Towards a Cashless Economy? Evidence from the Elasticity of Cash Deposits of Mexican Firms

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Abstract

Policies that increase the cost of using cash seek to promote the transition towards cashless and traceable transactions, but those that operate through the banking sector may also discourage participation in formal banking. We study this trade-off in the context of Mexico's tax on cash deposits (2008–2013), which levied a monthly tax on cash deposits while leaving non-cash deposits untaxed. Using account-level data on firms' deposits at a large commercial bank, we find that firms' cash deposits are highly elastic to the tax rate, while substitution towards non-cash deposits is incomplete, leading to a decrease in overall bank deposits. The elasticity of cash deposits is lower among firms in high-robbery areas, consistent with the frequent on-premises robberies reported by firms, and with high crime rates leading firms to prefer not to hold cash on site. We use firm census data to study real effects, and find that more-exposed firms reduce their economic activity in response, consistent with trying to limit the cash they hold on premises due to crime risk. Contrary to its intention, taxing cash caused financial disintermediation and had negative real effects on firms.

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1 Introduction

Economies are increasingly becoming cashless. Between 2014 and 2021, the fraction of wage earners paid in cash worldwide fell from 42% to 21% (Demirgüç-Kunt et al., 2022). Governments around the world have actively sought to encourage this trend, motivated by the idea that replacing cash—an anonymous transaction technology—with traceable transactions promises to reduce tax evasion, money laundering, and illegal activities (Rogoff, 2016). However, constraining cash as a mode of payment can also have negative economic impacts (Chodorow-Reich et al., 2020). The extent to which governments should encourage the transition towards a cashless economy by making cash more costly to use or by subsidizing electronic payments depends on the elasticity of cash use with respect to the relative price of using cash. If cash use is inelastic, then these policies might be ineffective at accelerating the digital transition, while imposing an unfair burden on small businesses and poor households.

Governments typically encourage the shift away from cash in two ways. First, they can subsidize the adoption or use of electronic payment technologies (Higgins, 2024; Brockmeyer and Sáenz Somarriba, 2025). Second, governments can increase the cost of using cash, for example by eliminating large bank notes as in India's 2016 demonetization (Chodorow-Reich et al., 2020; Crouzet et al., 2023) or by taxing cash when it is deposited and/or withdrawn from a bank (Coelho et al., 2001). While there is a large body of literature on the potential advantages and disadvantages of monetary and fiscal policies which impact cash (Coelho, 2009; Rogoff, 2016), there is little credibly-identified evidence, particularly on policies that explicitly aim to make cash more costly to use. This is largely due to a combination of uniform national policies enacted at a single point in time and a lack of firm-level data that distinguishes between cash and non-cash means of payment.

We leverage confidential account-level microdata on firms' deposits at a large Mexican bank and exploit a policy which sought to combat the informal cash economy by instituting a tax on cash deposits (while leaving non-cash deposits untaxed). The combination of these novel data and a tax exemption threshold allow us to create firm-level variation in exposure to the tax based on a firm's pre-tax intensity of cash usage, which we use as our identifying variation. Our results show that cash deposits are highly elastic to an increased tax rate, with the reduction in cash deposits being only incompletely replaced by an increase in non-cash deposits, resulting in financial disintermediation in the form of a drop in overall deposits. Furthermore, the tax on cash deposits causes some firms to stop depositing cash altogether. These results suggest that policies raising the cost of using cash may have important efficiency costs, and that subsidizing electronic payments instead may be better suited to encourage the transition to a cashless economy.

In July 2008, Mexico introduced the "Impuesto a los Depósitos en Efectivo" (IDE), a tax on cash deposited in bank accounts. The objective of the tax was to discourage the use of cash and

limit informal and illegal activity. Given the objective to encourage the transition towards digital payments, the tax did not apply to non-cash deposits (e.g., through point-of-sale terminals, mobile and online payments, and account-to-account transfers). Initially, the tax applied a rate of 2% to the flow of cash deposits in a given month in excess of 25,000 pesos (approximately 2,500 US dollars at the time), summed across the firm's bank accounts. In January 2010, the tax increased to a rate of 3% of the monthly flow of cash deposits in excess of 15,000 pesos (approximately 1,200 US dollars at the time). The tax was repealed in January 2014.

We show that firms react strongly to the tax. First, firms bunch sharply at the monthly exemption threshold; furthermore, the bunching follows the movement of the threshold when it is reduced from 25,000 pesos to 15,000 pesos and disappears once the tax is repealed. Second, while firms' average cash deposits were on a downward trend until December 2013, they increased by 40% in the first month after the repeal, kept increasing throughout 2014 and 2015, and then leveled off in 2016–2017 at almost double their end-of-2013 level.

To go beyond the descriptive evidence, we implement an identification strategy which combines the existence of the exemption threshold with varying baseline (pre-tax) intensity of cash usage across different industries by state. We focus on the repeal of IDE because we have monthly firm-level data from 2011 to 2018, but only have data for limited months prior to 2011. To illustrate our strategy, consider two industry–state groups: corner stores in Chiapas and medical service providers in Mexico City. For a given amount of total deposits, corner stores in Chiapas—which are heavily reliant on cash—face on average a much higher tax rate on cash deposits than firms providing medical services in Mexico City—which are much less reliant on cash and thus often below the IDE exemption threshold for monthly cash deposits. Specifically, the predicted average tax rate faced by corner stores in Chiapas (1.4% under the 2010–2013 tax schedule) is substantially larger than that faced by medical services in Mexico City (0.4%).[1]

We operationalize this identification strategy in an instrumental variables (IV) regression by using a simulated instrument (Gruber and Saez, 2002). Our simulated instrument captures the mechanical variation in effective tax rates induced by IDE (and its repeal) by applying the pre- and post-repeal tax schedules to each firm's cash deposits measured in the only month for which we have data before the tax existed (December 2006). We assign firms the average simulated rate in their industry–state, in order to limit the issues of mean reversion that arise when using a firm's pre-tax behavior to determine the tax rate they would pay if they did not respond to the tax (Weber, 2014). Consider a firm that experiences an idiosyncratic negative shock to their cash deposits in the pre-tax period: if the analysis is at the firm level, that firm will be assigned a lower-than-steady-state

[1]These differences in cash use across industries are similar in the US: according to the Diary of Consumer Payments, 13% of payment volume at grocery and convenience stores is made in cash, compared to 3% of payment volume for medical services.

predicted tax rate, and their reversion to the mean would be incorrectly attributed to an effect of the tax. By averaging across firms within industry–state, these idiosyncratic shocks are averaged out as long as they are mean-zero and not correlated across firms.

Even after averaging the simulated rate at the industry–state level, the instrument may not satisfy the exclusion restriction if firms with different initial deposit levels in 2006 follow different trends for reasons unrelated to the IDE reform. We make two adjustments to address this concern. First, because a firm's simulated rate rises mechanically with the size of its cash deposits, we residualize it on firm size: we regress each firm's simulated pre-repeal rate on the log of its total deposits in 2006 and use the residual as the instrument. Second, to address differential pre-trends, we estimate separate linear trends in each deposit outcome over the pre-repeal period (2011–2013) for firms above and below the median simulated tax rate and subtract the fitted trends throughout the sample period (including the post-repeal period). Event studies confirm that our main specification, which incorporates these adjustments, compares groups of firms that differ in their simulated tax rates yet exhibit parallel trends in deposit behavior before the repeal.

Turning to the IV, the results point to a very large and precisely estimated elasticity of cash deposits with respect to the tax rate. The average firm, which faces a 0.9 percentage point reduction in the tax rate following the IDE repeal, sees a 246% increase in cash deposits. Following the repeal, the average firm simultaneously decreases non-cash deposits by 27%. The non-cash substitution is incomplete, meaning that the average firm increases overall deposits following the tax repeal by 9%. The increase in total deposits after the repeal is likely from cash or assets that firms held outside of the banking system due to the tax on cash deposits.

We find heterogeneous responses to the IDE tax across location, industry, and firm size. For a given increase in the tax rate, firms in municipalities with higher rates of business robbery reduce cash deposits less, consistent with the greater risk of holding cash outside the bank. We observe the same pattern in more populous municipalities and in those with higher debit card penetration, where easier access to formal banking may make firms more willing to continue depositing cash in spite of the tax. Retailers reduce cash deposits more and increase non-cash deposits more than firms in other industries, possibly because they can steer customers between payment methods at the point of sale (Higgins, 2024; Gertler et al., 2025). Larger firms also reduce cash deposits more, perhaps because the fixed costs of changing how they handle cash are small relative to the volume of transactions affected by the tax.

By virtue of our estimation being based on transaction data from a bank, we can immediately interpret the reform-induced changes in deposit behavior as showing that IDE affected the financial choices of firms. Did IDE also have real effects, distorting firms' economic activity? Given that firms prefer to deposit cash at banks in the absence of the tax, but reduce overall bank deposits in the presence of the tax, how do they respond to holding more cash while the tax is in effect?

If firms receive some of their payments in cash that cannot be shifted to non-cash instruments, and if they preferred depositing that cash in the bank to avoid the high crime risk in Mexico—consistent with 14% of firms reporting being robbed on premises annually (INEGI, 2014) and with the heterogeneous elasticities of cash deposits by crime levels—they may have adjusted their real economic activity in order to avoid holding more cash while IDE was in place.

We study the real, economic effects of IDE using data from Mexico's Economic Census of firms and the same instrument at the industry–state level. We find that firms that are more exposed to the tax have 12% lower revenues and 13% lower non-labor input costs while IDE is in place compared to post-repeal, relative to less-exposed firms. To validate our identification strategy in the firm census data, we conduct a placebo test imposing the IDE tax and its repeal across pre-tax years, and do not find systematic effects on revenues or input costs. These results are consistent with the heterogeneous elasticities of cash deposits in high- and low-crime areas and with high rates of on-premises robberies reported by firms, suggesting that firms prefer to keep cash in the bank to avoid crime risk, and that they reduced economic activity to avoid that risk when a tax on cash deposits incentivized them to hold more cash on premises.

Our paper makes three main contributions to the literature. First, our results contribute to the literature on the impacts of policies to combat the informal economy, by measuring the financial and real effects of a policy aimed at increasing the cost of using cash. Policies to combat the informal economy typically seek to either increase the cost of cash (Rogoff, 2016) or subsidize the cost of non-cash payments (Higgins, 2024; Brockmeyer and Sáenz Somarriba, 2025). These costs and subsidies can operate through tax policies (including taxes on cash deposits and withdrawals), monetary policies (including restricting the currency denominations in place), or regulation (including mandating non-cash payment for transactions above certain values). While numerous policy articles review current practices across countries (Coelho et al., 2001; OECD, 2017; Awasthi and Engelschalk, 2018), there is limited credibly-identified evidence on the effects of these policies. One exception is Chodorow-Reich et al. (2020), who exploit variation across districts in exposure to the Indian demonetization shock to study its impacts on financial behavior and output. While Chodorow-Reich et al. (2020) study a monetary policy, our paper focuses on the financial and real effects of a tax policy. To the best of our knowledge, we provide some of the first well-identified causal evidence on a tax explicitly aimed at increasing the cost of cash.

Second, we use account-level data from firms' bank accounts to estimate the elasticities of firm behavior with respect to changes in the costs of different payment methods. These elasticities are key to understanding not only the effects of policies that change the relative costs of payment methods to encourage the transition to a cashless economy, but also outcomes in payment markets. In particular, they matter for the welfare effects of competition between payment networks (Wang, 2025) and for who bears the costs of credit card rewards, which depends on how merchants pass

payment costs through to prices (Egan et al., 2026). Several studies use bank account-level data in other ways, including to study *households'* consumption and savings behavior (Gelman et al., 2014; Baker, 2018; Olafsson and Pagel, 2018; Ganong and Noel, 2019; Bachas et al., 2021; Ganong et al., 2024) and savings elasticities (Karlan and Zinman, 2018; Schaner, 2018). Others use data on corporate loans (Khwaja and Mian, 2008; Schnabl, 2012; Chodorow-Reich, 2014; Morais et al., 2019), firm balance sheets (Restrepo et al., 2019), or firm bank accounts (Mester et al., 2007; Norden and Weber, 2010; Kim et al., 2025; Ghosh et al., 2026) to study firm-level financial and real outcomes, but not elasticities with respect to the costs of different payment methods. We combine account-level data from firms' bank accounts with a policy that taxed cash to measure these elasticities.

Third, our paper contributes to the growing literature in public finance that studies tax capacity in developing countries (Besley and Persson, 2014), by showing how firms respond when the government uses financial institutions as third-party information and withholding agents. This literature has documented the importance of third-party institutions that report information to the government and possibly withhold taxes (Kleven et al., 2011)—including business partners (Pomeranz, 2015), consumers (Naritomi, 2019), and employers (Jensen, 2022). Financial institutions are routinely used as third-party agents for tax administrative purposes around the world; the potential tax enforcement benefits are explored theoretically in Gordon and Li (2009). Our project provides empirical evidence on the financial information trail: although a shift toward electronic payments can raise the sales that firms report to the tax authority (Das et al., 2023), activating this trail has efficiency costs, as firms respond by reducing their attachment to the banking sector.

From a policy perspective, our results suggest that policies to encourage the transition towards a cashless economy by making cash more costly to use have two unintended consequences: firms continue using cash but remove it from the formal financial sector, and they reduce their economic activity to avoid the robbery risk of holding more cash on premises. Alternative policies to encourage the cashless transition, such as subsidizing the adoption of electronic payment technologies, are likely to be more effective. For example, Uruguay subsidized the adoption of point-of-sale terminals by small businesses (Brockmeyer and Sáenz Somarriba, 2025), while Mexico provided debit cards to low-income households, which led to an increase in point-of-sale terminal adoption by small businesses (Higgins, 2024). More broadly, when taxpayers can easily avoid a tax, its effects can differ sharply from its statutory design (Bachas et al., 2024): a tax intended to reach the informal cash economy led firms to move cash out of the banking system, where it is harder for the government to observe.

2 Policy Environment and Data

2.1 Policy Environment

Mexico has a large informal sector: 57% of workers were informal as of 2017 (INEGI, 2017). The economy is also largely cash-based: 37% of Mexican adults had a bank account in 2017, compared to 69% of adults worldwide. Similarly, 32% of Mexican adults had made or received a digital payment in the last year compared to 52% worldwide (Demirgüç-Kunt et al., 2018). On the supply side of the market, 24% of retailers had adopted point of sale terminals in 2011 (Higgins, 2024).

Mexico has implemented several policies to encourage the transition away from cash. In addition to the 2008 tax on cash deposits that we study here, the government's main cash transfer program for poor households provided bank accounts to cash transfer recipients in 2005 and provided debit cards to them starting in 2009 (Bachas et al., 2018, 2021), the National Chamber of Commerce subsidized firm adoption of POS terminals in 2015, and the Central Bank launched a digital payments system for retail transactions in 2019 and another for peer-to-peer transfers in 2023.

The tax on cash deposits ("Impuesto a los Depósitos en Efectivo") was introduced in July 2008. IDE applied a 2% tax rate to flow of monthly cash deposits in excess of 25,000 pesos (2,500 US dollars at the time). Non-cash deposits—including electronic transfers (through point-of-sale terminals, mobile and online payments, and account-to-account transfers) and cheques—were exempt. The tax applied to deposits across all bank accounts, as banks sent all information to the bank regulator. This should permit aggregating information across bank accounts, and prevent a firm or individual from avoiding the tax by making cash deposits in accounts across multiple institutions to fall under the threshold within each institution.[2] The government's motivation was that IDE would de-facto tax informal businesses and illegal activity, thereby encouraging the use of digital payments.[3] For large firms paying the corporate income tax, the tax could be credited against other tax payments, and therefore acted as a "withholding tax" or "control tax." If the firm did not take the steps to claim the credit, the IDE levy constituted a final payment for firms. For

[2]In practice, it is not clear how systematic this check was, and one can find online forums that recommended opening accounts in multiple banks to limit tax payments. Nevertheless, firms attempting to avoid the tax in this way cannot explain our results for four reasons. First, we see an *extensive margin* effect on cash deposits, which would not occur in the multiple-banks approach, as firms would need to spread cash deposits across multiple banks. Second, nearly all small firms like those we study here only have accounts with one bank (Competition and Markets Authority, 2016). Third, we would be highly unlikely to see real effects if this were the margin on which the firms in our sample were adjusting. Fourth, since we drew our sample during the period in which the tax was in place and study its repeal, if small firms did respond on this margin we would be unlikely to see such a large impact of the repeal on cash deposits: while firms whose primary account was in our sample would increase cash deposits post-repeal, this would be offset by firms whose secondary account was in our sample, as the latter group would *decrease* cash deposits at our bank after the tax was repealed (because they would return to depositing cash only at their primary bank).

[3]In 2005, India and Pakistan both introduced taxes on daily cash withdrawals above 25,000 rupees: India at 0.1% until 2009, and Pakistan at 0.1%, raised to 0.3% in 2008 (Coelho, 2009). India's finance minister justified the tax on the grounds that "these cash withdrawals leave no trail and presumably become part of the black economy."

the majority of micro, small, and medium enterprises, the tax was final: informal firms had no tax liabilities against which to credit it, and those registered with the tax authority typically were under the simplified regime for small taxpayers (REPECOS), which did not allow the credit.[4] Financial institutions were liable for withholding the tax directly from their clients' accounts and remitting the tax to the government. The tax was salient since the bank had to notify their clients by mail every month for which IDE was collected.

In January 2010, the government increased the tax rate to 3% and lowered the threshold to 15,000 pesos (1,500 US dollars at the time). Finally, in January 2014, IDE was repealed, as part of a tax reform that also increased the top marginal tax rate on individuals. The government's stated reasons were to simplify the tax system and reduce compliance costs, although the unpopularity of the tax likely also played a role. Notably, the repeal did not eliminate the information component of the tax: banks continued to report clients' monthly cash deposits to the tax authority post-repeal. Figure A1 provides the timeline of the policy's introduction, changes, and repeal.

The policy path of IDE shares many parallels with prior experiences of bank transaction taxes. Beginning in the 1980s, various governments in Latin America and Asia implemented bank transaction taxes, which raised meaningful amounts of revenue in the short term; this revenue eroded over time, however, as firms and individuals found ways to avoid the tax (Kirilenko and Summers, 2003; Coelho, 2009).[5] In turn, governments responded by raising the rate, but the tax base then tended to contract even further, and many of these taxes were repealed after a few years (Baca-Campodónico et al., 2006; Matheson, 2011).

At the same time, IDE is distinct from these past policies in several ways. First, its rate of 2–3% was four to six times the average rate of earlier bank transaction taxes in Latin America (Baca-Campodónico et al., 2006). Second, and perhaps related, the tax base was more narrow: IDE exempted non-cash transactions, focused on deposits, and allowed taxpayers with income tax liabilities to credit the levy against them; most previous bank taxes had limited exemptions, were levied on withdrawals, and could not be credited against other taxes. Third, governments typically introduced bank transaction taxes as temporary, emergency revenue measures during economic crises (Coelho et al., 2001). While it may have raised substantial revenue, IDE was ultimately implemented with the more narrow objective of combating the cash economy. Indeed, compared to previous bank transaction taxes, IDE levied a much higher rate on a much narrower base, with the ability for formalized firms to credit the tax paid; such design features were clearly intended to discourage informal cash activities, rather than serve as a broad-based revenue tool.

