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

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Abstract

Much work in political economy assumes a strong link between taxation and political representation, e.g., that taxation increases voters' bargaining power and therefore electoral accountability. We challenge one critical but implicit assumption in these arguments: that states always have sufficient capacity to collect taxes. Instead, we argue that fiscal capacity moderates the relationship between taxation and political representation. In low-capacity environments voters are less likely to approve of higher taxes than under higher capacity. 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 places with low capacity are severely punished for raising taxes, while those that have already received these loans are not. We additionally use survey data to test the microfoundations of our argument.

Key Words: Fiscal Capacity, Taxation, Democracy, Accountability

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The link between taxation and democratic accountability has long been at the forefront of debates in the political economy literature. Scholars have been particularly interested in whether (1) increasing taxation leads citizens to demand more representative institutions (e.g., Bates and Lien, 1985); and, on the flip-side, (2) whether changes towards more democratic systems lead to higher levels of taxation (e.g., Timmons, 2010). A majority of the literature, however, assumes that states are capable of enforcing tax policies.assumes that states are capable of enforcing tax policies. The state is generally assumed to be both able and willing to collect taxes efficient 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 proposed link between taxation and representation?

In this paper, we ask whether the state's fiscal capacity changes voters' attitudes about taxation and their demands on elected officials. We argue that, in low fiscal capacity settings, most citizens have less reason to demand higher taxation and thus are less (more) likely to reward (punish) politicians for tax increases. The logic of our argument is as follows. In an equilibrium where fiscal capacity is low, citizens have little reason to believe that an increase in taxation will affect other taxpayers equally and have positive returns. They are more likely to oppose higher taxes as any additional increase in taxation will exacerbate unfairness inherent in differential enforcement under low capacity. And, returns to taxation are more marginal when fewer others are also paying. In high-capacity settings, on the other hand, we expect the traditional theoretical argument to hold, such that the majority of voters

[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, 2019).

(who are generally below the median income) prefer higher taxation. We therefore expect that politicians are more likely to be punished for tax increases in low-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.

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.

In line with our theoretical argument, we find consistent evidence across many empirical specifications that among places with PMAT, increases in taxation yield electoral advantages for the incumbent relative to similar places without PMAT.These results are robust to a number of alternative specifications.In addition, to account for the fact that changes to the moderator (fiscal capacity) have a direct effects on the variable of interest (tax increases), we estimate a type of structural nested means model to isolate the interaction effect on the part of the tax increases not caused by changes in fiscal capacity. Our main findings remain

unchanged.

Lastly, we provide micro-level evidence in line with some of our theoretical expectations. Using survey evidence, 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.

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 assume sufficient fiscal capacity; our insights are particularly relevant for young, developing democracies where low fiscal capacity is the norm. A key theoretical contribution is to include the beliefs of voters about what other voters are paying in taxes in the electoral calculus.

Empirically, making use of a unique "shock" to fiscal capacity, we show that voters evaluate tax increases differently depending on levels of capacity. We also introduce the use of a Bayesian structural nested means model to isolate interaction effects with the treatment not caused by the moderator. 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 (encourage) taxation by incumbents in low (high) 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. Yet, citizens hold preferences over costly government policies, such that they may tolerate or even reward higher taxation (Bates and Lien, 1985). The social contract, or exchange of tax compliance for valued government services, is reinforced by elections 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).[2] We argue that the ability of the state to collect taxes fundamentally changes this this link between taxation and electoral accountability. If fiscal capacity is sufficiently low, voters will be less willing to submit to government taxation in anticipation of benefits. This will occur in low fiscal-capacity states for two reasons: 1) taxpayers are less likely to think their own payments are fair when fewer others are also paying; 2) taxpayers perceive lower returns on their investment (i.e., their tax payment) as the total collected revenue and thus government budget is smaller.

We argue that fiscal capacity moderates citizen perceptions of both fairness and efficiency 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

[2]However, de la Cuesta et al. (2019) find that, at least in Uganda and Ghana, this is not necessarily the case.

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, and Fairness

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 is communicated to them by the state, and direct interactions with agents of the state (Gottlieb, 2019). When the experience of paying taxes for an individual citizen is more systematic and regularized, they infer that other potential taxpayers are also engaging with the state in this way – increasing perceptions of tax compliance.

Table 1: Perceived Compliance and Fiscal Capacity

	Perceived Individual Compliance			Perceived Firm Compliance		
Probability of Sanction	0.33***			0.13***		
	(0.04)			(0.04)		
Impartiality of Collection		0.20***			0.32***	
		(0.04)			(0.04)	
Capacity Proxy			0.40***			0.30***
			(0.04)			(0.04)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8,102	9,259	7,906	8,034	9,206	7,860
R^2	0.08	0.05	0.09	0.07	0.09	0.09

Linear models (OLS) with robust standard errors and country fixed effects.
***p < .01; **p < .05; *p < .1

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.[3] In Table 1, we show evidence of a strong positive relationship between several measures of perceived fiscal capacity and perceived rates of tax compliance.[4] *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). Perceived capacity, the independent variable of interest is measured in several ways[5], 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. These observations and resulting inferences about the state's capacity, in turn, influence beliefs about the overall level of tax compliance.

Voter Beliefs

We thus contend that greater (perceptions of) fiscal capacity increases a citizen's confidence in others' tax compliance. We argue that these differences in beliefs about overall tax compliance fundamentally change voters' views over taxation through two channels: 1) fairness of collection; 2) material returns to tax payments. These strategic complementarities in which taxpayer i receives greater utility from paying taxes when more taxpayers $-i$ also pay, therefore, arise for two reasons.

[3] 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.

[4] In Appendix H, we also show perceived tax compliance is related to *actual* levels of capacity.

