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

Brandon de la Cuesta[†]   Lucy Martin[‡]   Helen V. Milner[§]   Daniel L. Nielson[¶]

October 14, 2018

Abstract

Citizens' willingness to punish wayward officials varies according to their expectations for government performance and across revenue sources—especially for taxes compared to windfalls from oil and aid. Yet little is known about what causes citizens to raise their expectations for public-goods provision. We argue that feelings of revenue ownership drive governance expectations, offer a new measure of the concept, and apply it in sub-Saharan Africa. Results from lab-in-the-field experiments in Uganda demonstrate that high ownership significantly increases citizens' willingness to sanction poor-performing leaders, accounts for differences between taxes and windfalls, and mediates the effect of taxation on punishment propensity. We replicate these results in Ghana and offer further observational evidence in Uganda. Crucially, additional lab experiments in Uganda demonstrate that simple prompts can experimentally manipulate feelings of ownership for windfall revenues, producing accountability pressures equal to taxes and suggesting that focused public-information interventions might heighten accountability demands on governments.

Word Count: 11,914

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

1   Introduction

Citizens in developing countries often fail to demand accountability for government spending through protests or elections. This appears especially true for windfall revenues from oil exploration and foreign aid, particularly when compared to tax money extracted from citizens' own pockets (Ross 2012; Morrison 2009, 2015). More generally, mounting evidence suggests that it is difficult to improve bottom-up accountability pressures; most interventions evaluated to date have either had no effect or seem to improve accountability only under difficult-to-replicate conditions.[1] We therefore need greater knowledge of what causes citizens to demand accountability for the spending of public revenue, especially for windfall revenues, which are associated with poor governance outcomes.

Research on citizens' willingness to take costly political action in pursuit of better services suggests that absolute levels of government performance matter less than performance relative to expectations (Gottlieb 2016; Ross 2004): citizens are most likely to sanction when their expectations are high but not being met. Yet, in many developing-country cases, expectations are extremely low; citizens believe that government either cannot or will not improve, leading to few complaints when these low expectations are confirmed through poor services and corruption scandals. Gottlieb (2016) also shows that improving citizens' perception of government capacity can increase citizens' expectations, and De Kadt and Lieberman (2016) suggest that citizens adjust expectations over time in response to existing levels of service provision. These encouraging results indicate that governance expectations may be malleable. Nonetheless, we still lack a strong understanding of how citizens form expectations over government spending in the first place.

Experimental work in cognitive psychology has identified ownership as a possible driver of expectations: when individuals have legal or psychological ownership over a good, they expect to benefit more from it and are more willing to punish allocations that they perceive as unfair (Wu et al. 2012).[2] Psychological ownership—the sense that something "is mine" or "belongs to me"—is especially important, as it can vary even when legal ownership remains constant. Intuitively, a similar idea may apply to budgets: we argue that citizens who feel that the budget belongs to them will be more willing to punish leaders for non-accountable behavior.

Previous work on taxation, the resource curse, and accountability suggests that the

---

[1] For example, both Olken (2007) and Casey, Glennerster and Miguel (2012) find that interventions to increase citizen monitoring had no impact, and while information interventions may sometimes appear to prove effective (Reinikka and Svensson 2005), in many cases information has no effect on citizen behavior (see e.g the metanalysis in Dunning et al. (2018)).

[2] In Section 2 we discuss how ownership differs from related concepts like loss aversion and the endowment effect.

source of revenues might help explain variation in feelings of public ownership. In general, governments that rely on taxation for funding are less corrupt, are more likely to democratize, and provide higher public services (Ross 2004; Timmons 2005; Baskaran and Bigsten 2013; Prichard 2015; Fisman and Gatti 2002; Brollo et al. 2013; Gadenne 2015). In contrast, funds from oil and aid are believed to be "windfall revenues" that enable corruption, undermine governance, foster repression, prolong autocratic rule, and increase conflict (Mahdavy 1970; Beblawi and Luciani 1987; Chaudhry 1998; Bräutigam and Knack 2004; Djankov et al. 2008; Smith 2008; Caselli and Cunningham 2009; Morrison 2009, 2015). A key explanation for the differences between taxes and windfalls focuses on taxpayers' relative willingness to enact costly sanctions on otherwise unaccountable leaders (Paler 2013; Martin 2014).[3] Prior research explains this effect as a mix of ownership, loss aversion, and the endowment effect (Sandbu 2006; Paler 2013; Martin 2014), and describes it in terms of citizens' "right to influence the use of 'their' own money" (Moore 2004). We show below that ownership differs conceptually from both the endowment effect and loss aversion and that it significantly drives taxation's effect on citizens' behavior.

While prior research invokes notions related to ownership to explain the accountability pressures produced by taxation (Moore 2004; Sandbu 2006; Paler 2013), the concept of ownership has not been sufficiently developed theoretically nor rigorously studied experimentally, and relatively little work has measured or tested the concept in the context of politics and policy.[4] While many large, cross-national surveys include multiple measures of citizens' willingness to hold leaders accountable, none to our knowledge asks questions about the degree to which citizens feel ownership over government revenues. As a result, despite the implied role of ownership in prominent theories of accountability—particularly with respect to taxation—we know very little about ownership empirically. Does ownership vary across individuals? Do citizens feel stronger ownership over tax-based sources of revenue than for aid and oil windfalls? What role, if any, does ownership play in making citizens willing to pay the (often high) costs of sanctioning officials for corruption or mismanagement? And critically, can deliberate interventions induce greater ownership over different sources of revenue?

This paper develops and tests a theory linking psychological ownership over government budgets to citizens' demands on leaders. There are many reasons to expect citizens'

---

[3]Other proposed mechanisms include tax bargaining, in which the state grants policy or institutional concessions to generate tax compliance, and theories that taxation increases citizens' information about government spending (Schumpeter 1991; Bates and Lien 1985; Levi 1989; North and Weingast 1989; Gadenne 2015; Paler 2013; Prichard 2015).

[4]To our knowledge, Paler (2013) is the only study that measures public-revenue ownership. She finds that citizens feel higher ownership over tax revenues relative to windfalls, but does not test whether ownership is a significant predictor of willingness to sanction.

ownership over government revenues to vary even conditional on revenue source, and these differences may help to explain when citizens will punish poor performance. We argue that citizens with high psychological ownership over the government budget will have higher expectations for how much they should benefit from government spending. Poor government performance generates negative emotions in citizens with high expectations, and these negative emotions are then alleviated through sanctioning those responsible. Psychological ownership therefore increases the expressive benefits citizens receive from punishment, making them more willing to pay to punish. We also argue that psychological ownership is malleable, and that as a result it is possible to increase citizens' sense of ownership over windfall revenues.

We test our hypotheses about ownership and its effects using lab-in-the-field behavioral games conducted in Kampala, Uganda. In the games, a "Citizen" has to decide whether to punish a "Leader" for how she allocates a group fund, which can come from a tax on the Citizen or from an exogenous windfall. We first establish that ownership is an important predictor of citizens' demands. Ownership appears to have a substantively large and highly significant effect on subjects' willingness to sanction a leader, even controlling for the source of the group fund.[5] We then show that ownership is a key component of the established finding that taxation increases citizens' willingness to punish (see e.g. Martin 2014; Paler 2013). Controlling for ownership significantly reduces taxation's effect on punishment, and mediation analysis suggests that higher ownership is responsible for approximately 41% of taxation's effect on punishment. We replicate these results using experiments conducted in Accra, Ghana. We also use observational survey data to show that these results hold outside of the lab; measures of ownership over the Ugandan government's budget are correlated with self-reported willingness to engage in political action when corruption is observed.

Additional experiments in Uganda provide compelling support for a claim that is especially relevant to policymakers: that it is possible to increase citizens' feelings of ownership over non-tax sources, and in doing so to generate accountability pressures equal to those of taxation. We develop experimental treatments that explicitly but simply prompt subjects that they as citizens "own" portions of oil or aid revenues. We find that this significantly increases both feelings of ownership and citizens' willingness to punish low transfers by a leader. This effect is substantively large: subjects in the ownership treatments demanded, on average, statistically indistinguishable amounts from leaders as those in the tax condition. Our work thus suggests that, to the extent that the resource curse is driven by low ownership over government revenues, it may be possible to reduce its impact on governance through

---

[5]We do not argue that ownership is the sole predictor of punishment. Other factors could include societal fairness norms, the cost of punishment, and other individual-level factors.

information interventions that increase psychological ownership. This is especially important in sub-Saharan Africa where a number of countries, including Uganda, have recently discovered oil and where many countries rely on aid for substantial portions of budgets. If ownership is most malleable in the years leading up to and immediately preceding the discovery of natural resources or the influx of budget support in the form of foreign aid, then efforts to foster high ownership over these non-tax revenues early on may be critical in determining whether the resource curse and its aid corollary—and the poor government performance that allegedly attends them—take hold.

2 A Theory of Ownership, Expectations, and Punishment

Why should we expect ownership to affect accountability pressures? This section starts by defining psychological ownership and discussing how it is distinct from legal ownership, loss aversion and the endowment effect. While the definition is relatively simple, its application to budgets is not. We argue that there are reasons to expect dramatic variation in the extent to which citizens feel ownership over government revenues, both across and within revenue sources. When citizens feel strong ownership over government revenues, this increases their expectations that they should benefit from spending. This, in turn, leads to higher dissatisfaction when they do not benefit; ultimately, this results in higher willingness to punish poor-performing leaders.

2.1 Conceptualizing Ownership

Following work in psychology, 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' " (Pierce, Kostova and Dirks 2001). While this definition seems simple, it provides the nuance necessary to understand variation in citizen perceptions while remaining intuitive enough to be easily comprehensible and measurable. One advantage is that psychological ownership is not limited to physical objects; Pierce, Kostova and Dirks (2003) argue that ownership "can also be felt toward nonphysical entities such as ideas, words, artistic creations, and other people." This suggests that it can apply to budgets, which are rarely experienced by citizens as tangible objects.

Another advantage is that this definition clearly 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, Kostova and Dirks 2003). Psychological

and legal ownership do not always align. For example, individuals may feel psychological ownership over something they have no legal right to; consider an employee who feels that she "owns" her work-provided computer, while legal ownership actually resides with the firm. It is also possible for an individual to legally own an item, but fail to feel psychological ownership—a couple may jointly own two cars, yet the individuals only feel psychological ownership over the car they specifically 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 is a much more useful concept for studying budgets, as it focuses on whether citizens *feel* that government revenues belong to them.

Finally, employing the definition of psychological ownership used in psychology research allows us to draw on previous findings to help us understand how ownership affects behavior. Individuals with psychological ownership associate the owned object or idea with the self, incorporating it into their identify (Dittmar 1992; Gawronski, Bodenhausen and Becker 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 therefore affects expectations. Psychological ownership has been shown to affect behavior: as we discuss further below, ownership increases expectations and makes individuals more likely to reject or punish "unfair" divisions of a resource.

When should we expect citizens to feel psychological ownership over government revenues? In democracies, budgets may legally "belong" to citizens, in that they are supposed to be used for their benefit; anti-corruption laws typically forbid bureaucrats' using government funds for their private gain. However, understanding when this translates into psychological ownership over the budget is more complicated. Existing research in psychology has focused on goods for which physical, legal ownership is clearly established and assigned to a particular individual; the classic example is a coffee mug (Kahneman, Knetsch and Thaler 1990). Government budgets, on the other hand, are collectively owned, and there is little work on how individuals develop psychological ownership of commonly held resources.

