Overclaimed Refunds, Undeclared Sales, and Invoice Mills: Nature and Extent of Noncompliance in a Value-Added Tax

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Abstract

Exploiting a Pakistani reform that cuts the tax rate on five major industries of the country from 15% to 0%, I explore the extent of and mechanisms driving VAT noncompliance in a representative emerging economy. I find that VAT evasion lowers the effective tax rate by nearly 3 percentage points in the baseline year. While the evasion is stronger in the later stages of the supply chain, to some extent it persists throughout the chain. A comparable level of noncompliance exists in export-related refunds, and invoice mills are an important channel through which the overclaim of refund takes place.

Keywords: VAT, Tax Evasion, Firm Behavior

JEL Classification: H25, H26, H32

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I Introduction

In one of the most influential results in public finance, Atkinson & Stiglitz (1976) show that under a fairly broad set of conditions employing a differential commodity tax when the government has access to nonlinear income taxation is not optimal. Notwithstanding this canonical result, a broad-based VAT based often on nonuniform rates continues to be applied together with the income tax in both rich and emerging economies. In fact, its share in government revenue is rising steadily (OECD, 2017; International Tax Dialogue, 2013). This apparent discord between theory and practice in large part reflects enforcement concerns: it is widely argued that among the class of production-efficient tax instruments VAT is perhaps the easiest to enforce (Slemrod & Velayudhan, 2020). This belief in the superiority of VAT in terms of enforcement has underpinned its remarkable expansion over the last half century.

Recently, however, a few cracks have begun to appear in this consensus. For example, Malaysia has replaced its VAT (called Goods and Services Tax) with a turnover tax (called Sales and Services Tax) from September 2018 and Zambia came quite close to doing so in 2019-2020.[1] The discontent with the VAT in these and similar other settings stems from its two well-known vulnerabilities (Keen, 2007). First, the self-enforcement forces built into a VAT work only on firm-to-firm transactions and break down at the final production stage, where sales to consumers take place (Pomeranz, 2015; Naritomi, 2019; Waseem, 2020b). This last mile problem of the VAT is particularly severe in developing economies where the final production stage is often fragmented, being composed of small, informal firms. Second, the destination-based design of VAT necessitates that any tax collected on intermediates be refunded to exporters. Refund payment provides another channel through which firms can appropriate government revenue.

While the theoretical mechanisms underlying VAT noncompliance are well-known, we still do not understand how important they are empirically. In fact, VAT's evasion in general is much less understood than income tax's. For example, there is little micro-based evidence from tax return data on how much VAT gets evaded and what mechanisms underpin it. In contrast, such evidence on income tax has been available for some time from both policy studies and academic research (see for example Slemrod, 2007; Kleven *et al.*, 2011; Artavanis *et al.*, 2016; Waseem, 2020a). This paper

[1] Please see Richard Asquith's blog at avalara.com for the policy changes in Malaysia and Zambia. Specifically, the Malaysian change is documented here and the Zambian here.

fills this gap in literature. I leverage a novel tax reform from Pakistan to uncover the pattern and drivers of VAT noncompliance in a representative emerging economy.

The reform I exploit reduced the tax rate applicable to five major industries of the country from 15% to 0%. Before the reform, in accordance with the destination principle, only exports of these industries were zero-rated and their imports and domestic supplies were subject to the standard rate. The reform reduced the rate applicable to both intermediates acquired and final goods supplied by the treated industries to zero, thus weakening considerably, if not eliminating entirely, the incentives to misreport. As the misreporting incentives decline, profit maximizing firms would begin reporting the true level of their sales and purchases to the government. This allows me to infer the extent of evasion as it existed in the treated industries at the baseline. I estimate it using a simple difference-in-differences research design, comparing outcomes across firms in the treated industries with the rest. Identification in this setup requires that the time path of outcomes would have been similar across the compared groups absent the tax reform. Using the standard event study plots spanning 84 pre-reform and 72 post-reform periods, I show that this indeed is highly plausible in my setting. I run a battery of additional tests to rule out other identification concerns, including any spillovers between the two groups.

If firms report truthfully, the reduction of the rate to zero should have no bearing on activity reported by them other than through positive, real effects, arising for example from the liquidity channel. I, however, find that reported outcomes fall sharply in the treatment group after the reform: purchases fall by 42 log-points, sales by 22 log-points, exports by 11 log-points, and non-export sales by 8 log-points. Not only are the elasticities implied by these responses of opposite sign, they are also large in magnitude, ranging between -0.5 to -2.6. Such a sharp, large fall in reported activity is consistent with, and can only be reconciled by, a large misreporting at the baseline. Using a simple conceptual framework, I back out the level of tax evasion implied by this misreporting, finding that it reduces the effective tax rate by 2.2-3.3 percentage points in the five baseline years. This noncompliance, though stronger at the later stages of the supply chain, to some extent persists throughout the chain, suggesting that the third-party information built into the VAT does not mitigate its evasion completely. Similar level of noncompliance occurs in export-related refunds with the overclaimed amount constituting around 3.4% of the exports.

VAT supply chains are rarely complete, especially in developing economies. The profusion of small, informal firms in such economies means that some part of the production process in almost every supply chain occurs in the informal sector (Waseem, 2018). When the VAT chain breaks, the tax charged at the pre-break stages cannot be claimed back in the post-break stage. This creates arbitrage opportunities, which are sometimes exploited by firms called invoice mills (Keen & Smith, 2006). These firms engage in no real business activity and exist solely to trade in VAT invoices. Invoice mills are a poorly understood phenomenon. There is little micro-based evidence in the existing literature on how they operate and how much revenue loss they cause.[2] I show that invoice mills exist primarily to help exporters claim exaggerated refunds. On average, around 83% of their reported output in the three baseline years was claimed by exporters as refund. I estimate that exporters overclaimed VAT refund by nearly 30%-48% during these years. Roughly 37% of the overclaimed amount was based on the invoices of mills.

Stiglitz (2010) argues that the central goal of a development-oriented tax policy should be to ensure that the tax structure is resistant to tax evasion. Understanding the extent of and the mechanisms driving tax evasion is the first step toward evolving such a tax design. The primary contribution of this paper is to empirically demonstrate these mechanisms in the context of an emerging economy. In this effort, the paper contributes to a small, emerging literature that studies the enforcement properties of VAT in weak enforcement settings (see for example Pomeranz, 2015; Agrawal & Zimmermann, 2019; Almunia *et al.*, 2019; Shah, 2019; Waseem, 2020b). This paper contributes to two other literatures as well. First, the traditional public finance theory has been built keeping in view the compliance and information environments of high-income economies, and there is now a growing realization that the policy predictions of this literature do not translate well to low-compliance, high-informality settings of emerging economies (Brockmeyer *et al.*, 2019b; Best *et al.*, 2015). To adapt the optimal tax theory to these settings, one needs to understand how the behavior of economic agents differs in diverse environments. This paper documents important dimensions along which VAT compliance differs across developing and rich economies, in particular how economic agents such as invoice mills exist in one setting but not in others. Finally, this paper adds to a literature that uses administrative tax return data to study tax compliance as a key constraint on the development of fiscal capacity in developing and emerging economies (see for example Bachas & Soto, 2019; Brockmeyer *et al.*,

[2]Mittal *et al.* (2018) develop a machine learning algorithm to identify bogus VAT firms. They implement their algorithm using VAT returns from the National Capital Territory of Delhi, India. The focus of this paper goes beyond identifying to documenting the behavior of invoice mills, in particular their role in facilitating noncompliance.

2019a; Slemrod *et al.*, 2019).

The rest of this paper is organized as follows. Section II develops a simple framework to guide the empirical analysis; section III describes institutional features of the Pakistani context; section IV develops empirical methodology used to tease out the effects of the zero-rating on firm behavior; section V presents the results for all firms; section V.C computes the extent of noncompliance using behavioral responses to the zero-rating reform; section VI documents the role of invoice mills in facilitating noncompliance, and section VII concludes.

II Conceptual Framework

This section develops a simple framework that characterizes key mechanisms underlying VAT noncompliance and derives a formula to compute the revenue loss caused by them.

II.A Setup

Consider a supply chain consisting of J production stages indexed by $j \in 1, 2, ..., J$. Figure I shows the supply chain, illustrating inputs and outputs of each firm along with their tax liability, focusing on the three consecutive production stages only. For simplicity, I ignore firm interactions within a production stage, assuming that it comprises a representative firm only. The firm in a given stage j uses intermediates c_j acquired from the preceding stage $j-1$ to produce output valuing s_j. Out of this output, an amount $s_{E,j}$ is exported out of the country, $s_{I,j}$ is supplied to the next production stage as intermediate, and $s_{F,j}$ is sold to the final consumer. The government implements the standard destination-based VAT, whereby exports are zero-rated, imports and domestic supplies are taxed at the standard rate τ, and a full credit of VAT paid on intermediates is allowed. By construction, the sale of intermediates at the final production stage is zero ($s_{I,J} = 0$), as is the purchase of intermediates at the first stage ($c_1 = 0$). In addition, purchases or some types of sales of firms in the middle stages could be zero, which means that the structure I use is extremely general, incorporating all types of firm interactions into it. Throughout this section, I focus solely on firms' reporting behavior. I show later that the reform does not affect firms' real production decisions significantly so that abstracting away from them is not too restrictive.

II.B First-Best Benchmark

I first consider behavior under the first-best, where all firms report their sales and purchases truthfully. Panel A of the figure illustrates the input-output linkages between firms under this scenario. In the first-best, the government receives a total revenue of

$$R \equiv \Sigma_j \ \tau. \left(s_j - s_{E,j} - c_j\right) \tag{1}$$

from the supply chain. The VAT remitted by each firm is the difference between its output tax $\tau.(s_j - s_{E,j})$ and input tax $\tau.c_j$, and the total government revenue R is the sum of VAT remitted by all firms. If the supply chain is vertical or the set of firms $j = 1, ..., J$ includes all firms in the economy, we can also write the total government revenue as $\Sigma_j \, \tau.s_{F,j}$. This alternative formulation uses the accounting identity $s_j - s_{E,j} \equiv s_{I,j} + s_{F,j}$ and the fact that the purchases and sales of intermediate reported at consecutive stages match each other under truthful reporting i.e. $c_j = s_{I,j-1}; \forall \, j = 1..J$.[3] Expressed in this way, the government revenue intuitively equals the tax rate times the total B2C sales of the supply chain. It captures the standard result that absent misreporting a VAT is equivalent to a retail sales tax given that the total government revenue under both tax systems equals the tax rate times the total B2C sales of firms.

II.C VAT with Weak Enforcement

I now consider a case closer to weak enforcement settings of developing economies, allowing firms to misreport their sales and purchases. Panel B of the figure shows input, outputs, and VAT payable of each firm in this case. I now denote sales and purchases reported by a firm by $\hat{s}_{K,j}; \ K \in \{E, I, F\}$ and $\hat{c}_j$, distinguishing them from their true values $s_{K,j}$ and c_j. Total government revenue from the supply chain in this case is

$$\hat{R} \equiv \Sigma_j \ \tau. \left(\hat{s}_j - \hat{s}_{E,j} - \hat{c}_j\right). \tag{2}$$

[3]Note that in a vertical supply chain, such as in Figure I, this condition is satisfied trivially. In non-vertical supply chains, this condition would be satisfied only if the set $j = 1, .., J$ includes all firms in the economy.

This revenue could be lower than the first-best for two reasons. First the reported values of sales and purchases may differ from their true values. Specifically, a firm has an incentive to overreport its purchases and exports $\hat{c}_j > c_j; \hat{s}_{E,j} > s_{E,j}$ and underreport its domestic B2B and B2C sales $\hat{s}_{I,j} < s_{I,j}; \hat{s}_{F,j} < s_{F,j}$. Underreporting the B2B sales, however, is much harder than underreporting the B2C sales as the former are recorded at two places (as sales at stage j and as purchases at stage $j+1$), creating information trails on such transactions. These information trails are argued to facilitate enforcement, making the VAT a better tax than its alternatives in terms of tax compliance (see for example Kopczuk & Slemrod, 2006). Given that all transactions at the last production stage are B2C transactions, the compliance problem is particularly worse in later production stages, a phenomenon known as the last mile problem of VAT. Second, the purchases of intermediates reported at a given production stage may not match the B2B sales reported at the preceding stage i.e. $\hat{c}_j \neq \hat{s}_{I,j-1}$. Such misreporting is referred to as one-sided evasion in the literature, signifying that the firm misreports on its own without colluding with its suppliers or buyers (see for example Pomeranz, 2015; Waseem, 2020b). It is a relatively crude form of evasion because it is not robust to cross-matching of sales-purchases records by the government. Comparing the government revenue in this setup with the first-best, we can write the total VAT evasion in the supply chain as

$$\Delta R \equiv R - \hat{R} \equiv \Sigma_j \; \tau . \left[(s_j - \hat{s}_j) - (s_{E,j} - \hat{s}_{E,j}) - (c_j - \hat{c}_j) \right]. \tag{3}$$

The expression within the sum here represents the VAT evaded by firm j. Summing the firm-level evasion across all firms, we can find the aggregate level of VAT evasion in the supply chain.

In my empirical application, I exploit a large tax cut that reduces the standard VAT rate on five major supply chains of the country from 15% to 0%. The reduction of the rate to zero seriously weakens, if not eliminates entirely, the incentives to misreport sales and purchases, pushing them toward their true values

$$\begin{gathered} \hat{s}_{K,j}\left(\tau = 0\right) \rightarrow s_{K,j} \\ \hat{c}_j\left(\tau = 0\right) \rightarrow c_j . \end{gathered} \tag{4}$$

It means that we can use the reform driven changes in sales ($\hat{s}_j$), export ($\hat{s}_{E,j}$), and purchases ($\hat{c}_j$) to estimate the three terms in Formula (3). For example, indexing the post-reform outcomes by t' and pre-reform ones by t, the reform-driven change in sales can be used to estimate the first term in the formula

$$\Delta \hat{s}_j = (\hat{s}_{jt'} - \hat{s}_{jt}) \approx (s_j - \hat{s}_j) \,. \tag{5}$$

The VAT evasion at the baseline, thus, can be identified at both the micro and the aggregate level.

Using the accounting identity $\hat{s}_j - \hat{s}_{E,j} \equiv \hat{s}_{I,j} + \hat{s}_{F,j}$, formula (3) can also be written as

$$\Delta R = \Sigma_j \; \tau. \left[(s_{F,j} - \hat{s}_{F,j}) - (\hat{s}_{I,j} - \hat{c}_j) \right] . \tag{6}$$

Expressed in this way, the total VAT evasion equals the underreported VAT on B2C sales (the first term) and the one-sided evasion (the second term). If one observes the break down of non-export sales of a firm $(\hat{s}_j - \hat{s}_{E,j})$ into its B2B and B2C components, one can estimate VAT evasion using this alternative formula as well. The benefit of doing so is that one can estimate the one-sided evasion separately. In my empirical setting, however, I do not observe the breakdown of the non-export sales of a firm into its B2B and B2C components. I therefore cannot estimate formula (6) directly.

II.D Overclaimed Refunds

The destination-based design of VAT requires that any tax collected on intermediates used for export be refunded to firms. We have seen above, how this creates incentives for firms to exaggerate exports, thus increasing the value of refunds they obtain from the government. Excessive refunds is one of the foremost concerns revenue authorities around the world have with the enforcement of a VAT (see for example Ebrill *et al.*, 2001; Bird & Gendron, 2007). To quantify the value of overclaimed input tax on account of exports, I rewrite formula (3) in the following format,[4] separating the VAT noncompliance in the export and domestic sectors of the economy

$$\Delta R \equiv \Sigma_j \; \tau. \left[-\left(c_{E,j} - \hat{c}_{E,j}\right) + \left(s_{NE,j} - \hat{s}_{NE,j}\right) - \left(c_{NE,j} - \hat{c}_{NE,j}\right) \right] . \tag{7}$$

Subscripts E and *NE* here denote the export and non-export values of a variable. The first term in the above expression is the overclaimed input tax on exports: it is

[4] To derive this formula, I simply substitute the two definitions $s - s_{E,j} \equiv s_{NE,j}$ and $c_j \equiv c_{E,j} + c_{NE,j}$ into (3) and rearrange terms.

the amount by which firms overreport the value of intermediates used to produce exports. In my empirical setting, I do not observe $\hat{c}_{E,j}$ directly. Firms in Pakistan report the total value of their purchases without breaking it down by its use (domestic vs. exports). I will therefore estimate the amount of excess input tax claim on account of exports using the following formula

$$\Delta R_E = \Sigma_j - \tau . f^{-1}(s_{E,j} - \hat{s}_{E,j}), \tag{8}$$

where $f(.)$ is the production function of exports i.e. $s_{E,j} = f(c_{E,j})$.

It is important to emphasize that the overclaim of export-related input tax identified by (7) is already accounted for in formula (3). Intuitively, the overclaimed input tax consists of either diverted input tax from domestic consumption toward exports or one-sided evasion. In both cases, it is already covered in the third term of formula (3). The formula therefore continues to identify the aggregate amount of VAT noncompliance in the economy. The purpose of estimating overclaimed refunds separately is only to gauge the significance of an issue that looms heavily in policy and academic discussions involving VAT noncompliance.

II.E Invoice Mills

VAT chains are rarely complete, especially in developing countries where high exemption threshold and profusion of small, informal firms mean that some part of the production process often occurs outside the VAT chain. When a VAT chain breaks, the tax remitted at the pre-break stages cannot be claimed at the post-break stage, becoming a part of the price. This creates arbitrage opportunities that are sometimes exploited by firms called invoice mills (Keen & Smith, 2006). These firms do not carry out any real business activity and exist solely to trade in VAT invoices. Figure A.I shows how this process works. In this example, I focus only on the last three production stages of a supply chain. The final stage is composed of two firms: the top firm deals exclusively in exports and the bottom in domestic sales. The firm in the preceding stage is not registered and supplies its output to the two firms, a fraction α to the exporter and the rest to the retailer. Because the firm is not in the VAT chain, the tax remitted at the $J-2$ and preceding stages, amounting to $\tau.\hat{s}_{I,J-2}$, cannot be claimed at the final production stage. Exploiting this gap, the invoice mill places itself in the $J-1$ stage, transferring the credit of VAT remitted at the preceding stages to the final stage. Doing so creates a gross surplus of $\tau.\hat{s}_{I,J-2}$, which can be shared by the colluding firms depending upon their bargaining weights.

Note that the bottom firm in the final stage has a lower incentive to deal with the invoice mill. All of its sales are to final consumers and hence do not generate any information trails. Dealing with the invoice mills brings the transaction on to books, thus creating an information trail and forcing the firm to pay tax to the extent of value-added on it. In contrast, the top firm in the final stage has a greater incentive to deal with the invoice mill. Given that all of its output is exported, its tax liability is negative and by dealing with the invoice mill it can exaggerate its purchases, increasing the refund it obtains from the government. This coincidence of incentives means that the invoice mill would divert a proportion of input tax from domestic consumption toward the export sector ($\hat{\alpha} > \alpha$), helping exporters claim exaggerated refunds. This phenomenon is knows as the diversion fraud in the literature (Keen & Smith, 2006). It results from the interaction of the two design features of the VAT—the last mile problem and the destination-principle.

