# Essays in Public Finance and Development Economics

by

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# ABSTRACT

This dissertation addresses various topics in public finance that are particularly relevant for developing countries. These topics include consumption taxes, tax evasion, and rent-seeking by government officials. The first and third chapters focus on how formal and informal taxes affect production and firm behavior in developing countries, while the second focuses on household responses to a consumption tax in a developed country context.

Chapter 1 examines firms response to a size-based exemption threshold for a Value Added Tax (VAT) in India and shows how compliance costs affect production and strategic misreporting among manufacturing firms. Although the marginal tax rate on production and compliance costs change discontinuously at the exemption threshold, firms response can be almost fully attributed to compliance costs alone. Patterns of input use corresponding to the reported output of firms just below the exemption threshold suggest that they are producing less output than they would have in the absence of the tax and not simply underreporting their true output to appear small.

Chapter 2, joint with Yeliz Kacamak and Eleanor Wilking, studies how the tax elasticity of the consumption tax base is affected by changing sales tax remittance rules. We study empirically how an important development in U.S. sales tax policy  the requirement of online retailers to remit the sales tax instead of the consumer, as part of Voluntary Collection Agreements (VCAs)  impacts this elasticity. Our identification strategy exploits quasi-experimental variation from the staggered state-wise introduction of the VCAs. We find that consumers reduce their online expenditure after the introduction of VCAs, consistent with an increase in compliance with sales taxes on online sales and suggesting that consumers took note of the tax change. On average, we do not find evidence of an impact of the remittance rule change on the elasticity of the tax base with respect to the tax rate.

Chapter 3, joint with Traviss Cassidy, studies whether competition between local governments can reduce rent-seeking by local officials in the context of a major period of decentralization in Indonesia that increased the number of local governments by 50 percent within a decade. District governments, which are responsible for most local public goods expenditure and receive revenue from business licensing and fees, split into smaller districts, increasing the number of local governments within original district boundaries.

We find that on average, there is a small increase in the probability of any bribe payments by firms and no change in the average size of payments. However, newly created establishments in districts that split are less likely to report bribe payments. We suggest that these results are consistent with a model of rent-maximizing bureaucrats in which bribe rates are constrained by mobility of firms.

# CHAPTER I

## Misallocation of Misreporting? Evidence from a Value Added Tax in India


### Abstract

This paper analyzes the effect of an Indian value added tax (VAT) on production. The VAT system features a revenue-based exemption threshold intended to exempt small firms, which also incentivizes firms to either appear or remain small. Analysis using a novel dataset created by linking detailed establishment and commodity-level survey data to time-varying and commodity-specific VAT rates indicates that firms' reported revenues are on average 10 percent lower in the neighborhood of the exemption threshold. This neighborhood represents about 1 percent of total manufacturing output. This output response is largely due to compliance costs and additional enforcement associated with VAT registration rather than increased tax liability. Based on the revenue-to-input cost ratio of firms just below the exemption threshold, the observed production distortions appears to correspond to production changes rather than tax noncompliance. These findings indicate that the VAT would distort production even with perfect enforcement and that efforts to reduce firms' compliance burden could be welfare enhancing. Because the exemption threshold for a VAT is a ubiquitous and salient size-based regulatory threshold for most firms in developing countries, the insights and challenges illustrated in the context of the Indian VAT on manufacturing are widely applicable.



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## 1.1 Introduction

An important measure of the welfare consequence of taxation is how it distorts economic activity. Recent literature has demonstrated large responses by firms to tax ”notches” and ”kinks” at size-based thresholds, which are ubiquitous features of tax systems where tax liability changes discontinuously. Yet evidence of real production distortions, as opposed to strategic misreporting of true production, is elusive. This paper finds evidence in survey data of a substantial real reduction in production. I estimate a 10-20 percent production decrease among firms in the neighborhood of the exemption threshold, a tax kink and compliance notch created by a value added tax (VAT) in India. The use of survey data instead of more commonly-used administrative tax return data is important for two reasons. First, we might expect to be able to better measure real effects of taxation in data that does not directly affect a firm's tax liability, unlike tax returns. Second, if survey data is nonetheless influenced by strategic misreporting in response to tax incentives, that has consequences for research on productivity estimation and comparisons, which relies on the veracity of such establishment data in India and other countries.

The empirical approach of this paper is in the tradition of papers like Hurst, Li and Pugsley (2014), which is described as the ”traces of evasion” approach by Slemrod and Weber (2012). Because evasion is difficult to observe directly, they use information on associated activities to reveal evidence of evasion. To arrive at their conclusion that the self employed underreport income even in survey data, Hurst, Li and Pugsley (2014) compare the income of self-reported individuals to the income of employees with the same reported consumption under the assumption that the consumption of both types of people and the income of the employees are truthfully reported. Analogously, this paper assumes that production inputs that have no bearing on tax liability are truthfully reported and uses this information to infer whether the observed output response is due to real production changes. Modeling heterogenous firms' responses to the CenVAT incentives shows that we would expect average revenue-to-input-cost ratios to be distorted upwards just below the exemption threshold if firms limit real production to remain below the threshold. On the other hand, if firms strategically underreport revenue, we would expect revenue to input cost ratios to be distorted downwards.

Existing research on firms responses to tax incentives in developing countries uses administrative data from tax returns and shows that reported output is highly responsive to kinks and notches in the tax schedule (Best et al. (2015); Alejos (2018), etc.). In fact, some authors argue the estimated output elasticity with respect to the tax rate is too high to be a real production response. Given reasonable output elasticities, Best et al. (2015)

estimate that evasion accounts for 15 to 70 percent of the change in the corporate tax base in Pakistan in response to the profit tax rate. This substantial misreporting might carry over to establishment survey data if firms believe this information may be accessible to tax officials or even if firms find it simpler to report the same figures in all reports and documents. We cannot take as given that establishment survey data is not plagued by the same mismeasurement.

The anatomy of the behavioral response to the VAT notch also matters for the appropriate policy response. As Slemrod and Kopczuk (2002*a*) argue, the elasticity of taxable income (ETI) is sensitive to the regulatory and enforcement environment. If a larger portion of the ETI is due to evasion response than to real response, as previous research has suggested, it may be affected by enforcement policy. Furthermore, to the extent that firms' evasion costs reflect transfers to others agents in the economy (e.g., revenue from penalties in tax audits), it is the elasticity of the real tax base with respect to the tax rather than reported base that is a sufficient statistic for excess burden (Chetty (2009*a*)). Although under some assumptions the distinction between evasion and real response does not affect the welfare consequences of a tax (Emmanuel Saez, Joel Slemrod and Seth H. Giertz (2012); Martin Feldstein (1999)), the *enforcement elasticity of the tax base* (as described in Keen and Slemrod (2017)) is in general an important parameter for optimal tax systems. Finally, we may have specific welfare objectives such as "fairness" for which we directly care about the level of evasion.

What is a VAT notch? Firms are responsible for remitting the VAT to the tax authority. Recognizing that the costs associated with this responsibility (such as tax filing and record keeping) may be burdensome for small firms, and the administrative costs to the tax authority for dealing with small firms may not be worth the additional revenue, most tax authorities give firms with revenue below a certain threshold the option to be exempt from the tax. For firms that would take this exemption, there is a discontinuous change in their tax liability around this revenue threshold. If their revenue is below the threshold, they do not remit any tax on their output. Above the threshold, their tax liability increases by their value-add, creating a "notch" in their tax schedule. In the context considered in this paper, which is a VAT on manufacturing called the "CenVAT", the rules create only a "kink" in the tax liability instead of a notch because firms have the option to only remit tax on their output above the exemption threshold. However, the compliance costs associated with registering for the CenVAT create a notch in firms' total costs.

The VAT is a major source of revenue in most countries, making the revenue-based exemption threshold for a VAT a widespread and salient size-based regulatory threshold for firms nearly everywhere. Moreover, the VAT is generally broad-based and covers most goods and services in an economy, which means the VAT notch is relevant for most firms within countries as well. A thorough understanding of firms' response to a VAT notch is therefore applicable in many contexts.

To document and analyze the response to the VAT threshold, I use data from the the Annual Survey of Industries (ASI), which is both an annual census of manufacturing establishments with over a 100 workers and a 20 percent random sample of the organized manufacturing sector with fewer than 100 workers. This data has been used in recent years to study various aspects of manufacturing productivity in India (Hsieh and Klenow (2009); Martin, Nataraj and Harrison (2017); Rotemberg (2017) etc.). I link this production data to information on CenVAT rates by 8-digit product code. As a survey intended to generate detailed production statistics about the manufacturing sector, the ASI contains balance sheet information on establishments that are never reported to the tax authority, such as fixed capital, working capital, loans, investment in plant and machinery, number of workers and man-hours worked. The data may also cover firms that are not registered with any tax authority. Hence information on firms' inputs potentially provide a second source of information about firms' true revenue.

Firms' response to the VAT is apparent in the excess mass of firms with revenue just below the exemption threshold, suggesting that the VAT lowers reported output (see Figure 1.4). The exemption threshold was raised by 50 percent in 2008, causing the excess mass in the firm revenue distribution to shift to the new threshold. This shift confirms that the shape of the distribution around the threshold was due to tax incentives. The revenue distribution of firms producing goods that are exempt from the CenVAT shows no such excess mass, providing further evidence that the excess mass in the revenue distribution of taxable goods producers is caused by the tax incentives at the threshold.

The analysis proceeds as follows: First, I use standard bunching estimation techniques as described in Kleven and Waseem (2013) to estimate the excess mass of firms due to the notch as well as the upper bound of the manipulation region. Firms whose potential revenue is in the manipulation region are the firms who may reduce output to remain below the threshold. Next, I examine the revenue to input cost ratio of firms in the estimated bunching region. As I show in the conceptual framework, the pattern we would expect to see if the bunching were caused by real production changes is the opposite of what we would expect under misreporting. This production response seems to be due to the compliance costs and increase in enforcement at the exemption threshold. Finally, I show that at least some of the change in revenue to input costs around the threshold is likely explained by the selection of firms into bunching based on factor-specific productivity.

Results show that reported output would have been higher by about 8 to 20 percent on average among taxable goods producers with reported revenue in the neighborhood of the exemption threshold, in absence of the CenVAT. The total reported output of these firms represent about 1 percent of total output of all taxable goods manufacturers. These figures are an average across firms that do and do not value the VAT exemption, which means the response of firms who value the exemption is even larger. Public and private limited companies are more responsive than sole proprietorships and partnerships. The magnitude of the response is similar even when the threshold increases by 50 percent in nominal terms from ₹10 million (approx $150,000) to ₹15 million (approx $230,000).

Comparing firms producing goods taxed at the standard CenVAT rate to those producing goods at the reduced rate, I find similar output reduction in both groups, suggesting that it is the compliance costs and additional enforcement associated with CenVAT registration that drives output reduction - not the change in tax liability. Finally, I address the concern that the observed difference in revenue to input cost ratio around the exemption threshold is driven by selection into bunching and voluntary registration. Even controlling for the determinants of voluntary registration, I find that revenue to input cost ratio is higher just below the exemption threshold, suggesting that the observed output response reflects real economic activity.

In Section 1.2, I describe how this paper relates to various strands of literature, Section 1.3 provides details of the empirical context and firms incentives in the CenVAT, Section 1.4 illustrates the theoretical framework linking firm's incentives, observed outcomes, and the assumptions required to identify evasion. Section 1.5 presents the empirical strategy to first estimate the extent of bunching at the threshold and then to separately estimate the extent of real response at the notch. Section 2.3 describes the data and provides relevant descriptive statistics, Section 1.7.1 presents evidence on bunching at the tax notch, Section 1.7.2 shows the extent of evasion, and Section 1.7.3 argues that the output reduction is due to compliance costs. Section 1.8 addresses selection and voluntary registration. Finally, Section 1.9 concludes.

## 1.2 Related Literature

This paper builds on the traces of evasion literature starting with Pissarides and Weber (1989) and summarized in Slemrod and Weber (2012). Evasion is difficult to measure directly except through audits. Instead, evasion is inferred from observed activity with the help of assumptions about the link between this activity and true income. For example, in their seminal work, Pissarides and Weber (1989) infer the extent of income underreporting among the self-employed by comparing the reported consumption to income ratio of the self-employed to employees with similar consumption profiles. Assuming that the self-employed and employees truthfully report their consumption, and that employees also truthfully report their income, the difference in the consumption to income ratio of self-employed individuals from that of employees tells us the extent of income underreporting.

Johnson, Kaufmann and Shleifer (1997) take a similar approach in aggregate data to estimate the extent of the informal sector that is not captured in official GDP estimates by using the total electricity consumption in various countries. Assuming the elasticity of GDP to electricity is approximately 1, deviation from this elasticity is an estimate of the underreported GDP because electricity consumption can be measured accurately and truthfully. In this paper, I apply the same intuition to compare the electricity use among firms just below the VAT exemption threshold to firms far away from the threshold.

Although I use data from a statutory survey of manufacturing establishments, which is never shared with the tax authority, the data may still reflect evasion. First, the data is presumably based on firms records, which may be maintained with possibility of audits in mind. If firm owners and accountants believe there is even a small chance of detection based on discrepancies between what is reported to the tax authority and their records or survey responses, they may not report truthfully to the survey. For example, Amirapu and Gechter (2018) find that firms underreport the number of employees in the Economic Census, which is used to construct the sampling frame for the survey of manufacturers. Labor regulations set in at various worker thresholds, which incentivizes firms to underreport their workers even in the census data. An important difference here is that the worker information reported in the economic census is shared with regulatory bodies.

The paper also contributes to the literature on behavioral responses of firms at tax kinks and notches. Size-based regulation are a common feature of tax systems. These regulations introduce kinks or notches where either compliance costs, enforcement or tax liability change discontinuously across a revenue threshold. Firms responses to these kinks inform us of the elasticity of their output with respect to these various cost margins. For example, Asatryan and Peichl (2017) estimate the elasticity of firms output with respect to a compliance cost notch. Jarkko Harju, Tuomas Matikka and Timo Rauhanen (2018) study firms responses to both a compliance cost notch and a tax liability notch and find that firms output is much more responsive to compliance costs than tax liability. Finally, Almunia and Lopez-Rodriguez (2018) examine the response of Spanish firms to a revenue-based enforcement notch created by the Large Taxpayers Unit. They find that firms reduce their output by 2 percent on average in response to the increase in enforcement at the threshold.

There are differences across contexts in the extent to which firms' response reflect strategic misreporting or real production response. The estimated extent of evasion in Pakistan (Best et al. (2015) stands in stark contrast to Harju, Matikka and Rauhanen (2018), who suggest that in an advanced economy like Sweden, firms bunch below the VAT registration threshold by reducing real rather than reported output. One contribution of this paper is to examine firm behavior in survey data, which captures firms outside of the tax net and information not reported to the tax authority.

While this paper separates the real and reporting response, it does not estimate an elasticity of the tax base with respect to the tax rate. Lockwood (2018) stresses that under a notch, the elasticity of the tax base with respect to the tax rate is no longer a sufficient statistic for the marginal excess burden due to the tax. This is because a change in the tax rate under a notch can have a first order effect on tax revenue. Under a notch, firms who bunch reduce their tax liability on all their income and not just by the amount they reduced their income above the threshold. This results in a discontinuous change in their tax liability unlike in the case of a tax kink, and therefore a first-order effect on tax revenue. However, the share of the real output response is still informative about how much of the true elasticity of the tax base can be influenced by enforcement.

Productivity differences across firms, both within and across countries, are a focus of much research in economics. Measures of productivity differences rely on establishment censuses, including in India, where the Annual Survey of Industries (ASI) is the only source for such information. Productivity differences are estimated using differences in measured input use relative to revenue. Strategic misreporting because of tax incentives can lead to incorrect estimates of productivity. For example, the cost-shares approach to production function estimation (Ackerberg, Caves and Frazer (2015) ) estimates Cobb-Douglas coefficients using the ratio of reported input costs to revenue. If revenue is underreported in response to taxes, these ratios are incorrectly measured.

More popular proxy-based methods of estimating production functions are neither appropriate nor necessary in the presence of misreporting. One of the key assumptions in the proxy-based approach  scalar unobservables - is that intermediate inputs depend only on observables like labor and capital input and a single unobservable, which is productivity. It is likely that this assumption fails in the VAT context as VAT-registered firms are incentivized to misreport intermediate inputs.

## 1.3 Empirical Context

### 1.3.1 Small Scale Industry (SSI) Exemption under the CenVAT

Until July 2017[1], the central government of India imposed a value added tax on manufactured goods called the "CenVAT"[2] . The CenVAT was nominally a tax on manufacturing, which for the purpose of this tax was defined as any activity that resulted in the creation of a new and marketable product. This definition included repackaging, relabeling and branding of products but exempted wholesalers and retailers.

The CenVAT operated like a standard VAT up to the manufacturing stage in most respects except for two key differences. Like other VAT systems, firms remitted tax on their output and could receive input tax credits on any taxable inputs purchased from CenVAT-registered firms, creating the classic self-enforcing chain mechanism of a VAT and preventing cascading taxes. It offered an exemption for firms whose annual revenue was below an exemption threshold, which was ₹10 million (approx. $150,000) until 2007 or below ₹15 million (approx. $230,000) thereafter. Firms whose revenue was below the exemption threshold, and therefore were eligible for the exemption, could choose to voluntarily register for and remit the CenVAT, another standard feature of VAT systems. The differences from a standard VAT arise because of two particular rules regarding the revenue-based exemption for the CenVAT, which was called the "Small Scale Industry" (SSI) exemption[3].

Unlike other VAT systems, firms had two options once they crossed the exemption and registration threshold. They could either remit tax on their entire revenue and claim input tax credits (i.e. remit tax on their value added) or they could remit tax only on the revenue above the exemption threshold without claiming any input tax credits. In a standard VAT, firms would have to remit tax on their entire value added once they register. A second difference from the standard VAT is that firms could only opt for this SSI exemption if their revenue in the previous fiscal year was below ₹40 million (approx. $600,000). This second condition turns out to have little effect on firm behavior for reasons discussed in Appendix A.1.1. However, the option to remit tax on your turnover in excess of the exemption

[1]In July 2017, India introduced the comprehensive Goods and Services Tax (GST), which subsumed this CenVAT along with many other taxes including the State Value Added Tax, Service Tax and others.

[2]This tax is also referred to as the Central Excise Tax. The use of the term excise tax in its description is due to its origins as an excise tax on salt under British rule. Over time the tax base was expanded to cover nearly all manufacturing. A major reform in 1999 introduced the value added tax structure to the Central Excise Tax, when it was named the CenVAT. Starting in 2001, firms could claim input tax credits on all taxed intermediate inputs and capital goods.

[3]Establishments designated as "SSI" received other preferential treatment such as the license to produce certain commodities or lower interest loans. But the criteria to qualify as an SSI firm for all other benefits was in terms of the original value of investment in plant and machinery, not their revenue. The revenue-based SSI classification only applied to the CenVAT exemption.

threshold instead of the entire value-add creates a kink in the tax liability instead of a notch. Taxpayers still face a compliance cost notch at the exemption threshold because firms must register once they cross the threshold regardless of whether they choose to remit tax on turnover or on value-add.

Registration for the CenVAT is separate from any other registrations of the business. To register, firms have to fill out paperwork and obtain a taxpayer ID number specifically for the CenVAT, which they will then use to file either monthly or quarterly returns. Once a firm is registered and filing returns, they have to keep certain records and could be subject to audit according to the selection criteria of the tax authority such as risk of evasion and potential tax revenue. These additional requirements introduce a fixed compliance cost once a firm registers. It is possible to de-register if a firms output remains below the exemption eligibility threshold.

### 1.3.2 Tax Liability Under the CenVAT

Consider a firm with pre-tax revenue of $R_{it}$ and pre-tax cost of taxable intermediate inputs of $p^M M_{it}$, where $p^M$ is the pre-tax unit price of intermediate inputs $M_{it}$. If the firm always registers for the CenVAT, regardless of whether they may be eligible for the SSI exemption, their tax liability is:

$$T(\tau, R_{it}, p^M M_{it}) = \tau R_{it}$$

where $\tau$ is the CenVAT rate. They remit tax on their revenue, $R_{it}$ and receive input tax credits on their taxable inputs[4].

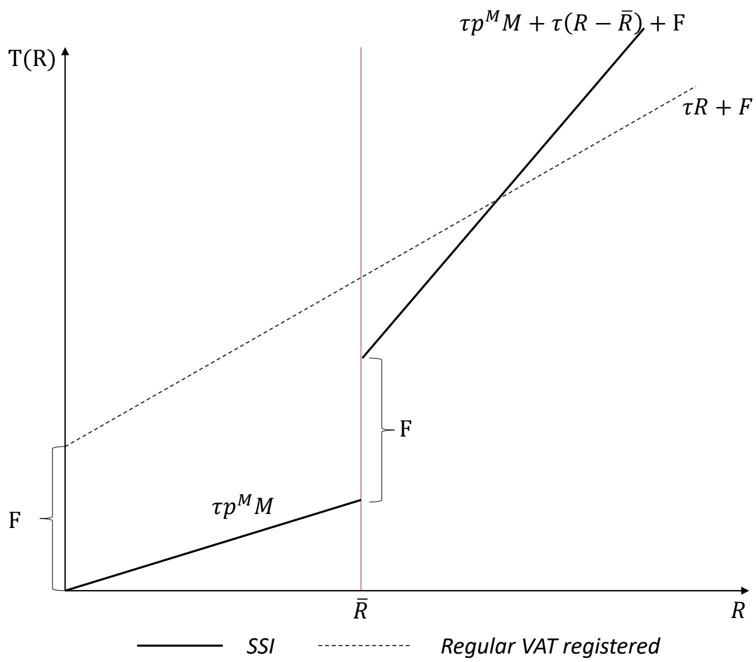
In addition, they incur compliance costs associated with CenVAT registration, which I treat as a fixed cost of compliance, $F$. The sum of their tax liability and compliance cost is:

$$T(\tau, R_{it}, p^M M_{it}) + F = \tau R_{it} + F \tag{1.1}$$

On the other hand, if a firm takes the SSI exemption when eligible, their tax liability and compliance costs are as follows:

[4]One might be expecting tax liability under a VAT to be written as the tax rate multiplied by the value added of firm, $\tau(R_{it} - p^M M_{it})$. However, here I have represented it as the tax on a registered firm's total revenue, so that it is clear to the reader that an unregistered firm still contributes to VAT revenue through their foregone input tax credits. Under this notation, the *difference* in tax liability for a registered and unregistered firm, is the tax on their value added, $\tau(R_{it} - p^M M_{it})$

Figure 1.1: Tax Liability and Compliance Cost under the CenVAT




$$T(\tau, R_{it}, p^M M_{it}, \bar{R}) = \begin{cases} \tau p^M M_{it} & \text{if } R_{it} < \bar{R} \\ \tau p^M M_{it} + \tau(R_{it} - \bar{R}) + F & \text{if } R_{it} > \bar{R} \end{cases} \tag{1.2}$$

They do not remit any tax on their output if their revenue is below the exemption threshold, $\bar{R}$ but must forgo their input tax credits. Once their revenue crosses the exemption threshold, they must remit tax on any revenue above the exemption threshold but still do not receive input tax credits and face a compliance cost notch equal to $F$ at this threshold, which represents the costs associated with monthly filing, record keeping and higher probability of audit once a firm is registered. Tax officials need permission from a senior official to enter the premises of SSI firms but not of registered firms, which creates an additional enforcement notch at this threshold.

Figure 1.1 shows how the CenVAT rules create a kink in the tax liability and a notch in compliance costs at the exemption threshold, $\bar{R}$. The dashed line shows the sum of their tax liability $T(R_{it})$ and fixed compliance cost, $F$ if they are always registered under the CenVAT. It is a linear function of their revenue with a slope of $\tau$ and an intercept of $F$.

On the other hand, if they take the SSI exemption, their tax liability is described by the solid line. Until their revenue reaches $\bar{R}$, they do not have to register for the CenVAT and therefore do not incur the fixed compliance cost. Their tax liability is the forgone input tax credits, $\tau p^M M_{it}$, which increases with revenue as they require more inputs to generate greater revenue. Once they cross the exemption threshold, they incur the fixed compliance cost $F$ and they must remit tax on revenue above threshold in addition to the forgone input tax credits. Their tax liability now increases more quickly with revenue, creating a tax kink and the fixed compliance cost creates the notch.

To summarize, firms faced a kink in tax liability at ₹15 million, a notch in tax liability at ₹40 million, and a notch in compliance costs at ₹15 million. This paper focuses on firm behavior at the ₹15 million exemption threshold, and treats it as a combination of a compliance cost notch and a tax kink[5].

Unlike the VAT threshold in many advanced economies, the exemption threshold for the CenVAT was relatively high. In 2004, nearly 50 percent of organized manufacturing firms were below the exemption threshold. Because the threshold is in nominal terms and not indexed to inflation, this share declined over time and in 2012 about 30 percent of organized manufacturing firms were below the exemption threshold. As we might expect, exempt firms have smaller output and therefore only represent between 1 to 3 percent of organized manufacturing output. They also represent a sizable proportion of total employment in organized manufacturing ranging from between 5 to 15 percent of total employment in the decade between 2005 and 2015. The substantial discrepancy between the output share and employment share is because these small firms are much less productive.

Although tax liability is higher for a registered firm, their output may also be more attractive to other registered businesses because they can provide input tax credits. Under the CenVAT, a registered downstream firm can only claim input tax credits on purchased from a registered upstream firm. As a result, in the CenVAT as with other VAT systems, upstream firms may have an incentive to voluntarily register. The voluntary registration decision depends primarily on the whether their potential buyers are registered CenVAT businesses, or if they are unregistered entities such as unregistered firms or final consumers, who cannot avail of input tax credits. A second determinant, conditional on firms being able to sell to both registered and unregistered firms, is their taxable input costs as a share of revenue. A more detailed explanation of the voluntary registration decision

[5]This SSI exemption threshold of ₹15 mn is still salient under the new tax regime that replaced the CenVAT - the Goods and Service Tax or GST. Firms whose revenue is below this threshold can opt for the composition scheme under the GST which means they are subject to a turnover tax instead of a VAT.

along with an example is provided in Appendix A.1.1. In the conceptual framework that follows, I derive the conditions under which a firm would voluntarily register based on parameters of the production function and the price of the firms' output if they are or are not registered.

### 1.3.3 Other features of the CenVAT

After 2001, the CenVAT had 3 to 4 applicable rate categories the standard rate, reduced rate, exempt and special rate categories. The applicable VAT rate within these categories changed over time. Some manufactured commodities (largely food items, medicines and publishing) were exempt from the CenVAT, but only exports were zero-rated, which means exporters faced a zero rate on their revenue but could claim input tax credits. On the other hand, firms that produced exempt commodities faced a zero rate on their revenue but could not claim input tax credits.

## 1.4 Conceptual Framework

This section presents a model that demonstrates how evasion and true production changes manifest themselves in the observed average revenue-to-labor cost ratio of firms that bunch at a size-based exemption threshold. The key intuition here is that the observed revenue-to-labor cost ratio in the bunching region is an average across firms in a range of productivity from whom it is optimal to report revenue at the threshold. When their true revenue is equal to or close to this reported revenue, higher productivity firms require less input to produce at that level. On the other hand, when firms' true revenue is unrelated to their report (i.e. when evasion costs are low), more productive firms use more inputs to produce more but nevertheless report output equal to the threshold, thereby driving down the average revenue-to-labor cost ratio.

### 1.4.1 Distribution of Revenue and Input Use

Ignoring voluntary registration for the moment, consider firms' optimization with and without evasion. In both cases, in a model with firms of varying productivity levels, the CenVAT notch incentivizes some firms to bunch below the threshold. Without evasion, this bunching represents firms optimally producing output at or below the threshold. Allowing for evasion, some firms who produce output greater than the exemption threshold under-report revenue to exactly the exemption threshold. As this model will show, as the mix of output response shifts from under-reporting to a real production response, the average revenue-to-labor cost ratio of bunching firms rises above the non-tax average.

**No misreporting** Consider firms that use only labor inputs, denoted by $E_i$ which are exempt from VAT. Without taxable inputs in production, voluntary registration is never optimal so we can characterize the revenue and revenue-to-labor cost distribution ignoring this factor. In a later section I will show that allowing voluntary registration does not affect these characterizations under certain conditions. Firms are heterogenous in an exogenously given productivity parameter, $\omega_i$, which gives rise to the firm size distribution, as firms' productivity determines their unique size given production with decreasing returns to scale.[6] Returns to scale is denoted by $\psi \in [0, 1)$ and firms' profit is given as:[7]

$$\pi_i = \omega_i E_i^{\psi} - p^E E_i - \bar{\tau}(R_i - \bar{R}) - \mu F \tag{1.3}$$

where

$$(\bar{\tau}, \mu) = \begin{cases} (0, 0) \text{ if } R_i \leq \bar{R} \\ (\tau, 1) \text{ if } R_i > \bar{R} \end{cases}$$

Price of labor $p^E$ is exogenously given. Tax liability is modified from equations (1.1) and (1.2) to ignore voluntary registration and $\mu$ is a dummy for whether the firms' revenue is above the exemption threshold. Note that in this benchmark model, I am not allowing for misreporting.

Solving the firm's optimization problem, their optimal revenue can be described as a function of $\omega_i$ as follows:

[6]Entry and exit are not explicitly modeled but this framework is consistent with models where firms must pay a fixed cost to enter the market and only realize their productivity draw upon entering. Their decision to enter or exit is based on the expected productivity draw.

[7]Because no firm would voluntarily register, the tax schedule is that of firms that would always choose the exemption.

$$R_i^* = \begin{cases} \omega_i^{\frac{1}{1-\psi}} \left[ \frac{p^E}{\psi} \right]^{\frac{\psi}{\psi-1}} & \text{if } \omega_i < \omega^1 \\ \bar{R} & \text{if } \omega^1 < \omega_i < \omega^2 \\ \omega_i^{\frac{1}{1-\psi}} \left[ \frac{p^E}{(1-\tau)\psi} \right]^{\frac{\psi}{\psi-1}} & \text{if } \omega_i > \omega^2 \end{cases} \tag{1.4}$$

where $\omega^1$ and $\omega^2$ are defined by the following conditions: $\omega^1$ is the productivity level at which profit-maximizing revenue is equal to the threshold level of revenue, i.e. $R_i^*(\omega^1) = \bar{R}$. The upper bound, $\omega^2$ is such that the firm is indifferent between constraining revenue at the exemption threshold and producing at a level of revenue above the exemption threshold. Firms with productivity between these two thresholds choose to bunch at the exemption threshold.

Their revenue-to-input cost ratio is given as:

$$\frac{R_i^*}{p^E E_i} = \begin{cases} \frac{1}{\psi} & \text{if } \omega_i < \omega^1 \\ \frac{\bar{R}^{\frac{\psi-1}{\psi}} \omega_i^{\frac{1}{\psi}}}{p^E} & \text{if } \omega^1 < \omega_i < \omega^2 \\ \frac{1}{\psi(1-\tau)} & \text{if } \omega_i > \omega^2 \end{cases}$$

**With Misreporting** Now consider firms' optimization allowing for misreporting. I modify firms' profit in equation 1.3 to allow firms to under-report their revenue at a cost $c(R_i - \hat{R}_i)$, where $c(.)$ is a convex function of the amount of misreporting $e = (R_i - \hat{R}_i)$ and reported revenue is $\hat{R}_i$. Firms' profit is:

$$\pi_i = \omega_i E_i^{\psi} - p^E E_i - \bar{\tau}(\hat{R}_i - \bar{R}) - \mu F - c(R_i - \hat{R}_i) \tag{1.5}$$

where

$$(\bar{\tau}, \mu) = \begin{cases} (0,0) \text{ if } \hat{R}_i \leq \bar{R} \\ (\tau, 1) \text{ if } \hat{R}_i > \bar{R} \end{cases}$$

Now the tax and compliance costs applicable depend on reported instead of true revenue. Again, there are threshold levels of productivity, $\omega^1$ and $\tilde{\omega}^2$, such that firms with

$\omega_i \in [\omega^1, \tilde{\omega}^2]$ will report revenue equal to the exemption threshold. Reported revenue-to-labor cost ratio is now given by:

$$\frac{\hat{R}_i^*}{p^E E_i} = \begin{cases} \frac{1}{\psi} & \text{if } \omega_i < \omega^1 \\ \bar{R}(\omega_i \psi)^{\frac{1}{\psi-1}} (p^E)^{\frac{-\psi}{\psi-1}} [1 - c_e(R_i - \bar{R})]^{\frac{1}{\psi-1}} & \text{if } \omega^1 < \omega_i < \tilde{\omega}^2 \\ \frac{1}{\psi(1-\tau)} - \frac{c_e^{-1}(\tau)}{p^E} \left[ \frac{p^E}{\omega_i \psi (1-\tau)} \right]^{\frac{1}{1-\psi}} & \text{if } \omega_i > \tilde{\omega}^2 \end{cases}$$

Note that for all firms with $\omega_i \in [\omega^1, \omega^2]$, their reported revenue will be $\bar{R}$. So the average revenue-to-labor cost ratio we will observe for firms reporting revenue at the exemption threshold (i.e. the bunching region) will be an average across all firms in this range of productivity:

$$E\left[ \bar{R} \left( \frac{\omega_i}{p^E} \right)^{\frac{\psi}{\psi-1}} [1 - c_e(R_i - \bar{R})]^{\frac{1}{\psi-1}} \mid \omega_i \in [\omega^1, \omega^2] \right] \tag{1.6}$$

This average revenue-to-labor cost ratio is a function of the marginal cost of misreporting at the equilibrium level of under-reporting, $R_i - \bar{R}$. As the marginal cost increases, firms underreport less and so the firms that bunch (i.e. report revenue of $\bar{R}$) must produce less, which increases the average reported revenue-to-labor cost. At one extreme when there is zero cost of misreporting, all firms would bunch at the threshold and true output would be equal to the no-tax counter-factual such that average revenue-to-labor cost ratio would be given by:

$$E\left[ \bar{R}(\omega_i \psi)^{\frac{1}{\psi-1}} (p^E)^{\frac{-\psi}{\psi-1}} \mid \omega_i \in [\omega^1, \bar{\omega}] \right] \tag{1.7}$$

where we get this expression by evaluating expression 1.6 with $R_i = \bar{R}$.

At the other extreme, when the cost of misreporting is prohibitively high such that no one can misreport, the average revenue-to-labor cost ratio of bunching firms is given by:

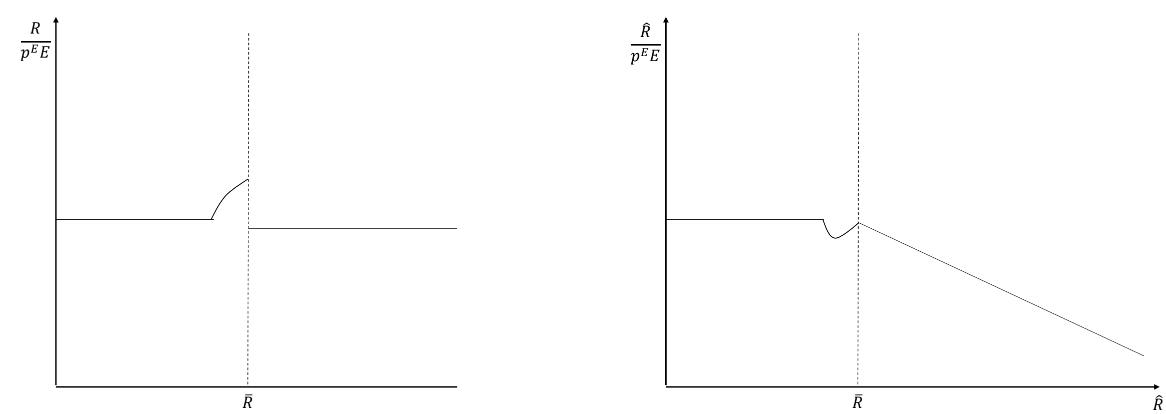
$$E\left[ \frac{\bar{R}^{\frac{\psi-1}{\psi}} \omega_i^{\frac{1}{\psi}}}{p^E} \mid \omega_i \in [\omega^1, \omega^2] \right] \tag{1.8}$$

By definition of $\omega^1$, it must be the case that the revenue-to-labor cost ratio of this marginal bunching firm is equal to $\frac{1}{\psi}$, its no-tax revenue-to-labor cost ratio. In Expression 1.7, the term inside the expectation is decreasing in productivity, which means that the average is lower than the revenue-to-labor cost ratio of the firm with $\omega^1$ productivity. In Expression 1.8, the term inside the expectation is increasing in productivity, which means that the average is higher than the revenue-to-labor cost ratio of the firm with $\omega^1$ productivity, i.e.:

$$E\left[\bar{R}(\omega_i\psi)^{\frac{1}{\psi-1}}(p^E)^{\frac{-\psi}{\psi-1}} \mid \omega_i \in [\omega^1, \bar{\omega}]\right] < \frac{\hat{R}}{p^E E_i}(\omega^1) = \frac{1}{\psi} < E\left[\frac{\bar{R}^{\frac{\psi-1}{\psi}}\omega_i^{\frac{1}{\psi}}}{p^E} \mid \omega_i \in [\omega^1, \omega^2]\right]$$

As the cost of evasion for a given level of evasion increases, fewer firms under-report their revenue, and those who do, under-report by a smaller amount and reduce their true production by more. Therefore, the average observed revenue-to-labor cost ratio among bunching firms would increase as the cost of misreporting rises and true production is distorted to a greater extent.

