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VAT Refunds and Firms' Performance

Evidence from a Withholding Reform in Honduras

David Pineda Pinto
Jose Carlo Bermúdez
Thiago Scot

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Abstract

Late or unreliable refunds of credits undermine the best traits of value-added tax (VAT) systems and might affect firms' growth and investment opportunities. This paper uses administrative tax records in Honduras to study a tax reform that decreased the withholding rate of value-added tax liabilities by credit and debit card (DCC) providers, aiming to curb unrefunded credits. Using a difference-in-differences approach, exploiting differential exposure to the reform, the paper documents that it caused a decrease in excessive withholding and was equivalent to a cut of 1.1 percentage points in effective tax rates faced by treated firms. The paper then evaluates the effects on firms' economic performance and estimate null effects on several indicators of economic growth and investment. The results challenge the premise that unrefunded VAT credits are an important constraint to firm growth in certain settings. Keywords: VAT refunds, withholding, firms' performance.

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

David Pineda Pinto
Utah State University

Jose Carlo Bermúdez
PUC-Chile & World Bank, DIME

Thiago Scot
World Bank, DIME

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Keywords: VAT refunds, withholding, firms' performance.

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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; Brockmeyer et al., 2024). VAT is 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. In practice, VAT systems are implemented with taxable sales generating liabilities that can be reduced by credits from taxable purchases. Credits might sometimes exceed liabilities, and firms should be refunded for the net amount. For example, this is a common case for exporters whose sales to consumers in other countries face zero VAT rates, but whose inputs are taxed (Gérard & Naritomi, 2018; Waseem, 2023). These refunds are large in magnitude: in high-income countries, VAT refunds are on average 30% of gross VAT receipts (Pessoa et al., 2021).

Implementing efficient refund systems is 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, thus requiring refunds.

In this paper, we shed light on the implications of the VAT refund policy in a lower-middle-income country, Honduras. Using administrative records on VAT and income tax filings, we investigate a reform that substantially reduced unrefunded credits and assess its impact on the performance of affected firms.

Similarly to many other low- and middle-income countries, refund policy is *de facto* almost non-existent in Honduras due to the high costs for taxpayers to claim a refund. This means that firms hold a vast and increasing balance of unrefunded credits 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). In aggregate, most of this stock is held by a small number of firms: out of over 100,000 firms registered as VAT taxpayers in 2019, less than 600 claimed 80% of the total stock of unrefunded credits. But a large share of firms are affected by the lack of refunds: approximately half of taxpayers in the VAT regime declare having a positive unrefunded credit in any given year. That probability is often higher for firms that claim being withheld by third parties, highlighting the role of withholding in generating unrefunded credits in the VAT system.

We then investigate the real economic implications of refund delays. It is widely observed that inefficient refund systems undermine some of the best traits of VAT and negatively impact firms – Pessoa et al. (2021) discuss how "a well-functioning VAT refund mechanism has profound implications for overall competitiveness, productivity, and capital formation", and, in the case of Honduras, International Finance Corporation (2022) assert that "accelerating the payment of VAT refunds the government owes to taxpayers is (...) important, since such delays may affect businesses' liquidity flow". The potential negative impacts of unrefunded credits are also predicted by theoretical models used in recent literature about firms' behavior in VAT systems (Liu et al., 2021; Gadenne et al., 2020). Under monopolistic competition, firms price their sales with a mark-up on costs. When taxes paid on purchases cannot be used as credits (e.g. refunds are incomplete or inexistent), the relevant costs become inclusive of value-added taxes, and prices to final consumers are higher, decreasing demand. Shocks to demand have been extensively shown to affect firms' growth, including investment and employment (Coviello et al., 2022; Hebous & Zimmermann, 2021).

We study 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 such as 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" (since 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 withholding 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 withheld L 1.5 billion (USD 60 million) less from their clients.

We then explore a Differences-in-Differences (DiD) design, comparing outcomes for firms with high ex-ante digital payment usage with firms that barely used it. Despite no change in the statutory VAT rate, we show that effective VAT rates for firms with high exposure sharply drop by more than 1.1 p.p. of pre-reform gross taxable sales after the reform. This is because, 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. The reform effectively allowed these firms to claim credits from their purchases, reducing their total tax

payments.

We then investigate whether these changes in unrefunded balances and effective taxes paid affected firms' growth across a range of dimensions. We first show that, before the reform, sales of treatment and control firms trended very similarly. Despite the sharp effects we document for treated firms, we see no differential impacts on sales: they behave very similarly for both groups in the two years after the reform. Consistently with the fact that we document no impacts on sales, we similarly estimate null impacts of the reform on investment in property, plant and equipment (PPE), employment (measured by wage bill), or liquid assets of the treated firms, measured using information from yearly income tax declarations.

We show that both the large first-stage effects and null effects on firms' performance are robust to different specifications from our baseline empirical approach, including alternative definitions of treatment and control firms. Additionally, we document that null effects persist when we consider heterogeneous effects, focusing on sets of firms that might be ex-ante expected to respond more strongly to the reform, such as smaller firms or those liquidity-constrained.

In interpreting our results, we highlight some caveats. First, we study a specific withholding reform that affected a subset of firms, namely those with high usage of digital payments – which we show to be disproportionately in the services and retail sectors. Our results are therefore not immediately applicable to other VAT refund reforms that might target exporters in the manufacturing sector, for example. Second, our analysis includes three years of firms' behavior post-reform - we restrict our sample to the period before 2020 to avoid the many issues of including the period affected by the COVID-19 pandemic. We are thus unable to measure effects that might have materialized over a longer period. Finally, we are unable to directly provide evidence on whether the reform affected prices to consumers (Benzarti et al., 2024). Our VAT data only includes information on total sales, and Honduras does not have an electronic invoice system that would allow the tax authority to observe data on quantities and prices of products. Despite these limitations, we believe this paper provides the first well-identified estimate of the effects of a *de facto* VAT refund reform on firm performance.

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). Brusco et al. (2024) exploit a change in VAT refund timeliness in South Africa and find that faster refund payments lead to large increases in investments by young firms. 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, we provide novel evidence on the practical implementation of VAT systems. Brockmeyer et al. (2024) provide cross-country evidence on how "real-life" VAT systems depart from textbook models, including the fact that many firms carry forward refunds for long periods. Specific impacts of withholding have been discussed by Waseem (2022) and Brockmeyer & Hernandez (2016). Other challenges faced when implementing value-added taxes include fraudulent input claims (Mittal & Mahajan, 2017; Mittal et al., 2018; Waseem, 2023) and the segmentation of trade networks between participants and non-participants of the VAT system (de Paula & Scheinkman, 2010; Gadenne et al., 2020).

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 provide background information on the VAT system in Honduras and highlight some main features of the VAT data. In Section 3 we describe the 2017 withholding reform we study, our sample, and empirical strategy, while in Section 4 we provide our key results, including robustness tests and heterogeneity analysis. Section 5 concludes.

2 Institutional Context

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 taxable and non-taxable sales, 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 zero-rated 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 preferential rates (including zero rating) for some products 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 liabilities). 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 zero-rated. 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 on those purchases[1] . In practice, using OCEs can be a cumbersome process, and we observe in the data exporters declaring taxable purchases and then filing for refunds.

Exemptions and zero-rating of products 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, a long list of products including several staple foods and medicines are zero-rated. From 2012 through 2019, zero-rated sales represented on average 40-45% of total sales, whereas exempt purchases were closer to 30% of total purchases (see Figure A1a and Figure A1b, respectively).

Finally, the withholding of VAT liabilities is performed by three agents. The first are 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 and some services provided for taxpayers formally identified as "large" are withheld immediately, and agents remit that value to the tax authority.

2.1 The VAT Refund System in Honduras

As a rule, Honduras applies an "indefinite carry-forward" approach (Harrison & Krelove, 2005) 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 must 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.

[1] Pessoa et al. (2021) note that these types of exemptions on purchase orders 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.

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

[3] The Tax Administration Diagnostic Assessment Tool (TADAT) sets a "best practice" standard of 90 percent of VAT refund claims being resolved within 30 days (Pessoa et al., 2021). In Honduras, Article 86 of the Tax Code establishes that refund resolution must be issued within 60 business days from the filing of the request. In any case, less than 1% of total refund requests are processed on time, even when accounting for 60 days.

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 some descriptive statistics on the full sample of VAT filers in Table A1, using data aggregated at the yearly level. In 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 an average taxable value-added rate of 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, third-party withholding agents might already have remitted part of the liability to the tax authority. The average claim of withholding in the sample is L40,000 or 20% of the average net liability. In 2018, the average amount withheld by digital payment providers was only 60% of the average amount withheld by the government, and less than one-third that withheld by large taxpayers. That is in stark contrast with the scenario three years earlier in 2015 when digital payment withholding was three times larger than the government's and 50% larger than large taxpayers'. These changes are the result of the decrease in withholding rates by digital payment providers, discussed in the next sections.

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.

Even though a large share of firms in the VAT regime face unrefunded credits over time, the total stock of unrefunded credits is concentrated in a few taxpayers. In Table 1 we start by documenting the concentration of taxable sales in columns (1) and (2), as a benchmark. In 2019, fewer than 2,000 firms, or 2% of all firms, were responsible for 80% of total taxable sales while fewer than 200 firms accounted for half of the sales. While taxable sales are concentrated in a few firms, the stock of unrefunded credits is even more concentrated (columns 3 and 4). Only 87 firms held half the stock of unrefunded credits by the end of 2019, while 600 firms held 80% of the stock. The changes in unrefunded credits were even more concentrated, as

shown in columns 5 and 6. 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 34 firms. This shows that, while 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.[4]

3 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 a withholding reform 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.[5]

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. After March 2017, the payment provider would only withhold \$1.5. Note that the *net VAT payment* that a firm should make in that transaction is not observable to the withholding agent, since it depends not only on sales but also on VAT credits from taxable purchases. In this same scenario, a firm with a value-added of 50% would not have to pay any additional VAT before 2017, since the payment provider already withheld exactly \$7.5. After the reform, nonetheless, the firm would file and pay an additional \$6. On the other hand, a firm with a value-added of 25% would be over withheld before the reform, since its true net tax liability is \$3.75 but the payment provider withheld \$7.5; while after the reform the withholding would not cover the entire liability, and the firm would pay an additional \$2.25.

We present preliminary evidence that the reform was reflected in firms' monthly sales tax

[4]We also document that firms with unrefunded credits are systematically different in several observable characteristics from other firms. In Figure A2, we show coefficients from an OLS regression with an indicator for having positive unrefunded credit at the end of the year 2016 on a series of covariates. Compared to 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. Exporters are also 5 p.p. more likely to have unrefunded credits. Holding unrefunded credits is also more common among firms deemed to be high-risk by the tax authority's risk model, and among firms in sectors such as wholesale and retail

[5]The reform was very salient and widely covered by the press (see Figure A3 in the Appendix).

declarations in Figure 3. In Figure 3a, 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 gross VAT liability changed over time. We document clear changes around the two reforms: average withholding as a share of gross 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.[6] Note that after both reforms there is a clear "transition" period in which the claimed withholding rate adjusts to the new level. For the main reform we studied in 2017, there were legal disputes over the precise withholding rate in the first months of the year which were resolved by April.[7] 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 A4a, 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.[8]

For the remainder of our empirical analysis, we focus on the effects of the 2017 withholding reform. We do that since the 2014 reform not only included an increase in the statutory VAT rate but also encompassed a series of other major tax reforms (for example, it enacted a corporate minimum tax studied in Lobel et al. (2024)).

3.1 Data

For our main analysis of the economic impacts of the 2017 withholding reform, we exploit two main administrative datasets. The first one is the panel of monthly VAT filings, discussed in the previous section. The second one is a panel of yearly income tax declarations (both corporate and personal income taxes), where we obtain information on the balance sheet of firms, including investment, profits, and wage bills. We now discuss in detail our sample restrictions for our empirical exercises and variable definitions.

Sample. Our main differences-in-differences analysis relies on a balanced sample of firms across the 2014-2019 period. We started our panel in 2014 to avoid including the period before the previous major reform in 2013, and end the panel before the onset of the COVID-19 crisis in early 2020. Since we aggregate VAT data for our analysis at the quarterly level, our balanced

[6] Consistent with these changes in averages, Figure A5 documents how the entire distribution of withholding as a share of liability shifts to the left after the reforms decreased withholding rates in 2014 and 2017, respectively. 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. We also document a similar pattern when considering all firm-period observations, including firms claiming zero withholding, in Figure A6.

[7] In Figure 3b, we instead plot the average amount (in L1,000) of withholding claimed by taxpayers in their 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 because, while withholding rates fell, the VAT tax rate increased so the net effect on withholding amounts is almost null.

[8] In Figure A4b, we show that the aggregate claimed withholding by firms closely tracks the withholding amounts that digital payment providers independently submit to the government.

panel is comprised of firms that filed VAT at least once every quarter in that period and also filed income taxes every year.[9]

Defining treated firms. 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 possible strategy would be comparing firms that, before the policy change, reported some withholding (and were therefore affected by the policy) with firms that were not withheld. The main limitation in taking this approach, as discussed in our robustness section, is that many firms that are withheld only register very small amounts of withholding, and therefore the "first stage" of the reform is small when comparing the extensive margin of digital payment usage.[10] Instead, we rely on an intensive margin of usage, comparing firms with high and low exposure to the reform (for similar approaches, see for example Carreri & Martinez (2021); Curtis et al. (2021)). We first consider only firms that filed VAT taxes in 2016, the year before the reform, and compute the ratio between the total amount of withholding by digital payment providers and total gross liabilities 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-liability ratio; control firms are those below the 25th percentile. In Figure A9 we report the distribution of the VAT withholding from digital 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 approximately over 25% through debit and credit card operators. In the robustness section, we show how our results differ when we consider alternative thresholds.

Normalization by Sales. Our baseline estimates follow a standard strategy in the literature: scaling every continuous outcome by firms' total sales in the pre-reform period (2016) (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. Balance sheet outcomes are normalized directly by the total annual sales in 2016.[11]

[9] When aggregating monthly data to quarterly level, we consider that outcomes for months not filed were zero.

[10] Furthermore, we document in Figure A8 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.

[11] Normalization is performed on variables coming from two different administrative records (VAT and income taxes), so there are possible differences in the value of annual sales reported (approximately 1% of observations have meaningful differences in taxable sales between both datasets). To deal with that, when performing analysis at the annual level we take the maximum turnover between the sum of monthly VAT sales and that declared in the yearly income tax form. Then, yearly balance sheet outcomes are directly normalized using the value

After scaling VAT and balance sheet outcomes by sales in the pre-reform period, we winsorize them at the 95th percentile of the distribution.[12]

Firms' characteristics. Our datasets also include time-invariant characteristics of taxpayers, such as economic activity sector, firm size, and legal category of the taxpayer (corporate or personal business). The economic activity classification is based on the International Standard of Industrial Classification of all Economic Activities (ISIC), Revision 4. We then aggregate economic activities into twelve (12) sub-sectors. Pre-reform characteristics used in heterogeneity exercises, such as measures of liquidity constraints and capital intensity, are discussed in more detail in Appendixes C and D.

3.1.1 Summary statistics

In Table 2, we display descriptive statistics for the sample of firms included in the empirical analysis of the reform. We report means and standard deviations for 2016, the year before the withholding reform. Statistics for the pooled sample are included in the first two columns, and then separately for firms with low- and high-usage of digital payments. In **Panel A**, we document some key differences in time-invariant characteristics of these firms. Compared to low-usage 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. We also highlight that only 3% of our sample declared exports in 2016. Because our focus is on a reform of digital payment withholding, it does not speak directly to the issue of refunds for large exporting companies.

In **Panel B** we display summary statistics for VAT outcomes. We note that firms with low digital payment usage are systematically larger than those with high usage - 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 high-usage firms claim on average approximately L630,000 vs. L250,000 for those with low usage. This is consistent with the fact 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.

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of this adjusted annual revenue in 2016. To assure that, at the firm level, normalization of VAT outcomes is done using the same reference turnover, we recompute quarterly turnovers for these firms by multiplying the adjusted annual revenue by the quarterly share of sales ($\phi_q Y_t$) that each firm reports in VAT records (with $\phi_q \equiv \frac{\text{VAT quarterly revenue}(Y_{q,t})}{\text{VAT annual revenue}(Y_t)}$ 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 ($\phi = 1/4$). This specific adjustment affects less than 0.2% of observations.

[12] For variables that can take negative values, we instead winsorize them at the 1st and 99th percentiles.

4 Empirical Strategy & Results

To assess the impact of the change in withholding rates by digital payment providers on affected firms, we start by assigning firms to high- or low-usage groups, based on pre-reform usage of digital payments. We then estimate Average Treatment on Treated (ATT) from a Two-Way Fixed Effect differences-in-differences (DiD) 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:

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

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 before the reform. $HighUsage_f$ is an indicator if firm f had high exposure to digital payments withholding (above the 75th percentile) vs. those with low exposure (below the 25th percentile). Our coefficients of interest are β_t, which capture the differential effects between high-usage (treatment) and 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 analysis period. Each β_t $\forall t \in \underline{T}$ tests parallel trends in the period before the intervention, while coefficients after the reform test 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 payments 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. Below we discuss some potential threats to the identification strategy and provide evidence of their plausibility.

Along with the dynamic DiD, we also pool pre- and post-reform periods to estimate the average effect during the post-reform period in a two-by-two specification of the form:

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

where $Post_t$ is a dummy taking the value of 1 for the post-reform period, and zero otherwise. 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.

4.1 Effects on Withholding, Taxes and Unrefunded credits

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 4 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. The left-hand graphs show the average levels of the outcome in every quarter for high- and low-usage groups, while the right-hand side ones show the coefficients for the dynamic DiD approach, as in 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 2, firms with high- and low-exposure to digital payment providers were systematically different: high-exposure firms claimed total 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 trends between 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 withholding amounts fall by the equivalent of 3 - 3.5 percentage points (p.p.) of pre-reform revenue.

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. As previously discussed, under an efficient refund system 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 panel (b), where the outcome is the effective VAT rate, defined as total taxes paid as a share of same-period revenues. Total taxes are defined as the sum of withheld taxes, any advance payment made, and any positive liability they claim to be due. What we document is that high- and low-exposure firms trend very similarly before the reform and diverge quickly after – during the two years after the reform, effective rates for firms with high usage of digital payments fall by approximately 1.5 p.p. To put that number in context, before the reform high-usage firms paid effective tax rates of 6% on average, meaning that their tax rates fell by approximately 25% after the reduction in withholding rates.[13]

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

[13]In Figure A10 we document that these large changes in taxes paid by firms with high usage of digital payments are not reflected in aggregate VAT payments, since this is a relatively small subset of all firms: total VAT payments were approximately L4.5 billion in 2016-2017 (Figure A10a), but payments from firms with any withholding from digital providers were close L 1 billion, and those by high-usage firms were closer to L250 million before the reform (Figure A10b). In relative terms, nonetheless, we see that aggregate payments from high-usage firms fall by over 30% in the years after the reform (Figure A10c).

