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Transparency, Elections, and Pakistani Politicians' Tax Compliance

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Abstract

A growing literature on political accountability focuses on the extent to which voters electorally punish politicians when provided with credible negative information about politicians' actions. Whether politicians respond to information provision by changing their behavior—thus appearing accountable to voters—is an integral part of this puzzle but has received comparatively little attention. I address this gap by exploiting an unforeseen decision by the Pakistani government to publicly release legislators' past income tax payments, and measure the effect of the information provision on their tax payments in the following year. Using new data on politicians' asset ownership and tax payments in a difference-in-differences research design, I provide strong evidence that the pressure to decrease tax evasion was highest for competitively and directly elected legislators. These heterogeneous effects are not explained by differences between legislators or electoral constituencies, supporting the hypothesis that electoral incentives condition legislator responsiveness to information shocks.

Keywords

political accountability, electoral incentives, Pakistan

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Introduction

Does information provision to citizens affect the accountability of politicians? The repercussions faced by some politicians in the wake of events like the Panama Papers leaks of 2015 suggest that it might. These leaks revealed previously unknown information about offshore holdings and personal finances of many elites and politicians around the world, bringing to the fore issues like tax evasion and illegal asset holdings. Although existing literature has often focused on whether voters electorally punish politicians when provided with new and credible information about politicians' negative actions, such as what these papers revealed, what has received considerably little attention is whether politicians react to information provision by changing their subsequent behavior. Information provision to citizens is important precisely because it can affect accountability but, to fully understand how this works, we also need to analyze whether politicians respond to such events in ways that make them appear responsive to their constituents.

In this article, I begin to address this gap in the literature by using a sequence of events in Pakistan that affected incumbent legislators. Specifically, I exploit an unforeseen "transparency shock" that publicly released information about legislators' federal income tax payments from the previous year, and measure its effect on their tax payments in the subsequent year. I use these data in a difference-in-differences framework to answer two related questions. First, does providing information to citizens about incumbent politicians affect politicians' subsequent actions? I find an average increase in tax compliance in the year after the information shock. Second, do politicians respond differently conditional on different levels of electoral pressures? Here, I find that competitively elected legislators increased their tax payments, on average, up to 12 times more than other legislators.

The elections I use to measure competition occurred before either year of taxes were filed, and I also show that these groups of legislators are comparable on other dimensions relevant to their tax payment propensity, including legislative experience, age, education levels, and so on. The results also hold when controlling for unobservable differences between geographical regions, individual legislators, and political parties, and for election-specific covariates. More broadly, legislators who were directly elected by citizens were approximately 4 times more responsive than indirectly elected legislators.

The sequence of events I use provide near-perfect conditions for identifying the effect of interest, which is often a challenge outside experimental settings. The transparency increase, detailed in the *Information Shock* subsection, occurred when the government responded to a set of external pressures, including from the International Monetary Fund (IMF), to do a better

job of raising tax revenue from its own citizens. In lieu of increasing transparency, the government announced, and subsequently released, income tax payment records of all federal legislators for taxes that had already been filed several months before these events. The timing ensures that legislators did not know about this public release ahead of time; therefore, their initial tax payments were not conditioned by it.

The argument and findings in this article are important in several ways. By focusing on tax payments, the article contributes to the relatively small body of work on income tax in developing countries (e.g., Bodea & LeBas, 2016; A. Q. Khan, Khwaja, & Olken, 2015), which is an important but difficult-to-study topic, and one that has gained even more traction and relevance since the Panama Papers leaks. The findings also relate to the literatures on corruption and tax compliance. Perhaps more importantly, even though evidence is mixed regarding the extent to which voters react to information by punishing or rewarding incumbents electorally, the findings here indicate that politicians nonetheless seem to care about appearing accountable and responsive to their constituents.

The focus on an action that is entirely within a politician's own control distinguishes this article substantively from existing scholarship on political accountability, where politicians' actions usually pertain to development spending, policymaking and other issues that they have only partial control over. In addition, this focus on tax payments indicates that "politician performance" can be interpreted fairly broadly when thinking about accountability: It is not necessary that voters interpret tax payments as a direct signal of politician performance. Rather, all else equal, it is sufficient that there are some voters who would perceive their representative as a better citizen if they paid taxes; in general, good citizens pay taxes, and being seen as a good citizen will matter to politicians at the margin. Finally, by using observational data from the entire country, I mitigate possible concerns regarding generalizability of the findings and treatment scalability.

This finding stemming from Pakistan is also interesting as it is a relatively unstable developing country, and one with low political transparency and accountability, falling in the low electoral integrity category on the *Perceptions of Electoral Integrity Index* (Norris & Grömping, 2017). If anything, it provides difficult conditions for finding significant effects of public information to citizens on legislators' actions. This difficulty is even starker given that these events did not take place in an election year, where presumably the pressure to respond is higher. Thus, not only does this contribute to our understanding of how political accountability functions in nonelection years, the results I find are potentially a lower bound on what may have happened closer to an election.

The article proceeds as follows. The next section discusses information and accountability, and why they might be related. The subsequent section traces the events that led to the unforeseen release of tax information and outlines the data and research design. The subsequent sections present the main results and robustness checks, respectively, and the last section concludes.

Why Does Information Matter?

Among other things, elections are blunt tools for citizens to hold representatives accountable (Downs, 1957; Fiorina, 1981; Manin, Przeworski, & Stokes, 1999; Riker, 1982), but their effectiveness in this regard is often constrained by citizens having limited information about legislator performance (Achen & Bartels, 2016; Canes-Wrone, Brady, & Cogan, 2002; Manin et al., 1999). As a result, the literature on accountability has increasingly focused on whether, when, and how citizens use reliable information about politicians' misconduct or poor performance at election time when they do have access to such information. The findings from this growing strand of research are fairly mixed.

On one hand, some scholars find that voters do care about the quality of candidates and about politician performance, and do learn from such information in a variety of contexts (Arias, Larreguy, Marshall, & Querubin, 2017; Bidwell, Casey, & Glennerster, 2019; Chauchard, Klasnja, & Harish, 2017; Platas & Raffler, 2017). Sometimes, they also take this information in to account when making election time decisions, such as in the case of political corruption being exposed in Brazil (Ferraz & Finan, 2008) and Mexico (Chong, Ana, Karlan, & Wantchekon, 2015; Larreguy, Marshall, & Snyder, 2016), which reduced support for incumbents, or through publicly screened debates in Sierra Leone (Bidwell et al., 2019) and Uganda (Platas & Raffler, 2017), which increased support for candidates who did well.[1] Although most of these studies focus on the developing world, where the general lack of information available to voters can make its provision particularly salient, there is also some relevant evidence from the U.K. parliamentary expenses scandal, for instance, where implicated legislators saw their vote shares being adversely affected in the next election (Eggers & Fisher, 2011), and from experimental work among U.S. voters that finds a negative effect of tax scandals on politicians' reputations and their support among voters (Funk, 1996).

