VAT Refunds and Firms' Performance: Evidence from a Withholding Reform in Honduras*

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Abstract

Value-added taxes (VAT) have been widely adopted across the world. One key issue in the effective administration of VAT are refunds: late or unreliable refund of credits undermine the best traits of VAT systems and might affect firms' growth and investment opportunities. In this paper, we use administrative data on the universe of VAT taxpayers in Honduras to estimate the impact of a tax reform aimed at curtailing excessive unrefunded credits. First, we document the substantial expansion of unrefunded credits in the period 2011-2019, equivalent to 1.5% of GDP, and characterize its drivers. We then study a reform that substantially decreased the withholding rate of VAT liabilities by credit card providers. Using a differences-in-differences approach, we document that the reform causally decreased unrefunded credits of affected firms and thus was equivalent to a tax cut. We then evaluate whether this reform affected firms' economic outcomes such as investment, wage bill or revenue, and are unable to reject the null hypothesis of zero effects on firm growth.

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1 Introduction

Value added-taxes (VAT) are popular around the world and often collect a large share of tax revenues (Ebrill et al., 2001). VAT are considered a particularly effective tool to mobilize revenue without several of the distortions introduced by turnover taxes and without the higher informational requirements needed to enforce income taxes.

One of the important dimensions in the functioning of an effective VAT is an efficient refund system for firms with negative liabilities. Taxable sales generate liabilities that can be reduced by credits generated by taxable purchases. In some cases, credits might exceed liabilities and firms should be refunded for the net amount. This is a common case, for example, for exporters whose sales to consumers in other countries are exempt from value-added taxes, but whose inputs are taxed (Gérard & Naritomi, 2018; Waseem, 2023).

Refund systems are particularly challenging in low- and middle-income countries for two reasons. First, these countries often have lower institutional capacity to perform timely checks to assert the veracity of refund claims, and for that reason often delay or outright refuse refunds (Harrison & Krelove, 2005; Pessoa et al., 2021). Second, faced with limitations to enforce VAT payments, particularly by medium and small companies, these countries have resorted to some form of withholding of VAT liabilities, usually by larger companies involved in a transaction (Brockmeyer & Hernandez, 2016; Waseem, 2022). This can compound the refund problem since setting a high withholding rate on sales means that a large share of firms will end up with negative tax liabilities after taking credits into account and requesting refunds.

In this paper, we shed light on the implications of the VAT refund policy in a lower-middle-income country, Honduras. Our analysis proceeds in two parts. First, we show how *de facto* the VAT refund policy is almost non-existent due to the high costs for taxpayers to claim a refund. This leads to a vast and increasing balance of unrefunded credits by firms against the government. In the period 2011-2019, that balance increased almost four-fold and reached approximately USD 300 million or the equivalent of 1.5% of gross domestic product (GDP). We also characterize firms that register net negative liabilities and show that this is a widespread phenomenon in the country: across our sample period, between one-third and two-thirds of firms hold an unrefunded credit balance at any given year. We also document that, while unrefunded credits are widespread, a small number of firms hold the vast majority of unrefunded balances. In 2019, for example, less than 600 firms were responsible for 80% of the total stock of unrefunded credits and 25 firms explained the net increase (flow) of this balance. Firms with large unrefunded balances were more likely to be in upstream industries such as manufacturing and wholesale, consistent with the fact that a large share of withholding is driven by large firms performing withholding of their suppliers.

In the second part of the paper, we investigate the real economic implications of refund delays. In the case of Honduras, International Finance Corporation (2022) discusses how "ac-

celerating the payment of VAT refunds the government owes to taxpayers is (...) important, since such delays may affect businesses' liquidity flow". We explore a change in withholding policy to estimate the causal effect of changing *de facto* refunds on the performance of firms. One form of withholding common in many countries is by digital payment providers (usually debit and credit card operators). When a transaction is performed using a digital payment, the providers automatically withhold and remit to the tax authority part or the entirety of the tax liability. From the point of view of the tax authority, this so-called "split payment method" (due to the fact that a third-party remits the liability) has the advantage that a large share of total liability is remitted by a small number of large taxpayers (the providers). These withholdings mechanisms by electronic payment operators are more common in middle- and low-income countries, but have garnered attention in higher-income countries recently due to the possibility of withholding VAT liability from electronic transactions involving digital businesses (Maciel & Troiani, 2018).

Until 2013, these providers in Honduras had to withhold and remit to the tax authority 100% of the VAT tax liability in any transaction. In 2014 that rate was decreased to 50% and in 2017 subsequently reduced to 10%. We first show that the reform had bite: in the year after the 2017 reform, digital payment providers remit L 1.5 billion (USD 60 million) less in withholding to the tax authority.

We then explore a differences-in-differences design, comparing outcomes for firms that had high ex-ante usage of digital payments with firms that barely used it. Despite no change in the VAT rate, we show that effective VAT rates for firms with high-exposure sharply drop after the reform, by more than 1% of pre-reform gross taxable sales. The reason for this is that before the reform these taxpayers were accumulating a large balance of unrefunded VAT credits due to the high withholding rate. With the reform, they started to draw down on those credits - the share of firms claiming to hold unrefunded credits fell by over 8 p.p. after the reform for firms with high exposure to debit and credit card withholding. Effectively, for several firms, this is equivalent to a tax cut since they never received refunds.

We then use income tax declaration data to compute measures of firm performance such as investment, cash flow, and wage bill; and evaluate whether the reform causally changed performance of highly affected firms. Our estimates suggest no changes in balance sheets, investments, sales, wage bills, or profits by affected firms, despite the large decrease in unrefunded credits and effective tax rates they face.

Our paper contributes to three strands of the literature. First as stated by Gérard & Naritomi (2018) "the aggregate implications of existing policies regarding tax refunds is unclear, as are the potential improvements that alternative policies could bring about". We contribute to the scant existing evidence on the features of VAT refunds in developing countries - a topic that has attracted a lot of attention in policy discussions (Harrison & Krelove, 2005; Pessoa et al., 2021). Waseem (2023) uses a VAT reform in Pakistan and documents that the worry that tax authorities express about overclaimed refunds is borne in the data: approximately

two-fifths of refunds seemed to be issued by invoice mills - "fake" firms that exist only to generate VAT credits. In our setting, we document that the tax reform we study had the intended effect of decreasing unrefunded credits but did not enact investment or growth by affected firms. Second, our investigation of the effects of a withholding reform also contributes to the literature on the role of withholding in tax compliance and firms' performance (Bagchi & Dušek, 2021; Waseem, 2022; Brockmeyer & Hernandez, 2016). Finally, our estimates of the causal effects of the change in withholding policy on firms' refund balances and real economic activity provide new evidence on the impacts of (effective) tax cuts on firms' investment and employment (Bilicka, 2020; Ohrn, 2018; Moon, 2022). Our results of null effects of changes in taxes on investment mirror those of Harju et al. (2022) for small firms in Finland.

The paper is organized as follows. In section 2 we discuss the most relevant characteristics of the VAT system in Honduras. Section 3 exploits administrative records from formal firms filling VAT between 2011 and 2019 to shed light on the expansion in the stock of unrefunded credits in the country, and characterize the firms that are affected the most. Throughout section 4 we exploit the variation induced by the 2017 reform that substantially decreased the VAT withholding rate for digital payment providers and analyze the effects of this policy on unrefunded balance, and firms' performance. Section 5 concludes.

2 VAT system in Honduras

The Value-Added Tax in Honduras was created in 1963 as a tax on wholesale distributors and nowadays covers the entire production chain. VAT revenue accounts for approximately 40% of total tax revenue (International Monetary Fund, 2018). In 2018, the standard rate was 15% for the majority of goods and services and 18% for specific products such as alcohol and tobacco. Firms must file monthly declarations reporting their total sales (including taxable and non-taxable), the resulting VAT liability, and any credits arising from the purchase of taxed inputs. They must also claim any withholding already applied to their sales, which are netted out to arrive at their final payable taxes.

The necessity for VAT refunds often arises from three main sources (Pessoa et al., 2021). In most high-income countries, exporters are the main source of refund claims since their sales are exempt while their purchases are not. In low- and middle-income countries, two other features of the VAT system often lead to refund claims. First, the existence of VAT exemptions (zero ratings) or preferential rates generates imbalances between taxable sales and purchases (e.g. basic foods being zero-rated for customers means grocery stores will have VAT credits from purchases but no debits). Second, the existence of withholding schemes by third parties can generate excess credits for taxpayers. We discuss each of these features in the VAT system in Honduras in turn.

Exports are fully exempt from VAT, and their purchases directly related to the production of exported goods are also exempt. Exporters can register with the Ministry of Finance and

receive an "exempt purchase order" (OCE, from the Spanish acronym *Orden de Compra Exenta*) which is presented to suppliers and proves VAT should not be charged in those purchases[1]. Alternatively, they can also claim non-taxable sales and credits when filing VAT declarations, and claim refunds. As we document below, exporters are among those firms with the highest unrefunded VAT credits.

Exemptions in Honduras are very broad. Firms with yearly sales below L250,000[2] and a single establishment are included in a simplified VAT regime and can file a single yearly declaration. Firms included in several "special regimes", including those operating in tourism, agriculture, and energy sectors, are fully exempt from VAT in their sales. According to Article 15 of the VAT Law, exemptions are granted to a long list of products including several staple foods and medicines. In the period of 2012-2019, exemptions reported by firms in the VAT accounted, on average, for up to 41.5% of total sales, and up to 32.6% of total purchases (see Figure A1a and Figure A1b, respectively).

Finally, withholding of VAT liabilities is performed by four agents. The first is digital payment providers, namely debit and credit card operators. Until 2014, they withheld the entirety of the VAT liability (12% of taxable sales) at the moment of the transaction and directly remitted that value to the tax authority. The withholding amount was reduced to 50% in 2014 (when the general VAT rate also increased to 15%) and subsequently to 10% in 2017. The remaining withholding agents still remit 100% of the VAT liability: any sales to government entities, taxpayers formally identified as "large", and airline companies are withheld immediately, and agents remit that value to the tax authority.

In this paper, we use three administrative datasets to characterize the VAT refund system in Honduras and evaluate the causal impact of changes in withholding rates. First, our main source of information is firm-level monthly VAT filings. We use those to compute firms' self-reported VAT liability, credits, and withholding. The monthly declarations also include the net liability, which is often negative in cases where credits are larger than liabilities or total withholdings exceed the net liability. Monthly declarations also report the balance of refunds taxpayers accumulate with the tax authority, which we use to compute total unrefunded aggregates. Our second main source of information is monthly declarations filed by digital payment providers informing, for each taxpayer, the total amount of taxable sales and amounts withheld. We use this to cross-check the self-declared information filed by taxpayers and also to compute the amount of withheld taxes remitted to the tax authority. Finally, we also use yearly income tax filings for all VAT taxpayers to assess the impact of the 2017 reform on firms' performance, using data on firms' balance sheets.

[1] Pessoa et al. (2021) note that these type of exemptions on purchase order can make the refund problem worse: instead of having few large exporters claiming credits, the problem is potentially transferred upstream in the production chain, with several suppliers having their sales exempt but not their purchases. In 2023, the government of Honduras is proposing eliminating OCEs as part of a broad tax reform branded "Tax Justice Law" (the draft law can be seen here, in Spanish).

[2] Approximately USD 10,000 at the 2018 average exchange rate of L25 = USD 1.

3 Characterizing the VAT refund system in Honduras

As a rule, Honduras applies an "indefinite carry-forward" (Harrison & Krelove, 2005) approach to VAT refunds: according to the VAT law, "excess credits in a given month will be carried forward to next month and successively until they are exhausted" (*Ley de Impuesto sobre Ventas*). Taxpayers can request a refund in specific circumstances but that is a cumbersome process and, accordingly, very few taxpayers do so. Taxpayers are required to provide extensive documentation and original receipts of sales and purchases; documents must be notarized and signed by an attorney; and the reimbursement, if accepted, might take months. As shown in Figure 1a, less than 1% of VAT refund requests are processed within 30 days[3]. In addition, requesting a refund almost automatically triggers an audit of firms' accounts, which can be even costlier. For those reasons, in practice, the issuing of refunds is extremely limited to the point that refunds represent less than 1% of total VAT revenue (see Figure 1b).

The absence of a simple and expedited refund process means that taxpayers accumulate large amounts of unrefunded credits with the tax authority. Using microdata from monthly VAT filing by taxpayers, in Figure 2 we show that the aggregate amount of unrefunded credits claimed by taxpayers increased almost four-fold between 2011 and 2019, from less than L2 billion to approximately L7 billion (or USD 300 million) in 2019. That stock is equivalent to approximately 1.5% of GDP or 40% of the net VAT liability claimed by firms.

We provide initial descriptive statistics on the full sample of VAT filers in Table 1, where we aggregate our dataset at the yearly level. For 2018, we observe almost 100,000 firms filing monthly VAT declarations. Approximately 30% of the VAT filers are corporations and a plurality (40%) belongs to the service sector, followed by retailers (21%), manufacturing firms (8%) and wholesalers (6%). Each year, approximately 90% of VAT filers also file a yearly income tax declaration.

Average yearly taxable sales are L4 million (USD 160,000) and net VAT liability (VAT debits minus VAT credits) is L190,000 (USD 7,500) – which implies that average taxable value-added is approximately 32% $\left(\frac{190*(1/.15)}{4{,}010} \approx .32\right)$. The net VAT liability is different from the actual amount that firms must remit to the tax authority for two reasons. First, part of the liability might already have been remitted to the tax authority by third-party withholding agents. The average claim of withholding in the sample is L40,000 or 20% of the net liability. In 2018, the average amount withheld by digital payment providers was only 60% of the average amount claimed to be withheld by the government, and less than one-third that withheld by large taxpayers. But that is in stark contrast with the scenario four years earlier, in 2015 when digital payment withholding was three times larger than the government's and 50% larger

[3] According to the Tax Administration Diagnostic Assessment Tool (TADAT), an effective VAT refund system lasts 30 days to process and give a resolution (accept or declined) to taxpayers requesting refunds. This might be quite restrictive in the Honduran context since Article 86 of the Tax Code establishes that refund resolutions must be issued within a maximum period of 60 business days from the filing of the request. However, less than 1% of total refund requests are processed on time, even when accounting for 60 days.

than larger taxpayers. These changes are the result of the decrease in the withholding rate by digital payment providers that we discuss in the second half of this paper.

The second reason is that firms might have accumulated unrefunded credits, and those are abated to arrive at the final VAT payment for the period. In panel D, we document that 40 - 50% of firms in the period reported positive unrefunded credits at least in one of their filings. In 2018, the average stock of unrefunded credits in December was L75,000 or 40% of what firms claimed to be their net VAT liability.

3.1 Characterizing firms with accumulated unrefunded credits

We have documented how the growing stock of unrefunded credits reflects the fact there is no systematic refund policy ongoing in Honduras. Automatic refunding can be a challenge in a setting of constrained administrative capabilities to assert the veracity of claimed refunds (Waseem, 2023). Below we illustrate two stylized facts that help to provide a diagnostic of the challenge of unrefunded credits.

Fact 1: Unrefunded credits are concentrated in a small number of firms. On Table 2 we start by documenting how concentrated are taxable sales in columns (1) and (2), as a benchmark. In 2019, less than 2,000 or 2% of all firms responded for 80% of total taxable sales and less than 200 firms responded for half the sales. While taxable sales are concentrated in a few firms, the stock of unrefunded credits is even more concentrated. Only 72 firms held half the stock of unrefunded credits by the end of 2019, while 554 firms held 80% of the stock. The changes in unrefunded credits were even more concentrated. In 2019, 20% of firms saw an increase in their stock of credits, but the change in the total stock is explained by a few firms - 80% of the increase in net unrefunded credits is due to 25 firms. This shows that, whereas from the firm side, a large number of firms are affected by unrefunded credits, from the point of view of the tax authority the growing stock of unrefunded credits is due to very few taxpayers.

Fact 2: Firms with unrefunded credits are systematically different from others. To illustrate the correlates of unrefunded credit, we run a simple OLS regression with an indicator for having positive unrefunded credit at the end of the year 2016 on a series of covariates. We report the coefficient on key correlates on Figure 3. Compared to a proportion of 33% of firms presenting unrefunded credit at the end of 2016, firms declaring withholding by digital payment providers were 2 p.p. more likely to have credits, firms withheld by the government were 8 p.p. more likely to have credits, and firms withheld by other large companies were 3 p.p. more likely to have credits. Being an exporter and therefore having zero-rated sales is correlated with an increase in 5 p.p. on the probability of having unrefunded credits. Taxpayers labeled with a high risk of tax non-compliance score by the tax authority are associated with an increase of 4 p.p. on the probability of having unpaid credits at the end of the year. In

terms of economic sectors, compared to the primary sector, firms upstream (manufacturing, utilities, automotive, and wholesale) are 6 - 20 p.p. more likely to have credits while firms in service sectors (transportation, technology, and real estate) are 4 - 9 p.p. less likely to be net creditors. The exception are retail firms, that are 20 p.p. more likely to have unrefunded credit. That is likely connected to the fact that many retailers (grocery stores, for example) sell goods that are zero-rated and therefore also end up with more VAT credits than debits.

4 The 2017 digital payments withholding reform

In the previous section, we document that the stock of unrefunded VAT credits in 2019 was equivalent to 1.5% of GDP; that it is increasing over time and affects almost 50% of VAT-filing firms at some point; and that withholding arrangements seem to contribute to these unrefunded credits. In this section, we evaluate the causal impact of withholding reforms that meaningfully changed the over-withholding that a set of firms faced.

Honduras enacted two major tax reforms on VAT withholding by digital payment providers in the last decade. In 2014, as part of a broader tax reform, it decreased the withholding rate from 100% of the VAT tax liability to 50%. At the same time, it also increased the VAT tax rate from 12% to 15%, such that, for a given sales amount, the VAT liability increased in 2014 while the withholding by payment providers decreased. In 2017, the withholding rate was decreased once again from 50% to 10% of the VAT tax liability[4]. This time the reform did not change the amount of sales taxes due in any transaction - those remained fixed at 15% of sales. To give a concrete example, consider a transaction worth $100. Before the 2017 reform that operation generated a $15 sales tax liability, $7.5 of which was immediately withheld by the payment provider operator and remitted to the tax authority by the 10th of the following month; the remaining $7.5 was remitted by the establishment in their sales declaration the following month. After March 2017, the payment provider would only withhold $1.5 and the remaining $13.5 should be declared and paid by the establishment.

We present preliminary evidence that the reform was reflected in firms' monthly sales tax declarations on Figure 4. In Figure 4a, we restrict the sample to firm-month observations where a positive amount of withholding by a digital payment provider is claimed and present how the withholding amount as a share of claimed VAT liability changed over time. We document clear changes around the two reforms: average withholding as a share of total liability is stable at approximately 30% from 2011-2013, falls abruptly to 20% immediately after the 2013 reform, and falls further to approximately 7-8% after the 2017 reform. Note that in both periods there is a clear "transition" of two/three months in which the claimed withholding rate adjusts to the new level. For the main reform we study in 2017, there were legal disputes over the precise withholding rate in the first months of the year which were resolved by April[5].

[4]The reform was very salient and widely covered by the press (see Figure A7 in the Appendix).

[5]In Figure 4b, we instead plot the average amount (in L1,000) of withholding claimed by taxpayers in their

We also note that is unlikely that these large changes we observe in withholding by digital payment providers around the reform were driven by other factors – in Figure 5a, we zoom in on the 2017 reform and show that aggregate withholding by payment providers fell more than five-fold between the Q4 2016 and Q2 2017 while withholding by large taxpayers and the government were stable[6].

Consistent with these changes in averages, Figure 6 documents how the entire distribution of withholding as a share of liability shifts to the left after the reforms that decreased withholding rates. It also documents the heterogeneity across firms in the use of debit and credit cards: for some firms withholding through digital payment providers represents a very small share of their total sales, while for others it is much more significant. Because the 2014 reform involved both a change in withholding and VAT rates, in the following sections we focus on the 2017 reform to estimate the causal impact of changes in withholding rates on VAT outcomes and firms' performance.

