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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

Keywords: Credit/debit card payment, tax compliance, third-party reporting, VAT

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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 perent. The rebates were immediately granted to customers paying by card, without the need for refund claims or other hassle costs. We evaluate this 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 lead 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 to wholesale sector firms. This 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 are directly treated by the reform, as the VAT rebates do not apply to firm-to-firm transactions. We find that retail and wholesale sector firms 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 very small and 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, but demonetization also had large economic costs, and is hence at best a debatable strategy for policy makers wishing to promote electronic payment technology with a view on improving tax compliance.

Our work also relates to a set of studies evaluating government policies to generate third-

[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.

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 (2020), 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 difference 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 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 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. 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

[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.

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

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, prior to the financial inclusion reforms, the tax administration estimates that at least 20 percent of potential VAT revenues were evaded in 2012, corresponding to a revenue loss of 2.5 percentof GDP (Dirección General Impositiva, 2019).[4] Uruguay also registered a higher level of informality than most other countries at a similar income level (Figure A.1, Panel B).

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.[5] The rebate rates vary across card types, transaction amounts, and over time, as shown in Figure A.2.

Debit card transactions of up to 500 USD (4,000 *Unidades Indexadas*, a Uruguayan accounting unit) 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 500 USD were granted a 2 ppt rebate. In August 2015, the rebates for debit card and credit card transactions up to 500 USD were decreased to 3 ppt and 1 ppt respectively. 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.3. 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

[4] Gomez Sabaini and Jiménez (2012) provide an even higher VAT evasion estimate of 26.3 percent. Bérgolo et al. (2020) find that 15.5 percent of income tax filers under-reported their wage.

[5] 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.

nor incur a hassle cost. The rebate is stated on a consumer's transaction receipt, which makes it highly salient, as show in Figure A.4. The rebates were also introduced with great media fanfare (Figure A.5), 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 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.6 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. 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.[6]

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 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

[6]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.

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 percentto 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 2 percent to 5 percent (see Figure A.7). 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[7] (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.[8] 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 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

[7]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).

[8]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.

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.).[9]

Figure A.8 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.[10] 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.).[11]

We merge the card transaction data with monthly VAT returns, containing all line items from the tax return.[12] 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 retail firms and division number 47 designates wholesale firms. The firm registry also documents in which one of Uruguay's 19 departments the firm is located.

Finally, we have access to the list of firms that received the subsidy for POS rental, with the months during which the firm received the subsidy and the total subsidy amount each month.

[9] 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.

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

[11] We are also in the process of trying to obtain data on the number of credit and debit cards issued around the time of the reform from a large private bank.

[12] These data are also used and described in Bérgolo et al. (2021) and Foremny et al. (2018).

We use corporate income tax records to confirm firms' turnover and hence their eligibility for the POS rental subsidy.

Figure A.9 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 and wholesale firms, the treated and control firms for part of 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. The key distinction between the two groups is that 52 percent of retail firms used a POS terminal even in 2013, before the reform, but only 16 percent of wholesale firms did. 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

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

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.[13] 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 A.2-A.5).[14]

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

[13] 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.

[14] 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.

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.[15]

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

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

The histograms and associated randomization-inference-style p-values also reveal that the reform-month growth rates (July to August 2014) for the number and the volume of card transactions are extreme outliers compared to the pre and post-reform growth rate distributions.[16] 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 variation 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

[15]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.

[16]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.

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.[17]

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.[18] 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 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.

[17] 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).

[18] 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.

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.[19] Finally, it is possible that our specification is affected by serially correlated unobservables and hence autocorrelation in the error term. We thus rerun the RD estimation using the Prais and Winsten (1954) correction for autocorrelated errors (see also Judge et al. (1985) and Davidson and MacKinnon (1993)). The results shown in 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, and to a smaller but still non-negligible increase in the number of POS in use, we now turn to analyze the impact on tax compliance. Applying an RD estimation, as used in

[19] 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.

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 to wholesalers. The following sections describe our methodology, the results and robustness tests.

5.1 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. We rely on the fact that retailers sell almost exclusively to final consumers, whereas wholesalers sell predominantly to other firms. The VAT compliance chain often breaks down at the stage of the business-to-consumer sale, as consumers have no incentive to request a receipt. Retailers are thus typically less tax compliant than wholesalers. In addition, only retailers are directly affected by the VAT rebates we study, which apply to business-to-consumer sales and not to business-to-business sales. Wholesalers are the most suitable control group, as they experience a similar time trend and seasonality as the retail sector.[20] We thus estimate

$$log(Y_{it}) = a_i + g_t + \beta \cdot Retailer_i \cdot PostReform_t + u_{it}, \tag{2}$$

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, $Retailer_i$ indicates retail sector firms, and u_{it} is the error term. The policy impact is measured by the coefficient β on the $Retailer_i \cdot PostReform_t$ interaction term. The identifying assumption is that the outcome for retail sector firms would have evolved in parallel to the outcome for wholesalers in the absence of the reform. To confirm this is the case, we estimate the following event-study version of equation 2

$$log(Y_{it}) = a_i + g_t + \sum_{k \neq -1, k=-3}^{2} \beta_k \cdot Retailer_i \cdot 1_k(k = t) + \epsilon_{it}, \tag{3}$$

and plot the β_k coefficients for each time period.

Our main outcome variables are total taxable sales, reported output VAT, and the net VAT

[20] Inspection of other sectors confirms that they are not suitable controls due to differences in pre-reform trends and seasonality. One may consider that wholesalers are an imperfect control group as they may be partially indirectly treated (if they sell to retailers and retailers become more tax compliant). However, this would result in tax compliance increasing among both retailers and wholesalers after the reform. The data reject this possibility, as we observe no deviation from the pre-reform trend in either group.

liability (=max(output VAT - input VAT, 0)). We use annual data for our main analysis and later show robustness of our results 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.[21] In our preferred specifications, we winsorize the outcome variables at the 99th percentile within each treatment group × year, and we confirm robustness of the results to alternative top-coding approaches.

5.2 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 $Retailer_i \cdot PostReform_t$ 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.

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 increased only by an additional 3.2 percent after the reform, compared to the control group, an effect which 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 (-2.6 percent), which is again statistically indistinguishable from zero (column B). The fact that the consumer response to the VAT rebates is immediate already suggests that any tax compliance response should also emerge relatively quickly. 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 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.

[21] 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.

5.3 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 considering a balanced sample of taxpayers who file regularly during 2011-2015.[22] All point estimates are small and statistically indistinguishable from zero. The same result emerges when we winsorize the outcome variables more conservatively, at the 95th percentile (columns 3, 7 and 11). It thus does not seem to be the case that smaller or larger firms are affected differently. Finally, we still obtain the same results when extending our main specification to include observations for the year 2016 (columns 4, 8 and 12). 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.[23]

While we would like to conduct heterogeneity analyses that split the sample based on firm characteristics predictive of responsiveness, e.g. comparing firms with above vs below medium size, share of card sales or firms with and without a POS, such sample splits are not suitable in our setting, as the treatment can lead consumers to switch from smaller firms or from firms without a POS to firms with a POS. Hence, if we observe an effect in a subsample of firms more susceptible to offer card payments, the effect could be a compliance effect or a real effect driven to consumer switching. The sectoral DiD is not affected by this concern, as consumers are unlikely to switch between buying from a retailer vs buying from a wholesaler.

As a refinement of the sectoral DiD, we present in Figure C.2 a DiD where we zoom in on the retail sector and compare firms in four-digit subsectors with above-median pre-reform POS penetration (treatment group) to firms in subsectors with below-median POS penetration (control group). These subsectors are mostly distinguished by the products sold, e.g. book vs clothes vs food, so that the treatment effect is less likely to be confounded by consumer switching between retailers. If firms in subsectors with higher POS penetration were more

[22] We require taxpayers to file at least once in each of those years, and to file once in 2010 and at least once in the last three months of 2015. The additional sample restrictions are required to avoid that 2010 and 2015 contain a disproportionate share of firms filing less than 12 months.

[23] In addition, we show in the Appendix that we obtain very similar results when adding additional controls (e.g. region × year/month and firm-size deciles × year/month fixed effects). These latter controls do little to reduce the variance of the estimates, as treated and control firms are almost equally distributed across the firm-size deciles and the overwhelming majority of firms is located in the capital city. The results are shown in Tables C.1-C.2.

responsive to the reform – either because they are more affected by consumers' response to the incentives, or because the pressure to adopt a POS is higher in these subsectors – we should see a positive treatment effect. Yet, this analysis again confirms our main results of no detectable impact on any tax compliance measure.[24]

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 retail firms. We now discuss the two main factors that explain the lack of a tax compliance response.

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 A.10-A.12 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.

As a second reason for the lack of a tax compliance impact, we highlight that firms that

[24] We also considered a cross-regional comparison, but this is less relevant as the share of retail firms with a POS varies little across the 19 regions, while it varies substantially across four-digit subsectors within retail (Figure C.3).

already used a POS prior to the introduction of VAT rebates registered a relatively low share of electronic transactions in total reported sales. As Figure 4, Panel C, shows, the mean (median) share of card sales in total reported sales was 25 percent (15 percent) in 2013. This suggests that firms already report a large share of their non-card sales to the government, meaning that there is room for an increase in card sales with no change in reported sales. Consistent with the gradual expansion of electronic payments, the distribution shifts rightward over the years, and especially between 2014 and 2015 with the implementation of consumer VAT rebates. However, the share of card sales in total sales is still very small for many firms, and below 20 percent for the majority of firms. Hence, given the low starting point, even the large increase in the number and volume of card sales in 2014 did not push the share of card sales towards close to 100 percent. Even if firms consider third-party reported (card) sales as a lower bound to their self-reported sales, as reporting sales lower than third-party reported sales might trigger an audit, third-party reported sales do not constitute a binding constraint.

In light of the low share of card sales in firms' total reported sales, it is also unlikely that the VAT rebates would have had an impact on tax compliance if more households had access to credit/debit cards. 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 have pushed the share of card sales in total sales to a point where card sales create a binding constraint for firms' reporting behavior.[25]

7 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

[25] Only a scenario in which consumers previously not using cards started to use cards and request payment with these cards at retailers not previously offering them could have generated an extensive margin POS adoption response from firms, and hence possibly a tax compliance effect.

cost of about 1.5 percent of VAT revenue (Figure A.13).

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. 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. 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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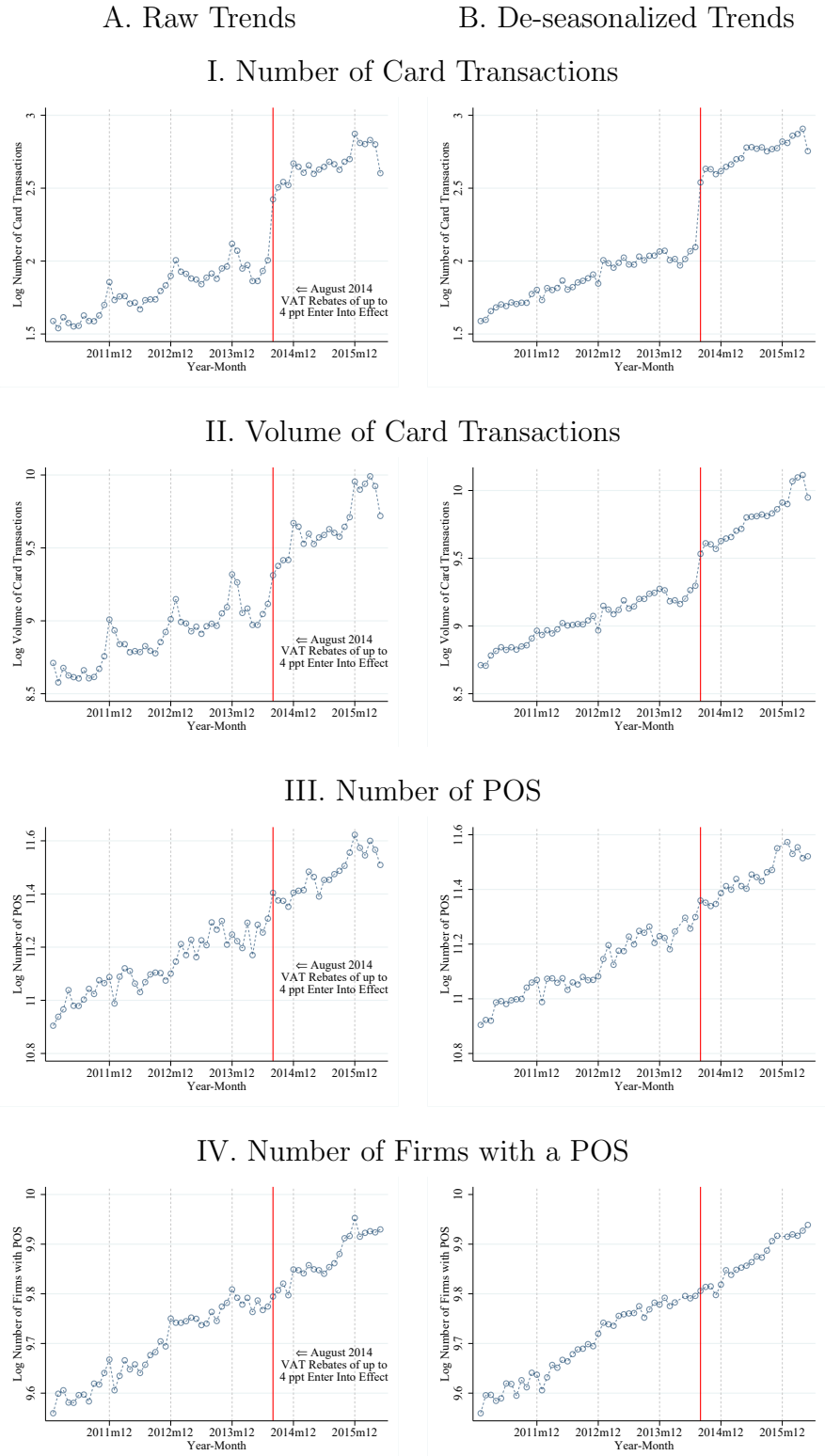
Figures and Tables

