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Corporate Tax Enforcement and Business Activity

John Gallemore
The University of North Carolina at Chapel Hill

Martin Jacob
IESE Business School

ABSTRACT: We examine the consequences of corporate tax enforcement for business activity. Using two different empirical approaches—a regional design and a firm-level design—we document that corporate tax enforcement is negatively associated with business activity, as measured by establishments and employment. This association is economically significant and is robust to tests that mitigate concerns regarding endogeneity and measurement. Furthermore, we find that the negative association between tax enforcement and business activity varies substantially in the cross-section. Specifically, we find that it is weaker for regions and firms with greater access to external financing sources and is stronger for regions and firms where compliance costs are likely higher and for which the *ex ante* costs of tax enforcement are greater. Our findings suggest that the effects of tax enforcement on business activity are economically important and heterogeneous, which should be of interest to academics and policymakers.

JEL Classifications: H25; H26; R11; R12.

Keywords: corporate tax enforcement; business activity; local economies; employment; compliance costs.

I. INTRODUCTION

Over the past decade, the Internal Revenue Service (IRS) has suffered significant budget cuts, negatively impacting its ability to conduct corporate tax return audits. Prominent media organizations and think tanks have highlighted this decline in corporate tax enforcement, and some policymakers have proposed significantly expanding the number of tax return audits.[1] For example, a centerpiece provision of the Inflation Reduction Act of 2022, signed into law on August 16, 2022, is $80 billion in new funding for the IRS, intended to help the agency increase tax enforcement. A key issue for academics and policymakers alike is understanding the unintended consequences of corporate tax enforcement beyond the effects on corporate tax collections. In this study, we explore how corporate tax enforcement affects business activity, namely investments in employment and establishments.[2]

The motivation for examining how tax enforcement affects business activity comes from both academic literature and policymaking. Although prior research has explored various consequences of tax enforcement, including tax avoidance (Hoopes, Mescall, and Pittman 2012), access to external capital (Guedhami and Pittman 2008; Gallemore and

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John Gallemore, The University of North Carolina at Chapel Hill, Kenan-Flager Business School, Department of Accounting, Chapel Hill, NC, USA; Martin Jacob, IESE Business School, Department of Accounting and Control, Barcelona, Spain.

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[1] Examples include MacMillan and Schaul (2021) and Horton (2018).
[2] By establishments, we refer to locations where business is conducted (e.g., factories, retail stores, and warehouses). We interchangeably refer to business activity and these specific aspects of such activity (growth in employment and establishments) throughout the study.

Jacob 2020), and transparency (Hanlon, Hoopes, and Shroff 2014; Bauer, Fang, and Pittman 2021), how tax enforcement affects business activity has received less attention. Regarding policy, documenting the effect of tax enforcement on investments in establishments and employment and how it varies in the cross-section is important for gauging the potential macro- and microeconomic effects of tax enforcement and thus improving the design of tax policy. Relatedly, one of the objectives of tax enforcement is to promote a fair and equitable tax system. If the effect of tax enforcement on business activity varies across firms or regions, then changes in tax authority resources and its monitoring of compliance with the corporate tax code can lead to winners and losers. Thus, it is critical to understand how tax enforcement affects business activity, both on average and in the cross-section.

Ex ante, the association between tax enforcement and business activity is ambiguous. On the one hand, tax enforcement could lead to lower business activity. For example, tax enforcement is associated with increased tax burdens (Hoopes et al. 2012), which all else equal should increase the cost of capital used when making investment decisions (Hall and Jorgenson 1967; Devereux and Griffith 1998; Graham, Hanlon, Shevlin, and Shroff 2017; Giroud and Rauh 2019), leading to lower business activity. Moreover, in the face of higher tax enforcement, firms will incur greater compliance costs to prepare for potential audits and comply with actual audits, and prior research suggests that these compliance costs can be a deterrent to growth (Djankov, Ganser, McLiesh, Ramalho, and Shleifer 2010). On the other hand, prior research also shows that tax enforcement may reduce information asymmetries (Hanlon et al. 2014; Bauer et al. 2021) and improve governance (Desai, Dyck, and Zingales 2007), which can improve firms' access to external capital (Gallemore and Jacob 2020) and reduce the cost of capital (Guedhami and Pittman 2008; El Ghoul, Guedhami, and Pittman 2011). These channels suggest that tax enforcement could be associated with more business activity. Thus, whether and how tax enforcement affects business activity is an empirical question.

To examine whether corporate tax enforcement affects business activity, we use two empirical approaches. In both approaches, we exploit the fact that U.S. corporate tax return audit rates vary both cross-sectionally (by firm size and, at times, by region) and intertemporally. In our first approach, we examine how tax enforcement affects business activity at the local economy level. This design exploits the fact that local economies vary in their exposure to firms of different sizes. Following Autor, Dorn, and Hanson (2013), we define the local economy at the commuting zone (CZ) level. We construct a CZ-level measure of exposure to corporate tax enforcement aimed at C-corporations of at least $5 million in assets (i.e., small- and medium-sized enterprises (SMEs) or larger firms) because firms below this threshold are rarely audited. Specifically, we use the share of CZ establishments in different firm size groups and the federal corporate tax return audit rates for those size groups. Because exposure to firms of different sizes varies across CZs as well as within a CZ over time and because the level of corporate tax enforcement faced by firms of a given size varies over time, our approach allows for both time-series and cross-sectional variation in CZ exposure to corporate tax enforcement. The CZ-level approach enables us to construct more complete measures of establishments and employment—our measures of CZ-level business activity—including from both public and private firms, at the cost of needing to estimate the CZ's exposure to corporate tax enforcement. In our second approach, we use a sample of publicly traded U.S. firms to explore whether tax enforcement is associated with firm employment growth (our firm-level measure of business activity). The benefit of this design is that we can match the firm to its size-based tax return audit rate, at the cost of not being able to examine private firms for which the consequences of tax enforcement may be more binding.

Using both the CZ-level and firm-level designs, we document a negative association between corporate tax enforcement and business activity. Moreover, our findings suggest that the impact of tax enforcement on business activity is economically important at the intensive margin. For example, in our CZ-level analyses, we find that an increase in the CZ-level corporate tax return audit rate by 5 percentage points (approximately an increase from the median to the 75th percentile) is associated with a reduction in employment growth of 0.9 percent, which, when aggregated across all sample CZs, would represent approximately 459,000 fewer jobs. The effect on the extensive margin (e.g., establishment growth) suggests that a 5 percentage point increase in the audit rate is associated with an aggregate decrease of approximately 590 establishments. In the firm-level specification, our findings imply that a 5 percentage point increase in the audit rate is associated with a decline in firms' employee growth of 0.85 percent; we find similar, albeit smaller, associations with other measures of firm-level business activity (e.g., R&D or capital expenditures).

Next, we explore the heterogeneity in the effect of tax enforcement on business activity. First, we find that the negative associations between CZ-level tax enforcement and both establishment and employment growth are weaker when the region has greater access to bank lending. Second, we find that the negative association between firm-level tax enforcement and employment growth is reduced for less financially constrained firms, which are not as reliant on internally generated cash flows to fund growth because of their access to external capital (Fazzari, Hubbard, and Petersen 1988). Thus, although the overall effect of tax enforcement on business activity is negative, these findings are consistent with certain benefits of tax enforcement (e.g., improved information environments) (Hanlon et al. 2014; Bauer et al. 2021), attenuating this effect by providing greater access to external capital (e.g., bank lending) (Gallemore and Jacob

2020). Third, we find that the negative association between tax enforcement and business activity is increasing in the expected compliance costs that firms likely face to prepare for potential audits or to comply with actual audits, consistent with tax enforcement negatively impacting business activity by imposing tax and compliance costs, which reduce the internal cash flows available to fund and the returns to growth (Djankov et al. 2010). Fourth, we find that tax enforcement exhibits a stronger negative association with business activity for firms and regions in closer proximity to IRS offices, which prior research finds to be positively associated with the likelihood of being audited (Kubick, Lockhart, Mills, and Robinson 2017). This finding is consistent with tax enforcement being more salient for these firms. Finally, we find that the negative effect of tax enforcement on firm-level employment growth is increasing in the extent of prior tax avoidance, consistent with these firms being more exposed to the tax and compliance costs of tax enforcement (Hoopes et al. 2012; Belnap, Hoopes, Maydew, and Turk 2022).

We also conduct several analyses to address endogeneity concerns and measurement issues with our baseline findings. First, we exploit the Internal Revenue Service Restructuring and Reorganization Act of 1998 (IRSRRA), which altered tax enforcement from being regionally directed to federally directed, to provide plausibly exogenous regional variation in tax enforcement. We continue to find a negative association between tax enforcement and business activity when comparing CZs (or firms) that experience a larger change in tax enforcement around the IRSRRA with CZs (or firms) experiencing a smaller decline. We also conduct two analyses specific to our CZ-level design. We continue to find a negative association using a shift-share approach, which fixes the CZ-level exposure to different size groups at the beginning of our sample period. We also conduct a falsification test, finding that the growth in self-employed establishments and income, which should be affected by local economic conditions but *not* by corporate tax enforcement, is indeed not associated with our CZ-level tax enforcement measure. Collectively, these three tests, each using a different approach, help mitigate endogeneity concerns. We also find that business entries, but not exits, are negatively associated with CZ-level tax enforcement, further reinforcing our primary findings.

Our study makes several contributions. First, we contribute to the literature on corporate tax enforcement, which has focused on the impact of tax enforcement on tax avoidance (Hoopes et al. 2012; Gupta and Lynch 2016; Kubick et al. 2017), tax evasion and tunneling (Desai et al. 2007; Mironov 2013), the cost of capital (Guedhami and Pittman 2008; El Ghoul et al. 2011), bank lending (Gallemore and Jacob 2020), firm performance (Mironov 2013; Belnap et al. 2022), and transparency (Hanlon et al. 2014; Bauer et al. 2021). We extend this literature in several important ways. First, we provide empirical evidence on how tax enforcement shapes business activity, as measured by growth in establishments and employment. We find that tax enforcement has a material effect on business activity and that this effect differs across firms and regions. Second, in contrast to prior work that focuses either on large publicly traded companies (Hoopes et al. 2012; El Ghoul et al. 2011) or on micro firms (Belnap et al. 2022), we examine the effect of tax enforcement for a broader sample of firms, including SMEs and private firms. We believe that it is valuable to understand how these firms are affected by tax enforcement because they make up a significant portion of overall business activity (Ayyagari, Demirguc-Kunt, and Maksimovic 2011; Doidge, Karolyi, and Stulz 2017; Lisowsky and Minnis 2020).

Our study also contributes to the literature on corporate taxation and business activity. Prior work in this area generally focuses on the effect of tax rules and rates on investment activity (Djankov et al. 2010; Ljungqvist and Smolyansky 2018; Ljungqvist, Zhang, and Zuo 2017; Ohrn 2018; Giroud and Rauh 2019; Langenmayr and Lester 2018; Lester 2019). Our study extends this literature and the literature on cross-sectional differences in the effect of tax policy on investment (Zwick and Mahon 2017; Zwick 2021; Gallemore, Hollander, Jacob, and Zheng 2024) by showing that other aspects of the corporate tax system, specifically the enforcement of the tax rules, also affect business activity in an economically significant and heterogeneous way.[3]

Finally, our study contributes to tax policymaking. Our findings point to tradeoffs in determining the optimal level of tax enforcement; although tax enforcement can provide benefits in the form of greater tax compliance, improved information environments, and better access to external capital, it can also reduce after-tax cash flows and thus limit business growth. Although the magnitudes implied by our findings suggest that the negative implications of tax enforcement for business activity are unlikely to outweigh the benefits of enforcement for tax compliance and revenues, our results collectively suggest that policymakers should consider the real implications—both on average and cross-sectional—of changing the level of tax enforcement. These findings are timely given the trend toward reduced tax authority budgets and tax enforcement around the globe (Alexander, De Vito, and Jacob 2020) and recent policies that seek to reverse that trend through higher tax enforcement expenditures (such as the Inflation Reduction Act of 2022). Relatedly, in the United States, the Congressional Budget Office scores the effects of tax enforcement expenditures on tax revenues.

[3] Fox, Jacob, Wilde, and Wilson (2022) provide some empirical evidence on this topic using a sample of multinationals. However, the tax enforcement actions in their study are unique (European Commission reviewing tax rulings of four EU countries), and their results might not generalize to more common tax enforcement methods (i.e., tax return audits).

However, to the best of our understanding, these estimates do not take into account the impact of tax enforcement on business activity, which affects future taxable income and, consequently, future tax revenues. Thus, our findings suggest that policymakers should consider the business activity consequences of tax enforcement when quantifying the budgetary impact of tax enforcement expenditures.

