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

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Abstract

We argue in this paper that the established taxation-accountability hypothesis according to which voters and politicians trade higher taxation for more accountability is conditional on the state having achieved a minimum threshold of fiscal capacity. There is a consensus that taxation strengthens electoral accountability by increasing the bargaining power of taxpaying citizens or by making taxpayers less tolerant of politician misbehavior. One critical but implicit assumption, however, is that states have sufficient capacity to collect taxes efficiently. We contend that in low fiscal capacity settings, citizens expect weaker benefits from taxation and are thus more likely to sanction politicians who increase taxes. We test our argument by examining the relationship between tax collection and subsequent voting behavior in comparable low and high fiscal capacity settings. We take advantage of a program in Brazil (PMAT) that served as a positive shock to fiscal capacity in some localities. Within past recipients and eventual recipients of the program, we compare local tax revenue to local election outcomes. We find that, indeed, the relationship between taxation and voting is highly conditional on fiscal capacity: places that have not yet received PMAT are significantly more likely to punish incumbents for tax increases relative to places that have already received the shock to fiscal capacity. We support these findings with additional survey evidence from Mexico and 36 African countries, which shows that voters' willingness to tax in response to government performance is conditional on fiscal capacity.

Key Words: Fiscal Capacity, Taxation, Democracy

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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, is at least in its theoretical origin based on the experience and development of the European tax state and assumes that states are capable of enforcing tax policies.[1] The state here 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 a 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 raising

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[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).

tax rates or collection capacity will result in sufficient revenue to produce public goods that will benefit them. Further, they more likely believe that any additional increase in taxation only exacerbates unfairness inherent in differential enforcement. 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 low income) prefer higher taxation when they benefit from public goods provision or redistribution. A key observable implication we test in this paper is: in low capacity settings, politicians are more likely to be punished for tax increases; 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 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 such as age of democracy or clientelistic politics. Additionally, fiscal capacity is generally 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 across-municipality

variation in tax collection and subsequent voting behavior in Brazil. We take advantage of a federal loans program to increase municipal fiscal capacity, PMAT, which serves as our quasi-exogenous shock to capacity. To better identify the effects of fiscal capacity, we only compare municipalities that receive PMAT at some point during the study. 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. We additionally address a secondary threat to causal inference generated by the endogeneity between our independent variable of interest (tax increases) and our moderating variable (fiscal capacity as measured by PMAT). PMAT has a direct effect on tax revenue increases, but we are more interested in the indirect effect: how PMAT changes voter expectations such that a similar increase in taxes in PMAT and non-PMAT places leads to differential electoral outcomes. We use a structural nested means model to isolate the part of tax revenue change that is not due to PMAT's introduction.

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 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

Mexican municipalities and African countries. While less well-identified, the survey results provide evidence from different contexts and illustrate a plausible theoretical mechanism. In particular, we argue that the electoral sanctioning of taxation in low-capacity municipalities in Brazil is driven by a lower willingness of citizens to trade off tax payment for good government performance (because government performance is less valuable in a low-capacity state). We show that, in Mexico, a respondent's confidence in government has a positive effect on willingness to pay taxes only in high capacity municipalities. Similarly, across 36 African countries, approval of national politicians is positively related to tax morale *only* among respondents who perceive capacity to be high. We interpret both findings as evidence that fiscal capacity moderates behavior at the ballot box because of how it moderates citizens' willingness to trade off tax payment for government performance.

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.[3] This innovation has implications for potential interventions to increase tax compliance. While we mainly focus on voter preferences and behavior, we discuss the implications of the voter's calculus for politician behavior. Our conclusions – that politicians in low capacity places have strategic disincentives to invest in fiscal capacity –

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[3]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.

can partly explain the stickiness of the high and low fiscal capacity equilibira we observe in the world. While these differences are often attributed to capacity only, our theory suggests there may additionally be strategic incentives that further discourage (encourage) taxation by incumbents in low (high) capacity places.

Theory

Models of electoral accountability generally assume that voters reward or punish politicians based on their performance in office. Whether raising taxes sends a good or bad signal to voters about incumbent performance is ambiguous, and depends on what the government 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, implying that only knowing tax levels is insufficient to reach a prediction about subsequent electoral behavior. In other models, taxes are more explicitly linked to electoral accountability in that the experience of being taxed increases incentives for voters to monitor politician performance because spending is more closely linked to their own pocketbook (Paler, 2013; Martin, 2014). So while electoral accountability models describe how voters evaluate politicians on the basis of their performance with no direct implication for taxation, the taxation-accountability hypothesis suggests that voters who are taxed relatively more should place more weight on their performance evaluation of the incumbent when making electoral decisions.[4]

We argue that another parameter – fiscal capacity – should enter into the voter's calculus in these latter models, informed by expectations of performance that motivate voter behavior in the former models. Because governments provide public goods that are financed through

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

collective taxation, effective public spending will depend on how many people are paying taxes, and how efficiently governments collect them. 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.

