

Tax exposure and digital exchange at the edges of the formal economy: Experimental evidence from Tanzania

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March 3, 2026

Abstract

Mobile phones and digital platforms reduce communication costs for small and medium enterprises (SMEs) in developing countries, but may also increase firms' exposure to tax authorities. We study this tradeoff with two experiments in the Kagera region of Tanzania. First, we measure whether households randomly provided with paper or digital Yellow Pages directories are more likely to receive tax invoices when making purchases, which would be consistent with a reallocation of spending toward formal firms. Second, we use incentive-compatible auctions to measure SME willingness to pay for a directory listing, randomly assigning some firms to a treatment that primes them to consider that the Tanzania Revenue Authority could access the directory. In neither experiment do we find statistically significant treatment effects. Directory access does not increase the likelihood of receiving tax invoices, and tax priming does not reduce firm willingness to pay for a listing. These null results suggest that concerns about tax exposure do not substantially deter SME participation in digital information services.

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1. Introduction

Mobile phones and digital platforms substantially reduce communication costs in developing countries, especially in rural areas characterized by high transport costs. Modern ICT services can be transformative for small and medium enterprises (SMEs), which can struggle to reach new markets, find better suppliers, and advertise directly to customers. But there is a potential downside to digital exposure. Any platform that makes it easier for customers to find a business also makes it easier for tax authorities to find that business. As digital services expand in countries with large informal sectors, firm owners must reassess the decision about whether and how to formalize. Little is known about how SMEs navigate the trade-offs between digital exchange, formality, and tax avoidance. Addressing this knowledge gap is essential for understanding how the composition of firms in the economy may change as digital exchange becomes more common. It is also critical for designing appropriate regulatory policies for digital platforms, and for estimating the value to tax authorities of enforcing minimum standards of formality for enterprises that wish to operate in the digital economy.

This study directly addresses that knowledge gap with two experiments in the Kagera region of Tanzania. The first experiment builds on a large-scale randomized control trial (RCT) focused on estimating the effects of a digital directory on households. We will test whether households that are randomly provided with descriptive and contact information for thousands of nearby SMEs, in the form of a Yellow Pages directory, are more likely to receive tax invoices for their purchases. If so, this pattern would be consistent with treated households reallocating their business toward formal firms and away from informal firms (perhaps unknowingly). Such a finding could arise because formal firms are more responsive to phone calls from unknown new customers. It could also arise if formal firms are more willing to let their business be listed in the directory, a mechanism that we explore in the second part of this study.

The second activity is an experiment to measure SME willingness-to-pay (WTP) to be listed in a digital directory using incentive-compatible auctions. Our interest is in

whether concerns about exposure to the Tanzania Revenue Authority (TRA) reduce WTP. We accomplish this by randomly assigning some firms to a “TRA priming” treatment, in which we highlight that the directories are public-facing and that we cannot directly limit access to the directory information by customers, competitors, the TRA, or other government bodies. The hypothesis to be tested is that priming the TRA reduces average firm WTP for a listing. This activity is motivated by our prior work. In our first effort to collect SME contact information for a directory in rural Tanzania, in 2014, approximately 70% of SMEs were willing to be listed. The most common reason for not listing given by the other 30% was “fear of exposure to the tax authorities.” Precisely measuring the distribution of WTP among the treatment and control groups will shed light on how firms trade off the value of additional visibility to customers against the potential for exposure to tax authorities through a directory listing.

In neither experiment do we find statistically significant differences between treatment and control groups. In the first study, households that receive access to the directory (in either paper or digital format) are not more likely to receive tax invoices when making purchases through various channels. In the second study, the tax priming treatment does not reduce firm manager WTP to be listed in the directory. These null results are both bad news and good news for tax policy. The first null result suggests that the tax revenue effects of this type of search and communication intervention are likely to be minimal, at least in the short term and in contexts like ours. The second null result, however, suggests that firms in Tanzania are not deterred from public exposure by mentions of the TRA. That bodes well for both broadening participation in communication platforms, and for what we understand about the public image of the TRA and firms’ attitudes toward it.

