

Electronic Payment Technology and Tax Compliance: Evidence from Uruguay’s Financial Inclusion Reform[†]

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Does the digitization of transactions in an economy increase tax compliance? We study the effect of financial incentives on the adoption of electronic payment technology and on tax compliance by firms. Exploiting administrative data and policy variation from Uruguay, we show that (i) consumer VAT rebates for credit and debit transactions trigger an immediate 50 percent increase in the number of card transactions, (ii) firms’ use of card machines increases only on the intensive margin, and (iii) tax compliance is unaffected. Endogenous card machine adoption and a low share of card sales in total reported sales can rationalize the findings. (JEL E42, H25, H26, O16)

The idea that the digitization of transactions through electronic payment technology can help increase tax compliance has been prominent in academic circles (e.g., Rogoff 2016) and in the policy advice provided by international organizations (OECD 2018; Gupta et al. eds 2017; World Bank 2016), and it is reflected in actual policy implementation, most prominently in India’s 2016 demonetization campaign (Das et al. 2022). Unlike cash transactions, electronic transactions are processed by a third party, distinct from the two transacting partners, creating a paper trail which governments can access for tax compliance purposes. The existence of such a third-party paper trail, combined with a tax audit function which leverages the information, can deter taxpayers from underreporting taxable transactions (Kleven

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et al. 2011; Pomeranz 2015; Naritomi 2019). This would increase reported taxable sales and tax liabilities. Following this logic, governments in numerous countries have attempted to accelerate the pace of digitization through fiscal incentives for transactions conducted with electronic payment methods (see Supplemental Appendix Table A.1 for an overview).

Yet, whether such policies have the intended effect on tax compliance depends on endogenous technology adoption decisions by firms and consumers and on the share of transactions ultimately covered by electronic records. If only firms which are already tax compliant respond to the incentives, or if electronic records cover a smaller share of transactions than the share which firms already report for tax purposes, an increase in electronic transactions might not affect tax compliance. In addition, electronic records can help deter evasion only if tax administrations actually use them to detect misreporting, and if taxpayers are aware of this.

We study the effect of VAT rebates on the adoption of electronic payment technology and on tax compliance, exploiting policy variation from Uruguay in regression discontinuity and difference-in-difference estimations. The rebate program was introduced in August 2014, at a time when Uruguay lagged behind peer countries in key financial inclusion measures (Supplemental Appendix Figure A.1). There was significant scope to increase the use of electronic payment technology, and the reform program provided large and salient incentives: The rebates reduced the VAT payable on debit card transactions by up to 40 percent. The rebates were immediately granted to customers paying by card, without the need for refund claims or other hassle costs. There was also significant scope to increase tax compliance, as Uruguay's VAT evasion rate of over 20 percent was double the evasion rate in higher-income countries (Dirección General Impositiva, Uruguay 2019). We evaluate the VAT rebate program using transaction-level data on all electronic transactions and monthly firm-level VAT declarations for the years 2006–2015.

We document three main results. First, we use the high frequency of our data and a regressions discontinuity design in time to show that the introduction of the rebates led to an immediate 50 percent increase in the number of debit and credit card transactions, and a 30 percent increase in the volume of card transactions. To establish the validity of our research design, we show that the increase emerges sharply in the first week of August 2014, when the rebates were introduced, after otherwise stable and approximately linear trends. The month-on-month growth rates of the number and volume of card transactions in the reform month are more than an order of magnitude higher than the month-on-month growth rate at any other point during 2011–2015. Consumers are hence extremely responsive to the incentives. Firms are much less responsive. The number of point-of-sales (POS) terminals in use increased by 10 percent between July and August 2014, but this effect is entirely driven by firms which already used a POS prior to the reform. The number of firms with at least one POS does not increase discontinuously with the reform, and there is no acceleration in the POS adoption trend after the reform. We also study the consumer response to a second reform in August 2015, which lowered the size of the VAT rebates. We find that the number and volume of card transactions does not decline, suggesting that even temporary incentives can generate a lasting increase in consumer use of electronic payment technology.

Second, we examine the impact of the rebate-triggered increase in card transactions on tax compliance, leveraging a difference-in-difference estimation that compares treated retail sector firms that had a POS prior to 2014 to wholesale sector firms. The comparison of retailers to wholesalers is motivated by the fact that retailers are *ex ante* less tax compliant than wholesalers, as the VAT self-enforcement mechanisms typically breaks down at the point of sale to the final consumer (Naritomi 2019); and only retailers with POS are directly treated by the reform, as the VAT rebates do not apply to firm-to-firm transactions nor to cash transactions. We find that retailers with POS and wholesalers exhibit parallel trends in reported sales and other outcomes prior to the introduction of the VAT rebates, and no divergence thereafter. The difference-in-difference treatment effect is close to zero and precisely estimated. Consistent with this, the treatment effect on reported output VAT and net VAT liability is also statistically indistinguishable from zero. This means that tax compliance was unaffected, and the VAT rebates generated an overall fiscal cost of about 1.5 percent of VAT revenue.

Finally, we discuss how to reconcile the large consumer response to the VAT rebates with the null effect on tax compliance. One explanation for the results is that firms self-select into using POS, weighing costs and benefits. The costs include variable and fixed costs for POS usage and a potential increase in required tax payments, while the benefits are the retention or attraction of customers and the speeding up of transactions. Our results suggest that the strong increase in consumer demand for card payments after the VAT rebate introduction was not sufficient to increase POS adoption by firms on the extensive margin. This is consistent with the fact that firms experience an increase in their tax liability after adopting a POS, as we show in monthly event studies. We also find no evidence that firm POS adoption responds to subsidies for POS usage, to a reduction in tax withholding rates applied by card processing companies or to a reduction in the commissions charged on card transactions. This suggests that accelerating firms' adoption of POS would require much larger financial incentives or a mandate obliging firms to offer card payment facilities.

The second explanation for our results is the fact that, even among retail and wholesale firms with a POS, card sales constitute on average less than 30 percent of total reported sales, and less than 20 percent in the majority of firms. This means that firms already report a large share of their cash sales. Thus, even if cross-checks between the card sales and firms' self-reported sales, combined with audits on misreporters, create a lower bound on what firms report for tax purposes, the relatively high compliance level means that firms have room to increase card sales without increasing their total reported sales.

This study connects to several sets of literature. First, financial inclusion and the use of financial technology have been shown to have far-reaching development benefits (Jack and Suri 2014; Dupas and Robinson 2013; Burgess and Pande 2005). Technologies for electronic identification and transaction processing have been shown to enhance governments' capacity to manage expenditure and prevent leakages (Muralidharan, Niehaus, and Sukhtankar 2016; Banerjee et al. 2020). It is thus a natural extension to investigate the contribution of electronic payment technology to enhancing also other aspects of state capacity, namely tax capacity (Okunogbe and Santoro 2021).

The mechanism through which electronic payment technology can impact tax capacity—the generation of third-party reports on taxable transactions—has been prominently discussed in the public finance literature (Kleven et al. 2011; Jensen 2019). Pomeranz (2015) and Naritomi (2019) show that third-party reporting improves VAT compliance in Chile and Brazil respectively.¹ Closely related to our study is Das et al. (2022), who show that India’s demonetization campaign led firms to significantly increase reported taxable sales, and likely also tax liabilities. Demonetization led to a much larger increase in electronic sales than Uruguay’s reforms, as it essentially made 86 percent of cash in circulation illegal overnight. Delays in printing new currency led to a sharp increase in the take-up of electronic payment methods. In fact, the volume of electronic sales increased by 500 percent for the average firm. This is not only driven by increased usage intensity but also by a 134 percent increase in the number of POS in use—a key finding that distinguishes India’s experience from Uruguay’s. However, while demonetization likely improved tax compliance, it also had large economic costs, and is hence at best a debatable strategy for policymakers wishing to promote electronic payment technology with a view to improving tax compliance.

Our work also relates to a set of studies evaluating government policies to generate third-party reports on firm-to-firm transactions through VAT annexes (Mittal and Mahajan 2017; Fan et al. 2018) or electronic billing systems (Ali et al. 2022; Lovics et al. 2019; Bellon et al. 2019; Bérgholo et al. 2017). These studies use firm-level data and leverage difference-in-difference or event studies techniques. They typically find positive effects of the technology on firms’ reported income or tax liabilities. The distinction between these studies and ours is twofold. On the one hand, we focus on a technology which has many benefits beyond its potential effect on tax compliance. On the other hand, unlike e-billing systems, the technology we focus on is intended to cover not all transactions a firm makes, but only a subset of transactions. This distinction is key for explaining the lack of a tax compliance effect we demonstrate, and has not previously been emphasized.

Finally, our study connects to parts of the finance literature studying the use of electronic payment technology by consumers (Arango et al. 2015; Agarwal et al. 2007; Bolt et al. 2010) and firms (Beck et al. 2018; Dalton et al. 2018; Arango and Taylor 2008). Our results differ from those in Higgins (2022), who shows that an increase in debit card ownership led retailers in Mexico to adopt POS. This may be due to differences in the policy variation—the Mexican government provided debit cards to one million households—or due to differences in the policy context—the Mexican government can access POS information only in the case of an audit, while the government in Uruguay automatically receives information on all electronic transactions from card processing companies.

The paper is organized as follows. Sections I and II lay out some conceptual considerations and present the policy background and the data we use. Sections III and IV examine the impact of VAT rebates on the use of electronic payment technology

¹ Carrillo, Pomeranz, and Singhal (2017) and Slemrod et al. (2017) show that third-party reporting is not a panacea, since firms might offset increased third-party reporting (and hence tax compliance) on the sales margin by increasing reported costs.

and on tax compliance. Section V discusses the interpretation of the results and Section VI discusses policy implications. Section VII concludes.

I. Conceptual Considerations

To guide our empirical analysis, we briefly discuss how the expansion of electronic transactions may affect tax compliance. Consider that firms have true sales $S = C + E$, where C are cash sales and E are electronic sales, i.e., sales paid for by electronic payment methods. Reported sales R may be smaller than true sales, $R \leq S$. That is, firms may misreport their true sales to minimize their tax liability. However, it is reasonable to assume that firms have to report at least $R_{min} = E$, as electronic sales are reported to the tax authority by credit/debit card companies and are routinely cross-checked with firms' tax declarations. Reporting $R < E$ would thus trigger a discontinuously higher audit probability, as discussed in Carrillo, Pomeranz, and Singhal (2017).

