When the Social Contract Breaks: Fiscal Capacity Moderates the Link between Taxation and Voting *

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Abstract

Where voters and politicians can trade off tax compliance for government performance through elections, democracy enhances the social contract. We argue that this relationship breaks down in contexts of weak fiscal capacity for two reasons: voters perceive collection as less fair and less generative of revenues. This less efficient and more unequal tax extraction causes citizens to view taxation more negatively and thus punish politicians relatively more for tax increases. To investigate our argument, we exploit a loan program in Brazil designed to increase local fiscal capacity. The payout to municipalities, conditional on program application, generates a quasi-random shock to fiscal capacity. Consistent with our theory, we show that incumbent mayors in low-capacity places are punished for raising taxes, while those that received loans are not. We then use cross-national survey data to show that voter willingness to pay tax is correlated with perceived government performance only in high-capacity contexts.

Key Words: Fiscal Capacity, Taxation, Democracy, Voter Behavior, Social Contract

Word Count: 11,840

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Elections play a special role in strengthening the social contract and institutionalizing the exchange of tax compliance for service provision. Monitoring what governments do with the money taken from their pocketbooks, voters in democracies can use the ballot rather than non-compliance to influence politician behavior. This link between taxation and democratic politics has long been at the forefront of debates in the political economy literature. Scholars have been particularly interested in (1) whether increasing political representation (i.e., democracy) affects levels of taxation (e.g., Cheibub, 1998; Gould and Baker, 2002; Timmons, 2010); (2) how elections shape tax policy (e.g., Berry and Berry, 1994; Cheibub, 1998; Ehrhart, 2013); and, on the flip side, (3) how taxation affects voters' evaluation of politicians (Cusack, 1999; Kone and Winters, 1993; Geys and Vermeir, 2008). A majority of the literature, however, assumes that states are capable of enforcing tax policies as well as willing to collect taxes efficiently and equally.[1] Yet, we know that this is not always the case: many states are unable (or unwilling) to efficiently collect taxes. How does the inefficient and differential enforcement of taxation, characteristic of weak fiscal states, change the links between taxation and representation?

In this paper, we ask whether the state's fiscal capacity changes voters' attitudes about taxation and their evaluation of elected officials. We argue that, in low compared to high fiscal capacity settings, citizens view taxation more negatively. This distaste for increased taxation under low fiscal capacity will be reflected in retrospective evaluations of elected politicians: voters are more likely to punish politicians for tax increases. A key component of the social contract in democracies – the ability of politicians to trade-off policy performance for tax compliance – is thus diminished or absent in contexts of weak fiscal capacity.

[1]There are a number of exceptions and more authors have recently become interested in the topic of fiscal capacity development in the less-developed world (e.g., see: Eubank, 2012; Kasara and Suryanarayan, 2015; Weigel, 2020).

The logic of our argument is as follows. In high capacity settings, we expect the traditional theoretical argument to hold. A majority of voters (who are generally below the median income) prefer higher taxation (or redistribution). Voters hold politicians accountable and may punish or reward politicians for tax increases depending on preferences over redistribution. In an equilibrium where fiscal capacity is low, on the other hand, citizens are less likely to expect positive returns from tax increases and have little reason to believe that an increase in their own taxation will affect other taxpayers equally. All else equal, this will make voters relatively more likely to punish politicians who increase taxes.

We expect that voters under low fiscal capacity especially dislike taxation for two reasons. First, with lower fiscal capacity, fewer taxpayers are complying, revenues are lower, and thus individual returns to taxation are more marginal. Second, any additional increase in taxation will exacerbate already existing unfairness inherent in differential enforcement under low capacity. Comparing poor voters in low and high capacity settings, both may prefer higher taxation and redistribution relative to rich voters. Poor voters in low capacity settings, however, should value tax increases relatively less compared to their counterparts in high capacity settings because they anticipate relatively less redistribution and more differential and unfair enforcement. We therefore expect that *electoral punishment (reward) of incumbents for tax increases is higher (lower) in low-capacity compared to high-capacity settings*. In sum, we argue that fiscal capacity works as a moderator in the link between taxation and electoral politics.

One likely reason there is scant evidence on this question is that the effects of fiscal capacity are hard to identify. Fiscal capacity generally develops slowly, is hard to measure, highly correlated with improvements in other aspects of state development, and improvements are endogenous to institutions and political processes. Any differential effect of taxation on electoral outcomes in

high and low fiscal capacity contexts would therefore be hard to assign to a specific theoretical mechanism.

We take advantage of a rare "shock" to fiscal capacity that occurred in some Brazilian municipalities at different times over a discrete period. We make use of a federal loans program to increase municipal fiscal capacity in Brazil, PMAT, which serves as our quasi-exogenous shock to capacity. This enables us to compare municipalities that have applied and received PMAT to a proper counterfactual: those municipalities that have also applied but will receive PMAT in the future. This time-varying shock allows us to find counterfactual municipality-years which did not yet experience increases in fiscal capacity but are sufficiently similar to those that did, mitigating concerns of omitted variable bias. We thus investigate our theoretical predictions by examining within- and across-municipality variation in tax collection and subsequent voting behavior in Brazil, conditional on the quasi-random timing of the receipt of the first PMAT payment (similar to Gadenne (2017*b*)).

In line with our theoretical argument, we find consistent evidence across many empirical specifications that incumbents in municipalities under PMAT are punished less severely for increases in taxation relative to incumbents in similar places without PMAT. These results are robust to a number of alternative specifications. In addition, we introduce a type of structural nested means model based on Wodtke, Alaca and Zhou (Forthcoming) to account for the fact that changes to the moderator (fiscal capacity) have direct effects on the variable of interest (tax increases). We thereby isolate the interaction effect on the part of the tax increases not caused by changes in fiscal capacity. Our main findings remain unchanged.

Using this identification strategy in the case of Brazilian municipalities, we can robustly show that fiscal capacity moderates voter evaluations of tax policy. We then examine micro-level data

from a cross-national survey in Latin America to return to our original motivation of how fiscal capacity affects the link between taxation and representation. There, we show that respondents' evaluation of the government only increases their willingness to pay tax under higher perceived capacity. In other words, when respondents believe fiscal capacity is low (high), high government performance does not (does) increase their willingness to pay tax. While this analysis does not benefit from the identification strategy we use in Brazil, it offers plausible evidence of an important implication of our initial findings: that lower fiscal capacity indeed undermines the functional relationship between taxation and representation in a democracy.

This study contributes to the theoretical and empirical literature on taxation in democracies by proposing a new twist on the well-studied link between political accountability and taxation. Much of the existing literature is motivated by theories that incorrectly assume sufficient fiscal capacity. While our insights are particularly relevant for young, developing democracies where low fiscal capacity is the norm, even in advanced industrialized democracies voters may question the state's ability to enforce tax laws equally (Kiel, 2019). A key insight from our study is that we should account for voters' beliefs about fiscal capacity in the electoral calculus. While we mainly focus on voter preferences and behavior in this paper, our findings have implications for how politicians may adjust their behavior in reaction to the voters' calculus. For instance, politicians in low-capacity places may have strategic disincentives to invest in fiscal capacity, which could partly explain the stickiness of high and low fiscal capacity equilibira we observe across countries. While these differences are often attributed to capacity only, our theory suggests there may be strategic electoral incentives that further discourage taxation by incumbents in low capacity places, leading to vicious or virtuous cycles.

Taxation, Fiscal Capacity, and Electoral Behavior

Taxes are necessary to fund a functioning state, however, no one likes when the taxman comes calling. While voters may prefer higher public spending or redistribution, a number of studies have documented the degree to which voters punish incumbents for tax increases, mostly focusing on industrialized democracies (Kone and Winters, 1993; Cusack, 1999; Geys and Vermeir, 2008; Sances, 2017). On the other hand, citizens hold preferences over costly government policies, such that they may tolerate or even reward higher taxation (Bates and Lien, 1985). Elections can reinforce this social contract, or exchange of tax compliance for valued government services, because tax-paying voters are more likely to monitor politician performance when spending is more closely linked to their own pocketbook (Paler, 2013; Martin, 2014).

We argue that the (in)ability of the state to collect taxes fundamentally changes the link between taxation and electoral politics. If fiscal capacity is sufficiently low, voters will be less willing to submit to government taxation in anticipation of lower benefits and are more likely to punish politicians for tax increases. This will occur in low fiscal-capacity states for two reasons: 1) taxpayers are less likely to think that the collection of taxes will affect citizens equally; 2) taxpayers expect returns on their investment (i.e., their tax payment) to be lower, as the total collected revenue and thus government budget is smaller.

We argue that fiscal capacity moderates citizen perceptions of both fairness and efficiency of tax collection through its effect on perceived tax compliance. While taxation occurs in both high- and low-capacity states, it manifests differently in each. In lower capacity states, taxation often occurs through informal channels or intermediaries, and formal tax extraction can be marred by irregularities and ambiguities. In higher capacity states, taxation is less targeted and more

broad-based, less idiosyncratic and more regularized. These different experiences of state taxation moderate individuals' perceptions of overall tax compliance. Citizens who observe irregular and targeted tax extraction in their community will perceive their own tax bill as a more unfair demand from the state and a less efficient investment. We discuss the logic of each, in turn.

Capacity and Perceptions of Tax Compliance

Fiscal capacity influences citizens' perceptions of overall tax compliance. To any individual, much about a state's actual fiscal capacity is unobserved, e.g., the number of bureaucrats hired to collect taxes, the sophistication of the technology used to process receipts, or even the share of taxpayers audited for possible tax evasion. Instead, an individual citizen may experience high or low levels of fiscal capacity as the regularity with which they are solicited to pay tax, the manner in which this demand is communicated by the state, and direct interactions with agents of the state (Gottlieb, 2020). Citizens extrapolate from their own experience and interactions with the state about that of other potential taxpayers. Where an individual citizen's experience of paying taxes is more systematic and regularized, they infer higher fiscal capacity, increasing perceptions of overall tax compliance.

We can see evidence for this in survey data from Latin American cities where respondents' perceived fiscal capacity is positively correlated with their estimates of other citizens' tax compliance.[2] In Table 1, we show evidence of a strong positive relationship between several measures of perceived fiscal capacity and perceived rates of tax compliance. *Perceived Compliance*, the dependent variable, is measured both as the proportion of individuals and companies that the respondent believes "comply with the payment of taxes that the law establishes" (Columns 1-3 and 4-6).

[2]In 2011, the Andean Development Corporation conducted a survey in 17 cities across seven Latin American countries, including Brazil. In each city, 600 heads of household, or an adult between 25 and 65, were interviewed.

Table 1: Perceived Compliance and Fiscal Capacity

	Perceived Individual Compliance			**Perceived Firm Compliance**		
	(1)	(2)	(3)	(4)	(5)	(6)
Probability of Sanction	0.332**			0.126**		
	(0.044)			(0.040)		
Impartiality of Collection		0.197**			0.316**	
		(0.042)			(0.039)	
Capacity Proxy			0.397**			0.303**
			(0.044)			(0.042)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
R2 Adj.	0.080	0.050	0.090	0.068	0.085	0.085
N	8102	9259	7906	8034	9206	7860

Note: Linear models (OLS) with robust standard errors and country fixed effects. $^{+}$ p < 0.1, * p < 0.05, ** p < 0.01,

Perceived capacity, the independent variable of interest is measured in several ways[3], but all point in the same direction with positive and significant correlations between perceptions of capacity and perceptions of tax compliance. The state's fiscal capacity influences how citizens experience interactions with the state and their observations of other state-taxpayer interactions. As we argue, these observations and resulting inferences about the state's capacity, in turn, influence beliefs about the overall level of tax compliance.[4]

Voter Beliefs

We have shown that (perceptions of) greater fiscal capacity increases a citizen's confidence in others' tax compliance. We argue that these differences in beliefs about overall tax compliance then fundamentally change voters' views over taxation through two channels: 1) fairness of collection; and, 2) material returns to tax payments. The strategic complementarities in which taxpayer i

[3]See Appendix A for details.

[4]These results are quite similar when, corresponding with our empirical case below, we subset the survey data to only include respondents from Brazil.

receives greater utility from paying taxes when more taxpayers $-i$ also pay, therefore, arise for two reasons.

