Revealed Corruption, taxation and fiscal accountability: evidence from Brazil

Abstract

Fiscal contract theories hypothesize that government performance affects tax collection and that institutions that foster representation and accountability link taxes and services. We use randomly generated municipal audit reports with objective measures of corruption from Brazil to assess whether new information about corruption affects municipal property tax collection and the structure of fiscal institutions. We find short-run effects consistent with this theory: property tax revenue rises with clean audit reports and falls as revealed corruption increases; furthermore revealed corruption increases the probability that a municipality adopts participatory budgeting. Our results indicate modest demand-side constraints on taxation and budgetary institutions.

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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. These propositions find considerable support in laboratory experiments and aggregate data, but they have yet to be tested rigorously with objective measures of corruption and relatively disaggregated data in a real world setting. This paper uses randomized auditing reports from the Brazilian federal government to test fiscal contract-like hypotheses about these relationships with a relatively high level of precision. The reports detail the way in which municipalities spent federal transfers: were projects finished, were there overpayments; were contracts awarded without bids, did politicians pocket the money, etc.?[1] We use the content of these reports to see whether revealed corruption affects local tax collection and/or affects the probability that a municipality will adopt Participatory Budgeting (PB) in the future.

The primary contribution of this paper is to use a high-quality research design with relatively disaggregated and objective real world data to demonstrate the link between exposed corruption and taxation. Our research design leverages both randomized selection and randomized timing of the audits, allowing us to make reasonably sound inferences. A second contribution is to reveal the link between revealed corruption and fiscal institutions. A third is to highlight the importance of high-quality information in terms of mediating the relationship between citizens and government. Our primary finding is that property tax yields fall as revealed corruption increases and rise when audits reveal little to no corruption. The main result, obtained with unit fixed-effects estimators, is relatively robust with several measures of corruption and taxation and with municipal time-trends. Placebo treatments of audit reports with prior property

[1] The audit reports and process are aptly described in Ferraz and Finan (2008, 2011); Brollo (2010); and Litschig and Zamboni (2014).

tax collection indicate no systematic difference before the audit, suggesting that the content of the audit reports only affects subsequent property tax collection. Although the effect is short-lived, the magnitude of the effect of revealed corruption would not be trivial for the multitude of municipalities that collect little revenue from property taxes. The difference in tax collection between municipalities audited before and after the 2004 election—assigned randomly to groups through the auditing process—is small and insignificant, indicating that elections are not the only mechanism at work.

Using a less rigorous research design, we also find evidence that increases in revealed corruption substantially increase the probability that a municipality will adopt PB in the future, suggesting that PB is viewed as a mechanism for increasing accountability. Placebo treatments indicate that just being audited does not affect the probability of PB adoption. The relationship between revealed corruption and PB adoption is unaffected by whether or not the audit was released before the election.

Our paper provides evidence that there are demand-side constraints on tax collection and fiscal institutions and suggests that the audits engender some increase in fiscal accountability: property tax revenue and fiscal institutions fluctuate based on the information citizens have about the integrity of their government. While the results are consistent with fiscal contract reasoning, they are only robust over the short-run. The general absence of persistence may indicate that re-occurring access to quality information is critical to maintaining a tight relationship between government performance and taxation, something Bobonis, Cámara Fuertes and Schwabe (2013) highlight in their work on long-run political accountability in Puerto Rico. Even with this caveat, the results are important. Given that municipalities collect a relatively small share of total revenue, Brazil could be an unlikely case for finding a fiscal response to revealed corruption;

nonetheless, our results are reasonably consistent with other papers indicating fiscal-contract-like tendencies at the local-level in Brazil (Schneider and Baquero 2009; Gadenne 2011).[2] They are also consistent with related work on Mexico that looks at the response of property taxes to the provision of local public services (e.g., Gonzalez-Navarro and Quintana-Domeque 2014).

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. One crucial conjecture is that states operate like fiscal contracts, exchanging services and policies for revenue (e.g., North 1981; Bates and Lien 1985; Levi 1988; Timmons 2005; and Brautigam 2008). Fiscal contract theories rest on the premise that states are not endowed with sufficient coercive power to impose their will on society. Instead, they must bargain with citizens for revenue. Because there are supply-side constraints on government power and information, tax yields depend on citizen willingness to accept them (Kirchler, Hoelzl and Wahl 2008).

Furthermore, fiscal contract theories posit that at least some tax compliance is quasi-voluntary (Levi 1988). That is, people accept taxes partly 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 public benefits relative to their contribution. Absent those beliefs, the cost of enforcing compliance will be higher (see Andreoni, Erard, and Feinstein 1998 for a

[2] Gadenne, in particular, details the connection between taxes paid and services rendered.

summary of tax compliance).[3] Hence, holding state capacity and tax effort constant, states that better satisfy taxpayer preferences will raise more tax revenue. In other words, the more governments ask citizens to pay in taxes, the more that citizens will expect from government.

Fiscal contract theories predict equilibrium relationships between taxes and services and a general correspondence between the distribution of taxes and the distribution of services, but only partially explain why bargains (in effect, why the supply and demand side constraints) vary across units in levels and structures (i.e., the tax mix). They also incompletely identify the mechanisms that consummate such bargains. Five general mechanisms, in particular, have been highlighted as devices that move units along the contractual line: external threats/war, voting, political parties, and labor market organizations are thought to increase taxes and services, while government corruption (Bratton 2012; Goodspeed 2011) and windfall revenues, say in the form of oil or foreign aid, are thought to reduce them (Brautigam 2008; Eubank 2012). Whereas the first set of mechanisms engage citizens with the state, the second set of mechanisms undermine citizen confidence in state officials and reduce governments need to bargain for revenue, thereby diminishing government fiscal and political accountability (Moore 1998; Morrison 2009). Elections are especially important because they are a low cost, regularized and inclusive means for citizens to sanction/reward politician performance. Because they generally enhance citizen control over taxes/expenditure, they not only help maintain the tax/spending equilibrium, but can push it upwards as better government enhances services and induces tax compliance.

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[3] Scholars working in the fiscal contract tradition generally equate compliance with the amount of money a taxpayer ultimately cedes to the government, regardless of the statutory rates in effect, largely because rates are endogenous.

Fiscal contract approaches to understanding the state find support in labs, public opinion research and real world fiscal data. Laboratory experiments, for example, consistently show that the belief that tax revenues are spent on things citizen's value shapes individual compliance decisions (Andreoni, Erard and Feinstein 1998; Bodea and Adrienne LeBas 2014). There is also considerable public opinion data consistent with fiscal contract approaches (e.g., Levi, Sachs and Tyler 2009; Ali, Fjeldstad and Sjursen 2014). Likewise, it is not too hard to find cross and within 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 (e.g., Ghura 1998.) Furthermore, there is also evidence in favor of the contractual mechanisms mentioned above (e.g., Kato 2003; Thies 2005; and Cusack and Beremendi 2006). Finally, the three-way macro-level correlation (between citizen stated willingness to pay taxes, mechanisms of citizen control over government, and citizen perception of government spending) also has support at lower levels of aggregation (e.g., Olken 2010 and Gibson and Hoffman 2007).

Even though data correlations are fairly well established, fiscal contract research has not generally reached the level of rigor or sophistication that one finds in some other areas of social science (Slemrod and Weber 2012). As a result, we still do not know with much precision whether more tax revenue causes better performance, whether better performance increases tax revenue, or whether omitted variables are causing both. We also do not know the role played by information in aligning taxes and performance.

Our research design and dataset allow us to test several hypotheses about the relationship between taxation, revealed corruption and accountability with greater reliability. Hence, one key contribution is inference quality, especially for taxes and revealed corruption. Our right hand-side measure of corruption is particular noteworthy, as much of the existing literature on corruption and public finance (e.g., Tanzi and Davoodi 1997; Ghura 1998) uses subjective measures of corruption. Because such measures (e.g., Transparency International) are probably influenced by tax rates/levels/evasion, endogeneity almost certainly biases existing estimates.

Second, we contribute to the understanding of the role that information plays with respect to taxes and performance. That is, although information mediates between taxes and performance, few have studied it explicitly.[4] While it is widely recognized that informational shortcomings should generate disconnects between the actual level of services/corruption and the actual amounts paid by citizens, it is generally assumed (rather than tested) that people have sufficient information about their tax burden and government performance for fiscal contracts to exist. While this assumption may be reasonable in the sense that good institutions may lead to more transparency and higher compliance, it essentially relegates information to the error term. With our research design, we can identify the effects of new information on fiscal outcomes. We can also see if the effect is stronger when information is revealed right before elections.

Third, we add participatory budgeting to the aforementioned list of general contracting devices. 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 of money are fairly substantial, and the decisions relatively binding, participatory budgeting should increase citizen knowledge of and control over

[4] Important exceptions include Levi (1988); Bowler and Donovan (1995); Paler (2013); Ortega, Ronconi, and Sanguinetti (2013); and Castro and Scartascini (2013). To our knowledge, this is the first paper that attempts to detect tax and institutional responses to corruption using a high-quality measure of corruption and relatively clean research design.

taxes and spending relative to other frameworks (e.g., via elected representatives). Increased citizen control over resource allocation should decrease government corruption (Zamboni 2007), improve government performance, and increase citizen satisfaction with government spending, thereby increasing tax revenue. Boulding and Wampler (2010), for example, show that PB is associated with increases in health and education spending; Goncalves (2014) shows that it is also associated with a decline in infant mortality; and Schneider and Baquero (2009) show that PB is associated with higher citizen satisfaction with government and a greater professed willingness to pay taxes. In other words, following revelations of corruption, governments have incentives to adopt PB to restore their credibility with taxpayers.

Finally, our paper contributes the burgeoning literature on the rise and types of accountability in democratic Brazil. Ferraz and Finan (2008) use the audits to show that revealed corruption decreases re-election rates; they also show the existence of re-election incentives curb corruption (2011). Our results show that exposing corruption via audits facilitates fiscal accountability over some range and in several dimensions. Alston et.al. (2013) have emphasized the changing social contract in Brazil, including greater acceptance of direct and indirect redistribution, while Arvate (2013) shows that greater local political competition in Brazil has increased the output of public goods.

Section II: Data and Research Design

In this part of the paper, we focus on explaining the research design and data that allow us to test two hypotheses consistent with fiscal contract reasoning: 1) tax revenue should react to revealed corruption, rising with good reports and falling with negative reports; and 2) higher levels of revealed corruption will increase the demand for fiscally accountable institutions, notably PB.

Later, we will also discuss potential mechanisms, including elections/electoral manipulation. Before setting up the hypotheses in specific terms, we discuss the data and research design.

We have a panel dataset. The panel consists of (nearly) the universe of Brazilian municipalities from 2001-2008. There are two groups within the population: a group that is audited and a group that is never audited. The audits are randomly assigned to units and in time. The research design described in detail below leverages the randomized timing of the audits' release.

The audits: process, sample and coding

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.[5] As Ferraz and Finan have shown (2008; 2011), the audits are relatively objective sources of information, meaning that citizens have the opportunity to re-assess the acceptability of their tax burden with high quality data about their government's performance.

