Taxation without Representation? Experimental Evidence from Ghana and Uganda on Citizen Action toward Taxes, Oil, and Aid

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Abstract

Seminal arguments in political economy hold that citizens will more readily demand accountability from governments for taxes than for non-tax revenue from oil or aid. Two identical experiments on large, representative subject pools in Ghana and Uganda probe the effects of different revenue types on citizens' actions to monitor government spending. Roughly half of all subjects willingly sign petitions and donate money in order to scrutinize all three sources. However, neither Ghanaians nor Ugandans are more likely to take action for tax revenues than for oil or aid. Results also suggest no differences among taxes, oil and aid in citizens' perceptions of transparency, misappropriation risk, or public goods provision. Results are robust to numerous alternative specifications and subgroup partitions, including the better educated, wealthier, and taxpaying population, suggesting a need for rethinking the axiom that taxation strengthens citizens' demands for accountability in developing countries.

Key words: resource curse, accountability, endowment effect, development

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1 Introduction

Social scientists have long believed that taxes have a special power to move citizens to action. Taxation implies a bargain in which constituents quasi-voluntarily give revenue to leaders in return for government services (Levi 1988, North 1990, Ross 2004). While leaders can coerce citizens to pay, frustrated citizens can, among other actions, exit the polity, reduce production, evade taxes, coordinate oversight, or organize a revolt, all of which are costly for rulers and presumably place limits on their ability to extort. Indeed, seminal accounts of political economy rest on taxpayers' capacity to demand policies in exchange for taxes (Levi 1988, North and Weingast 1989, Schumpeter 1991). In particular, taxes are thought to heighten citizens' attention to government accountability and thus make political elites more likely to provide public goods or face sanctions if tax revenues are misused (Bates and Lien 1985, Huntington 1991, Paler 2013, Robinson, Torvik and Verdier 2006, Ross 2001, 2004, 2012).

Oil and foreign aid, on the other hand, are said to move governments away from the "taxation-causes-accountability" model. Funds from oil and aid are believed to be "windfall revenues" that relieve tax burdens and pacify citizens (Morrison 2009, 2015). Because citizens do not pay direct costs to furnish non-tax revenue, they are less motivated to engage in oversight or demand policies in compensation. Windfalls thus enable elites to divert more funds to corruption and clientelism relatively free from citizen scrutiny; or, the money can buy citizen quiescence and repress the would-be monitors (Beblawi and Luciani 1987, Chaudhry 1997, Mahdavi 1970, Waterbury 1998). Oil and aid thus are claimed to enable corruption, undermine governance, foster repression, prolong autocratic rule, and increase conflict (Bräutigam and Knack 2004, Caselli and Cunningham 2009, Djankov, Montalvo and Reynal-Querol 2008, Morrison 2009, 2015, Smith 2008). The implications are clear: taxes promote citizen engagement and representation through heightening government accountability; oil and aid cause citizen disengagement and quiescence.

If the above arguments are correct, citizens' willingness to undertake political action to monitor spending should differ by revenue source. This was our expectation in designing the experiments. However, real-world governance settings may create conditions in which taxation does not cause greater citizen engagement in monitoring than windfalls. Indeed, current research rests on mixed findings, almost all of which are observational in nature and therefore suffer from well-known problems with causal identification. A stronger evidence base is thus needed for the claim that alternate types of revenue cause differential taxpayer scrutiny of government finances. Better empirical foundations for the claims that different revenue sources motivate divergent citizen action could also

inform important policy arenas in which international organizations advocate higher tax collection in poor countries, activists mobilize to discourage the exploration and use of natural resources, and donor governments look to cut aid budgets.

To test the effects of different revenue sources on citizen action toward monitoring, we conducted substantively identical large-N experiments with attitudinal and behavioral outcomes on nationally representative samples of Ghanaian and Ugandan citizens. To our knowledge this article reports the first multi-country experimental tests of the argument that taxes motivate citizen action differently than oil or aid. Selected to be representative of developing countries, Ghana and Uganda are both within a standard deviation of the mean across a wide array of indicators. However, Ghana is toward the high end of oil rents as a share of GDP (81st percentile) and Uganda is likewise high in terms of aid as a percentage of national income (77th percentile).[1] Both countries depend on all three sources of revenue, unlike some oil-exporting countries that depend solely on oil revenues and have no experience with aid or taxes. They thus serve as useful cases in which to evaluate the power of taxes to motivate citizen action compared to windfalls.

Respondents were randomized into one of three treatment conditions testing the between-subjects effects on behavior and attitudes of citizens toward taxes compared to oil revenue and aid funds managed by the government. In each condition, respondents heard a short statement about actual government spending. The statement included truthful information on the amount (held constant) and source (randomly assigned) of revenue that would be available to the government in the near future. Citizens were then invited to sign an anti-corruption petition (both anonymously and in their own name), send supportive SMS text messages, and donate money to their choice of good-government organizations. Leveraging the between-subjects design that avoided priming subjects to think about alternative revenue sources, we also asked them how transparent and accountable they believed management of the revenue would be, how susceptible it might be to misappropriation, and how likely it was to be used for the provision of public goods.