These findings connect to existing work on formalization, public finance, and firm behavior in developing countries. Prior evidence has found little impact of information treatments on the decision of firms to formalize, modest impacts on formalization from increasing the perceived cost of informality, and little net benefit to most informal SMEs

from formalizing (Andrade et al., 2016; Bruhn and McKenzie, 2014). An important frontier for that line of inquiry is to understand how digital platforms change both the costs and benefits of formalization.

Recent studies have highlighted the importance of repeated trade and firm-to-firm relational contracts when contract enforcement is weak (Machiavello and Morjaria, 2015, 2021; Startz, 2021; Rudder and Dillon, 2022; Machiavello, 2022). It is not known whether platforms that reduce search costs for firms change the relative importance of relationships. The opportunity to increase firm visibility through a digital directory may simultaneously reduce the need for and value of firm-to-firm relationships, and reduce the relative cost of exposure to tax authorities. These mutually reinforcing factors may lead to higher rates of formalization and increases in tax compliance by firms near the formality margin, while reducing the flow of customers to informal firms that choose not to participate in the digital economy.

Other recent papers have used experimental or quasi-experimental changes in the cost of tax avoidance to provide causal evidence related to compliance (Pomeranz, 2015; Naritomi, 2019; Waseem, 2018). In this vein, our project relates to the active body of work on SME formalization in low-income countries, and on the way that tax policy can influence the extent of trade between firms with different registration statuses (Gadenne et al., 2022).

2. Context and Design

Our study has two experimental components. The first builds on a large-scale RCT with paper and digital telephone directories as treatments, with tax-related outcomes measured during the endline survey. The second is based on a set of incentive-compatible auctions that we conducted with SMEs. In those auctions, SME managers bid to be included in the digital telephone directory.

2.1 Study 1: Directories and Tax Invoices

The overarching RCT addresses information frictions and their effect on small businesses and rural households. The key idea is that mobile phones have proliferated without services like the Yellow Pages that allow users to find new phone numbers at low cost. Instead, most mobile phone users tend to learn new numbers by interacting directly with someone, or through their face-to-face network. The lack of directory services reduces the productive benefits of phones, and biases those benefits toward individuals with stronger pre-existing social networks or the capacity to easily travel. In previous work our team established that a paper telephone directory is highly impactful for both households and firms in rural Tanzania (Dillon et al., 2025); that a digital directory using USSD technology can be feasibly deployed in this context (Weld et al., 2018); and that a directory listing affects firms’ relationships with suppliers and customers both by lowering search costs and by serving as a signal of quality or an endorsement for the firm (Rudder and Dillon, 2025).

In our team’s first effort to gather firm contact information for a directory in rural Tanzania, in 2014, approximately 70% of the 2100 small- and medium-sized firms that we approached were willing to be listed. The most common reason for not listing their business given by the other 30% was “fear of exposure to the tax authorities.” A key medium-term goal of our study has been to understand how access to information services that facilitate marketing, such as telephone directories, might be usefully linked to tax registration status as a way to nudge marginal firms to formalize.

To understand how directories affect household communication and search activity, farm investment, yields, prices received for crops, and non-farm enterprises, we assembled and delivered both paper and digital Yellow Pages to households in 120 villages in the Kagera region. The full study involves $\tilde{3}$ 300 households, randomly assigned to one of three groups: control, paper treatment, digital treatment. The baseline survey and intervention delivery began in took place in November-December 2022. Households randomly assigned to the paper treatment group receive a 150-page bound booklet, organized alphabetically by district,

village, sector, subsector, and firm name, with contact information and brief descriptions for firms in a wide range of sectors relevant to farming and the rural economy (retail, wholesale, transport, agricultural processing, various services, skilled trades). Treatment was delivered by enumerators from our survey partner, EDI Global, after they complete a baseline survey.

