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

Word Count: 11,940

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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 revenue needs of the state and developing taxation leads citizens to demand more representative institutions (e.g., Bates and Lien, 1985; North and Weingast, 1989; Ross, 2004; Eubank, 2012; De La Cuesta et al., 2017; Weigel, 2018); and, on the flip-side, (2) whether changes in political institutions towards more democratic systems lead to higher levels of taxation (e.g., Timmons, 2010; Acemoglu et al., 2015). A majority of the literature, however,assumes that states are capable of enforcing tax policies.[1] The state is generally assumed to be both able and willing to collect taxes efficient and equally.[2] Yet, we know that in many countries around the world this is not the case. Often states are unable (or unwilling) to collect a large share of the revenue they set out to raise, and formal tax laws are undermined by inefficiencies in tax collection and tax evasion. How then 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. Briefly, 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 their own taxation will have positive returns. Further, they are more likely to

[1] In a review of the literature on democracy and taxation (Gould and Baker, 2002), for instance, capacity is never mentioned as a potential moderator of this relationship.

[2] There are of course a number of exceptions to this rule and more recently a larger set of authors has become interested in the topic of fiscal capacity development in the less developed world (e.g., see: Eubank, 2012; Kasara and Suryanarayan, 2015; De La Cuesta et al., 2017; Weigel, 2018).

oppose higher taxes as any additional increase in taxation will only exacerbate unfairness inherent in differential enforcement under low capacity.[3] In high capacity settings, on the other hand, we expect the traditional theoretical argument to hold, such that the majority of voters (who are generally below the median income) prefer higher taxation if they benefit from public goods provision or redistribution. We therefore expect that politicians are more likely to be punished for tax increases in low capacity settings, whereas in high capacity settings, electoral rewards (or at least weaker sanctions) are more likely. In sum, we argue that fiscal capacity works as a moderator in the link between tax policy and electoral politics.

One of the likely reasons there is scant evidence on this question is that the direct or indirect effects of fiscal capacity are hard to identify. Fiscal capacity generally develops slowly, is highly correlated with improvements in other aspects of state capacity and political development, and improvements are endogenous to institutions and political processes. It is thus difficult to identify whether differential effects of taxation on electoral outcomes in high and low fiscal capacity contexts are the result of the theoretical mechanisms we propose above or other factors plausibly correlated with levels of fiscal capacity such as age of democracy or clientelistic politics. Additionally, fiscal capacity is hard to quantify and measure.

We take advantage of a rare "shock" to fiscal capacity that occurred in some Brazilian municipalities at different times over a discrete period. 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

[3] In one low capacity setting, Ecuador, Carrillo, Pomeranz and Singhal (2017) exploit a policy intervention to illustrate that even notices sent by the Ecuadorian tax authority to non-compliant tax payers were not heeded, suggesting that enforcement credibility in such settings is also low.

across-municipality variation in tax collection and subsequent voting behavior in Brazil. We make use of a federal loans program to increase municipal fiscal capacity, 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.By doing so, we avoid a particular threat to inference, in which municipalities that apply to PMAT are fundamentally different from those that do not. Using the timing of receipt of the first PMAT payment – which is out of the municipalities' control – as our treatment (similar to Gadenne (2017)), we can consider the shock to fiscal capacity as-if random.

In line with our theoretical argument, we find consistent evidence across many empirical specifications that among places with PMAT, higher taxation yields electoral advantages for the incumbent (or at least less punishment). Among similar places without PMAT, higher taxation is associated with much worse electoral fortunes for the incumbent party. These results are robust to a number of alternative specifications, including a fixed effects model in which we compare elections in the same municipality before and after the PMAT shock.

In addition to the empirical analysis of Brazilian municipalities, we provide more micro-level evidence in line with some of our theoretical expectations using survey data from 17 cities in seven countries in Latin America. First, we show evidence for one of our underlying assumptions: individuals have a higher expectation of tax compliance by others when the (perceived) level of fiscal capacity is higher. Second, we show direct evidence 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. Both of

these findings are in line with the micro-logic of our theoretical argument detailed below.

This study contributes to the theoretical and empirical literature on taxation in democracies by proposing a novel moderator of a widely examined hypothesis: that taxation affects political accountability. Because much of the existing literature is motivated by theories that assume sufficient fiscal capacity, our insights are particularly relevant for developing countries and young 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.[4] This innovation has implications for potential interventions to increase tax compliance. While we mainly focus on voter preferences and behavior, our findings also have implications for how politicians may adjust their behavior in reaction to the voters' calculus. As we discuss further in the conclusion, our argument and findings suggest that politicians in low capacity places have strategic disincentives to invest in fiscal capacity. This could partly explain the stickiness of the high and low fiscal capacity equilibira we observe across countries with varying levels of state strength. 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.

Theory

Models of electoral accountability generally assume that voters reward or punish politicians for their performance in office. Whether raising taxes sends a good or bad signal to voters about incumbent performance, however, is ambiguous. It may depend on what the govern-

[4]Del Carpio (2013) explicitly tests a similar insight when she shows that citizens are more likely to pay taxes when they believe their neighbors are paying as well.

ment does with those taxes. Bates and Lien (1985), for instance, argue that citizens hold preferences not only about tax levels but also about government policies, which implies that knowledge about tax levels is insufficient to predict subsequent electoral behavior. Electoral accountability models thus describe how voters evaluate politicians on the basis of their performance with no direct implication for taxation. The taxation-accountability hypothesis, on the other hand, suggests that tax paying voters are more likely to hold politicians accountable for performance than citizens who do not pay taxes. Paying taxes increases incentives for voters to monitor politician performance, as spending is more closely linked to their own pocketbook (Paler, 2013; Martin, 2014).[5]

We argue that how voters evaluate tax policy by incumbents is dependent upon another parameter: fiscal capacity. This ability of the state to collect taxes changes the expectations of voters about returns to government performance and how they evaluate incumbents' policies. When public goods are financed through collective taxation, the effectiveness of public spending will depend on the number of people that taxes are collected from and how efficiently the government collects these taxes. Greater fiscal capacity – or the ability to extract taxes from citizens – thus increases 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, e.g., whether the politician is a good or bad performer. At any level of politician performance, fiscal capacity changes the ability of that politician to raise revenue and thus to provide public goods.

At the individual level, there are two parts to our theoretical argument. First, fiscal

[5]However, De La Cuesta et al. (2017) find that, at least in Uganda and Ghana, this is not necessarily the case.

capacity influences voter beliefs about how many others are also paying taxes. Second, higher perceptions of tax compliance increase an individual's own willingness to pay tax. We then use these micro-foundations to derive implications for electoral behavior.

Voter Beliefs

In this section, we advance the underlying individual level logic from which we build our theoretical argument about electoral behavior. First, we establish the intuition for why fiscal capacity influences voters' perceived tax compliance. To an individual citizen, much about a state's actual fiscal capacity is unobserved, e.g., the number of bureaucrats hired to collect taxes or the sophistication of the technology used to process receipts. 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 general 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 will infer that other potential tax payers are also engaging with the state in this way – increasing their perception of total rates of tax compliance.