To eliminate incentives to map inputs onto a set of outcomes, we separated data construction from data analysis. We were able to hire one high-quality coder for the project. He

[5] Given space constraints and volume of work on the subject, we refer readers to the papers referenced in FN1 and the CGU's webpage (www.cgu.gov.br) for details about the process.

coded 369 of the 400 audits conducted in 2004, the year with the largest number of audits. [6] He created a series of variables that reflect different types of corruption, based on guidance from us and the Ferraz and Finan (2008) categories.[7] Those variables are briefly described below. Appendix II contains a complete set of variables with longer definitions, examples and a summary of the coding process/procedures.

There are several potential sources of selection bias arising from our audited sample. One threat is that the audits themselves are not random. Previous work (e.g., Ferraz and Finan (2008, 2011); Brollo (2010); and Litschig and Zamboni (2014)) has convincingly rejected that hypothesis; in other words, there is no evidence that the Brazilian government was stacking the

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[6] Multiple independent coders and more audits would have been preferable; we, unfortunately, had a hard budget constraint, limiting our capacity to hire coders. Given that our sample seems representative, we chose not to (potentially) contaminate the results by coding more audits ourselves. Presumably the main threats to inference from these constrains are selection bias (discussed in text) and measurement error. Because the coder never saw outcome data, we have no reason to believe that there are systematic coding errors correlated with outcomes. Given the number of audits, random error, however, probably exists; random error should generate attenuation bias, dampening down our estimates. Appendix II has more details.

[7] Note that the audits also contain information about the performance of local officials on dimensions besides corruption. Although we hypothesized that poor administrative performance might affect taxation (wasted money is wasted money, regardless of how it is wasted), we did not obtain statistical significance with any performance measure besides corruption. This (non)-finding has also been reported by others using similar data (e.g., Litschig and Zamboni (2014)). More explanation of these results can be found in the conclusion and appendices.

deck.[8] A second threat is that our sub-sample of audits is not random. Tests of means of our coded sample vis-a-vis municipalities audited but not-coded (shown in Table 1A) reveal no differences in observables used in the analysis, including the principle left-hand-side variable.[9] In other words, our sample seems to approximate a random sample of a random sample. A third source of selection bias could be missing values. That is, although we coded 369 audits, a few municipalities are missing some data, meaning our effective sample without controls drops to 340 treated units (338 with controls). Tests of means with available variables (e.g., property taxes, GDP per capita, population, corruption, etc.) prior to the audit also indicate no differences between the cases with and without complete data. Of our audited sample, 249 were released before the first round of municipal elections in 2004; 13 were released between the first and second rounds of elections; and 105 were released after the elections had finished (21 of this final set were published in the first quarter of 2005).

Measuring corruption

As detailed in Appendix II, our main measure of corruption (labelled Total Acts of Corruption) is an aggregate of the three primary and most obvious corruption "technologies" identified by Ferraz and Finan (2008): diversion of funds; over-invoicing for goods and services; and irregularities in the procurement process, including fraud and no-bid contracts. In raw counts and as a percent of effective number of audits (defined below), our figures are slightly larger but still

[8] Audit probabilities are equal within states, but vary slightly across states (Litschig and Zamboni 2014).

[9] Municipalities audited after 2004 but not coded were excluded from the analysis; given the randomized nature of the audits, this sampling restriction should not induce bias. Results are unaffected by their inclusion.

comparable to Ferraz and Finan (2008) and Brollo (2010).[10] Table 1B provides summary statistics of the corruption measures.

All variables are normalized in count form relative to the effective number of audits conducted, in effect creating a corruption rate. This normalization takes into account both the fact that the same event may have multiple audits in different dimensions and the fact that the number of audits varies across municipalities. (We think this normalization is sensible, but our results are virtually identical with the raw count). To avoid double-counting, we only classified an event once (based on the gravest offense). The count variable has no missing observations: 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.

We also created quantity variables for each category based on the stated amount missing, stolen, not subject to bidding, etc. We normalize these in per capita terms and in terms of the total amount audited, using the square root, the natural log, and categorical variables for different monetary increments.[11] The quantity variable takes into account magnitude, but is not as clean as the count variable because the 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

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[10] As shown in the summary statistics, 88 percent of municipalities have at least one act of corruption, narrowly defined, and the average municipality had 3.7 acts of corruption.

[11] The corruption quantity is right-skewed. We used multiple transformations because we do not know the functional form of the relationship between tax revenue (logged) and corruption. We make the square-root the benchmark because it requires no arbitrary decisions about how to treat zeros (0 squared is 0). To prevent the log measure from dropping observations that take on the value of zero, we added 1 to all observations and then took the log ((log(value+1)).

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. Our results are unaffected by a variety of alternative adjustments, such as dropping these observations or assigning them the sample mean corruption amount.

As part of the sensitivity analysis, we also employ categorical variables for different corruption levels and amounts (low, medium and high), along the lines used by Brollo (2010). The categorical variables (defined in Appendix I) are useful if people merely classify their government as good, ordinary, or very corrupt.

The initial hypotheses we test are as follows:

> Hypothesis 1: Tax revenue should respond to revealed corruption, rising with clean audit reports and falling with increases in revealed corruption.
>
> Hypothesis 2: Higher levels of revealed corruption should generate demands for accountability in the form of participatory budgeting.

These hypotheses rest on several assumptions. The first is that the audit reports reveal new information to citizens about the relationship between taxes and services, allowing them to update their priors about politician performance and forcing politicians to reconsider the tax burden they seek to impose. If the audit indicates that politicians are performing worse than expected, revenue should fall; if politicians are performing better than expected, revenue should increase. In other words, even though the audit concerns federal transfers and incomplete measures of corruption, it sends a general signal about the extent of corruption/quality of government that relaxes/tightens the demand-side constraint on taxation.[12]

[12] As long as some citizens interpret the audit as a general signal of the actual level of corruption, one might be able to detect a response. Presumably, audits of goods/services financed purely by

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, our primary models include municipal fixed effects.[13] The fixed-effects also control for state capacity, which is assumed to be time-invariant over short-time horizons. The municipal-level fixed-effects mean that we are identifying off of the within-municipality variation based on differences before and after the audit. In effect, when corruption is revealed randomly, do taxes respond? Note that if citizens believe that politicians in a municipality are relatively clean/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 accounted for ("priced-in") will be embedded in the fixed-effects and/or lagged values of the dependent variable (not generally employed). Note also that we include municipal-specific linear time trends as part of the sensitivity analysis. The time-trend should also absorb at least some time-varying factors associated with state-capacity and prior beliefs.

Second, the hypotheses assume that local governments would, in principle, like to maximize tax collection, meaning that they have incentives to acquire as much revenue as they can, given their capacity and demand-side constraints.

Third, the hypotheses assume that this information is credible and sufficiently well-disseminated to generate a response. Ferraz and Finan (2008), for example, provide considerable evidence showing that the information reached voters and was critical in local elections; they show that revealed corruption had a direct effect on electoral outcomes and show that this effect

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local taxes might send a more specific signal and, hence, might generate a larger response. Likewise, one could imagine the magnitude of the response increasing as tax burdens increase.

[13] Alternative econometric ways of dealing with priors are discussed later.

increased with the number of local radio stations.[14] More importantly, Brollo (2010) used municipal fixed-effects specifications without radio station data (as is done herein) to show that the effects of audits on electoral outcomes do not hinge on the number of radio stations. In effect, radio stations augment the effect of revealed corruption, but do not cause it.

Finally, these hypotheses assume that the relationship between revealed corruption and outcomes is not contingent on intervening events, notably elections. Later, we will modify this last assumption to see if and how the specific timing of the audits' release matters.

The dependent variables

We have relatively comprehensive annual data on sub-national revenue, including disaggregated tax and transfer categories for 2001-2008, taken from the Finance Ministry's on-line database (FINBRA, 2010), which is generally accepted as the most complete and reliable source of subnational data.[15] In Brazil, municipal revenues come primarily from three sources: local taxes and fees, shared taxes and fees, and pure 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

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[14] Obviously, modelling the radio-station channel allow for a more nuanced understanding of the relationship between media and fiscal accountability; it is difficult to do so with panel data and municipal fixed-effects, however, as the radio-station data are cross-sectional and the radio-station intercept would be consumed by unit-level fixed-effects.

[15] FINBRA covers around 90 percent of municipalities in any given year. As Afonso (2010) details, the database is high-quality, but has some inconsistencies over time, as definitions/categories change. Our primary variable, municipal property taxes, does not suffer from any definitional inconsistencies.

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 revenue. Transfers account for the rest. Two unrequited local taxes account for more than half of local revenue: the property tax on structures known as the IPTU (*Imposto sobre a Propriedade Predial e Territorial Urbana*) represents approximately 20 percent of revenue from purely local taxes, while the tax for general services known as the ISSQN (*Imposto sobre Serviços de Qualquer Natureza*) represents approximately 49 percent of local tax revenue.

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, as local governments have limited administrative capacity to monitor and sanction compliance; although the tax base is fairly broad, most municipalities collect far less than they should due to rampant evasion.[16] One IBGE survey, for example, found that only 13 percent of municipalities had

[16] The property tax does not automatically accrue to government, unlike, say, withholding taxes (e.g., most income taxes), and it does not have built in monitoring systems (like most value-added taxes), making it relatively easy to evade. According to our non-random sample of Brazilians, non-payment of property taxes is unlikely to be punished quickly, anywhere. In the few municipalities with capable tax administrations, one could face legal consequences for non-payment in several years. In many places, complications for non-payment (but not necessarily punishment) might only arise when the property is transferred. Bruno de Carvalho (2006) provides a description; he notes that around 60% of urban structures are included in the tax base.

property tax collection rates above 80 percent and a plurality (24 percent) had collection rates of 40-60 percent (IBAM 2001). Finally, the IPTU is one of Brazil's most regressive taxes.[17] In other words, it is a tax that touches many citizens and one that can move up or down depending on how people perceive their government. Our benchmark is the logged level of per capita property tax revenue. We supplement this measure with the property tax to GDP ratio.

We do not use the local service tax (the ISSQN) as a primary measure because the tax base and rates 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 to be taxed, something that, presumably, is not randomly distributed across municipalities. Because there is an unobserved confounding factor (a change in the tax base) that occurs simultaneously with the audits, we cannot attribute changes in tax collection solely to the revelation of information about corruption with confidence. Appendix I reports results with alternative measures of taxation, including total local tax revenue, *taxas*, *contribuições*, and the ISSQN.

Control variables

Tax receipts could be driven by a number of factors. To control for state capacity, state motivation, people's priors about government performance and people's trust in each other, we rely primarily on municipal fixed-effects.[18] Furthermore, we run placebo regressions with lagged levels of taxes on the left-hand side to see whether revealed corruption affected property tax

[17] For a full breakdown of the distributional content of different taxes, including the IPTU, see Rezende and Afonso (2010), notably pp. 82-3.

