Tax Policy at the Threshold:

A Network Perspective on Uganda's 2015 VAT Reform

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Abstract

This paper studies how firms reallocate sourcing in response to a VAT deregistration shock. In 2015, a reform that raised the VAT registration threshold in Uganda forced many small firms to exit the VAT system, disrupting existing buyer-supplier relationships. Using monthly buyer-seller data and Poisson pseudo-maximum likelihood with fixed effects, we relate outcomes to firms' pre-reform exposure to eventual deregistrants in the year leading up to the reform. Our findings show that buyers more exposed to deregistered suppliers reallocate input purchases away from deregistrants and towards the registered VAT network, adjusting at the intensive margin in the short run by increasing purchases per supplier, and at the extensive margin in the longer run by bringing on new registered suppliers. Quantitatively, exposed buyers increase purchases from registered suppliers by up to 59% within two years of the reform. At the intensive margin, they raise average inputs from pre-treatment partners by up to 52% and purchases per supplier by up to 108% within the first year. At the extensive margin, they expand their stock of registered suppliers by about 18% and the monthly inflow of new registered suppliers by roughly 44% from the second year onward. This reallocation of inputs from deregistered to more formal firms illustrates how VAT-induced shocks propagate through supplier linkages, reshaping the structure of production networks.

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1 Introduction

Value-added tax (VAT) is one of the most widespread forms of taxation globally. With 175 jurisdictions having established it in some form (Brockmeyer et al. 2024) and VAT receipts accounting for as much as 50% of total tax revenue in certain jurisdictions (UNU-WIDER 2025), it is a central pillar of the global tax system and a primary engine of domestic revenue mobilization worldwide. In practice, the reach of VAT is limited by administrative capacity, so even as VAT becomes more important for revenue, governments typically set a mandatory VAT registration threshold based on turnover to focus enforcement on larger firms.[1] Such thresholds are designed to reduce enforcement costs for tax administrations and compliance costs for small businesses. However, the literature shows that these thresholds can distort competition and encourage underreporting or firm fragmentation by creating discontinuities in the tax schedule.[2] Yet, evidence is thinner on how VAT systems shape the structure of firm-to-firm trading relationships. One important consequence of these thresholds is that they can segment production networks among firms subject to different tax regimes. Since VAT-registered firms can only deduct input taxes paid to other registered suppliers, they have an incentive to source from firms within the VAT system, whereas unregistered firms, unable to claim input tax credits, tend to trade with one another to avoid paying unrecoverable VAT on their inputs. Threshold changes can therefore rewire production networks by altering which suppliers sit inside the credit chain.

We study how a change in the VAT registration threshold affects firms' behaviour and the structure of production networks. Using a reform that raises the threshold and forces some suppliers to deregister, we examine how this shock propagates through buyer–supplier links and how downstream VAT-registered buyers adjust sourcing on the intensive and the extensive margins. When a VAT registration threshold determines which firms can issue creditable invoices, it effectively creates two regimes inside a production network, with registered buyers facing a higher effective input cost when sourcing from unregistered suppliers. Standard theories of tax incidence and firm boundaries in buyer-supplier relationships therefore imply that changes in VAT eligibility can reallocate sourcing across suppliers, altering both the extensive margin (which suppliers a buyer trades with) and the intensive margin (how much it purchases from each supplier). The central research question is: when a policy shock pushes some suppliers out of the VAT system, do downstream VAT-registered firms substitute toward registered suppliers, reorganize their supplier base, or rely more heavily on existing relationships, and how persistent are these adjustments?

We study this question in Uganda, which raised the VAT registration threshold in 2015 from UGX 50 million to UGX 150 million in annual turnover.[3] The reform induced a large and abrupt wave of deregistration among low-turnover firms, creating a plausibly exogenous disruption to the set of suppliers able to provide creditable invoices. Uganda is a useful setting because VAT administration is organized around invoice reporting, meaning that buyer-supplier transactions are recorded in VAT returns and can be linked to suppliers' registration status. This allows us to observe production-network adjustments at scale in administrative data, and to contrast registered buyers with zero pre-reform exposure to eventual deregistrants, to otherwise similar buyers with non-trivial exposure.

Empirically, we implement a difference-in-differences event-study design estimated by Poisson pseudo maximum likelihood (PPML) with high-dimensional fixed effects. We compare outcomes for buyers with higher pre-reform exposure to eventual deregistrants to those with no exposure, before and after the reform, while controlling for time-varying sector and location shocks. Exposure is defined as the pre-reform share of a buyer's inputs sourced from suppliers that get deregistered during the reform.

[1] According to the OECD (2024), among 37 surveyed OECD member and partner countries, only 6 do not apply a registration or collection threshold.

[2] For a thorough discussion of VAT registration thresholds and the trade-offs between efficiency, equity, and compliance costs, see Satterthwaite (2018).

[3] This corresponded roughly to an increase from USD 15,000 to USD 45,000.

Using transaction-level VAT records linked to registration histories and firm characteristics, we quantify reallocation of inputs toward VAT-registered suppliers, extensive-margin adjustments in supplier choice and entry, and intensive-margin changes in spending per supplier, including substitution toward pre-reform trading partners.

The 2015 threshold increase triggers a meaningful rewiring of buyer–supplier networks among VAT-registered firms. When many small suppliers exit the VAT system, exposed buyers face a sharper effective cost wedge on those links, and they respond by reorganizing sourcing toward the creditable VAT chain. In the data, this shows up as a sizeable and persistent reallocation of purchases toward continuously registered suppliers, rising to about 59% within two years after the reform. The adjustment is not instantaneous on all margins. Buyers initially absorb the disruption primarily on the intensive margin, scaling up purchases per registered supplier (up to 108% within the first year) and briefly increasing purchases from pre-reform partners (up to 52%). Over longer horizons, the response broadens into an extensive-margin rebuild: exposed firms expand their registered supplier base (about 18%) and add more new registered suppliers (around 44% from the second year onwards), sustaining the shift toward the VAT network even as the initial intensive-margin surge attenuates.

This paper sits at the intersection of three literatures and contributes to each. First, it relates to work on how VAT rules and registration thresholds shape firm behaviour. Second, it connects to evidence that tax status segments production networks by altering incentives to trade across formal and informal or registered and unregistered firms. Third, it speaks to research on compliance spillovers transmitted through invoice and input-credit chains. Across these strands, the central gap is dynamic: we know much less about how quickly buyer–supplier links reconfigure after a threshold change that forces firms out of the VAT system, and which margins drive adjustment over time. We address this gap by exploiting Uganda's 2015 threshold increase as a deregistration shock and tracing network reorganisation using monthly firm-to-firm VAT transaction data linked to registration histories.

A large literature shows that VAT registration thresholds induce distortions around the cutoff, including bunching, reporting responses, and organisational adjustments, as in the broader public finance evidence on notches and kinks (Kleven and Waseem 2013; Onji 2009; Muthitacharoen, Wanichthaworn and Burong 2021; Velayudhan 2018). While this work establishes that thresholds change firms' incentives to remain small or to misreport, it is typically less informative about how threshold policy reshapes inter-firm relationships. Our contribution is to shift the focus from firms at the threshold to downstream firms connected to marginal registrants. Rather than studying whether firms bunch at the threshold, we study how a threshold increase that induces supplier exit affects the sourcing choices of VAT-registered buyers, using an event-study difference-in-differences design.

Recent studies using transaction or linked buyer–seller data show that VAT creates a wedge on links that cross tax regimes, generating partial segmentation of trading networks. Evidence from India and Brazil shows that when firms become VAT-liable or change regime, they shift sales and purchases toward VAT-registered partners (Gadenne, Rathelot and Nandi 2020; Gerard, Naritomi and Seibold 2018). These papers establish that VAT status affects partner choice, but they mostly emphasise static differences or discrete regime switches by the focal firm. We contribute by studying a different policy margin and a different mechanism: a reform that pushes a mass of suppliers out of the VAT system, holding buyers' own VAT status fixed. This allows us to interpret changes as a response to the loss of creditable links, and to separate responses in supplier entry from changes in purchases per supplier.

A complementary literature highlights that the invoice-credit mechanism can transmit compliance incentives across trading partners, because registered buyers demand valid invoices to claim input tax credits. Models and evidence from Brazil and India emphasise strategic complementarities in formality and registration choices along supply chains (Paula and Scheinkman 2010; Rios and Seetharam 2017), and related empirical work shows that the benefits of (voluntary) registration depend on the registration

status of trading partners (Liu, Lockwood, Almunia et al. 2021; Muthitacharoen, Wanichthaworn and Burong 2021). Our contribution is to bring these ideas to a setting where a policy-induced deregistration shock provides a clean impulse to the network, in order to measure how the shock propagates through existing links and how buyers rebuild sourcing toward the creditable VAT chain.

Finally, we broaden the evidence base on VAT-induced network adjustments to a low-income setting where informality is pervasive, and the VAT threshold is an administratively salient policy lever in ongoing reform debates. This matters because most transaction-level evidence on VAT and production networks comes from higher-capacity tax environments, including the UK (Liu, Lockwood and Tam 2024), Brazil (Paula and Scheinkman 2010; Gerard, Naritomi and Seibold 2018), and India (Rios and Seetharam 2017; Gadenne, Rathelot and Nandi 2020). Exploiting Uganda's 2015 threshold increase, we combine transaction-level VAT returns with registration histories and firm characteristics to build a dynamic buyer–supplier panel that tracks adjustment month-by-month. This granularity lets us speak to a dimension that is typically unobserved in prior work: not only whether VAT policy segments networks, but how quickly networks rewire after a deregistration shock, and whether the early response is driven primarily by scaling up purchases along surviving registered links or by forming new registered relationships.

The remainder of the paper is organised as follows. Section 2 describes the institutional background and the 2015 VAT threshold reform in Uganda. Section 3 introduces the administrative data sources, sample construction, the exposure measure, and the main outcome variables. Section 4 presents the empirical framework and event-study difference-in-differences design, and reports the main results on reallocation towards continuously registered suppliers as well as the sequencing of intensive- and extensive-margin adjustments. Section 5 concludes with implications for VAT design, limitations, and planned extensions. The appendices provide additional results (including multi-bin and alternative exposure specifications), robustness checks, and supplementary tables and figures.

2 Institutional background and 2015 reform

Uganda operates a credit-invoice VAT in which registered firms charge VAT on sales and, in turn, claim input tax credits on VAT paid on business purchases supported by valid invoices. VAT registration is mandatory for firms above an annual turnover threshold. Firms below the threshold are outside the system unless they register voluntarily, but voluntary registration is uncommon and typically subject to strict checks focused on expected turnover. Registered firms are required to file regular VAT returns and report transaction details that link buyers and suppliers through invoice records, which the Uganda Revenue Authority (URA) can use for third-party verification and audit targeting. Because the right to claim input credits depends on the supplier's registration status, the VAT threshold determines not only who remits tax. It also determines which firms can issue creditable invoices, and therefore which trading links remain inside the formal VAT chain.

The case for raising the threshold hinges partly on how many firms sit in the low-turnover range, where monitoring costs are high relative to revenue potential. Figure 1 shows the 2014 distribution of annual turnover among VAT-eligible firms, based on corporate income tax (CIT) returns. The sample comprises 161,087 firms in VAT-eligible sectors with positive annual turnover, highlighting the breadth of Uganda's formal enterprise base. The distribution is highly right-skewed, with a dense concentration of firms reporting turnover below UGX 150 million and a steep decline in frequency as firm size increases. Notably, 39,632 firms, or 24.6% of the VAT-eligible pool, fall between UGX 50 million and UGX 150 million, the range that spans the pre- and post-reform thresholds. Small firms consequently account for a large share of the VAT-eligible pool, shaping the structure and performance of the consumption tax base. This substantial share of near-threshold firms suggests incomplete or inconsistent enforcement of VAT registration rules, as many firms within the eligible range appear not to have been registered for VAT despite meeting the turnover criterion.