One possibility is that, in the case of budgets, legal and psychological ownership are equivalent, and thus all citizens should feel strong psychological ownership over them. This is consistent with many 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 funds. In authoritarian or hybrid regimes, or in nominal democracies with weak democratic norms, it is not obvious that citizens should feel ownership over the budget at all.

This is especially true in countries where patronage and clientelism are common. Van de

Walle (2001), for example, argues that in many African countries *prebendalism*—a belief that resources are owned by the government officials who control them—is the norm. In such cases, there is no assumption that citizens should benefit at all, particularly if they are not clients or co-ethnics of the relevant officials (Ekeh 1975; Van de Walle 2001). We therefore expect substantial variation in whether citizens feel ownership over government budgets, even within a given regime type. Some citizens may feel strong ownership over the budget, while others may believe that the budget belongs to politicians, not to them.

Just as ownership may vary among individuals, existing work on the link between taxation and accountability suggests it may also vary according to the revenue's origin. Governments raise revenues from a number of sources including taxation, foreign aid, and natural resources such as oil and other minerals. Previous research suggests that citizens are more likely to hold leaders accountable for how they spend taxes relative to windfall revenues (Paler 2013; Martin 2014). One possible reason: citizens feel higher ownership over tax revenues compared to foreign aid or oil money.

Here, it is important to distinguish ownership as a causal mechanism from loss aversion and the related phenomenon of the endowment effect. Martin (2014) argues that taxation affects accountability through loss aversion; Paler (2013) and Sandbu (2006) discuss taxation's effect as an amalgam of ownership, loss aversion, and the endowment effect. Yet these concepts are distinct. Loss aversion shows that individuals' behavior differs according to whether they are above or below their reference point (in other words, based on expectations) (Kahneman and Tversky 1979), but is agnostic about how reference points are formed in the first place. Ownership, in contrast, explains where these expectations come from, and thus is causally prior to loss aversion. Further, the endowment effect is not a theoretical concept, but rather a specific empirical phenomenon in which there is a gap between the prices at which individuals will buy and sell goods depending on their prior physical possession of the gifted objects (Kahneman, Knetsch and Thaler 1990). We argue that ownership is the psychological mechanism that induces the endowment effect (Shu and Peck 2011), that it can also apply to ideas and collective resources not in the actual physical possession of subjects, and that it thus deserves greater theoretical and empirical attention.

When citizens pay taxes, they contribute their own earned income to the budget; this may induce greater feelings of ownership over public finances more generally. In contrast, citizens who pay few taxes may feel less ownership over the budget. In practice, paying taxes could increase ownership in two ways. Suppose, for example, that a citizen pays $100 in taxes. One possibility is that the citizen then feels very strong ownership over $100 of government spending, but does not feel increased ownership over the rest of the budget. This would lead to very low average ownership over the budget as a whole. An alternative is that

paying even $100 in taxes generates higher, although more diffuse, ownership over the entire budget. This second option is more in line with common rhetoric surrounding taxation: citizens often express dissatisfaction over reports of what they consider wasteful spending by noting that the misused money is "theirs." This may occur even if the amount at stake vastly exceeds the personal contribution. We therefore expect individuals who pay any taxes to feel stronger psychological ownership over all tax revenues. In contrast, individuals who do not pay taxes will feel lower ownership.

The direct contribution to the budget required by taxation increases ownership mechanically: the budget belongs, at least in part, to the contributor. No such mechanism exists for aid and oil revenues, and as such even citizens who pay taxes may feel much less ownership over windfall revenues. Yet lower ownership over windfalls is not automatic. Donors give foreign aid for the express purpose of helping beneficiaries, so recipient citizens may therefore feel that it "belongs" to them. Likewise, although oil revenues are not taken from citizens personally, they are extracted from the country itself, which therefore represents a common legacy. This may lead to higher feelings of resource ownership, especially in oil-producing regions. We therefore expect that, although on average citizens will feel lower ownership over aid and oil relative to taxes, there will be significant variation among citizens depending on how they view windfalls. Furthermore, we suggest that ownership over aid and oil may be malleable: if windfall ownership is based on norms, it may be possible to change these norms in such a way that citizens feel that windfall revenues indeed belong to them.

2.2 Ownership and Punishment

If there is variation in the degree to which citizens feel ownership over government budgets, how should we expect this to affect government accountability? We define an accountable government as one that implements citizens' preferred policies, efficiently and with a minimum of corruption and mismanagement (Fearon 1999). However, governments are unlikely to provide citizens with their preferred policies unless citizens can enforce serious consequences for deviations. 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, protest, or otherwise enact costs on unaccountable leaders? In general, citizens will take action when the expected benefits of doing so exceed the costs. Costs include those of monitoring government behavior, forgoing economic activity, engaging in collective action, and facing the possibility of repression.[6] Much previous work on accountability has focused on improving accountability

---

[6]Even conditional on going to the polls, voting for the opposition may be more costly than voting for the incumbent if it entails forgoing clientelistic transfers or facing repression by the regime.

by decreasing the costs of taking action.[7]

In return for paying the costs of taking action, citizens receive economic and expressive (psychological) benefits. By punishing relatively unaccountable governments, citizens hope to benefit economically from improved government policy in the future. In attempting to sanction, however, citizens face a collective-action problem: an individual's engagement in collective action is unlikely to be pivotal, and a rational citizen can therefore expect to receive economic benefits from successful collective action regardless of her personal participation. Individuals will therefore be most likely to participate when doing so garners private, excludable benefits (Olson 2009). One form these may take is the psychological, expressive benefits of taking action. 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). This is also in line with work on expressive voting, which shows that voting decisions are often not driven by economic considerations (Riker and Ordeshook 1968). Individuals who receive higher expressive benefits from taking action are more likely to punish poor behavior.

We argue that strong feelings of ownership over the government budget increases the expressive benefits citizens receive from punishing leaders' poor performance, and in turn makes citizens more willing to sanction. 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 will more likely pay the costs of action.

We argue that ownership is the lens through which citizens evaluate government performance. It determines citizens' answer to the question: how much should I 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. A citizen with the highest possible feelings of ownership should expect to benefit directly and heavily from government spending and should thus experience high levels of dissatisfaction when money is lost to corruption. Someone with low feelings of ownership, however, will have modest expectations and thus lower levels of dissatisfaction from observing the same level of corruption. As a citizens' dissatisfaction increases, so too will the value of the expressive benefit that comes with punishment. We are not arguing that ownership is the sole determinant of citizens' expectations—for example, Gottlieb (2016) shows that beliefs about state

---

[7]For example, information interventions attempt to decrease the costs of monitoring, while interventions that facilitate meetings between citizens and leaders attempt to minimize collective-action costs.

capacity are also important—only that increasing ownership should increase expectations, ceteris paribus.

There is some existing work suggesting that ownership affects willingness to punish—that "emotions spark when we experience the invasion of what we feel is 'ours'" (Pierce, Kostova and Dirks 2001). Ownership is closely related to the desire and ability to control how the object is used (Pierce, Kostova and Dirks 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, van Dijk and Van Beest 2008; Wu et al. 2012). However, there is little work testing whether these findings will transfer to political or policy contexts.

2.3 Testing the Ownership Effect

The ownership theory suggests three main testable hypotheses regarding how ownership affects punishment, how revenue source affects ownership, and how ownership may prove malleable. Our first hypothesis is that there will be a strong correlation between the degree to which citizens feel ownership over a shared resource and their willingness to punish government spending or corruption:

> **Hypothesis 1** *A greater sense of ownership over the government budget (group fund) will increase citizens' willingness to punish, even controlling for revenue source.*

We test this hypothesis using observational data from both survey questions and lab-in-the-field experiments.

Previous evidence suggests that taxation increases citizens' accountability demands relative to a case where the budget is derived from an unspecified windfall (Martin 2014). One of the hypothesized mechanisms for this effect is that taxation increases citizens' feelings of ownership over the budget (Sandbu 2006; Paler 2013). If taxation increases ownership, and ownership in turn leads to higher citizen demands, then we should find that controlling for ownership reduces the effect of taxation on punishment:

> **Hypothesis 2** *The effect of taxation on punishment is reduced when controlling for ownership.*

Our lab experiments randomly assign the source of the government budget, allowing us to experimentally test this hypothesis. As part of this test, we also replicate the Martin

(2014) finding that taxation increases punishment, and the Paler (2013) finding that taxation increases ownership.

Finally, we argue that psychological ownership over non-earned revenues may be malleable. Research on ownership in psychology has examined two methods of assigning ownership: physically giving individuals money or a good, and assigning indirect ownership through the design of the intervention. We focus on the latter option, using a treatment in the experiments below that simply tells individuals that a portion of the group fund of aid or oil money is theirs but without actually giving them possession of the funds. The intervention is therefore entirely verbal, without physical ownership, as opposed to the tax condition in which subjects hold their wage and then later pay a portion in taxes. We expect the information intervention to increase citizens' sense of ownership over the budget, and through that their willingness to punish. This hypothesis is tested experimentally below, and also gives us a causal estimate of the effect of ownership on punishment.

> **Hypothesis 3** *A treatment that increases ownership over aid or oil revenues will increase willingness to punish, reducing the gap between accountability demands in tax and non-tax conditions.*

3 Case Selection and Experimental Design

3.1 Case Selection

To test our hypotheses, we needed a setting where tax, aid, and oil revenues are all salient. As an initial test of whether ownership over windfall revenues is malleable, we also sought a setting where oil revenues are relatively new, and we might expect norms of ownership to be less firmly set. We also wanted to select a country where accountability is currently relatively low, and thus any findings that might increase accountability pressures would be directly relevant. Finally, we sought a country where our findings might reasonably generalize to other African countries. This is especially important as previous work has found significant cross-national differences in experimental results (Henrich et al. 2006; Dunning et al. 2018).

Uganda meets all of these requirements. As a quasi-authoritarian African state with low levels of development, Uganda is a particularly apt location to test the effects of different revenue sources on accountability pressures.[8] Taxes, foreign aid, and oil revenues are all salient revenue sources in Uganda. All citizens pay value-added taxes. Many also pay some

---

[8]Uganda has a per-capita GDP of $1,634 and development indicators that are at or near the mean for the continent (World Bank 2016).

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 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.

To improve the likelihood that subjects in our sample had exposure to all three sources of revenues, we sampled respondents from Uganda's main urban center, Kampala. Urban citizens often have higher exposure to taxation through their greater participation in the money economy and formal trade; we expect taxation to be more salient in a lab setting among citizens who have experience paying taxes. Urban citizens are also cognizant of the debates over the use of oil revenues and of recent aid scandals. Survey data from our sample shows that an average respondent thought that the government's budget consisted of 35% taxes, 25% aid, 17% oil and 22% debt. The high perceived percentage of oil revenues is especially noteworthy, as the country discovered oil only in 2011, and the government projected that only 0.6% of 2016 revenues would come from oil.[9]

3.2   Experimental Design

Testing the ownership theory requires identifying both an exogenous source of variation in psychological ownership and a credible measure of ownership's impact on citizens' willingness to engage in costly punishment behaviors. We also need to isolate the effect of ownership on the expressive benefits of punishment, holding other benefits and costs constant. Due to their ability to make nearly all parameters uniform, laboratory experiments provide an excellent way to overcome the noted challenges and identify our proposed psychological effect. In our experiments we can measure individuals' willingness to punish even when there is no economic benefit to doing so, and when the costs of punishment are visible and fixed for all citizens. By randomly varying the source of the budget, we can both generate exogenous variation in ownership and measure the effect of ownership within each revenue source. The lab setting also lets us test the effect of manipulating ownership over non-tax revenues. Additionally, post-treatment surveys can provide an initial indication of the external validity of the laboratory results.