[5] See Appendix H for details.

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. Independent of the rates of taxation, i.e., the progressivity or fairness of the tax system, impartial and equal enforcement of the *de jure* rates will improve voters' attitudes about taxation. 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, because tax enforcement is equally likely to capture taxpayers $-i$.Where fiscal capacity is higher, tax collection is seen as fairer and individual attitudes towards taxation are more positive since voters believe others are likely 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 (1988, 53) noted in her seminal 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 – 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.

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 (1988, 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.

To summarize, we contend that greater (perceptions of) 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 in terms of perceptions of fairness (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

[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.

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. A retrospective evaluation of tax increases (Ferejohn, 1986), should therefore lead to differential assessment of politicians under low and high levels of capacity. Introducing the moderator of fiscal capacity into the taxpayer's voting calculus thus yields an important 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 following election than when a high fiscal capacity government raises tax revenue by the same amount.*

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.

Fiscal Capacity Shocks and Taxation in Brazil

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 our theoretical argument using data on Brazilian municipalities from 2000 to 2012. 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).While a substantial share of municipal revenue 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 to understand whether it moderates the electoral response to increases in taxation.

In particular, we make use of a federal program in Brazil that was designed to increase the tax and administrative capacity of municipal governments. In 1998, after much dismay with the performance of local tax collection efforts, 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 tax. 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). As Gadenne (2017) 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

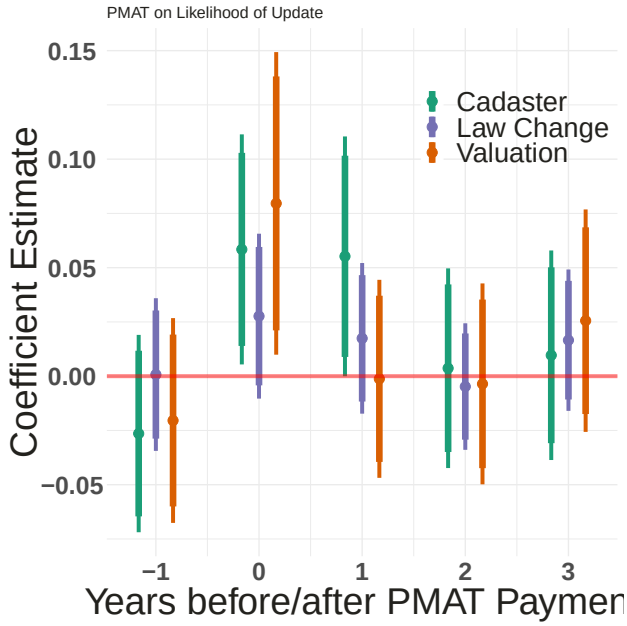
Property taxes in Brazilian municipalities are based on the *fair market value* of a given property to which the local IPTU rate is applied (Afonso, Araujo and Nóbrega, 2012). To assess property taxes, a municipality must therefore have a registration of buildings/properties (property cadastre) and an 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 (usually based on building type) (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. Indeed, 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). Figure 1 confirms that an infusion of fiscal capacity through the PMAT program has a positive and significant effect on the likelihood of cadaster and value updates but not on rate changes.[7]

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

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 cadaster updates, updates of the property valuations, or changes to the IPTU laws. Results are based on panel regressions estimated as linear probability models with municipality and year fixed effects. Standard errors are clustered by municipality.

Exogeneity of the Moderator – Fiscal Capacity

In an ideal world, we would randomly assign a fiscal capacity shock to incumbent governments in a random half of municipalities. We would assign a universal increase in tax revenue and study whether voter behavior in the subsequent election is different across groups. Instead, we approximate randomness of the initial fiscal capacity shock through the timing of selection into PMAT.

Of course, municipalities that apply to PMAT to increase fiscal capacity 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 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) 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). Rather, the timing of the first loan payment is subject to the vagaries of the loan processing schedule of the federal government, which is largely outside the control of any municipality. We thus exploit 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,

and mayor characteristics. The results (see Appendix Table A.4) 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.Substantively, we are thus interested in the post-PMAT measure of the independent variable. 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.

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. 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.

Data

To empirically evaluate the link between fiscal capacity, taxation, and electoral accountability, we assemble a panel dataset of socio-economic and political variables for Brazilian municipalities from 2000 until 2012. First, we collect data on property tax revenue raised at the municipal level made available by the Institute of Applied Economic Research (IPEA, 2016). From these data, we create our primary independent variable of interest: property tax increases during the mayor's term in office.[8]

Our main dependent variable of interest is whether incumbents are rewarded or punished for the policies they pursue as mayors. Based on electoral returns for the first round of mayoral elections in 2000, 2004, and 2008 we create two measures of electoral performance. The first is the vote share of the incumbent; the second is the margin of victory (negative or positive) for the incumbent. We code incumbency separately for the individual candidate or the political party, and show results of both. Data on election results and candidates were collected from the Superior Electoral Court (TSE do Brasil, 2016) in Brazil.

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

[8]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).

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, tax increases may not be. We therefore estimate our main models with and without a set of potentially important covariates. Based on the electoral data, we code the incumbent's education level and whether incumbents in a given election were of the same party as the state governor or the president. We also add a measure of incumbent party ideology, which is based on roll call votes and surveys of Brazilian legislators (Power and Zucco Jr., 2009, 2012; Samuels and Zucco Jr., 2014; Saiegh, 2015). Since economic development is likely to affect voters' evaluations of candidates and tax revenues, we add municipality GDP to our data. Similarly, population size is likely to be correlated with our main variables of interest. Data for both are from the Brazilian Institute of Geography and Statistics (IBGE, 2016). Next, we supplement our dataset with control variables for the rate of urbanization of a given locality (IBGE, 2016) and transfers the municipality receives from the federal and state government (IPEA, 2016). We also control for changes in total spending at the municipal level to control for any attempt by mayors to improve their electoral fortunes by increasing spending immediately before elections. 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.