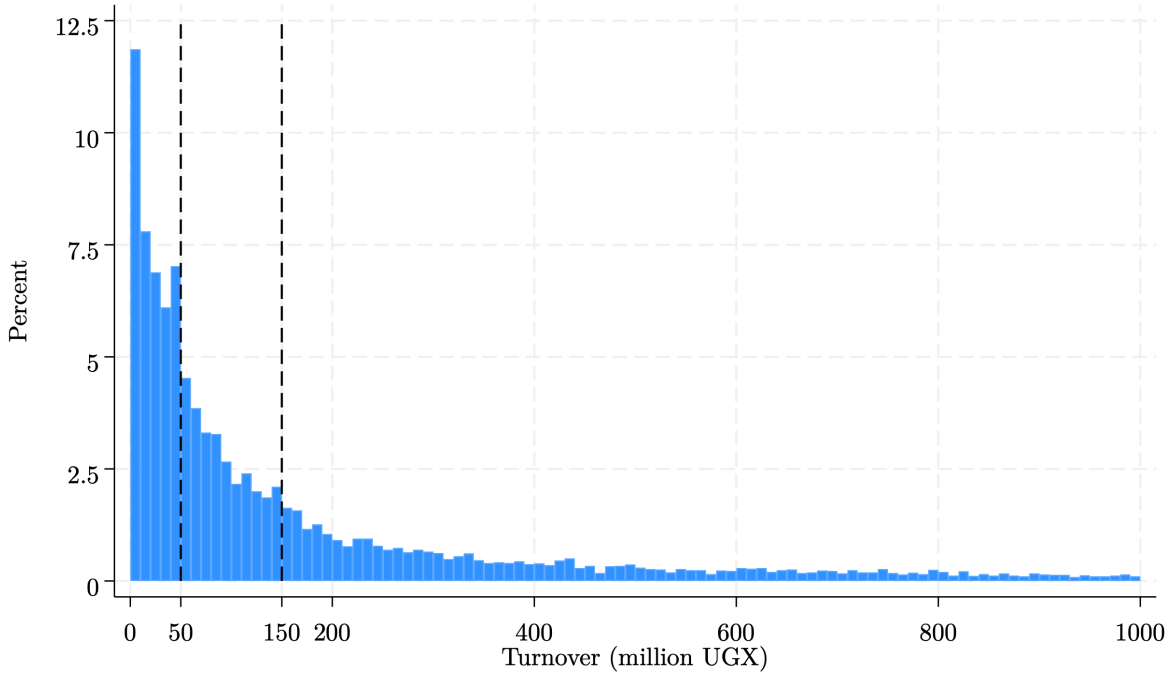
The 2015 reform was motivated by an administrative efficiency logic: the pre-reform threshold pulled into the VAT system many small firms whose revenue contributions were minimal relative to the administrative costs of monitoring them. These firms also faced disproportionate compliance burdens, including monthly filing obligations, invoice maintenance, and exposure to audits, often resulting in late filing or non-compliance penalties. Recognising these inefficiencies, the Government of Uganda raised the VAT registration threshold in 2015 from UGX 50 million to UGX 150 million, which removed 2,768 firms (about 14%) from the VAT register. The reform enabled the URA to redirect enforcement resources toward higher-turnover taxpayers whose transactions account for the majority of VAT revenue.

The 2015 threshold adjustment aligned Uganda's VAT regime with regional peers, namely Kenya (UGX 150 million), Rwanda (UGX 84 million), and Tanzania (UGX 62 million), and it also accounted for inflation, exchange-rate depreciation, and nominal turnover growth since VAT was introduced in 1996. By refining the boundary between small and medium-sized taxpayers, the new threshold improved administrative efficiency and compliance focus, better aligning taxable capacity with potential VAT liability.[4] Nonetheless, as Figure 1 shows, the large number of firms clustered near the threshold highlights the continuing challenge of maintaining consistent enforcement in a system where many businesses operate close to the registration cutoff.

The threshold reform caused an immediate contraction of Uganda's VAT network, followed later by renewed expansion as digital enforcement tools scaled up. As illustrated in Figure 2, the number of VAT-registered firms in Uganda rose steadily from 2013 to 2015, but experienced a sharp drop immediately after the 2015 threshold reform, which raised the registration cutoff from UGX 50 million to UGX 150 million. This inflection point indicates that many smaller firms were deregistered following the policy change, temporarily contracting the VAT network. However, the figure also reveals a subsequent surge

[4] *Revenue Performance Report FY 2015/16* (15th June 2016). Uganda Revenue Authority.

Figure 1: Distribution of annual turnover among VAT-eligible firms, 2014



Notes: *Turnover data are taken from corporate income tax (CIT) returns. Firms operating in VAT-exempt or zero-rated sectors are excluded.*

in VAT registrations beginning around 2021, coinciding with the rollout of Electronic Fiscal Receipting and Invoicing Solution (EFRIS), suggesting renewed integration and expansion of the formal trading network once digital compliance infrastructure was introduced.

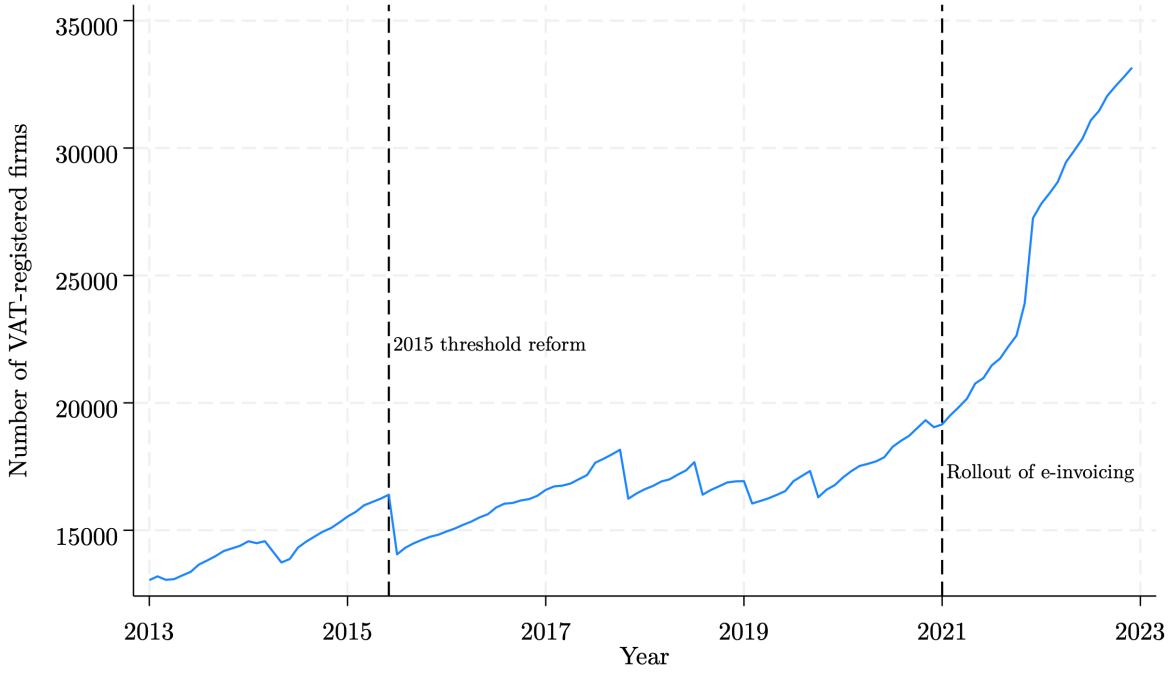
Uganda's VAT threshold policy reflects a fundamental trade-off between keeping the register administratively manageable and preserving the compliance and network benefits of broad coverage. A large register can become unwieldy for enforcement, since every additional registrant is entitled to claim input tax credits, which increases refund volumes, complicates risk-based audit selection, and stretches administrative capacity. Yet broad VAT coverage remains a core policy objective, because wider participation strengthens invoice chains, improves third-party verification, and broadens the base. The policy question is therefore not merely whether to raise or lower the threshold, but how to calibrate entry at the margin and pair threshold design with digital controls such as EFRIS, so that the administrative cost of an extra registrant is weighed against the network and compliance gains from keeping firms inside the VAT chain. A calibrated threshold can balance administrative efficiency and network connectivity, preserving valuable inter-firm links while supporting Uganda's structural transformation goals.

This design principle accords with Keen and Mintz (2004), which highlights the trade-off between administrative efficiency and economic connectivity. A higher threshold simplifies compliance and enforcement but can fragment the production network by excluding marginal firms that link formal and informal sectors. Designing adaptive or graduated VAT thresholds, complemented by digital compliance tools, could preserve beneficial inter-firm connections while maintaining a manageable administrative scope. Within Uganda's public finance management framework, such calibration would align tax design with structural transformation goals, ensuring that VAT reforms strengthen rather than weaken the economy's connective fabric.

With this trade-off in view, public discussions in Uganda have turned to whether the registration threshold should be raised even further. Stakeholders, spanning professional forums, policy institutions,

and industry associations, have proposed raising the VAT registration threshold well above UGX 150 million, including calls for UGX 500 million by accounting experts[5] and up to UGX 1 billion from industry groups.[6] Proponents argue that a higher cutoff would reduce compliance costs for small firms, reflect inflation and exchange rate movements since 2015, and let the URA focus on high-turnover taxpayers, while critics warn that further increases could detach small suppliers from VAT-linked buyer networks, particularly as EFRIS expands. This study is directly relevant to these debates, providing quantitative evidence on how threshold increases affect supply-chain links, and offering a baseline for ex ante evaluation of such scenarios.

Figure 2: Growth in VAT-registered firms



Uganda's 2015 increase in the VAT registration threshold provides a quasi-experimental setting to examine how fiscal policy reshapes the structure and functioning of production networks. Motivated primarily by administrative goals to reduce compliance burdens for small firms and improve enforcement efficiency, the reform also altered the composition of the formal VAT network. By shifting which firms sit inside the VAT chain, it generated network effects that extended beyond the directly treated firms.

The reform was accompanied by a clearer enforcement gradient around the new threshold, consistent with stronger de facto mandatory registration at and above UGX 150 million. Figure 3 plots, by turnover bins, the share of firms registered for VAT before (2013-2014) and after (2016-2017) the threshold reform, with dashed lines at the relevant thresholds. In 2013-2014, registration rates rise smoothly with turnover and are already substantial around UGX 50 million, with no pronounced discontinuity at the threshold. In 2016-2017, there is a clear jump at about UGX 150 million: registration is markedly lower

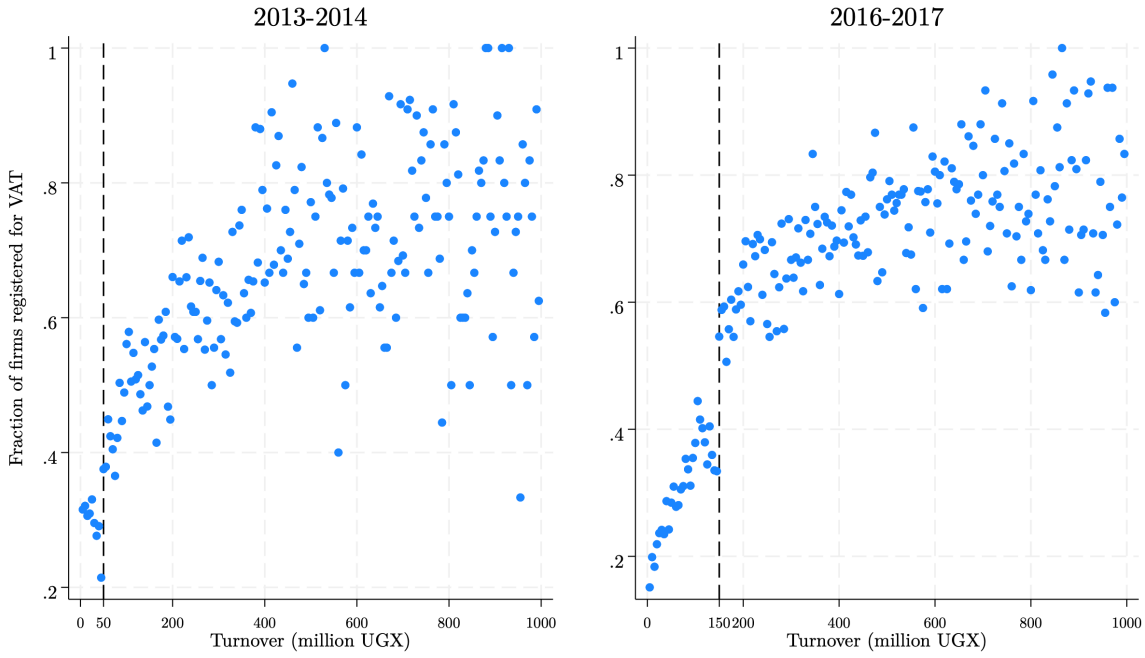
[5] C. Nassuuna (10th July 2025). *Accountants Recommend Increasing VAT Threshold from Shs 150 million to Shs 500 million.* Institute of Certified Public Accountants of Uganda (ICPAU). URL: `https://www.icpau.co.ug/news/accountants-recommend-increasing-vat-threshold-shs-150-million-shs-500-million` (visited on 08/11/2025).