It is important to emphasize that the presence of invoice mills does not invalidate formula (3). Given that the structure used to derive the formula is extremely general, it already accounts for the diversion fraud. Invoice mills are merely a mechanisms through which the total VAT noncompliance identified by the formula takes place. One other important point is that the incentive to operate as an invoice mill is strengthened if the government implements a policy commonly observed in developing countries whereby the sales of intermediates to unregistered firms are taxed at a higher rate. In our example, the gross surplus generated by the invoice mill would be $(\tau+\tau_a).\hat{s}_{I,J-2}$ if the government operates such a policy, where τ_a denotes the *additional* rate applied to the sales of intermediates to unregistered firms.

III Contextual Background

III.A Pakistani VAT System

Pakistan introduced its VAT in the 1990s. The legislation for this purpose was passed in July 1990, and although it envisaged the VAT to be a broad-based tax with standard features, the scope of the new tax was kept limited in the initial few years through large-scale exemptions. These exemptions were withdrawn rapidly from 1996 so that by the year 1998 the tax had been extended to all notable industries of the country other than the energy and services sectors (Waseem, 2020b).[5] In my empirical analysis, I focus primarily on the post-1998 period during which the VAT remained applicable to almost the entire goods sector of the country.

The Pakistani VAT largely follows the standard design. Firms with annual turnover above the exemption threshold are required to register with the tax administration.[6] Firms not required to register can do so voluntarily. While registered, whether voluntarily or otherwise, firms are required to charge VAT on their sales and are allowed to adjust the tax paid on inputs. In case the adjustment exceeds the output tax, they can carry forward or obtain the refund of the balance amount. A seller is required to issue a tax invoice for each sale transaction, and the buyer can claim the tax credit only if it possesses the invoice issued in its name. Firms are required to file a return and remit the tax due every month. The filing is based on the principle of self-assessment and there is no preaudit contact between taxpayers and tax collectors. Filed returns are considered final unless selected for audit.

The tax is destination-based: imports into the country are taxed at the standard rate and exports are zero-rated. Any tax charged on inputs used for exports is therefore refunded. To obtain refund, the exporter needs to file supporting documents in addition to the VAT return, which is treated as the refund claim. The supporting documents can be filed within reasonable time after the return has been filed, and include the customs and shipping documents showing the export of goods and VAT invoices showing the purchase of intermediates. No refund is sanctioned before an audit of the claim has been completed, and hence there is a natural delay between the claim and the payment of refund.

Panel A of Figure A.II plots the standard VAT rate in the country. The rate generally remained at 15% until 2008 when it was increased to 16%. Pakistan introduced a policy in 1998 through which supplies made to unregistered *firms* were taxed at a higher rate. Of course, the higher rate was not applicable on supplies made to end consumers. Panel B of the figure plots the *additional* rate—called Further Tax—imposed by the policy. The rate remained between 1 and 3 percentage points before it was eliminated in 2004. The policy, as I explained above, strengthens the incentive

[5]The energy sector was brought into the tax net in July 1999 and services in July 2000. Please see Waseem (2020b) for more details on the introduction and growth of VAT in Pakistan

[6]Exemption threshold is applicable to manufacturers and retailers only. For manufacturers, it was PKR 1 million in 1998, and was increased to 2.5 million in 1999 and to 5 million in 2004. For retailers, it remained at PKR 5 million throughout the sample period.

of a firm to operate as an invoice mill.

III.B Zero-rating Reform

In July 2005, Pakistan introduced a novel tax reform through which the VAT rate applicable to supply chains of five major industries of the country—textile, leather, carpets, sports goods, and surgical goods—was reduced from 15% to 0%. Before the reform, in accordance with the standard destination-based design, only exports of these industries were zero-rated and their domestic supplies were taxed at the standard rate of 15%. The reform zero-rated not only the supplies of *final* goods produced by these industries but also of their major inputs. For example, in addition to the finished goods produced by the textile industry (fabric, garments, etc.) all its major inputs including ginned cotton, polyester, yarn, undyed fabric, and important dyes and chemicals were zero-rated. The purpose of the change was two-fold (FBR, 2005).[7] First, a large proportion of the output of these industries was exported out of the country in one form or another and hence was zero-rated anyway. Long within-country supply chains of these industries, however, meant that VAT was to be remitted and claimed back whenever the goods moved from one production stage to the next. This created cash-flow problems for exporters who had to wait for the refund of VAT paid on their inputs.

Second, the VAT chains of these industries were rarely complete. The breaks had given rise to the phenomenon of spurious invoices. Over time, the volume of such invoices was growing, making it increasingly costly for the tax administration to distinguish between genuine and fraudulent refund claims. Ultimately the problems created by these two related issues became so severe that the government forsake the VAT revenue from the domestic consumption of these industries and zero-rated their entire supply chains.[8]

[7] The two purposes of the reform were described by the Federal Board of Revenue in the following words: "Delays in refunds payments has been a source of anxiety for the taxpayers. ... The measure was also necessary due to the rampant use of fake and flying invoices by unscrupulous agents to claim illegitimate refunds". The FBR hoped that the reform will lead to two benefits: "Firstly, the refund payments would be reduced considerably, and secondly there would be an improvement in the liquidity position of textile sector leading to investment and boosting export and growth further". Please see page 25-26 of FBR (2005) for details.

[8] Limited forms of such schemes have been implemented in Ireland and South Korea as well. In Ireland, for example, firms that export more than 75% of their output can obtain an authorization that allows their suppliers not to charge VAT. In South Korea, those who supply exporters are zero-rated in respect of selected transactions (see Ebrill *et al.*, 2001 for details of these two schemes). The Pakistani

The reform was announced on June 06, 2005 and became applicable from July 01, 2005. Although there was no firm commitment from the government prior to its announcement, the reform to some extent was anticipated in the sense that the government had been in negotiation with the stakeholders few months leading up to the reform to finalize details, such as the list of inputs to be zero-rated. In its final form, the reform zero-rated 152 items, which included both finished goods and major inputs of the five industries. Where an input was included in the list, its supply became zero-rated regardless of whether it was used in the production of the five industries or otherwise. For this reason, only inputs *predominantly* used by the five industries were zero-rated.[9] The list of 152 items did not include electricity and gas, two important inputs of these industries. These two inputs were also zero rated, but their zero-rating—unlike that of others—was made conditional on their use in the production of the five industries. Legislative instruments zero-rating electricity and gas were therefore issued at the firm level after verification that the firm was indeed operating in one of the five treated industries. This exercise caused some delay in the zero-rating of these two inputs. The first set of orders granting such zero-rating were issued in August 2005 but the exercise was completed only in April 2007.

The reform moved the treated firms from a standard VAT regime to a novel, new regime, where both their output tax liability and input tax entitlement reduced to *nearly* zero.[10] It thus seriously weakened, if not entirely eliminated, the incentives to misreport sales or purchases.

The Pakistani tax administration began obtaining transaction-level data from firms from July 2008. This requirement was introduced by adding an annex to the VAT return wherein firms were required to provide the details of their sales and purchases during the month, aggregating them up to the level of each supplier and buyer. Simultaneously, electronic filing of both the VAT return and the annex was made mandatory for all firms. Together, these changes reduce the costs of cross-matching sales and purchase records, enabling the tax administration to detect one-sided misreporting at a relatively low cost. This change in enforcement technology in 2008 affects

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tax reform is novel in the sense that it zero-rates the entire supply chain rather than just the pre-export production stage.

[9]Otherwise, the loss of revenue from zero-rating would have been unsustainable for the government.

[10]Note that the reform would not reduce the output tax and input tax of the treated firms to zero, although it would reduce both these variables sharply. The output tax charged would not go down to zero if the firm sells a byproduct not included in the list of zero-rated items. And the input tax would not go down to zero because not all inputs used by the treated firms were zero-rated.

both treated and untreated industries similarly and therefore should not matter in my empirical setting unless there is some interaction between it and the zero-rating reform, a point I come back to in section V of the paper.

III.C Invoice Mills

One key focus of the paper is to document the role of invoice mills in VAT noncompliance. Like all fraudulent enterprises, invoice mills take great care in disguising themselves as legitimate businesses so that distinguishing them from the others is not easy. In the Pakistani setting, however, I am able to exploit a legal mechanism used by the tax authority to identify these firms.

In the initial few years after the adoption of VAT in Pakistan, the tax authority did not have any specific mechanism to deal with invoice mills. Fraudulent input tax claims based on invoices issued by such firms were dealt with generically, like any other form of noncompliance. Over time, however, the volume of such transactions grew, making it necessary to have a tailored mechanism to deal with the issue. The new mechanism, which came into force in July 2003, empowered the tax authority to suspend the registration of a firm it suspected of being involved in the issuance of fake or flying invoices. The suspension was meant to be a temporary measure aimed at protecting revenue while an inquiry against the firm could proceed. This inquiry was of a quasi-judicial nature, where the firm was confronted with the evidence against it and was afforded an opportunity to present its case. On completion of the inquiry, either the registration of the firm was restored or it was blacklisted permanently. Once a firm was blacklisted, its invoices no longer remained valid and could not be used to claim input tax credit. I observe both the suspension and blacklisting of a firm and use these to proxy if the firm is an invoice mill.

III.D Data

I use administrative data from Pakistan, which include the universe of VAT returns filed in the country. The VAT return consists of three main sections. In the first section, firms report the aggregate value of their sales, breaking it down into three—domestic taxable, domestic exempt, and exports—components. In the second section, the aggregate value of inputs purchased are reported, divided likewise into the three components. In the final section, firms calculate their tax liability, indicating the tax charged on sales, the tax credited on inputs, and the final tax payable. They select one of the two options—carry forward or refund—in case the tax payable is negative. Each firm in the VAT net is assigned a unique registration number and is expected to file every tax period (month). The data, therefore, have a panel structure. In addition to the return data, I use information on firm characteristics from the tax register. This information includes the 4-digit industry, date of registration, and current registration status (suspended, blacklisted, or otherwise) of the firm. The 4-digit industry coding corresponds to the Harmonized Commodity Description and Coding System (HS Code) and classifies firms on the basis of goods or services they supply.[11] The industry coding allows me to determine if a firm belongs to one of the zero-rated industries.

As I note above, exporters have to file additional documents in support of their refund claims. These documents include supplier-wise details of purchases of intermediates acquired by them. These transaction-level data are available from the tax year 2002 onward, and I use them to construct linkages between exporters and invoice mills to see what proportion of a refund claim is based on spurious invoices.

IV Empirical Methodology

Given that the reform I exploit affects a subset of firms only, the natural research design in this setting is the difference-in-differences framework. I exploit the fact that the incentives to misreport reduce sharply at the time of the reform if the firm belongs to a zero-rated industry and remain unchanged otherwise, estimating the following model

$$y_{it} = \alpha_i + \lambda_t + \boldsymbol{\beta X_{it}} + \gamma.\, zero\text{-}rated_i \times after_t + \varepsilon_{it}, \tag{9}$$

where α_i and λ_t are the firm and time fixed effect, $\boldsymbol{X_{it}}$ is a vector of control variables, $zero\text{-}rated_i$ denotes that firm i belongs to an industry whose rate was cut to zero by the reform, and $after_t$ indicates a post-reform tax period (July 2005 or after). I use the model to estimate the impact of the reform on six outcomes (y_{it}) introduced as logs. In parallel specifications, I replace the double-difference term with log $(1 - \tau_{it})$, where τ_{it} is the standard VAT rate faced by firm i in period t. Given that the VAT

[11] This system is commonly used by customs administrations around the world to classify traded goods and services.

is a linear tax system, the tax rate is not endogenous to a firm's reporting decision. These parallel specifications deliver the elasticity of the corresponding outcome with respect to the net-of-tax rate.

Identification in this setup requires that a given outcome would have evolved similarly in the treatment and control groups in the absence of the tax reform. I exploit the long panel of VAT records to show that this assumption is indeed reasonable in this setting. Specifically, I plot results from the following event-study model

$$y_{it} = \alpha_i + \sum_{j=2}^{N} \delta_j.\, zero\text{-}rated_i \times 1.(tax\ period{=}j)_t + \lambda_t + u_{it}, \tag{10}$$

where j indexes the set of tax periods (months) included in the sample. I estimate the equation on a sample from July 1998 to June 2011, dropping the dummy for the first tax period. I then plot the coefficients on the interaction terms from these regressions for all six outcomes. Using these regressions, I show that all six outcomes evolve fairly similarly across the two groups in the 84 pre-reform periods, validating the key identification assumption.

Notwithstanding parallel trends, identification in this setting may fail if the zero-rating reform creates significant spillovers in the non-zero-rated industries, violating the SUTVA assumption (see for example Imbens & Rubin, 2015). These spillovers can take two broad forms. At the extensive margin, the reform could distort the entry decision of a firm.[12]. I address this concern by reporting parallel results from balanced panel samples, where the composition of the sample is held fixed. Shutting down the entry and exit, however, does not rule out spillovers along the intensive margin. These spillovers may arise from general equilibrium considerations (the zero-rating reform affecting prices or the cost of compliance generally) or from demand and supply linkages of control firms with the zero-rated industries (Waseem, 2020b). I present two sets of evidence to rule out this class of concerns (please see section VI).

I estimate the impact of the reform on the following six outcomes (y_{it}).

(i) Output Tax: By definition output tax equals the tax rate times the reported non-export sales. Given that the reform reduces the rate applicable to the treatment group from 15% to 0%, I expect a large, negative $\hat{\gamma}$ from these regressions. This decline in

[12]The differential tax treatment may force a firm which would otherwise have entered into a non-zero-rated industry to switch to a zero-rated industry or vice versa

output tax is a combination of the mechanical effect (reduction of the rate to zero) and the behavioral effect (changes in reported non-export sales). I isolate the behavioral effect by estimating the sales response separately (see below). The objective of showing the output tax response is to demonstrate that a strong first-stage exists in this setup, whereby the output tax liability of treated firms reduces sharply relative to the control firms as a result of the reform.

(ii) Input Tax: The case of input tax is exactly similar to that of output tax outlined above. Recall, however, that there was some delay in the zero-rating of two important inputs (electricity and gas) used by the treated firms.[13] I therefore expect the immediate impact of the reform on input tax to be smaller than that on output tax. This dynamics of the response can be seen clearly in the event study plots.

(iii) Sales: The variable denotes *total* sales—sum of exports and domestic (both B2B and B2C) sales—of the firm in a given tax period (month). Because the variable is not directly affected by the tax rate, its response represents pure behavioral effect of the reform, as do the responses of the three next outcomes outlined below.

(iv) Purchases: The variable denotes the reported value of taxable inputs purchased by the firm in a given tax period. These inputs include raw materials and intermediates acquired by the firm from other firms but do not include non-taxable inputs such as labor.

(v) Exports: The variable represents the total value of exports made by the firm in a given tax period.

(vi) Non-Export Sales: The variable denotes the value of non-export sales of the firm in a given tax period. I construct this variable by taking away exports from aggregate sales reported by the firm. It therefore includes both B2B and B2C domestic sales.

Table A.I presents summary statistics of the data. The treatment group here contains firms belonging to the five zero-rated industries; all other firms are included in the control group. The first row of the table reports firm-month observations of the two groups in the sample for the two baseline years, 2003 and 2004. The next rows present mean of the six VAT outcomes and other firm characteristics for the two

[13] Orders zero-rating electricity and gas were issued at the firm level. The first set of these orders were issued in August 2005 and the last in April 2007. See section III.B for details.

baseline years. On average, the treated firms are larger and are more likely to be engaged in exports. But they are not much different from the control sample in other characteristics such as location and age.

V Firm Responses to the Reform

V.A Nonparametric Evidence

I first present visual evidence on how firms respond to the reduction of the VAT rate to zero. The analysis is then formalized using the regression based framework.

V.A.1 First Stage

Before documenting firm behavior to the zero-rating, it is important to show that the reform creates large tax variation between the treated and untreated firms. I do so by presenting both aggregate and micro level evidence.

Figure II shows the aggregate level evidence, plotting the amount of VAT refund paid in Pakistan as a proportion of gross VAT collected in the country. The figure is based on annual aggregate statistics reported by the FBR on its website, which include both treated and untreated industries. The refund-to-gross-collection ratio in the country was roughly 20% at the baseline. It fell by nearly 10 percentage points in the first year after the reform. It fell even further in the later years as the backlog of pending refunds was cleared and more inputs of the treated industries were zero-rated (electricity and gas), settling at around the 5% level. The refund paid in the country thus dropped to one-fourth of the baseline level within three years of the reform; in terms of absolute numbers, the amount refunded reduced from PKR 55 billion in 2004 to PKR 27 billion in 2008.[14]

To show that this large drop in refund was triggered by the zero-rating reform, I next turn to micro level evidence showing the reform's effects on output tax charged and input tax claimed by the treated firms. Figure III plots the coefficient $\hat{\eta}_t$s from the

[14]For these statistics, see FBR Year Books from 1999 to 2009 available here. Year books prior to 1999 are unfortunately not available on the FBR's website.

following version of the event study equation (10)

$$y_{it} = \alpha_i + \sum_{j=2}^{N} \eta_j . 1.(tax\ period{=}j)_t + u_{it}, \tag{11}$$

where j indexes the tax periods (months) included in my estimation frame. I estimate the equation separately for the treatment and control groups, omitting the dummy for the first tax period (July 1998). A given $\hat{\eta}_t$, therefore, denotes the average within-firm change in the outcome from July 1998 to the period t for the corresponding group of firms. Figure IV presents the DD version of these plots, where I display the coefficients $\hat{\delta}_t$s from equation (10) along with the 95% confidence intervals around them. Panels A-B of the two figures together comprise the first-stage of the empirical setting, depicting the responses of output tax and input tax to the reform. Clearly, a very sharp drop occurs in the treated outcomes exactly from the time of the reform, while the two control outcomes continue to evolve on the preexisting trend. The dynamics of the two responses is also consistent with our expectations. Both output tax and input tax decline sharply as the reform comes into effect, but unlike the output tax the input tax continues to drop, stabilizing only around the beginning of the tax year 2008. The continuing drop of input tax, as noted in section III.B above, is very likely due to the time taken in zero-rating of the two important inputs—electricity and gas—of the treated firms.

Taken together, the above two pieces of evidence demonstrate that a very strong first stage exists in this setting. The incentives to misreport collapse in the treatment group at the time of the reform as both their output tax liability and input tax entitlement crash down to a near-zero level. Such a salient drop in incentives is likely to induce sharp behavioral responses to which I turn next.

V.A.2 Behavioral Responses

Panels C-F of Figures III and IV illustrate these responses. None of the four outcomes shown in these panels is directly influenced by the tax rate, and their responses therefore isolate *pure* behavioral effects induced by the rate cut. These behavioral effects are negative for all four outcomes. Sales, purchases, exports, and non-export sales decline sharply in the treatment group after the reform. This decline is the sharpest for purchases, which fall by 30-35 log points immediately after the reform. Compared to purchases, the decline in other outcomes is slow and gradual, materializing fully in the next two years or so. This pattern of responses is expected. Purchases are expected to go down immediately once the incentives to overreport them cease to exist. Past overreporting of purchases, however, would have left firms with large inventories even if only in books, which would not let the volume of reported sales and exports drop to the new equilibrium in the few periods following the reform.

The event study results also validate my empirical strategy. The preexisting trends were fairly parallel in the two groups for all the six outcomes I explore (see Figures III and IV). The reform causes sharp changes in the treatment outcomes, while the control outcomes continue to evolve on the preexisting trend with no appreciable break at the time of the reform.

