

Public Procurement and Firms

Evidence from Kenya

Justice Tei Mensah
Peter Chacha Wankuru
Benard K. Kirui



WORLD BANK GROUP

Africa Region

Office of the Chief Economist

October 2025

Abstract

How important is government business to the private sector in developing economies? This paper uses administrative tax data on firm-to-firm transactions in Kenya to examine the effects of becoming a government contractor on firm performance. Using an event study design, the paper documents significant gains from becoming a supplier to a government entity. Four years later, beneficiary firms experience a 27 percent increase in productivity and employ 10

percent more. These effects are somewhat comparable to the gains from joining a multinational supply chain. Beneficiary firms also expand their trading networks to other private firms. Relaxing credit constraints and improving resilience to shocks are likely operative channels of impact. These findings highlight the potential welfare gains from improving efficiency in public procurement.

This paper is a product of the Office of the Chief Economist, Africa Region. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at <http://www.worldbank.org/prwp>. The authors may be contacted at jmensah2@worldbank.org.

The Policy Research Working Paper Series disseminates the findings of work in progress to encourage the exchange of ideas about development issues. An objective of the series is to get the findings out quickly, even if the presentations are less than fully polished. The papers carry the names of the authors and should be cited accordingly. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

Public Procurement and Firms: Evidence from Kenya*

Justice Tei Mensah[†] Peter Chacha Wankuru[‡] Benard K. Kirui[§]

Authorized for distribution by Andrew Dabalen, Chief Economist, Africa Region, World Bank Group

*We thank the Kenya Revenue Authority (KRA) for the collaboration and access to underlying data for the analysis. We thank Terry M. Fleming for excellent research assistance to the team. We thank Verena Wiedemann for guidance and support related to the administrative data as well as her collaboration on previous work to build the database used in this paper. This project was made possible thanks to research funding from the World Bank's Africa Chief Economist Office as a background paper to the *Firms and Job Creation in Africa* report. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of The World Bank Group and its affiliated organizations or those of the Executive Directors of The World Bank Group, The International Monetary Fund, their executive board, management or the governments they represent. ©2025 The World Bank and International Monetary Fund

[†]Office of the Chief Economist, Africa Region, The World Bank. Email: jmensah2@worldbank.org.

[‡]International Monetary Fund. Email: pwankuru@imf.org.

[§]Privatization Commission of Kenya. Email: bkkirui@gmail.com.

1 Introduction

Government purchases of goods and services are sizable, accounting for approximately 12% of global GDP (Bosio et al., 2022). In many countries, government contracts are a primary channel through which public funds flow to private firms, stimulating growth, jobs, and innovation. However, the extent to which winning a government contract shapes the growth trajectory of firms in developing countries is not well understood. At the same time, firms in developing countries, particularly in Africa, face significant hurdles to growth resulting from a mix of demand- and supply-side constraints (Atkin et al., 2017; Quinn and Woodruff, 2019; Verhoogen, 2023; Mensah, 2024). Thus, questions on the extent to which demand shocks induced by government contracts affect firm performance deserve attention.

In this paper, we leverage administrative microdata on firm-to-firm transactions in Kenya to examine the extent to which winning a government contract affects firm performance and the underlying mechanisms. Specifically, we ask two key questions: (i) Do firms that win a government contract perform better relative to their peers who do not? (ii) What are the channels through which becoming a government contractor affects firm growth?

Using an event study difference-in-difference (DID) design, we exploit staggered variations in the transition to becoming a government contractor—the first time a firm develops a serious trading relationship with a government agency as in Amiti et al. (2024)—to identify the impact of the contract on firm outcomes. Essentially, we compare changes in the outcomes of firms that win a government contract with firms that do not before and after the start of the trading arrangement with a government agency. This relies on the assumption that absent the assignment of a government contract, the performance of treated and control firms would have evolved along a similar pattern. Our event study analysis supports this assumption.¹ To address concerns regarding selection into becoming a government contractor, we present additional analyses using a matched DID design and find similar results. We also provide additional robustness tests to rule out other threats to our identification strategy, such as the definition of who becomes a government contractor and the role of COVID-19 and associated fiscal stimulus in influencing our results.

Five key findings emerge from our analysis. First, firms that sell to a government agency

¹We estimate our baseline results using the Sun and Abraham (2021) estimator. We also find similar results using the Borusyak et al. (2024), stacked DID, and TWFE estimators.

perform better relative to their counterparts who do not, and these effects persist even in the medium term. We find that while treated and control firms experience similar trends in revenue, treated firms experience a large and sustained increase in revenue relative to control firms after becoming a supplier to government. Total firm sales (value added) increase by 36% (23%) in the first year reaching 73% (36%) by the fourth year. Firm productivity also improves: Value added (sales) per worker increases by 24% (35%) and 27% (56%) in the first and fourth years, respectively.

Second, we document positive labor market effects. Four years later, firms that win a government contract employ 10% more workers than non-government contractors. Average wages also increase by about 25% in treated relative to control firms.

Third, although firms that supply to a government experience an initial slump in sales to other firms, they recover thereafter. On a net basis, sales to other firms (excluding governments) are positive. This is suggestive that firms may face an initial capacity constraint which tends to relax overtime, and not necessarily a matter of government contracts "crowding out" sales to the private sector (Almunia et al., 2021; Alfaro-Urena et al., 2022; Amiti et al., 2024).

Fourth, and related to the above, we also find evidence that the positive effects of the demand shock induced by government contracts propagate through firm networks. On the extensive margin, we find a sustained increase in the number of suppliers with which a firm trades after securing a government contract, indicative of an expansion in the firm's trading network and/or diversification of its supply sources. In the intensive margin, the volume of purchases by the firm from its suppliers also increases after becoming a contractor, albeit this result is less robust. These provide suggestive evidence of a positive multiplier effect of government contracts on firm networks.

Finally, our findings also show that the effects of government contracts on firm performance are, in order of magnitude, similar to or sometimes higher than the gains from trading with a foreign multinational firm. Becoming a supplier to a multinational firm is one of the most coveted trading partnerships due to its positive impact on firm growth through channels such as technology transfer and timely payments (Alfaro-Urena et al., 2022; Amiti et al., 2024). Our findings of relatively similar effects on firms from trading with governments are indicative of how important government contracts are to firms and the economy at large. However, it should not be interpreted as more beneficial to firms than trading with

multinationals, largely because these trading arrangements, aside the demand shock, offer somewhat different benefits to firms.

To unpack the estimated impacts of government contracts on firms, we provide evidence on two mechanisms through which having a trading relationship with a government agency affects firm performance: relaxing credit constraints and improving resilience to shocks. First, we show that firms can leverage government contracts as a form of collateral to secure credit from the banking sector. Since governments "always" honor their payment obligations, banks are often willing to lend to firms that have a contractual arrangement with a government agency (di Giovanni et al., 2022). Our results show that firms with a government contract are about 18 percentage points more likely to have a credit line from a bank relative to firms without a government contract. Second, we provide evidence consistent with the notion that government contracts provide a cushion to firms during times of economic crisis, with the COVID-19 pandemic as a case in point.

This paper makes three main contributions to the literature. First, it is more related to the strand of literature that evaluates how public procurement matters for firm growth (Ramey, 2011; Ferraz et al., 2021; Goldman, 2020; di Giovanni et al., 2022; Hang and Zhan, 2023; Ilzetzi, 2024; Gabriel, 2024; Cappelletti et al., 2024). Despite the noticeable increase in studies on the effects of public procurement, there is a lack of evidence from a developing country perspective, with Carvalho et al. (2023) and Ferraz et al. (2021) among the few exceptions. However, even for these few studies in developing countries, they are mostly concentrated in Latin America, with little evidence from Africa. The only known studies that examine how public procurement influences firm outcomes in the African context are Hoekman and Sanfilippo (2018) and Hoekman et al. (2024) – a paper written concurrently with our paper.² Thus, our paper makes a significant contribution to the literature using unique administrative data on firms to show how government contracts influence firm dynamics in Africa.

Second, our study contributes to the growing literature on the effects of demand and supply side constraints on firm growth in developing countries (Verhoogen, 2023). While several studies have documented the effects of supply-side factors, such as access to infrastructure (Perra et al., 2024; Mensah and Traore, 2024; Mensah, 2024), finance (Beck and Demirgüç-Kunt, 2006), and regulatory hurdles (Aghion et al., 2008) on firm growth in developing eco-

²Hoekman and Sanfilippo (2018) examines the association between firms' participation in public procurement and their performance; while Hoekman et al. (2024) use administrative data from Uganda to examine the effects of government contracts on firm performance.

nomies, there is little evidence on the role of demand-side factors in stimulating firm growth in these economies. Meanwhile, firms in developing countries grapple with low "effective" demand resulting from, among other things, low income levels and market fragmentation which limit their ability to produce at optimal levels. This paper provides new and additional insights on how government purchases can relax these demand constraints and the consequent effect on firm performance.

This study also makes an important contribution to the emerging literature showing how trading networks matter to firm growth (Keller and Yeaple, 2009; Loecker, 2013; Bernard et al., 2019; Almunia et al., 2021; Alfaro-Urena et al., 2022; Amiti et al., 2024). Recent evidence in this area of the literature shows significant productivity gains for firms from joining a trading network of a foreign multinational Alfaro-Urena et al. (2022); Amiti et al. (2024). However, much of this literature has been concentrated in developed economies, in part due to the dearth of granular administrative data required for this type of analysis in developing economies. Recent collaborations between researchers, statistical offices, and revenue authorities in developing countries have opened the door to rigorous research, such as ours, using administrative tax data to study relevant questions in these economies (Chacha et al., 2024). Our results herein align with findings from developed economies of a positive growth impact on firms from demand shocks associated with establishing trading relations with a large corporation or governments. In fact, our findings suggest that the gains from trading with governments are in order of magnitude similar to the gains from trading with a multinational, although the channels underlying the impact may vary.

The remainder of the paper is organized as follows. Section 2 describes the datasets used in the paper. The empirical strategy that guides the analysis is presented in Section 3, while the results are presented and discussed in Section 4. Sections 5 and 6 explore robustness checks and mechanisms, respectively. The paper concludes with a summary of the main findings in Section 7.

2 Data

The main dataset used in this paper is administrative tax data from the Kenya Revenue Authority (KRA) covering the period 2015-2022, constructed in line with a recent paper by Chacha et al. (2024) and Chacha et al. (2022). We combine four key firm datasets: universe of

firm-to-firm transaction-level data from value-added tax (VAT) records; firms' total monthly sales and purchases from VAT returns; number of employees and payroll from pay-as-you-earn returns (PAYE); and information on basic firm characteristics (e.g. 4-digit sector, the business subtype, start date of operations, and the location of headquarters) from tax registration forms. Information on the number of employees is obtained from PAYE tax returns, which are filed on a monthly basis by employers with at least one employee. The data set captures the number of employees and the total monthly payroll. All data sets are merged using anonymized firm identifiers (see [Chacha et al. \(2022\)](#)). Table A.1 in the online appendix provides further details on the frequency, the unit of observation, and the key indicators used in this paper.

We start with 211,913 firms, making 9.5 million firms x monthly observations. We retain only firms with a minimum annual turnover of KES 5 million across the whole period, in line with KRA's VAT qualification threshold of at least KES 5 million in turnover. We also keep firms with annual purchases greater than zero in at least one year that we observe in the data. This results in a sample of 91,537 unique firms with 5.7 million firm x monthly observations.

