



## Regular article

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## ABSTRACT

Public procurement in developing countries accounts for approximately 13% of GDP, offering a significant market opportunity for domestic firms, especially those facing demand constraints. This paper explores the impact of selling to government entities on firm performance in Uganda, leveraging detailed administrative data and an event study methodology that corrects for firm self-selection and heterogeneity in the timing of the treatment. The results reveal that while firms increase their total sales after entering government contracts, they do not see improvements in productivity (measured by value added per worker). Moreover, sales to non-government customers drop sharply. This reallocation effect is persistent. This effect is not observed when firms start to sell to large private-sector buyers. Drawing on an original survey of firms engaged in public procurement, we highlight potential drivers of these dynamics, including capacity constraints and profitability differences between public and private sector sales.

## 1. Introduction

Government represents an important market for domestic firms in developing countries. In 2018, on average, government purchases of goods and services in low-income economies accounted for 13% of GDP (Bosio and Djankov, 2020). In many countries, public procurement is designed to pursue objectives in addition to obtaining value for money, such as distributional equity or industrial development.<sup>1</sup> More generally, as government expenditures on goods and services are an important component of GDP, public procurement will impact on economic activity (Cox et al., 2024). Firms in developing coun-

tries often find it difficult to increase and sustain growth because of limited demand for their products and constraints impeding product differentiation, technology adoption and the realization of scale economies (Woodruff, 2018; Goldberg and Reed, 2023; Verhoogen, 2023; Bassi et al., 2022). This is especially true in low-income settings, where capacity under-utilization is particularly high (Walker et al., 2024). Access to procurement contracts may help firms obtain external financing needed to expand supply capacity, improve quality and increase productivity.<sup>2</sup> Another channel for improving performance is that firms may benefit from learning by doing that leads to more efficient use of inputs or adopting new products/techniques in order

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<sup>1</sup> This may be reflected in price preferences or earmarking of certain types of contracts for domestic firms (“buy national” policies) (Day and Merkert, 2021).

<sup>2</sup> In the context of advanced economies, studies find that obtaining government contracts may facilitate access to credit, especially for enterprises most likely to be financially constrained — i.e. smaller firms (Hebous and Zimmermann, 2021; di Giovanni et al., 2022; Coviello et al., 2022).

to satisfy the terms of a government contract (Carrillo et al., 2023; Ferraz et al., 2015). Such benefits may have knock on effects on subsequent sales to the private sector.<sup>3</sup> Government demand can have drawbacks, too. Large demand shocks relative to firms' size can crowd-out sales to the private sector. This is especially true for (financially-constrained) smaller firms with limited production capacity that may have to decrease the amount of output sold to the private sector in order to deliver their government contracts (di Giovanni et al., 2022). Also, there might be differences in the prices paid by the government relative to private buyers that may introduce distortions in the market and that are usually related to features of the procurement process (Bandiera et al., 2009).

In this paper, we study the performance of Ugandan firms that sell to government entities, analyzing the implications for their growth, labor productivity and sales to and purchases from other firms in the economy. We also consider what happens when firms stop selling to the government.

In Uganda, public procurement is of above average importance, accounting for a large share of GDP (Colonnelli et al., 2024). The government has emphasized the role that public procurement can play in fostering private sector development (NPA, 2020; Behuria, 2021). This is reflected in procurement regulations that aim to increase the share of projects allocated to domestic companies, fostering their participation in procurement (PPDA, 2018b,a, 2019).<sup>4</sup>

We exploit administrative micro data provided by the Uganda Revenue Authority on firm-to-firm transactions from value added tax (VAT) declarations. These are matched with employment level information from the pay as you earn (PAYE) dataset, allowing us to assess different measures of firm performance. VAT declarations pertain to all of a firm's buyers and suppliers, including sales to (purchases by) government bodies. This permits us to identify whether and when a firm sells goods or services to a government entity and the value of any such transaction. This feature of the data marks an important difference between our analysis and studies that evaluate the effect of participation in public procurement using quasi-experimental settings based on the design of procurement auctions (e.g. Ferraz et al., 2015; Carrillo et al., 2023). Unlike these studies, we can precisely track the period when firms report sales to government entities and exploit differences in the relationship over time with the government as a buyer. Some firms sell to the government for most of the sample period, while others start or cease to sell to the government, allowing us to identify what happens when they "enter" and "exit".<sup>5</sup> In addition, we cover firms that may not be captured by public procurement auction data, e.g., smaller purchases awarded at the discretion of purchasing authorities. These are likely to be especially relevant to SMEs.

Our baseline specification links indicators of firms' performance to either the value of their sales to the government or to a dummy variable

that switches to one in the year in which they start selling to a public entity. To address potential endogeneity due, for instance, to possible self-selection by firms, we exploit information for a sub-sample of firms that start providing goods or services to the government after 2012. Given the differences between firms that sell to government entities and firms that do not, we construct the control group based only on not yet treated firms (i.e. firms that will start selling to the government in subsequent years). This reduces concerns about potential self-selection of some types of firms into government contracts, allowing for more precise identification of the effect of starting (or stopping) to sell to the public sector. As the distribution of the treatment timing is heterogeneous across firms over time, we exploit recent advances in the two-way fixed effects literature that address potential biases in event study coefficient estimates stemming from the presence of negative weights under parallel trends and no anticipation effect assumptions (De Chaisemartin and D'Haultfoeuille, 2022; Roth et al., 2023). More specifically, we use the doubly robust estimator proposed by Callaway and Sant'Anna (2021) for settings with multiple periods and multiple treatment cohorts (hereafter, CS estimator). The CS estimator combines the outcome regression approach and inverse probability weighting (IPW) using pre-treatment characteristics to either condition parallel trends or weight the control group by the probability of being treated.

We show that selling to government entities leads to increases in total sales but we do not observe an increase in value added per employee. For treated firms, government contracts do not simply add a new revenue stream that expands sales relative to what was realized before. Firms that start selling to the government reduce sales to other buyers both within the period in which sales to the government commence and thereafter. Transaction-level information reveals that this crowding-out of private sales is associated with a shift in the composition of existing buyers, with an increase in their number and a drop in the average size of each transaction. When firms stop selling to government buyers and do not start again we observe a symmetric relationship: total sales drop as firms are unable to fully replace lost procurement contracts with sales to private buyers. This pattern is specific to transactions involving the government. When replicating the analysis using the first contract with either a (i) large domestic buyer or (ii) foreign-owned firm, we find evidence of total sales increasing without an accompanying drop in sales to other private buyers.

Qualitative evidence obtained from an original survey of 236 Ugandan firms with experience with public procurement suggest two potential mechanisms that may drive our main findings: (1) capacity-cum-credit constraints, and (2) differences in the profitability of sales to public vs. private buyers. We draw on the survey findings to undertake additional data analysis. The survey confirms the prevalence of capacity constraints in many firms that may hamper the possibility of expanding production after obtaining government contracts. We document that firms in services sectors, which rely less on fixed assets and thus may have less need to finance investment in plant and equipment in order to expand production, experience a smaller crowding out effect, consistent with potential (short-term) capacity constraints.<sup>6</sup> However, a significant share (40% of surveyed firms) expanded production capacity and most surveyed firms did not regard access to finance as a significant factor inhibiting growth, suggesting capacity-cum-credit constraints do not uniquely explain our results. A majority of surveyed firms report that relative to private buyers, government entities pay higher prices and that this motivates reductions in sales to other firms. Suggestive evidence for this hypothesis is obtained both by evidence on firms reporting higher profits after selling to the Government, and by using customs clearance (trade transactions) data for firms active in distribution activities (wholesale and retail trade). Firms in this sector rely

<sup>3</sup> Lee (2022) finds that Korean firms selling to the Government increase their sales to the private sector because of a reputational effect associated with success in the public procurement market.

<sup>4</sup> The National Development Plans (NDP I, II and III) make specific reference to the importance of local content provisions in public procurement. The potential to increase the contribution of domestic factors of production in large-scale projects in the construction and energy sectors is stressed, but also emphasized for smaller-scale projects involving the supply of goods or services in the health, education and defence sectors (PPDA, 2019). The Buy Uganda Build Uganda (BUBU) policy, launched in 2014, aims to increase the value of procurement that goes to local firms (with a specific focus on micro and small firms, which account for some 90% of all firms).

<sup>5</sup> The richness of the bilateral VAT transaction data permits us to identify four different patterns: (1) the majority of firms (43%) reports positive sales to a government entity throughout the sample period (*Regular* suppliers); a quarter of the treated firms start and stop selling at some point during the period covered by the data (*Irregular* suppliers); (3) one fifth keep selling to the government for all periods after the first transaction (*Stayers*); and (4) about 10% of the firms stopped selling to the government on a definitive basis during the period covered by our sample (*Exiters*).

<sup>6</sup> di Giovanni et al. (2022) also discuss the reallocation of supply by firms. In their model, this is driven by short-run capital constraints which are resolved by procurement contracts acting as collateral.

extensively on imports for their sales. Trade data allow decomposing their imports into quantity and unit prices. Using information on the share of imports in total sales, we show that for this subsample of firms sales to the government are associated with higher prices.

**Related research.** Our paper speaks to the literature on barriers to firm growth in developing countries. Most extant research has focused on the supply-side (see Woodruff, 2018, for a review). Our contribution is on demand-side constraints (Goldberg and Reed, 2023) and the potential role of sales to the government in alleviating such constraints. Egger et al. (2022) document an increase in sales realized by firms following a positive demand shock (driven by a large, one time cash transfer to local households) without changes in firm investment behavior, i.e., increased sales do not reflect a relaxation of credit constraints that previously limited investment.