Next, we contend that greater (perceptions of) fiscal capacity increases a citizen's willingness to pay tax through this mechanism of perceived tax compliance. We assume that tax payers derive utility from both, material welfare but also perceptions of fairness, i.e., there is an intrinsic value to thinking that others are paying as well.[6] These strategic com-

[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 (Del Carpio, 2013). Yesegat and Fjeldstad (2016) similarly show that, in Ethiopia, a higher perceived tax compliance rate is positively correlated with one's own tax compliance – and one of the most important predictors of tax compliance. These studies do not, however, distinguish whether this relationship is driven by increased perceptions of material benefits or fairness.

plementarities in which tax payer i receives greater utility from paying taxes when more tax payers $-i$ also pay taxes, therefore, arise for two reasons.[7]

First, when more tax payers are complying and paying taxes, collected revenue will be higher all else equal. Higher government revenue, all else equal, should lead to more or better public goods provision or redistribution. The material benefits derived from government utilizing the collected revenue, therefore, will be larger for tax payer i when more tax payers $-i$ are also paying, i.e., total collected revenue is larger. Given that higher fiscal capacity leads to better tax collection and enforcement, more tax payers are complying. Thus, the same amount of taxes paid by person i will yield greater material benefits to person i in a high fiscal capacity setting relative to a low fiscal capacity setting with fewer tax payers.

In their model of tax evasion, Cowell and Gordon (1988) rationalize these beliefs by showing what happens to a standard model of free-riding when interdependence between citizens is taken into account. Whereas prior models made the assumption that individual i believed that if she paid taxes that all individuals $-i$ would not pay (Cornes and Sandler, 1985), these authors allow for some conformity of behavior across citizens, e.g. an individual takes into account the potentially positive effect of their decision to pay on other tax payers' decisions. Consistent with our argument that benefits to tax payment across individuals are complementary, their model recognizes that a higher degree of positive conformity in tax payment will effectively induce 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.

[7]In his recent review of the literature on tax compliance, Alm (2019) describes this particular type of behavioral interdependence within a class of theories about how social interactions condition tax payment. While most of these studies focus on the role of social norms in disciplining taxpaying behavior, there are a minority of studies, which we discuss below, that focus on the strategic complementarities of tax payment.

Second, we contend that tax payers derive higher utility from paying taxes in a high capacity setting because they perceive taxation 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. Tax payer i in a high fiscal capacity setting will perceive tax payments as relatively more fair than in a low fiscal capacity setting, all else equal, because tax enforcement is equally likely to capture tax payers $-i$, who therefore will also be paying taxes. This inclusion of fairness into the tax payer's utility function is consistent with Bordignon (1993) who rationalizes these beliefs in a formal model of tax evasion. Including fairness considerations in the model 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.

Thus, in high capacity settings, where individuals perceive higher rates of tax compliance, they will both expect greater benefits from taxation and believe fiscal extraction is more impartial – making them more willing to pay. We therefore expect that citizens will react differently to a government's change in fiscal extraction in high versus low capacity states. While governments can choose to raise revenue by increasing tax rates or by increasing the number of tax payers , i.e., the tax base, we argue that irrespective of which mechanism chosen, the differential response by tax payers in high and low capacity settings should accord with the pattern described above.

First, if a tax payer observes an increase in tax rates in a low (high) capacity setting, paying more individually will translate into relatively smaller (larger) collective gains. Similarly, whatever the new tax rates, in high capacity settings voters are more likely to believe that the new rates apply equally to all tax payers. Second, marginal increases in enforcement,

e.g., the state attempting to register more citizens as tax payers, are likely to be received by voters as further improvements in the fairness and efficiency of the current tax system in a high capacity setting. Whereas in a state with low initial capacity, such increases in enforcement are likely to give tax payers the impression of unsystematic tax extraction accompanied by a weak increase in public revenues, and thus appear less fair and even an arbitrary attack on some.Both going from being a non-tax payer to a tax payer and going from paying lower to higher taxes are, therefore, more likely to be tolerated in a high fiscal capacity setting than a low capacity setting.

We expect that distributional assumptions of standard theories of taxation may also differ in low capacity settings. While the marginal benefit of an increase in taxation and ergo higher redistribution may be positive for the poor under these models, there may be reasons for the poor to oppose tax increases in low capacity settings. First, richer or wealthier tax payers may be better able to evade taxes, i.e., take advantage of low capacity. In fact, the economic elite may have an interest in low capacity for this reason (Hollenbach and Silva, Forthcoming). Thus, under low capacity any tax increase may disproportionately affect the poor and deliver little benefit if the likelihood of non-payment increases in income. The negative impact of low capacity might de facto be worse on the poor than the rich. Second, 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. With these expectations in mind, the poor in particular see less value in higher taxes under low vs. high capacity.

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) an expectation of lesser (greater) returns to paying taxes in terms of public goods provision; 2) lesser (greater) perceived fairness of the tax system. A retrospective evaluation of tax increases (Ferejohn, 1986; Fearon, 1999; Besley, 2006), should therefore lead to differential assessment of politicians under low and high levels of capacity. Introducing the moderator of fiscal capacity into the tax payer'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. 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 the general policies or politicians.

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 with survey data below.

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

Fiscal Capacity Shocks in Brazilian Municipalities

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 identify whether differential effects of taxation on electoral outcomes in high and low fiscal capacity contexts are the result of the theoretical mechanisms we propose above, or other differences plausibly correlated with levels of fiscal capacity, e.g., age of democracy or clientelistic politics. 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 but are sufficiently similar to those that did to mitigate concerns of omitted variable bias.

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 the authority to raise tax revenue using multiple tax instruments, such as sales and property taxes (Nickson, 1995; Rodríguez and Velásquez, 1995). Whereas most other political responsibilities still lie with the federal and state governments, municipalities received these substantial powers in the 1988 constitution (Andrade, 2007; Baiocchi, 2006; Samuels, 2004). Moreover, federal and state transfers have been declining, thus increasing pressure on local mayors to raise revenue. 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. In this paper, we use variation in fiscal capacity across municipalities to understand whether it moderates the electoral response to changes in tax policy.

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 so-called *Programa de Modernização da Administra ção Tributarìa* program (PMAT). PMAT was created to raise the capacity of municipalities to engage in tax collection. Credit lines subsidized by the federal government would allow the municipalities to use these funds to update tax payer rolls, educate bureaucrats, and improve bureaucratic infrastructure Gadenne (2017). As Gadenne (2017) documents, all municipalities that apply to the program

eventually are approved to receive loans. Moreover, funds are restricted to investments in local capacity and municipalities that did receive PMAT saw a significant positive effect on tax collection.

We use the municipalities' applications to PMAT and the year in which they received their first payment as a positive shock to fiscal capacity. While not exogenous, we argue that PMAT is a unique opportunity to study this question as it is one of the few cases where the tax capacity of administrative units changes significantly over a short period – and where we can take advantage of differential timing of the program to restrict comparisons to places that all eventually apply for and receive PMAT. It, therefore, allows us to study how voters evaluate changes in tax policy in localities with differential levels of fiscal capacity.