[6] R. Ssuuna (May 2024). *Tax and Fiscal Governance: Is VAT milking the broken tax cow dry? An analysis of tax trends and impacts on EAC small traders and citizens, with a case of the recent traders' boycotts in Uganda.* Governance and Economic Policy Centre (GEPC). URL: `https://gepc.or.tz/tax-and-fiscal-governance-is-vat-milking-the-broken-tax-cow-dry-an-analysis-of-tax-trends-and-impacts-on-eac-small-traders-with-a-case-of-the-recent-traders-demonstrations-and-boycotts-in` (visited on 08/11/2025).

just below the threshold and higher just above it, consistent with tighter enforcement of mandatory registration. Enforcement remains imperfect, as registration is well below 100% even at two or three times the threshold, but the higher cutoff nonetheless strengthens de facto enforcement at and above the threshold, as reflected in the sharp level shift.

The Ugandan threshold adjustment redrew the boundary between formal and informal enterprise activity. Many small and medium-sized firms that previously issued VAT invoices deregistered, weakening their position as trading partners for VAT-registered buyers who seek to maintain input-credit chains. This pattern mirrors Banerjee et al. (2024), who show that exposure to formal institutions via microfinance crowds out informal links and shrinks networks, with losses spilling over to non-borrowers. By analogy, VAT deregistration should prompt buyers to re-optimise towards compliant suppliers, increasing centralisation around persistently registered firms. These mechanisms imply that threshold reforms can have consequences beyond static revenue effects, by reshaping the set of viable buyer–supplier links. By disconnecting small firms from the VAT chain, the reform may have intensified market concentration and reduced opportunities for formalisation spillovers. Over time, these micro-level adjustments can alter the aggregate topology of the economy, weakening forward and backward linkages and potentially slowing the diffusion of compliance norms.

Figure 3: Enforcement of mandatory registration around the turnover threshold, before and after the 2015 reform



Notes: *Turnover data are taken from corporate income tax (CIT) returns. Firms operating in VAT-exempt or zero-rated sectors are excluded.*

3 Data and descriptive statistics

This study draws on confidential administrative data from the URA, comprising three complementary sources. First, the tax registry provides firm-level information on VAT registration and deregistration dates. Second, monthly VAT returns record detailed breakdowns of firms' taxable activities, including sales, purchases, inputs acquired from VAT-registered suppliers, and output and input tax amounts. Third, annual CIT returns supply information on firms' sector of activity and geographic location. Together, these datasets enable the construction of a firm-level panel that links registration status, tax declarations, and key characteristics over time.

Using the datasets described above, we construct a panel of VAT-registered firms covering the period from January 2013 to June 2022. The panel includes firm-month observations with complete information in both the VAT annex (Schedule 2, which reports the VAT-registered suppliers and the value of purchases from each), and the VAT summary return, which provides an overview of a firm's monthly taxable activities. To this panel we add the time-invariant variables of sector (21 ISIC sections) and location (118 districts) from the CIT returns.

We define the key variable, *exposure*, as the share of inputs firm i purchased from the 2,768 firms that got deregistered from VAT in the 2015 reform, in the year leading up to it. For quality control, we restrict this variable to firms that filed at least six monthly VAT returns during that year. As shown in Figure 4, the distribution of exposure among treated (positive exposure) firms is highly right-skewed with most observations concentrated near zero. Figure 5, which plots mean exposure by turnover on a log scale, indicates that average exposure is low throughout the turnover distribution and slightly lower for higher-turnover firms. The differences across bins are modest and imprecise.

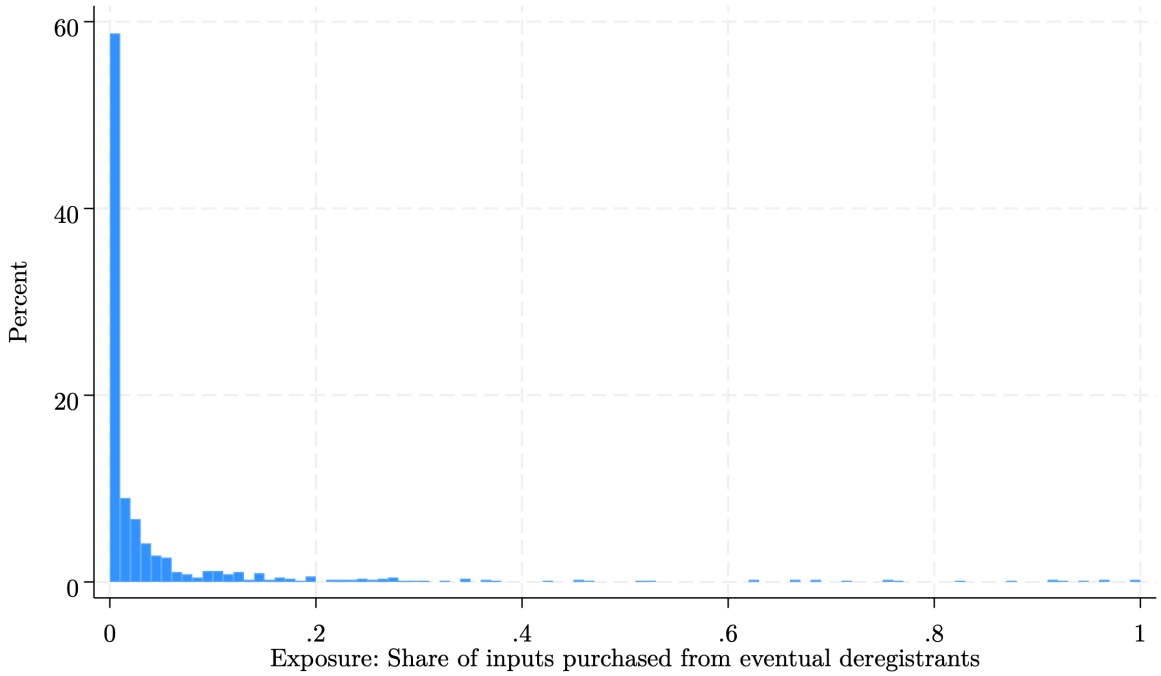
To simplify the assessment of differences across firms with varying levels of exposure, we divide exposed firms into three bins based on their exposure levels: 0-1%, 1-5%, and above 5% of total input purchases. Firms with zero exposure form the control group.

We construct a set of outcome variables at the monthly level that capture firms' sourcing behaviour and supplier dynamics following the 2015 VAT reform. Our outcomes include:

- *Inputs from registered suppliers*: the total value of inputs a firm purchased from *continuously* VAT-registered suppliers (i.e. that haven't been deregistered as part of the 2015 reform). We use this variable in order to study the reallocation of inputs away from firms that got excluded because of the reform, towards the VAT network. This is due to data constraints, where firms only disclose their purchases from -and sales to- other VAT-registered firms. Hence, even though we are able to construct the share of inputs firms purchased from the deregistrant firms prior to 2015, we can't reliably construct this measure for the post-treatment periods. Therefore, we measure the bulk of inputs purchased from continuously registered firms, and hypothesize a statistically significant increase for highly-exposed firms relative to those that are lowly-exposed.
- *Number of registered suppliers*: the total number of distinct VAT-registered suppliers from which the firm purchased in a given month.
- *Number of new registered suppliers*: the number of VAT-registered suppliers a firm transacts with for the first time that month.
- *Average input per supplier*: the average value of inputs purchased per VAT-registered supplier, computed as the ratio of total purchases from registered suppliers to the number of such suppliers.
- *Average inputs from pre-treatment trade partners*: the average value of inputs purchased per pre-treatment trade partner.

All currency variables are winsorized at the 99th percentile by month to limit the influence of extreme outliers. Together, these outcomes allow us to examine both the intensive and extensive margins of firms'

Figure 4: Distribution of positive exposure to VAT-deregistrants

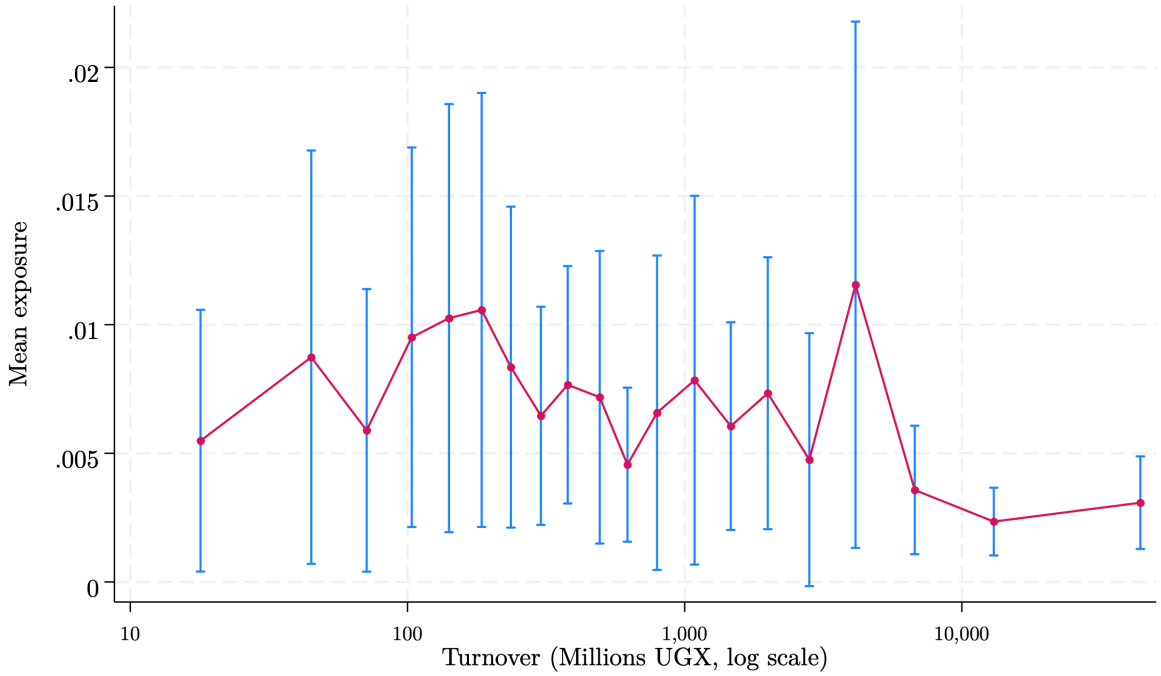


Notes: *There are 25,418 unique firms in our sample. Among these, 6,085 have non-missing exposure (i.e., they filed at least six monthly VAT returns in the year leading up to the reform), and 846 have non-zero exposure (indicating at least some purchases from eventually deregistered firms).*

input sourcing decisions, distinguishing between continuing and newly formed supplier relationships, and between pre-existing and newly established trade links.

Guided by Table 1, the treated group is defined as buyers with exposure +5%, and the control group as buyers with 0% exposure. This choice is driven by comparability. Relative to the 0-1% and 1-5% bins, firms in the +5% bin look most similar to the 0% bin along pre-treatment size and network measures. In particular, mean total inputs (cost of materials from the VAT Panel) and total sales are not statistically different between +5% and 0%; by contrast, both low-positive bins are significantly larger in total sales, and the 0-1% bin is significantly larger in inputs. Network size is also aligned; the number of registered suppliers is indistinguishable between +5% and 0%, yet markedly higher in the 0-1% and 1-5% bins. This pattern is consistent with the construction of the exposure measure; since it captures the share of purchases from eventual deregistrants, exposure tends to be either 0 or relatively high for smaller buyers that transact with small suppliers, whereas larger buyers with many suppliers register low but positive exposure mechanically. Restricting treatment to +5% therefore yields a comparison where pre-treatment size and network structure are well matched, and other outcomes in Table 1 also exhibit fewer significant differences versus 0% than in the low-positive bins. The primary specification thus compares +5% to 0%.

