Government Audits

Martina Cuneo, Jetson Leder-Luis, and Silvia Vannutelli *

This Version: October 14, 2023

Abstract

Audits are a commonly-used monitoring tool to ensure accountability in the management of public funds. Yet audits are costly and do not always produce valuable results. In this paper, we use theory and empirics to examine the effectiveness of government audits as a function of state capacity. We provide a simple conceptual framework that highlights the trade-offs involved in the decisions of governments when choosing their audit efforts. The value of audits depends on both the underlying presence of abuse and on the government's ability to enforce punishments, making auditing most effective in middling state-capacity environments. Consistent with our predictions, we survey all the existing credibly causal studies and show that government audits have positive effects mostly in middle-state-capacity environments like Brazil. Finally, we present novel empirical evidence on the effectiveness of audits for local governments in the US, a high-capacity and low-impropriety environment. Using a previously unexplored threshold in federal audit rules and a dynamic regression discontinuity design, we find no marginal effects of audits on any fiscal outcomes of local governments, a result that is in line with the predictions of our model. Overall, our findings suggest that countries like the US might benefit from relaxing audit requirements and reducing their regulatory burden.

JEL: H83, D73, H7, M42

*Martina Cuneo: NYU Economics
Jetson Leder-Luis: Boston University & NBER
Silvia Vannutelli, Northwestern University, & NBER, corresponding author. Email: silvia.vannutelli@northwestern.edu. We thank Ray Fisman, Benedikt Herrmann, Chiara Margaria, Maggie Shi and Andrei Shleifer, as well as participants at the IIPF 2022 congress, the CSEF-IGIER Symposium on Economics and Institutions and the Mannheim Taxation Conference for their valuable feedback.

1 Introduction

Governments worldwide provide goods and services financed via public money and are thus concerned about improving efficiency and reducing waste in public spending. Most governments are highly decentralized, so a substantial share of public spending is managed directly by local governments. While decentralization is commonly thought to improve public service delivery (Bank 2003), it also poses agency problems (Bardhan and Mookherjee 2006). Local officials might have diverging preferences and conflicting incentives from national ones (Shleifer and Vishny 1993, Bardhan 2002, Besley, Pande, Rahman and Rao 2004), leading to the arousal of forms of active waste (i.e. corruption or fraud) and passive waste (i.e. inefficiencies) (Bandiera, Prat and Valletti 2009).[1]

Audits are a common mechanism to ensure accountability in the management of public funds, particularly in decentralized organizations. While commonly used, audits are costly and do not always produce valuable results. In 2015, for example, the Canadian government spent $23.6 million to audit its Senate and only uncovered less than $1 Million of improper spending (Press 2015). When deciding whether or not to introduce audit requirements, or to expand audit efforts, governments thus face a trade-off between accountability and an efficient use of resources. The effectiveness of audits may vary between institutional environments in which they are conducted – both among poorer and wealthier nations or between better- and worse-performing organizations within a single government. While the existing empirical literature has largely found audits to be beneficial, these benefits may be highly contextual to the circumstances in which they have been studied: moderate government capacity environments, which may be non-representative.

In this paper, we use theory and empirics to examine the effectiveness of government audits as a function of state capacity. We start by developing a simple conceptual framework that highlights the trade-offs involved in the decisions of governments when choosing their audit efforts. Importantly, we focus on internal government audits, as opposed to tax or third-party audits, but our findings about the value of audits have natural extensions to those cases.[2]

In our framework, the value of audits depends on two factors: the underlying level of waste the audit can detect, and the ability to respond to audit findings. The size of waste is

[1] According to Bandiera et al. (2009), active waste is such that it entails direct or indirect benefit for the public decision maker, while passive waste does not. Passive waste might arise as a result of officials' lack of skills or lack of incentives to achieve efficient spending.

[2] We also abstract from other types of monitoring undertaken directly by citizens (Pande 2011) and/or the media, as they entail different types of trade-offs and do not pertain to the organizational design of government institutions.

negatively correlated with state capacity (Shleifer and Vishny 1993, Banerjee 1997), while the ability to respond to audit findings is increasing in state capacity, which indicates that auditing will be most effective in middling-capacity environments or countries.

When state capacity is low, there is a large amount of potential *active* waste to be detected by audits, including kickbacks, misappropriation of funds, patronage and favoritism. However, governments tend to have little ability to respond to the findings of audits by imposing and enforcing punishment. In these contexts, rather than being beneficial, the presence of audits may create further opportunities for abuse, by giving incentives to the officials in charge of performing audits to take bribes or extort the targets of audits (Khalil, Lawarrée and Yun 2010, Angelucci and Russo 2022).

On the other extreme, in high state-capacity contexts, as the Canadian example above, the underlying amount of waste to be detected is generally limited and mostly of the *passive* waste type, i.e. more likely to be related to agents' inability or lack of incentives to formally comply with regulations and/or to achieve the best value for money. In this context, audits might be costly and detrimental, leading to excessive regulatory burden and increasing, rather than combating, passive waste (Kelman 1990; 2005).[3] Despite their inefficiency, audits are still common in high-capacity countries. We think that this fact can easily be rationalized by taking into account that audit mandates are frequently set by politicians, who might be overly risk-averse and more worried about the potential costs of a rare corruption scandal than the benefit of reducing inefficient audit-related costs. Moreover, audit requirements have the political benefit of appearing proactive, even if they are ultimately ineffective.

Guided by the insights of our framework, we then move to review existing studies, using estimates from the literature as data points to discuss the effectiveness of audits as a function of state capacity. Auditing of governments has garnered much attention in both the economics and accounting literature, with most studies exploring the effects of individual auditing programs. We show that these patterns match our model, with audits being most effective in middle-capacity countries where there is both enough bad behavior to detect and sufficient government capacity to respond to these findings. However, there is very little evidence on audit effectiveness in both high-capacity and low-capacity contexts. Part of the (lack of) evidence might be due to publication bias against null effects, as our conceptual framework predicts that audits should be less likely to have effects in both of these contexts.

To help fill this gap in the literature, and in support of our theoretical findings, we present novel evidence from the U.S. context. We exploit the federal mandate that requires cities

[3]Indeed, Bosio, Djankov, Glaeser and Shleifer (2022) find that in high state-capacity countries, better practices are associated with better procurement outcomes, while stricter regulations are not, while the opposite is true in low-capacity contexts.

receiving revenue from the federal government above a threshold to perform a Single Audit, wherein they hire a third-party firm to audit the city's spending.

Identifying the causal effect of audits in this context is made challenging by the fact that governments can be audited multiple times and audits may have both contemporaneous as well as delayed impacts on outcomes. We thus implement a dynamic regression discontinuity design in line with Cellini, Ferreira and Rothstein (2010) that allows us to account for the dynamic treatment effects of audits on government outcomes. The first stage of our regression discontinuity shows that crossing the threshold increases audit likelihood by 20%, with no evidence of strategic sorting. However, our results show that these quasi-random audits have precise zero effects on city finances, including revenues, taxes, and administrative expenditures. This suggests that the US is on the right side of the state capacity vs. audit value curve, where the value of auditing is low.

Our discussion then extends to the policy implications of audit effectiveness. The lack of effectiveness of US city audits indicates the federal government can raise the threshold above which cities can be audited, saving resources without compromising city integrity. We discuss the appropriateness of audits in low and high-state capacity countries, as well as the other governmental factors that function as complements or substitutes to auditing.

This paper relates to three existing strands of the literature. First, we relate to theoretical work studying the role of monitoring and the trade-offs involved in agency relationships (Tirole 1986, Kofman and Lawarrée 1993, Mookherjee and Png 1995, Laffont and Martimort 1998, Mookherjee 2006). Most theoretical literature has focused on the use of monitoring as a tool to alleviate agency problems within organizations but has abstracted from understanding the determinants of monitoring effectiveness.

Second, we relate to the growing body of empirical work studying the role of audits as an anti-corruption and governance tool (e.g., Ferraz and Finan (2008)). While recent work has focused on specific programs and provided country-specific micro-evidence on the causal effects of government audits (Shi 2022, Vannutelli 2022, Montenegro 2020)), little attention has been devoted to understanding the overall circumstances required for audits to succeed.

Lastly, we speak to the more general literature thinking about the organization of the state and the tradeoffs involved in the regulation of public spending (e.g., Bandiera et al. (2009), Bosio et al. (2022)). While previous work has mostly focused on the trade-offs between rules and discretion in the context of public procurement, we highlight the importance of taking into account similar considerations when thinking about the design of monitoring institutions, that are a crucial complement to procurement regulation.

2 The Value of Audits

We present a simple conceptual framework that discusses the value of government audits as a function of state capacity. The intuition is simple: the effectiveness of audits depends on two components. First, the presence of abuse, and second, the ability to detect and enforce punishment against that behavior. The underlying level of abuse is negatively correlated with state capacity, and the ability to enforce is positively correlated with state capacity. Therefore, audits are most effective when the government has middling levels of state capacity.

This framework is related to several other theoretical discussions of auditing and government capacity, although it is unique it in its focus on the relationship between these phenomena. Most closely, Ortner and Chassang (2018) discuss a model of auditing where a principal-agent relationship is moderated by a monitor, such as an auditor, and where the auditor can collude with the agent. Bosio et al. (2022) discuss public sector capacity in a way that is similar to our metric of state capacity, and show the relationship between state capacity and optimal procurement rules, a different dimension along which governments seek to inhibit bad behavior.

2.1 When Should Governments Audit?

Consider a central government of type $s \in \mathbb{R}$, reflecting state capacity. The government presides over a local agent that engages in abuse $c \in \{0, 1\}$ with probability $p_c(s)$ that $c = 1$. The abuse of the agent can be thought of as either waste, intentional mismanagement, or corruption. The probability of abuse is decreasing in state capacity, so that $p_c'(s) < 0$. The intuition for this is that, as state capacity s increases, the share of bad agents declines, and so does the probability that any agent is bad. Abuse is costly to the government: when $c = 1$, the government pays a cost π_C.

The government has a choice of whether to audit the agent, at a cost of $a(s)$. Note that we allow the cost function to vary with state capacity. Audits allow the government to observe the otherwise-unknown value of c. If $c = 1$ is revealed by the audit, the government then undertakes enforcement action, and if $c = 0$, the government does nothing. Enforcement has a probability $p_e(s) \in (0, 1)$ of success. The probability of successful enforcement is increasing in state capacity, with $p_e'(s) > 0$. When the enforcement is successful, the government recovers k from the agent, where $k \geq \pi_C$.

Should the government undertake the audit?

The government's expected payouts if it conducts the audit are:

$$-a(s) + p_c(s) \times [-\pi_C + p_e(s)k]$$

The government's expected payouts if it does not audit are:

$$p_c(s) \times (-\pi_C)$$

Therefore, to maximize expected value, the government should audit if:

$$-a(s) + p_c(s) \times [-\pi_C + p_e(s)k] > p_c(s) \times (-\pi_C)$$

Which happens whenever:

$$-a(s) + p_c(s)p_e(s)k > 0$$

i.e. when:

$$p_c(s)p_e(s)k > a(s)$$

Both p_c and p_e are functions of state capacity, with p_c decreasing in s and p_e increasing in s. When state capacity is very high, $p_c(s)$ is low, shrinking the left term, meaning the central government has little to gain from audits because there is no abuse to detect. When state capacity is very low, $p_e(s)$ is low, and thus the central government has little to gain from audits because the probability of successful enforcement is small. Audits are thus most effective for middle ranges of $p_c(s)$ and $p_e(s)$.

