered by the *Superintendencia de Compras Públicas* (Public Procurement Bureau) and can be accessed at https://www.compraspublicas.gob.ec/ProcesoContratacion/compras/

use it to submit their bids.¹³

We scraped all webpages available on the public procurement portal during the summer of 2018 and constructed a dataset containing virtually every contract issued by government agencies between 2009 and 2018. For each contract, the data contains a description of the contract, starting date, initial budget, agreed value, length of the contract, type of contract, and the number of firms presenting bids. A large fraction of the contracts in the data is of very small value. Therefore, to keep a relevant and comparable sample, we drop contracts of value below the 1st and above the 99th percentile of the contract value distribution. We further exclude contracts that were either deserted, unilaterally terminated, or terminated by mutual agreement.

The exact procedure used to award a contract depends on the type and value of the goods or services provided. Normalized goods and services are procured through reverse auctions, in which the winner is selected based on the lowest price offered. Instead, non-normalized products are procured through scoring auctions. The exact scoring function depends on the value of the contract and takes into account the price offered as well as other more subjective elements. For relatively small purchases, there exists the option to contract directly without an auction or any other contest. Finally, public works of relatively small value are organized through a process denoted *menor cuantia* (lower value), where the winner is randomly selected through a lottery among pre-qualified contenders. For the analysis, we classify the contracts into three categories –auctions, discretionary, and random– depending on the degree of discretion of the allocation process.

In the Internet Appendix, we use the information for a set of standardized goods and services procured through an electronic catalog similar to the one studied in Bandiera et al. (2009). The electronic catalog allows an institution to purchase goods and services from a pre-specified list of providers, where each provider is free to choose the price at which they want to sell. For this data, we observe quantities and prices at a ten-digit product-level, so that we can infer unit prices very granularly. The products' classification allows us to distinguish, for example, between pencils with erasers and without erasers, or between different computer specifications. The data from the electronic catalog covers the period 2014-2018.

2.1.4 Balance Sheets and Income Statements

We use balance sheets and income statements covering the universe of formal private firms in Ecuador for the period 2007-2017. The data is collected by the Business Bureau

PC/buscarProceso.cpe?sg=1#.

¹³Registration requires only some basic information, which includes the type of company, economic sector, and products it can provide down to 10 digits of detail.

and it contains information on firms' annual revenues, input expenditures (e.g., wages, physical capital, energy consumption), assets, and debt. We also observe each firm's main economic activity at the 6-digit ISIC sector level and a unique firm identifier. We use this data to estimate the revenue productivity and capital-revenue shares of government contractors.

2.1.5 Linking Sources Together

We match the balance sheet and business ownership information using unique firm identifiers, which are assigned for tax purposes when a company is established. Similarly, to link the balance sheet data to the public procurement data, we use the firm IDs and their legal names.¹⁴

We use the individuals' IDs to match the bureaucrats and ownership datasets. The resulting matches identify owners who also work for the government. We additionally consider links between individuals and their siblings. These matches are obtained as follows. First, we construct "families" using the two last names of each individual recorded in our data.¹⁵ People sharing both last names are then assumed to be siblings. We only consider families of size less than or equal to 4 (corresponding to the 75th percentile of the family size distribution in our data). We impose this restriction to reduce the risk of false-positive indirect connections, which arise when unrelated individuals are erroneously classified as siblings.¹⁶ As shown in Internet Appendix Figure IA1, the family size distribution we obtain is similar to the family size distribution observed in census data.

2.2 Key Definitions

2.2.1 Government Contractors

Although we have balance sheet and ownership information for the universe of private firms in Ecuador, we focus our analysis on government contractors. We classify a firm as a contractor if we observe it at least once in the procurement dataset, so our final sample also includes firms that participated in a tender without winning it. As we need balance

¹⁴The use of the companies' legal names in our matching algorithm aims to limit the number of incorrect matches that could arise in case of reporting mistakes in the firm IDs between different data sources.

 $^{^{15}\}mathrm{In}$ Ecuador, individual identities are recorded with two last names. The first is the paternal last name and the second is the maternal last name.

 $^{^{16}}$ In results not reported, we use a family size threshold of 7 (approximately the 90th percentile of the family size distribution in our data) and obtain comparable results. Furthermore, in some falsification exercises, we use the set of families classified as having more than 15 siblings, as these are unlikely to be real connections.

sheet information to quantify excess costs, we exclude from the analysis (except where explicitly indicated) government contractors that operate as individuals and not as firms. While excluding individual contractors is restrictive in a developing country setting, our study still concentrates on 31 percent of all contracts, accounting for 45 percent of all dollars spent by the government in procurement contracts.

2.2.2 Political Connections

For our analysis, we consider two types of political connections: direct and indirect. We define a *direct* connection for a firm if any of its owners work as a public official. Instead, we classify a connection as *indirect* when one of the siblings of a shareholder holds a bureaucratic position. For both direct and indirect connections, we consider only owners controlling at least 20% of the firm's shares at some point in time. We choose this threshold as it is commonly used by government authorities as a rule of thumb to assess whether an owner exerts significant control over a firm.¹⁷

Since owners may sell their shares of a company to hide their political links, our definition of a political connection considers both current and past owners. However, we exclude two groups of connected firms from our analysis. The first group consists of businesses whose shares are bought by individuals already working as public officials (we refer to these connections as "strategic entry" connections). We drop these firms, as the decision to buy shares of a firm may be influenced by unobservables, such as growth opportunities, that could bias our analysis. Second, we exclude firms created by bureaucrats (or their siblings), since they mechanically lack a baseline period before the connection occurs. We additionally exclude observations for the years in which we do not have balance sheet information for a firm. This restriction is intended to create a uniform sample across all parts of the analysis.¹⁸

2.3 Descriptive Statistics

In this section, we present summary statistics for the data used in our analysis. Table 1 gives information on the average number of connections observed in the data. For our main analysis, we use data from 29,027 firms that are government contractors, of which 6,030 firms (around 21% of all contractors) are politically connected at some point in our data.¹⁹ Of the politically connected firms, 46% of connections are exclusively direct, 23%

¹⁷See, for example, European Commission (2015), section 4.4.

¹⁸In particular, the analysis of the excess costs of provision relies on production function estimation and thus on the availability of balance sheet data.

 $^{^{19}{\}rm The}$ 6,030 connected firms exclude 1,384 firms that are strategically connected and 509 firms that are created by bureaucrats.

are indirect connections, and the remaining firms are connected through the two margins. On average, each firm has about 1.6 connections.

In Figure 1, Panel (a), we present the top 20 bureaucratic positions in our data in terms of the aggregate value of contracts won by the firms connected to each position. The most valuable position is *Director*, which is a high-rank position. However, the data also includes links through low-rank positions as, for example, the second and third most valuable positions are *Analyst* and *Public servant 1-4*, which are low-ranked bureaucrats.²⁰ Notice that the large majority of top positions, such as Director, Adviser, Managers, are appointed bureaucrats. Other top positions, such as Public Servants and Judges are accessed through public contests. Finally, a limited number of positions, such as Local Council Member, are elected positions. In Figure 1, Panel (b), we present the top 20 positions in terms of the average amount awarded per individual in such a position. To reduce noise, we consider positions with at least 5 different individuals. In terms of average value, one can observe a significant presence of high-rank officials, such as Attorney, Governor, Minister, Vice Minister, Local Council Member, Notary, and even Public Defender. Again, except for Local Council Members, we see many appointed or career bureaucrats.

Table 2 provides summary statistics for 2015 for the firms included in the data. Panel A allows a comparison between all private firms (Column (1)) and the sample of contractors (Column (2)). Firms classified as contractors are, on average, larger in terms of revenue, capital, wages, inputs, and debt. In Panel B, we decompose the set of contractors between connected and non-connected firms. Politically connected firms, which account for about 31% of the government contractors, are considerably smaller than non-connected ones. This is also true for the set of connected firms used in our main analysis, shown in Column (5), which excludes firms acquired or created by a bureaucrat and firms with connections established before 2000.²¹ The remainder of the table (Panel C) shows that connected firms that establish direct, indirect, or both types of political connections are similar to each other. Note that cross-sectional differences between connected and non-connected firms do not pose a challenge to our identifying assumptions. In fact, our analysis of the effect of political connections exploits variations in the timing of connections, while the welfare analysis explicitly accounts for differences between the two types of firms.

²⁰We keep *Professors* as part of the bureaucratic force as anecdotal evidence suggests that they can affect the allocation of public funds. Moreover, public universities have large expenses of about US \$ 1 billion per year. See, for example, the report by the expenditure watchdog *Observatorio de Gasto Público* (https://www.gastopublico.org/informes-del-observatorio/el-presupuesto-de-las-universidades-dinero-bien-gastado).

²¹For 2015, around 20% of contractors have an active political connection according to our classification method.

Table 3 presents statistics for all government contracts issued between 2009 and 2017. Most of the contracts are allocated using auctions, which account for over 45% of the contracts. A typical auction has a value of about US \$49,000. These contracts are relatively competitive, as they have, on average, 2.2 firms bidding for the same contract. Note, however, that in practice 45% of the auctions have only one competitor. Publications are the second most common contract type, with almost 65,000 contracts. These contracts are about one-third the size of auctions and are used for "special" circumstances so that the issuing agency has complete discretion in selecting the winning firm. The table also presents statistics for other contracts are awarded using a scoring auction. Instead, contracts issued through direct contracting are allocated without a contest, as well as the vast majority of lower-value contracts for goods and services. The remaining category – lower value contracts entailing public works – is randomly allocated to firms through a lottery.

3 Motivating Evidence: Reallocation of Contracts

In this section, we provide evidence that owners of private firms can use their political connections to increase the allocation of government procurement contracts, either by exerting influence or by reducing informational asymmetries. This finding motivates our subsequent analysis of the welfare effects on society when politically connected firms win contracts.

3.1 Methodology

To identify the role played by political connections in the allocation of government contracts, we exploit the yearly variation in the number and size of contracts awarded to firms and their political connection status in an event-study design. Although firms can establish links with multiple bureaucrats, for simplicity, we focus our analysis on the first connection, i.e., the event is defined as the first appointment of one of the owners of a firm (or one of their siblings) as a public worker.

Following the recommendations of the recent literature in event-studies and dynamic difference-in-differences (e.g., Rambachan and Roth, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Borusyak and Jaravel, 2017), we do not implement the two-way fixed-effect (TWFE) dynamic regression as our main specification. Instead, as suggested by Rambachan and Roth (2020), we rely on the methodology of Callaway and Sant'Anna (2021) and implement other specifications as sensitivity analysis.

We consider the following framework, proposed by Callaway and Sant'Anna (2021). We observe data for calendar periods T, t = 1, 2, ..., T. A firm's treatment cohort is denoted by $G_i \subset \{2, ..., T, \infty\}$, where G_i is the first year firm gains a political connection.²² Note that a firm may never gain a political connection, which we denote as $G_i = \infty$. Let $Y_{i,t}(g)$ be the potential outcome that firm *i* would experience at time *t* if they first become treated at time *g*. Moreover, let $Y_{i,t}(0)$ be firm *i*'s untreated potential outcome at time *t* if they were to remain untreated through all the time periods.

Following Callaway and Sant'Anna (2021), we define the group-time average effect of treatment for members of treatment cohort g at a particular time t as :

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_i = g],$$
(1)

which captures the average treatment effect for the group. As highlighted by Callaway and Sant'Anna (2021), this parameter does not restrict treatment effect heterogeneity across cohorts or time.

We map this group-time average effect into the standard framework of event-studies, concentrating on the dynamic effects of exposure to treatment over time. Such formulation can be obtained through the following aggregation. First, let e denote the event-time relative to treatment, i.e., e = t - g, which tracks the number of years since the firm first obtained its political connection. Moreover, recall that G captures the time period in which cohorts first gain their political connection. Then, the treatment effect heterogeneity in e is given by:

$$\beta(e) = \sum_{g \in G} 1\{g + e \leqslant T\} P(G = g | G + e \leqslant T) ATT(g, g + e),$$

$$\tag{2}$$

where $P(G = g|G + e \leq T)$ captures the size of the group, i.e., the unconditional probability of treatment in year G = g, and $\beta(e)$ has the same interpretation as the dynamic treatment effects in TWFE regressions. This parameter is the average treatment effect e periods after a political connection is gained across all cohorts that ever obtain a connection. As in traditional event-studies, the instantaneous average effect of political connection occurs at e = 0, while the dynamic exposure effects occur at e > 0. Pre-trends will be then captured by $e < 0.23^{-24}$

²²The first treatment period considered is $G_i = 2$, to allow for pre-treatment observations to occur in all calendar periods.

²³For this representation to capture the causal treatment effect, the main two identifying assumptions are: 1) no treatment anticipation, and 2) unconditional parallel trends on the not treated groups.

²⁴In practice, we implement this event-study approach using the Stata package *staggered* by Jonathan Roth and Pedro H.C. Sant'Anna.

3.2 Results

3.2.1 Reallocation of contracts

Subfigure 2a shows the evolution in the yearly probability of being awarded a government contract for politically connected firms before and after the first connection is established. The plot reports coefficients from the event-study parameters in equation 2. The probability of winning a contract in a given year increases by 2 to 3 percentage points after establishing a connection, from a baseline average probability of about 20%, with an effect that is sustained even 4 years after the treatment date. The overall path remains consistent when examining the intensive margin, with the yearly value of procurement contracts won (subfigure 2b) showing effects of 35%, and the total number of contracts awarded (subfigure 2c) demonstrating effects of 9%.²⁵ Note that although in all three figures, there is a non-significant (at the 95% level) decrease two years before the connection, the overall path from four years prior to treatment is relatively stable, and the figures show a clear break after the connection is gained across all measures.²⁶ Overall, we take these results as motivation that political connection may generate efficiency gains or losses due to the reallocation of contracts.

In Internet Appendix Table IA2, we present sensitivity analyses of the post-treatment average treatment using various methodologies, which hold under different parallel trend assumptions and different control groups.²⁷ In Column (1), we present the post-treatment estimate using Callaway and Sant'Anna (2021), which uses the group of never-treated as control. In Column (2), we again implement Callaway and Sant'Anna (2021) but rely on never-treated and not-yet-treated as control. In Column (3), we implement Sun

 27 Note that we do not condition for time-varying covariates, so we rely on the corresponding unconditional parallel trend assumption in each methodology.

 $^{^{25}}$ These effects are also present when looking at the raw data. Internet Appendix Figure IA2 shows the distribution of contracts value by type of connection. Connected firms prior to the connection win less total value of contracts than non-connected (23% lower average). Yet, after the connection is established, they win 27% more than non-connected firms.

²⁶One potential explanation for the observed dip is the selection effect associated with political connections. The Internet Appendix Figure IA3 presents event studies for three measures of procurement contracts, distinguishing between connections stemming from large reshuffles (defined as instances where at least 50% of the staff in offices with ten or more employees are changed) and those not resulting from large reshuffles. Notably, large reshuffles, which are likely timed more exogenously, do not exhibit a dip prior to the treatment in any of the figures. In contrast, event studies associated with political connections from other types of appointments exhibit the dip two years prior to the establishment of the connection. Furthermore, an analysis of variables related to firm dynamics (for example, private sector revenue, firm profitability, or revenue productivity), as depicted in Internet Appendix Figure IA4, reveals no significant declines for any firm-specific variables at the "-2" period preceding the establishment of political connections. Overall, these patterns suggest that there may be some degree of firm selection into a connection. Given the possible concerns regarding selection effects, we also estimate the welfare costs of connections associated only with large reshuffle events and find similar results to those using the full sample.

and Abraham (2021), which uses the last-to-be-treated (cohort 2017) and never-treated as control. In Column (4), we use de Chaisemartin and D'Haultfoeuille (2020), which relies not-yet-treated as controls. In Column (5) we present the usual TWFE estimate. All different methodologies produce similar point estimates of around 2 to 3 percentage points for the extensive margin, around 25 to 35% for the value of contracts, and around 6 to 9% for the number of contracts.

3.2.2 Reduced-form test of reallocation efficiency within connected contractors

While the average connected firm sees an increase in the likelihood, amount, and value of contracts won, this reallocation may be efficient if the government is using a better informational flow to distinguish between competent and subpar contractors. In our main empirical exercises in the next sections, we will outline a method to test the efficiency of reallocation from non-connected to connected contractors. However, it is important to acknowledge that the reduction in informational frictions can also lead to a reallocation of contracts *within* the group of connected contractors. Within this group of connected firms, the government may allocate a greater number of contracts to high-performing contractors while reducing the allocation to underperforming ones. This scenario would result in a notable increase in the dispersion of contract allocations among connected contractors when the connection becomes active, akin to the effects observed in Rajan et al. (2015) with improved information.

We explore this intuition in Table 4. For different measures of contract allocation covering both intensive and extensive margins, we calculate the dispersion in standard deviations for connected contractors that are yet-to-be-connected and separately for firms with active connections, for each 3-digit sector and year.²⁸ Then, in regressions approach controlling for 3-digit sector fixed effects, we compare the dispersion by connection status by running the sector-year measure of dispersion on a dummy for connection status. Contrary to the expectation that dispersion should increase after a connection is gained, we find that across various definitions of contract allocation, the dispersion in contracts does not change for connected firms with an active connection. If anything, the only statistically and economically significant result suggests that dispersion decreases after connection.

²⁸Measure 1 deals with the intensive margin, conditional on a contract being award. As this measure requires a positive value of contracts per sector-year, the sample size is considerably smaller. Measure 2 deals with the intensive margin but accommodates for an extensive margin as well. Thus, capturing inequality in some firms potentially being completely excluded from procurement while some others concentrating a large volume of contracts. Measure 3 measures the extensive margin alone. Measure 4 captures both extensive and intensive margins in terms of the number of contracts, while Measure 5 relies on the logarithm of the number of contracts to correct for potential outlier effects. The measures in the table are all standardized to facilitate comparison.

All of these results indicate that the government is not utilizing the reduced informational frictions to reallocate contracts more effectively within the group of connected firms. Rather, the reallocation of contracts is mainly from non-connected firms to connected ones. For that reason, in the following pages, we focus on exploring the robustness and heterogeneity of the results of the reallocation of contracts from non-connected to connected contractors.

3.2.3 Heterogeneity by type of contract, location, and sector

To further understand the nature of the reallocation, Table 5 presents heterogeneity of treatment effects by type and location of the contracts. First, in Panel A, we explore the heterogeneity of treatment effect across different contract types —auction, discretionary, and random— which differ in their degree of discretionality. The dependent variable is replaced with the probability of being awarded a contract from one of these categories, without restricting the sample to ever-winners in the respective category.²⁹ Columns (1) and (2) show that the effects of establishing a political connection are milder for auctions (16% increase from a baseline probability of 6.3%) than for discretionary contracts (26% increase from a 13.8% basis). On the other hand, the effect on the set of contracts allocated randomly is precisely estimated at zero (Column (3)).³⁰

Next, in Panel B, we explore whether contract reallocation is concentrated in the same province as the headquarters (HQ) of the firm or elsewhere. Specifically, for a firm with HQ in province p, we study separately as outcome variables the probability that it wins a contract in province p and that it wins in any other province $\tilde{p} \neq p$. While firms are ex-ante slightly more likely to win contracts outside their home province, the effects are economically stronger (30% vs. 15% of the base probability) and individually statistically significant at home, although the difference in home vs. out-of-province estimates is not statistically significant. We interpret these location and contract-type findings to be consistent with both an informational and manipulation story and take no stance on the extent to which each one drives the reallocation results.

In Internet Appendix Table IA3 we explore heterogeneity by the type of sector of the firm. To categorize the sectors, we follow the classification of Caliendo et al. (2018) and divide them into i) Tradables, ii) Wholesale and Retail trade, and iii) Non-tradables, encompassing services and construction. In our analysis, we consider both the extensive margin (probability of winning a contract) and the intensive margin (total value of

²⁹We do not restrict the sample to ever-winners within a category to keep the sample constant across specifications. However, if we restrict the sample to ever-winners within the category, the general findings both in relative magnitude and statistical significance across categories are unaffected. The only difference is that the pre-treatment average and overall size of the treatment effect are larger.

³⁰These results across categories imply statistically significant differences in pair-wise tests.

contracts).

Our findings in Panel A reveal that sectors dealing with highly standardized products, such as Wholesale and Retail trade, exhibit weaker reallocative effects compared to industries involved in the production of more differentiated goods in tradables (e.g., manufacturing) and non-tradable goods and services. The sectors that demonstrate more significant reallocative effects, i.e., tradables and non-tradables, are those for which the costs of misallocation may be larger, as these sectors have greater heterogeneity in quality and efficiency.

Additionally, we categorize sectors as *High Concentration* if their Herfindahl-Hirschman Index (HHI) exceeds the median value for a given year, while those below the median are labeled as *Low Concentration*. In Panel B we observe that the reallocative effects are nonexistent in highly concentrated sectors, whereas they are present in competitive sectors. It is in these competitive sectors where the benefits of political connections for the firms are most pronounced, as connections may provide a competitive edge. One may expect that these sectors would experience greater inefficiencies if the procurement processes deviate significantly from market outcomes.

3.2.4 Unexpected shocks, type of connection, and falsification exercises

In Table 6, we provide additional robustness and falsification exercises for the reduce-form evidence of contract reallocation. In Panel A, we study the robustness of the estimate to the definition of treated units. In Column (1) we focus the analysis on treated units where the treatment is likely to be unexpected, namely, contractors that form bureaucratic links with agencies undergoing large reshuffles in their workforce.³¹ Usually, large reshuffles result from changes in the leadership of an agency, so their timing is more likely to be unanticipated. Second, Column (2) checks if considering only the first political connection of a firm (and not accounting for whether it establishes other connections at later periods) affects the results. We test this by restricting the sample to firms that we observe forming only one political link. Furthermore, recall that our definition of political connection includes both current and past shareholders. Our results may be biased by the fact that some bureaucrats sell their shares after starting to work in the public sector. We drop this set of potentially "strategic" exits in Column (3) and find similar results. We find overall consistent effects of political connections in all these robustness samples.

Instead, in Panel B, we investigate the robustness of the direct and indirect linkages,

 $^{^{31}}$ We say an agency is undergoing a large reshuffle if we observe at least ten bureaucrats working for the agency in a given year, and, at least, 50% of the agency's employees did not work there the previous year. For the analysis, we restrict the group of treated units to connections generated through a large reshuffle. Internet Appendix Figure IA3 plots the event-study figures for large government reshuffles, for the extensive and intensive margins.

that is, through the bureaucrat or the sibling of the bureaucrat. We consider indirect linkages to be more likely to be fortuitous than direct linkages. Still, we find positive and statistically significant effects for firms that are either owned by the sibling of the bureaucrat or by the bureaucrat themselves. Given their initial base probability, the effect size is (weakly) stronger with indirect connections than with direct connections.

Furthermore, in Panel C, we present three falsification exercises. First, Column (6) considers fake treatment years for non-connected firms, where we assign random treatment years to 20% of the non-connected contractors, leaving 80% of the sample as a control group. We further impose that the distribution of fake entry years matches the true distribution. The column shows that non-connected firms do not experience an increase in probability during these fake treatment years. Second, Column (7) considers only connections through families having more than 15 siblings,³² which likely generate a high share of false-positive links. Given that the set of treated firms in this exercise will have a combination of firms with actual links and false positives, we expect the coefficient to converge toward zero relative to the treatment effects documented above. Effectively, the coefficient is smaller and cannot be rejected as different from zero. Lastly, we consider the subsample of low-ranked bureaucrats and select firms in which they own less than 10% of the shares. The intuition underlying this test is that low-ranked bureaucrats should have fewer opportunities to allocate contracts to their firms. Furthermore, if firm shares are a proxy of how profits are redistributed across owners, bureaucrats with small shares should have less incentive to engage in contract reallocation activities. Consistent with our hypothesis, we do not find any evidence of an increase in the probability of winning contracts after the link is established (Column 8).

3.2.5 Sales, profitability, markups, and productivity

Previous studies (Cingano and Pinotti, 2013; Amore and Bennedsen, 2013; Haselmann et al., 2018; Moon and Schoenherr, 2022) have documented benefits in terms of sales and access to capital when firms gain political connections. Following these studies, we explore the effects of political connections on various firm-level outcomes in Internet Appendix Table IA4. We find small and statistically insignificant effects across the following variables: total revenue (Column 1), private revenue (Column 2), wages (Column 4), intermediate inputs (Column 5), capital (Column 6), profitability (profit share) (Column 7), accounting markups,³³ revenue productivity (Column 9),³⁴ and revenue-to-assets ra-

 $^{^{32}}$ This corresponds to the 95^{th} percentile of the family size distribution in our data.

 $^{^{33}}$ We follow the approach of Peters (2020), which argues that the inverse of material or labor share is sufficient to study *changes* in markups due to a policy.

 $^{^{34}}$ We outline our estimation procedure for revenue productivity in section 4.3 below. The revenue productivity estimate used in this regression comes from the fourth specification, which does not control

tio (Column 10). The results are reinforced by event-study plots in Internet Appendix Figure IA4, which show an overall lack of pre-trends or strong post-treatment effects.

Indeed, the bulk of the effect is concentrated in sales to the government, as demonstrated in Column (3) of Internet Appendix Table IA4, with a significant and substantial increase of over 35%, and Figure 2a, Panel (b), plotting the event-study evolution of government contracts. It is worth noting that government sales typically constitute a small share of total revenue in a given year (see Internet Appendix Table IA18), thereby dampening the overall impact on total revenue and other inputs. Thus, the effects of political connections on firms seem to primarily manifest as small demand shocks from government procurement, rather than exerting significant impacts on productivity or overall market power (as measured by changes in aggregate markups). Therefore, the main concern regarding government contracts is about static misallocation across firms.

3.2.6 Prices to the government

As a final piece of motivating evidence that political connections matter for contract allocation, we study the effect of connections on prices using a subset of our data with unit prices for standardized goods and services in Internet Appendix Section J. We find that before a political connection, the transaction prices of equivalent goods from connected firms cannot be statistically distinguished from those of non-connected firms. However, once the connection is established, we find statistically significant differences in prices, with connected contractors charging higher prices (between 3.5% to 6.4%) for the same goods. Thus, contrasting our measure of general markups above, we do see further benefits in terms of overpricing when transacting with the government.

3.2.7 Reduced-form conclusion

All in all, it appears that political connections to bureaucrats generate shifts in public procurement in favor of connected firms, with robust effects concentrated in more discretionary contracts, more competitive sectors, and those offering more differentiated goods and services. It is worth noting that besides the potential efficiency implications, which will be further explored in the subsequent analysis, these connections may directly impact the prices paid by the government for similar goods. Importantly, our results do not indicate a reallocation of contracts towards "good" connected firms as opposed to "bad" connected ones.

or correct for the political connection status of the firm. We believe this estimate is the most conservative in that it attributes all benefits of connections to increases in revenue productivity. Note as well that using any of the other measures for revenue productivity gives similar results.

4 An Empirical Model of Allocative Inefficiencies

In this section, we develop a model to estimate the allocative inefficiencies in public procurement generated by political connections when firms are heterogeneous in quality and productivity, and may face non-constant marginal costs. In the private sector, the final consumer optimally chooses levels of consumption from a mix of varieties based on quality and prices (determined by the firm). Instead, in the public sector, the government allocates contracts, potentially affected by political connections, in ways that may be worse for the consumer than market allocation. The model shows that the extent of misallocation created by political connections boils down to a novel sufficient statistic: the *average* gap in revenue productivity and capital share of revenue between connected and non-connected firms.

Our framework builds from the standard approach in production function estimation (e.g., De Loecker (2011) and De Loecker et al. (2016)). For clarity, we will only highlight the most relevant assumptions and refer the reader to Internet Appendix Section D for evidence and implications of these assumptions.