V.B Regression Results

Table I presents the results from the difference-in-differences model (9) for each of the six outcomes. I always include the full set of firm and tax period (month) fixed effects and cluster standard errors at the firm level (Abadie *et al.*, 2017). The results are consistent with the visual evidence presented above. Both output tax and input tax drop sharply as a result of the reform, showing that a strong first-stage exists in this setting. Columns 3-6 report pure behavioral responses induced by the rate cut. If firms report truthfully, the reduction of the rate to zero should induce no change in behavior other than some positive, real effects on activity operating through the demand or liquidity channels. But the observed responses reveal a different story. Not only are all elasticities of the opposite sign, they are also very large, ranging from -0.5 to -2.6. The large fall in reported activity in the treated industries is consistent with, and can only be reconciled by, a large misreporting at the baseline. In section V.C, I use formula (3) to estimate the extent of this misreporting and the mechanisms underlying it.

The event study results presented above show that the key identification assumption in my empirical setting—parallel trends—is plausible. Panel B of the table tests this formally. I estimate a parallel placebo specification, where I estimate equation (9) on the pre-reform panel only, pretending that the zero-rating reform occurred in July 2002. The results support the identification assumption. There is little to distinguish the treated and untreated groups in terms of the baseline evolution of the VAT outcomes.

Table II explores the dynamics of the responses. I estimate a flexible version of model (9) by replacing the double-difference term with six double-interactions, one each for every post-reform tax year. The results confirm the time pattern of response seen visually in Figures III and IV. The first year response as a proportion of the average post-reform response is 79% for input tax, 94% for output tax, 80% for purchases, 32% for sales, 18% for exports, and 56% for non-export sales. I have already noted the likely reasons for this pattern. The first two outcomes capture the mechanical impact of the reform, which unsurprisingly is immediate.[15] Of the other four items, purchases were likely to be impacted first given that once input tax credit available on purchases drove down to zero there was no incentive to overreport them. In contrast, sales, exports, and non-export sales would return to the new equilibrium only after inventories built up in the books through past overreporting of purchases have been cleared.

Two important events occur in 2008 that may influence the interpretation of my results. First, as I note in section III.B, Pakistan introduced new filing requirements from July 2008, which mandated firms to file transaction-level data along with their returns. Second, the financial crisis hit the world markets, initiating the Great Recession. Some of the negative responses documented above may reflect that these events affect the treated industries worse than the nontreated industries. For example, the negative export response in 2008 and later years might reflect that the Great Recession reduced the demand of Pakistani exports of treated industries more than those of others. Figure A.III addresses this class of concerns. It is a truncated version of Figure IV, where I show the post-reform periods only. Zooming in on these periods shows that the reform started a slow, downward trend in the outcomes of the treated industries. This downward trend did not accelerate during 2008. In fact, the exports of the treated industries started rising again from the mid of the financial year 2008-2009. The evolution of the responses thus rules out any significant *differential* impact of the two events on the treated industries.

Recall that the reform applied to five major industries. Of these, textile is the largest and the most important in terms of its VAT impact. Table A.II shows this formally. I break down the aggregate response reported in Table I into its constituent textile and non-textile components. The response of the textile industry roughly equals the average response for all the outcomes: all textile coefficients are within the 95% confidence interval of the corresponding baseline coefficient. The finding is signifi-

[15]The slightly lower first-year response of input tax, as I note above, was in large part due to the delay in the zero-rating of electricity and gas used by the treated firms.

cant in one important respect. The textile industry has a very well-defined supply chain comprising five distinct production stages: ginning, spinning, weaving, processing, and the made-up stage.[16] Given that I observe the production stage a textile firm operates in, I can explore any heterogeneity in response across the supply chain. One key difference between various production stages in the supply chain is that the upstream stages produce intermediates rather than consumer goods. For example, the outputs of the ginning and spinning industries—pressed cotton and cotton yarn—have no significant non-industrial use. To the extent that upstream firms engage primarily in B2B transactions, they have distinctly lower incentives to misreport their outcomes. The heterogeneity analysis can thus help us uncover the nature of the observed responses. Specifically, any real responses produced by the reform would be roughly symmetric throughout the supply chain. Reporting responses, on the other hand, would be stronger in the later stages.

Table III carries out this exercise. I restrict the treatment sample to the textile industry only and estimate a triple-difference version of model (9) by including interactions of the double-difference term with dummies indicating the production stage. I include dummies for the three upper-most production stages—ginning, spinning, and weaving—only, leaving the rest as the omitted category. The results reflect important heterogeneity in response across the supply chain. Responses are considerably weaker in the upstream stages, becoming progressively stronger as one moves down the supply chain. For example, both sales and purchases of the first production stage exhibit no significant change relative to the control group after the reform. This suggests that the real production activity did not change significantly in the treated industries.[17] In contrast, treated outcomes decline by a lot in the later production stages.[18] To the extent that this decline captures tax evasion, increasing response over

[16]Cotton ginning is the first production stage of the textile industry. In it, cotton fiber is separated from the seed and is compressed into bales. Spinning converts these cotton bales into cotton yarn, which then is converted into gray fabric by the weaving industry. Processing converts gray fabric into colored and printed fabric, which finally is converted into garments and other textile made-ups by the final production stage.

[17]I assume here that the zero-rating response at the first production stage would largely capture the real production effect as the stage only produces intermediates used exclusively by large spinning mills, making misreporting less likely.

[18]Another feature of the results is that the input tax drops more in the downstream stages, while the output tax does so in the upstream stages. This is expected because overreporting inputs becomes more feasible as one moves down the value-added chain with both the number and share of taxable inputs increasing in the downstream stages. For example, the first production stage—ginning—primarily uses two inputs only. Both these inputs—labor and raw cotton—are not taxable. Compared to this almost all non-labor inputs used by later production stages are taxable, increasing the margins

the supply chain from the second stage onward suggests that the tax evasion is not limited to the later production stages but to some extent persists throughout the supply chain.

One important concern in this setting is that the zero-rating reform may create spillovers in the nontreated industries, violating the SUTVA assumption. Table A.III shows that I get fairly similar results when the sample is reduced to a balanced panel of firms that file throughout the sample period. This mitigates the spillover concern along the extensive margin. Figure III further shows that the outcomes of nontreated industries do not exhibit any signs of a structural break at the time of the reform. Building on this evidence, Table A.IV and A.V explore spillovers more formally. If the reform creates significant spillovers, they would be stronger in industries whose products are close substitutes or complements of the treated goods. The tables test this by looking at the evolution of outcomes of industries similar to the treated ones. I have mentioned in section III.D that the Pakistani tax administration follows the HS Code to classify firms into industries. The first two digits of this eight-digit code divide firms into broad industry categories with similar industries getting adjoining codes.[19] The tables exploit this scheme of classifying industries. For Table A.IV, I drop all treated industries from the sample and compare the evolution of outcomes in their adjoining industries with that of others. The table reports results from specifications parallel to ones in Table I, the only difference being that the variable *treat* now indicates firms belonging to the adjoining industries. I experiment with three definitions of adjoining. For space considerations, I report results for the three main outcomes—sales, purchases, and exports—only. Table A.V is structured similarly with the difference that it retains the treated industries but includes the adjoining industries as an additional category. Reassuringly, all specifications return trivial or insignificant spillover coefficient, putting to rest the concern that the reform might have affected outcomes of the nontreated industries as well.

I have noted above that the reform was anticipated in the sense that the government was discussing its modalities with the stakeholders before its formal announcement on June 6, 2005. To the extent that the reform was anticipated, it may trigger some intertemporal shifting of activity by forward looking firms to calibrate their response to the new incentives. I show later (section VI) that invoice mills indeed en-

along which overreporting of purchases can take place.

[19]For example the code 08 is assigned to edible fruit and nuts; 09 to coffee, tea, mate and spices; and 10 to cereals. The adjoining codes thus contain fairly similar industries with their products being close substitutes or complements.

gaged in such intertemporal shifting, bringing activity forward to pre-reform periods. Here, I focus on firms other than invoice mills, which comprise 93% of the treatment sample, to see if they also engaged in such shifting. I begin with Figure A.IV, which plots the aggregate values of the VAT outcomes reported by these firms. Clearly, there are no signs of shifting of activity across the time period of the reform. This should not be surprising as the event study plots in Figures III and IV do no exhibit any concentration of activity on either sides of the reform: there is no peak or hole in the treated series close to the tax cut. Tables A.VI and A.VII rule out anticipation related concerns even further. The former shows that dropping invoice mills—the subsample that did engage in the intertemporal shifting of activity—does not make any material difference to the results. The latter defines the $after$ period from the date of announcement of the reform rather than the date of its coming into force. This specification also produces very similar results.

Table A.VIII rules out one alternative explanation of the results. It can be argued that the incentives to maintain records (receipts, invoices, etc.) and to report them correctly go down once the tax rate goes down to zero. In this world, the negative responses documented in Table I are explained by lazy reporting in the post-reform periods rather than a reduction in misreporting. To rule out this concern, the table looks at the responses of treated corporate and non-corporate firms separately. The idea behind the exercise is that lazy reporting is expected to be worse and the observed responses hence strongly negative among noncorporate firms whose quality of record maintenance is in general poorer. Contrary to this, the responses are on average less negative for noncorporate firms. Lazy reporting is therefore unlikely to be a significant explanation of the observed responses.

V.C Quantifying VAT Evasion

I show in section II.C that a lower bound on the amount of VAT evaded in the treated industries is provided by formula (3). Table IV uses the reform-driven changes in sales, exports, and purchases, as documented in Table I, to estimate this lower bound. Rows 1-3 of the table report the three terms in the formula for the five baseline years (2000-2004). To estimate each term, I multiply the percent change in the corresponding variable as implied by its response reported in Table I with the aggregate value of the variable reported in the given year. Row 4 estimates the total amount evaded in PKR billions using formula (3). In the final row of Panel A, I express the total amount evaded as a proportion of total sales reported in the treated industries in the year.

Panel B of the table identifies evasion arising from the overclaim of VAT refunds. Section II.D shows how formula (8) can be used to estimate this amount. Before I can use formula (8), I need to estimate the production function of exports $f : \mathbb{R}_+ \rightarrow \mathbb{R}_+$ $s_E = f(c_E)$. Figure V plots a nonparametric representation of this mapping for the pre- and post-reform years. To construct the plot, I group firms into small bins on the basis of log purchases reported by them. I then plot the average log exports of firms in each bin. The sample for this binned scatter plot consists of all firm-month observations of the treated industries, excluding those with the log purchases less than the 5th or more than the 95th percentile of the corresponding (pre- or post-reform) distribution. I also superimpose a linear regression line on the scatter plot. The relationship between the two variables in the log-log space is approximately linear with a slope parameter of 0.5, which means that the production function of exports is $s_E = A \,.\, c_E^{0.5}$. Note that this relationship does not need to be causal as I only use it to predict $\Sigma_j \left(c_{E,j} - \hat{c}_{E,j}\right)$ implied by $\Sigma_j \left(s_{E,j} - \hat{s}_{E,j}\right)$, the term identified through the DD estimator. Rows 6-10 compute the amount of overclaimed refund, expressing it as a percentage of total exports (Row 9) and total refund claimed (Row 10) by the treated industries.

The results reveal the large amount of evasion that existed in the treated industries at the baseline. The evaded amount varies from 2.2% of total sales in the year 2000 to 3.3% in 2004 (Row 5), with a continuous, increasing trend over time. To put these numbers into context, note that the average value-addition in the treated industries at the baseline was around 40%, which means that the average effective tax rate on sales in these industries was nearly 6%. The effective tax rate was lower by 2-3 percentage points or by 33%-50% because of tax evasion.[20] Comparable level of noncompliance exists in the export-related refunds. Overclaimed refunds amount to nearly 3.4% of total exports reported in the treated industries (Row 9). They constitute between 26%-48% of the total refund claimed by the treated industries (Row 10).[21]

The above estimates rely on the assumption that sales and purchases reported at

[20]Ideally, one would have liked to express the evaded amount as a percent of $\Sigma_j \, \tau.s_{F,j}$, the first-best revenue the government receives under the VAT or the alternative retail sales tax. Unfortunately, however, I do not observe the breakdown of total sales into its B2B and B2C components and therefore cannot compute $\Sigma_j \, \tau.s_{F,j}$ without making strong assumptions.

[21]Note that I observe the export-related refund claimed by firms $\tau.\hat{c}_{E,j}$ but not the export-related purchases $\hat{c}_{E,j}$. In the post-reform years, as $\tau \rightarrow 0$, the refund claimed also goes to zero. That is why I cannot look at the evolution of this variable directly to estimate the amount overclaimed as refund in the economy.

the zero tax rate would approach their true values

$$\hat{s}_{K,j}\left(\tau=0\right)\rightarrow s_{K,j}$$
$$\hat{c}_{j}\left(\tau=0\right)\rightarrow c_{j}.$$

How realistic is this assumption? Some of the firms in the VAT net are subject to other forms of taxation such as the corporate income tax. To the extent that firms need to match their reports across tax bases, it can be argued that the reduction of the VAT rate to zero does not eliminate the misreporting incentives entirely. It is important to emphasize, however, that reporting requirements differ substantially across tax bases. Importantly, firms can adjust along margins reported in one base but not in others to optimize their tax liability to incentives in each base (see Carrillo *et al.*, 2017; Waseem, 2020a for recent empirical evidence on such behavior).[22] It is particularly important in my setting as I look at a relatively long time frame after the reform when firms had sufficient time to make such adjustments. Note further that an empirical test of the strength of these *external* incentives is that behavioral responses to the zero-rating would be weaker among corporate firms who not only are required to pay tax on their profits but are also subject to greater reporting requirements under corporate laws.[23] We, however, do not see this. In fact, corporate firms respond more aggressively than other firms to the zero-rating (see Table A.VIII). The assumption that the post-reform behavior resembles the first-best closely therefore seems plausible in this setting, meaning that the estimates I report above provide tight lower bounds on VAT evasion in the treated industries.

The pattern of evasion I uncover has three important features. First, a substantial amount of VAT gets evaded each year (Table IV). This shows that the VAT's resilience to tax evasion is to some degree overstated. Second, while the evasion is stronger in the later stages of the supply chain, to some extent it persists throughout the chain (Table III). Given that upstream stages predominantly deal in B2B transactions subject to third-party information, the results suggest that the third-party information does not mitigate tax evasion completely. To this extent, the results are consistent with the recent public finance literature that finds important complementarity between

[22]For example, firms may adjust line items not reported on the VAT return such as labor or administrative costs to leave their corporate tax liability unchanged while reporting lower sales and purchases in the VAT.

[23]They, for example, are required to file audited accounts to the Securities and Exchange Commission of Pakistan.

the third party information and the administrative capacity of the revenue authority. Finally, the evasion is likely to be worse in export oriented supply chains. Negative tax liability of exports offers another channel through which firms can appropriate government revenue, in part by diverting input tax entitlement from domestic consumption toward exports. This diversion is sometimes facilitated by invoice mills, a mechanism to which I turn next.

VI Invoice Mills and VAT Evasion

One important focus of this paper is to understand the role of invoice mills in facilitating VAT's noncompliance. As I note in section II.E, invoice mills arise naturally in the low-enforcement, high-informality setting of developing countries, bridging the gap created by broken VAT chains. Figure A.V illustrates this empirically, plotting the number of blacklisted firms that file a return in the given tax period (month). Since blacklisting proxies if a firm is an invoice mill, the plot captures how the stock of invoice mills evolves over time in the Pakistani VAT system. Invoice mills appeared soon after the destination-based VAT was implemented in the country. Their stock grew sharply in the next few years, reaching a peak of around 1750 in 2003. The rising trend was partly reversed in 2003, when the new mechanism to blacklist firms came into effect.[24] But a sharper decline occurred after 2005, when the zero-rating reform became applicable. The initial evidence thus suggests that invoice mills primarily exist to serve the export refund market (their numbers decline as the tax rate on five major export-oriented industries declined to zero). To explore this point further, I next look at the *activity* reported by these firms

Figure A.VI looks at the evolution of VAT outcomes reported by invoice mills, replicating the event-study analysis of Figure III. Since invoice mills (blacklisted and suspended firms) form only 7% of the treatment sample, the results are noisier than the complete sample results. Yet the pattern of responses is very similar. All four outcomes—sales, purchases, exports, non-export sales—fall sharply at the time of the reform, with purchases falling more than any other outcome. Table A.IX formalizes these results. I estimate a triple-difference version of model (9), exploring any dif-

[24]Note that blacklisting began from the tax year 2003, and therefore most of the returns included in this plot were filed at the time the firms had still not been declared blacklist and invoices issued by them were still legal tender. In subsequent analysis, I break down these firms' behavior around the event of blacklisting, looking at the pre- and post-blacklisting periods separately.

ferential response of invoice mills from other treated firms. Unsurprisingly, mills' responses are larger than those of other treated firms. But since they constitute only a small proportion of the overall sample, their larger responses do not affect the average too much: the double-difference $treat \times after$ coefficient is always within the 95% confidence interval around the baseline coefficient.

One notable feature of the event study plots in Figure A.VI is that the outcomes of mills spike just before the reform. This spike is particularly prominent in the top four panels of the figure. To explore this finding further, Figure VI plots the aggregate values of the six outcomes of mills in each month. Relative to the within-firm average changes shown in Figure A.VI, this figure displays the aggregate *level* of each outcome, illustrating more clearly the magnitude of spurious input tax credit injected into the system by invoice mills. Strikingly, all outcomes exhibit a sharp spike just before the zero-rating reform took effect. For example, aggregate purchases jump from the prereform average level of around PKR 5 billion to 30 billion one tax period before the reform, *i.e.* in May 2005. This sharp jump is followed by an even sharper fall, whereby purchases reduce to PKR 3.6 billion in July 2005 and to 1.8 billion in November 2005.[25]

This large concentration of activity on the wrong side of the reform is puzzling. Under any standard model of tax behavior, a large tax cut reducing the rate from 15% to 0% would induce some inter-temporal shifting of activity from the high-tax periods to the low-tax periods.[26] The behavior I find is polar opposite to this. The only plausible explanation of the behavior therefore is that it captures invoice mills injecting input tax credit into the system while the goods are still standard-rated. Doing so maximizes spurious refunds in accordance with the mechanism laid out in section II.E.

How much of excess refund did invoice mills inject into the system? As I note in section III.A, exporters file supplier-wise details of their purchases at the time of claiming VAT refund. These transaction-level data are available from the tax year 2002 onward, and they allow me to quantify the role of invoice mills in the overclaim of refunds. Figure VII reports the results of this exercise. The blue curve plots the

[25]One other important feature of the plots is a very large spike in exports just before the beginning of the tax year 2008. This is very likely driven by an effort by invoice mills to clear their inventories before the requirement of filing transaction-level data comes into effect from the tax year 2008.

[26]For instance, booking a transaction that occurs just before the reform to a date just after the reform could save the seller the costs of remitting the tax, the buyer the costs of claiming the input tax credit, and any associated cash-flow costs.

aggregate value of refund claimed by exporters each month on the invoices of blacklisted firms. The red curve, on the other hand, plots the aggregate value of output tax in all invoices issued by blacklisted firms each month. Although the two curves are based on data from two different sources—the blue from refund claims filed by exporters and the red from VAT returns filed by blacklisted firms, they line up quite well. This shows that a vast majority of invoices issued by mills end up in the refund claims of exporters. A regression of blue series on the red estimated for the pre-reform years returns a coefficient of 0.83, suggesting that on average 83% of the output of invoice mills was being claimed as refund by exporters.[27] This ratio was particularly higher in 2004, when nearly 97% (PKR 8.6 billion) of the output of blacklisted firms was claimed as refund by exporters. I estimate in Table IV that exporters overclaimed VAT refund of nearly PKR 23 billion in 2004 (see Row 5 of the table). Roughly 37% of this amount was based on invoices of blacklisted firms.

