Strategic or Confused Firms? Evidence from "Missing" Transactions in Uganda*

Miguel Almunia	Jonas Hjort	Justine Knebelmann	Lin Tian
CUNEF Univ. & CEPR	Columbia Univ. & CEPR & NBER	Paris School of Economics	INSEAD & CEPR

July 2021

Abstract

Are firms sophisticated maximizers, or do they consistently make errors? Using transaction-level data from Ugandan value-added tax (VAT) returns, we show that sellers and buyers report different amounts 79% of the time, despite invoices being easily cross-checked. We estimate that 25% of firms are disadvantageous misreporters—they systematically misreport own sales and purchases such that their tax liability increases—while 75% are advantageous misreporters. Many firms—especially disadvantageous misreporters—fail to report imported inputs they themselves reported at Customs, increasing their VAT liability. On net, unilateral VAT misreporting cost Uganda about US$384 million in foregone 2013-2016 tax revenue.

*Contact: miguel.almunia@cunef.edu, hjort@columbia.edu, justine.knebelmann@psemail.eu, lin.tian@insead.edu.

We thank editor Rema Hanna, three anonymous referees, Michael C. Best, Ray Fisman, Francois Gerard, Jim Hines, Wojciech Kopczuk, David Margolis, Joana Naritomi, Daniel Reck, Floris Zoutman, and many seminar and conference participants for comments and suggestions that substantially improved the paper; Pablo García Guzmán, David Henning, and Claude Raisaro for outstanding research assistance; the Uganda Revenue Authority for data sharing and excellent collaboration; and the International Growth Centre, the British Academy, and the Leverhulme Foundation for funding. The views in this paper are those of the authors, and do not necessarily represent those of the Uganda Revenue Authority.

1 Introduction

In economics, firms are seen as sophisticated organizations—maximizers that make constrained but optimal decisions by carefully assessing the true costs and benefits to themselves. This assumption underlies the models that guide our understanding of how firms behave. Strategic decision-making by firms is by and large taken as self-evident.

There is, however, growing evidence that some firms deviate from optimal behavior.[1] If a significant proportion consistently makes mistakes, the consequences for theory and policy design would be far-reaching. Consider how firms in low-income countries should be taxed—one of the most important questions for economic development (Besley & Persson, 2009; Kleven *et al.* , 2016). The value-added tax (VAT)—now in use in 166 countries around the world—is popular among economists in part because of its enforcement properties. In firm-to-firm transactions, the seller and buyer face asymmetric (mis)reporting incentives and their reports can easily be cross-checked (Ebrill *et al.* , 2001; Kopczuk & Slemrod, 2006; Pomeranz, 2015). This is thought to make the VAT "self-enforcing," but the argument assumes a degree of cross-checking capacity and, more fundamentally, that firms infer the likelihood of such checks and accurately keep track of their sales and purchases.

In this paper, we study the sophistication of firms' decision-making in a low-income country context by analyzing their tax reporting behavior. We use 2013-2016 transaction-level VAT and Customs records on all domestic and international trade involving the 22,388 VAT-registered firms in Uganda. In the first part of our analysis, we document that sellers and buyers report different transacted amounts in 79% of reported firm-pair×month VAT observations. In 60% of mismatch transactions we find a *seller shortfall*, namely the seller reporting the lower value, and in the remaining 40% a *buyer shortfall*. The latter cases are harder to rationalize since the buyer reporting less than the seller raises one or both firms' tax liability, other things equal.

In the second part of our analysis, we develop a fixed-effects methodology that estimates what fraction of each reporting discrepancy can be attributed to the seller vs. the buyer, holding constant each firm's identity and those of its other trade partners. Combining individual firms' estimated reporting discrepancies as buyer and seller in turn allows us to categorize their reporting behavior. Some overreport total purchases and/or underreport total sales such that the firm's overall liability decreases—what we interpret as strategic behavior in a low-enforcement context and label *advantageous* misreporting; and some make systematic *disadvantageous* reporting mistakes that increase the firm's overall

[1] See, among others, Hortacsu & Puller (2008); Cho & Rust (2010); Goldfarb & Xiao (2011); DellaVigna & Gentzkow (2019); Kremer *et al.* (2019); Hjort *et al.* (2020); Dube *et al.* (2020); Tourek (2021).

liability.[2]

We find that 75% of VAT-registered Ugandan firms are advantageous misreporters and 25% are disadvantageous misreporters. Among advantageous misreporters, 10% "look small" by underreporting both sales and purchases and the firm's value-added (a form of fly-under-the-radar behavior first identified by Carrillo *et al.* (2017) in Ecuador). Another 78% are "conspicuous" advantageous misreporters that underreport their sales and overreport their purchases. The remaining 12% "look big" by overreporting both sales and purchases. Over time, 74% (65%) of firms classified as advantageous (disadvantageous) remain in the same category as in the previous year.

In a series of robustness checks, we analyze several ways in which our estimates could under- or overestimate the prevalence of reporting mistakes. We re-estimate our model assuming extensive final sales underreporting, finding that the proportion of disadvantageous firms remains large. When we restrict to firms for which we can reject liability-neutral tax reporting at conventional significance levels, the firm classification is very similar to the baseline, with 23% of disadvantageous firms. Finally, event studies looking at firms switching trade partners strongly substantiate a causal interpretation of the fixed-effects model estimates.

In the third part of our analysis we consider how sophisticated and less sophisticated firms behave in higher state capacity contexts. The case for the VAT assumes some degree of capacity to cross-check firms' tax reports. Our results suggest that low-income countries may not have such capacity. However, like models of firms' response to other public policies, the self-enforcing VAT hypothesis ultimately rests on a more fundamental assumption: that firms behave strategically. Mis-optimizing firms may not respond as anticipated to enforcement incentives.

To investigate, we take advantage of goods being more closely monitored when moving through Customs.[3] We compare an import transaction report at Customs versus the *same firm's* report of the same transaction on the credit side of its domestic VAT records. While, as expected, double reports are more consistent when the same firm makes both reports and one of the two is at Customs, we find discrepancies in a remarkable 48% of such cases. In particular, we again find evidence of firm mistakes. Firms reduce their tax liability by overreporting their imported inputs in VAT returns in 14% of import transactions, while they increase their liability by underreporting in VAT returns in 34% of trans-

[2] We interpret *systematic* underreporting of a firm's liability as strategic behavior and systematic overreporting of a firm's liability as mistakes. By classifying any systematic, self-advantageous reporting errors as strategic behavior, we possibly underestimate the true extent of reporting mistakes.

[3] It is well documented that tariffs are more stringently enforced than domestic taxes, perhaps because goods have to physically clear Customs (Riezman & Slemrod, 1987; Keen & Lighart, 2002; Emran & Stiglitz, 2005; Keen & Lighart, 2005; Baunsgaard & Keen, 2010; Cagé & Gadenne, 2018).

actions. Importantly, the latter form of disadvantageous behavior is significantly more common among firms classified as disadvantageous misreporters in domestic VAT data.

Overall, our findings suggest that the majority of Ugandan firms are sophisticated enough to respond to weak tax enforcement by considerably underreporting their tax liability, as conventional models of firm behavior assume. However, a non-negligible proportion consistently make costly errors. We quantify the consequences for tax collection, accounting for each firm's misreporting and outstanding VAT liability position. We estimate that the government revenue *gain* due to reporting errors by disadvantageous misreporters is large—around US$138 million during 2013-2016. However, the revenue loss due to misreporting by advantageous misreporters is even larger, at around US$522 million. On net, unilateral VAT misreporting cost the Ugandan government around US$384 million, or 4% of total tax revenue collected, during 2013-2016.

This paper provides what to our knowledge are the first direct estimates of the extent of mistakes in an economy-wide population of firms. The methodology we develop allows us to classify individual firms' behavior as self-advantageous or not, and we observe the entire population of formal, non-micro firms in Uganda's economy. Our analysis builds on an emerging body of evidence of seemingly erroneous firm behavior (see footnote 1).[4]

We also contribute new evidence on how tax evasion responds to the state's enforcement capacity, and in particular how firms characterized by different degrees of sophistication respond. In this sense, our analysis builds most closely—methodologically and thematically—on Fisman & Wei (2004)'s "mirror" data approach to measuring how tariff evasion responds to the tariff rate. However, our focus is on variation in enforcement capacity, linking our analysis with existing work on the causes and consequences of state capacity (Besley & Persson, 2009, 2010; Acemoglu *et al.* , 2015; Page & Pande, 2018; Best *et al.* , 2019). We also build on existing studies of more-vs.-less attentive taxpayers' response to tax rates.[5]

Finally, we show evidence that the VAT is far from self-enforcing in low state capacity settings. This qualifies the common argument that developing countries are especially likely to benefit from use of the VAT (see, e.g., Bird & Gendron, 2007).[6] In doing so, our

[4] Tourek (2021) documents another form of seemingly suboptimal taxpayer behavior—firms reporting identical amounts in their income tax year after year—in neighboring Rwanda.

[5] Chetty *et al.* (2009); Aghion *et al.* (2017); Benzarti (2020); Gillitzer & Skov (2018); Rees-Jones & Taubinsky (2018) provide direct evidence of tax-reporting mistakes by *individuals* (see also Reck (2016)). Like this paper, Aghion *et al.* (2017) show evidence that more sophisticated taxpayers tend to react as theory predicts to tax incentives, while less sophisticated taxpayers do so to a lesser extent.

[6] Tax evasion research has demonstrated the importance of third-party reporting (Slemrod *et al.* , 2001; Kleven *et al.* , 2011; Kleven, 2014), but also its limitations (Pomeranz, 2015; Carrillo *et al.* , 2017; Slemrod *et al.* , 2017; Almunia & Lopez-Rodriguez, 2018; Waseem, 2018). The existing literature shows that in middle-income countries whose enforcement capacity significantly exceeds Uganda's, authorities' ability to cross-

analysis builds on work studying how policy should be tailored to context (see, e.g., Laffont, 2005; Best *et al.* , 2015, 2019; Duflo *et al.* , 2018; Hansman *et al.* , 2019). The massive magnitude of the revenue loss from VAT evasion we document in Uganda—and the corresponding cross-country patterns in Cagé & Gadenne (2018)—suggests that the production efficiency benefits of VATs relative to tariffs are at least in part offset by capacity-constrained governments' ability to raise revenue on domestic transactions.

2 Background

Uganda's tax-to-GDP ratio, at 13% in 2016, is below the African and OECD averages of 18 and 34% (OECD, 2018), while its tax administration costs (2.4% of tax revenues) are similar to other low-income countries (IMF, 2013; Lemgruber *et al.* , 2015).

The VAT was introduced in 1996 and in 2016 contributed 32% of Uganda's total non-tariff tax revenue, similar to elsewhere in Africa (OECD, 2018). Its design is standard, with a general rate of 18%, a credit-invoice system, standard exemptions (e.g., financial services), and zero-rating (e.g., exports). Appendix A provides details.

Since 2012 all VAT-registered firms must file their monthly VAT declarations electronically, within 15 days of the transaction month ending.[7] These must include detailed transaction-level records—spreadsheets listing each sale to and purchase from other VAT-registered firms. This implies that the Uganda Revenue Authority (URA) receives two reports for each transaction between any two VAT-registered firms.

Our analysis exploits the complete administrative data from VAT-registered firms' declarations between 2013 and 2016.[8] The monthly firm-level VAT data include a scrambled Tax Identification Number (TIN), the declaration date, total sales/purchases (amount and VAT charged/paid), total VAT liabilities, and data from the spreadsheets—called VAT "schedules"—detailing each transaction. The schedules include the transaction date, the seller and buyer TINs, the transaction value, and the VAT charged or paid. Schedule 1 (VS1) contains all sales transactions to other VAT-registered firms. Sales to final consumers or non-VAT firms are recorded only as a monthly aggregate. Schedules 2, 3, and 4 contain domestic input purchases, imports, and administrative expenses, respectively. Importantly, the transaction-level records reported in the VAT schedules constitute mean-

check VAT records tends to reduce evasion (Ebrill *et al.* , 2001; Pomeranz, 2015; Carrillo *et al.* , 2017; Mittal & Mahajan, 2017; Waseem, 2020; Naritomi, 2019; Fan *et al.* , 2019). Discrepancies in VAT declarations comparable to what we observe in Uganda are found in Rwanda (Mascagni *et al.* , 2019).

[7] About 80% of VAT returns are reported within 15 days of the return month and another 9% within the next month.

[8] We refer to fiscal year 2013/14 as 2013.

ingful paper trails: they are consistent with the firm-level reports in 97% of cases.

Our dataset contains 22,388 unique VAT-registered firms submitting at least one monthly VAT return between 2013 and 2016, and the transactions data cover 15,569 sellers and 19,421 buyers, leading to 3,373,183 seller-buyer-month observations.[9]

The data on imports comes from Customs declarations submitted to the URA between 2012 and 2016. These are transaction-specific, submitted electronically, and include the value of the goods imported, the type and number of items, and the date of import. The TIN of the importer allows us to match the Customs data to the domestic VAT data. 9,998 VAT-registered firms import at least once.

3 Discrepancies in VAT Declarations

In this section, we document massive VAT reporting discrepancies in Uganda at the seller-buyer-month level.

3.1 Conceptual background

For a date j transaction, let y^S_{sbj} and y^B_{sbj} denote the output VAT charged (as reported by the seller s) and the input VAT paid (as reported by the buyer b). We aggregate transactions at the monthly level and define $Y^S_{sbt} \equiv \sum_{j \in J_t} y^S_{sbj}$ and $Y^B_{sbt} \equiv \sum_{j \in J_t} y^B_{sbj}$ where t denotes the transaction month. We define *seller shortfall* as the total VAT charged being *lower* than the total VAT paid, i.e., $Y^S_{sbt} < Y^B_{sbt}$, and *buyer shortfall* as $Y^S_{sbt} > Y^B_{sbt}$.

Seller shortfall may be due to the seller underreporting output VAT or the buyer overreporting input VAT (or both). In either case, it implies a potential financial gain for one or both firms, as the reported tax liability is lower than the true liability. Symmetrically, buyer shortfall may be due to the seller overreporting output VAT or the buyer underreporting input VAT (or both), which implies a potential, eventual financial *loss* for one or both firms.[10]

Other things equal, buyer shortfall points towards mistakes in firms' VAT declarations. However, it might be rational for buyers to understate their purchases if they simultaneously understate their sales, e.g., because this allows them to report a less suspicious (say, nonnegative) VAT liability. Carrillo *et al.* (2017) provide evidence of such "looking small"

[9]Out of 22,388 firms, 19,137 have non-missing firm-as-buyer and/or firm-as-seller fixed-effect estimated as described in Section 4 and therefore make up our main sample of analysis.

[10]This is true also in cases where a firm fully reporting its credits vis-a-vis the URA will not reduce its *current* dues, e.g. because of an (already-) nil or negative liability. Reporting negative VAT liabilities and carrying offsets forward is significantly associated with a lower probability of having a positive VAT liability in the future, both across and within firms.

behavior in Ecuador. Buyer shortfall cases could also be due to sellers engaging in also-liability-reducing "looking big" behavior by overstating both their purchases and sales—perhaps due to beliefs that the tax authority pays more attention to small than big firms (see, e.g., Amodio *et al.* , 2021)—while underreporting their value added. In and of themselves, transaction-pair level discrepancies thus do not allow us to distinguish between sophisticated, self-advantageous tax evasion and reporting mistakes.

3.2 Discrepancies

Ugandan firms' average monthly reported VAT liability for the 2013-2016 period is slightly negative, and the median is zero, as is common in developing countries (Lemgruber *et al.* , 2015; Pomeranz, 2015). While only 15% of firms report negative or zero value added in a full fiscal year, the reported VAT *liability* is zero or negative for 52% of firms (see Table G.1). This proportion is quite similar across firms of different sizes. Many can report positive value added but zero or negative VAT liability. This is because offsets are typically carried over, since refunds are restricted.

We observe seller shortfall in 47% and buyer shortfall in 32% of seller-buyer-month observations, with sellers and buyers reporting the same amount in only 21% of the observations.[11] Figure 1 provides a graphical illustration of these discrepancies. In the left panel, the vertical axis measures the (inverse hyperbolic sine of the) total monthly amounts declared by sellers, and the horizontal axis that of buyers. The data are grouped into a grid where the color of each square represents the number of observations, going from 1 (lightest gray) to more than 50,000 (black). Observations above (below) the 45-degree line correspond to cases of buyer (seller) shortfall. The figure's right panel displays the distribution of reporting discrepancies.