4.1 A Production Function Framework

Assume firm *i* produces total output Q_{it} , at time *t*, according to a Cobb-Douglas production function

$$Q_{it} = L_{it}^{\alpha_l} M_{it}^{\alpha_m} K_{it}^{\alpha_k} exp(\omega_{it} + u_{it}), \qquad (3)$$

where L_{it} denotes labor, M_{it} intermediate inputs, and K_{it} capital. The output elasticity of input h is α_h . Production also depends on a firm-specific Hicks-neutral productivity shock, ω_{it} , and on u_{it} , which captures measurement error and idiosyncratic production shocks. We assume that the u_{it} term is independent and identically distributed (i.i.d.) across producers and time. Total output is composed by output for the private market and for the government, such that $Q_{it} = Q_{it}^{pri} + Q_{it}^{gov}$.

4.1.1 Private Market

Demand in the private market comes from a representative consumer in each sector, whose preferences are summarized by a constant elasticity of substitution (CES) demand system. We allow firms to have differences in quality as in the quality ladder model of Grossman and Helpman (1991). Each firm *i* produces a variety *i* of differentiated goods in sector *s*, and each variety has heterogeneous quality z_{it} , which may vary over time. The representative agent in sector *s* prefers goods of higher quality, has a taste for variety, and is endowed with income E_{st} for the private market. The representative consumer maximizes utility given by:

$$U_{st}^{pri} = \left(\int_{i \in F_{st}} (exp(z_{it})Q_{it}^{pri})^{(\sigma-1)/\sigma} di\right)^{\sigma/(\sigma-1)},\tag{4}$$

where Q_{it}^{pri} is the private market quantity of good *i* consumed at time *t*, F_{st} is the measure of firms in sector *s*, and $\sigma > 1$ is the sector-specific elasticity of substitution.

Assuming an average private price index P_{st}^{priv} , the representative consumer maximization problem implies that the private demand for firm *i* at time *t* is given by:

$$Q_{it}^{pri} = exp(z_{it})^{\sigma-1} \left(\frac{P_{it}^{priv}}{P_{st}^{priv}}\right)^{-\sigma} \frac{E_{st}}{P_{st}^{priv}},\tag{5}$$

where P_{it}^{priv} is the firm's price. Higher quality implies that the firm obtains higher market shares, conditional on price. The CES demand system and monopolistic competition imply the firm chooses a constant markup over marginal costs at total quantity Q_{it} :

$$P_{it}^{pri} = \frac{\sigma}{\sigma - 1} C'(Q_{it}), \tag{6}$$

for some general cost function $C(\cdot)$.

4.1.2 Government Market

As in Kroft et al. (2020), we model firm-level government output, Q_{it}^{gov} , as exogenously set by the government. The government sets firm-level demand depending on the productivity and quality of the firm, as well as the firm's political connections.³⁵ Furthermore, there is an exogenously random component that captures the complexity of government demand, which depends on multiple elements such as the central budget allocation, or specific institutional needs requiring firms from specific sectors. The deterministic part of government demand is assumed to be determined in the following way.

Assumption 1 – Government Demand: For each sector s, deterministic government demand is increasing in i) political connection status PC_{it} and ii) firm-level quality z_i , and decreasing in iii) marginal costs $C'(Q_{it})$, under the following function $(1 + \tilde{d}_s PC_{it}) exp(z_{it})^{\tilde{\sigma}-1} C'(Q_{it})^{-\tilde{\sigma}}$.

The parametric assumption on government demand incorporates the increase in procurement contracts won after a firm gains a political connection, as well as CES-type

 $^{^{35}}$ We see this as a reduced-form simplification of an auction or bid contest where firms that are more efficient, of higher quality, or politically connected have an advantage and therefore are more likely to win procurement contracts.

demand function that depends on firm quality and (inverse) efficiency.³⁶ Together with the exogenous component, firm-level government demand is given by :

$$Q_{it}^{gov} = (1 + \tilde{d}_s P C_{it}) exp(z_{it})^{\tilde{\sigma} - 1} C'(Q_{it})^{-\tilde{\sigma}} \tilde{\xi}_{it}$$

$$\tag{7}$$

where PC_{it} is a binary variable capturing political connection status and \tilde{d}_s is the effect on increase government demand if the firm is politically connected, everything else equal, in line with our motivating evidence in Section 3. Moreover, $\tilde{\sigma}$ is the government demand elasticity with respect to marginal costs (prices), such that $\tilde{\sigma} > 1$. Thus, government demand increases in the quality and efficiency of the firm. The effect of marginal cost on government demand could be interpreted as more productive firms being able to win more competitive contracts by outbidding competitors, whereas quality may relate to scoring rules that require contractors to meet specific standards. Finally, $\tilde{\xi}_{it}$ is the exogenous demand shock that pins down the exact level of government contracts. We show in Internet Appendix Section D.1 evidence that government demand is indeed increasing in efficiency (inverse marginal cost) and quality.³⁷

Under Assumption 1, it is possible to show that private demand serves as a (partial) proxy for government demand. For a proportionality factor a_s between demand elasticities across sectors, such that $\tilde{\sigma} = a_s \sigma$, one can show the relationship between markets:

$$Q_{it}^{gov} = (1 + \tilde{d}_s P C_{it}) exp(z_{it})^{a_s - 1} Q_{it}^{priv^{a_s}} \tilde{\xi}_{it}.$$
(8)

If government demand is less (more) elastic than private demand, $a_s < 1$ ($a_s > 1$), then controlling for private quantity, and the other terms, higher quality firms have lower (greater) government demand. Moreover, everything else equal, the relationship between private and government demand is concave (convex). If $a_s = 1$ the relationship is linear.

The non-linearity in the relationship between government and private demand can potentially add difficulties to the production function estimation as it would become necessary to flexibly take into account the share of output that goes to the public and private sector.³⁸ However, previous work by Dubois et al. (2021) has shown that government and

³⁸This approach was used in De Loecker (2011) to account for varying shares of output in multiproduct

³⁶The CES-type parametrization could be microfounded by assume ing the policy-maker is maximizing its own utility \tilde{U} , defined as $\tilde{U}_{st} = \left(\int_{i \in F_{st}} \tilde{\xi}_{it}^{1/\tilde{\sigma}} (1 + d_s P C_{it})^{1/\tilde{\sigma}} (exp(z_{it})Q_{it}^{gov})^{(\tilde{\sigma}-1)/\tilde{\sigma}} di\right)^{\tilde{\sigma}/(\tilde{\sigma}-1)}$. This implies that the government derives higher utility from greater quality of the goods, greater quantity of public goods, as well as greater output from connected firms. Rather than microfounding it this way, we directly assume the main elements implied by the CES parametrization.

³⁷In particular, we use data from the pharmaceutical market in Ecuador from Brugués (2020). Using a quality-ladder approach from Khandelwal (2010), we obtain measures of firm-product-level quality using data from the private market. Moreover, we proxy for firm-product efficiency using private market prices. We show evidence that firm-product-level government demand increases with the proxy for quality and decreases with the proxy for inefficiency.

private markets have similar elasticities in low and middle income countries. Thus, the relationship between both markets is linear $a_s = 1$. We show in Internet Appendix Section D.2 that this is also the case in our setting.³⁹ This leads us to the following assumption.

Assumption 2 – Government Demand Elasticity: In each sector s, the elasticity of government demand to marginal cost is equal to the elasticity of private demand to prices, i.e., $\tilde{\sigma} = \sigma$ and $a_s = 1$.

Then, if government and private markets have the same demand elasticity within each sector, i.e., $\tilde{\sigma} = \sigma$, one can write government demand as:

$$Q_{it}^{gov} = (1 + \tilde{d}_s P C_{it}) Q_{it}^{priv} \xi_{it}, \qquad (9)$$

where $\xi_{it} \equiv \tilde{\xi}_{it} (\sigma/(\sigma-1))^{\sigma} P_s^{priv^{1-\sigma}}/E_s$. Hence, it is possible to express firm-level government demand as proportional to private demand.⁴⁰ Note however, that this is an statistical relationship, rather than an equilibrium condition.

Within a given sector s, we assume that prices in the government market are proportional to prices in the private market. This implies that the ranking of prices across firms is preserved in both markets. We present empirical support for this assumption in Internet Appendix Section D.3 using data for the price of medicine in the private and government markets. Moreover, in line with our previous reduced-form evidence⁴¹ and the literature,⁴² we allow politically connected firms to charge an additional premium to the government.

Assumption 3 – *Government Prices:* For each sector s, prices for the government are proportional to prices in the private market. Moreover, politically connected firms may charge an additional price premium.

Prices to the government are given by:

$$P_{it}^{gov} = \begin{cases} \tau_{st} P_{it}^{pri} & \text{if } PC_{it} = 0, \\ \tau_{st} P_{it}^{pri} (1 + \mu_s) & \text{if } PC_{it} = 1, \end{cases}$$
(10)

firms.

 $^{^{39}}$ We do this using pharmaceutical data from Brugués (2020) and customs data. In both cases, the government and private markets have similar demand elasticities.

⁴⁰In our setting, government demand shocks ξ_{it} will tend to be very small. Internet Appendix Table IA18 shows that for any given year, 75% of contractors receive (almost) no contracts. Only for the 95th percentile, government and private sales are in the one-to-one range. For years with positive contracts, the share of government sales is also low, as for the 75th percentile it is only 41%. Thus, even at the top of the distribution, private output is greater than output for government procurement.

 $^{^{41}}$ As mentioned above, we show evidence that in our setting connected firms may indeed receive an additional premium (refer to Internet Appendix Section J).

⁴²See Szucs (2023) and Baranek and Titl (2020), which also find that politically connected firms receive higher unit prices.

for a proportionality factor $\tau_{st} \ge 0$ and a price premium $\mu_s \ge 0$.

To close the model, we define the equilibrium conditions for prices and demand in the government sector. First, although firm-specific demand is random, total sectoral government demand must be equal to observed government demand in that sector. Namely,

$$\overline{Q}_{st}^{gov} = \int_{i \in F_{st}} Q_{it}^{gov} di$$

where \overline{Q}_{st}^{gov} is observed government demand in sector s in year t.

We assume the government exhausts all its budget in each sector. That is, $B_{st} = \int_{i \in F_{st}} P_{it}^{gov} Q_{it}^{gov} di$. Average prices for government goods in the sector are obtained by dividing B_{st} by total government quantity in the sector \overline{Q}_{st}^{gov} , which yields

$$\overline{P}_{st}^{gov} = \int_{i \in F_{st}} P_{it}^{gov} S_{it}^{gov} di, \tag{11}$$

for firm-specific government supply-share $S_{it}^{gov} = Q_{it}^{gov} / \overline{Q}_{st}^{gov}$.

Lastly, although the firm-level demand from the public sector is stochastic, consumers still derive a utility that depends on the quality and quantity of public goods. We assume that utility from public goods is linearly additive to private goods

$$U_{st} = U_{st}^{pri} + U_{st}^{gov},\tag{12}$$

with the experienced utility from public goods given by⁴³

$$U_{st}^{gov} = \iota \cdot \left(\int_{i \in F_{st}} (exp(z_{it})Q_{it}^{gov})^{(\sigma-1)/\sigma} di \right)^{\sigma/(\sigma-1)}, \tag{13}$$

where ι is a constant that discounts the utility that the representative consumer gets for each unit purchased in the public sector. As we only study misallocation within the government, rather than across government and private sectors, we normalize $\iota = 1$, without loss of generality. Note that, similar to the private sector, the end consumer benefits from enhanced quality (z_{it}) and increased quantity (Q_{it}^{gov}) of public goods. This formulation enables us to examine the welfare effects of reallocating contracts in utility terms. Specifically, we will investigate the waste of resources (excess cost) needed to achieve an equivalent utility level for the end consumer. If consumers were solely concerned with the quantity of goods and indifferent to their quality, our framework based on revenue productivity (TFPR) alone would be insufficient to capture allocative inefficiencies, as

⁴³This would be equivalent to thinking about allocative inefficiencies in terms of the sectoralaggregator in Hsieh and Klenow (2009).

the key variable in such a case is output productivity (TFPQ). However, there are good reasons to believe that the representative consumer also cares about the quality of public goods. For instance, better quality roads and bridges improve trade, better designed hospitals reduce congestion and may improve health, etc.

4.1.3 Total Output and Revenue

For each year t, we assume government and private demand are set contemporaneously and instantaneously. Total revenue of the firm is $R_{it} = P_{it}^{pri}Q_{it}^{pri} + P_{it}^{gov}Q_{it}^{gov} = P_{it}^{pri}Q_{it}^{pri}(1 + \tau_{st}(1 + \tilde{d}_s PC_{it})\xi_{it}(1 + \mu_s PC_{it})).$

Following De Loecker (2011), we use the inverse private demand function implied by 5 to obtain an expression for private prices. Substituting it into the revenue equation, we obtain:

$$R_{it} = exp(z_{it})^{\frac{\sigma-1}{\sigma}} (Q_{it}^{priv})^{\frac{\sigma-1}{\sigma}} \left(1 + \tau_{st} (1 + \tilde{d}_s P C_{it}) (1 + \mu_s P C_{it}) \xi_{it}\right) \kappa_{st}, \tag{14}$$

with κ_{st} collecting sectoral related terms. To map into production function terms, we need to write the expression into total output. Equation 9 implies that total firm-level demand is

$$Q_{it} = \left(1 + (1 + \tilde{d}_s P C_{it})\xi_{it}\right)Q_{it}^{priv}.$$
(15)

Clearing for private demand and replacing in 14 yields total revenue

$$R_{it} = exp(z_{it})^{\frac{\sigma-1}{\sigma}} Q_{it}^{\frac{\sigma-1}{\sigma}} X(PC_{it}, \xi_{it}) \kappa_{st},$$
(16)

where $X(PC_{it}, \xi_{it})$ groups the terms related to political connection and government demand.⁴⁴ As, in equilibrium, quantity demanded equals quantity produced, we substitute the production function 3 into 16

$$R_{it} = exp(z_{it})^{\frac{\sigma-1}{\sigma}} L_{it}^{\beta_l} M_{it}^{\beta_m} K_{it}^{\beta_k} exp(\omega_{it} + u_{it})^{\frac{\sigma-1}{\sigma}} X(PC_{it}, \xi_{it}) \kappa_{st},$$
(17)

where the revenue elasticity of input h is $\beta_h \equiv (\frac{\sigma-1}{\sigma})\alpha_h$, for $h = \{l, m, k\}$.

As we do not observe firms' physical inputs, we rewrite the previous expression in terms of input expenditures, $\overline{L}_{it} = w_{st}L_{it}$, $\overline{M}_{it} = \rho_{st}M_{it}$, and $\overline{K}_{it} = r_{st}K_{it}$, where input prices are constant within a given sector at a given point in time t.

Assumption 4 – *Input prices:* Within each sector s and year t, input prices are common for all firms, regardless of their connection status.

⁴⁴Precisely, let $X(PC_{it},\xi_{it}) \equiv \left(1 + \tau_{st}(1 + \tilde{d}_s PC_{it})(1 + \mu_s PC_{it})\xi_{it}\right) \left(1 + (1 + \tilde{d}_s PC_{it})\xi_{it}\right)^{-\frac{\sigma-1}{\sigma}}$.

Then, equation 17 becomes:

$$R_{it} = \overline{L}_{it}^{\beta_l} \overline{M}_{it}^{\beta_m} \overline{K}_{it}^{\beta_k} exp(\omega_{it} + z_{it} + u_{it})^{\frac{\sigma-1}{\sigma}} \Psi_{st}^{-1} X(PC_{it}, \xi_{it}) \kappa_{st},$$
(18)

where $\Psi_{st} = w_{st}^{\beta_l} \rho_{st}^{\beta_m} r_{st}^{\beta_k}$ collects the input prices, each one scaled by the elasticity of the corresponding input. In Internet Appendix Section D.4, we show evidence that input prices are similar for connected and non-connected firms using data from the pharmaceutical sector from Brugués (2020) and the credit sector from De Simone (2022).⁴⁵

Taking logs of equation 18, we obtain

$$r_{it} = \beta_l \overline{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \xi_{it}^* + \varepsilon_{it}, \qquad (19)$$

where $\omega_{it}^* = (\frac{\sigma-1}{\sigma})(\omega_{it} + z_{it})$ is the revenue-based total factor productivity (TFPR). Notice that the TFPR term collects the firms' efficiency in output (TFPQ), product-quality, and the constant sectoral markup. The term ψ_{st}^* captures time-varying, sector-specific terms $(\Psi_{it} \text{ and } \kappa_{st})$, and $\varepsilon_{it} = (\frac{\sigma-1}{\sigma})u_{it}$ is the transformed shock. The term $\xi_{it}^* = \ln(X(PC_{it},\xi_{it}))$ is an unknown firm-level parameter, capturing the government demand shocks and the effect of political connections on revenue.⁴⁶

4.2 Social Excess Costs

To derive an expression for the excess costs, we assume firms are cost-minimizing and face the following Lagrangian function

$$\mathcal{L}(L_{it}, M_{it}, K_{it}, w_{st}, \rho_{st}, r_{st}, \lambda_{it}) = w_{st}L_{it} + \rho_{st}M_{it} + r_{st}K_{it} + \lambda_{it} \left(Q_{it} - L_{it}^{\alpha_l}M_{it}^{\alpha_m}K_{it}^{\alpha_k}exp(\omega_{it})\right).$$
(20)

Recall that our formulation implies that all firms in a given sector face the same input prices and production technology. Additionally, we make the following assumption regarding returns to scale:

Assumption 5 – Constant Returns to Scale: In each sector s, the production function satisfies constant returns to scale (CRTS), or $\alpha_l + \alpha_m + \alpha_k = 1$.

 $^{^{45}\}mathrm{We}$ also show how to estimate the welfare effects of connections if the equal input price assumption does not hold for these two groups.

⁴⁶The term $\ln(X(PC_{it},\xi_{it}))$ equals $\ln((1+\tau_{st}(1+\tilde{d}_sPC_{it})(1+\mu_sPC_{it})\xi_{it}) - \frac{\sigma-1}{\sigma}\ln(1+(1+\tilde{d}_sPC_{it})\xi_{it}))$. As demand shock ξ_{it} tend to be small, one can approximate the log-values as $\tau_{st}(1+\tilde{d}_sPC_{it})(1+\mu_sPC_{it})\xi_{it} - \frac{\sigma-1}{\sigma}(1+\tilde{d}_sPC_{it})\xi_{it})$, which reduces to $\xi_{it}(1+\tilde{d}_sPC_{it})^2(\tau_{st}(1+\mu_sPC_{it}) - \frac{\sigma-1}{\sigma})$. Below, we decompose this firm-year-specific component into a sectoral-year component, a political connection component, and an exogenous firm-year component.

We provide support for the CRST assumption in Internet Appendix Section D.5, and offer a generalization of results for other arbitrary returns to scale assumptions in Section 5.3.1 below.

The first-order conditions for the Lagrangian allow us to derive expression for the marginal costs of quantity as functions of technological efficiency, quantity, capital, and output elasticities of inputs, and inputs prices (see Internet Appendix Section A):

$$C'(Q_{it}) \equiv \frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Gamma)}{\partial Q_{it}}, \qquad (21)$$

where $C(\cdot)$ is the total cost function and Γ collects the elasticities and input prices. Keeping quality constant, comparing these marginal costs between firm types would indicate the (in)efficiencies from different counterfactual allocations, as the exercise done by Asker et al. (2019) for the case of oil.

In our analysis, we consider the heterogeneity of firms' quality levels. Consequently, simply comparing the marginal cost of output could yield misleading estimates of welfare if higher-quality firms are also associated with higher marginal costs. To address this issue, we focus on quality-adjusted marginal costs. By doing so, we account for the fact that high-quality producers require less output to achieve the same utility level. As a result, firms with higher quality create efficiency gains by utilizing fewer inputs to maintain constant utility. This measure enables us to capture inefficiencies arising from low-utility and high-cost per unit of output scenarios, providing a more accurate assessment of welfare implications.

Let the quality-embedded quantity be $\tilde{Q}_{it} = Q_{it}exp(z_{it})$. Across sellers, each unit of quality-embedded quantity yields the same utility level. That is, if $\tilde{Q}_{it} = \tilde{Q}_{jt}$, for sellers i and j, the consumer is indifferent the identity of the supplier.

Given an observed level of output Q_{it} , the quality-adjusted marginal costs is given by

$$C'(\widetilde{Q}_{it}) \equiv \frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Gamma)}{\partial \widetilde{Q}_{it}} = \frac{C'(Q_{it})}{exp(z_{it})},$$

where the equality is given by the chain-rule, and the expression implies that qualityadjusted marginal costs can be obtained dividing output marginal costs by the firm-level of quality $exp(z_{it})$.

We provide welfare measures using the quality-adjusted marginal cost:

Definition 1 The social excess cost (SOEC) in percentage terms of obtaining the same marginal utility from firm-type c rather than firm-type u is defined as the ratio in quality-

embedded marginal costs:

$$SOEC = \frac{C'(\widetilde{Q}^c)}{C'(\widetilde{Q}^u)} - 1.$$

Our measure of welfare concentrates on the vacuous use of resources that do not provide further increases in utility. Moreover, this definition implies that the social planner is agnostic regarding the source of the excess cost: efficiency (marginal costs) or quality. Conditional on quality, procuring goods from firm-type c rather than u generates excess costs if firm-type u is more efficient. Conditional on efficiency, if firm-type c is of lower quality, obtaining the good from c rather than u implies a waste of resources, as the representative consumer requires more of the good (and therefore, more input usage) to reach the same utility level.

One can use this definition of SOEC to obtain an approximation of the deadweight loss in terms of costs (DWLC) from allocating a share $1 - \theta$ of an additional budget Bto firm-type c rather than u. To focus solely on the cost side, assume no political price premium as well as no price differences between the firm types.⁴⁷ Then, as a first-order approximation, the inefficiency from allocating such budget will be given by:

$$DWLC = (1 - \theta)B\Delta C'(\tilde{Q}) = (1 - \theta)B(SOEC)C'(\tilde{Q}^u),$$
(22)

where $C'(\tilde{Q}^u)$ is the quality-adjusted marginal cost of firm-type u. This formula for DWLC then captures the inefficiencies from greater expenditures in inputs to generate the same level of utility for the final consumer. We consider these inefficiencies as a deadweight loss to society. This is because the supply of quality-adjusted public goods remains unchanged, while the excess cost incurred could have been transferred to the final consumer as additional purchasing power, allowing them to acquire more consumption in other goods and services. Consequently, the inefficient allocation of contracts can be viewed as a tax on society, reducing overall output and welfare. For $\theta = 0$, the DWLC captures the effect on the marginal dollar of budget.

We now derive two formulas for the excess cost under two different assumptions on the timing of capital investment decisions. The first one assumes that capital can be freely adjusted to respond to realized demand shocks. The second builds on the idea that capital is a dynamic input, in the sense that it is pre-determined by the firm's investment decisions in period t - 1.

⁴⁷If, as in our case, connected firms are less efficient (and thus charge higher prices in the private market) and receive a political connection premium from government purchases, then the expression for DWLC is a lower bound. This is due to the fact that the same allocated dollar will purchase fewer quality-adjusted units of quantity, and thus, would generate lower utility for the same expenditure.

Flexible Capital

Consider a scenario in which capital is fully flexible, so that firms choose all inputs contemporaneously. Through the cost minimization problem of the firm, we derive the following proposition.⁴⁸

Proposition 1 With CRTS in production, constant elasticity of substitution, and flexible capital, the social excess cost of procuring from a politically connected contractor rather than a non-connected contractor is given by

$$SOEC_{flex} = exp\left(\frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\right) - 1.$$
(23)

Proposition 1 implies that we can identify the average social excess cost between connected and non-connected contractors by looking at differences in TFPR, weighted by the estimated revenue elasticities. Allocating contracts to connected contractors generate quality-adjusted welfare losses if connected contractors are less productive in revenue than their non-connected contractors.

In this situation, the DWLC expression is exact, rather than a first-order approximation, as marginal costs are constant for any level of output.

Fixed Capital

Proposition 1 offers a relatively straightforward way of computing social excess costs. However, it relies on the assumption that capital can be flexibly adjusted and therefore abstracts from any issue that arises when firms are close to their capital-utilization capacity. A more realistic approach assumes that capital at time t is predetermined by investments at time t - 1, allowing for non-linearities in the cost function. The cost minimization problem for a fixed level of capital leads to the next proposition.

Proposition 2 With CRTS in production, constant elasticity of substitution, and fixed capital, the social excess cost of procuring from a politically connected contractor rather than a non-connected contractor is given by

$$SOEC_{fixed} \approx exp\left(\frac{\beta_k}{\beta_l + \beta_m + \beta_k} [ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\right) - 1, \quad (24)$$

where $S_{it}^k = \overline{K}_{it}/R_{it}$ is the capital-revenue share, with $\overline{K}_{it} = r_{st}K_{it}$

Intuitively, the excess cost function depends on the productivity and quality differences (embedded in ω^*) between connected and non-connected contractors, as well as

⁴⁸Complete derivations of Propositions 1 and 2 are shown in Internet Appendix A.

gaps in their capital utilization. The convexity in the cost function introduced by fixed capital implies that firms with low levels of capital-revenue share will require more input usage to produce the same level of quality-adjusted quantity at the margin. Setting aside quality and productivity differences, allocating contracts to connected firms will generate a cost for society if non-connected firms are further away from their capacity constraint.

4.2.1 Discussion of flexible capital and fixed capital assumptions

In addition to enhancing realism, the fixed capital formulation for excess costs is also fully consistent with the typical identifying assumption required in the production function literature: the dynamic capital assumption, where capital is determined one year in advance. In this case, the DWLC expression is a first-order approximation, as we are linearly extrapolating the marginal cost at the observed output, even though the marginal cost is not constant.

Note that the flexible capital formulation for excess costs (Proposition 1) would also be consistent with the necessary identification assumption of production functions if the duration of contract provision extends beyond a year. Consequently, the exercise with flexible capital may be of particular relevance for evaluating long-term contracts. Additionally, the flexible capital formulation can be a useful tool for evaluating potential efficiency losses when selecting contractors for medium or long-term contracts that will be executed in the future.

In the case of Ecuador, the median duration of a contract is only 15 days, with just 5% of contracts lasting longer than 300 days. Thus, we present the results and methodology for flexible capital with the caveat of internal inconsistency with the assumptions for the production function estimation within the Ecuadorean context. For this reason, we consider the fixed capital formulation as the preferred estimate.

4.2.2 Model Extensions

In the Internet Appendix we present several extensions for our model. In Internet Appendix Section D.4, we sketch the reformulation to the excess costs expressions to account for (constant within-sectoral) differences in input prices by connection status. In Internet Appendix Section D.5 we sketch the reformulations if constant returns to scale does not hold, allowing for increasing or decreasing returns to scale. Lastly, in Internet Appendix Section K, we sketch a similar approach for multi-product firms that allows researchers to measure efficiency effects at the product-level rather than at the sectoral level. This approach may be implemented with additional product-level information on the output of multi-product firms.

4.2.3 Relationship with the Literature

Our approach has several advantages relative to past literature. First, contrary to the exercise in Hsieh and Klenow (2009), our measure of misallocation is not bench-marked against a frictionless world nor is focused on whether the allocation of inputs is efficient across firms. Instead, we are concerned about the allocation of a dollar of government expenditure between two arbitrary types of firms, both of which could be non-optimal, keeping all underlying distortions constant. For that reason, we see our contribution as an important tool that government officials can use to verify ex-ante whether a specific policy rule in public procurement may create unintended losses.⁴⁹

Second, our measure of inefficiency does not come from the dispersion of TFPR in the economy but rather from comparisons of average productivity across groups of firms. Therefore, our approach addresses concerns about measurement error being interpreted as misallocation (Bils et al., 2017; Rotemberg and White, 2017).

Third, as highlighted by Haltiwanger et al. (2018), measures of misallocation using the dispersion approach use the implicit assumption that marginal costs are constant and can only provide welfare statements under such an assumption. Instead, our approach relaxes this assumption by allowing non-constant marginal costs.