Our main results rely on a set of four lab-in-the-field experiments that follow Martin

---

[9]The approved budget for 2016/17 anticipated that funding would be 61.5% tax revenues; 0.6% oil, 7.4% grants, and 30.5% debt, with half of the loans coming at concessionary terms (Government of Uganda 2016).

(2014) in using variations on a standard ultimatum game. These conditions were the Tax game, and three versions of the Windfall game: Aid, Oil, and Grant. Each treatment follows a similar structure and involves a single-shot game between two players, a Citizen and a Leader. In all versions of the game, the Leader chooses how to allocate the group fund between his own salary and the Citizen. The Citizen observes the Leader's decision and decides whether to pay to punish the Leader. The key difference between the conditions is the source of the group fund.

At the start of the game, the Citizen completes a short task to earn a "wage" of 10 MU (Windfall conditions) or 15 MU (Tax condition).[10] Next, in all treatments the Citizen spends 5 monetary units (MU) to purchase a small item that they get to keep; this step is used in a related paper, and is thus not analyzed here.[11] In all treatments, the Leader is then given a group fund of 10 monetary units (MU) that he must divide between his own salary and the Citizen. Following Martin (2014), we refer to the Leader's salary as the group fund to signal that Citizens have some degree of discretion over its disbursement. Additional information about sampling and implementation is available in Section B in the online appendix.

The source of the group fund is the main experimental treatment randomly assigned by game session for all rounds. The group fund could come from taxes paid directly by the Citizen from "wage" payments earned after the small task or from one of three exogenous windfalls: aid explicitly identified and graphically labeled as given by foreign governments, oil revenue similarly highlighted as coming from Uganda's recently discovered oil reserves, or from an unnamed, unearned source referred to here as the "Grant" condition.[12] In the Tax condition, the Citizen has an initial endowment of 15 MU, buys a good for 5 MU, then pays a tax of 5 MU. The 5 MU is doubled to 10 MU before it is given to the Leader. In the Windfall conditions, the Citizen is given an endowment of 10 MU, pays 5 MU for a small item, and remains with 5 MU, which is not taxed. The 10 MU group fund is then given to the Leader as an exogenous transfer.

The Citizen and Leader then simultaneously make their decision. The Leader decides how much of the group fund to keep for his own "salary" and how much to pass to the Citizen. Before the Citizen observes the Leader's allocation decision, she is asked to specify whether she wishes to pay to punish the Leader for each of the 10 possible allocations of the group

---

[10]See full description in Section G of the online appendix. In survey measures 85% agreed with the statement that they had "earned their wage." Previous research suggests that individuals have stronger ownership over money that they earned (Pierce, Kostova and Dirks 2003).

[11]See details in Section G of the online appendix. Purchasing was implemented uniformly across all treatment conditions and thus poses no inferential threat. To control for any effects of purchasing and to improve precision, all analysis includes fixed-effects for the item each subject purchased during this phase.

[12]Because the source is unnamed in the grant condition, it serves as a pure control for the two named non-tax sources.

Stage	Tax Game	Windfall game		
		Unspecified Grant	Aid	Oil
1	The citizen earns a wage of 15 MU.	The citizen earns a wage of 10 MU.		
2	The citizen purchases a small item for 5 MU.	The citizen purchases a small item for 5 MU.		
3	The citizen is taxed 5 MU. This is doubled to 10 MU and given to the leader as the group fund.	The leader is given 10 MU as the group fund.		
4	The Leader allocates 10 MU between himself and the Citizen.			
5	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.			

Table 1: Timing of Tax and Windfall Games. This table displays the steps for each treatment condition.

fund from a transfer of 0 to the Citizen up to a payment of 10. If punishment occurs, the Citizen pays 1 MU and the Leader pays a fine of 4 MU; neither player receives the money that is taken away in punishment. After the Leader makes an allocation, punishment takes place or not according to the Citizen's pre-specified strategy, and payoffs accrue. Note that, at the time that each player makes their decision, the structure of each treatment is identical: the Citizen has 5 MU (plus the item they purchased), and the Leader has the 10 MU group fund. The steps of each treatment are given in Table 1.

The implementation protocols directly linked each component of the game to the desired theoretical concept. To make the group fund seem more like a government budget, respondents were told that the money the Leader keeps is "his own personal salary, and is not for the citizens or for the community." In contrast, they were instructed to think of the transfer to the Citizen as "money that politicians send to a community for development or other services that benefit the people living there." To draw a connection between punishment in the game and real accountability, the group training explained "You can think of punishment like this. If you are upset with a politician, you can go take action, like voting or going to a demonstration. But, these may cost you a bit—you have to leave work, pay for transport, or make other small sacrifices. If you succeed, the politician may lose office, or have to change a policy. So, he pays a cost as well. In the activity, we represent these costs with the money each loses in punishment."

The experimental design solves several inferential challenges. First, we can isolate the psychological benefits of punishment from economic benefits. Because this is a single-shot game, punishment strictly decreases the citizen's economic utility in all versions of the game. If a citizen receives no expressive benefit from punishment, the unique subgame-perfect Nash equilibrium is for the Leader to offer 0 MU to the Citizen, and for the Citizen to never sanction the leader for any transfer size. Punishment is therefore purely driven by the expressive benefits we discuss in Section 2. We expect high ownership to increase the

psychological disutility that citizens suffer from poor performance, which in turn makes them more willing to pay the costs required for sanctioning.

Second, the lab setting allows us to use revenue and other ownership treatments to create random variation in citizens' feelings of ownership over the group fund. This gives us a causal estimate of the effect of ownership on citizens' willingness to sanction: we expect taxation to increase ownership, and this will mediate the relationship between taxation and punishment, relative to the three windfall conditions. It also allows us to demonstrate that it is possible to increase psychological ownership over non-earned revenues. We do not argue that revenue type is the only source of variation in ownership, only that it is a significant and substantively meaningful source of variation.

Finally, one concern might be that, because the game involves only a single Citizen, the transfer from the Leader is closer to a clientelistic transfer than a public good. Our protocols addressed this by repeatedly stressing that respondents should think of the transfer as services for the community. In a post-treatment item that asks subjects to characterize the Leader's transfer, 86% of Citizens correctly identified the transfer made by the Leader as more akin to "services like health and education" than "a small something you get from a local leader or candidate" or "a job or other benefit you get from a local leader you know." Our results in Section 7 on the correlations between in-game behavior and "real-world" ownership and political action also help to alleviate these concerns.

3.3 Data and Key Outcomes

The experiments were implemented in six field sites in and around Kampala, Uganda in 2017. At each site, volunteers were recruited to participate from designated areas surrounding the site; we ran three enumeration sessions per day, with 16 respondents in each session. All enumeration was conducted in Luganda, the dominant local language. Treatment was randomly assigned at the session level, and within each session, subjects were randomly assigned to be Citizens or Leaders.[13] In each session, respondents received a group training that described the rules of the assigned treatment, including set examples. Respondents then met with enumerators and played a practice round, followed by five single-shot rounds of the game. Citizens were randomly paired with a Leader in each round, and all pairings were anonymous. While a Citizen could play with the same Leader more than once, it was never twice in a row, and respondents were told in between each round that their next pairing

---

[13]Each session consisted of 12 Citizens and 4 Leaders. Respondents were not told what the ratio of Citizens to Leaders would be, only that it would be randomly assigned. To avoid deception, each Leader played with 3 citizens in each round. All thresholds and resulting punishment decisions were communicated to the Leader, such that Leaders effectively played three sub-rounds, one for each Citizen pair. One of these sub-rounds was then chosen for the Leader's payout, so all pairings were potentially payoff-relevant.

would be different. Because pairings were anonymous, neither the Citizen nor the Leader could tell the identity of the other or whether they had played together before.[14] Each round can therefore be taken as a single-shot game. Following the final round, respondents completed a post-treatment survey that included our ownership measures and covariates. For enumeration 1 MU was set to 100 Ugandan Shillings (UGX), and respondents received their payouts from 3 randomly-selected rounds of the game.[15]

The resulting dataset consists of 570 Citizens who played a total of 3,420 rounds.[16] Approximately one-half the sample was assigned to the Tax treatment (n = 286), while the remaining half was split equally between the Oil (n = 95), Aid (n = 93) and Grant conditions (n = 96). Small discrepancies in cell size are driven by missingness due to attrition in recruitment or subjects leaving the session early. Chi-squared tests for covariate balance are consistent with successful randomization.[17]

Our key outcome of interest is the "punishment threshold" for each citizen, defined as the smallest transfer made by the Leader at which the Citizen does not punish. For example, if a Citizen reports that she would punish the Leader if he passed back 3 MU or less of the 10 MU group fund, but not 4 MU, her punishment threshold for that round is 4 MU. For clarity, our analysis translates the punishment thresholds from Ugandan shillings to MU (100UGX=1MU).

Our ownership measure is based on that used by Pierce, Kostova and Dirks (2001). At the end of the game, each subject was asked how much they agreed with the statement that "the group fund belonged to me." This item was measured using a 10-point ladder with anchors of "Do not agree at all" (a response of 0) and "Strongly agree" (a response of 10). Our measure differs from that of Paler (2013) in that we ask about whether an individual feels personal ownership, rather than asking if the budget belongs to citizens in the region more generally. This distinction is important, since our concept of ownership is an individual-level, psychological quantity.

4   Experimental Results

This section uses the experiments described above to test the relationship between ownership, taxation, and punishment. Analysis is based on a preanalysis plan filed with

---

[14]In our outtake survey only 16.5% of respondents believed that they played with the same Leader each time; our results are robust to controlling for whether subjects believed that they were playing a repeated game.

[15]The average Citizen payout was 4,500 UGX. The group fund of 1,000 UGX is approximately 66% of the median daily wage for our sample.

[16]Those who played as Leaders are not included in the analysis below as they did not set a threshold.

[17]Results available in Section G of the online appendix.

EGAP prior to data collection. First we show that subjects with higher ownership are more willing to pay the cost of sanctioning (Hypothesis 1). We then present support for our claim that a significant portion of taxation's effect on accountability—shown elsewhere and replicated here—is driven by the way in which the direct contribution required by taxation increases psychological ownership (Hypothesis 2).

4.1 Ownership Increases Willingness to Punish Poor Performance

To test whether psychological ownership over the government budget is positively correlated with citizens' willingness to punish, we estimate the following OLS model:

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

The dependent variable Y_{ij} is subject i's punishment threshold in round j and Ownership is the 10-point item discussed above. $\mathbf{X}_{ij}$ is a vector of controls. Some of the controls are from the experiment itself; we control for the Leader transfer in the previous round, and include enumerator and round fixed-effects. To alleviate concerns that ownership and punishment are both driven by some other factor, $\mathbf{X}_{ij}$ also includes a range of demographic characteristics that might plausibly affect ownership and punishment, including respondent age, gender, education, level of poverty, and the quality of local public services. Standard errors are clustered by respondent.