We observe these widespread discrepancies despite taking a number of steps to minimize mismatched transactions. First, we use transaction dates rather than filing dates. Second, we use firms' aggregate monthly records rather than individual transactions, and do not label cases where the seller and buyer declare the same amount, only with a one or two-month lag, as discrepancies. Finally, we allow for rounding errors of 1,000 Ugandan Shillings (about US$0.30).[12]

In Figure 1a, squares on the 45-degree line correspond to observations where seller and buyer-reported amounts match. The dashed curve shows the average amount re-

[11] At the quarterly level, we find discrepancies in 84% of cases, with seller shortfall in 50% of cases and buyer shortfall in 34% of cases.

[12] Alternatively, we consider rounding the value of discrepancies at 5% of the transaction value. The share of discrepancies remains very close to the baseline level with similar proportions of seller and buyer shortfalls.

ported by sellers for different values of the buyer-reported amounts. We see that seller shortfall is quantitatively more important than buyer shortfall in aggregate terms. This is apparent also in the right panel, Figure 1b. The total amount of seller shortfall across all discrepancies is US$906 million, while the total amount of buyer shortfall is US$735 million.

Eighty-four percent of discrepancies are on the extensive margin—one trade partner fails to report transacting in a given month—while 16% are on the intensive margin. Variations in these proportions by firm characteristics are shown in Table G.2: overall these shares are relatively stable across sectors and firm size categories. The share of extensive margin discrepancies decreases with transaction size, but the fraction of the transaction amount unreported is higher for larger transactions.

4 Classifying Firms' Reporting Behavior

In this section we show that most Ugandan firms engage in strategic tax reporting behavior, taking into account the country's low-enforcement environment, as economic theory predicts. We also show that, in contrast, a sizeable minority makes costly reporting mistakes. To do this we evaluate whether firms underreport their value added such that their liability falls, or erroneously overreport value added.

4.1 Assigning the blame: fixed-effects analysis

We allocate a share of the responsibility for each discrepancy to the seller and the buyer based on each firm's aggregate reporting accuracy in their respective transactions. The starting point is a fixed-effects model inspired by Abowd *et al.* (1999, 2002). We define the discrepancy between buyer f, and seller f' in month t as $d_{ff't} \equiv Y^B_{ff't} - Y^S_{ff't}$ such that $d_{ff't} > 0$ implies seller shortfall and $d_{ff't} < 0$ implies buyer shortfall. Then, we estimate:

$$d_{ff't} = \delta_c + \delta^b_f + \delta^s_{f'} + \delta_t + r_{ff't}, \tag{1}$$

where δ^b_f and $\delta^s_{f'}$ denote buyer and seller fixed-effects (defined at the firm level), respectively; δ_t is a month fixed effect; δ_c is a constant, and $r_{ff't}$ is an error term. Since $d_{ff't}$ is the nominal value of the discrepancy, δ^s_f can be interpreted as a firm's average discrepancy as a seller, in monetary terms, controlling for all time-invariant characteristics of its buyers, such as their size and reporting reliability. Similarly, $\delta^b_{f'}$ can be interpreted as a firm's average contribution to discrepancies as a buyer, controlling for all time-invariant characteristics of its sellers.[13]

As shown in Abowd *et al.* (1999, 2002), the two-dimensional fixed-effects are separately identified only within a "connected set"—firm-pairs that are linked by transaction and all of such firms' trade partners. The largest connected set observed during our 2013-2016 data period covers over 99% of all observations, 90% of sellers, and 94% of buyers. We thus restrict our analysis to this largest connected set of firms.

4.2 Firm-level reporting behavior

We now formalize our classification of firms' reporting behavior. We construct a firm-level discrepancy measure Q_f, adding up the firm's two estimated fixed-effects:

$$Q_f \equiv w_s \cdot \hat{\delta}_f^s + w_b \cdot \hat{\delta}_f^b, \tag{2}$$

where w_s and w_b represent the number of firm-trade partner monthly observations as a seller or buyer, respectively.[14] A firm engages in *advantageous* misreporting behavior if $Q_f > 0$, meaning that it reports in a way that reduces its aggregate VAT liability. Symmetrically, a firm engages in *disadvantageous* misreporting behavior if $Q_f < 0$, which implies that it reports in a way that increases its overall VAT liability.

We further classify advantageous misreporters into three subcategories. First, a firm engaging in *conspicuous* advantageous misreporting is one for which $\hat{\delta}_f^s \geq 0$ and $\hat{\delta}_f^b \geq 0$. This implies that the firm both underreports its sales and overreports its purchases. Second, a firm engaging in *looking-small* advantageous misreporting is one for which $\hat{\delta}_f^s \geq 0$ and $\hat{\delta}_f^b < 0$. This implies that the firm underreports its sales and underreports its purchases. Finally, a firm engaging in *looking-big* advantageous misreporting is one for which $\hat{\delta}_f^s < 0$ and $\hat{\delta}_f^b \geq 0$, thus overreporting its sales and its purchases.

Panel A of Table 1 shows the resulting classification of firms. We find that 14,358 of the 19,137 Ugandan VAT-eligible firms (75%) are *advantageous* misreporters. This suggests that when the VAT is implemented in a low-state capacity context without systematic cross-checks, the majority of firms misreport to lower their VAT liability.

Of the firms that misreport in an advantageous way, 78% are conspicuous advanta-

[13]In Table B.4 we show results from running (1) with various controls that affect the probability of two firms trading with each other. The results are very similar.

[14]More precisely, $\hat{\delta}_f^s = \hat{\delta}_f^{s'} + \hat{\delta}_c$ and $\hat{\delta}_f^b = \hat{\delta}_f^{b'} + \hat{\delta}_c$ where $\hat{\delta}_f^{s'}$ and $\hat{\delta}_f^{b'}$ are the fixed-effects estimated in (1). By adding the mean discrepancy ($\hat{\delta}_c$) to the deviations from the mean, $\hat{\delta}_f^s$ and $\hat{\delta}_f^b$ give us each firm's reporting discrepancies as a seller (respectively, a buyer) controlling for trade partners' effect and time variations. We replace missing buyer- or seller-FE estimates with zero. In Table B.3, we show that the classification is very similar when we drop firms with missing FEs from our analysis.

geous misreporters, only 10% are looking-small advantageous misreporters, and the remaining 12% are looking-big advantageous misreporters. The high proportion of conspicuous advantageous misreporters suggests that the majority of Ugandan firms believe that the tax authority is unlikely to detect evasion by monitoring firms' reported value added.

We also find that 4,779 firms (25%) misreport in a *disadvantageous* way. A substantial share of firms thus make systematic reporting errors. Such errors can take many different concrete forms, but are asymmetric in nature: *on net*, disadvantageous misreporting behavior raises a firm's tax liability. Our terminology thus labels a firm as "confused" if the systematic component of its (mis)reporting behavior increases the firm's tax liability, and vice versa for "strategic".[15]

Advantageous and disadvantageous misreporting occurs with comparable frequency among smaller, medium-sized, and somewhat larger VAT-registered firms, as shown in Figure F.1. However, the figure also shows that the average Q_f measure markedly increases among the largest firms, suggesting that they are more sophisticated tax (mis)reporters than other firms. A more detailed comparison of the two types of firms is in Appendix Table B.1.

4.3 Interpretation and robustness

We conjecture that the methodology we develop sheds new light on firms' decision-making. A first concern to consider is the potential influence of sampling error on the fixed-effect estimates used to construct Q_f (Lancaster, 2000). Fortunately, our sample is large in the relevant dimensions. Within our 3,373,183 observations, sellers appear 240 times and sell to 37 buyers on average; buyers appear 184 times and buy from 28 sellers on average; and seller-buyer pairs appear 21 times on average. This "connectedness" distinguishes the network we study from those in traditional applications of the Abowd *et al.* (1999, 2002) methodology to employer-employee data (see also Fontaine *et al.* , 2020).

Additionally, each additional firm yields more observations in both of the two fixed-effects dimensions in our setting, since each firm is itself both a seller and buyer. Therefore the estimated fixed-effects are arguably asymptotic both in N and T, instead of only in T,

[15] Our methodology cannot detect misreporting of individual firm-pair×month transaction values, and "nets out" any *symmetric* misreporting across a firm's various transaction partners. The advantageous and disadvantageous misreporting we capture is thus systematic. Given that negative liabilities can be carried over to later months, one example of the latter is not bothering to include all input purchases in the firm's tax declaration when its liability is in any case negative. We find, in fact, that firms classified as disadvantageous misreporters—especially those with a negative buyer fixed-effect—are 20% less likely to file a VAT return with a negative liability, but 18% more likely to file a null return (Table G.3). This is just one example of (systematic) disadvantageous misreporting behavior.

as is usually the case.[16] Cluster-bootstrapping to estimate standard errors on $\hat{\delta}_f^s$ and $\hat{\delta}_f^b$ (see Appendix B), we thus report the classification that results if we restrict attention to the 42% of firms for which we can reject $Q_f = 0$ at conventional significance levels.

For this subsample we find very similar proportions of advantageous (77%) and disadvantageous (23%) firms as in the full sample. We show this in Panel B of Table 1. Also as in the full sample, the majority of advantageous misreporters are "conspicuous" ones (83%), with a smaller share of "looking-small" (8%) and "looking-big" (9%) advantageous misreporters.

We next re-estimate (1) and classify firms via (2) separately for each year in our sample. We find that 74% (resp., 65%) of firms classified as advantageous (disadvantageous) misreporters in year t stay within that classification also in the subsequent year, as shown in Table B.2. Both these results and those in Panel B of Table 1 suggest that the fixed-effects model captures persistent forms of firm behavior (see also the simulation in Appendix D). However, disadvantageous behavior appears to be somewhat less persistent over time than advantageous behavior.

A second concern to consider is whether buyer-seller matching could bias our estimates of δ_f^b and δ_f^s. To investigate, we depict events in which a firm switches trade partners (see also Card *et al.* , 2013). We classify a firm's "old" and "new" trade partner into quartiles using the average discrepancy they each incur in their trade with *other* firms during periods around such a switch. As seen in Figures E.1 and E.2, the firm's reporting discrepancies do not appear to be trending up or down, nor dip or spike, before a switch in trade partner (type). However, its discrepancies change abruptly—and in the direction the change in trading-partner type predicts—when the switch happens. Finally, the discrepancy changes associated with switching trade partners appear symmetric: firms switching from a partner in the top quartile of average discrepancies to a partner in the bottom quartile experience a reduction of similar (absolute) magnitude to those switching in the opposite direction. These observations indicate that sellers and buyers do not sort into trade relationships based on unmodelled match effects in unilateral VAT (mis)reporting.

A third concern to consider is that we do not observe misreporting of sales to final consumers. If firms we classify as disadvantageous misreporters—those that overreport their firm-to-firm sales or underreport their inputs—also underreport a large enough share of sales to final consumers, their total misreporting may in principle be advantageous. To investigate, we re-estimate our model assuming that all firms underreport a given proportion of their sales to final consumers. As seen in Table 2, the proportion of advantageous

[16] This also distinguishes our setting from employer-employee data, where the two Abowd *et al.* (1999, 2002) fixed-effects dimensions are units of different nature.

firms increases to 77% when we assume that all firms underreport final sales by 10%. Even assuming an implausibly high degree of misreporting of sales to final consumers—50%—the share of disadvantageous firms remains high at about 19%.[17]

We conclude that the results in Table 1—a majority of strategic misreporters, but a notable minority of persistently confused firms—likely reflect true variation in firm type and unilateral VAT misreporting in Uganda, underscoring the importance of accounting for heterogeneity in firm sophistication in theory and policy design.

4.4 Revenue consequences

The results in Subsection 3.2 suggest that there may be significant positive revenue consequences for the Ugandan government of disadvantageous VAT misreporting, but also that, in aggregate, VAT misreporting likely decreases government revenues substantially. However, revenue consequences of VAT misreporting are not a simple sum of seller and buyer shortfalls: an increased (or decreased) liability attributed to one firm may have different revenue consequences from one attributed to the firm's trade partner because of rules for refunding negative VAT liabilities (see Appendix A and Almunia *et al.* (2017)).

We divide up each reporting discrepancy $d_{ff't}$ between the two firms using the seller and buyer fixed-effects estimated in Subsection 4.1. If the two fixed-effects have the same sign, we assign shares of the discrepancy in proportion to these. If they have opposite signs, we assign the entire discrepancy to the firm whose fixed effect matches the sign of the discrepancy. Details are in Appendix C.

Our estimates imply that the Ugandan government would have lost US$137 million in tax revenues during 2013-2016 if (only) disadvantageous misreporting were eliminated, as seen in the bottom rows of Table 3. If (only) advantageous misreporting were eliminated, our estimates imply a revenue *gain* of about US$522 million (assuming that liabilities can be collected). If both forms of misreporting were eliminated, our estimates imply a revenue gain of US$384 million, or about 28% of the total VAT collected.[18] These estimates are very similar if we use an alternative way to apportion discrepancies based on the estimated fixed-effects, and also if we naively assume that all instances of seller shortfall

[17] Assuming that the entire VAT compliance gap estimated for Uganda is due to evasion on sales to final consumers—which this paper shows is far from the case—would imply that firms misreport sales to final consumers by 50% (IMF, 2014).

[18] Many Ugandan firms have positive outstanding balances with the URA. This helps explain why the revenue consequences of eliminating disadvantageous misreporting are proportionally smaller (in absolute value) than those of eliminating advantageous misreporting. This, in combination with the correlation between individual firms' buyer and seller shortfalls (see Subsection 4.2), also helps explain why the revenue gain from eliminating all VAT misreporting is smaller than the sum of the gain from eliminating respectively disadvantageous and advantageous misreporting.

are entirely due to sellers and all instances of buyer shortfall due to buyers, as shown in Appendix C.

5 Enhanced Enforcement Capacity and VAT Evasion by Strategic and Confused Firms

We now show evidence that firms misreport less when the state's tax enforcement capacity is greater, but that less sophisticated firms adjust their behavior to a lesser extent. We leverage the fact that imports are subject to greater oversight than domestic transactions.

When Ugandan firms file for Customs clearance of an import transaction, they are required to pay the VAT on the imported goods plus tariffs. To later obtain the corresponding tax credit, they declare the input VAT paid on imports on their VAT "schedules". We thus compare, in firm-month observations, *a given firm's* double reports of the same transaction.[19]

The same amount is reported at Customs and in the firm's VAT declaration in 53% of observations. In 14% of cases, the firm claims a larger amount in VAT credit than what it reported at Customs, thus reducing its VAT liability. This self-advantageous misreporting is less frequent than occurrences of seller shortfall in domestic transactions, in line with the intuition that many firms adjust their behavior to the state's enforcement capacity.

In the remaining 34% of observations, firms report a lower amount in their VAT declaration than at Customs, thus leaving input tax credit unclaimed. This behavior, which we label *seemingly anomalous*, is analogous to buyer shortfall discrepancies in domestic VAT transactions, with the difference that here, the same firm makes both tax declarations.

Seemingly anomalous underclaiming of input tax credit from imported goods may reflect disadvantageous behavior. This appears to be part of the explanation. First, monthly VAT returns reporting a null tax liability are 22 percentage points more likely to display seemingly anomalous import reporting than returns with a positive VAT liability, perhaps because some firms with a null VAT liability do not bother claiming input VAT credits from imports (see Table 4 and footnote 15). Second, seemingly anomalous reporting is less frequent in the early and final months of each fiscal year, when tax matters may be more salient to taxpayers (see Table G.4). However, seemingly anomalous reporting may also represent strategic behavior. There is for example anecdotal evidence that some goods are imported into Uganda by businesses even though they are destined for consumption by

[19] We do so for the 9,318 firms that import and for which we estimate seller and buyer fixed effects in Section 4.

individuals. Because these are not actual business inputs, they do not generate input VAT credits and are legitimately not reported as such.

To investigate, we compare transaction amounts reported at Customs and in domestic VAT declarations separately for firms classified as advantageous and disadvantageous misreporters based on *domestic* VAT transactions in Section 4. In Table 4 the outcome variable is a dummy variable that is equal to one for monthly observations with seemingly anomalous reporting as defined above.[20]

We find that disadvantageous misreporters and firms with a negative buyer fixed-effect are respectively 4.4 and 8.3 percentage points (13% and 24%) more likely to engage in seemingly anomalous reporting of imports than other firms.[21] These estimates, shown in columns 1 and 3 of Table 4, point towards financially irrational behavior by (some) firms and help validate the classification procedure in Section 4.

In contrast, we find no statistically significant difference between advantageous and disadvantageous misreporters' propensity to engage in self-advantageous misreporting of imports (see Table G.5). Both types of firms appear to adjust their behavior to the verifiable nature of imported inputs and engage in less self-advantageous misreporting of imports than of domestic transactions.

Overall, the results in this section indicate that strategic firms misreport less when the state's tax enforcement capacity is greater, while confused firms do so to a lesser extent.