The findings suggest that Ghanaians and Ugandans are, on average, no more likely to take political action to monitor or encourage transparency of funds for tax-based revenue than for oil or aid money. Moreover, there were no average differences across revenue sources on attitudinal outcomes measuring perceptions of the revenues' transparency, their likelihood of misappropriation, or their propensity to provide for public goods. The point estimates are small in substantive terms and precisely estimated. These findings are robust to numerous alternative specifications, including

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[1]Calculated using oil rents and net official development assistance data from World Bank (2016).

scrutiny of many subgroup effects, including those on the wealthiest, best-educated, and income-taxpaying segment of the population and on supporters of opposition parties. Likewise, no indirect effects appear when examining different causal mechanisms through mediation analysis.

Importantly, our findings are not the result of general citizen indifference to bad governance. In both countries more than half of subjects signed transparency petitions and roughly half donated money to good-government NGOs aimed at scrutinizing government spending. Citizens were simply not differentially motivated toward political action across the revenue conditions. We suggest that these results are due to the institutional environment common to many developing countries. These findings should help to lead to a rethinking of the taxation-causes-accountability argument in developing-country contexts, as well as its corollary that non-tax revenue causes citizen quiescence. In what follows we situate the study in the relevant literature, develop hypotheses, describe the research design, and present the data and findings.

2 Revenue and Accountability

The intellectual history of the idea that taxation causes accountability runs through Joseph Schumpeter, Samuel Huntington, Douglass North and Barry Weingast, to Margaret Levi, Charles Tilly, Robert Bates, David Collier, Anke Hoeffler, Michael Ross, and numerous others, forming an important foundational concept in theories of democratization. Its essence was first and perhaps most famously intoned by Boston's Old West Church Reverend Jonathan Mayhew in 1750 as "no taxation without representation," becoming a rallying cry of the American Revolution. The logic is straightforward. Leaders need money to buy arms and enlist militaries in order to fight wars to secure and expand their realms. The increasing cost of militaries caused leaders to seek money from private sources through credit or expropriation through taxes. On the one hand, well-armed leaders are particularly bad credit risks; they can coerce creditors with relative impunity. On the other, mobile capital (including human capital) is more able than fixed capital to flee despots, so leaders are forced to bargain with private citizens in an exchange of public services for tax payments. Given their credibility problems in enticing creditors to finance their militaries or in persuading citizens to pay taxes, successful leaders offered a say in government in return, thus tying their hands against future coercion.

These mechanisms thus connect taxation to accountability. As Bates and Lien (1985, p. 53) write, "[r]evenue-seeking governments may well find it to their advantage to strike bargains with citizens whose assets they seek to tax. To induce a greater willingness to pay taxes, they may defer

to the citizens' policy preferences. Such bargains may become more beneficial from the citizens' point of view the more mobile the assets the citizens hold." Mick Moore (2004, p. 307) makes a similar argument in reverse, claiming that "the absence of direct taxes reduces the likelihood that citizens will be motivated to engage in politics through a sense of a right to influence the use of 'their' own money." Chaudhry (1997, p. 50) in an early analysis of the resource curse says "demands for political participation have more often than not been a response to taxation." Ross (2004) advances a more nuanced argument that taxes will only motivate citizens to demand representation to the degree that public goods are underprovided in relation to taxes. Yet this ratio will necessarily increase as oil or aid substitute for taxes.

Thus, the lack of accountability in resource- and aid-rich states follows logically from these arguments: if citizens are not taxed, accountability pressures will decrease. Huntington (1991, p. 65) articulates this point clearly: "Oil revenues accrue to the state: they therefore increase the power of the state bureaucracy and, because they reduce or eliminate the need for taxation, they also reduce the need for the government to solicit the acquiescence of its subjects to taxation." Ross (2001, p. 332) echoes this point, arguing that "when governments derive sufficient revenues from the sale of oil, they are likely to tax their populations less heavily or not at all, and the public in turn will be less likely to demand accountability from—and representation in—their government." Others note that the "rentier social contract [is such that] the state provides goods and services to society (such as subsidies on basic commodities) without imposing economic burdens, while society provides state officials with a degree of autonomy in decision-making and policy" (Wiktorowicz 1999, p. 608).

When leaders of developing countries structure their economies around the extraction of export-based natural resources, the literature holds that citizens are significantly less motivated to monitor and sanction the mismanagement of natural resource windfalls compared to taxes (Morrison 2015, Robinson, Torvik and Verdier 2006, Ross 2001, 2004, 2012). This phenomenon is believed to enable the "resource curse," at least in part, by undermining good governance and, as a result, economic development. Because governments engaged in natural resource exploitation receive large amounts of revenue from sources unattached to the democratic process, mechanisms for accountability weaken (Bulte, Damania and Deacon 2005, Dunning 2005). For leaders the returns to rent seeking outpace the returns of creating socially productive skills and knowledge. The value of public office spikes, causing leaders to increase internal security against uprisings and spend more to buy public support (Caselli and Cunningham 2009, Robinson, Torvik and Verdier 2006, Ross 2001). Thus, such govern-

ments often become more autocratic and prone to corruption and clientelism (Collier and Hoeffler 2005, Robinson, Torvik and Verdier 2006).

