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GOVERNMENT PERFORMANCE, TAXATION AND CITIZEN PARTICIPATION: EVIDENCE FROM BRAZIL

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DESEMPEÑO GUBERNAMENTAL, IMPUESTOS Y PARTICIPACIÓN CIUDADANA: EVIDENCIA DE BRASIL

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RESUMEN

Las teorías de contratos fiscales del Estado afirman que el desempeño del gobierno afecta la recolección de impuestos y que las instituciones que fomentan la representación y transparencia relacionan los impuestos y los servicios. Estas proposiciones todavía no han sido probadas utilizando un diseño de investigación causal y data desagregada. En esta investigación usamos un diseño de investigación cuasi-experimental con auditorías aleatorias en Brasil para evaluar si la corrupción revelada y otras métricas de desempeño gubernamental afectan la recolección de impuesto sobre la propiedad. Encontramos que la corrupción revelada robustamente reduce los ingresos por este tipo de impuesto, mientras que el desempeño revelado en muchas otras dimensiones no. También encontramos que mientras que la estructura de las instituciones fiscales (presupuestos participativos *vs*. presupuestos definidos a través de representantes electos) no tiene efecto sobre la cantidad de corrupción revelada o la calidad de desempeño. La corrupción revelada aumenta la probabilidad de que un municipio adopte en el futuro un sistema de presupuesto participativo.

Palabras clave: impuestos, corrupción, participación ciudadana, Brasil

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ABSTRACT

Fiscal contract theories of the state hypothesize that government performance affects tax collection and that institutions that foster representation and accountability link taxes and services. These propositions have yet to be tested with a causal research design and disaggregated data. In this paper, we use a quasi-experimental research design with randomized audits from Brazil to assess whether revealed corruption and other metrics of government performance affect municipal property tax collection. We find that revealed corruption robustly reduces property tax revenue; revealed performance on many other dimensions does not. We also find that while the structure of fiscal institutions (participatory budgeting versus budgeting via elected representatives) has no effect on the amount of corruption revealed or the quality of performance, revealed corruption increases the probability that a municipality will adopt participatory budgeting in the future.

Keywords: taxation, corruption, citizen participation, Brazil

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Government performance, taxation and citizen participation: evidence from Brazil*

Abstract

Fiscal contract theories of the state hypothesize that government performance affects tax collection and that institutions that foster representation and accountability link taxes and services. These propositions have yet to be tested with a causal research design and disaggregated data. In this paper, we use a quasi-experimental research design with randomized audits from Brazil to assess whether revealed corruption and other metrics of government performance affect municipal property tax collection. We find that revealed corruption robustly reduces property tax revenue; revealed performance on many other dimensions does not. We also find that while the structure of fiscal institutions (participatory budgeting versus budgeting via elected representatives) has no effect on the amount of corruption revealed or the quality of performance, revealed corruption increases the probability that a municipality will adopt participatory budgeting in the future.

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Fiscal contract theories of the state hypothesize that government performance affects tax collection and that institutions that foster representation and accountability link taxes and services. This paper uses a quasi-experimental research design with randomized auditing reports from the Brazilian federal government to test fiscal contract-like hypotheses about these relationships. These reports, described by Ferraz and Finan (2008), detail the way in which municipalities spent federal transfers—were projects completed, were there overpayments, were contracts awarded without bids, did politicians pocket the money, etc.? We use the content of these reports to shed light on three questions: whether revealed corruption/government performance has an effect on local tax collection; whether municipalities that use participatory budgeting have lower revealed corruption and/or higher performance; and whether the content of these audits affects the probability that a municipality will adopt participatory budgeting in the future.

The primary contribution of this paper is to use an experimental design with relatively disaggregated real world data, allowing for relatively high quality inferences with respect to the aforementioned questions. Specifically, because the audits are random, we are able to identify the effect of exogenous and objective information about government corruption and performance on tax collection and on demands for accountability. If fiscal contract theories of the state were correct, we would at a minimum expect to find that revealed performance affects local taxation; it might also affect accountability in terms of the form of fiscal institutions. Furthermore, we are able to see whether the dosage (low corruption to high corruption) is systematically related to the institutions governing spending.

We have answers to these questions based on an initial sample of approximately 334 audits, all conducted in 2004.[1] With difference-in-difference estimators that compare audited municipalities with each other and audited municipalities with non-audited municipalities, we find that revealed government corruption has a negative effect on local property tax collection (but not on overall local tax revenue). The primary results are robust with several measures of corruption and taxation, and with controls for other forms of revenue (notably transfers), GDP per capita and the fixed-characteristics of municipalities. Placebo treatments of audit reports with prior property tax collection indicate no systematic differences, suggesting that the content of the audit reports has a causal effect only on subsequent property tax collection. The magnitude of the effect would not be trivial for the multitude of municipalities that collect limited amounts of revenue from the property tax, but it is small in terms of overall revenue and expenditure—too small to explain more than a small fraction of the substantial differences in tax levels found within and between countries. Most measures of government performance have the expected sign (-) on property tax collection, but are generally insignificant.

Surprisingly, we do not find compelling evidence that participatory budgeting (PB) affects the level of revealed corruption or the level of government performance. Because we are using observational data and our participatory budget sample is small (N=9-16),[2] not too much should be read into these results. More interestingly, perhaps, we find that the contents of the audit

[1] When we started the analysis (July 25, 2011), we had reliably coded 360 of the 400 audits conducted in 2004. Our treated sample drops to around 331-334 because we are missing some covariates.

[2] Only 9 municipalities with participatory budgeting were audited in 2004. We can expand the PB sample to as many as 16 observations if we use any municipality that has ever had PB.

(specifically an increase in revealed corruption) positively affect the probability that a municipality will adopt participatory budgeting. The magnitude of the effect is quite large, suggesting that revealed corruption engenders demands for accountability. Placebo treatments indicate that just being audited does not affect the probability of PB adoption.

This report is structured as follows. Section I presents a somewhat longer than normal literature review (albeit one that is still incomplete) in which we lay out some of the existing hypotheses and empirics. Section II describes our research design and data. Section III presents the results. Section IV provides a discussion. Section V concludes.

Section I: Background

The various relationships between government performance, fiscal structure, citizen control and revenue collection have been the subject of considerable inquiry, dating at least back to Montesquieu (1748). One of the most important ideas is that states operate like fiscal contracts, exchanging services for revenue (North 1981; Levi 1988; Timmons 2005).

While there is considerable agreement about the general fiscal contract concept, there is no unified theory or compelling (or comprehensive) formal model; virtually all researchers use marginally different definitions, build on somewhat different assumptions and test their ideas with different (and not necessarily) comparable data. Furthermore, there are a number of theoretical ambiguities associated with the concept and mechanisms—some of which we highlight below.

In general, fiscal contract theories rest on the premise that states are not endowed with sufficient coercive power to impose their will on society. Instead, governments must bargain with citizens for revenue. The more governments ask citizens to pay in taxes, the more that citizens will expect from government. Furthermore, fiscal contract theories posit that at least some tax compliance is quasi-voluntary (Levi 1988). That is, part of the reason people pay taxes is because they believe government is doing useful things with their money (proxied by trust in government and/or by actual evaluations of government expenditure) and/or because they believe they are receiving an adequate and fair share of government benefits relative to the revenue they contribute. Absent those beliefs, citizens will raise the cost of enforcing compliance.[3] Hence, holding the amount of tax effort constant, states that better satisfy taxpayer preferences will raise more tax revenue.

Fiscal contract theories typically assume that governments and citizens have similar objective functions across units. Whereas government officials typically maximize revenue subject to the constraint of staying in office, citizens are generally thought to maximize consumption, subject to the constraint that governments can provide some services for lower tax prices than citizens could purchase them in the market; that is, for the concept of the fiscal contract to make sense, governments must have a comparative advantage in producing some services. Furthermore,

[3] One ambiguity worth noting is the definition of compliance. Most people working in the fiscal contract tradition equate compliance with the amount of money a taxpayer ultimately cedes to the government, regardless of the statutory rates in effect. Hence, a taxpayer who pays only half of his (flat) tax rate of 50% would be seen as more compliant than a taxpayer who pays all of his (flat) 10% tax rate. This interpretation follows from the assumption (next paragraph in text) that governments maximize revenue; if they could set a higher rate, they would.

governments have similar abilities (state capacity) to impose taxes and collect revenue across units, while citizens have similar capacities to resist taxation. To the extent there are differences in objective functions and capacities, empirical studies sometimes try to absorb these differences with control variables (such as a proxy for state capacity, a lagged dependent variable, or fixed effects). More sophisticated theoretical variants may allow for within country/unit variance in preferences over goods/services. That is, (groups of) citizens within societies have different preferences and states have different tax instruments, allowing states to carve out different bargains with different social groups (North 1981). Other variants may allow the cost of collection to vary across tax bases/groups, based on the elasticity of the tax base. In the Bates and Lien model (1981), for example, capital is relatively mobile, allowing it to carve out the most favorable bargain.

Fiscal contract theories predict equilibrium relationships between taxes and services, but only partially explain why bargains vary across units in levels and structures and have only partially identified the mechanisms that consummate such bargains. That is, why some units seem to be on a virtuous cycle in which high levels of services accompany high tax yields and high citizen compliance, while other units seem to be on a vicious cycle in which low services accompany low tax yields and low citizen compliance is a puzzle that has not yet been fully explained.

Four general mechanisms, in particular, have been highlighted as potential devices that move units up or down the contractual line: external threats/war, voting, political parties and labor market organizations. War and external threats shorten leader time horizons and increase immediate needs for revenue (without money today, there may be no kingdom tomorrow), giving leaders incentives to grant citizens more control over government decision-making, including public finances. Voting, political parties and labor market organizations are all mechanisms of representation/accountability. Giving (sets of) taxpayers more control over government actors and government expenditures should increase accountability and improve government performance. As a result, those with representation will accept higher taxes on themselves. Of course, these mechanisms can fail if citizens are unable to select, monitor and sanction government officials–things that may be related to the specific details of institutional design and to the information available to citizens. By contrast, windfall revenues, say in the form of oil or foreign aid, are thought to reduce governments need to bargain for revenue, thereby diminishing government fiscal and political accountability (Moore 1998).

In addition to the aforementioned list of general contracting devices, one could add participatory budgeting, along the lines used in Brazil. Participatory budgeting is a process in which citizens directly negotiate spending priorities with each other and with government officials in organized meetings. Because these meetings are widely publicized, the amounts are fairly substantial, and the decisions relatively binding, participatory budgeting should increase citizen knowledge of and control over taxes and spending relative to other frameworks (e.g., via elected representatives). Increased citizen control over resource allocation should decrease government corruption, improve government performance and increase citizen satisfaction with government spending, thereby increasing tax revenue. Nevertheless, because the fraction of the population that participates in such meetings is typically small and not necessarily representative, it has also been argued that participatory budgeting can be captured by government officials and by local elites, generating outcomes that are no better than, and potentially worse than, those resulting from the decisions of elected representatives (Wampler 2010).

Two final issues deserve mention. First, while information is a critical variable, with few exceptions (noted below), theorists have not generally modeled these informational issues and empirical work has largely relegated them to the error term. For the most part, fiscal contract theories assume that people have good, but imperfect, information about their tax burden, performance and corruption. In theory, informational shortcomings should generate disconnects between the actual level of services/corruption and the actual amounts paid by citizens. Anything that changes people's priors about government performance should affect revenue. When governments performs well, better information should increase compliance/tax revenue; when governments perform poorly, better information should reduce compliance/tax revenue.

