Working Paper MNG-20167 March 2022

Does consumer monitoring reduce corporate tax evasion along the supply chain? Evidence from Mongolia

This paper considers a Mongolian government program that incentivises consumers to report their purchases, tracking its effects on corporate income tax (CIT) and value-added tax (VAT).

Tsogsag Nyamdavaa

IGC

DIRECTED BY

LSE UNIVERSITY OF OXFORD

FUNDED BY

UKaid from the British people

Does Consumer Monitoring Reduce Corporate Tax Evasion Along the Supply Chain? Evidence from Mongolia*

Tsogsag Nyamdavaa†

March, 2022

Click here for the latest version

Abstract

This paper tracks the effects of consumer monitoring on firms' tax evasion along the supply chain. To do so, I study a Mongolian government program, which incentivises consumers to report their purchases. First, I estimate the effect of the program on corporate income tax (CIT) and value-added tax (VAT), by comparing retailers who are directly affected, and wholesalers, who are only indirectly affected. I find that retailers increase their reported sales, but partly offset this by artificially inflating their costs on CIT returns. As a result, retailers' CIT liabilities increase by 11%. In comparison, their VAT liabilities increase by 31% because VAT is less prone to such cost manipulation. Second, I find that the program also increases the VAT liabilities of upstream firms by about 15% when they are more likely to sell to (monitored) retailers, compared to the upstream firms that sell to firms that are not directly monitored. The program does not, however, affect the upstream firms' reported CIT liabilities. My findings highlight the enforcement advantage of VAT compared to CIT and that consumer monitoring enhances the self-enforcing mechanism in VAT along the supply chain.

JEL Codes: H25, H26, E26. **Keywords:** Anti Tax, Collection, Compliance, Evasion, Income Tax Evasion, Tax Compliance, Tax Evasion, Shadow Economy, Underground Economy.

*I am deeply indebted to Guy Michaels, Rachel Ngai and Joana Naritomi for invaluable supervision. I thank Andrea Alati, Miguel Bandeira, Fabio Bertolotti, Adrien Bussy, Francesco Caselli, Martina Fazio, Ethan Ilzetzki, Xavier Jaravel, Dana Kassem, Takafumi Kawakubo, Sevim Kosem, Per Krusell, Camille Landais, Kensuke Teshima, Kieu-Trang Nguyen, Daniel Reck, Arthur Seibold, Yoichi Sugita, Johannes Spinnewijn, and Torsten Walter and many others at LSE and Hitotsubashi University for their helpful comments and discussions.

†Hitotsubashi University. Email: tsogsag.nyamdavaa@r.hit-u.ac.jp

1 Introduction

Power to tax lies at the heart of state development, and state capacity, in turn, is an important factor for economic development. However, it is well known that developing countries tax very little. Specifically, the tax-to-GDP ratio is positively correlated with countries' level of development [Besley and Persson, 2014]. Therefore, it is crucial to study tax enforcement and explore the ways to strengthen it in developing countries. Firms play a crucial role in taxation in all modern tax systems. They remit the majority of tax revenues to the government, either with regard to their own tax liabilities or through the withholding of taxes of employees or other businesses ([Kopczuk and Slemrod, 2006]; [Slemrod and Velayudhan, 2017]).[1] Hence, much of the recent literature on tax enforcement and development has focused on firms.

A growing body of literature has documented that third-party information reporting in the form of consumer monitoring, whistle-blowers and paper trails could enhance tax enforcement because tax authorities can use it to verify firms' tax reporting (for example [Naritomi, 2019]; [Pomeranz, 2015]; [Carrillo et al., 2017]; [Slemrod et al., 2017]). In particular, it is well-known that VAT has a self-enforcing mechanism that creates a paper trail on transactions between firms, which makes it harder to hide the business-to-business (B2B) transactions from the authorities. The reason is that each B2B transaction is reported twice, once by the seller and once by the buyer, which enables the authorities to cross-check the information and detect any misreporting by the firms. However, this built-in enforcement mechanism breaks down at the end of the supply chain as final consumers do not typically report their purchases. This creates an opportunity for the firms at the end of the supply chain to under-report sales and potentially collude with upstream firms to evade tax.

This paper tracks the effects of using final consumers as "firms' sales auditors" on firms' tax reporting behaviour along the supply chain. To do so, I exploit an anti-tax evasion program, called E-receipt program, implemented by the Mongolian government in 2016, where consumers are incentivised to report their purchase.[2] Using rich confidential administrative tax data on firms' tax returns as well as their trade network that span the period between 2014 and 2018, I study the changes in tax liabilities of firms along the supply chain. Specifically, I focus on firms' VAT and CIT liabilities because they are the main taxes that firms remit in most countries.[3] More importantly, comparing these taxes in one setting highlights the importance of third-party information. For CIT, it is known that third-party information on firms' sales does not necessarily lead to more tax payments even though it increases firms' reported sales (for example, [Carrillo et al., 2017]; [Slemrod et al.,

[1] To be specific, firms remit 85% of total tax revenue in OECD countries and India ([Slemrod and Velayudhan, 2017]).

[2] Similar consumer monitoring programs were implemented in many other countries such as Brazil, Taiwan, Portugal and Slovakia. See Section 2.2 for details of the program.

[3] They constitute more than 40% of the total tax revenue all around the world as shown in Figure A1. The share is higher for low and middle-income countries. In particular, CIT and VAT together made up 47% of total tax revenue in Mongolia in 2016. In comparison, payroll tax accounted for 15% of the total tax revenue in Mongolia.

2017]). This is because firms take advantage of the fact that costs are less verifiable for tax authorities and offset the effect on their CIT liabilities by reporting higher costs. For VAT, as discussed above, firms' reported costs on VAT returns are constrained by the declarations of suppliers. Therefore, it is harder to manipulate costs on VAT returns.

I start by studying the direct effect of the program on the firms at the end of the supply chain — retailers — because their sales are directly monitored by final consumers. To identify the effects on retailers' CIT and VAT reporting behaviour I use variation in treatment intensity. I use a difference-in-difference (DiD) estimation method, where I take retailers as a treatment group and wholesalers as a control group.[4] I find that the program increases retailers' reported sales on CIT returns by 20%. However, retailers increase their reported costs by 23% in response to the program, leading to only 11% increase in their CIT obligations. Using tax audit data, I find that some of the increase in reported costs is due to increased misreporting. On the other hand, I find stronger effects on VAT reporting. Retailers' reported sales on VAT return increase by 42%. Even though they report higher input costs their VAT liabilities increase by 31%. There are two potential reasons why I find a larger increase in retailers' VAT liabilities than CIT liabilities. First, the composition of the firms used for CIT and VAT analysis is different. All firms submit CIT returns in Mongolia, but only large firms are liable for VAT. In other words, firms below the VAT threshold file only CIT returns, but larger firms submit both CIT and VAT returns. Therefore, to directly compare the CIT and VAT response, I restrict my sample to the large VAT-liable firms. I still find larger effect on retailers' VAT than CIT: their VAT and CIT liabilities increase by 25% and 17%, respectively. The second reason is the fact that it is relatively difficult for firms to over-report their costs on VAT returns because they could be cross-checked with reported sales of their trading partners. Consistent with this, the audit data do not show any sign of increased cost over-reporting on VAT returns, unlike CIT.

It is important to note that any increase in reported costs of a VAT-liable retailer must be associated with an increase in its upstream firms' sales because of the self-enforcing mechanism in VAT. This can happen if the retailer had been colluding with its upstream firms and hiding (some of) the B2B transactions before the intervention. In other words, any increase in reported input costs of retailers on VAT returns implies collusion along the supply chain. Clearly such collusion is beneficial for the upstream firms because it reduces their reported sales and tax liabilities. However it is not straightforward to see why retailers have an incentive to collude and underreport their costs, but there could be a number of reasons. For example, it would look suspicious to the tax authorities if a retailer declares purchasing costs of a good but does not report the sales. By hiding both purchases and sales of the good, the retailer can keep all the profits to themselves without paying any tax. Also the retailer would appear smaller on tax returns and hence could stay off the radar of the tax authorities.[5] Once final consumers start reporting their purchases to the tax authorities the

[4]This estimation method is commonly used to study the effects of similar policies in the literature. For example, Naritomi, 2019 uses the same DiD estimation strategy to estimate the direct effect of the "NFP" program in Brazil, which employs consumers as the third-party reporters.

[5]Another example is that the retailer could be offered a discount by the upstream firms, the sellers. If retailers do not report their purchases on their tax returns, the sellers would not have to pay tax on those

retailers would want to increase their reported costs and thus stop colluding with upstream firms. This leads to an increase in reported sales of the upstream firms and potentially their tax liabilities.

Therefore, next, I explore the indirect effects of the program up the supply chain. I utilise my firm network data that cover periods before and after the intervention. I define upstream firms as firms that have ever supplied a retailer before the intervention, and rank them in terms of their share of pre-intervention sales to retailers. Then I use the DiD estimation approach to estimate the effects on upstream firms, where the treatment group is the firms, whose average pre-intervention share of sales to retailers is above the median, and the firms below the median are categorised as the control group. I find no significant effect on CIT liabilities of upstream firms with above-median sales to retailers compared to the below-median firms. In contrast, their VAT liabilities are estimated to increase by at least 15%. As a robustness check, I run transaction-level DiD within each upstream firm, where I compare its sales to retailers to its sales to non-retail firms. I find that upstream firms' sales to retailers increase by at least 22% in contrast to their sales to other firms. These results suggest that the E-receipt program does not only affect the firms at the end of the supply chains, but also has a positive indirect effect on their suppliers.

It is worth noting that both analyses of the direct effect on retailers and the indirect effect on upstream firms underestimate the true effects of the E-receipt program. The direct effect analysis uses wholesalers as a control group for retailers. The underlying assumption for this strategy is that wholesalers would have behaved similarly to retailers in the absence of the intervention (parallel trend assumption) and that wholesalers are not affected by the program. The data indicate a reasonable parallel trend in the sales of retailers and wholesalers before the intervention, which validates the parallel trend assumption. However, the wholesalers are likely to be affected by the program both directly and indirectly. Wholesalers are likely to be directly affected because they could sell to final consumers. Also, not surprisingly, wholesalers are classified as upstream firms, and I find substantial spillover effect on the upstream firms. Therefore, the estimated effects are a lower bound of the true direct effects on retailers. To investigate the extent of underestimation, I run another version of DiD regression, in which I use the wholesalers that never sell to any retailers as a control group. I identify such wholesalers using the firm network data. The results suggest a substantial underestimation for both CIT and VAT analysis. Similarly, the indirect analysis lead to an underestimation because the upstream firms in the control group sells to retailers to some extent and thus affected by the program. Acknowledging these limitations of the analysis indicates that the overall impact of the E-receipt program on tax revenue is even larger.

Lastly, I do a simple cost-benefit analysis of the program. To implement the E-receipt program the Mongolian government promises 20% of the VAT to the consumers as well as it holds monthly lottery events. Moreover, it bears some administrative costs such as expenses associated with installing IT systems and wage costs of the IT engineers. Considering these

sales and transfer some of the gains to the buyer. Therefore, it can be profitable for both the seller and the buyer to hide their transactions. Alternatively, the retailers could be involved in some underground activities, such as selling alcohol without a license, hence have an incentive to hide both sales and costs from tax authorities.

costs, I find that a 30.4% increase in VAT payments is needed for the program to break even.[6] As discussed above, I find that retailers' VAT liabilities increase by 31%, which is just enough to cover the costs of the program. Hence if one focuses only on the direct effects on retailers' VAT liabilities, as in the previous literature, then the program would appear not being able to increase the total tax revenue for the government. Once we account for the spillover effect on upstream firms' VAT liabilities and other tax bases such as CIT, then it is clear that the program leads to larger tax revenue in Mongolia.

Related literature. This paper adopts a holistic approach to study the effects of a consumer monitoring program, and contributes to the literature on the role of third-party reporting in tax enforcement in two important ways. First, I show that it is crucial to include the final consumers into VAT reporting, as this ensures better enforcement along the whole supply chain. To date, the literature has studied the effects of consumer monitoring only on firms at the end of the supply chain [Naritomi, 2019]. I extend this further and document that consumer monitoring does not only affect the downstream firms but also upstream firms along the VAT chain indirectly. This chain effect in VAT has been studied in the literature both theoretically [de Paula and Scheinkman, 2010] and empirically [Pomeranz, 2015]. Specifically, [Pomeranz, 2015] finds that increased tax enforcement can have spillover effects on the targeted firms' trading partners. However, its analysis focuses on the firms suspected of tax evasion, and the data collection process potentially entails some attrition and selection bias concerns. I, on the other hand, study the entire population of firms in the trade sector and their network using official administrative tax data. Second, I reconcile the different effects of third-party information on CIT and VAT in the literature by studying these taxes together. In particular, the literature has found firms' limited ability to adjust their reported costs for VAT [Naritomi, 2019], but close to full adjustment of costs for CIT ([Carrillo et al., 2017]; [Slemrod et al., 2017]). Also, for CIT, firms' reported costs are found to be much more elastic than their reported sales [Bachas and Soto, 2015]. I reconcile these different findings by studying both CIT and VAT in one setting in the context of consumer monitoring. I find that the built-in enforcement mechanism in VAT is the driving force of larger effects on firms' VAT liabilities. On the other hand, for CIT, I discover that firms are substituting away from under-reporting sales to over-reporting costs when there is an improvement in sales enforcement. To the best of my knowledge, this paper is the first to offer direct evidence that firms respond to improved sales enforcement by increasing cost misreporting on CIT returns.

[6] My calculation does not include compliance costs for consumers and firms. The compliance cost for consumers is negligible because it is very easy to report their purchases to the government. See Section 2.2 for details of the program. Any compliance costs for firms should be reflected in their CIT liabilities and I find that retailers' CIT liabilities increase by 11%. In this sense, firms' compliance costs are accounted for in my analysis. Furthermore, there are other intangible aspects of the program. Firstly, the program could be changing certain societal norms that may have long-lasting effects even after the program ends. These changes include people getting used to asking for receipts, an increase in tax awareness, greater attention to the public expenditure, and more demand for efficient public spending and so on. On the other hand, the program increases firms' tax burden of the firms which could also increase the efficiency costs of the CIT and VAT. Moreover, I do not study any changes in tax incidence or transfer of the tax burden. Even though these are interesting and important aspects of tax enforcement they are beyond the scope of this project.

The remainder of this paper is structured as follows: Section 2 provides a background on the Mongolian tax system and explains the relevant datasets and their summary statistics. It also describes the policy intervention — the E-receipt program. Section 3 describes the empirical strategy and presents the results. Section 4 shows a simple cost-benefit analysis of the program, and Section 5 concludes.

2 Institutional Background and Policy Intervention

This paper utilises a nationwide anti-tax evasion program in Mongolia to study the effects of consumer monitoring on tax evasion behaviour of firms along the supply chain. In this section, first, I briefly describe the institutional background and tax system in Mongolia. Then I explain the anti-tax evasion program. Lastly, I discuss the datasets and provide summary statistics.

2.1 Mongolian Economy and Tax System

Mongolia is a lower-middle-income country and its GDP per capita (PPP) was around \$12,200 in 2018.[7] In this sense, the country's level of development is similar to Sri Lanka. However, Mongolia is often compared to Kyrgyzstan because both countries are landlocked, rich in mineral resources, both were under the influence of the Soviet Union and have a small population, even though Kyrgyzstan has a lower GDP than Mongolia.

Tax evasion is an indispensable part of the shadow economy, whose measures could indicate the extent of tax evasion in the economy.[8] The size of the shadow economy in Mongolia between 1999 and 2006 was estimated to be 18% of its GDP, while the average share of the informal economy for other 88 developing countries the same year was 35% [Schneider et al., 2010]. Therefore, Mongolia is not considered to have a relatively high share of activities in its unofficial economy.

In this paper, I focus on two taxes — CIT and VAT, which are the main taxes firms remit in most countries. In particular, together they made up 47% of the total tax revenue in Mongolia in 2016. For CIT, there is no threshold for eligibility, and hence all firms submit CIT returns. Hence my CIT dataset contains information on the universe of formal firms. However, not all firms submit VAT returns. There is a VAT threshold in Mongolia, whereby firms with sales above the threshold have to register as VAT-liable firms.[9] On average, 30% of the firms in the CIT data are VAT-liable each year. Once a firm becomes a VAT-liable, it

[7] Worldbank databank website, United Nations "World Economic Situation and Prospects 2018"

[8] One of the broadest definitions of the shadow economy is "those economic activities and the income derived from them that circumvent or otherwise avoid government regulation, taxation or observation" as defined in Schneider, 2012.

[9] If a firm is caught not having registered for VAT even though its annual sales are above the threshold there will be penalties. Also, it has to pay the owed VAT for the period it would have been a VAT-liable firm.

has to submit VAT returns on top of the CIT returns to the tax authorities. Having firms filing both CIT and VAT returns enables me to compare the tax reporting behaviour of firms of these taxes.

Interestingly, there does not seem any systematic cross-checking between the two tax returns even though both could be submitted by the same firms. Data show a large discrepancy between, for example, the reported total sales on the two tax returns for VAT-liable firms.[10] This is possibly due to the fact that the submission frequency as well as the way firms report their sales, costs and tax liabilities on CIT and VAT returns differ. Specifically, CIT returns are submitted quarterly, and values such as sales and costs are reported in cumulative terms. That is, in quarter one firms report sales and costs applicable for only quarter one, but in quarter two firms report the sum of quarter one and two. In quarter four, firms report their annual revenue and costs. In contrast, VAT returns are submitted monthly and reported values, such as sales and costs, corresponding to the respective month. Therefore, firms could take advantage of the fact that it is not straightforward to compare CIT and VAT returns for tax authorities and respond differently to changes in tax enforcement.