Foreign aid may be similar in its effects. Critics charge that both natural resource rents and foreign aid are "windfall revenues" or "sovereign rents" that promote corruption, undermine governance, increase violence, and stabilize autocratic regimes (Bräutigam and Knack 2004, Collier and Hoeffler 2005, Djankov, Montalvo and Reynal-Querol 2008, Humphreys 2005, Morrison 2009, Smith 2008). Moreover, because aid allocations from donors fluctuate, governments receiving large amounts of aid can experience economic and political instability, and when aid is suddenly reduced the likelihood of conflict appears to increase (Nielsen et al. 2011). Critically, natural resource and aid windfalls relieve leaders of the need to finance government through taxation (Beblawi and Luciani 1987, Mahdavi 1970, Morrison 2009, 2015).[2]

More recent scholarship has linked taxation to the well-known psychological phenomena of loss aversion and the endowment effect (Martin 2014, Paler 2013), in which people disproportionately fear losses and overvalue possessions, respectively (Kahneman and Tversky 1979, Kahneman, Knetsch and Thaler 1990). These studies activate psychological mechanisms related to endowments of income and losses in taxes and suggest that income taxes can motivate citizen action in poor countries as they are believed to do in rich nations (Martin 2014, Paler 2013). The theoretical and empirical foundations of the endowment effect and loss aversion are strong. But we emphasize that the prominent arguments reviewed above by Schumpeter, Huntington, North, Weingast, Bates, Tilly, Collier, Hoeffler, Levi, Ross, and others do not reference psychological biases. Rather, the taxation-causes-accountability argument and its resource-curse corollary are founded upon the presumption of rational action: because taxes prove costly to citizens and windfalls do not, taxes are more likely to motivate citizens to take action to obtain the beneficial public goods they desire and windfalls to cause quiescence. This argument underpins the key hypotheses the study was designed to test:

> **Hypothesis 1** *Taxation causes accountability. Tax revenues should motivate citizens to take more action to monitor public spending, particularly compared with oil and aid.*

> **Hypothesis 2** *Windfalls cause quiescence. Revenues from oil or aid should fail to motivate citizens to take action to monitor spending, particularly compared to taxes.*

However, some scholars have questioned the inevitability of the resource curse and claimed that

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[2]Collier (2006) argues that aid revenues are more accountable than oil revenues, in part because donors pursue "governance conditionality."

the effects of resources on governance are contingent on other factors (Dunning 2005, Haber and Menaldo 2011, Jensen and Wantchekon 2004).[3] More recent studies cast further doubt on the resource curse (Borge, Parmer and Torvik 2015, O'Connor, Blanco and Nugent 2016, van der Ploeg and Poelhekke 2009). This debate remains unresolved even in the context of large-N observational studies. As Ahmadov (2014, p. 1239) points out, examining "29 such studies that in total report 246 empirical estimates of the impact of resources [on democracy], they [...] range from negative through no association to positive. While 86% of statistically significant findings report negative coefficients, 14% find a positive link. Twenty-one percent do not find any statistically significant relationship." The majority of existing research on the resource curse has utilized such cross-national, observational data, limiting the ability to make causal inferences or address micro-level mechanisms. A similar set of points could be made about the mixed results reporting the effects of aid on governance (Bueno de Mesquita and Smith 2013, Busse and Gröning 2009, Djankov, Montalvo and Reynal-Querol 2008, Finkel, Pérez-Liñán and Seligson 2007, Knack 2004, Moss, Pettersson and Van de Walle 2008, Tavares 2003, Wright 2009).

Yet if the seminal arguments linking taxes to citizen action hold, taxes should also cause transparency and accountability. The bargain described by Bates and Lien (1985) referenced above requires that citizens know they are being taxed and can follow the money from their pockets to the production of public goods. As Manin, Przeworski and Stokes (1999, p. 40) explain, accountability implies that citizens can "discern whether governments are acting in their interest and sanction them appropriately." Even Morrison's (2009, 2015) more general argument that taxes lead not to representation but to regime instability implies microfoundations resting on an assumption that citizens are informed about their tax burden or at least feel it in a way that induces action. On the other hand, windfalls may lack transparency: "low levels of budget transparency in oil-dependent countries are common and may lead to poor management of resource wealth" (Devarajan, Le and Raballand 2010, p. 4). Likewise, the more nuanced point that Ross (2004) makes regarding taxes in relation to public services implies that citizens have a general sense of both their tax burden and the public services provided.

This argument has a similar implication for the likelihood of misappropriation. The taxation-causes-accountability argument holds that, because citizens care more about taxes than windfalls and therefore will more readily scrutinize spending from taxation, the risk of misappropriation for oil or aid should be significantly greater than for taxes. This is conceptually related to what Ross

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[3]Alexeev and Conrad (2009) even claim that natural resources such as oil are strongly positive for economic growth and have no negative effect on political institutions.

(2001, p. 333) calls the "spending effect" which predicts that "oil wealth may lead to greater spending on patronage, which in turn dampens latent pressures for democratization." Resource wealth and foreign aid both increase the size of government budgets and, critically, enable less budgetary restraint, presumably due to lower citizen scrutiny and engagement (Morrison 2009, 2015). Indeed, windfalls may enable governments to reduce tax burdens at the same time as they improve public goods (Morrison 2009, 2015, Ross 2001). Citizens may therefore be inclined to look the other way when leaders divert a greater share of resource windfalls than taxes to political or even personal ends. Finally, resource "revenues increase the returns to holding office, raising the stakes of winning reelection. Incumbents therefore have strong incentives to skim off at least some resource revenues to help them stay in power" (Bhavnani and Lupu 2016, p. 3-4).