Figure 1 displays the main intuition of our conceptual framework, as it relates the value of audits with state capacity. Our framework produces an upside-down U-curve, reflecting the fact that audits are most valuable in middle-state-capacity contexts. On the left-hand side of the graph, the value is low due to the limited ability to enforce when audits find bad behavior. On the right-hand side of the graph, while enforcement capacity is high, there is limited abuse to detect, diminishing the value of audits.

2.2 The Role of State Capacity

Our conceptual framework relies on two main ideas: that the underlying level of abuse to detect is decreasing in state capacity, and that the ability to respond to audits is increasing in state capacity.

Figure 2 presents empirical evidence in support of these ideas. On the X-axis, we measure state capacity using the World Bank government effectiveness measure, a measure also

used by Bosio et al. (2022). In the left panel, we look at how a measure of audit responsiveness varies with state capacity. The measure comes from the Public Expenditure and Financial Accountability (PEFA) 2016 framework, an international program organized by the World Bank and other organizations to measure performance in public finance management(Kristensen, Bowen, Long, Mustapha and Zrinski 2019)[4]. The figure shows clearly that the ability to respond to audits is positively correlated with state capacity, which validates our assertion that $p'_(s) > 0$. In the right panel, we look at the relationship between a proxy of underlying abuse and state capacity. Here, we exploit the Corruption Perception index rank from Transparency International, 2021.[5] We find that corruption is decreasing in state capacity, which supports our assertion that $p'_c(s) < 0$. The combination of these two patterns would produce an audit value that is inverse-U-shaped in state capacity, although in practice, empirical estimates of audit effectiveness only come from individual studies, which we discuss in Section 3.

2.3 Discussion

Our stylized conceptual framework, albeit simple, highlights two key ingredients that should be taken into account by governments when deciding whether or not to enact or expand audits. First, it shows how state capacity is necessary for audits to be effective because the government needs credible tools to punish those who misbehave. This is in line with empirical work on enforcement, such as Gonzalez Lira and Mobarak (2019), who show that preventing agents from engaging in illegal acts requires an institutional system that is not only able to enforce laws, but is also able to track agents' reactions to the enforcement, to prevent them from finding loopholes to skirt regulations. Second, it stresses the importance of having some significant underlying level of abuse to detect to justify the undertaking of audits. This is in line with the theory and evidence of Bosio et al. (2022), who show how laws, in general, are not correlated with procurement outcomes and suggest that regulation can be suboptimal in places characterized by a high public sector capacity, as it seems to be a far too great limitation to agents' discretion given the relatively low risk of abuse.

Similarly, our theory can be interpreted through the lenses of the distinction between active and passive waste in (Bandiera et al. 2009). While the presence of active waste requires monitoring, if the underlying form of abuse is of the kind of passive waste, audit require-

[4]The Public Expenditure and Financial Accountability (PEFA) program was initiated in 2001 by seven international development partners: The European Commission, International Monetary Fund, World Bank, and the governments of France, Norway, Switzerland, and the United Kingdom. The PEFA framework is a standard methodology for public financial management diagnostic assessment using 31 indicators. For additional details, see `https://www.pefa.org/about`

[5]We obtain very similar results when using a measure of bribe incidence from Bosio et al. (2022).

ments might make things even worse by introducing additional regulatory burden. In this sense, audits might have a low (or potentially negative) value even if they were costless for the government, as they could introduce indirect costs and sources of inefficiencies, leading to longer completion time for public works (Calvo, Cui and Serpa 2019a) and potentially inducing bureaucratic red tape. While our basic framework presents corruption as simply a share of bureaucrats engaged in a binary of corruption, one could imagine an extension where the type of corruption varies with state capacity, with similar results.

Direct and indirect audit costs are likely to vary themselves with the level of state capacity. In an effort to keep the framework as simple as possible, we abstract from costs here, but we provide a detailed discussion of their role in Appendix A.1. We show how the value function relates to various specifications of the cost function, including costs increasing or decreasing in state capacity. Appendix Figure A1 shows that for many functional forms of the cost, the marginal benefit of audits will exceed costs in the middle of the curve, i.e. for middle state capacity environments, as long as the cost function is not a steeper parabola than the value function.

Our stylized framework also abstracts away from some other features of audits, most notably their deterrence effects. Importantly, in our model, a bureaucrat's decision of whether to engage in bad behavior is not a function of the audit. This reflects the fact that our framework considers a marginal audit, which is unlikely to change the underlying level of bad behavior. Put another way, we consider a government in a steady state of state capacity, deciding on the margin whether to conduct an audit, which motivates our static game. On the margin, these decisions are unlikely to have broader impacts on overall deterrence. However, our work does not indicate that audits should be abandoned altogether, as that could eliminate valuable deterrence in the dynamic, long-run repeated game. In Section 5, we discuss systems of internal controls that are complementary forms of monitoring that governments can do instead of additional marginal audits.

Similarly, our theory simplifies various political aspects of the decision-making process. Auditing of governments is observed worldwide, even in circumstances where the returns are low either due to the low level of underlying corruption, or the low ability of the state to respond to the findings of the audit. It is reasonable that some countries conduct audits because they are politically popular, rather than efficient. For example, politicians in high state capacity environments might be risk-averse and afraid of facing electoral consequences for corruption scandals, and this could lead them to adopt overly-restrictive audit regimes even in the presence of a relatively little underlying amount of abuse. We extend our framework to account for this possibility in Appendix A.2.

3 Audits and State Capacity: Evidence from Existing Studies

The framework presented in Section 2 indicates that audits of governments are most valuable in middle-state-capacity contexts. In this section, we review the existing evidence on auditing, treating estimates from other studies like data. The literature has generally been very positive on audit effectiveness; however, most existing studies focus on middle-income countries where we predict audits will be most effective in curbing the misuse of resources.

Table 1 presents a survey of economics articles that causally identify the effect of internal government audits. Point estimates are divided by dependent variable means so as to obtain comparable magnitudes expressed in percentage changes. Notably, looking at the country column, we see that auditing has largely been studied in middle-income countries such as Brazil, India, Indonesia, or Puerto Rico. There is very limited empirical literature that studies highly developed or very low-income countries.

Several studies on Brazil have found audits to be highly effective, which aligns with our predictions, as Brazil has both a high level of underlying bad behavior and also a high administrative capacity for addressing its findings. Notably, these studies also highlight the key role of other aspects that would facilitate direct or indirect enforcement of punishments, such as the diffusion of information via radio and the presence of re-election motives (Ferraz and Finan 2008; 2011, Avis, Ferraz and Finan 2018).

Similar results were reported in the context of Puerto Rico by Bobonis, Cámara Fuertes and Schwabe (2016), who found a large percentage change of corruption due to the effects of audits. The authors show that municipalities that were audited before the elections were affected by systematically lower corruption in the short run, although with temporary results. Puerto Rico is unique in that its local institutions are relatively weak, allowing for local corruption, but its role as a US territory gives it access to high-quality enforcement tools like the US federal court system.

The timely diffusion of audits with information about misuse of resources has serious electoral consequences in Mexico, Brazil, and South Africa (Larreguy, Marshall and Snyder 2020, Ferraz and Finan 2008, Berliner and Wehner 2022), suggesting that voters both trust the information and that audits have the power to act as a tool promoting local accountability. Relatedly, expected audits are highly effective in reducing bureaucratic misappropriation of expenditures for public employment in India (Wong 2021).

Audits can improve procurement in middle-capacity environments. Di Tella and Schargrodsky (2003) show that, during the first 9 months of a corruption crackdown conducted

in Argentina, the price of medical supplies paid by hospitals decreased by 15%. Similarly, Olken (2007) describes a randomized control trial on over 600 Indonesian villages that were developing a road project, where increasing the probability of government audits to 100% significantly reduced missing expenditures. However, even in the presence of an audit probability of one, the amount of unaccounted funds didn't go down to zero but remained around 20%. A potential explanation, consistent with the channels highlighted in our models, is that even with an audit probability of 100%, the punishment probability was still much lower than 100% due to enforcement capacity constraints.

Zamboni and Litschig (2018) show that, in Brazil, a temporary increase in audit risk can efficiently discourage an individual from committing fraud in public procurement, as long as authorities have the capacity to both identify and punish those committing the act. On the other hand, audits are not useful to improve the quality of health-related services for primary and preventative care, since the government was not able to correctly pin down the party responsible in that specific sector.

In Italy, a country with relatively good state capacity but a high risk of corruption, Vannutelli (2022) illustrates that increasing auditors' independence by severing their possible ties with local mayors significantly improves municipal fiscal performance.

In China, Chu, Fisman, Tan and Wang (2021) show that local auditors, as opposed to external ones, find a significantly lower number of questionable expenditures in the municipalities' budget. According to the authors, this phenomenon is a result of greater leniency of the auditor, rather than the municipality decreasing corruption because of greater enforcement. China is another interesting example fitting the story of our model, a middle-income country with a large potential for corruption but also a government with a strong enforcement capacity.

Finally, audits have proven to have positive indirect effects, that could improve governance and spending quality more broadly. Again in the context of Brazil, Lauletta, Rossi and Ruzzier (2022) show that audited municipalities reduce the size of public employment for a given level of services and reduce the share of discretionary (as opposed to exam-based) hirings.

While it seems clear that audits are an efficient policy tool in improving the management of public resources in middle-capacity countries such as Brazil, Indonesia, and even Italy, the same is not necessarily true for both high and low-capacity countries. In the US, Calvo, Cui and Serpa (2019b) presents striking evidence of how audits and project oversight can meaningfully increase the bureaucratic burden and, consequently, create additional project delays. In terms of financial performance, there also appears to be no significant difference between the financial conditions of local governments that were closely monitored and mu-

nicipalities that were not. Spreen and Cheek (2016) and Nakhmurina (2020) investigate the effectiveness of state-level fiscal monitoring, as opposed to audits, finding conflicting results.

In Chile, a relatively low-corruption, high-capacity setting, Gerardino, Litschig and Pomeranz (2017) show that procurement audits are not only ineffective, but they introduce distortions in the process, inducing a shift away from open auctions towards less competitive and less transparent procedures that are less likely to be subject to audit checks.

Finally, Ensminger and Leder-Luis (2022) study audits in a low capacity and high corruption context: development projects joint between the World Bank and the Kenyan government. The authors find high levels of impropriety in expenditure reports and discuss how a similar World Bank forensic audit failed due to the inability to respond to audit findings. This aligns well with our predictions of the effectiveness of audits, where audits are ineffective if the organization lacks the capacity to enforce bureaucratic behavior based on its findings. Notably, this paper does not causally estimate treatment effects, but rather uses statistical patterns to detect misreporting, and is therefore excluded from Table 1.

4 Auditing Local Governments in the U.S.

The United States is highly decentralized, and the US federal government transfers a significant and ever-growing amount of money to state and local governments to finance public goods and services. In 2018, federal grants accounted for over 22% of the total revenues of state and local governments, for an amount of $570 billion (Office 2020). According to the federal government, this motivates the use of audits to ensure accountability in the use and management of federal funds.

On the other hand, the United States also has high levels of state capacity. Our conceptual framework predicts audits in the US to be of low marginal value given the high-enforcement-capacity, low-abuse environment. However, the effectiveness of these audits has never been evaluated empirically.

We use a new, unexplored regression discontinuity to causally estimate the effects of city audits on local finance. Local governments that expend a total amount of federal funds above a threshold are required to perform an annual Single Audit. Single Audits were introduced in 1984 by the Office of Management and Budget to standardize and simplify audit requirements for local governments. Audits are performed by third-party auditors. This threshold has changed over time: originally, it was set at $100,000 of expended federal funds; the threshold was increased to $300,000 in 1996, to $500,000 in 2004, and the current threshold of $750,000 has been in effect since 2015 (Tassin, Waymire and Hines 2019).