On the other hand, there are also instances where voters receive information about politicians' actions and do not punish or reward at the ballot box. Very recent literature, especially projects emerging from the Evidence in Governance and Politics (EGAP) Metaketa on information and accountability, has begun to address potential explanations for a lack of response. The

suggested mechanisms and experimental findings include voters reacting to information only if it is surprising (Arias, Larreguy, Marshall, & Querubin, 2018) or widely disseminated (Adida, Gottlieb, Kramon, & McClendon, 2016), voters not having benchmarks to judge new information against (Arias et al., 2017) or not being able to estimate information accurately in the real world (Chauchard et al., 2017), and other aspects of performance outweighing information about misconduct when it comes to the actual election (Adida et al., 2016; Chauchard et al., 2017; Vivyan, Wagner, & Tarlov, 2012).

Thus, citizens do care about what their representatives are doing, in terms of performance, quality, conduct, and personal actions—and at least sometimes use this information to punish or reward electorally—but what is not clear so far is how politicians respond to information provision to citizens. This is an integral question when thinking about accountability, and one that is starting to gain some traction in the literature, but also one that we do not really know the answer to as yet.

Ongoing work by Cruz, Keefer, and Labonne (2017) in Philippines finds that politicians increase their vote buying efforts when they know that voters have received negative information about incumbents' spending decisions, while Banerjee, Duflo, Imbert, and Pande (2017) analyze the effects of a voter awareness campaign about the responsibilities of local leaders in Rajasthan, finding that increased information reduces the likelihood of an incumbent rerunning for village council head and also reduces her vote share. Although these ongoing papers are also focused on the effects of information on what politicians do, the subsequent actions being studied are not the same as those that the information was provided on.

Recent work by Grossman and Michelitch (2018) does look at the effect of randomly disseminating information on parliamentarians' performance to citizens and politicians on subsequent performance by the same politicians, and finds that it affects their performance only in competitive constituencies. Humphreys and Weinstein (2012), however, do not find significant changes in politicians' own behavior in a somewhat similar set up; both focus on Uganda. Earlier work by Reinikka and Svensson (2011) in the same country finds that newspaper campaigns that increase information to schools and parents on local government spending from a public education grant reduce grant-capture by the local governments.

I build on this small literature in several ways. This article studies political accountability in an understudied country, and does so by focusing on politicians' tax payments, which is an important and controversial topic in most countries today. Tax payments are also interesting to look at because they are entirely within a politician's own control, unlike measures of policy performance, for instance, where observed outcomes are much harder to fully

attribute to a given politician because only a subset of factors determining them can be directly controlled by the politician. In addition, the analysis here is based on observational data, which is very rare in this strand of the accountability literature because it is more challenging to identify the effects of exogenous information shocks outside of experimental settings. Doing so has distinct advantages, however, because it allays potential concerns regarding generalizability of the results and scalability of the treatment. In this case, for instance, the information shock affected all legislators in the country, as I discuss in more detail below.

Information Shocks and Political Accountability

The empirical expectation in this case is that politicians will respond by increasing their tax compliance. The pressure to do so could be through at least two underlying mechanisms. First, they might fear social sanctioning or increased monitoring by the authorities as tax collectors may also feel greater pressure to punish evasion. This mechanism is similar to the experimental literature finding that increased monitoring lowers corruption (Callen, Gulzar, Hasanain, & Khan, 2013; Olken, 2007).[2] Second, voters care about tax evasion by politicians, which incentivizes greater compliance for electoral reasons. Both mechanisms should lead to an increase in tax compliance following an information release.

Within the second mechanism, which is what this article primarily focuses on, the intuition is based on politicians wanting to retain office, and possibly run for reelection, and on the assumption that voters will have access to the information that is being released and will care about it. Especially in developing countries, the private financial returns from holding office are high, such as in the case of India (Bhavnani, 2012; Chauchard et al., 2017; Fisman & Golden, 2017), and it is reasonable to assume that (most) politicians care about retaining office and about reelection. As discussed above, voters do pay attention to information about their representatives. From the politicians' perspective, when the topic of the information release is one that citizens care about and can easily access, there is a greater risk of electoral penalty.

Electoral pressures vary, however, which will play a part in conditioning politicians' reactions to information shocks. Generally, the more dependent a politician is on her voters to retain office, the greater the incentive to act in ways that constituents will approve of. This rests on a well-established finding in the literature that legislators elected in competitive races tend to work hardest to satisfy their constituents as they need to hold on to their narrow band of support to stay in power, and to win reelection (Berry, Burden, &

Howell, 2010; Keefer & Khemani, 2009; Lee, 2003; Rodden & Wilkinson, 2004; Ward & John, 1999).

Based on this, I expect legislators elected by the smallest margins to respond by improving their behavior more in the subsequent year than legislators elected in less competitive races. This logic should be especially true in a country like Pakistan where there are no term limits and, hence, legislators face low institutional constraints on rerunning. The heterogeneous response based on electoral incentives is independent of the first mechanism that could be driven by increased monitoring, fear of social sanctioning, or a general electoral accountability pressure. As I discuss in more detail in the empirical section, varying levels of electoral pressures do not correlate with these, or other relevant, considerations. More broadly, this mechanism also applies to legislators elected directly by citizens versus those who are elected indirectly, as the direct dependence on citizens is higher in the former case.

Overall, two key empirical expectations arise from this discussion that can be stated in terms of the particular information shock I use for analysis in this article. First, an exogenous increase in reliable information to citizens about their legislators' insufficient tax payments will induce greater subsequent tax payments. Second, legislators elected in competitive races, and directly elected legislators, will exhibit greater increases in their tax payments after the information release compared with those elected in less competitive races and those elected indirectly, respectively.

Data and Research Design

Information Shock

Low income tax compliance has always been a significant problem in Pakistan. A 2016 IMF Special Issues Paper estimated that the tax revenue gap in Pakistan is more than the total tax revenue the government collects. The shortfall in personal income tax collection is even more stark, with fewer than 1 million tax filers in a country of more than 56 million income earners (IMF, 2016). Out of an estimated 7 million people who are eligible to pay income tax, only about 7%, or half a million, actually do (Sherani, 2015). Partly to compensate for this low compliance, more than half the tax revenue in Pakistan is raised through indirect taxation on goods and services instead, disproportionately burdening the average citizen rather than the elites (Sherani, 2015). In addition, there is the common disgruntled notion among citizens that most elites, which includes politicians, do not pay their fair share of taxes and indulge in corrupt fiscal transactions—a view that gathered even more steam in the wake of the Panama Paper leaks. Despite citizens' priors about politicians' tax

Table 1. Sequence of Events.

Event	Date
National elections in Pakistan	May 11, 2013
End of 2012-2013 fiscal year	June 30, 2013
Tax filing deadline (for 2012-2013)	August 31, 2013
Finance Minister announces publishing of 2013 taxes	January 6, 2014
First Parliamentarians' Tax Directory published	February 28, 2014
End of 2013-2014 fiscal year	June 30, 2014
Tax filing deadline (for 2013-2014)	August 31, 2014
Second Parliamentarians' Tax Directory published	April 10, 2015

evasion, however, there had traditionally been no concrete information available on their actual income tax payments.

