Electronic Payment Technology and Tax Compliance: Evidence from Uruguay's Financial Inclusion Reform*

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Abstract

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 card transactions trigger an immediate 50% 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 classification: H26, H32, G18, O16.

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1 Introduction

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), 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 under-reporting taxable transactions (Kleven 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 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 (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, 2019). We evaluate the VAT rebate program using transaction-level data on all electronic transactions and monthly firm-level VAT declarations for 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 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 et al. 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 lead 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

[1] Carrillo et al. (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.

currency lead 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; Bérgolo 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 not intended to cover 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 2 and 3 lay out some conceptual considerations and present the policy background and the data we use. Sections 4 and 5 examine the impact of VAT rebates on the use of electronic payment technology and on tax compliance. Section 6 discusses the interpretation of the results and Section 7 discusses policy implications. Section 8 concludes.

2 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 et al. (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 $R_o < E_0 + \Delta E$, 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]

3 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.

3.1 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

[2]Our 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.

VAT, input VAT and net VAT for each month in their annual VAT declaration.[3] In 2015, there were 4099 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] Bérgolo 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, 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 (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"

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

[4] 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.

[5] https://www.gov.uk/government/statistics/measuring-tax-gaps-tables, accessed on November 9, 2022, and Barbone et al. (2013), p.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 (IMF 2017).

[6] Government 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, Bérgolo et al. (2020) find that 15.5 percent of income tax filers under-reported their wage.

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

3.2 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, transaction amounts, and over time, as shown in Figure A.2.

Debit card transactions of up to 4,000 *Unidades Indexadas* (UI, a Uruguayan accounting unit) – approximately 500 USD – 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 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

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

[8] For 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.

[9] Decree 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.

[10] 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 conversation rate is updated on a daily basis, making it difficult to bunching without updating prices frequently.

it highly salient, as show in Figure A.5. The rebates were also introduced with great media fanfare (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 debit card companies are then reimbursed for these credits by the government. These reimbursements happens monthly, so that firms should not experience a significant change in liquidity due to the granting of VAT rebates.

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]

3.3 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

[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 discriminaton, 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.

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 6 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 commission 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.

In exchange, the government reduced the tax withholding rates applied by card companies on card transactions, introduced legislative changes to facilitate the inter-operability 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 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 4,000,000[13] (approximately USD 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 10 USD 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

[13]Four 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).

[14]Decrees 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.

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 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]

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.

3.4 Data

To study the effect of electronic payments on tax compliance, we merge multiple data sets. 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 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(output VAT - 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

[15] Having 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 indicated having used a debit or credit card in the previous year.

[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 Bérgolo et al. (2021) and Foremny et al. (2018).

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.

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. 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-difference our 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.

4 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.

4.1 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 de-seasonalize

the data while estimating the regression discontinuity. Concretely, we estimate

$$log(Z_{t,m}) = g_m + \sum_{k=0}^{p} \left[\beta_k \cdot t^k + \gamma_k \cdot PostJuly2014_t \cdot t^k\right] + u_t, \quad (1)$$

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 de-seasonalized 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 non-parametric 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.

4.2 Results

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

[18] Here, 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.

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 trans-

[19]We 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.

[20]To 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 (Figure B.2). In general, the results in 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 lead 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.

actions 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 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] 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.

4.3 Robustness Tests

We now discuss a series of robustness tests, including those suggested in Hausman and Rapson (2018) for RD designs in time. Figure B.5 illustrates the robustness of our main RD results from Figure 2 to varying the bandwidth and the degree of the polynomial. Table B.1 shows that the results are similarly robust to varying the level of aggregation of outcomes, e.g. daily, weekly and biweekly. 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

[21] To 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 Figure B.7.

[22] One 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 (Figure B.3).

[23] The 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.

January 2013. Allowing for the trend break in January 2013 does not substantially alter our results.

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 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.

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. 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 de-seasonalized outcomes $log(\tilde{Z}_t) = log(Z_{t,m}) - \hat{g}_m$ and estimate the regression discontinuity with a shorter data set (bandwidth) around the reform. In this second step, we estimate equation 1 without the month-of-year fixed effects g_m and use the de-seasonalized outcomes as dependent variable. The point estimates from this procedure are hardly distinguishable from our main estimates.

Tables B.4 and B.5 show the results when controlling for potential autorcorrelation 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

[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.

Winsten (1954) correction for autocorrelated errors (see also Judge et al. (1985) and Davidson and MacKinnon (1993)). The results shown in Table B.6 are very similar to our main results, suggesting that autocorrelated errors are not an important concern.

5 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 (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.

5.1 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 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 4 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 et al. (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 5.4, 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.

5.2 Estimation

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

$$y_{it} = a_i + g_t + \beta \cdot Treated_i \cdot PostReform_t + \gamma \cdot X_{it} + u_{it}, \tag{2}$$

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, 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

$$y_{it} = a_i + g_t + \sum_{k \neq -1, k=-4}^{2} \beta_k \cdot Treated_i \cdot 1_k(k = t) + \gamma \cdot X_{it} + \epsilon_{it}, \tag{3}$$

and plot the β_k coefficients for each time period. In our baseline specification, 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(output VAT - 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 non-zero 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. ϵ=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.

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 99th percentile within each treatment group $\times$ year, and we confirm the robustness of the results to alternative top-

[25] Using 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.

[26] For 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.

coding approaches.

5.3 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 re-weights 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 tax liability is also close to zero (-0.020, SE=0.064) 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 roll-out of e-receipts in Brazil increased reported sales of retail firms by at least 21 percent.

In 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 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 non-zero outcomes.

5.4 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 95th 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 Figure C.1.

In addition, we show in Tables C.4 and C.5 that we obtain very similar results when adding additional controls (e.g. region × 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

[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.

throughout. Besides, 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 (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 essentially the same results are our preferred specification: all point estimates are small and statistically indistinguishable from zero (Figure C.2 and 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 (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 4-digit subsectors and across regions in Appendix Section C.3. 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 vs clothes vs 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. Figures C.5 and C.6 show that there is no evidence for this. 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.

6 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.

6.1 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 4.2 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. Figures D.1-D.3 show no evidence that these changes substantially increased the use of POS on the extensive or intensive margin. These 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 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.

6.2 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)

[28]The 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.

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 2), 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 their competitors. Higgins (2022) studies such a policy: the roll-out 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.

[29] On 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.

7 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.

7.1 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 2, 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

[30] In both Carrillo et al. (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.

POS subsidies, which is less than one percent of the cost of 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.

7.2 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.

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. 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 et al. (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]

[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 (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.

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. Figure D.5, Panel C, shows the best available proxy: the share of household expenditure paid for by debit card. Using this 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.

7.3 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 Figure D.4 that

[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 (Figure A.3).

the distribution of the variable is very similar in Costa Rica.

8 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 non-electronic 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 (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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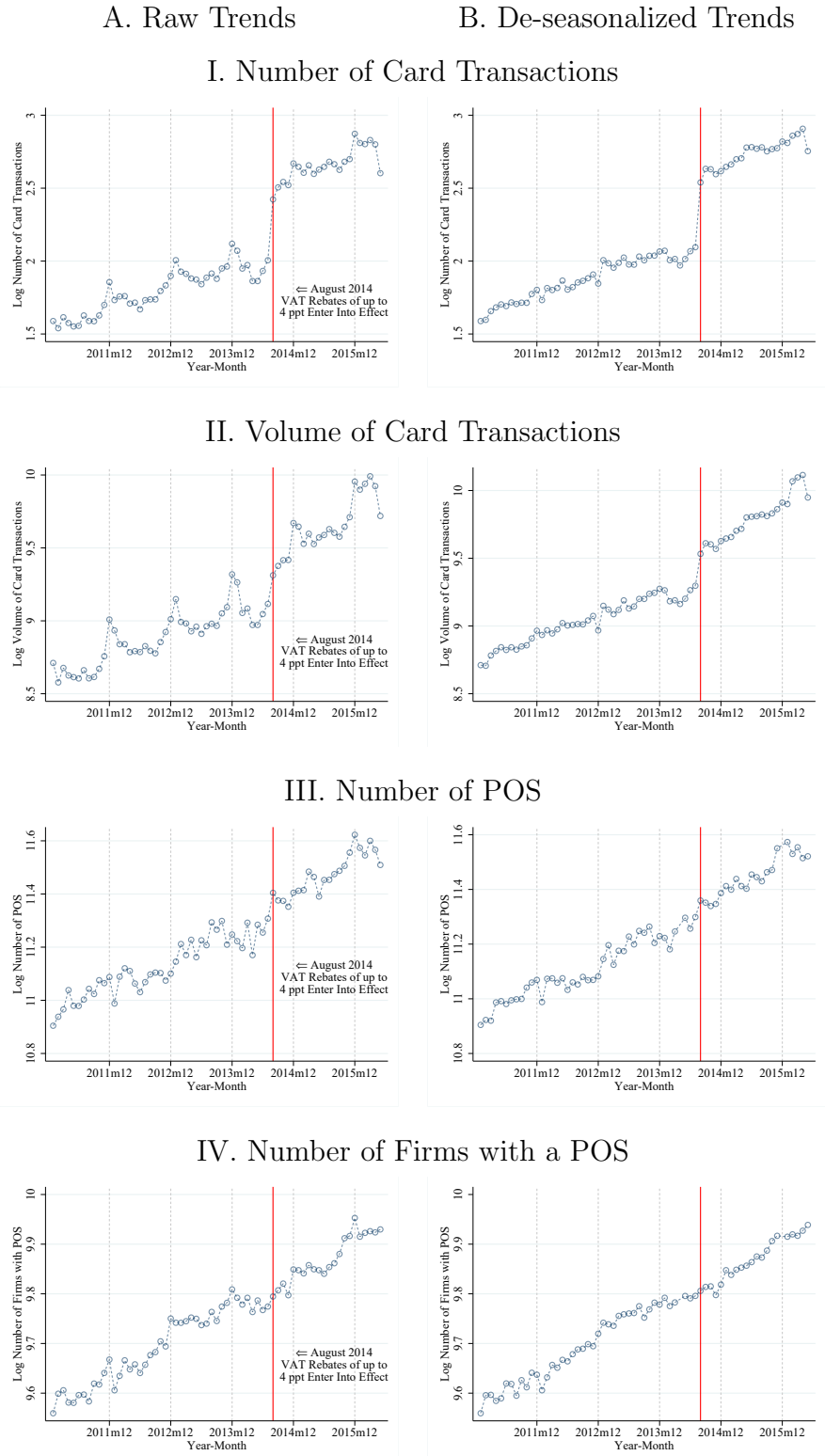
Figures and Tables

Figure 1: The Effect of VAT Rebates on the Use of Electronic Payment Technology
Raw and De-Seasonalized Data

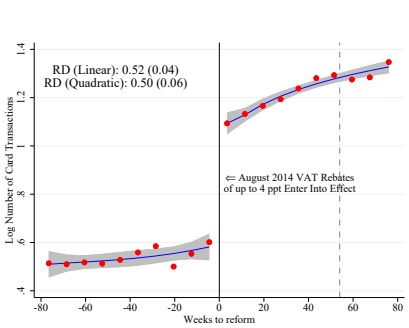


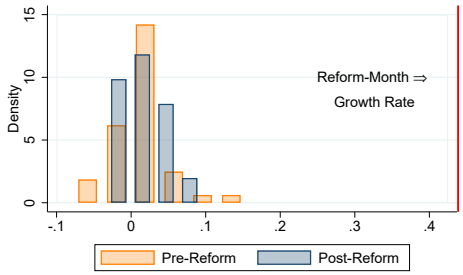
Notes: Panel A plots the monthly aggregate values for each of the outcomes. For row I, the outcome is log of millions of transactions. For row II, it is log of millions of pesos. POS stands for point-of-sales terminal, i.e. credit/debit card machine. We average over the months of April and May 2014, for reasons discussed in Figure B.1. Panel B plots the de-seasonalized trends after taking out month-of-year fixed effects, as per equation 1 (linear specification). This Figure is discussed in Section 4.1.

Figure 2: The Effect of VAT Rebates on the Use of Electronic Payment Technology
Regression Discontinuity Estimates and Month-on-Month Growth Rates

A. RD Estimation B. Month-on-Month Growth Rates

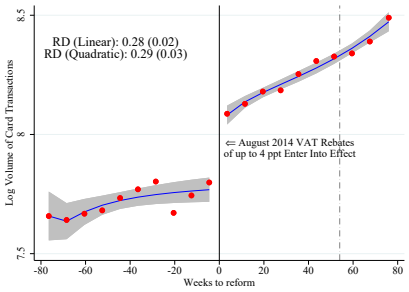
I. Number of Card Transactions

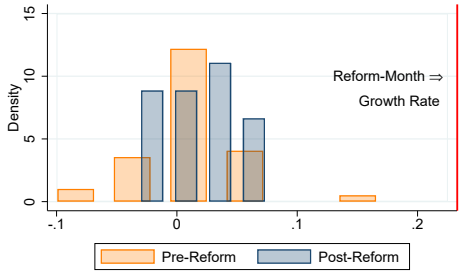




Mean Pre Reform (0.011) vs Mean Post Reform (0.018): ttest p-value (0.530)
Month of Introduction (0.439) vs Mean All Other Months (0.013): ttest p-value (0.000)
Mean Pre Reform (0.011) vs Month of Introduction (0.439): ttest p-value (0.000)
Randomization Inference p-value: (0.000)

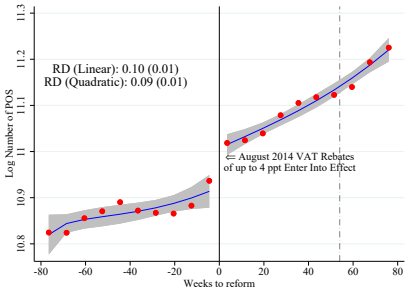
II. Volume of Card Transactions

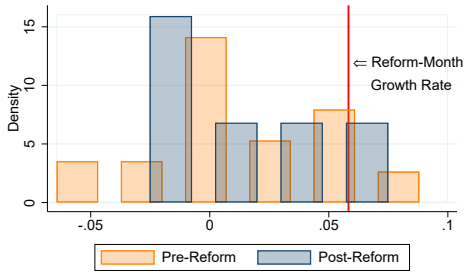




Mean Pre Reform (0.012) vs Mean Post Reform (0.024): ttest p-value (0.343)
Month of Introduction (0.233) vs Mean All Other Months (0.015): ttest p-value (0.000)
Mean Pre Reform (0.012) vs Month of Introduction (0.233): ttest p-value (0.000)
Randomization Inference p-value: (0.000)

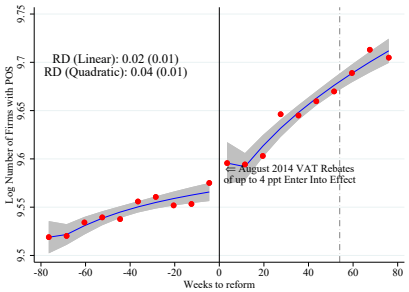
III. Number of POS

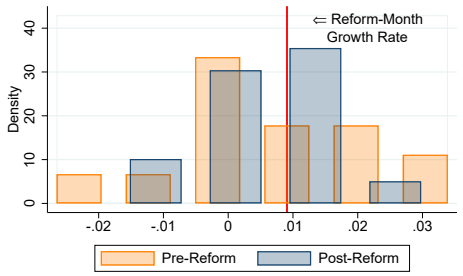




Mean Pre Reform (0.011) vs Mean Post Reform (0.014): ttest p-value (0.740)
Month of Introduction (0.058) vs Mean All Other Months (0.012): ttest p-value (0.000)
Mean Pre Reform (0.011) vs Month of Introduction (0.058): ttest p-value (0.000)
Randomization Inference p-value: (0.136)

IV. Number of Firms with a POS

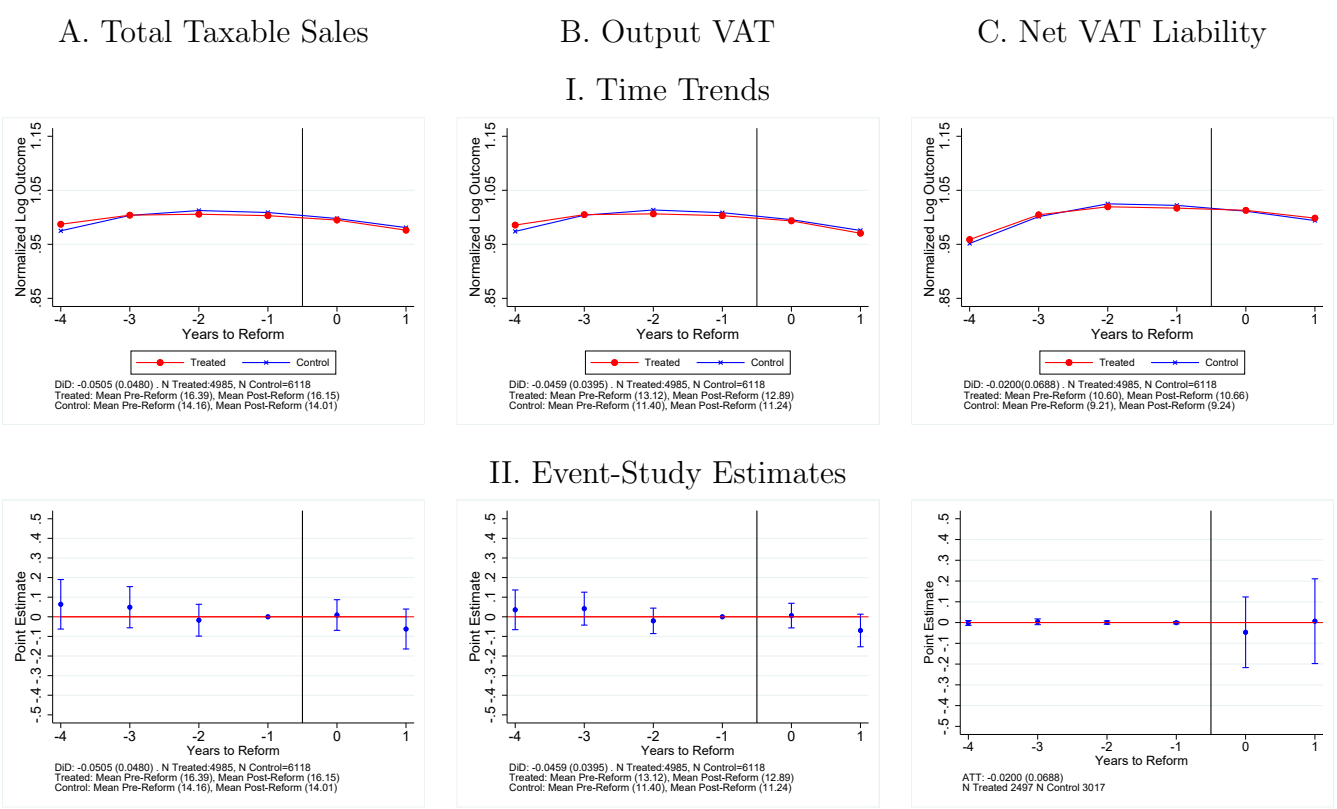




Mean Pre Reform (0.005) vs Mean Post Reform (0.007): ttest p-value (0.667)
Month of Introduction (0.009) vs Mean All Other Months (0.006): ttest p-value (0.086)
Mean Pre Reform (0.005) vs Month of Introduction (0.009): ttest p-value (0.137)
Randomization Inference p-value: (0.373)

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 where 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 4.2.

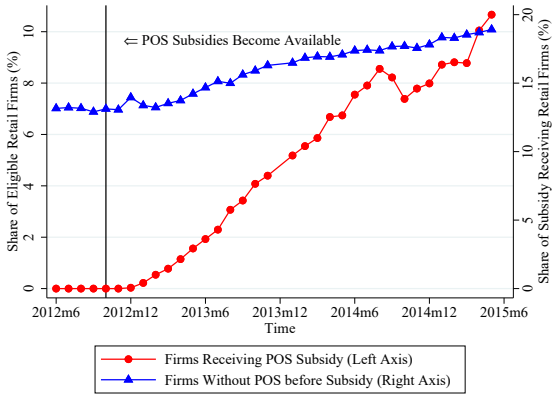
Figure 3: The Effect of VAT Rebates on Tax Compliance
Retailers w/ POS vs Wholesalers Difference-in-Difference Estimation



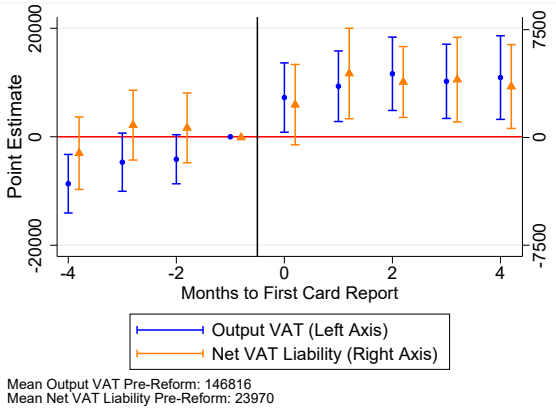
Notes: These graphs implement a DiD estimation comparing retail firms that had a POS at some point prior to 2014 (treated) to wholesale firms (control) around the introduction of the VAT rebates in 2014 (year 0). Panel I shows the normalized time trends and the DiD estimate β on the $Retailer_i \cdot PostReform_t$ interaction from equation 2. Panel II shows the event study estimates β_k 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 5.3. Table 1 shows the robustness of the results to various alternative specifications.

Figure 4: Explaining the Absence of a Tax Compliance Effect

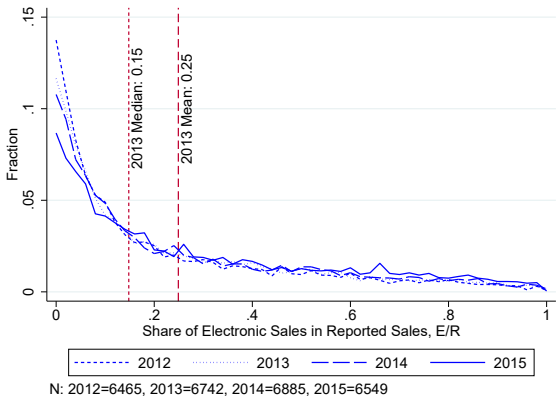
A. Firms' Slow Take-up of POS Subsidies



B. Tax Liability Response to POS Adoption



C. Share of Card Transactions in Total Sales



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 month before and after the event. Standard errors are clustered at the firm level and the outcome variable is winsorized at the 99th 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 6.

Table 1: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates

	Taxable Sales				Output VAT				Net Liability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post · Treated	-0.051	-0.042	-0.069	-0.031	-0.046	-0.039	-0.059	-0.034	-0.020	-0.014	-0.034
	(0.048)	(0.048)	(0.046)	(0.045)	(0.040)	(0.039)	(0.038)	(0.037)	(0.064)	(0.064)	(0.083)
Balanced Sample	Y	Y	-	-	Y	Y	-	-	Y	Y	Y
Unbalanced Sample	-	-	Y	Y	-	-	Y	Y	-	-	-
Winsor at p99	Y	-	Y	Y	Y	-	Y	Y	Y	-	Y
Winsor at p95	-	Y	-	-	-	Y	-	-	-	Y	-
Includes 2016 data	-	-	-	Y	-	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	4985	4985	6906	6819	4985	4985	6906	6819	2497	2497	1168
N Control (Wholesalers)	6118	6118	9044	9340	6118	6118	9044	9340	3017	3017	1812

Notes: This table documents the robustness of our main DiD specification discussed in Section 5.2. The table displays the DiD estimate β from equation 2 for the different outcomes. Columns 1, 5 and 9 reproduce our preferred specification (as shown in Figure 3). Columns 2, 6 and 10 show the robustness of our results to more conservative top coding (winsorizing at p95). Columns 3 and 7 show that the results are very similar when considering an unbalanced sample of taxpayers who file every year 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, columns 4, 8 and 11 show the robustness of our results to an extended sample which includes observations for the year 2016. This table is discussed in Section 5.3. Figure C.1 shows the graphical representation of these results.