Fourth, our measure of TFPR embeds quality differences. In this way, by focusing on TFPR, we can speak about losses to society stemming from the underprovision of quality. Therefore, even if detailed quantity information were available, we would still need to estimate TFPR and not TFPQ. However, if we were to obtain a measure of TFPQ, in addition to TFPR, we would be able to decompose the misallocation in terms of quality and efficiency.

Lastly, all the parameters we need to estimate excess costs can be recovered with standard production function estimation techniques applied to revenue production functions. Estimating revenue production functions does not require quantity information, so it relies on data that is more widely available both for policymakers and academics.

4.3 Estimating Production Function and Excess Costs

We describe the procedure to obtain estimates of the revenue elasticities and firm-level revenue productivity. The estimating equation is equation 19, rewritten here for convenience:

$$r_{it} = \beta_l l_{it} + \beta_m \overline{m}_{it} + \beta_k k_{it} + \omega_{it}^* + \psi_{st}^* + \xi_{it}^* + \varepsilon_{it}.$$

We parameterize the unobserved shocks related to the government ξ_{it}^* into an unob-

⁴⁹Besides this theoretical discussion, we illustrate the empirical benefits of our approach relative to what can be achieved with the standard (Hsieh and Klenow, 2009) in Internet Appendix Section I.

servable component, ϕ_{it} , a component dependent on the political connection status ξ_s^{PC} , and sectoral-year constants a_{st} capturing the government-private market price differences, Formally, let ξ_{it}^* be:

$$\xi_{it}^* = \xi_s^{PC} PC_{it} \cdot Contractor_{it} + a_{st} + \phi_{it}, \tag{25}$$

where $Contractor_{it}$ is an indicator equal to 1 when the firm is a government supplier in year t and ϕ_{it} denotes government demand shocks independently and identically distributed across firms and time within a sector. The common component ξ_s^{PC} captures at the same time the increased demand effect \tilde{d}_s and the political price premium μ_s .

This leads to the main estimating equation⁵⁰

$$r_{it} = \beta_l \overline{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \xi_s^{PC} PC_{it} \cdot Contractor_{it} + \varepsilon_{it}.$$
 (26)

To estimate equation 26, we follow the standard production function estimation literature to deal with the simultaneity and selection biases that arise from the correlation between productivity and inputs (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Wooldridge, 2009).⁵¹ As we do not have sectoral time-varying controls as in De Loecker (2011), we control for ψ_{st}^* by estimating separate production functions in each 2-digit sector and by including year fixed effects in the regressions. Internet Appendix Section E provides estimation and identification details.

While input parameters follow the usual identification arguments, the parameter ξ_s^{PC} capturing the effect on demand shocks and price markups due to political connections is novelty in our setting relative to previous literature. The parameter is identified in the second-stage equation proposed by Levinsohn and Petrin (2003), which relies on the assumption that the innovation to productivity (relative to previous year productivity) is uncorrelated with political connection status in the same year. This leads to two concerns. First, this identification assumption requires that political connection status does not affect physical productivity, markups in the private sector, and quality, conditional on their past measures. While strong, this assumption can be tested following the procedure of De Loecker (2007) and estimating productivity without any control for political connection status and then looking for changes in the estimate around the time the firm gains the connection. This is the test conducted in the reduced-form evidence section 3.2.5 above, where we observe null results.

Second, a reader may also be concerned about a possible simultaneity bias in the rela-

⁵⁰With some abuse of notation, the parameter a_{st} is included in ψ_{st}^* , while government demand shock ϕ_{it} is included in ε_{it} .

⁵¹Specifically, we adopt the Wooldridge (2009) one-step GMM version of Levinsohn and Petrin (2003), which we refer to as LP-Wooldridge.

tionship between productivity and political connections. For example, a firm experiencing an increase in productivity may also become more likely to establish political connections through a past shareholder, which could lead to a biased estimate of the productivity of connected firms. To address this concern, one possible approach would be to concentrate on cases in which political connection status is exogenous, such as close elections or major reshuffles. In the following section, we use the major reshuffle strategy.⁵² To complement the major reshuffle approach, we also focus on measuring productivity differences *before* the connection occurred, which reduces concerns about simultaneity bias.

Given the augmented revenue equation 26, estimates of firm-level TFPR can be obtained by the residuals

$$\hat{\omega}_{it}^* = r_{it} - \hat{\lambda}_s - \hat{\beta}_l l_{it} - \hat{\beta}_m m_{it} - \hat{\beta}_k k_{it} - \hat{\tau}_t - \hat{\xi}_{st}^{PC} PC_{it} \cdot Contractor_{it}, \qquad (27)$$

where $\hat{\lambda}_s$ is the sector-specific constant and $\hat{\tau}_t$ are year fixed effects.

With elasticities and productivities in hand, we use the empirical analogs of Proposition 1 and 2 to compute the average gap in quality-embedded marginal costs between politically connected firms and non-connected ones. In particular, assuming capital is fully flexible, we run the within-sector (at the 2-digits) regression

$$\hat{\omega}_{it}^* = \alpha_s^1 + \gamma_\omega P C_{it} + \tau_t^1 + \nu_{it}^1,$$
(28)

where PC_{it} is an indicator for contractors that establish a link with bureaucracy at some point in our data, τ_t^1 are sector-specific year dummies, and α_s^1 the sector-specific average for non-connected firms for each 3-digit subsector. The coefficient γ_{ω} identifies average differences in TFPR between connected and non-connected firms.⁵³ We can then measure excess costs as

$$\widehat{SOEC}_{flex} = exp\Big(\frac{-\hat{\gamma}_{\omega}}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\Big) - 1.$$
⁽²⁹⁾

On the other hand, under the assumption of fixed capital, we estimate the following two equations at the sector level

$$\hat{\omega}_{it}^{*} = \alpha_{s}^{1} + \gamma_{\omega} P C_{it} + \tau_{t}^{1} + \nu_{it}^{1}$$

$$s_{it} = \alpha_{s}^{2} + \gamma_{S} P C_{it} + \tau_{t}^{2} + \nu_{it}^{2},$$
(30)

 $^{^{52}}$ In particular, we study productivity differences between unconnected contractors and contractors with a connection generated due to a large reshuffle. We do not use the close-election methodology because a large proportion of connections in our data are non-elected bureaucrats, and using a close-election strategy would be too noisy.

⁵³This estimate is different from ξ_s^{PC} , which captures the demand and price effects of connections on revenue conditional on firm-level productivity, while γ_{ω} is just an estimate of the average differences in estimated productivity.
with $s_{it} = \bar{k}_{it} - r_{it}$. We then plug these estimates in the excess cost equation

$$\widehat{SOEC}_{fixed} = exp\left(-\frac{\hat{\beta}_k}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\hat{\gamma}_S - \frac{1}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\hat{\gamma}_\omega\right) - 1.$$
(31)

5 Results

This section presents the main results of the welfare analysis. We first discuss estimates of the production function elasticities. Then we present the estimated excess costs and use them to quantify the welfare cost caused by the misallocation of procurement contracts. Importantly, all parameters are estimated at the 2-digit sector level and the tables report weighted averages across industries, meaning that all results control for the industrial sector of the firm. We compute standard errors using 30 bootstrap repetitions.

5.1 Production Function Estimates

Cross-sectoral average labor, intermediate inputs, and capital elasticities are reported in Table 7, together with the corresponding revenue returns to scale.^{54,55} For each specification, we present the results obtained via an OLS regression, as well as the one-step GMM version of Levinsohn and Petrin (2003) proposed by Wooldridge (2009) (denoted LP-Wooldridge henceforth), which accounts for the correlation between inputs and unobserved productivity. The first two columns present our preferred specification and are based on the model adjusted for the government premium and demand shocks from political connections described in equation 26. Under the LP-Wooldridge procedure, we estimate an economy-wide labor elasticity of 0.39, an intermediate inputs elasticity of 0.51, and a capital elasticity of 0.03.

The remaining columns serve as robustness checks and estimate instead a more standard production function that does not control for political connection status.⁵⁶ By doing so, we relax the identifying assumption for the political connection term, i.e., productivity shocks are orthogonal to connection status. Thus, we attribute any increase in revenue related to political connection to potential increases in productivity.

In each of our robustness approaches, we adopt alternative methods to correct for the political connection premium. These checks have two purposes: 1) to validate that the

$$r_{it} = \beta_l \overline{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \varepsilon_{it}.$$
(32)

 $^{^{54}}$ Our definition of intermediate inputs includes both material inputs and services used in production. 55 In Internet Appendix Figure IA6 we plot the distribution of returns to scale across sectors.

⁵⁶The revenue production function we estimate is given by:

revenue elasticity estimates are robust, and 2) to create alternative productivity estimates to verify the robustness of the welfare exercises under different modeling assumptions.

Our first check (Columns (3)-(4)) aims to investigate whether introducing political connection status as a variable in the production function equation introduces bias in the elasticity estimates. Instead of capturing the known demand shock and political premium using the political connection variable, we adjust the revenue from government sales of connected contractors by a 6% premium.⁵⁷ We then proceed to estimate the standard revenue production function. This adjustment allows us to account for the increase in revenue attributed to the political connection, which would otherwise be mistakenly attributed to higher TPFR, despite not representing real improvements in total factor productivity.

As a second check, one may be worried that political connected firms use different production technologies, and hence, including that sample might introduce bias to the production function and productivity estimates. To overcome this issue, Columns (5)-(6)estimate the production functions by excluding firms with active political connections, keeping non-connected contracts and connected contractors prior to their connection. By doing so, we overcome three potential sources of bias. Firstly, we address the bias that could arise in elasticity estimates if politically connected firms systematically differ from non-connected firms after becoming connected. Secondly, we tackle the issue of simultaneity bias in productivity and political connection shocks. Thirdly, as this specification excludes contractors with active connections, there is no need to make adjustments for the political price premium.

Next, to verify that the premium correction does not mechanically affect the elasticities, in Columns (7) through (10) we include all available years but make no premium corrections. Instead, Columns (7)–(8) estimate the standard revenue production function equation 32 on the full sample of contractors. Besides checking whether correcting for political connection status affect our estimates, this specification attributes all the excess revenue enjoyed by connected firms, originating from either government demand shocks or price premiums, to an increase in total factor productivity ratio (TFPR). For that reason, it presents the most conservative estimate of welfare losses, if connected firms are less revenue efficient. At the same time, Columns (9)–(10) expand the analysis to include all Ecuadorian firms, not just government contractors. This broader inclusion helps verify whether the results are driven solely by contractor-specific production functions.

⁵⁷If there is a political premium, the sales to the government will overstate the amount of real output Q^{gov} for a given private market price p. That is, we would assume that the firm is able to create a lot of output given a set of inputs. Thus, they have higher TPFR. To account for this, we define adjusted revenue $R_{ad}^{tot} = R^{tot} - 0.06 * R^{gov}/1.06$ and use it as the true firm revenue, where we assume $\mu_s = 0.06$ for all sectors s.

Reassuringly, although point estimates differ across modeling assumptions, the relative importance of each input is similar across all specifications. Importantly, the estimated revenue elasticities are consistent with the assumption of constant return to scale in production for reasonable demand elasticity parameters σ in line with those in Halpern et al. (2015).⁵⁸

5.2 Excess Costs Estimates

The estimates of the excess costs from political connections are reported in Table 8. Panel A presents our main results, where productivity is computed as the residual from the augmented revenue equation 26. The first two columns assume that capital can be flexibly adjusted. We retrieve an average excess cost of about 1% using the OLS revenue productivity estimates, and of 3.9% with the LP-Wooldridge estimates. The significant excess cost gap under flexible capital implies that connected firms have lower revenue productivity.

Columns (3)–(4) consider capital as a fixed input, which implies non-constant marginal costs. As stated in Proposition 2, under this assumption, the excess cost of provision also depends on differences in the capital-revenue ratio between connected and non-connected contractors. We find excess costs of about 0.8% when productivity is estimated via OLS, and 3.8% using the LP-Wooldridge correction. The differences in estimates across OLS and LP-Wooldridge highlight the importance of correcting for the endogeneity bias that exists in production function estimates. However, the similarity in the point estimates relative to the flexible capital case suggests that within a given sector, connected firms are, on average, at a similar level of their capital capacity than non-connected firms. In terms of interpretation of the results, we find that switching contracts from connected to non-connected contractors would decrease 3.8-3.9% usage of factors of production *without* changing the utility of the final consumer, thereby implying significant efficiency gains to be obtained from such a policy.

The remaining panels of the table present results for the alternative specifications and samples used to estimate the production function parameters aimed at addressing the sensitivity of the results to different modeling assumptions.

Focusing on the excess costs obtained using LP-Wooldridge productivities and assuming fixed capital (Column (4)), we find overall consistent estimates ranging between 2.8% to 5.2%. First, Panel B shows that the results are robust to imputing the political premium rather than using the more flexible approach from Panel A.

 $^{^{58}}$ In Internet Appendix Section D.5 we discuss the empirical evidence for the constant return to scale assumption. In Section 5.3.1 we discuss how our estimates are affected under different returns to scale assumptions

Second, one also may be worried about biases in revenue productivity and elasticity estimates if politically connected firms change their relative input intensity after gaining a connection, or if productivity shocks are correlated with connectivity shocks. To ease these concerns, we perform ex-ante comparisons by relying on the estimated production function parameters that exclude firms with active political connections and by comparing non-connected contractors with connected contractors before they gain their link. Panel C, Column (4) shows excess costs of 5.1% in this counterfactual. This result addresses the bias concerns. Moreover, the results run against a narrative where political connections arise due to expected efficiency gains. If that were be the case, we should expect ex-ante positive efficiency gains.

Third, an additional concern may be that our estimation or imputation method is unjustly penalizing connected firms by attributing the additional revenue productivity to the political price premium. In Panel D, we address this concern by assuming the connected firm does not charge any additional premium. Given the evidence that connected firms do charge an additional premium, this exercise is the most conservative, as it assumes all the excess revenue is coming from productivity increase. However, we still find estimated statistically significant losses of 2.8%.

Fourth, as a last check in Panel E, we verify that the results are not driven by the reliance on contractor-specific production function. We find consistent results when we use production functions estimates that include all firms (not just contractors) in a given sector.

We can use the excess costs estimates, combined with equation 22 to compute the size of the implied welfare loss for the next dollar of expenditure, if we allocate the dollar to a politically connected firms, instead of a non-connected one (i.e., $\theta = 0$). We approximate the marginal cost of non-connected contractors, $C'(Q^u)$, with their variable costs-revenue ratio. We present the results in Table 8 as a share of the government budget that needs to be allocated—i.e., the share over the next marginal dollar. The estimates obtained using LP-Wooldridge productivities and flexible capital (Column (2)) range between 2.2% and 4.1%. Assuming fixed capital, we measure a welfare loss of 2.2% to 4.2% (Column (4)). The social cost implied by our main specification (Panel A, Column (4)) is approximately 3.0%, which indicates that, for every dollar spent, the government could transfer 3 cents to the final consumer while keeping their level of utility from government goods constant if the contracts were allocated to non-connected contractors.

5.3 Robustness Checks and Additional Results

5.3.1 Assumption on Constant Returns to Scale

Although our estimates for the elasticity of substitution implied by a CRTS assumption are close to Halpern et al. (2015), they are higher than usual estimates from Broda and Weinstein (2004). A reader may be concerned about the implications of a possible violation of the CRTS assumption. Here we offer a discussion on the implications for our estimates.

If one believes the CRTS assumption is valid but that the revenue elasticities are too large, then our estimates for welfare may be considered as lower bounds, as the expression is divided by the sum of revenue elasticities $\beta_m + \beta_l + \beta_k$, which would now be lower.

Instead, if the assumption of CRTS is invalid, we could use a commonly accepted estimate for the elasticity of substitution, e.g., from the study by Broda and Weinstein (2004), as a reference point. Under a returns to scale assumption of $\alpha_l + \alpha_m + \alpha_k = \overline{\alpha}$, then $\sigma/(\sigma - 1)(\beta_l + \beta_m + \beta_k) = \overline{\alpha}$. Thus, taking revenue returns to scale as 0.93 from our production function estimates and $\sigma = 3$ gives increasing returns to scale $\overline{\alpha} = 1.39$ as in De Loecker (2011).

By adjusting the returns to scale in production to align with this elasticity σ and our observed revenue returns to scale, we can derive modifications to the welfare expressions.

For flexible capital and returns to scale $\overline{\alpha}$, the excess cost measure now includes the revenue of the firm to capture the cost curvature from the returns to scale:

$$SOEC_{flexible}^{\overline{\alpha}} = exp\left(\frac{\sigma}{\sigma-1}(\omega_{it}^{*unc} - \omega_{it}^{*con}) + (1-\overline{\alpha})(ln(R_{it}^{con}) - ln(R_{it}^{unc}))\right) - 1.$$

Thus, relative to the original expression for social excess costs, the revenue productivity gap will produce larger allocative inefficiency statements. Moreover, if production has increasing (decreasing) returns to scale and connected firms are smaller, excess cost would be greater (smaller). As a back-of-envelope calculation, for $\sigma = 3$ and $\overline{\alpha} = 1.39$, and estimates from descriptive statistics $ln(R_{it}^{unc}) = 14.33$ and $ln(R_{it}^{con}) = 13.61$, the original SOEC estimate for flexible capital of 0.039 would be 0.37. Thus, if the economy indeed has increasing returns to scale, the gap in size between connected and non-connected would imply an even greater allocative inefficiency.

Instead, the excess costs expression for fixed capital and arbitrary returns to scale $\overline{\alpha}$ is given by:

$$SOEC_{fixed}^{\overline{\alpha}} \approx exp\Big(\frac{\beta_k}{\beta_l + \beta_m + \beta_k}[ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\Big)^{\overline{\alpha}} - 1.$$

Thus, if the economy presents increasing $(\overline{\alpha} > 1)$ our estimates in the main text are a

lower bound. Instead, if it presents decreasing returns to scale ($\overline{\alpha} < 1$), then our estimates in the main text are an upper bound. For instance, for $\overline{\alpha} = 1.39$, $SOEC_{fixed}^{\overline{\alpha}}$ from our main specification would be increased to 0.053 from 0.038.

5.3.2 Comparison-Sample Definition

A further main concern with the analysis above is that it compares all connected firms to all non-connected firms in a given sector. This approach might be flawed in that not all firms in a sector are capable of supplying a variety that is relevant for a specific procurement process. There exists the possibility that connected firms supply varieties that make them less revenue efficient and, as a result, we estimate welfare losses in the aggregate comparison. By refining the counterfactual group to those offering the same variety, the efficiency gap may disappear or reverse.

To tackle this concern, we perform the excess costs analysis using contract-level information to control for additional characteristics that might explain differences in revenue productivity and capital intensity. We estimate equations 30 from contract-firm level data (i.e., each contract-firm combination corresponds to an observation) and use as the sample all winning and losing firms among contracts with at least two competitors. In those regressions, we control for different contract specific characteristics.

Table 9, Panel A shows the results. Column (1) benchmarks the excess costs of connected winning firms relative to non-connected winning firms controlling only for 3-digit and year fixed effects, which are the same controls used in our initial specification above. In this exercise, excess costs from procuring are around 7%. In Column (2), we control for additional characteristics such as agency, province and contract-category fixed effects, thereby accounting for differences in location, contracts-types, and agency-specific requirement in TPFR and capital-intensity. While the estimate decreases, we still find excess costs of 6%. Lastly, in Column (3), we perform within-contract estimation using contract fixed effect, comparing politically connected winning firms to non-connected losers. In this specification, we are restricting the comparison to only actual competitors, serving as the most realistic counterfactual allocation. We still find a 7% excess cost. In all these specifications, the excess cost is statistically different from zero.

Although is not a main focus of this paper, our approach is easily implementable for any arbitrary groups of firms, for instance, non-connected winners relative to nonconnected losing firms. In Panel B of Table 9, we implement this counterfactual as a sanity check. In all three specifications, we find excess costs of around -2%. That is, we estimate cost gains from procuring from the winner. This is reassuring, as at least on average, the government procurement system is able to select better firms to sell goods to the government. Quality-adjusted excess costs could be overestimated if government specialization comes at a productivity loss in the private market, and politically connected firms are more likely to specialize. Similarly, our method would overestimate excess costs if specializing in public procurement gives higher utility to the final consumer through government consumption. In both ways, connected firms might be penalized and assumed to have either lower quality, given costs, or higher costs, given quality, or both. To address this concern, in Internet Appendix H, we conduct various robustness exercises that compare the excess costs of political connections for firms with different levels of government specialization. We estimate the excess cost for firms where the sales to the public sector represent at least 50% and 75% of the firm's total sales in a given year or across the period of analysis.⁵⁹ Reassuringly, for our main results, we estimate positive excess costs within different levels of specialization.⁶⁰

5.3.4 Contract Type

While a large majority of firms compete and win multiple types of contracts, some firms in our sample only sell in one specific category. Internet Appendix Table IA14 presents the results comparing firms that compete only in a specific contract type. Panel A shows the excess cost estimates for contractors that only compete in discretionary processes. For such contract types, we find excess costs of 5.4% stemming from political connections. Panel B, instead, shows results for contractors of auctions alone. The point estimate is smaller, at 4.1%, and not statistically significant. Panel C shows the results for a very small sample of firms that compete only in the set of random contracts. Here, we find excess costs of 2.2%, still not statistically significant. Lastly, Panel D shows statistically significant excess cost estimates of 6% for firms that procure multiple contract types. Although some estimates are noisy, the pecking order suggests that more discretionary contracts are also associated with higher allocative inefficiencies from political connections. Of course, discretionary contracts are likely more complex and may benefit highly from the positive effects of connections in contract performance due to monitoring. Our results imply that for connections to be welfare-increasing, the ex-post benefits coming from monitoring must be large enough to compensate for the ex-ante expected losses due to high quality-adjusted marginal costs of production.

⁵⁹The required assumption is that specialization leads to similar shifts in quality and/or productivity for both connected and non-connected firms, and that political connections only affect the likelihood of specialization.

⁶⁰Except for one noisy specification with a very small sample size of 108 firms.

5.3.5 Treatment-Sample Definition

To verify that the definition of treated firms does not drive the results, we construct excess cost estimates under different treatment definitions (see Table 10). First, as firms may gain political connections precisely due to some firm-specific characteristic (e.g., the product selection they offer), we focus solely on the set of firms with plausible exogenous linkages that were generated due to a large reshuffle in the bureaucratic agency. As mentioned above in the reduced-form evidence, these large reshuffles reduce the likelihood that the firm of interest was individually selected for some procurement-related process. That is, we exclude connected firms that did not gain a connection in a large reshuffle. Panel A presents the results, which find statistically significant excess costs of political connections are relevant. Recall that direct connections are those in which the firm owner becomes a bureaucrat. We find virtually identical results of 3.8% for both types of connections. Together with the findings using large reshuffles, the results indicate that fortuitous connections have similar efficiency effects as more endogenous connections.

5.3.6 Location

If contracts are location-specific, for instance, due to transportation or search costs, we may be overestimating the costs of connection. While some alternative far-away firm might be more efficient, it would simply not be feasible to hire them. To address this concern, we perform sectoral analysis restricting to firms within each province. Internet Appendix Figure IA7 shows the distribution of province-specific excess cost averages, weighted by the importance of a sector in the province. Although there is heterogeneity in the estimates, the majority of provinces (80%) have positive excess costs of political connections, with the median province having excess costs of 9%.⁶²

5.3.7 Size-dependent policies

From the descriptive statistics in Table 2, connected contractors tend to be smaller than non-connected ones. So, it is worth asking if it might be plausible to fix the adverse effects of political connections by targeting specific firm sizes, i.e., by implementing sizedependent procurement policies that favor large firms. Note that this policy would be

⁶¹Additionally, we find significant excess cost estimates similar to those in the baseline if we exclude strategic exit firms or firms that have more than one connection.

⁶²Seven out of 24 provinces show statistically significant losses. Of the remaining provinces, only two have precisely estimated zeros, while the other 15 provinces have small sample sizes that prevent efficient testing.

counter to more traditional approaches (both in Ecuador and abroad) that offer preferential treatment to small and medium-sized enterprises. In Internet Appendix Table IA15, we present the results of size-dependent policies.⁶³ In Panel A, we restrict to firms in the lowest quartile of assets. For this sample, we find positive effects of political connections, with connected firms generating efficiency gains, although the effect is not statistically significant. In Panels B and C, we study the second and third quartiles and find precisely estimated zero effect of connections. Lastly, in Panel D, we study the largest firms and find that the inefficiency concentrates in this sample. Here, we find 3.9% excess costs. Therefore, minimum-size policies would not be able to balance the negative effects of political connections.

5.3.8 Sectoral Differences

We present a decomposition of the excess cost estimates by industry in Figure 3 (for the 20 largest sectors in terms of public procurement expenditure) and in Internet Appendix Table IA10 (for all the sectors). We report the coefficients obtained assuming fixed capital and with production functions estimated by the LP-Wooldridge method on the augmented revenue equation 26.⁶⁴ Sectors related to construction, consultancy, real estate activities, and telecommunications show large excess costs of provision from political connections, in line with anecdotal evidence. However, for some sectors such as wholesale trade of goods (except motor vehicles), we estimate negative (though not significant) excess costs. The existing heterogeneity suggests that, although political connections induce welfare losses in the majority of the industries, we cannot rule out that they play a beneficial role in some specific sectors.⁶⁵

Our findings are further supported by the analysis using our previous definition of sectors, which includes tradables, wholesale/retail, and non-tradables. Consistent with the reallocation of contracts by sector in Section 3, the results presented in Internet Appendix Table IA16 indicate no significant effect of political connections in the wholesale and retail trade sectors. However, significant welfare losses are observed in the non-tradables sector, while the losses in the tradables sector, although large, are not statistically significant. Moreover, we observe that these welfare losses are more pronounced in sectors characterized by high competition (low concentration), while sectors with high concentration exhibit positive welfare effects, although not statistically significant.

Based on the insights gained from the motivation in Section 3, our analysis suggests

⁶³We first obtain the median value of assets for each firm, and then rank firms in quartiles for each given 2-digit industry.

⁶⁴Internet Appendix Table IA13 shows a positive and high correlation with the industry-level excess cost obtained using the other specifications and assumptions.

 $^{^{65}}$ Of the 42 sectors for which we estimate excess costs, 35 have positive point estimates.

that the sectors experiencing contract reallocations due to political connections are precisely the sectors where we find that political connections generate welfare losses.

5.3.9 Discussion

On the whole, we find significant welfare losses due to political connections, with distortions mainly concentrated in firms that procure only discretionary contracts (or a mix of contract types) and in sectors providing less standardized goods and products. These losses hold if we make ex-ante comparisons (i.e., before the firm gained a connection) or if we concentrate on likely exogenous connections, such as those coming from rotation in appointments or indirectly obtained through family members. This suggests that stories explaining political connections aimed at improving the quality-efficiency of the contracts are unlikely, and that firms may take advantage of fortuitous connections despite possible losses for society. These inefficiencies remain even if we restrict the potential sample of counterfactual allocations by focusing on cases within the same province, similar sizes of firms, similar levels of government specialization, or even to the set of firms competing for the exact same contract.

Our findings do come with caveats. First, due to data limitations, it is not possible for us to identify product-level quality-productivity for cases with multi-product firms, precluding us as well from making product-level comparisons. As such data becomes more widely available, it will be feasible to use our approach for product-level excess cost estimates (as sketched in Internet Appendix Section K). Second, given that our data does not have any information about cost overruns, renegotiations, and delays, we remain completely silent on the effects of political connections on issues related to moral hazard and monitoring. These effects could be important, especially in non-standardized sectors and in discretionary contracts. Hence, our welfare effects should be appropriately adjusted by the benefits or costs of political connections adversely affect delays, execution costs, and renegotiation (Schoenherr, 2019; Brogaard et al., 2021; Ryan, 2020), we deem it unlikely that in our setting, connections have the opposite effect on all of these issues, thus balancing the negative efficiency effects we found.