Voter Preferences

For taxpayers i and $-i$, there are strategic complementarities to paying taxes: taxpayer i benefits more from paying taxes when more taxpayers $-i$ also pay taxes. Given this, voters should respond to fiscal extraction by high versus low capacity states differently for two reasons. First, the same amount of taxes paid by person i will yield greater benefits to person i in a high fiscal capacity setting where more people are paying taxes relative to a low fiscal capacity setting with fewer taxpayers. We also know people maximize constructs other than material welfare when making costly decisions. If taxpayers care about fairness, then taxpayer i in a high fiscal capacity setting will perceive tax payment as relatively more fair than in a low fiscal capacity setting because more taxpayers $-i$ will also be paying taxes.

Governments can choose to raise revenue by increasing tax rate or by increasing the number of taxpayers. We argue that irrespective of which mechanism the government chooses, the differential response by taxpayers in high and low capacity settings should follow the pattern described above. First, if a taxpayer observes an increase in tax rates in a low capacity setting, paying more individually will translate into relatively small collective gains. However, in a high capacity setting, the taxpayer will assume that same tax increase translates into a larger total increase in public revenues.

Second, marginal increases in enforcement, e.g., the state attempting to register more

citizens as tax payers, are more likely to appear to voters as further improvements in the fairness and efficiency of the current tax system in high capacity settings. On the other hand, such increases in enforcement in low capacity settings are likely to give affected tax payers the impression that the government is simply engaging in unsystematic tax extraction accompanied by a weak increase in public revenues. Public spending in low capacity settings is also expected to be uneven across groups, often described as clientelistic targeting. Expectations of unequal spending (rather than unequal payment) is also found to moderate attitudes toward taxation: Sy-Sahande (2017) finds that in one setting with high clientilistic expectations, citizens who learn their district voted more (less) strongly for the winning candidate raised (lowered) their support for taxation. Both going from being a non-taxpayer to a taxpayer and going from paying lower to higher taxes are more likely to be tolerated in a high fiscal capacity setting than a low capacity setting.

While the marginal benefit of an increase in taxation and ergo higher redistribution may be positive for the poor under standard theoretical models, in reality, we think there may still 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, the negative impact of low capacity might de facto be worse on the poor than the rich. Second, large public goods may require some threshold level of tax revenue to be provided. In low capacity settings, the benefit from public goods investments may, therefore, not materialize due to the limited tax revenue collected. With this expectation in mind, the poor have less interest in higher taxes under low capacity.

Electoral Behavior

We have established that citizens 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 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, tax collection, voter attitudes about evasion, and government responsiveness work as complements to each other (Casaburi and Troiano, 2015).

One implication of Hypothesis 1 is that while politicians can hope to trade off taxation for representation in a high capacity setting, they are unable to make this bargain in a low

capacity setting. Given that voters in low capacity places are less likely to see value in tax increases, incumbents have little reason to propose policies that increase taxes and spending. There are thus electoral *disincentives* to ever raise taxes or fiscal capacity in a low capacity setting. We explicitly address the strategic behavior of politicians in the next section.

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 in our data.

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

Politician Behavior

Given the strategic nature of politician decision-making – politicians seek to maximize electoral returns given voter preferences, our insight about differential voter preferences in high and low capacity settings has implications for optimal politician behavior in each setting. Given greater voter aversion to taxation, in a low fiscal capacity setting, politicians will be disincentivized from tax collection. Since they still need to mobilize voters, they should "perform" in other ways than raising and spending public revenue. Politicians interested in reelection may thus prefer targeted benefits 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.

HYPOTHESIS 3 *In a low fiscal capacity setting, politicians are less likely to make marginal investments in revenue collection than in a high fiscal capacity setting.*

This prediction is consistent with that made in Martin's (2016) formal model that endogenizes fiscal capacity and argues that rent-seeking governments may abstain from taxing groups of citizens to avoid being held accountable by them. It similarly predicts an equilibrium condition in which few taxes are collected and few public goods are provided – which is supported by politician disincentives to invest in revenue collection. While this paper focus on the politician's incentives, our theory and empirics are more focused on voter preferences and behavior that end up supporting this low fiscal capacity equilibrium.