Online Appendix: Not for Publication

This appendix contains additional information and analyses. Appendix A provides additional contextual information. Appendix B provides additional results and robustness tests for the regression discontinuity estimation. Appendix C provides additional results and robustness tests for the difference-in-difference estimation. Appendix D provides additional support for our interpretation of the results and their policy implications. Appendix E provides additional analyses of the determinants and impact of POS adoption by firms.

A Context Appendix

Table A.1: Policies Incentivizing the Use of Electronic Payment Technologies

A. VAT Rebates	
Argentina	5 percent VAT refund on debit card purchases <ARS 1000 (USD 51) [2001-2017] 3 percent VAT refund for credit cards [2003-2009]
Brazil (SP)	3 percentage points VAT rebate fore consumers requesting e-receipt [2007-]
Colombia	2 percentage points VAT rebate for card purchases [2004-2014]
Japan	2 or 5 percentage points rebates for consumers making cashless purchases at registered business [2019-]
Korea, Rep.	VAT tax credit for merchants. 0.5% of credit card sales [1994], 1% [1996-2000] and 2% [2000-], with 5 million won ceiling
Uruguay	2-4 percentage point VAT rebates for card payments [2014-]
B. Income Tax Rebates	
Colombia	Cash payments deductible only below certain thresholds
Greece	Income tax discount of up to 22% of electronic purchases, up a threshold proportional to income [2017-]
Mexico	Allowable deductions of a company's expenditure must be backed by a digital tax receipt or electronic transaction if >2000 pesos (107$)
Korea, Rep.	Share of electronic payments deductible from taxable labor income: 10% of transaction amount [1999-2002] up to a ceiling of 3 million won or 10% of total labor income; rate was revised over the years, reaching 30% for some years
C. POS Subsidies	
Argentina	Up 50% of monthly POS rental fee can be claimed as fiscal credit by merchant; no transaction fee and rental fee waver for small merchants in first two years [2016-]
Japan	Subsidies to installing cashless payment systems to 2 million eligible small and medium sized businesses [2019-]
Malaysia	Subsidized POS terminals
Mexico	Free POS installation and fixed monthly merchant fee up to certain transaction volume [2004-]; Ministry of Finance subsidized tablet equipped with MPOS
Uruguay	Eligible merchants can claim an income tax exemption of up to 100% of the value of the POS investment (subsidy rate revised over time) [2012-]
D. Lotteries	
Brazil (SP)	Lotteries for consumers requesting an e-receipt, providing national ID [2007-]
Greece	Lotteries for consumers [October 2017-]; automatic participation when paying by electronic means; tickets awarded correspond to aggregate monthly amount spent by electronic means
India	Lotteries for merchants and consumers [2016-]
Mexico	Lotteries (cars) for consumers [2004-]
Netherlands	Lotteries for merchants and consumers [2002-]
Korea, Rep.	Lotteries for merchants and consumers, one credit card invoice stub per month randomly chosen as winner

Notes: This table compiles a non-exhaustive list of countries employing incentive schemes similar to those we study in this paper. Our compilation focuses on financial and fiscal policies to incentivize the use of electronic payment technology. It is based on World Bank Group (2014), Naritomi (2019) and Nicolaides (2021). The information for Brazil is for the state of São Paulo. This table is discussed in the introduction, Section 1.

Table A.2: Mandates for Payments to be Conducted Electronically

Type of Transactions	**Initial Deadline**	**Final Deadline**
Tax payments	06/01/2015	06/01/2015
Payments to service providers to the state	12/01/2014	07/01/2015
Rental payments	12/01/2014	12/01/2015
Purchase of apartments/houses, cars, any transactions > UI 160,000 (USD 20,000)	06/01/2015	12/01/2015
Payments over 60,000 UI (180,000 USD) to professional service providers	05/01/2016	05/01/2016
Wages, pensions, social security contributions	11/01/2015	05/02/2017

Notes: This table shows the types of payments which Uruguay's financial inclusion law mandated to be done through electronic payment methods, and the deadlines by which these mandates were initially meant to enter into effect, as well as the final deadlines which were ultimately applied, if applicable. Several of the deadlines had to be revised due to private sector opposition or logistical challenges. This table is discussed in Section 3.3.

Table A.3: Summary Statistics for 2013

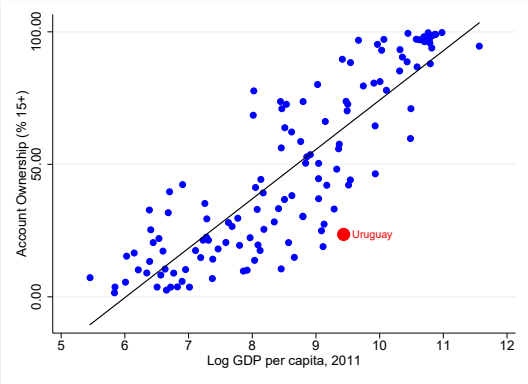
					Percentile				
		Mean	SD	Min	5th	25th	50th	75th	90th	Max
All Firms N=69892	Total Annual Sales	10,064	31,387	0	0	112	1,941	6,396	18,687	264,242
	Input VAT	863	2,392	0	0	0	134	614	1,905	18,687
	Output VAT	1,283	3,525	0	0	0	275	979	2,696	28,573
	Net VAT Liability	407	1,234	0	0	0	62	297	854	10,408
	Sole Proprietorship	0.33	0.47	0	0.02	0.04	0.31	0.37	0.36	0.33
	Corporation	0.38	0.49	0	0.78	0.76	0.46	0.36	0.36	0.38
	Has POS	0.18	0.38	0	0.00	0.01	0.08	0.14	0.17	0.18
	Number of Card Transactions	2.21	5.82	0.00	0.00	0.07	0.37	1.58	5.35	43.27
	Volume of Card Transactions	2,965	6,413	0	10	142	680	2,577	9,693	46,297
	Share of Electronic Sales	0.12	0.57	0.00	0.00	0.00	0.00	0.00	0.20	9.90
Retail Firms w/o POS N=4761	Total Annual Sales	7,369	14,087	0	0	1,173	2,899	6,813	16,439	93,056
	Input VAT	813	1,728	0	0	58	281	732	1,862	11,814
	Output VAT	992	2,079	0	0	93	363	881	2,210	14,408
	Net VAT Liability	164	356	0	0	3	47	145	404	2,396
	Sole Proprietorship	0.58	0.49	0	0.13	0.53	0.64	0.65	0.62	0.58
	Corporation	0.15	0.36	0	0.64	0.23	0.13	0.12	0.12	0.15
	Has POS	0.00	0.00	0	0.00	0.00	0.00	0.00	0.00	0.00
	Number of Card Transactions	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Volume of Card Transactions	0	0	0	0	0	0	0	0	0
	Share of Electronic Sales	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Retail Firms w/ POS N=6258	Total Annual Sales	10,586	15,070	0	556	2,293	5,269	12,070	25,664	93,056
	Input VAT	1,431	2,038	0	36	290	702	1,612	3,598	11,814
	Output VAT	1,695	2,367	0	55	372	859	1,943	4,184	14,408
	Net VAT Liability	259	374	0	0	45	128	310	654	2,396
	Sole Proprietorship	0.40	0.49	0	0.38	0.54	0.52	0.47	0.43	0.40
	Corporation	0.20	0.40	0	0.22	0.13	0.13	0.15	0.18	0.20
	Has POS	0.96	0.21	0	0.69	0.90	0.93	0.95	0.95	0.96
	Number of Card Transactions	3.45	8.04	0.00	0.01	0.14	0.55	2.13	9.10	43.27
	Volume of Card Transactions	3,896	8,546	0	12	184	787	2,854	10,460	46,297
	Share of Electronic Sales	0.51	1.10	0.00	0.00	0.03	0.14	0.44	1.22	9.51
Wholesale Firms N=7818	Total Annual Sales	21,934	49,165	0	0	1,070	4,851	16,512	52,690	264,242
	Input VAT	1,995	3,997	0	0	30	420	1,749	5,642	18,687
	Output VAT	2,638	5,531	0	0	61	628	2,241	6,830	28,573
	Net VAT Liability	581	1,544	0	0	0	106	431	1,207	10,408
	Sole Proprietorship	0.24	0.43	0	0.03	0.18	0.28	0.28	0.26	0.24
	Corporation	0.47	0.50	0	0.79	0.57	0.44	0.43	0.45	0.47
	Has POS	0.17	0.38	0	0.00	0.07	0.15	0.18	0.18	0.17
	Number of Card Transactions	1.13	1.70	0.00	0.00	0.05	0.27	1.35	5.09	5.35
	Volume of Card Transactions	2,330	3,314	0	13	143	734	2,800	10,392	10,392
	Share of Electronic Sales	0.10	0.51	0.00	0.00	0.00	0.00	0.00	0.13	8.95

Notes: This table reports summary statistics of relevant variables for four different samples: all firms, retail firms without POS, retails firms with POS (as observed at some point before 2014), and wholesale firms. The statistics shown are for 2013. The number and volume of card transactions and the share of electronic sales are limited to firms with a POS. All monetary values and the number of card transactions are winsorized at the 99th percentile and displayed in thousands of Uruguayan pesos (1 USD= 43 UYU in July 2021). The percentiles columns for the binary outcome "Has POS" show the mean outcome across the distribution of firms based on sales size. The group of retail firms with POS includes some firms that had a POS prior to 2013 but do not register card transactions in 2013. This table is discussed in Section 3.4.

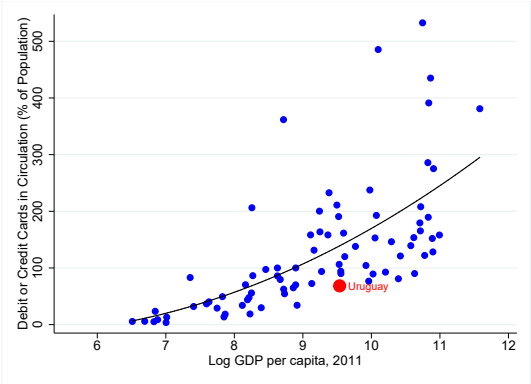
Figure A.1: Financial Inclusion and Tax Compliance
Uruguay in a Cross-Country Comparison

A. Financial Inclusion Indicators

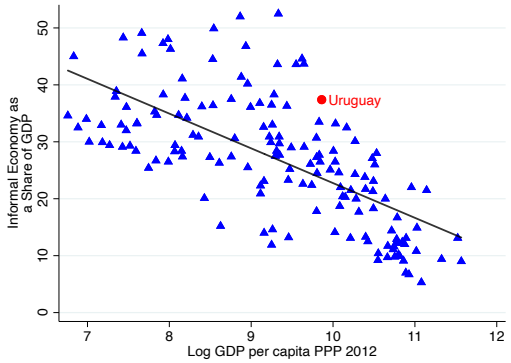
A.I: Account Ownership



A.II: Credit/Debit Cards in Circulation

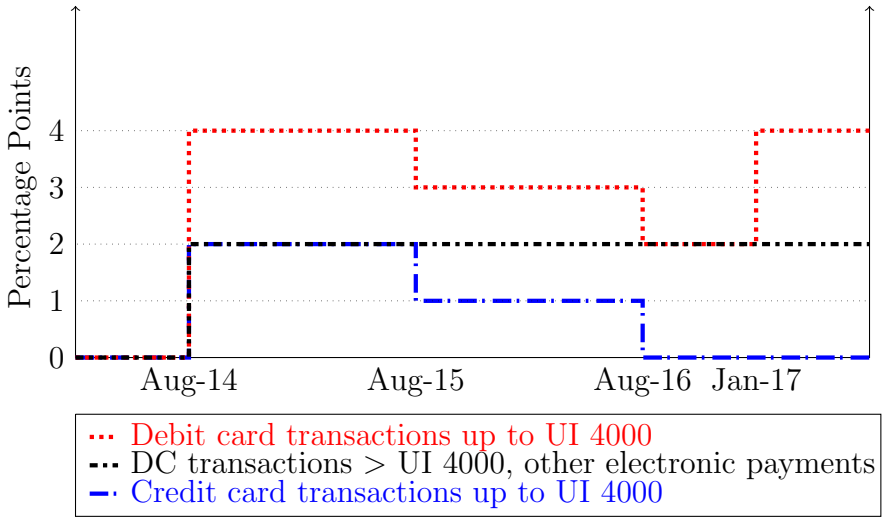


B. Size of the Informal Economy



Notes: As discussed in the Introduction and in Section 3.1, Uruguay lagged behind peer countries in terms of financial inclusion. Panel A plots the cross-country relationship between financial inclusion and GDP per capita. Panel A.I display data on account ownership, as measured by the percentage of the population (15 years +) with an account at any formal financial institution in 2011. Panel A.II display data on debit and credit card circulation, as measured by the percentage of the population (15 years +) with ownership of a debit and/or credit card in 2014. The GDP data is from the World Bank World Development Indicators Database. The account ownership data is from the World Bank Global Findex Database. The credit/debit card data is from the Global Payments System Survey. Panel B plots the cross-country relationship between the size of the informal economy (measured as a share of GDP) and GDP per capita for 158 countries in 2012. The measure for the size of the informal economy is from Medina and Schneider (2018). The GDP data is from the World Bank World Development Indicators Database.

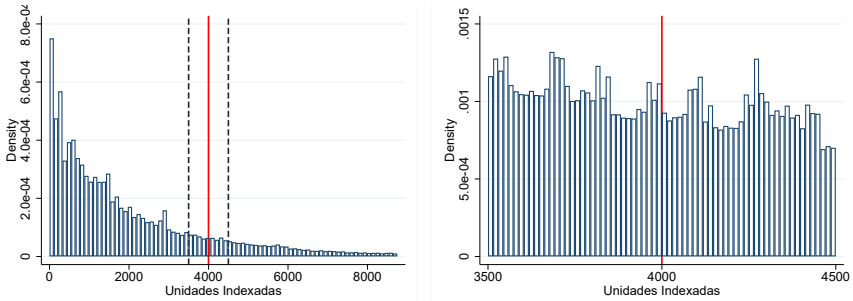
Figure A.2: VAT Rebates Applied to Credit/Debit Card Purchases



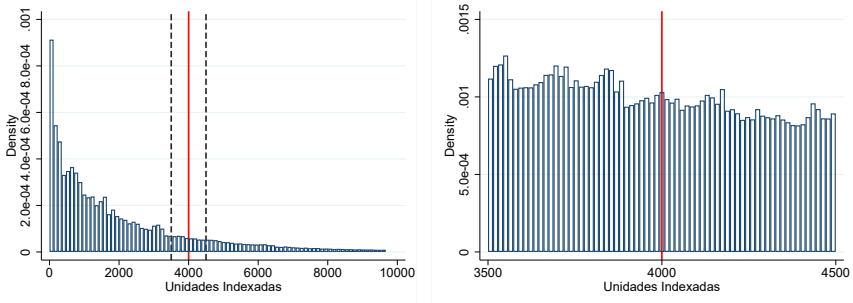
Notes: This figure displays the size of the VAT rebates (in percentage points) granted to consumers for various type of transactions with electronic payment technology. The rebate rates are differentiated by type of payment method and by transaction amount as measured in *Unidades Indexadas* (UI), a Uruguayan accounting unit. In August 2014, 4,000 UI were equivalent to approximately USD 500. The standard VAT rate in Uruguay was 22 percent during the period of the study, and the reduced rate was 10 percent. A four percentage point rebate thus implies that the consumer paid a VAT of 18 percent on standard-rated goods and a rate of 6 percent on reduced-rate goods. This Figure is discussed in Section 3.2.

Figure A.3: Absence of Bunching in Card Transaction Amounts at 4,000 UI Threshold

A. 2014

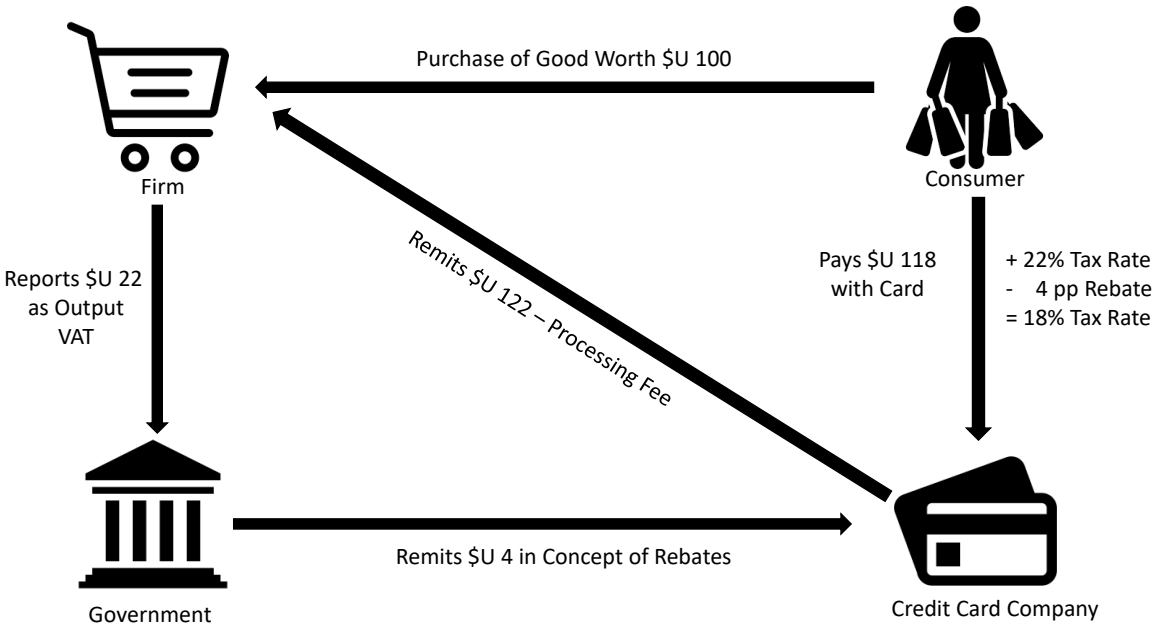


B. 2015



Notes: This figure shows histograms of credit and debit card transaction amounts for 2014 and 2015. The left panels show the entire distribution and the right panels zoom in on the distribution around the thresholds of 4,000 Unidades Indexades (UI), the red vertical line, at which the size of the VAT rebate drops discontinuously. The conversion rate from Uruguayan pesos to UI is updated daily. This figure is mentioned in Section 3.2, footnote 10.

Figure A.4: The Implementation of VAT Rebates



Notes: This Figure illustrates the implementation of the VAT rebates for all parties involved, as discussed in Section 3.2.

Figure A.5: Purchase Receipt with VAT Rebate

Notes: This figure shows an example of a receipt where a VAT rebate ("Descuento Ley 17934") was applied. This is discussed in Section 3.2.

Figure A.6: News Coverage of the VAT Rebates

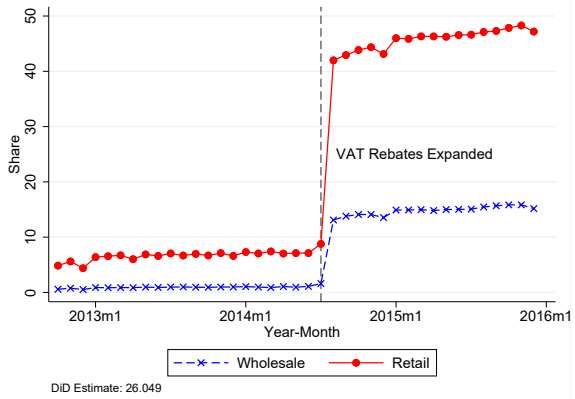
A. Information about VAT Rebate Introduction

B. Guide on How to Benefit from VAT Rebates

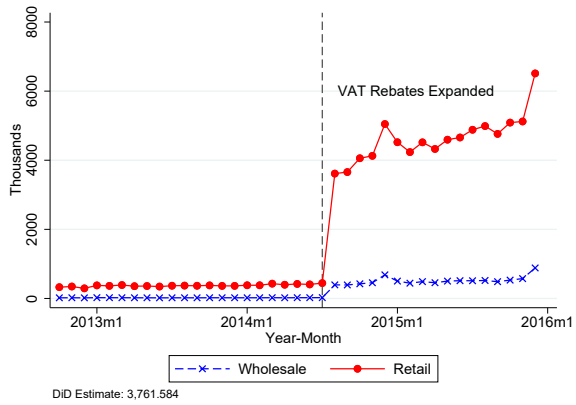
Notes: The figure displays examples of the media coverage of the VAT rebate introduction on August 1, 2014. The article in Panel A (published in June 2014) informs about the introduction on the VAT rebates, while the article in Panel B (published in August 2014) describes the steps consumers should follow to maximize their benefit from the VAT rebates. This is discussed in Section 3.2.

Figure A.7: Variation in VAT Rebates Across Sectors

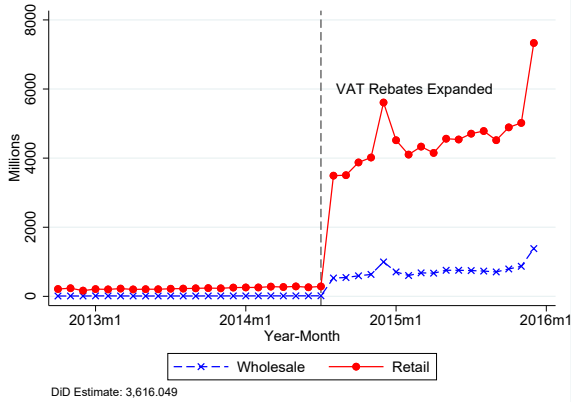
A: Share of Firms Registering any Rebate



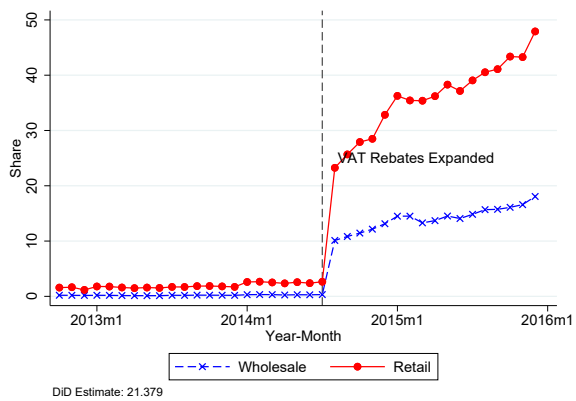
B: Number of Transactions with Rebate



C: Volume of Transactions with Rebate

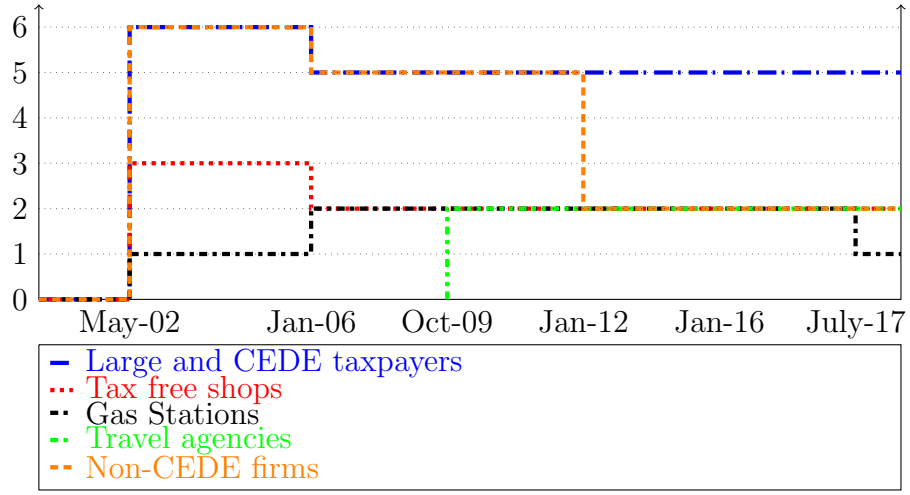


D: Volume of Rebate-Transactions/Total Sales



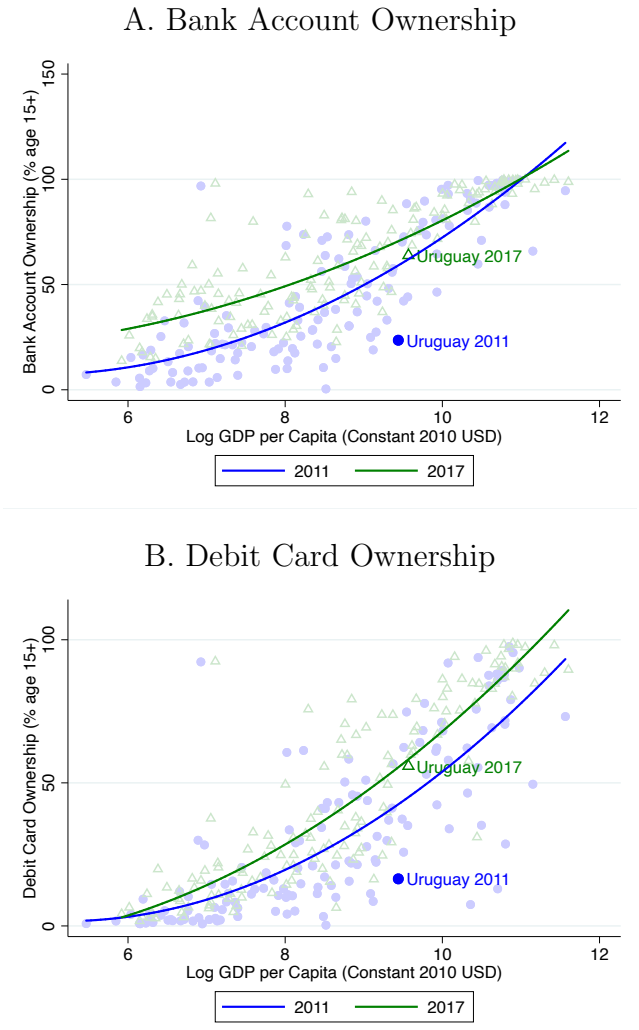
Notes: This figure shows that the VAT rebates were indeed implemented starting on August 2014, as stipulated by the Financial Inclusion Reform. The patterns are consistent with the fact that rebates were available only for business-to-consumer transactions. Panel A plots the percentage of firms registering VAT rebates for consumers paying by credit/debit card, as captured in the card transaction data. The share of firms receiving VAT rebates prior to the reform is not zero, as card purchases at hotels, restaurants and tourism businesses have been subject to a 9 ppt VAT rebate since 2006. These firms should not be part of the retail or wholesale sectors in the ISIC classification, but there is some measurement error in firms' sector classifications. Panels B and C show the aggregate number and volume of transactions with a rebate by sector. Panel D shows the volume of transactions with a rebate as a share of firms' total sales volume. This figure is discussed in Sections 3.2 and 5.1.