4.1 Economic implications of withholding reform

To provide a conceptual framework for the expected impacts of the reform, consider a system in which any refunds are automatic - if firms have negative tax liabilities, those are immediately returned to them. In that case, the withholding amount is simply an earlier remittance by the payment provider to the tax authority. That might affect firms in one of two ways. First, withholding affects the cash flow of firms. While in a system with no withholding, firms receive the full amount of the transaction and remit taxes at the beginning of the following month, under a withholding system the payment providers immediately remit part of the tax, decreasing the immediate liquidity of firms. If firms are cash-constrained, that might affect their economic decisions. Second, withholding might be a guarantee that firms will pay at least part of their tax liability if the government's ability to enforce compliance is low. But note that while the introduction of withholding might also change the information available to the government (Waseem, 2022; Bagchi & Dušek, 2021), when withholding rates change the information environment remains the same: the tax authority still knows how much taxpayers should be paying.

The scenario is different when, as is the case in Honduras, refunds are rarely ever paid and generate unrefunded credits. For firms that alternate between positive and negative tax liabilities, the "credit system" means longer delays in cash flow although (nominally) they will pay the same amount of taxes. On the other hand, firms that systematically run negative tax liabilities pay higher taxes in practice. For firms that are withheld at 100% of the VAT liability

VAT declaration. Whereas the average amount fell substantially in 2017, consistent with the fact that the withholding rate fell while the VAT rate was constant, the same did not happen in 2014. That is due to the fact that, while withholding rates fell, the VAT tax rate increased so the net effect on withholding amounts is almost null.

[6]In Figure 5b, we show that the aggregate claimed withholding by firms closely tracks the withholding amounts that digital payment providers independently submit to the government.

for their sales and are never refunded, they are in fact paying the equivalent of a turnover tax instead of a value-added tax – with possibly all the negative incentives involved in turnover taxes (Gérard & Naritomi, 2018).

4.2 Data

We exploit administrative records available from Honduran formal firms to understand the effects of a reduction of the VAT withholding rate in 2017. The empirical analysis of the withholding reform relies on two different datasets. First, we analyze the immediate effects of the reform on VAT payments, unrefunded credits, and sales based on monthly VAT records. In our preferred specification for analyzing the impacts of the reform on VAT outcomes, we rely on a balanced panel of approximately 2,000 firms and 33,000 firm-quarter observations covering the period between 2015q1 and 2018q4.

Additionally, we use yearly income tax declarations (both corporate and personal income) to obtain information on the balance sheet of firms and evaluate the effects of the reform on firms' performance, including investment, profits, and wage bills. For this stage of the analysis, income tax records are merged with a balanced panel of firms filling VAT regularly between 2014q1 and 2019q4, but the estimates are aggregated at the year and not quarter level.

Additional Firm Traits. VAT and income tax datasets are merged with a third database including different characteristics of firms according to tax registration records, such as economic activity sector, official taxpayer size defined by the tax authority (medium, large, small), and legal category of the taxpayer (corporate or personal business). The economic activity is based on the International Standard of Industrial Classification of all Economic Activities (ISIC) Revision 4. We then classify economic activities into twelve (12) sub-sectors ranging from agriculture and extraction to manufacturing and services. We also create a series of pre-reform firm characteristics that we hold fixed in time, such as quantiles of pre-reform turnover; and measures of liquidity constraints (pre-reform average of the cash-to-assets ratio) and capital-intensity (pre-reform average of firm's ratio between capital assets and sales). Appendix B.2 offers a further description of each of the variables that are built from the VAT and income tax records.

Treatment and Control Definition. The first challenge in estimating the causal effect of the reform is that the change in withholding rate is applied to all firms subject to withholding by digital payment providers. One intuitive strategy could be comparing firms that, before the policy change, reported some withholding (and were therefore affected by the policy) with firms that were not withheld. In Figure A4, we document that these two groups of firms were very different in terms of taxable sales before the policy change in 2017. Firms with some withholding were much larger in terms of taxable sales than those with no withholding (see

Figure A4a). Moreover, size heterogeneity is also present within firms with some usage of digital payments themselves. Low-usage firms are larger in terms of taxable sales than both firms without any withholding and high digital payments usage firms (see Figure A4b).

For our main analysis, we consider an *intensive margin* of withholding. We first consider only firms that filed VAT taxes in 2016, the year prior to the reform. We then compute the ratio between the total amount of withholding by digital payment providers and total sales for the year. Treated firms, those more likely to be affected by the reform, are defined as those above the 75th percentile of the withholding-to-sales ratio; control firms are those below the 25th percentile. In Figure A5 we report the distribution of the VAT withholding from debit and credit payments as a percentage of gross liability along with the cutoff where the treatment and control group were defined. For the control group of low-usage firms, the cutoff was located around 5%, while for the treatment group of high-usage firms, the cutoff was close to 25% - meaning that treated firms sold over 25% through debit and credit card operators. As a robustness check, we will change the intensive margin threshold to define treatment and control groups as those firms with a withholding-to-sales ratio above and below the median, respectively.

Normalization by Sales. Our baseline estimates follow a standard strategy in the literature: scaling every continuous outcome by firms' total sales in 2016, the pre-reform period[7] (Harju et al., 2022; Kennedy et al., 2022). Scaling by turnover reduces the variance of our outcomes and the differences in levels between treatment and control groups, which as discussed are meaningful. It also allows for a clear interpretation of the economic magnitudes of our coefficients.

Normalization is done at different frequencies. We aggregate monthly VAT filings at the quarterly level and normalize them using sales in the same quarter of 2016 – i.e. an observation in the first quarter of any year is normalized dividing it by the firm's sales in the first quarter of 2016, and so on. Balance sheet outcomes are normalized directly by the total annual sales in 2016. After scaling every VAT and balance sheet outcome by the sales in the pre-reform

[7]Normalization is performed on variables coming from two different administrative records (VAT and income tax), so there are possible differences in the value of annual sales reported in both data sources. In fact, even when they should be the same for annual taxable base purposes, there are $\approx 1\%$ of observations in the sample with discrepancies in the annual turnover from income tax records with respect to that reported in VAT filings. In this sense, some steps are conducted to properly normalize every variable before the empirical analysis. First, we redefine the annual revenue of every firm in year t as:

$$\text{Adjusted annual revenue } (Y_t) = \max\{\text{Annual turnover from income tax, Sum of annual turnover from VAT}\}$$

Then, yearly balance sheet outcomes are directly normalized using the value of this adjusted annual revenue in 2016 (Y_{2016}). Finally, we proceed to normalize VAT outcomes. Since VAT outcomes are scaled at the quarterly level, we rebuilt the quarterly turnover in 2016 by multiplying the adjusted annual revenue by the quarterly share of sales ϕY_t that each firm reports in VAT records (with $\phi \equiv \frac{\text{VAT quarterly revenue}(Y_{q,t})}{\text{VAT annual revenue}(Y_t)}$ for $q = 1, \ldots, 4$). To avoid the presence of missing values at the moment of the normalization, firms reporting all their sales in only one specific quarter, are assigned with an even share distribution of sales across the year ($\phi = 1/4$). Nonetheless, this specific adjustment is only implemented for 0.4% of observations in the VAT sample.

period -except those who are already measured as a percentage-, we then proceed to winsorize them at the 95th percentile of observations - except for profit margins, which are trimmed at the 1st and 99th fractions instead. We will conduct robustness checks trimming outcomes at different fractions.

4.2.1 Summary statistics

Table 3 displays descriptive statistics for the sample of unique firms included in the empirical analysis of the reform, separating them according to high- (treatment) and low-usage (control) of digital payment providers. We report means and standard deviations for 2016, the year before the withholding reform. In **Panel A**, we report some traits of the firms under analysis. Compared to control firms, those with high exposure to digital payments are more likely to be defined as "small taxpayers" by the Tax Authority and are more likely to be in the services sector vs. retail, wholesale, or manufacturing. By construction, all firms in our sample also filed income taxes in 2016.

In **Panel B** we display summary statistics for firms in the balanced panel between 2015q1 - 2018q4. We note that firms in the control group are systematically larger than those in the treatment group - their taxable sales, VAT liabilities, and credits are approximately three times as large on average. The main exception to that is in total withholding, where firms in the treatment group claim on average L600,000 vs. L250,000 in the control group. This is consistent with the facts that treatment firms are defined as those with high exposure to digital payment withholding and that this was the largest withholding source before 2017 – firms in the control group declare more withholding by the government and by large firms, but those are smaller in magnitude.

Finally, in **Panel C** we provide descriptive statistics for variables computed from yearly income tax records. Consistent with VAT returns, firms in the control group are three to four times larger in terms of assets, liabilities, several investment measures, wage bills, and profits.

4.3 Empirical strategy

Our goal is to estimate the impact of the change in withholding rates by digital payment providers on tax collection and firm behavior. The main challenge in estimating that causal relationship is that the change in withholding rate applied to all firms subject to withholding by digital payment providers. Our empirical strategy relies on the fact that firms were differentially affected by the reform, based on pre-reform usage of digital payments. We then estimate a differences-in-differences model that compares outcomes before and after the reform, for firms with ex-ante high- and low exposure to digital payments, according to the following specification:

$$y_{ft} = \gamma_f + \theta_{tis(f)} + \sum_{t=\underline{T}}^{\overline{T}} \beta_t 1\{HighUsage\}_f \times 1\{period = t\}_t + \epsilon_{ft} \quad (1)$$

where y_{ft} are outcomes of interest reported by firm f in period t; γ_f are firm fixed effects; $\theta_{tis(f)}$ are time-industry-size fixed effects, with size $s(f)$ defined as the quantiles of the average sales in the years prior to the reform. $HighUsage_f$ is an indicator if firm f had high exposure to digital payments withholding (above the 75th percentile). Control firms are those with low exposure to withholding in 2016 (below the 25th percentile). Our coefficients of interest are β_t, which capture the differential effects between high usage (treatment) and low usage (control) firms $\forall t \in (\underline{T}, \overline{T})$. In baseline results, whenever we refer to VAT outcomes, the period t is measured in quarters, hence, $\underline{T} = -8$ and $\overline{T} = 8$. When it comes to outcomes on firms' performance the time t will refer to years, thus, $\underline{T} = -3$ and $\overline{T} = 3$. In all cases, we set $\beta_{-1} = 0$; ϵ_{ft} is an error term, and standard errors are clustered at the firm level.

In Equation 1, the interpretation of coefficients β_t varies according to the period of analysis. In the period before the intervention, each β_t $\forall t \in \underline{T}$ is a test of parallel trends. While coefficients after the reform, each β_t $\forall t \in \overline{T}$ tests the null hypothesis of no effect of the reform. The underlying assumption for our identification strategy is that firms with high and low exposure to digital payment before the reform would have trended similarly in the absence of the 2017 withholding reform, implying that firms with ex-ante low exposure are a reasonable counterfactual of how high exposure firms would behave if no reform was implemented.

Along with the event studies, we also estimate average treatment effects by pooling the pre- and post-reform periods. We re-adapt equation 1 and run regressions of the form:

$$y_{ft} = \gamma_f + \theta_{tis(f)} + \beta HighUsage_f \times Post_t + \nu_{ft} \quad (2)$$

where $Post_t$ is a dummy taking the value of 1 for the post-reform period, and zero otherwise. In this model, we interpret β as the causal differential effect of the withholding reform on outcome y_{ft} between high and low DCC usage firms. As in the main specification, γ_f are firm fixed effects, and $\theta_{tis(f)}$ denotes time-industry-size fixed effects. ν_{ft} is an error term, and standard errors are clustered at the firm level.

We begin the empirical analysis documenting the immediate effects of the reform - how the reform affected the amounts of withholding, unrefunded credits, and total taxes paid for firms with high exposure to digital payment providers. We then investigate whether these changes impacted real economic outcomes of firms, such as profits, investment, and wage bills. We also conduct heterogeneity analyses to assess whether effects were differential according to other pre-reform firm characteristics, such as the degree of liquidity constraints.

4.4 Compliance effects of the reform

First stage effects. We start by documenting several immediate effects of the reform, which show that the policy caused important changes in unrefunded credits and tax payments for affected firms. In Figure 7 we present the first set of results for our balanced sample of firms with high- and low exposure to digital payments pre-reform. Each outcome is normalized by quarterly sales in 2016 and winsorized at the 95th percentile of observations. The left-hand graphs show the average levels of the outcome in every quarter for treated and control groups, while the right-hand side ones show the coefficients for the dynamic DID model as per Equation 1.

In panel (a), we document changes in the total amount withheld by all agents as a share of 2016 quarterly sales. We first note that, consistent with Table 1, firms with high- and low-exposure to digital payment providers were systematically different: high-exposure firms claimed withholding amounts equivalent to 4% of total sales while low-exposure firms' withholding amounted to less than 1%. While there is a small amount of differential trends in the pre-reform period, the two groups diverge substantially in the quarters after the 2017 reform: compared to firms with low exposure, firms in the high-exposure groups see withhold amounts fall by the equivalent of 3 percentage points (p.p.) of pre-reform revenues.

The changes we document in panel (a) are somewhat "mechanical" – they show that the new withholding rates were implemented as expected and caused a decrease in withholding for firms that used digital payments extensively. But *a priori* there is no reason to expect that this reform would change the total amount of VAT taxes paid by firms – the change in withholding rate could simply mean that firms see fewer taxes remitted immediately by payment providers, but compensate remitting more taxes when they file their declarations. This is not what we observe in panels (b), where the outcome variable is total taxes remitted as a share of pre-reform revenue, or panel (c), where the outcome is tax paid as a share of same-period revenues. In both figures, we see that high- and low-exposure firms trend very similarly before the reform, and diverge quickly after – throughout the following two years after the reform, high-exposure firms pay effective rates that are approximately 1.5 - 2 p.p. lower than the control group. These are large effects since average VAT payments before the reform were approximately 8% of revenue.

The reason why firms with high exposure to digital payment providers see a large decrease in their total VAT taxes when withholding rates are reduced in 2017 is illustrated in panels (d) and (e). Those firms become 10 p.p. less likely to declare having unrefunded credits on the extensive margin, and their stock of unrefunded balances falls by about 1 p.p. as a share of pre-reform revenue.

In Table 4 we pool pre and post-treatment periods together and report average treatment effects. In all cases, we document statistically significant responses in the first stage of the VAT reform: coefficients are often smaller than the final dynamic effects we observe due to

the lag for effects to be fully in place, but are overall very consistent with the previous figures. Withholding by digital providers as a share of revenue decreased by 2.5 p.p.; effective rates fall by about 1.5 p,p,; the probability of having unrefunded credits fall by 8.4 p.p. and the stock as a share of revenue by 0.6 p.p..

Additional VAT outcomes. We then turn to evaluate whether these increases in total remitted taxes by firms with high exposure to digital payments also impacted firms' sales and purchases, as reflected by VAT declarations. Here our results are much less clear. In Figure 8, we repeat a similar exercise to previous figures, presenting both levels of outcomes for high- and low-exposure firms, and the coefficients of the dynamic DID. For total and taxable sales, our coefficients are not statistically different from zero, but pre-trends, particularly for total sales, are somewhat concerning – the sales of low-exposure groups seem to be growing faster before the reform, although results are noisy. When we evaluate total and taxable purchases, the post-reform coefficient is positive and statistically different from zero for treated firms, but again pre-trends are not parallel between the two groups, so we do not claim that the post-reform coefficients estimate the causal effects of the cut in withholding rates.

4.5 Effects on firm's performance

In this section, we provide evidence on whether the reform affected firms' performance. Estimates are conducted using the yearly balanced panel dataset from income tax declarations between 2014-2019, so the estimates are at the year and not quarter level. We exploit the rich availability of the data to conduct the analysis of firms' performance including three sets of outcomes: i) balance sheet, ii) investment, and iii) additional outcomes such as wages, cash, and profits.

Effects on Balance Sheet. We start by breaking down firms' assets and liabilities from the balance sheets reported in the income tax forms. As a first attempt to get a general picture of the potential behavioral responses to the reform, we run regressions including the four main components of balance sheets: current assets, non-current assets, current liabilities, and non-current liabilities. Figure 9 describes trends between 2014 and 2019 for both low- and high-exposure firms. In the left-hand graphs, we show the average level of every balance sheet outcome -normalized by sales- relative to the pre-reform year. Raw means suggest two relevant facts. First, trends for both low- and high-exposure firms move in similar trends - even though in different levels - prior to the reduction of the VAT withholding rate in 2017. Second, there are no clear responses in trends after the reform since they remain parallel for all of the outcomes. In the right-hand graphs, we report the point estimates obtained from Equation 1 with 95% confidence intervals in the form of event studies. Difference-in-Difference estimations support the intuition derived from the visual analysis. We cannot reject

the null hypothesis of parallel trends in the periods before the 2017 reform, but also we do not find any statistically significant responses in any of the balance sheet outcomes, except for non-current liabilities which increased about 2% in the year following the reform, but then the response immediately vanished away.

Table 6 reports the point estimates when pooling the post-periods together, as per Equation 2. Results suggest we are not able to reject no differences between high and low-usage firms after the reform. As previously shown by the event studies strategy, non-current liabilities increased by $\approx 2\%$ percentage points (relative to sales in 2016) for treated firms, but when pooling post-treatment periods together, the average effect is not statistically different from zero.

Effects on Investment. Next, we turn to discuss the impacts of the reform on investment. We compute different measures of assets: the book value of gross Property, Plant, and Equipment (PPE), and yearly changes in PPE. This latter is our proposed definition of "investment". We turn from gross to net PPE by subtracting the accumulated book value of depreciation. A caveat about our investment outcomes is that more than 32% and 52% of the high- and low-exposure firms included in our sample, respectively, are in sectors such as wholesale, retail, and general services. This translates into a non-negligible amount of zeros in physical assets reported in the data. In this sense, we study not only the intensive but also the extensive margin of investment. We include the probability of investment as a dummy variable taking the value of 1 if firm f has $\Delta \text{PPE} > 0$ in year t, and zero otherwise. Notice that net/gross PPE might be understood as a stock measure, while changes in net/gross PPE refer to variations in stock.

Figure 10 plots the time series of each of the investment outcomes for both, low- and high-usage firms. Except for the probability of investment, all other continuous outcomes were scaled by the sales in 2016 and trimmed at the 95th fraction of observations. In the left-hand graphs, we report raw means. The book value of gross and net PPE shows more stable trends, which were parallel for both groups of firms before and even after the reform. The lagged values of gross/net value of PPE are more volatile, and so are the two measures for the probability of investment. In addition, the right-hand graphs show the results from estimating Equation 1 along with the error bands from the 95% confidence intervals. In all cases, we were able to confirm the identification hypothesis of pre-trends before the reform introduced in 2017. After that year, results confirm the visual intuition of no changes in the scaled value of gross/net PPE. In the case of gross and net investment, coefficients for the post-treatment period are imprecisely estimated and not different from zero. This is confirmed by the point estimates for the probability of gross/net investment, which turn out to be not statistically significant after the reform.

Table 7 quantifies the effects of the reform on investment by pooling together pre and post-treatment periods, as per Equation 2. Results are consistent with those reported by the

event studies estimates. Once again, we are not able to reject zero differences between high and low usage firms since the Difference-in-Difference estimates (*High usage*×*Post*) are not statistically significant at any standard level. Overall, our results show no sign of any causal effect on investment derived from the reduction in the withholding rate. These null effects of changes in taxes on investment mirror those obtained by Yagan (2015) when studying the dividend tax cut in 2003 in the United States, and more recently by Harju et al. (2022) for small firms in Finland.

Effects on Cash, Wages, and Profits. We also investigate if the cut in the VAT withholding rate affected the levels of cash, wages, and profitability of treated firms. The intuition behind studying these three specific outcomes is that when taxes are withheld at source then the immediate liquidity of firms decreases, and so does their capability to face their costs on time. At some point, reducing the VAT withholding rate should provoke improvements in cash flow, better compensations for workers or even more hiring, and larger markups due to less marginal costs in every transaction, but also because when VAT liabilities are never refunded, firms are in fact under a turnover tax, so reducing the stock of unrefunded balance implies that the marginal income on every transaction should increase. We measure cash flow as the sum of cash, cash at the bank, stock holdings, and other liquid funds. As a proxy of labor, we include employee compensation/wage bills as the sum of deductible and non-deductible labor costs. Pre and after-tax profits correspond to the taxable base in the income tax before and after-tax liabilities, respectively.