From this dataset, we build a monthly supplier-buyer panel that captures the total monthly volume and the number of transactions between each buyer-supplier pair, which are further aggregated to yearly levels. The network panel data also allows us to identify the start and end dates of trading relations between representative firm pairs.

In addition, using information from company registration, we classify firms as private or government-owned. Firms with full or partial ownership by the government are classified as government entities. These include public schools, local authorities, ministries, and parastatal companies (i.e., state-owned enterprises). For robustness, we show that our results hold even when the set of government-owned businesses is restricted to only parastatals (SOEs).

We rely on VAT data to identify firms that trade with government entities. Specifically, we follow the approach of [Amiti et al. \(2024\)](#) and classify a (private) firm as a government contractor if the value of its sales to a government entity constitutes at least 15% percent of its annual total sales. In the robustness test, we show that our results are not sensitive to this threshold.

Finally, we complement our analysis with data from the World Bank Enterprise Survey (WBES)

to identify the potential channels through which trading with governments affects firm performance. Table 1 presents the summary statistics of the key variables used in the analysis. Details on the data construction and cleaning process are described in Section A of the Online Appendix.

2.1 Stylized Facts

Before proceeding to evaluate the effect of a government contract on firm outcomes, we present four stylized facts about government contracts in Kenya based on our dataset.

Fact 1: Government contracts are largely allocated to local firms

About 61.4% of the total value of government contracts is allocated to Kenyan-owned businesses (Figure 1 in the appendix). Foreign-owned companies account for about 37%, while government-government contracts account for 1.4% of the value of government contracts in the country.

Fact 2: Large firms are the main beneficiaries of Government contracts

Among local firms that trade with the Kenyan government, micro-enterprises (0 to 4 workers) constitute the largest share (50%) by number (Figure 2a). However, in terms of the value of contracts awarded, large firms (100+ workers) dominate — accounting for more than a third (36%). Micro-enterprises receive about 26% of the total value, while small and medium-sized firms account for 14% and 23% respectively. The Access to Government Procurement Opportunities (AGPO) initiative — which aims to allocate 30% of all public tenders to small and micro businesses, as well as those owned by women, youth, and persons with disabilities — partly explains the high participation of microenterprises in public procurement. Nonetheless, half of total contract value goes to medium and large firms.

Fact 3: Government contracts are heavily concentrated on infrastructure

More than a third (35%) of the value of government contracts is allocated to firms in the construction sector— a reflection of the role of public financing in addressing infrastructure deficits in the country (Figure 3). Firms in manufacturing, transport, wholesale & retail each account for about 10% of government contracts.

Fact 4: Government contracts are disproportionately allocated to firms in the large metropolis

Firms in the Nairobi and Mombasa counties alone account for nearly 87% of all government contracts allocated (Figure 4). Although this suggests a sharp rural-urban gradient in the allocation of public contracts to firms, it is also reflective of the rural-urban gradient in the spatial distribution of formal firms in the country.

3 Empirical Strategy

In an ideal experimental setting where government contracts are randomly assigned to firms, a simple comparison between the eventual winners and losers would yield a causal estimate of the effects of becoming a government contractor on firm outcomes. However, in the real world, government contracts are rarely allocated randomly. In most countries, including Kenya, government contracts are allocated through a public procurement system where private companies either: (i) participate in competitive bids for government contracts; or (ii) are allocated a contract via sole-sourcing (Kang and Miller, 2022). With access to administrative data on public procurement, one could compare the outcomes of firms that barely won a bid with counterparts that barely lost a bid for a government contract to estimate the causal effects of winning a government contract (Ferraz et al., 2015). However, administrative data on public procurement in Kenya are partial, only capturing the eventual winners of the contract and leaving out bidders that did not win the tender. Therefore, to credibly estimate the effects of becoming a government contractor on firm outcomes, we use a number of techniques.

Our baseline results focus on firms with a "serious" and long-term trading relationship with a government entity. In many developing countries, such as Kenya, state presence is ubiquitous. As a result, some firms can have (minor/short-term) trading relations with government agencies (SOEs, government departments, etc.), without necessarily being a "major" supplier or "contractor". Thus, a correlation between whether a firm has any trading relations with a government agency and firm outcomes may not be informative. To this end, we focus on the effects of firms having a "serious" trading relationship with government entities, which we define as the first time a firm's sales to a government entity is at least 15% of total sales of the firm (Amiti et al., 2024).³ Also, to ensure that we observe outcomes

³This approach is similar in spirit to Amiti et al. (2024), who study the effects of trading with a superstar firm using a 10% threshold in identifying serious trading relationships between local firms and superstar firms. While these thresholds are arbitrary, we show in section 5.4 that our results are consistent regardless of the threshold used.

of treated and control firms before treatment, we exclude firms treated within the first two years of our sample period.

We estimate an event study regression which allows us to compare the changes in outcomes of treated and control firms before and after treatment using the specification below:

$$Y_{ijmt} = \sum_{\tau=-5, \tau \neq -1}^5 \delta_{\tau} \times G_{ijm, t-\tau} + \alpha_i + \lambda_{j \times t} + \gamma_a + \theta_{m \times t} + \varepsilon_{ijmt} \quad (1)$$

where Y_{ijmt} denotes the outcome of firm i in industry (4-digit sector) j located in county m in year t . $G_{ijm, t-\tau}$ are event time dummies for each year before and after the firm established a "serious" trading relationship with a government entity, hereafter referred to as a government contractor. We normalize the year before the event ($\tau = -1$) to zero, ensuring that all estimates of δ_{τ} are interpreted relative to the year before the firm became a contractor.⁴ α_i denotes firm fixed effects to absorb time-invariant differences across firms; $\lambda_{j \times t}$ denotes industry \times year fixed effects to absorb time-varying sectoral shocks that may influence firm outcomes, as well as the spillover effects of government contracts on other firms in the sector without direct trading relationships with governments (Amiti et al., 2024). γ_a denote (firm) age fixed effects to control for the (nonlinear) relationship between age and firm performance. We also control for county \times year ($\theta_{m \times t}$) to absorb temporal shocks across sub-national regions that could influence firm performance. We show that our results are robust to their exclusion as well (see Figures B.1 and B.2 in the appendix). Standard errors (ε_{it}) are clustered at the firm level which corresponds to our level of treatment (Bertrand et al., 2004).

Our coefficient of interest is δ_{τ} which traces the changes in firm outcomes before and after becoming a government contractor relative to comparable firms without government contracts. However, ascribing causal interpretations to δ_{τ} hinges on a number of assumptions. First, whether the performance of treated and control firms would have evolved along a similar path in the absence of the government contract, i.e. the so-called parallel trends assumption. While this assumption is empirically untestable, the event study analysis allows us to examine pre-trends to ascertain whether firms that eventually became government contractors were on a positive productivity trend before starting to supply a government entity relative to control firms.

⁴Thus our treatment variable is an absorbing treatment which turns on the first time a firm establishes a serious trading relationship with a government agency and remains so thereafter.

The second relates to selection into becoming a government contractor. Government contracts in Kenya can be awarded through several methods with the most common being request for quotations and open tendering. All these methods are susceptible to selection bias as politically connected firms may gain preferential treatment in the award of contracts (Faccio, 2006; Brugués et al., 2024). Also in some instances, the large size of government contracts may imply that firms of a certain size or capacity may be favored in the award process. These selection issues may potentially be correlated with unobserved determinants of firm performance thereby leading to biased estimates. Section 5 is devoted to addressing this concern. Specifically, we adopt two strategies: (i) We focus exclusively on firms that secure a government contract during the study period (2015-2022) and exploit variations in the timing of treatment in a stacked difference-in-difference design to estimate our impact. By so doing, we purge our estimates from any (un)observable differences between treated and control firms that may be correlated with firm outcomes. (ii) We use a matching procedure—the coarsened–exact matching (CEM) approach (Iacus et al., 2012)—to match treated and control firms based on pre-treatment characteristics and replicate our baseline results. Reassuringly, the findings from both approaches are consistent with our baseline results of a positive impact of government contracts on firm outcomes.

Finally, recent advances in the difference-in-difference literature suggest that in models with staggered treatment, the conventional two-way fixed effect estimator (TWFE) is plausibly biased when treatment effects are heterogeneous across groups and time (De Chaisemartin and D’Haultfoeuille, 2018, 2020a,b; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). To address these challenges and ensure robustness, we estimate our baseline results using the Sun and Abraham (2021) estimator. In Section 5, we compare our baseline results with results from alternative estimators including the TWFE and Borusyak et al. (2024) estimators, and find consistent results.

4 Results

4.1 Main Results

We start our analysis by looking at how government contracts influence the performance of firms, focusing on three broad outcomes: revenue,⁵ value-added, productivity,⁶ and employment. Figure 5 presents our event study estimates using the Sun and Abraham (2021) estimator. Before delving deep into the estimates, we evaluate the extent to which our results satisfy our identifying assumptions. As outlined in section 3, the main identifying assumption is that the performance of treated and control firms would have followed the same time path (trends) even in the absence of having a government contract at least conditional on our fixed effects. Our results provide comfort to assuage these concerns, as our event study estimates indicate similar "(pre)trends" in the pretreatment period, between the control firms and those that eventually got treated. After treatment, we see a sharp divergence between the performance of treated and control firms. Specifically, we observe a sharp and sustained increase in the performance (across all outcomes) of treated relative to control firms. This provides suggestive evidence that our estimates represent the causal impact of having a government contract rather than other shocks that may have occurred before or after the treatment.

We now turn to the results. We observe a significant and sustained increase in total firm sales following the start of a contractual relationship with a government agency. In the first year of treatment, sales increase by 36% (31 log points)⁷ and 70% (73%) by the fifth (fourth) year. A counter argument may be that the surge in sales is not particularly surprising given that the treatment is a demand shock to the firm. In other words, is there evidence of increased firm revenue, net of the sales to the government? The results on sales to private firms suggest so. Despite a significant dip (84%) in sales to other private firms, in the first year, perhaps suggestive of a short-run diversion away from other customers to meet the needs of an "important" new customer, they rebound in the second year and increase over time. In the long run, sales to other firms increase by about 65%. This pattern of dip and rebound in sales to other firms is consistent with the "venting out" thesis of Almunia et al. (2021), and findings of Amiti et al. (2024) and Alfaro-Urena et al. (2022) showing an initial diversion of sales from

⁵Measured by the log of total sales, and sales to other firms excluding government

⁶Value added per worker, sales per worker

⁷ $\exp(.309) - 1 = .362$

existing customers whenever a firm secures a large contract with a larger buyer.

Beyond revenue, we also explore the extent to which trading with governments affects value-added and firm productivity (sales per worker and value-added per worker). The results show that becoming a government contractor is associated with a significant and sustained increase in these productivity measures. Value-added (sales) per worker, for instance, increases by 24% (35%) and 27% (56%) in the first and fourth years respectively, reaching 34% (58%) by the fifth year.

In addition, we also explored the labor market impacts of government contracts. Are the observed increases in revenue and productivity driven by investments in labor-saving technologies or firms expand their labor inputs in response to the demand shock induced by government contracts? Due to data constraints, we are unable to examine the effects of the government contract on firms' technology investments but are able to examine the changes in employment before and after a firm secures a government contract. The results in Figure 6 provide evidence that government contracts are associated with increased hiring by firms, albeit the results are generally significant at 90% confidence level. Firm employment increases by 10% in the fourth year after becoming a government contractor. Returns to labor also increase. Average wages in treated firms increase by about 23% relative to control firms.