Much of the work on the role of government demand is on the design of public procurement processes to attain value for money and public policy objectives (e.g. Bosio et al., 2022) by reducing information barriers and increasing participation (Colonnelli et al., 2024; Hjort et al., 2024). Less attention has been given to the role participation in procurement as such can play as an instrument to promote private sector development. The evidence from developing countries is generally consistent with what we find, i.e. a positive effect of procurement on several dimensions of firms' performance (Ferraz et al., 2015; Hoekman and Sanfilippo, 2020; Fadic, 2020; Lee, 2022; Mensah et al., 2026). Most existing studies in the development literature focus on middle-income countries. We focus on a low-income setting where firms are more likely to exhibit slack (Walker et al., 2024), helping to improve understanding of the nuanced effects of demand shocks associated with winning government contracts. An important dimension we emphasize in this paper is that the relationship with total sales may not be transitory, a result in line with what Ferraz et al. (2015) and Hjort et al. (2024) find for Brazilian and Liberian firms, respectively.

To our knowledge, the observed reallocation of supply by firms that start to sell to government entities has not been a focus of analysis in the literature on the firm-level effects of participation in public procurement. This reallocation effect is a central feature of the seminal paper by Baldwin and Richardson (1972), who focus on the aggregate (economy-wide) impact of public procurement policies that provide preferential treatment for certain types of firms. They argue that in perfectly competitive markets where government demand is small relative to total market demand, directing procurement to preferred suppliers will have no effect on prices and simply serve to reallocate demand between public and private sector. As government demand is relatively large for many sectors in Uganda, procurement policy may have effects on prices.

Our paper provides firm level evidence of the reallocation effect. This marks a difference with the findings by Ferraz et al. (2015) and Carrillo et al. (2023), who find no evidence for sales to private sector being displaced by sales to the government in their samples of Brazilian, Ecuadorean, and Kenyan firms.<sup>7</sup> (di Giovanni et al., 2022) find a short-run crowding out effect of public procurement on private sector sales in Spain, attributing this to financial constraints that limit production capacity. A similar pattern is documented by Mensah et al. (2026) in Kenya: firms entering substantial sales relationships with government entities experience a contemporaneous crowding out of private-sector sales, followed by a subsequent rebound and growth.

<sup>7</sup> Ferraz et al. (2015) do not find sales to private sector are displaced by sales to the Government. Rather, they find the two to be complementary. A difference with our setting is that they report that Government contracts are fulfilled in less than 1 year, whereas the average exposure of Ugandan firms to transactions with Government entities in our sample is around 3 years. Carrillo et al. (2023) find no evidence of short-term declines in sales to other buyers for a sample of Ecuadorian firms winning procurement contracts in the construction sector.

Unlike previous research, a key, distinctive feature of our paper is that we distinguish between firms based on how frequently they sell to the government and whether they exit from government contractual relationships. We find that obtaining government contracts leads to a persistent decline in sales to the private sector, that is temporarily compensated by an overall increase in the value of total sales. Our survey results suggests that this decline stems partly from Ugandan firms' preference for selling to the government over serving private-sector buyers.

Our findings parallel those of Alfaro-Urena et al. (2022) for Costa Rican firms that start selling to foreign multinationals, which is accompanied with a drop in sales to other buyers. They attribute the decrease to short-term capacity constraints that increase marginal costs (a similar mechanism to that proposed by Almunia et al., 2021), making prices less attractive to private buyers. The majority of firms in our survey report that the government pays higher prices and that this is mostly relevant for firms with high fixed costs. This survey result is consistent with (Alfaro-Urena et al., 2022)'s model. However, in contrast to their conclusion that the decline in private sector sales is transitory, the Ugandan firms in our sample do not observe a recovery in sales to private buyers in subsequent years. This might imply that firms in Uganda cannot invest in new fixed assets, consistent with evidence from a survey of small firms in Uganda by Bassi et al. (2022), or that they do not have sufficient incentives to do so.

The remainder of the paper proceeds as follows. Section 2 presents the data and descriptive statistics on the extent to which firms in Uganda sell to government entities. Section 3 introduces the empirical framework used for analysis and descriptive evidence on the relationship between selling to government entities and firm performance. Section 4 presents our main results, while Section 5 reports robustness checks. An analysis of potential mechanisms, based on administrative data and a survey of Ugandan firms that participate in procurement, is presented in Section 6. Section 7 concludes.

## 2. Data

The transactions data used in this paper come from administrative records compiled by the Uganda Revenue Authority (URA) for 2012–2019. We use masked (i.e. pseudo-anonymized) records from the Ugandan VAT registry as the main source of data.<sup>8</sup> The VAT was introduced in Uganda in 1996, with a basic 18% rate and a set of exemptions. In a country with a tax-to-GDP ratio of just 14%, four percentage points below the Sub-Saharan Africa average,<sup>9</sup> the VAT accounts for over 30% of national tax revenues (Almunia et al., 2024; Brockmeyer et al., 2024). As of 2012, all registered firms above a certain size threshold were required to provide VAT declarations on a monthly basis using an electronic form.<sup>10</sup> The data therefore provide a representative snapshot of all formal bilateral transactions between registered firms in the country.<sup>11</sup>

Important given the scope of this work, registered firms are those entitled to supply the Government. Participation in standard competitive bidding typically requires registration as a provider with the PPDA and the electronic government procurement (e-GP) system, with local firms required to provide their Tax Identification Number (TIN) to validate their eligibility and complete their supplier registration on the system.<sup>12</sup> While PPDA provide rules for different procurement

<sup>8</sup> Each record is a precise VAT return event by a given registered entity, and might involve several distinct invoices and partner entities at once.

<sup>9</sup> See the UNU-Wider Government Revenue Dataset, accessed in July 2025.

<sup>10</sup> This threshold was in the order of UGX 50 million (some USD 13,700) up to 2015, and UGX 150 million thereafter (Almunia et al., 2017).

<sup>11</sup> If compared to GDP at market prices produced by the formal sector, as reported by the Uganda Bureau of Statistics (accessed on January 12, 2026), the share of sales reported by firms in our sample is, on average, 61%.

<sup>12</sup> See the e-GP supplier registration guide, accessed on January 12, 2026.

methods, depending on estimated contract values and circumstances, the standard competitive process requires formal documentation and compliance with regulatory requirements. We therefore expect firms covered by the tax registry to account for the vast majority of total public procurement in Uganda.<sup>13</sup>

We build our firm level dataset starting from the Schedule 1 of the VAT declaration form. Schedule 1 consists of a bilateral firm-to-firm transaction level dataset reporting all invoices registered by a firm while filing for the VAT return, and involving other registered firms as partners.<sup>14</sup>

We aggregate data at the level of each reporting firm and by year. In total, we count 18,110 reporting firms, for a total of 73,778 firm-year observations over the period considered. Many firms report sporadically prior to 2012 and coverage increases in more recent years. Over 76% of the firms report information for less than 6 years, slightly more than half for less than 3 years. This translates into an unbalanced panel in which firms are included based on whether they present a VAT declaration in a given year. A potential concern is that firms reporting behavior is influenced by their relationship with the Government. To verify if this kind of attrition is relevant in our sample, we run a specification in which a firm's likelihood of being in the data is related to a binary variable that equals one if the firm sell to the government. Results reported in Table L-1 in Appendix L show evidence of a small positive increase in the probability of reporting that disappears once we control for firm and year fixed effects simultaneously.

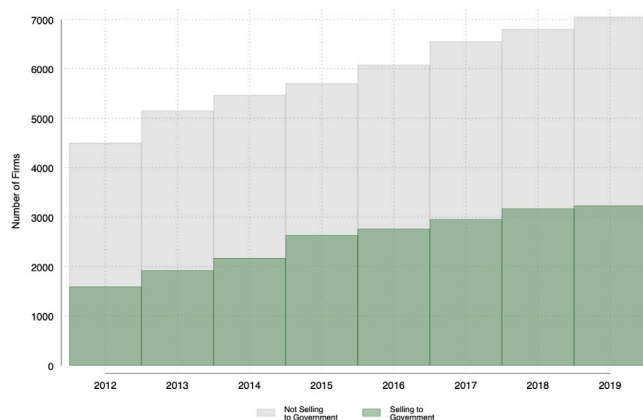
After aggregating, we match the VAT data to the registry of taxpayers to obtain basic information on firm characteristics such as the year of incorporation, sector of activity (at the 4 digit level of the ISIC rev. 4 classification), and type of organization.<sup>15</sup> The data distinguish two categories of public bodies, "Government bodies and government-funded project" and "Local Authority". We use this information to identify the year a firm reports sales to an entity classified in one of these categories. While many firms sell to a government entity at some point in the sample period, transactions involving the government as a buyer represent only 4.3% of the total number of VAT transactions recorded over the period considered. The average value of a transaction involving a government entity as a buyer is UGX 100 million (approximately US\$ 27,000), about 3 times larger than the average firm-to-firm transaction (UGX 36 million or US\$ 9700).

We integrate the records from the VAT declarations with employment information from the Pay-As-You-Earn (PAYE) dataset. Matching PAYE with VAT data reduces the size of the sample as the concordance between the two datasets is imperfect. Out of the 18,110 unique firms, only 7936 firms have a matching record in the PAYE database. This

<sup>13</sup> Uganda's PPDA also provides for simplified or micro procurement methods for low-value purchases (below USD 1000 USD or USD 2000 for works) where formal bids are not solicited through the standard tender process. In principle it is therefore possible for informal firms to apply. However, such transactions are structurally distinct from the competitive procurements that generate comprehensive contract award data. Calculations using [publicly available data on contracts reported by the PPDA](#) indicate that these contracts account for less than 2.4% of total procurement value in recent years.