(c) and (d). As the withholding rate is reduced, many firms go from a position of being often over-withheld (i.e. having negative net payments) to facing withholding amounts that are less than what they owe, i.e. positive net liabilities. Because of the indefinite carry-forward nature of the system, these firms start to draw down on their stock of unrefunded credits. At the extensive margin, in panel (c) we document that while more than 40% of high-usage firms had unrefunded balance in any quarter pre-reform, that rate falls sharply to below 30% two years later. While we also observe a small decline in the share of low-usage firms with unrefunded credits, our DiD estimates suggest the reform decreased the share of firms with unrefunded credits by over 10 percentage points. In terms of the magnitude of the unrefunded credit stocks, panel (d) shows that post-reform, the average stock of credits for high-exposure firms fell by approximately 1 p.p.

In Table 3 we pool pre- and post-treatment periods together and report results from estimating Equation 2. 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. Total withholding as a share of revenue decreased by 2.5 p.p.; effective rates fell by about 1.1 p.p.; the probability of having unrefunded credits fell by 8.4 p.p. and the stock as a share of revenue by 0.6 p.p..

The results of this section suggest that the 2017 withholding reform not only decreased the amount withheld by digital payment providers. It also curbed excessive withholding and led firms highly exposed to digital payments to pay less value-added taxes - they became less likely to hold unrefunded credits and overall remitted less taxes by being able to use credits from their purchases. By reducing over-withholding, the reform tackled some of the key problems generated by inefficient VAT refund systems. The next section evaluates whether these changes led to meaningful impacts on the economic performance of these firms.

4.2 Effects on Sales and Purchases

We have noted how one of the main effects of the VAT withholding reform might be to increase sales for affected firms - in a model of monopolistic competition where firms face a downward-sloping demand curve and price at a markup of their costs, allowing firms to use their VAT credits should decrease their prices and potentially increase demand and sales. In this section, we use firms' VAT declarations to assess the reform's impact on sales and purchases, using our same empirical strategy.

We start by documenting in Figure 5a that declared taxable sales for firms with high and low usage of digital payments had similar trends before the reform and, unlike other outcomes such as withholding or effective taxes, maintain a very similar trajectory after the cut in withholding rates in early 2017. Except for the last quarter of that year, where we see a significant decrease in sales for high-usage firms, in the remaining periods we estimate no

differential behavior in taxable sales for firms that faced a significant reduction in effective taxes paid. When pooling the post-periods together in Table 4, our point estimate is - 1.8 p.p and not statistically different from zero. Our estimates reject increases in sales for high-usage firms above 2 p.p. We similarly observe no impacts on total sales (including non-taxable ones) (Figure A12a).

These results suggest that, on average, firms that faced a significant decrease in effective VAT rates due to the withholding reform did not increase their sales. In the next section, we document that this result is corroborated by a series of other outcomes from firms' financial statements and balance sheets, suggesting an absence of meaningful economic impact of the reform.

The one dimension in which we do observe a meaningful change in behavior for high-usage firms is their purchases: firms more exposed to digital payment providers substantially increase their reported *taxable costs*, as documented in Figure 5b. We document some differential pre-trends during 2015, very similar behavior in 2016, and a substantial increase in taxable purchases from high-usage firms immediately after the reform: by 2018, their purchases increased by more than 5 p.p. (and completely close the gap in levels observed before the reform). Furthermore, we document in Figure A12b and Figure A12c that the increase in taxable purchases is much larger than for exempt purchases, driving most of the result in total purchases. The fact that most increases in purchases are driven by taxable purchases, and also that we do not see effects on sales, suggest that these are not necessarily real economic effects of increased purchase volume. One possible explanation, consistent with evidence on segmented trade networks under VAT systems (Gadenne et al., 2020), is that treated firms start to purchase more from other VAT-registered firms once withholding rates are cut and firms are more likely to receive refunds – effectively making input credits more valuable. Nonetheless, despite noisy results, we do not see an average decrease in non-taxable purchases, suggesting that this substitution between VAT-registered and unregistered firms might be limited. Alternatively, the increase in reported taxable purchases after the reform might be driven by misreporting: either firm were not filing correctly their taxable purchases pre-reform, since they were not being refunded (meaning that we under-estimate their tax payments pre-reform), or they started to over-report their true taxable costs post-reform because they now can be refunded. Unlike in other settings, VAT-registered firms in Honduras do not file the identity of their buyers (and few file information on suppliers networks), so cross-checking declarations between buyers and suppliers is not possible. Our data, therefore, provides no definitive evidence of the reason for this increase in taxable costs for firms highly affected by the reform.

4.3 Effects on Firm Performance

We now turn to firms' financial statements provided in their income tax returns. Due to the nature of the data, our estimates are at the yearly and not quarterly level, and we focus on a balanced panel of firms filing income taxes in the period 2014-2019.

Effects on Investment. Our baseline measure of firms' investment is the yearly change in property, plants, and equipment (PPE) assets, net of depreciation. As with other outcomes, we normalize these by turnover in 2016, the year before the reform. One caveat about our investment measure is that a non-negligible share of firm-year observations has zero net investment.[14] For that reason, we also use the probability of having any positive investment as an outcome, to study the extensive margin of making investments in PPE.

In Figure 6, panels (a) and (b), we plot average outcomes separately for PPE investment and the probably of positive investment, as well as the DiD coefficients from Equation 1. Starting in panel (a), we see that firms with high usage of digital payments have slightly higher levels of investment than low-usage ones. Still, their trends are similar in the pre-period and follow very similar paths after the reform: our differences-in-differences point estimates are very close to zero and reject changes larger than 1.5 p.p. in PPE investment in the two years after the reform. In panel (b), when we look at the extensive margin probability of having positive investment, we see that low-usage firms are more likely to invest but, once again, pre-trends are similar, and no meaningful changes are observed in the period after the withholding rates are reduced and high-usage firms face lower taxes. In Table 5, we present differences-in-differences results pooling all post-reform periods together. Results are consistent with the dynamic effects: we estimate the null effects of the reform on investment by firms with high-usage of digital payments. Point estimates are very close to zero for all our outcomes and confidence intervals reject large positive investment responses from affected firms. Below we investigate whether any subset of firms, such as those with low liquid assets before the reform, were more likely to respond, despite the null average effects we observe (Bilicka, 2020; Bilicka et al., 2022).

Effects on Cash, Wages, and Profits. Despite the null effects on investment, the decrease in effective VAT rates might have affected other dimensions of firms' performance: they might have more cash at hand, increase their wage bill, or report higher profits. We investigate the impacts of the reform in these three dimensions in Figure 7 and, once again, fail to reject the null hypothesis of zero effect on high-usage firms. For cash and cash-equivalent (highly liquid assets), as well as wage bills, we observe very similar trends for firms with high- and low usage of digital payments, and no noticeable change in behavior after the reform. For

[14] Approximately half of firms have zero investment in the year before the reform. This is likely partially driven by the fact that a large share of firms are in sectors such as general services and retail, where PPE investments are less relevant. But it is also consistent with evidence of "lumpy" investment behavior in other settings, where firms often have periods of investment inaction (Chen et al., 2023)

pre-tax profits, we do see a pre-trend, with high-usage firms reporting a relative increase in profits before the reform, and a flattening after 2016. Overall, nonetheless, none of the estimates suggest a meaningful increase in liquid assets, wage bill or profitability for firms most affected by the withholding reform (we summarize the post-reform effects in a regression table in Table 6).[15]

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. We now turn to assess possible identification threats to our empirical strategy, and then assess the robustness of our our results to alternative specifications.

4.4 Robustness

In this section, we provide a discussion on threats to identification and a series of robustness exercises that document how sensitive our estimates are to changes in baseline specification[16].

Threats to Identification. One concern with our empirical strategy is that control firms (which we define as those with low digital payment usage before the change in withholding rates) react to the reform by increasing their adoption of digital payment, which could potentially affect their sales and thus economic performance positively, and bias our estimates downward. To investigate whether this is the case, in Figure A15 we plot the average share of sales through digital payments, separately for treatment and control firms, over time.[17] For treatment firms, we observe a small decrease in their sales through digital payments but they remain at close to 60% on average. For control firms, we see a small increase but starting from a very small base: by the end of 2018, the gap between average usage in both groups remains above 50 p.p. We also investigate whether many control firms substantially increase their adoption, despite the small average effects: in Figure A16a, we show that only 1% of control firms increase their sales through digital payments enough so that they would be classified as "treated" before the reform[18] – again suggesting little meaningful changes in adoption among control firms.

[15] We also investigate whether broad components of firms' balance sheets changed in the post-reform period and estimate null effects (Figure A14).

[16] In addition to the robustness checks discussed in this section, we run an exercise testing if our estimates are sensitive to the intensive margin of DCC usage for treatment and control definition. In particular, we apply Athey & Imbens (2006) estimator that relies on the distribution of DCC usage as a continuous treatment and builds a counterfactual for control. Results are displayed in Table A4 for VAT outcomes, while Table A5 and Table A6 display results for balance sheet outcomes. Point estimates are quite similar to those we get with our baseline TWFE specification.

[17] We do not directly observe sales through digital payment providers, but estimate them from the declared withholding amounts, using the different withholding rates over time and assuming all sales are taxed at the flat standard rate of 15%.

[18] Before the reform, sales made with debit/credit cards accounted, on average, for approximately 2.0% of total sales for firms in the control group (low-usage) and approximately 65% for firms in the treatment group (high-usage), resulting in a difference in intensities of about 63 percentage points (p.p.). We define an ex-ante low-usage firm as becoming high-intensity post-reform if its inter-annual variation in the sales ratio is $\geqslant$ 63 p.p.

Changing Treatment and Control Definition. In our baseline analysis, we compare firms with ex-ante high usage of digital payments (those with withholding-to-liability ratios above the 75th percentile) with those below the 25th percentile. In this specification, only firms that declared some level of withholding by digital payment providers before the reform are retained in the analysis. In this section, we consider three alternative definitions of treatment vs. control sample and assess the robustness of our results to those definitions. First, we expand our sample to use all firms that declared some withholding by digital payment providers in 2016 and define treated firms as those above the median of withholding and control as those below the median. Second, we compare firms with any level of digital payment withholding pre-reform vs. those with no digital payment withholding, but some other source of withholding. Finally, we simply compare firms with any digital payment withholding with any other firm filing VAT without any withholding. In all these specifications, we maintain balanced panels (firms filing VAT every quarter and income taxes every year).

In Figure A17, we present DiD estimates for the "first-stage" effects, using our baseline specification and the three additional ones discussed above. In panel (a) we document that pre-trends are very similar when we consider firms below and above the median or firms with any withholding vs. those with none. The exception is when we use firms with some withholding other than digital payments as the control group when we observe significant differential pre-trends. What we document for all specifications, nonetheless, is that amounts withheld fall significantly more for treated firms after the reform, as expected. However, the magnitude of that decrease is much larger in our baseline specification (over 3 p.p.) when compared to the alternatives. This is consistent with the previous discussion that many firms face very small withholdings by digital payments, so considering all of them as treated attenuates the effects of the reform. The same patterns are observed when we consider the effective tax rate paid by firms, the probability they hold an unrefunded balance, and the amount unrefunded: we estimate the reform reduced tax rates and unrefunded balances across all specifications, but magnitudes are larger in our baseline approach.

We assess whether the finding of null effects on firms' performance is sensitive to the definition of treatment and control firms, and find that not to be the case. In Figure A18, we display DiD estimates using alternative sample definitions. Overall, results are very similar to the baseline: treated and control firms trend similarly before the reform and do not show divergent behavior in the years after. Estimates are often more precise in the alternative definitions, given larger sample sizes, and they are often centered around zero effects post-reform. In summary, these exercises suggest that the absence of significant effects on firms' performance we estimate in our baseline is not an artifact of our definition of treatment and control firms, but is robust to a series of alternative definitions.

Changing sample balancedness and outlier treatment. In our baseline estimates, we consider firms that file VAT every quarter between 2015q1-2018q4 and winsorize normalized outcomes at the 95th percentile of the distribution. In Table A2, we present our baseline

results for first-stage impacts in column (1) and then consider different combinations of panel balancedness (extending the pre- or post-periods) and treatment of outliers. Overall results are very consistent across the different combinations of sample and winsorization, with slightly larger treatment effects when we winsorize outcomes at the 99.9th percentile (but at the cost of less precise estimates). Despite the large first-stage effects across specifications, impacts on firm performance are consistently indistinguishable from zero (Table A3). Similarly, we obtain slightly larger point estimates for some outcomes when we make our outlier treatment less stringent - they are mostly only marginally significant, and often point to negative impacts on treated firms. But mostly our estimates, consistent with the baseline, suggest null impacts.

4.5 Heterogeneous Effects

In previous sections, we documented that, despite facing lower VAT rates and reducing their balance of unrefunded credits, the average firm highly exposed to the withholding reform did not show any sign of increased sales, investment, or employment. We now show that these average effects are not different for groups of firms that, a priori, might be more likely to respond to these reductions in effective rates.

We focus on five dimensions of firm heterogeneity, considering their pre-reform characteristics. First, we investigate whether the effects were larger for firms that had low liquidity (cash or cash equivalent as a share of total assets) before the reform[19] and therefore might be more likely to benefit from the lower withholding rates and reduction in effective tax rates. Second, we perform a similar exercise but considering firms below and above the median yearly revenue, again considering that small firms might be more likely to be cash-constrained and respond to the reform. Third, we consider firms' value-added before the reform: low-value-added firms should be more impacted by the policy changes since they are more likely to be withheld in excess (at the limit, firms with 100% value added can never face excess withholding since their gross VAT liability is equal to their net liability, given zero credits). Fourth, we consider firms with low- vs. high capital intensity and assess whether investment results are different for the two groups. Finally, we also estimate our results separately for firms in the manufacturing sector, to assess whether we see larger investment responses.

In Figure 8, we provide results for the key first-stage outcome, the effective tax rate remitted by firms. As we documented previously, we estimate an average decrease of approximately 1 p.p. of the ETR post-reform for firms with high exposure to digital payment providers. The results are very similar across most sub-groups, with the notable exception of firms with average low value-added pre-reform: they face a decrease in ETR that is double that of high value-added firms. This is consistent with expectations, given these firms were more affected by high withholding rates before the reform. In Figure A19, we show that first-stage results

[19]For the main heterogeneities in this section, we estimate heterogeneous effects separately for firms below and above the median of the dimension of interest. The exception is the heterogeneity for the manufacturing sector vs. other sectors.

are also similarly homogeneous for other outcomes such as total withholding, the probability of having unrefunded balances, and the stock of unrefunded balances. The other relevant feature of these results is that effects are always noisier when we restrict our data to manufacturing firms, consistent with the fact that this is a small sample of taxpayers.

Despite the mostly homogeneous effects of the reform in terms of effective tax rates, firms might still react very differently to those changes depending on their characteristics. In Figure 9, we assess whether the impacts of the reform on investment are significant for any of the subgroups we consider. Across our main measures of net investment, we consistently fail to reject the null hypothesis of no impacts for treated firms. This is true on the intensive margin of net investments, on the probability of investing and also on the stock of property, plant, and equipment. Point estimates are not very different from the average estimated for the entire sample, and are seldom significantly different from zero. The larger coefficients across sub-groups are for the manufacturing sample, but they are very noisily estimated: for the probability of investment, for example, the point estimate is an increase in 5 p.p. but we cannot rule out effects as large as +15 p.p. or -5 p.p. Given the small size of the sample, we are cautious in interpreting these results; but they further suggest that the null effects we find might not be externally valid when considering other VAT refund reforms that mostly target manufacturing or exporting firms, for example.[20]

5 Conclusions

In this paper, we provide new evidence on the workings of value-added tax refunds - a key dimension for the effective functioning of VAT systems. We use tax records in Honduras to first document the magnitude of unrefunded credits, which were equivalent to more than 1.5% of GDP in 2019.

We then exploit a reform aimed at curbing the accumulation of unrefunded credits for a specific set of taxpayers: firms that sell through debit and credit cards. In a differences-in-differences setting, we estimate the causal effect of decreasing withholding rates by these digital payment providers, comparing firms with ex-ante different exposure levels to these providers. Consistent with the reform's 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 p.p. of pre-reform sales.

Despite these significant changes in tax rates faced by firms, we do not observe real effects on economic activity: highly affected firms do not increase their sales, investment, wage bill, or profits. These results are robust across a range of alternative empirical specifications, and also for subgroups that one might expect to be more reactive to the decrease in effective rates, such as small firms or those with less liquid assets.

[20]Effects on other outcomes of firm performance, such as availability of cash, wage bills and profits are similarly homogeneous across groups, as documented in Figure A21.