Alternative Mechanisms

Hypothesis 1 makes a prediction that is observationally equivalent to another common theoretical prediction in the literature: increases in public spending should be rewarded by voters. Since fiscal capacity to tax and bureaucratic capacity to spend are likely highly correlated, our prediction – that voters should sanction incumbent politicians more for marginal tax revenue increases in low fiscal capacity settings relative to high fiscal capacity settings – could be entirely driven by the fact that voters sanction tax increases more when they are met with less public spending in low bureaucratic capacity places.

While we do not dispute that expected future spending levels figure into a voters' cal-

culus when deciding on whether or not to sanction an incumbent politician, we argue that this relationship is also conditioned by voters' perceptions of how many other citizens are simultaneously paying taxes and growing the public budget. In our empirical analysis, we will attempt to discriminate between whether increases in spending that result from higher tax revenue are responsible for any change in voter behavior, or if there are additional effects of tax increases outside of their direct effect on spending. Furthermore, finding evidence in favor of Hypothesis 2 – that politician performance also affects voters' willingness to pay taxes in high but not low capacity settings – would support the idea that fiscal capacity as a moderator of vote choice is working through its effect on voter beliefs about the benefit to paying taxes in addition to simply moderating voter expectations of future government spending.

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 such as age of democracy or clientelistic politics. We here take advantage of a rare "shock" to fiscal capacity that occurred in some Brazilian municipalities that 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). 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 Tributària* 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 taxpayer 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 their respective contract years 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 differently 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. First, we estimate a number of regressions modeling the onset of PMAT payments. We subset our data to only those municipalities who 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.[5] 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 we might be most concerned about: property tax revenue (at $t-1$ and

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

$t - 2$) 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.[6]

Data

To empirically evaluate the link between fiscal capacity, taxation, and electoral account-ability, 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: tax increases during the mayor's term in office.

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 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

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[6]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.

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 from the Brazilian Institute of Geography and Statistics (IBGE, 2016) and transfers the municipality receives from the federal and state government from the Institute of Applied Economic Research (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.

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 incum-

bent 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 to mitigate 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. 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 the alternative mechanism we discussed in the theory section – that voters are rewarding incumbents for increased spending on public goods – we add a control for the change in public spending

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

Figure 1: Relationship between Δ Tax Revenues and Electoral Outcome, by PMAT Status
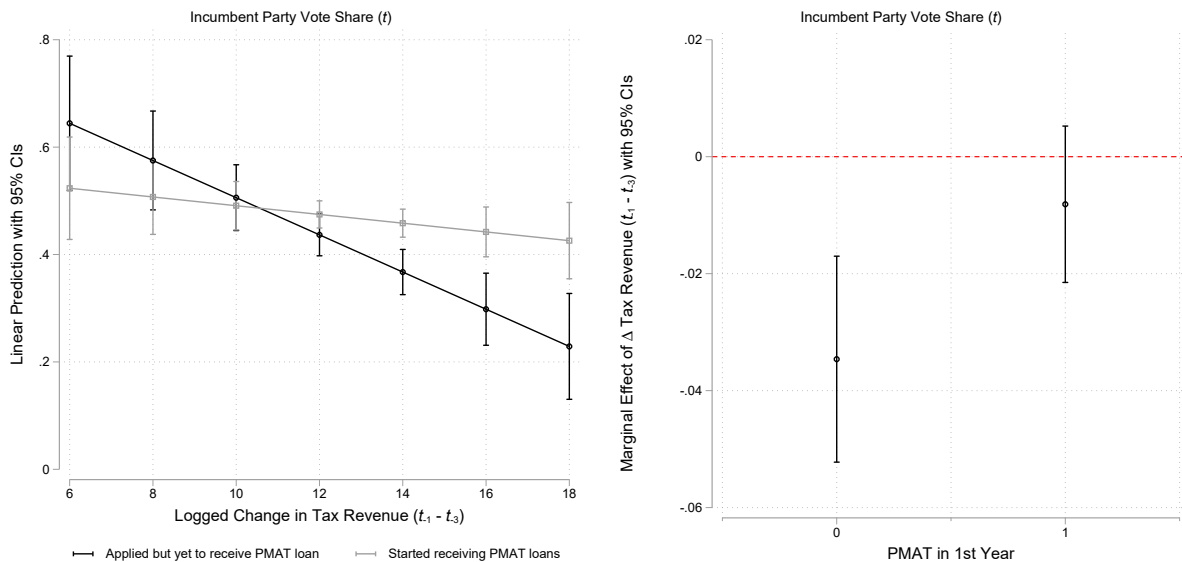
As a further test of our theory, we can check whether municipalities that receive PMAT loans earlier and thus have a longer time to develop fiscal capacity demonstrate a relatively stronger positive relationship between taxation and incumbent vote share relative to places that have only just received 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 between our comparison groups, we 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.2), 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. 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.[7] 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.2).