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

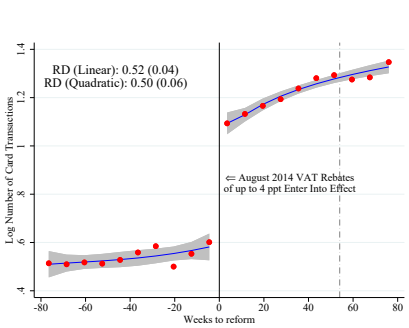


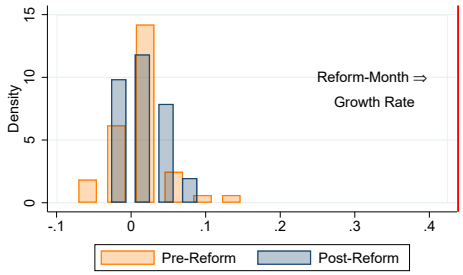
Notes: Panel A plots the monthly aggregate values for each of the outcomes. For row I, the outcome is log of millions of transactions. For row II, it is log of millions of pesos. POS stands for point-of-sales terminal, i.e. credit/debit card machine. We average over the months of April and May 2014, 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

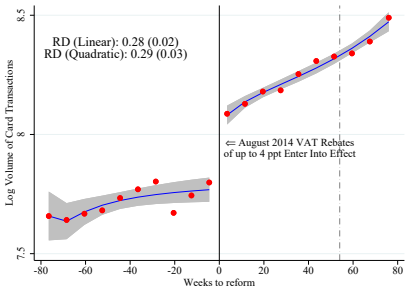
I. Number of Card Transactions

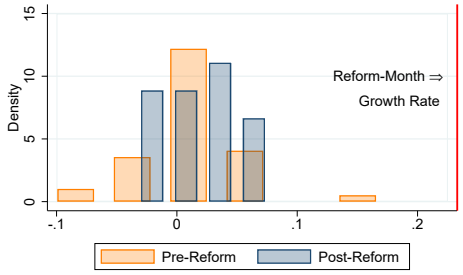




Mean Pre Reform (0.011) vs Mean Post Reform (0.018): ttest p-value (0.530)
Month of Introduction (0.439) vs Mean All Other Months (0.013): ttest p-value (0.000)
Mean Pre Reform (0.011) vs Month of Introduction (0.439): ttest p-value (0.000)
Randomization Inference p-value: (0.000)

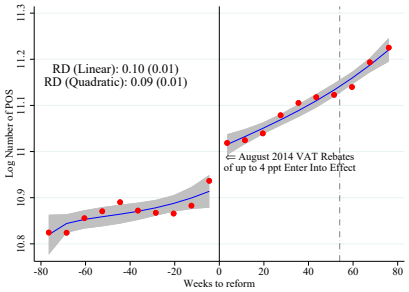
II. Volume of Card Transactions

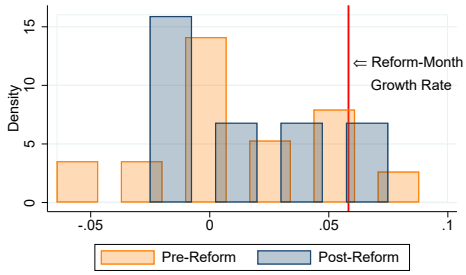




Mean Pre Reform (0.012) vs Mean Post Reform (0.024): ttest p-value (0.343)
Month of Introduction (0.233) vs Mean All Other Months (0.015): ttest p-value (0.000)
Mean Pre Reform (0.012) vs Month of Introduction (0.233): ttest p-value (0.000)
Randomization Inference p-value: (0.000)

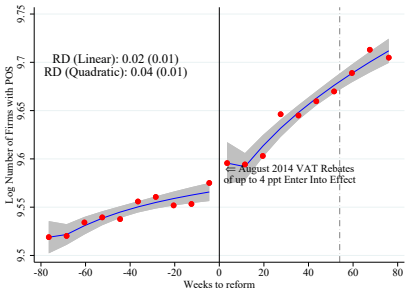
III. Number of POS

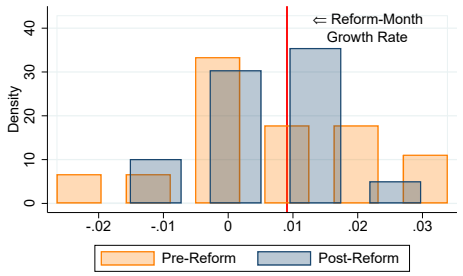




Mean Pre Reform (0.011) vs Mean Post Reform (0.014): ttest p-value (0.740)
Month of Introduction (0.058) vs Mean All Other Months (0.012): ttest p-value (0.000)
Mean Pre Reform (0.011) vs Month of Introduction (0.058): ttest p-value (0.000)
Randomization Inference p-value: (0.136)

IV. Number of Firms with a POS

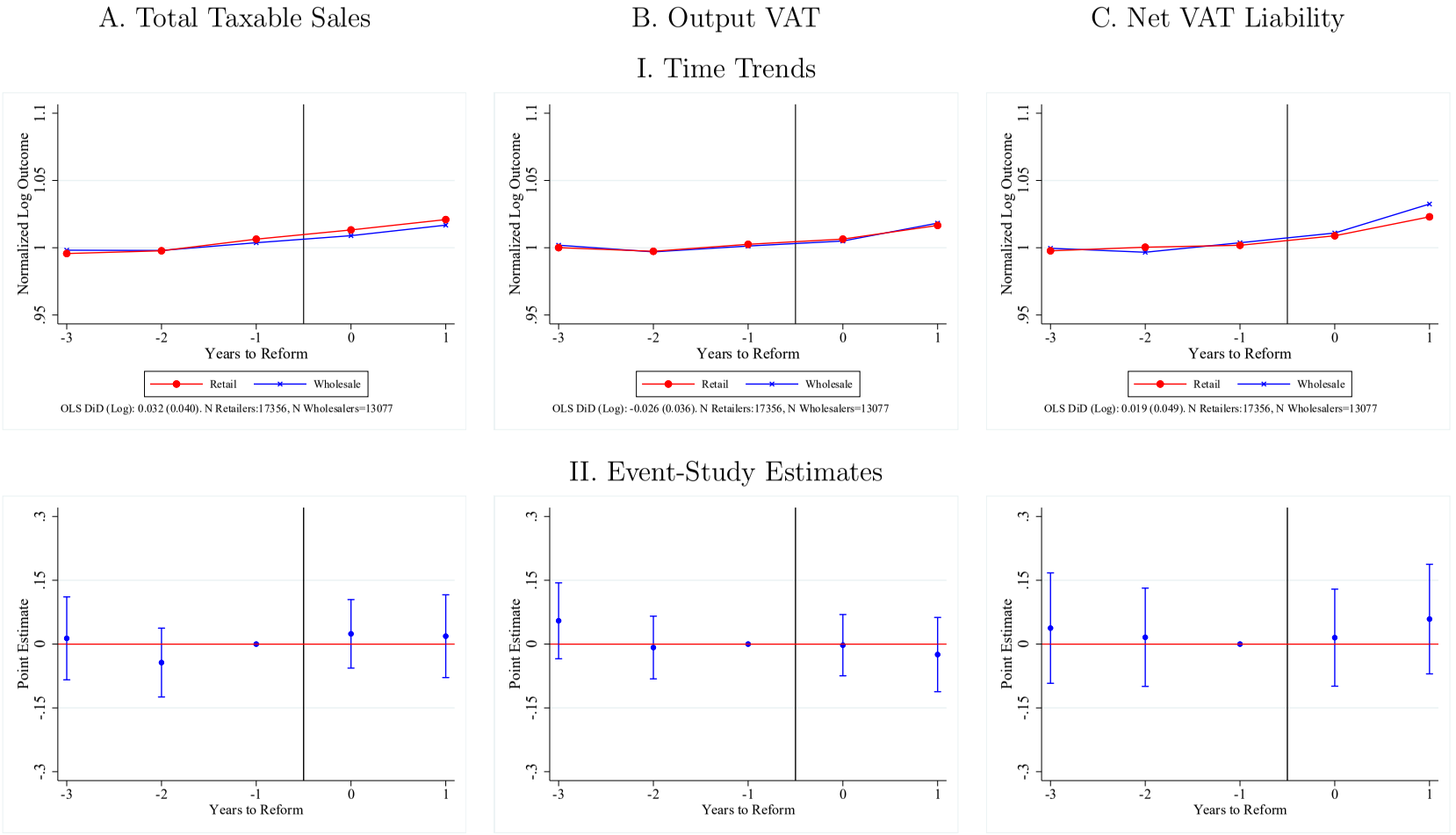




Mean Pre Reform (0.005) vs Mean Post Reform (0.007): ttest p-value (0.667)
Month of Introduction (0.009) vs Mean All Other Months (0.006): ttest p-value (0.086)
Mean Pre Reform (0.005) vs Month of Introduction (0.009): ttest p-value (0.137)
Randomization Inference p-value: (0.373)

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.

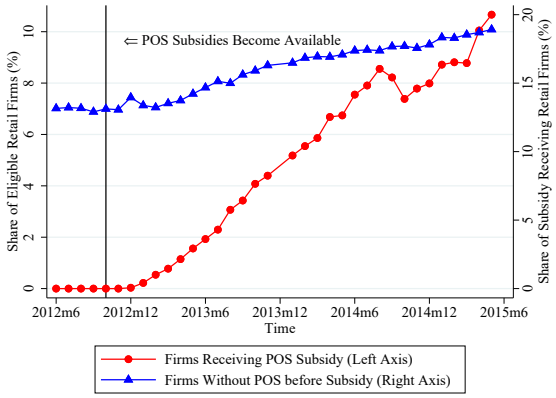
Figure 3: The Effect of VAT Rebates on Tax Compliance
Retailers vs Wholesalers Difference-in-Difference Estimation



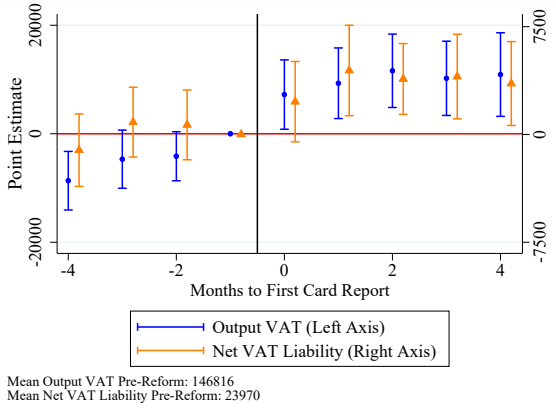
Notes: These graphs implement a DiD estimation comparing retail firms (treated) to wholesale firms (control) around the introduction of the VAT rebates in 2014 (year 0). Panel I shows the normalized time trends and the DiD estimate β on the $Retailer_i \cdot PostReform_t$ interaction from equation 2. Panel II shows the event study estimates β_k from Equation 3. Standard errors are robust to heteroskedasticity. This Figure is discussed in Section 5.2. Table 1 shows the robustness of the results to various alternative specifications.

Figure 4: Explaining the Absence of a Tax Compliance Effect

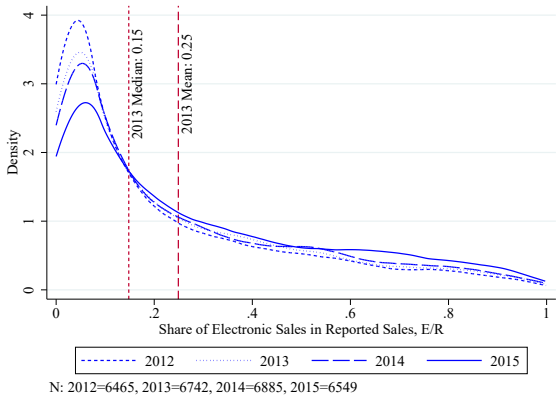
A. Firms' Slow Take-up of POS Subsidies



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 VAT Liability			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Retailer · PostReform	.032	-.065	.043	.06	-.026	.047	-.027	.004	.019	-.022	.02	.026
	(.04)	(.046)	(.04)	(.041)	(.036)	(.041)	(.036)	(.037)	(.049)	(.074)	(.049)	(.049)
Unbalanced Sample	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Balanced Sample	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
Winsor at p99	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Winsor at p95	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Includes 2016 Data	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
N Treated	17356	2822	17356	17416	17356	2822	17356	17416	17356	2822	17356	17416
N Control	13077	3014	13077	13176	13077	3014	13077	13176	13077	3014	13077	13176

Notes: This table documents the robustness of our main DiD specification discussed in Section 5.1. 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). In Columns 2, 6 and 10 we show that the results are very similar when considering a balanced sample of taxpayers who file every year during 2011-2015. Columns 3, 7 and 11 show the robustness of our results to more conservative top coding (winsorizing at p95). Lastly, columns 4, 8 and 12 show the robustness of our results to an extended sample which include observations for the year 2016. 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.

