Leveraging Trading Networks to Improve Tax Compliance: Experimental Evidence from Uganda*

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Abstract

We use information on firms' trading networks from VAT return data to design a randomized tax compliance intervention in Uganda. In treated pairs, either the seller, the buyer, or both receive letters listing discrepancies detected in past tax returns. The amendment rate is 22 percentage points higher in the treatment group, compared to 1.8% in the control group. We find spillover effects within treated firm pairs and in transactions with their untreated trading partners. Overall, there is a small increase in VAT liability for the amended returns. The intervention also leads to fewer discrepancies in subsequent tax declarations.

Keywords: tax enforcement; networks; firms; value-added tax (VAT); Uganda.
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1 Introduction

Trading networks play a substantial role in shaping economic outcomes (Acemoglu and Azar, 2020; Dhyne et al., 2020), notably firm revenue and growth. Business-to-business (B2B) transactions are therefore a major determinant of an economy's potential tax revenue. Importantly, not only do trading networks affect potential tax revenues through their effects on firms' output, but they also contribute to the visibility of taxable transactions, which facilitates enforcement. Indeed, recent research in public finance argues that the key to the high levels of tax compliance observed in advanced economies is the widespread existence of third-party information reporting (Kopczuk and Slemrod, 2006; Kleven et al., 2011, 2016; Jensen, 2022). This suggests that the more information authorities recover from different locations within the network, the better for tax enforcement. In particular, the value-added tax (VAT) has built-in mechanisms that facilitate tax enforcement because the seller and buyer in a transaction have opposing incentives to misreport. This is one of the arguments used for the widespread adoption of the VAT across the globe, including in countries with low administrative capacity (Keen and Lockwood, 2010). In many low and middle-income countries, the VAT was introduced with the explicit objective of helping achieve levels of tax revenue adequate for economic development.

Transaction-level data from VAT returns offers a uniquely rich mapping of firms' trading networks. However, in spite of this detailed information, there are still significant VAT compliance gaps, especially in countries with limited administrative capacity.[1] In these contexts, firms may be tempted to misreport on their VAT declarations with the expectation that they will not be detected and punished by regulatory authorities (Almunia et al., forthcoming).[2] There is limited evidence on how such trading networks can be used to improve tax compliance, with the notable exception of Pomeranz (2015).

We address this gap by providing experimental evidence on the effectiveness of using data on trading networks from VAT returns to increase tax compliance. We designed a randomized trial with the Uganda Revenue Authority (URA) making effective use of the information available from VAT returns. To the best of our knowledge, this is the first randomized tax intervention making use of such fine-grained information on taxpayer network both for the design of the experiment and the content of the message. The treatment consists of

[1] Available estimates of the VAT gap—the difference between expected and actual VAT revenues—range from about 10% in the European Union (Poniatowski et al., 2017) and 25% in Latin American countries, to 31-46% in Pakistan (Waseem, 2023) and up to 60% in Uganda (IMF, 2014).

[2] In earlier research we document widespread reporting discrepancies in B2B transactions between VAT-registered firms in Uganda (Almunia et al., forthcoming).

sending notification letters to a randomly chosen subset of trading partners, i.e., seller-buyer pairs, whose monthly VAT returns featured discrepancies. The letters notified either the seller, the buyer, or both firms that the revenue authority had developed a new method to detect misreporting, and listed up to three discrepancies from recent months with one specific trading partner. Discrepancies in this context are defined as cases where the seller reports a lower amount than the buyer in a given pair-month (labelled as "seller shortfall").[3] The letters request firms to amend their past returns promptly and to report accurately in the future.

The randomization procedure allows us to measure the effects of these letters on the reporting behavior of treated firm pairs, and also to examine spillover effects on their untreated trading partners. In the presence of network interference, simple randomization does not guarantee that treatment is assigned with equal probability, because more connected firms will be more exposed to the treatment indirectly through their trading partners (Aronow and Samii, 2017; Borusyak and Hull, 2022). To address this problem, we design a randomization procedure that selects a subsample of seller-buyer pairs separated from each other by at least two degrees in the network. Specifically, when a pair is selected for treatment, we remove all the trading partners of both the seller and buyer from the pool of eligible pairs. This avoids a situation where a firm is assigned to treatment multiple times with different trading partners. The procedure yields a "study sample" of 1,235 firm pairs which incurred discrepancies in the previous six months. We randomly assign each of these pairs to one of three treatment arms (20% each) or to a control group (40%). In each of the treatment arms, letters are sent to either the seller, the buyer, or both firms. Firms in the control group do not receive any communication from the revenue authority.

We conduct the empirical analysis in two broad steps. First, we analyze the impact of the letters on amendments of past returns, focusing on the behavior of treated pairs and also the spillover effects of the treatment on reporting with other trading partners. Second, we study the dynamic impact by examining the effects of the treatment on the subsequent reporting behavior of treated pairs.

We find a strong effect on amendments of past returns. Specifically, the probability that treated pairs amend the discrepancies mentioned in the letters increases by 22.1 percentage points (pp) across all treatment arms, from a baseline probability of 1.8% in the control group. This effect is largest (30.2pp) for the "buyer and seller" treatment group, and lowest for the "buyer only" group (11.1pp). If we only take into consideration amendments that fully

[3]Seller shortfall cases are suggestive of tax-evading behavior because they can potentially lead to lower net VAT liability for the firms involved. We provide more details in Section 2.

resolve the discrepancies, the combined effect of all treatments is lower at 11.5pp (15.7pp for the "buyer and seller" treatment), but still remains highly significant.

We examine the potential spillover effects of the letters at three levels: effects on discrepancies of the treated pairs *not* mentioned in the letters, effects on the member of the pair that did *not* receive the letter, and effects on transactions of treated firms with *other* untreated trading partners. For a subset of treated pairs with more than three discrepancies in the pre-treatment period, we analyze the impact on discrepancies not mentioned in the letters. We find significant positive effects on the probability of amendment, with a magnitude that is about half of that found for the discrepancies mentioned in the letters. We further explore which member within each pair actually amended their return, finding that sellers are more likely to amend regardless of who receives the letter. This is a clear indication of within-pair communication, because the effect on sellers is stronger even when the letter is only sent to the buyer. Finally, we find smaller but significant effects on the amendment rates in transactions of treated firms with other untreated trading partners. Overall, these amendments led to an estimated increase in VAT liability that is an order of magnitude larger than the cost of implementing the intervention. Thus, we conclude that the intervention was cost effective from the perspective of the revenue authority. These results illustrate the potential of exploiting trading networks to propagate improved tax reporting behavior.

When analyzing reporting behavior after the treatment, we find a 6.1pp (45%) significant increase in the probability of trading partners reporting matching amounts driven by a 13pp decrease in the probability of seller shortfall, which is sustained over ten months after treatment. This confirms that the intervention leads to sustained improved reporting behavior. We do not find significant effects on the seller shortfall amount (conditional on having seller shortfall), the transaction size, or the likelihood that the treated firm pairs continue reporting transactions together after the intervention. We also find a 1pp reduction in the probability of seller shortfall for treated firms with other trading partners, although this smaller effect fades out after five months. Despite this improvement in reporting behavior, we do not find a significant long-term effect on the reported VAT liability. One mechanism explaining this result is the fact that firms adjust their reporting on other margins, namely reporting lower sales to final consumers.

Our main contribution is to shed light on how the effects of tax enforcement interventions propagate through the network of firms.[4] In contrast to previous studies, the transaction-level

[4]de Paula and Scheinkman (2010) and Gadenne et al. (2022) study the reverse mechanism: how taxation affects the formation of trading networks. In our setting, we do not find a significant effect of the enforcement intervention on trade relationships within treated pairs.

data allows us to fully map the trading network both before and after the intervention.[5] The closest to our paper is Pomeranz (2015), who measures enforcement spillovers up the VAT chain through a letter experiment in Chile. In that study, the trading relationships are identified during an audit conducted after the letters were sent, so the structure of the network is not taken into account in the randomization process. Other recent studies find spillover effects of audit interventions on firms sharing the same tax preparers in Italy (Battaglini et al., 2019) and the United States (Boning et al., 2020), respectively. Drago et al. (2020) find evidence of spillovers in TV license fee payments by households in Austria and Cruces et al. (2022) find spillover effects in property tax compliance in Argentina but, in both cases, connections across taxpayers are based only on geographic proximity. In a non-experimental setting in South Africa, Lediga et al. (2022) find spillovers of business tax audits only in cases where firms share geographical proximity *and* trading links, although the latter are based on country-level input-output matrices because firm-to-firm transactions are not observed. To sum up, we contribute to this literature on tax enforcement and networks in two ways. First, by designing the experiment taking into account the existent network structure in order to account for potential bias due to interference. Second, by detecting spillovers across a variety of dimensions: between seller and buyer in treated pairs, across transaction months, and with other untreated partners.[6]

While empirical evidence on tax enforcement interventions has expanded recently, examples from low and lower-middle income are still scarce. Two examples of successful interventions are Shimeles et al. (2017), who find that threat and tax morale letters increased profits reported by businesses in Addis Ababa (Ethiopia), and Holz et al. (2023), who show that messages with threats of public disclosure and potential prison sentences increased tax payments by businesses in the Dominican Republic. On the other hand, there is also increasing evidence that enforcement interventions often fail to increase revenue collection if firms can easily adjust their declarations on other margins: in a non-experimental setting, Carrillo et al. (2017) find that when Ecuadorean firms were notified of revenue discrepancies, they adjusted their reported revenue but also their reported costs, leaving tax liability almost unchanged. Hoy et al. (2022) show that population-wide enforcement messages in Papua New Guinea did

[5] In this regard, our paper also connects to the fast expanding literature using transaction-level data to study firms' trading networks in the context of international trade (see, e.g., Dhyne et al., 2020; Huneeus, 2020; Demir et al., 2022).

[6] Our finding that targeting both firms of the pair has substantially stronger effects than treating only one party is also in line with Deserranno et al. (2022), who show that incentives in the healthcare sector in Sierra Leone are most effective when distributed equally between frontline workers and supervisors (as opposed to only one of the two parties).

not increase tax revenues, in spite of increasing the number of tax returns submitted. Similar results are found by Slemrod et al. (2017) in the United States and Best et al. (2021) in Pakistan. Our results lie in the middle of the range of these earlier studies: we find a positive (though small) effect on tax liability through amendments of past returns, but no effect on subsequent tax revenue. However, the strong direct and spillover effects on amendments and discrepancies suggest that exploiting firms' network information is a promising strategy in low administrative capacity settings, especially when combined with additional monitoring resources (Almunia and Lopez-Rodriguez, 2018; Basri et al., 2021).

Finally, in spite of its widespread introduction across the globe and its substantial contribution to tax revenues, we still have limited experimental evidence on how the VAT's functioning might be improved effectively in low administrative capacity settings.[7] As such, our results break new ground for future research on VAT compliance.[8] After the implementation of our experiment, the Uganda Revenue Authority generalized the data crosschecks and enforcement messages at the core of the intervention, highlighting the policy relevance of our results.

The rest of the paper is organized as follows. Section 2 describes the institutional context and the data. Section 3 explains the experimental design. Section 4 presents the results. In Section 5 we discuss the implications of our results and how they relate to existing evidence. Finally, Section 6 provides concluding remarks.

2 Background: the VAT in Uganda

This section starts by describing the institutional context of VAT in Uganda. We then present the data, and finally focus on VAT compliance gaps in Uganda by drawing connections to our earlier work (Almunia et al., forthcoming).

2.1 Institutional Context

Uganda is a low-income country with a per-capita income of $2,140 in PPP (World Bank, 2021). Its tax-to-GDP ratio—14.4% in 2020/21 (IMF, 2022)—is slightly below the 16.0% average in Sub-Saharan Africa (OECD/AUC/ATAF, 2022) and is substantially lower than

[7] The VAT accounts for 32% of total tax revenue in Sub-Saharan African countries (UNU-WIDER, 2022).

[8] To our knowledge Pomeranz (2015) and Best et al. (2021) are the two only examples of randomized experiments designed specifically to study VAT compliance. Naritomi (2019), Mascagni et al. (2021), Bellon et al. (2022), and Fan et al. (2018) provide quasi-experimental evidence on the effects of innovations in VAT enforcement in Brazil, Ethiopia, Peru, and China respectively.

the 34.1% average in OECD countries (OECD, 2022).

The VAT was introduced in Uganda in 1996 and currently contributes 30% of total tax revenue (IMF, 2022), a figure similar to the average in Sub-Saharan Africa. However, the IMF estimates that the VAT compliance gap—the difference between the potential net revenue and actual collections—is large in Uganda, at around 60% of potential VAT and 6% of GDP (IMF, 2014).

The Ugandan VAT has a standard design: a general rate of 18% applies to all domestic sales, with the usual exemptions for necessities and some services.[9] Firms with annual turnover above 150 million Ugandan shillings (UGX, equivalent to $45,000) are required to register for VAT, while smaller firms can choose to pay a simplified turnover tax.[10] As in other countries, exports are zero-rated, but the VAT applies to imports.[11]

VAT-registered firms have to submit monthly VAT declarations to the URA, and payments of positive tax liabilities are due within 15 days of the declaration. As in other low-income countries, there are restrictions on VAT refund applications when firms have negative liabilities.[12] After filing, firms may file amendments for past returns indefinitely.

There are two specific aspects of the VAT administration in Uganda that are at the core of our paper. First, all firms must file their monthly VAT declarations *electronically* since 2012.[13] Second, VAT firms are required to submit detailed transaction-level records together with their monthly VAT declarations on all their domestic sales and purchases with other VAT firms. This system is designed such that the URA receives two reports for each transaction, one from the seller and one from the buyer. In other words, the paper trails that are considered instrumental to the success of VAT systems in limiting tax evasion are available to the URA on a monthly basis. However, at the time of the study, the URA only

[9]For instance, unprocessed agricultural products and medical, educational and financial services are exempted from VAT. As is usually the case in VAT systems, a firm producing zero-rated goods may claim input tax credits, whereas VAT paid on inputs used in the production of exempted goods cannot be recovered (Uganda Revenue Authority, 2016).

