

Who Writes the Check to the Government Does Matter: Evidence from Firm-to-Firm Links

Lorenzo Pessina*

December 10, 2020

Job Market Paper

Please click [here](#) for the most recent version

The design of tax collection is largely considered to be inconsequential in the academic literature, yet policy makers frequently implement reforms to it. Combining a new administrative dataset on firm-to-firm links from Italy and a quasi-experimental research design, I study how firms and markets adapt to a reform of the collection of Value Added Tax (VAT). The reform shifted the responsibility to remit payments of VAT from sellers to “trusted” buyers, such as government entities and large firms. I present three main findings. First, firm-to-firm links subject to the new rules are more likely to become inactive after the introduction of the new rules. Second, I find that the reform was costly for the average firm. Firms more exposed to the reform experienced lower sales and higher exit rates, relative to the counterfactual. Third, I document that the burden of the reform is not evenly distributed across firms. Small firms are hit hardest, while large firms do not appear to be negatively affected. As a result, I show that markets more exposed to the reform became more concentrated.

*Department of Economics, Columbia University. Email: l.pessina@columbia.edu. I am grateful to Wojciech Kopczuk and Michael Best for their continuous guidance and support. I thank Arun Advani, Paul Bouscasse, Anne Brockmeyer, Rita de la Feria, François Gérard, Gaia Dossi, Cameron LaPoint, Alessandro Santoro, Maggie Shi, Eric Verhoogen, José Scheinkman and participants to the Applied Microeconomics colloquium at Columbia University for insightful comments and suggestions. I am thankful to Alfonso Carfora and Stefano Pisani from Agenzia delle Entrate for sharing the administrative data and making this work possible. All errors are mine.

1 Introduction

How do firms and markets respond to reforms to tax collection? Does it matter whether it is the buyer or the seller who “writes the check to the government?” It is widely accepted that the amount of taxes affects the operations of businesses.¹ Tax collection, instead, is often considered to be inconsequential in the academic literature. Indeed, several public finance textbooks and advance surveys suggest that firms should ignore tax collection ([Gruber 2015](#); [Kotlikoff and Summers 1987](#)).

In fact, policy makers often alter the side of the market that is responsible to collect and remit taxes to the government. Firms might need to adapt their operations to comply with these rules for several reasons. First, tax collection changes cash-flows between the two sides of the market. When the seller holds the responsibility to remit taxes, she receives the net price plus taxes at the time of the transaction. When the buyer remits taxes to the government, the seller receives the net price only. Second, tax collection affects the opportunities to evade. The side of the market that is supposed to remit taxes may fail to do so and evade the tax liability.

In this paper, I quantify the response of firms and markets to a reform to the collection of Value Added Tax (VAT). To do this, I use a new administrative dataset on firm-to-firm links from Italy and a quasi-experimental research design. The reform shifted the responsibility to remit payments of VAT from sellers to “trusted” buyers, such as government entities and large firms. I present three main findings. First, firm-to-firm links subject to the new rules are more likely to become inactive after the introduction of the new rules. Second, I find that the reform was costly for the average firm. Firms more exposed to the reform experienced lower sales and higher exit rates, relative to the counterfactual. Third, I document that the burden of the reform is not evenly distributed across firms. Small firms are hit hardest, while large firms do not appear to be negatively affected. As a result, I show that markets more exposed to the reform became more concentrated.

To establish a causal link between collection of VAT and firm behavior, I leverage a reform implemented in Italy. In 2015, the government shifted the responsibility to remit payments of VAT from the seller to the buyer for all transactions between firms and a subset of government entities. These rules were later extended to a broader set of entities in mid-2017. For each firm, I build a measure of exposure to the reform based on the share of pre-reform sales that would be subject to the new rules. In a difference-

¹See for example: [Suárez Serrato and Zidar \(2016\)](#); [Fuest, Peichl and Siegloch \(2018\)](#); [Yagan \(2015\)](#); [Zwick and Mahon \(2017\)](#); [Chen, Jiang, Liu, Suárez Serrato and Xu \(2019\)](#); [Auerbach \(2002\)](#); [Chetty and Saez \(2005\)](#); [Tørsløv, Wier and Zucman \(2020\)](#).

in-difference framework, I then compare firms that are relatively more exposed to the reform to firms that are less exposed to the reform.

To further clarify the logic of the exercise, consider two furniture manufacturers that sell office supplies to government and corporate clients. One of them predominantly sells its product to public entities, while the other to corporate clients. They follow the same set of rules to compute their VAT liability. However, the reform affects them differently because they serve separate sets of clients. The manufacturer doing business with government entities obtains lower cash-flows from its clients at the time of the transaction because it no longer receives VAT from its clients. The other manufacturer, instead, continues to collect VAT from its clients. The reform to the collection of VAT alters the availability of liquid funds, which are a key source of financing for many small and medium sized enterprises.

The research design of the paper is based on the assumption that suppliers more exposed to the reform would have behaved similarly to less exposed suppliers. Several factors suggest that this is indeed the case. First, the two groups of firms are similar on a number of observables at baseline. Second, they are on parallel trends before the introduction of the reform.² Finally, among government entities, the distinction between affected and non-affected ones appear somewhat arbitrary. The general principle underlying the classification is that “economic” government entities fall within the scope of the reform, whereas “non-economic” ones do not, but there is a substantial number of exceptions to this general principle.³

To analyze the response of firms and markets to VAT collection, I exploit a new administrative dataset on firm-to-firm links from the Italian tax authority (“Agenzia delle Entrate”). This dataset captures the entire size distribution of firms, from small firms with few hundred thousand euros in annual revenues to large businesses. The possibility to span the entire distribution, including very small firms, allows me to perform detailed heterogeneity analysis. The dataset contains a random sample of sellers from three of the largest regions in Italy (Lombardy, Lazio, and Campania) for the years 2014-2016, covering around 40% of national value added.

Moving to the empirical analysis of the paper, I find that the reform reduced the pro-

²I am able to provide evidence in support of the parallel trend assumption for variables measured at the infra annual frequency.

³It seems rather odd that all agencies supervising sea ports are exempted, while those supervising rivers are not. Departments of the national executive (including the Ministry of Economy and Finance) are subject to the reform, but the tax authority and the central bank are not. All public universities must apply the new rules, yet government agencies administering scholarships for university students are exempted.

portion of firms that regularly remit VAT to the tax authority by almost 20 percentage points. This result can be interpreted as the first stage of the reform. On the intensive margin, sellers reduce the average monthly payment of VAT by 30 percent. This represents a sizeable decline in the amount of cash-flows that each transaction generates, especially for firms that used to remit periodic payments to the tax authority and found themselves in a credit position vis-à-vis the tax authority.

To illustrate the response of firms and markets to VAT collection, I organize the results in three main sets. First, I show that the reform was costly for the average firm. Firm-to-firm links subject to the new rules are 2.5 percentage point more likely to become inactive. This altered the composition of buyers for sellers more exposed to the reform. They became less likely to continue trading with an affected client. Moreover, the reform caused a decline in reported sales by 2.2 percent and increased the exit probability by 1.1 percentage points on average. These results suggest that the reform to tax collection is far from irrelevant and imposes significant costs to firms.

Second, I document that the burden of the reform was not evenly distributed across firms. Small firms were hit hardest while large businesses appear to be unaffected, relative to the counterfactual. The reform lowered immediate cash-flows to the seller and reduced its opportunities to evade. Smaller firms are both more likely to evade and to have limited access to external funds. Consistent with this, small firms exhibit higher exit rates relative to large firms. Large businesses have more flexibility to undo the negative impact of the reform.

Third, moving the analysis at the aggregate level, I show that tax collection impacts the structure of markets. In particular, tax collection of VAT increased market concentration in markets more exposed to the reform. A 10 percentage point higher exposure led to an increase of 40 points in the Herfindahl-Hirschman Index. This result suggests that the reform caused a reallocation of economic activity from small to large firms, therefore increasing market concentration. It provides further evidence that tax collection is far from neutral and it does have economic consequences.

This paper speaks to several strands of the literature. First, it relates to a growing literature in public finance that quantifies the behavioral response of firms to tax administration and, in particular, to the point of collection of a tax. Public finance textbooks ([Gruber 2015](#)) and more advanced surveys ([Kotlikoff and Summers 1987](#)) make the case for the irrelevance of statutory incidence, at least in a partial equilibrium setting. Yet, recent papers challenge this view and provide empirical evidence that the point of collection of a tax has real economic effects. For example, assigning statutory incidence to the side of the market least likely to evade has been proven successful in improving com-

pliance (Kopczuk, Marion, Muehlegger and Slemrod 2016; Brockmeyer and Hernandez 2019). In a qualitative discussion, Slemrod (2008) suggests that holding the responsibility to remit is beneficial to the firm because it increases cash-flows. A related branch of literature has identified rationales why individual taxpayers are not neutral to different regimes of tax collection, such as incomplete adjustment to withholding rules (Jones 2012), underreaction to non-salient taxes (Chetty, Looney and Kroft 2009) and asymmetric pass-through of payroll taxes depending on whether the statutory incidence falls on the employer or on the employee (Saez, Matsaganis and Tsakloglou 2012).

In addition, this paper adds to the literature that exploits the availability of domestic firm-to-firm links to investigate how businesses are affected by their trading partners.⁴ Being the first to use firm-to-firm links from Italy, this paper provides evidence on how tax collection affects patterns of trade between firms. Within this burgeoning strand of the literature, a handful of papers analyze how the tax system affects firm-to-firm relations. For example, Pomeranz (2015) shows that the positive effects of audits propagate along the supply chain in Chile thanks to the credit-invoice system of VAT, whereas Gadenne, Nandi and Rathelot (2019) show that firms are more likely to trade with business with the same VAT registration status.

Finally, as the inversion of statutory incidence of VAT reduces cash-flows to seller, this paper adds to the empirical corporate finance literature that studies the role of cash-flows in firms' business decisions. A large empirical literature has shown that cash-flows affect investment (Fazzari, Hubbard and Petersen 1988; Kaplan and Zingales 1997; Lamont 1997; Rauh 2006; Meyer and Kuh 1957), and employment (Benmelech, Bergman and Seru 2011; Chodorow-Reich 2014; Barrot and Nanda 2020). Using surveys of firms in Vietnam, McMillan and Woodruff (1999) show that firms are more likely to conduct business with firms that have more generous trade credit policies.

The rest of the paper proceeds as follows. Section 2 provides background on VAT and the policy reform studied in the paper. Section 3 describes the data, while Section 4 lays out the empirical strategy of the paper. Section 5 shows the first stage of the reform and its effect on the amount of VAT remitted by firms exposed to the reform. Section 6 documents the effects of VAT collection on business links and Section 7 on firms. Section 8 describes the heterogeneity in the treatment effect and Section 9 shows the effects of the reform at the aggregate level. Section 10 concludes.

⁴Examples include papers using data from Belgium (Dhyne, Kikkawa and Magerman 2018; Tintelnot, Kikkawa, Mogstad and Dhyne 2018), Japan (Bernard, Dhyne, Magerman, Manova and Moxnes 2019; Furusawa, Inui, Ito and Tang 2018), the US (Barrot and Sauvagnat 2016), Chile (Huneus 2018) and Turkey (Demir, Javorcik, Michalski and Ors 2019).

2 Background on VAT Collection

To analyze the response of firms to the collection of VAT, I exploit a reform that shifted the responsibility to remit VAT from the seller to the buyer for all transactions between firms and a subset of government entities. This section provides details on the administration of VAT and on the rules around public procurement in Italy.

2.1 Periodic Payments of VAT

Italy introduced a broad-based VAT on sales of goods and services in the early 1970s, at around the same time as other Western European countries. Today, VAT represents one of the main sources of government revenues, accounting for 14.8% of total tax receipts in 2017 (OECD 2019b). The seller of taxable goods and services is responsible to remit VAT to the tax authority. For each firm, the tax base is the difference between taxable sales and taxable purchases recorded over a calendar year.

Firms are required to make payments of their VAT liability throughout the year. The frequency of payments depend on the size of the firm. Larger firms make payments every month, while smaller firms every quarter⁵. At the end of every calendar year, firms file an annual VAT return where they reconcile any discrepancy between payments made throughout the year and the actual tax liability. Firms in a debit position are required to remit payment of any amount outstanding, while firms in a credit position may carry forward the credit or ask for a cash refund.

VAT liabilities and credits experience an asymmetric treatment from the tax authority. Firms that report a tax liability are required to make payments to the tax authority within 45 days after the end of the filing period. On the other hand, businesses that report a credit could either ask for a cash refund or carry forward the tax credit⁶. More than 90% of firms chooses the second option. This is mainly due to the fact that asking for a cash refund involves additional costs, stemming from additional checks that the tax authority performs on firms' books and the time-delay with which the tax authority fulfills requests for cash refunds.

⁵The sales threshold for making monthly VAT payments is €400,000 for services firms, whereas it is €700,000 for all other firms. The frequency of payments in year t depends on annual sales reported in year $t - 1$.

⁶A third option would be to sell the credit to a third party, but this option is used very rarely.

2.1.1 Compliance Gaps in VAT

While VAT is generally considered to be easy to enforce because the credit-invoice system creates a long paper trails on transactions between firms (Pomeranz 2015), the economic literature has begun to analyze some issues with the administration of VAT. For example, VAT compliance is significantly lower at the retail stage because the credit-invoice system breaks down when the buyer is a final consumer (Naritomi 2019). Another issue with VAT compliance that received recent scrutiny are so-called invoice mills, that is firms whose sole purpose is to produce invoices used as deductions by other businesses (Waseem 2020).

Italy has a large VAT compliance gap relative to other developed countries (European Commission 2020; Ministero dell’Economia e delle Finanze 2019; D’Agosto and Santoro 2019)⁷. As a result, the Italian government has introduced a number of programs to increase VAT compliance among firms and customers. The reform studied in this paper has been introduced against this background.

2.2 Quasi-experimental variation in statutory incidence

In 2015, the government shifted the responsibility to remit VAT from the seller to the buyer for all transactions between a firm and some government entities in 2015. Before the reform, the seller used to receive the tax-inclusive price from the buyer. After the reform, the seller obtains the tax-exclusive price, while the buyer remits the VAT to the tax authority. This reform did not alter the tax liability of the reform.

Even though these new rules required approval by the Italian parliament and unanimous consent by the European Union⁸, they were approved on a relatively fast timeline. The reform was first presented by the government as one the many interventions that are bundled together in the annual budget process in mid-October 2014. After obtaining the final approval by the Italian parliament in late December, the new rules entered into force on January 1st, 2015. While the green light from the EU came in later on July 14th, 2015, the executive and the tax authority repeatedly issued binding guidelines on implementation and enforcement of the law⁹. Moreover, government entities affected by the reform issued notices to their suppliers that they would comply with the new rules and start withholding payment of VAT.