The tax base for CIT is profit, which is the difference between revenue and total costs.[11] On the other hand, the VAT base is the value-added of firms, which is equal to total sales minus the cost of input purchases.[12] In practice, VAT-liable firms collect VAT on their sales (from the final consumers and other firms) and subtract the value of VAT that they pay on their intermediate purchase and transfer the difference to the government. To prove the collected VAT as well as the VAT payment on their purchases, firms submit VAT invoices, which contain information such as the tax IDs of the trading partners, both upstream (suppliers) and downstream (buyers) firms, and the relevant transaction values. Therefore, each B2B transaction ends up being reported twice, once by the seller and once again by the buyer. This is called the VAT credit-invoice scheme that enables the tax authorities to verify firms' self-reported values on VAT returns by cross-checking. My data suggest reasonable cross-checking between reported values within VAT reporting, unlike the comparison between CIT and VAT as mentioned before.

Moreover, both CIT and VAT rates are 10% and stayed the same throughout the period of my analysis.[13] In comparison, the world average rate for CIT and VAT in 2017 was 25% and 16%, respectively.[14]

[10]More than 15% of the firms report different total sales on CIT and VAT returns, where the difference accounts for 10% of the sales declared on the CIT returns.

[11]There are some restrictions on the deductible costs. For example, firms are not allowed to deduct costs associated with paying fines, penalties, VAT and city tax payments.

[12]The difference between total costs for CIT and input purchasing costs for VAT is that total costs contain not only input purchasing costs but also wage costs, administrative costs and other costs.

[13]Actually there are two rates for CIT in Mongolia: 10% if the annual revenue is below 3 billion MNT ($\approx$ 1,150,000 USD), and 25% if the annual revenue is above 3 billion MNT. I assume the CIT rate is 10% for the sake of simplicity since most of the firms in the sample have an annual revenue below 3 billion MNT: around 1.5% of the firms have annual sales of more than 3 billion MNT each year.

[14]Sources: Tax foundation webpage — https://taxfoundation.org/publications/corporate-tax-rates-around-the-world/; and IMF Tax Policy Assessment Framework (TPAF) — https://www.imf.org/external/np/fad/tpaf/pages/vat.htm. Both accessed on 31 October 2020.

Lastly, I use tax audit data from operational tax audits. Each year the tax authorities calculate firms' tax evasion risk score using their internal and external (third-party) information. Based on these scores, they choose which firms to audit. There are also non-routine tax audits at the requests of third parties such as courts, the police or other types of whistle-blowers. Subsequently, on average, 10% of the firms are audited each year. Generally, the tax audits examine the last five years of tax returns and other financial documents and check for any irregularities and inconsistencies.[15] If any tax incompliance is found, the firm is urged to pay the corresponding tax duties and fines. More importantly, any discovered misreporting of sales and costs is aggregated to the annual value for each type of tax return and recorded in the audit reports. Therefore, the tax audit data show if a firm was found misreporting on its CIT and/or VAT returns, and if so, how much is the under-report sales and/or over-reported costs is for each audited year. It is worth noting that audit data are at an annual level, unlike the CIT and VAT returns data.

2.2 E-receipt Program

The Mongolian government introduced an anti-tax evasion program, called E-receipt program, in January 2016.[16] The purpose of the program is to use final consumers as informants about firms' sales to disincentivise the firms from hiding their revenue. The program incentivises consumers to report their receipts of purchase in two ways:

- Consumers receive 20% of the VAT that they paid on their purchase if they register the receipt. The tax rebate is transferred to the consumers' bank account annually in January the following year.
- The registered receipt automatically turns into a lottery ticket regardless of the face value. The tax authorities hold a lottery event every month. The prize amount varies month to month and ranges from 20 million MNT (≈7700 USD), which is equal to the current VAT threshold in Mongolia of 500 million MNT (≈190000 USD).

By law, all firms have to participate and issue E-receipts whenever they sell to final consumers. Once an E-receipt is issued by a firm the transaction information is semi-automatically sent to the tax authorities and the sales value has to be accounted for on the firm's tax returns, whether the consumer submits it or not.[17] E-receipts have to satisfy some requirements: they have to contain a unique, system-generated 35-digit code, a 10-digit lottery code and a QR code in addition to sales details such as the item's face value, item details, prices, date, and the tax ID of the firm. Therefore, to be able to issue E-receipts, firms

[15] The audit coverage period can be less than five years if the firm is established or was audited within the last five years.

[16] E-receipt program was put in place at the start of 2016 but the tax authorities already started publicizing it in late 2015. Even though it is possible, in principle, firms started reacting to the announcement by the end of 2015, as I show later that most of the effects began to appear after 2016.

[17] E-receipts are automatically sent to the E-receipt system if the POS machine that issues the receipt is connected to the Internet. Firms can delay this information transmission for at most three working days.

need to update or buy a new registry system and POS machines (receipt printing machines) that connect to the system via the Internet. Because of these fixed costs, there is a gradual enrollment of firms as shown in Figure A2.[18] If a firm is found not to issue E-receipts or if it refuses to issue E-receipts, consumers can report it to the tax authorities and the firm will be required to pay a penalty and could potentially face a tax audit.

The role of consumers is to make sure firms issue E-receipts and send them to the E-receipt system. It is easy for consumers to enrol in the program: they simply sign up to the E-receipt system via the website or the free mobile application by entering their details and bank account information.[19] Once the account is set up, the consumer can register receipts at any time using the E-receipt website or the mobile application as long as they have access to the Internet.[20]

Tax authorities hold a lottery event every month during which they choose the winners from that month's pooled E-receipts. An E-receipt has to be reported by both the seller firm and the consumer before the lottery event to be a valid receipt for the monthly lottery event.[21] The lottery event takes place in the middle of the month — around the 15th or 16th of each month — a few days after the VAT return submission deadline, which is the 10th of each month. This is to make sure that the consumers and firms submit their receipts before the VAT return submission date. If consumers submit their receipts after the lottery event, then the receipts will not be included in any future lottery event but they will still be eligible for the VAT rebate at the end of the year.

This lottery scheme is adopted to minimise the possibility of collusion between consumers and firms. However, there is a risk that firms may offer discounts to consumers to persuade them to collude and hide transactions from the authorities. For example, firms can collude with consumers by offering them a 10% discount, which is the VAT rate, if they do not ask for E-receipts. From the consumers' point of view, they need to choose between the firm's offer of a 10% discount now, and the government's offer of a 2% VAT rebate next year plus their luck in the lottery. If the consumers are myopic and/or do not believe that they have a high chance of winning the lottery then they might choose the firm's offer. This will attenuate the effects of the E-receipt program.

Lastly, it is noteworthy that the VAT threshold increased five-fold from 10 million MNT ($\approx$ 3800 USD) to 50 million MNT ($\approx$ 19000 USD) in January 2016, at the same time as the E-receipt program was initiated. This shift in the VAT threshold could, in general, affect the

[18] It is said that chain supermarkets or large retailers and wholesalers already had relatively modern registry systems even before the E-receipt program. Therefore, it is sufficient for them to simply update their system, which is cheaper. If it is not possible or suitable, large firms are in the financial position to invest in a registry system. On the other hand, for small firms it could be a considerable burden to buy a new registry system. To decrease the costs for small firms, it is made possible to print the system-generated receipts from the web-browser or to send them via email. Hence, for small firms with a small consumer base, it is sufficient to have a computer with Internet access and a printer.

[19] To be able to receive the VAT refunds and lottery prizes, consumers need to enter their full name, email address, phone number and government-issued ID number.

[20] Internet coverage is relatively advanced in Mongolia, and there are many places with free Wi-Fi, especially in the capital city, Ulaanbaatar.

[21] In the first three months the authorities held lottery events twice each month to attract more people.

estimation of the effects of the E-receipt program, especially in the case of CIT reporting.[22] But, as I show below, the results survive qualitatively even if I restrict my samples to the firms who have always been VAT-liable suggesting that the VAT threshold shift does not drive the results.

2.3 Datasets

This project uses four (unbalanced) panel datasets, which are CIT and VAT returns data, VAT invoice data and operational tax audit data. All of them span the period between 2014 and 2018. Since the E-receipt program started in January 2016, the datasets cover two years before the intervention and three years after the program was initiated.

As discussed before, the CIT data contain information on the universe of formal firms. The main type of information I use from CIT returns is the firms' the reported total sales, total costs and CIT liabilities. Similarly, I use the information on the reported total sales, purchasing costs and VAT liabilities from VAT returns, but only for VAT-liable firms. In the main analysis, I focus on firms with strictly positive tax liabilities and summary statistics of CIT and VAT data are presented in Table 1.[23] The CIT sample contains 25,000 firms, of which 6,500 are retailers and 18,500 are wholesalers. For VAT data, there are 14,900 VAT-liable firms, of which 3,200 are retailers and 11,700 are wholesalers.[24] As expected, wholesalers are larger in size than retailers and have larger tax bills. Also, VAT-liable firms report larger sales and costs.

Moreover, the VAT invoice data provide information on transactions between all VAT-liable seller-buyer pairs, which allows me to study the spillover effect of the E-receipt program on the upstream firms of retailers. I define the upstream firms as the firms that have ever sold to any retailer before the intervention. A total of 4,600 upstream firms are identified, most of which belong to trade (wholesale or retail), manufacturing and professional activities such as consulting sectors as shown in Table 2.

Table 3 presents descriptive statistics of transaction values from the VAT invoice data.

[22] It is known that firms bunch below the VAT threshold by misreporting their sales and/or restricting their production or activity. When the threshold increases firms could stop bunching and report larger revenue sums if misreporting had existed previously. If this is mainly true for retailers then it could result in over-estimation.

[23] The main results survive qualitatively if I use the entire sample that contains observations with non-positive tax liabilities as shown in Appendix A.15.
Also, as mentioned before, on CIT returns firms report their sales, costs and tax liabilities in cumulative terms. For example, in quarter two firms report the sum of quarter one and two, and in quarter four firms report their annual sales, costs and tax liability. Therefore, to calculate the quarterly revenue and costs, I subtract the previous quarter's value from the current quarter unless it is quarter one. Because I take the difference the quarterly sales and costs could result in a negative due to reporting or data quality issues. I drop such cases in the main analysis.

[24] It might seem unusual to have more wholesalers than retailers. My definition of retailer and wholesaler is based on firms' 4-digit industry classification code (ISIC Rev.2), which is reported on CIT returns. Even if some retailers are mistakenly classified as wholesalers, this will lead to an underestimation of the effect of the program.

Table 1: Summary statistics - CIT & VAT data

(a) CIT returns	mean	sd	count	(b) VAT returns	mean	sd	count
Retailers				*Retailers*			
Sales	20.68	74.12	71,454	Sales	61.52	241.22	32,816
Expenses	19.24	70.47	71,454	Purchases	43.01	180.30	32,816
CIT liab	0.10	0.48	71,454	VAT liab	1.48	5.79	32,816
Wholesalers				*Wholesalers*			
Sales	64.05	137.86	130,684	Sales	121.26	318.76	101,136
Expenses	58.88	131.93	130,684	Purchases	69.84	220.36	101,136
CIT liab	0.40	0.94	130,684	VAT liab	3.96	8.66	101,136

Note: Table 1a presents descriptive statistics of the main variables from CIT returns. Sales, Expenses and CIT liab. are the quarterly gross reported sales, purchases and CIT liabilities of firms. Table 1b presents descriptive statistics of the main variables from VAT returns. Sales, Expenses and VAT liab. are the quarterly gross reported sales, purchases and VAT liabilities of VAT-liable firms. All nominal values are in thousand USD (1 MNT = 2600 USD).

In particular, I sum the upstream firms' sales to retailers within each quarter and summarise it in the part *Sales to retailers*, and similarly their total quarterly sales to non-trade sector firms such as hotels and schools as shown in *Sales to other firms*. On average, upstream firms sell twice as much (in terms of value of transaction) to non-retail firms as the transaction values are more than twice as the value of the transaction to retailers.

Lastly, I use tax audit data that come from operational tax audits. As mentioned before, tax audits usually cover the last five years of tax returns and other financial documents. Therefore, I use the firms audited in 2017 or after so that the audited period covers both pre- and post-intervention periods.[25] The audit data contain information on the year of audit, whether any misreporting was discovered on their CIT and/or VAT returns, and if so, the value of the under-reported sales and/or over-reported costs for each audited year.

CIT and VAT audit data are summarised in Table 4, which report (annual) values of under-reported sales and their share in firms' true sales for each type of tax return. The true sales are calculated as the sum of reported sales and hidden sales. They also provide summary statistics of (annual) values of over-reported costs, and their share in the true costs in each type of tax return. The true costs equal the difference between reported costs and the over-reported costs.

In particular, in CIT audit data, there are in total 4,000 firms audited between 2017

[25] I drop firms that are audited before 2016 because the audited period will be between 2011 and 2015, which does not cover the post-intervention period.

Table 2: Industries of upstream firms

	Frequency	Percentage	Cum.Percentage
Administrative activities	56	1.22	1.22
Agriculture	46	1.00	2.23
Arts	9	0.20	2.42
Construction	171	3.74	6.16
Education	12	0.26	6.42
Electricity	64	1.40	7.82
Finance	35	0.76	8.58
Health	10	0.22	8.80
Hotel	91	1.99	10.79
IT	141	3.08	13.87
Manufacturing	386	8.43	22.30
Mining	31	0.68	22.98
Other services	35	0.76	23.74
Professional activities	311	6.79	30.54
Public administration	197	4.30	34.84
Real estate	32	0.70	35.54
Trade	2,855	62.36	97.90
Transportation	59	1.29	99.19
Water supply	37	0.81	100.00
Total	4,578	100.00	

Table 3: Summary statistics - VAT invoice data

	mean	sd	count
Sales to retailers			
Trans. Value	20.98	91.96	48,202
Sales to other firms			
Trans. Value	46.90	146.03	71,505

Note: Table 3 presents descriptive statistics of transaction values from VAT invoice data. *Sales to retailers* represents the average upstream firm's quarterly gross sales to retailers (summed over all retailers) and *Sales to other firms* shows its gross sales to non-retail and non-wholesale firms. All nominal values are in thousand USD (1 MNT = 2600 USD).

and 2018, of which 1,300 are retailers and 2,700 are wholesalers. The data are unbalanced, therefore, there are 960 retailers and 1,856 wholesalers in a year. Retailers under-report 2.33% (over-report 1.12%) of the total sales (costs). Wholesalers misreport 2.72% (1.76%) of the total sales (costs). For VAT audit data, there are fewer firms as expected: 1,060

VAT-liable retailers and 2,308 VAT-liable wholesalers in total. On average, 755 retailers and 1,566 wholesalers are audited in a year. VAT-liable firms are more likely to under-report their sales and less likely to over-report their costs on VAT returns compared to CIT data. In particular, VAT-liable retailers misreport 4.24% (0.63%) of the total sales (costs) and VAT-liable wholesalers under-report 4.28% (over-report 2.08%) of the total sales (costs).[26]

Table 4: Summary statistics - Audit data

(a) CIT returns

	mean	sd	count
Retailers			
Under.rep.sales ($)	0.72	6.13	4,819
share (%)	2.33	12.00	4,819
Over.rep.costs ($)	0.96	11.90	4,819
share (%)	1.12	5.09	4,819
Wholesalers			
Under.rep.sales ($)	3.73	30.34	9,281
share (%)	2.72	15.25	9,281
Over.rep.costs ($)	4.80	27.12	9,281
share (%)	1.76	6.96	9,281

(b) VAT returns

	mean	sd	count
Retailers			
Under.rep.sales ($)	1.81	15.60	3,775
share (%)	4.24	21.93	3,775
Over.rep.costs ($)	0.64	8.96	3,775
share (%)	0.63	4.78	3,775
Wholesalers			
Under.rep.sales ($)	4.32	29.53	7,831
share (%)	4.28	24.75	7,831
Over.rep.costs ($)	4.12	46.91	7,831
share (%)	2.08	9.30	7,831

Note: Table 4a and 4b present summary statistics of CIT and VAT audit data, respectively. Specifically, it summarises (annual) values of under-reported sales and their share in firms' true sales on each type of tax returns. The true sales are calculated as the sum of reported sales and hidden sales. Similarly, it provides summary statistics of (annual) values of over-reported costs, and their share in true costs in each type of tax return. The true costs are calculated as the difference between reported costs and the over-reported costs. All nominal values are in thousand USD (1 MNT = 2600 USD).

3 Empirical Analysis

This section empirically studies the effects of the E-receipt program on the tax evasion behaviour of firms along the supply chain. The purpose of the program is to use consumers as third-party reporters to reduce firms' sales misreporting. Therefore, firms at the end of supply chains — retailers — are directly affected by the program. I call the effects of the program on retailers as the "direct effect" and analyse it in Subsection 3.1. Next, I examine the spillover effects up the supply chain in Subsection 3.2, which I call the "indirect effect"

[26]Firm composition in CIT and VAT data is different because CIT audit data contain not only VAT-liable firms but also non-VAT-liable firms. Therefore, I compare CIT and VAT audit data for VAT-liable firms only and summary statistics are reported in Table A1. It shows that VAT-liable firms are more likely to under-report their sales and less likely to over-report their costs on VAT returns compared to CIT returns.

of the program. In particular, I study the changes in tax liabilities of retailers' upstream firms.

3.1 Direct Effects — Retailers

To identify the direct effect of the program on retailers, I use the difference-in-difference (DiD) estimation approach, where I take retailers as a treatment group and wholesalers as a control group. Wholesalers are considered to be a reasonable control group for retailers because they both belong to the trade sector, and are likely to be affected by the same macro shocks.[27] However, one can think of a few caveats with this approach. First, my analysis is restricted to the trade sector only. More importantly, this estimation approach underestimates the true direct effect of the program due to two reasons. First, wholesalers could be directly treated by the E-receipt program if they sell to final consumers. Second, as I discuss later, there could be a spillover effect of the E-receipt program on the wholesalers via retailers. To investigate this further, I run another version of DiD regression, in which I use the wholesalers that never sell to any retailers as a control group. I identify such wholesalers using the firm network data from the VAT invoices. The results suggest substantial underestimation. Therefore, it is important to acknowledge these caveats of the identification strategy.