[10]This turnover tax replaces both the VAT and the Corporate Income Tax (CIT). Firms below the registration threshold may choose to enter the VAT or the CIT system on a voluntary basis. 51.68% of firms in our analysis sample are above the threshold. Our analysis focuses on VAT-registered firms, and we do not observe that the intervention affects the likelihood of staying in the VAT system.

[11]Overall tax revenues are divided almost equally between the domestic VAT and the VAT on imports, which is directly paid at customs but can be credited as input in the monthly declarations (Uganda Revenue Authority, 2014).

[12]When the stock of negative liabilities is above 5 million UGX ($1,370), firms can claim a refund, but they have to agree to an audit by the revenue authority. If negative liabilities are less than that amount, they can only be carried over as an offset against future VAT liabilities (indefinitely). The strict regulation of VAT refunds is common practice in other low-income countries (Lemgruber et al., 2015).

[13]Some firms in the Large Taxpayers Office (LTO) began reporting electronically in the year 2010, and the rest moved to electronic reporting gradually during 2011.

extracted and analyzed the transaction-level information in the case of an audit. The process was not automated, and was hindered by technological bottlenecks and lack of manpower and qualified staff.

2.2 Data

The data for the analysis comes from firms' VAT returns, submitted monthly to the URA between 2013 and 2019.[14] Firms submit a monthly summary, which includes their tax identification number (TIN), the period covered by the return, the submission date, total sales and purchases to/from other VAT-registered firms, total sales to final consumers, total VAT liability, and VAT credit carried over from previous months. Additionally, firms submit their individual transactions with other VAT-registered companies. These include all domestic sales and purchases.[15] For each entry, firms report the TIN of the trading partner, the amount and the date of the transaction. VAT charged or paid is then computed automatically. The electronic filing system sums the individual transactions and pre-fills the monthly summary sheet of the return. Sales to final consumers and sales to firms not registered in the VAT are reported as a single aggregate figure in each monthly return.

We verify the consistency of the data along several dimensions. First, the law stipulates that firms must report transactions within 30 days. In the data, about 80% of registered transactions are reported within one month of the transaction date and another 15% within two months. Second, the information reported in the transaction-level data is consistent with the information from the monthly summary.[16] This indicates that the transaction-level records constitute meaningful paper trails for firms' VAT declarations and liabilities. In our analysis we aggregate all transactions data at the pair-month level.

To submit an amendment, the starting point is the initial return. Firms tick a box of the new version of the return indicating that it is an amendment. All entries can be modified. The return period is the same in the initial and amended return, while the submission date indicates when each was filed. At the time of the study, the data extraction process used by the URA eliminated the initial return when an amendment was filed. However, thanks to the

[14]For the letter experiment we focus on the 2017-2018 fiscal year, which goes from July 2017 to June 2018.

[15]Firms also have to report all their import transactions, but these are not included in our analysis because the trading partner is, by definition, a foreign entity.

[16]Specifically, total VAT charged reported in the monthly summary is equal to the sum of the VAT charged on individual transactions and VAT charged on sales final consumers and non-VAT firms reported by the same firm in its transactions records in 97.4% of monthly declarations. VAT paid reported in the monthly summary is equal to the sum of VAT paid on individual transactions reported by the same firm in 99.9% of monthly declarations.

frequent data extractions we set up with the administration for the study, we are able to observe both initial and amended returns for years 2014 to 2019.

2.3 Discrepancies in VAT reporting

In previous work, we analysed the performance of the VAT in the Ugandan context and showed that the distribution of reported value added was suspiciously low, with substantial consequences for tax revenue (Almunia et al., forthcoming). A large share of firms (30%) report a value added (sales minus purchases) that is negative or zero over the whole fiscal year. Such a pattern raises concerns of widespread VAT evasion. In order to explore the issue in more detail, we implemented a cross-checking methodology which consisted in comparing the transaction-level declarations submitted by sellers and buyers. Let us define S as being the amount reported by the seller in a given month on transactions with trading partner B. B is, by extension, the amount declared by the buyer in the same month on transactions with S. In the case of "seller shortfall" $S < B$ the seller declares less than the buyer on the same transactions in a given month. Conversely, in the case of "buyer shortfall" $S > B$, the seller declares more than the buyer on the same transactions in a given month. We documented widespread reporting discrepancies, with discrepancies in 79% of pair x month observations, of which 60% were cases of "seller shortfall" and 40% were cases of "buyer shortfall" (where the seller reports a larger amount than the buyer).

Seller shortfall could arise because the seller underreports its output VAT or because the buyer overreports its input VAT (or both at the same time). In contrast, buyer shortfall occurs when the seller overreports its output VAT or the buyer underreports its input VAT. It is important to note that seller shortfall potentially implies a reduction in the amount of tax remitted by firms, whereas buyer shortfall implies a potential increase in the amount remitted, compared to the true tax liabilities.[17] In both cases, the discrepancies could be due to voluntary evasion or to mistakes made when filing the VAT returns.

The analysis in Almunia et al. (forthcoming) concludes that in the period 2013-2016, Uganda lost approximately $384 million of VAT revenue due to misreporting in domestic B2B transactions.

[17]The qualifier "potential" is needed because the net revenue consequences also depend on whether the firms carry over a stock of negative liabilities against which they can offset new liabilities, as explained in Almunia et al. (forthcoming).

3 Experimental Design

In this section, we begin by presenting the randomization procedure that generates a subsample of firm pairs that are separated from each other by at least two degrees. We then describe the implementation details of the intervention.

3.1 Randomization Procedure

There are 17,428 unique firms submitting a VAT return during our 2017-2018 study period, and 285,089 firm pairs based on the transaction-level data. We define two firms as forming a pair in a given month if at least one of them lists the other as a client or a supplier in the transaction-level entries of its VAT return for that month and both are registered for VAT. The experimental sample is constructed in three steps. We first define a base sample of VAT-registered firm pairs with potential tax evasion. More precisely, we select firm pairs that fulfill the following criteria as of January 2018: both firms in the seller-buyer pair must be registered for VAT, must have filed a VAT return in the previous month, and must have an accumulated seller shortfall above UGX 1 million ($303) over the previous 10 months.[18] After applying these filters, the base sample of the analysis consists of 15,983 firm pairs.

Our research design of the experiment is illustrated in Figure 1. Starting from the base sample, we employ an iterative randomization procedure that allows us to study both the direct and spillover effects of the intervention. First, we randomly select a firm pair from the base sample and remove all trading partners of the selected pair from the sample.[19] Then, we randomly select another firm pair from the remaining sample, and similarly remove all their trading partners again. We repeat the procedure until there are no firms left from the base sample. Following this iterative approach, we obtain a set of 1,235 firm pairs, which we refer as the "*study sample*".[20] Through this procedure, we ensure that firm pairs in the study

[18] Additionally, we remove 2,352 firms that were part of the URA's annual audit plan for the financial year 2017/18 to avoid interfering with their normal operations.

[19] We use the term "trading partners" somewhat loosely, as their trading relationships are identified within the base sample of firms. Firms not labeled as trading partners for a given firm may still have transactions with the firm, for example transactions with buyer shortfalls or seller shortfalls with discrepancies below our threshold. Given the dense firm network in Uganda, it would be impossible to construct a study sample using the restrictive definition of trading partners.

[20] An error in one data extraction, in which all *input* transactions filed between 19/9/2017 and 15/10/2017 show up as duplicates in our data, caused a mechanical inflation in the occurrences and amounts of seller shortfalls between firm pairs. As a result, the study sample included some firm-pairs whose actual seller shortfall did not meet our discrepancy criteria (amounting to 28% of the study sample). We correct this in our analysis and exclude all pairs that did not feature any discrepancies after correcting for this error. We discuss the data correction in more details in Appendix D, and show that it is balanced across treatment arms, indicating that correcting for it does not bias our results.

sample are separated from each other by at least two degrees in the seller-shortfall relationship, i.e., no firm from a given pair in the sample would have a direct trading relationship that incurs seller shortfall above the threshold with a firm from another pair. Nor would two firms in the study sample have a shared trading partner with which they have a seller shortfall with reporting discrepancy above the threshold.

Finally, we apply a stratified randomization to assign the firm pairs in the study sample to three treatment arms and a control group. In the first treatment arm—"buyer only"—the letter is sent only to the buyer of the selected pair. In the second treatment arm—"seller only"—the letter is sent to the seller. In the third arm—"seller and buyer"—letters are sent to both the buyer and seller. The procedure assigns 494 pairs (or 40% of the study sample) to the control group, and the remaining 741 pairs are divided up equally across the three treatment arms.[21] The randomization is stratified by the ratio of sales to final consumers over total sales (above vs. below median), firm size measured by total output VAT (above vs. below median), and by whether or not the firms' headquarters are located in Kampala (the capital city of Uganda).

Having distinct "seller only" and "buyer only" treatment arms allows us to test whether the effects of the intervention are asymmetric, and if so, which trading party it is more effective to target to curb tax evasion in this context. The results are not clear ex ante: seller shortfall could be due to over-reporting by buyers or under-reporting by sellers (or a combination of both). Irrespective of the true underlying reporting behavior, firms could be differentially likely to solicit their client or their supplier for them to also respond to the URA's request. Based on previous studies we could expect enforcement effects to propagate upstream from buyers to sellers (Pomeranz, 2015; Naritomi, 2019) more than downstream. Finally, having the "seller and buyer" arm allows to assess whether it is most effective to have both trading partners treated at once and, if so, to what extent. Again, the results are not obvious ex ante since jointly treated firms could be more likely to collude.

Descriptive statistics on the final study sample are displayed in Table 1, which shows the average values of observable characteristics for the control group and the difference of each treatment arm with respect to control. For none of these characteristics is the treatment groups significantly different from the control group. Since differential attrition between the treatment and control groups is a common concern in RCTs, Appendix Figure C1 documents the proportion of firms filing VAT returns in the post-treatment months. The table shows that the attrition rate is very similar in both groups.

[21] In total 1,002 firms were meant to receive a letter: all firms from the "seller and buyer" arm, the sellers (respectively, buyers) from the "seller only" (resp., "buyer only") arm.

To fully exploit trading network information as a means of increasing the effectiveness of the intervention, the letters mentioned specific monthly discrepancy amounts with a single trading partner. We limit the listed discrepancies to three per letter for clarity purposes. We select the three largest discrepancies of each pair within the past 6 months as the ones to be listed on the letters.[22] This procedure allows us to rigorously define in the exact same way the counterfactual firm-pair × month discrepancies control pairs would have seen listed on letters had they been treated. We use *listed* months to refer to both the months listed in the letters (for treated pairs) and those that would have been listed (for control pairs).

3.2 Implementation of the Letter Experiment

The letter sent out to the 1,002 firms from the treatment group notifies them that the URA has developed new analytical methods to detect discrepancies in VAT declarations made by firms and their trading partners. It states the name of the specific trading partner with which discrepancies have been observed, and lists up to three examples of months in which there was seller shortfall, stating the corresponding amounts and also the total amount of seller shortfall accumulated by the firm pair over the past year.[23]

Finally, the letter requires the firm to resolve the issue by filing the relevant amendments, and warns them that the URA will carry out similar verifications in the future. It also reminds the taxpayer of the fines and prosecution it is exposed to in case of tax evasion. All the letters are officially signed by the Assistant Commissioner for Compliance Management, to ensure that they are reliable and credible in the eyes of taxpayers. A template of the letter is displayed in Appendix Figure A1. The letters were distributed by a courier company, based on firms' postal addresses from the URA's taxpayer register.

The timeline of the experiment was the following. The physical letters were delivered to treated firms between February 28 and April 17, 2018. Because a small percentage of letters could not be delivered, the URA emailed a copy of the letters to all treated firms on April 6, 2018. During the implementation period and the following months, URA staff kept track of all communications from treated firms with the URA. Appendix Table A1 and Appendix Figures A2 and A3 provide information on how firms reacted to the letters. The implementation was successful: 92% of firms selected for treatment either confirmed reception of the physical letter by a signature, or by contacting the URA.

[22] If a pair displays less than three discrepancies in the period under analysis, only those discrepancies are listed.

[23] The letter only mentions the trading partner forming with the treated firm the pair selected in the sampling and randomization procedures, even if the firm has discrepancies with other partners.

4 Results

We analyze the effects of the letters on taxpayers' behavior in two steps. First, we study how the treatment affects the probability that either firm in the treated pair amend their past returns to correct the discrepancies. We look separately at amendments for months that were mentioned in the letters and months that were not mentioned. We also explore whether it is the seller or the buyer that files the amendment, to identify potential within-pair spillovers. To analyze another dimension of spillovers, we evaluate whether treated firms also amend transactions with other trading partners not included in the study sample. Lastly, we study the impact of all these amendments on the resulting VAT liability in the past returns.

Second, we analyze the impact of the treatment on subsequent reporting behavior in the post-treatment period, which goes until December 2018 (approximately nine months after the letters were sent). In this case, we look at pair-level outcomes such as the probability of reporting discrepancies (within the treated pair or with other trading partners), the transaction size or the likelihood of reporting any trade within the pair. We also analyze firm-level outcomes such as reported taxable sales and purchases and the overall reported VAT liability.

4.1 Impact on Amendments of Past VAT Returns

Effect on amendments of treated firm pairs

To analyze the impact of the letters on amendments of past discrepancies, we estimate the following regression:

$$Y_{it} = \alpha + \sum_{h=1}^{3} \beta_h T_{ih} + \delta_t + \epsilon_{it}, \tag{4.1}$$

where Y_{it} is a dummy variable for whether any member of firm pair i has resolved or reduced the discrepancy by amending their past returns in the ten months after treatment, T_{ih} denotes a set of dummy variables capturing the three mutually exclusive treatment arms, δ_t is a month fixed effect, and ϵ_{it} is an error term. The coefficients of interest are the β_h's, which capture the intention-to-treat estimates defined at the firm-pair level. Observations are at the firm-pair-month level.

Table 2 reports the results. Columns 1 through 4 refer to the amendments of discrepancies mentioned in the letter (a maximum of three per firm pair, *listed* months as defined in Section 3.1) and columns 5 through 8 refer to other discrepancies of the same pair but not mentioned

in the letter. In each of the two sets of regressions, the outcome in the first two columns is a dummy variable for whether the discrepancy has been fully resolved. The outcome in the third and fourth column also includes amendments that partially resolved the discrepancy.