Moreover, the foundational bargain of leaders with citizens requires an exchange of tax revenue for influence over public policy in order to produce services that citizens value (Bates and Lien 1985, Levi 1988, Ross 2004). Indeed, Ross (2004) holds that citizens will only tolerate taxes if the public revenue produces actual public goods. As he writes, "[d]emocracy in this case is not necessarily a way for citizens to reduce their taxes, or to increase spending, but to get more for their money" (Ross 2004, p. 234-235). Citizens should therefore expect taxes to increase public-goods provision and should thus be more inclined to pay costs to monitor tax money. Or as Collier and Hoeffler (2005, p. 8) claim, "For a given total income, revenue for patronage rises as a result of resource rents not because the government commands more money, but because it is able to raise the same money while arousing less public scrutiny. As a result, less needs to be diverted to the provision of public goods."

Three additional observable implications thus flow from the argument that taxes cause accountability and its corollary that windfalls cause quiescence. We state them here as the conditions under which taxation should be most likely to lead to greater accountability. They are as follows:

Hypothesis 3 *Transparency. Citizens should realize they are being taxed and be able to track the spending of tax revenue better than windfalls.*

Hypothesis 4 *Misappropriation. Citizens should perceive that elites can less readily misappropriate taxes than other revenues for personal or political gain.*

Hypothesis 5 *Public Goods Benefits. Citizens should judge that leaders will use taxes more than oil or aid to provide public goods. They should thus expect greater public benefit from tax-based spending than from oil- or aid-based spending.*[4]

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[4]In the pre-analysis plan filed in advance of researcher access to outcome data, we labeled these three categories

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Nevertheless, there may be reasons to believe that these conditions are unlikely to be satisfied in many poor, developing countries. In institutional environments of low information, high corruption and a limited tax-base, respondents may not believe that the source of government revenues matters, since all revenues are likely to be equally opaque and vulnerable to corruption. If transparency is low and misappropriation risk high, it will be equally difficult to discover if funds have been misused across revenue sources. Waterbury (1994, p. 30), for example, counters the resource curse claims and notes that in developing countries few pay direct taxes and paying indirect taxes does not have the same effect as direct ones. He notes "the relation of taxation to political demands is not at all straightforward. On the one hand, there are the 'hard' states of East Asia which tax their populations heavily, but only recently and grudgingly have made any concessions to demands for accountability...there must be a number of intermediate variables between levels and kinds of taxation and demands for accountability, and until we are able to specify those variables we will only have an intuitive, and often inaccurate, appreciation of the dynamics at work" (p. 30). Thus, the strong theoretical claims that taxation causes accountability and windfalls demotivate citizen monitoring may not hold in developing countries. This brings us to the design of the experiment.

3 Experimental Design

The experimental strategy and data analysis plan were registered with the Evidence in Governance and Politics network prior to researcher access to the outcome data. For this experiment, we drew sizable samples of Ghanaian (n = 3,653) and Ugandan citizens (n = 3,186) that are nationally representative in most respects, except that we oversampled districts that are nearest the sites of oil exploration. We selected the two countries with attention to the representativeness of Ghana and Uganda among developing countries generally and among Sub-Saharan African countries in particular. Across a basket of conventional development indicators, including life expectancy, under-five mortality, adult literacy, unemployment, proportion of the population living on less than $1.90 per day, and many others, both countries in 2014 were at or near the means for lower-middle-income countries (World Bank 2016). In terms of generalizability, selecting typical developing countries is important. Focusing on extreme outliers here, as in nearly all social scientific endeavors, could introduce serious bias and undermine external validity. Thus, studying exceptionally oil- or aid-dependent countries would not provide a basis for generalizing to most other developing countries,

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"Accountability," "Repression," and "Spending". They are substantively similar with the exception that "Misappropriation" is a broader category that includes money spent to repress as well as to buy support and personally enrich officials.

which ought to be the objective in testing such general arguments as taxation causes accountability and windfalls cause quiescence.

However, selecting representative subjects from countries at either end of the normal range of developing nations (say, within one standard deviation of the mean) provides an opportunity to delineate the scope of populations to which the findings might generalize. Thus, within the typical span of low- to middle-income countries we selected Ghana and Uganda to provide some divergence across a set of important covariates. Critically, Ghana has been receiving revenues from oil since 2010, while Uganda's oil is not yet flowing in large amounts. In comparative terms, Ghana's oil rents of 5.7 percent of GDP in 2014 place the country near the higher end of the distribution at the 81st percentile among the 137 developing countries reporting data (just seven, six, and five ranks below Yemen, Russia, and Nigeria, respectively, and ahead of other well-known oil producers such as Norway, Bahrain, and Mexico).[5] Uganda does not report this statistic, but its lack of current oil rents would obviously place it at the low end. Moreover, publics in both countries know about the oil finds. In our sample for example, 85 percent of Ghanaians and 75 percent of Ugandans knew of the oil fields. Likewise, Uganda's relatively high aid dependence of 6.0 percent of GNI places the country in the 77th percentile among all developing countries; Ghana's aid per GNI of 3.1 percent is roughly half the value of Uganda's. Aid comprises roughly 21 percent of Ghana's government expenses (in 2011, the last year reported) compared to aid's 48.3 percent share of Uganda's government expenditure in 2014. Notably, Ghana and Uganda depend on all three sources of revenues, while the large oil-exporters rarely show any dependence on taxes or foreign aid.