[18] One alternative way of dealing with priors would be to include the lagged DV on the right-hand side and difference out the fixed-effects. Our results are (very) robust with such models.

revenues before the audit was published. In our regressions, we attempt to control for other things that could affect tax collection. Our main control variable is GDP per capita (logged), which should be a crude, albeit imperfect, proxy for the economy. We also include population, largely because municipal size correlates strongly with revenue. Furthermore, because revenue not collected from property taxes could be offset by other revenue, we included total transfers from other governmental units and revenues from other local taxes as part of the sensitivity analysis. Neither transfers nor other local revenue is included in our benchmark specifications because of endogeneity concerns, but including them does not change the point estimates or standard errors on the corruption variables. Our specifications are sparse, partly because there are few municipal level covariates with annual data that change quickly and that one could also link theoretically to tax collection.[19] To control for additional time-varying factors that might have been omitted, we include municipal-trends as part of the sensitivity analysis. Our regression models are as follows:

$$(1) logIPTU_{it} = \gamma \cdot audit_{it} + \beta X_{it} + \delta_t + \lambda_i + \varepsilon_{it}$$

[19] Participatory budgeting, for example, was sufficiently collinear with the fixed-effects to be dropped from the models. Likewise, we would have liked to include urbanization rates and inequality, but annual data are unavailable. Because they are slow-changing, they should be captured by the fixed–effects and time-trends. Although resource rents are not important in most municipalities (Caselli and Michaels 2012), we regret not having such a measure.

Where $audit_{it}$ is a categorical variable for whether or not the audit report was released for municipality i at time t;[20] X is a vector of control variables; δ_t is a year fixed-effect; λ_i is a municipality fixed effect; and ε_{it} is the error term. Under the assumption that $E(\varepsilon_{it}|audit_{it}, \delta_t, \lambda_i, X_{it}, \delta_t, \lambda_i) = 0$, provided by the randomization of the audits, the estimate of γ will be consistent. In the regressions shown, we cluster the standard errors at the municipal level. Using Newey-West standard errors, Huber-White standard errors, or clustering at the state level does not appreciably change the reported results. Note also that lagging the right-hand control variables by one period to ensure contemporaneous exogeneity yields virtually identical results.

Section III: Results

Corruption and Tax Property Taxes

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. The point estimates on being audited are generally small and negative (suggesting that, on average, the audits reveal bad news), but they are far from significant.

[20] One obvious question is whether we would expect a contemporaneous effect of the audit. Our results are similar if we treat audits released late in the year as though they were released the following year. Our results are sign consistent, but insignificant, if we lag by one full year.

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.[21] Our main regression model is as follows:

$$(2) logIPTU_{it} = \gamma \cdot audit_{it} + \phi[audit \cdot corruption]_{it} + \beta X_{it} + \delta_t + \lambda_i + \varepsilon_{it}$$

where ϕ captures the effect of revealed corruption/performance on property tax revenues. Note that ϕ will only capture those with positive corruption values; audits with zero corruption will be captured by the γ intercept. (Alternative means of dealing with zero revealed corruption in audited municipalities are presented in Appendix I). Note, too, that the interaction always takes on the value of zero for non-audited municipalities.[22] In other words, until an audit has been published, revealed corruption (representing the interaction of revelation (0-1) and the level of corruption revealed) always takes on a value of zero; when published, the revealed corruption interaction "turns on" and takes on the value indicated by the contents of the audit; we can keep the audit "on" by including lags. We can trick the data into believing that an audit has turned on (when, in fact, it has not) by including leads of revealed corruption. These placebo models reverse the temporal ordering, allowing us to confirm that taxes only respond following the audit, not before it. Note also that the never audited municipalities are included in the regression models because they allow us to more precisely estimate the audit intercept; our primary results are robust if we work with a restricted sample of only audited. Assuming that $E\left(\varepsilon_{it} \middle| audit_{it}, audit \cdot corruption_{it}, X_{it}, \delta_t, \lambda_i\right) = 0$, the estimate of ϕ will be consistent. In other

[21] Given the random nature of the audits, dropping the audited (but non-coded) municipalities in years other than 2004 should not induce bias in our estimation.

[22] The inferences do not change if we assume that non-audited municipalities have the same mean corruption value as the audited ones.

words, conditional on being audited, the effect of revealed corruption will be consistently estimated if there are no time-varying unobservables correlated with revealed corruption and the relationship is correctly specified.

Table 3 presents the primary results in both count and quantity form.[23] Note that all variables labeled "corruption" represent the interaction term of corruption and the audit. Columns 1 and 8 present the primary result without controls. 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 placebo treatments using the forward value of corruption (that is, corruption_{t+1} against taxation_{t}; we would expect no response to the forward value because the level of corruption has not yet been revealed.) Columns 6-7 and 13-14 include the contemporaneous and forward value of corruption, allowing us to test for a before/after break when the level of corruption is revealed. Note that the point estimates and standard errors on the corruption variables barely change across models, despite changes in the sample; with a constant sample (available), the results have even less variance.

With the count measure of corruption, the audit intercept is positive, albeit not quite significant: clean audit reports correspond with more revenue, indicating the relaxation of a demand-side constraint. The contemporaneous audit*corruption interactions, by contrast, are negative and significant, indicating that revenue falls as revealed corruption increases (see Figure

[23] A small amount of serial correlation is present in our baseline models (Table 3). Clustered standard errors should be fully robust to cross-sectional heteroskedasticity and within-panel (serial) correlation, given the large number of clusters. Explicitly correcting for serial correlation with an AR1 model better accounts for the memory of the tax series, but raises thorny endogeneity concerns; it yields more robust results, including persistence (see Appendix I).

1). With the benchmark quantity measure (the square-root of the quantity), the intercept is positive and the interaction negative. Although neither is significant at conventional levels with the benchmark measure (the square-root), both are significant with the log (the stand-alone coefficient/SE are shown in the bottom row of Table 3) and with categorical measures (see below and Table 3A). In other words, the magnitude of misappropriated funds matters when measured as a proportion and a step-function, although it is not significantly different from zero with all conceivable functional forms. The placebo regressions indicate no relationship between current audits and lagged property tax revenue, suggesting that corruption has not been fully priced-in.[24] Wald tests of the contemporaneous values of corruption versus the forward value generally suggest a break with the audit.[25]

The coefficients on revealed corruption are sizable: conditional on being audited, a one standard deviation increase in the corruption rate translates into a 10 percent fall in property tax collection, or roughly 1 real per capita.[26] The magnitude of the effect with the quantity measure is virtually identical. By way of comparison, the 50th percentile for IPTU per capita is only 3.7 reais, while the 75th percentile is 7.9. In other words, the effect would be meaningful in the large

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[24] Our comparison is within municipalities, meaning that corruption is priced-in through the fixed-effects.

[25] With the count variable, we can generally distinguish the current audit*corruption variable from placebos, including forward values such as T+2 and T+3.

[26] In models not shown, we included three-way interactions of audit, corruption, and the other right-hand side variables: per capita income, population and transfers. The revenue loss associated with corruption is greater (but not robust) in municipalities with lower per capita incomes and lower initial tax collection.

number of municipalities with relatively low property tax collection, but not in places that already collect substantial property tax revenue.

The primary results are robust to re-sampling, such as round-by-round exclusions. They are also robust with other control variables, such as transfers and other taxes, which we have excluded because of endogeneity concerns.[27] Tables 3A-H in Appendix I present sensitivity analysis, including a variety of alternative left and right hand-side measures and additional explanation. The main results to highlight can be found in Tables 3A, 3B and 3F. Table 3A uses categorical variables for different levels of corruption defined by percentile cut-offs (low, medium and high). With categorical variables, the point estimates are generally consistent with fiscal-contract hypothesis: high corruption equates with the highest average revenue loss, medium corruption with no effects, and low corruption with small and (generally) statistically significant gains. One will notice, in particular, that high corruption is negative and significant in both counts and quantities; low corruption is consistently positive, but only significant in quantities. Table 3B shows all of the main models with municipal-specific time-trends as a control; our main results remain similar across models and the categorical variable for low corruption is now positive and significant in both counts and quantities. Table 3F shows results with all local tax revenue (except the ISSQN); the primary results do not change appreciably.

The main qualification is that the duration of the effect is limited: the cumulative effect of revealed corruption remains negative over three periods, but it is far from significant with the primary models. We find limited evidence of persistence (up to t+2) with some alternative

[27] Brollo (2010) finds that transfers decline following revealed corruption, conditional on party alignment. Like us, she finds that audits have no effect on total local tax collection, but unlike us, she does not disaggregate local taxes and/or strip out the (potentially contaminating) ISSQN.

models/measures, notably with AR1 correction, but the range of data supporting persistence seems to be small. To the extent it exists, persistence is only present for high corruption and never for low corruption. The general absence of persistence is consistent with a multitude of similar studies. Brollo's (2010) replication of Ferraz and Finan (2008), for example, found that audits only affected elections when they were released shortly before the election, something Bobonis et. al. (2013) also highlight in their work on Puerto Rico. Likewise, Richter, Sampantharak and Timmons (2009) found that the effect of lobbying on taxes is short-lived. Nevertheless, it is puzzling and potentially problematic. A rapid bounce-back would be expected if politicians' behavior and/or municipal services changed quickly in the wake of the audit; otherwise, we would expect it to endure for some period of time. Unfortunately, we do not have data allowing us to assess whether the effect is ephemeral because new information comes along (in effect, cancelling out the signal sent by the audits), because governments change policies in response to the audits (presumably, most elected governments will respond by trying to do more for citizens), because the politicians themselves change, and/or because citizen attention evanesces rather quickly.[28] All three are possible. Before returning to the issues of mechanisms and persistence, we first examine whether revealed corruption also affects the adoption of PB.

[28] Our data bracket the election so there is electoral turnover in the sample, partly due to revealed corruption. Unfortunately, the samples of pre-election audits for different categories (re-elected, not re-elected, and ineligible for re-election) are very small. Given the small samples, we decided not to test for differences (notably in persistence) among these groups, partly because the theoretical expectations are not obvious. If new mayors start with a clean-slate (something that seems plausible) it would imply bounce-back. If corrupted and re-elected perform well ("*rouba*

Participatory budgeting

Table 4 examines the adoption of participatory budgeting. The presumption is that poor audit results will generate demands for the adoption of more transparent budgeting procedures. 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 (something that should capture many of the time-invariant properties of municipalities) and controls for party.[29] The regressions shown have a categorical variable for Left party as the winner of the 2004 elections; using individual parties, notably the PT, does not meaningfully alter the interpretation of the results.[30] While a more complete set of covariates might be desirable, pre-existing work finds no robust predictors, except political party (Wampler 2010).

We start with regression models (Models 1 and 2) that see whether merely being audited is correlated with either PB or the adoption of PB from 2005-2008. Our expectation is that merely

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mas faz”), then a bounce-back might also be expected. Follow-up questions like these would clearly be worth exploring with a larger dataset.

[29] One obvious question is whether revealed corruption is endogenous, especially given the lagged dependent variable. Although we do not have a rigorous way of eliminating the threat of endogeneity (e.g., instrumental variables), we can find no evidence that having PB in 2004 (or ever having had PB) relates to the level of revealed corruption. We omit these tests to conserve space, but details are available on request.

[30] The Left variable shown includes the Worker’s Party (PT), the Communist Party (PC do B), the Socialist Party (PSB), the Popular Party (PPS), and the Worker’s Democratic Party (PDT). Although PB started as a PT initiative, as many as nine parties have adopted it (Souza, ND).

being audited should not matter. The cross-sectional regression model that assesses whether the content of the audit matters for the adoption of PB is:

$$(3)\ P(PB_t = 1) = \Psi[\beta_0 + \beta_1 \mathrm{PB}_{t-1} + \beta_2 \mathrm{corruption}_{t-1} + \xi \cdot \mathrm{X}_t]$$

Note that the adoption of PB is a relatively rare event, like interstate wars, expropriation, and revolutions. Only 10 audited municipalities adopted it between 2005 and 2008; nine of them are in our sample. Hence, 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).