Figure 5: Mean exposure to eventual deregistrants by turnover



Notes: *Mean exposure by turnover (log scale). Sample restricted to 6,062 firms with non-missing exposure and positive turnover. Values are averaged within 20 equal-sized bins; points show bin means and bars show 95% confidence intervals.*

Table 1: Balance table of pre-treatment characteristics across exposure bins, and differences vs 0% exposure

		Exposure bins					Differences vs 0% exposure		
		0%	0–1%	1–5%	5%+		0–1% vs 0%	1–5% vs 0%	5%+ vs 0%
Cost of materials from the VAT Panel	Mean	162.28	553.04	209.60	117.97	Mean diff.	-390.76	-47.32	44.31
	(SD)	(464.75)	(1076.01)	(587.20)	(339.28)	P-val	0.000***	0.170	0.236
	N	5,239	497	192	157	(SE)	(25.60)	(34.51)	(37.39)
						SMD	-0.47	-0.09	0.11
Total sales from the VAT Panel	Mean	280.37	1077.51	462.45	173.42	Mean diff.	-797.14	-182.09	106.94
	(SD)	(849.56)	(1966.97)	(1321.12)	(499.88)	P-val	0.000***	0.004***	0.117
	N	5,239	497	192	157	(SE)	(46.79)	(63.96)	(68.16)
						SMD	-0.53	-0.16	0.15
Amount of inputs purchased from VAT-registered suppliers	Mean	108.22	354.79	141.11	56.45	Mean diff.	-246.58	-32.89	51.77
	(SD)	(306.61)	(677.79)	(338.86)	(99.68)	P-val	0.000***	0.146	0.035**
	N	5,239	497	192	156	(SE)	(16.64)	(22.62)	(24.59)
						SMD	-0.47	-0.10	0.23
Number of registered suppliers	Mean	5.09	22.30	20.35	5.14	Mean diff.	-17.21	-15.26	-0.05
	(SD)	(5.90)	(26.07)	(26.60)	(7.05)	P-val	0.000***	0.000***	0.925
	N	5,239	497	192	157	(SE)	(0.45)	(0.56)	(0.48)
						SMD	-0.91	-0.79	-0.01
Dummy for getting new supplier	Mean	0.36	0.71	0.63	0.44	Mean diff.	-0.35	-0.27	-0.07
	(SD)	(0.28)	(0.23)	(0.31)	(0.26)	P-val	0.000***	0.000***	0.003***
	N	4,560	469	172	143	(SE)	(0.01)	(0.02)	(0.02)
						SMD	-1.35	-0.92	-0.26
Number of new registered suppliers	Mean	0.65	1.97	1.79	0.73	Mean diff.	-1.32	-1.13	-0.07
	(SD)	(0.75)	(1.57)	(1.83)	(0.66)	P-val	0.000***	0.000***	0.248
	N	4,560	469	172	143	(SE)	(0.04)	(0.06)	(0.06)
						SMD	-1.07	-0.81	-0.10
Share of new suppliers	Mean	0.12	0.12	0.13	0.19	Mean diff.	0.00	-0.01	-0.07
	(SD)	(0.13)	(0.08)	(0.13)	(0.17)	P-val	0.640	0.259	0.000***
	N	4,560	469	172	141	(SE)	(0.01)	(0.01)	(0.01)
						SMD	0.03	-0.09	-0.48
Average inputs purchased per registered supplier	Mean	43.68	20.42	10.94	21.73	Mean diff.	23.26	32.75	21.95
	(SD)	(161.50)	(63.73)	(23.70)	(46.97)	P-val	0.001***	0.005***	0.090*
	N	5,239	497	192	156	(SE)	(7.30)	(11.66)	(12.95)
						SMD	0.19	0.28	0.18

Notes: *In the first four columns (summary statistics by exposure bins), each variable shows* ***mean, standard deviation in parentheses, and count.*** *In the three difference columns, each variable shows* ***mean difference, p-value with significance stars, standard error in parentheses, and standardized differences.*** *Monetary values are in millions of Ugandan shillings (UGX). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

4 Empirical framework and results

We estimate Poisson pseudo maximum likelihood models with fixed effects in an event-study specification. For firm i in month t, let $q(t)$ denote the calendar quarter that contains month t. Normalizing the last pre-reform quarter $q_0 = 2015\text{q}2$ as the reference period, the conditional mean is

$$\mathbb{E}[Y_{it} \mid \cdot] = \exp\left(\sum_{q \neq q_0} \beta_q \left[\mathbb{1}\{q(t) = q\} \times \text{Exposure}_i \right] + \alpha_i + \mu_{s(i),t} + \theta_{\ell(i),t} \right). \tag{1}$$

where α_i are firm fixed effects, $\mu_{s(i),t}$ are sector-by-month fixed effects, and $\theta_{\ell(i),t}$ are district-by-month fixed effects. Standard errors are clustered by firm.

Event time and reference quarter. Quarter indicators $\mathbb{1}\{q(t) = q\}$ partition the calendar into pre- and post-reform quarters. The omitted category is $q_0 = 2015\text{q}2$, the last pre-reform quarter.

Exposure definition and reference group. We estimate the event-study model using a binary exposure indicator. Let $B_i \in \{0, 1, 2, 3\}$ denote firm i's exposure bin, defined as $B_i = 0$ for 0%, $B_i = 1$ for 0–1%, $B_i = 2$ for 1–5%, and $B_i = 3$ for +5% exposure. In the main specification, we restrict the sample to firms in bins $B_i \in \{0, 3\}$ and collapse exposure to a single indicator, $\text{Exposure}_i = \mathbb{1}\{B_i = 3\}$, so treatment compares the +5% bin to the 0% bin. Each β_q is the relative change in $\mathbb{E}[Y_{it} \mid \cdot]$ for +5% exposed firms compared with 0% exposed firms in quarter q, relative to the reference quarter 2015q2.

Outcomes: Y_{it} is one of the following outcomes:

1. **Inputs from registered suppliers** (reg_amount_w_{it}): value of inputs (millions of UGX) purchased from suppliers not in the 2015 deregistrant cohort.
2. **Registered supplier count** ($\text{reg\_sup\_count}_{it}$), number of suppliers not in the 2015 deregistrant cohort.
3. **New supplier count** ($\text{new\_sup\_count}_{it}$): number of new registered suppliers.
4. **Average inputs per registered supplier** (avg_input_w_{it}), average inputs per registered supplier.
5. **Average inputs from pre-treatment trade partners** ($\text{avg\_inp\_pre\_partner\_w}_{it}$): mean inputs (millions of UGX) that buyer i purchases in month t from suppliers whose first trade with i was before 2015m7.

Sector and location definitions: Sectors are defined at the ISIC *1-digit* level (21 categories in our sample), and locations at the *district* level (118 districts). Accordingly, $\mu_{s(i),t}$ are sector-by-month fixed effects and $\theta_{\ell(i),t}$ are district-by-month fixed effects.

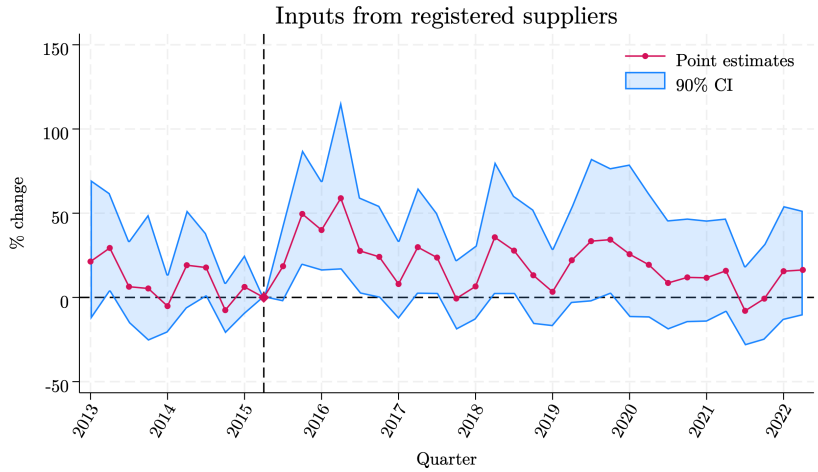
Sample: The estimating sample includes firm-months that appear in Schedule 2 (buyer declarations), appear in the VAT Panel, and are VAT-registered in the administrative registry.

Interpretation and reporting: Each panel shows quarterly event-study coefficients with 90% confidence intervals. Point estimates are transformed to percentage effects as $100\,[\exp(\widehat{\beta}) - 1]$. For binary outcomes, interpret these as relative changes in the probability. A plotted value of 20 means the reported group has about 20% higher conditional mean than the 0% group in that quarter, relative to 2015q2. Figures mark 2015q2 with a vertical line and the baseline point is zero by construction. We assess the parallel trends assumption using both a visual inspection of the pre-2015q2 coefficients, where estimates

near zero with confidence intervals covering zero are reassuring, and a joint F-test of the pre-treatment coefficients, reported under each figure.

Event-study estimates for +5% versus 0% show a sharp reallocation into the registered VAT chain after 2015q3, first via larger purchases per registered supplier, then via gradual entry of new registered suppliers. Following the reform, inputs from registered suppliers for the +5% group jump relative to 0%, with the biggest gains in the first 1-2 years before settling into a smaller but persistently positive plateau. On the extensive margin, the registered supplier base expands gradually, with many months after the second year featuring multiple new links. On the intensive margin, average purchases per registered supplier surge in the first year and remain above baseline, though attenuated. By contrast, average purchases from pre-treatment partners display only a short-lived uptick in the second and third post-reform quarters and no persistent rise. Taken together, the adjustment starts by increasing spending per registered supplier, then continues through gradual network expansion, with overall effects that decline in magnitude over time. Pre-trends are broadly flat for the reallocation and extensive-margin outcomes, while the intensive-margin outcomes show some evidence of pre-trend violations in the joint tests, so those panels are interpreted with caution.

Figure 6: Reallocation of inputs towards the VAT chain, +5% exposure vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on inputs from registered suppliers. Treated: +5% exposure bin. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Shaded area shows 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta)-1)$*. Pre-treatment leads joint tests: all pre* $p = 0.134$*, last 4 pre (1y)* $p = 0.068$*. N firms = 4,708. N obs = 361,878.*

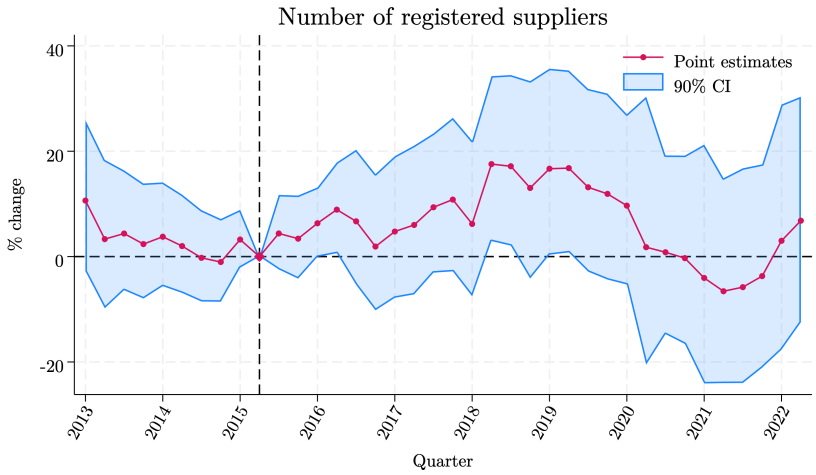
Figure 6 shows a sharp post-reform shift of purchasing towards continuously registered suppliers among exposed buyers. Starting in 2015q3, inputs from registered suppliers increase for the +5% exposure group relative to the 0% group, with point estimates typically in the 24-59% range during 2015-2017 and several quarters statistically different from zero at the 90% level. The rise is largest in the first one to two years after the reform.