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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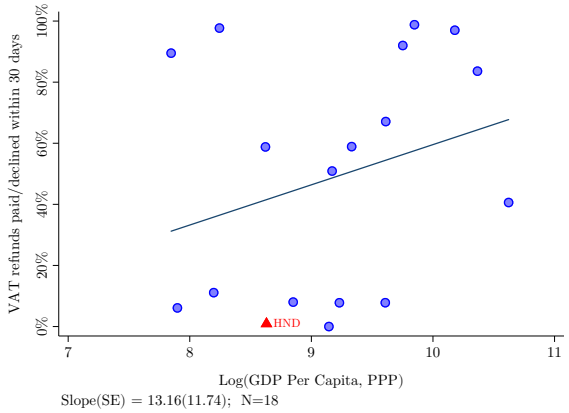
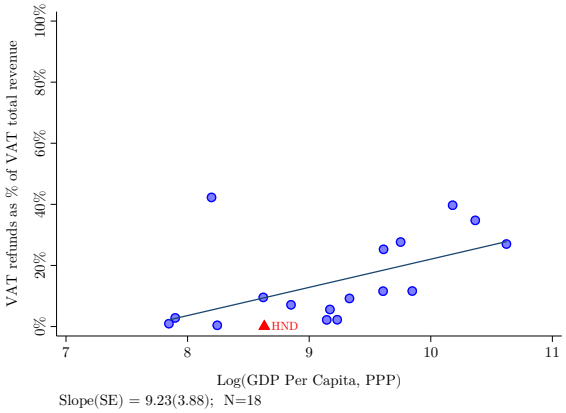
6 Tables and Figures

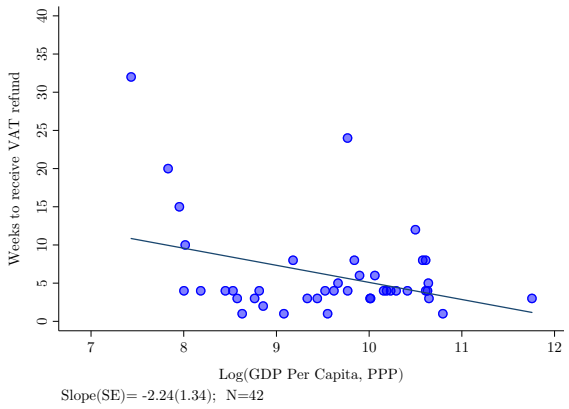
Figure 1: VAT REFUNDS ACROSS COUNTRIES



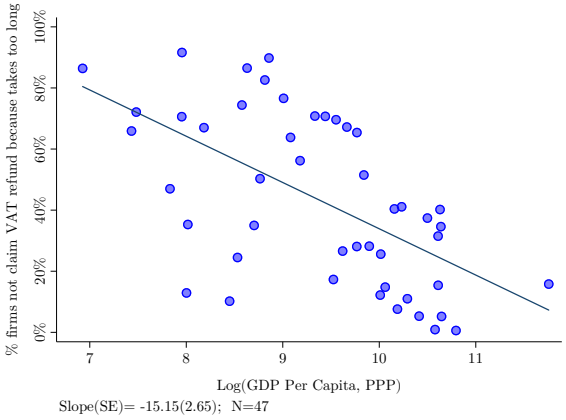
(a) Share of refunds unpaid after 30 days



(b) Refunds and VAT revenue



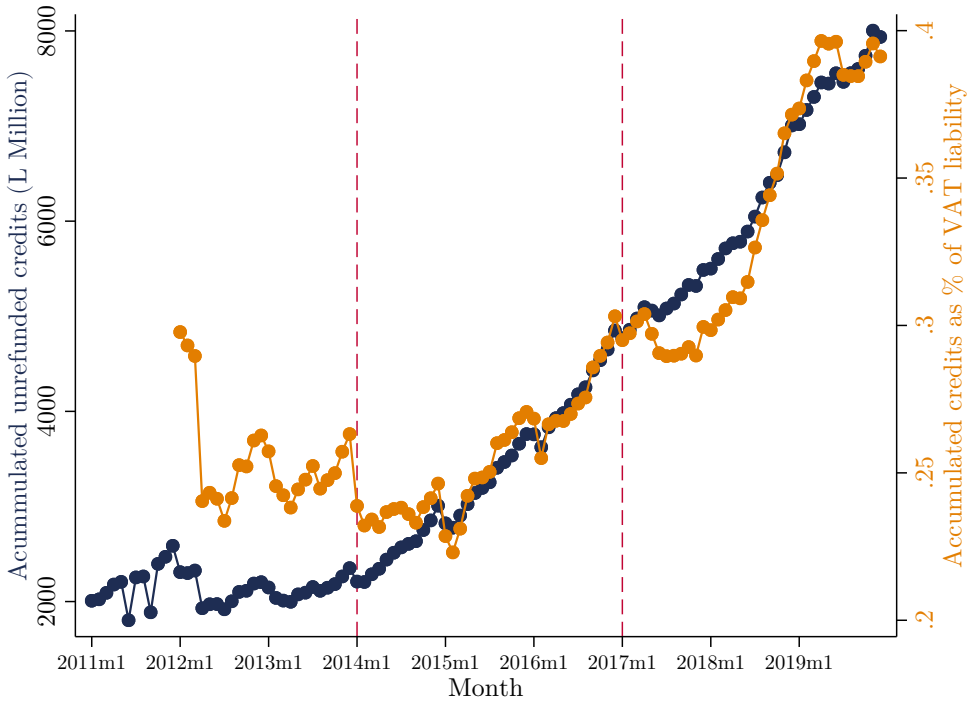
(c) Weeks to receive VAT refund



(d) % firms not claiming VAT refund

Note: Own calculations based on TADAT country reports (excluding assessments at the federal level), The World Bank B-Ready country reports, and the World Bank Open Data. Figures 1a and 1b were retrieved from TADAT, while Figures 1c and 1d are from B-ready Enterprise Surveys. Figure 1a plots the share of refund claims 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). Figure 1c displays the median number of weeks that took from when the firm submitted its application until the refund was received. Figure 1d plots the percentage of firms reporting too long or complicated refund process as the main reason for not applying for a VAT refund, among those providing a reason other than no need. Regression coefficients on raw data and robust standard error (in parenthesis) are presented.

Figure 2: Unrefunded credit aggregates



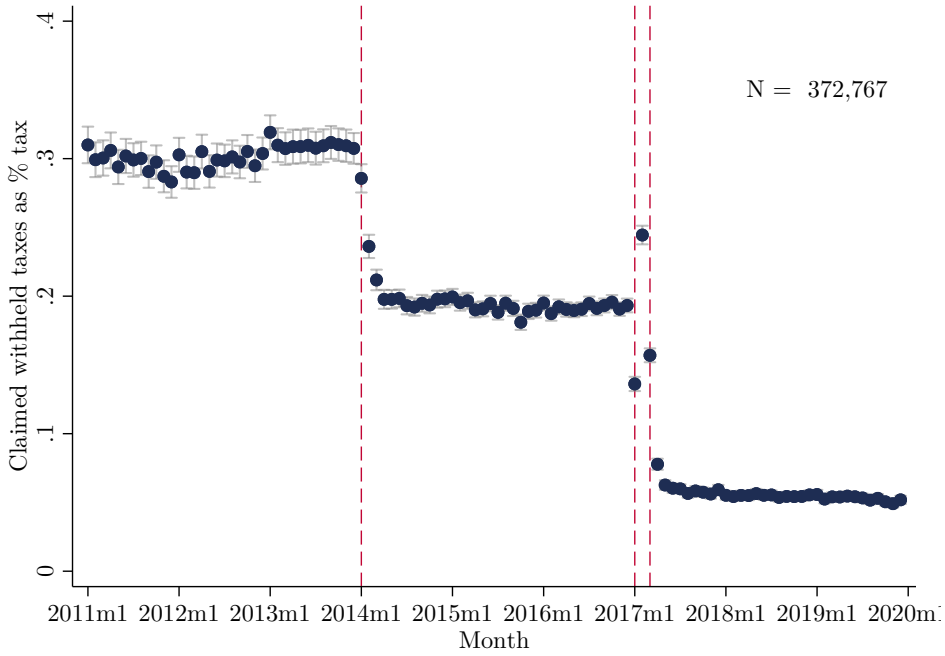
Note: This figure reports the sum of accumulated unrefunded credits reported by VAT filers monthly between January 2011 and December 2019. Accumulated unrefunded credits (reported in the blue line) are defined as the stock of VAT balance (input VAT - output VAT) not refunded from previous periods in millions of Lempiras. The series in orange represents the stock of unrefunded credits as a percentage of the annualized sum of the net VAT liability claimed by firms. To avoid seasonal effects of VAT liabilities, for each month the denominator of that series is defined as the cumulative net VAT liabilities from the previous 12 months.

Table 1: Concentration of unrefunded credits

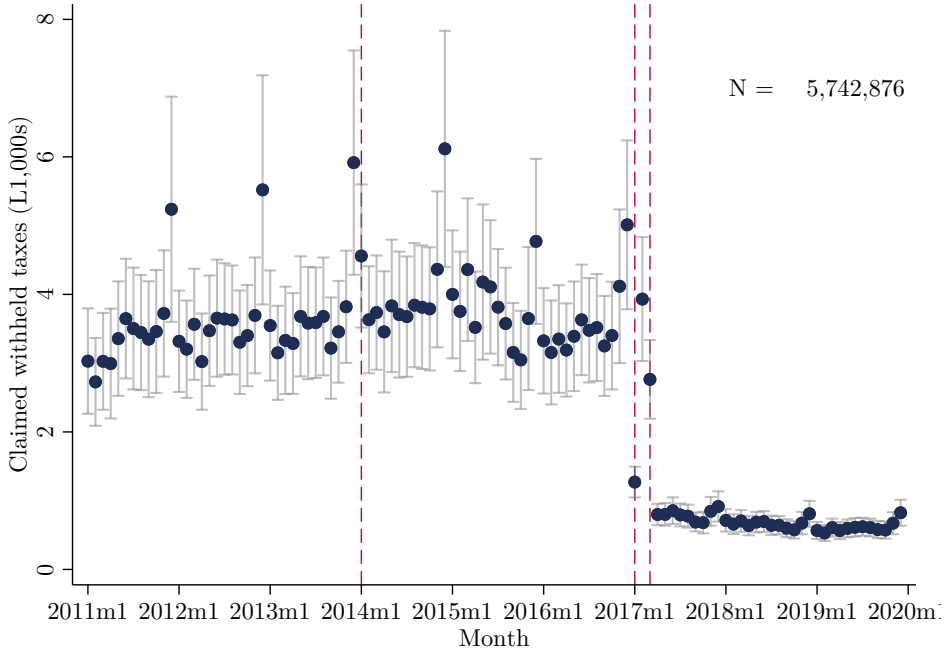
	Taxable Sales		**Stock unrefunded credits**		**Flow unrefunded credits**	
	Top 50%	Top 80%	Top 50%	Top 80%	Top 50%	Top 80%
2014	149	1,180	81	477	9	22
2015	184	1,542	92	516	10	30
2016	202	1,808	93	545	19	56
2017	187	1,727	91	572	7	14
2018	187	1,801	87	554	17	49
2019	193	1,972	87	601	13	34

Note: This table reports the number of firms that, jointly, represent the highest shares (50% and 80%) of taxable sales, stock of unrefunded credits, and flow of unrefunded credits, each year. The underlying sample is the universe of VAT fillers between 2014 and 2019.

Figure 3: Withholding changes, 2011-2019



(a) Withholding as a share of tax liability



(b) Withholding in L1,000s

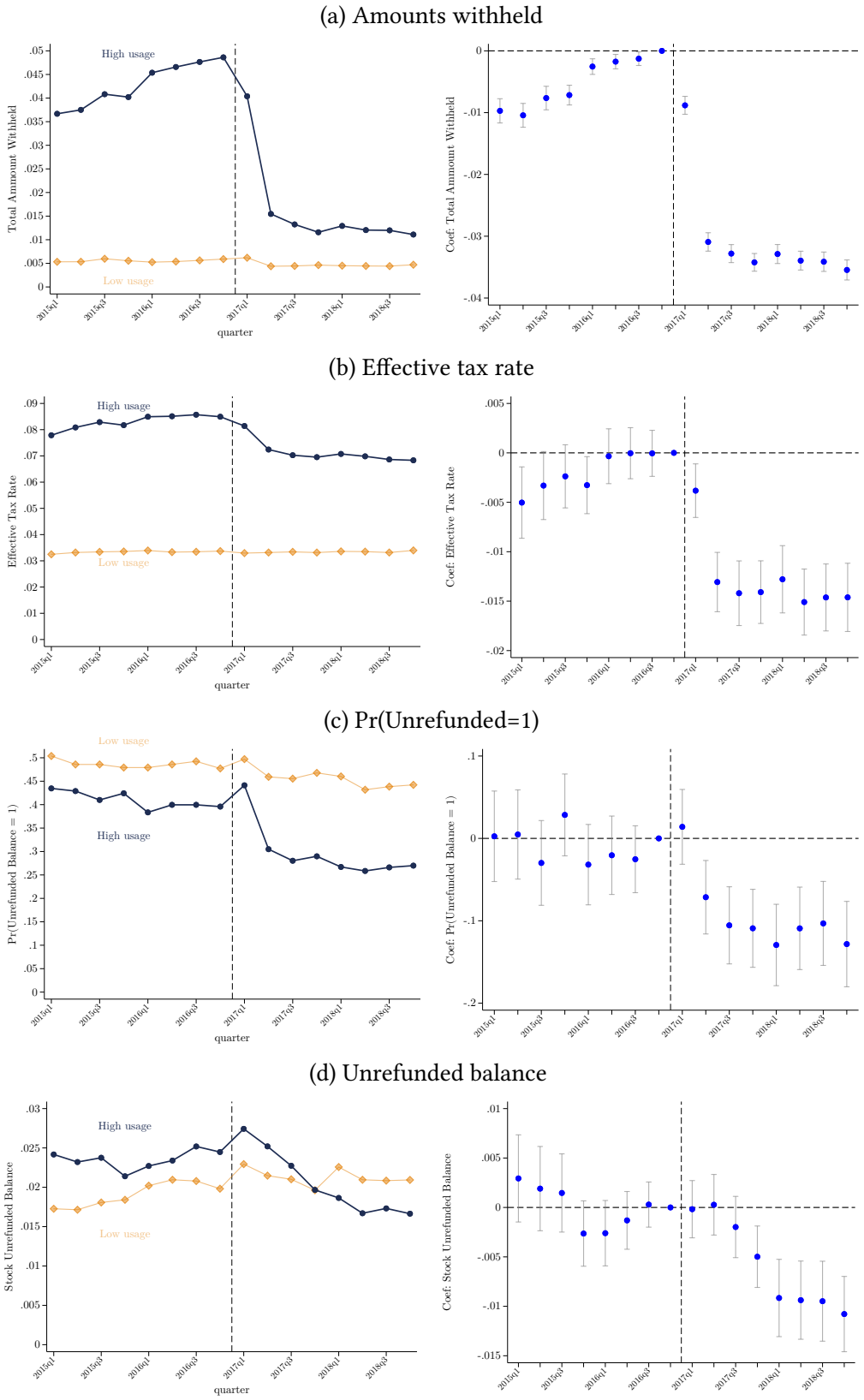
Note: These figures present changes in withholding by digital payment providers around the 2014 and 2017 reforms. Panel A presents the mean (with 95% CI) claimed withholding by payment providers as a share of gross tax liability in VAT filings. Panel B presents mean claimed withholding in L1,000s. Dashed 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.

Table 2: 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		0.61		0.57	
Official size: small	0.94		0.90		0.98	
Sector: Retail	0.28		0.35		0.21	
Sector: Manufacturing	0.11		0.14		0.08	
Sector: Wholesale	0.14		0.18		0.12	
Sector: Services	0.37		0.20		0.53	
Filed yearly income tax	1.00		1.00		1.00	
Exporter (%)	0.03		0.05		0.01	
Panel B: Outcomes for analysis of subsection 4.2						
VAT descriptives						
Total revenue (L1,000s)	44,327	(269,173)	70,652	(306,942)	18,003	(222,208)
Taxable Sales (L1,000s)	29,301	(196,302)	44,559	(238,182)	14,043	(141,098)
VAT liability (L1,000s)	4,404	(29,480)	6,692	(35,729)	2,117	(21,258)
VAT credits (L1,000s)	3,202	(24,624)	4,968	(29,410)	1,436	(18,494)
Net VAT liability (L1,000s)	1,202	(7,255)	1,724	(9,714)	681	(3,229)
Total withholding (L1,000s)	445	(4,236)	257	(1,372)	633	(5,827)
Digital payment withholding (L 1,000s)	354	(3,948)	100	(909)	607	(5,499)
Sales to government withholding (L1,000s)	44	(387)	84	(543)	3	(47)
Large firms withholding (L1,000s)	47	(451)	72	(511)	23	(380)
Net due VAT payment (L1,000s)	865	(6,359)	1,530	(8,926)	199	(578)
Withholding						
Claims Digital Payment withholding (%)	100	(0)	100	(0)	100	(0)
Digital payment withholding (L 1,000s)	354	(3,948)	100	(909)	607	(5,499)
Unrefunded credit						
Claims positive unrefunded balance (%)	66	(47)	72	(45)	61	(49)
Unrefunded balance in December (L1,000s)	287	(2,127)	342	(1,982)	232	(2,262)
Observations	2,112		1,056		1,056	
Panel C: Outcomes for analysis of subsection 4.3						
Balance sheet breakdown						
Current assets (L1,000s)	24,803	(138,010)	39,913	(172,630)	9,711	(88,712)
Non-current assets (L1,000s)	19,184	(166,226)	26,638	(201,432)	11,739	(120,922)
Current liabilities (L1,000s)	16,802	(99,010)	27,482	(129,943)	6,133	(50,121)
Non-current liabilities (L1,000s)	6,462	(48,502)	8,725	(48,614)	4,203	(48,311)
Investment						
Gross fixed assets (L1,000s)	15,899	(123,780)	21,051	(136,487)	10,753	(109,465)
Net fixed assets (L1,000s)	9,269	(66,351)	11,195	(54,212)	7,345	(76,562)
Gross investment (L1,000s)	9,299	(113,608)	14,136	(154,540)	4,466	(43,716)
Net investment (L1,000s)	2,886	(50,532)	4,613	(70,523)	1,162	(11,539)
Additional outcomes						
Cash stock (L1,000s)	3,416	(28,885)	5,028	(37,511)	1,807	(16,062)
Wage bill (L1,000s)	5,004	(25,004)	7,394	(29,827)	2,617	(18,717)
Pre-tax profits (L1,000s)	2,748	(20,251)	4,282	(24,382)	1,217	(14,889)
After-tax profits (L1,000s)	1,758	(14,257)	2,739	(17,245)	779	(10,370)
Observations	1,767		883		884	

Note: This table reports summary statistics in 2016 for our baseline 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 subsection 4.2 or subsection 4.3). **Panel B** only includes firms in the balanced panel between 2015q1-2018q4, which drives the analysis of subsection 4.2. Finally, **Panel C** only includes firms in the balanced panel between 2014-2019 for analysis in subsection 4.3.

Figure 4: Difference-in-Differences, First Stage



Note: This panel of figures reports dynamic DiD estimates 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.