Not only are the aggregate corruption count and quantity measures positive and highly significant with the adoption of PB but so are many of the disaggregated categories. Obviously, omitted variables are a concern due to the cross-sectional research design. Given the significance levels of the corruption measures, however, the omitted variables would have to be fairly highly correlated with both corruption and the adoption of PB to wipe-out the results. 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 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 (0.5-0.6 percent in any given year). In other words, even if PB does not matter substantively (clearly debatable), municipal governments that are exposed as being more corrupt respond by adopting PB at much higher rates than other municipalities; such measures should help restore their credibility with taxpayers.

Section IV: Discussion of results, mechanisms and confounding factors

Using a research design that exploits the timing of randomized audits, we found considerable evidence that property taxes co-vary with revealed corruption: property tax revenue rises with low levels of revealed corruption and falls as corruption increases. The effect is substantively meaningful, albeit short-lived. We recognize that the principle threat to this inference is that corruption is not randomly assigned, meaning that some time-varying municipal-level factor not in the regression models could be contaminating our results. We see no obvious omitted confounding variables (that would be both positive with low corruption and negative with high corruption) and attempted to control for such variables with time trends for each municipality.

The most obvious lingering question is what mechanism connects levels of revealed corruption with changes in property tax revenue? In principle, we believe that both demand (compliance changes on the part of citizens) and supply-side (rate/enforcement changes on the part of politicians) stories can be consistent with the assumptions detailed earlier. That is, while spontaneous increases/decreases in citizen compliance rates obviously square with fiscal contract theory, supply-side explanations are not incompatible with fiscal contract reasoning. Specifically, if the audits reveal that government is clean, it could signal that services exceed taxes, relaxing the demand side constraint. Revenue maximizing politicians could then use the legitimacy provided by the audit to try to increase revenue. By contrast, if revealed corruption signals that taxes charged exceed services provided, then politicians have incentives to restore the tax/service equilibrium by increasing services and/or reducing tax rates and/or lowering enforcement. The key issue is that politicians take these actions based on new information about the state of the tax/services equilibrium.

To distinguish between supply-side (rate/enforcement changes on the part of politicians) and demand-side (compliance changes on the part of citizens) equilibrating mechanism is well beyond the scope of this paper, largely because we would need detailed auxiliary municipal-level data on rates/enforcement and micro-level data on individual/household compliance.

Elections as a mechanism

One legitimate concern, however, is that we have neither theorized nor modelled the interaction between revealed corruption and elections, something that could be a problem because we have pooled pre- and post-election audits. Theoretically, we believe that elections should be seen as an additional constraint. Elections are a contracting mechanism that can enhance citizen control/representation, much like PB. Hence, it is perfectly compatible with the theory outlined previously that the combination of elections and revealed corruption could have a larger effect than revealed corruption alone. The main falsifying threat posed by elections, however, is that politicians who are "caught" strategically lower rates and/or reduce enforcement, but only because an election is imminent. In other words, we would like to discriminate between an electoral response that could be compatible with fiscal contract reasoning and pure electoral manipulation which is not.

Unfortunately, we do not have means of parsing out these observationally-equivalent stories. Instead, we merely assess whether our results are purely an electoral phenomenon, by allowing the effect of disclosed corruption to vary between municipalities audited before and after the local elections. Given that politicians exposed before elections have the greatest incentive to adopt epiphenomenal measures that only have a short-term effect (they just want to get through the election), this test may also shed light on the causes behind the absence of persistence. Because the specific round a municipality is audited in is randomly assigned by a lottery (which draws municipalities with replacement in each round), the introduction of a group-specific variable can credibly be taken as exogenous.[31] The model takes the following form:

[31] Ferraz and Finan's (2008) paper exploits the random timing of the audit with respect to municipal elections.

$$(4)\ logIPTU_{it} = \theta_1 \cdot pre\ audit_{it} + \theta_2 \cdot corruption_{it} + \theta_3[pre\ audit \cdot corruption]_{it} + \beta X_{it} + \delta_t + \lambda_i + \varepsilon_{it}$$

where $pre\ audit_{it}$ is an indicator that takes the value of one if the audit was released prior to the mayoral election and zero otherwise, $corruption_i$ is the disclosed level of corruption for municipality i in period t, X_{it} are municipal and year specific covariates, δ_t are year fixed effects and λ_i municipality fixed effects. This model allows for a clean test of the difference between the magnitude of the response of taxes to revealed corruption between the two groups. If municipalities audited before the election are driving our results, they should clearly display a sharper reduction in tax collection, and θ_3 should be negative.

The results are presented in Panel A of Table 5. The point estimates on the before-election audit interactions are negative, but not close to significant with either counts (Models 2 and 3) or quantities (Models 4 and 5). The point estimates indicate that having an audit prior to the election slightly reduces property tax collection vis-a-vis municipalities audited after the election, and that higher revealed levels of corruption are associated with lower tax collection in places audited before the election. In other words: the pre-election audits clearly contribute to our main result, but they not driving it. Regardless of whether the electoral results indicate the equilibration of taxes and services, or some form of pre-electoral fiscal manipulation, we know that electoral manipulation alone cannot account for the main result.

Another important question is what do the results reveal about the relationship between fiscal institutions, performance and demands for accountability? The fact that more revealed corruption affects the probability that a municipality will adopt PB is consistent with the widespread belief that participatory budgeting is superior on many dimensions to budgeting via representative institutions. This result is quite robust, subject to previous caveats. As with the property tax, we have no reliable way of knowing whether this outcome is supply or demand driven in the absence of auxiliary data (notably case studies). Per the suggestion of external readers, we decided to assess whether elections serve as an intervening variable, using the same framework as above (estimating equation (4) with PB status as dependent variable). Panel B of Table 5 reports those results.. The interaction of pre-election audits and corruption are larger in both quantities and, especially, counts, compared to the post-election baseline, which flips signs across measures. The results could indicate that the election focuses attention on corruption and the quality of public expenditure. Politicians in places with more revealed corruption campaign on promises of reform, and enact PB afterwards as a means of fulfilling their promises. (By definition, the response is not pre-electoral manipulation because it occurs after the election). In other words, even if the move to PB is driven by strategic politicians, it may be because they are reacting to 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. In this paper, we use relatively disaggregated data, with randomized selection and a reliable research design to test those propositions. Our results show that municipal property tax revenue in Brazil responds to revealed corruption, suggesting that government revenue acquisition is partly determined by what citizens know about government behavior. Our main results are reasonably robust in contemporaneous levels and substantively meaningful in magnitude. Furthermore, the point estimates with categorical variables of corruption were consistent with a fiscal contract story in terms of the magnitude and rank order of the coefficients: high corruption equates with the highest average revenue loss (and statistical significance across most measures); medium corruption with no effects and low corruption with gains. We also found that revealed corruption increases the probability that a municipality will adopt participatory budgeting, something that might have enduring consequences. Both results are relatively robust, suggesting that the audits facilitate fiscal accountability over some range. We find some evidence of electoral effects in terms of revealed corruption in terms of both property tax collection and the adoption of PB. The evidence suggests that, in neither case, are elections the sole mechanism connecting bad audit reports with fiscal outcomes.

We believe that the construct and internal validity of this paper is sound, subject to previously mentioned caveats, and the findings useful, but recognize that some issues remain unresolved. First, as detailed in the Appendices, it is important to note that we did not find that other measures of (revealed) performance systematically affect fiscal outcomes (Table 3H). Although all other measures of performance are negatively signed with taxes, none is statistically different from zero, consistent with every other paper we have seen on the subject (e.g., Ferraz and Finan 2008; Litschig and Zamboni 2014). This result could indicate that people pay less attention to other dimensions of government performance, that administrative performance has already been accounted for (in effect, performance is observed every day), and/or that the other performance measures are very noisy. (Appendix II explains why the other measures of performance are noisy, something that would induce attenuation bias..).

Second, as noted repeatedly, while the short-run effects are consistent with theory, the long-run effects are ambiguous. We do not have the data necessary to pinpoint the reasons why the effect dissipates rather quickly in most cases. While it is not inconceivable that governments improve their performance/services in the wake of the audits, the general absence of persistence raises the possibility that the demand side constraint may only be tightly binding episodically; in effect, the connection between taxes and performance can drift in the absence of repeated streams of high-quality information, a point raised by Bobonis et. al. (2013). Because we suspect that the magnitude of drift and the speed at which it occurs may be a function of the tax burden and information flows (that is, people's incentive to acquire more high-quality information increases as the tax burden rises), future studies on the subject might be designed around the manipulation of taxes and information flows, rather than just one-off interventions with a modest revenue source.

Third, we cannot yet identify the equilibrating mechanism with any precision. With more detailed municipal-level data, one might be able to parse out the changes in tax collection brought about by changes in tax rates/enforcement from those brought about by changes in tax compliance. Put slightly differently: the addition of qualitative/survey data would make this paper much better. We would love to have had surveys of people's perceptions of government before and after the audit and household-level data with tax payments over the same interval. With interviews we ought to be assess whether and how they received information about performance (in effect, deal with the intention to treat problem) and hear their explanation for whether/why their behavior changed to confirm that mental models and behaviour are consistent with the mechanism posited herein. Fourth, we are unable to separate out the triggering moment with much precision, something one could do with higher frequency data. Fifth, neither our theory nor results are nested in general equilibrium theory of public finance in which we address relevant second-order questions, notably, what happens when the government faces lower IPTU revenue. Clearly, all of these issues are relevant in terms of advancing the literature.

Finally, we cannot confirm the external validity of our study. Given that Brazil is a relatively hard case for local-level fiscal contracts, and 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, especially in settings where tax enforcement is weak. The main difference is that taxpayers in our setting are exogenously exposed to credible information about their government (and governments know that citizens have access to this information). With better information, we see a relatively rapid response between revealed corruption and property tax revenue, consistent with a fiscal contract model of the state. In the absence of these audits, by contrast, citizens still have information about government performance and can update their information based on newspaper reports and other sources. Because the baseline and the new information may not be of the same quality in other settings as they are in our setting, however, there would just be a larger margin of error.

In short, this paper uses relatively high quality data and a sound research design to establish a connection between revealed corruption and several important fiscal outcomes; it also highlights the role that information plays in connecting taxes and government performance. Although Brazil is a hard case in many ways for the propositions being tested, our results indicate demand-side constraints on taxation and fiscal institutions. While the long-run effects of revealed corruption are unclear, as we do not know whether the relative absence of persistence in our data reflects changes in performance or error creeping back in, the short-run effects are broadly consistent with fiscal contract notions about the relationship between performance, tax revenue and fiscal institutions; they also indicate that Brazil's randomized audits engender some degree of fiscal accountability. The core results are enhanced by, but do not depend exclusively on, elections, which could serve as an equilibrating device in their own right.