Figure 11 displays the time series for the cash flow, wages, and profits. In the left-hand graphs, we report every outcome scaled by sales in 2016 and winsorized at the 95th percentile of the distribution, except for profit margins which are scaled at the 1st and 99th fractions of observations. High-usage firms report larger averages in every outcome, and even though they behaved parallel between 2014 and 2016, after reform there does not seem to be any substantial change in trends. The right-hand graphs report the estimations from Equation 1 with 95% confidence intervals. The pre-trend assumption is satisfied but point estimates do not reject the null hypothesis of zero relative changes after the reform. Table 8 lists the results from pooling post-treatment periods, as per Equation 2. Point estimates from the differences-in-differences are small in magnitude but also not significant at any standard level.

Overall, our results suggest that, after the reform, firms with ex-ante high exposure to digital payment providers did not present significant changes in terms of financial, investment, or real economic activity. These results mirror other findings of null effects of tax cuts on firm investment and growth (Harju et al., 2022; Yagan, 2015). The reasons why these decreases in effective tax do not translate into increased firm activity can be manyfold. As a first approach to unpack our average null effects, in the next section we provide estimates of heterogeneous effects of the reform across several firm dimensions, with a particular focus on firms that

might be liquidity-constrained and for whom changes in cash flow might be more important for determining investment (Bilicka, 2020; Bilicka et al., 2022).

4.6 Firm heterogeneity

Beyond the direct compliance effects of the reform on tax remittance and unrefunded balances, we document no overall responses in firms' performance. Next, we conduct heterogeneity analysis in order to disentangle potential differences in responses across firms' characteristics. We include a vector of firms' traits ($Trait_f$) such as the legal form of the firm (corporate or non-incorporated taxpayers), whether it belongs to one of the three most relevant sectors (wholesale, retail or manufacturing), whether is liquidity-constrained (cash flow to assets ratio below the sample median for the average of the pre-reform period), whether is capital intensive (capital-to-assets ratio above the sample median for the average during years prior to the reform), and a categorical variable for firm size based on four quantiles on average yearly sales prior the reform period (in baseline sample, sales rank from 0.02-1.36 millions of Lempiras in the first quantile to more than 16.3 million Lempiras in the fourth quantile). We then adjust Equation 2 and run triple differences-in-differences models of the form:

$$\begin{aligned} y_{ft} = \gamma_f + \theta_{tis(f)} + \delta_1 HighUsage_f + \delta_2 Post_t + \delta_3 Trait_f + \\ \delta_4\{HighUsage_f \times Post_t\} + \delta_5\{HighUsage_f \times Trait_f\} + \delta_6\{Post_t \times Trait_f\} + \\ \beta\{HighUsage_f \times Post_t \times Trait_f\} + \upsilon_{ft} \end{aligned} \tag{3}$$

Our parameter of interest is β, interpreted as the causal differential response from high-usage firms across traits. Notice that $\theta_{tis(f)}$ is a year-industry-size fixed effect. We exclude the size fixed effects when using quantiles as the firm trait in Equation 3. υ_{ft} is an error term, and standard errors are clustered at the firm level. As in our baseline estimates, every outcome is scaled by firm sales in 2016 (quarterly sales when it comes to VAT outcomes, and year sales for balance sheet variables) and winsorized at the 95th fraction of the observations -except for profit margins, which are trimmed at the 1st and 99th fractions instead.

In Figure 12 and Figure 13 we report point estimates from Equation 3 using the VAT outcomes. Every point estimate -with the exception of the "firm size" results - is obtained separately from an independent regression. We also report the point estimate pooling all firms, as in section 4.4. Across specifications, we mostly fail to reject that effects were homogeneous across the firm corporate form, baseline liquidity or capital-intensive status, economic sector, or firm size. Firms in the largest quartile of baseline revenue size seem to face a larger decrease in taxes remitted and similarly a larger decrease in their stock of unrefunded credits, but results are noisy and not consistent across outcomes.

The panels of Figure 14, Figure 15, and Figure 16 display the heterogeneity analysis using balance sheet outcomes. Again, we observe no pattern of differential effects across a range of firm traits - across the board, while the reform meaningfully decreased firms' total tax burden

and alleviated their stock of unrefunded credit, it seems to have failed to generate additional investment or hiring.

4.7 Robustness

In this section, we provide several exercises to assess the robustness of our results to different specifications. We focus on three exercises. First, we show results using different definitions of panel balancedness and time spans. Second, we consider an alternative definition of high and low usage of digital payments, comparing below- and above-median usage – so we include in the estimates all firms that used digital payments in the year before the reform. Finally, we show how our results change when considering different treatments to deal with outliers in our variables. For the sake of the analysis, we focus on the interaction $\{\mathtt{HighUsage}_f \times \mathtt{Post}_t\}$, as in Equation 2.

Changing Balancedness on Baseline Estimates. Table 9 displays robustness tests for treatment effects on VAT outcomes. Baseline estimates for the VAT are implemented over a quarterly balanced panel between 2015q1 and 2018q4. Then, we conduct robustness checks with a first balanced panel between 2014q1-2018q4, a second one for 2014q1-2019q4, and a third balanced panel of firms regularly filling VAT forms during 2015q1-2019q4. Results are based on estimations at the quarterly level. The magnitude and significance of the "first-stage" effects of the reform are all quite robust: total remitted taxes fall by approximately 1-1.5% of pre-reform sales; the probability of having unrefunded credit falls by 8-10 p.p. and the amount of unrefunded credits by 1-2% of sales. Effects on taxable sales, purchases, or value-added, on the other hand, are mostly non-significant as in our baseline estimates.

Table 10 reports average treatment effects with balance sheet outcomes as dependent variables, using alternative spans of balancedness. Results are at the yearly level. In most of the cases, results are qualitatively consistent with those discussed in subsection 4.5. Across the board, we again see mostly null results and small magnitudes (with the exception of current assets which suggests a negative effect of the reform, but the size and significance of the effect seems very sensitive to outlier treatment).

Changing Treatment and Control Definition. We also consider an alternative definition of high- vs. low-usage of digital payments, instead defining treated firms as those with above median usage of digital payments. This strategy provides us with a larger sample since we now use all firms declaring some usage of digital payments in the year before the reform.

Table 11 reports robustness checks on VAT outcomes[8]. In most cases, results are qualitatively similar to those in the baseline analysis, but first-stage effects are as smaller as we

[8]Because of the presence of outliers, for this specific robustness exercise the "value-added" not only was winsorized at the right tail of the distribution but also in the 0.1st fraction.

might expect - by comparing firms above and below median exposure, the differential effect we observe should be smaller. Still, we estimate that the reform causally reduced unrefunded credits and led to smaller effective tax rates for firms with higher exposure to digital payments. In Table 12 we display robustness checks on firm's performance. Again, our results mostly show null results on firms' investment, wage bills, and profits, suggesting the reform did not spur growth for affected firms.

5 Conclusion

In this paper, we provide new evidence on the prevalence, evolution, and determinants of value-added tax refunds - a key policy lever for the effective functioning of VATs. We first characterize the existence of large unrefunded credits in Honduras, equivalent to more than 1.5% of GDP in 2019, explain its determinants, and highlight the role of withholding mechanisms in generating these refund needs. We then exploit a reform aimed precisely at decreasing this excessive withholding, using a differences-in-difference strategy to estimate the causal effects of the reform on affected firms. Consistent with its goals, we document that the reform decreased the probability of exposed firms having unrefunded credits by over 8 p.p. and decreased effective tax rates by approximately 1-1.5% of pre-reform sales. Despite these significant changes in tax rates faced by firms, we fail to identify real effects on economic activity: not only average changes in investment, sales, or hiring are overall null, but this is also the case across a range of different firm groups we separately consider.

Our results are informative for policy-makers facing similar decisions related to VAT refunds. In the absence of institutional capacity to timely evaluate refund requests, withholding mechanisms with high rates will generate increasing stocks of unrefunded credits. Whether this over-withholding is a deterrent to firm growth and investment, on the other hand, is less clear: in the setting we study, we do not observe an increase in economic activity when over-withholding was curtailed by reform on digital payment providers. Whether this result extends to other settings is likely an important question for future research.

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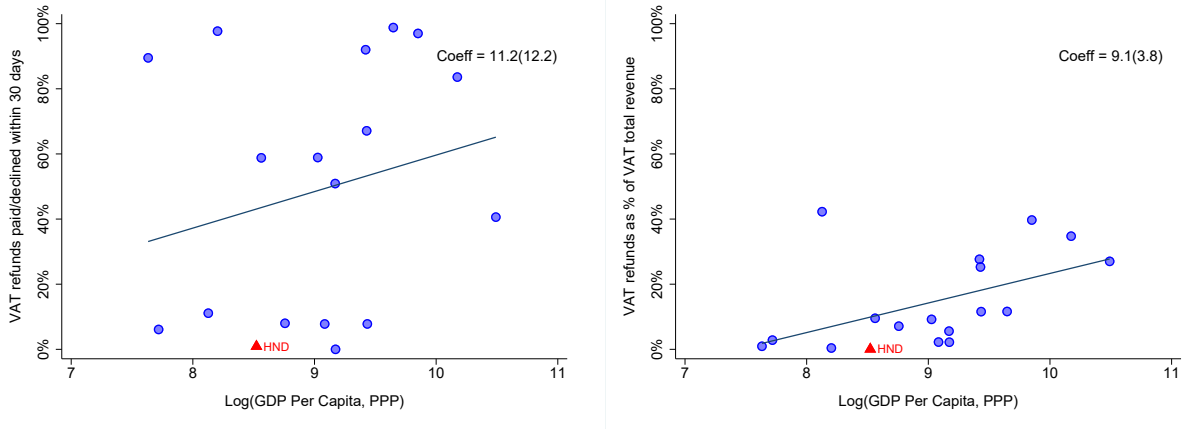
6 Tables and Graphs

Figure 1: VAT refunds across countries

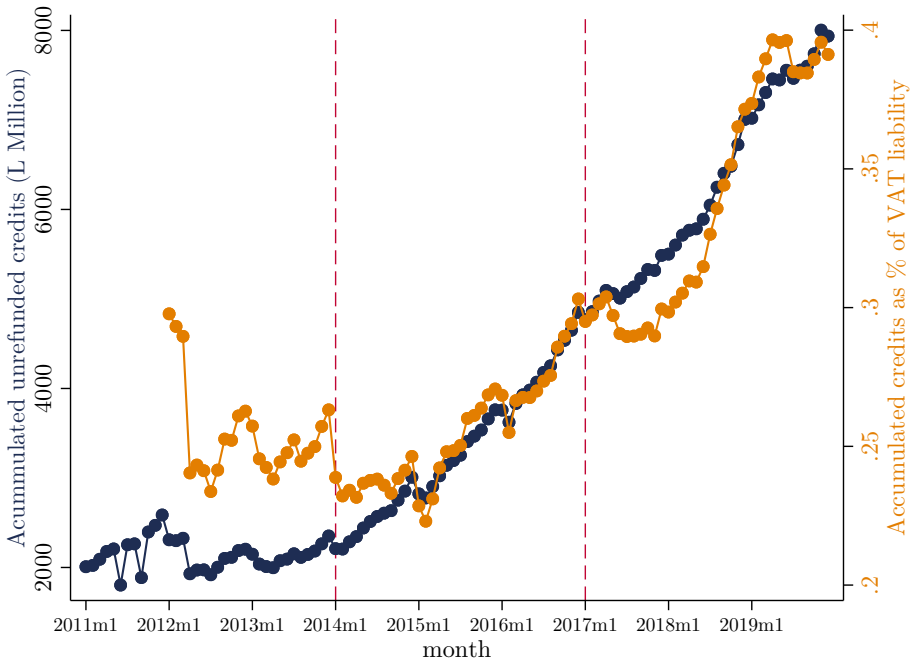


(a) Share of refunds unpaid after 30 days

(b) Refunds and VAT revenue

Note: Own calculations based on TADAT country reports (excluding assessments at the federal level) and the World Bank Open Data. Figure 1a plots the share of refund claims that were paid or declined within 30 days as a percentage of the total VAT refund claims, according to each TADAT country report. Figure 1b plots the VAT refunds effectively paid as a percentage of total VAT revenue (internal plus customs). In both graphs, the values for Honduras correspond to the 2019 fiscal year. Regression coefficients on raw data are presented along with the (robust) standard error in parenthesis.

Figure 2: Unrefunded credit aggregates



Note: This figure reports the sum of accumulated unrefunded credits reported by VAT filers on a monthly basis between January 2011 and December 2019. Accumulated unrefunded credits (reported in the blue line) are defined as the VAT balance (input VAT - output VAT) not refunded from the previous period in millions of Lempiras. The series in orange represent the unrefunded credits sum as a percentage of the sum of the net VAT liability claimed by firms.

Table 1: Descriptive statistics: Full sample of VAT filers

	2015	2016	2017	2018
Panel A: Firm characteristics				
Corporation	0.32	0.31	0.32	0.28
Official size: small	0.98	0.98	0.98	0.99
Sector: Services	0.40	0.41	0.41	0.41
Sector: Retail	0.22	0.22	0.22	0.21
Sector: Manufacturing	0.09	0.09	0.09	0.08
Sector: Wholesale	0.08	0.07	0.07	0.06
Filed yearly income tax	0.89	0.91	0.92	0.90
Panel B: VAT descriptives				
Total revenue (L1,000s)	12,303.5	11,543.0	12,214.3	10,165.9
	(189257)	(161997)	(175820)	(170816)
Taxable Sales (L1,000s)	4,939.4	4,976.2	5,181.8	4,010.7
	(78117)	(81793)	(93037)	(87906)
VAT liability (L1,000s)	747.1	752.7	783.5	605.5
	(12109)	(12676)	(14358)	(13572)
VAT credits (L1,000s)	506.4	506.1	520.9	413.3
	(7618)	(8309)	(8922)	(8669)
Net VAT liability (L1,000s)	240.8	246.6	262.6	192.2
	(5988)	(6039)	(6873)	(6106)
Panel C: Withholding				
Total withholding (L1,000s)	74.1	79.5	54.9	38.6
	(1263)	(1392)	(866)	(775)
Digital payment withholding (L 1,000s)	35.9	38.5	13.6	6.7
	(1035)	(1138)	(376)	(233)
Sales to government withholding (L1,000s)	12.4	12.8	13.4	9.7
	(521)	(567)	(579)	(559)
Large firms withholding (L1,000s)	25.7	28.2	27.8	22.2
	(418)	(456)	(481)	(456)
Total anticipated payments (L1,000s)	2.4	4.3	5.9	5.7
	(120)	(183)	(348)	(353)
Net due VAT payment (L1,000s)	190.7	190.4	214.8	165.2
	(5747)	(5744)	(6568)	(5932)
Claims Digital Payment withholding (%)	10.3	9.5	9.2	6.5
	(30)	(29)	(29)	(25)
Panel D: Unrefunded credit				
Claims positive unrefunded balance (%)	49.0	49.9	48.9	39.8
	(50)	(50)	(50)	(49)
Yearly change in unrefunded balance (L1,000s)	19.4	20.0	9.3	21.8
	(1115)	(694)	(749)	(868)
Unrefunded balance in December (L1,000s)	70.4	79.9	83.1	75.1
	(1083)	(1334)	(1516)	(1680)
Number of firms	57,714	64,895	69,764	98,132

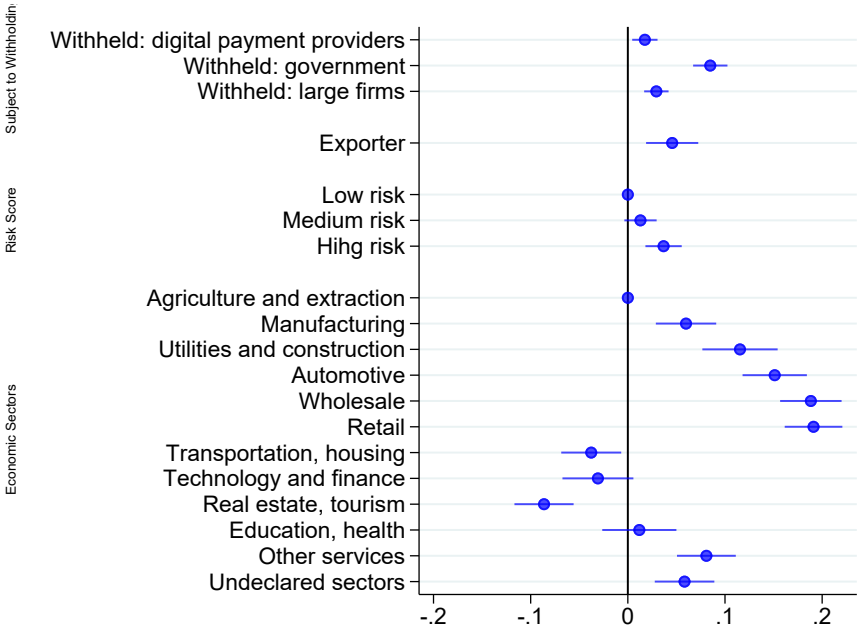
Note: This table reports unconditional means and standard deviations (in parenthesis) for the universe of VAT filers between 2015 and 2018. All monetary aggregates are annualized.

Table 2: How concentrated are unrefunded credits?

	Taxable Sales		Stock unrefunded credits		Flow unrefunded credits	
	Top 50%	Top 80%	Top 50%	Top 80%	Top 50%	Top 80%
2014	149	1,180	81	477	9	22
2015	184	1,542	92	516	10	30
2016	202	1,808	93	545	19	56
2017	187	1,727	91	572	7	14
2018	187	1,801	87	554	17	49
2019	193	1,972	87	601	13	34

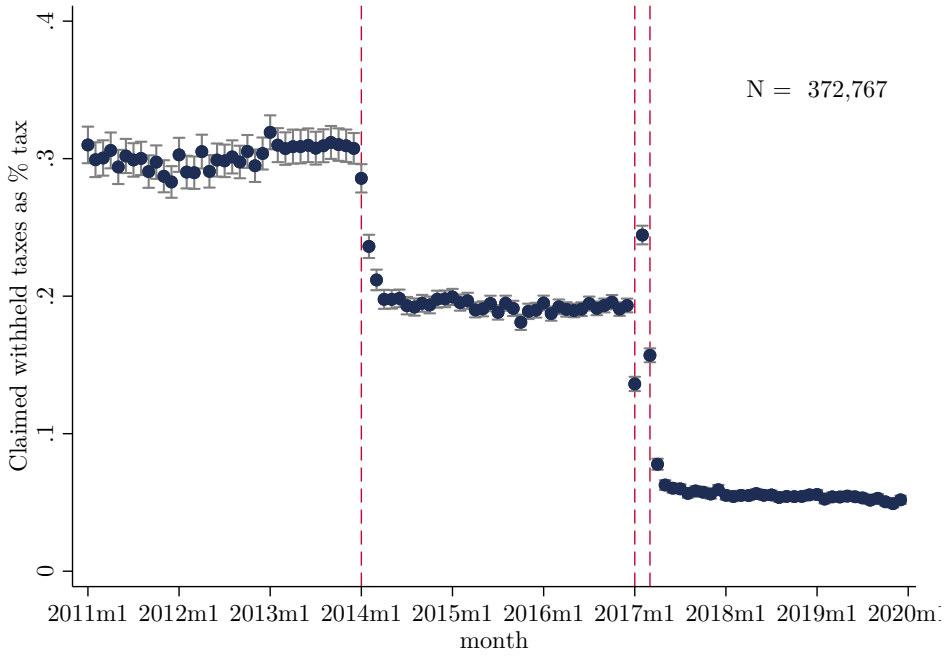
Note: This table reports the number of firms concentrating the highest shares (50% y 80%) in taxable sales, stock of unrefunded credits, and flow of unrefunded credits, respectively. This table was constructed over an unbalanced panel of the universe of VAT fillers between 2014 and 2019.