Aggregating all treatment groups (col. 1) we find that the share of fully resolved discrepancies is 11.5pp higher in the treatment group than in the control group, where the share is just 0.7%. Analyzing the effect for each treatment arm separately (col. 2), we find that the effect is strongest (15.7pp) for the "Seller and Buyer" treatment, and weakest for the "Buyer only" treatment (6.1pp). The corresponding coefficients are larger when the outcome includes partially resolved discrepancies, reaching 22.1pp for all treatments combined (col. 3) and 30.2pp for the "Seller and Buyer" group (col. 4), compared to an amendment rate of 1.8% in the control group. In both columns 2 and 4, the effect for the "Buyer only" treatment is significantly smaller than the "Buyer and Seller" and "Seller only" treatments, while the coefficients for the latter two are not statistically different.[24]

Spillovers across time periods and across firms

To study the impact on the discrepancies from months not mentioned in the letters, we focus on a subsample of pairs (530 instead of 1,235) for which there were more than three discrepancies in the pre-treatment period. Despite the fact that these discrepancies were not mentioned in the letters, we also observe a significant effect on amendment rates. As shown in columns 5-8 of Table 2, the overall effect is a 5.8pp increase in the share of resolved discrepancy (col. 5) and 10.6pp for any reduction (col. 7), reaching up to 8.4pp and 13.9pp respectively in the "Seller and Buyer" group (cols. 6 and 8).

In Table 3 we study spillovers *within* firm pairs included in the study sample. The outcome variable in this case distinguishes whether it is the seller or the buyer who amends the past return. We focus on the amendments of discrepancies mentioned in the letter.[25] The outcome variable in odd columns is a dummy taking value one if the seller amends the past return (either resolving or reducing the discrepancy), whereas in even columns it is the equivalently defined variable for buyers.

In the treatment arms in which only one firm receives the letter, we find that the seller is always more likely to amend than the buyer. Sellers are 11.3pp more likely to amend

[24] Appendix Figure B1 shows the distribution of discrepancies before and after treatment for treated groups (lumped together) and the control group separately. The intervention shifted the distribution of negative discrepancies towards 0.

[25] Appendix Table B1 reports the results for discrepancies not mentioned in the letter, which are qualitatively similar although smaller in magnitude.

compared to the control group when they receive the letter themselves, and 4.6pp more likely when the buyer—their trading partner—receives the letter (col. 1). In contrast, the effects on the amendment rate of buyers are small regardless of whether the letter is sent to them, 0.8pp, or to their seller, 0.5pp (col. 2). The results are qualitatively similar, although larger in magnitude, when the outcome includes also partially resolved discrepancies (cols. 3 and 4).

These results strongly suggest that some firms communicate with their trading partners after receiving the letters, and this communication leads to within-pair spillovers that increase the amendment rate of sellers. However, this spillover effect does not seem to operate in the opposite direction. This finding is consistent with the asymmetric spillover effects found by Pomeranz (2015), where a similar enforcement intervention had spillover effects upstream in the value chain, but not downstream.

In Table 4, we investigate whether firms also amend past discrepancies with trading partners that are not mentioned in the letter. The analysis is again conducted at the firm-pair level, but in this case we include all other trading partners for each of the treated firms, hence the much larger sample size (79,880 unique firm pairs and 260,190 pair-month observations). The outcome variable is a dummy variable taking value one when the discrepancy is partially or fully resolved by either firm in the pair (cols. 1 and 2), by the firm belonging to the study sample (cols. 3 and 4) or by their trading partner outside of the study sample (cols. 5 and 6).

Aggregating across all treatment groups (col. 1) we find that the share of fully resolved discrepancies is 0.42pp higher in the treatment group than in the control group, where the share is just 0.25%. The treatment effect is about twice as large when the trading partner is a supplier rather than a client (col. 2), and this difference is statistically significant (the p-value of the difference is 0.004). This asymmetry is again consistent with our previous results and those of Pomeranz (2015). The effect is almost entirely driven by amendments made by the firm in the study sample, as shown by the 0.41pp effect (col. 3) and the 0.01pp effect (col. 5).[26]

[26]In Appendix Table B2 we analyze the effects in the subsamples of eligible months (keeping months with the three largest discrepancy amounts of the past 6 months for each considered pair) and listed months (keeping only the exact months listed on the letters). As one would expect, the effect is stronger for eligible months, and even larger for listed months. As a further robustness check, Appendix Tables B3 and B4 are equivalent to Tables 4 and B2, respectively, but changing the outcome variable to a dummy for whether the discrepancy was fully resolved. The coefficients are qualitatively similar, but smaller in size and the difference between clients and suppliers it not statistically significant.

Effect of amendments on reported VAT liability

In Table 5, we analyze how the amendments of past discrepancies change the amounts reported in the monthly returns mentioned in the letters. We focus on four outcomes: the sales to other VAT-registered firms (labelled "B2B sales"), sales to final consumers or unregistered firms (labelled "Final Sales"), taxable inputs, and the overall VAT liability.[27] In this case, the analysis is conducted at the firm level, rather than firm-pair level, because the outcomes can only be defined at the firm level. The sample is restricted to listed months.[28] All amounts are in thousands of US dollars ($), and we winsorize all outcomes at the 0.5% level.[29]

In columns 1-2, we look at the change in reported sales to other VAT-registered firms (B2B sales) after amendments are filed. We find that all treatments combined lead to an increase of $235 in reported B2B sales (col. 1). When analyzing each treatment arm separately, we find that the "Buyer and Seller" treatment has the largest effect ($337), which is consistent with the previous results on amendments. In columns 3-4, we examine the impact on reported final sales, which include sales to final consumers and also to other firms not registered in the VAT. We find a small decrease of $141 ($205 in the "Buyer and Seller" group). In columns 5-6, we find that reported taxable inputs decrease by just $4 ($7 in the "Buyer and Seller" treatment). These coefficients indicate that amendments increase the amount of B2B sales reported, but about 60% of this increase is offset by a reduction in reported final sales.[30] The impact on reported inputs is small compared to the other variables, suggesting that few of the seller-shortfall discrepancies listed in the letters were due to buyers overstating their inputs.

Finally, in columns 7-8 we estimate the overall effect on the VAT liability reported in the tax returns referring to the pre-treatment months. VAT liability increases by $9 for all treatments combined ($14 for the "Buyer and Seller" group). This is consistent with the previous findings: adding up the estimates in columns 1, 3, and 5, we estimate a change in taxable value added equal to 235-141+4=$98. Applying the standard VAT rate of 18% would yield an increase of $17.6 in the VAT liability, a few dollars higher than our point estimate of $9 in column 7.

[27] To calculate the VAT liability for each monthly return, we calculate the difference between output VAT charged and input VAT paid. Hence, we do not consider the potential application of tax credits carried forward from past negative liabilities.

[28] In Appendix Table B5, we show the results for the sample of all months. The coefficients are smaller but similar in sign and still significant.

[29] Our results are unchanged for different levels of winsorization (0.4% and 0.3%). Without winsorizing, confidence intervals become larger due to noise at the tails of the distribution.

[30] The 60% is calculated as follows: $141/235 \approx 205/337 \approx 0.6$.

These positive, though small, effects on tax liability are consistent with the findings of Carrillo et al. (2017), where firms reacted to a similar tax compliance intervention by adjusting other margins of reporting in order to leave total tax liability almost unchanged.[31] In our setting, firms reacted to the letters by increasing reported taxable sales but reporting lower final sales, leading to an overall increase in reported tax liability for the past tax returns mentioned in the letters.

4.2 Impact on Post-treatment Reporting Behavior

Effect on reporting and discrepancies of treated firm pairs

To study the tax reporting behavior of firm pairs after the intervention, we run the following event-study specification:

$$Y_{it} = \beta_j \sum_{\substack{j-10 \\ j \neq -1}}^{10} D_{j(it)} + \delta_t + \gamma_i + \epsilon_{it} \tag{4.2}$$

where Y_{it} is the outcome variable of interest for firm-pair i in month t, $D_{j(it)}$ is a treatment indicator equal to one if firm-pair i was treated j months ago, parameters δ_t and γ_i represent month and firm-pair fixed effects, respectively. Our coefficients of interests are β_j's.[32]

The first set of results is reported in Figure 2, which displays the result of three different event-study specifications. The outcome variable in each specification is a dummy for whether the firm-pair has a seller shortfall or buyer shortfall discrepancy, or matching reports. We observe that the intervention leads to a sustained reduction of 12.3pp (16.8%) in the probability of reporting seller shortfall with the trading partner mentioned in the letter in the months following the intervention. This decline is compensated roughly equally by an increase in the probability of reporting buyer shortfall, 5.7pp (47%), and no reporting discrepancy, 6.1pp (45%), which suggests that the intervention leads to sustained improved reporting behavior *within* treated pairs.[33]

In Figure 3, we show that the seller shortfall amount (conditional on seller shortfall) does not appear to change significantly (Figure 3a), suggesting that the change in reporting behavior operates mainly along the extensive margin rather than the intensive margin. Figures 3b and 3c show that the intervention did not affect the transaction size, nor the likelihood

[31] In their case, the intervention was targeted to corporate income tax returns and did not have an experimental design.

[32] We find no evidence of differential attrition, as shown in Appendix Figure C1

[33] See Appendix Table C2 for detailed regression results underlying Figure 2, Appendix Table C2 for the difference-in-difference coefficient, and Appendix Table C3 for results by treatment arm.

that treated firm pairs continue trading after the intervention, suggesting the intervention did not disrupt trading relationships in a significant way.[34]

Spillovers across time periods and across firms

To study whether the intervention affected firms' behavior with other trading partners, we run a similar event-study analysis using as the outcome variable a dummy indicating whether the firm reported seller shortfall with each of its other trading partners not mentioned in the letter.[35] Results are shown in Figure 4. We find a much smaller (0.8pp), but statistically significant reduction in the probability of reporting seller shortfall with other trading partners after the intervention. The effect is short-lived and disappears after about four months. Figure 5 shows that there are no discernible effects on seller shortfall or transaction size, and a just small positive effect on the likelihood of reporting trade with these trading partners different from the one mentioned in the letter (which dies out after a few months).[36]

Effect on subsequent VAT liability

In Figure 6 we analyze the impact on the reported VAT liability in post-treatment months. We conduct this analysis at the firm level for the same reason as in Table 5. We find no increase in reported VAT liability in the months right after the letter is sent. This result suggests that the treatment was not strong enough to lead to a large change in subsequent tax-reporting behavior.

In Figure 7 we do, however, observe a sustained decrease in the ratio of final sales over total sales, suggesting that the intervention caused firms to reclassify some of their final sales as B2B transactions in post-treatment months. This result is consistent with the findings from the analysis of the amendments of past returns and again suggests a sustained change in reporting behavior after the intervention.[37]

The overall conclusion for this analysis is that, despite the reduction in the probability of seller shortfall, we do not find a significant increase in the VAT liability reported by treated firms in the months following the compliance intervention.

[34] The regression coefficients underlying the three panels of Figure 3 are reported in Appendix Table C1.

[35] Therefore, in these figures there are as many observations as firm pairs.

[36] See Appendix Table C4 for detailed regression results, Appendix Table C5 for the difference-in-difference coefficient, and Appendix Table C6 for the results by treatment arm.

[37] Appendix Table C7 reports event-study regression results for B2B sales, final sales, taxable inputs, total VAT liability and the ratio of final sales over total sales. The post-treatment event-study coefficients are insignificant for the first four variables, while we find negative and significant coefficients for the ratio of final sales, as shown in Figure 7.

5 Discussion

The results presented in the previous section suggest that the intervention was effective at eliciting amendments of the transactions listed in the letters, and had substantial spillover effects within trading pairs and also on transactions with other untreated trading partners. However, a substantial proportion of treated firms did not amend, suggesting that the threat of a letter from the revenue authority is not strong enough in this context for a relevant share of taxpayers. In this respect, our results resonate with the findings of Carrillo et al. (2017), despite the very different setting of their study, which takes place in a middle-income country (Ecuador) and refers to the corporate income tax rather than the VAT. Our results are also consistent with those of Hoy et al. (2022).

In terms of mechanisms, the results indicate that amending firms increase their reported B2B sales but they offset this with a decrease their reported sales to final consumers (or unregistered firms). The effects on reported inputs are negligible because most of the amendments are filed by sellers rather than buyers. Overall, we find a positive but small effect of the amendments on the VAT liability declared in part returns.

Finally, the asymmetric response by suppliers and clients of treated firms is consistent with the findings Pomeranz (2015), supporting the hypothesis that the effects of enforcement intervention are passed more strongly to upstream trading partners than downstream ones. However, a difference between our results and those of Pomeranz (2015) is that we find significant effects in both directions, while in her case only the spillovers to suppliers are significant.

Despite the small size of the increase in VAT liability due to amendments, the intervention was cost effective. We estimate that the revenue gain from the amendments of transactions listed in the letters is \$29,555.[38] The total cost of sending the letters—including hiring a courier company and the time spent by URA officers on the project—was \$5,716 (see Appendix E for details on the calculation). Thus, the additional revenue obtained was six times higher than the cost of the intervention. Therefore, we believe that a scale-up of this type of intervention could potentially raise a significant amount of additional revenue. This is especially the case because there is a limit to the reclassification strategy used by firms: if the amendments that increase reported B2B sales are always offset by a reduction in reported final sales, at some point the latter will be too small. If the revenue authority identifies all reporting discrepancies and notifies firms about them systematically, firms will be forced to

[38]This results from multiplying the change in tax liability estimated in Table 5 times the number of firm-month observations restricted to the treated group: \$9 $\times$ 3,284 = \$29,555.

file amendments in which they cannot apply that reclassification strategy.

That said, the intervention we study here was less successful at changing the subsequent tax-reporting behavior of firms. Although we observe a decrease in the share of seller-shortfall discrepancies *within* treated pairs, this effect dissipates when we look at the behavior of treated firms with *other* trading partners. Hence, it is not surprising that the overall effect on the subsequent reported VAT liability is insignificant. One possible explanation for the weakness of the effects on this margin is that there was no specific follow-up intervention to remind firms that the new crosschecking system was in place. A possible avenue for future research is to study the effectiveness of such follow-up interventions in this and other contexts.