⁷European Commission (2020) estimates the VAT compliance gap to reach 23.8% in Italy in 2017.

⁸While the main VAT rates are freely set by each EU country, all other aspects related to the implementation of VAT (including its collection) must be approved by the EU.

⁹The Ministry for Economic Affairs and Finance issued binding guidelines on 1/23/2015 and the tax authority followed on 2/9/2015.

The government broadened the set of transactions affected by the reform in mid-2017. This extension of the reform was not anticipated at the time of the first roll out of the policy in 2015. Yet, it created a set of clients that were not affected in the first phase of the reform that would later be affected by the same rules. These clients are (i) government entities that were exempted in the first phase, (ii) firms owned or controlled by the central and local government, (iii) firms traded on the Milan stock exchange and included in the FTSE MIB index.

This staggered implementation of the reform allows me to distinguish between clients that had to apply the new rules on VAT collection in 2015 and those that were exempted at first and had to apply the new rules on tax collection in mid 2017. The empirical strategy of this paper relies on this staggered implementation of the reform to create a treatment and control group.

2.3 Similar Reforms in Other Countries

Several countries adopted policies similar to the one studied in this paper. The US came quite close to implementing an almost identical reform that would have introduced a withholding provision on payments to government contractors. This section situates the reform implemented in Italy in the broader effort of governments around the world to curb tax evasion.

Several countries have shifted the responsibility to remit VAT from the seller to the buyer for a subset of transactions in the economy. This type of reforms is commonly known as “*reverse charge*.” Developing and developed countries have adopted it over the past decades. For example, the UK has implemented a reverse charge for all domestic business transactions involving mobile phones, computer chips, and wholesale gas and electricity¹⁰. Similarly to the reform implemented in Italy, the reverse charge assigns the responsibility to remit VAT to the buyer. However, the UK reform usually includes provisions that impose joint and several liability on the seller if the buyer fails to ultimately remit VAT to the tax authority. The reform in Italy does not impose any liability on the seller for the buyer’s failure to remit payments of VAT.

In developing countries, governments have introduced a number of withholding provisions on VAT that also result in deviation from the general rule. In Peru, for example, the buyer is required to withhold a fraction of the value of the transaction and deposit it on a separate bank account. The seller can access those funds to settle tax liabilities

¹⁰The UK tax authority provides a detailed and practical guidance on the domestic reverse charge implemented in the UK is available at <https://www.gov.uk/guidance/the-vat-domestic-reverse-charge-procedure-notice-735>.

within the first three months of the transaction.

The US came close to enacting a reform similar to the one passed in Italy. In 2006, the US approved a 3 percent withholding on all payments from federal, state and local governments. The amount withheld would be then be credited towards the firm's income tax liability. Firms would be able to reconcile withheld taxes with their tax liability on their annual returns. The law was scheduled to enter into force in 2009, but Congress delayed its implementation twice before repealing it in 2013. The reforms were both justified on the grounds of limiting tax evasion by government contractors.

While these reforms are introduced to limit tax evasions, businesses often complain about their costs. In particular, the impact of tax collection of cash-flows is one of the top concerns raised by firms. Indeed, this was one of the main reasons why the US Congress finally repealed the reform before it became effective. In Italy, business associations were vocal in their opposition to the reform and the trade group supporting construction companies is now suing the Italian government to obtain relief from the costs associated with the reform.

2.4 Government Procurement

Government procurement spans a large set of industries and its weight in the Italian economy is sizeable: it accounts for 10.4% of GDP in 2017 ([OECD 2019a](#)). In addition to the executive branch, the perimeter of government includes the majority of universities, schools, and hospitals. This is higher than the US, but in line with other European countries. Therefore, the reform to the collection of VAT affected a rather diverse set of industries and firms.

Transactions between firms and government entities receive more scrutiny relative to other business transactions. All public offices in Italy are subject to binding transparency requirements set by the European Union. As the size of the transaction increases, the publicity requirements increase as well. For goods and services that are expected to cost more than €40,000, government entities are required to use an action to select the winning firm. Whereas, contracts below the threshold of €40,000, government offices can procure goods or services without the need of a public tender. Despite all the rules on transparency, public procurement is not immune from instances of corruption or collusion among bidders ([Conley and Decarolis 2016](#); [Tulli 2019](#)).

3 Data

This section describes the main datasets used to measure the response of firms and markets to a reform to VAT collection.

3.1 Firm-to-Firm Links and VAT Returns

The main dataset contains information on firm-to-firm links for a random sample of sellers, for the years 2014-2016. This information comes from mandatory information reports that firms must file annually with the tax authority. Firms in this dataset are then linked to their annual VAT returns via unique identifiers. In this section, I describe the data sources and the process to select the samples.

For the purpose of this analysis, the tax authority created a stratified random sample of 100,000 firms. The stratification is based (i) on the geographic location of firms and (ii) on the pre-reform relationships of firms. Firms in my sample are located in three large Italian regions (Campania, Lazio, and Lombardia), accounting for slightly less than 40% of total value added in Italy. While this sample is not nationally representative, it captures the wide geographic heterogeneity within Italy since Lombardia is in the North, Lazio in the Center, and Campania in the South.

From this sample, I exclude firms with zero or negative sales. I also eliminate firms below the minimum VAT registration threshold, to limit the issues stemming from selective registration below the threshold. The exemption threshold for VAT registration in Italy ranges between €15,000 and 40,000 of annual sales, depending on the firm's industry. This leaves me with 63,267 firms in the *full sample*. For the main analysis on firm outcomes, I further restrict the sample to firms that have pre-reform relations with either affected clients or non-affected clients. This is the *analysis sample* and it includes 14,987 firms.

In addition to firm-to-firm links, this paper uses annual VAT returns for the universe firms registered in the same three regions. The dataset contains line-by-line items from annual VAT returns for all firms. Businesses are required to provide detailed information on sales, purchases, value added and to compute any VAT liability outstanding at the end of the fiscal year. In case firms are due a refund, they also file an annual return to settle their credit position. Some firms are not required to file an annual VAT return and therefore they do not appear in the dataset. These include (i) firms that exclusively perform domestic VAT-exempt transactions, (ii) firms that adopt a simplified regime to

calculate their tax liabilities¹¹, (iii) agricultural firms.

Table 1 provides summary statistics for the main variables in the sample. The median firms reports sales of €307 thousand at baseline, with the 10th and the 90th percentiles at €57 thousand and €3.5 million respectively. The distribution is skewed to the right as the mean is at 1.5 million thousand. Turning to tax variables, the median value added is €113 thousand, while the median tax base is even higher at €268 thousand. All costs measures do not include labor costs, as these expenses are not deductible for VAT purposes. Following standard practice with firm-level data, all variables are winsorized at the 99th percentile to limit the influence of outliers. In the main specifications, all variables enter in logs, while value added and tax base are scaled by baseline sales.

3.2 Financial Accounts

I use financial accounts of all registered firms in Italy to compare the distribution of firms across industries and provinces in the VAT dataset. This data is obtained from the Amadeus dataset from Bureau van Dijk. It includes the universe of limited liability entities, excluding most of sole proprietorship and unincorporated partnerships.

4 Empirical Strategy

To quantify the response of firms to the reform of VAT collection, I adopt a difference-in-difference framework. I compare firms with pre-reform relationships with affected clients and firms with pre-reform relationships with non-affected clients. Moreover, to reflect the intensity of these relationships, I measure the share of pre-reform business sales going to affected clients.

4.1 Measuring Reform Exposure at the Firm Level

I define the measure of intensity of the reform as the share of pre-reform sales to affected clients. In the economy, there are firms that develop highly specialized relationships with a handful of clients, while others do not specialize and have thousands of clients. As the reform targeted a subset of transactions in the economy, businesses vary

¹¹In 2014, there were three simplified regimes. In 2015, the government added a fourth one, which became the only simplified regime in force starting from 2016. All of these regimes are open to small firms with annual sales below some regime-specific threshold. However, they all prescribe additional eligibility requirements that further restrict the pool of eligible firms (More details on this in the Appendix).

in the degree of exposure to the new rules depending on their pre-reform relationships. Conditional on serving at least one affected client in 2014, firms differ in the share of business sales going to affected clients. To capture this ex-ante heterogeneity, the measure of exposure is the share of business sales going to affected clients.

Formally, let B_f be the set of buyers of goods and services produced by firm f . Then, consider the partition of B_f between clients that are affected by the new rules, denoted as B_f^A , and those that are not B_f^N . The latter group of clients continue to send the net price and VAT to its sellers after the reform, whereas the former withholds payment of VAT to its sellers. Let's denote the annual value of the link between firm f and buyer i as y_{if} . Then, the total value of business sales for firm f is $y_f = \sum_{i \in B_f} y_{if}$. With this notation, I define the exposure to the reform of VAT collection as the ratio of sales to affected clients to total sales:

$$\text{Exposure}_f = \frac{\sum_{i \in B_f^A} y_{if}}{y_f}$$

This measure of exposure is bounded between 0 and 1 by definition. The higher its value, the more exposed the firm is to the reform.

Figure 1 reports the distribution of this variable for all firms, conditional on having at least one pre-reform relationship with affected clients. The figure shows that firms are quite heterogeneous in their exposure to government entities as they span the entire interval between 0 and 1. Moreover, the distribution appear to be bi-modal with a significant mass of firms that are completely specialized and others that generate only a small share of business sales from government entities. Finally, to report summary statistics, the mean of *Exposure* is 0.116 with a standard deviation of 0.243.

4.2 Measuring Reform Exposure at the Market Level

To assess whether the adjustment affected the structure of markets, I compare markets more exposed to the reform to markets less exposed. Mirroring the empirical strategy adopted at the firm level, I measure the exposure to the reform at the market level as the share of business sales going to government entities, that is:

$$\text{Market exposure}_m = \frac{\sum_f y_f}{Y_m}$$

where m identifies a market and f firms within each market. Y_m represents total sales in market m . To ease notation, this variable does not have time subscript as it

pertains to pre-reform information. Markets exhibit a high degree of heterogeneity in terms of the prevalence of government procurement.

5 First Stage: Effect on VAT Payments

This section shows that assigning the responsibility to remit VAT to the buyer significantly reduced periodic payments of VAT by the seller. Heuristically, this can be interpreted as the first stage of the reform. Moreover, this section shows evidence that treated and control firms were on parallel trends before the reform, lending credence to the empirical design of the paper. Finally, I discuss the economic significance and magnitude of the shock for the median firm in the analysis sample.

5.1 Extensive Margin Results on VAT Remittance

The first outcome to consider is whether the reform reduced the proportion of firms that periodically remit VAT to the tax authority. Firms are required to compute their VAT position every quarter or every month for large firms. If any VAT is due, they must remit payment within the tax deadline. If they report a VAT credit, firms may decide to carry forward the credit or ask for a cash refund. In either case, they need to report the VAT balance in each period.

To quantify the effect of the reform on VAT remittance, I estimate the following event-study specification:

$$y_{ft} = \sum_{s \neq 0} \alpha_s \cdot Exposure_f + \sum_{s \neq 0} \beta_s \cdot Exposure_f \cdot \mathbb{1}\{Quarter_s = t\} + x'_{ft}\gamma + \delta_t + \varepsilon_{ft} \quad (1)$$

where $Exposure_f$ captures the exposure to the reform defined in Section 4.1, $\mathbb{1}\{Quarter_s = t\}$ is a dummy equal to one when the quarter equals t , $x'_{ft}\gamma$ include industry by filing period and province by filing period fixed effects to flexibly control for time trends common to all firms within each industry and province. In the extensive margin model, the dependent variable is an indicator variable equal to 1 for firm f when it remits any positive amount of VAT in period t . Standard errors are clustered at the firm level.

Figure 3 plots the estimate $\hat{\beta}_s$ from the above specification obtained via OLS. These coefficients represent the reduced-form effect of the reform on the probability to remit any VAT payment in a given filing period. I report results separately for monthly and

quarterly filers in order to deal with differences in the frequency of periodic VAT deadlines across these two groups of firms.

The results show a sharp and persistent drop in the probability of remitting any VAT among firms with pre-reform relationships with affected clients. To quantify the effect, Figure 3 shows that 10 percentage point higher exposure to the reform leads to a 2 percentage point decline in the probability of remitting any VAT, by the end of the second quarter following the reform. Moreover, the size of the effect remains stable afterwards. Finally, the probability of reporting a VAT credit increased by a similar amount.

Estimates are robust across specifications and are reported in Tables D.3. Columns (1) and (4) include fixed effects by industry-year and province-year, while Columns (2) and (5) have fixed effects by industry-filing period and province-filing period. Finally, results are broadly unchanged when defining VAT payments and credits on a gross rather than net basis. These results are in Column (3) and Column (6).

In terms of internal validity of the research design, these figures provide a visual inspection of the parallel trend assumption. The underlying identifying assumption of the research design is that firms less exposed to the reform offer a valid counterfactual for those firms that are more exposed. The periodic VAT balance reported on annual VAT returns of firms provides a high-frequency view of firms' operations before the introduction of the reform.

Indeed, Figures 3 and 4 show that there is no evidence of diverging trends between firms with pre-reform relations with affected buyers and those without in the period before the reform. These results lend credence to the key identification assumption that underpins the research design of the paper. In the Appendix, I report results estimated for the sample of quarterly filers, which also support the identification assumption of this research design.

5.2 Intensive Margin Results

Figure 5 shows the effect of the reform to VAT collection on the value of periodic VAT payments in panel (a) and of periodic VAT credits in panel (b). Similarly to the extensive margin results, the sample contains all monthly filers in the analysis sample and the specification includes fixed effects by industry-filing period, province-filing period, and firm.

The results show that the reform induced a sharp and large decline in the amount of VAT remitted by firms. In particular, 10 percentage point higher exposure leads to

a 20 percent decline in periodic VAT payments to the tax authority. For the remaining periods, the magnitude of the effect is broadly unchanged. As shown in Tables D.4 and D.5, results are robust to the inclusion of fixed effects by industry-year and province-year.

5.3 Magnitude of the Shock

At this point, it is worth assessing the size of the shock for the median firm in the analysis sample. One potential concern would be that the size of the reform is relatively small for most firms. In this section, I provide details on the magnitude to the reform to assuage this concern.

First, baseline monthly VAT payments amount to €17,000 which represent 24 percent of estimated monthly sales¹², whereas VAT credits are 13 percent of monthly sales, on average. These statistics are calculated on the full sample, thus including firms that make no VAT payment or report zero VAT credit in some filing period. If instead we consider firms that regularly remit payments or report a credit, the ratio of VAT payments and credits to monthly sales increases¹³.