Second, fiscal contract theories are typically modeled as individual citizens (or groups of citizens) bargaining with the state; with few exceptions (e.g., Levi 1998), other citizens (or groups of citizens) are treated as actors off-stage. Nevertheless, it is generally acknowledged (but not well-conceptualized or deeply theorized) that individual compliance decisions also hinge on expectations and beliefs about what other non-state actors are doing. That is, the belief that an individual's tax burden and benefits are fair may have absolute and relative components.

Empirical evidence

Fiscal contract approaches to understanding the state can find rich support in labs and considerable support in real world data. Laboratory experiments, for example, consistently show that the belief that tax revenues are spent on things citizen's value is an important factor shaping individual compliance decisions (Andreoni, Erard, and Feinstein 1998; Cummings, Martinez-Vazquez, McKee and Torgler 2006). To take just one example: in an experimental study, Alm, McClelland, and Schulze (1992) found that increasing the amount that individuals receive from a public good substantially increases average compliance rates; that some individuals will pay for services even when there are no sanctions; and that others will refuse to comply when services are non-existent, even when the probability of punishment is high.

Likewise, one can find public opinion data consistent with fiscal contract approaches. In a study of West Germany and the United States, Slemrod (2002), for example, found that individuals who had a higher trust in government were far less likely to find tax evasion acceptable behavior, a finding that Levy, Sachs and Tyler (2009) have extended to Africa. Similarly, Bowler and Donovan (1995) use data from the United States to show that individual opinions about taxes are directly related to both government performance and actual tax burdens.

Furthermore, it is not too hard to find cross-country relationships between measures of corruption and tax revenue, between government performance and tax revenue, and between perceptions of fairness/tax morale and tax collection. Ghura (1998), for example, uses panel data with instrumental variables to show that perceived government corruption decreases tax revenue in Africa, while Richardson (2006) shows that tax morale and fairness help explain differences in cross-country rates of tax evasion. Meanwhile, as Lindert (2005), Kato (2002) and Beramendi and Cusack (2006) have pointed out, there is a fairly strong overlap between the distribution of the tax burden and the distribution of government services within the OECD. In particular, countries with larger welfare states rely heavily on relative regressive taxes and labor, taxing intensely the relatively intense users of services. Timmons (2010b) has presented evidence that

political parties and labor market institutions serve as the contractual devices in this particular case. When partisan turnover is relatively low, indicating a dominant party, more Left-wing influence in government increases the equilibrium level of revenue from consumption taxes, while more Right-wing influence increases revenue from corporate taxes; furthermore, increased labor market institutional strength (proxied by union density and centralized bargaining) raises the equilibrium level of revenue from labor taxes.

There is also general evidence in favor of the contractual mechanisms mentioned above. The empirical connection between war, taxation (and the extension of the franchise) is quite strong in traditional OECD countries (Tilly 1990), though less compelling elsewhere. (For contrasting views on Latin America, see Centeno 2002 and Thies 2005). Likewise, extant evidence also points to positive relationships between democratic representation/citizen control over public finance and government performance and taxation, both generally and specifically with respect to participatory budgeting in Brazil. To take some examples at the macro-level: although democratization only seems to increase tax revenue on the margin, more democratic countries have higher tax/gdp ratios than non-democracies and, in the long-run, raise more revenue from progressive taxes on income (Timmons 2010a). They also provide higher levels of services by many metrics (Lake and Baum 2001). Furthermore, within democracies, voter turnout also seems to be robustly correlated with taxes and services.

The three-way macro-level correlation can find support at lower levels of aggregation. In an experimental study with data from Indonesia, Olken (2010), for example, finds that direct participation in political decision making (via plebecites as opposed to representative democracy) can substantially increase satisfaction with spending outcomes, the perceived legitimacy of the government and citizens' avowed willingness to contribute to public goods (more through labor than money). Finally, Boulding and Wampler (2010) use observational data to show that participatory budgeting is associated with a shift in the composition of spending, generating increases in the share and level devoted to health and education. Zamboni (2007) has the closest paper to our own in this respect. She uses the method of least difference to match 5 municipalities with participatory budgeting against 5 relatively similar municipalities without participatory budgeting to compare the level of reported corruption (using the same audits). With eyeball tests, she concludes that PB lowers the level of corruption and improves performance.

While the general fiscal contract literature is fairly rich, there is very little theory or evidence that is Latin America specific. For the most part, it is believed that most of Latin America is in the low-performance, low-compliance/revenue equilibrium; Brazil is a notable exception, something that might be attributed to earmarking, which accounts for a sizable share of total government revenue. The exact reasons for the generally low level of taxation and tax compliance in Latin America are unknown. Some (Bergman 2003) largely attribute it to poor tax administration. Others (e.g., Alm and Martinez-Vasquez 2007) largely attribute it to the absence of trust in government, particularly the widespread belief (reality?) that governments steal/waste money. Still others (Breceda, Rigolini and Saavedra 2008) have highlighted the discrepancy between the distribution of the tax burden and the distribution of benefits. In other words, despite an expanding volume of research on these questions, whether fiscal contracts exist and how these contracts work in Latin America is still relatively unknown.

Even though correlations in the data are fairly well established, fiscal contract research has not reached the level of rigor or sophistication that one finds in some other areas of social science. The laboratory and public opinion studies may not be externally valid, as it may be easier for people to part with play money or say they will part with money when real money is not at stake. Likewise, most non-laboratory studies use observational data, often at high-levels of aggregation, in which they do not fully address the thorny issues of omitted variable bias and endogenenity. That is, because many variables are not randomly assigned, we still not know with much precision whether more tax revenue causes better performance or whether better performance increases tax revenue, despite a wealth of data. Furthermore, because many details have yet to be nailed down with strong causal tests, we do not know with a high degree of reliability whether states can really be thought of as overlapping fiscal contracts in which different packages of services accompany different tax packages. The biggest missing links are at the micro-level. For example, we, have almost no non-laboratory evidence showing that an individual will pay more/resist less when he believes government is doing more for him, something that is virtually impossible to assess without access to confidential tax data. We also lack micro-level data showing that different people pay more/less based on their preferences for different packages of services.

Section II: Data and Research Design

In this part of the paper, we focus on explaining the research design and data that allows us to test four hypotheses consistent with fiscal contract reasoning. Before we set up those hypotheses, we discuss the data and research design.

To combat corruption, the Brazilian federal government has randomly audited sub-national expenditures associated with federal transfers. Between 2003 and 2008, when our data end, 1461 audits had been conducted. These audits contain detailed substantive and procedural information about the manner in which funds were spent. They explicitly identify corruption, theft and other improper expenditure; they also identify violations in the procedural rules governing expenditure and record-keeping. The content of these audits (50-150+ pages in some cases) is then posted on the internet and distributed to journalists, with hopes that it will provide citizens with objective information about the performance of their local officials.

The virtue of these audits from the standpoint of researchers is that the treatment is random: literally, all municipalities with populations less than 450,000 inhabitants have (nearly) the same probability of being audited. Because of this randomization, our point estimates comparing audited with non-audited municipalities should be unbiased as long as the rest of the model is properly specified. The virtue of these audits from the standpoint of citizens is that they are a relatively objective source of information. That is, not only are the monthly selections of the 50-60 municipalities to be audited conducted in front of a live audience that includes media representatives, but there seems to be no political bias in either the type or number of projects chosen to be audited, nor in the conduct of the auditors—well-paid and trained professionals from the federal comptroller's office (Controladoria Geral da Uniao (CGU)) (see Ferraz and Finan for a longer description).

Sample

Our current sample includes 331-34 audits. All of the audits were conducted in 2004, the year with the largest number of audits, though some of the reports were released in early 2005. Our sample is fairly representative of the country as a whole: we currently have all of the audits from Acre, Sao Paulo, Rio Grande do Sul, Mata Grosso do Sul, Minas Gerais, Paraná, Pernambuco, Piuai, Bahia, Rio Grande do Norte, Rondonia, Tocantins, Amazonas, Maranao, Mato Grosso, and Amapa. We have most of the audits from Rio de Janeiro, Santa Catarina, Goiás, Espírito Santo, Ceará, Pará and Paraíba, and a fraction from Sergipe. We do not yet have audits from Roraima and Alagoas. Of our sample, 235 were released before the first round of municipal elections in 2004; 96 were released after the first- round.

There are several potential sources of selection bias. The first threat is that the audits themselves are not random. We believe we can reject that hypothesis. In their analysis, Ferraz and Finan convincingly show that the reports are random. Besides detailing the ex-ante and ex-post procedures for audits, Ferraz and Finan use tests of means across a multitude of variables to show that there are no systematic differences between treated and non-treated groups.

The second threat is that our sub-sample of audits is not random. Our subsample (described below) largely appears to be random as well: tests of means on audited/non-audited groups reveal no differences across most variables used in the analysis, including the principle right-hand side variable, municipal property tax collection. There are two differences that are worth noting. The municipalities in our subsample have a slightly higher average population than non-audited municipalities, they receive slightly less in transfers per capita and they collect slightly more revenue from the municipal service tax (the ISSQN). We can adjust our sample to make it appear as-if random by restricting the sample to municipal populations greater than 7,500. Because that adjustment costs us approximately 75 coded observations, we do not make it our benchmark; instead, we control for population and transfers. Nevertheless, it is worth noting that with the adjusted sample, most of the reported results are robust. Table 1 shows tests of means across a variety of fiscal and demographic variables prior to the treatment, including the primary covariates used in the analysis with the full sample.[4]

The third source of selection bias could be missing values. That is, although we coded 360 municipalities, a few (approximately 30) municipalities are missing data for one or more of the other variables for one or more years. Tests of means for the primary variables (property taxes, GDP per capita, population, procurement fraud, corruption in amounts, etc.) indicate no differences between those without and without data.

Coding the reports

To minimize bias, we have relied primarily on one coder, who is not involved in the analysis of the data. Based on the Ferraz and Finan categories and some additional guidelines from us, he created a series of variables that reflect different types of corruption and different dimensions of administrative performance. Whereas Ferraz and Finan focus primarily on what appear to be relatively clear-cut cases of corruption, we also instructed our coder to create categories for anything that could be corruption and/or that could indicate poor administration/performance.

Because our general classification scheme is somewhat broader than that used by Ferraz and Finan, the raw counts and, especially, the financial amounts are higher in our data, particularly

[4] Tests of means and results with the adjusted sample can be made available.

with the aggregated categories. Presumably, our primary count variables and our quantity variables should be nearly identical to Ferraz and Finan, except for differences in the sample (they restrict themselves to municipalities with first term mayors). As a percent of effective number of audits, our figures are roughly comparable to Ferraz and Finan.

Our primary measures are described briefly below. A complete set of variables with longer definitions, examples and a description of the coding can be found in Appendix I. Table 1A presents summary statistics.

The three primary and most obvious corruption "technologies" (identified by Ferraz and Finan) are diversion of funds (labeled B in Table 1B), over-invoicing for goods and services (labeled C in Table 1B) and irregularities in the procurement process (labeled D in Table 1B)—all of which seem fairly common at the sub-national level in Brazil. This last category can be further disaggregated into three components: failure to meet the required minimum number of bids (labeled E), failure to execute the bidding process altogether (labeled F or "no contract bids"), and direct evidence of fraud in the procurement processes (labeled G).