Figure 3: Correlates of unrefunded credit

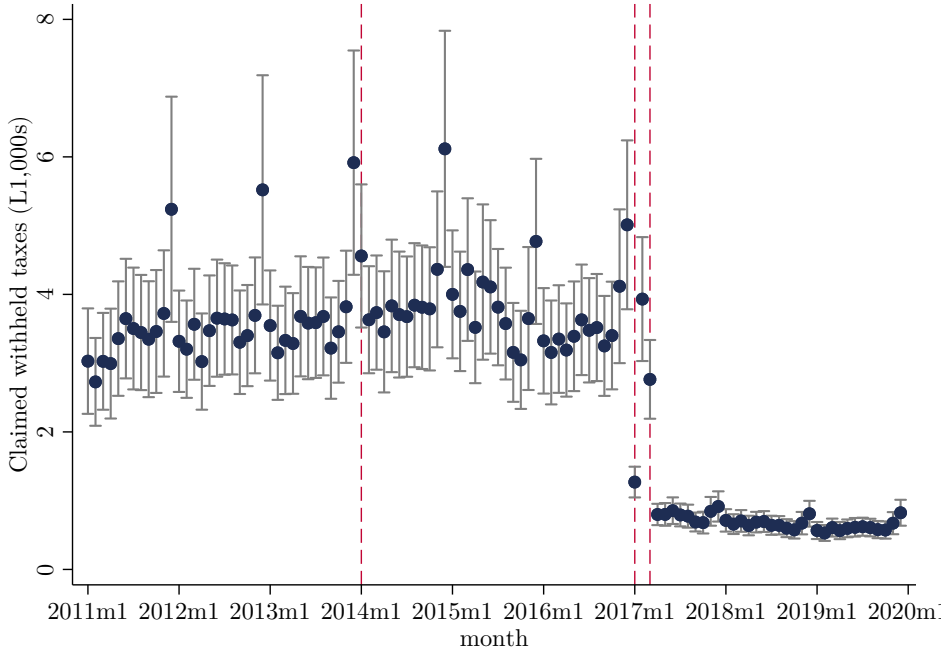


Note: This figure presents coefficients from a linear probability model using a dummy on having unrefunded credits by the end of 2016 as the outcome over a series of covariates. The risk score is computed according to the internal Risk Model of the Honduran tax authority and is defined as a measure that combines both the probability and the monetary consequence of discrepancies and anomalies reported by taxpayers. The regression also includes controls for firm size (not reported in the figure).

Figure 4: Withholding changes - 2011-2019



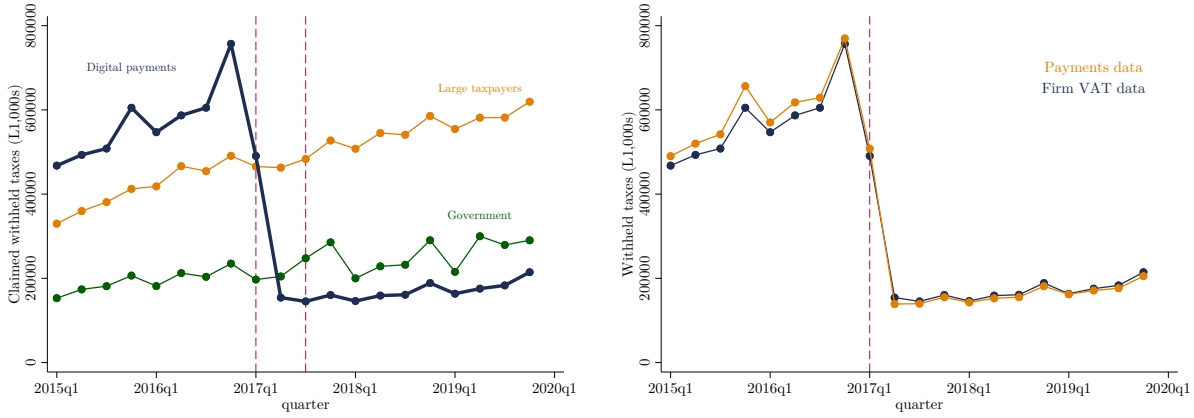
(a) Withholding as a share of tax liability



(b) Withholding in L1,000s

Note: These figures present changes in withholding by digital payment providers around the 2014 and 2017 reforms. Panel A presents the mean (with 95% CI) claimed withholding by payment providers as a share of total tax liability in VAT filings. Panel B presents mean claimed withholding in L1,000s. Dashed lines mark key reform months (January 2014 for the first reform and the transition period January-March 2017 for the second reform). Panel A only includes firms claiming some withholding by digital payment providers.

Figure 5: Total withheld taxes

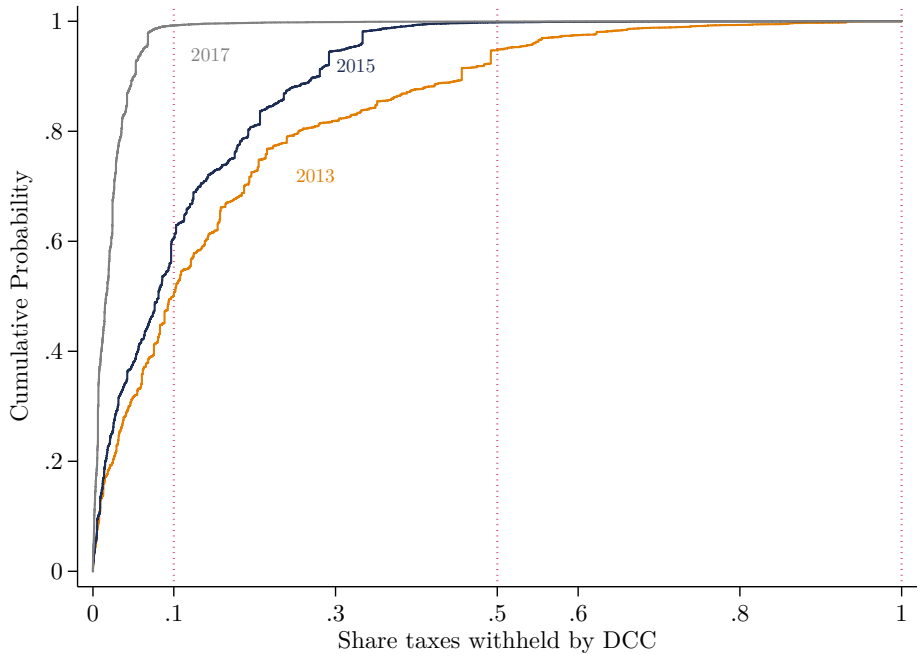


(a) Total withheld taxes by third parties

(b) VAT filing vs. payments data

Note: This panel reports the evolution of withheld taxes. Figure 5a presents the total amount of withholding claimed by VAT filers from digital payment providers, large taxpayers, and the government. Figure 5b presents the total amount of VAT withholding from two separate sources: the VAT filing of firms that claim to withhold and the payments data submitted by digital payment providers independently to the tax authority.

Figure 6: Cumulative distribution function of withholding share



Note: This figure presents cumulative distribution functions for total withholding by digital payment providers as a share of tax liability, for firms claiming withholding. The sample excludes the period January-April in each year in order to maintain consistency across years while excluding the transition period for the 2017 reform.

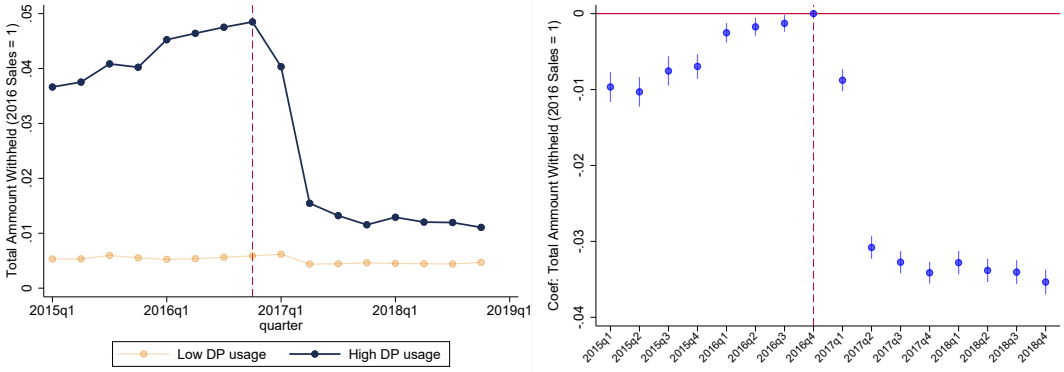
Table 3: Summary statistics in 2016 - Sample for section 4

	Pooled Sample		Low Usage Firms		High Usage Firms	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Firm's Traits						
Corporation	0.59	0.49	0.61	0.49	0.57	0.50
Official size: small	0.94	0.24	0.90	0.30	0.98	0.15
Sector: Retail	0.28	0.45	0.35	0.48	0.21	0.41
Sector: Manufacturing	0.11	0.31	0.14	0.34	0.08	0.28
Sector: Wholesale	0.14	0.35	0.18	0.38	0.12	0.32
Sector: Services	0.37	0.48	0.20	0.40	0.53	0.50
Filed yearly income tax	1.00	0.00	1.00	0.00	1.00	0.00
Panel B: Outcomes for analysis of subsection 4.4						
VAT descriptives						
Total revenue (L1,000s)	45,005	268,699	70,652	306,942	18,003	222,208
Taxable Sales (L1,000s)	30,366	198,101	44,559	238,182	14,043	141,098
VAT liability (L1,000s)	4,564	29,748	6,692	35,729	2,117	21,258
VAT credits (L1,000s)	3,305	24,708	4,968	29,410	1,436	18,494
Net VAT liability (L1,000s)	1,259	7,443	1,724	9,714	681	3,229
Total withholding (L1,000s)	498	4,434	257	1,372	633	5,827
Digital payment withholding (L 1,000s)	406	4,141	100	909	607	5,499
Sales to government withholding (L1,000s)	43	382	84	543	3	47
Large firms withholding (L1,000s)	48	455	72	511	23	380
Total anticipated payments (L1,000s)	20	269	33	378	5	33
Net due VAT payment (L1,000s)	873	6,338	1,530	8,926	199	578
Withholding						
Claims Digital Payment withholding (%)	100	0	100	0	100	0
Digital payment withholding (L 1,000s)	406	4,141	100	909	607	5,499
Unrefunded credit						
Claims positive unrefunded balance (%)	66	47	72	45	61	49
Yearly change in unrefunded balance (L1,000s)	39	1,044	86	1,046	-3	1,003
Unrefunded balance in December (L1,000s)	304	2,198	342	1,982	232	2,262
Observations	2.173		1,056		1,056	
Panel C: Outcomes for analysis of subsection 4.5						
Balance sheet breakdown						
Current assets (L1,000s)	21,946	134,092	39,913	172,630	9,711	88,712
Non-current assets (L1,000s)	24,543	396,123	26,638	201,432	11,739	120,922
Current liabilities (L1,000s)	15,090	99,250	27,482	129,943	6,133	50,121
Non-current liabilities (L1,000s)	6,634	60,703	8,725	48,614	4,203	48,311
Investment						
Gross fixed assets (L1,000s)	20,782	338,992	21,051	136,487	10,753	109,465
Net fixed assets (L1,000s)	10,088	103,712	11,195	54,212	7,345	76,562
Gross investment (L1,000s)	2,482	45,610	4,613	70,523	1,162	11,539
Net investment (L1,000s)	11,839	208,575	14,136	154,540	4,466	43,716
Additional outcomes						
Cash flow (L1,000s)	2,956	26,198	5,028	37,511	1,807	16,062
Wage bill (L1,000s)	4,554	25,932	7,394	29,827	2,617	18,717
Pre-tax profits (L1,000s)	2,133	20,139	4,282	24,382	1,217	14,889
After-tax profits (L1,000s)	1,266	16,179	2,739	17,245	779	10,370
Observations	2,173		883		884	

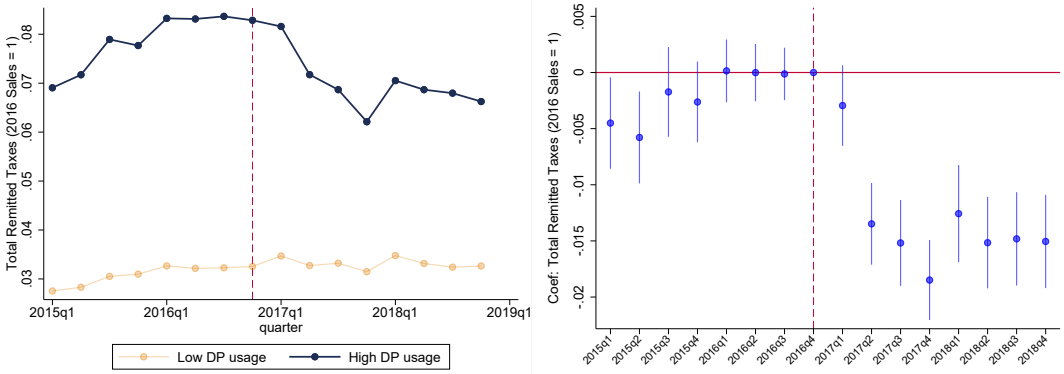
Note: This table reports summary statistics in 2016 for selected samples included in the empirical analysis of the withholding reform. **Panel A** displays statistics on selected traits for all firms included in the analysis (whether they appear in the compliance effects analysis or firm's performance analysis). **Panel B** only includes firms in the balanced panel between 2015q1-2018q4, which drives the analysis of subsection 4.4. Finally, **Panel C** only includes firms in the balanced panel between 2014q1-2019q4 on which analysis of subsection 4.5 is based on.

Figure 7: Event study: Compliance effects on VAT outcomes

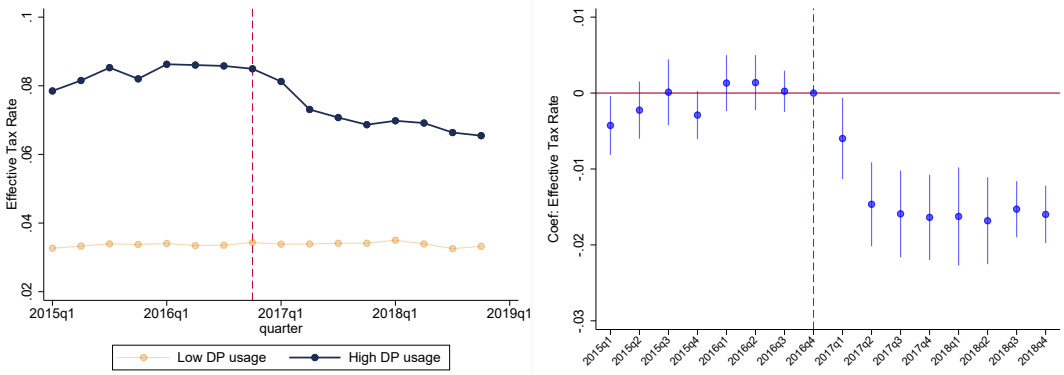
(a) Amounts withheld



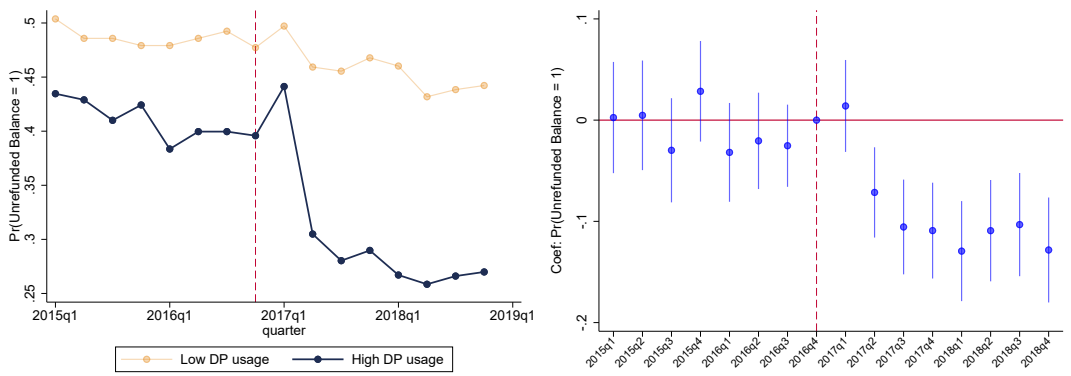
(b) Taxes remmited



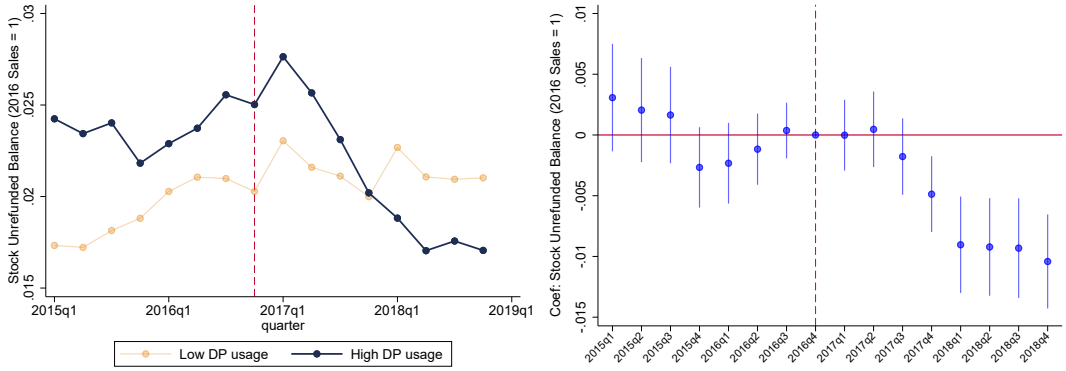
(c) Effective tax rate



(d) Pr(Unrefunded=1)



(e) Unrefunded balance



Note: This panel of figures reports quarterly-event studies for the compliance effects of the VAT withholding reform. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by quarterly sales in 2016 and winsorized at the 95th percentile of observations. The left-hand graphs show the average level of the outcome in every quarter relative to the pre-reform period, for the treated and control group of firms according to DP usage. The right-hand graphs show the coefficients for the dynamic DiD model as per Equation 1. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

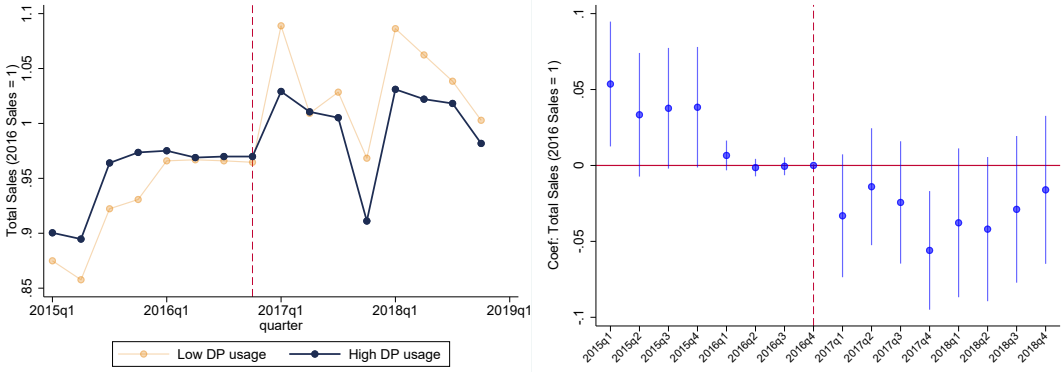
Table 4: Difference-in-Differences: Compliance effects

	(1) Digital Pay Withholding	(2) Remitted taxes	(3) Effective tax rate	(4) Pr(Unrefunded Balance = 1)	(5) Stock unrefunded balance
High usage × Post	-0.025***	-0.012***	-0.014***	-0.084***	-0.006***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Constant	0.024***	0.056***	0.059***	0.433***	0.023***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	33,792	33,792	33,792	33,792	33,792
# Firms	2,112	2,112	2,112	2,112	2,112
R-Squared	0.74	0.70	0.70	0.53	0.74
Mean Dep Var 2016	0.03	0.06	0.06	0.44	0.02
Firm FE?	Yes	Yes	Yes	Yes	Yes
Quarter-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes

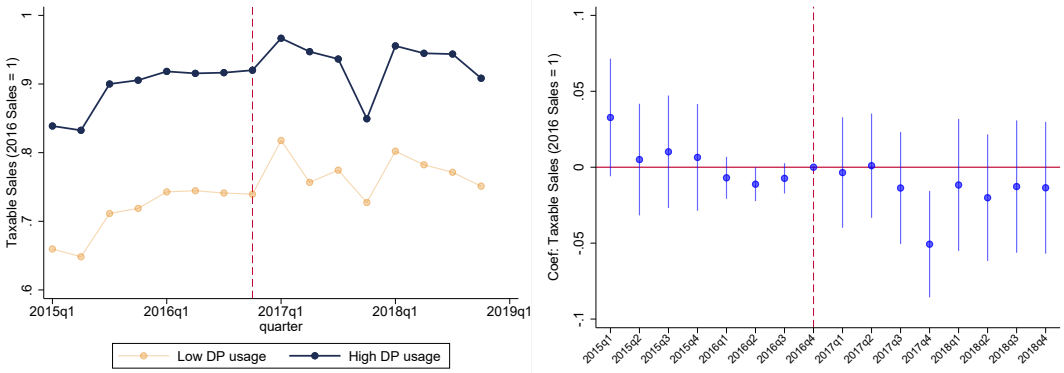
Note: This table reports difference-in-differences quarterly estimates on the first-stage effects of the VAT withholding reform, as per Equation 2. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by the pre-reform (2016) quarterly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Figure 8: Event study: Additional VAT outcomes