II. HYPOTHESIS DEVELOPMENT

Ex ante, it is unclear whether and how tax enforcement will be associated with business activity. On the one hand, greater tax enforcement may reduce growth in establishments or employment by imposing tax and compliance costs on existing and potential businesses. Regarding tax costs, the greater the likelihood of an audit, the higher the probability that tax avoidance or tax evasion will be detected and that the business will have to repay any taxes owed plus penalties and fines. This is consistent with prior research's findings that greater tax enforcement is associated with higher tax payments and effective tax rates (ETRs) (Hoopes et al. 2012; Gupta and Lynch 2016; Kubick et al. 2017).[4] Because prior research finds that higher tax rates are associated with less business investment (Hall and Jorgenson 1967; Devereux and Griffith 1998; Djankov et al. 2010) and reduced employment and establishments (Giroud and Rauh 2019) and that firms use ETRs (as a proxy for the marginal tax rate) when making investment decisions (Graham et al. 2017), this suggests that increased tax burdens due to higher tax enforcement can dampen business activity. Consistent with this line of thought, there is also evidence that ETRs, which are increasing in tax enforcement, or tax enforcement itself is positively associated with the cost of capital (Goh, Lee, Lim, and Shevlin 2016; Hopkins, Lusch, and Stekelberg 2023). Furthermore, an extensive literature finds that investment depends, in part, on internally generated funds, especially for firms with less access to external capital sources (Myers and Majluf 1984; Fazzari et al. 1988; Almeida and Campello 2007). All else equal, tax enforcement should reduce after-tax internally generated cash flows, which again could induce less business activity.

Additionally, tax enforcement can force firms to incur compliance costs both before and during an audit. Firms selected for an audit need to prepare for their IRS meetings, including supplying any documentation requested by the IRS. In doing so, firms may seek out professional expertise from accountants and lawyers. Moreover, when the likelihood of a tax audit is higher, firms may spend more time and resources on record-keeping and external assistance (e.g., lawyers and accountants) to prepare for a potential audit. Tax enforcement may also force firms to incur opportunity costs, as management and other employees shift efforts from productive activities (e.g., day-to-day operations or implementing strategy) to compliance tasks, such as collecting documentation and managing interactions with the relevant tax authorities.[5] Consequently, even if tax enforcement primarily discourages illegal tax evasion or aggressive tax avoidance, the increased compliance costs associated with greater tax enforcement may inhibit the expansion of existing firms (e.g., via employment) and reduce the incentives to form new businesses. Consistent with this idea, prior research suggests that higher compliance burdens reduce business activity (e.g., lower business entries) (Djankov, La Porta, Lopez-de-Silanes, and Shleifer 2002; Djankov et al. 2010). Furthermore, Belnap et al. (2022) find that actual tax audits are associated with a reduced likelihood of continuing as a going concern, albeit only for firms that underreported taxes.

On the other hand, stricter tax enforcement may increase business activity via improved access to external capital. Specifically, tax enforcement, by reducing tax avoidance, can improve both the quality of the information reported on the tax return as well as via other sources (e.g., audited financial statements). This in turn could facilitate greater access to external capital sources. Consistent with this idea, prior research shows that tax enforcement improves firm transparency and reduces information asymmetry between the firm and external stakeholders (Hanlon et al. 2014; Bauer et al. 2021). Other studies connect the improved information environments associated with corporate tax enforcement to lower bond yield spreads (Guedhami and Pittman 2008), greater access to bank lending (Gallemore and Jacob 2020), and a lower cost of capital (El Ghoul et al. 2011). This stream of research suggests that corporate tax enforcement, by increasing transparency and facilitating access to external capital markets, could lead to more business activity.[6]

In summary, tax enforcement can lead to higher ETRs, reduced internally generated cash flows, and an increased compliance burden, which may reduce business activity. Alternatively, tax enforcement may improve transparency and

[4] Additionally, tax code complexity can lead firms to pass up on tax breaks to which they are entitled (Zwick 2021), and this may be exacerbated when firms perceive a greater likelihood of a tax audit, leading to higher tax payments.

[5] As anecdotal evidence, we spoke to two individuals with prior experience with the preparation for and compliance with IRS audits on both the corporate or public accounting sides. They indicated that the time, effort, and cost that went into dealing with IRS audits were meaningful.

[6] Relatedly, prior research suggests that corporate tax avoidance and managerial diversion may be complements (Desai et al. 2007) and that tax enforcement can lead to improved corporate governance by reducing tax avoidance (Mironov 2013). Hence, tax enforcement could lead to a reallocation of internal funds from managerial diversion (e.g., perquisite consumption) to expanding business activity (e.g., hiring and new facilities).

facilitate access to external capital, thus leading to greater business activity. Because it is unclear which effect will dominate, we state our hypothesis in the null.

H1: Corporate tax enforcement is not associated with business activity.

Furthermore, the effect of tax enforcement on business activity may vary in the cross-section along several dimensions. First, this effect could depend on whether firms are financially constrained or have access to external capital, as these factors shape the importance of internally generated cash flows for funding growth (Fazzari et al. 1988; Almeida and Campello 2007). Additionally, firms may vary in their ability to bear tax-related compliance costs (Zwick 2021; Gallemore and Labro 2015). Finally, tax enforcement could differentially affect firms based on their proximity to the tax authority or the extent of their tax avoidance (Hoopes et al. 2012; Kubick et al. 2017). We explore whether the effect of tax enforcement on business activity varies in the cross-section in Section V.

III. RESEARCH DESIGN

U.S. Federal Corporate Tax Enforcement

Our analyses focus on U.S. federal tax enforcement of C-corporations. Following prior research (Guedhami and Pittman 2008; Hoopes et al. 2012; Hanlon et al. 2014; Gallemore and Jacob 2020; Bauer et al. 2021), our measure of tax enforcement is the probability of an IRS audit, which can be conducted either face-to-face, usually at either the taxpayer's headquarters or at an IRS office, or remotely (e.g., correspondence audits).[7] Until 1999, federal tax enforcement was performed by IRS district offices. Each district office was responsible for the corporate tax return auditing efforts for the firms headquartered in its district, resulting in both regional and firm size-based variation in federal corporate tax enforcement (Gallemore and Jacob 2020). Starting in 2000, as a result of the IRSRRA, the IRS reorganized into a federal structure, in which tax enforcement was directed by the main IRS office, resulting in firm-sized based variation in tax enforcement.

Since the IRSRRA, the IRS has two divisions that handle corporate audits: the Large Business & International (LB&I) division, which is responsible for tax enforcement for firms with total assets of at least $10 million, and the Small Business and Self-Employed (SBSE) division, which oversees tax enforcement for firms with total assets under $10 million. Both divisions use targeted audits (using algorithms to predict noncompliance or campaigns that focus on specific issues associated with noncompliance) and random audits to carry out tax enforcement responsibilities.[8] Firms are likely aware of the IRS auditing efforts and how they vary across time, firm size, and region. For larger firms, this awareness comes from their tax departments and access to tax planning intermediaries. For SMEs, it can be obtained via social networks, informing them of the experiences of similarly sized firms in their local area.[9] Both firm types, larger and smaller, would also have been aware of media coverage and government reports highlighting changes in tax enforcement.

In Figure 1, we present the variation in audit rates for C-corporations over our sample period (1992 to 2015) for the size classes used in our empirical analyses. We plot the audit rate by size group in Panel A, which shows that the audit rate is increasing in firm size and that audit rates have generally declined over our sample period. In Panel B, we present the annual changes in the scaled audit rate (1992 = 100) for each size group. Although the audit rates have generally declined, there is substantial variation in the extent and timing of audit rate changes across different size groups (see also Hoopes et al. 2012). For example, the audit rate generally declined over time for the $5 million to $10 million size group. By contrast, for firms with assets between $50 million and $100 million, there is a large decline from 1992 to 2003 and a subsequent increase between 2003 and 2012. Furthermore, before the year 2000, there is additional regional variation in audit rates not shown in Figure 1.

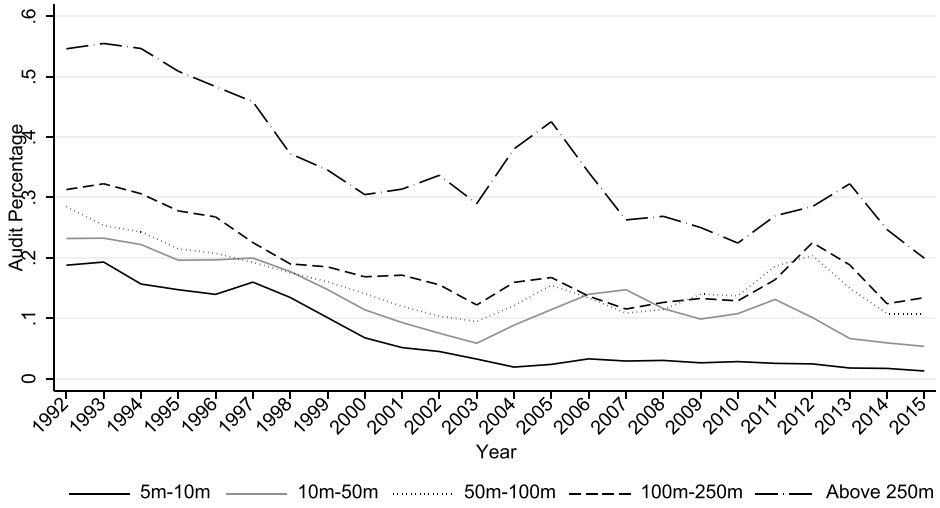
[7] The audit rate has advantages over other tax enforcement proxies, such as the amount of extra taxes assessed and penalties, for our purposes. Audits can impose tax costs (e.g., reduced tax avoidance) and compliance costs (e.g., record-keeping expenses), and these costs would not necessarily be captured by penalties or additionally collected taxes. For example, in the extreme of full tax enforcement, there may be no penalties or additional taxes assessed as firms do not avoid any taxes, but they still incur the tax and compliance costs of tax enforcement. Other possible alternatives (such as IRS staffing levels) are not broken out specifically for corporate audits or by firm size. We acknowledge that audit rates have drawbacks as a measure of tax enforcement (e.g., audit thoroughness may vary over time or by firm size). However, size-based audit rates have been shown by prior research to be associated with tax avoidance, supporting their usage as a measure of tax enforcement (Hoopes et al. 2012). Furthermore, aspects of our primary research design (e.g., including year fixed effects) and certain robustness tests (e.g., including size-class weights as additional controls in the "Additional Robustness Tests" section) help mitigate measurement error concerns.

[8] Our data do not break out audit rates by audit type (i.e., random versus targeted) or by IRS division (i.e., LB&I or SBSE).

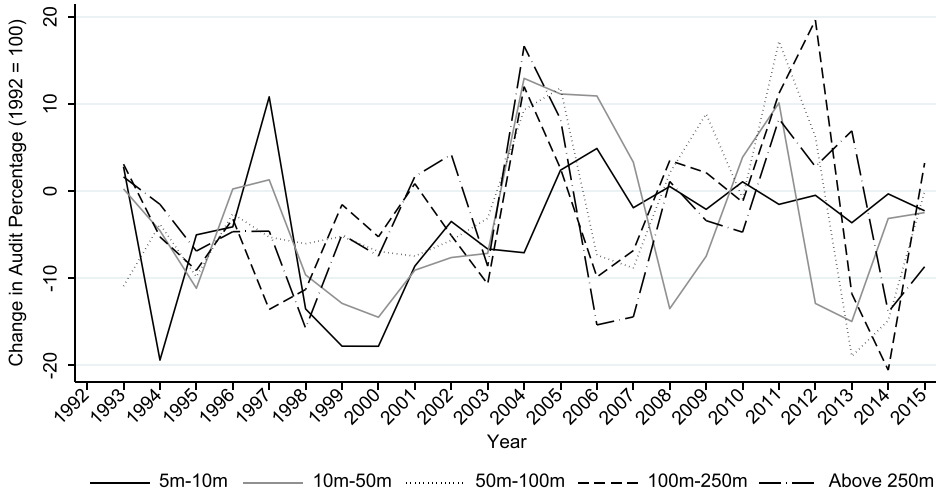
[9] This is particularly true for managers that are likely to be members of clubs and societies and have larger social networks (Carroll and Teo 1996), which prior research shows affects SME decision-making (BarNir and Smith 2002).

FIGURE 1
Trends in Audit Rates Across Size Groups Over Time

Panel A: Tax Return Audit Rates



Panel B: Changes in Tax Return Audit Rates



This figure presents the average audit rates by the IRS size category over the period 1992–2015 for the size categories used in our analyses. Panel A plots the absolute audit percentage by size category over our sample period. Panel B plots the relative change in audit percentage by size category over our sample period using 1992 as the base year (where each size category begins at 100 percent).

CZ-Level Research Design

In our regional analyses, we examine how the local economy's exposure to corporate tax enforcement affects its subsequent business activity. We define the local economy at the CZ level (Tolbert and Sizer 1996; Autor and Dorn 2013; Autor et al. 2013). CZs are regions (encompassing all U.S. metropolitan and nonmetropolitan areas) that exhibit strong commuting ties within the region and weak commuting ties across regions. Because the CZ concept combines economic activity and geography, it is well suited for defining local economies. There are 709 CZs in the United States using the definition from the year 2000. CZs vary widely in size, with populations ranging from 1,193 (Jones County, SD) to 16.4 million (the CZ including Los Angeles, CA) in 2000.