Upstream Impacts

An important yet often ignored question is the extent to which government contracts affect other firms in the value chain. To explore this question, we leverage our granular data on firm-to-firm transactions to analyze the potential effects of government contracts on a firm's upstream trading relationships. We focus on two main measures: the number of upstream suppliers that the government contractor trades with, and the value of purchases that it makes following the award of a government contract. A positive effect on these measures will offer suggestive evidence of how demand shocks from the government cascades through the value chain and generates economic benefits via the multiplier effect.

In Figure 7 panel (a), we find a sustained increase in the number of (input) suppliers that a firm trades with after securing a contract with a government agency. The increase in the number of suppliers to the treated firm is indicative of an expansion in the firm's network, and plausibly an increase in input demand. The latter is confirmed by the results in panel (b), where we observe an increase in the value of firm purchases.

4.2 Comparison with Effects of Trading with Other Large Firms

A well-known fact in the literature is that establishing trade links with large firms particularly, multinational firms, offers significant gains to local firms, mainly through technology and knowledge transfer as well as access to markets (Alfaro-Urena et al., 2022; Amiti et al., 2024). Meanwhile, our results herein also suggest significant gains to firms by trading with governments. Hence, a natural question that emerges is how the impact of government contracts on firm performance compares with the effects of trading with other (private) large firms.

To this end, we utilize our data to identify firms that develop "serious" trading relationships with large and multinational firms using the same threshold as in our baseline⁸ and estimates the effects of trading with a multinational entity (MNE) on firm performance using our baseline specification in equation 1.

The results shown in Figure 8 show fairly stable pre-trends, and a sharp and sustained increase in firm outcomes in the post-treatment period. In panel (a) we observe a 34% increase in firm sales in the first year of becoming a supplier to a multinational firm. The effect rises marginally to 38% in the fourth year. The effect is similar even if we focus on sales to other non-multinational firms. The effects on firm productivity are also positive and significant. Value-added per worker for instance increases by 41% and 36% in the first and fourth years respectively. The impact on employment is positive but not statistically significant. We also conduct similar analysis focusing on trading with large domestic firms (See Figure B.5 in Appendix), albeit the results are less robust.

Overall, by juxtaposing the impact of government contracts with the gains from trading with foreign multinationals, we can conclude that both relationships exert positive gains for the firms. In some instances, the gains from trading with government are slightly higher than those from trading with a multinational. These differences pertain even when we focus exclusively on the gains from trading with state-owned enterprises (SOEs)⁹ as shown in Figure B.6 in the appendix. However, this should not be interpreted as implying that trading with governments is more beneficial than trading with an MNE, as these trading relationships are not mutually exclusive, and these trading arrangements can be sector specific. Besides

⁸A firm is considered trading with a multinational firm as the first time its sales to a multinational firm exceed 15% of sales.

⁹These are quasi-public owned businesses that operate across sectors.

the technological gains trading with MNEs are essential for increasing economic complexity and reducing the susceptibility of domestic firms to local economic shocks.

5 Robustness Checks

Despite the stable pre-trends in our event study results, some concerns about identification of causal impacts still linger. In this section, we present several strategies to address these concerns and provide support to our baseline findings.

5.1 Stacked Difference-in-Difference Design

Endogenous selection of who becomes a government contractor is a potential challenge. As highlighted in Section 3, the endogenous selection could result from either (i) the manner in which firms are selected for government contracts, for example, sole sourcing or politically connected firms (Kang and Miller, 2022; Faccio, 2006; Brugués et al., 2024); or (ii) the type of firms that self-select into bidding for government contracts.

To assuage this concern, we present additional estimates using the stacked difference-in-difference (SDiD) design. The SDiD design relies on variations in the timing of becoming a government contractor rather than whether or not a firm is a contractor (Deshpande and Li, 2019; Fadlon and Nielsen, 2021). Unlike the conventional DiD approach that compares firms that eventually become contractors with those that do not, the SDiD compares contractor firms with similar firms that would become contractors at a future date. Thus, even though both the treated and control firms eventually get treated, at each point in time the estimator compares "currently" treated firms with "would be" treated firms. A nice appeal of the SDiD approach is that it only requires the timing of the contract allocation to be as good as random. It does not require the contract allocation to be random.¹⁰ Also, the SDiD is robust to biases induced by heterogeneous treatment effects often associated with the conventional TWFE estimates (Wing et al., 2024).

To perform the SDiD, we construct our sample as follows. First, we focus on firms that became contractors between 2017 and 2019 as treated. We then create separate datasets for firms that got treated in each of these years. In each dataset, we keep observations two years

¹⁰A major drawback of this approach is that it is well-suited for long panels.

before and after the treatment year as our event window. Second, for each dataset, we add observations for the same calendar years in the event window, on all firms that eventually became contractors three or more years after the respective treatment sample year. For instance, for firms that became contractors in 2017, their event window will be all observations between 2015 and 2019. Similarly, we keep the observations for the same calendar years in the event window on all firms that eventually became contractors in 2020 or later. We repeat this process for each treatment year cohort. We then stack the data for the respective treatment year cohorts and estimate the following specification:

$$Y_{ict} = \sum_{\tau=-2, \tau \neq -1}^2 \beta_{\tau} \times I_{\tau} + \sum_{\tau=-2, \tau \neq -1}^2 \delta_{\tau} \times I_{\tau} \times G_{ic} + \alpha_{ic} + \lambda_{j \times t} + \theta_{m \times t} + \gamma_a + \varepsilon_{ict} \quad (2)$$

where Y_{ict} represents the outcome of firm i of (treatment) cohort c in year t ; I_{τ} indexes leads and lags in event time; G_{ic} is a dummy variable defined equal to 1 for treated (contractor) firm in treatment cohort c and 0 if otherwise; α_{ic} indexes firm by cohort fixed effect; $\lambda_{j \times t}$ indexes industry by year fixed effect; $\theta_{m \times t}$ indexes county by year fixed effect; while γ_a indexes firm age fixed effects. The variable of interest is δ_{τ} , which measures the effects of becoming a contractor on firm outcomes. Standard errors are clustered at the firm by cohort level.

Results

Results from the SDiD design are presented in Figure 9. Despite a short event window,¹¹ the results largely point to stable 'pre-trends', and a sustained increase in the outcomes post-treatment. The only exception is employment, where we have found null effects. Overall, our SDiD results provide support for the robustness of our baseline results, thus playing down concerns of endogenous selection into treatment driving our results.

5.2 Matched Difference-in-Difference

There could also be concerns that firms that self-select into bidding for government contracts differ from firms that do not. In addition, those that eventually win these contracts may also differ from those that lose along some observable characteristics. To address this concern, we perform a matching exercise in which we match treated firms with control firms based on a set of pretreatment characteristics (i.e., firm start year, sector, county/location,

¹¹Due to the short panel nature of our data

and firm size) using the coarsened exact matching (CEM) approach (Iacus et al., 2012). The matching ensures that we find comparable firms to serve as the control group, although in principle our identification strategy relies on parallel trends between treated and control firms, not complete balance on covariates (see Table A.7 in the Appendix for the balance table). With the matched sample, we then replicate our baseline event study analysis using the Sun and Abraham (2021) estimator.

Once again, the results (Figure 10) show that winning a government contract has a positive and statistically significant impact on firm performance, with the effect sizes similar in magnitude to our baseline results.

5.3 Heterogeneous Treatment Effects

Next, we present additional results comparing the baseline results (from the Sun and Abraham (2021) estimator) with estimates from other variant estimators such as the conventional TWFE, and the Borusyak et al. (2024) estimators. This comparison helps identify the extent to which our results may be influenced by heterogeneity in the treatment effects across cohorts. Assuredly, the results from the three estimators as shown in (Figure 11) are qualitatively and quantitatively similar.

5.4 Threshold for Defining Treatment

Having established the robustness of our baseline estimates to the various identification issues, we now turn our attention to other concerns. Recall that our baseline definition of a government contractor is the first time a firm's sales to a government entity is at least 15% of total sales. In this section, we examine the extent to which our headline results are driven by the choice of this threshold. Specifically, we define treatment using varying thresholds including 5%, 10%, 15%, 20%, 25%, and 30%.

Figure 12 shows the results at varying thresholds. Once again the results are qualitatively and quantitatively similar across the various thresholds. One noticeable difference is that the results are less robust at the 5% threshold with a pattern of differential pretrends. Beyond that, the results generally satisfy the parallel trends assumption. A potential explanation for the weak results when using the 5% threshold for defining treatment is that it could be

picking up small trading arrangements between firms and a government agency that may not necessarily amount to a "serious" contractual agreement between the parties, hence the treatment, in that case, maybe picking up "noise".

Related to the above, if firms with small sales to government agencies make our treatment "noisy", then one may be concerned that even our baseline results may be contaminated by the same if we include these observations in our control group. In other words, would our baseline results suffice if we compare firms with "serious" trading with a government agency with firms without "any" trading with a government agency? To test this, we present additional results where we exclude all firms with "smaller" (i.e. sales to government less than 15%) from the sample, and instead consider those whose sales to a government agency is at least 15% as treatment and those with no sales to a government as control. The results in Figure B.3 in the Appendix are qualitatively and quantitatively similar to our baseline results in Figure 5, further confirming the robustness of our results.

5.5 Excluding the COVID-19 Era

Finally, we examine the extent to which the COVID-19 induced economic shocks and associated policy responses could be driving our results. During the pandemic, government interventions were critical to stimulating economies back to recovery. Since our sample period overlaps with the COVID-19 era, there may be concerns that our results are entirely driven by the stimulus measures the Kenyan government implemented which may have induced large benefits to government contractors.

To mitigate this concern, we replicate our analysis while focusing exclusively on the pre-covid period. The results as shown in Figure B.4 in the appendix, once again lend support to our baseline conclusions of a positive impact of government contracts on firm performance. Across the various outcomes, we find a sharp and sustained increase in the outcomes after a firm enters into a "serious" trading relationship with a government agency. The only exception is employment where we find null effects in some cases.

6 Exploring Mechanisms

After documenting the performance gains to firms from trading with government, we proceed to uncover the potential channels through which these gains arise. There are at least three candidate explanations of how winning a government contract is associated with increased firm performance. These include: (i) Price mark-ups: the possibility that government agencies offer high prices for goods and services relative to other private sector buyers, especially if procurement rules are not competitive; (ii). Access to credit: the ability of firms to use government contracts as collateral to secure credit from banks; and (iii). Resilience to shocks: the need for government services creates demand for goods and services to the extent that even in the phase of economic crisis, government suppliers will still be able to get demand for their services compared to their counterparts who may face negative demand shocks. However, due to data constraints, we only provide evidence on the credit and resilience channels.

6.1 Credit Channel

Credit constraints are a major challenge for firms in developing countries owing to the underdevelopment of capital markets with associated high lending rates and relatively high collateral requirements (Banerjee and Duflo, 2014; De Mel et al., 2008). Government contracts offer firms a "guaranteed" future cash flow that allows them the opportunity to overcome these credit constraints, as contracts can be leveraged as collateral (di Giovanni et al., 2022).¹² These loans often come at relatively low interest due to the perceived guarantee that governments always honor their payment obligations.

To test this channel, we use data from the World Bank Enterprise Surveys (WBES) on Kenya between 2007 and 2018¹³ and explore the association between firms that "secured (or attempted) a government contract" and proxy measures of access to credit: having a credit line with a bank/financial institution, having an overdraft facility, whether the firm needed collateral for their most recent credit line, and bank credit as a share of the firm's working capital.