Households randomly assigned to the “digital treatment” group receive access to a digital directory containing the same information, accessible through both USSD short code and smartphone app (therefore accessible on any mobile phone). The USSD service was built and maintained by a team from the University of Washington, working in conjunction with the Tanzania-based software and IT firm Niafikra Limited. The information to include in the directories was collected in summer 2022 through a census that we conducted in 102 villages in the six study districts of the Kagera region: Bukoba Rural, Karagwe, Kyerwa, Missenyi, and Muleba. The census collected information on 10,007 small and medium enterprises for listing in the directories.

With IGC support, we expanded the scope of the endline survey to cover tax-related issues. We were specifically interested in whether customers would be more likely to receive tax invoices when they purchased goods from previously unknown businesses that they locate through the experimentally provided information services. Such transactions must be based on something other than a longstanding buyer-seller relationship. If tax avoidance is easier in the context of a longstanding relationship—i.e., if firms are less likely to remit VAT or other taxes when selling to an established client—then we would find that treated households are more likely to receive tax invoices for their purchases. More broadly, our study will test whether a benevolent side effect (in public finance terms) of reducing search costs for rural households is that they reallocate business toward formal, tax compliant firms, and away from informal or tax avoidant firms.

2.2 Study 2: Firm WTP for a Directory Listing

Our second activity was an experiment to measure firms’ willingness-to-pay (WTP) to be listed in a digital directory using incentive-compatible auctions. Qualitative interviews during prior data collection efforts revealed that some firms hesitated to list their contact information in a phonebook due to concerns about exposure to the Tanzania Revenue Authority (TRA). Firms are often too small to be formally registered, but were concerned that a visible listing would increase pressure from tax authorities to pay taxes (either official or unofficial forms of taxation).

For this activity, we assess whether priming firms to think about tax collection authorities reduces firm WTP to be included in the directory. We randomly assigned some firms to a “TRA priming” treatment, in which we highlight that the directories are public-facing and that we cannot directly limit access to the directory information by customers, competitors, the TRA, or other government bodies.¹ The control group heard a ‘standard priming’ that offered them the opportunity to provide their contact information to a mobile-based directory that was available to customers throughout the region. The hypothesis is that priming the TRA reduces the average firm WTP for a listing. The magnitude of this deterrent effect is important for understanding firm responses to interventions by tax authorities in digital commerce.

To generate a sample for this exercise, we randomly selected 600 firms that had participated in the firm census in the larger directory RCT. We targeted villages with between 3,000-10,000 population and offered all firms the opportunity to list their information. Table 1 in the appendix reports experimental balance and descriptive statistics.

At the end of a baseline survey, firms were automatically randomized to groups with tax or standard priming. The enumerator read the priming directly to the respondent. The enumerator also trained the respondent to understand the WTP game and played several

¹The WTP experimental outcomes were separately pre-registered on the AEA RCT Registry (RCT ID: AEARCTR-0012332).

practice rounds to illustrate how a random draw affects outcomes. Second, the respondent was then asked a series of questions that elicited their willingness to pay to have their contact information listed in a phonebook directory. The prices range from 0.10*USD* upto 2.00 USD. After a respondent indicated their highest willingness to pay, the questions ended. (E.g. if a respondent indicates that they are willing to pay 0.30*but not* 0.50, then the price questions automatically stop) Finally, the enumerator drew a price from a bag. If the respondent's highest willingness-to-pay was below this price, they paid it and their phone number was listed, otherwise no other action was taken.

The primary outcome for this activity is the maximum WTP to be listed in the directory. We pre-registered additional analyses based on three sources of heterogeneity: competition, mobile money use, and firm assets.

Firms with more competitors may have different WTP. Their WTP may be higher if they perceive advertising as beneficial to help distinguish them from competitors. But, the effect may also go in the other direction - firms in competitive sectors often work in settings where many competitors are available on the same street and they face relatively inelastic demand because they sell or provide commodity services. If that is the case, firms with more competitors may have lower WTP. Put the other way, firms with fewer competitors may offer more specialized services that would benefit from advertising to a wider audience.