The selection of the two countries was also done to consider differences in political system and economic outlook while holding constant the broad geographic region. Ghana is a stable democracy, scoring near the top of political rights and civil liberties scales; Uganda is labeled as an anocracy with worsening rights and liberties, especially recently (Freedom House 2016, Marshall, Gurr and Jaggers 2016). As a middle-income country, Ghana is more than twice as wealthy as low-income Uganda, with Ghana's GDP per capita adjusted for purchasing power parity at $3,784 compared to Uganda's $1,634 (World Bank 2016). Consistent results across the two disparate settings should increase the study's external validity.

In interviews with the Ghanaian and Ugandan citizens, native enumerators presented a randomly assigned statement about the source of significant public funds and then invited subjects to participate in actions to monitor the money. Enumerators then asked subjects a series of questions about

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[5]See World Bank (2016).

what they thought the effects of the funds would be and how they thought the money should be spent. The amounts of these revenue streams were held constant across conditions in each country. All were based on best estimates of plausible future budget sources given publicly available information; thus, no deception was used in the experiment.[6]

The survey first asked questions covering a wide array of standard demographic characteristics. After this, we randomly assigned subjects to receive a statement about revenues from one of three sources: domestic taxes, oil receipts, and aid flows through government. Randomization of treatment assignment allows us to uncover systematic differences in subject actions and responses across conditions. We incentivized respondents to take the survey by giving them either 6 Ghanaian cedis or 1,000 Ugandan shillings at the start.[7] Later we invited them to donate (parts of these) sums to watchdog non-governmental organizations (NGOs) as a behavioral outcome. We tested the effects of the different revenue conditions both on subjects' attitudes and on their willingness to take action imposing personal costs by signing a petition calling for an independent resource tracking agency, sending an SMS message to their MP, and donating survey remuneration to watchdog NGOs.

The treatment conditions are as follows (with differences highlighted in boldface)[8]:

> "As part of this survey, we are also providing important information to [Ghanaians/Ugandans] about finances in [Ghana/Uganda]. In next few years, government agencies of [Ghana/Uganda] will receive at least [2.1 billion cedis/5 trillion shillings]. This money will come from [**the sale of the oil that was recently discovered in [Ghana/Uganda]/taxes on wages and purchases that will be paid by all [Ghanaians/Ugandans]/aid given by foreign governments**]. This money will become part of the [Ghanaian/Ugandan] government budget. Lawmakers and the President are supposed to use the money to improve the lives of [Ghanaians/Ugandans]."

Two features of these conditions are particularly important. First, the prompts are identical in terms of the absolute amount of revenue accruing to the government. By holding the absolute amount constant, the design attempts to isolate the effect of source from that of the source's size. Second, the prompt does not contain any additional information about government spending or budget behavior beyond the source and (fixed) amount to be derived from it. This is important

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[6]We also included a fourth condition in which the aid money was described as channeled through non-governmental organizations. We focus here only on two of the possible pairs: oil versus taxes and aid versus taxes, respectively. The remaining comparisons are the focus of ongoing research and will be reported elsewhere.

[7]At the time of the experiment 6 cedis were worth about $2.40 and 1000 shillings were worth about 40 cents at current exchange rates, or about $6 and $1 respectively in terms of local purchasing power.

[8]See Section A in the appendix for information on the randomization protocol.

for ecological validity, since the knowledge and transparency necessary for greater monitoring and sanctioning of tax-based revenue may be lacking in much of the developing world.

3.1 Survey and Key Outcomes of Interest

Following the experimental condition text, subjects could voice their support to create an independent agency to track the revenue and sign a petition anonymously or in their actual name that would later be sent to their constituency MP stating the respondent's desire for the agency to be created. Subjects were also invited to send an SMS text message reinforcing their position to their MP. Finally, they were invited to donate a portion of the money paid them for taking part in the survey—1,000 shillings for Ugandans and 6 cedis for Ghanaians—to watchdog groups promoting government accountability. With the exception of the donation amount, all measures are binary and take a value of one if the respondent acted affirmatively and zero otherwise. The prompt for the donation measure is in the appendix.

The behavioral measures were designed with two principles in mind. First, to ensure we captured not only whether someone would take costly action but how costly an action they were willing to take, we crafted behavioral measures that imposed increasing political and economic costs. Signing an anonymous petition is a statement of intent, while signing a named petition is more costly, particularly given the contentious partisan politics in Ghana or especially under Uganda's quasi-authoritarian regime. Sending an SMS message entails a small economic cost that many sub-Saharan Africans expend only reluctantly. In Uganda, for example, SMS texts cost between 50 and 130 shillings. In view of the average subject's low daily income of 2035 shillings or $1.08 US dollars, for the vast majority of subjects the cost likely appeared meaningful.[9] Finally, with the ability to pocket up to 6 cedis or 1,000 shillings by refusing to donate, the donation experiment is the most costly: because respondents were notified their donation would be given directly to the NGO of their choice, it constitutes action that is at once a political and economic cost. This sum is important: 6 cedis in Ghana or 1,000 shillings in Uganda may be as much as an entire day's wage for petty traders and manual laborers in rural areas, who constituted the majority of our subjects.