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

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

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 and independent variable are endogeneous. 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.3, 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 has the effect of making 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.4 demonstrates that our findings are indeed robust to this alternative specification.

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 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.

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

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

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.5 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. Lastly, Table A.6 in the Appendix shows the fixed effects model with the limited set of controls once we use the raw change in tax revenue in millions instead of the logged change as the main independent variable. The interaction remains positive and significant (at either $p \leq 0.05$ or $p \leq 0.1$) for incumbent party vote share, margin of victory, or the Herfindahl index as the dependent variable.

Finally, in Appendix F, we attempt to mitigate an unexplored issue for making causal inference: the fact that our moderator (PMAT) and explanatory variable ($\Delta\ Tax$) are endogenous. 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 Δ 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.

Willingness to Pay Tax and Government Performance

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 two other empirical contexts allows us to generate evidence in favor of this mechanism. Data from neither context – Mexico and 36 countries African countries – allows us to identify the moderating effect of fiscal capacity as cleanly as in the case of Brazil. Nevertheless, these data provide 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.

Individual Level Evidence from Mexico

In both Mexico and across African countries, we test whether the hypothesized positive relationship between perceived government performance and willingness to pay tax is moderated by fiscal capacity. In Mexico, we follow Garfias (2018) and measure municipality-level fiscal capacity using data on the number of bureaucrats per 1000 individuals. The independent variable, *perceived government performance*, is measured using individual-level survey data on confidence in the local government and the dependent variable, *willingness to pay tax* is measured using a series of questions from the same survey in which respondents were asked about their willingness to vote for a measure that would raise taxes to reduce crime by 30% (ICTD, 2013). Each participant is offered different hypothetical amounts of a tax increase until arriving at a ceiling or floor value to determine willingness to pay.

Results of a simple OLS regression of willingness to pay tax on the interaction between local fiscal capacity and confidence in local government are illustrated in Figure 3. As suggested by the theory, 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 2: Test of Hypothesis 2 in Mexico Data
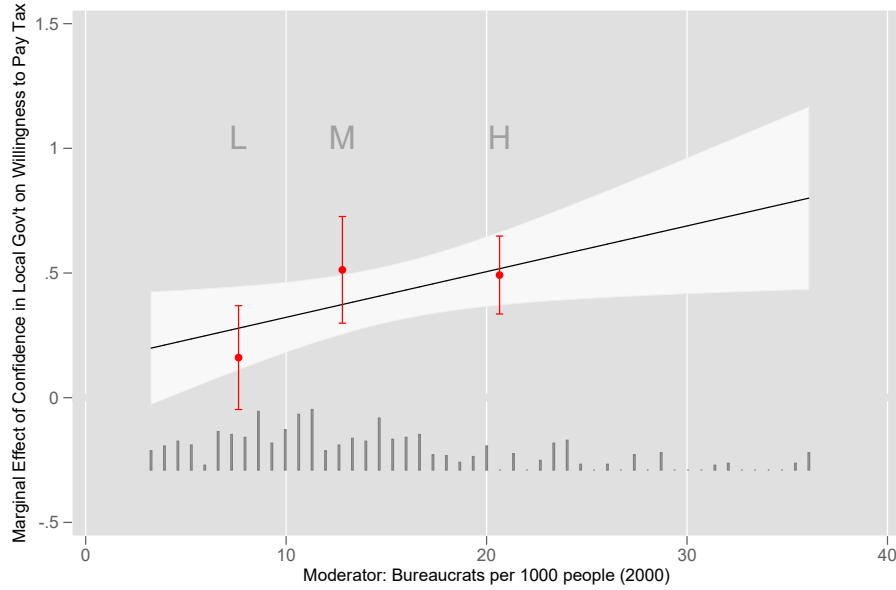
Note: This equation includes controls for gender, urban residence, monthly income and level of education. Results are substantively unchanged without them.