Figure A.8: Tax Withholding Rates Applied to Credit/Debit Card Sales



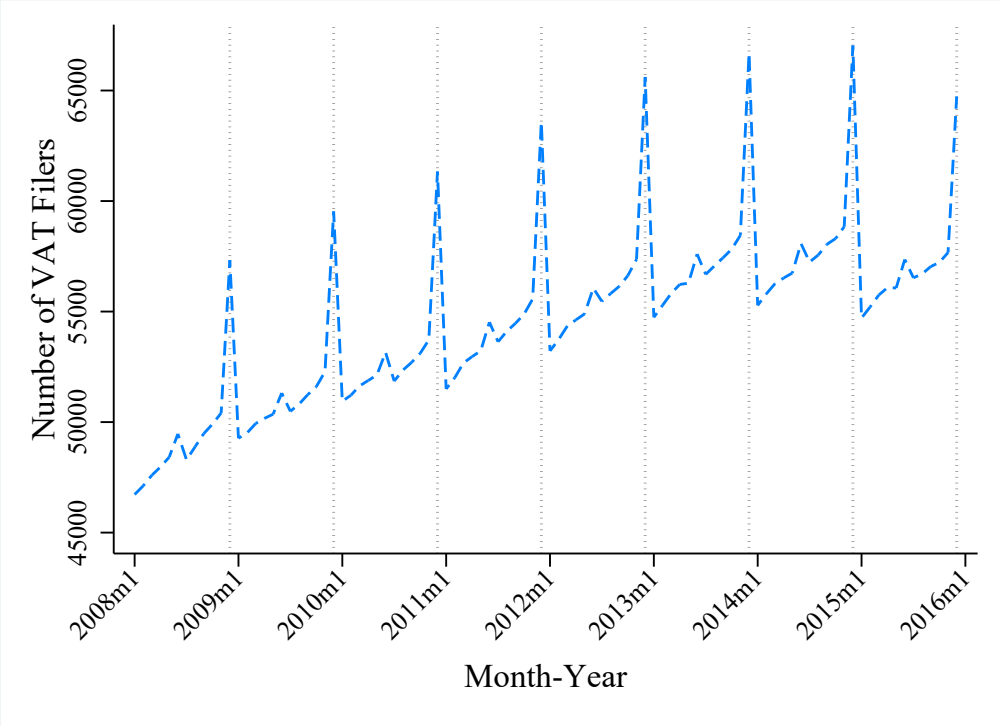
Notes: This figure displays the withholding rates applied by credit/debit card companies to firms making sales using a POS. The rates are differentiated by type of firm (receiving the income from the transaction). CEDE (*Control Especial de Empresas*) is the Uruguayan equivalent of the large taxpayer unit. This figure is discussed in Sections 3.3 and 6.

Figure A.9: Financial Inclusion in Uruguay and the World Over Time
Pace of Progress in Uruguay Relative to Other Countries



Notes: Similarly to figure A.1, this figure plots the cross-country relationship between financial inclusion indicators from the World Bank Global Findex Database and GDP per capita for 2011 and 2017. This figure is discussed in Section 3.3.

Figure A.10: Number of VAT Filers by Month



Notes: This figure plots the number of unique VAT filers in each month. The dotted vertical lines mark the month of December each year. For firms that file annually and retrospectively report output VAT and input VAT for each month, we consider that the firm filed for a particular month if it reported output VAT or input VAT for that month. This figure is discussed in Section 3.4.

A.1 Simplified Tax Regimes

Firms below certain size thresholds can opt into a simplified tax regime. The *monotributo* regime for micro firms unifies all taxes and social security contributions. The *literal E* regime for small firms unifies the CIT and VAT into a monthly lump-sum payment and allows firms to pay social security contributions at a reduced rate. Firms in these two regimes thus do not remit VAT on their sales nor claim credit for VAT paid on their inputs. As eligibility is partly based on turnover, and credit and debit card reports can help the tax administration confirm a firm's true turnover, the financial inclusion reforms might have generated an increase in the number of firms graduating from the simplified tax regimes into the general VAT regime. However, conditional on a firm remaining in a simplified regime, its tax liability and compliance behavior should not be affected by the financial inclusion reforms. Figure A.10 shows no indication that the introduction of the VAT rebates pushed an increased number of simplified regime firms to graduate into the regular VAT regime.

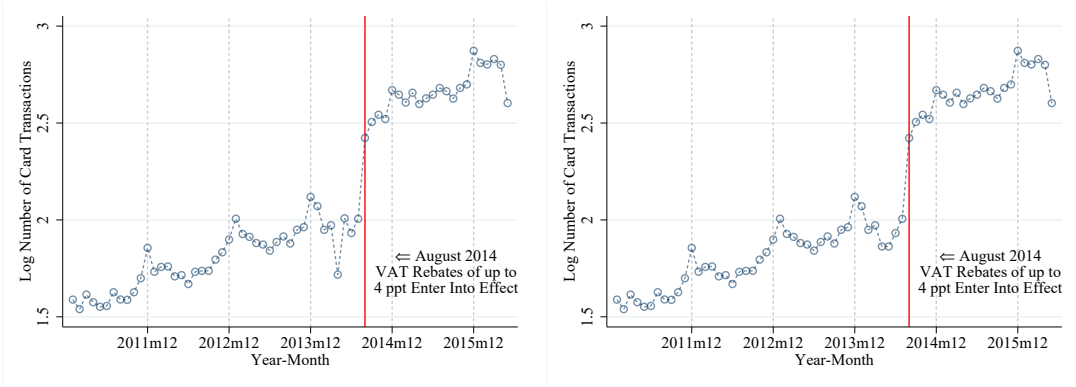
B Regression Discontinuity Appendix

B.1 Robustness Tests

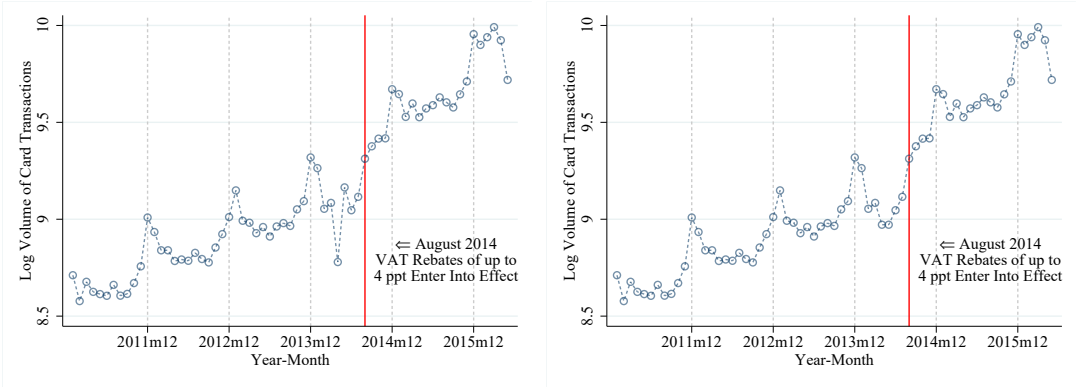
Figure B.1: Raw Data with Outlier in April 2014

A. Raw Trends B. Transformed Trends

I. Number of Card Transactions



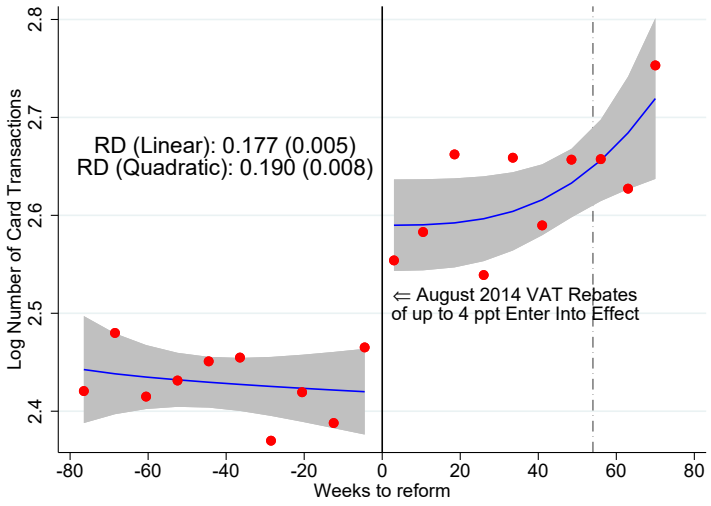
II. Volume of Card Transactions



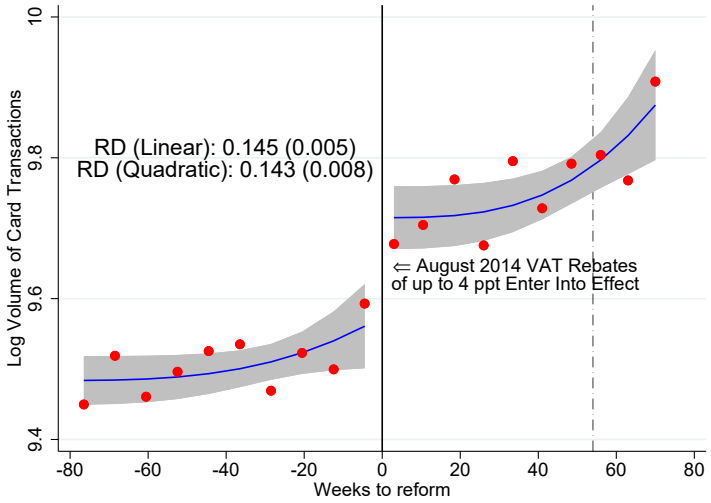
Notes: This figure shows that the months of April and May 2014 constitute outliers in terms of the number of card transactions and the volume of transactions, with a short-lived drop in both outcomes in April 2014 and a strong recovery in May 2014. We hypothesize that this might be due to consumers temporarily postponing purchases in anticipation of the passage of the financial inclusion reform. The VAT rebate provisions were indeed widely debated in the media and consumers might have falsely expected those provisions to enter into effect imminently. After realizing that the rebates would not enter into effect until August, they conducted in May the purchases they had initially postponed in April. To account for this, we average these two outcomes over April and May 2014 in Figure 1. No change is applied to the data used in the regression discontinuity estimations, as these are run on weekly data.

Figure B.2: The Effect of VAT Rebates on the Use of Electronic Payment Technology
RD Estimates Based on Firm-Level Data

A. Log Number of Card Transactions



B. Log Volume of Card Transactions

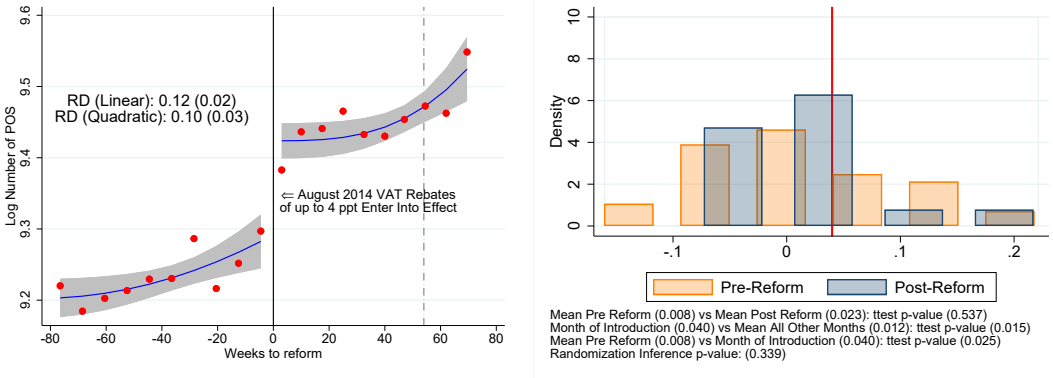


Notes: This figure is similar to Figure 2, Panels AI and AII, but relies on firm-level data to conduct the RD estimation. The estimation uses the firm-level version of equation 1 and controls for firm fixed effects. The estimate hence captures the average response to the VAT rebate introduction, weighing all firms equally. This figure is discussed in Section 4.2.

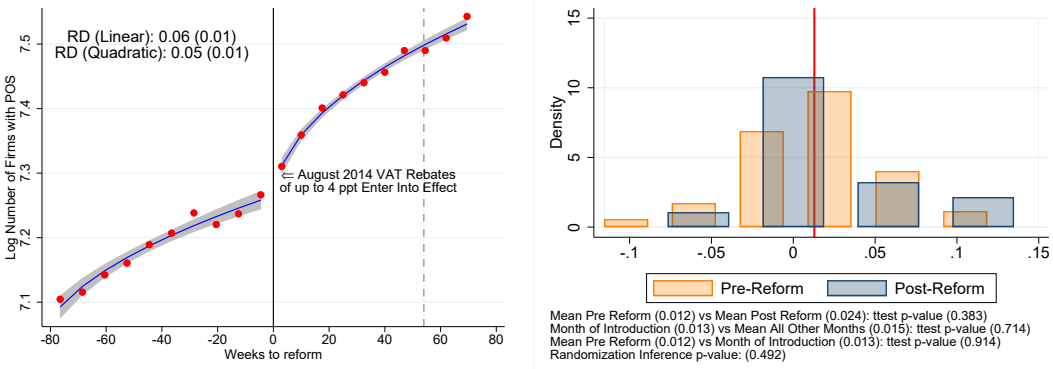
Figure B.3: The Effect of VAT Rebates on the Use of Electronic Payment Technology
RD Estimates and Month-on-Month Growth Rates for Sectors with Low POS Adoption

A. RD Estimation B. Month-on-Month Growth Rates

I. Log Number of Card Transactions

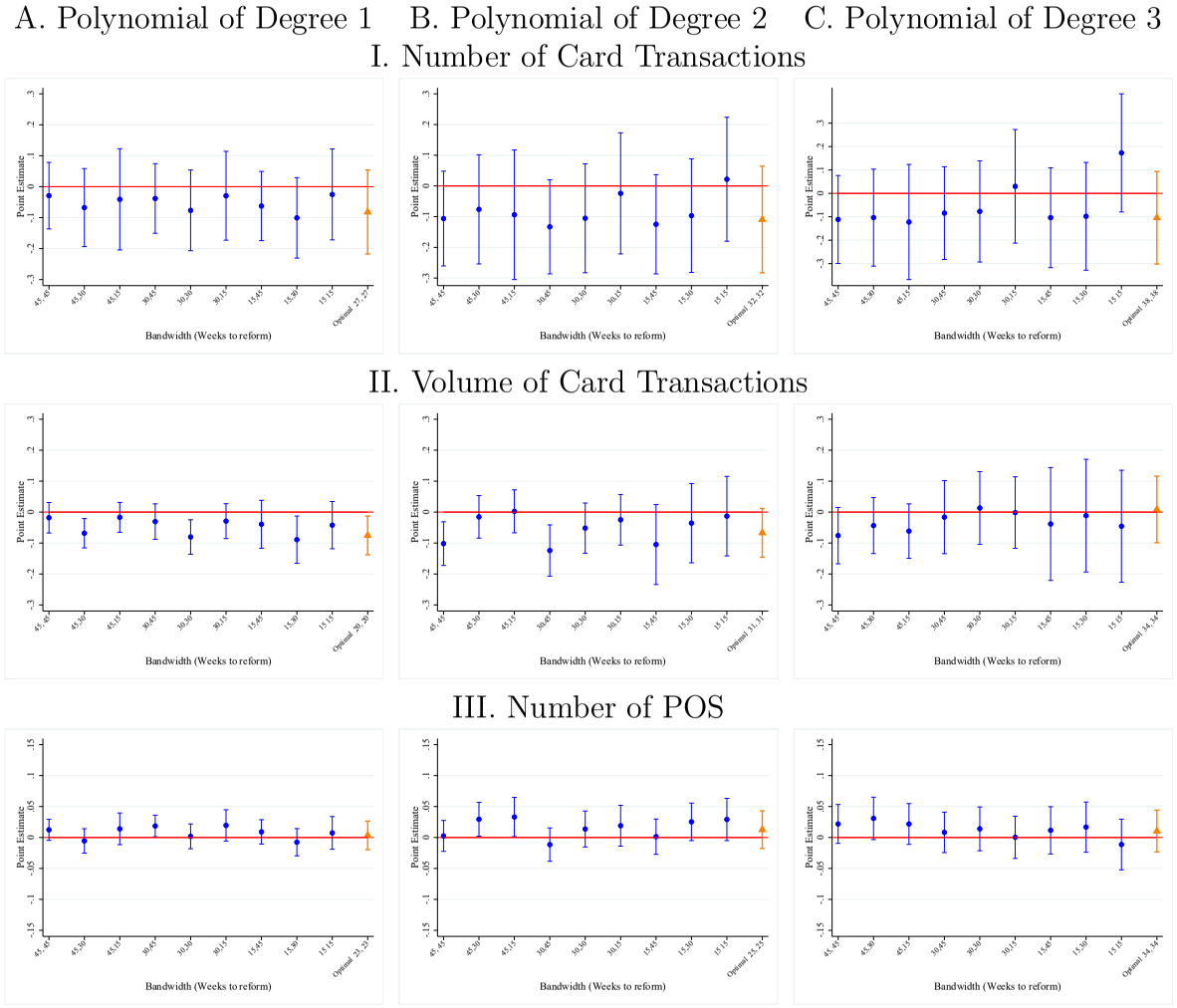


II. Log Volume of Card Transactions



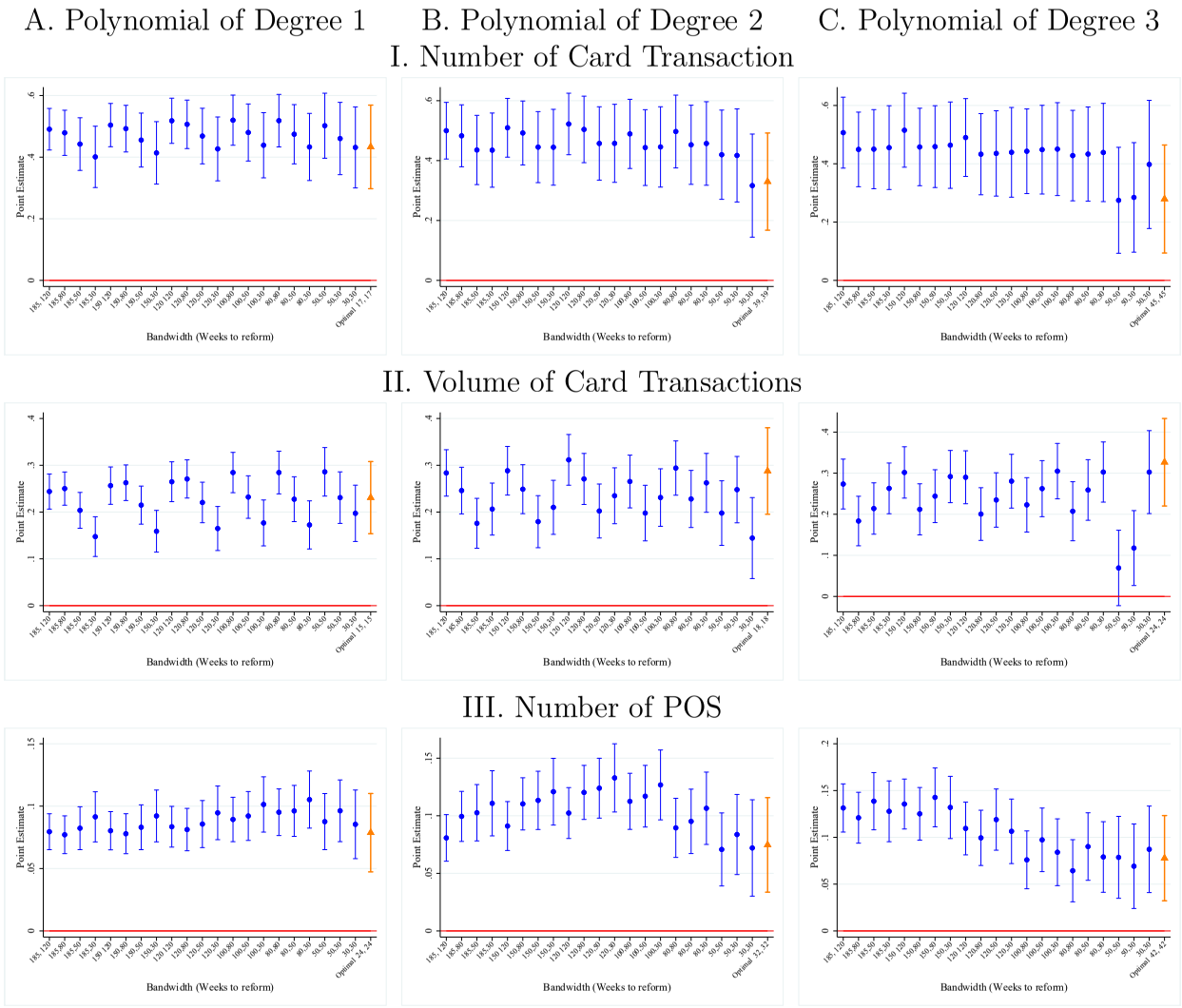
Notes: This figure is similar to Figure 2 but zooms in on retail firms in four-digit subsectors with low POS adoption prior to the reform (in 2013). Low POS adoption is defined as having a below-median share of firms with a POS. This figure is discussed in Section 4.2.

Figure B.4: The Effect of the Reduction of VAT Rebates in August 2015 - RD Estimates



Notes: This Figure is similar to Figure B.5, but documents the RD estimates around the reduction of the VAT rebates in August 2015, showing that the reduction did not have a statistically significant effect on any of the outcomes. Each panel plots the RD estimate γ_0 from equation 1 and the 95 percent confidence intervals, for different bandwidth values (weeks to reform). Each row reports results for a different outcome, and each column presents the estimates for a different order of polynomial. The orange triangle marker indicate the result from an RD estimation with optimal bandwidth as in (Calonico et al., 2014). This figure is discussed in Section 4.2.