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 of the median municipality for PMAT recipients is about six times larger, while property tax revenues are almost 100 times larger.

We, therefore, cannot simply compare differences in the relationship between tax policy 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 provide evidence that, conditional on having applied to PMAT, the timing of the first payment (i.e., our treatment) is as-if-random. We estimate a number of regressions modeling the onset of PMAT payments. We subset our data to only those municipalities that eventually receive PMAT and have applied to the program. We also drop all observations after the first payment was received. We then regress an indicator for the first payment year on a number of covariates included in our models, e.g., property tax revenue, GDP, transfers, and mayor characteristics. All independent variables are lagged by one year and models include year fixed effects. The results (see Appendix Table A.1) show that none of the estimates are significantly different from zero at the 5% level.[8] While this does not mean that the coefficients are equal to zero, the vast majority are estimated to be very small and close to zero. This is especially true for those variables

[8]The coefficient on urban share of the population is significant at the 10% level in one of the three models, but very small.

we might be most concerned about: property tax revenue (at $t-1$ and $t-2$) and mayor education. None of these variables seem to have a strong relationship with the timing of the fist payment, which lends some support to our as-if-random assumption.[9]

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 total tax revenue and 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.[10]

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. We additionally code whether incumbents in a given election were of the same party as the state governor and the president. Data on election results and candidates were collected from the Superior Electoral Court (TSE do Brasil, 2016) in Brazil. We also add a measure of incumbent party ideology, which is

[9]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.

[10]One benefit to using property tax is that it is a direct tax immediately felt by tax payers 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 tax payers will fail to condition voter behavior (Kao, Lu and Queralt, 2019).

based on roll call votes and surveys of Brazilian legislators (Power and Zucco Jr., 2009, 2012; Samuels and Zucco Jr., 2014; Saiegh, 2015).

We expect fiscal capacity to moderate the relationship between taxation and electoral performance. In this empirical analysis, 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.

Given the non-random way in which municipalities raise taxes and apply to PMAT, we add several control variables to our data set. 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. The data for GDP and Population 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 total spending at the municipal level to control for any attempt by mayors to improve their electoral fortunes by increasing spending immediately before elections.

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 mayor (if he or she ran again) or 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 or incumbent party did not run again which accounts for 48% and 38% of the sample, respectively.

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.

Because tax increases and electoral behavior may be correlated with other economic or political variables such as the level of development or ideological leaning of the locality, we include a range of control variables in some of our model specifications to mitigate potential omitted variable bias. 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 the mean Population, mean Urban Population Share, mean GDP, and mean Amount of Transfers (logged) from the government of the locality across the years for which we employ tax revenue data. Political control variables include congruence of the incumbent

party with the party of the President or Governor, the mayor's ideology, and the mayor's level of education. We always control for the lagged dependent variable as a way 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. Finally, as a test of an alternative mechanism – that voters are rewarding incumbents for increased spending on public goods – we add a control for the change in public spending over the three-year period for which we observe changes in taxation.

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 *Treated* 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 treated.

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

We observe a statistically significant difference in the relationship between increases in tax revenue and incumbent vote share conditional on being treated, indicated by the positive coefficient on the interaction term in Table 1. Columns 1 to 3 restrict the sample to cases in which the incumbent mayor ran again while columns 4 to 6 include the slightly larger number of places in which the incumbent party ran again. As illustrated in the left panel of Figure 1, 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.

Table 1: 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	-0.013	-0.032^{**}	-0.031^{**}	-0.021^{**}	-0.035^{***}	-0.039^{***}
	(0.008)	(0.010)	(0.010)	(0.007)	(0.009)	(0.009)
Received PMAT loan	-0.213^{+}	-0.232^{+}	-0.159	-0.214^{*}	-0.288^{**}	-0.278^{*}
	(0.122)	(0.123)	(0.132)	(0.103)	(0.110)	(0.125)
Δ Tax Revenue $\times$ Received PMAT loan	0.020^{*}	0.022^{*}	0.017^{+}	0.020^{*}	0.026^{**}	0.026^{**}
	(0.009)	(0.010)	(0.010)	(0.008)	(0.008)	(0.010)
Vote Share Prior Election	0.312^{**}	0.321^{**}	0.406^{**}	0.276^{**}	0.341^{**}	0.391^{**}
	(0.107)	(0.119)	(0.153)	(0.093)	(0.110)	(0.130)
Population (logged)		-0.003	-0.025		0.005	-0.019
		(0.026)	(0.029)		(0.029)	(0.031)
Urban Population Share		0.000	0.000		0.000	0.001^{**}
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		-0.119^{*}	-0.130^{*}		-0.045	-0.026
		(0.047)	(0.059)		(0.060)	(0.071)
Amount of Transfers (logged)		0.027	0.063^{+}		0.008	0.046
		(0.030)	(0.033)		(0.033)	(0.036)
Party of the Governor		0.015	0.021		0.037	0.035
		(0.021)	(0.024)		(0.023)	(0.027)
Party of the President		0.023	0.040		0.049	0.068^{+}
		(0.034)	(0.038)		(0.031)	(0.035)
Incumbent Mayor Ideology		0.020	0.027		0.014	0.029
		(0.015)	(0.017)		(0.017)	(0.020)
Incumbent Mayor Education		0.001	0.005		0.010	0.014^{+}
		(0.007)	(0.007)		(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			-0.007			-0.008
			(0.007)			(0.008)
Constant	0.471^{***}	0.188	-0.283	0.545^{***}	0.353	-0.086
	(0.131)	(0.332)	(0.371)	(0.110)	(0.303)	(0.348)
Observations	270	239	196	328	295	240

