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.

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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 adverse impact the execution of the 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 lower 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. As a result, the net effect of political connections is theoretically ambiguous, depending on which force dominates.

This paper studies the welfare effects of political connections in public procurement, a sector that represents 12% of the global GDP (Bosio et al., 2020). We develop a framework to flexibly measure the welfare consequences of political connections in terms of *costs of production per utility unit* for the final consumer that arise from assigning a contract to a connected firm instead of a non-connected one. 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 revenue given inputs) and capital intensity of the firms. In line with the theoretical ambiguity, our method allows for positive, neutral, or negative welfare effects. Moreover, our framework shifts the focus away from standard analysis studying allocative efficiency relative to first-best output (e.g., as in Hsieh and Klenow, 2009), by comparing allocations between two arbitrary groups, both of which may be possibly misallocated.[3]

We apply our methodology to study public procurement misallocation in Ecuador, by relying on several administrative databases for 2007-2017. Our data combines detailed micro-level data on procurement contracts, firms' balance sheet statements, and firms'

[1] See, 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), and Baltrunaite et al. (2020).

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

[3] One example of another application is to compare the efficiency effects of allocating a contract to small vs. large firms, in line with preferential policies implemented in public procurement in many countries.

political connections. We rely on data covering the universe of private business shareholders and bureaucratic employees to overcome the challenge of identifying firm-level political connections. Specifically, we consider the political connections of private firms obtained through the *ownership channel* and define a firm as politically connected if any of the shareholders or their siblings start working for the government as a bureaucrat. Our main empirical contribution is then measuring the welfare consequences of these political connections in public procurement. We find that political connections have a negative effect on welfare, suggesting that rent-seeking incentives may dominate the 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 data's time dimension, we implement an event-study methodology proposed by Callaway and Sant'Anna (2021) to estimate the dynamic effects of political connections on the probability of winning procurement contracts. We find that when firms establish their first political connection, they benefit from a 2.6 percentage points 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.[4] The effects are robust to various methodologies recently proposed in the event-study literature (Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020) and to the traditional two-way fixed-effects approach.

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 seem to be 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, and are stronger when the contract is executed in the same province as the headquarters of the firm. 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). As a final piece of motivating evidence, using a subset of contracts with prices for standardized goods and services, we document that politically connected firms charge higher unit prices *only after* the connection is active.

At face value, these results are indicative of reallocation of contracts but remain silent regarding the efficiency consequences. After all, the appointment of the bureaucrat may have the objective of reducing informational asymmetries, given their expertise in the

[4] This 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.

sector. Therefore, despite the additional rents that connected firms may collect due to the higher prices and larger number of contracts, if they are as quality-efficient as non-connected firms, these rents would simply be transfers. Alternatively, the appointments may create rent-seeking opportunities where the bureaucrat redirects resources to their family despite them not being the ideal contractors for the job.

For that reason, 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 of the two types of firms. Intuitively, revenue productivity captures both quantity productivity (how much each firm needs to spend in resources to reach a 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 output in 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 reach 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 previous empirical literature that shows that connected firms charge higher unit prices to the government (Szucs, 2020; 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. The 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 was allocated to a non-connected firm instead of a politically connected firm.

The estimated effects are robust to various specifications that address different potential biases. The results hold if we estimate the production functions using only observations before political connections are established. Besides dealing with possible endogeneity of input intensity and political connection status, this result also shows that political connections are not arising from expected efficiency gains, as if they were, we would expect positive efficiency effects. Moreover, we find that the welfare effects are not driven by the assumption of the political connection price premium, as the estimates are robust to imputing them directly (rather than dealing with them in estimation) or even after requiring prices offered to the government to be equal across all firm types in a given sector. We also find that the results are not driven by the cost curvature assumption as the welfare effects are still negative if we force constant marginal costs by allowing capital as a flexible input. Overall the robustness exercises, we find welfare losses of up to 6% of the government budget spent on politically connected firms.

To ease concerns about the definition of the comparison groups, we conduct several welfare calculations restricting to firms only within the same province, asset quartiles, and government demand (specialization) levels. In all such cases, we still find significant welfare losses coming from connections. Furthermore, as the strictest tests, we obtain welfare estimates after controlling for contract-level characteristics, such as contract type, province, or agency, or after limiting the comparison of firms to those competing for the same contract by using contract fixed effects. Using contract-level comparisons, we find losses that range between 5 to 6% of the government budget.[5] Looking at whether the nature of the connection matters for the effect, we still find welfare losses if we restrict to plausibly fortuitous connections, such as those coming from a large office reshuffle or indirectly to siblings. These results would suggest that is not only some set of inefficient firms that actively search to create connections, but instead, that given the opportunity, firms will exploit this resource to benefit privately at the expense of society.

Finally, we study the heterogeneity of the effects across sectors in the economy that differ on their level of standardization and find results in line with our priors: wholesaling and retailing sectors find small or no efficiency losses, whereas engineering, telecommunications, and consultancy services observe large and significant welfare losses. At the same time, we obtain that misallocation effects are smaller and non-significant when restricting the sample to firms that only participate in more competitive contracts, such as auctions and lotteries, while the effects are large and significant for the set of firms that participate in discretionary contracts or a mix of types of contracts. Overall, the results

[5] As discussed above, our estimator allows us to compare any arbitrary groups, for instance, non-connected winners to non-connected losers. When performing such a comparison at the contract level, we find that procuring from winning firms generates, on average, efficiency gains of around 2 cents per dollar spent. These results highlight the usefulness of our framework for policymakers for assessing the efficiency of different procurement methods, either already in use or consideration for implementation.

are consistent with political connections transferring rents at the expense of efficiency rather than solving informational asymmetries.

Our analysis has important limitations. First, despite our efforts to narrowly define counterfactual allocations by looking at sectoral competitors in the same contract or limiting 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 efficiency estimated primarily 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. Instead, if in line with previous evidence in the US (Brogaard et al., 2021), India (Ryan, 2020), and Korea (Schoenherr, 2019), connections worsen those properties, then our estimates would serve as a lower bound.

This paper contributes to several strands of literature. First, it speaks to the literature that studies the relationship between public procurement and political connections. Recent empirical papers have shown that politically connected firms win more contracts than non-connected firms (Goldman et al., 2013; Tahoun, 2014; Do et al., 2015). However, connected firms execute these contracts with more delays and at a higher cost (Schoenherr, 2019), obtain more favorable renegotiation terms (Brogaard et al., 2021; Ryan, 2020), charge higher prices (Szucs, 2020; Baranek and Titl, 2020), are less efficient (Szucs, 2020), and experience declines in sales after anti-corruption crackdowns in public spending (Colonnelli and Prem, 2020). Our results complement these findings by confirming that politically connected firms are allocated more procurement contracts, are less productive, and charge higher prices than non-connected firms in a new setting in the developing world. Moreover, our paper focuses on ownership as the connections channel, which has been relatively unexplored.[6]

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

[6] More broadly, our paper contributes to the literature that establishes the existence of a positive relationship between political connections and firm performance. This association has been recently documented for many developed and developing countries 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 closest paper to ours is Baltrunaite et al. (2020). Like them, we offer two innovations relative to previous works by focusing on private firms, which are more prevalent in the developing world, and by classifying a firm as connected using ownership information. Two additional papers also define a firm as politically connected through ownership and study private firms, although their sample is smaller than ours. Rijkers et al. (2017) classifies a firm as connected if it was 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 our study, instead, we follow 6,030 politically connected government contractors.

context of public procurement. To the best of our knowledge, only Schoenherr (2019), Baranek and Titl (2020), and Szucs (2020) study the allocative efficiency of procurement contracts. Schoenherr (2019) does it by quantifying the social costs of delay and estimating the additional government expenditures due to ex-post cost surges caused by political connections. In contemporaneous work, Baranek and Titl (2020) quantifies the total transfers from the government to connected firms due to overpricing. On the other hand, Szucs (2020) studies the welfare effects of different entry thresholds into high-discretion procurement procedures on production and administrative costs. In our paper, instead, we develop a framework that considers the social losses in terms of inefficient use of production inputs obtained from awarding contracts to less quality-efficient firms.[7]

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 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 capital-revenue 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. To our knowledge, the methodology of Hsieh and Klenow (2009) would not be able to obtain estimates of the welfare effects of such

[7]As Cingano and Pinotti (2013) do not have direct measurement of who wins a public procurement contract, they estimate the allocative effects of political connections in the aggregate, relative to a fully efficient, with estimates that range between 0 and 120%, depending on the calibration parameter. Instead, our framework directly estimates the misallocation (without the need for calibration), with precisely estimated excess costs. Moreover, our framework benchmarks against a (plausibly) inefficient world (those in which non-connected contractors win the contract), and explores heterogeneity effects by detailed contractor, location, and contract characteristics.

types of counterfactuals. 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-rank 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.[8] For our analysis, we exclude individuals with non-administrative jobs in schools, hospitals, and military institutions.[9]

[8] Even if records are reporting a start date as early as 1970, the coverage of the data becomes representative of the public labor force in the early 2000s.

[9] Although 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.

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.[10] In combination with the bureaucratic database, we are able to 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.[11] Agencies use the platform to post calls for tenders and registered suppliers use it to submit their bids.[12]

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 1$^{\text{st}}$ and above the 99$^{\text{th}}$ 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

[10]The dataset does not keep 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.

[11]The portal is administered by the *Superintendencia de Compras Públicas* (Public Procurement Bureau) and can be accessed at https://www.compraspublicas.gob.ec/ProcesoContratacion/compras/PC/buscarProceso.cpe?sg=1#.