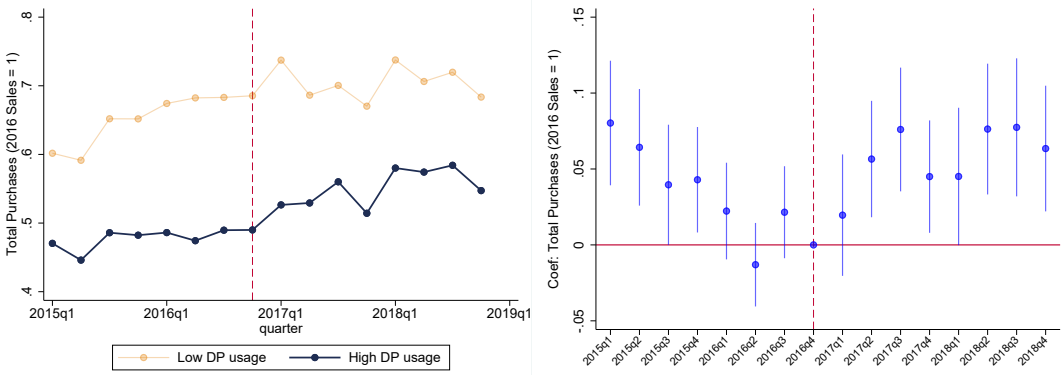
(a) Total sales



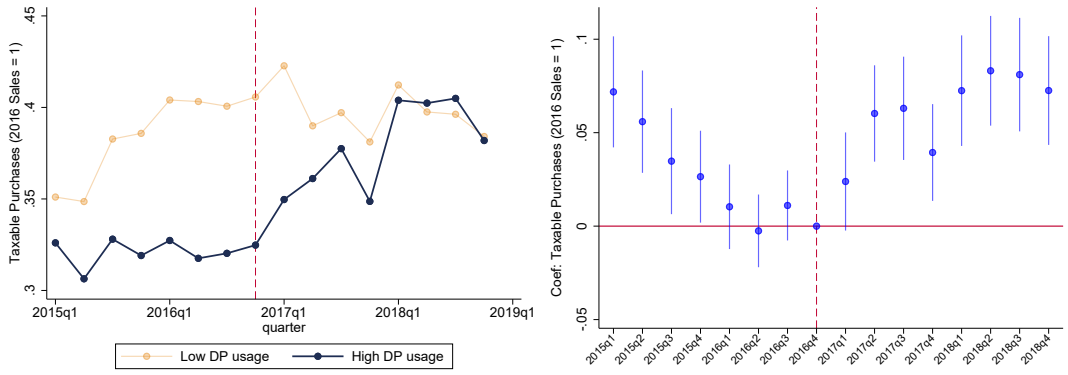
(b) Taxable sales



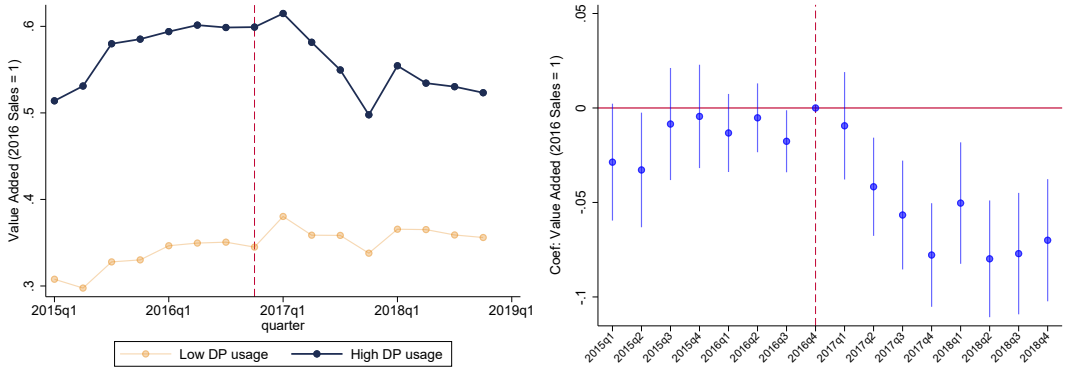
(c) Total purchases



(d) Taxable purchases



(e) Value-Added



Note: This panel of figures reports quarterly-event studies for the effects of the VAT withholding reform on secondary VAT outcomes. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by quarterly sales in 2016 and winsorized at the 95th percentile of observations. The left-hand graphs show the average level of the outcome in every quarter relative to the pre-reform period, for the treated and control group of firms according to DP usage. The right-hand graphs show the coefficients for the dynamic DiD model as per Equation 1. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

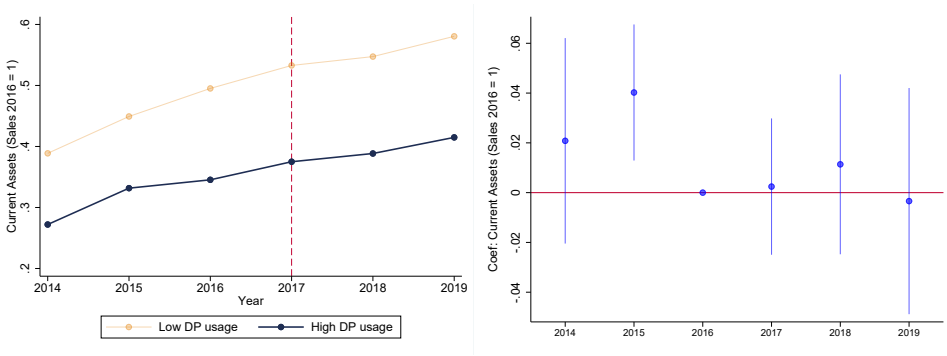
Table 5: Difference-in-Differences: Additional VAT outcomes

	(1) Total sales	(2) Taxable sales	(3) Total purchases	(4) Taxable purchases	(5) Value added
High usage × Post	-0.052**	-0.019	0.025	0.036***	-0.044***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Constant	0.993***	0.833***	0.591***	0.362***	0.465***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	33,792	33,792	33,792	33,792	33,792
# Firms	2,112	2,112	2,112	2,112	2,112
R-Squared	0.28	0.54	0.51	0.66	0.67
Mean Dep Var 2016	0.97	0.83	0.58	0.36	0.47
Firm FE?	Yes	Yes	Yes	Yes	Yes
Quarter-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes

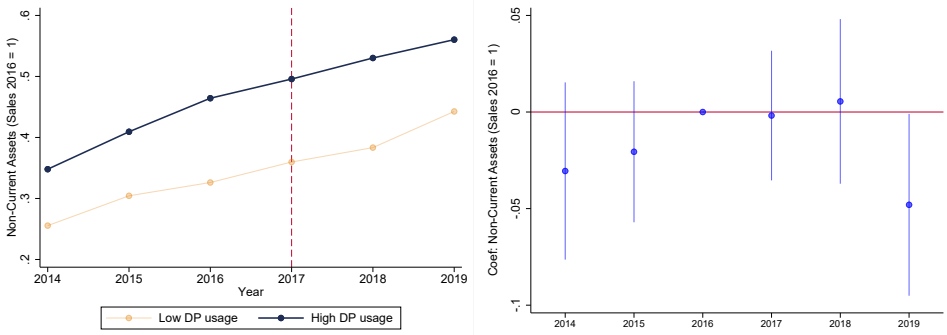
Note: This table reports difference-in-differences quarterly estimates on the effects of the VAT withholding reform on additional VAT outcomes, as per Equation 2. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by the pre-reform (2016) quarterly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Figure 9: Event study: Balance sheet breakdown

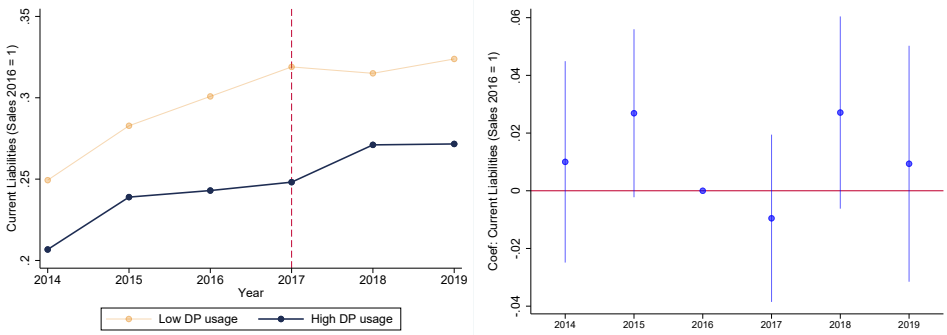
(a) Current assets



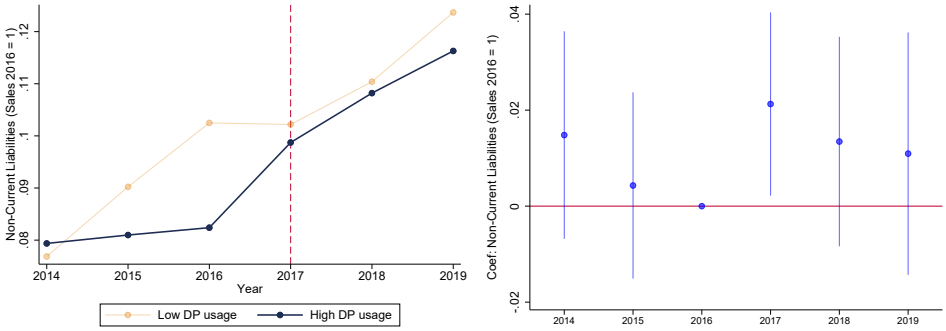
(b) Non-Current assets



(c) Current liabilities



(d) Non-Current liabilities



Note: This panel of figures reports yearly-event studies for the effects of the VAT withholding reform on the balance sheet. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by firm sales in 2016 (the year prior to the withholding reform) and winsorized at the 95th percentile of observations. The left-hand graphs show the average level of the outcome in every year relative to the pre-reform period, for the treated and control group of firms according to DP usage. The right-hand graphs show the coefficients for the dynamic DiD model as per Equation 1. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

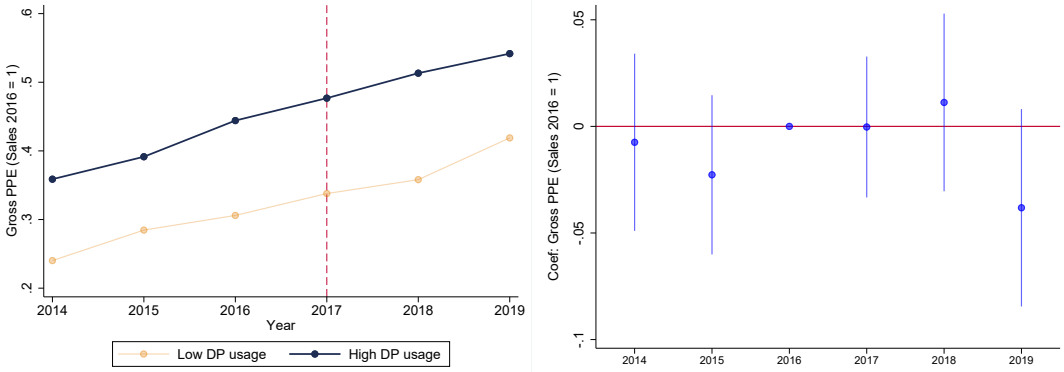
Table 6: Difference-in-Differences: Balance sheet breakdown

	(1) Current Assets	(2) Non-Current Assets	(3) Current Liabilities	(4) Non-Current Liabilities
High usage × Post	-0.017	0.002	-0.003	0.009
	(0.02)	(0.02)	(0.01)	(0.01)
Constant	0.431***	0.406***	0.273***	0.095***
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	10,602	10,602	10,602	10,602
# Firms	1,767	1,767	1,767	1,767
R-Squared	0.79	0.81	0.72	0.63
Mean Dep Var 2016	0.42	0.40	0.27	0.09
Firm FE?	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes

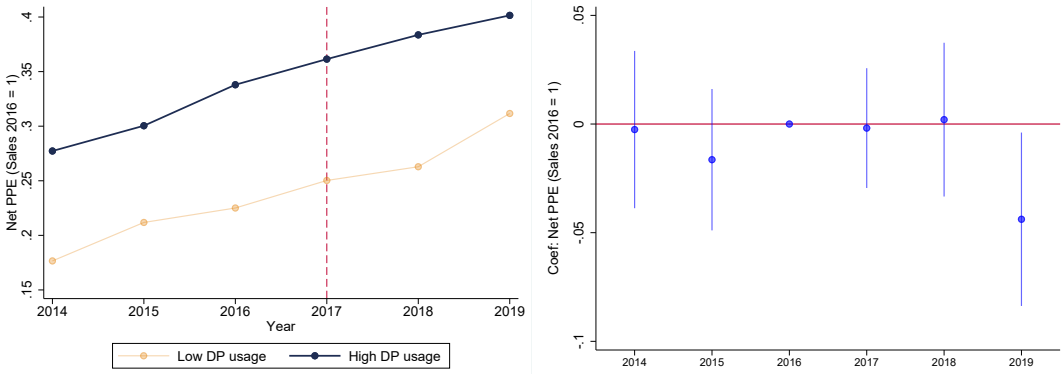
Note: This table reports difference-in-differences yearly estimates on the effects of the VAT withholding reform on balance sheet outcomes, as per Equation 2. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by the pre-reform (2016) sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Figure 10: Event study: Effects on investment

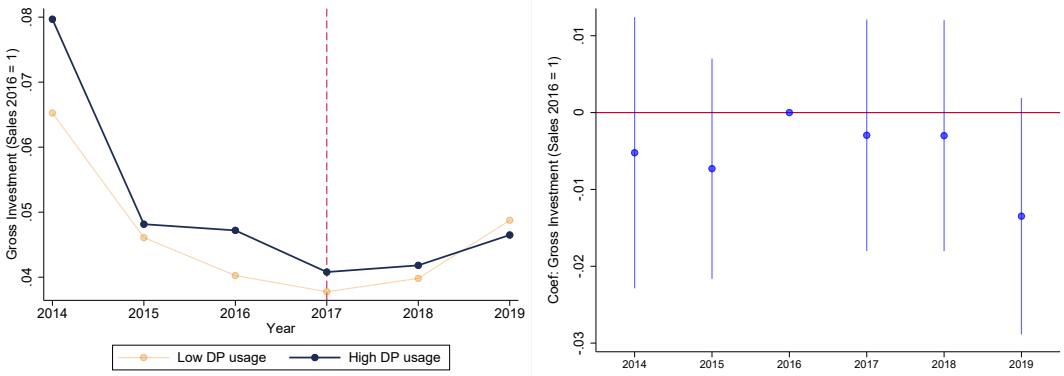
(a) Gross physical assets



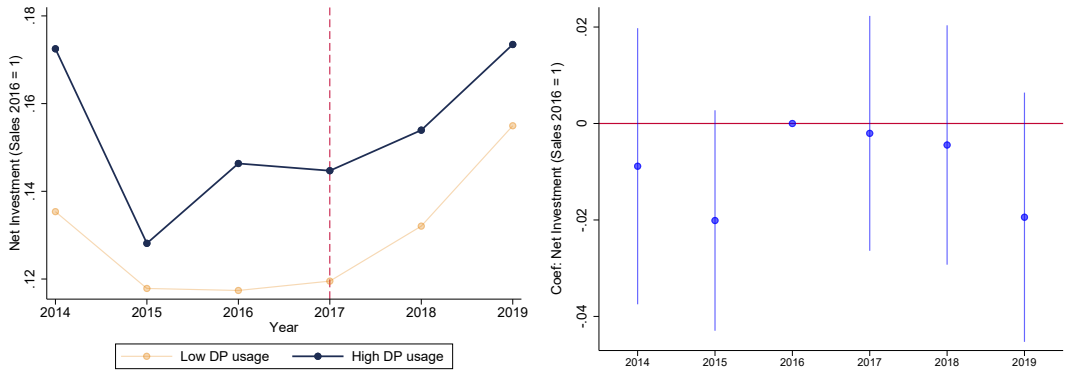
(b) Net physical assets



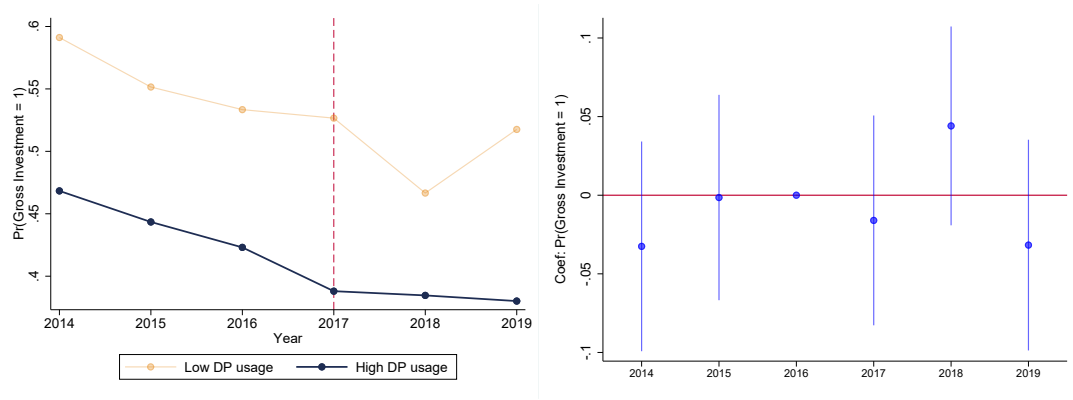
(c) Gross investment



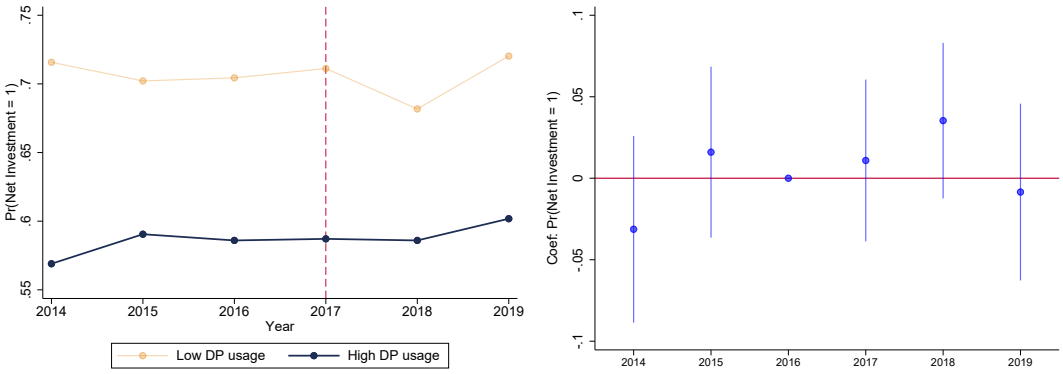
(d) Net investment



(e) Pr(Gross investment=1)



(f) Pr(Net investment=1)



Note: This panel of figures reports yearly-event studies for the effects of the VAT withholding reform on fixed assets and investment. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each of the continuous outcomes was normalized by firm sales in 2016 (the year prior to the withholding reform) and winsorized at the 95th percentile of observations. The left-hand graphs show the average level of the outcome in every year relative to the pre-reform period, for the treated and control group of firms according to DP usage. The right-hand graphs show the coefficients for the dynamic DiD model as per Equation 1. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