Column 1 of Table 2 reports the effect of ownership in the full sample. As expected, in this observational test ownership is a strong predictor of punishment thresholds: each one-unit increase in ownership corresponds to a 0.36 MU increase in subject thresholds (t = 12.92). Increasing the ownership variable one standard deviation thus corresponds to a 0.75 MU increase in subject thresholds, an increase of approximately 0.54 standard deviations.[18] Columns 2-5 report estimates for each treatment condition separately, demonstrating that ownership remains a substantial and significant predictor of punishment within all four revenue sources. This section provides, to the best of our knowledge, the first evidence suggesting that ownership over budgets is significantly related to willingness to punish how that budget is divided.[19]

---

[18]In contrast, we show below that, controlling for ownership, taxation increases punishment by only 0.17 standard deviations.

[19]All results are robust to controlling for whether a subject could correctly identify, post-treatment, the source of the group fund; this should limit concerns that differences in ownership are driven by differential comprehension across treatment conditions.

	DV: Subject Threshold				
	Pooled	Aid	Oil	Grant	Tax
Ownership	0.362***	0.314***	0.398***	0.218***	0.376***
	(0.028)	(0.071)	(0.059)	(0.071)	(0.038)
Enum + Round FE	✓	✓	✓	✓	✓
Source FE	✓	–	–	–	–
Other Controls	✓	✓	✓	✓	✓
Observations	2125	345	355	355	1070
Adjusted R^2	0.365	0.396	0.512	0.329	0.403

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

Table 2: Impact of Ownership on Punishment Behavior. The dependent variable is subject i's threshold in round j. Column 1 reports the effect of Ownership in the pooled sample; Columns 2-5 report estimates for each individual source. Standard errors (in parentheses) clustered at the respondent level.

4.2 Ownership Drives the Effect of Taxation

In addition to predicting a general effect of ownership on punishment behavior, the theory presented in Section 2 suggests that the well-known finding that taxation leads to increased accountability demands should be driven in part by taxation's effect on ownership. To test this, we show first that taxation positively impacts both ownership and punishment. Then, we show that taxation's effect is reduced when ownership and taxation are both included as predictors.

First, we replicate the main finding in Martin (2014): that taxation increases punishment. To do so, we use the same specification as above but include an indicator for assignment to the tax condition as our independent variable of interest and omit ownership. Column 1 of Table 3 reports the results from this model, and shows that citizens' average punishment threshold is 0.40 MU higher in the tax condition compared to those in the windfall conditions. In online appendix D, we also show that the effect of taxation on punishment is more than two times as large for those who report paying direct taxes in their daily lives; this would be unlikely to hold if the games were not successfully activating respondents' actual experiences with taxation and accountability.

Column 4 of Table 3 reports the results of a similar model in which the dependent variable is ownership. On average, taxation increases ownership by 0.49 points on the 10-point scale, a 0.26 standard deviation increase over the Windfall condition. Column 2 replicates the finding in the previous section that ownership predicts punishment; the slight difference in coefficient is due to not controlling for the source of the group fund. Finally, Column

| | DV: Subject Threshold | | | DV: Ownership |
	(1)	(2)	(3)	(4)
Tax Treatment	0.400***		0.226**	0.493*
	(0.111)		(0.088)	(0.277)
Ownership		0.362***	0.354***	
		(0.028)	(0.028)	
Enum + Round FE	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
Observations	2125	2125	2125	425
Adjusted R^2	0.171	0.302	0.233	0.100

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

Table 3: Impact of Ownership on Punishment and Effect of Taxation The dependent variable is subject i's threshold in round j. Columns 1-3 report the effects of taxation and ownership on citizens' punishment thresholds (SE clustered by subject). Column 4 shows the effect of taxation on ownership. Since there is no by-round variation in either ownership or treatment status, the model in Column 4 is run at the subject level, SE clustered by session.

3 includes both taxation and ownership in the estimation equation. While taxation still has a positive, significant effect on punishment, the size of the effect is reduced from 0.40 MU to 0.23 MU, a decrease of 43%. In contrast, controlling for taxation has little effect on the coefficient on ownership. Furthermore, ownership is a much more powerful predictor of punishment than taxation: a one-standard deviation increase is associated with a 0.68 MU increase in subject thresholds, while the Tax treatment increases thresholds by only 0.40 MU. Section E of the online appendix performs mediation analysis, and shows that ownership accounts for approximately 40% of the effect of taxation; sensitivity analysis—also available in Section E—suggests that this result is robust to a range of likely confounders.

5 Manipulating Ownership over Non-Tax Sources

In Section 2, we argued that assigning indirect ownership over non-tax revenues should produce increases in accountability demands similar to those of taxation (Hypothesis 3). To test this claim, we developed a set of experiments similar to those above, except that in some windfall conditions subjects were assigned ownership over a portion of the non-tax revenues. We demonstrate that, consistent with our expectations, doing so yields a higher willingness to punish and dramatically reduces the differences between willingness to punish across revenue sources.

To test whether ownership is malleable, we conducted two additional sets of experiments

in Uganda. The first set, conducted in January 2017, consisted of five treatments: Tax, Aid, Oil, Grant, Oil Ownership, and Oil Framing. The Tax, Aid, Oil, and Grant conditions were similar to those in the previous section.[20] In this experiment we implemented a similar, but less sensitive, ownership measure in which the text of the question was the same, but the response was coded on a 4-point Likert scale, rather than a 10-point ladder. This less-sensitive scale should bias against finding an effect of ownership on punishment.

The Oil Ownership condition is identical to the Oil condition, except that before the group fund is given to the Leader, citizens are told that 5 MU of the 10 MU group fund "represents the share of the oil money that belongs to you, as the citizen." This closely mimics the script for the tax game, with one key difference: the 5 MU that "belongs" to the citizen is never actually held by them or earned as part of a salary, and as such the citizen makes no direct contribution to the group fund. It also means that no money is ever physically removed from the Citizen. This design choice critically differentiates the Oil Ownership condition from prior studies of the endowment effect. Significant effects should entirely represent psychological as opposed to physical, legal ownership. Because of its anticipated effect on psychological ownership, we expect that this treatment will increase citizens' punishment thresholds.

One concern with the Oil Ownership treatment might be that any difference in punishment is caused, not by ownership, but by a more general framing effect that suggests that the group fund should be spent to benefit the citizen. We therefore designed the "Oil Framing" condition as a placebo treatment. At the same point in the protocols when citizens were assigned individual ownership over the group fund in the Oil Ownership condition, Citizens in the Oil Framing condition were told that "Oil money is meant to belong to all Ugandans, and to be used to benefit citizens like you." While this treatment mentions ownership, it does not assign ownership to individual citizens and thus should differentiate the effects of personal psychological ownership from the effects of a common resource meant to benefit citizens generally. We expect that this Oil Framing treatment will have no impact on citizens' willingness to punish.

The second set of experiments, 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 in the above paragraphs, except that the source was aid; citizens earned their endowment in each round; and psychological ownership was again measured using a 10-point ladder. In the Aid Ownership treatment Citizens were told that 5 MU of the 10 MU

---

[20]The key differences in the game protocols are (1) respondents did not purchase a good during the first stage; (2) due to the lack of purchasing, initial endowments are 5 MU lower in all conditions; and (3) endowments were given to respondents rather than earned through an effort task. All differences are consistent across treatment conditions and therefore pose no threat to inference.

	DV: Subject Threshold
Aid Ownership	0.375***
	(0.140)
Oil Ownership	0.331*
	(0.174)
Enumerator FE	✓
Round FE	✓
Observations	1042/1982
Adjusted R^2	0.206/0.042
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 4: Ownership Treatment Effects on Subject Thresholds. The coefficients on Aid-Ownership is relative to Aid; the omitted category in Row 2 is Oil and Oil Framing. Standard errors clustered by subject. Observations are subject-round and represent the subject-rounds used to estimate the effect of the ownership treatments for aid and oil respectively.

group fund "represents the share of the aid money that belongs to you, as the citizen." We expect that citizens' punishment thresholds will be significantly higher in the Aid Ownership condition relative to the Aid condition.

Row 1 of Table 4 reports the result of the Aid and Aid Ownership treatments. The coefficient is from a regression of citizens' punishment thresholds against a dummy for the Aid Ownership treatment; the omitted category is the Aid treatment.[21] Row 2 reports the coefficient on Oil Ownership from a similar regression in which Oil and Oil Framing form the omitted category. The ownership treatment raises thresholds substantially, increasing the average threshold by 0.375 MU ($p = 0.007$) in the Aid Ownership treatment and 0.303 MU ($p = 0.057$) in the Oil Ownership treatment. These changes are as Hypothesis 3 would predict: a prompt that induces psychological ownership over non-tax revenue increases subjects' willingness to punish. Also as predicted, the placebo Oil Framing treatment has no significant effect on Citizens' punishment thresholds and therefore was included as a comparison condition in the Oil Ownership estimates to improve statistical power (see Figure 1).

The increase in punishment thresholds induced by the ownership treatment is similar in magnitude to that produced by direct taxation. Figure 1 plots the estimated effects of each treatment. The Tax, Oil Ownership, and Oil Framing results all come from the January 2017 data, with Oil as the omitted baseline category. The Aid Ownership coefficient uses

---

[21]The estimation equation, similar to those above, includes enumerator and round fixed-effects and a one-round lag of the Leader's transfer. Unlike the models reported in the previous section, these do not include subject-level covariates. We exclude them here because, since we manipulate ownership with a treatment (and thus do so randomly), we do not need to control for potential confounders.

Aid as the reference condition and comes from the July 2017 data. The figure shows that the increase in willingness to punish generated by inducing subjects to feel psychological ownership over non-tax revenues is statistically indistinguishable from that generated by taxation.[22]

This finding is noteworthy for two reasons. First, it is in line with results reported in previous sections that suggest ownership is the major driver of punishment behavior. While many factors may be responsible for generating accountability pressures, the ability of a subtle ownership treatment to produce such large changes in subjects' punishment thresholds is consistent with our theory that ownership is a key causal mechanism that connects government spending with willingness to sanction misuse of government revenues.

Second, this finding has major implications for national policymakers and international organizations working in countries that rely on windfall revenues such as aid or oil. Both Ugandan citizens and international observers are concerned that recent oil discoveries may worsen accountability. Our results suggest that interventions designed to increase citizens' sense of personal ownership over non-tax revenues can have a large impact on citizens' willingness to pressure elected officials to use non-tax revenues efficaciously.

6   Ownership in Ghana: Evidence from a Replication

The previous sections demonstrate that ownership provides substantial analytic leverage in predicting when citizens are willing to punish leaders, that ownership varies both across and within revenue sources, and that manipulating ownership over non-tax sources can produce accountability pressures statistically indistinguishable from those caused by taxation. One potential criticism of these results may worry that they apply only (or at the very least particularly) to Uganda, perhaps due to its quasi-authoritarian nature or citizens' relatively limited experience with direct taxation. This is especially important in light of recent research suggesting that experimental results often fail to replicate in other contexts (Dunning et al. 2018).