[4] The simplified tax regime REPECOS levied a fixed payment based on the firm's estimated sales and did not allow any tax deductions. Thus, firms in REPECOS could not credit their IDE payments against other tax liabilities.

[5] Average revenue across such taxes in Latin America was 0.9% of GDP (Baca-Campodónico et al., 2006).

2.2 Data

2.2.1 Bank Data

We obtained panel data from the bank accounts of a random sample of micro, small, and medium enterprises (MSMEs) from a large Mexican commercial bank. The data consist of a monthly panel of deposits and withdrawals by category between April 2011 and December 2018.[6] This panel is complemented with data for the month of December in 2006, 2008, 2009 and 2010. This unusual data structure is due to the storage processes of the bank: as required by law, it had kept all data going back several years when we made our initial data request. The bank also had kept specific months of data from older years for audit purposes. The data allow us to observe the monthly value and number of deposits and withdrawals by each type of deposit and withdrawal that can be made at the bank. The data also contain information on the geographic location and industry of the firm. The data is at the bank account by month level; for firms with more than one bank account, we aggregate the data to the firm by month level, the level at which the tax applies. The data from 2006 allow us to measure deposit behavior that is not contaminated by the presence of the tax; we leverage this to create instruments.

Our main sample of interest consists of firms with non-missing total deposits (either zero or positive) in every year between 2011 and 2017, and excludes firms whose industry we do not observe.[7] When overlapped with the timeline of IDE (Figure A1), this sample provides us with the opportunity to comprehensively study the repeal of the tax in 2014. This sample contains 885,080 firm–year observations, with 126,440 unique firms. Table A1 provides summary statistics for this sample, separately for the periods prior to and after the repeal.

2.2.2 Economic Census of Firms

To measure real effects of firms holding more cash as a result of the tax on cash deposits, we turn to the Economic Census by Mexico's National Statistical Institute, INEGI. We primarily use data from the 2013 and 2018 census waves to measure outcomes before and after IDE's repeal, mimicking our analysis with bank account level data.[8] In robustness and placebo checks, we add additional

[6] More specifically, a stratified random sample of 200,000 client firms as of December 2012 was drawn: 100,000 "legal entities" and 100,000 individuals with an economic activity. We have data on all 333,192 accounts held by these firms as of that month. A panel of these firms' accounts was then constructed using all available observations for those accounts from 2011–2018. These data originally also include large firms and government entities, which could either fully credit the tax against their other tax liabilities or were exempt from it, respectively; we exclude these from our analysis, therefore leaving a sample of 176,884 MSMEs.

[7] A firm would have missing observations if all of its accounts are terminated, either at the firm's request or for administrative reasons. Conditioning on non-missing deposits and industry reduces the sample from 176,884 to 126,440 firms.

[8] These waves were conducted in 2014 and 2019 with survey questions asking about revenues and costs over the 2013 and 2018 calendar years. We refer to the census waves by the year about which the questions are asked to avoid confusion around the timing of the surveys relative to IDE's repeal.

years of data. In one specification, we add data from the 2008 wave, as IDE was introduced mid-2008, and find similar results. In another specification, we implement the normalized outcomes recommended by Chen and Roth (2024) using the 2003 wave as a pre-tax baseline year. In placebo tests designed to validate our identification strategy and mimic a parallel pre-trends test, we use data from the 1993 and 1998 waves and impose the 2013 and 2018 tax structures on those years as placebos.

INEGI's Economic Census collects data every five years on the universe of firms in Mexico (including informal firms), with 4.2 and 4.8 million firms in the 2013 and 2018 waves that we use for our main analysis. The survey asks questions about each component of revenues, labor costs, and non-labor input costs over the full year from January to December of the prior calendar year. We construct a balanced sample of firms across the 2013 and 2018 waves of the Economic Census for our main analysis and restrict to micro, small, and medium enterprises (omitting large firms), leaving us with a balanced panel of 2,364,624 firms. For the placebo test, we use a balanced panel of 833,812 firms in the 1993 and 1998 waves.[9]

2.2.3 Other Data Sources

We complement our analysis with several publicly available datasets. We use a postal code–municipality mapping from Mexico's Postal Service to map each firm's postal code in the bank data to their corresponding municipality. We use the National Statistical Institute's 2010 locality-level aggregate statistics from the 2010 Population and Housing Census to measure municipality population and split firms at the median municipality population across the firms in our sample. We use crime data from the Executive Secretariat of the National Public Security System to measure the rate of business robberies per capita by municipality. Finally, we use data from the National Banking and Securities Commission to measure municipality-level debit card penetration, as in Higgins (2024).

2.3 Potential Margins of Response

Now that we have outlined the IDE's features and our data, we are in a position to summarize the predicted margins of response of account holders.

In response to a tax rate on cash deposits, firms may respond by lowering the amount of cash deposited. For firms whose monthly cash deposits were below the threshold prior to IDE, there are no changes to the cost of deposits; indeed, this intuition will serve as the starting point for our

[9] We follow the employee thresholds in the Mexican government's official definition of large firms, which are over 100 employees in retail, wholesale, and services, and over 250 employees in industry (manufacturing, mining, utilities, and construction); see https://dof.gob.mx/nota_detalle.php?codigo=5096849&fecha=30/06/2009. We drop all fishing and aquaculture firms since there is no official government definition of large firms for this sector. The data include an anonymized firm identifier to link the firm across census waves beginning in 2008. For our robustness and placebo tests using earlier data, we use the anonymized identifiers generated by Busso et al. (2018), who validate their linking on a non-anonymized version of the data.

identification strategy. All firms with deposits above the threshold are affected, and may lower the amount of cash deposits—incrementally, or sufficiently to fall below the exemption threshold, or even all the way to making zero cash deposits. Since the IDE tax applies to cash deposits summed across all institutons, re-allocating the reduced cash deposit from one institution's bank account to another institution's bank account does not, in principle, help to avoid the tax. Instead, the reduced cash deposits can either be transformed into non-cash deposits, or held outside the banking system. In the latter case, while depositing the cash in offshore accounts is theoretically possible, it is unlikely that most of the firms in our sample have the ability to conduct such complex operations, unlike large investors and firms in developed countries (Umlauf, 1993; Galindo and Majnoni, 2006).

Instead, reduced cash deposits that are not converted into non-cash deposits are likely to be held, perhaps physically, outside the banking system.[10] The reduction in cash deposits, and the substitution to non-cash, can occur both on the intensive margin (for firms that continue to make cash deposits after IDE is introduced) and on the extensive margin (for firms that respond to IDE by no longer making any cash deposits). If there are important fixed costs to making cash deposits, for example having to physically travel to a bank branch, the introduction of IDE and its repeal may generate substantial responses on the extensive margin.

For the vast majority of firms in our sample, IDE is a final tax as it cannot be credited against other tax payments. That is, the IDE tax rate can be directly interpreted as a transaction cost on deposits for most firms. It is possible that there are additional, indirect transaction costs embedded in IDE. The idea of IDE as a control tax is that formalization is not achieved by the IDE tax payment per se, but rather from the provision of information on firms' activities to government through the centralized reporting of firms' aggregated cash deposits: governments have used taxes like IDE to obtain information otherwise protected by bank secrecy laws (Coelho, 2009). In practice, the extent to which IDE achieves this objective depends both on the extent to which the tax authority in charge of enforcement actually uses the newly provided information and the extent to which firms perceive that the new information sources are being used for enforcement. It is challenging to establish the existence and magnitude of this potential indirect cost—both because it is not clear how much the IDE information trails were used for enforcement, and because perceptions of enhanced enforcement may vary across firms in our sample.[11] At the same time, our identification strategy relies on incremental differences between firms in the extent to which they are subject to

[10] Firms that receive cash irregularly could also shift cash deposits across months to stay below the monthly threshold. Because this shifting within a calendar year leaves annual deposits unchanged, our annual estimates measure changes in the amount of cash that firms keep in the banking system net of any such shifting, which nevertheless implies holding cash on premises for longer than they would have liked.

[11] Closer enforcement may also benefit firms through other channels: De Simone (2022) finds that firms audited more closely by the tax authority obtain more and cheaper bank credit.

IDE; the extent and perception of these indirect costs may vary less between firms that differ slightly in their IDE tax exposure than, say, between firms that are exposed to IDE versus not exposed at all. In other words, while indirect costs are plausible in this setting, we will interpret our results as mainly reflecting responses to changes in the direct transaction cost of making cash deposits.

By virtue of using transaction data from a financial institution, we can immediately interpret any reform-induced changes in deposit behavior as reflecting efficiency costs. That is, any observed responses indicate that IDE distorts financial choices of firms. Reduced cash deposits that are not transformed into non-cash deposits indicate financial disintermediation, which can have significant economic costs. Substitution into non-cash deposits may be viewed favorably in some ways, including as it relates to tax compliance and financial development in the long run, but ultimately it reflects a distortion in the present, as IDE introduces a price-wedge between forms of deposits that distort the firm's optimal allocation of deposits between cash and non-cash. Our estimation of individual responses reflect these efficiency costs to individual firms.

At the level of the firm, these financial distortions may have real effects on firms' economic choices, including input purchases and sales: we study these economic margins of impact in Section 5. At a more macroeconomic level, these individual responses may aggregate into additional distortions to the economy (Coelho, 2009). One macroeconomic channel is that IDE, like other bank transaction taxes, is levied on gross transactions, so the tax accumulates at each stage of production in which firms pay one another through banks (Arbeláez et al., 2005; Matheson, 2011). Another macroeconomic mechanism is that, if the financial disintermediation response is sufficiently pronounced, taxes like IDE impact the availability of funds inside the banking system given that ..., which can negatively affect banks' operating processes and ultimately distort aggregate investment and borrowing by both firms and the government (Coelho, 2009). Establishing these macroeconomic impacts of IDE are beyond the scope of this paper; however, our results below indicate that the deposit responses to IDE by individual firms are very large, including for total deposits, which may potentially trigger some of these macroeconomic impacts.[12]

3 Descriptive Evidence

3.1 Bunching at the Exemption Threshold

To motivate our analysis, Figure 1 shows the density distribution of monthly of cash deposits. In Panel (a), we compare density distributions prior to the introduction of IDE (using data from December 2006) and after IDE was introduced in mid-2008 (using data from December 2008). In Panel (b), we compare distributions prior to the January 2010 reform that lowered the threshold

[12]Descriptive studies using macroeconomic time series show that cash holdings rose substantially, both in levels and relative to bank deposits, after countries introduced financial transaction taxes (Lozano Espitia and Ramos Forero, 2000; Arbeláez et al., 2005; Valero Varela and Sierra Reyes, 2007; Coelho, 2009).

from 25,000 pesos to 15,000 pesos (using data from December 2009) and after the threshold was lowered (December 2010). In Panel (c), we compare distributions prior to IDE repeal (2013) and after (2014). In all periods where IDE is active, we observe an excess mass of monthly cash deposits below the exemption threshold, and in particular a spike (bunching) at exactly the threshold. This bunching is not present prior to IDE and disappears following IDE's repeal. Moreover, the bunching moves as the threshold moves (from 25,000 pesos to 15,000). Given our data coverage, our main estimation will focus on the tax rate changes induced by the repeal of IDE in January 2014. The distribution in Panel (c) shows that firms' cash deposits are responsive to the repeal of the tax: for the 14,001–15,000 cash deposit interval, the density of deposits is more than twice as large in 2013 (prior to the repeal) than in the post-repeal year of 2014.[13] These density distributions are directly drawn from the raw data and provide prima facie evidence of a causal effect of the tax on deposit behavior.

However, our context does not satisfy the conditions required to apply conventional bunching estimators, which are accurate when the excess mass is precisely concentrated around the threshold (Saez, 2010). Here, the density of all cash deposits below 20,000 pesos is reduced post-repeal, which is compensated by a rise further up the distribution (part of which can be observed in the figure, and part of which is at values above 50,000 pesos that are cut off on the x-axis). While this first issue can be solved using the bunching estimator from Anagol et al. (2025), there are two additional issues in our context that this estimator does not address. First, bunching estimators need to rule out extensive margin responses: in our context, we find that some firms decide to stop making cash deposits entirely, invalidating this assumption. Second, a bunching estimator would only measure the response of cash deposits, the taxed variable, while we are also interested in the effect of the reform on non-cash and total deposits.

3.2 Average Changes in Cash Deposits

The distributions of cash deposits in Figure 1 show that firms responded to the repeal of the tax and that a share of firms increased their cash deposits between 2013 and 2014. How did average monthly deposit behavior change over time? Figure 2, Panel (a), shows firms' average monthly cash deposits between April 2011 and December 2017, after winsorizing at the 5th and 95th percentiles within each month and seasonally adjusting by removing calendar-month fixed effects. While cash deposits were on a downward trend between April 2011 and December 2013, they jumped suddenly in January 2014 from about 23,000 to 32,000 pesos, immediately following the repeal, and kept growing throughout 2014 and 2015, after which they leveled off over 2016–2017 at about 45,000 pesos—almost double their level just before the repeal.

[13] Note that deposit intervals which include round numbers (for example 10,000 or 20,000) are more frequent in all periods, presumably reflecting the mental accounting cost for clients to keep track of how much to deposit. Such round-number bunching is frequently observed in bunching studies.

Panel (b) displays the firm level average share of cash deposits out of total deposits. The average firm made 16.8% of its deposits in cash in April 2011, and cash intensity was on a downward trend pre-repeal, reaching about 13.7% in December 2013. When the tax was repealed in January 2014, there was a clear and immediate jump in the cash share back to its early-2012 level (about 15.5%), and the share held steady there for almost a year post-repeal. From 2015 onward, the average firm's cash intensity returned to its downward trend.

4 Identified Evidence

The previous section provided graphical, descriptive evidence which suggested that firms' cash deposit behavior responds to the tax repeal. In this section, we turn to a formal estimation strategy, which allows us to more precisely estimate the impacts of tax rate changes on firms' deposit behavior, and investigate mechanisms and sources of heterogeneity.

4.1 Identification Strategy

We are interested in estimating β in the following specification:

$$y_{it}^{d} = \beta \cdot \tau_{it} + \gamma_i + \gamma_t + e_{it}, \tag{1}$$

where y_{it}^{d} is the deposits of firm i in year t for deposit type $d = \{\text{cash, non-cash, total}\}$, and τ_{it} is the effective tax rate on cash deposits. γ_i and γ_t are firm and year fixed effects, respectively. During the time when IDE was active, the annual tax rate τ_{it} is the ratio of total IDE taxes paid to total cash deposits as long as annual cash deposits are greater than 0 (and τ_{it} is set to zero if annual cash deposits are 0). This is equivalent to summing over twelve months m the monthly effective tax rate τ_{im}, where $\tau_{im} = \frac{\text{tax rate}_t \cdot \max\left(y_{im}^{\text{cash}} - \text{threshold}_t,\, 0\right)}{y_{im}^{\text{cash}}}$ if $y_{im}^{\text{cash}} > 0$ and $\tau_{im} = 0$ if $y_{im}^{\text{cash}} = 0$, and weighting by the share of monthly deposits y_{im}^{cash} in yearly deposits y_{it}^{cash}:[14]

$$\begin{aligned}
\tau_{it} &= \frac{\sum_{m=1}^{m=12} \text{tax rate}_t \cdot \max\left(y_{im}^{\text{cash}} - \text{threshold}_t,\, 0\right)}{\sum_{m=1}^{m=12} y_{im}^{\text{cash}}} \\
&= \sum_{m=1}^{m=12} \left(\frac{\text{tax rate}_t \cdot \max\left(y_{im}^{\text{cash}} - \text{threshold}_t,\, 0\right)}{y_{im}^{\text{cash}}} \right) \cdot \left(\frac{y_{im}^{\text{cash}}}{y_{it}^{\text{cash}}} \right).
\end{aligned} \tag{2}$$

[14] This weighting makes τ_{it} equal to the tax paid per peso of cash deposited during the year, the relevant incentive for annual deposits. An unweighted average of monthly rates would understate it for firms whose deposits are concentrated in a few months: a firm that deposits 60,000 pesos in one month and nothing in the other 11 pays 2.25% of its annual cash deposits in tax, while the unweighted average of its monthly rates is 0.19%. Note also that, by measuring the incentive at the same yearly level as the outcome of interest even though the tax is initially levied on a higher frequency, we can exploit additional moments of the firm's deposit behavior, such as month-to-month variability, as sources of identifying variation.

Since the estimation sample for our main analysis is focused on the period between 2011 and 2017, the tax rate$_t$ and threshold$_t$ in (2) in practice only take on two values: 3% above a threshold of 15,000 between 2011 and 2013, and 0% above a threshold of 0 between 2014 and 2017. In robustness tests that include the initial IDE period, it takes a third value of 2% above a threshold of 25,000 pesos between July 2008 and December 2009.

Identifying variation Inspecting (2) shows that the effective tax rate τ_{it} is a function both of the tax system $T_t = T(\text{tax rate}_t; \text{threshold}_t)$ and of deposit behavior y_{it}: $\tau_{it} = \tau(T_t; y_{it})$. This creates a correlation between the tax rate, the outcome, and the error term e_{it} in equation (1), which biases the OLS estimation of β.

The literature typically solves this identification challenge by constructing an instrument for the tax rate which uses the mechanical tax changes driven by tax reforms (Gruber and Saez, 2002; Saez et al., 2012). Our estimation is based on the repeal of the tax: applied to our setting, the instrument is the simulated tax rate which combines the mechanical variation in the tax rate due to the repeal with the deposit behavior before the tax reform. The intuition is the following: compare a firm which, prior to the repeal, would deposit 20,000 pesos in cash every month, to a firm which would deposit 10,000 pesos in cash. Holding behavior constant, the latter faces no tax changes from the repeal (since its deposits were below the exemption threshold of 15,000 pesos), while the former firm faces a large tax cut. Thus, absent changes to their cash deposit behavior, these two firms face different changes in tax incentives due to the mechanical rate variation induced by the repeal.

Concretely, we create simulated tax rates $\tau_{it}^{sim} = \tau\left(T_t; y_{i,pre}\right)$ in all periods by applying the contemporaneous tax system T_t to the pre-repeal deposit behavior $y_{i,pre}$. In principle, we can choose any pre-repeal data on deposits $y_{i,pre}$. We choose to use the deposit behavior from 2006, which predates the existence of the tax, since variation across firms at that time reflect real differences in deposit management which are unrelated to taxation. The potential downside of using earlier deposit data is that it could weaken the first stage relation, an issue we revisit when presenting the results from the first-stage and IV estimation.