First, we contend that taxpayers in high-capacity settings are more positively dispositioned toward additional (or higher) tax payments because they perceive tax collection to be fairer. An extensive literature considers how perceived fairness of tax rates across income levels, i.e., the progressivity of the tax rates, affects attitudes about taxation. In contrast, we are specifically concerned with the perceived fairness of *tax collection*, or who taxes are collected from. We focus on the direct relationship between capacity and perceptions of fairness over collection that we expect to be uniform across voters: voters perceive taxation to be fairer when more rather than fewer citizens are paying. We argue that, independent of the rates of taxation, impartial and equal enforcement of *de jure* rates will improve voters' attitudes about taxation. Because tax enforcement is more likely to capture taxpayers $-i$, citizen and taxpayer i in a high fiscal capacity setting perceives tax payments as relatively more fair than in a low fiscal capacity setting, all else equal. This is consistent with the theoretical setup in Casaburi and Troiano (2015) wherein taxpayers derive non-monetary benefits from fiscal enforcement on other taxpayers due to fairness concerns. Where fiscal capacity is higher, tax collection is seen as fairer and individual attitudes towards taxation are more positive since voters are more likely to believe others are complying with the state's fiscal demands.

This inclusion of fairness into the taxpayer's utility function is consistent with existing ideas about the effect of values and trust on tax morale (Bräutigam, 2008) and Bordignon's (1993) formal model of tax evasion. Here, including fairness considerations leads to predictions that are more consistent with empirical patterns of tax compliance, e.g., some people do not evade even when it is in their self-interest to do so. Similarly, as Levi (1989, 53) noted in her foundational work: "The

compliance of each depends on the compliance of the others. No one prefers to be a 'sucker.'" In our view, this fairness/compliance idea extends to attitudes about tax increases.

Second, when public goods are financed through collective taxation, the benefit and effectiveness of public spending depends on the number of compliant taxpayers and efficiency of tax collection. All else equal, greater fiscal capacity – or the ability to extract taxes from citizens – mechanically increases the level of revenue and therefore the likelihood that public revenues will be translated into valued public goods. The influence of fiscal capacity on public goods provision is distinct from politician type. Higher government revenue, all else equal, should lead to more or better public goods provision or redistribution. Each individual taxpayer i's material benefits derived from government revenue, will therefore be larger in high-capacity settings where more taxpayers $-i$ are also paying, i.e., total collected revenue is larger.[5]

Consistent with our argument that benefits to tax payment across individuals are complementary, Cowell and Gordon (1988) show in their model of tax evasion that individuals taking the spillover effects of their own payments on others' compliance into account induces a lower "price" any individual pays (as tax) for public goods. Intuitively then, increases in perceived positive conformity (the belief that others are also paying) will increase one's own willingness to pay tax. As Levi (1989, 53) argues, governments "must create confidence in their credibility and their capacity to deliver promised returns for taxes" to induce higher compliance. A more extreme mechanism supporting this logic is that large public goods may require a threshold level of tax revenue to be provided. In very low capacity settings, the benefit from public goods investments may, therefore, not materialize due to the limited tax revenue collected.

[5]This logic has similar implications to that in Holland (2018) where support for redistribution is lower where people have low expectations of benefiting from state largesse. Our explanation is complementary in that these low expectations can be informed by expectations of small budgets due to low compliance.

To summarize, we contend that (perceptions of) greater fiscal capacity improves a citizen's attitudes about taxation through the mechanism of perceived tax compliance. All else equal, higher perceived compliance increases a taxpayer's utility with regards to perceptions of fairness of collection (the intrinsic value to thinking that others are paying as well) and material welfare (through higher government spending).[6] Differences in fiscal capacity, of course, may also change distributional assumptions of standard theories of taxation (Hollenbach and Silva, 2019). For example, richer or wealthier taxpayers may be better able to evade taxes, i.e., take advantage of low capacity. If low capacity has differential effects on compliance across income groups, with more evasion among high-income taxpayers, the detrimental effect on low-income taxpayers' attitudes should be even stronger.

Electoral Behavior

As developed in the previous section, citizens should have a greater (lesser) aversion to paying taxes in lower (higher) fiscal capacity settings for two reasons: 1) lesser (greater) perceived fairness of the tax system; 2) an expectation of lesser (greater) returns to paying taxes in terms of public goods provision. In a democracy where citizens evaluate tax policy, among other aspects of government performance, a voter's evaluation of tax increases should therefore lead to differential assessment of politicians under low and high levels of capacity.

Assuming a retrospective voting model (Ferejohn, 1986), voters and politicians have asymmetric preferences and incumbents are disciplined by the expectation of electoral sanctioning. Voters set their decision cutpoint for politician performance as close to their preferred level as possible

[6]Del Carpio (2013) shows that that disclosing the true rate of compliance in Peru, effectively increasing perceived compliance, raises the likelihood of tax payment. Yesegat and Fjeldstad (2016) similarly show that, in Ethiopia, a higher perceived compliance rate is positively correlated with one's own compliance – and one of the most important predictors of tax compliance.

without being so onerous that politicians would be better off shirking. Voters evaluate politician performance along several dimensions according to their preferences. Most retrospective accountability models consider preferences over spending when establishing a voter's cutpoint. We do not dispute the salience of this dimension, but rather wish to focus attention on the less well-studied effect of voter preferences over taxation.

How does fiscal capacity change voter behavior in response to preferences over taxation? We argue that voters in low-capacity places, due to their more negative attitudes on taxation, have significantly lower cutpoints for the maximal amount of tax extraction they are willing to endure before sanctioning the incumbent. Similar tax increases by politicians in low- relative to high-capacity municipalities are thus more likely to exceed the voter's cutpoint and trigger electoral sanctioning in the low-capacity setting. While retrospective voting models generally consider a representative voter, one can also think of a set of voters with distinct cutpoints due to differential willingness to pay. Our argument then implies that a tax increase in low-capacity settings causes more voters to see their cutpoint exceeded relative to that same tax increase in a high-capacity setting – leading to a smaller vote share for the incumbent.

Introducing the moderator of fiscal capacity into the taxpayer's voting calculus thus yields a novel empirical prediction:

HYPOTHESIS 1 *When a low fiscal capacity government raises tax revenue by X, the incumbent will suffer a relatively more substantial electoral sanction in the next election than when a high fiscal capacity government raises tax revenue by the same amount.*

It could be that in low fiscal capacity states, tax policy is less likely to figure into the electoral calculus of politicians and voters. If this were the case, it would bias against finding evidence for

this hypothesis. Voters would be more likely to ignore tax increases in lower capacity states rather than sanction at a higher rate.

While electoral accountability models do not make clear predictions about how increases in taxation condition voter sanctioning (it could go either way depending on how tax revenue is used), our insight offers a clear directional prediction about how voters evaluate marginal increases in tax revenue differently in high- and low-capacity settings. It suggests a complementarity between fiscal capacity, tax collection, and incumbent approval. This is consistent with the finding that voters in Italy reward incumbents for reducing tax evasion. Moreover, as Casaburi and Troiano (2015) show, tax collection, voter attitudes about evasion, and government responsiveness work as complements to each other. We want to emphasize that this argument is not about the relative level of taxation and electoral sanctioning in low- and high-capacity places. Rather, we are focused on the moderating effect of capacity: how marginal increases in tax extraction have differential effects across these two different equilibria.

The above logic implies that even though politicians in low capacity settings may be motivated to exhibit good performance for other reasons, they should not be motivated to trade off good performance for greater tax compliance. In other words, their performance records should not be associated with voter willingness to pay taxes. By the same measure, even if voters agree with politicians in power and trust their policy choices, e.g., their use of tax revenues, this congruence should not lead to a higher willingness to pay taxes in low capacity settings. We will thus test the following observable implication:

HYPOTHESIS 2 *Politician performance should act as a moderator of citizen willingness to pay taxes in a high fiscal capacity setting but not a low capacity one.*

Research Design

Fiscal capacity generally develops slowly and is highly correlated with improvements in other aspects of state capacity and political development. It is thus difficult to find comparable high- and low-capacity contexts within which to study the differential effects of taxation on voting behavior. We take advantage of a rare "shock" to fiscal capacity that occurred in some Brazilian municipalities which allows us to find counterfactual municipalities that did not experience the shock. Our research design ensures that we compare municipalities that are sufficiently similar to each other to mitigate concerns that our findings can be attributed to factors other than fiscal capacity, e.g., age of democracy or clientelistic politics. In addition to quasi-exogenous variation in fiscal capacity, Brazilian municipalities are also suited to our study in that they have independent fiscal policies and electoral races allowing for a large-N subnational analysis.

Thus, we investigate the first empirical prediction of our theoretical argument using data on Brazilian municipalities from 2000 to 2010. Municipal governments in Brazil have strong political autonomy when it comes to public spending (especially on elementary education and public health) and the authority to raise tax revenue using multiple tax instruments, such as sales and property taxes (Nickson, 1995; Rodríguez and Velásquez, 1995).

The administrative data from Brazil do not allow us to test the second empirical prediction of our argument because we cannot adequately observe variation in government performance and willingness to pay tax. Instead, we test this prediction using the above-mentioned survey data from seven Latin American countries for which we have measures on perceptions of government performance and willingness to pay tax. While this analysis does not have the same ability to mitigate potential threats to causal inference, it has the advantage of widening the scope of our

study to include multiple other contexts. In the remainder of this section, we focus on the research design to test Hypothesis 1 using the case of Brazil.

Fiscal Capacity Shocks and Taxation in Brazil

While a substantial share of municipal revenue in Brazil comes from state and federal transfers, pressure on local mayors to raise revenue has been increasing due to a decline in these transfers. One of the main sources of tax revenue for municipalities is the *Imposto Predial e Territorial Urbano* (IPTU) or urban property tax (De Cesare and Ruddock, 1999; Carvalho, Jr., 2017). For a large number of municipalities, however, collecting and administering taxes is difficult and highly problematic given outdated property registers, low valuations of property, as well as incompetent or corrupt officials. The lack of revenue from the IPTU given its potential has often been lamented (Carvalho, Jr., 2017; Afonso, Araujo and Nóbrega, 2012).

We use variation in fiscal capacity across municipalities in Brazil caused by a federal program that was designed to increase the tax and administrative capacity. In 1998, after much dismay with local tax collection, the Brazilian Development Bank started the *Programa de Modernização da Administra ção Tributarìa* program (PMAT). PMAT was created to raise the capacity of municipalities to engage in tax collection, specifically of property taxes. Credit lines subsidized by the federal government would allow the municipalities to use these funds to update taxpayer rolls and cadastres, educate bureaucrats, and improve bureaucratic infrastructure (Gadenne, 2017*b*). As Gadenne (2017*b*) documents, all municipalities that apply to the program are eventually approved to receive loans, and funds are restricted to investments in local capacity. Municipalities that did receive PMAT saw a significant positive effect on tax collection.

How Municipalities Increase Property Tax Revenues

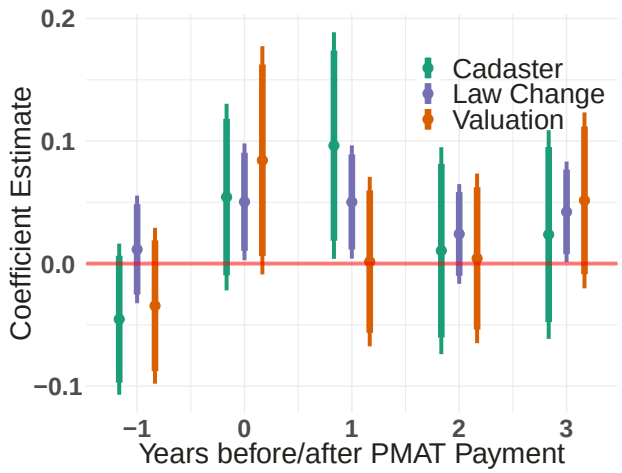
Property taxes in Brazilian municipalities are based on the *fair market value* of a given property to which the local IPTU rate is applied (Afonso, Araujo and Nóbrega, 2012). To assess property taxes, a municipality must therefore have a registration of buildings/properties (property cadastre) and an estimate of each property's *fair market value*. To estimate a property's value for tax purposes it must first be registered in the municipal cadastre, which records properties, buildings, and their characteristics. Based on each property's characteristics and location, its fair market value then has to be estimated. Most Brazilian municipalities create land value maps, i.e., estimate the value of land at some neighborhood level given characteristics of the locality. Property values are then estimated as their size (registered in the cadastre) multiplied by the applicable land value plus estimated replacement costs of the building (Afonso, Araujo and Nóbrega, 2012).