Following the behavioral outcomes, subjects were asked a series of questions about how transparent spending financed by the revenue source was likely to be. They were also asked to report how likely it was that elites would be able to misappropriate the money to themselves, their families, or for their political advancement (e.g., clientelism). The survey further queried them about the likelihood that the money would be used to supply public goods. Finally, questions probed whether

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[9]All subjects were later reimbursed for the costs of their texts but this intent was not conveyed during the survey.

subjects would be willing to pay taxes to finance a transparency agency to monitor the revenue or to contact local or national elected officials in the event the revenue was misused.

4 Data and Method

The sample contains 3,653 observations in Ghana and 3,186 in Uganda, which were collected using an area-probability sample designed to achieve national representativeness. Data collection in Uganda occurred from May to June 2014 and in Ghana during March and April 2015. To enhance the validity of our estimates, we implemented block randomization within each enumerator, resulting in perfect or near-perfect balance in the number of treatments of each type delivered by enumerators. Due to random selection of primary sampling units (polling stations), this resulted in a form of enumerator-PSU blocking that ensured assignment to our treatment conditions was balanced both across enumerators and across PSUs. Balance at the level of the PSU allows us to exploit not only the spatial correlation between many important respondent characteristics, such as education, wealth, and access to information, but also the strong spatial correlation between respondents' political experiences.[10] Blocking at the PSU increases the probability that our sample is balanced with respect to a range of macro-level political variables that might affect political behavior, including partisanship, corruption, and the responsiveness of local politicians. In terms of covariate balance, as expected, the block randomization algorithm was successful in randomizing respondents into equally sized treatment groups within our primary sampling unit, the polling station.[11]

We report results below for all subjects using randomization inference. Although we employ traditional difference-in-means and block-adjusted regression results as robustness tests, our primary analysis uses randomization inference, an assumption-free non-parametric estimation strategy that has become increasingly common in the analysis of randomized experiments in political science.[12] First, unlike traditional parametric estimation, which relies on the t or Normal distribution to establish statistical significance, randomization inference makes no distributional assumptions about the test statistic used. Instead, the distribution used to recover the test statistic's p-value is generated directly from the data. This is accomplished by considering all possible treatment assignment vectors,

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[10]In a randomization inference framework, the practical effect of blocking is to preclude randomization vectors that would violate the blocking algorithm—that is, vectors in which the proportion of treated and control units in a given block are not consistent with equal probability of assignment to each treatment group within the block. Intuitively, the effect is not to reduce bias—a wide range of test statistics are unbiased even under naive randomization—but to reduce noise by decreasing the probability that treatment and control groups are unbalanced along key covariates.

[11]See Table A.3 in the appendix for results of balance tests designed to test successful randomization.

[12]See Tables B.4 and B.5 in the appendix for these results. All regression specifications include block fixed-effects as well as the covariates reported in Table A.3. Given the efficiency gains from adjustment, this is an especially conservative robustness check as it increases the probability of obtaining significant results.

calculating the test statistic for each, and using these estimates to construct a distribution that represents the range of potential treatment effects that might arise purely from chance alone. In doing so, it accounts for treatment assignment vectors that would be both highly favorable to the experimenters—for example, those in which most or all treated units were also those with high values on the dependent variable of interest—as well as those that would be unfavorable.

Although the flexibility in choosing a substantively meaningful test statistic is another advantage of randomization inference, we focus here, as pre-registered, on a traditional test: a difference-in-means between the reference and comparison groups. Estimates from randomization inference can thus be interpreted similarly to conventional differences estimates, with one critical distinction: the distribution of the test statistic under the null hypothesis is generated from the data rather than assumed using the normality properties of random variables.

5 Analysis

In terms of descriptive statistics, a majority of subjects were willing to undertake political actions to promote budget transparency and monitor government spending, as shown in Table 1. In Ghana and Uganda, 53.2 and 50.7 percent of participants, respectively, signed the petition in their own name. An additional 8.3 percent in Ghana and 7.9 percent in Uganda signed the anonymous petition, for a total of 61.6 percent and 58.6 percent signing either the named or anonymous petition in Ghana and Uganda, respectively. Also, 48 percent of Ghanaian participants and 57.5 percent of Ugandans donated money to the good-government NGOs, and on average they donated 2.1 (of 6) cedis and 386 (of 1,000) shillings, which both constituted sizable shares of their grants for participating in the study. Table 1 shows that across many types of actions, citizens in both countries are not apathetic; they are willing to act to obtain public goods, as might be expected in these poor countries. It is also important to note that while norms of reciprocity or social desirability bias may artificially raise the absolute levels of donation, these norms are constant across all treatment conditions and would therefore not threaten any inference about relative differences across conditions.

Do we see differences in citizens' willingness to take action to monitor and sanction leaders across the revenue sources? Figures 1 and 2 directly test our first two hypotheses about differential action in favor of taxes. Figure 1 shows the tax treatment relative to the oil control, and Figure 2 shows the tax treatment relative to the aid control. We asked citizens to undertake various costly actions, and thus our measure reflects the nature and intensity of their preferences. The top panes of Figures 1 and 2 report the treatment effects across experimental conditions on our primary outcomes

	Ghana		Uganda	
N	3563		3186	
	Frequ.	**Share**	**Frequ.**	**Share**
Signed Anon. Petition	295	0.083	251	0.079
Signed Named Petition	1878	0.532	1589	0.507
Willing to Send SMS	1146	0.327	1514	0.486
Donated to NGO	1712	0.48	1833	0.575
Mean Amount Donated	2.1	Cedis	385.7	Shillings
Likely or Very Likely to Contact Village Elder	1628	0.46	1871	0.594
Likely or Very Likely to Contact Local Councilor	1786	0.506	1687	0.536
Likely or Very Likely to Contact MP	1241	0.35	1259	0.402