The objective of these audit is to review financial statements and evaluate the quality

of internal controls and the compliance with laws, regulations, and grant provisions of each major program for which the entity receives federal support. Auditors are also charged to check specific performance goals assigned from federal grantor agencies to grantees.[6] Audit outcomes are used by the federal government to evaluate future grant awards, so that poor-performing entities risk losing future grant access. We provide further details on the specifics of the audit process in Appendix B.

The threshold limit for auditing provides a source of quasi-experimental variation that allows us to estimate the causal effects of the audit. Cities that lie just above or just below the threshold are similar in many regards, except that those just above must be audited. We employ a dynamic regression discontinuity design to examine the effects of audits on city finance outcomes over a panel of years.

4.1 Data

In order to assess the causal effect of audits, we need data on both audited and unaudited local governments, which are not easily available.[7] We rely on data from the state of New York, which publishes detailed financial statements for all of its local governments, regardless of their federal audit status.

We begin with the entirety of cities in the State of New York. Our data include a total of 1,593 local governments. We collect data on the universe of financial statements from these local governments from 2001 to 2019. The data allow us to calculate the total amount of federal revenues *received* by each local entity in a given year, a proxy for federal revenue *expended* in a given year, which triggers a single audit. Moreover, these data give us detailed information about spending and revenues for several budgetary categories, including taxes, fees, administrative expenditures, and debt service expenditures, which we use as outcome variables to measure the effects of audits on local fiscal outcomes.

4.2 First Stage

Our empirical strategy exploits the threshold requirement for Single Audits using a regression discontinuity (RD) design, comparing cities just above the threshold prescribed by the law to cities just below, across a panel of years. We first consider the cross-sectional first stage across all years to validate that the threshold causes a shift in auditing behavior.

[6]The latter was introduced by the Uniform Guidance, issued in 2013 and effective in 2015. The Guidance raised the threshold and significantly streamlined and clarified previously existing guidance for single audits, intending to focus auditors' efforts towards programs and aspects at higher risk of waste and fraud, but also shifted the focus from simple compliance checks to a more performance-oriented review.

[7]The Census annually surveys city finances, but the survey is only voluntary so coverage is incomplete.

Figure 3 shows the first stage of the discontinuity, plotting the binned probability of audits against the federal revenue a city receives in a given year. The running variable has been normalized, where 0 is the auditing threshold in a given year. Crossing the threshold is associated with a 0.21 to 0.28 increase in audit probability, depending on whether a linear or polynomial fitting is specified. Under both specifications, this estimate is statistically significant at the 5% level. Crossing the single audit threshold for federal revenue causes cities to be audited. The linear specification of the first stage selects an optimal bandwidth of about $200,000, which we use for all further analysis.

Appendix Figure A2 shows the results of the McCrary test for running variable manipulation McCrary (2008). There is no evidence of sorting across the discontinuity. This may be because cities are unable to control the sum of their federal transfers that trigger auditing, as the total includes money transferred indirectly to cities in the form of pass-through grants given first to states. The lack of sorting supports the identifying assumptions of the RD design.

4.3 Dynamic Regression Discontinuity

Identifying the causal effect of audits in our context is made challenging by the fact that governments can be audited multiple times and audits may have both contemporaneous as well as delayed impacts on outcomes. To account for these issues, we use a dynamic regression discontinuity (DRD) design, following Cellini et al. (2010).

4.3.1 Sample Construction

We construct a sample of cities that are quasi-randomly audited. We consider city-years as having a quasi-experiment if the total amount of federal revenues they received in a given year falls within $200,000 of the relevant audit threshold. Cities that always receive a high amount of federal revenue are always audited and therefore are not identified by our RD design; the same applies to cities that are always below the threshold. We use the full panel of data for cities that ever have a quasi-experiment in order to compute the full dynamic effects of the quasi-random audit. Our final sample contains 271 unique cities and over 5,000 city-years.

Appendix Figure A3 shows the geographic variation in the number of audits per city in our estimation sample. There is wide variation and good geographic coverage, providing identifying variation which we exploit.

For our outcome variables, we use budget data from local governments, including revenues and expenditures by category, like taxes, debt services, and utilities. Dollar values

are inflation-adjusted into real 2018 dollars. We winsorize the data at the 99th and 1st percentile, compute per-capita values using the local population, and take the logarithm. Appendix C describes the data cleaning process, and how the detailed city budget categories were collapsed into outcome variables.

We complement the state-generated financial statements data with federal Single Audit FAC Data. This dataset contains detailed information about all entities that have been audited, from 1997 to 2020, with all the relevant data on the audit process and its outcome. Appendix C discusses the relationship between single audit exact expenditure data and state-provided federal transfer data.

4.3.2 Dynamic RD Specification

Consider fiscal year t. Define $b_{it} = 1$ if federal awards in fiscal year t exceeded the single audit threshold, and 0 otherwise. Cellini et al. (2010) define a one-step treatment-on-the-treated (TOT) dynamic RD estimator using the following specification:

$$y_{jt} = \sum_{\tau=0}^{\overline{\tau}} (b_{j,t-\tau}\theta_{\tau}^{TOT} + m_{j,t-\tau}\alpha_{\tau} + f(z_{j,t})) + \lambda_j + \kappa_t + u_{jt} \tag{1}$$

where the variable b_{jt} indicates whether the federal revenues is above the threshold; m_{jt} is an indicator for whether or not municipality j had a quasi-experiment in year t, which in our sample means being within \$200,000 from the threshold in year t. We consider the full panel of years for all cities with any quasi-experiment. z_{jt} represents the distance in federal revenues from the threshold and $f(z_{jt})$ controls for up to the third degree polynomial. Lastly, the specification also includes controls for municipality (λ_j) and calendar year (κ_t) fixed effects.

4.3.3 Results

Table 2 presents the effects of single audits on city finance outcomes using our dynamic regression discontinuity empirical design. Each column presents a different outcome variable, a category of the city's budget, including taxes, revenues, charges for services, debt expenditure, administrative expenditure, and more. If audits were to impact city finances, we would expect the budget to reflect these changes. Variables are reported in logs, and the causal effects of auditing with standard errors are reported in each cell.

Overall, Table 2 shows reasonably precise null effects of auditing on city financial outcomes. Estimates are close to zero and nearly none are statistically significant. The precise zero estimates indicate no effect of quasi-random auditing on city financial outcomes. Importantly, our results are not an artifact of the level of aggregation chosen for our outcome

variables. In Appendix Tables A3 and A2, we perform the same regressions using more disaggregated categories of revenues and spending and we still fail to detect any economically or statistically significant effects of audits.

The null effects of auditing indicate that US cities are not an environment where bad behavior can be both detected with audits and punishment can be successfully enforced. This leaves open the question of whether audits are not valuable due to lack of detection or lack of enforcement. We believe that these audits fail to detect meaningful bad mishandling of funds, i.e. active waste. Section B describes the single audit process and findings. First, through reading the results of many audit reports, we observe that most audits produce no findings and those that do tend to focus on very minor findings like basic paperwork errors. In support of this hypothesis, Appendix Table A4 shows the frequency of words based on their appearance in the text of audit findings. Words associated with active waste (e.g. "corruption", "embezzlement", "illegal") are unlikely to appear, even in audits with negative outcomes; while words related to passive waste, ("improper," "weakness") are heavily used. With regards to enforcement capacity, impropriety with federal funds at a city level can be prosecuted by the US federal government, and therefore we do not anticipate that inability to respond to audit findings is the limiting factor.

Another natural question to ask is, if audits were highly effective, whether we would expect to observe any change in local fiscal outcomes. An ideal study would observe direct measures of abuse; however, this is impossible to observe in our setting, given that audit findings are available only for audited cities and our identification rests on the comparison of audited and non-audited cities. Relatedly, it is impossible to study the effects of the bond interest rates faced by these cities, as a measure of their riskiness as perceived by markets, because cities of this size do not regularly produce bonds that are traded.

Given the specific objectives of Single Audits in the US, we argue that, if audits detected and eliminated abuse, we would expect to observe changes in the budgetary fiscal outcomes that we study. One of the main scopes of audits is to check on whether local governments are using federal money to finance expenditures towards which those funds were earmarked. Thus, for example, audits are supposed to check whether Title I funds are used to finance school spending. Therefore, should a local government be found liable for abuse of Title I funds, for example, because it used them to finance other types of spending (e.g. administrative expenditures), we would expect to observe changes in both its education expenditures as well as its administrative expenditures.

Finally, it is important to note that our results show the effect of a marginal audit on budgetary outcomes, and do not evaluate the average effect of audits, nor the deterrence effects of having an audit law on the books. However, we believe that the value of a marginal

audit is the policy-relevant question in this context. Given the current enforcement regime, governments face a question of whether to conduct additional audits. Moreover, because the government has a limited capacity for enforcement, marginal audits come at a cost, both in terms of money spent but also on other forgone policies that may improve governance. Our framework and empirical results shed light on the value of additional audits, and provide context for when we can expect these audits to be valuable.

5 Discussion and Conclusion

Our stylized model shows how state capacity is crucial for audits to be effective, because the government needs to have credible tools to punish those who misbehave. At the same time, high-capacity environments might be also associated with a low level of underlying abuse to detect, and thus audits might not be always an efficient choice for governments in these contexts. This provides some guidance toward how we could improve policy-making decisions in both high and low-capacity states.

For low-capacity organizations, the key priority should be to establish and strengthen institutions that are a precondition for audit effectiveness. Two important features here are judicial and media independence, without which the bad behavior of bureaucrats cannot be disciplined nor their bad conduct publicized. Moreover, low-capacity states can begin by ensuring transparency in their monitoring, auditing, and governmental budgetary process, as well as adopting changes to the regulatory structure that would help avoid auditors' capture and conflicts of interest. Before these features are in place, audits are a potentially wasteful way of combating impropriety, because while they may reveal bad behavior, the state lacks the ability to do anything about it.

In contrast, our framework shows that marginal audits may not be valuable in high state capacity environments, as it is an investment in detecting bad behavior that does not necessarily exist. This understanding is key for guiding policy. Increasing audits can potentially increase passive waste, adding bureaucratic red tape but without improving actual governance.

One potential reason audits remain politically popular, despite their potential inefficiency, is that they are relatively simple tools to deal with situations in which the government interacts with private parties, and conflicts of interests may arise, as in the case of legal but distortive lobbying or regulatory capture. Empirical work from high-state-capacity governments shows that audits and enforcement can be particularly valuable when targeted toward *external* firms spending government money (Shi 2022, Leder-Luis 2019). This reinforces our finding that optimal auditing is a function of organizational capacity, even within govern-

ments. As a practical consequence, governments should exert the greatest auditing effort on the parts of their organizations where bad behavior proliferates and the tools to enforce detected bad behavior are also present.

References

Angelucci, Charles and Antonio Russo, "Petty Corruption and Citizen Reports," *International Economic Review*, 2022, *63* (2), 831–848.

Avis, Eric, Claudio Ferraz, and Frederico Finan, "Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians," *Journal of Political Economy*, 2018, *126* (5), 1912–1964.

Bandiera, Oriana, Andrea Prat, and Tommaso Valletti, "Active and passive waste in government spending: evidence from a policy experiment," *American Economic Review*, 2009, *99* (4), 1278–1308.

Banerjee, Abhijit V, "A theory of misgovernance," *The Quarterly journal of economics*, 1997, *112* (4), 1289–1332.