To increase property tax revenues, a municipality can therefore pursue several strategies. First, it can add properties to the municipal cadastre or update the characteristics of buildings already registered. Second, a municipality could update the valuation of properties, generally by updating the valuation maps, and therefore increase the tax bill for properties with increased values. Third, municipalities could increase property tax rates. While updating the cadastre and valuation requires sufficient levels of capacity, changes to tax rates do not. Insufficient fiscal and bureaucratic capacity are commonly cited as reasons for the irregular and infrequent updates to cadastres and property valuations (Afonso, Araujo and Nóbrega, 2012; Carvalho, Jr., 2017). As we show in Figure 1, an infusion of fiscal capacity through the PMAT program has a positive and significant effect on the likelihood of cadastre and value updates as well as on rate changes.[7] This finding is in line

[7] Appendix B describes our data and reports the full regression output in tables.

with results from a survey of municipalities which received PMAT prior to 2005 undertaken by Gadenne (2017*b*,*a*). According to survey responses, 98% of surveyed PMAT participants updated their tax registries (Gadenne, 2017*a*, 54). The expansion of the tax base, in some cases, allowed municipalities to lower rates as well. Other common effects of PMAT were for municipalities to reform their audit method, put extra effort into recovering tax arrears, purchase software, and invest in skills that allowed for better data management to flag irregular payments. In our main analyses, we will consider the total moderating effect of PMAT on electoral sanctioning. But later in the discussion section we will return to two key policy levers – cadastre and value updates – to make inferences about the mechanisms through which net effects of PMAT are being generated.

Figure 1: Effect of PMAT on Tax Policies



Note: This Figure displays the coefficient estimates and 95% and 90% confidence intervals for leads and lags of the year of a municipality's first PMAT payment on the likelihood of cadastre updates, updates of the property valuations, or changes to the IPTU laws. Results are based on panel regressions estimated as linear probability models with each type of update/change as the dependent variable. Each model includes municipality and year fixed effects. Standard errors are clustered by municipality. PMAT significantly increases the probability of a municipality undertaking cadastre and valuation updates in the first year after payment.

Exogeneity of the Moderator – Fiscal Capacity

In an ideal world, we would want to randomly assign a fiscal capacity shock to incumbent governments in a random half of municipalities in our sample and evaluate whether a uniform tax increase leads to different voter behavior across the two groups. Instead, we approximate randomness of the initial fiscal capacity shock through the timing of selection into PMAT.

Municipalities that apply to PMAT to increase fiscal capacity, of course, are different from those that do not. They are richer, larger, and more developed. For example, for the 2008 electoral period in our sample, the median GDP for municipalities that receive PMAT at some point is more than ten times the median GDP of municipalities that have not applied. Similarly, the population size and property tax revenues are substantially larger. We, therefore, cannot simply compare differences in the relationship between tax increases and electoral outcomes across these different municipalities, as mayors and municipalities that select into PMAT are very distinct from those that do not.

Instead, we follow the strategy used by Gadenne (2017*b*) to identify the effects of PMAT on public spending. We compare localities that have begun to receive financial transfers through the PMAT program before or during an electoral mandate to places that have applied to the PMAT program but have not yet started receiving funding by the end of the same electoral mandate. This strategy overcomes the selection problem inherent in the voluntary nature of participation in the program by only comparing places that decide to apply in similar time periods. While the timing of the decision to apply is subject to the incumbent government's choice, the exact timing of the first payment is not (Gadenne, 2017*b*). Rather, the timing of the first loan payment is subject to the vagaries of the loan processing schedule of the federal government. We use the semi-randomness

in the date the first payment of PMAT is made to localities that have already applied – and whether that date occurs before or after the end of an incumbent's mandate.

While we cannot test this assumption directly, we have evidence that, conditional on having applied to PMAT, the timing of the first payment (i.e., our treatment) is as-if-random. Specifically, given the subset of municipalities that have applied to PMAT, we regress a right censored indicator variable for the year of the first payment on a number of time-lagged covariates included in our models, e.g., property tax revenue, GDP, transfers, incumbent vote share, vote share squared, and mayor characteristics. The results (see Appendix Table C.3) do not provide any evidence of relationships between the included covariates and likelihood of first payment.

The Independent Variable – Changes in Taxation

While we are confident we can identify a proper counterfactual for places that received PMAT, one issue remains when analyzing the effects of tax increases across these places. Since we are interested in the differential effect of tax increases under high versus low capacity, this independent variable is measured after the shock to capacity. Thus, similar tax increases may be different in quality in the low and high-capacity municipalities. This raises the question, if we do see differential voting behavior across places, is this because tax revenue increases in high-capacity places are being realized differently, or because voters are less opposed to taxes in high-capacity places irrespective of how they are implemented?

We are of two minds as to whether our moderator affecting our independent variable is a feature or a bug. On the one hand, our theory expects the reason that voters might react differently to tax increases in high-capacity places is precisely because the way those taxes are collected will be qualitatively different, e.g., more systematic and less targeted. Additionally, as our theory

specifies, the higher collected revenue in high-capacity places leads to greater individual benefits from and thus less negative evaluation of tax increases. Substantively, we are thus interested in the post-PMAT measure of the independent variable (*total effect*). On the other hand, we might be interested in knowing the effect of the *same marginal tax increase* across low- and high-capacity cases identified by our exogenous moderator (*partial effect*).

Because both constructs may be of interest, we attempt to estimate both types of effects in our analyses. In our first and main analysis, we use the identified moderator and the post-PMAT measure of tax revenue increases as our independent variable to estimate the *total effect*. In a secondary analysis, we use a structural nested means model to decompose our independent variable into tax increases that were produced by the moderator PMAT and those that were not. We then study the effect of the latter on voting behavior to mitigate concerns about tax increases being a function of PMAT status, resulting in an estimate of the *partial* effect.

Capacity as a Moderator of Voter Evaluations of Tax Policy

We test Hypothesis 1 by examining the differential effect of increasing property tax revenue on local political competition, conditional on whether a locality has received the positive shock to fiscal capacity (PMAT). To do so, we assemble a panel dataset of socio-economic and political variables for Brazilian municipalities from 2000 until 2010. Our main dependent variable is the vote share of the incumbent party in year $t \in \{2004, 2008\}$ based on electoral returns for the first round of mayoral elections in those years.[8] We code incumbency separately for the individual candidate or the political party, and show results of both. Because we are only interested in how tax revenue affects the fate of the incumbent, we exclude localities in which the incumbent party

[8]Data on election results and candidates were collected from the Superior Electoral Court (TSE do Brasil, 2016) in Brazil.

did not run again which accounts for 38% of the sample.

Our independent variable is the change in property tax revenue over the period of the incumbents' electoral mandate, which we measure using data from the Institute of Applied Economic Research (IPEA, 2016).[9] Because we are unsure how voters will attribute tax collection in the year of the election (whether they will attribute it to the incoming or outgoing government), we focus on the change in tax revenues in the intervening years, e.g. year t_{-3} to year t_{-1}. As our theory is specifically about differential reactions to tax increases, we drop observations where the change in tax revenues is negative (tax revenue increases over the period in all but 9% of cases). We correct for skew in our resulting variable by taking the log of the change in tax revenue.

We expect fiscal capacity to moderate the relationship between taxation and electoral performance. To test our expectation, we use the municipalities' participation in PMAT as a shock to fiscal capacity. Recall, PMAT provides municipalities with cheap credit lines to improve the local tax administration. We use the date at which a given municipality receives its first loan payment from the federal government to code a municipality's PMAT status. Municipalities that have applied to the PMAT program are coded as zero before the year the first loan payment was received and one for the year of payment and going forward. Our sample is limited to municipalities that apply to the program and eventually receive PMAT.

Even though the fiscal capacity shock is quasi-random, to increase precision of estimates and mitigate omitted variable bias, we estimate models with and without political and economic control variables. We add covariates that might affect both tax revenue increases and vote share of the incumbent. First, we control for changes in total spending in the municipality so we do not conflate

[9]One benefit to using property tax is that it is a direct tax immediately felt by taxpayers which avoids our findings being confounded by another mechanism that can weaken the taxation-accountability relationship – fiscal illusion, or the idea that indirect taxation that is less observable by taxpayers will fail to condition voter behavior (Kao, Lu and Queralt, 2019).

the effect of tax increases with mayors attempting to improve their electoral fortunes by increasing spending immediately before elections. We also include covariates measured as the average over three non-electoral years of the incumbent's mandate (year t_{-3} to year t_{-1}) for those variables where differences in levels should matter: Population (IBGE, 2016), Urban Population Share (IBGE, 2016), federal and state Transfers (logged) (IPEA, 2016). Since economic development is likely to affect voters' evaluations of candidates and tax revenues, we add growth in municipal GDP over the period at which we measure tax revenue increases.

As political control variables, we include characteristics of the incumbent running: co-partisanship with the president and governor, education level, and the party's political ideology based on roll call votes and surveys of Brazilian legislators (Power and Zucco Jr., 2009, 2012; Samuels and Zucco Jr., 2014; Saiegh, 2015). Lastly, all models include a covariate for incumbent vote share in the previous election to mitigate bias due to other *unobserved* variables – in other words, we control for unobserved differences across localities that make them more or less politically competitive.

For our main specification, we estimate Equation 1, which compares localities that have applied to PMAT in or before election year t to localities that have received the first transfer of payment from the PMAT program prior to election year t. We code a binary variable *Payment* as 1 if the locality received a PMAT payment by year t_{-1} and as 0 if the locality applied to PMAT by year t. Of the 368 cases where the incumbent party runs again, 72% are coded as having received PMAT payment. Since some municipalities are represented as multiple observations in our data set, we cluster standard errors at the level of the municipality.

$$\text{DV}_{i,t} = \Delta\text{Tax}_{i,t_{-3}-t_{-1}} + \text{Payment}_{i,t} + \Delta\text{Tax}_{i,t_{-3}-t_{-1}} \times \text{Payment}_{i,t} + \text{Controls}_{i,t} + \varepsilon \qquad (1)$$

We observe a statistically significant difference in the relationship between increases in tax revenue and incumbent vote share conditional on having received a PMAT payment, indicated by the positive coefficient on the interaction term in Table 2. In our main analysis in Columns 1 (without controls) and 2 (with controls), we restrict the sample to places where the incumbent party runs again. Columns 3 and 4 show that the findings are substantively unchanged when use the more restrictive sample where incumbent mayors run again.

As illustrated in the left panel of Figure 2, in places that do not start receiving PMAT loans during the incumbent's mandate, larger increases in tax revenue are increasingly bad for the incumbent. As the right panel shows, the marginal effect of tax increases among places without PMAT loans has a statistically significant negative effect on the incumbent's vote share. The receipt of PMAT loans during or before the incumbent mayor's mandate, however, eliminates any negative trend; the marginal effect of tax increases among this group is not statistically different from zero. In fact, as is visualized in Figure 2, for incumbent parties in municipalities that received PMAT (dark gray), the predicted vote share is effectively constant, no matter the tax increase. The predicted vote share for incumbents in low-capacity municipalities (those that have not yet received PMAT, light gray), on the other hand, decreases from about 65% to below 25% over the range of observed tax increases. These results underscore the differential effect of tax increases on incumbents' electoral returns under low versus high fiscal capacity.