With and without evasion, we would observe bunching in the revenue distribution. However, without evasion, we would expect a higher revenue to input cost ratio for the bunching firms, which is the opposite of what we would expect when there is no true production distortion. Whether or not firms would choose to under-report revenue depends on the marginal cost to doing so. I examine the revenue-to-labor cost observed in the data to see which of the two extremes described above better matches the data.




(a) High misreporting cost (b) No misreporting cost

Figure 1.2: Revenue to input cost ratio by Revenue

### 1.4.2 Voluntary Registration

Although firms with revenue less than the exemption threshold can choose to be exempt from the CenVAT, they can also voluntarily register for the CenVAT. Some firms do choose to voluntarily register, which means they have no incentive to bunch at the exemption threshold as there is no change in their tax liability. There is a concern therefore that the difference in input use efficiency of firms on either side of the exemption threshold may reflect selection of firms into voluntary registration. This section describes the determinants of voluntary registration and its converse - bunching - and shows how given standard production functions, selection would not result in systematic differences in revenue-to-input ratio around the exemption threshold among firms within a commodity market. Firms' propensity to voluntarily register only varies across commodity markets.

To analyze voluntary registration, firms' production must depend on taxable intermediate inputs in addition to tax exempt inputs. I modify the framework presented in the preceding section to have Cobb-Douglas production that depends on $M$ (taxable inputs) and $E$: $F(E, M) = E^{\alpha}M^{\beta}$. Returns to scale is decreasing such that $\alpha + \beta = \psi$. A key assumption is that pre-tax input prices, $p^M$ and $p^E$ can vary across commodity markets but not based on firms' registration status. Output prices vary across commodity markets and by firms' registration status. Registered firms receive $p^Y_B$ and unregistered firms receive $p^Y_B$. Under these conditions, it can be shown that a sufficient condition for firms to prefer not to register when we do not allow for evasion is that:

$$\frac{p^Y_C}{(1+\tau)^{\beta}} \geq (1-\tau)p^Y_B \tag{1.9}$$

This is also a necessary condition in the absence of fixed compliance costs. This condition encapsulates the results from Liu, Lockwood and Almunia (2017) that firms are more likely to select into bunching if the reduction in output tax is sufficient to compensate for the difference in price of their output as a registered and unregistered firm. They are also more likely to select into bunching if their production process is less reliant on taxable intermediate inputs for which they can only claim input tax credits if they are registered. The empirical analysis compares firms producing the same commodity, so we can abstract away from selection across commodities and focus on selection within more disaggregated categories.

The sufficient condition shows that there is no theoretical reason to expect that firms that select into voluntary registration would have systematically different revenue-to-labor cost ratios than bunching firms as the selection is independent of the unit price and intensity of use of tax-exempt intermediate inputs. If this sufficient condition fails, then there is a threshold productivity level conditional on all other parameters above which firms will voluntarily register. Therefore, more productive firms are more likely to register, which means we would expect revenue-to-input cost ratio (which is higher for more productive firms) to be lower among bunching firms. To the extent that there is selection on productivity in this manner, it would bias against finding revenue-to-labor cost ratios consistent with a true production response.

## 1.5 Empirical Strategy

### 1.5.1 Bunching Estimation

Following many previous examples in the literature (Saez (2010); Kleven and Waseem (2013); Almunia and Lopez-Rodriguez (2018)), I estimate the change in reported output of the marginal buncher and the average output response at the notch. For each of two periods (before and after 2008), I collapse the data into counts of firms within revenue bins of ₹200,000 (approx. $3000). I estimate the counterfactual density by fitting a 4th degree polynomial to these counts, with dummies for the manipulation region as follows:

$$F_k = \sum_{i=0}^{4} \beta_i R_k^i + \sum_{k=r^{lb}}^{r^{ub}} \delta_k \mathbf{I}(R_k = k) + \sum_{m \in M} \eta_m + \epsilon_k \tag{1.10}$$

where $\beta_i$ is the coefficient on each polynomial term and the coefficients, $\delta_k$ on dummies $\mathbf{I}(R_k = k)$, identify either the excess or missing mass within each revenue bin relative to the counterfactual density. $F_k$ is the actual density of firms in each revenue bin, $k$. $R_k$ is the midpoint of revenue in each bin. I also control for potential round-number bunching by including dummies for whether the interval contains a multiple of ₹50 K, 100K, 250K, 500K, 1000K or 5000K. These dummies are represented in the specification above by $\eta_m$ where $m \in M = \{50K, 100K, 250K, 500K, 1000K, 5000K\}$.

The revenue density is generally decreasing in revenue but increasing just below the exemption threshold. I set the lower bound $r^{lb}$ at the point where the density starts to increase and iterate over different choices of the upper bound $r^{ub}$ to find the upper bound such that the estimated excess mass to the left of the exemption threshold equals the missing mass to the right of the threshold as follows:

$$\sum_{k=r^{lb}}^{\bar{R}} \hat{\delta}_k = \sum_{k=\bar{R}}^{r^{ub}} \hat{\delta}_k$$

Average bunching response is estimated as:

$$b = \frac{\sum_{k=r^{lb}}^{\bar{R}} \hat{\delta}_k}{\frac{1}{2}(\hat{F}_{r^{lb}} + \hat{F}_{r^{ub}})}$$

which represents the average response across all firms, some of whom may not bunch. $\hat{F}_{r^{lb}}$ is the counterfactual density at the lower bound and $\hat{F}_{r^{ub}}$ is the counterfactual density at the estimated upper bound of the manipulation region.

The bunching estimates are translated into the percentage decrease in output they imply by multiplying the estimate by the bin size, which is ₹200,000 and dividing by revenue at the exemption threshold, which was ₹10 mn before 2008 and ₹15 mn afterward.

### 1.5.2 Strategic Misreporting and Real Response

What we would like to know is by how much the CenVAT reduces true output. If firms produce as if there was no CenVAT and simply report output at or below the exemption threshold (i.e. 100 percent of the measured output loss is due through underreporting), it has to be the case that these firms face no misreporting costs and that the revenue to labor cost ratio would be reduced by the average amount of output under-reporting. Suppose firm $i$ reports revenue $\hat{R}_i$ and the proportion of revenue underreporting is $\theta_i = \frac{R_i - \bar{R}}{R_i}$ for true revenue $R_i$, which is equal to their no-tax counterfactual revenue. Note that this expression for the amount of under-reporting takes into account that all bunching firms report revenue at the exemption threshold. Let $\bar{\theta} = E(\theta_i | \omega_i \in [\omega^1, \omega^2])$, which is the average percentage decrease in output of bunchers. Then the observed average revenue-to-labor cost ratio of bunchers is given as:

$$E\left[\frac{\hat{R}_i}{p^E E_i} \mid \omega_i \in [\omega^1, \omega^2]\right] = E\left[\frac{(1-\theta_i)R_i}{p^E E_i} \mid \omega_i \in [\omega^1, \omega^2]\right] \tag{1.11}$$
$$= E\left[(1-\theta_i) \mid \omega_i \in [\omega^1, \omega^2]\right] E\left[\frac{R_i}{p^E E_i} \mid \omega_i \in [\omega^1, \omega^2]\right]$$
$$- Cov\left(\frac{\bar{R}}{R_i}, \frac{R_i}{p^E E_i} \mid \omega_i \in [\omega^1, \omega^2]\right)$$

We can estimate the last three terms from the data. The conditional expectation of $\theta_i$ is directly estimated from bunching, which gives us the average percent decrease in output. I estimate the second expectation term and the covariance from the sample of non-bunchers in the manipulation region above the exemption threshold under the assumption that the revenue-to-labor cost ratio of non-bunchers is the same as that of bunchers.

Turning to the other extreme, where all of the observed output response is due to a decrease in true production, the percent change in revenue-to-input cost ratio as a function of revenue can be given as follows:

$$\log(\frac{R}{p^E E}) = B + \frac{\alpha + \beta - 1}{\alpha + \beta} \log R \tag{1.12}$$

where B is some constant and $\alpha + \beta$ is returns to scale in the economy. Equation 1.13 is derived from the cost-minimizing choices of tax-exempt inputs with and without the CenVAT. Therefore, the expected percent change in the revenue-to-labor cost ratio as a result of the percent change in output can be given as:

$$\frac{\partial \log(\frac{R}{p^E E})}{\partial \log R} = \frac{\alpha + \beta - 1}{\alpha + \beta} \tag{1.13}$$

Using the expression above, we can calculate the expected revenue-to-labor cost ratio from the counterfactual revenue-to-labor cost ratio, returns-to-scale as estimated in the data, and change in output estimated from bunching.

What we observe in the data is not only a mix of potential under-reporting and true production responses, but also firms whose output is unaffected by the threshold. We also see that the revenue to labor cost ratio is increasing in revenue instead of constant. The proportion of firms in the bunching region that are ”bunchers” is the definition of the bunching estimate. The average revenue-to-labor cost ratio we observe in the bunching region is an average across these bunchers and non-bunchers. To estimate the counterfactual ratio for non-bunchers in the bunching region, I estimate the relationship between revenue-to-input cost ratio and revenue and the deviation from this relationship in the bunching region as follows:

$$\frac{\hat{R}_{it}}{p^E E_{it}} = \beta_1 \hat{R}_{it} + \beta_2 \mathbf{I}(\hat{R}_{it} \in [r^{lb}, \bar{R}]) + \beta_3 \mathbf{I}(\hat{R}_{it} \in [\bar{R}, r^{ub}]) + \delta_t + \gamma_s + \eta_m + \mathbf{X}_{it} + \epsilon_{it} \quad (1.14)$$

where the dependent variable is the ratio of reported revenue to exempt or non-deductible input costs, the independent variables are reported revenue, a dummy for whether reported revenue is between the lower bound of the manipulation region and the exemption threshold $\bar{R}$ ($\mathbf{I}(\hat{R}_{it} \in [r^{lb}, \bar{R}])$), and a dummy for whether reported revenue is between the exemption threshold and the upper bound of the manipulation region, as estimated using the bunching method. Other controls include time, state and industry fixed effects ($\delta_t$,$\gamma_s$ and $\eta_m$), as well as a set of time-varying characteristics such as ownership and urban or rural sector. The theoretical framework I consider is a static setting but each observation is a firm in a given year so the specification includes time subscripts.

The empirical specification allows for a trend in the revenue to input cost ratio with respect to revenue even though a strict interpretation of the Cobb-Douglas (or more generally, CES) production, cost shares do not change with size. There are theoretical and econometric reasons to nonetheless expect a trend. First, the observed revenue to input cost ratio at a given level of revenue is an average of the ratio of all firms with that level of revenue. The share of registered firms could be increasing at any given level of revenue because within some commodity markets, probability of registration is increasing with productivity. This factor would bias against finding a real effect as it would lead to lower revenue to input cost ratios just below the threshold (as less productive firms are more likely to bunch). Second, true production could involve some fixed cost which would give rise to increasing revenue to input cost ratios. Third, measurement error in reported revenue would also result in an increasing trend. The second and third explanations do not affect the interpretation of deviations from trend near the exemption threshold.

Based on estimates of $\hat{\beta}_1$ and the fixed effects, we can predict the counterfactual ratio in the bunching region under the extremes of 100 percent misreport and real response. $\hat{\beta}_2$ tells us how much the observed ratio deviates from the counterfactual because of bunching and can be compared to the predicted ratios with and without evasion to reveal the extent of misreporting or real response.

## 1.6 Data and Descriptive Statistics

### 1.6.1 CenVAT rate data

The Central Board of Excise and Customs (CBEC), which administers the CenVAT publishes the CenVAT rates according to an 8-digit Indian Tariff Code (ITC) each year. Changes to the rates, if any, are usually announced in March of each year when the annual budget document for the central government of India is tabled in Parliament. However, there may be additional changes to rates or reclassifications, which are announced at other times in the year and published as Notifications from the CBEC. Using these various sources of information, I construct a novel dataset of of tax rates at the 8-digit ITC code from 2005 to present. Using a series of concordances, I link this tax rate information to detailed (5-digit) product information in an annual comprehensive survey of manufacturing establishments in India, the Annual Survey of Industries (ASI) [8].

### 1.6.2 Establishment-Level Production

I use annual data from the Annual Survey of Industries (ASI) between 2004 and 2015. This is a statutory survey administered by the Central Statistical Office (CSO) of the Government of India. It is a census of manufacturing establishments with at least a 100 workers and an approximately 15 percent random sample of manufacturing establishments with between 10 and 100 workers[9]. The ASI gathers balance sheet information about establishments including ownership structure, products manufactured, employees, fixed capital, and others. Some key variables from this data include annual establishment level revenue by 8-digit product code (gross and net of taxes and distribution costs), intermediate input costs, and electricity purchased and generated.

This data is not shared with the tax authority. Documents describing and evaluating the audit procedures of the Central Board of Excise and Customs (CBEC) never mention using data from the ASI as a source of third-party information (unlike other sources that are explicitly mentioned), suggesting they are unlikely to be used in an audit. However, the data are potentially entered by the establishment from their own records, which would

[8] The first five digits of the ITC code correspond to the international harmonized system codes or HS codes. There exists a correspondence between the HS codes and another international product classification system Central Product Classification (CPC) codes, which are then linked to the National Production Classification for Manufacturing Sector (NPCMS) codes used in the ASI from 2010 onward. The ASI provide a concordance between the NPCMS and the classification they use in earlier years, the Annual Survey of Industries Commodity Classification (ASICC-2009).

[9] There are some exceptions. All establishments in State X Industry cells with fewer than four establishments are included in the sample. The sampling probability is higher in a few states.

be available to the tax authority in case of an audit. Therefore, the firm may exhibit the same pattern of underreporting in the survey as they do in their own records.

This dataset contains 442,533 unique firm-year observations, and 820,987 firm-product-year observations because there are firms that produce multiple products. Although revenue is reported separately for each product, inputs and other firm-level variables are not. I apportion employment and input costs to each product produced by the firm according to its share in the total revenue of the establishment. The data pertain to establishments and not firms, but I treat them interchangeably because most are single-establishment firms. I clean the data using the procedure described in Appendix A.1.2, and end up with a sample of approximately 215,395 establishment-years, which excludes any establishments that closed over three years before the survey, are owned wholly or partially by a government entity or cooperative, are in states with area-based CenVAT exemptions, or have ever exported commodities. I also exclude observations which are severe outliers following a process used by Allcott, Collard-Wexler and O'Connell (2016). Most of the reduction in sample size is because I exclude establishments in exempt states and exporters, which I exclude because exports are zero-rated regardless of the commodity.

Most of the analysis in the paper focuses on establishments producing goods taxable at the standard CenVAT rate, which covers the majority of output and employment in the organized manufacturing sector [10]. Because the ASI is focused on manufacturing, most commodities (about 55 percent of observations) in the ASI fall into the standard CenVAT rate category (See Table 1.1). A large minority of commodities are exempt (about 21 percent of observations), and others are taxed at non-standard rates[11]. Overall output of taxable manufacturing commodities was about 86 percent of organized manufacturing output in 2005 and 84 percent of organized manufacturing output in 2012, and a similar proportion of employment in each year (82 percent in 2005 and 86 percent in 2012). I exclude petroleum from the analysis because petroleum producers do not receive input tax credits (and therefore the CenVAT is not a VAT for petroleum).

Each establishment can produce multiple products, which may not all belong in the same tax rate category. Multi-product establishments report revenue, distribution costs

[10]The ASI data is often referred to as data on the organized manufacturing sector as the frame for the ASI comes from factories that are registered under the Factory Act 1948. Firms can be registered under the Factory Act but unregistered with the tax authority. Firms that are unregistered under the Factories Act may still be registered with the tax authority. The organized sector as defined by registration under the Factories Act accounts for less than 20 percent of total manufacturing employment in India. The remaining firms are in the unorganized sector, which is covered in a similar but separate survey only in the years 2005 and 2010. I also combine data from these surveys for some parts of the analysis.

[11]As appendix tables 2 and 3 show, exempt industries are agriculture, manufacture of food products, publishing, and some primary stage products in non-metallic, leather and apparel industries.

and taxes remitted on each final product separately but do not separately report inputs. In such cases I apportion inputs to each product in proportion to its share in the total value of establishment output for analysis at the establishment-product level. I classify an establishment as a producer of standard CenVAT rate goods if at least 75 percent of its production value is taxable at the standard CenVAT rate. Results are similar if I change the definition to at least 90 percent of the production value taxable at the standard CenVAT rate. Most establishments that produce a good taxable at the standard rate, produce only goods that are taxable at the standard CenVAT rate (see Table 1.3)

There are area-based exemptions in the CenVAT, which in some cases exempts production in entire states such Himachal Pradesh or in designated manufacturing areas in other states. I exclude establishments in the 10 states and union territories where there are such special exemptions, which forms 9 percent of the total sample. Like most VAT systems, exports are zero-rated in the CenVAT, which means that even commodities that are taxable at the standard rate can remit zero tax on exports but receive all input tax credits. Starting in 2009, the ASI reports the share of an establishments output that is exported. I classify any establishment that has exported any of their output after 2009 as an exporter and exclude them from the analysis even in years prior to 2009. About 20 percent of establishment-years are exporters under this definition.

Although registration is not directly observable in the data, firms report CenVAT remitted on output. A suitable proxy for registration then is whether the firm reported remitting any CenVAT on output. Figure 1.3 plots the probability that a firm reports any CenVAT payment conditional on their gross revenue. As would be expected if there are firms that do not voluntarily register, there is a discrete jump in the probability of registration at the threshold from less than 10 percent to about 30 percent, continuing to rise with revenue. We would expect that the registration rate was a 100 percent above the threshold. However it is possible that firms are registered but do not report remitting CenVAT either because they only report amounts remitted above CenVAT credits or because their responses are incomplete. Still, the sharp increase in this proxy for registration suggests that the exemption threshold is a binding constraint.

## 1.7 Results

### 1.7.1 Bunching at the VAT Notch

The CenVAT induces bunching at the exemption threshold, confirming that at least some firms value the exemption and report output at or below the threshold. Figure 1.7

shows the revenue distribution of firms producing goods subject to the standard CenVAT rate between 2004 and 2007 when the exemption threshold was set at ₹10 million (approx. $150K). The y-axis shows the estimated number of firms (using survey weights) in green circles within ₹0.2 mn-wide revenue bins indicated on the x-axis. The solid black line is the counter-factual revenue distribution estimated using equation 1.10, where $r^{lb}$ was set to ₹7 mn and $r^{ub}$ was estimated as ₹13 mn. The region in between these two boundaries as indicated by the dashed vertical lines is the manipulation region. The observed revenue distribution clearly shows an excess mass relative to the counterfactual just below the exemption threshold, which is the solid red line at output of ₹10 mn.

That this excess mass is due to the CenVAT and the location of the exemption threshold is made apparent when the threshold shifts to ₹15 mn in 2008. From 2009 to 2015, there is no longer any excess mass below the original threshold of ₹10 mn and instead we see in Figure 1.8 that it has shifted to just below the new threshold. The manipulation region is now between ₹12 million and ₹20 mn. Finally, we see in Figure 1.9 that the revenue distribution is smooth around the exemption threshold for firms producing mainly goods that are exempt from CenVAT, providing further evidence that the bunching reflects a response to tax incentives.

I translate the excess mass shown in these figures into estimates of the decrease in reported revenue due to the CenVAT liability and compliance costs above the exemption threshold. Table 1.4 shows the bunching estimates for all establishments and for companies separately before and after the threshold change in 2008. All estimates are statistically significant at the 1 percent level. The bunching estimate of 5.3 for all establishments post 2008 translates to a reduction in output of about 7 percent on average across all firms producing goods taxable at the standard CenVAT rate. Average percent reduction in output is calculated by multiplying the excess bunching estimate with the bin size of ₹0.2 million to get the total reduction in output, and then dividing by the threshold level of ₹10 mn before 2008 and ₹15 mn after 2008 to get the percent change. Output reduction among all establishments at the old threshold before 2008 is similar – about 9.2 percent on average. Bunching is more pronounced among establishments owned by private or public limited companies before and after the threshold change. Output is reduced by about 18.6 percent on average among companies before the threshold change, and by 14 percent on average after the threshold changes.

This reduction in output might have been achieved either by reducing real revenue or by underreporting true revenue in the survey data. What firms report to the survey may not be identical to what they report to the tax authority, and there may be more underreporting of revenue in tax data. If firms underreport revenue to the tax authority but report truthfully in the survey data, we would not observe any bunching. Therefore, the estimated output response of firms in the ASI reflects either real production changes or underreporting in administrative and survey data. In the next section, I estimate to what extent this recorded reduction in output is due to underreporting of output in survey data.

### 1.7.2 Real or Reporting Behavior at the VAT Notch

The previous section described the total reported output response to the VAT exemption threshold. In this section I use the information on firms inputs to distinguish between real and reporting behavior in the context of the model presented in Section 1.4. I examine whether the revenue to input cost ratio is above or below trend in the bunching region.

Figure 1.10 is a binned scatter-plot of the revenue to labor cost ratio of firms. Each dot represents a conditional mean of the revenue to labor cost within revenue bins with an equal number of observations. The sample is restricted to all taxable goods producers between 2009 and 2015, and the dashed vertical line indicates the exemption threshold during this period, of ₹15 mn. The first thing to note is that the ratio of revenue-to-labor costs is increasing in revenue unlike in the stylized model where we focused on producers within a commodity market. The graph shows the average revenue-to-labor cost ratio of all producers. Accounting for the linear trend, we see that the revenue to labor costs rise just below the exemption threshold, consistent with a real response.

Table 1.8 shows the results of specification 1.14, which tests whether the revenue to input cost ratios of firms are systematically higher just below the exemption threshold, controlling for various firm characteristics like year of production, state, commodity and others. I find that revenue to labor cost ratios are systematically higher just below the exemption threshold. but not just above the threshold. For example, in column 5 controlling for year, state and product fixed effects, I find that revenue to input cost ratio is 2 units higher than what would be predicted by a linear trend in revenue. To give a sense of magnitude, the average revenue to labor cost ratio is about 12.

As a placebo check, Table 1.9 estimates the same specification on a sample of exporters and exempt goods producers. Column 2 shows the results for exporters and column 3 shows the results for exempt goods producers while column 1 repeats the estimates in column 5 of table 1.8. Comparing estimates between the three columns shows that not only is there no statistically significant deviation in the the revenue to labor cost ratio among the two samples where we see no bunching, but there is also no difference in the sign of the deviation above and below the threshold. These results suggest that the deviation we observe among taxable goods producers just below the threshold is due to the exemption threshold.

I estimate the average revenue-to-labor cost ratio we should expect to see for the bunching firms based on this data according to equations 1.11 and 1.13. Estimates of parameters used to calculate the revenue-to-labor cost ratio of bunchers under no evasion and with 100 percent evasion are given in Table 1.6. Estimates of the revenue-to-labor cost ratio of bunchers under different assumptions is given in Table 1.7. The observed ratio is a weighted average of the ratio of bunching firms and of firms whose optimal revenue would have been in the bunching region even in the absence of the tax. The proportion of bunchers as reported in Table 1.6 is 0.83, so the observed ratio of just the bunchers must be:

$$\frac{17.3 - 0.17 * 13.8}{0.87} = 18.01$$

where 17.3 is the observed revenue-to-labor cost ratio and 13.8 is the counter-factual revenue-to-labor cost ratio in the bunching region. The revenue-to-labor cost ratio of the bunching firms is actually slightly higher than the estimated revenue-to-labor cost ratio of bunching firms in the case without evasion and with returns to scale of 0.9 (see Table 1.7, suggesting that the output response is almost fully due to a true production decrease.

Given bunching estimates, we can estimate the revenue-to-labor cost ratio of bunching firms under full misreporting using equation 1.11. Estimates of each of the terms of the equation are given below along with the corresponding revenue-to-labor cost ratio.

In the case of a true production response, assuming returns-to-scale is approximately 0.95,[12] a 10 percent reduction in output, which is what we estimate on average, should result in a 0.05 percent increase in the revenue-to-input cost ratio as given by equation 1.13.

### 1.7.3 Response to Tax Kink vs Compliance Cost/ Enforcement Notch

Firms' response at the exemption threshold is due to the compliance costs and additional enforcement at the exemption threshold. The key piece of evidence for this is that the extent of bunching is similar among firms producing goods taxed at higher and lower rates. Figure 1.6 compares the revenue distribution of firms producing goods taxable at the standard rate and those producing goods taxable at the reduced rate. The extent of bunching by firms is similar in both sets of firms even though the reduced rate is between 4 to 6 percentage points lower than the standard rate. This similarity is apparent in the estimates

[12] For example, this is returns to scale estimated by Allcott, Collard-Wexler and O'Connell (2016) in the ASI data.

of excess mass shown in Table 1.5 where the output reduction for standard-rate taxable goods producers (8.2 percent) is similar to that of output reduction for reduced-rate taxable goods producers (11 percent), suggesting that the bunching is driven by compliance costs and additional enforcement at the exemption threshold rather than the tax kink.

Given an elasticity of output with respect to compliance cost of 2 (as estimated in Pakistan by Waseem (2018)) and assuming that about 50 percent of firms at the threshold voluntarily register for the VAT, the observed output response implies that compliance costs amount to about 5 percent of revenue on average for firms at the threshold. This is equivalent to a fixed compliance cost of approximately $\$14,000$ per firm.

## 1.8 Selection

Because firms can and choose to voluntarily register for the CenVAT, there is a concern that the observed difference in revenue-to-labor cost ratio in the neighborhood of the exemption threshold is because of the differences in the types of firms that bunch or choose to register. Section 1.4.2 showed that under a hicks-neutral production function, conditional on intensity of taxable intermediate input use and difference in price of output if registered and unregistered, more productive firms are more likely to register. Therefore we would expect that selection would result in higher revenue-to-labor cost ratio immediately above the exemption threshold, which is not what we find.

As shown in Liu, Lockwood and Almunia (2017), voluntary registration is more likely the greater the share of taxable intermediate inputs in total input costs, the greater the sales to businesses, and the more competitive the market. In their empirical setting, however, compliance is much higher and the likelihood of unregistered VAT chains is lower. What matters for registration is not just the share of taxable input costs or sales to businesses but more precisely, the share of inputs purchased from registered businesses and the share of sales to registered businesses. These determinants can be measured in the ASI at the establishment level based on each establishments' inputs and outputs. For example, the share of sales of an upstream firm to registered firms is estimated as the proportion of downstream firms that are registered among firms that use inputs produced by the upstream firm. Similarly, the share of inputs from registered firms used by a downstream firm is measured as the proportion of firms registered among producers of inputs used by the downstream firm.

Table 1.10 shows that these explanatory variables have the expected signs and are statistically significant in predicting registration. The dependent variable use any positive CenVAT reported as a proxy for registration. The likelihood of registration is increasing the share of downstream and upstream firms registered. The Lerner index, which is a measure of competitiveness of the firm's industry is not predictive in the Indian context. Overall, these results are similar to what Liu, Lockwood and Almunia (2017) find in the United Kingdom.

Conditioning on these determinants of voluntary registration, I still find that the revenue to labor ratio of firms is higher among the bunching firms, suggesting that there is a real reduction in production. Column (1) of Table 1.11 shows the results of specification 1.14 including controls for determinants of voluntary registration. $\hat{\beta}_2$ is estimated to be approximately 1.7 even controlling for selection.

There is still the possibility that some unobserved factors or peculiarity in the production process results in selection into bunching based on labor-specific productivity. To allow for this possibility, I condition on firms' revenue to labor cost ratio prior to 2008 before the threshold was newly set to ₹15 mn. The assumption I make here is that the firm's revenue to labor cost ratio prior to 2008 reflects the firms' true labor productivity and is unaffected by tax incentives beyond 2009. This is a reasonable assumption as there was uncertainty about the level of the new threshold even 6 months before it was changed.

Column (4) of Table 1.11 presents the results of specification 1.14 but uses the average revenue to labor cost between 2004 to 2007 as the dependent variable with revenue in the current year, which is between 2009 and 2015, as the independent variable. The coefficient on the dummy for the region just below the threshold is positive and statistically significant suggesting that the revenue to labor cost ratio of firms in the bunching region was higher than what would be predicted by the trend even before the exemption threshold was set. This result is consistent with some selection based on efficiency of labor use.

In column (3), the dependent variable is the current revenue to labor cost ratio, but includes a control for the previous revenue to labor cost share. We see that there is still a higher than predicted revenue to labor cost ratio just below the exemption threshold consistent with a real response.

These results suggest that there is some selection into bunching based on labor specific productivity of firms, although in the opposite direction as might be predicted by theory. It also implies that the total output response estimated from the excess mass of firms below the exemption threshold reflects a substantial real production response to the CenVAT. Even accounting for the increase in the observed revenue-to-labor cost due to selection, the revenue-to-labor cost ratio is higher than the counterfactual in the bunching region. The magnitude of this difference is consistent with 80 percent of the response being due to a real production change.

## 1.9 Conclusion

Some manufacturing firms in India whose potential revenue is in the neighborhood of the CenVAT exemption threshold limit production largely to avoid the compliance costs and additional enforcement associated with CenVAT registration. For other firms, the benefits of CenVAT registration outweigh the additional tax liability and costs so that registering is attractive. Consistent with findings in the United Kingdom, voluntary registration among Indian manufacturers is also predicted by the the share of upstream and downstream firms that are registered. On average across these firms that voluntarily register and those that choose to bunch at the threshold, the CenVAT reduces output by about 10 percent on average. Firms in the neighborhood of the CenVAT threshold that are affected by the threshold represent about 1 percent of total manufacturing output.

Revenue-to-labor cost ratios among the bunching firms are significantly higher than what would be predicted by extrapolating from the relationship between revenue-to-labor cost ratio and revenue outside the neighborhood of the exemption threshold. This finding suggests that, for at least 68 percent of bunching firms, the observed output response is due to real production changes rather than strategic under-reporting of revenue . Using revenue-to-labor cost ratio of firms between 2004-2007 when the threshold was at ₹10 mn as a proxy for true labor productivity of firms between 2009 and 2015 when the threshold was increased to ₹15 mn, I find that some of the difference in the revenue-to-labor cost ratio below the threshold might also be due to selection on labor productivity, such that more productive firms opt out of registration.

Finally, firms' output response seems to be due to the increase in compliance costs at the threshold rather than the increase in tax liability. Assuming an elasticity of output with respect to compliance cost of 2, I estimate that compliance cost due to the CenVAT amounts to about 5 percent of revenue for firms at the exemption threshold.

## 1.10 Tables

Table 1.1: Distribution of Establishments by CenVAT rate

| | **Number of Observations** | **Percent of Sample** |
|---|---|---|
| Standard | 215135 | 54.5 |
| Exempt | 81668 | 20.7 |
| Other CenVAT | 61934 | 15.7 |
| Other | 36034 | 9.1 |
| Exempt States | 62168 | 8.9 |
| *Establishment-Years* | 37568 | 9.1 |
| Exporter | 151720 | 21.8 |
| *Establishment-Years* | 80230 | 19.5 |

*Notes:* Unweighted counts, weighted proportion of sample. Annual data from 2004 - 2015. CenVAT rate categories are divided into ”Standard”, ”Exempt”, ”Other CenVAT”, and ”Other”. Goods falling into the ”Other” category are petroleum, tobacco etc., which sometimes have specific rates and are ineligible for input tax credits. ”Exempt States” are states where manufacturing in some regions or in the entire state are exempt from the CenVAT. Exports are zero-rated. Observations are Establishment X Product X Year, except for rows 6 and 8, which list Establishment X Year statistics.

Table 1.2: Establishment Characteristics

| | **Sole Proprietorship** | **Partnership** | **Public or Pvt. Limited Co.** |
|---|---|---|---|
| Median Revenue (Rs. Mn), 2005 | 3.865 | 8.934 | 39.738 |
| Median Revenue (Rs. Mn), 2015 | 9.531 | 19.495 | 89.481 |
| Avg. Share Output Taxable (%) | 68 | 67 | 78 |
| Share of ownership category below threshold (%) | 72 | 52 | 21 |
| Share below threshold in ownership category, 2005 (%) | 39 | 44 | 16 |
| Share below threshold in ownership category, 2015 (%) | 52 | 34 | 14 |
| *Establishment-Years* | 61328 | 71533 | 109948 |

*Notes:* Unweighted counts, weighted proportion of sample. Annual data from 2004 - 2015. Exemption threshold in 2005 was Rs. 10 mn, threshold in 2015 was Rs. 15mn.

Table 1.3: Input Costs as Share of Revenue

| | **Electricity** | **Labor** | **Exempt Intermediate Inputs** | **All Intermediate Inputs** |
|---|---|---|---|---|
| Average Cost as Share of Revenue | .03 | .086 | .398 | .624 |
| s.d. Across Products | .046 | .054 | .337 | .162 |
| Average Cost as Share of Revenue | .027 | .08 | .359 | .655 |
| s.d. Across Products | .051 | .047 | .324 | .152 |
| Number of Products with >100 obs | 321 | 324 | 98 | 320 |

*Notes:* Unweighted counts, weighted proportion of sample. Annual data from 2004 - 2015. Exemption threshold in 2005 was Rs. 10 mn, threshold in 2015 was Rs. 15mn.

Table 1.4: Estimates of Excess Mass

| | Pre-2008 | Post-2008 |
|---|---|---|
| All Firms | 4.559<br>(1.661) | 5.445<br>(.512) |
| Public and Private Limited Co. | 9.303<br>(1.55) | 10.588<br>(1.19) |

*Notes:* Bootstrapped standard errors in parentheses. Proportion of excess mass at threshold for all types of establishments in the first row, which includes sole proprietorships, partnerships, and public and private limited companies. Second row shows bunching only among public and private limited companies. Because before 2010, sole proprietorships and partnerships faced another compliance cost notch at ₹4 million, the estimates in column 1, row 1, use only the distribution above ₹4 million to construct the counterfactual and estimate bunching.

Table 1.5: Estimates of Excess Mass by Tax Category

| | Standard VAT Rate | Reduced VAT Rate |
|---|---|---|
| Post-2008 | 5.445<br>(.512) | 6.717<br>(1.433) |
| Pre-2008 | 4.559<br>(1.661) | 5.088<br>(2.098) |

*Notes:* Bootstrapped standard errors in parentheses. Average reduction in output in brackets. Because before 2010, sole proprietorships and partnerships faced another compliance cost notch at ₹4 million, the estimates in column 1, row 2, use only the distribution above ₹4 million to construct the counterfactual and estimate bunching.

Table 1.6: Parameter Estimates to Bound Real Response

| Parameter | Estimate |
|---|---|
| *Pct. Output Decrease* | .082 |
| *Avg. Counterfactual Ratio of Bunchers* | 16.97 |
| *Cov. bw. Ratio and Underreporting of Bunchers* | -.026 |
| *Proportion of Bunchers to Counterfactual Density* | .84 |

*Notes:* The percent change in output comes from bunching analysis - I translate the estimated excess mass to a corresponding output reduction. *Avg. Counterfactual Ratio of Bunchers* is the expectation of the revenue to labor cost ratio of bunching firms in equation 1.11 if there were no misreporting costs, which is identical to their ratio if there were no tax. *Cov. bw Ratio and Underreporting* is the covariance term in equation 1.11. Finally, *Proportion of Bunchers to Counterfactual Density* is the share of bunchers in the bunching region at the exemption threshold calculated from the estimate of excess mass at the threshold.

Table 1.7: Revenue-to-Labor Cost Ratio, Observed and Counterfactuals

| Revenue-to-Labor Cost Ratio | Estimate |
|---|---|
| *Bunchers, 100 % Evasion* | 15.6 |
| *Bunchers, No Evasion,* $\alpha + \beta = 0.95$ | 17.04 |
| *Bunchers, No Evasion,* $\alpha + \beta = 0.9$ | 17.12 |
| *Counterfactual at Bunching Region* | 13.8 |
| *Observed at Bunching Region* | 17.3 |

*Notes:* The percent change in output comes from bunching analysis - I translate the estimated excess mass to a corresponding output reduction. *Avg. Counterfactual Ratio of Bunchers* is the expectation of the revenue to labor cost ratio of bunching firms in equation 1.11 if there were no misreporting costs, which is identical to their ratio if there were no tax. *Cov. bw Ratio and Underreporting* is the covariance term in equation 1.11. Finally, *Proportion of Bunchers to Counterfactual Density* is the share of bunchers in the bunching region at the exemption threshold calculated from the estimte of excess mass at the threshold.