I am interested in estimating the effects of the program on retailers' CIT and VAT reporting behaviour. Below, I analyse them separately because of the following three reasons: First, the firms that submit the CIT & VAT returns are different. CIT data include all the firms whereas VAT data include only VAT-liable firms. Second, even though VAT-liable firms fill out both CIT and VAT returns, the reported values such as total sales and total costs do not necessarily match one-to-one between the two tax returns. This is because no systematic cross-checking is done by the authorities between the information on CIT and VAT returns.[28] Third, a more critical difference between CIT & VAT is the credit-invoice scheme inherent in VAT, which makes sure that VAT-liable trading partners monitor one another. As I explain later, this difference plays a vital role when interpreting the results. I start from the CIT data first because they cover all formal firms. Then I move on to VAT data and discuss the role of the credit-invoice design.

3.1.1 CIT

I start by showing that wholesalers are a valid control group, i.e., there is no pre-trend in reported sales before the intervention. I do this in two ways. First, I make a sector-level comparison between the retail and wholesale sector. Specifically, I aggregate sales of retailers each period and standardise it by dividing the sums by the pre-intervention mean value of

[27] This identification strategy is commonly found in the literature. For example, Naritomi, 2019 adopts this strategy to study the effects of a similar consumer monitoring intervention in Brazil.

[28] It is said that if the tax officers manually cross-check the tax returns, then any unusually large gap would be noticeable. In that case, they would contact the firm and ask them to justify such disparities.

the sums.[29] I do the same for wholesalers and plot them over time in panel (a) in Figure 1. As we can see from the plot, there is no pre-trend before the policy change, but total sales of retailers start to increase more compared to wholesalers in 2016. The gap between sales of retailers and wholesalers starts widening over time, and I attribute this divergence to the E-receipt program under the assumption that wholesalers are a valid control group.[30]

Next, to establish the parallel trend I do a firm-level analysis, where I run the following flexible DiD regression:

$$ln(Y_{its}) = \gamma_i + \delta Quarter_t + \sum_{t=-8}^{11} \beta^t (Treat_{is} \cdot Quarter_t) + u_{its} \tag{1}$$

where subscripts i, t, s represent firm, quarter and 4-digit ISIC Rev.2 industry code respectively. $Treat_{is}$ equals one if firm i is a retailer, otherwise zero. The left had side variable $ln(Y_{its})$ is the log of quarterly revenue of the firms i in sector s in period t. In this regression I include firm fixed effect γ_i and quarter fixed effect $Quarter_t$. Therefore, my coefficients of interest are βs. I cluster the error terms by using 4-digit industry code. The estimated βs are plotted in panel (b) in Figure 1 which prove that there is no pre-trend. In particular, the confidence intervals always include zero before 2016, and the βs after the intervention are positive and significantly different from zero. This means that the wholesale sector is a valid control for retailers, and that E-receipt program significantly increased retailers' reported sales relative to wholesalers.

To see the effect of the E-receipt program on other variables such as CIT liabilities reported on CIT returns, I run the following simple DiD regression:

$$ln(Y_{its}) = \gamma_i + \delta Post_t + \beta Treat_{is} \cdot Post_t + u_{its} \tag{2}$$

where subscripts i, t, s represent firm, quarter and 4-digit ISIC Rev.2 industry code respectively. $Treat_{is}$ equals one if firm i is a retailer, otherwise zero. Similarly, $Post_t$ equals one if the quarter falls after January 2016 and zero otherwise. The left-hand side variable $ln(Y_{its})$ is the variable of interest such as a log of quarterly revenue, costs or tax liabilities of the firms. Since I take the log of the dependent variable, the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. Only the firms with strictly positive profits are included in the analysis. I include firm fixed effect γ_i in the regressions and cluster the error terms by using 4-digit industry code. β represents the average percentage increase in reported sales, costs and liabilities of retailers in the 3-year time period after the intervention compared to wholesalers.

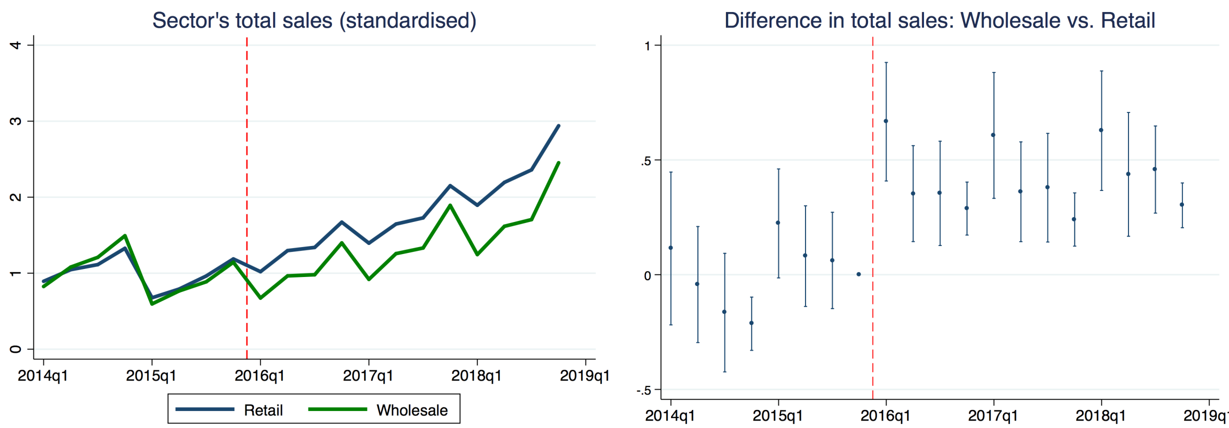
The results are presented in Table 5.[31] All regressions are weighted by firms' average

[29] The reason I divide the sums by the pre-intervention average sales is to make the visual comparison easier because wholesalers are larger in general.

[30] The graph shows spikes in quarter four each year because it plots raw aggregate sales. In Appendix A.5, I report a version of the graph where I plot aggregate sales of each sector after controlling for quarter-of-year FEs in Figure A4a. It corrects for the seasonality and still confirms the pre-trend assumption between retail and wholesale sectors.

[31] Parallel trend in costs and CIT liabilities are shown in Figure A6 in Appendix A.6.

Figure 1: Pre-trend in CIT data



(a) Sectors' standardised total sales (b) Coefficients from the flexible DiD regression

Note: Panel (a) displays the changes in the sales of retail and wholesale sectors reported on CIT returns. Each line is the total sales reported by all firms aggregated by retail or wholesale sectors scaled by the pre-intervention average quarterly sales each sector group. The graph plots the raw sales. Thus there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016. Panel (b) plots the coefficients from firm-level regression, expressed in equation 1, using CIT data.

Table 5: Direct effects - CIT returns

	(1) Sales	(2) Costs	(3) CIT
DD coef	0.198***	0.226***	0.114**
	(0.0697)	(0.0801)	(0.0536)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	202,138	202,138	202,138
Adjusted R^2	0.76	0.74	0.61

Note: This table displays the results from the regression equation 2. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The dependent variables are a log of firms' reported quarterly sales, costs or tax liabilities on CIT returns. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

quarterly sales before the intervention.[32] The dependent variables are a log of firms' reported quarterly total sales, costs or tax liabilities on CIT returns. Column 1 shows that the E-receipt program induced retailers to report 20% higher sales relative to wholesalers. However, in column 2, retailers reported an increase in costs by 22.6%. This increase in costs partially offsets the effect on CIT liabilities, and thus CIT liabilities increase by 11.4% in column 3.

As mentioned before, using wholesalers as a control group leads to an underestimation. This is because the wholesalers could be directly treated by the E-receipt program if they also sell to final consumers. Moreover, as I discuss later, there could be a spillover effect of the E-receipt program on the wholesalers via retailers. To investigate the extent of the underestimation, I run another version of DiD regression, in which I use the wholesalers that never sell to any retailers as a control group. I identify such wholesalers using the firm network data from the VAT invoices. The results are reported in Table A3. The estimated coefficients of sales and costs are above 60% suggesting a substantial underestimation.

These changes occur on retailers' CIT returns because consumers started monitoring them. To see if the program induced any real response, the number of workers and the total value of wages are analysed. The available payroll data cover the period between quarter one in 2015 to quarter three in 2018 only. Therefore, in Table 6, I report not only the changes in retailers' reported wages and workers, but also the regression results using the main variables (sales, costs, CIT) for this period. The first three columns confirm that the increase in costs offsets the increase in sales and thus leaving no significant increase in CIT liabilities. The last two columns show that retailers do not report a larger number of workers and wages after the intervention compared to wholesalers. This suggests that the increase in reported sales and costs is due to a reporting effect, and there is no actual increase in production.

Next, I focus on the increase in the reported costs. It has been documented in the literature that firms and individuals increase their reported costs on CIT returns in response to increased third-party information on firms' sales (Slemrod et al. 2017; Carrillo et al. 2017). Specifically, they tend to adjust costs that are more difficult to verify such as "other administrative costs". I study this in Table 7, where I decompose the increase in total costs into changes into its components: production, administrative and other costs. In particular, production costs contain material input costs, transportation, packaging and shipment costs, insurance costs and labour costs that are associated with production procedures. Administrative costs consist of marketing costs, travel expenses, labour costs of administrative staff etc. Other costs include non-operating costs such as interest payments, costs from currency exchange and other one-off or unusual costs. On average, production, administrative and other costs make 70%, 28% and 2% of the total costs, respectively. The last three columns in Table 7 show that the increase in total costs is mainly driven by an increase in production and administrative costs. The coefficient on other costs in column 4 is insignificant even though it is positive.[33]

[32] Table A2 in Appendix A.7 shows results from unweighted regressions, which are consistent with Table 5.

[33] Some of the firms do not classify the costs accurately and pool all their costs into one category such as production costs or administrative costs. This is the case for 30% of the sample. I do the same regression analysis by dropping those firms. The results are reported in Table A5 and they are consistent with the

Table 6: No real response by retailers

	Main variables			Real response	
	(1)	(2)	(3)	(4)	(5)
	Sales	Costs	CIT	Wages	Workers
DD coef	0.139***	0.151***	0.0181	-0.00455	0.0157
	(0.0482)	(0.0518)	(0.0275)	(0.0266)	(0.0354)
Firm FE	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry
Observations	79,610	79,610	79,610	79,610	79,610
Adjusted R^2	0.83	0.82	0.72	0.92	0.91

Note: This table displays the results from the regression equation 2. The first three columns take a log of quarterly sales, costs or tax liabilities as dependent variables. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. The payroll data covers Q1 in 2015 to Q3 in 2018 only. Therefore, less observation compared to Table 5. The dependent variables in the last two columns are log of total wages and number workers. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Decomposition of total costs reported on CIT returns

	(1)	(2)	(3)	(4)
	Total costs	Production	Admin	Other
DD coef	0.226***	0.233***	0.131***	0.213
	(0.0801)	(0.0698)	(0.0391)	(0.223)
Firm FE	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry
Observations	202,138	144,922	171,558	29,262
Adjusted R^2	0.74	0.70	0.78	0.40

Note: This table decomposes the total costs in column 1 into production, administrative and other costs, which are reported in columns 2-4. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

These results are slightly different from the findings in the existing literature mentioned

outcomes in Table 5.

above, namely, that so-called "hard to verify" other costs do not play a role in explaining total costs. Nevertheless, this does not rule out the possibility of firms artificially inflating their production and administrative costs to offset the effect of larger sales. In fact, an increase in the reported total costs could be either legitimate or illegitimate, or both. The increase is legitimate if the retailers start declaring the costs that are associated with the final sales that are disclosed by the consumers. This means the retailers used to hide both sales and costs associated with goods or services sold to the final consumers before the intervention. It is clear that retailers would have an incentive to hide their sales. However it is not straightforward to see why retailers have an incentive to hide and underreport their costs. But, if a retailer declares the purchasing costs of a good but does not report the sales, it would look suspicious to the tax authorities. Therefore, retailers are willing to underreport their costs as long as they can hide the corresponding final sales to consumers. Hiding both sales and costs associated with trading goods is beneficial to retailers. This is because they can keep the profits to themselves without paying any tax.[34] Moreover, there could be other reasons why retailers might want to suppress their reported costs. For example, they could be offered a discount by the upstream firms, the sellers. If retailers do not report their purchase on their tax returns, the sellers would not have to pay tax on those sales and transfer some of the gains to the buyer. Therefore, it can be profitable for both the seller and the buyer to hide their transactions. Alternatively, the retailers could be involved in some underground/illegal activities, selling alcohol without a license, hence hide both sales and costs from tax authorities. In all these cases, once the E-receipt program forces retailers to report their final sales, they would have an incentive to declare the previously hidden costs. Hence the increase in reported costs is legitimate.

On the other hand, the increase in reported costs is illegitimate if the retailers artificially inflate their costs to decrease the CIT liabilities. Since the E-receipt program makes it harder for firms to hide their sales they might want to substitute away from under-reporting their sales to over-reporting costs to keep their CIT liabilities small. This is feasible because the E-receipt program monitors only the sales of the firms, and not costs. Also, firms' reported costs on CIT returns are less verifiable for the tax authorities than sales.

Having a legitimate or illegitimate increase in reported costs has very different implications on the effectiveness of the E-receipt program to fight with tax evasion and increase tax revenue for the government. A genuine increase in reported costs result in the intervention (at least partially) successfully decreasing the size of the shadow economy even though the tax liability does not increase much. On the other hand, if firms increase their costs artificially, then the program is failing in its fight with tax evasion. It is easier for firms to misreport their sales and costs on CIT returns since there is no credit-invoice scheme as in VAT. Therefore, we cannot directly tell that the increase in retailers' reported costs is legitimate and should be associated with an increase in sales of upstream firms. Therefore, I investigate this further by using the audit data to shed light on the changes in retailers'

[34]Figure A3 in Appendix A.4 illustrates this using an example. A retailer buys a good from a wholesaler at a price 5 and sells it to a consumer at a price 8, generating a profit of 3. If the retailer declares both purchase and sales of the good, it has to pay at least the associated income tax. Therefore the gain of trading the good for the retailer is less than 3. If the retailer hides both its purchase and sales, then gain is 3.

misreporting behaviour.

To see the effect of the E-receipt program on the misreporting behaviour of retailers on their CIT returns I use firms' misreported sales and costs that are discovered during the operational audits. The audited data are summarised in Table 4.[35] Also, I calculate shares of misreported sales and costs in firms' true sales and costs. I define the true sales as the sum of misreported sales and reported sales, and true costs as the difference between reported costs and misreported costs. Here I implicitly assume that firms always want to under-report their sales and over-report their costs.[36] Then I run the simple DiD regression expressed in equation 2 and the results are presented in Table 8. The first (last) four columns in Table 8 analyse the misreporting of sales (costs). The dependent variables in columns 1 and 2 are the log of reported sales and calculated true sales. In column 3, I use the share of misreported sales as a dependent variable, which is the ratio between discovered hidden sales and true sales. Column 4 uses a log of the misreported sales. Similarly, in columns 5 and 6, I use a log of reported costs and calculated true costs as right-hand side variables. Columns 7 and 8 use the share and the (log of) value of misreported costs. I calculate the share of the misreported costs by dividing the value of misreported costs by true costs.

Columns 1 to 4 suggest that audited retailers misreport their sales less in the period after the intervention was initiated. As we can see from columns 1 and 2, retailers' reported sales increase more than true sales.[37] This is because retailers' tendency to under-report sales decrease after the intervention. Column 3 shows that retailers' share of misreported sales decreases by 0.7%. Column 4 shows that the value of hidden sales of retailers decreases by 1.5%, even though the coefficient is not significantly estimated.[38] On the other hand, columns 5 to 8 imply that retailers misreported their costs more after the intervention. In particular, reported costs increase by 20% and calculated true costs increase by 18%.[39] Column 7 indicates that the share of over-reported costs increases by 0.4% even though the estimated coefficient is not statistically significant. The value of misreported costs increases by 36% as shown in column 8.[40]

[35]As mentioned before, the audited firms are not chosen randomly. However, as long as the criteria to choose firms have not changed during the sample period the DiD estimation approach should estimate the effect of the program on audited firms. In contrast, the external validity of the estimated effect of the program is still questionable if the audited firms are systematically different from the general population of the firms.

[36]However, there are some evidence that firms' misreporting behaviour is not always optimal. For example, [Almunia et al., 2019] shows that 29% of firms misreport own sales and purchases such that their tax liabilities increase.

[37]However, the estimated coefficients are not statistically different from one another.

[38]This lack of statistical significance is potentially due to the smaller sample size used in Column 4. Sample size decreases in Column 4 because the dependent variable is a log of the misreported sales, and not all audited firms got caught misreporting their sales during the tax audits.

[39]However, the estimated coefficients are not statistically different from one another.

[40]Note that the number of observations used in columns 4 and 8 is smaller than the other columns. This is because some firms do not misreport sales and/or costs in some years and are dropped out because I take a log of the dependent variable. I use several other measures of misreported values sales and costs in Table A6. In particular, I use a log of one plus the misreported values and a dummy for positive misreported sales and costs. The results qualitatively confirm the fact that retailers misreport their sales less but are more likely to over-report their costs after the intervention.