The subsequent moderation in the series should be interpreted as an attenuation of relative differences rather than a reversal of reallocation. The event-study coefficients capture how purchases from registered suppliers evolve for exposed buyers relative to buyers with zero exposure. A large initial increase is consistent with a one-off re-optimisation of sourcing away from suppliers that exit the VAT system and toward creditable suppliers in the registered chain. Once this rewiring is largely completed, exposed

buyers no longer need to continue increasing registered purchases faster than the control group, so the relative effect naturally peaks and then settles at smaller, often less precisely estimated, positive values.

To understand how buyers implement this reallocation, the next subsections decompose the response into extensive- and intensive-margin adjustments. In particular, the post-reform dynamics in Figure 6 could reflect short-run scaling up of purchases among existing registered suppliers, gradual entry into new registered relationships, or both. We therefore examine (i) whether exposed buyers expand the stock and inflow of registered suppliers (extensive margin), and (ii) whether they raise spending per supplier and/or rely more heavily on pre-reform partners (intensive margin).

Figure 7: Extensive margin effects (registered suppliers, stock), +5% exposure vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on number of registered suppliers. Treated: +5% exposure bin. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Shaded area shows 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta)-1)$. *Pre-treatment leads joint tests: all pre* $p = 0.607$, *last 4 pre (1y)* $p = 0.633$. *N firms = 4,708. N obs = 362,127.*

Figure 8: Extensive margin effects (new suppliers, flow), +5% exposure vs 0%

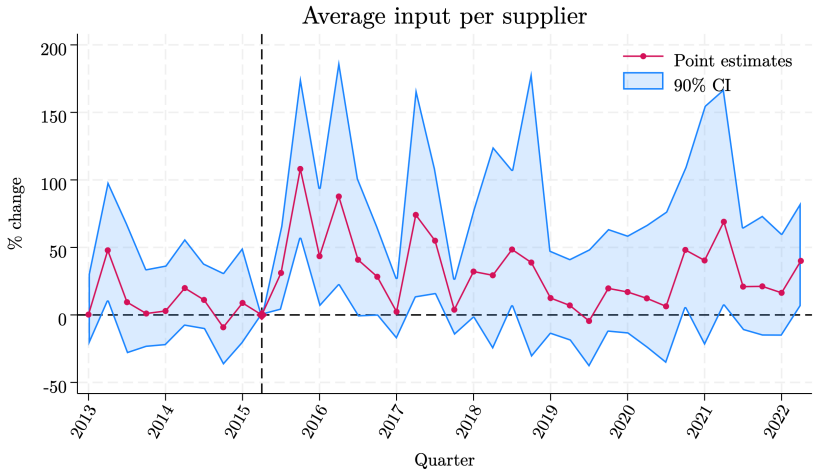
Notes: *Event-study estimates of the 2015 VAT threshold reform effect on number of new registered suppliers. Treated: +5% exposure bin. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Shaded area shows 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$*. Pre-treatment leads joint tests: all pre* $p = 0.825$*, last 4 pre (1y)* $p = 0.770$*. N firms = 4,416. N obs = 300,390.*

A key question is whether the post-reform reallocation is achieved by *deepening* relationships with existing registered suppliers, or by *rebuilding* the supplier base through entry into new registered links. Figures 7 and 8 show that the extensive-margin response is gradual, consistent with adjustment costs in search, screening, and relationship formation.

Figure 7 indicates a steady expansion in the stock of registered suppliers for exposed buyers relative to the 0% group. The effect accumulates over time and reaches roughly 17-18% around 2018-2019, with several mid-period quarters statistically above zero at the 90% level. Figure 8 complements this pattern by showing that the inflow of new registered suppliers rises with a delay: there is little immediate jump in entry, but from about year one onward the +5% group adds substantially more new registered suppliers per month, with estimates frequently in the 35-44% range during 2017-2019. Both series soften around 2020, consistent with a broader disruption to supplier search and matching during the pandemic period.

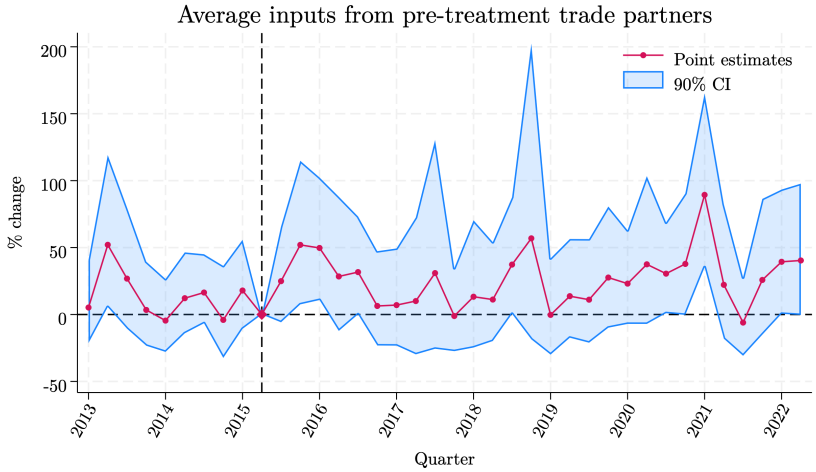
Taken together, these results suggest that the initial post-reform reallocation does not rely solely on switching one supplier for another at the moment of the shock. Instead, exposed buyers gradually rebuild their registered supplier network, sustaining the shift towards the VAT chain even after the immediate, mechanical reallocation from deregistrants has played out.

Figure 9: Intensive margin effects (average input per supplier), +5% exposure vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on average input per supplier. Treated: +5% exposure bin. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Shaded area shows 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta)-1)$*. Pre-treatment leads joint tests: all pre* $p = 0.002$*, last 4 pre (1y)* $p = 0.181$*. N firms = 4,708. N obs = 361,878.*

Figure 10: Intensive margin effects (average input from pre-treatment partners), +5% exposure vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on average inputs from pre-treatment trade partners. Treated: +5% exposure bin. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Shaded area shows 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta)-1)$*. Pre-treatment leads joint tests: all pre* $p = 0.004$*, last 4 pre (1y)* $p = 0.030$*. N firms = 4,708. N obs = 331,069.*

The intensive-margin results clarify how exposed buyers absorb the shock while the extensive-margin rebuild is still underway. If supplier search and onboarding is slow, buyers may respond initially by concentrating purchases among a smaller set of continuously registered suppliers, potentially at the cost of reduced variety or weaker price competition. Figures 9 and 10 show that this short-run adjustment operates primarily through higher spending per registered supplier, with only transitory reliance on legacy relationships.

Figure 9 shows a pronounced increase in average input per registered supplier for the +5% group

relative to 0%, concentrated in the first one to two years after the reform. Point estimates are typically in the 31-108% range during 2016-2017 for all quarters in that window statistically above zero at the 90% level. The estimates remain generally positive thereafter but are smaller and less precise, consistent with an early phase of intensive-margin concentration that gradually attenuates as buyers rebuild their supplier base. Because the joint test of all pre-treatment leads rejects at conventional levels ($p = 0.002$), we interpret the post-reform dynamics in Figure 9 with caution and place more weight on the timing and broad pattern.

By contrast, Figure 10 indicates that increased reliance on pre-treatment partners is, at most, a short-lived response. There is an uptick immediately after the reform, with estimates around 50-52% in the second and third post-reform quarters, which are statistically above zero at the 90% level, but the effect fades quickly and coefficients are subsequently noisy and close to zero on average. Here too, pre-treatment leads are jointly significant (all pre $p = 0.004$; last four pre $p = 0.030$), so the evidence is best read as suggestive of brief, short-run stabilisation through existing relationships rather than a persistent deepening of legacy ties.

Overall, the intensive-margin evidence supports a two-stage adjustment: exposed buyers initially cope with the loss of creditable links by concentrating purchases among continuing registered suppliers (and, briefly, among established partners), then progressively shift from concentration to diversification as new registered supplier relationships are formed on the extensive margin.

5 Discussion and conclusion

A recurring message in the VAT literature is that registration thresholds shape firm behaviour through two distinct channels. The first is a within-firm channel: thresholds create discontinuities that encourage bunching, misreporting, or organisational responses near the cutoff. The second is a between-firm channel: because input tax credits require valid invoices, VAT status changes the effective price of trading links that cross regimes, generating partial segmentation in buyer-supplier networks. A complementary body of work highlights that the invoice-credit mechanism can also propagate compliance incentives along supply chains, producing paper-trail externalities and strategic complementarities in formality decisions. Our results speak directly to this second and third set of mechanisms, and they do so in a setting where a policy-induced deregistration shock provides a clean impulse to the network.

Relative to existing evidence, the main contribution of the results is to quantify the time profile and margins of network adjustment after a threshold change that forces suppliers out of the VAT system, holding buyers' own VAT status fixed. Prior work using transaction data typically documents that VAT status affects partner choice and induces economically meaningful reallocation towards registered counterparts. Our estimates show that a mass deregistration episode triggers a similarly strong (and in our case, sharp and immediate) reallocation of purchases towards continuously registered suppliers among exposed buyers. In the short run, the adjustment operates primarily through the intensive margin, consistent with buyers responding quickly by scaling up procurement along surviving, creditable links while search and onboarding frictions delay the formation of new relationships. Over longer horizons, the response shifts towards the extensive margin, as exposed buyers gradually expand their stock of registered suppliers and increase inflows of new registered suppliers. This two-stage pattern is consistent with a simple network-rewiring interpretation: a threshold reform changes the set of creditable links, buyers first stabilise production by concentrating demand among available registered suppliers, and only subsequently rebuild and diversify the supplier base once new matches are formed.

These dynamics also connect to the compliance-spillover literature that emphasises invoice trails and third-party verification. Evidence from VAT enforcement reforms shows that strengthening invoice incentives can shift reporting and trading patterns by increasing the value of being inside the credit chain. Our results complement this perspective from the opposite direction: when suppliers are pushed out of the credit chain, downstream buyers reorganise in ways that plausibly reduce demand for newly uncreditable suppliers, potentially weakening their prospects for formalisation and amplifying segmentation.

Concretely, the results show a clear reallocation of inputs towards the VAT chain for buyers most exposed to eventual deregistrants. Following the 2015 reform, firms in the +5% exposure group increase purchases from registered suppliers markedly in the first 1-2 years, with event-study effects typically in the 24-59% range. This aggregate shift is driven first by a sharp rise in average purchases per registered supplier, often in the 31-108% range over the first six post-reform quarters, and only briefly by higher spending on pre-treatment partners, which peaks around 52% in the second quarter. At longer horizons, exposed buyers expand their registered networks: the stock of registered suppliers is roughly 17-18% higher in 2017-2020 and the monthly inflow of new registered suppliers is frequently 35-44% higher.

Taken together, the evidence points to a two-stage adjustment. In the short run, exposed firms primarily offset the deregistration shock by buying more per registered supplier. Over time, they add new registered suppliers and spread purchases across more links, so the intensive-margin boost declines while the extensive margin rises, leaving a smaller but persistent net reallocation towards the VAT chain.

The results indicate that threshold policy can rewire buyer-supplier networks. Raising thresholds reduces administrative load, yet it can fragment the VAT chain, push purchases away from small suppliers, and concentrate demand on continuously registered firms. Policymakers should therefore weigh administrative savings against network disruption, and anticipate short-run intensive-margin adjustments that

precede slower network rebuilding on the extensive margin.

Several design options can mitigate disruption while preserving administrative gains. First, phased or pre-announced threshold changes, coupled with transition windows in which buyers can claim input credits on legacy contracts may smooth the initial shock. Second, promoting voluntary registration for small suppliers that are central to registered buyers, for example with simplified filing, faster refunds, or reduced filing frequency, could lower fixed compliance costs without eroding the integrity of the credit chain. Third, pairing the threshold changes with digital invoicing and third-party reporting, would make it easier for buyers to find and verify compliant suppliers and for the administration to monitor emerging supply gaps.