6 Conclusion

In this paper we analyze the extent to which firms make decisions that benefit themselves. The context is tax reporting in a low-enforcement setting: Uganda. We document widespread discrepancies between seller and buyer VAT reports, with dramatic consequences for tax revenue collected. By comparing a given firm's misreporting of sales and purchases over time, we show that, while a majority of firms misreport in a way that reduces their tax liability, a non-negligible fraction—about a quarter—consistently misreports such that their tax liability increases.

In the second part of the paper, we show that firms classified as strategic and confused—advantageous and disadvantageous misreporters—appear to respond differently to the state's tax enforcement capacity. All firms misreport less at customs where goods are subject to greater monitoring, but confused firms are more likely to underreport their input

[20] We allow for rounding errors and pure timing mismatches, as in Section 4. We also control for firm size (deciles of reported annual turnover) and sector in all specifications.

[21] These estimates remain of the same order of magnitude when we control for null VAT reported or include dummies for the type of goods being imported, as seen in columns 2 and 4.

tax credit for imported goods on their VAT returns.[22]

These findings suggest that (i) the proportion of firms that do not engage in sophisticated optimization as usually assumed is high—with important implications for theory and policy—but (ii) the majority of firms nevertheless respond to low state capacity by evading taxes. Together, these two conclusions call into question the self-enforcement properties of the VAT in limited enforcement contexts.

[22]This analysis alone does not imply that the overarching reason for widespread misreporting is low enforcement capacity—there could be additional important contributors. To investigate, future research could leverage local tax enforcement shocks.

References

ABOWD, JOHN M., KRAMARZ, FRANCIS, & MARGOLIS, DAVID N. 1999. High Wage Workers and High Wage Firms. *Econometrica*, **67**(2), 251–333.

ABOWD, JOHN M, CREECY, R, & KRAMARZ, FRANCIS. 2002. *Computing Person and Firm Effects Using Linked Longitudinal Employer-Employee Data*. Census Bureau Technical paper No. TP 2002-06.

ACEMOGLU, DARON, GARCIA-JIMENO, CAMILO, & ROBINSON, JAMES A. 2015. State Capacity and Economic Development: A Network Approach. *American Economic Review*, **105**(8), 2364–2409.

AGHION, PHILIPPE, AKCIGIT, UFUK, , LEQUIEN, MATHIEU, & STANTCHEVA, STEFANIE. 2017. *Tax Simplicity and Heterogeneous Learning*. NBER Working Paper 24049.

ALMUNIA, MIGUEL, & LOPEZ-RODRIGUEZ, DAVID. 2018. Under the Radar: The Effects of Monitoring Firms on Tax Compliance. *American Economic Journal: Economic Policy*, **10**(1), 1–38.

ALMUNIA, MIGUEL, GERARD, FRANCOIS, HJORT, JONAS, KNEBELMANN, JUSTINE, NAKYAMBADDE, DOROTHY, RAISARO, CLAUDE, & TIAN, LIN. 2017. *An Analysis of Discrepancies in Tax Declarations submitted Under Value-Added Tax in Uganda*. International Growth Centre Project Report.

AMODIO, FRANCESCO, CHOI, JIEUN, DE GIORGI, GIACOMO, & RAHMAN, AMINUR. 2021. *Bribes vs. Taxes: Market Structure and Incentives*. working paper, McGill University.

BAUNSGAARD, THOMAS, & KEEN, MICHAEL. 2010. Tax revenue and (or?) trade liberalization. *Journal of Public Economics*, **94**(9-10), 563–577.

BENZARTI, YOUSSEF. 2020. How Taxing Is Tax Filing? Using Revealed Preferences to Estimate Compliance Costs. *American Economic Journal: Economic Policy*, **12**(4), 38–57.

BESLEY, T., & PERSSON, T. 2010. State capacity, conflict and development. *Econometrica*, **78**, 1–34.

BESLEY, TIMOTHY, & PERSSON, TORSTEN. 2009. The origins of state capacity: Property rights, taxation, and politics. *American Economic Review*, **99**, 1218–44.

BEST, MICHAEL, BROCKMEYER, ANNE, KLEVEN, HENRIK, SPINNEWIJN, JOHANNES, & WASEEM, MAZHAR. 2015. Production versus Revenue Efficiency with Limited Tax Capacity: Theory and Evidence from Pakistan. *Journal of Political Economy*, **123**(6), 1311–1355.

BEST, MICHAEL CARLOS, HJORT, JONAS, & SZAKONYI, DAVID. 2019. *Individuals and Organizations as Sources of State Effectiveness*. NBER Working Paper 23350.

BIRD, RICHARD, & GENDRON, PIERRE-PASCAL. 2007. *The VAT in Developing and Transitional Countries*. Cambridge and New York: Cambridge University Press.

CAGÉ, JULIA, & GADENNE, LUCIE. 2018. Tax Revenues, Development, and the Fiscal Cost of Trade Liberalization, 1792-2006. *Explorations in Economic History*, **70**(C), 1–24.

CAMERON, COLIN, GELBACH, JONAH, & MILLER, DOUGLAS. 2008. Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, **90**, 414–427.

CARD, DAVID, HEINING, JORG, & KLINE, PATRICK. 2013. Workplace Heterogeneity and the Rise of West German Wage Inequality. *The Quarterly Journal of Economics*, **128**(3), 967–1015.

CARRILLO, PAUL, POMERANZ, DINA, & SINGHAL, MONICA. 2017. Dodging the taxman: Firm misreporting and limits to tax enforcement. *American Economic Journal: Applied Economics*, **9**(2), 144–164.

CHETTY, RAJ, KROFT, KORY, & LOONEY, ADAM. 2009. Salience and Taxation: Theory and Evidence. *American Economic Review*, **99**(4), 1145–1177.

CHO, SUNGJIN, & RUST, JOHN. 2010. The flat rental puzzle. *Review of Economic Studies*, **77**(2), 560–594.

DELLAVIGNA, STEFANO, & GENTZKOW, MATTHEW. 2019. Uniform Pricing in US Retail Chains. *Quarterly Journal of Economics*, **134**(4), 2011–2084.

DUBE, ARINDRAJIT, MANNING, ALAN, & NAIDU, SURESH. 2020. *Monopsony and Employer Mis-Optimization Explain Why Wages Bunch at Round Numbers*. NBER Working Paper 24991.

DUFLO, ESTHER, GREENSTONE, MICHAEL, PANDE, ROHINI, & RYAN, NICHOLAS. 2018. The Value of Discretion in the Enforcement of Regulation: Experimental Evidence and Structural Estimates from Environmental Inspections in India. *Econometrica*, **86**(6), 2123–2160.

EBRILL, LIAM, BODIN, JEAN-PAUL, KEEN, MICHAEL, & SUMMERS, VICTORIA. 2001. *The Modern VAT*. Washington: International Monetary Fund.

ELECTORAL COMMISION. 2016. *The 2016 General Elections Statistics*. (link) Accessed on 23 April 2019.

EMRAN, M. SHAHE, & STIGLITZ, JOSEPH E. 2005. On Selective Indirect Tax Reform in Developing Countries. *Journal of Public Economics*, **89**(4), 599–623.

FAN, HAICHAO, QIAN, NANCY, WEN, JAYA, & YU, LIU. 2019. *The Dynamic Effects of Computerized VAT Invoices on Chinese Manufacturing Firms*. NBER Working Paper 24414.

FISMAN, RAYMOND, & WEI, SHANG-JIN. 2004. Tax Rates and Tax Evasion: Evidence from "Missing Imports" in China. *Journal of Political Economy*, **112**(2), 471–496.

FONTAINE, FRANÇOIS, MARTIN, JULIEN, & MEJEAN, ISABELLE. 2020. Price discrimination within and across EMU markets: Evidence from French exporters. *Journal of International Economics*, **124**, 103300.

GILLITZER, CHRISTIAN, & SKOV, PEER EBBESEN. 2018. The use of third-party information reporting for tax deductions: evidence and implications from charitable deductions in Denmark. *Oxford Economic Papers*, **70**(3), 892–916.

GOLDFARB, AVI, & XIAO, MO. 2011. Who thinks about the competition? Managerial ability and strategic entry in US local telephone markets. *American Economic Review*, **101**(7), 3130–3161.

HANSMAN, CHRISTOPHER, HJORT, JONAS, & LEÓN, GIANMARCO. 2019. Interlinked Firms and the Consequences of Piecemeal Regulation. *Journal of the European Economic Association*, **17**(3), 876–916.

HJORT, JONAS, LI, XUAN, & SARSONS, HEATHER. 2020. *Across-Country Wage Compression* *in Multinationals*. NBER working paper no. 26788.

HORTACSU, ALI, & PULLER, STEVEN L. 2008. Understanding strategic bidding in multi-unit auctions: a case study of the Texas electricity spot market. *The RAND Journal of Economics,*, **39**(1), 86–114.

IMF. 2013. *Uganda - Staff Report for 2013 Article IV Consultation*. International Monetary Fund Country Report, volume 13/215.

IMF. 2014. *Revenue Administration Gap Analysis Program-The Value-Added Tax Gap Uganda*. International Monetary Fund Fiscal Affairs Department.

KEEN, MICHAEL, & LIGHART, JENNY. 2002. Coordinating Tariff Reduction and Domestic Tax Reform. *Journal of International Economics*, **56**(2), 489–507.

KEEN, MICHAEL, & LIGHART, JENNY. 2005. Coordinating Tariff Reduction and Domestic Tax Reform under Imperfect Competition. *Review of International Economics*, **13**(2), 385–390.

KLEVEN, HENRIK. 2014. How Can Scandinavians Tax So Much? *Journal of Economic Perspectives*, **28**(4), 77–98.

KLEVEN, HENRIK, KNUDSEN, MARTIN, KREINER, CLAUS, PEDERSEN, SOREN, & SAEZ, EMMANUEL. 2011. Unwilling or Unable to Cheat? Evidence From a Tax Audit Experiment in Denmark. *Econometrica*, **79**(3), 651–692.

KLEVEN, HENRIK, KREINER, CLAUS, & SAEZ, EMMANUEL. 2016. Why Can Modern Governments Tax So Much? An Agency Model of Firms as Fiscal Intermediaries. *Economica*, **83**, 219–246.

KOPCZUK, WOJCIECH, & SLEMROD, JOEL. 2006. Putting Firms into Optimal Tax Theory. *American Economic Review, Papers and Proceedings*, **96**, 130–134.

KREMER, MICHAEL, RAO, GAUTAM, & SCHILBACH, FRANK. 2019. Behavioral Development Economics. *In:* BERNHEIM, DOUGLAS, DELLAVIGNA, STEFANO, & LAIBSON, DAVID (eds), *Handbook of Behavioral Economics*, vol. 2. Elsevier.

LAFFONT, JEAN-JAQUES. 2005. *Regulation and Development*. Cambridge University Press.

LANCASTER, TONY. 2000. The Incidental Parameter Problem since 1948. *Journal of Econometrics*, **95**, 391–413.

LEMGRUBER, ANDREA, MASTERS, ANDREW, & CLEARY, DUNCAN. 2015. *Understanding Revenue Administration: An Initial Data Analysis Using the Revenue Administration Fiscal Information Tool*. International Monetary Fund Fiscal Affairs Department Paper Series.

MASCAGNI, GIULIA, MUKAMA, DENIS, & SANTORO, FABRIZIO. 2019. An Analysis of Discrepancies in Taxpayers? VAT Declarations in Rwanda. *International Centre for Taxation and Development Working Paper 92*.

MITTAL, SHEKHAR, & MAHAJAN, APRAJIT. 2017. *VAT in Emerging Economies: Does Third Party Verification Matter*. Mimeo UCLA.

NARITOMI, JOANA. 2019. Consumers as Tax Auditors. *American Economic Review*, **109**(9), 3031–3072.

OECD. 2018. *Revenue Statistics in Africa 2018*. OECD Centre for Tax Policy and Administration, African Union Commission (AUC) and African Tax Administration Forum (ATAF).

PAGE, LUCY, & PANDE, ROHINI. 2018. Ending Global Poverty: Why Money Isn't Enough. *Journal of Economic Perspectives*, **32**, 173–200.

POMERANZ, DINA. 2015. No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax. *American Economic Review*, **105**(8), 2539–69.

RECK, DANIEL. 2016. *Taxes and Mistakes: What?s in a Sufficient Statistic?* Mimeo LSE.

REES-JONES, ALEX, & TAUBINSKY, DMITRY. 2018. Attention Variation and Welfare: Theory and Evidence from a Tax Salience Experiment. *Review of Economic Studies*, **85**(4), 317–330.

RIEZMAN, RAYMOND, & SLEMROD, JOEL. 1987. Tariffs and collection costs. *Review of World Economics*, **123**(3), 545–549.

SLEMROD, JOEL, BLUMENTHAL, MARSHA, & CHRISTIAN, CHARLES. 2001. Taxpayer Response to an Increased Probability of Audit: Evidence from a Controlled Experiment in Minnesota. *Journal of Public Economics*, **79**, 455–483.

SLEMROD, JOEL, COLLINS, BRETT, HOOPES, JEFFREY, RECK, DANIEL, & SEBASTIANI, MICHAEL. 2017. Does Credit-card Information Reporting Improve Small-business Tax Compliance? *Journal of Public Economics*, **149**, 1–19.

TOUREK, GABRIEL. 2021. *Targeting in Tax Behavior: Evidence from Rwandan Firms*. working paper, Massachusetts Institute of Technology.

UGANDA REVENUE AUTHORITY. 2016. *Domestic Tax Laws of Uganda: Updated and Tracked Compendium*. Government of Uganda.

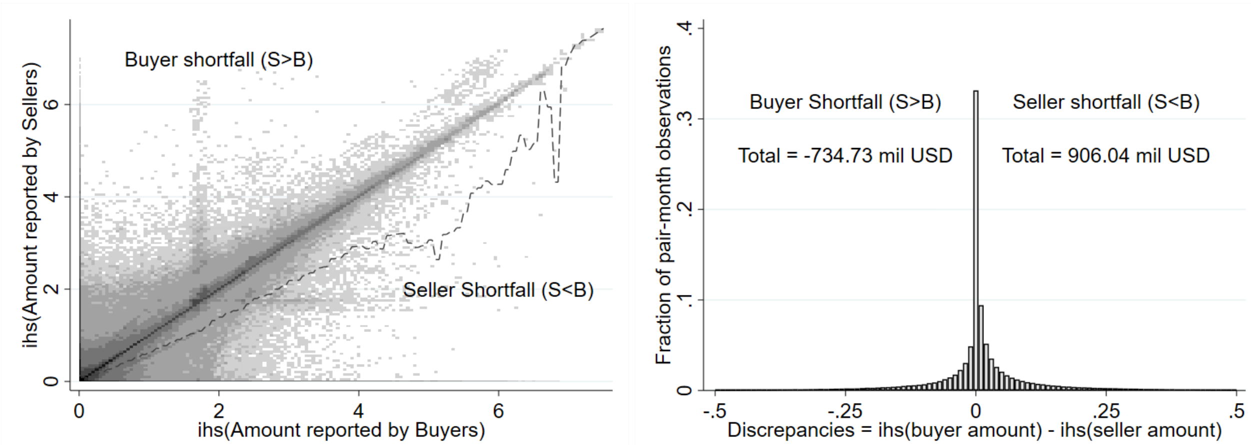
WASEEM, MAZHAR. 2018. Taxes, informality and income shifting: Evidence from a recent Pakistani tax reform. *Journal of Public Economics*, **157**, 41 – 77.

WASEEM, MAZHAR. 2020. The Role of Withholding in the Self-Enforcement of a Value-Added Tax: Evidence from Pakistan. *Review of Economics and Statistics, forthcoming*.

Figures

FIGURE 1
DISCREPANCIES IN THE DOMESTIC VAT DATA

(A) VAT AMOUNTS DECLARED BY SELLERS VS BUYERS **(B) DISTRIBUTION OF REPORTING DISCREPANCIES**



Notes: Data source: VAT Schedules for fiscal years 2013-2016. Panel (A) plots the inverse hyperbolic sine (ihs) transformation of amounts reported by sellers over that by buyers for all monthly transaction data in fiscal years 2013-2016. The data are grouped into a 0.05×0.05 grid and the color represents the number of observations in each square, going from 1 (lightest gray) to more than 50,000 (black). Squares on the 45-degree line correspond to observations where seller and buyer-reported amounts match. Observations above that line correspond to cases of buyer shortfall, while those below indicate cases of seller shortfall. The dashed line represents the conditional mean of ihs(Amount reported by sellers) for the values of ihs(Amount reported by buyers). In Panel (B), we show the distribution of discrepancies in the reporting of transactions by sellers and buyers for fiscal years 2013-2016, calculated by taking the difference between VAT charged in VS1 and VAT paid in VS24. We use the inverse hyperbolic sine transformation of VS1 and VS24. Share ≥ 1: 0.028; Share ≤ -1: 0.031.