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]
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	
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) and Nicolaides (2021). 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

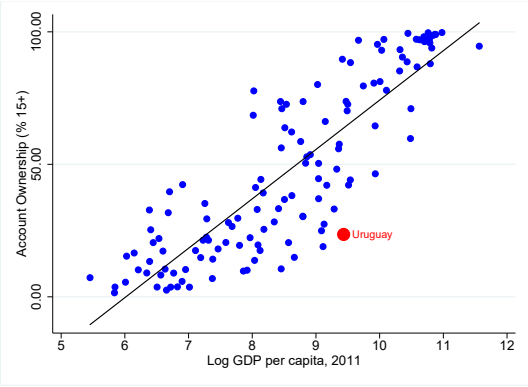
				Percentile				
		Mean	SD	5th	25th	50th	75th	90th
All Firms	Total Annual Sales	8,760	27,089	0	13	1,663	5,679	16,523
	Input VAT	1,301	4,049	0	0	135	672	2,572
	Output VAT	1,911	5,786	0	0	264	1,072	3,785
	Net VAT Liability	546	2,305	-36	0	54	326	1,181
	Net VAT Liability > 0	0.63	0.48	0.00	0.01	0.39	0.55	0.61
	Has POS	0.17	0.37	0.00	0.00	0.07	0.13	0.16
	Number of Card Transactions	1.93	5.21	0.00	0.06	0.33	1.39	4.33
	Volume of Card Transactions	2,587	5,791	9	124	603	2,287	8,085
	Share of Electronic Sales	0.22	0.25	0.00	0.03	0.11	0.32	0.59
Retail Firms	Total Annual Sales	8,324	13,695	16	1,438	3,564	8,790	20,609
	Input VAT	2,075	4,934	0	160	492	1,412	5,008
	Output VAT	2,464	5,761	0	206	599	1,681	5,904
	Net VAT Liability	346	1,512	-58	12	83	283	923
	Net VAT Liability > 0	0.81	0.39	0.03	0.58	0.72	0.78	0.80
	Has POS	0.52	0.50	0.04	0.33	0.41	0.47	0.50
	Number of Card Transactions	3.06	7.23	0.01	0.12	0.48	1.85	7.83
	Volume of Card Transactions	3,499	7,848	11	160	688	2,499	9,169
	Share of Electronic Sales	0.22	0.25	0.00	0.03	0.12	0.33	0.60
Wholesale Firms	Total Annual Sales	18,276	42,005	0	626	3,686	13,401	45,521
	Input VAT	2,788	6,007	0	16	427	2,161	7,888
	Output VAT	3,703	8,331	0	20	611	2,752	10,092
	Net VAT Liability	792	2,384	-94	0	87	518	2,014
	Net VAT Liability > 0	0.67	0.47	0.00	0.22	0.50	0.61	0.65
	Has POS	0.16	0.37	0.00	0.05	0.13	0.16	0.17
	Number of Card Transactions	0.94	1.39	0.00	0.04	0.24	1.13	4.21
	Volume of Card Transactions	1,903	2,622	11	120	641	2,420	8,085
	Share of Electronic Sales	0.22	0.25	0.00	0.02	0.11	0.33	0.60

Notes: This table reports summary statistics of relevant variables for all firms, retail firms and wholesale firms in 2013. The number and volume of card transaction 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 outcomes (Net VAT Liability >0 and Has POS) show the mean outcome across the distribution of firms based on sales size. This Table is discussed in Section 3.4.

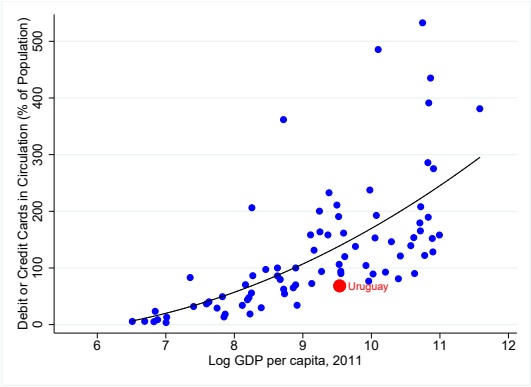
Figure A.1: Financial Inclusion and Tax Compliance
Uruguay in a Cross-Country Comparison

A. Financial Inclusion Indicators

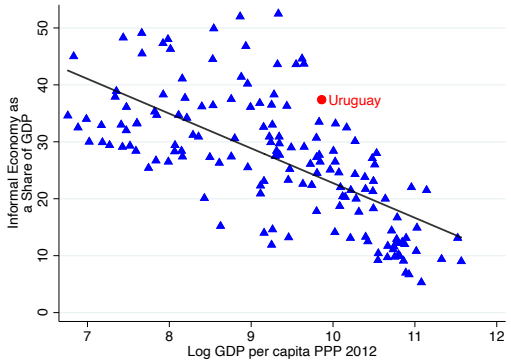
A.I: Account Ownership



A.II: Credit/Debit Cards in Circulation

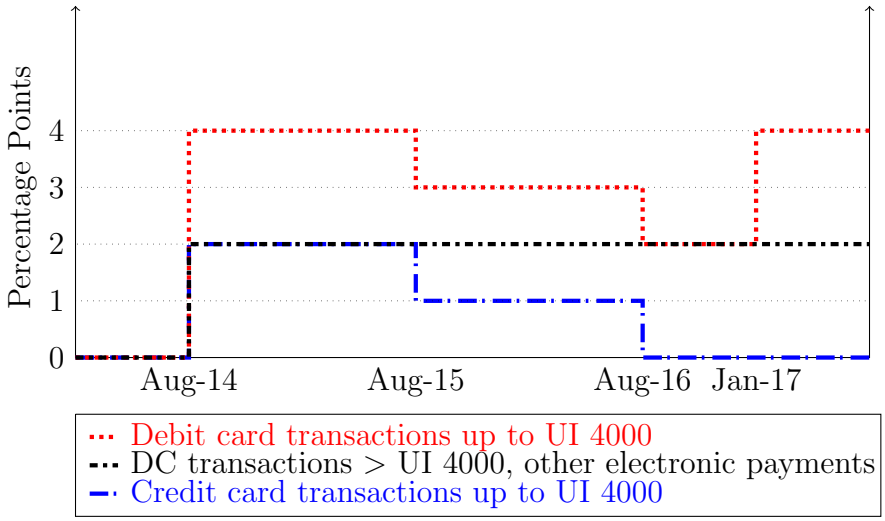


B. Size of the Informal Economy



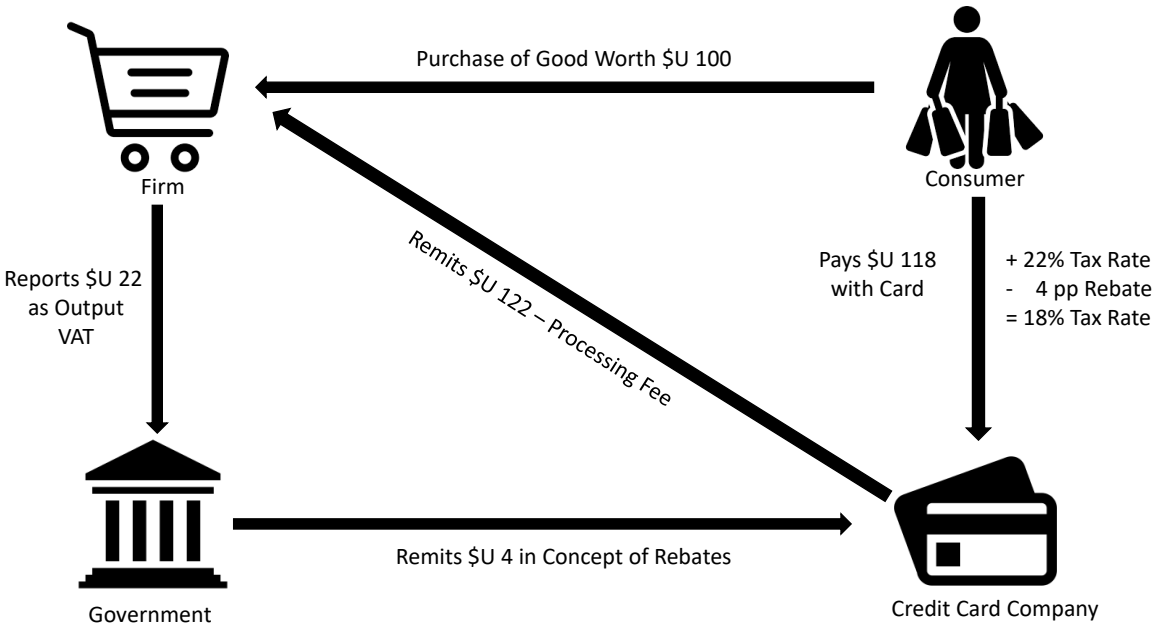
Notes: As discussed in the Introduction and in Section 3.1, Uruguay lagged behind peer countries in terms of financial inclusion. Panel A plots the cross-country relationship between financial inclusion and GDP per capita. Panel A.I display data on account ownership, as measured by the percentage of 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: 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.4: 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.

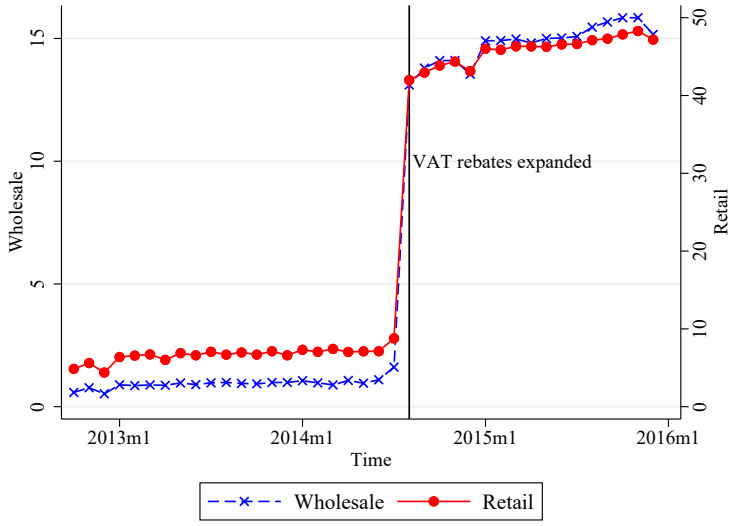
Figure A.5: News Coverage of the VAT Rebates

A. Information about VAT Rebate Introduction

B. Guide on How to Benefit from VAT Rebates

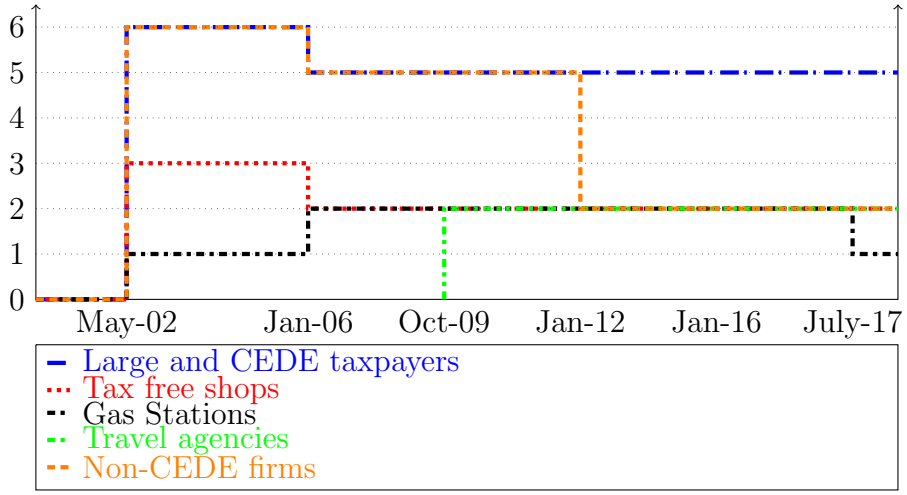
Notes: The figure displays examples of the media coverage of the VAT rebate introduction on August 1, 2014. The article in Panel A (published in June 2014) informs about the introduction on the VAT rebates, while the article in Panel B (published in August 2014) describes the steps consumers should follow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

Figure A.6: Share of Firms Registering VAT Rebates for Consumers Paying by Card



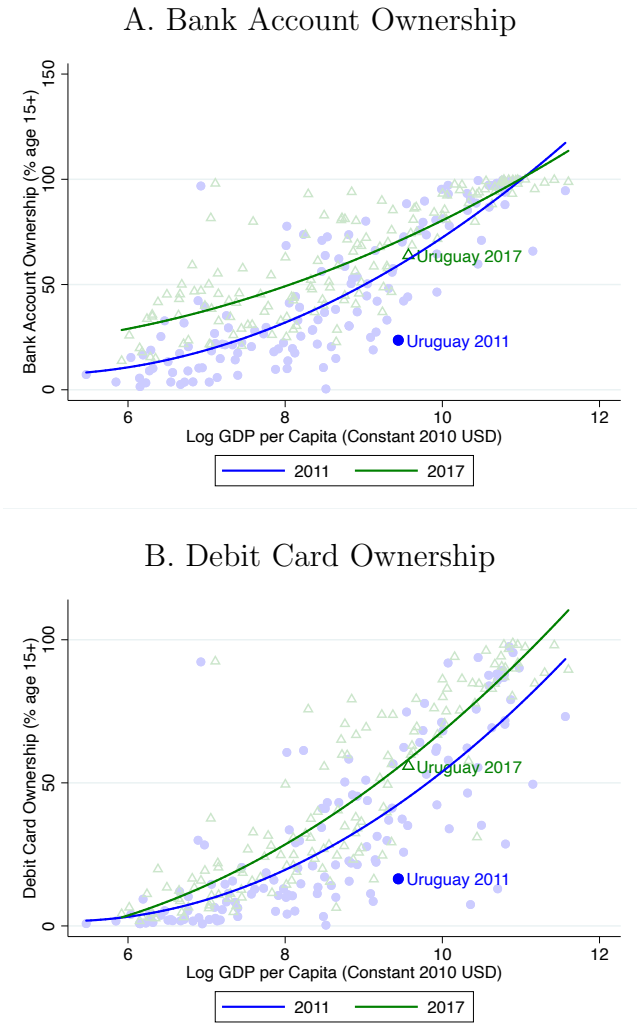
Notes: This figure shows that the VAT rebates were indeed implemented starting on August 2014, as stipulated by the Financial Inclusion Reform. The figure plots the percentage of firms registering VAT rebates for consumers paying by credit/debit card, as captured in the card transaction data. Wholesale firms are on the left y-axis and retail firms on the right y-axis. 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. This figure is discussed in Section 3.2.

