Owning It: Accountability and Citizens' Ownership over Oil, Aid, and Taxes

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Abstract

Government accountability is severely lacking in many developing countries, yet we know relatively little about the causal dynamics that produce citizen demands for greater responsiveness. We argue that a sense of ownership over public money heightens expectations for government services and induces expressive demands for accountability, and we apply the new theory in sub-Saharan Africa. Results from a series of lab-in-the-field experiments in Uganda and Ghana and from a nationally representative survey-based field experiment in Uganda all demonstrate that higher feelings of ownership over public revenues significantly increase citizens accountability pressures on leaders. Furthermore, simple interventions can significantly increase feelings of revenue ownership over oil and aid windfalls, producing demands for accountability indistinguishable from taxes.

Key words: resource curse, experiment, accountability, taxation, foreign aid

Supplementary material for this article is available in the appendix in the online edition.

Replication files are available in the JOP Data Archive on Dataverse (http://thedata.harvard.edu/dvn/dv/jop).

The studies were conducted in compliance with relevant laws and were approved by the appropriate institutional research ethics committee.

Political accountability, a concept central to democracy, implies that citizens can hold their government responsible for its policy actions and punish it if the public's preferences go unmet. But government responsiveness is frequently weakest in those places where it is most important: low-income countries with poor public-goods provision and high levels of corruption. Scholars across subfields in political science have offered various accounts of this empirical regularity. Resource-based explanations suggest that reliance on non-tax revenues such as oil or foreign aid generates fewer accountability pressures from citizens than taxes (Morrison, 2009; Ross, 2012). Low accountability may then produce a reinforcing negative cycle: poor government performance confirms citizens' pessimistic expectations about public services, leading to feelings of cynicism and despair (de Kadt and Lieberman, 2017; Gottlieb, 2016). Hence, improving accountability can foster economic development and promote democracy.

A desire to heighten government accountability has led researchers to provide information on government performance directly to voters (Dunning et al., 2019); establish democratic institutions at the local level (Olken, 2007; Raffler et al., 2019); and allow voters to observe important forms of political competition such as campaign debates (Bidwell et al., 2019; Platas and Raffler, 2019). However, these interventions have had mixed success in improving accountability. The preponderance of evidence suggests that either interventions are targeting mechanisms that only weakly affect accountability, or other institutional or material barriers undercut whatever accountability pressures these interventions create.

In this article, we develop and test an alternative theoretical basis for producing accountability pressures, built around the concept of budget *ownership*. Existing work suggests that citizens will more likely demand accountability when they have high expectations of government (Gottlieb, 2016). Drawing on cognitive psychology, we argue that citizens' subjective feelings of budget ownership, defined as a sense that government monies "belong to them," condition their expectations. Those with strong ownership expect to benefit from government spending, and thus they are more dissatisfied—and willing to take political action—when governments underperform. Critically, feelings of budget ownership vary across individuals and revenue sources: stronger for tax revenues, weaker for windfalls like aid and oil.

Yet it is not obvious that this ownership mechanism should drive accountability demands. While psychology experiments have connected a sense of ownership to action (Kahneman et al., 1990), prior studies have not extended psychological ownership to collective resources such as budgets. Indeed, some citizens may feel little ownership over budgets. The concept of *prebendalism* suggests that, in many African countries, revenues are perceived as belonging to government officials rather than citizens (Van de Walle, 2001). Alternately, citizens may feel budget ownership only when they contribute to it through taxation, and such feelings may be weaker when budgets rely mostly on non-tax revenues. Even when citizens do feel psychological ownership over budgets, such feelings may prove too weak to overcome barriers to take political action and demand accountability. No research to date has identified interventions that use ownership to increase accountability pressures.

We test the ownership theory using lab-in-the-field experiments and a national survey-based experiment in Uganda, supplemented with lab data from Ghana. We find that feelings of ownership strongly predict accountability demands. Additionally, the lab and survey experiments demonstrate that treatments designed to increase subjects' sense of *psychological ownership* over government revenues lead to substantively meaningful and statistically significant increases in subjects' willingness to hold elected officials to account. Contrary to conventional wisdom, the results also indicate that subjects care deeply about the fate of non-tax revenues, and that ownership-increasing treatments can produce accountability pressures for oil and aid windfalls that are statistically indistinguishable from those of taxation.

These results have several implications for the study of accountability. First, the findings on ownership's malleability provide a potential way to alleviate the resource curse (Morrison, 2009; Ross, 2004). Second, given frequent null findings for the effectiveness of other accountability-enhancing interventions (Dunning et al., 2019), our results provide a potential alternative path for increasing citizen demands on governments more generally.

Finally, our findings suggest several areas for future research. We find that several demographic variables correlate with higher budget ownership (see Section Discussion and Robustness), but more work is needed to understand how citizens develop a sense of ownership. Future work could also consider how ownership affects politicians' incentives. If leaders realize that ownership drives political action, they may attempt to lower citizens' ownership by using less visible forms of taxation or laying personal claim to revenues. They may also attempt to buy off or repress high-ownership segments of the population. Politicians in some contexts (perhaps especially in developed countries) may also attempt to heighten ownership for their own strategic purposes, putting rhetoric like "your tax dollars" into new perspective.

A Theory of Ownership, Expectations, and Punishment

Following Pierce et al. (2001), we define psychological ownership as "that state in which individuals feel as though the target of ownership (material or immaterial in nature) or a piece of it is 'theirs'." Ownership "can also be felt toward nonphysical entities such as ideas, words, artistic creations, and other people" (Pierce et al., 2003). This suggests that ownership can apply to budgets, which citizens rarely experience as tangible objects.

This definition distinguishes psychological ownership from legal ownership. While the latter is recognized primarily by society, the former is recognized primarily by the individual who feels it (Pierce et al., 2003). Psychological and legal ownership do not always align. Consider an employee who feels that she "owns" her work computer, while legal ownership resides with the firm. Alternatively, a couple may legally own two cars jointly, yet only feel psychological ownership over the specific car they drive.

The concept of legal ownership allows for variation in ownership across legal settings, but not within them: all citizens in a country have the same legal ownership over government revenues. Psychological ownership focuses on whether citizens *feel* that government revenues belong to them, making it useful in studying accountability pressures. Psychological ownership associates the owned object or idea with the self, incorporating it into individuals' identity (Dittmar, 1992; Gawronski et al., 2007). Ownership also determines "whether a loss is perceived" (Shu and Peck, 2011) when an individual loses or fails to benefit from an object; it affects expectations and makes individuals more likely to reject or punish "unfair" divisions of a resource. Ownership as a causal mechanism thus differs from the related concept of loss aversion (Kahneman and Tversky, 1979), which takes subjects' reference point as given. Ownership, in contrast, can explain where expectations come from. As individuals cannot feel the loss of something they do not own, ownership causally precedes loss aversion.[1]

In democracies, budgets may legally "belong" to citizens, in that they are supposed to be used for their benefit; anti-corruption laws typically forbid officials' diverting government funds toward their private gain. However, understanding when this translates into psychological ownership over the budget proves more complicated. Existing research in psychology has focused on goods for which physical, legal ownership is clearly established and assigned to a particular individual (e.g., Kahneman et al., 1990). Government budgets, on the other hand, are collectively owned, and there is little work on how individuals develop psychological ownership over commonly held resources.[2]

Citizen budget ownership conforms with theories of democracy in which government "belongs to citizens in a more abstract sense. However, even in democracies some citizens may not feel that government is truly "theirs or that they have a right to government services. In authoritarian or hybrid regimes, citizens may not feel any ownership over the budget. This is especially true in countries where patronage and clientelism are common. Van de Walle (2001) argues that in many African countries *prebendalism*—a belief that resources are owned by the government officials who control them—prevails. In such cases, citizens may only expect to benefit from public budgets if they are clients of the relevant officials (Van de Walle, 2001). We therefore expect variation in whether citizens feel ownership over government budgets, even within a given regime type. Some citizens may feel strong

[1] A related concept, the endowment effect, suggests individuals overprice owned objects (Kahneman et al., 1990). The ownership mechanism appears to produce the endowment effect (Shu and Peck, 2011).

[2] The discussion section describes the individual characteristics associated with high ownership.

ownership, while others believe that the budget belongs to politicians.

Individuals may also feel different levels of ownership over different revenue types. Previous research suggests that citizens hold leaders more accountable for how they spend taxes relative to windfall revenues from oil or aid (Martin, 2014; Paler, 2013; Weigel, 2019). When citizens pay taxes, they contribute their own income to the budget; this may increase budget ownership. Indeed, Paler (2013) finds that a simulated tax heightens ownership over government budgets, yet ownership there remains conceptually entangled with loss aversion and the endowment effect (see also Sandbu, 2006). Other work has focused more exclusively on loss aversion (e.g., Martin, 2014), which as noted above is theoretically distinct from ownership.

Linking Ownership to Accountability Demands

In contrast to a prebendalist system, an accountable government implements citizens preferred policies efficiently and with a minimum of corruption and mismanagement (Fearon, 1999). However, governments are unlikely to provide citizens' preferred policies unless deviations are punished. This makes citizens willingness to sanction poor government performance a key element of accountability. When, then, will citizens be willing to vote for the opposition, donate to civil activists, contact their representatives, or otherwise make demands on leaders? In general, citizens will take action when the expected benefits of doing so exceed the costs. Costs include monitoring government behavior, forgoing economic activity, engaging in collective action, and facing the possibility of repression.

By demanding accountability, citizens hope to benefit economically from improved government policy in the future. However, citizens face a collective-action problem: each individuals action is unlikely to prove pivotal, and a rational citizen can therefore free-ride on others' efforts. Individuals will thus most likely participate politically when doing so garners private, excludable benefits (Olson, 1965). One form these may take is psychological, expressive benefits. A large body of work in psychology and behavioral economics has demonstrated that individuals are willing to punish others for how they allocate a resource even when there is no economic benefit from doing so (see e.g., Henrich et al., 2006), and that punishing bad behavior appears to alleviate negative emotions (Fehr and Gächter, 2000).

We argue that high budget ownership increases the expressive benefits citizens receive from demanding accountability from leaders; this then makes citizens more willing to take action. The key mechanism through which ownership acts is citizen expectations. Recent work has shown that "performance relative to expectations" predicts sanctioning better than absolute levels of government performance (Gottlieb, 2016). Citizens effectively compare actual government performance to what they expected: as this difference increases, citizens are more likely to pay the costs of action.

We argue that ownership is a key lens through which citizens view government performance. It determines citizens' answer to the question: how much do I feel that I "should" benefit personally from government spending? Higher feelings of ownership thus yield higher expectations and, in turn, increase citizens' dissatisfaction when they observe corruption or poor performance. As a citizen's dissatisfaction increases, so too will the value of the expressive benefit that comes with punishment.

Some work in psychology suggests that ownership affects willingness to punish—that "emotions spark when we experience the invasion of what we feel is 'ours' (Pierce et al., 2001). Ownership is closely related to the *desire* to control how the object is used (Pierce et al., 2001). In Ultimatum games, individuals are more likely to reject low transfers when they have ownership over the resource being divided (Wu et al., 2012). Likewise, subjects who divide a resource in a Dictator or Ultimatum game give higher transfers when the receiver has initial ownership over the endowment (Leliveld et al., 2008; Wu et al., 2012). However, there is little work testing whether these findings will transfer to political or policy contexts.

Focusing on ownership helps us understand why accountability for tax revenues may be higher and the conditions under which some citizens may demand accountability for windfall revenues like aid and oil. Foreign donors give aid expressly to help beneficiaries, so some recipient citizens may feel that it "belongs to them. Likewise, oil is extracted from the country itself, which represents a common legacy and thus may lead to higher psychological ownership among some citizens, perhaps especially in oil-producing regions.

Furthermore, we suggest that ownership over the budget may be malleable. This paper focuses on a normatively positive effect of malleability: the potential to increase citizens' ownership over windfalls like aid and oil. Civil society groups may be able to develop campaigns to heighten public sentiment that windfalls belong to citizens, thereby increasing demands for accountability. Indeed, the survey-based field experiment tested below was designed with just such an application in mind.

More ominously, leaders may see strategic benefits to lowering any public sense of ownership. In Uganda, President Museveni has referred to the country's oil reserves as "my oil" (Mwesigwa, 2016); invoking prebendalism may dampen citizens' expected benefits. Politicians may also strategically allocate public goods disproportionately to citizens with high ownership—such as wealthier urban areas—especially when those areas have high collective-action capacity. This may help to account for the well-studied phenomenon of urban bias in sub-Saharan African public policy (Eicher and Baker, 1982; Bates, 2014; Sandbrook, 1986). Thus, ownership of public revenue has wide-reaching implications for practical politics and policy-making.

Hypotheses: Testing the Ownership Effect

The ownership theory suggests two hypotheses. First, we posit that there is substantial variation in the level of ownership individual citizens feel over a given government revenue source, and that greater ownership will positively correlate with each citizen's willingness to demand accountability through engaging in costly political behaviors. We test this using correlations between self-reported ownership and costly actions in our lab and survey data:

> **Hypothesis 1** *Variation in citizens sense of ownership over a particular revenue source will predict variation in their accountability demands.*

Our theory is agnostic about what individual characteristics predict high ownership.

Following our main experimental results we explore potential sources of variation, including demographics like age, wealth, education, and urban-rural status. Regardless of which individuals have higher budget ownership, our theory predicts that a sense of ownership is not fixed and can be manipulated by outside treatments.

> **Hypothesis 2** *Ownership is malleable: an experimental intervention that increases citizens' sense of ownership over government revenues will lead to higher accountability pressures from citizens.*

The theory explicated above was developed over multiple rounds of fieldwork and data collection. In 2016, we measured budget ownership as part of lab experiments in Ghana that focused on other aspects of governance. We expected (but did not find) that ownership might be higher for oil than aid. This led us to focus on ownership during subsequent data collection in Uganda, including devising treatments to manipulate ownership. Appendix A.4 discusses this process in more detail, including which hypotheses and tests were pre-specified in each phase of data collection.

We pre-registered an early version of the ownership hypothesis (H1) in the Ghana study, but did not pre-register the ownership manipulation hypothesis (H2) until the Uganda studies. Below we use the Ghana data, as it includes a tax treatment and a measure of ownership, to test—in an exploratory, out-of-sample analysis—taxation's effect on ownership and ownership's mediation of taxation's effect on accountability demands.

We test our hypotheses using multiple methods. We first use lab-in-the-field experiments for tests in a controlled setting. In the lab experiments, we designed a modified Ultimatum game in which a "Leader" must allocate resources to a "group fund," and a "Citizen can pay to punish the Leader if she does not approve of the allocation. To test Hypothesis 1, we examine whether Citizens' sense of ownership over the group fund predicts sanctions on Leaders for low transfers. To test Hypothesis 2, we used simple prompts identifying part of the group fund as belonging to the Citizen to test whether it is possible to manipulate ownership over windfalls and whether such manipulation increases punishment for low transfers from Leaders. The order we present the lab experiments is slightly different from the order in which they were conducted. We do this to streamline the explanation of each experimental protocol and to draw a clearer link between the ownership manipulations in the lab and the comparable treatments in our national survey data.

The controlled setting of lab experiments enables us to isolate the effect of ownership and to manipulate ownership while holding constant all other aspects of a citizen-leader interaction. However, lab results may not fully generalize to real-world political contexts. For this reason we also designed and ran a survey-based field experiment on a large, nationally representative sample of Ugandan adults. The survey experiment assesses both how ownership correlates with accountability demands and how ownership manipulations affect political activity for a representative sample of Ugandan citizens in a more naturalistic setting.

The theory presented here has significant implications for politicians' behavior and for general welfare. First, ownership may have redistributive consequences: if high ownership raises demands on the state, it makes those with high ownership more expensive to buy off, but also potentially more necessary to appease, especially if those citizens are a key voting bloc or able to credibly threaten protest. This could lead politicians to target policy or transfers to those with high ownership, or—in authoritarian countries—lead to higher repression of high-ownership groups. The exact redistributive consequences will depend on who has high ownership in the first place; while we argue here that taxpayers will have higher ownership, and that it is possible to raise ownership over windfall revenues, after our main results we briefly discuss the individual-level characteristics that correlate with high budget ownership.

In the long run, a second implication of our theory is that politicians have incentives to strategically manipulate budget ownership. Systems of clientelism and patronage rely in part on the idea that politicians and bureaucrats own the resources they have access to and at their discretion dole out benefits to supporters. This raises the possibility that accepting a clientelist offer lowers ownership, although this is not tested in the current study. Other ways of lowering ownership could be through making taxation less visible to reduce ownership over tax revenues or attempting to set norms around ownership over revenues like aid and oil. For example, Ugandas President Museveni has laid personal claim to the countrys oil reserves, even saying of the opposition that “[t]hey are targeting my oil” (Waswa, 2015).

Case Selection

As a low-income, quasi-authoritarian African country, Uganda is a particularly apt location to test the effects of different revenue sources on accountability pressures. Taxes, foreign aid, and oil revenues are all salient revenue sources in Uganda. All citizens pay value-added taxes. Many also pay some form of direct tax, although the government has eliminated several direct taxes—including head taxes and many property taxes—in the run-up to elections. Observers argue that this has led to lower accountability pressures from citizens (Persson and Rothstein, 2015). Significant oil reserves were discovered in 2006, and while oil production has not yet ramped up fully, there has been intense public debate over the use of oil-based revenues. Ugandan citizens are also highly aware of foreign aid. It forms a significant fraction of spending on services, and in 2012 donors cut over US$300 million in response to a corruption scandal involving aid money.

Testing Ownership in the Lab

We ran three sets of lab experiments in Uganda in 2017, all based on experiments in Martin (2014). All treatments involve a single-shot game between a Citizen and Leader who are randomly assigned to their roles. In all conditions, the Leader chooses how to allocate a group fund, and Citizens can pay to punish the Leader if they are not satisfied. Referring to the leader’s endowment as the group fund signals to Citizens that they should benefit from its disbursement. The experiments vary (1) the source of the group fund and (2) whether Citizens are given a treatment designed to strengthen ownership over the group fund. The outcome is Citizens’ willingness to punish the Leader for a given transfer; the independent variables are the source of the group fund and a post-treatment variable measuring the degree to which citizens feel ownership over the group fund, described below.