In late 2013, these issues became increasingly politically salient. The IMF was negotiating a loan with Pakistan, and cracking down on "rampant tax evasion" was one of the main conditions imposed (Houreld, 2013). The British parliament also stated at the time that U.K. taxpayers should not be expected to help provide development aid to Pakistan, "if the Pakistani elite do not pay meaningful amounts of income tax" ("U.K. Legislators Want Aid Linked to Commitment on Taxes," 2013). Partly in response to these pressures, the Finance Minister, Muhammad Ishaq Dar, announced in a Senate speech in early 2014 that a tax directory of all parliamentarians would be published in the following month. He deemed this a move toward greater tax transparency. On February 28, the Federal Board of Revenue (FBR) published the first "Parliamentarians Tax Directory" for the fiscal year ending on June 30, 2013, which listed federal income tax payments for all federal and provincial legislators (FBR, 2014). This list was compiled based on the FBR's records of actual income tax paid rather than politicians' self-reports.

This release of information was both unprecedented and unforeseen, and has two particularly relevant features. First the length of time that elapsed between the end of the 2013 fiscal year and this announcement makes it highly plausible that the release of tax information was unknown to all legislators when they filed their 2013 income tax returns (Table 1 summarizes the timing of relevant events.). Since such information had never been shared publicly before, there was no reason for legislators to expect differently that year. Thus, this "information shock" can be exploited to systematically analyze whether, and how, it affected legislators' tax payments in the subsequent 2013-2014 fiscal year, which was ongoing at the time the report was published; at this point, legislators likely assumed that tax payment information

would be shared again in this next year. That year's tax directory was subsequently released in April 2015.

Second, this information release received a lot of attention in the media and general public. Headlines such as "Directory of Shame" and "FBR publishes list to embarrass tax cheats in to paying up" emerged in national newspapers; another news article stated, "Income tax returns are the most imaginative fiction being written today" (M. Z. Khan, 2014). These articles not only explained where citizens could access the tax lists, but also summarized information on zero- and low tax paying politicians, discussed prominent politicians in particular, and often mentioned the values of their assets ("Directory of Shame," 2014; Reuters, 2014). Thus, not only was information about legislators' tax returns publicly available, they knew citizens were paying attention and had access to reliable information about their (very low) tax payments. The directory itself was a straightforward list, easily accessible through the FBR's website. Although the existence of tax evasion among politicians may not have been shocking to citizens, the extent to which it was a problem even among the country's leaders was something there had never been systematic information on before. In addition, tax evasion by elites is a particularly sensitive issue for the average citizen, especially because the tax burden falls on him through high taxes on every day consumption goods and services. To put this in perspective, note that the GDP per capita in Pakistan was US$1,272 in 2013, whereas the *minimum* taxable income calculated in my data set is US$9,600; it is perhaps unsurprising under such starkly different living conditions that the average citizen interprets low tax payments as stealing from the common man, so to speak.

It is, therefore, important to note that it was not necessary that citizens interpreted politicians' tax payments as a signal of their ability or competence. In other words, low tax compliance does not necessarily imply poor performance as a legislator. Rather, it is sufficient for politicians to want to portray themselves as good citizens if they believe that at least some voters will respond to this cue. And good citizens pay their taxes. In addition to the general belief of elites, including politicians, not paying their fair share of taxes, the last wave of the World Value Survey also corroborates that Pakistani citizens care about this issue. When asked how essential a characteristic of democracy it is that governments tax the rich and subsidize the poor, with 1 being "not an essential characteristic" and 10 being "an essential characteristic," the mean answer was 8.69, with more than 50% respondents picking 10. Similarly, when asked if cheating on one's taxes was ever justifiable, where 1 was "never" and 10 corresponded to "always," the mean answer was 1.82; more than 80% respondents chose 1 or 2 as their answer (Inglehart et al., 2014). Considering these questions in conjunction, even though they don't

explicitly ask how important it is for politicians specifically to pay taxes, it is reasonable to conclude that most respondents would believe that rich people not paying their taxes is not justifiable in the slightest.

Given this overall context, it is certainly plausible that politicians believed citizens would care about this information. In addition, for the hypothesis to hold, it is not necessary for citizens to actually care deeply about these tax payments; rather, politicians merely need to believe that citizens, on average, will take note and care. Nonetheless, even when considering some of the reasons highlighted in the literature for information shocks not causing voters to respond, this case is one where many conditions for information affecting voters were met. For instance, some scholars find that voters react to information only if it is widely disseminated (Adida et al., 2016), if they are able to estimate the information accurately in the real world (Chauchard et al., 2017), or if the information is surprising (Arias et al., 2018). In this case, the information was readily available through the Internet and news, was released for *all* legislators not just some, was credible because it came from the FBR, was easy to understand, and while low tax compliance by politicians was not necessarily surprising overall, exactly how low it was for individual politicians was certainly unprecedented information. Given these circumstances, I expect the information to have affected politicians' subsequent tax payments.

There may be concerns about the exogeneity of the information release as there is no explicit proof that no legislator knew about the decision beforehand. Based on newspaper reports and summaries of senate proceedings, however, there appears no reason to believe that even the Finance Minister himself knew of this decision well ahead of time. Although full transcripts of senate sessions are not publicly available, the *Free and Fair Election Network* (FAFEN) electronically publishes a *Daily Factsheet* that summarizes proceedings of each senate sitting. The Finance Minister made the announcement about releasing tax returns in the second sitting of the 100th senate session, which is mentioned in the relevant Factsheet (FAFEN, 2014). However, there is no mention of such an information release in the sessions leading up to this particular one, making it highly unlikely that the decision was premeditated well in advance.

Even so, if the Minister could still have somehow shared this decision before 2013 taxes were paid, presumably he would have done so with his copartisans from Pakistan Muslim League–Nawaz (PML-N), which controlled the federal government. If he did somehow warn them, they would have increased their tax compliance in 2013. First, that biases against finding a significant increase in 2014 tax payments, since PML-N legislators controlled more than half the seats in the Lower House. Second, if they did react,

their average tax payments should have been higher than others in 2013. However, ruling party legislators, if anything, had slightly lower tax compliance than others that year. Thus, I am not concerned about ruling party legislators knowing about the information shock beforehand.

Research Design

The transparency shock in conjunction with the expectation that legislators will respond differently depending on electoral pressures is well suited to a difference-in-differences research design. The treatment here is the exogenous change in information, and I am primarily interested in the heterogeneous reaction of the most competitively elected legislators, and secondarily in its average effect on everyone. This empirical strategy allows me to analyze the groups of interest while controlling for initial variation in their tax compliance, which helps to separate the *response* of the competitively elected group.

The parallel trends assumption requires that, in the absence of a shock, the initial differences between both groups would have been maintained. Here, that translates to assuming that, without the information release, all types of legislators would have maintained their earlier tax compliance trends; any initial differences in tax payment proportions between both groups would have persisted. Unfortunately, given the sensitivity of the data, obtaining legislators' tax payments for years before 2013 proved impossible, meaning that the assumption cannot be directly tested. However, the data section presents difference-in-means tests for other factors that could be related to both a legislator's tax compliance and his dependence on voters, such as previous legislative experience, age, education, and so on, to show similarity on other relevant dimensions. I also take in to account differences between geographical regions, political parties, and individual legislators in the empirical section.