As a further test of our theory, we can check whether municipalities that receive PMAT loans earlier and thus have a longer time to develop fiscal capacity demonstrate a relatively stronger positive relationship between taxation and incumbent vote share relative to places that have only just received loans. In Appendix D, we show that the moderating effect of fiscal capacity does

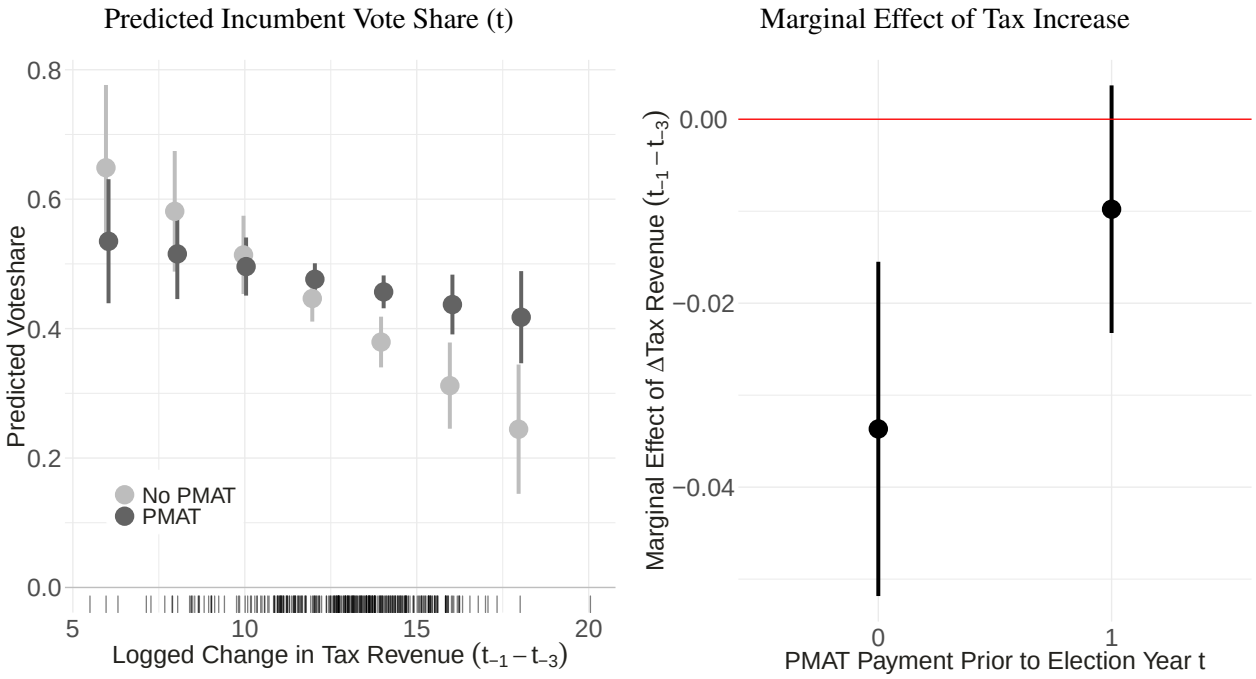
Table 2: Relationship between Δ Taxation $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t) Conditional on PMAT

	Incumbent Party Runs		Incumbent Mayor Runs	
	(1)	(2)	(3)	(4)
Δ Tax Revenue (logged)	-0.019^{**}	-0.031^{**}	-0.012	-0.030^{**}
	(0.007)	(0.009)	(0.008)	(0.010)
Received PMAT loan	-0.198^{+}	-0.223^{+}	-0.217^{+}	-0.176
	(0.101)	(0.114)	(0.117)	(0.125)
Δ Tax Revenue (logged) $\times$ Received PMAT loan	0.018^{*}	0.021^{*}	0.020^{*}	0.017^{+}
	(0.008)	(0.009)	(0.009)	(0.010)
Vote Share Prior Election	0.274^{**}	0.349^{**}	0.286^{**}	0.299^{*}
	(0.086)	(0.102)	(0.108)	(0.116)
Population (logged)		0.011		0.009
		(0.027)		(0.027)
Urban Population Share		0.000^{+}		0.000^{+}
		(0.000)		(0.000)
GDP Growth		-0.050		-0.127^{**}
		(0.057)		(0.045)
Amount of Transfers (logged)		0.003		0.018
		(0.030)		(0.030)
Party of the Governor		-0.016		-0.010
		(0.022)		(0.023)
Party of the President		0.045^{+}		0.060^{*}
		(0.024)		(0.025)
Incumbent Mayor Ideology		0.011		0.017
		(0.014)		(0.012)
Incumbent Mayor Education		0.013^{+}		0.006
		(0.007)		(0.008)
Δ Spending		-0.000		0.000
		(0.000)		(0.000)
Constant	0.530^{**}	0.336	0.492^{**}	0.197
	(0.107)	(0.291)	(0.131)	(0.333)
Observations	343	308	288	253

OLS models with standard errors clustered by municipality. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

in fact increase in this way.[10] Instead of the binary moderating variable, we create a continuous variable that represents the number of years between the first loan payment and the election in question. The positive and significant coefficient on the interaction term indicates that places that received the PMAT loans earlier exhibit a relatively more positive relationship between taxation and incumbent approval.

Figure 2: Relationship between Δ Tax Revenues & Incumbent Party Vote Share by PMAT



Note: The left plot shows the predicted vote share for incumbent parties and their 95% confidence intervals based on the change in taxation during the incumbent's term in office conditional on receiving PMAT prior to the election. For municipalities that received PMAT (dark gray), the predicted vote share for the incumbent party is effectively constant, no matter the size of the tax increase. For low capacity municipalities (those that have not yet received PMAT, light gray), on the other hand, the predicted incumbent party vote share decreases from almost 65% to below 25% over the range of observed tax increases. The right plot shows the marginal effects and their 95% confidence intervals of a one unit increase in taxation on the log scale for municipalities that have received PMAT payments and those that have not. Despite the overlapping confidence intervals, the two marginal effects are significantly different from each other.

In the next two subsections, we examine the robustness of these findings to several potential endogeneity concerns. First, we examine whether our identification strategy for the moderator,

[10]These results are even stronger in a restricted sample of municipalities that applied within one to five years of the election date (column 5 of Appendix Table D.4).

finding proper counterfactuals for high- and low-capacity places, is reasonable. Second, we examine the plausibility of reverse causality between the independent and dependent variables of interest. In particular, we mitigate concerns that prior electoral competition informs a politician's expected likelihood of getting re-elected, which in turn could affect strategic decisions about tax increases. And third, we discuss how the interrelationship between the moderating variable, capacity, and the independent variable of interest, tax increases, affects our interpretation of the findings.

Robustness of the Estimation Strategy

One concern with the above analysis is that places that receive PMAT loans many years prior are also less likely to have a counterfactual municipality that did not yet receive a loan in the sample. To ensure better comparability, we here limit the sample of municipalities to places that applied within one to five years of the election date, given that all municipalities with applications older than five years have received their first loan. Our findings remain unchanged in this reduced sample (column 4 of Table D.4), mitigating the concern.

Additionally, in our original specification, receiving a PMAT loan could be picking up differences between early appliers and late appliers, since an early application may lead to a municipality receiving a first payment earlier. Our identification strategy relies on comparing mayors or municipalities that are similar given that they both apply to the PMAT program, but for reasons beyond their control receive the first loan transfer before and after the end of the electoral mandate being studied. While all municipalities in our sample applied to the PMAT program during or before the electoral mandate in question, they do so at different times – and early-appliers could be different from late appliers. In column 3 of Appendix Table D.4, we thus control for the amount of time since the application to PMAT. The key quantity of interest – the coefficient on the interaction term – while

less precisely estimated is substantively unchanged.

Next, one might worry that the uncovered relationship is spurious and actually caused by differences in competitiveness. Mayors expecting close elections may be less likely to raise taxes for fear of electoral punishment. In this case, the positive relationship between tax increases and electoral punishment could be a function of the politician's strategic behavior rather than voter sanctioning as we propose. There are several reasons why we think it is unlikely that our results are due to differences in competitiveness. First, it is not entirely clear why competitiveness should lead to the differential effects in PMAT vs. non-PMAT municipalities, the focus of our analysis. Second, we control for lagged competitiveness in all estimated models by including incumbent (party) vote share from the previous election. Additionally, in Table E.8 we estimate our main set of models while including incumbent vote share and vote share squared, accounting for a potential non-linear effect of competitiveness. Our results remain unchanged. Lastly, recall that the probability of receiving the first PMAT payment is not significantly related to incumbent vote share in the previous election (either linearly nor non-linearly - see Table C.3).

As a final check that our main findings are not spurious correlations, we conduct three placebo tests. First, if the causal mechanism outlined in our theoretical argument is correct, receiving loan payments in a future government mandate should not affect the way voters condition behavior in the present electoral cycle. We thus replace our coding of the PMAT variable from an indicator of having received the loan before the election in year t to an indicator of having received the loan after the election t. As we show in Appendix Table E.5, in expectation with the placebo test, there is no evidence that the effect of tax increases on voter support for the incumbent is moderated by future receipts of payment.

One might be concerned that our results are caused by voters' reacting to changes in public

spending instead of taxation. And, voters might react more positively to increases in public spending, where the state is more capable and efficient. We attempt to mitigate this concern by controlling for contemporaneous changes in public spending in some of the empirical models above, thereby focusing on the partial effect of tax increases conditional on changes in spending.[11] In addition, we substitute logged increase in municipal spending into our main models in place of the tax increase variable. As shown in Table E.6, there is no comparable effect for spending increases. The coefficient on the interaction is considerably smaller and is never close to statistical significance. Similarly, if we use logged increases in transfers over the same period in the interaction instead of tax increases, we do not find evidence of an interaction effect on an incumbent party's electoral fortune (Table E.7).

Lastly, we undertake an additional robustness check in which we use an alternative specification of our outcome measure of electoral behavior. Table E.9 shows our main results when we use the incumbent party's electoral margin as the dependent variable. The interaction remains positive but is less precisely estimated, with significance at the 0.1 level for models where we do not control for time since PMAT application.[12]

Fixed Effect Estimation

One source of concern about our results is the potential for underlying differences between municipalities in the low (non-PMAT) and high (received PMAT) groups, i.e., that early-appliers are different from late-appliers. One way to mitigate the resulting biases, were this to be true, is to estimate the effect of tax increases on electoral outcomes within municipalities before and after receiving

[11] One might be concerned that changes in spending are post-treatment to changes in tax revenue and thus identify changes in spending as a *bad* control. To mitigate this concern, we verify that our results are substantively the same if we do not control for changes in spending.

[12] Additionally, our main results are substantively unchanged if we include election year and state fixed effects.

PMAT. A drawback of this approach is that it reduces our sample and thus statistical power considerably. Nevertheless, estimating models with election and municipality fixed effects can provide us with a very conservative test of our theoretical argument. Given the municipality and election fixed effects, the estimand is that of a difference-in-differences design, i.e., the average treatment effect on the treated (ATT). In other words, we are comparing the average effect of tax increases in municipalities prior to receiving a PMAT loan to the effect after the first loan. Additionally, the availability of similar places that did not yet get PMAT loans allows us to partial out time trends that are not related to PMAT. The main underlying assumption, of course, is the common trend assumption. In our case this means that municipalities under treatment (i.e., receiving a loan) would have developed similarly to the control group if they had not received the loan at that time.

Appendix Table F.10 shows the results from two-way fixed effects models with incumbent party vote share as the dependent variable and different sets of controls. Given the relatively small number of municipalities with observations before and after the first PMAT payment and the conservative nature of the model with both sets of fixed effects, we generally prefer the models with fewer covariates (and more observations). In all four models, we find evidence that tax increases led to less punishment of the incumbents after the first PMAT payment, in fact, the estimated coefficient is slightly larger. The interaction effect is significant at the 0.05 level in the model without controls and when controlling for population, urban share of the population, and GDP growth (columns 1 and 2 in Table F.10). Once we add covariates for federal transfers and changes in spending, the interaction is significant at the 0.1 level (column 3). When adding covariates for incumbent ideology, education, and partisan congruence with the president's or governor's party, we lose a number of observations and the interaction is no longer significant. Given the

conservativeness of the test, these results from the two-way fixed effects models lend additional credence to our findings and should alleviate concerns about potential alternative explanations.

Accounting for Capacity-Induced Changes In Tax Revenues – Estimating Partial Effects

In this section, we raise and address an issue with the interpretation of our main empirical analysis – namely, that the moderator of interest, PMAT loan payments, has direct effects on our independent variable of interest, tax revenues. In other words, even if mayors in two comparable municipalities undertake the same efforts to increase taxes, these efforts are likely to result in higher revenues in the high-capacity compared to the low-capacity municipality. In the empirical results presented above, the estimated coefficients on the interaction term are in part due to the differential impact of PMAT on tax increases, i.e., these are estimates of the *total effect*.