To address this concern, we use data from a similar but earlier set of experiments conducted by the authors in 2016 in Accra, Ghana. Ghana is substantially wealthier than Uganda, with a GDP per capita of US $3,784 compared to Uganda's $1,634. It also has a much larger tax base than Uganda, is less aid dependent, and receives significantly more government revenue from oil. Tax hikes there have provoked significant protest and other citizen

---

[22]Because the Aid Ownership results come from a different dataset, we cannot directly test whether the effect sizes of Aid Ownership and Oil Ownership are the same. When the Oil Framing treatment is not pooled, the effect of the Ownership treatment is diminished somewhat and fails to reach significance, but is of similar magnitude and in the correct direction (ATE = 0.28, $p = 0.15$).
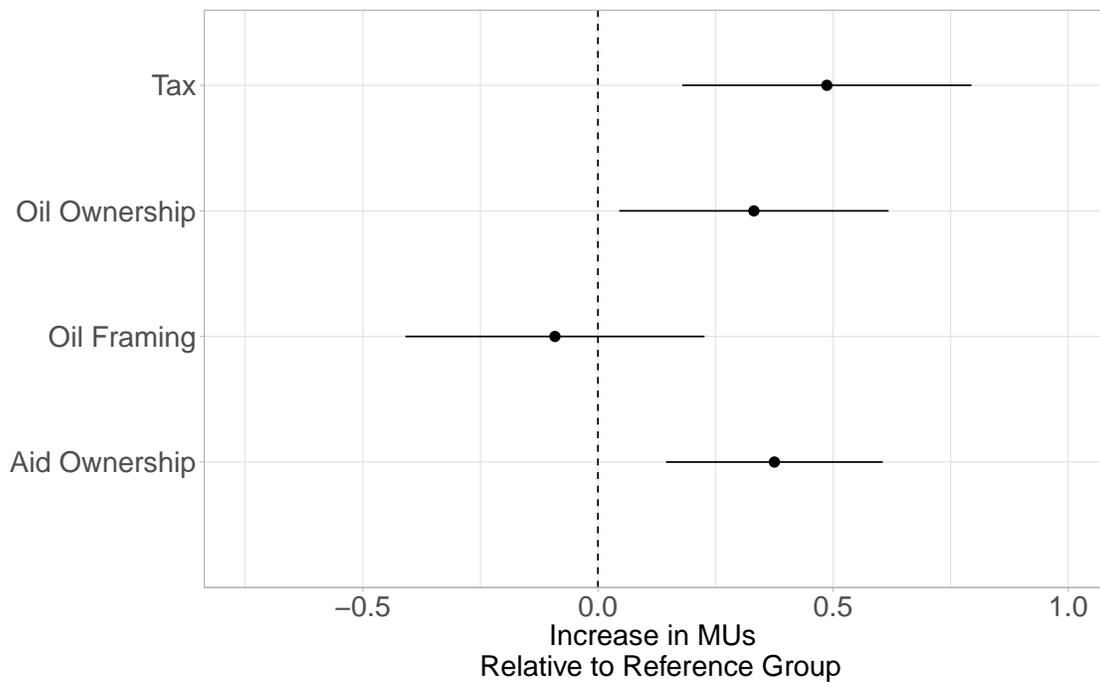
Figure 1: Comparing the Effect of Indirect Ownership to Direct Taxation. This figure plots the effects of the Aid Ownership, Oil Ownership and Oil Framing treatments against the effect of Taxation. Effects represent the increases in Monetary Units (MUs) compared to the reference group of each model. The comparisons are Tax vs. Oil; Oil Ownership vs. Oil; Oil Framing vs. Oil; and Aid Ownership vs. Aid. 90% confidence intervals constructed using subject-clustered standard errors.

	DV: Subject Threshold			DV: Ownership
	(1)	(2)	(3)	(4)
Tax Treatment	0.364**		0.226	0.123***
	(0.179)		(0.182)	(0.035)
Ownership		0.798***	0.761***	
		(0.189)	(0.194)	
Enum + Round FE	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓
Observations	2006	2006	2006	502
Adjusted R^2	0.157	0.189	0.191	–

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

Table 5: Impact of Ownership on Punishment Behavior. The dependent variable is subject i's threshold in round j, such that observations are subject-round. Because our ownership measure in Ghana is a binary variable and neither ownership nor treatment status varies by round, estimates in Column 4 generated using a linear probability model on a subject-level dataset, and as such the number of observations are equal to the number of subjects. Results are robust to a logistic model.

activism (Prichard 2015). Unlike Uganda, Ghana has competitive multi-party elections and a recent history of peaceful leadership transitions.[23]

The experiments consisted of Tax, Aid, Oil, and Grant treatments identical to those conducted in Uganda in January 2017. Section F.1 of the appendix describes the experiments in more detail. For enumeration, 1 MU was set equal to 0.5 Ghanaian cedis. The ownership measure used in Ghana is a binary variable that takes a value of 1 if subjects agreed or strongly agreed with the statement "the group fund belongs to me" and 0 if the subject disagreed or disagreed strongly. The Ghanaian sample was recruited via random-walk sampling and was designed to be representative of Accra. This partially addresses potential concerns that the effects in Uganda may have been driven in part by the tendency of convenience samples to over-represent young, uneducated men.

The Ghana results are consistent with our findings regarding the relationships between ownership, punishment, and taxation identified in Uganda. Table 5 reports the results from a specification nearly identical to that used to test Hypothesis 1 in Uganda, and it demonstrates that ownership is significantly and positively related to punishment thresholds: subjects who report feeling strong ownership over the group fund demand approximately 0.80 MU more from the Leaders than those who do not ($p \approx 0$). As in Uganda, taxation increases the

---

[23]These experiments were conducted prior to those in the previous sections, but were not collected with the express purpose of testing the ownership effect. The study was pre-registered, but hypotheses related to ownership were not included in the protocol.

likelihood a citizen reports strong ownership, here by 12.2 percentage points ($p \approx 0$). While taxation increases punishment when ownership is not included, the effect is reduced by 38% when ownership is included in the model and the coefficient on the tax treatment becomes statistically insignificant.[24]

7 Observational evidence

Another concern with the findings thus far might be that a stylized laboratory experiment, even one conducted in two different countries, is unlikely to produce findings that stand up in more "real-world" settings. This is especially important as the connection between taxation and the budget was extremely strong in our experiments: citizens paid a 50% tax that was directly transferred to the Leader, and Citizens could clearly observe how the group fund was spent. Similarly, citizens did not face the strong barriers to punishment they might confront outside the lab, where meaningful punishment requires collective action and could invite the possibility of repression.

To address this, we replicated our main analysis using questions from a post-treatment outtake survey to the experiments conducted in July 2017 in Uganda that measured "real-world" ownership and willingness to punish. The sample includes 1,397 respondents, consisting of subjects from the experiments in Section 4, plus respondents from additional treatments not related to this paper. One set of three questions measured on a 10-point scale the degree to which respondents felt ownership over central government revenues derived from aid, oil, and taxes; for the analysis below we use the average of these three measures. Another set of questions measured self-reported willingness to engage in four possible costly actions—going to a protest, talking to a neighbor, contacting an elected official, and campaigning against a corrupt official—that citizens could take in response to a corruption scandal. Responses were measured using a 10-point likelihood scale. For analysis we use average the responses to the four questions.[25]

Two initial tests suggest that the lab results are a good measure of actual citizen preferences. First, for subjects who played Citizens in the experiments, we have both lab and survey measures of ownership and punishment. OLS regressions find a strong correlation between the lab and survey measures—$t = 8.22$ and 3.16 for ownership and punishment, respectively—even controlling for the respondent's treatment condition and a range of subject-level covariates. These controls include respondents' level of poverty, the quality of local public goods

---

[24] Additional results, including a mediation analysis similar to that conducted in Uganda, are reported in Section F of the online appendix. Mediation analysis reveals that approximately 46% of the effect of taxation is driven by ownership.

[25] Full text of all questions available in Section H of online appendix.

	DV: Pr(Take Action)				
	Index	Campaign	Protest	Neighbors	Contact
Ownership	0.099***	0.069*	0.094**	0.110***	0.123***
	(0.026)	(0.038)	(0.040)	(0.037)	(0.038)
Enumerator FE	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
Observations	1397	1396	1397	1397	1397
Adjusted R^2	0.139	0.136	0.089	0.086	0.140

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

Table 6: Impact of Ownership over Ugandan Budget on Theoretical Willingness to Sanction. This table shows the coefficients for respondents' reported ownership over the Ugandan government's budget regressed on reported willingness to engage in political activity as a result of a corruption scandal. Ownership and willingness to punish are both measured on a 10-point scale. Estimation uses OLS regression and clusters errors by session.

in their community, gender, age, and education. Second, respondents who reported paying direct taxes report 0.43 points higher ownership over tax revenues, on average, relative to untaxed subjects ($p = 0.02$). This corresponds to a 0.17 standard deviation increase and replicates a similar finding in the lab that taxation increases Citizens' ownership over the group fund.

The results in Table 6 show that Hypothesis 1 also holds for our observational survey measures: respondents who report higher average ownership over the Ugandan government's budget also report significantly higher average willingness to engage in political actions as the result of a corruption scandal; the results hold for each possible political action. The effect of ownership on subjects' hypothetical willingness to take costly political action mirrors that found in the lab. It is also substantively meaningful: a one standard deviation increase in ownership corresponds to 0.23 unit change on the 10-point willingness index. This provides initial evidence that the ownership framework can be used to predict willingness to engage in collective action outside of a lab setting.

8 Discussion and Conclusion

This paper develops and tests a framework linking citizens' psychological ownership over the government budget to their willingness to punish poor government performance. While the idea that citizens will punish when they feel that government dollars "belong" to them is not new, this paper is the first to turn this intuition into a feasible framework for studying accountability. Drawing on research in cognitive psychology, we propose an

ownership-based framework to understand when and under what conditions citizens will hold leaders accountable for poor performance.

Using lab-in-the-field experiments in Uganda and Ghana, we show that ownership has a substantively large and statistically significant effect on subjects' willingness to punish leaders for their spending behavior, and that taxation increases citizens willingness to punish by increasing ownership. We further show that, contrary to previous theories of taxation and accountability, it is possible to increase citizens' psychological ownership over non-earned revenues like aid and oil, and that this induces untaxed subjects to demand accountability similar to that of taxed subjects. Together, these results thus constitute compelling evidence in favor of an ownership mechanism, one that explains both why taxation increases accountability pressure and the substantial between-subject variation in willingness to punish even when controlling for revenue source. Observational data demonstrates that ownership is a significant predictor of expressed willingness to punish outside of a lab setting.

These results help to explain why, in several studies, citizens' willingness to sanction misuse of non-tax revenues, while sometimes (but not always) lower relative to tax-based revenues, is high in absolute terms. For example, Paler (2013) finds high willingness to take action even in a windfall condition. Similarly, survey experiments in de la Cuesta et al. (2017) find that roughly half of all subjects in windfall conditions chose to donate to non-profit watchdog organizations. By highlighting both that subjects can have ownership over non-tax sources (Section 7), and that manipulating ownership can produce accountability pressures similar to those of taxation (Section 4), we provide a novel mechanism that can explain both why taxation induces greater willingness to punish but also why and under what conditions we should expect to see accountability for spending derived from non-tax sources.