Specifically, the simulated tax rate for firm i in year t, denoted τ_{it}^{sim}, replaces contemporaneous monthly deposits y_{im}^{cash} in equation (2) with corresponding monthly deposits from 2006. Practically, we have one month of data (December) for the year 2006. Effectively, this means that the simulated tax rate takes the following form:

$$\tau_{it}^{sim} = \begin{cases} \frac{0.02 \cdot \max\left(y_{i,\text{Dec 2006}}^{\text{cash}} - 25000, 0\right)}{y_{i,\text{Dec 2006}}^{\text{cash}}} & \text{if } 2008 \leq t \leq 2009 \text{ and } y_{i,\text{Dec 2006}}^{\text{cash}} > 0, \\ \frac{0.03 \cdot \max\left(y_{i,\text{Dec 2006}}^{\text{cash}} - 15000, 0\right)}{y_{i,\text{Dec 2006}}^{\text{cash}}} & \text{if } 2010 \leq t \leq 2013 \text{ and } y_{i,\text{Dec 2006}}^{\text{cash}} > 0, \\ 0 & \text{if } t \geq 2014 \text{ or } y_{i,\text{Dec 2006}}^{\text{cash}} = 0. \end{cases} \tag{3}$$

In our main sample (2011–2017), equation (3) makes clear that our identifying variation post versus pre-repeal collapses to the (negative of the) cross-section of simulated rates pre-repeal: $\tau_i^{sim} = \tau\left(T_{2010\text{–}2013}; y^{\text{cash}}_{i,\text{Dec 2006}}\right)$.[15] While this instrumental variation is exogenous to post-repeal deposit behavior, it does depend on the cross-section of pre-IDE cash deposit behavior, $y^{\text{cash}}_{i,\text{Dec 2006}}$. Hence, the instrument may be endogenous if the pre-IDE cash deposits are correlated with the error term in equation (1). The literature in public finance on simulated instruments discusses two reasons why this might occur (Saez et al., 2012; Kleven and Schultz, 2014). First, firms at different pre-IDE cash deposit levels $y^{\text{cash}}_{i,\text{Dec 2006}}$ may be on differential deposit trends for non-tax reasons. For example, deposit levels may reflect business performance trending differently across groups due to market conditions. Second, deposit behavior at a point in time might reflect both permanent and transitory components, which creates a mean-reversion challenge. We might observe a firm with low cash deposits at the pre-repeal point in time where we construct the simulated instrument because of an idiosyncratic negative shock, which in expectation should revert to its (unobserved) true mean, even in the absence of the repeal. We augment our benchmark specification in two ways to deal with the issues of mean reversion and confounding trends.

Addressing mean reversion A set of studies emphasize that, when applied at the individual level, simulated instruments are prone to mean reversion (Weber, 2014; Kopczuk, 2024). To overcome this challenge, researchers have proposed to aggregate firms in groups, calculate the simulated rates at the group level, and assign the group level simulated rate to all firms within a group (Burns and Ziliak, 2017). In our setting, we group firms at the industry–state level. We group at this level because sector of activity and state are two of the key determinants of payment methods and deposit behavior. There are 922 industry–state groups in our sample, denoted g, spanning 29 industries and 32 states; on average, each group comprises 137 firms. The number of firms per group ranges from 28 firms at the 10th percentile, to 74 firms at the median and 203 firms at the 90th percentile.

This method eases mean reversion as long as the idiosyncratic shocks are mean-zero and firm-specific; in this case, the idiosyncratic shocks are averaged out at the group level. Concretely, the simulated tax rate in industry–state group g in year t is the simple average of the simulated tax rate for all firms in that group:

$$\tau_{gt}^{sim} = \frac{1}{N_g} \sum_{i \in g} \tau_{it}^{sim}. \tag{4}$$

For this exercise to be relevant, meaningful variation in effective tax rates across groups g must remain. The heatmap of simulated rates in the pre-repeal period, τ_g^{sim}, across industries and states is depicted in Panel (a) of Figure A2. Variation across states and industries in the group-level simulated rate is driven by differences in the factors that determine the level of the tax rate in

[15] In the Economic Census robustness specification that adds the 2008 wave, the variation also includes the change from the 2008–2009 to the 2010–2013 schedule.

equation (2). Inspecting (2) shows that the tax rate is increasing in three factors: total deposits; the share of cash deposits in total deposits; and, the (monthly) variability of monthly cash deposits. Each of the three factors impact the tax rate, in a non-linear fashion and conditional on the level of the other factors; moreover, the levels of the three factors also interact with each other to influence the tax rate.

The variation in the simulated rate reflects differences in total deposits (conditional on variability and cash share). Total deposits is, in turn, closely correlated with firm size. The firm size influence can be seen from looking at differences in simulated rates across states (columns) within a given industry (rows) in Panel (a) of Figure A2. For example, among educational services, the effective tax rate is twice as large in the State of Mexico—which surrounds Mexico City and contains much of its metropolitan area—as in Tlaxcala. Most of this difference disappears once we remove the part of the simulated rate that is explained by firm size (Panel (b) of Figure A2), consistent with larger schools in the Mexico City metropolitan area.

The tax rate also depends on month-to-month variability, conditional on total deposits and cash share. That is, between two firms that have equal annual total deposits and share of cash in total deposits, the firm with more month-to-month variability will face a higher annual tax rate. This variation is consistent with findings from the US, where industry factors strongly shape the regularity of cash flows among small and medium enterprises. Some industries operate in markets with less consistency in the timing and volume of sales (e.g., complex industry supplies and manufacturing; construction), while other markets receive client payments on a frequent and consistent basis (e.g., personal services; financial services; utilities). Moreover, there may be variability within sectors between states; for example, an industry such as hotels may see differences in seasonality of consumer demand between states such as Mexico City (less seasonal) versus Campeche and Oaxaca (more seasonal).

Finally, the tax rate depends on the share of cash in total deposits (conditional on total deposits and variability). Cash intensity is likely also shaped by industry and geographical factors. For example, poorer consumers and less complex firms may be less likely to adopt non-cash payment technologies. Transactions at corner stores in Chiapas (a non-complex sector in a poor state) are thus much more likely to be in cash than transactions for medical services in Mexico City (a complex industry in a rich state), translating into differences in effective tax rates across industry–state groupings. It is also possible that cash is a more frequent payment form when the transaction value is lower; this would be consistent with the comparatively higher tax rate observed in grocery stores, relative to other industries, within every state. In summary, we think of variation in τ_g^{sim} across industry–states as reflecting the parameters of cash and deposit patterns that are dictated by the nature of economic activity and geographical demand-side parameters.

In practice, we replace the individual firm's simulated rate τ_{it}^{sim} with the simulated rate calculated

at the industry–state level that the firm belongs to, τ_{gt}^{sim}. The reduced form regression therefore takes the form

$$y_{igt}^{d} = \beta \cdot \tau_{gt}^{sim} + \gamma_i + \gamma_t + e_{igt}. \tag{5}$$

We cluster standard errors at the industry–state level g to match the level of the identifying variation. The corresponding instrumental variable regression considers (1) as the endogenous second stage, and the first stage regression is

$$\tau_{igt} = \beta \cdot \tau_{gt}^{sim} + \gamma_i + \gamma_t + e_{igt}. \tag{6}$$

Confounding trends Firms at different pre-IDE deposit levels $y_{i,\text{Dec 2006}}^{\text{cash}}$ may be on differential deposit trends for reasons that are unrelated to the reform. To deal with such a concern, the literature has proposed to control for non-reform confounders by extrapolating the pre-event trend (Kleven wealth, Roth, Kleven house mortgage). This approach is valid under the assumption that the post-event behavior of the confounding factor can be inferred from the pre-event trend (Freyaldenhoven et al., 2019). We revisit this assumption when we present the results.

To operationalize this, we first create a dummy variable, $\mathbb{1}\{\tau_g^{sim} > \text{p50}\}$ which takes a value of 1 if the firm's group-level simulated tax rate in the pre-reform period, τ_g^{sim}, lies above the median value. In turn, we estimate a time-trend that is specific to each value of the dummy variable using pre-repeal data and residualize the outcome variable based on the estimated time trend. Specifically, we first regress the outcome y_{igt} on a linear time trend $\theta_k \cdot t$, where $k \in \{0, 1\}$ is the value of the dummy variable, and firm fixed effects using data from 2011 to 2013. We then run the specification (1) on the full period, using the residualized outcome $\tilde{y_{igt}} = y_{igt} - \hat{\theta}_k \cdot t$.

Controlling for size Imposing a linear pre-trend as we do only fully resolves the endogeneity concern if the dynamic effect of the non-reform confound is linear (Freyaldenhoven et al., 2019). There might be remaining pre-trends if the dynamic confounding effect is not fully approximated by a linear trend. In this case, including proxies for specific confounds could help further improve identification. The most important confound in our setting is arguably size, proxied with total deposits—that is, firms with different total deposits may be on (non-linear) differential growth trends. Fortunately, our setting allows us to control for size. This is because, as described above, the annual tax rate depends not only on annual deposits, but also on the cash-share of total deposits and the monthly variability in deposits. When controlling for size, we are thus relying on differences across groups in cash-intensity and within-year variability in deposits, as identifying variation.

In practice, we first regress the firm's simulated tax rate in the pre-repeal period, τ_i^{sim}, on the log of total deposits in 2006. In turn, we construct the residual simulated tax rate for each firm, $\tau_{it}^{sim,size}$ and re-construct the group-level average simulated rate following equation (4), denoted

$\tau_{gt}^{sim,size}$.[16] Panel (b) of Figure A2 shows that there remains a meaningful amount of variation between industry-location groupings in the tax rate after controlling for size.

In summary, our main specification isolates plausibly exogenous variation in the firm's effective tax rate by instrumenting with the simulated tax rate, calculated at the group level after controlling for firm's pre-reform size, and controlling for linear pre-trends.

In the results below, we will inspect the pre-trends in an event-study design to gauge how much the various components of our main specification have helped to remove confounding trends and mean reversion.

Outcome The final task before proceeding to estimation is to specify how we measure the outcome variable. While using log of deposits would helpfully allow us to immediately interpret β in (1) as a semi-elasticity, this option is not available to us in the full sample where cash, non-cash and total deposits often equal zero in individual years for individual firms. In this setting, limiting our analysis to a balanced panel of firms with strictly positive deposits would created a selected sample which likely influences the estimated responses.

In our setting, where both the pre-reform data features zero deposit behavior and where the reform might impact both the intensive and extensive margins, we follow the suggested procedure from Chen and Roth (2024) which transforms the outcome in a manner that provides an "intuitive" percentage interpretation of β. We leverage the fact that a subset of firms are control firms, in the sense that their deposit behavior in the pre-repeal period is such that they faced no tax prior to the repeal and therefore no change in the simulated tax rate post-repeal. We proceed by first identifying all such firms - that is, firms with $\tau_i^{sim} = 0$ (that is, with cash deposits of at most 15,000 in December 2006). In turn, we calculate the average value of deposits, for all types $d = \{\text{total}, \text{cash}, \text{non cash}\}$, among all control firms, over the pre-repeal years (2011-2013), separately for each industry–state group g, denoted $\overline{y}_g^d$. Finally, we transform our outcome of interest by dividing all variables in all periods by the constant, pre-reform level of the variable in the control group: $\frac{y_{igt}^d}{\overline{y}_g^d}$.

In this case, the coefficient β in equation (1) tells us, when the tax rate on cash deposits increases by 100%, what is the percent change in the outcome d for the average firm, relative to the untreated firms within the same state and industry.[17] By relating the changes in deposit to a benchmark control group, this outcome transformation provides us with an "intuitive" percent interpretation in

[16] Recall that, in 2006 we only have one month of data. This implies that the residualized simulated rate, after controlling for size, captures variation across firms in cash intensity. Since we control for size with a linear-log specification, the residual variation also reflects the non-linear relationship between total deposits and the tax rate. However, this second dimension is only a minor source of empirical variation.

[17] As our focus is to obtain an intuitive percent interpretation, we refrain from "log-like" transformations that allow for zeros, such as $\log(y+1)$ or $\text{arcsinh}(y)$. Chen and Roth (2024) show that these transformations do not approximate percentage effects, as they arbitrarily depend on the units in which y is measured.

a setting with both extensive and intensive margins (Chen and Roth, 2024). At the same time, we will combine our main outcome transformation with specific investigations of the reform's impact on the extensive and intensive margins. One potential drawback of our transformation is that we do not impose any concave preferences over values of the outcome (unlike, for example, the log transformation)—a one peso of reform-induced deposit change contributes equally to the estimated β, regardless of whether the response comes from firms with very large or very small levels of deposit. To limit the influence of outliers, we winsorize all continuous variables at the 5th and 95th percentiles, separately for each year and separately by values of $\mathbb{1}\{\tau_g^{sim} > \text{p50}\}$.

4.2 Results in Full Sample

Table 1 presents the results from estimating the IV of equation (1) in the full sample using the main specification, where the first-stage regression is in equation (6). The first stage coefficient is 0.64; with an F-statistic of 743.9, the simulated tax rate is strongly predictive of the actual tax rate. Since both the simulated rate τ_g^{sim} and the actual rate τ_{it} go to zero in the post-repeal periods, the strength of the first stage essentially reflects how strongly the cross-section of deposit levels in 2006 predicts the average cross-section of deposit levels in the pre-repeal years 2011-2013.

Across columns, both the reduced form and IV coefficients are shown for four outcomes: cash deposits, non cash deposits, total deposits, and the share of cash in total deposits. The first three outcomes are transformed based on the procedure described in the previous sub-section ($\frac{y_{igt}^d}{\overline{y}_g^d}$). For each outcome, results are shown with our main specification that controls for size (in even numbered columns), and without the size control (in odd numbered columns)

In column (2), we uncover a large, negative coefficient for cash deposits (-303 in the reduced form; -475.6 in the IV). Consistent with the descriptive graphical evidence from Section 3, the estimation shows that bank account holders respond to a larger tax rate on cash deposits by reducing the amount of cash deposited in their bank account; in the context of the underlying reform that is used in the IV, this implies a significant inflow of cash deposits into bank accounts following the repeal of IDE. This immediately implies that IDE has efficiency costs, by distorting firm behavior. A key question for policy-makers is whether this increase in cash deposits is coming from within the banking system (due to substitution from non-cash) or from outside the banking system. The former case would validate the rationale of reforms like IDE, as it suggests that imposing a cost on cash deposits may, as intended, induce movement into non-cash deposits which are more traceable. The latter case would indicate that IDE backfired, as it suggests that firms kept deposits out of the banking system while the tax was in place.

In column (4), a higher effective tax rate on cash deposits causes an increase in the amount of non-cash deposits. The positive and significant coefficient indicates that there is a substitution effect, as some of the reduced cash deposits do not exit the banking system, but rather are converted

into non-cash deposits.[18]

Putting these two effects together, in column (6) we find that an increased tax rate on cash deposits causes a decrease in total deposits. In other words, the substitution into non-cash is partial and some deposits end up being entirely withdrawn. Based on the underlying reform, these results indicate that the repeal of IDE led to an overall increase in total deposits in the banking system. Finally, in column (8), the share of cash is found to decrease following an increase of the effective tax rate.

The estimated coefficients indicate percent changes in deposits, relative to the average deposits of control firms in the pre-period. We can translate this into a percent change in the level of deposits in response to the reform for the average firm, by combining the estimated coefficient $\hat{\beta}_{IV}$ with information on the average change in tax rate, $\Delta\tau^{\text{avg}}$, and the value of the transformed outcome in the pre-period, $\bar{y}_{\text{pre}}$: $\left(\hat{\beta}_{IV} \cdot \Delta\tau^{\text{avg}} / \bar{y}_{\text{pre}}\right) \times 100$. Table A1 shows that the average change in tax rate following the IDE repeal is -0.009, a reduction of 0.9 percentage points.

Using this formula, the middle row in Table 1 indicates the percent changes induced by the IDE repeal, evaluated for the average firm in the sample. Evaluated at the average firm, the tax repeal caused cash deposits to increase by 246%, non-cash deposits to decrease by 26.8% and total deposits to increase by 8.8%. These magnitudes are economically large, in particular for cash deposits; we relate them to comparable studies of financial transaction taxes in Appendix B, and find that our results on cash deposits are within the upper range of these papers' estimates.

4.3 Intensive and Extensive Margins

The estimated impacts in Table 1 combine responses to the reform along the intensive and extensive margins. We now turn to estimating the reform's impacts along these two margins.

Intensive margin Table 2 shows the intensive margin results. These results are based on a restricted, balanced sample of account holders that make strictly positive cash and non cash deposits in every year between 2011 and 2017. As a result, we can use the log transformation of the outcome, which yields an intuitive log-level interpretation for β in (1). The sample size drops significantly (to 431,613). The first-stage coefficient is also smaller in this restricted sample (0.49), though the instrument remains strong (F-statistic = 214.22).

We find similar qualitative patterns of results on the intensive margin as in the full sample: in response to an increase in the effective tax rate, cash deposits fall, non cash deposits rise and total deposits fall. The negative impact on the share of cash is larger than in Table 1.

We can interpret these magnitudes in light of the underlying reform.[19] These estimates are

[18] Strictly speaking, we cannot directly compare the cash and non-cash magnitudes to infer the margin of substitution. Such a margin should ideally be estimated within an account holder, as the impacts of the tax rate on cash and non cash deposits estimated in Table 1 are in principle distinct and could be driven by different subsets of firms.

[19] Due to the log specification, the percent change induced by the reform, when evaluated at the average firm level,

reported in the middle row of Table 2. For the average firm that regularly makes cash and non cash deposits throughout the sample period, the IDE repeal caused a 512.1% increase in cash deposits, a 35.6% decrease in non cash deposits, and a 6.6% increase in total deposits.

Extensive margin To investigate the extensive margin, we return to our full sample and create dummy variables for whether any positive annual deposit was made during, separately for cash, non cash and total deposits. Table A1 provides descriptive values for the variables: in the pre-repeal period, the average likelihood that an account holder would make a positive cash, non-cash and total deposits were 79%, 90% and 92%, respectively.

In turn, we use these dummy variables as the outcomes in the IV regression, using a linear probability model. Table 3 provides the results. The results reveal that an increase in the tax rate significantly decreases the likelihood that an account holder will make any positive cash deposits. Interestingly, unlike on the intensive margin, there is no significant substitution to non-cash on the extensive margin. As a result of these patterns, an increase in the tax rate has a negative impact on the likelihood of making any positive deposits, but the coefficient is not significant at conventional levels.

The absence of substitution on the extensive margin may reflect that transitioning from cash to non-cash sales transactions incurs fixed costs: for example, the firm may need to install a point of sale device to be able to accept card payments. Such fixed costs would imply that the reversal to cash from non cash in the post-repeal period may not be immediate, or may not in fact occur at all. Moreover, the existence of fixed costs suggests that the extent of deposit responses to IDE may not be linear in the tax incentives.