Bank, World, *World development report 2004: making services work for poor people*, The World Bank, 2003.

Bardhan, Pranab, "Decentralization of governance and development," *Journal of Economic perspectives*, 2002, *16* (4), 185–205.

_ **and Dilip Mookherjee**, "Decentralization, corruption and government accountability," *International handbook on the economics of corruption*, 2006, *6* (1), 161–188.

Berliner, Daniel and Joachim Wehner, "Audits for accountability: evidence from municipal by-elections in South Africa," *The Journal of Politics*, 2022, *84* (3), 000–000.

Besley, Timothy, Rohini Pande, Lupin Rahman, and Vijayendra Rao, "The politics of public good provision: Evidence from Indian local governments," *Journal of the European Economic Association*, 2004, *2* (2-3), 416–426.

Bobonis, Gustavo J, Luis R Cámara Fuertes, and Rainer Schwabe, "Monitoring corruptible politicians," *American Economic Review*, 2016, *106* (8), 2371–2405.

Bosio, Erica, Simeon Djankov, Edward Glaeser, and Andrei Shleifer, "Public Procurement in Law and Practice," *American Economic Review*, April 2022, *112* (4), 1091–1117.

Calvo, Eduard, Ruomeng Cui, and Juan Camilo Serpa, "Oversight and efficiency in public projects: A regression discontinuity analysis," *Management Science*, 2019, *65* (12), 5651–5675.

_ , _ , **and** _ , "Oversight and efficiency in public projects: A regression discontinuity analysis," *Management Science*, 2019, *65* (12), 5651–5675.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein, "The value of school facility investments: Evidence from a dynamic regression discontinuity design," *The Quarterly Journal of Economics*, 2010, *125* (1), 215–261.

Chu, Jian, Raymond Fisman, Songtao Tan, and Yongxiang Wang, "Hometown ties and the quality of government monitoring: evidence from rotation of Chinese auditors," *American Economic Journal: Applied Economics*, 2021, *13* (3), 176–201.

Ensminger, Jean and Jetson Leder-Luis, "Detecting Fraud in Development Aid," Working Paper 30768, National Bureau of Economic Research December 2022.

Ferraz, Claudio and Frederico Finan, "Exposing corrupt politicians: the effects of Brazil's publicly released audits on electoral outcomes," *The Quarterly journal of economics*, 2008, *123* (2), 703–745.

_ **and** _ , "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments," *American Economic Review*, June 2011, *101* (4), 1274–1311.

Gerardino, Maria Paula, Stephan Litschig, and Dina Pomeranz, "Distortion by Audit: Evidence from Public Procurement," Technical Report, National Bureau of Economic Research 2017.

Kelman, S., *Procurement and Public Management: The Fear of Discretion and the Quality of Government Performance* AEI studies, AEI Press, 1990.

Kelman, Steven, *Unleashing change: A study of organizational renewal in government*, Brookings Institution Press, 2005.

Khalil, Fahad, Jacques Lawarrée, and Sungho Yun, "Bribery versus extortion: allowing the lesser of two evils," *The Rand journal of economics*, 2010, *41* (1), 179–198.

Kofman, Fred and Jacques Lawarrée, "Collusion in hierarchical agency," *Econometrica: Journal of the Econometric Society*, 1993, pp. 629–656.

Kristensen, Jens Kromann, Martin Bowen, Cathal Long, Shakira Mustapha, and Urška Zrinski, *PEFA, public financial management, and good governance*, World Bank Publications, 2019.

Laffont, Jean-Jacques and David Martimort, "Collusion and delegation," *The Rand Journal of Economics*, 1998, pp. 280–305.

Larreguy, Horacio, John Marshall, and Jr. Snyder James M, "Publicising Malfeasance: When the Local Media Structure Facilitates Electoral Accountability in Mexico," *The Economic Journal*, 05 2020, *130* (631), 2291–2327.

Lauletta, Maximiliano, Martín A. Rossi, and Christian A. Ruzzier, "Audits and Government Hiring Practices," *Economica*, 2022, *89* (353), 214–227.

Leder-Luis, Jetson, "Can Whistleblowersr Root Out Public Expenditure Fraud? Evidence from Medicare," 2019.

Lira, Andres Gonzalez and Ahmed Mushfiq Mobarak, "Slippery Fish: Enforcing Regulation under Subversive Adaptation," 2019.

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of econometrics*, 2008, *142* (2), 698–714.

Montenegro, Mateo, "How Close Is Too Close When It Comes to Public Auditing?," in "in" 2020.

Mookherjee, Dilip, "Decentralization, hierarchies, and incentives: A mechanism design perspective," *Journal of Economic Literature*, 2006, *44* (2), 367–390.

_ **and Ivan Paak-Liang Png**, "Corruptible law enforcers: how should they be compensated?," *The Economic Journal*, 1995, *105* (428), 145–159.

Nakhmurina, Anya, "Does fiscal monitoring make better governments? Evidence from US municipalities," *Evidence from US Municipalities (April 22, 2020)*, 2020.

Office, United States Government Accountability, "Intergovernmental Issues: Key Trends and Issues Regarding State and Local Sector Finances," Technical Report 2020.

Olken, Benjamin A, "Monitoring corruption: evidence from a field experiment in Indonesia," *Journal of political Economy*, 2007, *115* (2), 200–249.

Ortner, Juan and Sylvain Chassang, "Making corruption harder: Asymmetric information, collusion, and crime," *Journal of Political Economy*, 2018, *126* (5), 2108–2133.

Pande, Rohini, "Can informed voters enforce better governance? Experiments in low-income democracies," *Annu. Rev. Econ.*, 2011, *3* (1), 215–237.

Press, Jordan, "Auditor general Michael Ferguson breaks down $23M cost of Senate audit," *CBC News*, 2015.

Shi, Maggie, "Monitoring for Waste: Evidence from Medicare Audits," 2022.

Shleifer, Andrei and Robert W Vishny, "Corruption," *The quarterly journal of economics*, 1993, *108* (3), 599–617.

Spreen, Thomas Luke and Caitlin M Cheek, "Does monitoring local government fiscal conditions affect outcomes? Evidence from Michigan," *Public Finance Review*, 2016, *44* (6), 722–745.

Tassin, Kerri L., Tammy R. Waymire, and Christopher S. Hines, "A Historical Evaluation of the Single Audit: Thirty Years from Initial Legislation to Uniform Guidance," *Journal of governmental & nonprofit accounting*, 2019, *8* (1), 21–35.

Tella, Rafael Di and Ernesto Schargrodsky, "The role of wages and auditing during a crackdown on corruption in the city of Buenos Aires," *The Journal of Law and Economics*, 2003, *46* (1), 269–292.

Tirole, Jean, "Hierarchies and bureaucracies: On the role of collusion in organizations," *The Journal of Law, Economics, and Organization*, 1986, *2* (2), 181–214.

Vannutelli, Silvia, "From Lapdogs to Watchdogs: Random Auditor Assignment and Municipal Fiscal Performance," Working Paper 30644, National Bureau of Economic Research November 2022.

Wong, Wendy, "Optimal Monitoring and Bureaucrat Adjustments," Technical Report, working paper 2021.

Zamboni, Yves and Stephan Litschig, "Audit risk and rent extraction: Evidence from a randomized evaluation in Brazil," *Journal of Development Economics*, 2018, *134*, 133–149.

Tables and Figures

Table 1
Survey of Studies on Government Audits

Article	Journal	Country	Treatment	Outcome Variable	Point Estimate	Dependent Variable Mean	% Change
Di Tella and Schargrodsky (2003)	Journal of Law & Economics	Argentina	Audits	Hospital prices (log)	−0.146	Not reported	15%
Avis et al. (2018)	Journal of Political Economy	Brazil	Random audits	Number of irregularities (logs)	−0.06	Not reported	−1.33%
Ferraz and Finan (2008)	The Quarterly Journal of Economics	Brazil	Timing of audits	Incumbent likelihood's of reelection	−0.03	Not reported	−9%
Ferraz and Finan (2011)	The American Economic Review	Brazil	Term Limit of Mayors	Corruption's detected by Audits	−0.019	0.06	−30.2%
Lauletta et al. (2022)	Economica	Brazil	Random audits	Discretionary employment	−1.445	12.24	−11.8%
Zamboni and Litschig (2018)	Journal of Development Economics	Brazil	Increase in audit risk	Audited funds in procurement	−0.118	Not reported	Not reported
Gerardino et al. (2017)	Working paper	Chile	Public sector audits	Usage of auctions in procurement	−0.079	0.67	−11.9%
Chu et al. (2021)	American Economic Journal: Applied Economics	China	Auditor's local ties	Suspicious expenditures	0.471	11.7	4%
Wong (2021)	Working Paper	India	Timing of random audits	Misappropriated expenditure	−-28.06	187.1	−15%
Olken (2007)	Journal of Political Economy	Indonesia	Random audits	Missing expenditure	−0.085	0.25	−36.8%
Vannutelli (2021)	Working paper	Italy	Change in auditing policy	Budget management	15.88	−306.97	−5.2%
Larreguy et al. (2020)	The Economic Journal	Mexico	Timely diffusion of audits	Incumbent mayors vote share	0.02	−0.035	−62.47%
Bobonis et al. (2016)	American Economic Review	Puerto Rico	Timing of audits	Number of corruption violation	−1.43	1.38	103.5%
Berliner and Wehner (2021)	Journal of Politics	South Africa	Timing of audits	Incumbent mayor's vote share	0.071	0.655	10.8%
Calvo,Cui and Serpa (2016)	Management Science	United States	Federal law requiring audits	Projects delays	0.59	Not reported	6.1%

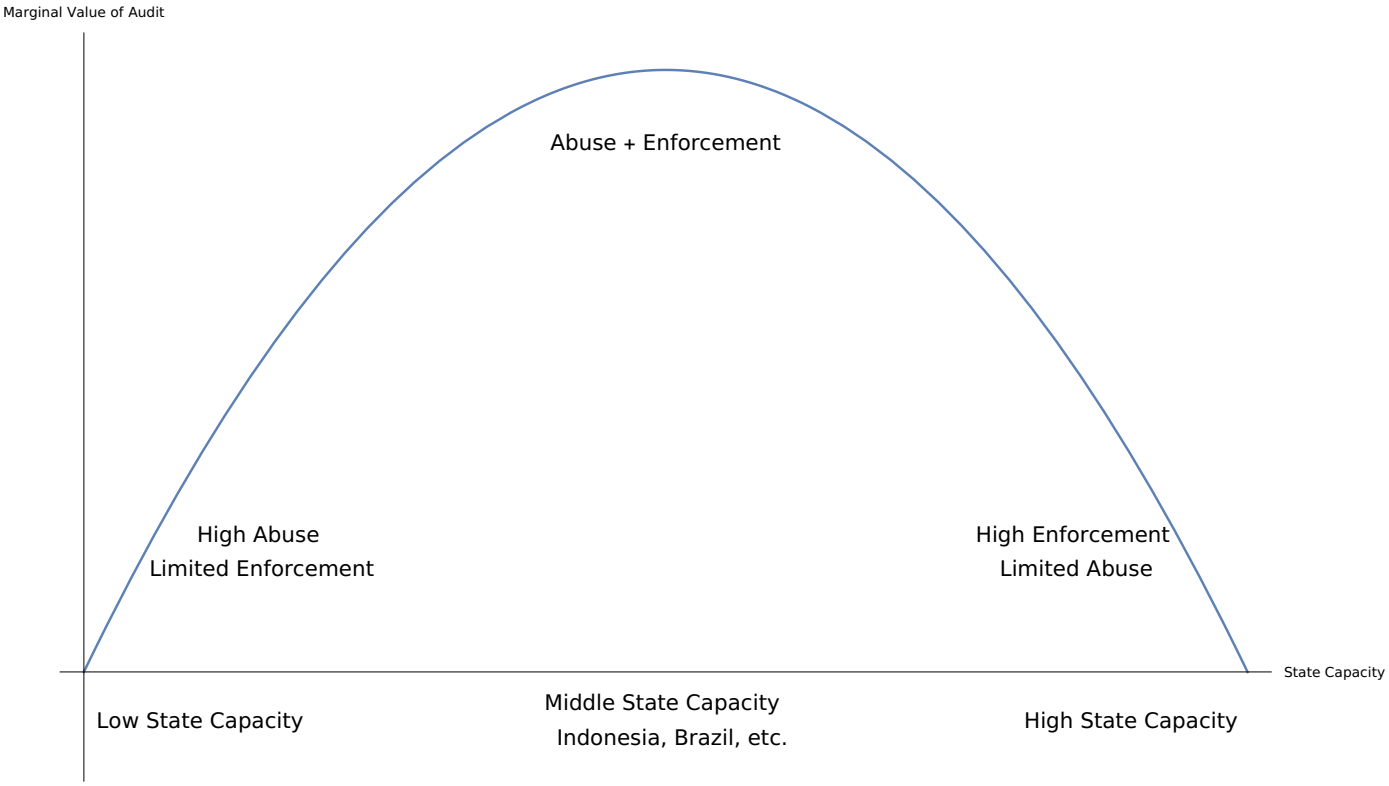
Notes: This table presents a survey of economics papers that causally identify the effects of government audits on a variety of outcomes. The main outcome of each paper is presented. Point estimates are scaled by the dependent variable mean to produce a percent change and allow an easier comparison across outcomes.