Tables

TABLE 1
FIRM TYPE CLASSIFICATION BASED ON Q STATISTIC

	Panel A: All firms	
	No. of firms	Share of Firms
Advantageous	14,358	0.75
Conspicuous	11,248	0.59
Looking small	1,404	0.07
Looking big	1,706	0.09
Disadvantageous	4,779	0.25
Ratio of Adv. to Disadv.		3.00
N	19,137	
	Panel B: Significant Q's	
	No. of firms	Share of Firms
Advantageous	6,150	0.77
Conspicuous	5,111	0.64
Looking small	474	0.06
Looking big	565	0.07
Disadvantageous	1,862	0.23
Ratio of Adv. to Disadv.		3.30
N	8,012	

Notes: Data Source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. Firm types are defined based on Q_f, calculated as the weighted sum of the estimated firm-as-buyer fixed-effect and firm-as-seller fixed-effect, i.e., : $Q_f = w_s \cdot \hat{\delta}^s_f + w_b \cdot \hat{\delta}^b_f$. w_s (respectively, w_b) is the number of firm-trade partner monthly observations as a seller (resp., as a buyer), and $\hat{\delta}^s_f = \hat{\delta}^{s'}_f + \hat{\delta}_c$ and $\hat{\delta}^b_f = \hat{\delta}^{b'}_f + \hat{\delta}_c$ where $\hat{\delta}^{s'}_f$ and $\hat{\delta}^{b'}_f$ are the fixed-effects and $\hat{\delta}_c$ is the constant estimated in equation (1). Firm classifications are defined as: (1) **Advantageous**: $Q_f > 0$. Advantageous firms are further categorized into: (1a) Conspicuous Advantageous: $w_s \cdot \hat{\delta}^s_f \geq 0$ and $w_b \cdot \hat{\delta}^b_f \geq 0$; (1b) Looking small Advantageous: $w_s \cdot \hat{\delta}^s_f \geq 0$ and $w_b \cdot \hat{\delta}^b_f < 0$; and (1c) Looking big Advantageous: $w_s \cdot \hat{\delta}^s_f < 0$ and $w_b \cdot \hat{\delta}^b_f \geq 0$. (2) **Disadvantageous**: $Q_f < 0$. *In Panel B*, the sample is restricted to firms for which the confidence interval around Q_f excludes 0. To compute the variance of Q_f, we use a pairs cluster bootstrap approach, details are in Appendix B.4.

TABLE 2
FIRM TYPES ASSUMING UNDERREPORTING OF SALES TO FINAL CONSUMERS

	Panel A 10% of sales to FC		*Panel B* 30% of sales to FC		*Panel C* 50% of sales to FC	
	No. of Firms	Share of firms	No. of Firms	Share of firms	No. of Firms	Share of firms
Disadvantageous	4,187	0.22	3,649	0.19	3,324	0.17
Advantageous	14,950	0.78	15,488	0.81	15,813	0.83
Conspicuous	12,080	0.63	12,607	0.66	12,934	0.68
Looking small	1,742	0.09	2,086	0.11	2,320	0.12
Looking big	1,128	0.06	795	0.04	559	0.03

Notes: Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. This table presents robustness of firm-type classification, assuming various percentages of sales to final consumers are subject to seller shortfall. Firm types are defined based on Q_f, which is calculated as the weighted sum of the estimated firm-as-buyer fixed-effect and firm-as-seller fixed-effect, i.e., : $Q_f = w_s \cdot (\hat{\delta}_f^s + FC) + w_b \cdot \hat{\delta}_f^b$. w_s (respectively, w_b) is the number of firm-trade partner monthly observations as a seller (resp., as a buyer), and $\hat{\delta}_f^s = \hat{\delta}_f^{s'} + \hat{\delta}_c$ and $\hat{\delta}_f^b = \hat{\delta}_f^{b'} + \hat{\delta}_c$ where $\hat{\delta}_f^{s'}$ and $\hat{\delta}_f^{b'}$ are the fixed-effects and $\hat{\delta}_c$ is the constant estimated in equation (1). FC indicates average monthly unreported sales to final consumers: in Panel A, we consider that sellers do not report 10% of their sales to final consumers, in Panel B, 30%, in Panel C, 50%. Firm classifications are defined as: (1) **Advantageous**: $Q_f > 0$. Advantageous firms are further categorized into: (1a) Conspicuous Advantageous: $w_s \cdot (\hat{\delta}_f^s + FC) \geq 0$ and $w_b \cdot \hat{\delta}_f^b \geq 0$; (1b) Looking small Advantageous: $w_s \cdot (\hat{\delta}_f^s + FC) \geq 0$ and $w_b \cdot \hat{\delta}_f^b < 0$; and (1c) Looking big Advantageous: $w_s \cdot (\hat{\delta}_f^s + FC) < 0$ and $w_b \cdot \hat{\delta}_f^b \geq 0$. (2) **Disadvantageous**: $Q_f < 0$.

TABLE 3
REVENUE CONSEQUENCES BY FIRM TYPE

	All	(1) Disadv.	(2) Adv.	(2a) Conspic.	(2b) Looking Small	(2c) Looking Big
No. of distinct firms	19,137	4,779	14,358	11,248	1,404	1,706
Percentage of all firms	(100%)	(25%)	(75%)	(59%)	(7%)	(9%)
Total net VAT due	1,553,971	672,052	881,919	562,235	107,358	212,326
Seller shortfall						
Number of distinct firms with seller shortfall	17,249	3,999	13,250	10,178	1,391	1,681
Total net VAT due from firms with seller shortfall	1,275,917	575,655	700,262	438,417	89,462	172,382
Total VAT subject to seller shortfall	899,736	101,959	797,776	351,397	396,986	49,393
Buyer shortfall						
Number of distinct firms with buyer shortfall	17,979	4,490	13,489	10,416	1,381	1,692
Total net VAT due from firms with buyer shortfall	1,316,813	614,770	702,043	439,842	89,107	173,094
Total VAT subject to buyer shortfall	727,354	419,675	307,679	147,921	51,920	107,838
Correcting seller shortfall and buyer shortfall						
Impact on total net VAT due	384,154	−138,442	522,597	207,688	326,193	−11,285
Percentage of total VAT collected	28.2%	−10.2%	38.4%	15.2%	24.0%	−0.8%

Notes: Data Source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. Revenue consequences are calculated by correcting the VAT liability in the last month of the year for the total VAT under seller shortfall and under buyer shortfall. Shortfall is assigned using firms' estimated fixed-effects, see Appendix C for details. The first column shows results for the whole sample, while Columns (1) to (2c), firms are divided into sub-types based on their Q_f statistic. All values are in thousands of USD.

TABLE 4
SEEMINGLY ANOMALOUS REPORTING AT CUSTOMS AND FIRM TYPE

	Dep.Var.: seemingly anomalous reporting			
Firm Type	(1)	(2)	(3)	(4)
Disadvantageous	0.049***	0.043***		
	(0.010)	(0.009)		
Null VAT		0.220***		0.211***
		(0.012)		(0.012)
Negative Buyer FE			0.091***	0.078***
			(0.010)	(0.009)
Negative Seller FE			-0.009	-0.001
			(0.009)	(0.008)
Month-Year FE	Yes	Yes	Yes	Yes
Size and Sector FE	Yes	Yes	Yes	Yes
HS Share of Import	No	Yes	No	Yes
Observations	123303	123303	123303	123303
R-squared	0.03	0.07	0.03	0.07
Mean of dep.	0.34	0.34	0.34	0.34

Notes: Data source: VAT Schedule 3, MVR and Customs data for fiscal years 2013-2016. This regression analyzes whether disadvantageous firms, and firms which have a negative seller (buyer) fixed-effect are more likely to behave in a seemingly anomalous way at Customs. Observations are at the firm-month level. The dependent variable is a dummy equal to one if the VAT amounts on imports claimed in VS3 are lower than the VAT paid on imports recorded in the Customs data in the same month. We allow for 1,000 UGX rounding and for pure timing mismatches. In Columns (1) and (2), the explanatory variable of interest is a (time invariant) dummy for firm type, equal to one if the firm is classified as Disadvantageous, based on the value of Q_f, as explained in Section 4.2. In Columns (3) and (4), the explanatory variables of interest are dummies equal to one if the buyer (resp. seller) fixed-effect estimated for the firm as described in Section 4.2 is negative. In all specifications, we control for firm size as measure by annual decile of reported turnover, and for firm sector. In Columns (2) and (4), we additionally control for a dummy indicating null monthly VAT liability reported and for the type of goods imported as measured by dummies for each of the 21 HS Good Code Sections (equal to one if the firm imports at least one good from the corresponding section). Standard errors, clustered at the firm level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Online Appendix for:

Strategic or Confused Firms? Evidence from "Missing" Transactions in Uganda

Miguel Almunia, Jonas Hjort,
Justine Knebelmann and Lin Tian

July 2021

A Background on the VAT in Uganda

The Ugandan VAT—introduced in 1996—follows a relatively standard design. A general rate of 18% applies to all sales, with the usual exemptions for necessities and some services.[23] Firms with an annual turnover above 50 million Ugandan Shillings (US$13,700)—a threshold raised to 150 million Ugandan Shillings (US$41,100) in fiscal year 2015-16—are required to be registered for the VAT, while smaller firms can choose to pay a simplified turnover tax.[24] As in other countries, exports are zero-rated, but the VAT applies to imports. The VAT on imports is directly paid at customs, and can be credited as input in the VAT declarations.[25] VAT firms are required to submit monthly VAT declarations to the Uganda Revenue Authority (URA). Payments of positive tax liabilities are due within 30 days of the declaration. Refunds in the case of negative VAT liabilities are restricted. Negative liabilities of less than 5 million Ugandan Shillings (US$1,370) can only be carried over as offset against future VAT liabilities (indefinitely). If the stock of negative liabilities is above this threshold, firms may request a refund but this triggers a desk audit by the URA. The strict regulation of VAT refunds is common practice in low-income countries (Lemgruber *et al.* , 2015).

While the rules regarding VAT declaration and payment are similar across all VAT firms,[26] the URA categorizes firms into three groups for monitoring and enforcement purposes: large taxpayers are handled by a specific Large Taxpayer Office (LTO); medium-size taxpayers are handled by the Medium Taxpayer Office (MTO); and smaller firms are handled by the local URA offices spread out across the country.[27] For further institutional details and descriptive statistics on the VAT system Uganda, see Almunia *et al.* (2017).

[23]For instance, unprocessed agricultural products and medical, educational and financial services are exempted from VAT. Another set of goods and services are zero-rated. A firm producing zero-rated goods may claim input tax credits, while VAT paid on inputs used in the production of exempted goods cannot be recovered (Uganda Revenue Authority, 2016).

[24]This turnover tax replaces both the VAT and the CIT. Firms below the registration threshold may choose to enter the VAT system on a voluntary basis. After the threshold was increased, the majority of firms between the new and the old threshold remained in the VAT system.

[25]Total VAT revenues are divided almost equally between the contributions from the domestic VAT and the VAT on imports.

[26]With the exception that firms with an annual turnover below 200 million Ugandan Shillings (US$55,026) may apply for their VAT to be calculated using cash basis accounting.

[27]LTOs are firms with an annual turnover above 15 billion Ugandan Shillings (US$4.1 million) and/or belonging to specific sectors such as oil and mining, banking, insurance, and government departments. MTOs are firms with a turnover above 2 billion Ugandan Shillings (US$550,260, threshold increased to 5 billion Ugandan Shillings/US$1.3 million in 2015). STOs are firms with an annual turnover lower than the MTO threshold, but above 50 million Ugandan Shillings (13,700 USD, threshold increased to 150 million Ugandan Shillings/US$41,100 in 2015). Below this threshold, which is the same as the mandatory VAT registration threshold, firms are classified as Micro Taxpayers.

B Fixed-effects analysis

In this section, we present further details for the fixed-effects analysis and results from the robustness checks .

B.1 Comparison of advantageous and disadvantageous firms

After classifying firms into Advantageous and Disadvantgeous type as described in Section 4, we compare the observable characteristics of each firm-type. Results are shown in Table B.1. We regress a dummy variable for being an Advantageous firm, on a set of firm characteristics. To facilitate comparison, all variables are standardized and have unit standard deviation. We display results for the OLS regression (Columns 1 and 2), and for a LASSO regression (Column 3). The LASSO results show that the characteristics which are significantly different across firm types are the following: Advantageous firms are less likely to belong to the Medium or Large Taxpayers Office (MTO or LTO). This seems consistent with the idea that MTO and LTO firms are under higher scrutiny. Advantageous firms on average have a lower amount of initial offset carried forward, and display lower total output amounts. Advantageous firms are also more downstream, this seems consistent with the idea that VAT compliance is stronger higher up in the production chain. Advantageous firms are more likely to be in the manufacturing, construction, wholesale and retail sectors, and less likely to be in the mining, water/electricity, transportation/accomodation, information/communication, financial, real estate and education sectors.

B.2 Panel estimation

Exploiting the panel dimension of the data, we investigate if firms that have self-advantageous reporting behaviors in one year tend to be the same ones that have them in the next year. This allows us to verify whether our classification is consistent over time.

We compute the transition matrix by comparing a firm's classifications for different years. That is, we run Equation (1) separately for each year in the sample:

$$d_{ff't} = \delta_c + \delta^b_{fy} + \delta^s_{f'y} + \delta_t + r_{ff't}, \tag{B.1}$$

where y = fiscal year 2013 to fiscal year 2016.

Since the buyer and seller fixed-effects are only identified within a "connected" set (Abowd *et al.* , 1999), we follow Card *et al.* (2013) and restrict the analysis to the largest connected set of buyer-seller network for each year. We also restrict the sample to firms that appear at least in two consecutive years. Table B.2 shows the results as a transition matrix laying out firms' classification in year $t+1$ conditional on their year t classification. We find that 74% of advantageous firms and 65% of disadvantageous firms stay within their classification in the following year.

TABLE B.1
COMPARISON OF ADVANTAGEOUS AND DISADVANTAGEOUS FIRMS

Dep. Variable: Probability of Being Advantageous			
	OLS		*LASSO*
	Coefficient	P-value	Coefficient
in Kampala	0.02	0.00***	0.00
Distance to URA office	−0.02	0.03**	0.00
MTO/LTO	−0.04	0.00***	−0.04
VAT Payable	−0.67	0.10	0.00
VAT Due	−0.02	0.21	0.00
Offset	−0.36	0.10*	0.00
Initial Offset	−0.01	0.01**	−0.01
Total input	−1.10	0.10*	0.00
Total output	1.43	0.10	−0.01
Ratio of sales to FC	0.01	0.17	0.00
Number of clients	0.02	0.00***	0.01
Number of suppliers	0.00	0.84	0.00
Upstreamness	−0.01	0.08*	−0.01
Distinct outputs (all good codes)	0.01	0.80	0.00
Distinct outputs (relevant good codes)	0.00	0.92	0.00
Distinct inputs (all good codes)	0.04	0.23	0.00
Distinct inputs (relevant good codes)	−0.04	0.25	0.00
Sectors:			
Agriculture, forestry, fishing	−0.01	0.30	0.00
Mining, Quarrying	−0.02	0.00***	−0.02
Manufacturing	0.01	0.17	0.01
Water, Electricity services	−0.01	0.00***	−0.01
Construction	0.01	0.10	0.01
Wholesale and retail	0.00	0.00	0.01
Transportation, accomodation services	−0.03	0.00***	−0.02
Information, communication	−0.01	0.02**	−0.01
Financial services	−0.03	0.00***	−0.03
Real estate	−0.03	0.00***	−0.02
Professional, Admin, Other Services	−0.01	0.38	0.00
Public Administration	−0.01	0.57	0.00
Education	−0.01	0.05**	−0.01
Health and social work	0.00	0.74	0.00
Arts and Entertainment	−0.01	0.21	0.00

Notes: Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. This table shows the results of the regression of a firm-type dummy variable – equal to one if the firm is categorized as Advantageous and zero otherwise – on a set of firm characteristics. *Panel A* displays the results from a multivariate regression including all variables listed. *Panel B* display the results from a LASSO regression. All variables are standardized to have unit standard deviation. *in Kampala* is a dummy equal to one if the firm is in Kampala. *Distance* is calculated by assigning each firm to a sub-county and calculating the distance from the center of the sub-county to the closest URA office. *MTO/LTO* is a dummy variable equal to one if the firm is registered in the Medium or Large Taxpayers' Office (as of June 2017). *Vat Payable, Vat Due, Offset, Total inputs and Total Output* are totals over years 2013-2016. *Initial Offset* is the amount of offset carried forward in the first time period of the firm in the time window of the data. *Ratio of sales to FC*, is the ratio of total sales to final consumers over total sales. *Number of clients and Number of suppliers* are the totals over years 2013-2016. *Upstreamness* indicates the firms' distance to final consumption—larger values indicate that the firm is higher up in the production chain. It is computed by creating an input-output matrix, based on firm-to-firm good code transactions. *Distinct outputs and Distinct inputs* are the number of unique good codes within the firm's sales/purchases over the 2013-2016 period. Good codes are based on the universe of transactions from year 2014 and are obtained by applying a machine learning text algorithm to the text descriptions included in the VAT Schedules. Sector is the firm's sector as listed in the tax registry.