Main Analyses

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 PMAT, or a positive shock to fiscal capacity. Our main dependent variable is the vote share of the incumbent party in year $t \in \{2004, 2008\}$. Because we are only interested in how tax revenue affects the fate of the incumbent, we exclude localities in which the incumbent party did not run again which accounts for 38% of the sample.

Our independent variable is the change in tax revenue over the period of the electoral mandate. 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} (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. For yearly control variables, we take the mean of the three non-electoral years of the incumbent's mandate: year t_{-3} to year t_{-1}. Economic control variables include mean Population, mean Urban Population Share, mean GDP, and mean Transfers (logged) for each locality across the years for which we employ tax revenue data.

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 this analysis, we restrict the sample to places where the incumbent party runs again. In Appendix Table A.5, we show findings are substantively unchanged when use the more restrictive sample where incumbent mayors run again. That table also includes estimates for all controls.

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 shown in the right panel, the marginal effect of tax increases among places without PMAT loans has a statistically significant negative effect on the incumbent's vote share. However, the receipt of PMAT loans during or before the incumbent mayor's mandate eliminates any negative trend; the marginal effect of tax increases among this group is not different from zero. This implies that increased fiscal capacity can remove the electoral disincentives of increasing tax revenues.

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

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

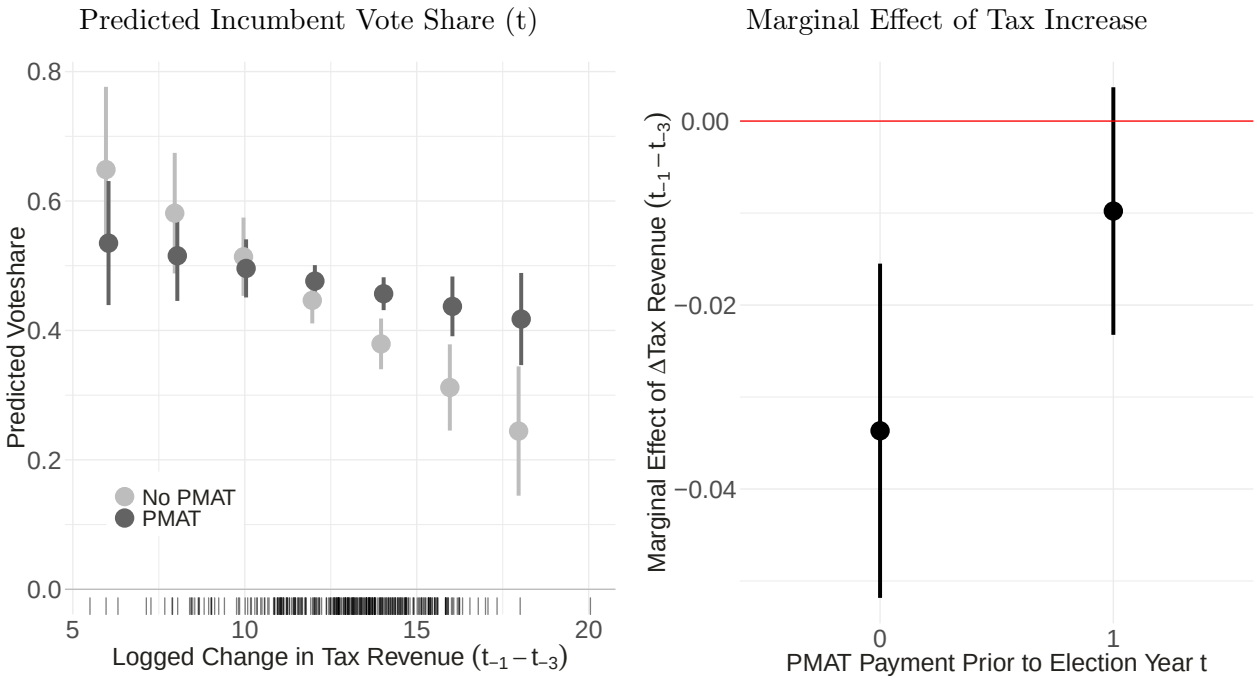
	(1)	(2)	(3)
Δ Tax Revenue	-0.019**	-0.034**	-0.038**
	(0.007)	(0.009)	(0.010)
Received PMAT loan	-0.198	-0.262*	-0.238
	(0.101)	(0.110)	(0.127)
Δ Tax Revenue $\times$ Received PMAT loan	0.018*	0.024**	0.022*
	(0.008)	(0.009)	(0.010)
Vote Share Prior Election	0.274**	0.339**	0.390**
	(0.086)	(0.106)	(0.124)
Observations	343	308	253
Controls	No	Yes	Yes & Time

OLS models with standard errors clustered by municipality. Sample restricted to municipalities where incumbent party re-ran. Models with controls include covariates 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. Model 3 includes an additional control for time since application. $^{*}p < 0.05$, $^{**}p < 0.01$.

stronger positive relationship between taxation and incumbent vote share relative to places that have only just received loans. In Appendix F, we show that the moderating effect of fiscal capacity does in fact increase in this way. We create a continuous instead of binary moderating variable that represents the number of years between the election in question and the first loan payment. The positive and significant coefficient on the interaction term indicates that places that received the PMAT loans earlier demonstrate a relatively more positive relationship between taxation and incumbent approval.

One concern with such an 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 next 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. In this reduced sample

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. The right plot shows the same result slightly differently. Here we plot 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.