This study and its results have some limitations, notably incomplete coverage of off-VAT purchases, overlapping shocks after 2020, and our limited ability to track deregistered firms that later re-register. The analysis measures reallocation within the observable VAT chain, not the full universe of sourcing. Schedule 2 only records purchases from VAT-registered suppliers, so we cannot see post-reform purchases from deregistered or never-registered firms. As a result, the estimated effects speak to how buyers rebalanced towards continuously registered suppliers, not to net changes in the composition of total inputs. In addition, the period after March 2020 overlaps with the COVID-19 shock, and from 2021 the rollout of e-invoicing (EFRIS) coincides with renewed registration and improved reporting. These overlapping changes may constitute a structural break in both measurement and behaviour, so post-2020 estimates should be interpreted with particular caution. A further limitation is our inability to track deregistered suppliers' status after the 2015 reform. The VAT registry available to us contains a single observation per firm, with entry and exit dates to VAT, so we cannot observe subsequent re-registration events, whether voluntary or mandatory after crossing the turnover threshold. Two misclassification risks follow. First, data constraints prevent us from identifying if a 2015 deregistrant later re-registers under the same tax identifier, our coding still labels it as part of the "deregistrant cohort", so purchases from that supplier are excluded from "registered supplier" totals. Second, if a deregistrant re-enters under a new legal entity or identifier, it will appear in our data as a "new" supplier, mechanically inflating extensive-margin measures and potentially overstating network rebuilding.

We are pursuing four extensions that are designed to sharpen identification, broaden measurement beyond the observable VAT chain, and connect the network reallocation to richer economic mechanisms. First, on the data side, we are working to incorporate information on non-VAT purchases so that we can measure total sourcing and the VAT versus non-VAT composition over time. In parallel, we plan to map input types and substitution patterns using sector classifications and invoice description strings, so that we can characterise whether buyers replace deregistered suppliers with close input substitutes, switch across product categories, or change the mix of intermediates in ways consistent with higher search costs or quality downgrading. Second, we will construct and validate measures of re-registration and firm exit for the deregistrant cohort. This is essential both for interpretation and for avoiding misclassification. If some deregistrants later re-enter the VAT system, or if a subset exit altogether, these trajectories change the economic consequences of "deregistration" and help distinguish persistent network segmentation from temporary compliance transitions. Measuring these dynamics will also allow us to quantify whether demand losses induced by the reform are concentrated among firms that subsequently disappear from the administrative data versus firms that remain active but outside the VAT chain. Third, on the modelling and identification side, we will move beyond discrete exposure bins to estimate dose-response relationships in exposure to deregistration shocks. In particular, we plan to implement generalised propensity score approaches and difference-in-differences designs with continuous treatment to recover how the magnitude and timing of network adjustment scale with baseline reliance on suppliers that eventually deregister. Fourth, to assess external validity, we will extend the analysis to other deregistration episodes in the registry. Replicating the empirical design across additional deregistration waves will help establish which

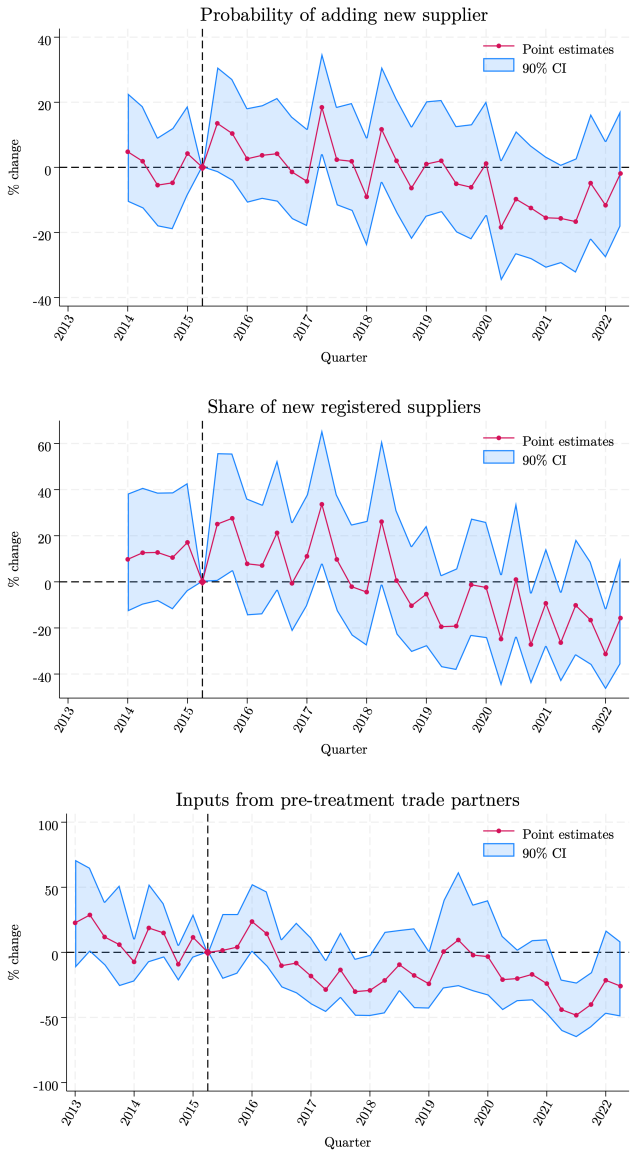
features of the dynamics are specific to the 2015 threshold increase versus characteristic of deregistration shocks more generally. It will also allow us to test whether the two-stage pattern is robust across contexts with different macro conditions, enforcement intensity, and digital compliance capacity.

Beyond this analysis, we still lack empirical quantification of the broader network spillovers of VAT threshold policy. Key questions remain unanswered. How large are the indirect effects on revenues and welfare when a threshold change induces some firms to join or leave the VAT system? Who are the winners and losers, and which types of firms benefit from changing VAT thresholds? Does a higher threshold inadvertently encourage the formation of large informal networks of firms that transact outside the VAT chain? Conversely, could a lower threshold bring more firms into the tax net and thereby strengthen inter-firm compliance links? These outcomes are hypothesized but not yet rigorously measured. In sum, the current literature provides only an initial glimpse at how VAT threshold policies reverberate through production networks. Many facets of firm-to-firm interactions under different threshold regimes are poorly understood empirically.

A Additional results: secondary outcomes

1. **Probability of adding a new supplier** (new_sup_dum$_{it}$): indicator equal to 1 if buyer i adds at least one new registered supplier in month t, and 0 otherwise. A registered seller j is "new" for buyer i at month t if j is absent from i's first twelve months $\{t_{0i}, t_{0i}+1, \ldots, t_{0i}+11\}$ and first supplies i at t thereafter.
2. **New supplier share** (new_sup_sh$_{it}$): share of new suppliers among all registered suppliers.
3. **Inputs from pre-treatment trade partners** (inp_pre_partner_w$_{it}$): total level of inputs (millions of UGX) that buyer i purchases in month t from suppliers whose first trade with i was before 2015m7.

Figure 11: Event-study estimates, 1-yearly exposure, +5% vs 0%



Quarterly event-study PPML estimates for buyers with +5% exposure versus buyers with 0% exposure. Baseline 2015q2 is normalized to zero and 2015q3 is the first post-reform quarter. The y-axis reports changes relative to 2015q2. Pre-2015q2 coefficients inform pre-trend checks.

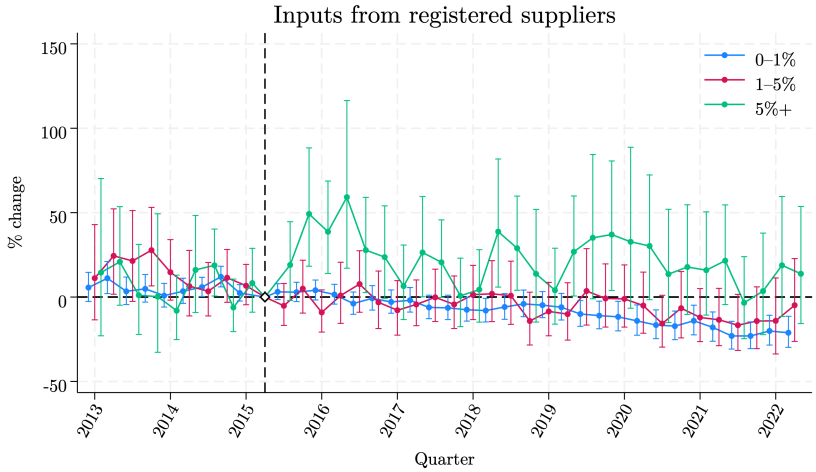
B Multi-bin exposure (0-1%, 1-5%, and +5% vs 0%)

As a robustness and heterogeneity check, we also estimate a multi-bin version of the event-study model. Using the exposure-bin definition $B_i \in \{0, 1, 2, 3\}$ with $B_i = 0$ for 0%, $B_i = 1$ for 0–1%, $B_i = 2$ for 1–5%, and $B_i = 3$ for +5%, we replace Exposure_i with a saturated set of indicators $\{\mathbb{1}\{B_i = 1\}, \mathbb{1}\{B_i = 2\}, \mathbb{1}\{B_i = 3\}\}$, leaving $B_i = 0$ as the reference group. We report the three event-time coefficient series $\{\beta_{q1}, \beta_{q2}, \beta_{q3}\}$, which trace the dynamics for the 0–1%, 1–5%, and +5% bins, each relative to the 0% group and relative to the reference quarter 2015q2.

The multi-bin exposure results show that the +5% group reallocates strongly towards the registered VAT chain after 2015q3, first via a first-year surge in average purchases per supplier then via gradual supplier entry, whereas the 1-5% group shows modest and imprecise responses, and the 0-1% group has near-zero or negative effects with non-parallel pre-trends. In other words, effects rise with exposure across the bins, and visual pre-trends assessments support focusing on taking the +5% group as treatment relative to the 0% control group.

For the +5% group, inputs from registered suppliers increase by roughly 24 to 59% in the two years following the reform. This increase appears first in the average input per supplier, which jumps by about 33 to 107% in the first post-reform year, and average inputs from pre-treatment partners show a short-run increase of about 45 to 52% in quarters 2 to 3. Subsequently, the monthly inflow of new registered suppliers is frequently 23 to 43% higher in later periods, and the number of registered suppliers about 18 to 19% higher. In contrast, the 1-5% bin displays modest and often imprecise responses. For the 0-1% bin, pre-treatment coefficients exhibit a mild downward drift with several quarters statistically different from zero, making parallel trends difficult to defend, and post-reform effects are near zero or negative. Taken together, the adjustment is concentrated among the most exposed firms, operating first through larger orders per registered supplier and later through gradual network expansion, consistent with highly exposed firms adjusting first on the intensive margin and then on the extensive margin.

Figure 12: Reallocation of inputs towards the VAT chain, 0-1%, 1-5%, and +5% vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on inputs from registered suppliers. Treated: 0-1%, 1-5%, and 5%+ exposure bins. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Error bars show 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$. *Pre-treatment leads joint tests (all pre, by bin): 0-1%* $p = 0.008$, *1-5%* $p = 0.312$, *5%+* $p = 0.215$. *Last 4 pre (ly, by bin): 0-1%* $p = 0.011$, *1–5%* $p = 0.741$, *+5%* $p = 0.104$. *N firms = 5,321. N obs = 418,454.*

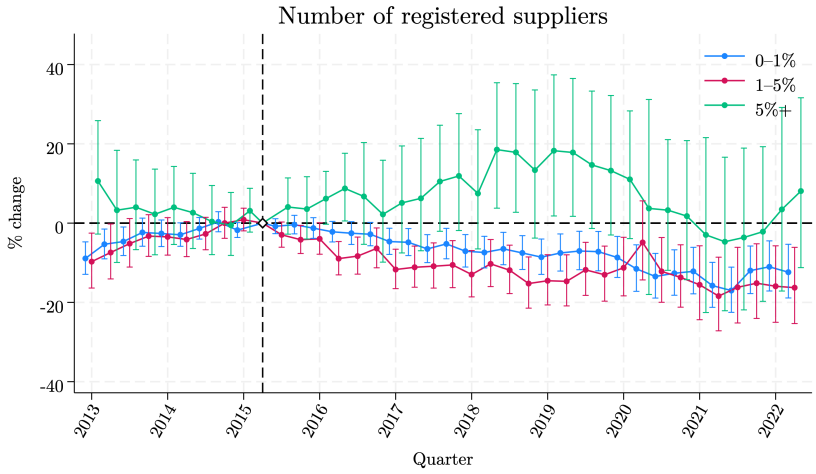
Reallocation rises with exposure: after 2015q3 the +5% bin shifts strongly towards registered suppli-

ers, the 1–5% bin shows modest and mostly imprecise effects with pre-trends that are broadly supportive (joint tests do not reject), and the 0–1% bin is near zero or negative with weaker pre-trend credibility.