We measure CZ exposure to corporate tax enforcement using the amount of business activity conducted in the CZ by firms in different size groups and the federal tax return audit rates for those size groups. We define *Audit Rate*, our measure of tax enforcement for CZ c in year t, as: $Audit\ Rate_{c,t} = \sum_{i=1}^{N} \frac{Establishment_{s,c,t-1}}{Establishment_{c,t-1}} \times Audit\ Rate_{s,t,d}$, where $Audit\ Rate_{s,t,d}$ is the audit rate for C-corporations in size class s in year t (and in district d until 2000). We weight these size class-specific audit rates by the ratio of the CZ's establishments within that size class to the total number of CZ establishments in year $t-1$.[10] Our measures only use corporate tax return auditing efforts aimed at C-corporations with at least $5 million in assets, as firms below this level are rarely audited (audit rates of on average 1.1 percent during our sample period; see also Hoopes et al. 2012). The key challenge in developing a CZ-level audit rate is that the IRS tax return audit rate size classes are defined by total assets, whereas Census data define establishment size by the number of employees.[11] We develop a crosswalk that connects the asset-based tax return audit rates to the employment-based Census size classes using data on public firms from Compustat, which we describe in Appendix A. For each IRS asset bandwidth (e.g., assets from $10 million to $50 million), we calculate the distribution of employment across these firms. Based on these statistics, we assign each IRS asset category to the employment category that it best represents.[12] Appendix A also provides descriptive statistics supporting the crosswalk. Our results are robust to using alternative crosswalks (untabulated).

We use two measures of CZ-level business activity: employment and establishments. Business employment is a key measure of business activity of interest to policymakers and academics. Establishments are physical locations at which businesses provide services or where industrial activities are performed (Giroud and Rauh 2019). Therefore, employment (establishments) is akin to an intensive (extensive) margin measure of business activity. We measure business employment (establishments) using *Employment Growth* (*Establishment Growth*), defined as change in the natural logarithm of CZ-level employment (establishments) from t to $t+1$.[13] Because we focus on tax enforcement aimed at firms of at least $5 million in assets, we remove business activity that is unlikely to be associated with these firms (i.e., microfirm employment and establishments). Specifically, we remove establishments with fewer than 50 employees from our *Establishment Growth* measure, and we adjust *Employment Growth* to remove the impact of these establishments.[14]

To test our hypothesis at the CZ level, we estimate the following regression:

$$Business\ Activity_{c,t+1} = \beta_1 Audit\ Rate_{c,t} + Controls_{c,t} + \alpha_c + \alpha_t + \epsilon_{c,t+1}. \quad (1)$$

Business Activity is either *Establishment Growth* or *Employment Growth* in CZ c in year $t+1$ (i.e., the growth rate from year t to $t+1$), as defined above. *Audit Rate* is the CZ-level corporate tax return audit rate in year t as defined above. We standardize *Audit Rate* to have a mean of 0 and a standard deviation of 1 in our regression models.

We include several control variables to account for factors related to local business growth (all variables are defined in Appendix B). First, we follow prior research (Barro 1996; Djankov et al. 2010; Strobl 2011; Acemoglu, Naidu, Restrepo, and Robinson 2019; Giroud and Rauh 2019) and control for the natural logarithm of growth domestic product (GDP) per capita (*GDP per Capita*), the level of unemployment (*Unemployment*), and the natural logarithm of income per employee (*Income*), all defined at the CZ level. We also control for the state-level corporate tax rate (*State Tax*), combined reporting (*Combined*), throwback rules (*Throwback*), and property and sales apportionment factors (*Property Factor* and *Sales Factor*), following prior research (Luchs-Nunez 2024; Shaw 2022). Following Smolyansky (2019), we also include demographic controls (share of the population that is Asian, Black, Hispanic, under 4 years in age, and over 65 years in age); to construct these variables, we aggregate county-level values to the CZ level using the

[10] We measure establishments at $t-1$ to ensure that the weights to calculate the audit rate are not mechanically associated with changes in our dependent variable (measured from t to $t+1$). In untabulated analyses, we find that the weights are not correlated with subsequent establishments, suggesting that this mechanical association is unlikely to be a concern.

[11] Starting in 2010, the IRS provides data on additional size groups for firms with $250 million or more in assets (Hopkins et al. 2023). Because this refinement is not available for most of our sample period, we do not use it in our primary audit rate measure. Our results are robust to using the refined data from 2010 onward (untabulated).

[12] Establishments are not the same as firms. The 2019 County Business Patterns data (the earliest year available with data on firms and establishments) indicate that firms have, on average, 1.1 establishments. Furthermore, for all but the largest firms—which constitute a small percentage of overall firms—the average establishments per firm is effectively one. The largest firms are more likely to have multiple establishments (on average, 1.6 establishments), meaning that our crosswalk may inaccurately assign the audit rates applicable to large firms if they do not also have large establishments. Our inferences are robust to using alternative crosswalks, including removing the audit rate and business activity of firms with greater than $250 million in assets (results untabulated).

[13] We use logarithmic growth rates because (1) they ensure that positive and negative changes of equal absolute magnitude receive equal values and (2) they are less skewed than linear growth rates. Our results are robust to using linear (i.e., percentage change) growth rates (untabulated).

[14] To accomplish this, we follow the approach of Smolyansky (2019). We take the midpoint of each employee range (2.5 for 1–4 employees, 7 for 5–9 employees, 14.5 for 10–19 employees, and 34.5 for 20–49 employees) and multiply these midpoints by the number of establishments in the respective range to get an estimate of the employment by these establishments, which we then remove from the total employment measure. We set negative imputed employment values to missing. Our results are robust to using the total (i.e., unadjusted) establishment and employee count (untabulated).

county's population as weights. We also include CZ fixed effects (α_c) and year fixed effects (α_t). Another key research design choice relates to standard errors. As explained in detail in Adao, Kolesár, and Morales (2019), shocks can be correlated across regions that are not geographically near each other if the regions share other characteristics. To address this concern, we cluster our standard errors based on 100 groups of CZs that are similar in terms of the distribution of their establishment size classes.[15]

Firm-Level Research Design

To complement our CZ-level analyses, we conduct a firm-level analysis, using data on publicly traded U.S. firms to estimate the following regression:

$$Employee\ Growth_{i,t+1} = \beta_1 Audit\ Rate_{d,s,t} + Controls_{i,t} + \alpha_i + \alpha_s + \alpha_{ind,t} + \epsilon_{i,t+1}. \quad (2)$$

The dependent variable *Employee Growth* is defined as the logarithmic growth rate in the number of employees from t to t+1. The main independent variable is $Audit\ Rate_{d,s,t}$, which is audit rate in size group s in year t (and in district d until year 2000). We assign the firm to IRS districts based on its headquarters location, as the tax audit is performed usually at the headquarters (Kubick et al. 2017). In Equation (2), we again include the vector of regional-level controls defined above (*GDP per Capita*, *Unemployment*, *House Price Index*, *State Tax*, *Combined*, *Throwback*, *Property Factor*, and *Sales Factor*), which we assign to firms based on the headquarters location. Additionally, we control for firm-level characteristics likely to be associated with business activity: *Cash Holdings*, *Sales Growth*, *Leverage*, *Profitability*, and *Tobin's q*. We include firm fixed effects (α_i), IRS firm size group fixed effects (α_s), and industry-year fixed effects ($\alpha_{ind,t}$). We cluster standard errors at the IRS district level.[16]

Data Sources and Sample Selection

We obtain data on corporate tax return audit rates from Syracuse University's Transactional Records Access Clearinghouse, a nonpartisan research institute that collects data from the IRS. The data have been cited widely in the media and have been used in prior research (Guedhami and Pittman 2008; El Ghoul et al. 2011; Hoopes et al. 2012; Hanlon et al. 2014; Gallemore and Jacob 2020; Bauer et al. 2021). Our CZ-level business activity measures are developed using data from the U.S. Census's County Business Patterns (CBP) survey, which are created from a variety of U.S. Census programs, including the Business Register, Economic Census, Annual Survey of Manufactures, and Current Business Surveys. These data provide the number of establishments and employment by county, establishment size (measured by the number of employees), and sector (using SIC classifications until 1997 and NAICS afterward). Data for the firm-level analyses are obtained from Compustat. Unemployment data are from the Bureau of Labor Statistics and gross state product data from the U.S. Department of Commerce (1992–1996) and Bureau of Economic Analysis (1997–2015). State-level tax information is obtained from Dyreng, Jacob, Jiang, and Müller (2022).

Table 1 summarizes the sample selection process. Our IRS audit rate data cover the period from 1992 to 2015. Because we require lags to calculate our variables, the regression sample starts in 1993. Our initial CZ sample begins with the CZs that we can merge with the CBP data during this period. We exclude all CZ-year observations with missing data for any dependent or independent variable. We also exclude CZs with little business activity (i.e., CZs with less than 20 establishments or 1,000 employees) to mitigate the possibility that large changes in small baseline values of CZ-level business activity drive our findings. This results in our final CZ sample of 607 CZs and 12,867 observations. For our firm-level sample, we start with all Compustat firm-years over the period from 1992 to 2015 and remove those without data for our regression variables or with negative total assets, sales, equity, or cash holdings. This results in the final firm-level sample of 8,786 (71,263) firms (firm-years). Table 2 presents descriptive statistics (Panels A and B) and correlations (Panels C and D) for our regression variables. The average CZ has 574 establishments with at least 50 employees and approximately 86,300 employees in these establishments. Establishments (employment) grow, on average, by 0.78 (0.82) percent, but growth rates vary significantly; the 25th percentile is −2.75 (−3.03) percent, whereas the 75th percentile is 4.65 (5.16) percent. Regarding the firm-level sample, the average employee growth rate is 4.2 percent. The average audit rate is 24.8 percent, which is larger than that in our CZ-level sample because the public firms are, on average, larger than those in the CZ-level analyses. Figure 2 plots the average values of the business activity variables by year.

[15] We use cluster analysis to partition CZs into 100 clusters based on the average establishment shares in the different size groups (Stata command *cluster kmeans*). We choose 100 clusters to avoid having too few clusters (Petersen 2009); in untabulated analyses, we find that our inferences are robust to using fewer than 100 clusters (e.g., 75). Our results are also robust to clustering by CZ or to applying the Adao et al. (2019) methodology (untabulated).

[16] Our inferences are statistically stronger when clustering by firm, size group-year, or industry-year (untabulated).

TABLE 1

Sample Selection

Panel A: CZ-Level Sample

Step	Number of CZs	Number of Observations
Initial sample of 707 CZs and 23 years	707	16,261
–Missing county-level data (e.g., missing population data, missing data on establishments or employment)	—	−730
–CZs with less than 20 establishments or 1,000 employees	−100	−2,655
Final Sample	**607**	**12,876**

Panel B: Firm-Level Sample

Step	Number of Firms	Number of Observations
Initial sample of firms headquartered in the United States with nonmissing assets and sales over the period 1992–2015	17,963	170,442
–Firms with negative total assets, sales, equity, or cash holdings	−1,246	−21,635
–Missing firm-level data (e.g., missing employment or missing address)	−7,931	−77,544
Final Sample	**8,786**	**71,263**

This table presents the sample selection steps to arrive at our final samples.

Growth rates are positive in most years, consistent with overall growth in business activity over time. The two periods of negative growth rates, 2001–2002 and 2008–2009, coincide with the two recessions during our sample period.

IV. MAIN RESULTS

CZ-Level Analysis

Table 3 presents the results of estimating Equation (1), with either *Establishment Growth* (columns (1) to (3)) or *Employment Growth* (columns (4) to (6)) as the dependent variable. Columns (1) and (4) include only year fixed effects, and columns (2), (3), (5), and (6) also include CZ fixed effects and the time-varying CZ-level control variables. In columns (1), (2), (4), and (5), we estimate Equation (1) using OLS, which gives each CZ the same weight. In columns (3) and (6), we follow Autor et al. (2013) and estimate a weighted least squares (WLS) model using the CZ's lagged total population as the weight. The motivation for the WLS approach is that policymakers likely care about the aggregate effects of tax enforcement on business growth, which are primarily driven by larger CZs.

Across all six specifications, we find negative and significant coefficients on *Audit Rate*, consistent with tax enforcement leading to less employment and establishment growth. We note that these findings contrast with the positive univariate correlations between *Audit Rate* and our two business activity measures. To reconcile this difference, we observe that there is a broad-based decline in both tax enforcement and business activity during our sample period (see Figures 1 and 2), suggesting that accounting for macroeconomic-level factors is likely important for understanding the effect of tax enforcement on business activity. To confirm this intuition, we first estimate Equation (1) without year fixed effects and find that *Audit Rate* is positive, consistent with the univariate correlation. We then expand this test by including several controls for macroeconomic conditions (national GDP growth, unemployment, inflation, household consumption, stock trading activity, and net foreign trade) and find that the coefficient on *Audit Rate* becomes negative and statistically significant. These findings suggest that accounting for macroeconomic factors (in our case, through year fixed effects) is important for understanding how tax enforcement affects business activity.