Figure A.VII tracks the evolution of VAT outcomes of blacklisted firms around the event of blacklisting. It illustrates that by the time a firm is blacklisted it is already past its peak activity and both its turnover and purchases are declining. In addition, almost all the activity of such firms occurs while their invoices are still legitimate. To see why invoice mills are detected so late in their life cycle, Figure A.VIII and Table A.X compare their characteristics with other firms. Invoice mills are essentially hit-and-run enterprises, and consistent with this notion their most distinguishing feature is the level of activity reported immediately after registration.[28] To catch them, the revenue authority therefore needs to move early when they are still distinguishable from other firms and when they have still not caused significant revenue damage. This, however, requires significant administrative capacity on the part of revenue authority, which is usually lacking in developing and emerging economies.

VII Conclusion

The value-added tax has seen remarkable expansion in recent decades. Its popularity in large part is driven by the belief that among the class of production-efficient tax in-

[27] This finding, however, is subject to an important caveat. I identify mills using the blacklisting procedure employed by the Pakistani tax administration. It is possible that this procedure is more effective in identifying mills connected to exporters either because the government is more concerned about the overclaim of refund than the underpayment of VAT on domestic transactions or because of any data limitation.

[28] *Early* in Figure A.VIII denotes the level of activity reported in the first six months after registration

struments it perhaps has the best enforcement properties. By creating (1) third-party information on firm-to-firm transactions; (2) tax withholding at the upstream production stages; and (3) asymmetric cheating incentives between sellers and buyers, a VAT facilitates tax compliance (Waseem, 2019). The revenue gains from these mechanisms, however, need to be weighed up against the losses arising from revenue-worsening mechanisms built into a VAT: (1) the destination principle that necessitates the payment of refund on exports, and (2) the lack of third-party reporting on B2C transactions, and (3) broken VAT chains that give rise to phenomena such as invoice mills. These mechanisms are emphasized a great deal in the policy literature (see, for example, Bird & Gendron, 2007), but due mainly to a lack of empirical evidence are largely absent from the economic literature. This paper fills the gap by casting light on the nature and strength of these mechanisms in a representative emerging economy.

For this purpose, I leverage a novel tax reform that seriously weakens the incentives of firms to misreport their VAT outcomes. The variation allows me to infer the level of misreporting in the treated industries as it existed at the baseline and study the mechanisms underlying it. I document four key facts. First, the evaded VAT constitutes a significant part of the tax base, lowering the effective tax rate by nearly 2-3 percentage points. Second, while the evasion is stronger in the later stages of the supply chain, which predominantly deal in B2C transactions, to some extent it persists throughout the chain, suggesting that the third-party information built into the VAT does not mitigate tax evasion completely. Third, comparable level of noncompliance occurs in the export-related refunds with the overclaimed amount constituting around 3.4% of the exports. Finally, invoice mills are an important conduit through which the overclaim of refund takes place. A vast majority of the output involved in their invoices (>80%) ends up in the refund claims of exporters. Roughly 37% of the amount overclaimed as refunds is based on invoices issued by mills. The results have important policy implications, highlighting that in weak-enforcement, large-informality settings of developing economies modern tax instruments do not work the same way they do in the standard economic models. Optimal tax theory and policy advice to developing economies needs to adjust to these realities.

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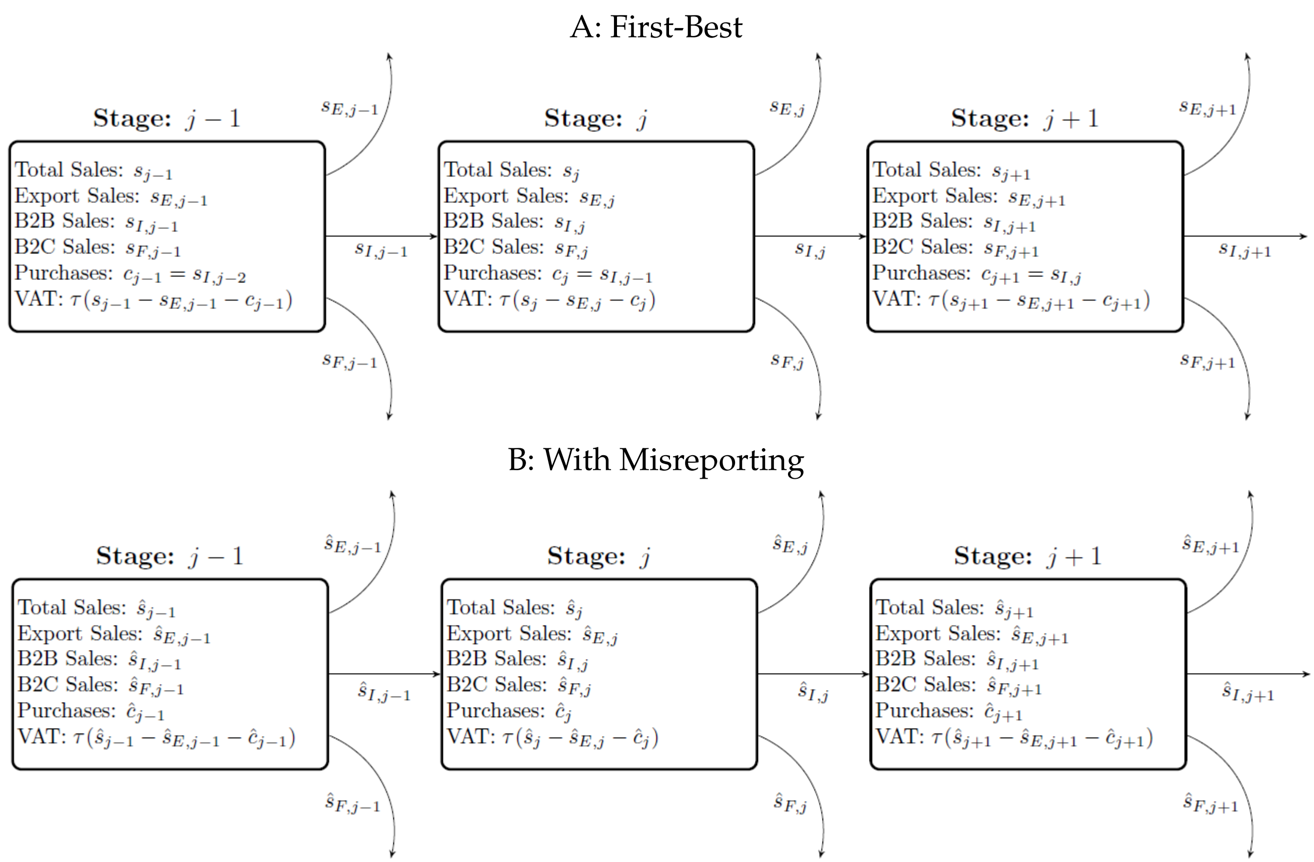
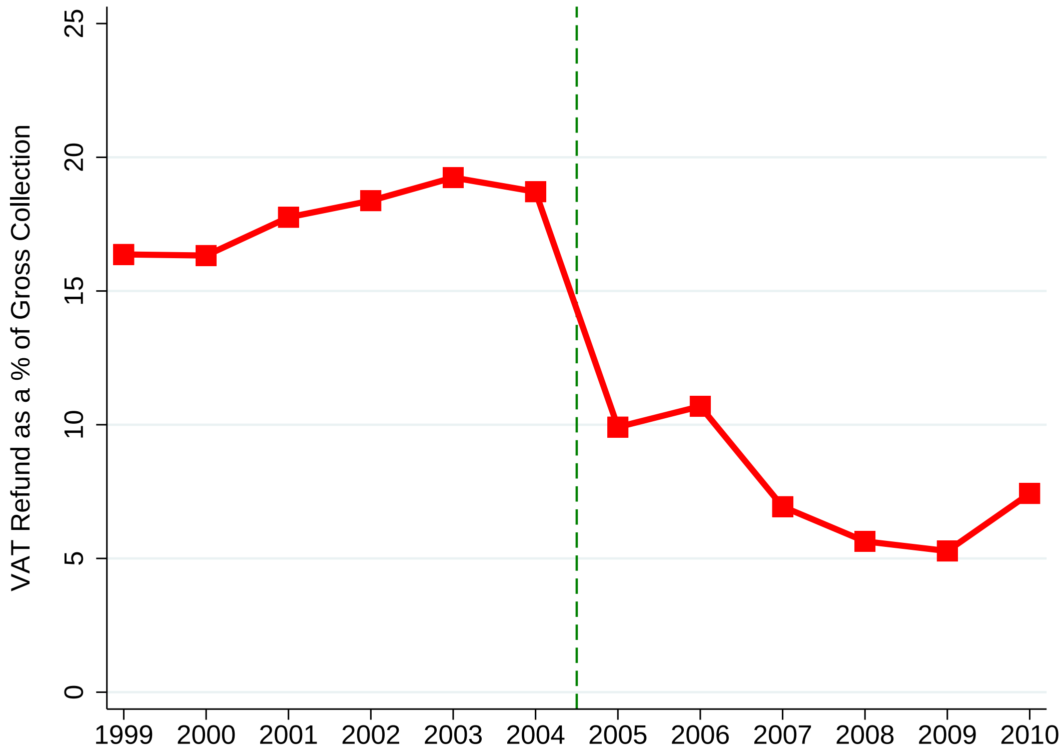
FIGURE I: **CONCEPTUAL FRAMEWORK**



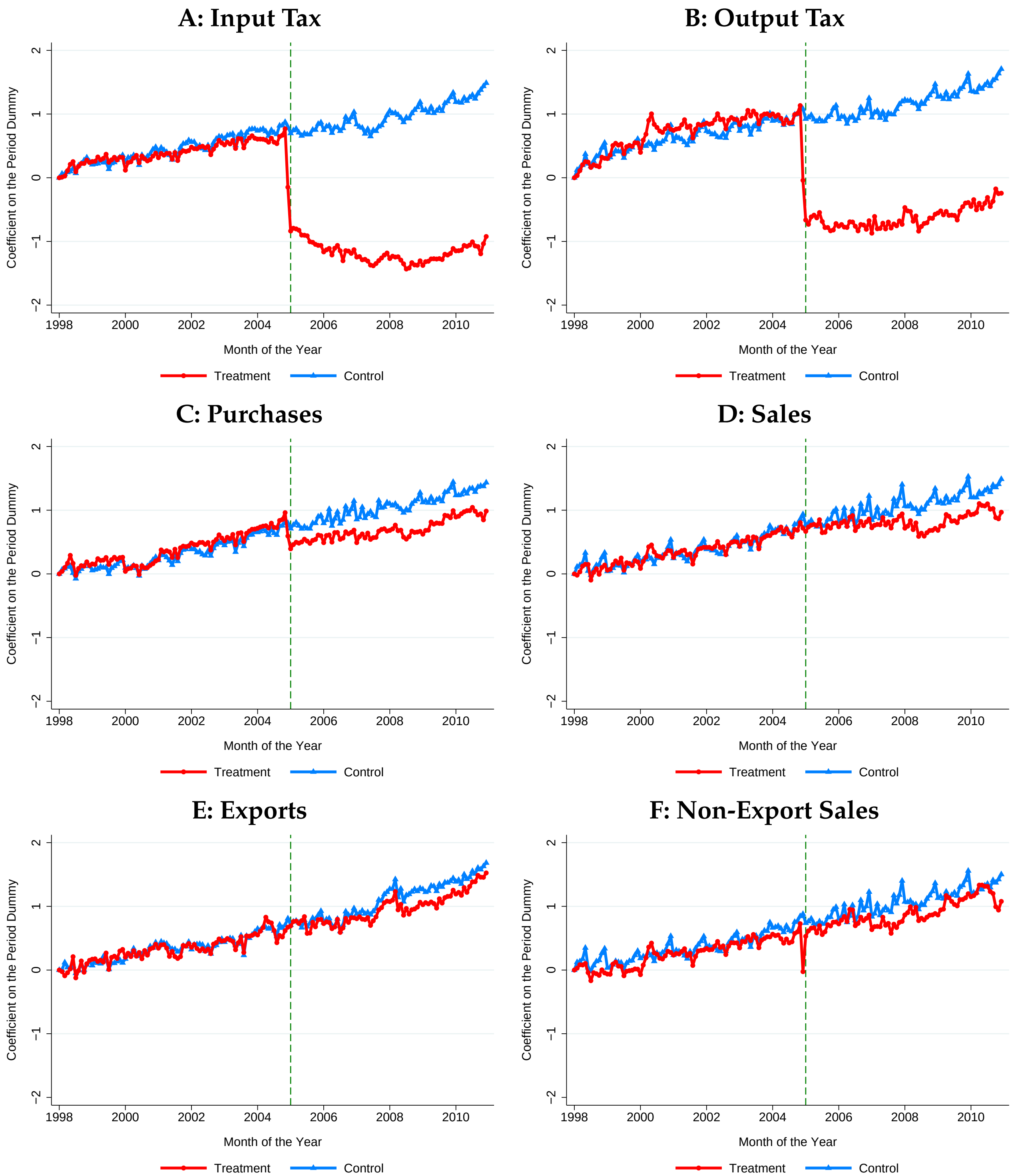
Notes: This figure displays three consecutive production stages of a supply chain. The top panel displays behavior under the first-best, where all firms report their sales and purchases truthfully. Total sales s_j of a firm consists of three components: (1) sales exported out of the country $s_{E,j}$; (2) sales made to other domestic firms $s_{I,j}$; and (3) sales made to final, domestic consumer $s_{F,j}$. Purchases here refer to the value of *taxable* intermediates, which here are all acquired from the previous production stage $c_j = s_{I,j-1}; \forall j \in 1, ..., J$. The VAT liability of a firm equals its output tax $\tau(s_j - s_{E,j})$ minus the input tax τc_j. Panel B displays the same supply chain, but firms are now allowed to misreport. Firms' sales and purchases in this second-best world are denoted by $\hat{s}_{K,j}; K \in \{E, I, F\}$ and $\hat{c}_j$ to distinguish them from their true values $s_{K,j}$ and c_j.

FIGURE II: **REFUND AS A PROPORTION OF GROSS COLLECTION**



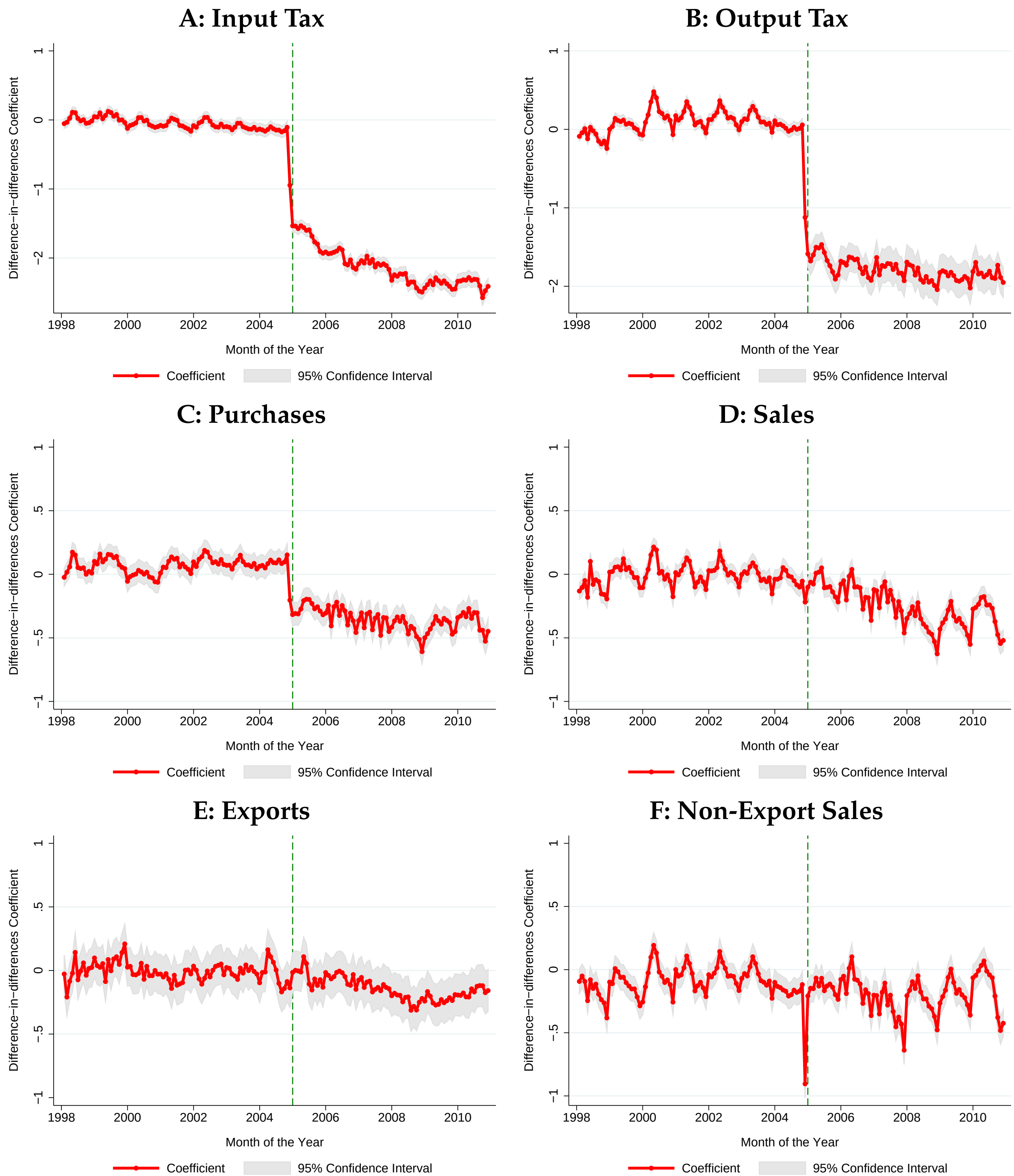
Notes: The figure shows the effects of the zero-rating reform on the VAT refund paid in Pakistan. Each marker in the plot denotes the aggregate VAT refund paid by the FBR to firms in *all* industries as a percentage of the gross VAT collection in that year. The data used for this plot are publicly available and have been compiled from the FBR yearbooks, containing annual tax collection statistics. These yearbooks are available here. The data are available from the tax year 1999 only. The year t in the horizontal axis denotes the month July of year t. The dashed, vertical line represents the time from which the zero-rating reform became applicable.