TABLE B.2
FIRM-TYPE TRANSITION MATRIX

	Firm-pairs observed throughout 2013-2016		
	Advantageous (t)	Disadvantageous (t)	Share
Advantageous (t+1)	45.75	13.49	59.23
	(73.93)	(35.38)	
Disadvantageous (t+1)	16.13	24.63	40.77
	(26.07)	(64.62)	
Share	61.88	38.12	100.00
	(100.00)	(100.00)	

Notes: Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. This table presents the transition matrix for yearly firm classifications. The sample is restricted to firms that appear at least in two consecutive years and within each year in the largest connected set.

B.3 Robustness

We perform four robustness checks of our firm classification by varying the sample used for the fixed-effect estimation. First, we re-calculate the estimates of Q_f without replacing the missing fixed-effects by zero, as done in the main analysis. Table B.3 reports the resulting firm-type classification dropping firms for which we do not have both seller and buyer fixed-effects estimated. We show that the classification is very similar to our benchmark fixed-effects model, as reported in Table 1, with a slightly higher share of advantageous misreporters.

Second, we re-run the fixed-effect regression by including controls that affect the propensity of two firms to trade with each other. The objective is that by controlling for these, the likelihood for a seller to trade with a particular buyer is as good as randomly assigned. Specifically, we include two variables, one accounting for geographical proximity, and one accounting for sectoral complementarity. The first one is a dummy variable for whether two firms are located in the same sub-county.[28] The second one is the share of products from the seller's sector that are sold to the buyer's sector. To compute this, we use the official aggregate sector-level Input-Output tables calculated by the Ugandan Bureau of Statistics for financial year 2009. Introducing the controls decreases the sample of firms from 19,137 to 18,629. The results are shown in Panel A of Table B.4. They are similar to what we obtained when running the regression without controls: 77% of firms are classified as Advantageous (against 75% in the main analysis) and 23% are classified as Disadvantageous. Among the Advantageous firms, the respective shares of Conspicuous, Looking-small and Looking-Big are very similar to the ones in the main analysis.

Third, we replicate the analysis on a more consistent sample, as a way to potentially reduce the noise in the estimation, by keeping only firm-pairs with a number of observations larger than ten. The firm classification is displayed in Panel B of Table B.4. The share of Advantageous firms increases to 88%. Among advantageous firms, a larger share are classified as conspicuous—91%, against 78% in the main analysis. The sample is reduced to 12,565 firms.

[28] Uganda is divided up into a total of 1,403 sub-counties (Electoral Commision, 2016).

Fourth, we vary the way the raw transactions data is treated and rounded for the fixed-effects estimation. We first run the estimation on the raw transactions data, without rounding nor adjusting for timing mismatches. Results are shown in Panel A of Table B.5. The shares of Advantageous and Disadvantageous firms are the same as in our main analysis (75 and 25%). Second, we run the estimation after rounding the value of discrepancies at 5% of the transaction value, defined here as the maximum of the values reported by the seller and the buyer. As shown in Panel B of Table B.5, in this case, we find 76% of Advantageous firms and 24% of Disadvantageous firms.

TABLE B.3
FIRM TYPE CLASSIFICATION BASED ON Q STATISTIC (WITHOUT REPLACEMENT)

	Panel A: All firms	
	No. of firms	Share of Firms
Advantageous	10,415	0.79
Conspicuous	7,305	0.55
Looking small	1,404	0.11
Looking big	1,706	0.13
Disadvantageous	2,812	0.21
Ratio of Adv. to Disadv.		3.70
N	13,227	
	Panel B: Significant Q's	
	No. of firms	Share of Firms
Advantageous	4,541	0.82
Conspicuous	3,502	0.63
Looking small	474	0.09
Looking big	565	0.10
Disadvantageous	1,016	0.18
Ratio of Adv. to Disadv.		4.47
N	5,557	

Notes: Data Source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. Firm types are defined based on Q_f, calculated as the weighted sum of the estimated firm-as-buyer fixed-effect and firm-as-seller fixed-effect, i.e., : $Q_f = w_s \cdot \hat{\delta}_f^s + w_b \cdot \hat{\delta}_f^b$. w_s (respectively, w_b) is the number of firm-trade partner monthly observations as a seller (resp., as a buyer), and $\hat{\delta}_f^s = \hat{\delta}_f^{s\prime} + \hat{\delta}_c$ and $\hat{\delta}_f^b = \hat{\delta}_f^{b\prime} + \hat{\delta}_c$ where $\hat{\delta}_f^{s\prime}$ and $\hat{\delta}_f^{b\prime}$ are the fixed-effects and $\hat{\delta}_c$ is the constant estimated in equation (1). In this version, we drop firms for which any of the two fixed-effects is missing. Firm classifications are defined as: (1) **Advantageous**: $Q_f > 0$. Advantageous firms are further categorized into: (1a) Conspicuous Advantageous: $w_s \cdot \hat{\delta}_f^s \geq 0$ and $w_b \cdot \hat{\delta}_f^b \geq 0$; (1b) Looking small Advantageous: $w_s \cdot \hat{\delta}_f^s \geq 0$ and $w_b \cdot \hat{\delta}_f^b < 0$; and (1c) Looking big Advantageous: $w_s \cdot \hat{\delta}_f^s < 0$ and $w_b \cdot \hat{\delta}_f^b \geq 0$. (2) **Disadvantageous**: $Q_f < 0$. *In Panel B*, the sample is restricted to firms for which the confidence interval around Q_f excludes 0. To compute the variance of Q_f, we use a pairs cluster bootstrap. approach, details are in Appendix B.4.

TABLE B.4
FIRM TYPE CLASSIFICATION BASED ON ROBUSTNESS ESTIMATIONS (1/2)

	No. of Firms	Share of firms
	Panel A: Two-way fixed effect estimation with controls	
Advantageous	14,318	0.77
Conspicuous	11,355	0.61
Looking small	1,262	0.07
Looking big	1,701	0.09
Disadvantageous	4,311	0.23
N	18,629	1.00
	Panel B: Sample of firm-pairs with ≥ 10 *observations*	
Advantageous	11,002	0.88
Conspicuous	10,052	0.80
Looking small	361	0.03
Looking big	589	0.05
Disadvantageous	1,563	0.12
N	12,565	1.00

Notes: Data Source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. *In Panel A:* We include variables for geographical proximity and for sectoral complementarity as controls in the fixed-effects model (Section B.3 describes how the control variables are computed). *In Panel B:* We run the fixed-effects model on the subset of firm-pairs that appear ten times or more in the initial dataset.

TABLE B.5
FIRM TYPE CLASSIFICATION BASED ON ROBUSTNESS ESTIMATIONS (2/2)

	No. of Firms	Share of firms
	Panel A: Raw data	
Advantageous	14,358	0.75
Conspicuous	11,248	0.59
Looking small	1,404	0.07
Looking big	1,706	0.09
Disadvantageous	4,779	0.25
N	19,137	1.00
	Panel B: Rounding at 5% of transaction value	
Advantageous	14,453	0.76
Conspicuous	11,354	0.59
Looking small	1,386	0.07
Looking big	1,713	0.09
Disadvantageous	4,684	0.24
N	19,137	1.00

Notes: Data Source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. *In Panel A:* We do not apply any corrections to the transactions data when defining discrepancies, nor rounding nor adjusting for timing mismatches. *In Panel B:* Discrepancies are defined by correcting for timing mismatches in the same way as in our main results, and rounding is done by setting to zero discrepancies that are inferior in absolute value to 5% of transaction size, defined as the maximum between seller declared transaction and buyer declared transaction.

B.4 Variability analysis

To further assess the robustness of the results and the extent of variability in the firm classification that could come from sampling error, we estimate 95% confidence intervals around the point estimates for Q_f. There is no standard methodology in the literature to recover confidence intervals for fixed-effects in AKM-type models.[29] We develop a bootstrap routine to recover the variance of Q_f for each firm f, defined as:

$$VAR(Q_f) = w_s^2 \cdot VAR(\hat{\delta}_f^s) + w_b^2 \cdot VAR(\hat{\delta}_f^b) + 2 \cdot w_s \cdot w_b \cdot COV(\hat{\delta}_f^s, \hat{\delta}_f^b)$$

The simple fixed-effects estimation cannot yield the covariance between $\hat{\delta}_f^s$ and $\hat{\delta}_f^b$. We use a bootstrap procedure in which we resample pairs of firms from our main sample and re-estimate the model 100 times, in the spirit of the pairs cluster bootstrap (Cameron *et al.* , 2008). When a seller-buyer pair is drawn all associated observations are drawn, and this is repeated until the sample includes as many pairs as in the main sample (519,111). The sample is then restricted to the largest connected set. The average number of observations in the bootstrap samples is 10,861,701, against 3,373,183 in the main sample. 99.9% of the pairs from our main connected set appear in at least one iteration, and the average (median) number of iterations a given pair appears in is 41 (30).

Then we estimate the standard errors of $\hat{\delta}_f^b$ and $\hat{\delta}_f^s$ by computing their standard deviation over the 100 iterations. We compute the covariance between $\hat{\delta}_f^b$ and $\hat{\delta}_f^s$ using the 100 estimations.

This allows to compute the confidence interval around Q_f as:

$$CI \equiv \left[Q_f - 1.96 \cdot \sqrt{VAR(Q_f)}\ ;\ Q_f + 1.96 \cdot \sqrt{VAR(Q_f)} \right]$$

Panel B of Table 1 displays the classification of firms when restricting to firms for which the 95% confidence interval around Q_f excludes 0. 8,012 firms (42% of firms from the main sample) are in this case. The respective shares of Advantageous and Disadvantageous are extremely similar to those in the main sample shown in Panel A, with 77% of Advantageous (against 75% in the main sample). This confirms that the share of Disadvantageous firms estimated in our model does not result from noise in the transactions data.

Taken together, the conclusions from subsections B.2, B.3 and B.4 show that our main results—the classification of firms by type of reporting behavior and the existence of a non trivial fraction of Disadvantageous reporters—are only very moderately sensitive to variations in the definition of discrepancies and in the estimation strategy, and importantly, also hold when addressing sampling error in the data.

[29] In the majority of settings (notably employer-employee data) the dimensionality of the data does not allow to rely on the standard errors of the fixed-effects. Furthermore, to our knowledge, there is no AKM-type paper that restricts the analysis to units for which the estimated fixed-effects are statistically significant. For this reason, our main results incorporate all firms from the largest connected set, and the restriction to firms with a significant Q_f should be taken as a complementary result to assess the robustness of the classification.

C Robustness Checks for the Computation of Revenue Consequences

In this section, we describe our approach to computing the revenue consequences of VAT misreporting by relying on information from the firm-type classification generated through the fixed-effects model.

C.1 Alternative Methods to Assign Blame to Seller or Buyer

Reporting discrepancy is given by the difference in the declared amounts of monthly transactions between a buyer and a seller. We consider three alternative ways to divide the "blame" for each reporting discrepancy to the two firms involved. The first two approaches use the estimated fixed effects, whereas the third one adopts a "naive" approach.

The baseline method assigns shares of the discrepancy proportionally based on the sign of each firm's fixed effect. Formally, let $s_{it} \in [0,1]$ be the share of the discrepancy assigned to buyer 1 and seller 2, and recall $\hat{\delta}_1^b$ and $\hat{\delta}_2^s$ denote the estimated fixed effects from buyer and seller respectively. Then:

$$s_{1t} = \begin{cases} \frac{\hat{\delta}_1^b}{\hat{\delta}_1^b + \hat{\delta}_2^s} & \text{if } \hat{\delta}_1^b \cdot \hat{\delta}_2^s > 0 \\ 0.5 & \text{if } \hat{\delta}_1^b = \hat{\delta}_1^s = 0 \\ 1 & \text{if } \hat{\delta}_1^b \cdot \hat{\delta}_2^s < 0 \text{ and } \hat{\delta}_1^b \cdot d_{12t} > 0 \end{cases}$$

For example, suppose $\hat{\delta}_1^b = 30$ and $\hat{\delta}_2^s = 10$. For seller shortfall cases ($d_{12t} > 0$), we assign $s_{1t} = 0.75$ and $s_{2t} = 0.25$. In the case of buyer shortfall ($d_{12t} < 0$), we assign $s_{1t} = 0.25$ and $s_{2t} = 0.75$. If the two relevant fixed effects have the opposite signs, e.g., $\hat{\delta}_1^b = 30$ and $\hat{\delta}_2^s = -10$, we assign $s_{1t} = 1$ and $s_{2t} = 0$ in case of seller shortfall, and $s_{1t} = 0$ and $s_{2t} = 1$ in case of buyer shortfall.

The second approach, while also using the estimated fixed effects, focuses on the relative sizes of the two estimated fixed effects. For a given discrepancy $d_{ff't}$ in a given month t between the two firms involved (say, a buyer $f = 1$ and a seller $f' = 2$), we first calculate the difference in the two estimated fixed effects for the two firms involved, i.e., $\hat{\delta}_1^b - \hat{\delta}_2^s$. If the absolute value of d_{12t} is greater than the absolute value of the difference, we allocate the discrepancy between the firm pair such that the assigned discrepancies reflect the difference in the estimated fixed effects.[30] If the absolute value of d_{12t} is less than the absolute value of the difference, we assign all the discrepancy to the more offending firm in the direction of the discrepancy. This means for a seller shortfall case, the entire discrepancy is assigned to the firm with a higher value of the fixed effects; whereas for a buyer shortfall case, the entire discrepancy is assigned to the firm with a lower value of the fixed effects. More formally, we assign the reporting discrepancies, for a given firm $f = 1$ in month t, according to the following equation:

[30]For example, if d_{12t} is 60, $\hat{\delta}_1^b$ is 30, and $\hat{\delta}_2^s$ is 20, the assigned discrepancies for the buyer $f = 1$ and the seller $f = 2$ are 35 and 25, respectively. Note that the difference in $\hat{\delta}_1^b$ and $\hat{\delta}_2^s$ of 10 is preserved in the assignment. If d_{12t} is 60, $\hat{\delta}_b$ is 30, and $\hat{\delta}_2^s$ is 30, the assigned discrepancies for the buyer $f = 1$ and the seller $f = 2$ are 30 and 20, respectively. Again, the difference in $\hat{\delta}_1^b$ and $\hat{\delta}_2^s$ of 0 is preserved in the assignment.

$$d_{1t} = \begin{cases} \frac{d_{12t}+(\hat{\delta}^b_1-\hat{\delta}^s_2)}{2}, & \text{if } \mid d_{12t} \mid > \mid \hat{\delta}^b_1 - \hat{\delta}^s_2 \mid. \\ d_{12t}\frac{\max(\hat{\delta}^b_1-\hat{\delta}^s_2,0)}{\hat{\delta}^b_1-\hat{\delta}^s_2}, & \text{if } \mid d_{12t} \mid \leq \mid \hat{\delta}^b_1 - \hat{\delta}^s_2 \mid \text{ and } d_{12t} > 0. \\ d_{12t}\frac{\min(\hat{\delta}^b_1-\hat{\delta}^s_2,0)}{\hat{\delta}^b_1-\hat{\delta}^s_2}, & \text{if } \mid d_{12t} \mid \leq \mid \hat{\delta}^b_1 - \hat{\delta}^s_2 \mid \text{ and } d_{12t} < 0. \end{cases} \tag{C.1}$$

Finally, in the third "naive" approach, we simply assign all seller shortfall to the seller and all buyer shortfall to the buyer.

C.2 Revenue Consequences with Alternative Methods

Once we assign the firm-pair level misreporting to each of the firms involved, we calculate the revenue consequences of misreporting for all individual firms, defined as the change in the VAT due that would result from correcting each firms' misreporting. In doing this, we take into account the VAT offsets (outstanding tax credits) carried forward. Specifically, we correct VAT declarations for each month in the study period, making sure to update the offsets carried forward in each subsequent month. Given the restrictions on VAT refunds, the impact on the total net VAT due will not be equal to the difference between the total VAT misreported and underreported. In particular, correcting the VAT liability downward in a given month will have no direct impact on the net VAT due in that month if the original VAT liability was already negative given the eligibility threshold for a VAT refund (and the low share of eligible firms asking and getting a refund). We then aggregate the revenue implications at the yearly level, and our main results further aggregate the revenue consequences over the Fiscal Year 2013-2016 period (the fiscal year in Uganda runs from July to June). More details can be found in Almunia *et al.* (2017).