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

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 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.[13]

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

[13] The use of the companies' legal names in our matching algorithm has the objective to limit the number of wrong matches that could arise in case of reporting mistakes in the firm IDs between different data sources.

follows. First, we construct "families" using the two last names of each individual recorded in our data.[14] 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.[15] As shown in 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 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 say that a firm has a *direct* connection 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.[16]

Since owners may sell their shares of a company to hide their political links, our definition of political connection considers both current and past owners. However, we exclude two groups of connected firms from our analysis. The first group consists of

[14] 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.

[15] 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 to have more than 15 siblings, as these are unlikely to be real connections.

[16] See, for example, European Commission (2015), section 4.4.

businesses whose shares are bought by individuals already working as public officials (we refer to these connections as a "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.[17]

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.[18] Of the politically connected firms, 46% of connections are exclusively direct, 23% 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.[19] 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

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

[18] The 6,030 connected firms exclude 1,384 firms that are strategically connected and 509 firms that are created by bureaucrats.

[19] We 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).

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.[20] 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.

Table 3 shows 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 that are allocated in a discretionary way. Quotations and other discretionary 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, either by exerting influence or by reducing informational asym-

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

metries, the allocation of government procurement contracts. This finding motivates our following analysis of the welfare effects for 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 of contracts awarded to firms and their political connection status in an event-study design. For simplicity, although firms can establish links with multiple bureaucrats, 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.[21] 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 :

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

which captures the causal 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 formula-

[21]The first treatment period consider is $G_i = 2$, to allow for pre-treatment observations to occur in all calendar periods.

tion 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. 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 $P(G = g|G + e \leqslant T)$ captures the size of the group, i.e., the unconditional probability of treatment in year $G_i = g$, and $\beta(e)$ is the equivalent of 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 $e < 0$.[22]

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

3.2 Results: Political Connections and the Reallocation of Contracts

Figure 2 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 is very similar if we replace the dependent variable with the yearly value of procurement contracts won (Appendix Figure IA2). Note that although both figures, there is a non-significant (at the 95% level) decrease two-years priors the connection, the overall path from four years prior treatment is relatively stable. Overall, we take these results as motivation that political connection *may* generate efficiency gains or losses due to the reallocation of contracts.

In Appendix Table IA1, we present sensitivity analyses of the post-treatment average treatment using various methodologies, which hold under different parallel trend assumptions and different control groups.[23] In Column (1), we present the post-treatment estimate using Callaway and Sant'Anna (2021), which uses the group of not-yet-treated

[22]For this representation to capture the casual treatment effect, the main two identifying assumptions are: 1) limited treatment anticipation, and 2) unconditional parallel trends on the not-yet-treated groups.

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

and never-treated as control. In Column (2), we implement Sun and Abraham (2021), which uses the last-to-be-treated and never-treated as control. In Column (3), we use De Chaisemartin and d'Haultfoeuille (2020), which uses not-yet-treated as controls. In Column (4) we present the usual TWFE estimate. All different methodologies produce similar point estimates of around 2-3 percentage points.

To further understand the nature of the reallocation, Table 4 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.[24] 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)).

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 $\widetilde{p} \neq p$. The results show that, although 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 statistically significant at home. 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 IA2, 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.[25] 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

[24]We 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.

[25]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.

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, our results may be biased by the fact that some bureaucrats sell their shares after they start working in the public sector. We drop this set of potentially "strategic" exits in Column (3). 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, that is, through the bureaucrat or the sibling of the bureaucrat. We consider indirect linkages to be more likely to be considered 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. Proportionally, given their initial base probability, the size of the effect is stronger with indirect connections than with direct connections.

Furthermore, in Panel C, we present three falsification exercises. First, Column (1) 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 in these fake treatment years. Second, Column (2) considers only connections through families having more than 15 siblings,[26] 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 to be 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.

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 Appendix Section F. 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. Yet, after the connection is established, we find statistically significant price differences, with connected

[26]This corresponds to the 95^{th} percentile of the family size distribution in our data

contractors charging higher prices (between 3.5% to 6.4%) for the same good.

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 and the same provinces as the firm's HQ. Moreover, besides plausible efficiency effects which are yet to be studied below, these connections may have a direct impact on the prices paid by the government for similar goods.

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.

4.1 A Production Function Framework

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

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

where L_{it} denotes labor, M_{it} intermediate inputs, and K_{it} capital. The output elasticities for 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:

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

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

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

where P_{it} 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.[27] 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. We consider firm-level demand to be determined by all of these factors in the following way:

$$Q_{it}^{gov} = \tilde{D}(Post_{it}^{PC}) G(C'(Q_{it}), z_{it}) exp(\xi_{it}) = D(Post_{it}^{PC}) exp(\xi_{it}) Q_{it}^{pri}, \tag{7}$$

where $Post_{it}^{PC}$ is a binary variable capturing political connection status and $\tilde{D}(Post_{it}^{PC})$ is an unknown function that increases government demand, everything else equal, if the firm

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

is politically connected in line with our motivating evidence. Moreover, $G(C'(Q_{it}), z_{it})$ is an unknown function that decreases in marginal cost and increases in quality. 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, $exp(\xi_{it})$ is the exogenous demand shock that pins down the exact level of government contracts. The last equality assumes that under a transformation to the function $D(\cdot)$, we can capture the effects of productivity and quality by the private sector quantity.

We assume that prices in the government sector are determined by a *no arbitrage* condition. That is, the government is only willing to pay as much as it would have to pay if it purchased the goods directly through the private sector. This forces government prices to be identical to prices in the private market. However, in line with our previous reduced-form evidence and the literature,[28] we allow politically connected firms to charge an additional premium μ_s to the government:

$$P_{it}^{gov} = \begin{cases} P_{it}^{pri} & \text{if } Post_{it}^{PC} = 0, \\ P_{it}^{pri}(1+\mu_s) & \text{if } Post_{it}^{PC} = 1. \end{cases} \tag{8}$$

As mentioned above, we show evidence that in our setting connected firms may indeed receive an additional premium (refer to Internet Appendix F).

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, $B_{st} = \int_{i \in F_{st}} P_{it}^{gov} Q_{it}^{gov} di$. By dividing by total government quantity in the sector, $\overline{Q}_{st}^{gov}$, average prices for government goods in the sector are then defined by

$$\overline{P}_{st}^{gov} = \int_{i \in F_{st}} P_{it}^{gov} S_{it}^{gov} di, \tag{9}$$

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

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

4.1.3 Total Output and Revenue

For each year t, we assume government and private demand are set contemporaneously and instantaneously. Then, equations 5 and 7 imply that total firm-level demand is

$$Q_{it} = \left(1 + D(Post_{it}^{PC})exp(\xi_{it})\right)exp(z_{it})^{\sigma-1}\left(\frac{P_{it}}{P_{st}}\right)^{-\sigma}\frac{E_{st}}{P_{st}}. \tag{10}$$

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

$$U_{st} = U_{st}^{pri} + U_{st}^{gov}, \tag{11}$$

with the experienced utility from public goods given by

$$U_{st}^{gov} = \iota \cdot \left(\int_{i \in F_{st}} (exp(z_{it})Q_{it}^{gov})^{(\sigma-1)/\sigma} di\right)^{\sigma/(\sigma-1)}, \tag{12}$$

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.

Total revenue of the firm is $R_{it} = P_{it}^{pri}Q_{it}^{pri}\left(1 + D(Post_{it}^{PC})exp(\xi_{it})(1 + \mu_s Post_{it}^{PC})\right)$. Following De Loecker (2011), we use the inverse demand function implied by 10 to obtain an expression for prices. Substituting in the revenue equation, we obtain:

$$R_{it} = exp(z_{it})^{\frac{\sigma-1}{\sigma}} Q_{it}^{\frac{\sigma-1}{\sigma}} X(Post_{it}^{PC}, \xi_{it})\kappa_{st}, \tag{13}$$

with κ_{st} collecting sectoral related terms, and $X(Post_{it}^{PC}, \xi_{it})$ grouping the terms related to political connection and government demand.[29] As, in equilibrium, quantity demanded equals quantity produced, we substitute the production function 3 into 13

$$R_{it} = exp(z_{it})^{\frac{\sigma-1}{\sigma}} L_{it}^{\beta_l} M_{it}^{\beta_m} K_{it}^{\beta_k} exp(\omega_{it} + u_{it})^{\frac{\sigma-1}{\sigma}} X(Post_{it}^{PC}, \xi_{it})\kappa_{st}, \tag{14}$$

where the revenue elasticity for input h is $\beta_h \equiv (\frac{\sigma-1}{\sigma})\alpha_h$.

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 $\overline{K}_{it} = r_{st}K_{it}$. Equation 14

[29] Precisely, let $X(Post_{it}^{PC}, \xi_{it}) \equiv \left(1 + D(Post_{it}^{PC})exp(\xi_{it})\right)^{\frac{1}{\sigma}}\left(1 + D(Post_{it}^{PC})exp(\xi_{it})(1 + \mu_s Post_{it}^{PC}\right)$.

becomes:

$$R_{it} = \overline{L}_{it}^{\beta_l} \overline{M}_{it}^{\beta_m} \overline{K}_{it}^{\beta_k} exp(\omega_{it} + z_{it} + u_{it})^{\frac{\sigma-1}{\sigma}} \Psi_{st}^{-1} X(Post_{it}^{PC}, \xi_{it}) \kappa_{st}, \tag{15}$$

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. Our formulation implicitly assumes that all firms in a given sector s face the same input prices at a given point in time t.

Taking logs of equation 15, we obtain

$$r_{it} = \beta_l \bar{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega_{it}^* + \psi_{st}^* + \xi_{it}^* + \varepsilon_{it}, \tag{16}$$

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 (Ψ_{it} and κ_{st}), and $\varepsilon_{it} = (\frac{\sigma-1}{\sigma})u_{it}$ is the transformed shock. The term $\xi_{it}^* = \ln(X(Post_{it}^{PC}, \xi_{it}))$ is an unknown firm-level parameter capturing the government demand shocks and the effect of political connections on revenue.