Define R_0 as the level of reported sales prior to the introduction of VAT rebates and R_1 as the level of reported sales after the introduction of VAT rebates. Define E_0 and E_1 analogously, so that ΔE is the increase in electronic sales triggered by the VAT rebates. For simplicity, we assume for now that $\Delta E = -\Delta C$, so the VAT rebates lead consumers to switch from paying in cash to paying by card, but do not affect overall consumption. We are interested in whether $R_1 > R_0$. Given the above-mentioned audit rule, firms have to report $R_1 \geq E_0 + \Delta E$ after the introduction of VAT rebates. So firms' reporting behavior will change if $R_0 < E_0 + \Delta E$, that is, if the consumer response $\Delta E/E_0$ to the VAT rebates is sufficiently large and the share of true sales reported to the government prior to the reform, R_0/S , is sufficiently low.²

II. Background and Data

This section describes the relevant aspects of Uruguay's tax system, the policy variation generated by the financial inclusion reforms and the data we use.

A. Tax System

Firms in Uruguay are liable for an annual corporate income tax (CIT) at 25 percent and remit a monthly VAT. The VAT is levied at a standard rate of 22 percent, with a reduced rate of 10 percent for necessity goods such as basic food products. Large firms which are part of the large taxpayer office called CEDE (*Control Especial de Empresas*) file and pay the VAT monthly. All other firms (henceforth called non-CEDE firms) file the VAT annually, but report output VAT, input VAT and

²Our discussion focuses on revenue reporting, as any change in compliance in our setting should be driven by a change in reported sales. Since there is no evidence for a change in reported sales in response to the VAT rebates, there is no reason for reported costs to change. We thus do not consider cost adjustments.

net VAT for each month in their annual VAT declaration.³ In 2015, there were 4,099 CEDE firms and 60,640 non-CEDE firms registered.

Credit and debit card companies in Uruguay report all card transactions of their client firms (i.e., firms using their POS) to the tax authority. The tax authority uses the card transaction reports to cross-check taxpayers' self-assessment declarations, and to strengthen the credibility of enforcement among taxpayers with discrepancies between self-reported and third party-reported income.⁴ Bérigolo et al. (2018) show that firms in Uruguay perceive the audit probability over a three-year period to be 40 percent, although the true audit probability is 8 percent. Taxpayer perceptions are roughly consistent with survey responses indicating that 20 percent of taxpayers had experienced some control activity from the tax administration in the previous year. Of these controls, about half focused on verifying discrepancies or third-party information (United Nations 2014). It is thus reasonable to consider that firms are aware of the use of third-party information in the tax enforcement process.

Despite this, Uruguay faced a significant tax evasion challenge, which the financial inclusion reform intended to tackle. According to tax administration estimates, at least 20 percent of potential VAT revenues were evaded in 2012, corresponding to a revenue loss of 2.5 percent of GDP (Dirección General Impositiva, Uruguay 2019). Gomez Sabaini and Jiménez (2012) provide an even higher VAT evasion estimate of 26.3 percent. In contrast, the VAT gap in the United Kingdom was only 11 percent in 2012, and it was below 10 percent in many Western European countries.⁵ Uruguay also registered a higher level of informality than most other countries at a similar income level (Supplemental Appendix Figure A.1, panel B). While the VAT represents less than 20 percent of tax revenue in high-income countries on average, Uruguay relied on the VAT for almost half of its tax revenue, meaning that the large VAT gap was particularly problematic for Uruguay.⁶

It is thus not surprising that one of the government's objectives in designing the financial inclusion reform was to reduce tax evasion. Specifically, the Ministry of Finance stated on its financial inclusion website that they intended to promote a "more efficient functioning of the payments system, strengthening the use of electronic payment technologies instead of cash" because "these measures [...] promote the formalization of the economy and combat tax evasion, in addition to strengthening efforts against money laundering."⁷ The tax administration made similar statements and in fact targeted some of its inspections at large retailers that did not accept

³In Supplemental Appendix Section A.1, we discuss why firms in simplified tax regimes should not be affected by the VAT rebates we study.

⁴https://www.dgi.gub.uy/wdgi/page?2,principal,_Ampliacion,O,es,0,PAG;CONC;30;11;D;dgi-inspecciona-comercios-que-no-permiten-el-pago-con-tarjetas-de-debito;6;PAG, accessed on November 9, 2022.

⁵<https://www.gov.uk/government/statistics/measuring-tax-gaps-tables>, accessed on November 9, 2022, and Barbone et al. (2013, 38). These macro-level tax gap estimates rely on input-output tables to estimate potential VAT revenue and then compare it to actual VAT revenue (Hutton 2017).

⁶Government Revenue Dataset: <https://www.ictd.ac/dataset/grd/>, accessed on November 9, 2022. Income tax evasion may contribute to exacerbating this challenge. In most countries, evasion is higher for the VAT than for other taxes, but in Uruguay, there is evidence of significant income tax evasion as well. For instance, Bérigolo et al. (2020) find that 15.5 percent of income tax filers underreported their wage.

⁷<https://inclusionfinanciera.uy/por-que/>, accessed on November 9, 2022.

card payments, a behavior that the government interprets as an indicator of evasion risk.⁸

B. VAT Rebates for Consumers

The main policy variation we exploit in this paper is generated by large VAT rebates for consumers using electronic payment methods. These rebates became available on August 1, 2014, and apply to all types of goods and services purchased by final consumers.⁹ The rebate rates vary across card types, across transaction amounts, and over time, as shown in Supplemental Appendix Figure A.2.

Debit card transactions of up to 4,000 *Unidades Indexadas* (UI, a Uruguayan accounting unit)—approximately US\$500—initially received the highest subsidy rate of 4 percentage points (ppt). Larger debit card transactions, other electronic payments, and credit card transactions of up to 4,000 UI were granted a 2 ppt rebate. In August 2015, the rebates for debit card and credit card transactions up to 4,000 UI were decreased to 3 ppt and 1 ppt respectively.¹⁰ Further rate changes took place in later years, but these are not considered in this study. The moderate VAT rates mean that the VAT rebates granted for card payments are very large, implying a 40 percent tax reduction in the case of reduced-rate goods of a value of less than 4,000 UI purchased with a debit card. This rebate corresponds to a reduction of the tax-inclusive price of 3.3 percent for standard-rated goods and of 3.6 percent for reduced-rated goods. For comparison, São Paulo's e-receipt program studied by Naritomi (2019) provided smaller consumer VAT rebates of on average one percent of the consumer's total purchase value.

The implementation of the rebate system is illustrated in Supplemental Appendix Figure A.4. Importantly, consumers pay the tax-inclusive price net of the rebate at the time of purchase, so rebates are immediately devolved to consumers. Put differently, consumers do not have to request a refund nor incur a hassle cost. The rebate is stated on a consumer's transaction receipt, which makes it highly salient, as shown in Supplemental Appendix Figure A.5. The rebates were also introduced with great media fanfare (Supplemental Appendix Figure A.6), so consumers should have been well aware of their existence.

Firms are required to file their VAT declaration as if they had charged the consumer the full VAT, at either the standard or the reduced rate, whichever applies. Credit and debit card companies processing the card transactions observe the amount of VAT rebates a firms' consumers have been granted each month. These companies then provide a fiscal credit of the monthly aggregate firm-specific rebate amount to their client firms. These fiscal credits are transferred to firms together with the processed credit/debit card transaction amounts. The credit and

⁸For instance, see https://www.dgi.gub.uy/wdgi/page?2,principal,_Ampliacion,O,es,0,PAG;CONC;30;11;D;dgi-inspecciona-comercios-que-no-permiten-el-pago-con-tarjetas-de-debito;6;PAG, accessed on November 9, 2022.

⁹Decree 203/014. Rebates are granted only for firm-to-consumer transactions, and not for firm-to-firm transactions, i.e., any transactions in which the client requests the tax ID number of the seller.

¹⁰Supplemental Appendix Figure A.3 shows that there is no bunching in transaction amounts at the 4,000 UI threshold, likely because the vast majority of the transactions are much smaller than the threshold, and because the UI-peso conversion rate is updated on a daily basis, making it difficult to bunching without updating prices frequently.

debit card companies are then reimbursed for these credits by the government. These reimbursements happen monthly, so that firms should not experience a significant change in liquidity due to the granting of VAT rebates.

Supplemental Appendix Figure A.7 shows that the VAT rebates were indeed granted starting in August 2014, as per the legislation. The figure displays a sharp increase in the share of firms registering VAT rebates to consumers in August 2014. The share of retail firms registering VAT rebates reaches almost 50 percent. This means that nearly all retailers with a POS (52 percent of retailers) registered VAT rebates. In contrast, only 15 percent of wholesale firms registered any VAT rebates, as these firms sell largely to other firms, with only a small share of their output going to final consumers. The jump in the number and volume of rebates granted immediately as the VAT rebates become available also suggests that price discrimination between consumers paying in cash and those paying by card is limited.^{11,12}

C. Other Financial Inclusion Measures

The VAT rebates were not introduced in isolation, but rather as part of a package of measures aimed at enhancing financial inclusion for its many benefits. The 2014 reforms were also accompanied by a large media and public engagement campaign raising awareness about the benefits of financial inclusion. Aside from the VAT rebates, the most important policy measures included the lowering of commissions for POS usage, the reduction of tax withholding rates applied by card companies, subsidies for POS rental for firms, mandates for wages and pensions to be paid into bank accounts and the provision of free bank accounts with debit cards to all citizens. While these other policies can amplify the effect of the VAT rebates, none of them was introduced concurrently with the VAT rebates. We hence leverage this additional policy variation in Section V to help interpret our main results. We now discuss each policy measure in turn.

The lowering of commission fees—the variable fee that card processing companies charge for transactions—preceded the main financial inclusion reform. As of January 1, 2012, the maximum commission for debit card payments was reduced from 7 percent to 2.5 percent, and the maximum commission for credit card payments to food retailers, pharmacies, and a specified number of other sectors fell to 4 percent. For foreign payment cards and some other types of transactions, the commissions were capped at 4.5 percent to 4.9 percent. These commission caps, affecting 96 percent of all transactions, were self-imposed by the card processing industry.

¹¹ Price discrimination is illegal and consumers are encouraged to report businesses engaging in this behavior to the Consumer Protection Agency. The tax administration aims to identify and inspect firms engaging in price discrimination, e.g., https://www.dgi.gub.uy/wdgi/page?2.principal_Ampliacion,O.es,0.PAG;CONC;30;11:D:dgi-inspecciona-comercios-que-no-permiten-el-pago-con-tarjetas-de-debito;6;PAG, accessed November 14, 2022.