Table 8: Misreporting on CIT returns (annual values)

	Sales				Costs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reported	True	Misreport (%)	Misreport ($)	Reported	True	Misreport (%)	Misreport ($)
DD coeff	0.193***	0.181***	-0.705*	-0.0151	0.199***	0.183***	0.376	0.362**
	(0.0385)	(0.0399)	(0.377)	(0.249)	(0.0397)	(0.0415)	(0.268)	(0.137)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry	Industry	Industry
Observations	14,100	14,100	14,100	2,246	14,100	14,100	14,100	3,091
Adjusted R^2	0.74	0.74	0.22	0.68	0.79	0.78	0.23	0.74

Note: This table displays the results from the regression equation 2 using CIT audit data. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The first (last) four columns analyses misreporting of sales (costs). The dependent variables in columns 1 and 2 are the log of reported sales and calculated true sales. I calculate true sales by adding misreported sales to reported annual sales. In column 3, I use the share of misreported sales as a dependent variable, which is the ratio between discovered hidden sales and true sales. Column 4 uses a log of the misreported sales. Similarly, in column 5 and 6, I use a log of reported costs and calculated true costs as right-hand side variables. True costs are calculated by subtracting misreported costs from reported annual costs. Column 7 and 8 use the share and the (log of) value of misreported costs. I calculate the share of the misreported costs by dividing the value of misreported costs by true costs. All regressions are weighted by firms' average annual sales reported on CIT returns before the intervention. Time and firm fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

These results support the hypothesis that retailers are substituting away from under-reporting sales to over-reporting costs on their CIT returns. In particular, audit data shows that retailers artificially increase their reported costs to decrease their tax liabilities since it is harder for them to hide sales due to the intervention. The estimated coefficients in columns 5 and 6 imply that at least 2% of the increase in reported costs is due to illegitimate cost over-reporting.[41] To the best of my knowledge, this is the first time direct evidence is provided showing that firms are over-reporting their costs more on CIT returns in response to increased enforcement on firms' sales.

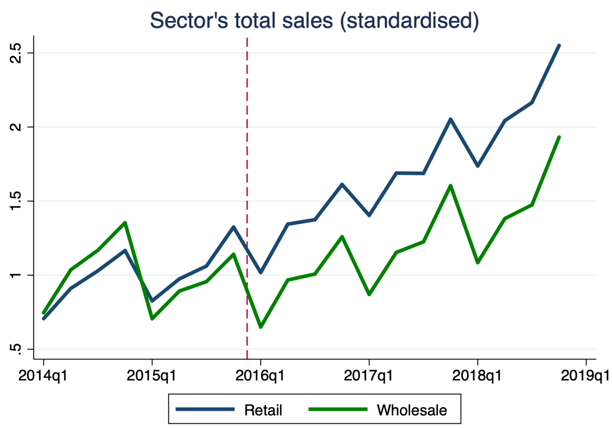
3.1.2 VAT

In this subsection, I analyse the VAT data employing the same research strategy used in the previous subsection 3.1.1. First, I establish that VAT-liable wholesalers are a valid control

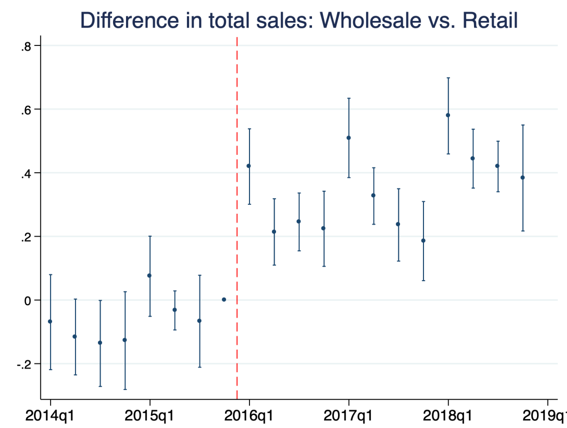
[41]It should be noted that this 2% is potentially an underestimate of the actual increase in retailers' artificial cost over-reporting. It is possible that tax audits do not reveal all misreporting. For example, tax auditors cannot identify cost misreporting due to their lack of knowledge about the business. Or tax auditors could be influenced by corruptions and collude with the audited retailers and hide their misreporting. In these cases, the calculated true costs could still be an overestimate of firms true costs. Moreover, since the operational audit data is subject to selection issues in terms of which firms get audited, these results need to be interpreted with caution.

group for VAT-liable retailers. Panel (a) in Figure 2 compares the total sales of VAT-liable retailers to VAT-liable wholesalers.[42]. The graph shows that the sales of VAT-liable retailers and wholesalers move roughly parallel to each other before the policy change, validating the no pre-trend assumption. Total sales of VAT-liable retailers start to increase more than VAT-liable wholesalers around the start of the E-receipt program.[43] The gap between the sales of retailers and wholesalers widens over time.

Figure 2: Pre-trend in VAT data



(a) Sectors' standardised total sales



(b) Coefficients from the flexible DiD regression

Note: Panel (a) displays the changes in the sales of retail and wholesale sectors reported on VAT returns. Each line is the raw sales reported by all firms aggregated by retail or wholesale sectors scaled by the pre-intervention average quarterly sales each sector group. The graph plots the raw sales. Thus there are spikes in the last quarter each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016. Panel (b) plots the coefficients from equation 1 estimated from firm-level regression using firms' reported sales on VAT returns.

Panel (b) in Figure 2 plots the estimated βs from equation 1, where the dependent variable is the log of the quarterly reported sales on firms' VAT returns. The graph shows

[42] I aggregate the sales of retailers each period and standardise it by dividing the sums by the pre-intervention mean value of the sum. I do the same thing for wholesalers and plot them over time in panel (a) in Figure 2. Since they are raw quarterly sales the lines show spikes in quarter four each year. To control for this seasonality I regress the aggregate sales on quarter-of-year FEs and analyse the residuals. The residuals are plotted in Figure A4b in Appendix A.5, and they show a similar pattern as in Figure 2 confirming the parallel trend assumption.

[43] It might seem that the divergence between the sales of retail and wholesale sectors appear in quarter one in 2015 already. One potential reason for this is the changes in the number of VAT-liable retailers compared to the number of VAT-liable wholesalers as depicted in Figure A5a in Appendix A.5. This suggests that the slight increase in the retail sales in quarter one in 2015 is partially due to adjustments at the extensive margin. A more noticeable gap between retail and wholesale sales emerges at the start of the E-receipt program in quarter one in 2016. Also, as I discuss next, analysis of firm-level sales in Panel (b) Figure 2 confirms this.

that the estimated coefficients before 2016 are not significantly different from zero, implying that the VAT-wholesalers are a valid control for VAT-retailers. The estimated βs increase and are significantly different from zero after the introduction of the E-receipt program, implying the program's differential effect on retailers' reported sales.

To see the effect on other variables such as reported costs and VAT liabilities, I run the simple DiD regression expressed in equation 2 using the VAT data. For dependent variables I use the log of quarterly total sales, purchasing costs and VAT liabilities of the firms.The results from the weighted regressions, weighted by the mean pre-intervention sales of firms, are presented in Table 9. Column 1 shows that VAT-liable retailers' reported sales increase by 42%. Even though the reported purchases increase by 38% as reported in column 2, they do not cancel out the effect on the final VAT liabilities of the retailers. VAT liabilities increased substantially, by 31%, in column 3.[44]

Table 9: Direct effects - VAT returns

	(1) Sales	(2) Purchase	(3) VAT
DD coef	0.416***	0.378***	0.312**
	(0.0380)	(0.0579)	(0.124)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	130,311	130,311	130,311
Adjusted R^2	0.74	0.73	0.62

Note: This table displays the results from the regression equation 2 using the VAT data. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The dependent variables are a log of firms' reported quarterly total sales, purchasing costs or VAT liabilities. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In comparison to the CIT analysis reported in Table 5 there is a larger effect on retailers' reported sales, purchasing costs and VAT-liabilities on VAT returns. This could be due to the following two reasons. First, the samples used in the CIT versus the VAT analysis consist of different firms. The VAT sample consists of only VAT-liable firms while the CIT sample includes both non-VAT-liable and VAT-liable firms. Second, there could be a genuinely differential effect on values reported on CIT and VAT returns. To disentangle these effects, I use the same sample of firms (only VAT-liable) for both CIT and VAT analysis.

The results are reported in Table 10.[45] The first three columns take a log of quarterly

[44] As before, to investigate the extent of underestimation, I run a DiD regression, in which I use the wholesalers that never sell to any retailers as a control group. I identify such wholesalers using the firm network data from the VAT invoices. The results are reported in Table A4. The estimated coefficients of sales, costs and VAT liabilities are above 64% suggesting a substantial underestimation.

[45] The sample size is 81,000, which is smaller than sample used in VAT analysis in Table 9. This is because

sales, total costs and CIT liabilities reported on CIT returns as dependent variables. Similarly, I use a log of quarterly sales, purchasing costs or VAT reported on VAT returns as dependent variables for the last three columns. Comparing the first three columns to the last three, I still find a larger effect on the reported sales and tax liabilities reported on VAT returns compared to CIT returns. Specifically, the reported sales on VAT returns increase more compared to CIT returns, as in columns 1 and 4. This might seem surprising because the reported sales on VAT and CIT returns should be equal to each other. However, as mentioned before, firms could be taking advantage of the fact that it is not so straightforward to compare CIT and VAT returns for tax authorities, and thus they may manipulate their reported values. The comparison of reported quarterly total sales on CIT and VAT returns are reported in Table A8 and it confirms the large discrepancy between the two reported sales.[46]

Unlike reported sales, I cannot directly compare the total costs reported on CIT returns and purchasing costs on VAT due to their different definitions. Total costs include all types of costs such as purchasing, labour and administrative costs. Unfortunately, on CIT returns, total costs are decomposed to only production, administrative and other costs.[47] Nevertheless, as shown in columns 2 and 5, the total costs reported on CIT returns increase more compared to the purchasing costs on VAT returns .[48] Moreover, VAT-liable retailers' VAT liabilities increase more (by 25%) compared to their CIT liabilities, which increase by 17%.

One potential explanation for the smaller increase in reported costs but larger increase in reported tax liabilities on VAT returns compared to CIT returns in Table 9 is the credit-invoice scheme inherent in VAT reporting. The credit-invoice scheme makes it harder for VAT-liable firms to misreport their costs (and B2B sales) on VAT returns because the values can be cross-checked with the declarations of firms' trading partners. That is, VAT-liable firms do not over-report their input costs on VAT returns, unlike CIT returns, to the extent that they offset the effect of higher reported sales.[49]To test this further I turn to the VAT

I match the VAT sample to the CIT sample, where all observations have positive VAT and CIT liabilities. An observation is dropped if, for example, a firm has positive CIT liabilities but non-positive VAT liabilities or vice versa.

[46]There are some legitimate reasons for why reported sales on CIT and VAT returns could differ. For example, there are different accounting rules such as revenue recognition rules for CIT and VAT. Unfortunately, the available data are not sufficient to separate how much of the discrepancy is due to these legitimate rules.

[47]The production costs contain not only purchasing costs but also labour, transportation and insurance costs that are associated with production procedures.

[48]I study the increase in reported purchasing costs on VAT returns further. I decompose the increase in purchases into its components: total input costs are split into the deductible and non-deductible input costs. Summary statistics are presented in Table A10 where it can be seen that deductible costs make 99% of the total input costs which equals the total purchasing costs for 90% of the sample. Nonetheless, I analyse the each component using the equation 2 and the results are presented in Table A9. The results show that both deductible and non-deductible costs increase. It is worth noting that non-deductible input costs increase more compared to deductible costs even though it does not affect firms' VAT liabilities.

[49]Of course, it is still possible that VAT firms can misreport their B2B transactions if the partner, especially the buyer, is a non-VAT firms. I discuss this further in section 3.2.

Table 10: CIT and VAT comparison

	CIT returns			VAT returns		
	(1) Sales	(2) Total costs	(3) CIT	(4) Sales	(5) Purchase	(6) VAT
DD coef	0.206***	0.256***	0.171***	0.254***	0.237***	0.249**
	(0.0548)	(0.0639)	(0.0368)	(0.0626)	(0.0810)	(0.0977)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	81,027	81,027	81,027	81,027	81,027	81,027
Adjusted R^2	0.77	0.76	0.59	0.77	0.75	0.68

Note: This table is to compare the effect of the E-receipt program on values reported on CIT and VAT returns. The first three columns take a log of quarterly sales, total costs or CIT reported on CIT returns as dependent variables. The last three columns use a log of quarterly sales, purchasing costs or VAT reported on VAT returns as dependent variables. The sample consists of only VAT-liable firms. The regression is specified in equation 2. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Time and firm fixed effects are included in all regressions. All regressions are weighted using pre-intervention average sales. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

audit data.

To examine the changes in misreporting behaviour of VAT-liable firms on their VAT returns I run the regression in equation 2. Table 11 presents the results and the first (last) four columns analyse the misreporting of sales (costs). The dependent variables in columns 1 and 2 are the log of reported sales and calculated true sales, respectively. I calculate true sales by adding misreported sales to reported annual sales. In column 3, I use the share of misreported sales as a dependent variable, which is the ratio between discovered hidden sales and true sales. Column 4 uses a log of the misreported sales. Similarly, in columns 5 and 6, I use a log of reported costs and calculated true costs as right-hand side variables. True costs are calculated by subtracting misreported costs from reported annual costs. Columns 7 and 8 use the share and (log of) value of misreported costs. I calculate the share of the misreported costs by dividing the value of misreported costs by true costs.

Similar to the CIT audit data analysis in Table 8, columns 1 to 4 in Table 11 suggest that audited retailers misreport their sales less after the intervention. In particular, column 3 shows that retailers' share of misreported sales decreases by 1.5%. Column 4 shows that the value of hidden sales of retailers decrease by 23%, even though the coefficient is not significantly estimated. Columns 5 to 8 imply that retailers misreported their costs less on their VAT returns after the intervention, unlike the case of CIT. Particularly, column 7 indicates that the share of over-reported costs decreases by 0.7%. The value of misreported costs increases by 18%, even though insignificantly estimated, as shown in column 8.[50] These

[50]Note that the number of observations used in columns 4 and 8 is smaller than the other columns. This is because some firms do not misreport sales and/or costs in some years and are dropped out because I take

Table 11: Misreporting on VAT returns (annual values)

	Sales				Costs			
	(1) Reported	(2) True	(3) Misreport (%)	(4) Misreport ($)	(5) Reported	(6) True	(7) Misreport (%)	(8) Misreport ($)
DD coeff	0.324***	0.307***	-1.513**	-0.231	0.277***	0.289***	-0.700***	-0.180
	(0.0379)	(0.0365)	(0.632)	(0.149)	(0.0435)	(0.0433)	(0.250)	(0.505)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry	Industry	Industry
Observations	11,606	11,606	11,606	2,174	11,606	11,606	11,606	1,983
Adjusted R^2	0.78	0.78	0.22	0.62	0.78	0.77	0.19	0.62

Note: This table displays the results from the regression equation 2 using VAT audit data. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The first (last) four columns analyses misreporting of sales (costs). The dependent variables in columns 1 and 2 are the log of reported sales and calculated true sales. I calculate true sales by adding misreported sales to reported annual sales. In column 3, I use the share of misreported sales as a dependent variable, which is the ratio between discovered hidden sales and true sales. Column 4 uses a log of the misreported sales. Similarly, in column 5 and 6, I use a log of reported costs and calculated true costs as right-hand side variables. True costs are calculated by subtracting misreported costs from reported annual costs. Column 7 and 8 use share and (log of) value of misreported costs. I calculate the share of the misreported costs by dividing the value of misreported costs by true costs. All regressions are weighted by firms' average annual sales reported on VAT returns before the intervention. Time and firm fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

results suggest that the combination of consumer reporting and the credit-invoice scheme in VAT makes it harder for VAT-liable firms to misreport not only sales but also their purchasing costs.

A summary of the direct effect analysis

On CIT returns, I find that retailers declare 20% higher sales after the implementation of the E-receipt program, and this is due to the pure reporting effect. I do not find any real effect on retailers' production, which is proxied by their number of workers. However, this increase in sales does not directly translate into larger CIT liabilities because of higher reported costs. I document that the increase in total reported costs is mainly due to changes in the production and administrative costs of retailers. Moreover, using CIT audit data I find that some part of the rise (at least 2%) in reported costs is due to misreporting. This is because of the combination of firms' incentive to decrease their tax liabilities and the inability of tax authorities to verify the reported costs. To the best of my knowledge, this paper is the first to document the fact that firms respond to improved sales enforcement by increasing cost misreporting on CIT returns.

a log of the dependent variable. I use several other measures of misreported values sales and costs in Table A7. In particular, I use a log of one plus the misreported values and a dummy for positive misreported sales and costs. The results qualitatively confirm the fact that retailers misreport both their sales and costs on VAT returns after the intervention.

On the other hand, on VAT returns, VAT-liable retailers' reported sales, costs and VAT liabilities all increase more than 30%. More importantly, the increase in input costs does not offset the increase in sales, and thus VAT liabilities increase by 31%. As discussed above, one of the reasons for the E-receipt program having a substantial effect on VAT liabilities compared to CIT liabilities is the existence of the credit-invoice scheme in VAT reporting.

One thing worth analysing further is the increase in reported input costs of the VAT liabilities. Because of the credit-invoice scheme, the increase in input purchase should also mean an increase in the sales of the upstream firms — the suppliers to the VAT-retailers. This can happen if the upstream firms and retailers were colluding and hiding their trade from the authorities before the intervention. The E-receipt program forces retailers to report their sales truthfully. This, in turn, will induce an incentive for retailers to increase the reported costs; hence collusion with the upstream firms may break. If this hypothesis is true, then it means that consumer monitoring — the E-receipt program — affects not only the firms at the end of the supply chain but also the upstream firms. Therefore, the whole supply chain may well be affected by the E-receipt program. I analyse this in the next section.

Lastly, it is worth noting that the above results are the lower bounds of the effects of the E-receipt program. This is because I exploit the variation in the intensity of treatment to estimate the direct effect of the program on retailers. In particular, I compare retailers' tax reporting behaviour to that of wholesalers. The implicit assumption for this identification strategy is that the wholesalers are not affected by the program. However, in reality, some wholesalers may sell to final consumers and be affected by the program directly. Also, as briefly explained above, the wholesalers could be treated by the program indirectly. Therefore, my analysis in this subsection underestimates the true direct effects of the intervention on retailers.