To test Hypothesis 1 we examine whether, across conditions, ownership over the group fund predicts willingness to punish the Leader for low allocations. To test Hypothesis 2, we designed treatments to manipulate ownership and then assess the treatments' effects on punishment thresholds. A taxation treatment in the Tax Experiment tests how assigning physical ownership over revenues increases psychological ownership and, hence, punishment. The Oil Ownership and Aid Ownership Experiments test whether it is possible to increase psychological ownership over oil and foreign aid revenues, respectively, by identifying a portion of the group fund as belonging to the subject. We also assess whether the treatments—through increased ownership—heighten Citizens' punishment of Leaders for low transfers.

Lab Experiment 1: Manipulating Ownership through Taxation

The Tax Experiment uses four treatments to test whether whether ownership and punishment increase when Citizens pay taxes compared to their receipt of windfalls from aid, oil, or an unspecified grant. The steps of each treatment are given in Table 1.[3] In all treatments, the Leader receives a group fund of 10 monetary units (MU) to divide between her own salary and the Citizen. The treatments vary the source of the group fund. In the Tax condition, the Citizen receives 10 MU then pays 5 MU as a tax. The enumerator doubles the 5 MU tax and gives 10 MU to the Leader as the group fund. In the Windfall conditions the citizen receives a 5 MU wage and pays no tax; the enumerator gives the Leader a 10 MU group fund as an unearned transfer. The treatments are identical for the rest of the game. The Leader then allocates the group fund between her own "salary" and a transfer to the Citizen. Before observing the Leader's allocation decision, the Citizen chooses the lowest transfer he is willing to accept from the Leader. If the Leader transfers back less than this amount, the Citizen pays a punishment cost of 1 MU, and the Leader loses 4 MU. No one receives the money lost in punishment.[4]

[3] Appendix A provides a detailed overview of key game mechanics, implementation, sampling, and wording.

[4] Implementation protocols linked each game component to real-world concepts. The Leader's share of the

<table>
<tr><th rowspan="2">Stage</th><th rowspan="2">Tax Game</th><th colspan="3">Windfall game</th></tr>
<tr><th>Unspecified Grant</th><th>Aid</th><th>Oil</th></tr>
<tr><td>1</td><td>The citizen earns a wage of 10 MU.</td><td colspan="3">The citizen earns a wage of 5 MU.</td></tr>
<tr><td>2</td><td>The citizen is taxed 5 MU. This is doubled to 10 MU and given to the leader as the group fund.</td><td colspan="3">The leader is given 10 MU as the group fund.</td></tr>
<tr><td>3</td><td colspan="4">The Leader allocates 10 MU between himself and the Citizen.</td></tr>
<tr><td>4</td><td colspan="4">The Citizen observes the Leader's decision and, based on the decision rule they specified, decides whether to pay 1 MU to have enumerators remove 4 MU from the Leader.</td></tr>
</table>

Table 1: Experiment 1. Steps for each treatment condition in Tax Experiment shown.

The games capture two quantities of interest. First, the Citizen's *punishment threshold* represents the lowest allocation of the group fund by the Leader below which the Citizen will punish. The enumerator asks, "if the Leader keeps 10 MU and passes back 0 MU to you, will you pay 1 MU so that the leader loses 4 MU?" If the Citizen says yes, the enumerator repeats the question, increasing the Citizen transfer by 1 MU, until the Citizen no longer punishes. This transfer value is the punishment threshold. All games are single shot, so punishment strictly decreases the citizen's economic utility and thus is purely expressive. The Citizen's threshold represents the point at which the expressive benefits of punishment outweigh the economic costs. Second, we measure Citizens' post-treatment ownership over the group fund by asking how much they agreed with the statement that "the group fund belonged to me," on an 11-point ladder with anchors of "Do not agree at all" at 0 and "Strongly agree" at 10. This measure corresponds to those used in cognitive psychology to measure ownership over private goods (see e.g. Shu and Peck, 2011).

Lab Experiments 2: Manipulating Oil and Aid Ownership

We designed the Oil Ownership and Aid Ownership Experiments to test whether psychological ownership over windfall revenues is malleable even when subjects never physically or legally own any part of the group fund. The Oil Ownership Experiment, conducted in January 2017, consisted of three treatments: Oil, Oil Ownership, and Oil Framing. In the base Oil condition, enumerators told Citizens only that the group fund came from oil rev-

group fund was described as salary, while the Citizen transfer represented public services. Survey results show that 86% of citizens saw the transfer as more like services than a personalistic transfer.

enues.[5] In the otherwise identical Oil Ownership condition, before Enumerators gave the 10 MU group fund to the Leader, they told Citizens that 5 MU of group fund represents the share of the oil money that belongs to you, as the citizen. As we designed this treatment to increase psychological not physical ownership, Citizens never physically held any portion of the group fund. Citizens in the Oil Framing condition were only told that Oil money is meant to belong to all Ugandans, and to be used to benefit citizens like you. It thus mentions ownership, but does not assign individual citizens ownership.

The Aid Ownership Experiment, run in Kampala in July 2017, consisted of two treatments: Aid and Aid Ownership. These were identical to the Oil and Oil Ownership treatments described above except that enumerators identified the revenue source as foreign aid. Both the Aid and Oil Ownership Experiments use the same punishment threshold measure as the Tax Experiment. The Aid Ownership Experiment also uses the same 11-point ownership measure used in the Tax Experiment. The Oil Ownership Experiment used a similar question with a 4-point response scale; this less-sensitive measure biases against finding an effect of the treatment on ownership or of ownership on punishment.

Lab Experiments: Data and Key Outcomes

Ugandan enumerators implemented all three experiments at field sites in Kampala, Uganda in 2017. At each site, volunteers were recruited for 16-person sessions, conducted in the Luganda language. We randomly assigned treatments at the session level; within each session, subjects were randomly assigned to be Citizens or Leaders. In each session, respondents received a group training on the assigned treatment, then met one-on-one with enumerators to play one practice round and five single-shot rounds of the assigned game. To ensure each round represented a single-shot game, Citizens were randomly and anonymously paired with a different Leader in each round. After round 5, respondents completed a survey that included our ownership measure. For enumeration, 1 MU was set to 100 Ugandan

[5] This was nearly identical to the windfall conditions above; see Appendix A.1.2 for details.

Tax Experiment		Oil Experiment		Aid Experiment	
Tax	208	Oil Ownership	138	Aid Ownership	104
Windfall	207	Oil Framing	143	Aid	101
		Oil	131		
TOTAL	415	TOTAL	412	TOTAL	205

Table 2: Citizens per Treatment in Lab Experiments.

Shillings (UGX); respondents received payouts from 3 randomly selected game rounds. The average Citizen payout was 4,500 UGX, three times our sample's median daily wage.

Across all three experiments 1,032 Citizens played five rounds each. Leaders are not included in the analysis as they did not set a threshold. Variation in the actual number of observations is due to covariate missingness or respondent attrition across the five rounds. Table 2 shows the distribution of Citizens across each experiment. Chi-squared tests for covariate balance are consistent with successful randomization (See Appendix G).

Lab Experiments: Results

Hypothesis 1 expects that individual variation in ownership will predict Citizens' punishment thresholds *within* each treatment condition. Hypothesis 2 predicts that the ownership manipulations in each experiment—Taxation in the Tax Experiment, Oil Ownership in the Oil Ownership Experiment, and Aid Ownership in the Aid Ownership Experiment—will increase punishment thresholds relative to baseline. H2 also implies that this increase will be mediated by our ownership measure. This section provides evidence in support of these hypotheses. All tests except the mediation analysis were included in our pre-analysis plans, which were registered with the Evidence in Governance and Politics network (http://egap.org/design-registrations). The Aid Ownership analysis was not registered separately, as we followed the Oil Ownership pre-analysis plan (see Appendix A.4 for details).

Ownership Increases Willingness to Punish in the Lab

We estimate the OLS model $Y_{ij} = \alpha + \beta \text{Ownership}_i + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i$. The dependent variable is subject i's punishment threshold in round j; Ownership is the 11-point (or 4-point) post-treatment ownership measure discussed above. $\mathbf{X}_{ij}$ is a vector of controls, including the Leader transfer in the previous round and enumerator and round fixed-effects. Because ownership is not randomly assigned, to alleviate omitted variable concerns $\mathbf{X}_{ij}$ also includes respondent age, gender, education, level of poverty, and perceived quality of local services. For pooled results, we include indicators for each revenue treatment. Standard errors are clustered by respondent. All results are robust to controlling for subjects who failed a source manipulation check.

The first column in each panel of Table 3 reports the relationship between the ownership measure and punishment in the three experiments. In the Tax Experiment, a one standard-deviation (SD) increase in the ownership measure corresponds to a 0.749 MU increase in subject thresholds, equivalent to 0.54 SD. The effects are similar in the Oil and Aid Ownership Experiments; even using our less sensitive 4-point ownership measure in the Oil Ownership Experiment, a 1 SD increase in ownership corresponds to a 0.653 MU punishment threshold increase.[6] The remaining columns in each panel show that the results hold when we analyze each experiment's treatment conditions separately, reducing concerns that the pooled result simply reflects a priming effect of the ownership treatments. This provides, to the best of our knowledge, the first evidence that budget ownership predicts willingness to punish leaders for their spending behavior. Appendix B shows that group fund ownership is correlated with ownership over the actual Ugandan budget and with hypothetical willingness to take action in response to a corruption scandal. So there is some evidence that psychological ownership might travel beyond the laboratory setting.

As the results in Table 3 are correlational, one concern might be that our ownership

[6] The mean punishment thresholds in the lab experiments range from 4.3 MU to 5.4 MU (i.e. 43-54% of the group fund), depending on the exact experiment and treatment condition; see Appendix G for details.

	DV: Citizen Punishment Threshold				
Panel A: Tax Experiment	Pooled	Aid	Oil	Grant	Tax
Ownership	0.749***	0.650***	0.814***	0.519***	0.802***
	(0.059)	(0.171)	(0.132)	(0.149)	(0.084)
Adjusted R^2	0.380	0.370	0.472	0.351	0.401
Subjects	415	67	71	69	208
Rounds	2075	335	355	345	1040
Panel B: Oil Experiment	Pooled	Oil Ownership	Oil Framing	Basic Oil	
Ownership	0.582***	0.653***	0.777***	0.477***	
	(0.106)	(0.25)	(0.16)	(0.146)	
Adjusted R^2	0.139	0.114	0.176	0.252	
Subjects	387	127	131	129	
Rounds	1927	631	653	643	
Panel C: Aid Experiment	Pooled	Basic Aid	Aid Ownership		
Ownership	1.045***	0.999***	1.047***		
	(0.07)	(0.133)	(0.091)		
Adjusted R^2	0.473	0.460	0.481		
Subjects	205	101	104		
Rounds	1025	505	520		
Note:	*p<0.1; **p<0.05; ***p<0.01				

Table 3: Ownership Predicts Punishment Thresholds Within Treatment Conditions. OLS models for each treatment condition estimated separately. SE clustered by respondent. Enumerator and Round FE and subject covariates used in all models.

mechanism is proxying for an underlying characteristic not captured by our model. For example, perhaps ownership does not drive punishment, but rather both ownership and punishment are caused by respondents' education level, or perhaps a pyschological characteristic such as egosim or aggression. We are able to partially, but not entirely, control for this by including available covariates in our regressions. To test ownership's ability to causally affect punishment, we turn to our experimental manipulations of ownership.

Manipulating Ownership Increases Punishment in the Lab

This section shows that each treatment successfully increased both ownership and punishment thresholds, and that ownership mediates the effect on punishment. In the analysis, the reference categories are: the three Windfall conditions (Aid, Oil, and Tax) for the Tax

	DV: Ownership			DV: Punishment Threshold		
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Experiment:						
Treatment: Tax	0.244**			0.422***		
Control: Windfall	(0.095)			(0.113)		
Oil Ownership Experiment:						
Treatment: Oil Ownership		0.088			0.435**	
Control: Oil & Oil Framing		(0.084)			(0.176)	
Aid Ownership Experiment:						
Treatment: Aid Ownership			0.229**			0.396***
Control: Aid			(0.106)			(0.146)
Round FE				✓	✓	✓
Enum FE	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.075	0.391	0.432	0.153	0.087	0.219
Observations	415	387	205	2075	1927	1025

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: Effect of Ownership Treatments on Punishment and Ownership in Uganda. As predicted by H2, Ownership treatments significantly increase ownership (Columns 1-3, 1 observation/respondent) and punishment thresholds (Columns 4-6, 5 observations/respondent). Heteroskedasticity-consistent (HC3) SEs used in Columns 1-3 and subject-clustered (CR2) SEs used in Columns 4-6.

Experiment; the pooled Oil and Oil Framing conditions for the Oil Ownership Experiment; and the Aid Condition for the Aid Ownership Experiment.

The effect of treatment on ownership is analyzed at the subject level. Columns 1-3 of Table 4 show that all three treatments produced positive, though not always statistically significant, increases in ownership. For the Tax and Aid Ownership conditions, the effect is both substantively large and statistically significant. In the Oil Ownership condition, in which we used the less-sensitive 4-point ownership scale, the coefficient is smaller and not significant. This is likely due to the lower sensitivity of the 4-point ownership measure, which generated significant ceiling effects.

Columns 4-6 of Table 4 show that all three ownership treatments significantly increased Citizens' punishment thresholds. All specifications are OLS and include subject covariates plus game-round and enumerator fixed effects.[7] Taxation increases punishment by 0.422 MU (p=.000) relative to the three Windfall conditions (Aid, Oil, and Grant). Similarly, the Oil Ownership condition increases punishment by 0.435 MU (p=0.013) relative to the Oil and Oil Framing conditions; Appendix Table E.2 shows that the Oil Framing placebo treatment has no significant effect. Finally, relative to the Aid condition, the Aid Ownership treatment increased punishment by 0.396 MU (p=0.007). Strikingly, the increase caused by the Aid and Oil Ownership treatments is similar in magnitude to that induced by taxation, even though respondents never physically own part of the group fund as in the Tax treatment.

To test whether increases in ownership are driving the observed increase in punishment, Appendix C.1 performs mediation analysis for the Oil Ownership, Aid Ownership, and Taxation treatments. While mediation analysis was not pre-registered for the lab data, it follows directly from our main hypotheses; running it on multiple samples decreases concerns that any findings are due to chance. We find that the ownership measure, when used as a mediator, accounts for approximately half of the effect of the Tax treatment and Aid Ownership treatments. In the Oil Ownership condition, the mediation effect of ownership is significant at the 10% level but substantively smaller, likely due to the less sensitive ownership measure.

To rule out the possibility that the Aid and Oil Ownership treatments affect punishment by signalling that the group fund *should* be spent to benefit the citizen, we use the "Oil Framing" treatment described above, which told subjects that oil revenues were meant to belong to all Ugandans generally, but did not assign subjects individual ownership. Punishment thresholds in the Oil Framing condition are nearly identical to those in the basic Oil treatment (See Appendix Table E.2).

As predicted by Hypothesis 1, our psychological ownership measure predicts citizens demands on leaders in all experimental conditions. The results also indicate, in line with Hypothesis 2, that ownership is malleable. Indeed, in the lab setting manipulating ownership

[7] Covariates are age, education, gender, perceived quality of local public services, and a deprivation index. We include them to keep model specification consistent across tables.

over non-tax sources can produce accountability pressures statistically indistinguishable from those caused by taxation. However, lab experiments have several weaknesses. In particular, many lab results fail in other settings and may not translate to real-world outcomes. In the following sections we use two additional experiments to address these issues: a lab experiment in Ghana, conducted prior to the experiments presented above, and a nationally representative survey-based field experiment in Uganda.

External Validity Check Using Lab Results from Ghana

As noted above, before the Uganda experiments we designed and implemented a similar lab experiment in Ghana in 2016. While the ownership manipulation hypothesis (H2) was not conceived at that time, the Ghana data offer the opportunity to probe the ownership mechanism in an exploratory, out-of-sample check for external validity. We thus re-examine the Ghana data to learn if taxation increases psychological ownership and if ownership mediates taxation's effect on punishment thresholds in this sample. Compared to Uganda, Ghana is substantially wealthier, relies more on taxation, is less aid dependent, and draws more heavily on oil revenues. Unlike Uganda, Ghana has competitive multi-party elections and a recent history of peaceful leadership transitions.

The Ghana experiment consisted of Tax, Aid, Oil, and Grant treatments similar to those in the Uganda Tax Experiment. The main outcome is still the punishment threshold. The ownership measure is a three-point indicator that takes 0 if the respondent disagreed or disagreed strongly with the statement "the group fund belonged to me," 1 if they agreed and 2 if they strongly agreed. Coding both types of disagreement as 0 more closely matches our 11-point measure in which subjects cannot give ownership values below 0. Appendix A.4 reports the full experimental design and a description of the original analysis plan. Appendix Tables D.1-D.3 show that, in support of Hypothesis 1, the ownership measure robustly predicts punishment behavior. In support of Hypothesis 2, the Tax treatment increases self-reported ownership as well as punishment thresholds. Finally, as in Uganda, taxation's effect on punishment behavior is strongly mediated by the ownership mechanism. We therefore find similar effects of ownership in Ghana and Uganda.

Increasing Ownership in the Field

To test whether our laboratory results hold in a more naturalistic setting, we designed a survey-based field experiment to test the link between ownership and political engagement. In particular, we wanted to use a more representative group of citizens that tested the theory on rural as well as urban dwellers, and to work in a less controlled setting where citizens face more barriers to action. We therefore drew an area probability sample in Uganda and employed an intervention and behavioral outcome measures designed to reflect both civil organizers' information campaigns and real-world political action. The experiment focuses on two questions. First, is it possible to increase citizens' ownership over real-world revenue sources? Second, does higher ownership increase accountability pressures in the field? The experiment was embedded in a national survey of 2,514 Ugandan citizens in 11 districts. Appendix A.3 includes full information on sampling and implementation, along with balance tests, summary statistics, and the wording of treatments and outcome measures.[8]

Respondents were randomly assigned to the control group, Aid Ownership treatment, or Oil Ownership treatment. The control group moved straight from the pre-treatment to post-treatment survey modules. The two ownership conditions received additional treatment modules immediately after the pre-treatment survey. Treatments were designed to mimic the structure of the laboratory experiments while accounting for the more complicated real-world accountability process that Ugandan citizens face. Ugandans theoretically own aid and oil revenues but never physically possess them, making them good candidates for a psychological ownership treatment. Because any treatments that reference actual govern-

[8]Our analysis focuses on household heads only, which our pre-analysis plan identified as more likely to be moved by treatment. Non-heads of household have little experience budgeting or making large purchases, diminishing the treatment's realism. Appendix Table F.3 reports the results for non-heads of household.

ment revenues are inherently informational, our ownership treatments were constructed as information treatments designed to increase subjects' feelings of indirect ownership.[9]

The Aid and Oil Ownership treatments each had two parts. The first part gave respondents information about the actual aid or oil revenues that had accrued to Uganda's government in the past 10 years. It then helped respondents think about how this money could have benefited them, giving personalized information about what kinds of local public goods and household expenditures the aid (or oil) could have financed were it distributed equally to all Ugandans, assuming a 15% overhead cost of distribution. Such a scheme, while not under consideration in Uganda, is similar to oil-fund distribution in Alaska and elsewhere or to using aid funds for unconditional cash transfers.