Third, while this paper takes into account both differences in quality and productive efficiency, it is beyond the scope of this paper to measure each contribution to losses. In Internet Appendix Section \mathbf{F} , we validate that connected firms may offer lower quality goods and services by studying a small sample of audited construction contracts. We find that connected firms indeed offered lower quality goods. However, the sample size is too small to offer definite evidence. To effectively assess the role of quality and productive efficiency, additional data would be necessary. In particular, a researcher would need

access to both price and quantity data for a large number of firms, which is typically not available. Additionally, they would need access to a large-scale evaluation of the quality of the goods procured by the government. Future research may address this as more data becomes available.

5.4 Comparison to Alternative Methods

In Internet Appendix Section I, we benchmark our result relative to two alternative methods to obtain welfare estimates. Both considered methods apply estimation methods to capture the change in TFPR variance from moving from a non-connected economy to a fully connected one, thus approximating our main empirical exercise. As these methods rely on dispersion measures, we find they tend to be sensitive to the removal of outliers, while our method that relies on first moments is unaffected. Second, as the methods rely on cross-sectoral differences in dispersion rather than on firm-level differences in averages, they are extremely underpowered to explore the heterogeneity uncovered with the method introduced in this paper.

6 Conclusion

This paper studies the welfare costs of the misallocation of procurement contracts caused by political connections. Using a novel dataset that combines several administrative sources for Ecuador, we provide evidence that firms that form links with the bureaucracy through an ownership channel experience a significant increase in the probability of being awarded a contract. This effect is robust across a variety of samples and specifications.

We develop a methodology to quantify the welfare losses induced by political connections and provide a new sufficient statistic that compares the average revenue productivity and capital-revenue share differences between the observed allocation of contracts (connected firms) and a counterfactual allocation (non-connected firms). Using production function estimation, we find that politically connected firms have higher quality-adjusted marginal costs compared to non-connected firms. This gap translates into welfare losses of up to 6% of the procurement budget.

Although our definition of political connections is relatively narrow, this paper finds significant welfare losses when political connections are used to influence the allocation of procurement contracts. Alternative implicit allocation practices (such as favoring individuals in the same social network) and explicit allocation rules (e.g., preferential selection of small firms) may also have important welfare consequences. Given that public procurement represents a large share of GDP across most countries, we believe that further evidence on these margins would be a valuable avenue for future research.

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Figure 1: Ranking Bureaucratic Positions

Notes: The figure shows the top 20 bureaucrat positions ranked by the aggregate value of the contracts won by firms connected to bureaucrats in each position [panel (a)] and average value of contracts obtained per individual in the position [panel (b)]. The value of contracts won is constructed as follows. First, we consider the set of firms owners who are appointed as bureaucrats and exclude firms created or acquired by bureaucrats in office and those that establish their first political connection before 2000. For every bureaucrat, we take the last position they hold in the data, and each bureaucrat is assigned the value of the contracts won by the firms they own. The value of contracts awarded to firms connected to more than one bureaucrat is equally split among them. We compute the aggregate value of contracts won at the bureaucrat position level and report it in million USD on the x-axis in panel (a) and average value per individual at the bureaucrat position level and report it in thousands USD in panel (b). The numbers shown next to each bar indicates the number of distinct bureaucrats observed in a given position. For panel (b), we restrict positions that have at least 5 unique individuals.



Figure 2: Probability of Being Awarded a Contract Before and After Political Connection

Notes: This figure presents the coefficients for event-studies for winning government procurement contracts on the firm's first political connection using the methodology of Callaway and Sant'Anna (2021) relying on never treated as control. Subfigure A shows the probability of winning a contract. The dependent variable is equal to one when the value of contracts won in a given year is larger than US \$3,000, which roughly corresponds to the 10th percentile of the yearly contract value distribution for firms winning a non-zero number of contracts. Subfigure B has as dependent variable the (Inverse Hyperbolic Sine Transformation) value of all contracts awarded in a given year. Subfigure C has as dependent variable the (Inverse Hyperbolic Sine Transformation) number of contracts won in a given year. We set the year prior to the first connection (-1) as the omitted category. The control group includes non-connected contractors (never-treated). The sample is the set of firms classified as government contractors (see Section 2.2.1). The unit of observation is contractor-year. We include only years in which a contractor files balance sheet information. We exclude firms created or acquired by bureaucrats, and firms that established the first political connection before 2000. Error bars indicate 90 and 95% confidence intervals with efficient standard errors from Roth and Sant'Anna (2021). The dotted line shows the sample mean in the years before the event, and each coefficient is shifted by this constant.



Figure 3: Excess Costs Estimates, Largest Sectors

Notes: The figure reports averages and 95% confidence intervals of the excess costs of political connection at the 2-digit sector level. We report estimates only for the 20 largest sectors in the data in terms of public production function elasticities and firm TFPR used as inputs to the excess costs regressions are obtained using the LP-Wooldridge methodology with the specification detailed in equation 26. The sample for each industry is the set of firms classified as government contractors. Each regression includes year and 3-digit sector fixed effects. Standard errors are obtained via 30 bootstrap simulations.

	All connections	Only direct connections	Only indirect connections	Both direct and indirect connections
	(1)	(2)	(3)	(4)
Panel A: Politically connected (not s	trategic)			
Number of firms	6,030	2,789	1,370	1,871
Avg. nbr. distinct connection years	1.232	1.144	1.031	1.517
Avg. nbr. connections	1.631	1.177	1.102	2.719
Panel B: Politically connected (strate	egic entry)			
Number of firms	1,384	507	223	654
Avg. nbr. distinct connection years	1.686	1.387	1.108	2.114
Avg. nbr. connections	2.280	1.435	1.171	3.303
Panel C: Created by bureaucrat				
Number of firms	509	236	97	176
Avg. nbr. distinct connection years	1.298	1.156	1.065	1.639
Avg. nbr. connections	1.724	1.178	1.092	2.879

Table 1: Sample Size for Different Categories of Connected Contractors

Notes: The table presents sample sizes and statistics regarding the number of bureaucratic connections across various categories of politically connected firms. Panel A covers the sample of contractors analyzed. Panel B focuses on firms that have had shares purchased by an office-holding bureaucrat. Panel C examines firms founded by a bureaucrat. All columns exclude firms that formed their initial political connection prior to 2000 and those failing to submit balance sheet information. Additionally, the dataset includes 22,997 contractors without political connections

	Pan Eull S	el A	Panel B Contractors Sample		Panel C Connected Contractors Sample					
				Sittactors Samp		Connect	Connected Contractors Sample			
	All firms	All contractors	Not politically connected	All politically connected	Connected in final sample	Only direct connections	Only indirect connections	Both direct and indirect connections		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Revenue	810,647 (3,317,781)	$\substack{1,340,678\\(4,447,662)}$	$\substack{1,677,244\\(5,068,397)}$	602,489 (2,456,432)	815,973 (2,972,394)	749,802 (2,830,820)	$999,055 \\ (3,199,652)$	771,809 (2,994,810)		
Capital	325,902 (1,373,586)	380,484 (1,553,196)	476,583 (1,772,079)	169,711 (866,911)	225,226 (1,011,721)	218,479 (1,033,252)	235,544 (968,168)	227,478 (1,012,668)		
Wage bills	128,916 (460,268)	221,214 (627,813)	263,260 (698,629)	128,994 (419,254)	$168,925 \\ (499,233)$	152,581 (473,394)	202,778 (531,463)	167,213 (510,762)		
Intermediate inputs	542,330 (2,361,077)	893,766 (3,135,226)	$\substack{1,132,297\\(3,576,742)}$	370,597 (1,712,347)	503,149 (2,058,365)	469,293 (1,967,839)	623,025 (2,191,545)	459,354 (2,083,934)		
Debt	441,808 (1,714,406)	646,554 (2,186,380)	810,890 (2,486,208)	286,117 (1,232,358)	377,571 (1,444,691)	$342,120 \\ (1,341,569)$	460,428 (1,629,782)	366,234 (1,440,076)		
Revenue-asset ratio	1.689 (3.577)	$1.900 \\ (3.329)$	1.896 (3.242)	$1.908 \\ (3.514)$	1.867 (3.374)	1.859 (3.423)	1.865 (3.011)	1.881 (3.572)		
Age	9.528 (10.112)	$9.902 \\ (9.922)$	$10.593 \\ (10.653)$	8.387 (7.881)	$ \begin{array}{l} 11.100 \\ (8.373) \end{array} $	10.610 (8.034)	$11.406 \\ (8.466)$	11.623 (8.774)		
Sample size	73,133	27,058	18,585	8,473	4,532	2,106	1,085	1,341		

Notes: The table displays means and standard deviations (in parentheses) for balance sheet data from 2015. Column (1) encompasses all private firms in Ecuador, whereas Column (2) focuses on government contractors, as detailed in Section 2.2.1. Columns (3) and (4) differentiate between non-connected and connected contractors, respectively. Column (5) omits firms founded or acquired by bureaucrats during their tenure and those that formed their initial political connection before 2000. Columns (6) through (8) further dissect Column (5) based on the nature of the political connection. Each variable undergoes winsorization for non-zero entries at the 1st and 99th percentiles of their respective distributions. Dollar values are deflated by the consumer price index series computed by the World Bank (https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=EC).

	Contract value (\$) (1)	Contract budget (\$) (2)	Contract length (days) (3)	Number of contracts (4)	Number of competitors (5)
Overall	41,286 (80,086)	103,418 (252,887)	$70 \\ (151)$	199,727	1.671 (1.484)
Auctions	48,859 (81,845)	127,285 (216,014)	90 (179)	90,272	2.240 (1.832)
Publication	$15,316 \\ (51,074)$	32,537 (110,066)	26 (85)	65,093	$1.000 \\ (0.008)$
Direct contracting	$21,914 \\ (15,238)$	50,081 (35,391)	97 (122)	8,607	$1.000 \\ (0.000)$
Quotations	$198,\!800$ (126,892)	$\begin{array}{c} 481,\!793 \\ (330,\!293) \end{array}$	156 (230)	6,440	$1.392 \\ (0.916)$
Other discretionary	214,282 (154,315)	631,661 (1,266,095)	210 (287)	2,954	$1.437 \\ (1.267)$
Lower value (goods and services)	$16,198 \\ (13,450)$	35,831 (30,221)	$63 \\ (110)$	16,462	$1.130 \\ (0.604)$
Lower value (public works) (Lottery allocation)	$47,474 \\ (40,602)$	106,844 (93,029)	$\begin{array}{c} 63 \\ (35) \end{array}$	9,899	$1.333 \\ (1.482)$

 Table 3: Descriptive Statistics of Government Procurement Contracts

Notes: The table reports means and standard deviations (in parenthesis) for the sample of Ecuadorian government procurement contracts won by firm contractors between January 2009 and December 2017. We exclude contracts of total value below the 1st percentile and above the 99th percentile of the contract value distribution. Other discretionary contracts include public contests, trade fairs, tenders, and short lists. Statistics on the number of competitors are computed using the subset of contracts detailing this information and refer to the number of firms competing for each tender. Dollar values are deflated by the consumer price index series computed by the World Bank (https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=EC).

	Log(Value Contracts) (1)	IHS(Value Contracts) (2)	Prob(Winning) (3)	# Contracts (4)	$\begin{array}{c} \text{IHS}(\# \\ \text{Contracts} \end{array}) \\ (5) \end{array}$
After first political connection	-0.3147^{***} (0.0948)	0.0713 (0.0611)	0.0428 (0.0632)	-0.0582 (0.0585)	-0.0104 (0.0598)
Observations R-squared	712 0.2977	$1,815 \\ 0.3350$	$1,832 \\ 0.3278$	$1,832 \\ 0.3401$	$1,821 \\ 0.3520$
Sector FE	YES	YES	YES	YES	YES

Table 4: Dispersion (SD) in Contracts for Connected Firms

Notes: The table reports changes in dispersion (standard deviations) in government contracts for connected contractors before and after the connection is established. Observations are segmented at the 3-digit sectoral level, categorized by year and connection status (pre- and post-connection). Column (1) displays the standard deviation (SD) of the log-transformed total contract values, Column (2) shows the SD of the contracts' values using the Inverse Hyperbolic Sine Transformation to include instances where firms did not secure contracts, Column (3) illustrates the SD of the likelihood of winning a contract, Column (4) outlines the SD of the count of contracts won, and Column (5) depicts the SD of the contract counts post-Inverse Hyperbolic Sine Transformation to mitigate outlier effects. All metrics are normalized to standard units for ease of comparison. Note that year-fixed effects are netted out before calculating the dispersion in each metric to remove seasonal trends in procurement. Standard errors are clustered at the 3-digit sectoral level. *** p < 0.01, ** p < 0.05, * p < 0.1.

	Panel A By Type of Contract			Panel B Provinces	
	Auction (1)	Discretionary (2)	Lottery (3)	$\frac{\text{Same}}{(4)}$	Other (5)
After first political connection	0.0106^{*} (0.0062)	0.0354^{***} (0.0088)	-0.0001 (0.0032)	0.0317^{**} (0.0130)	0.0210 (0.0166)
Sample Size	180,711	180,711	180,711	176,615	176,615
Number contractors	$27,\!659$	$27,\!659$	$27,\!659$	$27,\!533$	$27,\!533$
Connected contractors	4,662	4,662	4,662	4,536	4,536
Mean before connection	0.062	0.141	0.032	0.0853	0.131

Table 5: Probability of Being Awarded a Contract - Heterogeneity by Type and Location

Notes: The table reports heterogeneity of treatment effects of the first political connection on the allocation of contracts by type of contract and location using the methodology of Callaway and Sant'Anna (2021) relying on never treated as controls. Panel A explores the effects across different types of contracts: discretionary, auction, or random. Panel B examines the effects based on geographical location, specifically identifying a firm as being in the same province as the contract when the contract's registered location aligns with the firm's headquarters. Efficient standard errors from Roth and Sant'Anna (2021) in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The p-values of test of equality of coefficients are the following. (1) = (2): 0.021; (1) = (3): 0.096, (2) = (3): 0.000; (4) = (5): 0.611.

Table 6: Pre	obability of Being	Awarded a	Contract -	${\rm Robustness}$
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	Panel A Restricted Sample		Panel B By Type Linkage		Panel C Falsification			
	Large reshuffles (1)	Single entry year (2)	No strategic exits (3)	Direct Only (4)	Indirect Only (5)	Fake treatment years (6)	Families with 15+ siblings (7)	Low rank and low shares (8)
After first political connection	0.0300^{**} (0.0144)	0.0296^{**} (0.0117)	0.0413^{***} (0.0114)	0.0435^{***} (0.0127)	0.0742^{***} (0.0175)	0.0031 (0.0113)	$ \begin{array}{c} 0.0098 \\ (0.0145) \end{array} $	-0.0109 (0.0155)
Sample Size	161,536	169,883	170,473	166,462	157,380	111,019	95,078	134,162
Number contractors	24,750	26,029	26,184	25,491	24,099	16,818	14,321	20,782
Connected contractors	1,753	3,032	3,187	2,494	1,102	2,205	1,282	1,023
Mean before connection	0.193	0.196	0.184	0.195	0.207	0.213	0.227	0.225

Notes: The table reports aggregated treatment effects of the first political connection on the allocation of contracts for different subsamples using the methodology of Callaway and Sant'Anna (2021) with never-treated firms as controls. In Panel A, we report robustness exercises for the type of event. In Column (1), we consider connections through large reshuffles of government agencies. Column (2) limits the treatment group to the set of contractors that establish their political connections in a single year. Column (3) drops firms for which owners sell their shares after being appointed as bureaucrats. In Panel B, we report the heterogeneity in the type of connection, with Column (4) looking at Direct connections (where a bureaucrat is the owner) and Column (5) at Indirect ones (where a sible netry year distribution is equal to the true one. In Column (7), we consider connections through families classified as having more than 15 siblings. In Column (8), we consider connections to bureaucrats who own less than 10% of the firm's shares and have low-rank positions. Efficient standard errors from Roth and Sant'Anna (2021) in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

	l spec	Main ification	Premiur	m-adjusted venue	Exclud	le political ction years	No p adju	oremium 1stment	f	All irms
	OLS (1)	LP- Wooldridge (2)	$\begin{array}{c} \text{OLS} \\ (3) \end{array}$	LP- Wooldridge (4)	OLS (5)	LP- Wooldridge (6)	OLS (7)	LP- Wooldridge (8)	OLS (9)	LP- Wooldridge (10)
Labor	$0.3808 \\ (0.1034)$	0.3875 (0.1262)	0.3808 (0.1029)	0.3873 (0.1252)	$0.3549 \\ (0.0991)$	$0.3612 \\ (0.1210)$	$0.3624 \\ (0.1014)$	0.3688 (0.1237)	0.3461 (0.0882)	$0.3536 \\ (0.1060)$
Intermediate Inputs	$\begin{array}{c} 0.5327 \\ (0.1076) \end{array}$	$0.5121 \\ (0.1257)$	$\begin{array}{c} 0.5326 \\ (0.1062) \end{array}$	$0.5130 \\ (0.1260)$	$0.5599 \\ (0.1069)$	$0.5347 \\ (0.1290)$	$0.5509 \\ (0.1061)$	$0.5304 \\ (0.1262)$	$0.5253 \\ (0.1028)$	0.4971 (0.1119)
Capital	$0.0497 \\ (0.0241)$	$0.0309 \\ (0.0193)$	$\begin{array}{c} 0.0498 \\ (0.0239) \end{array}$	0.0308 (0.0197)	0.0488 (0.0229)	$0.0304 \\ (0.0199)$	$0.0492 \\ (0.0220)$	$0.0304 \\ (0.0187)$	$\begin{array}{c} 0.0689 \\ (0.0369) \end{array}$	$0.0400 \\ (0.0274)$
Returns to scale	$0.9632 \\ (0.0228)$	$0.9304 \\ (0.0432)$	$0.9632 \\ (0.0226)$	0.9311 (0.0446)	$0.9635 \\ (0.0219)$	$0.9263 \\ (0.0457)$	$0.9625 \\ (0.0211)$	$0.9296 \\ (0.0411)$	$0.9403 \\ (0.0367)$	$\begin{array}{c} 0.8907 \\ (0.0683) \end{array}$
Number firms	20,866	$16,\!398$	20,866	$16,\!398$	20,155	15,484	21,396	17,164	54,482	38,295
Sample size	$118,\!057$	75,791	$118,\!057$	75,791	$120,\!173$	79,636	$137,\!556$	$93,\!408$	290,919	183,927
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Production Function Elasticities

Notes: The table presents economy-wide average elasticities, derived by estimating production functions at the 2-digit industry level and then calculating across-sector means weighted by the number of firms in each sector. Industries with fewer than 750 observations are excluded. The standard deviation of the distribution of sector-level elasticities, obtained via 30 bootstrap simulations, is reported in parentheses. For each bootstrap replicate, firms are sampled with replacement to match the original number of firms in each sector. Columns (1)-(8) include samples corresponding to firms classified as government contractors, whereas columns (9)-(10) encompass all Ecuadorian private firms. Columns (1) and (2) provide estimates from the specification in equation 26. Subsequent columns estimate production functions following equation 32. In columns (3)-(4), revenue from government sales by politically connected contractors in the years following their connection is deflated by a 6% government premium. Columns (5)-(6) omit observations from connected contractors in the years after establishing a link with the bureaucracy. Columns (7) through (10) do not adjust for the government premium. All specifications exclude firms that were acquired or created by a bureaucrat in office and those that formed their first political connection before 2000. The observation unit is contractor-year. Non-zero observations of each variable are winsorized at the 1st and 99th percentiles of their respective distributions. Dollar values are deflated by the consumer price index series computed by the World Bank (https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=EC). All regressions control for year fixed effects.

	Flexit	ole capital	Fixe	d capital
	OLS (1)	LP-Wooldridge (2)	OLS (3)	LP-Wooldridge (4)
Panel A: Main specification				
Excess Costs	$0.010 \\ (0.007)$	0.039^{***} (0.009)	$0.008 \\ (0.007)$	0.038^{***} (0.009)
Welfare cost (% of proc. budget)	$0.765 \\ (0.567)$	3.046^{***} (0.774)	$\begin{array}{c} 0.571 \\ (0.589) \end{array}$	$2.975^{***} \\ (0.777)$
Sample size	$118,\!057$	75,791	$118,\!057$	75,791
Panel B: Premium-adjusted revenue				
Excess Costs	0.018^{***} (0.006)	0.044^{***} (0.008)	0.016^{***} (0.005)	0.044^{***} (0.008)
Welfare cost (% of proc. budget)	1.433^{***} (0.464)	3.544^{***} (0.639)	$\begin{array}{c} 1.237^{***} \\ (0.450) \end{array}$	3.472^{***} (0.632)
Sample size	$118,\!057$	75,791	$118,\!057$	75,791
Panel C: Exclude political connection	years			
Excess Costs	0.015^{**} (0.007)	0.048^{***} (0.013)	0.018^{***} (0.007)	0.051^{***} (0.014)
Welfare cost (% of proc. budget)	1.259^{**} (0.609)	3.847^{***} (1.067)	1.41^{**} (0.617)	$\begin{array}{c} 4.052^{***} \\ (1.101) \end{array}$
Sample size	$120,\!173$	82,004	$123,\!553$	83,709
Panel D: No premium adjustment				
Excess Costs	$0.006 \\ (0.006)$	0.027^{***} (0.008)	$0.004 \\ (0.006)$	0.028^{***} (0.009)
Welfare cost (% of proc. budget)	$0.431 \\ (0.481)$	2.161^{***} (0.688)	$0.289 \\ (0.494)$	$2.159^{***} \\ (0.707)$
Sample size	$137,\!556$	93,408	$137,\!556$	93,408
Panel E: All firms				
Excess Costs	0.019^{**} (0.009)	0.051^{***} (0.015)	0.018^{**} (0.008)	0.052^{***} (0.014)
Welfare cost (% of proc. budget)	1.505^{**} (0.743)	$4.101^{***} (1.228)$	1.38^{**} (0.638)	$\begin{array}{c} 4.155^{***} \\ (1.165) \end{array}$
Sample size	$137,\!556$	93,408	$137,\!555$	93,408

Table 8: Social Excess Cost Estimates

Notes: The table reports excess cost estimates and corresponding welfare costs as percentage of the procurement budget. We estimate excess costs at the 2-digit industry level, and compute economy-wide averages using as weights the number of firms in each sector. Standard errors (in parenthesis) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 22, assuming that $\theta = 0$. Outcomes in Columns (1)–(2) assume flexible capital and are estimated as in equation 28. Specifications (3)–(4) assume fixed capital and are estimated via equation 30. All excess cost regressions control for year and 3-digit sector fixed effects. Panels differ on the estimation source for elasticities and TFPR. Panel A uses the sample of government contractors and the specification presented in equation 26. In Panel B, we deflate the revenue from government sales of politically connected contractors in the years following connection by a 6% government premium. In Panel C, we exclude observations after the connection for politically connected contractors for estimating the production function and make comparisons between contractors that will gain a political connection to never-treated contractors. Panel D makes no adjustment for the government premium. Panel E uses production function estimates obtained using the sample of all Ecuadorian private firms but makes welfare comparisons for contractors only. Panels B through E estimate production functions using equation 32. From all specifications, we exclude firms acquired or created by a bureaucrat already working in the public sector, and those that establish their first political connection before 2000. All panels compute then TFPR residuals for firms classified as government contractors and estimate excess costs and welfare costs on this sample. In the regressions with fixed capital, we correct the capital-revenue share of connected firms in Panels A and B by deflate the share of revenue from government sales of politically connected firms by a 6% government premium, while the other panels make no further adjustment. Differences in sample sizes between OLS and LP-Wooldridge come from the fact that LP-Wooldridge uses two years of lags as instruments. Panel C, D, and E use all years of data, while Panel A and B use only information from 2009 onward, as they require information on government contracts to adjust for the government premium. *** p<0.01, ** p<0.05, * p<0.1 61

	Fixed capital and LP-Wooldridge				
	(1)	(2)	(3)		
Panel A: Connected					
Excess Costs	0.068***	0.060***	0.073*		
Welfare cost (% of proc. budget)	$(0.024) \\ 5.723^{***} \\ (2.093)$	$(0.024) \\ 4.950^{***} \\ (2.027)$	(0.041) 6.225^{*} (3.556)		
Sample Size	74,955	69,487	30,044		
Panel B: Winner					
Excess Costs	-0.021^{***} (0.008)	-0.019^{***} (0.007)	-0.022 (0.019)		
Welfare cost (% of proc. budget)	-1.718^{***} (0.694)	-1.589^{***} (0.566)	-1.839 (1.612)		
Sample size	$74,\!955$	69,487	30,044		
Sector FE	Yes	Yes	Yes		
Year FE	Yes	Yes	No		
Agency FE	No	Yes	No		
Province FE	No	Yes	No		
Contract-Category FE	No	Yes	No		
Contract FE	No	No	Yes		

Table 9: Excess Cost Estimates (Contract-level) - Robustness

Notes: The table outlines excess cost estimates and the associated welfare costs as a percentage of the procurement budget at the contract-firm level. Excess costs are calculated at the 2-digit industry level, with economy-wide averages determined by weighting the number of contract-firm observations in each sector. Standard errors, reported in parentheses, derive from the same 30 bootstrap simulations used for calculating production function elasticities. Welfare costs are computed using equation 22, based on the assumption that $\theta = 0$. These outcomes are estimated utilizing the fixed capital framework of equation 30, leveraging estimates from the LP-Wooldridge and the primary model specified in equation 26. Column (1) includes year and 3-digit sector fixed effects. Column (2) further accounts for fixed effects associated with the purchasing agency, province, and contract category. Conversely, column (3) incorporates 3-digit sector and contract fixed effects. Panel A details the excess costs linked to purchasing from politically connected firms as opposed to non-connected ones. Panel B presents the welfare costs (or gains) associated with selecting non-connected winners over any losing firms. *** p<0.01, ** p<0.05, * p<0.1.

	Flexible Woold (1)	Fixed Woold (2)
Panel A: Only Large Reshuffles		
Excess Costs	.022*	.021*
	(.016)	(.016)
Welfare cost (% of proc. budget)	1.686	1.605
	(1.332)	(1.31)
Sample size	66,371	66,371
Panel B: Direct Only		
Excess Costs	.041***	.038***
	(.01)	(.01)
Welfare cost (% of proc. budget)	3.223^{***}	3.032^{***}
	(.791)	(.794)
Sample size	71,776	71,776
Panel C: Indirect Only		
Excess Costs	.034*	.038**
	(.022)	(.022)
Welfare cost (% of proc. budget)	2.616^{*}	2.985^{*}
	(1.793)	(1.843)
Sample size	64,440	64,440

Table 10: Excess Cost Estimates - Sample Definition Robustness

Notes: The table presents estimates of excess costs and related welfare costs as a percentage of the procurement budget, categorized by different definitions of the treatment sample. These excess costs are calculated at the 2-digit industry level, with economy-wide averages determined by weighting the number of firms within each sample-sector group. Standard errors, shown in parentheses, are derived from the same 30 bootstrap simulations utilized for calculating production function elasticities. Welfare costs are computed following equation 22, under the assumption that $\theta = 0$. Outcomes presented in Column (1) are based on the assumption of flexible capital and follow the estimation procedure outlined in equation 28. Specifications in Column (2) presume fixed capital, with estimations conducted as per equation 30. All regressions accounting for excess costs include controls for year and 3-digit sector fixed effects. Panel A is limited to firms that established connections during large reshuffles within government agencies. Panel B concentrates on firms with direct connections, where a bureaucrat is the owner. Panel C is focused on firms with indirect connections, involving ownership by a sibling. *** p<0.01, ** p<0.05, * p<0.1.