Second, the share of sales subject to the inversion of statutory incidence is sizeable. Conditional on doing business with at least one affected client, the share of sales subject to the reform is 10 percent for the average firm. This increases to 19 percent if we exclude sales to final consumers and exports, which are not reported in the firm-to-firm dataset. Similarly, the share of VAT due on sales subject to the reform is 18 percent of total VAT.

6 Effects of VAT Collection on Business Links

This section presents the effects of VAT collection on firm-to-firm links. I find that the reform had an adverse effect on business links on the extensive margin. Firm-to-firm links subject to the new rules were 2.5 percentage point more likely to become inactive, relative to the counterfactual. On the intensive margin, the reform led to a 3.4 percent decline in the value of transactions. While the point estimate is not statistically different from zero, the 95 percent confidence interval excludes an increase larger than 0.3 percent. This suggests that the reform did not increase the reported value of links

¹²VAT returns report sales at the annual frequency. To obtain an estimate for monthly sales, I divide the annual value by 12.

¹³Firms that regularly remit payments are defined as those that make a payment for at least 8 months in a year.

that remain active throughout the reform.

6.1 Specifications

To conduct the analysis at the link level, I adopt the following specification:

$$y_{lft} = \beta_0 + \sum_{s \neq 0} \beta_s \cdot T_l \cdot \mathbb{1}\{Year_t = s\} + x'_{lft} \gamma + \delta_t + \varepsilon_{lft} \quad (2)$$

where each observation is a firm-to-firm link l for firm f in year t . The dummy T_l indicates whether the link is subject to the new rules on tax collection. The dependent variable y_{lft} is either the probability that the link is active in year t or the net value of the link. A link is active if the annual value of transactions is above €1,000. The vector x_{lft} contains fixed effects for firm, industry by year, and province by year. In specifications without firm fixed effects, it also includes pre-determined controls for the number of links and total sales of each seller. The sample of firm-to-firm links include links that account for at least 0.1% of firm sales to other businesses. This sample restriction ensures that results are not driven by firms that are marginally important. Results estimated on the full sample of firm-to-firm links are reported in the Appendix. Standard errors are clustered at the seller level.

To understand whether the reform changed the types of clients that firms are trading with, I run the following specification at the firm-level:

$$y_{ft} = \beta_0 + \sum_{s \neq 0} \beta_s \cdot Exposure_f \cdot \mathbb{1}\{Year_t = s\} + x'_{ft} \gamma + \delta_t + \varepsilon_{ft} \quad (3)$$

where each observation is a firm i in year t . The dependent variable is an indicator that signals whether the firm continues to do business with an affected clients or starts a new link in year t . The vector x_{it} includes a full set of fixed effects. To measure the adjustment to the types of clients, I use the full sample of firms.

6.2 Extensive Margin Effects on Business Links

In this section, I document how the reform to VAT collection affected firm-to-firm links on the extensive margin. Firm-to-firm links that are subject to the new rules are 2.5 percentage point more likely to become inactive. Moreover, firms more exposed to the reform are 10 percentage point less likely to continue trading with affected clients, relative to the counterfactual.

Table 3 shows that firm-to-firm links subject to the new rules on VAT statutory incidence were more likely to become inactive after the reform, relative to links non-affected by the reform. I estimate the conditional probability of the link remaining active after the reform via OLS. In the specification with a full range of fixed effects, the survival probability of links to affected clients is 2.5 percentage points lower relative to other links (Column (4) in Table 3). This specification includes fixed effects for industry by year, province by year, and firm. Columns (1) to (3) reduce the set of fixed effects included in the specifications. The results are robust and they indicate a lower survival probability for links subject to the new rules on VAT remittance.

To understand which links became inactive, I explore the heterogeneity of the effects of the new rules on VAT statutory incidence next. Table 4 shows that links that make up a larger share of business sales exhibit the largest decline in survival probability, relative to the counterfactual. The table also shows that all links affected by the reform have a lower survival probability. The estimates are all significant at the 1 percent level and are precisely estimated. Reflecting the smaller sample size, standard errors for strategic links are wider. The large and negative effect for links that account for a large share of business sales mechanically implies a significant adjustment on the part of firms.

The magnitude of the effect is more uniform across the size distribution, with small and large links roughly equally likely to remain active after the reform. The relationship between size of the link and the effect of the reform is non-monotonic with links in the mid-range of the distribution having the highest survival probability. That said, the coefficient of each quantile are not statistically different from each other.

I then check whether the negative effect on the survival of firm-to-firm links altered the composition of buyers of firms. Figure 6 shows that firms more exposed to the reform are 4 percentage points less likely to trade with affected clients. This effect manifest the year following the reform and it remains stable afterwards. Table 7 provides the coefficient estimates.

6.3 Intensive Margin Response on Existing Links

In this section, I compute the effect of the reform to VAT collection on the value of links that remain active throughout the sample. The results on the intensive margin detects any change in reporting behavior of sellers. Brockmeyer and Hernandez (2019) finds that a reform to tax collection of sales taxes increased the reported value of transactions. Moreover, the value of links reported by firms is one of the determinant of the amount of taxes effectively collected by the tax authority.

Table 6 shows that the value of firm-to-firm links declined by 3.4 percent, on average. While the point estimate is rather stable across specifications, the effect is not statistically different from zero at common levels of significance. Yet, the 95 percent confidence interval can exclude any increase in the value of firm-to-firm transactions larger than 0.3 percent. This result can therefore exclude any economically significant increase in the reported value of existing links.

7 Effects of VAT Collection on Firm Outcomes

In this section, I present the results on the effects of the reform to VAT collection on the operations of sellers. I showed earlier that the reform altered relationships that were in place between sellers and affected clients and shifted the composition of buyers away from affected clients. This response is consistent with higher costs of trading between sellers and affected clients. Yet, firms have ample margins to adapt. For example, they can substitute government clients with non-affected ones. Or they expand already existing relationships with non-affected clients. Therefore, it is worth investigating what happens to the outcomes of firms more exposed to the reform. I first look at the effects on sales and purchases and then to the exit rates of firms.

7.1 Sales and Purchases

The first set of firm-level outcomes I examine are sales and purchases. These variables provide a good measure of how the reform affected businesses.

Table 8 shows that firms more exposed to the reform reduced their reported business sales. Column (4) shows firms with 10 percentage point more pre-reform sales to affected clients experienced a decline in reported business sales by 2.2 percentage points. The effect is precisely estimated as the 95 percent confidence interval ranges from -1.1 to -3.3. While point estimates are noisy across specifications, the models with the most comprehensive sets of fixed effects point to a decline in business sales.

7.2 Firm Exit

The adjustment of business operations to the new rules on VAT collection is costly for the average firm. The results in the previous section show that firms did not manage to offset affected links that became inactive with new clients in the aftermath of the reform. As a result, some firms might hit the participation constraint and shut down.

In this section, I compare exit rates for more and less exposed firms.

Table 9 shows that firms with pre-reform relationships with affected clients were more likely to become inactive after the reform to VAT collection. An active firm is defined as a business with at least €50,000 in annual revenues. While this definition would mis-classify some active businesses as inactive, it has the advantage of avoiding any interaction with the exemption thresholds for VAT registration. The qualitative results are robust to the choice of the threshold and I will report heterogeneity results for different firm sizes.

In the OLS specifications, the inversion of statutory incidence for VAT has a negative effect on firm survival. Column 1 reports the results from the specification without fixed effects or controls, while Column 4 includes industry and province fixed effects. The coefficients are precisely estimated and very similar across specifications. Column 4 shows that firms that were more exposed to affected clients before the reform were more likely to become inactive. More specifically, a 10 percentage point increase in the proportion of sales to affected clients lead to a decline in the probability of remaining active by 1.1 percentage points. The implied magnitude is not trivial. As reference, the unadjusted exit rates for firms without pre-reform relationships with affected clients is 14pp two years after the base year. This implies that the reform caused a 7.8 percent increase in the exit rate of firms exposed to affected clients.

Table 10 presents the results of the heterogeneity analysis. I divide firms into quintiles based on their pre-reform total sales. The Table shows that firms in the smallest quintile of the size distribution exhibited the largest increase in exit rates, with the effect decreasing in absolute terms as the size of the firm increases. That said, the effect of the reform on exit rates is negative for all quintiles of the size distribution. This pattern of heterogeneity in the treatment effect is not surprising. Previous literature has shown that smaller firms are more likely to evade and are also more likely to hold little cash at their disposal.

8 Heterogeneous Response

In this section, I explore the heterogeneity in the response of firms. I do so by estimating the impact of VAT collection for different groups of firms based on pre-reform characteristics. I measure any treatment heterogeneity along two dimensions: firm size and geographical location. I run the following specification on firms in the analysis sample:

$$y_{ft} = \beta_0 + \sum_{g \in G} \beta_{1,g} Exposure_f \cdot After_t \cdot \mathbb{1}\{Group = g\} + \beta_2 Exposure_f \cdot After_t + x'_{ft} \gamma + \delta_t + \eta_g + \varepsilon_{ft} \quad (4)$$

where $Exposure_f$ captures the exposure to the reform defined in Section 4.1, $After_t$ is a dummy equal to 1 for years 2015 and 2016 and $\mathbb{1}\{Group = g\}$ is a dummy that uniquely assigns each firm to a group based on pre-determined size and location. Specifications include fixed effects for firms, industry by year, and province by year. Standard errors are clustered at the firm level.

Heterogeneity by Size. First, I find that there is significant treatment heterogeneity by firm size. I divide firms into four groups by pre-reform annual sales¹⁴. Figure 7 shows that smaller firms exhibit higher exit rates, compared to larger firms. For the group containing the smallest firms, the point estimate is negative and statistically different from zero. Interestingly, the point estimate turns positive for the two largest groups of firms by size. This suggests that the burden of the reform to VAT collection is unevenly distributed across the size distribution. In addition the higher exit rates for small firms and lower exit rates for large firms point to a reallocation of economic activity among firms exposed to the reform towards larger firms. This has implications for the structure of markets that I explore in the next Section.

The pattern of heterogeneity of the effect is not as clear when examining the effect of the reform on business sales in a balanced panel of firms (Table 11). Point estimates are lowest for larger firms, but they are not statistically different across size groups.

9 Effects of VAT Collection on Markets

Does VAT collection influence supply chains and markets at the aggregate level? Did overall tax collection improve in markets more exposed to the reform? As the reform to VAT collection altered interactions between firms, it is worth investigating whether the reform altered the structure of markets.

In this section, I show that VAT collection and the ensuing adjustment at the firm level affected market structure and increased the overall tax base. In particular, I find that concentration in industries more exposed to the reform increased. As smaller firms exhibit a lower survival probability and lower sales, larger firms stood to gain and accounted for a larger share of market sales after the reform. To quantify these aggregate

¹⁴The four groups are determined by the intervals €50,000-250,000; €250,000-500,000; €500,000-1,000,000; and firms above €1,000,000 in annual sales.

effects, I use the full sample of firms and I define markets by 2-digit industries and provinces. Mirroring the empirical strategy adopted at the firm level in the previous sections, I distinguish markets more exposed to VAT collection based on the share of pre-reform market sales that would be subject to the new reform.

9.1 Market level Specifications

To estimate the effects of VAT collection on markets, I distinguish markets more exposed to the reform from markets less exposed. This strategy mirrors the empirical strategy adopted in previous sections to measure the effects of the reform at the firm level. The measure of exposure is the share of pre-reform sales that would be subject to the reform to VAT collection. In symbols:

$$\text{Market exposure} = \frac{\sum_f s_f}{\text{sales}}$$

Markets are defined as pairs of industry and location. In the full dataset, there are 80 two-digit industries and 21 industries which create 1,463 markets. Table 12 provides summary statistics for markets. They are heterogeneous in terms of size, number of active firms, and concentration. As this dataset comes from information reported on VAT returns, they exclude sales that are exempted from VAT and they do not include firms below the VAT registration threshold. More fundamentally, one might be concerned that the sampling process might lead to bias in the analysis at the market level¹⁵. To assuage this concerns, I cross-check the distribution of sales by industry in the full sample and in an external dataset. In particular, I use mandatory financial accounts provided that all limited liability entities must file with the company registry. Figure 9 shows that the proportion of sales by industry is relatively similar across datasets, supporting the use of the full sample for the market level analysis.

The empirical specification at the aggregate level is equivalent to the firm level specification

$$y_{mt} = \beta_0 + \beta_1 \text{Market Exposure}_m \times \text{After}_t + \varepsilon_{mt} \quad (5)$$

where *MarketExposure* is defined above.

At the aggregate level, I will focus on two main sets of variables. First, I will examine how VAT collection has changed the interactions between firms. Given that taxable

¹⁵The full sample is random sample of buyers of affected and non-affected clients, stratified by geography.

events are transactions between firms along the supply chain, the administration and collection of VAT could alter the concentration of firms in a market and the types of transactions occurring between them. Second, I will focus on the effect of the reform on the VAT tax base. As the stated goal of the reform is to improve tax compliance, it is natural to examine whether the reform managed to increase the reported tax base.

9.2 Market Concentration

In Section 7.2, I showed that smaller firms exhibit the highest increase in exit rates. This highlights the uneven distribution of the burden of the reform of VAT collection. In this section, I show that this pattern translate into a higher concentration of markets. Therefore, larger firms stood to gain from the exit of smaller firms and they increased their market power. I use two measures of market concentration that are standard in the literature: the Herfindahl-Hirschman (HH) Index and the share of sales made by the top 3 firms in each market. The correlation between these two measures is high and results are qualitatively similar across the two measures.

Table 13 shows that the HH Index increased by 444 points in markets more exposed to the reform. The coefficient is stable to the inclusion of finer sets of fixed effects. Column (1) reports the results for the specification without fixed effect, Column (2) includes fixed effects for industry, Column (3) has fixed effects for provinces, while Column (4) includes fixed effects at the level of individual markets. As a robustness check, I measure the importance of large firms in each market by computing the share of sales made by the top 1, 2, or 3 firms in each markets over time. Table 14 suggests that VAT collection increased the share of sales by the largest firms in each market.

These results suggest that the adjustment that occurred among firms altered the structure of markets. Indeed, with larger appearing to benefit from the reform and smaller firms being the ones hardest hit, economic activity shifted towards larger firms. It is ex-ante unclear whether this effect improves overall welfare or not. On the one hand, market concentration is usually associated with higher market prices. In the current context, higher prices for government procurement would have a negative effect on welfare. On the other hand, if firms that exit the market are firms that are evading or, even more extreme, if they were operating because they were evading, then the reform successfully increased the cost of operations of those businesses. In order to assess the overall benefits, I turn to the effect of the reform on the tax base.

9.3 Aggregate Tax Variables

To measure whether the reform achieved the stated goal of reducing tax evasion, I examine the effect on the aggregate tax base in markets more exposed to the reform. [Keen and Slemrod \(2017\)](#) propose to use the responsiveness of tax revenues collected as a sufficient statistics to evaluate interventions in tax administration. Tax revenues are proportional to the tax base, in this section I measure the change in the aggregate tax base in markets that are more exposed to the reform.