(column 4 of Table A.7), the findings are actually stronger, mitigating this concern.

Additionally, in our original specification, receiving a PMAT loan could be picking up differences between early appliers and late appliers, since early appliers are more likely to be in the treated group. 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 Table 2, we thus control for the amount

of time since the application to PMAT is made and see that the key quantity of interest – the coefficient on the interaction term – is substantively unchanged. We also see that when we restrict the sample to places that applied within one to five years of the election date for this original specification, results are exactly the same (column 3 of Table A.7). Another way to address the concern that early-appliers are somehow different from late-appliers is to run a fixed-effects model in which we make within-municipality comparisons of the effect of tax increases on electoral outcomes before and after receiving PMAT. This reduces our sample considerably, but we show that results are qualitatively unchanged in Appendix Table A.8.

As a final check that our main finding is not spurious, we conduct a simple placebo test. 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. Receiving a loan in a future government mandate should not affect the way voters condition behavior in the present electoral cycle. The placebo test, depicted in Appendix Table A.9 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.

Accounting for Capacity-Induced Changes In Tax Revenues

One potential concern with the empirical analysis above is that our moderator of interest, PMAT loan payments, have 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.

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 high/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. Above, the results 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 our independent variable *caused* by the moderator.

We, therefore, 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 interaction 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 models estimated above explaining vote share outcomes. Specifically, we estimate the following two stage model:

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

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

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

The underlying idea directly follows from Wodtke, Alaca and Zhou (Forthcoming) in that 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

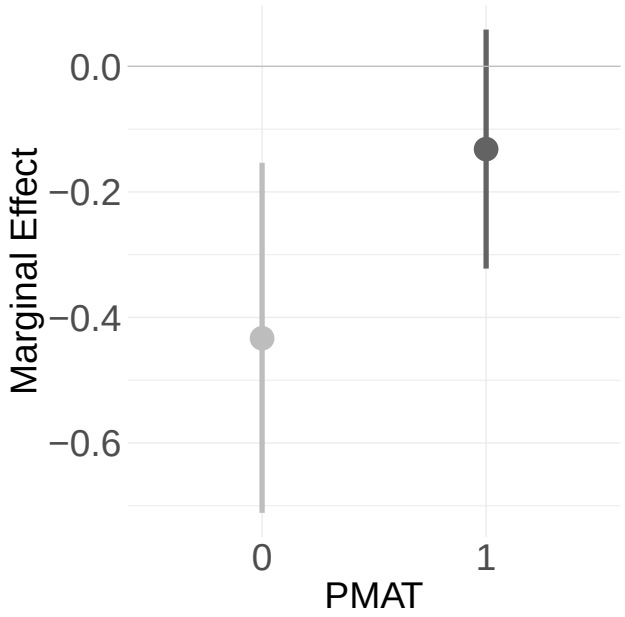
of PMAT on tax revenues. The as-if-random assignment of PMAT payment status in our sample provides additional confidence in these results.

Regular standard errors would be underestimated, given the use of the residualized independent variable. One natural approach to estimating these regression-with-residuals (RWR) models is Bayesian estimation. By estimating a Bayesian model in which both regressions are estimated sequentially at each iteration of the sampler, uncertainty from the first stage residualization automatically travels through to the second stage model. Model estimates thus reflect the full uncertainty associated with both regressions. Using this technique, we estimate our main analyses and present here the results from the model with covariates (Column 2 in Table 2). We standardize all continuous variables and models are estimated using Stan (Stan Development Team, 2017).[9]

Figure 3 shows the marginal effects and 95% uncertainty intervals of the residualized change 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 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 effect is larger than zero with probability 0.98. For the models without controls and when the time since application is included this probability is 0.96. Table A.6 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.

[9]Full details on priors, estimation, and convergence are discussed in Appendix E.

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. If we remove the change of scale (model is estimated with continuous variables standardized), the estimated coefficient on the interaction is almost the same as that in the standard regression model in Table 2.

In our view, these results lend additional credence to the conclusion drawn from our general models. 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.

Discussion

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 no more likely to introduce rate increases. But how do these three levers affect tax revenue? In Tables A.2 and A.3 in the Appendix, we follow Christensen and Garfias (2018) and estimate how each relates to subsequent changes in tax revenues in municipalities that have applied to PMAT (Table A.2) and the full sample of municipalities (Table A.3).[10] To summarize, there is strong evidence in both samples that updates to valuation maps are associated with increases in revenue from the IPTU in subsequent years, whereas there is no evidence that law changes increased revenues. Updates to the cadastre have no discernible impact on revenues in the PMAT sub-sample, but, sensitive to the specification, are estimated to increase revenue in the full sample.Substantively, updating valuations has a much larger effect on tax revenues than cadastre updates, and the effect is more durable.

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). Valuation updates on the other hand are more likely to affect and increase revenue from more valuable properties. Not

[10]Our results are slightly different from theirs, most likely due to differences in samples.

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 then affect both poorer and wealthier properties. Additionally, both policies are more likely after receipt of PMAT loans. Consistent with our argument, these efforts to raise taxes in PMAT municipalities affecting all taxpayers will induce more positive attitudes toward tax compliance and tax increases. In other words, we expect this more positive reaction compared to if we had shown that tax increases were just coming from an increase in the tax base affecting 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, it could be that higher capacity (receipt of PMAT) similarly increases their willingness to pay, as it ensures more equal treatment of all taxpayers, i.e., even the wealthy are more confident that their wealthy counterparts also pay. Further research is required to confirm these distributional consequences of tax capacity increases and how they might differentially affect the willingness to pay of rich and poor voters.