<sup>14</sup> Overall, the data include 6,496,014 transactions over the period 2012–2019. Of these, 6,279,938 can be assigned to one of the registered firms listed in the tax registry (i.e. an average about 320 transactions per firm). Table A-3 in the Appendix report detailed information on the number of transactions per firm and firms' pair. Note that these data are based on monthly reports, i.e. before we aggregate them up at the firm-year level.

<sup>15</sup> Not all firms in the VAT data match with an entry in the tax registry. More significant is a large number of unmatched entries on the declared partner side. In total, there are 15,150 matched reporting firms in the VAT data (accounting for 83.4% of the aggregated sample from Schedule 1 declarations). This discrepancy is likely due to the registry covering firms established up to 2018 (so that younger ones cannot be matched), as well as the possibility that some tax return forms report the wrong partner or reporter tax identification numbers.



**Fig. 1.** Government suppliers over time.

*Notes:* The figure plots the distribution of firms selling and not selling to the government over the period of interest in the full sample.

*Source:* Authors elaboration on VAT data.

results in 21,749 firm-year pairs. Appendix Tables B-3 and B-4 report the summary statistics for the main outcomes of interest and the distribution of firm-year observations by type of suppliers showing the samples resulting from the different steps of the merge.

### 2.1. Descriptive evidence on firms selling to the government

About 35% of the firms report at least one transaction with the government over the sample period, with an increase in their number over time (Fig. 1). Appendix Table A-1 provides descriptive statistics on the sectoral distribution of firms with a match in the VAT registry and the share of firms reporting at least one transaction with a government entity.

Compared to other firms, those selling to the government (i) are more likely to be concentrated in distribution services, construction, manufacturing and high value-added services activities (ICT and Technical Professions) (Table A-1); (ii) tend to be larger in terms of sales, a feature that is particularly evident for firms that keep on selling to the government from the time they become a supplier (Table A-2); (iii) have more partners to which they sell and from which they source (Table A-4); and (iv) are older (Figure A-1).

An important feature of the VAT data is that it is possible to track the different patterns of firm interactions with the government entities. Aside from firms that never sell to the government, we can distinguish between four groups of firms:

1. *Regular suppliers:* 43.7% of firms report sales to government bodies throughout the sample period;
2. *Stayers:* 20.2% of firms record a first sale to a government entity after they appear in the sample and continue to report transactions thereafter;
3. *Irregular suppliers:* 25.1% of firms record a transaction with a public body after they first appear in the sample but subsequently engages irregularly with the government during the remainder of the sample period<sup>16</sup>;
4. *Exiters:* 11.1% of firms sell to a government entity the first time they appear in the sample, but stop doing so at some point subsequently.

<sup>16</sup> For instance, a firm reports its first sale to the government in 2013, another in 2016, stops in 2017 and starts again in 2018.

**Table 1**  
Summary statistics and group comparisons on selected outcomes.

	Never suppliers		Government suppliers		Mean Diff (G2-G1)	t-stat
	Mean	SD	Mean	SD		
Total sales	1782.65	(18 997.64)	5125.38	(44 509.23)	3342.73***	12.32
Sales Excluding Govt.	1782.65	(18 997.64)	4281.07	(41 972.21)	2498.42***	9.69
Buyers Excluding Govt.	9.55	(23.47)	37.60	(145.61)	28.05***	33.59
Average Sales excl. Govt.	360.42	(3585.07)	540.58	(18 721.40)	180.16*	1.67
Sales per employee	420.94	(1961.67)	408.14	(1733.22)	-12.81	-0.59
Value added per employee	-172.63	(6736.87)	-22.39	(1366.86)	150.23**	2.57

Notes: Standard deviations in parentheses. All outcomes except for *Buyers Excluding Govt.* are expressed in millions UGX. Mean Diff shows the difference between firms that sell to the government at least once over the period of interest and firms that never sell to the government - i.e. the “never treated”. The negative mean values of value added per employee for never-suppliers and government suppliers are driven by outlier firms. The median values for the two groups are 1.38 million UGX and 7.26 million UGX, respectively. The *t*-statistic tests whether the mean difference is significantly different from zero. Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

Descriptive evidence on the main differences between firms that sell to the government at least once as compared to those that never sell to the government over the period of interest is reported in Table 1. Overall, firms that at some point sell to government entities at least once have larger sales volume, a bigger sales network (as proxied by the average number of buyers excluding government), have larger average gross profits and appear to be relatively more productive than firms that never sell to the government. Additional descriptive statistics on the variables of interest for the whole sample are reported in Appendix Tables B-1 and B-2.

### 3. Empirical analysis

#### 3.1. Regression analysis

We begin by examining the relationship between selling to the government and firm performance in a standard two-Way fixed effects (TWFE) setting:

$$Y_{it} = \beta_1 Gov_{it} + \lambda_i + \theta_{st} + \varepsilon_{it} \tag{1}$$

where  $Y_{it}$  is the outcome of interest for firm  $i$  in year  $t$ . Specifically, we look at sales to different registered partners using the full sample from the bilateral VAT data. We also look at the domestic network of firms, i.e. the number of buyers and the average size of transactions with private domestic buyers. For the sample of firms that can be matched with PAYE data, we develop measures of labor productivity (sales and value-added per employee).<sup>17</sup>

In this specification, the explanatory variable,  $Gov_{it}$ , measures either the (log) value of sales to government entities by firm  $i$  at time  $t$  or a dummy, taking the value 1 when a firm starts selling to the government and zero otherwise.  $\lambda_i$ , and  $\theta_{st}$  are firm fixed effects and sector (at the 4 digit ISIC classification) time trends, respectively. Firm fixed effects account for firm-specific unobserved factors, while the addition of sector-specific fixed effects accounts for industry-specific factors that may influence firm performance, such as sector-specific trade or industrial policies. Standard errors are clustered at the firm level. Estimates based on Eq. (1) are reported in Appendix Table C-1 for both the discrete definition of the treatment (selling to the government) and a continuous definition of the treatment (value of firm sales to the government). Results are consistent across the two treatment specifications. Selling to government entities is associated with (i) higher levels of total sales and (ii) lower levels of sales to non-government buyers. The latter finding provides some suggestive evidence that sales to government entities come at the expense of sales to other buyers, i.e., involve some reallocation of capacity instead of being additional. We also observe changes in the composition of the group of private buyers after a firm obtains a government contract.

<sup>17</sup> The value-added measure is computed by subtracting purchases from registered partners and imports from sales to registered partners and exports.

While the firm expands its network of private sector buyers, the average size of transactions with private sector buyers reduces. Selling to the government is associated with an improvement in labor productivity, defined as sales per worker, and in value-added per worker. This preliminary set of results are robust to (a) using a sub-sample of firms that is the same across all specifications (Appendix Table C-2); and (b) a sub-sample of firms with a balanced panel (Appendix Table C-3).

#### 3.2. Event study analysis

While including a broad set of fixed effects helps to account for omitted variables, both reverse causation due to self-selection of certain types of firms and measurement error are potential sources of bias that we cannot account for. The results from Eq. (1) therefore cannot be interpreted in a causal manner given that firms that sell to government entities are unlikely to have performed similarly to firms that do not, absent the treatment. Table 2 uses the four-fold categorization discussed previously to compare the characteristics of treated firms before the first transaction with the government with firms that never sell to the government. The differences are substantial for the variables included.<sup>18</sup> There are also relevant differences among firms that sell to the government. Firms in the group of *Stayers* are larger and have higher turnover and assets than those in the irregular supplier group.

In light of this we implement an event study approach to examine if the trajectory of the estimated effect on firm performance changes discretely in response to the first sale to a government buyer.<sup>19</sup> The event study design also enables us to identify if there is a persistent effect of receiving government contracts on firm performance. To rule out any pre-trends in the performance across firms that begin selling to the government and those that never do so, as appears to be the case (Table 2 and Appendix Table A-5), we restrict the control group to firms that will sell to the government in the future, that is, the not-yet-treated firms (i.e. firms that will eventually start selling to the government over our period of interest).

Even in the absence of pre-trends, estimates could be contaminated by the treatment effects from other periods (see Roth et al., 2023; De Chaisemartin and D’Haultfoeuille, 2022). As the treatment is heterogeneous over time across different units, the effects we find may not provide the correct weighted average of treatment effects across units (in our case, firms) and time. This is because, with heterogeneous treatment, we end up comparing treated firms with not yet treated ones, which is correct, and with already-treated firms, which is not.

<sup>18</sup> Appendix Table A-5 shows that firms selling to the government tend to have fewer buyers and are more profitable relative to firms that never sell to the government, even before they start selling to the government (Panel b vs. Panel a). Table A-6 reports similar statistics for suppliers using the pattern identified in Section 2.

<sup>19</sup> In the recent FDI literature, event study designs have been used to study the effect of becoming a supplier to multinational corporations in Costa Rica and Belgium (Alfaro-Urena et al., 2022; Amity et al., 2024).

**Table 2**  
Group difference (Before first sale to the government).

	Total sales (UGX mln)	Total assets (UGX mln)	Number of employees	Number of buyers	Number of suppliers
Stayers	5076.85	9083.93	41.50	16.19	18.87
Irregular sellers	1959.96	3438.35	35.30	16.89	18.50
Stayers or Irregular sellers	3563.68	6702.27	38.41	16.53	18.69
Never sellers	1099.40	1966.54	21.96	6.89	10.25
Stayers vs. Never	0	0	.00007	0	0
Irregulars vs. Never	.09232	.08046	.00703	0	0

Notes: Mean values for selected outcomes across groups, defined by the type of economic relationship over the period of interest. Row 4 and 5 report the p-value from a two-tailed Wald test for mean difference across groups for stayers vs. never sellers and intermittent vs. never sellers respectively. Stayers consist of firms that switched to selling to the government at some point during the sample period and continued doing so. Irregular sellers comprise firms that sold periodically to the government since they switched to selling to the government in our sample.