Enumerators told respondents the amount of money that would have come to their village under equal distribution, using actual village size data. Enumerators then listed local public goods that could have been provided using the money.[10] Next, enumerators told respondents the amount of money that would have accrued to their actual household (based on a pre-treatment household size question) along with a list of common household and business purchases that the money could have financed. The result was a highly-personalized information treatment that implicitly transferred ownership of aid (oil) revenues to the citizen (See Appendices A.3.3 and A.3.2 for further detail).

The second part of the ownership treatment further increases psychological ownership by having respondents think of how they actually would have spent the aid (oil) funds had their household received a share. Respondents completed a budgeting task in which they told enumerators which purchases they would have made for themselves or their households using the aid (oil) money. In practice, most subjects "purchased business or farming inputs plus some consumer goods. Enumerators recorded each simulated purchase on a card and, at the end of the budgeting task, gave respondents a summary of the decisions. Finally,

[9]Details on a further set of information placebo treatments are provided in Appendix A.3.

[10]These goods, based on Ugandan project budgets, included health clinic supplies; textbooks; new boreholes; new schools; and road paving. See Appendix A.3.7 for more detail.

enumerators gave respondents additional information, based on budget projections, about total amount of revenues from aid (oil) that the government expects to receive in the future, given in present, absolute terms (assuming a future discount rate of 6% per annum) and also broken down by village and household size (See Appendix A.3.7 for details).

Immediately following treatment, we measured respondents' budget ownership and other potential mechanisms, discussed further below. Our ownership measure closely matched the 11-point scale used in the lab games, changing the text to reflect that we were asking about actual government revenues. Respondents were asked about their ownership over tax, oil, and aid revenues. The relevant source was always asked first in treatment conditions; the order was randomized in control.

To measure citizen action, we then gave respondents the opportunity to take four costly political actions. Given Uganda's increasingly closed political space, we focused on measures that did not collect respondents' names or otherwise expose them to a risk of government reprisals. We then used these four measures (described fully below) to create an inverse covariance weighted accountability index that summarizes citizens' accountability demands immediately after treatment was delivered. For ease of exposition, we standardize this index against the control group, such that positive values represent increases in standard deviation units relative to those who did not receive the ownership treatment. The variables used in the index and its construction were pre-registered prior to data collection.

For the first behavioral outcome, *Donation*, we told respondents that we would donate 1,000 Ugandan shillings on their behalf to a healthcare- or corruption-focused NGO. Respondents chose which organization to donate to; we coded *Donation* as a 1 if the respondent chose the anti-corruption NGO and 0 otherwise.

Our second behavioral outcome, *Send Message*, asked respondents whether they wanted to anonymously send a message to a government official of their choice. If so, they chose an official, then the enumerator helped them fill out a comment card. As most respondents sent a card, we focus on the level of government targeted by the respondent. As aid and oil money are controlled by the national government, we created a binary variable that took a value of 1 if the respondent chose to contact a national-level official and 0 otherwise.

Our third and fourth behavioral outcomes measure willingness to pay for additional information about government behavior by sending SMS messages.[11] The third outcome (*SMS*) measures whether respondents paid to send a text message to sign up for an NGOs SMS platform that distributes information about government spending. The final outcome (*Report*) measures whether respondents paid to send an SMS to the researchers requesting more information about survey results, government revenues, or Ugandan demographics (see Appendix A.3 for details).

Results of Survey Experiment

To test Hypothesis 1, Table 5 reports OLS regressions of the (standardized) accountability index on subjects' (standardized) average ownership over aid, oil and tax revenues, controlling for age, education, household size, gender, and logged personal income. Column 1 pools the treatment conditions together; Columns 2-4 report results by treatment. Coefficients represent the standard deviation change in our accountability index associated with a one-standard-deviation change in subjects' feelings of ownership. Consistent with our lab results, we find that ownership strongly predicts willingness to take action in both the pooled and disaggregated samples.

[11] SMS messages cost approximately 100 UGX (US$0.03); even this small amount is meaningful for respondents.

	Pooled	Pure Control	Aid Ownership	Oil Ownership
Accountability Index	0.187***	0.121**	0.201***	0.243***
	(0.035)	(0.056)	(0.061)	(0.066)
N	834	282	276	276
Subject Covariates	✓	✓	✓	✓

*p < 0.1; **p < 0.05; ***p < 0.01

Table 5: Correlation between Ownership and Accountability Demands. Ownership is the average of ownership over aid, oil and taxes. All models use classical standard errors.

To test Hypothesis 2 regarding ownership manipulation, we conducted difference-in-means tests. First, we test whether subjects in the Aid (Oil) Ownership treatment reported higher values for the aid (oil) ownership measure, relative to the control group. Because subjects received treatments about either aid *or* oil, we estimate separate models for each source. As feelings of political efficacy can moderate political behavior, we also estimate treatment effects separately for those falling above and below the sample mean of an inverse covariance weighted efficacy index. We pre-registered that we expected larger treatment effects among those with higher efficacy.[12] These results are reported in Panel A of Table 6. Consistent with the lab results and Hypothesis 2, we find that ownership is malleable: both the Aid and Oil Ownership conditions significantly increase (ATE=0.31 and 0.33 SD, respectively) ownership over the relevant source (Col 1). This effect is largest for those with low efficacy, and notably smaller for high-efficacy types. Since low efficacy types are also those with the lowest ownership, the results suggest that the treatment was most effective for low-ownership types.

Next, we test whether the ownership treatments increased subjects' willingness to demand accountability as measured by the summary index introduced above. These results are reported in Panel B of Table 6. The full-sample treatment effect (Col 1), while positive, is not statistically significant (ATE=0.04 and 0.02 SD for Aid and Oil Ownership, respectively).

[12] The (pre-treatment) efficacy index measures subjects' internal and external efficacy and their belief about government efficacy. See Appendix A.3.4 for details.

Panel A: Effect of Treatment on Ownership Mechanism			
	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	0.307***	0.373***	0.252**
	(0.076)	(0.109)	(0.107)
Oil Ownership	0.332***	0.388***	0.305***
	(0.074)	(0.108)	(0.102)
Aid/Oil N	574/565	281/270	291/292
Panel B: Effect of Treatment on Accountability Index			
	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	0.037	0.205*	−0.116
	(0.086)	(0.124)	(0.119)
Oil Ownership	0.024	0.213*	−0.124
	(0.086)	(0.125)	(0.118)
Total N	845	412	428
Panel C: Ownership Mechanism as Mediator			
	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	0.064***	0.083***	0.051***
	[0.026, 0.109]	[0.023, 0.164]	[0.008, 0.120]
Oil Ownership	0.057***	0.072**	0.042*
	[0.021, 0.102]	[0.018, 0.149]	[-0.002, 0.106]
Aid/Oil N	558/557	272/266	284/288
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 6: Effect of Ownership Treatments on Accountability Index and Ownership Mechanism. All models use classical standard errors. Covariate adjustment is used in mediation models. For further details, see Appendix C. 95% confidence intervals in brackets.

As Columns 2 and 3 demonstrate, however, the overall effect masks substantial variation by subjects' level of efficacy: among subjects with efficacy at or below the control group mean, the Aid and Oil Ownership treatments produce substantively large and statistically significant increases in accountability pressures ($p = 0.075$ and 0.071 respectively), while for those above the mean the ATE is slightly negative and insignificant.

To test for non-linearity in the treatment effect across the range of our efficacy index, we model the heterogeneous effect of our treatment with respect to efficacy using the kernel smoothing estimator implemented in the `Interflex` package in `R`. The results, presented in Appendix Figure F.2, reveal the same pattern presented here and suggest that, for the least efficacious respondents, treatment effects are more than twice as large as the overall low-efficacy estimates reported here. The pattern is particularly stark in the Aid Ownership condition, where treatmnet effects in the bottom quartile of the efficacy distribution average 1.08 standard deviations (average p=0.0004).

Our treatments are, by necessity, compound treatments. In addition to increasing ownership, it is likely that they also gave citizens new information about the budget and may have changed other attitudes toward government, such as the perceived costs of taking action. To test this possibility, we use mediation analysis to isolate the ownership mechanism directly, decomposing the overall treatment effect into two parts: the effect of the ownership treatment on punishment *caused by the increase in ownership* (called the Average Causal Mediation Effect, or ACME), and the combined effect of other (unmodeled) mechanisms (the Average Direct Effect, or ADE). Mediation analysis was pre-specified for this experiment. Following the previous two analyses, we conduct mediation analysis on the full sample as well as the high- and low-efficacy subsamples. In Panel C, we report the ACME for each of these subsamples for both the Oil and Aid Ownership treatments. As before, we take the Pure Control condition as the reference group.[13] Potential threats to inference for the mediation analysis are discussed in the following section.

The estimates of the ACME show that, consistent with Hypothesis 2, subjects' feelings of ownership mediate the relationship between the ownership treatments and the accountability index. As in the lab, the ownership mechanism is a substantively meaningful and highly significant mediator of the overall treatment effect. This pattern persists across both the low- and high-efficacy subgroups. The fact that the ownership mechanism remains significant even in the high-efficacy group—for whom the overall effect of the ownership treatment is slightly *negative*—suggests that the ownership mechanism can enhance accountability pressures even as the informational content of the treatment demotivates subjects.

[13]Estimates of the Average Direct Effect (ADE) and Total Effect are available in Appendix Table F.4.

Discussion and Robustness

In sum, the results imply that psychological ownership is an under-explored mechanism driving accountability pressures. From a policy-making perspective, the findings also suggest that ownership is malleable even for real-world revenue sources. In all five experiments, subjects' feelings of ownership over revenues significantly predicted demands for accountability in substantively meaningful ways. Moreover, across all five experiments, interventions that prompted subjects to believe that a share of the revenues belonged to them as citizens—i.e., assigning physical ownership in the tax condition or psychological ownership in the aid and oil ownership conditions—augmented subjects' feelings of ownership and in turn increased their accountability demands.

Nevertheless, multiple concerns might arise over the interpretation of our findings. First, the strong results in our highly controlled lab experiments may not generalize well to citizens' real-world political behavior. We thus designed the survey-based field experiment to address such concerns. Outcomes measured accountability pressures through typical political actions such as donations to good-government organizations, messages sent to leaders, and requests for further information. Given that we could not control the actual distribution and ownership of public budgets, we designed a field intervention that sought to capture the essence of the lab treatments. We did this by providing detailed, village- and household-relevant information about the amount of the public money in question and by assigning a budgeting task that encouraged subjects to imagine how the money might have affected their households had they physically controlled their per-capita shares. We styled the intervention after canvassing campaigns undertaken by civil-society organizations, and thus we intended it to serve as a basis for future campaigns.

It is important to note here that the average main treatment effects in the survey experiment were not substantively large nor significant statistically. This suggests that political action demanding accountability is difficult to manipulate in the field, even with an elaborate intervention putting subjects strongly in mind of the potential personal effects of public money. Surprising information about huge national budgets with personal implications may dishearten citizens in ways that offset ownership effects on action—a finding to which future research should attend. However, the intervention did show marked ability to increase psychological ownership over revenues. Moreover, to the degree the treatments manipulated ownership, as seen in the mediation results, the interventions also augmented accountability demands. Direct effects were also present for the low-efficacy subgroup, perhaps because the experiment presented these subjects with opportunities they typically lack to demand accountability and request additional information. As many experiments have demonstrated, it is difficult to increase public demands for accountability generally (Dunning et al., 2019; Olken, 2007; Raffler et al., 2019). In that context, the combined results from the four lab experiments plus the findings of the survey experiment regarding the effects of treatment on ownership, ownership's mediation of accountability pressures, and the direct effects for the low-efficacy subgroup, all point to promising directions for future studies.

Second, the lab experiment outcome of expressive punishment—paying monetary costs to fine greedy leaders—appears distinct from the survey-experiment outcomes involving donations to a transparency organization, messages to leaders, and requests for information. Can both measures capture accountability pressures? In our lab orientation for subjects we explicitly linked the costs paid for punishment to examples of actions that citizens take in the real world, such as voting or protesting. But the perfect efficacy of citizens' punishment of leaders in the lab does not reflect real-world political contexts, especially in low-income countries. Thus, in designing our survey experiment, we used behavioral outcomes that required subjects to pay costs in the pursuit of greater government accountability. The opportunity cost of foregoing donation to a health-care NGO in favor of a transparency organization, the inconvenience and potential exposure of contacting leaders, and the payment of SMS fees to receive relevant information all are steps citizens take when motivated to seek more accountable governance. Both the lab and survey behavioral outcomes were thus designed to capture the same underlying concept of tangible costs for accountability. The overall consistency in results between the lab and survey-based field experiments despite the distinctive outcome measures ought to be read as a notable strength in the findings. That results cohere across such distinct settings and outcomes speaks to their robustness.

Third, while our mediation analysis suggests that ownership is driving the effect on citizen action in our experiments, we cannot preclude the possibility of a confounding mediator. We address this in two ways. First, for both the lab and survey experiments we designed and deployed survey modules measuring plausible alternative mechanisms that could be activated by our treatment. For the lab experiments we measure beliefs about fairness; for the survey-based experiment we measure beliefs about the composition of government revenues, levels of corruption, and post-treatment personal efficacy (as compared to the pre-treatment efficacy measure used in the heterogeneity analysis above). We model each of these alternative mechanisms as potential mediators in the same vein as our ownership measure above. In the lab games, we find that our ownership treatments have a small, *negative* effect on the importance of fairness norms that is statistically significant only in Experiment 1, and that our ownership and fairness measures are weakly, negatively correlated. This pattern eliminates the possibility that it may act as a confounding mediator.

In the survey experiment we find no evidence that our treatments moved the corruption or efficacy measures at all and, by extension, find no mediation effects. Although we do find that our ownership treatments increase citizens' perceptions of the relative importance of aid (oil) to the Ugandan budget, which implies subjects may have thought the budget was larger in those treatment groups, we find no evidence that this increase is causally related to subjects' accountability demands as measured by our summary index. These results are available in Appendix C.2, along with a more thorough discussion of alternative mechanisms and identification concerns. Appendix C.3 also includes sensitivity tests to evaluate the likelihood that a generic unmodeled mediator could account for our results. In both the survey and lab experiments, results suggest that the unmodeled mediator would need to explain approximately two to four times as much variance as all the right-hand side variables combined. We view this possibility as unlikely but discuss it in more detail in Appendix C.3.

Fourth, we acknowledge that all study outcomes were measured shortly after treatment, so the results reported here should be read to reflect treatment effects in the immediate term. While we can only speculate about whether the effects might persist, evidence from one recent study suggests that the effects of a single information intervention can persist for at least one month (Bhandari et al., 2019). In future research, it would prove informative to conduct medium- and long-term follow-ups to learn about treatment persistence, particularly in the survey experiment. It would also be fruitful to combine long-term follow-up with "booster" doses of the treatment, which would help the treatment more closely resemble a large-scale NGO campaign to remind citizens of their meaningful claims on aid and oil funds and their ability to demand accountability for leaders' use of the money.

Fifth, concerns may arise that social-desirability bias is driving the ownership treatment results. The same enumerators inform subjects that aid funds and oil money belong to them, observe accountability demands, and administer surveys. At least two elements of the studies counter our worries here. First, the main outcomes measure behavior, which—in the lab games especially but also in the survey experiment—is costly to subjects and thus their self interest should mitigate against social desirability. Second, any bias driven by researcher demand should have similar effects across the oil and oil-framing conditions in the lab experiments. However, as shown above, the oil framing condition did not have the same effect on outcomes as the oil ownership manipulation. These considerations ought to diminish concerns about social desirability.

Sixth, some may worry that all of our experimental manipulations and measurement took place between enumerators and subjects one-on-one and in relative privacy, but real-world accountability pressures inherently require public collective action. As Olson's classic work argued at the topic's inception, initiating and sustaining collective action usually requires some kind of individually focused selective incentive (Olson, 1965). Our study identifies and provides compelling evidence for such a selective incentive built into many citizens' psychologies: the expressive benefit of punishment, which is heightened by a sense of ownership. When people believe that public money in some sense belongs to them, they gain measurable utility from punishing leaders they believe to be corrupt.[14] Such expressive punishment, as with selective incentives generally, sows the seeds of collective action.

Finally, the evidence presented thus far does not shed light on what factors, other than revenue source, might drive variation in individuals' sense of ownership. To address this, Appendix F uses covariates from our nationally-representative sample to examine the demographic and economic factors that correlate with ownership over aid, oil, and tax revenues.[15] We find that men have higher ownership than women over all revenue sources, as do wealthier respondents and those with more education.[16] Other factors—including age and whether the respondent is from an urban or rural area—do not appear to impact ownership. More research is needed to understand how individual factors give rise to ownership, and the extent to which these factors vary across contexts.

Conclusion

The results from the five experiments reported here present compelling evidence that psychological ownership—the subjective sense that public money belongs to citizens—significantly predicts accountability demands. These results are substantively large and highly significant statistically. Moreover, our experiments demonstrate that relatively simple interventions making public money appear to belong to citizens appreciably boosts their sense of ownership over the revenue. To the degree the interventions manipulate ownership, mediation analysis indicates that heightened ownership leads to greater accountability pressures on leaders.

The present study thus gives shape to an under-explored causal mechanism linking citizen psychology to demands that political leaders be held accountable for their spending.

[14] The positive utility of expressive punishment is documented in Section E.1 of the appendix.

[15] This analysis was not pre-registered and should be viewed as exploratory.

[16] Overall levels of wealth and education in our sample are relatively low; high education or wealth in this context is relative, not absolute; we have no actual political or economic elites in our sample.

It defines ownership and distinguishes it from related concepts, offers a tractable conceptual measure, and employs the measure across multiple countries and experimental designs. Our theory provides an alternative to previous accountability-enhancing mechanisms, such as the provision of information or the securing of free and fair elections. Our finding that ownership is key to accountability pressures thus suggests fruitful avenues for both researchers and activists as they seek interventions capable of encouraging citizen engagement.