FIGURE III: **FIRM BEHAVIOR TO THE TAX CUT**



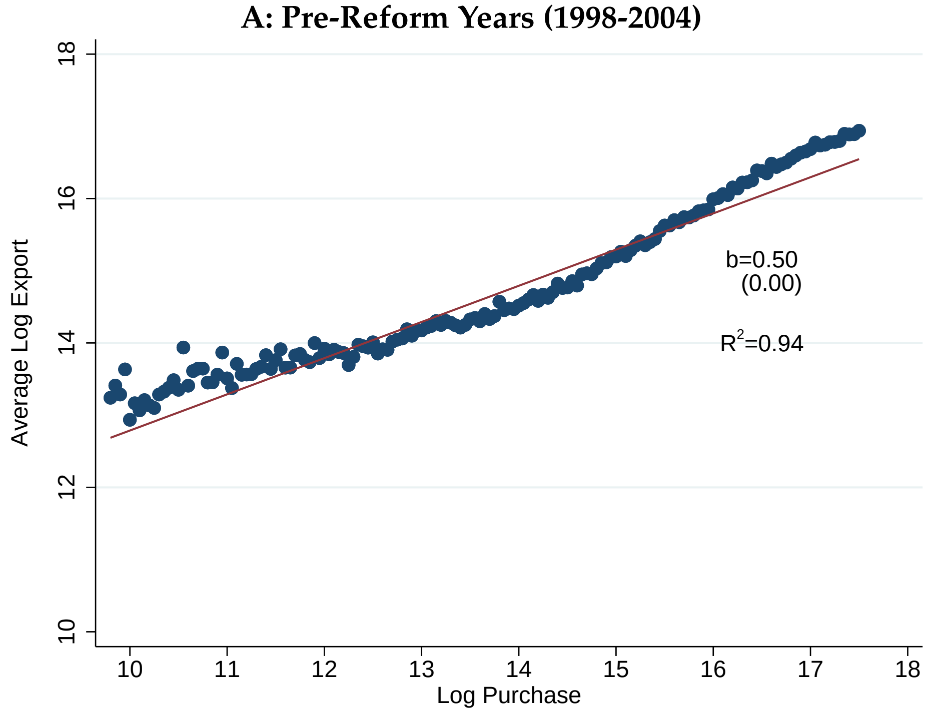
Notes: The figure compares the evolution of six VAT outcomes from the tax year 1998 to 2010 across the treatment and control groups . Treatment and control groups consist respectively of firms in the zero-rated and non-zero-rated industries. To construct these charts, I regress the log of the outcome variable shown in the title of each panel on the full set of firm and month fixed effects, dropping the dummy for July 1998. I then plot the coefficients on the time dummies of these regressions. The regressions are run separately for the two groups of firms. Year t on the horizontal axis indicates July of the corresponding year. Vertical dashed lines demarcate the time from which the zero-rating reform became applicable.

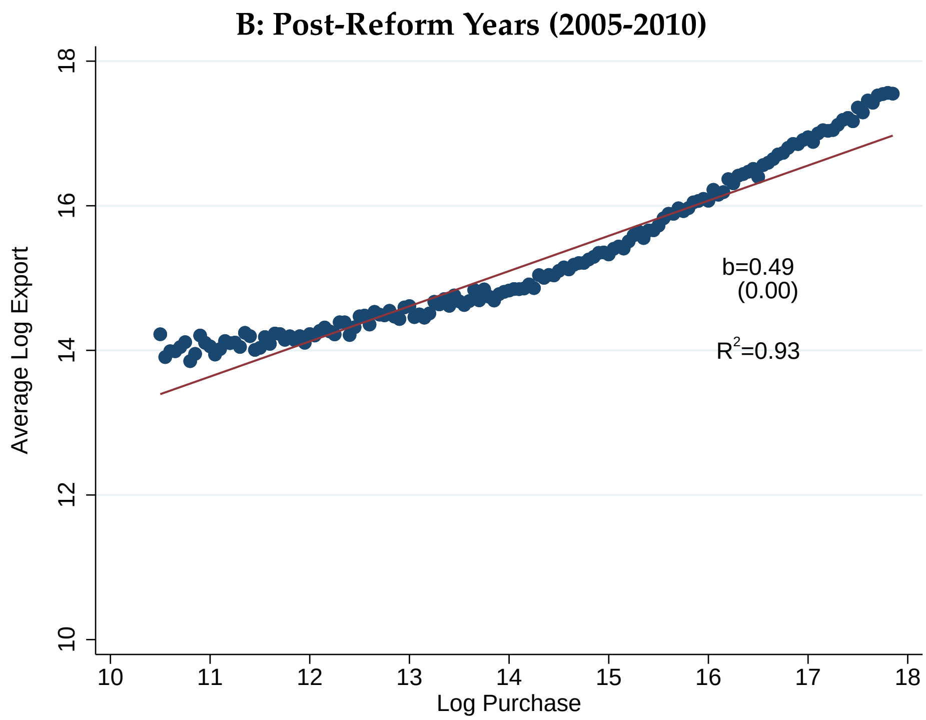
FIGURE IV: **FIRM BEHAVIOR TO THE TAX CUT**



Notes: The figure shows the difference-in-differences version of the plots in Figure III. To construct these charts, I regress the log of the outcome variable shown in the title of each panel on the full set of firm, month, and month×treat dummies, dropping the dummies for July 1998. I then plot the coefficients on the month×treat dummies from these regressions, where treat$_i$ denotes that firm i belongs to a zero-rated industry. The gray surface plot shows the 95% confidence interval around the coefficient. I cluster standard errors at the firm level. Year t on the horizontal axis indicates July of the corresponding year. The vertical, dashed lines demarcate the time from which the zero-rating reform became applicable.

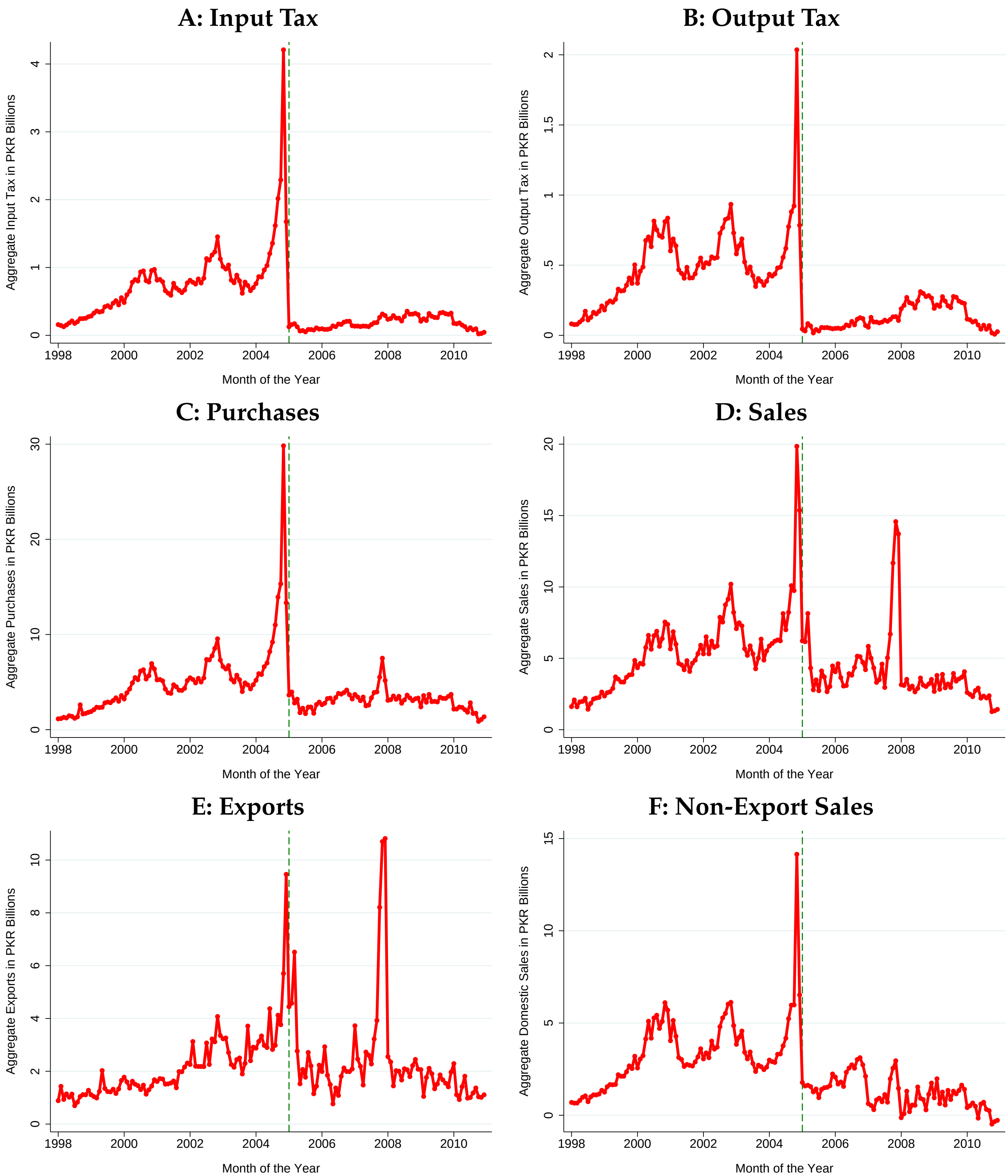
FIGURE V: **EXPORTS AS A FUNCTION OF PURCHASES**





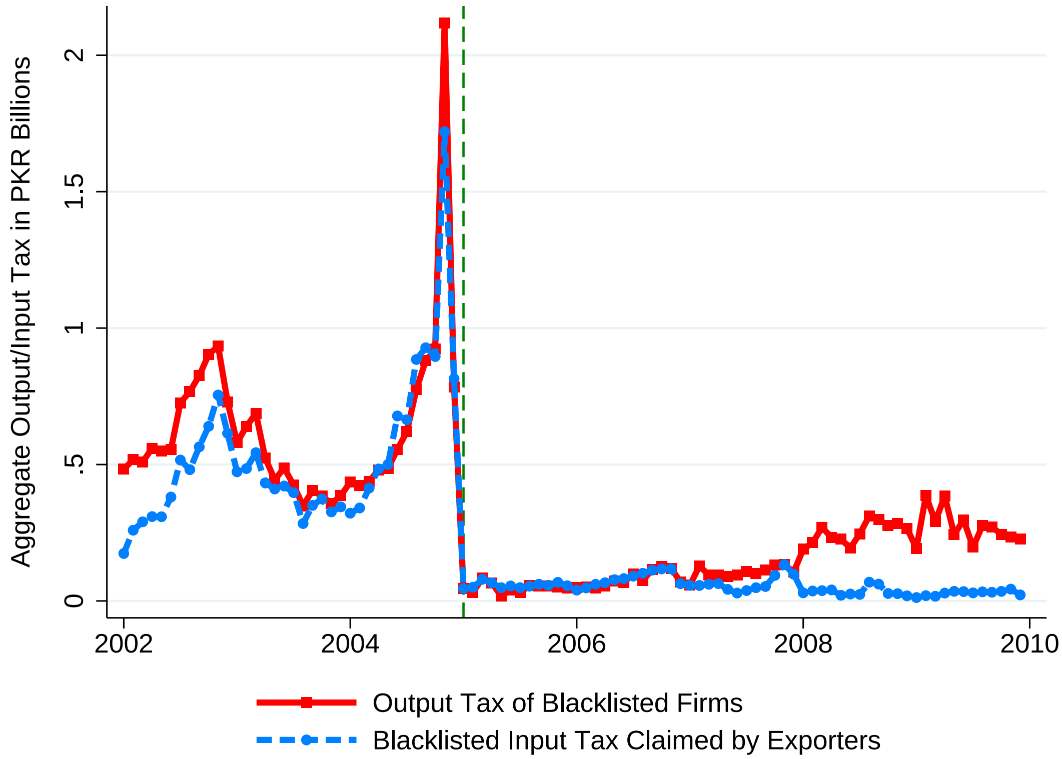
Notes: The figure explores the relationship between purchases of intermediates and exports reported by firms of the treated industries. The blue curve shows a binned scatter plot for the data. I group firms into bins on the basis of log of purchases reported by them. Each blue marker represents the average log export of firms within the bin. I restrict the sample to firms with log purchases within the fifth and 95th percentile of the aggregate log purchase distribution. The bin width is 0.05. The red curve is the straight line fitted onto the data using ordinary least squares. The slope of the fitted line and R^2 from the regression are reported in the panel. Panel A relates to the pre-reform years and Panel B to the post-reform years.

FIGURE VI: **AGGREGATE VALUES OF VAT OUTCOMES – INVOICE MILLS**



Notes: The figure illustrates how VAT outcomes reported by invoice mills evolve around the time of the zero-rating reform. The sample contains blacklisted and suspended firms operating in the zero-rated industries. Each panel of the figure shows the aggregate value of the outcome indicated in the title of the panel for the given month. Year t indicated in the horizontal axis denotes the month July of the corresponding year. To deal with outliers, I drop ten observations with the highest values of the given outcome in the entire sample. For example, for constructing Panel A, I sort all firm-month observations on the basis of Output Tax in a descending order and drop the top-ten observations. The dashed, vertical lines in the plots demarcate the time from which the zero-rated reform takes effect.

FIGURE VII: **INVOICE MILLS AND VAT REFUND**



Notes: The figure explores the linkages between invoice mills and exporters. The solid, red curve in the figure plots the aggregate value of output tax involved in VAT returns filed by all blacklisted and suspended firms in the given tax period (month). The dashed, blue curve, on the other hand, shows the aggregate value of input tax claimed by exporters on the invoices of blacklisted firm for the given tax period (month). This curve has been plotted using transaction-level data filed by exporters in support of their refund claims, which provides the supplier-wise details of all purchases of intermediates made by them in the corresponding tax period. That these two curves almost lie above each other shows that the primary purpose of the existence of invoice mills is to help exporters claim exaggerated refunds. Year t indicated in the horizontal axis denotes the month July of the year. Dashed vertical lines in the plots demarcate the time from which the zero-rated reform takes effect.

TABLE I: **FIRM BEHAVIOR TO THE TAX CUT**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Baseline Specification						
treat × after	-1.961	-1.842	-0.419	-0.223	-0.106	-0.082
	(0.026)	(0.071)	(0.017)	(0.012)	(0.025)	(0.014)
Elasticity	-12.065	-11.334	-2.578	-1.369	-0.651	-0.504
	(0.158)	(0.439)	(0.103)	(0.076)	(0.154)	(0.083)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
B: Placebo Specification						
treat × after	-0.004	0.001	0.052	0.019	-0.022	-0.014
	(0.012)	(0.011)	(0.012)	(0.010)	(0.026)	(0.011)
Elasticity	-0.026	0.005	0.318	0.117	-0.134	-0.083
	(0.073)	(0.069)	(0.076)	(0.062)	(0.162)	(0.067)
Observations	1,999,987	2,423,815	1,979,624	2,583,424	306,931	2,381,448
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores how firms respond to the reduction of the rate applicable to their supply chain to zero. The coefficient treat × after shows $\hat{\gamma}$ from model (9), where the dummy variable treat$_i$ denotes that firm i belongs to a zero-rated industry and the dummy variable after$_t$ that month t falls in the tax year 2005 and later. The coefficient $Elasticity$ shows $\hat{\gamma}$ from the same model (9) when I replace the double difference term with $\log(1-\tau_{it})$. Panel B shows results from parallel placebo regressions, where I restrict the sample to pre-reform years only, defining the period beginning from July 2002 as the $after$ period. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE II: **FIRM BEHAVIOR TO THE TAX CUT – DYNAMICS**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
treat × 2005	-1.568	-1.732	-0.335	-0.072	-0.019	-0.046
	(0.022)	(0.062)	(0.015)	(0.010)	(0.022)	(0.013)
treat × 2006	-1.891	-1.816	-0.391	-0.126	-0.035	-0.028
	(0.027)	(0.070)	(0.017)	(0.012)	(0.027)	(0.014)
treat × 2007	-1.984	-1.855	-0.440	-0.203	-0.103	-0.220
	(0.029)	(0.088)	(0.021)	(0.016)	(0.030)	(0.022)
treat × 2008	-2.240	-1.949	-0.498	-0.383	-0.216	-0.135
	(0.033)	(0.090)	(0.021)	(0.018)	(0.033)	(0.017)
treat × 2009	-2.283	-1.956	-0.475	-0.365	-0.212	-0.083
	(0.035)	(0.090)	(0.023)	(0.019)	(0.036)	(0.018)
treat × 2010	-2.270	-1.911	-0.432	-0.295	-0.148	-0.021
	(0.036)	(0.084)	(0.024)	(0.020)	(0.038)	(0.019)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table explores how firm response to the zero-rating reform evolves over time. I estimate an augmented version of the difference-in-differences model (9), including interactions of the treatment variable with all post-reform years. The dummy variable treat_i denotes that firm i belongs to a zero-rated industry. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE III: **FIRM BEHAVIOR TO THE TAX CUT – ACROSS THE SUPPLY CHAIN**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Response						
treat × after	-2.040	-0.468	-0.429	-0.266	-0.108	0.092
	(0.053)	(0.074)	(0.034)	(0.027)	(0.035)	(0.033)
treat × after × ginning	2.053	-2.184	0.389	0.305	0.193	-0.001
	(0.075)	(0.096)	(0.067)	(0.036)	(0.057)	(0.042)
treat × after × spinning	-0.504	-3.613	0.110	0.030	0.210	-0.351
	(0.074)	(0.207)	(0.045)	(0.035)	(0.071)	(0.042)
treat × after × weaving	0.018	-1.831	0.001	0.026	0.025	-0.204
	(0.061)	(0.147)	(0.039)	(0.031)	(0.044)	(0.037)
Baseline Coefficient	-1.978	-1.925	-0.394	-0.221	-0.065	-0.082
	(0.026)	(0.076)	(0.017)	(0.013)	(0.027)	(0.014)
Observations	3,685,909	4,158,258	3,934,914	4,987,477	563,822	4,597,087
B: Elasticities						
$\log(1-\tau)$	-12.550	-2.881	-2.641	-1.634	-0.666	0.568
	(0.325)	(0.454)	(0.207)	(0.166)	(0.213)	(0.201)
$\log(1-\tau)$ × ginning	12.633	-13.441	2.394	1.876	1.187	-0.003
	(0.461)	(0.593)	(0.411)	(0.221)	(0.351)	(0.259)
$\log(1-\tau)$ × spinning	-3.102	-22.230	0.675	0.186	1.294	-2.162
	(0.458)	(1.277)	(0.279)	(0.218)	(0.438)	(0.258)
$\log(1-\tau)$ × weaving	0.111	-11.266	0.004	0.158	0.156	-1.255
	(0.378)	(0.907)	(0.243)	(0.192)	(0.271)	(0.225)
Baseline Coefficient	-12.169	-11.844	-2.425	-1.358	-0.398	-0.504
	(0.162)	(0.465)	(0.103)	(0.079)	(0.165)	(0.084)
Observations	3,685,909	4,158,258	3,934,914	4,987,477	563,822	4,597,087
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table explores heterogeneity in firm-response to the zero-rating reform within the textile industry. I restrict the treatment sample to firms in the textile industry only and estimate a triple-difference version of model (9). The dummy variables $ginning_i$, $spinning_i$ and $weaving_i$ denote that the firm i belongs to the corresponding production stage within the textile industry. Baseline coefficient reports the $treat \times after$ coefficient I obtain from estimating the model without the triple-interaction terms. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE IV: **CALCULATING VAT EVASION**

	2004	2003	2002	2001	2000
	(1)	(2)	(3)	(4)	(5)
A: Underpaid VAT					
1. $\Sigma_j \, \tau.(s_j - \hat{s}_j)$	-47.6	-38.9	-31.6	-27.1	-27.9
	(3.4)	(2.8)	(2.3)	(2.0)	(2.0)
2. $\Sigma_j \, \tau.(s_{E,j} - \hat{s}_{E,j})$	-11.5	-7.6	-6.2	-5.2	-5.2
	(3.9)	(2.6)	(2.1)	(1.8)	(1.8)
3. $\Sigma_j \, \tau.(c_j - \hat{c}_j)$	-78.5	-61.9	-49.3	-40.8	-38.8
	(3.9)	(3.1)	(2.4)	(2.0)	(1.9)
4. $\Delta R = R - \hat{R}$	42.4	30.5	23.9	18.9	16.1
	(6.5)	(4.9)	(3.9)	(3.3)	(3.3)
5. $\Delta R / \Sigma_j \, \hat{s}_j$	0.033	0.029	0.028	0.026	0.022
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
B: Overclaimed Refund					
6. $\Sigma_j \, \tau.(s_{E,j} - \hat{s}_{E,j})$	-11.5	-7.6	-6.2	-5.2	-5.2
	(3.9)	(2.6)	(2.1)	(1.8)	(1.8)
7. $\Sigma_j \, \tau.(c_{E,j} - \hat{c}_{E,j})$	-23.0	-15.1	-12.3	-10.5	-10.4
	(15.7)	(10.3)	(8.4)	(7.1)	(7.1)
8. ΔR_E	23.0	15.1	12.3	10.5	10.4
	(15.7)	(10.3)	(8.4)	(7.1)	(7.1)
9. $\Delta R_E / \Sigma_j \hat{s}_{E,j}$	0.034	0.034	0.034	0.034	0.034
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
10. $\Delta R_E / \Sigma_j \tau.\hat{c}_{E,j}$	0.476	0.303	0.285	0.275	0.259
	(0.007)	(0.004)	(0.004)	(0.005)	(0.004)

Notes: The table uses formulas (3) and (7) to quantify VAT noncompliance implied by the estimates in Table I. Panel A computes total VAT evasion in the treated industries in the five baseline years using formula (3). Rows 1-3 compute the three terms on the RHS of the formula. To compute each term, I proceed in two steps. First, I convert the response of the corresponding item in Table I into percentage terms (for example, sales response of -0.223 implies a percent change of $-[exp(0.223) - 1] = -25\%$). I multiply the percent change with the aggregate value of the variable reported in the corresponding year to estimate the extent by which the variable was overreported in the year. Row 4 displays the sum of Rows 1-3, and Row 5 expresses it as a percent of total sales reported in the treated industries in that year. Panel B estimates the first term on the RHS of formula (7), illustrating the level of refund overclaimed in the treated industries. Row 6 reproduces the exports response computed in Row 2. Using the production function of exports estimated in Figure V, Row 7 converts the exports response into the extent by which purchases meant for export were overreported. Row 8 reports the PKR value of excess claimed refund (negative of Row7). Row 9 expresses the overclaimed amount as a percent of total exports and Row 10 as a percent of total refund claimed in the corresponding year. All amounts are in PKR billions. To deal with outliers, I exclude ten observations with the highest values of exports and non-export sales in the entire sample (1998-2010).