3.2 Indirect Effects — Upstream Firms of Retailers

This subsection explores if the E-receipt program has any effect on the upstream firms of the retailers. To identify the upstream firms, I use VAT invoice data where I observe the VAT-liable buyer-seller pairs and their volume of transactions at a quarterly frequency. I define the upstream firms as the firms that have ever sold to any retailer before the intervention. A total of 4,600 upstream firms are identified and most of them belong to trade (wholesale or retail), manufacturing and professional activities such as consulting sectors as shown in Table 2. Using these upstream firms, I estimate the spillover effect in two ways, which are transaction-level within an upstream firm analysis and firm-level between upstream firm analysis.

I start with the transaction-level within an upstream firm analysis. I adopt a DiD estimation approach where I take the upstream firms' sales to retailers as a treatment group, and their sales to buyers in non-trade sectors as a control group.[51] In essence, I compare the change in sales to the retail sector to the change in sales to other sectors within each

[51] The non-trade buyers can be, for example, manufacturing, consulting, or construction firms and are non-retail and non-wholesale buyers.

upstream firm. Sales to retailers is the treatment group because the retailer is directly monitored by the consumers. For each seller I calculate the total sales to the retail sector by aggregating the volume of transaction over retail buyers each quarter. Similarly, I compute the quarterly total sales to buyers in other sectors.[52] Using these transaction values I run the following DiD regression:

$$ln(T_{itr}) = \gamma_i + \sigma Treat_{ir} + \delta Post_t + \beta Treat_{ir} \cdot Post_t + u_{itr} \tag{3}$$

where subscripts i and t correspond to (upstream) firm and quarter as before. The subscript r represents the industry of the buyers (retail vs non-retail). Variable $Treat_{ir}$ equals one if firm i sells to retail sector r, otherwise zero.[53] $Post_t$ equals one if the quarter falls after January 2016 and zero otherwise. The left-hand side variable $ln(T_{itr})$ is the log of firm i's total sales to sector r in month t. I include firm fixed effect γ_i and the error terms are clustered at sellers' 4-digit industry level.

It is important to recall that, most of the upstream firms belong to the trade sector, either retailers or wholesalers, as shown in Table 2. And we know that retailer and wholesalers could be directly affected by the E-receipt program as discussed in the previous section. Including them in the analysis of indirect effects could contaminate the estimation of the indirect effect of the intervention. Therefore, I run several regressions for robustness by including and excluding them from the sample.

I expect the coefficient on the cross term to be positive if there is an indirect effect on the upstream firms. Table 12 presents the results. The first column uses all upstream firms regardless of their industry. In columns 2, I drop the upstream firms that are in the retail sector from the sample. The last column excludes both retailers and wholesalers from the analysis. All regressions are weighted by suppliers' average quarterly total sales before the intervention.[54]

Table 12 shows that there is a positive effect on upstream firms' sales to retailers compared to their sales to non-trade buyers. The effect increases as I exclude retailers and wholesalers from the analysis. Specifically, in column 1, sales to retailers increase by 22.4% when I use all upstream firms. The estimated coefficient increases to 33.5% when I drop retail upstream firms from the sample. In column 3, where I keep only non-trade sellers, the estimated coefficient is 40%. This means non-trade upstream firms sales to retailers increase more than their sales to other firms. The main idea behind this result is the change in retailers' incentive to collude with upstream firms due to consumer monitoring. The intuition behind these results is the change in retailers' incentive to collude with upstream firms. Because of the consumer monitoring retailers are forced to disclose their previously hidden

[52] Summary statistics of the share of sales to each group of buyers are shown in Table 3.

[53] The main difference between this equation 3 and 2 is the variable $Treat$. In equation 2 the variable $Treat_{is}$ is at firm level and equals one if a firm belongs to retail sector, zero otherwise. In equation 3 the variable $Treat_{ir}$ is at transaction level and equals one if the buyer is a retailer, zero otherwise.

[54] Table A11 presents results from unweighted regressions. Estimated coefficients are positive even though they are not significant.

Table 12: Indirect effects — Transaction-level DiD

	log(Transaction value)		
	(1)	(2)	(3)
	All sellers	Non-retail	Non-trade
DD coef	0.224**	0.335***	0.397**
	(0.113)	(0.0885)	(0.182)
Buyer Ind.FE	Yes	Yes	Yes
Seller FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	119,053	105,776	44,956
Adjusted R^2	0.63	0.63	0.63

Note: This table displays the results from the regression equation 3. The variable DD coef is defined as the interaction between a dummy for time period and a dummy that equals one if a buyer's sector is retail, zero otherwise. The dependent variables is a log of firm i's sales to sector r in quarter t. The first column uses all upstream firms regardless of their industry. In columns 2 I drop upstream firms that are retailer sector. The last column excludes both retailers and wholesalers from the analysis. All regressions are weighted by suppliers' average quarterly total sales before the intervention. Time and supplier fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

sales. This will induce retailers to break the collusion and report larger costs to decrease their tax liabilities.

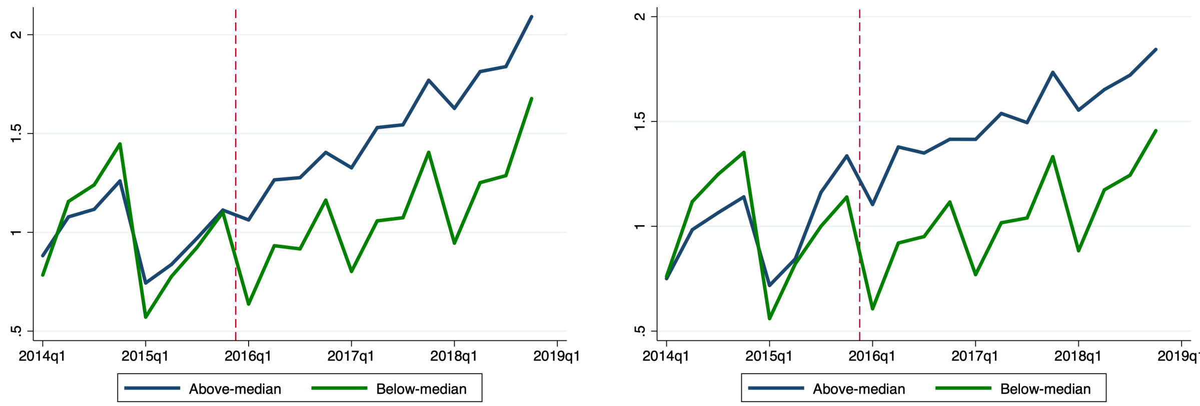
Next, I estimate the firm-level indirect effect on upstream firms. To do so, I rank the upstream firms and divide them into two groups based on their share of sales to retailers before the intervention. The firms, whose share of sales to retailers is above the median are classified as a treatment group and firms below the median are used as a control group.

Since the analysis is at the firm-level, I can examine whether the parallel-trend assumption holds. As before, for each quarter, I aggregate the reported sales on CIT returns of the upstream firms in the treatment group and standardise it by dividing the sums by the pre-intervention mean value of the sums. I do the same for the firms in the control group and plot them over time in panel (a) in Figure 3. Similarly, panel (b) plots the aggregate sales reported on VAT returns for each group. As we can see from the plots, there is no pre-trend before the policy change, but total sales of the treatment group start to increase more compared to the control group in 2016. The gap between them starts widening over time, and I attribute this divergence to the E-receipt program.[55]

Then, I run firm-level DiD regressions specified in equation 4 to examine the changes in reported sales further.

[55] Figure A9 plots the same graphs using CIT and VAT liabilities and it confirms the parallel-trend assumption as well.

Figure 3: Pre-trend in upstream firms' sales



(a) Sales on CIT returns

(b) Sales on VAT returns

Note: Panel a (b) displays the changes in the total sales of the upstream firms in treatment and control groups reported on CIT (VAT) returns. Each line is the sum of sales reported by firms in the treatment or control groups scaled by the pre-intervention average quarterly sales of each group. The graph plots the raw sales. Thus there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

$$ln(Y_{itR}) = \gamma_i + \sigma Treat_{iR} + \delta Post_t + \beta Treat_{iR} \cdot Post_t + u_{itR} \qquad (4)$$

where subscripts i and t correspond to firm and quarter as before. The variable $Treat_{iR}$ takes one if firm i is above the median in terms of its volume of sales to retailers pre-intervention, zero otherwise. $Post_t$ equals one if the quarter falls after January 2016 and zero otherwise. The left-hand side variable $ln(Y_{it})$ is the log of firm i's quarterly total sales in month t. I include firm fixed effect γ_i and the error terms are clustered at sellers' 4-digit industry level.

Table 13 presents the results. The first (last) three columns correspond to changes in sales reported on CIT (VAT) returns. Columns 1 and 4 include all upstream firms in the analysis regardless of their industry. In columns 2 and 5, I drop upstream firms that are in the retail sector. Columns 3 and 5 exclude both retailers and wholesalers from the analysis. All regressions are weighted by suppliers' average quarterly total sales before the intervention. Columns 1, 2, 3 show that reported sales of upstream firms with above-median sales to retailers increase by 26% compared to those who sell less to retailers. In contrast, there is a larger effect on reported sales on VAT. Specifically, upstream firms' reported sales on VAT returns increase by at least 30%. However, it is documented that this increase in reported sales does not necessarily lead to larger tax liabilities, especially for CIT. To see this, I study the changes in the upstream firms' tax liabilities. Table 14 shows the results. As shown in columns 1-3, there is no significant effect on CIT liabilities. By contrast, VAT liabilities increase at least by 15% in columns 4-6. It is worth noting that these estimates are the lower bound of the true indirect effect since the control group is affected by the program

Table 13: Indirect effects — Firm-level DiD — Sales

	Sales on CIT returns			Sales on VAT returns		
	(1)	(2)	(3)	(4)	(5)	(6)
	All sellers	Non-retail	Non-trade	All sellers	Non-retail	Non-trade
DD coeff	0.258***	0.265***	0.257***	0.298***	0.315***	0.351***
	(0.0431)	(0.0399)	(0.0956)	(0.0616)	(0.0559)	(0.134)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Weight	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	69,314	61,658	26,349	69,314	61,658	26,349
Adjusted R^2	0.64	0.63	0.65	0.64	0.63	0.64

Note: This table displays the results from the regression equation 4. The variable DD coef is defined as the interaction between a period (pre- and post-intervention) dummy and a dummy variable, which equals one if firm i is above the median in terms of its volume of sales to retailers pre-intervention, zero otherwise. The dependent variables in the firms three columns are a log of firm i's quarterly total sales reported on CIT returns. The last three columns use reported sales on VAT returns as a dependent variable. Columns 1 and 4 include all upstream firms in the analysis regardless of their industry. In columns 2 and 5, I drop upstream firms that are retailer sector. Columns 3 and 5 exclude both retailers and wholesalers from the analysis. All regressions are weighted by suppliers' average quarterly total sales before the intervention. Period and supplier fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to some degree. This is because sales of firms with lower rank are expected to increase to some degree since they also sell to retailers.

These results suggest that the E-receipt program has a positive effect on upstream firms along the VAT chain for the following reasons: first, retailers are forced to report their sales truthfully and increase their reported sales because of consumer monitoring. To decrease VAT liabilities they increase their reported purchasing costs. This, in turn, is likely to result in less collusion between retailers and their upstream firms and reveal previously hidden transactions between them. In other words, because of the credit-invoice mechanism in VAT, the increase in retailers' reported purchasing costs has to be associated with a rise in upstream firms' sales and VAT liabilities. Hence, consumer monitoring does not only affect the firms at the end of the supply chain, the retailers, as documented in the literature; rather, its effects propagate up the VAT chain. Therefore, the total impact on the economy is larger than previously thought.

4 Cost-benefit Analysis

In this section, I show a simple cost-benefit analysis of the E-receipt program. To implement the program, the government had to bear some costs and it is still not clear whether the

Table 14: Indirect effects — Firm-level DiD — Tax liabilities

	CIT liabilities			VAT liabilities		
	(1) All sellers	(2) Non-retail	(3) Non-trade	(4) All sellers	(5) Non-retail	(6) Non-trade
DD coeff	0.0270	0.0172	-0.0232	0.179***	0.151***	0.1771***
	(0.0395)	(0.0424)	(0.0910)	(0.0315)	(0.0312)	(0.0448)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Weight	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	46,727	40,718	16,986	51,741	46,375	20,611
Adjusted R^2	0.54	0.53	0.54	0.67	0.65	0.68

Note: This table displays the results from the regression equation 4. The variable DD coef is defined as the interaction between a period (pre- and post-intervention) dummy and a dummy variable, which equals one if firm i is above the median in terms of its volume of sales to retailers pre-intervention, zero otherwise. The dependent variables in the firms three columns are a log of firm i's quarterly CIT liabilities. The last three columns use reported VAT liabilities as a dependent variable. Columns 1 and 4 include all upstream firms in the analysis regardless of their industry. In columns 2 and 5, I drop upstream firms that are retailer sector. Columns 3 and 5 exclude both retailers and wholesalers from the analysis. All regressions are weighted by suppliers' average quarterly total sales before the intervention. Period and supplier fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

program leads to an increase in tax revenue. First of all, it promises consumers 20% of the VAT paid on their purchases. Second, a lottery event is held every month and the lottery prizes get transferred to the winners' bank account every month. The total money spent on lottery prizes corresponds to 13.7% of the total VAT rebate costs. Lastly, there are other costs associated with developing an IT system, preparing the infrastructure of the E-receipt database, wage salaries of the IT workers, etc. These administrative costs account for 2.9% of the total VAT rebate costs. These cost estimates correspond to the Mongolian economy as a whole. Unfortunately, the portion of the costs that correspond to the trade sector is unknown. Therefore, I assume the same pattern holds for the trade sector.

To see whether the program pays off, I calculate the percentage increase in VAT revenue to break even. I define $VAT\,Rev_0$ as the VAT revenue of the trade sector in the absence of the E-receipt program. VAT revenue after implementing the program is denoted by $VAT\ Rev_1$.

$$VAT\ Rev_0 = VAT\ Rev_1 * (1 - \underbrace{0.2}_{\text{rebate}} - (\underbrace{0.137}_{\text{lottery}} + \underbrace{0.029}_{\text{admin}}) * 0.2)$$

$$\frac{VAT\ Rev_1}{VAT\ Rev_0} - 1 = 0.304$$

The above calculation shows that a 30.4% increase in VAT payment will generate the same VAT revenue for the government net of the costs. As we have seen in Section 3.1, retailers' VAT liability increased by 31.2%, which is just enough to break even. Therefore, the previous literature significantly underestimates the effects of the consumer monitoring program since it does not consider the spillover effect on retailers' CIT liabilities as well as the indirect effect of the program on the upstream firms' VAT liabilities. If we include them in the calculation, the program is successful for increasing the government's tax revenue.

It is worth noting that I did not include firms' and consumers' compliance costs into the calculation. As discussed before, the compliance cost for consumers is negligible because it is possible to register a receipt as long as consumers have a cell phone. In contrast, there is a higher cost for firms since some firms have to update or buy a new registry system. Unfortunately estimates of such costs do not exist.[56] Furthermore, there are other intangible aspects of the program in terms of both benefits and costs. As for the former, the program may change societal norms that have long-lasting effects even after the program ends. These changes include people getting used to asking for receipts, an increase in tax awareness, greater attention to the public expenditure and demand for more efficient public spending and so on. On the other hand, the program increases the tax burden of the firms and thus could increase the efficiency costs of the CIT and VAT. Moreover, I do not study any changes in tax incidence or transfer of the tax burden. Even though these are interesting and important aspects of tax enforcement they are beyond the scope of this project.

5 Conclusion

This paper studies the role of consumer monitoring on firms' tax reporting behaviour along the supply chain. To do so, I exploit rich administrative tax data and an anti-tax evasion program implemented by the Mongolian government that incentivises consumers to report their transactions.

I start by studying the effect of the program on tax reporting behaviour of firms at the end of the supply chain — retailers. Retailers mainly sell to final consumers, and thus they are directly affected by the program. I document that consumer monitoring increases retailers' reported sales on their CIT returns by 20%. However, the effect of larger sales is partially offset by over-reporting costs. I confirm this by using tax audit data that suggest a large increase in reported costs on CIT returns is partly explained by cost misreporting. In other words, because of the consumer monitoring firms find it harder to misreport their sales and thus substitute away from under-reporting sales to over-reporting costs to decrease their CIT liabilities. Thus, I find retailers' CIT liabilities increase by 11%. On the other hand, I find a stronger effect on retailers' VAT liabilities, which increase by 31%. This is because retailers' reported costs on VAT returns are constrained by the declarations of suppliers hence they are not freely adjusted. These results suggest that different opportunities for

[56] However, such costs should be reflected in firms' CIT liabilities and I find that retailers' CIT liabilities increase by 11%. In that sense, compliance costs for firms are reflected in my analysis.

cost adjustment faced by firms in CIT and VAT ultimately lead to the different effects of consumer monitoring on their CIT and VAT liabilities.

Next, I examine how the effects of consumer monitoring propagate through the firm network. Because of the self-enforcing mechanism in B2B trade in VAT, any increase in reported input costs should be associated with an increase in upstream firms' sales. Accordingly, I find that upstream firms that sell to retailers increase their VAT liabilities by 17%. In contrast, I do not find any significant effect on their CIT liabilities. These results highlight the enforcement advantage of VAT compared to CIT and suggest that consumer monitoring enhances the self-enforcement mechanism in VAT. At the same time, it also highlights the fact that the credit-invoice system in B2B trade is not a silver bullet. This is because the self-enforcing mechanism breaks down at the end of the supply chain since consumers do not usually report their purchase. This creates opportunities for firms to evade VAT along the supply chain by, for example, colluding with one another. Therefore, it is important to include the final consumers into VAT reporting and thus ensuring better enforcement along the whole supply chain.