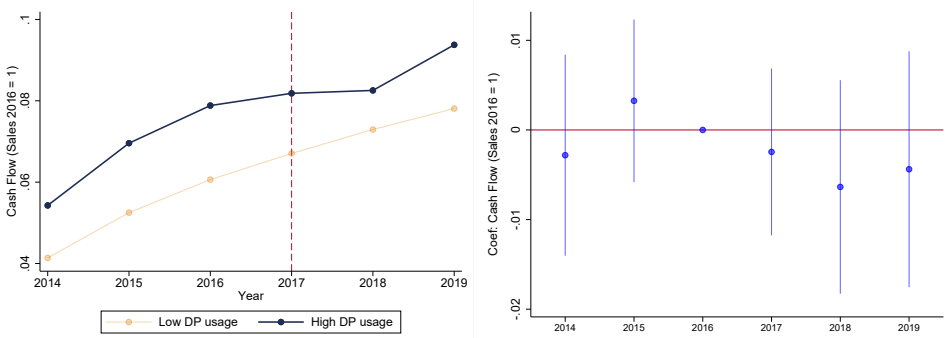
Table 7: Difference-in-Differences: Effects on investment

	(1) Gross PPE	(2) Net PPE	(3) Gross Investment	(4) Net Investment	(5) Pr(Gross investment = 1)	(6) Pr(Net investment = 1)
High usage × Post	0.001	-0.008	-0.002	0.001	0.010	0.018
	(0.02)	(0.02)	(0.00)	(0.01)	(0.02)	(0.02)
Constant	0.389***	0.294***	0.049***	0.141***	0.470***	0.642***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Observations	10,602	10,602	10,602	10,602	10,602	10,602
# Firms	1,767	1,767	1,767	1,767	1,767	1,767
R-Squared	0.82	0.81	0.29	0.57	0.42	0.57
Mean Dep Var 2016	0.38	0.28	0.04	0.13	0.48	0.65
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes

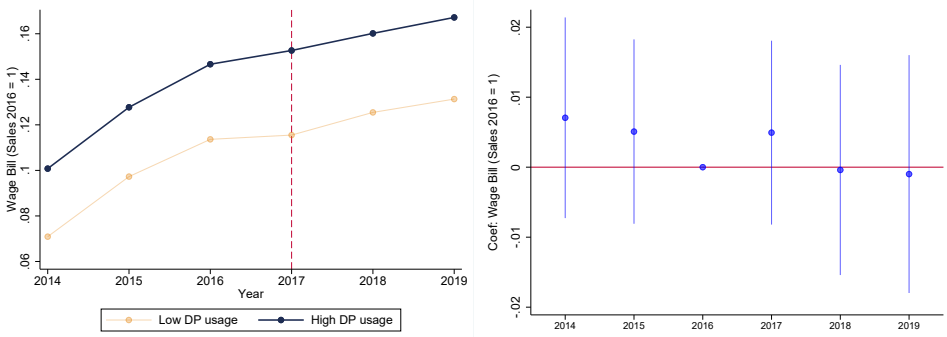
Note: This table reports difference-in-differences yearly estimates on the effects of the VAT withholding reform on fixed assets and investment, as per Equation 2. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome from columns (1) to (4) was normalized by the pre-reform (2016) sales and winsorized at the 95th percentile of observations. Outcomes reported in columns (5) and (6) correspond to a binary variable taking the value of 1 if firm i had investment > 0 in year t, and 0 otherwise. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Figure 11: Event study: Effects on cash flow, employment, and profits

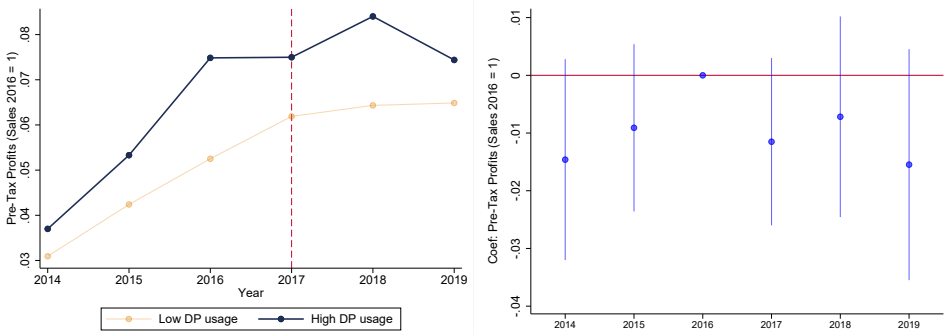
(a) Cash flow



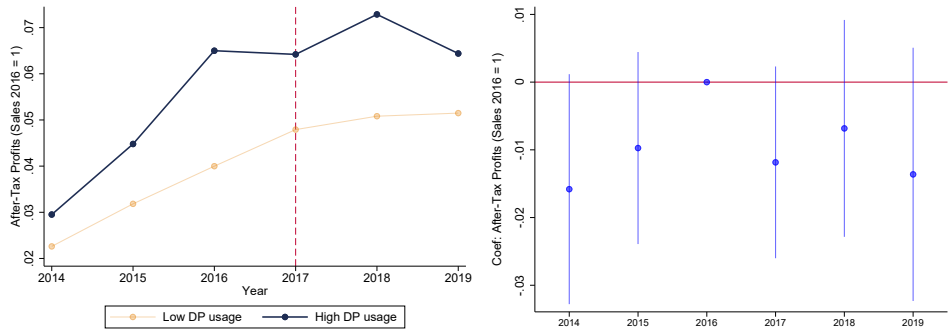
(b) Wage bill



(c) Pre-Tax profits



(d) After-Tax profits



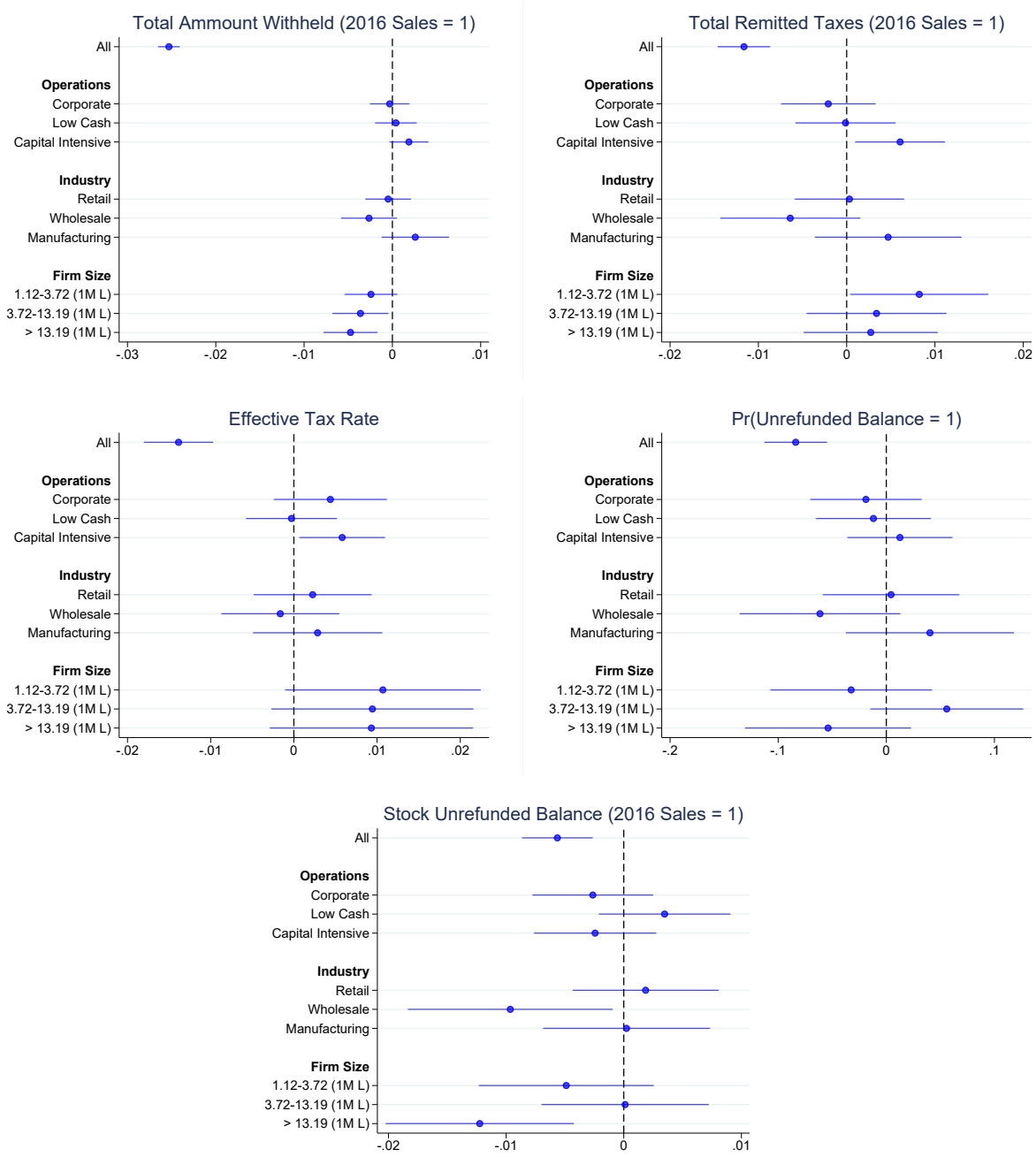
Note: This panel of figures reports yearly-event studies for the effects of the VAT withholding reform on cash flow, wages, and profits. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by firm sales in 2016 (the year prior to the withholding reform) and winsorized at the 95th percentile of observations, except for profits, which are winsorized at the 1st and 99th percentile. The left-hand graphs show the average level of the outcome in every year relative to the pre-reform period, for the treated and control group of firms according to DP usage. The right-hand graphs show the coefficients for the dynamic DiD model as per Equation 1. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

Table 8: Difference-in-Differences: Effects on cash, wages, and profits

	(1) Cash Flow	(2) Wage Bill	(3) Pre-Tax Profits	(4) After-Tax Profits
High usage × Post	-0.005	-0.003	-0.003	-0.002
	(0.00)	(0.01)	(0.01)	(0.01)
Constant	0.071***	0.126***	0.060***	0.049***
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	10,602	10,602	10,602	10,602
# Firms	1,767	1,767	1,767	1,767
R-Squared	0.66	0.61	0.64	0.65
Mean Dep Var 2016	0.07	0.13	0.06	0.05
Firm FE?	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes

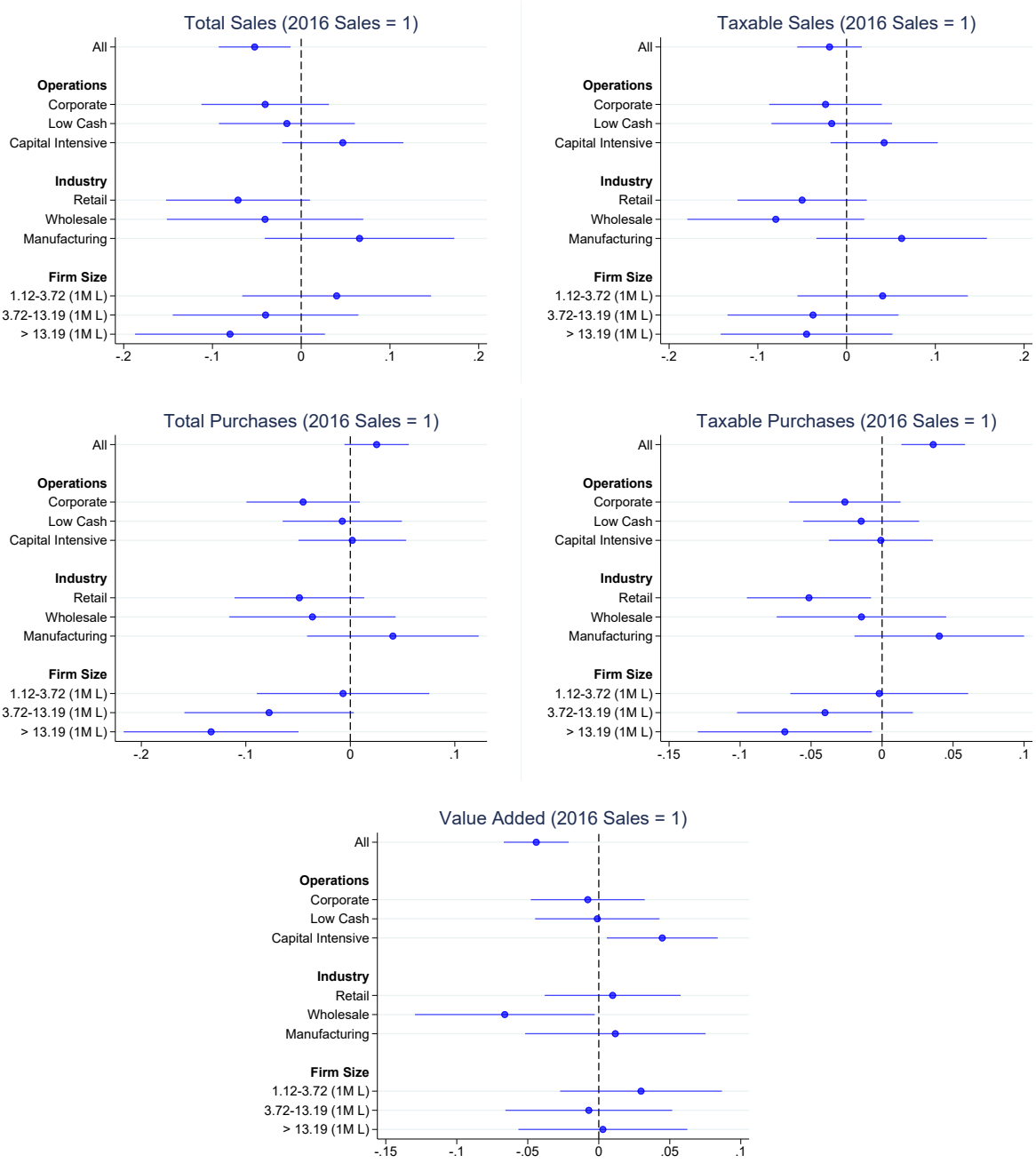
Note: This table reports difference-in-differences yearly estimates on the effects of the VAT withholding reform on cash flow, wages, and profits as per Equation 2. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by the pre-reform (2016) sales and winsorized at the 95th percentile of observations, except for profits, which are winsorized at the 1st and 99th fractions of observations. Regressions also include firm-fixed effects and year-fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Figure 12: Heterogeneity: Compliance effects



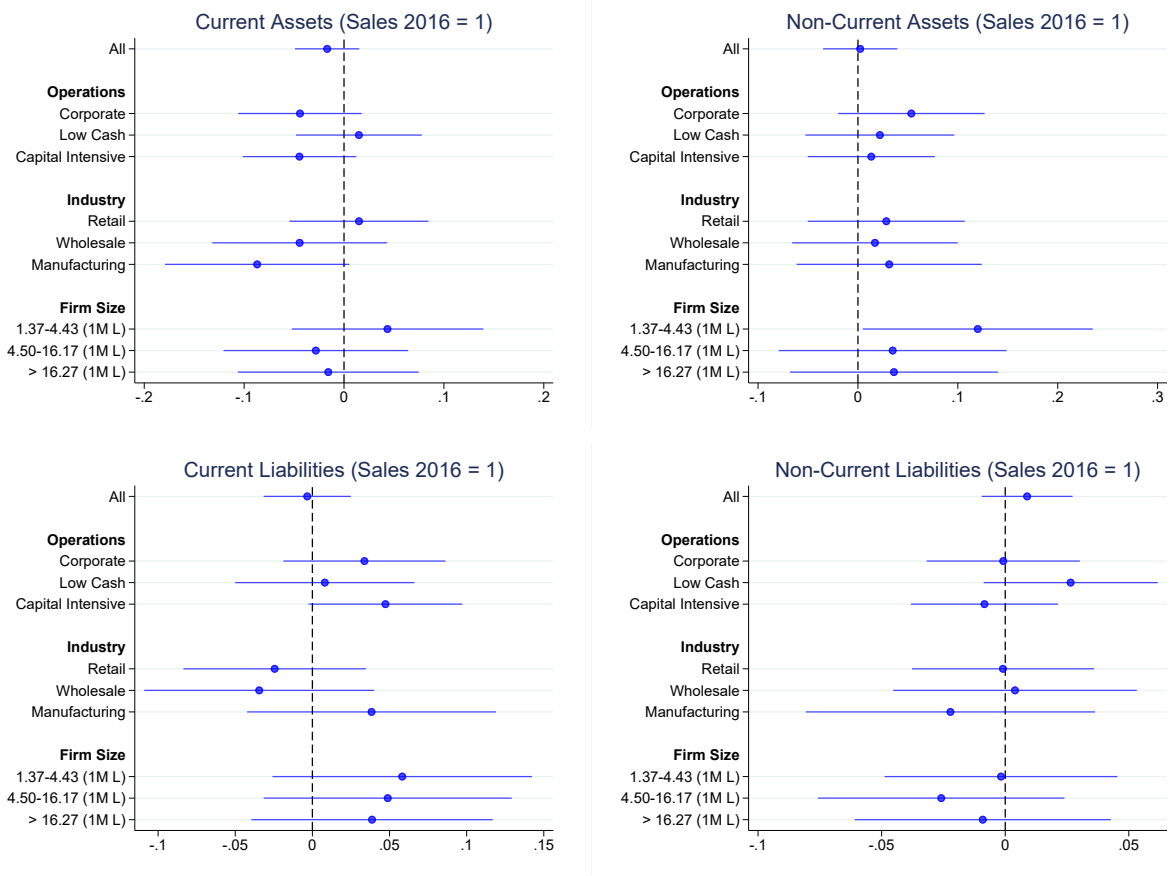
Note: This table reports triple difference-in-differences estimates on heterogeneity analysis of the VAT withholding reform, as per Equation 3. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by the pre-reform (2016) quarterly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Figure 13: Heterogeneity: Additional VAT outcomes



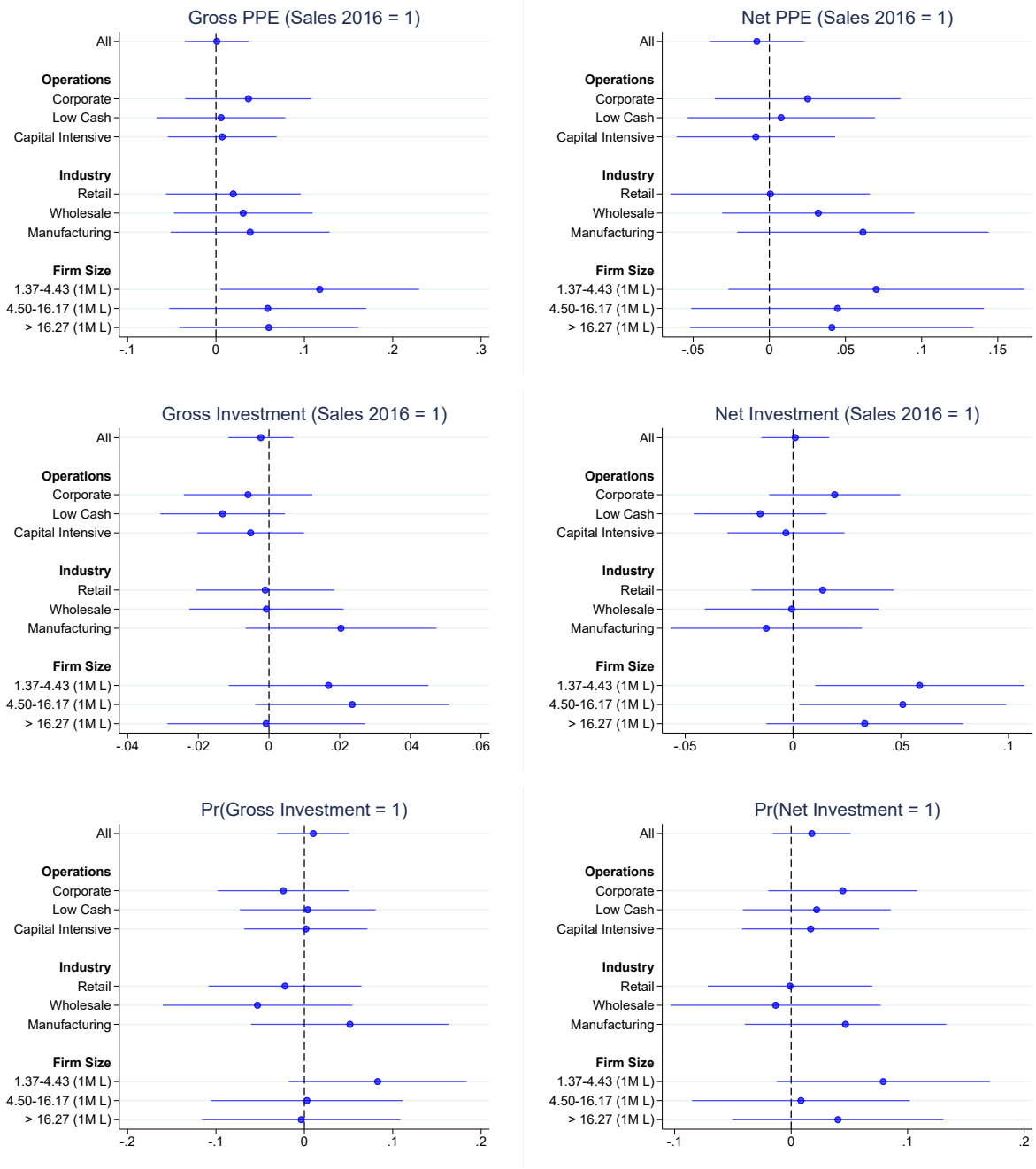
Note: This table reports triple difference-in-differences estimates on heterogeneity analysis of the VAT withholding reform, as per Equation 3. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by the pre-reform (2016) quarterly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Figure 14: Heterogeneity: Balance sheet