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Table 1A: Tests of means of main variables. Audited and coded sample vs. audited and not-coded

	Coded vs Non-Audited (minus audited but not coded)					
	Obs. audited & coded	Obs. audited but not coded	Mean audited & coded	Mean audited but not coded	p-value	t
IPTU PC (log)	341	25	1.129	1.088	0.925	0.094
ITBI PC (log)	336	27	0.802	0.229	0.104	1.631
ISSQN PC (log)	348	29	2.200	2.119	0.707	0.377
IRRF PC (log)	344	29	1.923	1.917	0.968	0.04
Sum local impostos PC (log) (IPTU, ITBI, ISSQN, IRRF)	349	29	3.383	3.173	0.226	1.213
Local impostos minus IPTU PC (log)	349	29	3.327	3.185	0.387	0.867
Sum local taxas PC (log) (Police etc.)	334	29	0.861	0.575	0.433	0.786
Total taxes per capita(log)	349	29	3.539	3.352	0.295	1.049
Total transfers PC (log)	351	29	6.450	6.344	0.215	1.241
Valid votes 2000 election (percent)	357	29	55.847	53.241	0.261	1.126
Deficit PC (log)	355	31	-1.691	-0.747	0.95	-0.063
Population (log)	351	29	9.68	9.946	0.167	-1.384
GDP PC (log)	362	29	8.597	8.403	0.215	1.243

The test of means compares 2003 values for fiscal, demographic, and electoral variables for municipalities audited in 2004 based on whether they are in our sample (audited and coded) or not in our sample (audited, but not coded). Given that the audits themselves are random, there seems to be no obvious selection problems with our sub-sample.

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
Municipalities with at least one act of corruption	Count	327	182	119	283	191	100	131
	% of audited municipalities	0.886	0.493	0.323	0.767	0.518	0.271	0.355
Corruption counts	mean	3.76	0.916	0.455	2.393	1.038	0.509	0.846
	std. dev.	3.12	1.294	0.779	2.458	1.437	1.061	1.630
	N	369	369	369	369	369	369	369
Corruption acts as a percentage of effective number of audits	mean	0.117	0.03	0.014	0.073	0.032	0.016	0.025
	std. dev.	0.105	0.047	0.025	0.077	0.05	0.034	0.048
	N	369	369	369	369	369	369	369
Amount associated with corruption as a percentage of total amount audited	mean	0.062	0.02	0.003	0.042	0.022	0.002	0.021
	std. dev.	0.112	0.069	0.018	0.086	0.052	0.012	0.07
	N	369	328	340	369	332	305	352
Per capita amount associated with corruption	mean	19.57	5.15	0.7	14.36	5.84	0.77	8.89
	std. dev.	56.61	30.50	3.33	48.62	16.35	6.69	44.89
	N	352	312	324	352	318	288	335
Amount associated with corruption	mean	388,272	137,071	13,373	254,833	121,577	9,425	145,753
	std. dev.	1,389,643	1,165,032	76,922	778,408	403,009	77,044	673,724

The effective number of audits is equivalent to the corruption rate (that is, 11.3 percent of audited events had at least one act of corruption).

Table 2: The Effect of an Audit on Property Taxes

VARIABLES	DV=Log of Property Tax Per Capita (IPTU)				
	1	2	3	4	5
Audited$_{(t)}$	-0.004	0.003			0.018
(any year)	(0.026)	(0.028)			(0.032)
Audited$_{(t)}$			-0.042	-0.042	-0.055
(our sample)			(0.048)	(0.048)	(0.055)
Population (log) $_{(t)}$		-0.948***		-0.817***	-0.949***
		(0.098)		(0.105)	(0.098)
GDPPC (log) $_{(t)}$		0.050		0.069*	0.050
		(0.039)		(0.040)	(0.039)
Constant	0.759***	9.201***	0.812***	7.858***	9.205***
	(0.011)	(1.037)	(0.012)	(1.104)	(1.037)
Observations	39,880	34,979	32,540	28,550	34,979
R-squared	0.892	0.897	0.896	0.900	0.897

Note: *<0.10; **<0.05; ***<0.01. All models use clustered standard errors, unless otherwise noted. They include fixed-effects for years and units. Audited is a 0-1 dummy, indicating whether or not a municipality was audited in a given year.

Table 3: Property Taxes Per Capita and Revealed Corruption

	DV=Log of Property Tax Per Capita (IPTU)													
	Corruption Counts							Corruption Quantities						
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Audited$_t$	0.085	0.083	0.089			0.083	0.081	0.025	0.021	0.022			0.017	0.017
	(0.059)	(0.059)	(0.063)			(0.065)	(0.065)	(0.046)	(0.046)	(0.049)			(0.050)	(0.050)
Audited$_{t-1}$			-0.065							-0.051				
			(0.076)							(0.060)				
Audited$_{t-2}$			-0.011							-0.019				
			(0.063)							(0.049)				
Audited$_{t+1}$				-0.014	-0.017	0.001	-0.003				0.005	0.003	0.008	0.006
(placebo)				(0.052)	(0.052)	(0.056)	(0.056)				(0.044)	(0.044)	(0.048)	(0.048)
Corruption$_t$	-1.127**	-1.117**	-1.129**			-1.195**	-1.195**	-0.025	-0.024	-0.023			-0.026	-0.027
	(0.505)	(0.519)	(0.508)			(0.555)	(0.557)	(0.017)	(0.017)	(0.019)			(0.018)	(0.018)
Corruption$_{t-1}$			0.607							0.021				
			(0.555)							(0.021)				
Corruption$_{t-2}$			-0.150							-0.003				
			(0.438)							(0.019)				
Corruption$_{t+1}$				-0.039	-0.030	-0.243	-0.232				-0.009	-0.009	-0.013	-0.014
(placebo)				(0.324)	(0.324)	(0.359)	(0.359)				(0.013)	(0.013)	(0.013)	(0.013)
Population $_t$		-0.817***	-0.956***		-0.789***		-0.790***		-0.817***	-0.957***		-0.790***		-0.790***
(log)		(0.105)	(0.114)		(0.107)		(0.107)		(0.105)	(0.114)		(0.107)		(0.107)
GDPPC$_t$		0.068*	0.019		0.066		0.065		0.069*	0.017		0.066		0.066
(log)		(0.040)	(0.050)		(0.040)		(0.040)		(0.040)	(0.050)		(0.040)		(0.040)
Constant	0.812***	7.862***	10.256***	0.832***	7.650***	0.832***	7.659***	0.812***	7.856***	10.278***	0.832***	7.654***	0.832***	7.656***
	(0.012)	(1.105)	(1.250)	(0.012)	(1.123)	(0.012)	(1.124)	(0.012)	(1.105)	(1.250)	(0.012)	(1.123)	(0.012)	(1.124)
Observations	32,540	28,550	19,798	27,954	27,873	27,954	27,873	32,540	28,550	19,798	27,954	27,873	27,954	27,873
R-squared	0.896	0.900	0.920	0.901	0.902	0.901	0.902	0.896	0.900	0.920	0.901	0.902	0.901	0.902
Lincom $_{(t+t-1+t-2)}$			-0.672							-0.005				
			(1.028)							(0.039)				
Wald						0.0512	0.0505						0.5324	0.5260
Audited$_t$								0.083*	0.082*	0.074			0.084*	0.086*
								(0.044)	(0.044)	(0.048)			(0.049)	(0.049)
Corruption$_t$								-0.078**	-0.078**	-0.071*			-0.086**	-0.089**
Log (value+1)								(0.035)	(0.036)	(0.039)			(0.037)	(0.037)

Notes: *<0.10; **<0.05; ***<0.01. All variables labelled corruption represent the interaction term of corruption and the audit. All models include unit and year fixed effects. Models 1-7 use corruption counts as a percentage of effective number of audits. Models 8-14 use the square-root of the quantity; the final rows in columns 8-14 just report the coefficient and SE of the audit intercept and contemporaneous corruption quantity when measured with the log. Wald reports the p-value that the coefficient on Corruption$_t$=Corruption$_{t+1}$.Using the raw count yields similar results. Extensive sensitivity analysis (including disaggregated corruption categories) can be found in Appendix I. Table 3A, for example, uses categorical variables for corruption. Table 3B includes municipal time-trends. Table 3C shows results with an AR1 correction.

Figure 1: Marginal Effects of Revealed Corruption with Counts (Table 3, model 1)

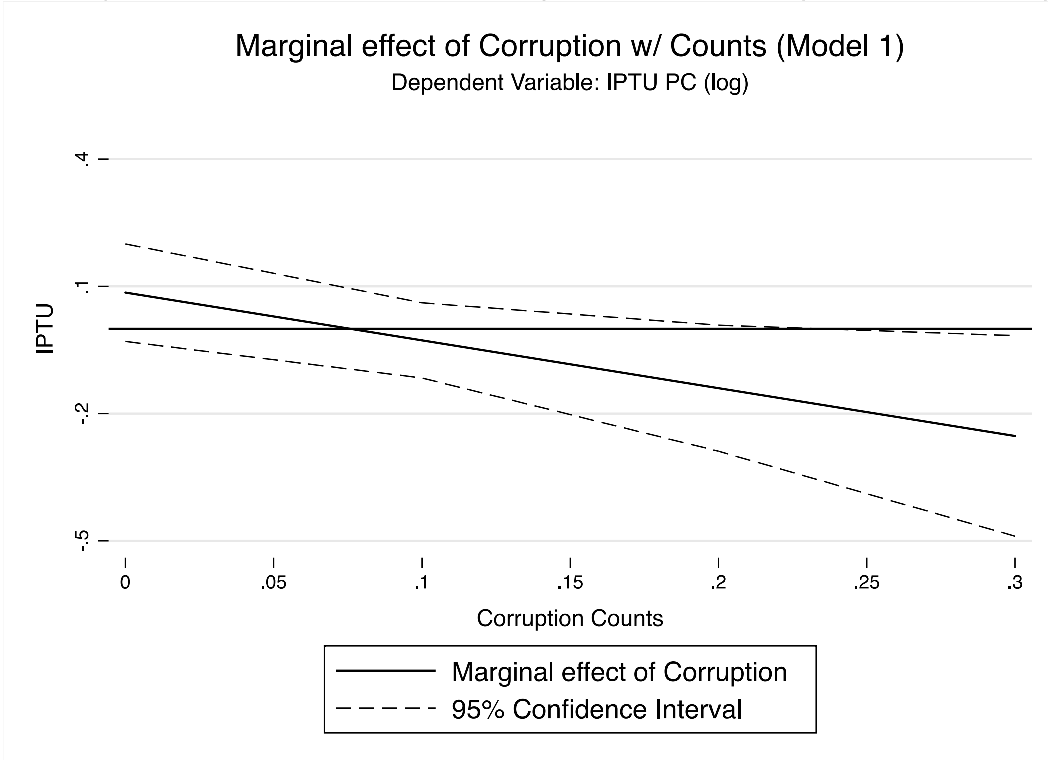

Figure 2: Marginal Effects of Revealed Corruption with Quantities (Table 3, model 8)