Like Ferraz and Finan, we consider all events/cases that fit these descriptions as corruption. In the empirical analysis, we use an aggregate measure of corruption (labeled Total Acts of Corruption, A) as well as the disaggregated measures. The aggregate measures have considerably larger samples, while the disaggregated categories allow us to pinpoint with more precision the specific types of acts that engender responses in terms of tax collection. We make "diversion of funds," "direct evidence of fraud in the procurement processes," and "no-contract" bids our primary disaggregated measures because we think they are the most obvious and unambiguous measures of corruption. They are also the largest from a financial standpoint.

Some other cases/events were not necessarily clear-cut cases of corruption/malfeasance. Rather than include these events as part of the primary measures, we created two borderline categories: spending of public resources for purposes other than those specified in the transfer agreement with the federal government, and irregular or unexecuted budget. The former might not be corruption because the funds might have been used for other legitimate, perhaps even more pressing, public purposes. The latter might not be corruption as governments may have legitimate reasons for halting a project and not spending the associated revenue. Our broadest aggregate category (Total Acts of Corruption and Potential Corruption) includes these borderline events.

All variables are normalized in count form relative to the effective number of audits conducted. The number of effective audits is equal to (or lower than) the actual number of audits conducted because the same event may have multiple audits in different dimensions. To avoid double-counting, we only classified an event once (based on the gravest offense), even if there was some evidence of multiple types of corruption/poor administration. We also use the quantity of "missing" money relative to population and relative to the total amount audited. The count variable is cleaner, in that there are no missing observations. That is, the auditor either found a specific type of problem, yielding a value of one for a particular category, or did not find a specific problem, yielding a value of zero. One downside of using counts is that they do not take into account the magnitude of the event (in effect, treating every event as being equal, when

there is heterogeneity in the data); it is also not obvious to us that counts resonate as strongly with citizens as quantity variables. That is, saying an administration stole 1M reais seems more damning than saying the auditor found x acts of corruption. Hence, we also create quantity variables for each category based on the stated amount missing, stolen, not-subject to bidding, etc. We normalize these in per capita terms (our benchmark) and in terms of total amount audited. Because the amounts are not normally distributed, we use the square root, categorical variables for different monetary increments and the log. The quantity variables take into account magnitude and, hence, we believe are probably more likely to engender a response by citizens. The downside of the quantity variable is that auditors sometimes found corruption (a one on the count variable), but did not quantify it, meaning that not all zero's in the quantity columns are in fact zero. In the regressions presented, we adjusted the zeros by arbitrarily assigning one-half the lowest revealed amount to municipalities with positive counts and no monetary values.. As part of the sensitivity analysis, we also assigned the sample mean amount to those with positive counts and no monetary values.. Our results are not affected by these arbitrary ways of dealing with missing values.

The audits also contain valuable information on the performance of the local administration, especially with respect to established formal regulations. We attempted to create a series of categories reflecting somewhat different dimensions of performance, based on the belief that citizens have no particular reason to respond more strongly to outright corruption than they do to the failure to produce services (misspent money, however it is misspent, means fewer services for them). The two primary categories we consider are: "deficiencies in the operation of government programs executed or supervised by the local administration;" and "inferior quality in the provision of goods and services." Conceptually, these categories attempt to capture whether the government is providing higher/lower levels of services to its citizens.

Other categories include failure to comply with the terms of agreement with the federal government (generally in terms of outcomes/accomplishments) and labor irregularities (generally not withholding social security taxes). Conceptually, we believe these categories tap into government competence with respect to the federation, more than government performance with respect to citizens.

In addition, we created variables for "irregularities in administrative processes," "administrative irregularities associated with the use of resources," "with the procurement process," and "with the use of financial accounts." While these administrative categories could be indicators of corruption, the only thing we know for sure is that the paperwork and procedures associated with particular projects were incomplete/missing/incorrect. That is, there was no direct evidence of fraud, but there is direct evidence that the local government did not follow rules. The aggregate variable for this category is labeled "total administrative failures."

For the most part, we only have count variables for performance. The only variable in a form other than counts is labor irregularities. We also normalize these variables in the same manner as above.

The hypotheses we want to test can be stated as follows:

Hypothesis 1: Greater corruption/deficiencies in performance decrease tax revenue, while good reports increase tax revenue.

Hypothesis 2: Participatory budgeting should have lower levels of reported corruption and higher levels of government performance.

Hypothesis 3: Higher levels of reported corruption and lower levels of revealed government performance should generate demands for accountability. These demands could show up in the form of adoption of participatory budgeting.

These hypotheses rest on several assumptions. The first is that the audit reports reveal new information to taxpayers, allowing them to update their priors about politician performance. If the new information indicates that politicians are performing worse than expected, we would expect revenue to fall; if politicians are performing better than expected, we would expect revenue to increase. Because priors about government may systematically vary across municipalities and because these priors may be correlated with the content of the audits and because we have no direct way of observing these priors, we include municipal fixed effects.[5] Note that if citizens believe that politicians in a given municipality are corrupt and the audit merely confirms this prior, we would not expect revenue to change with the audit. Likewise, if citizens believe that politicians in a given municipality are not corrupt and the audit merely confirms this prior, we would not expect revenue to change with the audit. In other words, corruption that has already been properly priced in will be embedded in the fixed-effects and/or lagged values of the dependent variable. Note also that the newness of the information may vary between municipalities and between corruption and performance, as the latter may already be more visible.

Second, we are assuming that local governments would like to increase tax collection and that citizens respond similarly to corruption and performance (that is, the effect is symmetric across both types of failures).

Third, we assume that this information is credible because the integrity of the audit process is high and that information is sufficiently well-disseminated. Ferraz and Finan (2008), for example, argue that the information reached voters and was critical in local elections; they also find an interaction effect between the content of the report and the number of local radio stations. We do not have the radio station data; presumably, those data would only change the interpretation. Fourth, we posit that good audits should strengthen the governments bargaining power, while revealed corruption/poor performance should weaken it. Our research design does not, however, allow us to precisely identify the mechanism that changes tax revenue. We do not know, for example, if subsequent declines in revenue are because of decreased compliance or

[5] An alternative way of dealing with priors would be to include the lagged dependent variable on the right-hand side and difference out the fixed effects. Such models probably better account for priors than fixed-effects alone and they also present fewer problems with serial correlation. We do not make models with the lagged dependent variable the benchmark because they raise thorny endogeneity issues, but it is worth noting that first-differenced models with (and w/out) the lagged dependent variable on the right-hand side do not yield appreciably different results than the ones we report (available).

reduced rates (or both). We do believe, however, that we can rule out many other potential sources of change.

The dependent variables

We have relatively comprehensive data on sub-national revenue, including disaggregated tax and transfer categories. These data come from the Finance Ministry's (Secretaria do Tesouro Nacional) on-line database (Finanças do Brasil: Dados Contábeis dos Municípios-FINBRA), which is generally thought to be the most complete and reliable source of subnational data (Afonso 2010).[6] In Brazil, municipal revenues come primarily from three sources: local taxes and fees, shared taxes and fees and transfers from higher levels of government. Local taxes and fees come in a variety of forms, including unrequited taxes (called impostos), taxes earmarked for specific types of spending (known as contribuições), and taxes that are linked with specific services along the lines of user fees (known as taxas). Taken together these revenues account for only about 5 percent of total tax collection in Brazil, although their level and share of tax collection has increased substantially over the past two decades, partly because of pressure from the federal government. Purely local revenues account for approximately 15 percent of average municipal expenditure. Shared revenue from federal and state taxes that are tied to revenue collection in municipalities account for an additional 10 percent of local expenditure.[7] Transfers based on other criteria (e.g., population) account for the rest of municipal revenue. Two unrequited local taxes account for more than half of revenue from local taxes and fees: the property tax on structures known as the IPTU (Imposto sobre a Propriedade Predial e Territorial Urbana) represents approximately 20 percent of local tax revenue on average and the tax for general services known as the ISSQN (Imposto sobre Serviços de Qualquer Natureza) represents approximately 49 percent of local tax revenue.[8]

We make property taxes our benchmark because it is the cleanest measure. That is, not only are property tax rates and rolls (assessed values) largely set and administered at the local level, but they are also ones in which quasi-voluntary compliance is important, particularly in Brazil. Because local governments have limited administrative capacity to monitor and sanction compliance, good will matters, particularly with respect to updating assessed values. Furthermore, because property tax revenue collection evasion is quite high, most municipalities collect far less than they should (Afonso et. al. ND; Mora and Varsano 2001). In other words, it is a tax that can move up or down depending on how people perceive their government is doing. Our benchmark is the logged level of per capita property tax revenue in the year the audit is released. We supplement this measure with the change in the logged level of per capita property tax revenue and the ratio of property tax to GDP.

[6] FINBRA covers around 90 percent of municipalities in any given year. As Afonso (2010) notes, there are some inconsistencies over time with some variables as definitions and categories change over time, particularly with respect to disaggregated categories. Our primary variable, municipal property taxes (IPTU), does not suffer from any definitional inconsistencies, though there are some missing values.

[7] A fraction of some federal and state taxes are earmarked for the locality in which the revenue was produced. For example, 18.75 percent of the state-level VAT, known as the Imposto sobre Circulação de Mercadorias e Serviços (ICMS), is returned to the municipality which generated the revenue. Likewise, 70 percent of the tax on gold transactions is returned to the municipalities that generate the revenue.

[8] Like Afonso (2010), we do not count the federal tax on government employees (Imposto de Renda Retido nas Fontes sobre o Rendimento do Trabalho (IRRF)) as a local tax. Including it, the share of revenue accounted for by the IPTU falls to around 15 percent, while the ISSQN falls to around 36 percent.

Ideally, we would also use the ISSQN, which also relies on quasi-voluntary compliance. Unfortunately, the ISSQN presents some problems. The most important problem is that the tax base and rates for the ISSQN were adjusted by the federal government during the period under study (specifically, at the start of 2004). Among other things, for example, the federal government allowed services related to information technology/computers to be taxed, something that will not be randomly distributed across municipalities. Because there is an unobserved confounding factor (a change in the tax base) that occurs simultaneously with the treatment, we cannot do a clean before/after test.

As part of the sensitivity analysis we present results with the ISSQN and with combined local tax category that includes the IPTU, the local turnover tax on property sales (IBTI), and local service taxes and contributions destined local public goods.[9] The aggregate category accounts for approximately 50 percent of purely local tax/fee revenue. This aggregate category excludes the ISSQN and federal and state taxes collected in the municipality, even though some of the revenues from these taxes are destined for the municipality in which they are collected.[10]

Control variables

Changes in tax revenue could be driven by a number of factors. We use fixed-effects (and in regressions not shown, the lagged dependent variable) to control for state capacity, state motivation, people's priors about government performance and people's trust in each other.. Furthermore, we run placebo regressions with the lagged level of taxes on the left-hand side to confirm that revealed corruption did not affect property tax revenues before the treatment. In our regressions, we attempt to control for anything that could reduce tax effort. Because revenue not collected from property taxes could be offset by other revenue, we include all other tax revenue in the locality (tax receipts-iptu)) and total transfers from other governmental units. We also want to include economic variables that could spuriously cause taxes to go up and down. We use change in GDP per capita, which should be a crude, albeit imperfect proxy for economic cycles.