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

$$\begin{aligned} \mathcal{L}(L_{it}, M_{it}, K_{it}, w_{st}, \rho_{st}, r_{st}, \lambda_{it}) = {} & w_{st} L_{it} + \rho_{st} M_{it} + r_{st} K_{it} \\ & + \lambda_{it} \left(Q_{it} - L_{it}^{\alpha_l} M_{it}^{\alpha_m} K_{it}^{\alpha_k} exp(\omega_{it}) \right). \end{aligned} \tag{17}$$

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:

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

Let the quality-embedded quantity be $\widetilde{Q}_{it} = Q_{it} exp(z_{it})$. Across sellers, each unit of quality-embedded quantity yields the same utility level. In this respect, the relevant measure of excess costs refers to the additional expenditures per unit of quality-embedded quantity. Therefore, although firms minimize costs given gross output, Q_{it}, we derive an expression of the social excess cost in terms of quality-embedded marginal costs.

The next lemma links quality-embedded quantity marginal costs to gross quantity marginal costs.

Lemma 1 *Quality-embedded marginal costs are proportional to gross quantity marginal costs:*

$$C'(\widetilde{Q}_{it}) = \frac{C'(Q_{it})}{exp(z_{it})}.$$

We use the equivalence in Lemma 1 to obtain quality-adjusted expressions for social excess costs.

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. To quantify the dollar value of losses of allocating B dollars to a share $1 - \theta$ of firm-type c rather than u, we define the deadweight loss in costs:

$$DWLC = (1 - \theta)B\Delta C' = (1 - \theta)B(SOEC)C'_u, \tag{18}$$

where C'_u is the marginal cost of firm-type u.

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 $t - 1$.

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.[30]

[30]A complete derivation is shown in Appendix D.

Proposition 1 *With CRST 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*

$$SOEC_{flex} = exp\Big(\frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\Big) - 1. \tag{19}$$

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.

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 pre-determined by investments at time $t-1$, allowing for non-linearity in the cost function. The cost minimization problem for a fixed level of capital leads to the next proposition.

Proposition 2 *With CRST 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*

$$SOEC_{fixed} = exp\Big(\frac{\beta_k}{\beta_l + \beta_m + \beta_k}[ln(S_{it}^{k,unc}) - ln(S_{it}^{k,con})] + \frac{\omega_{it}^{*unc} - \omega_{it}^{*con}}{\beta_l + \beta_m + \beta_k}\Big) - 1, \tag{20}$$

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 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 a larger 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.

In Internet Appendix G, 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.

4.2.1 Relationship with the Literature

Our approach has several advantages relative to past literature. First, 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.

Second, 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.

Third, 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.

Fourth, 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, we alleviate concerns about measurement error being interpreted as misallocation (Bils et al., 2017; Rotemberg and White, 2017).

Lastly, 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.

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 16, rewritten here for convenience:

$$r_{it} = \beta_l \bar{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega^*_{it} + \psi^*_{st} + \xi^*_{it} + \varepsilon_{it}.$$

We parametrize the unobserved government-related shocks ξ^*_{it} into an unobservable component, ϕ_{it}, and a common component dependent on the political connection status. Formally, let ξ^*_{it} be:

$$\xi^*_{it} = \xi^{PC}_{st} Post^{PC}_{it} \cdot Contractor_{it} + \phi_{it}, \tag{21}$$

where ξ^{PC}_{st} is a common component for politically connected firms in the sector, $Contractor_{it}$ 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.

This leads to the main estimating equation

$$r_{it} = \beta_l \bar{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \overline{k}_{it} + \omega^*_{it} + \psi^*_{st} + \xi^{PC}_{st} Post^{PC}_{it} \cdot Contractor_{it} + \phi_{it} + \varepsilon_{it}. \tag{22}$$

To estimate equation 22, 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).[31] We control for ψ^*_{st} by estimating separate production functions in each 2-digit sector and by including year fixed effects in the regressions.

A reader may be concerned about a possible simultaneity bias in the relationship 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 use instruments for political connections, such as close elections or major reshuffles, which create exogenous changes in connection status. In the following section, we use the major reshuffle strategy. However, 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. To complement the major reshuffle approach, we also focus on measuring productivity differences *before* the connection occurred, which reduces concerns about simultaneity bias.

[31] Specifically, we adopt the Wooldridge (2009) one-step GMM version of Levinsohn and Petrin (2003), which we refer to as LP-Wooldridge. The known assumption of dynamic capital required for identification might seem at odds with the flexible capital scenario described in Proposition 1. However, these two assumptions can be reconciled if the time horizon relevant for the provision of a contract is longer than that of period-to-period production (e.g., for contracts lasting multiple years or when the winner's identity is known with sufficient time in advance).

Given the augmented revenue equation 22, estimates of firm-level TFPR can be obtained by the residuals

$$\hat{\omega}^*_{it} = r_{it} - \hat{\lambda}_s - \hat{\beta}_l l_{it} - \hat{\beta}_m m_{it} - \hat{\beta}_k k_{it} - \hat{\tau}_t - \hat{\xi}^{PC}_{st} Post^{PC}_{it} \cdot Contractor_{it}, \tag{23}$$

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

$$\hat{\omega}^*_{it} = \alpha^1_s + \gamma_\omega PC_{it} + \tau^1_t + \nu^1_{it}, \tag{24}$$

where PC_{it} is an indicator for contractors that establish a link with bureaucracy at some point in our data, τ^1_t are sector-specific year dummies, and α^1_s 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. We can then measure excess costs as

$$\widehat{SOEC}_{flex} = exp\Big(\frac{-\hat{\gamma}_\omega}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\Big) - 1. \tag{25}$$

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

$$\begin{aligned} \hat{\omega}^*_{it} &= \alpha^1_s + \gamma_\omega PC_{it} + \tau^1_t + \nu^1_{it} \\ s_{it} &= \alpha^2_s + \gamma_S PC_{it} + \tau^2_t + \nu^2_{it}, \end{aligned} \tag{26}$$

with $s_{it} = \bar{k}_{it} - r_{it}$. We then plug these estimates in the excess cost equation

$$\widehat{SOEC}_{fixed} = exp\Big(- \frac{\hat{\beta}_k}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\hat{\gamma}_S - \frac{1}{\hat{\beta}_l + \hat{\beta}_m + \hat{\beta}_k}\hat{\gamma}_\omega\Big) - 1. \tag{27}$$

5 Results

This section presents the main results of the welfare analysis. We first discuss estimates of the production function elasticities. Then 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 all results control for the industrial sector of the firm. We compute standard errors via 30 bootstrap repetitions.

5.1 Production Function Estimates

Economy-wide average labor, intermediate inputs, and capital elasticities are reported in Table 5, together with the corresponding returns to scale.[32] 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 from political connections described in equation 22. Under the LP-Wooldridge procedure, we estimate an economy-wide labor elasticity of 0.39, an intermediate inputs elasticity of 0.51, and capital elasticity of 0.03.

The remaining columns serve as robustness and estimate instead a more standard production function where the unobserved government shocks ξ_{it}^* are set to zero, and differ in the approach we adopt to correct for the connection premium.[33] These checks have two purposes: 1) validate that the revenue elasticity estimates are robust, and 2) create alternative productivity estimates to verify the robustness of the welfare exercises under different modelling assumptions.

To check that the use of government share as a variable in production function equation does not introduce bias in the elasticity estimates, Columns (3)–(4) adjust the revenue from government sales of connected contractors by a 6% premium and then proceeds to estimate standard revenue production function. Instead, Columns (5) and (6) rely on the sample of non-connected contractors and connected ones in the years before their first link is established. This specification attempts to eliminate the bias that would emerge in the elasticities estimates if politically connected firms systematically differ from non-connected ones after becoming connected, and the simultaneity bias in productivity and political connection shocks.

To verify that the premium correction does not mechanically affect the elasticities, in Columns (7) through (10) we make no premium corrections. Columns (7)–(8) estimate standard revenue production function equation 28 on the sample of contractors. Lastly, Columns (9)–(10) use all Ecuadorian firms, rather than only government contractors, which would serve to verify whether contractor-specific production functions are driving the results. Reassuringly, although point estimates differ across modelling assumptions, the relative importance of each input is similar across all specifications. Importantly, the

[32] Our definition of intermediate inputs includes both material inputs and services used in production.

[33] The revenue production function we estimate is given by:

$$r_{it} = \beta_l \bar{l}_{it} + \beta_m \overline{m}_{it} + \beta_k \bar{k}_{it} + \omega_{it}^* + \psi_{st}^* + \varepsilon_{it}. \tag{28}$$

estimated revenue elasticities are consistent with the assumption of constant return to scale in production for reasonable demand elasticity parameters.

5.2 Excess Costs Estimates

The estimates of the excess costs from political connections are reported in Table 6. Panel A presents our main results, where productivity is computed as residual from the augmented revenue equation 22. 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 imply 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.

Second, one also may be worried of biases in revenue productivity and elasticity estimates if politically connected firms change their relative input intensity after gaining a connection, or if productivity shocks 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, it runs against a narrative where political connections arise due to expected efficiency gains. If that would 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 18 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 to a non-connected one (i.e., $\theta = 0$). We approximate the marginal cost of non-connected contractors, C'_{SP}, with their variable costs-revenue ratio. We present the results in Table 6 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 Comparison-Sample Definition

One main concern with the analysis above is that it compares all connected firms to all non-connected firms in a given sector. This might be incorrect in that not all firms in a sector could supply a variety that is relevant for a specific procurement process. It could be the case that connected firms supply varieties that make them less revenue efficient and for that reason we estimate welfare losses in the aggregate comparison. If we would better define the counterfactual group to those offering the same variety, the efficiency gap may disappear or reverse.