Table 2
THE IMPACT OF US SINGLE AUDITS ON CITY FISCAL OUTCOMES: DYNAMIC REGRESSION DISCONTINUITY ESTIMATES

Outcomes	(1) Taxes	(2) Charges for services	(3) Total revenue	(4) Administrative expenditure	(5) Culture and education	(6) Debt services	(7) Employment	(8) General services	(9) Utilities
Same Year	-0.0166	0.0252	-0.000666	-0.0461	0.121	0.0483	-0.0124	0.00636	0.124
	(0.0179)	(0.0524)	(0.0234)	(0.0378)	(0.0928)	(0.116)	(0.0802)	(0.0481)	(0.138)
+ 1 Year	-0.0140	0.0137	0.0198	0.000101	-0.0810	-0.0341	-0.0441	0.0614	-0.0921
	(0.0188)	(0.0562)	(0.0200)	(0.0324)	(0.0737)	(0.121)	(0.0736)	(0.0458)	(0.127)
+ 2 Year	-0.0121	0.0432	0.0355	-0.0254	-0.0267	-0.0660	0.0170	0.000867	-0.105
	(0.0200)	(0.0571)	(0.0259)	(0.0393)	(0.0682)	(0.117)	(0.0692)	(0.0512)	(0.148)
+ 3 Year	-0.0141	0.0474	-0.0161	-0.0128	0.0450	-0.0168	-0.00796	-0.000972	-0.219
	(0.0211)	(0.0636)	(0.0236)	(0.0424)	(0.0755)	(0.111)	(0.0747)	(0.0548)	(0.143)
+ 4 Year	-0.0135	-0.0247	-0.0255	-0.00361	0.0340	-0.0949	-0.00942	-0.123	-0.332
	(0.0237)	(0.0625)	(0.0251)	(0.0438)	(0.0754)	(0.112)	(0.0689)	(0.0539)	(0.170)
+ 5 Year	-0.0228	-0.0264	-0.0234	0.0405	0.0839	-0.160	-0.0807	-0.111	-0.330
	(0.0255)	(0.0649)	(0.0280)	(0.0543)	(0.0956)	(0.135)	(0.0771)	(0.0535)	(0.165)
+ 6 Year	-0.0160	-0.0500	-0.0218	-0.00787	0.0868	-0.206	-0.0116	-0.181+	-0.191
	(0.0259)	(0.0673)	(0.0282)	(0.0499)	(0.0954)	(0.169)	(0.0781)	(0.0648)	(0.167)
Observations	5,149	5,149	5,149	5,149	5,149	5,149	5,149	5,149	5,149
R^2	0.980	0.948	0.964	0.923	0.902	0.791	0.911	0.852	0.813
Y mean	960.6	369.6	1607	450.3	164.6	174.8	100.5	933.6	197.5

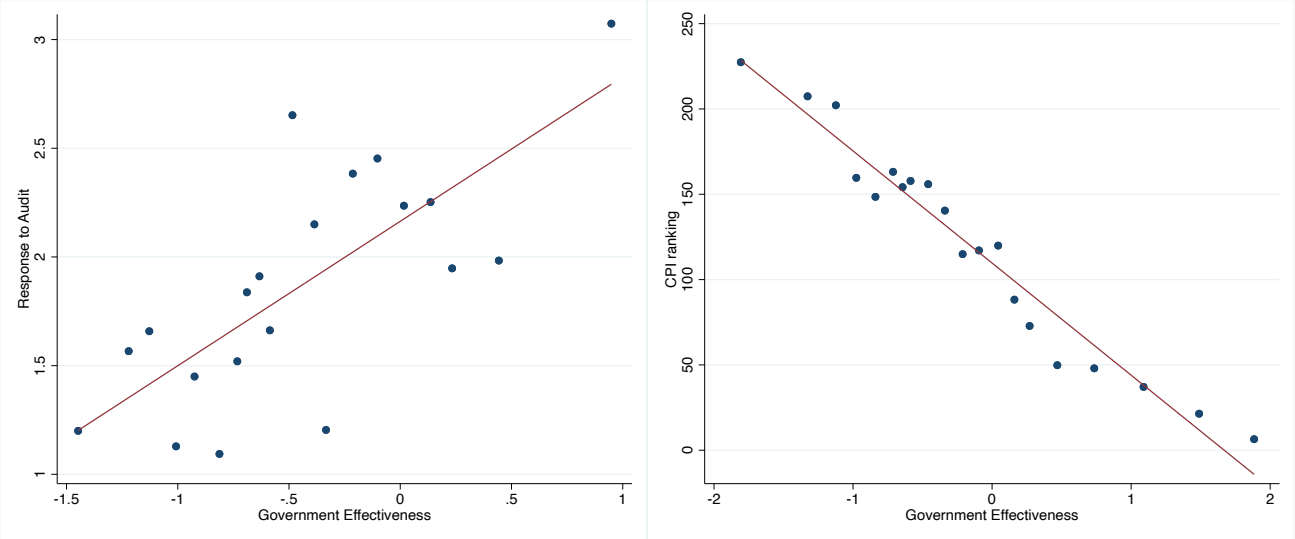
Notes: This table presents dynamic regression discontinuity estimates of the effects of single audits on city fiscal outcomes. The sample includes a balanced panel of all cities that, at any point in time in our sample period, have received a total amount of federal revenues within $200,000 from the Single Audits threshold. The specification corresponds to equation (1). The coefficients reported correspond to the θ_{τ}^{TOT}, where τ represents the relative time elapsed since a given audit event. We include all possible values of τ in our estimation, but for simplicity, we only report coefficients for values of τ from 1 to 6, though we verify that none of the others are significant. Given the high number of coefficients and outcomes, we use a Bonferroni correction and set the significance thresholds at 0.01 (marked +) and 0.005 (marked *).

Figure 1
Audit Value as a Function of State Capacity



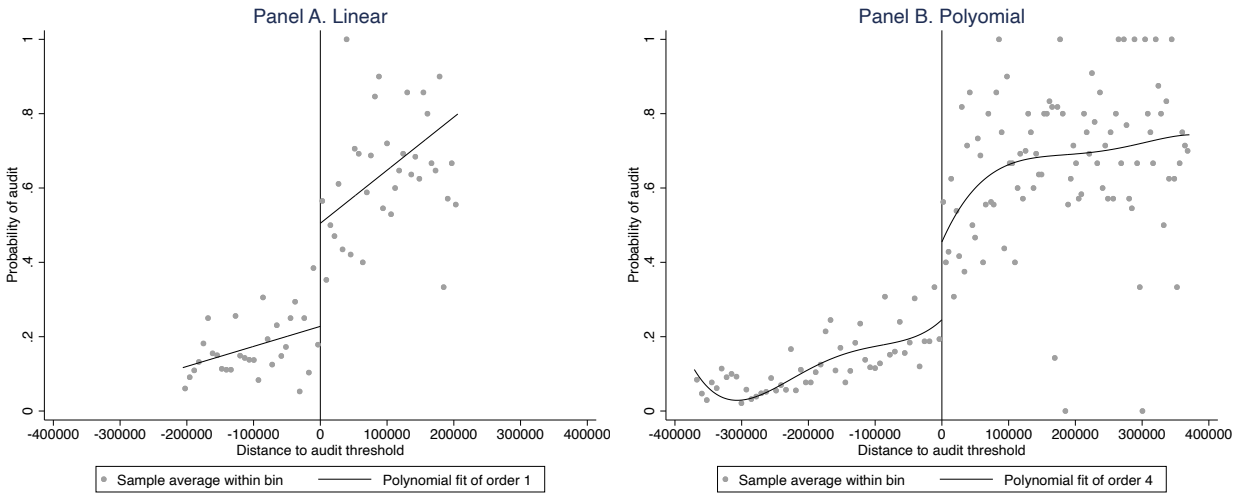
Notes: This figure shows our model's prediction about how the marginal value of an audit varies with state capacity. According to our model, the value of an audit depends on both the level of abuse, which is decreasing in state capacity, and the ability to enforce against bad behavior, which is increasing in state capacity. Therefore, audits are most valuable in middle-state-capacity contexts. Appendix A.1 discusses how this value compares to costs.

Figure 2
State Capacity, Corruption and Audit Responsiveness



Notes: The figure presents evidence in line with our conceptual framework, which predicts that corruption is decreasing in state capacity and that the ability to respond to audits is increasing in state capacity. We combine data from three sources: state capacity measures from Bosio et al. (2022) on the X axis, the ability to respond to an internal audit, as measured by Kristensen et al. (2019) on the Y axis of the left panel, and the corruption ranking from the 2021 Transparency International Corruption Perceptions Index on the Y axis of the right panel. In line with our predictions, the ability to respond to audit findings is increasing in state capacity, while corruption is decreasing in state capacity.

Figure 3
First Stage Effect of Passing the Federal Revenues' Threshold on Single Audits' Probability



Notes: This figure presents the first stage estimate of the discontinuous effect of federal revenue on auditing, using the full sample of city-years from New York State. The x-axis value of 0 corresponds to the threshold above which cities are required to undertake a Single Audit. There are 29,048 observations. The coefficient for Panel A is 0.28 with a p-value less than 0.001. The coefficient for Panel B is 0.21 with a p-value of 0.029.

Appendix

A Theory Extensions

A.1 Allowing for Varying Costs

The framework presented in Section 2 shows that the marginal benefit of audits is a downward parabola in state capacity, without discussing costs. This combination indicates that audits have the highest value for middling state capacity countries. In this appendix, we discuss the relationship to costs.

The model's condition for audit benefits to exceed costs is given by:

$$p_c(s)p_e(s)k > a(s)$$

Where $a(s)$ indicates that costs can vary in state capacity.