¹²<https://mwananchicredit.com/contract-financing-kenya/>

¹³Three rounds of repeated cross-section for the years 2007, 2013, and 2018. Analysis undertaken using OLS estimations.

The results are shown in Table 2. Starting with column 2, the results show that firms with a government contract are about 18-22 percentage points (pp) (about 45% relative to the mean) more likely to secure a credit line from a bank relative to firms without a government contract. Similarly, in column 3, government contractors have an 8 pp chance of having an overdraft relative to non-government contractors, albeit the results are not significant when we control for firm characteristics in column 4. In terms of the requirement of using collateral for their most recent loan, we do not find any statistically significant differences between government and non-government contractors. Finally, we see in columns 7-8 that bank credit constitutes a greater share of working capital for government contractors than non-government contractors. The share of bank credit in working capital is about 5.2 pp higher for government contractors than non-government contractors. Put together, the results herein provide suggestive evidence that firms with government contracts have higher likelihood of accessing credit from the financial sector relative to firms without a government contract. The ensuing credit may allow the beneficiary firms to expand investments into productive capital thereby boosting productivity, and these impacts may linger on even after the expiration of the contracts with a government agency.

6.2 Resilience to Shocks

We also provide evidence consistent with the notion that government contracts provide a cushion to firms during times of economic crisis. A case in point is the COVID-19 pandemic and the economic crisis that ensued which had significant negative effects on firms around the world (Apedo-Amah et al., 2020; Chacha et al., 2024). Apedo-Amah et al. (2020) for instance showed that even firms that otherwise had sound financial standing before the crises were suddenly faced with liquidity constraints and solvency issues due to a slump in demand from their private sector clients. At the same time, governments around the world stepped in to provide services to prevent economies from falling into recession. Government contractors would therefore benefit from the boom in government operations.

To test this mechanism, we compare the outcomes of firms that had a government contract before the COVID-19 pandemic with non-government contractors before and after the COVID period and find that firms that had any government contract before the pandemic performed relatively better across all the metrics during the COVID-19 era relative to firms

without a government contract, as shown in Table 3.¹⁴ This provides suggestive evidence of the effects of government contracts in helping firms adapt to external shocks in the economy. Admittedly, there is the possibility that cyclicalities in government spending may affect the performance of firms that are reliant on government contracts thereby limiting their growth.

7 Concluding Remarks

This paper examines the effects of developing a serious trading relationship with a government agency on the performance of firms in a developing country context. We also provide evidence on potential mechanisms.

To this end, we assemble unique administrative data on (i) business-to-business transactions of the universe of formal firms in the VAT registry, (ii) VAT tax returns, and (iii) PAYE tax returns in Kenya over the period 2015-2022. Our identification strategy exploits the staggered variations in the transition to becoming a government contractor—the first time a firm develops a serious trading relationship with a government agency—in an event study design.

Our results show that government contracts have positive and sizable effects on firm growth. These effects are, in order of magnitude, similar to the gains from trading with a foreign multinational firm. Four years after becoming a government contractor, the firm's productivity (value added per worker) increases by 27% and employs 10% more workers. Sales to other private sector firms also increase despite an initial large drop, thus ruling out concerns of government contracts "crowding out" private sector businesses. In addition, firms that win a government contract expand their network by increasing the number of suppliers they buy from as well as the volume of goods and services they purchase, which is indicative of the multiplier effects of these contracts. Returns to labor increase, with the average wage rising by about 25% relative to non-government contractors. Finally, our results provide suggestive evidence on two potential channels underlying the results: (i) firms leverage government contracts to secure access to credit, thus relaxing their credit constraints; and (ii) government contracts provide cushion to firms during times of economic crisis, with the COVID-19 pandemic as a case in point.

¹⁴To avoid the problem of reverse causality we excluded firms that become government contractors during the COVID-19 era

Overall, the findings from this paper provide insights into the potential effects of government businesses in stimulating growth in the private sector, particularly in developing countries where demand constraints are binding. It further underscores the importance of improving efficiency in public procurement.

References

- Aghion, P., Braun, M., and Fedderke, J. (2008). Competition and productivity growth in south africa. *Economics of transition*, 16(4):741–768.
- Alfaro-Urena, A., Manelici, I., and Vasquez, J. P. (2022). The effects of joining multinational supply chains: New evidence from firm-to-firm linkages. *The Quarterly Journal of Economics*, 137(3):1495–1552.
- Almunia, M., Antràs, P., Lopez-Rodriguez, D., and Morales, E. (2021). Venting out: Exports during a domestic slump. *American Economic Review*, 111(11):3611–3662.
- Amiti, M., Duprez, C., Konings, J., and Van Reenen, J. (2024). Fdi and superstar spillovers: Evidence from firm-to-firm transactions. *Journal of International Economics*, 152:103972.
- Apedo-Amah, M. C., Avdiu, B., Cirera, X., Cruz, M., Davies, E., Grover, A., Iacovone, L., Kilinc, U., Medvedev, D., Maduko, F. O., et al. (2020). Unmasking the impact of covid-19 on businesses: Firm level evidence from across the world. Technical report.
- Atkin, D., Khandelwal, A. K., and Osman, A. (2017). Exporting and firm performance: Evidence from a randomized experiment. *The quarterly journal of economics*, 132(2):551–615.
- Banerjee, A. V. and Duflo, E. (2014). Do firms want to borrow more? testing credit constraints using a directed lending program. *Review of Economic Studies*, 81(2):572–607.
- Beck, T. and Demirguc-Kunt, A. (2006). Small and medium-size enterprises: Access to finance as a growth constraint. *Journal of Banking & finance*, 30(11):2931–2943.
- Bernard, A. B., Dhyne, E., Magerman, G., Manova, K., and Moxnes, A. (2019). The origins of firm heterogeneity: A production network approach. national bank of belgium, working paper no. 362.
- Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, 119(1):249–275.
- Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: robust and efficient estimation. *Review of Economic Studies*, page rdae007.
- Bosio, E., Djankov, S., Glaeser, E., and Shleifer, A. (2022). Public procurement in law and practice. *American Economic Review*, 112(4):1091–1117.

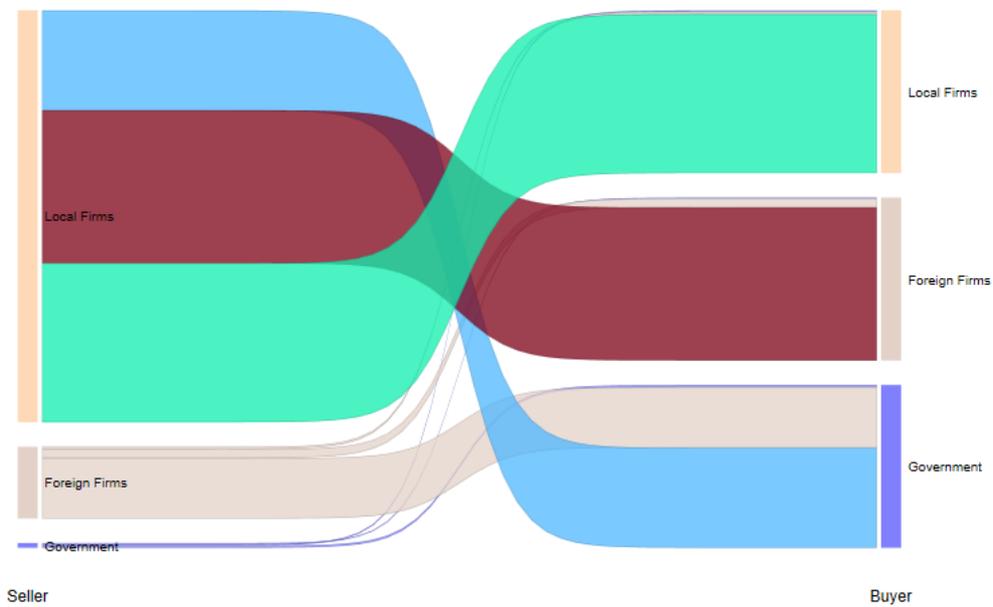
- Brugués, F., Brugués, J., and Giambra, S. (2024). Political connections and misallocation of procurement contracts: evidence from Ecuador. *Journal of Development Economics*, 170:103296.
- Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, 225(2):200–230.
- Cappelletti, M., Giuffrida, L. M., and Rovigatti, G. (2024). Procuring survival. *The Journal of Industrial Economics*.
- Carvalho, M., Galindo da Fonseca, J., and Santarrosa, R. (2023). How are wages determined?: a quasi-experimental test of wage determination theories. *Cahier de recherche*, (2023-01).
- Chacha, P., Kirui, B., and Wiedemann, V. (2022). Mapping Kenya's production network transaction by transaction. *Available at SSRN 4315810*.
- Chacha, P. W., Kirui, B. K., and Wiedemann, V. (2024). Supply chains in times of crisis: Evidence from Kenya's production network. *World Development*, 173:106363.
- De Chaisemartin, C. and D'Haultfoeuille, X. (2018). Fuzzy differences-in-differences. *The Review of Economic Studies*, 85(2):999–1028.
- De Chaisemartin, C. and D'Haultfoeuille, X. (2020a). Difference-in-differences estimators of intertemporal treatment effects. Technical report.
- De Chaisemartin, C. and D'Haultfoeuille, X. (2020b). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.
- De Mel, S., McKenzie, D., and Woodruff, C. (2008). Returns to capital in microenterprises: evidence from a field experiment. *The quarterly journal of Economics*, 123(4):1329–1372.
- Deshpande, M. and Li, Y. (2019). Who is screened out? application costs and the targeting of disability programs. *American Economic Journal: Economic Policy*, 11(4):213–248.
- di Giovanni, J., García-Santana, M., Jeenas, P., Moral-Benito, E., and Pijoan-Mas, J. (2022). Buy big or buy small? procurement policies, firms' financing, and the macroeconomy. *Procurement Policies, Firms' Financing, and the Macroeconomy (February 1, 2022)*. *FRB of New York Staff Report*, (1006).
- Faccio, M. (2006). Politically connected firms. *American economic review*, 96(1):369–386.

- Fadlon, I. and Nielsen, T. H. (2021). Family labor supply responses to severe health shocks: Evidence from danish administrative records. *American Economic Journal: Applied Economics*, 13(3):1–30.
- Ferraz, C., Finan, F., and Szerman, D. (2015). Procuring firm growth: the effects of government purchases on firm dynamics. Technical report, National Bureau of Economic Research.
- Ferraz, C., Finan, F., and Szerman, D. (2021). Procuring firm growth: the effects of government purchases on firm dynamics. Technical report.
- Gabriel, R. D. (2024). The credit channel of public procurement. *Journal of Monetary Economics*, page 103601.
- Goldman, J. (2020). Government as customer of last resort: The stabilizing effects of government purchases on firms. *The Review of Financial Studies*, 33(2):610–643.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.
- Hang, J. and Zhan, C. (2023). Government procurement and resource misallocation: Evidence from china. *Journal of Economic Behavior & Organization*, 216:568–589.
- Hoekman, B. and Sanfilippo, M. (2018). Firm performance and participation in public procurement: Evidence from sub-saharan africa. *Robert Schuman Centre for Advanced Studies Research Paper No. RSCAS*, 16.
- Hoekman, B., Sanfilippo, M., Santi, F., and Ticku, R. (2024). DP19673 Government Demand and Firm Growth. CEPR Discussion Paper 19673, CEPR Press, Paris & London.
- Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political analysis*, 20(1):1–24.
- Ilzetzi, E. (2024). Learning by necessity: Government demand, capacity constraints, and productivity growth. *American Economic Review*, 114(8):2436–2471.
- Kang, K. and Miller, R. A. (2022). Winning by default: Why is there so little competition in government procurement? *The Review of Economic Studies*, 89(3):1495–1556.