Willingness to Pay Tax and Government Performance

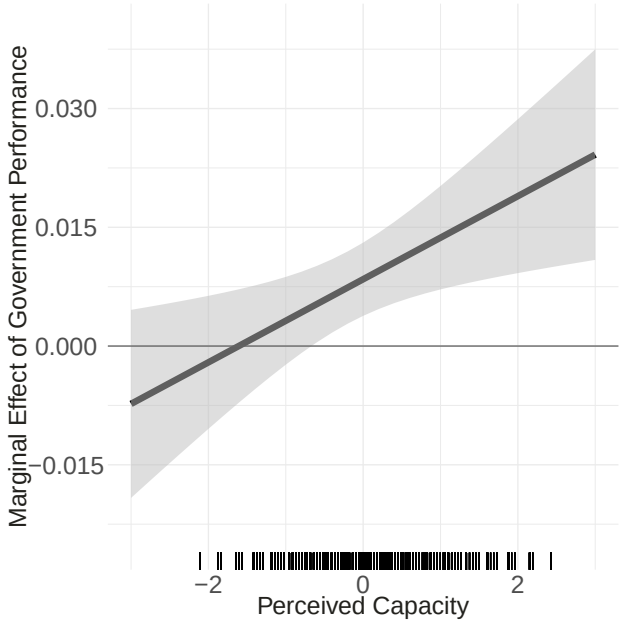
One implication of Hypothesis 1 is that in high-capacity settings politicians can use their popularity or good performance to generate support for (or lessen opposition to) tax increases from voters. In low-capacity settings, on the other hand, voters see less value in tax increases,

even when they approve of politicians. 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 can thus test the following observable implication: politician performance should influence citizen willingness to pay taxes in a high fiscal capacity setting but not a low capacity one.

Using the same individual-level survey data as above, we test whether the positive relationship between perceived government performance and willingness to pay tax posited by the existing literature is moderated by fiscal capacity. We use the *Capacity Proxy* defined in Table 1 as our 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 national government performance on willingness to pay tax under varying levels of perceived fiscal capacity (Column 2 in Table A.10). 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.

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.

Conclusion

We have argued that the capacity of states to collect taxes will moderate the relationship between voters and politicians. 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 accountability 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, 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 capacity, positive evaluations of the government have no influence on the willingness to pay tax. Our theory and evidence strongly suggests that fiscal capacity moderates the relationship between taxation and electoral accountability 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, 2018). It 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.

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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Supporting Information:
Fiscal Capacity as a Moderator of the Taxation-Accountability Hypothesis

A Testing Assumption with Actual Capacity Data

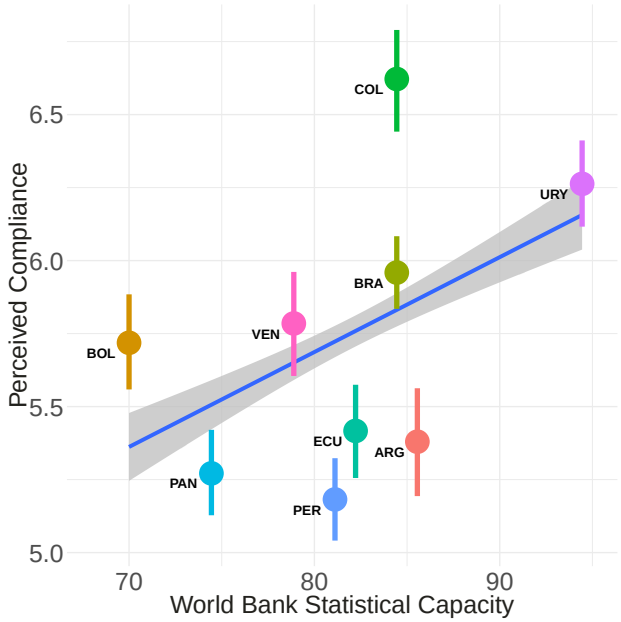
In Table 1, the *Probability of Sanction* is a respondent's belief about the likelihood of being sanctioned should he or she evade taxes. The *Impartiality of Collection* is measured using the following survey question: "On a scale of 1 to 10, where 1 is Favoritism and 10 is Impartiality, would you say 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.

As another way of validating our key assumption, we examine whether perceived tax compliance is also related to actual levels of capacity rather than perceived capacity. To do so, we merge a country-level measure of statistical capacity with the survey data from Latin America (World Bank Group, 2019). Figure A.1 plots the World Bank measure of statistical capacity on the x-axis, and the country means and 95% confidence intervals for the perceived level of compliance are plotted on the y-axis.[11] As one can see, and as further support of our assumption, statistical capacity at the country level does quite well at predicting beliefs about compliance.

[11]Since we are using a national level measure of capacity here, we subset the survey to respondents from capital cities to make the cross-national comparisons more comparable. For Brazil, the actual capital of Brasìlia was not surveyed and we use the responses from Sao Paolo and Rio de Janeiro. For Bolivia we use the respondents from La Paz.

Figure A.1: Relationship between Countries' Statistical Capacity and Perceived Compliance



Note: This figure shows the relationship between the countries' statistical capacity and respondents' perceived compliance. The points show the mean level of perceived compliance, while the error bars display 95% confidence intervals. In general, we observe a strong positive relationship between the two measures across our cases.