¹² Two earlier types of VAT rebates are worth mentioning, as they explain why the share of firms registering VAT rebates is slightly above zero prior to August 2014. First, starting in January 2006, consumers received a 9 percentage point (ppt) VAT rebate on credit/debit card purchases in hotels and restaurants (Law 17.934 and decree 537/005). The retail and wholesale sector does not include hotels and restaurants, but sector codes are prone to errors, so we expect a certain degree of misclassification. The reform predates data availability, and is thus not part of this study. Second, starting in September 2012, users of social security debit cards (*Tarjeta Uruguay Social* or *BPS Prestaciones*) benefited from a 22 percentage point reduction—i.e., a complete elimination—of the VAT and firms benefited from a waiver of VAT withholding on these transactions (Decree 288/012). We do not study this reform, as it should affect tax compliance only in upstream firms and not in the directly affected firms selling to incentivized consumers.

In exchange, the government reduced the tax withholding rates applied by card companies on card transactions, introduced legislative changes to facilitate the interoperability of card networks, and provided financial subsidies to expand the use of POS. Starting from January 2012, tax withholding rates on non-CEDE firms were reduced from 5 percent to 2 percent (see Supplemental Appendix Figure A.8). Card network businesses investing in POS and POS accessories that would be rented out to firms were granted tax credits for their investments. Starting from September 1, 2012, firms with a turnover below UI 4,000,000¹³ (approximately US\$500,000) and newly created firms were eligible for a subsidy for POS rental fees. Eligibility was determined based on a firm's turnover reported in the last corporate income tax declaration, and the high turnover threshold implied that roughly 80 percent of all firms were eligible for the subsidy.¹⁴ Until December 2013, the subsidy rate was 100 percent of the rental cost of a POS, which is equivalent to approximately US\$10 per month. Starting in January 2014, the subsidy rate was reduced to 70 percent, and remained at this level until December 2017.

Together with the passage of the financial inclusion law on April 24, 2014, it was announced that many types of payments would gradually have to be made through electronic payment channels. The law set out a schedule for these mandates to enter into effect over 2014–2015 (see Supplemental Appendix Table A.2), though several of the timelines were ultimately postponed. Most importantly, wage earners and pensioners were given the option to request payment into a bank account (rather than in cash) starting in October 2015. To prepare for the implementation of the mandates, the financial inclusion law required banks to offer free bank accounts that fulfilled certain criteria (specified numbers of free transfers, withdrawal etc.).¹⁵

Supplemental Appendix Figure A.9 shows that the use of bank accounts and electronic payment technology in Uruguay increased significantly between 2011 and 2017, much more than in most other countries over the same period.

D. Data

To study the effect of electronic payments on tax compliance, we merge multiple administrative datasets (Dirección General Impositiva, Uruguay 2009–2016). First, we use transaction-level card payment data, which contain the universe of transactions between 2007 and 2016. Credit and debit card companies send these data to the tax administration every month. The data contain the transaction date, transaction

¹³Four million UI is also a threshold for other laws and regulations. For example, firms whose income in the previous fiscal year was above 4 millions UI are required to have formal accounting and no longer qualify for the simplified income tax regime (Decree 150/007, article 168).

¹⁴Decrees 288/012, 319/014, and 351/015. Very few firms that were not eligible for the subsidy received it. There is little mass and no bunching in the distribution of turnover at the eligibility threshold, suggesting no manipulation of the eligibility criteria. There is also no discontinuity in any of the outcomes studied below at the turnover threshold. It is unclear whether firms would have expected the subsidy to be temporary.

¹⁵Having to offer the free bank accounts became a mandate for banks in October 2015. For wage earners and social benefit recipients who did not exercise the option to create a bank account by June 2016, the employer or social security agency had to choose a financial institution for the beneficiary by September 2016. It became mandatory for wages and pensions to be transferred into bank accounts from May 2017 onwards. In 2014, 43 percent of respondents in the World Bank Global Findex Survey (Demirguc-Kunt et al. 2011 and 2017) indicated having used a debit or credit card in the previous year.

amount, VAT rebate amount, the tax ID of the firm, and a POS identifier. We can thus count the number of POS a firm uses. We collapse the data at the firm-month level.¹⁶ While we refer to these data as the card payment data for simplicity, it is important to note that these data contain all electronic transactions (e.g., including transactions via apps such as PayPal, Square, etc.).

We merge the card transaction data with monthly VAT returns, containing all line items from the tax return.¹⁷ Our main outcome variables are output VAT (i.e., VAT on sales), input VAT (i.e., VAT paid on inputs and deducted from output VAT), and the net VAT liability ($= \max\{\text{output VAT} - \text{input VAT}, 0\}$).

Information on firms' sector of activity is obtained from the firm registry, which contains the six-digit CIIU industry code for all firms (*Clasificación industrial internacional uniforme*). In the CIIU, the first two digits of the CIIU code capture the division. Division number 46 designates wholesale firms and division number 47 designates retail firms. The firm registry also documents in which one of Uruguay's 19 departments the firm is located.

Finally, we have access to the list of firms that received the subsidy for POS rental, with the months during which the firm received the subsidy and the total subsidy amount each month. We use corporate income tax records to confirm firms' turnover and hence their eligibility for the POS rental subsidy.

Supplemental Appendix Figure A.10 shows that the number of VAT filers has increased steadily over time, with a mild slowdown in the growth rate in 2014 and 2015. There is thus no indication that the introduction of the VAT rebates motivated previously informal firms to register. Supplemental Appendix Table A.3 provides summary statistics for the full sample of VAT filers, and for retail firms without POS, retailers with a POS, and wholesale firms, the latter two groups being the treated and control firms in our difference-in-differences analysis. Retail firms are very similar to wholesale firms in terms of the distribution of their annual sales and VAT liability, except at the top of the distribution, where wholesale firms are larger. Retailers with POS are more similar to wholesalers than retailers without POS. They have higher turnover and tax liability, are less likely to be sole proprietorships and more likely to be incorporated. The share of firms using a POS is over 50 percent in the retail sector, but only 16 percent of the wholesale sector. In both sectors, POS usage increases with firm size.

III. Use of Electronic Payment Technology

We begin our analysis by evaluating the impact of VAT rebates on the use of electronic payment technology. As the rebates became available to all consumers nation-wide on the same day, we examine the effect of the rebates on aggregate outcomes. We use a regression discontinuity estimation in time around August 1, 2014, when the rebates became available. In the following sections, we present our empirical strategy, the results and robustness tests.

¹⁶ A variable indicating the type of card transactions (debit or credit card) is available only since August 2014.

¹⁷ These data are also used and described in Bérigolo et al. (2023) and Foremny et al. (2018).

A. Empirical Strategy

We use the following variables to measure the use of electronic payment technology on the extensive and intensive margin: the aggregate number of card transactions, the volume of card transactions, the number of POS in use, and the number of firms with at least one POS. Figure 1, panel A, plots the raw time series of these outcomes between January 2010 and June 2016. Some of the series, especially the number and volume of transactions, exhibit seasonal variation with peaks in December and during the spring holiday season. We thus need to deseasonalize the data while estimating the regression discontinuity. Concretely, we estimate

$$(1) \quad \log(Z_{t,m}) = g_m + \sum_{k=0}^p (\beta_k \cdot t^k + \gamma_k \cdot \text{PostJuly2014}_t \cdot t^k) + u_t,$$

where $Z_{t,m}$ is the aggregate outcome in time period t and month-of-year m , g_m are month-of-year fixed effects, t^k is a time trend, the *PostJuly2014* dummy indicates months after July 2014 (i.e., post-reform months), p is the degree of the polynomial we fit (either 1 or 2), and u_t is the error term.¹⁸ The inclusion of the post-reform indicator and its interaction with the time trend allows both the trend and the level of the outcome to change with the reform. In our preferred specification, we set $p = 1$, fitting a linear trend. Figure 1, panel B, plots the deseasonalized outcomes $\log(\tilde{Z}_t) = \log(Z_{t,m}) - \hat{g}_m$.

Our coefficient of interest is γ_0 , which measures the VAT-rebate-driven jump in the outcome in August 2014, under the assumption that no other policy or economic change coincides with the reform to provoke a change in the outcome. Put differently, the outcomes are assumed to evolve smoothly around the reform time in the absence of the reform. Our preferred specification uses weekly outcome data and weeks as running variable. Weeks are defined such that the first day of a week coincides with the first post-reform day. In auxiliary analyses, we also estimate a firm-level version of equation (1), in which we include firm fixed effects, hence estimating the average effect of the reform across firms while weighting all firms equally.

Ideally, we would also like to examine the estimate for γ_1 , capturing whether the reform was associated with a change in the growth rate of the outcome. However, a causal identification of γ_1 would require us to make the very strong assumption that the outcome would have evolved according to the same growth trajectory before and after the reform, in the absence of the reform. This is unlikely to be true. Instead, we conduct a nonparametric comparison of the month-on-month growth-rate distributions before and after the reform, to evaluate the presence of suggestive evidence for a trend acceleration.

B. Results

Considering first the raw and deseasonalized data (Figure 1), it is clear that the number of card transactions jumps sharply in August 2014, precisely when the VAT

¹⁸Here, t can be a week or a month. For weeks that stretch across two months, we consider that each week falls into the month in which it has more days.