Regarding economic magnitudes, the estimates in columns (2) and (3) suggest that an increase in the audit rate by 5 percentage points (which is approximately the difference between the median and the 75th percentile or 87 percent of the standard deviation measured within CZ fixed effects) reduces establishment growth by 0.38 (0.43) percent in the OLS (WLS) specification. In aggregate terms, this represents about 590 establishments, suggesting that the aggregate effect on the number of establishments (i.e., the extensive margin) is small compared with the likely benefits of

TABLE 2

Summary Statistics

Panel A: CZ-Level Sample

Variable	Mean	Std. Dev.	P25	Median	P75
Establishments	574	1,251	65	155	458
Establishment Growth (in %)	0.785	10.730	−2.754	1.245	4.652
Employment	86,313	199,820	8,088	20,869	66,063
Employment Growth (in %)	0.824	14.492	−3.034	1.259	5.160
Audit Rate	0.102	0.059	0.064	0.076	0.132
Unemployment	0.062	0.025	0.044	0.057	0.074
GDP per Capita	10.543	0.287	10.337	10.531	10.753
Tax Rate (in %)	6.356	2.776	5.429	6.500	8.000
Throwback	0.489	0.484	0.000	0.367	1.000
Combined	0.322	0.454	0.000	0.000	1.000
Sales Factor	58.199	27.726	33.340	50.000	90.000
Payroll Factor	18.280	12.933	0.000	25.000	25.000
Property Factor	18.280	12.933	0.000	25.000	25.000
Income	9.355	0.479	9.013	9.348	9.691
Asian	0.169	1.637	0.011	0.044	0.146
Black	0.042	0.317	0.004	0.008	0.019
Hispanic	0.063	0.269	0.004	0.021	0.057
Over 65	0.160	0.074	0.127	0.147	0.171
Under 4	0.058	0.061	0.044	0.051	0.056

Panel B: Firm-Level Sample

Variable	Mean	Std. Dev.	P25	Median	P75
Employee Growth	0.042	0.262	−0.051	0.026	0.129
Audit Rate	0.248	0.151	0.134	0.245	0.323
Cash Holdings	0.179	0.206	0.026	0.095	0.262
Sales Growth	0.188	0.507	−0.028	0.151	0.376
Leverage	0.205	0.191	0.020	0.172	0.335
Pre-tax income	0.014	0.227	−0.034	0.050	0.121
Size	5.563	2.265	3.915	5.490	7.092
MtB	1.433	1.724	0.489	0.915	1.687
Capex	0.127	0.114	0.054	0.094	0.162
R&D	0.102	0.156	0.005	0.042	0.133
Wage Growth	0.107	0.235	0.001	0.074	0.180

(continued on next page)

enforcement. By contrast, the employment growth results suggest that tax enforcement affects the intensive margin of business activity in an economically meaningful way. Specifically, the findings in column (5) suggest that a 5 percentage point audit rate is associated with a reduction in employment growth of 0.88 percent. When aggregating across all CZs, the OLS employment growth findings suggest that a 5 percentage point increase in the tax return audit rate is associated with a reduction in jobs between 459,000 (using the estimate from column (5)) and 486,000 (using the estimate from column (4)).[17]

Firm-Level Analysis

We report the findings from estimating Equation (2) using the firm-level sample in Table 4. Column (1) includes only size group and industry-year fixed effects, and column (2) also includes firm fixed effects. Column (3) presents the

[17] To arrive at this estimate, we multiply the coefficient estimate of −1.0362 by 0.05 (i.e., a 5 percentage point audit rate change) divided by the standard deviation of 0.059 (from Table 2) because *Audit Rate* is standardized. We then multiply this product times 607 CZs and the average number of jobs per CZ (86,312). We then divide by 100 as growth rates are expressed in percent. Hence, −1.0362 × (0.05 / 0.059) × 86,312 × 607 CZs / 100 = 0.459 million jobs.

TABLE 2 (continued)

Panel C: Correlations for the CZ-Level Sample

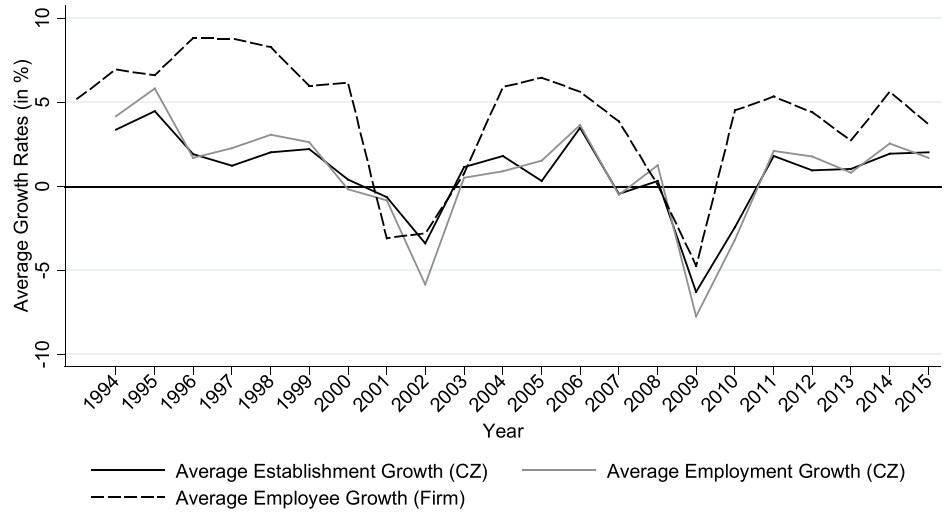
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	*Establishment Growth*	1														
2	*Employment Growth*	0.85	1													
3	*Audit Rate*	0.07	0.07	1												
4	*Unemployment*	−0.01	−0.01	−0.03	1											
5	*GDP per Capita*	−0.04	−0.03	−0.65	0.12	1										
6	*Tax Rate* (in %)	0.00	0.00	0.00	−0.07	−0.07	1									
7	*Income*	0.02	0.01	−0.36	0.00	0.46	−0.17	1								
8	*Black*	−0.01	−0.01	−0.02	0.04	0.05	−0.01	0.03	1							
9	*Asian*	−0.02	−0.02	−0.06	0.08	0.12	0.00	0.10	0.57	1						
10	*Hispanic*	−0.03	−0.03	−0.02	0.03	0.03	−0.04	0.08	0.73	0.43	1					
11	*Under 4*	0.00	0.00	−0.01	0.08	0.08	−0.01	0.06	0.00	0.01	−0.02	1				
12	*Over 65*	0.02	0.02	0.05	0.07	0.00	0.07	−0.06	0.03	0.02	−0.04	−0.18	1			
13	*Combined*	0.01	0.01	−0.16	−0.01	0.25	0.14	0.30	0.00	0.06	0.04	0.02	0.02	1		
14	*Throwback*	0.01	0.02	0.06	0.01	−0.20	0.08	0.08	−0.01	0.00	0.08	0.01	−0.03	0.26	1	
15	*Property Factor*	0.01	0.00	0.27	−0.04	−0.39	0.21	−0.23	0.00	−0.05	−0.05	−0.04	−0.04	−0.12	0.30	1
16	*Sales Factor*	−0.01	−0.01	−0.23	0.10	0.27	0.21	0.07	0.01	0.02	0.05	0.04	0.07	0.22	−0.11	−0.67

Panel D: Correlations for the Firm-Level Sample

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	*Employee Growth*	1														
2	*Audit Rate*	0.03	1													
3	*Cash Holdings*	0.07	−0.25	1												
4	*Sales Growth*	0.16	0.06	0.03	1											
5	*Leverage*	−0.05	0.21	−0.46	0.02	1										
6	*Pre-tax income*	0.12	0.23	−0.21	0.08	−0.02	1									
7	*MtB*	0.19	−0.13	0.40	0.19	−0.32	−0.07	1								
8	*Size*	−0.01	0.55	−0.25	0.01	0.25	0.27	−0.17	1							
9	*Tax Rate* (in %)	0.00	−0.02	0.12	0.00	−0.10	−0.02	0.07	−0.08	1						
10	*GDP per Capita*	0.05	0.12	−0.02	0.12	0.01	0.03	0.07	−0.11	0.08	1					
11	*HPI*	−0.02	−0.18	0.31	−0.02	−0.18	−0.10	0.12	0.05	0.19	−0.15	1				
12	*Unemployment*	0.01	−0.02	0.00	−0.12	−0.01	0.01	−0.06	0.12	−0.06	−0.38	0.08	1			
13	*Combined*	0.00	−0.12	0.15	−0.01	−0.11	−0.04	0.08	0.00	0.15	−0.01	0.34	0.13	1		
14	*Throwback*	0.02	−0.04	0.10	0.04	−0.07	−0.04	0.06	−0.04	0.10	0.10	0.25	0.03	0.39	1	
15	*Property Factor*	0.01	0.08	0.02	0.03	−0.02	−0.02	0.04	−0.16	0.41	0.12	−0.02	−0.18	−0.14	0.01	1
16	*Sales Factor*	−0.01	−0.06	−0.05	−0.04	0.01	0.04	−0.05	0.16	−0.11	−0.07	−0.02	0.16	0.22	0.10	−0.77

This table presents descriptive statistics (Panels A and B) and correlations (Panels C and D) for the variables used in our primary analyses.
All variables are defined in Appendix B.

FIGURE 2
Trends in Establishment and Employment Growth Over Time



This figure presents the annual averages of our main dependent variables in the CZ-level and firm-level analyses.

TABLE 3
Tax Enforcement and Business Activity, CZ-Level Analysis

Dependent Variable:	***Establishment Growth*$_{t+1}$**			***Employment Growth*$_{t+1}$**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Audit Rate	**–0.4156*****	**–0.4475****	**–0.5130****	**–1.0984*****	**–1.0362*****	**–0.9011*****
	(0.1479)	**(0.1933)**	**(0.2554)**	**(0.1997)**	**(0.2699)**	**(0.3156)**
Unemployment		11.2294	3.7243		12.9517	7.9059
		(9.9415)	(12.1547)		(15.2409)	(16.7853)
GDP per Capita		4.3665	3.6961		6.0453	7.3208
		(3.2622)	(3.4580)		(4.5795)	(4.6719)
Tax Rate		0.0592	−0.0237		0.1543*	0.0060
		(0.0645)	(0.0774)		(0.0833)	(0.1013)
Income		16.9614***	23.4468***		13.9177**	21.1837***
		(3.9182)	(4.7041)		(5.4855)	(5.3164)
Black		0.2865*	0.2745		0.4821*	0.4415*
		(0.1526)	(0.1811)		(0.2522)	(0.2484)
Asian		−0.9395**	−0.8810		−1.3428**	−1.3020
		(0.4519)	(0.5752)		(0.6340)	(0.7849)
Hispanic		−2.4257**	−2.0615		−3.6746*	−2.9647*
		(1.1520)	(1.2545)		(1.9348)	(1.7392)
Under 4		−0.1615	7.6433		−0.2136	9.4255
		(4.0439)	(7.4323)		(6.0278)	(10.9521)
Over 65		3.7481	−0.9721		3.7017	1.9106
		(5.2995)	(6.2516)		(8.2983)	(7.7478)
Combined		0.7888	1.9677***		0.2339	2.7371***
		(0.6332)	(0.7339)		(0.8038)	(0.9855)

(continued on next page)

TABLE 3 (continued)

Dependent Variable:	***Establishment Growth*$_{t+1}$**			***Employment Growth*$_{t+1}$**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Throwback		1.1023	1.7559**		1.5010*	2.7992**
		(0.6780)	(0.8644)		(0.8716)	(1.1231)
Property Factor		0.3795	0.7316**		0.1007	0.4106
		(1.3173)	(0.3596)		(1.9640)	(0.3488)
Sales Factor		0.2041	0.3848**		0.0625	0.2237
		(0.6581)	(0.1818)		(0.9809)	(0.1761)
CZ FE	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Model	OLS	OLS	WLS	OLS	OLS	WLS
Observations	12,876	12,876	12,876	12,876	12,876	12,876
R^2	0.046	0.089	0.166	0.045	0.075	0.146

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents the results of estimating Equation (1) using the CZ-level sample. In columns (1) and (2) (columns (3) and (4)), the dependent variable is *Establishment Growth* (*Employment Growth*). The main independent variable is *Audit Rate*, which is the weighted average tax return audit rate in the CZ. All columns include control variables, CZ fixed effects, and year fixed effects. In columns (1) and (3), we use OLS. In columns (2) and (4), we use WLS, with the CZ population in year *t* as weights. We report standard errors clustered at the level of 100 clusters of similar CZs in parentheses.
All variables are defined in Appendix B.

baseline model with all controls and fixed effects. In all three specifications, we find that tax enforcement is negatively associated with employment growth. In untabulated analyses, we find that the difference between the negative relation between tax enforcement and firm-level business activity in the multivariate analysis and the positive univariate correlation in Table 2 is driven by the inclusion of firm fixed effects, highlighting the importance of accounting for time-invariant firm-level factors. Regarding economic significance, the coefficient estimate from column (3) implies that a 7.8 percentage point increase in the audit rate (which is the difference between the median and the 75th percentile of the *Audit Rate* in this sample) is associated with a reduction in employment growth of 1.3 percent, which equals 348,500 fewer jobs when aggregated across all sample firms.[18] The difference between this estimate and the estimate from the CZ-level analyses can likely be attributed to the firm-level tests capturing only public firm business activity, whereas the CZ-level analysis captures business activity from both public and private firms.

In columns (4) to (6), we find similar negative associations between tax enforcement and three alternative firm-level business activity measures: capital expenditures (*Capex*), R&D expense (*R&D*), and wage growth (*Wage Growth*; we define these measures in Appendix B). The findings in column (4) (5) suggest that a 5 percentage point increase in the audit rate is associated with a 0.11 (0.13) percent decrease in *Capex* (*R&D*) or $3.9 million ($3.6 million) for the average firm and $0.26 million ($0.24 million) for the median firm. The findings in column (6) imply that a 5 percentage point increase in the audit rate is associated with a decline in *Wage Growth* of approximately 0.65 percent, representing $5.24 million ($0.81) for the average (median) firm. Overall, the Table 4 findings suggest that tax enforcement reduces firm-level business activity, corroborating the results from our CZ-level analyses.