Table 15 shows that the aggregate tax went up in markets more exposed to the reform. The effect is positive in all specifications, yet it is imprecisely estimated and not statistically different from zero. Therefore, the overall effort of the reform to increase compliance remains limited, yet the costs to individual firms are substantial.

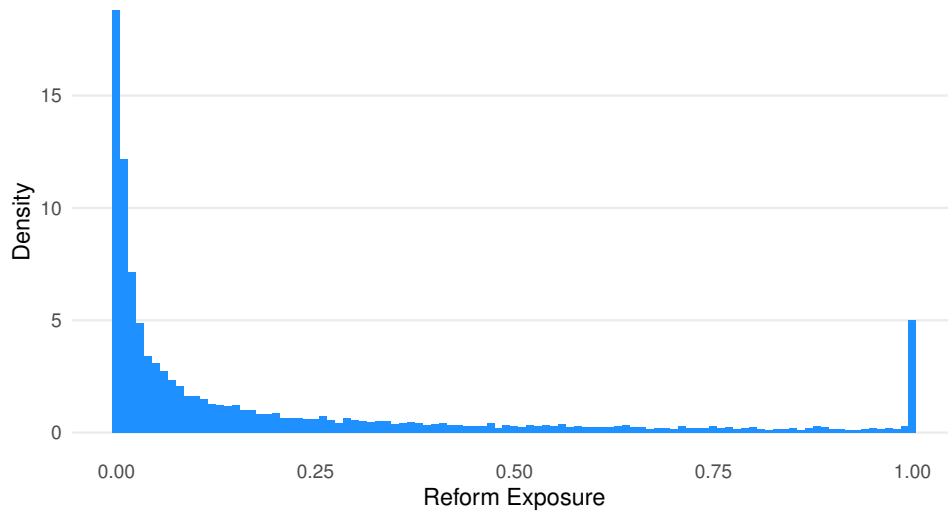
10 Conclusions

This paper shows that assigning the responsibility to remit taxes to the buyer or the seller has economic consequences. It affects the operations of businesses and the structure of markets. Combining a new administrative data set on firm-to-firm links and a quasi-experimental research design, I find that links subject to the new rules are more likely to become inactive. Moreover, firms exposed to the reform exhibit lower sales and a higher exit probability. I document that the burden of the reform is not evenly distributed across firms. Smaller firms were hardest hit, while larger firms did not appear to be negatively affected. Finally, I find that this heterogeneity translates into an effect at the aggregate level. I show that markets more exposed to the reform became more concentrated, suggesting a reallocation of economic activity from small to large firms.

While the reform achieved its stated goal of increasing tax collection, it imposed costs on firms. Interventions in tax administration and collection have generally proven to be successful in raising revenues. Yet, the costs of these measures are usually poorly studied or ignored by policy makers. Several interventions in tax administration might alter the cash-flows to each side of the market. It is crucial that policy makers have in mind the distribution of the burden of the reform.

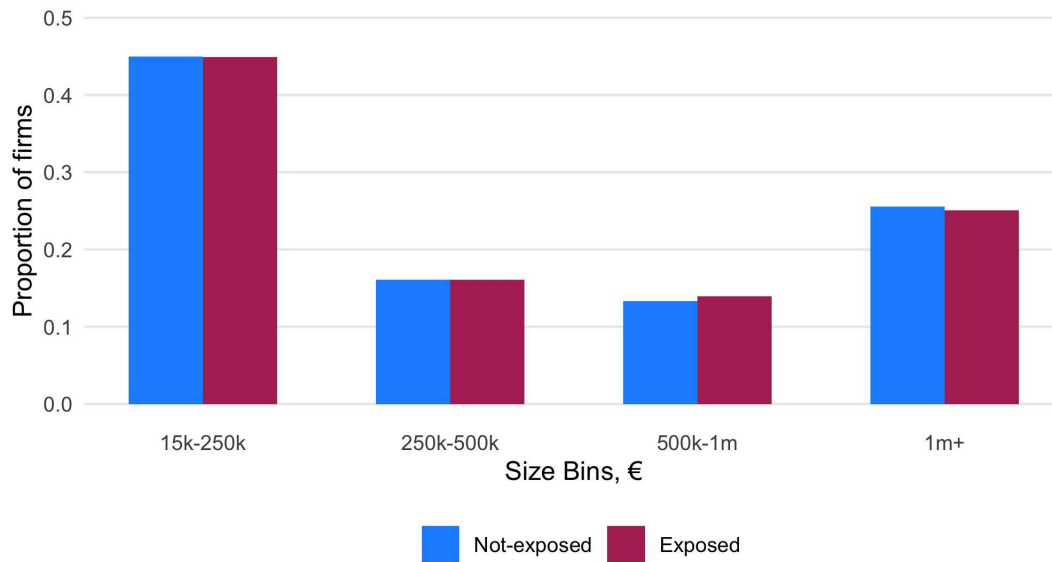
Figures

Figure 1: Treatment Exposure



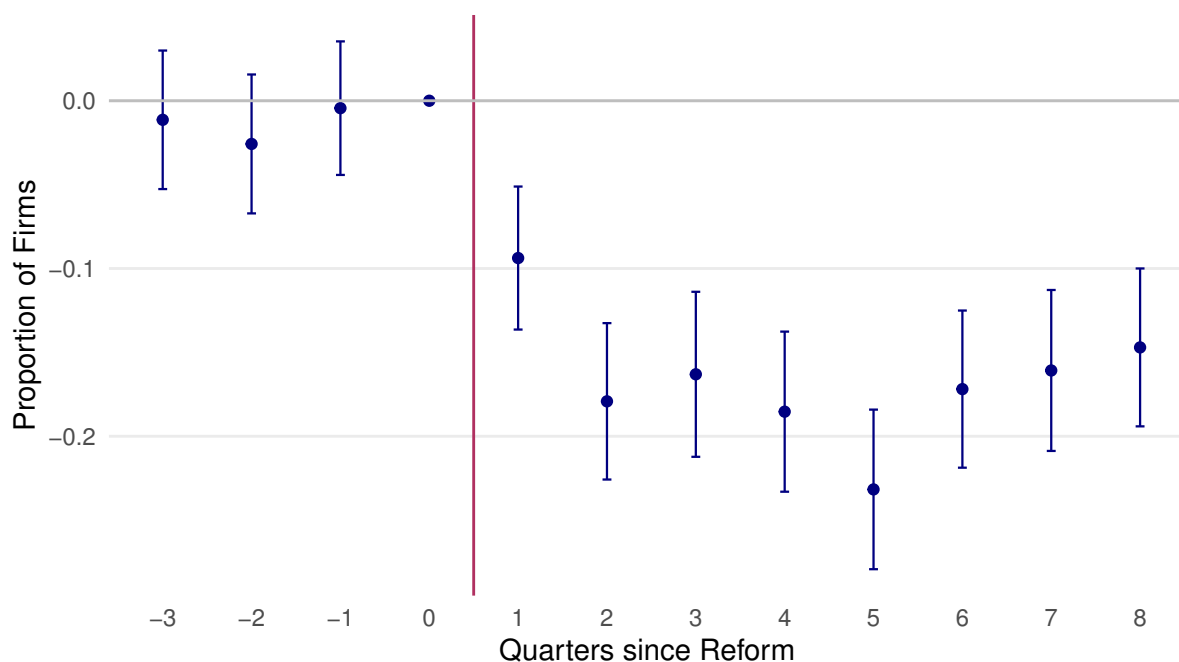
Notes: The figure represents the univariate distribution of the variable *Exposure* calculated for sellers with at least one link with an affected buyer. The variable *Exposure* represents the intensity of the treatment effect for each and it is defined as the share of pre-reform sales going to affected clients (Section 4.1) This variables is defined using data from 2014, before the introduction of the reform.

Figure 2: Size Distribution of Firms in the Analysis Sample



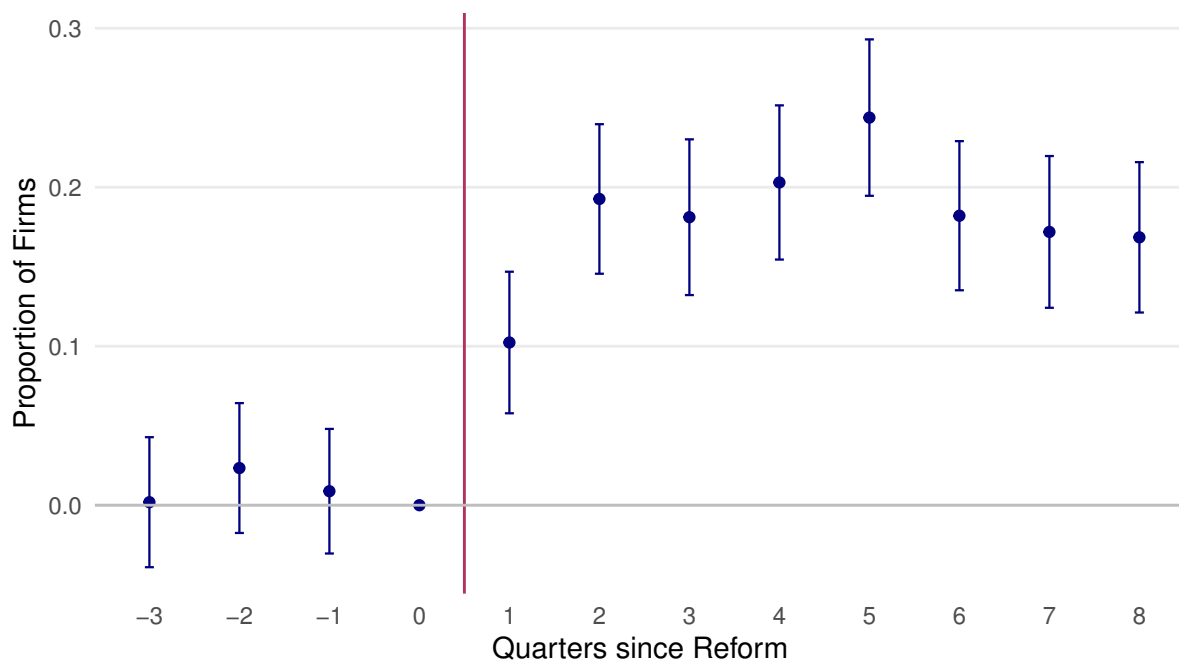
Notes: The figure shows the distribution of firms according to total annual sales, separately for firms with pre-reform relations with affected clients (“Exposed”) and firms with pre-reform relations with non-affected clients (“Not-exposed”). The proportion of firms in each size group is calculated with respect to the total count of firms in each group. Total sales is defined as the sum of business sales and sales to final consumers. This graph pertains to the distribution of firms in 2014, before the introduction of the reform.

Figure 3: Firms Reduce their VAT Payments on the Extensive Margin



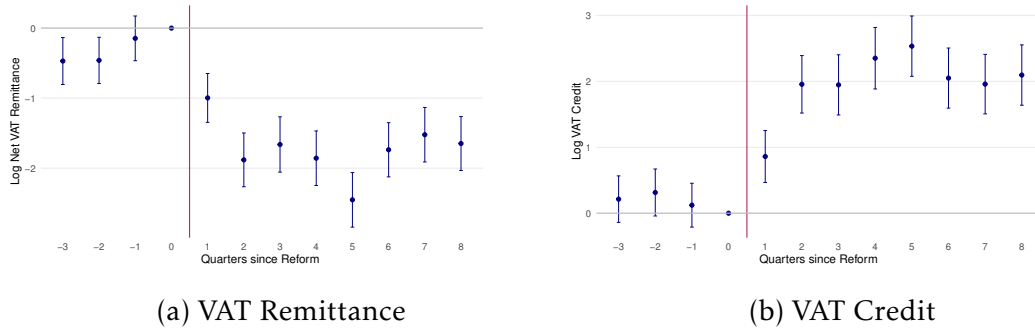
Notes: This figure plots the estimated β_t of event-study coefficients from a regression specification given in equation 1. The dependent variable is an indicator variable equal to 1 if the firm remits payments of VAT in a filing period. The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. The specification includes fixed effects for industry by filing-period, province by filing-period, and firm. The vertical blue lines represents 95 percent confidence interval. The red vertical lines highlights the timing of the introduction of the reform. The coefficient plotted correspond to Column (1) in Table D.3. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Figure 4: Firms Increase their VAT Credit on the Extensive Margin



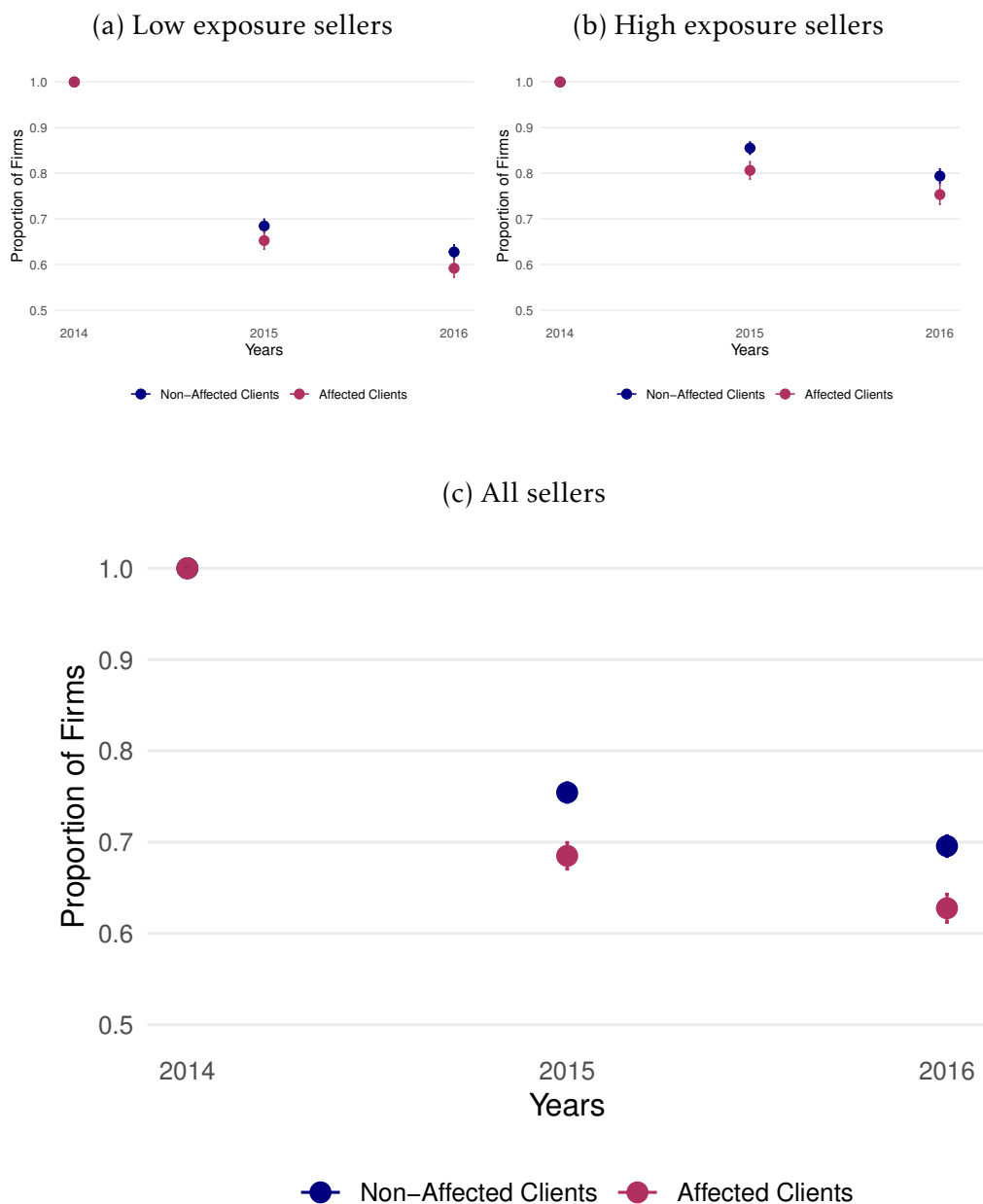
Notes: This figure plots the estimated β_t of event-study coefficients from a regression specification given in equation 1. The dependent variable is an indicator variable equal to 1 if the firm reports a VAT credit in a filing period. The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. The specification includes fixed effects for industry by filing-period, province by filing-period, and firm. The vertical blue lines represents 95 percent confidence interval. The red vertical lines highlights the timing of the introduction of the reform. The coefficient plotted correspond to Column (4) in Table D.3. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Figure 5: Firm Reduce VAT Payments and Increase VAT Credits