Regarding Figure 12, the pre-period coefficients for the 0-1% bin are visually small, but the joint tests reject (all pre $p = 0.008$; last four pre $p = 0.011$), so parallel trends are less credible for that bin; by contrast, pre-treatment coefficients for the +5% bin fluctuate around zero with 90% intervals generally covering zero. For the 1-5% bin, the pre-treatment coefficients are generally close to zero and the joint tests do not reject (all pre $p = 0.312$; last four pre $p = 0.741$), so the pre-trend evidence is more supportive than for the 0-1% bin.

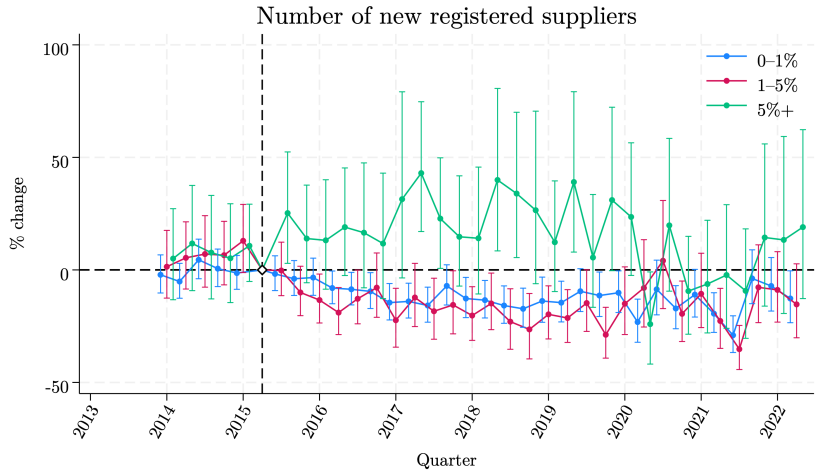
After 2015q3, the +5% series rises sharply, between 24-59% through 2016-2019, with several quarters statistically above zero at the 90% level, then gradually attenuates. The 1-5% series is modest and mostly indistinguishable from zero, while the 0-1% series stays near zero in the early post period and turns slightly negative from 2020 onward; for both lower-exposure bins, the 90% intervals largely cover zero. The response is largest for +5%, muted for 1-5%, and negligible or negative for 0-1%, which aligns with the interpretation that firms most reliant on eventual deregistrants faced the largest supply shock and therefore reallocated purchases most strongly towards the registered VAT chain.

Figure 13: Extensive margin effects (registered suppliers, stock), 0-1%, 1-5%, and +5% vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on number of registered suppliers. Treated: 0-1%, 1-5%, and 5%+ exposure bins. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Error bars show 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$. *Pre-treatment leads joint tests (all pre, by bin): 0-1%* $p = 0.019$, *1-5%* $p = 0.532$, *5%+* $p = 0.659$. *Last 4 pre (ly, by bin): 0-1%* $p = 0.101$, *1–5%* $p = 0.470$, *+5%* $p = 0.695$. *N firms = 5,321. N obs = 418,742.*

Figure 14: Extensive margin effects (new suppliers, flow), 0-1%, 1-5%, and +5% vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on number of new registered suppliers. Treated: 0-1%, 1-5%, and 5%+ exposure bins. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Error bars show 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$. *Pre-treatment leads joint tests (all pre, by bin): 0-1%* $p = 0.434$, *1-5%* $p = 0.723$, *5%+* $p = 0.892$. *Last 4 pre (ly, by bin): 0-1%* $p = 0.305$, *1–5%* $p = 0.668$, *+5%* $p = 0.811$. *N firms = 5,028. N obs = 350,068.*

Extensive margin adjustment is concentrated among the +5% group: after 2015q3 their registered-supplier stock rises about 13-19% and monthly new-supplier inflows about 23-43% in 2018-2019, while lower bins show little change; for supplier counts, the 0-1% bin has weaker pre-trend credibility, whereas the 1-5% and +5% bins show more supportive pre-trend evidence (joint tests do not reject).

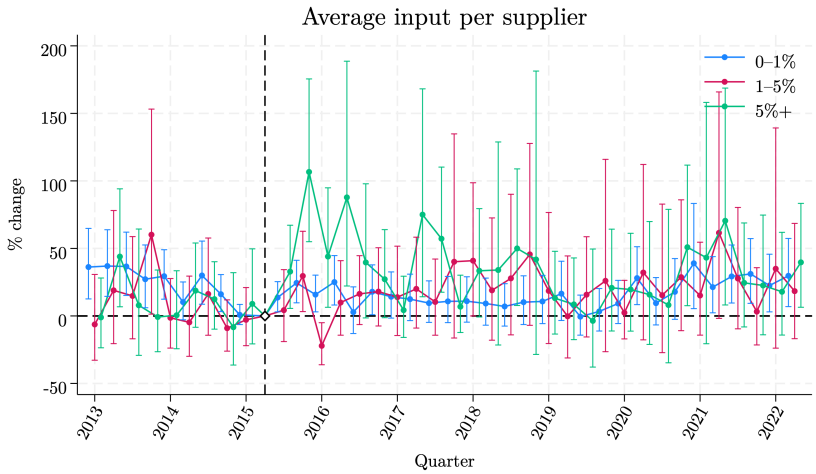
Prior to treatment, Figure 13 shows weaker pre-trend credibility for the 0-1% group (all-pre joint test rejects), whereas the 1-5% and +5% series fluctuate around zero with intervals generally overlapping zero and joint tests that do not reject.

Prior to treatment, Figure 13 shows significantly different pre-trends for the 0-1% and 1-5% groups relative to the 0% control group in terms of the monthly number of registered suppliers, yet the +5% series has point estimates fluctuating around zero with confidence intervals overlapping zero. For Figure 14 focusing on the number of new registered suppliers, pre-trends seem to hold up for all three positive exposure groups relative to the control group.

After 2015q3, the +5% bin gradually expands its registered network: the stock of registered suppliers (Figure 13) is roughly 13-19% higher during 2018-2019, and the monthly number of new registered suppliers (Figure 14) is frequently 23-43% higher, with several quarters statistically above zero at the 90% level. Both series soften around 2020. By contrast, the 0-1% and 1-5% bins show little change or mild declines. In short, registered network expansion is concentrated among the most exposed firms,

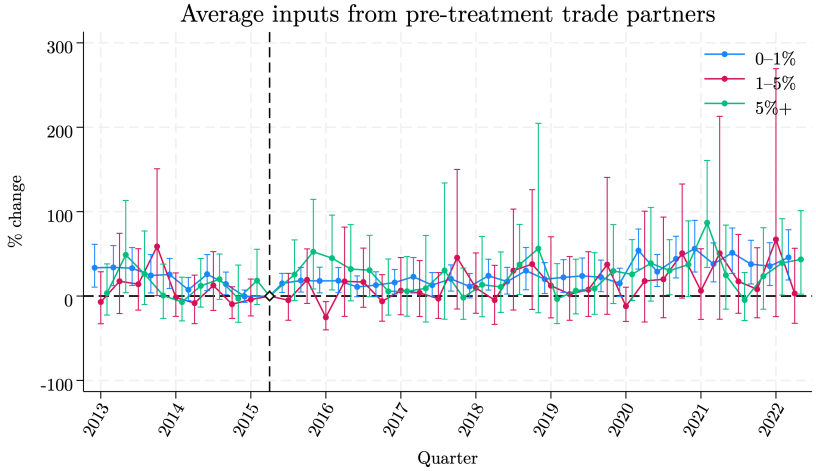
with the timing of the response coinciding with 2-5 years post-treatment.

Figure 15: Intensive margin effects (average input per supplier), 0-1%, 1-5%, and +5% vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on average input per supplier. Treated: 0-1%, 1-5%, and 5%+ exposure bins. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Error bars show 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$. *Pre-treatment leads joint tests (all pre, by bin): 0-1%* $p = 0.026$, *1-5%* $p = 0.272$, *5%+* $p = 0.018$. *Last 4 pre (ly, by bin): 0-1%* $p = 0.061$, *1–5%* $p = 0.721$, *+5%* $p = 0.171$. *N firms = 5,321. N obs = 418,454.*

Figure 16: Intensive margin effects (average input from pre-treatment partners), 0-1%, 1-5%, and +5% vs 0%



Notes: *Event-study estimates of the 2015 VAT threshold reform effect on average inputs from pre-treatment trade partners. Treated: 0-1%, 1-5%, and 5%+ exposure bins. Control: 0% exposure bin. Exposure: 1-year exposure measure. Points are PPML estimates using a firm-month panel with quarter indicators. Error bars show 90% CI, SE clustered at firm level. Baseline: 2015q2 normalised to zero. Vertical dashed line marks the last pre-reform period. Fixed effects: firm, sector×month, district×month. Effects are percent changes,* $100(\exp(\beta) - 1)$. *Pre-treatment leads joint tests (all pre, by bin): 0-1%* $p = 0.041$, *1-5%* $p = 0.234$, *5%+* $p = 0.009$. *Last 4 pre (ly, by bin): 0-1%* $p = 0.091$, *1–5%* $p = 0.758$, *+5%* $p = 0.019$. *N obs = 385,635.*

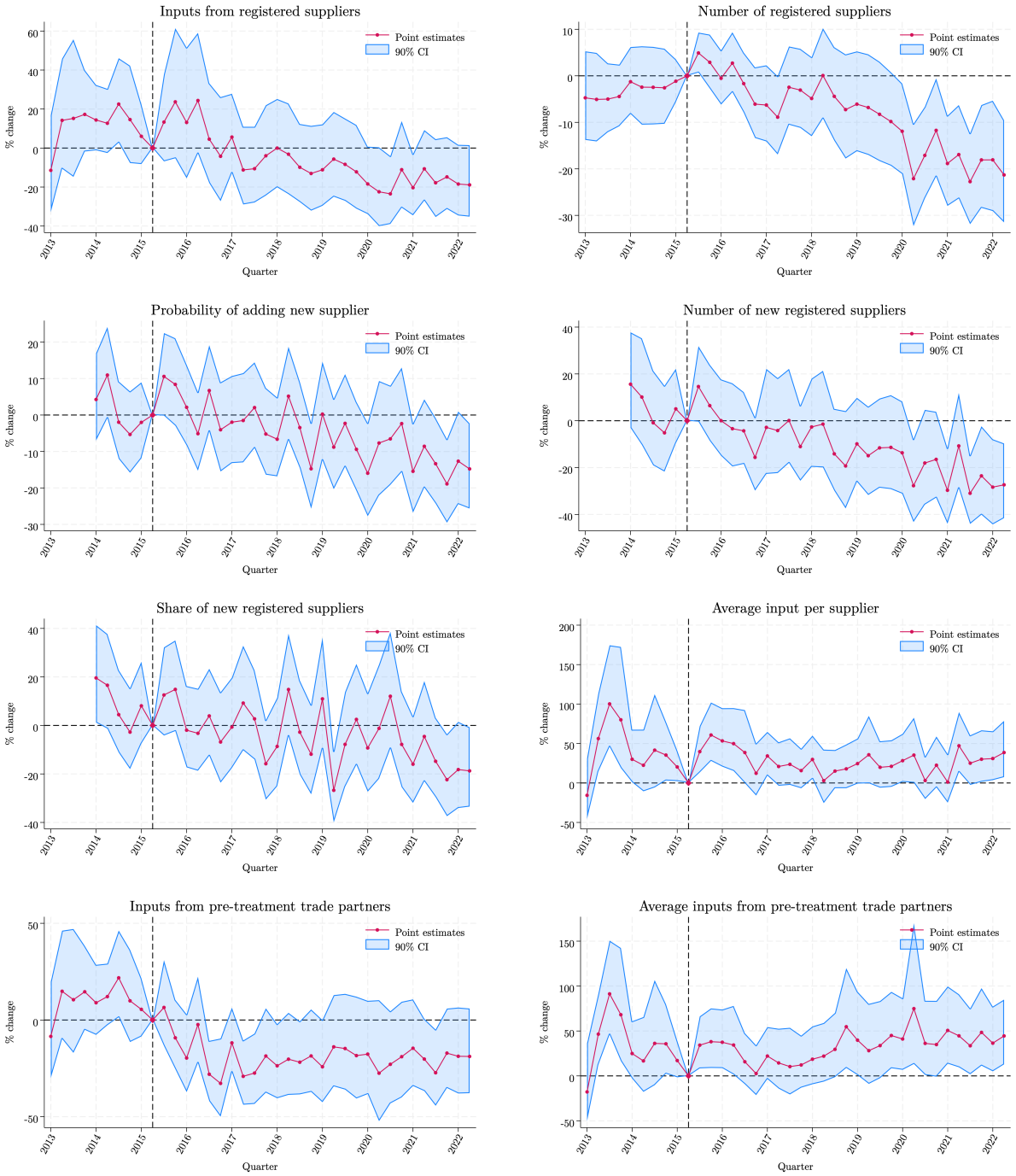
Intensive margin responses are more pronounced in the +5% group: average purchases per supplier jump about 33-107% in the first year and purchases from pre-treatment partners rise briefly about 45-52%, but the joint tests indicate weaker pre-trend credibility for these intensive-margin outcomes (so we interpret magnitudes with caution and emphasise timing and patterns).