The final column in Table 3 reveals that there are no impacts of tax rate changes on a broader measure of the extensive margin: whether a firm is observed in the sample or not. A firm will no longer be observed once all of its accounts are terminated, either at the firm's request or for administrative reasons. To establish this null result, we create an even larger panel, which is balanced in all sample years (2011-2017) for all firms in the sample drawn in December 2012. The outcome in this case is an indicator for whether a firm is observed (with either 0 or positive deposit values) in a given year, or not observed. In other words, this null result indicates that the reform did not have an impact on whether a firm would be observed in the bank system.[20]

We next evaluate the magnitude of the reform-induced extensive margin responses for the average firm, using the same formula as for Table 1, where $\bar{y}_{\text{pre}}$ is now the pre-repeal share of firms making positive deposits. This exercise reveals that the repeal led to a 8.1% increase in the

is given by: $\left(\exp(\hat{\beta}_{IV} \cdot \Delta\tau^{\text{avg}}) - 1\right) \times 100$, where $\hat{\beta}_{IV}$ is the IV coefficient on τ_{it}, and $\Delta\tau^{\text{avg}} = -0.009$ is the average firm-level tax rate change.

[20]The sample was drawn prior to the repeal. As such, this regression can only test whether or not, as a function of the simulated tax rate, firms that were observed at one point prior to the repeal are more or less likely to be observed in the bank system after the repeal.

likelihood that an account holder would make any positive cash deposits, and an insignificant 0.5% increase for total deposits. In summary, the repeal caused some account holders to start making use of cash deposits all together.

We can combine information across Tables 1, 2 and 3 to characterize further the account holders that responded on the extensive margin. Indeed, for cash deposits, observe that there is a positive reform effect on the extensive margin and the percent change is larger on the intensive margin than in the full sample. Together, these results suggest that the average cash deposits brought in post reform by firms that were making zero cash deposits pre-reform are smaller than the average cash deposits made pre-reform by firms that deposited both before and after the reform. This result is intuitive, and indicates that it is disproportionately firms with smaller cash deposits that reacted to the reform on the extensive margin.

In summary, the IDE repeal caused large inflows of cash deposits and total deposits on the intensive margin, and on the extensive margin for cash. We observe partial substitution from non cash to cash among regular depositors on the intensive margin, but no such substitution on the extensive margin. These results on substitution between cash and non-cash and total deposit inflows are crucial to determine the desirability of reforms like IDE; past studies on similar tax reforms which increase the cost of cash have, however, not been able to study this substitution mechanism due to an absence of data on means of payment. Our result that cash and non-cash are partial substitutes on the intensive margin is consistent with other studies that also find partial substitution when there are changes in the availability of cash versus non-cash means of payment (Chodorow-Reich et al., 2020; Gandelman et al., 2023). While most of these studies focus on means of payment from consumers' perspective, our results are related to firm's decision to transact in cash versus non-cash.

4.4 Discussion of Identifying Assumptions

We now turn to gauging the plausibility of the identifying assumptions which underlie the main results presented in the previous two subsections. We implement a simple event-study design, which allows us to investigate pre-trends.

To operationalize the event-study, we create a dummy, denoted $\mathbb{1}\{\tau_i^{sim} > \text{p50}\}$, which takes a value of 1 if the firm's simulated tax rate in the pre-repeal period, τ_i^{sim}, lies above the median value. We include a full set of year fixed effects and firm fixed effects. We use the transformed outcome $\frac{y_{igt}^d}{\overline{y}_g^d}$ in the full sample. The omitted year is 2013. We estimate regressions of the form

$$\frac{y_{igt}^d}{\overline{y}_g^d} = \sum_{j=2011}^{2017} \theta_j \cdot \mathbb{1}\{t = j\} \cdot \mathbb{1}\{\tau_i^{sim} > \text{p50}\} + \gamma_i + \gamma_t + e_{igt}, \tag{7}$$

where the standard error is clustered at the group level. The event-dummies θ_j for each year are presented for cash and non-cash deposits in Panel (a) of Figure 3. The corresponding graphs for the extensive margin are presented in parallel format in Figure A3. For both deposit types, the pre-trends differ between firms with different simulated tax rates. In the full sample, the trends in cash are consistent with mean reversion. On the extensive margin, there are clear differential trends, both for cash and non cash. In the remaining panels, we illustrate how our implemented extensions to this specification deal with the two issues of differential trends and mean reversion.

In Panel (b), we implement the strategy to deal with mean reversion, by replacing each firm's simulated rate with the average simulated rate at the group level. In the event-study, we illustrate the impact of this extension by simply replacing the individual level dummy with the group-equivalent dummy, $\mathbb{1}\{\tau_g^{sim} > \text{p50}\}$, that was constructed earlier:

$$\frac{y_{igt}^d}{\overline{y}_g^d} = \sum_{j=2011}^{2017} \theta_j \cdot \mathbb{1}\{t = j\} \cdot \mathbb{1}\{\tau_g^{sim} > \text{p50}\} + \gamma_i + \gamma_t + e_{igt}. \tag{8}$$

The result of this extension is to significantly improve the pre-trends for cash deposits (left side of Panel (b), Figure 3). The impact on the pre-trend is smaller for non-cash deposits, both in the full sample and on the extensive margin (right side of Panel (b) in Figures 3 and A3).

In Panel (c) of Figure 3, we augment the specification from Panel (b) with the extension that controls for the linear pre-trend by level of $\mathbb{1}\{\tau_g^{sim} > \text{p50}\}$. In practice, for each value $k \in \{0, 1\}$ of $\mathbb{1}\{\tau_g^{sim} > \text{p50}\}$, we regress the outcome $\frac{y_{igt}^d}{\overline{y}_g^d}$ on a linear time trend $\theta_k \cdot t$ and firm fixed effects using data from 2011 to 2013. We then estimate the event study from Panel (b), using the residualized outcome $\frac{\tilde{y_{igt}^d}}{\overline{y}_g^d} := \frac{y_{igt}^d}{\overline{y}_g^d} - \hat{\theta}_k \cdot t$:

$$\frac{\tilde{y_{igt}^d}}{\overline{y}_g^d} = \sum_{j=2011}^{2017} \theta_j \cdot \mathbb{1}\{t = j\} \cdot \mathbb{1}\{\tau_g^{sim} > \text{p50}\} + \gamma_i + \gamma_t + e_{igt}. \tag{9}$$

Controlling for the linear pre-trend eliminates the pre-trends on the extensive margin: in Panel (c) of Figure A3, the 2012 coefficients for cash and non-cash are close to zero and not statistically significant.

However, a small pre-trend remains for cash and non-cash deposits in the full sample (Panel (c), Figure 3). As explained above (Section 4.1), this may reflect the fact that the dynamic confound effect is not fully approximated by a linear trend. In Panel (d), we augment the specification from Panel (c) with the control for size. In practice, we regress the firm's simulated tax rate in the pre-repeal period, τ_i^{sim} on the log of the firm's total deposits in 2006, and capture the residual rate, $\tau_i^{sim,size}$. In turn, we construct the group-level simulated rate, $\tau_g^{sim,size}$ based on the individual

residualized rates and re-create the group-level dummy, $\mathbb{1}\{\tau_g^{sim,size} > \text{p50}\}$, using equation (4). We then detrend the outcome separately by values of $\mathbb{1}\{\tau_g^{sim,size} > \text{p50}\}$ and estimate

$$\frac{\tilde{y}_{igt}^{d}}{\overline{y}_g^{d}} = \sum_{j=2011}^{2017} \theta_j \cdot \mathbb{1}\{t = j\} \cdot \mathbb{1}\{\tau_g^{sim,size} > \text{p50}\} + \gamma_i + \gamma_t + e_{igt}. \tag{10}$$

In Panel (d) of Figure 3, this extension further reduces the pre-trend for cash and non-cash deposits in the full sample.

Taken together, by comparing to Panel (a), Panels (b)–(d) in the full sample and on the extensive margin demonstrate that the well-established methods to deal with mean reversion and confounding trends have improved the identification strategy substantially. The specification in Panel (d) features only small statistically significant coefficients in 2012 relative to the post-repeal effects, and sharp, immediate impacts post-reform. The reform effects in the full sample grow in magnitude until 2016, after which they stabilize. The specification in Panel (d) constitutes the basis for the results in the previous subsections, which provides a solid basis for interpreting our results as reflecting the causal impact of changes in tax rate induced by the IDE repeal.

4.5 Robustness

In this sub-section, we explore the robustness of our main results to changes in the specification and sample.

4.5.1 Sample Selection

Table A2 introduces changes to the estimation sample. In all panels, we use the transformed outcome and the benchmark specification (group level instrument, control for linear pre-trend and size). The four outcomes studied are cash deposits, non cash deposits, total deposits, and the share of cash deposits. Recall that our main results were estimated in a balanced sample, where all account holders are observed (but can make positive or zero deposits) in every year between 2011 and 2017. In Panel A, the sample in every column is restricted to a balanced panel of account holders that have strictly positive values of the given outcome. In Panel B, the sample is restricted across all columns to the sample of account holders that make strictly positive cash deposits in every year. These changes restrict the type of account holder that is included in the estimation sample. Nonetheless, the main findings are robust in these samples: an increased tax rate on cash causes a large decrease in cash deposits and total deposits, with partial substitution to non cash deposits.

In Panel C, we investigate the robustness to aggregating monthly data to yearly observations. In our full sample, due to data limitations, the yearly observation for 2011 is based on summing up the months of deposits from April to December while in all remaining years, we sum across the

full 12 calendar months. In Panel C, we re-create all yearly outcomes based on summing across the months of April to December only. The main results do not change with this restriction that ensures consistent seasonal sampling across all years.

In the full sample, the number of years is unbalanced between the pre-period (2011-2013) and the post-period (2014-2017). Panel D re-estimates the main results, using the firms that are observed in each of two years in the pre-period (2012-2013) and each of two years in the post-period (2014-2015). The results are robust to this alternative sample selection.

Finally, Panel E further reduces the sample by collapsing the estimation to a simple pre-post sample. We do this by taking the average values of the outcome, for each firm in the full sample, pre- and post-repeal. We continue to observe a large negative effect on cash and total deposits, while the coefficient for non cash deposits remains positive and significant.

4.5.2 Residualizing the Simulated Rate

In our main specification, we removed any confounding correlation between the simulated instrument and size by regressing τ_i^{sim} on the log of total deposits in December 2006, $y_{i,\text{Dec 2006}}^{\text{total}}$, and then aggregating the residualized simulated rates at the group level g. In the following two sets of exercises, we explore the robustness of our results to this specification. We use the full sample and the transformed outcome.

Simulated rate based on 2006 deposit behavior Table A3 investigates the simulated instrument that is built based on deposit behavior in 2006. Panels A and B reproduce results from Table 1 where we, respectively, do not and do control for size at the individual level before aggregating the simulated rates in the pre-repeal period into the group-level. The first-stage F-statistics are comparable across panels, suggesting that, conditional on size, variation in cash intensity and deposit variability across industries and locations are strongly predictive of the effective tax rate. Controlling for size only marginally changes the estimated impacts on all deposit types.

In Panel C, we remove the correlation between the simulated instrument in 2006 and industries by regressing τ_i^{sim} on a set of 29 industry dummies, and then aggregating the residual simulated rate to the group level g. In this case, the identifying variation stems from differences in size, cash intensity and deposit variability that exist between geographical locations within the same industry and which determine the effective tax rate. Alternatively, in Panel D, we construct the residual simulated rates after removing any correlation with geographical location (through a set of 32 state dummies). In this case, the identifying variation comes from differences in the factors that determine the effective tax rate which exist between industries within state. In both cases, the first-stage F-statistic drops significantly relative to Panel A and B. In other words, focusing on variation in the determinants of the effective tax rate within industries or within states is significantly less predictive of the tax rate than using the full variation across all industries and states. Notwithstanding, the

results are qualitative similar to Panels A and B. For total deposits, perhaps owing to the reduced strength of the first stage, the coefficients are comparable in magnitude to Panels A-B but they are much less precisely estimated (insignificant in Panel C, significant at 10% in Panel D).

Importantly, Table A4 and Table A5 show that the results are also qualitatively similar on the intensive margin and extensive margins, respectively, for these alternative residualizations of the simulated instrument in 2006.

Simulated rate based on 2018 behavior Our simulated instrument in 2006 is based on data from a single month. As such, while the effective tax rate conceptually depends on month-to-month deposit variability, in practice we cannot leverage variability as a source variation across firms (or investigate the impacts of controlling for it) in 2006.

To control for variability requires a monthly panel. We have monthly panel data in several years after the IDE repeal. If deposit behavior in a year sufficiently removed from the repeal-time is no longer affected by the reform, then this data can be useful to construct simulated rates. This strategy has been leveraged in other public finance settings: in the context of income tax changes, Blomquist and Selin (2010) formally discuss the conditions under which post-reform data can be applied with simulated instruments to calculate the elasticity of taxable income.

While this assumption is not without its challenges, the dynamic effects of the reform seem to largely materialize in the immediate post-repeal years (Figure 3). Importantly, the year-on-year effects for cash and non cash deposits between 2016 and 2017 are almost null, or at least tiny in comparison to the previous year-on-year changes. Using 2018 data may therefore be a valid approach.[21]

Table A6 provides results based on replacing all data from 2006 with data from 2018. Panels A and B, which show result without and with controlling for size using simulated instruments from 2018, can be compared to Panels A and B of Table A3 which use simulated rates from 2006 without and with controlling for size. The first-stage coefficients are similar across tables without controls and somewhat larger with the 2018 data when controlling for size, while the F-statistics are larger based on the 2018 data. The broad similarity in first-stage coefficients suggests that using deposit behavior well before the reform or well after the reform effectively captures similar (perhaps structural) differences across firms, industries and locations as identifying variation. Consequently, the estimated effects of increasing the tax rate on cash deposits are also comparable across the tables, when using the 2006 or 2018 simulated rates. Panel C residualizes the simulated rate by controlling both for size and monthly variability. We measure variability as the standard deviation of monthly deposits within 2018, calculated separately for each firm. We then regress each firm's simulated rate based on 2018 deposits on both variability and total deposits, and create the group

[21] On the extensive margin, there seems to be some remaining year-on-year effects between 2016 and 2017 (Figure A3). As such, the results from using the 2018 data should be interpreted as a suggestive robustness check.

level residual simulated rate. The identifying variation now arises primarily from variation in cash intensity across industries and states. By comparing Panel B to Panel C, we can see that controlling for variability has very little impact on the estimated effects: we continue to find large negative effects on cash and total deposits, with partial substitution to non-cash deposits.

Table A7 and A8 show that results on the extensive and intensive margins, respectively, are also robust to using instruments based on the 2018 deposit behavior.

In summary, this exercise has revealed that the main results are robust to using an entirely different sample to create the simulated tax rates, and to both leveraging variability as identifying variation or controlling for it. As a side-point that is relevant beyond our specific context, these robustness exercises shed light on the methodology of simulated instruments. In particular, our exercise highlights how identification can be based on higher-order moments than the individual averages of the distribution which governs treatment intensity. Exploiting higher-order moments may be relevant for reform evaluation in multiple settings.[22]

The large behavioral responses found throughout this section are consistent with the descriptive bunching evidence, which suggested that the firms sharply responded to changes in IDE. At the same time, it is important to note that these IV estimates are estimated based on deposit behavior that is driven specifically by IDE's repeal. The growing unpopularity of IDE may have made this tax particularly salient by the time it was repealed; if other IDE reforms (like its introduction or change in rates) are less salient, they may translate to smaller responses (Saez et al., 2012).

4.6 Heterogeneity

In this sub-section, we investigate whether there are heterogeneous responses to IDE across different dimensions of firms and the industries and locations they operate in.

We consider five dimensions of heterogeneity. The first dimension is the level of crime in the location of the firm. Here, the theory is that firms in locations with higher crime incidence face a higher likelihood that cash held outside the banking system may be taken by force or theft. The higher risk of theft raises the cost of holding cash outside the banking system (Arbeláez et al., 2005). In turn, for a given increase in the tax rate, firms that face higher risk of theft may be less likely to reduce cash holdings in the bank system; they may also be more likely to substitute into non cash deposits, to avoid paying the extra tax while also avoiding to take the cash out of bank accounts. We measure crime incidence at the level of the municipality of the firm. For each municipality we sum all recorded robberies of businesses in the pre-repeal years 2011–2013 and

[22] In public finance, for example, simulated instruments for income tax reforms are correlated with average pre-reform taxable income. If pre-reform income levels are correlated with non-reform confounders, higher-order moments such as the variance of taxable income can provide additional identifying variation. This improves identification only if these moments are less correlated with non-reform confounders than averages are, and only if they predict the endogenous tax rate conditional on averages. The latter requires a tax that is levied at a higher frequency (e.g., monthly or quarterly) than the outcome of interest is measured (e.g., annually), as with IDE.

divide this number by the municipality's population in 2010. In turn, we create a dummy variable equal to 1 if the per capita robbery rate in the firm's municipality lies above the median of this measure across the firms in our sample, and 0 otherwise.

The second dimension of heterogeneity is the extent to which the industry of the firm is oriented towards final consumers versus other firms. We measure this by coding as consumer facing all industries that are in retail, and non-consumer facing all other industries. We create a dummy variable which equals 1 if the industry of the firm is consumer facing, and 0 otherwise.

The third dimension of heterogeneity is debit card penetration in the firm's municipality, which has two opposing implications. Higher penetration may proxy for easier access to formal banking, which would make firms more willing to keep depositing cash despite the tax, so that they reduce cash deposits less. Higher penetration may also mean that more customers can pay electronically, which would make it easier for firms to substitute from cash toward non-cash deposits. We measure the number of debit cards in the firm's municipality, averaged over December 2008 to December 2013, and divide this by the municipality's population. In turn, we create a dummy variable equal to 1 if this per capita penetration rate lies above the median of this measure across the firms in our sample, and 0 otherwise.

A fourth dimension of heterogeneity is whether the firm's municipality has an above-median population. We measure this with a dummy variable equal to 1 if the population of the firm's municipality lies above the median of municipality population across the firms in our sample, and 0 otherwise. Being located in a municipality with an above-median population may, for our purposes, proxy for the availability of formal banking - for example, greater presence of bank branches or ATMs. It can also be thought of as a proxy for reduced cost to travel to withdraw (and potentially deposit) cash. In both cases, we would expect firms in municipalities with an above-median population to be less responsive in terms of drawing down cash deposits following a tax rate increase, and possibly more responsive in terms of substituting into non cash deposits.

These first four dimensions of heterogeneity are all at the location or industry level. The final dimension of heterogeneity is size, which is measured at the firm level as the sum of cash and non cash deposits of the firm in 2006. In turn, we create a dummy variable equal to 1 if the firm's size lies above the median firm size measured by total deposits, and 0 otherwise.