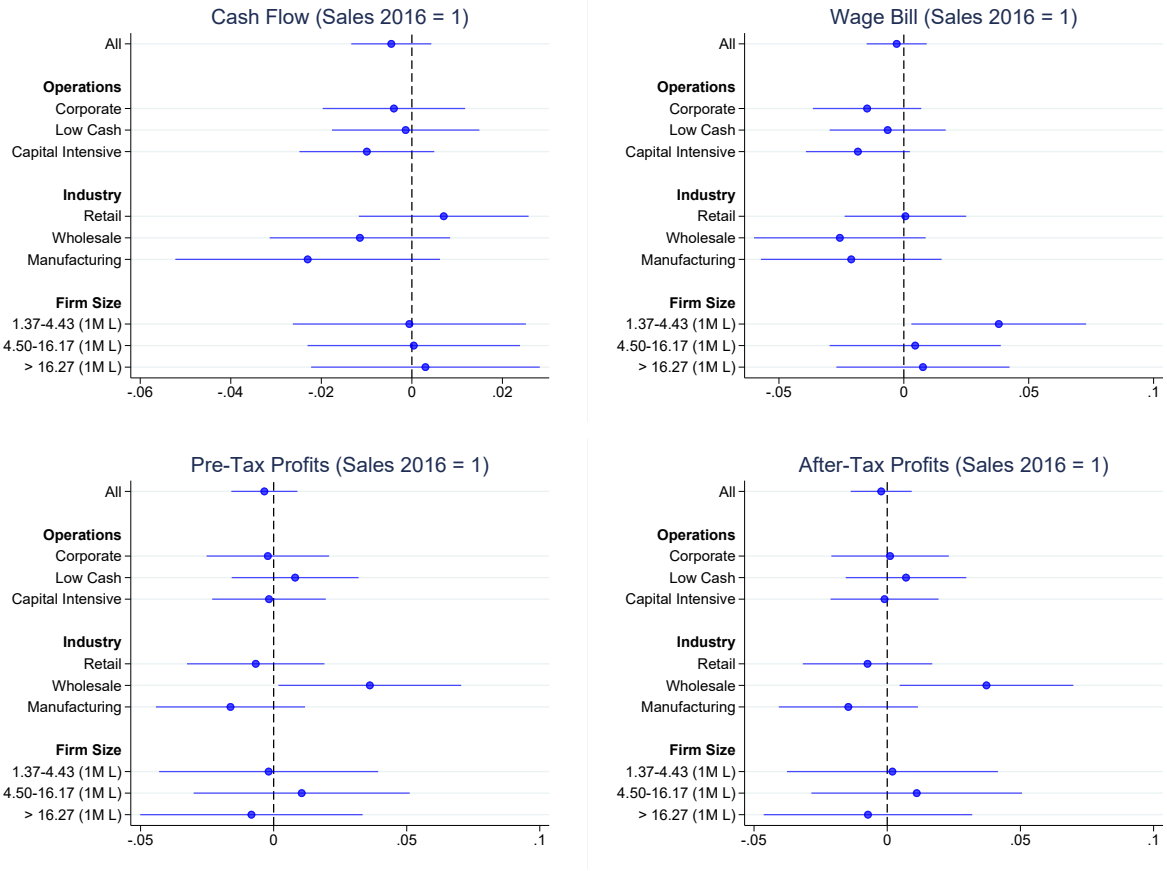
Note: This table reports triple difference-in-differences estimates on heterogeneity analysis of the VAT withholding reform, as per Equation 3. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019, but estimations are made at the year not the quarter level. Each outcome was normalized by the pre-reform (2016) yearly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Figure 15: Heterogeneity: Investment



Note: This table reports triple difference-in-differences estimates on heterogeneity analysis of the VAT withholding reform, as per Equation 3. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019, but estimations are made at the year not the quarter level. Every continuous outcome was normalized by the pre-reform (2016) yearly sales and winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Figure 16: Heterogeneity: Cash, wage bill, and profits



Note: This table reports triple difference-in-differences estimates on heterogeneity analysis of the VAT withholding reform, as per Equation 3. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019, but estimations are made at the year not the quarter level. Every outcome was normalized by the pre-reform (2016) yearly sales and winsorized at the 95th percentile of observations, except for profits, which are winsorized at the 1st and 99th fractions of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Table 9: Robustness check on baseline estimates changing balancedness: VAT outcomes

	Balanced 2014q1 - 2018q4			Balanced 2014q1 - 2019q4			Balanced 2015q1 - 2019q4		
	(1) 99.9th	(2) 99th	(3) 95th	(4) 99.9th	(5) 99th	(6) 95th	(7) 99.9th	(8) 99th	(9) 95th
Total Ammount Withheld	-0.036***	-0.024***	-0.023***	-0.038***	-0.026***	-0.024***	-0.032***	-0.028***	-0.027***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Total Remitted Taxes	-0.096**	-0.010***	-0.010***	-0.134**	-0.009***	-0.010***	-0.036***	-0.012***	-0.012***
	(0.04)	(0.00)	(0.00)	(0.07)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Effective Tax Rate	-0.011***	-0.011***	-0.011***	-0.011***	-0.011***	-0.011***	-0.012***	-0.012***	-0.012***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Pr(Unrefunded Balance = 1)	-0.084***	-0.084***	-0.084***	-0.107***	-0.107***	-0.107***	-0.102***	-0.102***	-0.102***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Stock Unrefunded Balance	-0.016*	-0.011***	-0.008***	-0.022*	-0.018***	-0.011***	-0.023***	-0.013***	-0.009***
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Total Revenue	-1.367**	-0.085***	-0.067***	-1.392**	-0.088**	-0.063**	-0.773***	-0.102***	-0.068***
	(0.59)	(0.03)	(0.02)	(0.65)	(0.04)	(0.03)	(0.29)	(0.03)	(0.02)
Taxable Sales	-1.272**	-0.034	-0.030	-1.237*	-0.029	-0.024	-0.603**	-0.042	-0.031
	(0.58)	(0.03)	(0.02)	(0.64)	(0.03)	(0.02)	(0.26)	(0.03)	(0.02)
Total Purchases	-0.607**	-0.005	0.006	-0.764*	0.001	0.015	-0.366**	-0.002	0.021
	(0.30)	(0.02)	(0.02)	(0.42)	(0.03)	(0.02)	(0.17)	(0.02)	(0.02)
Taxable Purchases	-0.183**	0.003	0.018	-0.212*	0.011	0.024*	-0.099	0.015	0.033***
	(0.09)	(0.02)	(0.01)	(0.11)	(0.02)	(0.01)	(0.06)	(0.02)	(0.01)
Value Added	-0.686*	0.048	0.049	-0.795	0.067	0.067	-0.247**	-0.025	-0.022
	(0.37)	(0.07)	(0.07)	(0.49)	(0.08)	(0.08)	(0.12)	(0.04)	(0.04)
# Observations	37,560	37,560	37,560	42,408	42,408	42,408	39,600	39,600	39,600
# Firms	1,878	1,878	1,878	1,767	1,767	1,767	1,980	1,980	1,980
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports point estimates from independent regressions for the interaction $\{\texttt{HighUsage}_f \times \texttt{Post}_t\}$ on VAT outcomes, as per Equation 2, and according to different samples and winsorizing at alternative fractions of the distribution. Each column is named according to the cut implemented, thus, "99.9th" refers to that outcomes are winsorized at the 99.9th fraction of observations, and so on. Regressions also include firm fixed effects and quarter fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Table 10: Robustness check on baseline estimates changing balancedness: Firm's performance

	Balanced 2014q1 - 2018q4			Balanced 2015q1 - 2018q4			Balanced 2015q1 - 2019q4		
	(1) 99.9th	(2) 99th	(3) 95th	(4) 99.9th	(5) 99th	(6) 95th	(7) 99.9th	(8) 99th	(9) 95th
Current Assets	-0.057*	-0.045**	-0.028*	-0.059**	-0.052***	-0.032**	-0.066**	-0.058***	-0.030*
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)
Non-Current Assets	-0.087	0.008	0.013	-0.112	-0.027	-0.010	-0.182	-0.047	-0.021
	(0.09)	(0.03)	(0.02)	(0.08)	(0.03)	(0.02)	(0.12)	(0.03)	(0.02)
Current Liabilities	0.021	-0.009	-0.003	0.002	-0.016	-0.007	-0.025	-0.026	-0.006
	(0.04)	(0.02)	(0.01)	(0.04)	(0.02)	(0.01)	(0.04)	(0.02)	(0.01)
Non-Current Liabilities	0.046	0.039**	0.009	0.026	0.028*	0.007	0.029	0.032*	0.008
	(0.03)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
Gross PPE	-0.095	0.001	0.009	-0.104	-0.018	-0.006	-0.170	-0.037	-0.014
	(0.09)	(0.03)	(0.02)	(0.08)	(0.03)	(0.01)	(0.12)	(0.03)	(0.02)
Net PPE	-0.045	0.006	0.002	-0.069	-0.017	-0.011	-0.156	-0.037	-0.020
	(0.09)	(0.03)	(0.01)	(0.07)	(0.02)	(0.01)	(0.11)	(0.03)	(0.01)
Gross Investment	-0.043	-0.007	-0.001	-0.029	-0.005	0.000	-0.057*	-0.012	-0.003
	(0.03)	(0.01)	(0.00)	(0.03)	(0.01)	(0.00)	(0.03)	(0.01)	(0.00)
Net Investment	-0.075	-0.002	0.003	-0.031	0.001	0.006	-0.053	-0.007	0.001
	(0.05)	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)
Pr(Gross Investment = 1)	0.024	0.024	0.024	0.014	0.014	0.014	-0.005	-0.005	-0.005
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Pr(Net Investment = 1)	0.016	0.016	0.016	-0.004	-0.004	-0.004	-0.010	-0.010	-0.010
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Cash Flow	-0.008	-0.006	-0.005	-0.018*	-0.013**	-0.007*	-0.016	-0.012	-0.007
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
Wage Bill	-0.014	-0.006	-0.005	-0.009	-0.003	-0.002	-0.012	-0.006	-0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-Tax Profits	0.240	0.243	0.244	0.128	0.130	0.130	0.141	0.145	0.145
	(0.28)	(0.28)	(0.28)	(0.17)	(0.17)	(0.17)	(0.18)	(0.18)	(0.18)
After-Tax Profits	0.257	0.261	0.261	0.141	0.142	0.142	0.156	0.159	0.159
	(0.29)	(0.29)	(0.29)	(0.18)	(0.18)	(0.18)	(0.19)	(0.19)	(0.19)
# Observations	9,390	9,390	9,390	8,448	8,448	8,448	9,900	9,900	9,900
# Firms	1,878	1,878	1,878	2,112	2,112	2,112	1,980	1,980	1,980
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports point estimates from independent regressions for the interaction $\{\texttt{HighUsage}_f \times \texttt{Post}_t\}$ on balance sheet outcomes, as per Equation 2, and according to different samples and winsorizing at alternative fractions of the distribution. Each column is named according to the cut implemented, thus, "99.9th" refers to that outcomes are winsorized at the 99.9th fraction of observations, and so on. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Table 11: Robustness check changing treatment definition: VAT outcomes

	Balanced 2014q1 - 2018q4			Balanced 2014q1 - 2019q4			Balanced 2015q1 - 2018q4			Balanced 2015q1 - 2019q4		
	(1) 99.9th	(2) 99th	(3) 95th	(4) 99.9th	(5) 99th	(6) 95th	(7) 99.9th	(8) 99th	(9) 95th	(10) 99.9th	(11) 99th	(12) 95th
Total Ammount Withheld	-0.019***	-0.015***	-0.014***	-0.019***	-0.016***	-0.014***	-0.019***	-0.017***	-0.016***	-0.019***	-0.017***	-0.016***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Total Remitted Taxes	-0.020***	-0.006***	-0.006***	-0.018***	-0.005***	-0.005***	-0.013***	-0.008***	-0.008***	-0.013***	-0.007***	-0.007***
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Effective Tax Rate	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.007***	-0.007***	-0.007***	-0.008***	-0.008***	-0.007***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Pr(Unrefunded Balance = 1)	-0.053***	-0.053***	-0.053***	-0.061***	-0.061***	-0.061***	-0.052***	-0.052***	-0.052***	-0.057***	-0.057***	-0.057***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Stock Unrefunded Balance	-0.010*	-0.006***	-0.005***	-0.015***	-0.010***	-0.006***	-0.009**	-0.006***	-0.004***	-0.016***	-0.009***	-0.005***
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Total Revenue	-0.373***	-0.058***	-0.045***	-0.339***	-0.064***	-0.047***	-0.215***	-0.066***	-0.048***	-0.247***	-0.067***	-0.045***
	(0.14)	(0.02)	(0.01)	(0.13)	(0.02)	(0.02)	(0.07)	(0.02)	(0.01)	(0.08)	(0.02)	(0.01)
Taxable Sales	-0.267**	-0.020	-0.021	-0.243**	-0.021	-0.022	-0.164**	-0.029*	-0.025**	-0.180**	-0.023	-0.019
	(0.12)	(0.02)	(0.01)	(0.12)	(0.02)	(0.01)	(0.07)	(0.02)	(0.01)	(0.07)	(0.02)	(0.01)
Total Purchases	-0.177**	-0.009	-0.002	-0.176**	-0.013	-0.001	-0.084*	-0.006	0.006	-0.123**	-0.007	0.010
	(0.08)	(0.01)	(0.01)	(0.08)	(0.02)	(0.01)	(0.05)	(0.01)	(0.01)	(0.05)	(0.01)	(0.01)
Taxable Purchases	-0.054	-0.003	0.005	-0.050	-0.003	0.004	-0.029	0.005	0.013*	-0.036	0.007	0.015**
	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
Value Added	-0.118**	-0.021*	-0.023***	-0.108**	-0.020	-0.023**	-0.087***	-0.037***	-0.037***	-0.082***	-0.032***	-0.032***
	(0.05)	(0.01)	(0.01)	(0.05)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
# Observations	75,100	75,100	75,100	84,816	84,816	84,816	67,600	67,600	67,600	79,240	79,240	79,240
# Firms	3,755	3,755	3,755	3,534	3,534	3,534	4,225	4,225	4,225	3,962	3,962	3,962
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports point estimates from independent regressions for the interaction $\{\texttt{HighUsage}_f \times \texttt{Post}_t\}$ on VAT outcomes, as per Equation 2, and according to different samples and winsorizing at alternative fractions of the distribution. Estimations differ from baseline estimates in the sense that treatment and control groups are defined as firms with DCC usage above and below the median in 2016, respectively. Each column is named according to the cut implemented, thus, "99.9th" refers to that outcomes are winsorized at the 99.9th fraction of observations, and so on. Because of the presence of outliers, for this specific exercise the "value-added" not only was winsorized at the right tail of the distribution but also in the 0.1st fraction. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Table 12: Robustness check changing treatment definition: Firm's performance

	Balanced 2014q1 - 2018q4			Balanced 2014q1 - 2019q4			Balanced 2015q1 - 2018q4			Balanced 2015q1 - 2019q4		
	(1) 99.9th	(2) 99th	(3) 95th	(4) 99.9th	(5) 99th	(6) 95th	(7) 99.9th	(8) 99th	(9) 95th	(10) 99.9th	(11) 99th	(12) 95th
Current Assets	-0.039**	-0.035**	-0.023**	-0.038*	-0.036**	-0.023**	-0.045***	-0.042***	-0.027***	-0.045**	-0.044***	-0.029***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Non-Current Assets	-0.039	-0.039	-0.039	-0.833	-0.833	-0.833	1.527	1.527	1.527	0.920	0.920	0.920
	(1.83)	(1.83)	(1.83)	(1.79)	(1.79)	(1.79)	(2.11)	(2.11)	(2.11)	(2.35)	(2.35)	(2.35)
Current Liabilities	-0.030	-0.030	-0.030	-2.402**	-2.402**	-2.402**	-1.776	-1.776	-1.776	-3.637*	-3.637*	-3.637*
	(3.29)	(3.29)	(3.29)	(1.11)	(1.11)	(1.11)	(3.54)	(3.54)	(3.54)	(2.15)	(2.15)	(2.15)
Non-Current Liabilities	1.441*	1.441*	1.441*	1.372	1.372	1.372	1.222	1.222	1.222	1.381	1.381	1.381
	(0.85)	(0.85)	(0.85)	(0.96)	(0.96)	(0.96)	(0.81)	(0.81)	(0.81)	(0.91)	(0.91)	(0.91)
Gross PPE	0.484	0.484	0.484	-0.479	-0.479	-0.479	1.903	1.903	1.903	1.232	1.232	1.232
	(1.34)	(1.34)	(1.34)	(1.22)	(1.22)	(1.22)	(1.78)	(1.78)	(1.78)	(1.93)	(1.93)	(1.93)
Net PPE	0.132	0.132	0.132	-0.731	-0.731	-0.731	0.932	0.932	0.932	0.110	0.110	0.110
	(0.97)	(0.97)	(0.97)	(0.82)	(0.82)	(0.82)	(1.17)	(1.17)	(1.17)	(1.11)	(1.11)	(1.11)
Gross Investment	0.287	0.287	0.287	-0.411	-0.411	-0.411	0.407	0.407	0.407	-0.172	-0.172	-0.172
	(0.78)	(0.78)	(0.78)	(0.42)	(0.42)	(0.42)	(0.68)	(0.68)	(0.68)	(0.42)	(0.42)	(0.42)
Net Investment	0.456	0.456	0.456	-0.246	-0.246	-0.246	1.184	1.184	1.184	0.750	0.750	0.750
	(1.07)	(1.07)	(1.07)	(0.78)	(0.78)	(0.78)	(1.01)	(1.01)	(1.01)	(0.90)	(0.90)	(0.90)
Pr(Gross Investment = 1)	-0.002	-0.002	-0.002	-0.009	-0.009	-0.009	-0.006	-0.006	-0.006	-0.012	-0.012	-0.012
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pr(Net Investment = 1)	-0.006	-0.006	-0.006	-0.003	-0.003	-0.003	-0.017	-0.017	-0.017	-0.010	-0.010	-0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Cash Flow	-0.250	-0.250	-0.250	-0.393	-0.393	-0.393	0.468	0.468	0.468	0.492	0.492	0.492
	(0.43)	(0.43)	(0.43)	(0.36)	(0.36)	(0.36)	(0.68)	(0.68)	(0.68)	(0.84)	(0.84)	(0.84)
Wage Bill	0.182	0.182	0.182	0.010	0.010	0.010	0.233	0.233	0.233	0.027	0.027	0.027
	(0.37)	(0.37)	(0.37)	(0.39)	(0.39)	(0.39)	(0.32)	(0.32)	(0.32)	(0.33)	(0.33)	(0.33)
Pre-Tax Profits	0.103	0.103	0.104	0.108	0.108	0.109	0.050	0.052	0.053	0.061	0.062	0.063
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
After-Tax Profits	0.110	0.110	0.111	0.116	0.116	0.117	0.056	0.057	0.058	0.067	0.068	0.069
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
# Observations	18,775	18,775	18,775	21,204	21,204	21,204	16,900	16,900	16,900	19,810	19,810	19,810
# Firms	3,755	3,755	3,755	3,534	3,534	3,534	4,225	4,225	4,225	3,962	3,962	3,962
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