To address 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 26 from contract-firm level data (i.e., each contract-firm combination corresponds to an observation) and use as sample all winning and losing firms among contracts with at least two competitors. In those regressions, we control for different contract specific characteristics. Table 7, 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, the equivalent 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, effectively removing location, type of contracts, and agency-specific requirement differences 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 such specification, we are restricting the comparison to be only among 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 non-connected losing firms. In Panel B of Table 7, 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.

5.3.2 Specialization

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 E, 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 sales in a given year or across the period of analysis.[34] Reassuringly for our main results, we estimate positive excess costs within different levels of specializations.[35]

5.3.3 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. Appendix Table IA4 presents the results comparing firms that compete only in a specific contract type. Panel A shows the excess costs estimates for contractors that only compete in discretionary processes. For such contract type, we find excess costs of 5.4% stemming from political connections. Panel B instead shows for contractors of auctions alone. The point estimate is smaller, of 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 costs estimates of 6% for firms that procure multiple contract types. Although some estimates are noisy, the pecking order suggests 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 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.

5.3.4 Treatment-Sample Definition

To verify that the definition of treated firms does not drive the result, we construct excess costs estimates under different treatment definitions (see Appendix Table IA5). First, as firms may gain political connections precisely due to some firm-specific characteristic (e.g., the product selection they have), 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 interested was individually selected for some procurement-related process. Panel A presents the results, which finds statistically significant excess costs of political connection of 2%.[36] Second, in Panel B and C, we verify that both direct

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

[35]Except for one noisy specification with a very small sample size of 108 firms.

[36]Additionally, we find significant excess costs estimates and similar to those in baseline, if we concentrate exclude strategic exit firms or firms that have more than one connection.

and indirect connections are relevant. Recall, direct connections are those in which the firm owner becomes a bureaucrat, whereas indirect connections are those in which the sibling of the owner becomes a bureaucrat. We find virtually identical results for both types of connections of 3.8%. Together with the findings using large reshuffles, the results indicate that fortuitous connections have similar efficiency effects than more endogenous connections.

5.3.5 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 be not feasible to hire them. To address this concern, we perform the sectoral analysis restricting to firms within each province. Appendix Figure IA3 shows the distribution of province-specific excess costs 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%.

5.3.6 Size-dependent policies

It is worth asking whether implementing size-dependent procurement policies may be sufficient to overcome the negative effects of political connections. After all, gauging from the descriptive statistics in Table 2, connected contractors tend to be smaller than non-connected ones. Hence, it might be plausible to fix the adverse effects by targeting specific firm sizes. Note that this policy would be counter to more traditional approaches (both in Ecuador and abroad) that offers preferential treatment to small and medium-sized enterprises. In Appendix Table IA6, we present the results of size-dependent policies.[37] 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 Panel B and C, we study the second and third quartile and find precisely estimated zero effect of connections. Lastly, in Panel D, we study the largest firms and find that the inefficiency concentrate in this sample. Here, we find 3.9% excess costs. Therefore, minimum-size policies would not be able to balance the negative effects from political connections.

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

5.3.7 *Sectoral Differences*

As a final exercise, 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 Appendix Table IA3 (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 22.[38] 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 (not significant) excess costs. The existing heterogeneity is suggestive 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.[39]

5.3.8 *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 mixed 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 in likely exogenous connections, such as those coming from rotation in appointments or indirectly obtained through family members, suggesting that stories explaining political connections aimed at improving quality-efficiency of the contracts is 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 set of firms competing for the exact same contract.

Our findings do come with important 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 is feasible to use our approach for product-level excess costs estimates (as sketched in Appendix Section G). 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, specially in non-standardized sectors

[38] Appendix Table IA3 shows a positive and high correlation with the industry level excess cost obtained using the other specifications and assumptions.

[39] Of the 42 sectors for which we estimate excess costs, 35 have positive point estimates.

and in discretionary contracts. Hence, our welfare effects should be appropriately adjusted by the benefits or costs of political connections in those areas. However, given previous results in the literature showing that connections adversely affect delays, execution costs, and renegotation (Schoenherr, 2019; Brogaard et al., 2021; Ryan, 2020), we deem less likely that in our setting, connections may have the opposite effect on all of these issues, thus balancing the negative efficiencies 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 determine how well this joint measure accurately reflects observed differences in quality. To effectively assess this, 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. With this information, it would be possible to determine how well the quality-share of revenue productivity measure captures the observed differences in quality of government procurement. Future research may address this as more data becomes available.

6 Conclusion

This paper studies the welfare costs of the misallocation of procurement contracts caused by political connections. Using a novel dataset that joins 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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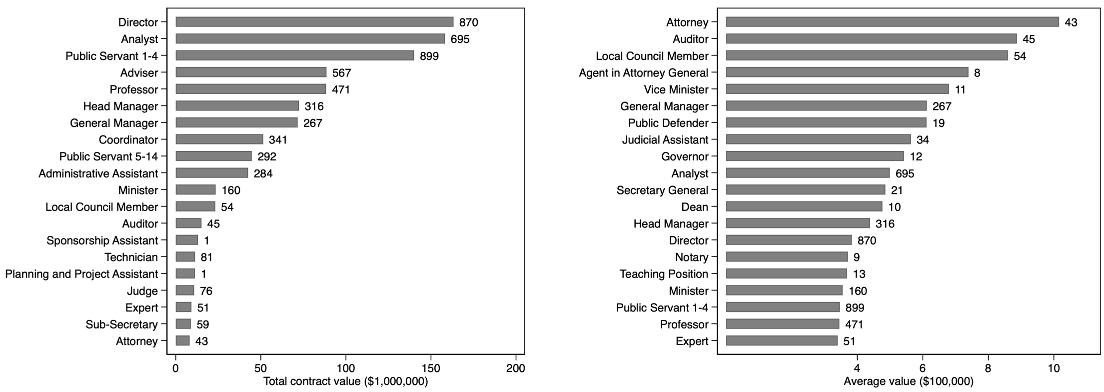
Figures and tables

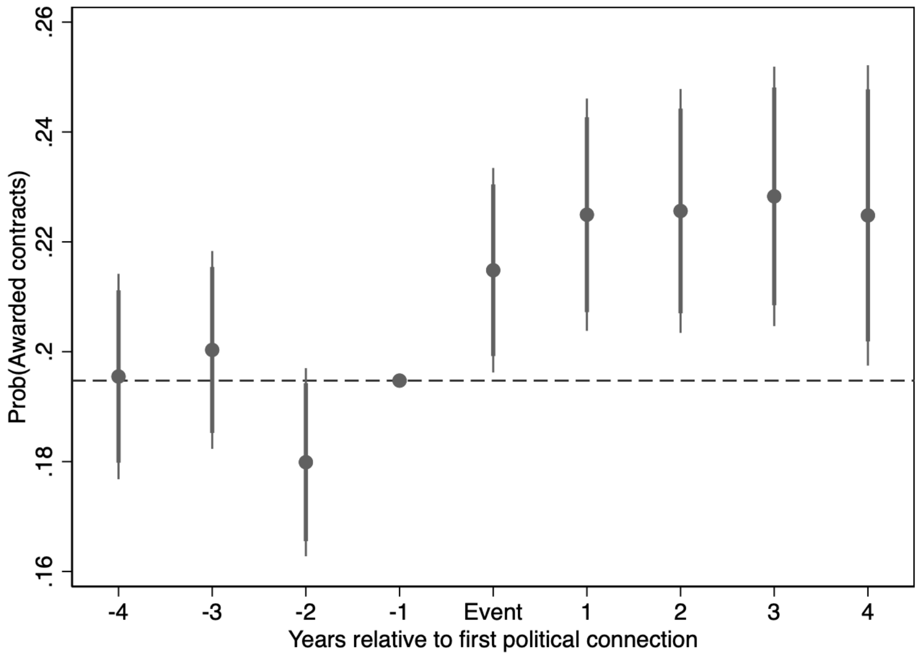
Figure 1: Ranking Bureaucratic Positions