Individual Level Evidence from African Countries

As one test of whether this phenomenon generalizes beyond Latin America, we use data for 36 countries in Africa from the sixth wave of the *Afrobarometer* survey (Afrobarometer Data, 2016). We estimate similar models to those for Mexico above. Here, however, we evaluate whether the effect of citizen approval on their willingness to pay taxes is moderated by the respondent's *perceived* level of fiscal capacity rather than an objective measure. Given our theoretical arguments above, we expect that fiscal capacity increases citizens' willingness to pay taxes and does so, in particular, for those that approve of politicians in power. Or in other words, especially for voters who are in congruence with elected officials, higher levels of perceived capacity will increase their willingness to pay taxes. In contrast, even when voters' preferences are aligned with politicians in power (i.e., high approval), they should be

less willing to pay taxes if they believe capacity is low.

In the *Afrobarometer* survey, respondents are asked whether good citizens should always pay taxes, never, or only if they choose to. We use the response to this question as our measure of willingness to pay taxes – our dependent variable. To measure perceived capacity, we dichotomize whether respondents have difficulty finding out what taxes and fees they are supposed to pay to the government. The idea is that in high capacity settings the rules should be clear, consistently applied, and voters would have no trouble finding out what they owe the state.[8] We interact our measure of perceived capacity with dichotomized responses about politician approval. Specifically, we estimate our models by interacting perceived capacity with the respondent's approval of the president, the respondent's approval of his/her parliamentarian, and the respondent's approval of the local elected official.

Given that our dependent variable is an ordered outcome with three categories, we estimate ordered logit models. We add random intercepts for countries and cluster standard errors at the country level. We include the following available control variables to increase the precision of our estimates: the respondent's gender, race, highest level of education, income (proxied for by whether the respondent has a job that pays cash and how often in the past year family members have gone without food or cash), and urban vs. rural residence.

We present the tabular results for the random effects model in Appendix Table A.8.[9] Most importantly for our theoretical argument, we find that the interaction between leader approval and perceived capacity is positive for all three types of elected official; however, the interactions are only statistically significant for national politicians (president and par-

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[8]Our main results are substantively the same if we measure capacity by whether citizens think it is difficult to evade taxes.

[9]The substantive results are the same across random and fixed effects models (see Appendix Table A.9 for the latter.)

Figure 3: Test of Hypothesis 2 in Afrobarometer Data
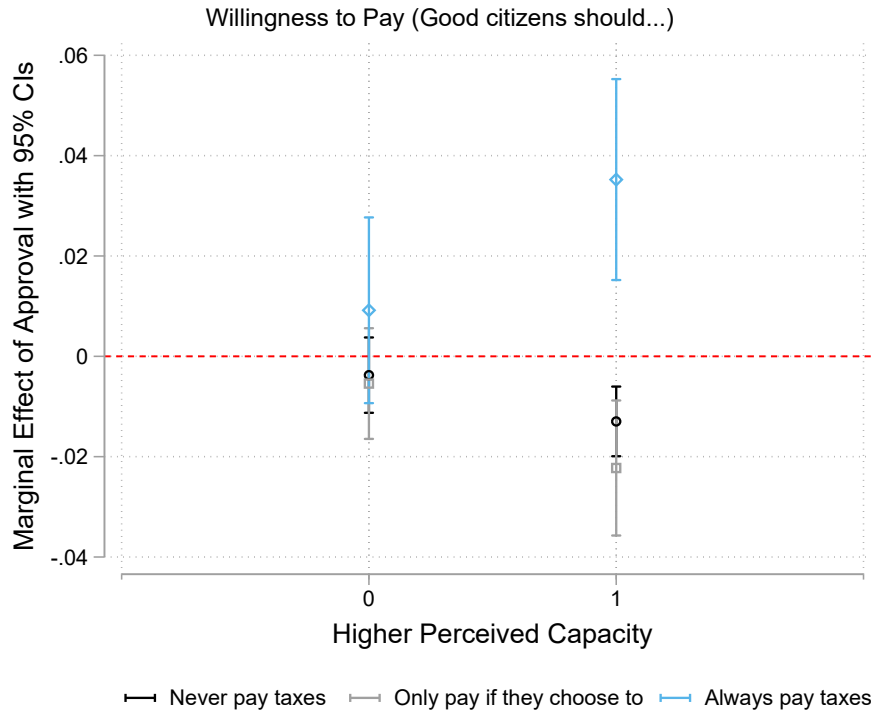
Note: Marginal effects of approval of the parliamentarian on willingness to pay under low and high perceived fiscal capacity. This estimation includes controls for gender, urban residence, level of education, proxies for income as well as country random effects.