Table 1.8: Deviation From Trend in Revenue to Input Cost Ratio, 2009 to 2015

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $\mathbf{I}(\hat{R}_{\text{it}} \in [\bar{R}, r^{ub}])$ | 0.942*** | 0.082 | 0.062 | 0.090 | 0.290 | 11.177** |
| | (0.314) | (0.297) | (0.341) | (0.342) | (0.354) | (5.384) |
| $\mathbf{I}(\hat{R}_{\text{it}} \in [r^{lb}, \bar{R}])$ | 3.187*** | 1.724*** | 1.843*** | 1.749*** | 1.936*** | 7.435 |
| | (0.360) | (0.335) | (0.381) | (0.384) | (0.389) | (5.132) |
| Year FE | X | X | X | X | X | X |
| State FE | X | X | X | X | X | X |
| Industry FE | | X | X | | | |
| Class FE | | | | X | X | X |
| Class X Year FE | | | | | X | X |
| $R^2$ | 0.100 | 0.200 | 0.204 | 0.214 | 0.278 | 0.275 |
| N | 41365.000 | 41365.000 | 34749.000 | 34749.000 | 34147.000 | 31899.000 |

*Notes:* Standard errors in parentheses, * p<0.1, ** p<0.05 *p<0.01. Columns 1 - 5 show deviations from trend in revenue to labor cost ratios. Table presents coefficients on dummy for region above threshold upto the upper bound of manipulation region and a dummy for region below threshold starting from lower bound of manipulation region. Results are from estimation of specification in 1.14 with controls as indicated in each column. Average revenue to labor cost ratio is 12 and average revenue to electricity cost ratio is 50. Sample in columns 3 onward restricted to single-product establishments or multiproduct establishments whose products are all in the same product group or "class".

Table 1.9: Deviation From Trend in Revenue to Input Cost Ratio, 2009 to 2015, Placebo

| | (1) | (2) | (3) |
|---|---|---|---|
| $\mathbf{I}(\hat{R}_{\text{it}} \in [\bar{R}, r^{ub}])$ | 0.164 | 2.067 | -0.682 |
| | (0.359) | (1.387) | (1.317) |
| $\mathbf{I}(\hat{R}_{\text{it}} \in [r^{lb}, \bar{R}])$ | 1.886*** | 1.603 | -0.569 |
| | (0.393) | (1.433) | (1.489) |
| Year FE | X | X | X |
| State FE | X | X | X |
| Industry FE | | | |
| Class FE | X | X | X |
| Class X Year FE | X | X | X |
| Sample | Taxable | Exempt | Exporter |
| $R^2$ | 0.256 | 0.322 | 0.390 |
| N | 34539.000 | 5837.000 | 1682.000 |

*Notes:* Standard errors in parentheses, * p<0.1, ** p<0.05 *p<0.01. Table presents coefficients on dummy for region above threshold upto the upper bound of manipulation region and a dummy for region below threshold starting from lower bound of manipulation region. Results are from estimation of specification in 1.14 with controls as indicated in each column. Average revenue to labor cost ratio is 12 and average revenue to electricity cost ratio is 50. Sample is restricted to single-product establishments or multiproduct establishments whose products are all in the same product group or "class".

Table 1.10: Determinants of Registration

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Share Registered Downstream | 0.157*** | | | | 0.069*** | 0.019 |
| | (0.010) | | | | (0.010) | (0.012) |
| Lerner Index | | 0.009 | | | 0.001 | 0.045 |
| | | (0.031) | | | (0.036) | (0.039) |
| Share Registered Upstream | | | 0.208*** | | 0.139*** | 0.001 |
| | | | (0.008) | | (0.007) | (0.011) |
| Share of B2C Sales | | | | -0.123*** | -0.070*** | 0.003 |
| | | | | (0.011) | (0.011) | (0.038) |
| Sole Proprietor | | | | -0.199*** | -0.190*** | -0.026 |
| | | | | (0.008) | (0.008) | (0.024) |
| Partnership | | | | -0.165*** | -0.159*** | -0.026 |
| | | | | (0.008) | (0.008) | (0.024) |
| Distance to Threshold | | | | 0.003*** | 0.003*** | 0.002*** |
| | | | | (0.000) | (0.000) | (0.000) |
| Year FE | X | X | X | X | X | X |
| Firm FE | | | | | | X |
| $R^2$ | 0.008 | 0.001 | 0.024 | 0.178 | 0.190 | 0.892 |
| N | 89476 | 89738 | 89000 | 89196 | 88497 | 67629 |

*Note:* Standard errors in parentheses clustered by establishment, * $p<0.1$, ** $p<0.05$ *$p<0.01$. Time period restricted to 2010 to 2015. Dependent variable is whether the firm reports any positive CenVAT payment on output, which is the proxy for registration. Share registered downstream is defined as the proportion of firms who demand firm i's product that are registered. Share registered upstream is the proportion of firms producing goods demanded by firm i that are registered. Share of B2C sales is an industry-level measure of proportion of output in each industry directly consumed, as reported in Input-Output tables. Distance to threshold is the difference between reported revenue and the exemption threshold, in millions.

Table 1.11: Deviation From Trend in Revenue to Input Cost Ratio, 2009 to 2015, Selection

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| $\mathbf{I}(\hat{R}_{\text{it}} \in [\bar{R}, r^{ub}])$ | 0.472 | 0.164 | -0.697 | 1.036 | 10.949 | 6.042 |
| | (0.348) | (0.359) | (0.470) | (0.638) | (7.178) | (8.272) |
| $\mathbf{I}(\hat{R}_{\text{it}} \in [r^{lb}, \bar{R}])$ | 1.771*** | 1.886*** | 1.668*** | 1.571** | 3.221 | 6.336 |
| | (0.388) | (0.393) | (0.526) | (0.665) | (6.316) | (6.854) |
| Rev to Lab Cost , 2004-2007 | | | 0.314*** | | | |
| | | | (0.040) | | | |
| Rev to Elec Cost , 2004-2007 | | | | | 0.429*** | |
| | | | | | (0.033) | |
| Year FE | X | X | X | X | X | X |
| State FE | X | X | X | X | X | X |
| Industry FE | | | | | | |
| Class FE | X | X | X | X | X | X |
| Class X Year FE | X | X | X | X | X | X |
| Sample | Taxable | Taxable | Taxable | Taxable | Taxable | Taxable |
| $R^2$ | 0.293 | 0.256 | 0.397 | 0.321 | 0.396 | 0.309 |
| N | 34169.000 | 34539.000 | 15497.000 | 15510.000 | 13753.000 | 13948.000 |

*Note:* Standard errors in parentheses, * p<0.1, ** p<0.05 *p<0.01. Table presents coefficients on dummy for region above threshold upto the upper bound of manipulation region and a dummy for region below threshold starting from lower bound of manipulation region. Results are from estimation of specification in 1.14 with controls as indicated in each column. Column 2 includes a control for revenue to labor cost share from 2004 to 2007 and the dependent variable in column 3 is the revenue to labor cost ratio from 2004 to 2007. Average revenue to labor cost ratio is 12 and average revenue to electricity cost ratio is 50. Sample in columns 3 onward restricted to single-product establishments or multiproduct establishments whose products are all in the same product group or "class".

## 1.11 Figures

Figure 1.3: Probability of Any Excise Payment, 2009-2015

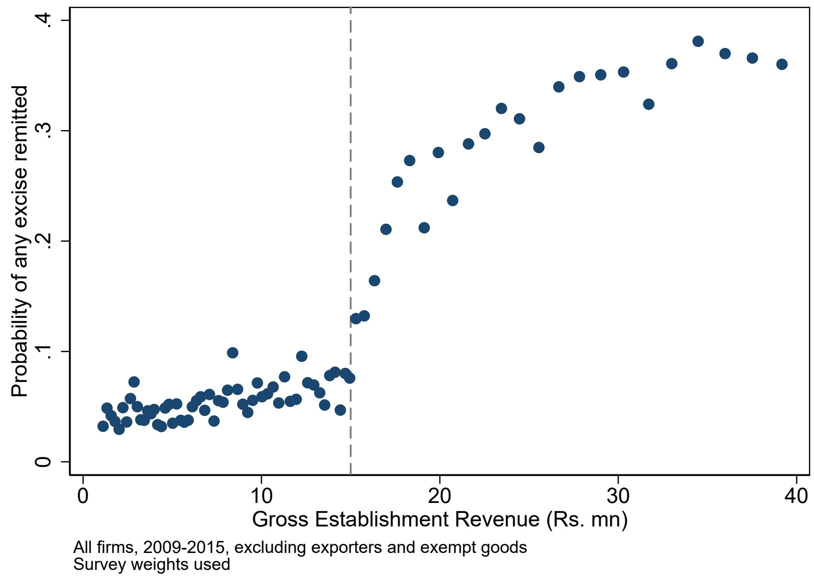



Figure 1.4: Revenue Distribution of Taxable vs Exempt Goods Producers, 2009 - 2015

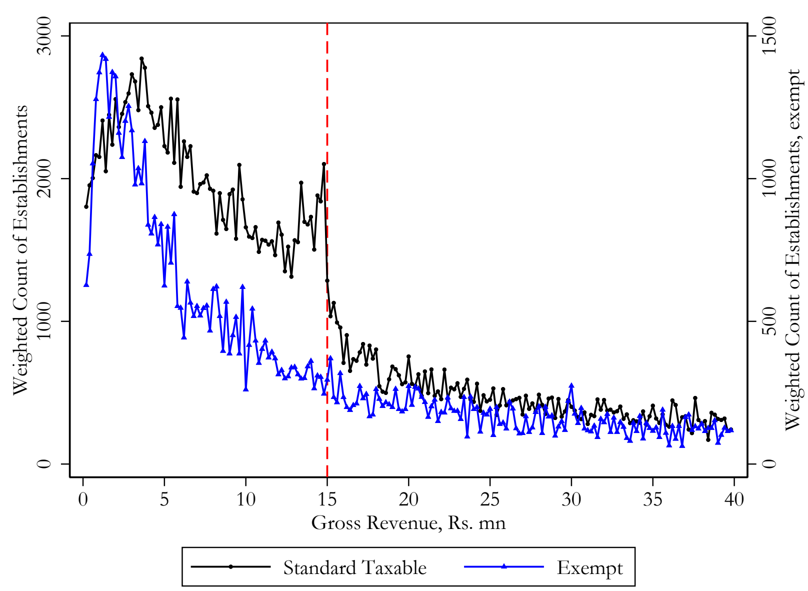


Figure 1.5: Revenue Distribution of Taxable Goods Producers, 2004-2007 and 2009 - 2015

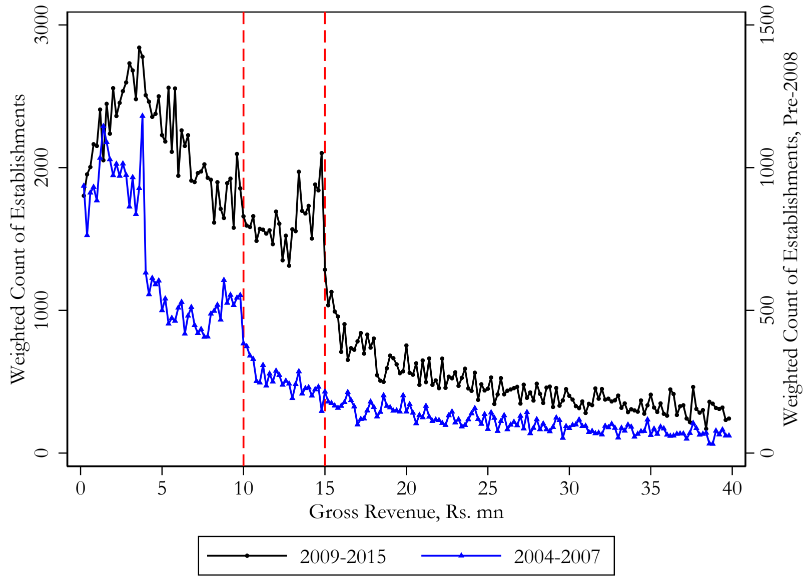



Figure 1.6: Revenue Distribution of Standard vs Reduced Rate Taxable Goods Producers, 2009 - 2015

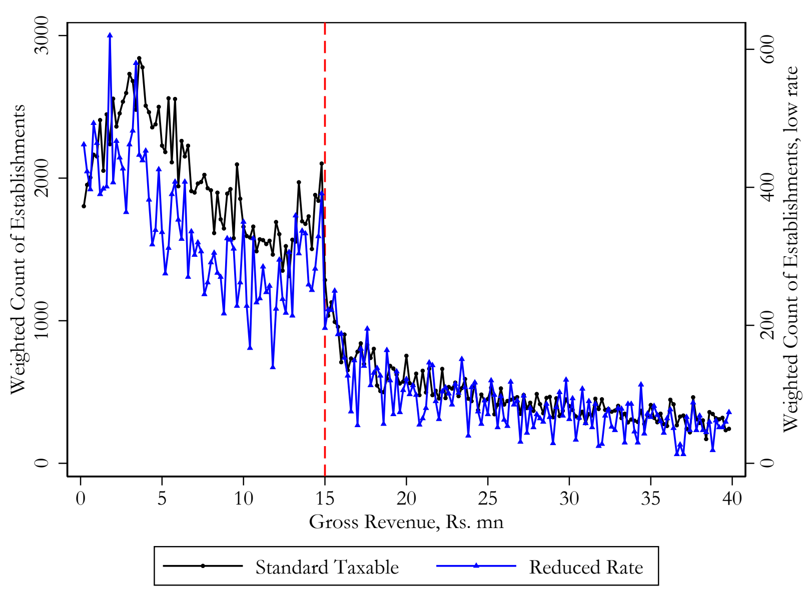


Figure 1.7: Bunching at the Exemption Threshold, 2004 to 2007

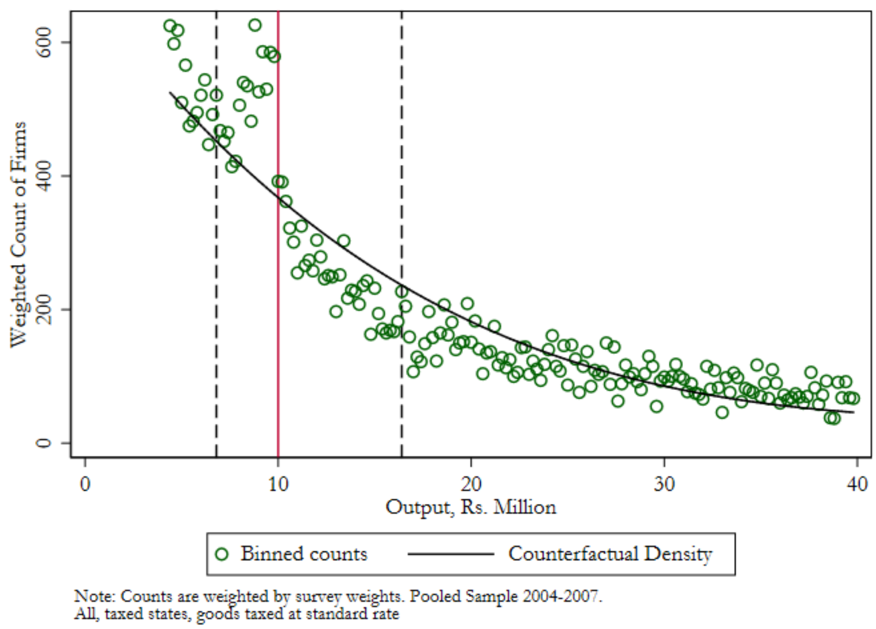



Figure 1.8: Bunching at the Exemption Threshold, 2010 to 2015

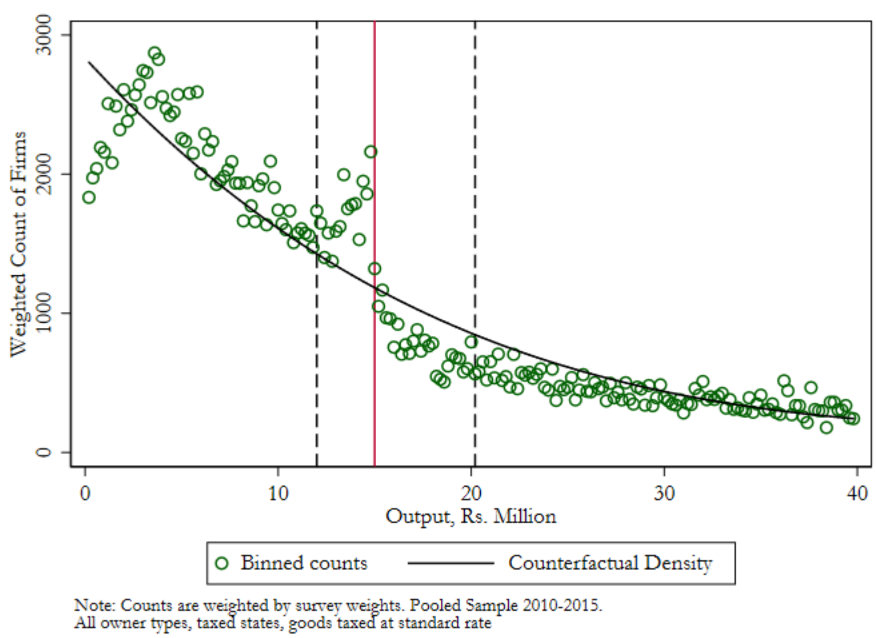


Figure 1.9: Bunching at the Exemption Threshold among Exempt Goods Producers, 2010 to 2015

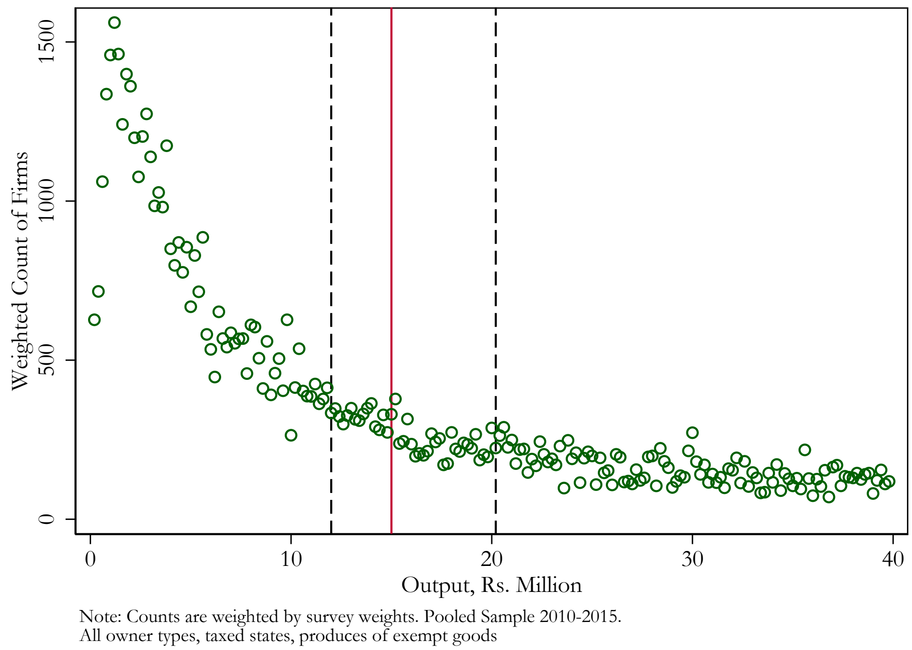
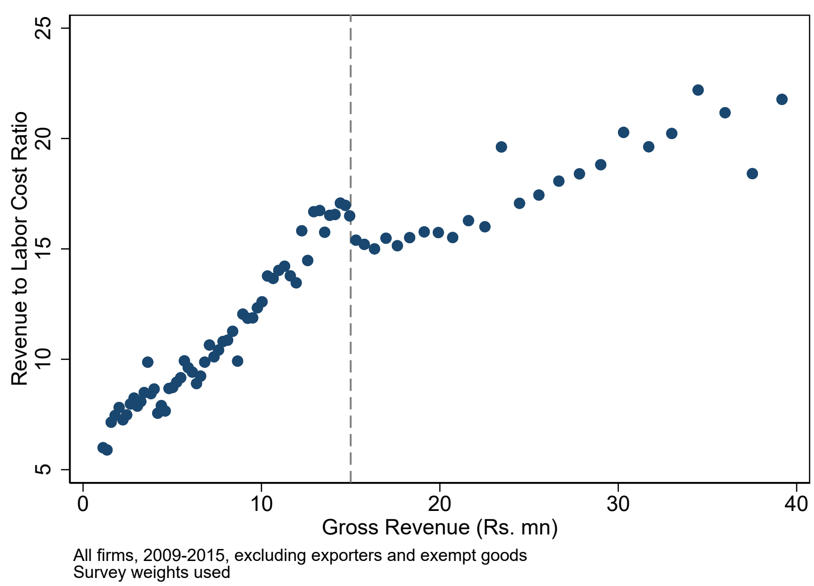



Figure 1.10: Revenue to Labor Cost Ratio, 2009 to 2015, All firms



# CHAPTER II

# Does the Elasticity of the Sales Tax Base Depend on Enforcement? Evidence from U.S. states' Voluntary Collection Agreements

**From a work with Yeliz Kacamak and Eleanor Wilking**

**Abstract**

Sales taxes are an important source of revenue for U.S. states. A key parameter that determines the marginal excess burden associated with this tax is the elasticity of the consumption tax base with respect to the tax rate. We study empirically how an important development in U.S. sales tax policy - the requirement of online retailers to remit the sales tax instead of the consumer - impacts this elasticity using quasi-experimental variation from the staggered state-wise introduction of Voluntary Collection Agreements (VCAs). Using detailed purchase data from the Nielsen Consumer Panel and monthly, zip-code level information on local sales tax rates, we find that consumers reduce their online expenditure after the introduction of VCAs, consistent with an increase in compliance with sales taxes on online sales and suggesting that consumers took note of the tax change. We test whether consumers are less responsive to sales tax rate changes and more responsive to sales tax holidays as a results. On average, we do not find evidence of an impact of the remittance rule change on the elasticity of the tax base with respect to the tax rate.

**JEL Codes**: H26
**Keywords**: Sales tax, Tax evasion, Online Retail, Remittance

## 2.1 Introduction

In standard economic models, demand for a taxed good does not depend on which side of the market remits the tax. However, recent theoretical and empirical literature casts doubt on this equivalence; for example, if the tax is less salient than the tax exclusive price at the point of decision (Chetty (2009*b*)), or if the tax can be mitigated by avoidance or evasion behavior in a way that a price increase could not (Slemrod and Yitzhaki (1996)). In the same vein, Slemrod and Kopczuk (2002*b*) argue that the elasticity of taxable income is not a structural parameter. Rather, response is conditional on various parameters of the tax system such as enforcement. In this paper, we explore whether the behavioral response of consumers to a consumption tax is affected by whether consumers have the responsibility to remit. We do so in the context of U.S. states' voluntary tax collection agreements (VCAs), which are also known as the "Amazon laws". The VCAs constitute a structural change in remittance assignment from consumers to retailers for online transactions, which substantially increased compliance with state sales taxes for online purchases. If consumers become less price-elastic as a result of this enforcement measure, states can potentially raise more revenue while lowering excess burden. This question has attained new relevance with the recent Supreme Court decision on *South Dakota v. Wayfair*, which allows states to require remote sellers to remit sales taxes due on purchases by their residents.

Existing literature has established that consumers purchase online in part to evade sales taxes. Goolsbee (2001) was the first to suggest this channel of evasion. Einav et al. (2014) find evidence of consumption tax evasion in consumers' online shopping response to taxes on the Ebay marketplace. Baker, Johnson and Kueng (2017) show that the internet is used as a means of evading the sales tax on a broader set of consumption goods, not just those subject to high sales and excise taxes. There is also evidence that the potential for evasion through online purchasing affects the price elasticity of taxed sales. Goolsbee, Lovenheim and Slemrod (2010) use data on state-level smoking rates and internet penetration from 1980 to 2005 to show that the price elasticity of taxed cigarette sales rose as ability to purchase cigarettes online increased. Consumer's online shopping response to the VCA would therefore shed light on whether this change in elasticity was because of the remittance structure and the associated ease of evasion, and to what extent the elasticity of a broader consumption tax base as opposed to particular goods like cigarettes is affected by remittance structure and tax compliance. One recent paper shows that the VCA agreements had a measurable impact on consumers' shopping behavior on Amazon. Baugh, Ben-David and Park (2018) show that this increase in tax on online purchases was salient to consumers, and that they reduced their Amazon purchases by about 9 %. We add to this evidence by examining consumer response in data that is a representative sample of the U.S population, and of online consumption at retailers other than Amazon. Furthermore, using detailed purchase data, we are able to separate the response in price and quantity and examine the effect of VCAs on the elasticity of the tax base.

Because the VCAs were negotiated separately by states, they were enacted and implemented at different times by different states. We exploit this temporal and geographic variation to test whether monthly online expenditure of households in states that enacted a VCA between 2010 and 2014 decreases following the VCA adoption. To understand the likely effects of VCAs on consumption elasticity, we first build a simple theoretical framework to predict what might happen to the elasticity of the effective tax base when tax-exclusive prices remain fixed and consumers choose to either purchase a commodity online or at a brick-and-mortar store. Next, we test the underlying assumptions and predictions of our framework using a large panel of household purchases from the Nielsen Consumer Survey. The rich information in the Nielsen data, which includes unique product identifiers, allows us to observe the elasticity of consumers' purchases with respect to tax changes at both online and brick-and-mortar retailers. Because there are very few instances of tax increases, particularly at the state-level following the VCAs (and possibly because of the revenue increase from the VCA), we test our hypotheses using sales tax holidays. We would expect that sales tax holidays are more important to consumers after the VCA when tax evasion through online purchasing is no longer feasible.

We show that consumers reduce their online taxable expenditure in response to a VCA, while maintaining consumption of tax-exempt items. Next, we decompose this reduction in total expenditure into a change in reported tax-exclusive prices of online goods and a change in quantity demanded by consumers. The decrease in tax-exclusive expenditure online comes from consumers who continue to purchase online, but switch to cheaper varieties and cheaper commodities; and from consumers who simply stop shopping online - an extensive margin response. Some households switch from purchasing the same products online to brick-and-mortar stores. Because online retailers typically price their goods for sale anywhere in the United States and the VCAs are implemented by state, it is reasonable that producers do not change their tax-exclusive price in response to a VCA and that any effective tax increase is passed through to the consumer. Finally, we show that the VCA did not significantly affect the tax elasticity of the consumption tax base.

## 2.2 Context

Forty-five U.S. states levy sales taxes on goods purchased for consumption within their physical borders, and require sellers in these transactions to assess and remit the tax. To mitigate the incentive to purchase products in low-tax jurisdictions, states with general sales taxes often levy parallel ”use taxes” on goods consumed in their states by their residents, but purchased outside the state or online.

Use tax provisions require residents to declare and self-assess the value of goods purchased elsewhere that would have been subject to sales tax if purchased in-state, and then to remit the equivalent sales tax amount to the state tax authority[1]. In theory, this minimizes revenue loss and distortion by equalizing after-tax prices. However, in practice, very few residents remit use taxes from either purchases made online, or those made in other states. Often, states require use-tax liability to be reported and remitted at the time of filing state income tax returns and these reports give us some indication of use-tax compliance. In 2012, the percent of income tax returns reporting use tax (i.e. reporting tax liability on online purchases) ranged from 0.2 percent in Mississippi to 10 percent in Maine[2].

### 2.2.1 Collecting Use Tax on Online Sales

States may impose a sales or use tax on purchases made by their residents, even if the retailer is out of state[3]. However, the state cannot legally impel the retailer to remit said tax unless there is a constitutionally sufficient relationship (a ”nexus”) between the retailer and the state[4].

As internet sales have grown in volume, states have used a variety of strategies to recoup uncollected use taxes without running afoul of the constitution’s nexus provision. Broadly, state actions can be divided into two categories; legislation, which tried to expand the definition of nexus to (large) online retailers, and voluntary collection agreements (VCAs), essentially agreements between a single retailer and the tax authority in which the retailer agreed to remit future sales tax in return for some benefit. Although collectively

[1]States differ in their procedure for remitting use taxes. Several states require residents to report and remit use taxes annually, frequently via state income tax return. However, Vermont requires residents to report and remit each month. Additionally, most states allow residents to deduct any sales tax that was paid in the source state.

[2]See report published by Maine’s tax authority: ¡http://www.house.leg.state.mn.us/hrd/pubs/usetax.pdf¿

[3]The nexus requirement arises from two provisions in the U.S. Constitution: the Due Process Clause and the Interstate Commerce Clause. In the seminal case on this issue, Quill v. North Dakota (1992), the Supreme court held that a nexus exists only if the online retailer has a physical presence in the state (such as a store, office, warehouse or employees) or, if the retailer has purposefully solicited the state’s residents.

[4]In addition to remittance, a state cannot impose any kind of ”tax duty” (such as, requiring the retailer to report sales information to the state tax authority.)

referred to in popular parlance as ”Amazon Laws”, this term is a misnomer; in most cases, states signed VCAs with Amazon and other large retailers either before or in conjunction with legislation.

Legislation, pioneered by New York and referred to as ”click-through nexus,” imposes a duty to remit sales taxes on any retailer with in-state affiliate or associate that directs residents to the retailer’s website[5]. This extended the duty to remit to large retailers such as Amazon or BackCountry, unless they dropped all affiliated sellers in the state that sold through their platform. In several states, Amazon initially dropped affiliates to avoid nexus (CO, NC, TN), but in large states with hundreds of affiliates, Amazon acknowledged nexus and began remitting. In our study period, four states (California, New Jersey, Pennsylvania and Virginia) passed such legislation.

Fourteen states announced VCAs with Amazon during our study period. In general terms, a VCA is a non-standard contract between a business and a state or local tax authority in which the business ”voluntarily” agrees to assess and remit taxes going forward, even if not legally required to do so. In the context of online sales, large retailers signed these agreements in exchange for some concession by the state, such as release from back taxes, or a commitment by the state not to require the retailer to disclose individual buyer data. For example, in July 2012, Amazon signed a VCA with the state of Texas promising to remit future taxes and to increase capital investment in the state. In exchange, the Texas State Comptroller agreed not to pursue collection of the estimated $269 million in sales tax that Amazon had not collected between 2005-2009.

Our design relies on variation in state sales tax rate, variation in VCA adoption (See Figure 2.1), and variation in the tax base to which the VCA applies (i.e. exemptions). Several states have also enacted temporary exemptions ”sales tax holidays” for specific product categories (e.g. school supplies), which we exploit for further variation.

Sales taxes in the United States are set by states and local option sales taxes at the county or city level supplement these standard rates. Sales tax exemptions can vary by state. In addition, some goods are taxed at a special discounted or higher rate. Some goods like alcohol and tobacco are also subject to additional excise taxes. We focus on goods taxed at the standard sales tax rate and exempt goods only for now, excluding items taxed at a special rate.

[5]The language of the 2008 New York statute creates a rebuttable presumption of nexus ”if the seller enters into an agreement with a resident of this state under which the resident, for a commission or other consideration, directly or indirectly refers potential customers, whether by a link on the Internet website or otherwise, to the seller.” N.Y. Tax Law

## 2.3 Data

Our analysis uses detailed consumption data from 2010 to 2014 from the Nielsen Consumer Panel, which is a nationally and regionally representative, stratified longitudinal panel of between 40,000-60,000 households[6]. We focus on the households observed between 2010 and 2014, which is the period when most VCA agreements were signed. Households self-report their purchasing behavior to Nielsen through in-home scanners for a set of ”Nielsen-tracked” products, which cover goods that represent about 30 percent of household consumption (as compared to total consumption in BLS statistics), including purchases made online. Households record their purchases from each shopping trip, which includes information on total amount spent, retailer type, payment type, value of each item purchased and quantity of each item purchased. Items are identified by a unique product code (UPC) with details on brand variation, size, multi-packs, etc. This detailed product and quantity information allows us to more accurately measure the impact of the VCA on consumer purchase behavior than existing studies because we can separate taxable goods from exempt. We also use this information to decompose the expenditure response into that on price and quantity demanded, showing the pass-through of the VCA.

Because the Nielsen-tracked product groups capture approximately 30 percent of total household consumption, our estimates of consumption elasticity with respect to the tax rate mainly reflect the purchase elasticity of this subset of household consumption rather than total household consumption. Notably, Nielsen emphasizes fast-moving consumer goods over durables like washing machines or cars. Therefore, our price elasticity estimates are likely to be smaller than the elasticity of total consumption since durables consumption is generally more elastic.

For tax rate changes, we use data on monthly sales tax rates at the state, county, and local (school district, etc.) level purchased from zip2tax. Table 2.1 shows the number of sales tax rate changes in our data at each administrative level. Most changes over this time period occur at the city level. One concern might be that the introduction of the VCAs changed how and when states change sales tax rates in a way that affects the elasticity estimate. For example, states may become less likely to increase rates because the VCAs raised revenue and therefore more likely to only increase rates when the local economy is faring poorly, which would lead to a higher estimated tax elasticity. We test for this policy endogeneity directly and find that there is only a small decrease in the probability that sales tax rates change after the VCA and no difference in the average magnitude of the change when they do. Table 2.2 shows that sales tax rates at the zip-code level change in

[6]The sample was increased from 40,000 to 60,000 in 2007.

about 1 percent of the zip-code months in our sample and that the average change is less than 0.2 percentage points when rates do change.

Using the Nielsen Consumer Panel, we construct a measure of total tax-exclusive expenditure at each household. Each shopping trip a household makes is assigned a retailer code and each retailer is assigned a ”channel type”. One of the channel categories is ”Online Retailer”, which allows us to distinguish online shopping trips[7]. We construct a measure of total monthly total online expenditure for each household by adding the reported item-level expenditure, which are exclusive of tax. Similarly, we measure total online taxable expenditure and exempt expenditure separately by adding up item-level expenditure of items within each category. To study the pass-through of the VCA, we measure UPC-level unit prices as the total price after any coupons divided by the quantity recorded for each purchase.

Finally, we turn to whether the reduction in online expenditure as a result of the VCA agreements, also translates to lower sensitivity of the effective tax base to sales tax changes. Assuming that use tax compliance prior to the VCA is zero and 100 percent afterward, we define the ”effective tax base” as brick-and-mortar expenditure prior to the VCA and the sum of brick-and-mortar expenditure and online expenditure after the VCA. This definition is intended to capture the expenditure that is most likely reported to the tax authority.

## 2.4 Model

In this section, we present a model of how VCA adoption affects online and offline consumption elasticities and the elasticity of the effective sales tax base.

We assume consumers' choice set consists of four types of goods: taxable online ($x_o$), taxable offline (brick and mortar) ($x_b$) goods; tax-exempt online ($e_o$) and tax-exempt offline ($e_b$) goods. Taxable goods are subject to an excise tax where $q_j$ is the after-tax unit price of good j and the before-tax price is denoted by $p_j$.

For the tax-exempt goods before and after-tax prices are always equal, i.e., $q_{e^k} = p_{e^k}, k = o, b$. Whereas, after-tax price of taxable online goods differ before and after the VCA adoption; prior to the VCA, online sales were effectively treated as tax-exempt, i.e. $q_{x_o} = p_{x_o}$; after the VCA, they were subject to sales tax, i.e. $q_{x_o} = p_{x_o}(1 + t)$. On the other hand, an ad valorem tax of $t$ is always effective for taxable offline goods, i.e.,

[7]Although the identity of individual retailers is unknown, we can identify ”large online retailers” through the volume, diversity and ubiquity of sales recorded on Nielsen. One retailer code is a generic ”Other” category but we believe we can identify this retailer code.

$q_{x_b} = p_{x_b}(1+t)$. We also assume that the tax-exclusive prices are fixed and exogenously determined, i.e. perfectly elastic supply curves, an assumption we will justify in the next section.

We first present an identity for tax elasticity of demand for taxable goods and then move to the consumer's problem. We conclude with three predictions that we can take to the data.

### 2.4.1 Tax Elasticity of Demand for Taxable Goods

We can calculate the tax elasticity of demand for taxable goods, given the setting- pre-VCA and post-VCA.

Total demand for taxable goods, $D_x$, is the sum of demand for taxable online goods, $D_{x_o}$, and taxable offline goods, $D_{x_b}$. So, the tax elasticity of demand for taxable goods, where $t$ denotes the tax rate, is:

$$\begin{aligned}\epsilon_{x,t} &= \frac{\partial D_x}{\partial t}\frac{t}{D_x} = (\frac{\partial D_{x_o}}{\partial t} + \frac{\partial D_{x_b}}{\partial t})\frac{t}{D_x} = \frac{\epsilon_{x^0,t}D_{x_o} + \epsilon_{x_b,t}D_{x_b}}{D_x} \\ &= \epsilon_{x^0,t}\theta + \epsilon_{x_b,t}(1-\theta)\end{aligned} \tag{2.1}$$

Where $\theta = \frac{D_{x_o}}{D_x}$ denotes the online demand for the product as a share of the total demand. The smaller the $\theta$ is, the closer tax elasticity of total demand is to tax elasticity of demand for offline products. $\theta$ is also directly affected by the tax rate, whether the VCA is in place, the relative price of the good online and offline, as well as consumers' relative preference for online and offline purchasing. We present a simple model below that illustrates how $\theta$, $\epsilon_{x_o,t}$, and $\epsilon_{x_b,t}$ might change as a result of the VCA.