Our regression models are as follows

[9]The aggregate category (borrowing on Afonso 2010 and Sampaio et. at. 2008) only includes tax revenues derived from current taxes and contributions collected by the municipality. It is compromised of the sum (a) Tax on Real State Property (Imposto sobre a Propriedade Predial e Territorial Urbana (IPTU)); (b) taxes on property transmission (Imposto sobre Transmissão "Inter Vivos" de Bens Móveis e de Direitos Reais sobre Imóveis (ITBI); (c)Taxes for Local Police Services (Taxas pelo Poder de Polícia); (d) contribution on Services (Taxas pela Prestação de Serviços, CPS); (e) an improvement contribution (Contribuição de Melhoria (TCM)). We exclude the Tax on Services (Imposto sobre Serviços de Qualquer Natureza (ISSQN)) for reasons explained above. Following Afonso (2010), we also exclude the federal income tax on government employees which is collected at the municipal level (Imposto de Renda Retido nas Fontes sobre o Rendimento do Trabalho (IRRF)). It is not obvious that income tax withheld from government employees is a quasi-voluntary tax; it would seem to us that the number of local employees and their salaries would determine revenue more than employees willingness to pay. It is also not entirely clear that FINBRA records the data consistently over time (in some years, the IRRF seems to include only labor income; in other years, it seems to include non-labor income). Starting in 2007, FINBRA separated out labor from non-labor income.

[10] Local governments obtain revenue from some federal and state taxes, but have no control over the rates and only limited enforcement responsibilities.

Where γ is a categorical variable for whether or not the municipality was audited in time t;[11] is a vector of control variables that might also capture tax effort; is fixed-effects for years; is fixed effects for units and is a random error term for municipalities in it. In the regressions shown, we cluster the standard errors at the municipal level. Using Huber-White standard errors or clustering at the state level does not appreciably change the reported results.

Section III: Results

Table 2 presents the consequences of being audited using per capita property tax collection (Columns 1-5) and property taxes as a percentage of GDP (Columns 6-10). Column 1 shows just the bivariate regression without any controls with the universe of audited municipalities from 2003-2008 (not just those we coded). Column 2 includes all of our controls. Columns 3 and 4 restrict the sample to the audits we coded and non-audited municipalities. Column 5 includes categorical variables for the ones we coded and the ones we did not code. Columns 6-10 do the same with property taxes as a percentage of GDP on the left-hand side. There is nothing to indicate that merely being audited affects tax collection, nor is there any indication that our sample is anomalous. This result that just being audited has no systematic effect on tax collected is not surprising, given that the audits can reveal both good and bad performance.

To see if revealed performance matters, we now include an interaction of the audit and the contents of the audit and exclude all non-coded audits from the sample[12]. Our regression model is as follows:

Where captures the effect of revealed corruption/performance on property tax revenues. Note that will only capture those with positive corruption values; audits with zero corruption will be captured by the intercept, (Later, we will present alternative means of dealing with zero revealed corruption).

Table 3 presents the primary results in both count and quantity form. Columns 1 and 8 present the basic difference-in-difference model. Columns 2 and 9 include control variables. Columns 3 and 10 include the contemporaneous value and two lags of revealed corruption to test for persistence. Columns 4-5 and 11-12 present the placebo treatments. Both the contemporaneous count and the quantity measures are negative and significant with property tax collection.[13] Although there is no evidence of persistence with these models, we find persistence (up to t+2)

[11] One obvious question is whether we would expect a contemporaneous effect of the audit, especially given that some of the audits are released late in the fiscal year. Our results do not change appreciably if one treats audits released late in the year as though they were released in 2005 (we used October 1 and November 1 2004 as cut-off dates) .

[12] Given the random nature of the audits, dropping the non-coded municipalities should not bias estimations of the conditional average treatment.

[13] We only show the quantity measures with the square root and categorical variables for different increments (Table 5). Results with the log are similar. They are available.

with the log (not shown) and/or an AR1 correction (Table 3A). The placebo regressions indicate no relationship between current audits and lagged property tax revenue, suggesting that corruption has not been fully priced in. The coefficients on corruption are small: a one standard deviation increase in the number of acts of corruption vis-à-vis the number of effective audits (or a one-standard deviation increase in the amount of missing money) translate into a decrease in per capita property tax collection of approximately 1 real. By way of comparison, the 50th percentile for IPTU per capita is only 3.7 reais, while the 75th percentile is 7.9. The mean is 12.5 and the standard deviation is 37.3. In other words, the average treatment effect would be meaningful in the very large number of municipalities with relatively low property tax collection, but not in places that already collect substantial property tax revenue.[14]

Tables 3A-D in the Appendix present sensitivity analysis. Table 3A presents identical models with an AR1 correction and robust standard errors.[15] Those results are more robust than the ones above. Table 3B uses the alternative dependent variable, taxes as a percentage of GDP. The point estimates for the count variables remain negative across every specification, but are not significant; the quantity variables, by contrast, remain negative and significant across most of the models. In all cases, the placebo treatments remain indistinguishable from zero.

Table 3C presents results with the ISSQN and Table 3D shows the results with the aggregate local tax variable. While the ISSQN is never indistinguishable from zero, the aggregate category yields similar, albeit not as robust, results as the IPTU alone. The results with the aggregate category are primarily driven by the IPTU (that is, if we exclude the IPTU from the aggregate category, the corruption variables are negative but not significant).

Table 4 presents an alternative way of dealing with the zeros and, arguably, a better way of assessing whether relatively good performance is rewarded using just the IPTU. We create dummy variables for municipalities with revealed corruption counts one-standard deviation below the mean (a category that includes all zeros), those within one-standard deviation of the mean, and those that are one standard deviation above the mean. For the quantity, we do the same using up to the 50th percentile in raw quantities (below 3 reais per capita), the 50th-90th percentile (ranging from 3 to 50 reais per capita); and the 90th percentile and above (from 50 to 1000 reais per capita).

The point estimates of the categorical variables are consistent with fiscal-contract hypothesis in both counts and quantities. Specifically, the audits with the highest corruption counts (Columns

[14] If we arbitrarily split the sample based on the 75th percentile for IPTU in 2004, the quantitative results hold on both sides; the coefficient is actually larger for municipalities that collect less than the 75th percentile. Furthermore, the count variable is significant for those below the 75th percentile, with a slighter larger coefficient than with the entire sample.

[15] There is no consensus on how one should correct for persistence with difference-in-difference models, especially with a short time series. While serial correlation is present in our baseline models (Table 3 and 4), clustered standard errors should be robust to cross-sectional heteroskedasticity and within-panel (serial) correlation, provided that there is a large number of clusters. With the explicit correction, we used Stata's standard fixed-effect model for AR1 correction (xtregar), which uses the Cochrane-Orcutt transformation to subtract out the serial correlation. The explicit correction for serial correlation increases the robustness of our results. With the corrected models, the Baltagi-Wu locally best invariant (LBI) test statistic is approximately 2, suggesting that an AR1 correction suffices to wipe out serial correlation.

1-3), are negative and significant. Wald tests show that they can generally be distinguished from the other categories and from themselves in the previous period (the forward values/placebo in column 2). In effect, when the contents of the audit are revealed, the high corruption places experience a distinct fall in property tax revenue. Furthermore, the negative effect of corruption persists for three periods. The point estimates on both the intermediate and low corruption categories, by contrast, are positive but they are not distinct from zero, from each other or from their previous values; (it is worth noting that the point estimate on low corruption is four times larger than that for intermediate corruption). More importantly, perhaps, the three categories are not distinct from each other before the contents of the audit are released.

With the quantity measures, by contrast, very low levels of quantifiable corruption are positive and significant, suggesting that good performance (or at least not quantifiable corruption) is rewarded. Intermediate values are negative and significant. Large values (above the 90^{th} percentile) are negative and have the smallest point estimate, but they are not distinguishable from zero or from the other categories (arguably because we have a small sample and, hence, large standard errors). The low values are distinct from the intermediate values, but not from the large values. Both the low and intermediate values are distinguishable from themselves in the previous period; as with the counts, none of the categories are distinct prior to the publication of the audit report. In other words, while high corruption is clearly punished, low corruption seems to be rewarded. As before, both the reward and punishment would only be relatively meaningful in municipalities with low average collection to begin with.

Table 5 shows the disaggregated categories for corruption and the main disaggregated categories for performance. The point estimates for all of the disaggregated categories of corruption are negative, except for whether or not the number of minimum bids was reached (which is positive, but insignificant). Irregularities in the procurement process and no-bid contracts are negative and significant. Surprisingly, neither procurement fraud, nor diversion of funds are significant (unless we include an AR1 correction, not shown).

Table 6 turns to performance. While all but one of the measures of performance are negative, none are significant with property tax revenue, including what we believe to be the best measures, inferior quality and operational deficiencies (both of which capture failure in services used by citizens). Although there is nothing indicating that worse performance increases property tax revenue, there is not much evidence that worse performance on these metrics decreases revenue either.

Because we do not know what other covariates would predict the content of the audits and because we only have one observation per unit on our outcome variables, we use tests of means to see if, on average, municipalities that have ever had PB have less corruption and better performance. We use the within audited sample because we are no longer using revelation of information as a treatment. There are no systematic differences across any of the corruption categories, a result that also holds if we use municipalities that only used PB in 2004. The only systematic difference is found in "fail contract" as PB municipalities are more inclined to abide by the terms of agreement with the federal government. Given the number of variables in the analysis, the result could be spurious; randomly, we would expect at least one variable to be significant. Obviously, this simple test does not take into account the possibility that more

corruption municipalities might be the ones that adopt PB in the first place, meaning that PB has an effect, just not one that can be captured by a cross-section.

Table 8 turns to the adoption of participatory budgeting. Fiscal contract theories might predict that poor audit results would generate demands for the adoption of more transparent budgeting procedures. (Without cases studies we have no way of knowing whether the adoption of PB was driven by demand or supply; that is, to give the illusion of control, politicians may have strategically chosen to implement it). For now, we interpret it in terms of demand. The dependent variable is the adoption of PB anytime between 2005 and 2008 (because of the way the data were compiled, we cannot use a more fine-grained measure). We include a categorical variable for whether a municipality ever had participatory budgeting and we include controls for party. The regressions shown only include a categorical variable for Left party (which includes the PT, PC do B, PSB, PPS and PDT); including individual parties, notably the PT, does not meaningfully alter the interpretation of the results. While a more complete set of covariates might be desirable, pre-existing work on the subject (Wampler ND) finds no robust predictors, except possibly party. Because the adoption of PB is a relatively rare event (only 10 audited municipalities adopted it between 2005-2008, 9 of which are in our sample), we use an extreme value model (gompit models are shown; the results are robust with a variety of alternatives, ranging from a normal to negative binomial distribution).

The cross-sectional regression model with the within-audited sample is:

16

Both the aggregate count and quantity measures of corruption are positive and significant with the adoption of PB, generally at the 1 percent level. The disaggregated categories for contracts without bids, procurement irregularities and overinvoicing are positive and significant. None of the performance variables are significant, though the combined category for inferior quality and operational irregularities comes close. Holding the other variables at their means, a one standard deviation increase in corruption with the primary count and quantity measure would increase the probability of adoption by around 0.6-0.8 percent. While this seems small, it needs to be put in context: the baseline probability that a municipality will adopt PB is low (around 0.5-0.6 percent in any given year). The minimum to maximum changes are especially large, 37 percent for the counts and 64 percent for quantities.