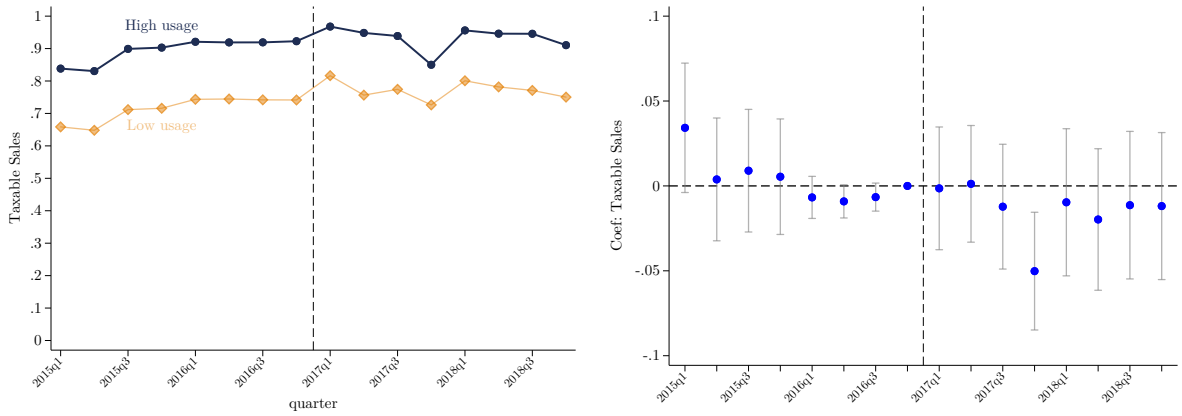
Table 3: Difference-in-Differences, First Stage

	(1) Total amount withheld	(2) Remitted taxes	(3) Effective tax rate	(4) Pr(Unrefunded Balance = 1)	(5) Stock unrefunded balance
High usage × Post	-0.025***	-0.012***	-0.011***	-0.084***	-0.006***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Constant	0.024***	0.056***	0.058***	0.433***	0.023***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	33,720	33,720	33,421	33,792	33,720
# Firms	2,112	2,112	2,112	2,112	2,112
R-Squared	0.74	0.70	0.81	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

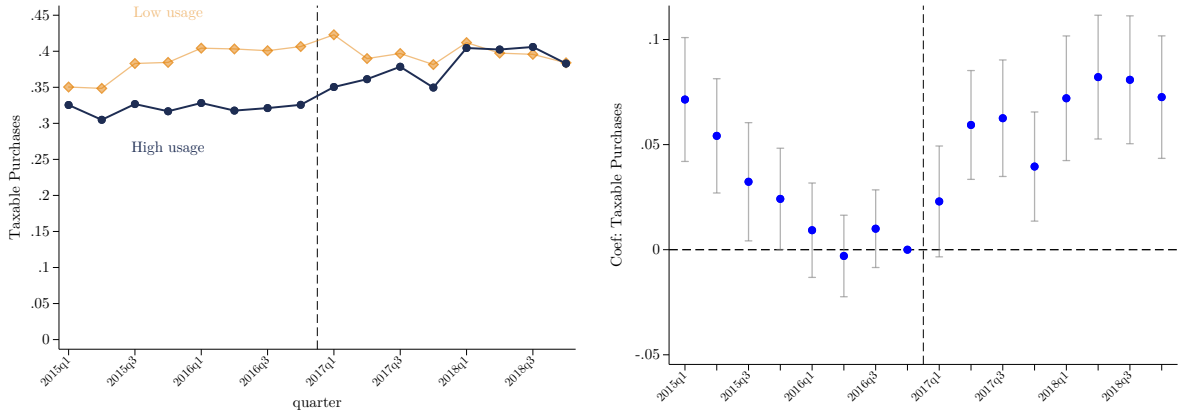
Note: This table reports difference-in-differences quarterly estimates on the first-stage effects of the VAT withholding reform, as per Equation 2. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by the pre-reform (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 5: Difference-in-Differences, VAT liabilities

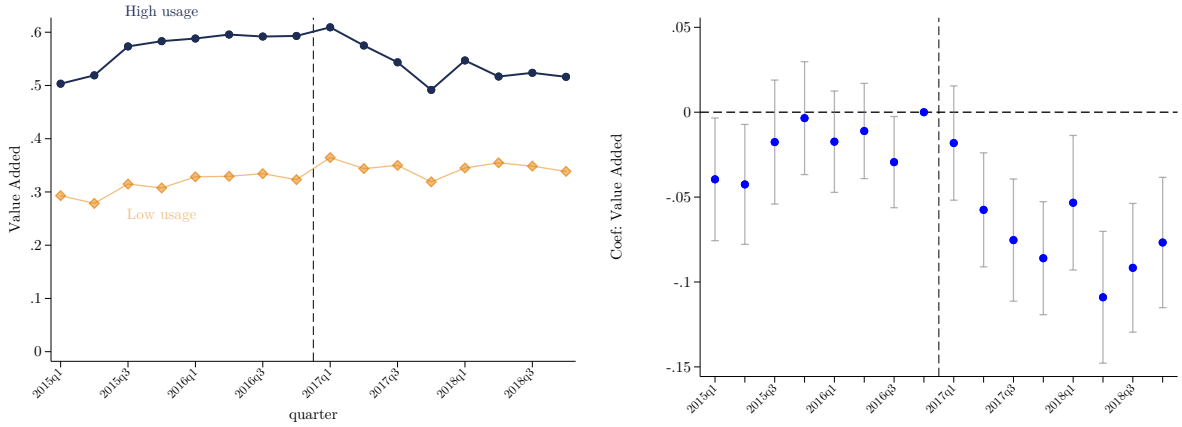
(a) Taxable sales



(b) Taxable purchases



(c) Value-Added



Note: This panel of figures reports dynamic DiD estimates for the effects of the VAT withholding reform on VAT liabilities. 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.

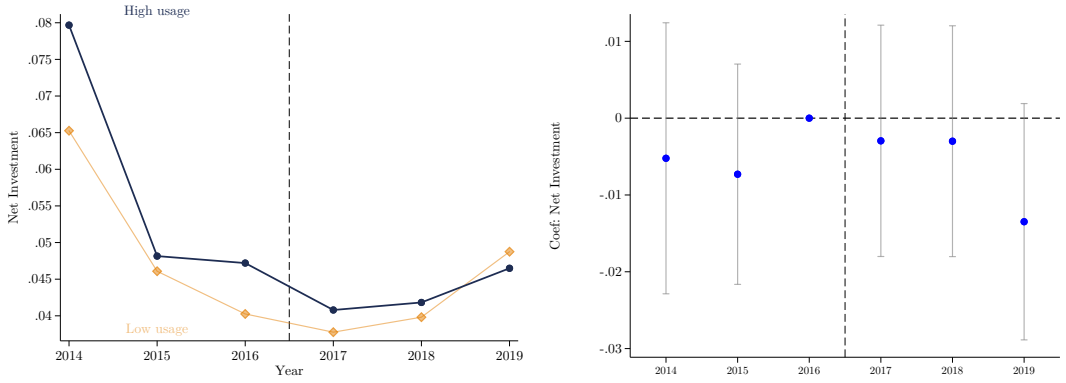
Table 4: Difference-in-Differences, VAT liabilities

	(1) Taxable sales	(2) Taxable purchases	(3) Value added
High usage × Post	-0.018	0.037***	-0.051***
	(0.02)	(0.01)	(0.01)
Constant	0.833***	0.362***	0.455***
	(0.00)	(0.00)	(0.00)
Observations	33,720	33,720	33,720
# Firms	2,112	2,112	2,112
R-Squared	0.54	0.66	0.63
Mean Dep Var 2016	0.83	0.36	0.46
Firm FE?	Yes	Yes	Yes
Quarter-Industry-Size FE?	Yes	Yes	Yes

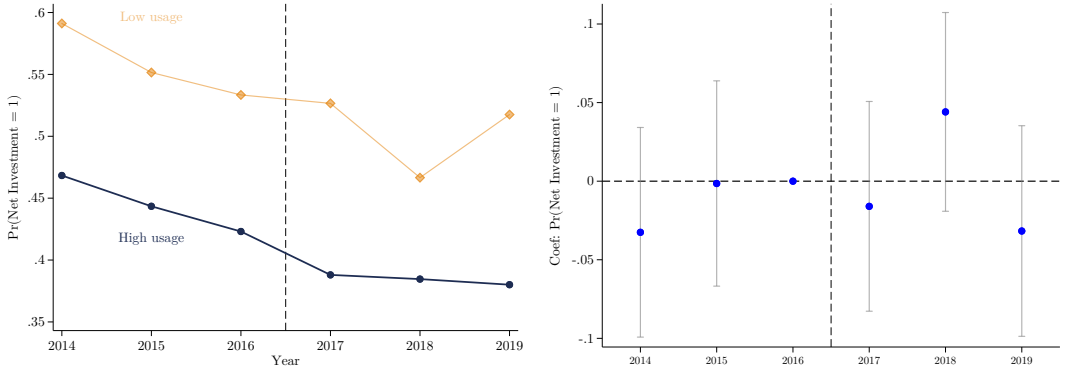
Note: This table reports difference-in-differences quarterly estimates on the effects of the VAT withholding reform on additional VAT outcomes, as per Equation 2. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by 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 6: Difference-in-Differences, Effects on net investment

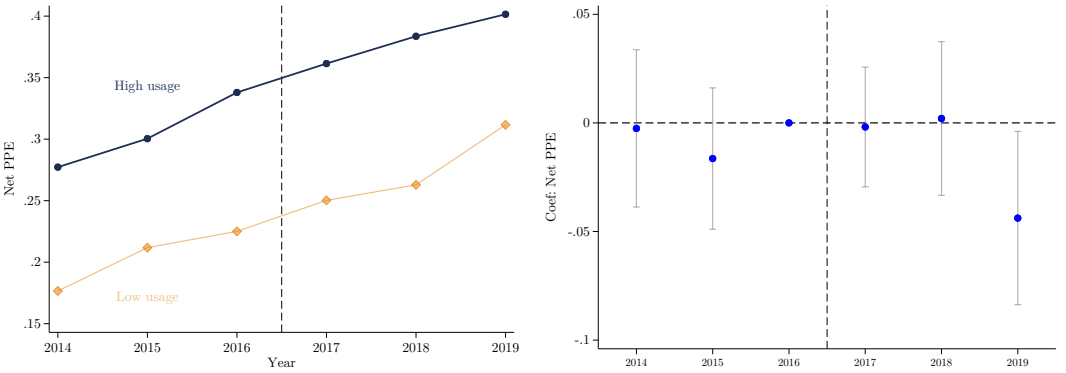
(a) Net investment



(b) Pr(Net investment > 0)



(c) Net PPE



Note: This panel of figures reports dynamic DiD estimates 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 before 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.

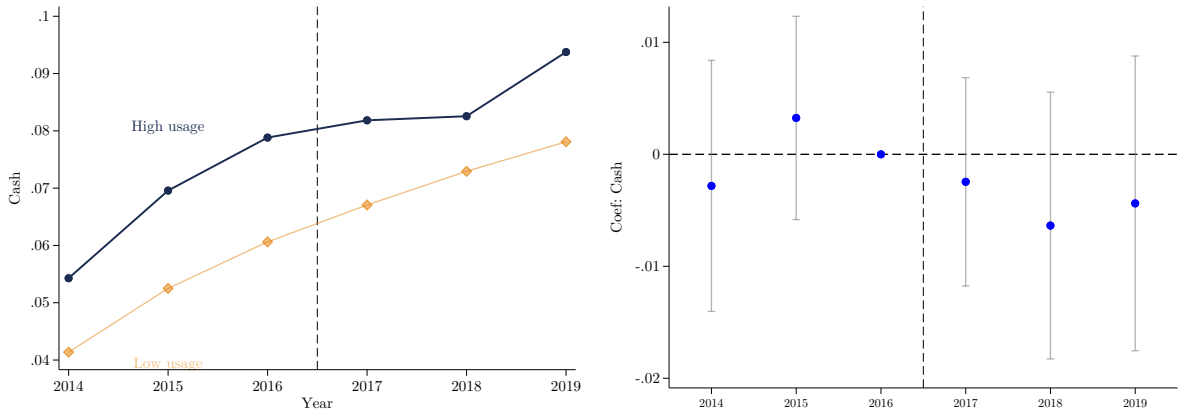
Table 5: Difference-in-Differences, Effects on investment

	(1) Net Investment	(2) Pr(Net investment > 0)	(3) Net PPE	(4) Gross Investment	(5) Pr(Gross investment > 0)	(6) Gross PPE
High usage × Post	-0.002	0.010	-0.008	0.001	0.018	0.001
	(0.00)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Constant	0.049***	0.470***	0.294***	0.141***	0.642***	0.389***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(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	0.29	0.42	0.81	0.57	0.57	0.82
Mean Dep Var 2016	0.04	0.48	0.28	0.13	0.65	0.38
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes	Yes	Yes

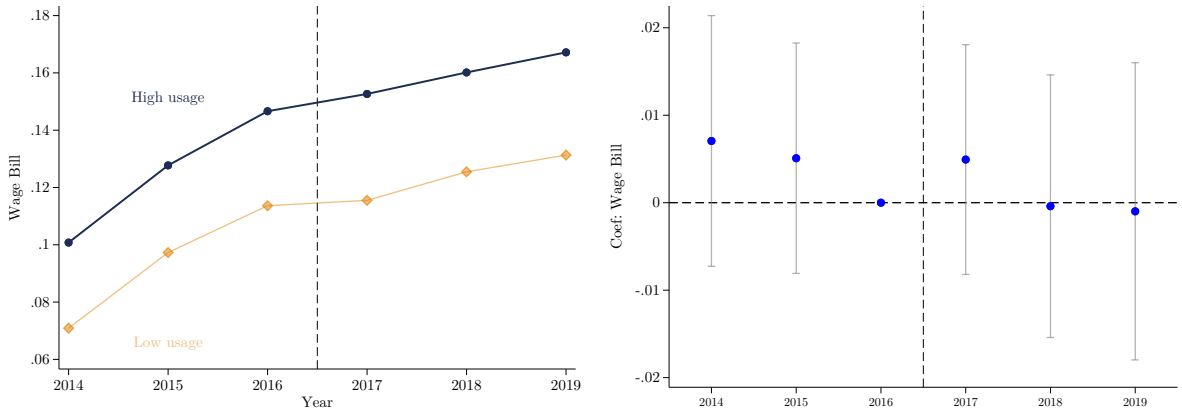
Note: This table reports difference-in-differences yearly estimates on the effects of the VAT withholding reform on fixed assets and investment, as per Equation 2. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome from columns (1) to (4) was normalized 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 7: Difference-in-Differences, Effects on cash, employment, and profits

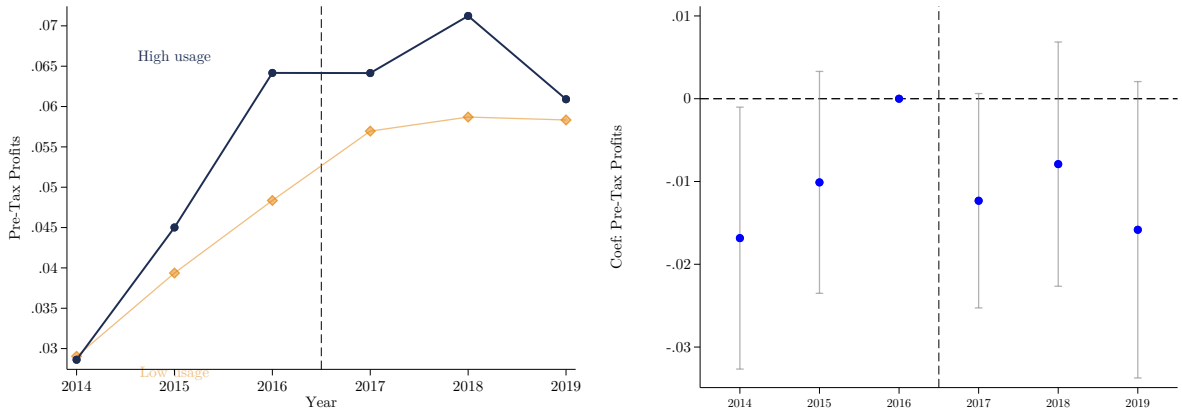
(a) Cash



(b) Wage bill



(c) Pre-Tax profits



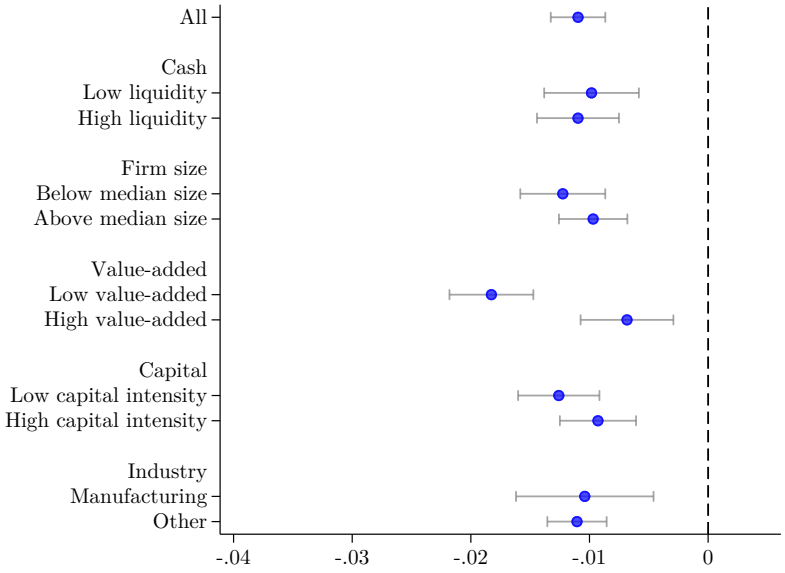
Note: This panel of figures reports dynamic DiD estimates for the effects of the VAT withholding reform on cash, wages, and profits. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by firm sales in 2016 (the year prior to the withholding 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 6: Difference-in-Differences, Effects on cash, wages, and profits

	(1) Cash	(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***	0.053***	0.042***
	(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	0.62	0.62
Mean Dep Var 2016	0.07	0.13	0.06	0.04
Firm FE?	Yes	Yes	Yes	Yes
Year-Industry-Size FE?	Yes	Yes	Yes	Yes

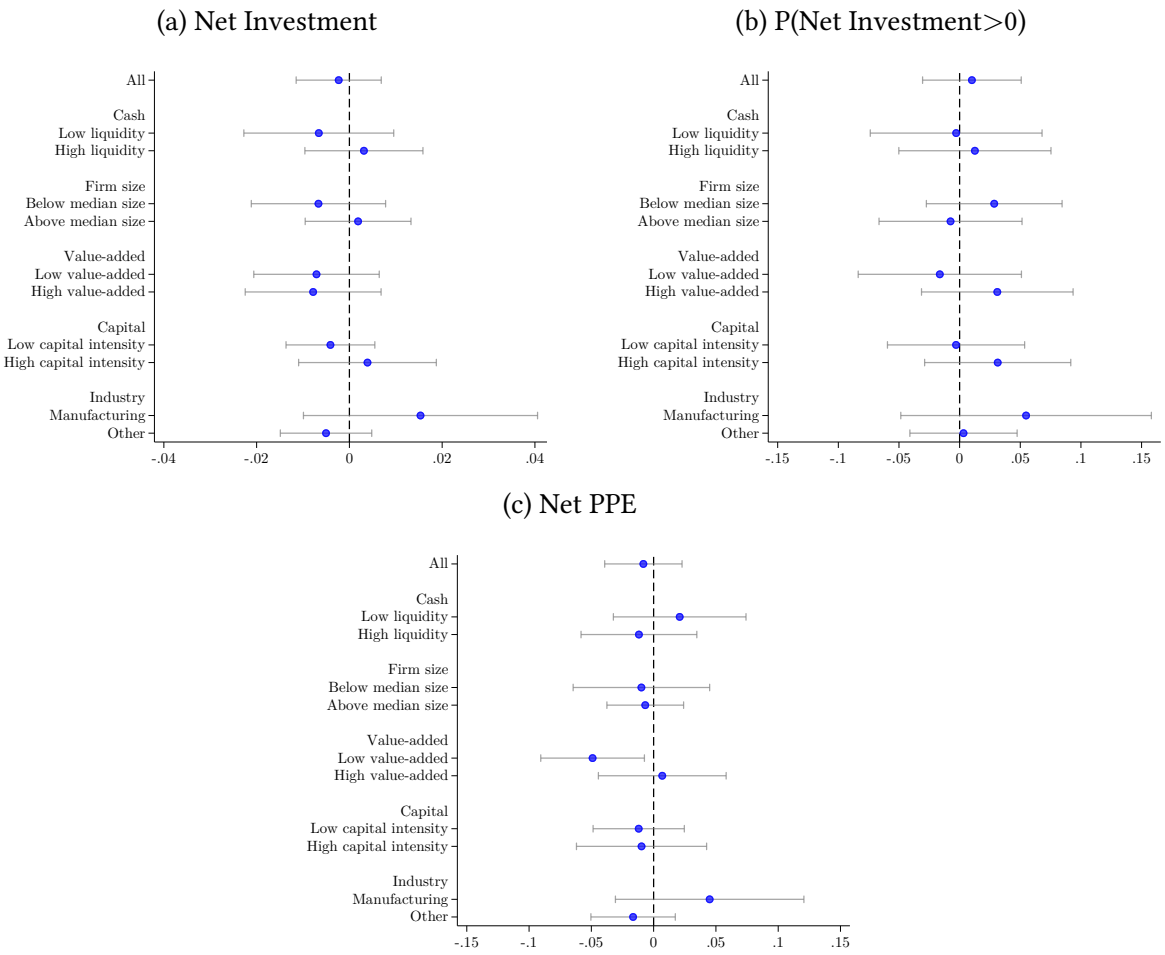
Note: This table reports difference-in-differences yearly estimates on the effects of the VAT withholding reform on cash flow, wages, and profits as per Equation 2. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by the pre-reform (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 8: Heterogeneity, Effective Tax Rate



Note: This table reports heterogeneity for effective tax rate. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. The outcome was 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 9: Heterogeneity, Net Investment



Note: This table reports heterogeneity for net investment. 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.