Columns 1-3 of Table C.1 report the revenue consequence calculations using the three approaches described above. The revenue loss due to misreporting remains in the same order of magnitude across the three approaches.

TABLE C.1
SELLER SHORTFALL AND BUYER SHORTFALL IN THE DOMESTIC VAT ADJUSTING FOR FIRM-SPECIFIC CONTRIBUTION TO DISCREPANCIES

	(1) Main	(2) Alt.	(3) Naive
No. of distinct firms	19,137	19,137	19,137
Percentage of all firms	(100%)	(100%)	(100%)
Total net VAT due	1,553,971	1,553,971	1,553,971
Seller shortfall			
Number of distinct firms with seller shortfall	17,249	17,249	13,448
Total net VAT due from firms with seller shortfall	1,275,917	1,275,917	1,133,456
Total VAT subject to seller shortfall	899,736	899,736	899,736
Buyer shortfall			
Number of distinct firms with buyer shortfall	17,979	17,979	17,181
Total net VAT due from firms with buyer shortfall	1,316,813	1,316,813	1,262,499
Total VAT subject to buyer shortfall	727,354	727,354	727,354
Correcting seller shortfall and buyer shortfall			
Impact on total net VAT due	384,154	436,152	492,844
Percentage of total VAT collected	28.2%	32.0%	36.2%

Notes: Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. In this table we display the revenue consequence analysis using three different methods to assign discrepancies to firms (as described in Section C.1). Revenue consequences are calculated by taking the difference between VAT charged in VS1 and VAT paid in VS24, and correcting the VAT liability in the last month of the year for the total VAT under seller shortfall and under buyer shortfall, as explained in Section C.2. In column (1) (main approach), discrepancies are assigned to firms based on the sign of each firm's estimated fixed-effects, in column (2) (alternative approach) discrepancies are assigned to firms based on the relative size of each firm's estimated fixed-effects, and in column (3) (naive approach), we assign all seller shortfall to the seller, and all buyer shortfall to the buyer. All values are in thousands of USD.

D Simulation

In order to have a better understanding of whether the fixed-effects model from Section 4 correctly captures firm behavior, we conduct a simple simulation exercise. The basic idea is to generate a set of firms that follow advantageous, disadvantageous or neutral reporting behavior, and then check whether the fixed-effects model accurately classifies them.

D.1 Setup

We model the distribution of true transaction amounts (Y) for firm pairs as a chi-squared distribution: $Y \sim \chi^2_3$. We randomly generate 400,000 transactions and then allocate 10,000 firms to be the sellers or buyers in these pairs.[31] Then, we specify three types of firms, whose relative proportion will change across two different scenarios. More concretely, we model advantageous, disadvantageous, and neutral firms.

Specifically, we assume that advantageous firms operating as sellers report the true transaction amount in 50% of their transactions, and they do not report anything in the other 50%. When operating as buyers, advantageous firms report the true amount in 50% of the transactions and overreport their purchases by a factor $b_A \sim U(1,2)$ in the remaining 50%. Formally, the amounts reported by advantageous firms when operating as sellers (Y_A^S) and buyers (Y_A^B) are specified as follows:

$$Y_A^S = \begin{cases} Y & prob = 0.5 \\ 0 & prob = 0.5 \end{cases} \qquad Y_A^B = \begin{cases} Y & prob = 0.5 \\ Y * b_A & prob = 0.5 \end{cases}$$

Disadvantageous firms are modelled as a mirror image of advantageous ones. We assume that disadvantageous firms operating as sellers report the true amount in 50% of transactions, but they overreport the amount sold by a factor of $b_D \sim U(1,2)$ in the remaining 50%. When operating as buyers, disadvantageous firms report the true amount in 50% of transactions, and they do not report anything in the remaining 50%. Formally, the amounts reported by disadvantageous firms when operating as sellers (Y_D^S) and buyers (Y_D^B) are specified as follows:

$$Y_D^S = \begin{cases} Y & prob = 0.5 \\ Y * b_D & prob = 0.5 \end{cases} \qquad Y_D^B = \begin{cases} Y & prob = 0.5 \\ 0 & prob = 0.5 \end{cases}$$

The amount reported by neutral firms is set to be always equal to the true amount, such that:

$$Y_C^S = Y \qquad Y_C^B = Y$$

Finally, in order to obtain a distribution of discrepancies that more closely resembles the true data, we incorporate the possibility of symmetric reporting mistakes. We assume

[31] The ratio of firms to transactions corresponds to the average of (i) the median number of times that firms appear as sellers and (ii) the median number that firms appear as buyers in the real data.

these mistakes occur on the extensive margin, meaning that a certain proportion (p) of transactions is not reported either by the seller or the buyer, regardless of their firm type.

$$Y^S = \begin{cases} Y^S & prob = 1-p \\ 0 & prob = p \end{cases} \qquad\qquad Y^B = \begin{cases} Y^B & prob = 1-p \\ 0 & prob = p \end{cases}$$

where Y^S and Y^B represent the amounts reported by sellers and buyers *regardless of their type*, and p is a given probability that will vary across scenarios.

D.2 Static Simulation

We consider two different cases. In the first case, we assign one third of firms to behave as advantageous misreporters, one third to behave as neutral reporters, and one third to behave as disadvantageous reporters. In the second case, we assign 75% of the firms to behave as advantageous misreporters and the remaining 25% as disadvantageous misreporters, roughly following the proportions found in the analysis presented in Section 4 of the paper.

Table D.1 reports the distribution of outcomes at the firm pair level, which can be either seller shortfall ($Y^S < Y^B$), buyer shortfall ($Y^S > Y^B$), or neutral reporting ($Y^S = Y^B$). Column 1 reports the firm-pair level outcomes in the real data, where 48% of transactions feature seller shortfall, 32% feature buyer shortfall and 21% feature consistent reporting. Columns 2-4 report the distribution of firm-pair level outcomes in the simulated data for three different values of p, the share of mistakes. The number of observations declines as we increase p because when there are more extensive-margin mistakes the chance that neither buyer nor seller reports anything ($Y^S = Y^B = 0$) increases, and those firm pairs are treated as unobserved.

In Panel A, where the simulated data includes equally distributed firm types, seller shortfall is less common (28%) than in the real data while consistent reporting is higher (45%). As we increase the share of mistakes in columns 3 and 4, the proportion of pairs with either seller shortfall or buyer shortfall increases, while neutral reporting declines.

In Panel B, where the simulated data includes 75% advantageous and 25% disadvantageous firms, seller shortfall is more common (57%) than in the real data, while the share with buyer shortfall is lower (17%). Consistent reporting is more common than in the real data (26%). As we increase the share of mistakes in columns 3 and 4, the share of seller shortfall remains stable at 58%, while the share of buyer shortfall converges to 29%, similar to that in the real data.

TABLE D.1
SIMULATED FIRM-PAIR OUTCOMES

	Panel A: 1/3 advantageous, 1/3 neutral, 1/3 disadvantageous			
	Real data	Simulated data		
		Mistakes: $p = 0$	Mistakes: $p = 0.2$	Mistakes: $p = 0.4$
	(1)	(2)	(3)	(4)
Seller shortfall	0.48	0.28	0.34	0.39
Buyer shortfall	0.32	0.27	0.34	0.39
Consistent	0.21	0.45	0.32	0.21
Observations	3,370,462	388,687	355,295	299,620
	Panel B: 75% advantageous & 25% disadvantageous			
	Real data	Simulated data		
		Mistakes: $p = 0$	Mistakes: $p = 0.2$	Mistakes: $p = 0.4$
	(1)	(2)	(3)	(4)
Seller shortfall	0.48	0.58	0.59	0.59
Buyer shortfall	0.32	0.16	0.23	0.29
Consistent	0.21	0.26	0.19	0.13
Observations	3,370,462	382,302	340,501	281,393

Notes: This table displays the firm-pair outcomes using simulated data under two different scenarios. *In Panel A*, we consider a scenario in which firms are equally distributed across the three types (advantageous, neutral, and disadvantageous). *In Panel B*, the distribution of firm types matches the proportions obtained in our benchmark fixed-effects models (Table 1). Column (1) shows the distribution of firm-pair outcomes observed in the real data. Column (2) shows the classification with no symmetric mistakes ($p = 0$). Column (3) and (4), respectively, display the classification with 20 ($p = 0.2$) and 40 ($p = 0.4$) percents of symmetric mistakes in reporting.

Table D.2 reports the distribution of firm types obtained by estimating the fixed-effects model described in Section 4 of the paper to the simulated data, for different shares of mistakes. Column 1 reports the fraction of advantageous-type firms that the model identifies as advantageous (and within that broad type as "conspicuous", "looking small" or "looking big") or disadvantageous, for the case in which there is no mistakes ($p = 0$). In turn, column 2 reports the fraction of disadvantageous-type firms that the model identifies with each of the types. Finally, column 3 reports the fraction of neutral firms that are classified with each of the other types. Note that, as in the main analysis presented in the paper, we do not classifiy any firm as neutral because we never obtain the point estimate $Q = 0$.

In Panel A, we find that advantageous-type and disadvantageous-type firms are correctly classified essentially in all cases (see columns 1-2, 4-5, and 7-8). In contrast, neutral firms are split evenly between advantageous and disadvantageous regardless of the amount of mistakes we introduce (see columns 3, 6, and 9). These results suggest that, if we assume firm-types to be equally distributed among the firm population, the model correctly identifies advantageous and disadvantageous behavior, while neutral firms are split between advantageous and disadvantageous with an equal probability.

In Panel B, where 75% of firms are modelled as advantageous and 25% as disadvantageous, all the advantageous-type firms are also correctly classified as advantageous, regardless of the share of mistakes (columns 1, 4, and 7). In the case of disadvantageous firms, 94% are correctly classified, while the remaining 6% are incorrectly classified as advantageous when there are no mistakes ($p = 0$, column 2). This small error rate is due to the fact that a majority of firms in this scenario are advantageous, so disadvantageous firms are disproportionately likely to interact with firms that engage in seller shortfall more frequently. Then, in a small fraction of cases, the seller shortfall discrepancies dominate the buyer shortfall discrepancies, leading to $Q < 0$. As the share of mistakes increases (columns 5 and 8), the success rate in identifying disadvantageous firms falls to 85% and 77%, respectively.

We conclude from this simulation exercise that the fixed-effects model accurately identifies advantageous firms even in the presence of symmetric mistakes, while it does a good job (though not perfect) at identifying disadvantageous firms, with an accuracy rate that declines as the share of mistakes increases. When advantageous firms are more numerous in the population than disadvantageous ones, the fixed-effects model tends to *underestimate* the proportion of disadvantageous firms in the population. This suggests that, if anything, our application of this estimation method to Ugandan firms is likely to underestimate the true share of disadvantageous firms.

TABLE D.2
SIMULATED FIRM-TYPE CLASSIFICATION

	Panel A: 1/3 advantageous, 1/3 neutral, 1/3 disadvantageous								
	$p = 0$			$p = 0.2$			$p = 0.4$		
	% of Adv.	% of Disadv.	% of Neutral	% of Adv.	% of Disadv.	% of Neutral	% of Adv.	% of Disadv.	% of Neutral
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Advantageous	1.00	0.00	0.50	1.00	0.01	0.51	0.97	0.03	0.50
Conspicuous	1.00	0.00	0.15	0.93	0.00	0.22	0.82	0.01	0.24
Looking small	0.00	0.00	0.02	0.05	0.00	0.05	0.12	0.02	0.08
Looking big	0.00	0.00	0.33	0.02	0.00	0.24	0.04	0.00	0.18
Disadvantageous	0.00	1.00	0.50	0.00	0.99	0.49	0.03	0.97	0.50
	Panel B: 75% advantageous & 25% disadvantageous								
	$p = 0$			$p = 0.2$			$p = 0.4$		
	% of Adv.	% of Disadv.	% of Neutral	% of Adv.	% of Disadv.	% of Neutral	% of Adv.	% of Disadv.	% of Neutral
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Advantageous	1.00	0.06	n.a.	1.00	0.15	n.a.	1.00	0.23	n.a.
Conspicuous	1.00	0.02	n.a.	0.99	0.05	n.a.	0.96	0.09	n.a.
Looking small	0.00	0.01	n.a.	0.00	0.06	n.a.	0.02	0.09	n.a.
Looking big	0.00	0.03	n.a.	0.00	0.03	n.a.	0.01	0.05	n.a.
Disadvantageous	0.00	0.94	n.a.	0.00	0.85	n.a.	0.00	0.77	n.a.

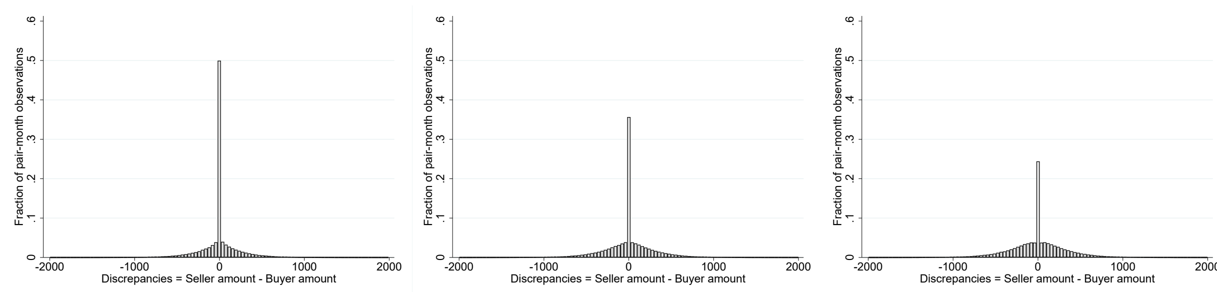
Notes: This table displays the firm-type classification using simulated data under two different scenarios. *In Panel A*, we consider a scenario in which firms are equally distributed across the three types (advantageous, neutral, and disadvantageous). *In Panel B*, the distribution of firm types matches the proportions obtained in our benchmark fixed-effects models (Table 1). Columns (1) to (3) show the classification with no symmetric mistakes ($p = 0$). Columns (4) to (6) and (7) to (9), respectively, display the classification with 20 ($p = 0.2$) and 40 ($p = 0.4$) percents of symmetric mistakes in reporting.

Figure D.1 depicts the distribution of the raw discrepancy in the amounts reported by seller and buyer in the simulated data, for different proportions of mistakes. In Panel A, the distribution when there are no mistakes ($p = 0$) is clearly centered around 0, while it becomes more spread out as we introduce more mistakes ($p = 0.2$ and $p = 0.4$). In Panel B, the distribution is also concentrated around zero but shows a small tilt towards positive values (where a positive value implies seller shortfall) similarly to the real firm-pair level data. As the share of mistakes increases, the distribution is also more spread out, as expected.

FIGURE D.1
SIMULATED DISTRIBUTION OF DISCREPANCIES

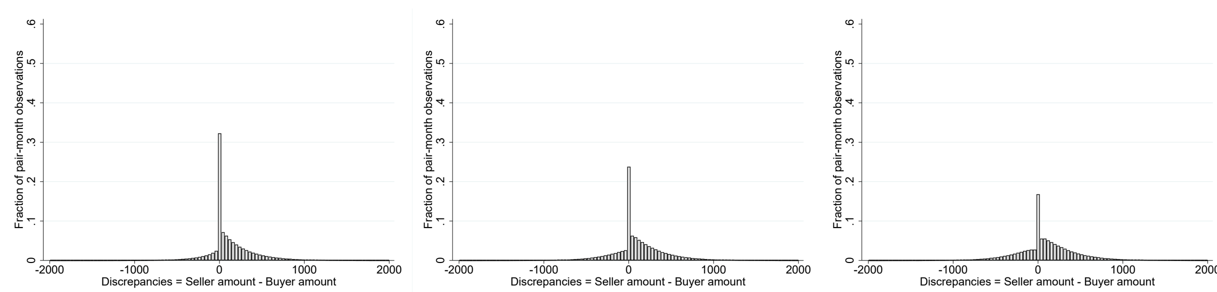
PANEL A: 1/3 ADVANTAGEOUS, 1/3 NEUTRAL, 1/3 DISADVANTAGEOUS

$p = 0$ $\quad$ $p = 0.2$ $\quad$ $p = 0.4$



PANEL B: 75% ADVANTAGEOUS & 25% DISADVANTAGEOUS

$p = 0$ $\quad$ $p = 0.2$ $\quad$ $p = 0.4$



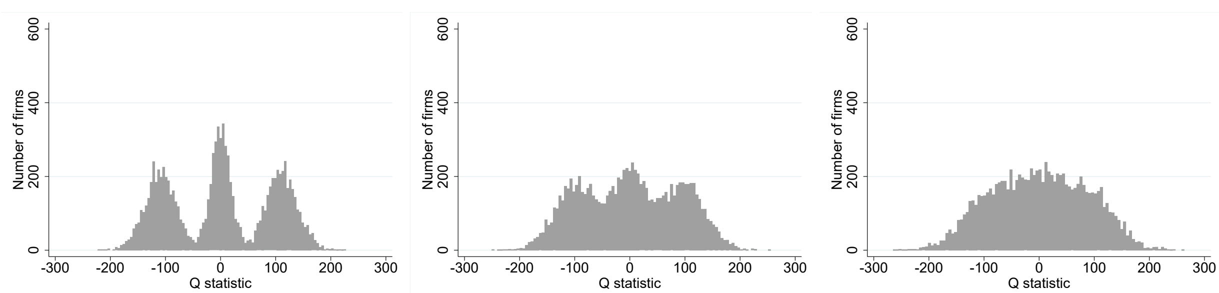
Notes: This figure displays the simulated distribution of discrepancies in the amounts reported by seller and buyer, under two different scenarios. *In Panel A*, we consider a scenario in which firms are equally distributed across the three types (advantageous, neutral, and disadvantageous). *In Panel B*, the distribution of firm types matches the proportions obtained in our benchmark fixed-effects models (Table 1). In both Panels, the figure to the left shows the classification with no symmetric mistakes ($p = 0$). The center and right figures, respectively, display the classification with 20 ($p = 0.2$) and 40 ($p = 0.4$) percents of symmetric mistakes in reporting.