Federal Income Taxes in Pakistan

Pakistan is a parliamentary democracy with a bicameral federal legislature comprising the National Assembly (Lower House) and the Senate (Upper House). The Assembly has 342 members, 272 of which are directly elected (at least) every 5 years in single-member districts with plurality electoral rules. The remaining 70 Members of the National Assembly (MNAs) and all 104 Senators are indirectly elected, and are discussed towards the end of the article where I generalize the electoral incentives argument. The main results focus on the 272 directly elected MNAs as these are the only federal politicians who come to office solely based on citizens' votes, making it

meaningful to talk about electoral pressures using their vote margins.[3] Thus, the unit of analysis is an electoral constituency/legislator_year, with data from 2013 and 2014.[4]

To analyze the effect of the information shock on subsequent tax payments, we ideally need to know not just the amount of federal income tax each legislator paid in either year but also how much he owed. In Pakistan, this tax is levied exclusively on all sources of income rather than directly on assets. Thus, it includes an individual's salary and any income earned from renting out a property, returns on investments, yields on government bonds, and so on. The total income generated from all such sources is taxed progressively, with the minimum annual taxable income being PKR400,000 in the relevant years (approximately US$4,000 in 2016). As this total income is not listed for each legislator, I approximate it using legislator salary and relevant information on each legislator's asset ownership for both years.[5] Details on asset statements and how taxable income is calculated from these are in Supplemental Appendix A.

Based on the estimated taxable income for each individual, I calculate the amount of tax owed using the federal income tax rates (summarized in Table A2 of Supplemental Appendix A). Data on the actual tax payments come from the FBR, so they are not self-reported amounts.[6] Using tax paid as the numerator and tax owed as the denominator, I calculate *Tax Proportion Paid*, which is one of my two main dependent variables. I use *Actual Tax Paid* as the other dependent variable in the main specifications, which is simply the numerator from the first dependent variable.

Using both as dependent variables strengthens the empirical approach as each has different advantages. *Actual Tax Paid* comes from the FBR and, hence, does not rely on self-reporting of any sort. In addition, perhaps less sophisticated voters only pay attention to the amount of tax they see their representatives paying. However, to know more precisely if a legislator's tax payment is high or low requires considering the amount owed, which *Tax Proportion Paid* is able to do. Thus, using both in conjunction provides a clearer overall picture. Summary statistics for these two and other variables are in Table A3 in Supplemental Appendix B. I log both dependent variables, primarily because the distributions of the raw variables are very right-skewed as there is a very small number of legislators paying high proportions or amounts of taxes; logging the variables resolves this problem to a great extent (see Figures A1 and A2 in Supplemental Appendix B for the unlogged and logged comparisons).[7]

A possible concern with using legislators' self-reported statements to estimate taxable income for the first dependent variable is asset underreporting. Although asset declaration reliability cannot be directly tested, if there is

systematic underreporting it will be a problem for the research design in two instances. First, if legislators systematically underreport assets in 2014, my tax owed calculation will be artificially low for that year. Consequently, what appears to be higher tax compliance in 2014 is merely a lower "denominator" for *Tax Proportion Paid*. Second, if only competitively elected legislators underreport their assets, especially in 2014, then a disproportionate increase in their tax compliance post-information shock is driven by a lower tax owed calculation rather than genuinely higher tax payments.

I conduct multiple tests to ensure this is not a problem. First, I find that a dummy variable for 2014 is not a significant predictor of asset ownership or taxable income, implying that asset declarations in 2014 are *not* significantly lower than 2013 (see Table A5 in Supplemental Appendix B). Second, competition—defined in the next subsection—does not predict asset change between 2013 and 2014 (Table A6, Supplemental Appendix B). In addition, the groups of competitively elected and other legislators are similar on various important dimensions, including their tax owed and asset declarations (see Table 2). Finally, as mentioned above, I use a second dependent variable, which is independent of asset declarations, to allay such concerns; both variables yield substantively and statistically similar results. The penultimate section of the article presents further robustness checks on the dependent variables.

The median *Tax Proportion Paid* is a low 0.056, as Table A3 (Supplemental Appendix B) indicates. The maximum value of this variable is artificially high because taxable income is approximated based on available asset information; however, fewer than 10% observations are higher than 1. (Details on this calculation, as well as alternative ones, can be found in Supplemental Appendix A.) The maximum value is not a concern, though, because if someone is seemingly "overpaying" their taxes in my data, that must be because I underestimate tax owed rather than overestimate it. Thus, this conservative approach, if anything, biases against finding results. Furthermore, the results presented in the next section are robust to limiting *Tax Proportion Paid* to 1, and to using raw amounts of tax paid as the dependent variable. Almost 20% observations (68 of 362) have PKR0 being paid in income tax; 61 of these cases occur in 2013, with only seven "total evaders" in 2014.[8]

Competitive Elections

The main independent variable, *Competitive*, is coded 1 for legislators who won by a 5% or smaller electoral margin in the 2013 election. Note that the election took place in May, so both instances of tax filing occur afterward. That also means that both tax directories refer to payments by the same legislators,

Table 2. Difference-in-Means Tests.

Variable	Competitive *Mean*	Uncompetitive *Mean*	*p* value of difference	*N*
Age	52.50	53.29	.679	362
High School	0.982	0.957	.245	362
College	0.804	0.803	.987	362
Masters	0.143	0.174	.546	362
Turnout	**0.503**	**0.547**	**.054**	362
(Log) Population Density	**0.705**	**1.839**	**.000**	362
# Candidates	16.88	16.56	.772	362
Previous MNA	0.375	0.484	.131	362
# Previous MNA Terms	**0.643**	**0.915**	**.075**	362
# Previous MNA Years	3.018	3.873	.191	362
Tax owed (PKR Million)	2.490	3.198	.355	362
Residential Property. (PKR Million)	13.25	17.95	.210	362

Bold values indicate that the difference-in-means is significant at the 90% level. MNA = Members of National Assembly; PKR = Pakistani Rupees.

making the overtime comparison even more meaningful. Robustness checks, presented in subsequent sections, include using a range of thresholds for defining competitive races. With the 5% threshold, just over 15% of the races qualify as close. The remaining variables in the descriptive statistics (Table A3, Supplemental Appendix B) primarily refer to the 2013 general election and to characteristics of the legislators themselves, which are also used as covariates in various specifications.

Before the analysis, it is relevant to ensure that the two groups of legislators whose reactions to the information shock I am interested in comparing—competitively elected versus others—are similar on other dimensions that could be relevant for tax compliance. For instance, perhaps educational attainment is positively correlated with tax payment. Or, urbanized constituencies, through better access to media and information, elicit more accountability from their representatives. It could also be the case that as legislators gain more political experience, they learn "better" ways to evade taxes without being caught. And so on. Such factors could confound the empirical analysis if they are correlated both with tax payment propensities and with either group of legislators. Table 2 summarizes difference-in-means t-tests for a host of such variables, indicating that the two groups are comparable on relevant observables.