It is unclear *a priori* whether audit costs are constant, increasing or decreasing in state capacity, or of some other functional form. Largely, it does not matter.

Appendix Figure A1 shows the relationship between various cost functions and the value function presented by our theoretical model. If costs are constant in state capacity, then middle-income countries are the ones for which audit value exceeds costs, for any constant cost as long as it does not exceed the maximum of the parabola. Next, consider costs that are increasing in state capacity, of the form $a(s) = \alpha + \beta * s$ with $\beta > 0$. Then, as long as the line intersects the parabola in two places – that is, it is not steeper than the parabola, nor tangent to it – it still holds that audits are optimal in some middle range of state capacity. Similarly, consider costs that are decreasing in state capacity, of the form $a(s) = \alpha + \beta * s$ with $\beta < 0$. Then, once again, as long as the line intersects the parabola in two places – that is, it is not steeper than the parabola, nor tangent to it – it still holds that audits are optimal in some middle range of state capacity. Finally, the monotonicity of costs in state capacity is not necessary for the condition to hold that audits are optimal for middle-state-capacity countries. Consider, instead, if costs are parabolic in state capacity; then, for any parabola less steep than the marginal value parabola, there is still a central region of state capacity where audit values exceed costs.

There is a condition where middle-state-capacity countries are not the ones that should audit under this model. It is only where audit costs are somehow of a steeper parabola than audit value – or some non-monotonic functional form, such as a sinusoid – that audit values do not exceed costs in some central regions. There is no evidence or motivation for this cost

structure.

The framework presented in the paper is useful in conveying intuition about the value of audits, more so than the costs; because the value of audits depends both on detecting bad behavior *and* being able to enforce against it, the value of audits are highest for middle ranges of state capacity. The examples in this appendix further demonstrate that the particular functional form of costs may determine which exact countries should audit, but does not eliminate the aforementioned intuition.

A.2 Allowing for Politicians' Risk-Aversion

The conceptual framework presented in Section 2 highlights the tradeoffs for marginal audit decisions abstracting from the consequences of not auditing. However, in reality, politicians might face an additional trade-off when deciding whether or not to audit. This is related to the risk of scandal occurrence Z. The probability of scandal occurrence $p_z(s)$ is increasing with state capacity. This is reasonable if one thinks that scandals are more likely to emerge in the presence of free and resourceful media, which are more likely to exist in high-state capacity countries.

If the government decides not to audit, then she cannot tell apart whether or not the agent is engaging in bad behavior. In this case, the government maximizes the following expected payoff:

$$p_c(s)\left[-\pi_C - p_z(s)Z\right] + (1 - p_c(s)) * 0$$

Thus the government faces an additional cost from not auditing, that is the cost of the scandal occurrence.

When deciding whether or not to audit, the government thus equates this expected cost to the expected benefits of the audit, and therefore the relevant inequality now becomes:

$$-a(s) + p_c(s) \times [-\pi_C + p_e(s)k] > p_c(s) \times [-\pi_C - p_z(s)Z]$$

Which happens whenever:

$$p_c(s)p_e(s)k + p_c(s)p_z(s)Z > a(s)$$

Because the politician now wants to avoid the potential additional cost of the scandal if the bureaucrat is corrupt and she decided not to audit, audits will occur more often than they would have in the baseline model without scandals.

B Description of the Single Audits Process

Non-federal entities are required by the Single Audit Act Amendments of 1996 and OMB Circular A-133 to have an annual audit of their federal awards.

A single audit consists of (1) an audit and opinions on the fair presentation of the financial statements and the schedule of expenditures of Federal Awards; (2) gaining an understanding of and testing internal control over financial reporting and the entity's compliance with laws, regulations, and contract or grant provisions that have a direct and material effect on certain federal programs (i.e., the program requirements); and (3) an audit and an opinion on compliance with applicable program requirements for certain federal programs.

Before the introduction of the Single Audit legislation, financial audits of recipients of government grants were carried out grant-by-grant, an ineffective procedure that allowed gaps in audit coverage. The single audit concept was created in 1984 as a response, calling for an entity-wide, thorough audit that included a review of the grant recipient's internal controls, as opposed to a detailed audit of each award.

The single audit adheres to Generally Accepted Government Auditing Standards for evaluating internal controls (GAGAS, also commonly referred to as the Yellow Book). The auditor also needs to determine so-called "major programs", that is federal programs from which a given entity receives money above a certain threshold. Compliance with grant provisions needs to be verified for each major program. An auditee that performs poorly could lose access to grant money in the future (Tassin et al. 2019).

From 1997 onwards, the federal government instituted the Federal Audit Clearinghouse, where all single audit reports for all entities can be freely downloaded. The FAC was introduced with the objective of enhancing transparency and providing stakeholders with access to potentially useful information to hold auditees accountable.

Entities are allowed to choose any Certified Public Accountant to perform the Audit, who needs to complete and submit the audit within nine months of the end of the audit period. Auditors can provide four different types of opinions:

- Unqualified opinion – auditee complied, in all material respects, with the requirements of the major program.
- Qualified opinion – auditee complied, except for certain instances of material noncompliance, in all material respects with the requirements of the major program.
- Adverse opinion – auditee did not comply, in all material respects, with the requirements of the major program.

- Disclaimer of opinion – the scope of the auditor's work is insufficient to express an opinion on the auditee's compliance with the requirements applicable to the major program.

The opinions are based on the findings of the audit. Findings can be broadly organized into the following categories: a) financial statement findings, b) federal award findings, c) deficiencies in internal control and d) instances of material noncompliance.

C Single Audits and Local Public Finance Data

We use data from New York State on city finances, as well as from the Federal Audit Clearinghouse on city audits. This appendix describes the data used.

The dynamic regression discontinuity analysis starts with the city finance data. We produce broad expenditure and revenue categories to limit the number of left-hand side variables. Table A1 shows the list of variables and how they are aggregated. For example, the Culture and Education category includes expenditures listed by the state under the categories: "cultural services, library, recreation services, instruction, community college, and miscellaneous education." The aggregation is based on the account code and narrative, which can be found in the Account and Reporting Manual published by the Office of the New York State Comptroller.

The Federal Audit Clearinghouse provides a source of data to validate the city finance outcomes among audited cities. Technically, the city data provides us with dollars received, while the audit threshold is based on dollars expended. For the sample of audited entities, we can inspect whether the total amount of federal revenues received in a given year from the state data represents a good proxy for the total amount of federal revenues expended in a given year for the audit. Figure A4 displays a binned scatterplot of the relationship between these two variables, using our main estimation sample for the dynamic regression discontinuity. There is a very strong correlation between the two across all ranges of federal revenue.

Appendix Tables and Figures

Table A1
Budget Categories

Variables	Category	Description
Administrative Expenditure	Expenditure	General government costs, employee insurance, related expenditures and employee administration expenditures
Charges for service	Revenue	Community services, culture and recreation, public safety sanitation, social services and transportation fees
Culture and education	Expenditure	Cultural services, library, recreation services, instruction, community college and miscellaneous education
Debt services	Expenditure	Debt principal and interests on debt
Employment	Expenditure	Exmployee benefits (retirement, life insurance, unclassified benefits), social services
General services	Expenditure	Community services, health, public safety, sanitation, social services, transportation
State revenue	Revenue	State aid in community services, economic development health, utilities
Taxes	Revenue	City income tax, property tax, special assessments, sales and use tax
Utilities	Expenditures	Economic development, general government zoning and planning, water utilities
Total revenue	Revenue	All revenue excluding federal aid

Table A2
The Impact of US Single Audits on City Spending, Disaggregated Measures, Dynamic Regression Discontinuity Estimates