liamentarian).[10] Figure 3 displays the marginal effect of approving of the parliamentarian under low and high perceived fiscal capacity.[11] As one can see, the predicted values for the lower two categories of whether citizens have a duty to pay taxes (*Never* or *Only if they choose*) are almost indistinguishable). And, at low levels of capacity the marginal effect of approval on the predicted values of the three outcome categories is very small. On the other hand, the marginal effect of leader approval on the probability that someone answers

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[10]Given that respondents were asked about general taxes (as opposed to local security as in the Mexican context above), it is not surprising that the moderating effect is observable for national politicians but not as pronounced for local government officials. Voters in this context may think of taxation as a function of the central state, and thus evaluate those representatives when thinking about tax policy and whether paying taxes is a citizen's duty.

[11]The marginal effects for presidential approval look very similar, though slightly less pronounced.

with the top willingness-to-pay category (*"Citizens should always pay taxes"*) is large and statistically significant among people who perceive higher state capacity. Thus, under high perceived fiscal capacity, respondents aligned with the politician in power are much more likely to believe that citizens should *always* pay taxes and less likely to think they should *never* do so or only if they choose to. No such relationship exists between government approval and fiscal compliance among people who perceive weaker fiscal capacity. Overall, this evidence, even if only correlational, lends additional credence to our theoretical argument.

Relationship between Fiscal Capacity and Tax Increases

Hypothesis 3 suggests that places with low fiscal capacity should be less likely to increase taxes, given expectations of electoral retribution, relative to places with higher fiscal capacity. Here, we provide a suggestive empirical test of this theoretical implication, though it is subject to greater concerns of omitted variable bias than our prior tests of Hypothesis 1. Because places that receive PMAT payments at different times during the electoral mandate are arguably "as-if" randomly assigned, or similar on all other dimensions except for the receipt of PMAT, these are not appropriate comparison groups for a test of Hypothesis 3. In this theoretical prediction, we expect politicians are responding to different equilibrium conditions and that many more aspects of localities should thus differ across equilibria. As a result, we compare places that never receive PMAT in our data to places that do. Because we know these groups differ on many other dimensions than just fiscal capacity, we control for all the same variables as in Equation 1.

To test the relationship between fiscal capacity and tax increases, we first disaggregate our main independent variable, a proxy for fiscal capacity, by the timing of the receipt of

PMAT loans: whether the locality received its first PMAT payment prior to the electoral mandate during which tax changes are being measured, during that mandate, or subsequent to that mandate. We then regress the change in taxes on this independent variable of interest and all controls. In column 1 of Table 3, the dependent variable is raw changes in tax revenue in 1000 $R; in column 2, we take the log of the raw data. Because we are now interested in both increases and decreases in tax changes (relative to Equation 1 where we only examine tax increases), we use a log-negative transformation where we log the absolute value of the change and then multiply originally negative values by −1.

The results provide suggestive evidence in line with Hypothesis 3: in both specifications, places with PMAT loans are more likely to increase tax revenues than the reference group – places that do not receive the PMAT program. This is especially true for places that receive PMAT transfers in that mandate, which is unsurprising given the objective of PMAT. However, we additionally see a negative though insignificant coefficient in column 1 on localities that receive PMAT before the electoral mandate in question. This could reflect a regression to the mean after a prior mandate in which tax revenue was substantially raised.

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

Table 3: Relationship between Fiscal Capacity and Δ Taxation ($t_{-}-1 - t_{-}-3$)

	Δ Tax Revenue (1,000 \$R)	Δ Tax Revenue (log-negative)
PMAT pre-mandate	-1774.954^+	1.662^{***}
	(948.732)	(0.213)
PMAT during mandate	892.369^{**}	1.956^{***}
	(317.270)	(0.179)
PMAT post-mandate	545.814	1.701^{***}
	(471.344)	(0.388)
Vote Share Prior Election	143.882	-0.108
	(252.560)	(0.213)
Population (logged)	256.029^{***}	0.212^{***}
	(74.066)	(0.063)
Urban Population Share	-0.135	0.002^{***}
	(0.728)	(0.000)
GDP Growth	-110.004^*	0.103
	(55.879)	(0.090)
Amount of Transfers (logged)	-795.520^*	0.880^{***}
	(356.694)	(0.081)
Party of the Governor	-36.608	0.100^+
	(68.381)	(0.059)
Party of the President	-255.394^+	-0.027
	(140.766)	(0.096)
Mayor Ideology Prior Election	30.542	-0.077^*
	(49.604)	(0.037)
Mayor Education Prior Election	26.033^*	0.050^{***}
	(12.503)	(0.013)
Δ Spending	0.000^{***}	0.000^*
	(0.000)	(0.000)
Incumbent Reelected Prior Election	-50.451	-0.334^{***}
	(77.917)	(0.079)
Constant	9800.064^+	-14.520^{***}
	(5063.691)	(0.846)
Observations	9373	9373

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

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.