Figure D.2 depicts the distribution of the Q-statistic, which is a weighted sum of the estimated seller and buyer fixed effects at the firm level (see Section 4 for details on the estimation of Q). In Panel A, when $p = 0$ we observe a trimodal distribution, where there is a clear clustering of estimates around three groups: advantageous-type firms have Q estimates distributed around a positive mean, whereas the opposite is true for disadvantageous-type firms. Neutral firms have Q estimates distributed around 0, showing both positive and negative values. This three-group clustering fades as we increase the share of mistakes, such that the mean Q estimate for advantageous firms declines, and the mean Q estimate for disadvantageous firms increases. In Panel B, when there is no mistakes we observe two groups of estimates: disadvantageous-type firms tend to have estimates of Q below zero, with a few estimates just above zero, while the more numerous advantageous-type firms have Q estimates distributed around a positive mean, and none of them fall below zero. As the share of mistakes increases (to $p = 0.2$ and $p = 0.4$), the two distributions become flatter and they have some overlap. Consistent with the results presented in Table D.2, this leads mainly to misclassification of disadvantageous firms as advantageous, but not the other way around.

FIGURE D.2
SIMULATED DISTRIBUTION OF ESTIMATED Q-STATISTICS

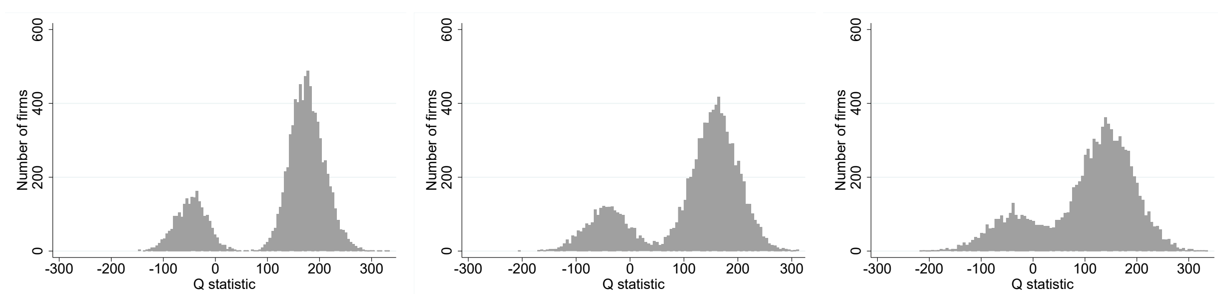
PANEL A: 1/3 ADVANTAGEOUS, 1/3 NEUTRAL 1/3 DISADVANTAGEOUS

$p = 0$ $\quad$ $p = 0.2$ $\quad$ $p = 0.4$



PANEL B: 75% ADVANTAGEOUS & 25% DISADVANTAGEOUS

$p = 0$ $\quad$ $p = 0.2$ $\quad$ $p = 0.4$



Notes: This figure displays the simulated distribution of $Q(f)$, under two different scenarios. *In Panel A*, we consider a scenario in which firms are equally distributed across the three types (advantageous, neutral, and disadvantageous). *In Panel B*, the distribution of firm types matches the proportions obtained in our benchmark fixed-effects models (Table 1). In both Panels, the figure to the left shows the classification with no symmetric mistakes ($p = 0$). The center and right figures, respectively, display the classification with 20 ($p = 0.2$) and 40 ($p = 0.4$) percents of symmetric mistakes in reporting.

D.3 Panel Simulation

In Section 4.3 of the paper, we report that a firm identified as advantageous in year t has a 74% chance of being identified as advantageous in year $t + 1$, while the equivalent proportion is 65% for disadvantageous firms (see Table B.2). One question that arises from those results is whether the proportions are below 100% because firms switch types over time, or because of noise in the data due to (potentially symmetric) reporting mistakes. In order to explore this question, we incorporate a time dimension to our simulation. In particular, we follow the procedure described in Section D.2 twice to generate a two-period dataset of simulated firm pairs. We also allow firms to change their deterministic type across periods, denoting with c the share of firms that change their type between t and $t+1$ due to a real behavior change.

The results of this exercise are reported in Table D.3, which shows the proportion of firms that are classified as advantageous (and, in parentheses, disadvantageous) in period t and also in period $t+1$. The parameter c denotes the share of firms that are assigned ex-ante to switch their type between the two periods. The parameter p indicates the share of extensive-margin mistakes, as described in Section D.2.

Panel A of Table D.3 reports the results for the simulation with one-third of firms of each type: advantageous, neutral and disadvantageous. When there are no mistakes ($p=0$) and we allow no type switches ($c=0$), the estimated firm type (advantageous or disadvantageous) is the same in both periods for about 83-84% of firms. This percentage masks considerable heterogeneity: firms whose true type is advantageous are correctly classified 100% of the time in both periods, as well as firms whose true type is disadvantageous. However, firms whose reporting behavior is neutral (i.e., those that always report the true amount) are evenly split in period t, and then again in period $t+1$. Therefore, we should expect only half of these neutral firms to be classified with the same type across periods. Since 33.3% of firms are neutral, about half of them (16.6%) are classified with the same type in both periods. Adding that to the 66.6% of firms that are consistently classified as advantageous or disadvantageous yields the 83-84% obtained in the aggregate. As we increase the share of firms who switch types (c) or the share of reporting mistakes (p), the percentage of firms that receive the same classification in both periods declines, but less than proportionally. For example, with $c=0.1$ and $p=0$, the percentages go down to 81% and 80% (for advantageous and disadvantageous, respectively). With $c=0$ and $p=0.2$, they decline to 79% for both types. When we assume that $c=0.3$ and $p=0.4$, the percentages decline to 65% and 63%, respectively.

Panel B of Table D.3 reports the results for the version of the simulation with 75% advantageous and 25% disadvantageous firms. In this case, the estimated firm types are highly consistent over time, though not perfectly so: 98% of firms identified as advantageous in period t have the same type in period $t+1$, while the same is true for 96% of firms classified as disadvantageous in period t. This is consistent with the results obtained in the static simulation: since 6% of disadvantageous firms are incorrectly classified as advantageous in period t, we would expect some of them to be correctly classified as disadvantageous in period $t+1$. As we increase the proportion of type switchers (c), these percentages decline mechanically both for firms initially classified as advantageous and disadvantageous. When $c=0.3$ and $p=0$, we find that 70% of firms classified as advantageous (and 67% of those classified as disadvantageous) in period t receive the same label in $t+1$. At the other extreme, when $c=0$ and $p=0.4$, the percentages are 94% and 77%, respectively. Finally, when $c=0.3$ and $p=0.4$, the percentages decline to 70% and 58%, respectively. These results imply that the increase in the proportion of switchers (c) has a mechanical (negative) effect on the proportion of firms classified with the same type in both periods. Meanwhile, an increase in the share of mistakes has a small effect on the consistency of advantageous classification, but a larger effect on the consistency of disadvantageous classification.

The overall conclusion from this panel simulation is that the estimated transitions across types observed in Section 4.3 is consistent with a situation where there is a substantial share of symmetric reporting mistakes, as well as a non-negligible fraction of type switchers across periods. That said, the fixed-effects analysis is able to capture a systematic component of firm behavior that is broadly persistent across periods.

TABLE D.3
SIMULATED FIRM-TYPE CLASSIFICATION: CONSISTENCY ACROSS PERIODS

		Panel A: 1/3 advantageous, 1/3 neutral, 1/3 disadvantageous		
		Mistakes		
		$p = 0$	$p = 0.2$	$p = 0.4$
Switchers	$c = 0$	0.84 (0.83)	0.83 (0.83)	0.80 (0.79)
	$c = 0.1$	0.81 (0.80)	0.80 (0.79)	0.76 (0.75)
	$c = 0.2$	0.77 (0.77)	0.77 (0.75)	0.74 (0.72)
	$c = 0.3$	0.73 (0.74)	0.72 (0.72)	0.70 (0.68)
		Panel B: 75% advantageous & 25% disadvantageous		
		Mistakes		
		$p = 0$	$p = 0.2$	$p = 0.4$
Switchers	$c = 0$	0.98 (0.96)	0.96 (0.87)	0.94 (0.77)
	$c = 0.1$	0.90 (0.84)	0.90 (0.78)	0.89 (0.72)
	$c = 0.2$	0.81 (0.76)	0.81 (0.70)	0.80 (0.64)
	$c = 0.3$	0.70 (0.67)	0.71 (0.62)	0.70 (0.58)

Notes: This table displays the percentage of firms labelled as advantageous (disadvantageous) in period t which are also classified as advantageous (disadvantageous) in period $t+1$. The parameter p represents the proportion of symmetric mistakes within transactions, whereas c stands for the share of type-changing firms.

E Switchers' Graph

We use an event study approach to analyze reporting discrepancies incurred by firms that switch trade partners. We define a switch as chronological pairs of trading spells involving the a given buyer (resp. seller) but two different sellers (resp. buyers). Figures E.1 and E.2 show how reporting discrepancies change around such events. An old trading spell is defined as a sequence of at least two consecutive months in which a buyer-seller pair trades with each other, and subsequently stops trading for at least two months. The second to last period is labeled as time $t-2$, hence the last period before they stop trading is labeled as $t-1$. At time t, the firm stops trading with the set of old trade partners by construction. Starting from t, we similarly identify a set of new trade partners for each buyer (seller), composed of new sellers (buyers) which a given buyer (seller) has not traded with in at least the last two periods, but maintains the trading relationship for at least two consecutive months following the switch.

We classify the two trade partners (old and new) involved in the event into quartiles using the average detrended discrepancies they incur in the VAT reporting with *other* firms during the two months that the spell ends (for the earlier spell) or starts (or the later spell)—-analogous to firm's coworker wages in Card *et al.* (2013). In the figures, the horizontal axis displays event time, i.e., trading months. The vertical axis displays the average detrended discrepancies the firms of interest (sellers in Figure E.1 and buyers in Figure E.2) have in a given month.[32]

The results are shown in Figures E.1 and E.2. We make two initial observations. First, discrepancies change sharply, and in the expected direction when a firm trades with different partners associated with different degrees of reporting discrepancies: For example, sellers switching from a lowest quartile-buyer to a highest quartile-buyer experience a significant increase in reporting discrepancies; whereas sellers switching from a highest quartile-buyer to a lowest quartile-buyer experience a significant decrease in reporting discrepancies. Second, the figures show no systematic pattern that discrepancy is rising for firms that subsequently switch to a higher quartile-partner, and vice versa, thus suggesting that drift in discrepancies in switches are uncorrelated.

More importantly, the discrepancy changes associated with switching trade partners appear symmetric: firms switching from a partner in the highest quartile to another partner in the lowest quartile experience an increase in the average reporting discrepancies of similar magnitude to those switching in the other direction. This provides strong evidence against selection of trade partners based on discrepancies—a concern potentially invalidating the estimates from the fixed-effects model.[33] The striking symmetry in the switchers' graphs indicates that the selection effect is unlikely to bias our estimates.

[32] We detrend raw discrepancies by regressing them on month-year fixed effects.

[33] This selection or sorting would imply that firms switching from partners associated with large discrepancies to partners associated with small discrepancies experience greater decreases than the increase experienced from moving in the opposite direction: firms trading with a small-discrepancy partner enjoy both a small average discrepancy and an improved match effect, and firms trading with a large-discrepancy partner is hit with a large average discrepancy but an offsetting improved match effect.

FIGURE E.1
SWITCHERS' GRAPH: SELLERS

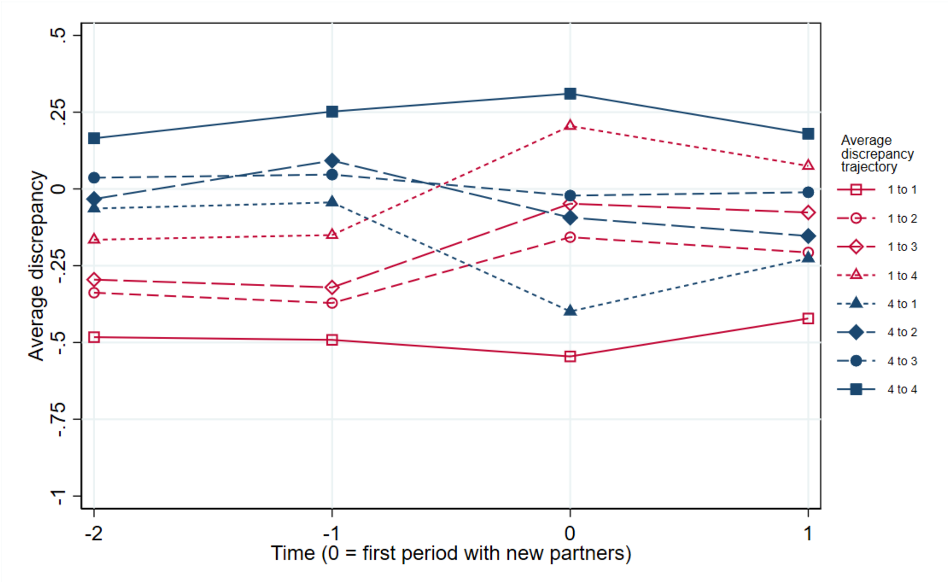
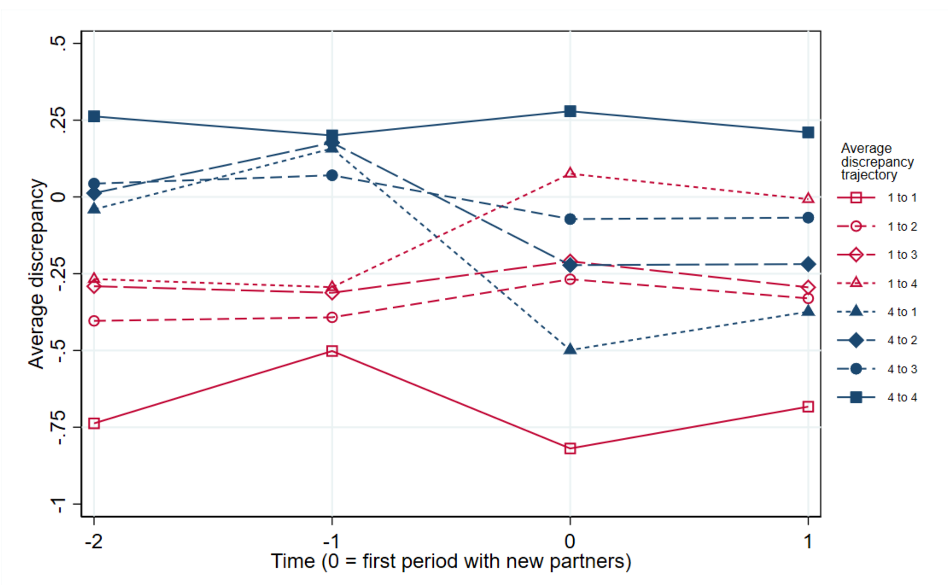


FIGURE E.2
SWITCHERS' GRAPH: BUYERS



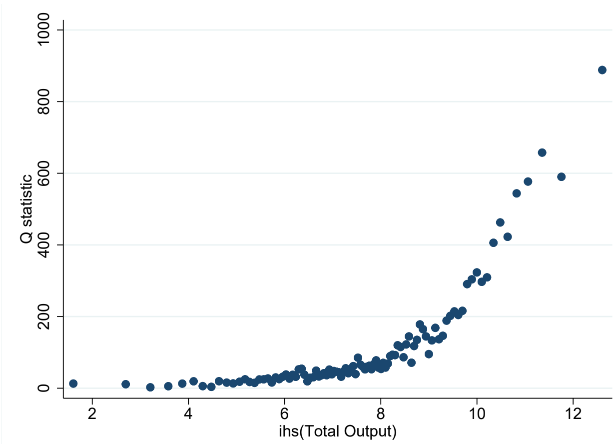
Notes: The figures show time trends in discrepancies around the time that firms switch trade partners, for sellers in the top panel and buyers in the bottom panel. We define an "old trade partner" as a firm that has at least two consecutive months of trade with the firm under consideration and subsequently stops trading for at least two months; whereas a "new trade partner" is one that the firm has not traded with previously. The figures plot the average discrepancy on the vertical axis and event time (i.e., trading months) on the horizontal axis, for different types of quartile-to-quartile switches.