Internet Appendix

A Internet Appendix: Derivation of Propositions

This section presents proofs and derivations for Proposition 1 and 2. We assume that firms are cost-minimizing and face the following Lagrangian function:

$$\mathcal{L}(L_{it}, M_{it}, K_{it}, w_{st}, \rho_{st}, r_{st}, \lambda_{it}) = w_{st}L_{it} + \rho_{st}M_{it} + r_{st}K_{it} + \lambda_{it}\left(Q_{it} - L_{it}^{\alpha_l}M_{it}^{\alpha_m}K_{it}^{\alpha_k}exp(\omega_{it})\right).$$
(33)

Proof of Proposition 1 Assuming flexible capital, the quantity-conditional demand for intermediate inputs is given by:

$$M_{it}(Q_{it},\omega_{it},\boldsymbol{\alpha}) = \left(\frac{Q_{it}}{exp(\omega_{it})} \left(\frac{\alpha_m}{p_{st}}\right)^{(\alpha_l+\alpha_k)} \left(\frac{\alpha_l}{w_{st}}\right)^{-\alpha_l} \left(\frac{\alpha_k}{r_{st}}\right)^{-\alpha_k}\right)^{\frac{1}{\alpha_l+\alpha_m+\alpha_k}} = \left(\frac{Q_{it}}{exp(\omega_{it})}\right)^{\frac{1}{\alpha_l+\alpha_m+\alpha_k}} \tilde{\Gamma}_m,$$
(34)

where $\tilde{\Gamma}_m$ is a sector-yearly constant incorporating factor elasticities and sector-level prices. Following a similar derivation for labor and capital, the total cost function for each firm can be expressed as:

$$C_{it}(Q_{it},\omega_{it},\Gamma) = w_{st}L_{it} + \rho_{st}M_{it} + r_{st}K_{it}$$
$$= \left(\frac{Q_{it}}{exp(\omega_{it})}\right)^{\frac{1}{\alpha_l + \alpha_m + \alpha_k}} (\Gamma_l + \Gamma_m + \Gamma_k), \tag{35}$$

where $\Gamma_m = p_{st} \tilde{\Gamma}_m$, and analogously for Γ_l and Γ_k . Under constant returns to scale (CRTS) and taking derivatives with respect to quantity, we obtain:

$$\frac{\partial C_{it}(Q_{it},\omega_{it},\Gamma)}{\partial Q_{it}} = exp(\omega_{it})^{-1}(\Gamma_l + \Gamma_m + \Gamma_k).$$
(36)

Thus, a firm's cost function is linear in quantity, with a different slope depending on the productivity level. Finally, we use our definition for quality-adjusted marginal costs and the fact that under CRTS $\alpha_l + \alpha_m + \alpha_k = \sigma/(\sigma - 1)(\beta_l + \beta_m + \beta_k) = 1$ to obtain an expression for the quality-adjusted marginal costs

$$\frac{\partial C_{it}(Q_{it},\omega_{it},\Gamma)}{\partial \tilde{Q}_{it}} = \frac{\partial C_{it}(Q_{it},\omega_{it},\Gamma)}{\partial Q_{it}}exp(z_{it})^{-1}$$

$$= exp(\omega_{it}+z_{it})^{-\frac{(\sigma-1)}{\sigma(\beta_l+\beta_m+\beta_k)}}(\Gamma_l+\Gamma_m+\Gamma_k)$$

$$= exp(\frac{-\omega_{it}^*}{\beta_l+\beta_m+\beta_k})(\Gamma_l+\Gamma_m+\Gamma_k),$$
(37)

where $\omega_{it}^* = (\sigma - 1)/\sigma(\omega_{it} + z_{it}).$

For the excess costs due to political connections, the comparison between connected and non-connected firms within the same sector yields:

$$SOEC_{flex} = \frac{\partial C_{it}(Q_{it}^{con}, \omega_{it}^{*con}, \Gamma) / \partial \tilde{Q}_{it}}{\partial C_{it}(Q_{it}^{unc}, \omega_{it}^{*unc}, \Gamma) / \partial \tilde{Q}_{it}} - 1 = exp\left(\frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\right) - 1, \qquad (38)$$

highlighting that average excess costs can be estimated by within-sector differences in TFPR, as stated in Proposition 1.

Proof of Proposition 2 Assume now that the firm's capital cannot be freely adjusted, so that the quantity-conditional demand for intermediate inputs becomes

$$M_{it}(Q_{it}, K_{it}, \omega_{it}, \boldsymbol{\alpha}) = \left(\frac{Q_{it}}{K_{it}^{\alpha_k} exp(\omega_{it})} \left(\frac{\alpha_m w_{st}}{\alpha_l p_{st}}\right)^{\alpha_l}\right)^{\frac{1}{\alpha_l + \alpha_m}} \\ = \left(\frac{Q_{it}}{K_{it}^{\alpha_k} exp(\omega_{it})}\right)^{\frac{1}{\alpha_l + \alpha_m}} \tilde{\Lambda}_m,$$
(39)

with $\tilde{\Lambda}_m$ denoting a constant that collects the remaining sector-specific parameters of the model. Using a similar expression for labor, we can write the following cost function for variable inputs

$$C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda) = w_{st}L_{it} + \rho_{st}M_{it}$$
$$= \left(\frac{Q_{it}}{K_{it}^{\alpha_k}exp(\omega_{it})}\right)^{\frac{1}{\alpha_l + \alpha_m}} (\Lambda_l + \Lambda_m),$$
(40)

where $\Lambda_m = p_{st} \tilde{\Lambda}_m$, and similarly for labor. Assuming CRTS, the derivative of the cost function with respect to quantity is

$$\frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial Q_{it}} = \frac{1}{1 - \alpha_k} Q_{it}^{\frac{\alpha_k}{1 - \alpha_k}} \bar{K}_{it}^{-\frac{\alpha_k}{1 - \alpha_k}} exp(\omega_{it})^{-\frac{1}{1 - \alpha_k}} (\Lambda_l + \Lambda_m).$$
(41)

We can modify the previous equality by multiplying by weighted-average price $\overline{P}_{it}^{\frac{\alpha_k}{1-\alpha_k}}$ and quality $exp(z_{it})^{-\frac{1}{1-\alpha_k}}$ on both sides and get⁶⁶

$$\overline{P}_{it}^{\frac{\alpha_k}{1-\alpha_k}} exp(z_{it})^{-\frac{1}{1-\alpha_k}} \frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial Q_{it}} = \frac{1}{1-\alpha_k} R_{it}^{\frac{\alpha_k}{1-\alpha_k}} K_{it}^{-\frac{\alpha_k}{1-\alpha_k}} exp(\omega_{it}+z_{it})^{-\frac{1}{1-\alpha_k}} (\Lambda_l + \Lambda_m).$$
(42)

By definition, $\overline{P}_{it} = s_{it}P_{it}^{priv} + (1 - s_{it})P_{it}^{gov}$, for some s_{it} . Using the expression for government prices, we obtain $\overline{P}_{it} = P_{it}^{priv}[s_{it} + \tau_{st}(1 - s_{it})(1 + \mu_s PC_{it})]$. For brevity, let $v_{it} = [s_{it} + \tau_{st}(1 - s_{it})(1 + \mu_s PC_{it})]$, so $\overline{P}_{it} = v_{it}P_{it}^{priv}$. Applying our definition for quality-adjusted excess costs, the expression for weighted-average prices, and the fact that with

⁶⁶Weighted-average price is such that $R_{it} = \overline{P}_{it}Q_{it}$.

a CES demand function, $P_{it}^{priv} = \sigma/(\sigma - 1)c'(Q_{it})$. We get

$$v_{it}^{\frac{\alpha_k}{1-\alpha_k}} \frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial \tilde{Q}_{it}}^{\frac{1}{1-\alpha_k}} = R_{it}^{\frac{\alpha_k}{1-\alpha_k}} K_{it}^{-\frac{\alpha_k}{1-\alpha_k}} exp(\omega_{it} + z_{it})^{-\frac{1}{1-\alpha_k}} (\Lambda'_l + \Lambda'_m).$$
(43)

where $(\Lambda'_l + \Lambda'_m)$ captures new additional constant terms in the expression. The next step consists of solving for the marginal cost. Using the fact that under CRTS $\alpha_l + \alpha_m + \alpha_k = \sigma/(\sigma - 1)(\beta_l + \beta_m + \beta_k) = 1$, we can derive the following expression

$$\frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial \tilde{Q}_{it}} = v_{it}^{-\alpha_k} R_{it}^{\frac{\beta_k}{\beta_l + \beta_m + \beta_k}} K_{it}^{-\frac{\beta_k}{\beta_l + \beta_m + \beta_k}} exp(\omega_{it} + z_{it})^{-\frac{(\sigma-1)}{\sigma(\beta_l + \beta_m + \beta_k)}} (\Lambda'_l + \Lambda'_m)$$
$$= v_{it}^{-\alpha_k} R_{it}^{\frac{\beta_k}{\beta_l + \beta_m + \beta_k}} K_{it}^{-\frac{\beta_k}{\beta_l + \beta_m + \beta_k}} exp\left(\frac{-\omega_{it}^*}{\beta_l + \beta_m + \beta_k}\right) (\Lambda'_l + \Lambda'_m)$$
(44)

The final step is creating quality-adjusted marginal cost ratios between the connected and non-connected firms. Under the assumption of equal input costs within the sector, we can multiply and divide the marginal cost ratio by r_{st} to obtain an expression that is a function of $\bar{K}_{it} = r_{st}K_{it}$. Defining the capital-revenue share as $S_{it}^k = \bar{K}_{it}/R_{it}$, we obtain the following expression.

$$SOEC_{fixed} = \left(\frac{v_{it}^{unc}}{v_{it}^{con}}\right)^{\alpha_k} exp\left(\frac{\beta_k}{\beta_l + \beta_m + \beta_k} \left[ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})\right] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\right) - 1,$$
(45)

where $\omega_{it}^* = (\sigma - 1)/\sigma(\omega_{it} + z_{it})$.

Notice that, unlike the other components of the social excess cost, the multiplicative factor for prices v_{it} contains two possible unknowns τ_{st} and μ_s . While in principle, one may calibrate the equation with data, we performed simulations to study the behavior of such factor for reasonable values for the different components, reported below. In our setting, with low elasticity α , the ratio is close to one. For that reason, we write the expression for excess costs stated in Proposition 2,

$$SOEC_{fixed} \approx exp\left(\frac{\beta_k}{\beta_l + \beta_m + \beta_k} [ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\right) - 1.$$
(46)

A.1 Simulation of Ratio $(v^u/v^c)^{\alpha_k}$

We run simulations for the ratios in the following way. First, we define the ratio as

$$\left(\frac{v^{u}}{v_{c}}\right)^{\alpha_{k}} = \left(\frac{s^{u} + \tau_{s}(1 - s^{u}))}{s^{c} + \tau_{s}(1 - s^{c})(1 + \mu_{s})}\right)^{\alpha_{k}}.$$
(47)

We assume that connected firms have a greater share of their price accounted by government prices. That is, we assume $0 \le 1 - s^u < 1 - s^c \le 1$, and in particular, $s^u = s^c + 0.1$.⁶⁷ Lastly, we explore how the ratio changes for different ranges in s^u, s^c , as well as different

⁶⁷We studied the behavior of the ratio of values of $s^u = s^c + 0.25$ and $s^u = s^c + 0.5$ and the findings are relatively unchanged.

assumptions on the proportionality factor $\tau_s = \{0.5, 0.8, 1.5\}$, political connection premium $1 + m_s = \{1, 1.05, 1.25\}$, and capital elasticity of output $\alpha_k = \{0.05, 0.1, 0.3\}$. All of these cover realistic combinations of parameters, relevant in developing and higher-income countries.

Internet Appendix Table IA1 presents the results. In our case, given that the output of firms is mostly for the private market, the share s^u and s^c representing the share of the average firm-level price coming from private prices will likely be above 30%. Moreover, the political premium markup is close to 5%, the capital elasticity of output will be close to 0.05, while the proportionality factor for government to private prices is such that government prices are below private ones (we calibrate $\tau_s = 0.8$). With all of these values, the ratio will be close to 1 with differences up to 0.1%, as shown in the highlighted areas in the table.

The column "Flag" indicates if the difference is greater than 1%. In general, the ratio of multiplicative factor is close to one, with differences being mostly smaller than 1%, for elasticity estimates $0 < \alpha_k < 0.15$, unless government prices show 50% differences relative to private prices and the political connections markup is large (above 20%). Even then, the ratio implies differences of at most 5%.⁶⁸ For greater elasticities, $\alpha_k = 0.3$, the ratio is close to 1, but with differences of up to 10%.⁶⁹ Note that for developing countries, capital elasticities will tend to be small Demirer (2020), so our expression for social excess cost below will not impose significant restrictions.

⁶⁸For $\alpha_k < 0.3$, the distribution of ratios is the following: p10=0.985, p50=0.998, p90=1.005. ⁶⁹For $\alpha_k = 0.3$, the distribution of ratios is the following: p10=0.947, p50=0.992, p90=1.015.

Table IA1:	Simulation	Ratios	$(v^u/v^c)^{\alpha_k}$

V.	s^u	$(1 - s^{u})$	s^{c}	$(1 - s^{c})$	τ_s	$1 + \mu_s$	α_k	$(v^u/v^c)^{\alpha_k}$	Flag
1	0	1	0	1	0.8	1.05	0.05	0.998	0
1	0.2	0.8	0.1	0.9	0.8	1.05	0.05	0.999	0
1	0.4	0.6	0.3	0.7	0.8	1.05	0.05	1	0
1	0.6	0.4	0.5	0.5	0.8	1.05	0.05	1	0
1	0.8	0.2	0.7	0.3	0.8	1.05	0.05	1	0
1	1	0	0.9	0.1	0.8	1.05	0.05	1.001	0
2	0	1	0	1	0.8	1.25	0.05	0.989	1
2	0.2	0.8	0.1	0.9	0.8	1.25	0.05	0.991	0
2	0.4	0.6	0.3	0.7	0.8	1.25	0.05	0.994	0
2	0.6	0.4	0.5	0.5	0.8	1.25	0.05	0.996	0
2	0.8	0.2	0.7	0.3	0.8	1.25	0.05	0.998	0
2	1	0	0.9	0.1	0.8	1.25	0.05	1	0
3	0	1	0	1	0.8	1	0.05	1	0
3	0.2	0.8	0.1	0.9	0.8	1	0.05	1.001	0
3	0.4	0.6	0.3	0.7	0.8	1	0.05	1.001	0
3	0.6	0.4	0.5	0.5	0.8	1	0.05	1.001	0
3	0.8	0.2	0.7	0.3	0.8	1	0.05	1.001	0
3	1	0	0.9	0.1	0.8	1	0.05	1.001	0
4	0	1			0.5	1.05	0.05	0.998	0
4	0.2	0.8	0.1	0.9	0.5	1.05	0.05	1.002	0
4	0.4	0.0	0.3	0.7	0.5	1.05	0.05	1.002	0
4	0.0	0.4	0.5	0.5	0.5	1.05	0.05	1.002	
4	1.0	0.2	0.7	0.5	0.5	1.05	0.05	1.002	0
-4 -5	1	1	0.9	0.1	0.5	1.05	0.05	0.080	0
5	0.2	0.8			0.5	1.25	0.05	0.989	0
5	0.2	0.8	0.1	0.9	0.5	1.25	0.05	0.995	0
5	0.4	0.0	0.5	0.5	0.5	1.25	0.05	0.991	
5	0.0	0.4	0.5	0.3	0.5	1.25	0.05	1.001	0
5	1	0.2	0.7	0.5	0.5	1.25	0.05	1.001	
6	0	1	0.0	1	0.5	1.20	0.05	1.002	0
6	0.2	0.8	01	0.0	0.5	1	0.05	1 004	0
6	0.4	0.6	0.1	0.7	0.5	1	0.05	1.004	ŏ
6	0.4	0.4	0.5	0.5	0.5	1	0.05	1.004	0
6	0.8	0.2	0.7	0.3	0.5	1	0.05	1.003	ő
6	1	0	0.9	0.1	0.5	1	0.05	1.003	Õ
7	0	1	0	1	1.5	1.05	0.05	0.998	0
7	0.2	0.8	0.1	0.9	1.5	1.05	0.05	0.996	0
7	0.4	0.6	0.3	0.7	1.5	1.05	0.05	0.996	0
7	0.6	0.4	0.5	0.5	1.5	1.05	0.05	0.996	0
7	0.8	0.2	0.7	0.3	1.5	1.05	0.05	0.997	0
7	1	0	0.9	0.1	1.5	1.05	0.05	0.997	0
8	0	1	0	1	1.5	1.25	0.05	0.989	1
8	0.2	0.8	0.1	0.9	1.5	1.25	0.05	0.988	1
8	0.4	0.6	0.3	0.7	1.5	1.25	0.05	0.989	1
8	0.6	0.4	0.5	0.5	1.5	1.25	0.05	0.991	0
8	0.8	0.2	0.7	0.3	1.5	1.25	0.05	0.993	0
8	1	0	0.9	0.1	1.5	1.25	0.05	0.996	0
9	0	1	0	1	1.5	1	0.05	1	0
9	0.2	0.8	0.1	0.9	1.5	1	0.05	0.998	0
9	0.4	0.6	0.3	0.7	1.5	1	0.05	0.998	0
9	0.6	0.4	0.5	0.5	1.5		0.05	0.998	0
9	0.8	0.2	0.7	0.3	1.5	1	0.05	0.998	0
9	1	0	0.9	0.1	1.5	1	0.05	0.998	0
10	0	1			0.8	1.05	0.1	0.995	
10	0.2	0.8	0.1	0.9	0.8	1.05	0.1	0.998	0
10	0.4	0.6	0.3	0.7	0.8	1.05	0.1	0.999	
10	0.6	0.4	0.5	0.5	0.8	1.05	0.1	1 001	
10	0.8	0.2	0.7	0.3	0.8	1.05	0.1	1.001	
10	1	1	0.9	1	0.0	1.00	0.1	0.079	1
	0	1			0.8	1.20	0.1	0.978	
11	0.2	0.0	0.1	0.9	0.0	1.20	0.1	0.965	
	0.4	0.0	0.5	0.5	0.8	1.25	0.1	0.907	
	0.8	0.4	0.7	0.3	0.8	1.25	0.1	0.996	
11	1	0	0.9	0.1	0.8	1.25	0.1	1	l õ l
	-	~	0.0		0.0	1.20	···+	÷ .	- ×

ν.	s^{a}	$(1 - s^{a})$	s^{c}	$(1 - s^{c})$	τ_s	$1 + \mu_s$	α_k	$(v^a/v^c)^{\alpha_k}$	Flag
12	0	1	0	1	0.8	1	0.1	1	0
12	0.2	0.8	0.1	0.9	0.8	1	0.1	1.002	0
12	0.4	0.6	0.3	0.7	0.8	1	0.1	1.002	0
12	0.6	0.4	0.5	0.5	0.8	1	0.1	1.002	ň
12	0.0	0.4	0.0	0.0	0.0	1	0.1	1.002	ő
12	0.8	0.2	0.7	0.5	0.8	1	0.1	1.002	0
12	1	0	0.9	0.1	0.8	1	0.1	1.002	0
13	0	1	0	1	0.5	1.05	0.1	0.995	0
13	0.2	0.8	0.1	0.9	0.5	1.05	0.1	1.005	0
13	0.4	0.6	0.3	0.7	0.5	1.05	0.1	1.005	0
13	0.6	0.4	0.5	0.5	0.5	1.05	0.1	1.005	ň
19	0.0	0.4	0.0	0.0	0.5	1.05	0.1	1.005	ő
10	0.8	0.2	0.7	0.5	0.5	1.05	0.1	1.005	0
13	1	0	0.9	0.1	0.5	1.05	0.1	1.005	0
14	0	1	0	1	0.5	1.25	0.1	0.978	1
14	0.2	0.8	0.1	0.9	0.5	1.25	0.1	0.99	1
14	0.4	0.6	0.3	0.7	0.5	1.25	0.1	0.995	0
1.4	0.6	0.4	0.5	0.5	0.5	1.95	0.1	0.009	ő
14	0.0	0.4	0.5	0.5	0.5	1.25	0.1	0.998	0
14	0.8	0.2	0.7	0.3	0.5	1.25	0.1	1.001	0
14	1	0	0.9	0.1	0.5	1.25	0.1	1.004	0
15	0	1	0	1	0.5	1	0.1	1	0
15	0.2	0.8	0.1	0.9	0.5	1	0.1	1.009	0
15	0.4	0.6	0.3	0.7	0.5	1	0.1	1.007	Ó.
15	0.4	0.0	0.5	0.7	0.5	1	0.1	1.007	0
15	0.0	0.4	0.5	0.5	0.5	1	0.1	1.000	0
15	0.8	0.2	0.7	0.3	0.5	1	0.1	1.006	0
15	1	0	0.9	0.1	0.5	1	0.1	1.005	0
16	0	1	0	1	1.5	1.05	0.1	0.995	0
16	0.2	0.8	0.1	0.9	1.5	1.05	0.1	0.992	0
16	0.4	0.6	0.3	0.7	1.5	1.05	0.1	0.992	ň
16	0.4	0.0	0.5	0.7	1.0	1.05	0.1	0.002	ő
10	0.6	0.4	0.5	0.5	1.5	1.05	0.1	0.993	0
16	0.8	0.2	0.7	0.3	1.5	1.05	0.1	0.994	0
16	1	0	0.9	0.1	1.5	1.05	0.1	0.994	0
17	0	1	0	1	1.5	1.25	0.1	0.978	1
17	0.2	0.8	0.1	0.9	1.5	1.25	0.1	0.976	1
17	0.4	0.6	0.1	0.7	1.5	1.25	0.1	0.070	1
17	0.4	0.0	0.5	0.7	1.0	1.20	0.1	0.979	1
17	0.6	0.4	0.5	0.5	1.5	1.25	0.1	0.982	1
17	0.8	0.2	0.7	0.3	1.5	1.25	0.1	0.986	1
17	1	0	0.9	0.1	1.5	1.25	0.1	0.992	0
18	0	1	0	1	1.5	1	0.1	1	0
18	0.2	0.8	0.1	0.9	1.5	1	0.1	0.996	Ó.
19	0.4	0.6	0.2	0.7	1.5	1	0.1	0.006	ŏ
10	0.4	0.0	0.5	0.7	1.5	1	0.1	0.990	0
18	0.6	0.4	0.5	0.5	1.5	1	0.1	0.996	0
18	0.8	0.2	0.7	0.3	1.5	1	0.1	0.996	0
18	1	0	0.9	0.1	1.5	1	0.1	0.995	0
19	0	1	0	1	0.8	1.05	0.15	0.993	0
19	0.2	0.8	0.1	0.0	0.8	1.05	0.15	0.997	0
10	0.4	0.0	0.1	0.5	0.0	1.05	0.15	0.000	ő
19	0.4	0.0	0.5	0.7	0.8	1.05	0.15	0.999	0
19	0.6	0.4	0.5	0.5	0.8	1.05	0.15		0
19	0.8	0.2	0.7	0.3	0.8	1.05	0.15	1.001	0
19	1	0	0.9	0.1	0.8	1.05	0.15	1.002	0
20	0	1	0	1	0.8	1.25	0.15	0.967	1
20	0.2	0.8	0.1	0.9	0.8	1.25	0.15	0.974	1
20	0.4	0.6	0.3	0.7	0.8	1.20	0.15	0.081	1
20	0.4	0.0	0.0	0.7	0.0	1.20	0.15	0.301	1
20	0.0	0.4	0.0	0.0	0.8	1.20	0.15	0.988	
20	0.8	0.2	0.7	0.3	0.8	1.25	0.15	0.994	0
20	1	0	0.9	0.1	0.8	1.25	0.15	1	0
21	0	1	0	1	0.8	1	0.15	1	0
21	0.2	0.8	01	0.9	0.8	1	0.15	1 004	0
91	0.4	0.6	0.1	0.7	0.0	1	0.15	1.004	l õ
21	0.4	0.0	0.3	0.7	0.0	1	0.15	1.005	
21	0.6	0.4	0.5	0.5	0.8		0.15	1.003	0
21	0.8	0.2	0.7	0.3	0.8	1	0.15	1.003	0
21	1	0	0.9	0.1	0.8	1	0.15	1.003	0
22	0	1	0	1	0.5	1.05	0.15	0.993	0
22	0.2	0.8	01		0.5	1.05	0.15	1.007	Ň
22	0.2	0.0	0.1	0.9	0.5	1.05	0.15	1.007	
22	0.4	0.6	0.3	0.7	0.5	1.05	0.15	1.007	0
22	0.6	0.4	0.5	0.5	0.5	1.05	0.15	1.007	0
22	0.8	0.2	0.7	0.3	0.5	1.05	0.15	1.007	0
22	1	0	0.9	0.1	0.5	1.05	0.15	1.007	0
	-	, v	0.0		0.0		0.20		, v

V.	s^u	$(1 - s^u)$	s^{c}	$(1 - s^{c})$	τ_s	$1 + \mu_s$	α_k	$(v_{st}^c/v_{st}^u)^{a_k}$	Flag
23	0	1	0	1	0.5	1.25	0.15	0.967	1
23	0.2	0.8	0.1	0.9	0.5	1.25	0.15	0.985	1
23	0.4	0.6	0.3	0.7	0.5	1.25	0.15	0.992	0
23	0.6	0.4	0.5	0.5	0.5	1.25	0.15	0.998	0
23	0.8	0.2	0.7	0.3	0.5	1.25	0.15	1.002	
23	1	0	0.9	0.1	0.5	1.25	0.15	1.006	0
24		1		1	0.5	1	0.15	1	
24	0.2	0.8	0.1	0.9	0.5	1	0.15	1.013	1
24	0.4	0.0	0.5	0.7	0.5	1	0.15	1.011	1
24	0.0	0.4	0.5	0.5	0.5	1	0.15	1.01	
24	1	0.2	0.7	0.5	0.5	1	0.15	1.009	
24	0	1	0.3	1	1.5	1.05	0.15	0.993	0
25	0.2	0.8	01	0.0	1.5	1.05	0.15	0.988	1
25	0.4	0.6	0.3	0.7	1.5	1.05	0.15	0.989	1
25	0.6	0.4	0.5	0.5	1.5	1.05	0.15	0.989	1
25	0.8	0.2	0.7	0.3	1.5	1.05	0.15	0.99	1
25	1	0	0.9	0.1	1.5	1.05	0.15	0.992	0
26	0	1	0	1	1.5	1.25	0.15	0.967	1
26	0.2	0.8	0.1	0.9	1.5	1.25	0.15	0.964	1
26	0.4	0.6	0.3	0.7	1.5	1.25	0.15	0.968	1
26	0.6	0.4	0.5	0.5	1.5	1.25	0.15	0.973	1
26	0.8	0.2	0.7	0.3	1.5	1.25	0.15	0.98	1
26	1	0	0.9	0.1	1.5	1.25	0.15	0.987	1
27	0	1	0	1	1.5	1	0.15	1	0
27	0.2	0.8	0.1	0.9	1.5	1	0.15	0.995	0
27	0.4	0.6	0.3	0.7	1.5	1	0.15	0.994	0
27	0.6	0.4	0.5	0.5	1.5	1	0.15	0.994	0
27	0.8	0.2	0.7	0.3	1.5	1	0.15	0.993	0
27	1	0	0.9	0.1	1.5	1	0.15	0.993	0
28	0	1	0	1	0.8	1.05	0.3	0.985	1
28	0.2	0.8	0.1	0.9	0.8	1.05	0.3	0.994	0
28	0.4	0.6	0.3	0.7	0.8	1.05	0.3	0.997	
28	0.0	0.4	0.5	0.5	0.8	1.05	0.3	1 1 0 0 2	
20	0.8	0.2	0.7	0.5	0.0	1.05	0.5	1.005	
20	1	1	0.9	0.1	0.8	1.05	0.3	0.035	1
29	0.2	0.8	0.1	0.0	0.8	1.25	0.3	0.935	1
20	0.4	0.6	0.1	0.7	0.8	1.25	0.0	0.949	1
29	0.6	0.4	0.5	0.5	0.8	1.25	0.3	0.975	1
29	0.8	0.2	0.7	0.3	0.8	1.25	0.3	0.988	1
29	1	0	0.9	0.1	0.8	1.25	0.3	1	0
30	0	1	0	1	0.8	1	0.3	1	0
30	0.2	0.8	0.1	0.9	0.8	1	0.3	1.007	0
30	0.4	0.6	0.3	0.7	0.8	1	0.3	1.007	0
30	0.6	0.4	0.5	0.5	0.8	1	0.3	1.007	0
30	0.8	0.2	0.7	0.3	0.8	1	0.3	1.006	0
30	1	0	0.9	0.1	0.8	1	0.3	1.006	0
31	0	1	0	1	0.5	1.05	0.3	0.985	1
31	0.2	0.8	0.1	0.9	0.5	1.05	0.3	1.014	
31	0.4	0.6	0.3	0.7	0.5	1.05	0.3	1.014	1
31	0.6	0.4	0.5	0.5	0.5	1.05	0.3	1.015	1
31	0.8	0.2	0.7	0.3	0.5	1.05	0.3	1.015	1
31	1	0	0.9	0.1	0.5	1.05	0.3	1.015	1
32		1			0.5	1.25	0.3	0.935	
32	0.2	0.8	0.1	0.9	0.5	1.20	0.3	0.971	
32	0.4	0.0	0.5	0.7	0.5	1.20	0.3	0.964	
32	0.0	0.4	0.5	0.3	0.5	1.20	0.3	1 004	
32	1	0.2	0.1	0.1	0.5	1.25	0.3	1.012	1
33	0	1	0	1	0.5	1	0.3	1.012	0
33	0.2	0.8	0.1	0.9	0.5	1	0.3	1.026	Ĭ
33	0.4	0.6	0.3	0.7	0.5	1	0.3	1.022	1
33	0.6	0.4	0.5	0.5	0.5	1	0.3	1.02	1
33	0.8	0.2	0.7	0.3	0.5	1	0.3	1.017	1
33	1	0	0.9	0.1	0.5	1	0.3	1.016	1