The introduction of the latter type of comparison results in negative weights for some of the coefficients estimated.

We use the doubly robust estimator proposed by Sant’Anna and Zhao (2020) as generalized by Callaway and Sant’Anna (2021) to a setting with multiple periods and multiple treatment cohorts. The CS estimator combines the outcome regression approach and the inverse probability weighting (IPW) using pre-treatment characteristics to either condition parallel trends or weight the control group by the probability of being treated.

More formally, the outcome of interest in the CS estimator is the average treatment effect on the treated (ATT) for each cohort of treated units  $g$  at time  $t$

$$ATT(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(NT) \mid G_i = g, X_i] \tag{2}$$

where  $ATT(g, t)$  is the average treatment effect on the treated for group  $g$  at time  $t$ ;  $Y_{i,t}(g)$  is the potential outcome for unit  $i$  at time  $t$  if first treated at time  $g$ ;  $Y_{i,t}(NT)$  is the potential outcome if not treated;  $G_i = g$  indicates unit  $i$  belongs to cohort  $g$  (first treated at time  $g$ ); and  $X_i$  represents the vector of covariates.

Callaway and Sant’Anna (2021) define the doubly-robust estimator as

$$\hat{\mathbb{E}}[ATT(g, t)] = \frac{1}{n_g} \sum_{i: G_i=g} [\omega_{i,g,t}(X_i) \times (Y_{i,t} - \hat{\mu}_t(X_i))] \tag{3}$$

with  $\omega_{i,g,t}(X_i)$  being the inverse probability weights based on the set of covariates  $X_i$ ;  $\hat{\mu}_t(X_i)$  is the outcome regression estimate for the control group; and  $n_g$  is the number of units in each treatment cohort  $g$  respectively.

Put in an event study-like form, the CS estimator can be re-written as

$$Y_{i,t} = \sum_{e=-3}^3 \theta_e \times D_{i,t}^e + X_i' \gamma + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{4}$$

where  $D_{i,t}^e = \mathbf{1}$  if firm  $i$  is exactly  $e$  periods since first treatment at time  $t$ , and  $\theta_e$  represents the dynamic treatment effect at event time  $e$  (in our empirical application we include 3 pre- and 3 post-treatment periods). Since every cohort-specific ATT is already computed as a change from its own pre-treatment baseline, the pooled estimate is automatically centered on zero. No further normalization is required, so we do not have to omit a reference period (e.g.  $t - 1$ ) as in standard TWFE event-study regressions. Last,  $\alpha_i$  and  $\lambda_t$  correspond to firm- and time-fixed effects respectively.

In this event-study consistent form, the term  $\theta_e = \sum_g w_g \times ATT(g, g + e)$  is the weighted average of each cohort-specific ATT.<sup>20</sup> In this way, the CS estimator ensures that  $\theta_e$  represents a properly weighted average

<sup>20</sup> The subscript  $e$  is a time marker relative to the event time for each cohort (periods since treatment), and  $w_g$  are the aggregation weights that are distributed so that more numerous cohorts “weigh” more on the aggregated dynamic ATT.

of treatment effects. The term  $X_i$  include a set of pre-treatment characteristics which we use to improve the matching between treated and not-yet treated units: the sector of the firm, the level of exports and imports registered by the firm, and the size of the economic network of a firm as proxied by the total number of buyers and suppliers.<sup>21</sup> Each dot in the resulting event-study figure is the point estimate of  $\theta_e$ , i.e the weighted average, across treatment cohorts, of the change in the outcome between calendar period  $t$  and the last period before that cohort’s treatment.

The Sections that follow report the main findings of the event study analysis, by outcomes of interest. To ease the interpretation of the findings, our baseline estimations always refer to the group of firms that start selling to the government at some point, and do not stop during the sample period (i.e. the *Stayers*). We adopt this approach for several reasons. The first is that, together the *Regular* suppliers, selling continuously to the government is the most frequently observed pattern in the data, although the time limitation going forward does not allow to know whether this is true for more recently treated firms. Second, this pattern is more consistent with the definition of the treatment as an absorbing state, which is common in settings similar to ours (e.g. Alfaro-Urena et al., 2022), and makes the results comparable to other papers studying the effects of winning procurement contracts (e.g. Ferraz et al., 2015). Focusing on *Stayers* also facilitates comparison of results for *Exiters*. Since exit points to a reversal of some of the post-treatment trends, excluding *Irregular* suppliers provides a clearer understanding of how the relationship with the government shapes firms’ performance. The pattern of the *Irregular* suppliers is more erratic, with a majority of such firms selling more than once, and for about 3 years each time (see Appendix Table B-5). We report results using a sample that includes both *Stayers* and the *Irregular* suppliers in the baseline estimation and uses the not yet treated as a control in Appendix D.

## 4. Results

### 4.1. Sales

Fig. 2 plots the estimates of the evolution in firms total sales (left-hand panel) and sales excluding government contracts (right-hand panel) around the first year in which they start selling to government entities. We find no significant differences in the evolution of the outcomes across the treated and control groups before the treatment,<sup>22</sup> and a sharp increase in sales relative to the control group in the year of treatment. The difference in total sales persists over the period for which we can observe treated firms.

<sup>21</sup> We exclude the pre-treatment proxies of the economic network when we evaluate the effect of treatment on the buyers’ network.

<sup>22</sup> Numerical estimates for the Average Treatment on Treated (ATT) for all groups across all periods are reported in Appendix Section E, alongside the cohort size in each exercise proposed in this and the next sections.

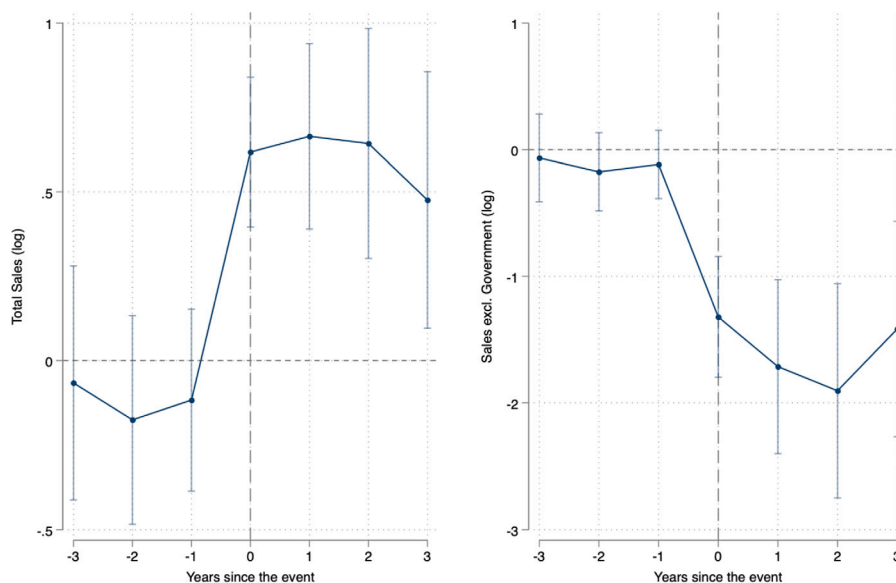


Fig. 2. Sales — Stayers only.

Notes: The event study compares firms that start selling to the government and continue doing so afterward to firms that will sell continuously to the government in the later years. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. Sales outcomes are reported in  $\log s + 0.01$ .

The point estimate of 0.64 at event-time 0 implies an increase in total sales of about 90 percent  $((e^{0.64} - 1) \times 100)$  relative to the counterfactual level absent treatment. Using the mean pre-treatment sales of treated firms (UGX 3564 million, or roughly USD 0.98 million), this corresponds to an absolute increase of UGX 3195 million (about USD 0.88 million) in the first treatment year. In contrast, sales excluding government decline substantially at treatment. The point estimate of  $-1.26$  at event time 0 implies a reduction of about 72%  $((e^{-1.26} - 1) \times 100)$ , which corresponds to a loss of about 2554 million UGX (USD 0.70 million), leaving non-government sales at approximately UGX 1010 million (USD 0.28 million). The loss of sales to non-government buyers is not simply a short-term effect; sales do not begin to recover until three years after firms start selling to government entities.

We can exploit the granularity of the transaction level data to unpack sales to the private sector into (i) the total number of buyers and (ii) the average size of transactions. Results for private sector sales are plotted in Fig. 3. After the first transaction with the government, there are visible changes within the network: the number of private buyers increases (left-side panel), but the average size of transactions falls (right-side panel). Although the decline in the average size of transactions is partly mechanical, it also reflects a change in the composition of the firm’s private buyers. Additional analysis presented in Appendix Figure G-1 shows a systematic reallocation across buyer types. Specifically, firms experience a persistent decline in the number and share of pre-treatment private buyers classified as large, defined as private firms whose average purchase from a seller in a given year exceeds the seller’s median buyer-year purchase size across all years, following treatment. While firms do add new large private buyers following treatment, this extensive margin expansion is insufficient to offset the contraction among pre-existing relationships with large private buyers. As a result, the overall share of large private buyers among a firm’s customers declines following treatment. These compositional changes suggest that firms drop big private buyers to create slack to cater to the government. The findings are consistent with the survey, suggesting this pattern reflects the greater attractiveness of selling to the government (we discuss this further in Section 6).

4.2. Productivity

As in our initial descriptive analysis (Appendix Table C-1), labor productivity, measured as sales per employee, increases for firms that

sell to government entities (Fig. 4, left-hand panel). However, we no longer find a corresponding increase in value-added per employee (Fig. 4, right-hand panel).<sup>23</sup> This might reflect an increase in total sales and revenues without a corresponding increase in capacity or efficiency.<sup>24</sup>

If productivity growth is driven by efficiency gains, we should observe a corresponding improvement in wages. When we replicate our event study-based analysis using compensation per employee as an outcome, we do not find evidence of a significant improvement. In Section 6, we explore potential channels that could underlie this finding, including capacity expansion constraints and price differences between contracts for the public sector and those with private buyers.