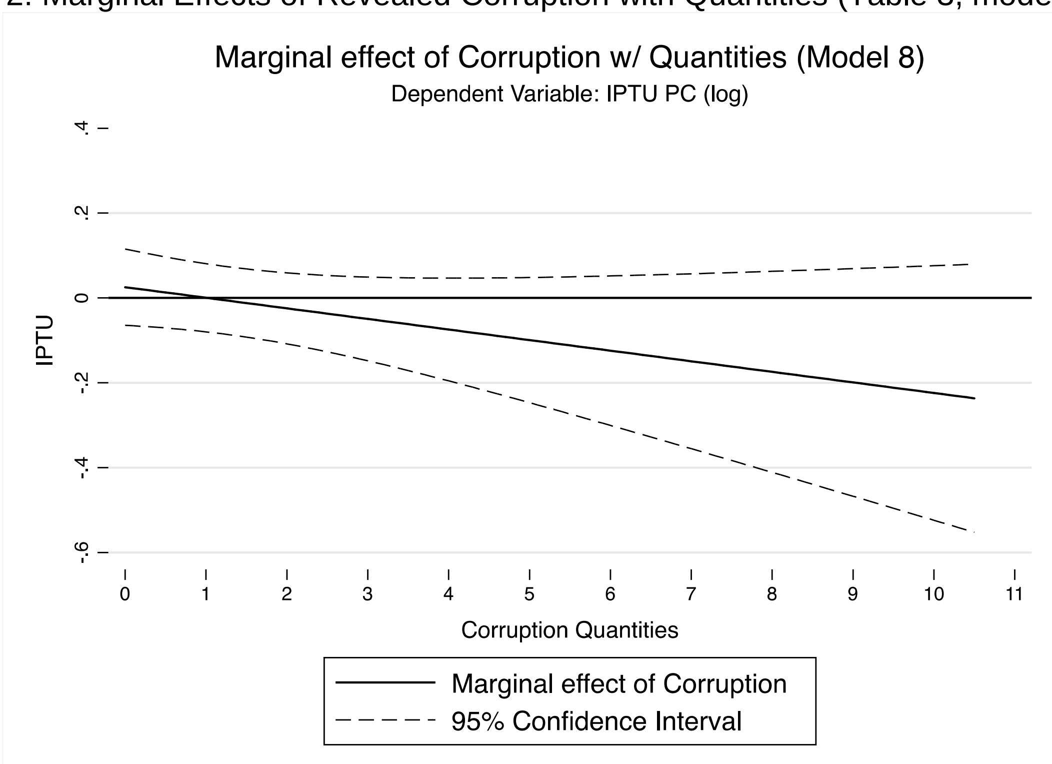

Table 4: Revealed corruption and the adoption of PB

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10
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		-0.062								
		(0.367)								
Corruption Count			0.251***							
			(0.087)							
Corruption Count %				5.946**						
Effective audits				(2.336)						
Count No bid contracts					9.557***					
% Effective audits					(2.641)					
Corruption Quantity PC						0.138***				
						(0.049)				
Corruption % of							4.321***			
money audited							(1.224)			
Overinvoicing quantity PC								0.636***		
								(0.193)		
Procurement Irregularities									0.140***	
Quantity PC									(0.050)	
Corruption Quantity PC										0.065**
(probit)										(0.029)
pbever		3.356***	4.316***	4.261***	4.287***	4.151***	4.148***	3.770***	4.090***	2.425***
		(0.185)	(0.684)	(0.750)	(0.800)	(0.855)	(0.874)	(0.789)	(0.830)	(0.444)
Left		1.957***	2.020***	1.883**	1.704**	1.563**	1.658**	2.155***	1.552**	1.224***
		(0.200)	(0.780)	(0.915)	(0.819)	(0.728)	(0.720)	(0.783)	(0.734)	(0.370)
Constant	0.038***	-5.107***	-6.846***	-6.326***	-5.884***	-5.761***	-5.729***	-5.791***	-5.649***	-3.206***
	(0.001)	(0.137)	(0.979)	(0.797)	(0.595)	(0.625)	(0.596)	(0.652)	(0.589)	(0.318)
Observations	42,880	5,176	358	358	358	345	358	317	345	345

Notes: *<0.10; **<0.05; ***<0.01. 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.

Table 5: The Electoral Effect

	PANEL A						PANEL B	
VARIABLES	DV=Log of Property Tax Per Capita (IPTU)						DV=adoption of PB	
	1	2	3	4	5	6	7	8
Corruption (count)	-0.967**	-0.594	-0.584				-3.973	
	(0.459)	(0.619)	(0.616)				(7.281)	
Corruption (quantity)				-0.022	0.009	0.005		0.053
				(0.016)	(0.045)	(0.045)		(0.333)
Pre audit * corruption (count)		-0.615	-0.619				12.067	
		(0.780)	(0.784)				(7.538)	
Pre audit * corruption (quantity)					-0.039	-0.033		0.081
					(0.047)	(0.048)		(0.331)
Population (log)	-0.624**		-0.618**	-0.621**		-0.609**		
	(0.285)		(0.285)	(0.285)		(0.286)		
GDPPC (log)	0.070		0.069	0.074		0.070		
	(0.124)		(0.124)	(0.124)		(0.125)		
Pre audit							-0.315	0.485
							(1.137)	(0.965)
PB in the past							4.405***	4.190***
							(0.765)	(0.834)
Left party							1.969**	1.680**
							(0.948)	(0.779)
Constant	6.279**	0.830***	6.223*	6.212*	0.830***	6.130*	-6.199***	-6.132***
	(3.178)	(0.038)	(3.183)	(3.183)	(0.038)	(3.181)	(1.260)	(0.912)
Observations	2,409	2,751	2,409	2,409	2,751	2,409	358	345
R-squared	0.895	0.891	0.895	0.895	0.891	0.895		

Notes: *<0.10; **<0.05; ***<0.01. Models 1 and 4 are the benchmark specifications for the audited sample with property taxes; the benchmark specifications for the adoption of PB are Models 4 and 6 in Table 4. As noted in the text, the interactions in the remaining columns test for different responses to revealed corruption between groups audited before and after the election.

Appendix 1: Sensitivity Analysis
Revealed corruption, taxation and fiscal accountability: evidence from Brazil

This appendix presents most of the results/alternative models/measures referenced in the paper. We would happily supply more tables in the event that additional information is necessary.

Table 3A presents the results with one of our alternative measures—categorical variables for different levels of corruption (low, medium and high). These measures are useful if people merely classify their government as good, ordinary, or very corrupt. The dummy variables represent municipalities with revealed corruption counts divided by the 30th and 70th percentiles of the distribution of corruption within the sample. For quantities, these correspond to approximately 0.1 and 11.7 reais respectively. The point estimates of the categorical variables are consistent with fiscal-contract hypothesis, as high corruption equates with the highest average revenue loss, medium corruption with no effects, and low corruption with small and (sometimes) significant gains. These monotonic increases in magnitude of the effect from low to high hold for most other arbitrarily assigned thresholds, such as the 20-80th percentiles. The effects are generally short-lived, as the test statistics suggest, except when the high corruption dummy variable in counts is defined in fairly particular ways.

Table 3B presents models with municipal-specific time trends. The primary results are substantively similar across virtually all measures. The variables for increasing corruption remain negative and significant with all count measures, while the low corruption dummy is positive and significant across counts and quantities.

Table 3C presents identical models to Table 3 with an AR1 correction and robust standard errors. Not only are the results more robust with an AR1 correction, but there is also clear evidence that the effects of revealed corruption are persistent once we account for the memory of the property tax series. (The results with Newey-West standard errors are very similar to those with an AR1 correction).

Table 3D shows the results with IPTU as a percentage of GDP. Only the quantity measure is significant, though the count is still correctly signed. While self-serving, we presume that the per capita normalization used throughout the paper is superior to one based on GDP; annual population figures are probably less prone to measurement error (!) and are less likely to respond to revealed corruption instantaneously.

Table 3E presents results with the ISSQN. As reported in the text, nothing is distinct from zero before or after the audit.

Table 3F shows results with the aggregate local tax variable. The aggregate variable yields similar, albeit not as robust, results as the IPTU alone. These results are partly driven by the IPTU. If we exclude the IPTU from the aggregate category, the corruption variables are negative but not significant, although *contribuições* are nearly significant on their own, across a variety of models/measures.

Table 3G presents the primary results with disaggregated categories for corruption. Note that testing the disaggregated categories on their own is somewhat problematic, as it forces us to treat the other corruption categories as being zero when, in fact, they are not (the disaggregated categories are positively correlated), and/or drop the cases where the other categories take on a value greater than zero, inducing considerable selection bias. In the models shown, we use the full sample. The point estimates for every disaggregated corruption category 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. Neither procurement fraud, nor diversion of funds is significant (unless we include an AR1 correction, not shown). Note, however, that diversion of funds and procurement are highly correlated with the other categories (the intercept changes sign because zero fraud does not mean zero corruption!). The final column includes all of the disaggregated categories together using dummy variables for whether 1 or more act was committed for that particular category; one will notice that the intercept is positive and significant, indicating that no corruption is rewarded.

Table 3H presents results with other indicators of performance (see definitions in Appendix II). Although correctly signed (-), none of the measures is statistically different from zero. While the data appendix offers a variety of potential explanations for the absence of statistical significance, we presume that attenuation bias poses a non-trivial threat.

Table 3A: Property Taxes PC with Indicator Variables for Corruption

	Corruption Counts			Corruption Quantities		
VARIABLES	1	2	3	4	5	6
Corruption High$_t$	-0.271**	-0.298**	-0.256**	-0.227*	-0.262**	-0.219
	(0.122)	(0.126)	(0.126)	(0.125)	(0.130)	(0.135)
Corruption Medium$_t$	0.036	0.027	0.066	-0.036	-0.058	0.003
	(0.066)	(0.069)	(0.074)	(0.067)	(0.071)	(0.074)
Corruption Low$_t$	0.050	0.052	0.009	0.119***	0.138***	0.074*
	(0.055)	(0.064)	(0.063)	(0.038)	(0.045)	(0.038)
Corruption High$_{t-1}$			0.083			0.093
			(0.107)			(0.120)
Corruption Medium $_{t-1}$			0.062			0.011
			(0.055)			(0.064)
Corruption Low $_{t-1}$			-0.151			-0.077
			(0.095)			(0.074)
Corruption High $_{t-2}$			-0.010			-0.036
			(0.113)			(0.137)
Corruption Medium $_{t-2}$			-0.026			-0.022
			(0.072)			(0.062)
Corruption Low $_{t-2}$			-0.048			-0.028
			(0.067)			(0.056)
Corruption High $_{t+1}$		-0.091			-0.113	
(placebo)		(0.083)			(0.098)	
Corruption Medium $_{t+1}$		-0.028			-0.084	
(placebo)		(0.065)			(0.052)	
Corruption Low $_{t+1}$		0.025			0.109*	
(placebo)		(0.063)			(0.066)	
Population (log)	-0.817***	-0.790***	-0.954***	-0.818***	-0.791***	-0.957***
	(0.105)	(0.107)	(0.114)	(0.105)	(0.107)	(0.114)
GDPPC (log)	0.068*	0.065	0.019	0.069*	0.066	0.018
	(0.040)	(0.040)	(0.050)	(0.040)	(0.040)	(0.050)
Constant	7.860***	7.653***	10.245***	7.861***	7.664***	10.281***
	(1.105)	(1.124)	(1.250)	(1.104)	(1.124)	(1.250)
Observations	28,550	27,873	19,798	28,550	27,873	19,798
R-squared	0.900	0.902	0.920	0.900	0.902	0.920
Wald high vs. medium (p-value)	0.0258	0.0224		0.0389	0.0171	
Wald high vs. low	0.0155	0.0124		0.0077	0.0033	
Wald medium vs. low	0.8659	0.7875		0.1756	0.1641	
Wald high placebo vs. medium placebo		0.5445			0.7890	
Wald high placebo vs. low placebo		0.2614			0.0574	
Wald medium placebo vs. low placebo		0.5539			0.0176	
Wald high current vs. high placebo		0.1173			0.3310	
Wald medium current vs. medium placebo		0.5203			0.6730	
Wald low current vs. low placebo		0.6093			0.6135	
Lincom High $_{(t+t-1+t-2)}$			-0.183			-0.032
(SE)			(0.215)			(0.131)
Lincom Medium$_{(t+t-1+t-2)}$			0.102			-0.008
(SE)			(0.136)			(0.135)
Lincom Low$_{(t+t-1+t-2)}$			-0.190			-0.163
(SE)			(0.173)			(0.254)