Regarding Figures 15 and 16, pre-period coefficients for the 1-5% bin are generally close to zero and the joint tests do not reject, whereas the 0-1% bin shows weaker pre-trend credibility and the +5% bin also exhibits joint-test rejections for these intensive-margin outcomes; we therefore treat these panels as suggestive about dynamics and sequencing rather than as clean estimates of levels.

After the reform, average input per supplier (left) rises sharply for the +5% bin, often in the 33-107% range over the first four quarters. Average inputs from pre-treatment partners (right) also increase in the short run, with about 45-52% statistically significant gains in the second and third post-reform quarters. Overall, relative to the 0% group, the +5% firms display a pronounced short-run intensive margin response, concentrated in larger orders per supplier and a brief increase in purchases from existing partners.

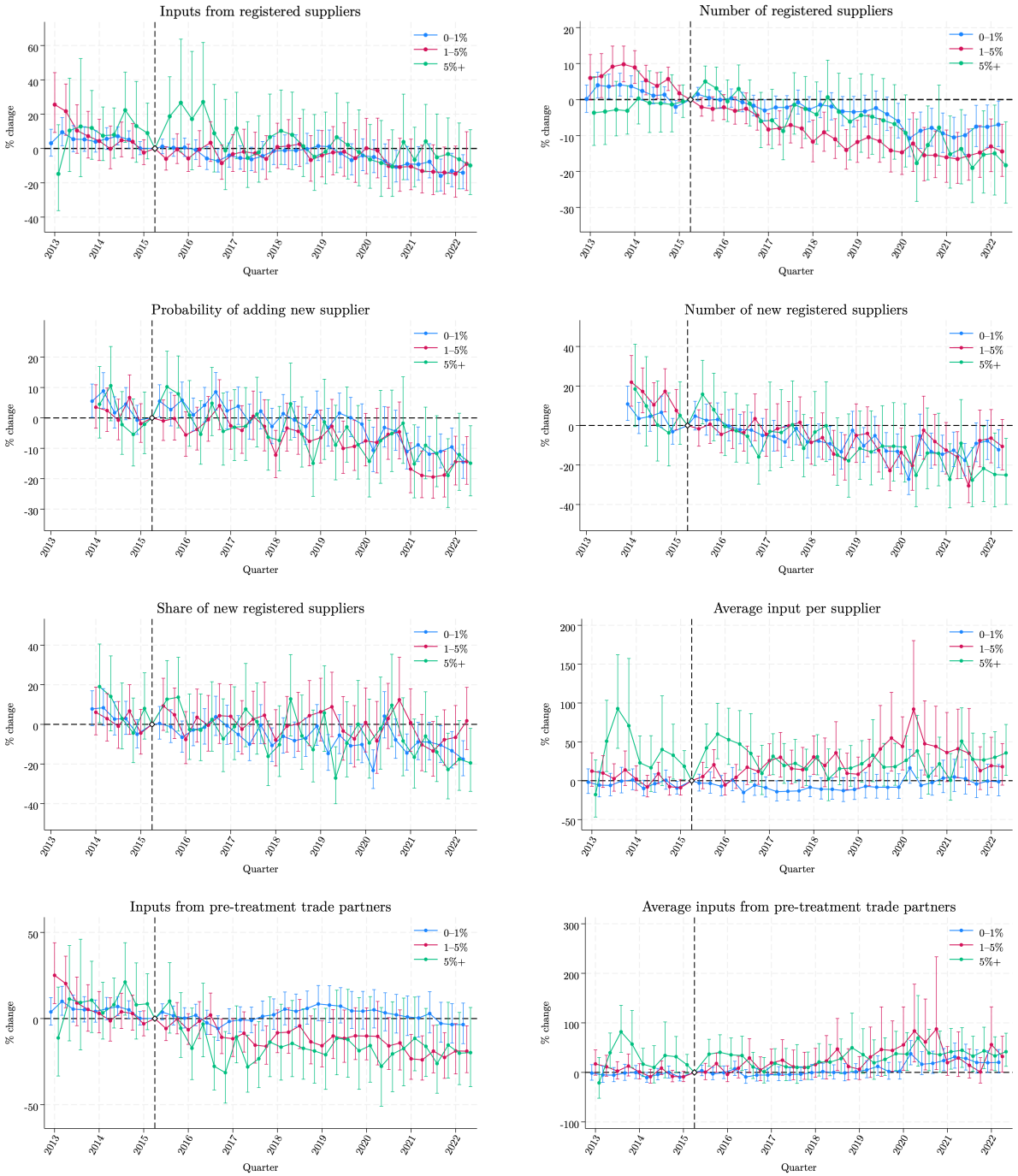
C Additional results: 2.5-year exposure

Figure 17: Event-study estimates, 2.5-yearly exposure, +5% vs 0%



Quarterly event-study PPML estimates for buyers with +5% exposure versus buyers with 0% exposure. Baseline 2015q2 is normalized to zero and 2015q3 is the first post-reform quarter. The y-axis reports changes relative to 2015q2. Pre-2015q2 coefficients inform pre-trend checks.

Figure 18: Event-study estimates, 2.5-yearly exposure, (0-1%, 1-5%, and +5% vs 0%)



Quarterly event-study PPML estimates for buyers in three positive exposure bins (0-1%, 1-5%, and +5%) relative to buyers with 0% exposure. Baseline 2015q2 is normalized to zero and 2015q3 is the first post-reform quarter. The y-axis reports changes relative to 2015q2. Pre-2015q2 coefficients inform pre-trend checks.

D Filing status of deregistrants

This annex documents where deregistered firms appear across administrative datasets, and whether available data indicate turnover above or below the statutory threshold prior to the July 2015 reform.

Taken together, Tables 2 and 3 show that most deregistered firms haven't been consistently filing monthly VAT returns. Only 21% of deregistrants ever filed a Schedule 1 (seller returns) and only 30% of them have ever filed buyer returns. A higher proportion of deregistrants do however appear in the filings of other firms, either as buyers in seller returns (64%), or as sellers in buyer returns (36%).

Table 2: Appearance of Deregistered Firms in Different Data Sources

	Seller in Seller Returns	Buyer in Seller Returns	Buyer in Buyer Returns	Seller in Buyer Returns	In VAT Panel
Appears	574 (20.7%)	1780 (64.3%)	831 (30.0%)	1004 (36.3%)	2186 (79.0%)
Doesn't	2194 (79.3%)	988 (35.7%)	1937 (70.0%)	1764 (63.7%)	582 (21.0%)
Total	2768	2768	2768	2768	2768

Table 3: Cross-tabulation of Firm Appearances in Schedule 1 and Schedule 2 Data

Has filed seller returns (Sch 1)	Has filed buyer returns (Sch 2)		Total
	No	Yes	
No	1,771	423	2,194
Yes	166	408	574
Total	1,937	831	2,768

URA assesses eligibility for deregistration using two turnover checks prior to July 2015: a three-month sum below 37.5 million UGX and a one-year sum below 150 million UGX. Using the VAT Panel for firms that appear at least once (79%), most deregistrants lack observable turnover near the assessment window (strikingly, 64% of these firms never filed a single VAT return), and very few exceed the threshold. Of the 2,186 firms observed in the panel, 47% have missing turnover in the three months prior to July 2015 and 35% have missing turnover for the prior year. Only 3% exceed the three-month threshold and 2.2% exceed the annual threshold.

Table 4: Turnover Status of Firms in the VAT Panel

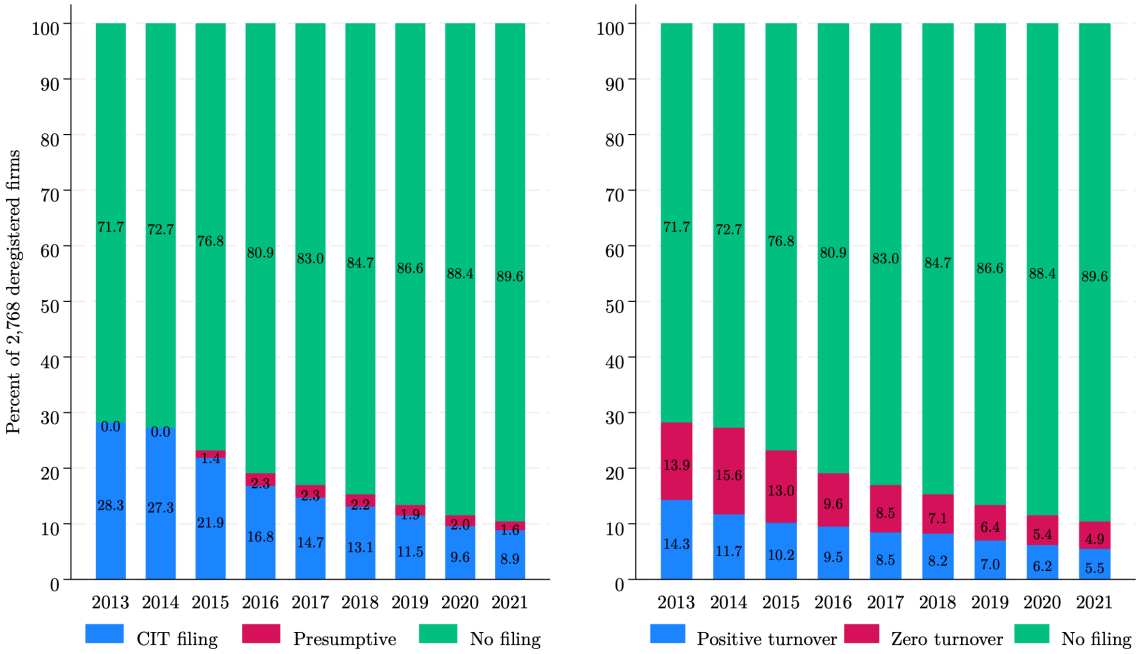
	Three-month missing (# / %)	One-year missing (# / %)	Three-month zero (# / %)	One-year zero (# / %)	Three-month above threshold (# / %)	One-year above threshold (# / %)
Yes	1021 (46.7%)	757 (34.6%)	919 (42.1%)	1092 (50.0%)	67 (3.1%)	49 (2.2%)

Table 5: Cross-tabulation of Firm Appearances in Presumptive and CIT Data

Appears in the Presumptive data	Appears in the CIT data		Total
	No	Yes	
No	1,567	1,028	2,595
Yes	64	109	173
Total	1,631	1,137	2,768

To gauge continued production, deregistrants are matched to corporate income tax (CIT) and Presumptive returns. Most firms (57%) don't appear to have submitted a single income statement. The

Figure 19: Income filing status of deregistered firms by year



Notes: *The left-hand panel indicates whether a firm has filed an income tax return (either CIT or Presumptive) in a given year, while the right-hand panel indicates whether a firm has indicated a positive turnover.*

available evidence indicates that fewer than 15% of firms deregistered in the 2015 reform report positive sales in any year of the study period. While a small share continue filing CIT or Presumptive returns, most either cease income reporting or file only zero-turnover declarations, suggesting no observable production activity before or after deregistration.

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