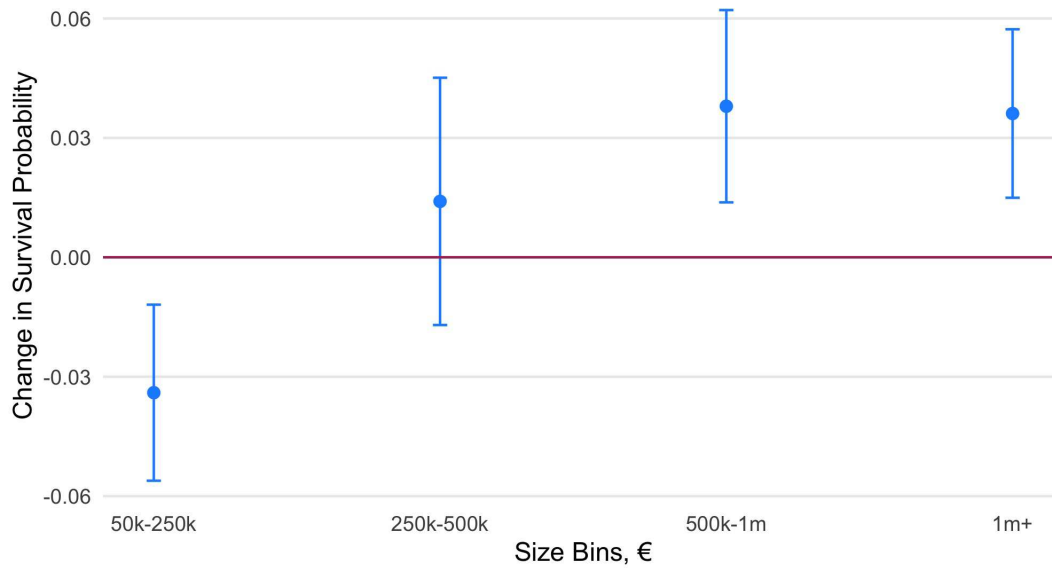
Notes: This figure plots the estimated β_t of event-study coefficients from a regression specification given in equation 1. The dependent variable is the value of periodic VAT remittances in panel (a) and the value of periodic VAT credit in panel (b). The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. The specification includes fixed effects for industry by filing-period, province by filing-period, and firm. The vertical blue lines represents 95 percent confidence interval. The red vertical lines highlights the timing of the introduction of the reform. The coefficients plotted in panel (a) correspond to Column (4) in Table D.4, while the coefficients plotted in panel (b) correspond to Column (4) in Table D.5. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Figure 6: The Composition of Buyers Changed for Firms More Exposed to the Reform



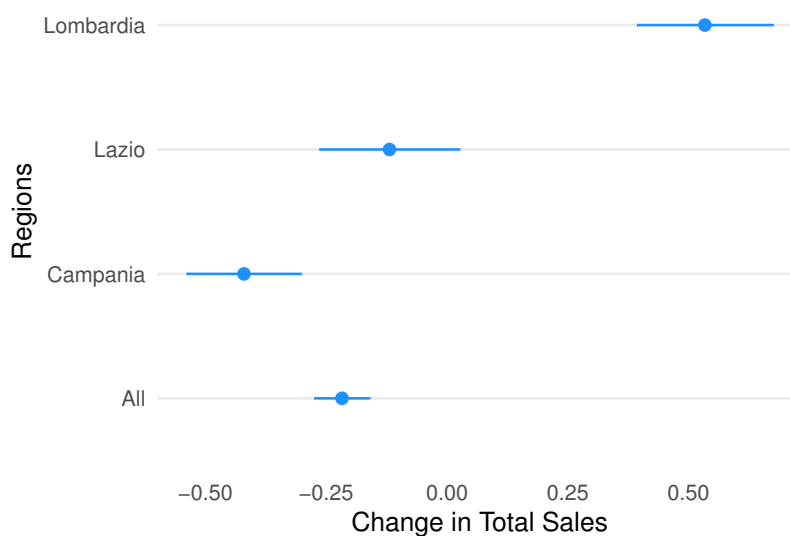
Notes: This figures represent the conditional probability of selling to the same type of client for firms with pre-reform relationships with affected clients and for firm with pre-reform relationship with non-affected clients. The unit of observation is a seller. Panel (a) includes firms with a pre-reform share of sales to affected or non-affected clients below 0.2, panel (b) includes firms with a pre-reform share of sales to affected clients above 0.2, panel (c) includes all firms. Standard errors are clustered at the seller level.

Figure 7: Size Heterogeneity in Exit Rates



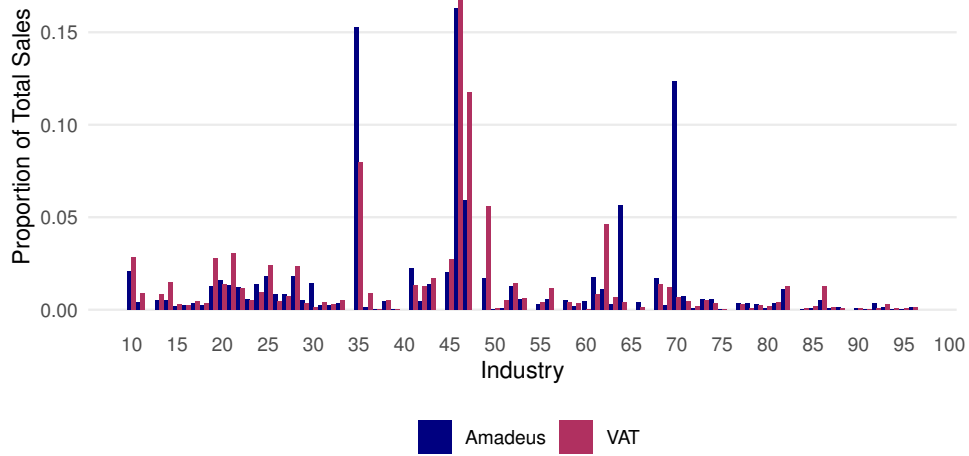
Notes: This figure plots the coefficients from a regression where the dependent variable is an indicator variable equal to 1 when the firm is active. An active firm is defined as a firm reporting at least €50,000 in annual sales. The unit of observation is a seller. The estimation is performed on the sample of firms that are active in 2014. The regression specification includes the following explanatory variables: a dummy variable *Post* equal to 1 for years 2015 and 2016, a categorical variable that assigns each firm to one of the four size bins based on pre-reform annual sales, and interaction terms. The four size groups correspond to the following intervals: €50,000-250,000; €250,000-500,000; €500,000-1,000,000; and above €1,000,000. The number of firms in each bin is 6,853; 2,731; 2,305; 4,153, respectively. Each specification includes fixed effects for industry by year and province by year. Standard errors are clustered at the seller level.

Figure 8: Geographic Heterogeneity



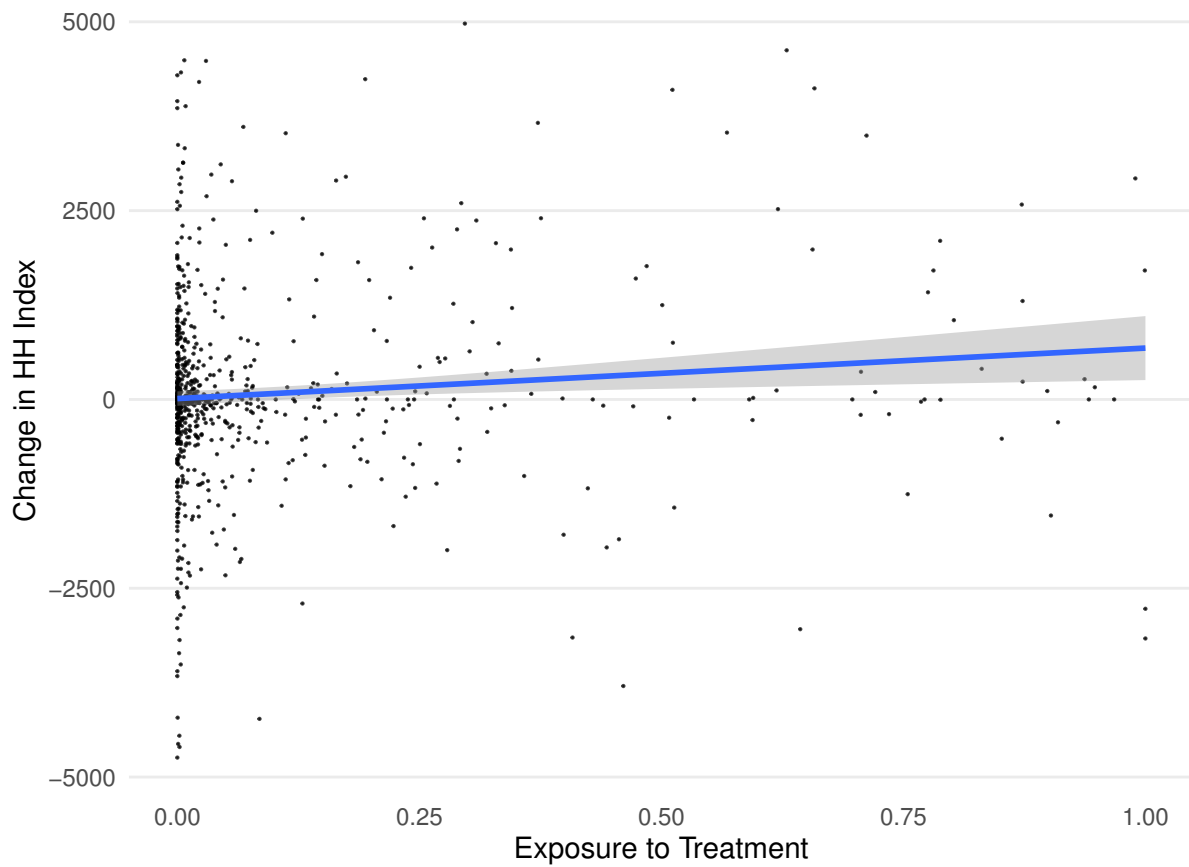
Notes: This figure plots the coefficients from a regression where the dependent variable is *Business sales*. The unit of observation is a firm. The specification includes the interaction between the variable $Exposure_f$ which represents the share of pre-reform business sales subject to the reform, the dummy $After_t$ equal to 1 for years 2015 and 2016, and a categorical variable for the location of the firm. Lombardia, Lazio, and Campania are the three Italian regions in the sample. Each specification includes fixed effects for industry by year and province by year. Standard errors are clustered at the seller level.

Figure 9: Distribution of Sales By Industry



Notes: The figure plots the distribution of sales across 2-digit industries. Each bar represents one industry. The height is each industry's share of total sales. The blue series is obtained using data from financial accounts (Amadeus dataset), while the red series comes from the VAT dataset.

Figure 10: Markets Became More Concentrated



Notes: This figure plots the change in the HH index from 2014 to 2016 in each market against the exposure to the reform, defined in Section (9.1). The unit of analysis is a market, defined as an industry-province pair. The fitted is obtained via OLS and the shaded area represents the 95-percent confidence interval. Standard errors are clustered at the market level.

Tables

Table 1: Summary Statistics for Firms in the Analysis Sample

	Mean	P10	Median	P90
Total Sales (000s)	1,501.74	56.66	306.59	3,468.80
Business Sales (000s)	789.21	14.19	111.11	1,675.42
Total Purchases (000s)	1,082.24	12.83	166.35	2,430.25
Total Purchases / Sales	0.55	0.15	0.57	0.92
Taxable Sales (000s)	1,167.65	45.48	272.41	2,852.50
Taxable Sales / Sales	0.89	0.59	1.00	1.00
Taxable Purchases (000s)	16.54	0.00	0.00	1.98
Taxable Purchases / Sales	0.01	0.00	0.00	0.00
Value Added (000s)	406.37	17.63	112.57	966.63
Value Added / Sales	0.45	0.08	0.43	0.85
Tax Base / Sales	0.88	0.58	1.00	1.00
Tax Base (000s)	1,127.04	44.30	267.54	2,836.94
Firms	14,987			

Notes: This table reports summary statistics for firms in the analysis sample. Except for ratios, variables are expressed in Euros. Variables are defined in Appendix A. The columns report the mean (labeled “Mean”), the 10th percentile (“P10”), the median (“Median”) and the 90th percentile (“P90”). Summary statistics are reported at baseline, in 2014. Continuous variables are winsorized at the 99th percentile. Variables that represent ratios are scaled by the value of total sales in 2014 and then censored at 0 and 1.