V. HETEROGENEITY ANALYSES

Our main results suggest that, on average, tax enforcement is associated with less business activity. In our next set of analyses, we explore whether this association varies across regions and firms. The objective of these tests is twofold. First, they allow us to explore the mechanisms through which tax enforcement impacts business activity. Second, they speak to the potential winners and losers of tax enforcement, which should be of interest to both academics and policymakers.

[18] We obtain the 1.3 percent from multiplying the column (3) coefficient estimate by 7.8 percentage points divided by the standard deviation (because *Audit Rate* is standardized to have a mean of 0 and a standard deviation of 1). To arrive at the aggregate estimate, we multiply the average employees per firm (8,516), the average firms per year in our sample (3,098), and the 1.3 percent employment change attributable to a 7.8 percentage point audit rate change. If we use the same audit rate increase as in the CZ-level calculations (5 percentage points), our results suggest that this increase is associated with a reduction in employment growth of 0.85 percent (or 223,500 jobs).

TABLE 4

Tax Enforcement and Business Activity, Firm-Level Analysis

	Baseline			Alternative Dependent Variables		
Dependent Variable:	*Employee Growth*$_{t+1}$			*Capex*$_{t+1}$	*R&D*$_{t+1}$	*Wages Growth*$_{t+1}$
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Audit Rate$_t$	**–0.0481*****	**–0.0438*****	**–0.0257*****	**–0.0035*****	**–0.0044*****	**–0.0194****
	(0.0044)	**(0.0044)**	**(0.0027)**	**(0.0006)**	**(0.0009)**	**(0.0079)**
Cash Holdings$_t$			0.2267***	0.0821***	−0.0165***	−0.0032
			(0.0135)	(0.0073)	(0.0043)	(0.0666)
Sales Growth$_t$			0.0041	0.0235***	−0.0002	0.0137
			(0.0036)	(0.0015)	(0.0012)	(0.0164)
Leverage$_t$			−0.0459***	−0.0425***	−0.0050	0.0227
			(0.0145)	(0.0062)	(0.0070)	(0.0453)
Pre-tax income$_t$			0.0961***	0.0361***	−0.0434***	0.1631**
			(0.0109)	(0.0046)	(0.0030)	(0.0598)
Size$_t$			−0.1171***	−0.0010	−0.0488***	−0.0380***
			(0.0048)	(0.0017)	(0.0042)	(0.0105)
MtB$_t$			0.0181***	0.0119***	0.0090***	0.0331***
			(0.0014)	(0.0008)	(0.0007)	(0.0053)
Macroeconomic controls	No	No	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	71,264	71,264	71,264	71,264	40,236	6,086
R^2	0.083	0.286	0.348	0.532	0.808	0.515

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.

This table presents the results of estimating Equation (2) using the firm-level sample. In columns (1) to (3), the dependent variable is *Employee Growth*. In columns (4) to (6), the dependent variable is *Capex* (capital expenditures scaled by lagged total assets), *R&D* (research and development expenditures scaled by gross property, plant, and equipment), or *Wages Growth* (logarithmic growth rate in staff expenditures from *t* to *t*+1). The main independent variable is *Audit Rate*. All columns include control variables, firm fixed effects, IRS-size group effects, and industry-year fixed effects. Standard errors are clustered at the IRS district level.

Access to External Financing and Financial Constraints

First, we explore the impact of access to external financing. Prior research finds that tax enforcement can improve information environments (Hanlon et al. 2014; Bauer et al. 2021) and increase the provision of capital to businesses by reducing informational asymmetries between the firm and potential lenders (Gallemore and Jacob 2020). Prior work also finds that greater access to external capital reduces the importance of internally generated funds for business activity (Almeida and Campello 2007; Fazzari et al. 1988). Thus, we expect that regions and firms with greater access to external capital sources are likely to benefit in part from greater tax enforcement, which should in turn mitigate the on-average negative association between tax enforcement and business activity.

To test this idea at the CZ level, we exploit regional variation in the presence of commercial banks, as bank loans are a primary source of funding for private firms and SMEs (Strahan and Weston 1998; Berger, Klapper, and Udell 2001; Cetorelli and Strahan 2006). Bank lending to these firms is often based on geographic proximity to the bank. Thus, all else equal, greater CZ-level bank density should be associated with having greater access to bank lending, reducing the importance of internally generated cash flows for funding growth. Thus, we predict that the negative association between tax enforcement and business activity will be weaker in CZs with greater bank density.

Using data from the Federal Reserve Bank of Chicago on the lending portfolios and branch locations for U.S. commercial banks, we first identify banks that lend to businesses (i.e., those with non-zero commercial loans). We then measure the density of bank branches in the region using the ratio of the number of branches of these banks in the CZ to the number of establishments. Finally, we create *High Banking Density*, an indicator variable equal to 1 if this ratio is above

TABLE 5

Access to External Financing and Financial Constraints

Sample:	**CZ Level**		**Firm Level**	
Dependent Variable:	***Establishment Growth*$_{t+1}$**	***Employment Growth*$_{t+1}$**	***Employee Growth*$_{t+1}$**	
Split by:	***High Banking Density***		***Low WW Index***	***Low KZ Index***
	(1)	**(2)**	**(3)**	**(4)**
Audit Rate	−0.5138***	−1.2529***	−0.0340***	−0.0309***
	(0.1900)	(0.2668)	(0.0036)	(0.0038)
Audit Rate* × *High Banking Density	**0.1035**	**0.4115****		
	(0.1690)	**(0.2054)**		
Audit Rate* × *Low Constraint			**0.0097*****	**0.0068****
			(0.0033)	**(0.0029)**
Sort Variable	0.7632**	0.6737	−0.0127***	0.0028
	(0.3595)	(0.4881)	(0.0039)	(0.0036)
Joint effect of *Audit Rate* + Interaction	−0.4103*	−0.8414***	−0.0243***	−0.0241***
[t-statistic]	[1.77]	[2.66]	[8.66]	[7.98]
Controls	Yes		Yes	
Fixed effects	Commuter Zone and Year		Firm and Industry-Year	
Observations	12,876	12,876	70,474	66,315
R^2	0.090	0.075	0.349	0.350

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.

This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample (columns (1) and (2)) or Equation 2 using the firm-level sample (columns (3)–(5)). In column (1) (column (2)), the dependent variable is CZ-level *Establishment Growth* (*Employment Growth*). In columns (3)–(5), the dependent variable is firm-level *Employee Growth*. The main independent variable is *Audit Rate*. In columns (1) and (2), we additionally include *High Banking Density* and its interaction with *Audit Rate*. In columns (3) and (4), we include *Low Constraint*, along with its interaction with *Audit Rate*, where *Low Constraint* is either *Low WW Index* or *Low KZ Index*. All columns include control variables and fixed effects from the respective main specification. In each column, we additionally include a sorting variable (*Sort Variable*) along with its interaction with *Audit Rate*. We report standard errors clustered at the level of 100 clusters of similar CZs (IRS district level) in parentheses in columns (1) and (2) ((3)–(5)).

All variables are defined in Appendix B.

the sample annual median, and re-estimate Equation (1) including *High Banking Density* and its interaction with *Audit Rate*. We report these findings in columns (1) and (2) of Table 5. We find a positive coefficient on the interaction term in both models, and the coefficient is statistically significant when examining *Employment Growth*. These results suggest that more access to external capital attenuates the negative effect of tax enforcement on business activity, consistent with firms in these regions benefitting in part from greater tax enforcement facilitating access to bank lending.

To examine this idea at the firm-level, we examine whether the negative association between tax enforcement and business activity is attenuated when firms are less financially constrained (e.g., have less costly access to external capital). Prior research finds that business activity is less sensitive to internally generated cash flows if firms are less financially constrained (Almeida and Campello 2007; Fazzari et al. 1988). Additionally, prior research finds that greater tax enforcement is associated with higher cash tax payments (Hoopes et al. 2012), indicating that tax enforcement reduces internally generated funds.[19] Thus, we predict that the negative impact of tax enforcement on business activity is weaker for less financially constrained firms, as these firms can absorb the negative cash flow effects from tax enforcement more easily. We use two measures of financial constraints, the Kaplan and Zingales (1997) ("KZ") index and the Whited and Wu (2006) ("WW") index, each measured in year $t-1$.[20] We define firms below the top quartile within an industry as being less financially constrained using an indicator variable, which we refer to as *Low KZ Index* or *Low WW Index*, respectively. We estimate Equation (2) including the respective indicator and its interaction with *Audit Rate* and report these findings in columns (3) and (4) of Table 5. In both models, we find a positive coefficient on the interaction of *Audit*

[19] This argument is supported by research that finds that financially constrained firms engage in more tax avoidance, ostensibly to alleviate the lack of access to external funds (Law and Mills 2015; Edwards, Schwab, and Shevlin 2016).

[20] In untabulated tests, we find similar results when using cash holdings as a measure of financial constraints.

Rate with the financial constraint measure, consistent with financial constraints weakening the negative effect of tax enforcement on business activity. These findings collectively suggest that the negative impact of tax enforcement on business activity is attenuated for firms that are more likely to benefit from the positive externalities associated with tax enforcement (e.g., better information environment quality facilitating access to external capital).

Compliance Costs

Next, we examine whether the increased compliance costs associated with tax audits—either during an audit or preparing for a potential audit—are partly responsible for the negative association between tax enforcement and business activity. Gallemore and Labro (2015) examine the relation between internal information quality, which they argue reduces tax-related compliance costs, and tax avoidance. They find that firms with better internal information quality exhibit greater tax avoidance and lower levels of tax risk, consistent with the notion that firms that can better manage tax-related compliance costs are less concerned about the consequences of tax enforcement. In a similar vein, we expect that firms that can more efficiently manage the compliance costs associated with tax audits are less likely to reduce business activity in response to tax enforcement.

To examine this idea at the CZ level, we test whether this negative association is stronger for regions that likely lack access to a key source—accounting professionals—that can help firms mitigate these compliance costs. We expect that when a region has a lower supply of accounting professionals, the accountants in these regions can charge higher prices to clients. When tax enforcement is greater, firms may have to pay these higher prices or instead spend more time and resources internally on complying with the tax code and record-keeping requirements. Both forces would lead firms to incur more compliance-related costs when tax enforcement is greater, and thus these firms might curtail their business activity in response (Djankov et al. 2010).

To identify regions in which access to accounting professionals is relatively limited, we define *Low Accountants* as an indicator variable equal to 1 if the ratio of accounting professionals located in that CZ to the CZ's total employment is in the bottom quartile in that sample year.[21] We re-estimate Equation (1) including this indicator variable and its interaction with *Audit Rate* and present these findings in Table 5. We find that the negative association of tax enforcement on business activity is stronger in the CZs with lower accountant density, consistent with firms in these regions facing greater compliance costs related to tax enforcement. We continue to document a negative coefficient on *Audit Rate*, suggesting that business activity in other regions is still negatively affected by tax enforcement.

To test this idea at the firm level, we exploit differences in internal information environments across firms. We expect that firms with a lower-quality internal information environment have fewer internal resources (e.g., internal accountants and/or information systems) to help with compliance, and thus these firms are more affected by the compliance costs associated with tax enforcement. To proxy for the quality of the internal information environment, we follow Gallemore and Labro (2015) and define an indicator variable, *ICW*, which equals 1 if the firm reports an internal control weakness in the prior year and 0 otherwise. We re-estimate Equation (2) including *ICW* along with its interaction with *Audit Rate*. We report these findings in column (3) of Table 6. We find that the coefficient on the interaction term is negative, again consistent with firms adjusting their business activity more strongly to tax enforcement when their expected compliance costs are higher.[22] We also document that the coefficient on *Audit Rate* is negative, suggesting that the business activity of firms without any internal control weaknesses is also sensitive to tax enforcement.

Strength of Tax Enforcement and Extent of Tax Avoidance

In our final heterogeneity analyses, we explore whether the effect of tax enforcement on business activity varies with the strength of tax enforcement and extent of tax avoidance. First, we examine whether the impact of tax enforcement on business activity varies with the proximity to an IRS territory manager office. Kubick et al. (2017) find that the IRS is more likely to audit companies that are in closer proximity to these offices. Therefore, the tax and compliance costs of tax enforcement are likely to be more salient for these firms, and thus we predict that the negative effect of tax enforcement on business activity will be stronger for regions and firms that are closer to IRS offices.

[21] The number of accountants is based on the SIC codes 7291 and 8721 and NAICS codes starting with 54121. This categorization includes both CPAs and tax accountants. We do not focus solely on tax accountants because this variable is missing for most county-years; the CBP does this to ensure anonymity. Furthermore, these data will capture local accounting professionals regardless of affiliation (e.g., Big 4, smaller accounting firm, or independent).

[22] Given that Gallemore and Labro (2015) find that internal information quality is positively (negatively) associated with tax avoidance (tax avoidance risk), one concern is that lower-quality tax planning, rather than expected compliance costs, could be the mechanism behind these findings. In untabulated tests, we find that these results are robust to excluding observations with tax-related internal control weaknesses and/or to controlling for tax planning outcomes, mitigating this concern.