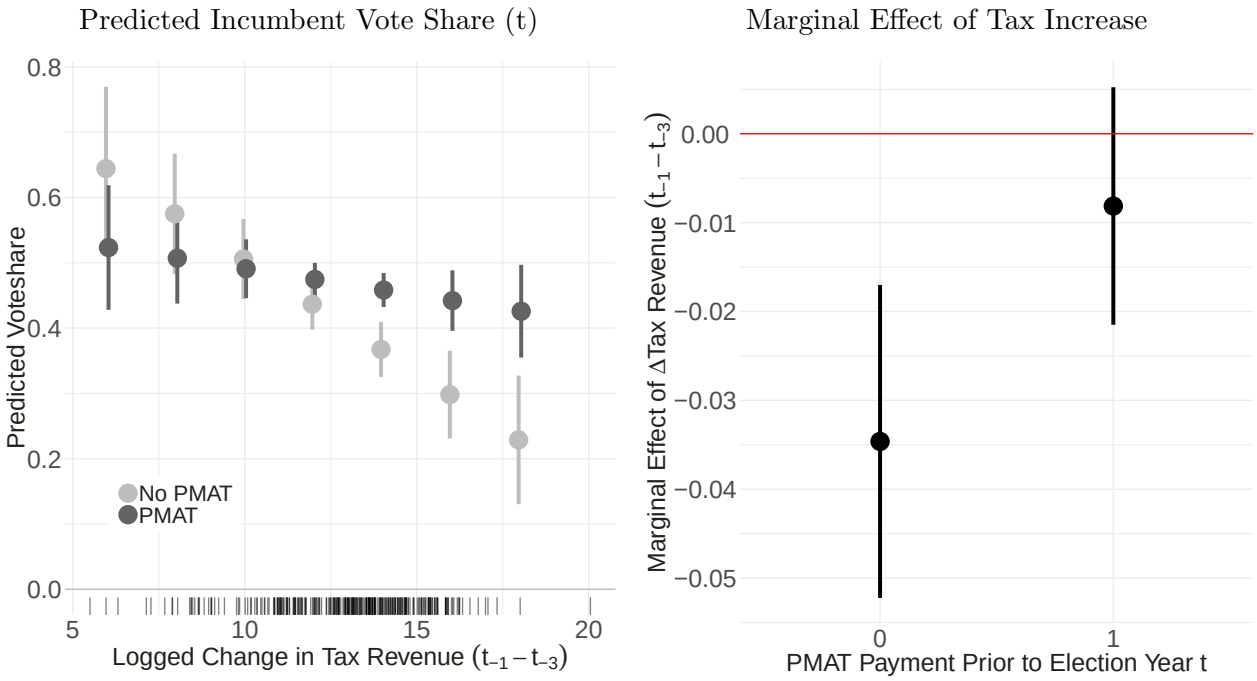
OLS models with robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

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 B, we show that the moderating effect of fiscal capacity does 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. This is because the maximum amount of time between application and first payment is six years, while the modal time lag is one to two years. To ensure better comparability, we next limit the sample of municipalities to places that applied within one to five years of the election date since there are no places that applied six or more years prior that have still not received a loan. In this reduced sample (column 4 of Table A.3), the findings are actually stronger, mitigating this concern.

This discussion also raises the concern that, in our original specification, participation in treatment (or getting a PMAT loan) could instead 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 two sets of mayors or municipalities that are similar in 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.

Figure 1: 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. The plot displays the predicted values under two scenarios: municipalities that received PMAT payments prior to the election year (dark gray) and municipalities that have not yet received PMAT (light gray). The observed relationship is strong evidence for our theoretical argument. 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 municipalities that have not yet received PMAT on the other hand (light gray), 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 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. The effect is slightly negative but very close to and not significantly different from zero for municipalities under PMAT, but we observe a strong negative effect for those municipalities that have not yet received PMAT. Despite the overlapping confidence intervals, the two marginal effects are significantly different from each other.

While all the 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 columns 3 and 6 of Table 1, 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.[11] We also see that when

[11]The number of observations reduces for these specifications 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

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.3).

Robustness Checks

In this section we check the robustness of our findings to different measurement strategies and different estimation strategies. First, we show the results hold up when we use alternate measures of electoral competition than incumbent vote share. Second, we use a fixed-effects model to try to address the endogeneity concern presented by the possibility that our original specification does not fully mitigate bias induced by selection into treatment – that early-appliers are somehow different from late-appliers and more likely to show up in the treated group. Third, we raise and attempt to address another inferential issue: that our moderator has an independent effect on the main variable of interest. This poses a challenge for the interpretation of causal effects, which we attempt to decompose in an additional analysis.

First, we investigate whether other measures of political competition similarly have a conditional relationship with fiscal capacity and taxation. In particular, we examine the incumbent's margin of victory (positive or negative) and the Herfindahl index of vote shares of the parties competing in the election in year $t \in \{2004, 2008\}$. In Table A.4, we show that, indeed, there is a statistically significant and positive coefficient on the interaction term on both measures of electoral concentration. This implies that increasing taxes in lower fiscal capacity places makes future elections more competitive for the incumbent. We additionally investigate robustness to using an alternative independent variable – Total Tax Revenue rather than revenue coming only from property taxes. Appendix Table A.5 demonstrates

PMAT contract was signed (which is available for all observations).

that our findings are indeed robust to this alternative specification.

Second, 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 Table A.2 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.

Next, we estimate several fixed effects models on the same data sets. 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 2 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 2 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.

Table 2: 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) $\times$ 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

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Standard Errors Clustered at Municipality

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).

In the Appendix, we present results for our preferred specification (column 2 in Table 2 above) with a different set of dependent variables. We estimate the same models but now use incumbent individual vote share, margin of victory, and the Herfindahl index of election results as the dependent variable (Table A.6 in the Appendix). The results are quite similar, the interaction between changes in tax revenue and receiving the first PMAT payment is positive in all three specifications and significant at conventional levels ($p \leq 0.05$) with margin of victory and the Herfindahl index as the dependent variable. When we use the individual incumbent vote share as the dependent variable, we are left with only 24 observations that change payment status, the high uncertainty about these results is therefore not surprising. Across all models the coefficient estimate on the interaction is in the expected direction.

Finally, in Appendix F, we attempt to mitigate an unexplored issue for making causal inference: the fact that our moderator (PMAT) may directly increase our main explanatory variable (Δ *Tax*). In other words, PMAT can have both a direct and indirect effect on the outcome of interest – the vote share of the incumbent. We are less interested in the direct effect – how PMAT directly increases tax revenues and thereby affects voting behavior, and

more interested in the indirect or moderated effect – how PMAT changes voter expectations such that a similar increase in taxes in PMAT and non-PMAT places leads to differential electoral outcomes. In an effort to make claims about this indirect effect, we attempt to isolate the two different effects of PMAT by empirically decomposing them. Following Wodtke and Almirall (2017), we use a structural nested means model to first regress our independent variable of interest Δ *Tax* on PMAT.

By predicting changes in tax revenue with PMAT, we can then isolate the part of tax revenue changes that is due to PMAT's introduction. Next, we use the residuals from the first stage (or $\Delta\ Tax_{policy}$), i.e. the increase in tax revenue that is not caused by PMAT, as the independent variable in our main model. We find that the results are quite similar to those presented above, the interaction is in the expected direction and 95% uncertainty intervals do not cover zero. We present the full results and explain the analysis in detail in section F in the Appendix.

Alternative Explanations

We attempted to rule out a key concern that changes in electoral behavior are being driven by changes in public spending rather than perceived or actual changes in fiscal capacity by including changes in spending as a control in our specification. Here, we discuss and refute two additional alternative explanations: 1) that increased mobilization rather than changed voter preferences, and 2) that political business cycles are driving the outcomes.

Several studies suggest that being taxed at a higher rate can mobilize voters to turn out. For instance, Weigel (2018) finds that increased tax collection is associated with more political participation of citizens. Kasara and Suryanarayan (2015) additionally find that

the turnout behavior is sensitive to fiscal capacity – while the rich vote more than the poor where concerns about redistribution are high (in high fiscal capacity settings), this is not true in places with low fiscal capacity. To rule out this potential explanation, we re-run Equation 1 with turnout as the dependent variable. We find no effect of fiscal capacity or of tax increases on local election turnout.