(a) Total Award (b) Average Award



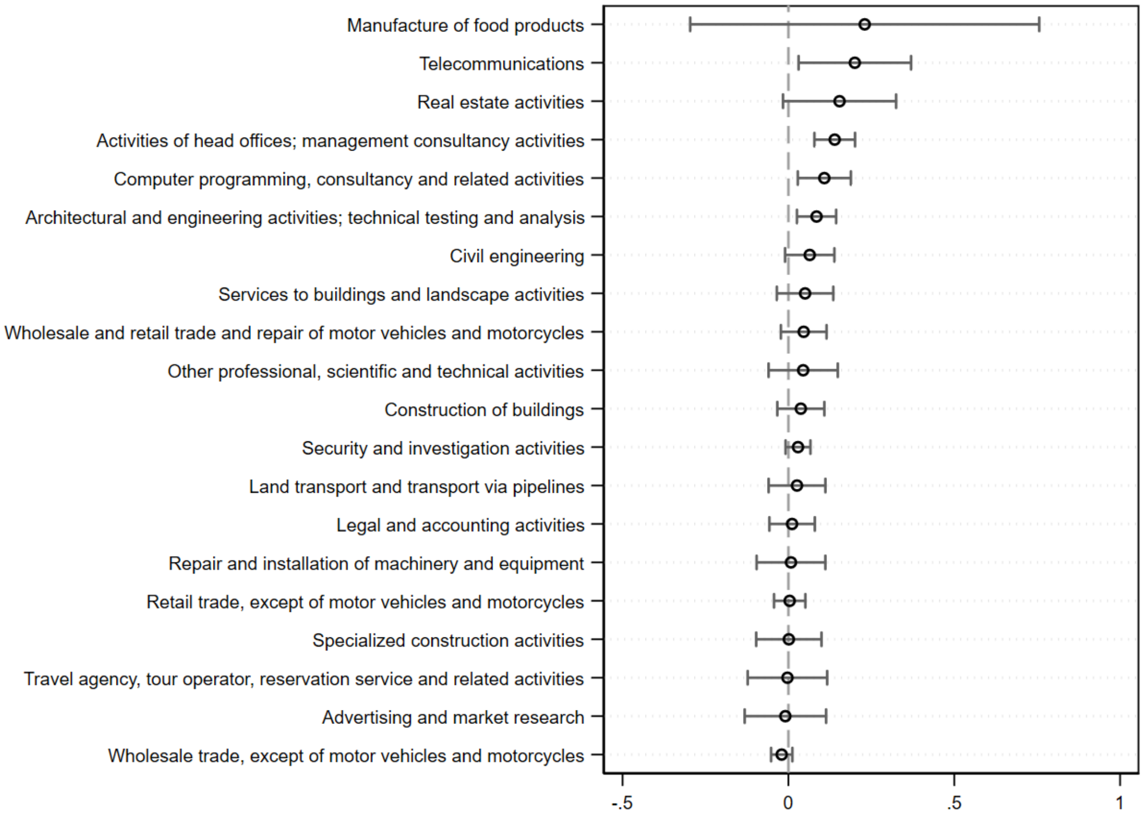
Notes: The figure shows the top 20 bureaucrat positions ranked by the aggregate value of the contracts won by firms connected to bureaucrats in each position [panel (a)] and average value of contracts obtained per individual in the position [panel (b)]. The value of contracts won is constructed as follows. 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 bureaccrat 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 an event-study of the probability of winning government procurement contracts on the firm's first political connection using the methodology of Callaway and Sant'Anna (2021). 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 10$^{\text{th}}$ percentile of the yearly contract value distribution for firms winning a non-zero number of contracts. We set the year prior to the first connection (-1) as the omitted category. The control group includes non-connected contractors (never-treated) as well as yet-to-be-treated connected contractos. 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 standard errors clustered at the contractor level. 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 procurement expenditure. Excess costs are estimated from equation 26 assuming fixed capital. 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 22. 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.

Table 1: Sample Size for Different Categories of Connected Contractors

	All connections	Only direct connections	Only indirect connections	Both direct and indirect connections
	(1)	(2)	(3)	(4)
Panel A: Politically connected (not strategic)				
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 (strategic 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

Notes: The table reports sample size and statistics on the number of links to bureaucracy for different categories of politically connected firms. In Panel A, we include the sample of contractors used for the analysis. Panel B, considers firms with shares bought by a bureaucrat in office. Panel C, considers the set of firms created by a bureaucrat. In all columns, we drop firms that establish their first political connection before 2000 and firms that do not file balance sheet information. Our data additionally comprises 22,997 non-connected contractors.

Table 2: Descriptive Statistics of Ecuadorian Firms in 2015

	Panel A Full Sample		*Panel B* Contractors Sample			*Panel C* 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	1,340,678	1,677,244	602,489	815,973	749,802	999,055	771,809
	(3,317,781)	(4,447,662)	(5,068,397)	(2,456,432)	(2,972,394)	(2,830,820)	(3,199,652)	(2,994,810)
Capital	325,902	380,484	476,583	169,711	225,226	218,479	235,544	227,478
	(1,373,586)	(1,553,196)	(1,772,079)	(866,911)	(1,011,721)	(1,033,252)	(968,168)	(1,012,668)
Wage bills	128,916	221,214	263,260	128,994	168,925	152,581	202,778	167,213
	(460,268)	(627,813)	(698,629)	(419,254)	(499,233)	(473,394)	(531,463)	(510,762)
Intermediate inputs	542,330	893,766	1,132,297	370,597	503,149	469,293	623,025	459,354
	(2,361,077)	(3,135,226)	(3,576,742)	(1,712,347)	(2,058,365)	(1,967,839)	(2,191,545)	(2,083,934)
Debt	441,808	646,554	810,890	286,117	377,571	342,120	460,428	366,234
	(1,714,406)	(2,186,380)	(2,486,208)	(1,232,358)	(1,444,691)	(1,341,569)	(1,629,782)	(1,440,076)
Revenue-asset ratio	1.689	1.900	1.896	1.908	1.867	1.859	1.865	1.881
	(3.577)	(3.329)	(3.242)	(3.514)	(3.374)	(3.423)	(3.011)	(3.572)
Age	9.528	9.902	10.593	8.387	11.100	10.610	11.406	11.623
	(10.112)	(9.922)	(10.653)	(7.881)	(8.373)	(8.034)	(8.466)	(8.774)
Sample size	73,133	27,058	18,585	8,473	4,532	2,106	1,085	1,341

Notes: The table reports means and standard deviations (in parenthesis) of balance sheet information in 2015. In Column (1), the sample is all Ecuadorian private firms, while Column (2) only includes government contractors (see Section 2.2.1). Columns (3) and (4) present statistics for non-connected and connected contractors, respectively. Column (5) excludes firms created or acquired by bureaucrats in office and those that establish their first political connection before 2000. Columns (6)–(8) present a decomposition of Column (5) by type of political connection. For each variable, we winsor non-zero observations at the 1st and 99th percentile of the respective distribution. 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).

Table 3: Descriptive Statistics of Government Procurement Contracts

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

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 1$^{\text{st}}$ percentile and above the 99$^{\text{th}}$ 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).

Table 4: Probability of Being Awarded a Contract - Heterogeneity by Type and Location

	Panel A By Type of Contract		
	Auction (1)	Discretionary (2)	Lottery (3)
After first political connection	0.0105* (0.0062)	0.0354*** (0.0088)	-0.0001 (0.0032)
Sample Size	181,790	181,790	181,790
Number contractors	27,838	27,838	27,838
Connected contractors	4,841	4,841	4,841
Mean before connection	0.063	0.138	0.032

	Panel B Provinces	
	Same (1)	Other (2)
After first political connection	0.0317** (0.0130)	0.0210 (0.0166)
Sample Size	177,147	177,147
Number contractors	27,692	27,692
Connected contractors	4,695	4,695
Mean before connection	0.0869	0.129

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). In Panel A, we present heterogeneity by type of contract (discretionary, auction, or random). In Panel B, we present heterogeneity by location. Namely, we say a firm is in the same province as the contract if the contract is registered in the same location as the headquarters of the firm. In all the regressions, the control group includes never-treated firms and yet-to-be-treated firms, where treatment is defined according to each exercise.

Table 5: Production Function Elasticities

	Main specification		Premium-adjusted revenue		Exclude political connection years		No premium adjustment		All firms	
	OLS	LP-Wooldridge	OLS	LP-Wooldridge	OLS	LP-Wooldridge	OLS	LP-Wooldridge	OLS	LP-Wooldridge
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Labor	0.3808	0.3875	0.3808	0.3873	0.3549	0.3612	0.3624	0.3688	0.3461	0.3536
	(0.1034)	(0.1262)	(0.1029)	(0.1252)	(0.0991)	(0.1210)	(0.1014)	(0.1237)	(0.0882)	(0.1060)
Intermediate Inputs	0.5327	0.5121	0.5326	0.5130	0.5599	0.5347	0.5509	0.5304	0.5253	0.4971
	(0.1076)	(0.1257)	(0.1062)	(0.1260)	(0.1069)	(0.1290)	(0.1061)	(0.1262)	(0.1028)	(0.1119)
Capital	0.0497	0.0309	0.0498	0.0308	0.0488	0.0304	0.0492	0.0304	0.0689	0.0400
	(0.0241)	(0.0193)	(0.0239)	(0.0197)	(0.0229)	(0.0199)	(0.0220)	(0.0187)	(0.0369)	(0.0274)
Returns to scale	0.9632	0.9304	0.9632	0.9311	0.9635	0.9263	0.9625	0.9296	0.9403	0.8907
	(0.0228)	(0.0432)	(0.0226)	(0.0446)	(0.0219)	(0.0457)	(0.0211)	(0.0411)	(0.0367)	(0.0683)
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

Notes: The table reports economy-wide average elasticities. These are obtained by estimating production functions at the 2-digit industry level, and then computing across-sector means weighted by the number of firms in each sector. We drop industries having less than 750 observations. We report in parenthesis the standard deviation of the distribution of sector-level elasticities obtained via 30 bootstrap simulations. In each bootstrap replicate, we sample firms with replacement to match the original number of firms in each sector. In columns (1)–(8) the sample correspond to firms classified as government contractors. For columns (9)–(10) the sample is all Ecuadorian private firms. Columns (1) and (2) report estimates from the specification in equation 22. The remaining columns estimate production functions following equation 28. In Columns (3)–(4), we deflate the revenue from government sales of politically connected contractors in the years following connection by a 6% government premium. Columns (5)–(6) exclude observations from connected contractors in the years after they establish a link with the bureaucracy. Columns (7) through (10) do not make any adjustment for the government premium. We exclude from all specifications firms acquired or created by a bureaucrat in office and those that establish their first political connection before 2000. The unit of observation is the contractor-year. We winsor non-zero observations of each variable at the 1^{st} and 99^{th} percentile of the respective distribution. 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.

Table 6: Social Excess Cost Estimates

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

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 18, assuming that $\theta = 0$. Outcomes in Columns (1)–(2) assume flexible capital and are estimated as in equation 24. Specifications (3)–(4) assume fixed capital and are estimated via equation 26. 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 22. 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 28. 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 goverment premium.