TABLE 6

Compliance Costs

Sample:	**CZ Level**		**Firm Level**
Dependent Variable:	***Establishment Growth*$_{t+1}$**	***Employment Growth*$_{t+1}$**	***Employee Growth*$_{t+1}$**
Split by:	***Low Accountants***		***ICW***
	(1)	**(2)**	**(3)**
Audit Rate	−0.4144**	−0.9761***	−0.0217**
	(0.1934)	(0.2713)	(0.0082)
Audit Rate* × *High Compliance Costs	**−1.1311***	**−2.4528*****	**−0.0201***
	(0.5778)	**(0.8203)**	**(0.0111)**
High Compliance Costs	−0.9017**	−1.1193	−0.0018
	(0.4542)	(0.6809)	(0.0082)
Joint effect of *Audit Rate* + Interaction	−1.5455***	−3.4289***	−0.0418***
[t-statistic]	[2.70]	[4.37]	[2.98]
Controls	Yes		Yes
Fixed effects	Commuter Zone and Year		Firm and Industry-Year
Observations	12,876	12,876	22,429
R^2	0.090	0.075	0.410

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample (columns (1) and (2)) or Equation 2 using the firm-level sample (column (3)). In column (1) (column (2)), the dependent variable is CZ-level *Establishment Growth* (*Employment Growth*). In column (3), the dependent variable is firm-level *Employee Growth*. The main independent variable is *Audit Rate*. In each column, we additionally include *High Compliance Costs* along with its interaction with *Audit Rate*, where *High Compliance Costs* is *Low Accountants* in columns (1) and (2) and *ICW* in column (3). All columns include control variables and fixed effects from the respective main specification. We report standard errors clustered at the level of 100 clusters of similar CZs (IRS district level) in parentheses in columns (1) and (2) (column (3)).
All variables are defined in Appendix B.

To examine this idea at the CZ level (firm level), we define *Low IRS Distance* as an indicator variable equal to 1 if the CZ-level average distance (firm headquarters' distance) to the nearest IRS territory manager office is in the bottom quartile of their respective sample. We then re-estimate both Equations (1) and (2) additionally including *Low IRS Distance* and its interaction with *Audit Rate*. We report these findings in Table 7, with columns (1) and (2) (column (3)) using the CZ-level (firm-level) design. The coefficient on the interaction term is negative in each model and statistically significant in two of the three regressions, consistent with the negative effect of tax enforcement on business activity being stronger for CZs and firms that are closer to IRS offices. The coefficient on *Audit Rate* remains negative and statistically significant in each model, suggesting that tax enforcement still reduces business activity in CZs and in firms that are further away from these offices.

We corroborate these findings by exploring whether the negative effect of tax enforcement on business activity varies with the extent of firm-level tax avoidance. We expect that tax enforcement is more likely to lead to both tax costs (e.g., back taxes owed and penalties) and compliance costs (e.g., costs related to preparing for and complying with an actual audit) when the firm is engaging in greater tax avoidance (Hoopes et al. 2012; Belnap et al. 2022). Furthermore, firms with greater tax avoidance may be more reliant on tax avoidance to fund growth. Both forces would suggest that the negative impact of tax enforcement on business activity will be increasing in tax avoidance.

To test this idea, we create an indicator variable, *Low ETR*, which is equal to 1 if the firm's three-year cash ETR is below the bottom quartile of its respective industry. We sort within industries to account for differences in tax planning opportunities across sectors (Balakrishnan, Blouin, and Guay 2019). For this test, we exclude firms with a negative ETR denominator (i.e., negative three-year pre-tax income). We then re-estimate Equation (2) including *Low ETR* along with its interaction with *Audit Rate* and present these findings in column (4) of Table 7. We find that the negative effect of tax enforcement on employee growth is stronger for firms with low ETRs, consistent with tax-aggressive firms being more

TABLE 7

Strength of Tax Enforcement and Extent of Tax Avoidance

Sample:	**CZ Level**		**Firm Level**	
Dependent Variable:	***Establishment Growth*$_{t+1}$**	***Employment Growth*$_{t+1}$**	***Employee Growth*$_{t+1}$**	
Split by:	***Low IRS Office Distance***		***Low IRS Office Distance***	***Low 3-Year Cash ETR***
	(1)	**(2)**	**(3)**	**(4)**
Audit Rate	−0.3962**	−0.9476***	−0.0216***	−0.0159***
	(0.1904)	(0.2603)	(0.0025)	(0.0025)
Audit Rate* × *Sort Variable	**–0.2202**	**–0.3802***	**–0.0062***	**–0.0069****
	(0.1498)	**(0.2025)**	**(0.0037)**	**(0.0028)**
Sort Variable	—	—	0.0025	0.0091***
			(0.0082)	(0.0032)
Joint effect of *Audit Rate* + Interaction	−0.6164**	−1.3278***	−0.0278***	−0.0228***
[t-statistic]	[2.57]	[3.79]	[6.92]	[8.60]
Controls	Yes		Yes	
Fixed effects	Commuter Zone and Year		Firm and Industry-Year	
Observations	12,876	12,876	61,734	46,883
R^2	0.089	0.075	0.359	0.379

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample (columns (1) and (2)) or Equation (2) using the firm-level sample (columns (3) and (4)). In column (1) (column (2)), the dependent variable is CZ-level *Establishment Growth* (*Employment Growth*). In columns (3) and (4), the dependent variable is firm-level *Employee Growth*. The main independent variable is *Audit Rate*. In each column, we additionally include a sorting variable (*Sort Variable*) along with its interaction with *Audit Rate*. In columns (1)–(3), the sorting variable is *Low IRS Office Distance*. In column (4), the sorting variable is *Low 3-Year Cash ETR*. All columns include control variables and fixed effects from the respective main specification. We report standard errors clustered at the level of 100 clusters of similar CZs (IRS district level) in parentheses in columns (1) and (2) (columns (3) and (4)).
All variables are defined in Appendix B.

exposed to the tax and compliance costs of tax enforcement and thus reducing business activity more strongly in response to tax enforcement.[23]

VI. SUPPLEMENTAL AND ROBUSTNESS TESTS

In this section, we present a supplemental analysis examining business formation and job creation. Moreover, we discuss several analyses that mitigate endogeneity concerns, address measurement issues, and assess the sensitivity of the baseline findings to alternative design choices.

Tax Enforcement, Business Formation, and Job Creation

If tax enforcement is costly, this could reduce the incentives to start new businesses and hire and could cause existing businesses to close (Belnap et al. 2022). To test this idea, we use data from the U.S. Census to construct six variables of CZ-level business formation and job creation: *Establishment Entry Rate* (number of new establishments), *Establishment Exit Rate* (number of exiting establishments), *Net Entry Rate* (new minus exiting establishments), *Job Creation Rate* (number of new jobs), *Job Destruction Rate* (number of eliminated jobs), and *Net Job Creation Rate* (new minus eliminated jobs). All variables are measured as the change from t to t+1. Establishment (employment) variables are scaled by total CZ establishments (employees).

[23] The cash ETR captures a continuum of tax planning activities including benign, low-risk tax strategies. In untabulated tests, we find similar results when using three measures of risky tax avoidance: discretionary permanent book-tax differences following Frank, Lynch, and Rego (2009) and Rego and Wilson (2012), uncertain tax benefits scaled by total assets, or GAAP ETR volatility. When examining cash ETR volatility or a measure of tax sheltering probability, we continue to find negative interaction coefficients, but they are not statistically significant at conventional levels.

TABLE 8

The Role of Tax Enforcement in Business Formation and Job Creation

Dependent Variable:	*Establishment Entry Rate* (1)	*Establishment Exit Rate* (2)	*Net Entry Rate* (3)	*Job Creation Rate* (4)	*Job Destruction Rate* (5)	*Net Job Creation Rate* (6)
Audit Rate	−0.0509**	0.0408	−0.0917*	−0.2461***	−0.0961	−0.1501**
	(0.0242)	(0.0412)	(0.0489)	(0.0444)	(0.0601)	(0.0733)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,876	12,876	12,876	12,876	12,876	12,876
R^2	0.800	0.771	0.441	0.750	0.645	0.498

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample. We use the following six dependent variables in columns (1) to (6): *Establishment Entry Rate*, *Establishment Exit Rate*, *Net Entry Rate*, *Job Creation Rate*, *Job Destruction Rate*, and *Net Job Creation Rate*. All columns include control variables and fixed effects from the respective main specification. We report standard errors clustered at the level of 100 clusters of similar CZs.
All variables are defined in Appendix B.

In Table 8, we report the results of re-estimating Equation (1) using these dependent variables and WLS, where observations are weighted by lagged CZ population. We find that *Audit Rate* is negatively associated with net establishment entries (column (1)). When we separate this into entries and exits, we find that *Audit Rate* is negatively associated with entries (column (2)) but not with exits (column (3)). We find a similar pattern of results when examining job creation and destruction (columns (4) to (6)). These results suggest that tax enforcement hinders new business entry and job creation but does not affect the exit of businesses or jobs. These findings are consistent with prior research showing that higher compliance burdens reduce business entry (Djankov et al. 2002; Djankov et al. 2010).

The IRSRRA of 1998

Next, we address identification concerns by exploiting the IRSRRA of 1998 as a source of plausibly exogenous variation in tax enforcement (Gallemore and Jacob 2020). The IRSRRA altered the organizational structure of the IRS from a district-based to a federal-based structure starting late in the year 2000, meaning that tax enforcement efforts went from being directed by regional offices to being centrally directed post-IRSRRA. Thus, the change in the audit rates around the IRSRRA are less likely to be determined by local economic conditions and regulations. We follow Gallemore and Jacob (2020) by implementing a research design that exploits the change in tax enforcement due to the IRSRRA. Because the IRSRRA affects all U.S. CZs and firms, we exploit variation in treatment intensity, that is, differences across CZs or firms in the extent to which tax return audit rates changed around the IRSRRA. Specifically, we calculate *IRSRRA Audit Change* as the change in the IRS district-size class audit rate from the three years before the IRSRRA (1998–2000) to the three years following the IRSRRA (2001–2003); negative values represent audit rate declines for that IRS district-size class group. We then estimate the following regression over the period from 1998 to 2003:

$$Business\ Activity_{i,t} = \alpha_1 \times Large\ Decline_{d,s} \times Post_t + Controls + \alpha_i + \alpha_t + \varepsilon. \quad (3)$$

Business Activity is either *Establishment Growth* or *Employment Growth* in the CZ-level test and *Employee Growth* in the firm-level test. Our variable of interest is *Large Decline* × *Post*. We define *Large Decline* equal to 1 if the applicable *IRSRRA Audit Change* is below the median (bottom quartile) value in the CZ-level (firm-level) analyses.[24] Thus, this design compares changes in business activity around the IRSRRA for CZs/firms with larger declines in *IRSRRA Audit Change* with CZs/firms with smaller declines. We define *Post* as an indicator variable equal to 1 if the year is after the

[24] This approach results in *Large Decline* observations being associated with, on average, a 7.1 (10.7) percentage point audit rate decline in the CZ-level (firm-level) design. In untabulated analyses, we find that the CZ-level (firm-level) inferences are robust to a quintile (tercile, quartile, or quintile) cutoff for defining *Large Decline*.

TABLE 9

IRSRRA Analysis

	CZ-Level Test		Firm-Level Tests
Dependent Variable:	***Establishment Growth*** **(1)**	***Employment Growth*** **(2)**	***Employee Growth*** **(3)**
Large Decline × *Post*	1.2202*	2.1831**	0.0185**
	(0.6405)	(0.9131)	(0.0093)
Controls	Yes	Yes	Yes
CZ FE	Yes	Yes	—
Year FE	Yes	Yes	—
Firm FE	—	—	Yes
Industry-Year FE	—	—	Yes
Observations	3,498	3,498	22,472
R^2	0.151	0.153	0.443

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents regression results of estimating Equation (3) using the CZ-level (firm-level) sample in columns (1) and (2) (column (3)). In column (1) (column (2)), the dependent variable is CZ-level *Establishment Growth* (*Employment Growth*). In columns (3) and (4), the dependent variable is firm-level *Employee Growth*. In columns (1) and (2) (column (3)), *Large Decline* is an indicator variable equal to 1 if *IRSRRA Audit Change* is below the median (bottom quartile) value in the CZ-level (firm-level) sample. *Post* is an indicator variable equal to 1 if the year is 2001 to 2003. The sample period is 1998 to 2003. Standard errors are clustered at the level of 100 clusters of similar CZs (IRS district level) in columns (1) and (2) (column (3)).
All variables are defined in Appendix B.

IRSRRA was implemented (2001–2003).[25] Because we include CZ or firm fixed effects as well as year fixed effects, the main effects of *Large Decline* and *Post* are not identified in the model. We use the same controls and fixed effects as in Equation (1) (Equation (2)) for the CZ-level (firm-level) analysis. We report the results of this analysis in Table 9. Across both designs, we document positive and significant coefficients on *Large Decline* × *Post*, suggesting that CZs or firms with larger IRSRRA-induced audit rate declines subsequently increased business activity more than other CZs or firms. In terms of economic magnitudes, the results are comparable to those in Tables 3 and 4. For example, the coefficient in column (3) suggests that a 5 percentage point decline in the audit rate is associated with a 0.89 percent increase in firm-level employee growth, which is close to the estimate of 0.85 percent for a 5 percentage point decline from Table 4.