Figure B.5: Robustness of RD Estimates to Different Bandwidths and Degrees of Polynomial

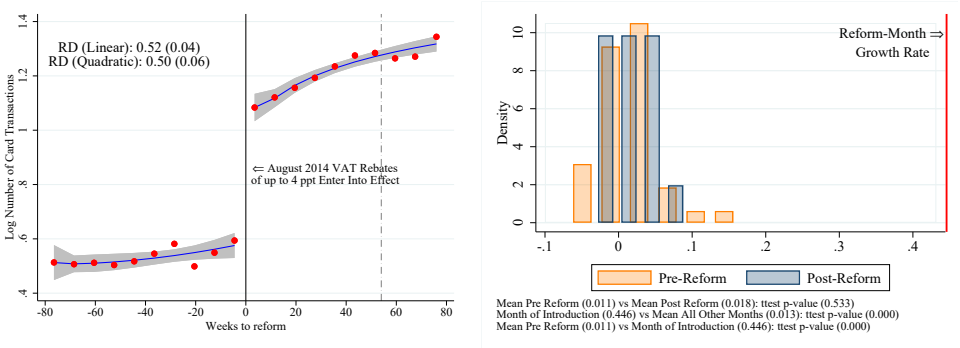


Notes: This figure documents the robustness of the RD estimations displayed in Figure 1. Each panel plots the RD estimate γ_0 from equation 1 and the 95 percent confidence intervals, for different bandwidth values (weeks to reform). Each row reports results for a different outcome, and each column presents the estimates for a different order of polynomial. The orange triangle marker indicate the result from an RD estimation with optimal bandwidth as in (Calonico et al., 2014). This figure is discussed in Section 4.3.

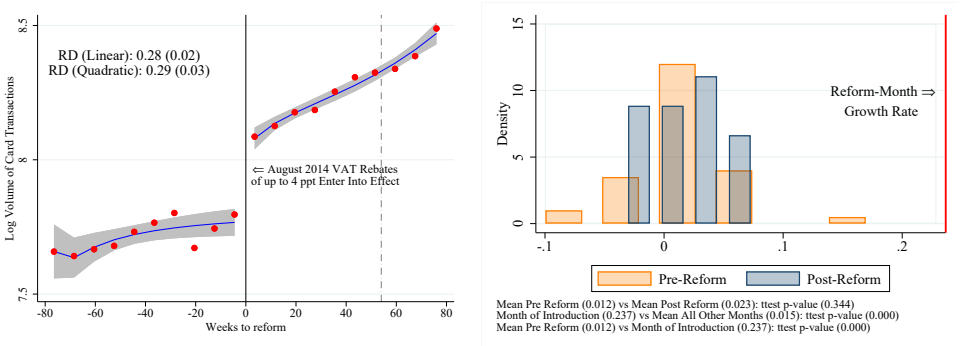
Figure B.6: Robustness of RD Estimates to Controlling for POS Subsidy Roll-Out

A. RD Estimates B.Month-on-Month Growth Rates

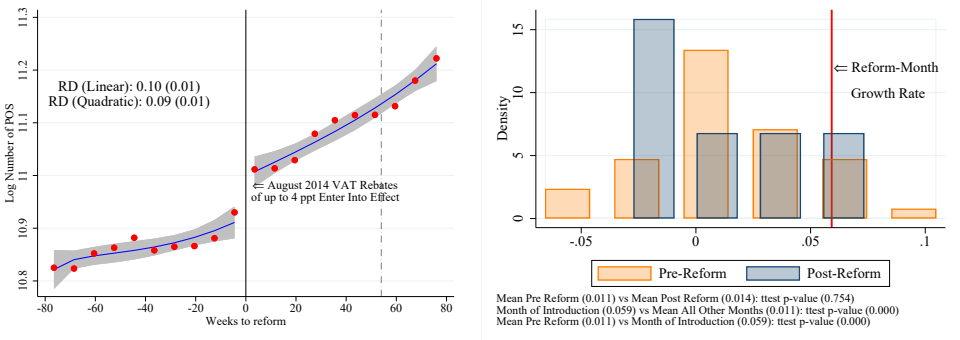
I. Number of Card Transactions



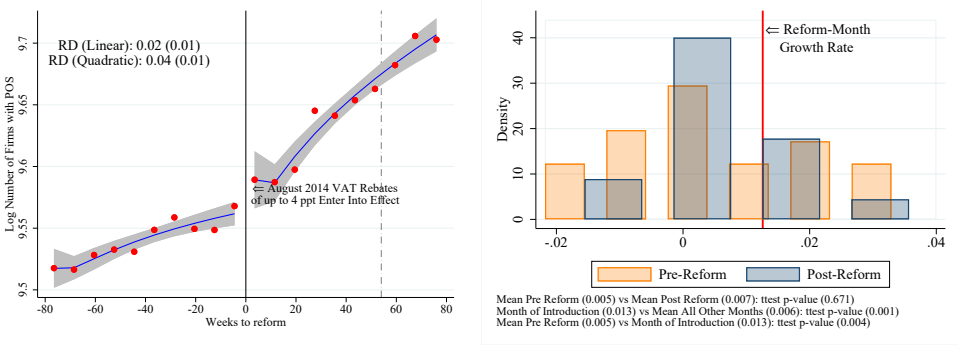
II. Volume of Card Transactions



III. Number of POS

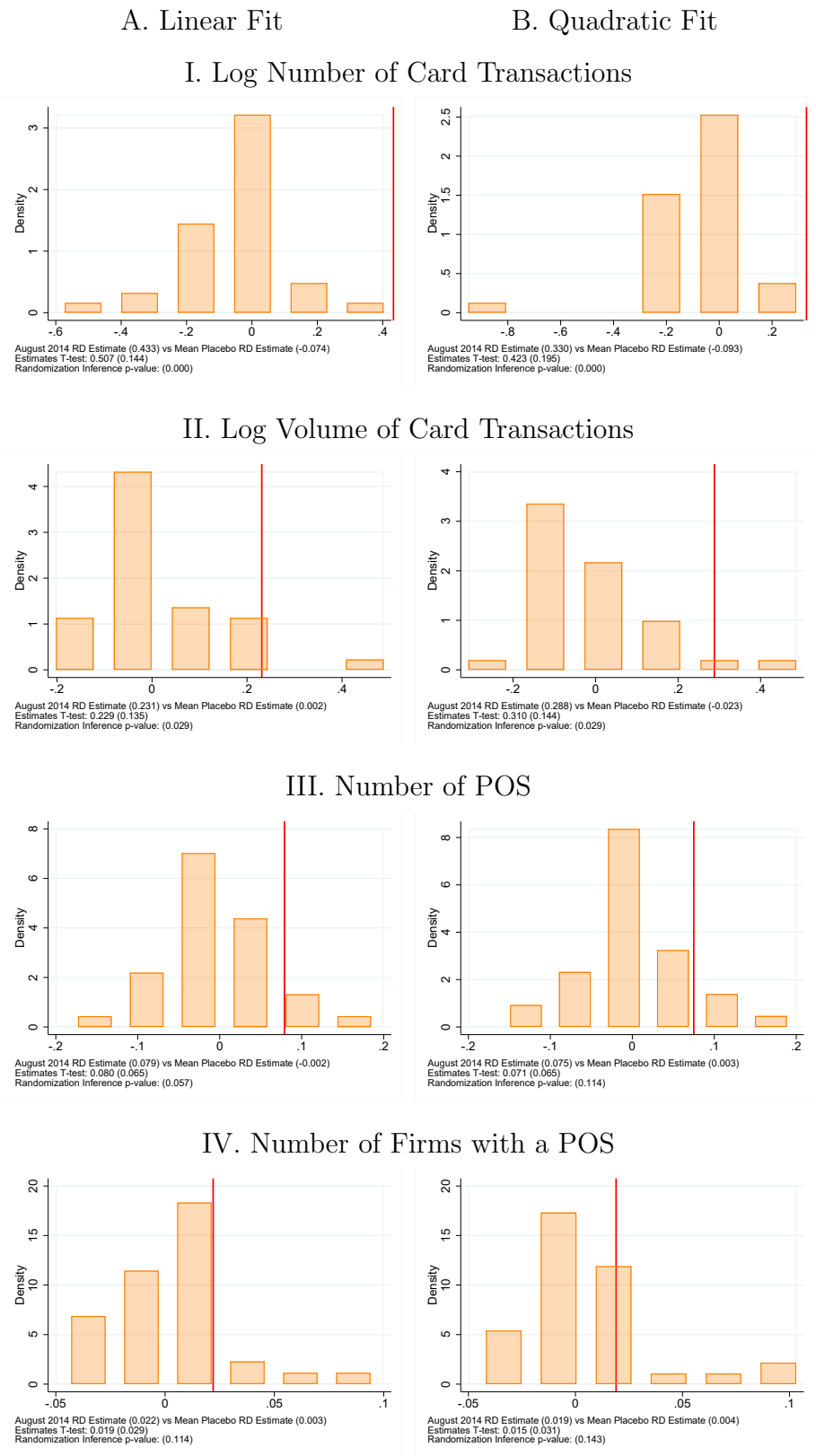


IV. Number of Firms with a POS



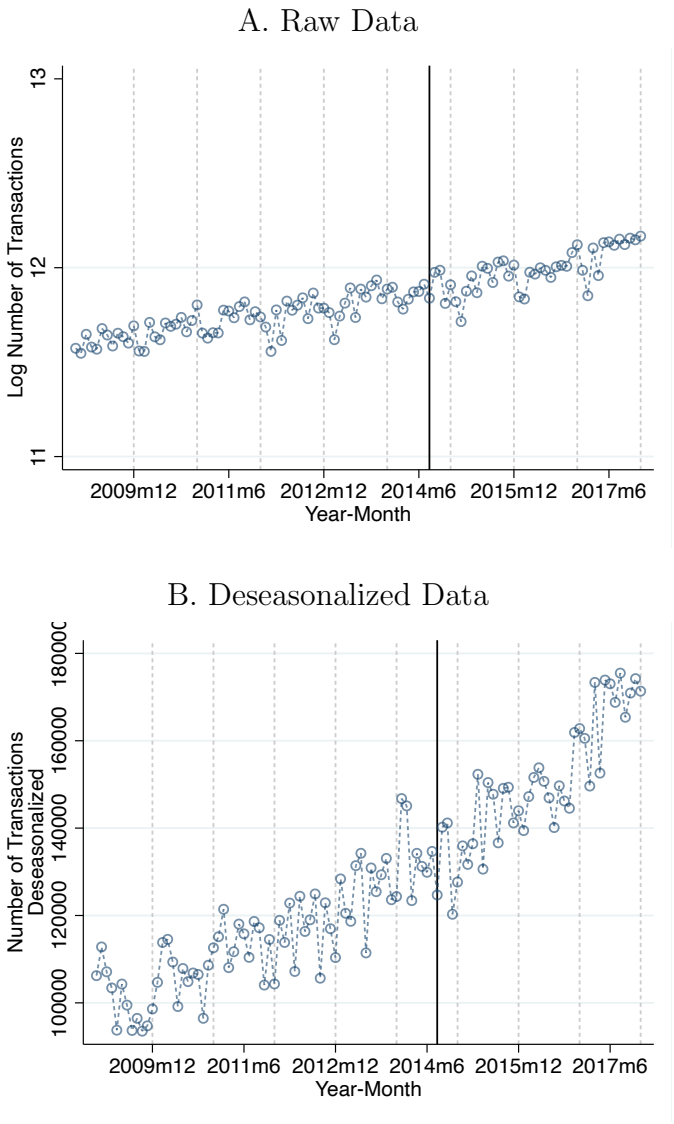
Notes: This Figure is similar to Figure 2, except that, when de-seasonalizing the data and estimating the RD and month-on-month growth rates, we include an additional term that allows for a trend break in January 2013, when the roll-out of the POS subsidies for firms began. This additional control does not substantially alter our results compared to our main specification. This figure is discussed in Section 4.3.

Figure B.7: Distribution of Placebo RD Estimates and Randomization Inference P-Values



Notes: This figure shows the distribution of estimates from placebo RD estimations, using equation 1 with optimal bandwidths as per Calonico et al. (2014) and pretending the reform happened in a month other than August 2014 (one estimation per month, using all months between January 2013 and December 2015). The vertical red line shows the estimate for August 2014. We report the point estimate and standard error on a t-test comparing the August 2014 estimate to the placebo estimates, and randomization inference p-values. This figure is discussed in section 4.3.

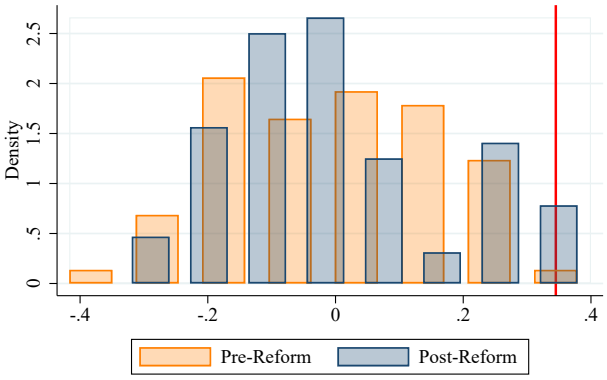
Figure B.8: Number of Electronic Payment Transactions in Argentina (Placebo)



Notes: This figure plots the log number of transactions with electronic payment technology in Argentina between 2009 and 2017. The data is obtained from the Central Bank of Argentina. Panel A plots the raw monthly aggregate values. Panel B plots the the de-seasonalized series after taking out month-of-year fixed effects, as per equation 1 (linear specification). The vertical line marks August 2014, when the VAT rebates in Uruguay entered into effect. This figure is discussed in Section 4.3.

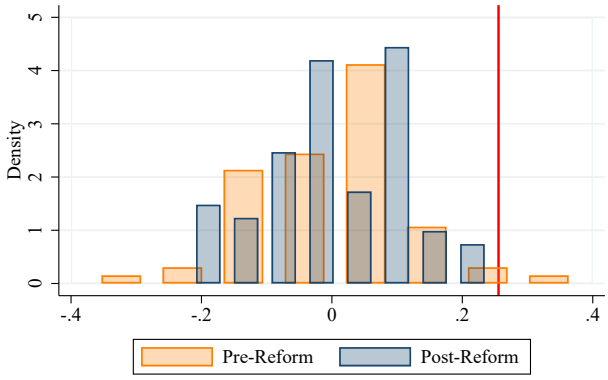
Figure B.9: Week-on-Week Growth Rates in Key Outcomes

A. Number of Card Transactions



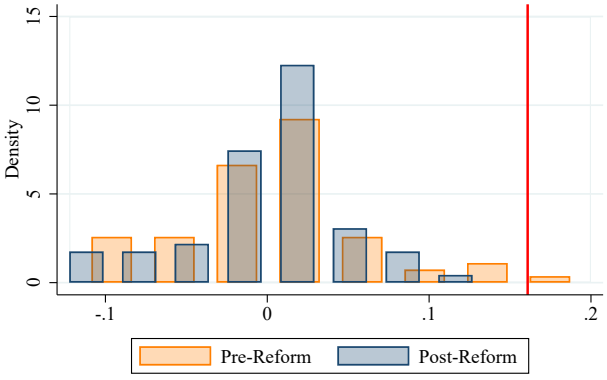
Mean Pre Reform (-0.002) vs Mean Post Reform (0.003): ttest p-value (0.868)
Week of Introduction (0.345) vs Mean All Other Weeks (0.001): ttest p-value (0.000)
Mean Pre Reform (-0.002) vs Week of Introduction (0.345): ttest p-value (0.000)

B. Volume of Card Transactions



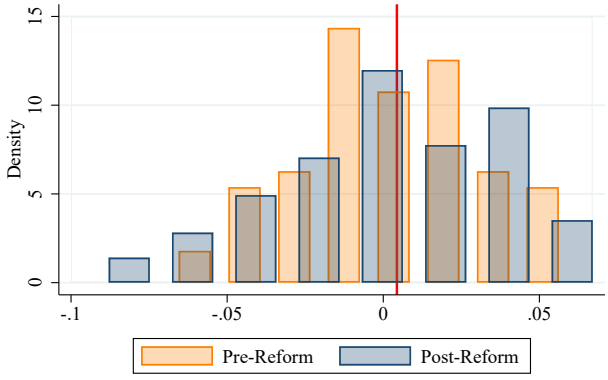
Mean Pre Reform (0.001) vs Mean Post Reform (0.007): ttest p-value (0.746)
Week of Introduction (0.256) vs Mean All Other Weeks (0.004): ttest p-value (0.000)
Mean Pre Reform (0.001) vs Week of Introduction (0.256): ttest p-value (0.000)

C. Number of POS



Mean Pre Reform (0.002) vs Mean Post Reform (0.002): ttest p-value (0.994)
Week of Introduction (0.161) vs Mean All Other Weeks (0.002): ttest p-value (0.000)
Mean Pre Reform (0.002) vs Week of Introduction (0.161): ttest p-value (0.000)

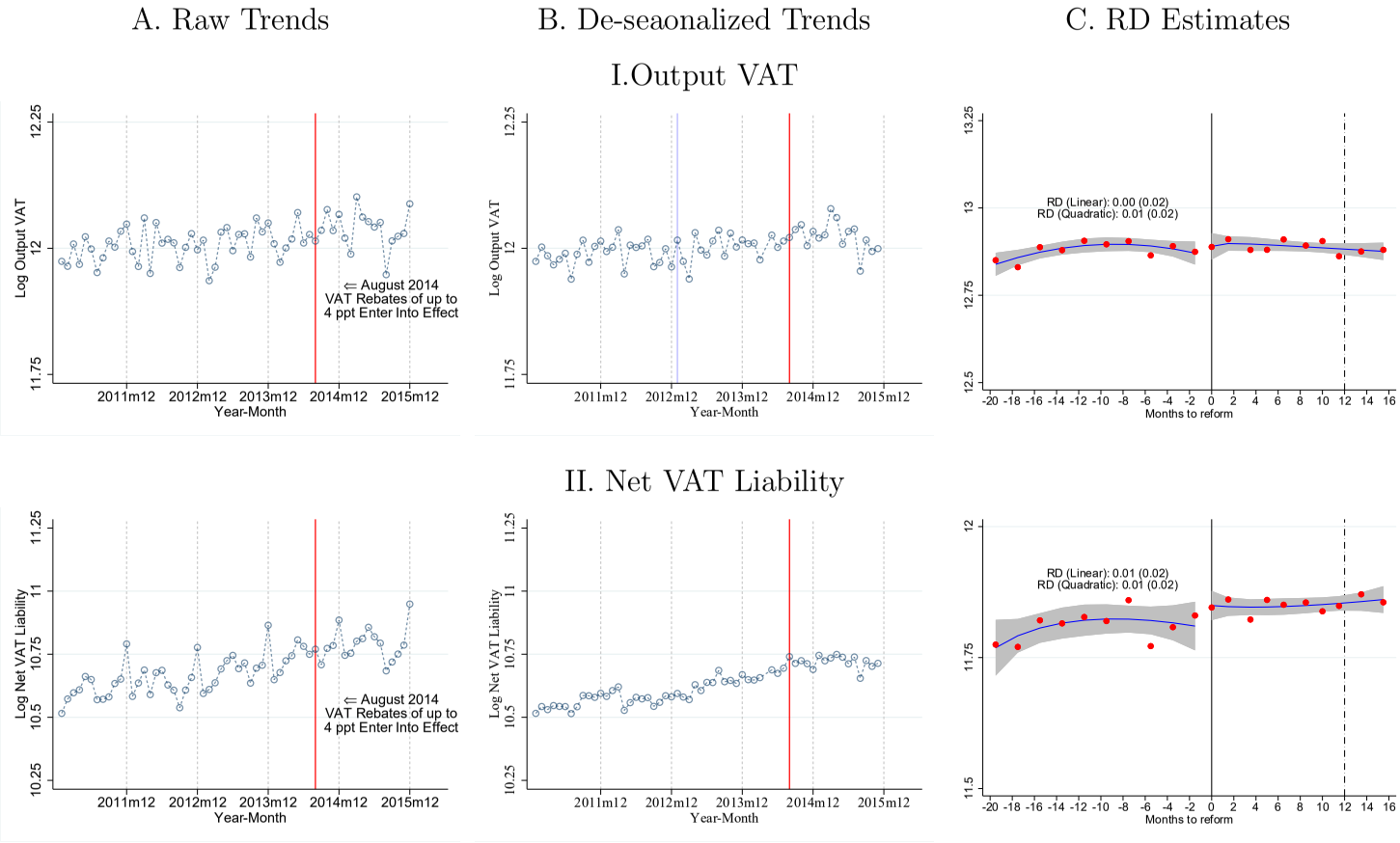
D. Number of Firms with a POS



Mean Pre Reform (0.001) vs Mean Post Reform (0.003): ttest p-value (0.736)
Week of Introduction (0.004) vs Mean All Other Weeks (0.002): ttest p-value (0.315)
Mean Pre Reform (0.001) vs Week of Introduction (0.004): ttest p-value (0.285)

Notes: This figures is similar to Figure 2, but plots the distribution of weekly instead of monthly growth rates. This figure is mentioned in Section 4.2.

Figure B.10: The Effect of VAT Rebates on VAT Compliance
Raw Data, De-seaonalized Data and Regression Discontinuity Estimates for Monthly Aggregate VAT Outcomes



Notes: This figure examines the effect of the VAT rebate introduction on aggregate reported output VAT and net VAT liability. Note that the rebates are disbursed directly to consumers, with no change to how firms file their VAT declaration. The rebates should therefore affect VAT liability only through a compliance channel. Panel A plots the monthly aggregate values for each of the outcomes. Panel B plots the de-seasonalized trends of monthly outcomes as per equation 1 (with $p = 1$, i.e. a linear time trend). Panel C implements the RD estimation similar to equation 1 but using monthly aggregated data, and month to reform as a running variable. This Figure is discussed in Section 5.

Table B.1: Robustness of RD Estimates to Varying the Level of Aggregation of Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
A: Log Total Number of Transactions						
Point Estimate	0.518	0.497	0.524	0.515	0.499	0.440
SE	(0.043)	(0.062)	(0.035)	(0.041)	(0.044)	(0.065)
B: Log Volume of Card Transactions						
Point Estimate	0.285	0.294	0.268	0.300	0.283	0.238
SE	(0.023)	(0.030)	(0.045)	(0.049)	(0.037)	(0.053)
Aggregation	Weekly	Weekly	Bi-weekly	Bi-weekly	Daily	Daily
Model Fit	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Notes: This table shows the robustness of our main RD estimates to different ways of aggregating the outcome data. The table displays the estimate γ_0 from equation 1 for an RD in time around August 2014. Columns 1 and 2 reproduce estimates from our preferred specification, using weekly aggregation, as shown in Figure 2. Results for data aggregated at the bi-weekly and daily level are shown in columns 3-4 and 5-6 respectively. This table is discussed in Section 4.3.

Table B.2: Robustness of RD Estimates to Short-run Selection — Donut RD

	(1)	(2)	(3)	(4)
	All Weeks	Cut 2 Weeks	Cut 4 Weeks	Cut 8 Weeks
	I. Number of Card Transactions			
i. 80 Weeks	0.52	0.56	0.54	0.57
	(0.043)	(0.049)	(0.051)	(0.054)
ii. 40 Weeks	0.47	0.56	0.49	0.62
	(0.060)	(0.077)	(0.083)	(0.086)
iii. Optimal	0.44	0.47	0.37	0.11
	(0.071)	(0.098)	(0.189)	(0.302)
	II. Volume of Card Transactions			
i. 80 Weeks	0.28	0.30	0.31	0.33
	(0.023)	(0.027)	(0.029)	(0.036)
ii. 40 Weeks	0.25	0.25	0.28	0.38
	(0.028)	(0.038)	(0.039)	(0.059)
iii. Optimal	0.24	0.19	0.28	-0.43
	(0.039)	(0.094)	(0.097)	(0.250)

Notes: This table displays the results of "donut RD" estimations that account for potential selection into treatment (in our case: retiming of purchases), as suggested by Hausman and Rapson (2018). The table shows treatment effect estimates for our two main outcomes, the number of card transactions (Panel I) and the volume of card transactions (Panel II) using either an 80-week or a 40-week bandwidth or the optimal bandwidths for each outcome as per Calonico et al. (2014). Column 1 displays our baseline estimates from equation 1 (linear specification). In columns 2-4, we exclude from the estimation 2, 4 or 8 weeks, both before and after the reform (in addition to the reform week itself). Note that the optimal bandwidth for the number (volume) of card transactions is estimated to be 17 (15). This table is discussed in Section 4.3.