Table 7: Excess Cost Estimates (Contract-level) - Robustness

	Fixed capital and LP-Wooldridge		
	(1)	(2)	(3)
Panel A: Connected			
Excess Costs	0.068***	0.060***	0.073*
	(0.024)	(0.024)	(0.041)
Welfare cost (% of proc. budget)	5.723***	4.950***	6.225*
	(2.093)	(2.027)	(3.556)
Sample Size	74,955	69,487	30,044
Panel B: Winner			
Excess Costs	-0.021***	-0.019***	-0.022
	(0.008)	(0.007)	(0.019)
Welfare cost (% of proc. budget)	-1.718***	-1.589***	-1.839
	(0.694)	(0.566)	(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

Notes: The table reports excess cost estimates and corresponding welfare costs as percentage of the procurement budget at the contract-firm level. We estimate excess costs at the 2-digit industry level, and compute economy-wide averages using as weights the number of contract-firm observations 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 18, assuming that $\theta = 0$. All outcomes are estimated under the fixed capital equation 26 using estimates from the LP-Wooldridge and the main specification in equation 22. Column (1) controls for year and 3-digit sector fixed effects. Column (2) controls additionally for buying agency, province, and contract-category fixed effects. Instead, column (3) controls for 3-digit sector and contract fixed effects. Panel A shows the excess costs of procuring from a politically connected firm rather than an non-connected one. Panel B shows the welfare costs (gains) from procuring from an non-connected winner rather than any losing firm.

Internet Appendix

A Internet appendix: Proofs

This section presents proofs of Proposition 1 and 2. For both, we assume firms are cost minimizing and face the following Lagrangian function

$$\begin{aligned}\mathcal{L}(L_{it}, M_{it}, K_{it}, w_{st}, \rho_{st}, r_{st}, \lambda_{it}) = {} & w_{st}L_{it} + \rho_{st}M_{it} + r_{st}K_{it} \\ & + \lambda_{it}\left(Q_{it} - L_{it}^{\alpha_l} M_{it}^{\alpha_m} K_{it}^{\alpha_k} exp(\omega_{it})\right). \end{aligned} \tag{29}$$

Proof of Proposition 1 Assuming flexible capital, the quantity-conditional demand for intermediate inputs can be written as

$$\begin{aligned} M_{it}(Q_{it}, \omega_{it}, \boldsymbol{\alpha}) &= \left(\frac{Q_{it}}{exp(\omega_{it}^{*})}\alpha_m^{(\alpha_l+\alpha_k)}\alpha_l^{-\alpha_l}\alpha_k^{-\alpha_k}\right)^{\frac{1}{\alpha_l+\alpha_m+\alpha_k}} \\ &= \left(\frac{Q_{it}}{exp(\omega_{it})}\right)^{\frac{1}{\alpha_l+\alpha_m+\alpha_k}} \Gamma_m, \end{aligned} \tag{30}$$

where Γ_m is a constant that collects factor elasticities and the sector-level multiplier. We can derive corresponding quantity-conditional demand functions for labor and capital. Given these expressions, each firm's total cost function can be written as

$$\begin{aligned} C_{it}(Q_{it}, \omega_{it}, \Gamma) &= w_{st}L_{it} + \rho_{st}M_{it} + r_{st}K_{it} \\ &= \left(\frac{Q_{it}}{exp(\omega_{it})}\right)^{\frac{1}{\alpha_l+\alpha_m+\alpha_k}} (\Gamma_l + \Gamma_m + \Gamma_k). \end{aligned} \tag{31}$$

Assuming CRTS and taking derivatives with respect to quantity, we obtain

$$\frac{\partial C_{it}(Q_{it}, \omega_{it}, \Gamma)}{\partial Q_{it}} = exp(\omega_{it})^{-1}(\Gamma_l + \Gamma_m + \Gamma_k). \tag{32}$$

Thus, a firm's cost function is linear in quantity, with a different slope depending on the productivity level. Finally, can use Lemma 1, 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

$$\begin{aligned} \frac{\partial C_{it}(\tilde{Q}_{it}, \omega_{it}, \Gamma)}{\partial \tilde{Q}_{it}} = \frac{\partial C_{it}(Q_{it}, \omega_{it}, \Gamma)}{\partial Q_{it}} exp(z_{it})^{-1} = \\ exp(\omega_{it} + z_{it})^{-\frac{(\sigma-1)}{\sigma(\beta_l+\beta_m+\beta_k)}}(\Gamma_l + \Gamma_m + \Gamma_k) \end{aligned} \tag{33}$$

To get a measure of excess costs of political connection it is sufficient to compare this expression between connected and non-connected firms in the same sector

$$SOEC_{flex} = \frac{\partial C_{it}(\tilde{Q}_{it}^{con}, \omega_{it}^{*con}, \Gamma)/\partial \tilde{Q}_{it}}{\partial C_{it}(\tilde{Q}_{it}^{unc}, \omega_{it}^{*unc}, \Gamma)/\partial \tilde{Q}_{it}} - 1 = exp\Big(\frac{\omega_{it}^{*unc} - \omega_{it}^{*unc}}{\beta_l + \beta_m + \beta_k}\Big) - 1., \tag{34}$$

where $\omega*_{it} = (\sigma - 1)/\sigma(\omega_{it} + z_{it})$, so 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 firm's capital cannot be freely adjusted, so that the quantity-conditional demand for intermediate inputs becomes

$$\begin{aligned} M_{it}(Q_{it}, K_{it}, \omega_{it}, \boldsymbol{\alpha}) &= \left(\frac{Q_{it}}{K_{it}^{\alpha_k} exp(\omega_{it})}\left(\frac{\alpha_m}{\alpha_l}\right)^{\beta_l}\right)^{\frac{1}{\alpha_l+\alpha_m}} \\ &= \left(\frac{R_{it}}{K_{it}^{\alpha_k} exp(\omega_{it})}\right)^{\frac{1}{\alpha_l+\alpha_m}} \Lambda_m, \end{aligned} \tag{35}$$

with Λ_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

$$\begin{aligned} C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda) &= w_{st}L_{it} + \rho_{st}M_{it} \\ &= \left(\frac{Q_{it}}{K_{it}^{\alpha_k} exp(\omega_{it})}\right)^{\frac{1}{\alpha_l+\alpha_m}} (\Lambda_l + \Lambda_m). \end{aligned} \tag{36}$$

Assuming CRTS, the derivative of the cost function with respect to quantity is

$$\frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial Q_{it}} = \frac{1}{1-\alpha_k} Q_{it}^{\frac{\alpha_k}{1-\alpha_k}} \bar{K}_{it}^{-\frac{\alpha_k}{1-\alpha_k}} exp(\omega_{it})^{-\frac{1}{1-\alpha_k}} (\Lambda_l + \Lambda_m). \tag{37}$$

We can modify the previous equality by multiplying by $P_{it}^{\frac{\alpha_k}{1-\alpha_k}} exp(z_{it})^{-\frac{1}{1-\alpha_k}}$ in both sides and get

$$P_{it}^{\frac{\alpha_k}{1-\alpha_k}} exp(z_{it})^{-\frac{1}{1-\alpha_k}} \frac{\partial C_{it}(Q_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial Q_{it}} = \frac{1}{1-\alpha_k} R_{it}^{\frac{\alpha_k}{1-\alpha_k}} K_{it}^{-\frac{\alpha_k}{1-\alpha_k}} exp(\omega_{it} + z_{it})^{-\frac{1}{1-\alpha_k}} (\Lambda_l + \Lambda_m). \tag{38}$$

The previous expression can be modified by using Lemma 1, and the fact that with a CES demand function, $P_{it} = \sigma/(\sigma - 1)c'(Q_{it})$. We get

$$\frac{\partial C_{it}(\tilde{Q}_{it}, K_{it}, \omega_{it}, \Lambda)}{\partial \tilde{Q}_{it}}^{\frac{1}{1-\alpha_k}} = R_{it}^{\frac{\alpha_k}{1-\alpha_k}} K_{it}^{-\frac{\alpha_k}{1-\alpha_k}} exp(\omega_{it} + z_{it})^{-\frac{1}{1-\alpha_k}} (\Lambda_l' + \Lambda_m'). \tag{39}$$

where $(\Lambda_l' + \Lambda_m')$ captures new additional constant terms in the expression. The next step consists in 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

$$\frac{\partial C_{it}(\tilde{Q}_{it},K_{it},\omega_{it},\Lambda)}{\partial \tilde{Q}_{it}} = R_{it}^{\frac{\beta_k}{\beta_l+\beta_m+\beta_k}} K_{it}^{-\frac{\beta_k}{\beta_l+\beta_m+\beta_k}} exp(\omega_{it}+z_{it})^{-\frac{(\sigma-1)}{\sigma(\beta_l+\beta_m+\beta_k)}} (\Lambda_l'+\Lambda_m'). \quad (40)$$

The final step consists on taking logs to the quality-adjusted marginal cost ratios between the connected and non-connected firms. Note that under the assumption of equal inputs costs within 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 expression for excess costs stated in Proposition 2

$$SOEC_{fixed} = exp\Big(\frac{\beta_k}{\beta_l+\beta_m+\beta_k}[ln(S_{it}^{k,unc})-ln(S_{it}^{k,con})]+\frac{\omega_{it}^{*unc}-\omega_{it}^{*con}}{\beta_l+\beta_m+\beta_k}\Big)-1, \quad (41)$$