Online Appendix For

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

David Pineda Pinto Jose Carlo Bermúdez Thiago Scot

A Additional Tables

Table A1: 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)	12,304	11,543	12,214	10,166
	(189,257)	(161,997)	(175,820)	(170,816)
Taxable Sales (L1,000s)	4,939	4,976	5,182	4,011
	(78,117)	(81,793)	(93,037)	(87,906)
VAT liability (L1,000s)	747	753	783	606
	(12,109)	(12,676)	(14,358)	(13,572)
VAT credits (L1,000s)	506	506	521	413
	(7,618)	(8,309)	(8,922)	(8,669)
Net VAT liability (L1,000s)	241	247	263	192
	(5,988)	(6,039)	(6,873)	(6,106)
Panel C: Withholding				
Total withholding (L1,000s)	74	80	55	39
	(1,263)	(1,392)	(866)	(775)
Digital payment withholding (L 1,000s)	36	38	14	7
	(1,035)	(1,138)	(376)	(233)
Sales to government withholding (L1,000s)	12	13	13	10
	(521)	(567)	(579)	(559)
Large firms withholding (L1,000s)	26	28	28	22
	(418)	(456)	(481)	(456)
Net due VAT payment (L1,000s)	191	190	215	165
	(5,747)	(5,744)	(6,568)	(5,932)
Claims Digital Payment withholding (%)	10	10	9	7
	(30)	(29)	(29)	(25)
Panel D: Unrefunded credit				
Claims positive unrefunded balance (%)	49	50	49	40
	(50)	(50)	(50)	(49)
Unrefunded balance in December (L1,000s)	70	80	83	75
	(1,083)	(1,334)	(1,516)	(1,680)
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 A2: Robustness changing balancedness and winsorizing, VAT outcomes

	Balanced 2015q1 - 2018q4			Balanced 2015q1 - 2019q4			Balanced 2014q1 - 2018q4			Balanced 2014q1 - 2019q4		
	(1) 95th	(2) 99th	(3) 99.9th	(4) 95th	(5) 99th	(6) 99.9th	(7) 95th	(8) 99th	(9) 99.9th	(10) 95th	(11) 99th	(12) 99.9th
Total Ammount Withheld	-0.025***	-0.027***	-0.031***	-0.027***	-0.028***	-0.032***	-0.023***	-0.024***	-0.035***	-0.024***	-0.026***	-0.038***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Total Remitted Taxes	-0.012***	-0.012***	-0.037***	-0.012***	-0.012***	-0.036***	-0.010***	-0.010***	-0.096**	-0.009***	-0.009***	-0.134**
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.04)	(0.00)	(0.00)	(0.07)
Effective Tax Rate	-0.011***	-0.011***	-0.011***	-0.012***	-0.012***	-0.012***	-0.010***	-0.010***	-0.010***	-0.010***	-0.010***	-0.010***
	(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)	-0.084***	-0.084***	-0.084***	-0.102***	-0.102***	-0.102***	-0.084***	-0.084***	-0.084***	-0.107***	-0.107***	-0.107***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Stock Unrefunded Balance	-0.006***	-0.007**	-0.015*	-0.009***	-0.014***	-0.023***	-0.008***	-0.012***	-0.017*	-0.011***	-0.018***	-0.022**
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
# Observations	33,720	33,720	33,720	39,510	39,510	39,510	37,490	37,490	37,490	42,336	42,336	42,336
# Firms	2,112	2,112	2,112	1,980	1,980	1,980	1,878	1,878	1,878	1,767	1,767	1,767
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. For this estimation, the treatment group is defined as those firms with a DCC usage-to-tax liability ratio above the 75th percentile of the usage distribution ($\geqslant$ p75), and the control group is based on firms with usage below the 25th percentile ($\leqslant$ p25). For comparison, column (1) displays our baseline estimates, as in Table 3. Each column is named according to the cut implemented. Thus, "99.9th" means that outcomes are winsorized at the 99.9th fraction of observations, and so on. Value added is also winsorized at the 1st fraction of observations because the presence of negative values creates outliers. 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 A3: Robustness changing balancedness and winsorizing, Firm's performance

	Balanced 2014q1 - 2019q4			Balanced 2014q1 - 2018q4			Balanced 2015q1 - 2018q4			Balanced 2015q1 - 2019q4		
	(1) 95th	(2) 99th	(3) 99.9th	(4) 95th	(5) 99th	(6) 99.9th	(7) 95th	(8) 99th	(9) 99.9th	(10) 95th	(11) 99th	(12) 99.9th
Net investment	-0.002	-0.018	-0.084**	-0.001	-0.007	-0.043	0.000	-0.005	-0.029	-0.003	-0.012	-0.057*
	(0.00)	(0.01)	(0.04)	(0.00)	(0.01)	(0.03)	(0.00)	(0.01)	(0.03)	(0.00)	(0.01)	(0.03)
Pr(Net Investment = 1)	0.010	0.010	0.010	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)	(0.02)	(0.02)	(0.02)
Net PPE	-0.008	-0.027	-0.145	0.002	0.006	-0.045	-0.011	-0.017	-0.069	-0.020	-0.037	-0.156
	(0.02)	(0.03)	(0.13)	(0.01)	(0.03)	(0.09)	(0.01)	(0.02)	(0.07)	(0.01)	(0.03)	(0.11)
Cash	-0.005	-0.004	-0.005	-0.005	-0.006	-0.008	-0.007*	-0.013**	-0.018*	-0.007	-0.012	-0.016
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Wage Bill	-0.003	-0.006	-0.010	-0.005	-0.006	-0.014	-0.002	-0.003	-0.009	-0.003	-0.006	-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)
Pre-Tax Profits	-0.003	-0.003	-0.005	-0.005	-0.006	-0.009	-0.009	-0.009	-0.011	-0.006	-0.006	-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)
# Observations	10,602	10,602	10,602	9,390	9,390	9,390	8,448	8,448	8,448	9,900	9,900	9,900
# Firms	1,767	1,767	1,767	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	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. For this estimation, the treatment group is defined as those firms with a DCC usage-to-tax liability ratio above the 75th percentile of the usage distribution ($\geqslant$ p75), and the control group is based on firms with usage below the 25th percentile ($\leqslant$ p25). For comparison, column (1) displays our baseline estimates, as in Table 5, and Table 6. Each column is named according to the cut implemented, thus, "99.9th" means that outcomes are winsorized at the 99.9th fraction of observations, and so on. Profits (pre and after-tax) are also winsorized at the 1st fraction of observations because negative values create outliers. 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 A4: Robustness changing estimator: VAT outcomes

	(1) Digital Pay Withholding	(2) Remitted taxes	(3) Effective tax rate	(4) Pr(Unrefunded Balance = 1)	(5) Stock unrefunded balance
TWFE estimator	-0.025***	-0.012***	-0.011***	-0.084***	-0.006***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
CiC estimator	-0.024***	-0.017***	-0.012***	-0.087***	-0.005***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Observations	33,720	33,720	33,421	33,792	33,720
Mean Dep Var 2016	0.03	0.06	0.06	0.44	0.02

Note: This table reports point estimates for first-stage. TWFE estimator comes from Equation 2, and standard errors are clustered at the firm level. For the CiC estimator, bootstrapped standard errors are displayed. The sample is 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. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

Table A5: Robustness changing estimator: Investment

	(1) Gross PPE	(2) Net PPE	(3) Gross Investment	(4) Net Investment	(5) Pr(Gross investment = 1)	(6) Pr(Net investment = 1)
TWFE estimator	0.001	-0.008	0.001	-0.002	0.018	0.010
	(0.02)	(0.02)	(0.01)	(0.00)	(0.02)	(0.02)
CiC estimator	0.009	-0.003	-0.002	-0.007	0.012	-0.017
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	10,602	10,602	10,602	10,602	10,602	10,602
Mean Dep Var 2016	0.38	0.28	0.13	0.04	0.65	0.48

Note: This table reports point estimates for investment outcomes. TWFE estimator comes from Equation 2, and standard errors are clustered at the firm level. For the CiC estimator, bootstrapped standard errors are displayed. The sample is based on a yearly balanced panel of firms filling VAT between 2015-2019. Each outcome was normalized by the pre-reform (2016) sales and winsorized at the 95th percentile of observations. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

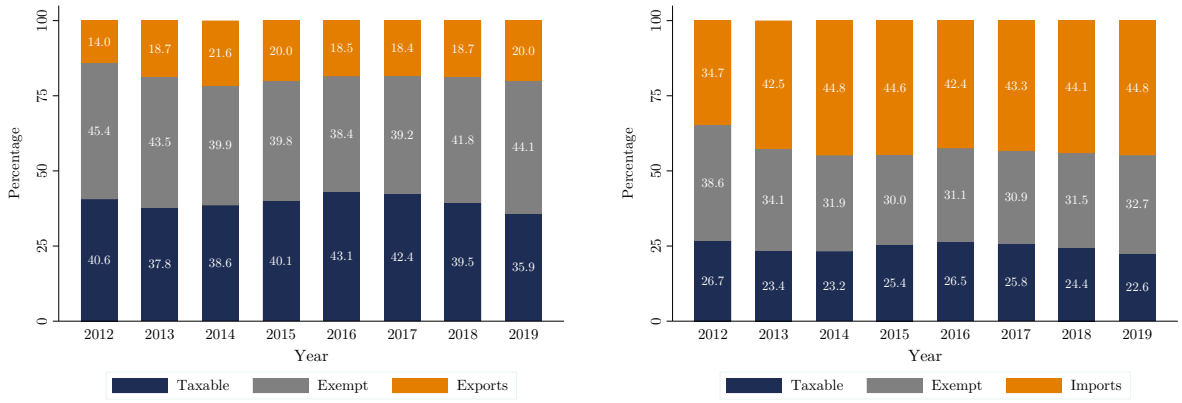
Table A6: Robustness changing estimator: Performance

	(1) Cash	(2) Wage Bill	(3) Pre-Tax Profits	(4) After-Tax Profits
TWFE estimator	-0.005	-0.003	-0.003	-0.002
	(0.00)	(0.01)	(0.01)	(0.01)
CiC estimator	-0.005	0.006	-0.005	-0.003
	(0.00)	(0.01)	(0.01)	(0.01)
Observations	10,602	10,602	10,602	10,602
Mean Dep Var 2016	0.07	0.13	0.06	0.04

Note: This table reports point estimates for additional outcomes related to firm's performance. TWFE estimator comes from Equation 2, and standard errors are clustered at the firm level. For the CiC estimator, bootstrapped standard errors are displayed. The sample is based on a yearly balanced panel of firms filling VA between 2015-2019. Each outcome was normalized by the pre-reform (2016) sales and winsorized at the 95th percentile of observations. Statistical significance is denoted as *0.10, **0.05, and ***0.01, respectively.

B Additional Figures

Figure A1: Composition of reported sales and purchases

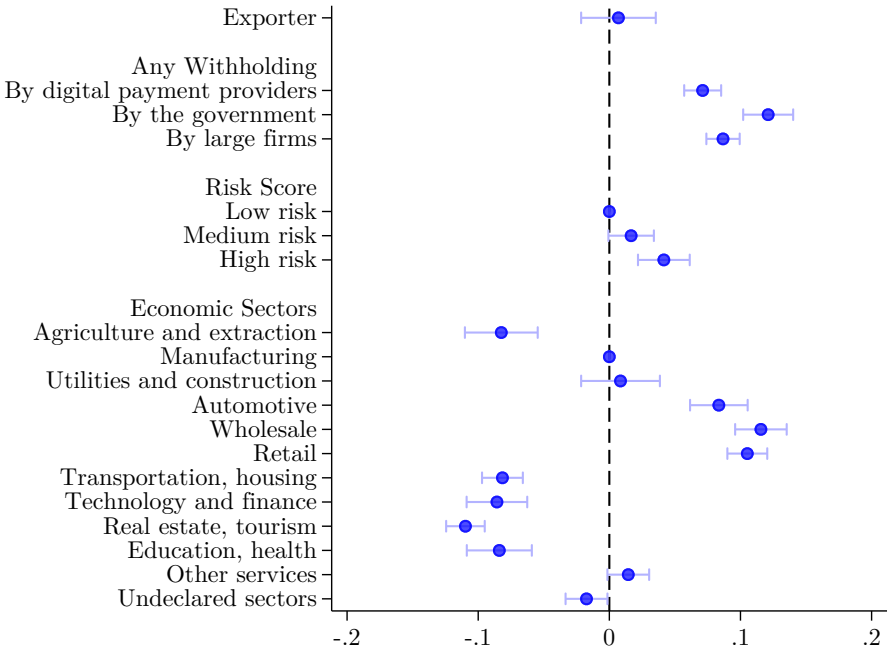


(a) Reported sales

(b) Reported purchases

Note: This figure displays aggregates on the yearly composition of sales and purchases reported by firms in the VAT forms. In 2012 and 2013, taxable sales and taxable purchases included 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: Correlates of unrefunded credits

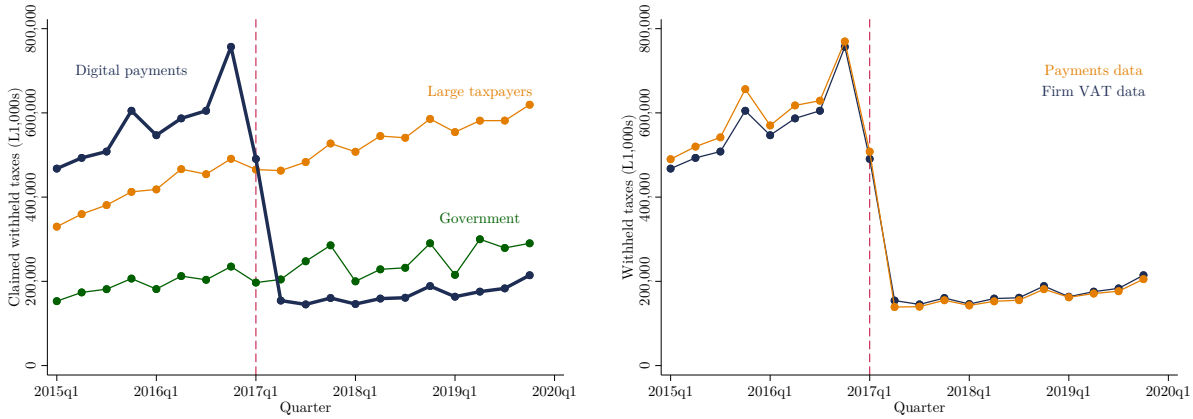


Note: This figure presents coefficients from a linear probability model using a dummy on having unrefunded credits by the end of 2016 as the outcome over a series of covariates. The risk score is computed according to the internal Risk Model of the Honduran tax authority and is defined as a measure that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

Figure A3: 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.

Figure A4: Total withheld taxes

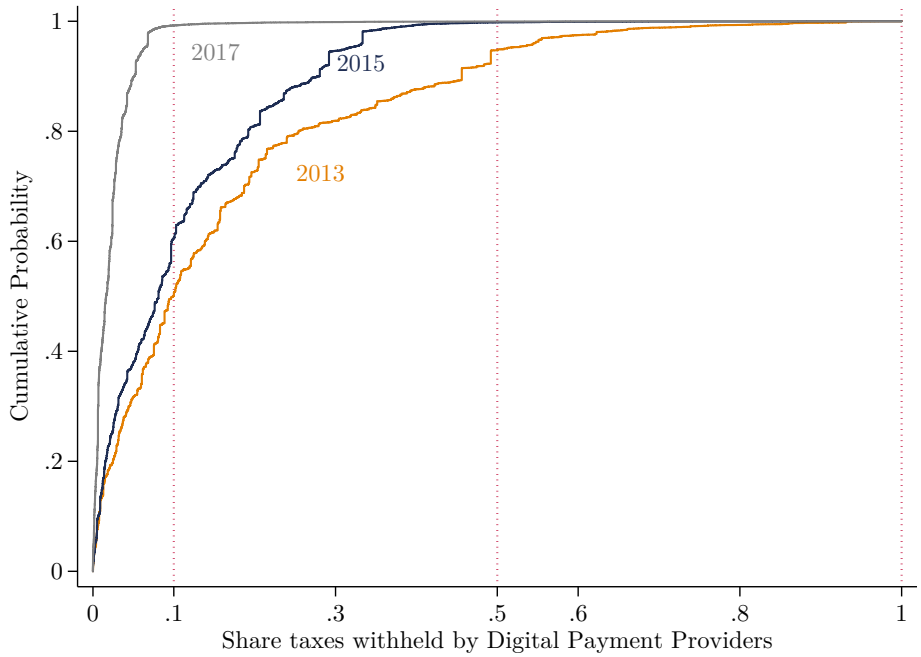


(a) Total withheld taxes by third parties

(b) VAT filing vs. payment data

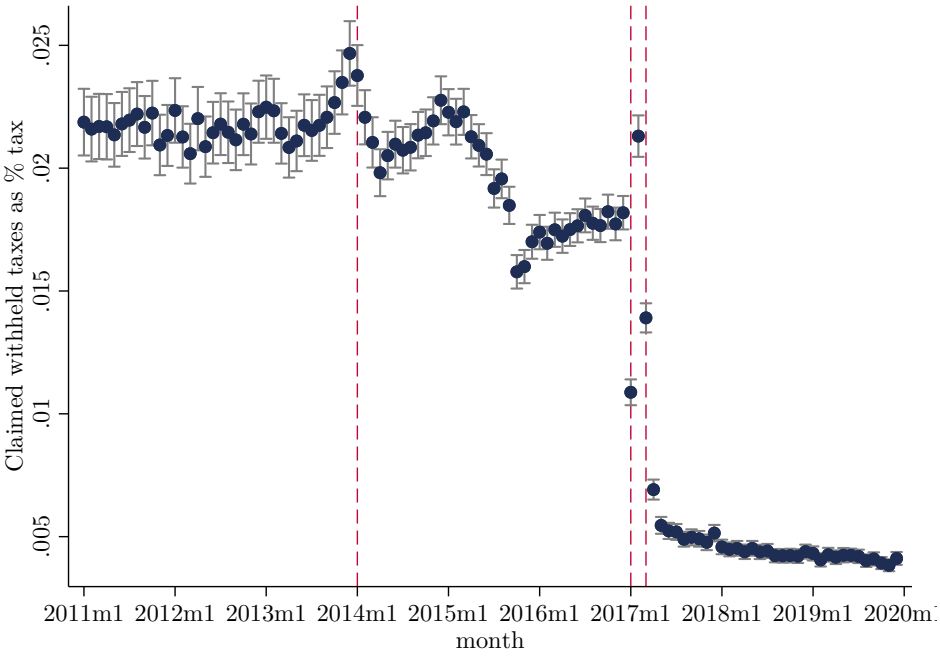
Note: This panel reports the evolution of withheld taxes. Figure A4a presents the total amount of withholding claimed by VAT filers from digital payment providers, large taxpayers, and the government. Figure A4b presents the total amount of VAT withholding from two separate sources: the VAT filing of firms that claim to withhold and the payment data submitted by digital payment providers independently to the tax authority.