The ability to interpret the Table 9 results as indicating a causal effect of tax enforcement on business activity requires the assumption of parallel trends. As this assumption is inherently untestable, we examine the pre-event differences in trends in Figure 3. In these tests, the base year is 1997, and we compare the differences between groups for 1998, 1999, and 2000 while accounting for CZ (firm) fixed effects and year fixed effects in the CZ-level (firm-level) tests. In each panel, we find that the pre-event differences in the dependent variable between the *Large Decline* group and the other group are not statistically significant, nor is there a visible trend in these differences, suggesting that the parallel trends assumption is reasonable in our setting.[26] Overall, the evidence in Table 9 and Figure 3 is consistent with the findings in our main analysis, helping to mitigate endogeneity concerns.

Shift-Share Design

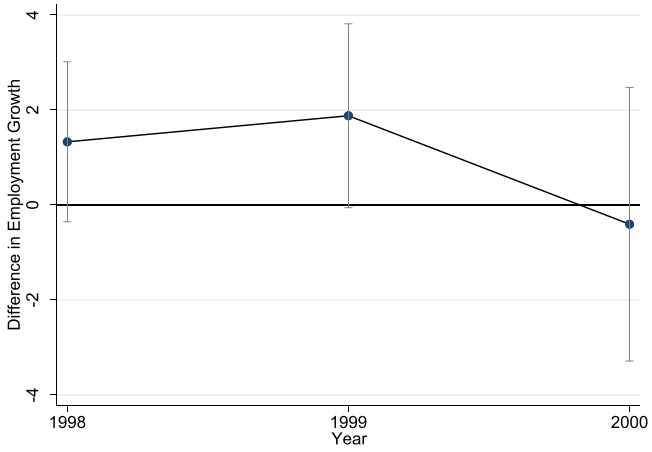
Using our CZ-level design, we also address concerns over regional time-varying omitted variables using a shift-share design that replaces the one-year lagged CZ size class shares with CZ size shares from 1992 (the beginning of our sample period) when calculating *Audit Rate*. Effectively, this design isolates treatment variation due to the differential

[25] We use 1998–2000 as the preperiod because the regional structure was used for most of the year 2000. Our findings are robust to instead using 1997–1999 as the pre-IRSRRA period (results untabulated).

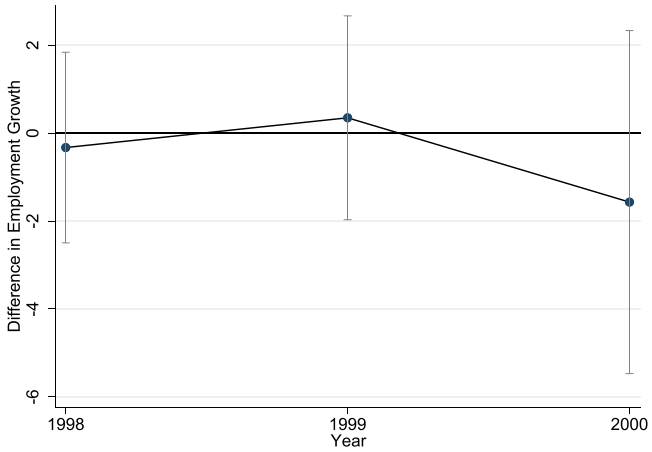
[26] A potential issue with this test is that the IRSRRA overlaps in time with the 2000 tech bubble collapse. Classifying firms (CZs) as being highly exposed to the tech bubble if the firm (the industries within the CZ) had market-adjusted returns during 2000 in the bottom 5 percent of our sample, we find that these results are robust to excluding these observations, suggesting that the tech bubble collapse is unlikely to drive these findings (results untabulated).

FIGURE 3
IRSRRA Analysis, Pre-Event Trends

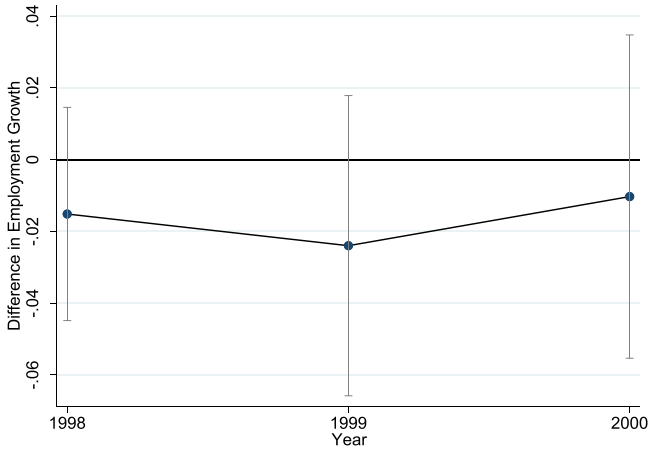
Panel A: Establishment Growth CZ Level



Panel B: Employment Growth CZ Level



Panel C: Employee Growth Firm Level



This figure presents the results from estimating a modified version of Equation (3), where we replace *Post* with indicators for the years 1998, 1999, and 2000. We estimate this regression on the pretreatment period (1997–2000). The dependent variable is CZ-level *Establishment Growth* (Panel A), CZ-level *Employment Growth* (Panel B), and firm-level *Employee Growth* (Panel C). We also present that 95 percent confidence bounds are based on standard errors clustered at the level of 100 clusters of similar CZs (IRS district level) in Panels A and B (Panel C).
All variables are defined in Appendix B and in the "Tax Enforcement, Business Formation, and Job Creation" section.

TABLE 10

Shift-Share Design

Dependent Variable:	*Establishment Growth*$_{t+1}$		*Employment Growth*$_{t+1}$	
	(1)	(2)	(3)	(4)
Audit Rate^{1992}	−0.5247**	−0.6251**	−0.8271***	−0.8928***
	(0.2054)	(0.2504)	(0.2897)	(0.2851)
Controls	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Model	OLS	WLS	OLS	WLS
Observations	12,876	12,876	12,876	12,876
R^2	0.089	0.169	0.075	0.146

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
Panel A presents regression results of estimating Equation (1) using the CZ-level sample. In columns (1) and (2) (columns (3) and (4)) of Panel A, the dependent variable is CZ-level *Establishment Growth* (*Employment Growth*). The main independent variable is *Audit Rate*^{1992}, which is similar to *Audit Rate* except that it uses establishment weights in 1992 (see the section "Shift-Share Design" for more details). All columns include control variables, CZ fixed effects, and year fixed effects. We estimate the models in columns (1) and (3) (columns (2) and (4)) using OLS (WLS using the CZ population in year *t* as weights). We report standard errors clustered at the level of 100 clusters of similar CZs in parentheses.
All variables are defined in Appendix B.

impact of the common shock of federal tax enforcement (i.e., the differential corporate tax enforcement across firm size groups over time) on CZs with different pre-determined exposures to that common shock (i.e., CZs' lagged exposure to firms in different size groups) (Bartik 1991; Goldsmith-Pinkham, Sorkin, and Swift 2020; Breuer 2022). We report the results of this approach in Table 10, finding results similar (in both sign and magnitude) as those in Table 3, mitigating concerns over time-varying CZ-level factors.[27]

Falsification Test Using Self-Employment Activity

Next, we conduct a falsification test by re-estimating our CZ-level regression using the growth in the number of self-employed establishments and the growth in total income by self-employed individuals as dependent variables. The idea is that self-employed activity should be affected by local economic conditions, but not by tax enforcement of C-corporations. We obtain data on self-employed establishments, which are mostly sole proprietorships, and income from the U.S. Census, which are available beginning in 1997. Using these data, we create two dependent variables: *Self-Employed Establishment Growth* (*Self-Employed Income Growth*), the logarithmic growth rate in self-employed establishments (total self-employed income) from *t* to *t*+1, multiplied by 100. Columns (1) and (2) of Table 11 present the results of estimating Equation (1) using these two variables. We find no statistical association between corporate tax enforcement and either variable. In column (3), we show that the association in column (1) is statistically different from the association in column (2) of Table 3 (t-statistic = 2.43). Thus, under the assumption that local factors equally shape business activity by both self-employed establishments and business establishments, but that only the latter are affected by corporate tax enforcement, these findings further mitigate concerns about unobserved local factors.[28]

C-Corporations versus other Legal Forms

Our primary independent variable, *Audit Rate*, only captures tax enforcement aimed at C-corporations. However, our CZ-level dependent variables capture the total number of establishments and employment, which includes other

[27] One concern with this approach is that if the weights do not change over time, then the endogenous factors in the current year may have also existed at the beginning of the sample period. In untabulated results, we find that for each size group, the correlations between 1992 weights and future weights are positive but below 0.5 for most sample years.

[28] As an additional falsification test, we examine whether we find a similar association between *Audit Rate* and business activity for firms subject to continuous audit under the Coordinated Industry Case (CIC) program of the IRS. If *Audit Rate* explains variation in business activity for CIC firms, this would suggest that our main findings may instead capture the impact of some nontax enforcement size-based characteristic on business activity. Using the findings from Ayers, Seidman, and Towery (2019) to estimate the probability that a firm is part of the CIC program, we find that *Audit Rate* is not associated with employment growth for firms that are likely under continuous audit (results untabulated)

TABLE 11

CZ-Level Falsification Analysis

	Falsification Outcomes		
Dependent Variable:	***Self-Employed Establishment Growth*$_{t+1}$**	***Self-Employed Income Growth*$_{t+1}$**	**Affected Establishments** ***Establishment Growth*$_{t+1}$**
	(1)	**(2)**	**(3)**
Audit Rate	0.0796	0.0370	−0.5860**
	(0.0786)	(0.1630)	(0.2538)
Difference to column (1)	—	—	0.6686**
[t-statistic]			[2.55]
Controls and FE	Yes	Yes	Yes
Observations	10,537	10,537	12,876
R^2	0.678	0.510	0.169

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.
This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample. In column (1) (column (2)), the dependent variable is CZ-level *Self-Employed Establishment Growth* (*Self-Employed Income Growth*). In column (3), the dependent variable is CZ-level *Establishment Growth*. The main independent variable is *Audit Rate*. We estimate these regressions using WLS, with the CZ population in year *t* as weights. All columns include control variables, CZ fixed effects, and year fixed effects. We report standard errors clustered at the level of 100 clusters of similar CZs in parentheses.

organizational forms, such as S-corporations or partnerships. Although there is a trend toward increased business activity occurring in non-C-corporation legal forms during our sample period, C-corporations were still responsible for a material portion of employment and establishments during our sample period. According to the Statistics of Income program of the IRS, 66.2 percent of total revenues of U.S. businesses during our sample period (1992 to 2015) are from C-corporations. Furthermore, the CBP data provide employment by legal form beginning in 2008. Over the 2008 to 2015 period, these data indicate that in the size groups that we use in our study (i.e., establishments with 50 or more employees), 56 (54) percent of employment is done by C-corporations in 2008 (2015). These percentages were likely larger for the earlier sample years. These statistics suggest that most of the business activity that we examine relates to C-corporations.

Ideally, we would directly examine CZ-level business activity by C-corporations. Unfortunately, data on business activity by legal form are only available at the state level as of 2010. We use these data to conduct two robustness tests. First, we re-estimate our primary specifications at the state level using data from 2010 to 2016. We examine two sets of business activity measures: those for C-corporations and those for other legal forms. For this test, we redefine *Audit Rate* at the state level using a similar approach to the one described in the "CZ-Level Research Design" section. We also include all control variables from our primary specification, aggregated to the state-year level. We then re-estimate Equation (1), clustering standard errors at the state level. We report these findings in Panel A of Table 12. Supporting our main findings, we find that *Audit Rate* is negatively associated with growth in establishments and employment by C-corporations and not associated with establishment or employment growth by non-C-corporations. Importantly, the coefficients on *Audit Rate* for C-corporations are significantly different from their counterparts for non-C-corporations.

Second, we use these state-level data to impute CZ-level values of C-corporation business activity. We first calculate the state-level fraction of total business activity (establishments or employment) conducted by C-corporations. We then apply this statistic to create CZ-level measures of C-corporation activity using either the average fraction at the state level over the 2010–2016 period or by extrapolating the trend in the fraction back to 1992. This approach assumes that the state-level fraction (or trends in the fraction) of business activity conducted by C-corporations is representative of the similar share of business activity at the CZ level. We then use these imputed CZ-level business activity variables in our CZ-level approach, which we report in Panel B of Table 12. These findings largely mirror those from our primary analyses. Collectively, the Table 12 results suggest that our findings are unlikely to be attributable to measurement error in the local business activity proxies.