Section IV: Discussion

Using a quasi-experimental design with a randomized treatment, we found that increases in revealed corruption decreases property tax revenue. These results are relatively robust in contemporaneous levels; the evidence in favor of persistence is model and measure dependent. The primary corruption variables that decrease revenue were irregularities in the procurement process, notably the awarding of bids without contract. The magnitude of the effects are small, around 1 real per capita when multiplied by one-standard deviation; the minimum to maximum changes are fairly substantial. Furthermore, the point estimates were consistent with a fiscal contract story, even though not all of the coefficients were significant. That is, while revealed corruption was clearly punished, it is not obvious that low corruption systematically increased

[16] We use a cross section because we do not have annual data for the outcome variable. The results are robust when we also include the non-audited municipalities.

revenue from property taxes. (It is important to note that these results almost exclusively hold for the property tax. If we switch the dependent variable to total local tax revenue or the ISSQN, most corruption/performance variables are indistinguishable from zero).

One question is why is there a (small, but noticeable) difference between the count and quantity variables in terms of robustness. That is, while the count variables are consistently negative and generally robust, when auditors quantify the amount of stolen/missing money and the sums are large relative to population or the amount audited, tax revenue clearly falls. We obviously do not know the exact reason for the asymmetry. One possibility is the quantity of corruption may have been more widely publicized, in part because opposition politicians and media believed it would resonate more strongly with citizens. A second possibility is that this result relates to the mental models people use to decide tax compliance. Not only may quantities be easier to understand, but they may also allow citizens to weigh the gravity of the events more carefully and at relatively low cost. In our coding of the reports, the count variables weigh large and small transgressions equally; taxpayers might not. But for taxpayers to weigh the gravity of the events with counts alone, they would need fairly detailed information about the events, something that requires an investment on their part.

A second question is why the asymmetric results between corruption and performance. We think the likely explanations are related to those hypothesized above. The simplest explanation is that it may be easier for citizens to understand corruption. Corruption, especially diversion of funds and procurement fraud, are easy to process mentally once they are detected. Corruption is also easier to measure and, in most cases, readily quantifiable. It is harder to put a dollar figure on the lost value of inferior quality services. That is, while the auditors could identify inferior quality, neither they, nor we, could identify the lost value associated with such failures. Finally, and what may be different, is that performance failures may already be discounted. That is, whereas revelations of corruption expose a considerable amount of new information, revelations of poor performance may not bring as much new information.

A third question is what mechanism connects higher levels of revealed corruption with decreases in property tax revenue. Unfortunately, our research design does not allow us to identify the mechanism with much precision. We do not know, for example, if the subsequent fall in revenue is because of decreased compliance or reduced rates (or both). We do believe, however, that it is not caused primarily by something else. That is, we can rule out mechanical changes associated with changes in GDP and changes due to other potential off-setting revenues, notably transfers. Given the instantaneous response of revenue to revealed corruption, we strongly suspect that politicians who are "caught" strategically lower rates and/or reduce enforcement. Nevertheless, we are not sure that politician behavior alone can explain why we find a positive shock in the event of zero revealed corruption with the quantitative variables and why we find at least some evidence of persistence.

A fourth question is what do the results reveal about the relationship between fiscal institutions, performance and demands for accountability. Surprisingly, perhaps, we found that PB made no systematic difference in terms of the counts or quantities of revealed corruption. It only made a difference with respect to one performance variable. Although it is possible that the unexpected result is largely a function of sample size, it is equally possible that there is heterogeneity among

municipalities with PB, something that Wampler (2008) has documented. That is, while PB clearly works as intended in some municipalities, it has not worked as intended in others.

The more interesting result in many respects is the fact that more revealed corruption affects the probability that a municipality will adopt PB. This result is quite robust.. We do not know whether this is supply or demand driven. If nothing else, it suggests a widespread belief that participatory budgeting is superior in many dimensions to budgeting via representative institutions. That is, even if the move to PB is driven by strategic politicians, it is because they believe that it these institutions will better satisfy demands for accountability.

Section V: Conclusions

Fiscal contract theories of the state hinge on tax revenue responding to government performance; they also posit that representation and accountability link taxes and performance. These propositions have yet to be tested with a causal research designs and disaggregated data. In this paper, we use relatively disaggregated data and a causal research design to test those propositions. Using difference-in-difference estimators with a randomized treatment, we show that municipal property tax revenue in Brazil responds to revealed corruption and that revealed corruption increases the probability that a municipality will adopt participatory budgeting, one mechanism of accountability. The results with the quantity variables are quite robust. We did not find that tax revenue responds to other metrics of performance, possibly because our measures are not as refined as one would like. Nor did we find that participatory budgeting affects revealed corruption or performance.

We believe that the construct validity of this paper is sound, but can only speculate about the mechanisms at work and its external validity. Given the robust correlation between performance and tax collection in other settings (e.g., labs and cross-country data), we suspect that the results are relatively generalizable. The main difference is that taxpayers in our experiment are exogenously exposed to good information about their government, allowing them to more precisely calibrate their compliance decisions. With good information, we see a relatively rapid response between revealed corruption and property tax revenue, consistent with a fiscal contract model of the state. In non-experimental settings, by contrast, citizen's still have information about government performance and can update their information based on newspaper reports and the like. But because the baseline and the new information may not be of the same quality in other settings as they are in our setting, there would just be a larger margin of error. Although we cannot test this proposition, we suspect, for example, that that taxation would respond to the interaction of government performance and a free media; because the latter should be able to send clearer signals, there should be a tighter correspondence between revenue and taxation.

In others words, while our paper establishes a causal connection between performance and one revenue stream and highlights the role that information plays, there is considerably more work that could be done, particularly if one wants to explain the difference between Sweden and Nicaragua from a fiscal contract perspective. That is, while our point estimates are entirely consistent with a fiscal contract story, particularly with respect to changes in taxation over time, they are too small to explain more than a limited fraction of the cross-country variation in tax levels.

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Table 1A: Tests of means of main variables. Audited sample vs. non-audited in 2003.

	Coded vs Non-Audited (minus audited but not coded)					
	Obs. non-audited	Obs. audited & coded	Mean non-audited	Mean audited & coded	p-value	t
IPTU PC (log)	3879	340	1.1	1.2	0.318	-0.999
ITBI PC (log)	3836	336	0.7	0.8	0.694	-0.394
ISSQN PC (log)	3999	347	1.7	1.9	0.027	-2.217
Sum local impostos PC (log) (IPTU, ITBI, ISSQN, IRRF)	4006	347	3.2	3.2	0.162	-1.399
Sum local taxas PC (log) (Police etc.)	3801	337	0.9	1.0	0.484	-0.700
Sum local impostos, taxas and contribuciones w/out IPTU PC (log)	4005	347	3.158	3.228	0.154	-1.425
Sum local impostos, taxas and contribuciones PC (log)	4007	347	3.4	3.4	0.132	-1.508
Deficit PC (log)	4008	347	-21.4	-1.6	0.789	-0.268
Total transfers PC (log)	4008	347	6.4	6.3	0.000	4.819
Population (log)	4008	347	9.3	9.6	0.000	-6.076
GDP PC (log)	3995	345	8.5	8.5	0.219	-1.230

Table 1B: Summary Statistics Corruption

		Total corruption	Diversion of funds	Overinvoicing	Irregularities in the procurement process	No bidding process	No minimum number of bids reached	Fraud found in the procurement process
Corruption counts	mean	3.67	0.82	0.48	2.37	1.01	0.54	0.82
	std. dev.	3.13	1.16	0.80	2.50	1.46	1.08	1.66
	N	331	331	331	331	331	331	331
Corruption acts as a percentage of effective number of audits	mean	0.113	0.027	0.015	0.071	0.031	0.017	0.024
	std. dev.	0.105	0.043	0.026	0.078	0.051	0.035	0.049
	N	331	331	331	331	331	331	331
Amount associated with corruption as a percentage of total amount audited	mean	0.073	0.027	0.003	0.047	0.025	0.002	0.023
	std. dev.	0.209	0.176	0.019	0.125	0.087	0.010	0.095
	N	331	292	303	331	299	269	315
Per capita amount associated with corruption	mean	22.8	7.3	0.7	15.7	5.9	0.4	10.5
	std. dev.	85.5	50.3	3.4	71.4	17.2	2.5	69.4
	N	331	292	303	331	299	269	315

Table 2: The Effect of an Audit on Property Taxes

VARIABLES	DV=IPTU_PC (log)					DV=IPTU_GDP				
	1	2	3	4	5	6	7	8	9	10
Audited$_{(t)}$	-0.004	0.005			0.020	-0.009	-0.007			-0.014
(any year)	(0.026)	(0.028)			(0.032)	(0.020)	(0.019)			(0.022)
Audited$_{(t)}$			-0.032	-0.044	-0.059			0.012	0.013	0.028
(our sample)			(0.049)	(0.049)	(0.057)			(0.030)	(0.031)	(0.037)
Population (log) $_{(t)}$		-0.952***		-0.880***	-0.952***		-0.830***		-0.752***	-0.830***
		(0.125)		(0.141)	(0.125)		(0.155)		(0.183)	(0.155)
GDPPC (log) $_{(t)}$		-0.025		-0.012	-0.025		-0.959***		-1.019***	-0.959***
		(0.039)		(0.044)	(0.039)		(0.129)		(0.159)	(0.129)
Transfers_PC (log) $_{(t)}$		-0.019		-0.036	-0.018		0.042		0.042	0.042
		(0.085)		(0.099)	(0.085)		(0.112)		(0.138)	(0.113)
Other Taxes PC (log) $_{(t)}$		0.087***		0.063**	0.087***		0.062**		0.070*	0.062**
		(0.028)		(0.032)	(0.028)		(0.029)		(0.036)	(0.029)
Constant	0.733***	9.876***	0.787***	9.317***	9.870***	1.546***	16.824***	1.570***	16.597***	16.826***
	(0.011)	(1.567)	(0.012)	(1.791)	(1.567)	(0.013)	(2.277)	(0.014)	(2.756)	(2.277)
Observations	40765	30654	33248	25001	30654	37133	31742	30206	25823	31742
R-squared	0.890	0.904	0.894	0.907	0.904	0.971	0.974	0.970	0.973	0.974

Note: All models use clustered standard errors, unless otherwise noted.