We hypothesize that the use of mobile money services and knowledge of mobile money fees and taxes could significantly affect firms' WTP for directory listing depending on tax priming. Firms using the mobile money services in tax primed group might modify their WTP for directory listing depending on their view of how taxes would impact the cost and benefit of directory listing. If firms often use mobile money for transactions, they may be used to mobile transactions with customers and suppliers that would increase their WTP to list their firm.

Finally, we look at heterogeneity by firm infrastructure/assets and tenure to understand if more established and larger firms have higher WTP with and without tax priming.

We hypothesize that more established businesses may be more likely to be registered with the local government - especially if they operate a firm that has a defined storefront, have been operating for more years, or have more business assets.

3. Results and Discussion

We present the key findings of the two studies in sequence.

3.1 Results: Directory Access and Tax Invoices

Tables 1 and 2 show estimated average treatment effects from receiving access to the telephone directory on five outcomes: receiving a formal invoice when purchasing from a supplier for a non-farm enterprise controlled by the household (column 1); receiving a formal invoice when traveling to make a purchase of a farm input or piece of capital equipment (column 2); receiving a formal invoice when ordering inputs over the phone (column 3); receiving a formal invoice when asking someone else to make a purchase on the respondent's behalf (column 4); and receiving any formal invoice in any of the previous four scenarios (column 5). Table 1 reports separate effects for the paper and digital directories, while Table 2 reports the effect of receiving other type of directory, relative to control.

Across all specifications in Tables 1 and 2, we find no statistically significant effects. Confidence intervals include zero, but in many cases are wide enough to include effect sizes that would be of interest. For two of the outcomes, based on purchasing inputs over the phone (column 3) and via proxies (column 4), null results may be due to low power, as each is based on fewer than 80 purchases.

3.2 Results: Firm WTP and Fear of Tax Exposure

Table 4 reports the main treatment effects on willingness to pay for a directory listing. Contrary to our hypothesis, the tax priming treatment does not reduce firm WTP. The

point estimate is small and negative (-0.048 in the baseline specification, -0.038 with LASSO-selected controls) and is not statistically significant under either wild-cluster bootstrap or conventional standard errors. The control group mean WTP is 4.526 (in inverse hyperbolic sine-transformed Tanzanian shillings). These results suggest that, on average, highlighting the possibility of TRA access to directory information does not meaningfully deter firms from seeking a listing.

In Tables 5-7 we explore heterogeneity in the treatment effect along pre-registered dimensions. Table 5 examines interactions with local competition and whether competitors match the firm on price or quality. While the main tax priming effect becomes marginally significant and negative in some specifications, this is offset by a positive and significant interaction with competitor price-matching, suggesting that firms whose competitors charge similar prices are less deterred by the tax prime. Interactions with competition intensity and competitor quality are not significant. Table 6 tests whether firm size, measured by inventory value and business supplies, moderates the treatment effect; none of the interaction terms are statistically significant. Table 7 considers whether mobile money use and digital transaction channels shape the response to tax priming. Across specifications interacting the treatment with mobile money use, payments to suppliers via mobile money, receipt of customer payments via mobile money, and use of a mobile money agent, we find no significant heterogeneity. Taken together, the heterogeneity analyses reveal little systematic variation in the WTP response to tax priming, with the possible exception of competitive price dynamics.

4. Conclusion

In this paper we examined whether digital information services that increase firm visibility also increase exposure to tax authorities, and whether that exposure deters firm participation. Using two experiments in rural Tanzania, we find little evidence for either channel. Household access to telephone directories does not increase the probability of receiving tax invoices,

suggesting that directory-driven search does not meaningfully shift purchasing toward formal, tax-compliant firms in the short run. Firm WTP for a directory listing is not reduced by priming managers to consider TRA access to the directory, and this null result holds across a range of heterogeneity dimensions including competition, inventory, and mobile money use. Together, these findings suggest that the tax exposure costs of digital visibility are not a binding constraint on SME participation in information platforms, at least in the Kagera region of Tanzania. This is encouraging for policymakers seeking to expand digital exchange without discouraging firm engagement, although it is unclear whether these patterns will hold over time, as digital platforms become more widespread, or in settings with different levels of tax enforcement capacity.