Taking together the effects of consumer monitoring on downstream and upstream firms, the economy-wide impact of the policy is larger than previously found in the literature.

References

Miguel Almunia and David Lopez-Rodriguez. Under the radar: The effects of monitoring firms on tax compliance. *American Economic Journal: Economic Policy*, 10(1):1–38, 2018. ISSN 1945774X. doi: 10.1257/pol.20160229.

Miguel Almunia, François Gerard, Jonas Hjort, Justine Knebelmann, Dorothy Nakyambadde, Claude Raisaro, and Lin Tian. An Analysis of Discrepancies in Tax Declarations Submitted Under Value-Added Tax in Uganda. *IGC report*, (February):36, 2017.

Miguel Almunia, Jonas Hjort, Justine Knebelmann, and Lin Tian. Strategic or Confused Firms? Evidence from " Missing " Transactions in Uganda. 2019.

Annette Alstadsæter, Niels Johannesen, and Gabriel Zucman. Tax evasion and inequality. *American Economic Review*, 109(6):2073–2103, 2019. ISSN 19447981. doi: 10.1257/aer.20172043.

Marcelo Arbex and Enlinson Mattos. Optimal sales tax rebates and tax enforcement consumers. *Oxford Economic Papers*, 67(2):479–493, 2015. ISSN 14643812. doi: 10.1093/oep/gpu029.

Pierre Bachas and Mauricio Soto. Not(ch) Your Average Tax System : Corporate Taxation Under Weak Enforcement. pages 1–56, 2015.

M. Chatib Basri, Mayara Felix, Rema Hanna, and Benjamin Olken. Tax Administration vs. Tax Rates: Evidence from Corporate Taxation in Indonesia. 2020. ISSN 0898-2937.

Youssef Benzarti, Dorian Carloni, and France Youssef Benzarti. Who Really Benefits from Consumption Tax Cuts? Evidence from a Large VAT Reform in. 2017a. URL http://www.nber.org/papers/w23848.

Youssef Benzarti, Dorian Carloni, Jarkko Harju, and Tuomas Kosonen. What Goes Up May Not Come Down: Asymmetric Incidence of Value-Added Taxes. *National Bureau of Economic Research*, 2017b. ISSN 0898-2937. doi: 10.3386/w23849.

Timothy Besley and Torsten Persson. *Pillars of Prosperity*. 2011. ISBN 9780691152684. doi: 10.23943/princeton/9780691152684.001.0001.

Timothy Besley and Torsten Persson. Taxation and Development. *Handbook of Public Economics*, 5(December 2011):51–110, 2013. ISSN 15734420. doi: 10.1016/B978-0-444-53759-1.00002-9.

Timothy Besley and Torsten Persson. Why do developing countries tax so little? *Journal of Economic Perspectives*, 28(4):99–120, 2014. ISSN 08953309. doi: 10.1257/jep.28.4.99.

Paul Carrillo, Dina Pomeranz, and Monica Singha. Dodging the taxman: Firm misreporting and limits to tax enforcement. *American Economic Journal: Applied Economics*, 9(2):144–164, apr 2017. ISSN 19457790. doi: 10.1257/app.20140495.

Áureo de Paula and José A. Scheinkman. Value - Added Taxes, Chain Effects, and Informality. *American Economic Journal: Macroeconomics*, 2(October):195–221, 2010.

Abel Embaye. Underground economy estimates for non-OECD countries using currency demand method , 1984-2005. *MPRA Paper No. 20308*, 2007.

Roger Gordon and Wei Li. Tax structures in developing countries: Many puzzles and a possible explanation. *Journal of Public Economics*, 93(7-8):855–866, 2009. ISSN 00472727. doi: 10.1016/j.jpubeco.2009.04.001. URL http://dx.doi.org/10.1016/j.jpubeco.2009.04.001.

Jarkko Harju, Tuomas Matikka, and Timo Rauhanen. The Effect of VAT Threshold on the Behavior of Small Businesses: Evidence and Implications. Technical report, 2015.

Jarkko Harju, Tuomas Matikka, and Timo Rauhanen. Compliance costs vs. tax incentives: Why do entrepreneurs respond to size-based regulations? *Journal of Public Economics*, 173:139–164, 2019. ISSN 00472727. doi: 10.1016/j.jpubeco.2019.02.003.

Jensen. Employment structure and the rise of the modern tax system. *NBER Working Paper*, 2019. ISSN 1098-6596. doi: 10.1017/CBO9781107415324.004.

Andrew Johns and Joel Slemrod. The distribution of income tax noncompliance. *National Tax Journal*, 63(3):397–418, 2010. ISSN 00280283. doi: 10.17310/ntj.2010.3.01.

Michael Keen. What Do (and Don't) We Know about the Value Added Tax ? A Review of Richard M. Bird and Pierre-Pascal Gendron's The VAT in Developing and Transitional Countries. *Journal of Economics Literature*, 47(1):159–170, 2009. ISSN 0022-0515. doi: 10.1257/jel.47.1.159.

Michael Keen. The Anatomy of the VAT; by Michael Keen; IMF Working Paper 13/111; May 1, 2013. Technical report, 2013.

Michael Keen and Ben Lockwood. IMF Working Paper- The Value-Added Tax: Its Causes and Consequences. pages 1–36, 2007.

Michael Keen and Joel Slemrod. Optimal tax administration. *Journal of Public Economics*, 152:133–142, 2017. ISSN 00472727. doi: 10.1016/j.jpubeco.2017.04.006. URL http://dx.doi.org/10.1016/j.jpubeco.2017.04.006.

Henrik Jacobsen Kleven, Martin Knudsen, Claus Thustrup Kreiner, Soren Pedersen, and Emmanuel Saez. Unwilling or Unable to Cheat? Evidence From a Tax Audit Experiment in Denmark. *Econometrica*, 79(3):651–692, 2011. ISSN 0012-9682. doi: 10.3982/ecta9113.

Henrik Jacobsen Kleven, Claus Thustrup Kreiner, and Emmanuel Saez. Why Can Modern Governments Tax So Much? An Agency Model of Firms as Fiscal Intermediaries. *Economica*, 83(330):219–246, 2016. ISSN 14680335. doi: 10.1111/ecca.12182.

Wojciech Kopczuk and Joel Slemrod. Putting Firms into Optimal Tax Theory. *AEA PAPERS AND PROCEEDINGS*, 2006.

Adriana Veronica Litra. The Underground Economy in Romania. *Ovidius University Annals, Economic Sciences Series*, XVIII(2):173–176, 2018.

Joana Naritomi. Consumers as Tax Auditors. *American Economic Review*, 109(9):3031–3072, 2019.

ONS. Multi-factor Productivity, Indicative Estimates to 2012. (January):1–20, 2014.

Dina Pomeranz. No taxation without information: Deterrence and self-enforcement in the value added tax. *American Economic Review*, 105(8):2539–2569, 2015. ISSN 00028282. doi: 10.1257/aer.20130393.

Dina Pomeranz and Joseacute Vila-Belda. Taking State-Capacity Research to the Field: Insights from Collaborations with Tax Authorities. *Annual Review of Economics*, 11: 755–781, 2019. ISSN 19411391. doi: 10.1146/annurev-economics-080218-030312.

Friedrich Schneider. The shadow economy and tax evasion: What do we (not) know? *CESifo Forum*, 13(2):3–12, 2012. ISSN 1615245X.

Friedrich Schneider, Andreas Buehn, and Claudio E. Montenegro. Shadow Economies All Over the World: New Estimates for 162 Countries from 1999 to 2007. *Policy Research Working Paper 5356, World Bank*, 2010. doi: 10.4337/9780857930880.00007.

Joel Slemrod. Cheating ourselves: The economics of tax evasion. *Journal of Economic Perspectives*, 21(1):25–48, 2007. ISSN 08953309. doi: 10.1257/jep.21.1.25.

Joel Slemrod. Does it matter who writes the check to the government? The economics of tax remittance. *National Tax Journal*, 61(2):251–275, 2008. ISSN 00280283. doi: 10.17310/ntj.2008.2.05.

Joel Slemrod. Tax compliance and enforcement†. *Journal of Economic Literature*, 57(4): 904–954, 2019. ISSN 00220515. doi: 10.1257/jel.20181437.

Joel Slemrod and Shlomo Yitzhaki. Tax avoidance, evasion, and administration. *Handbook of Public Economics*, 3(2):201–202, 2002.

Joel Slemrod, Brett Collins, Jeffrey L. Hoopes, Daniel Reck, and Michael Sebastiani. Does credit-card information reporting improve small-business tax compliance? *Journal of Public Economics*, 149:1–19, 2017. ISSN 00472727. doi: 10.1016/j.jpubeco.2017.02.010. URL http://dx.doi.org/10.1016/j.jpubeco.2017.02.010.

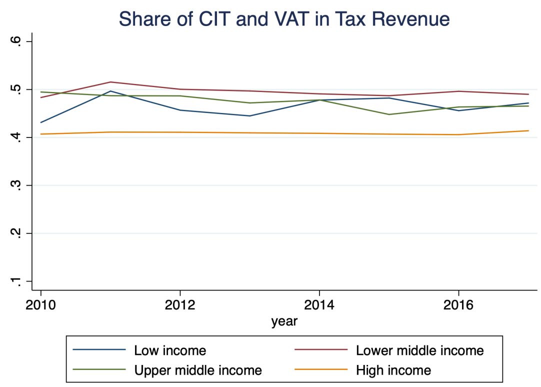
Joel B. Slemrod and Tejaswi Velayudhan. Do Firms Remit at Least 85 Percent of Tax Everywhere? *SSRN Electronic Journal*, 2017. doi: 10.2139/ssrn.2970229.

Appendix A

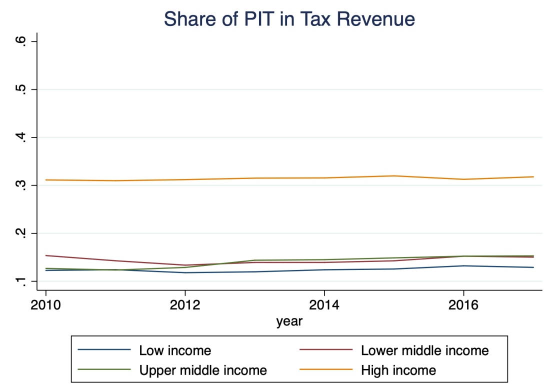
A.1 Share of CIT and VAT in Tax Revenue Across Countries

Figure A1 depicts the composition of total tax revenue across a group of countries: low, lower-middle, upper-middle and high-income countries. The sample data consist of 115 countries, of which 42 are high-income, 38 are upper-middle, 20 are lower-middle and 15 are low-income.[57]

Figure A1: Composition of tax revenue



(a) Share of CIT and VAT



(b) Share of PIT

Subfigure A1a shows that CIT and VAT constitute around 40% of total tax revenue in high-income countries. It is slightly higher — 47% — for low and middle-income countries. On the other hand, Subfigure A1b shows that personal income tax (PIT) make 30% of tax revenue for developed countries. The share of PIT for low and middle-income countries is around 15% of tax revenue, which is much lower compared to that of high-income countries. Therefore, CIT and VAT together make the largest share of tax revenue, especially in low and middle-income countries.

[57] Data sources are IMF Macroeconomic and financial data (https://data.imf.org/) and WorldBank open data (https://data.worldbank.org/)

A.2 Misreporting on CIT vs VAT Returns for VAT-liable Firms

Audited VAT-liable firms and their misreporting behaviour is summarised in Table A1. In particular, summary statistics of misreported values on the CIT audit data is reported in Table A1a and VAT audit data in Table A1b. They show that VAT-liable firms are more likely to under-report their sales and less likely to over-report their costs on VAT returns compared to CIT returns. A plausible explanation for this observation is the existence of the credit-invoice scheme in VAT. For VAT, firms reported purchasing costs are constrained by suppliers' declaration and hence it is harder to over-report costs on VAT returns.

Table A1: Summary statistics - Audit data for VAT-liable firms

(a) CIT returns

	mean	sd	count
Retailers			
Under.rep.sales ($)	0.70	5.25	3,723
share (%)	1.60	8.48	3,723
Over.rep.costs ($)	1.15	13.36	3,723
share (%)	0.88	4.30	3,723
Wholesalers			
Under.rep.sales ($)	4.04	31.32	7,636
share (%)	2.18	13.18	7,636
Over.rep.costs ($)	5.29	28.76	7,636
share (%)	1.63	6.54	7,636

(b) VAT returns

	mean	sd	count
Retailers			
Under.rep.sales ($)	1.80	15.66	3,738
share (%)	3.98	20.62	3,738
Over.rep.costs ($)	0.64	9.00	3,738
share (%)	0.63	4.79	3,738
Wholesalers			
Under.rep.sales ($)	4.33	29.73	7,621
share (%)	4.06	23.99	7,621
Over.rep.costs ($)	3.78	40.29	7,621
share (%)	2.04	9.23	7,621

Note: Table A1a and A1b present summary statistics of CIT and VAT audit data, respectively. Specifically, it summarises (annual) values of under-reported sales and their share in firms' true sales on each type of tax returns. The true sales are calculated as the sum of reported sales and hidden sales. Similarly, it provides summary statistics of (annual) values of over-reported costs, and their share in true costs in each type of tax return. The true costs are calculated as the difference between reported costs and the over-reported costs. All nominal values are in thousand USD (1 MNT = 2600 USD).

A.3 Firms Enrollment in E-receipt Program

Figure A2: Share of retailers issuing E-receipts

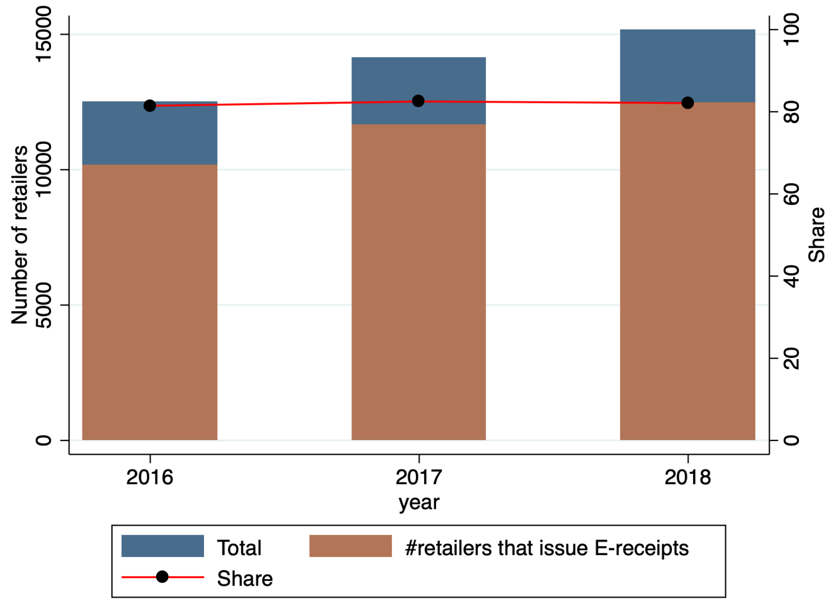


Figure A2 shows the total number of retailers that submit corporate income tax as well as the number and share of retailers that issue E-receipts. It illustrates the gradual enrollment of the retailers: the share of retailers that were enrolled in the E-receipt program was 51% in 2016 and increased to 56% in 2018.

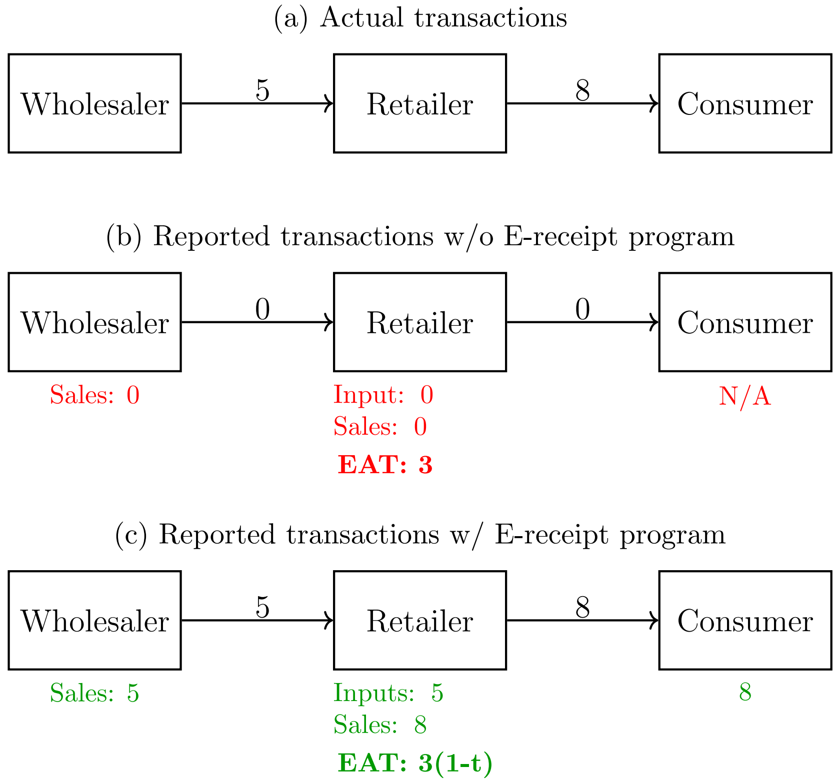
A.4 Retailers' incentive to collude with upstream firms

In this section I explain why the effects of the E-receipt program propagate up the supply chain. To do so, I use a simple illustration of a supply chain, where I assume a retailer buys a good from a wholesaler at a price 5, and sells it to a consumer at a price 8 as illustrated in Figure A3a.