All dimensions of heterogeneity are measured as time-invariant characteristics in the pre-repeal period. We estimate heterogeneous tax rate effects by interacting the firm's tax rate with the dimension of heterogeneity:

$$\frac{\tilde{y^d_{igt}}}{\overline{y}^d_{gH}} = \beta \cdot \tau_{it} + \mu \cdot \tau_{it} \cdot \mathbb{1}(H)_i + \delta_t \cdot \mathbb{1}(H)_i + \gamma_i + \gamma_t + e_{igt}, \tag{11}$$

where $\mathbb{1}(H)_i$ is a dummy variable that captures one of the five dimensions of heterogeneity H described above, and $\delta_t \cdot \mathbb{1}(H)_i$ are time fixed effects interacted with the heterogeneity dummy. We instrument for τ_{it} and $\tau_{it} \cdot \mathbb{1}(H)_i$ with the simulated instrument and its interaction, τ_{gt}^{sim} and $\tau_{gt}^{sim} \cdot \mathbb{1}(H)_i$, based on the main specification. In equation (11), the denominator in the outcome is denoted with a subscript H. This reflects the fact that the control group for firm i is calculated separately for each level of the heterogeneity H within each group g. This alleviates the concern that μ would reflect differences across H in baseline levels of the outcome, rather than heterogeneous treatment effects. We use the benchmark specification (simulated rate aggregated at g level, linear pre-trend removed, control for size). Because we test five dimensions of heterogeneity for each outcome, we adjust inference on the interaction coefficients $\hat{\mu}$ for multiple hypothesis testing using the sharpened false discovery rate q-values of Anderson (2008) across the five heterogeneity variables.

Table 4 shows the results from estimating equation (11) in the full sample. Heterogeneity dimensions H_i are separated by panel, and we estimate effects for cash, non-cash and total deposits across columns within each panel. The table also reports the estimated linear combination for the effect of the tax rate increase for firms with $\mathbb{1}(H)_i = 1$, which is $\hat{\beta} + \hat{\mu}$.[23]

Focusing on Panel A, we observe that, in the case of cash deposits, the interaction coefficient μ is positive: firms that are located in areas with more crime see a smaller (less negative) reduction in cash deposits following an increase in the tax rate. This could reflect the increased risk of theft of cash held outside the bank. This pattern supports the conjecture of Arbeláez et al. (2005) that bank transaction taxes impose a higher burden on those in less safe areas, for whom holding cash is riskier. Likely as a reflection of the curbed cash response, non-cash deposits are also less responsive (less positive) among firms in areas with more crime.

In Panel B, we observe that firms that transact with consumers (versus with firms) reduce their cash deposits to a much larger extent following an increase in the tax rate, and increase their non-cash deposits. One potential explanation is that retailers can influence how customers pay at the point of sale to a larger extent than firms can influence other firms to change their method of transacting. In this case, for a given increase in the rate, consumer-facing firms are more able, at the margin, to convert cash transactions into non cash transactions.

In Panel C, firms in areas with high debit card penetration are less likely to reduce cash in response to a tax increase, and their non-cash deposits are similarly less responsive. Similar patterns are observed in Panel D, where firms in areas with larger populations are less responsive to a tax rate increase, both in terms of reducing cash deposits and in terms of increasing non-cash

[23]The sample size for the heterogeneity exercises is smaller than the full sample, because some firms do not have the required data to calculate the heterogeneity measure. Table A9 confirms that the main results from Table 1 are unchanged when estimated in the smaller heterogeneity sample. Because the heterogeneity sample varies by variable from 765,429 to 884,891, we use the sample of firms in all heterogeneity tests (N = 734,937) in Table A9.

deposits. Areas with large populations and areas with higher debit card penetration are likely to both proxy for locations with more availability of formal banking. In those areas, we would expect firms to be less responsive in terms of drawing down cash in response to the tax rate: To the extent that firms with more availability face lower costs to access formal banking, a given increase in the tax may be less likely to induce them to forgo a cash deposit.

Finally, in Panel E, we observe that larger firms draw down cash deposits to a much larger extent in response to the tax rate increase. We do not, in this case, observe a significant differential response of non cash deposits between large and small firms. One possible explanation is that larger firms can spread the fixed costs of changing their cash-management practices over a greater volume of transactions. For example, the tax increase may make it worthwhile for a larger firm to use cash receipts to pay suppliers directly, a change that reduces cash deposits without necessarily increasing non-cash deposits.[24]

It is important to note that, while we uncover significant differential cash responses in all H_i dimensions, the table also highlights that the magnitude of the negative cash response to taxation remains substantial in both cases where $\mathbb{1}(H)_i$ equals 0 or 1. For example, even though the reduction is smaller in magnitude than in safer areas, firms in areas with more crime still substantially reduce cash deposits in response to the IDE tax rate (Panel A). Moreover, in all heterogeneity dimensions except firm size, we consistently see firms which are more responsive in terms of their cash deposits are also more responsive in terms of their non-cash deposits. As a result, except for that of firm size, none of the interaction coefficients μ are statistically significant for total deposits.

5 Real Effects: Results from the Economic Census

Our results so far show that firms responded to IDE by making fewer cash deposits and overall deposits at banks. We next use data from Mexico's Economic Census to explore whether making less cash deposits, and holding more cash on hand, has *real effects* on firms' growth.

5.1 Identification Strategy

We create a mapping between the bank's sector categories and those of Mexico's version of the North American Industry Classification System (NAICS), accounting for changes to industry codes over time.[25] We then merge our instrument τ_{gt}^{sim} to firms in the Economic Census at the industry–state level. Our main analysis focuses on the 2013 and 2018 waves of the Economic Census (conducted in 2014 and 2019 with questions about January 1 to December 31 of 2013 and 2018,

[24] If larger firms are more likely to be formal with respect to their tax obligations, IDE is less likely to represent a final tax and more likely to capture a liquidity cost than for smaller firms. In this case, we might have expected to see a smaller drop in cash deposits than for smaller firms. The fact that we do not observe such a pattern may suggest that, for most firms in our sample, IDE does represent a final tax (Section 4.1).

[25] We also create a mapping to its predecessor, the Mexican Classification of Activities and Products, which was used for the 1993 Economic Census.

respectively), as 2013 was the last pre-repeal wave and 2018 was the first post-repeal wave.

Our analysis focuses on revenues, non-labor input costs, and labor input costs proxied by the number of workers including the owner. Importantly, survey responses are strictly confidential by law, and explicitly cannot be shared with anyone for "tax, judicial, administrative, or any other purposes"; firms appear to trust this guarantee from the National Statistical Institute and report their full revenues and costs truthfully (Higgins, 2024), even though they only report a portion of their revenues and costs to the tax and social security authorities (Kumler et al., 2020). Because these variables are highly skewed but contain almost no zeros, we take log transformations and restrict the balanced 2013–2018 sample to firms that have positive values in both waves; this reduces the balanced sample only slightly, from 2,364,624 firms to 2,306,824 firms. We use number of workers as a proxy of labor costs because it is always positive (since we include the owner as a worker), whereas the labor costs measure has many zeros for firms with no paid employees, which precludes taking a log transformation. As a robustness check, we keep zeros and use the level of the outcome variable normalized by the industry–state's mean winsorized baseline value of the outcome, as recommended by Chen and Roth (2024).[26] We again winsorize all continuous variables at the 95th percentile and—if the variable is not bounded below by zero—the 5th percentile.

We then implement the following specification, analogous to the reduced-form version of the specification we used in the bank data, on the balanced sample of firms from 2013–2018:

$$y_{igt} = \beta \cdot \tau_{gt}^{sim} + \gamma_i + \gamma_t + e_{igt}, \tag{12}$$

where y_{igt} is the outcome of firm i in industry–state group g in census year t, τ_{gt}^{sim} is the group-level simulated tax rate from equation (4), which equals the group's pre-repeal simulated rate in 2013 and 0 in 2018, and γ_i and γ_t are firm and year fixed effects. Since the Economic Census does not have a line item for the IDE tax firms paid, we estimate the reduced form only. As in our main bank-data specification, we residualize the simulated rate on firm size before aggregating it to the group level, and we cluster standard errors at the industry–state level g. To test robustness, we also report specifications that use the simulated rate without residualizing it, and that replace τ_{gt}^{sim} with the indicator $\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$ equal to 1 for firms above the median residualized simulated rate in 2013, and equal to 0 for all firms in 2018.

We conduct a placebo test using 1993–1998 data, where we assign the 2013 values of the instrument to 1993 and the 2018 values (zero for all firms) to 1998, thus assuming there was a placebo tax in 1993 repealed before 1998. This is a test of whether our results using the actual repeal reflect a true effect of the tax and its repeal, rather than an issue with our identification strategy.

[26] We use 2003 as the baseline year since we use 2008 as a partially-treated year in a robustness check.

5.2 Results

Across all specifications, firms more affected by the tax significantly reduce their sales and non-labor input costs.[27] To interpret magnitudes, we focus on comparing firms with above vs. below the median simulated tax rate in Figure 4 and Table A10, column 2. More-affected firms (those with an above-median industry–state simulated tax rate) had 12% lower revenues and 13% lower non-labor input costs than less-affected firms while the tax was in place compared to after its repeal. These results are robust to the specification using τ_{gt}^{sim} rather than the above-median indicator $\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$ (column 1) and to using outcomes in levels normalized by the industry–state pre-tax means, following Chen and Roth (2024) (column 3). They are also robust to including 2008 in the estimation using the 2008–2009 tax structure for that year's instrument, which differed from the tax structure over 2010–2013 (columns 4–6). In contrast, in the placebo tests where we impose the tax regime and its repeal on two pre-tax years (1993 and 1998; column 8), the coefficients for sales and non-labor input costs are substantially smaller, not statistically significant from zero, and statistically significantly different from the negative coefficients in column 2.[28]

When combined with the previous results from bank account data (Section 4), these results from the Economic Census allow us to conclude that, conditional on size, firms facing a higher tax on cash deposits reduce their cash and total bank deposits, their sales, and their spending on non-labor inputs. To avoid the tax, a firm may wish to preserve a sale by steering the customer toward electronic payment, consistent with evidence in Gertler et al. (2025) that small firms in Mexico have a high elasticity of electronic sales with respect to a 50–100 basis point change in merchant fees. If the customer only has cash, however, the firm must decide whether to deposit the proceeds and incur the tax or hold them until they can be spent, risking theft. If either option would absorb the margin on the sale, the firm may forgo such sales, thus making fewer sales and purchasing fewer inputs, as we observe.

The heterogeneity results in Section 4.6 are consistent with both decisions: whether to deposit the proceeds of a cash sale, and whether to make the sale at all. Firms in high-crime areas reduced their cash deposits less in response to the tax. Because holding cash is riskier for these firms, they are more willing to pay the tax to deposit the proceeds of the cash sales they make. For the same reason, holding the proceeds outside the bank is a costlier way for them to preserve a marginal sale, which would lead them to forgo more such sales.

[27] We also find a small and statistically significant reduction in labor measured by the number of workers, but unlike for our results for sales and non-labor input costs, we cannot reject that the effect on number of workers is different from the (not statistically significant) placebo coefficient.

[28] For the other placebo specifications using τ_{gt}^{sim} and the normalized outcome in levels, one out of six coefficients is statistically significant at the 5% level and one at the 10% level, but with inconsistent signs. Furthermore, we can still reject that the placebo coefficients equal the coefficients from columns 1 and 3 for revenues and non-labor inputs in all cases. We conclude that there are no systematic effects in the placebo regressions.

6 Conclusion

Policies that increase the cost of depositing cash seek to promote traceable transactions and strengthen tax enforcement, but may also discourage participation in formal finance. We examine this trade-off through Mexico's tax on cash deposits, introduced in July 2008 at 2% on monthly deposits exceeding 25,000 pesos, increased in January 2010 to 3% above 15,000 pesos, and repealed in January 2014. Non-cash deposits remained exempt throughout. Using firm-level bank account data, we exploit differences in exposure to the repeal generated by pre-tax deposit behavior to estimate firms' tax-induced responses in cash, non-cash, and total deposits. Our preferred estimates imply that, following the tax repeal, firms facing the average tax rate reduction increased cash deposits by approximately 246%, reduced non-cash deposits by 27%, and increased total deposits by 9%. These responses occur on both the intensive and extensive margins: in addition to firms adjusting the amounts they deposit, some firms made no cash deposits while the tax was in place and began depositing cash following the repeal.

Using Economic Census data on the universe of firms in Mexico, we find that the tax also affected firms' economic activity. While the tax was in effect, firms with above-median exposure had 12% lower revenues and 13% lower non-labor input costs than less-exposed firms, relative to after its repeal. These responses are consistent with firms forgoing cash sales when both the tax on depositing the proceeds and the risk of theft from holding them on premises would exceed the profit margin on the sale.

Because our estimates from bank account data capture changes in actual bank transactions, they reveal distortions to firms' financial choices. In particular, the incomplete substitution from cash into non-cash deposits indicates financial disintermediation, as the tax reduces total funds deposited in the banking system. At the firm level, this disintermediation has real, economic effects on sales and inputs. At the macroeconomic level, the disintermediation may reduce the availability of bank funding, constrain credit supply, and distort aggregate investment and borrowing by firms. Although we do not directly identify these macroeconomic effects, our economically large estimated deposit responses and real effects on firms suggest that the tax on cash deposits may have contributed to such macroeconomic distortions.

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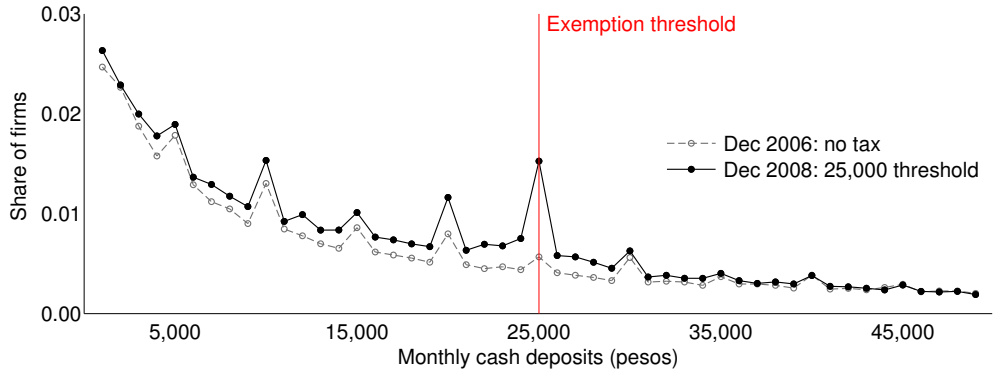
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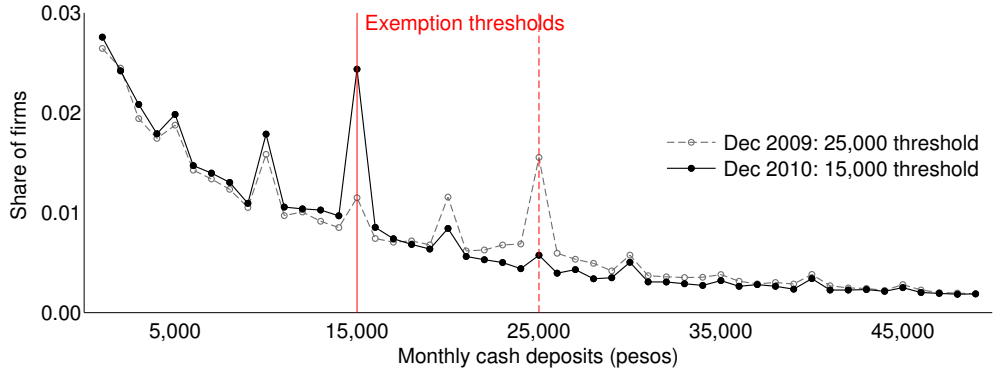
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Figure 1: Distribution of Monthly Cash Deposits

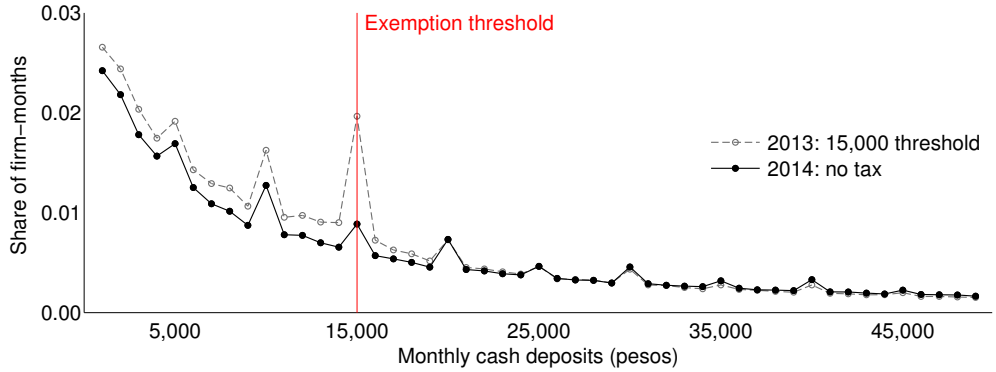
(a) Tax Implemented in 2008



(b) Threshold Changed in 2010

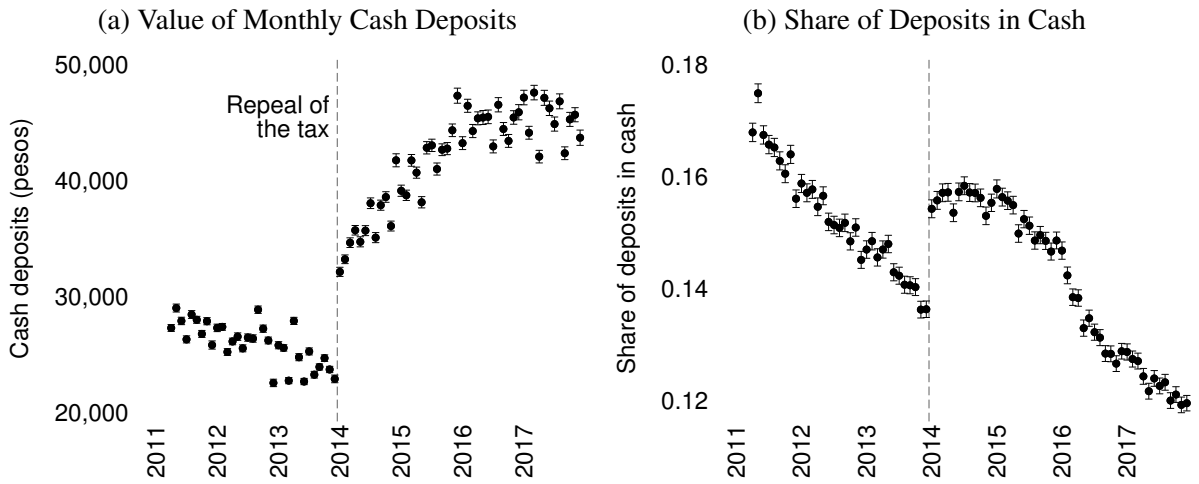


(c) Tax Repealed in 2014



Notes: Panels (a) and (b) plot the share of firms whose monthly cash deposits fall in each 1,000-peso bin, and panel (c) plots the share of firm–months, since we have up to twelve months of data for each firm in those years. Panel (a) compares December 2006 (dashed line with open circles) and December 2008 (solid line with filled circles), panel (b) compares December 2009 (dashed line) and December 2010 (solid line), and panel (c) compares 2013 (dashed line) and 2014 (solid line). Each panel uses the subset of the 126,440 firms in the regression sample that had accounts during the relevant time period: (a) 125,964 firms, (b) 125,775 firms, and (c) 126,440 firms. The figure excludes observations with exactly 0 cash deposits and with more than 49,000 pesos in cash deposits for legibility, but shares are computed over the full sample including these omitted observations. Vertical red lines mark the exemption thresholds.