Figure A.7: Tax Withholding Rates Applied to Credit/Debit Card Sales



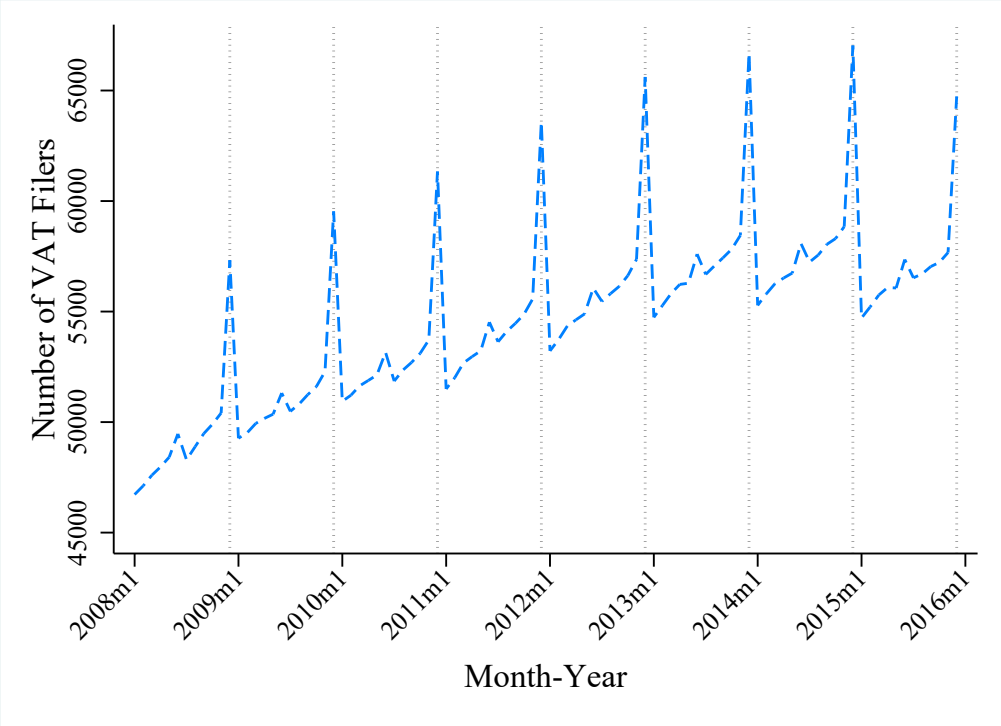
Notes: This figure displays the withholding rates applied by credit/debit card companies to firms making sales using a POS. The rates are differentiated by type of firm (receiving the income from the transaction). CEDE (*Control Especial de Empresas*) is the Uruguayan equivalent of the large taxpayer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

Figure A.8: 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.

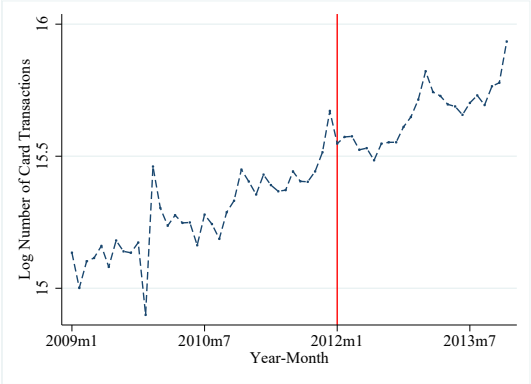
Figure A.9: Number of VAT Filers by Month



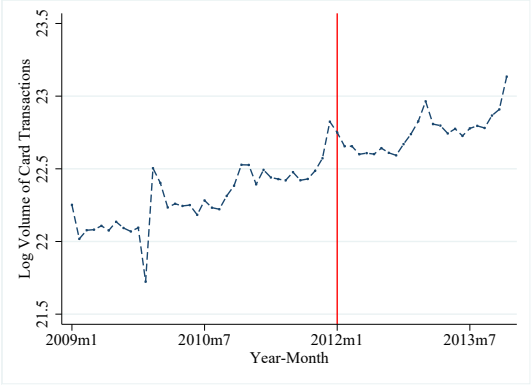
Notes: This figure plots the number of unique VAT filers in each month. The dotted vertical lines mark the month of December each year. For firms that file annually and retrospectively report output VAT and input VAT for each month, we consider that the firm filed for a particular month if it reported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

Figure A.10: The Impact of Reductions in Commission Fees and Tax Withholding Rates On The Use of Electronic Payment Technology

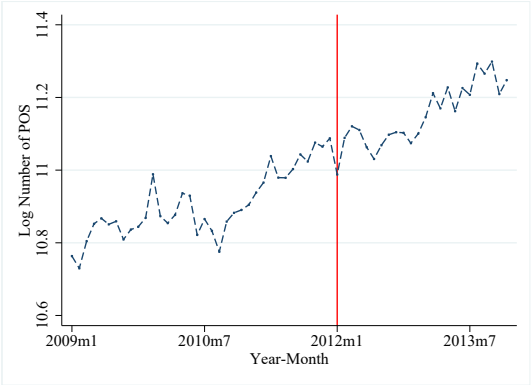
A. Number of Card Transactions



B. Volume of Card Transactions

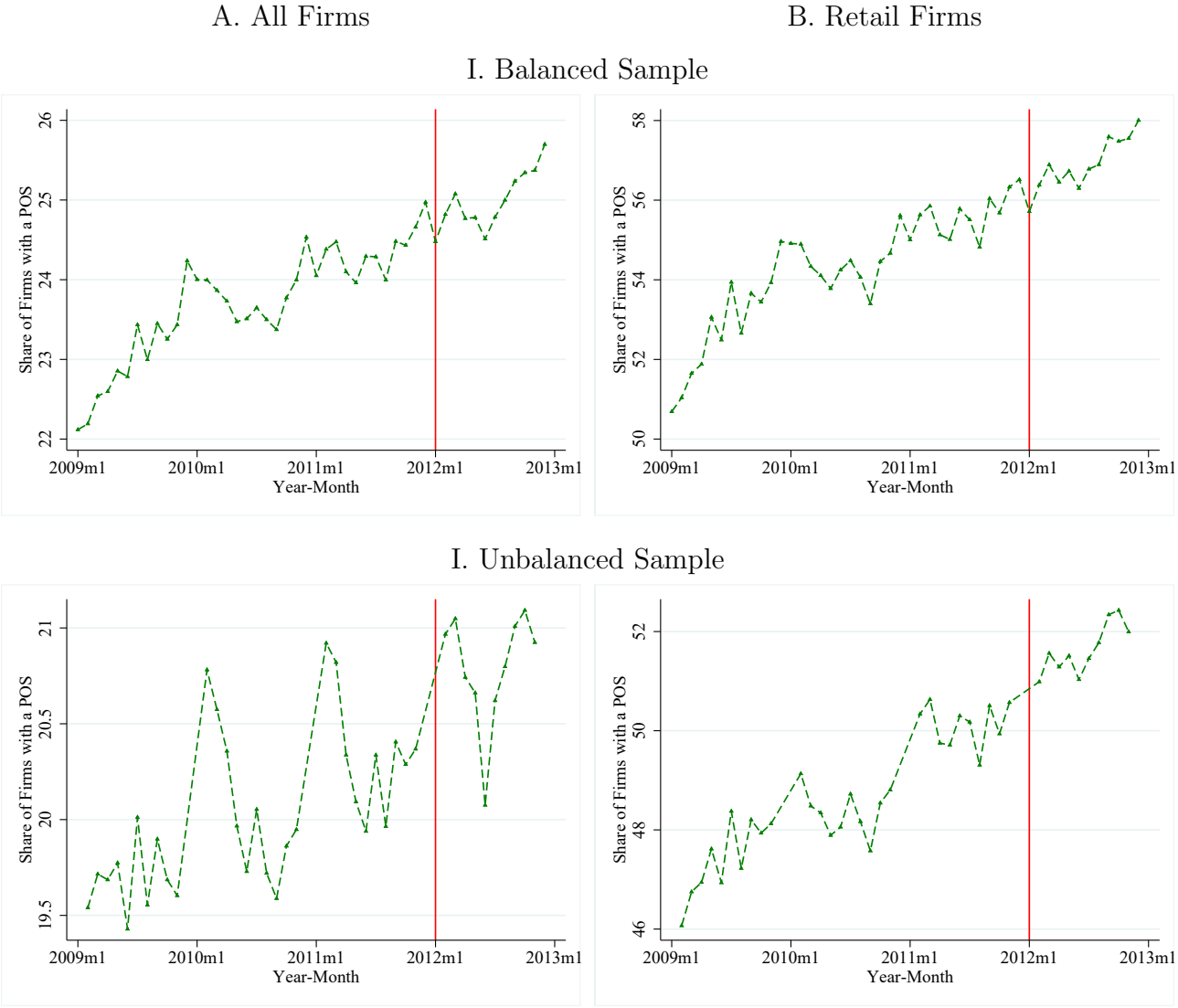


C. Number of POS



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.7) and commissions charged by credit/debit card companies were lowered (see Section 3.3). This figure is discussed in Section 6.

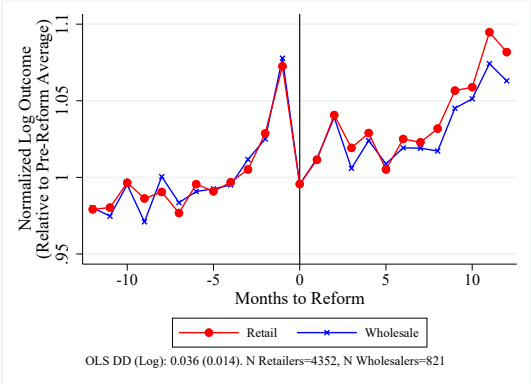
Figure A.11: The Impact of Reductions in Commission Fees and Tax Withholding Rates On The Share of Firms with a POS Around January 2012



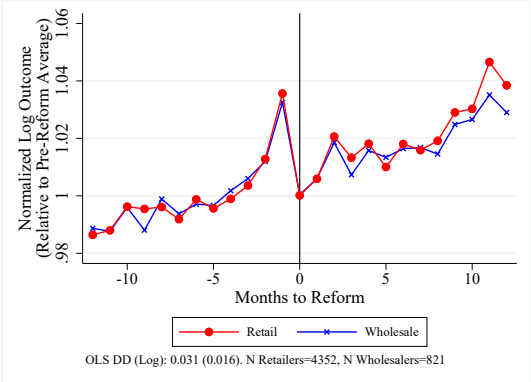
Notes: This figure plots the share of firms that had a POS around January 2012, when withholding rates applied by credit/debit card companies were reduced (see Figure A.7) and commissions charged by credit/debit card companies were lowered (see Section 3.3). In the unbalanced sample, we omit the months 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 A.12: The Impact of Reductions in Commission Fees and Tax Withholding Rates
Difference-in-Difference Estimation on Monthly Data

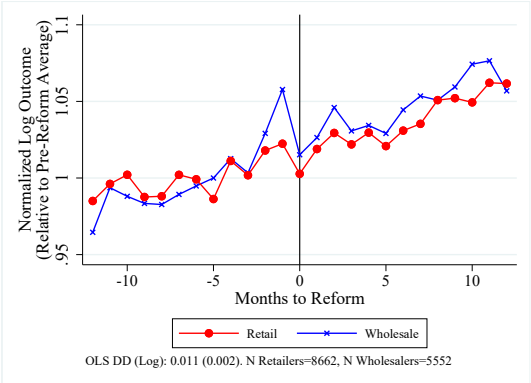
A. Number of Card Transactions



B. Volume of Card Transactions



C. Having a POS

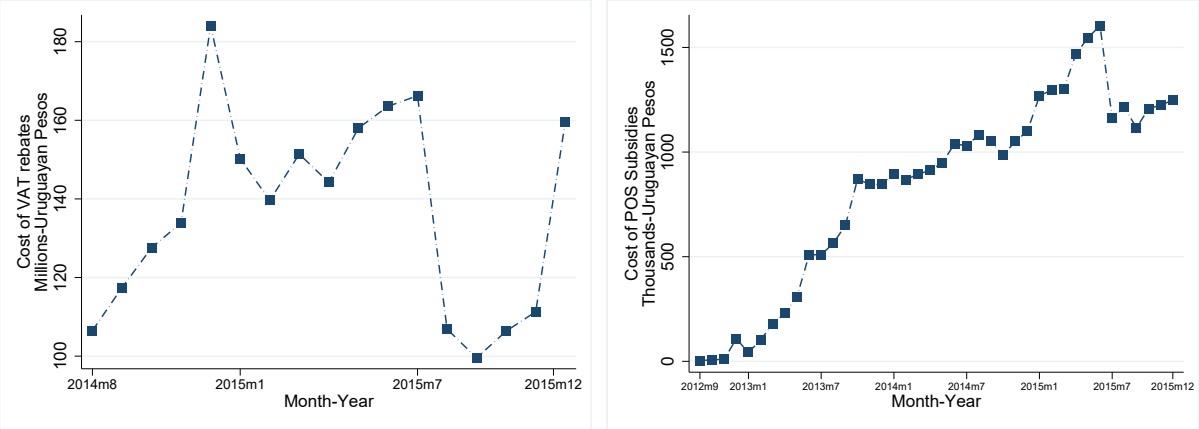


Notes: These graphs implement a DID estimation similar to the one from Section 5.1, equation 2, except that the post-reform period starts in January 2012, when withholding rates applied by credit/debit card companies on card purchases from non-CEDE firms were reduced (see Figure A.7) and commissions charged by credit/debit card companies were lowered (see Section 3.3). This figure is discussed in Section 6.