Table B.3: Comparison of One-Step and Two-Step RD Estimations

		A. One-Step Estimation	B. Two-Step Estimation
I. Number of Card Transactions			
Linear	80 Weeks BW	0.52	0.50
		(0.043)	(0.044)
	Optimal BW	0.44	0.42
		(0.071)	(0.081)
Quadratic	80 Weeks BW	0.50	0.48
		(0.062)	(0.063)
	Optimal BW	0.33	0.38
		(0.083)	(0.084)
II. Volume of Card Transactions			
Linear	80 Weeks BW	0.28	0.28
		(0.023)	(0.024)
	Optimal BW	0.24	0.27
		(0.039)	(0.039)
Quadratic	80 Weeks BW	0.29	0.30
		(0.030)	(0.030)
	Optimal BW	0.29	0.32
		(0.047)	(0.047)

Notes: Column A displays our main (benchmark) RD estimates obtained from equation 1. Column B displays estimates from a two-step procedure. 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 de-seasonalized outcomes $log(\tilde{Z}_t) = log(Z_{t,m}) - \hat{g}_m$ and estimate the regression discontinuity with a shorter data set (bandwidth) around the reform. In this second step, we estimate equation 1 without the month-of-year fixed effects g_m and use the de-seasonalized outcomes as dependent variable. The standard errors from this procedure would need to be adjusted for the fact that we use a predicted outcome in the second-stage estimation. For both methods (columns), the table displays the estimates for our preferred specification using an 80-week bandwidth and for the optimal bandwidth as in Calonico et al. (2014) and shown in Figure B.5. This table is discussed in Section 4.3.

Table B.4: Robustness of RD Estimates to Accounting for Autocorrelation - First Lag

	Preferred Specification		Control: Lag 1	
	(1)	(2)	(3)	(4)
A: Bi-weekly Specification				
Number of Card Transactions	0.411	0.304	0.411	0.354
	(0.311)	(0.193)	(0.311)	(0.218)
Volume of Card Transactions	0.180	0.250	0.189	0.259
	(0.081)	(0.122)	(0.086)	(0.124)
Number POS	-0.057	-0.079	-0.060	-0.080
	(0.053)	(0.084)	(0.056)	(0.084)
Number of Firms with a POS	0.009	0.003	0.008	0.004
	(0.024)	(0.034)	(0.026)	(0.034)
B: Weekly Specification				
Number of Card Transactions	0.430	0.328	0.486	0.285
	(0.069)	(0.083)	(0.068)	(0.086)
Volume of Card Transactions	0.239	0.286	0.292	0.283
	(0.039)	(0.047)	(0.035)	(0.048)
Number POS	0.078	0.075	0.042	0.047
	(0.016)	(0.021)	(0.019)	(0.024)
Number of Firms with a POS	0.023	0.019	0.018	0.014
	(0.009)	(0.011)	(0.011)	(0.013)
C: Daily Specification				
Number of Card Transactions	0.361	0.316	0.401	0.330
	(0.091)	(0.105)	(0.087)	(0.103)
Volume of Card Transactions	0.144	0.206	0.178	0.251
	(0.071)	(0.097)	(0.061)	(0.085)
Number POS	0.016	0.023	0.027	0.032
	(0.066)	(0.076)	(0.051)	(0.061)
Number of Firms with a POS	0.012	0.010	0.021	0.017
	(0.069)	(0.080)	(0.055)	(0.065)
Model Fit	Linear	Quadratic	Linear	Quadratic

Notes: This table demonstrates the robustness of our results to controlling for the lagged dependent variable. In columns 1-2 we reproduce our main RD estimates using the optimal bandwidth as per Calonico et al. (2014) and showing results for different ways of aggregating the dependent variable, as per the panel titles. Column 1 is for the linear fit and column 2 for the quadratic fit. In columns 3-4, we control for the first lag of the dependent variable in the estimation. This table is discussed in Section 4.3.

Table B.5: Robustness of RD Estimates to Accounting for Autocorrelation - First Two Lags

	Preferred Specification		Control: 2 Lags	
	(1)	(2)	(3)	(4)
A: Bi-weekly Specification				
Number of Card Transactions	0.411	0.304	0.409	0.359
	(0.311)	(0.193)	(0.317)	(0.207)
Volume of Card Transactions	0.180	0.250	0.201	0.249
	(0.081)	(0.122)	(0.076)	(0.098)
Number POS	-0.057	-0.079	-0.018	-0.045
	(0.053)	(0.084)	(0.030)	(0.053)
Number of Firms with a POS	0.009	0.003	0.039	0.026
	(0.024)	(0.034)	(0.017)	(0.025)
B: Weekly Specification				
Number of Card Transactions	0.430	0.328	0.490	0.317
	(0.069)	(0.083)	(0.068)	(0.083)
Volume of Card Transactions	0.239	0.286	0.289	0.277
	(0.039)	(0.047)	(0.034)	(0.045)
Number POS	0.078	0.075	0.047	0.047
	(0.016)	(0.021)	(0.020)	(0.026)
Number of Firms with a POS	0.023	0.019	0.018	0.013
	(0.009)	(0.011)	(0.010)	(0.014)
C: Daily Specification				
Number of Card Transactions	0.361	0.316	0.431	0.338
	(0.091)	(0.105)	(0.083)	(0.101)
Volume of Card Transactions	0.144	0.206	0.195	0.272
	(0.071)	(0.097)	(0.058)	(0.080)
Number POS	0.016	0.023	0.040	0.040
	(0.066)	(0.076)	(0.042)	(0.052)
Number of Firms with a POS	0.012	0.010	0.036	0.029
	(0.069)	(0.080)	(0.044)	(0.053)
Model Fit	Linear	Quadratic	Linear	Quadratic

Notes: This table is identical to Table B.4, but controls for the first two lags of the dependent variable in columns 3 and 4. This table is discussed in Section 4.3.

Table B.6: Robustness of RD Estimates to Prais-Winsten Correction for Autocorrelated Errors

	Prefered Specification		Prais-Winsten Adjustment	
	(1)	(2)	(3)	(4)
A: Bi-weekly Specification				
Number of Card Transactions	0.411	0.304	0.453	0.311
	(0.311)	(0.193)	(0.041)	(0.086)
Volume of Card Transactions	0.180	0.250	0.212	0.388
	(0.081)	(0.122)	(0.057)	(0.128)
Number POS	-0.057	-0.079	-0.099	-0.112
	(0.053)	(0.084)	(0.049)	(0.088)
Number of Firms with a POS	0.009	0.003	0.015	0.006
	(0.024)	(0.034)	(0.025)	(0.036)
B: Weekly Specification				
Number of Card Transactions	0.430	0.328	0.385	0.377
	(0.069)	(0.083)	(0.074)	(0.079)
Volume of Card Transactions	0.239	0.286	0.231	0.356
	(0.039)	(0.047)	(0.056)	(0.090)
Number POS	0.078	0.075	0.068	0.081
	(0.016)	(0.021)	(0.021)	(0.032)
Number of Firms with a POS	0.023	0.019	0.009	0.012
	(0.009)	(0.011)	(0.017)	(0.017)
C: Daily Specification				
Number of Card Transactions	0.361	0.316	0.350	0.376
	(0.091)	(0.105)	(0.079)	(0.085)
Volume of Card Transactions	0.144	0.206	0.104	0.166
	(0.071)	(0.097)	(0.069)	(0.090)
Number POS	0.016	0.023	0.030	0.067
	(0.066)	(0.076)	(0.075)	(0.075)
Number of Firms with a POS	0.012	0.010	0.050	0.037
	(0.069)	(0.080)	(0.077)	(0.079)
Model Fit	Linear	Quadratic	Linear	Quadratic

Notes: This table is similar to Table B.4, but shows in columns 3 and 4 the robustness of our results to controlling for autocorrelation in the error term via the Prais and Winsten (1954) procedure. For details, see Judge et al. (1985) and Davidson and MacKinnon (1993). This table is discussed in Section 4.3.

B.2 Exploiting Variation in Rebate Rates Across Firms

Figure A.2 shows how rebate rates vary by payment card type and transaction amount. In this section, we exploit this variation in heterogeneity analyses. The hypothesis is that higher rebate rates may generate a larger consumer response and potentially a tax compliance impact. In what follows, we first explain how we calculate the rebate rate for each transaction. We then calculate the average rebate rate for each firm, and divide the sample into firms with high vs low rebate rates. We then conduct RD estimations for each subsample.[33]

As a first step, we calculate the rebate rate on each transaction as (rebate amount/VAT inclusive transaction amount)*122, i.e. assuming that the VAT rate is 22 percent. Figure B.11, Panel A, shows the distribution of estimated rebate rates for August 2014 with this method. The figure suggests that our implicit assumption on the VAT rate is correct for most transactions. We then round the estimated rebate rate to obtain rates that correspond to the statutory rebate rates. This rounding also ensures that we do not overestimate rebate rates for transactions taxed at 10 percent. As Panel B shows, most transactions obtain a 2 ppt rebate, 30 percent receive no rebate (i.e. are firm-to-firm transactions) and 15 percent of transactions obtain a rebate of 4 ppt or higher.[34] Given that few transactions are above the threshold value of 4,000 UI where the rebate rate drops (see Figure A.3), the variation in rebate rates is primarily driven by the type of payment card used, with most transactions conducted by credit card.

We then calculate the average rebate rate at the firm level, taking a simple average over the firms' card transactions. The resulting distribution is displayed in Panel C. The distribution features a mass point at zero, indicating that over 40 percent of firms register no rebates,[35] while the other firms provide rebates on part of their transactions, with a majority of firms providing the 2 ppt rebate on a large share but not on all transactions.

We can now divide the sample into firms that provide a higher vs a lower rebate rate on average.[36] Low-rebate firms sell a larger fraction of their output to other firms and/or to consumers using a credit card. Figure B.12 shows RD estimations of the reform impact on

[33] A caveat is that we have to use post-reform data to estimate rebate rates, as the type of payment method (credit or debit card) was not captured before the reform. The post-reform distribution of transactions (and rebates) is of course endogenous to consumer responses to the rebates.

[34] Recall that transactions at hotels or restaurants — some of which might be misclassified as retail — receive a 9 ppt rebate and transactions with a BPS social security card receive a full VAT waiver, i.e. et 10 or 22 ppt rebate.

[35] Recall that purchases by firms, purchases with foreign payment cards and credit card purchases of a value above 4,000 UI are not eligible for any VAT rebates.

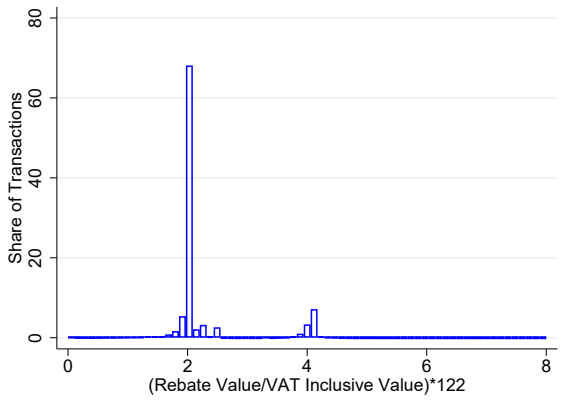
[36] When constructing this sample split, we ignore transactions with rebate rates above 5 ppt, as these rebates existed prior to the reform we study and did not vary with the reform.

the number and volume of card transactions, splitting the sample either by the mean or the median of the distribution of firm-level average rebate rates. We observe a significant increase in card transactions in all samples. The increase in the transaction volume is larger among firms with a below-average or below-median rebate rate. Similarly, the increase in the number of card transactions is larger among firms with a below-average or below-median rebate rate. The results are similar when we use a weighted average to construct the firm-level average rebate rates based on which we divide the sample. A possible explanation for the results is that firms providing lower average rebate rates serve customers who did not have the habit of using their payment cards prior to the reform and hence had more scope for increasing the use of this technology. The results are also consistent with the idea that consumers increased their use of electronic payment technologies overall, without necessarily targeting this behavioral change to specific retailers/transactions that provided high(er) rebate rates.

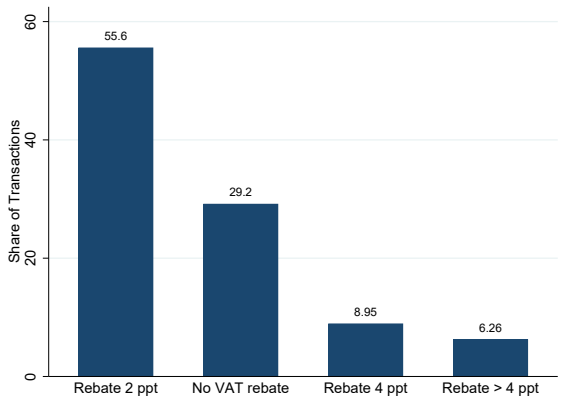
If a larger increase in (the volume of) card transactions was associated with a larger increase in tax compliance, we should observe this by comparing low-rebate retailers (treated) to high-rebate retailers (control) in a difference-in-difference analysis. Figures B.13 and B.14 show that there is no indication that an increase in tax compliance materialized. While the standard difference-in-difference estimation suggests that the treatment and control group have slightly different trends prior to the reform (columns A. and B.), re-weighting the control group in the synthetic difference-in-difference estimation achieves parallel trends and suggests precisely estimated zero effects on the outcomes of interest (column C.).

Figure B.11: Distribution of the Rebate Rates, September 2014

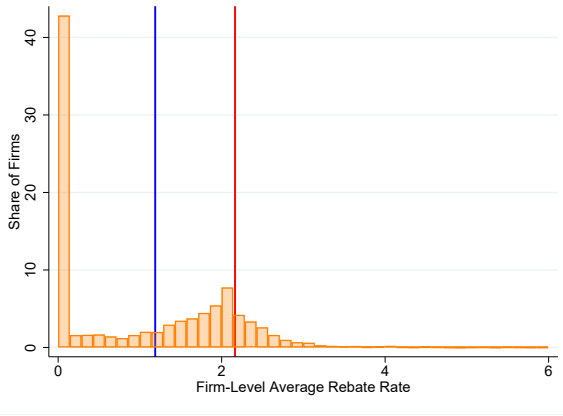
A: Distribution of Estimated Rebate Rates



B: Distribution of Rebate Rates



C: Distribution of Firm-Level Average Rebate Rate

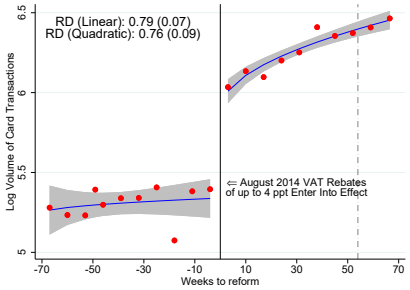


Notes: Panel A shows the distribution of rebate values as a share of the VAT-inclusive purchase price. We include all transactions with non-zero rebate value, for all firms, in September 2014. The results are very similar for August or October 2014. Panel B shows the distribution of the rounded rebate rates for September 2014. Panel C shows the distribution of the average rebate rate at the firm level. We take a simple average across all transactions of each firm in September 2014. The distribution is very similar when using the transaction amount as weight when averaging. The blue and red vertical lines indicate the median and the mean of the distribution. Panel C is for all firms with a POS, while panel B is for all card transactions.

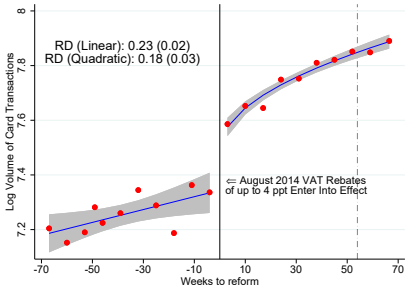
Figure B.12: The Effect of VAT Rebates on the Use of Electronic Payment Technology
Heterogeneity of RD Estimates by Firm-Level Average Rebate Rate

A. Volume of Card Transactions

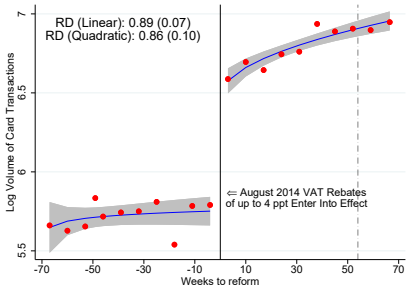
1. Firm Rebate < Mean



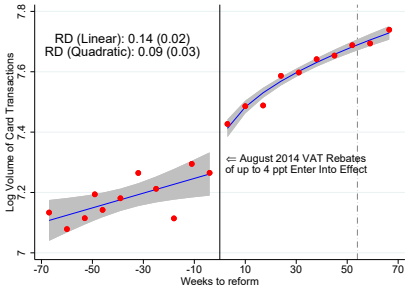
2. Firm Rebate ≥ Mean



3. Firm Rebate < Median

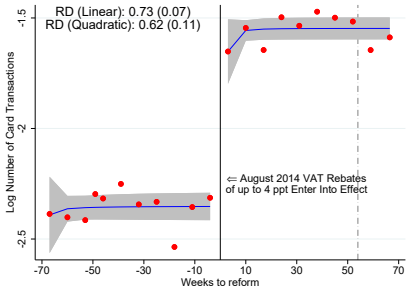


4. Firm Rebate ≥ Median

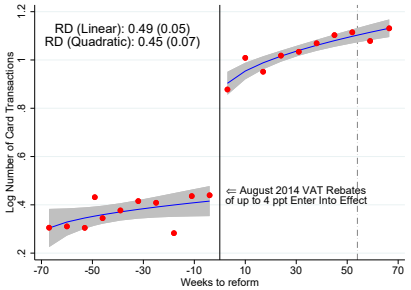


B. Number of Card Transactions

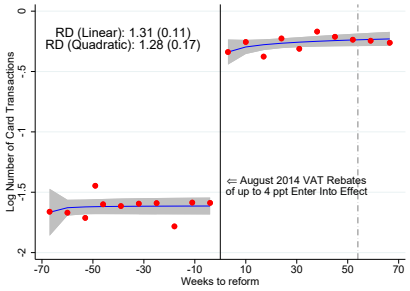
1. Firm Rebate < Mean



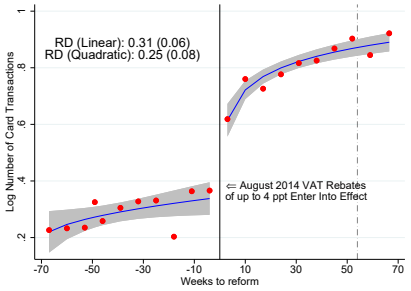
2. Firm Rebate ≥ Mean



3. Firm Rebate < Median



4. Firm Rebate ≥ Median

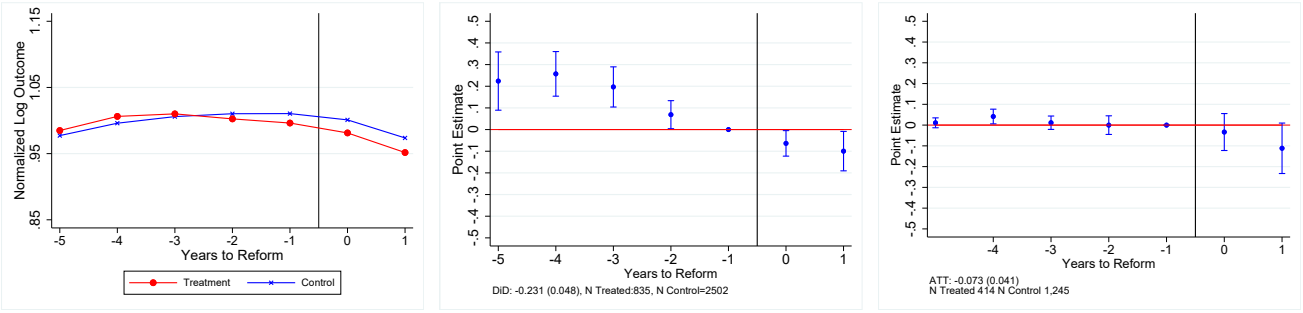


Notes: This figure shows the response of the volume and number of card transactions to the introduction of VAT rebates, studying heterogeneity by the firm-level average rebate rate, calculated in September 2014. We limit the analysis to firms with card transactions and divide the sample by the mean/median of the distribution of firm-leverage average rebate rates in this sample. Everything else is as in Figure 2.

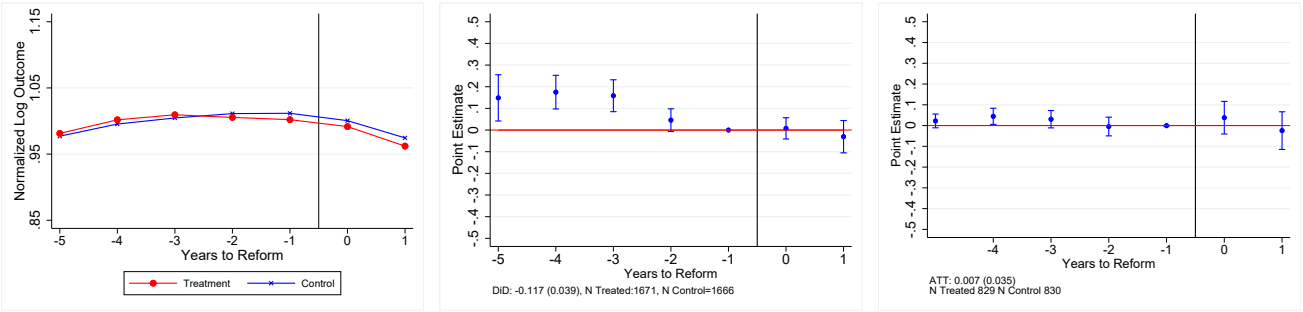
Figure B.13: The Effect of VAT Rebates on Tax Compliance
DiD Estimations Exploiting Variation in Firm-Level Average Rebate Rate (1/2)

A. Trends B. Standard DiD C. Synthetic DiD

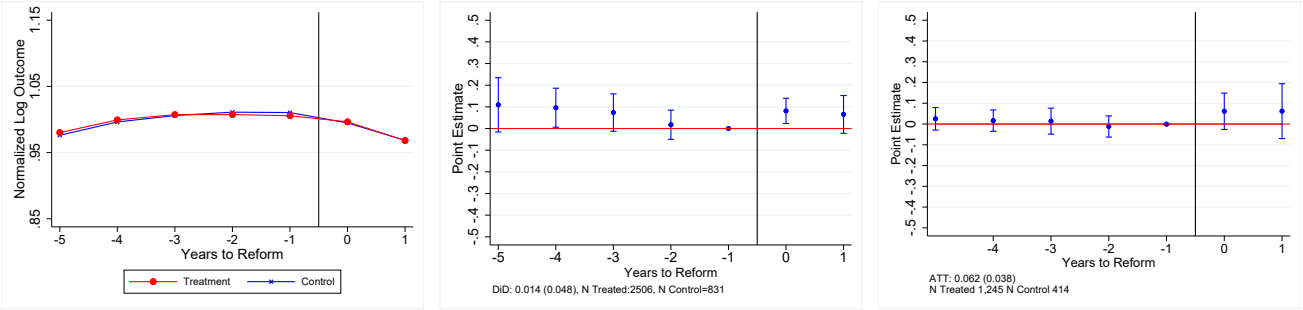
I. Treated: Firm-level Average Rebate Rate < p25



II. Treated: Firm-level Average Rebate Rate < p50



III. Treated: Firm-level Average Rebate Rate < p75

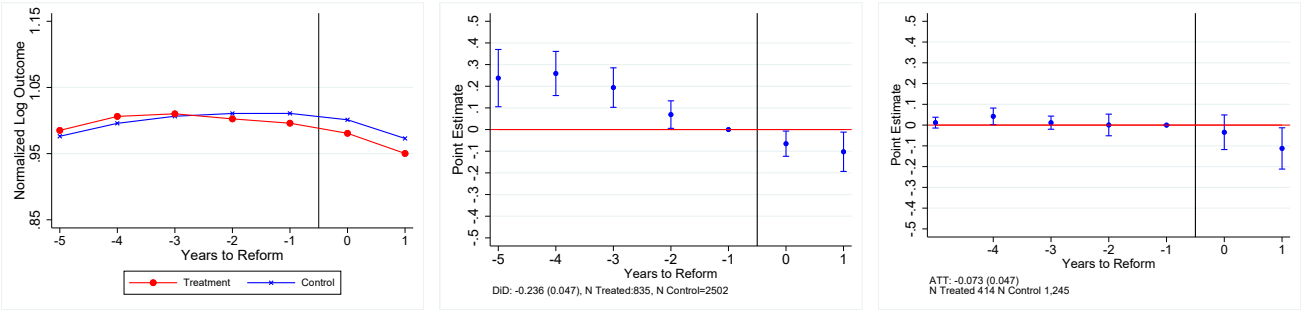


Notes: This figure shows difference-in-difference estimations for retail firms, comparing firms with low firm-level average rebate rates (treated) to firms with high firm-level average rebate rates (control), ignoring firms that register no rebates. The outcome variable is taxable sales. The low/high division is as per the panel titles. The designation of firms with relatively lower rebate rates as treated is motivated by Figure B.12 which shows that the post-reform jump in the volume and number of card transactions is larger among firms with lower average rebate rates. The specifications are otherwise the same as in Figure 3. Column A. shows time trends in the treatment and control group. Column B. shows event-study coefficients from a standard difference-in-difference estimation. Panel C. shows event-study coefficients from a synthetic difference-in-difference estimation.