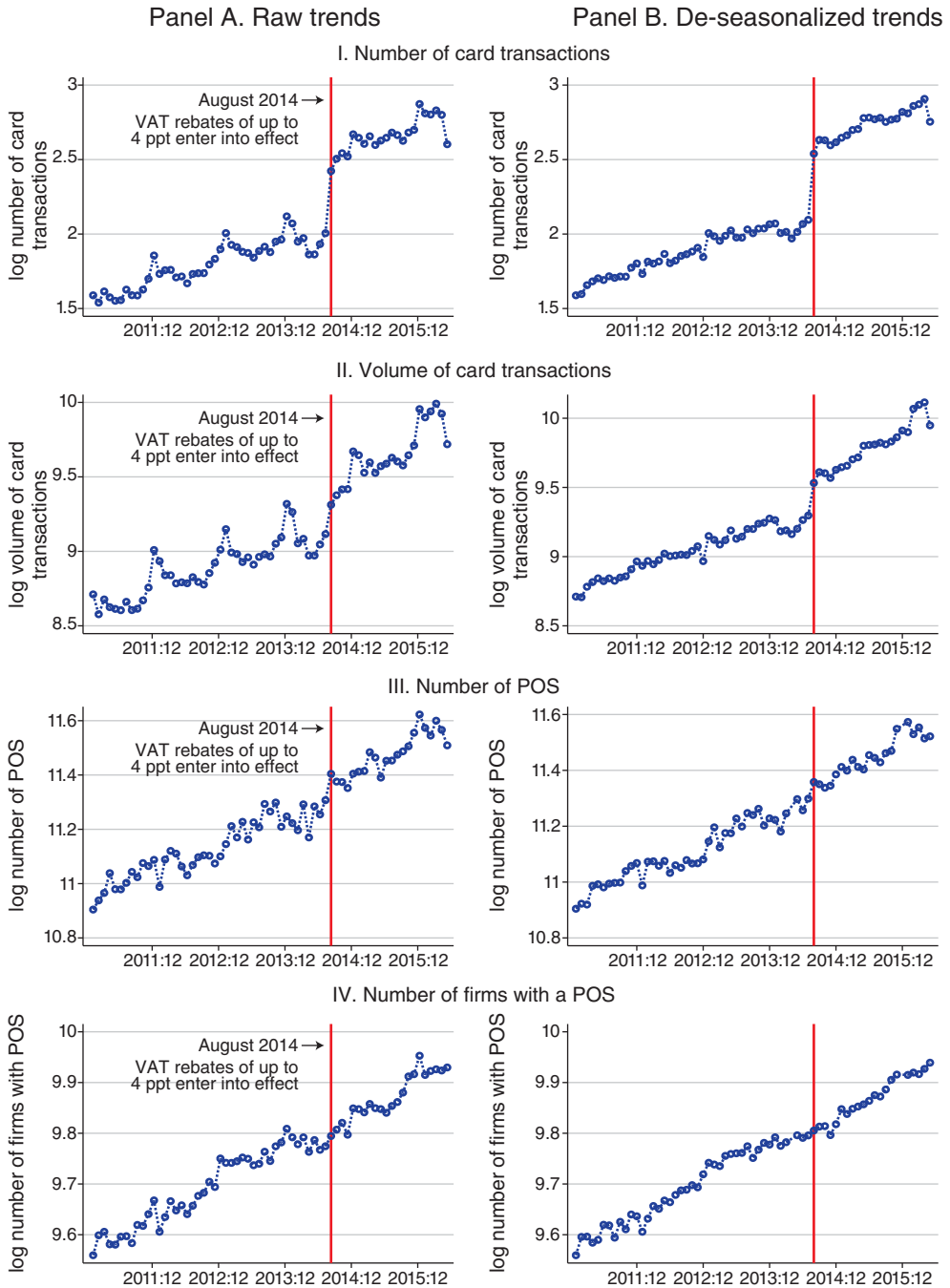


FIGURE 1. THE EFFECT OF VAT REBATES ON THE USE OF ELECTRONIC PAYMENT TECHNOLOGY RAW AND DESEASONALIZED DATA

Notes: Panel A plots the monthly aggregate values for each of the outcomes. For row I, the outcome is log of millions of transactions. For row II, it is log of millions of pesos. POS stands for point-of-sales terminal, i.e., credit/debit card machine. We average over the months of April and May 2014, for reasons discussed in Supplemental Appendix Figure B.1. Panel B plots the deseasonalized trends after taking out month-of-year fixed effects, as per equation (1) (linear specification). This figure is discussed in Section IIIA.

rebates first become available. This immediate and large response is not surprising, as the VAT rebates were large in size, were introduced with great media fanfare, and were very salient to consumers (Supplemental Appendix Figures A.2–A.6).¹⁹

The second outcome of interest, the volume of card transactions, also increases with the reform, but the increase here is less pronounced. The increase in the number of transactions is hence driven by smaller transactions. This is consistent with the fact that the VAT rebates were proportionally smaller for larger transaction amounts, and that a larger share of large transactions was likely already carried out through electronic payment methods before the introduction of VAT rebates. The number of POS in use and the number of firms with at least one POS also increases over time, but only the former series displays a slight jump around the time of the reform.

To precisely estimate the size of the discontinuity in outcomes in August 2014, we now turn to our regression discontinuity estimations, the results of which are displayed in Figure 2, panel A. The introduction of the VAT rebates is associated with a 50 percent increase in the number of card transactions, and an almost 30 percent increase in the volume of card transactions.²⁰

Despite the increase in consumer demand for card payments, the number of POS in use increased by only 10 percent in the month of the reform. It is possible that firms need time to adjust to the increase in consumer demand, in which case the response in the number of POS would be delayed compared to the consumer response. However, there is no sign of an acceleration in the growth trend in POS after the reform.

To examine the possibility of a growth acceleration, we compare the distribution of month-on-month growth rates prior to the reform to the post-reform distribution of growth rates. Figure 2, panel B, shows these distributions of growth rates for the pre- and post-reform period. The graphs and the associated statistical tests reported below each panel confirm that the introduction of VAT rebates is not associated with an acceleration in the month-on-month growth trend in any of the outcomes.

The histograms and associated randomization-inference-style p -values also reveal that the reform-month growth rates (July to August 2014) for the number and the volume of card transactions are extreme outliers compared to the pre- and post-reform growth rate distributions.²¹ This supports our interpretation of these effects as being driven by the introduction of the VAT rebates as opposed to being driven by other policy changes or random variations over time. For the number of POS, the reform-month growth rate also lies statistically significantly above the mean of the distribution. A different result emerges, however, when considering the number of firms with a

¹⁹We do not observe prices or the incidence of the VAT rebate, but the strong consumer response suggests that a substantial share of the rebate was passed through to consumers.

²⁰To appreciate the size of this effect, consider that the average share of card sales in total reported sales is 25 percent prior to the reform. Estimates from the firm-level version of equation (1) suggest that the firm-level volume of card sales increased on average by 15 percent (Supplemental Appendix Figure B.2). In general, the results in Supplemental Appendix Figure B.2 are qualitatively similar to our main results, though with smaller point estimates, suggesting that the aggregate impact of the VAT rebates is driven by larger firms. For comparison, India's demonetization campaign led to increases in electronic sales that are an order of magnitude larger than what we observe here, but this shock also generated a large and negative real effect, meaning this is not a commendable policy nor one whose causal effect on tax compliance can easily be identified.

²¹To construct the randomization inference p -values, we divide the number of times a month-on-month growth rate is higher than the reform-month rate by the number of months–1. We also show placebo RD estimates with randomization inference p -values in Supplemental Appendix Figure B.7.

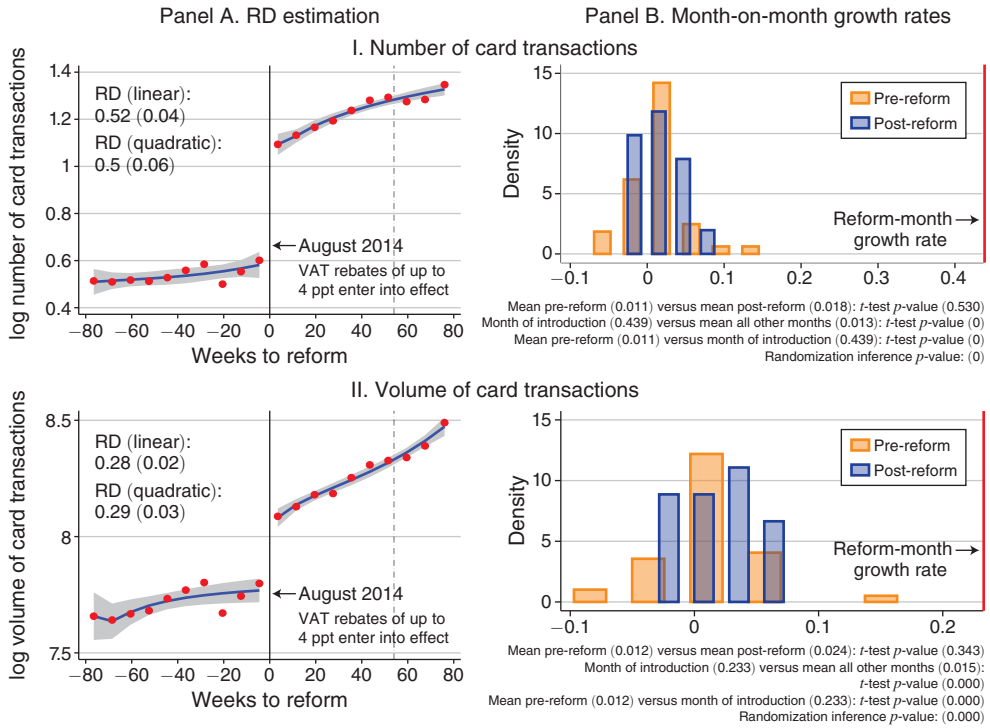


FIGURE 2. THE EFFECT OF VAT REBATES ON THE USE OF ELECTRONIC PAYMENT TECHNOLOGY REGRESSION DISCONTINUITY ESTIMATES AND MONTH-ON-MONTH GROWTH RATES

(continued)

POS, for which the reform-month growth rate is in fact close to the mean and mode of the distribution of growth rates, and the randomization-inference *p*-value is 0.373. There is thus no evidence for a reform-triggered increase in POS take-up on the extensive margin, above and beyond the gradual growth over time in the number of firms that employ POS. The reform did, however, trigger an increase in POS take-up on the intensive margin, among firms that were already using POS. This is not surprising, as the cost of adopting another POS is likely much smaller for firms already using POS.²²

Lastly, we note that none of the outcomes considered in Figure 2 exhibits a discontinuity in August 2015 (marked by a dashed line), when the VAT rebates were reduced.²³ Supplemental Appendix Figure B.4 formally shows that there is no statistically significant discontinuity in any of the outcomes in August 2015. This is consistent with two possible explanations. Either the introduction of the VAT rebates induced a permanent change in consumer behavior which persists even after the incentives are reduced, or consumers respond more strongly to extensive margin changes in rebates (introduction) than to intensive margin changes in rebate rates.

²²One might expect competition among retailers in the same sector and location, combined with the consumer demand for card payments, to incentivize firms without POS to adopt POS. However, even in subsectors with initially low POS penetration, we see little to no POS adoption response on the extensive margin (Supplemental Appendix Figure B.3).

²³The rebates on debit card transactions up to 4,000 UI fell from 4 to 3 percent, and the rebates for credit card transactions up to 4,000 UI fell from 2 to 1 percent.