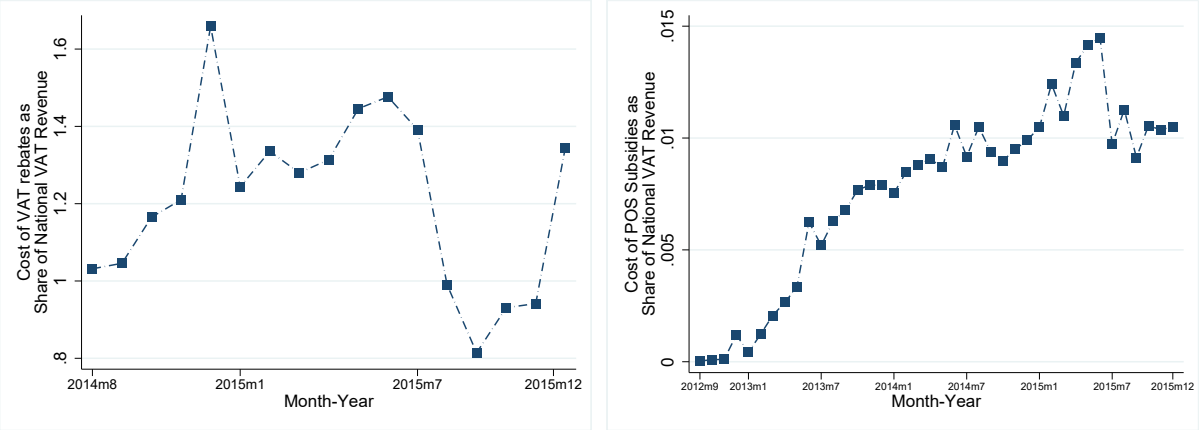
Figure A.13: The Cost of VAT Rebates and POS Subsidies

A. VAT Rebates B. POS Subsidies

I. Nominal Cost



II. Cost as Share of Total VAT Revenue



III. Cost as Share of VAT Liability

Notes: This figures 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 7.

A.1 Simplified Tax Regimes

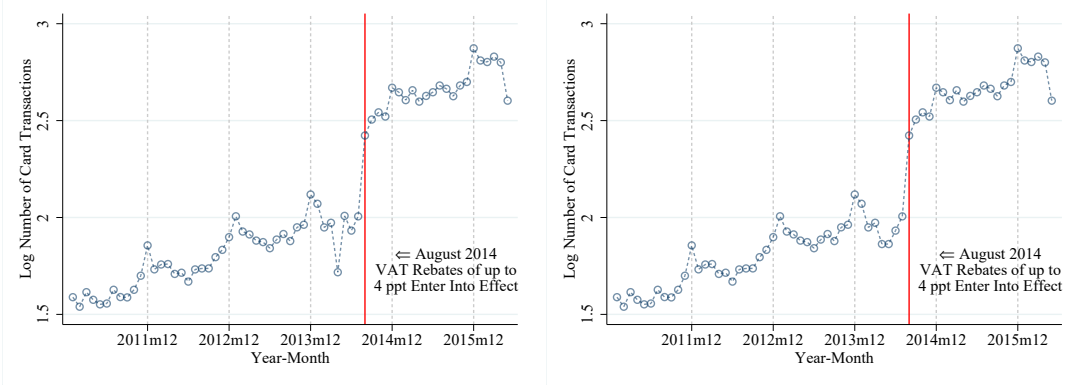
Firms below certain size thresholds can opt into a simplified tax regime. The *monotributo* regime for micro firms unifies all taxes and social security contributions. The *literal E* regime for small firms unifies the CIT and VAT into a monthly lump-sum payment and allows firms to pay social security 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.9 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

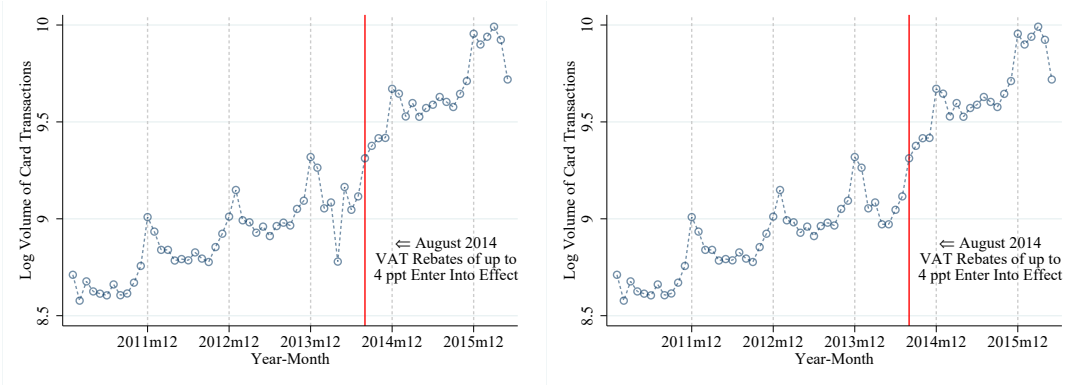
Figure B.1: Raw Data with Outlier in April 2014

A. Raw Trends B. Transformed Trends

I. Number of Card Transactions



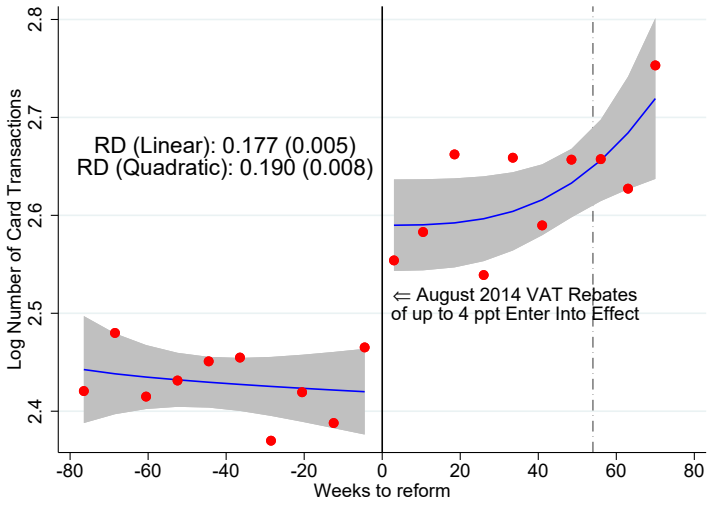
II. Volume of Card Transactions



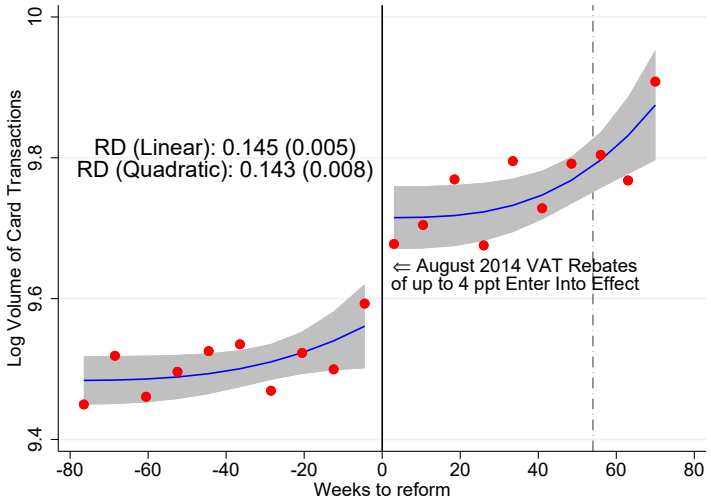
Notes: This figure shows that the months of April and May 2014 constitute outliers in terms of the number of card transactions and the volume of transactions, with a short-lived drop in both outcomes in April 2014 and a strong recovery in May 2014. We hypothesize that this might be due to consumers temporarily 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

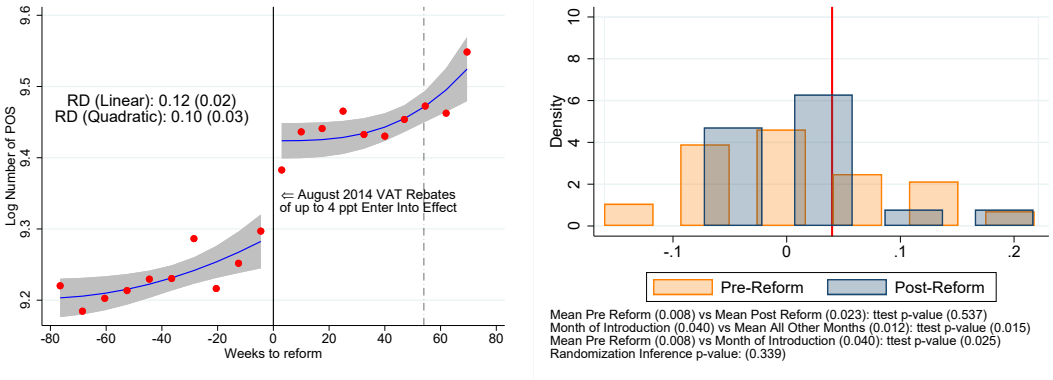


Notes: This figure is similar to Figure 2, Panels AI and AII, but relies on firm-level data to conduct the RD estimation. The estimation uses the firm-level version of equation 1 and controls for firm fixed effects. The estimate hence captures the average response to the VAT rebate introduction, weighing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

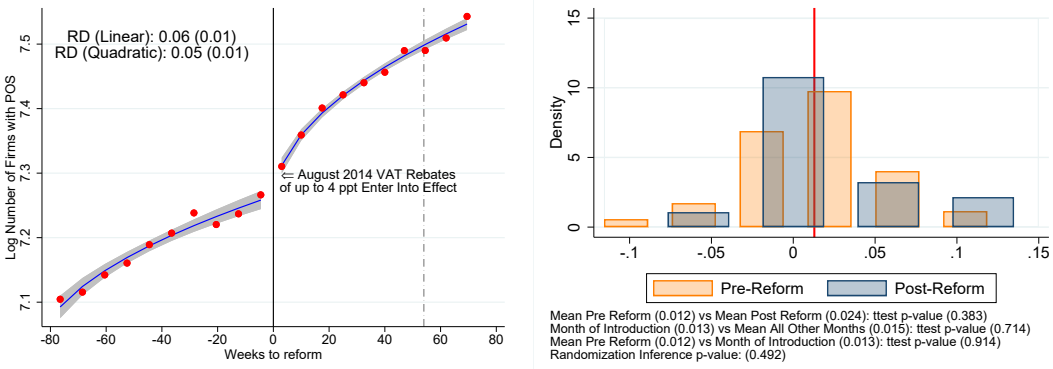
Figure B.3: The Effect of VAT Rebates on the Use of Electronic Payment Technology
RD Estimates and Month-on-Month Growth Rates for Sectors with Low POS Penetration