F Additional figures

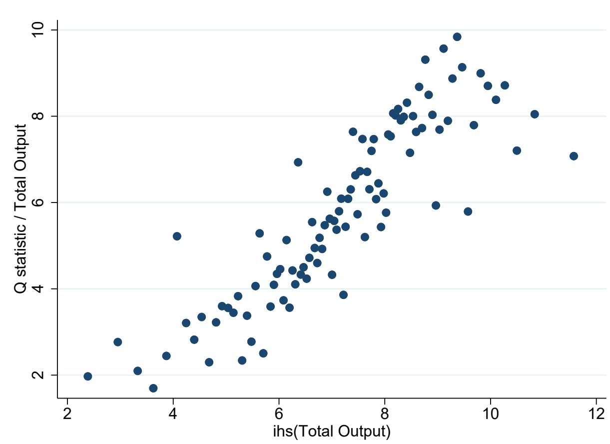
In this section, we present additional figures mentioned in the paper. Figure F.1 shows how firms' estimated Q-statistic correlates with firm size. The average Q_f measure is similarly distributed across most of the distribution of firm size. However, the figure also shows that the average Q_f measure markedly increases among the largest firms, suggesting that the largest firms are more sophisticated tax (mis)reporters than other firms.

FIGURE F.1
Q STATISTIC OVER FIRM SIZE

(A) Q STATISTIC (ABSOLUTE)



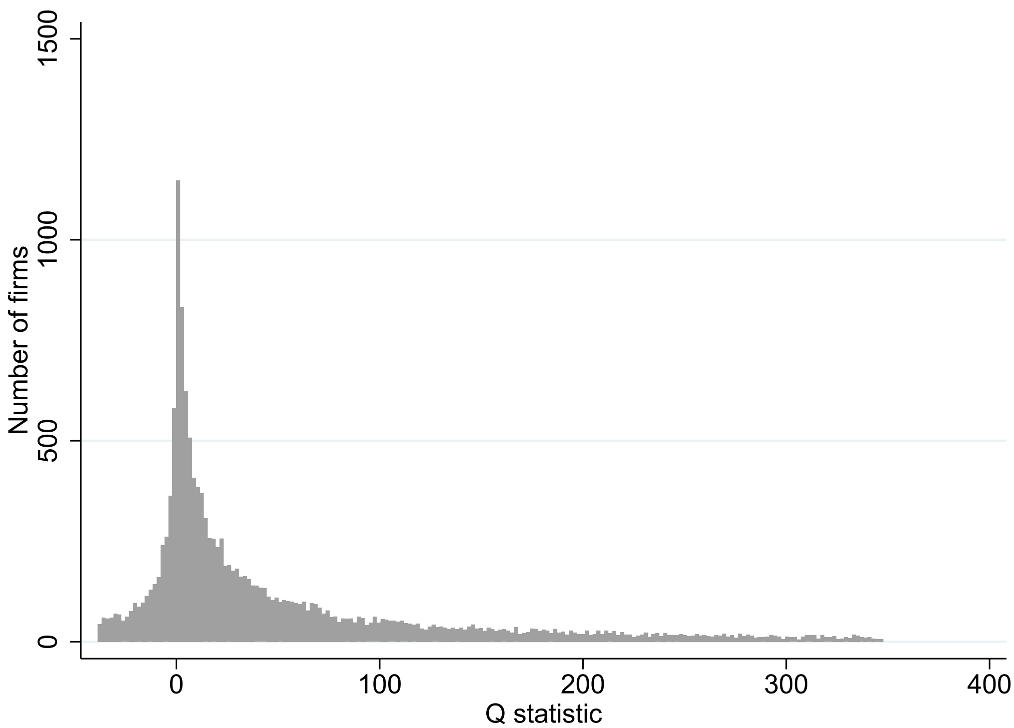
(B) Q STATISTIC (RELATIVE TO FIRM SIZE)



Notes: *In Panel A*, we plot firms' estimated Q statistic — Q_f in equation (2) — over the inverse hyperbolic sine transformation of firms' total output in the study period. *In Panel B*, we first normalize Q estimates by firm sizes, and then plot them against firm size. Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016.

Figure F.2 shows the distribution of Q-statistic across firms. The histogram is concentrated around 0, and clearly skewed to the right. This illustrates the fact that a majority of firms have a positive Q-statistic and are labeled as advantageous reporters.

FIGURE F.2
DISTRIBUTION OF Q STATISTIC.



Notes: In this Figure, we plot the distribution of firms' estimated Q statistic (Q_f in Equation (2)). Data source: VAT Schedules data for fiscal years 2013-2016.

G Additional tables

Table G.1 shows the distribution of value-added and VAT liability by firm size for fiscal years 2013-2016. While only 15% of firms report negative or zero value-added over the four fiscal years (the difference between total output VAT and total input VAT is also low at 22%), the reported VAT liability is zero or negative for 52% of the firms. Value-added proportions are similar for LTO and MTO firms, while the share with a value-added equal or less than zero is higher for other VAT firms. The proportions for the difference between total output VAT and total input VAT as well as for VAT liability are generally similar across firm size.

TABLE G.1
DISTRIBUTION OF VALUE-ADDED AND VAT LIABILITY BY FIRM SIZE

		(1) Value added	(2) Output-Input VAT	(3) VAT liability
	Share > 0	84.33%	77.36%	48.26%
All VAT firms	Share $= 0$	5.12%	7.43%	6.47%
(N = 22,388)	Share < 0	10.55%	15.21%	45.27%
	Share > 0	93.08%	77.75%	48.64%
LTO firms	Share $= 0$	0.81%	0.77%	1.28%
(N = 738)	Share < 0	6.11%	21.49%	50.07%
	Share > 0	91.85%	79.94%	50.69%
MTO firms	Share $= 0$	0.71%	1.39%	1.41%
(N = 1,635)	Share < 0	7.43%	18.66%	47.91%
	Share > 0	82.82%	77.00%	47.92%
Other VAT firms	Share $= 0$	5.95%	8.62%	7.44%
(N = 20,015)	Share < 0	11.22%	14.39%	44.63%

Notes: Data source: VAT Monthly Summary data for fiscal years 2013-2016. Column (1) shows total value added, including goods that are VAT-exempt. Column (2) shows the difference between total output VAT and total input VAT. Column (3) shows total tax liability, taking into account VAT credits carried over from previous periods. Firms can display a positive Output-Input VAT, but a nil or negative VAT liability once offsets are subtracted. LTOs are firms with an annual turnover above 15 billion Ugandan Shillings (USD 4.1 million) and/or belonging to specific sectors such as oil and mining, banking, insurance, and government departments. MTOs are firms with a turnover above 2 billion Ugandan Shillings (USD 550,260, threshold increased to 5 billion Ugandan Shillings/USD 1.3 million in 2015). Other VAT firms refer to VAT-paying firms with an annual turnover lower than the MTO threshold.

Table G.2 shows variation in extensive (i.e., either trade partner fails to report any transaction with the trade partner in a given month) vs. intensive margin (i.e., conditioning on reporting, the reported amounts are different between sellers and buyers) proportions by firm characteristics. These shares are relatively stable, across sectors, and across firm size categories. Regarding transaction sizes, the share of extensive margin discrepancies decreases with transaction size.

Table G.2
Extensive margin versus Intensive margin discrepancies by firm characteristics

	Share of transactions with...		
Firm characteristics	No Discrepancy	Extensive Margin discr.	Intensive Margin discr
MTO/LTO	0.21	0.63	0.16
STO	0.20	0.70	0.11
Transaction size: Large	0.21	0.53	0.26
Transaction size: Medium	0.21	0.68	0.11
Transaction size: Small	0.19	0.78	0.03
Agriculture, forestry, fishing	0.27	0.59	0.13
Mining, Quarrying	0.24	0.64	0.12
Manufacturing	0.25	0.59	0.16
Water, Electricity services	0.09	0.84	0.07
Construction	0.26	0.58	0.16
Wholesale and retail	0.22	0.64	0.14
Transportation, accomodation services	0.15	0.74	0.11
Information, communication	0.11	0.74	0.15
Financial services	0.08	0.84	0.08
Real estate	0.18	0.70	0.12
Professional, Admin, Other Services	0.20	0.68	0.11
Public Administration	0.17	0.73	0.08
Education	0.09	0.85	0.05
Health and social work	0.26	0.67	0.06
Arts and Entertainment	0.15	0.75	0.10
Total	0.21	0.66	0.13

Notes: Data source: VAT Monthly Summary and VAT Schedules data for fiscal years 2013-2016. This table displays the share of pair-month transactions that display no discrepancy (the seller and the buyer declare the same amount, we allow for rounding of 1,000 UGX and for pure timing mismatches.), a discrepancy on the extensive margin (either the seller or the buyer doesn't declare the transaction at all), and a discrepancy on the intensive margin (the seller and the buyer declare different positive amounts), by firm characteristics. Observations are at the firm-month level, and are associated to firms' characteristics irrespective of whether the firm is the buyer or the seller. Firms are categorized either as MTO/LTO (Medium Taxpayer Office, Large Taxpayer Office), or STO (Small Taxpayer Office). Transaction size is defined by tercile of the maximum amount declared by either trade partner. The sector categories correspond to the firm's sector as listed in the tax registry.

Table G.3 shows how VAT liability is related to firm types. In particular, firms with null or positive VAT liability are more likely to be disadvantageous misreporters, whereas firms with negative VAT liabilities correlate with advantageous misreporting.

TABLE G.3
FIRM-TYPE AND VAT MONTHLY LIABILITY

	Dep. Var.: VAT Liability					
	Null	Null	Positive	Positive	Negative	Negative
Firm Type	(1)	(2)	(3)	(4)	(5)	(6)
Disadvantageous	0.034***		0.035***		-0.069***	
	(0.005)		(0.007)		(0.006)	
Negative Buyer FE		0.030***		0.039***		-0.069***
		(0.005)		(0.007)		(0.006)
Negative Seller FE		-0.035***		0.025***		0.010
		(0.005)		(0.007)		(0.007)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size	Yes	Yes	Yes	Yes	Yes	Yes
Observations	712927	712927	712927	712927	712927	712927
R-squared	0.02	0.02	0.00	0.01	0.01	0.02
Mean of dep.	0.19	0.19	0.46	0.46	0.35	0.35

Notes: Data source: VAT Schedules and Monthly Summary data for fiscal years 2013-2016. This table shows the results of the regression of monthly VAT liability on firm-type. In Columns (1) and (2) (resp., Columns (3) and (4), resp. Columns (5) and (6)), the dependent variable is a dummy equal to one if the VAT liability is null (resp., positive, resp. negative). In Columns (1), (3), (5), the regressor of interest is $Disadvantageous$, a dummy equal to one if the firm is categorized as Disadvantageous and zero otherwise. In Columns (2), (4), (6), the regressors of interest are two dummies, *Negative Buyer FE* and *Negative Seller FE*, equal to one if the firm's buyer (resp., seller) fixed-effect is equal to zero. We control for firm size in all specifications, with a categorical variable indicating whether a firm is classified as medium taxpayer (MTO), large taxpayer (LTO), or none (STO). Standard errors, clustered at the firm level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table G.4 analyzes whether the likelihood of seemingly anomalous beneficial behavior at customs varies across months within the year, and with monthly VAT liability as reported in the MVR. The seemingly anomalous reporting is less frequent in the early and final months of each fiscal year, when tax matters may be more salient to taxpayers.

TABLE G.4
SEEMINGLY ANOMALOUS CUSTOMS REPORTING BY VAT LIABILITY AND BY MONTH

Dependent variable:	SA	SA Extensive	SA Intensive	SA	SA Extensive	SA Intensive
Customs behavior	(1)	(2)	(3)	(4)	(5)	(6)
July	-0.013**	-0.020***	0.001			
	(0.006)	(0.005)	(0.007)			
August	-0.015**	-0.016***	-0.003			
	(0.006)	(0.005)	(0.007)			
September	-0.010*	-0.011**	-0.002			
	(0.006)	(0.005)	(0.007)			
October	-0.006	-0.007	-0.003			
	(0.006)	(0.005)	(0.007)			
November	-0.002	-0.003	-0.001			
	(0.006)	(0.005)	(0.007)			
January	-0.011**	-0.007	-0.007			
	(0.006)	(0.005)	(0.007)			
February	-0.040***	-0.017***	-0.032***			
	(0.006)	(0.005)	(0.007)			
March	-0.019***	-0.019***	-0.005			
	(0.006)	(0.005)	(0.007)			
April	-0.024***	-0.021***	-0.010			
	(0.006)	(0.005)	(0.007)			
May	-0.026***	-0.028***	-0.007			
	(0.006)	(0.005)	(0.007)			
June	-0.021***	-0.027***	-0.002			
	(0.006)	(0.005)	(0.007)			
Null VAT				0.220***	0.295***	0.102***
				(0.014)	(0.015)	(0.018)
Year FE	Yes	Yes	Yes			
Month-Year FE				Yes	Yes	Yes
Size and Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	123304	123304	76510	123304	123304	76510
R-squared	0.02	0.06	0.01	0.04	0.09	0.01
Mean of dep.	0.34	0.20	0.23	0.34	0.20	0.23

Notes: Data source: VAT Schedule 3, MVR and Customs data for fiscal years 2013-2016. Observations are at the firm-month level. The dependent variable in Columns (1) and (4) is a dummy equal to one if the firm claims lower VAT amounts incurred on imports in VS3 than VAT paid on imports as recorded in the Customs data for the same month. We allow for rounding of 1,000 UGX and for pure timing mismatches. In Columns (2) and (5), the outcome variable indicates seemingly anomalous reporting on the extensive margin, equal to one if the firm reports nothing in VS3 for a month in which VAT paid on imports at customs is non-zero. In Columns (3) and (6), we restrict the sample to firm-month observations where a positive amount is reported both at Customs and in VS3, and the dependent variable is a dummy indicating seemingly anomalous behavior on the intensive margin, equal to one if the VAT claimed in VS3 is lower than the VAT paid on imports as reported in Customs. In Columns (1) to (3), the explanatory variables are dummies for each month. The reference is December. Note that the fiscal year in Uganda runs from July to June. Months are based on invoice dates. In Columns (4) to (6), the explanatory variable of interest is a dummy equal to one if the VAT liability reported in the MVR is zero. In all specifications, we control for firm size as measure by annual decile of reported turnover, and for firm sector. Standard errors, clustered at the firm level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table G.5 analyzes whether disadvantageous firms are more likely to behave in a self-beneficial way at customs. Specifically, we find that self-advantageous misreporting of imports is unrelated to firm-types.

TABLE G.5
SELF-ADVANTAGEOUS MISREPORTING OF IMPORTS

	Dep.Var.: Self-advantageous misreporting	
Firm Type	(1)	(2)
Disadvantageous	0.001	0.005
	(0.007)	(0.007)
Null VAT		-0.081***
		(0.006)
Month-Year FE	Yes	Yes
Size and Sector FE	Yes	Yes
HS Share of Import	No	Yes
Observations	123303	123303
R-squared	0.05	0.06
Mean of dep.	0.14	0.14

Notes: Data source: VAT Schedule 3, MVR and Customs data for fiscal years 2013-2016. Observations are at the firm-month level. The dependent variable is a dummy equal to one if the firm claims higher VAT amounts incurred on imports in VS3 than VAT paid on imports recorded in the Customs data in the same month. We allow for 1,000 UGX rounding and for pure timing mismatches. The explanatory variable of interest is a (time invariant) dummy for firm type, equal to one if the firm is classified as Disadvantageous, based on the value of Q_f from equation (2). In all specifications, we control for firm size as measure by annual decile of reported turnover, and for firm sector. In Column (2), we additionally control for null monthly VAT liability as reported in MVR, and for the type of goods imported as measured by dummies for each of the 21 HS Good Code Sections, equal to one if the firm imports at least one good from the corresponding section. Standard errors, clustered at the firm level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.