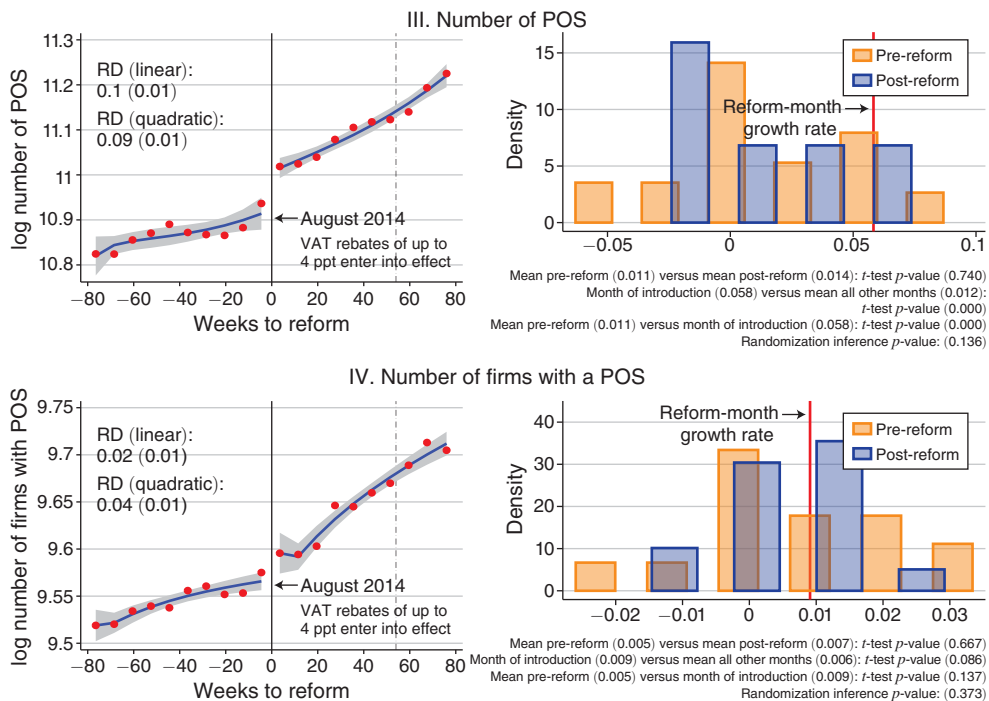


FIGURE 2. THE EFFECT OF VAT REBATES ON THE USE OF ELECTRONIC PAYMENT TECHNOLOGY REGRESSION DISCONTINUITY ESTIMATES AND MONTH-ON-MONTH GROWTH RATES (continued)

Notes: Panel A implements an RD estimation around the time of introduction of the VAT rebates. The red dots represent the mean outcome in equally spaced weekly bins. The solid blue lines (grey areas) depict a fitted second-order polynomial (the corresponding 95 percent confidence intervals). The solid black line marks August 2014, when VAT rebates were introduced. The dotted black line marks August 2015, when the rebate rates were reduced. The notes display the estimate γ_0 from equation (1) for an RD around August 2014. Standard errors are robust to heteroscedasticity. Panel B plots the distribution of monthly growth rates (log difference) between January 2011 and December 2015. The vertical red lines represent the growth rate corresponding to the month of introduction of VAT rebates (August 2014). This Figure is discussed in Section IIIB.

C. Robustness Tests

We now discuss a series of robustness tests, including those suggested in Hausman and Rapson (2018) for RD designs in time. Supplemental Appendix Figure B.5 illustrates the robustness of our main RD results from Figure 2 to varying the bandwidth and the degree of the polynomial. Supplemental Appendix Table B.1 shows that the results are similarly robust to varying the level of aggregation of outcomes, e.g., daily, weekly and biweekly. Supplemental Appendix Figure B.6 shows the results when adding to our main estimation in equation (1) a trend break in January 2013, when the POS subsidies for firms were phased in. These subsidies were technically available starting September 2012, but take-up began only in January 2013. Allowing for the trend break in January 2013 does not substantially alter our results.

Supplemental Appendix Figure B.7 shows the distribution of placebo RD estimates, assuming the reform happened in a month other than August 2014, and the associated randomization inference *p*-values. The results show that there is a significant increase in August 2014 only in the number and volume of card transactions, but not in the

number of POS or number of firms with a POS. In Supplemental Appendix Figure B.8, we conduct another placebo analysis, showing that there is no jump or trend break in August 2014 in the number and volume of card transactions in Argentina, Uruguay's large neighbor.

Supplemental Appendix Table B.2 shows that our results are robust to conducting a “donut RD” in which we remove observations around the reform time to account for potential selective sorting (i.e., retiming of purchases in our case). Another potential challenge with our estimation procedure is that shorter bandwidths, which allow us to achieve a better fit of the data around the reform, require us to estimate the month fixed effects on fewer observations. Supplemental Appendix Table B.3 shows that this is not a concern, as our results are almost identical when using an alternative two-step estimation procedure. This procedure is similar to the “augmented local linear” methodology suggested in Hausman and Rapson (2018). We first estimate equation (1) on the full 2010–2016 data to estimate the month-of-year fixed effects with the highest possible degree of precision. We then recover the deseasonalized outcomes $\log(\tilde{Z}_t) = \log(Z_{t,m}) - \hat{g}_m$ and estimate the regression discontinuity with a shorter dataset (bandwidth) around the reform. In this second step, we estimate equation (1) without the month-of-year fixed effects g_m and use the deseasonalized outcomes as dependent variable. The point estimates from this procedure are hardly distinguishable from our main estimates.

Supplemental Appendix Tables B.4 and B.5 show the results when controlling for potential autocorrelation in the outcome by including the first and second lag of the dependent variable in the estimation. With this correction, the effects are only slightly smaller than in our main estimations, suggesting that the number of card transactions increased by 40 percent and the volume of transactions increased by 20–30 percent. All point estimates continue to be highly statistically significant.²⁴ Finally, it is possible that our specification is affected by serially correlated unobservables and hence autocorrelation in the error term. We thus rerun the RD estimation using the Prais and Winsten (1954) correction for autocorrelated errors (see also Judge et al. 1985 and Davidson and MacKinnon 1993). The results shown in Supplemental Appendix Table B.6 are very similar to our main results, suggesting that autocorrelated errors are not an important concern.

IV. Tax Compliance

Having established that the VAT rebates lead to a large increase in the number and volume of card transactions, but did not generate an increase in POS adoption on the extensive margin, we now turn to analyze the impact on tax compliance. Applying an RD estimation, as used in the previous section, to aggregate monthly VAT payments of retail firms reveals no detectable discontinuity in August 2014

²⁴ As Hausman and Rapson (2018) discuss, the point estimate on the treatment indicator in an estimation that includes the lagged outcome variable captures only the short-run effect of the policy change, while our main estimates capture the medium-term effect, i.e., the short-term effect plus any additional impact that arises from a combination of the short-term effect and the autoregressive nature of the outcome. This latter effect is arguably the policy-relevant one in our context, which is why we use it for our main analysis, but it is reassuring that the short and medium-term effects are similar.

(Supplemental Appendix Figure B.10). This is not surprising, as aggregate tax revenues are disproportionately driven by a small number of large firms, which are likely already tax-compliant. We therefore study the tax compliance impact through a difference-in-difference estimation, comparing retail sector firms using a POS to wholesaler sector firms. The following sections describe our methodology, the results, and robustness tests.

A. *Motivation for Empirical Strategy*

Our difference-in-difference estimation is inspired by Naritomi (2019) who studies the tax-compliance effect of consumer incentives to request e-receipts in Brazil, relying on a comparison of treated retail sector firms with a control group of wholesale sector firms. Our use of a similar empirical strategy is motivated by the following four observations.

First, retail firms sell primarily to final consumers whereas wholesale firms sell predominantly to other firms, meaning that retailers disproportionately benefited from the VAT rebates which applied only to firm-to-consumer transactions. This is evident in Supplemental Appendix Figure A.7, which shows that retailers compared to wholesalers are (i) 26 percentage points more likely to grant VAT rebates to their customers (panel A), (ii) register a much higher number and volume of transactions that give rise to VAT rebates (panels B and C), and (iii) have a 21 percentage points higher share of sales volume associated with any rebate, conditional on having card transactions (panel D).

Second, we can further tighten the link between the policy variation and our empirical analysis by focusing our treatment group on the 52 percent of retail firms that already had a POS prior to 2014, as we have seen in Section III that firms do not respond to the reform by adopting POS technology on the extensive margin. Among wholesalers, only 16 percent used a POS prior to the reform, suggesting that the wholesale sector would be weakly treated by the introduction of VAT rebates, if at all.

The third motivation for our empirical strategy is based on the fact that the self-enforcement mechanism of the VAT typically breaks down at the sale to the final consumer, as the consumer has no incentive to ask for a receipt documenting the purchase, whereas firms have an incentive to claim input VAT on their purchases (Pomeranz 2015; Naritomi 2019). This means that firms in the retail sector should be *ex ante* less tax compliant than wholesale sector firms, and hence have more scope for improving compliance in response to the reform. Higher tax evasion rates for downstream sectors have been documented in multiple studies (e.g., in Best, Waseem, and Shah 2022 using random tax audits and in Waseem 2023 using quasi-experimental variation).

Finally, wholesalers are a suitable control group for retailers as they experience a similar time trend in the outcome pre-reform, which makes sense as wholesalers sell directly to retailers. The time trends in manufacturing sectors, for instance, are typically different from that in the retail sector, as manufacturers may produce for export, and there may be delays between the manufacturing of a good and its final sale due to further value addition along the supply chain.

However, we recognize that wholesalers are not a pure control group, as some wholesalers do sell to final consumers. In Section IVD, we show that our results are robust to either restricting the control group to wholesalers who do not sell to final consumers or using service sector firms as an alternative control group. We also consider the possibility that wholesalers may be indirectly treated if their retail customers become more tax compliant, and the compliance increase is transmitted upwards in the value chain. However, this would result in tax compliance increasing among both retailers and wholesalers after the reform. We show below that the data reject this possibility, as we observe no positive deviation from the pre-reform trend in either group.

B. Estimation

In our main analysis, we estimate the difference-in-difference specification

$$(2) \quad y_{it} = a_i + g_t + \beta \cdot Treated_i \cdot PostReform_t + \gamma' \mathbf{X}_{it} + u_{it},$$

where y_{it} is the outcome for firm i in time period t , a_i and g_t are firm and time period fixed effects, $Treated_i$ indicates retail sector firms using a POS, the control group consists of wholesale firms, \mathbf{X}_{it} is a vector of pre-reform firm characteristics interacted with year fixed effects and u_{it} is the error term. The policy impact is measured by the coefficient β on the $Treated_i \cdot PostReform_t$ interaction term. The identifying assumption is that the outcome for treated firms would have evolved in parallel to the outcome for control firms in the absence of the reform. To confirm this is the case, we estimate the following event-study version of equation (2):

$$(3) \quad y_{it} = a_i + g_t + \sum_{\substack{k=-4 \\ k \neq -1}}^2 \beta_k \cdot Treated_i \cdot \mathbf{1}_k\{k = t\} + \gamma' \mathbf{X}_{it} + \epsilon_{it},$$

and plot the β_k coefficients for each time period. In our baseline specification, \mathbf{X}_{it} contains a vector of firm age \times year fixed effects. This allows us to account for differences in firm growth over the life cycle. The results are robust to using simple year fixed effects or using year fixed effects interacted with other firm characteristics.