Table 2: Summary Statistics by Type of Pre-reform Relationship

Variables	Mean		T-stat
	No Relation	With Relation	
Total Sales (000s)	1,528	1,482	-0.82
Total Purchases (000s)	1,099	1,070	-0.64
Business Sales (000s)	822	765	-1.64
Taxable Sales (000s)	1,121	1,164	1.08
Taxable Purchases (000s)	17	16	-0.35
Value Added (000s)	418	397	-1.53
Tax Base (000s)	1,102	1,146	1.12
Value Added Scaled	0.39	0.41	1.55
Tax Base Scaled	0.88	0.90	2.63
Firms	6,458	8,529	

Notes: This table reports the mean of variables for firms in the analysis sample. The column labeled “*No Relationship*” refers to firms with no pre-reform relationship with affected clients, while the column “*With Relationship*” refers to firms with pre-reform relationship with affected clients. The column “*T-stat*” reports the t-statistics for a two-sided t test. Variables are defined in Appendix A. Summary statistics are reported at baseline, in 2014. Continuous variables are winsorized at the 99th percentile. Variables that represent ratios are scaled by the value of total sales in 2014 and then censored at 0 and 1. Except for ratios, variables are expressed in Euros.

Table 3: Firm-to-Firm Links with Affected Clients Are More Likely to Become Inactive

	Survival Rate of Firm-to-Firm Links				
	(1)	(2)	(3)	(4)	(5)
Affected Client	-0.042** (0.013)	-0.035** (0.012)	-0.037** (0.011)	-0.036*** (0.011)	-0.025*** (0.005)
Observations	577084	577084	577084	577084	577084
Clusters (Seller)	13,590	13,590	13,590	13,590	13,590
Seller Controls		Y	Y	Y	Y
Industry \times Year FE			Y	Y	Y
Province \times Year FE				Y	Y
Seller FE					Y

Notes: This table reports the survival probability that a link between a seller and an affected client remains active after the reform relative to links between a seller and a non-affected client. The unit of observation is a firm-to-firm link. Affected links are firm-to-firm links between a seller and a client subject to the new rule on VAT collection, while unaffected links are the other firm-to-firm links. Control variables are the log of pre-reform business sales for each seller and the log of the pre-reform number of links for each seller. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firm-to-firm links that account for at least 0.1% of each seller's business sales in 2014. The sample includes all sellers in the analysis sample. An active link is defined as a link with an annual value of at least €1,000. Standard errors are clustered at the seller level.

Table 4: Survival Rates Links, by Importance of the Link

	Survival Probability
Group 1	-0.022*** (0.006)
Group 2	-0.029* (0.015)
Group 3	-0.040 (0.023)
Group 4	-0.086** (0.031)
Group 5	-0.115** (0.040)
Observations	577,084
Clusters (Sellers)	13,590
Seller Controls	Y
Industry \times Year FE	Y
Province \times Year FE	Y
Seller FE	Y

Notes: This table reports the survival probability that a link between a seller and an affected client remains active after the reform relative to links between a seller and a non-affected client. Every link is assigned to one of 5 groups based on its share of the seller's total business sales before the reform. Group 1 corresponds to values in the interval 0-0.2; Group 2 to 0.2-0.4; Group 3 to 0.4-0.6; Group 4 to 0.6-0.8; Group 5 to 0.8-1. The unit of observation is a firm-to-firm link. Affected links are firm-to-firm links between a seller and a client subject to the new rule on VAT collection, while unaffected links are the other firm-to-firm links. Control variables are the log of pre-reform business sales for each seller and the log of the pre-reform number of links for each seller. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firm-to-firm links that account for at least 0.1% of each seller's business sales in 2014. The sample includes all sellers in the analysis sample. An active link is defined as a link with an annual value of at least €1,000. Standard errors are clustered at the seller level.

Table 5: Survival Rates Links, by Size of the Link

	Survival Probability
Group 1	-0.008 (0.009)
Group 2	-0.054*** (0.011)
Group 3	-0.050*** (0.012)
Group 4	-0.037** (0.012)
Group 5	-0.022* (0.011)
Observations	577,084
Clusters (Sellers)	13,590
Seller Controls	Y
Industry \times Year FE	Y
Province \times Year FE	Y
Seller FE	Y

Notes: This table reports the survival probability that a link between a seller and an affected client remains active after the reform relative to links between a seller and a non-affected client. Every link is assigned to a quintile of the size distribution of the value of firm-to-firm links before the reform. Group 1 corresponds to values in the interval €1,000-2,035; Group 2 to €2,036-4,004; Group 3 to €4,005-8,756; Group 4 to 8,756-26,856; Group 5 above €26,856 . The unit of observation is a firm-to-firm link. Affected links are firm-to-firm links between a seller and a client subject to the new rule on VAT collection, while unaffected links are the other firm-to-firm links. Control variables are the log of pre-reform business sales for each seller and the log of the pre-reform number of links for each seller. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firm-to-firm links that account for at least 0.1% of each seller's business sales in 2014. The sample includes all sellers in the analysis sample. An active link is defined as a link with an annual value of at least €1,000. Standard errors are clustered at the seller level.

Table 6: Effects on Value of Firm-to-firm Links

	LHS: Log Firm-to-Firm Sales				
	(1)	(2)	(3)	(4)	(5)
Affected Client	0.604*** (0.147)	0.153*** (0.044)	0.153*** (0.043)	0.153*** (0.041)	0.060* (0.030)
Affected Client \times Post	-0.040 (0.021)	-0.040 (0.021)	-0.031 (0.019)	-0.034 (0.019)	-0.034 (0.019)
Observations	411,675	411,675	411,675	411,675	411,675
Clusters (Seller)	11,775	11,775	11,775	11,775	11,775
Seller Controls		Y	Y	Y	Y
Industry \times Year FE			Y	Y	Y
Province \times Year FE				Y	Y
Seller FE					Y

Notes: This table reports coefficients estimate from equation 2. The dependent variable is the Log value of firm-to-firm links. The coefficient labeled “*Affected Client \times Post*” represents the causal effect of the reform. The unit of analysis is a firm-to-firm link. Control variables are the log of pre-reform business sales for each seller and the log of the pre-reform number of links for each seller. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes active firm-to-firm links that account for at least 0.1% of each seller’s business sales in 2014. The sample includes all sellers in the analysis sample. An active link is defined as a link with an annual value of at least €1,000. Standard errors are clustered at the seller level.

Table 7: Firms Adjust their Customer Base

	Conditional Prob of Selling to	
	Affected Clients	Non-affected Clients
Year 2015	-0.306*** (0.005)	-0.208*** (0.004)
Year 2016	-0.361*** (0.005)	-0.259*** (0.004)
Observations	25587	31218
Clusters (firms)	8,529	10,406

Notes: This table shows the difference in the probability of trading with affected clients or non-affected clients. The probability is estimated separately for firms with pre-reform relationships with affected clients and firms with pre-reform relationships with non-affected clients. The specification includes fixed effects for industry by year, province by year and firm. The sample is made of firms in the analysis sample. Standard errors are clustered at the seller level.

Table 8: Firms Exposed to the Reform Experienced Lower Business Sales

	LHS variable: Business Sales				
	(1)	(2)	(3)	(4)	(5)
Exposure \times Post	0.029 (0.047)	-0.140* (0.057)	-0.015 (0.047)	-0.150** (0.057)	-0.218*** (0.055)
Exposure	-1.178*** (0.073)	-0.856*** (0.080)	-0.940*** (0.074)	-0.669*** (0.080)	
Observations	44137	44134	44137	44134	43998
Clusters (sellers)	14,986	14,985	14,986	14,985	14,849
Industry \times Year FE		Y		Y	Y
Province \times Year FE			Y	Y	Y
Firm FE					Y

Notes: This table reports the coefficients from specification 3. The unit of observation is a seller. The dependent variable is *Business Sales*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firms in the analysis sample. Standard errors are clustered at the seller level.

Table 9: Firms Exposed to the Reform Have Lower Probability of Survival

	LHS variable: Prob(Total Sales > 50000)			
	(1)	(2)	(3)	(4)
Exposure \times Post	-0.158*** (0.010)	-0.133*** (0.012)	-0.141*** (0.011)	-0.114*** (0.012)
Observations	48129	48129	48129	48129
Clusters (Sellers)	16,043	16,043	16,043	16,043
Industry \times Year FE		Y		Y
Province \times Year FE			Y	Y

Notes: This table reports the survival probability that a seller more exposed to the reform remains active after the reform relative to a less exposed seller. The unit of analysis is a seller. The sample is the analysis sample. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. Standard errors are clustered at the seller level.

Table 10: Smaller Firms Are More Likely to Become Inactive

	Survival Probability
Group 1	-.0981 (.0159)
Group 2	-.0396 (.0266)
Group 3	.0509 (.0251)
Group 4	.0632 (.0198)
Observations	48129
Clusters (Sellers)	16043
Industry \times Year	Y
Province \times Year	Y

Notes: This table reports the survival probability that a seller more exposed to the reform remains active after the reform relative to a less exposed seller. Every seller is assigned to one of four groups based on its pre-reform total sales. Group 1 includes sellers with sales in the interval €50,000-250,000; Group 2 to €250,000-500,000; Group 3 to €500,000-1,000,000; Group 4 above €1,000,000. The unit of observation is a firm. The sample is the analysis sample. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. The specification includes fixed effects for industry by year and province by year. Active firms are defined as firms with at least €50,000 in annual sales. Standard errors are clustered at the seller level.

Table 11: Larger Firms Experience a Larger Decline in Business Sales

	Business Sales
Group 1	-.2678 (.0729)
Group 2	-.1224 (.1257)
Group 3	-.6592 (.1032)
Group 4	-.5324 (.0925)
Observations	43998
Clusters (Sellers)	14849
Industry \times Year	Y
Province \times Year	Y
Seller FE	Y

Notes: This table reports the coefficients $\beta_{1,g}$ from specification 4. The dependent variable is “*Business Sales*”. Every seller is assigned to one of four groups based on its pre-reform total sales. Group 1 includes sellers with sales in the interval €50,000-250,000; Group 2 to €250,000-500,000; Group 3 to €500,000-1,000,000; Group 4 above €1,000,000. The unit of observation is a firm. The sample is the analysis sample. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. The specification includes fixed effects for industry by year, province by year, and firm. Standard errors are clustered at the seller level.

Table 12: Summary Statistics for Markets

Variables	Min	P10	Mean	Median	P90	Max
Total Sales (000s)	21	152	20,810	3,495	48,247	1,844,049
Total Purchases (000s)	0	70	14,993	1,980	35,497	1,458,888
Value Added (000s)	-894,989	46	15,953	1,159	18,518	2,435,801
Tax Base (000s)	-28,939	100	62,893	2,627	58,161	7,812,190
HHI	92	1,152	5,460	5,019	10,000	10,000
Affected Sales (%)	0.0	0.0	0.088	0.011	0.26	1.0

Notes: This table reports summary statistics for key variables at the market level. Markets are defined as pairs of industry and provinces. The variables *Total Sales*, *Total Purchases*, *Value Added*, *Tax Base* are created by summing up the corresponding firm level variables. The variable *HHI* represents the Herfindahl-Hirschman Index of total sales for each market. The variable *Affected Sales* represents the share of pre-reform total sales that would be subject to the new rules.

Table 13: Market Concentration Increases After the Reform

	LHS Variable: HH Index			
	(1)	(2)	(3)	(4)
Exposure \times Post	420.4 (124.3)	420.4 (126.0)	420.4 (124.8)	420.4 (126.5)
Observations	2,736	2,736	2,736	2,736
Industry FE		Y		Y
Province FE			Y	Y

Notes: This table reports the coefficient β_1 from specification 5. The unit of analysis is a market, defined as a pair of industry and province. There are 1,129 markets. The variable *Exposure* is equal to the share of pre-reform business sales of each market that would be subject to the new rules on VAT collection. The variable *Post* is equal to 1 for years 2015 and 2016. The dependent variable is the Herfindahl-Hirschman (HH) Index defined in Section 9.2. Standard errors are clustered at the market level.

Table 14: Market Concentration Increases After the Reform

	LHS Variable: Market Share Top 3 Firms			
	(1)	(2)	(3)	(4)
Exposure \times Post	0.056 (0.014)	0.056 (0.014)	0.056 (0.014)	0.056 (0.014)
Observations	2,736	2,736	2,736	2,736
Industry FE		Y		Y
Province FE			Y	Y

Notes: This table reports the coefficient β_1 from specification 5. The unit of analysis is a market, defined as a pair of industry and province. There are 1,129 markets. The variable *Exposure* is equal to the share of pre-reform business sales of each market that would be subject to the new rules on VAT collection. The variable *Post* is equal to 1 for years 2015 and 2016. The dependent variable is the share of market sales made by the 3 largest firms. Standard errors are clustered at the market level.

Table 15: Tax Base Increased in Markets More Exposed to the Reform

	LHS Variable: Tax Base Scaled			
	(1)	(2)	(3)	(4)
Exposure \times Post	0.03 (0.033)	0.03 (0.034)	0.03 (0.033)	0.03 (0.034)
Observations	2,736	2,736	2,736	2,736
Industry FE		Y		Y
Province FE			Y	Y

Notes: This table reports the coefficient β_1 from specification 5. The unit of analysis is a market, defined as a pair of industry and province. There are 1,129 markets. The variable *Exposure* is equal to the share of pre-reform business sales of each market that would be subject to the new rules on VAT collection. The variable *Post* is equal to 1 for years 2015 and 2016. The dependent variable is the ratio of the tax base to pre-reform sales. Standard errors are clustered at the market level.

References

- Auerbach, Alan J.** 2002. "Taxation and Corporate Financial Policy." In *Handbook of Public Economics*. Vol. 3, 1251–1292. Elsevier Masson SAS.
- Barrot, Jean-Noël, and Julien Sauvagnat.** 2016. "Input Specificity and the Propagation of Idiosyncratic Shocks in Production Networks." *The Quarterly Journal of Economics*, 131(3): 1543–1592.
- Barrot, Jean-Noël, and Ramana Nanda.** 2020. "The Employment Effects of Faster Payment: Evidence from the Federal Quickpay Reform." *The Journal of Finance*, 75(6): 3139–3173.
- Benmelech, Efraim, Nittai Bergman, and Amit Seru.** 2011. "Financing Labor." National Bureau of Economic Research Working Paper 17144.
- Bernard, Andrew, Emmanuel Dhyne, Glenn Magerman, Kalina Manova, and Andreas Moxnes.** 2019. "The Origins of Firm Heterogeneity: A Production Network Approach." National Bureau of Economic Research Working Paper 25441.
- Brockmeyer, Anne, and Marco Hernandez.** 2019. "Taxation, Information and Withholding: Evidence from Costa Rica." <https://www.annebrockmeyer.com>. Accessed on 2020-11-03.
- Carfora, Alfonso, Massimiliano Marigliani, Stefano Pisani, and Andrea Spingola.** 2017. "Gli effetti dello split payment sulla compliance IVA." Agenzia dell'Entrate Argomenti di Discussione N. 01/2017.
- Carrillo, Paul, Dina Pomeranz, and Monica Singhal.** 2017. "Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement." *American Economic Journal: Applied Economics*, 9(2): 144–164.
- Chen, Zhao, Xian Jiang, Zhikuo Liu, Juan Carlos Suárez Serrato, and Daniel Xu.** 2019. "Tax Policy and Lumpy Investment Behavior: Evidence from China's VAT Reform." National Bureau of Economic Research Working Paper 26336.
- Chetty, Raj, Adam Looney, and Kory Kroft.** 2009. "Salience and Taxation: Theory and Evidence." *American Economic Review*, 99(4): 1145–1177.
- Chetty, R., and E. Saez.** 2005. "Dividend Taxes and Corporate Behavior: Evidence from the 2003 Dividend Tax Cut." *The Quarterly Journal of Economics*, 120(3): 791–833.