A Online Appendix

A.1 Definition of Variables

(*i*) **Input Tax.** The value of VAT credit claimed on purchases of intermediates made by a registered firm in a given tax period (month). It equals $\tau.\hat{c}_{it}$, where τ is the applicable VAT rate and $\hat{c}_{it}$ is the value of purchases of intermediates claimed by firm i in period t.

(*ii*) **Output Tax.** The value of VAT charged on non-export sales made by a registered firm in a given tax period (month). It equals $\tau.\left(\hat{s}_{it} - \hat{s}_{E,it}\right)$, where τ is the applicable VAT rate and $\left(\hat{s}_{it} - \hat{s}_{E,it}\right)$ is the value of non-export sales reported by firm i in period t.

(*iii*) **Purchases.** The value of all taxable intermediates acquired by a firm in a given tax period (month).

(*iv*) **Sales.** The value of all goods and services supplied by a firm in a given tax period (month).

(*v*) **Exports.** The value of all goods and services exported by a firm in a given tax period (month).

(*vi*) **Non-Export Sales.** The value of all goods and services supplied by a firm minus the value of all goods and services exported by a firm in a given tax period (month).

(*vii*) **Manufacturer.** A firm whose principal business activity is the manufacture of goods. Manufacturing is the process whereby a firm converts inputs into a distinct article capable of being put to use differently than inputs and includes any process incidental or ancillary to it.

(*viii*) **Wholesaler.** Wholesaler′ includes a dealer and means any person who carries on, whether regularly or otherwise, the business of buying and selling goods by wholesale or of supplying or distributing goods, directly or indirectly, by wholesale for cash or deferred payment or for commission or other valuable consideration or stores such goods belonging to others as an agent for the purpose of sale; and includes a person supplying taxable goods to a person who deducts income tax at source under the Income Tax Ordinance, 2001.

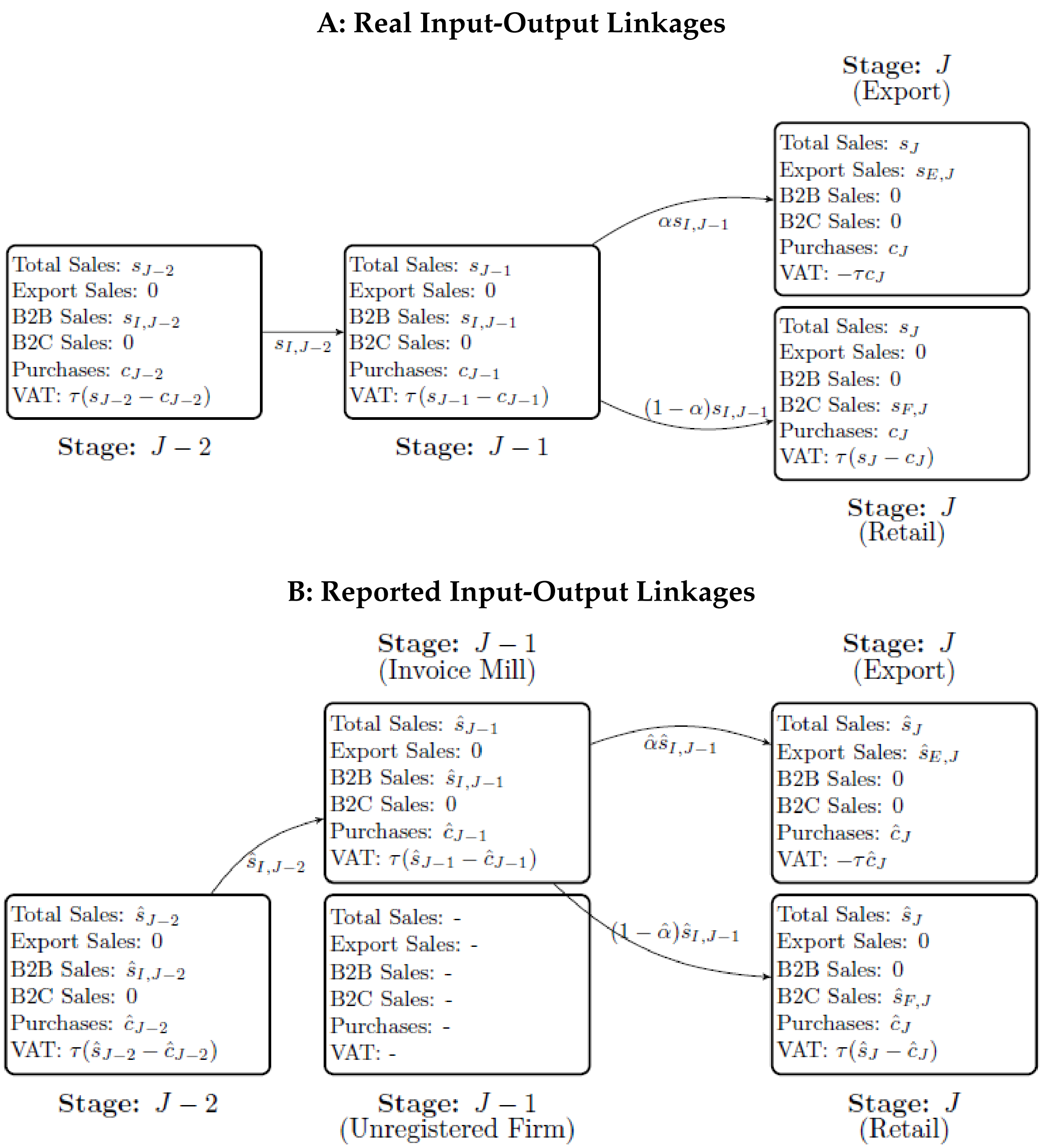
(*ix*) **Retailer.** A person, supplying goods to general public for the purpose of consumption.

(*x*) **Industry.** The Pakistani tax administration uses 4-digit Harmonized Commodity Description and Coding System (HS code) to classify firms into industry. The code, used by customs administrations throughout the world, divides all goods and services into 99 chapters (the first two digits in the code) and 21 sections. The sections broadly correspond to major industries in the country. I take the section a firm falls in as its industry. Table shows the sections, HS code, and description of these industries.

(*xi*) **Major City** A firm registered either in Karachi or Lahore, the two largest cities in Pakistan on the basis of both population and GDP.

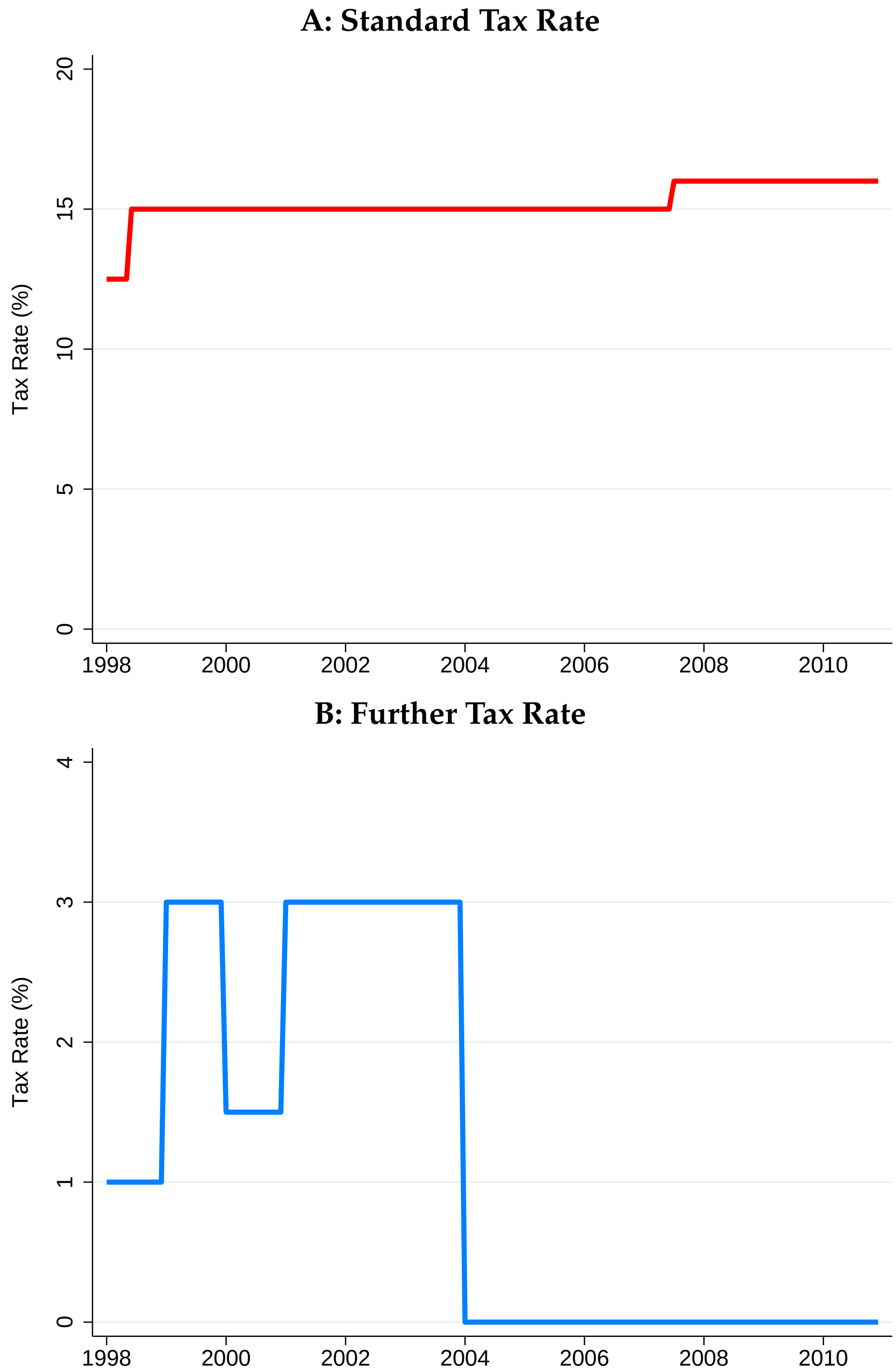
(*xii*) **Initial Capital.** The value of initial capital of the firm, as reported by it at the time of registration for VAT.

FIGURE A.I: **HOW INVOICE MILLS WORK**



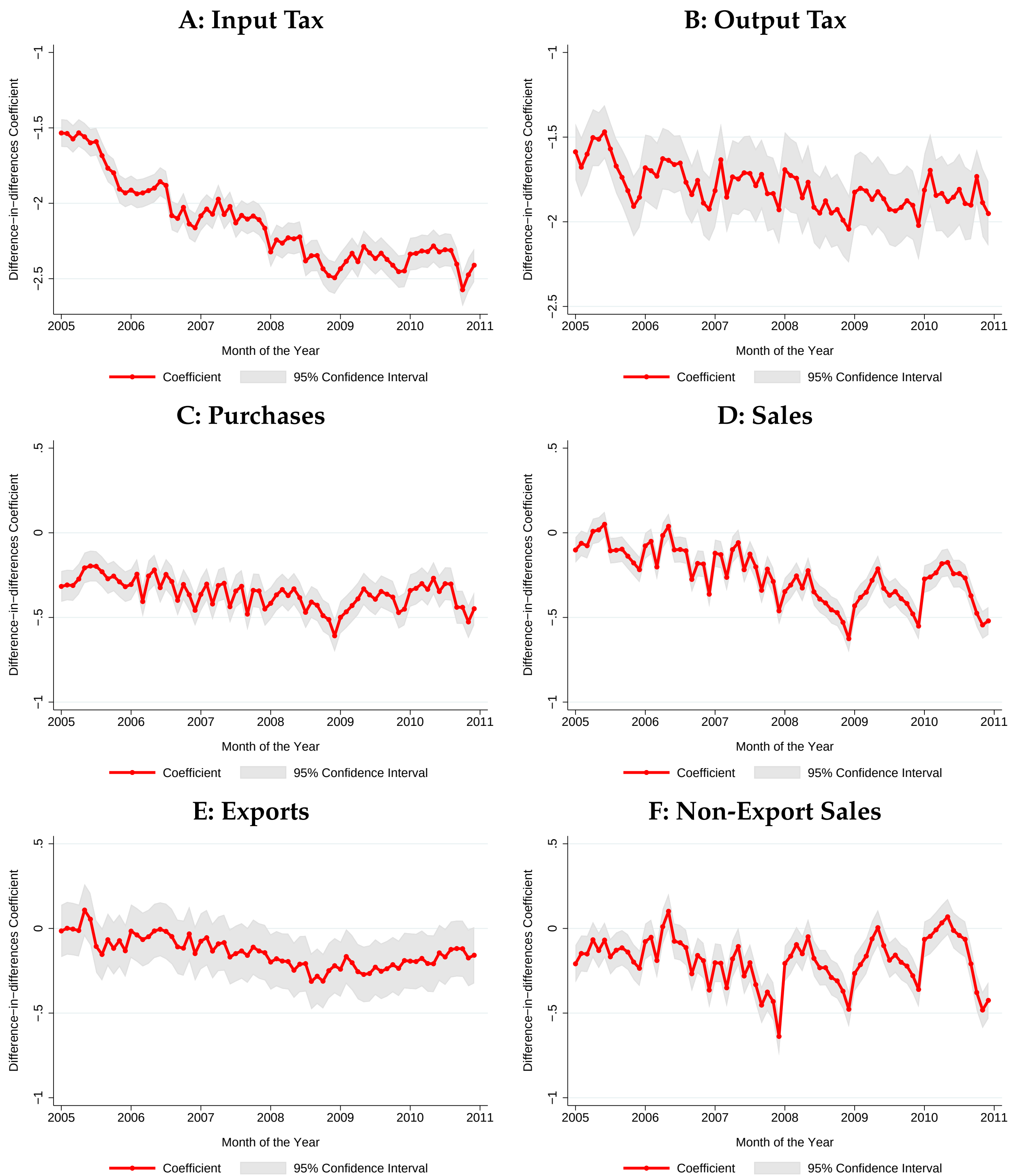
Notes: This figure shows how an invoice mills work. The top panel displays real input-output linkages in the last three stages of a supply chain. The final stage of the chain consists of two firms: the bottom firm deals exclusively in domestic sales and the top in exports. The middle firm sells a proportion α of its output to the exporter and the rest to the retailer. The middle firm is not in the VAT net. The two firms in the final stage thus cannot adjust input tax paid at the $J-2$ and previous stages. To exploit the gap, an invoice mills places itself in the stage $J-1$. It buys invoices from the bottom firm and sells them to the firms in the final stage. The bottom panel displays the reported input-output linkages in this setup. Given that the exporting firm has a greater incentive to exaggerate the value of its purchases, the invoice mills shifts a greater proportion of tax remitted at the bottom stages toward the exporting firms i.e. $\hat{\alpha} > \alpha$.