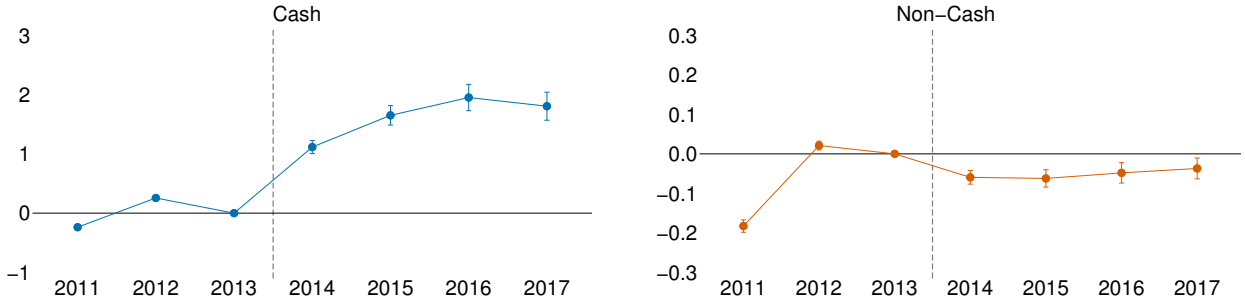
Figure 2: Effects of the Tax Repeal on Cash Deposits (Time Series)



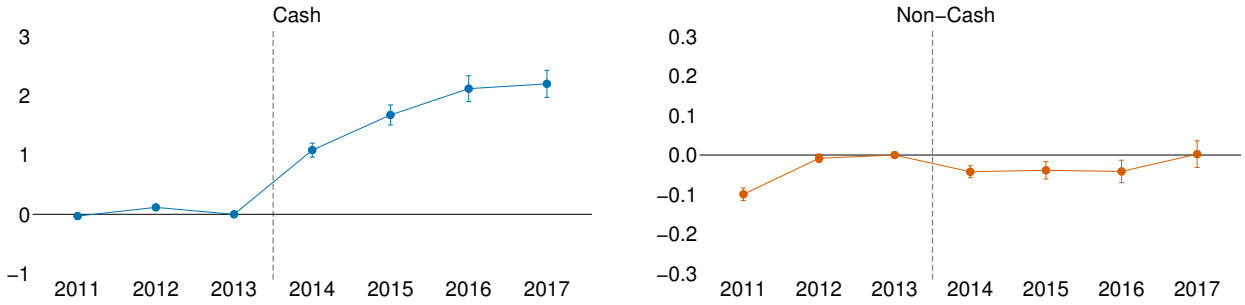
Notes: This figure shows monthly averages across the 126,440 firms in the regression sample, from April 2011 to December 2017. Panel (a) shows the average value of a firm's cash deposits at the bank in the month, in nominal pesos. Panel (b) shows the average across firms of the share of a firm's monthly deposits made in cash; the share is coded as 0 for firm–months without any deposits. Before computing the averages and the cash share, we winsorize cash, non-cash, and total deposits at the 5th and 95th percentiles of each month. Both panels are seasonally adjusted by subtracting estimated calendar-month fixed effects, with January as the base month. The dashed vertical line marks the repeal of the tax in January 2014. Whiskers show 95% confidence intervals for each month's average.

Figure 3: Effects of the Tax Repeal on Deposits (Event Studies)

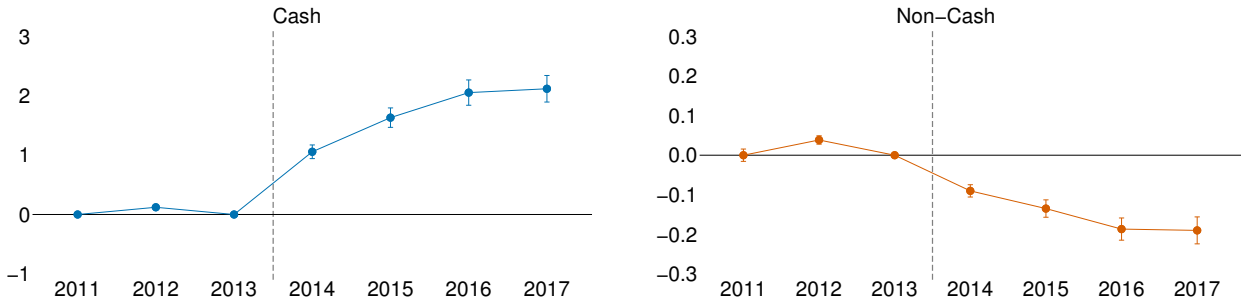
(a) Firm-level instrument



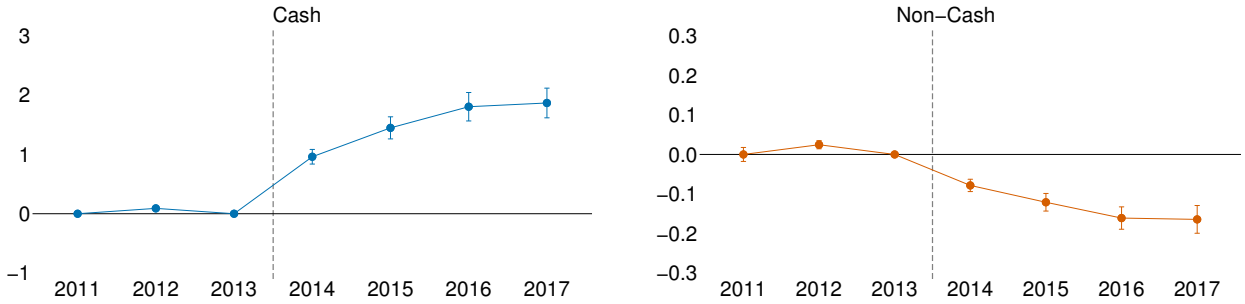
(b) Group-level instrument



(c) Group-level instrument, control for linear pre-trend

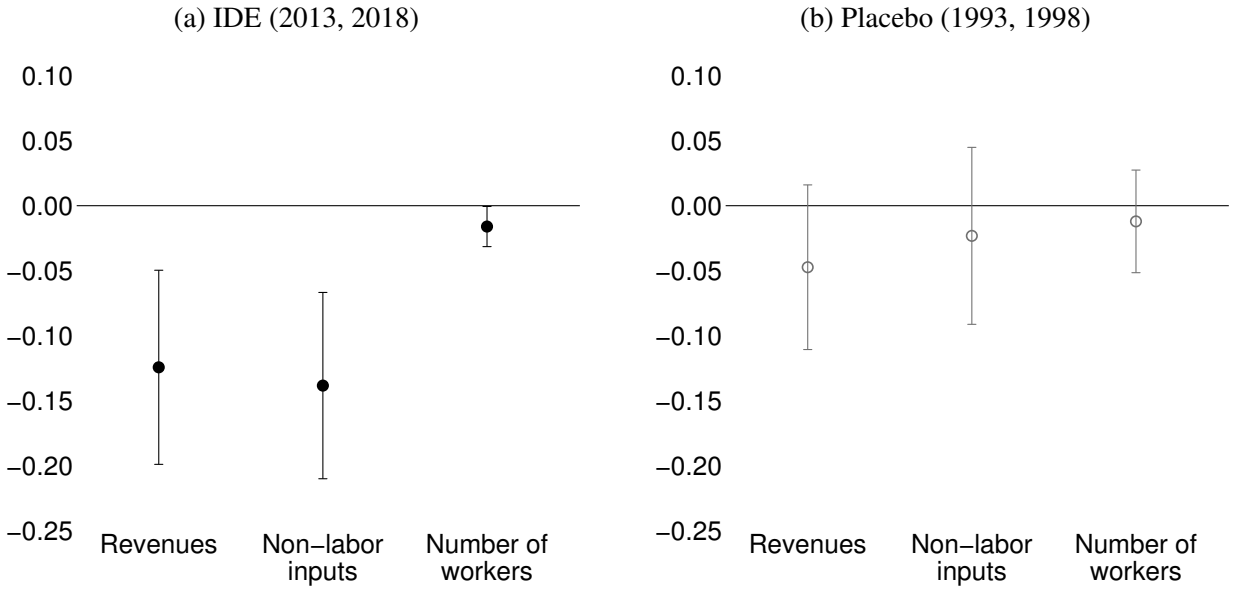


(d) Group-level instrument, control for linear pre-trend and size



Notes: Panels (a), (b), (c), and (d) plot the coefficients θ_j from equations (7), (8), (9), and (10), respectively. The left graph in each panel shows results for cash deposits and the right graph for non-cash deposits, each divided by the 2011–2013 average deposits of "control" firms in the same industry–state (i.e., whose simulated tax rate is zero). The dashed vertical line marks the repeal of the tax in January 2014. Whiskers show 95% confidence intervals based on standard errors clustered at the industry–state level.

Figure 4: Real Effects from Economic Census



Notes: Panel (a) shows the effects of the repeal of the tax, estimated on the 2013 and 2018 census waves: the reduced-form coefficients on the indicator $\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$, which equals 1 in 2013 for firms whose industry–state simulated tax rate, residualized on firm size, is above the median, and 0 for all firms in 2018 (column 2 of Table A10). Panel (b) shows the placebo test, which assigns the 2013 and 2018 values of the indicator to the 1993 and 1998 waves (column 8 of Table A10). The outcomes are in logs. Both panels use the same vertical scale. Filled black circles indicate estimates that are statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles estimates that are not statistically significant. Whiskers show 95% confidence intervals based on standard errors clustered at the industry–state level.

Table 1: Effects of the Tax on Deposits

	Cash Deposits		Non-Cash Deposits		Total Deposits		Share Cash	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced Form								
τ_{gt}^{sim}	–300.5***	–303.0***	21.7***	21.1***	–5.9***	–7.3***	–9.1***	–9.4***
	(13.6)	(15.6)	(2.3)	(2.8)	(2.0)	(2.1)	(0.5)	(0.5)
Instrumental Variable								
τ_{it}	–478.0***	–475.6***	34.5***	33.1***	–9.4***	–11.4***	–14.7***	–15.1***
	(31.8)	(33.4)	(4.6)	(5.3)	(3.0)	(3.1)	(1.2)	(1.3)
% change at avg τ_{it} change	247.2	246.0	–27.9	–26.8	7.2	8.8	80.8	83.1
Control		Size		Size		Size		Size
1st stage coefficient on τ_{gt}^{sim}	0.63	0.64	0.63	0.64	0.63	0.64	0.63	0.64
1st stage F-statistic	709.7	743.9	709.7	743.9	709.7	743.9	709.7	743.9
N	884,891	884,891	884,891	884,891	884,891	884,891	795,576	795,576
Number of firms	126,413	126,413	126,413	126,413	126,413	126,413	118,016	118,016

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. In columns with "Size" in the "Control" row, the instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. Firm–years with zero total deposits are excluded from the share-of-cash regressions because the share is undefined for them. The sample consists of a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The reduced-form rows report the coefficient from regressing the outcome directly on τ_{gt}^{sim}. The percent change at the average τ_{it} change is computed as $\left(\hat{\beta}_{IV} \cdot \Delta\tau^{\text{avg}} / \bar{y}_{\text{pre}}\right) \times 100$, where $\hat{\beta}_{IV}$ is the IV coefficient on τ_{it}, $\Delta\tau^{\text{avg}} = -0.009$ is the average firm-level tax rate change, and $\bar{y}_{\text{pre}}$ is the pre-period mean of the dependent variable. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table 2: Intensive-Margin Effects of the Tax on Deposits

	Cash Deposits		Non-Cash Deposits		Total Deposits		Share Cash	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced Form								
τ_{gt}^{sim}	–92.5***	–95.2***	21.8***	23.1***	–3.3**	–3.4**	–12.0***	–12.5***
	(4.0)	(4.4)	(2.1)	(2.4)	(1.4)	(1.5)	(0.6)	(0.7)
Instrumental Variable								
τ_{it}	–199.3***	–195.7***	47.0***	47.5***	–7.1**	–6.9**	–25.8***	–25.8***
	(15.8)	(16.4)	(6.7)	(7.2)	(2.8)	(2.8)	(2.4)	(2.5)
% change at avg τ_{it} change	532.7	512.1	–35.3	–35.6	6.8	6.6	128.0	127.8
Control		Size		Size		Size		Size
1st stage coefficient on τ_{gt}^{sim}	0.46	0.49	0.46	0.49	0.46	0.49	0.46	0.49
1st stage F-statistic	222.6	214.2	222.6	214.2	222.6	214.2	222.6	214.2
N	431,613	431,613	431,613	431,613	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659	61,659	61,659	61,659	61,659

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. In columns with "Size" in the "Control" row, the instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. In columns 1–6, the dependent variable is the logarithm of the observed deposit value for firm i in year t. The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. The sample consists of firms with strictly positive cash and non-cash deposit values in all years of the analysis. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The reduced-form rows report the coefficient from regressing the outcome directly on τ_{gt}^{sim}. The percent change at the average τ_{it} change is computed as $\left(\exp(\hat{\beta}_{IV} \cdot \Delta\tau^{\text{avg}}) - 1\right) \times 100$, where $\hat{\beta}_{IV}$ is the IV coefficient on τ_{it} and $\Delta\tau^{\text{avg}} = -0.009$ is the average firm-level tax rate change. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table 3: Extensive-Margin Effects of the Tax on Deposits

	Cash Deposits	Non-Cash Deposits	Total Deposits	Total Observed
	(1)	(2)	(3)	(4)
Reduced Form				
τ_{gt}^{sim}	–4.43***	0.24	–0.32	0.26
	(0.47)	(0.34)	(0.24)	(0.45)
Instrumental Variable				
τ_{it}	–6.96***	0.37	–0.50	
	(0.68)	(0.54)	(0.38)	
% change at avg τ_{it} change	8.1	–0.4	0.5	
Control	Size	Size	Size	Size
1st stage coefficient on τ_{gt}^{sim}	0.64	0.64	0.64	
1st stage F-statistic	743.9	743.9	743.9	
N	885,080	885,080	885,080	1,193,815
Number of firms	126,440	126,440	126,440	170,545

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. In columns with "Size" in the "Control" row, the instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. Each dependent variable is a binary indicator equal to 1 if firm i has strictly positive deposits of that type in year t, and 0 otherwise. In column 4, the "Total Observed" outcome is instead a binary indicator for whether the firm is observed in a given year, regardless of whether it has any transactions. Columns 1–3 use a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period; column 4 uses the 170,545 firms in the sample drawn in December 2012 in all years 2011–2017, including years in which a firm is not observed, and reports only the reduced form. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The reduced-form rows report the coefficient from regressing the outcome directly on τ_{gt}^{sim}. The percent change at the average τ_{it} change is computed as $\left(\hat{\beta}_{IV} \cdot \Delta\tau^{\text{avg}} \big/ \bar{y}_{\text{pre}}\right) \times 100$, where $\hat{\beta}_{IV}$ is the IV coefficient on τ_{it}, $\Delta\tau^{\text{avg}} = -0.009$ is the average firm-level tax rate change, and $\bar{y}_{\text{pre}}$ is the pre-period mean of the dependent variable. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table 4: Heterogeneous Effects of Tax Repeal on Deposits

	Cash Deposits (1)	Non-Cash Deposits (2)	Total Deposits (3)
Panel A: Crime			
τ_{it}	–543.4***	44.0***	–10.5**
	(35.6)	(5.4)	(4.3)
$\tau_{it} \times$ Crime	142.1***	–19.7***	2.8
	(44.6)	(7.4)	(5.7)
	[0.001]	[0.004]	[1.000]
$\tau_{it} + \tau_{it} \times$ Crime	–401.3***	24.4***	–7.6*
	(36.7)	(6.5)	(4.3)
N	830,683	830,683	830,683
Panel B: Consumer Facing			
τ_{it}	–350.1***	16.3***	–10.8***
	(27.6)	(4.3)	(3.4)
$\tau_{it} \times$ Consumer Facing	–293.3***	43.6***	3.5
	(50.4)	(7.8)	(7.3)
	[0.001]	[0.001]	[1.000]
$\tau_{it} + \tau_{it} \times$ Consumer Facing	–643.5***	59.9***	–7.3
	(42.2)	(6.5)	(6.5)
N	884,891	884,891	884,891
Panel C: Debit			
τ_{it}	–590.7***	48.9***	–10.6**
	(33.3)	(6.3)	(4.7)
$\tau_{it} \times$ Debit	217.4***	–24.4***	5.5
	(32.2)	(5.1)	(5.2)
	[0.001]	[0.001]	[0.716]
$\tau_{it} + \tau_{it} \times$ Debit	–373.3***	24.6***	–5.1
	(32.2)	(5.0)	(3.7)
N	866,943	866,943	866,943
Panel D: Above-Median Population			
τ_{it}	–529.6***	19.3***	–16.6***
	(40.7)	(3.9)	(2.9)
$\tau_{it} \times$ Above-Median Population	219.6***	–12.0***	3.6
	(33.5)	(2.9)	(3.5)
	[0.001]	[0.001]	[0.716]
$\tau_{it} + \tau_{it} \times$ Above-Median Population	–310.0***	7.3*	–13.1***
	(28.8)	(3.8)	(2.7)
N	765,429	765,429	765,429
Panel E: Large Firm			
τ_{it}	–315.8***	41.5***	8.8**
	(23.2)	(4.9)	(3.8)
$\tau_{it} \times$ Large Firm	–258.2***	–3.3	–26.6***
	(32.6)	(4.7)	(4.1)
	[0.001]	[0.108]	[0.001]
$\tau_{it} + \tau_{it} \times$ Large Firm	–573.9***	38.2***	–17.9***
	(47.9)	(6.8)	(3.5)
N	766,640	766,640	766,640

Notes: The table reports IV estimates of β and μ in equation (11), with the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. The instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. The heterogeneity dummies are: (A) above-median crime incidence, (B) consumer-facing sectors, (C) above-median debit card penetration, (D) above-median municipality population, and (E) above-median 2006 firm size based on total deposits. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state and subgroup over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. For the interaction terms, stars are instead based on Anderson (2008) sharpened q-values, shown in square brackets, which adjust for multiple hypothesis testing. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