Variables	Same Year	Year +1	Year +2	Year +3	Year +4	Year +5	Year +6	Observations	Y Mean
Administrative Expenditure	0.057	0.028	0.002	0.034	0.058	0.046	0.066	5681	5.045
	(0.034)	(0.034)	(0.034)	(0.035)	(0.036)	(0.037)	(0.047)		
Constituent Services	-0.013	-0.031	-0.078	0.016	0.122	0.087	0.138	3428	1.015
	(0.097)	(0.098)	(0.122)	(0.105)	(0.112)	(0.118)	(0.117)		
Cultural Services	0.051	0.080	0.022	0.061	0.022	0.039	-0.024	5187	1.202
	(0.078)	(0.075)	(0.086)	(0.082)	(0.09)	(0.092)	(0.096)		
Debt Principal	0.185	0.101	0.161	0.003	-0.024	-0.025	-0.032	5410	4.737
	(0.122)	(0.103)	(0.091)	(0.104)	(0.106)	(0.107)	(0.152)		
Development Infrastructure	0.193	0.411	0.432	0.160	0.043	0.993	0.288	1787	3.354
	(0.304)	(0.349)	(0.314)	(0.341)	(0.349)	(0.422)	(0.378)		
Disability Insurance	-0.007	-0.042	0.016	-0.010	0.014	0.075	-0.022	3409	0.406
	(0.046)	(0.047)	(0.051)	(0.044)	(0.052)	(0.059)	(0.061)		
Drainage	-0.140	-0.262	-0.273	-0.760	-0.009	-0.159	-0.032	1223	1.913
	(0.298)	(0.42)	(0.459)	(0.413)	(0.359)	(0.414)	(0.443)		
Economic Development Administration	0.446	0.395	0.167	-0.016	-0.553	-0.333	0.336	1561	2.323
	(0.271)	(0.274)	(0.272)	(0.303)	(0.344)	(0.33)	(0.347)		
Elderly Services	0.038	0.014	0.155	0.089	0.099	0.157	0.086	2524	1.492
	(0.088)	(0.08)	(0.114)	(0.109)	(0.099)	(0.099)	(0.102)		
Emergency Response	0.168	0.051	-0.118	0.030	-0.228	0.277	-0.100	2279	2.769
	(0.193)	(0.238)	(0.22)	(0.211)	(0.309)	(0.324)	(0.329)		
Fire Protection	-0.002	0.082	-0.042	0.068	0.039	0.045	-0.063	4557	4.509
	(0.105)	(0.082)	(0.087)	(0.096)	(0.11)	(0.1)	(0.12)		
Highways	0.002	-0.062	0.028	0.053	-0.024	-0.035	0.017	5653	5.715
	(0.055)	(0.061)	(0.063)	(0.055)	(0.052)	(0.059)	(0.063)		
Debt Interest	0.119	0.068	0.023	-0.065	0.038	0.131	0.224	5319	3.607
	(0.085)	(0.082)	(0.087)	(0.095)	(0.105)	(0.119)	(0.134)		
Judgments	-0.384	-0.228	-0.041	-0.199	0.071	-0.241	-0.246	2422	1.600
	(0.269)	(0.251)	(0.265)	(0.228)	(0.267)	(0.291)	(0.316)		
Library	0.114	-0.068	0.048	0.042	-0.060	0.046	0.108	2881	2.790
	(0.137)	(0.091)	(0.106)	(0.11)	(0.103)	(0.108)	(0.11)		
Life Insurance	0.006	-0.025	0.036	0.096	0.090	0.032	-0.043	1172	0.734
	(0.083)	(0.077)	(0.089)	(0.106)	(0.156)	(0.149)	(0.139)		
Medical Insurance	-0.070	-0.021	0.000	-0.002	-0.038	-0.016	-0.013	5479	4.874
	(0.049)	(0.045)	(0.048)	(0.05)	(0.052)	(0.056)	(0.061)		
Natural Resources	0.249	0.115	0.232	-0.017	0.218	0.190	-0.091	3044	1.715
	(0.136)	(0.139)	(0.15)	(0.138)	(0.143)	(0.151)	(0.192)		
Operations	-0.057	-0.084	0.087	0.078	-0.024	-0.014	0.086	5678	4.228
	(0.064)	(0.062)	(0.068)	(0.075)	(0.068)	(0.083)	(0.08)		
Police	0.079	0.039	0.051	-0.009	-0.074	-0.045	-0.047	4967	4.040
	(0.046)	(0.041)	(0.048)	(0.063)	(0.053)	(0.059)	(0.058)		
Promotion	0.094	0.072	0.055	0.142	0.118	0.140	0.268	2098	1.108
	(0.135)	(0.14)	(0.127)	(0.11)	(0.142)	(0.137)	(0.192)		
Public Health Adm	-0.009	0.049	0.105	0.072	0.078	0.090	0.112	3879	0.573
	(0.05)	(0.048)	(0.07)	(0.051)	(0.059)	(0.076)	(0.085)		
Safety Administration	-0.227	-0.216	-0.603	-0.312	-0.019	0.155	0.300	1291	2.418
	(0.218)	(0.284)	(0.315)	(0.272)	(0.236)	(0.244)	(0.231)		
Recreation Services	0.249	0.097	0.197	0.168	0.023	0.122	0.131	5114	3.678
	(0.11)	(0.093)	(0.095)	(0.084)	(0.09)	(0.114)	(0.098)		
Refuse and Garbage	0.078	0.103	0.142	0.102	0.120	0.137	0.201	4491	3.565
	(0.087)	(0.1)	(0.104)	(0.105)	(0.112)	(0.161)	(0.197)		
Retirement Police	0.056	0.003	0.153	-0.052	0.029	0.017	-0.005	2187	3.474
	(0.164)	(0.169)	(0.174)	(0.183)	(0.176)	(0.179)	(0.211)		
Retirement State	-0.047	-0.082	0.035	-0.011	0.093	0.087	0.117	5379	3.590
	(0.083)	(0.091)	(0.113)	(0.112)	(0.091)	(0.103)	(0.09)		
Sewer	0.211	0.121	0.105	0.103	0.039	0.060	0.188	4324	4.755
	(0.132)	(0.128)	(0.136)	(0.125)	(0.132)	(0.136)	(0.144)		
Social Security	-0.070	-0.083	-0.053	-0.058	-0.070	-0.071	-0.060	5388	3.755
	(0.039)	(0.044)	(0.039)	(0.039)	(0.046)	(0.046)	(0.058)		
Storm Sewer	-0.008	-0.071	-0.070	0.010	-0.666	0.318	-0.120	1219	2.099
	(0.302)	(0.389)	(0.353)	(0.553)	(0.562)	(0.57)	(0.653)		
Transport Facility	-0.177	0.000	0.052	0.130	-0.044	0.027	0.202	3195	4.152
	(0.133)	(0.133)	(0.135)	(0.115)	(0.128)	(0.134)	(0.174)		
Unclassified Employee benefit	-0.120	-0.020	0.151	-0.086	-0.153	-0.170	0.033	2961	2.825
	(0.229)	(0.226)	(0.229)	(0.251)	(0.23)	(0.247)	(0.252)		
Unemployment Insurance	0.002	0.085	-0.086	0.072	0.013	0.096	-0.026	4097	0.807
	(0.074)	(0.096)	(0.09)	(0.1)	(0.085)	(0.107)	(0.102)		
Water	0.300	0.238	0.052	0.134	0.106	0.097	0.102	4611	4.894
	(0.152)	(0.13)	(0.163)	(0.177)	(0.181)	(0.197)	(0.216)		
Workers Compensation	-0.191	-0.113	-0.248	-0.214	-0.232	-0.141	-0.093	4401	3.276
	(0.087)	(0.098)	(0.115)	(0.121)	(0.113)	(0.126)	(0.13)		
Youth Recreation	-0.033	0.014	0.001	0.002	-0.076	-0.080	-0.115	4466	2.293
	(0.09)	(0.106)	(0.103)	(0.108)	(0.111)	(0.13)	(0.137)		
Zoning and Planning	0.032	-0.049	-0.055	-0.068	-0.077	-0.061	0.006	5164	2.099
	(0.089)	(0.111)	(0.115)	(0.1)	(0.108)	(0.106)	(0.128)		

Notes: This table presents dynamic regression discontinuity estimates of the effects of single audits on city spending, providing more disaggregated measures of the categories that are combined in Table 2. The sample includes a balanced panel of all cities that, at any point in time in our sample period, have received a total amount of federal revenues within $200,000 from the Single Audits threshold. The specification corresponds to equation (1). The coefficients reported in the various columns correspond to the θ_{τ}^{TOT}, where τ represents the relative time elapsed since a given audit event. We include all possible values of τ in our estimation, but for simplicity, we only report coefficients for values of τ from 1 to 6, though we verify that none of the others are significant. Given the high number of coefficients and outcomes, we use a Bonferroni correction and set the significance thresholds at 0.01 (marked +) and 0.005 (marked *).

Table A3
The Impact of US Single Audits on City Revenues, Disaggregated Measures, Dynamic Regression Discontinuity Estimates

Variables	Same Year	Year +1	Year +2	Year +3	Year +4	Year +5	Year +6	Observations	Y Mean
Community Services Fees	-0.052	-0.075	-0.129	-0.079	-0.145	-0.035	-0.100	5453	2.457
	(0.082)	(0.086)	(0.085)	(0.084)	(0.094)	(0.103)	(0.113)		
Compensation for Loss	-0.163	-0.206	-0.070	-0.046	-0.109	0.038	-0.058	4407	1.656
	(0.169)	(0.159)	(0.182)	(0.155)	(0.188)	(0.202)	(0.211)		
Culture and Recreation Charges	-0.058	0.032	0.089	0.201	-0.182	-0.135	-0.093	1203	1.872
	(0.173)	(0.177)	(0.184)	(0.214)	(0.219)	(0.266)	(0.196)		
Culture and Recreation Fees	0.125	0.173	0.176	0.031	0.144	0.098	0.054	3648	2.846
	(0.109)	(0.108)	(0.103)	(0.111)	(0.112)	(0.116)	(0.13)		
Economic Development Fees	-0.294	0.017	-0.013	0.104	-0.064	0.143	0.341	1315	2.572
	(0.414)	(0.347)	(0.394)	(0.399)	(0.342)	(0.363)	(0.425)		
Fines	-0.002	0.071	0.012	0.009	0.049	0.016	0.125	5419	3.091
	(0.079)	(0.086)	(0.088)	(0.097)	(0.1)	(0.104)	(0.101)		
Franchises	-0.033	-0.025	-0.027	-0.018	0.129	0.131	0.052	4492	2.903
	(0.064)	(0.061)	(0.068)	(0.065)	(0.057)	(0.092)	(0.098)		
General Government Charges	0.016	0.068	0.143	0.140	0.094	0.235	0.164	1147	1.283
	(0.17)	(0.164)	(0.169)	(0.18)	(0.171)	(0.186)	(0.221)		
General Government Fees	-0.029	-0.006	0.064	0.013	-0.011	0.114	0.150	5580	1.523
	(0.077)	(0.094)	(0.079)	(0.085)	(0.104)	(0.121)	(0.146)		
Gifts	0.064	0.157	0.105	-0.074	-0.126	-0.195	-0.080	3752	1.484
	(0.177)	(0.189)	(0.187)	(0.161)	(0.179)	(0.198)	(0.22)		
Health Fees	0.267	0.277	0.232	0.225	0.368	0.193	0.186	2767	1.283
	(0.133)	(0.135)	(0.166)	(0.148)	(0.199)	(0.207)	(0.221)		
Interest Penalties	-0.040	-0.117	-0.032	-0.097	-0.095	-0.070	-0.120	5436	2.074
	(0.055)	(0.052)	(0.047)	(0.055)	(0.051)	(0.058)	(0.057)		
Interest Earnings	-0.098	-0.087	-0.005	-0.006	0.173	0.205	0.246	5654	2.348
	(0.12)	(0.128)	(0.121)	(0.129)	(0.124)	(0.127)	(0.146)		
Mortgage Tax	-0.074	-0.025	-0.062	-0.070	-0.025	-0.037	-0.009	5655	3.436
	(0.047)	(0.058)	(0.051)	(0.057)	(0.056)	(0.061)	(0.058)		
Payments in Lieu of Taxes	0.032	-0.145	-0.165	0.021	-0.076	-0.164	-0.099	4280	2.431
	(0.153)	(0.159)	(0.176)	(0.167)	(0.19)	(0.187)	(0.22)		
Public Safety Charges	0.128	0.081	0.270	0.122	0.093	-0.060	0.002	2446	2.937
	(0.155)	(0.169)	(0.158)	(0.133)	(0.141)	(0.2)	(0.19)		
Public Safety Fees	0.103	0.050	0.131	0.111	0.032	0.043	-0.103	3783	1.622
	(0.121)	(0.126)	(0.12)	(0.125)	(0.127)	(0.139)	(0.156)		
Real Property Taxes	0.003	-0.005	0.010	0.006	0.008	0.016	-0.006	5681	6.698
	(0.026)	(0.025)	(0.029)	(0.029)	(0.03)	(0.032)	(0.036)		
Rental of Property	0.077	0.080	0.056	0.052	-0.003	-0.014	0.011	4281	2.251
	(0.095)	(0.096)	(0.105)	(0.104)	(0.131)	(0.126)	(0.143)		
Sale of Property	0.054	-0.066	-0.111	-0.234	-0.155	0.153	0.271	5222	1.857
	(0.164)	(0.155)	(0.164)	(0.219)	(0.203)	(0.195)	(0.226)		
Sales Tax	0.008	-0.006	-0.002	-0.004	0.025	0.006	0.008	4206	5.172
	(0.052)	(0.049)	(0.045)	(0.051)	(0.057)	(0.058)	(0.059)		
Sanitation Charges	-0.140	0.177	-0.078	0.065	0.075	0.394	0.126	1310	2.515
	(0.276)	(0.204)	(0.244)	(0.177)	(0.253)	(0.167)	(0.239)		
Sanitation Fees	0.122	0.065	0.152	0.082	0.028	-0.026	0.051	4502	4.489
	(0.117)	(0.13)	(0.121)	(0.195)	(0.171)	(0.182)	(0.184)		
Special Assessments	0.068	0.218	0.125	-0.146	-0.315	-0.364	-0.069	1624	2.797
	(0.203)	(0.213)	(0.229)	(0.189)	(0.205)	(0.21)	(0.24)		
State Aid - Community Services	-0.735	-0.470	-0.739	-0.347	-0.479	-0.118	-0.125	1657	1.540
	(0.368)	(0.409)	(0.442)	(0.41)	(0.402)	(0.471)	(0.512)		
State Aid - Culture and Recreation	-0.058	-0.057	0.025	0.210	0.053	-0.013	-0.194	4022	1.109
	(0.147)	(0.146)	(0.154)	(0.148)	(0.159)	(0.189)	(0.196)		
State Aid - General Government	-0.225	-0.073	-0.226	-0.029	-0.189	-0.046	0.277	2598	1.160
	(0.17)	(0.168)	(0.173)	(0.199)	(0.22)	(0.215)	(0.273)		
State Aid - Public Safety	-0.178	-0.426	-0.183	-0.351	0.084	0.266	-0.067	2800	1.767
	(0.26)	(0.236)	(0.25)	(0.281)	(0.291)	(0.319)	(0.361)		
State Aid - Transportation	0.091	-0.042	-0.191	0.073	-0.194	-0.026	0.231	5339	3.695
	(0.098)	(0.105)	(0.137)	(0.119)	(0.124)	(0.184)	(0.134)		
Transportation Charges	-0.077	0.045	0.015	0.080	0.098	0.123	0.467	3181	2.664
	(0.133)	(0.109)	(0.117)	(0.121)	(0.152)	(0.143)	(0.194)		
Transportation Fees	0.149	-0.008	0.051	0.221	0.189	0.148	0.149	1677	3.001
	(0.125)	(0.125)	(0.112)	(0.163)	(0.204)	(0.232)	(0.205)		
Unrestricted State Aid	0.042	0.019	0.010	0.074	0.061	0.105	0.151	5534	3.054
	(0.058)	(0.057)	(0.056)	(0.057)	(0.085)	(0.093)	(0.11)		
Utilities Gross Receipt Tax	0.026	-0.012	-0.027	-0.010	-0.089	-0.094	-0.080	2276	3.492
	(0.066)	(0.068)	(0.064)	(0.066)	(0.071)	(0.083)	(0.085)		
Utility Fees	-0.073	0.054	-0.029	-0.050	-0.079	-0.087	-0.071	4180	5.194
	(0.074)	(0.082)	(0.091)	(0.093)	(0.085)	(0.087)	(0.091)		