Discussion

We have argued that the capacity of states to collect taxes will moderate the relationship between voters and politicians. For politicians and their 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 moderating effect of fiscal capacity across different specifications on the sample of Brazilian municipalities. Moreover, we recover a similar effect when we estimate the within-municipality effect of receiving the first PMAT loan in a fixed effect model.

What this research design gains in internal validity, it loses in external validity. The estimated effect is not generalizable to all municipalities. Our estimand is similar to an average treatment effect on the treated (ATT). In a difference-in-differences design, the estimated effect of the treatment is the ATT, as one compares units that have received treatment before and after the treatment occurred (as in our fixed effect model above). 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

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effect, changes in tax revenue, and all other covariates should, therefore, be interpreted similarly to a treatment effect on the treated.

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. But as we discuss above, when states or administrative units are in a low fiscal capacity equilibrium, minor improvements in capacity may not be enough to change the equilibrium pattern of behavior. It is possible that those municipalities that did select into the PMAT program already had higher initial levels of capacity, which made the capacity shock more meaningful in changing the electoral behavior. At a minimum, if we estimate similar models to those above on the full sample of Brazilian municipalities and measure fiscal capacity as collected revenue as percent of local GDP (instead of using the PMAT shock), we generate similar findings. While not as clean, this does suggest that our results may hold across all of Brazil.

We do, however, provide survey evidence that voters seem to care about capacity when they think about politicians and paying taxes. In both the Mexican case and across 36 African countries, survey respondents' evaluation of the government has different effects on their willingness to pay taxes depending on (perceived) fiscal capacity. Combining both pieces of evidence, from Brazilian municipalities and surveys, we believe provides a strong case in favor of the theoretical argument made above. Voters seem to take capacity into account when voting on politicians' performance.

Nevertheless, it may be that places with very low initial capacity additionally face strong electoral incentives *not* to increase tax revenue, and so 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. It is also possible that local governments face different strategic incentives than national governments. Eubank (2012), for instance, finds that in the low-capacity setting of Somaliland, a lack of access to foreign aid made the central government more likely to tax and be responsive to citizens relative to its aid-prone neighbors. This could imply that the kinds of goods that a central government provides, such as security, may generate greater incentives to raise revenue than the local public goods that municipal governments tend to provide.

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

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.2: 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.3: 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 $\times$ 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.4: 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.5: 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

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

	Inc Party	Margin Victory	Herfindahl
	(1)	(2)	(3)
Δ Tax Revenue in Mio	-0.018	-0.007^{+}	-0.013
	(0.012)	(0.004)	(0.009)
FirstPaymentMade	-0.046	-0.027	-0.021
	(0.044)	(0.023)	(0.028)
Population (logged)	0.378	0.133	0.335
	(0.693)	(0.247)	(0.491)
GDP Growth	-0.064	-0.016	-0.056
	(0.125)	(0.052)	(0.087)
Amount of Transfers (logged)	-0.153	0.103	0.090
	(0.191)	(0.132)	(0.124)
Urban population share (logged)	0.000	0.000	-0.000
	(0.001)	(0.000)	(0.000)
Δ Spending			-0.000
			(0.000)
Party of the President			0.050
			(0.035)
Party of the Governor			0.001
			(0.019)
Constant	-1.121	-3.068	-4.810
	(7.234)	(3.044)	(4.794)
Observations	481	773	481
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 models presented above 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 (ΔTax_{PMAT}) and those due to other policy changes by elected officials (ΔTax_{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). Note, that we show this to be the case above.