Table 3: Property Taxes Per Capita and Revealed Corruption

	DV= IPTU_PC$_t$ (log)													
	Corruption Counts							Corruption Quantities						
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Audited$_t$	0.092	0.079	0.086			0.086	0.078	0.033	0.024	0.031			0.024	0.019
	(0.059)	(0.060)	(0.065)			(0.065)	(0.066)	(0.045)	(0.045)	(0.051)			(0.049)	(0.050)
Audited$_{t-1}$			-0.042							-0.041				
			(0.073)							(0.055)				
Audited$_{t-2}$			-0.005							0.009				
			(0.062)							(0.047)				
Audited$_{t+1}$				-0.011	-0.027	0.004	-0.010				-0.001	-0.010	0.003	-0.006
(placebo)				(0.053)	(0.054)	(0.057)	(0.058)				(0.043)	(0.045)	(0.046)	(0.049)
Corruption$_t$	-1.106**	-1.088**	-1.124**			-1.172**	-1.184**	-0.024*	-0.025*	-0.026			-0.026*	-0.028*
	(0.508)	(0.516)	(0.514)			(0.560)	(0.558)	(0.014)	(0.015)	(0.017)			(0.015)	(0.015)
Corruption$_{t-1}$			0.622							0.028*				
			(0.545)							(0.016)				
Corruption$_{t-2}$			-0.339							-0.020				
			(0.413)							(0.012)				
Corruption$_{t+1}$				-0.035	0.023	-0.235	-0.219				-0.005	-0.005	-0.010	-0.011
(placebo)				(0.326)	(0.311)	(0.361)	(0.345)				(0.011)	(0.012)	(0.011)	(0.011)
Population $_t$		-0.880***	-0.876***		-0.869***		-0.869***		-0.880***	-0.875***		-0.870***		-0.870***
(log)		(0.141)	(0.174)		(0.141)		(0.141)		(0.141)	(0.175)		(0.141)		(0.142)
GDPPC$_t$		-0.012	0.002		-0.020		-0.020		-0.012	0.000		-0.020		-0.021
(log)		(0.044)	(0.052)		(0.045)		(0.045)		(0.044)	(0.052)		(0.045)		(0.045)
Transfers_PC$_t$		-0.035	0.087		-0.027		-0.025		-0.034	0.088		-0.027		-0.025
(log)		(0.099)	(0.130)		(0.101)		(0.101)		(0.099)	(0.130)		(0.101)		(0.101)
Other taxes_PC$_t$		0.063**	0.064		0.065**		0.065**		0.063**	0.065		0.065**		0.065**
(log)		(0.032)	(0.041)		(0.033)		(0.033)		(0.032)	(0.041)		(0.033)		(0.033)
Constant	0.787***	9.312***	8.640***	0.815***	9.718***	0.815***	9.710***	0.787***	9.312***	8.647***	0.815***	9.731***	0.815***	9.734***
	(0.012)	(1.791)	(2.388)	(0.012)	(1.885)	(0.012)	(1.885)	(0.012)	(1.791)	(2.390)	(0.012)	(1.886)	(0.012)	(1.886)
Observations	33248	25001	19924	28364	24196	28364	24196	33248	25001	19924	28364	24196	28364	24196
R-squared	0.894	0.907	0.921	0.902	0.911	0.902	0.911	0.894	0.907	0.921	0.902	0.911	0.902	0.911
Lincom $_{(t+t-1+t-2)}$			-0.841							-0.017				
			(0.905)							(0.033)				

Notes: Models 1-7 use the corruption counts as a percentage of effective number of audits. Models 8-14 use the square-root of the quantity.

Table 3A: Property Taxes Per Capita and Revealed Corruption (w/ AR1 correction)

	DV= IPTU_PC$_t$ (log)													
	Corruption Counts							Corruption Quantities						
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Audited$_t$	0.094*	0.075	0.116*			0.083	0.078	0.035	0.039	0.043			0.018	0.030
	(0.054)	(0.055)	(0.066)			(0.058)	(0.059)	(0.045)	(0.046)	(0.055)			(0.049)	(0.049)
Audited$_{t-1}$			0.030							0.021				
			(0.072)							(0.060)				
Audited$_{t-2}$			0.033							0.056				
			(0.066)							(0.054)				
Audited$_{t+1}$				-0.053	-0.030	-0.021	0.002				-0.035	-0.033	-0.028	-0.021
(placebo)				(0.053)	(0.053)	(0.058)	(0.058)				(0.044)	(0.045)	(0.048)	(0.048)
Corruption$_t$	-1.282***	-1.251***	-1.550***			-1.409***	-1.439***	-0.031***	-0.039***	-0.038***			-0.035***	-0.043***
	(0.343)	(0.349)	(0.428)			(0.373)	(0.374)	(0.009)	(0.009)	(0.012)			(0.010)	(0.010)
Corruption$_{t-1}$			0.132							0.010				
			(0.462)							(0.013)				
Corruption$_{t-2}$			-0.557							-0.033***				
			(0.417)							(0.011)				
Corruption$_{t+1}$				0.329	0.258	-0.202	-0.313				0.007	0.012	-0.006	-0.005
(placebo)				(0.339)	(0.339)	(0.367)	(0.370)				(0.009)	(0.009)	(0.010)	(0.010)
Population $_t$		-0.467***	-0.540***		-0.488***		-0.489***		-0.466***	-0.539***		-0.486***		-0.489***
(log)		(0.105)	(0.133)		(0.106)		(0.106)		(0.105)	(0.133)		(0.106)		(0.106)
GDPPC$_t$		0.058	0.042		0.058		0.058		0.057	0.040		0.059		0.057
(log)		(0.040)	(0.049)		(0.041)		(0.041)		(0.040)	(0.049)		(0.041)		(0.041)
Transfers_PC$_t$		0.154**	0.228***		0.182***		0.184***		0.156**	0.229***		0.184***		0.185***
(log)		(0.068)	(0.083)		(0.068)		(0.068)		(0.068)	(0.083)		(0.068)		(0.068)
Other taxes_PC$_t$		0.072***	0.078***		0.083***		0.082***		0.073***	0.080***		0.083***		0.082***
(log)		(0.016)	(0.020)		(0.017)		(0.017)		(0.016)	(0.020)		(0.017)		(0.017)
Constant	2.068***	2.024***	2.088***	1.994***	2.170***	2.000***	2.176***	2.072***	2.032***	2.049***	1.994***	2.170***	2.004***	2.182***
	(0.093)	(0.136)	(0.334)	(0.113)	(0.141)	(0.113)	(0.141)	(0.093)	(0.136)	(0.333)	(0.113)	(0.141)	(0.113)	(0.141)
Observations	28780	20548	15534	23922	19764	23922	19764	28780	20548	15534	23922	19764	23922	19764
R-squared	4437	4409	4266	4388	4342	4388	4342	4437	4409	4266	4388	4342	4388	4342
Lincom $_{(t+t-1+t-2)}$			-1.975*							-0.062**				
			(1.070)							(0.029)				
Baltagi-Wu LBI	1.840	2.001	2.156	1.941	2.012	1.940	2.0189	1.840	2.002	2.155	1.941	2.020	1.940	2.019

Notes: Models 1-7 use the corruption counts as a percentage of effective number of audits. Models 8-14 use the square-root of the quantity. All models include an AR1 correction.

Table 3B: Property Taxes % GDP and Revealed Corruption

	Corruption Counts						Corruption Quantities					
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12
$Audited_t$	0.024	0.021	0.024			0.025	0.039	0.039	0.044			0.045
	(0.035)	(0.036)	(0.039)			(0.041)	(0.035)	(0.035)	(0.039)			(0.039)
$Audited_{t-1}$			-0.011						-0.020			
			(0.044)						(0.046)			
$Audited_{t-2}$			0.022						0.005			
			(0.046)						(0.034)			
$Audited_{t+1}$				0.019	0.010	0.015				0.019	0.021	0.030
(placebo)				(0.038)	(0.041)	(0.045)				(0.031)	(0.033)	(0.036)
$Corruption_t$	-0.102	-0.085	-0.088			-0.051	-0.010**	-0.010**	-0.011*			-0.009
	(0.232)	(0.256)	(0.240)			(0.327)	(0.005)	(0.005)	(0.006)			(0.006)
$Corruption_{t-1}$			-0.104						-0.001			
			(0.358)						(0.015)			
$Corruption_{t-2}$			-0.530						-0.017**			
			(0.464)						(0.008)			
$Corruption_{t+1}$				0.142	0.232	0.222				0.006	0.006	0.004
(placebo)				(0.335)	(0.370)	(0.421)				(0.008)	(0.009)	(0.010)
$Population_t$		-0.755***	-0.474***		-0.739***	-0.739***		-0.756***	-0.472***		-0.739***	-0.739***
(log)		(0.183)	(0.169)		(0.206)	(0.206)		(0.183)	(0.168)		(0.206)	(0.206)
$GDPPC_t$		-0.934***	-0.948***		-0.985***	-0.985***		-0.934***	-0.948***		-0.985***	-0.985***
(log)		(0.147)	(0.112)		(0.155)	(0.155)		(0.147)	(0.112)		(0.155)	(0.155)
$Transfers\_PC_t$		0.022	0.255*		0.021	0.021		0.023	0.256*		0.022	0.022
(log)		(0.137)	(0.151)		(0.141)	(0.141)		(0.137)	(0.151)		(0.141)	(0.141)
$Other\ taxes\_PC_t$		0.016**	0.014*		0.017**	0.017**		0.016**	0.014*		0.017**	0.017**
(log)		(0.007)	(0.008)		(0.008)	(0.008)		(0.007)	(0.008)		(0.008)	(0.008)
Constant	1.570***	16.248***	12.327***	1.595***	16.921***	16.922***	1.570***	16.320***	12.295***	1.595***	16.563***	16.913***
	(0.014)	(2.696)	(2.365)	(0.014)	(2.963)	(2.963)	(0.014)	(2.716)	(2.363)	(0.014)	(2.903)	(2.961)
Observations	30206	25836	20487	29226	24950	24950	30206	25836	20487	29226	24950	24950
R-squared	0.970	0.973	0.976	0.970	0.973	0.973	0.970	0.973	0.976	0.970	0.973	0.973
$Lincom_{(t+t-1+t-2)}$			-0.723						-0.028			
			(0.594)						(0.020)			

Note: With AR1 correction, the corruption variables are not significant with counts and marginally insignificant with quantities.

Table 3D: ISSQN and Revealed Corruption

	Corruption Count					Corruption Quantity					Corruption Count (AR1 correction)					Corruption Quantity (AR1 correction)				
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
$Audited_t$	0.044	0.027	0.031			0.072*	0.060*	0.046			0.023	0.010	0.058			0.054	0.045	0.048		
	(0.042)	(0.039)	(0.044)			(0.037)	(0.035)	(0.039)			(0.040)	(0.041)	(0.049)			(0.034)	(0.035)	(0.041)		
$Audited_{t-1}$			0.038					0.001					0.069					0.014		
			(0.049)					(0.045)					(0.054)					(0.045)		
$Audited_{t-2}$			-0.016					-0.004					0.037					0.008		
			(0.045)					(0.036)					(0.049)					(0.040)		
$Audited_{t+1}$				0.071	0.052				0.042	0.025				0.038	0.026				0.014	0.002
(placebo)				(0.052)	(0.052)				(0.042)	(0.042)				(0.041)	(0.040)				(0.034)	(0.034)
$Corruption_t$	0.051	0.068	0.057			-0.008	-0.009	-0.003			0.136	0.164	-0.071			-0.006	-0.006	0.001		
	(0.204)	(0.198)	(0.242)			(0.008)	(0.008)	(0.008)			(0.256)	(0.263)	(0.317)			(0.007)	(0.007)	(0.009)		
$Corruption_{t-1}$			-0.193					0.006					-0.287					0.009		
			(0.304)					(0.009)					(0.351)					(0.010)		
$Corruption_{t-2}$			0.207					0.004					-0.141					0.005		
			(0.271)					(0.006)					(0.311)					(0.008)		
$Corruption_{t+1}$				-0.435	-0.335				-0.008	-0.004				-0.301	-0.236				-0.004	-0.001
(placebo)				(0.299)	(0.293)				(0.008)	(0.007)				(0.261)	(0.256)				(0.007)	(0.007)
$Population_t$		-0.721***	-0.608***		-0.733***		-0.721***	-0.610***		-0.733***		-0.361***	-0.481***		-0.410***		-0.361***	-0.481***		-0.409***
(log)		(0.123)	(0.140)		(0.124)		(0.123)	(0.140)		(0.124)		(0.081)	(0.102)		(0.082)		(0.081)	(0.102)		(0.082)
$GDPPC_t$		0.094*	0.075		0.100*		0.094*	0.074		0.100*		0.058*	0.018		0.048		0.058*	0.017		0.048
(log)		(0.050)	(0.057)		(0.052)		(0.050)	(0.057)		(0.052)		(0.031)	(0.037)		(0.032)		(0.031)	(0.037)		(0.032)
$Transfers\_PC_t$		0.253***	0.356***		0.263***		0.253***	0.356***		0.263***		0.371***	0.329***		0.354***		0.371***	0.329***		0.354***
(log)		(0.074)	(0.099)		(0.077)		(0.074)	(0.100)		(0.077)		(0.051)	(0.062)		(0.052)		(0.051)	(0.062)		(0.052)
$Oth\_taxes\_PC_t$		0.186***	0.197***		0.174***		0.186***	0.196***		0.174***		0.200***	0.216***		0.199***		0.200***	0.215***		0.199***
(log)		(0.030)	(0.041)		(0.031)		(0.030)	(0.041)		(0.031)		(0.012)	(0.015)		(0.013)		(0.012)	(0.015)		(0.013)
Constant	2.365***	5.438***	4.134**	2.062***	5.475***	3.057***	5.438***	4.158**	2.062***	5.532***	0.096	-0.162*	-0.665*	-0.059	-0.190**	0.097	-0.160*	-0.613	-0.061	-0.191**
	(0.007)	(1.517)	(1.859)	(0.008)	(1.555)	(0.009)	(1.517)	(1.861)	(0.008)	(1.566)	(0.078)	(0.096)	(0.380)	(0.093)	(0.090)	(0.078)	(0.096)	(0.380)	(0.093)	(0.090)
Observations	29911	25761	20444	24945	24879	29911	25761	20444	24945	24879	25427	21283	16011	20477	20415	25427	21283	16011	20477	20415
R-squared	0.823	0.834	0.855	0.834	0.838	0.823	0.834	0.855	0.834	0.838	4475	4464	4338	4422	4417	4475	4464	4338	4422	4417
BW LBI											1.558958	1.69822	1.882194	1.705588	1.705597	1.55899	1.698261	1.882197	1.705484	1.705513