FIGURE A.II: **VAT RATES**



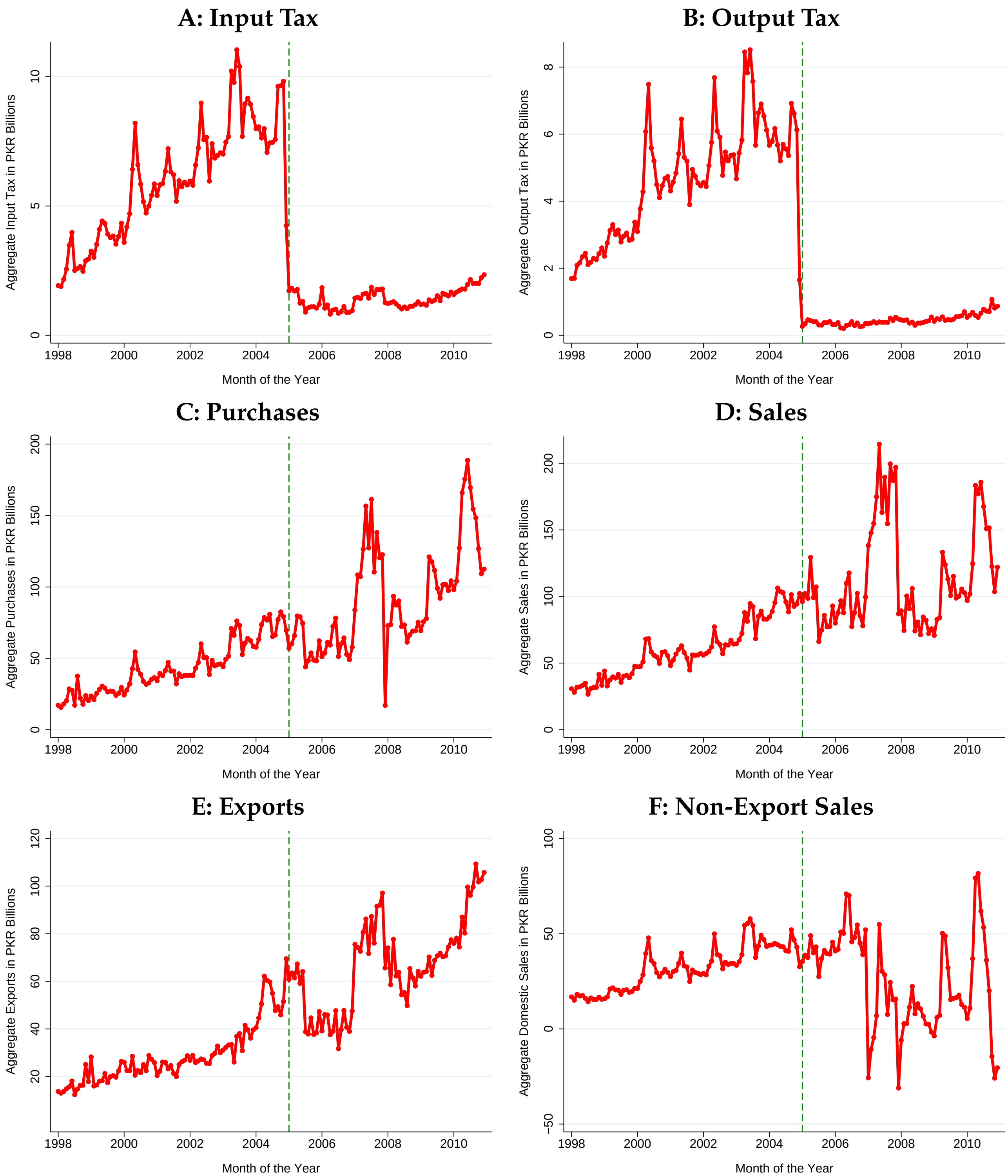
Notes: The figure shows the VAT rates applicable in Pakistan from July 1998 to June 2011. Panel A shows the standard VAT rate, which is applied to all sales made by a registered firm regardless of whether the recipient is registered or not. The rate largely stayed at 15%, but was increased to 16% from July 2008. Panel B reports the Further Tax Rate. This rate is added to the standard rate whenever the sale is made to an unregistered firm. For example, supplies made by a registered firm in July 1998 were subject to a rate of 15% if the recipient was a registered firm or an end-consumer and 16% if the recipient was an unregistered firm. To claim that a sale has been made to end-consumer, the selling firm must be registered as a retailer.

FIGURE A.III: **FIRM BEHAVIOR TO THE TAX CUT**



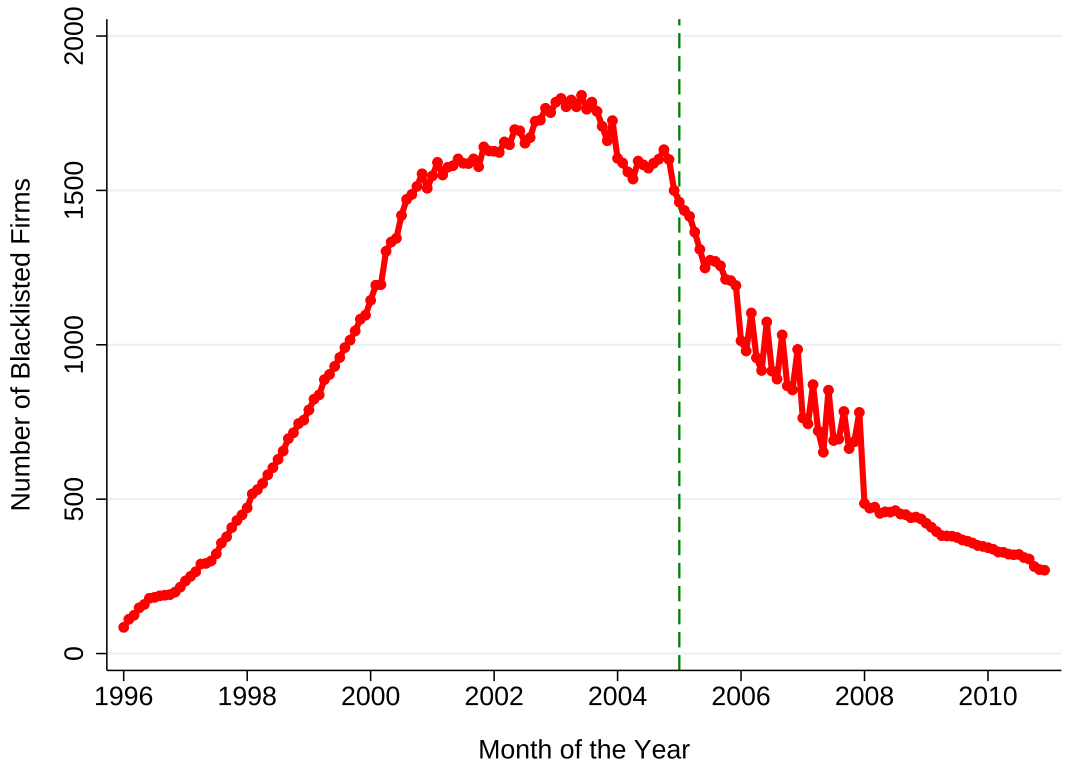
Notes: The figure reproduces the event study results reported in Figure III. The only difference between the two figure is that I show only the post-reform periods here. To construct these charts, I regress the log of the outcome variable shown in the title of each panel on the full set of firm, month, and month$\times$treat dummies, dropping the dummies for July 1998. I then plot the coefficients on the month$\times$treat dummies from these regressions, where treat$_i$ denotes that firm i belongs to a zero-rated industry. The gray surface plot shows the 95% confidence interval around the coefficient. I cluster standard errors at the firm level. Year t on the horizontal axis indicates July of the corresponding year.

FIGURE A.IV: AGGREGATE VALUES OF VAT OUTCOMES – EXCLUDING INVOICE MILLS



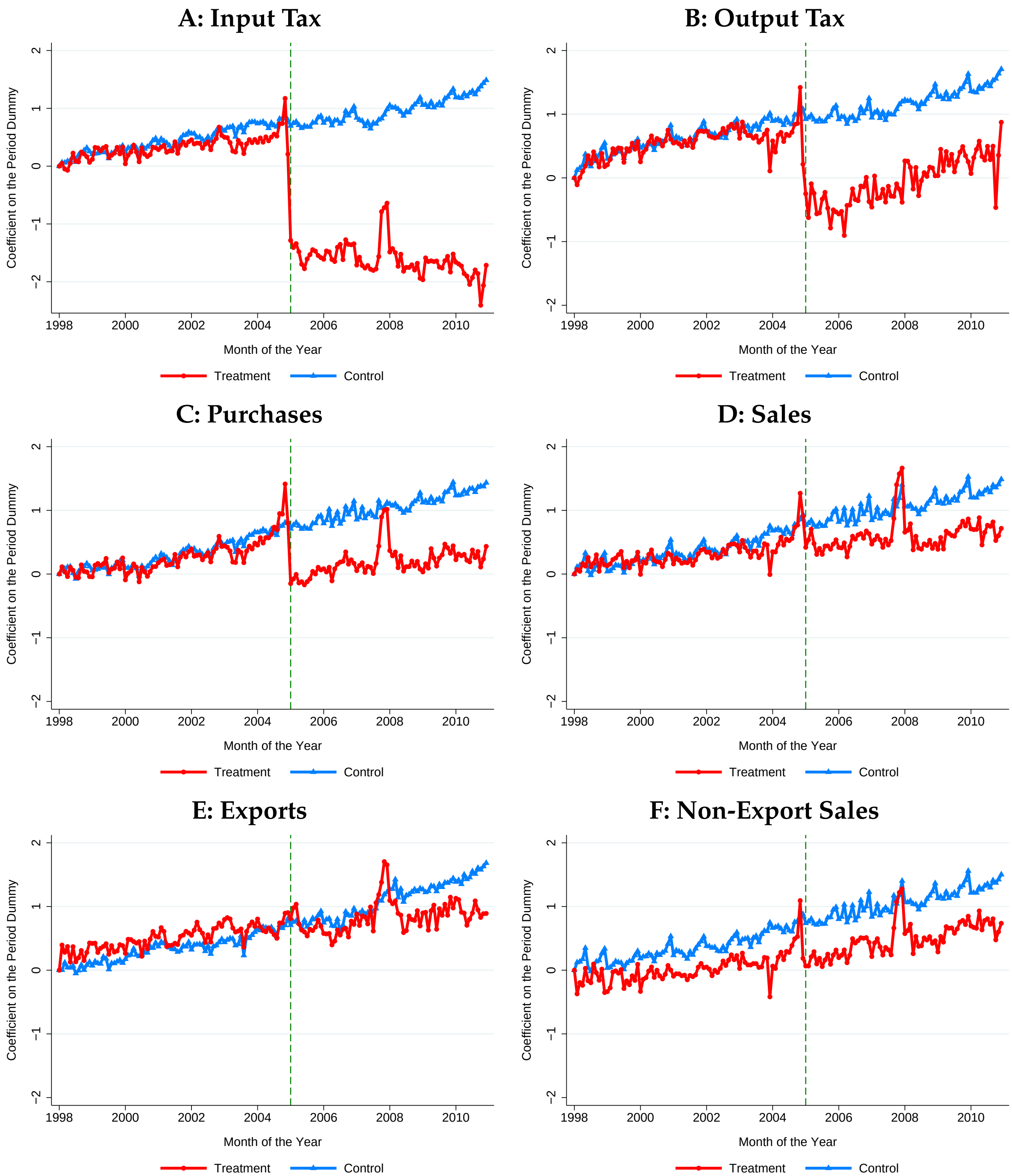
Notes: The figure illustrates how VAT outcomes evolve around the time of the zero-rating reform. The sample contains all firms operating in the zero-rated industries other than the blacklisted and suspended firms. Each panel of the figure shows the aggregate value of the outcome indicated in the title of the panel for the given month. Year t indicated in the horizontal axis denotes the month July of the corresponding year. To deal with outliers, I drop ten observations with the highest values of the given outcome in the entire sample. For example, for constructing Panel A, I sort all firm-month observations on the basis of Output Tax in a descending order and drop the top-ten observations. The dashed, vertical lines in the plots demarcate the time from which the zero-rated reform takes effect.

FIGURE A.V: **EMERGENCE, GROWTH, AND DECLINE OF INVOICE MILLS**



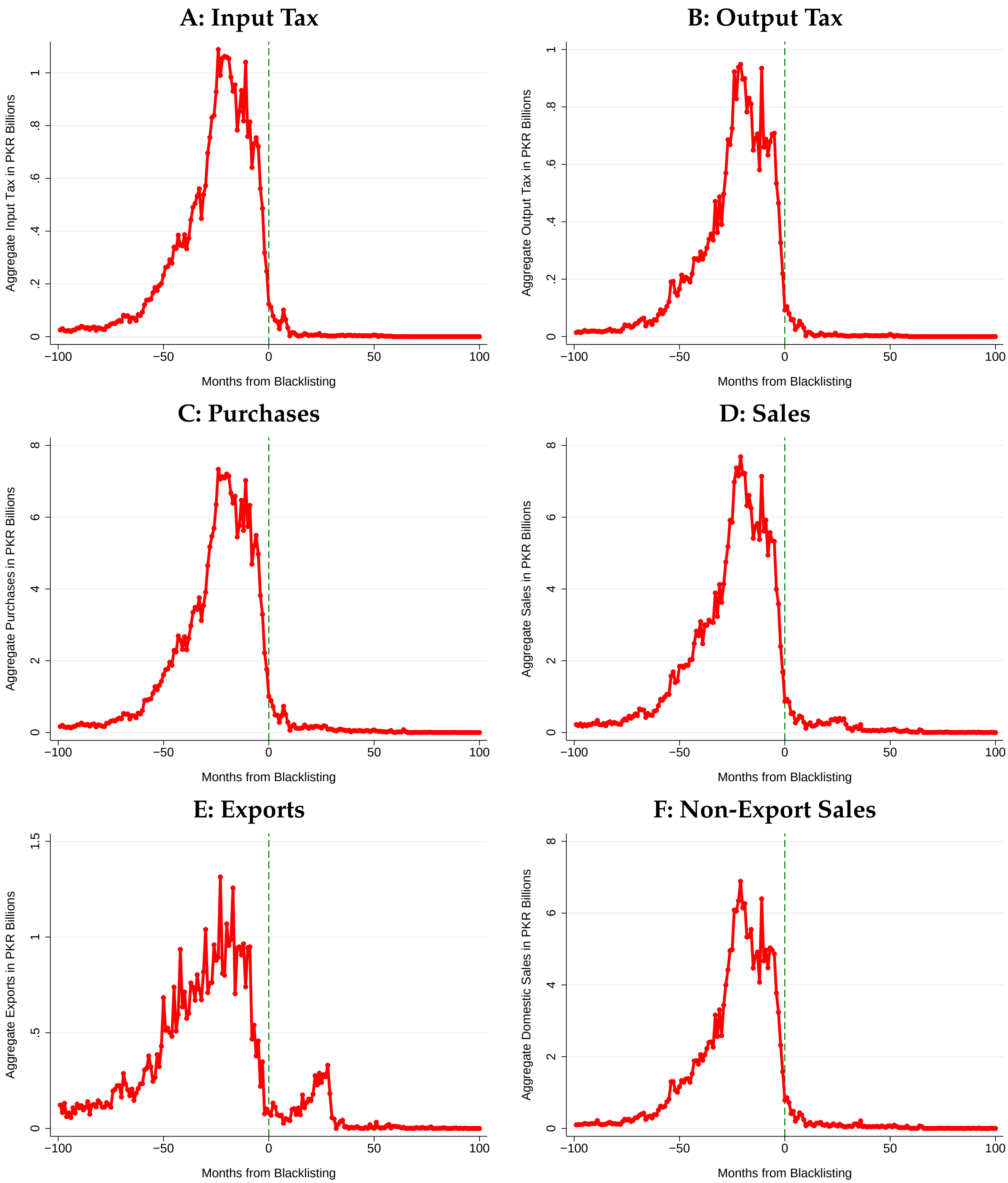
Notes: The figure shows the emergence, growth, and subsequent decline of invoice mills in the Pakistani setting. The sample begins from 1996, when a *broad-based* VAT with coverage extended to almost entire manufacturing and exports stages begun in the country. Each marker in the curve denotes the number of blacklisted firms that file a return in the given month. The year t in the horizontal axis denotes the month July of year t. The dashed, vertical line represents the time from which the zero-rating reform became applicable.

FIGURE A.VI: **FIRM BEHAVIOR TO THE TAX CUT – INVOICE MILLS**



Notes: The figure compares the evolution of six VAT outcomes from the tax year 1998 to 2010 across the treatment and control groups . The treatment group here consist of blacklisted and suspended firms in the zero-rated industries only. The control group, as earlier, comprises all firms of the non-zero-rated industries. To construct these charts, I regress the log of the outcome variable shown in the title of each panel on the full set of firm and month fixed effects, dropping the dummy for July 1998. I then plot the coefficients on the time dummies of these regressions. The regressions are run separately for the two groups of firms. Year t on the horizontal axis indicates July of the year. The vertical, dashed lines demarcate the time from which the zero-rating reform became applicable.

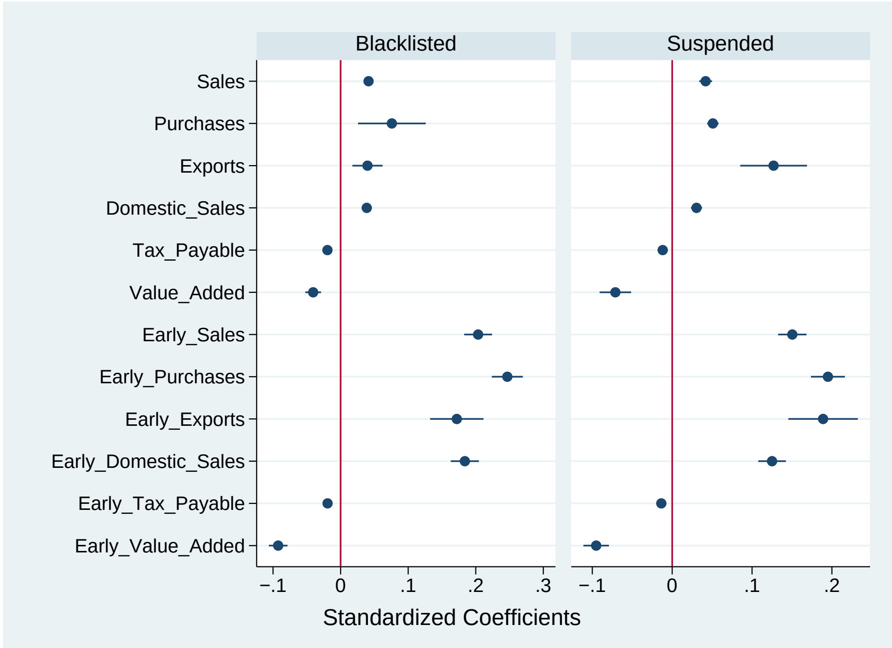
FIGURE A.VII: **AGGREGATE VALUES OF VAT OUTCOMES – INVOICE MILLS**



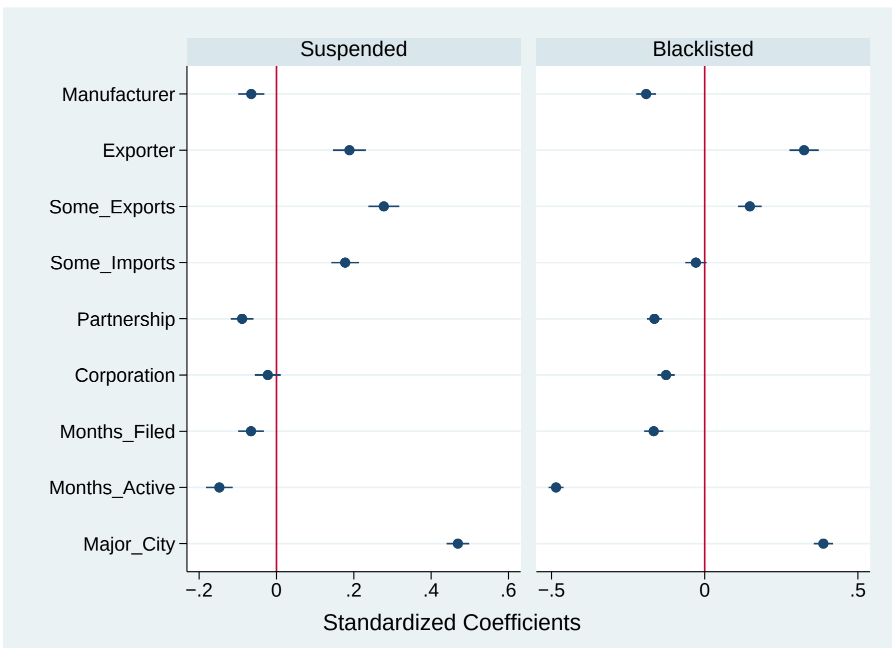
Notes: The figure explores the change in activity carried out by blacklisted firms around the event of blacklisting. The sample contains all blacklisted and suspended firms of the treated industries. Time 0 in the horizontal axis (marked by the vertical, dashed line) denotes the month in which the firm was declared blacklist or its registration was suspended. Each panel of the figure shows the aggregate value of the outcome in 100 months prior to and 100 months after the event of blacklisting. To deal with outliers, I exclude ten observations with the highest values of the given outcome in the entire sample. For example, for constructing Panel A, I sort all firm-month observations on the basis of Output Tax in a descending order and exclude the top-ten observations.