- Keller, W. and Yeaple, S. R. (2009). Multinational enterprises, international trade, and productivity growth: firm-level evidence from the united states. *The review of economics and statistics*, 91(4):821–831.
- KNBS (2016). Micro, small and medium establishment (msme) survey: Basic report 2016. Technical report, Kenya National Bureau of Statistics.
- Loecker, J. D. (2013). Detecting learning by exporting. *American Economic Journal: Microeconomics*, 5(3):1–21.
- Mensah, J. T. (2024). Jobs! electricity shortages and unemployment in africa. *Journal of Development Economics*, 167:103231.
- Mensah, J. T. and Traore, N. (2024). Infrastructure quality and fdi inflows: Evidence from the arrival of high-speed internet in africa. *The World Bank Economic Review*, 38(1):1–23.
- Perra, E., Sanfilippo, M., and Sundaram, A. (2024). Roads, competition, and the informal sector. *Journal of Development Economics*, 171:103339.
- Quinn, S. and Woodruff, C. (2019). Experiments and entrepreneurship in developing countries. *Annual Review of Economics*, 11(1):225–248.
- Ramey, V. A. (2011). Can government purchases stimulate the economy? *Journal of Economic Literature*, 49(3):673–685.
- Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of econometrics*, 225(2):175–199.
- Verhoogen, E. (2023). Firm-level upgrading in developing countries. *Journal of Economic Literature*, 61(4):1410–1464.
- Wiedemann, V., Kirui, B. K., Khandelwal, V., and Chacha, P. W. (2024). Spatial inequality and informality in kenya’s firm network. *World Bank Policy Research Working Paper*, No. 10932.
- Wing, C., Freedman, S. M., and Hollingsworth, A. (2024). Stacked difference-in-differences. Technical report, National Bureau of Economic Research.

Figures

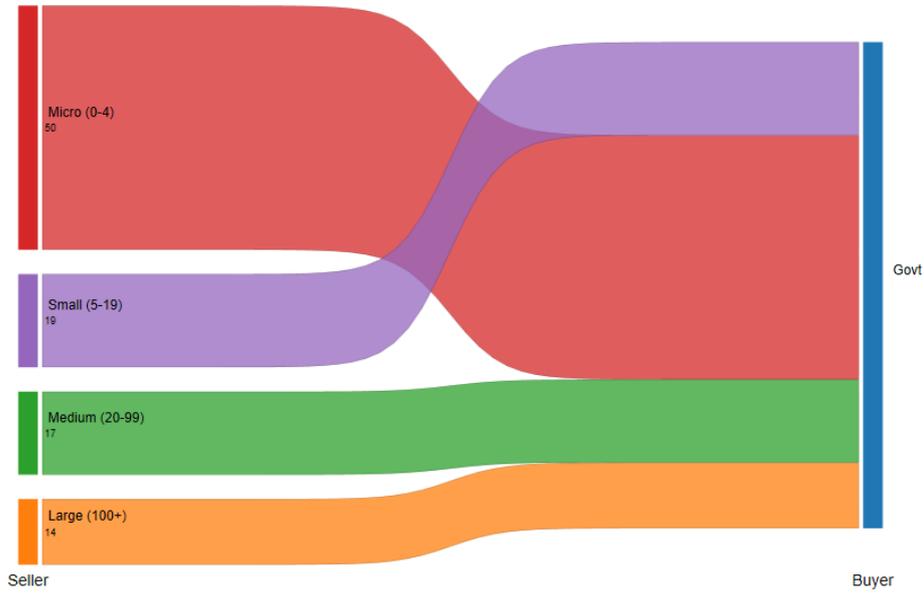
Figure 1: Mapping Trading Networks in Kenya



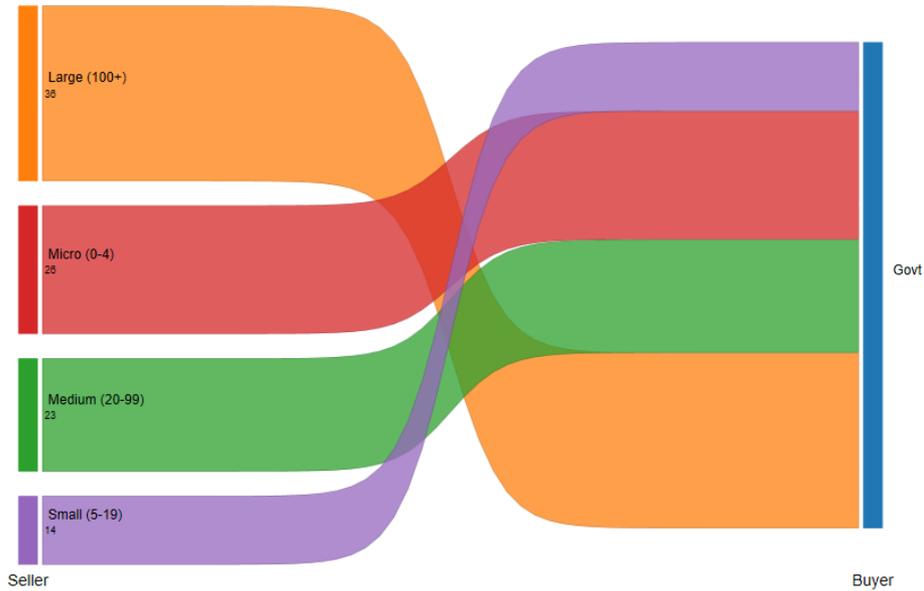
Notes: The top panel shows the share of the share of total value of trade across various firm types. For each of the sellers on the left side of the chart, the figure shows the share of its total sales that the firm types on the right side of the chart.

Figure 2: Distribution of Government Contracts Across Firm Size

a Share of firms

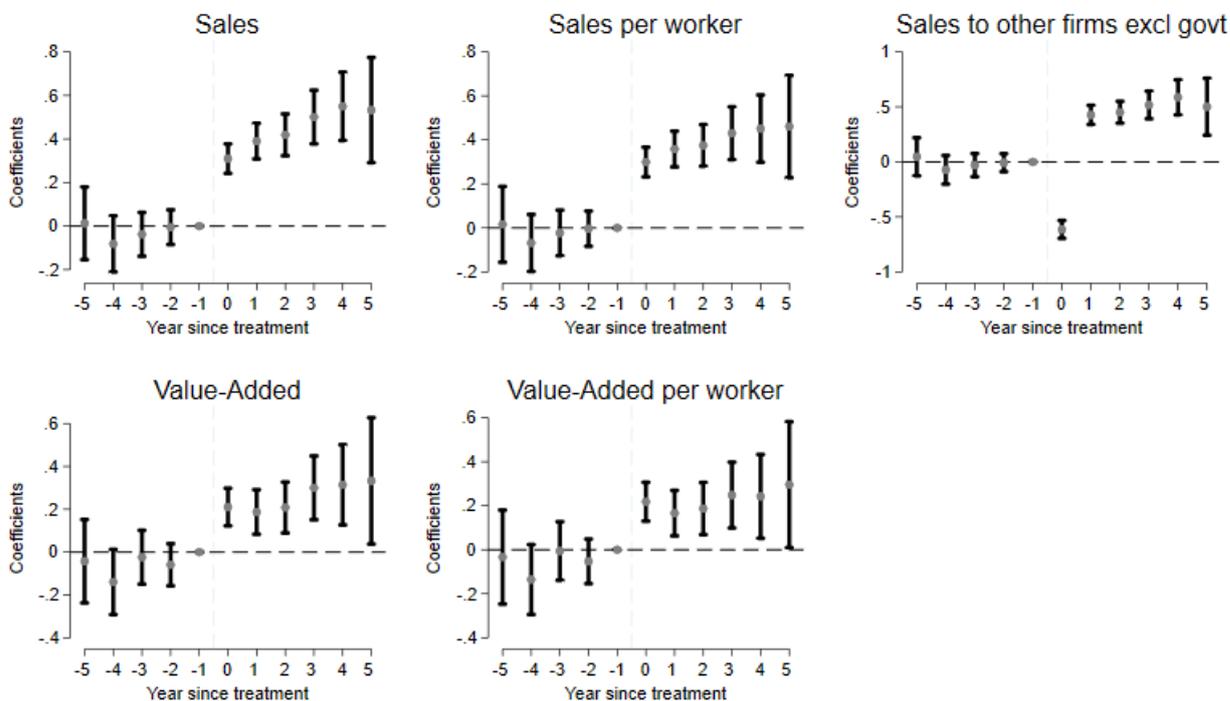


b Share of trade value



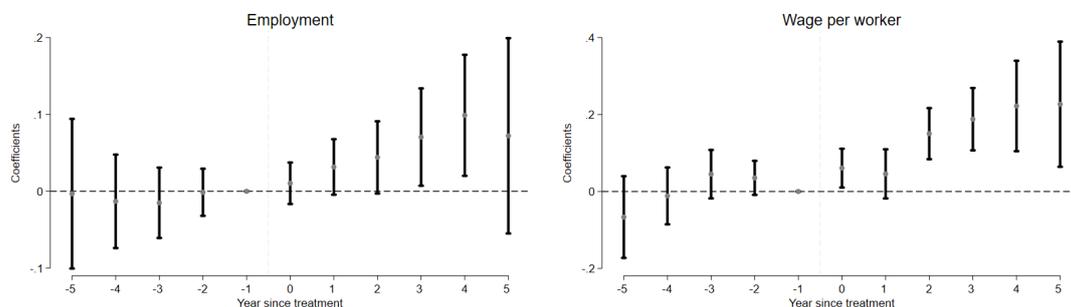
Notes: The top panel shows the share of firms by size category that sells to a government entity. The lower panel shows the share of the total value of sales by the respective firm size categories to a government entity.

Figure 5: Effects of Selling to a Government Entity on Firm Performance



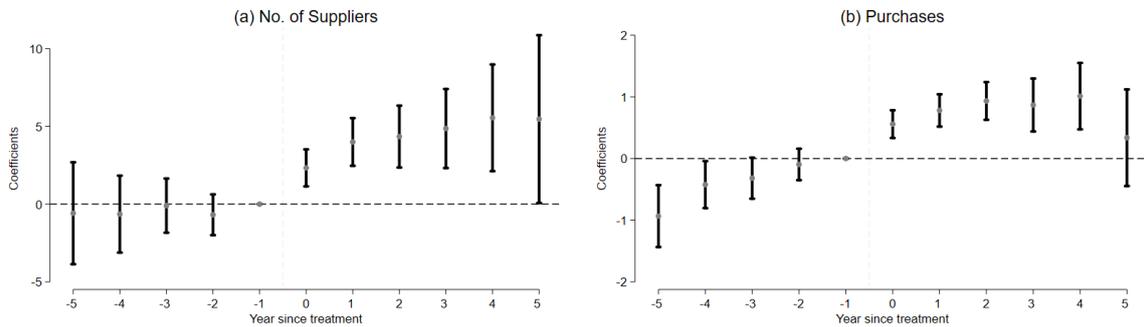
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level. All outcome variables are expressed in logs.