Figure A5: CDF of withholding share



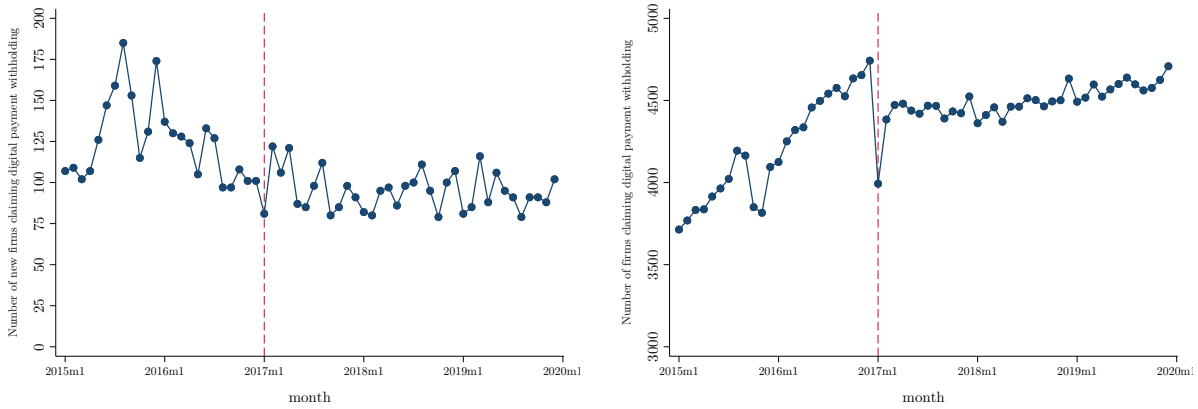
Note: This figure presents cumulative distribution functions for total withholding by digital payment providers as a share of tax liability, for firms claiming withholding. The sample excludes the period January-April in each year to maintain consistency across years while excluding the transition period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

Figure A6: Withholding changes, 2011-2019 - Full panel



Note: This figure presents changes in withholding by digital payment providers around the 2014 and 2017 reforms. It plots the mean (with 95% CI) claimed withholding by digital payment providers as a share of gross tax liability in VAT filings. Dashed lines mark key reform months (January 2014 for the first reform and the transition period January-March 2017 for the second reform). The sample includes all firm-period observations, including for firms claiming zero withholding.

Figure A7: Number of taxpayers with DCC withholding, 2011-2019

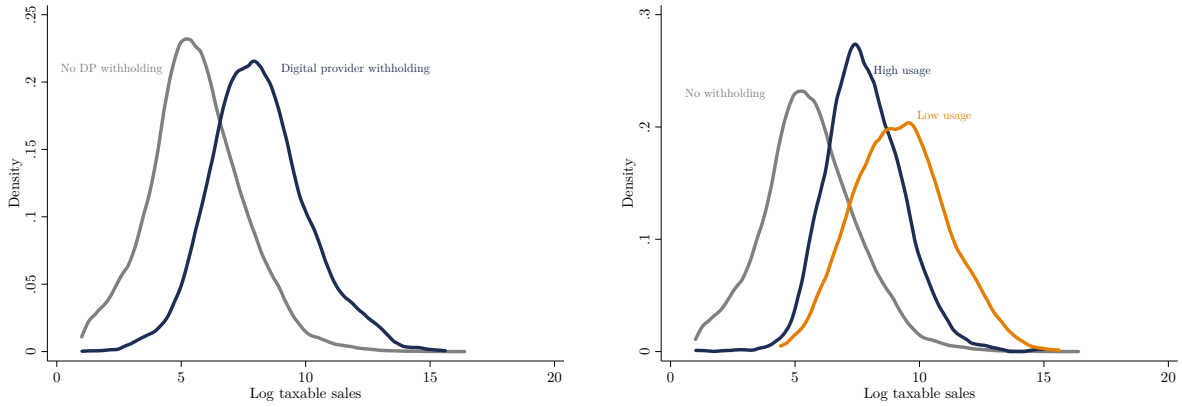


(a) DCC withholding claimers

(b) DCC usage entry

Note: This panel reports dynamics in VAT filers being withheld at source by DCC providers. Figure A7a reports the share of VAT filers claiming DCC withholdings. Figure A7b 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 A8: Firm size according to debit/credit card usage

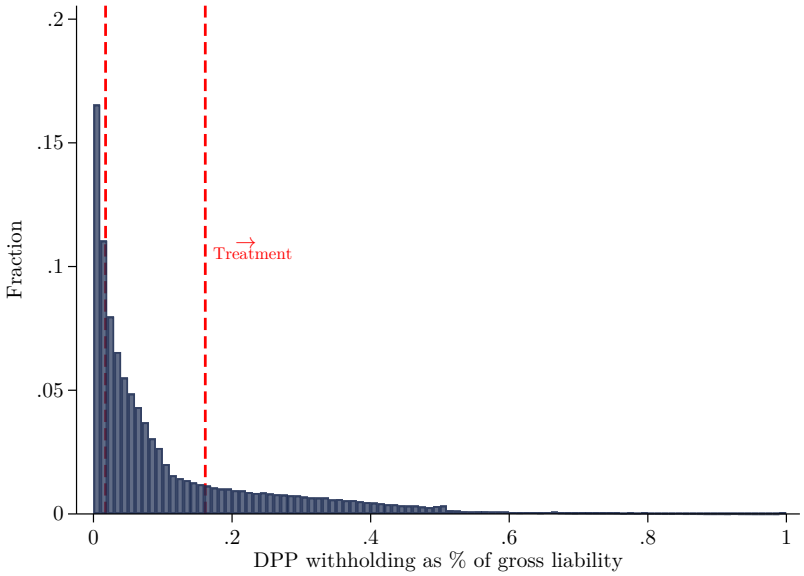


(a) Some usage vs. no usage of DCC operators

(b) Intensity of usage of DCC operator

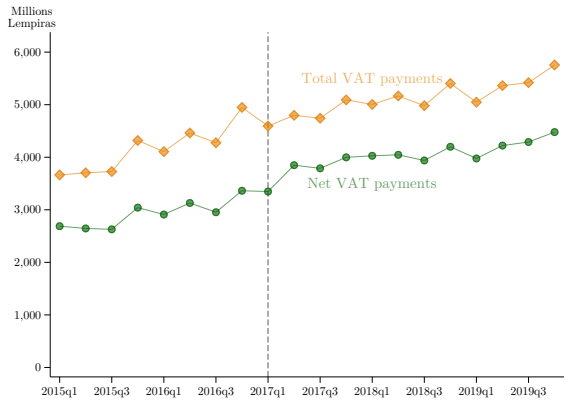
Note: This figure presents the distribution of taxable sales for VAT filers' according to their debit and credit card usage. Figure A8a presents the density for the log of taxable sales in 2016, before the 2017 reform, separately for taxpayers claiming some DCC withholding and for those claiming no withholding. Figure A8b 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 A9: DCC usage for treatment and control groups in baseline analysis

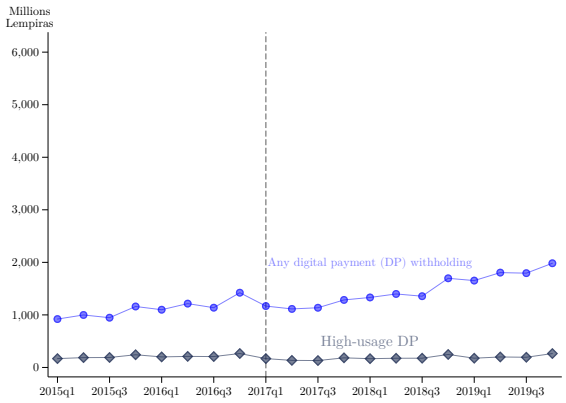


Note: This figure displays the distribution of the VAT withholding from debit and credit card payments as a percentage of the gross VAT liability. In baseline estimations, we rely on this intensive margin, so the treatment group is defined according to digital payments usage above the 75th percentile,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

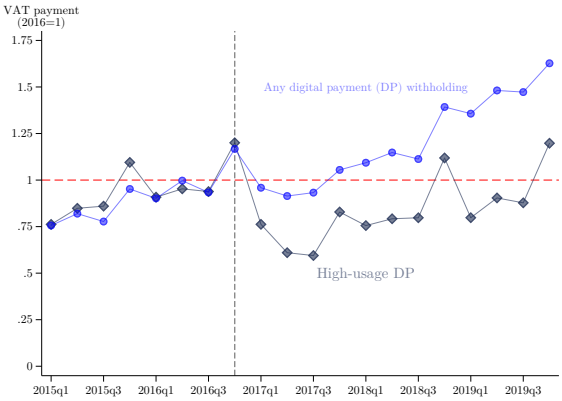
Figure A10: Dynamics of VAT payments



(a) All VAT fillers



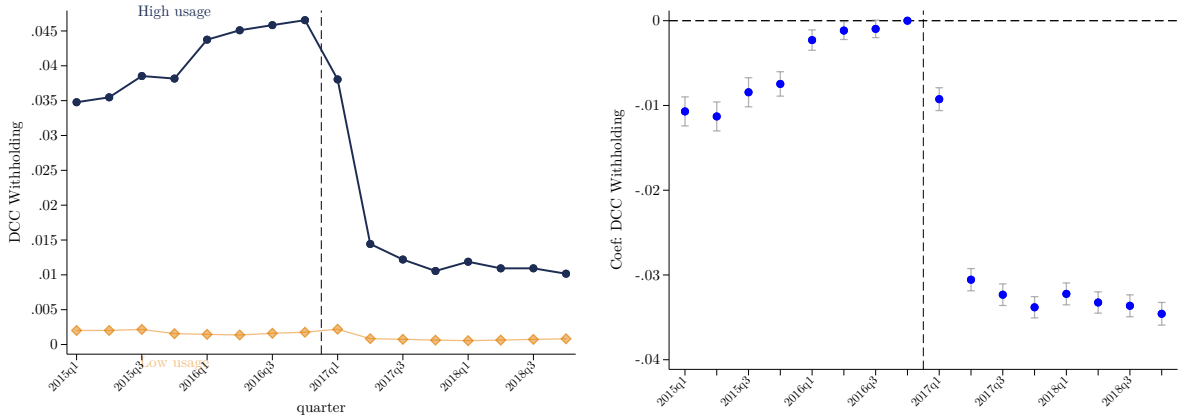
(b) VAT Payments: Digital payment users



(c) VAT Payments (2016=1): Digital payment users

Note: This panel of figures shows the dynamics of VAT payments at the quarterly level before and after the withholding reform. Figure A10a compares VAT revenue and net VAT payments for a balanced sample on the universe of VAT fillers. Total VAT payments sum all withholdings, anticipated payments, and net positive VAT due. Figure A10b shows VAT payments for firms with any digital payment withholding (in light blue) and high-usage firms, defined as those above the 75th percentile of usage (our baseline treatment status) - it shows that payments from these firms represent a small share of total VAT payments. Finally, Figure A10c shows a normalized version of VAT payments of the panel (b), where quarterly payments are divided by average payments in 2016.

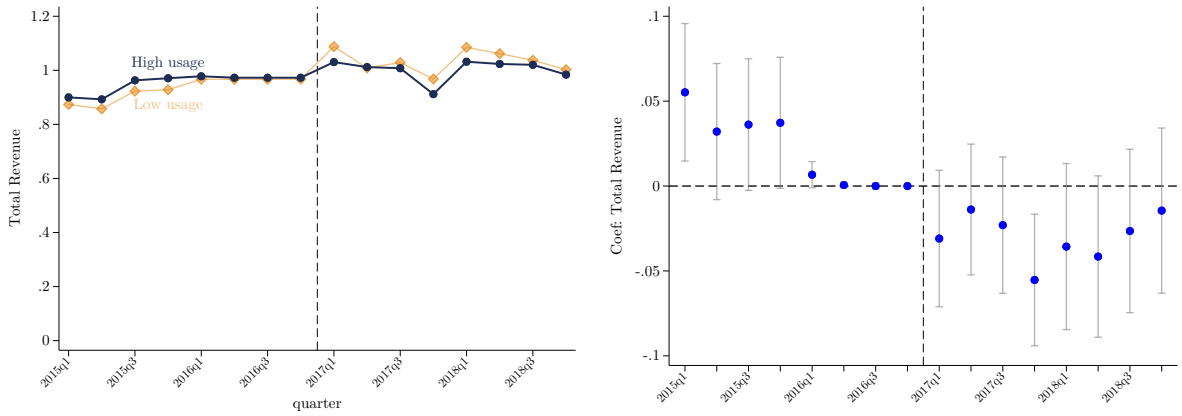
Figure A11: Difference-in-Differences, DCC withholding



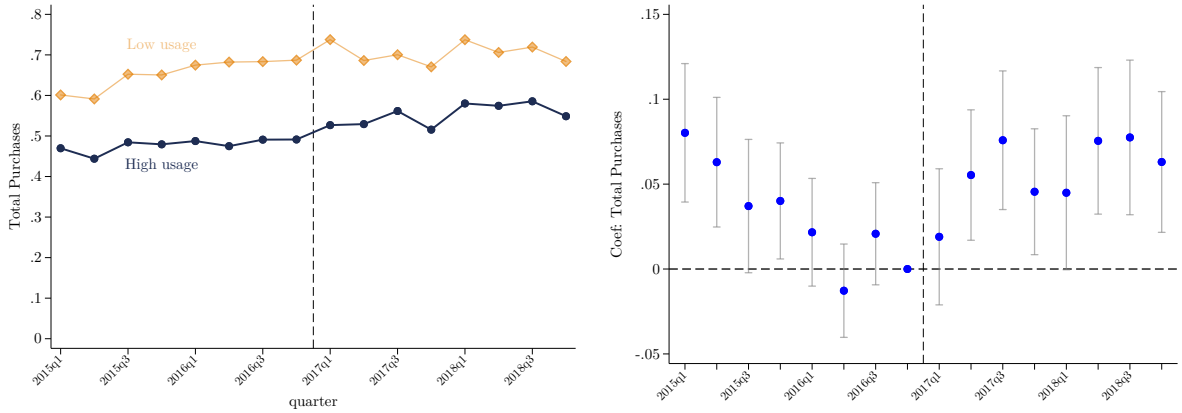
Note: This panel of figures reports dynamic DiD estimates for the effects of the reform on debit and credit card VAT withholding. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by quarterly sales in 2016 and winsorized 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.

Figure A12: Difference-in-Differences, additional VAT liabilities

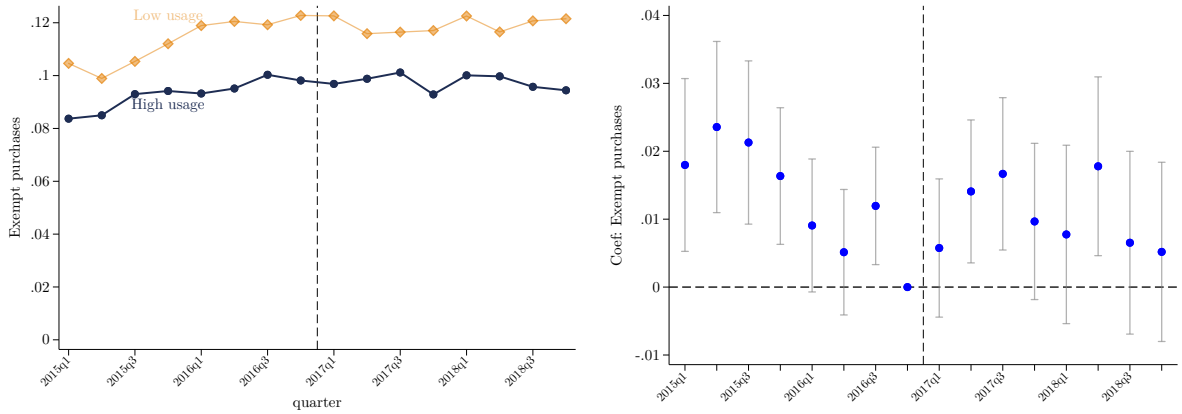
(a) Total sales



(b) Total purchases



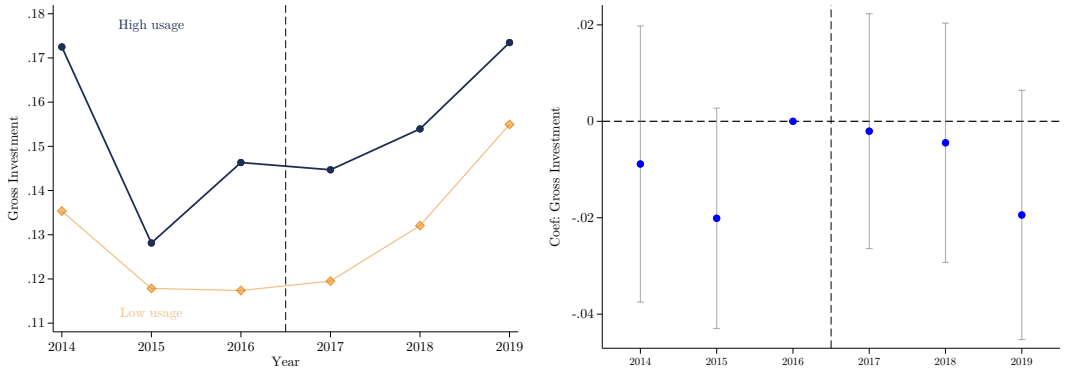
(c) Exempt purchases



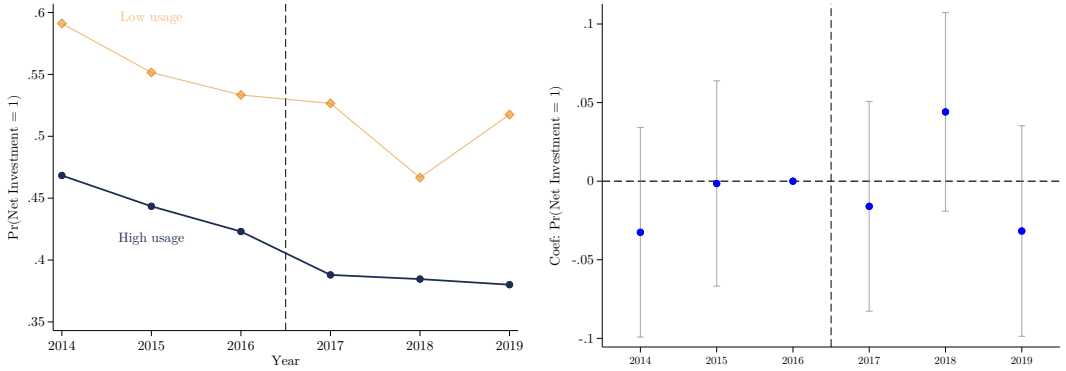
Note: This panel of figures reports dynamic DiD estimates for the effects of the VAT withholding reform on additional VAT liabilities. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. Each outcome was normalized by quarterly sales in 2016 and 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.