In Figure A3b I show how firms report these transactions in the absence of the E-receipt program. Since the consumer does not report their purchase to tax authorities the retailer can hide its final sales of 8. However, if the retailer declares the associated purchasing costs of 5 but not sales, it might send a red signal to the authorities. Therefore, the retailer potentially has an incentive to collude with the wholesaler and hide its purchasing costs of 5. Such misreporting of sales and costs allows the retailer to obtain the profits of 3 without paying any taxes. On the other hand, it is profitable for the wholesaler to hide its sales of 5, which leads to less tax liabilities. Also, the wholesaler may collude with its upstream firms/suppliers. Hence such collusion can happen along the whole supply chain.

One E-receipt program is in place, the consumer start reporting the purchase of 8, this

Figure A3: Collusion along supply chain



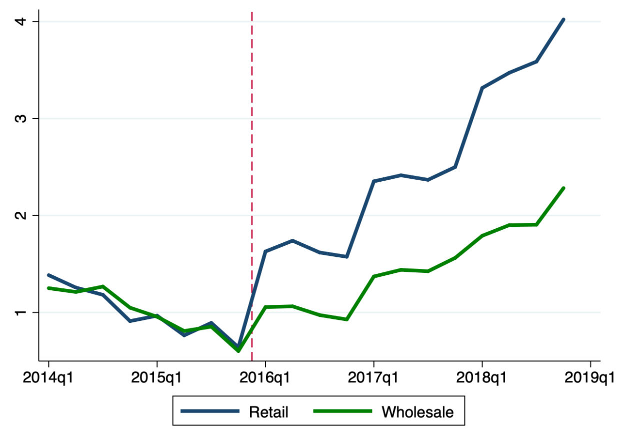
forces the retailer to declare its final sales. This, in turn, leads to a break of the collusion between the retailer and wholesaler as the retailer now has an incentive to declare the purchasing costs of 5. The sales and costs reported by the firms are illustrated in Figure A3c. In this case, the retailer has to pay taxes, thus its earnings after tax is $3(1-t)$.

Moreover, there could be other reasons why retailers might want to collude with their upstream firms. For example, upstream firms could offer a discount if they agree to hide their trade. If retailers do not report their purchase on their tax returns, the sellers would not have to pay tax on those sales and transfer some of the gains to the retailers. Therefore, it can be profitable for both upstream firms and retailers to hide their transactions. Alternatively, the firms could be involved in some underground/illegal activities, selling alcohol without a license, hence have an incentive to collude and hide their transactions from tax authorities.

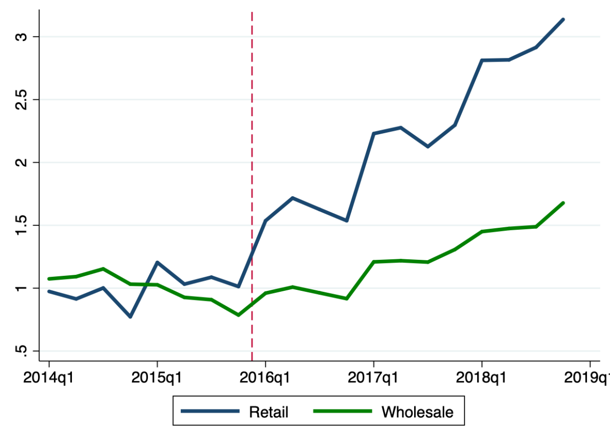
A.5 Pre-trend assumption in reported sales after quarter fixed effects

In the main text I plot sector-level sales of wholesalers and retailers using firms' CIT (VAT) returns in Figure 1 (Figure 2) to see if the wholesalers are a valid control group. However, since they are the aggregates of raw reported sales of each firm, they exhibit spikes in quarter four each year. To control for this seasonality I regress the aggregate sales on quarter-of-year FEs and analyse the residuals. Figure A4a and Figure A4b show the CIT and VAT residuals, respectively. They are consistent with the no pre-trend assumption in the reported sales on CIT and VAT returns.

Figure A4: Pre-trend in sector-level sales, correcting for seasonality



(a) Sales reported on CIT returns

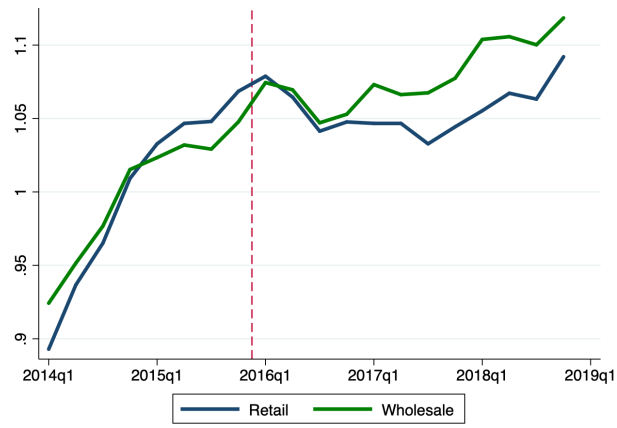


(b) Sales reported on VAT returns

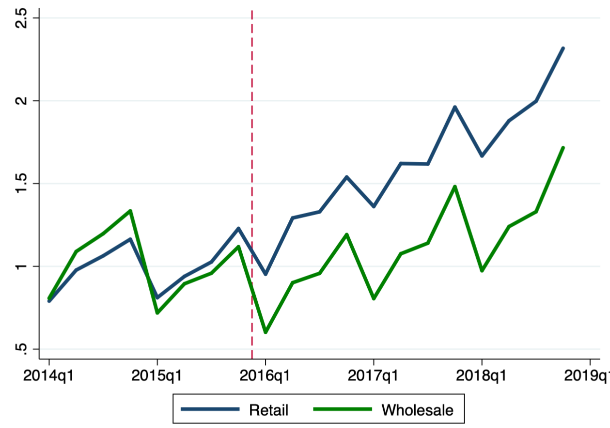
Note: Panel (a) displays the changes in the sector-level sales of retailers and wholesalers reported on CIT returns after controlling for quarter-of-year fixed effect. In other words, each line plots the residuals after regressing industry-level sales of retailers and wholesalers on quarter fixed effects. Similarly, Panel (b) displays the residuals using reported sales on VAT returns by retailers and wholesalers. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

One might think that the divergence between retailers and wholesalers' reported sales appear in quarter one in 2015. This is especially visible for reported sales on VAT returns in Figure 2 and Figure A4b. However, as depicted in Figure A5a, number of VAT-liable retailers increase more compared to number of VAT-liable wholesalers in quarter one in 2015. This suggests that the slight increase in the retail sales in quarter one in 2015 is partially due to adjustments at the extensive margin, not due to the changes in firm-level sales. Once I plot the average reported sales of each sector, which is the ratio between the aggregate sales divided by the number of firms, in Figure A5b, such early divergence is not as apparent as before in Figure 2.

Figure A5: Number of VAT-liable firms and average sales in each sector



(a) Number of VAT-liable firms



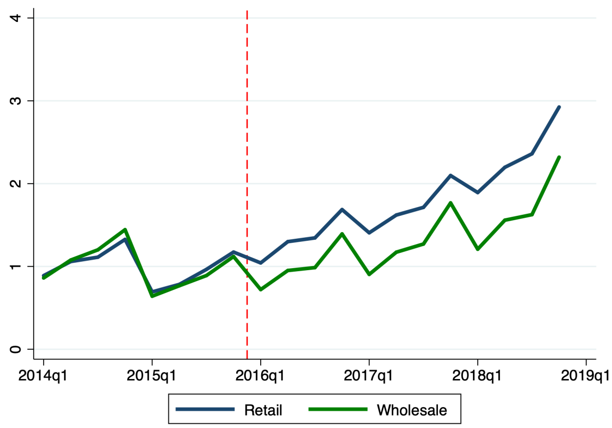
(b) Average sales reported on VAT returns

Note: Panel (a) displays the changes in the number of VAT-liable retailers and wholesalers, scaled by the pre-intervention average number of retailers and wholesalers. Panel (b) shows the average sales of VAT-liable retailers and wholesalers reported on VAT returns. Average sales in each sector are calculated by aggregating reported sales of the firms and dividing the sum by the number of firms. The average sales are also standardised by dividing by the mean pre-intervention average sales of each sector. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

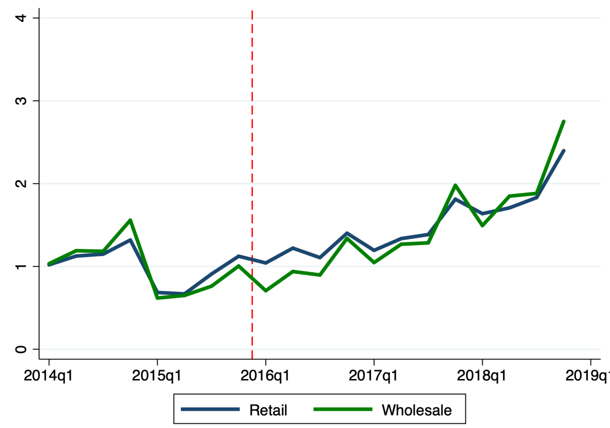
A.6 Pre-trend assumption in terms of reported costs and tax liabilities

In Figure 1 and 2 I show the parallel trend assumption holds in terms of firms' reported sales on CIT and VAT returns. In Figure A6 I do the same thing using firms' total costs and CIT liabilities reported on CIT returns in panels (a) and (b), and total purchasing costs and VAT liabilities from firms' VAT returns in panels (c) and (d). In particular, panel (a) plots the aggregate total costs reported on CIT returns of all retailers and wholesalers scaled by the pre-intervention average quarterly costs each sector group. Panel (b) shows the CIT liabilities of retailers and wholesalers standardised in the same way using pre-intervention average CIT liabilities. It shows that there is no pre-trend before January 2016, but total costs of retailers start to increase more compared to wholesalers after 2016. Panel (b) plots the standardised sector-level CIT liabilities of retail and wholesale sector. It also confirms the parallel trend assumption, but it exhibits only a short-lived larger effect on retailers' CIT liabilities. Panel (c) displays total purchasing costs of retailers and wholesalers divided by pre-2016 average sector-level purchasing costs, and confirms there is no-pre trend. Lastly, panel (d) shows sector-level VAT liabilities reported by retailers and wholesalers, and it confirms the parallel trend assumption as well.

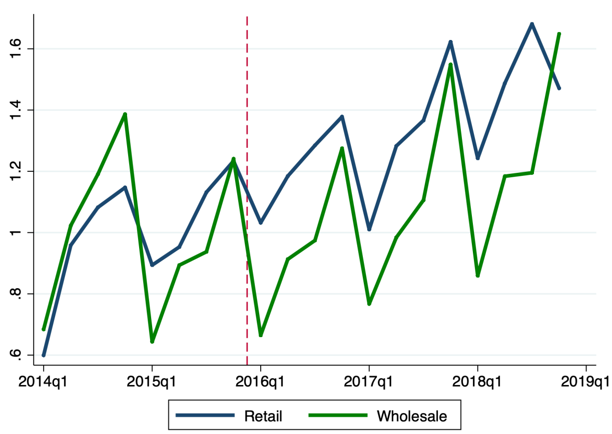
Figure A6: Pre-trend in CIT and VAT data



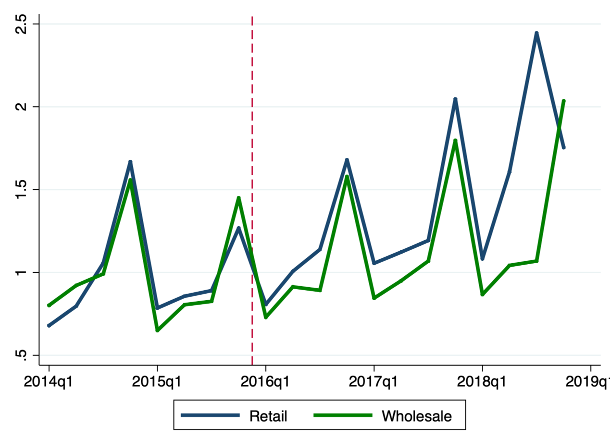
(a) Sectors' standardised total costs - CIT data



(b) Sectors' standardised CIT liabilities



(c) Sectors' standardised purchase - VAT data



(d) Sectors' standardised VAT liabilities

Note: In panel (a) and (b) I use data from firms' CIT returns. They display sector-level total costs and CIT liabilities reported by retailers and wholesalers on their CIT returns. In particular, panel (a) shows total costs reported by all firms aggregated by retail or wholesale sectors scaled by the pre-intervention average quarterly costs each sector group. Panel (b) shows the CIT liabilities of retailers and wholesalers standardised in the same way using pre-intervention average CIT liabilities. Panel (c) and (d) use data from firms' VAT returns. In panel (c) I plot standardised total purchasing costs reported by retailers and wholesalers on their VAT returns. Panel (d) shows the standardised VAT liabilities of retailers and wholesalers. The graphs plot the raw data, hence there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

Table A2: Direct effect - CIT returns (unweighted)

	Main variables			Cost decomposition		
	(1) Sales	(2) Costs	(3) CIT	(4) Production	(5) Admin	(6) Other
DD coef	0.147***	0.178***	0.00894	0.216***	0.0403*	0.131
	(0.0473)	(0.0519)	(0.0185)	(0.0403)	(0.0210)	(0.104)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	202,138	202,138	202,138	144,922	171,558	29,262
Adjusted R^2	0.83	0.81	0.69	0.79	0.79	0.59

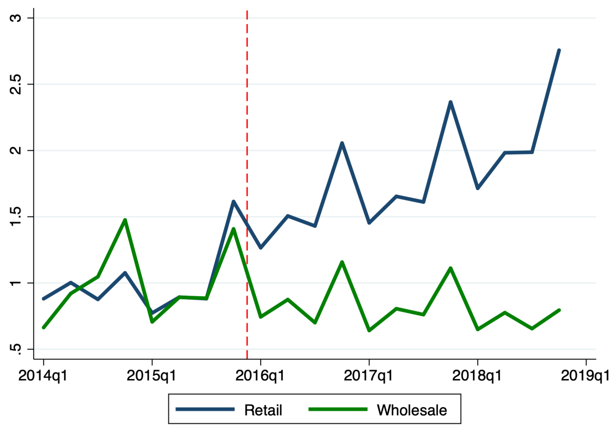
Note: This table displays the results from the regression equation 2. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The first three columns take a log of quarterly sales, costs or tax liabilities as dependent variables. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. The last three columns decompose the change in total costs into changes into its components: they take a log of production, administrative and other costs as dependent variables. Time and firm fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.7 Complementary Tables — Unweighted regression

Table A2 presents results from unweighted regressions specified in equation 2. The first three columns show the coefficients from regressions that use a log of quarterly sales, costs or tax liabilities as dependent variables. In the last three columns I decompose the change in total costs into changes into its components: log of production, administrative and other costs are the dependent variables. The results are consistent with outcomes from weighted regressions shown in Table 5. From column 1 we can see that the E-receipt program induced retailers to report 15% higher sales relative to wholesalers. However, in column 2, retailers' reported costs increased by 18%. This increase in costs offsets the effect on CIT liabilities, and there is no significant increase in CIT liabilities. The last three columns show that an increase in total costs is mainly driven by the rise in production and administrative costs. The coefficient on other costs is insignificant even though it is positive. This result is slightly different from the findings in the existing literature (for example Carrillo et al. 2017), where they document that firms in Ecuador tend to increase costs that are more difficult to verify such as "other administrative costs" in response to increased third-party information on sales.

Figure A7: Underestimation: Pre-trend in CIT data



(a) Sectors' standardised total sales

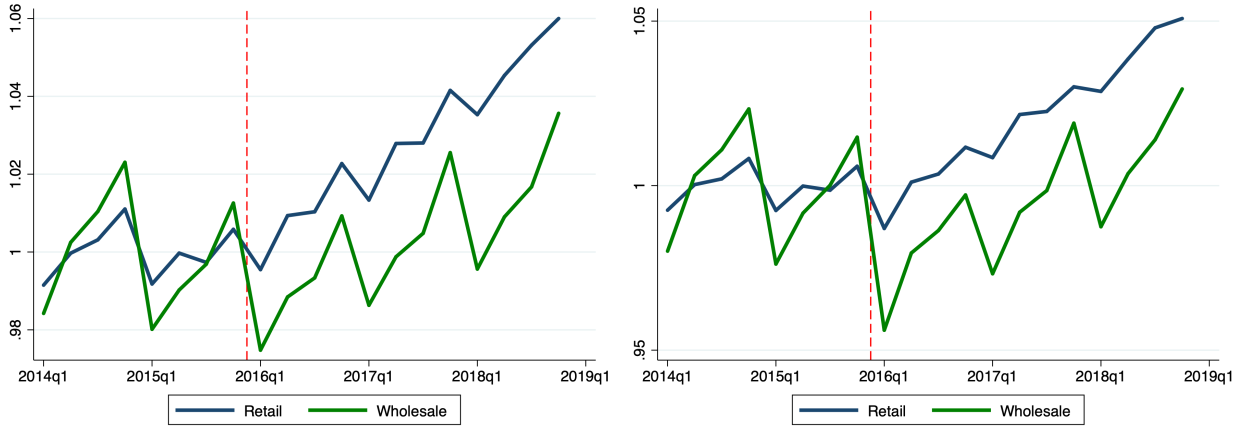
(b) Sectors' standardised total costs

Note: Panel (a) and (b) display the changes in the total sales and costs of retail and wholesale sectors reported on CIT returns. Wholesale sector contains only the firms that never sell to any retailers between 2014 and 2018. Each line is the total sales reported by all firms aggregated by retail or wholesale sectors scaled by the pre-intervention average quarterly sales each sector group. The graph plots the raw sales. Thus there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

A.8 Underestimation of Direct Effects

It is important to note that analyses of the direct effect on retailers underestimate the true effects of the E-receipt program. To identify the direct effect, I use wholesalers as a control group for retailers. The underlying assumption for this strategy is that wholesalers would have behaved similarly to retailers in the absence of the intervention (parallel trend assumption) and that wholesalers are not affected by the program. The data exhibit a reasonable parallel trend in the sales of retailers and wholesalers before the intervention, which validates the parallel trend assumption. However, the wholesalers are likely to be affected by the program both directly and indirectly. Wholesalers are likely to be directly affected because they could sell to final consumers. Also, not surprisingly, wholesalers are classified as upstream firms, and I find substantial spillover effect on the upstream firms in Section 3.2. Therefore, the estimated effects are a lower bound of the true direct effects on retailers. To investigate the extent of the underestimation, I change the control group to the wholesalers that never sell to any retailers. I identify such wholesalers using the firm network data from VAT invoice. To test the no pre-trend assumption I plot the industry level sales and costs from firms' CIT (VAT) returns in Figure A7 (Figure A8) and they show reasonable parallel trend.