6 Conclusion

In this paper, we have relied on detailed B2B trading records reported by Ugandan firms in their VAT returns to design a randomized controlled trial aiming to improve tax compliance and reporting behavior of VAT-registered firms.

The sampling and randomization procedures, as well as the transaction-level data, allow us to rigorously study direct and spillover effects of the intervention. To our knowledge this is the first experimental study allowing to causally estimate the impact of a tax enforcement intervention in a firm network in such a fine-grained way.

The intervention was implemented successfully since 92% of treated firms confirmed reception of the letter. It led to a strong reaction by treated firms, raising the probability that a discrepancy is corrected fully (partially) by up to 11.5 (22.1) percentage points compared to just 0.7% (1.8%) in the control group. The letters had spillover effects to other trading partners both within the treated pairs and with partners outside of the study sample. They also had spillover effects across time periods, on months not listed in the letters. Overall, the intervention slightly increased VAT liability of past months due to amendments, and was cost-effective by a wide margin. However we do not find a significant effect of tax liabilities going forward.

Taken together, the results suggest that this letter intervention had a strong impact on amendments of past returns and on reporting behavior, but a more limited impact on overall tax revenue. However, the results also highlight the potential of exploiting detailed information on trading networks to maximize the efficiency of tax enforcement interventions.

Finally, it is worth noting that this study has had a direct impact on actual policies implemented. Following the 2018 experimental study described in this paper, data cross-

checks by the URA were substantially expanded. In 2021, the URA established a new program called electronic fiscal receipting and invoicing (EFRIS), whereby all input claims must be validated electronically with the corresponding invoice. Furthermore, in 2022 the URA started rolling out electronic billing machines which record transaction information at the time of the purchase. Looking forward, our results pave the way for studying how these interventions might affect VAT compliance by adopting firms and their trading partners.

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Tables

Table 1
Descriptive Statistics and Randomization Balance

Firm-pair characteristics	Mean	Difference with respect to control			
	Control	All Treatment	Buyer Treatment	Seller treatment	Seller and Buyer Treatment
	(1)	(2)	(3)	(4)	(5)
Transaction amount[1]	1.61	−0.14	−0.15	−0.19	−0.07
	(4.14)	[0.51]	[0.61]	[0.51]	[0.80]
Buyer characteristics					
Total input VAT	10.88	2.56	2.59	7.83	−2.29
	(43.21)	[0.43]	[0.45]	[0.13]	[0.41]
Total output VAT	14.30	2.87	1.48	11.43	−3.65
	(57.20)	[0.58]	[0.73]	[0.20]	[0.33]
Total value added	3.41	0.33	−1.10	3.62	−1.35
	(20.77)	[0.89]	[0.44]	[0.37]	[0.38]
Share of sales to final consumers	0.51	0.02	0.03	0.01	0.01
	(0.45)	[0.50]	[0.37]	[0.79]	[0.71]
Number of trading partners (Total)	72.80	5.72	14.89	−1.42	3.72
	(97.72)	[0.34]	[0.08]	[0.85]	[0.62]
Number of clients per month	7.19	0.13	−0.91	0.45	0.80
	(35.87)	[0.95]	[0.72]	[0.87]	[0.82]
Number of suppliers per month	11.1	0.96	2.14	−0.04	0.76
	(15.05)	[0.32]	[0.11]	[0.97]	[0.53]
Seller characteristics					
Total input VAT	25.72	−3.54	−2.11	−4.55	−3.96
	(61.18)	[0.28]	[0.66]	[0.31]	[0.37]
Total output VAT	29.02	−2.32	0.21	−4.07	−3.07
	(67.52)	[0.56]	[0.97]	[0.42]	[0.55]
Total value added	3.30	1.22	2.32	0.48	0.89
	(15.16)	[0.35]	[0.21]	[0.69]	[0.54]
Share of sales to final consumers	0.56	0.01	−0.03	0.06	0.01
	(0.42)	[0.64]	[0.42]	[0.09]	[0.84]
Number of trading partners (total)	112.10	14.93	16.40	14.17	14.26
	(253.24)	[0.40]	[0.38]	[0.59]	[0.49]
Number of clients per month	15.79	2.37	1.21	3.09	2.83
	(40.48)	[0.41]	[0.68]	[0.48]	[0.42]
Number of suppliers per month	11.1	0.96	2.14	−0.04	0.76
	(9.76)	[0.48]	[0.17]	[0.83]	[0.92]
Number of firm-pair observations	494	741	242	238	261

Notes: This table displays the balance between the various categories of treatment and the control group. Observations are reported at the *firm-pair* level. Column 1 reports the mean and standard deviation (in parentheses) for the control group. Columns 2-5 report the differences of the other groups with respect to control, and the p-values (in square brackets) of two-sided tests for equality of means. All monetary values are in thousands of US$. *Source*: Data from VAT returns submitted to the URA. *Definition:*[1] defined as the average of the highest amount reported by either member of the pair (seller or buyer) between July and December 2017. The table is cited on page 11.

Table 2
Impact of Letters on Past Discrepancies within Firm Pairs

Dependent variable:	Mentioned in the letter				Not mentioned in the letter			
	Resolved		Reduced		Resolved		Reduced	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.115		0.221		0.058		0.106	
	(0.012)		(0.017)		(0.012)		(0.018)	
Buyer Only		0.061		0.111		0.025		0.040
		(0.015)		(0.022)		(0.014)		(0.019)
Seller Only		0.121		0.243		0.068		0.144
		(0.021)		(0.030)		(0.024)		(0.036)
Buyer and Seller		0.157		0.302		0.084		0.139
		(0.022)		(0.030)		(0.021)		(0.034)
R-squared	0.047	0.061	0.093	0.124	0.024	0.035	0.040	0.061
Observations	2733	2733	2733	2733	2155	2155	2155	2155
No. of Unique Pairs	1235	1235	1235	1235	530	530	530	530
Mean of Dep. in Control	0.007	0.007	0.018	0.018	0.001	0.001	0.010	0.010
P-value of β_S=β_B		0.019		0.000		0.115		0.009
P-value of β_{SB}=β_B		0.000		0.000		0.022		0.011
P-value of β_{SB}=β_S		0.232		0.151		0.621		0.905
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample of VAT returns for pre-treatment months (Mar-2017 to Dec-2017). Analysis is conducted at the *firm-pair month* level. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data from either firm in the pair, resolving or reducing the discrepancy. Columns 1-4 refer to discrepancies that were mentioned in the letter. Columns 5-8 refer to discrepancies between the two firms that could have been reported in the letter, but were not because only the three largest discrepancies were reported. The sample size is smaller in columns 5-8 because for some pairs there were no more than three discrepancies in the pre-treatment period. Robust standard errors clustered at the firm-pair level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 14.

Table 3
Direct and Spillover Effects within Firm Pairs: Who Amends?

Dependent variable:	Mentioned in the letter			
	Resolved		Reduced	
Amendment by:	Seller (1)	Buyer (2)	Seller (3)	Buyer (4)
Buyer Only	0.046	0.008	0.077	0.027
	(0.014)	(0.004)	(0.020)	(0.009)
Seller Only	0.113	0.005	0.224	0.012
	(0.020)	(0.005)	(0.029)	(0.008)
Buyer and Seller	0.141	0.006	0.249	0.031
	(0.021)	(0.003)	(0.028)	(0.010)
R-squared	0.057	0.004	0.107	0.014
Observations	2733	2733	2733	2733
No. of Unique Pairs	1235	1235	1235	1235
Mean of Dep. in Control	0.006	0.001	0.016	0.002
P-value of β_S=β_B	0.006	0.546	0.000	0.171
P-value of β_{SB}=β_B	0.000	0.667	0.000	0.731
P-value of β_{SB}=β_S	0.321	0.814	0.520	0.105
Month-Year FE	Yes	Yes	Yes	Yes

Notes: Sample of VAT returns for pre-treatment months (Mar-2017 to Dec-2017). Analysis is conducted at the *firm-pair month* level. The columns refer to discrepancies that were mentioned in the letter, i.e. the three largest discrepancies between the firms in the indicated time period. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data resolving or reducing the discrepancy. The results for the months not mentioned in the letter are shown in Table B1 of Appendix C. Robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 14.

Table 4
Spillover Effects on Past Discrepancies with Other Trading Partners

Dependent variable:	Reduced		Reduced by Treated		Reduced by Partner	
	(1)	(2)	(3)	(4)	(5)	(6)
Any treatment	0.0042		0.0041		0.0001	
	(0.0004)		(0.0004)		(0.0000)	
Any treatment x Client		0.0029		0.0028		0.0000
		(0.0005)		(0.0005)		(0.0000)
Any treatment x Supplier		0.0050		0.0049		0.0001
		(0.0006)		(0.0006)		(0.0000)
R-squared	0.001	0.002	0.001	0.002	0.000	0.000
Observations	260190	260190	260190	260190	260190	260190
No. of Unique Pairs	79880	79880	79880	79880	79880	79880
Mean of Dep. in Control	0.0025	0.0025	0.0024	0.0024	0.0000	0.0000
P-value of β_C=β_S		0.004		0.005		0.552
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample of VAT returns for pre-treatment months (Mar-2017 to Dec-2017). Analysis is conducted at the *firm-pair month* level, with one firm being treated in the study sample (either in the treatment or control group) and their trading partner not included in the study sample. The sample refers to all pre-treatment months where firms in our sample trade with other partners. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data reducing the discrepancy. Columns 1-2 refer to discrepancies reduced by any of the firms in the pair, columns 3-4 refer to discrepancies reduced by the firm in the study sample, columns 5-6 refer to discrepancies reduced by the trading partner of the firm included in the study sample. The client and supplier dummies refer to the status of the other trading partners with respect to the firms included in the study sample. Additional results using the same outcome but different subsets of discrepancies are shown in Table B2 of Appendix B. Additional results using resolving discrepancies as the main outcome and different subsets of discrepancies are shown in Table B3 and Table B4 in Appendix B. Robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 15.

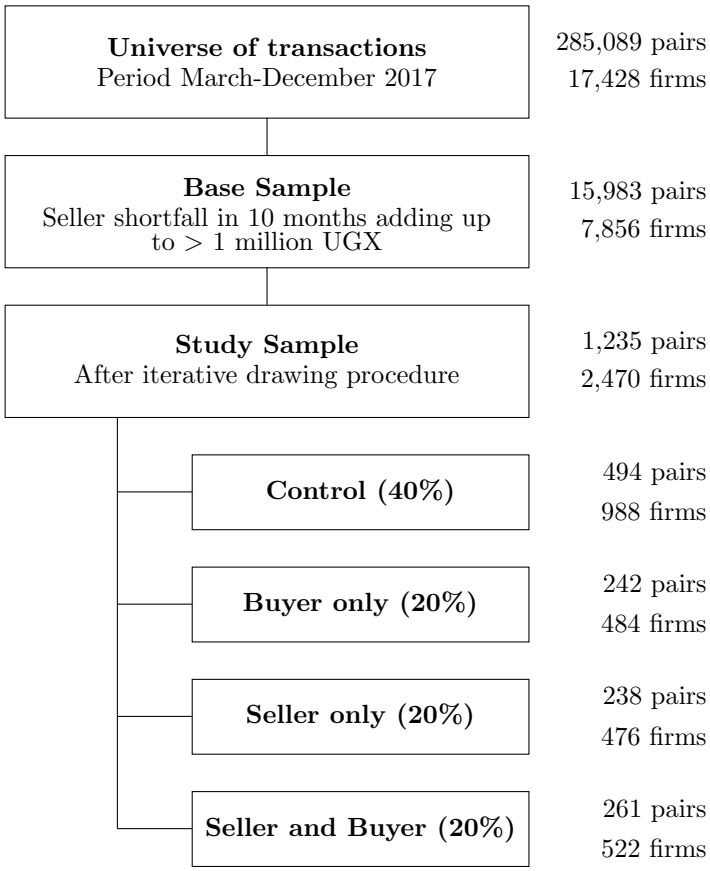
Table 5
Impact of Amendments on VAT Liability of Past Returns, at Firm Level

Dependent variable:	ΔB2B Sales		ΔFinal Sales		ΔTaxable Inputs		ΔVAT Liability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.235		-0.141		-0.004		0.009	
	(0.029)		(0.016)		(0.002)		(0.003)	
Buyer Only		0.054		-0.035		-0.003		0.003
		(0.039)		(0.022)		(0.003)		(0.003)
Seller Only		0.308		-0.178		-0.003		0.009
		(0.040)		(0.022)		(0.003)		(0.003)
Buyer and Seller		0.337		-0.205		-0.007		0.014
		(0.038)		(0.021)		(0.002)		(0.003)
R-squared	0.015	0.023	0.014	0.024	0.004	0.005	0.005	0.006
Observations	5473	5473	5473	5473	5473	5473	5473	5473
No. of Firms	2470	2470	2470	2470	2470	2470	2470	2470
Mean of Dep. in Control	45.254	45.254	76.458	76.458	105.194	105.194	2.973	2.973
Mean of Diff. in Control	0.046	0.046	-0.014	-0.014	0.001	0.001	0.007	0.007
Median of Dep. in Control	1.110	1.110	4.838	4.838	15.770	15.770	0.163	0.163
P-value of β_S=β_B		0.000		0.000		0.914		0.141
P-value of β_{SB}=β_B		0.000		0.000		0.213		0.004
P-value of β_{SB}=β_S		0.519		0.278		0.179		0.185
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effects of treatment on the monthly VAT returns after the amendments, at the *firm* level. Sample of VAT returns for pre-treatment months (Mar-2017 to Dec-2017). Analysis is conducted at the *firm* level and the sample is restricted to listed months. The outcome is the pre- and post-treatment change in each of the variables listed above the columns measured in thousands of US$. Data are winsorized at 0.5%. The outcomes are calculated as the difference between the pre- and post-treatment of: the difference between total taxable sales and final sales for *B2B Sales*, total input tax/0.18 for *Taxable Inputs*, total VAT output tax/0.18 for *Final Sales*, and the difference between total output tax and total input tax for *VAT Liability*. The results relative to all months are displayed in Table B5 of Appendix B. Robust standard errors clustered at the firm level *Source*: Data from VAT returns submitted to the URA. The table is cited on page 15.