Table IA1: Simulation Ratios $(v^u/v^c)^{\alpha_k}$ [Continued]

V.	s^u	$(1 - s^{u})$	s^{c}	$(1 - s^{c})$	τ_s	μ_s	α_k	$(v_{st}^c/v_{st}^u)^{a_k}$	Flag
34	0	1	0	1	1.5	1.05	0.3	0.985	1
34	0.2	0.8	0.1	0.9	1.5	1.05	0.3	0.976	1
34	0.4	0.6	0.3	0.7	1.5	1.05	0.3	0.977	1
34	0.6	0.4	0.5	0.5	1.5	1.05	0.3	0.979	1
34	0.8	0.2	0.7	0.3	1.5	1.05	0.3	0.981	1
34	1	0	0.9	0.1	1.5	1.05	0.3	0.983	1
35	0	1	0	1	1.5	1.25	0.3	0.935	1
35	0.2	0.8	0.1	0.9	1.5	1.25	0.3	0.929	1
35	0.4	0.6	0.3	0.7	1.5	1.25	0.3	0.937	1
35	0.6	0.4	0.5	0.5	1.5	1.25	0.3	0.947	1
35	0.8	0.2	0.7	0.3	1.5	1.25	0.3	0.96	1
35	1	0	0.9	0.1	1.5	1.25	0.3	0.975	1
36	0	1	0	1	1.5	1	0.3	1	0
36	0.2	0.8	0.1	0.9	1.5	1	0.3	0.99	1
36	0.4	0.6	0.3	0.7	1.5	1	0.3	0.989	1
36	0.6	0.4	0.5	0.5	1.5	1	0.3	0.988	1
36	0.8	0.2	0.7	0.3	1.5	1	0.3	0.987	1
36	1	0	0.9	0.1	1.5	1	0.3	0.985	1

Notes: The table simulation ratios $(v^u/v^c)^{\alpha_k}$ measuring the approximation error in equation 24 $(SOEC_{fixed})$ under different assumptions capital elasticity α_k , political connection markups μ_s , government proportionality factor τ_s , private price shares for connected and unconnected s^c and s^u . The column "Flag" is equal to one when approximation error is greater than 1%.

B Internet Appendix: Data Construction

B.1 Identifying Families

We identify families using the universe of people in the individual tax-income data for the years 2007-2015 and the bureaucratic and shareholder databases, which covers years 2000-2017. We observe over 5.3 million different individuals and classify them into 1.3 million different families. To have a sense of proportionality, in 2017, 12.4 million people were eligible to vote (i.e., people over 16 years of age). Given the large informal economy (around 45 percent according to surveys conducted by the Ecuadorian Statistical Institute), we actually cover a substantial share of the formal population.

To determine family links, we considered that two persons are part of the same family if they share their first and second last names (ordered). Note that using the first two words in a name string as the last names could misclassify families. Given last name conventions in Hispanic countries, compounded last-names as "De la Torre" are actually just one last name rather than three. For this purpose, we have to identify which words in a name belonged to each of the individual's last names. We separate the names into different words and consider as one last name all the combination of words that started with "De la", "Del", "De los", "Di", "San", "Von" and "Van der", etc. As there are other combinations of compound last names, we manually imputed together words that are consistently repeated in the same order for more than three people. This allows us to identify the first and second last names of each person with higher accuracy.

B.2 Family Size CDF





Notes: The figure shows the cumulative distribution of family size computed using the two last names of the individuals in our data, as well as the distribution of number of children obtained from the 2010 Census data available from IPUMS (https://international.ipums.org/international/index.shtml). For the distribution based on the individuals in our data, families are constructed combining the sample of individuals in the IRS data, firms' owners registry, and bureaucrat registry. The distributions are truncated at the 99th percentile.

C Internet Appendix: Reallocation of Contracts - Additional Results, Robustness and Falsifications
	Callaway- Sant'Anna (1)	Callaway- Sant'Anna (II) (2)	Sun-Abraham (3)	de Chaisemartin- D'Haultfoeuille (4)	Two-way Fixed Effects (5)
Panel A: Probability of	of Winning a Co	ntract (Extensive	Margin)		
After first political connection	0.0260^{***} (0.0096)	0.0259^{***} (0.0096)	0.0260^{***} (0.0096)	$\begin{array}{c} 0.0179^{***} \\ (0.0065) \end{array}$	$\begin{array}{c} 0.0268^{***} \\ (0.0054) \end{array}$
Panel B: Total Value	of Contracts (In	tensive Margin)			
After first political connection	$\begin{array}{c} 0.3511^{***} \\ (0.1130) \end{array}$	$\begin{array}{c} 0.3486^{***} \\ (0.1129) \end{array}$	$\begin{array}{c} 0.3519^{***} \\ (0.1124) \end{array}$	$\begin{array}{c} 0.2479^{***} \\ (0.0722) \end{array}$	$\begin{array}{c} 0.3912^{***} \\ (0.0643) \end{array}$
Panel C: # of Contra	acts				
After first political connection	$\begin{array}{c} 0.0870^{***} \\ (0.0187) \end{array}$	$\begin{array}{c} 0.0865^{***} \\ (0.0187) \end{array}$	$\begin{array}{c} 0.0881^{***} \\ (0.0187) \end{array}$	0.0600^{***} (0.0122)	$\begin{array}{c} 0.0811^{***} \\ (0.0106) \end{array}$

Table IA2: Probability of Being Awarded a Contract - Sensitivity to Specification

Notes: The table reports different estimated coefficients for the effect of political connection on the allocation of contracts. Each column title describes the methodology used to obtain the point estimate. Column (1) uses Callaway and Sant'Anna (2021) relying on never-treated firms as the control group, Column (2) uses Callaway and Sant'Anna (2021) relying only on never-treated and yet-to-be-treated firms as the control group, Column (3) uses Sun and Abraham (2021) relying on both last-to-be-treated (cohort 2017) and never-treated firms as the control group, Column (4) presents estimates from de Chaisemartin and D'Haultfoeuille (2020), and Column (5) presents a usual two-way fixed-effect estimate. Panel A shows the probability of winning a contract. The dependent variable is equal to one when the value of contracts won in a given year is larger than US \$3,000, which roughly corresponds to the 10th percentile of the yearly contract value distribution for firms winning a non-zero number of contracts. Panel B has the dependent variable as the (Inverse Hyperbolic Sine Transformation) value of all contracts awarded in a given year. Panel C has the dependent variable as the (Inverse Hyperbolic Sine Transformation) number of contracts won in a given year. The number of observations is 180,573 with 26,620 unique contractors, out of which 4,841 have a political connection. The mean probability of winning a contract is 19.5% before treatment for treated firms, the average log total value of contracts is 2.29, and the average log number of contracts is 0.274. Standard errors (in parentheses) are as follows: columns (1)-(3) efficient standard errors from Roth and Sant'Anna (2021), column (4) from 30 bootstrap simulations, and column (5) robust standard errors clustered at the firm-level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Figure IA2: Distribution of Government Contracts by Type of Connection



Notes: This figure presents the distribution of the log total value of contracts at the firm-year level, excluding observations for which the total value is zero. The figure shows the distributions for non-connected firms, for firms connected *prior* to the connection, and for firms connected *after* the connection.



Figure IA3: Event-studies and Large Reshuffles (LR)

Notes: This figure presents the coefficients for event-studies for winning government procurement contracts on the firm's first political connection using the methodology of Callaway and Sant'Anna (2021) relying on never treated as control. The figures show separately the effect of connections stemming from large government reshuffles vs. those that do not come from large reshuffles. Subfigures A, B, and C show the effects of connections stemming from large government reshuffles, while Subfigures D, E, and F show event-studies for all other connections. Subfigures A and D show the probability of winning a contract. The dependent variable is equal to one when the value of contracts won in a given year is larger than US \$3,000, which roughly corresponds to the 10th percentile of the yearly contract value distribution for firms winning a non-zero number of contracts. Subfigures B and E have the (Inverse Hyperbolic Sine Transformation) value of all contracts awarded in a given year as a dependent variable. Subfigures C and F have the (Inverse Hyperbolic Sine Transformation) number of contracts won in a given year as a dependent variable. We set the year prior to the first connection (-1) as the omitted category. The sample is the set of firms classified as government contractors (see Section 2.2.1). The unit of observation is contractor-year. We include only years in which a contractor files balance sheet information. We exclude firms created or acquired by bureaucrats, and firms that established the first political connection before 2000. Error bars indicate 90 and 95% confidence intervals with efficient standard errors from Roth and Sant'Anna (2021). The dotted line shows the sample mean in the years before the event, and each coefficient is shifted by this constant.

			Pan By Type	el A of Sector		
	Tradable - Ext. (1)	Tradable - Int. (2)	Wholesale - Ext. (3)	Wholesale - Int. (4)	Non- Tradable - Ext. (5)	Non- Tradable- Int. (6)
After first political connection	$0.0304 \\ (0.0313)$	$\begin{array}{c} 0.3573 \\ (0.3626) \end{array}$	$\begin{array}{c} 0.0125\\ (0.0181) \end{array}$	$0.1446 \\ (0.2111)$	$\begin{array}{c} 0.0251^{**} \\ (0.0122) \end{array}$	$\begin{array}{c} 0.3130^{**} \\ (0.1446) \end{array}$
Sample Size	$23,\!689$	$23,\!689$	67,628	67,628	89,395	89,395
Number contractors	$3,\!444$	3,444	9,850	9,850	14,366	$14,\!366$
Connected contractors	436	436	1,206	1,206	3,021	3,021
Mean before connection	0.156	1.987	0.212	2.757	0.195	2.422
			By I	Pan Level of Secto	el B ral Concentra	tion
			Low - Ext. (1)	Low - Int. (2)	High - Ext. (3)	High - Int. (4)
After first political connection			$\begin{array}{c} 0.0276^{***} \\ (0.0099) \end{array}$	$\begin{array}{c} 0.3841^{***} \\ (0.1165) \end{array}$	0.0049 (0.0406)	-0.0952 (0.4554)
Sample Size			166,674	166,674	14,038	14,038
Number contractors			$25,\!399$	$25,\!399$	2,260	2,260
Connected contractors			4,339	4,339	323	323
Mean before connection			0.198	2.485	0.168	2.212

Table IA3: Probability of Being Awarded a Contract - Sector and Concentration

Notes: ChatGPT ChatGPT The table reports the extensive and intensive reallocative effects by sector, using the methodology of Callaway and Sant'Anna (2021) with never-treated firms as controls. In Panel A, we explore heterogeneity by classifying sectors into i) Tradables, ii) Wholesale and Retail trade, and iii) Non-tradables, as in Caliendo et al. (2018). In Panel B, we explore heterogeneity by industrial concentration above (High) or below (Low) the median sectoral Herfindahl-Hirschman Index. Columns with the identifier *Ext.* explore the extensive margin defined as the probability of winning a contract. The dependent variable is equal to one when the value of contracts won in a given year is larger than US \$3,000, which roughly corresponds to the 10th percentile of the yearly contract value distribution for firms winning a non-zero number of contracts. Instead, columns with the identifier *Int.* explore the intensive margin, defined as the (Inverse Hyperbolic Sine Transformation) value of all contracts awarded in a given year. Efficient standard errors from Roth and Sant'Anna (2021) in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table IA4: Firm Dynamics

	r	r^{priv}	r^{gov}	l	m	k	π/R	s_m^{-1}	ω*	R/Assets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After first political connection	$0.0309 \\ (0.0283)$	$0.0358 \\ (0.0314)$	0.3511^{**} (0.1130)	$^{*0.0347}_{(0.0363)}$	0.0241 (0.0351)	-0.0219 (0.0293)	0.0059 (0.0065)	0.0091 (0.0202)	-0.0145 (0.0165)	$0.0225 \\ (0.0686)$
Sample Size	149,391	144,510	180,573	140,651	148,268	$135,\!247$	132,729	145,464	87,436	$177,\!133$
Number contractors	$27,\!659$	$27,\!659$	$26,\!620$	$27,\!659$	$27,\!659$	$27,\!659$	$27,\!659$	$27,\!659$	$27,\!659$	$27,\!659$
Connected contractors	$4,\!662$	$4,\!662$	4,841	$4,\!662$	$4,\!622$	$4,\!662$	$4,\!662$	$4,\!662$	$4,\!662$	4,662
Mean before connection	11.58	11.51	2.45	10.23	10.77	9.51	0.15	0.81	1.89	2.364

Notes: The table reports aggregated treatment effects of the first political connection on various firm-level variables using the methodology of Callaway and Sant'Anna (2021). The dependent variables are defined as follows: (1) $r \equiv log(revenue)$, (2) $r^{priv} \equiv log(total revenue net of government sales)$, (3) $r^{gov} = IHS(totalvaluecontracts)$, (4) $l \equiv log(wages)$, (5) $m \equiv log(inputs)$, (6) $k \equiv log(capital)$, (7) $\pi/R \equiv (Revenue - wages - inputs)/Revenue$, (8) $s_m^{-1} \equiv r - m$, (9) $omega^*$ is estimated TFPR from our fourth specification that does not control for political connection status, and (10) $R/Assets \equiv revenue/assets$. In all the regressions, the control group includes never-treated firms. Standard errors are clustered at the firm-level. *** p<0.01, ** p<0.05, * p<0.1.



Figure IA4: Firm Dynamics Before and After Political Connection

Notes: This figure presents the coefficients from event-study analyses of various firm observables on the firm's first political connection, utilizing the methodology of Callaway and Sant'Anna (2021) and relying on never-treated firms as the control group. The dependent variables are defined as follows: (a) $r \equiv log(revenue)$, (b) $r^{priv} \equiv log(total revenue net of government sales), (c) <math>l \equiv log(wages)$, (d) $m \equiv log(inputs)$, (e) $k \equiv log(capital)$, (f) $\pi/R \equiv (Revenue - wages - inputs)/Revenue$, (g) $s_m^{-1} \equiv r - m - l$, (h) omega^{*} is estimated TFPR from our fourth specification that does not control for political connection status, and (i) $R/Assets \equiv revenue/assets$. The sample includes firms classified as government contractors (see Section 2.2.1). The unit of observation is contractor-year, including only years in which a contractor files balance sheet information. Firms created or acquired by bureaucrats, and those that established the first political connection before 2000, are excluded from the analysis. Error bars indicate 90% and 95% confidence intervals, with standard errors clustered at the firm level. The dotted line represents the sample mean in the years before the event, and each coefficient is adjusted by this constant for presentation.

D Internet Appendix: Empirical evidence for model assumptions

D.1 Government Demand, Efficiency and Quality

Previous work, such as that by Kroft et al. (2020), has documented the assumption that government demand increases with a firm's efficiency. However, it is not necessarily clear that government demand also increases with a firm's quality. In this section, we provide empirical evidence to support the assertion that firm-level government demand increases with proxies for both firm efficiency and quality.

Under the assumption of Constant Elasticity of Substitution (CES) for *private* demand, private prices reflect the firm's efficiency through marginal costs. Indeed, firms with lower marginal costs tend to offer lower prices. This relationship allows us to propose a simple test for the connection between government demand and firm efficiency: *firms* offering lower prices in the private market (indicating higher efficiency) should experience greater government demand.

Furthermore, building on the insights from Khandelwal (2010), firms with higher quality tend to achieve larger market shares in their market, conditional on prices. Hence, a firm's quality can be proxied by the residual from a regression of quantity on prices within a market. This leads to the following hypothesis: firms with a higher quality proxy should attract greater government demand.

We explore these hypotheses using data from Brugués (2020), which includes prices and quantities in both the private and government markets for the pharmaceutical sector. The dataset encompasses the medicine market in Ecuador from 2010 to 2015, providing yearly details on the manufacturer (firm), product (brand), main molecule, total output, and total revenue for both the private wholesale market and the government market. Products sharing the same main molecule are classified within the same market.

To construct a measure of quality, we first perform the following regression for firms i, product j, molecule-market m, at time t, in the private market:

$$ln(q_{ijmt}^{priv}) = \beta ln(p_{ijmt}^{priv}) + \lambda_{mt} + quality_{ijmt}^{priv},$$

where λ_{mt} represents molecule-year fixed effects, q denotes total quantity, p signifies average price, and quality is estimated as the residual of the regression, aligning with the approach of Khandelwal (2010).

Next, we examine the proposed hypotheses through the following regression for the government market:

$$ln(q_{ijmt}^{gov}) = \gamma^e ln(p_{ijmt}^{priv}) + \gamma^q quality_{ijmt}^{priv} + \varepsilon_{ijmt}$$

where $\gamma^e < 0$ would suggest that government demand increases with efficiency (as indicated by private market prices), and $\gamma^q > 0$ would imply that government demand rises with quality proxies.

Internet Appendix Table IA5 presents the results. Specifically, we observe that government demand decreases as prices in the private market rise, and it increases with the proxy for quality.

	$ \begin{array}{c} ln(q^{gov})\\(1) \end{array} $	$ \begin{array}{c} ln(q^{gov})\\(2) \end{array} $
$ln(p^{priv})$	-1.045***	-0.641***
	(0.0543)	(0.150)
Quality	0.166 +	0.166 +
	(0.104)	(0.105)
Molecule FE	No	Yes
Observations	534	534
R-squared	0.566	0.748

Table IA5: Government Demand, Quality and Efficiency

Notes: The table reports the relationship between demand in the government sector (quantity purchased by molecule) and measures for efficiency (prices in the private market) and quality (residual market share controlling for prices) for the medicine market in 2012-2015 using data from **Brugués** (2020). Robust standard errors clustered at the molecule-level are reported in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1, + p < 0.15

D.2 Equal demand elasticity in government and private markets

Dubois et al. (2021) demonstrated that, in low- and middle-income countries, both the government and private sectors exhibit identical demand elasticities. Our study extends this finding to the Ecuadorian context, covering not only medicines but also other sectors. We achieve this through two approaches.

First, we utilize data from Brugués (2020) on the medicine market in Ecuador from 2012 to 2015, encompassing both government and private markets. Since the government procures not the specific variety sold by a company but rather the molecule compound, we initially sum the total quantity for each molecule-year in each market and compute the molecule's average price. Following the classification by Brugués (2020), we use standardized units that measure price and quantity per molecule weight, rather than in packages. For each molecule m, market k, and time t, we estimate the following instrumental variable regression:

$$ln(q_{mt}^k) = -\sigma^k ln(p_{mt}^k) + \varepsilon_{mt},$$

where q^k represents the total quantity, p^k the average price, and σ^k the market elasticity in medicines. We employ a Hausman-style instrument, relying on total quantity and average prices in the other market -k as instruments.

Internet Appendix Table IA6 reports the results from the instrumental variable estimation. Consistent with the findings of Dubois et al. (2021), we do not reject the hypothesis of unit-elastic demand -1 in both the government and private markets. These estimates are less elastic compared to those in Brugués (2020), as we estimate moleculelevel elasticities, while Brugués (2020) focused on variety-molecule-level elasticities.

Second, we evaluate the elasticity of import demand in the private market, as well

	Gov (1)	Priv (2)
$-\sigma^{gov}$	-1.011^{***} (0.0728)	
$-\sigma^{priv}$		-0.899^{***} (0.0954)
Observations	548	548

Table IA6:Government and Private De-
mand Elasticity - Medicine

Notes: The table reports estimates of the government and private market elasticity obtained by an instrumental variable approach of log quantity procured on average log wholesale prices using medicine data from Brugués (2020). The unit of observation is the molecule-year level. Standard errors are clustered at the molecule level. In all columns, prices are instrumented using prices and quantities from the other market. *** p<0.01, ** p<0.05, * p<0.1.

as government demand for products affected by import prices, using shocks to import prices. This analysis is supported by i) Ecuadorian customs data from 2013 to 2016, ii) bilateral trade data from the Observatory of Economic Complexity (OEC), iii) exchange rate shocks from the OECD, iv) product concordance tables (CPC-HS) from the World Bank, and v) procurement data from Ecuador.

For each imported product j at the HS6 revision 2007 level, we construct Bartikstyle instruments following Brambilla et al. (2012), computing unexpected exchange rate shocks as $\sum_{c} \Delta \text{ExchangeRate}_{ct} \frac{\text{Imports}_{jct-1}}{\sum_{c} \text{Imports}_{jct-1}}$, where Imports_{jct-1} denotes total imports from country c at time t - 1, and $\Delta \text{ExchangeRate}_{ct}$ represents the exchange rate change between Ecuador's currency (U.S. dollar) and the currency of country c from time t - 1to t. We then aggregate the total quantity imported by the private market and average unit prices for each product j at time t by presentation style s (e.g., KG or units).

For the government market, each product is categorized under CPC-5. Unit prices are defined as the product-contract-level ratio of total value over total quantity. We then aggregate total quantity and derive average prices for each CPC-year, applying the World Bank's concordance table to map the same import shocks to aggregate government demand.

We conduct the following instrumental variable regression for each market k separately for products j at time t:

$$ln(q_{jt}^k) = -\sigma^k ln(p_{jt}^k) + \gamma_j + \varepsilon_{jt},$$

where γ_j represents fixed effects for products (1-digit or 2-digit sector), and prices are instrumented using the exchange-rate Bartik shocks.

Internet Appendix Table IA7 presents the results. Columns (1) - (3) detail elasticity estimates without controlling for product-fixed effects: Column (1) for government elasticity, Column (2) for all imported products, and Column (3) for imported products demanded by the government. Across all scenarios, we observe similar elasticities of around -3. Columns (4)-(9) incorporate 1-digit and 2-digit product-level fixed effects, consistently showing that government demand elasticity closely matches private demand elasticity.

Through two distinct data sources, we conclude that the elasticity of government demand does not significantly differ from that of private demand. These findings align with those reported by Broda and Weinstein (2004) but are smaller than those implied by our production function estimates under the assumption of constant returns to scale. We further discuss this discrepancy in Internet Appendix Section D.5.

Table IA7: Elasticity of Private and Government Demand

	$\operatorname{Gov}(1)$	Priv (2)	Priv (3)	Gov (4)	Priv (5)	Priv (6)	Gov (7)	Priv (8)	Priv (9)
$-\sigma^{gov}$	-3.567*** (1.142)			-4.448** (1.963)			-5.492* (3.226)		
$-\sigma^{priv}$		-2.669^{***} (0.340)	-3.269^{***} (0.879)		-4.550^{***} (0.885)	-5.554^{**} (2.636)		-5.199^{***} (1.105)	-8.098 (5.805)
Product-1 FE	No	No	No	Yes	Yes	Yes	No	No	No
Product-2 FE	No	No	No	No	No	No	Yes	Yes	Yes
Restricted Sample		No	Yes		No	Yes		No	Yes
Observations	1,170	15,290	5,212	$1,\!170$	15,289	5,212	1,170	15,287	5,209

Notes: The table reports estimates of the government and private market elasticity obtained by an instrumental variable approach of log quantity procured on average log import prices. The unit of observation is the CPC-5 product level-year. Standard errors are clustered at the CPC-5 product level. In all columns, prices are instrumented with unexpected shocks to supply from international trade, computed as $\sum_{c} \Delta ExchangeRate_{ct} \frac{Imports_{jet-1}}{\sum_{c} Imports_{jet-1}}$. Bilateral trade data comes from the Observatory of Economic Complexity (https://oec.world/en/resources/data/) and is available for products at the HS6 revision 2007 (6-digit depth). HS6 products are mapped to CPC-5 products using the WITS concordance table (https://wits.worldbank.org/product_concordance.html). Yearly exchange rates between countries are obtained from the OECD (https://data.oecd.org/conversion/exchange-rates.htm). Columns (1)-(3) do not control for products at the CPC 1-digit level. Columns (7)-(9) control for products at the CPC 2-digit level. Columns (1), (4), and (6) are for the government market, and other columns for the private market. Columns (3), (6), and (9) restrict the sample of private products to those that are also covered in the government market.*** p<0.01, ** p<0.05, * p<0.1.

D.3 Proportionality in output prices between government and private sector

Working with revenue production functions linked to demand systems necessitates an assumption regarding the setting of prices in the private and government sectors. We posit that prices in the government sector are proportional to those in the private sector, with politically connected firms able to charge an additional price premium from the government. This section furnishes empirical evidence supporting the proportionality assumption. Detailed discussion on the assumption related to the political price premium is available in Internet Appendix Section J.

Utilizing data on private and government sales for medicines from 2012 to 2015, sourced from Brugués (2020), we directly compare prices at the product-firm-year level across the two markets. The dataset encompasses approximately 10,000 unique products, sold by 247 firms, with 85% of all output directed to the private market. These products are highly granular, exemplified by items such as a box of 50 Ibuprofen Capsules, each containing 600 Mg. Among these products, 300 corporation-products are represented in both the private and government markets, yielding a total of 861 corporation-product-year observations appearing in both sectors.

In the figure below, we compare the average wholesale price in the private market for a given Corporation-Product-Year on the y-axis to the price of the product when sold to the government on the x-axis. The diagonal marks the boundary of equal pricing. As shown in the figure, prices for the same product and seller tend to be *higher* in the private market (91% of observations). Importantly, however, there is an extremely high correlation in the rank of the prices (89%): products that are more expensive in the government are also more expensive in the private sector. Indeed, the slope of a regression of log prices government on log prices private cannot be rejected to be equal to 1 at any relevant significance level. Therefore, our assumption of proportional prices is supported in this sector.



Figure IA5: Prices in the government and private sector

Notes: This figure presents the correlation between prices in the private and public sectors, for the same product, seller, and year. The figure uses information by Brugués (2020) for medicines sold in both sectors in 2012-2015.