Large shares of citizens feel a sense of ownership over public revenues, and as those feelings of ownership strengthen, citizens increase their expectations for good governance. They are more likely to ask, “what are you doing with my money?” When leaders fall short of the heightened expectations held by citizens with high budget ownership, those same citizens seem increasingly willing to demand accountability. This willingness appears expressive rather than instrumental, and the related psychological benefits generate selective incentives central to collective action needed for more accountable governance.

The results suggest possibilities for future research. First, our results point to avenues for practical interventions by academics and activists seeking to increase citizens’ willingness to monitor and sanction governments who misuse windfall revenues. Simple and straightforward treatments encouraging citizens to consider revenue as belonging to them may well prove effective in heightening accountability pressures. Second, as discussed above, if ownership mediates action, then this has implications for politicians’ behavior. More work is needed to determine whether, and how, politicians attempt to manipulate citizens’ ownership, and how ownership affects who benefits from state resources and redistribution. Some politicians may seek to dampen public perceptions of ownership through personal claims or repression, others might strategically seek to heighten it by invoking “your tax dollars,” and still others could skew policies toward high-ownership populations. Finally, while we show that revenue source is a key driver of ownership, and provide initial evidence on what individual factors correlate with ownership, more work needs to be done to better understand how citizens develop a sense of ownership.

The findings should prove especially informative to those interested in the resource curse, which portends dire political consequences for countries receiving substantial revenues from mineral wealth and foreign aid (Morrison, 2009; Ross, 2004). Citizens' sense of ownership over aid and oil may begin to reverse the curse to the degree that their heightened ownership motivates accountability pressures. Indeed, it is tempting to speculate that just such a sense of ownership underlies the relative immunity to the resource curse in mineral-rich countries such as Norway, the United Kingdom, and arguably even Botswana. Citizen ownership over revenues cannot cure all political ills, but our findings suggest that it could provide an essential component in motivating greater citizen pressure toward accountability.

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Appendix

Contents

A Implementation and Design

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A.1 Uganda Lab Experiments

A.1.1 Recruiting and Game Protocol

In the Uganda experiments, subjects were recruited for three enumeration sessions per day, each consisting of 16 respondents. We ran the Uganda experiments in January, June and July of 2017 using a convenience sample from Kampala. We rented a set of field sites in and around Kampala and recruited volunteers from the neighborhoods surrounding each site. Each session was randomly assigned to one of the possible treatment conditions, blocked on enumeration site.

At the beginning of each session, subjects were given a short group training, lasting approximately 10 minutes, which laid out the basic rules of the assigned game. After group training, enumerators then administered a short on-on-one training with each subject, explaining a sample round of the game and probing subjects on their comprehension of the key game steps, particularly the allocation decision. After one-on-one training was complete, subjects completed a practice round that was not payoff-relevant, then were then sent back to the group training room. After the practice round, respondents were called up one at a time to complete five single-shot rounds of the game. At the start of each round except the first, respondents were told what had happened in their pairing in the previous round, but were not told the decisions of any other respondents. Subjects were instructed not to speak about the game between rounds and were monitored at all times by project staff to ensure this rule was followed. At the end of the final round, respondents completed an outtake survey. They were then paid a show-up fee, plus their earnings from all five rounds.

Within each game session, we randomly assigned subjects to the role of Citizen or Leader at a ratio of 3 Citizens per Leader. In the first round, each Citizen was randomly assigned to a play with a Leader. During the game, each Citizen received the transfer decided by the Leader to whom he or she was assigned. In each round, leaders thus played three sub-rounds, one with each Citizen that he or she was paired with. In each subsequent round, the subjects' roles remained the same, but Citizen-Leader pairs were re-randomized. Citizens could play with a single Leader multiple times, but never twice in a row.

Similarly, an individual Citizen-Leader pair might appear more than once, but the Citizen-Leader 3-tuple—that is, the combination of Citizens with which each Leader played in a given a round—could never be repeated. This was done to reduce the possibility that the Leader observed nearly identical thresholds in back-to-back rounds and inferred (despite explanations to the contrary) that the game was repeated rather than one-shot. Our randomization algorithm took an arbitrary n subjects, k leaders, and l rounds as arguments and returned a series of pairings that satisfied the above criteria. To stress that each round was a single-shot game, in between rounds enumerators reminded respondents that the pairings would be different than in the previous round.

A.1.2 Game Design

The steps for the Tax and Grant versions are very similar to those in Martin (2014).[1] We then add two additional revenue source treatments: Aid and Oil. The basic steps for these games are the same as the Grant game, with one key difference: whereas in the Grant game the source of the group fund is not specified, in the Aid and Oil games respondents are told either that the group fund is money that was given by a donor as foreign aid, or that the money comes from Ghanaian or Ugandan oil revenues. For enumeration purposes, 1 money unit (MU) was set equal 100 Uganda Shillings (UGX).[2] All enumeration employed real coins to better convey the decisions to respondents.

The source treatments were built into the game scripts used by the enumerators as well as illustrated on the game boards. During both participant training and actual gameplay enumerators stated the revenue source each time the group fund was mentioned. In order to emphasize the treatment, enumerators placed the coins representing the group fund on a tile illustrating the source, and verbally stated the source, before moving the group fund to the leader's tile. Game boards for the remaining conditions differ only in the image on the source tile. In all three experiments, subjects first played a practice round in which the enumerator was allowed to clarify any issues that remained after training.[3]

Finally, Table 1 describes the citizen as making a punishment decision after the Leader allocates the group fund. For implementation purposes, Citizens were instead asked to make an *ex ante* decision rule; they were asked to decide which possible allocations of the group fund they would punish. This substantially increased the experiment's power. For example, in the two-player games, enumerators would start by asking the Citizen "If the Leader kept 10 MU, and gave you 0 MU, would you pay 1 MU to punish the leader?" If the Citizen replied "yes," the enumerator would keep asking for different allocations, increasing the share the Citizen receives in 1 MU increments.[4] Enumerators stopped when they received a transfer level at which the Citizen would no longer punish: this becomes the punishment threshold in the analysis below. All games were implemented using real coins to make the decisions concrete for respondents.

In the Tax Ownership and Aid Ownership games—both conducted in June and July 2017—all respondents purchased a small good for 500 UGX after they received their wage. All respondents had the same choice between four goods, each of which had a true market value of approximately 500 UGX: a bar of soap; a small bag of maize meal (posho); a small packet of cooking oil; or a small bag of rice. The items, and their price, was held constant across all treatment conditions analyzed here and thus poses no inferential threat. Respondents were generally excited about the opportunity to purchase these items; they were chosen because they are all highly valued, even in the small quantities provided. The text below provides an example of how the purchasing was explained in the group training and in each individual round of the game. The purchasing was

[1] We also retain many of the rules and constraints Martin used. These include the notion that taxes are exogenous and mandatory, preventing bargaining between Leaders and Citizens. Additionally, government budgets are constant and observable across treatments.

[2] At the time of data collection, exchange rates were one US dollar to 3,500 UGX.

[3] In Experiments 1 and 3—those which included the Tax and Aid Ownership treatments, respectively—subjects played a single practice round followed by 5 rounds of unassisted play. In Experiment 2, subjects played only 4 rounds of unassisted play.

[4] i.e. the next step would be to ask "If the Leader kept 9 MU, and passed you 1 MU, would you pay to punish?"

included to allow comparison with an additional set of treatments that compared direct taxes to a VAT on goods. These treatment conditions are analyzed in a separate paper.

Text from Group Training: Windfall Conditions
"At the start of the activity, the citizen earns a wage of 1,000 Sh… Next, the Citizen uses 500 Sh. to buy a real item. If you are a Citizen you will get to choose which of 4 items you wish to buy: Soap, Rice, Oil, Posho. [Hold up each item as you say it.]"

Say the Citizen decides to buy rice. He pays 500 Sh. to the store, and gets the rice.

Text from Individual rounds: Windfall Conditions
"Here is the wage of 1,000 Sh you have earned for this round… You now have the opportunity to purchase one of the four goods we spoke about earlier. Each of these goods costs 500 Sh. Remember that you MUST choose one."

A.2 Ghana Lab Experiment

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A.2.1 Recruiting and Game Protocol

The Ghana experiments were conducted in Accra in June and July 2016, using subjects recruited from 8 constituencies in the Greater Accra region. Chosen constituencies fell into a "low" or "medium/high" income category.[5] Each session was randomly assigned to one of the possible treatment conditions, blocked on the average income level in the constituency.

Each session of 16 was recruited from a single polling station and then provided transport to the field office, located in Adabraca, Accra. While recent representative statistics on Metropolitan Accra are difficult to obtain, a comparison with a large-scale study of 5,484 respondents from 1,250 households conducted from 2008 to 2010 (Fink, Weeks and Hill 2012) suggests that our sample performs favorably in terms of representativeness given that we did not sample from high-income areas of the city. The results for this more representative sample mirror those of the convenience sample in Uganda, reducing concerns about potential selection and the poor external validity it might produce.

Table A.1 reports the means of several socioeconomic characteristics of interest (Column 1) alongside the Fink et al estimates where available (Column 2). On balance, our sampling strategy yielded a sample that is approximately gender-balanced and considerably wealthier than a pure convenience sample. While our sample is relatively less educated, we nearly match the Fink figures for age, employment, and ethnicity. Approximately 60% of our sample was employed, with 10.7% of those employed by a firm and 11.7% as traders. Critically for our purposes, 30.8% paid some form of direct tax in the previous 6 months. The average per-month household income was 588.5 GHC, slightly higher than the inflation-adjusted average of 479 GHC reported for the Greater Accra region by Ghana's national statistics bureau in 2008. Expanding the sample of educated, high-income respondents with experience paying taxes was our primary motivation for a more rigorous sampling strategy than is often used in experimental games. Nonetheless, to the extent that our

[5]We avoid high-income enclaves of the city because of low recruitment rates during piloting, due both to the difficulty of finding high-income individuals at home during the day and also because the compensation offered was far less attractive to high-income individuals.

Statistic	Sample Mean	Fink et al. Estimate
Age	31.7	29
Female*	52.2	62.4
Employed	60.9	60.2
No Schooling	5.6	17.6
Completed Primary	30.9	21.9
Completed Secondary	23.3	52.0
Ga	55.6	42.2
Akan	31.5	31.0
Ewe	6.3	12.4
Household Income	588.5	NA

Table A.1: Summary Statistics for Experimental Sample in Ghana. The Fink, Weeks and Hill (2012) sample is highly imbalanced along the gender dimension because the sample was recruited based on participation in an earlier survey of at least one adult woman in the household. As such, these estimates should be taken as rough estimates rather than as definitive values for a representative sample.

sample in Ghana more closely resembles the broader population, this also increases the external validity of our findings.[6]

A.3 Survey Experiment

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A.3.1 Additional Design Details

In addition to our ownership treatments, described in Section Increasing Ownership in the Field, one-third of the sample was assigned to one of two possible Information placebo conditions, Aid and Oil. These conditions were designed to control for the fact that both the Aid and Oil Ownership treatments convey information to respondents. In the Aid Information placebo, respondents were told the inflation-adjusted amount of foreign aid money the Ugandan government had received over the past 10 years. To help respondents process this information, they were then told how much this would be if the government had distributed the money to every Ugandan household equally. They were told the projected amount of aid money the central government would receive in the next 5 years. All information was then re-enforced using a visual aid. The Oil Information placebo was the same, except that the information was oil revenues. A third information condition gave subjects information about tax revenues. Although these information conditions are the subject of ongoing analysis and are not discussed here, we provide here our full design.

Pure Control: This treatment provides no information to respondents, who go straight from the pre-treatment questions to outcome measures (described below).

Information Placebo Treatments (Aid, Oil, Tax): The three information treatments are designed to test whether simply giving citizens more information about each revenue source, and

[6]The figures given in Table A.1 are drawn from the entire sample, including those that played as Leaders and who are therefore dropped from the games data. The full sample figures are given to provide a more accurate comparison with the Fink estimates.

helping them process the information, affects behavior. This is important to help us disentangle the effects of ownership and information in the ownership treatments. Each of the three information treatments gives respondents information about the inflation-adjusted amount of [OIL/AID/TAX] money Uganda has received in the past 10 years. These absolute amounts are broken down by village and by household, with the amount determined by the average village and average household size throughout all Uganda. This breakdown is presented as how much the government could have given to the average village/household if they had divided the money among all Ugandans, rather than spending it as the budget. In this calculation we assume a 15% overhead cost.

Information is then given about the projected amount of future [OIL/AID/TAX] revenues Uganda will receive. This information is gone over once verbally, and then again using a board to help respondents understand and process the amounts. This helps make the treatments closer in length, and also helps control for the possibility that the Ownership treatment is also affecting information processing. In the Tax Information treatment, the first part of the module entails having respondents answer a set of questions about all the taxes they pay. They then receive the Tax information, similar to the other Information treatments. This additional step of asking questions about tax exposure is designed to make the tax treatment salient, by priming respondents on the taxes they pay, especially those (like VAT) that are typically less visible.

Ownership Treatments (Aid & Oil): The final two treatments are designed to test whether it is possible to increase respondents' sense of ownership over aid and oil revenues. The treatment consists of the following steps:

Aggregated Information. Each respondent first receives a variation of the text of the [Aid/Oil] information treatment. The key difference is that the household and village amounts are personalized according to the respondents' household and village size: we tell respondents how much their actual household or village would have received if the government had divided up the money equally to all Ugandans, rather than spending it as part of the budget. In calculating this we assume a 15% overhead rate. We also make it very clear that we are talking about "your" village and household.

Tailored Information. Respondents are given information about what the money given to their household (and village) could have purchased. For household, we focus on valued consumption and investment goods. For villages, we focus on important local public goods.

Budgeting Task. Subjects are then asked to think about how they would have spent the household-level amount of money. Subjects engage in budgeting task in which they "spend" the household-level amount, with enumerators writing down the items they would purchase as well as the amount they would spend on them on a small card. This card is then placed on a budgeting board. Subjects may "purchase" up to 8 unique items in this task. Our pilot data suggests that most subjects purchase between 4 and 6 items, with the first purchases being heavily weighted toward large capital investments in business and farming inputs. At the end of the task, enumerators place each item into one of eight expenditure categories and record the category and the amount on the tablet. They also take a picture of the cards as they are arranged on the board to allow for individual items to be coded by-hand at a later date. At the end of the budgeting task, enumerators give respondents a summary of the information and their choices in the budgeting task.

Future Revenue. Subjects receive additional information about total amount of revenues from AID/OIL that will come to government in the future, given in present, absolute terms (assuming a future discount rate of 6% per annum) and also broken down by village and household size.

A.3.2 Personalized Information Treatments

In the Aid and Oil Ownership treatments, the per-village and per-household figures were adjusted according to the size of their household (self-reported) and their village/LC1 unit (collected from the LC1 prior to enumeration). This makes the treatment very realistic, and means that the budgeting task is always consistent in terms of amount per person. It also furthers the goal of giving subjects ownership over non-tax revenues by giving them information in a form that encourages them to think about the revenue as their own. While the amount of revenues that accrue to each respondent in the survey will vary by their household and village size, this has no inferential consequence because household size are balanced under randomization, and the treatments are given at the respondent level, ensuring that treatments are also balanced across village size.

Given the highly specific information given to subjects, we made a special effort to get accurate measures for all revenue sources and to be very explicit about the use of inflation-adjusted measures, which avoided understating the amount of revenue. There are also policies in place in other countries that closely resemble the counterfactual we are simulating with the direct transfer; these are conditional and unconditional cash transfers for aid, and direct transfers of resource profits from governments in the case of oil revenues. Nonetheless, the treatments were designed to make it as clear as possible that the village, household and individually-denominated shares are what would happen if the government distributed this money equally, not the amount they would actually get if the government initiated a program to distribute money. When calculating these figures, we did not assume that the government could implement what amounts to a large-scale policy of targeted redistribution without substantial cost. Benchmarking against data on the implementation costs of similar programs that are currently in operation elsewhere, we discounted the total revenues by 15% to account for administrative fees.

A.3.3 Example Treatment Text

Village-Level Revenue Text

If the Ugandan government had distributed this money equally all villages, your village's share of the 3.8 trillion shillings in oil money would have been [BLANK] shillings. This [BLANK] would have been enough to provide your village the following things: 1. Free medicine and supplies to a health clinic near you for [BLANK] years OR 2. Buy textbooks for [BLANK] children in your area OR 3. Drill [BLANK] new boreholes in your area OR 4. Build [BLANK] brand new primary schools in your area OR 5. Grade [BLANK] km of roads in your area

Household-Level Revenue Text

Earlier, you told us that there were [BLANK] people in your household. If the money from oil revenue over the last 10 years was shared equally among all Ugandans, your household's share of the oil money would be [BLANK] shillings. This [BLANK] shillings would be enough to provide your household with the following things: 1. Primary school fees for a child in your household for [BLANK] terms OR 2. [BLANK] young goats for your household OR 3. [BLANK] pesticides (28kg bag) for your household OR 4. [BLANK] seeds (maize; 5kg bag) for your household OR 5. [BLANK] full grown chicken for your household OR 6. [BLANK] SolarNow Solar panel for your household OR 7. [BLANK] bicycle for your household

Future Revenue Text

Now I'd like to give you some information about revenues from oil that will come to the Ugandan government in the future. The Ugandan government is projected to get at least 8 trillion shillings from oil revenue in the future. Although the government will decide how to spend this money, revenues from the recently discovered oil belongs to all Ugandans and is supposed to be used to benefit you and your family. If the government distributed this money equally, your household's share of the oil money would be at least [BLANK] shillings.

A.3.4 Efficacy Measures

Efficacy measures were asked pre-treatment. We targeted three dimensions of efficacy: internal efficacy, external efficacy, and government efficacy. The efficacy index thus measures subjects' generalized sense that an action is both feasible and likely to be successful, two necessary conditions for political action. To construct our inverse-covariance weighted efficacy index, we asked subjects to rate their agreement with the following items on a 10 point scale where 0 meant strong disagreement and 10 meant strong agreement:

1. I have a good understanding of the important political issues facing our country.
2. I consider myself well qualified to participate in politics. People like me don't have any say in what the government does.
3. No matter whom I vote for, it won't make a difference. My government does not favor any group of people when making decisions.
4. My government makes decisions based on what citizens want.
5. My government is good at delivering important services, like healthcare and education, to citizens.

A.3.5 Behavioral Measures

Here we include the text of the four behavioral measures with which we measure the effectiveness of our treatment (both directly and as mediated by the ownership mechanism).