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Tables

Table 1: Directory effects on receipt of tax invoices: Separate treatments

	(1) NFE supplier	(2) When traveling	(3) Phone order	(4) Proxy buyer	(5) Any invoice
Paper directory	0.0330 (0.0362)	0.0164 (0.0194)	-0.155 (0.225)	-0.166 (0.167)	0.00579 (0.0220)
Digital directory	0.0212 (0.0337)	0.00846 (0.0169)	0.0586 (0.258)	-0.173 (0.164)	0.00874 (0.0188)
Observations	339	638	63	77	898
R-squared	0.296	0.194	0.508	0.493	0.180
Control mean	0.0403	0.0255	0.120	0.367	0.0608

Notes: Authors' calculations from endline survey data. All outcome variables are binary variables capturing receipt of a formal tax invoice when making a purchase. "NFE supplier" refers to inputs purchases from suppliers for non-farm enterprises; "When traveling" refers to input purchases made by traveling to nearby towns or markets; "Phone order" refers to purchases made over the phone; "Proxy buyer" refers to purchases made via an intermediary, for example by asking a neighbor to make a purchase on one's behalf when they travel to a market; "Any invoice" is the maximum of the outcomes in columns 1-4, defined only for households that appear in at least one of the regressions in columns 1-4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Directory effects on receipt of tax invoices: Any treatment

	(1)	(2)	(3)	(4)	(5)
	NFE supplier	When traveling	Phone order	Proxy buyer	Any invoice
Treated	0.0261 (0.0303)	0.0119 (0.0150)	-0.0433 (0.223)	-0.170 (0.130)	0.00746 (0.0168)
Observations	339	638	63	77	898
R-squared	0.295	0.194	0.482	0.493	0.180
Control mean	0.0403	0.0255	0.120	0.367	0.0608

Notes: Authors' calculations from endline survey data. All outcome variables are binary variables capturing receipt of a formal tax invoice when making a purchase. "NFE supplier" refers to inputs purchases from suppliers for non-farm enterprises; "When traveling" refers to input purchases made by traveling to nearby towns or markets; "Phone order" refers to purchases made over the phone; "Proxy buyer" refers to purchases made via an intermediary, for example by asking a neighbor to make a purchase on one's behalf when they travel to a market; "Any invoice" is the maximum of the outcomes in columns 1-4, defined only for households that appear in at least one of the regressions in columns 1-4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Baseline Characteristics

Variable	Mean	SD
Panel A: General Characteristics		
Owner age	37.67	10.65
Woman-owned firm (1=yes)	0.57	0.50
Primary education (1=yes)	0.93	0.26
Located in marketplace (1=yes)	0.25	0.43
Has open stall (1=yes)	0.50	0.50
Started from scratch (1=yes)	0.77	0.42
Retail sector (1=yes)	0.65	0.48
Pays rent (1=yes)	0.63	0.48
Weekly operating hours	77.41	20.51
Applied for loan (1=yes)	0.11	0.32
Had any business shock (1=yes)	0.71	0.45
Distance to city (km)	18.91	25.71
Farming (1=yes)	0.32	0.47
Number of competitors	22.68	21.27
Firm density (firms/km ²)	15.29	29.68
Subsector firm density (firms/km ²)	3.36	9.29
Matches competitor prices (1=yes)	0.84	0.37
Matches competitor quality (1=yes)	0.80	0.40
Uses mobile money (1=yes)	0.27	0.44
Panel B: Mobile-Money Awareness		
Uses mobile money (1=yes)	0.27	0.44
Aware of mobile-money sending fees (1=yes)	0.22	0.41
Aware of mobile-money sending tax (1=yes)	0.23	0.42
Aware of mobile-money receiving tax (1=yes)	0.45	0.50
Aware of mobile-money receiving fees (1=yes)	0.56	0.50
Panel C: Supplier Relationship Benefits		
Pays supplier via mobile money (1=yes)	0.11	0.31
Uses mobile agent for supplier payments (1=yes)	0.57	0.50
Receives goods on credit (1=yes)	0.29	0.45
Receives bulk discount (1=yes)	0.34	0.47
Receives frequent-buyer discount (1=yes)	0.35	0.48
Supplier shipped goods (1=yes)	0.19	0.39
Receives none	0.50	0.50
Receives all	0.07	0.26
Panel D: Customer Relationship Benefits		
Offers frequent-buyer discount (1=yes)	0.46	0.50
Offers cash loan (1=yes)	0.10	0.30
Offers special-order service (1=yes)	0.22	0.41
Offers delivery service (1=yes)	0.28	0.45
Receives mobile payments (1=yes)	0.25	0.43
Offers none	0.31	0.46
Offers all	0.01	0.08
Panel E: Inventory & Investment		
Inventory value (TZS)	379183.77	1124336.79
Business supplies (TZS)	1212628.67	2310000.43
Large investment (≥ 250000 TZS, 1=yes)	0.21	0.41