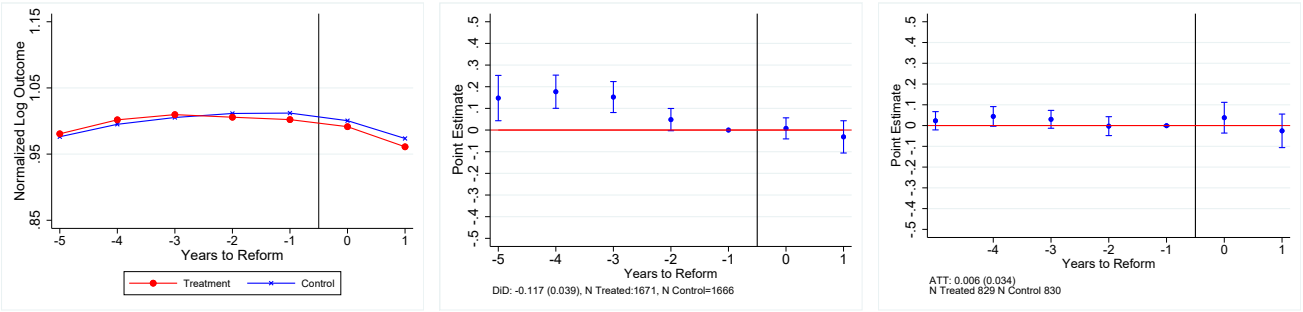
Figure B.14: The Effect of VAT Rebates on Tax Compliance
DiD Estimations Exploiting Variation in Firm-Level Average Rebate Rate (2/2)

A. Trends B. Standard DiD C. Synthetic DiD

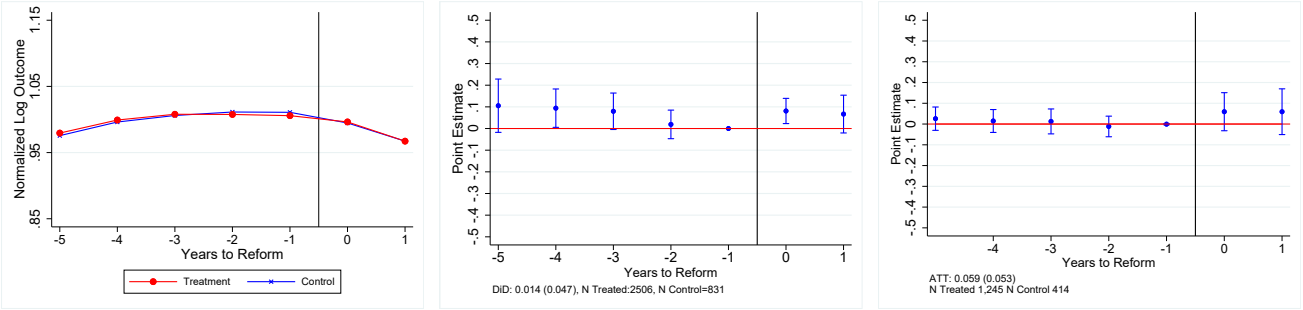
I. Treated: Firm-level Average Rebate Rate < p25



II. Treated: Firm-level Average Rebate Rate < p50



III. Treated: Firm-level Average Rebate Rate < p75



Notes: This figure is identical to figure B.13, except that the outcome variable here is output VAT.

C Difference-in-Difference Appendix

C.1 Dealing with Zeros in the Outcome Variables

Table C.1: Robustness of Difference-in-Difference Estimates to Varying the Value Attributed to Extensive Margin Changes

	Taxable Sales				Output VAT				Net Liability			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post · Treated	-0.0505	-0.0504	-0.0505	-0.0521	-0.0459	-0.0459	-0.0460	-0.0471	-0.0200	-0.0200	-0.0200	-0.0200
	(0.0480)	(0.0478)	(0.0483)	(0.0557)	(0.0395)	(0.0392)	(0.0397)	(0.0470)	(0.0643)	(0.0640)	(0.0650)	(0.0780)
ϵ	0.1	0	0.2	3	0.1	0	0.2	3	0.1	0	0.2	3
N Treated (Retailers w/ POS)	4985	4985	4985	4985	4985	4985	4985	4985	2497	2497	2497	2497
N Control (Wholesalers)	6118	6118	6118	6118	6118	6118	6118	6118	3017	3017	3017	3017

Notes: This table documents the robustness of our main DiD results shown in Table 1 to varying the value ϵ we attribute to extensive margin changes of the outcomes. Columns 1, 5 and 9 show our preferred estimates from columns 1, 5, and 9 of Table 1. The other columns vary ϵ to 0, 0.2 and 3, as indicated. Everything else is as in Table Table 1 and Figure 3. This table is discussed in Section 5.3.

Table C.2: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates for the Extensive Margin

	Taxable Sales				Output VAT				Net Liability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post · Retailer	-0.056	-0.056	-0.266	0.124	-0.039	-0.039	-0.257	0.142	-0.027	-0.027	0.683
	(0.284)	(0.284)	(0.276)	(0.264)	(0.283)	(0.283)	(0.276)	(0.263)	(0.518)	(0.518)	(0.661)
Balanced Sample	Y	Y	-	-	Y	Y	-	-	Y	Y	Y
Unbalanced Sample	-	-	Y	Y	-	-	Y	Y	-	-	-
Winsor at p99	Y	-	Y	Y	Y	-	Y	Y	Y	-	Y
Winsor at p95	-	Y	-	-	-	Y	-	-	-	Y	-
Includes 2016 data	-	-	-	Y	-	-	-	Y	-	-	Y
N Treated (Retailers)	4985	4985	6906	6819	4985	4985	6906	6819	2497	2497	1168
N Control (Wholesalers)	6118	6118	9044	9340	6118	6118	9044	9340	3017	3017	1812

Notes: This table is identical to Table 1, except that the outcome variable here is a dummy taking value 1 if the outcome is positive, and value 0 otherwise. This table is discussed in Section 5.3.

Table C.3: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates for the Intensive Margin

	Taxable Sales				Output VAT				Net Liability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post · Retailer	-0.004	0.002	0.009	0.003	-0.003	0.002	0.009	0.002	-0.023	-0.018	-0.085
	(0.019)	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)	(0.031)	(0.030)	(0.040)
Balanced Sample	Y	Y	-	-	Y	Y	-	-	Y	Y	Y
Unbalanced Sample	-	-	Y	Y	-	-	Y	Y	-	-	-
Winsor at p99	Y	-	Y	Y	Y	-	Y	Y	Y	-	Y
Winsor at p95	-	Y	-	-	-	Y	-	-	-	Y	-
Includes 2016 data	-	-	-	Y	-	-	-	Y	-	-	Y
N Treated (Retailers)	4763	4763	6800	6711	4765	4765	6801	6712	1959	1959	956
N Control (Wholesalers)	4694	4694	7316	7451	4696	4696	7316	7450	1793	1793	1120

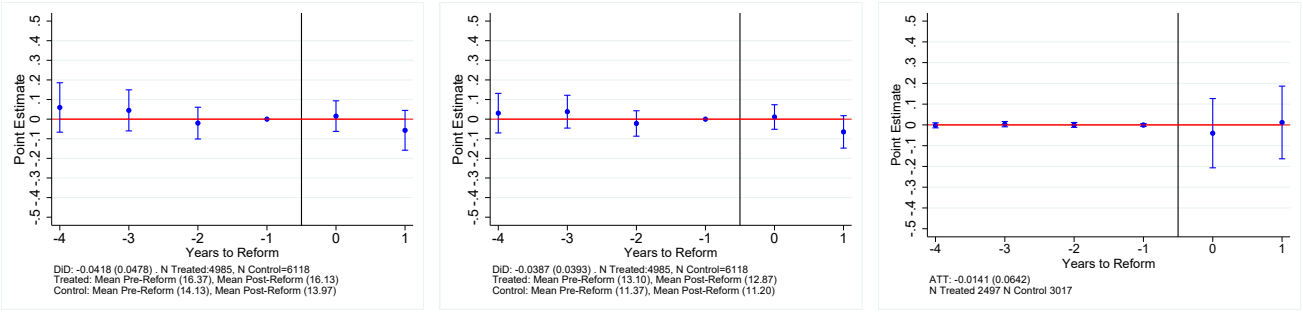
Notes: This table is identical to Table 1, except that we restrict the sample to a balanced panel of firms that report a non-zero outcome in each year during the period of analysis, 2010-2015. This table is discussed in Section 5.3.

C.2 Robustness Tests

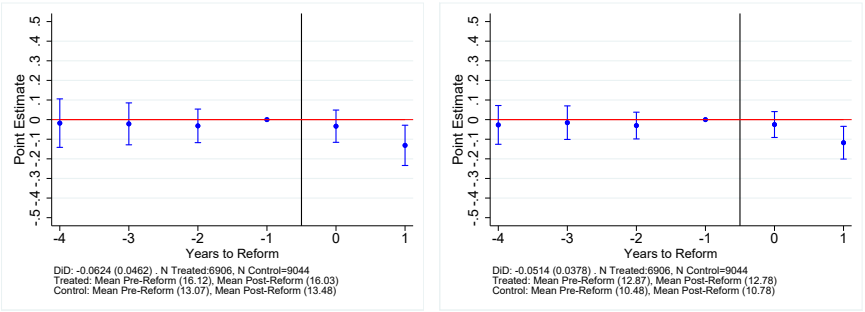
Figure C.1: Robustness of Difference-in-Difference Estimations to Alternative Specifications

A. Total Taxable Sales | B. Output VAT | C. Net VAT

I. Balanced sample, winsorizing at p95, excludes 2016 data

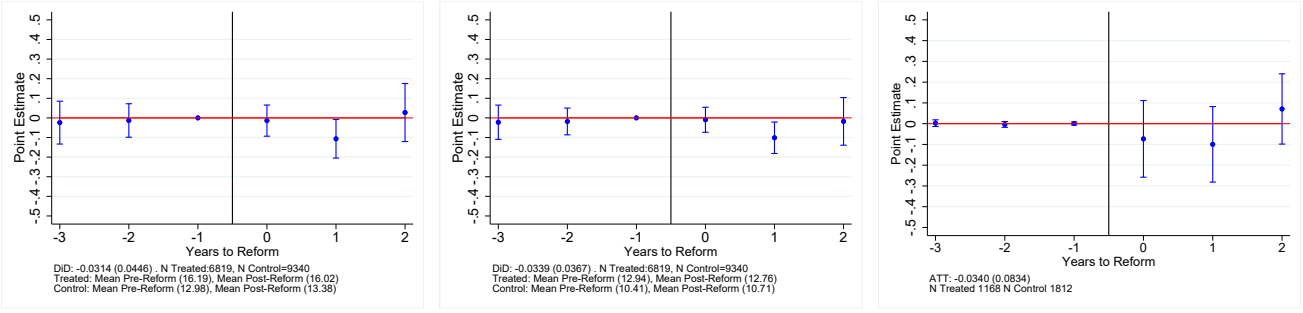


II. Unbalanced sample, winsorizing at p99, excludes 2016 data



III. Including 2016 data, winsorizing at p99

Unbalanced | Unbalanced | Balanced



Notes: This figure is similar to Figure 3. It provides the graphical representation of the robustness tests presented in Table 1 and discussed in Section 5.4.

Table C.4: Robustness of Difference-in-Difference Estimates to Controlling for Differential Trends and Varying Balancing of Panel - Annual Data

(a) Annually Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	-0.051	-0.056	-0.069	-0.046	-0.047	-0.068
	(0.048)	(0.050)	(0.050)	(0.040)	(0.041)	(0.041)
Incorporation Year*Year	Y	Y	Y	Y	Y	Y
Region*Year	-	Y	Y	-	Y	Y
Firm Size Decile*Year	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	4985	4985	4985	4985	4985	4985
N Control (Wholesalers)	6118	6118	6118	6118	6118	6118

(b) Quarterly Balanced Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	0.010	0.020	-0.099	0.013	0.023	-0.097
	(0.028)	(0.029)	(0.027)	(0.028)	(0.029)	(0.027)
Incorporation Year*Year	Y	Y	Y	Y	Y	Y
Region*Year	-	Y	Y	-	Y	Y
Firm Size Decile*Year	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	4329	4329	4329	4329	4329	4329
N Control (Wholesalers)	4353	4353	4353	4353	4353	4353

(c) Unbalanced Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	-0.069	-0.086	-0.042	-0.059	-0.070	-0.046
	(0.046)	(0.049)	(0.049)	(0.038)	(0.040)	(0.040)
Incorporation Year*Year	Y	Y	Y	Y	Y	Y
Region*Year	-	Y	Y	-	Y	Y
Firm Size Decile*Year	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	6906	6906	6906	6906	6906	6906
N Control (Wholesalers)	9044	9044	8964	9044	9044	8964

Notes: This table examines the robustness of the DiD estimates from equation 2. We start with the baseline specification from column (1) in Table 1 and then vary the fixed effects we control for, as explained in the row titles, and the data we use, as explained in the panel titles. The firm-size deciles are constructed using the average annual sales during the pre-reform period. Outcome variables are winsorized at the 99th percentile. We focus on total taxable sales and output VAT as key outcomes for this table, as we used the synthetic difference-in-difference estimation for the net liability outcome, which makes the addition of more flexible fixed effects redundant. Standard errors are robust to heteroskedasticity and clustered at the firm level. This table is discussed in Section 5.4.

Table C.5: Robustness of Difference-in-Difference Estimates to Controlling for Differential Trends and Varying Balancing of Panel - Monthly Data

(a) Annually Balanced Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	-0.023	-0.011	-0.020	-0.023	-0.010	-0.019
	(0.014)	(0.015)	(0.016)	(0.014)	(0.015)	(0.015)
N Treated (Retailers w/ POS)	6203	6203	6203	6203	6203	6203
N Control (Wholesalers)	7278	7278	7278	7278	7278	7278
Incorporation Year*Month FE	Y	Y	Y	Y	Y	Y
Region*Month FE	-	Y	Y	-	Y	Y
Large Firm*Month FE	-	-	Y	-	-	Y

(b) Quarterly Balanced Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	-0.010	0.004	0.002	-0.010	0.004	0.002
	(0.014)	(0.015)	(0.016)	(0.014)	(0.015)	(0.015)
N Treated (Retailers w/ POS)	5424	5424	5424	5424	5424	5424
N Control (Wholesalers)	5747	5747	5747	5747	5747	5747
Incorporation Year*Month FE	Y	Y	Y	Y	Y	Y
Region*Month FE	-	Y	Y	-	Y	Y
Large Firm*Month FE	-	-	Y	-	-	Y

(c) Unbalanced Sample

	Taxable Sales			Output VAT		
	(1)	(2)	(3)	(4)	(5)	(6)
Post · Treated	-0.027	-0.014	-0.024	-0.027	-0.014	-0.023
	(0.014)	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)
N Treated (Retailers w/ POS)	6809	6809	6809	6809	6809	6809
N Control (Wholesalers)	9414	9414	9414	9414	9414	9414
Incorporation Year*Month FE	Y	Y	Y	Y	Y	Y
Region*Month FE	-	Y	Y	-	Y	Y
Large Firm*Month FE	-	-	Y	-	-	Y

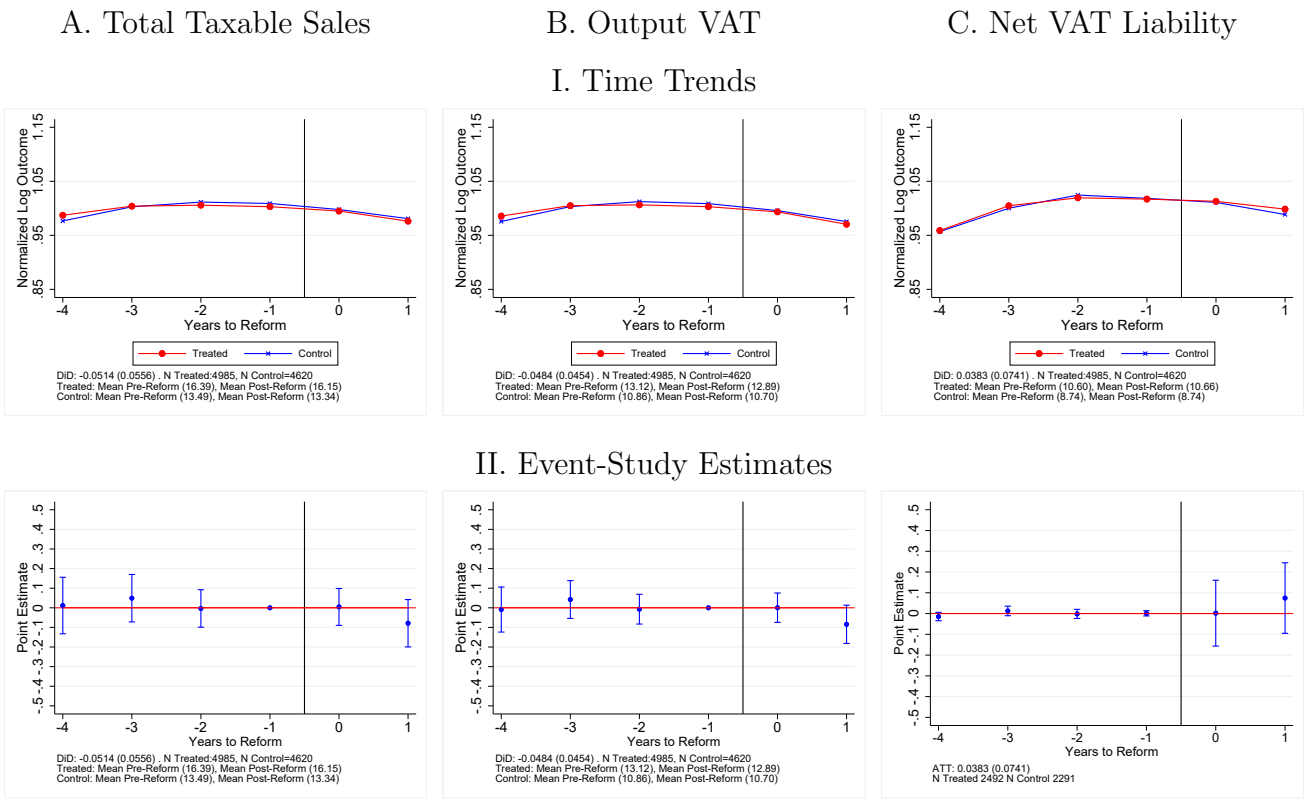
Notes: This table is similar to Table C.4 but uses monthly data for the period August 2013 to August 2015. This table is discussed in Section 5.4.

Table C.6: Robustness of Difference-in-Difference Estimations to Varying the Panel Length

	Taxable Sales			Output VAT			Net Liability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post · Treated	-0.051	-0.017	-0.051	-0.046	-0.019	-0.046	-0.020	0.085	0.038
	(0.048)	(0.050)	(0.045)	(0.040)	(0.041)	(0.037)	(0.064)	(0.061)	(0.077)
Balanced Sample	Y	Y	Y	Y	Y	Y	Y	Y	Y
Winsor at p99	Y	Y	Y	Y	Y	Y	Y	Y	Y
Start in 2010	Y	-	-	Y	-	-	Y	-	-
Start in 2009	-	Y	-	-	Y	-	-	Y	-
Start in 2011	-	-	Y	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	4985	4717	5241	4985	4717	5241	2497	2348	2607
N Control (Wholesalers)	6118	5629	6699	6118	5629	6699	3017	2792	3324

Notes: This table documents the robustness of our main DiD results shown in Table 1 to varying the length of the panel we use for estimation. Columns 1, 4 and 7 reproduce our preferred estimates from Figure 3. The remaining columns show estimates for a longer and shorter panel. Everything else is as in Table 1 and Figure 3. This table is discussed in Section 5.4.

Figure C.2: Robustness of Difference-in-Difference Estimates to Excluding Wholesale Firms With POS



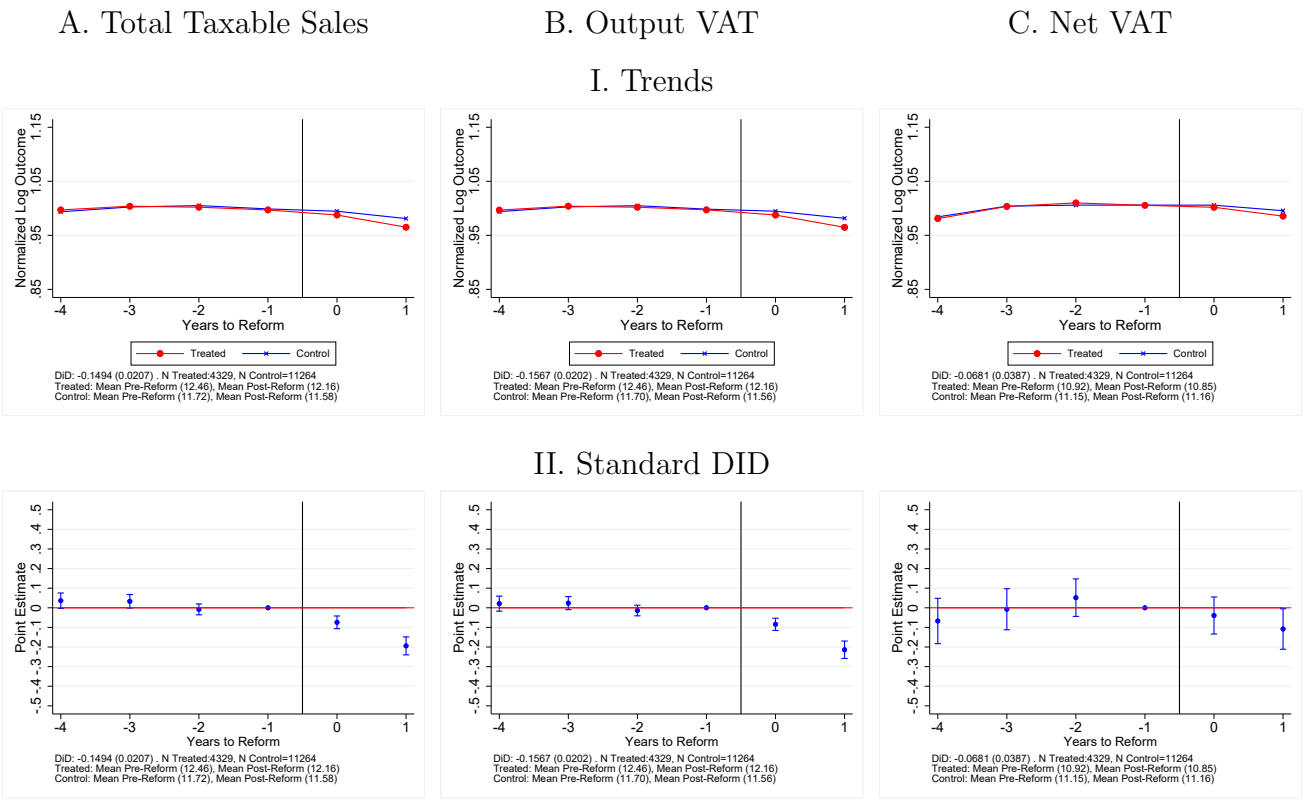
Notes: This Figure is identical to Figure 3, except that we exclude from the control group all wholesale firms that ever used a POS. This figure is discussed in Section 5.4.