The main exception is *Population Density*, which I use as a proxy for urbanization at the constituency level. This is constructed as the average number of people within each 0.01 km^2 in the constituency. However, the imbalance points in the opposite direction; this difference would be problematic if competition was correlated with higher urbanization, making it difficult to disentangle my proposed mechanism from a pure information story where citizens having greater access to information induces a change in legislators' tax payments rather than electoral pressures also being relevant. Here, though, less competitive constituencies are more urbanized, which runs against the hypothesis and, thus, presents less of a concern. I return to the question of information access, particularly in terms of its possible correlation with competition, in the next section.

The other two exceptions, albeit marginal, are *Turnout* and # *Previous MNA Terms*, both of which are significantly different with a 90% confidence interval. In the case of *Turnout*, not only is the substantive difference between both groups very small, less competitive districts, perhaps counterintuitively, have a slightly higher average turnout rate. Based on the conventional wisdom that higher turnout induces more responsiveness, one would expect a greater response to the information shock in the less competitive districts, which again runs counter to the proposed argument. Previous experience, when measured as the number of past terms an MNA has served, is significantly associated with lower competition. By itself, this does not present serious concerns because the other two measures of past experience—*Previous MNA* and # *Previous MNA Years*—are not different in any meaningful way, and all three variables aim to measure the same underlying concept.[9] Furthermore, the results in the next section also use variables from Table 2 as controls to ensure that the main findings still hold. Table A4 in Supplemental Appendix B presents difference-in-means tests for various specific assets owned by both groups of legislators; although the research design does not require balance on such dimensions, it is nonetheless interesting to see that the two groups are also comparable in this regard.

Results and Implications

The main specification used is,

$$\begin{aligned} Y_{it} = {} & \beta_0 + \beta_1 Yr2014_t + \beta_2 \text{Competitive}_i \\ & + \beta_3 2014 \times \text{Competitive}_{it} + f(X_i) + g(Z_j) + \epsilon_{it}, \end{aligned} \tag{1}$$

Where, Y_{it} is the logged tax proportion/amount paid by legislator i in time t (such that t is either 2013 or 2014), $Yr2014_t$ is a dummy variable that

Table 3. Competitive Elections and Taxes.

	(Log) Tax proportion paid		(Log) Actual tax paid	
Competitive × 2014	2.45*	2.22**	2.19*	2.20**
	(1.27)	(1.12)	(1.20)	(1.08)
Yr2014	2.93***	3.12***	3.06***	3.17***
	(0.48)	(0.41)	(0.45)	(0.40)
Competitive	−1.01		−0.80	
	(1.07)		(1.01)	
District FE	✓		✓	
Legislator FE		✓		✓
N	362	362	362	362
Adjusted R^2	.59	.72	.85	.89

This table presents models that measure the effect of being a competitively elected legislator on the proportion of tax paid (Columns 1 and 2) and on the actual tax paid (Columns 3 and 4) in the post-information shock year. The first and third models include administrative district fixed effects, and the second and fourth include legislator fixed effects (same as electoral district/constituency here). FE = fixed effects.
*$p < .1$. **$p < .05$. ***$p < .01$.

accounts for the time trend, Competitive_i indicates whether legislator i won in a competitive race, X_i is the set of other covariates associated with legislator i, and Z_j refers to the fixed effect for each specification, where j denotes administrative district or individual legislator, depending on the particular model.[10]

Electoral Competition and Tax Compliance

First, I consider whether there is a difference in how competitively elected legislators reacted to the information shock compared with legislators who won more comfortably. As the primary focus is on their *response*, β_3 from Equation 1 is the main quantity of interest, which estimates the interaction effect of being in the "post-information shock state of the world" and being a "competitively elected" legislator. Table 3 summarizes results from the four main specifications, all of which provide support for the main hypotheses.

The positive, significant interaction coefficient across all specifications implies that, on average, competitively elected legislators showed a bigger increase in their 2014 tax payments than others, both when measuring tax payments as a proportion of what they owed (first two columns) and when looking at just the actual income tax amounts paid (Columns 3 and 4). Specifically, the coefficient of 2.45 from the first model indicates approximately a 12 times higher tax payment in 2014 compared with 2013 for

competitively elected legislators.[11] Exactly how much more it is depends on what proportion of federal income tax the legislator paid in 2013. To put it in context, a legislator who paid the mean (logged) tax proportion of 0.013 in 2013 will now pay 0.15 (or 15%) of what he owes in 2014.[12] A legislator who instead paid the median tax proportion (0.056) will now pay 0.62 (or 62%) of what he owes, which is likely a much larger increase in absolute terms. Although these effects are substantively very large, they are plausible given how low average tax payments were in 2013.

The exact numbers from these two models should be interpreted with some caution, though, as the exact amount of taxable income is an approximation based on asset ownership. Despite the approximation, however, the results are illustrative of a significant and discernible pattern, which holds up to more conservative calculations of taxable income. Results using those calculations are in Tables A12 and A13 in Supplemental Appendix B. The substantive importance of the findings is also bolstered by the models using the second dependent variable. The interaction coefficient of 2.19 in the third model corresponds to an almost 9-time increase in tax paid, meaning that a hypothetical legislator who was paying the median amount of *Actual Tax Paid* initially (PKR31,382) is estimated to be paying about PKR282,440 in 2014 based on the interaction coefficient.

Although I am primarily interested in whether different groups of legislators responded heterogeneously to the information shock, it is noticeable that the coefficient on *Yr2014* is also positive and significant, indicating a big average increase in all legislators' tax payments after the information shock. Presumably, this change is also due to the public release of tax records and expecting higher tax enforcement, especially because the result persists when controlling for different types of fixed effects and other political factors. The baseline coefficient on *Competitive* is negative but insignificant in these—and most other—specifications, indicating that these legislators maybe paid a lower tax proportion in 2013 but the differences are not necessarily meaningful.[13] From the raw data, it is also not the case that competitively elected legislators responded to the information release "more" simply because other legislators already paid all of their taxes; the median tax payment for the rest of the legislators, though higher in 2013, was still a very low 0.063 (or 6.3%).

The first and third models take in to account differences between administrative districts, whereas the second and fourth are even more restrictive, controlling for unobservable differences between individual legislators.[14] As the election I use was held before taxes were filed for either year, using legislator or constituency fixed effects is equivalent.[15] A legislator's propensity to pay taxes might vary based on many factors such as age, education, gender,

political experience, personal wealth, family's political experience, previous employment, and so on, as well as intangible qualities such as an inherent comfort with evading taxes. Although some of these can be explicitly controlled for—as I do below—others do not have data available or cannot be easily measured, which is where the legislator fixed effects are especially effective. The interaction coefficients are, unsurprisingly, smaller but still indicate approximately a 9-time increase in both average tax proportion paid and average tax paid by competitively elected legislators in 2014.

Further Results and Discussion

In Table 4, I also take in to account various constituency-level factors for both dependent variables, including the turnout and number of candidates for each race, and the constituency's population density (as a proxy for urbanization). These models also control for legislator-specific characteristics, both in terms of previous federal legislative experience and educational attainment. The variables represent a variety of alternative explanations for the findings but, as the table shows, the main coefficient of interest is comparable in both specifications.[16] In particular, controlling for population density/urbanization takes in to account alternative explanations based on fixed bureaucratic or sanctioning capacity, or based on access to information. That is, bureaucratic presence tends to be higher in urban areas and is likely positively correlated with a greater capacity to enforce tax collection laws. However, controlling for it does not affect the results.