Contact Measure

As part of our research, we are also required to deliver a summary of some of our findings to officials in this district. This summary is just general information. It does not include any information about the people we talk to. However, as part of this project, we will also be speaking to politicians and government officials from your area and constituency. We would like to give you the chance to include a message for them if you are interested. We will be collecting these messages from all the people we talk to in your area. If you wish to leave a message, we will combine it with the other messages we receive and deliver them together. Is there an official you would like to send a message to? This message will be completely anonymous; it will not include your name or any other information about who you are or where you are from. Nobody will be able to know that you will have left these messages if you choose to do so. Would you like to leave a message?

Report Participation

As part of this project, we are also conducting interviews all over the country with all Ugandans asking them about various topics. When we are finished with our project, we will be producing a report that includes some information about what we have found. We are also giving participants a chance to learn about some of the results through SMS if they are interested. Would you like to receive a SMS with a small selection of survey results, in your own language?

SMS Measure

As part of this project, we are partnering with a non-profit organization that provides citizens with information about how the government spends the money it gets. One of the services this organization offers is sharing information about government spending through SMS. Would you be interested in signing up for this platform? If you don't have a phone with you at the moment or don't have enough credit right now, I can help you sign up using my phone or the phone of someone from your household.

Donation Measure

Thank you. As a token of our appreciation for talking with us today, the researchers conducting this survey would like to donate 1,000 UGX to a Ugandan NGO on your behalf of you and other people taking this survey. We would like to give you the opportunity to choose between two different options. The first organization is an NGO that helps Ugandans get access to healthcare and medicine. The second organization is an NGO that focuses on fighting corruption and improving governance. Which organization would you like to donate to?[7]

A.3.6 Sampling Strategy

Our sampling strategy was a modified area probability sample in which we intentionally over-sampled urban areas. We did so because our population of interest is those Ugandans who are the most likely to take political action. The characteristics of this population have countervailing effects on their responsiveness to treatment. On the one hand, city-dwellers are more likely to be informed about government behavior, meaning that the informational content of the treatment will be less valuable. They may also have stronger feelings of ownership, making our ownership treatments less effective. On the other hand, urban citizens tend to be wealthier, better-educated and more efficacious, making it more likely that they are willing to take costly political action and that they would believe it is valuable to do so.

At the same time, however, the effect of the treatment on more rural respondents is also of interest. As such, we split our sample between municipalities—a special administrative designation reserved for urban areas—and non-municipalities. In each of ten districts, split across Uganda's four regions proportional to their respective shares of total population, we sampled one municipality and one non-municipality, both of which are counties. In the urban county (the municipality), our sampling frame was the universe of polling stations present in the 2016 elections. We then binned polling stations into quartiles according to the number of registered voters.[8] In municipalities, we draw 8 polling stations, taking 2 from each quartile to ensure that we cover the polling-station size distribution, which is highly correlated with an area's level of urbanization. In non-municipalities we draw 4 polling stations, one from each quartile. Sampling at the PSU level is random walk starting from the polling station.

[7] To avoid priming subjects, we omitted the names of the non-profit organizations which we had partnered with. Enumerators were instructed to give them this information if they asked for it.

[8] The number of registered voters is an excellent proxy for total population, which is not available at the polling-station level. We verified this by aggregating the number of registered voters up to the district level—the lowest unit for which reliable population data was available—and examining the correlation between the total number of registered voters and the district population.

Respondents were selected according to their head-of-household status, with the goal of a two-thirds/one-thirds split. We define head of households for our purposes to be the male or female that is responsible for making financial decisions and/or handles household expenditures. Once enumerators reached each residence, sampling is done using a randomly selected respondent card that could be one of four types: male household head, female household head, male non-household head, and female non-household head.

A.3.7 Calculation of Revenue Totals and Personalized Information

All past revenues (both aid and oil) were calculated with inflation to avoid understating the amount in current shillings, with 2008 taken as the anchor year for the inflation index. All future revenues were calculated assuming a 6% discount rate up until 2018 (the year of the intervention). In both cases, per-capita figures were calculated using 2017 nationwide population. The village-level figures were scaled according to village population, which was obtained from the LC1 (Village Chief) at the start of each enumeration day. This population was then entered on the tablet, after which point the survey software calculated the village-level amount and piped it in to the treatment vignette. Household-level figures were based on subjects' answer to a survey item on household size. We reduced these amounts by a flat 15% to account for the administration fees that would be necessary to run what amounts to a large-scale unconditional cash tranfer program. These figure was chosen conservatively based on a survey of similar programs already in existence.

Figures for total past oil revenue use 2.7 trillion UGX as a baseline, a number that is widely cited in media reports. Annual oil revenue calculation is based on media reports of all known payments as well as the 2015-2017 Petroleum Fund Statements available from the Ugandan Ministry of Finance. The total inflation adjusted figure for the past 10 years was approximately 3.8 trillion UGX. The total net per-capita value of past oil revenues was 88,955 UGX. Under estimates of the average village and household size (1,238 and 4.7 respectively), this amount would yield a figure of 418,090 UGX for an average households and approximately 110 million for a village. Future oil revenue was based on estimates by the Natural Resource Governance Institution.[9] Past aid figures were based on several years' worth of Government of Uganda Annual Budget Performance Reports and the World Bank Development Indicator dataset (2018). Once administrative costs were taken into account, the per-capita amount of aid revenue that would have accrued to each Ugandan was approximately 300,000 UGX. This figure yields a household-level amount of approximately 1.4 million UGX and a village-level amount of approximately 378 million UGX. Future aid revenues were based on Government of Ugandan Annual Budget Performance Reports, which provide projected grant revenues up to 2021. Given the relatively recent OPM scandal and a general drawing down of on-budget aid in recent years, we assume that revenues will decline 15% each year starting from 2022 onwards.

In addition to the village-level denominations, subjects were also given information about important village-level public goods that could have been provided given their village size. The per-unit costs for these goods—which were then used to calculate the total number that could have been provided—were based on publicly available data sources. Data on the cost of grading 1km of road was particularly difficult to obtain. In the end, we chose an estimate provided by the African Development Bank in a study of road infrastructure costs. Figures for the costs of an ambulance were taken from an article in a prominent Ugandan newspaper, the Daily Monitor, which gave an estimate for the purchase of 100 ambulances over five years. Estimates for educational equipment were taken from estimates available on Aid for Africa's website. Estimates for the cost of a borehole

[9] The full report is available as a PDF from the Civil Society Coalition on Oil an Gas website.

were taken from Return Hope International, which estimates a per-well cost of approximately 8,000 USD. Estimates for the construction of an HC-III health clinic and the costs of critical medications were taken from reporting on the financial shortfalls in the Uganda Ministry of Health.

A.4 Evolution of Theory and Analysis Plans

This project began during our data collection in Ghana in 2016. When we designed our experiments we were interested in the idea of resource ownership, but the overall project was more focused on the institutional differences between how aid, oil, and taxes are used. We also did not theoretically separate loss aversion and ownership, rather considering them both parts of the "endowment effect". We did include treatments in the 2016 Ghana experiments that we thought of as ownership treatments. However, these were very general and did not in fact assign ownership to citizens, rather told citizens that public budgets were meant to benefit them, similar to the language in the "oil framing" treatment conducted in Uganda. These treatments had null effects on both the ownership measure and willingness to punish in the lab games, suggesting that they were not truly ownership treatments.

When we ran the Uganda January 2017 experiment, which included the Oil Ownership condition, we had not fully fleshed out our ownership theory, but did further develop and focus on the Oil Ownership treatment. We framed our experiment around the possibility of inducing higher ownership over non-earned revenues as a way to reduce the resource curse. In the pre-analysis plan for the Oil Ownership experiment, we explicitly say that we expect the Oil Ownership condition to induce the same level of punishment as the Tax game, and that we expect ownership to predict punishment within each treatment condition. However, the mediation analysis was not pre-specified.

Before we ran the Uganda July 2017 laboratory experiments, which included the Tax Experiment and the Aid Ownership Experiment, we had fully fleshed out our theory. We pre-specified that we expected to find higher ownership in the Tax condition, relative to the three Windfall conditions. We did not pre-specify the mediation analysis. However, the PAP for the July 2017 data collection does not include the aid ownership conditions. We decided that because the analysis would be the same as for the January 2017 experiment, we did not need an additional document. Following this, our analysis is consistent across the Aid and Oil Ownership conditions.

We pre-registered a more extensive set of hypotheses for the Uganda 2018 national survey. These included that we expected ownership to predict behavior within each treatment condition; that ownership-inducing treatments should increase political action, and that this should be mediated by the ownership treatment. We also pre-registered how we would test for potential alternative mechanisms, namely perceptions of the budget and corruption perceptions. Finally, we say that we expect larger treatment effects among household heads, and that we think those with high personal efficacy should have larger treatment effects.

To summarize, below is a list of the relevant pre-specified tests for each experiment. This is not exhaustive – tests and hypotheses not directly related to this paper have been excluded for space reasons, but are available in the EGAP registries for both the lab-in-the-field and survey experiments.

- Ghana 2016: Lab Experiments on Taxation

  1. We expected to find higher ownership for oil than aid.
  2. We expected to find higher ownership and punishment in those assigned to a set of "valence" treatments that told respondents that public monies should be used for their benefit.

- Uganda January 2017: Oil Ownership Experiment
  1. That the Oil Ownership treatment will increase punishment, making it equal to Tax.
  2. That the oil ownership treatment will increase ownership over the group fund.
  3. That we expect to see higher ownership predict higher punishment thresholds within each condition.
- Uganda July 2017: Tax Experiment and Aid Ownership Experiment
  1. The Aid Ownership treatment was not included in the PAP filed at this time, as we analyzed it following the same process as the Oil Ownership data.
  2. For the experiments covered by the PAP, we pre-specified that we expected higher ownership in the Tax condition, compared to the Windfall groups.
  3. Mediation analysis was not pre-specified.
- Uganda July 2018: National Survey
  1. We expected the ownership treatments to increase the ownership measure.
  2. We expected the ownership treatments to increase willingness to take action in the ICW index.
  3. We pre-specified that ownership should mediate any increase in action.
  4. We wrote that we expected larger effects among household heads, as the budgeting task would be more effective among those with experience managing household funds. (This had become apparent during piloting).
  5. We specified that we expected larger treatment effects among those with high efficacy.
  6. We specified that we expected larger effects when household size is larger (Results from this test are not included in the main paper. We have run these and find no significant differences by household size.)

B Connecting Lab Measures to Real-World Outcomes

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One potential criticism of the lab results is that our two primary quantities of interest—ownership and willingness to punish—may be only weakly related to their real-world counterparts, and that the strong relationship between ownership and accountability pressures may not exist. We address this criticism in part through the survey experiment presented in Section Increasing Ownership in the Field. The survey experiment, however, is conducted on a different and broader sample than the lab games, which were confined to the capital city and its relatively more urban, knowledgeable, and wealthier residents. Here, we exploit a battery of questions included in the outtake survey of Experiments 1 and 3 that included measures of ownership over real-world revenues as well as a battery designed to capture subjects' theoretical willingness to sanction misbehavior. Unlike punishment in the lab, the latter measure is not economic costly. Nonetheless, the availability of both lab- and real-world measures of ownership and punishment behavior for the same sample allows us to (1) examine whether the correlation between ownership and punishment behavior holds outside of the lab and (2) validate our lab measures by evaluating whether there is a correlation between ownership and punishment in the lab and their real-world analogues.

To investigate whether this is the case, we use responses to a battery of questions that were included on the outtake survey for Experiments 1 and 3. In addition to the sample on which the experimental results are based, we also include responses from an additional 710 subjects who received other experimental treatments that are part of an ongoing project on indirect taxation and are not analyzed here. In addition, we include the 142 subjects who, because they played as Leaders and did not set thresholds, do not appear in the experimental data presented in Section Lab Experiments: Results. This substantially larger sample (total $n = 1,423$) improves power while also giving us better geographical coverage, including as it does several additional field sites.

B.1 Measures of Ownership and Punishment Outside the Lab

Both the ownership and punishment measures were designed as observational analogues to our experimental measures. The ownership items were identical to the one asked in the lab, except that they replaced the word "group fund" with "government budget" and gave the source of the revenue directly. The question thus asked subjects to indicate their agreement with the statement that "the government revenues from [AID/OIL/TAXES] belong to the citizens of Uganda." Each subject was asked this question for each of the three revenue types, but the order of the three was randomized. We then construct a summary index, Ownership, that takes the simple weighted average of these three individual items.[10]

We measure willingness to punish with four items that ask subjects to report the likelihood that they would engage in increasingly costly political behaviors in the event they learned that government officials had been misusing revenues. Like the ownership items, these were all measured using a 10-point scale, with a 0 corresponding to a zero probability ("I would never do this") and a 10 corresponding to certain probability ("I would definitely do this").[11] The behaviors subjects were asked to consider were (in order of costliness): contacting a local leader, calling a neighbor,

[10] All results reported in this section are robust to using alternative constructions of this variable, including inverse covariance and principal component weighting.

[11] The scale used for these and all 10-point questions was a "ladder" in which subjects were given anchors at either end and told to place themselves on the ladder.

participating in a protest, and working on a political campaign. We include the exact wording of both measures below:

Measuring Ownership

How much do you agree with the following statements:

1. The government revenues from foreign aid belong to the citizens of Uganda.
2. The government revenues from oil belong to the citizens of Uganda.
3. The government revenues from taxes belong to the citizens of Uganda.

Note: the response options were on a 10-point ladder where 0 was marked "strongly disagree" and 10 was marked "strongly agree".

Measuring Punishment

Say that you heard rumors that a leader in your community has been stealing money. How likely would you be to do each of the following:

1. Talk to your neighbors about this.
2. Go to a protest.
3. Campaign against the official in the next election.
4. Contact the official about your concerns.

Note: For all four questions, answers were recorded on a 10-point scale where 0 was "I would definitely not do this" and 10 was "I will definitely do this".

B.2 Results

As an initial check of the external validity of our laboratory data, we regressed each experimental measure on its survey-based counterpart, controlling, as in Section Lab Experiments: Results, for the respondent's treatment condition. We also include fixed effects for subject role (i.e. Citizen or Leader). This latter variable accounts for any differences in response that could arise as a result of playing the game as a Leader instead of a Citizen. The results reveal that both experimental measures are strongly correlated with their corresponding survey measure. This constitutes strong evidence of construct validity and suggests that the experimental outcomes are measuring the same concepts as the survey-based outcomes introduced above.

We next test whether subjects' ownership over real-world revenue sources is correlated with their willingness to punish. To do so, we estimate an OLS model identical to that used in the experimental test of Hypothesis 1.

The results, reported in Column 1 of Table B.1, show that a one-unit increase in ownership corresponds to a 0.104 unit average increase in willingness to engage in political activity.[12] This

[12]Here we leave the ownership measure on its original 10 point scale so that its scale matches that of our theoretical willingness to punish index.

	DV: Pr(Take Action)				
	Index	Campaign	Protest	Neighbors	Contact
Ownership	0.104***	0.084**	0.099**	0.114***	0.121***
	(0.0261)	(0.0378)	(0.040)	(0.038)	(0.038)
Enumerator FE	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
Observations	1393	1392	1393	1393	1393
Adjusted R^2	0.142	0.142	0.090	0.088	0.141

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.1: Impact of Ownership on Theoretical Willingness to Sanction. Standard errors in parentheses.

result is robust to weighting the contribution of a subject's ownership over a given source by the size of the budget they believe comes from that source. Columns 2-5 demonstrate that the result also holds for all constituent items of the index variable.

Ownership is strongly related to willingness to punish misuse of government revenues, but what drives ownership? The experimental results in Section Lab Experiments: Results supports our argument that direct contribution to the government budget via taxation should increase ownership. To test whether this is the case observationally, we estimate the following OLS model:

$$Y_i = \alpha + \beta \text{Tax}_i + \boldsymbol{\gamma} \mathbf{X}_i + \epsilon_i$$

where Y_i is subject i's ownership over government tax revenues and Tax is a binary measure that takes 1 if the subject reported paying an income tax in the past year and 0 otherwise.[13] $\mathbf{X}_i$ is a vector of covariates identical to those used Equation B.2. The results reveal that taxation is a strong and significant predictor of ownership over tax-based revenues, with taxed subjects reporting ownership values 0.413 points higher, on average, than untaxed subjects ($p = 0.0003$). Not only do taxed subjects report higher ownerhsip over tax-based revenues in the real world, they also report stronger ownership in the lab. Taken as a whole, the consistency between our lab measures and their real-world analogues strongly suggests that the lab games are activating the desired mechanism and that subjects were internalizing their roles as Citizens when choosing their thresholds.

Finally, we show that taxed citizens respond more strongly to the Tax Ownership condition than do non-taxed citizens. Table B.2 breaks down the effect of taxation on punishment by whether the respondent reported paying at least one direct tax in the outtake survey. While we find a positive treatment effect in both groups, the effect of taxation in the lab on subjects who pay direct taxes is more than twice as large as those with no experience paying direct taxes. This suggests a strong

[13] As one might expect, payment of direct income taxes is relatively rare in Uganda; only 14.4% of subjects reported paying an income tax in the previous 12 months. Given that this is a heavily urban sample, the national rate is likely to be substantially lower.

connection between subjects' real-world experience and their behavior in the lab: subjects' who have experience paying direct taxes respond much more strongly to the Tax Ownership treatment than those who do not.

	DV: Subject Threshold		
	Full	No Income Tax	Paid Income Tax
Tax Ownership	0.413***	0.344***	0.837**
	(0.113)	(0.121)	(0.316)
Enumerator + Round FE	✓	✓	✓
Number of Obs	2050	1775	275
Adjusted R^2	0.156	0.156	0.322

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.2: Heterogeneous Response to Tax Ownership Treatment by Income Tax Status. Column 1 reports the full-sample estimates for all subjects who answered the question on taxation. Column 2 reports the effect of the Tax Ownership treatment on those who did not report paying an income tax in the previous six months. Column 3 reports the effect of taxation for those who paid a tax in the past six months. While the number of subjects paying a tax was small, the effect of the Tax Ownership treatment on them is nearly three times that for non-taxpayers.

C Mediation Analysis

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Because treatment assignment is random, subjects' strength of ownership over the group fund is therefore a function of an endogenous, intrinsic ownership and the exogenous ownership induced by our three ownership treatments. We exploit this exogenous variation through mediation analysis, taking ownership as the mediator through which our ownership treatments may increase subjects' punishment thresholds. Under this design, the effect of ownership is causally identified as long as the sequential ignorability assumption holds. We discuss this assumption in greater detail below, and also show provide evidence that other mechanisms that could violate this assumption do not mediate the effect of our ownership treatments.