Table C.7: Robustness of Difference-in-Difference Estimates to Excluding Wholesale Firms With POS

	Taxable Sales				Output VAT				Net Liability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post · Treated	-0.051	-0.046	-0.054	-0.008	-0.048	-0.043	-0.049	-0.017	0.038	0.039	0.006
	(0.056)	(0.055)	(0.053)	(0.051)	(0.045)	(0.045)	(0.043)	(0.042)	(0.074)	(0.074)	(0.095)
Balanced Sample	Y	Y	-	-	Y	Y	-	-	Y	Y	Y
Unbalanced Sample	-	-	Y	Y	-	-	Y	Y	-	-	-
Winsor at p99	Y	-	Y	Y	Y	-	Y	Y	Y	-	Y
Winsor at p95	-	Y	-	-	-	Y	-	-	-	Y	-
Includes 2016 data	-	-	-	Y	-	-	-	Y	-	-	Y
N Treated (Retailers w/ POS)	4985	4985	6906	6819	4985	4985	6906	6819	2492	2492	1135
N Control (Wholesalers w/o POS)	4620	4620	7052	7310	4620	4620	7052	7310	2291	2291	1350

Notes: This Table is identical to Table 1, except that we exclude from the control group all wholesale firms that ever used a POS. This table is discussed in Section 5.4.

Figure C.3: Robustness of Difference-in-Difference Estimates to Using the Service Sector as an Alternative Control Group



Notes: This figure is similar to Figure 3 but uses the service sector as a control group, excluding hotels and restaurants, which benefited from a 9 percentage point VAT rebate since 2006. We use the standard difference-in-difference estimation for all outcomes. We focus on firms that report non-zero sales at least once every quarter, to avoid the results being affected by firms with highly seasonal activity. This figure is discussed in Section 5.4.

C.3 Exploiting Variation Across Subsectors and Across Regions

This section exploits variation across subsectors and across regions in the context of DiD and interaction designs to examine whether there are any detectable effects of the introduction of VAT rebates on tax compliance. If a tax compliance impact exists, we should expect it to be larger in subsectors/regions with a larger first stage, i.e. a larger impact of VAT rebates on the volume of card transactions. This is because an impact on VAT compliance must be driven by increased usage of existing POS.[37]

We start by documenting the variation in the size of the first-stage estimates across regions and sectors in Figure C.4. The variation across regions is most striking, with the volume of card transactions increasing by over 45 percent in some regions, which contrasts with insignificant or even slightly negative point estimates in other regions. The capital region Montevideo is in the middle of the range of estimates. The variation of estimates across sectors is less extreme, as many sectors experience increases in the volume of transactions around 20-30 percent, but other subsectors experience changes that are both economically and statistically insignificant.[38] Variation across subsectors/regions in the RD coefficient for total sales is somewhat but not perfectly correlated with the RD coefficient for the number of card transactions.

How to divide retail firms (with POS) into more and less intensely treated groups based on the size of the first stage is hence not obvious, as we have two outcome variables in the RD (the volume and number of card transactions) and could consider several cutoffs. We consider various different specifications in Figure C.5, focusing on output VAT as our outcome of interest. In the first four panels, we consider firms as treated if they are in a region for which the RD jump in the volume of card sales is above the 50th or above the 75th percentile of the distribution across regions (panels I.A. and I.B.) or if the RD jump in the number of card transactions is above the 50th or above the 75th percentile of the distribution respectively (panels I.C. and I.D.). In panel I.E., we compare retailers firms in Montevideo (treated) to retailers in all other regions in the country. This is motivated by the fact that the RD estimate for Montevideo is the most precise. The second row of the figure shows similar cuts applied across subsectors. Not all of the subsector-specific RD coefficients are statistically significant. In panel II.E., we hence consider firms as treated if they operate in a subsector with a statistically significant RD jump in either total sales or the number of transactions.

The figures show precisely estimated zero effects in all specifications except in panel II.A.

[37] We exploit variation either across subsectors or across regions, rather than across subsector*region cells, as the latter cells exhibit large variation in size, and because spillovers across subsectors and across regions are limited, but spillovers across subsector*region cells are harder to trace and therefore harder to exclude.

[38] We focus on firms with at least 50 firms.

However, the significant point estimate in this panel is due to a pre-existing trend, and driven by firms in the middle of the treatment distribution, as the point estimate becomes much smaller and insignificant when we cut by the 75th percentile of the distribution of RD coefficients (panel II.B.), and even smaller when cutting the sample by the size of the effect on the number of card transactions (panels II.C. and II.D). Furthermore, most of the estimates are closer to zero when estimating a synthetic difference-in-difference model, as shown in Figure C.6. We hence consider that these analyses confirm our main result of no significant effect of the VAT rebates on tax compliance.[39]

Finally, 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. Concretely, we estimate

$$\begin{aligned} y_{ist} = a_i + g_t + \beta_1 \cdot PostReform_t \cdot Treated_i + \beta_2 \cdot PostReform_t \cdot Intensity_s \\ + \beta_3 \cdot PostReform_t \cdot Treated_i \cdot Intensity_s + \gamma \cdot X_{it} + u_{it}, \end{aligned} \tag{C.1}$$

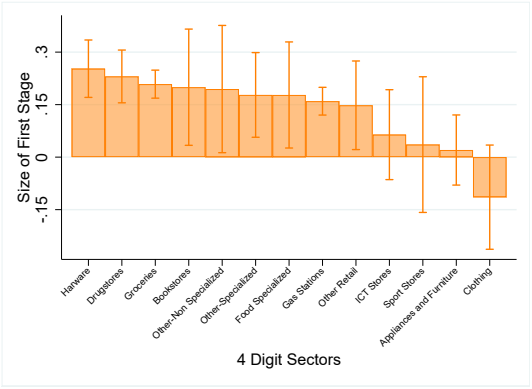
where s indexes groups (either subsectors or regions), and we use indicators for an above-median first stage coefficient in the group as an intensity measure. Everything else is as in Equation 2 in the paper. The interaction effects displayed in the table are all either statistically insignificant or negative, hence corroborating our main finding of no tax compliance impact of the reform.

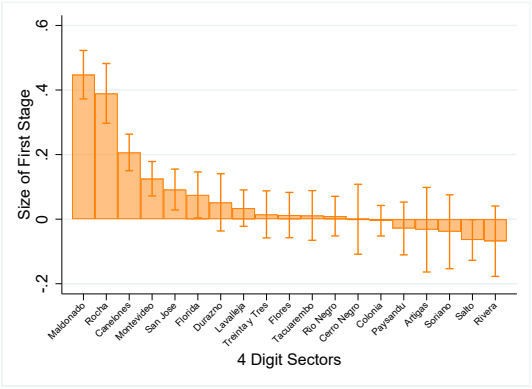
[39] These results hold also for the other outcome variables.

Figure C.4: Variation in the Size of the First-Stage Effect (RD Coefficient) on Card Usage

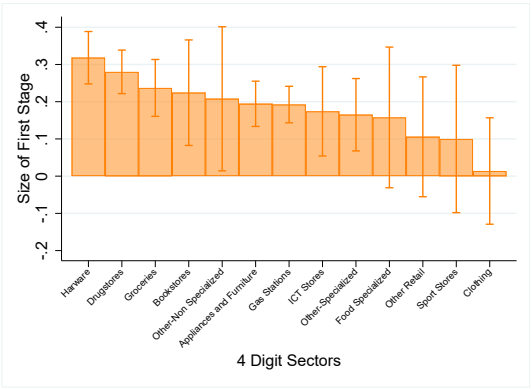
A. Across Four-Digit Subsectors B. Across Departments (Among Retailers)

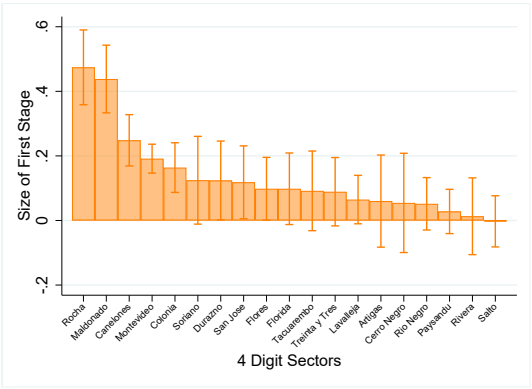
I. RD Point Estimates - Volume of Card Transactions



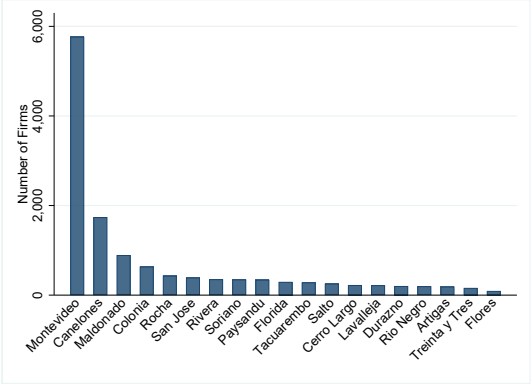


II. RD Point Estimates - Number of Card Transactions





III. Number of Firms

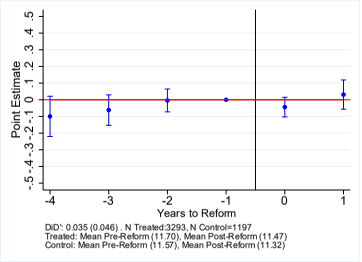


Notes: Panels A.I. and B.I. show variation in the size of the first stage effect (RD coefficient) for the volume of card transactions across 4-digit subsectors and across departments (among retailers). Panels A.II. and B.II. show variation in the size of the first stage effect (RD coefficient) for the number of card transactions. For context, Panels A.III. and B.III. show the number of firms by subsector and by department. This Figure is discussed in Section 5.4.

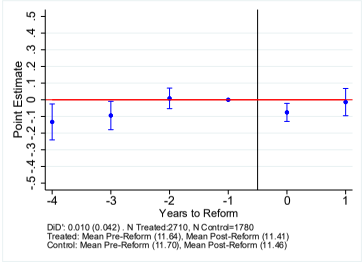
Figure C.5: The Impact of VAT Rebates on Tax Compliance
DiD Estimates Exploiting Variation in RD Estimates to Capture Treatment Status (1/2)

I. Across regions

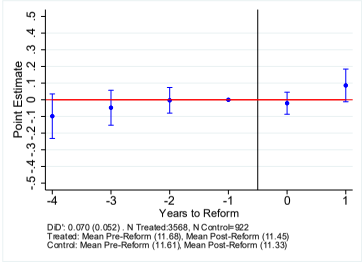
A. P50 sales



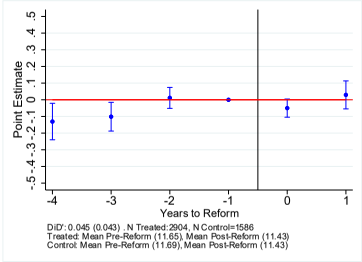
B. P75 sales



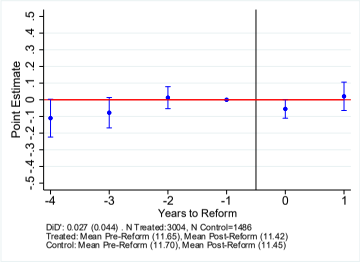
C. P50 N Transactions



D. P75 N Transactions

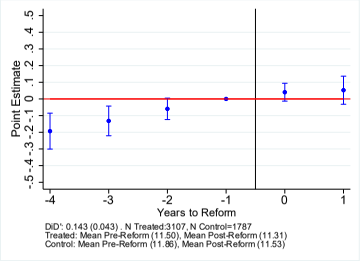


E. Montevideo

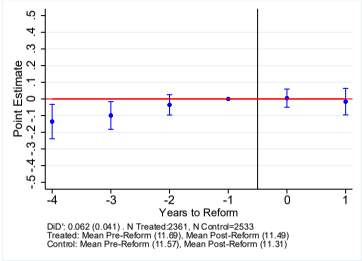


II. Across sectors

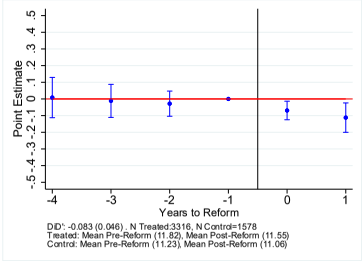
A. P50 sales



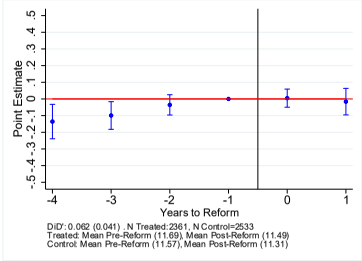
B. P75 sales



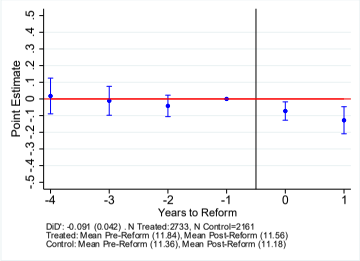
C. P50 N Transactions



D. P75 N Transactions



E. Significant RD Coefficient

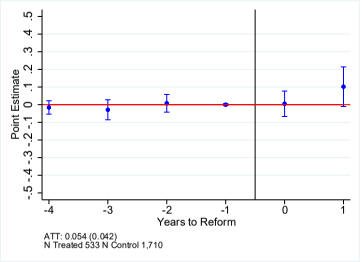


Notes: This figure shows event-study difference-in-difference estimates, comparing retailers with POS in regions (panel A) or subsectors (panel B) with high first stage estimates (treatment group) to retailers in regions/subsectors with low first stage estimates (control group). We run our main RD estimation for each retail subsector and each region with at least 50 firms. The sample division into high/low is indicated in the panel titles. For instance, in panel I.A., retailers in regions with an above-median RD jump in total card sales are considered as treated. In panels C. and D., the division depends on the RD jump in the number of card transactions. In panel I.E, we compared Montevideo (treated) to the rest of the country. In panel II.E., we compared sectors in which the RD coefficient is statistically significant to those in which it is not. The outcome variable is output VAT. This figure is discussed in Section 5.4.

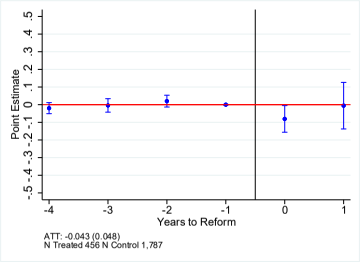
Figure C.6: The Impact of VAT Rebates on Tax Compliance
DiD Estimates Exploiting Variation in RD Estimates to Capture Treatment Status (1/2)

I. Across regions

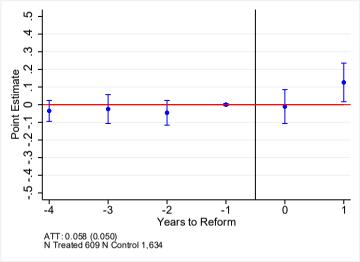
A. P50 sales



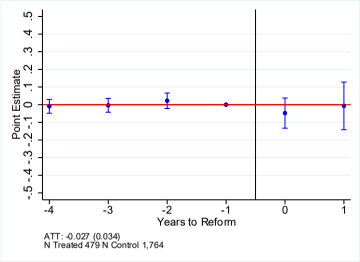
B. P75 sales



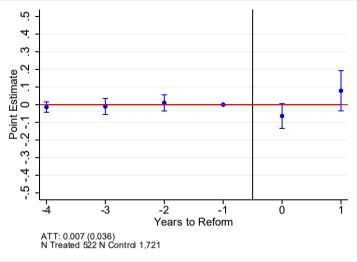
C. P50 N Transactions



D. P75 N Transactions

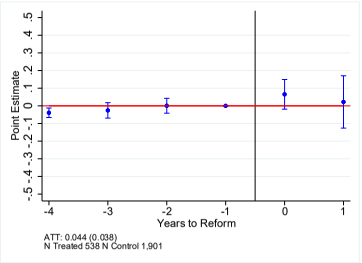


E. Montevideo

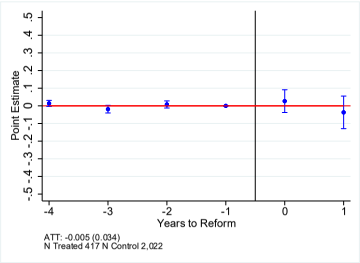


II. Across sectors

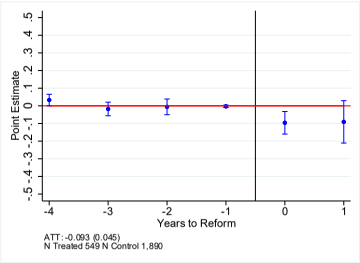
A. P50 sales



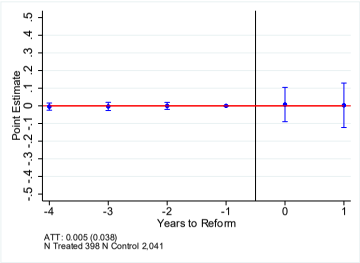
B. P75 sales



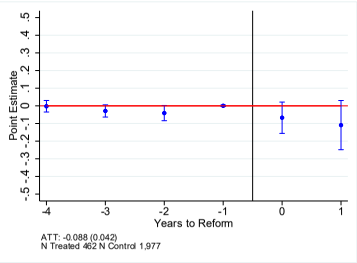
C. P50 N Transactions



D. P75 N Transactions



E. Significant RD Coefficient



Notes: This figure is identical to C.5, except that the estimates are based on the synthetic difference-in-difference estimation. This figure is discussed in Section 5.4.

Table C.8: The Effect of VAT Rebates on Tax Compliance
DiD Interaction with Size of the First Stage (RD Coefficient)

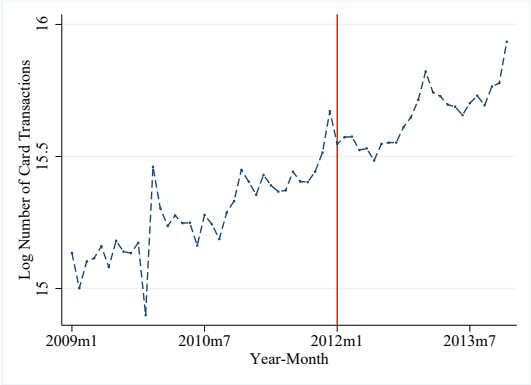
	Taxable Sales	Output VAT	Input VAT	Net Liability
	(1)	(2)	(3)	(4)
A. Interaction With Subsector-Level RD Coefficent for Volume of Card Transactions				
Post · Treated	-0.087	-0.078	-0.047	-0.023
	(0.069)	(0.057)	(0.080)	(0.076)
Post · (RD Coefficient> $p50$)	0.154	0.144	0.264	0.030
	(0.084)	(0.068)	(0.099)	(0.081)
Post · Treated · (RD Coefficient> $p50$)	0.017	0.012	-0.089	0.192
	(0.100)	(0.082)	(0.116)	(0.107)
B. Interaction With Subsector-Level RD Coefficent for Number of Card Transactions				
Post · Treated	0.087	0.085	0.082	0.228
	(0.072)	(0.059)	(0.085)	(0.082)
Post · (RD Coefficient> $p50$)	0.209	0.188	0.262	0.182
	(0.085)	(0.069)	(0.100)	(0.082)
Post · Treated · (RD Coefficient> $p50$)	-0.281	-0.264	-0.302	-0.257
	(0.103)	(0.085)	(0.120)	(0.111)
C. Interaction With Region-Level RD Coefficent for Volume of Card Transactions				
Post · Treated	0.045	0.067	0.153	0.180
	(0.123)	(0.099)	(0.142)	(0.132)
Post · (RD Coefficient> $p50$)	0.147	0.178	0.243	0.148
	(0.119)	(0.095)	(0.139)	(0.117)
Post · Treated · (RD Coefficient> $p50$)	-0.104	-0.122	-0.239	-0.080
	(0.134)	(0.108)	(0.156)	(0.144)
D. Interaction With Region-Level RD Coefficent for Number of Card Transactions				
Post · Treated	-0.196	-0.159	-0.107	0.051
	(0.136)	(0.110)	(0.158)	(0.144)
Post · (RD Coefficient> $p50$)	-0.114	-0.072	-0.043	-0.028
	(0.130)	(0.105)	(0.154)	(0.126)
Post · Treated · (RD Coefficient> $p50$)	0.167	0.131	0.058	0.060
	(0.146)	(0.118)	(0.170)	(0.154)
N Treated	4,985	4,985	4,985	4,985
N Control	6,118	6,118	6,118	6,118

Notes: This table presents estimates of the DiD-interaction specification in Equation C.1. The panel titles indicate which RD coefficient we use to construct the interaction dummy. The percentiles are constructed across retail subsectors and across wholesale subsectors separately. Everything else is as in Table 1. This table is discussed in Section 5.4.

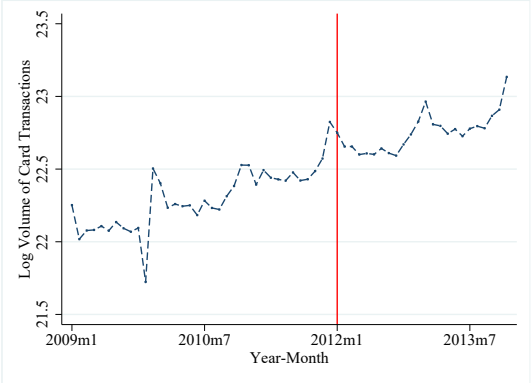
D Interpretation and Policy Implications Appendix

Figure D.1: The Impact of Reductions in Commission Fees and Tax Withholding Rates On The Use of Electronic Payment Technology

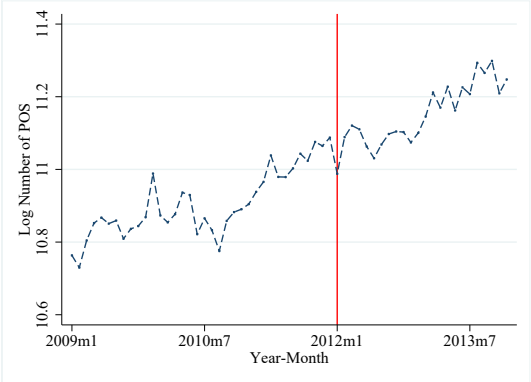
A. Number of Card Transactions



B. Volume of Card Transactions



C. Number of POS

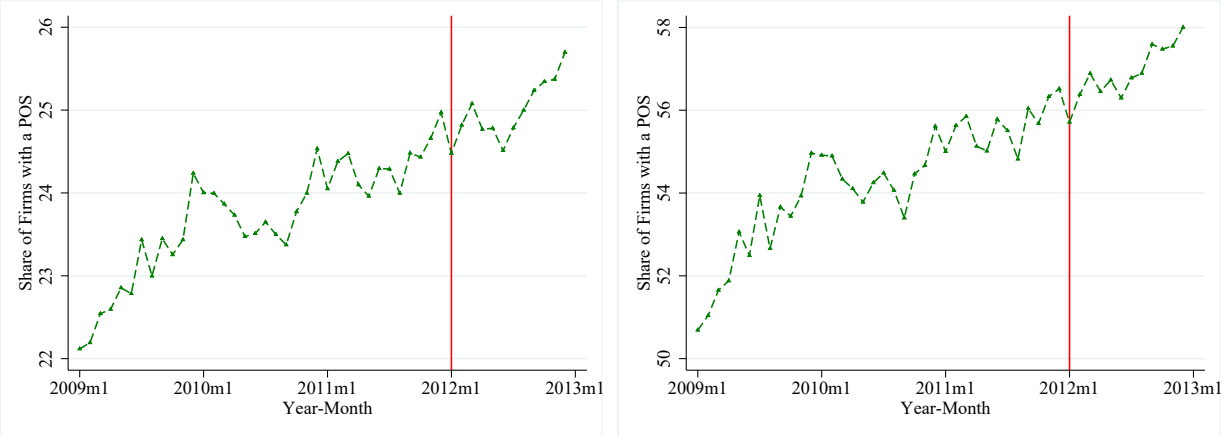


Notes: These graphs are similar to those in Figure 1, Panel A, displaying time series aggregates, as per the panel titles. The vertical line marks January 2012, when withholding rates applied by credit/debit card companies were reduced (see Figure A.8) and commissions charged by credit/debit card companies were lowered (see Section 3.3). This figure is discussed in Section 6.