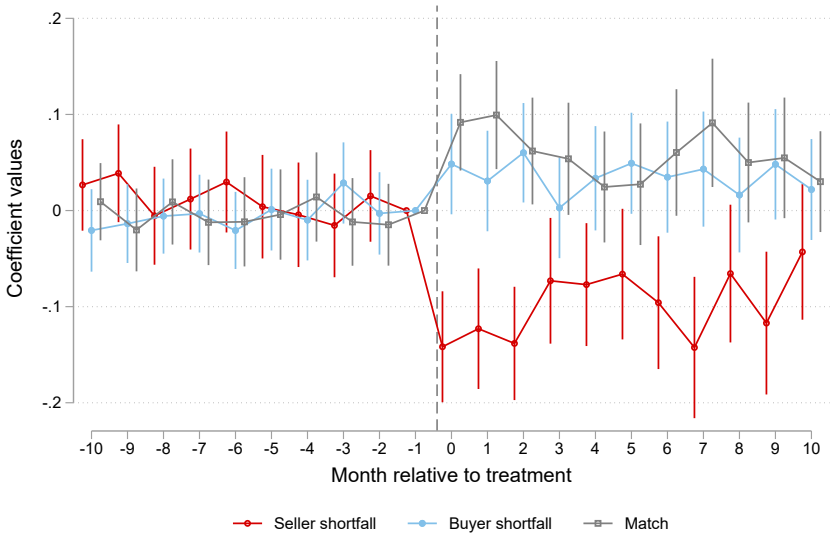
Figures

Figure 1
Research Design



Note: This figure shows the number of firms and unique firm pairs included in the study. The universe of transactions refers to the entire transaction-level data between March and December 2017, the base sample refers to the firms satisfying the shortfall conditions, and the study sample refers to the firms included in the analysis. The criteria for the selection of pairs in the base sample and the iterative drawing procedure to generate the study sample are described in Section 3.1. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 10.

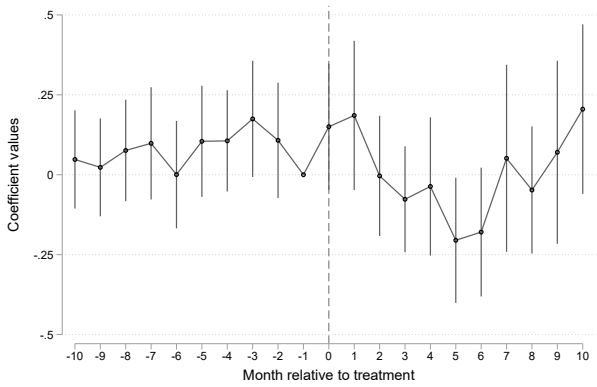
Figure 2
Pair-level Event Studies: Main Outcomes



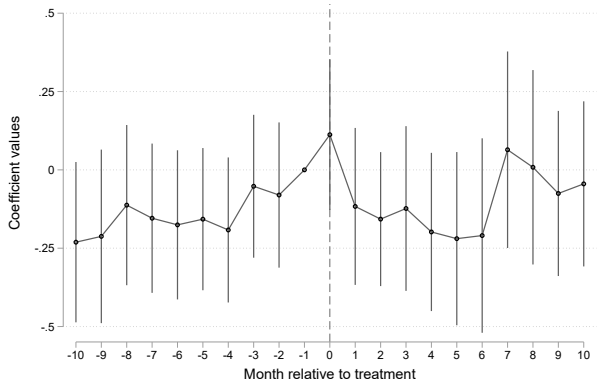
Note: This figure shows the event-study estimates for the treatment effect on the probability of reporting i) seller shortfall, ii) buyer shortfall, and iii) a null discrepancy. Standard errors are clustered at the firm-pair level. The estimated coefficients are displayed in Appendix Table C1. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 17.

Figure 3
Pair-level Event Studies: Other Outcomes

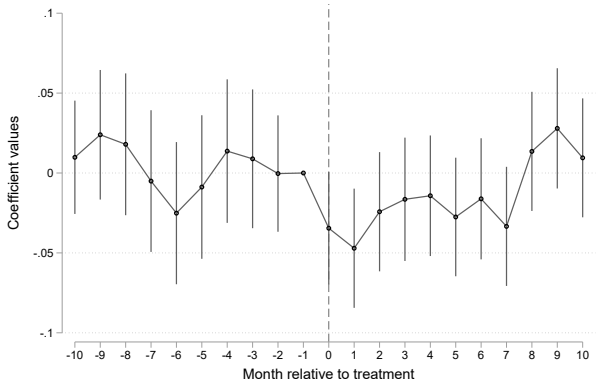
(a) Seller shortfall amount



(b) Transaction size

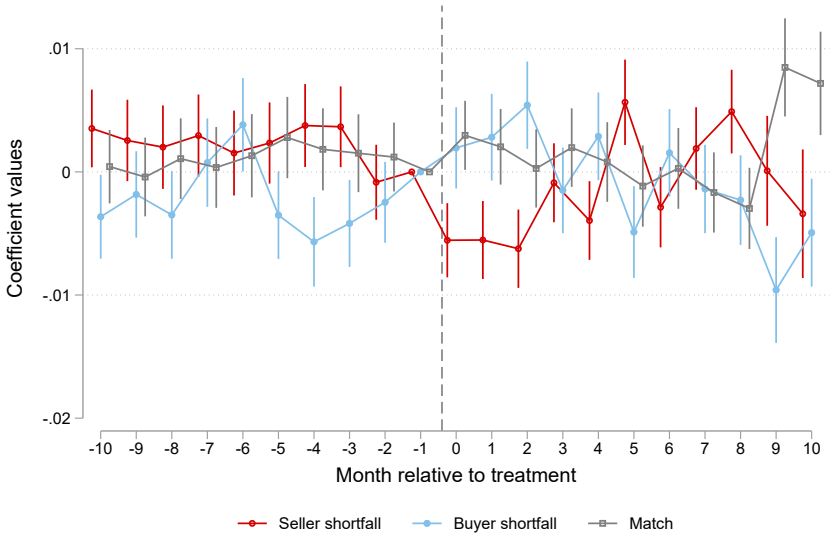


(c) Likelihood of reporting trade



Note: In Panel A, the figure shows the event-study estimates for the treatment effect on seller shortfall amount. The outcome is conditional on having seller shortfall. The mean of the dependent variable in the control group is equal to 0.54. Standard errors are clustered at the firm-pair level. In Panel B, we show the event-study estimates for the treatment effect on transaction size. The outcome is conditional on reporting a transaction. Data in Panel A and B are winsorized at 0.5%. In Panel C, we show the event-study estimates for the treatment effect on the likelihood of reporting trade. Standard errors are clustered at the firm-pair level. The means of the dependent variable in the control group are 1.01 and 0.39 in Panel B and C, respectively. The estimated coefficients are displayed in Table C1 of Appendix C. *Source*: Data from VAT returns submitted to the URA. The figures are cited on page 17.

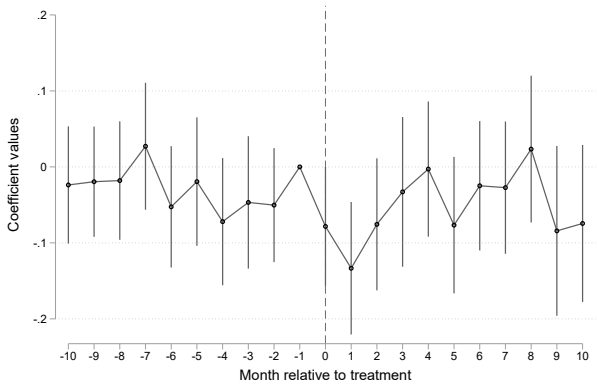
Figure 4
Pair-level Spillover Effects with Other Trading Partners



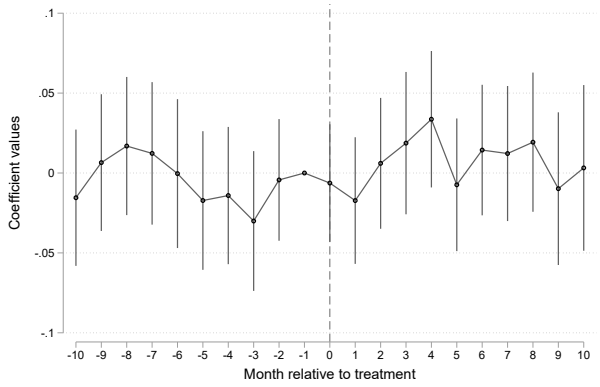
Note: This figure shows the event-study estimates for the treatment effect on the probability of reporting i) seller shortfall, ii) buyer shortfall, and iii) a null discrepancy. Standard errors are clustered at the firm-pair level. The estimated coefficients are displayed in Appendix Table C4. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 17.

Figure 5
Pair-level Event-studies with Other Trading Partners: Other Outcomes

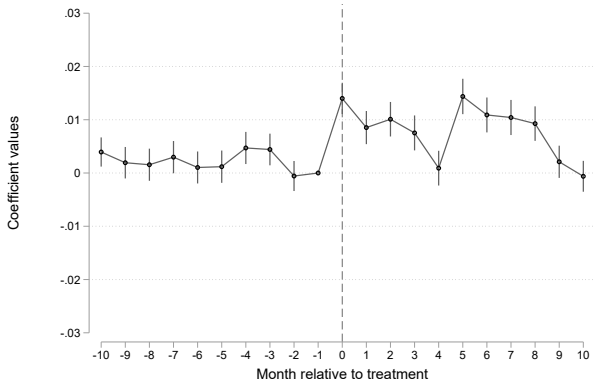
(a) Seller shortfall amount



(b) Transaction size

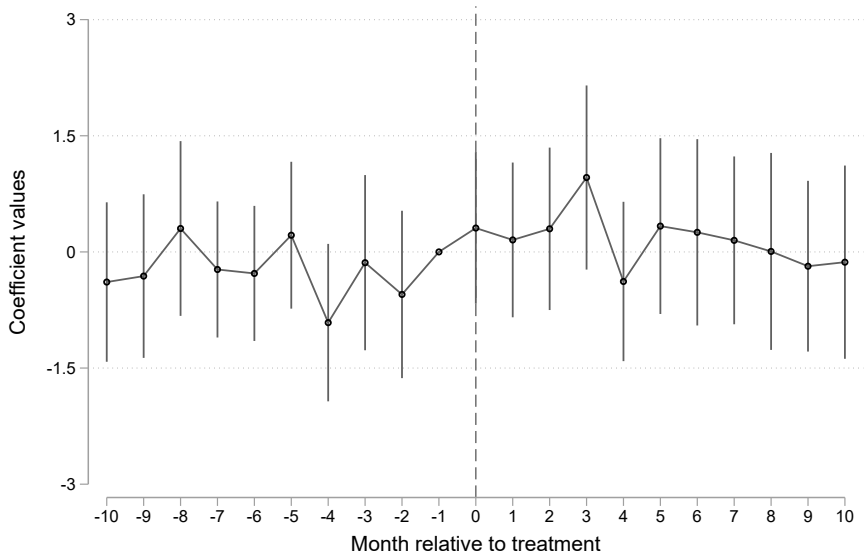


(c) Likelihood of reporting trade



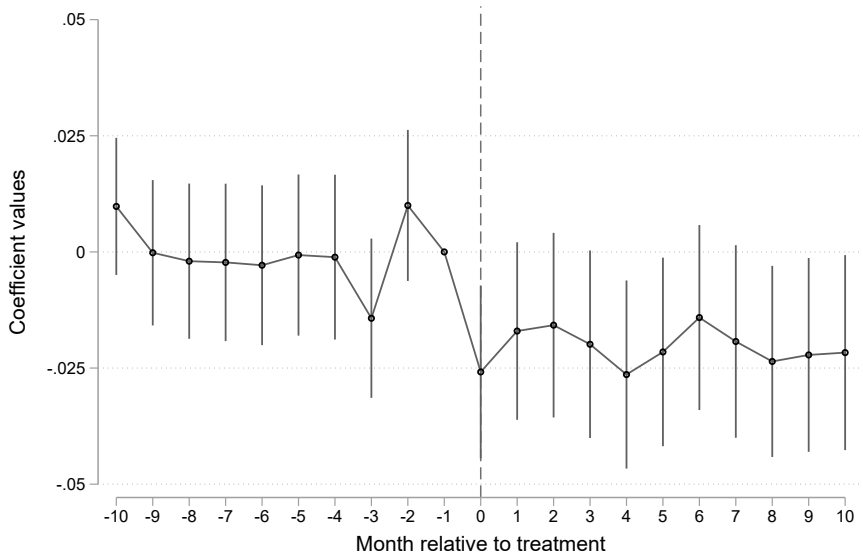
Note: In Panel A, the figure shows the event-study estimates for the treatment effect on seller shortfall amount. The outcome is conditional on having seller shortfall. The mean of the dependent variable in the control group is equal to 1.47. Standard errors are clustered at the firm-pair level. In Panel B, we show the event-study estimates for the treatment effect on transaction size. Data in Panel A and B are winsorized at 0.5%. In Panel C, we show the event-study estimates for the treatment effect on the likelihood of reporting trade. The outcome is conditional on reporting a trade. Standard errors are clustered at the firm-pair level. The means of the dependent variable in the control group are 1.06 and 0.17 in Panel B and C, respectively. The estimated coefficients are displayed in Appendix Table C4. *Source*: Data from VAT returns submitted to the URA. The figures are cited on page 18.

Figure 6
VAT Liability



Notes: This figure displays the effect of treatment on the VAT liability submitted in a given month. Data are winsorized at 0.5%. The line displays the coefficient on treatment for each of the months before and after the treatment. The coefficients are derived from a regression that includes month fixed effects, firm fixed effects, and pre-treatment dummies. The vertical lines are the 95% confidence intervals based on robust standard errors clustered at the firm level. The estimated coefficients are displayed in column (4) of Table C7. The figure is based on 2470 unique firms, 1482 treated firms and 988 control firms. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 18.