A Additional Tables and Figures

Figure A1: Timeline of tax on cash deposits

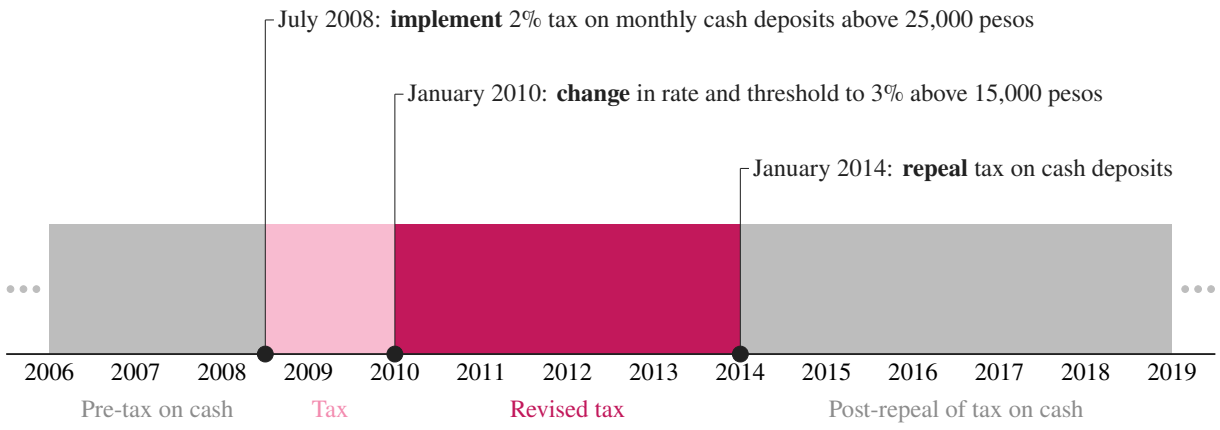
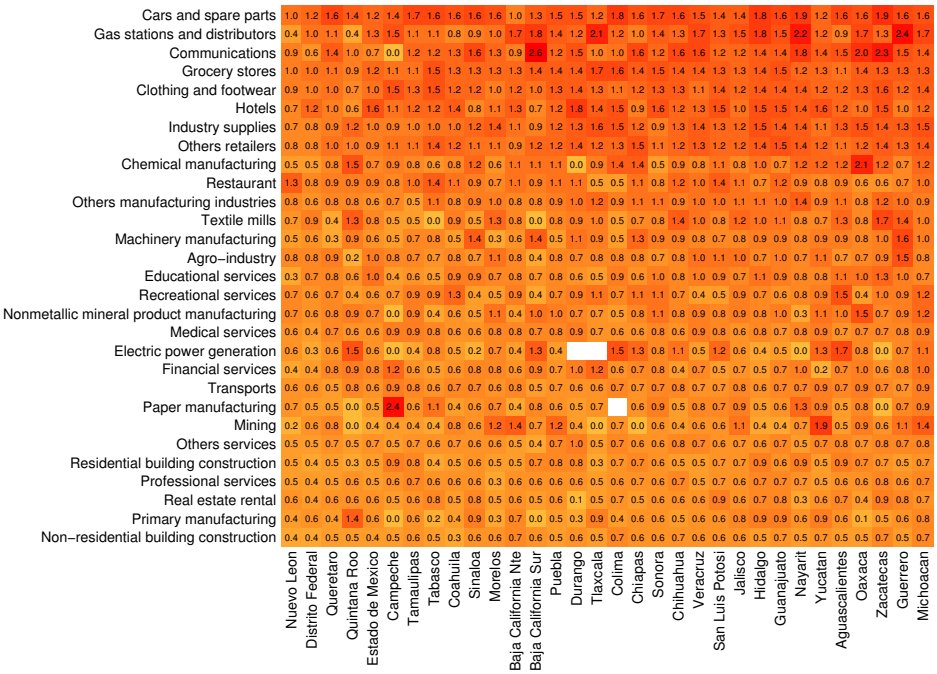
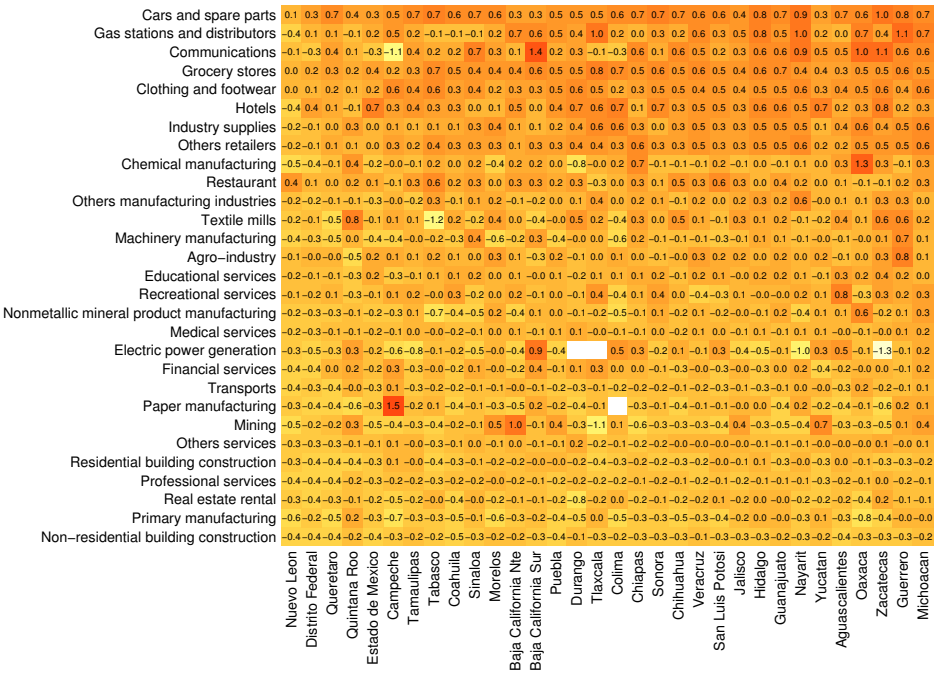


Figure A2: Variation in Simulated Tax Rates by Industry–State

(a) Simulated Tax Rate



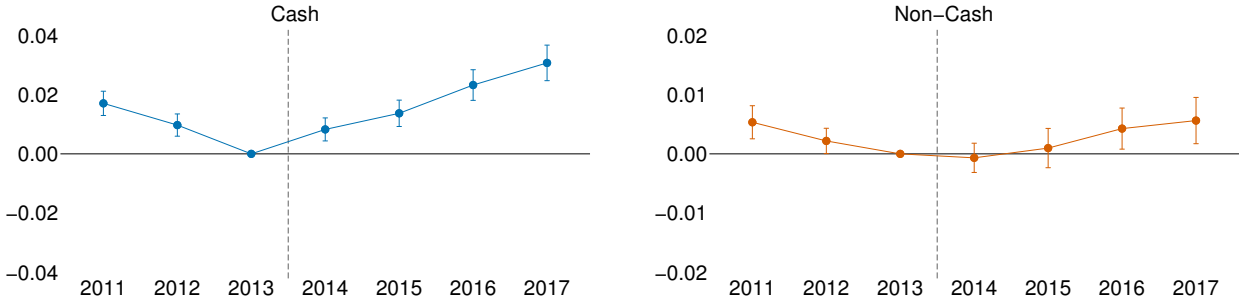
(b) Simulated Tax Rate, Residualized on Size



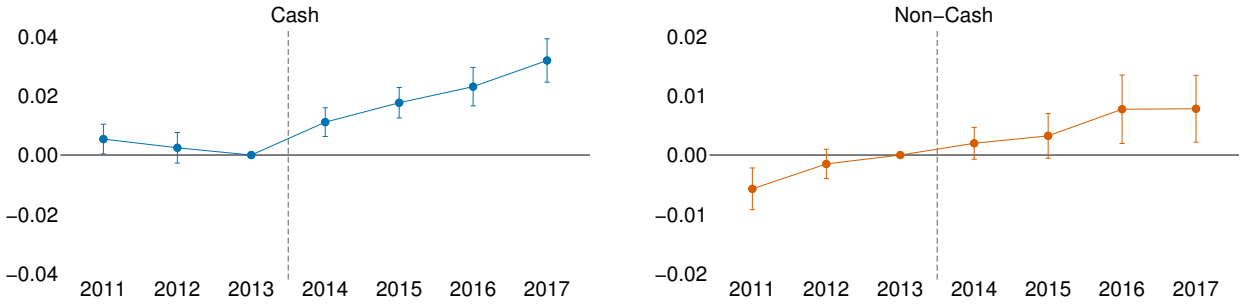
Notes: Panel (a) displays the simulated tax rate τ_g^{sim} in each industry–state, in percent. Panel (b) displays the size-residualized simulated tax rate $\tau_g^{sim,size}$. Darker shades indicate higher values. Blank cells are industry–states with no firms in the sample.

Figure A3: Event Studies, Extensive Margin

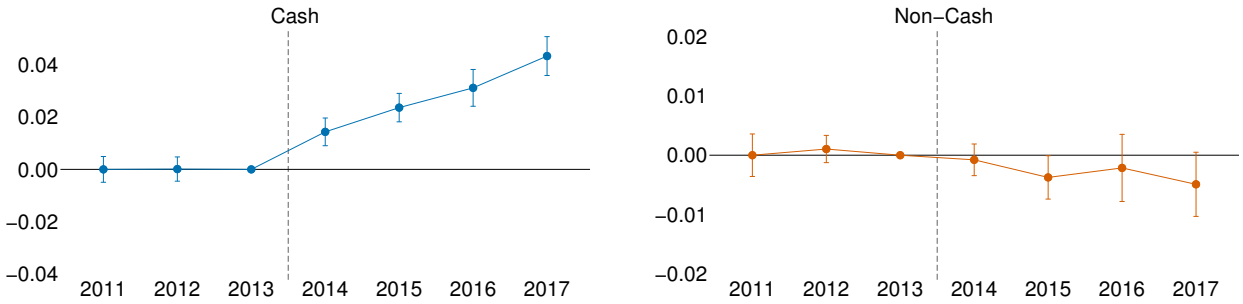
(a) Firm-level instrument, Pre-trends not eliminated



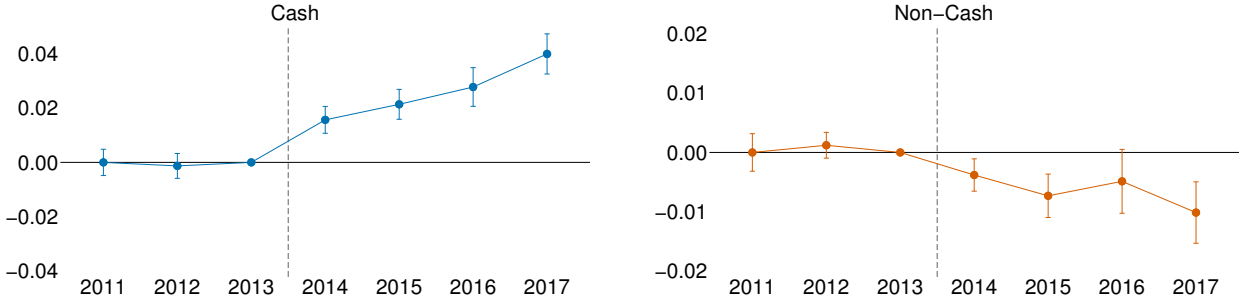
(b) Group-level instrument, Pre-trends not eliminated



(c) Group-level instrument, Pre-trends eliminated



(d) Group-level instrument, Pre-trends eliminated, Control for size



Notes: Panels (a), (b), (c), and (d) plot the coefficients θ_j from equations (7), (8), (9), and (10), respectively. The left graph in each panel shows results for an indicator equal to 1 if the firm made any cash deposits in the year, and the right graph for an indicator equal to 1 if the firm made any non-cash deposits in the year. The dashed vertical line marks the repeal of the tax in January 2014. Whiskers show 95% confidence intervals based on standard errors clustered at the industry–state level.

Table A1: Summary Statistics Pre- and Post-Repeal

	Mean (1)	Median (2)	p10 (3)	p25 (4)	p75 (5)	p90 (6)	N (7)	Number of Firms (8)
Panel A: Pre-Repeal Period								
τ_{it}	0.009	0.003	0.000	0.000	0.019	0.027	379,320	126,440
τ_{gt}^{sim}	0.008	0.008	0.004	0.006	0.011	0.013	379,320	126,440
Cash (level)	329,078	60,341	0	4,000	257,640	1,041,227	379,320	126,440
Cash (ratio)	1.790	0.407	0.000	0.028	1.732	6.407	379,239	126,413
Cash (binary)	0.794	1.000	0.000	1.000	1.000	1.000	379,320	126,440
Non-cash (level)	5,552,474	1,273,793	0	227,132	5,269,394	18,392,868	379,320	126,440
Non-cash (ratio)	1.145	0.328	0.000	0.062	1.269	4.001	379,239	126,413
Non-cash (binary)	0.902	1.000	1.000	1.000	1.000	1.000	379,320	126,440
Total (level)	6,064,419	1,579,663	28,000	363,352	5,928,409	19,784,186	379,320	126,440
Total (ratio)	1.200	0.384	0.007	0.092	1.362	4.091	379,239	126,413
Total (binary)	0.922	1.000	1.000	1.000	1.000	1.000	379,320	126,440
Share cash	0.168	0.041	0.000	0.004	0.199	0.594	349,680	118,697
Panel B: Post-Repeal Period								
τ_{it}	0.000	0.000	0.000	0.000	0.000	0.000	505,760	126,440
τ_{gt}^{sim}	0.000	0.000	0.000	0.000	0.000	0.000	505,760	126,440
Cash (level)	607,570	50,000	0	0	366,564	1,624,008	505,760	126,440
Cash (ratio)	2.902	0.349	0.000	0.000	2.415	10.244	505,652	126,413
Cash (binary)	0.725	1.000	0.000	0.000	1.000	1.000	505,760	126,440
Non-cash (level)	6,449,111	1,224,743	0	169,864	5,659,106	21,537,734	505,760	126,440
Non-cash (ratio)	1.314	0.322	0.000	0.047	1.389	4.750	505,652	126,413
Non-cash (binary)	0.870	1.000	0.000	1.000	1.000	1.000	505,760	126,440
Total (level)	7,461,988	1,602,650	0	288,600	6,758,879	24,681,530	505,760	126,440
Total (ratio)	1.460	0.397	0.000	0.073	1.572	5.168	505,652	126,413
Total (binary)	0.884	1.000	0.000	1.000	1.000	1.000	505,760	126,440
Share cash	0.170	0.040	0.000	0.002	0.218	0.593	447,214	115,813

Notes: This table reports the mean, selected percentiles, and the numbers of observations and firms with nonmissing values for each variable. Panel A covers the pre-repeal years 2011–2013 and Panel B the post-repeal years 2014–2017. The effective tax rate τ_{it} is the tax paid by firm i on its cash deposits in year t as a share of those deposits from equation (2), and τ_{gt}^{sim} is the simulated tax rate averaged across firms in industry–state g from equation (4). The level rows report annual cash, non-cash, and total deposits in pesos. In the ratio rows, cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The binary rows report a dummy variable equal to 1 if the amount of annual deposits is strictly positive, and 0 otherwise. Share cash is the ratio of cash to total deposits; it is undefined, and therefore missing, for firm–years with zero total deposits.

Table A2: Full Sample, Various Sample Restrictions

	Cash Deposits (1)	Non-Cash Deposits (2)	Total Deposits (3)	Share Cash (4)
Panel A: Positive Outcome				
τ_{it}	–941.5***	38.8***	–13.8***	–26.5***
	(82.4)	(6.4)	(3.5)	(2.7)
1st stage coefficient on τ_{gt}^{sim}	0.45	0.67	0.63	0.45
1st stage F-statistic	193.0	500.6	455.2	193.0
N	460,866	698,803	738,542	460,999
Number of firms	65,838	99,829	105,506	65,857
Panel B: Positive Cash				
τ_{it}	–941.5***	58.9***	–26.8***	–26.5***
	(82.4)	(10.5)	(5.8)	(2.7)
1st stage coefficient on τ_{gt}^{sim}	0.45	0.45	0.45	0.45
1st stage F-statistic	193.0	193.0	193.0	193.0
N	460,866	460,866	460,866	460,999
Number of firms	65,838	65,838	65,838	65,857
Panel C: All Accounts (April-December)				
τ_{it}	–492.7***	29.1***	–18.9***	–15.4***
	(33.2)	(4.9)	(2.6)	(1.3)
1st stage coefficient on τ_{gt}^{sim}	0.64	0.64	0.64	0.64
1st stage F-statistic	780.2	780.2	780.2	780.2
N	867,811	867,811	867,811	778,527
Number of firms	123,973	123,973	123,973	115,787
Panel D: 2 Years Pre/Post				
τ_{it}	–346.7***	17.7***	–14.7***	–14.0***
	(22.0)	(3.2)	(1.9)	(1.0)
1st stage coefficient on τ_{gt}^{sim}	0.59	0.59	0.59	0.59
1st stage F-statistic	744.6	744.6	744.6	744.6
N	643,397	643,397	643,397	560,541
Number of firms	169,586	169,586	169,586	150,049
Panel E: Mean Pre/Post				
τ_{it}	–475.6***	33.1***	–11.4***	–14.9***
	(33.4)	(5.3)	(3.1)	(1.3)
1st stage coefficient on τ_{gt}^{sim}	0.64	0.64	0.64	0.64
1st stage F-statistic	743.9	743.9	743.9	743.9
N	252,826	252,826	252,826	230,352
Number of firms	126,413	126,413	126,413	115,176

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. Panel A uses the yearly panel of firms with strictly positive values of the corresponding outcome in all years 2011–2017; Panel B restricts the yearly panel to firms with strictly positive cash deposits in all years; Panel C uses monthly data (April–December) aggregated to annual sums for all firms; Panel D uses a balanced panel of firms with deposits in the two years before and after repeal (2012–2015); Panel E uses firm-level means over the pre-repeal (2011–2013) and post-repeal (2014–2017) periods. The instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. Firm–years with zero total deposits are excluded from the share-of-cash regressions because the share is undefined for them. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A3: Full Sample, 2006 IV, Different Residualizations

	Cash Deposits	Non-Cash Deposits	Total Deposits	Share Cash
	(1)	(2)	(3)	(4)
Panel A: No Residualization				
τ_{it}	–478.0***	34.5***	–9.4***	–14.7***
	(31.8)	(4.6)	(3.0)	(1.2)
1st stage coefficient on τ_{gt}^{sim}	0.63	0.63	0.63	0.63
1st stage F-statistic	709.7	709.7	709.7	709.7
N	884,891	884,891	884,891	795,576
Number of firms	126,413	126,413	126,413	118,016
Panel B: Residualized by Size				
τ_{it}	–475.6***	33.1***	–11.4***	–15.1***
	(33.4)	(5.3)	(3.1)	(1.3)
1st stage coefficient on τ_{gt}^{sim}	0.64	0.64	0.64	0.64
1st stage F-statistic	743.9	743.9	743.9	743.9
N	884,891	884,891	884,891	795,576
Number of firms	126,413	126,413	126,413	118,016
Panel C: Residualized by Sector				
τ_{it}	–420.9***	31.8***	–7.7	–14.2***
	(46.7)	(8.3)	(6.9)	(2.6)
1st stage coefficient on τ_{gt}^{sim}	0.57	0.57	0.57	0.57
1st stage F-statistic	32.8	32.8	32.8	32.8
N	782,334	782,334	782,334	703,566
Number of firms	111,762	111,762	111,762	104,163
Panel D: Residualized by State				
τ_{it}	–498.7***	39.1***	–6.4*	–13.9***
	(32.2)	(4.8)	(3.8)	(1.1)
1st stage coefficient on τ_{gt}^{sim}	0.62	0.62	0.62	0.62
1st stage F-statistic	204.3	204.3	204.3	204.3
N	782,334	782,334	782,334	703,566
Number of firms	111,762	111,762	111,762	104,163