### 2.4.2 Consumer's Problem

We use a nested CES utility to represent consumer's preferences. Using a nested model rather than a regular CES model allows us to have a different elasticity of substitution within goods (online and offline) and across goods (taxable, exempt). However, we assume that the elasticity of substitution within goods is the same across goods. In other words, the elasticity of substitution between online and offline goods, given a type of good, i.e. taxable or tax-exempt, is the same.

$$X(x_o, x_b) = (\psi {x_o}^{\gamma} + (1-\psi){x_b}^{\gamma})^{\frac{1}{\gamma}} \tag{2.2}$$

$$E(e_o, e_b) = (\psi {e_o}^{\gamma} + (1-\psi){e_b}^{\gamma})^{\frac{1}{\gamma}} \tag{2.3}$$

$$U(x_o, x_b, e_o, e_b) = \left((1-\alpha)E\left(e_o, e_b\right)^{\rho} + \alpha X\left(x_o, x_b\right)^{\rho}\right)^{\frac{1}{\rho}} \tag{2.4}$$

Then the consumer problem can be stated as:

$$\max_{x_o, x_b, e_o, e_b} U(x_o, x_b, e_o, e_b) = \left(\alpha\left(\left(\psi\left(x_b\left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} + (1-\psi){x_b}^{\gamma}\right)^{\frac{1}{\gamma}}\right)^{\rho}\right.$$
$$\left. + (1-\alpha)\left(\left(\psi\left(e_b\left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} + (1-\psi){e_b}^{\gamma}\right)^{\frac{1}{\gamma}}\right)^{\rho}\right)^{\frac{1}{\rho}}$$

such that

$$q_{x_o}x_o + q_{x_b}x_b + q_{e_o}e_o + q_{e_b}e_b \leq I. \tag{2.5}$$

Where $x_o$ and $x_b$ represent composite taxable online and brick-and-mortar goods, respectively and $e_o$ and $e_b$ represent composite tax-exempt online and brick-and-mortar goods. $I$ denotes the income and $q_j$ is the after-tax unit price of good j and the before-tax price is denoted by $p^i_j$.

A simplifying assumption we are making is that offline and online versions of the taxable and exempt goods are substitutes. This should hold generally -any individual consumer is not likely to purchase the same good both online and offline.

We can do sequential maximization where we can define $x_o$ and $e_o$ in terms of $x_b$ and $e_b$ respectively.

$$\begin{aligned} x_o &= x_b\left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}} \\ e_o &= e_b\left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}} \end{aligned} \tag{2.6}$$

Substituting these expressions into the utility function and the budget constraint will produce the following reduced problem.

$$\begin{aligned} &U(x_b, e_b) = \left(\alpha\left(\left(\psi\left(x_b\left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} + (1-\psi){x_b}^{\gamma}\right)^{\frac{1}{\gamma}}\right)^{\rho}\right. \\ &\left. + (1-\alpha)\left(\left(\psi\left(e_b\left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} + (1-\psi){e_b}^{\gamma}\right)^{\frac{1}{\gamma}}\right)^{\rho}\right)^{\frac{1}{\rho}} \\ &\text{such that} \\ &x_b\left(q_{x_o}\left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}} + q_{x_b}\right) + e_b\left(q_{e_o}\left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}} + q_{e_b}\right) \leq I. \end{aligned} \tag{2.7}$$

To simplify the problem, define the following constants

$$
\begin{aligned}
s_{x_b} &= \left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}} \\
s_{e_b} &= \left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}} \\
z_{e_b} &= \left(\psi\left(\left(\frac{q_{x_o}(1-\psi)}{q_{x_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} - \psi + 1\right) s_{x_b}{}^{\frac{1}{\gamma}} \\
z_{e_b} &= \left(\psi\left(\left(\frac{q_{e_o}(1-\psi)}{q_{e_b}\psi}\right)^{\frac{1}{\gamma-1}}\right)^{\gamma} - \psi + 1\right) s_{e_b}{}^{\frac{1}{\gamma}}
\end{aligned}
\tag{2.8}
$$

And finally define new prices

$$
\begin{aligned}
r_{x_b} &= q_{x_o} s_{x_b} + q_{x_b} \\
r_{e_b} &= q_{e_o} s_{e_b} + q_{e_b}
\end{aligned}
\tag{2.9}
$$

The consumer problem can be stated in the simplified CES form

$$
\begin{aligned}
&U(x_b, e_b) = \left(\alpha\left(x_b s_{x_b}\right)^{\rho} + (1-\alpha)\left(e_b z_{e_b}\right)^{\rho}\right)^{\frac{1}{\rho}} \\
&\text{such that} \\
&x_b r_{x_b} + e_b r_{e_b} \leq I.
\end{aligned}
\tag{2.10}
$$

Solving for $x_b$ and $e_b$ and substituting them into previously defined $x_o$ and $e_o$ provides us with the following Marshallian demand functions:

$$
\begin{aligned}
x_o &= \frac{s_{x_b} I \left(\frac{\alpha}{r_{x_b}}\right)^{\sigma}}{s_{x_b}\left(\alpha^{\sigma} r_{x_b}^{1-\sigma} + (1-\alpha)^{\sigma} r_{e_b}^{1-\sigma}\right)} \\
x_b &= \frac{I \left(\frac{\alpha}{r_{x_b}}\right)^{\sigma}}{s_{x_b}\left(\alpha^{\sigma} r_{x_b}^{1-\sigma} + (1-\alpha)^{\sigma} r_{e_b}^{1-\sigma}\right)} \\
e_o &= \frac{s_{e_b} I \left(\frac{1-\alpha}{r_{e_b}}\right)^{\sigma}}{z_{e_b}\left(\alpha^{\sigma} r_{x_b}^{1-\sigma} + (1-\alpha)^{\sigma} r_{e_b}^{1-\sigma}\right)} \\
e_b &= \frac{I \left(\frac{1-\alpha}{r_{e_b}}\right)^{\sigma}}{z_{e_b}\left(\alpha^{\sigma} r_{x_b}^{1-\sigma} + (1-\alpha)^{\sigma} r_{e_b}^{1-\sigma}\right)}
\end{aligned}
\tag{2.11}
$$

Where $\sigma = \frac{1}{1-\rho}$ is the elasticity of substitution between the taxable and tax-exempt goods.

### 2.4.3 Predictions

Comparative statistics yield three testable predictions relevant to the effect of the policy on consumption elasticities:

If the VCA increases sales tax compliance for online purchases, the tax elasticity of online taxable goods changes sign and becomes negative. Using the Marshallian demand functions we can calculate the tax elasticity of taxable goods before and after the VCA. Let $\epsilon^{pre}_{x_o,t}, \epsilon^{post}_{x_o,t}$ denote elasticity of taxable online goods before and after respectively.

$$\begin{aligned}\epsilon^{pre}_{x_o,t} = -\frac{t}{1+t}(&\frac{tz_{x_b}^{-\gamma}(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}\left(z^{\gamma}_{x_b}\left(r_{x_b}-\gamma p_{x_b}(t+1)\right)+r_{x_b}(\psi-1)\right)}{(1-\gamma)r_{x_b}\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)}\\ &+\frac{r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\left(\sigma z^{\gamma}_{x_b}\left(r_{x_b}-\gamma p_{x_b}(t+1)\right)+r_{x_b}(\psi-1)\right))}{(1-\gamma)r_{x_b}\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)}) > 0\end{aligned} \tag{2.12}$$

Notice that the denominator of the second term is always positive. The numerator can be positive or negative depending on the level of substitution between online and brick and mortar goods. However because online and offline goods are substitutes, the pre-VCA tax elasticity of taxable online goods is positive.

Now consider the after VCA tax elasticity of $x_o$ where $q_{x^0} = p_{x_o}(1+t)$

$$\epsilon^{post}_{x_o,t} = -\frac{t}{1+t}\frac{\alpha^{\sigma}r^{1-\sigma}_{x_b}+\sigma(1-\alpha)^{\sigma}r^{1-\sigma}_{e_b}}{\alpha^{\sigma}r^{1-\sigma}_{x_b}+(1-\alpha)^{\sigma}r^{1-\sigma}_{e_b}} < 0 \tag{2.13}$$

Tax elasticity of brick and mortar taxable goods is negative before and after the VCA but becomes smaller in magnitude post-VCA

Similar to the taxable online goods case, let $\epsilon^{pre}_{x_b,t}, \epsilon^{post}_{x_b,t}$ denote tax elasticity of taxable brick and mortar goods before and after respectively.

Prior to VCA, tax elasticity is as follows:

$$\begin{aligned}\epsilon^{pre}_{x_b,t} = -\frac{t}{(1+t)}\Bigg[&\frac{z^{-\gamma}_{x_b}\left(\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}\sigma(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)\left(-\gamma p_{x_b}(t+1)+r_{x_b}\right)\right)}{(1-\gamma)r_{x_b}\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)}\\ &+\frac{\psi s^{\gamma}_{x_b}\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)}{(1-\gamma)r_{x_b}\left(r_{x_b}\alpha^{\sigma}r^{\sigma}_{e_b}+r_{e_b}(1-\alpha)^{\sigma}r^{\sigma}_{x_b}\right)}\Bigg] < 0\end{aligned} \tag{2.14}$$

After the VCA, a change in the tax rate does not change the relative price of online and offline products, and so it does not affect the share of online demand. The demand elasticity for online goods is the same as that of offline goods.

$$\epsilon^{post}_{x_b,t} = \epsilon^{post}_{x_o,t} = -\frac{t}{1+t}\frac{\alpha^\sigma r^{1-\sigma}_{x_b} + \sigma(1-\alpha)^\sigma r^{1-\sigma}_{e_b}}{\alpha^\sigma r^{1-\sigma}_{x_b} + (1-\alpha)^\sigma r^{1-\sigma}_{e_b}} < 0 \tag{2.15}$$

The first expression is smaller in magnitude than the second expression. This is in accord with standard models: $\epsilon_{(x_b,t)}$ is generally negative prior to the VCA since an increase in the local sales tax rate would induce individuals to switch to purchasing online, or to demand less offline.

After the implementation of the VCA, $\epsilon_{(x_o,t)}$, should become negative since an increase in the tax rate would also increase the relative after-tax price of online goods. $\epsilon_(x_b,t)$ will become smaller in magnitude as individuals will no longer switch from purchasing offline to online. How these changes in demand elasticity for online and offline products affects overall elasticity will depend on the relative importance of the online and offline demand for the product as well as the magnitude of the change in elasticity.

The elasticity of the effective tax base, defined as the value of goods on which tax is remitted, becomes smaller in magnitude after VCA.

We define the effective tax base as purchases reported to the tax authority (and on which tax is remitted). Prior to the VCA, the base is simply the offline purchases as almost no online purchase is reported. After the VCA we assume full compliance on both online and offline purchases. Therefore, the effective tax base is now both online and offline purchases. Post VCA the tax-elasticity of the online and offline tax base is identical and the elasticity of the effective tax base is equal to $\epsilon^{post}_{x_b,t} = \epsilon^{post}_{x_o,t}$. Prior to the VCA, the base is equal to offline expenditure and therefore the elasticity is $\epsilon^{pre}_{x_b,t}$.

Therefore, by Proposition 2.4.3 we know that the elasticity of tax base became smaller in magnitude.

## 2.5 VCA Effect on Online Prices and Consumption

In this section, we establish several empirical facts assumed by our model. We find that the VCA decreased online expenditure by households, particularly at large online retailers, consistent with what we would expect following an increase in the after-tax price. We also show direct evidence that VCAs substantially increased the number of online purchases on which sales taxes were collected by online retailers. Together, these findings suggest that use-tax compliance was low prior the VCA and that compliance increased once retailers were required to remit. They also suggest that the resulting change in the tax-inclusive price (as the effective tax increase was fully passed-through) was noticed by the consumers, who decreased online consumption.

### 2.5.1 More Online Goods Are Taxed at Point of Sale After the VCA

Given that compliance with use taxes were low as we argued in Section 2.2 and that the tax enforcement measures are not always effective, we first verify that the VCA had the intended effect of raising compliance on retail taxes on online sales. To measure compliance, we test whether there is more likely to be a difference between the pre-tax and after-tax price post VCA. Nielsen records expenditure in two variables - *item-level* expenditure and *trip-level* expenditure. The *trip-level* expenditure is always tax-inclusive while the *item-level* is tax-exclusive[8]. If no sales tax is collected at the point of transaction, the aggregate of all item expenditures for a given trip will equal the *trip-level* expenditure. If the VCA induced retailer remittance, we expect the fraction of online transactions where no sales tax was apparently collected to fall.

Figure 2.2 shows that this is indeed the case. After restricting the data to trips in which only taxable items were purchased, we separately plot the share of trips where the sum of the *item-level* expenditure equals the *trip-level* expenditure for online and brick-and-mortar purchases, relative to the time of VCA adoption. Prior to the VCA agreements, about 25 percent of online trips have no tax collected, whereas only about 12 percent of offline trips have no tax collected (or report *item-level* tax-inclusive expenditures). We see a sharp drop in this fraction for online trips immediately following state-level VCA adoption, suggesting that online retailers began collecting sales taxes soon after implementation of the VCA.

Having established that online retailers remitted after the policy, we now turn to the consumer response. Standard incidence theory, which assumes full salience and compliance, would predict that shifting the remittance duty from the consumer to the retailer should have no effect on equilibrium quantities and prices. However, if, as we suspect, compliance with use taxes was low, for most consumers the policy increased the tax-inclusive price of online goods[9].

### 2.5.2 Consumers Reduced Total Online Expenditure on Taxed Goods

We estimate the effect of the policy on online purchasing behavior by estimating the following difference in difference specification:

$$Y_{hm} = \beta_0 + \beta_1 \text{Ever VCA}_h * \text{Post VCA}_{hm} + \beta_2 X_{hm} + \gamma_m + \delta_h + \epsilon_{hm} \tag{2.16}$$

[8]We discuss this crucial aspect of the data in detail in the data appendix.

[9]The exact amount that after tax prices increase depends on relative demand and supply elasticities, but, as most Nielsen tracked products are commodities, we think 0% pass through is unlikely.

where $Y_{hm}$ is either (1) total online taxable expenditure or (2) total online exempt expenditure of household $h$ in month $m$. The impact of the VCA on expenditure is captured by $\beta_1$ where *Ever VCA*$_h$ indicates whether the household $h$ is in a state that adopts a VCA between 2010 and 2014 and *Post VCA*$_{hm}$ is an indicator for whether we are observing household $h$ in a month $m$ following adoption of VCA in that state. We also control for time fixed effects ($\gamma_m$) and household fixed effects ($\delta_h$), as well as time-varying area-level characteristics ($X_{hm}$) such as a local cost of living index[10]. If the parallel trends assumption holds - that is, if the online purchasing habits of households in states that did not adopt VCAs seem to be a suitable counterfactual for the purchasing habits of households in states that adopted VCAs - then this parameter represents the difference-in-differences estimator of the effect of VCA adoption on the extensive and intensive margin of online sales. We would expect that online expenditure on taxable items falls as a result of the VCA, but that online expenditure on exempt items does not change.

We find that the introduction of the VCA reduced total monthly tax-exclusive expenditure online by about 8 percent relative to the mean (Table 2.3, column 1), about 25 cents per month. In contrast, there is close to no change in the purchase of exempt goods online (column 4). Nearly all of this decrease in online expenditure occurred at large retailers (column 2) and not at small retailers (column 3). Total monthly expenditure at brick and mortar stores increase by about one dollar but these effects are not statistically significant. Figure 2.3 shows, there is no anticipatory effect of the VCA in the quarter before and the parallel trends assumption holds. Figures showing parallel trends in other outcome variables are shown in Appendix **??**.

### 2.5.3 Pass-Through to Consumers

We decompose the decrease in tax-exclusive online expenditure into the change in the tax-exclusive price of goods and change in consumer demand using the following specification:

$$\log(P_{cmu}) = \beta_0 + \beta_1 \text{Ever VCA}_c * \text{Post VCA}_{cm} + \gamma_m + \delta_u + \alpha_c + \epsilon_{cmu} \tag{2.17}$$

where the coefficient of interest is again $\beta_1$, which represents the average percent change in the tax-exclusive price across all products due to the VCA. $P_{cmu}$ is the unit price of UPC-level commodity $u$ in county $c$ in month $m$. We control for month, upc and county-level fixed effects ($\gamma_m, \delta_u, \alpha_c$). This is similar to our baseline specification 2.16, except that the unit of observation is a purchase within households.

[10]We create this measure following steps outlined in Baugh, Ben-David and Park (2018)

Similarly, we test the effect on consumer demand by estimating specification 2.17 with *Log(quantity demanded)* as the dependent variable. The coefficient of interest then is the estimate of average percent change in quantity demanded for product, conditional on purchase (i.e. intensive margin effect on quantity). The drawback of this specification is that a null effect could be consistent with three different interpretations: (1) Consumers do not reduce their quantity demanded on most goods, conditional on online purchase, as a result of the VCA, (2) Consumers reduce their quantity demanded of higher price goods and substitute to purchasing lower price goods (therefore increasing quantity demanded of these goods). On average, this would translate to no measured effect on quantity demanded. For example, if consumers switch from a higher-priced variety of household cleaner to a lower-priced variety, this would appear on average as no measured change in quantity demanded across UPC. Or, if consumers decide not to purchase an expensive kitchen appliance and instead spend more of their budget on other lower priced items - they would have decreased quantity demanded in one UPC but increased demand for another. (3) Consumers only respond on the extensive margin, i.e. they stop purchasing any amount of the product online.

To distinguish between (1) and (2), we examine the effect of the VCA on quantity interacted with the average price of each UPC across purchases from all states in 2011, a year in which no state introduced a VCA. This price is by definition, unaffected by the VCA. In this way, we can examine heterogeneous effects on demand due to the VCA across high and low-price commodities.

$$\begin{aligned}\log(Q_{cmu}) =& \beta_0 + \beta_1 \text{Ever VCA}_c * \text{Post VCA}_{cm} * P_{2011cu} \\ &+ \beta_2 \text{Ever VCA}_c * P_{2011cu} + \beta_3 \text{Ever VCA}_c * \text{Post VCA}_{cm} \\ &+ \gamma_m + \delta_u + \alpha_c + \epsilon_{cmu}\end{aligned} \tag{2.18}$$

Now $\beta_1$ measures the average decrease in consumer demand across UPC, scaled by the price of each UPC. If consumers behave as described in (2), we would expect $\beta_1$ to be negative. On the other hand, if consumers behave as described in (1), we would expect $\beta_1$ to be zero.

#### 2.5.3.1 Effect of VCA on tax-exclusive prices.

We find that tax-exclusive prices do not change after the VCA. Table 2.4 decomposes the effect on total online expenditure into the effect on prices and quantity separately. Columns 3-6 shows the effect on log of prices. We find that the VCA reduced prices by 0.9

percent, but this reduction is coming mostly from purchases of video products. We find no evidence of a statistically significant change in the tax-exclusive price of most goods purchased online, suggesting that any effective tax increase due to the VCA was fully passed through to consumers.

#### 2.5.3.2 Quantity Purchased Online - Intensive Margin

Columns 1-3 in Table 2.4 show no effect of the VCA on quantity, which is surprising at first given that we see a decrease in total expenditure. We find no evidence of an intensive margin effect on quantity on average. That is, conditional on an online purchase, we do not see a decrease in quantity on average across all commodities. However, as discussed, this result could be consistent with a decrease in quantity purchased of some goods and an increase in quantity purchased of others. For example, if consumers substituted away from a more expensive to a less expensive variety, we would not find evidence of a decrease in quantity on average. Table 2.5 shows the heterogenous impact on quantity demanded by price of the commodity prior the VCA as a proxy for quality. Column 1 shows that quantity demanded decreases as a result of the VCA by more for higher price taxable goods, suggesting that consumers substitute away from higher price varieties to lower price varieties or lower price goods.

## 2.6 VCA Effect on Tax Elasticities

To test the impact of the VCA on elasticity of the consumption tax base with respect to the sales tax rate, we augment specification 2.16 by adding interaction terms for the sales tax rate application at the household's zip-code. This is the tax rate that is likely to prevail at most of the household's brick-and-mortar consumption and is the tax rate applied on online sales. We estimate the following specification at the household-month level:

$$\begin{aligned}\Delta log(e_{ht}) = \beta_0 + \beta_1 \Delta\tau_{ht} + +\beta_2 \Delta\tau_{ht} * \text{Ever VCA}_h + \beta_3 \Delta\tau_{ht} * \text{Post VCA}_{ht} \\ + \beta_4 * \text{Post VCA}_{ht} + \pi X_{ht} + \gamma_h + \gamma_t + \epsilon_{ht}\end{aligned} \quad (2.19)$$

The tax elasticity of the tax base in untreated states is estimated captured by $\beta_1$ while $\beta_2$ captures any difference in this elasticity in states that ever adopt the VCA, and $\beta_4$ represents the tax rate-invariant effect of VCA adoption on the base. The coefficient of interest, $\beta_3$, captures how a VCA impacts the tax elasticity of the base[11]. $X_{ct}$ is a vector of time-varying county-level controls, including the unemployment rate. Household and time fixed effects are included to control for any time-invariant household characteristics and time trends, respectively.

Specifying at the household-month level has two advantages: we can include household effects which absorb idiosyncratic variation in expenditures within a household, thus making our estimates considerably more precise; and it further mitigates omitted variable concerns by partially controlling for endogenous sorting of households into local tax jurisdictions. After transformation, the coefficient estimate for $\beta_3$ in Col. 3 suggests that households' taxed expenditures became somewhat less elastic to a tax rate change, but this change is not statistically significant.

In Table 2.6, we estimate the above equation over three tax bases: first, the effective tax base, second the subset of the effective tax base that is purchased both online and offline, and finally, the brick-and-mortar tax base.

Contrary to what Goolsbee, Lovenheim and Slemrod (2010) find in response to the spread of the internet, we do not find strong evidence that consumption of non-durable consumption goods is less elastic after the VCAs, which remove the sales tax evasion channel presented by the initial introduction of the internet. We estimate the effect of the VCA on the elasticity of the "effective tax base", which we define as the expenditure that is reported to the tax authority. Prior to the VCA, this base is only expenditure at brick-and-mortar retailers. We assume that no online expenditure is reported, which we feel is reasonable given the near zero compliance rate on use taxes. After the VCA, the base is the sum of both online and offline expenditure. In column 1, we estimate the elasticity of the tax base with respect to the tax rate for all goods that are subject to the standard sales tax rate over all time periods. This base excludes goods like soda, alcohol or cigarettes and other goods that may be taxed at special rates. It also excludes goods that are tax exempt.

[11]Let the sales tax base in jurisdiction of household h at time t be defined as

$$B_{ht} = \sum_{i}^{I} p_{iht}(\tau) X_{iht}(q_{iht}(p_{iht}(\tau), \tau)) \tag{2.20}$$

where I is the set of all taxable goods in the jurisdiction of household h, and $\tau$ is the sales tax rate. The first term, $p_{iht}(\tau)$, denotes the tax exclusive price; $X_{ict}$ is the aggregate demand for product i in jurisdiction c at time t, and is a function of the tax inclusive price $q_{iht} = p_{iht}(\tau)(1+\tau)$. The effect of the VCA on tax rate elasticity of demand can be expressed as

$$\frac{\partial B_{ht}}{\partial VCA \partial \tau} = \frac{\partial B_{ht}}{\partial \tau}|_{VCA=1} - \frac{\partial B_{ht}}{\partial \tau}|_{VCA=0} \tag{2.21}$$

which is the difference in the derivative of tax base with respect to the tax rate when a VCA is in place.

We estimate that a one percent increase in the tax rate leads to a 0.02 percent decrease in the sales tax base on average. Prior to the VCA, the elasticity is higher in absolute value at -0.05 (column 2) and the change in the elasticity due to a VCA is large in magnitude and positive (0.05), although not statistically significant.

We also test the effect of the VCA on a subset of this base - expenditure on goods that are purchased online between 5 and 95 percent of the time. Since the change in elasticity is expected to come from consumers who no longer purchase the good online in response to a tax change, we would expect that the effect is strongest on goods that can be purchased both online and at brick-and-mortar retailers. We estimate that the average elasticity of this base is higher at -0.5 but that the different between the pre-VCA and post VCA elasticity (0.062) is not statistically significant. However, the size of this tax base does increase by 14.5 percent because online consumption is added. Finally, when we focus solely on brick-and-mortar purchases of goods that are purchased online at least some of the time (column 6), we estimate a smaller tax base elasticity overall and statistically significant change after the VCA.

These results suggest that although the VCA increased the overall size of the tax base and decreased online consumption, it did not change the overall responsiveness of non-durables consumption with respect to the tax rate. This might be because online consumption of these goods is a smaller share of total consumption and therefore does not significantly impact the elasticity, which itself my be smaller than the elasticity of durables consumption with respect to the tax rate.

## 2.7 VCA Effect on Sales Tax Holidays

In this section, we use a second source of variation to study the impact of the VCA on responsiveness of consumption to tax rate changes.

About 23 U.S. states hold sales tax holidays annually, often in the month of August, when sales of certain goods are exempt from regular sales taxes. Items that are made exempt during tax holidays include clothing, footwear and school supplies, although some holidays are intended to encourage the consumption of specific items like energy-efficient electronics. We examine whether consumers become more responsive to tax holidays when sales tax evasion through online purchases is no longer viable because of VCAs.

Tax holidays could be thought of as a tax avoidance channel rather than a decrease in the tax rate. Cole (2009) showed that although tax holidays lead to an overall increase in the expenditure on exempted goods, nearly 90 percent of the response is due to re-timing of purchases. That is, consumers wait to make purchases during a sales tax holiday that they would have otherwise purchased at a different time. After the VCAs, because consumers can no longer evade use or sales taxes by purchasing online instead of at brick-and-mortar stores, the tax holiday channel may become more attractive.

To test the impact of the VCA on responsiveness during sales tax holidays, we estimate the elasticity of consumption with respect to tax holidays before and after the VCA of goods that are and are not exempt during the holiday. We estimate the following specification:

$$\begin{aligned} \log(y_{hcst}) = {} & \beta_1 \text{Tax Holiday}_{st} + \beta_2 \text{Post VCA}_{st} + \beta_3 \text{Tax Holiday}_{st} * \text{Post}_{st} \\ & + \beta_4 \text{Tax Holiday}_{st} * \text{Ever VCA}_s + \pi X_{ct} + \gamma_h + \delta_t + \eta_s * t + \nu_c + \epsilon_{hcst} \end{aligned} \tag{2.22}$$

where Tax $\text{Holiday}_{st}$ is a dummy for whether at least one day of the week $t$ in state $s$, where the household resides, had a tax holiday. Post $\text{VCA}_{st}$ is a dummy for whether the VCA has been implement in state $s$ in week $t$m and Ever VCA is a dummy for whether the state enacts a VCA between 2010 and 2014. The dependent variable $y_{hcst}$ is total household consumption in product group $c$ at time $t$. As a placebo check, we estimate this specification separately on products that are exempt during sales tax holidays and those that are not. We control for time-varying local characteristics such as a county-level price index and also include time ($\delta_t$), household ($\gamma_h$), and commodity ($\nu_c$) fixed effects.

The coefficient of interest is $\beta_3$, which captures the effect of the VCA on expenditure during sales tax holidays. We would expect $\beta_3 > 0$ as some consumers who may have purchased the same goods online switch to purchasing during sales tax holiday or increase their consumption during tax holidays.

Table 2.7 reports the results from estimating specification 2.22. As expected, sales tax holidays raise consumption of goods that are exempted during the sales tax holiday, but not others. The coefficient on *Tax Holiday* is positive and statistically significant in column 1 where the sample is restricted to goods that are exempt during holidays but not in column 3, which is a sub-sample of goods that are not exempt. However, the coefficient of interest on the interaction between *Tax Holiday* and *Post VCA* is close to zero and not statistically significant. Although household consumption increases by 10 percent during sales tax holidays, there is almost no change in this increase after the VCA. Similarly, on the extensive margin, there is no change in the likelihood that households make a purchase during a sales tax holiday week after the VCA (column 2). This is consistent with the earlier analysis that showed that there was no measurable change in the elasticity of the tax base due to the VCA.

We might expect the attractiveness of sales tax holidays and the change after the VCA to vary by the sales tax rate faced by the household. Those in areas with higher sales tax rates experience a bigger shock to their effective tax rate due to the VCA and might be more likely to wait to shop during tax holidays. A second dimension of heterogeneity might be that the VCA is more likely to impact households that actually made online purchases prior to the VCA. We test these dimensions of heterogeneity by augmenting specification 2.22 with interaction terms for the sales tax rate applicable at the household's zip code and a dummy for whether the household had made any online purchase prior to the VCA. We report the coefficient of interest from these specifications in Table 2.8. Overall, we do not find strong evidence that the VCA had an impact on responsiveness during sales tax holidays in states with higher tax rates (column 1), nor do we find strong evidence that it had a differential impact on households that had made online purchases prior to the VCA (column 2).

Together, these results suggest that the elasticity of the consumption tax base captured in the Nielsen data (which excludes large durables) was not significantly changed by the change in remittance rules introduced by the VCA.

## 2.8 Conclusion

With the share of consumer purchases made online expected to grow, policymakers are understandably focused on ways of ensuring that online retailers remit sales taxes. In this paper, we study the impact of states adopting VCAs with Amazon, the largest online retailer, on the prices and purchases of online goods. We are also interested in the effect of VCA adoption, which makes it more difficult for consumers to purchase products online from non-remitting retailers, on the sales tax elasticity. To investigate these questions, we exploit variation in the location and timing of VCA adoption by states between 2010 and 2014, and we use data from the Nielsen Consumer Panel.

First, we find that VCA adoption increases the share of online goods sold that are taxed at the point of sale. To establish this, we measure the percentage of taxable sales where the after-tax item price is equal to the pre-tax item price, implying that sales taxes were not being remitted by the online retailer. The proportion of online sales meeting this criterion falls by nearly half in response to the VCA, with the most pronounced changes at the largest retailers who are likeliest to comply; the analogous proportion for brick-and-mortar sales remains constant. Second, we find that consumers respond to VCA adoption by reducing their online consumption. On average, households in VCA-adopting states reduce online purchases by 8%, similar to the findings of Baugh, Ben-David and Park (2018) that households reduce purchases on Amazon following VCA adoption by 9-12%.

The discrepancy in these estimates is likely caused by the fact that, in our data, we capture all online expenditures, rather only those for Amazon, and many small retailers did not sign VCAs.

Finally, we show that although online consumption decreased as a result of the VCAs, its impact on the elasticity of the overall (non-durables) tax base was not significant. We do not find evidence that the elasticity of the sales tax base estimated from sales tax rate changes at the zip-code level, changed significantly after the VCA. To address concerns of policy endogeneity, we also examine where households' response to sales tax holidays change after the VCA. Although we would expect sales tax holidays to have become even more attractive after the VCA since households can no longer evade taxes at other times by shopping online, we do not find strong evidence of a change. These results suggest that although the change in remittance rules was effective at raising the overall size of the tax base, it did not have a large impact on the sensitivity of consumption to the sales tax rate.

## 2.9 Figures

Figure 2.1: Date of Implementation of Amazon VCAs

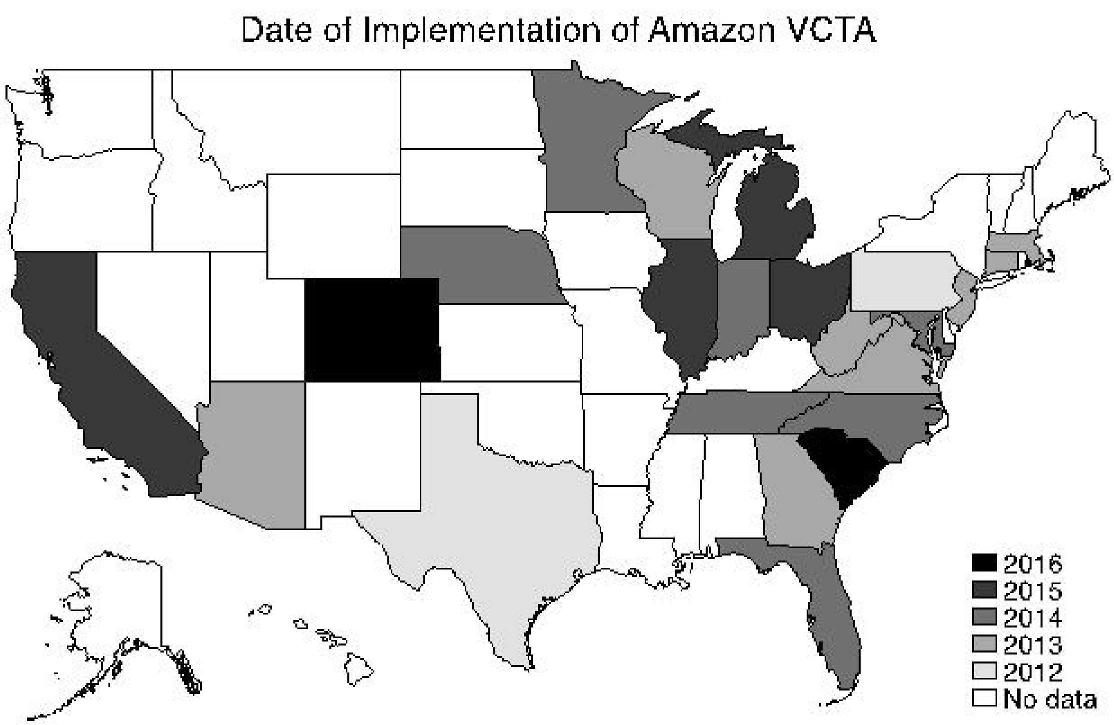


Figure 2.2: Fraction of Trips with Only Taxed Items that Paid No Sales Tax

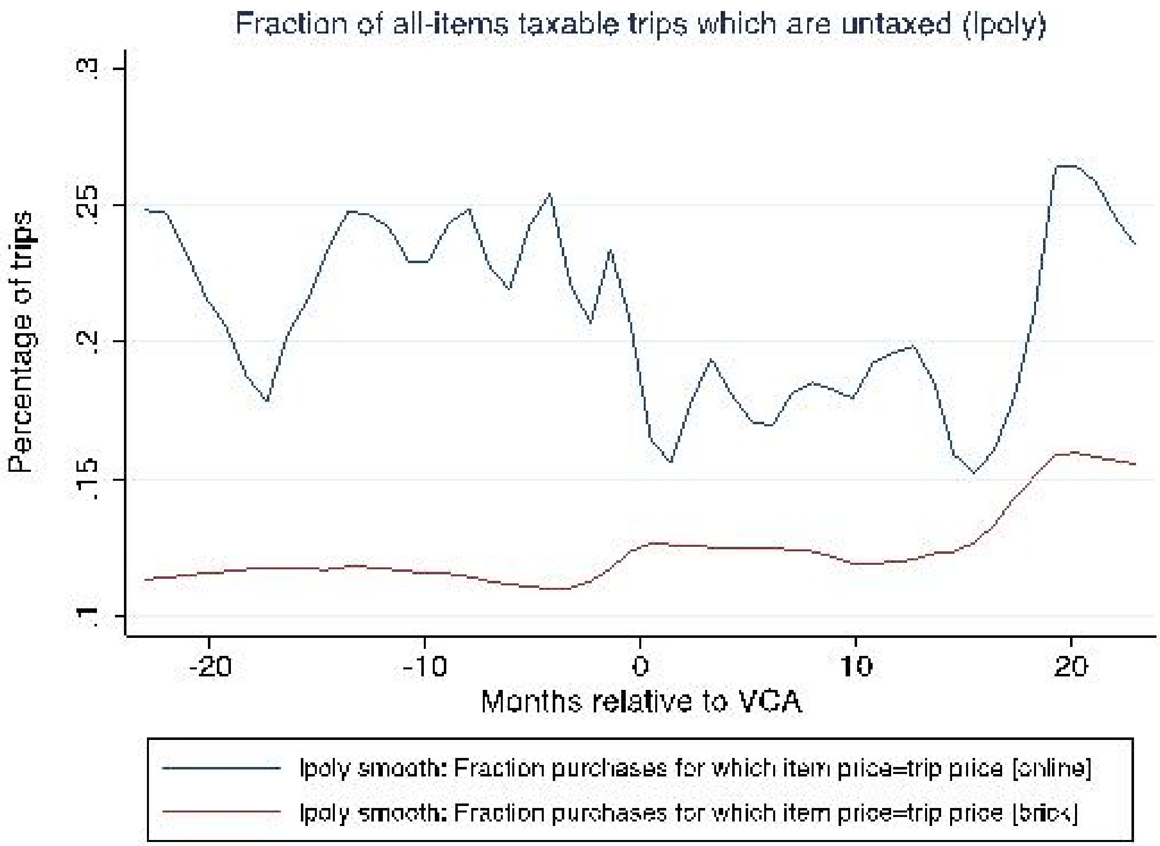
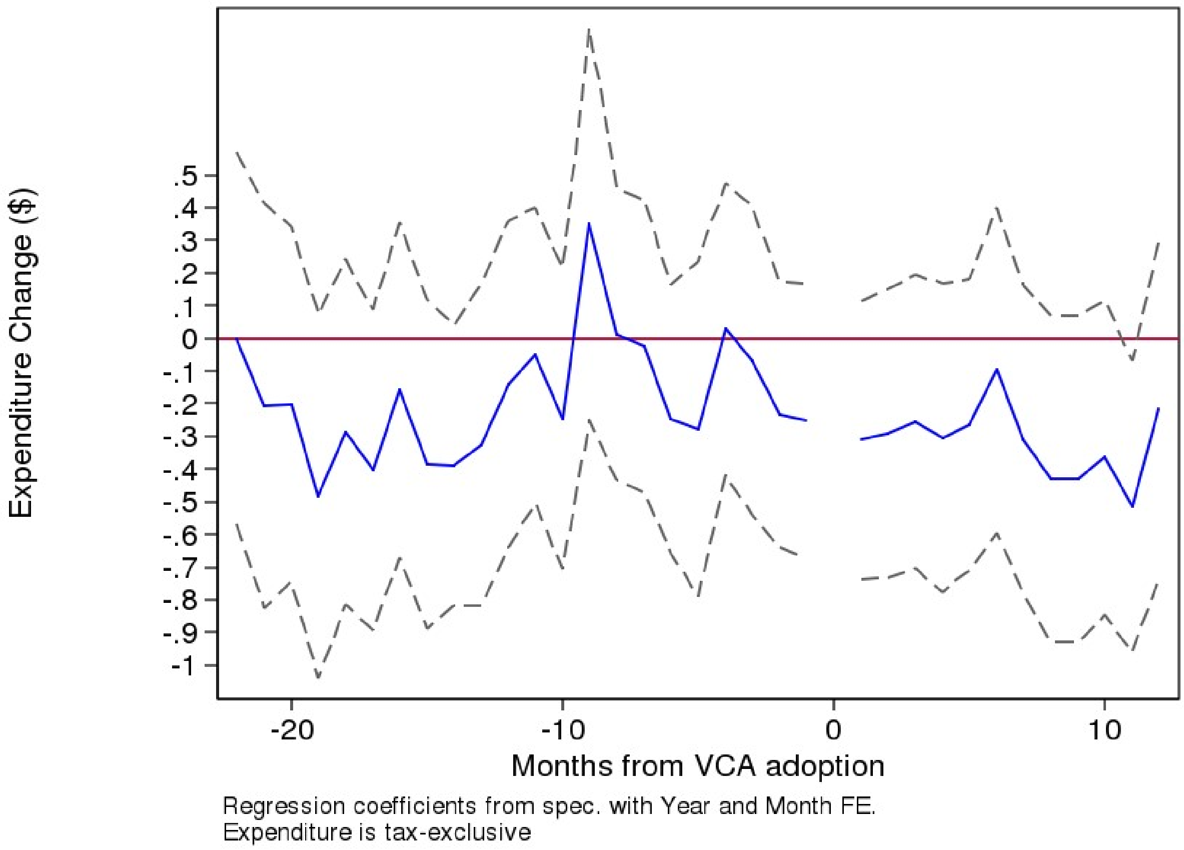


Figure 2.3: Change in Average Monthly Household Expenditure on Taxable Goods Online



## 2.10 Tables

Table 2.1: Tax Rate Changes by Administrative Unit and Year

| | 2010 | 2011 | 2012 | 2013 | 2014 | Total |
|---|---|---|---|---|---|---|
| *State* | 65 | 3 | 0 | 8 | 0 | 78 |
| *County* | 73 | 185 | 103 | 146 | 103 | 751 |
| *City* | 233 | 454 | 496 | 307 | 249 | 2029 |
| *Total* | 371 | 642 | 599 | 461 | 352 | 2858 |

*Notes:* Author's calculation based on data from zip2tax

Table 2.2: Tax Rate Changes Before and After the VCA

| | Pre VCA | | Post VCA |
|---|---|---|---|
| *Any Rate Change:* | | | |
| Mean | 0.011 | | 0.009 |
| | | (.028) | |
| *Conditional on Rate Change:* | | | |
| Mean | -0.143 | | 0.186 |
| | | (.571) | |
| s.d. | 0.818 | | 0.505 |
| Median | -0.150 | | 0.250 |
| Min | -3.000 | | -7.000 |
| Max | 2.000 | | 7.000 |
| N | 877350.000 | | 613522.000 |

*Notes:* Each observation is a zipcode-month in states that introduced a VCA between 2010 and 2014. T-statistics of difference in means calculated using wild bootstrap and clustering by state are reported in parentheses below the means.