Our main outcome variables Y are total taxable sales, reported output VAT, and the net VAT liability ($= \max\{\text{output VAT} - \text{input VAT}, 0\}$).²⁵ These outcomes take the value zero for a small but non-negligible share of observations. We include extensive-margin responses of the outcome from zero to a nonzero value in our estimation by assigning a specific value ϵ to these changes, as suggested in Chen and Roth (2023). So $y_{it} = \log(Y_{it}/Y_{min})$ for $Y_{it} > 0$, where Y_{min} is the minimum value Y , and $y_{it} = -\epsilon$ for $Y_{it} = 0$. Our preferred specification considers that an extensive margin change from reporting a zero outcome to reporting the minimum positive value is the same as a 10 percent increase on the intensive margin, i.e., $\epsilon = 0.1$. We then vary ϵ to show that the specific value we assign to the extensive margin response does not matter for our results as there is no detectable extensive margin response.

²⁵Using these outcome variables to capture changes in tax compliance means that we make the implicit assumption that there are no differential changes in real value added in treated firms and that any divergence between treated and control firms can hence be attributed to changes in evasion.

We also show this explicitly by estimating the extensive and intensive margin response separately, in a two-part model, as suggested in Chen and Roth (2023) and Mullahy and Norton (2022). While the two-part model is clear and transparent, we chose the above-mentioned method as our main specification to facilitate the display of a large number of robustness tests.

We use annual data for our main analysis and later show the robustness of our results to using monthly data. This is because firms outside the large taxpayer unit report taxable sales—a key outcome variable—only annually, and they report output VAT and net liability monthly but retrospectively at the end of each year.²⁶ Using annual data also limits the occurrence of zeros and maximizes the size of the sample we can use for estimating intensive margin effects. In our preferred specifications, we winsorize the outcome variables at the ninety-ninth percentile within each treatment group \times year, and we confirm the robustness of the results to alternative top-coding approaches.

C. Results

Our main DiD results are shown in Figure 3. Each column pertains to a different outcome variable. In the top row, we show the normalized trends over time in the treatment and control group, and the DiD point estimate $\hat{\beta}$ on the $Treated_i \cdot PostReform_t$ interaction from equation (2). In the bottom row, we plot the period-specific β_k estimates from equation (3) to confirm that we cannot reject the parallel trends assumption. For the net liability outcome, we use the synthetic difference-in-difference estimation proposed in Arkhangelsky et al. (2021), which reweights observations in the control group to minimize the difference in trends between the treatment and control group.

If the expansion of electronic transactions triggered an improvement in tax compliance, it should first manifest through an increase in reported taxable sales. However, we observe parallel trends in this outcome and hardly any divergence between the treatment and the control group. We estimate that taxable sales in the treatment group actually decreased slightly after the reform, compared to the control group, but this difference is statistically indistinguishable from zero (Figure 3, column A). The fact that reported sales do not change differentially in the treatment group after the reform, and that the statutory VAT rates did not change, would imply that the output VAT remitted should also be unchanged. Indeed, we find that the DiD point estimate on reported output VAT is also close to zero and again statistically indistinguishable from zero (-0.045 , $SE = 0.040$, column B). The fact that the consumer response to the VAT rebates is immediate already suggests that any tax compliance response, if present, should also emerge relatively quickly. The event study graphs and estimates show that this is not the case. The empirical results also contradict the possibility of a gradually emerging effect, as the event-study estimates for 2015 are smaller than those for 2014 (bottom row). Consistent with the absence of an impact on reported sales and output VAT, the effect on the reported net

²⁶For the annual specification, $PostReform_t$ indicates the years 2014 and beyond, taking into account that the year 2014 is partially treated as the VAT rebates enter into effect in August.

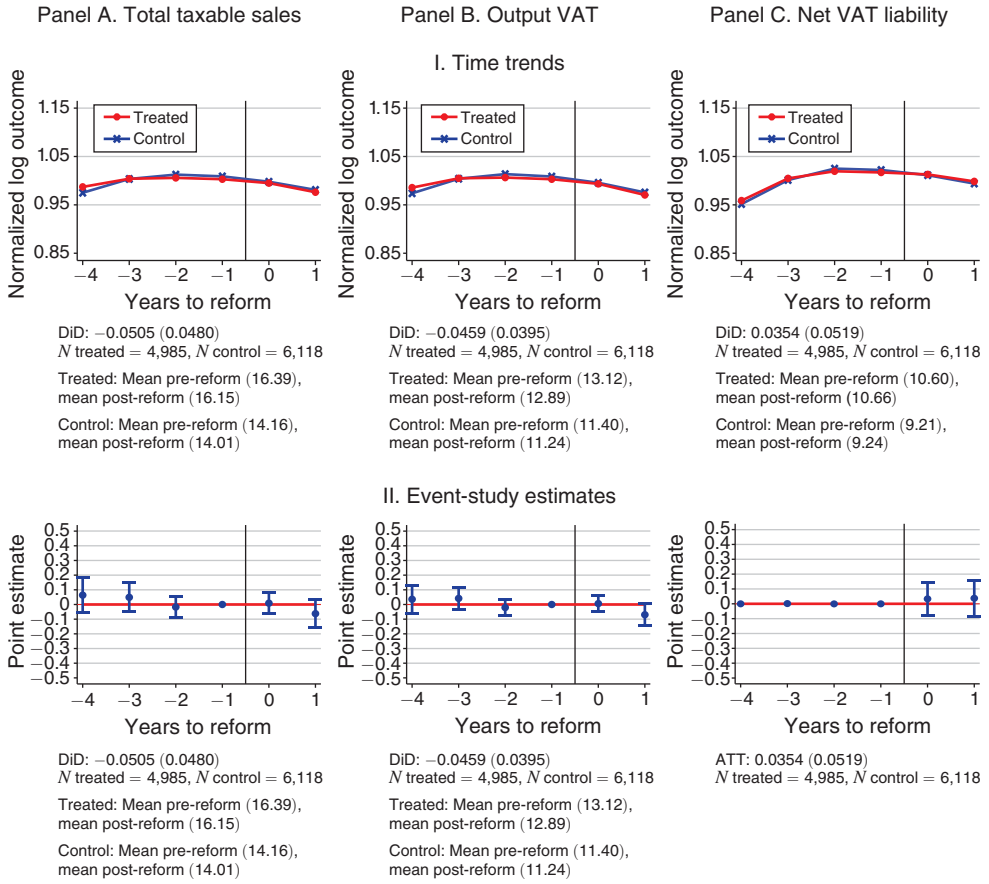


FIGURE 3. THE EFFECT OF VAT REBATES ON TAX COMPLIANCE RETAILERS w/ POS vs WHOLESALERS DIFFERENCE-IN-DIFFERENCE ESTIMATION

Notes: These graphs implement a DiD estimation comparing retail firms that had a POS at some point prior to 2014 (treated) to wholesale firms (control) around the introduction of the VAT rebates in 2014 (year 0). Panel I shows the normalized time trends and the DiD estimate β on the $Retailer_i \cdot PostReform_t$ interaction from equation (2). Panel II shows the event study estimates β_t from equation (3). Standard errors are robust to heteroskedasticity. In the last column (net liability outcome) we use the synthetic difference-in-difference estimator of Arkhangelsky et al. (2021) to minimize the difference in trends between the treatment and control group. We run the SDID with 500 iterations, as in Viviano and Bradic (2023). This Figure is discussed in Section IVC. Table 1 shows the robustness of the results to various alternative specifications.

tax liability is also close to zero (0.035, SE = 0.052) and statistically insignificant (column C). The reform thus had no impact on treated firms' reporting behavior or tax remittance. Our findings starkly contrast with the findings in Naritomi (2019), who shows that the rollout of e-receipts in Brazil increased reported sales of retail firms by at least 21 percent.

In Supplemental Appendix Table C.1, we show that the results from our preferred specifications displayed in Figure 3 are very insensitive to changes in ϵ , the value we attribute to extensive-margin changes in the outcome, even if we set a high value of $\epsilon = 3$, i.e., weighing an extensive margin change as much as a 300 percent change on the intensive margin. This is because there is no significant extensive margin response.

We document this explicitly in Supplemental Appendix Tables C.2 and C.3 which estimate the difference-in-difference model for the extensive margin and the intensive margin separately. There is no evidence for a statistically significant response on either margin. The extensive margin point estimates are mostly negative and insignificant, and the intensive margin effects are particularly precisely estimated zero effects. The negative point estimates on the extensive margin are driven more by changes in the wholesale group than by changes among the treated firms, as almost all treated firms register nonzero outcomes.

D. Robustness Tests

In Table 1, we demonstrate the robustness of our results to different specifications. In columns 1, 5, and 9, we reproduce the results from our preferred specification from Figure 3 for comparison purposes. In columns 2, 6, and 10 we show that the results are very similar when winzorising the outcome more conservatively, at the ninetieth percentile. The point estimates are now even closer to zero than in our main specification and still statistically indistinguishable from zero. The results are again very similar when using an unbalanced panel (columns 3 and 7).²⁷ Finally, we still obtain the same results when extending the panel to include observations for the year 2016 (columns 4, 8, and 11). The 2016 data we have access to is only partial, covering CEDE firms and about 3,500 non-CEDE firms. The results are hence tentative, but they do not provide any indication that a treatment effect emerges over the medium-term horizon. Graphical representations of these results and confirmations of the parallel trends assumption in each estimation are shown in Supplemental Appendix Figure C.1.

In addition, we show in Supplemental Appendix Tables C.4 and C.5 that we obtain very similar results when adding additional controls (e.g., region \times year/month fixed effects), using a more or less strictly balanced panel, and when using monthly instead of annual data. Controlling more flexibly for differential time trends across regions does little to reduce the variance of the estimates, as treated and control firms are similarly distributed across regions. Accounting for differential time trends by initial firm size leads to more negative though still statistically insignificant point estimates. The point estimates become smaller in absolute value when moving to a quarterly balanced panel. This is consistent with the fact that this way of balancing eliminates firms with highly seasonal activities, making the treatment and control group more similar in terms of their time trend. The patterns in the monthly data are similar, with estimates closer to zero throughout. Besides, Supplemental Appendix Table C.6 shows the robustness of our results to varying the length of the panel, when using an annually balanced panel.