Mediation analysis decomposes the treatment effect into a direct effect—in this case, the effect of our ownership treatments—and the indirect effect of ownership on transfer thresholds. The key quantity of interest is the Average Causal Mediation Effect (ACME), which measures the extent to which the exogenous increase in ownership (caused by taxation) influences punishment behavior. We expect that while the Average Direct Effect (ADE) of the tax treatment may remain significant, a substantial portion of Total Effect (TE) should come indirectly through the effect of taxation on ownership.[14]

To estimate the causal effect of ownership on punishment behavior, we specify the mediator model as

$$\text{OwnershipMeasure}_i = \alpha + \beta\text{OwnershipTreatment}_i + \boldsymbol{\gamma}\mathbf{X}_{ij} + \epsilon_i$$

and the outcome model as:

$$\text{AccountabilityMeasure}_{ij} = \alpha + \beta\text{OwnershipTreatment}_i + \delta\text{OwnershipMeasure}_i + \boldsymbol{\gamma}\mathbf{X}_{ij} + \epsilon_i$$

We specify similar mediation models for both the lab and field experiments with two key differences. First, the lab games included a clustered standard error to account for the fact that a single subjects plays multiple rounds and thus we expect errors to be correlated within-subject. For the survey experiment mediation models classical standard errors are used. Similarly, the control vector $\mathbf{X}_{ij}$ is indexed by round (j) because, as in the main text, we include the lagged (i.e. previous) transfer from the Leader. The dependent variable in the outcome model is also indexed by round (j) in the lab games. In the survey experiment, where each subject appears only once in the data and the dependent variable is the inverse-covariance weighted of our four behavioral measures, the outcome model is indexed only by i.

[14] The causal setup of mediation analysis is similar in spirit to that of instrumental variable estimation, but differs in one critical respect: instrumental variable approaches require that the treatment affect the outcome only through the mediator. In an experimental context, this is equivalent to saying that the treatment cannot have a direct effect on the outcome. This is a stronger (and far less defensible) assumption than that of causal mediation analysis, which allows a direct effect and requires only that the causal mediator of interest is uncorrelated with any omitted mediators. As we show in the main results, there is a substantial direct effect of the treatment in the lab results, making our ownership treatments a poor candidate instrument for estimating the impact of ownership on punishment behavior in a two-stage least-squares framework.

C.1 Lab-in-the-Field Experiments

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Table C.1 shows that the strength of a respondent's ownership over the group fund accounts for approximately half of the overall effect in two of our three ownership treatments. Because the mediation effect comes only from the exogenous change in ownership induced by our ownership treatments, it is causally identified under the sequential ignorability assumption, which stipulates that there must be no omitted mediator that is positively correlated with both punishment and ownership. While this assumption is strong, it is more defensible in an experimental context, where the only plausible alternative mechanisms must be activated differentially by treatment condition. The most plausible alternative candidate mechanism is that of fairness. If, for example, our ownership treatments caused subjects to see division of the group fund more as a matter of fairness than in the windfall conditions, and if stronger fairness norms lead subjects to ask for larger transfers, it is possible that fairness could act as a confounding mediator. To test whether our ownership measure could be proxying for a fairness mechanism, we examined whether fairness considerations differed across treatment groups or predicted subjects' punishment behavior. We measured fairness with a survey item that asked subjects to indicate the importance of fairness in their allocation decisions. The exact wording of the fairness question asked citizens to rate the importance of "whether the allocation of the group fund between me and the Leader was fair" on a 4-category Likert scale ranging from 0 (not at all important) to 3 (very important).

	DV: Subject Threshold					
	Experiment 1		**Experiment 2**		**Experiment 3**	
	Estimate	p-value	Estimate	p-value	Estimate	p-value
Causal Mediation Effect (ACME)	0.190***	0.006	0.042*	0.074	0.240**	0.016
Direct Effect (ADE)	0.244***	0.002	0.375**	0.022	0.162	0.140
Total Effect	0.434***	0.000	0.416**	0.014	0.401***	0.004
Observations	2075		1932		1025	

Note: *p<0.1; **p<0.05; ***p<0.01

Table C.1: Subjects' Feeling of Ownership Mediates Effect of Ownership Treatment on Punishment Threshold. For each of the three experiments, we report the ACME, ADE and Total Effect as estimated by the `mediate` package in `R`. The quantity of interest, the ACME, is given in Row 1 and its expected direction is positive. Results from Experiment 1 (Columns 1-2), Experiment 2 (Columns 3-4), and Experiment 3 (Columns 5-6) show that the ownership mechanism accounts for approximately half of the total effect of the ownership treatment.

In all three experiments, we find that fairness considerations are in fact negatively affected by our ownership treatments. In only one of the three experiments—Experiment 1—was the negative relationship between treatment and fairness statistically significant. Furthermore, the correlation between our ownership and fairness measures is weak and often negative (ρ = -0.21, 0.005, and -0.41 in Experiments 1, 2 and 3 respectively). Since any potential confounding mediator is must be positively affected by our ownership treatment and positively correlated with both our ownership measure and punishment thresholds, the constellation of results above precludes the possibility that a fairness mechanism is acting to confound our results.[15]

Another potential omitted mediator is that the treatments change perceptions of budget size. While this is plausible for the 2018 survey-based experiment (see discussion in next section), we can rule it out in the lab experiments: because the size of the budget is fixed at 10 MU in all treatment conditions, and as the budget size is visible to citizens, our ownership treatments should not be able to shift perceptions of budget size.

C.2 Alternative Mediators in Survey Experiment

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The identifying assumption of mediation analysis for the survey experiments is sequential ignorability, which requires that the treatment is randomized and that other, unmodeled mediators do not act as confounders. A violation of sequential ignorability would occur if there exists an unmodeled mediator that is (1) affected by our ownership treatments; (2) causally related to our ownership measure; and (3) predicts punishment behavior. In substantive terms, such a violation would require that there exists another mechanism connecting our ownership treatments and punishment behavior, and that this mechanism is also causally related to ownership. This means that many other mechanisms that might affect accountability are only a threat to inference if it they are plausibly correlated with subjects' ownership.

We consider here three candidate mechanisms: subjects' self-reported efficacy (*Efficacy*), perceptions of corruption (*Corruption*), and the change in their beliefs about the contribution of aid (oil) revenues to the overall budget (*Info*).

- The information measure is constructed using a question that asks subjects to allocate 10 coins to represent how much of the budget comes from four sources of revenue: taxes, aid revenues, oil revenues, and debt. We use as the mediator the number of coins chosen for the revenue of interest (i.e. aid in the aid treatment). This measures whether the treatments primed respondents to think that the revenue in question is more important in the budget.

- The corruption measure is an average of four items that asked, after treatment but before the outcome module, to rate the extent of corruption at three different levels of government on a 10-point scale. The levels were: national, district, and sub-county. The corruption mediator directly captures whether the treatment made respondents believe corruption is higher, which could lead to higher punishment.

- The efficacy measure is taken from a post-treatment item asking subjects how much of a difference they can hold leaders accountable for misuse of government revenues. If the ownership treatments increase efficacy, this could then make citizens more likely to take action for reasons other than higher ownership.

One possible mediator that is not directly captured above is a respondent's belief about the total size of the budget. If the treatment makes respondents think that the government has more revenue available, this could increase both ownership and punishment. While we do not have a direct measure of budget size perceptions, we can address this with our general sensitivity analysis (see next section), and two of the mediators described above can serve as partial proxies for budget size.

[15] The code that produces these results is contained in Appendix E of the replication archive.

First, if the aid (oil) treatments lead respondents to believe that there is more aid (oil) money than previously thought, without affecting beliefs about other revenue sources to the same extent, then our information measure is a reasonable proxy for beliefs about budget size. However, this will not be a good proxy if our treatments affect beliefs about all revenue sources equally, or if the treatments do not alter perceptions of budget size, only the composition of the budget.

For this reason we also look at corruption perceptions as a second proxy for budget size. If subjects are updating their beliefs about the total size of the budget, but not levels of public goods provision, then they will infer higher levels of corruption given existing levels of public goods provision. As our treatments provide information about existing revenues but not existing levels of public goods provision, this seems reasonable.

For each of the three potential mediator measures, we estimate a separate AMCE to examine whether it is possible that some portion of the treatment effect is coming through one of the respective mechanisms. If the AMCE is statistically and substantively small, it cannot act as a confounding mediator. The results are reported in Table C.2 below. In all cases we observe that the substantive effects of these mediators are extremely weak. We also recompute the ownership AMCEs reported in Table 6 in the presence of each of potential confounding mediator via the `multimed` function of the `mediation` package, which allows the inclusion of a second mediator alongside ownership. Their inclusion does not alter the effect of ownership.

	Aid Ownership		Oil Ownership	
	Estimate	p-value	Estimate	p-value
Efficacy Mechanism				
Causal Mediation Effect (ACME)	-0.001	0.882	0.000	0.970
Direct Effect (ADE)	0.055	0.520	0.008	0.932
Total Effect	0.055	0.532	0.007	0.932
Observations	558		556	
Corruption Mechanism				
Causal Mediation Effect (ACME)	0.003	0.832	-0.016	0.372
Direct Effect (ADE)	0.051	0.530	0.030	0.730
Total Effect	0.054	0.530	0.015	0.882
Observations	558		555	

Information Mechanism				
Causal Mediation Effect (ACME)	-0.014	0.364	-0.001	0.960
Direct Effect (ADE)	0.046	0.618	-0.018	0.876
Total Effect	0.032	0.704	-0.019	0.868
Observations	504		445	

Table C.2: Mediation Analysis with Alternative Mechanisms. Top panel reports the results of a mediation analysis identical to that conducted for the ownership mechanism, but with a post-treatment measure of efficacy. Middle panel reports results taking as the mediator the simple weighted average of three items asking about corruption at different levels of government. Bottom panel reports estimates taken from an item that asked respondents to allocate 10 coins across what they viewed as the most important sources of government revenues. The information measure is the proportion of coins allocated to the source of revenue about which they received information. The ACMEs for all three mechanisms (Row 1 in each panel) are substantively small and insignificant.

C.3 General Sensitivity Analysis for Unmodeled Mediators in Lab and Survey Experiments

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In both the lab games and the survey experiment the interrogation of plausible confounding mediators reveals that our mediation results are unlikely to be driven by other mechanisms that could be reasonably correlated with both ownership and our outcomes of interest. It is possible, however, that there exists an unmeasured mediator that does fulfill both of these conditions. Here, we examine this possibility with general sensitivity analysis in both the lab and survey experiments. The sensitivity analysis is conducted via the `medsens` function in R's `mediate` package.

Because the common quantity of interest in sensitivity analysis, ρ is not easily interpreted in substantive terms, we focus on how jointly predictive the unmodeled mediator would have to be of both our ownership measure and our behavioral outcomes — in the lab games this is subjects' punishment thresholds and in the survey experiment it is the behavioral index. As the correlation between the unmodeled mediator and our ownership or outcome measures increase, the ACME of ownership will be driven towards zero. In Figures C.1 and C.2 , we plot four sets of indifference curves that visualize this dynamic. The two panes in Figure C.1 are from the Tax Experiment and Aid Ownership Experiment[16], while the panes in C.2 show the same analysis for the Aid and Oil Ownership treatments in the survey experiment. In each, the indifference curves show all the possible combinations of residual variance in the outcome and mediator models that would have to be explained by the unmodeled mediator to drive our result to zero.

As would be expected given the controlled setting of the lab, any unmodeled mediator would have to be extremely predictive of both subjects' punishment threshold as well as our ownership mediator. As the middle (bolded) indifference curve demonstrates, the unmodeled mediator would need to explain approximately 50% of the residual variance in both the outcome and mediator models in the Tax Experiment, or approximately 90% of the residual variance in one and 30% in the other. The result is even starker in the Aid Ownership Experiment, where the unmodeled mediator would have to explain approximately 55% of the residual variance in both models. We view this possibility as extremely unlikely given that the most plausible confounding mediator, fairness, has an AMCE indistinguishable from zero.

The strength of the results in the survey experiment are considerably weaker, which is to be expected given the difficult of predicting real-world political behavior. In the Aid Ownership treatment, the unmodeled mediator would have to explain approximately 10% of the residual variance in one model and 20% in the other. In the Oil Ownership treatment, the equivalent figures are approximately 15% and 20% respectively. However, unlike the lab games, the R^2 of the outcome and mediator models range between 0.05 and 0.075. This means that the unmodeled mediator would have to explain between two and four times more variation than all the right-hand side variables combined. We view this possibility as very unlikely given the inclusion of several important covariates in the mediator model and of the ownership mediator itself in the outcome model.

[16] We cannot compute the equivalent quantities of interest for the Oil Ownership Experiment due to the use of ordered logistic regression in the mediator model. As mentioned in the maintext, the Oil Ownership Experiment uses a four-category ownership measure that was recoded to three categories for interpretability. Unfortunately, `medsens` function does not currently implement sensitivity analysis when the mediator model uses ordered logistic regression.

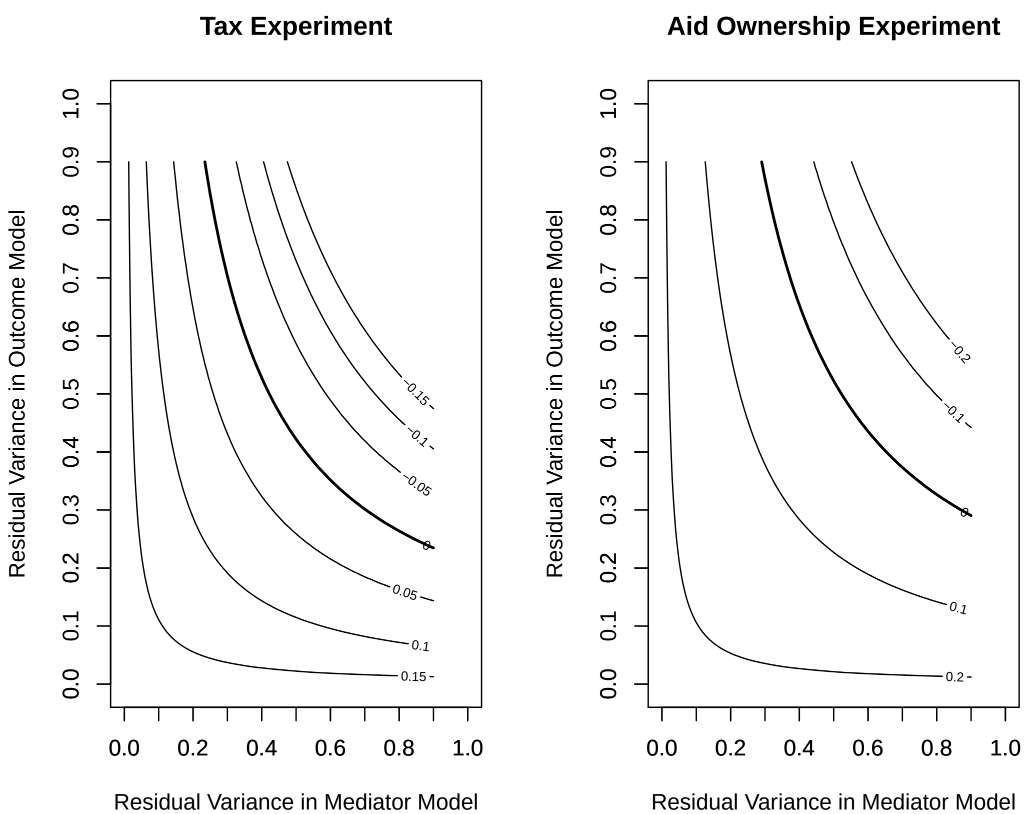

Figure C.1: Sensitivity Analysis for Lab Games. Figure demonstrates the amount of predictive power that the unmodeled mediator would have to possess to drive the AMCE of ownership to zero. Results suggest that the unmodeled mediator would have to be an incredibly power predictor of both subjects' feelings of ownership as well as their punishment thresholds to render the ownership mediator insignificant.

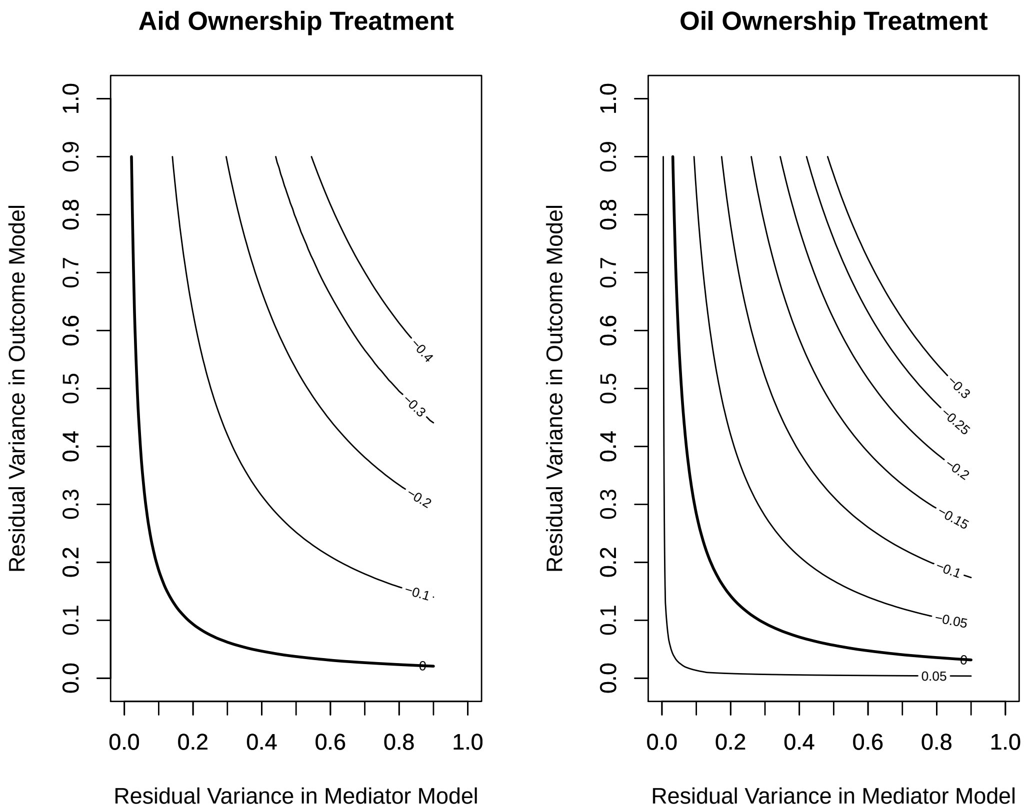

Figure C.2: Sensitivity Analysis for Survey Experiment. Figure demonstrates the amount of predictive power that the unmodeled mediator would have to possess to drive the AMCE of ownership to zero. Results suggest that the unmodeled mediator would have to be an incredibly power predictor of both subjects' feelings of ownership as well as their punishment thresholds to render the ownership mediator insignificant.