- Chodorow-Reich, Gabriel.** 2014. "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis." *The Quarterly Journal of Economics*, 129(1): 1–59.
- Conley, Timothy G, and Francesco Decarolis.** 2016. "Detecting Bidders Groups in Collusive Auctions." *American Economic Journal: Microeconomics*, 8(2): 1–38.
- D’Agosto, Elena, and Alessandro Santoro.** 2019. "The anatomy of VAT efficiency: evidence from Italy 2009-2014." Agenzia delle Entrate Argomenti di Discussione N. 01/2019.
- Demir, Banu, Beata Javorcik, Tomasz K Michalski, and Evren Ors.** 2019. "Financial Constraints and Propagation of Shocks in Production Networks."
- Dhyne, Emmanuel, Ken Kikkawa, and Glenn Magerman.** 2018. "Imperfect Competition on Firm-to-Firm Trade."
- European Commission.** 2020. "Study and Reports on the VAT Gap in the EU-28 Member States: 2020 Final Report." https://ec.europa.eu/taxation_customs/sites/taxation/files/vat-gap-full-report-2020_en.pdf. Accessed on 2020-11-03.
- Fazzari, Steven M., R. Glenn Hubbard, and Bruce C. Petersen.** 1988. "Financing Constraints and Corporate Investment." *Brookings Papers on Economic Activity*, 1988(1): 141–206.
- Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch.** 2018. "Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany." *American Economic Review*, 108(2): 393–418.
- Furusawa, Taiji, Tomohiko Inui, Keiko Ito, and Heiwai Tang.** 2018. "Global Sourcing and Domestic Production Networks." CESifo Working Paper No. 6658.
- Gadenne, Lucie, Tushar K Nandi, and Roland Rathelot.** 2019. "Taxation and Supplier Networks: Evidence from India." Centre for Economic Policy Research Discussion Paper No. 13971 March.
- Gomes, Joao F.** 2001. "Financing Investment." *American Economic Review*, 91(5): 1263–1285.
- Gruber, Jonathan.** 2015. *Public Finance and Public Policy*. New York:Worth Publishers.
- Huneus, Federico.** 2018. "Production network dynamics and the propagation of shocks." <https://www.fedehuneus.com/>. Accessed on 2019-03-21.

- Jones, Damon.** 2012. "Inertia and Overwithholding: Explaining the Prevalence of Income Tax Refunds." *American Economic Journal: Economic Policy*, 4(1): 158–185.
- Kaplan, S. N., and L. Zingales.** 1997. "Do Investment-Cash Flow Sensitivities Provide Useful Measures of Financing Constraints?" *The Quarterly Journal of Economics*, 112(1): 169–215.
- Keen, Michael, and Joel Slemrod.** 2017. "Optimal tax administration." *Journal of Public Economics*, 152: 133–142.
- Kopczuk, Wojciech, Justin Marion, Erich Muehlegger, and Joel Slemrod.** 2016. "Does Tax-Collection Invariance Hold? Evasion and the Pass-Through of State Diesel Taxes." *American Economic Journal: Economic Policy*, 8(2): 251–286.
- Kotlikoff, Laurence J, and Lawrence H Summers.** 1987. "Tax incidence." In *Handbook of Public Economics, Vol. 2.*, ed. Alan J. Auerbach and Martin Feldstein, Chapter 16, 1043–1092. Amsterdam:Elsevier.
- Lamont, Owen.** 1997. "Cash Flow and Investment: Evidence from Internal Capital Markets." *The Journal of Finance*, 52(1): 83–109.
- McMillan, J., and C. Woodruff.** 1999. "Interfirm Relationships and Informal Credit in Vietnam." *The Quarterly Journal of Economics*, 114(4): 1285–1320.
- Meyer, John R, and Edwin Kuh.** 1957. *The Investment Decision: An Empirical Study.* Cambridge, MA:Harvard University Press.
- Ministero dell’Economia e delle Finanze.** 2019. "Relazione Sull’Economia Non Osservata E Sull’Evasione Fiscale E Contributiva." https://www.mef.gov.it/ministero/commissioni/rel_ev/index.html. Accessed on 2020-08-03.
- Naritomi, Joana.** 2019. "Consumers as Tax Auditors." *American Economic Review*, 109(9): 3031–3072.
- OECD.** 2019a. *Government at a Glance.* Paris:OECD Publishing. Online version. <https://doi.org/10.1787/8ccf5c38-en>.
- OECD.** 2019b. *Revenue Statistics 2019.* Paris:OECD Publishing. Online version.
- Pomeranz, Dina.** 2015. "No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax." *American Economic Review*, 105(8): 2539–2569.

- Rauh, Joshua.** 2006. "Investment and Financing Constraints: Evidence from the Funding of Corporate Pension Plans." *The Journal of Finance*, 61(1): 33–71.
- Saez, E., M. Matsaganis, and P. Tsakloglou.** 2012. "Earnings Determination and Taxes: Evidence From a Cohort-Based Payroll Tax Reform in Greece." *The Quarterly Journal of Economics*, 127(1): 493–533.
- Slemrod, Joel.** 2008. "Does It Matter Who Writes the Check to the Government? The Economics of Tax Remittance." *National Tax Journal*, 61(2): 251–275.
- Strebulaev, Ilya A., and Toni M. Whited.** 2011. "Dynamic Models and Structural Estimation in Corporate Finance." *Foundations and Trends® in Finance*, 6(1-2): 1–163.
- Suárez Serrato, Juan Carlos, and Owen Zidar.** 2016. "Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms." *American Economic Review*, 106(9): 2582–2624.
- Tintelnot, Felix, Ayumu Ken Kikkawa, Magne Mogstad, and Emmanuel Dhyne.** 2018. "Trade and Domestic Production Networks." National Bureau of Economic Research Working Paper 25120.
- Tørsløv, Thomas, Ludvig Wier, and Gabriel Zucman.** 2020. "The Missing Profits of Nations." National Bureau of Economic Research Working Paper 24701.
- Tulli, Andrea.** 2019. "Sweeping the Dirt Under the Rug: Measuring Spillovers from an Anti-Corruption Measure." <https://sites.google.com/view/andreatulli>. Accessed on 2020-08-04.
- Waseem, Mazhar.** 2020. "Overclaimed Refunds, Undeclared Sales, and Invoice Mills: Nature and Extent of Noncompliance in a Value-Added Tax." CESifo Working Paper No. 8231.
- Yagan, Danny.** 2015. "Capital Tax Reform and the Real Economy: The Effects of the 2003 Dividend Tax Cut." *American Economic Review*, 105(12): 3531–3563.
- Zwick, Eric, and James Mahon.** 2017. "Tax Policy and Heterogeneous Investment Behavior." *American Economic Review*, 107(1): 217–248.

Appendixes

A Variable Definition

Annual VAT returns contain information on sales, purchases, value added, annual VAT liability, periodic remittances (or credits), and annual outstanding balance.

Business variables. *Sales* and *purchases* are equal to the total value of transactions completed in a given year with customers and suppliers, respectively. These variables include the total value of all goods and services traded, regardless of whether transactions are taxable, exempt, or zero-rated. *Value added* is defined as the difference between sales and purchases, whereas *value added margin* is the ratio of value added over total sales.

Tax variables. For each firm, I can compute the annual VAT liability, the periodic balance, and the balance outstanding at the time of filing the annual VAT return. First, an accounting identity links tax liability, remittance, and withholding. The amount of taxes that a firm remits to the tax authority is equal to its tax liability minus any amount withheld by third parties:

$$\text{Remittance} = \text{Liability} - \text{Withholding}$$

Each of these concept is defined in terms of sales S , purchases P , and the statutory tax rate t . Sales and purchases are classified into taxable, exempt, and those subject to withholding (labeled via a subscript T , E , and W , respectively). Therefore, I adopt the following definitions:

$$\text{VAT Liability} = t \cdot (S_T + S_W) - t \cdot P_T$$

$$\text{Remittance} = t \cdot S_T - t \cdot (P_T - P_W)$$

$$\text{Withholding} = t \cdot (S_W - P_W)$$

All these concepts are readable off annual VAT returns, except for the tax rate applied to sales subject to withholding¹⁶. To compute the VAT liability, I assume that the average tax rate applied to S_T is equal to S_W .

While the above variables are defined for the whole calendar year, firms are required to remit their VAT every month or every quarter. In case a firm owes VAT, it needs to remit

¹⁶Remittance is defined as the amount reported on item VL3 minus item VL4 of the VAT return.

the entire liability within 45 days after the end of the period. If instead a firm records a credit, it can offset future tax liabilities with it or ask for a cash refund¹⁷. In other words, firms that record a credit and do not ask for a cash refund need to report their past VAT credits in their periodic VAT balances. Therefore, I defined the periodic VAT balance as the difference between the new flow of VAT and the stock of past VAT credits that were not redeemed by the firm,

$$\text{Periodic VAT Balance} = t \cdot (S_T - P_T) - \text{Past VAT Credits}$$

Finally, at the time of filing the annual VAT return, firms need to settle any outstanding balance. That is, firms need to reconcile their annual and periodic remittances. Firms with any outstanding debt must settle their position with the tax authority by March 16th of the year following the VAT year, whereas firms with a credit position must decide how to settle their VAT credit. I defined the balance outstanding as¹⁸:

$$\text{Balance Outstanding} = \text{Annual Remittance} - \sum_s \text{Periodic Remittance}_s$$

¹⁷The panoply of rules surrounding VAT credits generate a ranking of the options available based on the time that usually lapses between the recognition of the credit and its settlement. In particular, a firm can use a VAT credit against a debit in the following VAT declaration (provided such debit arises), it can offset a non-VAT tax liability, or it can ask for a cash refund. The most important rules are provided in the Appendix. Firms must report period VAT balances on lines VH1-VH12.

¹⁸This variable is equal to the amount on item VL38 minus item VL39.

B Data Cleaning Steps

1. Eliminate firms with many trailing zeros, 2 firms.
2. Eliminate links with zero or negative sales. 81,990 unique firms have at least one link with strictly positive sales.
3. Eliminate firms with total annual sales ("*volume d'affari*") below the minimum registration threshold. This registration threshold varies according to the industry of the firm. There are 82,259 firms.
4. Take the intersection of firms that have no missing values of Total Sales and no missing value of Spesometro. These are 63,267 firms.
5. **Balanced panel.** Using an active threshold of €15,000, the balanced panel contains 49,632 firms.

C Conceptual Framework

The inversion of statutory incidence of VAT might induce a behavioral response at the level of the individual firm through mainly two channels: evasion and cash-flows. Moreover, at the aggregate level, I expect the behavioral response of firm to result in a change in the composition of government suppliers.

First, at the individual level, the inversion of statutory incidence represents a change in the evasion technology available to firms as it eliminates the possibility to evade through late or non-payments. Indeed, the stated goal for the policy intervention considered in this paper was to reduce evasion¹⁹. Moreover, similar policies increased compliance (Brockmeyer and Hernandez 2019; Kopczuk, Marion, Muehlegger and Slemrod 2016), yet others were not entirely successful in preventing evasion. (Carrillo, Pomeranz and Singhal 2017).

Second, the reform mechanically reduces cash-flows for firms with pre-reform relationships with affected buyers. While some businesses are well positioned to operate with lower cash-flows or are quick to adjust to it, others might need to access (costly) external funds. Others without that option might decide to shut down.

C.1 Costly External Finance

A firm is active for two periods. In the first period, the firms produces, sells its output and pays its suppliers. In the second one, it remits any outstanding tax liability to the tax authority. This represents a good approximation to the way actual collection of VAT occurs. Tax deadlines generally fall some weeks or months after the end of the filing period.

The firm's objective function is made of two components: net value added and the cost of external funds. Let's define net value added as the difference between revenues and costs, net of VAT:

$$NVA = (1 - \tau)[y - c(y)]$$

where $c(\cdot)$ is a strictly convex function and τ is the VAT rate.

Now, we consider cash-flows to the firm over two periods. To capture the effect of statutory incidence on cash-flows to the firm, I introduce the parameter μ . This is the share of VAT on sales for which the buyer is responsible to remit. Therefore, cash-flows are:

¹⁹A study conducted by the tax authority finds that the policy reduced VAT evasion in the aggregate (Carfora, Marigliani, Pisani and Spingola 2017)

$$CF_1(y) = (1 - \mu\tau)y - c(y)$$

$$CF_2(y) = -\tau(y - c(y)) + \mu\tau y$$

Two comments. Negative cash-flows in period 2 indicates that the firm remits its outstanding VAT liability to the tax authority, while positive cash-flows implies that it is eligible for a refund. Moreover, total cash-flows over the two periods sum up to net value added.

When $\mu = 0$, statutory incidence of VAT falls entirely on the seller and cash-flows coincide with gross value added. As μ and the proportion of sales subject to the inversion of statutory incidence increase, cash-flows decline. At the extreme, when $\mu = 1$, statutory incidence falls entirely on the buyer and the firm does not remit VAT.

When cash-flows in the first period are negative, the firm need external funds to pay its suppliers and fund its operations. These costs could stem from accessing the credit-lines or short-term loans. Following [Gomes \(2001\)](#) and [Strebulaev and Whited \(2011\)](#), I assume that the costs, defined by the function $\lambda(\cdot)$, are positive and increasing if the firm uses external funds, but they are null if the firm does not use external funds. Therefore,

$$\lambda(CF_1) \begin{cases} = 0 & \text{if } CF_1 \geq 0 \\ > 0 & \text{if } CF_1 < 0 \end{cases}$$

where CF_1 represents cash-flows in the first period²⁰.