Table 2.3: Effect of VCA on Taxable Expenditure

| | Online Expenditure | | | | Brick and Mortar Expenditure | |
|---|---|---|---|---|---|---|
| | (1) Taxable | (2) Taxable | (3) Taxable | (4) Exempt | (5) Taxable | (6) Exempt |
| Post VCA | -0.252** | -0.245** | -0.007 | 0.107 | 0.015 | 0.250 |
| | (0.119) | (0.092) | (0.103) | (0.090) | (0.640) | (1.060) |
| Sample | Total | Large Retailer | Small Retailer | | | |
| Household FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month of Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,591,230 | 3,591,230 | 3,591,230 | 3,591,230 | 3,591,230 | 3,591,230 |
| State Clusters | 49 | 49 | 49 | 49 | 49 | 49 |

*Notes:* Reports results from specification 2.16. *Post VCA* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state. Samples vary in each column according to what is indicated in the ”Sample” row. The first four columns show results on online expenditure of households while the last two show results on brick and mortar expenditure. Each regression includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.4: Effect of VCA on Prices and Quantity

| | Log(Quantity) | | | Log(Tax Exclusive Price) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post VCA | -0.006 | -0.007 | -0.002 | -0.007 | 0.001 | 0.005 |
| | (0.006) | (0.008) | (0.009) | (0.005) | (0.005) | (0.007) |
| UPC FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month of Yr FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Taxable? | Taxable | Taxable | Exempt | Taxable | Taxable | Exempt |
| Incl. Video? | Yes | No | – | Yes | No | – |
| Observations | 884,647 | 778,420 | 888,773 | 879,392 | 773,596 | 885,008 |
| State Clusters | 49 | 49 | 48 | 49 | 49 | 48 |

*Notes:* Reports results from specification 2.17. Each observation is a purchase. *Post VCA* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state. Each regression includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.5: Effect of VCA on Quantity by Quality

| | Taxable | | Exempt |
|---|---|---|---|
| | (1) | (2) | (3) |
| Post VCA X 2011 Price | -0.001** | -0.002** | -0.009*** |
| | (0.001) | (0.001) | (0.003) |
| Post VCA | -0.004 | -0.004 | 0.006 |
| | (0.007) | (0.009) | (0.011) |
| Ever VCA X 2011 Price | -0.000 | -0.000 | 0.002 |
| | (0.000) | (0.000) | (0.002) |
| UPC FE | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Month of Yr FE | Yes | Yes | Yes |
| Incl. Video? | Yes | No | – |
| Observations | 884,647 | 778,420 | 888,773 |
| State Clusters | 49 | 49 | 48 |

*Notes:* Results from specification 2.17 augmented with price of the good (UPC) in 2011 are reported. *Post VCA* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state and *2011 Price* is the average price for the UPC in the period before VCA is introduced. Each regression includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Effect of VCA on the Elasticity of the Consumption Tax Base

| | Effective Base | | Effective Base-Select | | Brick and Mortar Base | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Δ Tax Rate | -0.017 | -0.046** | -0.559* | -0.710* | -0.035 | 0.009 |
| | (0.011) | (0.018) | (0.299) | (0.404) | (0.036) | (0.043) |
| Δ Tax Rate X Treat | | 0.053** | | 0.296 | | -0.085 |
| | | (0.025) | | (0.683) | | (0.080) |
| Δ Tax Rate X Post VCA | | 0.047 | | 0.062 | | -0.102 |
| | | (0.034) | | (1.049) | | (0.163) |
| Post VCA | | 0.002 | | 0.145*** | | 0.010 |
| | | (0.002) | | (0.047) | | (0.011) |
| Household FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month of Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 3,316,704 | 3,316,704 | 3,435,106 | 3,435,106 | 536,488 | 536,488 |
| County Clusters | 2,863 | 2,863 | 2,864 | 2,864 | 2,726 | 2,726 |

*Notes:* Results from specification 2.17 are reported. *Post* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state. Each regression includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by household and state X year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Effect of VCA on Responsiveness to Sales Tax Holidays

| | Exempt During Holiday | | Not Exempt During Holiday | |
|---|---|---|---|---|
| | (1) Log(HH Weekly Expenditure) | (2) Any Expenditure | (3) Log(HH Weekly Expenditure) | (4) Any Expenditure |
| Post VCA | 0.019 | 0.001 | -0.004 | 0.001 |
| | (0.016) | (0.001) | (0.003) | (0.001) |
| Tax Holiday | 0.108*** | -0.002 | 0.000 | -0.002 |
| | (0.035) | (0.003) | (0.009) | (0.003) |
| Tax Holiday X Post VCA | -0.015 | 0.002 | 0.007 | 0.002 |
| | (0.034) | (0.003) | (0.012) | (0.003) |
| Tax Holiday X Ever VCA | 0.013 | 0.002 | 0.011 | 0.002 |
| | (0.035) | (0.003) | (0.010) | (0.003) |
| Household FE | Yes | Yes | Yes | Yes |
| Product FE | Yes | Yes | Yes | Yes |
| Week of Year FE | Yes | Yes | Yes | Yes |
| County X Year FE | Yes | Yes | Yes | Yes |
| Observations | 629,857 | 634,805 | 21,300,558 | 634,805 |
| Household Clusters | 1,296 | 1,297 | 1,431 | 1,297 |

*Notes:* Results from specification 2.7 are reported. *Post VCA* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state. *Tax Holiday* is an indicator for whether there is a tax holiday in place on any day during a given week in a given state, and *Ever VCA* is a dummy for whether the state enacted a VCA between 2010 and 2014. The first two columns are restricted to consumption of goods that are exempted during sales tax holidays and the last two are restricted to consumption of goods that are taxed but not exempt during holidays. Each regression includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by household and state X year. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.8: Effect of VCA on Responsiveness to Sales Tax Holidays, By Tax Rate and Online Purchasing Behavior

| | (1)<br>Log(HH expenditure) | (2)<br>Log(HH expenditure) |
|---|---|---|
| Tax Holiday X Post VCA X Tax Rate | 0.037<br>(0.032) | |
| Tax Holiday X Post VCA X Any Online | | -0.055<br>(0.062) |
| Household FE | Yes | Yes |
| Product FE | Yes | Yes |
| Week of Year FE | Yes | Yes |
| County X Year FE | Yes | Yes |
| Observations | 629,850 | 629,850 |
| County Clusters | 1,296 | 1,296 |

*Notes:* Table reports the estimates of a regression where specification 2.22 is augmented with interaction terms for the *Tax Rate* (column 1) and *Any Online* (column 2). *Tax Rate* is the prevailing sales tax rate inclusive of state, county and local rates at the household's zip-code in a given week. *Any Online* is an indicator for whether the household made any online purchase in a given week prior to the VCA. Each regression also includes interaction terms of *Tax Rate* with *Tax Holiday* and *Post VCA*. *Post VCA* is an indicator variable that equals 1 after the first week that the VCA is introduced in a state. *Tax Holiday* is an indicator for whether there is a tax holiday in place on any day during a given week in a given state. Each regression also includes includes a full set of household, week, product and county X year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by county. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

# CHAPTER III

# Does Intergovernmental Competition Reduce Rent-Seeking?

**From a work with Traviss Cassidy**

## 3.1 Introduction

The World Bank estimates that 18 percent of businesses around the world and 28 percent of businesses in low-income countries have been asked to pay a bribe at least once (Bank, 2017). Often these bribe payments are made for routine business activities like registration or licensing, which are the purview of local government officials. Local governments may also rely on the support of local businesses for own tax revenue or for re-election. Thus local governments face a trade-off between rent extraction and attracting and retaining firms, which have the option of "voting with their feet" by relocating to a more business-friendly jurisdiction (Tiebout, 1956). Competition among local governments may thereby limit rent-seeking. This is one reason why international agencies have promoted decentralization as a tool for promoting economic development (Bank, 1999; Nations, 2009; Fund, 2009).

This paper examines the extent to which competition between local governments can constrain rent-seeking behavior and improve the business environment. We examine an extraordinary period of decentralization in Indonesia from 2001 to 2014, which increased the number of district governments by about 50 percent. District governments in Indonesia are responsible for the majority of local public expenditure and receive revenue from business licensing and fees. Districts also receive shared revenue from taxes paid by local businesses, which are designed and administered by the central government. The splitting of districts into into smaller districts increased the number of local governments within original district boundaries. Our design exploits cross-district and over-time variation in splitting, as well as idiosyncratic variation in the timing of splits generated by a national moratorium, to estimate the effect of intergovernmental competition on reported business fees and bribes paid by manufacturing firms to local officials.

We find little evidence that competition among local governments reduces the business fees and bribes paid by firms on average. This null effect applies to firms irrespective of their moving costs—proxied by either the number of employees or the spatial concentration of the industry—contrary to theoretical intuition. There is little evidence that district splits have spillover effects on firms located in neighboring districts.

However, we do find that district splits reduce the bribe and fee payments reported by new entrants, suggesting that districts may compete for new firms that have a choice in location. Furthermore, the benefits of district splits for new firms are greater in smaller original districts, suggesting that competition for new entrants is fiercer when many jurisdictions are nearby.

There is empirical support for the "Leviathan Hypothesis" put forth by Brennan and Buchanan (1980), who characterize governments as revenue-maximizers constrained by the interjurisdictional mobility of individuals and firms. Diamond (2017) shows that local governments in the United States are able to extract greater rents when the housing supply is more inelastic, due to a reduction in the threat of out-migration. Further evidence is provided by Burgess et al. (2012), who also examine the Indonesian context. They find that district governments behave like monopolistically competitive firms: an increase in the number of districts within a province lowers price and increases the quantity of illegal logging, as predicted by a model of Cournot competition. While illegal logging is a type of corruption that benefit firms that rely on timber inputs, corruption in the form of bribes adversely affects all businesses. We study whether and how districts compete on this margin.

This paper contributes to the literature on the impact of decentralization and the closely related literature on intergovernmental competition. One of the channels through which decentralization can improve welfare is by creating the opportunity to "vote with your feet." Because we focus on the fragmentation of a particular level of government rather than the transfer of power from a central to subnational government, our results isolate the contribution of competition in decentralization.

Previous literature on decentralization has mainly focused on public goods such as health and education—amenities that a household might consider when choosing their location. We add to this literature by studying the impact on outcomes that are particularly important for firms and consider firms as the unit holding governments accountable through the threat of exit rather than households.

The empirical setting of Indonesia's decentralization offers many advantages. First, an increase in the number of local governments is perhaps the most natural way to conceptualize an increase in horizontal competition among governments. Few papers on intergovernmental competition exploit this type of variation.[1] One exception is Lima and Neto (2018), who examine the effect of municipal secessions on local public expenditure in Brazil. On the other hand, a large literature examines situations in which municipal jurisdictions grow in size by annexing or merging with nearby areas (e.g., Reingewertz, 2012; Breuille and Zanaj, 2013).

Second, our panel dataset allows us to control for time-invariant regional characteristics. This is important, because areas with many local governments may differ from those with few local governments along many dimensions. Some prior work has addressed this problem by finding an instrument that explains cross-sectional variation in interjurisdictional competition. For example, Hoxby (2000) uses the number of streams in a metropolitan area as an instrument for the number of school districts, and **?** uses the location of a town within a county as an instrument for the number of nearby county governments. Valid instruments are often hard to come by in cross-sectional settings. Exploiting the creation of new governments over time therefore offers a promising alternative approach that will prove useful in countries besides Indonesia.

Finally, redistricting is a widespread phenomenon, and many developing countries have increased the number of local jurisdictions in recent years. Our study is a first step toward answering a key question facing policymakers in these countries: What is the effect of creating an additional local government?

Section 3.2 describes the institutional setting of our study, Section 3.3 summarizes the data construction, Section 3.4 explains the empirical strategy, Section 3.5 presents the results, and Section 3.6 offers some concluding remarks.

## 3.2 Empirical Context

Following the ouster of autocratic ruler Suharto in 1998, Indonesia transitioned to democracy and instituted a series of political and fiscal reforms. A key element of these reforms was a large-scale decentralization program, which resulted in the creation of 212 new districts between 1999 and 2014, ending with 514 total districts. Districts are the

[1] In a cross-country study of the relationship between decentralization and corruption, Arikan (2004) measures decentralization as the number of local jurisdictions per capita. The cross-country literature often measures decentralization using the subnational share of total public expenditure (e.g., Fisman and Gatti, 2002).

second tier of government, provinces are aggregations of districts, and sub-districts and villages are the third and fourth tiers of government. Local parliamentarians could petition to split a district into one or more child districts, with the approval of the mayor of the original district.

Starting in 2001, districts were empowered to make decisions on most local public expenditure. We therefore limit our sample to the years 2001–2014 to hold constant the authority and responsibilities of local governments. During this period, the number of districts increased from 341 to 514, an increase of 50 percent.

Figure 3.1 provides a map of district borders in 2000 (thick black lines) and 2012 (thin gray lines), with districts that split over this period shaded in purple.[2] About one third of the original districts split at least once between 2001 and 2014. The map shows that island of Java, the historical center of economic and political power, has relatively few districts that split. By contrast, district splitting was widespread in the "outer islands" of Sumatra, Kalimantan, Sulawesi, Maluku, Papua, and Nusa Tenggara.

The central government sets tax rates on property, individual income, and business income, and administers these taxes. It then returns a portion of the revenue to the district where the taxes were collected. District governments are directly responsible for some business licenses. The central government funds the operation of local government and promotes fiscal equalization though the General Grant (*Dana Alokasi Umum*). This grant includes a basic allocation consisting of a lump-sum transfer and portion that depends on the civil service wage bill. The rest of the grant is apportioned according to a formula that uses proxies for expenditure needs (e.g., population, land area, poverty) and fiscal capacity (e.g., predicted revenue from other sources) (World Bank, 2007; Cassidy, 2019).

Bazzi and Gudgeon (2018) find that all child and parent districts experience an increase in per capita lifetime transfers of about 20 percent on average after the split, because of these transfers. Our empirical strategy will account for the mechanical change in district revenue due to these transfers, and examine the change in district revenue and expenditure above this mechanical increase.

We hypothesize that the district splits exogenously raised competition between local governments within the boundaries of the original district, and potentially in neighboring districts. As a result, we expect that the formal and informal tax burdens of firms would decrease as districts compete to attract and retain businesses. District governments receive revenue from local businesses. Moreover, local district officials are also held accountable

[2]We were unable to find a shapefile of 2014 district borders. No districts became newly autonomous in 2013, and 14 districts because newly autonomous in 2014. At the level of 2000 borders, only four districts experienced their first split in 2014.

through elections since district heads were elected by members of the local parliaments (instead of appointed by the central government) from 2001 and then directly elected from 2005 in staggered quinquennial elections.

We conceive of bureaucrats in local governments as providing a good (licenses) whose quantity they can restrict, for example by delaying or denying permits, and for which they can charge firms a bribe on top of official fees. Bureacrats in each local government act in their own interest, do not collude with other governments, and face a downward sloping demand curve for their product. This is the basic model in Shleifer and Vishny (1993) who use standard models of Bertrand or Cournot competition to show that in such a framework, an increase in the number of districts would increases competition among governments, leading to an increase in the quantity of goods supplied (more licenses) and a decrease in the price (a lower average bribe). One counter-intuitive implication is that more firms might be able to obtain licenses, thereby increasing the probability that the average firm pays a bribe but decreasing the average bribe price.

Firms' choice of location (and therefore, government) may depend on factors other than the bribe price or supply of licenses such as the availability of natural resources, labor or specific intermediate inputs. These factors constrain the competitive impact of new districts. Bureaucrats may also price discriminate according to firms' bargaining power or location-specific preferences. For example, districts might attract firms with lower moving costs (such as new firms) by offering to ease procedures without making such concessions for established firms with high moving costs. We test for heterogeneity in the impact of district splits along these dimensions.

## 3.3 Data

Our dataset combines establishment-level survey data with institutional data describing the proliferation of districts over the period 2001–14. For ease of exposition, we will use the terms "firm" and "establishment" interchangeably, even though we cannot link establishments belonging to the same firm. We use data from the Indonesian Annual Manufacturing survey, which covers the universe of establishments with 20 workers or more. It captures information on establishment production such as total value of production, number of employees, and industry of operation. Establishments report their total tax payments, including land and building tax, and "company license fees", which are administered by local governments. However, most tax rates and rules are set by the central government.

Another outcome of interest is "gifts" to others specifically by the firm and not by the owner or manager, which can include payments to government officials. We interpret this variable as including bribe payments to officials as others have Henderson and Kuncoro (2006, 2011). The terminology of "gifts" is often used in surveys such as the World Bank enterprise surveys to elicit truthful information on informal payments and therefore we interpret the response to this variable, which explicitly instructs the respondent to exclude gift payments by individuals and to consider only the firm, as referring to gifts that are part of the cost of doing business. The probability that any firm in our sample reports positive "gift" payments is 67 percent on average (see Table 3.1), which is higher than the probability of any gift payments by companies in Indonesia as reported in the World Bank enterprise surveys in 2015 (30 percent) but slightly lower than the probability of any bribe payment in Vietnam as reported in Bai et al. (2019) of slightly over 80 percent. Part of the discrepancy between the World Bank estimates and our estimates are due to differences in the sample. When we restrict the World Bank sample to firms with over 20 employees in the manufacturing industry, the prevalence of bribery rises from 25 percent to 40 percent in 2009. To lend further credence to our interpretation of the gifts variable as bribes, Table 3.2 shows that the prevalence of bribery is positively correlated with firms' activities that require permits or licenses from the local government such as electricity connection from the government, exports, land contracts and building construction. In contrast, bribe prevalence is negatively correlated with any own generation of electricity or purchase of electricity from non-governmental sources.

We drop all districts in the province of Jakarta. These districts are managed at the province level and hence do not compete with other districts in same province. Dropping Jakarta reduces the number of firm-year observations by 22,414, or almost 8 percent of the original sample.

## 3.4 Empirical Strategy

We use the district splits in Indonesia as an exogenous shock to intergovernmental competition among local governments. We first estimate using a reduced-form specification, the effect of these local government splits on formal and informal tax payments by local firms within the boundaries of the original district. Because the splits might have had spillover effects on neighboring districts, our estimated effects are a lower bound on the direct effect of the splits on the tax burden.

A district's decision of whether to split is endogenous. Examining the first wave of splits (2001–03), Fitrani, Hofman and Kaiser (2005) find that splits are more likely among districts with low population density, high ethnic diversity, and a bloated bureaucracy. Rather than relying on cross-sectional variation in whether a district ever split, our identification strategy exploits idiosyncratic variation in the *timing* of splits. This variation comes from two sources.

First, there is generally a multi-year lag between when a district applies for a split and when the government approves the split, and there is considerable uncertainty over whether the split will be approved.

Second, the national government imposed moratoria on district splitting from 2004 to 2006 and from 2009 to 2012, generating additional idiosyncratic variation in the timing of splits. In fact, more than 100 applications awaited consideration by the end of the first moratorium. (See Bazzi and Gudgeon, 2018, for details.) We report both full-sample results, which rely on the first source of variation, as well as results based on the subsample of districts that first split during 2001–03 or 2007–08, which focus on the second source of variation. We assume that the idiosyncratic regulatory factors that influence the timing of splits are exogenous with respect to local economic and political conditions that may otherwise affect our outcomes of interest. Specifically, we assume that average firm outcomes in districts that split in a given year, and districts that did not split in that year, would have followed parallel paths in the absence of splitting.

When a new district is created, an interim government is appointed. One to three years later, a democratically elected government takes over and the district starts receiving fiscal transfers from the central government. We define the split year as the year when these transfers first arrive, as this is when the new government is autonomous and can credibly compete with neighboring districts. However, we define the subsample of districts that split during 2001–03 or 2007–08 using the date the interim government took power, as the first moratorium generated idiosyncratic variation in the *de jure* creation of new governments.

Districts are defined according to the original district boundaries in 2000. Establishments that are observed in 2000 are assigned to their recorded district in 2000. Establishments that are first observed after 2000 are assigned to the district whose 2000 borders contain their first observed district.

The baseline specification is

$$Y_{fdit} = \beta Split_{dit} + \alpha_f + \lambda_{it} + \varepsilon_{fdit}, \tag{3.1}$$

where $Y_{fdit}$ is an outcome of establishment $f$, located in district $d$ and island group $i$, in year $t$. $Y$ is an establishment-level outcome variable. $Split$ is an indicator variable that equals 1 in the year of the district's first split and following years. The model allows for establishment fixed effects, $\alpha_f$, and arbitrary island-specific time trends, $\lambda_{it}$.[3]

The parameter of interest is $\beta$, the average effect of the first district split on firm outcomes. Two related assumptions are needed to identify $\beta$. First, the outcomes of firms in splitting districts and non-splitting districts would have experienced similar trends, on average, in the absence of splitting. Second, firms in districts that split early and districts that split late would have experienced similar trends, on average, had all splits occurred at the same time.

We focus on two main outcomes: taxes and bribes paid by firms. Intergovernmental competition may affect these outcomes along both an extensive margin and an intensive margin. To examine extensive-margin responses, we define $Y$ to be an indicator variable that equals 1 if the firm paid any taxes (or bribes). To examine the intensive margin, we define $Y$ to be the taxes (or bribes) paid by the firm as a share of the firm's total revenue. We measure total revenue as the total value of goods produced, as reported in the census.

We also measure the impact of splits on district-level outcomes such as the the total number of establishments, entry and exit of firms, and total employment in the district. Another margin of rent-seeking that is often considered is through the allocation of the district budget toward public goods or to internal expenditure on wages and salaries of bureaucrats. We look at how district expenditure in three key categories - personnel, general administration, and infrastructure - changed following a split.

While equation (3.1) is attractive due to its parsimony, it imposes the parametric assumption that the effect of the first district split is the same in all years following the split. To allow for dynamic treatment effects, we estimate the flexible model

$$Y_{fdit} = \sum_{s \in \mathcal{S}} \beta_s 1(t - T_d = s) + \alpha_f + \lambda_{it} + \varepsilon_{fdit}, \tag{3.2}$$

where $T_d$ is the year district $d$ first split, and $s$ indexes event time periods.[4] The omitted reference period is $s = -1$, the year prior to the first split. The indicator variable $1(t - T_d = s)$ is zero for all periods in districts that never split. The parameter $\beta_s$ represents the effect of the first split on outcomes $s$ years after the split occurred, relative to the effect of splitting on outcomes one year prior to the split.

In addition to estimating dynamic effects, the flexible model allows us to test for differential trends prior to the split among firms in splitting and non-splitting districts. The null hypothesis of no differential pre-trends is $\beta_s = 0$ for all $s < 0$.

[3]Following the Indonesian Statistical Bureau, we code seven island groups: Sumatra, Java, Nusa Tenggara, Kalimantan, Sulawesi, Maluku, and Papua.

[4]The set of event time periods is $\mathcal{S} = \{-4+, -3, -2, 0, 1, 2, 3, 4, 5, 6+\}$.

## 3.5 Results

### 3.5.1 Summary Statistics

Table 3.1 provides the summary statistics. Panel A summarizes the firm-level variables. The sample contains over 260,000 firm-year observations. Most firms make payments to local officials: firms paid formal taxes or business license fees in 76 percent of the observations, and they paid gifts in 67 percent of the observations. These payments represented a small fraction of total firm revenue on average, though there is considerable variation across firms. Formal taxes and fees were 0.91 percent of revenue on average with a standard deviation of 3.75 percent. Gifts represented 0.23 percent of revenue on average with a standard deviation of 1.25 percent. Both types of payments ranged from 0 percent to nearly 100 percent of revenue in a given year.[5] Firm size also varies considerably. Total revenue is 68 million IDR (in constant 2010 IDR), or roughly 6,800 USD, on average, with a standard deviation of 686 million IDR (68,600 USD). The maximum revenue observed in the sample is 142 billion IDR (14.2 million USD). The number of employees has a mean of 185, standard deviation of 726, a minimum of 20, and a maximum of over 56,000. Twenty-five percent of firms are "large" in the sense of having employed at least 200 workers at some point during the sample period.

Panel B of Table 3.1 summarizes the district-level variables. While just under one third of the districts that existed in 2000 eventually split, only 8 percent of the firm-year observations occur following the first split of a district. This is because many splits occurred later in the sample, and a majority of firms are located on the island of Java, where splitting was less common. The number of districts observed within the 2000 district borders ranges from one to five. Fifty-seven percent of observations occur under the leadership of a directly elected district mayor, and districts received a general grant of 580,000 IDR (58 USD) per capita on average, calculated at the level of the borders in 2000.

### 3.5.2 Baseline Estimates

Table 3.3 presents the baseline estimates of (3.1). The sample sizes are smaller than those reported in Table 3.1, because we drop singleton groups in order to ensure valid inference (Correia, 2015).[6] The results in Panel A are based on the full sample of districts.

[5]In a small number of cases, taxes or gifts exceeded revenue, sometimes by a very large amount. We treat these observations as survey errors and drop them. This problem occurs in only 0.1 percent of the observations.

[6]Singleton groups are groups defined by the fixed-effects structure that contain only one observation. In our case, a singleton group is either a firm observed in only one year or an island-year pair in which only one firm is observed. We use the Stata package `reghdfe`, which identifies and drops singleton groups (Correia,

Our first result seems counter-intuitive - the first district split had no impact on the probability of paying some taxes or fees, but it raised the probability of paying a gift by 3.7 percentage points. There was no change in the bribe rates paid by firms after the split. We interpret this as no evidence of a change in the bribe rate (the informal price of services provided by the bureaucrat) but potentially an increase in access to these services with a payment of fee. In Shleifer and Vishny (1993), competition between local governments can increase the prevalence of corruption in the same way that Cournot competition between firms increases total quantity supplied. More firms might now be able to get the licenses they require or construction approval, after payment of a fee. In line with the model's predictions, we would have also expected the official and unofficial price of these licenses to decrease but we do not find any measurable evidence that they did. The first district split had no measurable impact on tax or gift payments as a percentage of firm revenue as shown in table 3.3, columns 3 and 4.

Another explanation might be that newly created districts have less experienced officials and that the increase in prevalence of bribes might be driven by this inexperience. In fact, we find that there is no difference between "parent" and "child" districts - the increase in bribe prevalence as a result of splits is the same in both. The probability of any gift payments increase by 4 percentage points relative to the baseline of 67 percent in the parent district following a split, and the interaction of this split with the child district indicator is small and statistically insignificant (Table 3.5). One caveat to these results is that we measure firms' location in the child or parent district based on where they are observed in the first year of the split. The assumption is that firms do not move within the first year. To the extent that this assumption fails and that there is some immediate movement of firms without a relationship with local officials out of the parent district, we are likely to find that those who remain are more likely to pay bribes.

We also confirm all our results in a smaller sub-sample, shown in Panel B, of districts whose *de jure* split date occurred either right before or right after the 2004–06 moratorium on splitting. In this subsample, splitting appears to have no measurable effect on any of the firm outcomes. However, the sample is also restricted to only about 70 districts, reducing power.

The timing of the first split seems to be exogenous to our main outcome variables as we find that the parallel trends assumption holds. Figure 3.2 displays the estimates of (3.2) for the extensive-margin outcomes. The figures show an absence of differential pre-trends in the years prior to the first district split. The first column shows the results for the full sample. Panel (a) visual confirms the absence of an effect on the probability of paying any

2016).

formal taxes or license fees. Panel (b) shows that the first split increased the probability of paying any gifts, and this effect grew over time. The second column shows the results for the subsample of districts that split right before or after the first moratorium. These estimates are less precise, which is unsurprising as they are based on only 23 percent of the districts and 10 percent of the firm-year observations. The lack of precision makes it difficult to judge whether the results differ in the full sample and the subsample.

Figure 3.3 displays the estimates of (3.2) for the intensive-margin outcomes. Again, the estimates confirm an absence of differential trends prior to the first split. In line with the baseline estimates, the full-sample results in the first column show that splitting had no impact on tax or gift payments as a percentage of firm revenue. The estimates even rule out small effects: we can reject an absolute effect of 0.4 percent for taxes and 0.2 percent for gifts. The subsample results in the second column are less precise but tell the same general story as the full-sample results.

### 3.5.3 Robustness Checks

Several confounding factors may cause the baseline estimates to be biased. Prior to 2005, the mayor of each district was appointed by the district legislature. Districts then introduced direct mayoral elections in a staggered fashion over the period 2005–08. Direct elections were introduced in different years because the last appointed mayor was allowed to finish his or her term, and mayoral terms were not synchronized across districts. Among firm-year observations in districts that eventually split, 62 percent occur in districts that experienced a *de facto* split prior to 2005. If directly elected mayors matter for rent-seeking, the potential correlation between the timing of splits and the introduction of direct elections could bias our baseline estimates.

A second potential source of bias relates to the structure of fiscal transfers from the central government to district governments. The most important intergovernmental grant, the General Grant (*Dana Alokasi Umum*), is allocated according to a formula that includes a lump-sum component which does not depend on district population. As a result, when a district splits into multiple districts, grant revenue per capita mechanically increases as measured at the level of the original district borders. Henderson and Kuncoro (2006) argue that grant revenue can crowd out license fees and bribes by causing an increase in the salaries of local bureaucrats. Therefore our baseline estimates may partially reflect the influence of the General Grant, which increases after a split, on fees and gifts.

Table 3.4 displays the results from augmenting (3.1) with two control variables. The first control, *Directly Elected Mayor*, is an indicator variable that equals 1 in the year following the first direct election of the mayor and subsequent years. The second control is log

General Grant revenue per capita. The results in Panel A, based on the full sample of districts, show that the presence of a directly elected mayor has no impact on firm payments to local officials—on the extensive margin or the intensive margin. An increase General Grant revenue per capita reduces the likelihood that the firm pays any taxes or fees, but it does not significantly affect payments as a share of revenue. Importantly, adding these two controls has virtually no impact on the estimated effect of the first district split.

Panel B of Table 3.4 shows that adding the two control variables does not significantly alter the estimated effect of *Post 1st Split*, though the estimated effects of a directly elected mayor and General Grant revenue differ compared to the full sample.

### 3.5.4 Spillover Effects

The baseline estimates represent the effect of a district split on the average difference in outcomes between splitting and non-splitting districts. The estimates thus capture *relative* effects, because a split could affect outcomes in both splitting and non-splitting districts through the channel of intergovernmental competition. Arguably, the policy-relevant parameter is an *absolute* effect, defined as average difference in district potential outcomes in the "split" and "no-split" scenarios.

To estimate the absolute effect, it is necessary to control for the geographic spillover effects of splitting. These spillover effects are also of independent interest, and are useful for quantifying the aggregate effects of creating a new district. It is not possible to identify spillover effects when the nature of spillovers is left fully unspecified. We therefore assume that district splits can affect "neighboring" districts—those districts that share a border—but have no impact on more distant districts.

To allow for spillover effects, we augment equation (3.1) with one of two variables. The first is *Post 1st Neighbor Split*, an indicator variable that equals 1 after the first time a neighboring district experiences a split. The second is *Frac. Neighbors Split*, which equals the fraction of neighboring districts that have split.[7]

Table 3.6 presents the results. Controlling for spillover effects has little impact on the estimated direct effect of splitting. This result holds across all outcomes, samples, and neighbor-split measures. This implies that the relative and absolute effects of splitting on outcomes in the district that split are very similar. Consistent with this interpretation, the estimated spillover effects of neighbor splits tend to be small and statistically insignificant. The only exception is the regression with taxes and fees and a share of revenue as the outcome. However, the effect of the first split by a neighbor is positive and significant in

[7]This variable equals zero for districts with no neighbors.

the full sample, and negative and significant in the subsample of districts that split around the moratorium. We conclude that there is no consistent evidence that district splits have meaningful spillover effects across districts.

### 3.5.5 Firm Entry, Exit and Impact of District Splits on New Entrants

The baseline results and heterogeneity analysis by firm size and concentration (see appendix C.1.1) tell us that mobility costs for most firms in Indonesia are possibly too high to credibly threaten exit from districts. If firms, once established, find it costly to move, new firms might carefully consider where to locate. Additionally, the kinds of business fees and licenses that local government in Indonesia control might be more salient for new firms rather than existing businesses. For example, local governments are responsible for construction, location, and nuisance permits, business registration, and trading or industrial licenses. Only licenses for large business activities such as major natural resource, mining and forestry permits remain with the national government. Business registration can take between 1 to 15 days to process, and can cost up to 1 million Indonesian Rupiah (Sudjana, 2007). Given the importance of bureaucracy in local governments for new businesses, we test how district splits impact entry, exit, taxes and gift payments for new businesses.

Because our data covers the universe of manufacturing establishments with over 20 workers, we can construct district-level measures of the total number of establishments, total employment, and the number of new entrants and exits. Of course, some of these entrants and exits into or out of the survey might be of firms who grow to have over 20 workers (or shrunk to have fewer than 20). Therefore, the entry and exit can be more rightly thought of as into and out of the formal, registered sector.