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. Panels A–D correspond to different residualization strategies: none, by firm size, by sector, and by state. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. Firm–years with zero total deposits are excluded from the share-of-cash regressions because the share is undefined for them. The sample consists of a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A4: Intensive Margin, 2006 IV, Different Residualizations

	Cash Deposits	Non-Cash Deposits	Total Deposits	Share Cash
	(1)	(2)	(3)	(4)
Panel A: No Residualization				
τ_{it}	–199.3***	47.0***	–7.1**	–25.8***
	(15.8)	(6.7)	(2.8)	(2.4)
1st stage coefficient on τ_{gt}^{sim}	0.46	0.46	0.46	0.46
1st stage F-statistic	222.6	222.6	222.6	222.6
N	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659
Panel B: Residualized by Size				
τ_{it}	–195.7***	47.5***	–6.9**	–25.8***
	(16.4)	(7.2)	(2.8)	(2.5)
1st stage coefficient on τ_{gt}^{sim}	0.49	0.49	0.49	0.49
1st stage F-statistic	214.2	214.2	214.2	214.2
N	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659
Panel C: Residualized by Sector				
τ_{it}	–196.2***	43.6***	–7.2	–25.2***
	(37.3)	(12.7)	(7.0)	(5.2)
1st stage coefficient on τ_{gt}^{sim}	0.36	0.36	0.36	0.36
1st stage F-statistic	18.0	18.0	18.0	18.0
N	389,431	389,431	389,431	389,431
Number of firms	55,633	55,633	55,633	55,633
Panel D: Residualized by State				
τ_{it}	–179.2***	39.1***	–7.1**	–23.1***
	(13.9)	(5.3)	(2.9)	(1.9)
1st stage coefficient on τ_{gt}^{sim}	0.50	0.50	0.50	0.50
1st stage F-statistic	169.6	169.6	169.6	169.6
N	389,431	389,431	389,431	389,431
Number of firms	55,633	55,633	55,633	55,633

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. Panels A–D correspond to different residualization strategies: none, by firm size, by sector, and by state. In columns 1–3, the dependent variable is the logarithm of the observed deposit value for firm i in year t. The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. The sample consists of firms with strictly positive cash and non-cash deposit values in all years of the analysis. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A5: Extensive Margin, 2006 IV, Different Residualizations

	Cash Deposits (1)	Non-Cash Deposits (2)	Total Deposits (3)
Panel A: No Residualization			
τ_{it}	–6.90***	–0.29	–1.11***
	(0.69)	(0.59)	(0.36)
1st stage coefficient on τ_{gt}^{sim}	0.63	0.63	0.63
1st stage F-statistic	709.7	709.7	709.7
N	885,080	885,080	885,080
Number of firms	126,440	126,440	126,440
Panel B: Residualized by Size			
τ_{it}	–6.96***	0.37	–0.50
	(0.68)	(0.54)	(0.38)
1st stage coefficient on τ_{gt}^{sim}	0.64	0.64	0.64
1st stage F-statistic	743.9	743.9	743.9
N	885,080	885,080	885,080
Number of firms	126,440	126,440	126,440
Panel C: Residualized by Sector			
τ_{it}	–3.09***	–0.63	–1.66**
	(1.00)	(1.26)	(0.67)
1st stage coefficient on τ_{gt}^{sim}	0.57	0.57	0.57
1st stage F-statistic	32.8	32.8	32.8
N	782,488	782,488	782,488
Number of firms	111,784	111,784	111,784
Panel D: Residualized by State			
τ_{it}	–6.85***	0.51	–0.46
	(0.65)	(0.66)	(0.46)
1st stage coefficient on τ_{gt}^{sim}	0.62	0.62	0.62
1st stage F-statistic	204.3	204.3	204.3
N	782,488	782,488	782,488
Number of firms	111,784	111,784	111,784

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. Panels A–D correspond to different residualization strategies: none, by firm size, by sector, and by state. Each dependent variable is a binary indicator equal to 1 if firm i has strictly positive deposits of that type in year t, and 0 otherwise. The sample consists of a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A6: Full Sample, 2018 Instruments

	Cash Deposits	Non-Cash Deposits	Total Deposits	Share Cash
	(1)	(2)	(3)	(4)
Panel A: No Controls				
τ_{it}	–458.2***	23.1***	–18.1***	–14.4***
	(30.1)	(3.7)	(2.7)	(1.2)
1st stage coefficient on τ_{gt}^{sim}	0.67	0.67	0.67	0.67
1st stage F-statistic	1301.4	1301.4	1301.4	1301.4
N	884,891	884,891	884,891	795,576
Number of firms	126,413	126,413	126,413	118,016
Panel B: Controlling for Size				
τ_{it}	–470.0***	26.7***	–15.8***	–14.5***
	(31.3)	(4.1)	(2.7)	(1.2)
1st stage coefficient on τ_{gt}^{sim}	0.79	0.79	0.79	0.79
1st stage F-statistic	1257.5	1257.5	1257.5	1257.5
N	884,891	884,891	884,891	795,576
Number of firms	126,413	126,413	126,413	118,016
Panel C: Controlling for Size and Variability				
τ_{it}	–455.9***	26.0***	–15.2***	–14.4***
	(29.6)	(4.2)	(2.6)	(1.2)
1st stage coefficient on τ_{gt}^{sim}	0.83	0.83	0.83	0.83
1st stage F-statistic	1350.8	1350.8	1350.8	1350.8
N	884,891	884,891	884,891	795,576
Number of firms	126,413	126,413	126,413	118,016

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from 2018 deposits. Panels A–C correspond to different control strategies: none, firm size, and firm size and variability. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. Firm–years with zero total deposits are excluded from the share-of-cash regressions because the share is undefined for them. The sample consists of a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A7: Extensive Margin, 2018 IV

	Cash Deposits (1)	Non-Cash Deposits (2)	Total Deposits (3)
Panel A: No Controls			
τ_{it}	–6.00***	–0.48	–1.02***
	(0.59)	(0.44)	(0.31)
1st stage coefficient on τ_{gt}^{sim}	0.67	0.67	0.67
1st stage F-statistic	1301.4	1301.4	1301.4
N	885,080	885,080	885,080
Number of firms	126,440	126,440	126,440
Panel B: Controlling for Size			
τ_{it}	–6.72***	–0.10	–0.62
	(0.65)	(0.53)	(0.38)
1st stage coefficient on τ_{gt}^{sim}	0.79	0.79	0.79
1st stage F-statistic	1257.5	1257.5	1257.5
N	885,080	885,080	885,080
Number of firms	126,440	126,440	126,440
Panel C: Controlling for Size and Variability			
τ_{it}	–6.86***	–0.07	–0.62
	(0.66)	(0.54)	(0.39)
1st stage coefficient on τ_{gt}^{sim}	0.83	0.83	0.83
1st stage F-statistic	1350.8	1350.8	1350.8
N	885,080	885,080	885,080
Number of firms	126,440	126,440	126,440

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from 2018 deposits. Panels A–C correspond to different control strategies: none, firm size, and firm size and variability. Each dependent variable is a binary indicator equal to 1 if firm i has strictly positive deposits of that type in year t, and 0 otherwise. The sample consists of a balanced panel in which firms are observed in all years of the analysis, with deposit values recorded as either positive or zero in each period. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A8: Intensive Margin, 2018 IV

	Cash Deposits	Non-Cash Deposits	Total Deposits	Share Cash
	(1)	(2)	(3)	(4)
Panel A: No Controls				
τ_{it}	–196.7***	32.7***	–17.0***	–23.3***
	(15.4)	(5.1)	(2.5)	(2.1)
1st stage coefficient on τ_{gt}^{sim}	0.51	0.51	0.51	0.51
1st stage F-statistic	331.9	331.9	331.9	331.9
N	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659
Panel B: Controlling for Size				
τ_{it}	–188.4***	35.4***	–13.6***	–23.1***
	(14.0)	(5.1)	(2.3)	(2.0)
1st stage coefficient on τ_{gt}^{sim}	0.63	0.63	0.63	0.63
1st stage F-statistic	450.4	450.4	450.4	450.4
N	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659
Panel C: Controlling for Size and Variability				
τ_{it}	–186.5***	34.9***	–13.5***	–23.1***
	(13.9)	(5.2)	(2.2)	(2.0)
1st stage coefficient on τ_{gt}^{sim}	0.66	0.66	0.66	0.66
1st stage F-statistic	422.3	422.3	422.3	422.3
N	431,613	431,613	431,613	431,613
Number of firms	61,659	61,659	61,659	61,659

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from 2018 deposits. Panels A–C correspond to different control strategies: none, firm size, and firm size and variability. In columns 1–3, the dependent variable is the logarithm of the observed deposit value for firm i in year t. The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. The sample consists of firms with strictly positive cash and non-cash deposit values in all years of the analysis. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A9: Results in Restricted Sample where Observe Heterogeneity Variables

	Cash Deposits		Non-Cash Deposits		Total Deposits		Share Cash	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced Form								
τ_{gt}^{sim}	–295.7***	–298.9***	21.7***	21.3***	–5.7***	–6.6***	–8.6***	–8.9***
	(14.0)	(15.9)	(2.3)	(2.8)	(2.0)	(2.1)	(0.5)	(0.6)
Instrumental Variable								
τ_{it}	–459.1***	–457.3***	33.7***	32.5***	–8.8***	–10.1***	–13.5***	–13.8***
	(32.7)	(34.0)	(4.5)	(5.2)	(2.9)	(3.0)	(1.2)	(1.3)
Control		Size		Size		Size		Size
1st stage coefficient on τ_{gt}^{sim}	0.64	0.65	0.64	0.65	0.64	0.65	0.64	0.65
1st stage F-statistic	667.9	743.3	667.9	743.3	667.9	743.3	667.9	743.3
N	734,937	734,937	734,937	734,937	734,937	734,937	660,264	660,264
Number of firms	104,991	104,991	104,991	104,991	104,991	104,991	97,772	97,772

Notes: The table reports instrumental variable (IV) estimates of β in equation (1), with the first stage in equation (6) and the simulated tax rate τ_{gt}^{sim} built from December 2006 deposits. In columns with "Size" in the "Control" row, the instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. Cash, non-cash, and total deposits are each divided by the average deposits of the same type among control firms in the same industry–state over the pre-repeal years 2011–2013; control firms are firms with December 2006 cash deposits of at most 15,000 pesos ($\tau_i^{sim} = 0$). The share of cash deposits is computed as the ratio of cash deposits to total deposits, $d_{it}^{\text{cash}}/d_{it}^{\text{total}}$. Firm–years with zero total deposits are excluded from the share-of-cash regressions because the share is undefined for them. The sample consists of the firms with nonmissing values of all five heterogeneity variables in Table 4. We remove linear pre-trends from each outcome: separately for firms above and below the median simulated tax rate, we estimate a linear trend over the pre-repeal years 2011–2013 and subtract it from the outcome in all years. The instrumental variable is set to zero for the years 2014–2017, reflecting the repeal of the tax. The reduced-form rows report the coefficient from regressing the outcome directly on τ_{gt}^{sim}. The first-stage coefficient and F-statistic in each column are from the first-stage regression in that column's estimation sample. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels. Subscripts g, i, and t denote industry–state, firm, and year, respectively.

Table A10: Economic Census Results

	IDE Repeal (2013, 2018)			IDE Repeal (2008, 2013, 2018)			Placebo Repeal (1993, 1998)		
Dependent variable	Log	Log	Normalized	Log	Log	Normalized	Log	Log	Normalized
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Revenues	−26.56***	−0.12***	−56.79***	−24.72***	−0.09***	−43.65***	−10.46**	−0.05	111.09*
	(6.95)	(0.04)	(9.47)	(5.99)	(0.03)	(7.66)	(4.21)	(0.03)	(62.88)
Non-labor inputs	−29.32***	−0.14***	−56.02***	−22.53***	−0.07*	−39.50***	−5.27	−0.02	−11.85
	(6.52)	(0.04)	(9.24)	(6.07)	(0.03)	(7.80)	(4.53)	(0.03)	(22.65)
Number of workers	−2.98**	−0.02**	−36.89***	−2.70***	−0.02***	−28.15***	−4.92	−0.01	−13.93
	(1.24)	(0.01)	(6.97)	(0.88)	(0.01)	(5.61)	(4.77)	(0.02)	(17.38)
Independent variable	τ_{gt}^{sim}	$\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$	τ_{gt}^{sim}	τ_{gt}^{sim}	$\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$	τ_{gt}^{sim}	τ_{gt}^{sim}	$\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$	τ_{gt}^{sim}
Control	Size	Size	Size	Size	Size	Size	Size	Size	Size
N	4,613,648	4,613,648	4,188,326	3,635,694	3,635,694	3,725,184	1,631,318	1,631,318	1,667,614
Number of firms	2,306,824	2,306,824	2,094,163	1,211,898	1,211,898	1,241,728	815,659	815,659	833,807

Notes: The table reports reduced-form regressions of firm outcomes from the Economic Census on the industry–state simulated tax rate τ_{gt}^{sim} (columns 1, 3, 4, 6, 7, and 9) or on the indicator $\mathbb{1}\{\tau_{gt}^{sim} > \text{p50}\}$ (columns 2, 5, and 8), with firm and census-year fixed effects. Columns 1–3 use the 2013 and 2018 waves, columns 4–6 add the 2008 wave, and columns 7–9 are placebo tests that assign the 2013 and 2018 values of the simulated tax rate to the 1993 and 1998 waves. The instrument is residualized on firm size: each firm's simulated tax rate is residualized on the logarithm of its total deposits in December 2006 before averaging within the industry–state. In the log columns, the outcome is the logarithm of the variable; in the normalized columns (3, 6, and 9), it is the variable divided by its industry–state mean in the 2003 wave (the 1993 wave in column 9). Each sample is a balanced panel of micro, small, and medium firms; the log columns are further restricted to firms with positive revenues and non-labor input costs in every wave. The number of workers includes the owner. Standard errors (in parentheses) are clustered at the industry–state level. Stars denote significance at the 1% (***), 5% (**), and 10% (*) levels.

B Comparison of Estimated Magnitudes with Literature

We seek to compare the magnitude of our main estimates from Section 4.2 to the literature. To make the comparison meaningful, we focus on studies that estimate the impact on the value of financial transactions of a change in the tax rate on financial transactions.

To the best of our knowledge, there are no studies which estimate the impacts on tax rates on cash deposits; more generally, there are limited rigorous estimates of the impacts of bank transaction taxes. Therefore, we have to expand our search to include other forms of financial transactions. We focus on taxes on transactions that occur frequently and that could, in principle, have a highly elastic base. Following the typology of Matheson (2011), these include securities transaction taxes, which apply to trades in equity, debt, or their derivatives; currency transaction taxes (Tobin taxes), which apply to foreign exchange trades; and capital levies, which apply when firms raise capital by issuing shares or bonds or by borrowing. We exclude taxes on insurance premiums and on real estate transactions.

We found four papers in the literature which studied these types of transaction taxes while providing enough detail to allow us to create comparable estimates with our own setting. On the latter, we specifically require knowing the magnitude of the tax rate change under the reform and the magnitude of the percent change in the value of transactions. In turn, we can convert these papers' results into a β coefficient that would result from the following regression:

$$\log(y_{it}) = \beta \cdot \tau_{it} + \gamma_i + \gamma_t + e_{it}. \tag{13}$$

Due to the resemblance to equation (1), the coefficient β can directly be compared to the coefficients estimated in Table 2 on the intensive margin. For the results in the full sample (Table 1), whose outcomes are ratios, we convert each estimate to the coefficient of a log-linear specification that implies the same percent change at the average change in the tax rate, $\hat{\beta} = \ln(1 + \%\Delta/100)/\Delta\tau^{\text{avg}}$. For cash deposits, the implied coefficient is $\hat{\beta} = -134$ (column 2); for total deposits, it is $\hat{\beta} = -9.1$ (column 6).

Across the comparable papers, we can summarize the setting, reform of interest and the implied $\hat{\beta}$:

- Coelho (2016) studies the taxes on trades in shares of large domestic companies that France introduced in 2012 (0.2% on purchases, for firms with market value above 1 billion euro) and Italy in 2013 (0.1%–0.2%, above 500 million euro). In both cases, the outcome of interest is log turnover, the number of shares traded over shares outstanding, and the control group is Dutch, Belgian and Spanish firms above the same thresholds. This study finds results which imply $\hat{\beta}$ between -135 and -264.

- Umlauf (1993) studies Sweden's tax on equity trades, introduced at 1% (round trip) in 1984 and raised to 2% in July 1986. He cites a Stockholm Stock Exchange estimate that 60% of trading in Sweden's 11 most-traded share classes moved to London after the 1986 increase was announced, which implies $\hat{\beta} = -92$.

- Baltagi et al. (2006) use data from the Shanghai and Shenzhen stock markets to analyze China's 1997 increase in the stamp tax on stock trades from 0.3% to 0.5%. The implied $\hat{\beta}$ is -223.

- Chou and Wang (2006) study a reform where the Taiwanese government reduced the tax levied on futures transactions on the Taiwan Futures Exchange from 0.05% to 0.025%. The implied $\hat{\beta}$ is -447.

The range of estimated magnitudes is broad, from -92 to -447, and our IDE estimates for cash deposits in the full sample ($\hat{\beta} = -134$) and on the intensive margin ($\hat{\beta} = -195.7$) lie within this range. On the other hand, our estimated magnitudes for total deposits in the full sample and on the intensive margin ($\hat{\beta} = -9.1$ and $\hat{\beta} = -6.9$, respectively) are substantially smaller than the other findings from the literature.

The extent to which our results are comparable to these papers may reflect the underlying differences in context, where the other papers' settings are mainly in high income countries, with a focus on sophisticated financial agents and more complex types of transactions. Moreover, the other studied reforms are mainly guided by a need for revenue and a desire to reduce market volatility and risk, rather than combating informality. In this sense, the fact that our estimates are within the range of these papers' estimates might even be considered surprising. On the other hand, the comparability of magnitudes across studies perhaps reflects the simple fact that, despite important differences in the underlying financial environment and policy objectives, the financial transaction bases that are affected by the tax rate changes are, in our setting as in the other papers, all highly elastic.

Appendix References

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