B Tax Policies, Tax Increases, and PMAT

In Table A.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: cadaster 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. 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. 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 A.1: Effect of PMAT on Tax Policy (less conservative coding)

	Cadaster Update	Value Update	Law Change
F.Year of 1st PMAT payment	−0.026	−0.020	0.001
	(0.023)	(0.024)	(0.018)
Year of 1st PMAT payment	0.058*	0.080*	0.028
	(0.027)	(0.036)	(0.019)
L.Year of 1st PMAT payment	0.055	−0.001	0.017
	(0.028)	(0.023)	(0.018)
L2.Year of 1st PMAT payment	0.004	−0.004	−0.005
	(0.023)	(0.024)	(0.015)
L3.Year of 1st PMAT payment	0.010	0.026	0.017
	(0.025)	(0.026)	(0.017)
Constant	0.123**	0.121**	0.074**
	(0.017)	(0.030)	(0.016)
Observations	2498	1517	2696

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

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

Cadaster Update	−0.008	−0.021	0.011
	(0.020)	(0.022)	(0.026)
L.Cadaster Update	0.023	0.009	−0.002
	(0.027)	(0.029)	(0.030)
L2.Cadaster Update		0.002	0.006
		(0.031)	(0.029)
L3.Cadaster Update			0.038
			(0.025)
Value Update	0.047	0.046	0.017
	(0.025)	(0.026)	(0.029)
L.Value Update	0.108**	0.106**	0.106**
	(0.029)	(0.029)	(0.031)
L2.Value Update		0.129**	0.111**
		(0.027)	(0.026)
L3.Value Update			0.061
			(0.032)
Law Change	0.018	0.003	−0.043
	(0.037)	(0.036)	(0.038)
L.Law Change	0.055	0.060*	0.040
	(0.030)	(0.030)	(0.032)
L2.Law Change		−0.019	−0.030
		(0.033)	(0.035)
L3.Law Change			−0.023
			(0.031)
L.Population (logged)	0.470	0.393	0.827*
	(0.376)	(0.305)	(0.386)
L.Urban Population Share	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)
L.GDP (logged)	0.067	0.087	0.137
	(0.090)	(0.087)	(0.092)
L.Transfers (logged)	−0.014	−0.082	−0.062
	(0.125)	(0.103)	(0.106)
Constant	7.844	9.902**	4.265
	(5.131)	(3.463)	(4.408)
Observations	1345	1022	816

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

Table A.3: Effect of Tax Policies on Tax Levels (Full Sample)

Cadaster Update	0.077*	0.077	0.079
	(0.036)	(0.043)	(0.061)
L.Cadaster Update	0.079*	0.072	0.110*
	(0.035)	(0.045)	(0.054)
L2.Cadaster Update		0.037	0.046
		(0.038)	(0.052)
L3.Cadaster Update			0.015
			(0.059)
Value Update	−0.038	−0.041	0.015
	(0.039)	(0.044)	(0.055)
L.Value Update	0.133**	0.132**	0.144*
	(0.038)	(0.045)	(0.056)
L2.Value Update		0.157**	0.168**
		(0.046)	(0.060)
L3.Value Update			0.119*
			(0.057)
Law Change	−0.044	−0.043	−0.018
	(0.047)	(0.051)	(0.062)
L.Law Change	0.019	0.024	−0.014
	(0.058)	(0.071)	(0.082)
L2.Law Change		0.063	0.070
		(0.063)	(0.083)
L3.Law Change			0.016
			(0.060)
L.Population (logged)	0.115	−0.133	−0.308
	(0.302)	(0.338)	(0.489)
L.Urban Population Share	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
L.GDP (logged)	0.254**	0.244**	0.272**
	(0.079)	(0.076)	(0.094)
L.Transfers (logged)	0.020	0.020	0.024
	(0.022)	(0.020)	(0.019)
Constant	6.510*	9.318**	10.892*
	(3.018)	(3.360)	(4.698)
Observations	9402	7026	5523

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

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. 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, and mayor characteristics. The results below do not provide any evidence of relationships between the included covariates and likelihood of first payment. The results are very similar if we estimate probit or Cox hazard model, for ease of interpretability we present the linear probability model results here.

Table A.4: Relationship between Covariates and First PMAT Payment

	(1)	(2)	(3)
Population (t-1) (logged)	−0.164	−0.102	−0.042
	(0.102)	(0.174)	(0.219)
Amount of Transfers (t-1) (logged)	0.035	−0.188	−0.107
	(0.295)	(0.595)	(0.662)
Spending (t-1) (looged)	0.231	0.447	0.261
	(0.220)	(0.447)	(0.594)
Property Tax Rev (t-1) (logged)	0.052	0.086	0.025
	(0.153)	(0.262)	(0.289)
Property Tax Rev (t-2) (logged)	−0.054	−0.130	−0.060
	(0.151)	(0.273)	(0.315)
GDP (t-1) (logged)	−0.076	−0.149	−0.151
	(0.135)	(0.238)	(0.257)
UrbanShare	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)
Party of the President (t-1)		−0.106	−0.075
		(0.167)	(0.199)
Party of the Govenor (t-1)		0.093	0.143
		(0.113)	(0.129)
Mayor's Education (t-1)			−0.027
			(0.043)
Leftist Mayor (t-1)			0.052
			(0.137)
Constant	−1.356	−0.403	0.778
	(1.237)	(2.226)	(3.032)
Observations	185	71	61

OLS models with year fixed effects and robust standard errors.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

D Main Analyses Tables

Table A.5 corresponds to Table 2 in the text but depicts coefficients for the control variables as well as an alternative specification where the sample is limited to races in which incumbent mayor rather than the incumbent party runs again. In columns 3 and 6 where we include the length of time since application as a control, the number of observations reduces because there are a small number of municipalities for which we do not have the application date. In the original specifications, we still use these observations by making the conservative decision to replace the application date with the date at which the PMAT contract was signed (which is available for all observations).