We modify our baseline specification (3.1) to consider district-level outcomes and use district fixed effects instead of firm fixed effects. The first four district-level outcomes—number of establishments, total employment, and the number of new entrants and exits—are non-negative, have highly skewed distributions, and frequently take a value of zero. Therefore for these outcomes we use a fixed-effects Poisson quasi-maximum likelihood (QMLE) model.[8] The coefficients in these regressions, $\beta$, multiplied by 100 are interpreted

[8] Letting $Y$ denote the outcome variable, the model is

$$(Y_{dt} \mid Split_{dt}, \mu_d, \gamma_t) = \exp(\beta Split_{dt} + \mu_d + \gamma_t). \tag{3.3}$$

The parameter of interest is

$$\beta = \log(Y_{dt} \mid Split_{dt} = 1, \mu_d, \gamma_t) - \log(Y_{dt} \mid Split_{dt} = 0, \mu_d, \gamma_t),$$

so $100 \cdot \beta$ approximately equals the percentage change in the mean of $Y$ due to splitting. This approximation is good for $\beta$ close to zero. The exact percentage change in the mean of $Y$ due to splitting is $100 \cdot (\exp(\beta) - 1)$.

as the percent change in the mean of the dependent variable in response to a one unit change in the independent variable.

For the remaining outcomes—tax and bribe payments as a percentage of revenue for new and existing firms, and the share of new and existing firms that report any tax or gift payments—we use linear specification

$$Y_{dit} = \beta Split_{dit} + \alpha_d + \lambda_{it} + \varepsilon_{dit}. \tag{3.4}$$

If new firms have more choice on where to locate and register, we would expect that the proliferation of districts would reduce the cost of licenses through competition, particularly for new entrants. Results in columns 5-12 of Table 3.7 report the results of specification 3.4, and show that in fact, new firms are less likely to pay any taxes or gifts following district splits while there is no evidence of a change for the full sample of firms (columns 9 and 10). After the first split, the share of new establishments reporting any formal tax or fee payments falls by 0.15, or 15 percentage points, and the share of new establishments reporting any gift payments falls by 0.1, or 10 percentage points. The corresponding estimates for the sub-sample of districts that split around the moratorium are positive, small, and statistically insignificant. New firms that do report tax do see a decrease in the share of these taxes in total revenue that is large in magnitude (16 percent reduction in taxes as a share of revenue, column 7) but there is little change in the bribe rate. Consistent with the firm-level results, the average establishment does not see a measurable change in either of these outcomes (columns 5 and 6)

Although new entrants are less likely to pay formal or informal taxes, we find that there are surprisingly fewer entries, and fewer establishments in the district overall following splits. This is surprising as we would expect that the increase in government expenditure or decrease in rent-seeking would have positive effects on firm growth. Columns 1–4 of Table 3.7 show the effect of any district splits on the total number of establishments, employment, entries and exits in the district. In the full sample, we find that splits lead to a 57-percent reduction in new entrants each year on average after splits. Similarly, in the sub-sample of firms that split around the moratorium, districts see a 27-percent reduction in new entrants following splits. There is a small, negative but statistically insignificant effect on exits.

Splits in neighboring districts have an additional negative impact on the likelihood of

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The fixed-effects Poisson QMLE is consistent for $\beta$ when the conditional mean is correctly specified in (3.3). The outcome variable, $Y$, need not have a conditionally Poisson distribution, and no restrictions on the temporal dependence of the variables are necessary (Wooldridge, 1999). For all models we report standard errors that are robust to clustering at the original 2000 district borders to account for arbitrary serial correlation within each district.

bribe payments by new entrants into the main district. A one percentage point increase in the share of neighboring districts that split in any given year, decreases the likelihood that new entrants report a bribe payment by half a percentage point (Column 12, Table 3.8). The direct and spillover effects of district splits are likely to vary by the size of the district since a split within a small district (by land area) has a smaller effect on mobility. Firms in smaller districts, by definition, have a smaller distance to cover to the border with another district. One caveat is that the size of the general grant transfers account for district land area in its formula and that the weight given to land area in this formula was increased in 2006. We control for this change by interacting land area with a dummy for post 2006. In fact, we find that the negative impact of splits on bribe payments and tax/ fees for new firms is stronger in smaller districts. As land area increases in one square kilometer, the effect of a split is smaller by 0.6 percentage points. Results in columns 1, 2, and 3 in Table 3.9 consistently show that splits in larger districts lead to even fewer total establishments and employment within the 2000-borders of the main district.

### 3.5.6 District Expenditure

In addition to bribe payments, district officials may personally benefit by directing expenditure toward wages instead of public goods. We find that splits temper this channel of rents. While total district expenditure increases, it does so through an increase in infrastructure spending and not on personnel expenditure. Table 3.10 shows the impact of district splits on expenditure, controlling for any change in the per capita general grant, which mechanically increases when districts split. Total district level expenditure increases by 8 percentage points following a split. Infrastructure expenditure increases by 24 percentage points on average immediated following a split while personnel expenditure does not change. Expenditure on administration also increases by 24 percentage points in the full sample but not when the sample is restricted to only districts that split around the moratorium.

## 3.6 Conclusion

The fragmentation of districts in Indonesia represents a major exercise in decentralization in a populous country. Because the timing of district splits is exogenous to characteristics of the districts, this episode presents a unique natural experiment in which to study the effects of intergovernmental competition—an important channel through which the effects of decentralization operate—on the business environment. In particular, we examine the impact on formal and informal tax payments by manufacturing establishments.

Contrary to expectation, we find that the proliferation of districts did not significantly affect tax and bribe payments by the average establishment, and in fact, slightly increased the prevalence of gift (bribe) payments by 0.04 percentage points. This finding is consistent with a model where the licenses provided by bureaucrats are a desirable good for firms and the quantity of these license provided is determined in equilibrium to maximize bureaucrat rents. In this context, it might be desirable for bureaucrats to limit quantity of these licenses provided in exchange for an informal fee, which would also limit the prevalence of bribes. The increase in number districts raises competition and therefore increases total quantity supplied. However, we do not find strong evidence of a decrease in the bribe rate, which would also be predicted in such a model. The increase in bribery prevalence occurs mainly in parent districts so it cannot be the case that bribes increase because of inexperienced government officials that one might find in newly created child districts.

We do find, however, that *new entrants* are much less likely to report any gift payments. The share of new entrants reporting any gift payments falls by 10 percentage points relative to a mean of 27 percent. Similarly, the share of new entrants paying any taxes or fees falls by 15 percentage points relative to a mean of 31 percent, suggesting that districts may compete for new firms that have a choice in location. We also find that district expenditure on infrastucture increases after splits even though total expenditure does not change by much, suggesting that the budget was reallocated toward infrastructure expenditure. There is some evidence of spillover effects of neighboring district splits on the prevalence of bribery among new entrants but not established firms.

Our findings suggest that even a massive decentralization effort may not be effective in reducing overall corruption or improve the ease of doing business for firms when moving costs are high.

The results of this paper are especially informative for policymaking in Indonesia. As of July 2017, there were 246 pending applications to create new provinces and districts (Tempo, 2017). In deciding whether to approve each application, the central government must weigh the costs and benefits of district proliferation. Our paper provides valuable input into this decision, by quantifying the effect of inter-district competition on the business environment.

## 3.7 Tables

Table 3.1: Summary Statistics

| | Mean | Std. Dev. | Min. | Max. | Obs. |
|---|---|---|---|---|---|
| *Panel A: Firm-Level Variables* | | | | | |
| Firm Remitted Any Formal Taxes or Fees | 0.76 | 0.43 | 0.00 | 1.00 | 262,818 |
| Firm Recorded Some Gifts to Others | 0.67 | 0.47 | 0.00 | 1.00 | 261,568 |
| Formal Taxes and Fees as % of Revenue | 0.91 | 3.75 | 0.00 | 99.49 | 245,509 |
| Gifts as % of Revenue | 0.23 | 1.25 | 0.00 | 99.15 | 244,409 |
| Total Revenue (IDR 1 million) | 67.80 | 685.59 | 0.00 | 142,003.66 | 264,357 |
| Number of Employees | 184.61 | 726.40 | 20.00 | 56,139.00 | 264,360 |
| Large Firm (Max # Employees $\geq$ 200) | 0.24 | 0.43 | 0.00 | 1.00 | 264,360 |
| Any Government Ownership | 0.48 | 0.50 | 0.00 | 1.00 | 264,360 |
| Industry Spatial Concentration | 0.03 | 0.04 | -0.06 | 0.50 | 264,248 |
| *Panel B: District-Level Variables* | | | | | |
| Post 1st Split | 0.08 | 0.27 | 0.00 | 1.00 | 264,942 |
| Post 1st Neighbor Split | 0.30 | 0.46 | 0.00 | 1.00 | 264,942 |
| Land Area (1000s $\mathrm{km}^2$, 2000 Borders) | 1.67 | 3.09 | 0.02 | 119.75 | 264,942 |
| Number of Districts in Original District | 1.09 | 0.32 | 1.00 | 8.00 | 264,942 |
| Number of Neighboring Districts (2000 Borders) | 4.12 | 1.94 | 0.00 | 10.00 | 264,942 |
| Number of Neighbors that Split | 0.41 | 0.75 | 0.00 | 8.00 | 264,942 |
| Fraction of Neighbors that Split | 0.13 | 0.27 | 0.00 | 1.00 | 264,942 |
| Number of Establishments | 288.27 | 249.42 | 0.00 | 952.00 | 264,942 |
| District Manufacturing Employment | 58,901.71 | 65,121.53 | 0.00 | 309,958.00 | 264,942 |
| Firm Entries | 123.00 | 150.07 | 0.00 | 764.00 | 264,942 |
| Firm Exits | 14.67 | 21.31 | 0.00 | 200.00 | 247,608 |
| Taxes/Fees as % of Revenue (All Establishments) | 0.87 | 0.77 | 0.00 | 49.74 | 264,340 |
| Gifts as % of Revenue (All Establishments) | 0.23 | 0.26 | 0.00 | 34.63 | 264,334 |
| Taxes/Fees as % of Revenue (New Establishments) | 0.28 | 0.48 | 0.00 | 49.74 | 264,340 |
| Gifts as % of Revenue (New Establishments) | 0.08 | 0.16 | 0.00 | 18.57 | 264,334 |
| Share of Firms Paying Any Taxes or Fees | 0.73 | 0.16 | 0.00 | 1.00 | 264,353 |
| Share of New Firms Paying Any Taxes or Fees | 0.31 | 0.24 | 0.00 | 1.00 | 264,353 |
| Share of Firms Paying Any Gifts | 0.64 | 0.18 | 0.00 | 1.00 | 264,345 |
| Share of New Firms Paying Any Gifts | 0.27 | 0.22 | 0.00 | 1.00 | 264,345 |
| Directly Elected Mayor | 0.57 | 0.49 | 0.00 | 1.00 | 264,942 |
| General Grant Revenue per Capita | 0.58 | 0.37 | 0.00 | 8.74 | 264,942 |

*Notes:* The value of firm output is measured in constant 2010 IDR 1 million ($\approx$ USD 100). District fiscal variables are measured in constant 2010 IDR 1 million per capita.

Table 3.2: Correlation between "Gifts" and Activities Requiring Permits

| | Firm Paid Any Bribes | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Any Export | 0.009* | | | | | | |
| | (0.005) | | | | | | |
| *Any Expenditure On:* | | | | | | | |
| Land Contract | | 0.039*** | | | | | 0.032*** |
| | | (0.009) | | | | | (0.010) |
| Building | | | 0.024*** | | | | 0.023*** |
| | | | (0.008) | | | | (0.008) |
| *Any Electricity Purchased from:* | | | | | | | |
| Government | | | | 0.050*** | | | 0.055*** |
| | | | | (0.008) | | | (0.008) |
| Non Government | | | | | -0.243*** | | -0.102*** |
| | | | | | (0.029) | | (0.026) |
| Any Electricity Generated | | | | | | -0.037*** | 0.007 |
| | | | | | | (0.010) | (0.007) |
| Mean Indep. Var | .18 | .07 | .12 | .88 | .05 | .18 | |
| Observations | 173,320 | 257,859 | 228,678 | 257,859 | 257,859 | 248,937 | 219,988 |
| District Clusters | 309 | 314 | 313 | 314 | 314 | 314 | 313 |

*Notes:* Reports results from the linear regression in (3.1), where an observation is a firm-year. All regressions control for log firm revenues as a measure of firm size. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: The Effect of the First District Split on Rent-Seeking

*Panel A: All Districts*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | 0.002 | 0.037*** | -0.064 | 0.007 |
| | (0.016) | (0.012) | (0.064) | (0.030) |
| Observations | 259,237 | 258,025 | 241,716 | 240,653 |
| District Clusters | 314 | 314 | 313 | 313 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.020 | -0.002 | 0.031 | 0.009 |
| | (0.024) | (0.019) | (0.084) | (0.040) |
| Observations | 25,649 | 25,602 | 23,929 | 23,882 |
| District Clusters | 73 | 73 | 73 | 73 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 3.4: Robustness Checks: Controlling for Directly Elected Mayor and Grant Revenue

*Panel A: All Districts*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | 0.005 | 0.039*** | -0.051 | 0.009 |
| | (0.015) | (0.012) | (0.063) | (0.030) |
| Directly Elected Mayor | 0.001 | 0.005 | -0.099 | -0.005 |
| | (0.011) | (0.008) | (0.061) | (0.012) |
| Log General Grant p.c. | -0.019*** | -0.020*** | -0.025 | -0.009 |
| | (0.005) | (0.007) | (0.056) | (0.012) |
| Observations | 259,130 | 257,918 | 241,609 | 240,546 |
| District Clusters | 314 | 314 | 313 | 313 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.018 | 0.002 | 0.034 | -0.011 |
| | (0.023) | (0.018) | (0.095) | (0.040) |
| Directly Elected Mayor | 0.055*** | 0.021 | 0.048 | -0.057 |
| | (0.018) | (0.018) | (0.089) | (0.052) |
| Log General Grant p.c. | -0.012 | -0.031 | -0.017 | 0.183 |
| | (0.037) | (0.035) | (0.220) | (0.129) |
| Observations | 25,649 | 25,602 | 23,929 | 23,882 |
| District Clusters | 73 | 73 | 73 | 73 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Directly Elected Mayor* is an indicator variable that equals 1 in the year following the first direct election of the mayor and subsequent years. *Log General Grant p.c.* is the log of revenue from the General Grant, in constant 2010 IDR 1 million (≈ USD 100) per capita, aggregated to the level of the district borders in 2000. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: Heterogeneous Effects of the First Split by Parent and Child District

*Panel A: All Districts*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.013 | 0.042*** | -0.099 | -0.038 |
| | (0.021) | (0.015) | (0.106) | (0.053) |
| Post 1st Split × Child District | 0.034 | -0.007 | 0.089 | 0.087 |
| | (0.029) | (0.022) | (0.113) | (0.062) |
| Directly Elected Mayor | 0.001 | 0.005 | -0.099 | -0.005 |
| | (0.011) | (0.008) | (0.061) | (0.012) |
| Log General Grant p.c. | -0.020*** | -0.020*** | -0.027 | -0.010 |
| | (0.005) | (0.007) | (0.057) | (0.012) |
| Observations | 257,680 | 256,484 | 240,257 | 239,212 |
| District Clusters | 312 | 312 | 311 | 311 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.019 | 0.008 | 0.018 | -0.088 |
| | (0.034) | (0.021) | (0.143) | (0.062) |
| Post 1st Split × Child District | 0.003 | -0.022 | 0.069 | 0.160* |
| | (0.028) | (0.019) | (0.190) | (0.082) |
| Directly Elected Mayor | 0.051*** | 0.006 | 0.099 | -0.069 |
| | (0.017) | (0.017) | (0.101) | (0.045) |
| Log General Grant p.c. | -0.011 | -0.029 | -0.056 | 0.140 |
| | (0.038) | (0.034) | (0.211) | (0.136) |
| Observations | 24,800 | 24,763 | 23,124 | 23,087 |
| District Clusters | 71 | 71 | 71 | 71 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Child District* is an indicator for whether a firm in a district that split was observed to be in a newly created district in the first year of the split. We assume that firms do not move in the very first year of the *de jure* split. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Spillover Effects of District Splits on Rent-Seeking

*Panel A: All Districts*

| | Firm Paid Any: | | | | Payments as % of Revenue: | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Taxes/Fees | (3) Gifts | (4) Gifts | (5) Taxes/Fees | (6) Taxes/Fees | (7) Gifts | (8) Gifts |
| Post 1st Split | -0.003 | 0.002 | 0.031*** | 0.036*** | -0.092 | -0.074 | 0.007 | 0.008 |
| | (0.014) | (0.015) | (0.010) | (0.012) | (0.072) | (0.065) | (0.031) | (0.030) |
| Post 1st Neighbor Split | 0.016 | | 0.017 | | 0.097* | | 0.001 | |
| | (0.013) | | (0.013) | | (0.058) | | (0.024) | |
| Frac. Neigh. Split | | 0.014 | | 0.007 | | 0.309** | | -0.023 |
| | | (0.019) | | (0.019) | | (0.145) | | (0.041) |
| Observations | 259,237 | 259,237 | 258,025 | 258,025 | 241,716 | 241,716 | 240,653 | 240,653 |
| District Clusters | 314 | 314 | 314 | 314 | 313 | 313 | 313 | 313 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Firm Paid Any: | | | | Payments as % of Revenue: | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Taxes/Fees | (3) Gifts | (4) Gifts | (5) Taxes/Fees | (6) Taxes/Fees | (7) Gifts | (8) Gifts |
| Post 1st Split | -0.020 | -0.020 | -0.002 | -0.002 | 0.033 | 0.027 | 0.008 | 0.009 |
| | (0.024) | (0.024) | (0.020) | (0.020) | (0.086) | (0.084) | (0.038) | (0.042) |
| Post 1st Neighbor Split | 0.001 | | -0.002 | | -0.110* | | 0.043 | |
| | (0.016) | | (0.012) | | (0.058) | | (0.048) | |
| Frac. Neigh. Split | | 0.018 | | -0.016 | | -0.256 | | -0.006 |
| | | (0.037) | | (0.038) | | (0.194) | | (0.185) |
| Observations | 25,649 | 25,649 | 25,602 | 25,602 | 23,929 | 23,929 | 23,882 | 23,882 |
| District Clusters | 73 | 73 | 73 | 73 | 73 | 73 | 73 | 73 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Post 1st Neighbor Split* is an indicator variable that equals 1 after the first time a neighboring district experiences a split, and *Frac. Neighbors Split* equals the fraction of neighboring districts that have split. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 3.7: The Effect of the First District Split on Rent-Seeking: Aggregate District-Level Outcomes

*Panel A: All Districts*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.368*** | -0.253** | -0.568*** | -0.334 | -0.026 | -0.004 | -0.165* | -0.014 | -0.044 | 0.029 | -0.148*** | -0.103*** |
| | (0.132) | (0.100) | (0.199) | (0.247) | (0.142) | (0.107) | (0.091) | (0.037) | (0.030) | (0.029) | (0.029) | (0.027) |
| Observations | 4,396 | 4,424 | 3,990 | 3,952 | 4,106 | 4,102 | 4,106 | 4,102 | 4,117 | 4,112 | 4,117 | 4,112 |
| Districts (2000) | 314 | 316 | 285 | 304 | 315 | 314 | 315 | 314 | 316 | 315 | 316 | 315 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.142** | 0.116 | -0.271** | -0.055 | -0.162 | -0.060 | -0.080 | 0.048 | -0.025 | 0.053 | 0.008 | 0.020 |
| | (0.070) | (0.101) | (0.133) | (0.248) | (0.153) | (0.151) | (0.105) | (0.049) | (0.043) | (0.037) | (0.039) | (0.023) |
| Observations | 1,050 | 1,064 | 686 | 897 | 913 | 910 | 913 | 910 | 917 | 913 | 917 | 913 |
| Districts (2000) | 75 | 76 | 49 | 69 | 76 | 75 | 76 | 75 | 76 | 75 | 76 | 75 |

*Notes:* Columns 1–4 report results from the fixed-effects Poisson regression in (3.3), and columns 5–10 report results from the linear regression in (3.4), where an observation is a district-year. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.8: Spillover Effects of District Splits: Aggregate District-Level Outcomes

*Panel A: All Districts*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.336** | -0.205** | -0.549*** | -0.280 | -0.048 | 0.016 | -0.176* | -0.010 | -0.042 | 0.033 | -0.146*** | -0.099*** |
| | (0.131) | (0.097) | (0.196) | (0.238) | (0.141) | (0.107) | (0.092) | (0.038) | (0.030) | (0.029) | (0.029) | (0.027) |
| Frac. Neigh. Split | -0.181** | -0.270*** | -0.172 | -0.313* | 0.355* | -0.318 | 0.176 | -0.061 | -0.018 | -0.060* | -0.045 | -0.056** |
| | (0.085) | (0.102) | (0.131) | (0.169) | (0.212) | (0.231) | (0.169) | (0.064) | (0.035) | (0.034) | (0.029) | (0.028) |
| Observations | 4,396 | 4,424 | 3,990 | 3,952 | 4,106 | 4,102 | 4,106 | 4,102 | 4,117 | 4,112 | 4,117 | 4,112 |
| Districts (2000) | 314 | 316 | 285 | 304 | 315 | 314 | 315 | 314 | 316 | 315 | 316 | 315 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.129** | 0.129 | -0.287** | -0.083 | -0.163 | -0.055 | -0.082 | 0.048 | -0.024 | 0.054 | 0.007 | 0.019 |
| | (0.064) | (0.114) | (0.118) | (0.235) | (0.152) | (0.139) | (0.104) | (0.049) | (0.044) | (0.037) | (0.039) | (0.023) |
| Frac. Neigh. Split | -0.131 | -0.120 | 0.396 | 0.370 | 0.213 | -0.716 | 0.289 | 0.081 | -0.100 | -0.126 | 0.031 | 0.017 |
| | (0.269) | (0.300) | (0.799) | (0.614) | (0.510) | (0.818) | (0.306) | (0.129) | (0.102) | (0.106) | (0.060) | (0.065) |
| Observations | 1,050 | 1,064 | 686 | 897 | 913 | 910 | 913 | 910 | 917 | 913 | 917 | 913 |
| Districts (2000) | 75 | 76 | 49 | 69 | 76 | 75 | 76 | 75 | 76 | 75 | 76 | 75 |

*Notes:* Columns 1–4 report results from the fixed-effects Poisson regression in (3.3), and columns 5–10 report results from the linear regression in (3.4), where an observation is a district-year. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Frac. Neighbors Split* equals the fraction of neighboring districts that have split. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.9: Heterogeneous Effects of District Splits: Aggregate District-Level Outcomes

*Panel A: All Districts*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.192 | -0.202 | -0.327** | -0.285 | -0.132 | 0.063 | -0.229** | -0.011 | -0.074* | -0.024 | -0.180*** | -0.136*** |
| | (0.144) | (0.135) | (0.133) | (0.302) | (0.162) | (0.084) | (0.108) | (0.048) | (0.041) | (0.039) | (0.040) | (0.039) |
| Post Split $\times$ Area | -0.023** | 0.006 | -0.048** | 0.011 | 0.006 | -0.010 | 0.009 | 0.002 | 0.003 | 0.005* | 0.007** | 0.006** |
| | (0.011) | (0.013) | (0.022) | (0.019) | (0.013) | (0.016) | (0.010) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| (Yr $\geq$ 2006) $\times$ Area | -0.017* | -0.022** | -0.031** | -0.042*** | 0.015 | 0.007 | -0.005 | -0.005* | 0.001 | 0.003 | -0.009*** | -0.007*** |
| | (0.010) | (0.009) | (0.014) | (0.013) | (0.011) | (0.007) | (0.010) | (0.003) | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 4,100 | 4,121 | 3,723 | 3,662 | 4,106 | 4,102 | 4,106 | 4,102 | 4,117 | 4,112 | 4,117 | 4,112 |
| Districts (2000) | 313 | 315 | 284 | 292 | 315 | 314 | 315 | 314 | 316 | 315 | 316 | 315 |

*Panel B: Districts that Split in 2001–03, 2007–08*

| | Totals Across All Plants (Poisson) | | | | % Rev.: All Plants | | % Rev.: New Plants | | Share Plants Paying | | Share New Plants Paying | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) # Plants | (2) Employ. | (3) Entries | (4) Exits | (5) Taxes/Fees | (6) Gifts | (7) Taxes/Fees | (8) Gifts | (9) Taxes/Fees | (10) Gifts | (11) Taxes/Fees | (12) Gifts |
| Post 1st Split | -0.025 | 0.142 | -0.189 | -0.156 | -0.123 | 0.215** | -0.137 | 0.061 | 0.002 | 0.042 | -0.020 | 0.007 |
| | (0.064) | (0.121) | (0.170) | (0.272) | (0.197) | (0.101) | (0.129) | (0.055) | (0.064) | (0.051) | (0.054) | (0.030) |
| Post Split $\times$ Area | -0.014*** | 0.002 | -0.014 | 0.014 | -0.004 | -0.030 | 0.006 | -0.001 | -0.003 | 0.001 | 0.003 | 0.001 |
| | (0.004) | (0.009) | (0.012) | (0.018) | (0.016) | (0.021) | (0.012) | (0.004) | (0.004) | (0.003) | (0.002) | (0.002) |
| (Yr $\geq$ 2006) $\times$ Area | -0.016*** | -0.010 | -0.047** | -0.027 | 0.015 | 0.014 | -0.011 | -0.003 | 0.008** | 0.004 | -0.004 | -0.002 |
| | (0.005) | (0.007) | (0.021) | (0.017) | (0.009) | (0.012) | (0.008) | (0.003) | (0.004) | (0.004) | (0.003) | (0.002) |
| Observations | 905 | 919 | 561 | 752 | 913 | 910 | 913 | 910 | 917 | 913 | 917 | 913 |
| Districts (2000) | 74 | 75 | 48 | 63 | 76 | 75 | 76 | 75 | 76 | 75 | 76 | 75 |

*Notes:* Columns 1–4 report results from the fixed-effects Poisson regression in (3.3), and columns 5–10 report results from the linear regression in (3.4), where an observation is a district-year. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Area* is land area in square kilometers, according to 2000 borders. *(Yr $\geq$ 2006)* is an indicator variable that equals 1 in years 2006 and later. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.10: The Effect of the First District Split on District Expenditure

*Panel A: All Districts*

| | (1) Total | (2) Personnel | (3) Administration | (4) Infrastructure |
|---|---|---|---|---|
| Post 1st Split | 0.049* | -0.034 | 0.190*** | 0.192*** |
| | (0.028) | (0.024) | (0.033) | (0.067) |
| Observations | 4,377 | 4,407 | 3,791 | 3,754 |
| Districts (2000) | 336 | 336 | 336 | 336 |

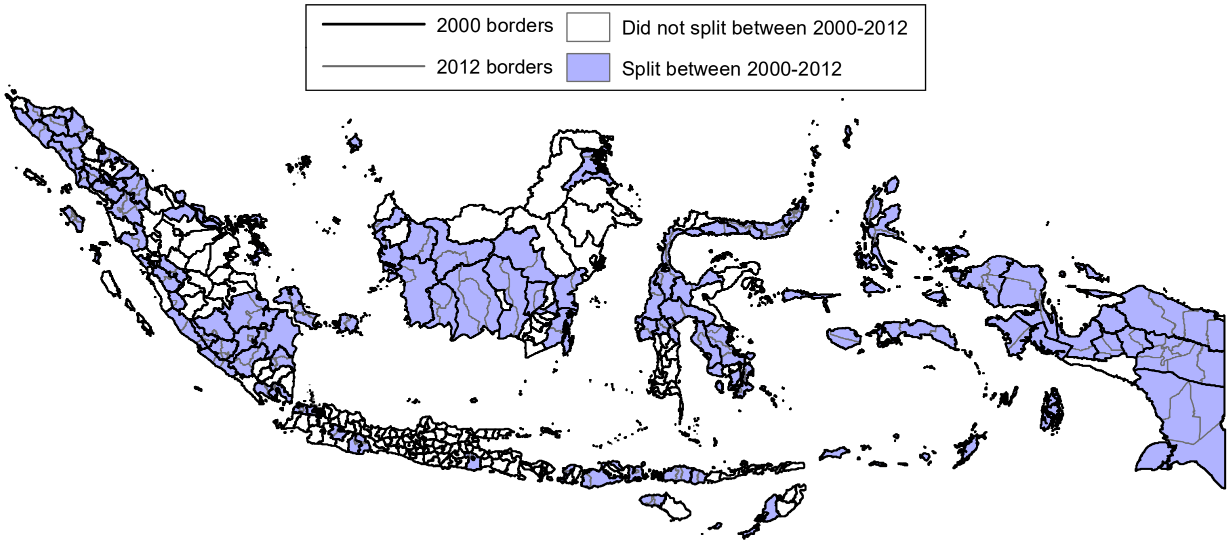
*Panel B: Districts that Split in 2001–03, 2007–08*

| | (1) Total | (2) Personnel | (3) Administration | (4) Infrastructure |
|---|---|---|---|---|
| Post 1st Split | -0.072 | -0.130*** | 0.055 | 0.161 |
| | (0.046) | (0.044) | (0.054) | (0.110) |
| Observations | 1,237 | 1,237 | 1,053 | 1,050 |
| Districts (2000) | 93 | 93 | 93 | 93 |

*Notes:* Reports results from the linear regression in (3.4), where an observation is a district-year. Dependent variables are log district-level expenditure per capita in four expenditure categories: *Total*, *Personnel*, *Administration*, and *Infrastructure*. All regressions control for the log of General Grant revenue per capita. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. Each regression includes a full set of firm fixed effects and island $\times$ year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## 3.8 Figures

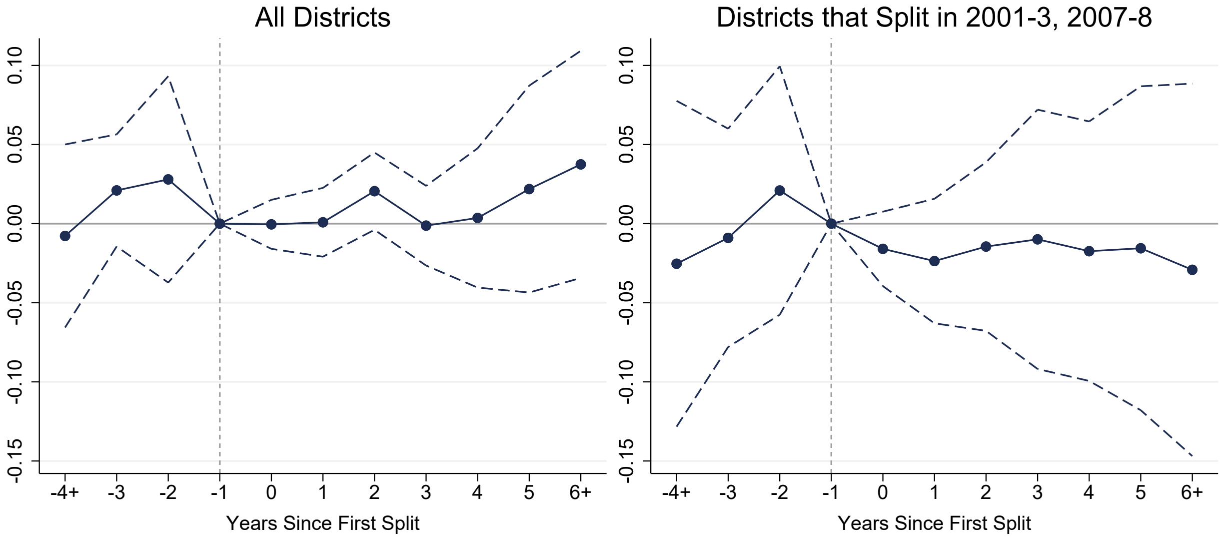
Figure 3.1: District Proliferation, 2000–2012



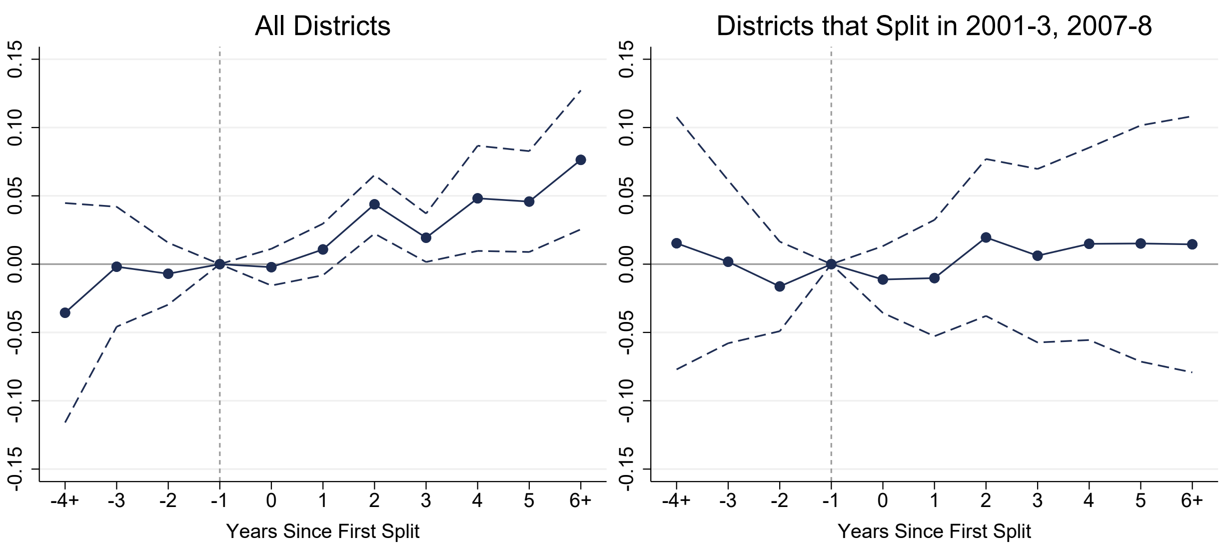

*Notes:* This map displays district borders in 2000 and 2012 based on the 2012 district shapefile provided by the Indonesian Statistical Bureau and the district crosswalk provided by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER).