Figure 6: Effects of Government Contracts on Employment and Wages



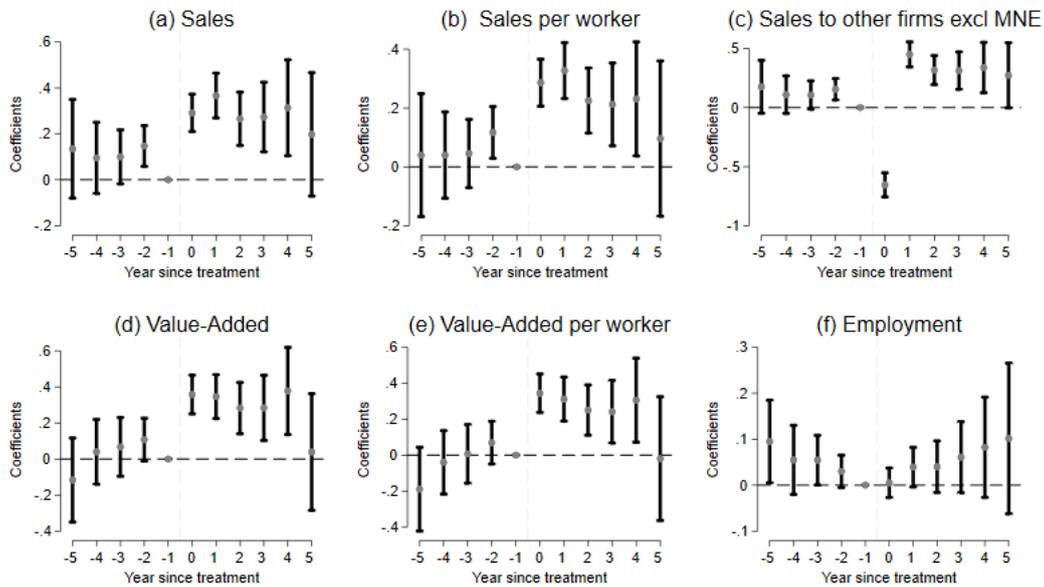
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level. All outcome variables are expressed in logs.

Figure 7: Upstream Impacts



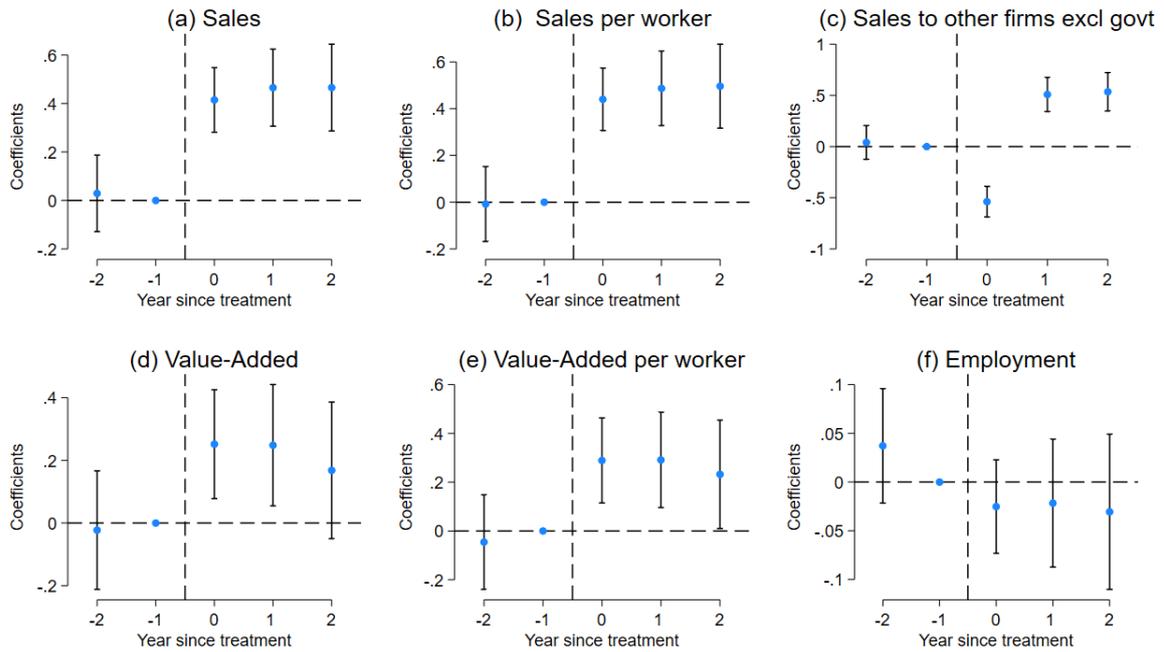
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. The outcome variable in the left panel is the count of the number of suppliers to the firms, while the outcome variable in the right panel is the log of total purchases from upstream suppliers.

Figure 8: Effects of Selling to a Multinational Entity on Firm Performance



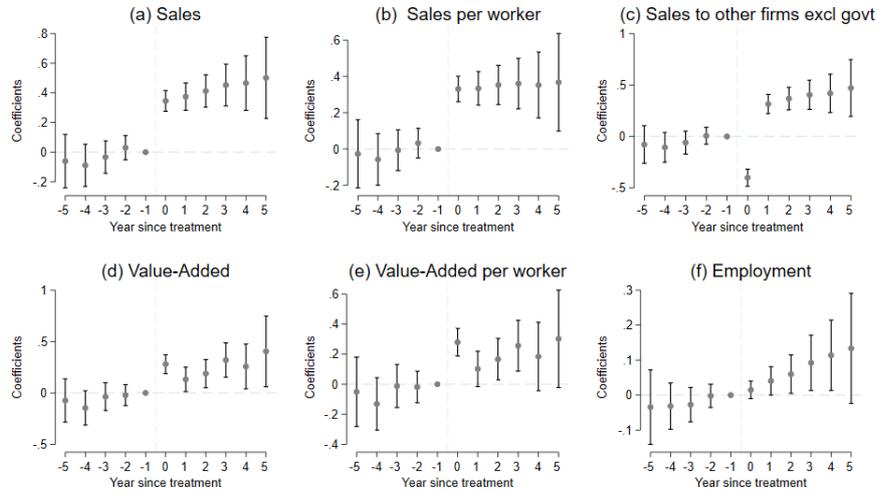
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a multinational entity while the control group consists of firms that do not sell to a multinational entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure 9: Effects of Selling to Government on Firm Performance: Stacked DID



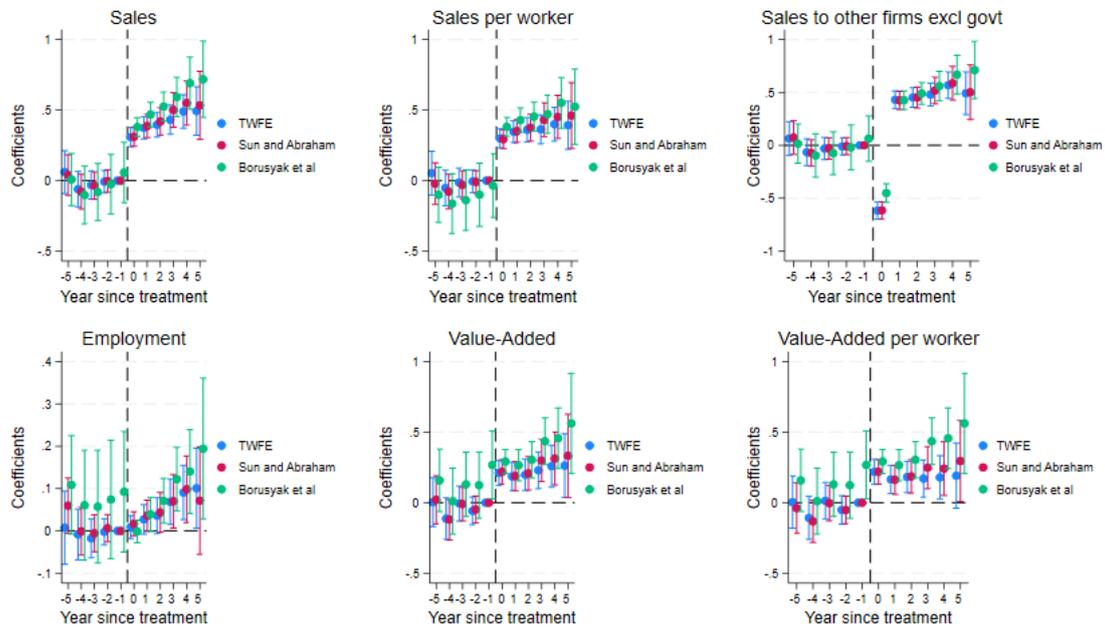
Notes: This figure presents event study estimates and their 95% confidence intervals obtained from alternative approaches of estimating DiD with staggered treatment. The horizontal axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure 10: Sensitivity Analysis: Matched Difference-in-Difference



Notes: This figure presents event study estimates and their 95% confidence intervals of the effects of becoming a government contractor on firm performance using a matched difference-in-difference design. A firm is considered a government contractor if the value of its sales to a government entity as a share of total sales exceeds a given threshold, with 15% as our baseline threshold. Control firms are obtained after performing a Coarsened Exact Matching (CEM) approach. The horizontal axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure 11: Effects of Selling to Government on Firm Performance: DID with Staggered Treatment



Notes: This figure presents event study estimates and their 95% confidence intervals obtained from alternative approaches of estimating DiD with staggered treatment. The horizontal axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure 12: Sensitivity Analysis: Threshold for defining Treatment



Notes: This figure presents event study estimates and their 95% confidence intervals of the effects of becoming a government contractor on firm performance. A firm is considered a government contractor if the value of its sales to a government entity as a share of total sales exceeds a given threshold, with 15% as our baseline threshold. Thus, the results show the estimated impacts at varying thresholds. The horizontal axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Tables

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Sales (log)	16.564	1.78	3.144	21.685	347923
Sales per worker (log)	15.485	1.56	-0.361	21.681	347923
Sales to other firms (log)	16.548	1.804	-2.258	21.685	347923
Employment (log)	1.08	1.367	0	12.758	347923
Value added (log)	15.733	1.984	-2.303	21.54	275223
Value added per worker (log)	14.577	1.687	-2.303	21.376	275223
No. of Suppliers	34.37	40.36	0	208	347923
Total value of purchases (log)	15.241	3.921	-2.303	20.258	347923
Government Contractor	0.014	0.118	0	1	347923

Table 2: Selling to Government and Access to Credit

	Credit Line (0/1)		Overdraft (0/1)		Collateral for Loan (0/1)		Bank credit %	Working Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gov't Contract (0/1)	0.2171*** (0.0389)	0.1765*** (0.0374)	0.0824** (0.0310)	0.0463 (0.0366)	0.0091 (0.0359)	0.0059 (0.0369)	0.0645** (0.0252)	0.0520* (0.0251)
Firm Ctrls	No	Yes	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MDV	0.3895	0.3891	0.3737	0.3755	0.7407	0.7391	0.1635	0.1626
R-squared	0.0871	0.1179	0.0751	0.1245	0.0975	0.1186	0.0536	0.0730
Observations	1715	1645	1726	1654	644	621	1707	1633

Notes: This table shows the relationship between being a government contractor and access to credit using data on Kenyan firms from the World Bank Enterprise Survey. Firm controls include firm age, gender of owners, and firm size. Sampling weights are applied. Standard errors are clustered at the city level.