Figure 7
Ratio of Sales to Final Consumers



Notes: This figure displays the effect of treatment on the ratio of sales to final consumers over total sales in a given month. The coefficients are derived from a regression that includes month fixed effects, firm fixed effects, and pre-treatment dummies. The vertical lines are the 95% confidence intervals based on robust standard errors clustered at the firm level. The estimated coefficients are displayed in column (5) of Table C7. The figure is based on 2470 unique firms, 1482 treated firms and 988 control firms. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 18.

Online Appendix

For web publication only

A Evidence on Implementation

Figures

Figure A1
Template of the letter sent to firms

Ref: URA/DTD/CMHQ/RISK/«TIN_of_the_buyer»

28th February, 2018

«Name_Of_The_Buyer»
«Physical_Location»
«District», Uganda
Tel: «Tel_no»

Dear Sir/Madam,

RE: NOTIFICATION OF INCREASED VAT COMPLIANCE MONITORING

The Uganda Revenue Authority has developed a new system of monitoring value-added tax (VAT) compliance through reviewing VAT declarations. Therefore, from now on, your VAT declarations are being **closely monitored** to determine your compliance status.

This communication is to draw your attention to a discrepancy **«Sum_of_Des» UGX** that has been detected arising from mismatches between the input VAT claimed by your company and the output VAT declared by **your trading partner «Nameof_The_Seller» (TIN: «TIN_of_the_seller»)** on their sales to you for transactions reported from March 2017 to December 2017. For clarification, some illustrative cases are reported in ***annexure***, attached.

Given these inconsistencies, this is to request you to check your VAT declarations and amend your returns accordingly. You are advised to comply with the above requirement by the 16th of April, 2018. Failure to comply will not only result in additional enforcement measures raised against you in accordance with Section 23 of the Tax Procedures Code (TPC) Act, but could also lead to prosecution in accordance with Section 58 of the TPC. Both sections are quoted in ***annexure***, attached.

If you require any clarification, please contact **XXXX** (email: xxx or Tel: xxx) or at 3rd Floor, Tall tower-Crested Towers or the under signed.

We thank you for your usual cooperation with the URA as we Develop Uganda Together.

«Assistant Commissioner Compliance Management»
For: Commissioner General

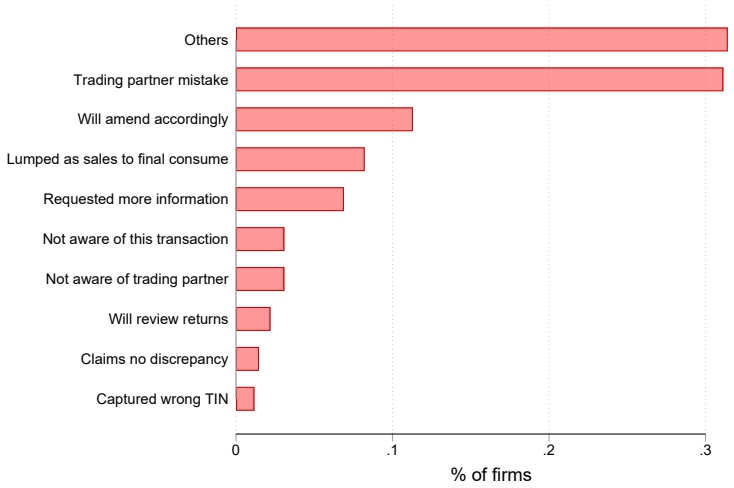
Copy: «Jurisdiction»

Table A1
Feedback from Firms

	LTO	MTO	STO	Overall
Number of firms	80	168	754	1002
Share receiving letters or giving feedback	0.98	0.93	0.89	0.91
Share contacting URA	0.55	0.44	0.34	0.37

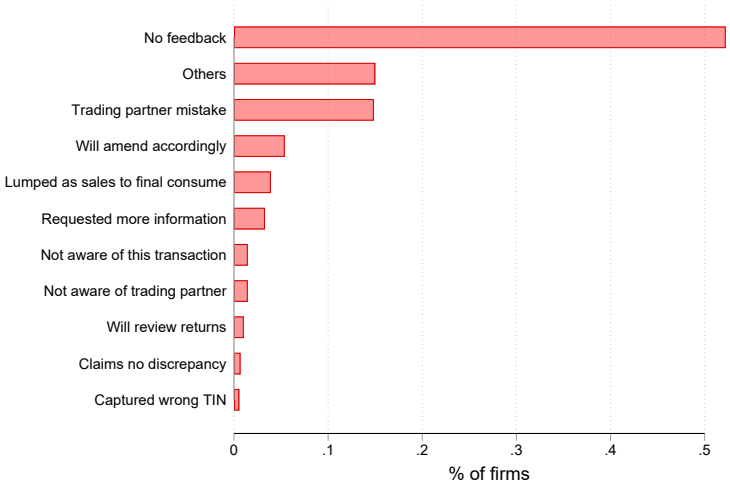
Notes: 1335 firms were initially selected for treatment. 244 firms were removed due to an error in the data extraction that caused some invoices to be duplicated. Out of the remaining 1091 firms, there were 89 firms for which the tin was deactivated or they were already undergoing other procedures in DT and therefore letters were not sent to them. We assume a firm received the communication if it either: 1. received the physical letter, 2. communicated with the URA. Fractions are calculated using the number of firms receiving communication as denominator. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 12.

Figure A2
Responses from Firms that Gave Feedback



Note: This figures display the main feedback submitted by all treated firms that gave feedback to the Ugandan Revenue Authority. If firms gave several feedbacks, all are counted. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 12.

Figure A3
Responses from All Firms

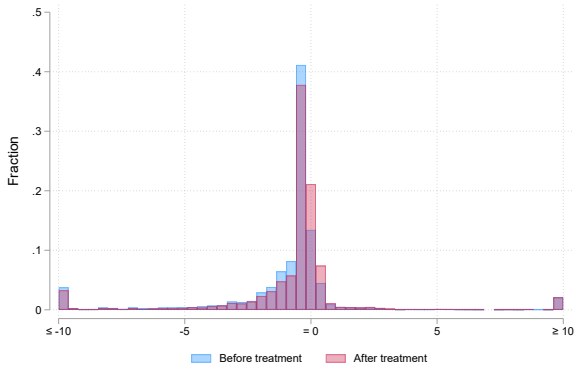


Note: This figures display the main feedback submitted by all treated firms to the Ugandan Revenue Authority. If firms gave several feedbacks, all are counted. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 12.

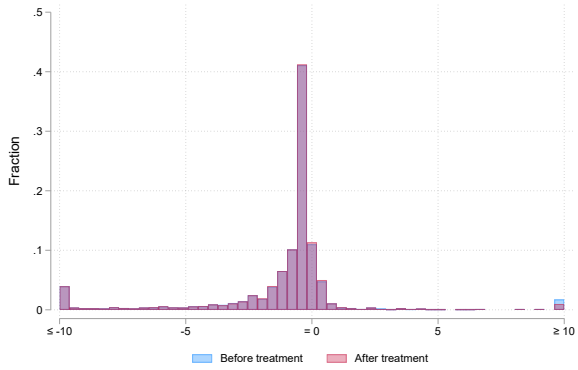
B Impact on Amendments of Past VAT Returns: Additional Results

Figure B1
Distribution of discrepancies before and after treatment

(a) Treatment



(b) Control



Notes: In *Panel A*, we show the distribution of discrepancies before and after the treatment for treated firms. In *Panel B*, we show distribution of discrepancies before and after the treatment for control firms. The figure is based on 2470 unique firms, 1482 treated firms and 988 control firms. Discrepancies smaller than zero imply seller shortfall, while discrepancies larger than zero imply buyer shortfall. *Source*: Data from VAT returns submitted to the URA. The figures are cited on page 14.

Table B1
Direct and Spillover Effects within Firm Pairs: Who Amends?

	Not mentioned in the letter			
Dependent variable:	Resolved		Reduced	
Amendment by:	Seller	Buyer	Seller	Buyer
	(1)	(2)	(3)	(4)
Buyer Only	0.025	0.000	0.040	0.003
	(0.014)	(.)	(0.019)	(0.002)
Seller Only	0.063	0.000	0.129	0.012
	(0.023)	(.)	(0.035)	(0.009)
Buyer and Seller	0.066	0.000	0.121	0.001
	(0.019)	(.)	(0.032)	(0.001)
R-squared	0.028	.	0.052	0.012
Observations	2155	2155	2155	2155
No. of Unique Pairs	530	530	530	530
Mean of Dep. in Control	0.001	0.000	0.008	0.000
P-value of β_S=β_B	0.158	.	0.024	0.263
P-value of β_{SB}=β_B	0.078	.	0.028	0.326
P-value of β_{SB}=β_S	0.913	.	0.853	0.161
Month-Year FE	No	Yes	Yes	Yes

Notes: Sample of VAT returns for pre-treatment months (Mar-2017 to Dec-2017). Analysis is conducted at the *firm-pair month* level. The columns refer to discrepancies between the two firms that could have been reported in the letter, but were not because we only reported the three largest. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data resolving the discrepancy. The results for the months listed in the letter are shown in Table 3 in the paper. Robust standard errors clustered at the firm level *Source*: Data from VAT returns submitted to the URA. The table is cited on page 14.

Table B2
Spillover Effects of Letters on Reducing Past Discrepancies with Other Trading Partners - Eligible and Listed Months

	Eligible Months				Listed Months			
	Reduced		Reduced by Treated		Reduced		Reduced by Treated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.0087		0.0085		0.0098		0.0095	
	(0.0008)		(0.0008)		(0.0009)		(0.0009)	
Any treatment x Client		0.0047		0.0046		0.0068		0.0064
		(0.0008)		(0.0008)		(0.0011)		(0.0011)
Any treatment x Supplier		0.0117		0.0115		0.0120		0.0117
		(0.0011)		(0.0011)		(0.0012)		(0.0012)
R-squared	0.003	0.004	0.003	0.004	0.004	0.004	0.004	0.004
Observations	109384	109384	109384	109384	60978	60978	60978	60978
No. of Unique Pairs	45986	45986	45986	45986	38536	38536	38536	38536
Mean of Dep. in Control	0.0024	0.0024	0.0024	0.0024	0.0027	0.0027	0.0027	0.0027
P-value of β_C=β_S		0.000		0.000		0.001		0.000
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the spillover effects with other trading partners for all eligible months (columns 1 to 4) and for the three listed months (columns 5 to 8). Eligible months refer to months where firms in our sample trade with other partners and in which the transaction displays a discrepancy, keeping the top 3 largest discrepancies for that pair over the period Jul 2017-Dec 2017. Listed months refer to months where firms in our sample trade with other partners and in which the transaction displays a discrepancy, keeping the top 3 largest discrepancies for that pair over the period Jul 2017-Dec 2017, and these months were listed on letters received by firms in our sample. Sample of VAT returns for pre-treatment months (Mar 2017 to Dec 2017). Analysis is conducted at the *firm-pair month* level, with one firm being treated in the study while trading partner is not included. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data reducing the discrepancy. Columns 1, 2, 5 and 6 refer to discrepancies reduced by any firm, while columns 3, 4, 7 and 8 refer to discrepancies reduced by the treated firm. Client refers to trading partners being buyer of treated firm, supplier refers to trading partners being seller of treated firm. The results on reducing discrepancies for all months are shown in Table 4 in the paper. Robust standard errors clustered at the firm level *Source*: Data from VAT returns submitted to the URA. The table is cited on page 15.

Table B3
Spillover Effects of Letters on Resolving Past Discrepancies with Other Trading Partners - All Months

	Resolved		Resolved by Treated		Resolved by Other partner	
	(1)	(2)	(3)	(4)	(5)	(6)
Any treatment	0.0013		0.0012		0.0001	
	(0.0002)		(0.0002)		(0.0001)	
Any treatment x Client		0.0010		0.0009		0.0002
		(0.0003)		(0.0002)		(0.0001)
Any treatment x Supplier		0.0014		0.0014		-0.0000
		(0.0003)		(0.0002)		(0.0001)
R-squared	0.001	0.001	0.001	0.001	0.000	0.000
Observations	260190	260190	260190	260190	260190	260190
No. of Unique Pairs	79880	79880	79880	79880	79880	79880
Mean of Dep. in Control	0.0005	0.0005	0.0002	0.0002	0.0003	0.0003
P-value of β_C=β_S		0.320		0.122		0.164
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the spillover effects with other trading partners for all months. All pre-treatment months where firms in our sample trade with other partners. Sample of VAT returns for pre-treatment months (Mar 2017 to Dec 2017). Analysis is conducted at the *firm-pair month* level, with one firm being treated in the study while trading partner is not included. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data resolving the discrepancy. The first two columns refer to discrepancies resolved by any firm, columns 3ad 4rfer to discrepancies resolved by the treated firm, the last two columns refer to discrepancies resolved by the trading partner of the treated firm. Client refers to trading partners being buyer of treated firm, supplier refers to trading partners being seller of treated firm. The table is a variation of Table 4 in the paper using discrepancies being resolved as the main outcome. Robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 15.

Table B4
Spillover Effects of Letters on Resolving Past Discrepancies with Other Trading Partners - Eligible and Listed Months

	Eligible Months				Listed Months			
	Resolved		Resolved by Treated		Resolved		Resolved by Treated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.0026		0.0026		0.0035		0.0034	
	(0.0004)		(0.0004)		(0.0005)		(0.0004)	
Any treatment x Client		0.0017		0.0016		0.0027		0.0026
		(0.0005)		(0.0004)		(0.0007)		(0.0006)
Any treatment x Supplier		0.0033		0.0033		0.0041		0.0040
		(0.0006)		(0.0005)		(0.0007)		(0.0006)
R-squared	0.001	0.001	0.001	0.001	0.002	0.002	0.002	0.002
Observations	109384	109384	109384	109384	60978	60978	60978	60978
No. of Unique Pairs	45986	45986	45986	45986	38536	38536	38536	38536
Mean of Dep. in Control	0.0005	0.0005	0.0001	0.0001	0.0005	0.0005	0.0001	0.0001
P-value of β_C=β_S		0.022		0.016		0.130		0.080
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the spillover effects with other trading partners for all eligible months (columns 1 to 4) and for the three listed months (columns 5 to 8). Eligible months refer to months where firms in our sample trade with other partners and in which the transaction displays a discrepancy, keeping the top 3 largest discrepancies for that pair over the period Jul 2017-Dec 2017. Listed months refer to months where firms in our sample trade with other partners and in which the transaction displays a discrepancy, keeping the top 3 largest discrepancies for that pair over the period Jul 2017-Dec 2017, and these months were listed on letters received by firms in our sample. Sample of VAT returns for pre-treatment months (Mar 2017 to Dec 2017). Analysis is conducted at the *firm-pair month* level, with one firm being treated in the study while trading partner is not included. The outcome is a dummy variable taking value 1 if there is an amendment in the post-treatment data resolving the discrepancy. Columns 1, 2, 5 and 6 refer to discrepancies resolved by any firm, while columns 3, 4, 7 and 8 refer to discrepancies resolved by the treated firm. Client refers to trading partners being buyer of treated firm, supplier refers to trading partners being seller of treated firm. This table is a variation of Table B2 in Appendix B using resolved discrepancies as the main outcome. Robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 15.