Table 3B: Primary regression models with municipal trends

	DV= IPTU_PC$_t$ (log)								
	1	2	3	4		5	6	7	8
	Count	Count	Quantity	Quantity		Count	Count	Quantity	Quantity
Audited$_t$	0.076	0.080	0.020	0.025	High Corruption	-0.539**	-0.547**	-0.223	-0.213
	(0.060)	(0.061)	(0.048)	(0.049)		(0.231)	(0.237)	(0.258)	(0.266)
Corruption Count	-1.088**	-1.124**			Medium Corruption	0.002	0.003	-0.176*	-0.191*
	(0.543)	(0.565)				(0.050)	(0.050)	(0.095)	(0.098)
Corruption Quantity			-0.025	-0.027	Low Corruption	0.149*	0.152*	0.080**	0.088**
			(0.018)	(0.018)		(0.076)	(0.078)	(0.036)	(0.035)
Population$_t$		-0.917***		-0.917**	Population$_t$		-0.918***		-0.919***
(log)		(0.131)		(0.131)	(log)		(0.130)		(0.131)
GDPPC$_t$		0.091**		0.091**	GDPPC$_t$		0.090**		0.092**
(log)		(0.042)		(0.042)	(log)		(0.042)		(0.042)
Constant	-16.390	100.655	-16.390	100.628	Constant	-16.415	100.744	-16.364	100.694
	(74.422)	(75.875)	(74.422)	(75.897)		(74.423)	(75.825)	(74.404)	(75.912)
Observations	32,540	28,550	32,540	28,550	Observations	32,540	28,550	32,533	28,544
R-squared	0.930	0.933	0.930	0.933	R-squared	0.930	0.933	0.930	0.933

Notes: Models 1-4 are the benchmark models from Table 3 (columns 1,2, 8 and 9). Models 5-6 use the dummy variables in counts from Table 3A, while models 7-8 use the dummy variables in quantities from Table 3A. All models include municipal-specific trends and municipal fixed-effects.

Table 3C: Property Taxes Per Capita and Revealed Corruption AR1

	DV= IPTU_PC$_t$ (log)													
	Corruption Counts Narrowly Defined							Corruption Quantities Narrowly Defined						
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Audited$_t$	0.097*	0.095*	0.120*			0.088	0.085	0.034	0.030	0.042			0.020	0.019
	(0.053)	(0.054)	(0.067)			(0.058)	(0.058)	(0.046)	(0.046)	(0.057)			(0.050)	(0.050)
Audited$_{t-1}$			0.006							0.014				
			(0.072)							(0.061)				
Audited$_{t-2}$			0.029							0.032				
			(0.066)							(0.056)				
Audited$_{t+1}$				-0.056	-0.059	-0.023	-0.026				-0.028	-0.031	-0.021	-0.024
(placebo)				(0.053)	(0.053)	(0.057)	(0.057)				(0.045)	(0.045)	(0.049)	(0.049)
Corruption$_t$	-1.276***	-1.261***	-1.561***			-1.399***	-1.385***	-0.030***	-0.029***	-0.037***			-0.034***	-0.034***
	(0.340)	(0.346)	(0.433)			(0.371)	(0.371)	(0.010)	(0.010)	(0.013)			(0.011)	(0.011)
Corruption$_{t-1}$			0.111							0.002				
			(0.464)							(0.014)				
Corruption$_{t-2}$			-0.370							-0.018				
			(0.421)							(0.013)				
Corruption$_{t+1}$				0.331	0.319	-0.189	-0.193				0.004	0.003	-0.009	-0.010
(placebo)				(0.338)	(0.338)	(0.365)	(0.365)				(0.010)	(0.010)	(0.011)	(0.011)
Population $_t$		-0.575***	-0.802***		-0.543***		-0.544***		-0.575***	-0.803***		-0.543***		-0.544***
(log)		(0.087)	(0.109)		(0.088)		(0.088)		(0.087)	(0.109)		(0.088)		(0.088)
GDPPC$_t$		0.064*	0.062		0.058		0.058		0.064*	0.061		0.058		0.057
(log)		(0.037)	(0.049)		(0.037)		(0.037)		(0.037)	(0.049)		(0.037)		(0.037)
Constant	2.016***	1.978***	2.182***	2.132***	2.101***	2.139***	2.108***	2.019***	1.981***	2.173***	2.133***	2.102***	2.141***	2.110***
	(0.105)	(0.129)	(0.365)	(0.128)	(0.130)	(0.128)	(0.129)	(0.105)	(0.129)	(0.365)	(0.129)	(0.130)	(0.128)	(0.130)
Observations	28,234	24,250	15,524	23,655	23,577	23,655	23,577	28,234	24,250	15,524	23,655	23,577	23,655	23,577
Lincom $_{(t+t-1+t-2)}$			-1.820*							-0.054				
			(1.079)							(0.033)				
Wald						0.0032	0.0037						0.0439	0.0457

Table 3D: Property Taxes % GDP and Revealed Corruption

	Corruption Counts						Corruption Quantities					
VARIABLES	1	2	3	4	5	6	7	8	9	10	11	12
$Audited_t$	0.014	0.009	0.021			0.013	0.033	0.022	0.035			0.026
	(0.034)	(0.034)	(0.038)			(0.037)	(0.035)	(0.035)	(0.038)			(0.038)
$Audited_{t-1}$			-0.016						-0.020			
			(0.044)						(0.044)			
$Audited_{t-2}$			0.013						0.000			
			(0.046)						(0.033)			
$Audited_{t+1}$				0.026	0.024	0.026				0.023	0.029	0.033
(placebo)				(0.038)	(0.037)	(0.040)				(0.032)	(0.030)	(0.033)
$Corruption_t$	-0.094	-0.125	-0.117			-0.116	-0.011*	-0.010*	-0.010			-0.010
	(0.230)	(0.234)	(0.240)			(0.280)	(0.006)	(0.006)	(0.007)			(0.007)
$Corruption_{t-1}$			-0.071						-0.002			
			(0.355)						(0.014)			
$Corruption_{t-2}$			-0.432						-0.014*			
			(0.456)						(0.008)			
$Corruption_{t+1}$				0.125	0.129	0.110				0.007	0.004	0.002
(placebo)				(0.331)	(0.328)	(0.362)				(0.009)	(0.009)	(0.010)
$Population_t$		-0.833***	-0.725***		-0.775***	-0.775***		-0.833***	-0.724***		-0.775***	-0.775***
(log)		(0.192)	(0.169)		(0.212)	(0.212)		(0.192)	(0.169)		(0.212)	(0.212)
$GDPPC_t$		-0.995***	-0.982***		-1.011***	-1.011***		-0.995***	-0.982***		-1.011***	-1.011***
(log)		(0.136)	(0.110)		(0.140)	(0.140)		(0.136)	(0.110)		(0.140)	(0.140)
Constant	1.588***	17.465***	17.071***	1.608***	17.085***	17.086***	1.589***	17.464***	17.055***	1.608***	17.083***	17.083***
	(0.014)	(2.566)	(2.237)	(0.014)	(2.742)	(2.742)	(0.014)	(2.566)	(2.236)	(0.014)	(2.741)	(2.742)
Observations	29,469	29,469	20,331	28,735	28,735	28,735	29,469	29,469	20,331	28,735	28,735	28,735
R-squared	0.970	0.971	0.976	0.970	0.971	0.971	0.970	0.971	0.976	0.970	0.971	0.971
Lincom $_{(t+t-1+t-2)}$			-0.620						-0.026			
			(0.667)						(0.022)			

Table 3E: 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
$Audited_t$	0.043	0.039	0.039			0.056	0.055	0.033			0.019	0.022	0.070			0.040	0.042	0.038		
	(0.041)	(0.039)	(0.044)			(0.038)	(0.037)	(0.040)			(0.040)	(0.041)	(0.049)			(0.034)	(0.035)	(0.042)		
$Audited_{t-1}$			0.024					-0.016					0.064					-0.002		
			(0.047)					(0.044)					(0.055)					(0.047)		
$Audited_{t-2}$			-0.022					-0.027					0.042					-0.007		
			(0.043)					(0.037)					(0.049)					(0.042)		
$Audited_{t+1}$				0.075	0.070				0.049	0.045				0.041	0.037				0.020	0.016
(placebo)				(0.051)	(0.051)				(0.043)	(0.042)				(0.041)	(0.041)				(0.035)	(0.035)
$Corruption_t$	0.055	0.024	0.008			-0.003	-0.005	0.003			0.143	0.109	-0.139			-0.002	-0.003	0.007		
	(0.203)	(0.194)	(0.237)			(0.008)	(0.008)	(0.009)			(0.254)	(0.264)	(0.319)			(0.008)	(0.008)	(0.010)		
$Corruption_{t-1}$			-0.125					0.010					-0.262					0.015		
			(0.292)					(0.010)					(0.353)					(0.011)		
$Corruption_{t-2}$			0.196					0.011					-0.189					0.011		
			(0.290)					(0.007)					(0.314)					(0.010)		
$Corruption_{t+1}$				-0.413	-0.402				-0.008	-0.008				-0.309	-0.311				-0.006	-0.005
(placebo)				(0.295)	(0.294)				(0.009)	(0.009)				(0.259)	(0.258)				(0.008)	(0.008)
$Population_t$		-1.062***	-1.072***		-1.067***		-1.062***	-1.074***		-1.068***		- 0.870***	-0.978***		-0.880***		-0.870***	-0.978***		-0.880***
(log)		(0.110)	(0.105)		(0.108)		(0.110)	(0.105)		(0.108)		(0.073)	(0.083)		(0.074)		(0.073)	(0.083)		(0.074)
$GDPPC_t$		0.163***	0.153***		0.161***		0.163***	0.152***		0.161***		0.120***	0.062		0.102***		0.120***	0.061		0.102***
(log)		(0.052)	(0.057)		(0.052)		(0.052)	(0.057)		(0.052)		(0.032)	(0.038)		(0.032)		(0.032)	(0.038)		(0.032)
Constant	3.066***	11.295***	11.490***	2.377***	10.938***	3.066***	11.297***	11.506***	2.377***	10.946***	0.139	-0.156	-0.412	-0.111	-0.184*	0.140	-0.155	-0.373	-0.113	-0.186*
	(0.010)	(1.235)	(1.244)	(0.007)	(1.215)	(0.010)	(1.235)	(1.244)	(0.007)	(1.215)	(0.092)	(0.117)	(0.389)	(0.107)	(0.106)	(0.092)	(0.117)	(0.388)	(0.107)	(0.106)
Observations	29,217	25,187	20,300	24,576	24,524	29,217	25,187	20,300	24,576	24,524	24,898	20,870	15,999	20,258	20,208	24,898	20,870	15,999	20,258	20,208
R-squared	0.823	0.831	0.851	0.833	0.835	0.823	0.831	0.851	0.833	0.835										
BW LBI											1.551	1.694	1.877	1.701	1.702	1.551	1.695	1.877	1.701	1.702