Another possible mechanism driving a relationship between taxation and electoral outcomes is political business cycles where incumbents purposefully manipulate tax collection just before an election. Cheibub (1998), for instance, finds that tax revenues go down in election years compared to non-election years. And just focusing on Africa where we would expect fiscal capacity to be generally low, Block (2002) finds evidence of the same trend. However, greater fiscal deficits in election years are driven by increased expenditures rather than decreased tax revenue. We test for the existence of political business cycles in our data and find no evidence either in the full data, or differentially across places with and without PMAT, that tax revenues are decreasing in the run-up to elections.

Testing Mechanisms

The data from Brazil allow us to assess the relationship between fiscal revenue and voter behavior across arguably equivalent localities with different levels of fiscal capacity. One weakness of that analysis is the lack of micro-level evidence of the mechanisms that are driving the differential relationships in PMAT and non-PMAT localities. Our theory implies that greater tax aversion in general is what leads voters to sanction the incumbent after tax increases in low capacity places. Only where capacity is high can politicians satisfy voters who might prefer more government spending, or hope to trade off good performance for

increased willingness to pay tax as proposed in Hypothesis 2.

Individual-level survey data on willingness to pay tax from the Brazilian, and neighboring, empirical contexts allows us to generate evidence in favor of this mechanism. These data do not allow us to identify the moderating effect of fiscal capacity as cleanly as in the case of PMAT in Brazil. Nevertheless, they allow us to generate correlational evidence that even at the individual level, citizens' willingness to pay is moderated by the state's fiscal capacity or the citizen's perception of said capacity.

In 2011, the Andean Development Corporation conducted a survey in 17 cities across seven Latin American countries, including Brazil. In each cit, 600 heads of household, or an adult between 25 and 65, were interviewed. While similar surveys are run annually, this particular survey included a long battery about taxation. The resulting data permit us to examine some of the individual-level assumptions and arguments in our proposed theory.

Before testing the main micro-level mechanism posited in Hypothesis 2, we provide evidence in support of one of our key assumptions: that fiscal capacity is positively correlated with perceptions of tax compliance. In Table 3, we show evidence of a strong positive relationship between several measures of perceived fiscal capacity and perceived rates of tax compliance. *Perceived Compliance*, our 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).

We measure perceived capacity, the independent variable of interest in several ways. In Columns 1 and 4, the *Probability of Sanction* is a respondent's belief about the likelihood of being sanctioned should he or she evade taxes. In Columns 2 and 5, 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.[12] Using the combined Capacity Proxy increases the explained variation in the outcome variable, Perceived Compliance, compared to either component variable on its own. Going forward, we thus use the combined measure as our proxy for perceived capacity.

Table 3: 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

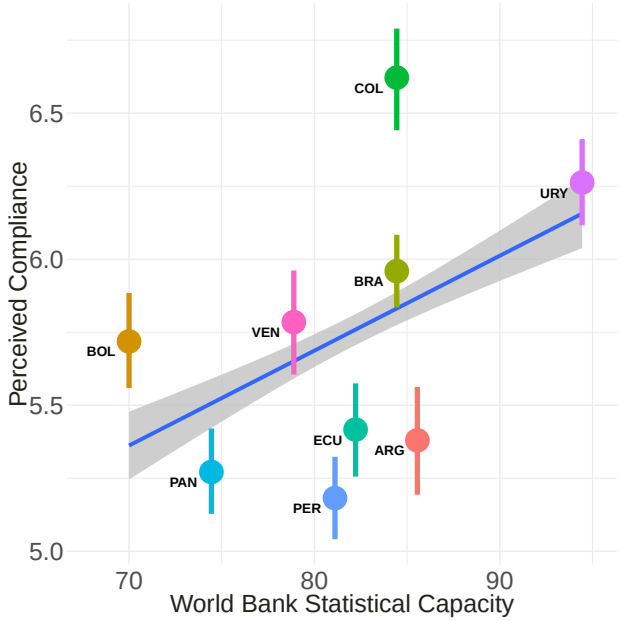
Linear models (OLS) with robust standard errors and country fixed effects.
***p < .01; **p < .05; *p < .1

As another way of validating our key assumption, we examine whether the perceived tax compliance is also related to actual levels of capacity in addition to perceived capacity. To do so, we merge a country level measure of statistical capacity with the survey data (World Bank Group, 2019). While not a direct measure of fiscal capacity, it should be strongly related to cross-national variation in levels of fiscal capacity. Figure 2 plots the World Bank

[12]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.

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.[13] As one can see, and further in line with our assumption, statistical capacity at the country level does quite well in predicting beliefs about compliance.

Figure 2: 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.

Next, 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 above discussed capacity proxy from the survey data as our moderating variable. The independent variable, *Perceived Government Performance*, is measured using

[13]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.2 in the Appendix shows the same plot for survey respondents in all cities in the sample.

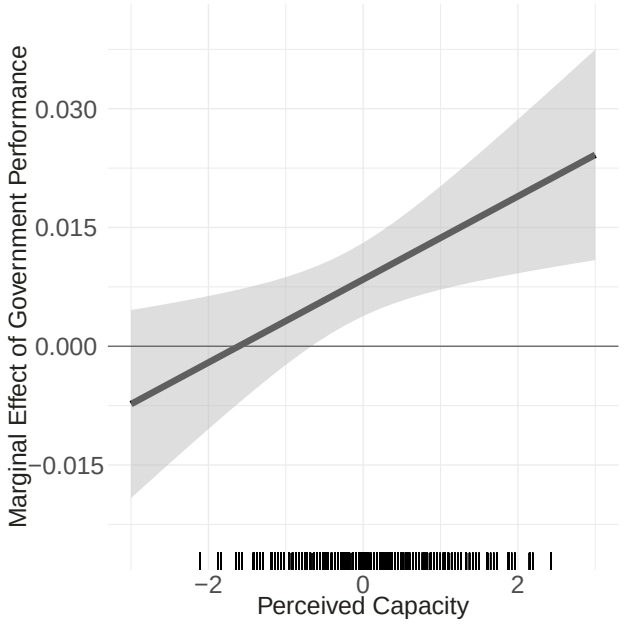
the following survey question: "Do you agree with the following sentence: the resources received by the National Government are used to improve welfare through better public services?" As the dependent variable, *Willingness to Pay Tax*, we create an indicator for whether the respondent would be willing to pay higher taxes on income, value added (VAT) or fuel. We estimate linear probability models with and without survey weights, as well as logit models. All presented results include robust standard errors. 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. Table A.9 presents the results from all six models for the three variables of interest.[14]

To better illustrate the results, Figure 3 shows the marginal effect of national government performance on willingness to pay tax under varying levels of perceived fiscal capacity. The results are based on the interaction of fiscal capacity and national government performance in the linear probability model with survey weights and control variables (column 2 in Table A.9). 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.[15]

[14]As we show in Table A.10 in the Appendix, 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.

[15]Figure A.3 displays the interaction effect from the linear probability model when binning the capacity variable in line with the recommendation by Hainmueller, Mummolo and Xu (2018).