Of course, our theoretical argument explicitly builds on the idea that tax collection in higher capacity environments is different and likely more efficient. The same increase in revenues may be more formalized and raised less intrusively or haphazardly under high capacity. More explicitly, the same hypothetical increase in tax rates would lead to higher/lower tax revenues under higher/lower fiscal capacity. In that sense, we believe the *total effects* estimated above are the correct quantity of interest given our theoretical argument.

Nevertheless, one might wonder how the estimated effect would change if we compared similar processes of tax increases in low- and high-capacity municipalities. Consider our main independent variable Δ *Tax* above. In cases where PMAT is introduced, one part of the change in revenues is caused by increases in capacity ($\Delta\ Tax_{PMAT}$). Another part is due to other policy changes by elected officials ($\Delta\ Tax_{policy}$), e.g., changes in tax rates. Our results above are a combination of

both, which we view as consistent with our theory. Others may question whether our findings are due to this difference in taxation *caused* by the moderator.

We attempt to isolate the changes in tax revenues that are *not caused* by the reception of PMAT. We empirically decompose Δ *Tax* into $\Delta\ Tax_{PMAT}$ and $\Delta\ Tax_{policy}$. We then estimate the same model from above but using only the estimated $\Delta\ Tax_{policy}$ as our independent variable of interest. To do so, we adapt models recently suggested by Wodtke, Alaca and Zhou (Forthcoming) to adjust for treatment-induced moderators. When time-varying treatments induce changes in potential moderators, standard regression models with interactions would be biased, due to the inclusion of and interaction with the post-treatment variable. As Wodtke, Alaca and Zhou (Forthcoming) show, structural nested means models (SNMM) can be used to arrive at unbiased estimates given treatment induced confounders or moderators. This is achieved by first regressing the moderator on earlier period treatment or confounders and saving the residuals. In a second stage, these first-stage residuals, instead of the *post-treatment moderator*, are then interacted with the treatments in later periods, giving an unbiased estimate of the moderation effect.

The problem in our case is closely related. While our moderator is independent of treatment, we want to isolate the part of the tax increase that is not due to the increase in capacity. We therefore regress our main independent variable of interest, tax increases (Δ *Tax*), on the PMAT status in each municipality. By predicting changes in tax revenue with PMAT, we then isolate the increase in revenue that is due to its introduction. Next, we use the residuals from the first stage (or $\Delta\ Tax_{policy}$) as the main independent variable and interaction term in the model explaining

incumbent party vote share. Specifically, we estimate the following two stage model:

$$\Delta\text{Tax} \sim \alpha_1 + \delta\text{PMAT} + \varepsilon_1$$

$$\Delta\hat{\text{Tax}}_{\text{policy}} = \Delta\text{Tax} - \Delta\hat{\text{Tax}}$$

$$\text{Voteshare} \sim \alpha_2 + \beta_1\Delta\hat{\text{Tax}}_{\text{policy}} + \beta_2\text{PMAT}_{i,t} + \beta_3\Delta\hat{\text{Tax}}_{\text{policy}} \times \text{PMAT}_{i,t} + \gamma\text{Controls}_{i,t} + \varepsilon_2$$

By residualizing the change in tax revenue based on PMAT payment status, we are able to isolate the changes that are not directly caused by PMAT. We can then estimate the moderating effect of fiscal capacity on tax increases without conflating it with the effect of PMAT on tax revenues, i.e., we here estimate the moderated *partial effect*. The as-if-random assignment of PMAT payment status in our sample provides additional confidence in these results.

Given the use of the residualized independent variable, regular standard errors would be an underestimate. We instead estimate a Bayesian model in which both regressions are estimated sequentially at each iteration of the sampler. Uncertainty from the first stage residualization thus automatically travels through to the second stage. Model estimates reflect the full uncertainty associated with both regressions.

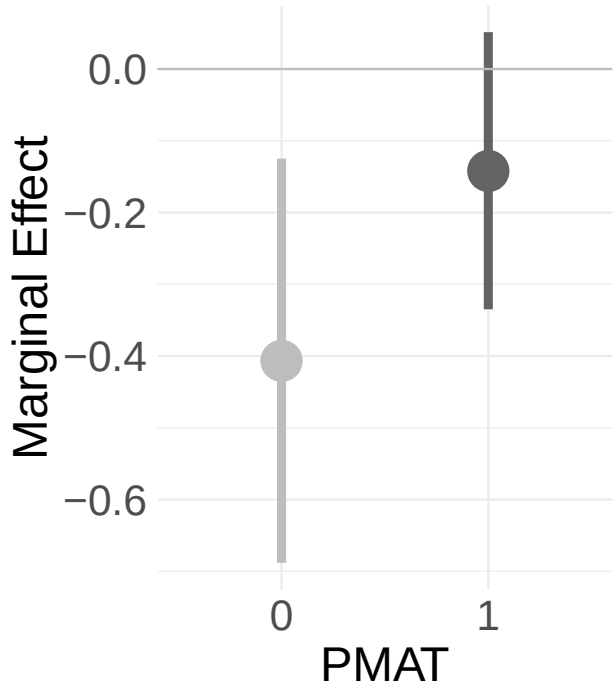
Using this technique, we estimate our main regression models with incumbent party vote share as the dependent variable. In Figure 3 we present the main result from the model with covariates (corresponding to the model presented in Column 2 in Table 2). We standardize all continuous variables and models are estimated using Stan (Stan Development Team, 2017).[13]

Figure 3 shows the marginal effects and 95% uncertainty intervals of the residualized change

[13]Full details on priors, estimation, and convergence are discussed in Appendix G.

in tax revenue on incumbent party vote share conditional on PMAT. The interaction effect is quite similar to that in the standard regression model, even when the interaction is an estimate of the partial effect, i.e., is only based on tax revenue increases estimated to not be caused by PMAT. The political consequence for mayors in low-capacity places is significantly worse than that of mayors in municipalities that already received PMAT. The difference between the two marginal effects is larger than zero with probability 0.96. Table G.11 in the Appendix shows the median estimate and 95% uncertainty intervals for the main variables of interest and the interaction for all estimated SNMM models, including a more robust model with a student t error distribution.

Figure 3: Marginal Effect of residualized Δ Tax Revenues on Incumbent Party Vote Share by PMAT



Note: This plot shows the marginal effects and 95% uncertainty intervals of the residualized change in tax revenue on incumbent party vote share conditional on PMAT payment status. As in the results presented above, we find that a unit increase in tax revenue leads to significantly worse electoral performance of the incumbent in low-capacity compared to high-capacity municipalities.

In our view, these results provide additional evidence in line with our general theoretical

argument. Even when we estimate the interaction of interest between the fiscal capacity shock and tax increases only on the share of tax increases that can not be explained by PMAT, we find effectively the same interaction effect. Voters are much more likely to punish incumbents for tax increases in low-capacity settings.

Capacity Moderates Tax Morale in Exchange for Performance

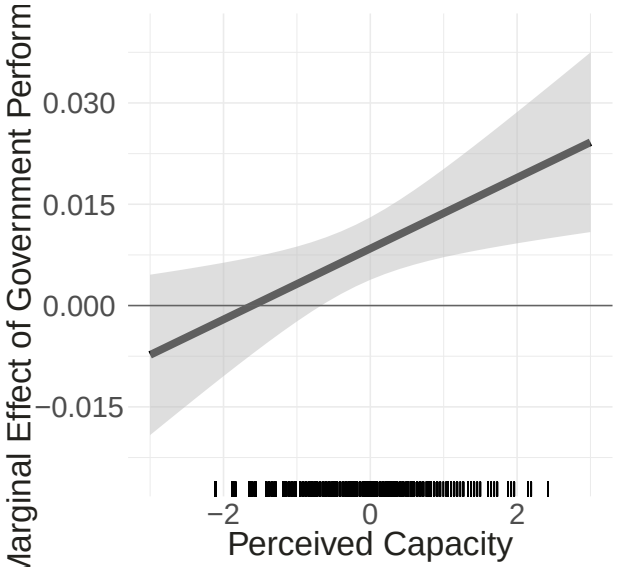
A second implication of our theory, articulated in Hypothesis 2, is that in high-capacity settings politicians can use their popularity or good performance to generate support for (or lessen opposition to) tax increases from voters. In low-capacity settings, on the other hand, voters should see less value in tax increases, even when they approve of politicians. Put differently, even if a voter believes that the politician in power is pursuing her interests, this congruence should not lead to a higher willingness to pay taxes in low-capacity settings. We will thus examine whether the positive relationship between perceived government performance and willingness to pay tax posited by the existing literature is moderated by fiscal capacity.

We test H2 – that politician performance should be more likely to influence citizens' willingness to pay taxes under high vs. low fiscal capacity – using the cross-national survey data from Latin America. The *Capacity Proxy* from Table 1 – an additive index of the perceived *Probability of Sanction* and *Impartiality of Collection* – is the moderating variable. The independent variable, *Perceived Government Performance*, is measured using the following survey question: "Do you agree with the following sentence: the resources received by the National Government are used to improve welfare through better public services?" As the dependent variable, *Willingness to Pay Tax*, we create an indicator for whether the respondent would be willing to pay higher taxes on income, value added (VAT) or fuel. Appendix H presents the result tables and shows robustness to

several specifications.

Figure 4 shows the marginal effect of evaluations of national government performance on willingness to pay tax under varying levels of perceived fiscal capacity (Column 2 in Table H.12). As our theory suggests, there is no relationship between one's evaluation of the government and one's willingness to pay tax at low levels of fiscal capacity. At middling to high levels, however, there is a statistically significant positive relationship between confidence in the government and willingness to pay into the public budget. The coefficient on the interaction term is statistically significant. As illustrated in Appendix Table H.12, the conditionality of the relationship is largely driven by the perceived impartiality of tax collection. This provides evidence in favor of one mechanism we posited in the theory section: that lower willingness to pay when the government performs well is due to perceptions of unfair tax collection in low-capacity states.

Figure 4: Marginal Effect of Government Performance on Willingness to Pay Tax by Capacity



Note: This figure shows the marginal effect of an increase in a respondent's evaluation of the government on the willingness to pay higher taxes at increasing levels of perceived fiscal capacity. In line with our theoretical argument, higher evaluation of the government only increases respondents willingness to pay under higher levels of perceived fiscal capacity.

Discussion: How PMAT Affects Revenue Collection and Beliefs

We have shown that, consistent with our theoretical expectation, incumbents who increase tax revenues in places with higher levels of fiscal capacity are less likely to be punished than incumbents in low-capacity places. We argued voters are more willing to pay tax under higher capacity because such capacity in and of itself, and the manner in which taxes are raised, will induce greater perceptions of compliance. We now return to a closer examination of how taxes are being raised in places with PMAT to further explore how these capacity changes might be affecting voter beliefs.

As we show above, the introduction of PMAT relieves capacity constraints and makes localities more likely to update the cadastre and property valuation. But how do these three levers affect tax revenue? In Appendix Table B.2, we estimate how each relates to subsequent changes in tax revenues in municipalities that have applied to PMAT and the full sample of municipalities.

Both samples show strong evidence that updating valuation maps increases property tax revenues in subsequent years. The results in the PMAT sub-sample show some evidence that law changes also increased revenues. Depending on specification, cadastre updates are associated with higher IPTU revenues in the full sample, but there is no clear evidence of positive effects in the PMAT subsample. Substantively, updating valuations has a much larger and more durable effect on tax revenues than cadastre updates.

Updating valuations may be more effective in increasing IPTU revenue because cadastre updates often add previously unregistered low income properties and informal housing, that will not necessarily pay IPTU. For example, approximately 22% of registered properties in Recife in 2010 were exempt from the IPTU due to their low property values making tax collection unprofitable (Afonso, Araujo and Nóbrega, 2012, 53). Adding more such properties to the cadastre might not significantly add

to the revenue potential. Valuation updates on the other hand are more likely to affect and increase revenue from more valuable properties. Not surprisingly, the difference in true market value to assessed values is largest for high value properties (Afonso, Araujo and Nóbrega, 2012, 22).