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 Δ *Tax* into ΔTax_{PMAT} and ΔTax_{policy}. We then estimate the same model from above but using only the estimated ΔTax_{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:

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

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

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

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. Commonly this is 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 3rd 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

Parameter	Rhat	effective Sample	Mean	SD	2.5%	Median	97.5%
Model without Controls							
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							
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 **??** above, except that we here residualize the Δ *Tax* variable first. The results are quite similar; 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 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 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
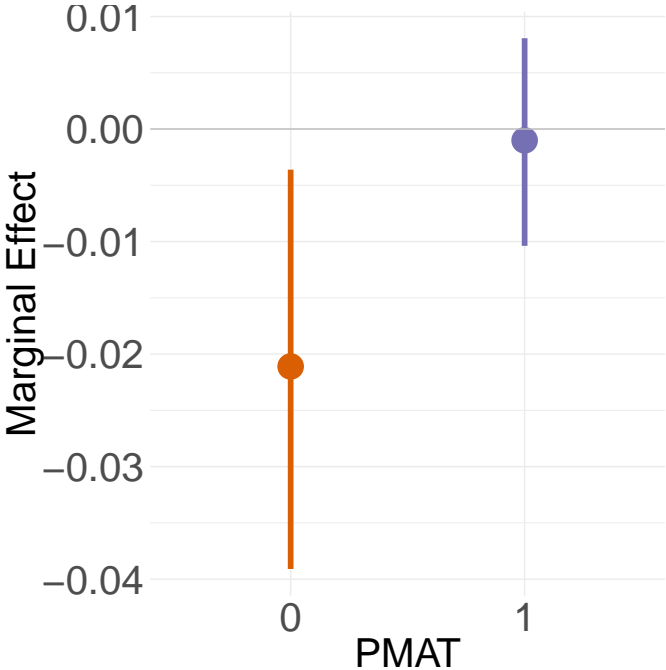
G   Tables Afrobarometer Models

Table A.8: Afrobarometer Evidence: Estimating Effect of Approval on Willingness to Pay Conditional on Capacity

	(President)	(Parliament)	(local)
Approve	0.157*	0.052	0.083
	(0.062)	(0.053)	(0.051)
High Perceived Capacity	0.175*	0.162**	0.239***
	(0.069)	(0.057)	(0.067)
Approve × Capacity	0.187*	0.179*	0.066
	(0.093)	(0.071)	(0.088)
Cash Paying Job	0.082***	0.079***	0.077***
	(0.015)	(0.016)	(0.018)
Gone w.o. Enough to Eat	−0.069***	−0.075***	−0.080***
	(0.018)	(0.019)	(0.022)
Gone w.o. Cash	−0.026	−0.022	−0.026
	(0.023)	(0.026)	(0.029)
Highest Level of Edu	0.050*	0.057**	0.070***
	(0.021)	(0.020)	(0.017)
Gender	−0.046*	−0.050*	−0.029
	(0.020)	(0.024)	(0.024)
Race	−0.034	−0.033	−0.104***
	(0.030)	(0.026)	(0.018)
Urban/Rural	0.152+	0.165+	0.141
	(0.092)	(0.091)	(0.098)
cut1			
Constant	−2.164***	−2.220***	−2.341***
	(0.263)	(0.269)	(0.285)
cut2			
Constant	−0.694**	−0.795***	−0.888***
	(0.240)	(0.241)	(0.264)
Observations	40966	37606	34706
Country RE	Yes	Yes	Yes

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

Standard Errors Clustered at Country Level

Table A.9: Afrobarometer Evidence: Estimating Effect of Approval on Willingness to Pay Conditional on Capacity (FE model)

	(President)	(Parliament)	(local)
Approve	0.157*	0.051	0.083
	(0.062)	(0.053)	(0.051)
High Perceived Capacity	0.175*	0.162**	0.239***
	(0.069)	(0.057)	(0.067)
Approve × Capacity	0.187*	0.179*	0.065
	(0.093)	(0.071)	(0.088)
Cash Paying Job	0.082***	0.079***	0.077***
	(0.015)	(0.016)	(0.018)
Gone w.o. Enough to Eat	−0.069***	−0.076***	−0.080***
	(0.018)	(0.019)	(0.022)
Gone w.o. Cash	−0.027	−0.023	−0.027
	(0.024)	(0.026)	(0.029)
Highest Level of Edu	0.051*	0.058**	0.071***
	(0.021)	(0.020)	(0.017)
Gender	−0.046*	−0.050*	−0.028
	(0.020)	(0.024)	(0.023)
Race	−0.026	−0.028	−0.098***
	(0.028)	(0.026)	(0.017)
Urban/Rural	0.150	0.163+	0.140
	(0.092)	(0.091)	(0.098)
cut1			
Constant	−1.428***	−1.408***	−1.634***
	(0.256)	(0.250)	(0.270)
cut2			
Constant	0.043	0.018	−0.180
	(0.223)	(0.215)	(0.242)
Observations	40966	37606	34706
Country FE	Yes	Yes	Yes

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

Standard Errors Clustered at Country Level