4.3. Losing government contracts

Fig. 5 reports the results for all the outcomes of interest in a setting in which a firm is considered treated when it stops selling to the government and does not obtain a contract again in the future (the “Exiters”). In this context, the treatment group includes firms in our sample period that commence selling to a government entity but that subsequently stop doing so at some point, while the control group includes firms that will stop selling to the government in future.

Results largely mirror our main findings in terms of the immediate effect, although the effect for total sales and sales to the private sector is transitory. Thus, in the year when firms exit, their total sales decline by approximately 300 million UGX, computed as  $(e^{-0.5} - 1) \times 763.26$  million UGX. This temporary decline in overall sales following the loss of government demand coincides with a sharp but short-lived increase in sales to private firms. Specifically, private sales rise by roughly UGX 2420 million UGX in the treatment year, computed as  $(e^{1.65} - 1) \times 574.96$  million UGX. However, this spike is transitory and does not persist

<sup>23</sup> Numerical estimates of the average ATT for all groups across all periods are reported in Appendix Section E.

<sup>24</sup> Note that the sample is substantially reduced compared to the one available when focusing on sales. To exclude the possibility that the lack of a significant effect on VA per employee is driven by sample selection, we replicate the event study for total sales and sales excluding the government (Fig. 2) on the sub-sample of firms for which we can match data from the VAT and PAYE. The resulting event study plot is reported in Appendix Figure D-1 and it remains consistent with our main finding.

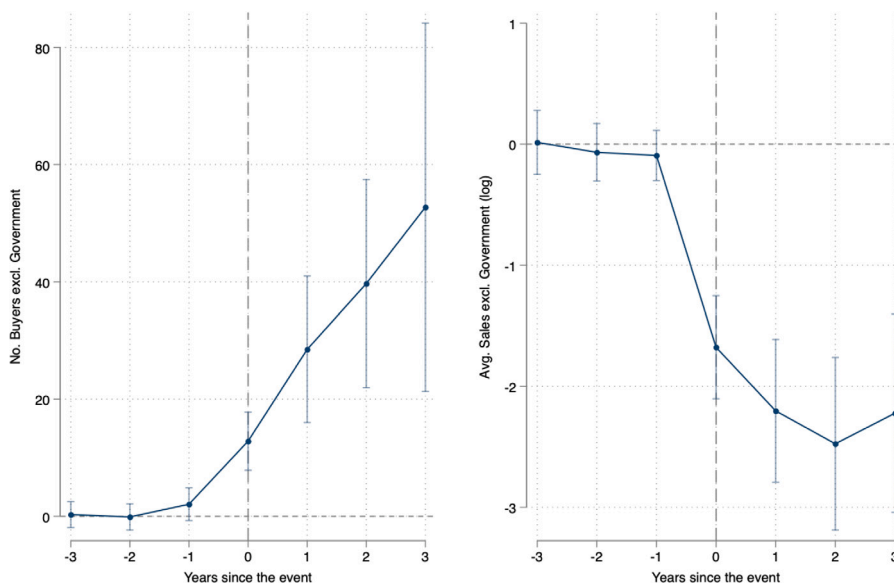


Fig. 3. Linkages.

Notes: The event study compares firms that start selling to the government and continue doing so afterward to firms that will sell continuously to the government in the later years. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. The number of non-government buyers is reported in absolute values while the average size of the transaction with non-government buyers is reported in  $\log s + 0.01$ .

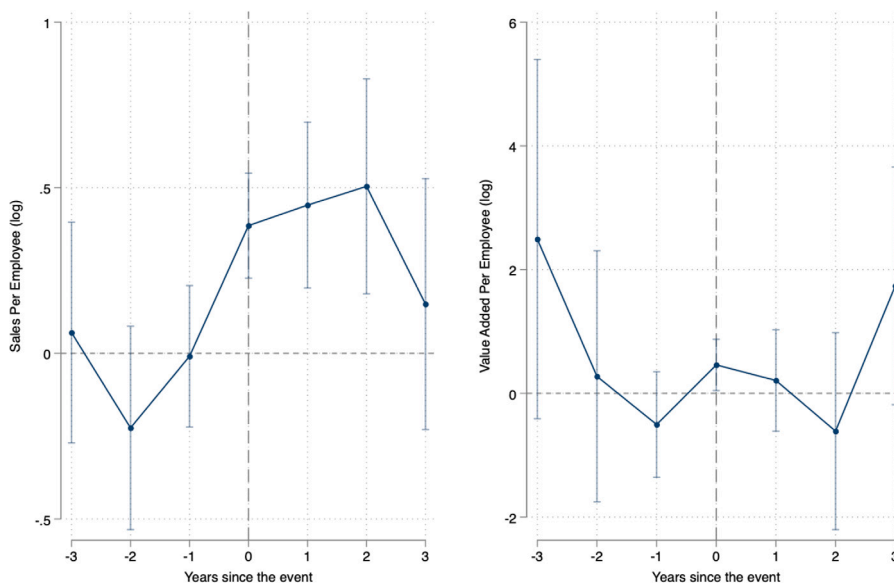


Fig. 4. Productivity.

Notes: The event study compares firms that start selling to the government and continue doing so afterward to firms that will sell continuously to the government in the later years. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. Both sales and value-added per employee are reported in  $\log s + 0.01$ .

beyond the immediate treatment period. Firms also adjust by reducing the number of private sector interactions, but as in the case of sales, this is transitory. Firms losing government demand increase the size of transactions with private buyers, but the effect, while sizable, is short-lived. Exit also leads to a temporary decline in productivity measured by sales per employee, with no corresponding effect on value added per employee.

#### 4.4. Selling to other large buyers

An important question in interpreting our findings is to understand whether they are specific to selling to the government or generalize to instances involving large non-government buyers. To assess this, we

replicate the analysis by adopting two alternative definitions of the treatment, based on either (i) the first transaction with a large domestic non-government buyer, defined as one whose average income over the sample period was in the top 10th percentile of the distribution (using the CIT records); or (ii) a foreign buyer based in Uganda. For consistency with previous analysis, the treatment group only includes firms that record a sale to a large (or a foreign) buyer sometime after first appearing in the sample and continue recording sales thereafter, while the control group is restricted to “not yet treated”. Results are reported in Fig. 6. The drop in sales to other private partners is unique to instances in which the government is a buyer. No such relationship is observed for firms that begin selling to large domestic private buyers or to foreign firms. This finding is not due to large domestic firms and

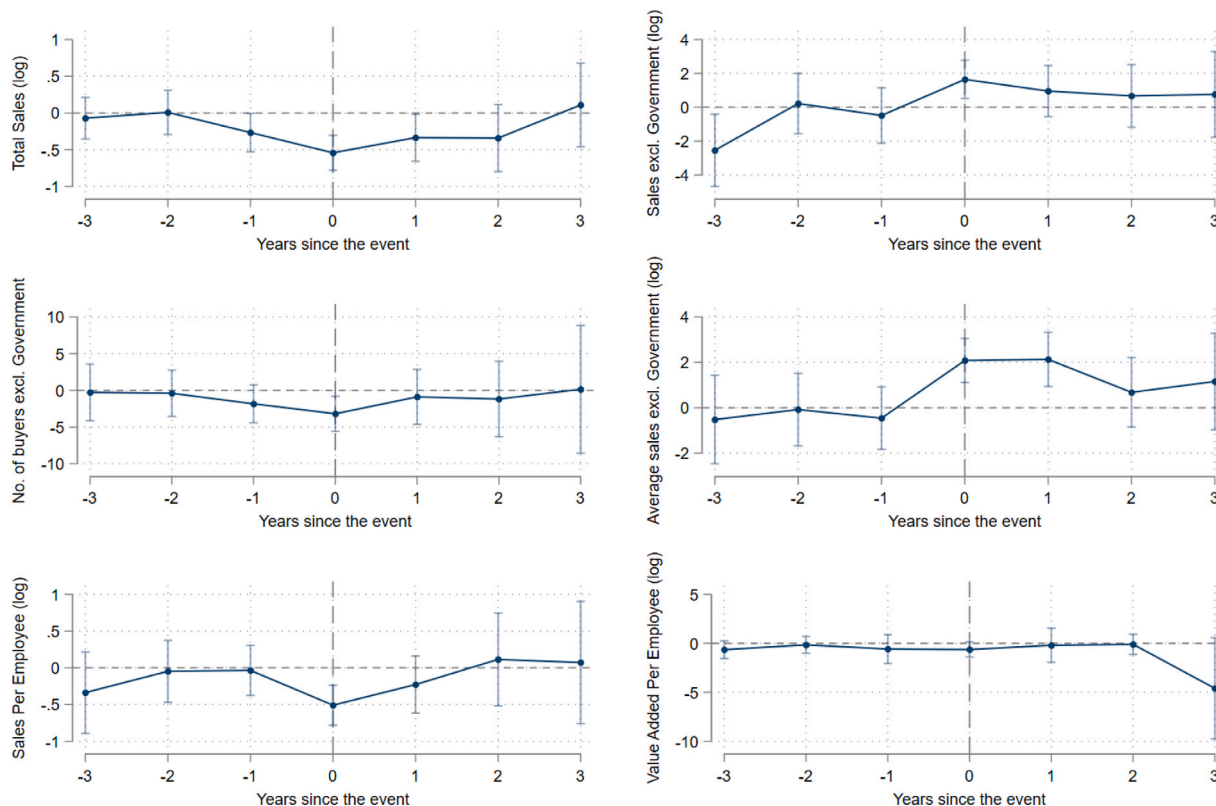


Fig. 5. Exit.