A potential concern is whether bureaucratic capacity or information access are highly correlated with electoral competition. That is, if electorally competitive areas have better revenue collection bodies or better access to information then it becomes difficult to disentangle the effect of competition on tax compliance from these other potentially relevant variables. However, I address these concerns with several additional empirical tests, with results presented in Supplemental Appendix B.

First, it is unlikely that bureaucratic capacity affected tax enforcement in general, given that there were no high profile tax evasion arrests made in 2014. Furthermore, the extent to which voters actually had access to information about tax payments is irrelevant to some degree, because what matters is politicians' beliefs that voters might care. Nonetheless, two measures help to alleviate such concerns more systematically, based on each constituency's population density and its distance from the provincial capital.[17] Both measures are reasonable proxies for bureaucratic capacity; urban centers—which are densely populated—tend to attract the best bureaucrats and are the most lucrative postings. Similarly, the closer a constituency is to a very large urban

Table 4. Other Political Factors and Taxes.

	(Log) Tax proportion paid	(Log) Actual tax paid
Competitive × 2014	2.27*	1.94*
	(1.21)	(1.17)
Yr2014	3.03***	3.20***
	(0.46)	(0.45)
Competitive	−1.36	−1.14
	(1.05)	(1.02)
Turnout	−4.31	−1.64
	(4.74)	(4.58)
Number of candidates	0.10*	0.06
	(0.05)	(0.05)
(Log) population density	0.84***	0.50*
	(0.29)	(0.28)
# previous years	0.24	0.08
	(0.19)	(0.18)
# previous terms	−0.86	−0.15
	(0.75)	(0.73)
High school	3.11*	2.72*
	(1.59)	(1.53)
College	1.68*	1.16
	(0.95)	(0.92)
Masters	−1.00	−0.16
	(0.78)	(0.76)
PhD	−5.86	−10.79**
	(4.33)	(4.18)
Age	0.03	0.02
	(0.02)	(0.02)
Female	3.48*	1.87
	(1.77)	(1.71)
District FE	✓	✓
N	350	350
Adjusted R^2	.64	.86

This table presents models that measure the effect of being a competitively elected legislator on the (logged) proportion of tax paid (Column 1) and on the (logged) actual tax paid (Column 2) in the post-information shock year, controlling for other electoral characteristics, legislative experience, education, and gender. Note that there are 12 legislators for whom either the *Age* or *Education* information was missing, which is why those observations are necessarily dropped from both models. FE = fixed effects.
*$p < .1$. **$p < .05$. ***$p < .01$.

center, the higher the bureaucratic capacity *and* the higher the access to information. As provincial capitals are the largest urban centers within a region, I calculate the distance from each constituency to the relevant capital. Distance will be inversely correlated with capacity and information access.[18]

Table A7 in Supplemental Appendix B summarizes results from specifications where I interact *2014* × *Competitive* with a dummy variable indicating high population density and low distance from the provincial capital, respectively. The lack of significance on the triple interaction indicates that, within competitive legislators, those in constituencies with better access to information or higher capacity bureaucracies did *not* respond differently from others. Relevant difference-in-means tests (Table A9) also indicate no significant difference in tax compliance or tax payment between competitively elected legislators in high versus low density constituencies, both overall and in 2014; the same is true for high and low distance constituencies. Therefore, it is not the case that legislators in competitive constituencies responded to the information shock simply because of a better-functioning bureaucracy or because their constituents had higher access to this information.[19]

Finally, it is relevant to note that the main findings are not driven by a particular geographical region or political party. Given that Pakistan is a politically unstable developing country, personalistic politics tend to be a significant feature of the political landscape and the importance of the individual politician versus the political party varies across the country to some extent. For instance, most of the Independent MNAs—that is, those unaffiliated with any political party—are associated with the northern tribal areas. In other words, political parties are particularly weak in that part of the country, and relationships between constituents and their representatives perhaps more important. That could imply that competitively elected legislators respond to the information shock only in areas where parties are relatively weak. Conversely, if parties punish co-partisans who do not pay taxes as it hurts their brand, we might expect competitively elected legislators to respond more in areas where parties are stronger. Again, party strength being correlated with electoral competition could complicate what mechanism is at play here.

Table A8 in Supplemental Appendix B summarizes results where I interact three different measures of party strength with the *Competitive* × *Yr2014* interaction term; the measures are explained in the tables.[20] Again, the lack of significance on the triple interaction terms suggests that it is *not* the case that competitively elected legislators in constituencies with high party strength react differently to the information shock than those in areas of low party strength. The same is indicated in the difference-in-means tests summarized in Table A9 in the same section, which shows that tax compliance

and payments among competitively elected legislators do not vary meaningfully based on party strength, either overall or post-information shock.[21]

Robustness Checks

The results, thus far, support the hypothesis that high electoral competition incentivized a greater response to the information shock. This section further substantiates this interpretation, using alternative measures for tax payments and competition, and generalizing the proposed mechanism by comparing directly and indirectly elected legislators, where the former group faces higher electoral pressures than the latter.

Tax Payments

I ensure in three ways that the findings are not driven by how the dependent variables are measured. First, I calculate tax owed based *only* on legislator salary, which is a tax threshold we are certain every legislator reaches. Here, I assume that every legislator only earns his basic salary, and has no taxable income beyond that, which puts each individual in the lowest tax bracket (owing PKR640; approximately US$6.5 in 2016). Using this, I construct a dummy dependent variable, coded 1 if the legislator paid at least this minimum amount. Despite this unrealistically low threshold, the main result holds. Second, I use the tax-to-asset ratio as the dependent variable, which insulates the measure from any estimation choices I make about asset returns. These two sets of results are in Table A11, whereas the next ones are in Tables A12 and A13. As before, the main findings hold.

Competition Threshold

Figure 1 summarizes the interaction effect for a range of thresholds for *Competitive*, ranging from 0.02—that is, a 2% margin of victory being the threshold for a race being labeled "competitive"—to 0.4.[22] As the figure indicates, the coefficient is positive and significant throughout almost the entire range. The coefficient size does vary a little through the range because, when the competition threshold is varied, it affects both the number of races that count as competitive *as well as* those that count as noncompetitive. In other words, rather than simply increasing observations, the number of observations in both groups being compared changes for each competition threshold, leading to the coefficient moving around a little more. Substantively, however, the smallest coefficient still corresponds to a 2.5 times increase in tax payment, with most of the coefficient sizes being similar to those in the main