Figure A13: Difference-in-Differences, Gross investment

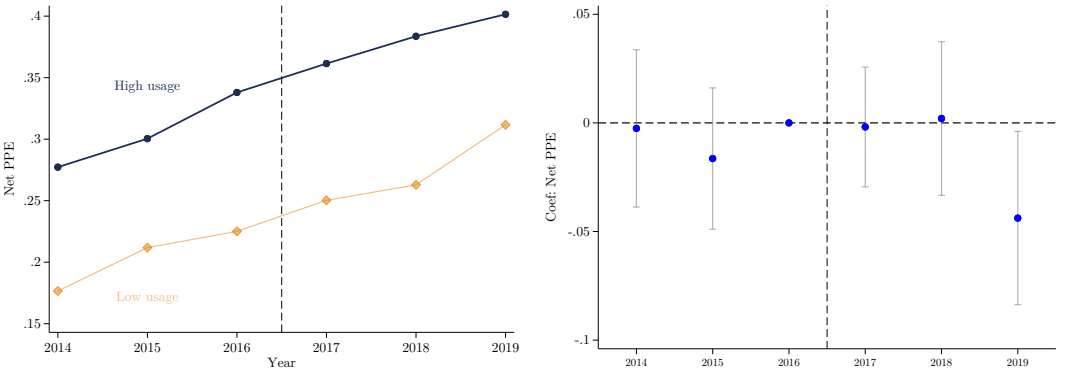
(a) Gross investment



(b) Pr(Net investment > 0)



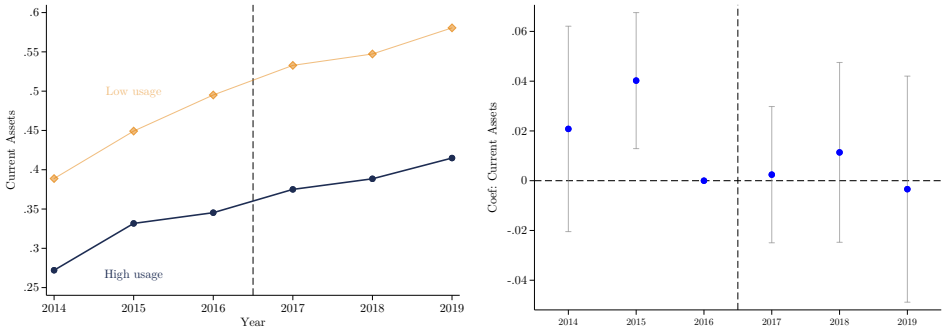
(c) Net physical assets



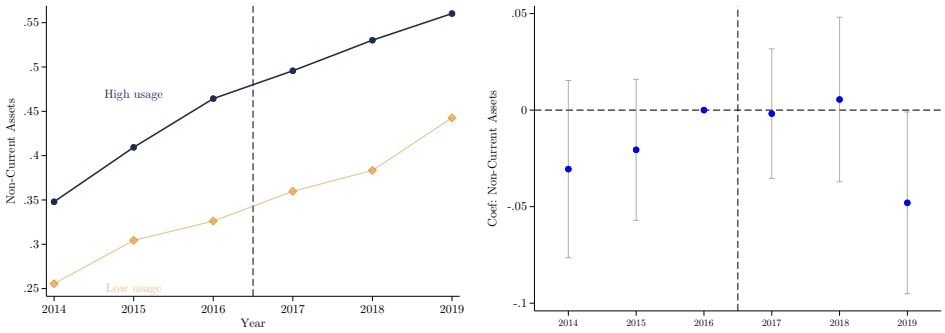
Note: This panel of figures reports dynamic DiD estimates for the effects of the VAT withholding reform on fixed assets and investment. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each of the continuous outcomes was normalized by firm sales in 2016 (the year before 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.

Figure A14: Difference-in-Differences, Balance sheet breakdown

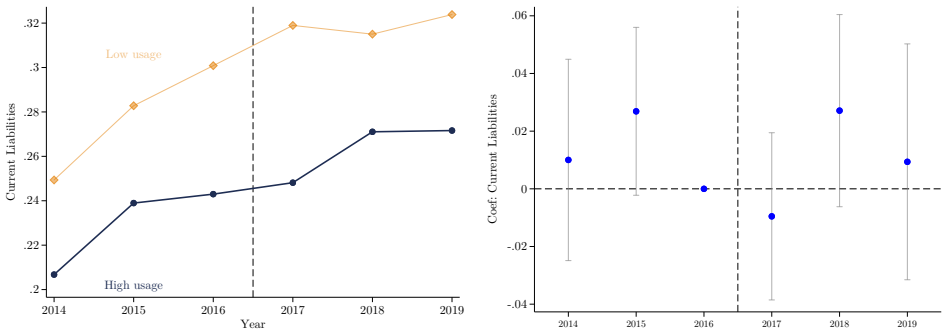
(a) Current assets



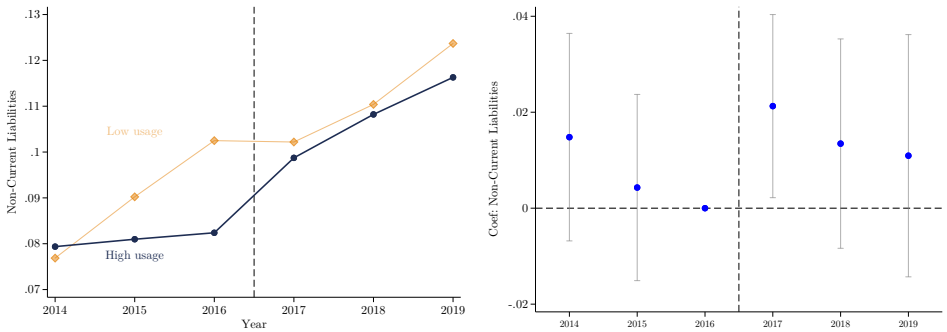
(b) Non-Current assets



(c) Current liabilities

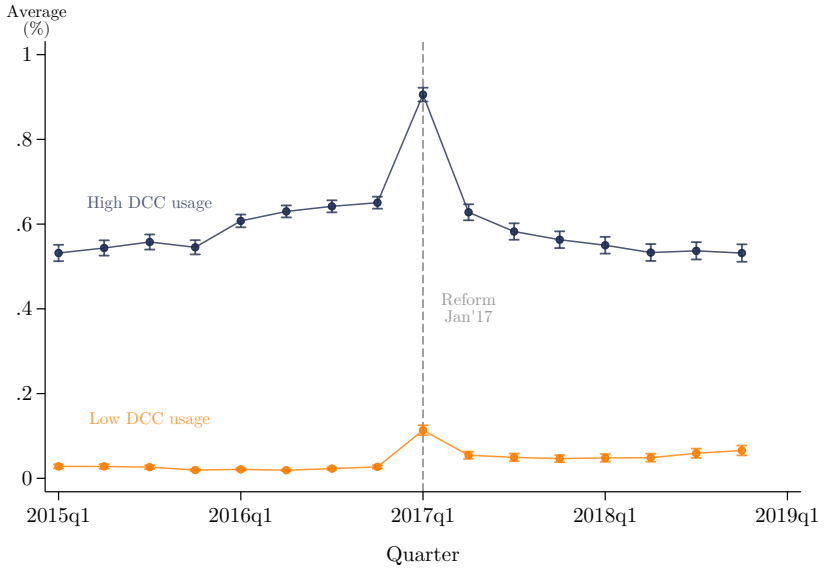


(d) Non-Current liabilities



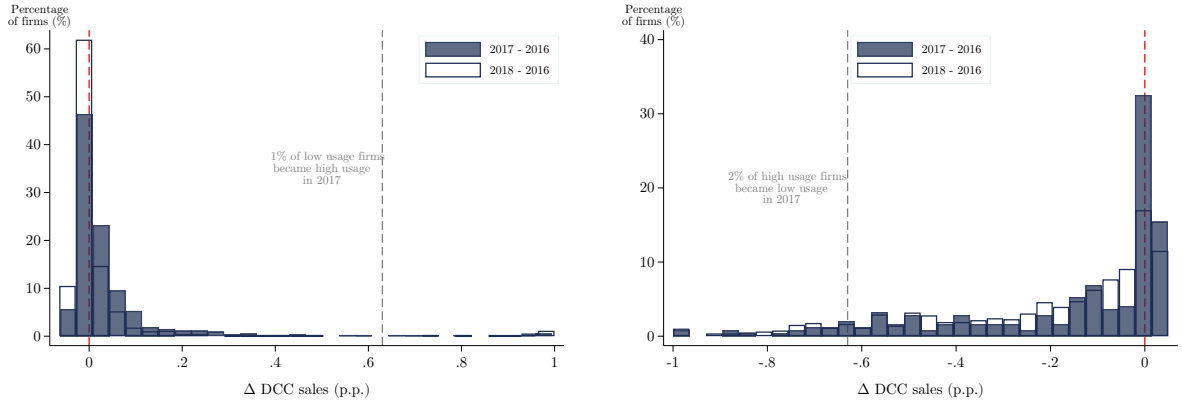
Note: This panel of figures reports dynamic DiD estimates for the effects of the VAT withholding reform on the balance sheet. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019. Each outcome was normalized by firm sales in 2016 (the year before the withholding 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.

Figure A15: Dynamics of sales with DCC payments



Note: This figure presents the time series for the average DCC sales intensity for our baseline sample of treatment and control firms, respectively. We extrapolate DCC sales intensity from VAT data as $\frac{\text{DCC withholding}_t}{\theta_{1(.)} \times \text{total sales}_t}$, with $\theta_{1(.)} \in \{0.015_{1(t\in[2015,2016])}, 0.075_{1(t\in[2017,2019])}\}$

Figure A16: Variations in sales with DCC payments

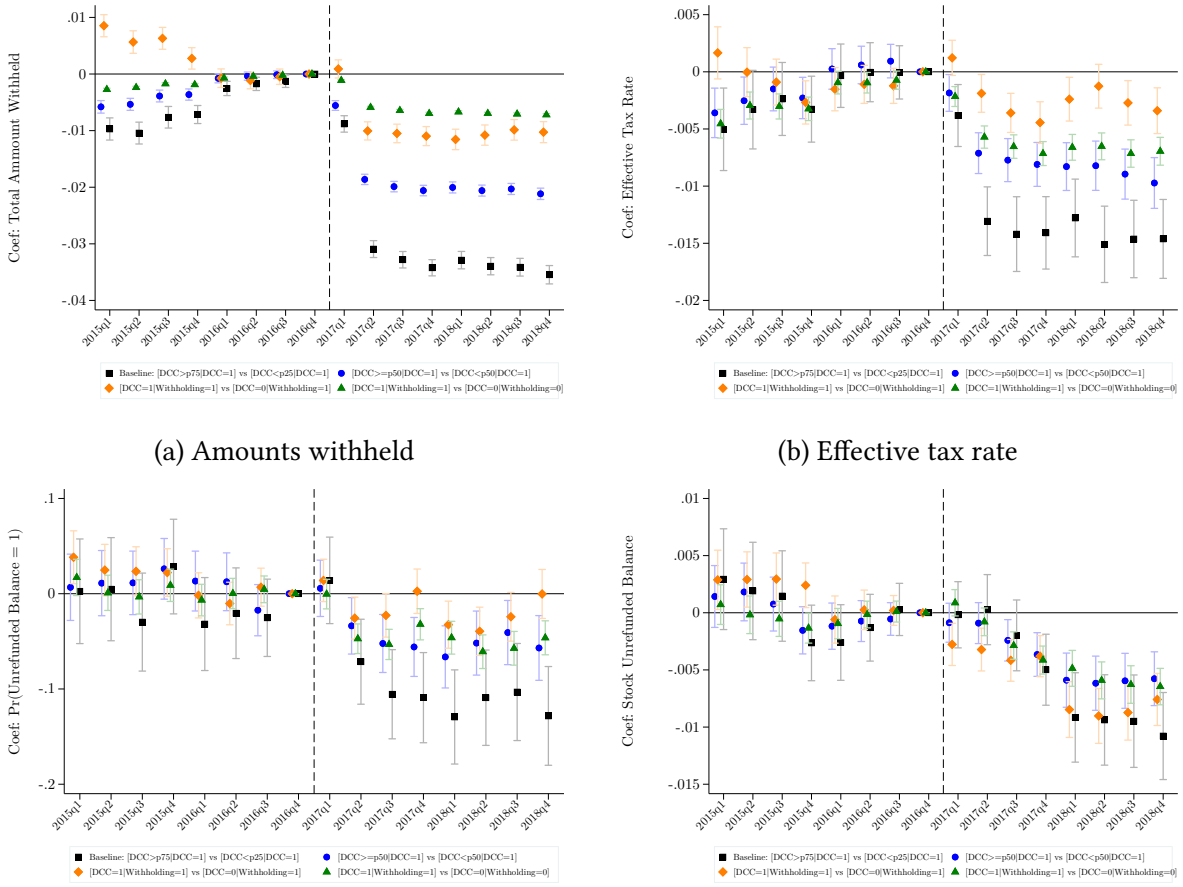


(a) Control firms

(b) Treatment firms

Note: This panel displays inter-annual variations of DCC sales intensity, measured as the ratio between sales with DCC and total turnover, as in Figure A15, but this time at the yearly level. Red dashed lines indicate zero variation, and gray dashed lines represent the average of DCC sales in 2016. Before the reform, sales made with debit/credit cards accounted, on average, for approximately 2.0% of total sales for firms in the control group (low intensity) and approximately 64.7% for firms in the treatment group (high intensity), resulting in a difference in intensities of about 63 percentage points (p.p.). We define an ex-ante low-usage firm as becoming high-intensity post-reform if its inter-annual variation in the DCC sales ratio is $\geqslant$ 63 p.p., and an ex-ante high-usage firm as becoming low-intensity if its inter-annual variation is $\leqslant -63$ p.p.

Figure A17: Robustness changing treatment definition, VAT outcomes



(a) Amounts withheld

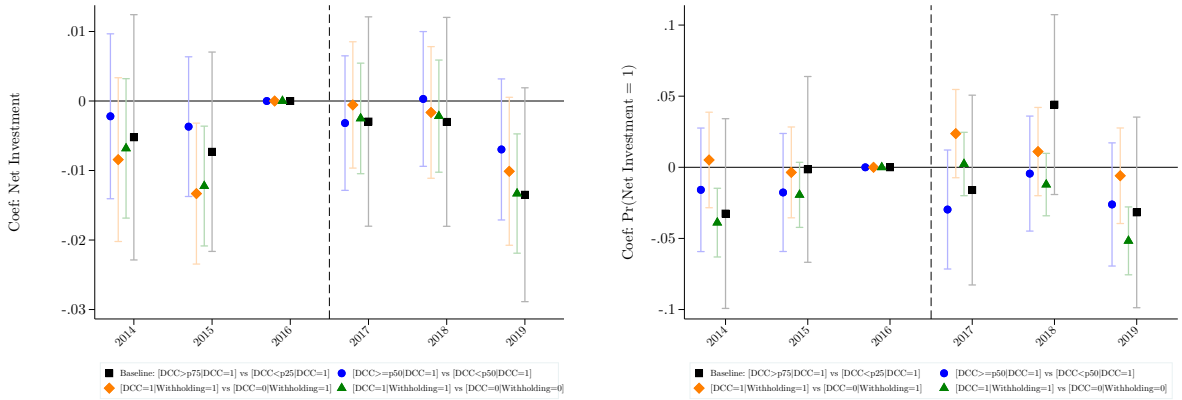
(b) Effective tax rate

(c) Pr(Unrefunded=1)

(d) Unrefunded balance

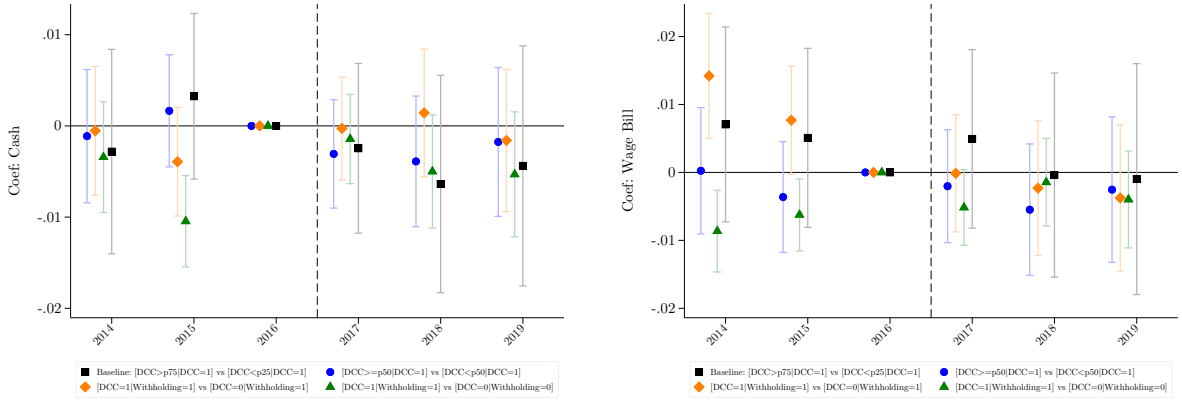
Note: This panel of figures plots the β_t for dynamic DiD coefficients from Equation 1 for compliance effects of the reform adapted to four different treatment and control groups. The dark navy square markers correspond to our baseline preferred specification where the treatment group is defined as those firms with a DCC usage-to-tax liability ratio above the 75th percentile of the usage distribution ($DCC \geqslant p75|DCC = 1$), and the control group is based on firms with usage below the 25th percentile ($DCC \leqslant p25|DCC = 1$). The circle ocher-colored markers come from regressions where the treatment group is firms with a DCC usage-to-tax liability ratio above the median of the usage distribution ($DCC \geqslant p50|DCC = 1$), and the control group is based on firms with usage below the median ($DCC < p50|DCC = 1$). The diamond olive-colored markers come from regressions where the treatment group is firms with some DCC usage ($DCC = 1|withholding = 1$), while control firms are those with any other type of withholding different from DCC, like the government or large taxpayers ($DCC = 0|withholding = 1$). The orange triangle markers come from regressions where treatment firms are those with some DCC usage ($DCC = 1|withholding = 1$), while control firms are those without any sort of withholding ($DCC = 0|withholding = 0$). Every model is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. 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 for the 95% confidence intervals (reported with bars in the right-hand graphs) are clustered at the firm level.