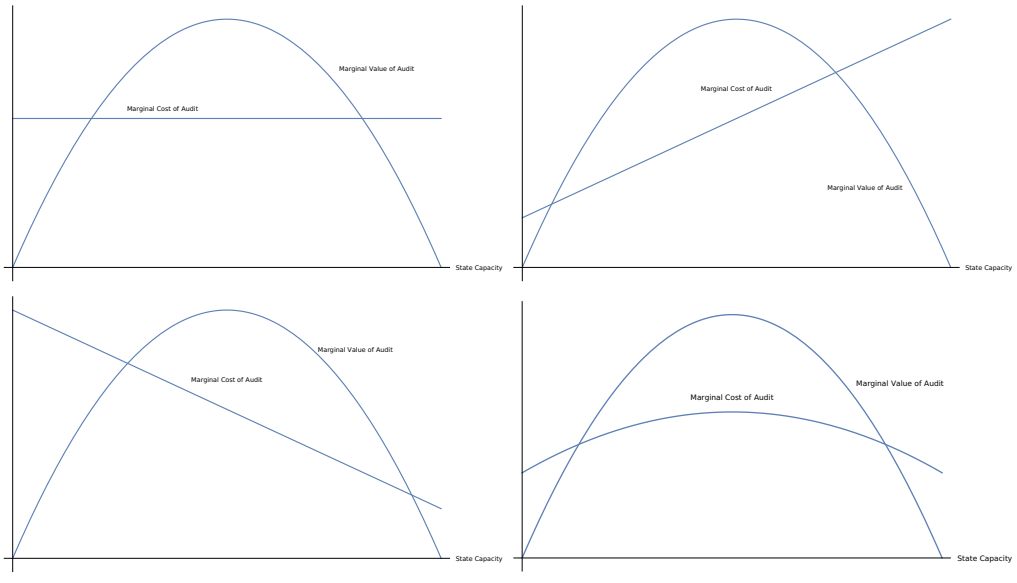
Notes: This table presents dynamic regression discontinuity estimates of the effects of single audits on city revenues, providing more disaggregated measures of the categories that are combined in Table 2. The sample includes a balanced panel of all cities that, at any point in time in our sample period, have received a total amount of federal revenues within $200,000 from the Single Audits threshold. The specification corresponds to equation (1). The coefficients reported in the various columns correspond to the θ_{τ}^{TOT}, where τ represents the relative time elapsed since a given audit event. We include all possible values of τ in our estimation, but for simplicity, we only report coefficients for values of τ from 1 to 6, though we verify that none of the others are significant. Given the high number of coefficients and outcomes, we use a Bonferroni correction and set the significance thresholds at 0.01 (marked +) and 0.005 (marked *).

Table A4
Active and Passive Waste Words in Audit Findings

Audit Finding	Panel A. Share of Active Waste Terms					
	Embezzlement	Corruption	Illegal	Stolen	Misappropriated	N
Significant deficiency	0	0	0	0.001	0.002	1463
Material weakness	0	0	0	0.002	0	1191
Material non compliance	0	0	0	0.002	0.002	507
	Panel B. Share of Passive Waste Terms					
	Waste	Not allowed	Improper	Deficient	Weakness	N
Significant deficiency	0.01	0.04	0.06	0.67	0.53	1463
Material weakness	0.04	0.04	0.06	0.25	0.77	1191
Material non compliance	0.03	0.05	0.06	0.25	0.56	507

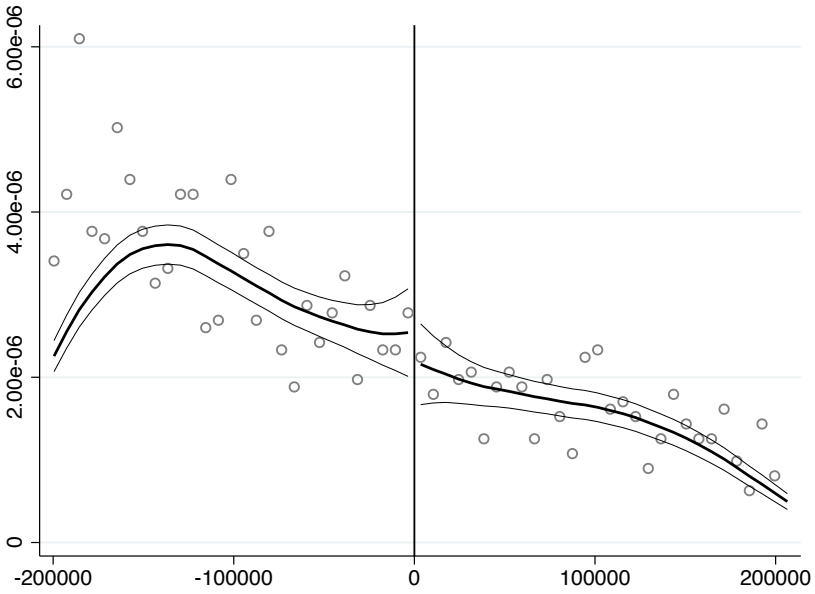
Notes: This table shows the use of active waste (Panel A) and passive waste (Panel B) terms in the text of audit findings, by the type of finding. The fraction shows the share of documents that contain the word at least once. US single audits are unlikely to detect active waste, and much more likely to detect passive waste.

Figure A1
Audit Costs and State Capacity



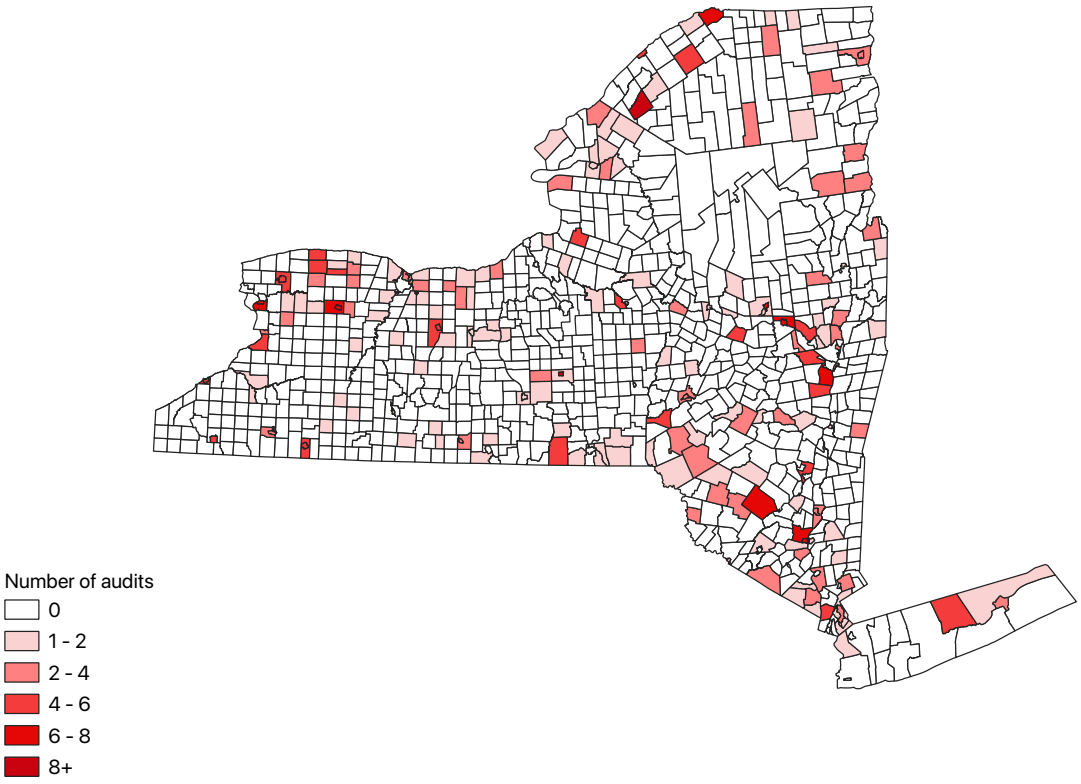
Notes: This figure shows the value of audits compared to their costs, with different cost functions that are either constant (top-left), increasing (top-right), decreasing (bottom-left), or parabolic (bottom-right) in state capacity. Our main theoretical results are robust to allowing for various functional forms of audit costs.

Figure A2
McCrary Test for the First Stage



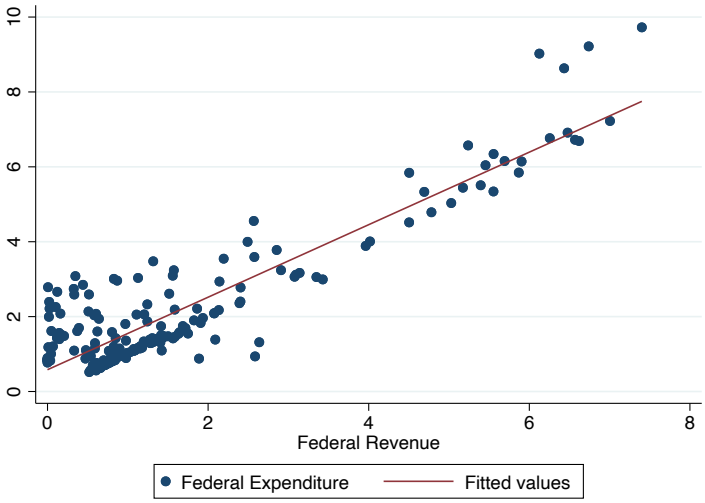
Notes: This figure presents the McCrary test of the first stage, using all city-years within $200,000 of the audit threshold. The x-axis value of 0 corresponds to the threshold above which cities are required to undertake a Single Audit. We fail to reject that the densities are the same on the two sides of the cut-off. The estimated discontinuity is equal to -.125, with a standard error of .173.

Figure A3
Geographic Variation in Audits



Notes: This figure shows a map of the cities included in our dynamic regression discontinuity estimation sample. We report the total number of audits per city in our full sample period. Darker colors correspond to higher numbers.

Figure A4
Correlation of Total Amount of Federal Revenues Received and Expended - Audited Cities



Notes: This figure shows the correlation between the total amount of federal revenues received and expended in a given year and the fitted values using the coefficients from the regression of Federal Expenditure on Federal Revenue. The two numbers are available only for audited cities, as the total amount of federal revenues expended is the key variable determining single audit assignment and it is reported by audited cities as part of the single audit reports.