Increased tax collection through both cadastre updates and valuation updates should affect both poorer and wealthier properties. Additionally, both policies are more likely after receipt of PMAT loans. Consistent with our theory, if taxpayers perceive capacity increases as affecting all taxpayers through cadastre *and* valuation updates, they should have more positive attitudes towards tax increases. In other words, we expect this more positive reaction compared to a world in which tax increases are only due to updates of the tax base, which is likely to primarily affect poorer taxpayers.

One important difference between cadastre updates and valuation updates, however, is that new valuations have to be approved by the municipal legislative council and can thus face more political resistance (Afonso, Araujo and Nóbrega, 2012). With wealthier property owners well-represented on councils, we would generally expect resistance to higher valuations. Our theory suggests, however, that a municipality's higher capacity (i.e., receipt of PMAT) may increase their willingness to pay by ensuring more equal treatment of all taxpayers. Under higher capacity, even the wealthy should be more confident that their wealthy counterparts also pay, as well as that expansion of the tax base will bring new taxpayers into the tax net. Gadenne (2017*a*) cites one example where such dynamics are borne out. Following the receipt of PMAT, Nova Iguacu doubled the number of properties in its registry, and the average property value doubled in size. This enabled the municipality to introduce a more progressive property tax rate and lower the average rate by nearly 50% – suggesting how, at least in one case, increased capacity leads to more fair tax policy that would likely be valued by both rich and poor because they pay lower rates and

because more taxpayers are now paying.

In addition to cadastre updates, Gadenne (2017*a*) notes that PMAT was often used to increase control of taxpayers (e.g., updating audit methods) and facilitate tax payments. Notably for our theoretical argument made above, the increase in enforcement often went hand in hand with attempts to update population beliefs that widespread tax amnesty was in fact not the norm. The campaigns also emphasized the benefits of increased tax revenues and their necessity for financing essential public services (Gadenne, 2017*a*). Further research is required to confirm these speculative insights about the material consequences of tax capacity increases and how they might differentially affect the willingness to pay of rich and poor voters.

Conclusion

We have argued that the capacity of states to collect taxes will moderate the relationship between voters and politicians that is at the heart of the social contract in a democracy. For politicians and constituents to effectively trade good performance for tax revenue, the state must be capable of collecting revenue efficiently and equally. Without sufficient levels of fiscal capacity, the nexus between taxation and voter evaluations of politicians breaks down for two reasons. First, the unevenness and unreliability that often goes along with low capacity leads to higher levels of perceived unfairness. Second, voters place relatively lower value on tax increases as they are less likely to yield sufficient personal benefits.

We first show this to be true in Brazilian municipalities, where voters react more negatively to tax increases in low capacity settings. We use the federal PMAT program as a quasi-random shock to fiscal capacity mitigating biased inference due to selection effects. We find a significant and robust moderating effect of fiscal capacity across different specifications on the sample of

Brazilian municipalities. We also show that these results remain when we isolate the interaction effect to the tax increase not caused by improvements in capacity.

Additionally, we present survey evidence from Latin American cities to test one of our key assumptions and to generate evidence in support of a secondary observable implication of our argument. First, we show that under higher (perceived) fiscal capacity, voters indeed have higher perceptions of tax compliance. Second, we evaluate whether voters' willingness to pay taxes is moderated by government performance only when perceived fiscal capacity is sufficiently high. We find that under low levels of perceived capacity, positive evaluations of the government have no influence on the willingness to pay tax. Our theory and evidence thus indicate that fiscal capacity moderates the strength of the social contract – or the link between willingness to pay tax and government performance – and that a key mechanism through which this works is an individual's beliefs about others' tax compliance.

What our main research design gains in internal validity, however, it loses in external validity. Our estimand is similar to an average treatment effect on the treated (ATT). By design, all the empirical models testing Hypothesis 1 in this paper only use data on municipalities that eventually receive PMAT loans and should, therefore, be interpreted similarly to a treatment effect on the treated. It is not clear whether we can generalize these results to all municipalities. At a minimum, similar shocks to fiscal capacity should have similar effects in places that look like our counterfactual localities – places where there is political will to increase capacity.

In fact, our theory and empirical results suggest that in places with very low initial capacity, politicians face strong electoral incentives *not* to increase tax revenue or fiscal capacity. An available fiscal capacity shock (similar to PMAT) should not be expected to work there in the same way it works in places with higher initial capacity. We believe, this has important positive

and negative implications for democratic politics.

In bad news, our theory and findings suggest that politicians in electoral democracies will refrain from raising taxes when they fear they can be sanctioned at the ballot box. This is consistent with the finding that mayors up for re-election in Brazil are less likely to update cadastres prior to elections relative to mayors who are term-limited (Christensen and Garfias, Forthcoming) and Holland's (2016) work which argues that politicians actively forego enforcement of tax policy to gain electoral advantage. But different from these studies, we introduce intuition about when these electoral disincentives to tax and enforce will be more likely to bind: in contexts of weak capacity. Our study also implies that the ability of incumbents to exchange electoral support for broad-based public goods provision is limited by weaker revenue collection. To mobilize voters, politicians might instead "perform" in other ways, e.g., targeting smaller benefits to narrower constituencies via private goods or forbearance of enforcement. Notably, such a strategy further undermines the state, creating two distinct equilibria: a low-capacity equilibrium in which politicians do not invest in raising and spending public revenue, and a high-capacity equilibrium in which politicians do invest in raising and spending public revenue. This strategic behavior could help explain the stickiness of clientelistic politics and weak fiscal capacity in many young democracies.

In better news, however, our study suggests that in a place like Brazil, voters are willing to trade off more taxes for better performance but only when fiscal capacity is higher. This is consistent with the finding that when audit reports reveal better-performing incumbents, Brazilians were more willing to pay property tax (Timmons and Garfias, 2015). Another optimistic conclusion from our study is the important role of strategic complementarities in motivating tax compliance. In places where citizens underestimate the actual rate of tax compliance, our findings suggest that simply helping individuals update their priors may increase overall compliance.

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Appendix: *Fiscal Capacity as a Moderator of the Taxation-Accountability Hypothesis*

A Testing Assumption about Perceived Compliance and Capacity

In Table 1, the *Probability of Sanction* is a respondent's belief about the likelihood of being sanctioned should he or she evade taxes. The *Impartiality of Collection* is measured using the following survey question: "On a scale of 1 to 10, where 1 is Favoritism and 10 is Impartiality, would you say that taxes are collected with impartiality or are there favoritism for people who have power?" These two variables capture different aspects of fiscal capacity: the ability to enforce tax regulations and the ability to achieve greater coverage and thus greater impartiality in collection. We thus create a third variable, *Capacity Proxy*, that combines the two in an additive index.

We create the proxy by first standardizing the two individual measures (i.e. subtracting the mean and dividing by the standard error) and then taking their sum. We also standardize the combined proxy and use the standardized individual measures to have all three variables on the same scale. Using the combined Capacity Proxy increases the explained variation in the outcome variable, Perceived Compliance, compared to either component variable on its own. We thus use the combined measure as our proxy for perceived capacity in subsequent analyses.

B Tax Policies, Tax Increases, and PMAT

In Table B.1, we assess the effect of getting a PMAT loan on the three following tax policies that municipalities might use to raise property tax revenue: cadastre updates, valuation updates, and law changes. Our data on these policy changes come from municipality surveys in 2004 and 2015 that reported on the year of the most recent policy change. The year a municipality implements a tax policy is coded as 1. The years since the latest update recorded are coded as 0 in Table B.2 where we examine the effects of the tax policies on tax increases. When we estimate the effect of PMAT on the onset of the three policies in Table B.1, we drop observations in the four years

immediately after a policy comes into effect, since coding these as zero or ones could bias results.[14] The rationale behind the five-year period is that these updates are not meant to occur annually and the updates can be considered current for a period of about five years.

We also code the previous five years before an update as 0 if no update is reported during that time; years prior to this are coded as missing if no other information is available. Our findings are robust to a more conservative coding where we only code the years we know, i.e., the years specifically mentioned in the 2004 or 2015 files and years in between.

Table B.1: Effect of PMAT on Tax Policy (onset coding)

	Cadaster Update	Value Update	Law Change
F.Year of 1st PMAT payment	-0.045	-0.034	0.012
	(0.031)	(0.032)	(0.022)
Year of 1st PMAT payment	0.054	0.084^{+}	0.050^{*}
	(0.039)	(0.048)	(0.024)
L.Year of 1st PMAT payment	0.096^{*}	0.002	0.050^{*}
	(0.047)	(0.035)	(0.024)
L2.Year of 1st PMAT payment	0.011	0.004	0.024
	(0.043)	(0.035)	(0.021)
L3.Year of 1st PMAT payment	0.024	0.052	0.042^{*}
	(0.043)	(0.037)	(0.021)
Constant	0.081^{**}	0.085^{*}	0.041^{*}
	(0.021)	(0.040)	(0.019)
Observations	1700	1232	2224

Panel regression with municipality and year fixed effects. Standard errors clustered by municipality.
$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

[14] See: McGrath, Liam F. 2015. Estimating Onsets of Binary Events in Panel Data. Political Analysis, 23(4), 534-549. doi:10.1093/pan/mpv019

Table B.2: Effect of Tax Policies on Tax Levels

	PMAT municipalities			Full sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Cadaster Update	−0.008	−0.021	0.011	0.077*	0.077+	0.079
	(0.020)	(0.022)	(0.026)	(0.036)	(0.043)	(0.061)
L.Cadaster Update	0.023	0.009	−0.002	0.079*	0.072	0.110*
	(0.027)	(0.029)	(0.030)	(0.035)	(0.045)	(0.054)
L2.Cadaster Update		0.002	0.006		0.037	0.046
		(0.031)	(0.029)		(0.038)	(0.052)
L3.Cadaster Update			0.038			0.015
			(0.025)			(0.059)
Value Update	0.047+	0.046+	0.017	−0.038	−0.041	0.015
	(0.025)	(0.026)	(0.029)	(0.039)	(0.044)	(0.055)
L.Value Update	0.108**	0.106**	0.106**	0.133**	0.132**	0.144*
	(0.029)	(0.029)	(0.031)	(0.038)	(0.045)	(0.056)
L2.Value Update		0.129**	0.111**		0.157**	0.168**
		(0.027)	(0.026)		(0.046)	(0.060)
L3.Value Update			0.061+			0.119*
			(0.032)			(0.057)
Law Change	0.018	0.003	−0.043	−0.044	−0.043	−0.018
	(0.037)	(0.036)	(0.038)	(0.047)	(0.051)	(0.062)
L.Law Change	0.055+	0.060*	0.040	0.019	0.024	−0.014
	(0.030)	(0.030)	(0.032)	(0.058)	(0.071)	(0.082)
L2.Law Change		−0.019	−0.030		0.063	0.070
		(0.033)	(0.035)		(0.063)	(0.083)
L3.Law Change			−0.023			0.016
			(0.031)			(0.060)
L.Population (logged)	0.470	0.393	0.827*	0.115	−0.133	−0.308
	(0.376)	(0.305)	(0.386)	(0.302)	(0.338)	(0.489)
L.Urban Population Share	0.000	0.000*	0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
L.GDP (logged)	0.067	0.087	0.137	0.254**	0.244**	0.272**
	(0.090)	(0.087)	(0.092)	(0.079)	(0.076)	(0.094)
L.Transfers (logged)	−0.014	−0.082	−0.062	0.020	0.020	0.024
	(0.125)	(0.103)	(0.106)	(0.022)	(0.020)	(0.019)
Constant	7.844	9.902**	4.265	6.510*	9.318**	10.892*
	(5.131)	(3.463)	(4.408)	(3.018)	(3.360)	(4.698)
Observations	1345	1022	816	9402	7026	5523

Panel regression with municipality and year fixed effects. Standard errors clustered by municipality.
$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

C As-if-Randomness of Payment

Here, we provide support for the assumption that, conditional on having applied to PMAT, the timing of the first payment is as-if-random. We regress an indicator of whether a municipality has received their first loan payment in the current electoral period on a host of independent variables, e.g., property tax revenue, GDP, transfers, spending, population size, mayoral characteristics, previous incumbent vote share and vote share squared, etc. All independent variables here are measured at the beginning of the electoral term. The results below do not provide any evidence that would lead us to believe that payment order is somehow manipulated or politically determined. It is important to note that the difference in significance between columns 1 and 2 seems to be entirely due to the loss in observations associated with the inclusion of the additional covariates. Specifically, if we subset the included observations to those non-missing for all variables in column 2 but do not include the additional covariates, the results are quite similar.