Figure A8: Underestimation: Pre-trend in VAT data



(a) Sectors' standardised total sales (b) Sectors' standardised total purchasing costs

Note: Panel (a) and (b) display the changes in the total sales and purchasing costs of retail and wholesale sectors reported on VAT returns. Wholesale sector contains only the firms that never sell to any retailers between 2014 and 2018. Each line is the total sales reported by all firms aggregated by retail or wholesale sectors scaled by the pre-intervention average quarterly sales each sector group. The graph plots the raw sales. Thus there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

In Table A3 I report the regression results from the simple DiD specifications in equation 2. The results using CIT data are reported in. Similarly, Table A4 reports the results using VAT data.The estimated coefficients on sales and costs (as well as on VAT liabilities) are above 60% suggesting a substantial underestimation.

Table A3: Underestimation of the direct effect - CIT returns

	(1) Sales	(2) Costs	(3) CIT
DD coef	0.608***	0.755***	-0.0613
	(0.202)	(0.224)	(0.226)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	38,939	38,939	38,939
Adjusted R^2	0.87	0.86	0.88

Note: This table displays the results from the regression equation 2. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Unlike the main specification in Table 5, I use the wholesalers that never sell to retailers as a control group, which are identified from the firm network data. The dependent variables are a log of firms' reported quarterly sales, costs or tax liabilities on CIT returns. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Underestimation of the direct effect - VAT returns

	(1) Sales	(2) Purchase	(3) VAT (final)
DD coef	0.695***	0.696***	0.640***
	(0.0375)	(0.0597)	(0.125)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	76,973	76,973	76,973
Adjusted R^2	0.72	0.70	0.65

Note: This table displays the results from the regression equation 2 using the VAT data. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Unlike the main specification in Table 9, I use the wholesalers that never sell to retailers as a control group, which are identified from the firm network data. The dependent variables are a log of firms' reported quarterly sales, costs or tax liabilities on CIT returns. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.9 Cost Decomposition Sample

Some firms do not report the decomposition of the total costs accurately. For example, production costs are equal to the total costs and the other two components are zero. I do the same regression analysis by dropping those firms. The results are reported in Table A5 and they are consistent with outcomes in Table 5.

Table A5: Direct effect - CIT returns

	Main variables			Cost decomposition		
	(1) Sales	(2) Costs	(3) CIT	(4) Production	(5) Admin	(6) Other
DD coef	0.182***	0.195***	0.0986*	0.223***	0.118***	0.206
	(0.0646)	(0.0714)	(0.0567)	(0.0698)	(0.0351)	(0.230)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	139,257	139,257	139,257	125,803	129,517	27,494
Adjusted R^2	0.76	0.74	0.59	0.70	0.80	0.40

Note: This table displays the results from the regression equation 2 using the cost decomposition sample: I drop firms that do not report the decomposition of the total costs accurately. For example, I exclude the firms that report production costs equal to the total costs but the other two components are zero. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The first three columns take a log of quarterly sales, costs or tax liabilities as dependent variables. Only the firms with strictly positive profits are included in the analysis because I take a log of the dependent variable. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. The last three columns decompose the change in total costs into changes into its components: they take a log of production, administrative and other costs as dependent variables. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.10 Robustness Checks for Changes in Retailers' Misreporting Behaviour

To study the changes in misreporting behaviour of retailers on CIT returns I use a log of the original value of misreported sales and costs in Table 8. Since some of the audited firms do not misreport their sales and/or costs in some years and such observations are dropped out of my sample because I take log. To avoid this I use several other measures of misreported values sales and costs in Table A6. In particular, I use a log of one plus the misreported values and a dummy for positive misreported sales and costs. The results qualitatively confirm the fact that retailers misreport their sales less but more likely to over-report their costs after the intervention as in Table 8. Table A7 presents the estimated coefficients from the same analysis using VAT audit data.

Table A6: Other measures for misreported sales and costs on CIT returns

	Misreported Sales ($)			Misreported Costs ($)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Original	log(x+1)	Dummy	Original	log(x+1)	Dummy
DD coeff	-0.0151	-0.0328	-0.00531	0.362**	1.053***	0.0582***
	(0.249)	(0.275)	(0.0181)	(0.137)	(0.296)	(0.0192)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	2,246	14,100	14,100	3,091	14,100	14,100
Adjusted R^2	0.68	0.14	0.14	0.74	0.30	0.29

Note: This table CIT audit data and uses different measures for values of misreported sales and costs. In particular, for columns 1-3 (4-6), I use a log of the original value misreported sales (costs), a log of one plus the misreported sales (costs), and a dummy for positive misreported sales (costs), respectively. Since some firms do not misreport sales and costs (zero value), the number of observations is smaller in columns 1 and 4. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. All regressions are weighted by firms' average annual sales reported on CIT returns before the intervention. Time and firm fixed effects are included in all regressions as expressed in equation 2. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.11 Comparison of Sales Reported on CIT and VAT returns

In section 3.1.2 I document that retailers' reported sales on VAT returns respond more compared to CIT returns. One potential explanation is that firms could be taking advantage of the fact that it is not straightforward to compare CIT and VAT returns for tax authorities and manipulate their reported values. Specifically, values reported on both tax returns are cross-checked manually by tax officers and it is not done for all firms. And it is not straightforward to compare because VAT returns are submitted monthly and values corresponding

Table A7: Other measures for misreported sales and costs on VAT returns

	Misreported Sales ($)			Misreported Costs ($)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Original	log(x+1)	Dummy	Original	log(x+1)	Dummy
DD coeff	-0.231	-0.362*	-0.0206	-0.180	-1.073***	-0.0671***
	(0.149)	(0.201)	(0.0139)	(0.505)	(0.278)	(0.0180)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	2,174	11,606	11,606	1,983	11,606	11,606
Adjusted R^2	0.62	0.14	0.14	0.62	0.30	0.31

Note: This table VAT audit data and uses different measures for values of misreported sales and costs. In particular, for columns 1-3 (4-6), I use a log of the original value misreported sales (costs), a log of one plus the misreported sales (costs), and a dummy for positive misreported sales (costs), respectively. Since some firms do not misreport sales and costs (zero value), the number of observations is smaller in columns 1 and 4. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. All regressions are weighted by firms' average annual sales reported on CIT returns before the intervention. Time and firm fixed effects are included in all regressions as expressed in equation 2. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to the respective month are reported. In contrast, CIT returns are sent quarterly and values are in cumulative values. Table A8 compares quarterly total reported sales (in thousand USD) on CIT and VAT returns and their difference. It shows that there is a large difference between the sales reported on CIT and VAT returns.

Table A8: Comparison of sales reported on CIT and VAT returns

	min	mean	med	max	sd	count
Sales on CIT return	0	84	18	689	153	82,023
Sales on VAT return	0	124	18	2,442	331	82,023
Difference (CIT-VAT)	-2,430	-41	0	674	213	82,023
Share of diff in CIT sales	-1,324,575	-58	0	100	4,946	82,023

Note: This table presents summary statistics of total reported sales on CIT and VAT returns as well as their difference. All nominal values are in thousand USD (1 MNT = 2600 USD).

A.12 VAT Data — Decomposition of Purchasing Costs

In Table 9 I document that the purchasing costs of VAT-liable retailers increase by 39%. I decompose the increase in purchases into its components using the equation 2: total purchasing costs are split into the deductible and non-deductible input costs on VAT returns.[58] The results are presented in Table A9. It results show that both deductible and non-deductible costs increase. It is worth noticing that non-deductible input costs increase more compared to deductible costs even though it does not affect firms' VAT liabilities.

Table A9: Decompostion of purchasing costs reported on VAT returns

	Weighted			No weight		
	(1) Total	(2) Deductible	(3) Non-deductible	(4) Total	(5) Deductible	(6) Non-deductible
DD coef	0.378***	0.296**	0.883***	0.303***	0.312***	1.010***
	(0.0579)	(0.128)	(0.247)	(0.0357)	(0.0387)	(0.198)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry	Industry
Observations	130,309	130,164	9,099	130,309	130,164	9,099
Adjusted R^2	0.73	0.74	0.55	0.70	0.70	0.60

Note: This table displays the results from regressions expressed in equation 2. The first three columns represent regressions weighted by firms' average quarterly sales before the intervention. The last three columns are for unweighted regressions. The dependent variables are a log of the total, deductible and non-deductible input costs. Variable DD coef is defined as the interaction between a dummy for retail sectors, and a dummy variable that equals one for the periods after January 2016, zero otherwise. Time period (before and after-intervention) and firm fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[58] Summary statistics are presented in Table A10 and it can be seen that deductible costs make 99% of the total input costs and it equals the total purchasing costs in the 90% of the sample.

Table A10: Summary statistics - Purchasing costs decomposition

	min	mean	med	max	sd	count
Value						
Total purchase	0	57	6	13,064	237	132,586
Deductible	0	55	5	13,064	226	132,586
Non-deductible	0	2	0	8,259	55	132,586
	.					
Share (%)						
Deductible	0	98.9	100	100	8	132,586
Non-deductible	0	1.1	0	100	8	132,586

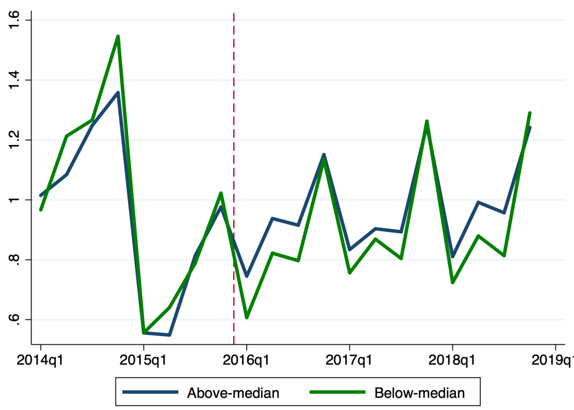
Note: Table A10 presents descriptive statistics of quarterly total, deductible and non-deductible purchasing costs reported on VAT returns. All nominal values are in thousand USD (1 MNT = 2600 USD).

Table A10 presents summary statistics of quarterly total, deductible and non-deductible purchasing costs reported on VAT returns. It can be seen that deductible costs make 99% of the total input costs and it equals the total purchasing costs in 90% of the sample.

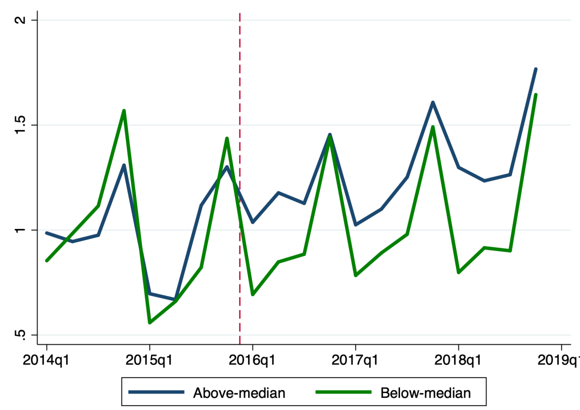
A.13 Analysis of Indirect Effects — Pre-trend in Tax Liabilities

I examine whether the parallel-trend assumption holds for CIT and VAT liabilities of the upstream firms in Figure A9. In particular, for each quarter, I aggregate the reported CIT liabilities of the upstream firms in the treatment group and standardise it by dividing the sums by pre-intervention mean value of the sums. I do the same for the firms in the control group and plot them over time in panel (a) in Figure A9. Similarly, panel (b) plots the aggregate VAT liabilities for each group. As we can see from the plots, there is no pre-trend before the policy change, but total sales of the treatment group start to increase more compared to the control group in 2016. The gap between them starts widening over time, and I attribute this divergence to the E-receipt program.

Figure A9: Indirect effects — Pre-trend in tax liabilities



(a) CIT liabilities



(b) VAT liabilities

Note: Panel a (b) displays the changes in the total CIT (VAT) liabilities of the upstream firms in treatment and control groups. Each line is the sum of sales reported by firms in the treatment or control groups scaled by the pre-intervention average quarterly tax liabilities of each group. The graph plots the raw sales. Thus there are spikes in quarter four each year due to seasonality. The vertical dashed red line represents the start of the E-receipt program, which is January 1, 2016.

A.14 Analysis of Indirect Effects — Transaction-level DiD (no weight)

Table A11 presents results from unweighted regressions specified in equation 3. The first column uses all upstream firms regardless of their industry. In columns 2 I drop upstream firms that are retailer sector. The last column excludes both retailers and wholesalers from the analysis. The estimated coefficients are positive even though they are not significant. Therefore, it also suggests that there is a positive effect on upstream firms' sales to retailers compared to their sales to non-trade buyers as discussed in section 3.2.

Table A11: Indirect effects — Transaction-level DiD (no weight)

	log(Transaction value)		
	(1)	(2)	(3)
	All sellers	Non-retail	Non-trade
DD coef	0.0178	0.0253	0.00825
	(0.0454)	(0.0305)	(0.0551)
Buyer Ind.FE	Yes	Yes	Yes
Seller FE	Yes	Yes	Yes
Weight			
Cluster	Industry	Industry	Industry
Observations	119,053	105,776	44,956
Adjusted R^2	0.61	0.61	0.60

Note: This table displays the results from the regression equation 3. The variable DD coef is defined as the interaction between a dummy for time period and a dummy that equals one if a buyer's sector is retail, zero otherwise. The dependent variable is a log of upstream firms' quarterly sales to (retail vs non-retail) downstream firms. The first column uses all upstream firms regardless of their industry. In columns 2 I drop upstream firms that are retailer sector. The last column excludes both retailers and wholesalers from the analysis. All regressions are unweighted. Time and supplier fixed effects are included in all regressions. Standard errors are clustered at 4-digit industry level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.15 Robustness Checks Using Unbalanced Data

In the main analysis, I focus on firms with strictly positive profits and tax liabilities. This is because I take a log of the variables but the firms' profits and tax liabilities can be zero or even negative. In this subsection, I show that the main results survive qualitatively even if I include the firms with non-zero profits and tax liabilities in the analysis.

I start by analysing the CIT data as in the main text. Table A12 corresponds to the Table 5, and it is consistent the main result. If anything, it suggests that the effects of the E-receipt program on firms' reported sales, costs and CIT liabilities are stronger as the estimated coefficients are larger.

Table A12: Direct effects - CIT returns — Unbalanced data

	(1) Sales	(2) Costs	(3) CIT
DD coef	0.403***	0.344***	0.141
	(0.0331)	(0.0424)	(0.105)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	314,097	314,097	213,168
Adjusted R^2	0.51	0.65	0.50

Note: This table displays the results from the regression equation 2. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The dependent variables are a log of firms' reported quarterly sales, costs or tax liabilities on CIT returns. That is the firms with zero tax liabilities (firms with zero or negative profits) drop out of the sample. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, I analyse if there the E-receipt program caused is any real response. That is, I examine if there are any differential changes in retailers' number of workers and the value of wages. Table A13 corresponds to the Table 6, and it confirms that there is no real response. I don't find any significant effect on retailers' workers and wages. In other words, any changes in retailers' reported sales, costs, and CIT liabilities are due to changes in reporting behaviour.

Table A13: No real response by retailers — Unbalanced data

	Main variables			Real response	
	(1)	(2)	(3)	(4)	(5)
	Sales	Expenses	CIT	Wages	Workers
DD coef	0.280***	0.228***	0.0763	0.0522	0.0715
	(0.0367)	(0.0284)	(0.0766)	(0.0587)	(0.0929)
Firm FE	Yes	Yes	Yes	Yes	Yes
Cluster	Industry	Industry	Industry	Industry	Industry
Observations	143,795	143,795	94,099	143,795	143,795
Adjusted R^2	0.57	0.74	0.53	0.89	0.88

Note: This table displays the results from the regression equation 2. The first three columns take a log of quarterly sales, costs or tax liabilities as dependent variables. The payroll data covers Q1 in 2015 to Q3 in 2018 only. Therefore, less observation compared to Table 5. The dependent variables in the last two columns are log of total wages and number workers. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lastly, I turn to VAT data. Table A14 corresponds to the Table 9, and it is consistent the main result. If anything it implies even stronger effects of the E-receipt program on VAT-liable firms. In particular, it shows that VAT-liable retailers' reported sales increase by 45%. Even though the reported purchases increase by 39% as reported in column 2, they do not cancel out the effect on the final VAT liabilities of the retailers. VAT liabilities increased substantially, by 31%, in column 3.

Table A14: Direct effects - VAT returns — Unbalanced data

	(1)	(2)	(3)
	Sales	Purchase	VAT
DD coef	0.450***	0.386***	0.312**
	(0.0287)	(0.0523)	(0.124)
Firm FE	Yes	Yes	Yes
Cluster	Industry	Industry	Industry
Observations	183,793	183,793	130,316
Adjusted R^2	0.64	0.70	0.62

Note: This table displays the results from the regression equation 2 using the VAT data. The variable DD coef is defined as the interaction between a dummy for retail sectors and a dummy that equals 1 for the periods after January 2016. The dependent variables are a log of firms' reported quarterly total sales, purchasing costs or VAT liabilities. Time and firm fixed effects are included in all regressions. All regressions are weighted by firms' average quarterly sales before the intervention and standard errors are clustered at 4-digit industry level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

www.theigc.org