Table 4: Treatment effects on WTP for directory listing services

	Willingness to Pay	
	(1)	(2)
Tax priming	-0.048 (0.111)	-0.038 (0.107)
Control mean	4.526	4.526
CSEs: Tax priming	(0.114)	(0.108)
LASSO selected controls		X
Number of villages	17	17
Number of observations	576	576
Pseudo R-squared	0.000	0.044

Notes: Column (2) includes the LASSO-selected controls: applied for a loan, experienced a business shock, awareness of mobile-money receiving fees, receipt of bulk discounts, and having made a large investment. The willingness-to-pay variable is measured in Tanzanian shillings and transformed using the inverse hyperbolic sine transformation following Bellemare and Wichman (2020). For the main coefficient rows, the estimates use wild-cluster bootstrap standard errors clustered at the village level (17), with significance stars based on 1,000 clustered bootstrap replications following Cameron, Gelbach, and Miller (2008). Conventional standard errors (CSEs) are clustered at the village level, with significance stars based on the corresponding clustered p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneity in WTP by competition, competitor price, and quality

	Willingness to Pay			
	(1)	(2)	(3)	(4)
Tax priming	-0.450*	-0.442*	-0.288	-0.325
	(0.239)	(0.231)	(0.220)	(0.227)
Tax priming $\times$ High competition	-0.101	-0.011	-0.065	0.016
	(0.207)	(0.255)	(0.206)	(0.254)
Tax priming $\times$ Competitor's price	0.529***	0.483**		
	(0.193)	(0.215)		
Tax priming $\times$ Competitor's quality			0.341	0.348
			(0.247)	(0.281)
Control mean	4.526	4.526	4.526	4.526
CSEs: Tax priming	(0.236)*	(0.225)**	(0.205)	(0.204)
CSEs: Tax priming $\times$ High competition	(0.212)	(0.250)	(0.210)	(0.249)
CSEs: Tax priming $\times$ Competitor's price	(0.183)***	(0.199)**		
CSEs: Tax priming $\times$ Competitor's quality			(0.234)	(0.262)
LASSO selected controls		X		X
Number of villages	17	17	17	17
Number of observations	576	576	576	576
Pseudo R-squared	0.006	0.049	0.009	0.050

Notes: High competition is a binary indicator equal to 1 if the firm faces above-median competition in its local market. Competitor's price and Competitor's quality are binary indicators equal to 1 if the competitor's price/quality matches the focal firm and 0 if they differ. Columns (2) and (4) include the LASSO-selected controls: applied for a loan, experienced a business shock, awareness of mobile-money receiving fees, receipt of bulk discounts, and having made a large investment. The willingness-to-pay variable is measured in Tanzanian shillings and transformed using the inverse hyperbolic sine transformation following Bellemare and Wichman (2020). For all coefficient rows, estimates use bootstrap standard errors clustered at the village level (17). Conventional standard errors (CSEs) are reported below the control mean; these are clustered at the village level, with significance stars based on the corresponding clustered p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneity in WTP by firm inventory and supplies