Figure 3: 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

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, voters place relatively lower value on tax increases as they are less likely to yield sufficient revenue to finance public goods. Second, the unevenness and unreliability that often goes along with low capacity leads to higher levels of perceived unfairness. In these ways, fiscal capacity moderates the relationship between taxation and accountability.

We first show this to be true in Brazilian municipalities, where voters react more negatively (positively) to tax increases in low (high) capacity settings. We use the federal PMAT program as a quasi-random shock to fiscal capacity. As we discuss above, municipalities that have applied for and received PMAT are significantly different from those municipalities that have not applied for PMAT. One of the advantages of our research design is that we only compare places that will eventually receive loans to increase capacity. And crucially, we take advantage of the fact that, while municipalities can choose when to apply to the PMAT program, they cannot choose when they receive their first loan payment. We thus mitigate 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.

Additionally, we present survey evidence from Latin American cities in line with the micro-logic of our theoretical argument. First, under higher (perceived) fiscal capacity, vot-

ers have higher perceptions of tax compliance. Second, voters' willingness to pay taxes is only increased by positive government performance when perceived fiscal capacity is sufficiently high. Under low levels of capacity, in line with our argument, 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), as in a difference-in-differences design. By design, all the empirical models testing Hypothesis 1 in this paper only use data on municipalities that eventually receive PMAT loans. Our estimate of both the moderated effect, changes in tax revenue, and all other covariates should, therefore, be interpreted similarly to a treatment effect on the treated. It is, therefore, 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 raise property taxes 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. 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 when fiscal capacity is high. 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). Our study suggests that this effect may be restricted to places with higher fiscal capacity. 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 As-if-Randomness of Payment

Table A.1: 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.
$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A.2: 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	−0.003	0.003	0.005	0.027	0.020	0.022
	(0.014)	(0.045)	(0.045)	(0.020)	(0.032)	(0.032)
PMAT Payment Lead	−0.028	0.332	0.357	0.401	0.444	0.504
	(0.227)	(0.596)	(0.592)	(0.254)	(0.373)	(0.377)
Δ Tax Revenue × PMAT Payment Lead	−0.001	−0.029	−0.029	−0.035	−0.037	−0.038
	(0.017)	(0.048)	(0.048)	(0.022)	(0.033)	(0.033)
Vote Share Prior Election	0.231	0.141	0.147	0.254	0.284	0.322^{+}
	(0.192)	(0.255)	(0.263)	(0.170)	(0.184)	(0.185)
Population (logged)		0.069	0.070		0.054	0.062
		(0.083)	(0.085)		(0.068)	(0.072)
Urban Population Share		0.001	0.001		0.001	0.001
		(0.001)	(0.001)		(0.001)	(0.001)
GDP Growth		−0.171*	−0.175*		−0.125	−0.142
		(0.067)	(0.070)		(0.114)	(0.122)
Amount of Transfers (logged)		−0.060	−0.057		−0.077	−0.080
		(0.106)	(0.107)		(0.080)	(0.082)
Party of the Governor		−0.009	−0.010		0.053	0.058
		(0.044)	(0.046)		(0.043)	(0.043)
Party of the President		−0.007	−0.012		0.026	0.027
		(0.050)	(0.052)		(0.066)	(0.064)
Incumbent Mayor Ideology		−0.027	−0.025		−0.010	−0.013
		(0.037)	(0.037)		(0.035)	(0.036)
Incumbent Mayor Education		0.015	0.015		0.027^{+}	0.028^{+}
		(0.017)	(0.017)		(0.015)	(0.016)
Δ Spending		0.000	0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Years since PMAT application			−0.007			−0.013
			(0.021)			(0.016)
Constant	0.419*	0.580	0.483	−0.013	0.573	0.467
	(0.181)	(0.882)	(0.919)	(0.247)	(0.870)	(0.869)
Observations	85	69	68	105	92	91

OLS models with robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. 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.

B Continuous instead of Binary Moderator Variable

In the below table, Column 1 repeats the main specification from the paper (Column 5 in Table 1) 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.3: Relationship between Δ Taxation ($t_{-1} - t_{-3}$) and Incumbent Vote Share (t)

	Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)
Δ Tax Revenue	−0.035***	−0.026**	−0.036***	−0.041***
	(0.009)	(0.008)	(0.010)	(0.010)
Received PMAT loan	−0.288**		−0.341*	
	(0.110)		(0.141)	
Years since PMAT loan		−0.048+		−0.166***
		(0.025)		(0.038)
Δ Tax Revenue × Received PMAT loan	0.026**		0.028*	
	(0.008)		(0.011)	
Δ Tax Revenue × Years since PMAT loan		0.004*		0.013***
		(0.002)		(0.003)
Vote Share Prior Election	0.341**	0.340**	0.378*	0.357*
	(0.110)	(0.112)	(0.162)	(0.158)
Population (logged)	0.005	−0.005	−0.027	−0.047
	(0.029)	(0.031)	(0.038)	(0.040)
Urban Population Share	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
GDP Growth	−0.045	−0.051	−0.017	−0.011
	(0.060)	(0.059)	(0.080)	(0.071)
Amount of Transfers (logged)	0.008	0.017	0.041	0.067
	(0.033)	(0.035)	(0.040)	(0.043)
Party of the Governor	0.037	0.035	0.027	0.031
	(0.023)	(0.022)	(0.029)	(0.028)
Party of the President	0.049	0.046	0.097*	0.091*
	(0.031)	(0.032)	(0.043)	(0.043)
Incumbent Mayor Ideology	0.014	0.016	0.049*	0.052*
	(0.017)	(0.017)	(0.023)	(0.021)
Incumbent Mayor Education	0.010	0.010	0.022*	0.021*
	(0.007)	(0.007)	(0.009)	(0.009)
Δ Spending	−0.000	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.353	0.213	−0.040	−0.186
	(0.303)	(0.311)	(0.389)	(0.395)
Observations	295	294	162	162

OLS models with robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

C Robustness to Different Dependent Variables

Table A.4: Relationship between Δ Taxation ($t_{-1} - t_{-3}$) and Electoral Outcome (t)