Figure A18: Robustness changing treatment definition, Firm's performance



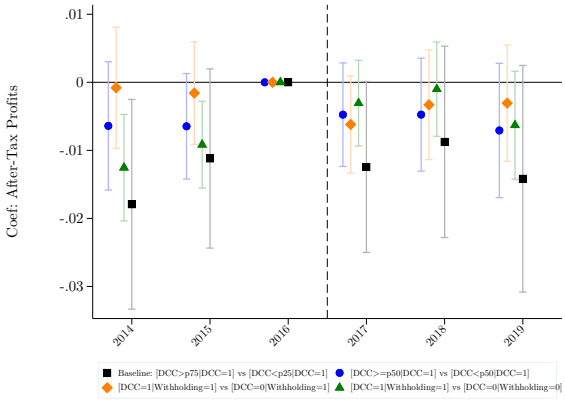
(a) Net investment

(b) Pr(Net investment = 1)



(c) Cash

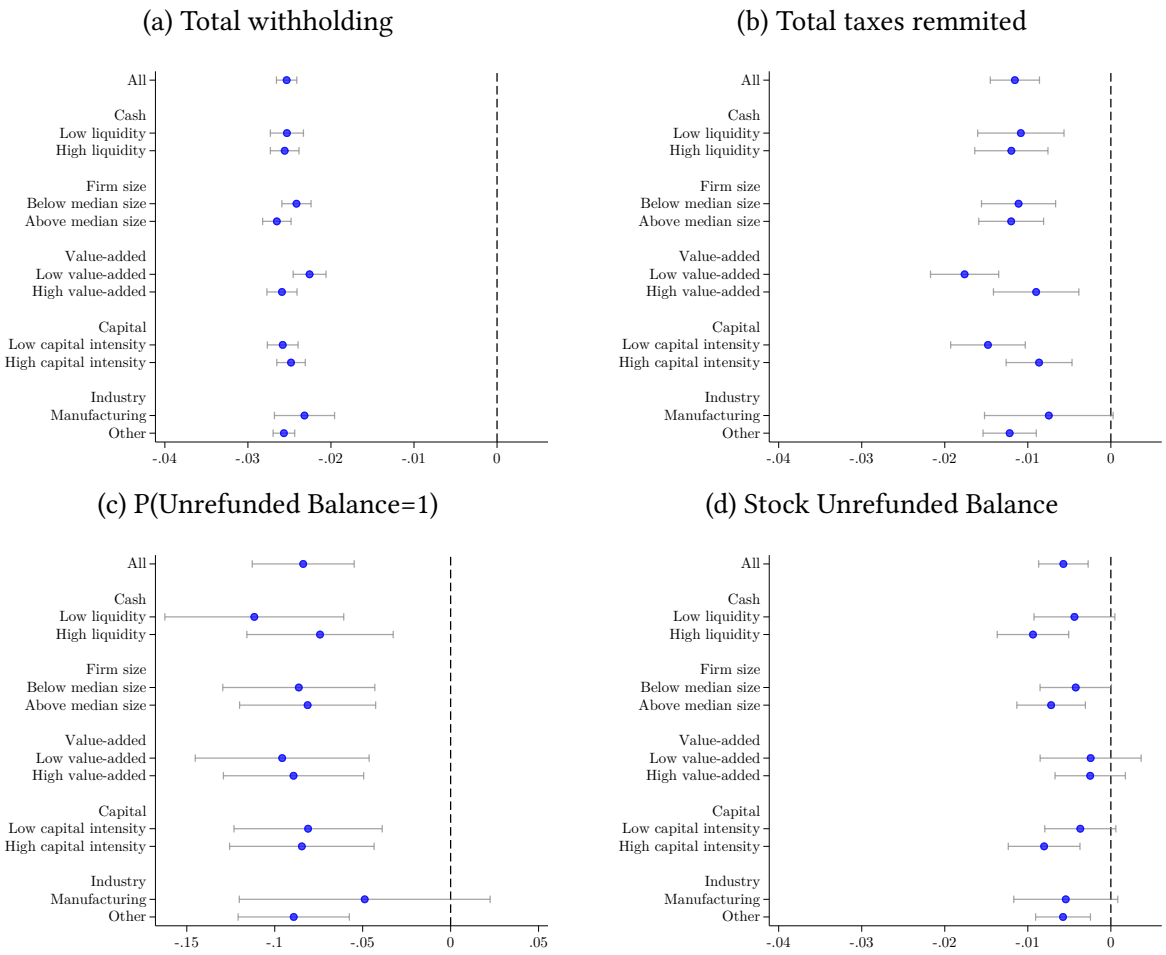
(d) Wage bill



(e) After-tax profits

Note: This panel of figures plots the β_t for yearly dynamic DiD coefficients from Equation 1 for the effects of the reform on firm's performance, adapted to four different treatment and control groups. The dark navy square markers correspond to our baseline preferred specification where the treatment group is defined as those firms with a DCC usage-to-tax liability ratio above the 75th percentile of the usage distribution ($DCC \geqslant p75|DCC = 1$), and the control group is based on firms with usage below the 25th percentile ($DCC \leqslant p25|DCC = 1$). The circle ocher-colored markers come from regressions where the treatment group is firms with a DCC usage-to-tax liability ratio above the median of the usage distribution ($DCC \geqslant p50|DCC = 1$), and the control group is based on firms with usage below the median ($DCC < p50|DCC = 1$). The diamond olive-colored markers come from regressions where the treatment group is firms with some DCC usage ($DCC = 1|withholding = 1$), while control firms are those with any other type of withholding different from DCC, like the government or large taxpayers ($DCC = 0|withholding = 1$). The orange triangle markers come from regressions where treatment firms are those with some DCC usage ($DCC = 1|withholding = 1$), while control firms are those without any sort of withholding ($DCC = 0|withholding = 0$). Every model is based on a yearly balanced panel of firms filling VAT every quarter between 2014-2019. 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.

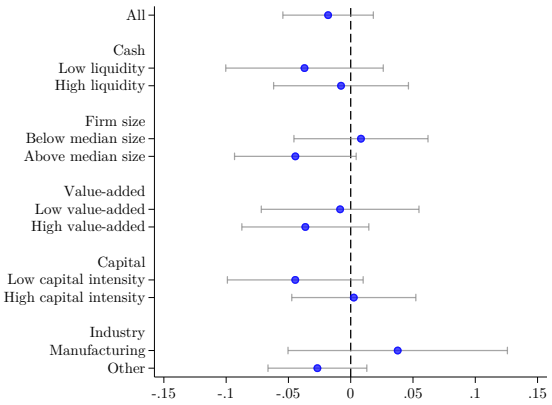
Figure A19: Heterogeneity, first stage outcomes



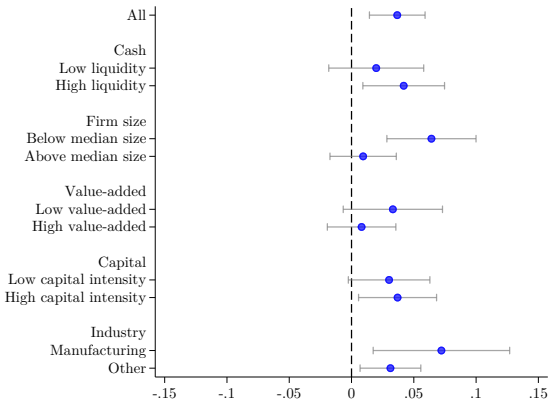
Note: This table reports heterogeneity for first stage outcomes. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. The outcome was winsorized at the 95th percentile of observations. Regressions also include firm fixed effects and year fixed effects interacted with industry and firm's size (measured by the pre-reform quantile of turnover). Standard errors are clustered at the firm level.

Figure A20: Heterogeneity, VAT liabilities

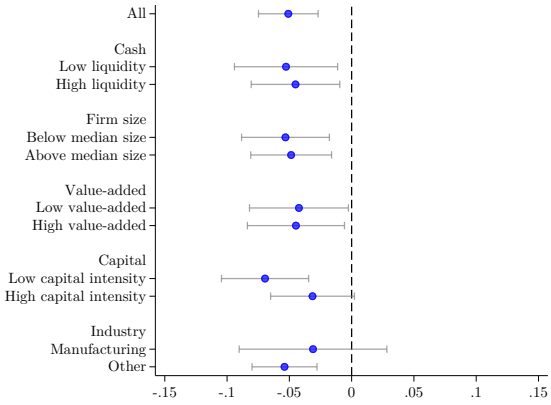
(a) Taxable sales



(b) Taxable purchases

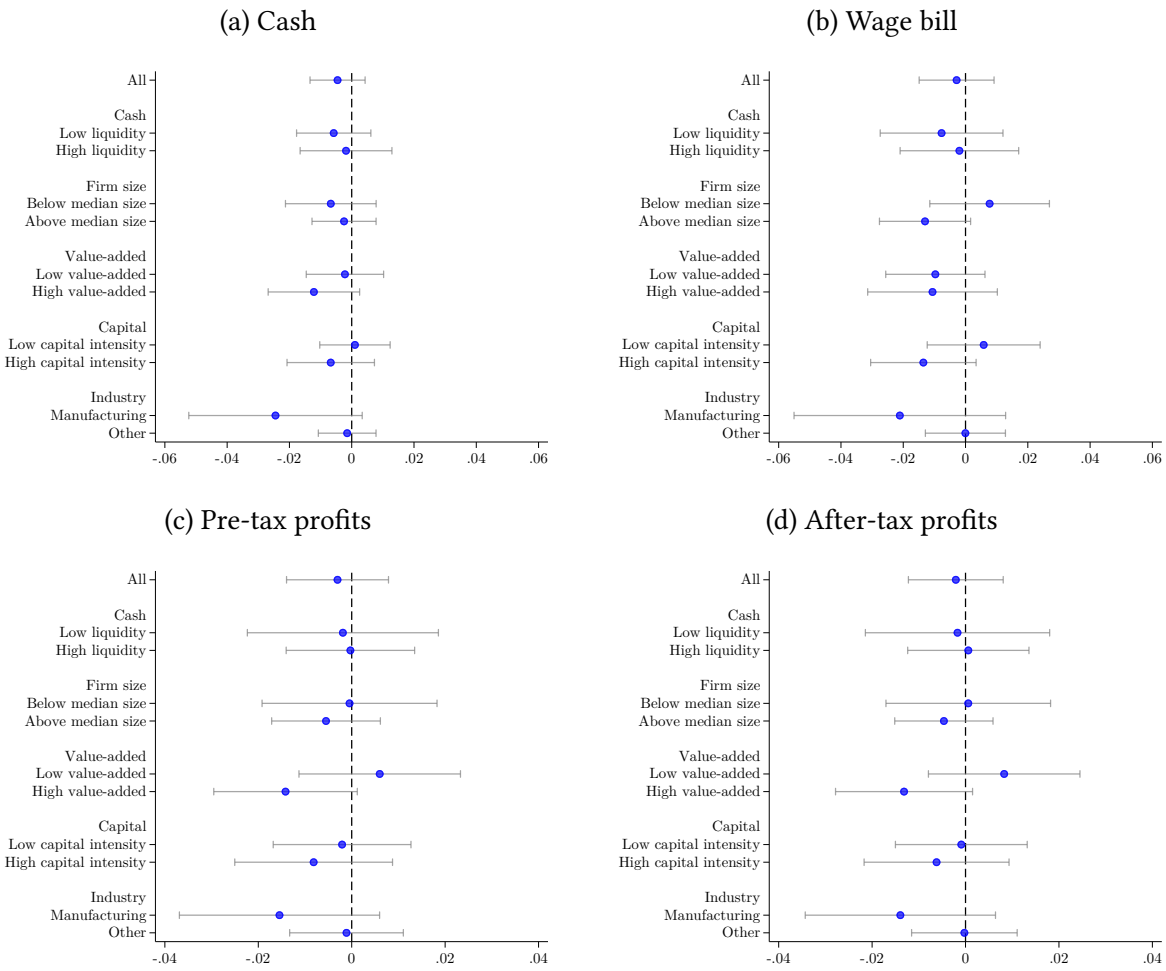


(c) Value-Added



Note: This table reports heterogeneity for VAT liabilities. The sample is based on a quarterly balanced panel of firms filling VAT every quarter between 2015q1-2018q4. The outcome was 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.

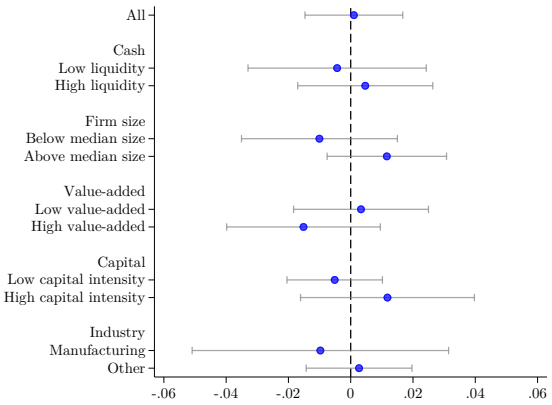
Figure A21: Heterogeneity, cash, wage bill, and profits



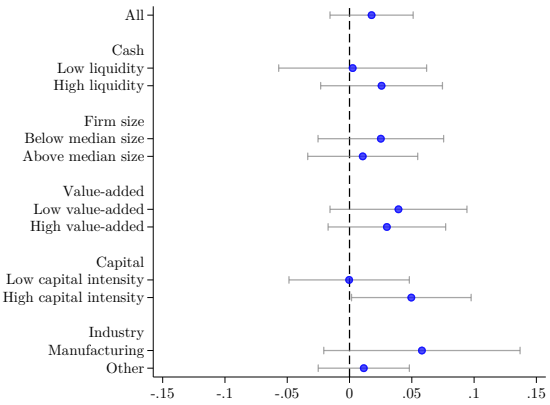
Note: This table reports heterogeneity for cash, wage bill, and profits. The sample is based on a balanced panel of firms filling VAT every quarter between 2014-2019, but estimations are made at the year not the quarter level. Every continuous outcome was normalized by the pre-reform (2016) yearly sales 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 A22: Heterogeneity, gross investment

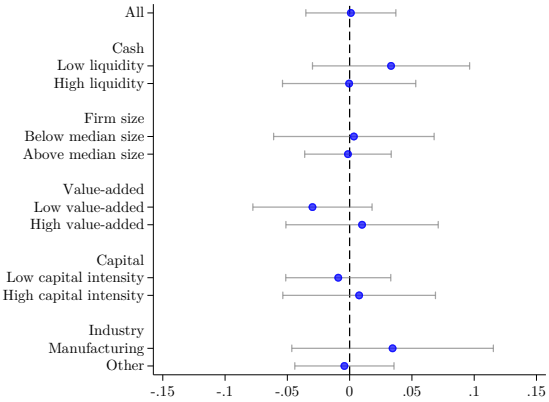
(a) Gross Investment



(b) P(Gross Investment>0)



(c) Gross PPE



Note: This table reports heterogeneity for gross investment. 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.

C Definition of VAT Outcomes

Empirical analysis of the 2017 withholding reform is based on two different sets of outcomes from two administrative sources[1]: 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, tax remittance, unrefunded balances, 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.

[1] 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/

D Definition of Balance Sheet Outcomes

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 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, following 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 including depreciation and amortizations. That is:

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

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

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

- **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.

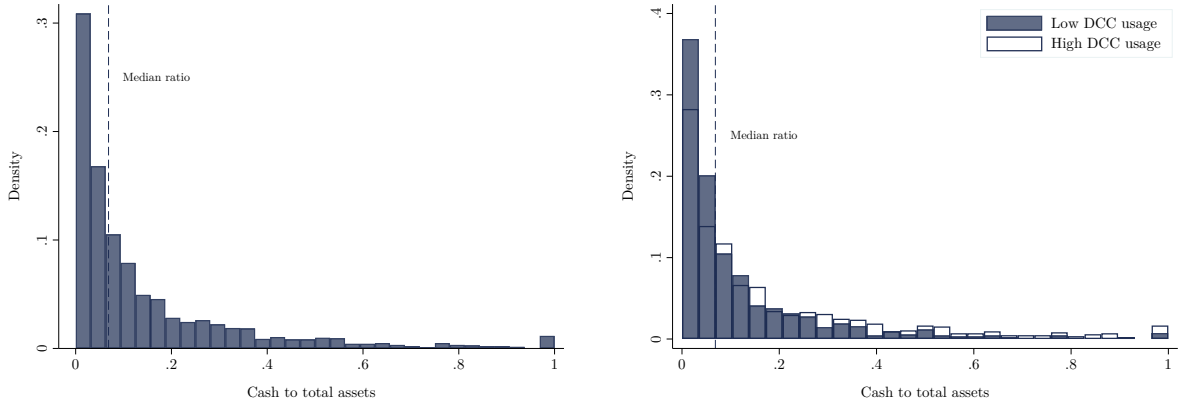
- **Pre Tax Profits.** Correspond to the taxable income or the result of the calendar year (taxable income - deductible costs) before taxes.

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

E Defining Liquidity Constrained and Capital Intensive Firms

This section illustrates the metrics for our definition of liquidity-constrained and capital-intensive firms for heterogeneity analysis. Liquidity-constrained firms have a cash-to-assets ratio below the median of the unconditional distribution (see Figure A23). In addition, capital-intensive firms are those with a physical assets-to-sales ratio above the median of the unconditional distribution (see Figure A24).

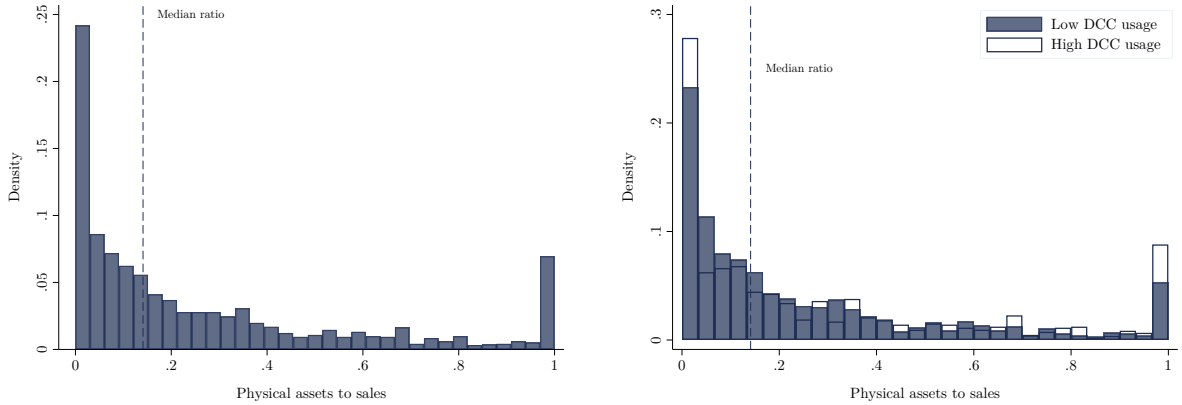
Figure A23: Distribution of cash to assets ratio: Pre-reform average



(a) Unconditional distribution (b) Conditional distribution

Note: This figure reports the distribution for the pre-reform average of the cash to total gross assets ratio. The histogram comes from our baseline sample, a balanced panel dataset covering firms filing VAT between 2015-2018. The histogram is built as follows: we first compute the yearly cash-to-total gross assets ratio, then aggregate its mean for all the years before the reform (2015-2016), and this average is what we plot.

Figure A24: Distribution of physical assets to sales ratio: Pre-reform average



(a) Unconditional distribution (b) Conditional distribution

Note: This figure reports the distribution for the pre-reform average of the physical assets to sales ratio. The histogram comes from our baseline sample, a balanced panel dataset covering firms filing VAT between 2015-2018. The histogram is built as follows: we first compute the yearly physical assets to sales ratio, then we aggregate its mean for all the years before the reform (2015-2016), and this average is what we plot.