	Willingness to Pay			
	(1)	(2)	(3)	(4)
Tax priming	-0.161 (0.192)	-0.152 (0.219)	-0.400 (0.401)	-0.322 (0.419)
Tax priming $\times$ Firm inventory	0.011 (0.019)	0.012 (0.020)		
Tax priming $\times$ Firm supplies			0.030 (0.033)	0.023 (0.036)
Control mean	4.526	4.526	4.526	4.526
CSEs: Tax priming	(0.185)	(0.207)	(0.385)	(0.388)
CSEs: Tax priming $\times$ Firm inventory	(0.018)	(0.019)		
CSEs: Tax priming $\times$ Firm supplies			(0.032)	(0.034)
LASSO selected controls		X		X
Number of villages	17	17	17	17
Number of observations	576	576	576	576
Pseudo R-squared	0.019	0.060	0.015	0.056

Notes: Columns (2) and (4) include the LASSO-selected controls: applied for a loan, experienced a business shock, awareness of mobile-money receiving fees, receipt of bulk discounts, and having made a large investment. Firm inventory is the log of the value of the inventory the business holds over the past month, while Firm supplies is the log of the total value of all the firm's supplies over the past month. The willingness-to-pay variable is measured in Tanzanian shillings and is transformed using the inverse hyperbolic sine transformation (Bellemare and Wichman, 2020). For all coefficient rows, the reported standard errors in parentheses are wild-cluster bootstrap standard errors clustered at the village level (17). Conventional standard errors (CSEs) are reported below the control mean; these are clustered at the village level, with significance stars based on the corresponding clustered p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity in WTP by mobile-money use and transaction channels

	Willingness to Pay							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax priming	-0.035 (0.109)	-0.036 (0.106)	-0.091 (0.140)	-0.098 (0.133)	-0.117 (0.126)	-0.113 (0.118)	-0.057 (0.144)	-0.036 (0.155)
Tax priming $\times$ Payments to suppliers	-0.100 (0.391)	-0.052 (0.458)						
Tax priming $\times$ Receiving customer payments			0.133 (0.318)	0.189 (0.318)				
Tax priming $\times$ Mobile-money (MM) use					0.281 (0.201)	0.287 (0.207)		
Tax priming $\times$ MM agent (supplier pay)							0.003 (0.152)	-0.023 (0.161)
Control mean	4.526 (0.112)	4.526 (0.105)	4.526 (0.142)	4.526 (0.131)	4.526 (0.125)	4.526 (0.116)	4.526 (0.145)	4.526 (0.152)
CSEs: Tax priming								
CSEs: Tax priming $\times$ Payments to suppliers								
CSEs: Tax priming $\times$ Receiving customer payments								
CSEs: Tax priming $\times$ Mobile-money (MM) use								
CSEs: Tax priming $\times$ MM agent (supplier pay)								
LASSO selected controls		X		X		X		X
Number of villages	17	17	17	17	17	17	17	17
Number of observations	576	576	576	576	576	576	576	576
Pseudo R-squared	0.009	0.049	0.013	0.054	0.015	0.053	0.013	0.049

Notes: Columns (2), (4), (6), and (8) include the LASSO-selected controls: applied for a loan, experienced a business shock, awareness of mobile-money receiving fees, receipt of bulk discounts, and having made a large investment. The mobile-money heterogeneity variables are binary indicators measured over the past month. Mobile-money usage equals 1 if the firm primarily uses mobile-money services for its business transactions. Payments to suppliers equals 1 if the firm pays suppliers and vendors using mobile money. Receiving customer payments equals 1 if the firm receives customer payments via mobile money. Using a wakala (agent) for supplier payments equals 1 if the firm uses a mobile-money agent (wakala) to pay suppliers. The willingness-to-pay variable is measured in Tanzanian shillings and is transformed using the inverse hyperbolic sine transformation (Bellemare and Wichman, 2020). For all coefficient rows, the reported standard errors in parentheses are wild-cluster bootstrap standard errors clustered at the village level ($v = 17$). Conventional standard errors (CSEs) are reported below the control mean; these are clustered at the village level, with significance stars based on the corresponding clustered p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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