Figure 3.2: The Effect of the First District Split on Firm-Level Extensive-Margin Outcomes

(a) Outcome: Firm Remitted Any Formal Taxes or License Fees



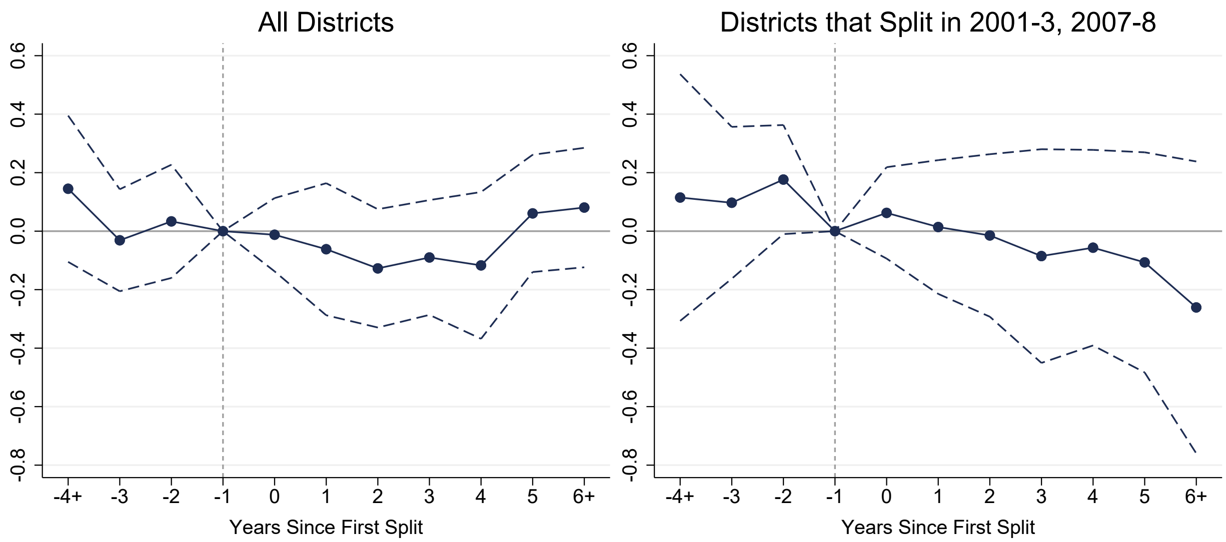

(b) Outcome: Firm Recorded Any Gifts to Others



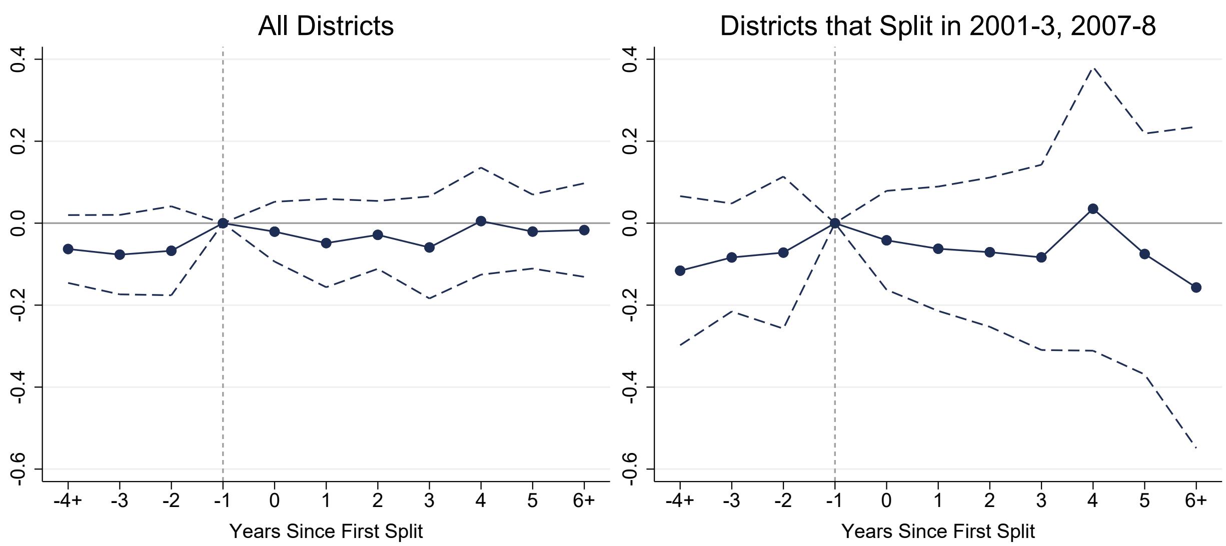

*Notes:* This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure 3.3: The Effect of the First District Split on Firm-Level Intensive-Margin Outcomes

(a) Outcome: Formal Taxes and License Fees as % of Revenue

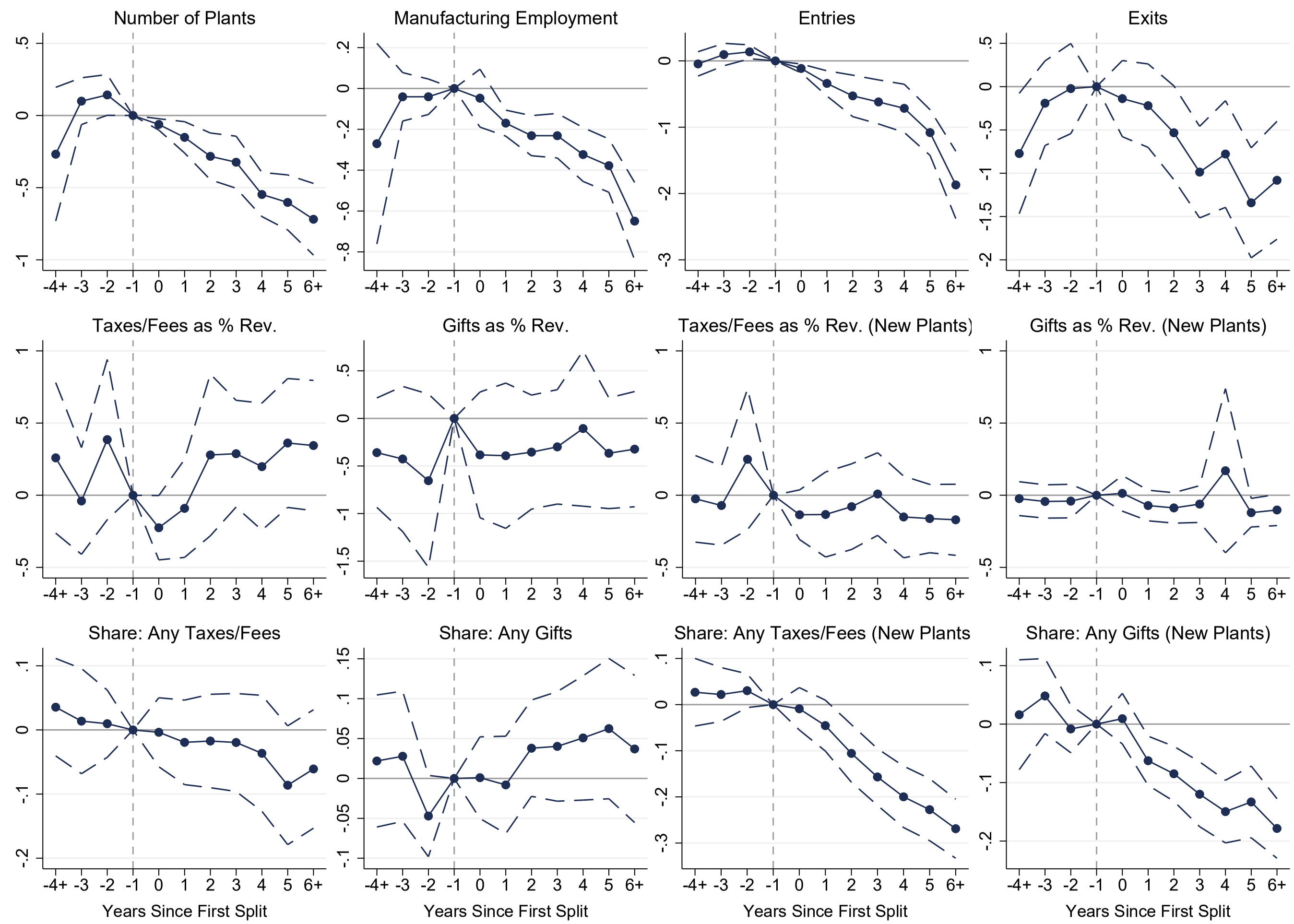



(b) Outcome: Gifts to Others as % of Revenue




*Notes:* This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure 3.4: The Effect of the First District Split on Aggregate District-Level Outcomes




*Notes:* Each figure plots the event-study coefficients from specifications 3.3 (graphs in top row) and 3.4 (graphs in rows 2 and 3), modified as an event-study specification to estimate coefficients in each year preceding and following a district split. All graphs use the full sample of district splits.

# APPENDIX A

# Chapter I Supporting Material

## A.1 Appendix

### A.1.1 Analysis of the Exemption Eligibility Threshold in the CenVAT

In the main text, I discuss firms' response to the *CenVAT exemption threshold*, which is similar to the standard VAT exemption threshold. One of the key differences from a standard VAT is that not all firms are eligible to take this exemption even if their revenue is below the exemption threshold in that year. Exemption eligibility depends on whether their revenue in the previous year was below the *exemption eligibility threshold* of ₹40 mn (or ₹35 mn before 2008).

The exemption eligibility threshold creates a notch in firms' tax liability in the following year at the threshold in revenue this year. The size of this notch is firm and time-specific, and depends on a number of factors.

Consider the one-period profit maximization problem of a firm. Let $\Pi_{it|AR}$ denote the profit a firm that is CenVAT registered and $\Pi_{it|E}$ that takes the SSI exemption at time $t$ and may or may not be CenVAT registered depending on their revenue in $t$. Firm's profit in period $t$ if they are eligible to take the SSI exemption denoted by $\Pi_{it|C}$ is:

$$\Pi_{it|C} = \max\left\{\Pi_{it|AR}, \Pi_{it|E}\right\}$$

$\Pi_{it|C} \geq \Pi_{it|AR}$ by definition, highlighting that firms' profit if they can choose the exemption will be at least as high as their profit if they are ineligible for the exemption.

Now consider the firm's problem in period $t-1$:

$$\Pi_i = \max_{M_{it-1}, E_{it-1}, \hat{R}_{it-1}} \Pi_{it-1}(M_{it-1}, E_{it-1}, \hat{R}_{it-1}) + \beta\{\mathbf{I}(\hat{R}_{it-1} > \tilde{R})\Pi_{it|AR} + \mathbf{I}(\hat{R}_{it-1} < \tilde{R})\Pi_{it|C}\}$$

where $\Pi_i$ is the present value of total profit in both periods, $\Pi_{it-1}$ is the one-period profit at time $t-1$, which is a function of inputs $(M_{it-1}, E_{it-1})$ and reported revenue $\hat{R}_{it-1}$ at time $t-1$, and $\beta$ is their discount factor. The above expression makes it clear that the notch depends $\beta$ and the difference in firm's profit in period $t$ with and without the exemption eligibility, $(\Pi_{it|C} - \Pi_{it|AR})$.

I find that the distribution of output among taxable goods producers around the threshold $\tilde{R}$ is smooth and similar to the distribution of exempt goods producers. This could be due to a number of factors: (1) the size of the notch is small for firms whose potential period $t-1$ output is close to but greater than $\tilde{R}$, i.e. firms who might be induced to bunch at the threshold, (2) optimization error is large, (3) enforcement is higher for firms at this output level and so output elasticity with respect to tax is lower  the audit manual of the tax authority specifically targets firms who have been below this exemption eligibility threshold for a few years, (4) the discount factor is very high, or (5) what firms value most about the exemption is not the reduction in tax liability but the reduction in compliance cost.

Ultimately, firms do not seem to respond to the exemption eligibility notch, and it does not affect firms' behavior at the exemption threshold.

### A.1.2 Data Cleaning

There are approximately 442,533 unique firm-year observations, and 820,987 firm-product-year observations because there are firms that produce multiple products. Although revenue is reported separately for each product, inputs and other firm-level variables are not. I apportion employment and input costs to each product produced by the firm according to its share in the total revenue of the establishment. Although the data pertain to establishments and not firms, I treat them interchangeably because most of them are single-establishment firms. I exclude establishment-year observations where the establishment had been closed or not operating in the last three years, which reduced the sample to 441,394 establishment-years. I also exclude government owned or cooperative establishments, which further reduces the sample to 427,938 establishment-years. Excluding establishments wholly or partially owned by public entities, I am left with 413,202 establishment-years. For most of the analysis I exclude establishments in states with area-based CenVAT exemptions either for the whole state or for large manufacturing hubs within the state. This reduces the sample to 321,635 observations. Similarly, I exclude establishments that have ever exported because exports are zero-rated regardless of the commodity produced. This leaves me with a final sample of 272,592 observations. Finally, I follow Allcott, Collard-Wexler and O'Connell (2016) to identify and exclude extreme outliers in terms of key production variables such as revenue, employment, electricity use, input use, and productivity, which leaves me with 215,395 establishment-year observations.

### A.1.3 Voluntary Registration

Although tax liability is higher for a registered firm under the CenVAT, some firms' profits may also be higher if they register. As a result, in the CenVAT as with other VAT systems, some firms choose to voluntarily register. The voluntary registration decision depends primarily on the whether their potential buyers are registered CenVAT businesses, or if they are unregistered entities such as unregistered firms or final consumers, who cannot avail of input tax credits. A second determinant, conditional on firms being able to sell to both registered and unregistered firms, is their taxable input costs as a share of revenue.

From equations (1.1) and (1.2), we can see that holding all prices and quantities fixed, tax liability is higher under the CenVAT. However, in practice, firms can face a different output price depending on whether they are registered, which means the increase in their revenue from registering can outweigh the increase in tax liability. Whether this is the case depends on share of the firm's sales to registered businesses compared to their share of sales to unregistered businesses or final consumers.

Consider an upstream firm A, deciding whether to register, who can sell their output to two potential downstream entities whose inputs are $G$ (produced by firm A) and $L$: (1) B, a CenVAT registered business and (2) C, an unregistered business or a final consumer. Profit for B and C is given by $\pi_B$ and $\pi_C$ as follows:

$$\pi_B = \begin{cases} R(G,L) - p^G G - wL - \tau R(G,L) & \text{if A registered} \\ R(G,L) - \tilde{p}^G G - wL - \tau R(G,L) & \text{if A not registered} \end{cases}$$

and,

$$\pi_C = \begin{cases} R(G,L) - p^G G - wL - \tau p^G G & \text{if A registered} \\ R(G,L) - \tilde{p}^G G - wL & \text{if A not registered} \end{cases}$$

$\pi_B$ only depends on the pre-tax price of input $G$ because the registered downstream firm B can avail of input tax credits, while $\pi_C$ depends on the after-tax price of $G$. If the market for firm A's product is mainly composed of registered businesses, like B, then A can fully "pass-through" the CenVAT to the downstream firm[1], leaving firm A's revenue, net of tax on their output, unchanged if they register. Moreover, once A registers, they can claim input tax credits, which lowers their tax liability on inputs.

On the other hand, if the market for A's output is composed of final consumers or unregistered businesses, and pass-through is less than 1, their net-of-output tax revenue falls. In the extreme case where firms sell only to registered businesses, they would always voluntarily register and at the other extreme if they only sell to unregistered businesses and final consumers, they would never register. Conditional on firms selling to both types in some proportion, Li Liu, Benjamin Lockwood and Miguel Almunia (2017) show that the registration decision depends on the share of their demand coming from final consumers or businesses, their reliance on taxable intermediate inputs in production, and competitiveness of the market matter for whether the firm values the exemption. The intuition is that the share of demand from final consumers determines the change in revenue net of output tax when firms register, while the intensity of taxable input use determines the decrease in tax liability on inputs. The higher the demand from final consumers, the lower benefit from registration and the higher the intensity of taxable input use, the higher the benefit from registration.

One concern that I address throughout the paper is that the observed difference in input cost shares of firms on either side of the exemption threshold arises because of the difference between the type of firms that value the exemption and therefore bunch below the threshold, and the type of firms that register voluntarily and do not bunch. I argue that the characteristics that determine whether firms will voluntarily register are determined by the product market in which the firm participates, but that the choice to bunch within a given product market depends on the firm's productivity.

In Liu, Lockwood and Almunia (2017)'s model, a firm's product is completely determined by their type. In this paper, there are characteristics of the commodity that a firm produces that are independent of the firm's own type. Specifically, the share of demand coming from final consumers, reliance on taxable intermediate inputs and competitiveness are independent of the firm's own type. Firms of various productivities can produce a given commodity. In the conceptual framework that I lay out in section 1.4, I analyze firm behavior within a commodity market taking the firm's choice of commodity and relevant characteristics of that commodity market as given.

[1]Because B receives input tax credits, there is no tax wedge in the price paid by B and the price received by A, and the concept of an "after-tax" price is purely theoretical.

## A.2 Theoretical Framework

Firms could be producing one of many commodities, indexed by $j$. Their choice of commodity determines the price received for their output if they register ($p_B^Y$), the price if they do not register ($p_C^Y$), and their reliance on taxable intermediate inputs ($\beta$). The price difference arises because of differences in demand for the product from other registered businesses and consumers. Below I describe firms' decision-making holding their commodity group (and therefore $p_C^Y, p_B^Y$ and $\beta$) as fixed. The commodity group also determines the applicable CenVAT rate $\tau$ and the output elasticity of tax-exempt intermediate inputs $\alpha$. I do not explicitly consider firms' choice of commodity market and treat this as exogenous. Within commodity markets, firms vary in their productivity $\omega_i$, which generates the revenue distribution.

Firms have two discrete choices: whether or not to underreport output and whether to register for the standard CenVAT. We can think of the firms' profit maximization in multiple steps. First, conditional on these two discrete choices they choose inputs to maximize conditional profits. Then they maximize profits over all conditions. Inputs are exempt intermediate inputs ($E_i$) like electricity and labor, and taxable intermediate inputs ($M_i$).[2] Production is Cobb-Douglas $F(E_i, M_i) = E_i^\alpha M_i^\beta$. $M_i$ and $E_i$ are costlessly adjustable each period[3].

To simplify the exposition and build intuition, first consider firms that do not underreport output.

### A.2.1 No Evasion

Firms' tax liability and output prices, and therefore their profits, depend discretely on their registration status and level of revenue. It is simplest to split their profit maximization decision into four possible cases. For a registration threshold of $\bar{R}$, firms maximize profit either: (I) Conditional on registering for CenVAT at any level of revenue (II) conditional on being unregistered with revenue below $\bar{R}$, (III) conditional on being unregistered with revenue equal to $\bar{R}$ or (IV) conditional on taking the SSI exemption with revenue above $\bar{R}$, whereby they register and remit tax only on turnover above the exemption threshold. The profit function in each of these four cases is described below where $\omega_i$ is the firm's

[2] In reality, capital is an important inputs as well, but including it does not change the analysis. I therefore omit them for simplicity.

[3] Because exemption eligibility depends on revenue in the previous period, there may be a dynamic aspect to firms' decision making. However, as I describe in appendix A.1.1 , this exemption eligibility threshold does not seem to influence firms' optimization and therefore I focus on the single period optimization of firms and drop the time subscript.

productivity and $\rho = 0$ if the firm takes the SSI exemption:

1. Case I: Firm registers at any level of revenue

$$\Pi^*_{\rho=1}(\omega_i) = \max_{E_i, M_i} \omega_i(1-\tau)p^Y_B E_i^\alpha M_i^\beta - p^E E_i - p^M M_i$$

2. Case II: Firm is unregistered with revenue below threshold

$$\Pi^*_{R_i<\bar{R},\rho=0}(\omega_i) = \max_{E_i, M_i} \omega_i p^Y_C E_i^\alpha M_i^\beta - p^E E_i - (1+\tau)p^M M_i$$

3. Case III: Firm is unregistered with revenue at exemption threshold

$$\Pi^*_{R_i=\bar{R},\rho=0}(\omega_i) = \max_{E_i, M_i} \bar{R} - p^E E_i - (1+\tau)p^M M_i$$

4. Case IV: Firm is registered with revenue above the exemption threshold, remits tax only on turnover above exemption threshold, and does not receive input tax credits

$$\Pi^*_{R_i>\bar{R},\rho=0}(\omega_i) = \max_{E_i, M_i} \omega_i p^Y_B E_i^\alpha M_i^\beta - p^E E_i - (1+\tau)p^M M_i - \tau(R_i - \bar{R}) - F$$

In Cases II and III, firms receive the price $p^Y_C$ on their output while in case I an IV firms receive $p^Y_B$. In Case IV, firms who take the SSI exemption must still register because their revenue is above the exemption threshold, but they only remit tax on their output above the exemption threshold. They incur the fixed compliance cost $F$, and they can provide input tax credits to downstream firms and so receive the price $p^Y_B$. In cases II, III and IV, firms cannot claim input tax credits and so the after-tax price of their taxable inputs is $(1+\tau)p^M$.

A comparison of equilibrium profits in Cases I and II gives us a sufficient condition for firms with revenue below $\bar{R}$ not to register:

$$\frac{p^Y_C}{(1+\tau)^\beta} \geq (1-\tau)p^Y_B \tag{A.1}$$

In the absence of fixed compliance costs, equation (A.1) is also a necessary condition. The quantity on the right-hand side is the after-tax price received by a registered firm. For a firm to prefer the exemption, the price they receive if unregistered must be greater than the price received if registered, scaled by $(1+\tau)^\beta$, which captures the additional input costs to forgo input tax credits. The lower the firm's reliance on taxable inputs ($\beta$), the more likely they are to prefer the exemption. If equation (A.1) fails for a given commodity market, there is a threshold level of productivity above which all firms would voluntarily register.

Empirically, any differences in the revenue to exempt input cost ratio around the exemption threshold that arise because of selection into bunching based on $p_C^Y$, $p_B^Y$, $\tau$ and $\beta$ should dissipate once we condition on commodity. Differences that remain depend on how the revenue to input cost ratio changes for firms that bunch based on their productivity draw within commodity markets.

Conditional on choosing the exemption, solving cases II to IV for firms' optimal input use yields the revenue distribution and corresponding revenue to input cost ratio:

$$R_i^* = \begin{cases} \left[\omega_i p_C^Y \left(\frac{\alpha}{p^E}\right)^{\alpha} \left(\frac{\beta}{(1+\tau)p^M}\right)^{\beta}\right]^{\frac{1}{1-\alpha-\beta}} & \text{if } \omega_i < \omega^1 \\ \bar{R} & \text{if } \omega^1 < \omega_i < \omega^2 \\ \left[\omega_i (1-\tau) p_B^Y \left(\frac{\alpha}{p^E}\right)^{\alpha} \left(\frac{\beta}{(1+\tau)p^M}\right)^{\beta}\right]^{\frac{1}{1-\alpha-\beta}} & \text{if } \omega_i > \omega^2 \end{cases}$$

where $\omega^1$ and $\omega^2$ are defined by the following conditions: $\omega^1$ is such that optimal revenue in case I is equal to the exemption threshold. $\omega^2$ is such that $\Pi^*_{R_i=\bar{R},\rho=0}(\omega^2) = \Pi^*_{R_i>\bar{R},\rho=0}(\omega^2)$.

Equilibrium revenue to exempt input cost ratio is

$$\frac{R_i^*}{p^E E_i} = \begin{cases} \frac{1}{\alpha} & \text{if } \omega_i < \omega^1 \\ \left(\frac{\bar{R}}{p^E}\right) \left(\frac{p_C^Y \omega_i}{\bar{R}}\right)^{\frac{1}{\alpha+\beta}} \left(\frac{\beta}{\alpha} \frac{p^E}{(1+\tau)p^M}\right)^{\frac{\beta}{\alpha+\beta}} & \text{if } \omega^1 < \omega_i < \omega^2 \\ \frac{1}{\alpha(1-\tau)} & \text{if } \omega_i > \omega^2 \end{cases}$$

Therefore, the average revenue to exempt input cost ratio of firms producing at the exemption threshold is higher than that of firms producing either above or below the exemption threshold. As we will see, this is in contrast with what we would expect when we allow for misreporting. This is because more productive firms require less inputs to produce the threshold level of output.

### A.2.2 With Evasion

Allowing for evasion, unregistered firms' optimization can be broken down into four cases, analogous to the cases without evasion:

1. Case I: Firm registers at any level of revenue
2. Case II: True revenue is below the exemption threshold and the firm is unregistered:
$$\Pi_{R_i<\bar{R},\rho=0} = \omega_i p_C^Y E_i^\alpha M_i^\beta - p^E E_i - (1+\tau)p^M M_i$$
3. Case III: True revenue is above the threshold, but the firm is unregistered and reports revenue at the threshold:
$$\Pi_{R_i>\bar{R},\hat{R}_i=\bar{R},\rho=0} = \omega_i p_C^Y E_i^\alpha M_i^\beta - p^E E_i - (1+\tau)p^M M_i - c(R_i - \bar{R})$$
4. Case IV: True revenue is above the threshold, the firm is registered and opts for the SSI exemption:
$$\Pi_{R_i>\bar{R},\hat{R}_i<R_i,\rho=0} = \omega_i p_B^Y E_i^\alpha M_i^\beta - p^E E_i - (1+\tau)p^M M_i - c(R_i - \hat{R}_i) - \tau(\hat{R}_i - \bar{R}) - F$$

where $\hat{R}_i$ is reported revenue, $c(R_i - \hat{R}_i)$ is the cost of misreporting and I make the widely used[4] assumption that the cost of misreporting depends on the *amount* of misreporting, $e_i = R_i - \hat{R}_i$, which gives optimal evasion as a function of the known CenVAT rate.

Let $\tilde{\omega}^1$ and $\tilde{\omega}^2$ denote productivity thresholds at which firms switch from case 1 to 2 and from 2 to 3. Reported revenue distribution and revenue to electricity ratio can then be stated as:

$$\hat{R}_i^* = \begin{cases} \left[\omega_i p_C^Y \left(\frac{\alpha}{p^E}\right)^\alpha \left(\frac{\beta}{(1+\tau)p^M}\right)^\beta\right]^{\frac{1}{1-\alpha-\beta}} & \text{if } \omega_i < \tilde{\omega}^1 \\ \bar{R} & \text{if } \tilde{\omega}^1 < \omega_i < \tilde{\omega}^2 \\ \left[\omega_i (1-\tau) p_B^Y \left(\frac{\alpha}{p^E}\right)^\alpha \left(\frac{\beta}{p^M}\right)^\beta\right]^{\frac{1}{1-\alpha-\beta}} - c_e^{-1}(\tau) & \text{if } \omega_i > \tilde{\omega}^2 \end{cases}$$

[4]see Amirapu and Gechter (2018); Best et al. (2015); Bachas and Soto (2018)

Reported revenue to electricity ratio:

$$\frac{\hat{R}_i^*}{p^E E_i} = \begin{cases} \frac{1}{\alpha} & \text{if } \omega_i < \tilde{\omega}^1 \\ \left(\frac{\bar{R}}{p^E}\right)\left[\left(\frac{\beta}{(1+\tau)p^M}\right)^{\beta}\left(\frac{\alpha}{p^E}\right)^{1-\beta} p_C^Y\right]^{\frac{1}{1-\alpha-\beta}} \left[(1-c_e(R_i-\bar{R}))\omega_i\right]^{\frac{-1}{1-\alpha-\beta}} & \text{if } \tilde{\omega}^1 < \omega_i < \tilde{\omega}^2 \\ \frac{1}{\alpha(1-\tau)} - \frac{c_e^{-1}(\tau)}{p^E}\left[\left(\frac{\beta}{(1+\tau)p^M}\right)^{\beta}\left(\frac{\alpha}{p^E}\right)^{1-\beta}(1-\tau)p_B^Y\right]^{\frac{-1}{1-\alpha-\beta}} \omega_i^{\frac{-1}{1-\alpha-\beta}} & \text{if } \omega_i > \tilde{\omega}^2 \end{cases}$$

# APPENDIX B

# Chapter II Supporting Material

## B.1 Appendix

**Measuring Tax-Exclusive and Tax-Inclusive Price in the Nielsen Data**

This appendix describes our investigation of Nielsen's price data to determine the accuracy with which tax-exclusive and tax-inclusive prices are recorded.

### B.1.1 Are Nielsen's Recorded Prices and Expenditure Tax-inclusive or Tax-exclusive?

The distinction between tax-inclusive and tax-exclusive price is crucial for an analysis of incidence or other impacts of taxation. Nielsen does not explicitly request consumers to enter the tax-exclusive price. Two variables provide information on expenditure. One is the *trip-level* total expenditure, the other is *item-level* expenditure given separately for each item purchased in the trip. Nielsen's documentation states that the *trip-level* total expenditure is tax inclusive but that the *item-level* expenditure is generally exclusive of tax. We test how often this is true by imputing our own measure of total *trip-level* tax inclusive expenditure from the *item-level* expenditure by adding up expenditure on each item, along with our measure of the applicable tax. If the *item-level* expenditure is always tax exclusive, and we are able to accurately impute the tax then the *imputed* measure of the *trip-level* expenditure should match the *actual trip-level* expenditure.

In the Nielsen documentation, they specify a number of reasons the *imputed trip-level* expenditure might not equal the actual *trip-level* expenditure ("total spent"). These include the trip price is generally tax inclusive, whereas the item prices are not; not all items in the trip are recorded by the panelist[1]; not all items purchased by the panelist are tracked by

[1]Nielsen Documentation, p66. "The panelist didn't scan all products purchased. Some items never make it into the home to get scanned. Consider items purchased at a hardware store that might get stored in the

Nielsen (only "fast moving" goods tracked)[2]; the scanner malfunctioned; and item price is censored (capped) at $999.99 for non-magnet items.

### B.1.2 Analysis of discrepancy: Predicted vs. Actual Tax-inclusive Expenditures

Applicable tax rates on items are estimated using zip-code level information on local sales tax rate and the exemption status of products recorded in LexisNexis. Any errors in *item-level* expenditure makes it more likely that there are discrepancies between *imputed* and *actual trip-level* expenditure in trips where more than one item was purchased, we separately analyze trips with one item versus multiple items (See Figure B.1 for the respective distributions of items per trip).

We generate two measures of discrepancies in tax inclusive expenditure. First, we calculate the difference between the *imputed trip-level* expenditure and the *actual trip-level* expenditure ("tax discrepancy"). We plot the densities of this measure separately for online and brick-and-mortar purchases. For both markets, there are mass points at common sales tax rates, suggesting an error in correctly applying the tax rather than an error in item price recording (See Figure B.2).

Next, because the *imputed tax-inclusive* expenditure may not have accurately assigned the tax rate, we restrict the sample to trips in which no exempt items were purchased and identify trips in this sample where *imputed* expenditure equals *actual* expenditure.

We collapse the total number of such purchases separately for online and BM retailers, from the trip level to the state -treatment month level (approximately 40 periods *50 states= 2080 observations), and plot weighted kernel smoothers for online and BM separately relative to VCA passage (See Figure 2.2). As expected, the number of online purchases with no sales tax is much higher than for brick purchases in the pre-treatment period, and fall sharply after VCA passage. However, the drop in online purchases without sales tax belies a minimal change in the levels: up to 30 months after a VCA, approximately 1 out of 4 purchases are untaxed compared to 1 out of 10 for brick purchases.

garage rather than being brought into the home, or a candy bar that was purchased and eaten before the consumer got home."

[2]Nielsen Documentation, p66. "Some items aren't "coded" by Nielsen - Nielsen mostly tracks fast-moving consumer goods (e.g. not most apparel, electronics or home furnishings, etc.)."

Figure B.1: Histogram of Number of Items Purchased per Trip

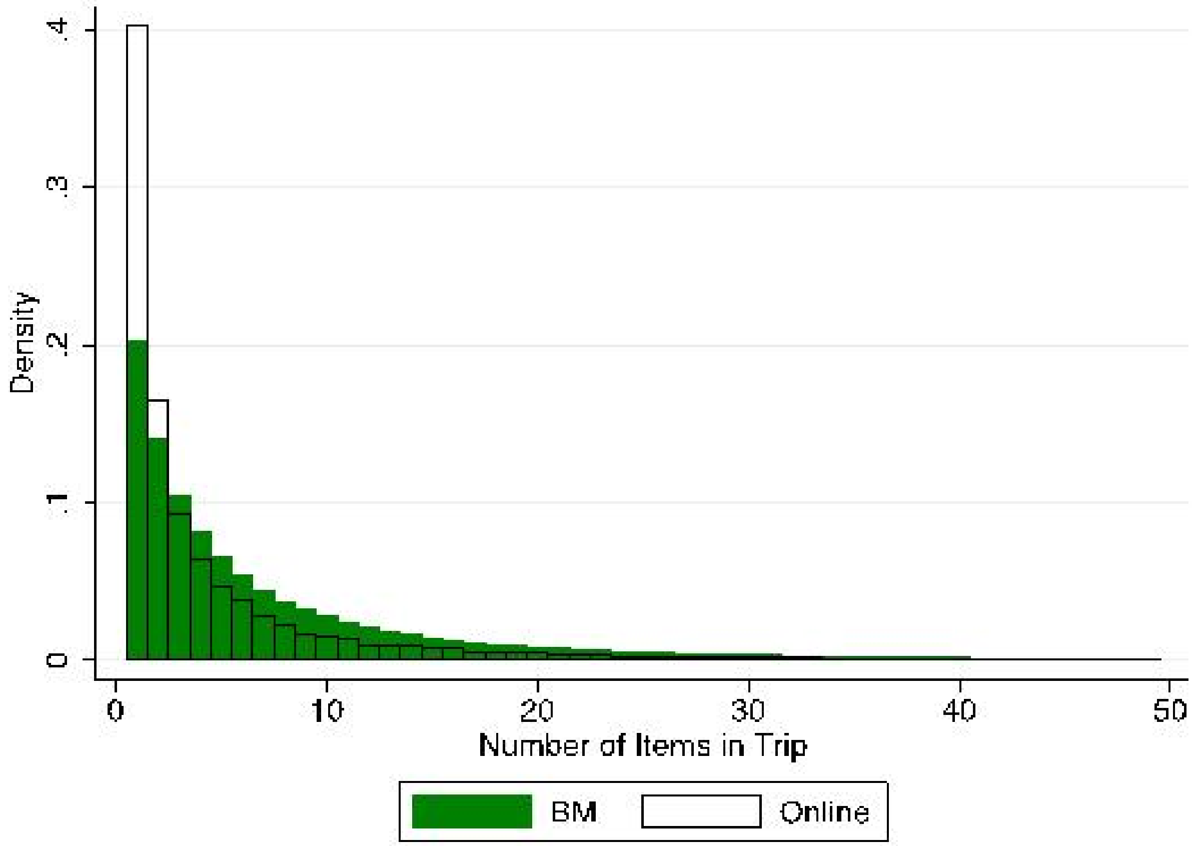
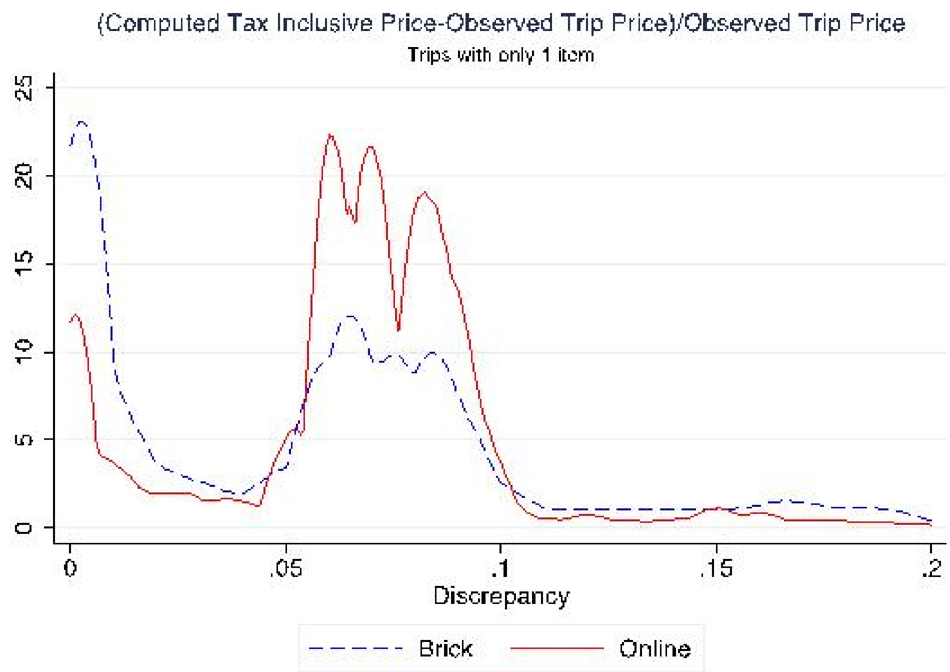
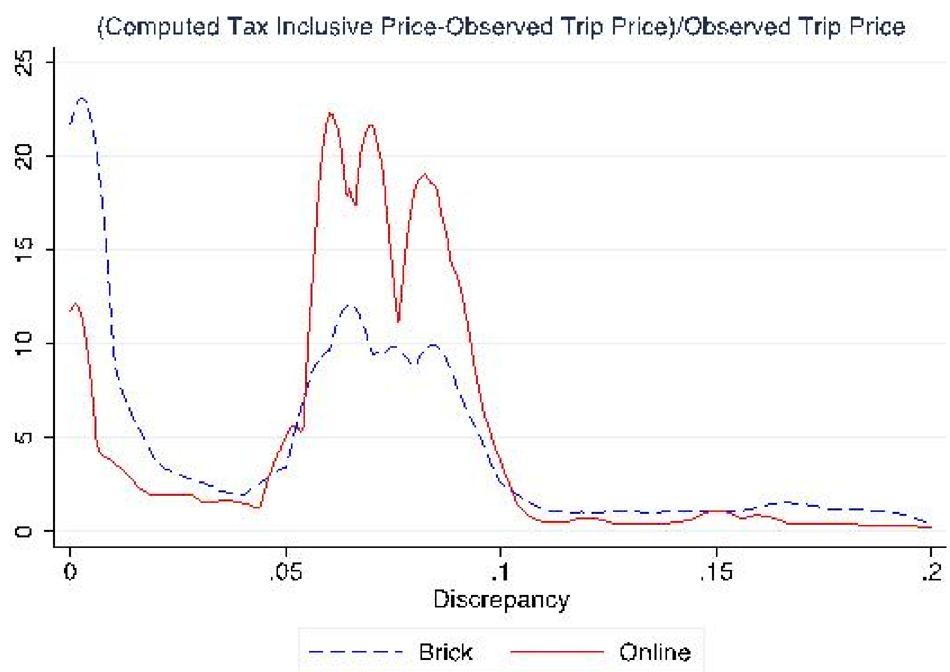


Figure B.2: Discrepancy between Computed and Observed Tax-inclusive Prices



### B.1.3 Appendix Figures

Figure B.3: Change in Average Monthly Household Expenditure on Taxable Goods Online at Large Retailers