The firm chooses net value added, taking into account the cost of external funds:

$$\max_y (1 - \tau)[y - c(y)] - \lambda(CF_1)$$

When cash-flows are positive, the first order condition is:

$$c'(y^+) = 1$$

Otherwise the first order condition becomes:

$$c'(y^-) = 1 - \frac{\lambda'(CF_1)\mu\tau}{\lambda'(CF_1) - (1 - \tau)}$$

The firm reduces output if it needs external finance when $c'(y^-) < c'(y^+)$. This is true when $\lambda'(CF_1) \geq (1 - \tau)$. Intuitively, the firm reduces its output when the marginal cost

²⁰To ease notation, I do not indicate that cash-flows depend on output.

of an additional euro of external funds, $\lambda'(CF_1)$, is higher than the marginal retention rate of output, $(1 - \tau)$.

C.2 Evasion

The firm may decide to evade parts of its VAT liability by over-reporting its costs. Let's denote reported costs as \hat{c} and the amount evaded as $e = \hat{c} - c(y)$. If it evades, the firm bears a cost which depends on (i) the amount evaded and (ii) the proportion of sales subject to inversion of statutory incidence, μ . Thus the cost of evasion becomes $g(e, \mu)$, which is strictly increasing in both arguments. Then, the firm's objective function becomes:

$$\Pi = (1 - \tau)[y - c(y)] + te - g(e, \mu) - \lambda(CF)$$

When cash-flows are positive, the first order conditions are:

$$\begin{aligned} c'(y) &= 1 \\ t &= g'(e, \mu) \end{aligned}$$

When cash-flows are negative, the first order conditions are:

$$\begin{aligned} c'(y) &= 1 - \frac{\mu t \lambda'(CF(y_-^*))}{\lambda'(CF(y_-^*)) - (1-t)} \\ t &= g'(e, \mu) \end{aligned}$$

D Additional Tables

Table D.1: Firms Exposed to the Reform Have Lower Probability of Survival

	LHS variable: Prob(Total Sales > 15000)			
	(1)	(2)	(3)	(4)
Exposure \times Post	-0.169*** (0.009)	-0.147*** (0.010)	-0.149*** (0.010)	-0.126*** (0.010)
Observations	54621	54621	54621	54621
Clusters (Sellers)	18,207	18,207	18,207	18,207
Industry \times Year FE		Y		Y
Province \times Year FE			Y	Y

Notes: This table reports the survival probability that a seller more exposed to the reform remains active after the reform relative to a less exposed seller. The unit of analysis is a seller. The sample is the analysis sample. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. An active firm is defined as a firm with more than €15,000 in total sales. Standard errors are clustered at the seller level.

Table D.2: Firms Adjust their Customer Base

	Conditional Prob of Selling to	
	Affected Clients	Non-affected Clients
Year 2015	-0.160*** (0.007)	-0.120*** (0.005)
Year 2016	-0.201*** (0.008)	-0.179*** (0.006)
Observations	7572	11109
Clusters (firms)	2,524	3,703

Notes: This table shows the difference in the probability of trading with affected clients or non-affected clients. The probability is estimated separately for firms with pre-reform relationships with affected clients and firms with pre-reform relationships with non-affected clients. The specification includes fixed effects for industry by year, province by year and firm. The sample is made of firms in the analysis sample with at least 20 percent of their sales to either non-affected clients or affected clients. Standard errors are clustered at the seller level.

Table D.3: Fewer Firms Remit VAT after the Reform

	VAT Remittance			VAT Credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Q1 2014	-0.052*	-0.031	-0.040	0.044*	0.022	0.032
	(0.022)	(0.026)	(0.025)	(0.022)	(0.026)	(0.025)
Treat × Q2 2014	-0.054*	-0.057*	-0.035	0.055*	0.055*	0.037
	(0.022)	(0.026)	(0.022)	(0.022)	(0.025)	(0.023)
Treat × Q3 2014	-0.016	-0.016	0.013	0.025	0.023	0.001
	(0.021)	(0.026)	(0.021)	(0.021)	(0.025)	(0.021)
Treat × Q1 2015	-0.113***	-0.097***	-0.083**	0.114***	0.097***	0.079**
	(0.025)	(0.027)	(0.028)	(0.025)	(0.027)	(0.029)
Treat × Q2 2015	-0.183***	-0.177***	-0.198***	0.192***	0.186***	0.207***
	(0.026)	(0.029)	(0.029)	(0.026)	(0.028)	(0.029)
Treat × Q3 2015	-0.159***	-0.162***	-0.198***	0.174***	0.174***	0.214***
	(0.027)	(0.031)	(0.029)	(0.027)	(0.030)	(0.030)
Treat × Q4 2015	-0.196***	-0.194***	-0.239***	0.203***	0.203***	0.244***
	(0.026)	(0.029)	(0.029)	(0.027)	(0.029)	(0.030)
Treat × Q1 2016	-0.258***	-0.234***	-0.266***	0.263***	0.247***	0.271***
	(0.027)	(0.029)	(0.031)	(0.028)	(0.030)	(0.033)
Treat × Q2 2016	-0.176***	-0.179***	-0.262***	0.180***	0.179***	0.262***
	(0.027)	(0.030)	(0.031)	(0.027)	(0.029)	(0.032)
Treat × Q3 2016	-0.159***	-0.161***	-0.216***	0.163***	0.158***	0.218***
	(0.027)	(0.030)	(0.031)	(0.027)	(0.029)	(0.033)
Treat × Q4 2016	-0.162***	-0.161***	-0.235***	0.171***	0.169***	0.245***
	(0.027)	(0.030)	(0.031)	(0.027)	(0.029)	(0.033)
Observations	88956	88668	88956	88956	88668	88956
Clusters (Sellers)	2471	2463	2471	2471	2463	2471
Baseline Mean	.68	.68	.55	.30	.30	.42

Notes: This table reports the coefficients β_s from specification 1. The dependent variable is an indicator variable equal to 1 if the firm remits payments of VAT in a filing period for Columns (1)-(3), while it is an indicator variable equal to 1 if the firm reports VAT credit for Columns (4)-(6). The dependent variable is calculated on a net basis in Columns (1), (2), (4) and (5), while it is calculated on a gross basis in Columns (3) and (6). The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. All specifications include fixed effects for industry by year, province by year and firm, except for Columns (2) and (5) that includes fixed effects for industry by filing period, province by filing period and firm. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Table D.4: Firms Reduce their VAT Payments after the Reform

	VAT Remittance		Net Remittance	
	(1)	(2)	(3)	(4)
Treat × Q1 2014	-0.374 (0.216)	-0.176 (0.216)	-0.514** (0.193)	-0.272 (0.192)
Treat × Q2 2014	-0.382* (0.191)	-0.256 (0.191)	-0.540** (0.186)	-0.411* (0.184)
Treat × Q3 2014	0.120 (0.180)	0.180 (0.180)	-0.173 (0.183)	-0.125 (0.182)
Treat × Q1 2015	-0.854*** (0.242)	-0.655** (0.242)	-1.054*** (0.212)	-0.812*** (0.214)
Treat × Q2 2015	-1.861*** (0.248)	-1.735*** (0.249)	-1.686*** (0.223)	-1.558*** (0.224)
Treat × Q3 2015	-1.844*** (0.256)	-1.784*** (0.257)	-1.503*** (0.230)	-1.455*** (0.230)
Treat × Q4 2015	-2.259*** (0.254)	-2.259*** (0.254)	-1.790*** (0.226)	-1.790*** (0.226)
Treat × Q1 2016	-2.413*** (0.273)	-2.214*** (0.273)	-2.371*** (0.230)	-2.130*** (0.231)
Treat × Q2 2016	-2.411*** (0.274)	-2.285*** (0.275)	-1.596*** (0.229)	-1.468*** (0.230)
Treat × Q3 2016	-1.999*** (0.276)	-1.939*** (0.277)	-1.439*** (0.229)	-1.392*** (0.230)
Treat × Q4 2016	-2.179*** (0.273)	-2.179*** (0.273)	-1.540*** (0.229)	-1.540*** (0.229)
Observations	88956	88956	88956	88956
Clusters (firms)	2471	2471	2471	2471
Baseline Level	47726		42444	

Notes: This table reports the coefficients β_s from specification 1. The dependent variable is the value of periodic VAT remittances. The dependent variable is calculated on a gross basis in Columns (1)-(2), while it is calculated on a net basis in Columns (3)-(4). The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. All specifications include fixed effects for industry by year, province by year and firm. Columns (2) and (4) control for industry-specific quadratic trends. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Table D.5: Firms Increase their VAT Credit after the Reform

	VAT Credit		Net Credit	
	(1)	(2)	(3)	(4)
Treat × Q1 2014	0.183 (0.225)	0.045 (0.224)	0.263 (0.203)	0.086 (0.202)
Treat × Q2 2014	0.255 (0.199)	0.122 (0.199)	0.415* (0.200)	0.267 (0.197)
Treat × Q3 2014	0.026 (0.177)	-0.061 (0.177)	0.258 (0.189)	0.170 (0.187)
Treat × Q1 2015	0.554* (0.264)	0.416 (0.263)	0.819*** (0.230)	0.642** (0.230)
Treat × Q2 2015	1.861*** (0.273)	1.727*** (0.273)	1.713*** (0.248)	1.566*** (0.249)
Treat × Q3 2015	2.175*** (0.282)	2.088*** (0.283)	1.759*** (0.256)	1.670*** (0.257)
Treat × Q4 2015	2.620*** (0.291)	2.620*** (0.291)	2.171*** (0.263)	2.171*** (0.263)
Treat × Q1 2016	2.424*** (0.314)	2.286*** (0.313)	2.343*** (0.266)	2.165*** (0.267)
Treat × Q2 2016	2.546*** (0.319)	2.412*** (0.320)	1.760*** (0.266)	1.613*** (0.265)
Treat × Q3 2016	2.338*** (0.322)	2.250*** (0.323)	1.732*** (0.264)	1.644*** (0.264)
Treat × Q4 2016	2.606*** (0.324)	2.606*** (0.324)	1.862*** (0.269)	1.862*** (0.269)
Observations	88,956	88,956	88,956	88,956
Clusters (firms)	2,471	2,471	2,471	2,471
Baseline Level	37,309		14,430	

Notes: This table reports the coefficients β_s from specification 1. The dependent variable is the value of periodic VAT credits. The dependent variable is calculated on a gross basis in Columns (1)-(2), while it is calculated on a net basis in Columns (3)-(4). The reform is enacted in January 2015 and the coefficient corresponding to the 4th quarter of 2014 (β_0) is normalized to 0. All specifications include fixed effects for industry by year, province by year and firm. Columns (2) and (4) control for industry-specific quadratic trends. The sample is made of monthly filers. Standard errors are clustered at the firm level.

Table D.6: Total Sales Remained Stable

	LHS variable: Total Sales				
	(1)	(2)	(3)	(4)	(5)
Exposure \times Post	-0.002 (0.015)	0.004 (0.017)	-0.006 (0.015)	-0.001 (0.017)	-0.001 (0.017)
Exposure	-0.969*** (0.050)	-0.820*** (0.048)	-0.784*** (0.051)	-0.625*** (0.048)	0.000 (.)
Observations	44958	44955	44958	44955	44955
Clusters (sellers)	14,986	14,985	14,986	14,985	14,985
Industry \times Year FE		Y		Y	Y
Province \times Year FE			Y	Y	Y
Firm FE					Y

Notes: This table reports the coefficients from specification 3 where the dependent variable is *Total Sales*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Standard errors are clustered at the seller level.

Table D.7: Total Purchases Remained Stable

	LHS variable: Total Purchases				
	(1)	(2)	(3)	(4)	(5)
Exposure \times Post	0.010 (0.026)	-0.002 (0.029)	0.009 (0.026)	-0.003 (0.029)	-0.003 (0.029)
Exposure	-1.450*** (0.075)	-1.023*** (0.066)	-1.271*** (0.076)	-0.822*** (0.065)	0.000 (.)
Observations	44958	44955	44958	44955	44955
Clusters (sellers)	14,986	14,985	14,986	14,985	14,985
Industry \times Year FE		Y		Y	Y
Province \times Year FE			Y	Y	Y
Firm FE					Y

Notes: This table reports the coefficients from specification 3 where the dependent variable is *Total Purchases*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Standard errors are clustered at the seller level.

Table D.8: The Effect of Tax Collection on Taxable Sales

	LHS variable: Taxable Sales				
	(1)	(2)	(3)	(4)	(5)
Exposure × Post	0.047 (0.039)	0.043 (0.038)	0.041 (0.038)	0.034 (0.038)	0.034 (0.038)
Exposure	-1.982*** (0.101)	-1.131*** (0.076)	-1.819*** (0.101)	-0.953*** (0.077)	
Observations	44958	44955	44958	44955	44955
Clusters (Sellers)	14,986	14,985	14,986	14,985	14,985
Industry × Year FE		Y		Y	Y
Province × Year FE			Y	Y	Y
Sellers FE					Y

Notes: This table reports the coefficients from specification 3. The unit of observation is a seller. The dependent variable is *Taxable Sales*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firms in the analysis sample. Standard errors are clustered at the seller level.

Table D.9: The Effect of Tax Collection on Taxable Purchases

	LHS variable: Taxable Purchases				
	(1)	(2)	(3)	(4)	(5)
Exposure \times Post	-0.563*** (0.121)	-0.809*** (0.123)	-0.154 (0.122)	-0.367** (0.123)	-0.367** (0.123)
Exposure	-1.310*** (0.087)	-0.643*** (0.089)	-1.104*** (0.089)	-0.465*** (0.091)	
Observations	44958	44955	44958	44955	44955
Clusters (Sellers)	14,986	14,985	14,986	14,985	14,985
Industry \times Year FE		Y		Y	Y
Province \times Year FE			Y	Y	Y
Sellers FE					Y

Notes: This table reports the coefficients from specification 3. The unit of observation is a seller. The dependent variable is *Taxable Purchases*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firms in the analysis sample. Standard errors are clustered at the seller level.

Table D.10: The Effect of Tax Collection on the Tax Base

	LHS variable: Tax Base scaled				
	(1)	(2)	(3)	(4)	(5)
Exposure \times Post	-0.003 (0.007)	-0.030*** (0.008)	0.000 (0.007)	-0.027*** (0.008)	-0.027*** (0.008)
Exposure	-0.139*** (0.013)	-0.009 (0.008)	-0.149*** (0.013)	-0.016 (0.008)	
Observations	44958	44955	44958	44955	44955
Clusters (Sellers)	14,986	14,985	14,986	14,985	14,985
Industry \times Year FE		Y		Y	Y
Province \times Year FE			Y	Y	Y
Sellers FE					Y

Notes: This table reports the coefficients from specification 3. The unit of observation is a seller. The dependent variable is *Taxable Sales*. The variable *Exposure* is defined as the share of business sales that would be subject to the new rules of VAT collection, as explained in Section 4.1. The variable *Post* is dummy equal to 1 for the years 2015 and 2016. Specifications may include fixed effects for industry by year, province by year, and firm, as indicated in each column. The sample includes firms in the analysis sample. Standard errors are clustered at the seller level.