As discussed above, a concern with our empirical strategy might be that wholesalers are partially treated, as some of them sell part of their output to the final consumer (Supplemental Appendix Figure A.7). We hence rerun our analysis with a restricted control group of wholesale firms that did not use a POS machine. This test yields

²⁷ We do not conduct this analysis for the net liability outcome, as we rely on a synthetic difference-in-difference estimation for this outcome, which requires a balanced panel.

TABLE 1—THE EFFECT OF VAT REBATES ON TAX COMPLIANCE DIFFERENCE-IN-DIFFERENCE ESTIMATES

	(1)	(2)	(3)	(4)
<i>Panel A. Taxable sales</i>				
Post · Treated	−0.051 (0.048)	−0.042 (0.048)	−0.069 (0.046)	−0.031 (0.045)
<i>N</i> treated (retailers w/ POS)	4985	4985	6906	6819
<i>N</i> control (wholesalers)	6118	6118	9044	9340
<i>Panel B. Output VAT</i>				
Post · Treated	−0.046 (0.040)	−0.039 (0.039)	−0.059 (0.038)	−0.034 (0.037)
<i>N</i> treated (retailers w/ POS)	4985	4985	6906	6819
<i>N</i> control (wholesalers)	6118	6118	9044	9340
<i>Panel C. Net liability</i>				
Post · Treated	0.035 (0.052)	0.039 (0.052)		−0.017 (0.070)
<i>N</i> treated (retailers w/ POS)	4985	4985		2321
<i>N</i> control (wholesalers)	6118	6118		3721
Balanced sample	Y	Y	−	−
Unbalanced sample	−	−	Y	Y*
Winsor at p99	Y	−	Y	Y
Winsor at p95	−	Y	−	−
Includes 2016 data	−	−	−	Y

Notes: This table documents the robustness of our main DiD specification discussed in Section IVB. The table displays the DiD estimate β from equation (2) for the different outcomes, as per the panel titles. Column 1 reproduces our preferred specification (as shown in Figure 3), using a balanced panel during 2010–2015. Column 2 shows the robustness of our results to more conservative top coding (winsorizing at p95). Column 3 show that the results are very similar when considering an unbalanced sample of taxpayers during 2010–2015. We do not conduct this analysis for the net liability outcome, as the synthetic difference-in-difference estimation requires a balanced panel. Lastly, column 4 shows the robustness of our results to an extended sample which includes observations for the year 2016. When the outcome is the net liability, we use a synthetic difference-in-difference estimation to achieve parallel trends. The SDID requires a balanced panel, so in column 4, we use an unbalanced panel for the first two outcomes and a balanced panel for the last outcome. This table is discussed in Section IVC. Supplemental Appendix Figure C.1 shows the graphical representation of these results.

essentially the same results are our preferred specification: all point estimates are small and statistically indistinguishable from zero (Supplemental Appendix Figure C.2 and Supplemental Appendix Table C.7). We also do not find any positive treatment effects when we use firms in the service sector, which predominantly supply other firms, as an alternative control group (Supplemental Appendix Figure C.3).

Although we find no significant effects of the VAT rebates on treated firms' VAT compliance overall, a question that remains is whether the rebates might have increased compliance among certain subsamples of the treatment group. These effects might not be detectable in the general DiD if both the treatment effects and the relevant subsample are small. As consumers strongly responded to the VAT rebates by using payment cards more, we would expect any effect to be concentrated among groups of firms where the consumer response was strongest. We thus leverage variation in the size of the “first stage”—the impact of VAT rebates on card transactions—across four-digit subsectors and across regions in Supplemental

Appendix Section C.3. Supplemental Appendix Figure C.4 shows that the size of the first stage effect substantially varies across regions and sectors. Subsectors are mostly distinguished by the products sold, e.g., book versus clothes versus food, so that any heterogeneity in treatment effects across subsectors is unlikely to be confounded by consumer switching between retailers.

We test whether treated firms in subsectors/regions with a larger (or more statistically significant) first-stage effect exhibit faster growth in reported outcomes post-reform, compared to other treated firms. Supplemental Appendix Figures C.5 and C.6 show that there is no evidence for this. Supplemental Appendix Table C.8 shows results from an alternative way of conducting this analysis, interacting the treatment in our main difference-in-difference estimations with an indicator for treatment intensity based on the size of the first stage effect. The point estimates on the interaction term are all either statistically insignificant or negative, hence corroborating our main finding of no tax compliance impact of the reform.

V. Interpreting the Results

Overall, we find that the introduction of VAT rebates led to a large increase in the number and volume of card transactions, but had no effect on tax compliance among treated retail firms. We now discuss the two main factors that explain the lack of a tax compliance response.

A. *Endogenous POS Adoption by Firms*

First, firms self-select into POS adoption based on a cost-benefit trade-off, and the VAT rebates did not significantly increase POS adoption on the extensive margin. As the analysis in Section IIIB showed, despite a large increase in consumer demand for electronic transactions, only firms that already accepted card payments prior to the reform increased the number of POS in use. The lack of an extensive-margin POS adoption response is consistent with several additional pieces of evidence. First, we observe a slow and gradual uptake of firm-level POS subsidies, which became available in September 2012 (Figure 4, panel A). In fact, only 6.5 percent of eligible retail firms had taken up the subsidy within two years of its introduction (2.2 percent of all eligible firms). More importantly, since the subsidy was not restricted to firms that had never used a POS, 87.7 percent of firms that took the subsidy already had used a card machine before, and for 83.9 percent of these, POS adoption preceded subsidy take-up by at least three months. It thus seems that the subsidy program had little impact on the use of the technology.

This is consistent with our second piece of additional evidence, suggesting that using a POS is costly for firms that are not yet very tax compliant. Indeed, an event study of firm behavior around the time of POS adoption (Figure 4, panel B) shows that a firm's reported output VAT and net liability increases with POS adoption. As a final piece of evidence, we examine firm responses to the January 2012 reduction in tax withholding rates and commissions applied by card companies. Supplemental Appendix Figures D.1–D.3 show no evidence that these changes substantially increased the use of POS on the extensive or intensive margin. These

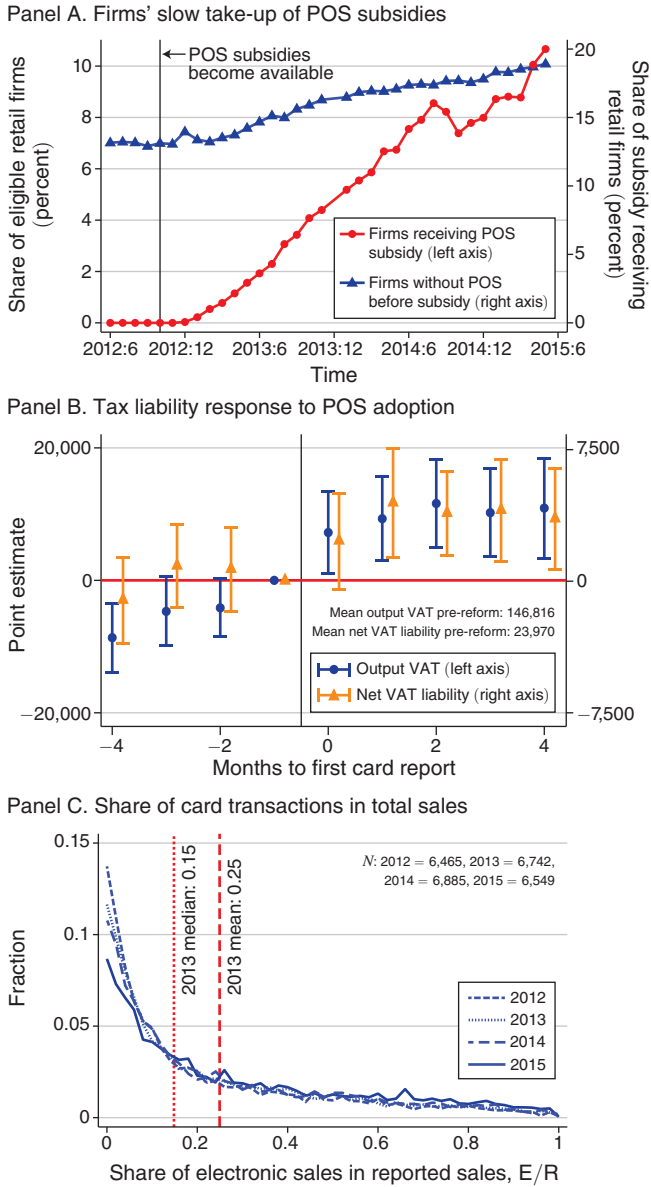


FIGURE 4. EXPLAINING THE ABSENCE OF A TAX COMPLIANCE EFFECT

Notes: Panel A plots the share of eligible retail firms receiving a subsidy for renting a POS (red dotted line), and the share of subsidy-receiving firms that did not have a POS before receiving the subsidy (blue line with triangle markers). Panel B displays event study estimates of firm behavior around the month of POS adoption. We estimate $Y_{it} = \mu_i + g_t + \sum_{k=a}^b \delta_k \cdot D_{it}^k + u_{it}$, where Y_{it} is the outcome for firm i in month t , μ_i , and g_t are firm and month fixed effects respectively and D_{it}^k are event time indicators. The sample is composed of retail and wholesale firms that used a POS for the first time between January 2008 and December 2015 and are observed for four months before and after the event. Standard errors are clustered at the firm level and the outcome variable is winsorized at the ninety-ninth percentile. Panel C plots the distribution of electronic sales as a share of a firm's total self-reported sales, for retail and wholesale firms that use a card machine in 2012–2015. We exclude a firm-year observation if the firm uses the card machine for less than 11 months in a particular year. This means that we exclude firms in the year in which they adopt a card machine, unless they adopt it in January or February. This Figure is discussed in Section V.

findings suggest that it may be difficult to increase POS take-up among firms via financial incentives. Much larger incentives or a mandate might be needed.²⁸ In Supplemental Appendix E, we provide more descriptive evidence on the firm characteristics correlating with POS adoption and on the association of POS adoption and tax compliance outcomes.