Table 3E: Aggregated Local Taxes, Taxas and Contribuciones and Revealed Corruption

	Corruption Count					Corruption Quantity					Corruption Count (AR1 correction)					Corruption Quantity (AR1 correction)				
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
$Audited_t$	0.064	0.051	0.043			0.026	0.019	0.007			0.067	0.057	0.068			0.032	0.026	0.010		
	(0.050)	(0.050)	(0.053)			(0.041)	(0.040)	(0.044)			(0.047)	(0.048)	(0.059)			(0.039)	(0.040)	(0.049)		
$Audited_{t-1}$			-0.078					-0.059					-0.042					-0.056		
			(0.054)					(0.042)					(0.063)					(0.053)		
$Audited_{t-2}$			-0.016					-0.015					0.002					0.008		
			(0.058)					(0.050)					(0.058)					(0.048)		
$Audited_{t+1}$				0.041	0.039				0.031	0.027				0.021	0.020				0.025	0.020
(placebo)				(0.051)	(0.051)				(0.043)	(0.043)				(0.047)	(0.047)				(0.039)	(0.039)
$Corruption_t$	-0.697**	-0.621*	-0.556			-0.015	-0.014	-0.010			-0.717**	-0.674**	-0.842**			-0.017**	-0.017**	-0.013		
	(0.318)	(0.327)	(0.354)			(0.013)	(0.013)	(0.014)			(0.300)	(0.307)	(0.381)			(0.008)	(0.008)	(0.010)		
$Corruption_{t-1}$			0.630*					0.021**					0.159					0.013		
			(0.364)					(0.009)					(0.408)					(0.011)		
$Corruption_{t-2}$			0.138					0.006					-0.048					-0.004		
			(0.359)					(0.011)					(0.372)					(0.010)		
$Corruption_{t+1}$				-0.435	-0.437				-0.015	-0.014				-0.223	-0.221				-0.011	-0.009
(placebo)				(0.294)	(0.294)				(0.009)	(0.009)				(0.299)	(0.298)				(0.008)	(0.008)
$Population_t$		-0.634***	-0.722***		-0.516***		-0.634***	-0.725***		-0.517***		-0.596***	-0.851***		-0.543***		-0.596***	-0.852***		-0.544***
(log)		(0.155)	(0.188)		(0.153)		(0.155)	(0.188)		(0.153)		(0.092)	(0.116)		(0.093)		(0.092)	(0.116)		(0.093)
$GDPPC_t$		0.148***	0.151***		0.143***		0.148***	0.149***		0.142***		0.198***	0.151***		0.196***		0.197***	0.150***		0.196***
(log)		(0.041)	(0.042)		(0.040)		(0.041)	(0.042)		(0.040)		(0.035)	(0.043)		(0.035)		(0.035)	(0.043)		(0.035)
$Transfers\_PC_t$		0.084	0.123		0.081		0.085	0.123		0.079		0.115**	0.116*		0.140**		0.116**	0.117*		0.139**
(log)		(0.100)	(0.132)		(0.104)		(0.100)	(0.132)		(0.104)		(0.058)	(0.071)		(0.058)		(0.058)	(0.071)		(0.058)
Constant	2.379***	6.432***	7.176***	2.630***	5.614***	2.231***	6.431***	7.222***	2.630***	5.646***	1.501***	1.295***	3.116***	1.533***	1.438***	1.503***	1.297***	3.074***	1.533***	1.437***
	(0.008)	(2.005)	(2.541)	(0.009)	(2.023)	(0.010)	(2.005)	(2.542)	(0.009)	(2.025)	(0.108)	(0.128)	(0.531)	(0.138)	(0.136)	(0.108)	(0.128)	(0.531)	(0.138)	(0.136)
Observations	29732	25616	20343	24807	24754	29732	25616	20343	24807	24754	25255	21145	15925	20348	20297	25255	21145	15925	20348	20297
R-squared	0.901	0.904	0.916	0.907	0.908	0.901	0.904	0.916	0.907	0.908	4463	4455	4328	4406	4403	4463	4455	4328	4406	4403
BW LBI											1.847	1.919	2.128449	1.9614	1.959	1.845	1.919	2.128	1.962	1.959

Note: The Aggregate local tax category excludes the ISSQN and IRRF.The linear combination is never significant.

Table 4: Property Taxes PC with Indicator Variables for Corruption

	Corruption Counts			Corruption Quantities		
VARIABLES	1	2	3	4	5	6
Corruption High$_t$	-0.568***	-0.630***	-0.590***	-0.240	-0.287	-0.225
	(0.217)	(0.229)	(0.206)	(0.275)	(0.271)	(0.312)
Corruption Medium$_t$	0.022	0.012	0.032	-0.158*	-0.170*	-0.177*
	(0.051)	(0.053)	(0.057)	(0.093)	(0.100)	(0.094)
Corruption Low$_t$	0.076	0.092	0.058	0.078**	0.071*	0.095**
	(0.075)	(0.091)	(0.070)	(0.035)	(0.040)	(0.041)
Corruption High$_{t-1}$			0.191			0.204
			(0.149)			(0.226)
Corruption High $_{t-2}$			-0.085			-0.271
			(0.122)			(0.212)
Corruption Medium $_{t-1}$			0.031			0.074
			(0.048)			(0.067)
Corruption Medium $_{t-2}$			-0.043			-0.105
			(0.054)			(0.088)
Corruption Low $_{t-1}$			-0.168			-0.034
			(0.155)			(0.058)
Corruption Low $_{t-2}$			-0.005			0.035
			(0.107)			(0.042)
Corruption High $_{t+1}$		-0.122			-0.019	
(placebo)		(0.118)			(0.053)	
Corruption Medium $_{t+1}$		-0.036			-0.028	
(placebo)		(0.053)			(0.076)	
Corruption Low $_{t+1}$		0.059			-0.164	
(placebo)		(0.104)			(0.203)	
Population (log)	-0.881***	-0.871***	-0.877***	-0.881***	-0.872***	-0.876***
	(0.140)	(0.141)	(0.174)	(0.141)	(0.142)	(0.175)
GDPPC (log)	-0.013	-0.021	0.001	-0.012	-0.021	0.002
	(0.044)	(0.045)	(0.052)	(0.044)	(0.045)	(0.052)
Transfers PC log	-0.037	-0.027	0.085	-0.034	-0.026	0.088
	(0.099)	(0.101)	(0.130)	(0.099)	(0.101)	(0.130)
Other Taxes PC (log)	0.063**	0.065**	0.064	0.063**	0.065**	0.065
	(0.032)	(0.033)	(0.041)	(0.032)	(0.033)	(0.041)
Constant	9.342***	9.748***	8.676***	9.318***	9.743***	8.641***
	(1.791)	(1.884)	(2.388)	(1.791)	(1.887)	(2.391)
Observations	25001	24196	19924	25001	24196	19924
R-squared	0.907	0.911	0.921	0.907	0.911	0.921
Wald high vs. medium (p-value)	0.008			0.7776		
Wald high vs. low (p-value)	0.0049			0.2522		
Wald medium vs. low (p-value)	0.5358			0.0159		
Wald high vs. medium placebo (p-value)		0.5026			0.5274	
Wald high vs. low placebo (p-value)		0.2476			0.4873	
Wald medium vs. low placebo (p-value)		0.4106			0.9212	
Wald high current vs. high placebo (p-value)		0.0193			0.7422	
Wald medium current vs. medium placebo (p-value)		0.4431			0.1283	
Wald low current vs. low placebo (p-value)		0.6630			0.0488	
Lincom High $_{(t+t-1+t-2)}$			-0.483*			-0.291
Lincom Medium$_{(t+t-1+t-2)}$			0.020			-0.207
Lincom Low$_{(t+t-1+t-2)}$			-0.115			0.097

Table 5: Property Taxes Per Capita with Disaggregated Counts and Quantities

VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14
audited	0.077	0.030	-0.042	-0.040	-0.032	-0.033	0.089	0.031	-0.013	-0.028	-0.064	-0.028	0.070	0.067
	(0.064)	(0.047)	(0.048)	(0.048)	(0.056)	(0.052)	(0.062)	(0.050)	(0.051)	(0.050)	(0.057)	(0.051)	(0.063)	(0.049)
Count Broadest Measure	-0.706*													
	(0.391)													
Quantity Broadest Measure		-0.023												
		(0.014)												
Count Diversion of Funds			-0.060											
			(1.061)											
Quantity Diversion of Funds				-0.004										
				(0.030)										
Count Overinvoicing					-0.744									
					(1.506)									
Quantity Overinvoicing						-0.037								
						(0.063)								
Count Procurement							-1.886**							
Irregularities							(0.793)							
Quantity Procurement								-0.037*						
Irregularities								(0.019)						
Fraud Count									-1.351					
									(1.024)					
Fraud Quantity										-0.015				
										(0.017)				
Count No Minimum Bids											1.170			
											(0.803)			
Quantity No Min Bids												-0.062		
												(0.108)		
Count No-Bid Contracts													-3.732*	
													(2.020)	
Quantity No-Bid Contract														-0.094***
														(0.033)
Constant	9.321***	9.314***	9.317***	9.317***	9.312***	9.308***	9.313***	9.314***	9.315***	9.315***	9.326***	9.315***	9.344***	9.326***
	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)
Observations	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001
R-squared	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907

Notes: All models estimated with controls. We suppressed them for space reasons. Models with an AR1 correction are available. The main difference with those models is that procurement fraud is significant in counts and quantities.