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

B Internet Appendix: Data Construction

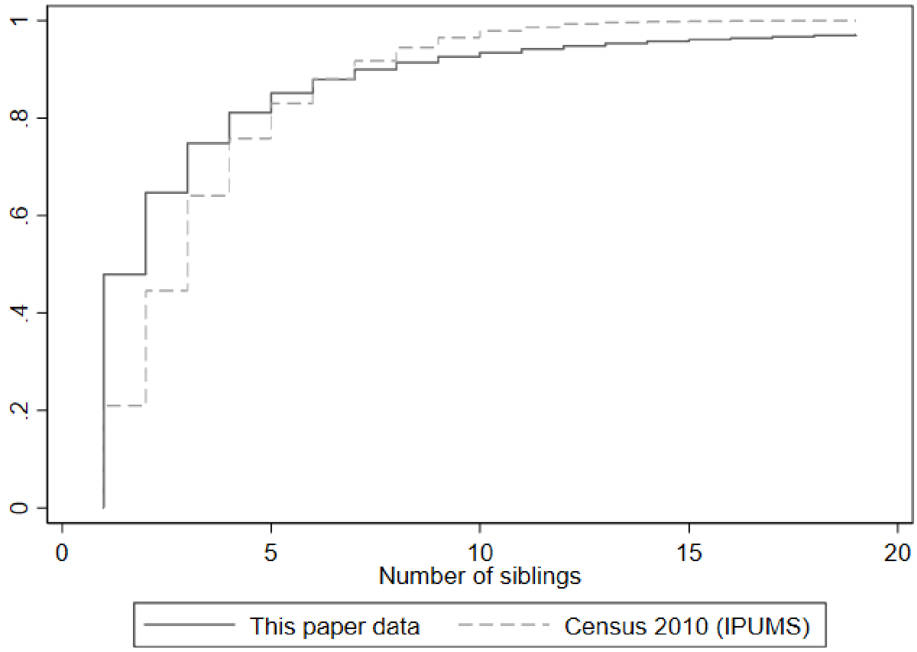
B.1 Identifying Families

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

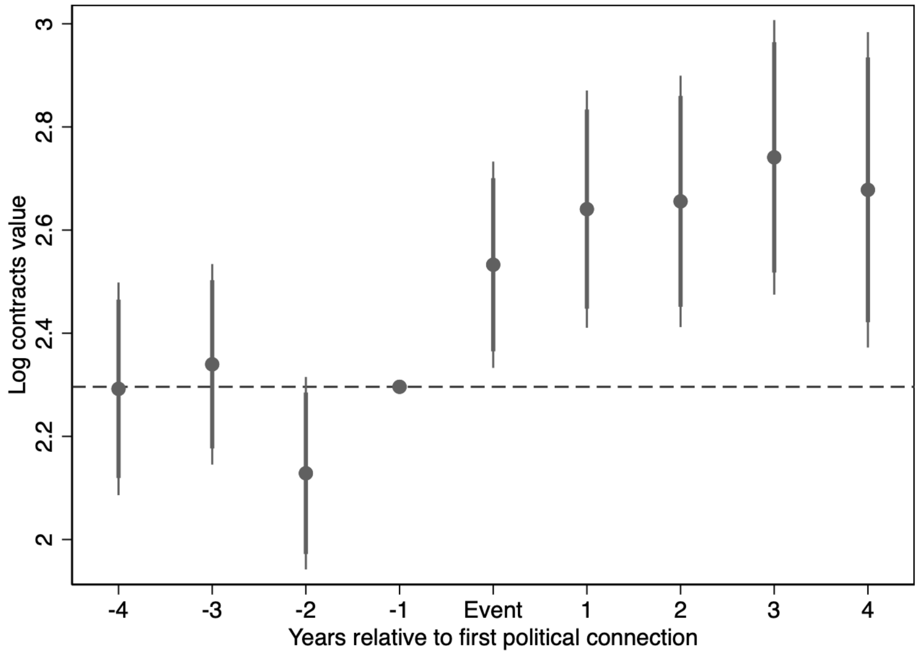
Figure IA1: Family Size CDF



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 99$^{\text{th}}$ percentile.

C Internet Appendix: Reallocation of Contracts - Additional Results, Robustness and Falsifications

Figure IA2: Value of Contracts Won Before and After Political Connection



Notes: This figure presents the coefficients for an event-study of log value of contracts one plus one on the firm's first political connection using the methodology of Callaway and Sant'Anna (2021). We set the year prior to the first connection (-1) as the omitted category. The control group includes non-connected contractors (never-treated) as well as yet-to-be-treated connected contractors. 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 standard errors clustered at the contractor level. The dotted line shows the sample mean in the years before the event, and each coefficient is shifted by this constant.

Table IA1: Probability of Being Awarded a Contract - Sensitivity to Specification

	Callaway-Sant'Anna	Sun-Abraham	De Chaisemartin-D'Haultfoeuille	Two-way Fixed Effects
	(1)	(2)	(3)	(4)
After first political connection	0.0259***	0.0262***	0.0179***	0.0268***
	(0.0096)	(0.0096)	(0.0065)	(0.0054)

Notes: The table reports different estimated coefficients for the effect of political connection on the probability of being awarded a government contract. Each column title describes the methodology used to obtain the point estimate. Column (1) uses Callaway and Sant'Anna (2021), Column (2) Sun and Abraham (2021), Column (3) De Chaisemartin and d'Haultfoeuille (2020), and Column (4) presents usual a two-way fixed-effect estimate. The number of observations are 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.

Table IA2: Probability of Being Awarded a Contract - Robustness

	Panel A Restricted Sample		
	Large reshuffles (1)	Single entry year (2)	No strategic exits (3)
After first political connection	0.0300** (0.0144)	0.0296** (0.0117)	0.0413*** (0.0114)
Sample Size	161,536	169,883	170,473
Number contractors	24,750	26,029	26,184
Connected contractors	1,753	3,032	3,187
Mean before connection	0.193	0.196	0.184

	Panel B By Type Linkage	
	Direct Only (1)	Indirect Only (2)
After first political connection	0.0435*** (0.0127)	0.0741*** (0.0175)
Sample Size	167,030	157,675
Number contractors	25,584	24,147
Connected contractors	2,587	1,150
Mean before connection	0.194	0.204

	Panel C Falsification		
	Fake treatment years (1)	Families with 15+ siblings (2)	Low rank and low shares (3)
After first political connection	0.0034 (0.0113)	0.0099 (0.0145)	-0.0110 (0.0155)
Sample Size	111,741	95,275	134,605
Number contractors	16,919	14,352	20,853
Connected contractors	2,205	1,282	1,023
Mean before connection	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). 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 type of connection, with Column (1) looking at Direct connections (bureaucrat is owner) and Column (2) at Indirect ones (sibling is owner). In Panel C, we present falsification exercises. In Column (1), we assign a random treatment year to non-connected contractors (20% of the non-connected sample), imposing that the entry year distribution is equal to the true one. In Column (2), we consider connections through families classified as having more than 15 siblings. In Column (3), we consider connections to bureaucrats who own less than 10% of the firm's shares and have a low-rank positions. In all the regressions, the control group includes never-treated firms and yet-to-be-treated firms, where treatment is defined according to each exercise.

D Internet Appendix: Additional Excess Cost Estimates

Table IA3: 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 $)
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

Continued on next page

Table IA2: Excess Costs Estimates, All Sectors (Continued)

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

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 26 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 22. 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.

Table IA3: Correlation Between Sectoral Misallocation Estimates

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

Notes: The table shows 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 22) and assuming fixed capital. The unit of observation is the 2-digit sector level.

D.1 Robustness Checks and Additional Welfare Costs Results

Table IA4: Excess Cost Estimates by Contract Type

	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

Notes: The table reports excess cost estimates and corresponding welfare costs as 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 parenthesis) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 18, assuming that $\theta = 0$. Outcomes in Column (1) assume flexible capital and are estimated as in equation 24. Specifications in (2) assume fixed capital and are estimated via equation 26. 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.

Table IA5: Excess Cost Estimates - Sample Definition Robustness

	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

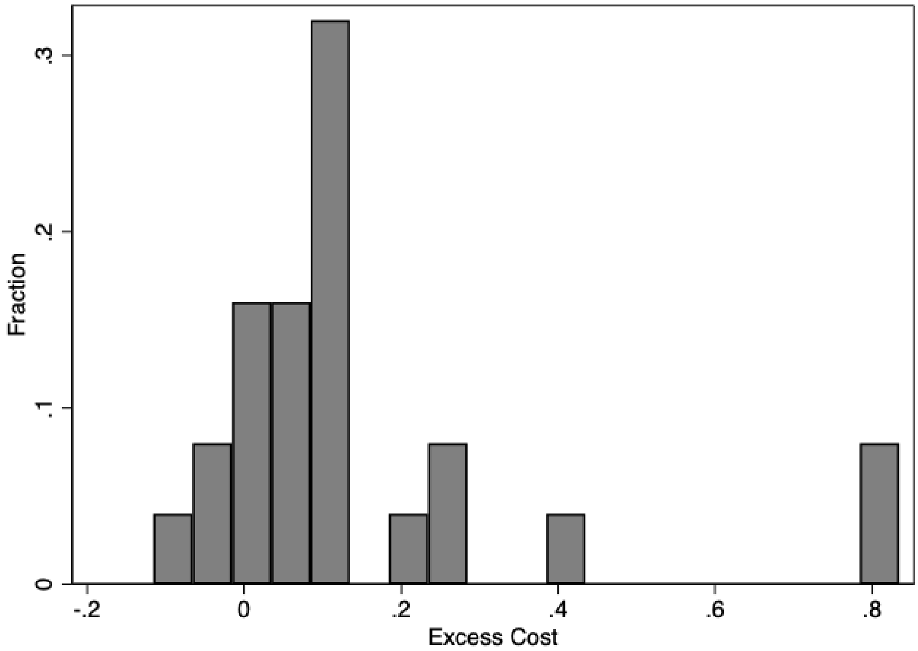
Notes: The table reports excess cost estimates and corresponding welfare costs as percentage of the procurement budget by different definition of treatment sample. We estimate excess costs at the 2-digit industry level, and compute economy-wide averages using as weights the number of firms in each sample-sector group. Standard errors (in parenthesis) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 18, assuming that $\theta = 0$. Outcomes in Columns (1) assume flexible capital and are estimated as in equation 24. Specifications in (2) assume fixed capital and are estimated via equation 26. All excess cost regressions control for year and 3-digit sector fixed effects. Panel A restricts to firms that gain a connection through large reshuffles of government agencies. Panel B focuses on firms with direct (bureaucrat is owner) connection. Panel C restricts to firms with indirect (sibling is owner) connection.