Notes: The event study compares firms that stopped selling to the government to firms that will do so in future. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. Total sales, sales excl. govt., average sales to the private sector, sales per employee and value-added per employee are reported in  $\log s + 0.01$ . The number of non-govt. buyers is reported in absolute value.

foreign firms being less relevant buyers than the government entities. In related estimation samples, average sales to the government (9% of total sales) are not significantly greater than the average sales to large domestic firms (10% of total sales) or the average sales to foreign firms (5% of total sales).

### 5. Robustness checks

**Alternative estimators.** We check the consistency of our findings using an alternative event study estimator based on (De Chaisemartin and d’Haultfoeuille, 2020). Their estimator is a weighted average of the DiD estimands that compare the evolution of the mean outcome in two groups: those switching from no treatment to treatment between  $t-1$  and  $t$ , and those remaining untreated. The estimator assumes that the treatment is an absorbing state, that is, there are no groups that switch from treatment to no treatment. A limitation of the (De Chaisemartin and d’Haultfoeuille, 2020) DiD estimator is that it does not allow matching the treated and control units based on pre-treatment characteristics. To better compare the results to our original approach, we also replicate the event study estimates by using the Callaway and Sant’Anna (2021) estimator, but without using the pre-treatment covariates for matching. The event study plots for both estimators are reported in Appendix Figures F-1 and F-2. The two plots visually confirm the main findings presented in Section 4.1, suggesting that our results do not depend on the choice of the estimator.

**Sample.** Fewer firms report both VAT and PAYE data than those reporting VAT only (see Appendix Table B-3). Thus, the sample used to estimate the role of Government contracts on sales and linkages (which are based on bilateral VAT data) is different from the sample used to estimate the effect on productivity (which are based on VAT and PAYE data). The difference in the sample composition might affect the results,

as some of the firms that must register for the VAT are not obliged to report on the other forms.<sup>25</sup> Hence, we replicate our event study analysis on the sub-sample of firms that report both VAT and PAYE data. The sub-sample analyses presented in Appendix Figures D-1 to D-3 are consistent with our baseline event study results.

**Alternative definitions of the treatment and control groups.** In Appendix D we report two sets of results in which the sample changes as follows. In Figures D-4-D-6 we consider the *Stayers* and *Regular* suppliers together, with the control group made up of not yet treated firms. Figures D-7-D-9 augment the previous exercise by adding the never treated firms to the control group. These exercises are useful in understanding whether the choices we made about the sample of the treatment and control groups influence the findings. The evidence suggests this is not the case, though with some nuances. Including the group of *Irregular* suppliers yields patterns broadly consistent with our main findings, with the exception that total sales decline in subsequent periods, a result that aligns with the patterns observed when firms stop selling to the government in Section 4.3.

Results based on a larger control group (Figures D-7–D-9) broadly reflect the direction of the estimates in our main specification. However, most event-study graphs exhibit evidence of pre-trends, which is not surprising given the substantial differences observed between treated firms (at  $t - 1$ ) and never-treated firms (see Table 2).

**Misreporting.** The reallocation in firms’ sales that we observe in our analysis might be due to a change in their reporting behavior when they start selling to the government. For instance, firms might expect

<sup>25</sup> VAT reporting applies to most legal forms of firms, while PAYE applies to employees whose monthly pay exceeds a certain threshold.

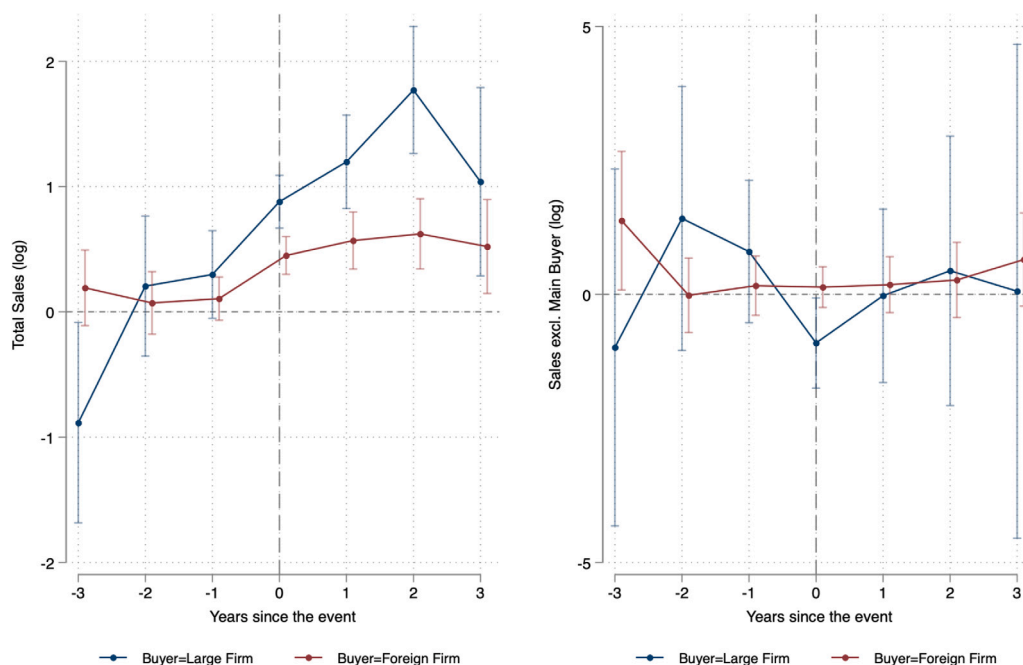


Fig. 6. Large private buyers.

Notes: The event study compares the change in outcomes due to selling to the gov. against two alternative treatments: (1) Selling to large domestic buyers and (2) Selling to foreign firms. We exclude large domestic and foreign firms when computing the number of non-govt. buyers when we estimate the effect of selling to large domestic firms or foreign firms, respectively. The (Callaway and Sant’Anna, 2021) correction is applied to account for heterogeneity in treatment timing. Both Total sales and Sales to private buyers are reported in  $\log s + 0.01$ . See Figure H-1 in the Appendix for the event study plot for firms average sales, number of buyers (incl. and excl. the government), and sales/value added per employee.

greater scrutiny of their VAT disclosures, and therefore begin reporting their sales more precisely. This might explain a persistent increase in overall sales when they start selling to the government, even though it cannot explain a corresponding decline in sales to the private sector. Alternatively, even if firms do not expect more scrutiny, they may want to limit their tax payments in expectation of higher revenues and therefore may decide to under report their sales to the private sector as they begin selling to the government. There is evidence that a large share of registered firms in Uganda misreport their sales and purchases to reduce tax payments (Almunia et al., 2024).

To check whether our results are affected by misreporting, we use the methodology suggested by Almunia et al. (2024). Specifically, we construct a proxy measure of misreporting, which is the difference between seller  $i$ 's reported sales ( $S$ ) to domestic buyer  $j$  in year  $t$  and buyer  $j$ 's reported purchases ( $P$ ) from seller  $i$  in the same year, treating non-reporting by either side as a zero. We then take the average of the discrepancies across all buyers to construct a seller misreporting measure in a given year:

$$d_{ijt} = (S_{ijt}) - (P_{jit}), \tag{5}$$

$$\bar{d}_{it} = \frac{1}{J_{it}} \sum_{j=1}^{J_{it}} d_{ijt} \tag{6}$$

Approximately three-quarters of discrepancies arise because one side does not report any transaction in a year. Notably, around one-third of these non-reporting cases are driven by buyers that never report any purchase throughout the sample period. Such buyers are plausibly below the VAT registration threshold, suggesting that a substantial share of discrepancies may reflect tax exemptions rather than misreporting behavior. Consistent with this interpretation, Mascagni et al. (2023) discuss discrepancies driven by VAT-exempt buyers in Rwanda and restrict their analysis to buyers that report a VAT declaration at least once.

We therefore present event-study estimates of discrepancies around firms’ starting to sell to the government both for the full sample and

after excluding seller–buyer–year observations involving buyers that never report any purchase during the sample period (Fig. 7). Across both specifications, we find no statistically significant change in the discrepancy measure following treatment. If anything, patterns are even less clear once excluding potentially VAT-exempt buyers.

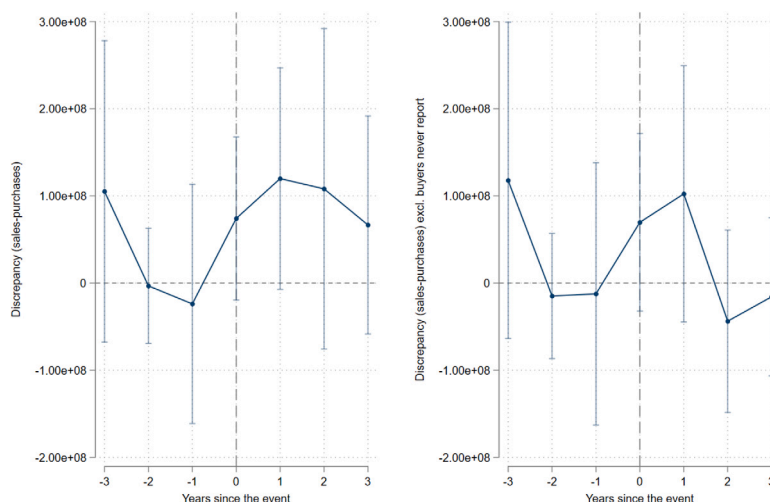
## 6. Mechanisms

The foregoing analysis generates four findings. First, firms that begin to sell to the government tend to grow larger. Second, there is an increase in revenue-based labor productivity, but no changes in value-added productivity. Third, selling to government buyers is associated with a reduction in sales to the private sector and a re-organization of the domestic supply chain towards more buyers but smaller transactions. Finally, firms that exit from selling to government entities do not attain previous levels of total sales, i.e., the loss of government sales is not offset by increasing sales to private companies.