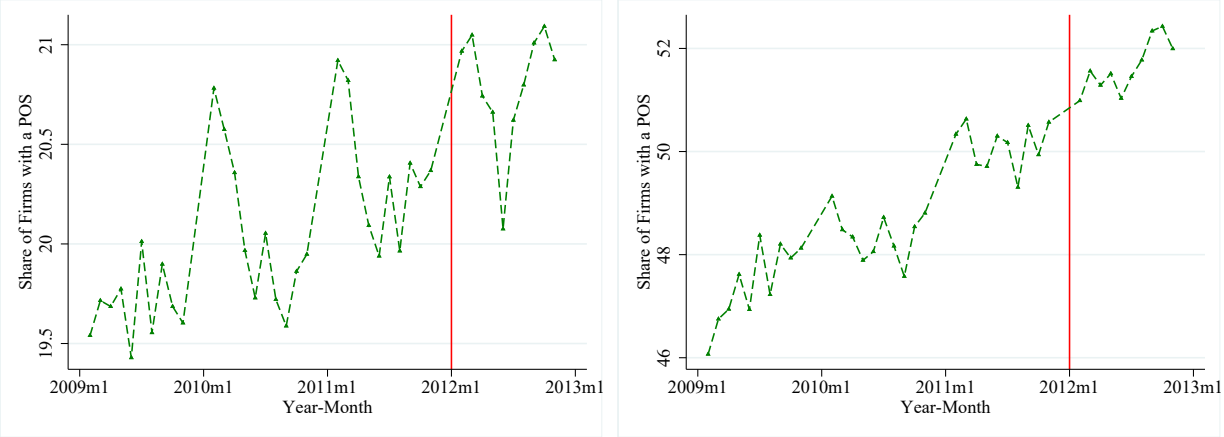
Figure D.2: The Impact of Reductions in Commission Fees and Tax Withholding Rates On The Share of Firms with a POS Around January 2012

A. All Firms B. Retail Firms

I. Balanced Sample



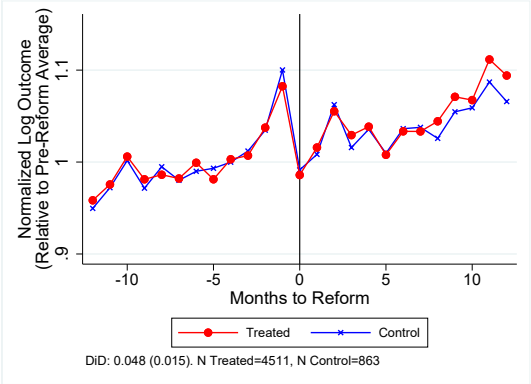
I. Unbalanced Sample



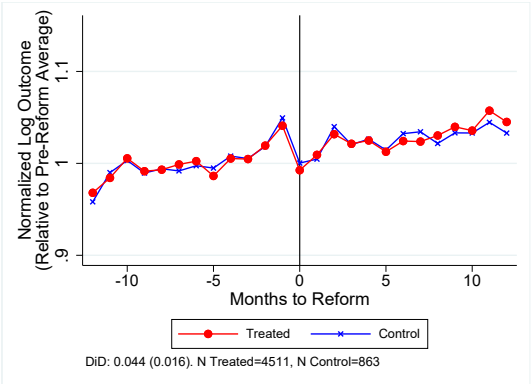
Notes: This figure plots the share of firms that had a POS around January 2012, when withholding rates applied by credit/debit card companies were reduced (see Figure A.8) and commissions charged by credit/debit card companies were lowered (see Section 3.3). In the unbalanced sample, we omit the months of December and January each year to avoid outliers, which arise from the fact that many firms file in only these months. This figure is discussed in Section 6.

Figure D.3: The Impact of Reductions in Commission Fees and Tax Withholding Rates
Difference-in-Difference Estimation on Monthly Data

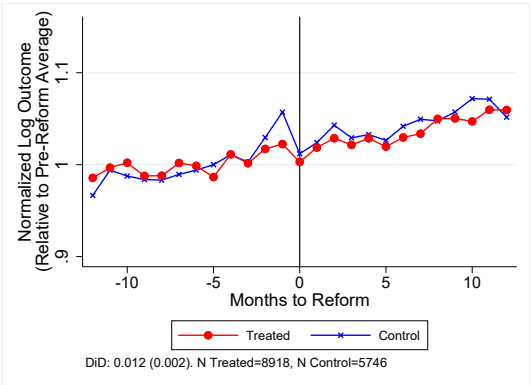
A. Number of Card Transactions



B. Volume of Card Transactions

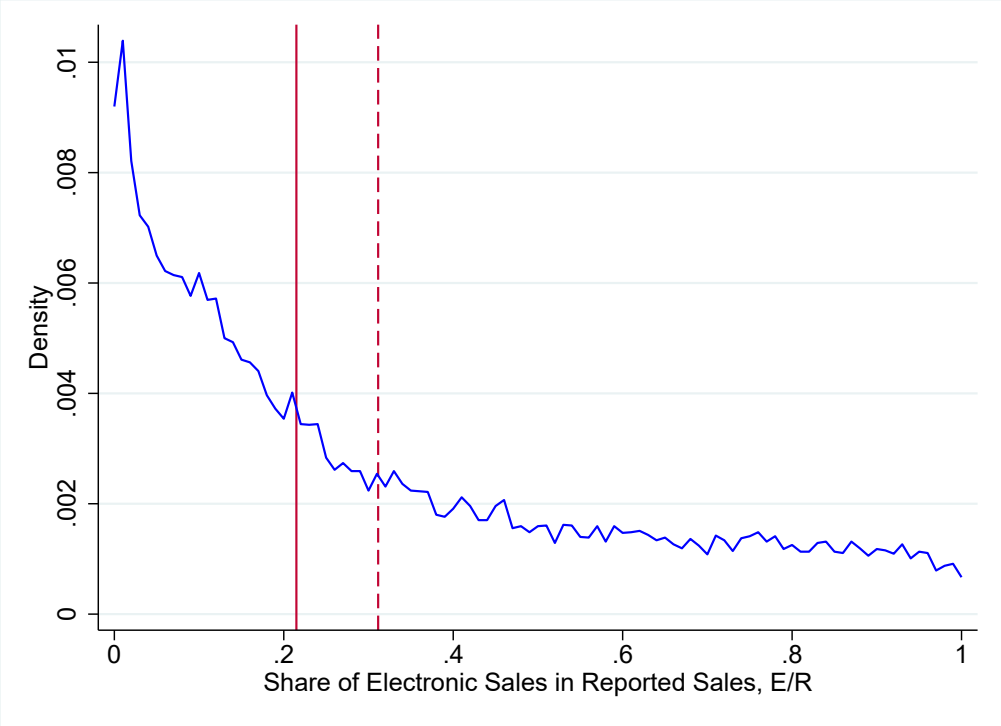


C. Having a POS



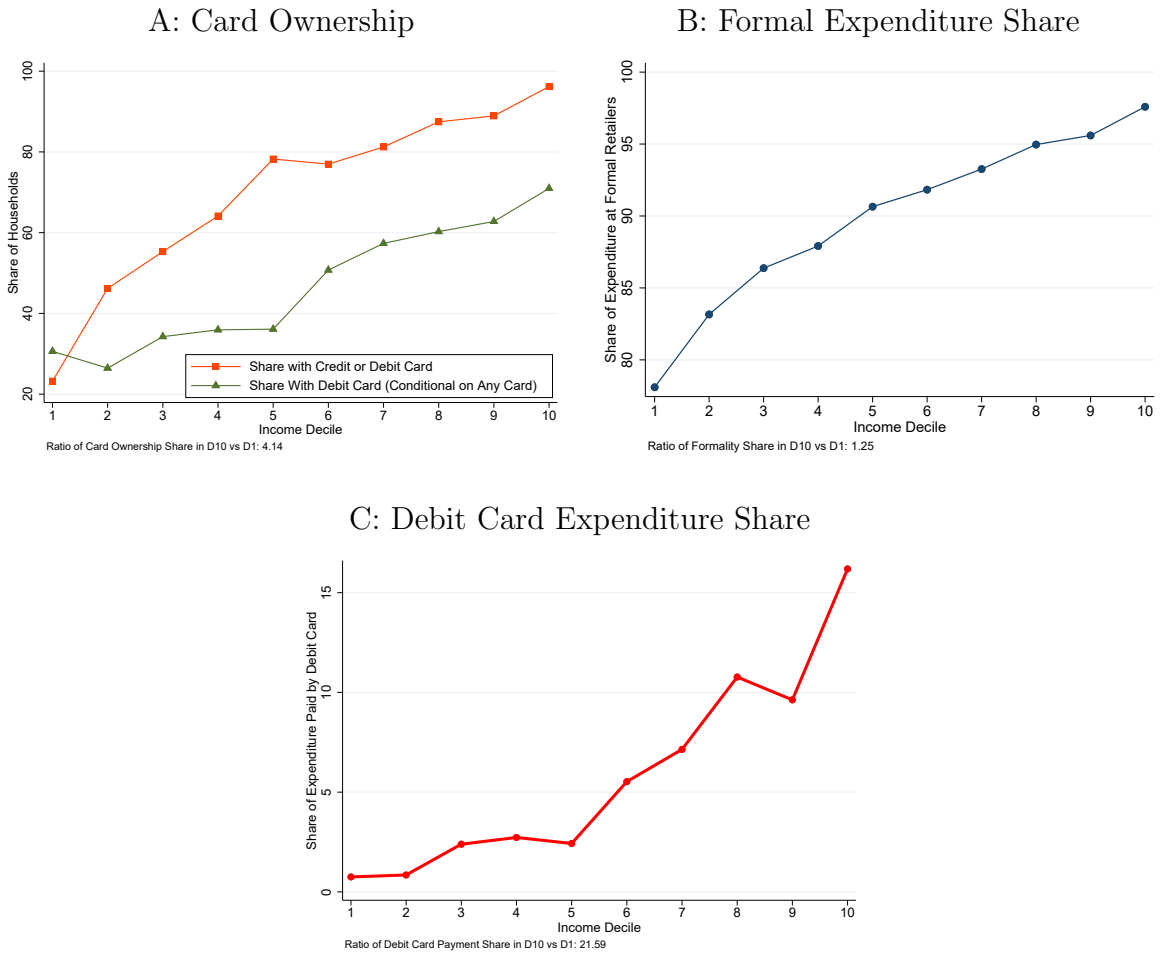
Notes: These graphs implement a difference-in-difference estimation similar to the one from Section 5.1, equation 2, on monthly data. We retain all firms that have card transactions at least once per quarter during 2011q1-2013q1. The post-reform period for the difference-in-difference estimation starts in January 2012, when withholding rates applied by credit/debit card companies on card purchases from non-CEDE firms were reduced (see Figure A.8) and commissions charged by credit/debit card companies were lowered (see Section 3.3). The outcome is the log of the volume/number of card transactions in panels A and B. We deal with zeros in the outcome in the same way as we do in the main difference-in-difference analysis, by valuing an extension margin change from zero to the minimum non-zero value the same as a 10 percent increase on the intensive margin. The outcome in panel C is a dummy for having a POS. This figure is discussed in Section 6.1.

Figure D.4: Share of Card Sales in Reported Sales in Costa Rica



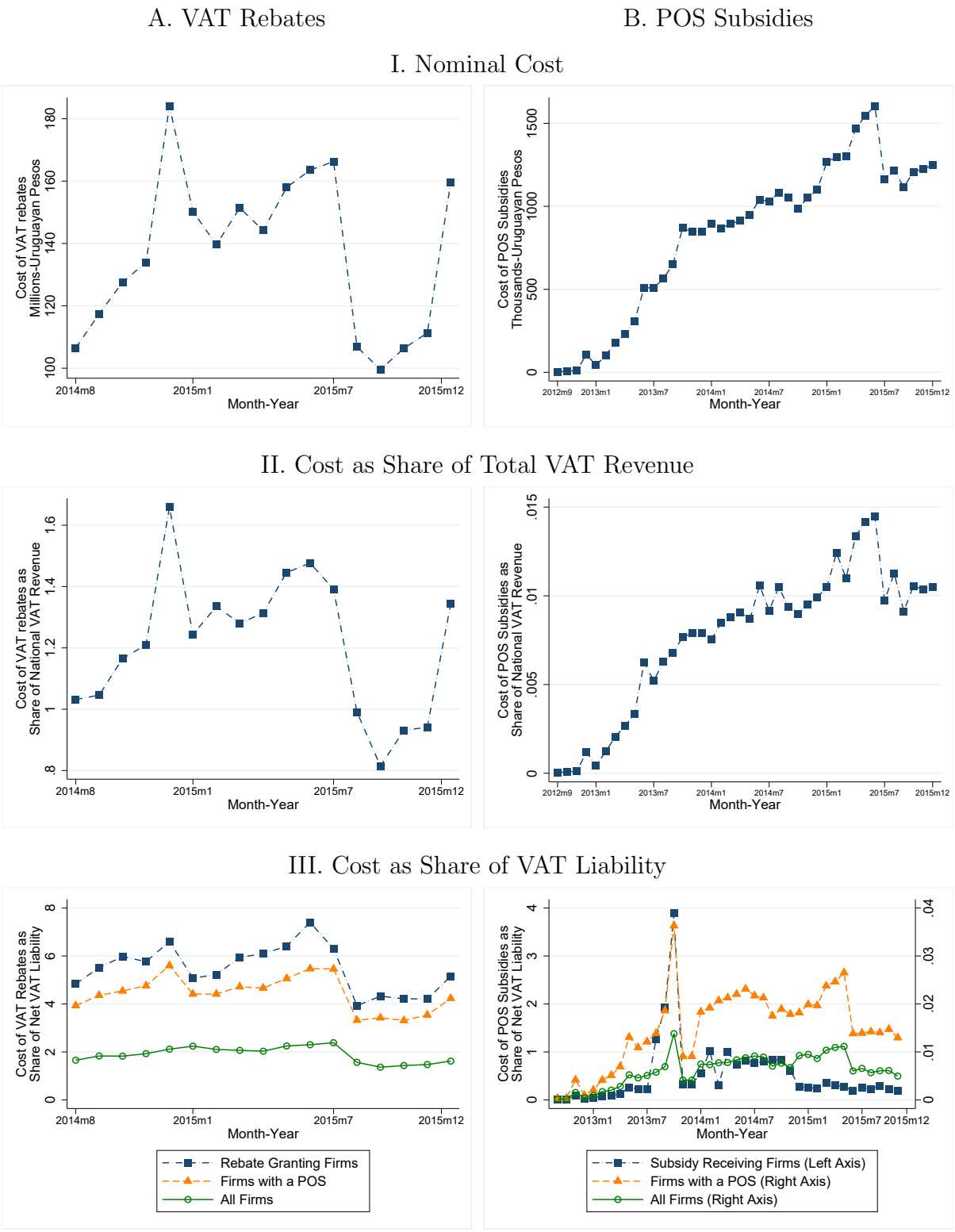
Notes: This is similar to Figure 4, Panel C, but show the share of card sales in reported sales in Costa Rica. In Costa Rica, as in Uruguay, credit and debit card companies report all card sales to the government and remit a small fraction of the transaction amount as advance tax payment.

Figure D.5: Statistics Informing the Distributional Impact of VAT Rebates



Notes: Panel A shows the share of households that have a credit or debit card, and the share of households that have a debit card, conditional on having any card. Panel B shows the share of household expenditure at formal retailers, using the 2004 household survey from the National Statistics Institute and following the methodology in Bachas et al. (2023) to categorize retailers as formal and informal. Panel C shows the share of household expenditure that is paid for by debit card. The share of debit card payments is approximated from categorical data that allows respondents to choose between 0-25%, 25-50%, 50-75% and 75-100%. For each response category, we impute the maximum of the range as the value. We impute a zero share for households that do not have a debit card. We then average across households within each income decile. Panels A and C are based on the National Financial Inclusion Survey 2014. This figure is discussed in Section 7.2.

Figure D.6: The Cost of VAT Rebates and POS Subsidies



Notes: This figure examines the cost of the VAT rebates and POS subsidies. Panel A1 plots the nominal cost (in millions or Uruguayan pesos) of the VAT rebates. Panel A2 plots the cost of the rebates as a share of total VAT revenue (extracted from dgi.gob). Total VAT revenue includes domestic VAT revenue and VAT collected at customs. Panel A3 plots the cost of the VAT rebates of VAT-filing-firms relative to the net VAT liability of three different groups of firms, as per the labels. Panel B displays similar measures for the POS subsidies. For panel B, the values for November and December 2013 are an average over the two months, as we observe no subsidy payments in December 2013, and a disproportionately high number in November, suggesting that December payments were erroneously recorded in November. This figure is mentioned in the conclusion, Section 8.

E POS Adoption Appendix

This section analyzes the characteristics of firms with and without a POS, the predictors of POS adoption, and the association between POS adoption and tax compliance outcomes. Table E.1 compares firms with a POS and those without POS in 2013, finding that firms with a POS are larger, older and have more branches, report a higher VAT liability, are more likely to be in retail, hotels and restaurants and less likely to be in services. Table E.2 shows that the same differences in terms of size, age and VAT liability between firms with and without POS also hold within the retail sector. Retail firms with a POS are also found to pay a higher effective tax rate, defined as the net VAT liability divided by sales.

In Table E.3, we present the results of a Cox hazard model to predict POS adoption, treating adoption as an absorbing state. Consistent with the descriptive statistics, the likelihood of POS adoption is significantly increasing in firm size as measured by turnover and the number of branches, firm location in the capital city, and sector (especially retail, hotels and restaurants).

Finally, in Table E.4, we show results of panel regressions linking changes in firm reporting behavior to POS adoption, controlling for firm fixed effects and year effects that we allow to vary by deciles of base-year turnover. The analysis suggests that POS adoption is associated with significant increases in reported output VAT, input VAT, net VAT and in the likelihood of reporting a positive net VAT liability.

This appendix is mentioned in Section 6.1 in the paper.

Table E.1: Comparing the Characteristics of Firms With and Without a POS

	POS Terminal	No POS Terminal	Difference	P-value
Log(Turnover+1)	15.33	10.39	4.946	[0.000]
Log(Output VAT+1)	13.80	8.51	5.292	[0.000]
Log(Input VAT+1)	13.50	8.14	5.354	[0.000]
Log(Net Liability+1)	11.08	7.08	4.004	[0.000]
Positive Liability	0.88	0.58	0.302	[0.000]
Effective Tax Rate	0.10	0.10	-0.002	[0.276]
Branches	2.72	1.61	1.107	[0.000]
Firm Age	15.32	12.86	2.455	[0.000]
Retail	0.48	0.09	0.388	[0.000]
Wholesale	0.11	0.12	-0.007	[0.016]
Construction	0.01	0.02	-0.014	[0.000]
Hotels and Restaurants	0.08	0.02	0.061	[0.000]
Finance	0.00	0.02	-0.016	[0.000]
Entretaiment	0.01	0.00	0.001	[0.019]
Other Services	0.14	0.51	-0.368	[0.000]
All Other Sectors	0.17	0.22	-0.045	[0.000]
CEDE Status	0.02	0.06	-0.047	[0.000]
N	14,199	70,028		

Notes: This table compares the characteristics of firms with and without a POS, in 2013. Columns 1 and 2 show means for the two groups, column 3 shows the difference and column 4 shows the p-value on the difference.

Table E.2: Comparing the Characteristics of Firms With and Without a POS: Retail Sector Firms

	POS Terminal	No POS Terminal	Difference	P-value
Log(Turnover+1)	15.46	13.35	2.106	[0.000]
Log(Output VAT+1)	13.83	10.84	2.991	[0.000]
Log(Input VAT+1)	13.65	10.65	3.000	[0.000]
Log(Net Liability+1)	10.81	8.16	2.648	[0.000]
Positive Liability	0.88	0.73	0.155	[0.000]
Effective Tax Rate	0.05	0.04	0.009	[0.000]
Branches	2.44	1.39	1.056	[0.000]
Firm Age	15.32	12.60	2.717	[0.000]
CEDE Status	0.01	0.04	-0.036	[0.000]
N	6,774	6,253		

Notes: This table is similar to Table E.1, except that we here focus on retail sector firms only.

Table E.3: Predicting POS Adoption Via a Cox Hazard Model

	Hazard Ratio	Coefficient
Log(Turnover)	1.173	0.159
	(0.007)	(0.006)
N Branches	1.012	0.012
	(0.003)	(0.003)
Montevideo	1.460	0.379
	(0.039)	(0.027)
Age	1.004	0.004
	(0.001)	(0.001)
CEDE	0.158	-1.848
	(0.014)	(0.086)
Retail	2.288	0.828
	(0.084)	(0.037)
Wholesale	1.326	0.282
	(0.059)	(0.044)
Hotel and Restaurants	1.717	0.540
	(0.112)	(0.065)
Entretaiment	1.160	0.148
	(0.233)	(0.201)
Construction	0.812	-0.208
	(0.124)	(0.153)
Finance	0.238	-1.433
	(0.071)	(0.296)
Other Services	0.704	-0.351
	(0.032)	(0.046)
N	10,030	10,030

Notes: This table presents the results of a Cox proportional hazard model predicting POS adoption between 2007 and 2016, considering the first POS adoption for a firm as an absorbing state. We deal with zeros in turnover in the same way as we do in the main difference-in-difference analysis, i.e. by valuing an extension margin change from zero to the minimum non-zero value the same as a 10 percent increase on the intensive margin.

Table E.4: Panel Analysis of POS Adoption and Tax Compliance

(a) All Firms

	Log Output VAT	Log Input VAT	Positive Liability	Net Liability	ETR
Has POS Terminal	0.80	1.24	0.06	1.24	0.02
	0.012	0.022	0.002	0.035	0.004
Mean	8.469	12.191	0.641	0.095	
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Turnover Control FE	Yes	Yes	Yes	Yes	Yes
N Treated	14,711	14,706	14,711	14,711	15,429
N Control	102,865	102,863	102,866	102,866	74,085

(b) Retail Firms

	Log Output VAT	Log Input VAT	Positive Liability	Net Liability	ETR
Has POS Terminal	0.60	0.84	0.04	0.92	0.00
	0.016	0.029	0.004	0.068	0.001
Mean	11.003	16.046	0.804	0.046	
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Turnover Control FE	Yes	Yes	Yes	Yes	Yes
N Treated	7,207	7,204	7,207	7,207	7,438
N Control	10,797	10,796	10,797	10,797	9,786

Notes: This table displays the results of panel regressions relating various tax compliance outcomes (column titles) to POS adoption. We deal with zeros in the outcome in the same way as we do in the main difference-in-difference analysis, by valuing an extension margin change from zero to the minimum non-zero value the same as a 10 percent increase on the intensive margin. The key independent variable is a dummy that switches on once the firm adoptions a POS. The regressions control for firm FE and year FE interacted with base year turnover decile indicators. The dataset is an unbalanced panel of firms between 2007 and 2016.