D Analysis of Tax Ownership Results in Ghana

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In this section we provide a more in-depth discussion of the Ghana results, including results on the Tax treatment disaggregated by Windfall source. The first and most important test of the ownership effect is simply whether strength of ownership *matters* in determining subjects' willingness to punish.[17] To do so, we estimate the following OLS model:

$$Y_i = \alpha + \beta \text{Ownership}_i + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i$$

where Y_{ij} is subject i's punishment threshold in round j and Ownership is the independent variable of interest, a three-point indicator for whether subjects felt ownership over the group fund, where 0 represents no ownership, 1 represents weak ownership and 2 represents strong ownership.[18] The vector $\mathbf{X}_i$ contains the same subject covariates included in the Uganda models, enumerator fixed-effects, a one-round lag of the leaders' transfer, and dummies for additional cross-cutting treatments not analyzed here. [19] Results are reported in Table D.1.

The results show that high ownership is a substantively strong and statistically significant predictor of subject thresholds. Subjects who report strong ownership ask for larger transfers from Leaders, with strong ownership corresponding to an increase in transfer thresholds of 0.798 MU ($p \approx 0$). As in Uganda, the independent effect of ownership on punishment behavior is far stronger even than the effect of taxation. One potential concern in estimating the effect of ownership is that ownership may vary with pre-treatment covariates. To account for this possibility, we estimated models with a range of pre-treatment covariates. The coefficient on `Ownership` is stable across these models, nearly identical to that presented in Table D.1, and remains strongly significant.

We next show that, as in Uganda, our ownership treatment—in this case, taxation—increases self-reported ownership and punishment thresholds. The first four columns of Table D.2 report the increase in ownership that occurs as the result of taxation. Column 2 reports the results when the Aid, Oil and Grant conditions are pooled, while Columns 3-5 report the disaggregated results. Column 6 reports the effect of taxation on the punishment threshold.

As in Uganda, we also conducted mediation analysis to test whether ownership is acting as a mediator in explaining the effect of the Tax Ownership treatment on subjects' accountability demands. The results are reported in Table D.3 below. As in Uganda, the Average Causal Mediation

[17] The ownership question was added three days after data collection began, resulting in the loss of 131 subjects from our sample. Of the total, 51 were assigned to the Oil condition, 28 to Grant, 29 to Aid, and 23 to Tax.

[18] This was recoded from the original 4-point measure, with weak and strong disagreement with the ownership statement collpased into a single category. This was done to facilitate comparison with the 10-point measures used in 2 of the 3 Uganda experiments. Results are similar using the original 4 point measure.

[19] Two cross-cutting treatments, one manipulating the punishment probability and one introducing a valence prime during the group training, are discussed in the pre-analysis plan and are the subject of ongoing analysis. Because the number of sessions in each block (36) was not a multiple of the number of unique treatments (24), there was minor imbalance in these dimensions. We thus include them in our estimating equation to control for the effect of this imbalance.

	DV: Subject (Punishment) Threshold				
	Pooled	Aid	Oil	Grant	Tax
Ownership	0.463***	0.550***	0.396	0.566**	0.503**
	(0.094)	(0.189)	(0.244)	(0.223)	(0.221)
Enum + Round FE	✓	✓	✓	✓	✓
Source FE	✓	-	-	-	-
Other Controls	✓	✓	✓	✓	✓
Observations	2040	523	463	484	570
Adjusted-R^2	0.211	0.341	0.249	0.136	0.218

Note: *p<0.1; **p<0.05; ***p<0.01

Table D.1: Impact of Ownership on Punishment Behavior in Ghana. The dependent variable is subject i's threshold in round j and the independent variable is a 3-point ownership measure with 0 representing no ownership, 1 representing weak ownership and 2 representing strong ownership. Column 1 reports the effect of Ownership in the pooled sample; Columns 2-5 report estimates for each individual source. Standard errors clustered by subject in parentheses. Observations are subject-round.

	DV: Ownership				*DV: Punishment*
	Pooled	Aid	Oil	Grant	Windfall
Tax Ownership	0.302***	0.369***	0.289***	0.286***	0.426***
	(0.079)	(0.096)	(0.104)	(0.103)	(0.164)
Enumerator FE	✓	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓	✓
Observations	511	274	259	264	2107
Adjusted-R^2	0.396	0.371	0.408	0.357	0.168

Note: *p<0.1; **p<0.05; ***p<0.01

Table D.2: Taxation Increases Self-Reported Ownership and Punishment Thresholds in Ghana. All models use enumerator and round fixed effects and the same set of subject covariates as in Uganda. Dummies for cross-cutting treatment conditions not analyzed here are also included. The ownership measure (Columns 2-5) is standardized such that coefficients represent the standard-deviation change in ownership due to the tax treatment. Note that Columns 2-5 use subject-level data because there is no variation in ownership values across round for a given subject. Column 6 uses subject-round data. HC3 standard errors used in Columns 2-5 and CR2 standard errors used in Column 6.

Effect (ACME) is highly significant and in the expected direction; the effect of the exogenous variation in ownership induced by taxation increases subjects' willingness to punish low transfers from the Leader.

	Estimate	p-value
Causal Mediation Effect (ACME)	0.135***	0.000
Direct Effect (ADE)	0.254	0.130
Total Effect	0.390**	0.024
Observations	2041	

Note: *p<0.1; **p<0.05; ***p<0.01

Table D.3: Replication of Mediation Results in Ghana. Covariates and dummies for cross-cutting conditions included in both mediator and outcome models. Standard errors clustered at individual level. Round and enumerator fixed-effects included. As in the Uganda experiments, the first round was a practice round and is dropped from the analysis.

E Additional Results: Lab Experiments

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E.1 Expressive Benefits to Punishment

In order to capture expressive benefits, we exploit the inclusion of a "utility ladder" in the Tax Ownership experiment (Experiment 1). The utility ladder is a novel measurement tool developed to capture changes in respondents' subjective utility at four distinct points in the game. The ladder is a 21-point scale ranging from 0 to 20. At the beginning of the round, subjects are anchored at a value of ten by the enumerator. This allows us to anchor all subjects at an identical baseline utility and avoid the possibility of different starting positions, which could be a function of subjects' economic wellbeing, among other things. After anchoring subjects at the midpoint, all changes can occur only as the result of citizen or Leader actions. Once subjects are anchored, we then ask the citizen to update their ladder (1) once the citizen purchases a good, (2) when the leader is given the group fund, (3) after the Citizen observes the Leader's allocation, and (4) at the end of the round (e.g. after any punishment has occurred and final payouts realized). The utility ladder is included below in Figure E.1.

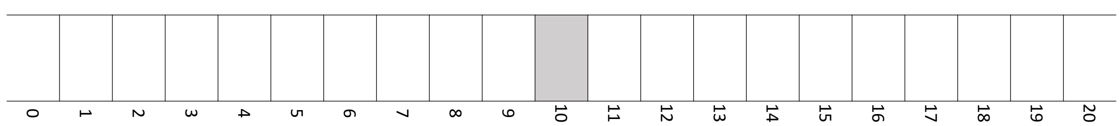

Figure E.1: Utility Ladder. Subjects are anchored at the midpoint of the ladder at the start of each round. This facilitates comparing subjects who may, due to covariates such as age, education, wealth and gender, give different starting positions. Changes in the ladder as the game proceeds can thus be considered changes from a common reference point. The combination of a finely gradated scale and the ability for subjects to move 10 full rungs in either direction makes the ladder extremely sensitive to changes in utility as a function of game outcomes.

The key ladder measurements for the purposes of establishing an expressive benefit to punishment are Ladder 3 (L3) and Ladder 4 (L4). Ladder 3 is taken after the Citizen has learned the size of the Leader's transfer, while Ladder 4 is taken after punishment is assessed and the round is concluded. Since the Citizen has already learned of the Leader's transfer prior to Ladder 3, the only change between Ladder 3 and Ladder 4 is whether or not punishment has occurred. We can therefore use the difference in the two ladders to identify the effect that punishment has subjects' ladder position. To do so, we model the difference between Ladder 3 and Ladder 4 as a function of treatment condition; the transfer the Citizen received in the previous round; the Leader transfer in the current round; the Citizen's threshold in the current round; and, finally, an indicator for whether punishment occurred.[20] If there are expressive benefits to punishment, we should expect that this final measure will be positive. We note that, because punishment is economically costly to the Citizen, a positive coefficient could only be the result of a psychological, expressive benefit that is realized due to punishment. Table E.1 reports the results of this regression for each of the four ladders. As predicted, the coefficient on `Punishment` (bolded) is positive and statistically significant: observing successful punishment results in a 0.925 point increase in subjects' ladder position.

	Ladder 3	**Ladder 4**
Tax Ownership	-0.590^{***}	-0.684^{***}
	(0.205)	(0.171)
Previous Transfer	0.004	-0.070^{*}
	(0.048)	(0.038)
Subject Threshold	-1.603^{***}	-0.077
	(0.092)	(0.087)
Leader Transfer	2.103^{***}	0.726^{***}
	(0.056)	(0.073)
Previous Ladder	0.397^{***}	0.374^{***}
	(0.038)	(0.028)
Punishment		$\mathbf{0.925}^{***}$
		(0.281)
Enumerator FE	Yes	Yes
Purchased Item FE Yes	Yes	Yes
N	1760	1754

Table E.1: Punishment Produces Increase in Subjective Utility. Each column reports the estimates of changes in subject utility over two stages in the game: after the subject has observed the leader transfer (Column 1), and after subjects have observed leaders being punished for low transfers (Column 2). All models include treatment, enumerator, round and item fixed effects but are omitted here for presentation purposes. Due to inclusion of the previous ladder value as a lag, coefficients should be interpreted as the change in ladder value as a function of the covariate of interest. We note that the reference treatment condition here is Windfall, such that the coefficient on `Tax Ownership` represents the change in the ladder as a result of paying a direct tax at the start of the round. As expected, paying a tax causes a large decrease in subjects' ladder position.

[20] As in previous lab results, we also include enumerator and round fixed-effects, as well as dummies for the good that was purchased by subjects during the purchasing round at the start of the game.

	Dependent variable:
	Punishment Threshold
Oil Framing	-0.106
	(0.190)
Previous Transfer	0.030
	(0.021)
Observations	1,927
Note:	*p<0.1; **p<0.05; ***p<0.01

Table E.2: Oil Framing Has No Effect on Punishment. Model specification is identical to those reported in of the main text. Reference condition is the base oil condition in which subjects were not given implicit ownership (Oil Ownership condition) over the group fund nor reminded that the oil revenues are supposed to be used to improve the lives of average Ugandans (Oil Framing condition). Positive coefficients represent an increase in punishment thresholds and negative coefficients a decrease. The substantively small and statistically insigificant estimate for `Oil Framing` demonstrates that subjects did not behave differently in the Oil Framing condition compared to the base Oil treatment. To improve power, we thus pool the Base Oil and Oil Framing conditions when estimate the treatment effect for Oil Ownership.

	Dependent variable:
	Leader Transfer
Direct Tax	−0.103
	(0.272)
Windfall Aid	−0.352
	(0.320)
Windfall Oil	−0.008
	(0.344)
Age	0.024**
	(0.012)
Education	0.033
	(0.031)
Services	0.118
	(0.174)
Poverty	−0.364***
	(0.134)
Female	0.056
	(0.222)
Observations	2,132
Note:	*p<0.1; **p<0.05; ***p<0.01

Table E.3: Effect of Treatment Assignment on Leader Transfers. Given that recruited subjects were average citizens and not elites, we view the behavior of Leaders as unlikely to be reflective of the behavior of actual politicians or bureaucrats. Nonetheless, we include here estimates of the effect of treatment conditions and a selection of subject covariates on leader transfers. As expected given the low external validity of subjects' behavior when acting as Leaders, none of the three treatment coefficients (Rows 1-3) are significant. Estimating equation includes enumerator and round fixed effects with standard errors clustered by leader. Because each leader played with three citizens per round and made separate transfer decisions for each Citizen, each leader appears three times each round for five rounds for a total of 15 observations per leader. The Windfall Grant condition is the baseline, such that coefficients on the remaining three treatment conditions can be interpreted as the change in the Leader transfer (given in monetary units) relative to Leaders in the Windfall Grant treatment. Additional covariates included for purposes of comparison.

F Additional Results: Survey Experiment

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F.1 Individual correlates of Ownership

This section draws on additional covariates from our national survey to test which individual-level factors correlate with high levels of ownership. As we did not pre-specify this analysis, all results should be viewed as exploratory; we are not attempting to make causal claims. Our analysis focuses on basic demographic and economic variables that are unlikely to be directly affected by either our treatments or to be the effect, rather than cause, of high ownership. These include whether the respondent is head of household; age; sex; the highest level of education attained; cellphone ownership; logged income; and urban or rural status. We also include a geographic variable, namely the population of the respondent's village or town.

We look at how these variables correlate with four measures of ownership: ownership over foreign aid; oil; taxes; and the average of the three measures. The order in which the three revenue ownership measures were asked in the survey depended on the treatment group. Those in aid or oil ownership treatment groups were asked about that source first, then the others were randomized. In the control group the full order was randomized. We see strong anchoring effects of the first ownership measure asked, and so we view the average ownership variable as most informative. However, we also show results for each revenue source separately.

Figure F.1 shows the results of regressions of ownership on the covariates just described. All specifications are OLS and include indicators for treatment condition and enumerator fixed effects, though these are omitted for presentation purposes. The figure includes four panels. The top left panel uses aid ownership as the dependent variable. The other three use ownership over oil, ownership over tax revenues, and the average ownership measure described above.

Several factors predict ownership. Men have higher ownership than women over all forms of revenue, as do wealthier respondents, as measured by self-reported household income and cell phone ownership. Likewise, those with more education report higher levels of ownership. It is important to note that high education or income are not simply proxying for elite status: median education in our sample is 9 years, and median income is the equivalent of US$8.38 per day.

Age and household head status are not strong predictors of ownership, other than possibly in the oil condition. Rural respondents have lower tax ownership—possibly because taxation is low in rural areas—but we find no differences for other revenue sources.

These results suggest that individuals' status and experience may affect ownership, but more research is needed to understand how and why some groups appear to have higher ownership than others.

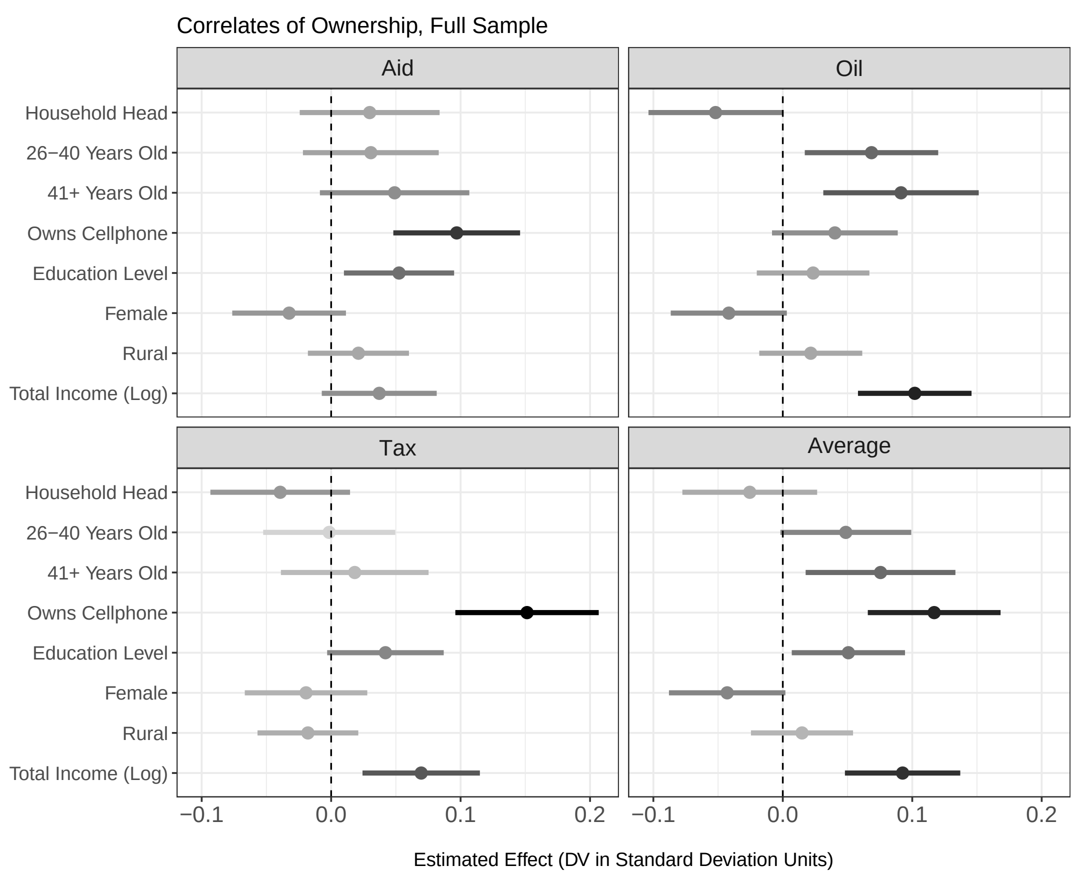

Figure F.1: Correlates of Ownership

F.2 Additional experimental results

	Pooled	Pure Control	Aid Ownership	Oil Ownership
Accountability Index	1.190***	0.717	1.627***	1.281**
	(0.300)	(0.476)	(0.533)	(0.595)
Donation	0.301**	0.341	0.231	0.402
	(0.148)	(0.243)	(0.259)	(0.288)
Contact National Politician	0.194	−0.114	0.593**	0.157
	(0.147)	(0.238)	(0.257)	(0.288)
Report Participation	0.509***	0.485**	0.563**	0.290
	(0.144)	(0.227)	(0.256)	(0.281)
SMS	0.435**	0.293	0.475*	0.670**
	(0.142)	(0.228)	(0.251)	(0.280)

Note: *p<0.1; **p<0.05; ***p<0.01

Table F.1: Correlation Between Individual Accountability Measures and Ownership. Due to space constraints, our main results report only the correlation between our Accountability Index and Ownership. Here, we include the results of the same models run on the index as well as each of its constituent behavioral measures. All models are identical and include subject covariates as in the main text. Column 1 reports the correlation between our behavioral measures and ownership for the entire sample, while Columns 2-4 report estimates for each treatment condition separately. In all but the Pure Control condition the correlation between accountability behaviors and ownership is statistically significant. Classical standard errors used.