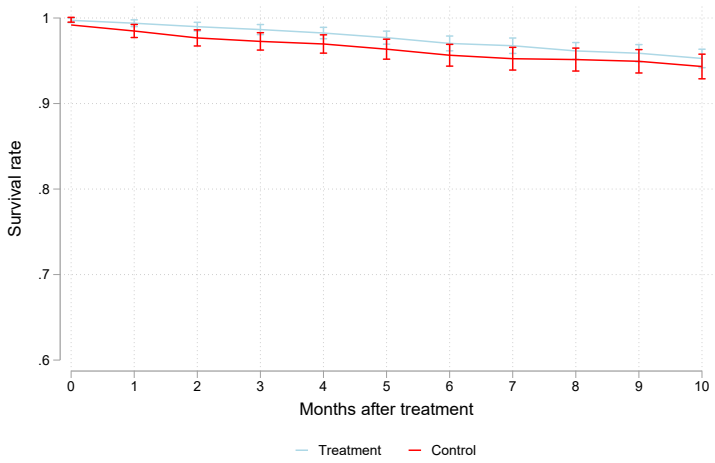
Table B5
Impact of Amendments on VAT Liability of Past Returns, at Firm Level

Dependent variable:	ΔB2B Sales		ΔFinal		ΔTaxable Inputs		ΔVAT Liability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.090		-0.059		-0.002		0.003	
	(0.010)		(0.005)		(0.001)		(0.001)	
Buyer Only		0.028		-0.018		-0.000		0.001
		(0.013)		(0.007)		(0.001)		(0.001)
Seller Only		0.096		-0.064		-0.002		0.003
		(0.013)		(0.007)		(0.001)		(0.001)
Buyer and Seller		0.142		-0.094		-0.003		0.006
		(0.013)		(0.007)		(0.001)		(0.001)
R-squared	0.006	0.008	0.006	0.010	0.001	0.001	0.003	0.004
Observations	23811	23811	23811	23811	23811	23811	23811	23811
No. of Firms	2470	2470	2470	2470	2470	2470	2470	2470
Mean of Dep. in Control	54.066	54.066	68.478	68.478	103.527	103.527	3.423	3.423
Mean of Diff. in Control	0.030	0.030	-0.010	-0.010	0.002	0.002	0.004	0.004
Median of Dep. in Control	1.548	1.548	3.332	3.332	14.175	14.175	0.165	0.165
P-value of β_S=β_B		0.000		0.000		0.203		0.085
P-value of β_{SB}=β_B		0.000		0.000		0.012		0.000
P-value of β_{SB}=β_S		0.002		0.000		0.233		0.022
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the effects of treatment on the monthly VAT returns after the amendments, at the *firm* level. Sample of VAT returns for pre-treatment months (Mar 2017 to Dec 2017). Analysis is conducted at the *firm* level and the sample refers to all months. The outcome is the pre- and post-treatment change in each of the variables listed above the columns measured in thousands of US$. Data are winsorized at 0.5%. The outcomes are calculated as the difference between the pre- and post-treatment of the difference between total taxable sales and final sales for *B2B Sales*, total input tax/0.18 for *Taxable Inputs*, total VAT output tax/0.18 for *Final Sales*, and the difference between total output tax and total input tax for *VAT Liability*. The results for the listed months are shown in Table 5 in the paper. Robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 16.

C Analysis of subsequent behavior: event-study regressions

Figure C1
Balance in Attrition Rates across Treatment Arms



Notes: This figure displays the rate of firms filing in every month after the intervention between treatment and control group. The total number of treated firms is 1482, and the total number of control firms is 988. *Source*: Data from VAT returns submitted to the URA. The figure is cited on page 11.

Figure C2
Taxable sales

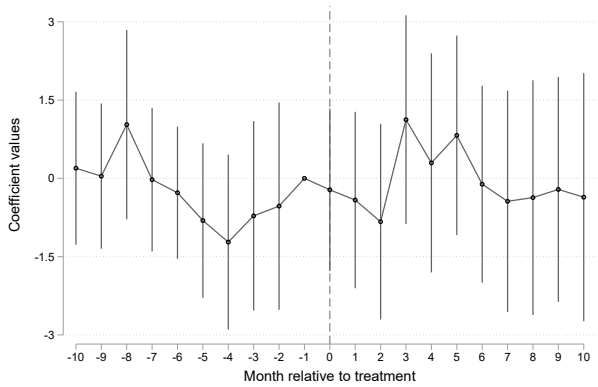
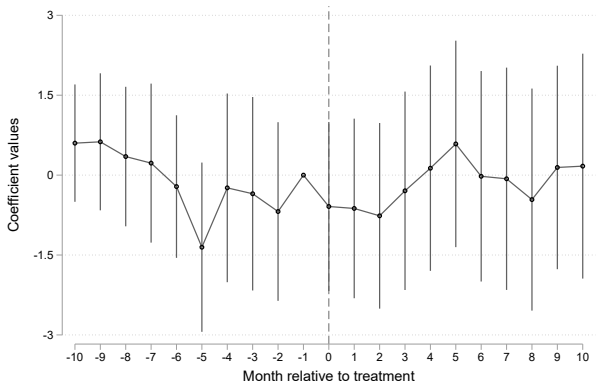


Figure C3
Taxable purchases



Note: The figures display the average of the total taxable sales and total taxable purchases submitted in a given month. Data are winsorized at 0.5%. The line displays the coefficient on treatment for each of the months before and after the treatment. The coefficients are derived from a regression that includes month fixed effects, firm fixed effects, and pre-treatment dummies. The vertical lines are the 95% confidence intervals based on robust standard errors clustered at the firm level. The estimated coefficients are displayed in Table C7. The figure is based on 2470 unique firms, 1482 treated firms and 988 control firms. *Source*: Data from VAT returns submitted to the URA. The figures are cited on page 17.

Figure C4
Probability that VAT Payable is Positive

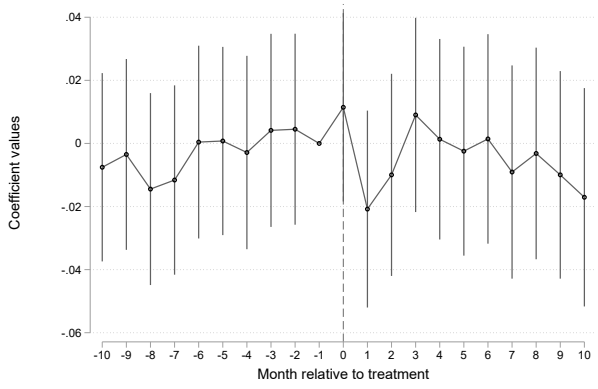
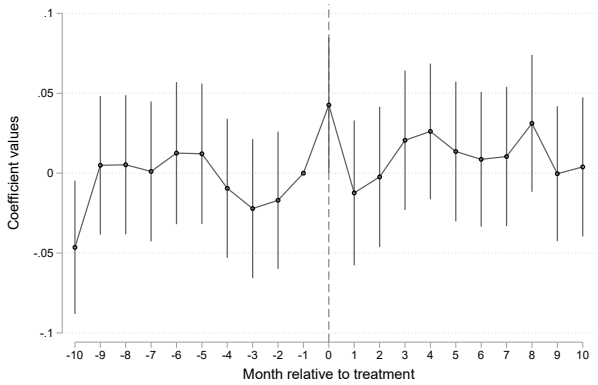


Figure C5
Probability that VAT Payable is Higher than the Same Month in Previous Year



Note: Figure C4 displays effect of treatment on the ratio of firms submitting a VAT return where VAT payable is above 0 in a given month. Figure C5 displays the effect of treatment on the ratio of firms submitting a VAT return where VAT payable in that month is larger than the VAT payable submitted in the same month the year before. The coefficients are derived from a regression that includes month fixed effects, firm fixed effects, and pre-treatment dummies. The vertical lines are the 95% confidence intervals based on robust standard errors clustered at the firm level. The estimated coefficients are displayed in Table C7. The figure is based on 2470 unique firms, 1482 treated firms and 988 control firms. *Source*: Data from VAT returns submitted to the URA. The figures are cited on page 17.

C.1 Trading partner mentioned in letter

Table C1
Event study regressions - Coefficients

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount	(6) Stopping trade
Treatment at time T-10	0.027	-0.021	0.009	-0.231	0.048	0.010
	(0.024)	(0.022)	(0.020)	(0.130)	(0.078)	(0.018)
Treatment at time T-9	0.039	-0.014	-0.020	-0.212	0.023	0.024
	(0.026)	(0.021)	(0.022)	(0.141)	(0.078)	(0.021)
Treatment at time T-8	-0.005	-0.006	0.009	-0.112	0.076	0.018
	(0.026)	(0.020)	(0.023)	(0.130)	(0.081)	(0.023)
Treatment at time T-7	0.012	-0.003	-0.012	-0.154	0.098	-0.005
	(0.027)	(0.021)	(0.023)	(0.121)	(0.089)	(0.023)
Treatment at time T-6	0.030	-0.021	-0.012	-0.176	0.000	-0.025
	(0.027)	(0.020)	(0.024)	(0.121)	(0.086)	(0.023)
Treatment at time T-5	0.004	0.001	-0.004	-0.157	0.105	-0.009
	(0.027)	(0.022)	(0.024)	(0.116)	(0.089)	(0.023)
Treatment at time T-4	-0.004	-0.010	0.014	-0.192	0.106	0.014
	(0.028)	(0.021)	(0.024)	(0.118)	(0.081)	(0.023)
Treatment at time T-3	-0.015	0.029	-0.012	-0.052	0.175	0.009
	(0.027)	(0.022)	(0.023)	(0.116)	(0.093)	(0.022)
Treatment at time T-2	0.015	-0.003	-0.015	-0.080	0.108	-0.000
	(0.024)	(0.022)	(0.022)	(0.118)	(0.092)	(0.019)
Treatment at time T-1	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Treatment at time T	-0.142	0.048	0.092	0.112	0.150	-0.035
	(0.029)	(0.027)	(0.026)	(0.122)	(0.101)	(0.018)
Treatment at time T+1	-0.123	0.031	0.099	-0.117	0.185	-0.047
	(0.032)	(0.027)	(0.029)	(0.128)	(0.119)	(0.019)
Treatment at time T+2	-0.138	0.060	0.062	-0.157	-0.004	-0.024
	(0.030)	(0.026)	(0.028)	(0.109)	(0.096)	(0.019)
Treatment at time T+3	-0.073	0.003	0.054	-0.123	-0.077	-0.016
	(0.033)	(0.027)	(0.030)	(0.134)	(0.084)	(0.020)
Treatment at time T+4	-0.077	0.034	0.025	-0.198	-0.037	-0.014
	(0.033)	(0.028)	(0.029)	(0.129)	(0.110)	(0.019)
Treatment at time T+5	-0.066	0.049	0.027	-0.220	-0.205	-0.028
	(0.035)	(0.027)	(0.032)	(0.141)	(0.100)	(0.019)
Treatment at time T+6	-0.096	0.035	0.060	-0.210	-0.179	-0.016
	(0.035)	(0.029)	(0.034)	(0.158)	(0.103)	(0.019)
Treatment at time T+7	-0.143	0.043	0.091	0.064	0.051	-0.033
	(0.037)	(0.031)	(0.034)	(0.160)	(0.149)	(0.019)
Treatment at time T+8	-0.066	0.016	0.050	0.008	-0.048	0.014
	(0.037)	(0.030)	(0.032)	(0.158)	(0.101)	(0.019)
Treatment at time T+9	-0.117	0.048	0.055	-0.075	0.070	0.028
	(0.038)	(0.029)	(0.032)	(0.134)	(0.146)	(0.019)
Treatment at time T+10	-0.043	0.022	0.030	-0.045	0.205	0.009
	(0.036)	(0.027)	(0.027)	(0.134)	(0.135)	(0.019)
R-squared	0.521	0.342	0.447	0.754	0.675	0.463
Observations	12743	12743	12743	12743	8700	31996
No. of Unique Pairs	1104	1104	1104	1104	964	1260
Mean of Dep. in Control	0.733	0.120	0.135	1.010	0.541	0.392
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, transaction size, and probability of stopping to report trade. The values refer to the coefficients in Figure 2 and Figure 3. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

Table C2
Event study regressions

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount	(6) Stopping trade
DiD specification						
Treatment	-0.123	0.057	0.061	0.007	0.013	-0.012
	(0.025)	(0.019)	(0.020)	(0.108)	(0.061)	(0.017)
R-squared	0.521	0.342	0.447	0.754	0.675	0.463
Observations	12743	12743	12743	12743	8700	31996
No. of Unique Pairs	1104	1104	1104	1104	964	1260
Mean of Dep. in Control	0.733	0.120	0.135	1.010	0.541	0.392
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, transaction size, and probability of stopping to report trade. In the *DiD specification*, the coefficient displayed is the average coefficients for the 10 months post-treatment. In the *Event-study specification*, the coefficients displayed correspond the effect of treatment in a given month. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