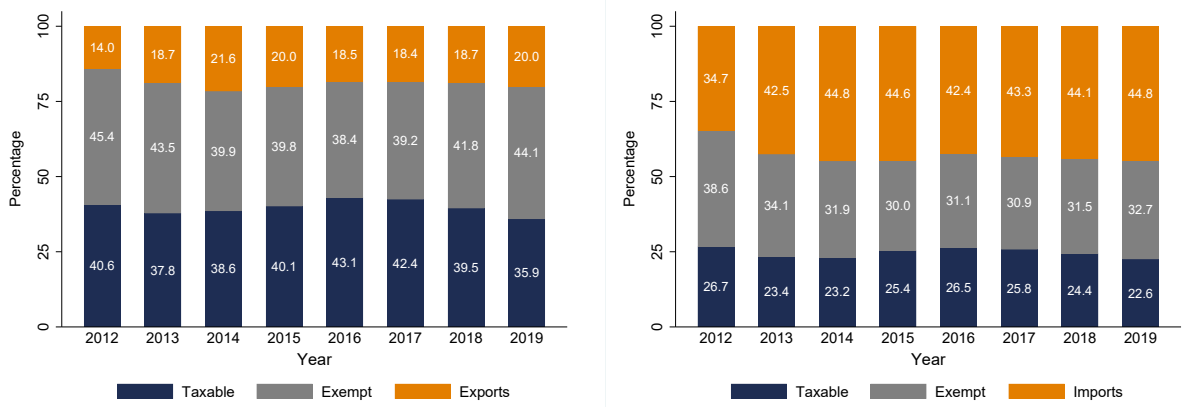
Note: This table reports point estimates from independent regressions for the interaction $\{\texttt{HighUsage}_f \times \texttt{Post}_t\}$ on balance sheet outcomes, as per Equation 2, and according to different samples and winsorizing at alternative fractions of the distribution. Estimations differ from baseline estimates in the sense that treatment and control groups are defined as firms with DCC usage above and below the median in 2016, respectively. Each column is named according to the cut implemented, thus, "99.9th" refers to that outcomes are winsorized at the 99.9th fraction of observations, and so on. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Complementary Material For

"VAT refunds and firms' performance: Evidence from a withholding reform in Honduras"

A Appendix for Section 2

Figure A1: Composition of reported sales and purchases

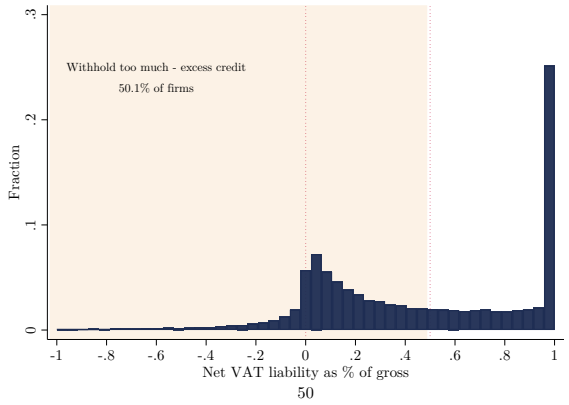


(a) Reported sales

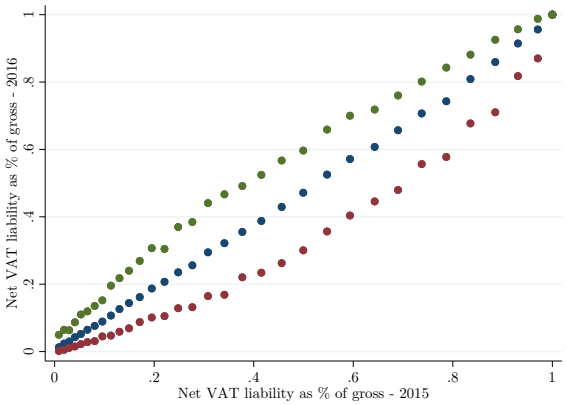
(b) Reported purchases

Note: This figure displays the yearly composition for the sum of sales and purchases reported by firms in the VAT forms. In 2012 and 2013, taxable sales and taxable purchases include transactions levied with the VAT rates of 12% and 15%. After 2014 taxable sales and taxable purchases are levied with the VAT rates of 15% and 18%. Exempt sales and exempt purchases include transactions with "exempt purchase order" (OCE, for the Spanish acronym *Orden de Compra Exenta*). Finally, imports include taxable and exempt imports of goods and services.

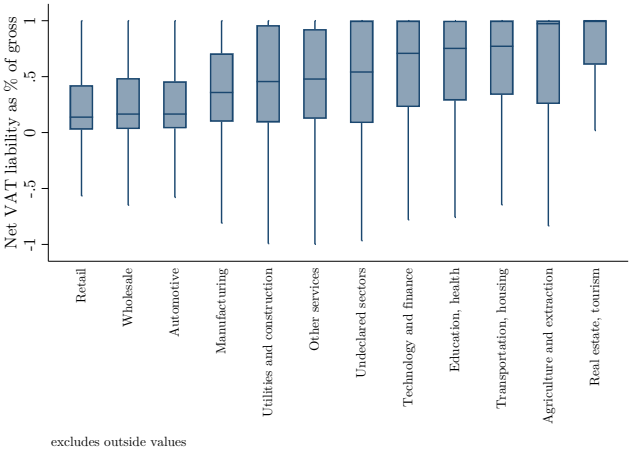
Figure A2: Variation of net VAT liabilities



(a) Distribution for net VAT liabilities



(b) Net VAT liabilities 2015-2016



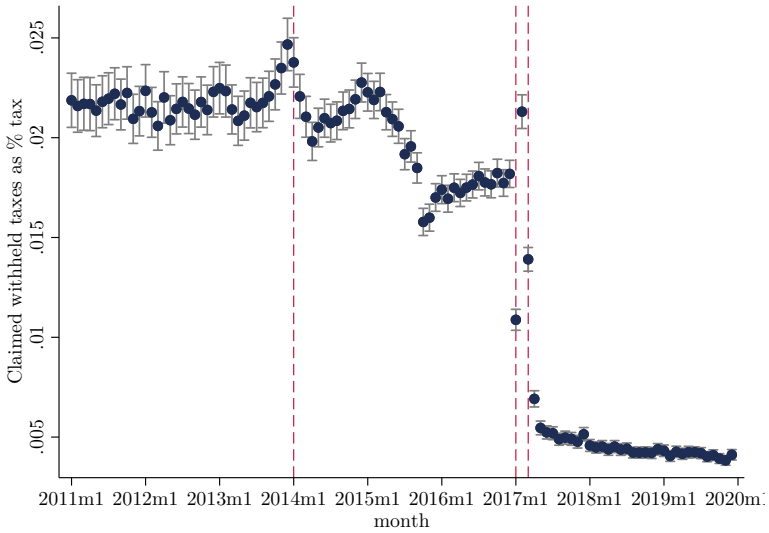
(c) Net VAT liabilities by sector

Note: This panel of figures reports the ratio between net VAT liabilities as a percentage of gross liabilities from different approaches. Figure A2a displays a histogram for the distribution of net VAT liabilities as a percentage of gross liabilities. Figure A2b displays an interval over 50 quantiles for the net VAT liabilities in 2016 as a function of VAT liabilities in 2015. The dots in blue represent the median, and the red and green dots represent the 25th and 75th percentiles, respectively. Figure A2c displays scatter boxes of the net VAT liability as a percentage of gross liabilities across sectors. In figures A2a and A2b, we trimmed the ratio in order to only include values in the range $[-1, 1]$.

B Appendix for Section 4

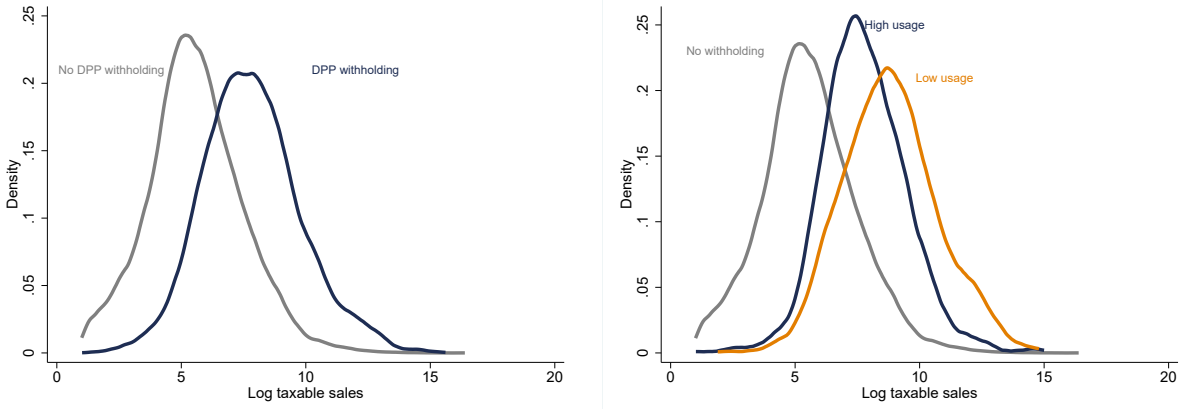
B.1 Additional Figures

Figure A3: Withholding as a share of total due taxes



Note: This figure reports the share of VAT withholdings from DCC payments as a percentage of due taxes between January 2011 and December 2019. The sample excludes filings with monthly sales above L10 million.

Figure A4: Firm size according to debit/credit card usage

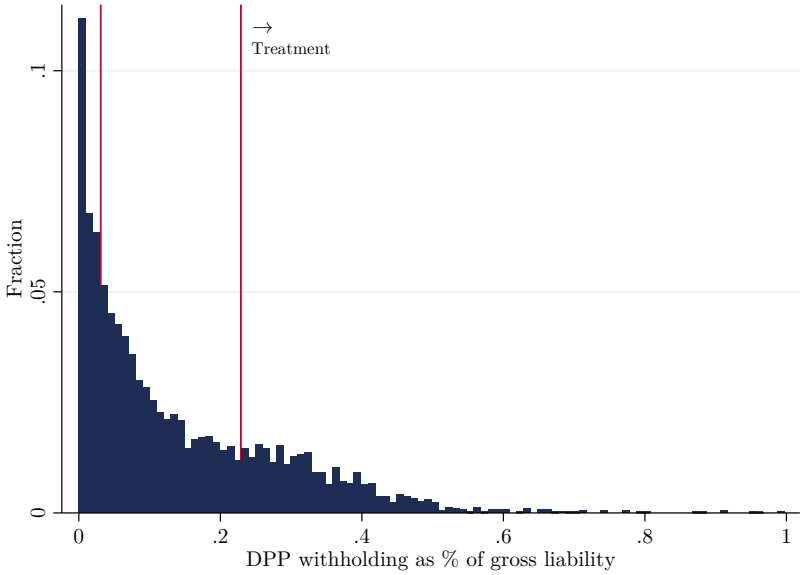


(a) Some usage vs. no usage of DCC operators

(b) Intensity of usage of DCC operator

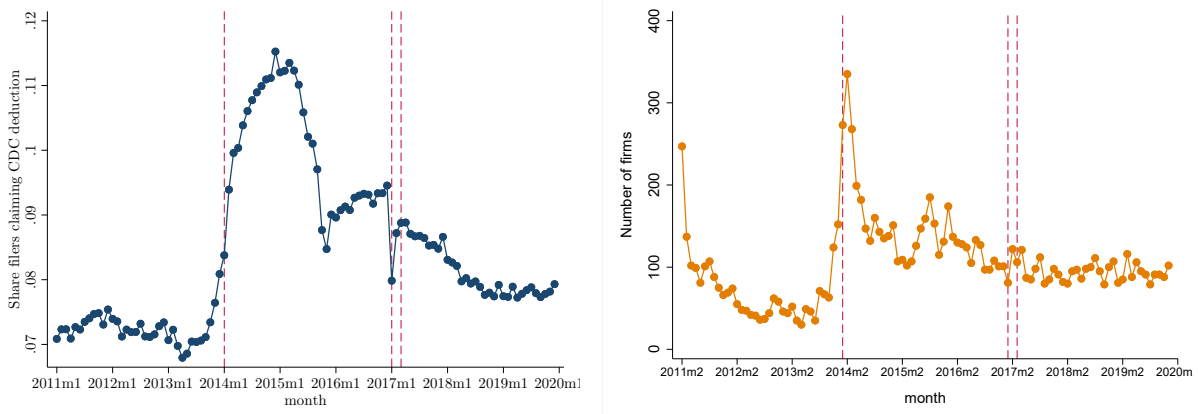
Note: This figure presents the distribution of taxable sales for VAT filers' according to their debit and credit card usage. Figure A4a presents the density for the log of taxable sales in 2016, before the 2017 reform, separately for taxpayers claiming some DCC withholding and for those claiming no withholding. Figure A4b reports the same outcome but separates those claiming DCC withholding between those with claims above the 75th percentile and below the 25th percentile of usage.

Figure A5: DCC usage for treatment and control groups in baseline analysis



Note: This figure displays the distribution of the VAT withholding from debit and credit card payments as a percentage of gross liability. In baseline estimations, we rely on this intensive margin, so the treatment group is defined according to DCC usage above the 75th percentile, while the control group is defined for firms below the 25th percentile of DCC usage.

Figure A6: Number of taxpayers with DCC withholding, 2011-2019



(a) DCC withholding claimers

(b) DCC usage entry

Note: This panel reports dynamics in VAT filers being withheld at source by DCC providers. Figure A6a reports the share of VAT filers claiming DCC withholdings. Figure A6b reports the number of firms claiming DCC withholding for the very first time. Both graphs were obtained from an unbalanced panel dataset of VAT filers between January 2011 and December 2019.

Figure A7: Media coverage of the VAT withholding reform

Note: This panel of pictures shows the media coverage of the VAT withholding reform during 2017 in three of the leading newspapers of Honduras.

B.2 Variables Definition

Empirical analysis of the 2017 withholding reform is based on two different sets of outcomes from two administrative sources[9]: VAT records and balance sheet reported in the income tax. In the first place, VAT records come from SAR-222 and SAR-227 tax forms, which are reported to the tax authority manually and electronically, respectively. They include outcomes such as withholding amounts, taxes remittance, unrefunded balance, and effective tax rates. Also, we conduct additional analysis on second-stage VAT outcomes such as taxable sales, taxable purchases, and VAT value-added. Next, we describe the definition for every VAT outcome implemented in the empirical analysis.

- **Total Amount Withheld.** Sum of the VAT withheld at source by third parties such as the debit and credit card operators, large taxpayers, the government, and airlines. These items correspond to cells # 46, 47, 48, and 70 of the VAT form.
- **Unrefunded Balance.** Sum of the unrefunded balance of VAT credits from the previous period. Corresponds to cell # 45 in the VAT form.
- **Total Remmited Taxes.** Sum of tax liabilities coming from due tax liabilities, the total amount withheld, payments through Official Payment Receipts (*Recibo Oficial de Pago*, ROP), and the total amount of VAT liabilities to be compensated in the period. These correspond to cells # 44, 46, 47, 48, 49, 51, and 70 of the VAT form.
- **Effective tax rates.** Sum of total remitted taxes as a percentage of the firm sales in the corresponding period. After being built, effective tax rates are trimmed to lie in the interval $[-1, 1]$, and missing values were set to zero.
- **Taxable Sales.** Sum of sales conducted in the local market levied with the VAT rates of 15 and 18%, respectively. These items correspond to cells # 24 and 61 in the VAT form.
- **Total Sales.** Sum of taxable sales, exempt sales (in the local market levied with the VAT rates of 15 and 18%, respectively), and exports (conducted in the Central American region or abroad). These items correspond to cells # 23, 24, 25, 26, 27, 61, 62, 125, 126, 127, 129, 130, 131, 132, 226, 227, 230, 231, and 232 of the VAT form.
- **Taxable Purchases.** Sum of purchases conducted in the local market and imports, levied with the VAT rates of 15 and 18%, respectively. These items correspond to cells # 32, 35, 64, and 65 in the VAT form.
- **Total Purchases.** Sum of taxable purchases in the local market or abroad levied with the VAT rates of 15 and 18%, respectively, plus exempt purchases in the local market, and imports conducted in the Central American region or abroad. These items correspond to cells # 23, 31, 32, 33, 34, 35, 36, 37, 64, 65, 133, 134, 136, 137, 138, 139, 233, 236, 237, 238, and 239 of the VAT form.
- **Value-Added.** Is the difference between taxable sales and taxable purchases.

[9]In most of the cases, we rely on concepts provided by DetLive. This is an online platform where the Revenue Administration Service of Honduras provides detailed information to taxpayers on tax forms and filling processes. See: http://detlive.sar.gob.hn/

In the second place, we harmonized personal and income tax records. These correspond to SAR-272 and SAR-277 in the case of Personal Income Tax (PIT) forms. While SAR-352 and SAR-357 correspond to Corporate Income Tax (CIT) forms. We exploit balance sheet records to analyze the effects on firms' performance. We split balance sheets into two different measures, whether they are assets and liabilities as the balance sheet structure itself, or whether they must be constructed from inside lines information such as investment, wages, cash flow, and profits. In this section, we provide a granular description of variable definitions and measurements, including lines in the tax forms and accountant-conceptual definitions. Next, we turn to describe the definition for every balance sheet outcome implemented in the empirical analysis.

- **Current Assets.** According to international accounting standards, an asset is considered "current" when:

  It expects to realize the asset or intends to sell or consume it in its normal operating cycle; You hold the asset primarily for trading purposes; You expect to realize the asset within twelve months after the reporting period; The asset is cash or cash equivalent unless cash is restricted and cannot be exchanged or used to settle a liability for a period of at least twelve months after the reporting period.

- **Non-Current Assets.** According to international accounting standards, an asset is considered "non-current" when they do not apply to any of the previous definitions for "current assets". In other words, all those assets that are not easily convertible to cash/liquidated within a year.

- **Current Liabilities.** According to international accounting standards, any liability is considered as "current" when:

  You expect to settle the liability in its normal operating cycle; Hold the liability primarily for trading purposes; The liability must be settled within twelve months following the date of the reporting period; or does not have an unconditional right to defer settlement of the liability for at least twelve months after the reporting period.

- **Non-Current Liabilities.** According to international accounting standards, any liability is considered as "non-current" if an entity has the expectation and, in addition, the power to renew or refinance an obligation for at least twelve months after the date of the reporting period, in accordance with the existing financing conditions, it will classify the obligation as non-existent. even if it would otherwise come due in a shorter period.

- **Gross Property, Plant, and Equipment.** Book value of tangible/physical assets held by an entity for use in the production or supply of goods and services, for leasing to third parties, or for administrative purposes; and are expected to be used for more than one period (IAS 16). The sum of box 265 to box 275 in both, the PIT and CIT tax forms.

- **Net Property, Plant, and Equipment.** Gross property, plant, and equipment are discounted by depreciation and amortizations.

- **Gross Investment.** Changes in the Lempiras value of the book value of the gross property, plant, and equipment. That is: $\text{Gross investment}_t = \text{Gross PPE}_t - \text{Gross PPE}_{t-1}$

- **Net Investment.** Changes in the Lempiras value of the book value of the gross property, plant, and equipment excluding depreciation and amortizations. That is:

  $$\text{Net investment}_t = \{\text{Gross PPE}_t - \text{Gross PPE}_{t-1}\} + \text{Depreciation}_t$$

- **Cash Flow.** Cash and Cash Equivalents. Cash: includes both cash and demand bank deposits (IAS 7). Cash Equivalents: are short-term, highly liquid investments, which are readily convertible into specified amounts of cash and are subject to an insignificant risk of changes in value (IAS 7). These correspond to box 501 in both, the PIT and CIT forms.

- **Wages.** Sum of deductible and non-deductible labor costs, such as wages, salaries, and employee compensations. These correspond to boxes 412 and 413 in both, the PIT and CIT forms.

- **Pre Tax Profits.** Correspond to the taxable income or the result of the calendar year (taxable income - deductible costs) before taxes.

- **After Tax Profits.** Correspond to the taxable income or the result of the calendar year (taxable income - deductible costs) minus the tax liability.