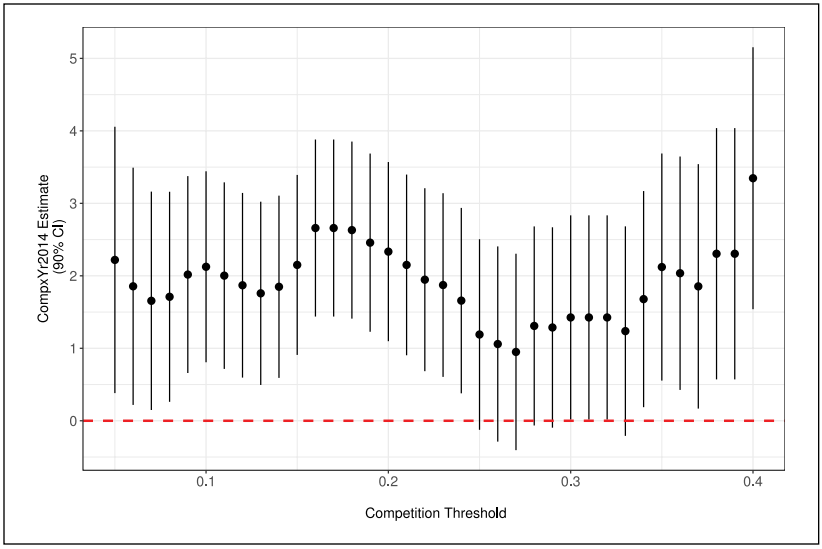

Figure 1. Effect of Competition and Information Shock on Tax Compliance.
This figure plots the coefficient (and 90% CI) from the interaction term *Competitive x Yr2014* for a range of regressions that vary in the threshold used to define a competitive race. For instance, the minimum value of 0.02 on the x-axis means that races decided by a 2% margin of victory (or lower) count as competitive for that regression, and so on. CI = confidence interval.

results. Table A14 in Supplemental Appendix B presents balance tests for these extended results, showing similar difference-in-means tests to the main specification. That is, with the exception of *Population Density*, none of the other variables are consistently different. As discussed in earlier sections, *Population Density* is, in fact, unbalanced in the "wrong" direction.

Furthermore, Table A15 in Supplemental Appendix B replicates the main results using two alternative measures of competition. The first uses the median margin of victory (16.1%) as the threshold for a competitive race—with similar results as before—whereas the second measure uses the continuous *Victory Margin* variable itself. For the latter, as an increasing margin of victory depicts *lower* competition, a consistent result would yield a negative interaction coefficient between the *Yr2014* dummy and this continuous measure, which is precisely what I find in all four relevant specifications.

Directly Elected Legislators

The proposed intuition through this article has been that the more dependent legislators are on constituents' support, the greater the incentive to react to an

information shock, in this case by increasing their subsequent tax payments. In this section, I operationalize the dependence in a broader way, comparing legislators who are elected directly by citizens versus those who are elected indirectly.

The former group comprises the 272 directly elected MNAs used for analysis so far. There are a total of 342 seats in the National Assembly (Lower House), with 70 reserved for women (60) and religious minorities (10). These are awarded to political parties on a proportional basis after the general election results for the directly contested seats have come out. Parties then assign female and minority party members to their share of the reserved seats accordingly. These 70 MNAs do *not* vie directly for citizens' votes and are not associated with geographical constituencies. There are also 104 Senators in the Upper House, each elected for 6-year terms.[23] These senators are all elected by the Provincial Assemblies rather than by citizens.[24]

Therefore, for both the reserved legislators and the Senators, there is an additional layer of insulation from being directly accountable to the average citizen, especially compared with the 272 directly elected MNAs. I code a dummy, *Directly Elected*, which is 1 for the directly elected legislators, and 0 otherwise. It should be noted that the these two groups, though well suited for my argument, may not be similar in other respects. While I have no reason to believe that the two groups would have changed their tax compliance in different ways between 2013 and 2014 in the absence of an information shock, I do not have sufficient data on other covariates—such as age, education levels, previous experience, and so on—to show conclusively that they are similar on other dimensions. Thus, the results presented in Table 5 should be regarded as suggestive evidence.

The models in Table 5 have the same specifications as the main results presented earlier, with different fixed effects since indirectly elected legislators are not associated with administrative districts. Results across all four models support the hypothesis that directly elected legislators, compared with indirectly elected ones, showed a greater change in their tax payments following the information shock. Even with the restrictive legislator fixed effects in the second and fourth columns, the interaction term remains positive and significant. The interaction coefficient of 1.33 from the second model indicates an almost 4 times greater increase in tax payment proportion among directly elected legislators, on average, compared with everyone else.[25]

Conclusion

This article has focused on an important yet understudied piece of the political accountability puzzle, showing that information shocks about legislator

Table 5. Direct Elections and Taxes.

	(Log) Tax proportion paid		(Log) Actual tax paid	
Direct × 2014	1.42**	1.33**	2.14***	1.35**
	(0.72)	(0.63)	(0.71)	(0.60)
Yr2014	1.76***	2.10***	1.35**	2.18***
	(0.58)	(0.51)	(0.57)	(0.48)
Directly elected	−2.65***		−2.23***	
	(0.48)		(0.47)	
Province FE	✓		✓	
Legislator FE		✓		✓
N	551	579	551	579
Adjusted R^2	.54	.69	.85	.91

This table presents models that measure the effect of being a directly elected legislator on the proportion of tax paid (Columns 1 and 2) and on the amount of tax paid (Columns 3 and 4) in the post-information shock year. The main quantity of interest is the interaction effect of being in a post-information shock world (Yr2014) and being a directly elected legislator (as opposed to a reserved seat legislator or senator). The first and third models include provincial fixed effects, whereas the second and fourth use legislator fixed effects. FE = fixed effects.
*$p < .1$. **$p < .05$. ***$p < .01$.

behavior can have "positive" effects on their subsequent actions. Using new data on asset ownership and tax payments by federal legislators in Pakistan, in conjunction with an unforeseen release of information by the Finance Ministry about their income tax payments, I find robust evidence that legislators' tax compliance increased in the following year, especially among competitively elected legislators, who I argue face higher electoral incentives to change their behavior in response to the information shock. The results are robust to controlling for other factors and to alternative measurements of the two core variables. I also provide suggestive evidence that the mechanism generalizes to other levels of analysis where one group faces greater pressure to be accountable to citizens than another—directly elected legislators responded to the information shock more sharply than indirectly elected legislators.

A skeptical interpretation of the results could argue that tax compliance did not increase to a 100% and, in fact, even the number of legislators paying absolutely *no* tax did not drop to zero. However, given how widespread tax evasion is in Pakistan, both within and outside the political sphere, it is remarkable that a seemingly small change had a systematic impact on politicians. The findings are perhaps even more meaningful given that the information was released in a nonelection year when electoral pressures are relatively low.

These findings speak to both the political accountability and electoral incentives literatures and, more broadly, relate to scholarship on corruption and tax compliance as well. In particular, these findings add to existing scholarship on information and voters' responses at the ballot box by illustrating that it does appear that politicians are cognizant of voters and care about electoral accountability, even in a country where accountability is generally quite low. More broadly, the findings are also informative for policymakers interested in the effectiveness of increased transparency in reducing tax evasion in political environments where corruption is pervasive.

Finally, these findings lead to important and interesting questions for future research. For instance, what was the overall effect of this information shock on politicians? Was it possible that those who did not increase tax compliance instead tried to "perform" better in other dimensions, or that those who *did* increase their tax payments tried to accumulate resources from office in other ways? Similarly, if legislators did respond in terms of tax payments, does that imply that in equilibrium voters would be unlikely to punish them because the threat of electoral punishment induced responsiveness? Addressing these issues is beyond the scope of this article but the findings certainly leave open important questions for future research.