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

	Incumbent Runs Again			Incumbent Party Runs		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue	−0.019**	−0.034**	−0.038**	−0.012	−0.032**	−0.032**
	(0.007)	(0.009)	(0.010)	(0.008)	(0.009)	(0.009)
Received PMAT loan	−0.198	−0.262*	−0.238	−0.217	−0.206	−0.123
	(0.101)	(0.110)	(0.127)	(0.117)	(0.123)	(0.137)
Δ Tax Revenue × Received PMAT loan	0.018*	0.024**	0.022*	0.020*	0.019*	0.014
	(0.008)	(0.009)	(0.010)	(0.009)	(0.010)	(0.011)
Vote Share Prior Election	0.274**	0.339**	0.390**	0.286**	0.289*	0.363*
	(0.086)	(0.106)	(0.124)	(0.108)	(0.120)	(0.152)
Population (logged)		0.016	−0.007		0.010	−0.010
		(0.027)	(0.029)		(0.027)	(0.030)
Urban Population Share		0.000	0.001**		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		−0.046	−0.030		−0.125**	−0.135*
		(0.060)	(0.071)		(0.047)	(0.057)
Amount of Transfers (logged)		−0.003	0.036		0.017	0.052
		(0.030)	(0.031)		(0.030)	(0.033)
Party of the Governor		0.036	0.034		0.014	0.020
		(0.022)	(0.027)		(0.021)	(0.024)
Party of the President		0.054	0.073*		0.024	0.038
		(0.028)	(0.031)		(0.032)	(0.034)
Incumbent Mayor Ideology		0.012	0.026		0.014	0.021
		(0.016)	(0.018)		(0.014)	(0.016)
Incumbent Mayor Education		0.014	0.018*		0.006	0.011
		(0.007)	(0.008)		(0.008)	(0.008)
Δ Spending		−0.000	−0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Years since PMAT application			−0.009			−0.007
			(0.007)			(0.008)
Constant	0.530**	0.401	−0.059	0.492**	0.241	−0.230
	(0.107)	(0.297)	(0.324)	(0.131)	(0.335)	(0.376)
Observations	343	308	253	288	253	210

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

E 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. All evidence suggests that the chains are converged. 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 A.6: 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	-0.433	-0.491	-0.433
	[-0.481, -0.009]	[-0.711, -0.154]	[-0.802, -0.186]	[-0.704, -0.157]
Received PMAT Loan	0.197	0.227	0.206	0.233
	[-0.032, 0.423]	[-0.058, 0.509]	[-0.191, 0.593]	[-0.057, 0.518]
Residualized Δ Tax $\times$ PMAT	0.233	0.302	0.282	0.296
	[-0.028, 0.499]	[0.018, 0.584]	[-0.030, 0.598]	[0.019, 0.580]
Vote Share Prior Election	0.176	0.219	0.252	0.215
	[0.079, 0.276]	[0.103, 0.334]	[0.127, 0.380]	[0.098, 0.335]

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

F 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. Columns 3 and 4 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 A.7: Relationship between Δ Taxation ($t_{-1} - t_{-3}$) and Incumbent Vote Share (t)

	Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)
Δ Tax Revenue	-0.034^{**}	-0.026^{**}	-0.034^{**}	-0.040^{**}
	(0.009)	(0.008)	(0.011)	(0.010)
Received PMAT loan	-0.262^{*}		-0.281^{*}	
	(0.110)		(0.141)	
Δ Tax Revenue $\times$ Received PMAT loan	0.024^{**}		0.023^{*}	
	(0.009)		(0.011)	
Δ Tax Revenue $\times$ Years since PMAT loan		0.004^{*}		0.012^{**}
		(0.002)		(0.003)
Vote Share Prior Election	0.339^{**}	0.340^{**}	0.399^{*}	0.382^{*}
	(0.106)	(0.106)	(0.154)	(0.149)
Observations	308	307	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. $^{*}p < 0.05$, $^{**}p < 0.01$.

G Robustness Checks

Here, we estimate a fixed effects models on the same data. In these models we make comparisons within municipalities over time rather than across municipalities in a cross-section. In the previous analysis, we compare elections in municipalities that started receiving PMAT loans to municipalities that had applied to PMAT but had not yet begun receiving loans. In the fixed effects estimation, we tackle the problem from a different angle. We now estimate models with 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 estimating 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.

To estimate the fixed effects model we first create a panel data set based on the three elections in our data: 2000, 2004, and 2008. Given that we are only interested in observations that receive PMAT loans at some point in the period studied, we then drop all "never takers" from the data. In our view this make the common trends assumption much more defensible, because we only compare municipalities that will eventually receive PMAT. Once we subset only to municipalities that receive PMAT at some point, we are left with 320 unique municipalities and 445 observations with data on incumbent vote share, taxation, and payment status. Of those 320 municipalities, however, only 44 municipalities have a change in payment status (our treatment) and have multiple elections with the incumbent party running. While this sample is becoming very small, the more conservative test of the fixed effects model can lend additional credibility to the original findings.

Table A.8 shows the results from our preferred specification with incumbent party vote share as the dependent variable. We estimate a standard OLS model with fixed effects for municipalities and election years. Standard errors are clustered at the municipality level. Because of the inclusion of municipality and year fixed effects, these results are robust to potential time-invariant omitted variables at the municipal level or election-specific trends.

Our main interest again lies in the interaction between changes in taxation and the receipt of the first PMAT loan. Table A.8 shows the results from three models. The first column presents the result when we only include the interaction and its constituent terms (plus fixed effects) in the regression. In the second column, we include what we view as the most important set of controls, especially in light of the parallel trends assumption: logged population size, GDP growth, logged transfers, and share of the urban population. In the third column, we add additional controls for congruence of the incumbent with the president's or governor's party.

As in the original specification, the effect of changes in taxation on incumbent party vote share is moderated by fiscal capacity. The interaction effect is estimated to be positive in all three model specifications and significant at the 5% level in the first model. Once we add additional controls, the interaction is significant only at the 10% level which is unsurprising given the small number of cases.