Table 1: **Frequencies and Proportions of Subjects Taking Action and Expressing Willingness to Take Action to Monitor Revenue.** Results show that large proportions of respondents are willing to take various forms of costly political action to monitor spending or to strengthen transparency institutions.

of interest: the rate with which subjects sign anonymous or named petitions stating they would like for a new agency to be created to monitor revenues from either taxes, oil, or aid through government (`Anon. Petition` and `Named Petition`, respectively), whether they sent an SMS stating as much (`Sent SMS`), whether they donated to a third party transparency organization (`Donated Any`) and, if they donated, the amount (`Donated Amount`).[13]

Hypotheses 1 and 2 imply that we should see positive treatment effects, in which citizens are more likely to act for taxes. As is clear, there are few meaningful differences between the tax and oil conditions or between the tax and aid prompts on any of the outcomes. For all tests the relatively tight confidence intervals reflect comparatively good statistical power and mitigate against the possibility of small sample size masking non-zero effects. Due to the large number of tests, and given our commitments in the registered pre-analysis plan, we also implement multiple-testing corrections by controlling the false discovery rate (FDR) at an alpha of 0.05 for all individual tests. The FDR correction eliminates the two marginally significant results. We thus find no support for Hypothesis 1 or Hypothesis 2 for both Ghana and Uganda.

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[13]For illustration purposes, the donation amount measure is standardized to more closely match the scale of the remaining variables.

Figure 1: Main Tax Treatment Effects, Oil as Control. Effects in standard deviation units shown for both behavioral (top pane) and attitudinal measures (bottom pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

Behavioral Measures


Theoretical Willingness
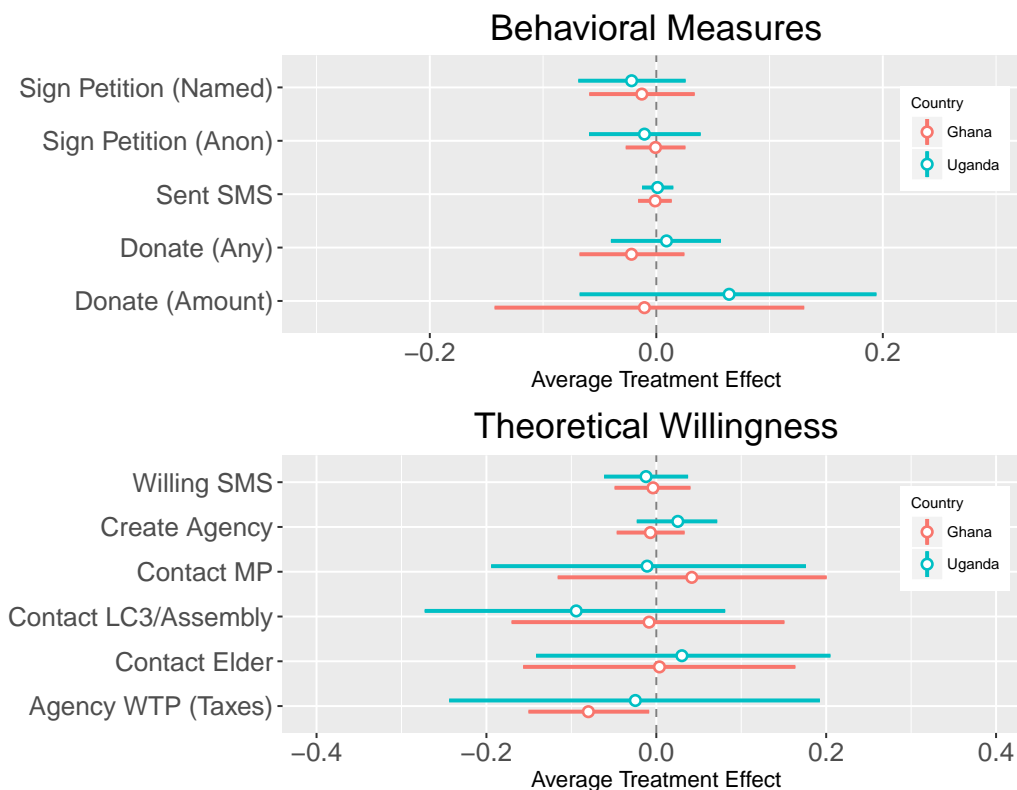
Figure 2: Main Tax Treatment Effects, Aid as Control. Effects in standard deviation units shown for both behavioral (top pane) and attitudinal measures (bottom pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

We also included several measures designed to capture respondents' willingness to both monitor the treatment source as well as sanction its misuse. Specifically, we asked respondents whether they desired an independent monitoring agency to be created to monitor spending from the revenue (`Create Agency`), how much in taxes they would be willing to pay monthly to fund it (`Agency WTP`), whether they would be willing to send an SMS to their MP stating their support for the agency (`Willing SMS`), and how likely the respondent was to contact their local village elder, LC3 Chairman (roughly equivalent to a US mayor), or MP in the event money from the source was misused (`Contact Elder`, `Contact LC3`, and `Contact MP` respectively).[14] The bottom panes of Figure 1 and Figure 2 report the treatment effects on these measures, with positive average treatment effects (ATEs) for respondents implying that the tax treatment induced more action, as Hypotheses 1 and 2 suggest. As with our behavioral measures, we see little differential willingness for respondents in the tax condition relative to those in oil (Figure 1) and aid (Figure 2) conditions for either Ugandan or Ghanaian subjects.