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

I. Log Number of Card Transactions

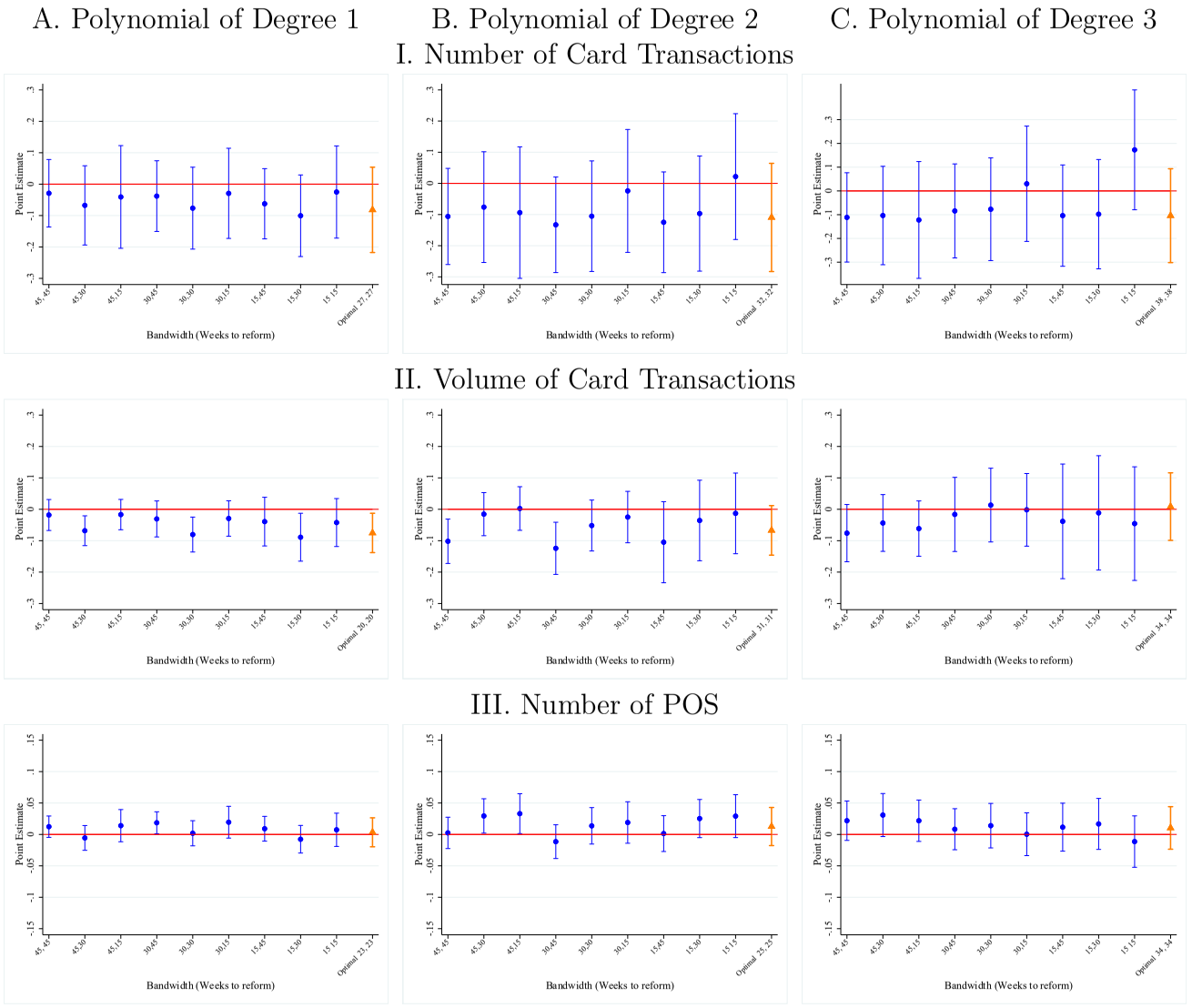


II. Log Volume of Card Transactions



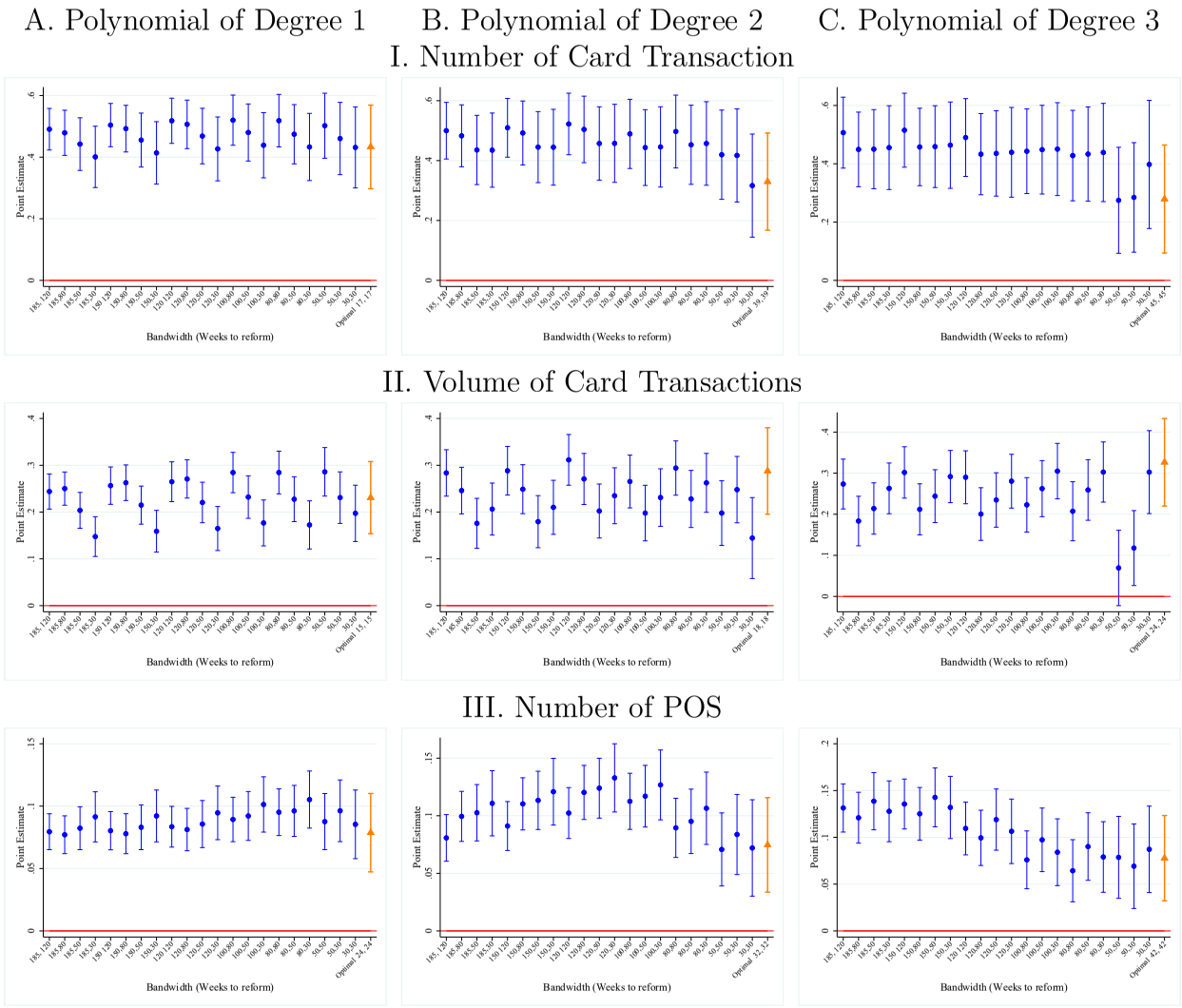
Notes: This figure is similar to Figure 2 but zooms in on retail firms in four-digit subsectors with low POS penetration prior to the reform (in 2013). Low POS penetration is defined as having a below-median share of firms with a POS. This figure is discussed in Section 4.2.

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

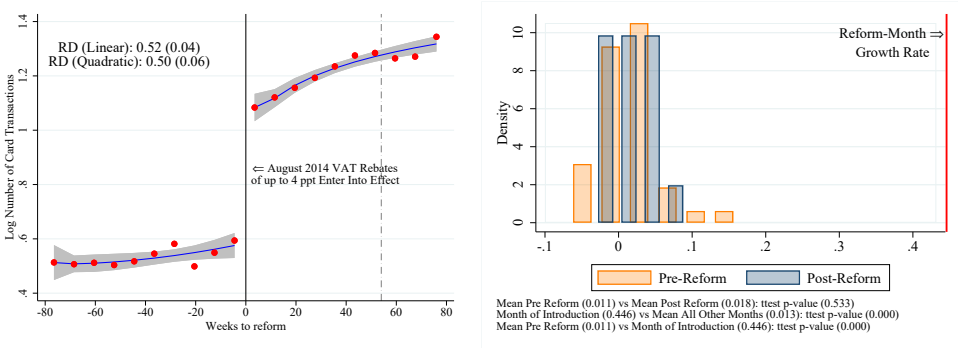


Notes: This figure documents the robustness of the RD estimations displayed in Figure 1. Each panel plots the RD estimate γ_0 from equation 1 and the 95 percent confidence intervals, for different bandwidth values (weeks to reform). Each row reports results for a different outcome, and each column presents 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.

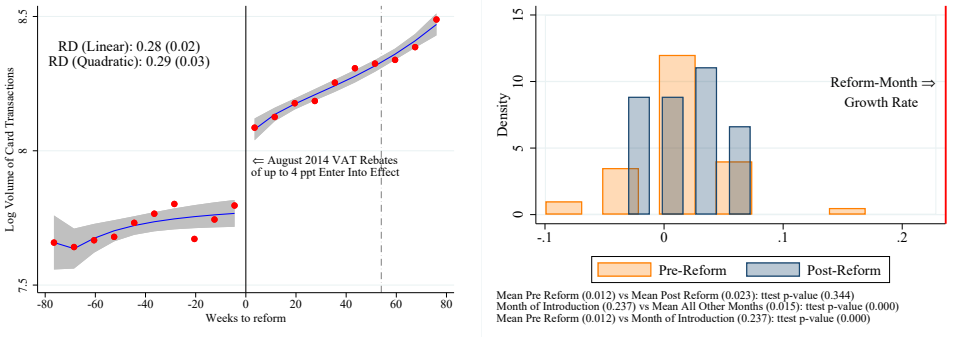
Figure B.6: Robustness of RD Estimates to Controlling for POS Subsidy Roll-Out