Table 3F: Aggregated Local Taxes, Fees and Contributions and Revealed Corruption

	Corruption Count					Corruption Quantity (sqrt)					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
$Audited_t$	0.049	0.041	0.036			0.022	0.019	0.008			0.051	0.045	0.062			0.026	0.024	0.012		
	(0.050)	(0.049)	(0.052)			(0.043)	(0.042)	(0.046)			(0.046)	(0.047)	(0.059)			(0.040)	(0.041)	(0.050)		
$Audited_{t-1}$			-0.062					-0.043					-0.025					-0.035		
			(0.054)					(0.044)					(0.063)					(0.054)		
$Audited_{t-2}$			-0.023					-0.030					-0.004					-0.003		
			(0.059)					(0.052)					(0.058)					(0.049)		
$Audited_{t+1}$				0.040	0.038				0.037	0.034				0.022	0.020				0.033	0.028
(placebo)				(0.051)	(0.051)				(0.044)	(0.044)				(0.047)	(0.047)				(0.040)	(0.040)
$Corruption_t$	-0.635**	-0.557*	-0.508			-0.016	-0.015	-0.010			-0.634**	-0.593*	-0.802**			-0.017*	-0.017*	-0.015		
	(0.315)	(0.322)	(0.349)			(0.015)	(0.015)	(0.016)			(0.297)	(0.304)	(0.380)			(0.009)	(0.009)	(0.012)		
$Corruption_{t-1}$			0.505					0.015					0.020					0.005		
			(0.367)					(0.014)					(0.407)					(0.012)		
$Corruption_{t-2}$			0.228					0.013					0.034					0.001		
			(0.363)					(0.014)					(0.372)					(0.011)		
$Corruption_{t+1}$				-0.431	-0.437				-0.018	-0.017				-0.228	-0.233				-0.014	-0.013
(placebo)				(0.293)	(0.293)				(0.011)	(0.011)				(0.297)	(0.296)				(0.009)	(0.009)
$Population_t$		-0.720***	-0.822***		-0.584***		-0.720***	-0.825***		-0.584***		-0.757***	-0.958***		-0.662***		-0.757***	-0.959***		-0.663***
(log)		(0.120)	(0.131)		(0.120)		(0.120)	(0.131)		(0.120)		(0.082)	(0.095)		(0.083)		(0.082)	(0.095)		(0.083)
$GDPPC_t$		0.155***	0.158***		0.152***		0.155***	0.157***		0.152***		0.202***	0.159***		0.199***		0.202***	0.158***		0.198***
(log)		(0.041)	(0.042)		(0.041)		(0.041)	(0.042)		(0.041)		(0.036)	(0.043)		(0.035)		(0.036)	(0.043)		(0.035)
Constant	2.686***	7.957***	8.915***	2.420***	6.547***	2.686***	7.958***	8.949***	2.420***	6.561***	1.598***	1.465***	3.225***	1.655***	1.582***	1.599***	1.465***	3.203***	1.653***	1.580***
	(0.009)	(1.271)	(1.357)	(0.008)	(1.254)	(0.009)	(1.271)	(1.358)	(0.008)	(1.253)	(0.127)	(0.155)	(0.557)	(0.161)	(0.159)	(0.127)	(0.155)	(0.556)	(0.161)	(0.159)
Observations	29,066	25,052	20,198	24,454	24,402	29,066	25,052	20,198	24,454	24,402	24,752	20,740	15,905	20,141	20,091	24,752	20,740	15,905	20,141	20,091
R-squared	0.901	0.905	0.916	0.907	0.908	0.901	0.905	0.916	0.907	0.908										
BW LBI											1.848	1.923	2.133	1.957	1.955	1.848	1.923	2.133	1.957	1.955

Note: The Aggregate local tax category excludes the ISSQN. With the log (rather than the square-root), corruption is generally significant.

Table 3G: IPTU per capita with disaggregated corruption counts and quantities

	DV=IPTU_PC (log)										
VARIABLES	1	2	3	4	5	6	7	8	9	10	11
Audited	0.084	0.025	-0.038	-0.053	-0.028	-0.029	0.091	0.038	-0.020	-0.030	-0.055
	(0.064)	(0.049)	(0.047)	(0.047)	(0.055)	(0.051)	(0.060)	(0.048)	(0.050)	(0.049)	(0.055)
Count Broadest Measure	-0.738*										
	(0.386)										
Quantity Broadest Measure		-0.021									
		(0.016)									
Count Diversion of Funds			-0.167								
			(1.037)								
Quantity Diversion of Funds				0.013							
				(0.036)							
Count Overinvoicing					-0.926						
					(1.498)						
Quantity Overinvoicing						-0.045					
						(0.065)					
Count Procurement Irregularities							-1.880**				
							(0.790)				
Quantity Procurement Irregularities								-0.039**			
								(0.019)			
Fraud Count									-0.936		
									(0.981)		
Fraud Quantity										-0.012	
										(0.019)	
Count No Minimum Bids											0.745
											(0.806)
Constant	7.864***	7.855***	7.858***	7.864***	7.855***	7.855***	7.872***	7.870***	7.863***	7.860***	7.863***
	(1.105)	(1.105)	(1.105)	(1.104)	(1.104)	(1.104)	(1.104)	(1.105)	(1.105)	(1.105)	(1.104)
Observations	28,550	28,550	28,550	28,550	28,550	28,550	28,550	28,550	28,550	28,550	28,550
R-squared	0.900	0.900	0.900	0.900	0.900	0.900	0.900	0.900	0.900	0.900	0.900

Notes: All models estimated with controls. We suppressed them for space reasons. As noted previously, the models incorrectly assume that the other corruption categories are zero, when in fact they are not. Corruption is correlated across categories, which is part of the reason the intercept switches signs. If we include all categories simultaneously, the intercept is positive and significant. Additional results with the disaggregated categories are available.

Table 3H: Performance failures and property taxes per capita

	DV=IPTU_PC (log)										
VARIABLES	1	2	3	4	5	6	7	8	9	10	11
Audited	-0.033	-0.087	0.005	0.042	0.029	-0.043	0.031	-0.029	-0.043	-0.000	-0.007
	(0.080)	(0.084)	(0.078)	(0.080)	(0.074)	(0.065)	(0.064)	(0.067)	(0.055)	(0.050)	(0.050)
Sum	-0.028										
Performance	(0.146)										
Inferior Quality		0.220									
		(0.369)									
Irregularities in Operation			-0.197								
			(0.248)								
Sum Administrative				-0.148							
Deficiencies				(0.135)							
Irregularities in					-0.307						
Administrative Procedures					(0.330)						
Irregularities in use of						-0.024					
Resources						(0.327)					
Procurement							-0.449				
Irregularities in							(0.316)				
Accounting								-0.124			
Irregularities								(0.378)			
Fail contract									-0.055		
									(0.672)		
Labor Irregularities										-1.293	
Count										(1.012)	
Labor Irregularities											-0.054
Quantity											(0.043)
Constant	10.029***	10.026***	10.032***	10.041***	10.040***	10.029***	10.040***	10.031***	10.029***	10.030***	10.032***
	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)	(1.810)
Observations	24,505	24,505	24,505	24,505	24,505	24,505	24,505	24,505	24,505	24,505	24,505
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.

Appendix II, Data

Revealed corruption, taxation and fiscal accountability: evidence from Brazil

This note briefly discusses the coding measures and procedures.

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. For the most part, explicit corruption is well-labeled in the reports; the exact corruption technology is sometimes harder to distinguish.

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 borderline categories were not included in the primary measure.

The audits also contain valuable (albeit incomplete) 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. Examples of each are given below. In the absence of negative finding, we assume the projects were completed as specified by the contract (though not necessarily as desired by citizens). Based on the premise that wasted revenue is wasted revenue (whether or not it is stolen), we tested to see if taxes responded these other dimensions of performance.

The performance measures were sign consistent (-) but indistinct from zero across the various outcomes. The non-result is consistent with every other paper we have seen on the subject (e.g., Ferraz and Finan 2008; Litschig and Zamboni 2011). There are a multitude of potential explanations for the non-findings. They could indicate that people pay less attention to other dimensions of government performance; that administrative performance has already been priced-in (as people can observe many aspects of performance every day); and/or that the other performance measures are very noisy. The performance variables (as we have measured them) may include too many relatively trivial events and we do not have a precise way of detailing their magnitude. In other words, attenuation bias is clearly a concern.

The procedure for the coding/supervising the audits was as follows. After reading several dozen audits, we designed the coding scheme and employed two coders. They were instructed to code violations and explain in writing why they put an event in a category and/or excluded an event from the counts/quantities. We then checked their codings against ones done by us. After the first attempts, we decided that one coder was clearly superior in the sense that his initial codings were similar to ours. When there were discrepancies, his explanations were sufficiently clear, detailed and sensible that we saw no reason to not trust his judgment. The other coder was released from service at that point. Another coder was not employed at that point because of budgetary constraints. We then spot-checked some of the remaining audits done by our primary coder. The coder never saw the outcome data, so they had no capacity to doctor the reports. Note that the primary corruption category is pretty easy to get right as the audit reports are quite specific. We conducted first statistical analysis with approximately 120 cases, a second one with approximately 260 cases, and the third and final analysis with 369 cases. The results have been similar throughout. The entire dataset will be released, including the coding notes, upon publication. We choose 2004 for two reasons. First, we wanted to allow researchers to map our data onto the Ferraz and Finan data. Second, 2004 had more audits than other year. Given that our sample seems representative of the audited population, we chose not to code out the remaining 2004 audits ourselves because it would expose us to charges that we contaminated the data.

Corruption	Irregularities in procurement processes	No bidding process (nolicit)	When contracts were awarded without bids.
		Minimum number of bidders not reached	When a procurement process did not have the minimum number of bidders, as required by law. The minimum number of bids is a function of the amounts involved.
		Evidence of fraud in the procurement process	Direct evidence suggesting 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		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		When there's evidence that purchases were made (or reported) at an above-market value. For example, in Bastos SP the municipality overpaid for inferior-quality material to replace the wooden floor of a Cultural Center. The budget called for a better material.
	Borderline categories	Irregular unexecuted budget	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	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 Promotion (and not through the Municipal Ministry of Health, the proper institution for such expenses).

Note: the borderline categories do not figure into the primary measure.

Performance	Sum Performance failure	Quality of service provided inferior than specified by law	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	When evidence is found on irregularities in the operation of locally-implemented programs. In Sao Felix BA, for example, a number of eligible 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)	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)	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	When labor irregularities are present. In particular, when the social security contributions are not retained/collected.
	Sum Admin. deficiencies	Irregularities in administrative processes	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 had expired, but they retained their position.
		Administrative irregularities associated with the procurement process	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	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	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