Table C.3: Regression of First PMAT Payment on Covariates

	(1)	(2)
IPTU (logged)	−0.027	−0.010
	(0.034)	(0.040)
Transfers (logged)	−0.053	0.079
	(0.106)	(0.125)
GDP (logged)	−0.064	−0.105^{+}
	(0.054)	(0.060)
Population (logged)	−0.078	−0.107
	(0.058)	(0.067)
Total Spending (logged)	0.118	0.085
	(0.108)	(0.119)
Total Taxes (logged)	0.085	0.052
	(0.064)	(0.071)
Urban Population Share	−0.000	0.000
	(0.000)	(0.000)
Vote Share Prior Election	−0.246	−0.464
	(1.095)	(1.293)
Vote Share Prior Election × Vote Share Prior Election	0.211	0.495
	(0.978)	(1.173)
Years since PMAT application	−0.015	−0.025*
	(0.009)	(0.011)
Party of the President		−0.019
		(0.059)
Party of the Governor		0.043
		(0.053)
Leftist Mayor Prior Election		0.002
		(0.050)
Incumbent Mayor Education		−0.003
		(0.016)
Constant	0.128	−0.435
	(0.804)	(0.907)
Observations	507	436

OLS models with standard errors clustered by municipality. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

D Continuous instead of Binary Moderator Variable

In the below table, Column 1 repeats the main specification from the paper (Column 2 in Table 2) as a point of comparison. Column 2 implements a continuous rather than binary moderating variable measuring the number of years since the first PMAT loan was received; and coded zero if the loan has not been received by the election year t. Column 3 simply re-runs the main specification in Column 1 with and additional control for years since PMAT application, which we discuss in the main text as a robustness check. Columns 4 to 6 replicate these same specifications on a restricted sample that includes places that applied to the PMAT program between one and five years before the election at time t.

Table D.4: Relationship between Δ Taxation $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t)

	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue (logged)	-0.031^{**}	-0.024^{**}	-0.036^{**}	-0.032^{**}	-0.038^{**}	-0.033^{**}
	(0.009)	(0.008)	(0.010)	(0.011)	(0.011)	(0.011)
Received PMAT loan	-0.223^{+}		-0.193	-0.239		-0.150
	(0.114)		(0.131)	(0.149)		(0.147)
Δ Tax Revenue (logged) $\times$ Received PMAT loan	0.021^{*}		0.018^{+}	0.020^{+}		0.018
	(0.009)		(0.010)	(0.011)		(0.011)
Δ Tax Revenue (logged) $\times$ Years since PMAT loan		0.003^{+}			0.011^{**}	
		(0.002)			(0.003)	
Vote Share Prior Election	0.349^{**}	0.351^{**}	0.406^{**}	0.407^{**}	0.392^{**}	0.438^{**}
	(0.102)	(0.102)	(0.117)	(0.141)	(0.135)	(0.132)
Years since PMAT application			-0.008			-0.035^{**}
			(0.007)			(0.013)
Observations	308	307	253	165	165	165

OLS models with standard errors clustered by municipality. Sample restricted to municipalities where incumbent party re-ran. All models include controls for Population (ln), Share of Urban Population, GDP growth, Federal Transfers (ln), Same Party as Governor, Same Party as President, Incumbent Ideology, Incumbent Education, and Change in Public Spending. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$.

E Robustness Checks

In Table E.5, we report results of the falsification test described in the main text. Table E.5 shows the results from a simple placebo test where we replace our coding of the PMAT variable from an indicator of having received the loan before the election in year t to an indicator of having received the loan after the election t, i.e., we shift the coding of having received the first payment by four years into the future. Receiving a loan in a future government mandate should not affect the way voters condition behavior in the present electoral cycle. The placebo test confirms the absence of any trend in the data in which voter support for the incumbent is conditional on the current change in taxation moderated by future receipts of payment.

Table E.5: Relationship between Δ Taxation $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t) Conditional on PMAT Lead

	Incumbent Party Runs			Incumbent Mayor Runs		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue (logged)	0.007	-0.011	-0.014	-0.008	-0.029	-0.033
	(0.019)	(0.024)	(0.025)	(0.011)	(0.023)	(0.024)
PMAT Payment Lead	0.211	0.122	0.132	-0.065	-0.026	-0.066
	(0.248)	(0.277)	(0.288)	(0.204)	(0.321)	(0.323)
Δ Tax Revenue (logged) $\times$ PMAT Payment Lead	-0.016	-0.009	-0.006	0.004	0.001	0.006
	(0.021)	(0.023)	(0.024)	(0.014)	(0.023)	(0.024)
Vote Share Prior Election	0.139	0.231	0.275^{+}	0.171	0.154	0.162
	(0.138)	(0.156)	(0.157)	(0.185)	(0.207)	(0.206)
Observations	133	118	117	109	92	91

OLS models with standard errors clustered by municipality. All models include controls for Population (ln), Share of Urban Population, GDP growth, Federal Transfers (ln), Same Party as Governor, Same Party as President, Incumbent Ideology, Incumbent Education, and Change in Public Spending. The small sample size reflects the use of only the 2004 election data. We cannot used the 2008 election with a 4-year payment lead because all municipalities that apply get paid by 2010, leaving no counterfactual. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$.

In Table E.6 we show the results from a placebo test where we include logged changes in spending instead of taxation as our main independent variable. The coefficient on the interaction is substantially smaller and never statistically significant. In contrast to tax increases, there is no discernible differential effect of spending increases on incumbent vote share across low and high capacity municipalities. Similarly, Table E.7 shows the results when we use logged increases in

transfers instead of tax increases in the interaction. Again, in contrast to our results with taxation, there is no clear evidence of an interaction effect.

Table E.6: Relationship between Δ Spending $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t) Conditional on PMAT

	Incumbent Party Runs Again			Incumbent Mayor Runs Again		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Spending (logged)	0.011	0.043*	0.049*	0.008	0.027	0.030
	(0.013)	(0.017)	(0.019)	(0.015)	(0.018)	(0.019)
Received PMAT loan	0.040	0.018	0.035	−0.190	−0.188	−0.157
	(0.218)	(0.242)	(0.273)	(0.248)	(0.280)	(0.282)
Δ Spending (logged) × Received PMAT loan	−0.001	0.001	0.001	0.013	0.014	0.011
	(0.014)	(0.016)	(0.019)	(0.016)	(0.018)	(0.019)
Vote Share Prior Election	0.238*	0.253*	0.316*	0.197+	0.186+	0.228+
	(0.098)	(0.104)	(0.123)	(0.102)	(0.108)	(0.136)
Population (logged)		0.030	0.002		0.007	−0.015
		(0.026)	(0.026)		(0.028)	(0.029)
Urban Population Share		0.000+	0.001**		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		−0.044	−0.057		−0.054	−0.034
		(0.059)	(0.076)		(0.061)	(0.085)
Amount of Transfers (logged)		−0.085*	−0.054		−0.041	−0.010
		(0.034)	(0.035)		(0.035)	(0.035)
Party of the Governor		−0.020	−0.013		−0.013	−0.003
		(0.021)	(0.023)		(0.022)	(0.024)
Party of the President		0.016	0.001		0.048+	0.037
		(0.027)	(0.030)		(0.026)	(0.028)
Incumbent Mayor Ideology		0.007	0.009		0.020	0.025
		(0.014)	(0.017)		(0.014)	(0.015)
Incumbent Mayor Education		0.017*	0.021**		0.010	0.015+
		(0.007)	(0.008)		(0.008)	(0.008)
Years since PMAT application			−0.009			−0.002
			(0.007)			(0.006)
Constant	0.156	0.657+	0.291	0.265	0.460	0.069
	(0.205)	(0.374)	(0.389)	(0.240)	(0.406)	(0.400)
Observations	296	270	225	264	237	200

OLS models with standard errors clustered by municipality. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

Additionally, we undertake a number of robustness checks where we add covariates to the models presented in our main Table 2. First, Table E.8 shows that our results are robust to including previous incumbent vote share squared, i.e., accounting for a potential non-linear effect of previous incumbent vote share. Next, Table E.9 presents the results from our three main models when we use the incumbent party's vote share margin instead of its vote share as the dependent variable. Specifically, the variable is calculated as the difference between the incumbent party's vote share

Table E.7: Relationship between Δ Transfers $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t) Conditional on PMAT

	Incumbent Runs Again			Incumbent Party Runs		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Transfers	0.004	0.036^{+}	0.043^{+}	0.001	0.060^{**}	0.069^{**}
	(0.016)	(0.020)	(0.022)	(0.014)	(0.020)	(0.023)
Received PMAT loan	-0.341	-0.321	-0.151	-0.144	-0.246	-0.224
	(0.279)	(0.324)	(0.342)	(0.257)	(0.264)	(0.298)
Δ Transfers $\times$ Received PMAT loan	0.023	0.023	0.012	0.012	0.019	0.018
	(0.018)	(0.021)	(0.022)	(0.016)	(0.017)	(0.019)
Vote Share Prior Election	0.325^{**}	0.264^{*}	0.329^{*}	0.322^{**}	0.354^{**}	0.416^{**}
	(0.105)	(0.119)	(0.145)	(0.083)	(0.097)	(0.109)
Population (logged)		-0.030	-0.053		-0.010	-0.040
		(0.030)	(0.033)		(0.026)	(0.028)
Urban Population Share		0.000	0.000		0.000	0.001^{**}
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		-0.072	-0.062		-0.057	-0.061
		(0.054)	(0.073)		(0.057)	(0.073)
Amount of Transfers (logged)		-0.014	0.020		-0.066^{*}	-0.035
		(0.036)	(0.040)		(0.032)	(0.035)
Party of the Governor		0.001	0.006		0.003	0.010
		(0.023)	(0.025)		(0.021)	(0.023)
Party of the President		0.043^{+}	0.033		0.023	0.011
		(0.026)	(0.028)		(0.025)	(0.028)
Incumbent Mayor Ideology		0.018	0.020		0.012	0.020
		(0.013)	(0.014)		(0.013)	(0.016)
Incumbent Mayor Education		0.007	0.010		0.012^{+}	0.015^{+}
		(0.008)	(0.008)		(0.007)	(0.008)
Δ Spending		0.000	0.000		-0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Years since PMAT application			-0.005			-0.008
			(0.007)			(0.007)
Constant	0.250	0.226	-0.246	0.235	0.403	-0.013
	(0.264)	(0.415)	(0.450)	(0.233)	(0.350)	(0.388)
Observations	307	270	226	364	326	269

OLS models with robust standard errors. $^{*}p < 0.05$, $^{**}p < 0.01$

and the party with the most votes (if the incumbent party did not win) or the party with the second most votes. Column 1 in Table E.9 shows the results when we control only for the incumbent party's margin in the previous election, in column 2 we add our main control variables, and column 3 shows the results when we add the control for time since the application to PMAT. In line with the results presented in the main text, the coefficient on the interaction between having received the first PMAT payment and increases in tax revenues is estimated to be positive in all the models and significant at the 0.1 in columns 1 and 2. Once we control for time since application, the interaction effect is not significant with incumbent party margin as the dependent variable.

F Two-Way Fixed Effects Models

To estimate the fixed effects model we create a panel of all municipalities that eventually apply to PMAT at three time points: the municipal elections in 2000, 2004, and 2008 (though due to lack of data for incumbency status and other covariates for 2000, the analysis is limited to the elections in 2004 and 2008). The dependent and independent variables are measured as in our main analysis above, but models now include municipal and election fixed effects. While the assumptions for the fixed effect model are more restrictive and allow us to control for potential time-invariant omitted variables at the municipal level or election-specific trends, our sample is limited in the number of cases that have a change in PMAT status within the two election periods.