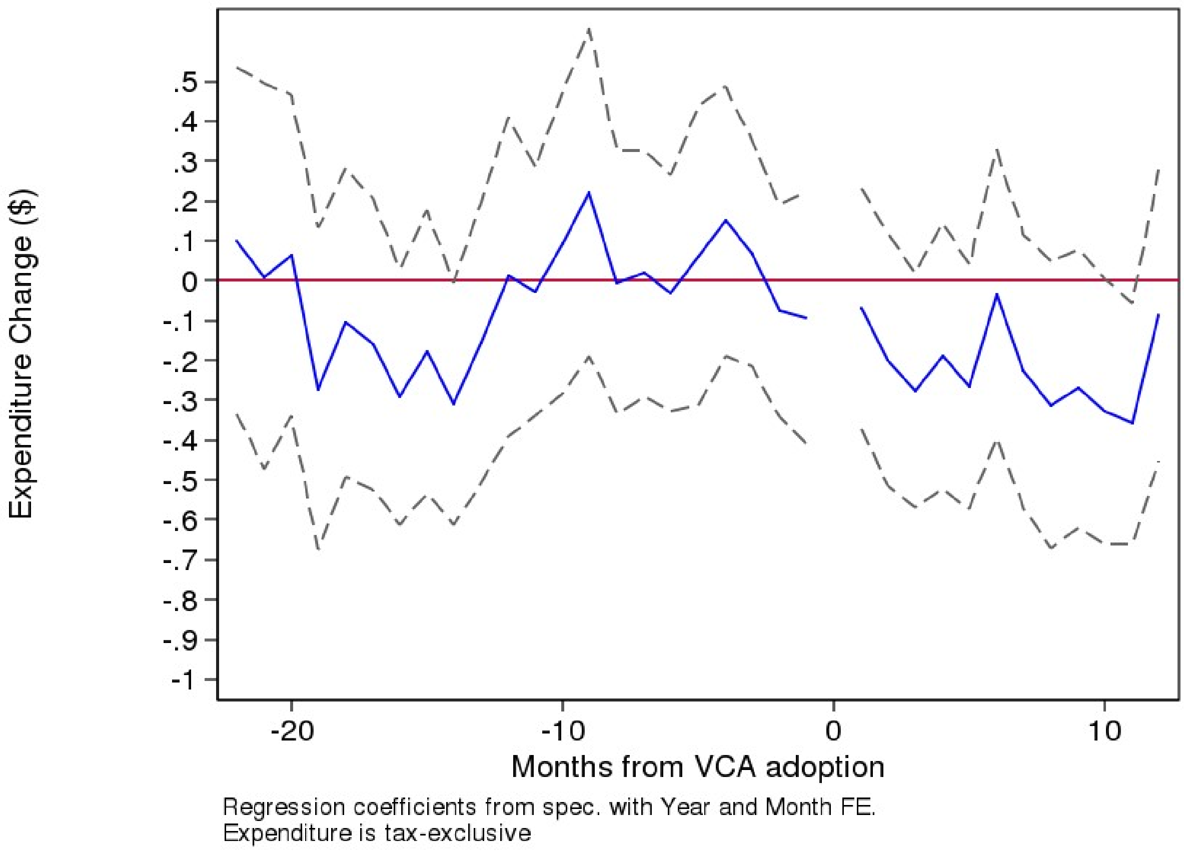


Figure B.4: Change in Average Monthly Household Expenditure on Taxable Goods Online at Small Retailers

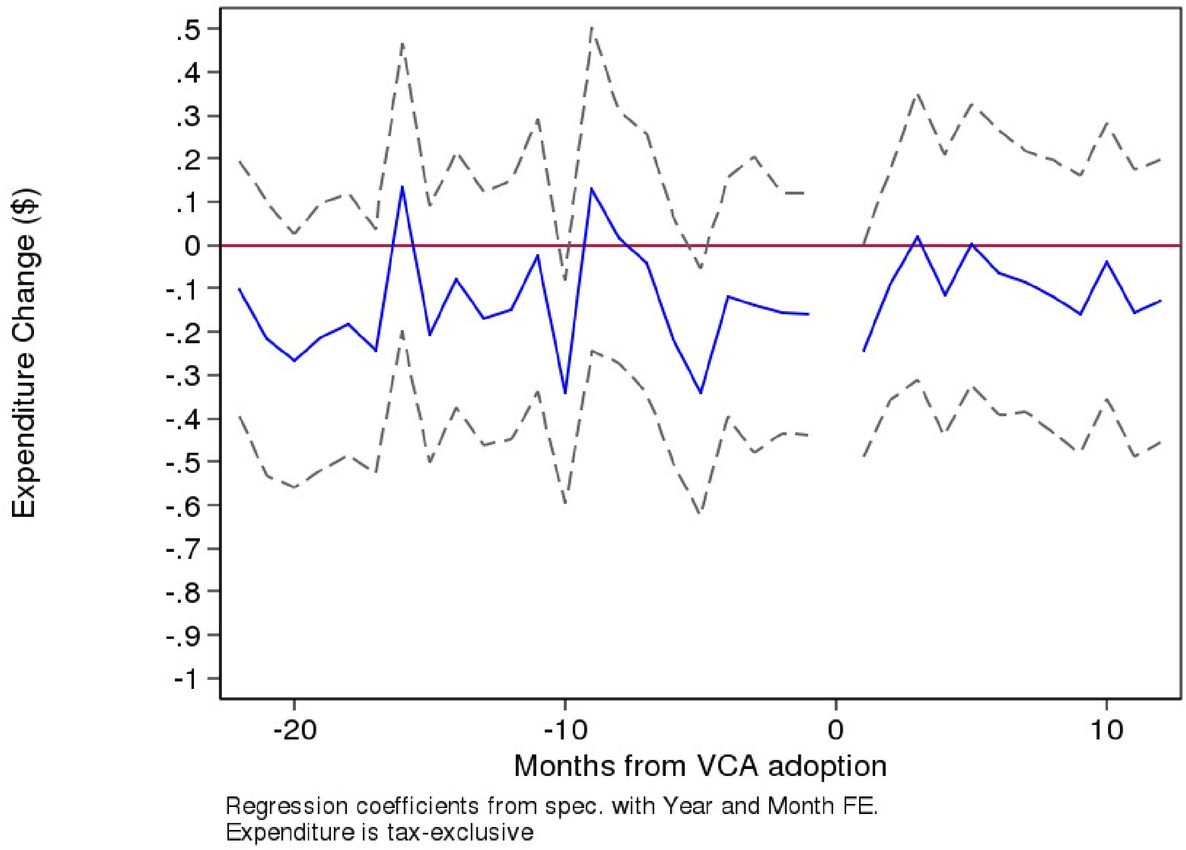
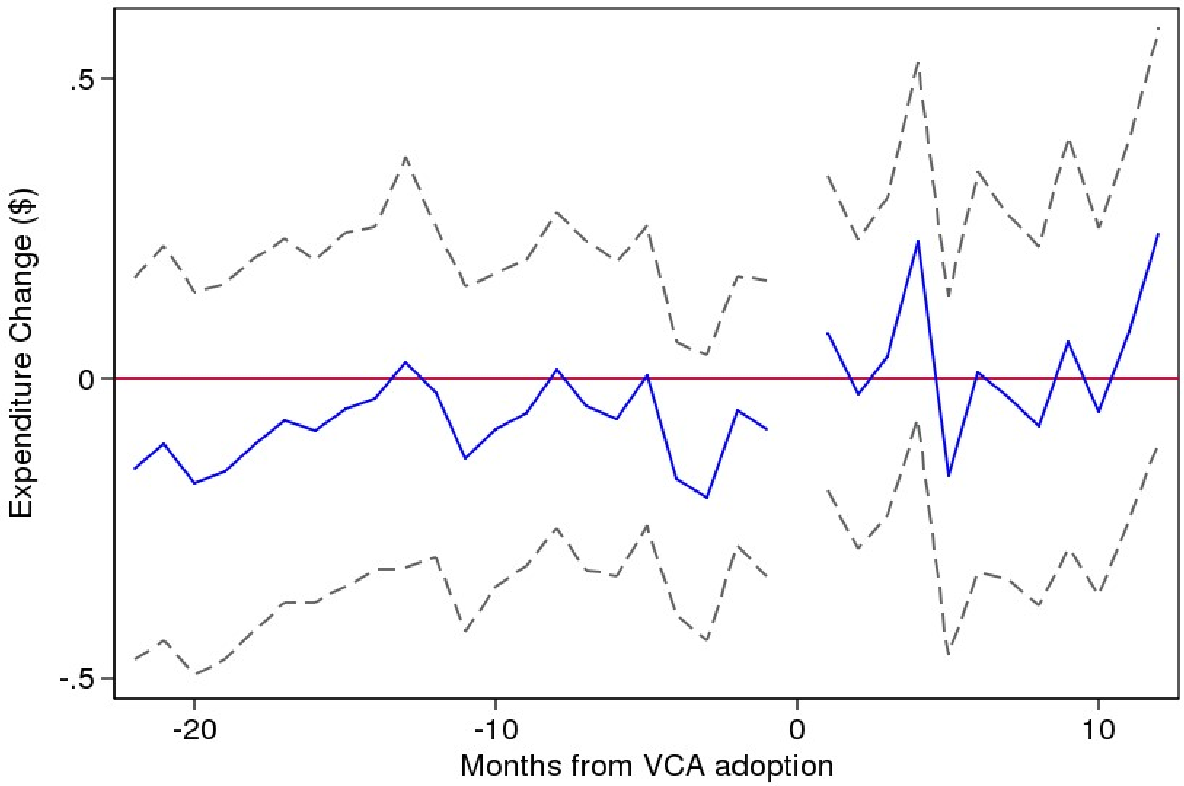
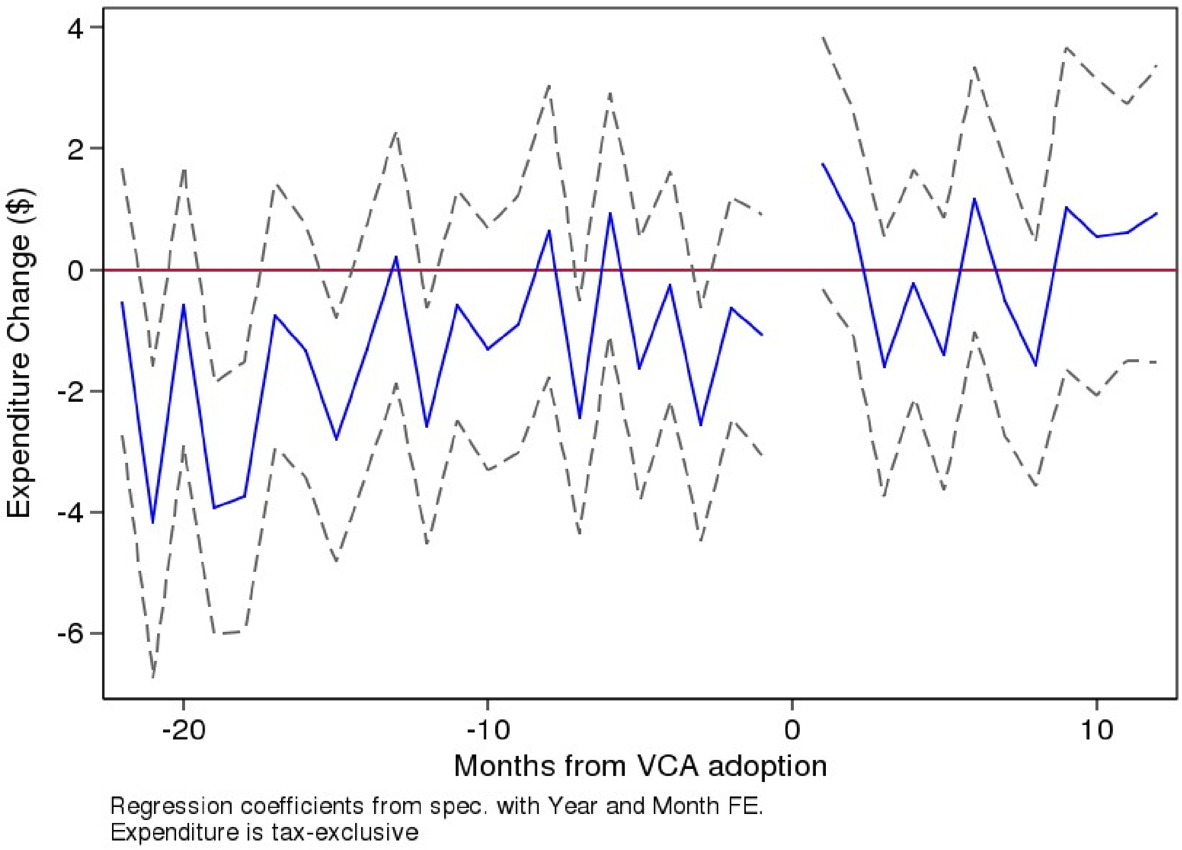


Figure B.5: Change in Average Monthly Household Expenditure on Exempt Goods Online




Regression coefficients from spec. with Year and Month FE.
Expenditure is tax-exclusive

Figure B.6: Change in Average Monthly Household Expenditure on Taxable Goods at Brick-and-Mortar Stores



# APPENDIX C

# Chapter III Supporting Material

## C.1 Appendix

### C.1.1 Additional Heterogeneity Analysis

#### C.1.1.1 Heterogeneity by Firm Size

Next we test whether the effect of district splits varies according to the size of the firm. Bai et al. (2019) find that when Vietnamese firms grow larger, they pay less in bribes as a share of firm revenue. They claim that the result operates through intergovernmental competition, arguing that larger firms are more mobile and thus can more credibly threaten to leave the jurisdiction. If firm size is negatively correlated with mobility costs, we would also expect that the fragmentation of districts would benefit larger firms more.

We do not find strong evidence of the impact of firm size on district splits in this context. Larger firms are somewhat less likely to see an increase in bribes as a result of splits in their original district and also less likely to see an increase in gift payments as a share of total revenue. Table C.1 displays the results from augmenting the baseline specification with the interaction term *Post 1st Split* $\times$ *Large Firm*, where *Large Firm* is an indicator variable that equals 1 for firms that recorded 200 or more employees at least once during the years prior to the district's first split. Now the coefficient on *Post 1st Split* measures the effect of splitting on small firms, and the coefficient on *Post 1st Split* $\times$ *Firm Size* measures how the effect of the first split varies according to firm size.

The full-sample results in Panel A show that overall, consistent with the baseline results, there is very litte impact on either the intensive or extensive margin of tax payments. On the other hand, there is a positive impact on gift payments in the both the intensive and extensive margin that is decreasing with firm size. If we assume that the effect is linear as we do in this specification, the impact of splits only decreases the probability of gift payments for very large firms. An increase in the number of employees at a firm by 100 is associate with a decrease in the effect of splits of gift payments by 0.1 percentage points. However, the impact of firm size on the effect of district splits is not statistically significant. Results in the sub sample of districts that split around the moratorium are consistent with the full sample results. The major difference in this sub-sample is that the impact of the first split on the probability of any gift payments is negative in magnitude but not statistically significant. Figures C.1 and C.2 show that for all outcomes in both groups of large and small firms, districts that split do not vary systematically from those that do not or have not yet split, before splits.

#### C.1.1.2 Heterogeneity by Spatial Concentration of Industry

Firm size is only one indicator of moving costs for a firm. Firms might rely on location-specific resources that make it more difficult for them to move in search of better local governance. Rothenberg et al. (2016) show that certain industries in Indonesia are more spatially concentrated than others because they tend to rely on locally available inputs such as natural resources and labor. The more spatially concentrated an industry, the higher the firms' moving costs and lower their bargaining power with district officials. We would therefore expect that firms in less spatially concentrated industries are more likely to benefit from district splits.

We examine how the impact of the district splits varies by industrial concentration of a firm's industry prior to the split using the Ellison and Glaeser (1997) measure of industrial concentration of industry $j$ as

$$\theta_j = \frac{G_j - \left(1 - \sum_d x_d^2\right) H_j}{\left(1 - \sum_d x_d^2\right)(1 - H_j)}, \tag{C.1}$$

where $H_j = \sum_f z_f^2$ is the Herfindahl index of concentration of employment across all establishments $f$ within industry $j$, and $G_j = \sum_d \left(x_d - s_{dj}\right)^2$ measures the sum of squared deviations between $s_{dj}$, the share of industry $j$'s national employment in district $d$, and $x_d$, the share of national employment in all industries in district $d$, where the district is defined according to the boundaries in 2000. Our variable of interest is the average value of spatial concentration of the firm's industry prior to the any district split where the firm was located in 2000 or in the earliest year they are observed in the data. Industry is identified by 4-digit ISIC codes. Higher values of $\theta_j$ indicate greater spatial concentration.

Table C.2 shows the results of interacting the post-split variable with this spatial concentration measure. We see some weak evidence of heterogeneity by spatial concentration.

The probability of any bribe payments is decreasing in spatial concentration, contrary to what we would expect since firms with high moving costs should have less bargaining power. However, on the intensive-margin, bribe payments as a share of total revenue increase by more for firms in more spatially concentrated industries. Tax payments decrease following splits on both the intensive and intensive margin and they fall by more for more spatially concentrated industries. Results from only the districts that split around the moratorium are shown in panel B and are very similar to results from the full sample.

Figures C.3 and C.4 show the results of the event-study specification 3.2 split by a sample of “low-concentration” firms, which are firms in industries with below-median spatial concentration (i.e. less than 0.028), and a sample of above-median or “high-concetration” firms. We see that for all of our outcome variables of interest, firms in these two groups exhibit parallel trends prior to the first split.

### C.1.2 Appendix Tables

Table C.1: Heterogeneous Effects of the First Split by Firm Size

| *Panel A: All Districts* | | | | |
|---|---|---|---|---|
| | Firm Paid Any: | | Payments as % of Revenue: | |
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | 0.001 | 0.039*** | -0.071 | 0.010 |
| | (0.015) | (0.012) | (0.060) | (0.030) |
| Post 1st Split × Firm Size | 0.005 | -0.010 | 0.022 | -0.011 |
| | (0.007) | (0.010) | (0.032) | (0.020) |
| Observations | 259,237 | 258,025 | 241,716 | 240,653 |
| District Clusters | 314 | 314 | 313 | 313 |

| *Panel B: Districts that Split in 2001–03, 2007–08* | | | | |
|---|---|---|---|---|
| | Firm Paid Any: | | Payments as % of Revenue: | |
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.023 | -0.001 | 0.024 | 0.013 |
| | (0.023) | (0.019) | (0.082) | (0.039) |
| Post 1st Split × Firm Size | 0.009 | -0.003 | 0.025 | -0.014 |
| | (0.007) | (0.008) | (0.033) | (0.022) |
| Observations | 25,649 | 25,602 | 23,929 | 23,882 |
| District Clusters | 73 | 73 | 73 | 73 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Firm Size* measured the maximum number of employees (in 1000s) recorded for a firm prior to the first split in their district. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table C.2: Heterogeneous Effects of the First Split by Industry Spatial Concentration

*Panel A: All Districts*

| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | 0.020 | 0.042*** | -0.067 | -0.029 |
| | (0.019) | (0.016) | (0.074) | (0.042) |
| Post 1st Split × Industry Spatial Concentration | -1.053 | -0.206 | -0.647 | 1.624* |
| | (0.669) | (0.320) | (1.437) | (0.956) |
| Observations | 248,730 | 247,528 | 231,944 | 230,880 |
| District Clusters | 311 | 311 | 310 | 310 |

*Panel B: Districts that Split in 2001–03, 2007–08*

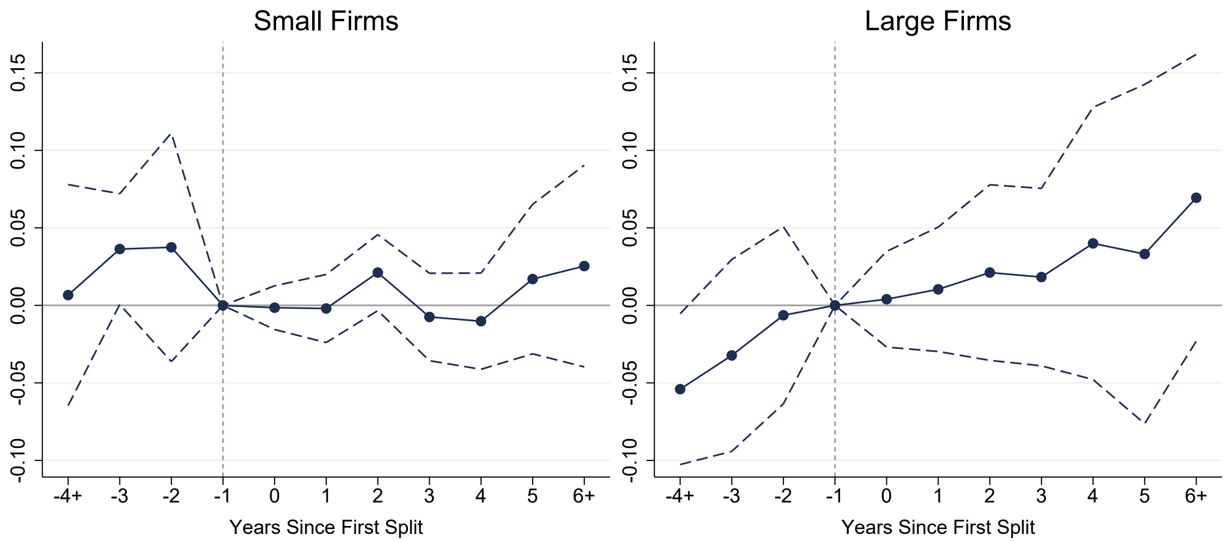
| | Firm Paid Any: | | Payments as % of Revenue: | |
|---|---|---|---|---|
| | (1) Taxes/Fees | (2) Gifts | (3) Taxes/Fees | (4) Gifts |
| Post 1st Split | -0.011 | -0.027 | 0.043 | -0.047 |
| | (0.025) | (0.022) | (0.084) | (0.052) |
| Post 1st Split × Industry Spatial Concentration | -0.828 | 1.159* | -1.684 | 2.910* |
| | (1.140) | (0.632) | (2.668) | (1.686) |
| Observations | 23,268 | 23,221 | 21,755 | 21,705 |
| District Clusters | 72 | 72 | 72 | 72 |

*Notes: Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Concentration* the value of the Ellison-Glaeser index of spatial concentration as given in equation (C.1). Higher values of the index indicate greater spatial concentration. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

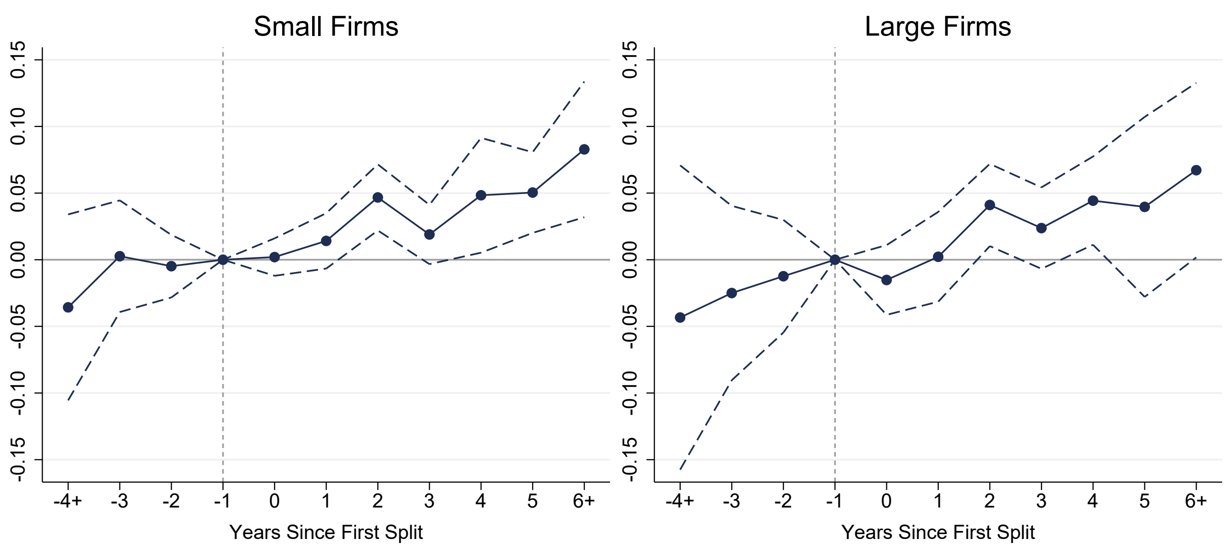
### C.1.3 Appendix Figures

Figure C.1: Heterogeneous Effects of the First Split by Firm Size: Extensive-Margin Outcomes

(a) Outcome: Firm Remitted Any Formal Taxes or License Fees



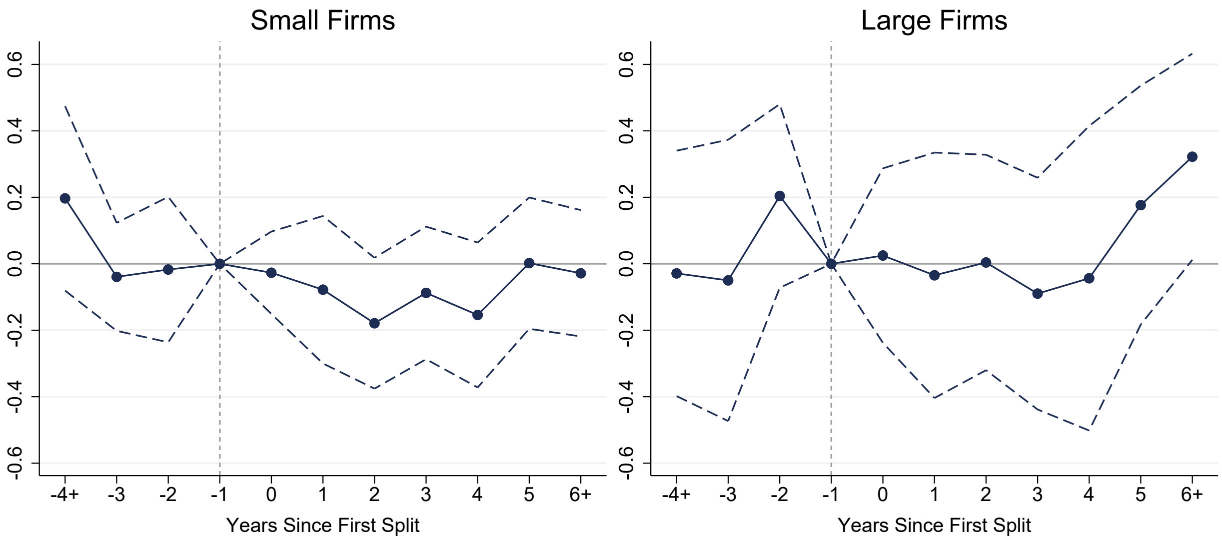

(b) Outcome: Firm Recorded Any Gifts to Others



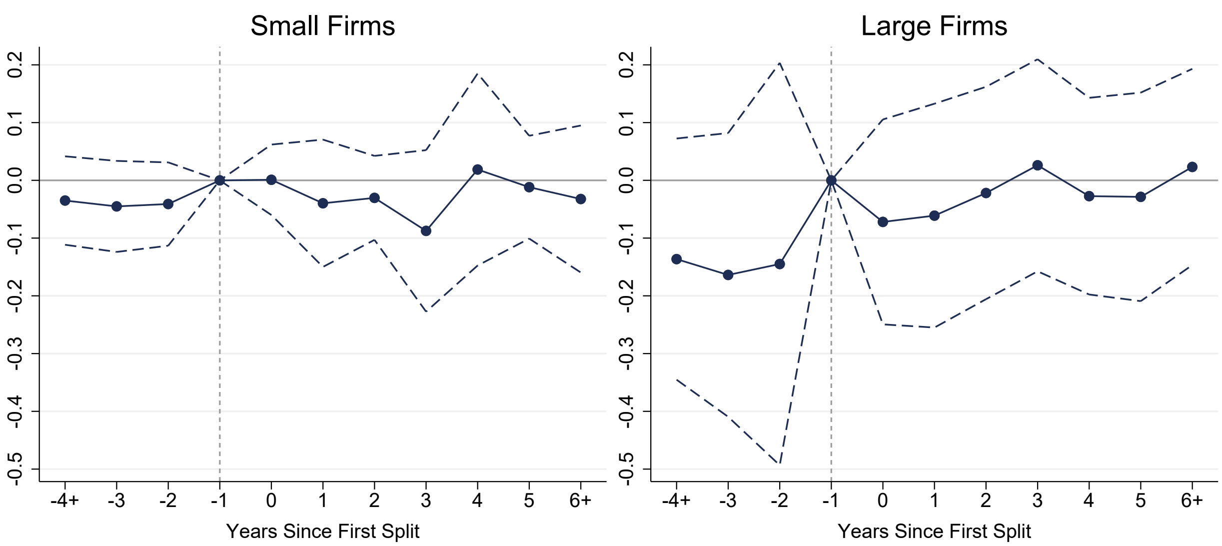

*Notes:* This figure plots the estimates of $\{\delta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure C.2: Heterogeneous Effects of the First Split by Firm Size: Intensive-Margin Outcomes

(a) Outcome: Formal Taxes and License Fees as % of Revenue



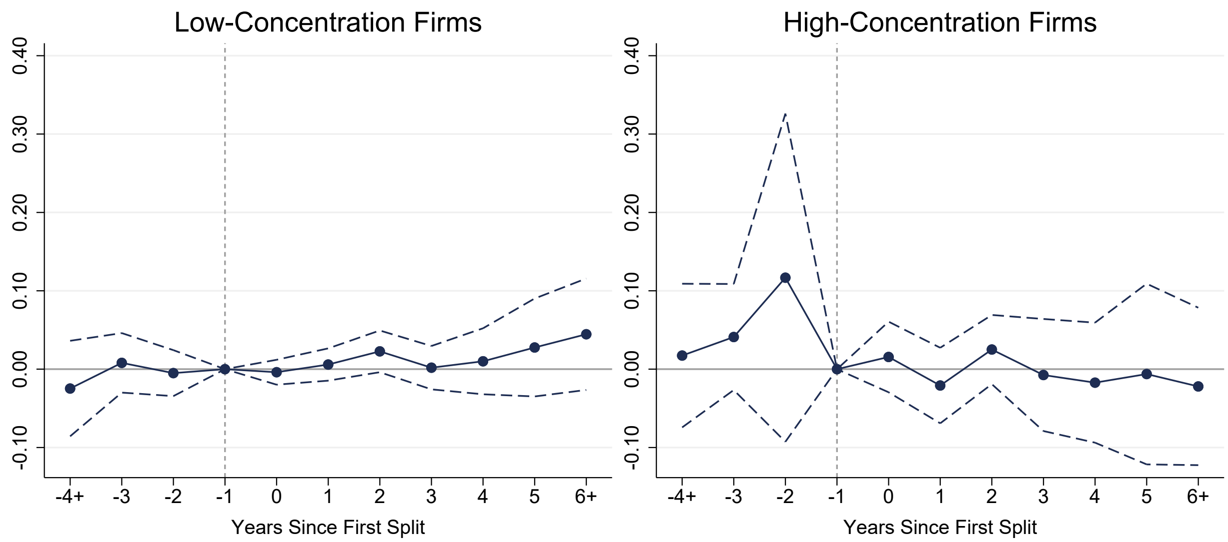

(b) Outcome: Gifts to Others as % of Revenue



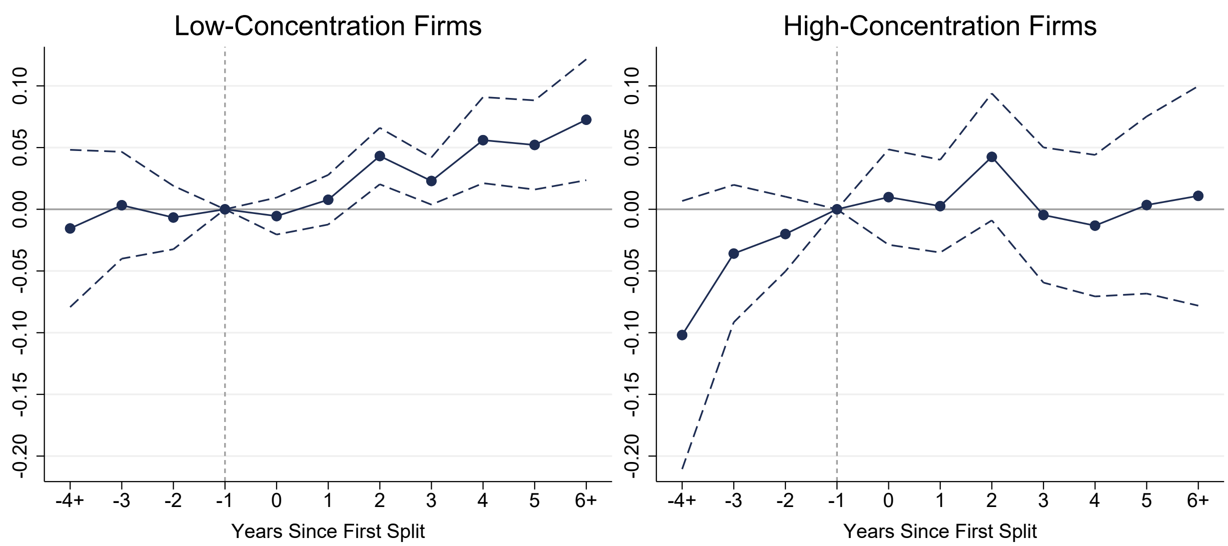

*Notes:* This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure C.3: Heterogeneous Effects of the First Split by Industry Concentration: Extensive-Margin Outcomes

(a) Outcome: Firm Remitted Any Formal Taxes or License Fees



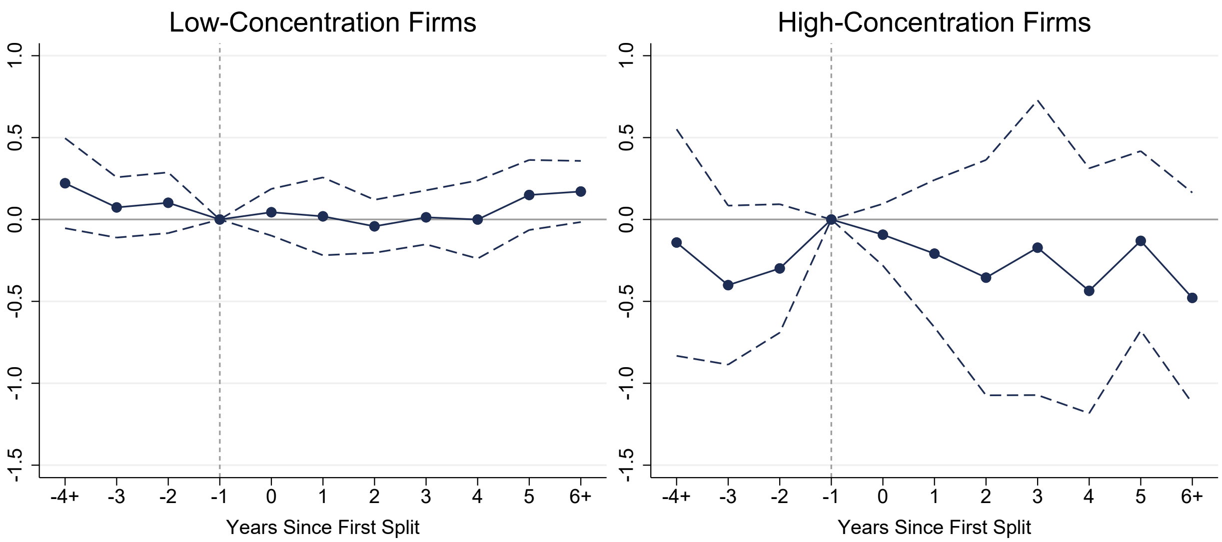

(b) Outcome: Firm Recorded Any Gifts to Others



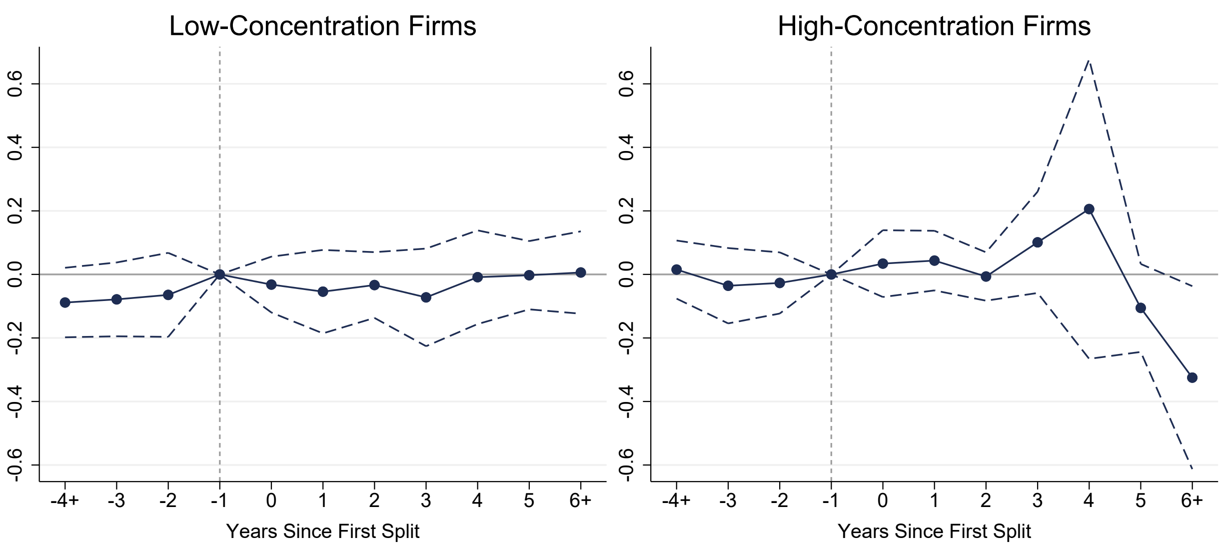

*Notes:* This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left is for a sample of "low-concentration" firms (i.e. firms in industries with below-median value of spatial concentration), and the graph on the right uses is for a sample of "high-concentration" firms.

Figure C.4: Heterogeneous Effects of the First Split by Industry Concentration: Intensive-Margin Outcomes

(a) Outcome: Formal Taxes and License Fees as % of Revenue




(b) Outcome: Gifts to Others as % of Revenue




*Notes:* This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (3.2) and their 95-percent confidence intervals. In each panel, the graph on the left is for a sample of "low-concentration" firms (i.e. firms in industries with below-median value of spatial concentration), and the graph on the right uses is for a sample of "high-concentration" firms.

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