Additional Robustness Tests

We conduct several sets of untabulated robustness tests. First, because audit rates are correlated with firm size, our findings may be driven by macroeconomic trends differentially affecting the business activity of firms of different size

TABLE 12

CZ-Level Business Activity by C-Corporations versus Other Legal Forms

Panel A: State-Level Analysis

Dependent Variable:	*Growth in C-Corp Establishments* (1)	*Growth in Non-C-Corp Establishments* (2)	*Growth in C-Corp Employment* (3)	*Growth in Non-C-Corp Employment* (4)
Audit Rate	−0.0670**	−0.0150	−0.1036**	−0.0185
(State-level)	(0.0326)	(0.0122)	(0.0402)	(0.0202)
Difference: C-Corp versus non-C-Corp	0.0521*		0.0851**	
[t-statistic]	[1.96]		[2.50]	
Controls	Yes	Yes	Yes	Yes
State and Year FE	Yes	Yes	Yes	Yes
Observations	250	250	250	250
R^2	0.475	0.678	0.342	0.501

Panel B: Using Measures of CZ-Level Imputed C-Corp Business Activity

Dependent Variable:	Imputed C-Corp Establishment Growth		Imputed C-Corp Employment Growth	
Audit Rate	−0.5421***	−0.4407**	−0.9551***	−1.0895***
	(0.1957)	(0.1926)	(0.3317)	(0.2800)
Controls and FE	Controls, CZ FE, Year FE			
Share C-Corp	Extrapolated	Average 10–16	Extrapolated	Average 10–16
Observations	12,876	12,876	12,867	12,876
R^2	0.095	0.090	0.085	0.069

*, **, *** Denote significance at the 10, 5, and 1 percent levels, respectively.

This table presents the results of estimating a modified version of Equation (1) using the CZ-level sample. In Panel A, we redefine Equation (1) to be estimated at the state-year level using a sample of state-years from 2010 to 2016. We create state-year versions of *Audit Rate* based on the state's exposure to C-corporation establishments in different size groups. The dependent variable is one of four measures, each defined similar to *Establishment* (*Employment*) *Growth*: *Growth in C-Corp Establishments* (*Employment*) only uses C-corporation establishments (employment), and *Growth in Non-C-Corp Establishments* (*Employment*) only uses non-C-corporation establishments (employment). We include all control variables from our primary specification, aggregated to the state level, as well as state and year fixed effects. In Panel B, we redefine our dependent variables as follows. We first calculate the fraction of total business activity (establishments or employment) at the state level that is conducted by C-corporations, and then we extrapolate this fraction to the CZ level using either one of two approaches: using the average fraction at the state level over the 2010–2016 period or using the trend in the fraction back to 1992. With these imputed variables, we recreate our measures of establishment and employment growth using the approach described in the "CZ-Level Research Design" subsection of Section III and re-estimate Equation (1) using these measures. We report standard errors clustered at the state level (the level of 100 clusters of similar CZs) in parentheses in Panel A (Panel B).

groups. We address this issue in two ways. First, we include several CZ-level control variables based on the firm-level controls from Equation (2) using the same approach used to create *Audit Rate*. Second, we account for differences in CZ exposure to firms of various sizes by including the CZ-level proportion of total firms in each size group in their linear, squared, and cubed forms as controls. Our results are robust to both approaches.

Second, our results are robust to using the change in the *Audit Rate* (either a continuous version Winsorized at 5 and 95 percent or a ranked version due to outliers) instead of *Audit Rate* or additionally controlling for the lagged *Audit Rate*. Our CZ-level results are additionally robust to (1) using employment weights rather than establishment weights in creating the CZ-level *Audit Rate* measure, (2) using the total number of establishments or employment when constructing our dependent variables, (3) different cutoffs to determine which CZs are included in our analyses, (4) alternative crosswalks matching our asset-based IRS tax return audit rate data to the employment-based Census business activity data, and (5) using an alternative weighting scheme to capture the CZ's importance in a size bucket to define *Audit Rate*.

Finally, we assess the sensitivity of our findings to unobservable factors by using the Oster (2019) methodology. Specifically, we calculate the Oster delta, which represents the importance of unobserved factors to observable (i.e., controlled for) factors that would be needed to overturn our findings.[29] In our CZ-level analyses, we document an Oster

[29] We follow Oster (2019) and assume an R^2 ($R(max)$) that is 1.3 times the R^2 from the "controlled" regression (i.e., the regression with all controls and fixed effects). In the uncontrolled model, we include year (year and size group) fixed effects and *Audit Rate* in the CZ-level (firm-level) test.

delta of 4.61 (2.48) for establishment (employment) growth. In our firm-level analysis, the Oster delta is 1.51. These deltas suggest that unobservable factors would have to be at least as important as the included observable controls to overturn our findings. Given that we control for many determinants of business activity, we believe these results suggest that our main inferences are unlikely to be driven by unobservable factors.

VII. CONCLUSION

This study explores whether corporate tax enforcement affects business activity. Exploiting variation in tax return audit rates across firm size groups and time and using two different methodologies—a regional design and a firm-level design—we find that corporate tax enforcement is negatively associated with business activity. This association is economically important and is robust to different specifications that mitigate endogeneity concerns and measurement issues. We also show that the negative effect of corporate tax enforcement is weaker in regions with more access to bank lending and for firms with greater access to external financing sources and is stronger in regions and firms for which compliance costs are likely greater and for which tax enforcement is more binding. Overall, our findings suggest that corporate tax enforcement has material implications for business activity, both on average and in how this activity is distributed across regions and firms. Although the magnitudes implied by our estimates suggest that the negative effects of tax enforcement on business activity are unlikely to outweigh the benefits of such enforcement for tax revenues, our findings are relevant for academics and policymakers seeking to understand the consequences of tax enforcement.

Our inferences should be interpreted with several caveats in mind. First, we focus on documenting one cost of tax enforcement, reduced short-run business activity. Because we do not study the potential benefits that come from government spending of collected tax revenues, we cannot speak to the overall net effect of tax enforcement. Second, although our measure of tax enforcement has been used in prior research, it has certain drawbacks, such as not capturing audit intensity.

We believe there are several fruitful paths for future research. First, it would be worthwhile to understand whether the cross-sectional variation in the consequences of tax enforcement documented in this study are driven by direct or deterrence effects. Second, our data do not allow us to separate out audits by type (e.g., random audits, targeted audits, or audits conducted as part of specific IRS campaigns) or capture differences in audit intensity. Future research could explore how these audit characteristics differentially impact business activity and tax collections.

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APPENDIX A

Statistics on the Crosswalk between CPB and IRS Size Groups

CBP Data (Number of Employees)	Category in IRS Data (Total Assets)	Number of Employees (from Compustat)		
		P25	Median	P75
50 to 99	$5 million to $10 million	22	52	95
100 to 249	$10 million to $50 million	63	137	275
250 to 499	$50 million to $100 million	154	357	765
500 to 999	$100 million to $250 million	238	657	1,547
1,000 or more	$250 million or above	674	2,700	8,675

This table presents evidence supporting our crosswalk between the IRS size groups and CBP size groups described in the "CZ-level Research Design" subsection of Section III. We present descriptive statistics on the number of employees for firms in Compustat for different size groups over our sample period from 1992 to 2015.

APPENDIX B

Variable Definitions

Panel A: CZ-Level Variables

Variable	Description
Establishments	Number of establishments with at least 50 employees in the CZ.
Employment	Number of employees in firms with at least 50 employees in the CZ. We approximate this value by taking the total number of employees minus the number of establishments with 1 to 4 employees times 2.5 (the midpoint of 1 and 4), establishments with 5 to 9 employees times 7, establishments with 10 to 19 employees times 14.5, and establishments with 20 to 49 employees times 34.5. Negative values of this approximation are set to missing.
Establishment Growth	The change in the natural logarithm of *Establishments* from t to t+1.
Employment Growth	The change in the natural logarithm of *Employment* from t to t+1.
Audit Rate	The weighted average tax return audit rate in the CZ, as explained in the "CZ-Level Research Design" section and Appendix A and defined by the following formula: $Audit\ Rate_{c,t} = \sum_{i=1}^{N} \frac{Establishment_{s,c,t-1}}{Establishment_{c,t-1}} \times Audit\ Rate_{s,t,d}$. $Audit\ Rate_{s,t,d}$ is the audit rate for size class s in year t (and in district d until 1999). We weight size-class-specific audit rates by the ratio of the establishments in that CZ-year within that size class to the total number of establishments in that CZ. We use the following assignments of establishments to audit size classes: 50 to 99 employees to the $5 million–$10 million size class, 100 to 249 employees to the $10 million–$50 million size class, 250 to 499 employees to the $50 million–$100 million size class, 500 to 999 employees to the $100 million–$250 million size class, and 1,000 and more employees to the $250 million and more size class. If a CZ spans multiple districts, we use the weighted average audit percentage in the respective district-size class.
Audit Rate^{1992}	The weighted average tax return audit rate in the CZ, as defined by the following formula: $Audit\ Rate_{c,t}^{1992} = \sum_{i=1}^{N} \frac{Establishment_{s,c,1992}}{Establishment_{c,1992}} \times Audit\ Rate_{s,c,t}$. This is similar to *Audit Rate*, except that rather than using weights for each size group as measured in year $t-1$, we use the CZ's weights as measured in 1992.
Unemployment	Seasonally adjusted unemployment rate in the CZ.
GDP per Capita	Natural logarithm of state-level GDP per capita. If a CZ spans multiple states, we weight each of those state tax rates by the share of the CZ population in each state.
Tax Rate	State-level corporate income tax rate. If a CZ spans multiple states, we weight each of those state tax rates by the share of the CZ population in each state.
Throwback	Indicator variable equal to 1 if the state implements a throwback rule. If a CZ spans multiple states, we weight the indicator by the share of the CZ population in each state.

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APPENDIX B (continued)

Variable	Description
Combined	Indicator variable equal to 1 if the state implements combined reporting. If a CZ spans multiple states, we weight the indicator by the share of the CZ population in each state.
Sales Factor	State-level sales apportionment factor in percent. If a CZ spans multiple states, we weight the factor by the share of the CZ population in each state.
Payroll Factor	State-level payroll apportionment factor in percent. If a CZ spans multiple states, we weight the factor by the share of the CZ population in each state.
Property Factor	State-level property apportionment factor in percent. If a CZ spans multiple states, we weight the factor by the share of the CZ population in each state.
Asian	Proportion of the CZ's population that is Asian.
Black	Proportion of the CZ's population that is Black.
Hispanic	Proportion of the CZ's population that is Hispanic.
Over 65	Proportion of the CZ's population that is over 65 years old.
Under 4	Proportion of the CZ's population that is under the age of 4.
Income	Natural logarithm of average income per employee in the CZ.
IRSRRA Audit Change	Difference in CZ-specific audit rates over the period 1998–2000 minus the same average over the period 2001–2003.
Large Decline	Indicator variable equal to 1 if *IRSRRA Audit Change* is in the bottom quartile and 0 otherwise.
Low IRS Office Distance	Indicator variable equal to 1 if the CZ distance to the nearest IRS office using the locations from Kubick et al. (2017) is within the bottom quartile. The CZ-level distance is measured using the weighted average distance across counties in a CZ (using employees in each county as the weight).
High Banking Density	Indicator variable equal to 1 if the number of bank branches per capita is above the median across all CZs in a given year and 0 otherwise. We only use bank branches associated with commercial banks with non-zero commercial lending.
Low Accountants	Indicator variable equal to 1 if the percentage of the total workforce that works in the accounting sector (SIC codes 7291 and 8721 or NAICS code 54,121) is in the bottom quartile across all CZs in a given year and 0 otherwise.
Establishment Entry Rate	Number of new establishments scaled by the total number of establishments in a CZ.
Establishment Exit Rate	Number of exiting establishments scaled by the total number of establishments in a CZ.
Net Entry Rate	New minus exiting establishments scaled by the total number of establishments in a CZ.
Job Creation Rate	Number of new jobs scaled by the total number of employees in a CZ.
Job Destruction Rate	Number of eliminated jobs scaled by the total number of employees in a CZ.
Net Job Creation Rate	New minus eliminated jobs scaled by the total number of employees in a CZ.

Panel B: Firm-Level Variables

Variable	Description
Employee Growth	Natural logarithm of growth in the number of employees from t to t+1.
Audit Rate	Audit rate for the firm's size group in year t (and in the respective IRS district until 1999).
Cash Holdings	Cash holdings scaled by total assets.
Sales Growth	Natural logarithm of growth in sales from $t-1$ to t.
Leverage	Total debt scaled by total assets.
Pre-tax income	Pre-tax income scaled by lagged total assets.
Size	Natural logarithm of total assets.
MtB	Market value scaled by total assets.
Large Decline	Indicator variable equal to 1 if the change in the audit percentage from before to after the IRSRRA is below the sample median.
Capex	Capital expenditures scaled by gross property, plant, and equipment.
R&D	R&D expenditures scaled by gross property, plant, and equipment.
Wage Growth	Logarithmic growth rate in staff expenditures from t to t+1.

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APPENDIX B (continued)

Variable	Description
Low 3-Year ETR	Indicator variable equal to 1 if the three-year cash ETR is below the bottom quartile within an industry and 0 otherwise; set to missing if the ETR denominator is negative.
Low IRS Distance	Indicator variable equal to 1 if the distance to the nearest IRS office is below the bottom quartile within an IRS size group and 0 otherwise.
Low WW Index	Indicator variable equal to 1 if the Whited and Wu (2006) index is below the top quartile within an industry and 0 otherwise.
Low KZ Index	Indicator variable equal to 1 if the Kaplan and Zingales (1997) index is below the top quartile within an industry and 0 otherwise.
ICW	Indicator variable equal to 1 if the firm has an internal control weakness and 0 otherwise.
IRSRRA Audit Change	The average size-district group-specific audit rate over the period 1998–2000 minus the average size-district group-specific audit rate over the period 2001–2003.
Large Decline	Indicator variable equal to 1 if *IRSRRA Audit Change* is in the bottom quartile and 0 otherwise.