FIGURE A.VIII: **DISTINGUISHING MARKERS OF INVOICE MILLS**

A: VAT Outcomes



B: Firm Characteristics



Notes: The figures explores if invoice mills have any distinguishing markers that could help identify them. I compare the VAT outcomes reported by (Panel A) and and firm characteristics of (Panel B) blacklisted and suspended firms with other firms. The sample here consists of pre-reform years only (1998-2004). To construct these plots, I standardize each variable by subtracting the mean and dividing by the standard deviation of each variable. I then regress the standardized variable on two dummies, indicating if the firm is a blacklisted or a suspended firm. The coefficients on these dummy variables along with the standard errors are plotted with the heading of each row indicating the LHS variable in the regression. Early here denotes the first six months after the firm's registration For example Early_sales denotes the average sales reported by the firm in its first six returns after registration. The definitions of the compared variables are provided in Appendix A.1.

TABLE A.I: **SUMMARY STATISTICS**

	2003		2004	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
1. # Observations	172,321	743,281	163,327	709,560
2. Input Tax	0.686	0.431	0.707	0.639
	(0.014)	(0.017)	(0.013)	(0.031)
3. Output Tax	0.514	0.602	0.479	0.850
	(0.013)	(0.028)	(0.012)	(0.045)
4. Purchases	4.600	2.678	6.158	4.223
	(0.084)	(0.096)	(0.116)	(0.175)
5. Sales	6.919	3.782	7.776	5.623
	(0.895)	(0.122)	(0.116)	(0.202)
6. Exports	2.614	0.311	4.207	0.503
	(0.054)	(0.010)	(0.065)	(0.016)
7. Domestic Sales	4.306	3.471	3.569	5.120
	(0.893)	(0.118)	(0.082)	(0.194)
8. Major City	0.486	0.563	0.507	0.576
	(0.001)	(0.001)	(0.001)	(0.001)
9. # Years Registered	3.695	3.592	3.238	3.108
	(0.009)	(0.004)	(0.010)	(0.004)
10. # Years Active	8.083	8.608	8.401	8.883
	(0.009)	(0.004)	(0.008)	(0.004)
11. Some Export	0.472	0.135	0.545	0.152
	(0.001)	(0.000)	(0.001)	(0.000)
12. Some Import	0.416	0.369	0.476	0.415
	(0.001)	(0.001)	(0.001)	(0.001)

Notes*:* The table presents summary statistics for the treatment and control groups. Treatment group comprises firms whose supplies were zero-rated by the zero-rating reform from 2005. The control group comprises all other firms. The first row of the table compares the number of firm-month observations for the two groups in the two prereform years. Subsequent rows compare the mean of eleven VAT outcomes and firm characteristics across the two groups. Major City denotes that the firm is registered in Karachi or Lahore, the two major cities of Pakistan. The variable # Years Registered reports the number of years up to 2003 since the firm's registration; # Years Active reports the number of years the firm remained active, filing its VAT return. Standard errors of the mean are in parenthesis.

TABLE A.II: **FIRM BEHAVIOR TO THE TAX CUT – TEXTILE VS. OTHERS**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Response						
treat $\times$ after	-1.978	-1.925	-0.394	-0.221	-0.065	-0.082
	(0.026)	(0.076)	(0.017)	(0.013)	(0.027)	(0.014)
treat $\times$ after $\times$ non-textile	0.260	0.958	-0.403	-0.023	-0.224	-0.001
	(0.102)	(0.196)	(0.078)	(0.040)	(0.042)	(0.081)
Baseline Coefficient	-1.961	-1.842	-0.419	-0.223	-0.106	-0.082
	(0.026)	(0.071)	(0.017)	(0.012)	(0.025)	(0.014)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
B: Elasticity						
log $(1-\tau)$	-12.169	-11.844	-2.425	-1.358	-0.401	-0.503
	(0.162)	(0.465)	(0.103)	(0.079)	(0.164)	(0.084)
log $(1-\tau) \times$ non-textile	1.602	5.893	-2.479	-0.141	-1.377	-0.009
	(0.628)	(1.208)	(0.481)	(0.247)	(0.258)	(0.499)
Baseline Coefficient	-12.065	-11.334	-2.578	-1.369	-0.651	-0.504
	(0.158)	(0.439)	(0.103)	(0.076)	(0.154)	(0.083)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table decomposes the average response to the zero-rating reform estimated in Table I into its constituent textile and non-textile components. I estimate a triple-differences version of model (9), including all double interactions of the binary variables treat$_i$, Non-textile$_i$, and after$_t$ and their triple-interaction. The dummy variable treat$_i$ denotes that firm i belongs to a zero-rated industry; the dummy variable Non-textile$_i$ denotes that the treated firm i does not belong to the textile industry; and the dummy variable after$_t$ denotes that month t falls in the tax year 2005 and later. Baseline coefficient reports the $treat \times after$ or log $(1-\tau)$ coefficient I obtain from estimating the baseline model (9). Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.III: **FIRM BEHAVIOR TO THE TAX CUT – BALANCED PANEL**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Baseline Specification						
treat × after	-2.337	-2.536	-0.484	-0.405	-0.158	-0.101
	(0.043)	(0.139)	(0.031)	(0.024)	(0.037)	(0.029)
Elasticity	-14.382	-15.606	-2.977	-2.494	-0.973	-0.620
	(0.267)	(0.857)	(0.193)	(0.150)	(0.227)	(0.180)
Observations	948,385	877,354	981,954	1,126,539	264,719	960,697
B: Placebo Specification						
treat × after	-0.076	-0.009	-0.008	0.008	-0.031	-0.051
	(0.021)	(0.024)	(0.021)	(0.019)	(0.034)	(0.024)
Elasticity	-0.468	-0.055	-0.048	0.047	-0.189	-0.314
	(0.128)	(0.146)	(0.131)	(0.118)	(0.212)	(0.147)
Observations	560,180	562,860	564,044	646,637	142,345	564,505
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table explores how firms respond to the reduction of the rate applicable to their supply chain to zero. The sample here is restricted to a balanced panel of firms that file at least once in every quarter included in the sample. The coefficient treat × after shows $\hat{\gamma}$ from model (9), where the dummy variable treat$_i$ denotes that firm i belongs to a zero-rated industry and the dummy variable after$_t$ denotes that month t falls in the tax year 2005 and later. The coefficient $Elasticity$ shows $\hat{\gamma}$ from the same model (9) when I replace the double difference term with $\log(1-\tau)$. Panel B shows results from parallel placebo regressions, where I restrict the sample to pre-reform years only, defining the period beginning from July 2002 as the $after$ period. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.IV: **SPILLOVER EFFECTS ON THE NON-TREATED INDUSTRIES**

Industries Within:	Next Two Digits			Next Five Digits			Next Ten Digits		
	Purchases	Sales	Exports	Purchases	Sales	Exports	Purchases	Sales	Exports
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: Complete Panel									
treat $\times$ after	-0.234	0.002	0.067	-0.059	0.038	0.012	-0.066	0.012	0.044
	(0.121)	(0.083)	(0.172)	(0.082)	(0.062)	(0.128)	(0.022)	(0.019)	(0.077)
Observations	3,123,769	4,075,853	312,320	3,123,769	4,075,853	312,320	3,123,769	4,075,853	312,320
B: Balanced Panel									
treat $\times$ after	-0.152	-0.018	0.430	0.048	0.041	0.187	-0.100	0.014	0.072
	(0.168)	(0.130)	(0.319)	(0.121)	(0.108)	(0.228)	(0.037)	(0.031)	(0.119)
Observations	764,271	892,064	133,421	764,271	892,064	133,421	764,271	892,064	133,421
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table explores if the reform produces any spillovers on the nontreated industries. I drop all industries zero-rated by the reform from the sample. I then estimate the difference-in-differences model (9). The dummy variable treat$_i$ here denotes that firm i belongs to an industry indicated in the title of each column. For example, the first three columns regard the two 2-digit industries immediately succeeding the zero-rated ones as treated. Panel B restricts the sample to a balance panel, including only the firms who file at least once in every quarter included in the sample. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.V: **SPILLOVER EFFECTS ON THE NON-TREATED INDUSTRIES – ALTERNATIVE SPECIFICATION**

Industries Within:	Next Two Digits			Next Five Digits			Next Ten Digits		
	Purchases	Sales	Exports	Purchases	Sales	Exports	Purchases	Sales	Exports
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: Complete Panel									
treat × after	-0.420	-0.222	-0.105	-0.420	-0.222	-0.105	-0.426	-0.222	-0.103
	(0.017)	(0.012)	(0.025)	(0.017)	(0.012)	(0.025)	(0.017)	(0.012)	(0.025)
adjoining × after	-0.242	0.015	0.068	-0.065	0.044	0.015	-0.067	0.012	0.046
	(0.119)	(0.082)	(0.165)	(0.082)	(0.062)	(0.125)	(0.022)	(0.019)	(0.075)
Observations	3,983,213	5,058,579	612,993	3,983,213	5,058,579	612,993	3,983,213	5,058,579	612,993
B: Balanced Panel									
treat × after	-0.485	-0.405	-0.154	-0.483	-0.405	-0.155	-0.496	-0.404	-0.155
	(0.031)	(0.024)	(0.037)	(0.031)	(0.024)	(0.037)	(0.032)	(0.025)	(0.037)
adjoining × after	-0.154	-0.020	0.428	0.047	0.039	0.186	-0.100	0.012	0.070
	(0.168)	(0.131)	(0.318)	(0.121)	(0.108)	(0.226)	(0.037)	(0.031)	(0.119)
Observations	981,954	1,126,539	264,719	981,954	1,126,539	264,719	981,954	1,126,539	264,719
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores if the reform produces any spillovers on the nontreated industries. I estimate an augmented version of the difference-in-differences model (9), including the interaction term adjoining × after into the model. The dummy variable adjoining_i here denotes that firm i belongs to an industry indicated in the title of each column. For example, the first three columns regard the two 2-digit industries immediately succeeding the zero-rated ones as *adjoining*. Panel B restricts the sample to a balance panel, including only the firms who file at least once in every quarter included in the sample. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.VI: **FIRM BEHAVIOR TO THE TAX CUT – WITHOUT BLACKLISTED FIRMS**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Complete Panel						
treat × after	-1.953	-1.871	-0.406	-0.221	-0.098	-0.079
	(0.026)	(0.074)	(0.017)	(0.013)	(0.026)	(0.014)
Elasticity	-12.016	-11.515	-2.499	-1.357	-0.603	-0.487
	(0.161)	(0.453)	(0.105)	(0.077)	(0.158)	(0.084)
Observations	3,562,585	4,020,573	3,808,066	4,865,461	584,747	4,451,965
B: Balanced Panel						
treat × after	-2.316	-2.530	-0.469	-0.402	-0.152	-0.101
	(0.044)	(0.141)	(0.032)	(0.025)	(0.038)	(0.029)
Elasticity	-14.250	-15.569	-2.886	-2.473	-0.938	-0.622
	(0.270)	(0.868)	(0.196)	(0.152)	(0.231)	(0.181)
Observations	923,512	857,195	955,603	1,098,880	256,765	938,355
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores how firms respond to the reduction of the rate applicable to their supply chain to zero. The sample here excludes blacklisted and suspended firms. The coefficient treat × after shows $\hat{\gamma}$ from model (9), where the dummy variable treat$_i$ denotes that firm i belongs to a zero-rated industry and the dummy variable after$_t$ that month t falls in the tax year 2005 and later. The coefficient $Elasticity$ shows $\hat{\gamma}$ from the same model (9) when I replace the double difference term with $\log{(1-\tau_{it})}$. Panel B shows results from parallel placebo regressions, where I restrict the sample to pre-reform years only, defining the period beginning from July 2002 as the $after$ period. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.VII: **FIRM BEHAVIOR TO THE TAX CUT – FROM DATE OF ANNOUNCEMENT**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Complete Panel						
treat × after	-1.944	-1.793	-0.422	-0.227	-0.114	-0.113
	(0.025)	(0.064)	(0.017)	(0.012)	(0.026)	(0.013)
Elasticity	-11.964	-11.034	-2.594	-1.395	-0.699	-0.697
	(0.156)	(0.395)	(0.102)	(0.075)	(0.157)	(0.083)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
B: Balanced Panel						
treat × after	-2.325	-2.476	-0.485	-0.404	-0.156	-0.121
	(0.043)	(0.131)	(0.031)	(0.024)	(0.037)	(0.029)
Elasticity	-14.304	-15.234	-2.984	-2.485	-0.963	-0.744
	(0.264)	(0.806)	(0.192)	(0.149)	(0.228)	(0.179)
Observations	948,385	877,354	981,954	1,126,539	264,719	960,697
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table explores how firms respond to the reduction of the rate applicable to their supply chain to zero. The only difference from the baseline specification (results reported in Table I) is that the $after$ period here begins from the date of announcement of the reform i.e. from June 2005. The coefficient treat × after shows $\hat{\gamma}$ from model (9), where the dummy variable treat$_i$ denotes that firm i belongs to a zero-rated industry and the dummy variable after$_t$ that month t is June 2005 or a later period. The coefficient $Elasticity$ shows $\hat{\gamma}$ from the same model (9) when I replace the double difference term with $\log(1-\tau_{it})$. Panel B shows results from parallel regressions, where I restrict the sample to a balanced panel of firms that report in every quarter included in the sample. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.VIII: **FIRM BEHAVIOR TO THE TAX CUT – CORPORATIONS VS. OTHERS**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Response						
treat $\times$ after	-1.745	-1.135	-0.407	-0.148	-0.099	-0.056
	(0.030)	(0.059)	(0.018)	(0.013)	(0.028)	(0.014)
treat $\times$ after $\times$ corporation	-0.599	-1.973	-0.040	-0.271	-0.017	-0.099
	(0.050)	(0.154)	(0.037)	(0.027)	(0.037)	(0.033)
Baseline Coefficient	-1.961	-1.842	-0.419	-0.223	-0.106	-0.082
	(0.026)	(0.071)	(0.017)	(0.012)	(0.025)	(0.014)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
A: Response						
$\log(1-\tau)$	-10.740	-6.981	-2.505	-0.910	-0.606	-0.347
	(0.187)	(0.365)	(0.111)	(0.081)	(0.173)	(0.086)
$\log(1-\tau) \times$ corporation	-3.683	-12.141	-0.248	-1.670	-0.106	-0.611
	(0.311)	(0.949)	(0.228)	(0.165)	(0.228)	(0.202)
Baseline Coefficient	-12.065	-11.334	-2.578	-1.369	-0.651	-0.504
	(0.158)	(0.439)	(0.103)	(0.076)	(0.154)	(0.083)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table rules out lazy reporting as an alternative explanation of the responses documented in Table I. I divide the treatment sample into corporate and non-corporate firms. I then estimate a triple-differences version of model (9), including all double interactions of the binary variables treat$_i$, corporation$_i$, and after$_t$ and their triple-interaction. Baseline coefficient reports the $treat \times after$ or $\log(1-\tau_{it})$ coefficient I obtain from estimating the baseline model (9). Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.IX: **FIRM BEHAVIOR TO THE TAX CUT – BLACKLISTED VS. OTHERS**

	Input Tax	Output Tax	Purchases	Sales	Exports	Non-Export Sales
	(1)	(2)	(3)	(4)	(5)	(6)
A: Response						
treat × after	-1.948	-1.870	-0.401	-0.218	-0.095	-0.077
	(0.026)	(0.074)	(0.017)	(0.013)	(0.026)	(0.014)
treat × after × blacklisted	0.208	0.697	-0.475	-0.306	-0.312	-0.445
	(0.210)	(0.292)	(0.125)	(0.097)	(0.229)	(0.103)
treat × after × suspended	-0.435	0.694	-0.374	-0.069	-0.218	-0.043
	(0.142)	(0.334)	(0.090)	(0.069)	(0.066)	(0.092)
Baseline Coefficient	-1.961	-1.842	-0.419	-0.223	-0.106	-0.082
	(0.026)	(0.071)	(0.017)	(0.012)	(0.025)	(0.014)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
B: Elasticity						
log $(1-\tau)$	-11.989	-11.506	-2.467	-1.339	-0.586	-0.471
	(0.161)	(0.453)	(0.104)	(0.077)	(0.157)	(0.084)
log $(1-\tau) \times$ blacklisted	1.277	4.289	-2.923	-1.884	-1.920	-2.738
	(1.290)	(1.798)	(0.771)	(0.598)	(1.411)	(0.633)
log $(1-\tau) \times$ suspended	-2.678	4.271	-2.299	-0.423	-1.343	-0.262
	(0.873)	(2.055)	(0.553)	(0.424)	(0.407)	(0.564)
Baseline Coefficient	-12.065	-11.334	-2.578	-1.369	-0.651	-0.504
	(0.158)	(0.439)	(0.103)	(0.076)	(0.154)	(0.083)
Observations	3,728,660	4,179,561	3,983,213	5,058,579	612,993	4,623,907
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Notes*:* The table compares the responses of blacklisted and suspended firms with other treated firms. I estimate a triple-difference version of model (9), partitioning the treatment dummy into three dummies treat$_i$, blacklisted$_i$, and suspended$_i$. The dummy variables blacklisted$_i$ and suspended$_i$ denote that a treated firm i is blacklisted or its registration has been suspended. Baseline coefficient reports the $treat \times after$ coefficient I obtain from estimating the model without the triple-interaction terms. Standard errors are in parenthesis, which have been clustered at the firm level.

TABLE A.X: **SUMMARY STATISTICS – BLACKLISTED FIRMS**

	Blacklisted (1)	Suspended (2)	Others (3)
A: VAT Outcomes (PKR Millions)			
1. Purchases	7.043	5.781	1.875
2. Sales	4.609	5.465	2.183
3. Exports	0.649	1.187	0.388
4. Domestic Sales	3.960	4.277	1.795
5. Tax Payable	0.010	0.038	0.082
6. First Year Sales	6.105	5.636	1.484
7. First Year Purchases	6.246	6.019	1.071
8. First Year Exports	0.899	1.069	0.309
9. First Year Domestic Sales	5.206	4.566	1.175
10. First Year Tax Payable	0.013	0.024	0.053
11. Sales Minus Purchases	-3.422	0.633	0.861
12. Output Tax Minus Input Tax	-0.121	-0.152	0.105
B: Firm Characteristics			
13. Manufacturer	0.267	0.286	0.373
14. Wholesaler	0.338	0.339	0.253
15. Exporter	0.175	0.146	0.080
16. Some Export	0.236	0.262	0.160
17. Some Import	0.303	0.381	0.307
18. Company	0.062	0.092	0.092
19. Partnership	0.036	0.054	0.069
20. # Months Filed	38.329	40.580	41.450
21. # Months Active	15.572	23.662	27.763
22. Major City	0.718	0.752	0.503
23. Initial Capital	0.713	1.111	7.749

Notes: The table compares VAT outcomes and firm characteristics of blacklisted and suspended firms with other firms. Each row reports the mean value of the variable for the corresponding group of firms. The definitions of the compared variables are provided in Appendix A.1.