Nevertheless, the estimated effect on the interaction is actually larger in the fixed effects model than the models presented in the main text. Table F.10 shows the results from three models with incumbent party vote share as the dependent variable. Standard errors are clustered at the municipality. Our main interest again lies in the interaction between changes in taxation and the receipt of the first PMAT loan. As in the results presented in the main text, tax increases have differential effects pre- and post-PMAT payments. The interaction effect is estimated to be positive and quite substantial in all four models. The precision of the estimated effect, however, decreases as we add more covariates and the sample size decreases.

Table E.8: Relationship between Δ Taxation $(t_{-1} - t_{-3})$ and Incumbent Vote Share (t) Conditional on PMAT (Previous Vote Share Squared)

	Incumbent Party			Incumbent Mayor		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue (logged)	-0.019^{**}	-0.031^{**}	-0.030^{**}	-0.013^{+}	-0.031^{**}	-0.028^{**}
	(0.007)	(0.009)	(0.010)	(0.008)	(0.010)	(0.009)
Received PMAT loan	-0.195^{+}	-0.223^{+}	-0.206^{+}	-0.212^{+}	-0.174	-0.089
	(0.102)	(0.114)	(0.119)	(0.117)	(0.125)	(0.123)
Δ Tax Revenue (logged) $\times$ Received PMAT loan	0.018^{*}	0.021^{*}	0.019^{*}	0.019^{*}	0.017^{+}	0.013
	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Vote Share Prior Election	0.418	0.277	0.285	-0.379	-0.430	-0.424
	(0.423)	(0.546)	(0.553)	(0.603)	(0.653)	(0.626)
Vote Share Prior Election $\times$ Vote Share Prior Election	-0.126	0.065	0.057	0.632	0.682	0.683
	(0.346)	(0.475)	(0.481)	(0.585)	(0.626)	(0.593)
Population (logged)		0.011	0.011		0.010	-0.003
		(0.027)	(0.027)		(0.026)	(0.027)
Urban Population Share		0.000^{+}	0.000^{+}		0.000^{+}	0.000^{+}
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		-0.050	-0.050		-0.122^{**}	-0.119^{**}
		(0.057)	(0.057)		(0.045)	(0.045)
Amount of Transfers (logged)		0.004	0.004		0.017	0.036
		(0.030)	(0.031)		(0.030)	(0.030)
Party of the Governor		-0.016	-0.016		-0.010	-0.009
		(0.022)	(0.023)		(0.023)	(0.023)
Party of the President		0.045^{+}	0.045^{+}		0.057^{*}	0.051^{*}
		(0.025)	(0.025)		(0.025)	(0.025)
Incumbent Mayor Ideology		0.011	0.010		0.018	0.018
		(0.014)	(0.014)		(0.012)	(0.012)
Incumbent Mayor Education		0.013^{+}	0.013^{+}		0.005	0.005
		(0.007)	(0.007)		(0.008)	(0.008)
Δ Spending		-0.000	-0.000		0.000	0.000^{+}
		(0.000)	(0.000)		(0.000)	(0.000)
(mean) distance			0.000			-0.010
			(0.007)			(0.007)
Constant	0.485^{**}	0.355	0.329	0.670^{**}	0.395	0.176
	(0.172)	(0.326)	(0.320)	(0.202)	(0.373)	(0.356)
Observations	343	308	307	288	253	252

OLS models with with standard errors clustered by municipality. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

Table E.9: Relationship between Δ Taxation $(t_{-1} - t_{-3})$ and Vote Margin (t) Conditional on PMAT

	Incumbent Party		
	(1)	(2)	(3)
Δ Tax Revenue (logged)	-0.012	-0.040^{**}	-0.042^{**}
	(0.010)	(0.014)	(0.015)
Received PMAT loan	-0.232	-0.289	-0.220
	(0.161)	(0.177)	(0.193)
Δ Tax Revenue (logged) $\times$ Received PMAT loan	0.021^{+}	0.025^{+}	0.020
	(0.013)	(0.014)	(0.015)
(mean) Margin04	0.329^{**}	0.384^{**}	0.411^{**}
	(0.104)	(0.103)	(0.106)
Population (logged)		0.024	0.003
		(0.041)	(0.043)
Urban Population Share		0.001^{*}	0.001^{**}
		(0.000)	(0.000)
GDP Growth		0.063	0.066
		(0.079)	(0.103)
Amount of Transfers (logged)		0.019	0.053
		(0.043)	(0.046)
Party of the Governor		-0.043	-0.046
		(0.031)	(0.035)
Party of the President		0.094^{**}	0.086^{*}
		(0.034)	(0.039)
Incumbent Mayor Ideology		0.026	0.030
		(0.020)	(0.024)
Incumbent Mayor Education		0.025^{**}	0.027^{**}
		(0.009)	(0.010)
Years since PMAT application			-0.009
			(0.011)
Constant	0.110	-0.403	-0.764^{+}
	(0.138)	(0.390)	(0.431)
Observations	339	306	252

OLS models with standard errors clustered by municipality. Sample restricted to municipalities where incumbent party re-ran. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$.

Table F.10: Fixed Effects Regression Estimating Effect of Δ Taxation $(t_{-1} - t_{-3})$ on Incumbent Party Vote Share (t) Conditional on PMAT

	Inc Party (1)	Inc Party (2)	Inc Party (3)	(4)
Δ Tax Revenue (logged)	-0.005	-0.007	-0.006	-0.035
	(0.024)	(0.025)	(0.025)	(0.025)
Received PMAT loan	-0.449^{*}	-0.435^{*}	-0.399^{+}	-0.298
	(0.191)	(0.198)	(0.213)	(0.272)
Δ Tax Revenue (logged) $\times$ Received PMAT loan	0.033^{*}	0.032^{*}	0.030^{+}	0.024
	(0.015)	(0.015)	(0.016)	(0.021)
Population (logged)		0.254	0.316	0.840
		(0.773)	(0.826)	(0.806)
Urban Population Share		-0.000	0.000	0.001
		(0.001)	(0.001)	(0.001)
GDP Growth		-0.120	-0.130	-0.253^{+}
		(0.148)	(0.147)	(0.146)
Amount of Transfers (logged)			-0.068	-0.442
			(0.237)	(0.288)
Δ Spending			-0.000	-0.000
			(0.000)	(0.000)
Party of the Governor				-0.047
				(0.047)
Party of the President				-0.065
				(0.043)
Incumbent Mayor Education				-0.006
				(0.016)
Incumbent Mayor Ideology				-0.101^{*}
				(0.040)
Constant	0.485	-2.297	-1.834	-0.524
	(0.313)	(8.585)	(8.382)	(7.946)
Observations	445	444	444	399
Municipal FE	Yes	Yes	Yes	
Election FE	Yes	Yes	Yes	

Panel regression with municipality and year fixed effects. Standard errors clustered by municipality.
$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$

G Bayesian Structural Nested Means Model

Models are estimated using Hamiltonian Monte Carlo in Stan (Stan Development Team, 2017). We standardize all continuous variables and specify uninformative Gaussian (0, 1) priors for all coefficient estimates and half-Cauchy priors for the scale parameters in both stages. For each of these models, we run four chains of 5000 iterations (1000 warmups) and save every second iteration. This leaves us us with 8000 draws from the posterior distribution. We inspected chains visually and checked *rhat* values. None of the *rhat* values across all models and parameters exceeded 1.01. All evidence suggested that the chains converged.

Table G.11: Relationship between Residualized Δ Taxation $(t_1 t_3)$ and Incumbent Vote Share (t) Conditional on PMAT (SNMM-Model)

	Gaussian Errors			Student t Errors
	No Controls	w. Controls	w. Controls & Time	w. Controls
Residualized Δ Tax	-0.247	-0.407	-0.456	-0.411
	[-0.475, -0.023]	[-0.688, -0.125]	[-0.762, -0.149]	[-0.688, -0.135]
Received PMAT Loan	0.197	0.213	0.158	0.220
	[-0.038, 0.431]	[-0.076, 0.494]	[-0.239, 0.546]	[-0.065, 0.503]
Residualized Δ Tax $\times$ PMAT	0.235	0.262	0.225	0.265
	[-0.026, 0.484]	[-0.020, 0.539]	[-0.092, 0.535]	[-0.020, 0.541]
Vote Share Prior Election	0.177	0.224	0.261	0.214
	[0.077, 0.277]	[0.114, 0.339]	[0.133, 0.391]	[0.096, 0.330]

Models estimated in rstan. First stage regresses change in tax revenue on PMAT status. Second stage includes the first stage residuals and their interaction with PMAT status as well as relevant covariates. All second stage models include a control for incumbent vote share in the previous election. All models estimated on four chains with 5000 iterations each. The first 1000 iterations are warm up and we save every second iterations, resulting in 8000 total samples in the posterior

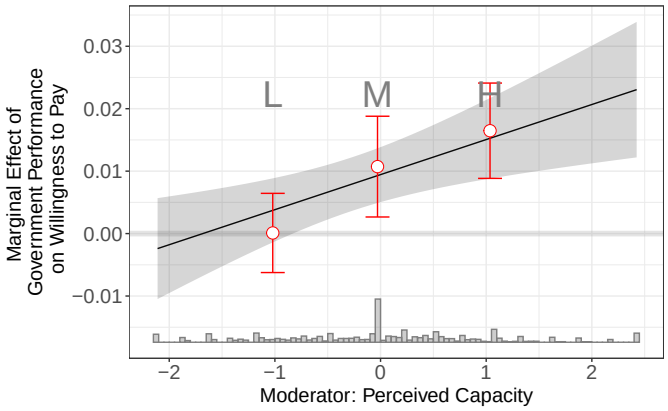
H Micro-Evidence

Table H.12 estimates linear probability models for our analyses of survey data with and without survey weights, as well as logit models. We first estimate the three types of models only with the variables of interest and country intercepts. We then add covariates to control for housing type, home ownership, occupation, education, and financial sophistication. All results include robust standard errors. As we then show in Table H.13, the results are very similar when using the individual measures of willingness to pay tax for the income, VAT, or fuel taxes as separate

dependent variables.

Figure H.1 displays the interaction effect from the linear probability model in Table H.12 when binning the capacity variable in line with the recommendation by Hainmueller, Mummolo and Xu (2018).[15]

Figure H.1: Marginal Effect of Government Performance on Willingness to Pay by Perceived Capacity with Binning



[15]Hainmueller, Jens, Jonathan Mummolo and Yiqing Xu. 2018. "How Much Should We Trust Estimates From Multiplicative Interaction Models? Simple Tools To Improve Empirical Practice." *Political Analysis* 27(2):163–192. https://doi.org/10.1017/pan.2018.46

Table H.12: Willingness to Pay

	(1)	(2)	(3)	(4)
Gov't Performance	0.009**	0.008**	0.009**	0.008**
	(0.002)	(0.002)	(0.002)	(0.002)
Capacity Proxy	-0.020*	-0.017		
	(0.009)	(0.010)		
Impartiality of Collection			-0.019*	-0.017
			(0.008)	(0.009)
Performance × Capacity	0.006**	0.005**		
	(0.002)	(0.002)		
Performance × Collection			0.005**	0.005*
			(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
R2 Adj.	0.095	0.114	0.090	0.109
N	7906	7358	9308	8628

Note: Linear models (OLS) with robust standard errors and country fixed effects. Models with controls include covariates for level of education, occupation, main income source, housing type, home ownership status, and financial sophistication. $^{+}$ p < 0.1, * p < 0.05, ** p < 0.01,

Table H.13: Willingness to Pay by Tax Type

	Combined Indicator	**Income Tax**	**VAT**	**Fuel Tax**
	(1)	(2)	(3)	(4)
Gov't Performance	0.009**	0.006**	0.007**	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Capacity Proxy	-0.020*	-0.013	-0.015	-0.013
	(0.009)	(0.007)	(0.008)	(0.008)
Performance × Capacity	0.006**	0.003	0.004*	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
R2 Adj.	0.095	0.033	0.087	0.030
N	7906	7747	7824	6085
Country FE	Yes	Yes	Yes	Yes

Note: Linear models (OLS) with robust standard errors and country fixed effects.
$^{+}$ p < 0.1, * p < 0.05, ** p < 0.01,