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Supplemental material

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Notes

1. See Pande (2011) and Ashworth (2012) for detailed reviews of the relevant literature.
2. A tangentially similar mechanism could be through explicit incentives being introduced for tax authorities to increase revenue collection. Although that was not the case here, recent experimental work on provincial tax collection in Pakistan finds that incentivizing higher tax collection through performance payments for tax collectors did increase revenue but also increased the bribes they were paid by citizens (A. Q. Khan, Khwaja, & Olken, 2015).
3. "Solely based on citizens" votes' refers to candidates who run on a party ticket or independently. Parties in Pakistan do not hold primaries, and who runs on the ticket is decided by party elites. However, independent candidates are also very common and quite successful; the 2013-2018 legislature had 18 independent Members of the National Assembly (MNAs) out of 272.
4. As the focus of this article is how legislators respond to information shocks, I restrict my analysis to their tax payments in the year immediately following the information release, rather than a longer post-treatment panel. How sustained the reaction was, is an interesting but distinct issue that is beyond the current analysis.
5. Legislators are legally required to declare all their personally owned assets every year, and these reports were available through the Election Commission of Pakistan until April 2016, when they were permanently taken down under controversial circumstances (I. A. Khan, 2016). I am grateful to Muddassir Rizvi, CEO at Free and Fair Election Network (FAFEN), for directing me to their archives of all relevant legislators' asset statements from 2013 to 2015. These were accessed in May 2016 through, http://openparliament.pk/. Information on legislator salaries is based on DAWN's newspaper reporting, http://www.dawn.com/news/1,259,375. (Accessed in July 2016.)
6. As of October 2019, the two tax directories can be accessed through the FBR's website: https://www.fbr.gov.pk/Categ/income-tax-directory/742
7. In addition, given the raw distribution and scale of both variables, it is unsurprising that the results are not robust to using the unlogged versions. That is, even a few (outlier) legislators decreasing their payments from 2013 to 2014, for instance, would be more than offset the average (smaller) increase among a much larger group of legislators. In addition, various tests of model fit—including looking at the Adjusted R^2, the Akaike information criterion (AIC) and Bayesian

information criterion (BIC)—indicate that the logged models better explain the data. Due to many zeros in tax payments, the exact calculation of the dependent variables is as follows: $(Log)TaxProportionPaid = log\left(\frac{TaxPaid+1}{TaxOwed+1}\right)$ and $(Log)TaxPaid = log(TaxPaid+1)$.

8. To guard against extreme outliers in the dependent variable driving the main results, all the data summarized and used in the main paper exclude five observations where the *Tax Proportion Paid* is greater than 15. These unnaturally high values of the dependent variable are the inevitable result of approximating income earned on a given set of assets, which necessitates making the same assumptions about returns on assets for all individuals. Consequently, there is bound to be some discrepancy. However, as long as there is no correlation between this variation and the treatment group, which the balance tests indicate there is not, the main results of interest will not be affected. In addition, running robustness checks without excluding the outliers strengthens the results.
9. Note that # *Previous MNA Years* is not a linear function of # *Previous MNA Terms* as Pakistan has faced a lot of electoral instability, such that administrations have not always served the same number of years.
10. Due to data availability, the total number of observations is 362 rather than $272 \times 2 = 544$, as Table A3 in Appendix B also indicates. There are various reasons for the dropped observations: 40 did not file taxes, five are extreme outliers (discussed earlier), 10 did not report asset ownership, almost 100 have incomplete property ownership information due to which their "tax owed" cannot be calculated, and the remaining have incomplete information on other types of asset ownership, with the same outcome that their tax owed cannot be estimated. Incomplete information in this case means that the report clearly indicates that the legislator *does* own that type of asset but an associated value is not clearly provided. As a result, the *N* in the main results is 362. However, for those who do not file, the difference-in-means between Competitive and Noncompetitive legislators is insignificant, indicating that this is not correlated with electoral competition.
11. That is, exp(2.45) = 11.6
12. The mean from the logged variable is: exp(−4.334) = 0.013.
13. Looking at the raw data for context, the 2013 tax paid is, indeed, lower for competitively elected legislators (mean of 5.77 vs. 7.67 for *[Log] Actual Tax Paid*), and this difference is only marginally significant. Although explaining the lower average payment among competitively elected legislators is not the focus of this article, it could simply reflect the fact that competitive legislators also seemed to owe lower amounts of taxes overall (Table 2), though that difference is not significant.
14. Administrative districts in Pakistan are larger than electoral districts, and are the second tier of administrative units in the country after the provinces. As of 2016, there are 149 administrative districts in the country.

15. The two sets are not equivalent only if a legislator is changed during an administration, which does sometimes happen in Pakistan. However, given data missingness, the two are the same in my final data set.
16. As the control variables are at the legislator/constituency level, these specifications cannot have legislator-fixed effects and, hence, I only use district fixed effects here.
17. The four provincial capitals are as follows: Lahore (Punjab), Karachi (Sindh), Peshawar (Khyber Pakhtunkhwa), and Quetta (Balochistan). For constituencies in Federally Administered Tribal Areas (FATA), I use Islamabad as a substitute for their "provincial capital."
18. To calculate this distance, I use the longitude and latitude coordinates of each constituency's polygon centroid, as well as the average coordinates for the provincial capital. The latter is calculated because all provincial capitals contain multiple constituencies. I calculate the distance between these two points using the Haversine distance formula.
19. A somewhat related potential explanation is that competitively elected legislators respond more to the information shock because well-established legislators know they can control bureaucratic appointments and, therefore, they know tax officials will not interfere with their low compliance. Conversely, competitively elected legislators are weaker in the face of bureaucrats. However, that seems unlikely in this case because the measures discussed here are inversely correlated with competitiveness. That is, competitively elected legislators are likely to be in areas with *weaker* bureaucracies in this case, making it unlikely that they increase their tax compliance simply because they are less able to exert influence over tax officials.
20. I use dichotomous measures for party strength rather than triple interactions for each individual party given the high number of political parties in Pakistan.
21. It is also substantively interesting to look at the main results with party-fixed effects, which I do in Table A10 in Appendix B; the results are substantively similar to the main ones but marginally significant. The marginal significance is not surprising as many political parties hold only a handful of seats in the legislature so not all of them will register meaningful increases and/or be elected in competitive races. It is reassuring, however, that the size and direction of the coefficient are the same as the main results, and the p values are all close to 0.1.
22. The dependent variable here is *(Log) Tax Proportion Paid*; using *(Log) Actual Tax Paid* yields similar results.
23. Each of the four provinces has 23 senators, with eight senators representing the FATA, and four representing the Federal Capital (Islamabad). There is reserved representation for females within the 23 senators as well. For full electoral details on the Senate, see http://www.senate.gov.pk/en/essence.php?id=24&catid=4&subcatid=138&cattitle=About%20the%20Senate.
24. Pakistan is a federal political system, with a national legislature and four provincial legislatures.

25. The number of observations is lower for the models with province fixed effects because the 10 religious minority MNAs are not associated with specific geographical regions; similarly, this information was missing for 18 other female MNAs (out of the 70) elected on reserved seats.

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