* Significant at 10 percent level ** Significant at 5 percent level *** Significant at 1 percent level

Table 3: Selling to Government and Resilience to Shocks

	Sales	Sales/worker	Sales to other firms excl govt	Employment	Value-Added	Value-Added/worker	Wage/Worker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gov't Contractor X Covid	0.2609*** (0.0342)	0.1902*** (0.0342)	0.6572*** (0.0398)	0.1285*** (0.0413)	0.0641 (0.0421)	0.0707*** (0.0199)	0.1233*** (0.0276)
Firm Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County X linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7608	0.7070	0.7593	0.7577	0.6655	0.8925	0.8086
Observations	263625	263625	263625	211113	211113	263625	161507

Notes: This table shows the differences in firm performance between firms that sell to a government entity and comparable firms that do not sell to a government entity during the COVID-19 pandemic. The treatment group consists of firms that were selling to a government entity prior to the start of the pandemic. Firms that made their first sales to a government entity are excluded from the sample. Standard errors are clustered at the firm level.

* Significant at 10 percent level ** Significant at 5 percent level *** Significant at 1 percent level

Appendix

Public Procurement and Firms: Evidence from Kenya

A Appendix: Data

Data Cleaning

The primary dataset is the monthly Value-Added Tax (VAT) returns. These VAT returns provide not only information on the firms' total monthly sales and purchases but also feature two sections where firms can report detailed information on sales and purchase transactions with other businesses and VAT-registered entities. This data enables us to identify the supplier and buyer for each transaction between firms, the date of the transaction, a general product description, and the volume of the transaction. VAT is applicable to individuals and firms with a turnover of KES 5 million or more (\approx \$40,500 as of December 2022). Once a firm becomes VAT-registered and surpasses the KES 5 million threshold, it is obliged to continue filing VAT returns even in years when its turnover is lower. Sales to and purchases from non-registered parties (such as exempt entities, non-registered businesses, and final consumers) are documented as an aggregated monthly figure.

The full dataset consists of 211,913 unique firms, making 9.5 million transactions, with average annual sales of KES of 155.5 million over the period 2015-2022. In 2015, for example, 76,458 firms made up 616,933 sales transactions with average annual sales of KES 258.4 million (Table A.2). We can see that firms with sales below KES 5 million (below the threshold) were 117,068 (or 55.2 percent of the sample) but accounted for only 1.1 percent of average annual sales. Firms below the KES 5 million threshold, as well as those providing financial and educational services, and largely those involved in agricultural goods, pharmaceuticals, and passenger goods, are exempt from VAT. They are not required to register for VAT and do not need to submit monthly returns.

Most businesses that are not registered for VAT are essentially informal. According to [KNBS \(2016\)](#), there were 5.85 million unlicensed businesses active in Kenya in 2016, with 94% of these unlicensed entities reporting a monthly revenue of less than KES 50,000 (approximately \$400) ([Wiedemann et al., 2024](#)). These types do buy from registered VAT firms, but they never sell to VAT registered firms. In addition, some businesses register for VAT to be eligible for government contracts, but are generally not active. These firms add a lot of noise

to the VAT database. For our purpose, we study firms that sell or supply to downstream firm types that are owned by the government as well as if a firm is foreign owned or a large firm. We restricted our analysis to firms with annual purchases greater than zero and annual sales of KES 5 million or more in at least one year, which we observe in the data. We apply the threshold to exclude firms that registered for VAT to bid for tender but were not operational while keeping almost all buyers (Table A.3). Thus, the relevant firms (and linkages) remain in the final sample of interest for our study.

We expand the data to create an annual panel of supplier-buyer year tuple, creating a firm-to-firm transaction matrix that is sparse. Numerous zeros in the matrix capture where there is no domestic trade. To achieve this, we start from the cleaned monthly firm panel dataset and build a monthly supplier-buyer panel that captures the total monthly volume and the number of transactions between each buyer-supplier pair. The network panel data also allows us to identify the start and end dates of the trading relationship between firm pairs. Table A.4 provides information on the number of suppliers and their buyers over time. We show the mean, standard deviation (sd), and 10th-99th percentile of the distribution. We document significant heterogeneity in the number of buyers per supplier. Suppliers in the 99th percentile have very many buyers. Similarly, from the perspective of buyers, the number of suppliers per firm is heterogeneous and skewed to the right. Large buyers tend to have many suppliers (Table A.5).

Identification of government and foreign owned firms

Information on basic firm characteristics (e.g., 4-digit sector, the business subtype, start date of operations, and the location of headquarters) is obtained from tax registration forms, which is a self-declared information during taxpayer's registration. We use the variable business subtype to identify firms owned by the private sector, government, foreign, non-government, trusts, among others. Government-owned entities include schools, local authorities, ministries, and parastatal companies (i.e. state-owned enterprises). Table A.6 shows the number of buyers grouped into six main buyer ownership classifications (including the unclassified) for 2021. The table also shows the distribution of the number of suppliers per buyer within each class. About 18 percent of buyer firms are unclassified, constituting buyers with positive purchases from the network but with sales below the KES 5 million threshold. The number of buyers exceeds the number of suppliers in our cleaned transaction level data. This is not a concern because we are interested in the event that a

supplier (excluding government, foreign, and large entities) first establishes a serious sales relationship with a government entity. As in [Amiti et al. \(2024\)](#) we label a relationship to be serious, when a private or partnerships (henceforth private) firms' value of its sales to a government entity constitutes at least 15% percent of its annual total sales, an absorbing treatment event.

A.1 Appendix: Tables

Table A.1: Overview of administrative data

Name	Frequency	Unit of observation	Key indicators
Tax registration forms	static	firms	sector, location, start date
Value-added tax records	daily	transactions	trading partners, volume, product description
	monthly	firms	sales, purchases
Pay-As-You-Earn	monthly	firms	payroll, # employees

All data are collected by the Kenya Revenue Authority.

Table A.2: Comprehensive panel from VAT records

VAT threshold (KES mill)	Firms	Transactions	Average annual sales
Above	94,845	5.7mn	257,068,656
Below	117,068	3.8mn	1,731,482
Total	211,913	9.5mn	155,525,504
O/w in 2015	76,458	616,933	258,397,984

Notes: The table shows descriptive statistics about firms in the full dataset in relation to their eligibility for the VAT threshold of KES 5 million as required by the Kenya Revenue Authority.

Table A.3: Cleaned panel of VAT records

Year	# of firms	Frequency of transactions (sales)	Avg. Sales
2015	45,144	418,478	379,006,272
2016	53,014	546,836	311,792,512
2017	59,419	625,948	282,800,352
2018	66,381	689,289	265,645,616
2019	72,523	767,222	246,574,352
2020	77,948	817,401	237,401,184
2021	82,475	882,630	224,736,608
2022	82,091	892,135	222,707,408

This table shows the distribution of firms, transaction frequency and average annual sales over the period 2015-2022.

Table A.4: Distribution of Buyers over time

Year	Number of firms (suppliers)	Number of buyers per firm							
		Mean	Sd	10th	25th	50th	75th	90th	99th
2015	36000	33	149	1	2	6	21	68	443
2016	42007	37	171	1	2	6	22	75	498
2017	45609	36	170	1	2	6	21	73	503
2018	49444	37	171	1	2	6	21	75	517
2019	54051	40	182	1	2	6	22	77	570
2020	57004	38	180	1	2	6	21	73	548
2021	60207	39	181	1	2	6	21	74	571
2022	59291	41	186	1	2	6	22	77	587

This table shows the average number of suppliers and the distribution of buyer over time .

Table A.5: Distribution of Suppliers over time

Year	Number of firms	Number of suppliers per firm							
		Mean	Sd	10th	25th	50th	75th	90th	99th
2015	40196	30	47	1	4	15	37	74	211
2016	47981	32	50	1	5	16	41	81	224
2017	53198	31	48	1	5	15	39	78	220
2018	59512	31	48	1	4	15	39	78	213
2019	65535	33	53	2	4	15	40	82	238
2020	70497	31	49	2	4	15	38	78	224
2021	74404	32	50	2	5	15	39	80	228
2022	73864	33	50	2	5	15	40	82	232

This table shows the average number of buyers and the distribution of suppliers over time .

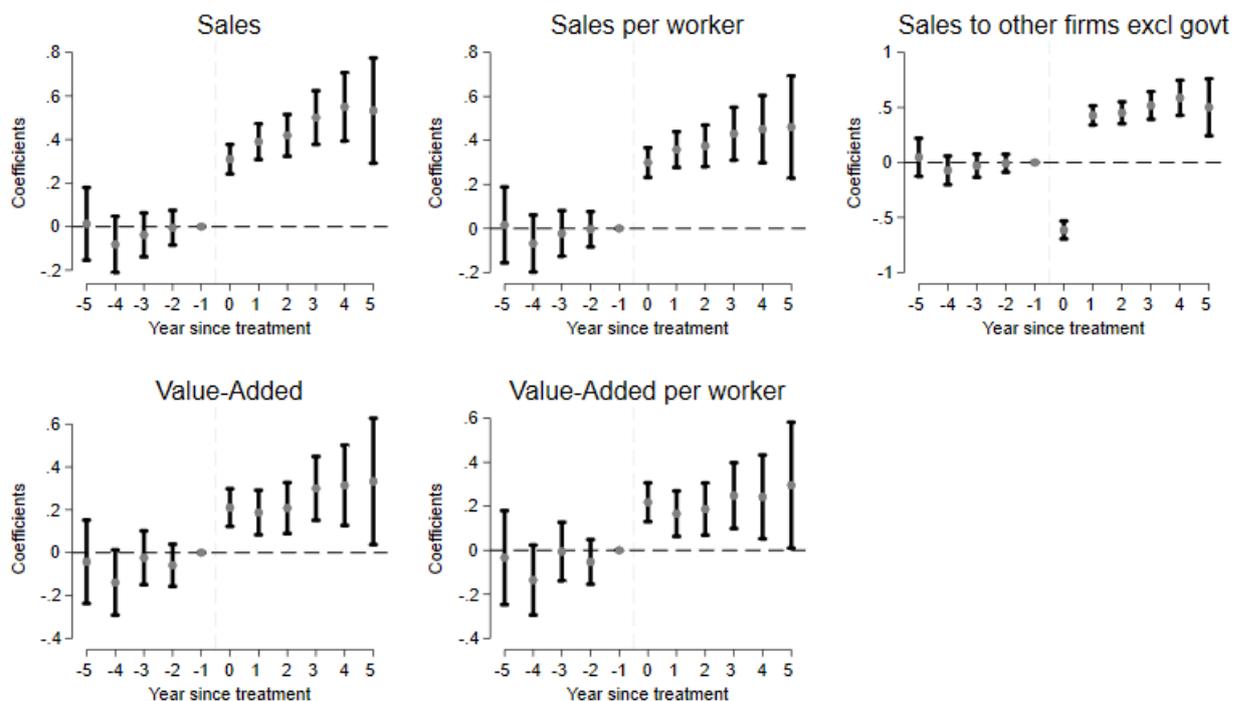
Table A.6: Distribution of Suppliers by buyer type in 2021

Buyer Type	Number of firms	Number of suppliers per firm					Average transaction(KES, mn)
		Mean	Sd	50th	90th	99th	
Government Entities	130	109	143	59	244	775	7.06
Foreign Entities	393	51	119	13	110	671	4.72
Private Entities	55713	34	53	16	88	244	2.35
Partnerships	3904	25	32	13	61	152	2.12
NGOs and Trusts	895	44	52	25	109	233	2.31
Unclassified	13369	21	26	12	52	118	2.16

Number of supplier relationships in 2021.