D.4 Equal input prices between connected and non-connected firms

In our model, we assume that all firms within a sector, both connected and non-connected, pay the same price for inputs. However, previous literature has documented that connected firms may receive preferential terms in access to capital and other inputs (Khwaja and Mian, 2005; Boubakri et al., 2012; Haselmann et al., 2018; Moon and Schoenherr, 2022). Contrary to these findings, our analysis suggests that in our sample, connected firms do not enjoy lower input prices compared to their non-connected counterparts. In fact, if there is any difference, it appears that they pay marginally higher prices for equivalent inputs, although the overall evidence aligns more closely with the notion of equal input pricing. Subsequently, we discuss the implications of this result on our welfare estimates.

We explore this empirically through two methods. First, we analyze transactionlevel customs data from the Ecuadorian customs agency spanning from 2013 to 2016. Each transaction is associated with an importer ID, allowing us to categorize them as connected or not. Among over 2.4 million transactions, approximately 13% are attributed to connected contractors, with the remaining transactions linked to firms for which we possess balance sheet data. The unit price is determined by dividing the total import value by the quantity. Products are categorized by a 10-digit HS code, country of origin, type of quantity (weight or units), and product description. For a less granular analysis, country of origin is omitted. Log unit prices are standardized by removing product definition by time (year-month) fixed effects, and a similar standardization is applied to log quantity. The following regression is then conducted:

$$p_{ikscet}^* = \beta PC_i + q_{ikscet}^* + \gamma_{kct} + \gamma_s + \gamma_e + X_{it} + \varepsilon_{ikscet},$$

where p^* represents standardized log unit prices, q^* standardized log quantity, PC the political connection of the firm, γ 's fixed effects, and X firm-year controls such as revenue, capital, labor, and materials. Observations are at the level of firm *i*, product *k*, firm 3-digit sector *s*, country of origin *c*, port of entry *e*, and month-year *t*. The coefficient β is indicative of price differentials attributable to political connections.

Internet Appendix Table IA8 showcases the comparison of standardized log unit prices between connected and non-connected contractors. Across diverse specifications, we observe a modest, positive coefficient (ranging from 2 to 5%) that is statistically insignificant, implying that connected contractors do not benefit from lower import prices for identical goods.

This exercise has its limitations. Some benefits of connections might derive from reduced import tariffs, which are not observed in this analysis. Furthermore, given that trading partners are international, connected contractors may not be able to leverage their influence to improve their standing abroad. Therefore, we adopt a second strategy to examine preferential loan terms from local banks in Ecuador. For this analysis, we rely on data from De Simone (2022), who conducted the necessary regressions for us. The dataset encompasses the universe of loans from the Superintendencia de Bancos from 2010 to 2017, totaling approximately 0.8 million loans. Around 7% of these loans were granted to connected contractors, with the remaining going to firms for which balance

sheet information is available. Using this dataset, we conduct the following regression:

$$r_{iblt}^* = \beta P C_i + \gamma_{sbt} + X_{it} + C_{iblt} + \varepsilon_{iblt}$$

where r is the loan nominal interest rate, PC is political connection of the firm, γ 's are fixed-effects, X are firm-year controls such as revenue, capital, labor, and materials, while C are loan level controls such as loan size and term-to-maturity. The unit of observation is at the firm i, loan l, bank b, firm 3-digit sector s, year t The coefficient β is intended to capture differences in interest rates attributable to political connections.

Although exact point estimates are not disclosed, we observe that connected firms pay higher interest rates than non-connected firms across all specifications. It is only after adjusting for firm characteristics, such as revenue and inputs, that we find no statistically significant difference in the cost of capital between the two groups. Additionally, in results not presented here, we leverage the extensive temporal scope of the data to investigate changes in pricing within firms after establishing a connection and discover no discernible impact. Consequently, even within Ecuadorian markets, connected firms in our sample do not benefit from preferential terms for inputs. If anything, they face higher prices, although these estimated effects lack robustness.

	Input Price					
	(1)	(2)	(3)	(4)		
Panel A: Imports' Prices (Price of Interme	diate Inputs)					
Political Connection	0.0419 (0.0446)	$0.0190 \\ (0.0589)$	$0.0774 \\ (0.0648)$	$0.0530 \\ (0.0772)$		
Observations	2,135,741	2,135,728	$2,\!485,\!947$	2,485,933		
R-squared	0.278	0.280	0.338	0.340		
Product X Country Origin X Time FE	Yes	Yes	No	No		
Product X Time FE	No	No	Yes	Yes		
Sector FE	Yes	Yes	Yes	Yes		
Port of Entry FE	Yes	Yes	Yes	Yes		
Quantity Controls	Yes	Yes	Yes	Yes		
Firm Controls	No	Yes	No	Yes		
Panel B: Interest Rates (Price of Capital)						
Political Connection	$\operatorname{sign}(+)^{***}$	$\operatorname{sign}(+)^{***}$	$\operatorname{sign}(+)^{***}$	$\operatorname{sign}(+)$		
Sample Size	0.8M	0.8M	0.8M	0.8M		
Sector x Year FE	Yes	No	No	No		
Sector x Bank x Year FE	No	Yes	Yes	Yes		
Loan Controls	No	No	Yes	Yes		
Firm Controls	No	No	No	Yes		

Table IA8: Input Prices differences

Notes: This table reports input price differences across connected and non-connected firms. Panel A uses import price data from the Ecuadorian Customs agency from 2013 to 2016. The unit of observation is at the transaction-level, with details on the importer-firm, country of origin, product ID, and port of origin. Panel B uses commercial credit interest rates from the Superintendencia de Bancos (Bank Regulator). The results are censored in point estimates and number of observations. They were obtained thanks to Rebecca De Simone, who ran it in the full sample of (De Simone, 2022) and provided the sign and significance of the coefficients. The unit of observation is at the loan level, with information on the bank of origin, amount, and maturity. Firm controls include 3-digit sector FE, revenue, capital, materials, and wages. Standard errors are clustered at the 3-digit sector level. *** p < 0.01, ** p < 0.05, * p < 0.1.

All in all, this supports our equal input pricing assumption. Yet, if this assumption does not hold (here or in a different setting), these are the required modifications to our estimation model. Let τ^i be the proportionality factor for input prices between connected and non-connected firms: e.g., $r_{st}^{con} = \tau^k r_{st}^{unc}$. The estimation equation of the revenue production function would now be:

$$R_{it} = \overline{L}_{it}^{\beta_l} \overline{M}_{it}^{\beta_m} \overline{K}_{it}^{\beta_k} exp(\omega_{it} + z_{it} + u_{it})^{\frac{\sigma-1}{\sigma}} \Psi_{st}^{-1}(\tau^k)^{-Post_{it}\beta_k} X(Post_{it}^{PC}, \xi_{it}) \kappa_{st}.$$

As the constant gap τ^k would be absorbed by our binary variable capturing the political connection status of the firm, there is no modification needed in the estimation of the production function.

However, the proportionality factor will affect the estimates of excess costs. The expression for flexible capital cost will now be given by:

$$SOEC_{flex}(\tau^k) = exp\Big(\frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\Big)\frac{\Gamma_l + \Gamma_m + (\tau^k)^{1+\alpha_k}\Gamma_k}{\Gamma_l + \Gamma_m + \Gamma_k} - 1,$$

so for $\tau^k > 1$ ($\tau^k < 1$) our previous expression in the main text is a lower (upper) bound. The results are identical if the proportionality factor applies to only labor or only intermediate inputs, or if all factor-specific τ_f have the same sign relative to 1. If the input factors have an asymmetric sign relative to 1, then one would need to input estimates for input prices, proportionality factors, and output elasticities and the expressions derived in Internet Appendix Section A to derive an exact value. As shown above, it seems likely that all inputs have the same sign relative to 1.

For the social excess cost expression for fixed capital, if the proportionality factor is for the flexible inputs, the result is the same as above. However, if the factor is for capital, the expression in this case is given by:

$$SOEC_{fixed}(\tau^k) \approx \frac{1}{\tau^k} exp\Big(\frac{\beta_k}{\beta_l + \beta_m + \beta_k} [ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\Big) - 1.$$

$$(48)$$

Thus, the proportionality factor would reduce (increase) the excess cost estimate for $\tau^k > 1$ ($\tau^k < 1$).

D.5 Constant Returns to Scale (CRTS)

Our welfare estimation relies on CRTS to map gaps in TFPR and capital-revenue ratios to changes in utility. Here we provide some support for the plausibility of this assumption given our estimates of revenue returns to scale.

In Internet Appendix Figure IA6, we plot the distribution of sectoral (average) revenue returns to scale and their implied markups if constant returns to scale in production holds. As shown in the figure, implied markups are all positive, with an average markup of 7%.

The implied elasticity of substitution σ for our main specification is 13.68 (with a lower CI 95% of 8.58), which is above the usual estimate used in the literature of $\sigma = 3$ from Broda and Weinstein (2004) but very close to the numbers in Halpern et al. (2015)

Figure IA6: Distribution of Revenue Returns to Scale and Implied Markups



Notes: This figure presents the distribution of (average) revenue returns to scale, and the distribution of implied aggregate markups under the assumption of constant returns to scale (CRTS) in output.

that find $\sigma = 15$ and markups around 6.5%.

Despite the fact that we use 2-digit sectors, and thus we should generally expect lower elasticity of substitution estimates, it must be highlighted that our estimates are for the set of government contractors, which may be considered a narrower definition of the industry. Indeed, when looking at the industry as a whole, using all available firms rather than only contractors, estimates for the median elasticity of substitution are lower, at 11.85 with a lower CI 95% of 7.9. Therefore, even though our estimates are higher than those of Broda and Weinstein (2004), our range falls within the usual ranges in the literature.

E Internet Appendix: Production Function Estimation Procedure

In this section, we detail the estimation procedure, including the necessary modifications to De Loecker (2011) to adapt it to our context.

For firm i at time t, let their revenue production function be given by:

$$r_{it} = \beta_l l_{it} + \beta_m \overline{m}_{it} + \beta_k k_{it} + \omega_{it}^* + \psi_{st}^* + a_{st} + \xi_{it}^{PC} PC_{it} \cdot Contractor_{it} + \phi_{it} + \varepsilon_{it},$$

where \overline{input} ($\overline{l}_{it}, \overline{m}_{it}$, or \overline{k}_{it}) are the total expenses in the input, β_{input} is the revenue elasticity of the input, ω_{it}^* is TFPR, ψ_{st}^* captures time-varying sector-specific terms, a_{st} captures the sectoral government-private price differential, ξ_{it}^{PC} captures demand and effects of political connections on revenue, ϕ_{it} are i.i.d. government demand shocks, and ε_{it} are exogenous productivity and demand shocks. As ψ_{st}^* is approximated via fixed effects, and demand and productivity shocks are i.i.d. from the same distributions, then, with some abuse of notation the estimating equation becomes

$$r_{it} = \beta_l \overline{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \xi_{it}^{PC} PC_{it} \cdot Contractor_{it} + \varepsilon_{it}.$$

We consider productivity to be determined by the following law of motion:

$$\omega_{it}^* = g(\omega_{it-1}^*) + v_{it}, \tag{49}$$

for some unknown function $g(\cdot)$. Thus, we assume productivity is not directly affected by political connections. This assumption is tested in Section 3 by conducting event studies around political connection status for productivity estimates from specifications that do not control at all for political connections.⁷⁰

We rely on the control-function approach of Levinsohn and Petrin (2003) that uses materials as a proxy variable. The starting point is to observe that intermediate inputs \overline{m}_{it} are directly related to a firm's productivity ω_{it}^* and capital stock, as well as demand variables PC_{it}, ψ_{st}^* , which impact the firm's residual demand and therefore optimal input usage. Hence, the intermediate inputs demand equation is given by:

$$\overline{m}_{it} = m(\overline{k}_{it}, \omega_{it}^*, PC_{it}, \psi_{st}^*)$$

Following the intuition in De Loecker (2011), input demand is monotonic in productivity under a monopolistic competition framework with constant markups, as markups are not related to productivity. Hence, one can invert the input demand equation to obtain a proxy for productivity:

$$\omega_{it}^* = h(k_{it}, \overline{m}_{it}, PC_{it}, \psi_{st}^*),$$

for some function $h(\cdot)$.

The estimating procedure follows the two stages of Levinsohn and Petrin (2003). The first stage consists of the model:

$$r_{it} = \beta_l \bar{l}_{it} + \phi_t(\bar{k}_{it}, \overline{m}_{it}, PC_{it}, \psi^*_{st}) + \varepsilon_{it},$$
(50)

where $\phi_t(\overline{k}_{it}, \overline{m}_{it}, PC_{it}, \psi_{st}^*) = \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \xi_{it}^{PC} PC_{it} \cdot Contractor_{it} + h(\cdot)$. As in De Loecker et al. (2016), we approximate the function $\phi_t(\cdot)$ as a third-degree polynomial with all its elements, except for the dummy variables, which enter linearly. Estimation of this stage purges out unexpected demand shocks and measurement errors ε_{it} .

Then, from the first stage, for given values of parameters, we can express productivity as a function of data and parameters:

$$\omega_{it}^*(\beta_m, \beta_k, \xi_{it}^{PC}, \psi_{st}^*) = \widehat{\phi}_{it} - \beta_m \overline{m}_{it} - \beta_k \overline{k}_{it} - \psi_{st}^* - \xi_{it}^{PC} PC_{it} \cdot Contractor_{it}.$$
 (51)

Using the law of motion of productivity, we can express the innovation v_{it} as a function

⁷⁰This would be similar to testing for the effect of exporting on productivity as in De Loecker (2007).

of parameters:

$$v_{it}(\beta_m, \beta_k, \xi_{it}^{PC}, \psi_{st}^*) = \omega_{it}^*(\beta_m, \beta_k, \xi_{it}^{PC}, \psi_{st}^*) - E\Big(\omega_{it}^*(\beta_m, \beta_k, \xi_{it}^{PC}, \psi_{st}^*) | \omega_{it-1}^*(\beta_m, \beta_k, \xi_{it-1}^{PC}, \psi_{st-1}^*) \Big)$$
(52)

The moment that identifies the parameters is:

$$E(\upsilon_{it}(\beta_m, \beta_k, \xi_{it}^{PC}, \psi_{st}^*)Y_{it}) = 0,$$

where Y_{it} includes lagged materials, current capital, their higher order interaction terms, as well as the year dummies and the political connection status for contractors. The only difference to the usual approach is our inclusion of political connection status. The parameter ξ_{it}^{PC} is identified from the assumption that shocks or innovations to productivity are not correlated with political connection status. This assumption is tested empirically by an event study approach on the evolution of a productivity estimate that does not control for political connection status (see Section 3).

As shown by Ackerberg et al. (2015), the elasticity of labor is not identified in the first stage. Instead, the elasticity is identified by applying the GMM approach of Wooldridge (2009), where both stages are estimated jointly.

Once the parameters are estimated, we compute productivity estimates as follows:

$$\widehat{\omega}_{it}^* = r_{it} - \widehat{\beta}_l \overline{l}_{it} - \widehat{\beta}_m \overline{m}_{it} - \widehat{\beta}_k \overline{k}_{it} - \widehat{\psi}_{st}^* - \widehat{\xi}_{it}^{PC} PC_{it} \cdot Contractor_{it}$$

F Validation with Audit Data on Quality

To provide some evidence supporting the notion that political connections may lead to lower quality of goods, we link our firm-level connection indicators with external measures of school infrastructure quality obtained from the Ministry of Education's 2022 external audits. The audit covered 627 public schools, of which we were able to match 97 with contracts in the procurement data. The Ministry of Education categorized school infrastructure as follows: "Green" for minimal required intervention, "Orange" for needing corrective intervention, and "Red" for requiring substantial reconstruction. Following this classification, we define the school infrastructure level "Green" as good and "Orange" or "Red" as not good. Of the 97 matched schools, a majority (58%) are classified as good. Among these good schools, 10 projects were carried out by connected firms.

Although our matched sample is small, we find suggestive evidence in Internet Appendix Table IA9 that projects by connected firms are less likely to be of good quality, even after controlling for project year. We present this as qualitative evidence that aligns with the paper's overall narrative. However, due to the small sample size, these findings are not conclusive.

	$\begin{array}{c} 1\{Good\}\\(1)\end{array}$
Political Connection	-0.220^{***} (0.061)
Sample Size	97
Contract-Year FE	Yes

Data on School Insfrastructure

Table IA9: Validation with Audit

Notes: The table presents correlations between the quality of school infrastructure and political connections using data from the Ministry of Education. Clustered standard errors at the contract-year level are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

G Internet Appendix: Additional Excess Cost Estimates

Rank	ISIC2	Description	Excess costs	CI (95%)	Avg. number firms	Avg. share connected	Avg. sectoral revenue (million \$)	Avg. public expend. (million \$)
1	J59	Video and television programme production, music publishing activities	23.39%**	[3.12%, 43.66%]	174	18.82%	129.0	1.6
2	C10	Manufacture of food products	23.00%	[-29.61%, 75.62%]	846	4.34%	3334.2	6.6
3	I55	Accommodation	$20.92\%^{*}$	[-1.00%, 42.84%]	455	8.56%	282.3	1.3
4	J61	Telecommunications	$20.03\%^{**}$	[3.07%, 36.98%]	557	20.06%	684.8	6.8
5	K65	Insurance, reinsurance and pension funding, except compulsory social security	18.09%***	[3.99%, 32.18%]	303	15.35%	303.3	0.5
6	J58	Publishing activities	$16.67\%^{**}$	[1.55%, 31.80%]	277	24.66%	143.8	3.4
7	L68	Real estate activities	$15.42\%^{*}$	[-1.63%, 32.47%]	7920	3.44%	1180.6	11.2
8	M70	Activities of head offices; management consultancy activities	$13.96\%^{***}$	[7.83%, 20.09%]	2005	20.25%	480.1	16.7
9	N77	Rental and leasing activities	12.16%	$[-19.25\%,\!43.57\%]$	355	10.32%	227.5	3.5
10	I56	Food and beverage service activities	11.95%	[-5.51%, 29.40%]	752	4.86%	535.7	2.4
11	P85	Education	11.15%	$[-6.49\%,\!28.79\%]$	687	10.91%	255.2	1.7
12	J62	Computer programming, consultancy and related activities	$10.83\%^{***}$	[2.83%, 18.83%]	688	27.31%	278.0	13.9
13	M71	Architectural and engineering activities; technical testing and analysis	8.48%***	[2.58%, 14.40%]	1142	27.73%	578.0	29.7
14	C25	Manufacture of fabricated metal products, except machinery and equipment	8.40%	[-4.29%,21.09%]	283	12.55%	372.4	4.8
15	Q86	Human health activities	7.03%	[-2.75%, 16.82%]	659	10.97%	531.7	1.7
16	F42	Civil engineering	$6.43\%^{*}$	[-1.01%, 13.86%]	1940	31.51%	1232.2	64.9
17	C20	Manufacture of chemicals and chemical products	5.71%	[-2.54%, 13.95%]	437	9.75%	823.9	4.3
18	K66	Activities auxiliary to financial service and insurance activities	5.20%	[-15.42%, 25.82%]	371	11.00%	195.3	0.6
19	C18	Printing and reproduction of recorded media	5.06%	$[-9.61\%,\!19.73\%]$	345	15.15%	364.2	4.0
20	N81	Services to buildings and landscape activities	5.02%	[-3.49%, 13.54%]	448	22.43%	179.3	12.6
21	G45	Wholesale and retail trade and repair of motor vehicles and motorcycles	4.61%	[-2.28%,11.50%]	1567	6.92%	3187.4	26.4
22	M74	Other professional, scientific and technical activities	4.45%	[-5.98%, 14.88%]	665	29.94%	123.2	9.9

Table IA10: Excess Costs Estimates, All Sectors

Rank	ISIC2	Description	Excess costs	CI (95%)	Avg. number firms	Avg. share connected	Avg. sectoral revenue (million \$)	Avg. public expend. (million \$)
23	B09	Mining support service activities	3.94%	[-15.50%, 23.38%]	234	17.79%	700.6	2.6
24	C22	Manufacture of rubber and plastics products	3.79%	[-4.50%, 12.09%]	298	5.79%	877.2	2.1
25	F41	Construction of buildings	3.75%	[-3.35%, 10.84%]	2734	24.08%	1034.8	51.0
26	C23	Manufacture of other non-metallic mineral products	3.26%	[-9.44%, 15.97%]	226	12.68%	616.1	5.6
27	N80	Security and investigation activities	2.89%	[-0.86%, 6.64%]	892	32.21%	593.9	33.3
28	H49	Land transport and transport via pipelines	2.59%	[-5.97%, 11.15%]	5028	3.80%	1152.3	20.0
29	S95	Repair of computers and personal and household goods	1.36%	[-17.41%, 20.13%]	170	15.57%	95.8	2.1
30	M69	Legal and accounting activities	1.10%	[-5.75%, 7.96%]	1525	19.10%	275.2	6.4
31	C33	Repair and installation of machinery and equipment	0.78%	[-9.59%, 11.14%]	487	17.73%	263.2	11.3
32	H52	Warehousing and support activities for transportation	0.54%	[-7.75%, 8.83%]	895	8.55%	765.9	5.0
33	G47	Retail trade, except of motor vehicles and motorcycles	0.38%	[-4.34%, 5.11%]	3093	8.67%	4037.3	41.2
34	J60	Programming and broadcasting activities	0.20%	[-11.65%, 12.06%]	298	27.37%	206.2	2.0
35	F43	Specialized construction activities	0.13%	[-9.72%, 9.97%]	731	19.98%	473.9	16.9
36	N79	Travel agency, tour operator, reservation service and related activities	-0.29%	[-12.27%,11.69%]	1595	9.67%	338.2	7.4
37	A01	Crop and animal production, hunting and related service activities	-0.78%	[-21.04%, 19.48%]	3039	2.79%	2885.8	2.4
38	M73	Advertising and market research	-0.92%	[-13.20%, 11.35%]	968	19.97%	547.7	15.3
39	G46	Wholesale trade, except of motor vehicles and motorcycles	-2.02%	[-5.23%, 1.19%]	10908	9.64%	14509.9	232.2
40	H51	Air transport	-2.86%	[-33.63%, 27.91%]	361	8.12%	650.3	1.1
41	N82	Office administrative, office support and other business support activities	-7.02%	[-22.94%, 8.89%]	464	12.90%	251.3	4.4
42	D35	Electricity, gas, steam and air conditioning supply	-16.15%	[-46.43%, 14.13%]	286	9.79%	555.3	4.6

Table IA12: Excess Costs Estimates, All Sectors (Continued)

Notes: The table reports coefficients and confidence intervals of the excess costs of political connection at the 2-digit sector level. Excess costs are estimated from equation 30 assuming each firm's capital level is fixed. The production function elasticities and firm TFPR used as inputs to the excess costs regressions are obtained using the LP-Wooldridge methodology with the specification detailed in equation 26. The sample is the set of firms classified as government contractors in sectors with at least 750 observations. The regressions to estimate the productivity and capital utilization include year and 3-digit sector fixed effects. Confidence intervals (CI) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. The table additionally reports the yearly average number of contractors operating in the sector, the yearly average share of politically connected firms, the average total revenue of the sector per year, and the average total public expenditure in the year. *** p < 0.01, ** p < 0.05, * p < 0.1.

Capital	Model	Sample for production function est.	Correlation
Flexible	LP-Wooldrige	Main specification	0.989
Fixed	OLS	Main specification	0.755
Flexible	OLS	Main specification	0.704
Fixed	LP-Wooldrige	Before connection	0.761
Flexible	LP-Wooldrige	Before connection	0.735
Fixed	OLS	Before connection	0.696
Flexible	OLS	Before connection	0.611
Fixed	LP-Wooldrige	Premium-adjusted revenue	0.983
Flexible	LP-Wooldrige	Premium-adjusted revenue	0.966
Fixed	OLS	Premium-adjusted revenue	0.788
Flexible	OLS	Premium-adjusted revenue	0.734
Fixed	LP-Wooldrige	No premium adjustment	0.959
Flexible	LP-Wooldrige	No premium adjustment	0.938
Fixed	OLS	No premium adjustment	0.735
Flexible	OLS	No premium adjustment	0.657
Fixed	LP-Wooldrige	All firms	0.891
Flexible	LP-Wooldrige	All firms	0.861
Fixed	OLS	All firms	0.667
Flexible	OLS	All firms	0.555

Table IA13: Correlation Between Sectoral Misallocation Estimates

Notes: The table displays pairwise correlation coefficients between sector-level estimates of excess costs computed with different samples and model specifications. The reference estimates are obtained with LP-Wooldridge production functions estimated on the sample of government contractors using our main specification (see Equation 26) and assuming fixed capital. The unit of observation is the 2-digit sector level.

G.1 Robustness Checks and Additional Welfare Costs Results

	Flexible Woold (1)	Fixed Woold (2)
Panel A: Discretionary Only		
Excess Costs	.051*	.054**
	(.028)	(.027)
Welfare cost ($\%$ of proc. budget)	3.835^{*}	4.047^{*}
	(2.251)	(2.176)
Sample size	17,022	17,022
Panel B: Auction Only		
Excess Costs	.039	.041
	(.031)	(.033)
Welfare cost ($\%$ of proc. budget)	2.11	2.112
	(2.83)	(2.859)
Sample size	5,131	5,131
Panel C: Random Only		
Excess Costs	.056	.022
	(.154)	(.14)
Welfare cost ($\%$ of proc. budget)	3.889	.927
	(13.947)	(12.887)
Sample size	531	531
Panel D: Mixed Only		
Excess Costs	.058***	.06***
	(.017)	(.017)
Welfare cost (% of proc. budget)	4.617***	4.789***
	(1.411)	(1.416)
Sample size	48,839	48,839

Table IA14: Excess Cost Estimates by Contract Type

Notes: The table reports excess cost estimates and corresponding welfare costs as a percentage of the procurement budget by contract type. For each panel, we restrict to the set of contractors that only supply the specified type of contract. Then, we estimate excess costs at the 2-digit industry level and compute economy-wide averages using as weights the number of firms in each contract-type-sector group. Standard errors (in parentheses) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 22, assuming that $\theta = 0$. Outcomes in Column (1) assume flexible capital and are estimated as in equation 28. Specifications in (2) assume fixed capital and are estimated via equation 30. All excess cost regressions control for year and 3-digit sector fixed effects. Panel A restricts to firms that only supply discretionary contracts, Panel B to firms that supplied only auctions, Panel C only random contracts, and Panel D mixed combinations. *** p<0.01, ** p<0.05, * p<0.1.

	Flexible Woold (1)	Fixed Woold (2)
Panel A: Quartile 1		
Excess Costs	035	04
	(.157)	(.124)
Welfare cost (% of proc. budget)	-3.514	-4.022
	(12.757)	(10.166)
Sample size	2,821	2,821
Panel B: Quartile 2		
Excess Costs	003	005
	(.018)	(.019)
Welfare cost ($\%$ of proc. budget)	504	688
	(1.646)	(1.647)
Sample size	14,153	14,153
Panel C: Quartile 3		
Excess Costs	.01	.007
	(.014)	(.015)
Welfare cost ($\%$ of proc. budget)	.816	.562
	(1.179)	(1.21)
Sample size	24,385	24,385
Panel D: Quartile 4		
Excess Costs	.041***	.039***
	(.015)	(.015)
Welfare cost ($\%$ of proc. budget)	3.081***	2.85***
	(1.163)	(1.145)
Sample size	34,054	34,054

Table IA15: Excess Cost Estimates by Quartile Assets

Notes: The table reports excess cost estimates and corresponding welfare costs as a percentage of the procurement budget by the size of firms. We first obtain the median value of assets for each firm, and then rank firms in quartiles for each given 2-digit industry. Then, we estimate excess costs at the 2-digit industry level and compute economy-wide averages using as weights the number of firms in each quartile-sector group. Standard errors (in parentheses) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 22, assuming that $\theta = 0$. Outcomes in Columns (1) assume flexible capital and are estimated as in equation 30. All excess cost regressions control for year and 3-digit sector fixed effects. Quartile 1 includes the smallest firms in terms of assets and quartile 4 includes the largest firms. *** p<0.01, ** p<0.05, * p<0.1.