	Margin of Victory			Herfindahl Index		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Tax Revenue	−0.016	−0.048***	−0.052***	−0.019***	−0.022***	−0.025***
	(0.010)	(0.014)	(0.015)	(0.004)	(0.006)	(0.007)
Received PMAT loan	−0.286+	−0.422*	−0.390*	−0.169*	−0.198**	−0.215*
	(0.159)	(0.176)	(0.193)	(0.067)	(0.073)	(0.086)
Δ Tax Revenue × Received PMAT loan	0.026*	0.036**	0.035*	0.015**	0.018***	0.018**
	(0.012)	(0.013)	(0.015)	(0.005)	(0.005)	(0.006)
Margin of Victory Prior Election	0.319**	0.343**	0.352**			
	(0.105)	(0.116)	(0.124)			
Population (logged)		0.020	0.001		−0.001	−0.010
		(0.044)	(0.048)		(0.019)	(0.021)
Urban Population Share		0.000	0.001**		0.000	0.000**
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		0.056	0.092		−0.058	−0.033
		(0.080)	(0.099)		(0.046)	(0.057)
Amount of Transfers (logged)		0.023	0.055		−0.001	0.017
		(0.048)	(0.054)		(0.021)	(0.023)
Party of the Governor		0.067*	0.075+		−0.001	−0.002
		(0.033)	(0.039)		(0.015)	(0.018)
Party of the President		0.076+	0.089+		−0.003	0.006
		(0.045)	(0.052)		(0.017)	(0.019)
Incumbent Mayor Ideology		0.024	0.033		0.001	0.013
		(0.025)	(0.030)		(0.010)	(0.011)
Incumbent Mayor Education		0.023*	0.026*		−0.001	0.001
		(0.009)	(0.011)		(0.005)	(0.005)
Δ Spending		−0.000	−0.000		−0.000	−0.000*
		(0.000)	(0.000)		(0.000)	(0.000)
Years since PMAT application			−0.010			0.002
			(0.011)			(0.005)
Herfindahl Prior Election				0.170**	0.193**	0.236**
				(0.063)	(0.074)	(0.088)
Constant	0.165	−0.325	−0.674	0.594***	0.621**	0.372+
	(0.138)	(0.421)	(0.496)	(0.065)	(0.197)	(0.211)
Observations	324	293	239	328	295	240

OLS models with robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

D Robustness to a Different Independent Variable

Table A.5: Relationship between Δ Total 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 Total Tax Revenue	−0.014	−0.035*	−0.034*	−0.012	−0.012	−0.015
	(0.010)	(0.015)	(0.016)	(0.010)	(0.014)	(0.015)
Received PMAT loan	−0.319*	−0.343+	−0.285	−0.169	−0.263	−0.283
	(0.160)	(0.175)	(0.184)	(0.158)	(0.164)	(0.182)
Δ Total Tax Revenue × Received PMAT loan	0.026*	0.027*	0.024+	0.016	0.023*	0.025+
	(0.011)	(0.012)	(0.013)	(0.011)	(0.011)	(0.013)
Vote Share Prior Election	0.308**	0.307**	0.387**	0.303***	0.373***	0.426***
	(0.104)	(0.113)	(0.141)	(0.091)	(0.105)	(0.125)
Population (logged)		−0.033	−0.059+		−0.015	−0.039
		(0.031)	(0.034)		(0.030)	(0.032)
Urban Population Share		0.000	0.000		0.000	0.001*
		(0.000)	(0.000)		(0.000)	(0.000)
GDP Growth		−0.080	−0.078		−0.040	−0.022
		(0.053)	(0.069)		(0.059)	(0.071)
Amount of Transfers (logged)		0.057+	0.093*		0.003	0.036
		(0.033)	(0.037)		(0.032)	(0.037)
Party of the Governor		0.011	0.021		0.034	0.033
		(0.020)	(0.023)		(0.022)	(0.026)
Party of the President		0.030	0.048		0.056+	0.079*
		(0.033)	(0.037)		(0.031)	(0.035)
Incumbent Mayor Ideology		0.021	0.030+		0.016	0.032+
		(0.015)	(0.017)		(0.016)	(0.019)
Incumbent Mayor Education		0.003	0.007		0.010	0.014+
		(0.007)	(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			−0.006			−0.006
			(0.007)			(0.007)
Constant	0.504**	0.097	−0.353	0.421**	0.360	−0.003
	(0.161)	(0.340)	(0.391)	(0.154)	(0.312)	(0.367)
Observations	290	257	213	348	312	257

OLS models with robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

E Robustness Checks for Fixed Effects Model

Table A.6: Fixed Effects Regression Estimating Effect of Δ Taxation ($t_{-1} - t_{-3}$) on Electoral Outcomes (t) Conditional on PMAT

	Inc Ind Margin Victory	Herfindahl
	(1)	(2)	(3)
Δ Tax Revenue (logged)	0.026	−0.007	0.002
	(0.026)	(0.011)	(0.014)
FirstPaymentMade	−0.131	−0.263*	−0.357*
	(0.338)	(0.113)	(0.173)
Δ Tax Revenue (logged) × FirstPaymentMade	0.014	0.019*	0.027*
	(0.027)	(0.009)	(0.013)
Population (logged)	−0.267	0.027	0.299
	(0.459)	(0.298)	(0.574)
GDP Growth	−0.055	−0.006	−0.069
	(0.100)	(0.056)	(0.103)
Amount of Transfers (logged)	−0.048	0.140	0.142
	(0.310)	(0.144)	(0.162)
Urban population share (logged)	0.001	0.000	−0.000
	(0.001)	(0.000)	(0.000)
Δ Spending			−0.000
			(0.000)
Party of the President			0.032
			(0.036)
Party of the Governor			−0.012
			(0.022)
Constant	3.768	−2.456	−5.374
	(7.086)	(3.648)	(5.493)
Observations	377	713	445
Municipal FE	Yes	Yes	Yes
Election FE	Yes	Yes	Yes

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Standard Errors Clustered at Municipality

F Mitigating Bias Caused by Endogeneity of Moderator and Explanatory Variable

One potential problem with the main interaction models presented in the paper is that PMAT can have both a direct and indirect effect on the outcome of interest, and we are conflating the two in our current specification. In other words, PMAT could by itself lead to a increase in tax revenues, which might bias our results when estimating the interaction effect between changes in tax revenues and receiving the first PMAT loan. Overall, we are less interested in the direct effect – how PMAT increases tax revenues and thereby effects voting behavior, and more interested in the indirect or moderated effect – how PMAT changes voter expectations such that a similar increase in taxes in PMAT and non-PMAT places leads to differential electoral outcomes. Consider our main independent variable Δ *Tax*. In cases where PMAT is introduced, we could decompose changes in tax revenue into those directly caused by PMAT policies ($\Delta\ Tax_{PMAT}$) and those due to other policy changes by elected officials ($\Delta\ Tax_{policy}$). In the models above, however, we are only able to estimate the moderating effect of PMAT on the total change in tax revenue. Thus, our results may suffer from a kind of "post-treatment" bias in which our moderator (PMAT) has a positive effect on the treatment (Δ Tax).

Ideally, we would like to isolate the two different effects of PMAT: the moderating effect and the direct effect on changes in tax revenue. In this section, we, therefore, attempt to empirically decompose $\Delta\ Tax$ 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 treatment variable of interest.

Based on previous work in the biostatistics literature (e.g., VanderWeele and Robins, 2007*b*; VanderWeele and Robins, 2007*a*), Wodtke and Almirall (2017) propose the structural nested means model as a way to correct for post-treatment bias in models where the moderator is partly affected by earlier treatments. The idea is to regress the moderator on earlier

treatments. One then "purges" the post-treatment part by using the residuals from the first stage as the independent variable the model of interest. While our problem is not quite the same, we adjust this strategy for our purposes. We regress our independent variable of interest Δ *Tax* on PMAT. 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 vote share outcomes. Specifically, we estimate the following two stage model:

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

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

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

The idea is that by residualizing the change in tax revenue, we are able to isolate the changes that are not directly caused by PMAT and can, therefore, estimate the moderating effect of fiscal capacity on taxes without conflating it with the effect of PMAT on revenues. Given that our independent variable in the second stage is based on estimates from the first stage, standard errors have to be adjusted. This can be done by bootstrapping. Instead, we estimate a Bayesian model in which both stages are estimated sequentially at each iteration of the sampler. Thus, the uncertainty from the first stage estimation is automatically accounted for in the estimates of the second stage model.