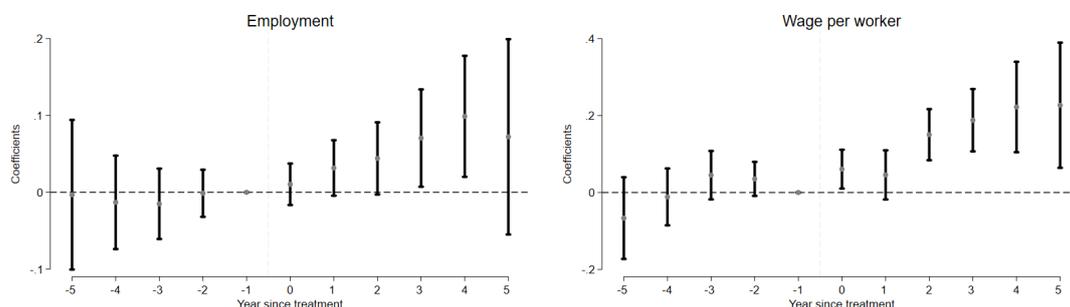
B Appendix: Figures

Figure B.1: Effects of Selling to a Government Entity on Firm Performance: Excluding county \times year Fixed Effects



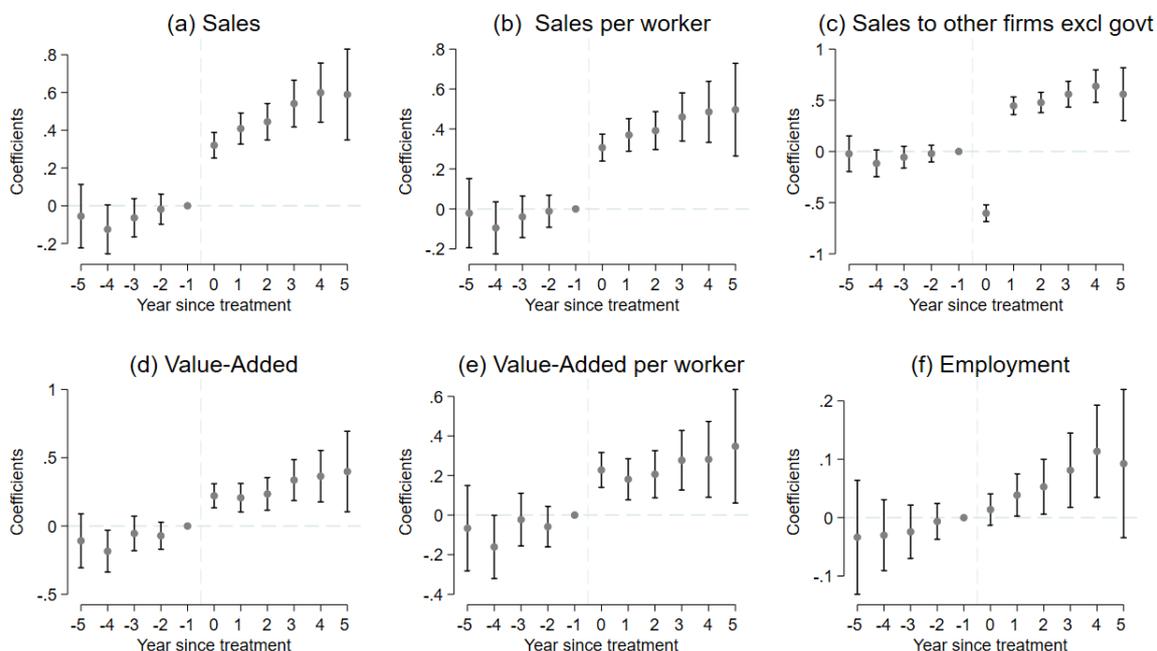
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. All outcome variables are expressed in logs.

Figure B.2: Effects of Government Contracts on Employment and Wages: Excluding county \times year Fixed Effects



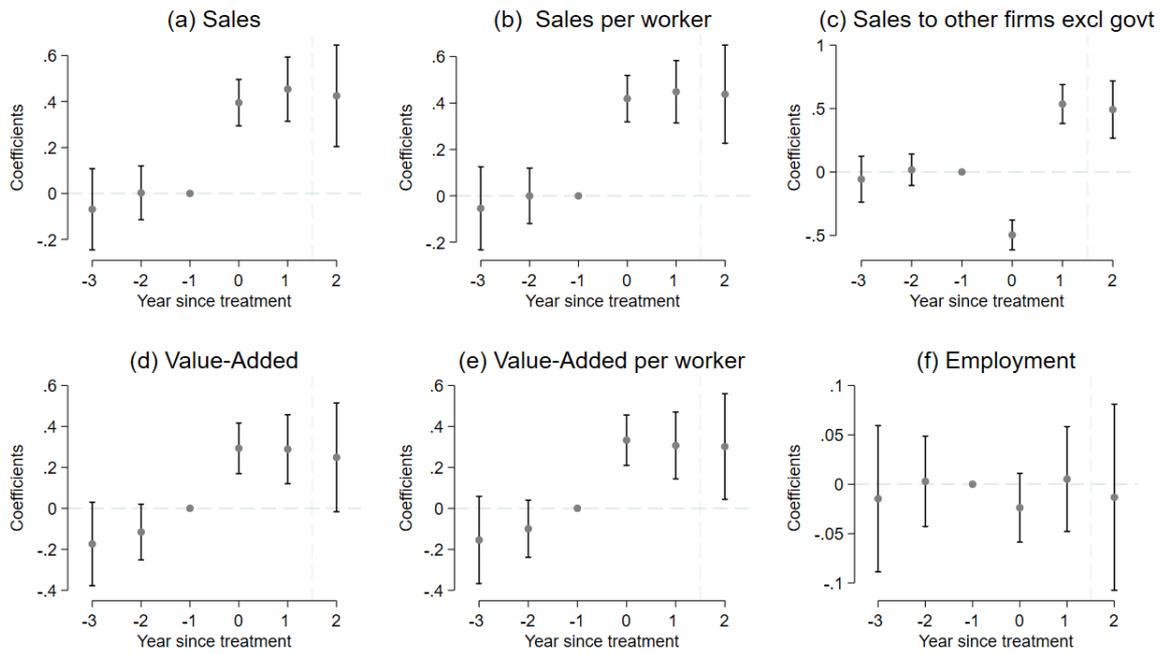
Notes: This figure presents event study estimates and their 95% confidence intervals. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level. All outcome variables are expressed in logs.

Figure B.3: Effects of Selling to a Government Entity on Firm Performance: Excluding Firms with Small Sales to Government



Notes: This figure presents event study estimates and their 95% confidence intervals. We exclude firms with minor sales to government agencies from the control. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure B.4: Effects of Selling to a Government Entity on Firm Performance: Excluding the COVID Period



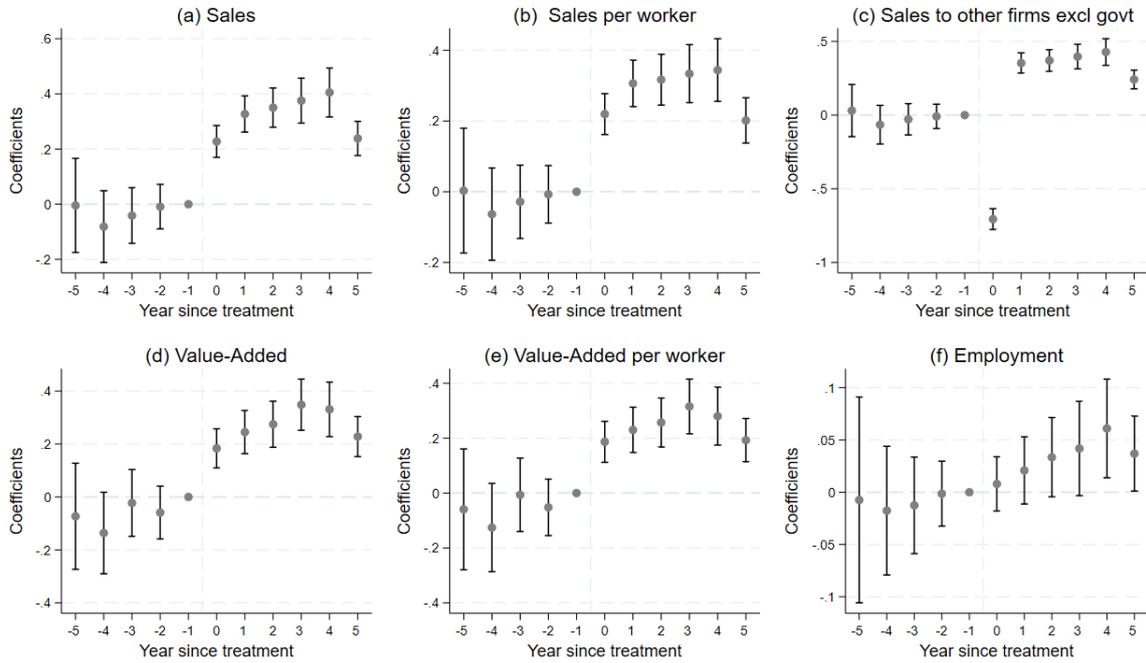
Notes: This figure presents event study estimates and their 95% confidence intervals, obtained from a regression using data from 2015-2019. The horizontal axis shows the year relative to the first time a firm begins selling to a government entity, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to a government entity. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

Figure B.5: Effects of Selling to a Large Domestic Firm on Firm Performance



Notes: This figure presents event study estimates and their 95% confidence intervals using alternative DID with staggered treatment estimators. The horizontal-axis shows the year relative to the first time a firm begins selling to a foreign multinational firm, while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a large domestic firm while the control group consists of firms that do not sell to a large domestic firm. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at firm level.

Figure B.6: Effects of Selling to a State-Owned Enterprise



Notes: This figure presents event study estimates and their 95% confidence intervals. We exclude firms with minor sales to government agencies from the control. The horizontal axis shows the year relative to the first time a firm begins selling to a state-owned enterprise (SOE), while the vertical axis shows the coefficients of the regressions with the outcome variables shown on top of the respective charts. The treatment group consists of firms that sell to a government entity while the control group consists of firms that do not sell to an SOE. All regressions include firm, industry (4-digits ISIC) \times year, county \times year and firm age fixed effects. Standard errors are clustered at the firm level.

C Appendix: Balance Tables

Table A.7: Balance Table after matching

Variable	Control	Treated	Diff	p-value
<i>Sector</i>				
Agriculture	0.014	0.015	0.001	0.836
Mining & Quarry	0.003	0.002	-0.001	0.680
Manufacturing	0.048	0.020	-0.028***	0.000
Construction	0.176	0.269	0.093***	0.000
Services	0.733	0.677	-0.055***	0.000
<i>Firm Size</i>				
Size bin=1	0.890	0.857	-0.033***	0.000
Size bin=1	0.083	0.110	0.027***	0.000
Size bin=3	0.025	0.029	0.004	0.246
Size bin=4	0.002	0.004	0.002*	0.050
Size bin=5	0.000	0.000	0.000	.
Firm start year	2011.900	2010.133	-1.767***	0.000