Table IA6: Excess Cost Estimates by Quartile Assets

	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)
	3.081***	2.85***
Welfare cost (% of proc. budget)	(1.163)	(1.145)
Sample size	34,054	34,054

Notes: The table reports excess cost estimates and corresponding welfare costs as percentage of the procurement budget by size of firms. We first obtain 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 parenthesis) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 18, assuming that $\theta = 0$. Outcomes in Columns (1) assume flexible capital and are estimated as in equation 24, while specifications in (2) assume fixed capital and are estimated via equation 26. 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 the largest firms.

Figure IA3: 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.

E Internet Appendix: Results for Firm Specialization

A policymaker conducting our set of counterfactuals might be worried that the exercise unjustly penalizes politically connected firms if 1) firms that specialize in government output have to make investments that make them less productive overall but more efficient for the government sector, 2) firms that specialize 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 suffered from the same shift in average productivity and the same shift in the utility to the final consumer. Therefore, as our social excess cost estimators in 19 and 20 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.

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 Appendix Table IA8, we show the distribution of 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 of 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%.

Table IA7 shows the result.[40] 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 the estimates that 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 large significant excess costs of 5% for firms supplying at least 75% of the output to the government and of 9% for firms supplying at least 50%.

Given that a majority of the 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 that range from 3.5 to 4%, similar to those in the main text. These results give confidence that the worry of possible bias against politically connected firms due to specialization might be of second order.

[40] Given that the excess cost estimation is done at the sectoral level, we require that at least 30 firms of each type are present in the sector.

Table IA7: Excess Cost Estimates with Specialization

	Non-Specialized		Specialized	
	Flexible capital (1)	Fixed capital (2)	Flexible capital (3)	Fixed capital (4)
Panel A: Dynamic, at least 75% total revenue from government				
Excess Costs	.039***	.038***	-.196	-.184
	(0.01)	(0.01)	(0.323)	(0.322)
Welfare cost (% of proc. budget)	3.064***	2.991***	-18.56	-17.408
	(0.778)	(0.781)	(30.782)	(30.722)
Sample size	75,567	75,567	108	108
Panel B: Dynamic, at least 50% total revenue from government				
Excess Costs	.039***	.038**	.123	.114
	(0.01)	(0.01)	(0.158)	(0.153)
Welfare cost (% of proc. budget)	3.066***	2.98***	10.258	9.579
	(0.782)	(0.786)	(13.152)	(12.762)
Sample size	75,352	75,352	216	216
Panel C: Static, at least 75% total revenue from government				
Excess Costs	.037***	.036***	.052*	.053*
	(0.01)	(0.01)	(0.03)	(0.028)
Welfare cost (% of proc. budget)	2.939***	2.858***	4.256*	4.305*
	(0.791)	(0.796)	(2.465)	(2.268)
Sample size	73,498	73,498	2,086	2,086
Panel D: Static, at least 50% total revenue from government				
Excess Costs	.036***	.035***	.087***	.089***
	(0.01)	(0.01)	(0.022)	(0.022)
Welfare cost (% of proc. budget)	2.8***	2.704***	6.889***	6.999***
	(0.805)	(0.809)	(1.733)	(1.696)
Sample size	72,066	72,066	3,435	3,435

Notes: The table reports excess cost estimates and corresponding welfare costs as 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 parenthesis) are obtained from the same 30 bootstrap simulations used to compute production function elasticities. Welfare costs are estimated via equation 18, assuming that $\theta = 0$. Outcomes in Columns (1) and (3) assume flexible capital and are estimated as in equation 24. Specifications (2) and (4) assume fixed capital and are estimated via equation 26. 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 is the share of revenue obtain 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 is the share of revenue obtain 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 is the share of revenue obtain from the government is at least 75% at time t. Panel D (Static) defines a firm as specialized at time t is the share of revenue obtain from the government is at least 50% at time t.

Table IA8: Distribution of Government Supply Shares

P25 (1)	Median (2)	P75 (3)	P90 (4)	P95 (5)
Panel A: All Years				
0	0	0	.18	.49
Panel B: Only Years with Positive Government Sales				
.03	.12	.41	.89	1

Notes: The table reports 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.

F 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 9, the average government prices at the sector can be decomposed into average prices for the politically connected and non-connected firms:

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

where average price for non-connected firms is given by

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

while for connected firms is

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

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:

$$\Delta ln(\overline{P}) = \Delta ln(\overline{C'(Q)}) + ln(1 + \mu_s), \tag{42}$$

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

Equation 42 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 42 to obtain an estimate of the political connection premium in the sector.

F.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 42. For this, we use data price data of standardized goods and services observed in the e-catalogue. Appendix Table IA1 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.[41] To make the standardization meaningful, we drop observations of goods that are sold by a single contractor over the course of a year.[42] 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

$$\begin{aligned} p_{ijat} = &(\beta_1 Pre^{PC}_{it} + \beta_2 Post^{PC}_{it}) \cdot FirmContractor_{it} \\ &+ (\beta_3 Pre^{PC}_{it} + \beta_4 Post^{PC}_{it}) \cdot PersonContractor_{it} \\ &+ \gamma q_{ijat} + \nu_a + \nu_t + \varepsilon_{ijat}, \end{aligned} \tag{43}$$

where Pre^{PC}_{it} is an indicator for politically connected contractors that have not yet established their first link with bureaucracy, while $Post^{PC}_{it}$ is an indicator for the years following the connection.[43] These two variables capture the average over- or under-pricing behavior

[41] Similar normalizations are used, for example, by DellaVigna and Gentzkow (2019).

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

[43] Notice that the coefficients of interest capture averages at the contractor-year level, while the unit

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, ε_{ijat} denotes the error-term.

Appendix Table IA2 reports the differences in prices. Under the transaction-level 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.

F.2 Back-of-envelope Premium Estimate

We now use equation 42, the estimated price differences, and marginal cost differences to obtain a back-of-envelope political premium estimate:

$$\mu_s = exp\Big(\Delta ln(\overline{P}) - \Delta ln(\overline{C'(Q)})\Big) - 1. \tag{44}$$

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 obtained 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 44 implies political connection premiums that range from 3.1% up to 8.8%. This range is consistent with previous empirical work,

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.

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.

Table IA1: Descriptive Statistics of Electronic-Catalog Transactions

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)

Notes: The table reports means and standard deviations (in parenthesis) 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.

Table IA2: Price Inflation Estimates

	Standardized price (1)	Average price (2)
Before political connection	-0.0085	0.0245
	(0.0066)	(0.0993)
After political connection	0.0348***	0.0642***
	(0.0024)	(0.0207)
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

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 1^{st} and 99^{th} 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 controls 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).

G Internet Appendix: Extension for Multi-Products Firms

A natural concern is that firm-level welfare comparisons may require too much of the model's assumptions if firms sell a variety of products. In such situations, firm-level revenue productivity might not correspond directly to firm-product-level revenue productivity. Here, we note that our methodological contribution is flexible enough to accommodate for this. If a researcher had access to product-level information, as in De Loecker et al. (2016), the welfare analysis conducted would also be feasible at the product-level. Below we sketch the model and the required modifications to our sufficient statistics equations provided in the main text.

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

$$U_{jst}^{pri} = \Big(\int_{i \in F_{jst}} (exp(z_{ijt}) Q_{ijt}^{pri})^{(\sigma-1)/\sigma} di \Big)^{\sigma/(\sigma-1)}, \tag{45}$$

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:

$$Q_{ijt} = L_{it}^{\alpha_l^j} M_{ijt}^{\alpha_m^j} K_{ijt}^{\alpha_k^j} exp(\omega_{it} + u_{ijt}), \tag{46}$$

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.

With revenue and input expenditures, the econometrician estimates the revenue production function at the product level:

$$r_{ijt} = \beta_l^j \bar{l}_{ijt} + \beta_m^j \overline{m}_{ijt} + \beta_k^j \overline{k}_{ijt} + \omega_{ijt}^* + \psi_{jst}^* + \xi_{ijt}^* + \varepsilon_{ijt}, \tag{47}$$

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.[44] Then, the estimated production functions are used to obtain estimates of the firm-product-level revenue productivities.

Lastly, the social excess costs of procuring product j from politically connected firms rather than non-connected firms under flexible capital are given by:

$$SOEC_{flex}^j = exp\Big(\frac{\omega_{ijt}^{*unc} - \omega_{ijt}^{*unc}}{\beta_l^j + \beta_m^j + \beta_k^j}\Big) - 1. \tag{48}$$

For fixed capital, the researcher would need to estimate input shares ρ_{ijt} across the different products within the firm, using the methodology outlined in De Loecker et al. (2016). With the input shares in hand, the social excess costs at the product-level when capital is fixed are given by:

$$SOEC_{fixed} = exp\Big(\frac{\beta_k^j}{\beta_l^j + \beta_m^j + \beta_k^j}[ln(S_{ijt}^{k,unc}) - ln(S_{ijt}^{k,con})] + \frac{\omega_{ijt}^{*unc} - \omega_{ijt}^{*con}}{\beta_l^j + \beta_m^j + \beta_k^j}\Big) - 1, \tag{49}$$

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

[44]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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