	Dependent variable:					
	Contact National Politician			**Donate**		
	Full Sample	Low Efficacy	High Efficacy	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	−0.007	0.035	−0.052	0.022	0.065	−0.013
	(0.041)	(0.060)	(0.058)	(0.041)	(0.059)	(0.057)
Oil Ownership	−0.005	0.033	−0.033	−0.017	0.042	−0.064
	(0.042)	(0.061)	(0.058)	(0.041)	(0.060)	(0.057)
Observations	853	416	432	848	413	430
	Dependent variable:					
	Sent SMS			**Report Participation**		
	Full Sample	Low Efficacy	High Efficacy	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	−0.000	0.093	−0.082	0.030	0.045	0.022
	(0.040)	(0.057)	(0.057)	(0.040)	(0.058)	(0.056)
Oil Ownership	0.017	0.091	−0.042	0.055	0.115*	0.005
	(0.040)	(0.058)	(0.057)	(0.041)	(0.059)	(0.056)
Observations	853	416	432	850	415	430
Note:						*p<0.1; **p<0.05; ***p<0.01

Table F.2: Average Treatment Effect for Individual Outcome Measures. All estimates are convention difference-in-means with classical standard errors. Results are broken into two panels due to space constraints. For each dependent variable, we report estimates for the full sample as well as the low- and high-efficacy subgroups examined in the main text. As with the Accountability Index, estimates from the full sample mask substantial variation by subjects' level of efficacy, which is measured via an inverse covariance weighted index of seven items from the pre-treatment battery. The top panel reports results for the *Contact* and *Donate* variables, while the bottom panels report estimates for *Sent SMS* and *Report Participation.* For all four measures, the effect for low-efficacy respondents are larger than those for high-efficacy respondents.

Effect of Treatment on Ownership Mechanism

	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	0.416***	0.507***	0.329**
	(0.110)	(0.151)	(0.160)
Oil Ownership	0.140	0.266*	0.047
	(0.116)	(0.150)	(0.179)
N (Aid)	259	132	126
N (Oil)	266	138	125

Effect of Treatment on Accountability Index

	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	−0.105	0.024	−0.253
	(0.126)	(0.168)	(0.187)
Oil Ownership	−0.211*	−0.036	−0.385**
	(0.124)	(0.163)	(0.188)
N (Total)	396	202	191

Ownership Mechanism as Mediator

	Full Sample	Low Efficacy	High Efficacy
Aid Ownership	0.059**	0.110**	0.011
	[0.006, 0.129]	[0.016, 0.233]	[-0.045, 0.086]
Oil Ownership	0.023	0.037	0.014
	[-0.019, 0.073]	[-0.016, 0.126]	[-0.058, 0.098]
N (Aid)	254	128	125
N (Oil)	263	136	124
Note:			*p<0.1; **p<0.05; ***p<0.01

Table F.3: Effect of Ownership Treatments for Non-Household Heads.

Mediation Results for Aid Ownership

	Full Sample	Low Efficacy	High Efficacy
Casual Mediation Effect (ACME)	0.064***	0.083***	0.051**
	[0.026, 0.109]	[0.023, 0.164]	[0.008, 0.120]
Direct Effect (ADE)	-0.016	0.108	-0.161
	[-0.176, 0.149]	[-0.139, 0.338]	[-0.389, 0.066]
Total Effect	0.048	0.191	-0.110
	[-0.113, 0.212]	[-0.056, 0.426]	[-0.342, 0.122]
Observations	558	272	284

Mediation Results for Oil Ownership

	Full Sample	Low Efficacy	High Efficacy
Casual Mediation Effect (ACME)	0.057***	0.072***	0.042*
	[0.021, 0.102]	[0.018, 0.149]	[-0.002, 0.106]
Direct Effect (ADE)	-0.054	0.089	-0.183
	[-0.222, 0.123]	[-0.169, 0.340]	[-0.419, 0.053]
Total Effect	0.003	0.161	-0.141
	[-0.159, 0.178]	[-0.096, 0.418]	[-0.369, 0.085]
Observations	557	266	288

Table F.4: Full Mediation Results, Field Experiment. For both the full sample (Column 1) and the two efficacy subsets (Columns 2-3), all three major quantities of interest in mediation analysis are reported. The top panel reports these estimates for the Aid Ownership treatment (relative to the Pure Control condition), while the bottom does the same for the Oil Ownership treatment. As would be expected from the small ATEs reported in Table 6 , the ADEs are insignificant. Differences in the ATEs reported in Table 6 and the ADEs reported here occur due to the fact that both the outcome and mediator models include the following subject-level covariates: gender, household size, age, level of education, and logged total income. These covariates are included to account for possible confounding mediators that are strongly determined by key demographic characteristics. Estimates are obtained via bootstrapping and the 95% confidence interval is constructed by taking the values at the 5th and 95th percentiles of the bootstrap distribution.

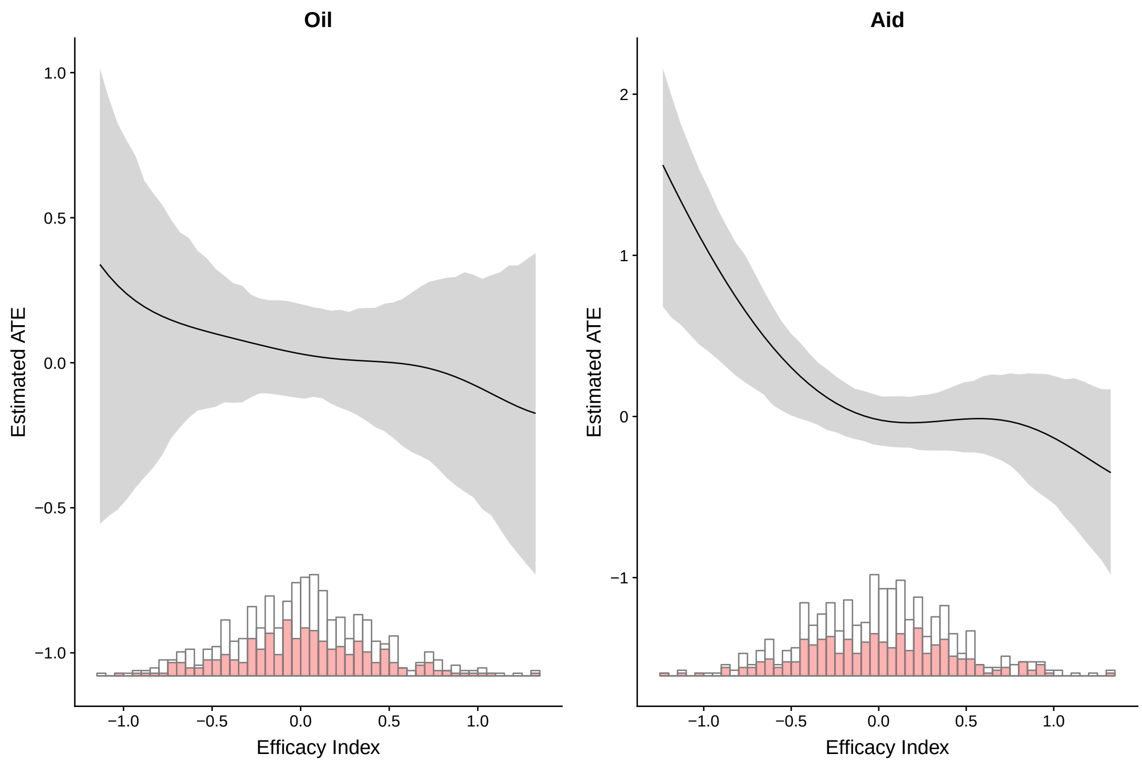

Figure F.2: Heterogeneous Treatment Effects on Accountability Index by Subject Efficacy. Efficacy Index is constructed via inverse-covariance weighting of seven items discussed in Section A.1. Estimates produced via `Interflex` package in `R` with the bandwidth for the local regression chosen via cross-validation. Left pane shows the effect of the Oil Ownership treatment on *Accountability Index* across the distribution of *Efficacy Index*. Right pane shows the same estimates for the Aid Ownership treatment. *Accountability Index* is standardized by the control group, such that the y-axis represents the control group standard deviation change in the index caused by the Oil and Aid Ownership treatments. The downward sloping non-linear estimates demonstrate that the effect of both ownership treatments declines in subjects' efficacy. The distribution of the efficacy index is plotted at the bottom of each pane and colored according to treatment status. Each bar gives the proportion of control units (white) relative to treated units (salmon) falling inside the interval.

G Balance Tests and Descriptive Statistics

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G.1 Means

	Treatment	Subject Threshold	Leader Transfer
Experiment 1			
1	Direct Tax	4.71	4.17
2	Windfall Aid	4.28	3.67
3	Windfall Grant	4.52	3.74
4	Windfall Oil	4.68	3.94
Experiment 2			
1	Oil	4.38	4.8
2	Oil Framing	4.57	4.55
3	Oil Ownership	4.66	4.48
Experiment 3			
1	Aid Ownership	5.41	4.59
2	Basic Aid	5.14	4.24

Table G.1: Mean subject thresholds and leader transfers per treatment condition and experiment

G.2 Experiment 1: Tax Ownership Treatment

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	415	29.14	8.68	20.00	23.00	34.00	69.00
Education	415	9.42	4.29	0.00	7.00	13.00	17.00
Female	415	0.48	0.50	0.00	0.00	1.00	1.00
Lacked Food in Prev 6 months	415	2.26	1.09	1.00	1.00	3.00	5.00
Lacked Water in Prev 6 months	415	1.64	0.93	1.00	1.00	2.00	5.00
Trust in Member of Parliament	414	2.09	1.02	1.00	1.00	3.00	5.00
Voted in Prior Election	415	0.72	0.45	0.00	0.00	1.00	1.00
Average Quality of Local Services	415	3.84	0.55	2.50	3.50	4.25	5.00
Ownership (Lab Game)	415	−0.15	0.99	−3.31	−0.86	0.61	1.59
Quality of Local Schools	414	4.54	0.69	2.00	4.00	5.00	6.00
Quality of Local Clinics	415	4.15	0.80	2.00	4.00	5.00	6.00
Qualit yof Local Roads	415	3.69	0.92	2.00	3.00	4.00	6.00
Quality of Local Sewage/Sanitation	415	2.98	0.92	1.00	2.00	4.00	5.00

Table G.2: Experiment 1: Descriptive Statistics

	Grant	Tax	Aid	Oil	F-test p	FDR q
Age	29.040	28.490	29.280	29.900	0.810	0.810
Education	9.770	9.220	8.870	9.110	0.380	0.770
Female	0.430	0.490	0.580	0.540	0.120	0.310
Lacked Food in Prev 6 months	2.210	1.970	2.410	2.510	0.020	0.140
Lacked Water in Prev 6 months	1.580	1.760	1.650	1.690	0.540	0.810
Trust in Member of Parliament	2.090	1.950	2.120	2.170	0.660	0.810
Voted in Prior Election	0.730	0.700	0.670	0.750	0.730	0.810
Average Quality of Local Services	3.850	3.710	3.840	3.940	0.090	0.310

Table G.3: Experiment 1: Balance Tests. FDR q value is calculated via the Benjamini-Hochberg procedure and controls the false discovery rate at $\alpha = 0.05$.

G.3 Experiment 2: Oil Ownership Treatment

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	387	30.95	9.54	18.00	24.50	36.00	87.00
Education	388	2.71	1.35	0.00	2.00	3.00	6.00
Female	388	0.46	0.50	0.00	0.00	1.00	1.00
Lacked Food in Prev 6 months	386	2.62	1.14	1.00	2.00	3.00	5.00
Lacked Water in Prev 6 months	388	1.90	1.09	1.00	1.00	3.00	5.00
Trust in Member of Parliament	364	2.86	1.26	1.00	2.00	4.00	5.00
Voted in Prior Election	339	0.89	0.32	0.00	1.00	1.00	1.00
Average Quality of Local Services	388	4.19	0.52	2.43	3.86	4.57	5.57

Table G.4: Experiment 2: Descriptive Statistics

	Oil	Oil Framing	Oil Ownership	F-test p	FDR q
Age	31.560	29.980	31.310	0.360	0.720
Education	2.550	2.900	2.690	0.110	0.720
Female	0.500	0.470	0.410	0.340	0.720
Lacked Food in Prev 6 months	2.500	2.650	2.710	0.320	0.720
Lacked Water in Prev 6 months	1.850	1.920	1.930	0.830	0.830
Trust in Member of Parliament	2.810	2.910	2.870	0.820	0.830
Voted in Prior Election	0.860	0.900	0.900	0.560	0.830
Average Quality of Local Services	4.170	4.180	4.210	0.820	0.830

Table G.5: Experiment 2: Balance Tests. FDR q value is calculated via the Benjamini-Hochberg procedure and controls the false discovery rate at $\alpha = 0.05$.

G.4 Experiment 3: Aid Ownership Treatment

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	205	27.15	7.31	20.00	22.00	30.00	63.00
Education	205	10.46	3.61	0.00	8.00	13.00	17.00
Female	205	0.23	0.42	0.00	0.00	0.00	1.00
Lacked Food in Prev 6 months	205	2.60	1.13	1.00	2.00	3.00	5.00
Lacked Water in Prev 6 months	205	1.86	1.02	1.00	1.00	2.00	5.00
Trust in Member of Parliament	204	2.44	1.02	1.00	2.00	3.00	5.00
Voted in Prior Election	205	0.67	0.47	0.00	0.00	1.00	1.00
Average Quality of Local Services	205	4.06	0.60	2.50	3.75	4.50	5.50

Table G.6: Experiment 3: Descriptive Statistics

	Aid Ownership	Basic Aid	F-test p	FDR q
Age	28.020	26.250	0.080	0.330
Education	10.200	10.720	0.300	0.360
Female	0.290	0.180	0.060	0.330
Lacked Food in Prev 6 months	2.670	2.510	0.320	0.360
Lacked Water in Prev 6 months	1.890	1.830	0.660	0.660
Trust in Member of Parliament	2.360	2.510	0.280	0.360
Voted in Prior Election	0.620	0.710	0.180	0.360
Average Quality of Local Services	4.010	4.110	0.250	0.360

Table G.7: Experiment 3: Balance Tests. While only the Oil, Oil Framing and Oil Ownership conditions are used in the analysis, the remaining conditions were used to verify the results from the Tax Ownership data. We thus include all conditions here. FDR q value is calculated via the Benjamini-Hochberg procedure and controls the false discovery rate at $\alpha = 0.05$.

G.5 Field Experiment

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	852	40.22	13.72	20.00	29.00	50.00	85.00
Education	843	8.65	4.62	0.00	5.50	11.00	17.00
Female	853	0.32	0.47	0	0	1	1
Lacked Food in Prev 6 months	17	1.65	0.86	1.00	1.00	2.00	3.00
Lacked Water in Prev 6 months	17	1.94	1.56	1.00	1.00	3.00	5.00
Trust in Member of Parliament	573	2.20	1.17	1.00	1.00	3.00	4.00
Voted in Prior Election	853	0.85	0.36	0	1	1	1
Average Quality of Local Services	578	2.85	0.62	1.00	2.40	3.20	4.40

Table G.8: Field Experiment: Descriptive Statistics

	Aid Own	Oil Own	Pure Control	F-test p	FDR q
Age	39.760	39.470	41.410	0.190	0.570
Education	8.530	8.880	8.540	0.600	0.810
Female	0.350	0.290	0.310	0.360	0.570
Lacked Food in Prev 6 months	1.570	1.500	1.830	0.820	0.830
Lacked Water in Prev 6 months	2.570	1	1.830	0.280	0.570
Trust in Member of Parliament	2.150	2.320	2.160	0.300	0.570
Voted in Prior Election	0.850	0.840	0.850	0.830	0.830
Average Quality of Local Services	2.880	2.890	2.770	0.130	0.570

Table G.9: Field Experiment: Balance Tests. FDR q value is calculated via the Benjamini-Hochberg procedure and controls the false discovery rate at $\alpha = 0.05$.

G.6 Ghana Data

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	691	32.74	12.48	18.00	23.00	40.00	75.00
Education	691	2.63	1.39	0.00	2.00	4.00	7.00
Female	691	0.53	0.50	0.00	0.00	1.00	1.00
Average Quality of Local Services	691	4.40	0.57	2.43	4.14	4.71	6.00
Lacked Water in Prev 6 months	691	1.38	0.70	1.00	1.00	2.00	5.00
Lacked Food in Prev 6 months	691	1.45	0.79	1.00	1.00	2.00	5.00
Voted in Prior Election	701	0.70	0.46	0.00	0.00	1.00	1.00
Trust in Member of Parliament	690	2.57	1.17	1.00	1.00	4.00	5.00

Table G.10: Ghana Data: Descriptive Statistics

	Aid	Grant	Oil	Tax	F-test p	FDR q
Age	33.970	33.330	30.380	33.150	0.040	0.350
Education	2.610	2.670	2.650	2.580	0.930	0.970
Female	0.540	0.560	0.510	0.520	0.800	0.970
Average Quality of Local Services	4.400	4.420	4.340	4.420	0.560	0.970
Lacked Water in Prev 6 months	1.370	1.380	1.400	1.380	0.970	0.970
Lacked Food in Prev 6 months	1.420	1.450	1.490	1.450	0.880	0.970
Voted in Prior Election	0.690	0.720	0.640	0.730	0.270	0.970
Trust in Member of Parliament	2.580	2.620	2.550	2.550	0.950	0.970
Observations	192	174	192	206		

Table G.11: Ghana Data: Balance Tests. FDR q value is calculated via the Benjamini-Hochberg procedure and controls the false discovery rate at $\alpha = 0.05$.

References

Fink, Günther, John R. Weeks and Allan G. Hill. 2012. "Income and Health in Accra, Ghana: Results From a Time Use and Health Study." *American Journal of Tropical Medicine and Hygiene* 87(4):608–615.