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

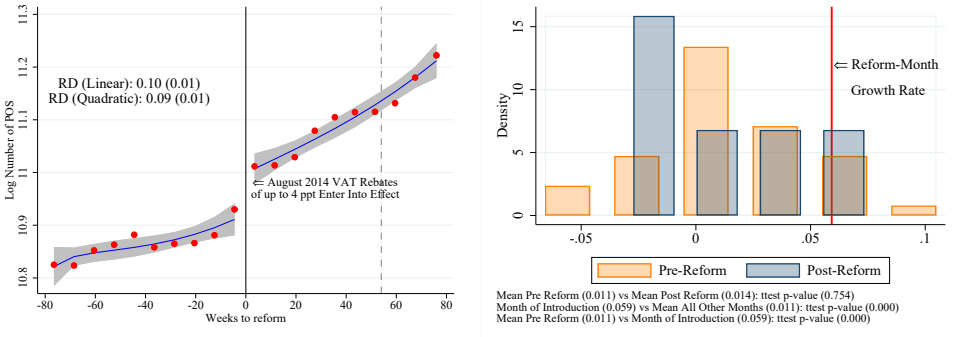
I. Number of Card Transactions



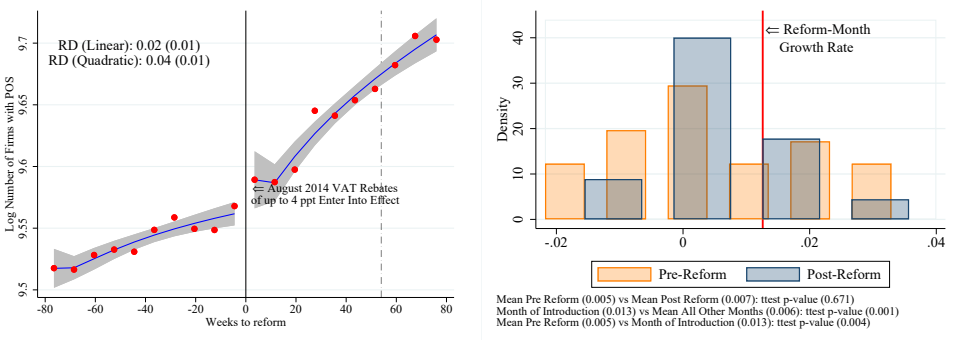
II. Volume of Card Transactions



III. Number of POS

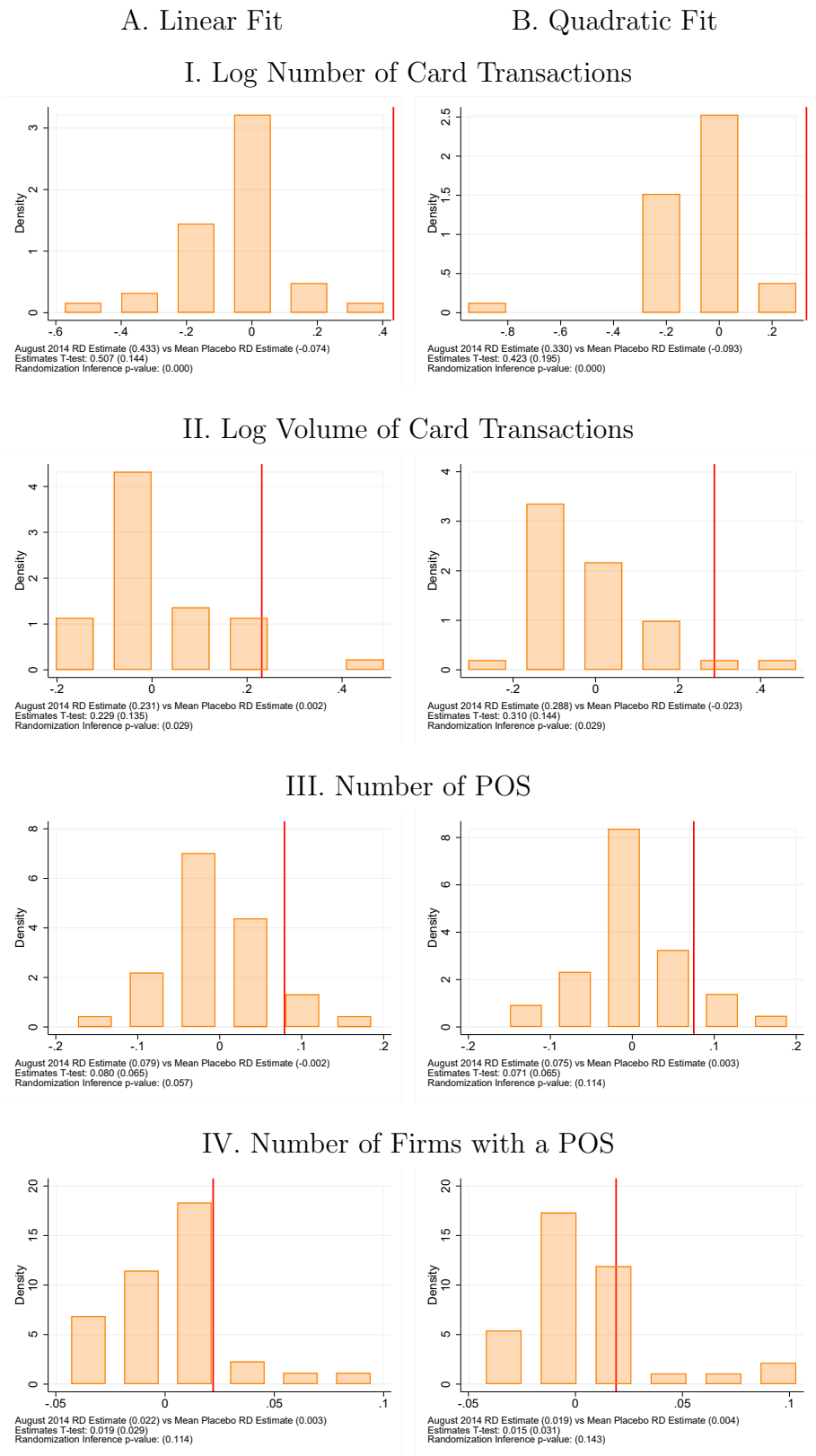


IV. Number of Firms with a POS



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.

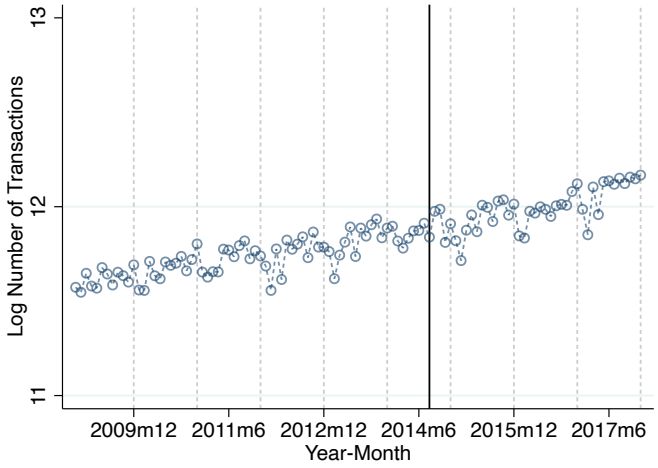
Figure B.7: Distribution of Placebo RD Estimates and Randomization Inference P-Values



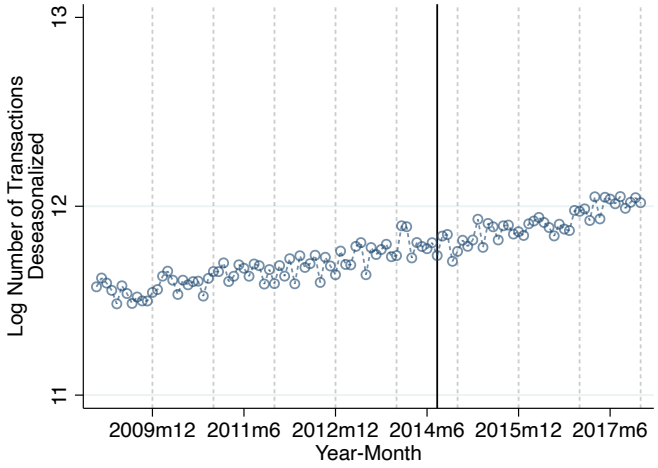
Notes: This figure shows the distribution of estimates from placebo RD estimations, using equation 1 with optimal bandwidths as per Calonico et al. (2014) and pretending the reform happened in a month other than August 2014 (one estimation per month, using all months between January 2013 and December 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)

A. Raw Data



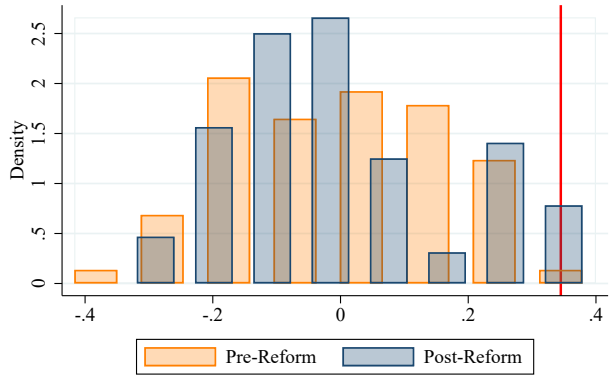
B. Deseasonalized Data



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.

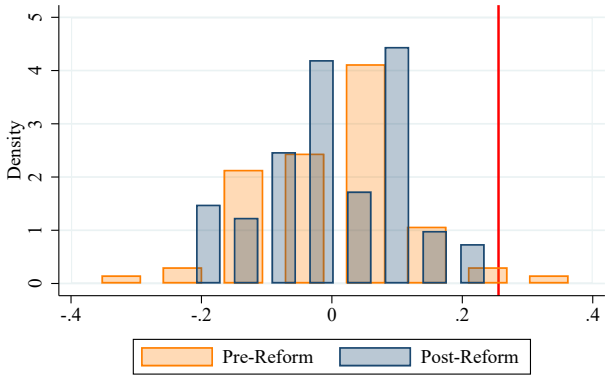
Figure B.9: Week-on-Week Growth Rates in Key Outcomes

A. Number of Card Transactions



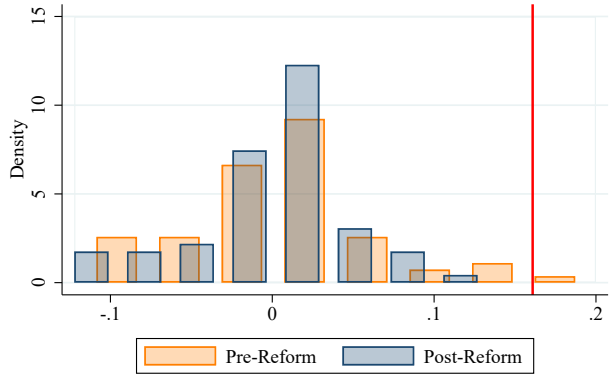
Mean Pre Reform (-0.002) vs Mean Post Reform (0.003): ttest p-value (0.868)
Week of Introduction (0.345) vs Mean All Other Weeks (0.001): ttest p-value (0.000)
Mean Pre Reform (-0.002) vs Week of Introduction (0.345): ttest p-value (0.000)

B. Volume of Card Transactions



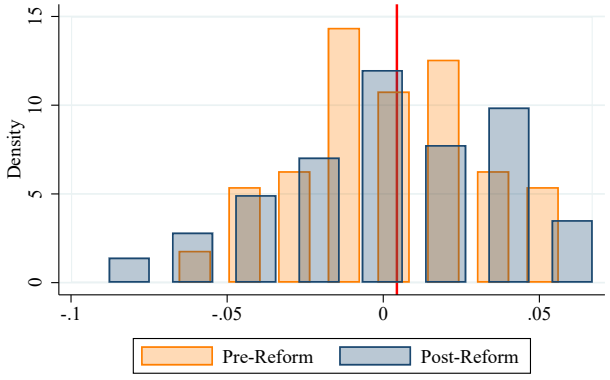
Mean Pre Reform (0.001) vs Mean Post Reform (0.007): ttest p-value (0.746)
Week of Introduction (0.256) vs Mean All Other Weeks (0.004): ttest p-value (0.000)
Mean Pre Reform (0.001) vs Week of Introduction (0.256): ttest p-value (0.000)

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)

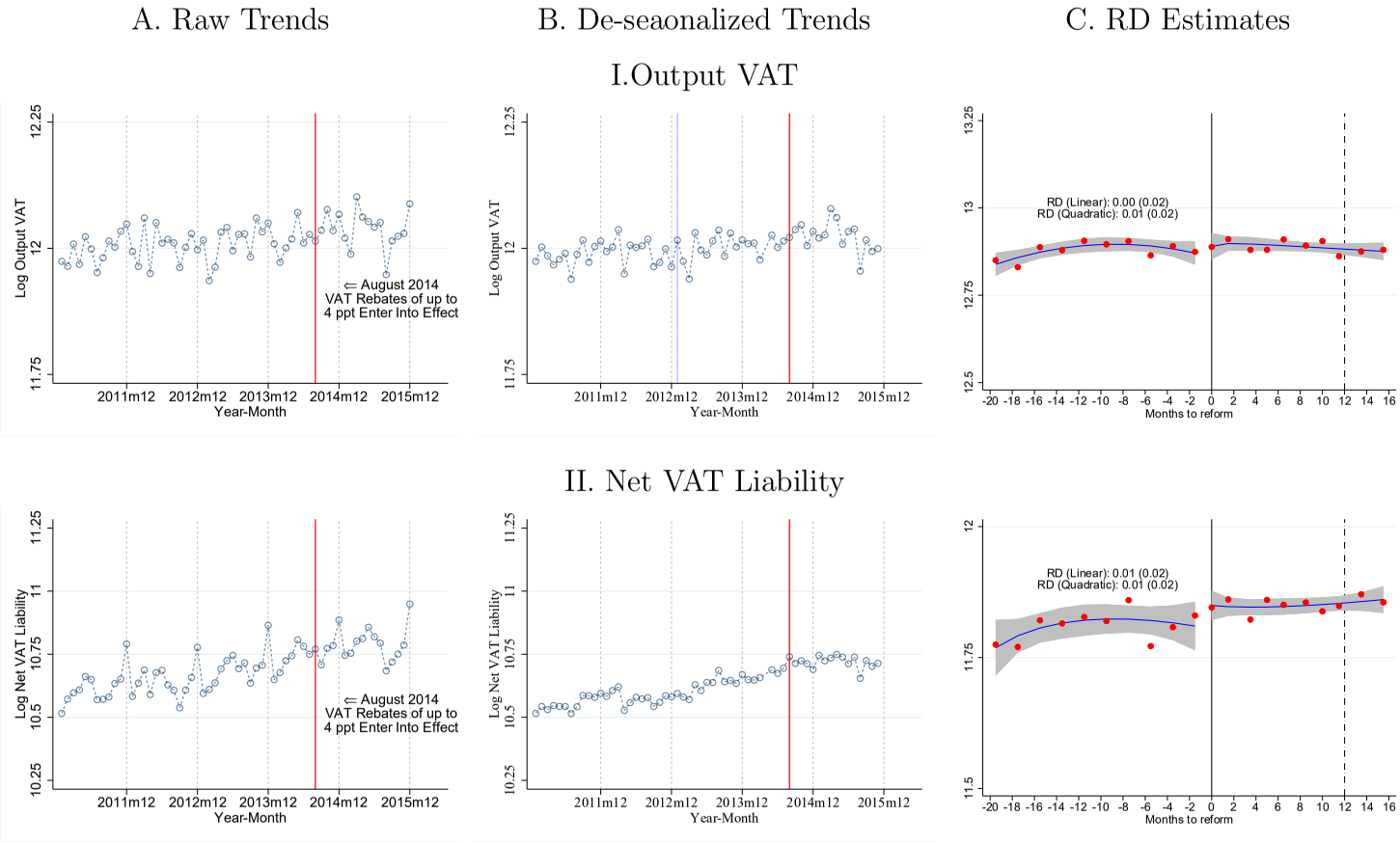
D. Number of Firms with a POS



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.