These findings raise several questions. What makes the government a preferred buyer relative to private customers and what prevents firms from keeping relationships with other private buyers at scale once they start selling to the government? What can explain the absence of improvement in efficiency, proxied by value added per worker in our analysis? To gain greater insight into the mechanisms that underlie our findings we undertook an ad-hoc survey of Ugandan firms that participate in public procurement bids. The goal was to gather qualitative information on their strategies, with a focus on assessing whether and why firms shift away from sales to private buyers following transactions with the government to motivate additional analysis using available administrative data sources.

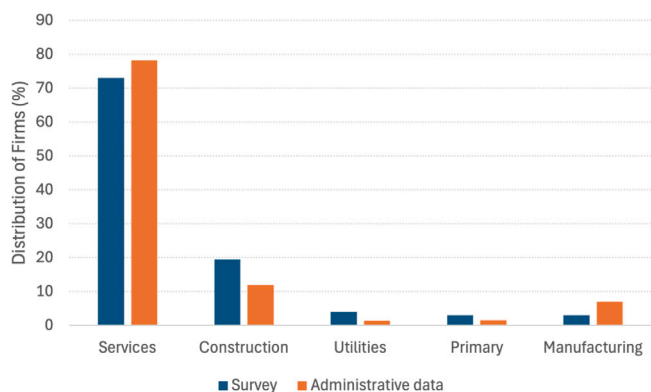
### 6.1. Qualitative firm survey evidence

The survey was designed and piloted in the fall of 2023 and conducted in the first quarter of 2024, targeting firms with experience with public procurement. The sampling frame was based on a list of firms



**Fig. 7.** Discrepancies in domestic sales.

Notes: Baseline compares early participants in procurement to late participants in procurement. Discrepancy is a proxy for the extent of misreporting in sales reported by a seller in a year. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing.



**Fig. 8.** Sectoral distribution of firms (surveyed firms vs. administrative dataset).

Notes: The figure compares the sectoral composition of firms in Uganda’s administrative data to the sectoral composition of procurement firms in our survey.

that pre-registered with the Ugandan Public Procurement and Disposal of Public Assets Authority (PPDA). We focused on firms located in the Greater Kampala area (Kampala, Wakiso, and Mukono) that were awarded at least one government contract during the period 2017–2022 and excluded firms whose contact details provided to the PPDA were no longer valid. Additional details on the survey, including on the sample strategy and the questionnaire, are reported in Section K in Appendix.

The selection procedure resulted in a sample of 282 firms, of which a total of 236 firms were successfully interviewed. The sectoral distribution of the firms in the survey sample is similar to that in the VAT database, with most firms active in services sectors (Fig. 8). The survey sample comprises a somewhat higher share of firms in construction, while manufacturing firms are slightly underrepresented relative to the VAT data. Consistent with our main findings, 49.7% of the firms interviewed see the government as their main client, and 85.7% regard government entities as potential buyers of their goods or services. Almost 95% of respondents obtained more than one government contract over the period considered, and 54.7% indicated their firm regularly supplies government entities. Appendix Table K-1 provides summary statistics for the surveyed firms.

Government purchases on average accounted for approximately half of the respondents’ turnover in the previous year (Appendix Figure K-1). Additionally, firms that define themselves as (a) continuous; or (b) frequent government suppliers tend to be larger than firms that see the government as a marginal source of demand (i.e that only sell to the government once over the period of interest). Government entities tend to be larger buyers (91% of respondents), provide longer-term contracts (78%), and pay higher prices (64%).

### 6.2. Analysis of potential mechanisms

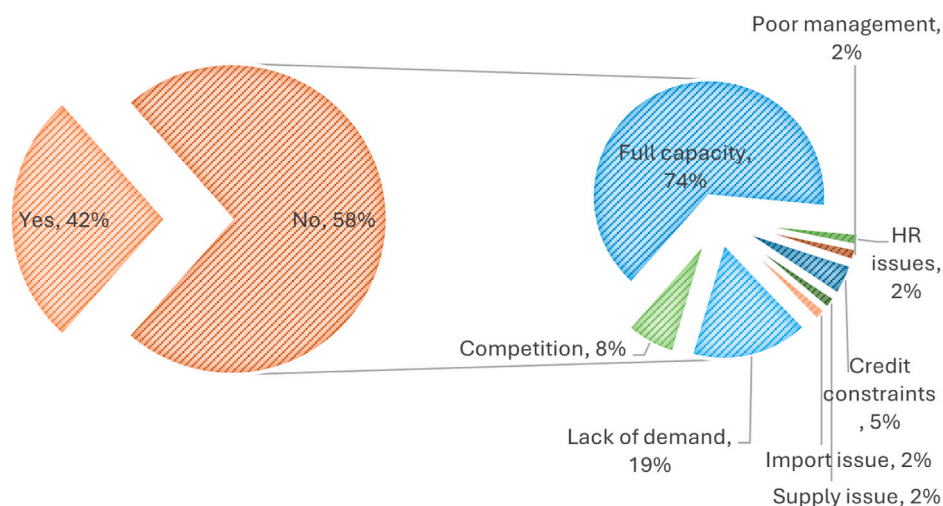
We draw on the survey findings, as well as on additional data analysis, to evaluate two potential explanations for why sales to private buyers fall following entry into government procurement: (1) capacity constraints; and (2) price differentials across public and private sector buyers.

#### 6.2.1. Capacity constraints

Two-fifths of respondents (42%) report that their firm expanded production following award of a government contract, indicating that for many of the surveyed firms capacity constraints were not a major factor (Fig. 9).<sup>26</sup> Of the firms that did not expand, three-quarters (74%) declared they were already operating at full capacity (Fig. 9). Respondents for these firms also mentioned lack of demand and competition as other factors that restricted investment. Only 5% of firms cited access to credit as an impediment to expanding production capacity.

We cannot directly evaluate to what extent capacity constraints drive the decline in sales to private buyers that we estimate using the VAT data, but we can use the administrative data to provide supportive evidence by differentiating between firms based on sector of activity and associated average capital intensity of production. Services firms comprise a majority of those expanding capacity (62%). Firms engaging in relatively low-skill labor-intensive activities (e.g., cleaning services) will tend to have lower fixed asset ratios than manufacturing or mining activities. In principle, they may therefore be less constrained in expanding production in response to a positive demand shock. We investigate this hypothesis by exploiting the variation in capital

<sup>26</sup> Among the firms that expanded capacity following a government contract, 55% invested in their workforce (hiring more employees), while 71% invested in assets (Figure K-2 in Appendix). Shares sum up to more than 100% since some firms did both.



**Fig. 9.** Expanding capacity following government contracts.

*Notes:* The left side pie chart reports the responses to the following question: “Regarding your last contract awarded, did you expand production after obtaining the contract?”. The right side pie chart lists reasons given for not expanding production (multiple choices were possible). Only firms responding “no” to the first question were asked to respond to the second question. Overall, 217 firms responded to the first question and 125 to the second.

intensity across sectors under the assumption that firms in sectors with higher fixed asset ratios may be more constrained in expanding output in the short run if capacity utilization rates are high. Firms in services are less capital intensive (see Appendix Figure J-1), making it easier to expand in the short run after obtaining a contract to supply government entities. Fig. 10 (left hand panel) shows that total sales increase by similar magnitudes for both services and non-services firms. However, the adjustment margin differs. Services firms, which face less constraints on expanding capacity, increase total sales without reducing private sector sales (right hand panel). In contrast, non-services firms, which have higher fixed assets and confront greater capacity constraints, expand sales to the government primarily by reallocating away from private sector sales. Despite these different adjustment margins, the overall growth in total sales is similar across the two groups. This suggests that non-services firms are able to achieve comparable revenue gains without expanding production to the same extent, which is consistent with government contracts offering higher prices relative to private sector sales. We explore this as a potential explanation in the next sub-section.

We next assess whether firms expand production capacity when they start selling to the government. Fig. 11 shows that selling to the government is not related to a clear expansion in total investment or fixed assets when they start selling to the government. While fixed assets exhibit an increase, the estimates are imprecise and do not reflect a discrete investment response at the time firms begin selling to a government entity. Consistent with this, the capital–labor ratio exhibits substantial volatility but no sustained post-treatment increase, and the ratio of fixed assets to sales declines following treatment.