Our results suggest several avenues for future research. First, more work is needed to determine whether tax modality affects the degree to which citizens feel ownership over the budget. In a related paper, we show that taxation's effect on citizen behavior is higher for direct taxes, relative to less-visible indirect taxes. Second, our results suggest a potential way to increase citizens' willingness to monitor and sanction governments who misuse windfall revenues. A natural next step is to replicate our ownership treatments in a natural field setting to test their ability to increase real-world sanctioning by citizens in countries that rely heavily on windfalls.

References

Baskaran, Thushyanthan and Arne Bigsten. 2013. "Fiscal capacity and the quality of government in sub-Saharan Africa." *World Development* 45:92–107.

Bates, Robert and Da-hsiang Donald Lien. 1985. "Taxation , Development , Representative Government." *Politics and Society* 14(1):53–70.

Beblawi, Hazem and Giacomo Luciani. 1987. *The Rentier State.* Croom Helm.

Bräutigam, Deborah A and Stephen Knack. 2004. "Foreign Aid, Institutions, and Governance in Sub-Saharan Africa." *Economic Development and Cultural Change* 52(2):255–285.

Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti and Guido Tabellini. 2013. "The Political Resource Curse." *American Economic Review* 103(5):1759–96.

Caselli, Francesco and Tom Cunningham. 2009. "Leader behaviour and the natural resource curse." *Oxford Economic Papers* 61(4):628–650.

Casey, Katherine E., Rachel Glennerster and Edward Miguel. 2012. "Reshaping Institutions: Evidence on Aid Impacts Using a Pre-analysis Plan." *Quarterly Journal of Economics* 127(4):1755–1812.

Chaudhry, Kiren Aziz. 1998. *The Price of Wealth: Economies and Institutions in the Middle East.* Cornell University Press.

De Kadt, Daniel and Evan S Lieberman. 2016. Nuanced Accountability: Voter Responses to Service Delivery in Southern Africa. Technical report Mimeo: Massachusetts Institute of Technology.

de la Cuesta, Brandon, Helen V Milner, Daniel Nielson and Steve Knack. 2017. "Taxation Without Representation? Experimental evidence from Ghana and Uganda on citizen action toward oil, aid, and taxes.".
URL: *https://scholar.princeton.edu/sites/default/files/hvmilner/files/uganda-survey-experiment.pdf*

Dittmar, Helga. 1992. *The social psychology of material possessions: To have is to be.* Harvester Wheatsheaf and St. Martin's Press.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-de Silanes and Andrei Shleifer. 2008. "The law and economics of self-dealing." *Journal of Financial Economics* 88(3):430–465.

Dunning, Thad, Guy Grossman, Macartan Humphreys, Susan Hyde and Craig McIntosh. 2018. *Metaketa I: The Limits of Electoral Accountability.* Cambridge University Press, forthcoming.

Ekeh, Peter P. 1975. "Colonialism and the two publics in Africa: A theoretical statement." *Comparative studies in society and history* 17(01):91–112.

Fearon, James D. 1999. Electoral Accountability and the Control of Politicians: Selecting Good Types versus Sanctioning Poor Performance. In *Democracy, Accountability, and Representation*, ed. Adam Przeworski, Susan C Stokes and Bernard Manin. Cambridge University Press.

Fehr, Ernst and Simon Gächter. 2000. "Cooperation and Punishment in Public Goods Experiments." *American Economic Review* 90(4):980–994.

Fisman, Raymond and Roberta Gatti. 2002. "Decentralization and Corruption: Evidence from US Federal Transfer Programs." *Public Choice* 113(1-2):25–35.

Gadenne, Lucie. 2015. "Tax Me, But Spend Wisely: The Political Economy of Taxes, Evidence from Brazilian Local Governments." *https://sites.google.com/site/lgadenne/home/research* .

Gawronski, Bertram, Galen V Bodenhausen and Andrew P Becker. 2007. "I like it, because I like myself: Associative self-anchoring and post-decisional change of implicit evaluations." *Journal of Experimental Social Psychology* 43(2):221–232.

Gottlieb, Jessica. 2016. "Greater Expectations: A Field Experiment to Improve Accountability in Mali." *American Journal of Political Science* 60(1):143–157.

Government of Uganda. 2016. "Approved Budget Estimates of Revenue and Expenditure (Recurrent and Development), FY 2016/17.".
URL: *http://budget.go.ug/budget/national-budgets-documents?field_ document_ typenational_ budg_ tid=*

Henrich, Joseph, Richard McElreath, Abigail Barr, Jean Ensminger, 4Clark Barrett, Alexander Bolyanatz, Juan Camilo Cardenas, Michael Gurven, Edwins Gwako, Natalie Henrich et al. 2006. "Costly punishment across human societies." *Science* 312(5781):1767–1770.

Kahneman, Daniel and Amos Tversky. 1979. "Prospect theory: An analysis of decision under risk." *Econometrica: Journal of the econometric society* pp. 263–291.

Kahneman, Daniel, Jack L Knetsch and Richard H Thaler. 1990. "Experimental tests of the endowment effect and the Coase theorem." *Journal of political Economy* 98(6):1325–1348.

Leliveld, Marijke C, Eric van Dijk and Ilja Van Beest. 2008. "Initial ownership in bargaining: Introducing the giving, splitting, and taking ultimatum bargaining game." *Personality and Social Psychology Bulletin* 34(9):1214–1225.

Levi, Margaret. 1989. *Of Rule and Revenue.* University of California Press.

Mahdavy, H. 1970. The Patterns and Problems of Economic Development in Rentier States: the Case of Iran. In *Studies in the Economic History of the Middle East*, ed. M.A. Cook. pp. 428–677.

Martin, Lucy. 2014. "Taxation , Loss Aversion , and Accountability: Theory and Experimental Evidence for Taxation's Effect on Citizen Behavior.".

Moore, Mick. 2004. "Revenues, state formation, and the quality of governance in developing countries." *International Political Science Review* 25(3):297–319.

Morrison, Kevin M. 2009. "Oil, Nontax Revenue, and the Redistributional Foundations of Regime Stability." *International Organization* 63(01):107.

Morrison, Kevin M. 2015. "Natural Resources and Development." *Emerging Trends in the Social and Behavioral Sciences* .
URL: *http://search.ebscohost.com/login.aspx?direct=true&db=ecn&AN=0972994&site=ehost-live*

North, Douglas C. and Barry R. Weingast. 1989. "Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England." *The Journal of Economic History* 49(04):803–832.

Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence From a Field Experiment in Indonesia." *Journal of Political Economy* 115(2):200–249.

Olson, Mancur. 2009. *The logic of collective action.* Vol. 124 Harvard University Press.

Paler, Laura. 2013. "Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government." *American Political Science Review* 107(04):706–725.
URL: *http://www.journals.cambridge.org/abstract_S0003055413000415*

Persson, Anna and Bo Rothstein. 2015. "It's My Money: Why Big Government May Be Good Government." *Comparative Politics* 47(2):231–249.

Pierce, Jon L, Tatiana Kostova and Kurt T Dirks. 2001. "Toward a theory of psychological ownership in organizations." *Academy of management review* 26(2):298–310.

Pierce, Jon L, Tatiana Kostova and Kurt T Dirks. 2003. "The state of psychological ownership: Integrating and extending a century of research." *Review of general psychology* 7(1):84.

Prichard, Wilson. 2015. *Taxation, Responsiveness and Accountability in Sub-Saharan Africa: The Dynamics of Tax Bargaining.* Cambridge University Press.

Reinikka, Ritva and Jakob Svensson. 2005. "Fighting corruption to improve schooling: Evidence from a newspaper campaign in Uganda." *Journal of the European Economic Association* 3(2-3):259–267.

Riker, William H and Peter C Ordeshook. 1968. "A Theory of the Calculus of Voting." *American political science review* 62(1):25–42.

Ross, Michael L. 2004. "Does Taxation Lead to Representation?" *British Journal of Political Science* 34(02):229–249.

Ross, Michael L. 2012. *The Oil Curse: How Petroleum Wealth Shapes the Development of Nations.* Princeton University Press.

Sandbu, Martin E. 2006. "Natural Wealth Accounts: A Proposal for Alleviating the Natural Resource Curse." *World Development* 34(7):1153–1170.

Schumpeter, Joseph A. 1991. The crisis of the tax state. In *The Economics and Sociology of Capitalism*, ed. Richard Swedberg. Princeton: Princeton University Press pp. 99–141.

Shu, Suzanne B and Joann Peck. 2011. "Psychological ownership and affective reaction: Emotional attachment process variables and the endowment effect." *Journal of Consumer Psychology* 21(4):439–452.

Smith, Alistair. 2008. "The Perils of Unearned Income." *The Journal of Politics* 70(03):780–793.

Timmons, Jeffrey F. 2005. "The Fiscal Contract: States, Taxes, and Public Services." *World Politics* 57(4):530–67.

Van de Walle, Nicolas. 2001. *African economies and the politics of permanent crisis, 1979-1999.* Cambridge University Press.

World Bank. 2016. *World Bank Development Indicators.* Washington, D.C.: The World Bank.
URL: *http://data.worldbank.org/data-catalog/world-development-indicators*

Wu, Yin, Jie Hu, Eric van Dijk, Marijke C Leliveld and Xiaolin Zhou. 2012. "Brain activity in fairness consideration during asset distribution: does the initial ownership play a role?" *PloS one* 7(6):e39627.

A Appendix

B Implementation in Uganda

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 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.

C   Experimental Game Design

The steps for the Tax and Grant versions are very similar to those in Martin (2014).[26] 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).[27] 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. The game board for the Oil condition is given in Figure 5; game boards for the remaining conditions differ only in the image on the source tile.

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.[28] 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.

---

[26]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.

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

[28]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?"

D Additional Experimental Results from Uganda

Table 7 reports the estimates of models identical to those run Section 4.1 but for each non-tax source individually. It demonstrates that taxation increases ownership over the government budget by approximately 8% relative to the windfall conditions. Comparisons within specific non-tax sources are underpowered owing to small sample size, but the point estimates remain substantively large and in the predicted direction even when they fail to reach conventional significance levels. The one exception is the coefficient on the Oil result in Panel A, where the relatively small magnitude of the coefficient suggests that punishment for those in the Oil condition is not significantly different than those in the Tax condition. We are hesitant to interpret these results too strongly due to the small sample size of the by-source comparisons.

Table 8 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.

PANEL A: Effect of Taxation on Punishment

	Reference Group			
	Non-Tax	Aid	Oil	Grant
Tax Effect	0.400***	0.528***	0.185	0.567***
	(0.111)	(0.149)	(0.162)	(0.139)

PANEL B: Effect of Taxation on Ownership

	Reference Group			
	Non-Tax	Aid	Oil	Grant
Tax Effect	0.493*	0.461	0.470	0.591*
	(0.277)	(0.470)	(0.362)	(0.330)

PANEL C: Taxation and Ownership on Punishment

	Reference Group			
	Non-Tax	Aid	Oil	Grant
Tax Effect	0.226**	0.379***	0.00	0.362**
	(0.08)	(0.114)	(0.121)	(0.127)
Ownership Effect	0.354***	0.360***	0.383***	0.351**
	(0.028)	(0.033)	(0.032)	(0.034)
Enumerator FE	✓	✓	✓	✓
Other Controls	✓	✓	✓	✓

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

Table 7: Impact of Taxation and Ownership on Punishment. Top rows report regression coefficients; bottom rows report standard errors. Enumerator fixed-effects and additional controls used but omitted for presentation purposes. Standard errors clustered on subject for Panels A and C. Estimates in Panel B are from an individual-level model since neither ownership nor treatment status varies across rounds. Standard errors for these models are clustered at the session level.