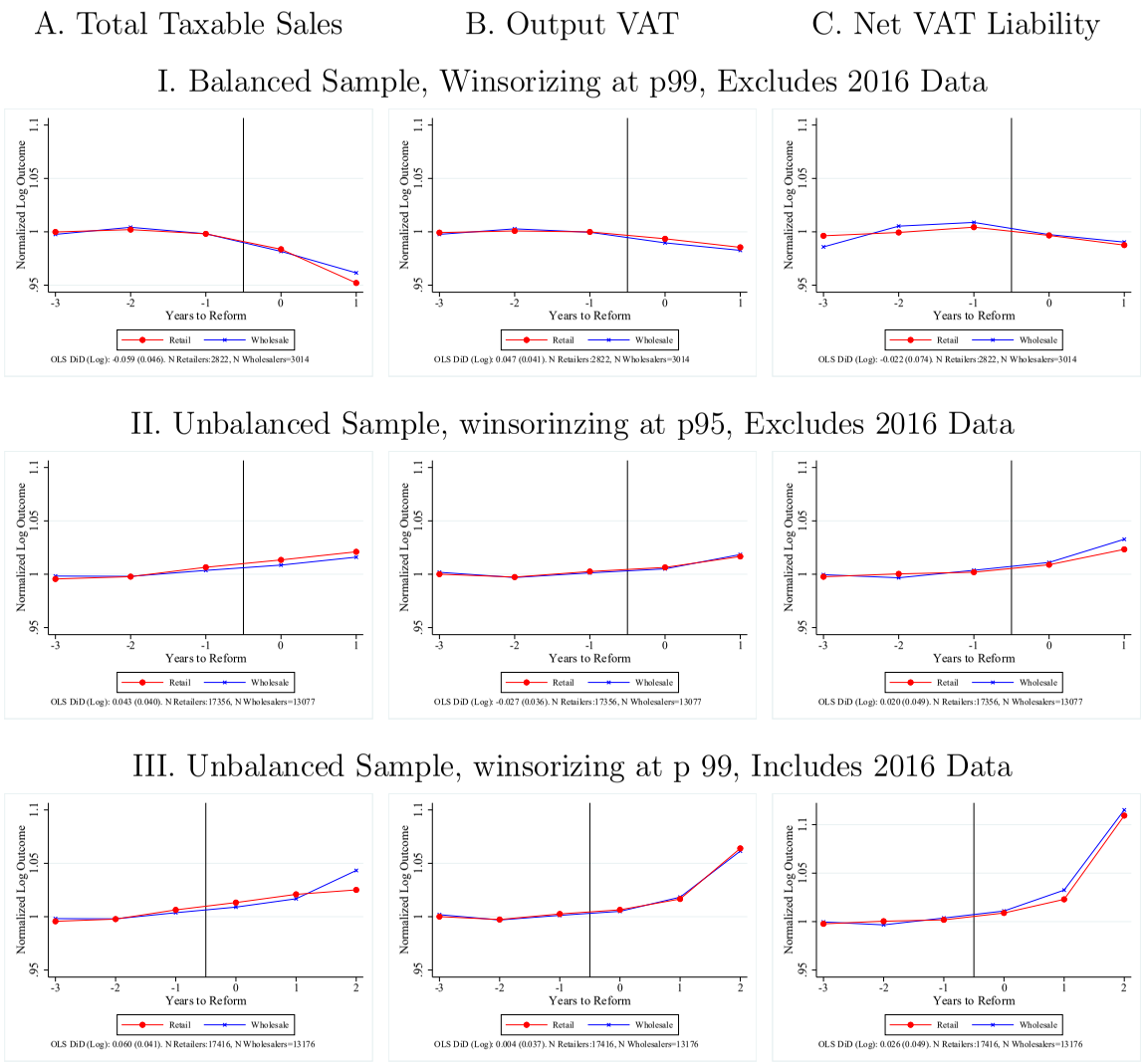
Table B.6: Robustness of RD Estimates to Prais-Winsten Correction for Autocorrelated Errors

	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.

C Difference-in-Difference Appendix

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

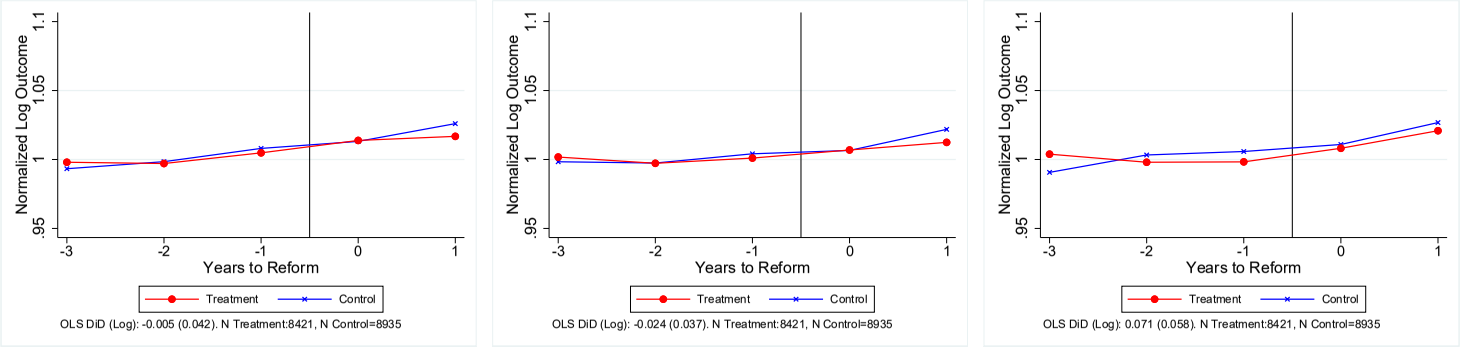


Notes: This figure is similar to Figure 3. It provides the graphical representation of the robustness tests presented in Table 1 and discussed in Section 5.3.

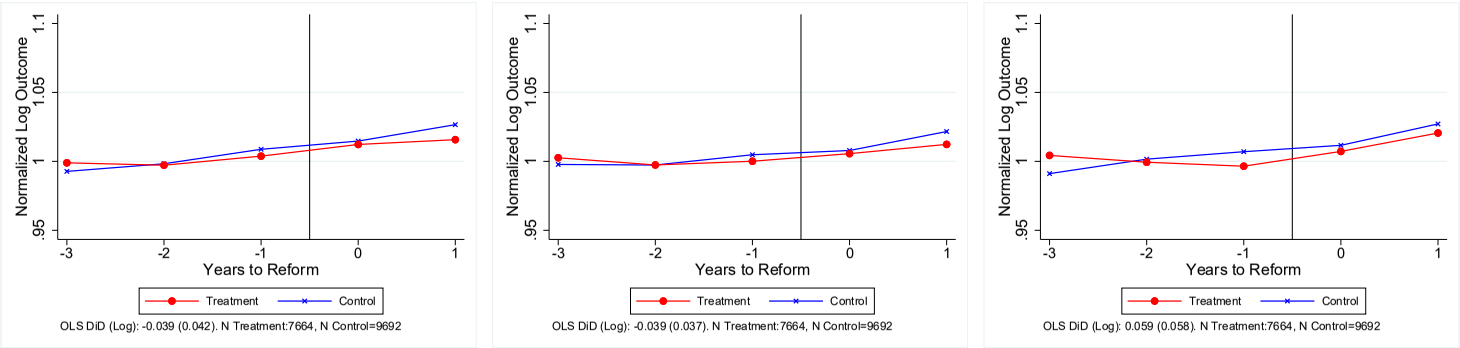
Figure C.2: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates Comparing Retail Sub-Sectors with High vs Low POS Penetration in 2013

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

I. Treated: Firms in Sectors with Above-Median Share of Firms with a POS in 2013

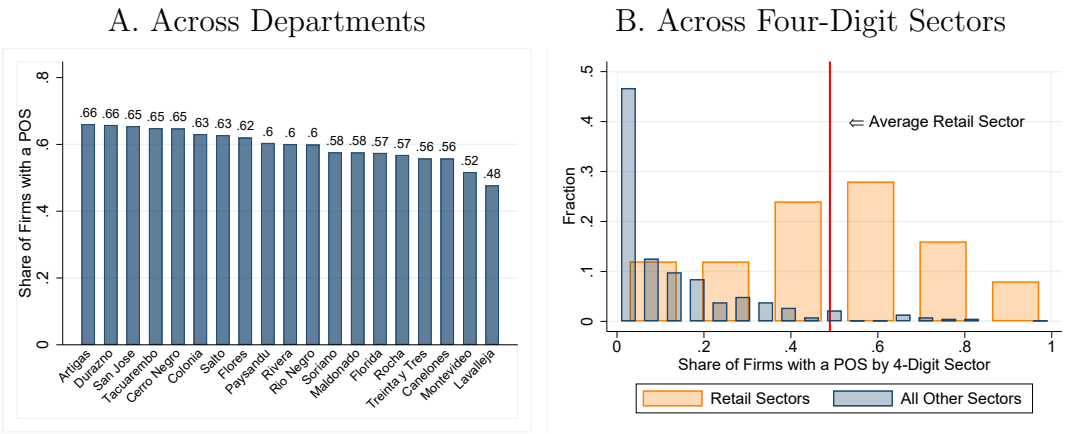


II. Treated: Firms in Sectors within the Top Quartile of the Share of Firms with a POS in 2013



Notes: These graphs implement a DiD estimation around the introduction of the VAT rebates in 2014 (year 0), comparing retail firms in sectors with high POS penetration (treated) to retail firms in sectors with low POS penetration (control). The treatment assignment is based on a sector's share of firms with a POS in 2013. Panels I and II use different cutoffs to define the treatment group, as indicated by the panel titles. Each graph shows the normalized time trends and the DiD estimate β on the $treated_i \cdot PostReform_t$ interaction from equation 2. Standard errors are robust to heteroskedasticity. This figure is discussed in Section 5.3.

Figure C.3: Distribution of Pre-Reform POS Penetration Among Retailers



Notes: This figure examines the distribution of POS penetration (share of firms with at least one POS) in the retail sector in 2013. Panel A plots the share of retail firms with a POS by department. Panel B plots the distribution of the share of firms with a POS across 4-digit sector codes. This Figure is discussed in Section 5.3.

Table C.1: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates, Annual Data

(a) Balanced Sample

	Taxable Sales		Output VAT		Net VAT Liability	
	(1)	(2)	(3)	(4)	(5)	(6)
Retailer · PostReform	-.059	-.056	.047	.037	-.022	-.065
	(.046)	(.046)	(.041)	(.041)	(.074)	(.077)
State · Year FE	No	Yes	No	Yes	No	Yes
Firm Size Decile · Year	No	Yes	No	Yes	No	Yes
N Treated	2822	2822	2822	2822	2822	2822
N Control	3014	3014	3014	3014	3014	3014

(b) Unbalanced Sample

	Taxable Sales		Output VAT		Net VAT Liability	
	(1)	(2)	(3)	(4)	(5)	(6)
Retailer · PostReform	.032	.018	-.026	-.045	.019	-.01
	(.04)	(.041)	(.036)	(.037)	(.049)	(.051)
State · Year FE	No	Yes	No	Yes	No	Yes
N Treated	17356	17356	17356	17356	17356	17356
N Control	13077	13077	13077	13077	13077	13077

Notes: This table examines the robustness of the DiD estimates from equation 2. The estimation compares retail firms (treatment group) and wholesale firms (control group) between 2011 and 2015 to capture the effect of VAT rebates for electronic purchases introduced in August 2014 in different tax compliance outcomes (as per the column titles). Estimations in panel (a) use a balanced sample of firms that file a VAT return at least once in every quarter between 2010-2016. Estimations in panel (b) include all firms that file a VAT return at any time between 2010-2016. For each outcome we display the baseline specification and an additional specification introducing group-specific (state and firm-size decile) trends as controls. The firm-size deciles are constructed using the average annual sales during the pre-reform period (2011-2013). Outcome variables are winsorized at the 99th percentile. Standard errors are robust to heteroskedasticity and clustered at the firm level. This table is discussed in Section 5.3.

Table C.2: The Effect of VAT Rebates on Tax Compliance
Difference-in-Difference Estimates, Monthly Data

(a) Balanced Sample

	Output VAT				Net VAT Liability			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Retailer · PostReform	.0278	-.0377	.0284	-.037	.0304	-.001	.03	-.0004
	(.0177)	(.0063)	(.0177)	(.0063)	(.0281)	(.01)	(.0282)	(.01)
State · Year FE	No	No	Yes	Yes	No	No	Yes	Yes
Firm Size Decile · Year	No	No	Yes	Yes	No	No	Yes	Yes
OLS (Log)	Yes	No	Yes	No	Yes	No	Yes	No
PPML	No	Yes	No	Yes	No	Yes	No	Yes
N Treated	9089	9089	9089	9089	9089	9089	9089	9089
N Control	6074	6074	6074	6074	6074	6074	6074	6074

(b) Unbalanced Sample

	Output VAT				Net VAT Liability			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Retailer · PostReform	.0159	-.0379	.0167	-.0372	.0351	.011	.0362	.0118
	(.0182)	(.0061)	(.0182)	(.0061)	(.0266)	(.0095)	(.0266)	(.0095)
State · Year FE	No	No	Yes	Yes	No	No	Yes	Yes
OLS (Log)	Yes	No	Yes	No	Yes	No	Yes	No
PPML	No	Yes	No	Yes	No	Yes	No	Yes
N Treated	14632	14632	14632	14632	14632	14632	14632	14632
N Control	10990	10990	10990	10990	10990	10990	10990	10990

Notes: This table examines the robustness of the DiD estimates from equation 2. The estimation compares retail firms (treatment group) and wholesale firms (control group) between August 2013 and August 2015 to capture the effect of VAT rebates for electronic purchases introduced in August 2014 in different tax compliance outcomes (as per the column titles). Estimations in panel (a) use a balanced sample of firms that file a VAT return at least once in every quarter between the third quarter of 2013 and the third quarter of 2015. Estimations in panel (b) includes all firms that file a VAT return at any time between 2013-2015. For each outcome we display the baseline specification and an additional specification introducing group-specific (state and firm-size decile) trends as controls. For both specifications we show the β obtained from both equations estimated via OLS and pseudo-Poisson maximum likelihood. Outcome variables are winsorized at the 99th percentile. Standard errors are robust to heteroskedasticity and clustered at the firm level. This table is discussed in Section 5.3.