Table C3
Event study regressions - By treatment arm

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount
DiD specification					
Treat Buyer	-0.083	0.010	0.060	-0.054	0.067
	(0.031)	(0.022)	(0.026)	(0.154)	(0.103)
Treat Seller	-0.129	0.089	0.051	0.201	-0.004
	(0.040)	(0.032)	(0.032)	(0.185)	(0.123)
Treat Buyer & Seller	-0.151	0.068	0.071	-0.110	-0.036
	(0.036)	(0.026)	(0.031)	(0.148)	(0.072)
R-squared	0.522	0.344	0.447	0.754	0.675
Observations	12743	12743	12743	12743	8700
No. of Unique Pairs	1104	1104	1104	1104	964
Mean of Dep. in Control	0.733	0.120	0.135	1.010	0.541
Firm FE	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, and transaction size. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

C.2 Other trading partners

Table C4
Event study regressions - Coefficients

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount	(6) Stopping trade
Treatment at time T-10	0.004	-0.004	0.000	-0.015	-0.024	0.004
	(0.002)	(0.002)	(0.002)	(0.022)	(0.039)	(0.001)
Treatment at time T-9	0.003	-0.002	-0.000	0.007	-0.019	0.002
	(0.002)	(0.002)	(0.002)	(0.022)	(0.037)	(0.002)
Treatment at time T-8	0.002	-0.003	0.001	0.017	-0.018	0.002
	(0.002)	(0.002)	(0.002)	(0.022)	(0.040)	(0.002)
Treatment at time T-7	0.003	0.001	0.000	0.012	0.027	0.003
	(0.002)	(0.002)	(0.002)	(0.023)	(0.043)	(0.002)
Treatment at time T-6	0.002	0.004	0.001	-0.000	-0.053	0.001
	(0.002)	(0.002)	(0.002)	(0.024)	(0.041)	(0.002)
Treatment at time T-5	0.002	-0.004	0.003	-0.017	-0.019	0.001
	(0.002)	(0.002)	(0.002)	(0.022)	(0.043)	(0.002)
Treatment at time T-4	0.004	-0.006	0.002	-0.014	-0.072	0.005
	(0.002)	(0.002)	(0.002)	(0.022)	(0.043)	(0.002)
Treatment at time T-3	0.004	-0.004	0.002	-0.030	-0.047	0.004
	(0.002)	(0.002)	(0.002)	(0.022)	(0.044)	(0.002)
Treatment at time T-2	-0.001	-0.002	0.001	-0.004	-0.050	-0.001
	(0.002)	(0.002)	(0.001)	(0.019)	(0.038)	(0.001)
Treatment at time T-1	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Treatment at time T	-0.006	0.002	0.003	-0.006	-0.078	0.014
	(0.002)	(0.002)	(0.001)	(0.019)	(0.040)	(0.001)
Treatment at time T+1	-0.006	0.003	0.002	-0.017	-0.133	0.009
	(0.002)	(0.002)	(0.002)	(0.020)	(0.044)	(0.002)
Treatment at time T+2	-0.006	0.005	0.000	0.006	-0.076	0.010
	(0.002)	(0.002)	(0.002)	(0.021)	(0.044)	(0.002)
Treatment at time T+3	-0.001	-0.001	0.002	0.019	-0.033	0.008
	(0.002)	(0.002)	(0.002)	(0.023)	(0.050)	(0.002)
Treatment at time T+4	-0.004	0.003	0.001	0.034	-0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.022)	(0.045)	(0.002)
Treatment at time T+5	0.006	-0.005	-0.001	-0.007	-0.077	0.014
	(0.002)	(0.002)	(0.002)	(0.021)	(0.046)	(0.002)
Treatment at time T+6	-0.003	0.002	0.000	0.014	-0.025	0.011
	(0.002)	(0.002)	(0.002)	(0.021)	(0.043)	(0.002)
Treatment at time T+7	0.002	-0.001	-0.002	0.012	-0.027	0.010
	(0.002)	(0.002)	(0.002)	(0.022)	(0.044)	(0.002)
Treatment at time T+8	0.005	-0.002	-0.003	0.019	0.023	0.009
	(0.002)	(0.002)	(0.002)	(0.022)	(0.049)	(0.002)
Treatment at time T+9	0.000	-0.010	0.008	-0.010	-0.084	0.002
	(0.002)	(0.002)	(0.002)	(0.024)	(0.057)	(0.002)
Treatment at time T+10	-0.003	-0.005	0.007	0.003	-0.074	-0.001
	(0.003)	(0.002)	(0.002)	(0.026)	(0.053)	(0.001)
R-squared	0.934	0.926	0.675	0.830	0.872	0.364
Observations	629206	629206	629206	629206	230432	3981093
No. of Unique Pairs	82700	82700	82700	82700	28776	145071
Mean of Dep. in Control	0.384	0.567	0.044	1.055	1.472	0.168
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, transaction size, and probability of stopping to report trade. The values refer to the coefficients in Figure 4 and Figure 5. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

Table C5
Event study regressions

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount	(6) Stopping trade
DiD specification						
Treatment	-0.002	0.001	0.001	-0.008	-0.063	0.006
	(0.001)	(0.001)	(0.001)	(0.018)	(0.041)	(0.003)
R-squared	0.934	0.926	0.675	0.830	0.872	0.455
Observations	629206	629206	629206	629206	230432	18972333
No. of Unique Pairs	82700	82700	82700	82700	28776	145071
Mean of Dep. in Control	0.384	0.567	0.044	1.055	1.472	0.168
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, transaction size, and probability of stopping to report trade. In the *DiD specification*, the coefficient displayed is the average coefficients for the 10 months post-treatment. In the *Event-study specification*, the coefficients displayed correspond to the effect of treatment in a given month. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

Table C6
Event study regressions - By treatment arm

	(1) Seller shortfall	(2) Buyer shortfall	(3) No discrepancy	(4) Transaction size	(5) Seller shortfall amount
DiD specification					
Treat Buyer	-0.010	0.005	0.004	0.016	-0.072
	(0.003)	(0.002)	(0.002)	(0.032)	(0.051)
Treat Seller	-0.008	0.004	0.002	-0.032	-0.078
	(0.002)	(0.002)	(0.002)	(0.031)	(0.101)
Treat Buyer & Seller	0.003	-0.003	-0.001	-0.006	-0.050
	(0.001)	(0.002)	(0.001)	(0.022)	(0.055)
R-squared	0.934	0.926	0.675	0.830	0.872
Observations	629206	629206	629206	629206	230432
No. of Unique Pairs	82700	82700	82700	82700	28776
Mean of Dep. in Control	0.384	0.567	0.044	1.055	1.472
Firm FE	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the effect of treatment on the probability of seller shortfall, the probability of reporting no discrepancy, and transaction size. All regression include robust standard errors clustered at the firm level. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 17.

C.3 Firm-level outcomes

Table C7
Event study regressions - Coefficients

	(1) B2B Sales	(2) Final Sales	(3) Taxable Inputs	(4) VAT Liability	(5) FC Ratio
Treatment at time T-10	0.689	-0.269	3.193	-0.378	0.010
	(2.708)	(1.759)	(3.049)	(0.518)	(0.008)
Treatment at time T-9	0.938	-0.518	3.289	-0.299	-0.000
	(2.793)	(1.996)	(3.601)	(0.532)	(0.008)
Treatment at time T-8	3.280	-0.331	1.741	0.306	-0.002
	(3.222)	(2.073)	(3.632)	(0.566)	(0.009)
Treatment at time T-7	1.347	-0.195	1.534	-0.222	-0.002
	(2.864)	(2.049)	(4.123)	(0.445)	(0.009)
Treatment at time T-6	-1.026	0.447	-0.930	-0.275	-0.003
	(2.299)	(2.155)	(3.686)	(0.440)	(0.009)
Treatment at time T-5	-0.547	-1.851	-7.034	0.199	-0.001
	(2.806)	(2.589)	(4.352)	(0.478)	(0.009)
Treatment at time T-4	-4.182	-1.548	-1.109	-0.902	-0.001
	(2.971)	(3.027)	(4.849)	(0.511)	(0.009)
Treatment at time T-3	-0.047	-2.479	-1.321	-0.151	-0.014
	(3.029)	(3.410)	(5.003)	(0.571)	(0.009)
Treatment at time T-2	-0.704	-2.449	-3.604	-0.554	0.010
	(3.319)	(2.890)	(4.553)	(0.543)	(0.008)
Treatment at time T-1	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)
Treatment at time T	-1.628	0.540	-3.001	0.307	-0.026
	(2.674)	(3.185)	(4.407)	(0.495)	(0.009)
Treatment at time T+1	-3.060	0.829	-3.715	0.155	-0.017
	(2.983)	(3.378)	(4.683)	(0.503)	(0.010)
Treatment at time T+2	-4.474	-0.191	-4.267	0.296	-0.016
	(3.471)	(3.450)	(4.805)	(0.530)	(0.010)
Treatment at time T+3	6.797	-0.982	-1.580	0.936	-0.020
	(3.950)	(3.407)	(5.162)	(0.597)	(0.010)
Treatment at time T+4	0.670	-0.409	0.680	-0.377	-0.026
	(3.454)	(3.486)	(5.323)	(0.518)	(0.010)
Treatment at time T+5	5.580	-0.731	3.242	0.341	-0.022
	(3.459)	(3.302)	(5.382)	(0.572)	(0.010)
Treatment at time T+6	1.235	-1.002	-0.162	0.253	-0.014
	(3.328)	(3.371)	(5.441)	(0.608)	(0.010)
Treatment at time T+7	0.352	-0.615	0.116	0.161	-0.019
	(3.564)	(3.753)	(5.735)	(0.546)	(0.011)
Treatment at time T+8	1.466	-0.212	-2.236	0.004	-0.024
	(4.068)	(3.641)	(5.705)	(0.641)	(0.010)
Treatment at time T+9	-1.073	1.854	1.001	-0.197	-0.022
	(3.653)	(3.963)	(5.265)	(0.555)	(0.011)
Treatment at time T+10	1.254	-2.197	0.918	-0.125	-0.022
	(4.050)	(3.729)	(5.811)	(0.631)	(0.011)
R-squared	0.795	0.886	0.843	0.554	0.825
Observations	61768	61768	61768	61768	55024
No. of Firms	1990	1990	1990	1990	1971
Mean of Dep. in Control	49.772	62.660	96.641	3.446	0.562
Firm FE	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Pre-treatment dummies	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the event-study coefficients of the effect of treatment on (1) B2B sales, the difference between total taxables sales and total final sales, (2) Total final sales, (3) Total taxable inputs, (4) The difference between total taxable sales and total taxable inputs, and (5) the ratio of sales to final consumers. Data are winsorized at 0.5%. The coefficients are derived from a regression that includes month fixed effects, firm fixed effects, and pre-treatment dummies. All regression include robust standard errors clustered at the firm level. The values in columns (4) and (5) refer to the coefficients in Figure 6 and Figure 7, respectively. *Source*: Data from VAT returns submitted to the URA. The table is cited on page 18.

D Data correction following extraction problem

In this appendix we discuss the error that cause us to inflate the occurrences and amounts of seller shortfall between firm-pairs. We show that it is balanced across treatment arm, indicating that correcting for it did not bias our results.

The error occurs as a result of a mistake in the script that extracted the administrative data from the URA database. As one can observe in table D1, the share of firm-pairs this affected was balanced across treatment arms indicating that this did not bias our sample.

Table D1
Balance table for number of firm-pairs missing

	N pairs	N missing pairs	Share missing pairs
Control	676	182	0.27
Treatment	1029	288	0.28
By treatment arm			
Buyer and seller	346	85	0.25
Buyer only	343	101	0.29
T-Seller only	340	102	0.30

Notes: This table displays the number and share of firm pairs that were notified of a discrepancy purely because of the data extraction error that duplicated the reported inputs by firms for the period 19/9/2017 to 15/10/2017.

We formally test whether being part of a treatment arm is correlated with whether the firm was included because of the data correction error in table D2. In column (1) we run the regression using an indicator variable for whether the firm-pair was treated and in column (2) we run the regression with separate indicator variables for each treatment arm. The omitted indicator is therefore always the control group. The individual coefficient are small and never statistically significant. Furthermore, the F-test for the joint significance of all coefficients is clearly rejecting that they are different from the constant.

Table D2
Testing whether treatment predicts that the firm-pair is missing

Dependent variable:	Prob. of missing	
	(1)	(2)
Treatment	0.011	
	(0.022)	
Buyer and seller		-0.024
		(0.029)
Buyer only		0.025
		(0.030)
T-Seller only		0.031
		(0.030)
Constant	0.269	0.269
	(0.017)	(0.017)
R-squared	0.000	0.002
Observations	1705	1705
P-value of F-test	0.630	0.341

Notes: Sample refers to all firm-pairs that were originally included in our base-sample. Analysis is conducted at the *firm-pair* level. The outcome is a dummy variable taking value 1 if the firm-pair only featured a discrepancy because of the data extraction error and is hence excluded from our analysis. Robust standard errors clustered at the firm-pair level. *Source*: Data from VAT returns submitted to the URA.

E Cost calculation

Two types of costs are associated with this programme: 1) delivering the physical letters 2) the time of URA staff.

The first is calculated by multiplying the price of sending a letter to a given region with the number of letters sent to that region. The total cost of sending all letters is $3,301.

The second is calculated based on lengthy discussion with officers at the URA. We calculate that it took 8 full working days for 2 officers to prepare the 1325 letters for send-off.[39] Taxpayers got in touch with the URA upon receiving the letter. The time it took to respond to each taxpayer depended on the type of communication. Specifically, when a firm visited the URA it took around 45 minutes, when a firm sent an email or physical letter it took around 25 minutes to respond (since the information provided by the taxpayer in the email often needed to be reconciled with information in the URA's database), and if a firm called it took around 5 minutes. Finally, after firms had responded, DT compared the firms' responses to the information in their database. All in all, responding and reconciling the information from taxpayers took approximately 5 full working days for 2 officers. Assuming a full working day is 8 hours, the total numbers of hours it took to undertake this study was approximately 450.

We convert this to monetary costs by multiply the number of hours with the average hourly salary, before deductions, for a junior URA officer. We calculate that the monetary cost associated with the number of hours worked is $2,415.

The total cost incurred by the URA for the intervention is thus $5,716.[40]

[39] Note that this is larger than the number quoted throughout the paper, which is due to the data error discussed in detail in Appendix D.

[40] In this calculation we have excluded the cost of the time used by the researchers and staff in RPD to identify the discrepancies and firm-pairs. We have chosen to do so because this can be automated.