	DV: Subject Threshold		
	Full	No Income Tax	Paid Income Tax
Effect of Taxation	0.391***	0.315***	0.837***
	(0.111)	(0.118)	(0.288)
Enumerator + Round FE	✓	✓	✓
Number of Obs	2100	1825	275
Adjusted R^2	0.161	0.162	0.374

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

Table 8: Heterogeneous Response to Tax 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 taxation 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 treatment on them is nearly three times that for non-taxpayers.

E Causal Claims using Mediation Analysis

In Section 4.2, we demonstrated that the effect of taxation comes largely through ownership, and the relative effect of ownership is greater than that of taxation. This section uses mediation analysis to test what fraction of taxation's effect comes from ownership. To sustain a causal interpretation of these results, we rely on the standard ignorability assumption. We must do so because, while our ownership measure is taken with respect to an experimental quantity—the government budget over which the Leader makes an allocation—it may be driven by subject-level traits such as education or income. These traits may also be correlated with subjects' willingness to punish Leaders, inducing omitted variable bias. While the effect is strongly robust to the inclusion of plausible confounders, this test remains quasi-experimental, combining as it does a post-treatment survey item (our ownership measure) with an experimental dependent-variable (subject punishment behavior in the game).

To overcome this problem, we first show that direct contribution to the government budget via taxation increases subjects' ownership over that budget (referred to in the game as the "group fund"). Because assignment to the Tax condition is random, subjects' ownership in the Tax condition is therefore a function of an endogenous, intrinsic ownership and the exogenous ownership induced by the Tax treatment. We then exploit this exogenous variation through mediation analysis, treating taxation as the treatment and ownership as the mediator through which taxation increases accountability pressures. 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 that our results are robust to strong violations of the assumption.

Mediation analysis decomposes the treatment effect into a direct effect—in this case, the effect of taxation—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.[29]

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

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

and the outcome model as:

$$\text{Threshold}_{ij} = \alpha + \beta \text{Tax}_i + \delta \text{Ownership}_i + \boldsymbol{\gamma} \mathbf{X}_{ij} + \epsilon_i$$

where the definition of all variables is the same as in Section 4.1.[30]

Figure 2 shows that Hypothesis 3 holds: the strength of a respondent's ownership over the group fund accounts for approximately 41% of the higher willingness to punish in the Tax condition relative to Windfall conditions (ACME = 0.169, $p = 0.002$). The raw results on which the figure are built are available in Table 9 below. Because the mediation effect comes only from the exogenous change in ownership induced by taxation, 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. In Section E.1 below we show that the effect is robust to a multiple-mediator model that includes a measure of fairness considerations, a mechanism that might reasonably be correlated with both ownership and punishment behavior. We also conduct a general sensitivity analysis and

---

[29]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 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 below, there is a substantial direct effect of the treatment, making the tax treatment a poor candidate instrument for ownership.

[30]The inclusion of the control variables increases precision but does not affect the substantive magnitude of the results or change their statistical significance.
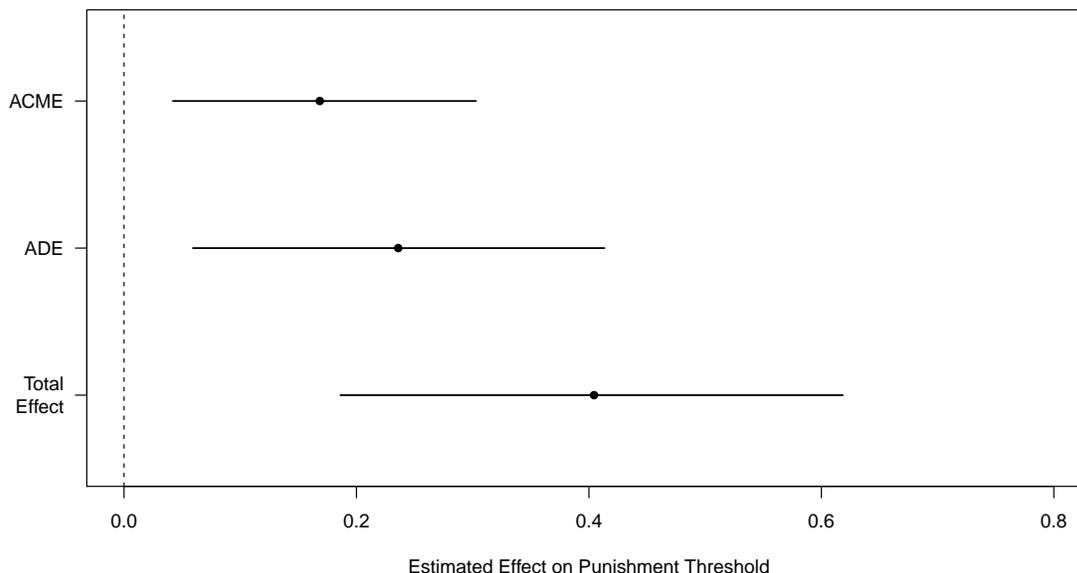
6

Figure 2: Identifying the Causal Effect of Ownership via Mediation Analysis. The ACME demonstrates that ownership strongly mediates the relationship between taxation and punishment. Observations are subject-round.

find that our effect is robust to an omitted mechanism that explains upwards of 90% of the unexplained variance in either the outcome or mediator model.

E.1 Sensitivity Analysis

The identifying assumption of mediation analysis 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 taxation; (2) causally related to ownership; and (3) predicts punishment behavior. In substantive terms, such a violation would require that there exists another mechanism connecting taxation and punishment behavior, and that this mechanism is also causally related to ownership.

One mediator that might meet these conditions is a fairness mechanism. If taxation induces a financial loss that subjects wish to recover, this should manifest itself with greater sensitivity to receiving a "fair" transfer from the leader, and a greater disutility from receiving a lower-than-expected transfer. If fairness is a function of how much of the group fund subjects believe they are rightfully entitled to, then fairness may also affect ownership. To

	Estimate	95% Conf. Int.	p
ACME	0.169***	[0.038, 0.300]	0.002
ADE	0.238***	[0.071, 0.410]	0.006
Total Effect	0.407***	[0.194, 0.610]	≈ 0
Prop. Mediated	0.413***	[0.128, 0.740]	0.002

Table 9: Identifying the Causal Effect of Ownership via Mediation Analysis. The quantity of interest is the AMCE (Row 1), which demonstrates that ownership strongly mediates the relationship between taxation and punishment. The large and statistically significant ACME is consistent with Hypothesis 2, which predicts that the effect of taxation should come in part through ownership. Observations are subject-round. Standard errors clustered on subjects.

test whether this was the case, we modeled fairness as an independent mediator and found that it was neither affected by taxation nor correlated with ownership.

The fairness results notwithstanding, one might be concerned about other potential confounders. In order to assess the sensitivity of ownership to unmodeled confounders more generally, we conduct a sensitivity analysis to determine the amount of selection on unobserved mediators that would be necessary to account for the ownership effect we observe.[31]

There are two relationships of interest with respect to the potential confounder: how strongly does it predict ownership, our mediator of interest, and how strongly does it predict punishment behavior, our outcome? To answer this question, we consider the predictive power of the hypothetical confounding mediator in explaining the variance in the mediator and outcome models that is not already accounted for with pre-treatment controls, experimental variables, and, in the case of the outcome itself, our ownership measure. The left pane of Figure 3 plots the mediation effect of ownership against selection as measured by ρ, the traditional summary measure in sensitivity analysis. The figure shows that the ownership effect we uncover is robust to a value of ρ as large as 0.43.

Substantive interpretation of ρ is difficult, however. A more informative quantity is the degree of selection as a function of the remaining unexplained outcome- and mediator-model variances that would produce an ownership effect of zero. This quantity is a set of variance-pairs, one for each model, representing the strength of the relationship between our mediator (ownership) and a possible confounder. The right pane of Figure 3 graphs these pairs at a series of lower bounds on the recovered mediation effect of ownership. Of interest is the zero-effect curve, which represents all possible combinations of outcome- and mediator-model selection that could render the mediation effect of ownership indistinguishable from zero at the 95% level.

In substantive terms, the zero-effect curve can aid us in characterizing how likely it is

---

[31]All sensitivity analysis is conducted in R using the `medsens` function from the `mediate` package.
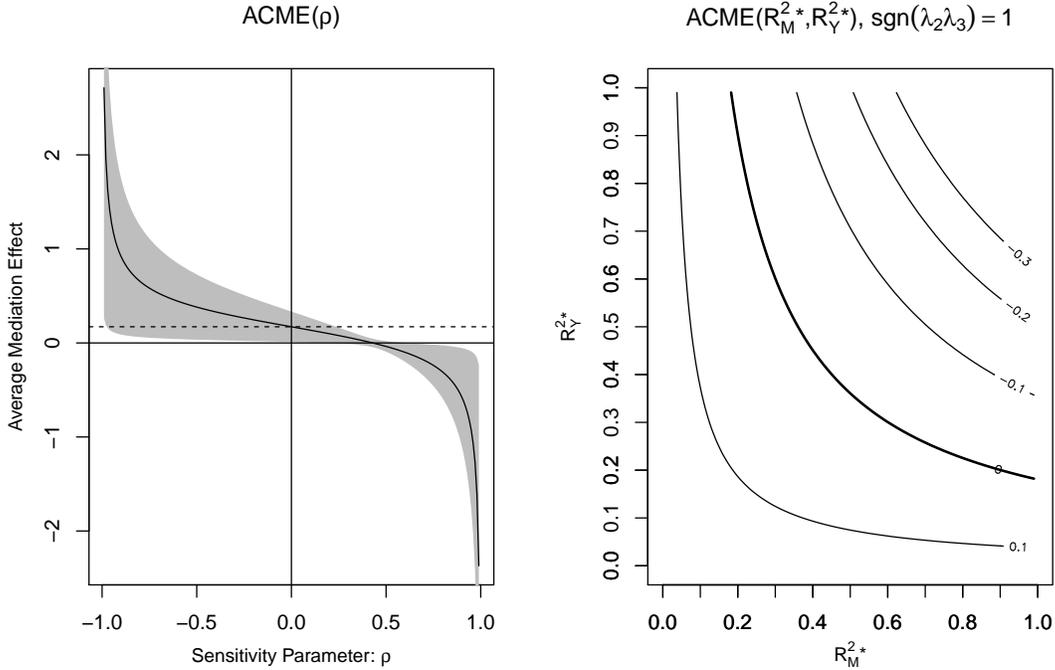
Figure 3: The Sensitivity of the Ownership Effect to Confounding Mediators.

that an unmodeled confounder exists. Consider two possible patterns of correlation implied by the curve: an "extreme" case in which the unmodeled confounder is highly correlated with either ownership or subjects' punishment behavior (but not both), and a more reasonable case in which it is moderately correlated with both. The zero-effect curve implies that, in the extreme case, the unobserved confounding mediator would have to be extraordinarily informative about the outcome (subject threshold) or the mediator (ownership), explaining upwards of 90% of the remaining variance in one model and approximately 20% in the other. In the moderate case in which we assume that the unobserved confounder explains the same amount of previously unexplained variance in both models, the confounder would have to be highly informative about both models, explaining approximately 40% of the previously unexplained variance in each case.