Table 6: Performance failures and property taxes per capita

	DV=IPTU_PC (log)										
VARIABLES	1	2	3	4	5	6	7	8	9	10	11
Audited	-0.044	-0.103	0.006	0.043	0.030	0.032	-0.026	-0.044	-0.042	-0.002	-0.007
	(0.082)	(0.085)	(0.081)	(0.081)	(0.076)	(0.065)	(0.068)	(0.056)	(0.066)	(0.052)	(0.050)
Sum Performance	0.000										
	(0.153)										
Inferior Quality		0.322									
		(0.379)									
Irregularities in Operation			-0.198								
			(0.265)								
Sum Administrative Deficiencies				-0.148							
				(0.139)							
Irregularities in Administrative Procedures					-0.302						
					(0.339)						
Irregularities in use of Resources						-0.447					
						(0.323)					
Procurement Irregularities in Accounting							-0.140				
							(0.391)				
								-0.003			
Irregularities								(0.681)			
Fail contract									-0.015		
									(0.333)		
Labor Irregularities Count										-1.212	
										(1.108)	
Labor Irregularities Quantity											-0.054
											(0.045)
Constant	9.317***	9.315***	9.319***	9.325***	9.326***	9.323***	9.318***	9.317***	9.318***	9.318***	9.316***
	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)	(1.791)
Observations	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001	25001
R-squared	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907	0.907

Notes: All models estimated with controls. We suppressed them for space reasons. Models with an AR1 correction do not differ appreciably.

Table 7: Participatory budget and revealed corruption and performance, tests of means (see note below)

	Difference of Means Test					
	Obs. non-PB	Obs. PB	Mean non-PB	Mean PB	p-value	t
Corruption count % of effective audited	327	16	0.12	0.11	0.84	0.20
Corruption count with borderline categories, % effective audited	327	16	0.18	0.17	0.98	0.03
Diversion of funds as % of effective audited	327	16	0.03	0.03	0.88	-0.15
Overinvoicing detected as % of effective audited	327	16	0.01	0.02	0.41	-0.83
Irregularities found in the procurement process	327	16	0.07	0.06	0.54	0.62
Count No Minimum bids	327	16	0.02	0.02	0.86	-0.18
Count Procurement fraud	327	16	0.02	0.02	0.85	0.20
Count No bid contracts	327	16	0.03	0.02	0.38	0.88
	Obs. non-PB	Obs. PB	Mean non-PB	Mean PB	p-value	t
Quantity corruption pc (zeros adjusted)*	239	12	1.80	1.22	0.36	0.92
Diversion quantity pc (zeros adjusted)*	290	14	-2.92	-3.20	0.76	0.31
Overinvoicing quantity pc (zeros adjusted)*	301	14	-5.64	-5.14	0.56	-0.59
Irregularities in procurement quantity pc (zeros adjusted)*	183	8	1.90	1.72	0.79	0.27
No bid contract quantity pc (zeros adjusted)*	296	15	-1.46	-2.62	0.14	1.48
Procurement fraud quantity pc (zeros adjusted)*	311	16	-3.16	-2.81	0.68	-0.41
No minimum bids quantity pc (zeros adjusted)*	267	13	-2.08	-2.22	0.62	0.50
	Obs. non-PB	Obs. PB	Mean non-PB	Mean PB	p-value	t
Total Performance failures as % of effective audits	327	16	0.44	0.44	0.94	-0.08
Inferior quality as % of effective audits	327	16	0.60	0.49	0.28	1.09
Operational irregularities as % of effective audits	327	16	0.19	0.18	0.84	0.20
Total administrative irregularities	327	16	0.25	0.27	0.78	-0.28
Fail contract	**327**	**16**	**0.25**	**0.14**	**0.01**	**2.52**
Labor Irregularities (count)	327	16	0.12	0.13	0.63	-0.48
Log labor irregularities (quantity zeros adjusted)*	327	16	0.04	0.02	0.28	1.08

This table shows the comparison using between municipalities that had PB anytime prior to 2005. Using the PB 2004 sample (9 obs) yields similar results. With quantities, we used the square root. Observations with a positive count in the respective category and no reported quantity were assigned one-half the lowest reported amount. The manner in which zero's are treated (e.g, dropped) does not change significance level.

Table 8: Revealed corruption and the Adoption of PB

VARIABLES	pb	pb05_08	pb05_08	pb05_08	pb05_08	pb05_08	pb05_08	pb05_08	pb05_08	pb05_08
Audited before 2005	0.000									
	(0.002)									
Audited in sample		-0.067								
		(0.367)								
Corruption Count			0.250***							
			(0.086)							
Corruption Count %				5.910**						
Effective audits				(2.330)						
Count No bid contracts					9.510***					
% Effective audits					(2.636)					
Corruption Quantity PC						0.171***				
						(0.041)				
Corruption % of							3.068***			
money audited							(0.868)			
Overinvoicing quantity PC								0.632***		
								(0.193)		
Procurement Irregularities									0.129***	
Quantity PC									(0.036)	
Corruption Quantity PC										0.089***
(probit)										(0.027)
pbever		3.397***	4.294***	4.238***	4.263***	4.537***	4.296***	3.742***	4.051***	2.586***
		(0.182)	(0.685)	(0.751)	(0.802)	(0.955)	(0.982)	(0.792)	(0.820)	(0.502)
Left		1.880***	2.015***	1.883**	1.707**	1.714**	1.626**	2.160***	1.640**	1.296***
		(0.193)	(0.777)	(0.914)	(0.819)	(0.726)	(0.819)	(0.784)	(0.729)	(0.419)
Constant	0.038***	-5.065***	-6.815***	-6.300***	-5.861***	-6.331***	-5.984***	-5.764***	-5.676***	-3.482***
	(0.001)	(0.133)	(0.976)	(0.795)	(0.594)	(0.826)	(0.701)	(0.650)	(0.582)	(0.427)
Observations	42880	5291	352	352	352	339	352	311	339	339

Notes: Model 1 is a fixed-effects regression of year of audit on PB. Model 2 is a cross-sectional probability model (gompit) on audited in 2004 and adopting PB anytime between 2005 and 2008. Models 3-9 replace the audit variable with the corruption counts and quantities. Model 10 is a probit with the just the primary corruption quantity variable. Probits with the other measures of corruption do not differ substantially from the gompits presented.

Appendix

The corruption measures were coded following the ideas of Ferraz and Finan (2008), who attempted to distinguish between different qualitative aspects of corruption. The three main categories of corruption "technologies" were diversion of funds, over-invoicing for goods and services and irregularities in the procurement processes. This last category can be further disaggregated into three components: failure to meet with the minimum number of bids requirement in procurement processes, failure to execute the bidding process altogether, or direct evidence of fraud in the procurement processes.

Some cases were hard to consider clear-cut cases of corruption, and were coded in borderline categories. These include the spending of public resources for other purposes than specified, and irregular unexecuted budget.

The audits also contain valuable information on the performance of the local administration, especially as it relates to established formal regulations. Three crucial categories considered are irregularities in the operation of government programs executed of supervised by the local administration, inferior quality in the provision of goods and services and irregularities in administrative processes. Other categories include the instances of failure to comply with the program covenant (with the federal government), labor irregularities, administrative irregularities associated with the use of resources, with the procurement process, and with the use of financial accounts.

Corruption	Irregularities in procurement processes (irrprocur)	No bidding process (nolicit)	-
		Minimum number of bidders not reached (nominbids)	When a procurement process did not reached the minimum number of bidders. Brazilian law stipulates this number depending on the amount of money involved.
		Evidence of fraud in the procurement process (procufraud)	When any direct evidence suggesting the presence of fraud in the procurement process is found. For example, in Carinhanha BA direct evidence of a simulated bidding process was revealed. While two companies systematically appeared as bidders, the legal representatives formally denied having participated in any such processes. In all instances the same third company won the bid.
	Diversion of funds (diversion)		In general, when expenditures cannot be backed with receipts or proof of purchase. Additionally, when direct evidence of diversion is found. The general guideline is the use of public resources for private ends (not included in the covenant). For example, in Valentim Gentil SP, a 5,316.52 reais expenditure was not accredited (with fiscal receipts) by the Epidemiology and Disease Control Team during the implementation of a prevention program.
	Over-invoicing (overinvoicing)		When there's evidence that purchases were made (or reported) at an above-market value. For example, in Bastos SP an inferior-quality material was used to replace the wooden floor of a Cultural Center, despite a more expensive one was included in the construction plan.
	Borderline categories	Irregular unexecuted budget (unexbud)	When evidence is found that resources originally targeted to a particular end were not used. For example, in Morro Agudo, SP a covenant with the federal government fixed a specific amount to be spent on outpatient services in a municipal Hospital. The Hospital spent 4,759.36 reais less than the specified amount.
		Spending in other projects/services tan specified (pecu)	When public resources are used for a different end than originally targeted (and not for private ends, which would qualify the act as diversion of funds). For example, in Bastos SP expenditure for medicine was carried out through the Municipal Ministry of Social Pormotion (and not through the Municipal Ministry of Health, the proper institution for such expenses).

Performance	Sum Performance failure	Quality of service provided inferior tan specified by law (infqual)	When a deficient quality is found in the provision of a locally-delivered service/good/program. In particular, when it does not reach the minimum level allowable by the relevant regulation/covenant/law. For example, in Mauá SP inappropriate storage (conductive to faster expiration of the products) of medicine in the Municipal Health Unit was reported.
		irregularities in the operation of government programs (opirr)	When evidence is found on irregularities in the operation of locally-implemented programs. In Sao Felix BA, for example, a number of elegible beneficiaries were excluded of the program "Programa Bolsa Família" because of negligence by the local Ministry of Health or lack of coordination about the proper eligibility criteria at the local level
		Failure to comply with the program covenant (with the federal government) (failcontract)	When the covenant with the federal government is violated. For example, in Bastos SP a covenant was written to restore a Historical Museum. After the restoration was made and the resources executed, the main problem (continuous flooding) had not been solved, as was stipulated in the covenant.
		Failure to comply with the administrative terms of covenant (with government entities) (adminfailgov)	When administrative requirements specified in covenants with other government branches—especially with the federal government—are not met. For example, in Valentim Gentil SP the guidelines for an epidemiological program were not presented, blocking proper evaluation of performance.
		Labor irregularities (laborirr)	When labor irregularities are present. In particular, when the social security contribution is not retained/collected.
	Sum Admin. deficiencies	Irregularities in administrative processes (adminirr)	When administrative faults are found in the implementation of a program or in the administration activities in general. For example, in Tarabai SP, at the time of the audit the term limit for the members of the Rural Development Council was already overdue, and yet they were still in functions.
		Administrative irregularities associated with the procurement process (adminirrprocu)	When an administrative irregularity in the procurement processes that does not fit the corruption categories is found. For example, in Tarabai SP the Food Council did not participate in the procurement process, when it should have, according to regulations.
		Administrative irregularities associated with the use of resources (adminirrres)	When the verification of a possible corrupt act was made impossible because of administrative irregularities; in particular, when the reports include modified levels of aggregation for receipts or prices. For example, in Tarabai SP neither the quality nor price of meat acquired was reported, making it impossible to verify the presence of over-invoicing.
		Administrative irregularities associated with financial accounts (adminirraccount)	When irregularities related to the use of financial accounts which could suggest faulty use of resources arise. For example, in Bastos SP the city account balances do not match the reported dates of spending. However, no direct evidence of corruption associated with these financial movements was found