Figure IA7: Distribution of Province-level Excess Cost Estimates



Notes: This figure presents the distribution of province-level excess costs averages. For each province, we obtain the excess cost estimate for each 2-digit sector, and then obtain weighted averages using the number of firms in the sector for the province as weights.

	Flexible Woold (1)	Fixed Woold (2)	
Panel A: Tradables			
Excess Costs	$0.048 \\ (0.039)$	$0.046 \\ (0.041)$	
Welfare cost (% of proc. budget)	4.023 (3.321)	$3.828 \\ (3.412)$	
Sample size	7,885	7,885	
Panel B: Wholesale			
Excess Costs	$0.000 \\ (0.011)$	-0.003 (0.012)	
Welfare cost (% of proc. budget)	-0.047 (0.973)	-0.254 (0.997)	
Sample size	32,245	32,245	
Panel C: Non-tradables			
Excess Costs	0.068***	0.07***	
	(0.011)	(0.011)	
Welfare cost (% of proc. budget)	5.409^{***} (0.86)	5.473^{***} (0.853)	
Sample size	35,661	35,661	
Panel D: Low Concentration			
Excess Costs	0.039^{***} (0.009)	0.038^{***} (0.009)	
Welfare cost (% of proc. budget)	3.048^{***} (0.768)	2.95^{***} (0.773)	
Sample size	72,212	72,212	
Panel E: High Concentration			
Excess Costs	-0.042 (0.082)	-0.033 (0.082)	
Welfare cost (% of proc. budget)	-3.916 (7.262)	-3.123 (7.27)	
Sample size	3,443	3,443	

Table IA16: Excess Cost Estimates by Sector and Concentration

Notes: The table reports excess cost estimates and corresponding welfare costs as a percentage of the procurement budget by type of sector. For each type of sector, we estimate excess costs at the 2-digit industry level and compute economy-wide averages using as weights the number of firms in each sector group. We explore heterogeneity by classifying sectors into A) Tradables, B) Wholesale and Retail trade, and C) Non-tradables, as in Caliendo et al. (2018). We also explore heterogeneity by industrial concentration below (Low, Panel D) or above (High, Panel E) the median sectoral Herfindahl-Hirschman Index. Welfare costs are estimated via equation 22, assuming that $\theta = 0$. Outcomes in Columns (1) assume flexible capital and are estimated via equation 30. All excess cost regressions control for year and 3-digit sector fixed effects. Standard errors (in parentheses) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. *** p<0.01, ** p<0.05, * p<0.1.

H Internet Appendix: Results for Firm Specialization

A policymaker conducting our set of counterfactuals might be concerned that the exercise unjustly penalizes politically connected firms if: 1) firms specializing in government output have to make investments that render them less productive overall but more efficient for the government sector; 2) firms specializing in government output provide higher utility in the government sector than in the private one; and 3) politically connected firms are more likely to specialize in government output. If this is the case, then comparing firms across different levels of specialization will bias the excess cost comparison against the specialized set of firms, and it will also bias the results against politically connected firms.

We offer a simple exercise to verify the robustness of our results to this concern. To do this, we conduct welfare comparisons within different levels of specialization. The working assumption is that within each specialization group, all firms suffer from the same shift in average productivity and the same shift in utility to the final consumer. Therefore, as our social excess cost estimators in 23 and 24 rely on differences between connected and non-connected firms, the unobserved parameters capturing the change in productivity or quality due to specialization cancel each other out.

To implement this new counterfactual, we must define which firms qualify as specialized. For most firms, government contracts represent only a small share of their total revenue. In Internet Appendix Table IA18, we show the distribution of the government supply share for government contractors at any point in time (Panel A) and restrict only to years in which they actively supply to the government (Panel B). In both cases, it is evident that government demand represents a small share of the sales for a government contractor, with a median firm supplying 0% of their output in any given year and only 12% of the output in years in which they actively supply to the government.

For the welfare analysis, we classify firms as specialized under various criteria. First, a static criterion defines a firm as specialized if its government supply share is greater than 50% or 75% in a given year. Second, a dynamic criterion requires that in the current time period and all future years, the government share is at least 50% or 75%.

Internet Appendix Table IA17 shows the results.⁷¹ For specialized firms, the results are volatile and noisy. For instance, under the dynamic definition, excess costs flip from negative when firms supply at least 75% of their output to the government to positive when we consider the 50% threshold, in both cases we cannot reject that the estimates are different from zero. The main reason is that we only have around 100 to 200 firms that are specialized under this strict criterion. Under the looser definition of static specialization, we find positive and significantly large excess costs of 5% for firms supplying at least 75% of their output to the government and 9% for firms supplying at least 50%.

Given that the majority of firms are not specialized in government supply, we highlight the counterfactual results for this set of firms as the most relevant exercise. Under all the different specifications, we find excess costs ranging from 3.5 to 4%, similar to those in the main text. These results support the notion that any possible bias against politically connected firms due to specialization is, if anything, of second order.

 $^{^{71}}$ Given that the excess cost estimation is conducted at the sectoral level, we require that at least 30 firms of each type are present in the sector.

	Non-Specialized		Specialized	
	Flexible capital (1)	Fixed capital (2)	Flexible capital (3)	Fixed capital (4)
Panel A: Dynamic, at least 75% toto	ıl revenue from goı	vernment		
Excess Costs	0.039^{***} (0.01)	0.038^{***} (0.01)	-0.196 (0.323)	-0.184 (0.322)
Welfare cost (% of proc. budget)	3.064^{***} (0.778)	2.991^{***} (0.781)	-18.56 (30.782)	-17.408 (30.722)
Sample size	75,567	75,567	108	108
Panel B: Dynamic, at least 50% tota	l revenue from gov	vernment		
Excess Costs	0.039^{***} (0.01)	0.038^{**} (0.01)	$0.123 \\ (0.158)$	$0.114 \\ (0.153)$
Welfare cost (% of proc. budget)	3.066^{***} (0.782)	2.98^{***} (0.786)	10.258 (13.152)	9.579 (12.762)
Sample size	75,352	75,352	216	216
Panel C: Static, at least 75% total re	evenue from govern	ament		
Excess Costs	0.037^{***} (0.01)	0.036^{***} (0.01)	0.052^{*} (0.03)	0.053^{*} (0.028)
Welfare cost (% of proc. budget)	2.939^{***} (0.791)	2.858^{***} (0.796)	4.256^{*} (2.465)	4.305^{*} (2.268)
Sample size	73,498	73,498	2,086	2,086
Panel D: Static, at least 50% total revenue from government				
Excess Costs	0.036^{***} (0.01)	0.035^{***} (0.01)	0.087^{***} (0.022)	0.089^{***} (0.022)
Welfare cost (% of proc. budget)	2.8^{***} (0.805)	$\begin{array}{c} 2.704^{***} \\ (0.809) \end{array}$	6.889^{***} (1.733)	6.999^{***} (1.696)
Sample size	72,066	72,066	$3,\!435$	$3,\!435$

Notes: The table reports excess cost estimates and corresponding welfare costs as a percentage of the procurement budget for specialized and non-specialized firms. We estimate excess costs at the 2-digit industry level and compute economy-wide averages using as weights the number of firms in each specialization-sector group. Standard errors (in parentheses) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 22, assuming that $\theta = 0$. Outcomes in Columns (1) and (3) assume flexible capital and are estimated as in equation 28. Specifications (2) and (4) assume fixed capital and are estimated via equation 30. All excess cost regressions control for year and 3-digit sector fixed effects. Columns (1)–(2) report results for non-specialized firms while (3)–(4) for specialized firms. Panel A (Dynamic) defines a firm as specialized at time t if the share of revenue obtained from the government is at least 75% at time t and all future years. Panel B (Dynamic) defines a firm as specialized at time t if the share of revenue obtained from the government is at least 50% at time t and all future years. Panel C (Static) defines a firm as specialized at time t if the share of revenue obtained from the government is at least 75% at time t. *** p<0.01, ** p<0.05, * p<0.1.

P25	Median	P75	P90	P95
(1)	(2)	(3)	(4)	(5)
Panel A: Al	l Years 0	0	.18	.49
Panel B: Or	nly Years with F	Positive Gover	rnment Sales	1
.03	.12	.41	.89	

Table IA18: Distribution of Government Supply Shares

Notes: The table reports the distribution of government supply share (total value of government contracts over total revenue). Panel A reports the distribution for all years, whereas Panel B reports the distribution for years when the firm sells positive output to the government.

I Internet Appendix: Hsieh and Klenow Estimates

To put our estimates into context, we present misallocation estimates using the traditional approach of Hsieh and Klenow (2009) (HK).

In addition to the assumptions made in our model, it is necessary to assume the following: 1) the policy does not alter technical productivity (TFPQ), and 2) TFPR and TFPQ follow a jointly log-normal distribution. Following Bau and Matray (n.d.), the HK framework measures misallocation from the variance in TFPR:

$$\Delta Welfare_s = -\frac{\sigma}{2} \Delta Var_s(\log(TFPR_i)) \times 100.$$
(53)

Relative to the HK framework, our methodology does not require TPFR and TFPQ to be jointly log-normal. As demonstrated by Bau and Matray (2023), this concern is not purely theoretical, as policies may affect the distributions, breaking log-normality.

We apply the HK framework using two approaches: 1) a slope framework based on the regression of the variance of TFPR on the share of connected firms, and 2) a change in variance method from removing connected firms from the sectors. We use $\sigma = 3$, as it is the common parameter used in the literature, and $\sigma = 12$, which matches our revenue elasticity estimates under constant returns to scale. We present the stability of the estimates to winsorization, a common practice since Hsieh and Klenow (2009), to address measurement error in TFPR, at the 0%, 1%, and 5%. We then compare these results with the welfare estimates from our main specification for fixed capital. The estimates are presented in Internet Appendix Table IA19. In all cases, standard errors for the estimates are obtained from 30 bootstrapped simulations, as in the main text.

In the first methodology, we utilize a regression framework to examine the change in the variance of TFPR. We link this change to the policy of interest, namely the variation in political connections, at the 2-digit sector and year level. Specifically, we estimate the variance of log TFPR for each 2-digit sector and year, and then regress this change in variance on the percentage share of connected firms in that particular year.

$$\widehat{\Delta Var_{st}} = \beta ShareCon_{st} + \varepsilon_{st},\tag{54}$$

where ΔVar_{st} is the empirical estimate of TFPR variance at the sector and year level, and $ShareCon_{st}$ is the share of connected firms. The slope coefficient in the regression captures the change in variance resulting from the policy. This coefficient provides an approximate linear measure of the effect of transitioning from an economy with 0% connected firms to a fully connected (100%) economy. This approach aligns with the focus of our welfare analysis.⁷²

Panels B and C show the results for the different winsorization levels for $\sigma = 3$ and $\sigma = 12$, respectively. The estimates appear somewhat stable after the 1% winsorization. The estimates suggest welfare losses that range between 23 to 38% for $\sigma = 3$ and between 92% to 150% for $\sigma = 12$. It is important to note that, first, the extrapolation from the regression coefficient greatly overstates the level of misallocation due to political connections,⁷³ as we move from an average connection rate at the sectoral level of 21% to 100%. Second, as the approach relies on cross-sectoral comparisons, we are not able to flexibly characterize the degree of sectoral inefficiencies. Third, these estimates are indeed affected by winsorization, whereas the results with our methodology (presented in Panel A) are remarkably stable.

In the second methodology, we consider two economies: the status quo economy, which includes both connected and non-connected firms, and an alternative economy where all connected firms are replaced randomly with non-connected firms while maintaining the same number of firms overall. We apply equation 53 above and compare the variance in TFPR in those two economies for each sector-year.⁷⁴ Our main results compare the welfare effects of reallocating one contract from a connected to a non-connected, so we adjust the change in welfare by the share of connected firms in the status quo economy. Lastly, as in our aggregate estimates, we use as weight the number of firms in the sector in the status quo economy to calculate the mean change in variance following the policy across sectors-years.

Panels D and E present the results. The welfare costs are now closer to our main results. We obtain welfare loss estimates that range from 0.3% to 2.2% for $\sigma = 3$ and from 0.9% to 8.9% for $\sigma = 12$. While indeed closer to our main results, we would like to highlight that, first, the estimates are greatly affected by winsorization, indicating that measurement error may bias results based on TFPR variance as suggested by Rotemberg and White (2017). Second, estimates are again extremely noisy, and we fail to reject the null hypothesis of no inefficiencies at any relevant level. Instead, our method is robust to winsorization and, as showcased in various exercises, is powerful enough to significantly explore heterogeneity across sectors, provinces, and other dimensions.

⁷²Note that it should be feasible to estimate sector-level misallocation estimates through this approach, by looking at, for instance, dispersion at the 3-digit level and running the regression for each 2-digit sector. In practice, however, the methodology might be underpowered to explore significant heterogeneity.

 $^{^{73}}$ Carrillo et al. (2023) finds that HK greatly overstates misallocation in public procurement in Ecuador.

⁷⁴This should also be possible to conduct at more granular levels to obtain wide sector-level estimates. In practice, the results might be underpowered.

	Winsorization Level		
	0%	1%	5%
Panel A: Main S	Specification - Fix	ed Capital and LP	-Wooldridge
Welfare cost	2.975^{***}	2.891***	2.703^{***}
	(0.777)	(0.750)	(0.660)
Sample Size	75,791	75,791	75,791
Panel B: HK - s	lope of var(TFPR	R) on $%$ connected	for $\sigma = 3$
Welfare cost	22.891	37.520	34.711***
	(28.464)	(24.261)	(10.6237)
Sample Size	294	294	294
Panel C: HK - s	lope of var(TFPR	R) on % connected	for $\sigma = 12$
Welfare cost	91.563	150.078	138.842***
	(113.856)	(97.044)	(42.494)
Sample Size	294	294	294
Panel D: HK - Δ var(TFPR) for $\sigma = 3$			
Welfare cost	2.231	0.774	0.228
	(2.834)	(2.304)	(1.017)
Sample Size	75,791	75,791	75,791
Panel E: HK - Δ var(TFPR) for for $\sigma = 12$			
Welfare cost	8.926	3.094	0.914
	(11.336)	(9.215)	(4.066)
Sample Size	75,791	75,791	75,791

Notes: The table reports welfare costs from our main results and those obtain through methods similar to Hsieh and Klenow (2009). Panel (A) details welfare costs according to our main framework, employing LP-Wooldridge and Fixed Capital approaches, with sector weighting by firm count. Panels (B) and (C) report outcomes from regressing var(log(TFPR)) on the percentage of connected firms at the 2-digit sector level and year, for $\sigma = 3$ and $\sigma = 12$ respectively. Panels (D) and (E) outline the average welfare change, quantified as the average $\Delta var(log(TFPR))$ resulting from substituting connected firms with randomly selected non-connected firms, at the 2-digit sector level and year, for $\sigma = 3$ and $\sigma = 12$, respectively, again with sector weighting by firm count. Column (1) displays results without winsorization, Column (2) applies winsorization at the 1% level, and Column (3) at the 5% level. Standard errors in the main analysis are derived from 30 bootstrap simulations used to calculate production function elasticities. *** p < 0.01, ** p<0.05, * p<0.1.

J Internet Appendix: Estimating the Political Connection Premium

In our model, we assumed that politically connected firms charge an additional premium to the government, in line with other papers in the literature. We also provided an empirical framework to deal with the premium in production function estimation. In this section, using a small subset of the data, we show that the assumption of the political premium is also likely to hold in our setting.

Verifying that the political premiums exist is not as simple as comparing prices between connected and non-connected firms. It could be the case that prices are different only because of productivity differences. After all, absent the political premium, prices are determined using a markup rule over marginal costs, and marginal costs are proportional to the productivity of firms.

With a political premium, price differences will include both the premium and the marginal cost differences. To see this, notice that, from equation 11, the average government prices at the sector can be decomposed into average prices for the politically connected and non-connected firms:

$$\overline{P}_{st}^{gov} = \overline{P}_{st}^{gov,c} S_{st}^c + \overline{P}_{st}^{gov,u} S_{st}^u,$$

where the average price for non-connected firms is given by

$$\overline{P}_{st}^{gov,u} = \tau_{st} \int_{i \in F_{st}^u} \frac{\sigma}{\sigma - 1} C'(Q_{it}) S_{it}^{gov} di,$$

and for connected firms is

$$\overline{P}_{st}^{gov,c} = \tau_{st} (1+\mu_s) \int_{i \in F_{st}^c} \frac{\sigma}{\sigma-1} C'(Q_{it}) S_{it}^{gov} di.$$

Then, the difference in log government prices of connected relative to non-connected will be given by the difference in marginal costs and the political connection premium μ_s :

$$\Delta ln(\overline{P}) = \Delta ln(\overline{C'(Q)}) + ln(1+\mu_s), \tag{55}$$

where $\Delta \overline{C'(Q)}$ is the difference in the weighted-average marginal cost across groups.

Equation 55 tells us that information on prices is not enough to infer the political premium. We also need a measure of the marginal cost differences across the groups.

While we do not have estimates of marginal costs for all sectors, in our main exercise we do obtain measures of average differences in *quality-adjusted* marginal costs. For sectors in which we expected relatively no quality difference across firms, the excess costs estimates will capture only differences in marginal costs. Hence, we can use information on prices, the excess cost estimates, and equation 55 to obtain an estimate of the political connection premium in the sector.

J.1 Estimating Price Differences

In this section, we estimate the price difference between politically connected and unconnected firms, i.e., the left-hand side of equation 55. For this, we use price data of standardized goods and services observed in the e-catalogue. Internet Appendix Table IA20 presents basic summary statistics of the data. Our data contains 958,823 transactions, with an average transaction value of US \$1,621, and an average unit price of US \$161. There is considerable competition for the goods and services provided through the electronic catalog, as the yearly average number of suppliers for a given product is 50.1.

Let P_{ijat} denote the price charged by firm *i* for one unit of good *j* to a government agency *a* at time *t*. This is computed as the ratio between the total value of the contract and the quantity of goods procured. We then define the standardized log price $p_{ijat} = \log(P_{ijat}) - \bar{p}jt$, with $\bar{p}jt$ denoting the average log price of product *j* across all firms in a given year *t*. Similarly, let $q_{ijat} = \log(Q_{ijat}) - \bar{q}_{jt}$ be the demeaned log quantity of good *j*.⁷⁵ To make the standardization meaningful, we drop observations of goods that are sold by a single contractor over the course of a year.⁷⁶ This allows us to compare the price that a firm charges for a given standardized good relative to other contractors supplying the same good in the same year. We can then use the demeaned price to measure the premium charged by politically connected firms for the goods they provide. In practice, we estimate the following regression:

$$p_{ijat} = (\beta_1 Pre_{it}^{PC} + \beta_2 Post_{it}^{PC}) \cdot FirmContractor_{it} + (\beta_3 Pre_{it}^{PC} + \beta_4 Post_{it}^{PC}) \cdot PersonContractor_{it} + \gamma q_{ijat} + \nu_a + \nu_t + \varepsilon_{ijat},$$
(56)

where Pre_{it}^{PC} is an indicator for politically connected contractors that have not yet established their first link with bureaucracy, while $Post_{it}^{PC}$ is an indicator for the years following the connection.⁷⁷ These two variables capture the average over- or under-pricing behavior relative to non-connected contractors. For this part of the analysis, we include contractors registered as individuals (as opposed to firms only), as they provide valuable information to calculate the mean prices \bar{p}_{jt} . The indicator variable $PersonContractor_{it}$ is equal to one when the contractor is registered as an individual, whereas $FirmContractor_{it}$ is equal to one when the contractor is registered as a firm. The coefficient on the interaction $Post_{it}^{PC} \cdot FirmContractor_{it}$ is our estimate of the average price difference between politically connected and non-connected firms.

We control for agency and year fixed effects, represented by ν_a and ν_t respectively. Agency fixed effects are introduced to account for the possibility that some agencies systematically pay more than others for the same good (Bandiera et al., 2009). We include deviations from the average quantity, q_{ijat} , to entertain the possibility that bulk discounts are applied to contracts involving large quantities of goods or services. Lastly,

⁷⁵Similar normalizations are used, for example, by DellaVigna and Gentzkow (2019).

⁷⁶We also exclude medicine purchases, as the process for defining the set of providers differs from the other products procured through the electronic catalog.

⁷⁷Notice that the coefficients of interest capture averages at the contractor-year level, while the unit of observation in the regressions is the transaction level. This introduces differential weighting across contractors if transactions are unevenly distributed among them. With this in mind, we run a second set of regressions where we average all variables at the contractor-year level.

 ε_{ijat} denotes the error-term.

Internet Appendix Table IA21 reports the differences in prices. Under the transactionlevel sample, the estimated price difference after the connection is active is of 3.5% (Column (1)). While the connection was not active, we do not find any difference in prices between contractor types. Column (2) reruns the analyzing by using as dependent variable the average demeaned price charged by a contractor in a given year. Adopting this specification, we estimate a price difference of 6.4%, and again no statistically significant difference prior to the connection.

J.2 Back-of-envelope Premium Estimate

We now use equation 55, the estimated price differences, and marginal cost differences to obtain a back-of-envelope political premium estimate:

$$\mu_s = exp\left(\Delta ln(\overline{P}) - \Delta ln(\overline{C'(Q)})\right) - 1.$$
(57)

Note that 68% of the transactions in the e-catalogue came from ISIC sector "G-46" (wholesale) and 23% from sector "G-47" (retail) when the items were sold by a firm. These sectors have excess costs point-estimates of -2.0% and 0.4% (both not significantly different from zero). Given that these sectors trade more homogeneous goods, it could be argued firms do not have quality differences in the goods they offer. If that is the case, then the excess costs estimates actually provide estimates for the differences in marginal costs. For these sectors, we therefore find marginal cost differences between politically connected and non-connected firms that range between -2.0 and 0.4% (although these differences are not statistically significant).

Prior to the political connection, the null differences between contractor types in both marginal costs and prices imply that politically connected firms did not obtain a price premium. Instead, once the connection is active, we do find a political connection premium. Given a price difference of political connection between 3.5% and 6.4% and excess costs estimates of -2.0% and 0.4%, equation 57 implies political connection premiums that range from 3.1% up to 8.8%. This range is consistent with previous empirical work, our assumption of a positive political premium, as well as the imputed political premium of 6% in some of the sensitivity specifications in the main text.

Contract value (\$) (1)	Unit price (\$) (2)	Quantity (units) (3)	Number of transactions (4)	Number of competitors (5)
$1,\!621$	161	1,224	$958,\!823$	50.11
(50, 882)	(8,496)	(186,759)		(257.95)

 Table IA20:
 Descriptive Statistics of Electronic-Catalog Transactions

Notes: The table reports means and standard deviations (in parentheses) for the universe of transactions recorded in the electronic catalog in the period 2014-2018. We exclude all medicine purchases. Dollar values are deflated by the consumer price index series computed by the World Bank (https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=EC). The number of competitors corresponds to the number of sellers for a specific product in a given year.

	Standardized price (1)	Average price (2)
Before political connection	-0.0085 (0.0066)	$0.0245 \\ (0.0993)$
After political connection	$\begin{array}{c} 0.0348^{***} \\ (0.0024) \end{array}$	$\begin{array}{c} 0.0642^{***} \\ (0.0207) \end{array}$
P-value difference	0.000	0.693
Sample size	881,709	$23,\!378$
R-squared	0.1120	0.0049
Year FE	Yes	Yes
Agency FE	Yes	No
Quantity Control	Yes	Yes

Table IA21: Price Inflation Estimates

Notes: Columns (1)–(2) use electronic catalog transactions (excluding medicine). We drop observations for products provided by a single contractor in a given year and compute product-level demeaned log prices (winsored at the 1st and 99th percentile of the respective distribution). In Column (1), the unit of observation is the transaction level, while Column (2) takes averages at the contractor-year level. In all specifications, we report coefficients of an indicator for firm contractors in the years before their first political connection and an indicator for the years after connection. The omitted category is an indicator for transactions executed by non-connected firm or person contractors. The estimates for the years after connection correspond to our price inflation estimates. All regressions control for indicators for politically connected person contractors before and after connection (not reported). Columns (1) additionally control for standardized log quantities at the transaction level, while Column (2) controls for average log quantities at the contractor level. We control for year and agency fixed effects as indicated in each column. We cluster standard errors at the agency level in Column (1) and use robust standard errors in specification (2). *** p<0.01, ** p<0.05, * p<0.1.

K Internet Appendix: Extension for Multi-Products Firms

A natural concern is that firm-level welfare comparisons may not fully capture the nuances if firms sell a variety of products, as firm-level revenue productivity might not directly correspond to firm-product-level revenue productivity. Here, we illustrate that our methodological contribution is sufficiently flexible to accommodate this complexity. Given access to product-level information, similar to the approach in De Loecker et al. (2016), conducting a welfare analysis at the product level becomes feasible. We outline
the model and the necessary modifications to our sufficient statistics equations provided in the main text.

The consumer preferences and production technology follow the frameworks of Bernard et al. (2011) and De Loecker et al. (2016). Consumer preferences within a product j in sector s at time t are given by:

$$U_{jst}^{pri} = \left(\int_{i\in F_{jst}} (exp(z_{ijt})Q_{ijt}^{pri})^{(\sigma-1)/\sigma} di\right)^{\sigma/(\sigma-1)},\tag{58}$$

where F_{jst} is the set of firms selling good j, $exp(z_{ijt})$ is the firm-specific product quality, Q_{ijt}^{pri} is the amount of good supplying by firm i of product j.

The firm i has a production function for good j at time t given by:

$$Q_{ijt} = L_{it}^{\alpha_l^j} M_{ijt}^{\alpha_m^j} K_{ijt}^{\alpha_k^j} exp(\omega_{it} + u_{ijt}), \qquad (59)$$

where the production function is product-specific, reflected in the product-specific elasticities, but productivity is firm-specific. As highlighted by De Loecker et al. (2016), this formulation allows for economies of scope.

The revenue production function at the product level is estimated by the econometrician as follows:

$$r_{ijt} = \beta_l^j \overline{l}_{ijt} + \beta_m^j \overline{m}_{ijt} + \beta_k^j \overline{k}_{ijt} + \omega_{ijt}^* + \psi_{jst}^* + \xi_{ijt}^* + \varepsilon_{ijt}, \tag{60}$$

where revenue productivity ω_{ijt}^* combines firm-level productivity ω_{it} , sectoral-demand elasticities, and firm-product quality z_{ijt} .

The estimation strategy would then follow the methodology of De Loecker et al. (2016), which uses single-product firms to estimate the elasticities for each product.⁷⁸ Then, the estimated production functions are used to obtain estimates of the firm-product-level revenue productivities.

For flexible capital, the social excess costs of procuring product j from politically connected firms, in contrast to non-connected firms, are expressed as:

$$SOEC_{flex}^{j} = exp\left(\frac{\omega_{ijt}^{*unc} - \omega_{ijt}^{*unc}}{\beta_{l}^{j} + \beta_{m}^{j} + \beta_{k}^{j}}\right) - 1.$$
(61)

For fixed capital scenarios, the methodology necessitates estimating product-specific input shares ρ_{ijt} , as outlined by De Loecker et al. (2016). The social excess costs at the product level, when capital is fixed, are approximated by:

$$SOEC_{fixed} \approx exp\left(\frac{\beta_k^j}{\beta_l^j + \beta_m^j + \beta_k^j} [ln(S_{ijt}^{k,unc}) - ln(S_{ijt}^{k,con})] + \frac{\omega_{ijt}^{*unc} - \omega_{ijt}^{*con}}{\beta_l^j + \beta_m^j + \beta_k^j}\right) - 1, \quad (62)$$

where $S_{ijt} = \rho_{ijt} \overline{K}_{it} / R_{ijt}$ for firm-level capital \overline{K}_{it} and product-level revenue R_{ijt} .

 $^{^{78}{\}rm The}$ estimation requires that the effect of political connections on demand is the same for single firms as for multi-product firms.

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