How plausible are either of these cases? Even with important covariates—including some that are predictive of ownership and punishment in the survey-item models in Section 7— the total variance explained in the mediation model (e.g. ownership as dependent variable) is 11%, and in the outcome model the total variance explained is 28%. Covariates include enumerator and round fixed effects, which are highly significant and yet together soak up less than 10% of the variance in the mediator model. Given the predictive power of the covariates already in our model, the possibility that some unobserved confounding mediator explains

9

upwards of 90% of remaining unexplained variance in either the outcome or mediator model or approximately 40% in both seems to us unlikely.

F   Replication of the Ownership Results in Ghana

F.1   Sampling in Ghana

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.[32] 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.

Statistic	Sample Mean	Fink et al. Estimate
Age	32.238	29
Female*	52.5	62.4
Employed	62.2	60.2
No Schooling	5.5	17.6
Completed Primary	33.6	21.9
Completed Secondary	23.1	52.0
Ga	56.7	42.2
Akan	30.7	31.0
Ewe	6.5	12.4
Household Income	604.579	NA

Table 10: **Summary Statistics for Experimental Sample in Ghana.** The Fink et al 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.

---

[32]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.

Table 10 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 (52.5% women) 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 11% formally employed by a firm and 12.6% as traders. Critically for our purposes, over half were primary earners, and a full 31% paid some form of direct tax in the previous 6 months. The average per-month household income was 604 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 sample in Ghana more closely resembles the broader population, this also increases the external validity of our findings.

F.2 Testing for the Ownership Effect in Ghana

In section we provide a more in-depth discussion of the Ghana results, including results on the Tax treatment disaggregated by Windfall source.

F.2.1 Hypothesis 1: A greater sense of ownership over the government budget (group fund) will increase citizens' willingness to punish, even controlling for revenue source.

The first and most important test of the ownership effect is simply whether strength of ownership *matters* in determining subjects' willingness to punish.[33] 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 binary indicator for whether subjects felt ownership over the group fund. 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

---

[33]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.

	DV: Subject Threshold				
	Pooled	Aid	Oil	Grant	Tax
Ownership	0.798***	0.824**	1.13***	0.867**	0.190
	(0.189)	(0.348)	(0.396)	(0.368)	(0.616)
Enum + Round FE	✓	✓	✓	✓	✓
Source FE	✓	–	–	–	–
Other Controls	✓	✓	✓	✓	✓
Observations	2006	531	487	508	480
Adjusted R^2	0.190	0.347	0.269	0.139	0.249

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

Table 11: Impact of Ownership on Punishment Behavior. The dependent variable is subject i's threshold in round j. Column 1 reports the effect of Ownership in the pooled sample; Columns 2-5 report estimates for each individual source. The results on the pooled sample are robust to the inclusion of the a dummy for each individual treatment, but is excluded here for the purpose of holding constant the estimation equation. The null result on the Tax subgroup is likely driven by a ceiling effect in the ownership variable, which only contained two values to express agreement ("somewhat agree" and "strongly agree"). The use of a 10-point scale in Uganda was driven by a desire to reduce such effects. Standard errors clustered by subject in parentheses.

cross-cutting treatments not analyzed here. [34] Results are reported in Table 11.

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 0.77 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 11, and remains strongly significant.

We also conducted a mediation analysis similar to that reported in Section E. The results are reported in Table 12 below. As in Uganda, the Average Causal Mediation 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.

---

[34]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 the valence dimension. We thus include the valence term in our estimating equation to control for the effect of this imbalance.

	Estimate	Lower CI	Upper CI	p-value
ACME	0.158	0.060	0.279	0***
ADE	0.178	-0.116	0.496	0.25
Total Effect	0.336	0.039	0.660	0.02**
Prop. Mediated	0.462	0.141	2.233	0.02**

Table 12: Mediation Analysis Results from Ghana.

G   Additional Protocol Materials

G.1   Balance Table

G.2   Effort Task

At the beginning of each round, subjects earned their wage through an effort task in which they were asked to choose between products. They were then given a piece of paper with pictures representing the different products from which they could choose. The task was different for each round. The effort task for Round 3 is given in Figure 4 below. During the group training, respondents were read the following text:

> In order to earn the wage for each round, you will have to complete a marketing task. Before the start of each round, we will give you a sheet of paper with some commonly bought items. The paper will display in pictures three pairs of items, for each pair we want you to circle the one you think most people in your community prefer. You will have to return the sheet to us at the beginning of each round. For your time and help, you will receive a wage, and it is this wage that you will be using for the activity. You do not need to be able to read or write to complete this activity. Also, there are no right or wrong answers, we are just interested in knowing your opinions.

	p-value	FDR q-value
Age	0.293	0.683
Education	0.030	0.269
Female*	0.150	0.426
% Budget from Aid	0.634	0.829
% Budget from Oil	0.109	0.372
% Budget from Taxes	0.995	0.995
Income (discretized)	0.926	0.984
Paid Income Tax*	0.362	0.683
Went without Food	0.047	0.269
Primary Earner	0.570	0.808
Paid Property Tax*	0.102	0.372
Registered to Vote*	0.477	0.808
Quality of Clinics	0.817	0.925
Quality of Roads	0.560	0.808
Quality of Schools	0.335	0.683
Quality of Sanitation	0.034	0.269
Trust in MP	0.715	0.868

Table 13: Chi-Squared Test for Covariate Balance Across All Treatment Conditions. Column 1 gives uncorrected p-values for the chi-squared test of equal proportions across treatment conditions. Column 2 reports FDR-corrected q value via the Benjamini-Hochberg procedure. Income variable discretized to facilitate a chi-squared test. While some p-values reach conventional significance, the large number of tests make a false discovery likely. No variables are significant after correcting for the likelihood of false discovery. Variables denoted with an asterisk are binary.
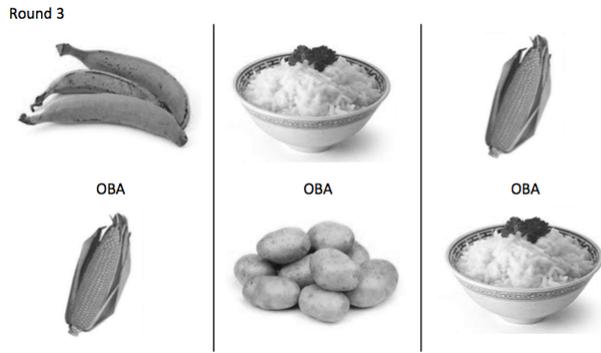
G.3 Game Board

Round 3

OBA   OBA   OBA

Figure 4: Sample Effort Task to earn wages. This figure presents an example of the short "effort tasks" that all respondents completed in between rounds to earn their wage for that round. The task asked subjects to choose between multiple products by circling the desired product among those shown. They then returned the slip of paper to the enumerator at the start of each round and were given their wage.
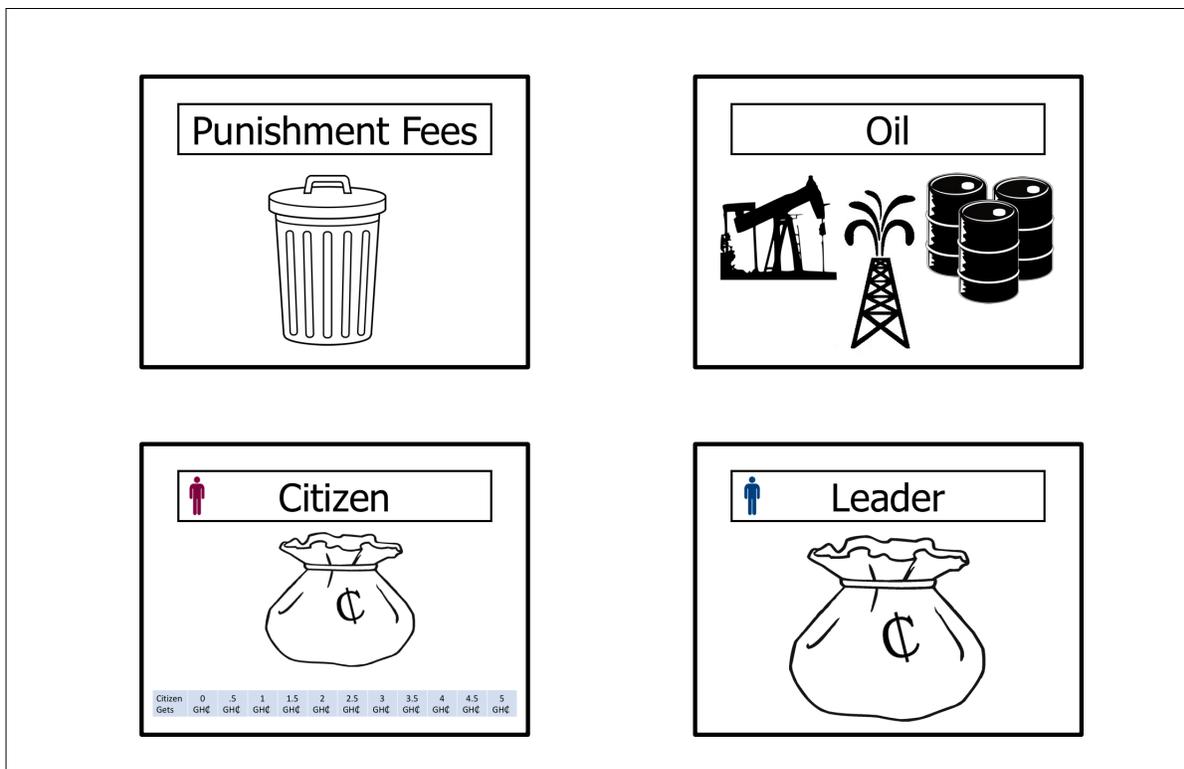

| Punishment Fees | Oil |
| Citizen | Leader |

| Citizen Gets | 0 GH¢ | .5 GH¢ | 1 GH¢ | 1.5 GH¢ | 2 GH¢ | 2.5 GH¢ | 3 GH¢ | 3.5 GH¢ | 4 GH¢ | 4.5 GH¢ | 5 GH¢ |

Figure 5: Example of Game Board, Oil Condition. Game boards for the Aid, Tax and Grant conditions differ only with respect to the image on the source tile. The Grant condition source tile has no image and is simply titled "Group Fund". To emphasize the source of the group fund, the enumerator places the money on the source tile before transferring it to the Leader. At the time the Citizen sets her transfer threshold, the revenue is on the Leader's tile.

G.4   Purchasing Phase of Game

In all treatment conditions in the July 2017 games, 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 included to allow comparison with an additional set of treatments that compared direct taxes to a VAT on goods.

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."

H   Outtake Measurements

This section details the ownership and punishment measures used in the outtake survey in the July 2017 experiments. These questions are the basis for Section 7 of this paper.

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"