We estimate the models using Hamiltonian Monte Carlo in Stan (Team, 2017). We specify uninformative Gaussian (0, 10) priors for all coefficient estimates and Cauchy priors for the errors in both stages. We run four chains of 4000 iterations (1000 warmups) and save every third iteration, which leaves us with 4000 draws from the posterior distribution. All evidence suggests that the chains are converged.

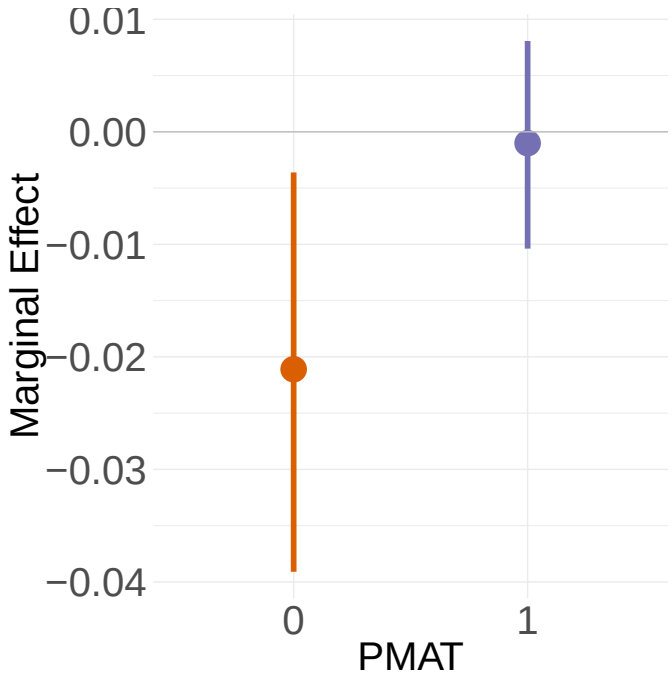
Table A.7: Second Stage Results – Structural Nested Means Model

	Model without Controls						
Parameter	Rhat	effective Sample	Mean	SD	2.5%	Median	97.5%
Intercept	1.000	3878	0.276	0.047	0.184	0.275	0.369
Vote Share Prior Election	1.001	3756	0.277	0.083	0.118	0.276	0.438
Residualized Δ Tax	0.999	3874	-0.021	0.009	-0.039	-0.021	-0.004
PMAT Payment	0.999	3731	0.045	0.022	0.001	0.044	0.088
PMAT $\times$ Residualized Δ Tax	0.999	3947	0.020	0.010	-0.000	0.020	0.040
	Model with Additional Controls						
Parameter	Rhat	effective Sample	Mean	SD	2.5%	Median	97.5%
Intercept	1.000	3145	0.237	0.133	-0.023	0.236	0.501
Vote Share Prior Election	1.000	3920	0.282	0.080	0.127	0.282	0.440
Population (logged)	1.000	3410	0.003	0.011	-0.018	0.004	0.025
GDP Growth	1.000	3812	-0.033	0.060	-0.152	-0.034	0.083
Residualized Δ Tax	1.000	3752	-0.022	0.010	-0.043	-0.023	-0.002
PMAT Payment	1.000	3719	0.046	0.022	0.002	0.046	0.089
PMAT $\times$ Residualized Δ Tax	1.000	3863	0.019	0.010	-0.001	0.020	0.040

Table A.7 shows the results for the second stage of the model with incumbent party vote share as the dependent variable. The top five rows show the estimated coefficients for the model when we only include a control for lagged incumbent vote share. The lower part of the model shows the results when add additional controls for logged population size and GDP growth. The results in both correspond closely to those in Table 1 above, except that here we residualize the Δ *Tax* variable first. The only stark difference is that the coefficient on the constituent PMAT term become positive. This suggests that it potentially suffered from post-treatment bias in the previous specification. Importantly, however, the interaction term is positive, and the 95% credible interval does not include zero for the model where we only control for the lagged incumbent vote share. Once we add additional controls, the interaction term is still positive and the 90% credible interval does not cover zero. Similarly, Figure A.1 shows the marginal effect of changes in tax revenue for municipalities with and without having received a first PMAT payment. Here we plot the marginal effect for the model without the additional controls. The results are very similar to those presented above.

Contrary to the visual impression, the two marginal effects are significantly different from each other, i.e., the 95% credible interval for the difference between the two marginal effects does not include zero. This is true for the 90% credible interval in the model with additional controls.

Figure A.1: Marginal Effect of Residualized Δ Tax Revenues on Incumbent Party Vote-share given PMAT Payment Status



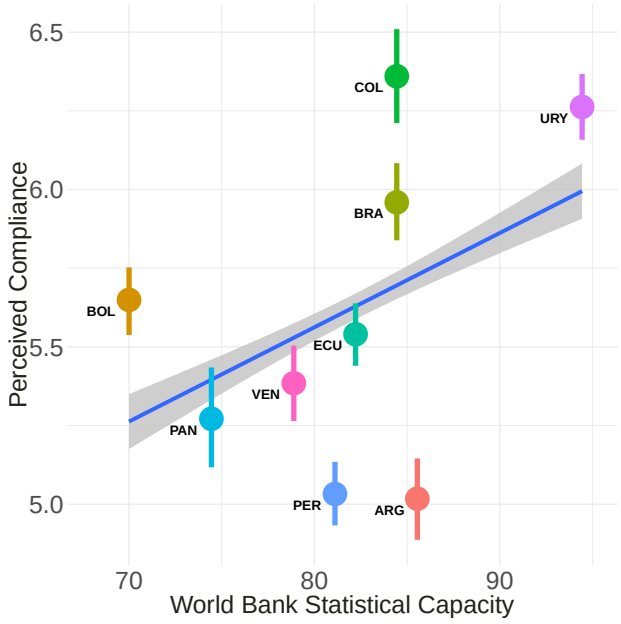
G Tables Micro-Evidence

Table A.8: Perceived Compliance and Fiscal Capacity (no weights)

	Perceived Individual Compliance			Perceived Firm Compliance		
Probability of Sanction	0.36***			0.23***		
	(0.02)			(0.02)		
Impartiality of Collection		0.24***			0.34***	
		(0.02)			(0.02)	
Capacity Proxy			0.43***			0.40***
			(0.02)			(0.02)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8,102	9,259	7,906	8,034	9,206	7,860
R^2	0.09	0.07	0.11	0.10	0.10	0.12

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

Figure A.2: Relationship between Countries' Statistical Capacity and Perceived Compliance



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

Table A.9: 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.10: 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.3: Marginal Effect of Government Performance on Willingness to Pay by Perceived Capacity with Binning