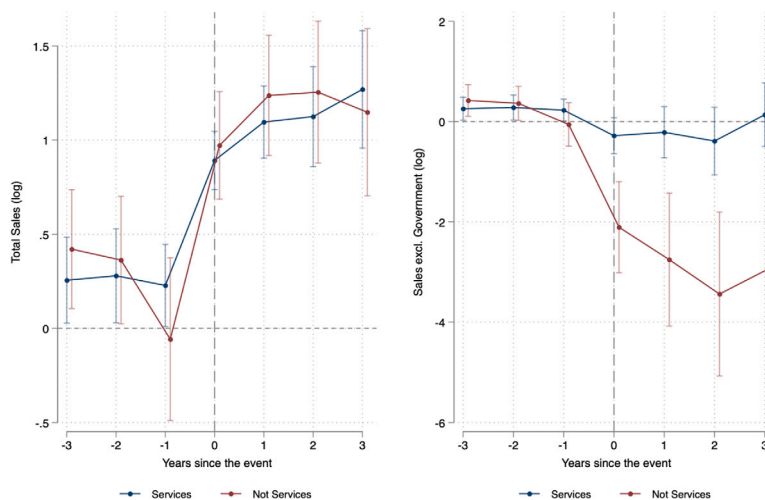
### 6.2.2. Government as a more profitable buyer

As noted previously, 64% of the survey respondents state that government entities pay higher prices for their goods or services than private buyers. Related, when asked why they reduce sales to other firms following a government contract, a majority (60%) indicate this is because private firms pay lower prices. While it is not possible to directly test this channel using the VAT data due to the absence of information on prices and quantities, we provide suggestive evidence for this mechanism using three diverse strategies. First, suggestive support for the price mechanism is provided by changes in the pattern of purchases of firms selling to the government from their suppliers. If the overall sales growth observed in the VAT data is due to the government offering higher prices, the firm’s purchase of inputs

should be relatively stable. Appendix Figure I-1 shows that there is no effect on total purchases from suppliers in the year a firm begins selling to the government, and while we observe an increase in later years, these effects are imprecisely estimated. Further, there is no significant increase in the number of suppliers from the time a firm starts selling to the government and continues doing so. Second, we exploit information from Customs clearance transactions data. These data include information on the quantity and the value of goods that are exported and imported by a given firm. We focus on the group of firms in the “Wholesaler, Retailer and Repair” sector where input quantities, proxied by imports, can be expected to be highly correlated with sales, as their activity does not generally involve transformation. If a firm imports most of what it sells, the quantities imported should approximate the quantities that are sold, and any difference in the value of sales after beginning to sell to the government should then reflect a change in prices. We use two thresholds for the share of imported inputs in total purchases: (i) greater than or equal to 65%, and (ii) greater than or equal to 95%. Table 3 reports results of an OLS regression estimating the relationship between the value of sales and import quantities subsequent to a firm in the “Wholesale and Retail” sector obtaining a government contract. Columns (1) and (2) use the 65% threshold and columns (3) and (4) show results when using the 95% threshold ratio of import value to total sales. There is a positive relationship between commencing to sell to the government and the value of total sales, but not for imported quantities. This suggests that sales to government entities are associated with higher prices. Third, we exploit profitability data from firms’ CIT records and apply the baseline event-study framework to examine how reported profits evolve when firms begin supplying the government. Fig. 12 shows an increase in profit levels once firms start selling to the government. This additional evidence is again consistent with the interpretation that selling to the government is more profitable.

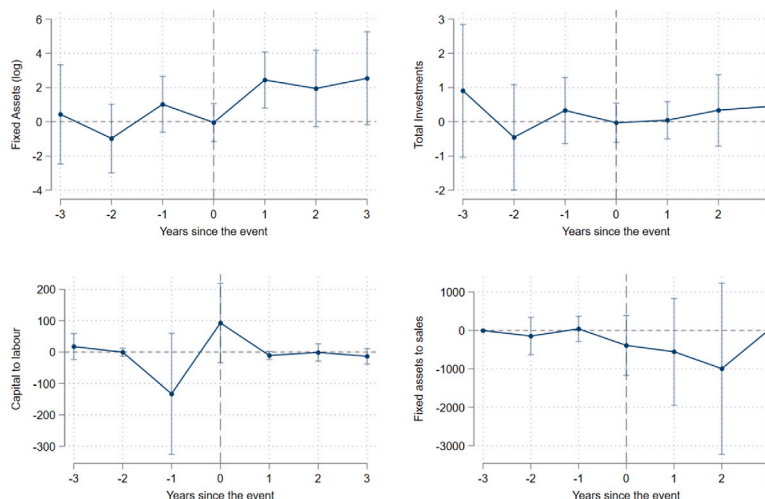
### 6.2.3. Price differentials, capacity constraints and uncertainty

While higher prices for government contracts appear to play a role in the observed reallocation of supply capacity away from private buyers, there is also evidence that capacity constraints may be pertinent. A potential explanatory factor for both decisions not to expand capacity when this involves fixed costs (investment in tangible assets) and for the observed price differentials is uncertainty associated with the procurement process. Four fifths of surveyed firms (81%) report that the government is unlikely to pay them on a timely basis. When



**Fig. 10.** Services vs. non-services firms: Total Sales and Sales excluding the Government.

*Notes:* Baseline compares early participants in procurement to late participants in procurement. The change in total sales and sales excluding government is compared across Services and Non-Services sectors. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. Total sales and sales excluding government is reported in  $\log s + 0.01$ .



**Fig. 11.** Assets and investments.

*Notes:* Baseline compares early participants in procurement to late participants in procurement. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. Outcome variables are respectively (i) fixed assets (ii) total investment (iii) capital to labor ratio and (iv) fixed assets to sales. Fixed assets and total investments are in logs.

**Table 3**

Value of sales to government and imported quantities (firms in the Distribution sector).

	(1)	(2)	(3)	(4)
	Total sales (value)	Import quantity	Total sales (value)	Import quantity
After first sale	0.714*** (0.147)	-0.120 (0.190)	0.458** (0.230)	-0.175 (0.322)
Sample	3525	3525	1480	1480

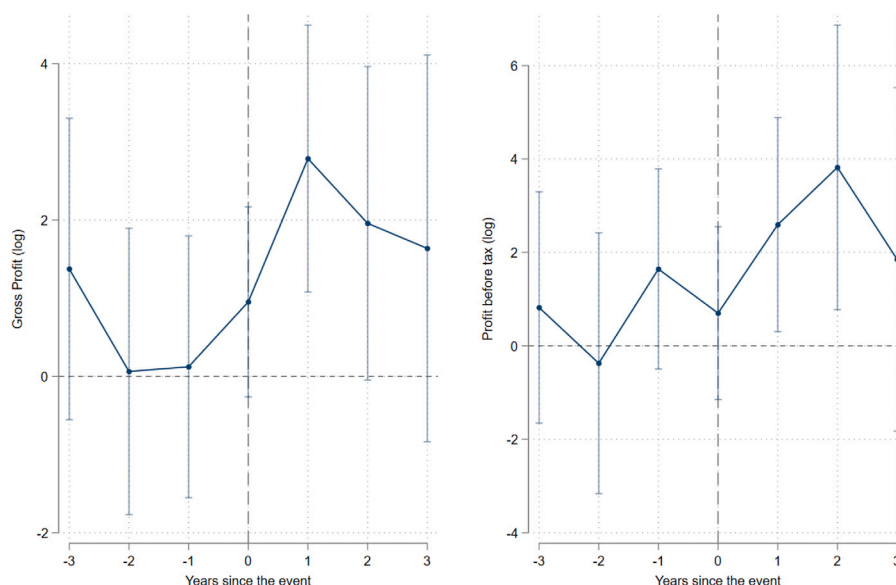
*Notes:* This table reports the relationship between a dummy indicating the year in which firms in the wholesale sector (ISIC rev. 4 Section G, Divisions 45, 46, and 47) start selling to the government, their value of sales and imported quantities. Columns 1 and 2 (3 and 4) report estimates for wholesalers with a share imports in total purchases above 65% (95%). Standard errors are clustered at the firm level. Estimates include firm and sector-year fixed effects. \*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

asked about their *last* contract with the government, 64% of the firms stated that it was not paid on time. The average delay in payment

was five months, with a maximum delay of more than two years (Appendix Figure K-3). Such delays have potential consequences for investment planning and may have an effect on the willingness as well as the ability of firms to expand capacity. Expected payment delays also may be reflected in contract prices, insofar as firms factor this into their bids. Uncertainty regarding the duration of relationships with government entities and the prospects of renewal and obtaining government contracts over a long period of time can also be expected to influence firm decisions. This is an issue that calls for further research, as our data do not permit assessment of the potential role of uncertainty as a factor explaining investment or pricing differentials.

**7. Conclusion**

This paper contributes to the literature analyzing the relationship between government demand and firm performance, using detailed administrative data for Ugandan firms. Applying an event study approach that accounts for self-selection and the heterogeneity in the timing of



**Fig. 12.** Profitability.

*Notes:* Baseline compares early participants in procurement to late participants in procurement. Callaway and Sant’Anna (2021) correction is applied to account for heterogeneity in treatment timing. The variables “gross profits” and “profit before tax” are from the Corporate Income Tax return form. Both the outcome variables are reported in  $\log s + 0.01$ .

the treatment we find evidence of positive effects on sales and output per worker in firms that start to sell to government entities. The positive association with total sales is accompanied by a reduction in sales to non-government entities, i.e., overall sales growth is partly at the expense of a reallocation of firm-level supply to government buyers.

Our findings suggest that many firms are limited in their ability to expand capacity and/or that government contracts are more profitable. We find some suggestive evidence for the capacity constraint hypothesis, reflected in the reallocation of sales away from private buyers being associated with differences in factor intensity of production: it is less pronounced for firms in services that are less capital intensive and thus may more easily be able to expand output in the short run by increasing employment. While the firm-level survey provides support for the existence of capacity constraints, some 40% of respondents active in the procurement market report an increase in production capacity during the period considered. This suggests capacity constraints are not a major driver of the sustained reduction in sales to the private sector. This assessment is bolstered by most interviewees indicating that access to finance was not a major concern. Instead, most respondents reported that relative to private buyers, the government pays higher prices and that this was the main motivation for reducing sales to other firms.

Understanding the reasons underlying these patterns is important both for the design of procurement processes, an issue our paper does not engage with, and for the evaluation of the effectiveness of efforts to use public procurement as an industrial policy instrument in low-income country contexts where government demand is significant. This will require researchers to have access to both VAT bilateral transactions data and transaction-level public procurement information (contract awards).

#### CRediT authorship contribution statement

**Bernard Hoekman:** Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Marco Sanfilippo:** Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Filippo Santi:** Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Rohit Ticku:** Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization.

#### Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2026.103832>.

#### Data availability

The authors do not have permission to share data.

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