As in the regression models above, positive changes in taxes before the election are associated with lower incumbent vote shares in low capacity settings (though statistically indistinguishable from zero). In high-capacity settings, on the other hand, the marginal effect of positive changes in taxation is slightly positive (again, however, statistically indistinguishable from zero). While both marginal effects are not significantly different from zero, the marginal effect of changes in taxation with and without PMAT is statistically different: the marginal effect in places without PMAT is significantly smaller than that of elections in which PMAT was present (with $p \leq 0.05$ for model 1 and $p \leq 0.1$ for columns 2 & 3).

Table A.8: 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)
Δ Tax Revenue (logged)	−0.005	−0.006	−0.011
	(0.024)	(0.025)	(0.025)
FirstPaymentMade	−0.449*	−0.408	−0.388
	(0.191)	(0.210)	(0.220)
Δ Tax Revenue (logged) × FirstPaymentMade	0.033*	0.030	0.029
	(0.015)	(0.016)	(0.017)
Population (logged)		0.273	0.300
		(0.835)	(0.821)
GDP Growth		−0.139	−0.188
		(0.141)	(0.149)
Amount of Transfers (logged)		−0.096	−0.122
		(0.237)	(0.247)
Urban population share (logged)		0.000	0.000
		(0.001)	(0.001)
Δ Spending			−0.000
			(0.000)
Party of the President			0.084
			(0.059)
Party of the Governor			−0.003
			(0.042)
Constant	0.485	−0.890	−0.699
	(0.313)	(8.488)	(8.149)
Observations	445	445	445
Municipal FE	Yes	Yes	Yes
Election FE	Yes	Yes	Yes

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

Standard Errors Clustered at Municipality

Next, we report results of a simple placebo test. 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. Receiving a loan in a future government mandate should not affect the way voters condition behavior in the present electoral cycle. The placebo test, depicted in below Table A.9 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 A.9: Relationship between Δ Taxation ($t_{-1} - t_{-3}$) and Incumbent Vote Share (t) Conditional on PMAT Lead

	Incumbent Runs Again			Incumbent Party Runs		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue	−0.002	0.004	0.005	0.022	0.012	0.013
	(0.015)	(0.037)	(0.037)	(0.020)	(0.031)	(0.031)
PMAT Payment Lead	−0.020	0.341	0.362	0.347	0.354	0.389
	(0.231)	(0.500)	(0.497)	(0.262)	(0.362)	(0.369)
Δ Tax Revenue × PMAT Payment Lead	−0.001	−0.029	−0.030	−0.030	−0.028	−0.029
	(0.017)	(0.040)	(0.040)	(0.022)	(0.031)	(0.032)
Vote Share Prior Election	0.227	0.140	0.146	0.247	0.273	0.303
	(0.189)	(0.241)	(0.252)	(0.169)	(0.178)	(0.179)
Observations	86	70	69	107	94	93

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. $^{*}p < 0.05$, $^{**}p < 0.01$.

H Micro-Evidence

Table A.10 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 A.11, 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.

Table A.10: Willingness to Pay

	OLS w. Weights		OLS w.o. Weights		Logit w.o. Weights	
Capacity Proxy	0.01***	0.01***	0.01***	0.01***	0.07***	0.08***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.02)	(0.02)
Gov't Performance	−0.02**	−0.02*	−0.02***	−0.02**	−0.22***	−0.22**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.08)	(0.09)
Interaction	0.01***	0.01***	0.004***	0.004***	0.04***	0.04***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.01)	(0.01)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Survey Weights	Yes	Yes	No	No	No	No
N	7,906	7,358	7,906	7,358	7,906	7,358
Adjusted R^2	0.10	0.11	0.06	0.07		
Akaike Inf. Crit.					5,901.64	5,491.15

Models estimated 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 < .01; **p < .05; *p < .1

Table A.11: Willingness to Pay by Tax Type

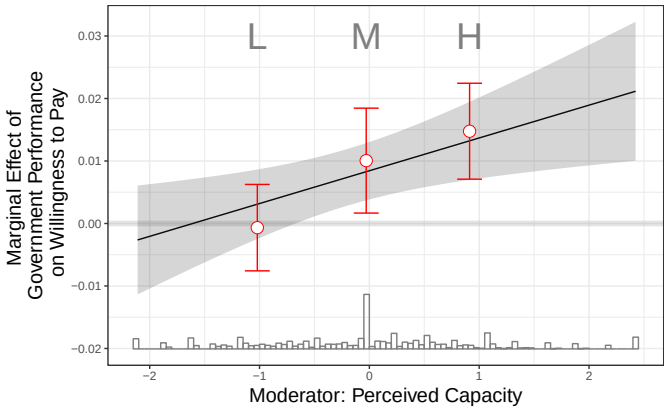
	Combined Indicator	Income Tax	VAT	Fuel Tax
Capacity Proxy	0.01***	0.01***	0.01***	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Gov't Performance	−0.02**	−0.01*	−0.01*	−0.01*
	(0.01)	(0.01)	(0.01)	(0.01)
Interaction	0.01***	0.003*	0.004**	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes
Survey Weights	Yes	Yes	Yes	Yes
N	7,906	7,747	7,824	6,085
Adjusted R^2	0.10	0.03	0.09	0.03

Models estimated with robust standard errors and country fixed effects.

***p < .01; **p < .05; *p < .1

Figure A.2 displays the interaction effect from the linear probability model in Table A.10 when binning the capacity variable in line with the recommendation by Hainmueller, Mummolo and Xu (2018).[12]

Figure A.2: Marginal Effect of Government Performance on Willingness to Pay by Perceived Capacity with Binning



[12]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