B. *Low Share of Card Sales in Reported Sales*

As a second reason for the lack of a tax compliance impact, we highlight that firms that already used a POS prior to the introduction of VAT rebates registered a relatively low share of electronic transactions in total reported sales. As Figure 4, panel C, shows, the mean (median) share of card sales in total reported sales was 25 percent (15 percent) in 2013. This suggests that firms already report a large share of their non-card sales to the government, meaning that there is room for an increase in card sales with no change in reported sales. Consistent with the gradual expansion of electronic payments, the distribution shifts rightward over the years, and especially between 2014 and 2015 with the implementation of consumer VAT rebates. However, the share of card sales in total sales is still very small for many firms, and below 20 percent for the majority of firms. Hence, given the low starting point, even the large increase in the number and volume of card sales in 2014 did not push the share of card sales towards close to 100 percent. Even if firms were intent on reporting sales at least as high as third-party reported sales, as reporting sales lower than third-party reported sales might trigger a higher audit probability (see Section I), the level of third-party reported sales (and marginal increases thereof) does not create a constraint for firms, as they already report sales much higher than the third-party reported sales.²⁹

In light of the low share of card sales in firms' total reported sales, it is also unlikely that the VAT rebates would have an impact on tax compliance if the share of households with access to credit/debit cards at baseline was higher. The consumer response is very large anyway—a 50 percent increase in the number of card transactions and 30 percent increase in the volume of transactions. Even if the response had been twice as large, the increase in card sales would still not push the share of card sales in total sales to a point where card sales create a binding constraint for firms' reporting behavior.

However, a policy which gives more consumers access to a credit/debit card for the first time could *qualitatively* change the consumer response and hence potentially the firm response, thereby impacting tax compliance. If consumers with newly acquired credit/debit cards are sufficiently concentrated as customers of firms that have yet to adopt a POS, and if they have sufficiently strong bargaining power, their demand for EPTs may push firms to adopt POS terminals on the extensive margin. This can in turn increase tax compliance among these firms and possibly among

²⁸The cost of the POS subsidy is currently less 1 percent of the cost of VAT rebates. So even a large increase in the POS subsidy would not be very costly for the government, especially if targeted at new POS users.

²⁹On the other hand, we could consider that the amount of third-party reported sales constitutes a binding constraint for firms if firms were matching their self-reports to the third-party reports.

their competitors. Higgins (2022) studies such a policy: the rollout of debit cards to social benefit recipients in Mexico. He finds that this led small retailers to adopt POS or POS-like technologies. He also finds that retailers' tax payments increase, though he cannot disentangle improvements in compliance and increases in real profits. Indeed, small retailers' profits increase as a result of the policy as richer consumers shift part of their consumption to small retailers.

VI. Policy Implications

We now discuss additional policy considerations related to our study. First, we present back-of-the envelope calculations of minimum required effects that would have allowed the policy to achieve its goals. Second, we examine the distributional impact of the VAT rebates. Finally, we discuss the external validity of our findings.

A. Minimum Required Effects

As the VAT rebates were successful at increasing the volume of card transactions but unsuccessful at raising tax compliance, it is relevant to inquire how much bigger the effect on card transactions would have needed to be to trigger a compliance response. Following Section I, we assume that firms' report sales at least as large as third-party reported sales (card sales). As card sales constitute on average 25 percent of total reported sales, the volume of card sales would have had to increase at least fourfold on average to push firms to increase their reported sales. This is of course a very simplified calculation, ignoring heterogeneity across firms. If the reform had a larger impact on the increase in card transactions among firms that already had a higher share of card sales in reported sales prior to the reform, or if firms' strategy was to report sales discretely higher than third-party reported sales, so as to avoid raising suspicions, the impact required to generate a change in reported sales would be smaller. Besides, whether or not an increase in reported sales (and hence output VAT) translates into an increase in the reported net VAT liability depends on the extent to which there is an offsetting adjustment on the cost side.³⁰

If a positive compliance impact is detected, the question arises as to what effect size would render the reform revenue neutral. For this, we consider the following elements. The cost of the VAT rebates is about two percent of domestic VAT revenue in 2014 and 2015. Retailers remit 4.2 percent of aggregate domestic VAT revenue. This means that retailers' reported VAT liability would have had to increase by close to 50 percent to make the rebate policy revenue neutral in the short term. Alternatively, if the rebates had been in place for only a year, and consumer use of their cards had remained stable after the removal of the rebates, an increase in retailers' reported tax liability of about 17 percent, sustained over three years would have made the reform revenue neutral within that time frame (and revenue-positive thereafter, if the compliance improvements were persistent). In these calculations, we ignore the cost of the POS subsidies, which is less than one percent of the cost of

³⁰In both Carrillo, Pomeranz, and Singhal (2017) and Slemrod et al. (2017), the taxpayer's reported net tax liability increased, but by less than would be expected in the absence of cost offsetting.

the VAT rebates. We also assume that the compliance effect is limited to retail sector firms only and does not transmit upwards in the supply chain.

B. *Distributional Impact*

In addition to its lack of success in improving tax compliance, a concern with the policy we study is its distributional impact. The distributional impact depends on how consumer prices adjust in equilibrium, which is governed by the relative demand and supply elasticities, and on which consumers benefit from potentially lower after-tax prices. We do not observe prices, but the large consumer response suggests that the pass-through of VAT rebates is likely high. This is consistent also with evidence on the pass-through of VAT rate changes in other contexts (Benedek et al. 2020; Gaarder 2019). We thus focus on analyzing which consumers benefit from lower prices via VAT rebates.

Consider that a business-to-consumer transaction needs to meet three conditions to be eligible for a rebate: (i) the buyer needs to have a debit or credit card, (ii) the retailer needs to have a POS, and (iii) the retailer needs to be VAT liable (i.e., not informal or in a simplified tax regime). Poorer individuals' purchase transactions are less likely to meet these criteria, and hence less likely to be eligible for a rebate.

Supplemental Appendix Figure D.5, panel A, shows that the likelihood of having a debit or credit card is strongly positively correlated with household income. In addition, conditional on having a card, richer households are more likely to have a debit card (as opposed to only a credit card) which generates a higher rebate rate.

A different way of proxying the regressivity of the policy is to consider households' share of expenditure at formal retailers, assuming that formal retailers are VAT liable and can offer card payments while informal retailers typically do not pay VAT nor offer card payments. Supplemental Appendix Figure D.5, panel B, shows that richer households spend a larger share of their budget at formal retailers. This analysis is based on the methodology in Bachas, Gadenne, and Jensen (2023) who categorize purchases in markets, non brick and mortar stores, corner stores and convenience shops as informal, and purchases at specialized stores (e.g., clothing stores) and large stores (e.g., supermarket chains) as formal.³¹

The VAT rebates are thus likely regressive.³² To make a more precise statement about the degree of regressivity of the policy, we would need to know the share of total household expenditure that is paid for by debit card, credit card and other payment methods. Supplemental Appendix Figure D.5, panel C, shows the best available proxy: the share of household expenditure paid for by debit card. Using this

³¹ While panel B focuses on a simple formal vs informal distinction, not accounting directly for whether or not a firm has a POS, it is well-known that there is a positive correlation between firm size and formality, and between firm size and having a POS even among formal-sector firms (Supplemental Appendix Table A.3). As poorer consumers are more likely to shop at informal vs formal stores, it seems reasonable to assume that, when they do shop in the formal sector, they spend a larger share of their formal sector expenditure at small as opposed to large retailers, hence missing out on some opportunities to obtain VAT rebates.

³² While poorer households are more likely to make smaller purchases which would be granted larger rebate rates (if the household has a debit card and purchases from a formal retailer offering card payment), this is unlikely to make the policy progressive, as only a very small fraction of transactions are above the threshold at which the rebate rate drops (Supplemental Appendix Figure A.3).

proxy indicates an upper bound on the regressivity of the policy, as the measure does not account for the fact that poorer households may have a credit card even if they do not have a debit card.

However, given how strongly the debit card payment share is correlated with income—the top decile’s share is over 20 times the bottom decile’s share—even a more complete measure of card expenditure share is likely to indicate that the policy is regressive.

C. External Validity

Our findings are derived in a particular country and reform context. Uruguay is a small open economy and may therefore have shorter supply chains than other countries. Assuming that VAT is collected at the import stage and that VAT compliance trickles down the supply chain, this may imply that VAT compliance is higher in Uruguay than in other countries with a similar level of development but with longer domestic supply chains.

In addition, our findings are specific to retail sector firms and do not necessarily extend to hotels, restaurants and tourism businesses. These sectors are not included in our analysis, as they were given VAT rebates for card payments prior to the period covered by the available microdata (see footnote 12). As these sectors are generally thought of as being evasion prone, an increase in electronic transactions in these sectors could possibly have generated a positive tax compliance effect.

Lastly, the nature of Uruguay’s reform, combining multiple policy tools to make a big push on financial inclusion was certainly unique and may be difficult to replicate in other contexts.

On the other hand, the fact that consumers are relatively responsive to financial incentives for using EPTs is likely generalizable, as it is consistent with previous findings in the finance literature (Arango et al. 2015; Agarwal et al. 2007; Bolt et al. 2010). In addition, our point that an increase in card transactions does not impact tax compliance if the share of card sales in total reported sales is low should hold generally. Besides, there is no reason to think that Uruguay is an outlier in terms of the share of card sales. Indeed, we show in Supplemental Appendix Figure D.4 that the distribution of the variable is very similar in Costa Rica.

VII. Conclusion

We have studied whether the digitization of transactions through electronic payment technology can help improve tax compliance. Leveraging variation generated by Uruguay’s financial inclusion reform, notably the introduction of large VAT rebates for credit and debit card payments, we find no evidence that digitization spurs tax compliance. We show that consumers are highly responsive to VAT rebates, increasing the use of payment cards, but firms are largely unresponsive, increasing POS usage only on the intensive margin. The consumer-driven increase in card transactions is not sufficient to generate an increase in tax compliance, as it only affects firms that already have a card machine and are relatively tax compliant, reporting a large share of nonelectronic sales for tax purposes. Overall, the VAT

rebates generated a fiscal cost of about 6 percent of the VAT liability of firms granting VAT rebates, which is equivalent to 1.5 percent of total VAT revenue (Supplemental Appendix Figure D.6).

As consumers are highly responsive to financial incentives, it is likely that even smaller and more targeted and/or temporary incentives, e.g., only for small card payments, could generate a sizeable increase in card transactions. More research into the elasticity of consumer card usage to differently-size rebate rates, and into the response to rebate rate reductions vs rebate removals would be useful to design rebates with a view on minimizing fiscal costs. Ultimately, however, an impact on tax compliance is more likely to be achieved with policies that successfully incentivize more firms to adopt a POS, which may require much larger financial incentives than those used in Uruguay or a mandate. Evaluating the effect of mandates from a welfare perspective requires estimating compliance with the mandate, and impacts on formality, firms' real outcomes, and tax compliance behavior. More generally, studying incentives for firms to adopt POS and the network and equilibrium effects of POS adoption in competitive markets are important avenues for future research.

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