[14]The equivalent in Ghana was the district chief executive.

We also presented three additional empirical implications that flow from the taxation-causes-accountability hypothesis. First, according to Hypothesis 3 citizens should realize they are being taxed and be able to track the spending of tax revenue better than windfalls. Second, according to Hypothesis 4, because of heightened accountability, citizens should perceive that elites can less readily misappropriate taxes than other revenues for personal or political gain. And third, according to Hypothesis 5, citizens should judge that leaders will use taxes more than oil or aid to provide public goods. Our results suggest that none of these three conditions holds in Ghana or Uganda.

To measure transparency and misappropriation risk, we asked respondents how likely it was that they would be able to observe how the new revenue was spent (`Difficulty Observe`), how likely their MP would be able to do so (`MP Know`), and the likelihood that politicians would use the new revenue to benefit themselves and their families (`Politicians Steal`). In the event that social desirability bias might drive respondents to under-report on the third measure, we also conducted a list experiment that contained as its sensitive item an option identical to the direct measure, asking subjects whether politicians might "use the [tax/oil] money to do favors for people and try to win their votes." We then took the subset of respondents who (randomly) were assigned to see the sensitive item and compared them across treatment groups (`Politicians Steal (List Exp)`). To measure the likelihood that spending from a given source would yield a measurable increase in the quality of public goods, we asked participants about the likelihood that the revenue would be used for public goods provision by helping their families, their communities, and the economy (`Help Family`, `Help Commun`, `Help Econ`). The conditions yield three empirical implications: that perceptions of misappropriation risk should be lower for taxes, and that transparency and public goods benefit should be higher.
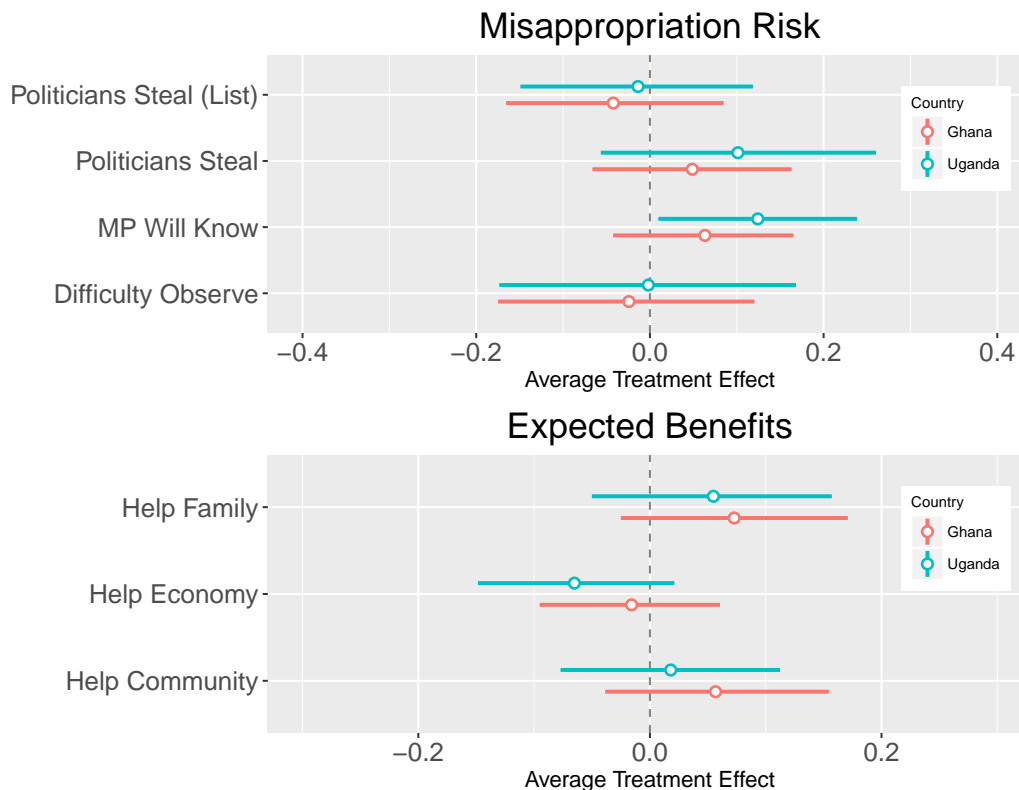
Figure 3: Tax Treatment Effects, Oil as Control. Effects in standard deviation units shown for transparency and misappropriation (top pane) and potential public goods benefit measures (bottom pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

Figures 3 and 4 report the results of randomization inference tests for these measures of transparency, misappropriation risk and potential benefit for three different revenue sources: oil, taxes, and aid. Both figures take non-tax revenue as the control condition and tax-based revenue as the treatment, such that positive values represent a positive treatment effect for taxes. Hypotheses 3- 5 find no support in our data. Consistent with the prior reported results, we find that respondents perceive very few differences between tax and non-tax revenue. All told, we find little evidence that perceptions of transparency, misappropriation risk, or propensity for public-goods provision are operating as expected by the argument that taxation causes accountability. Overall, there is no significant difference between tax and non-tax revenues in these key areas.
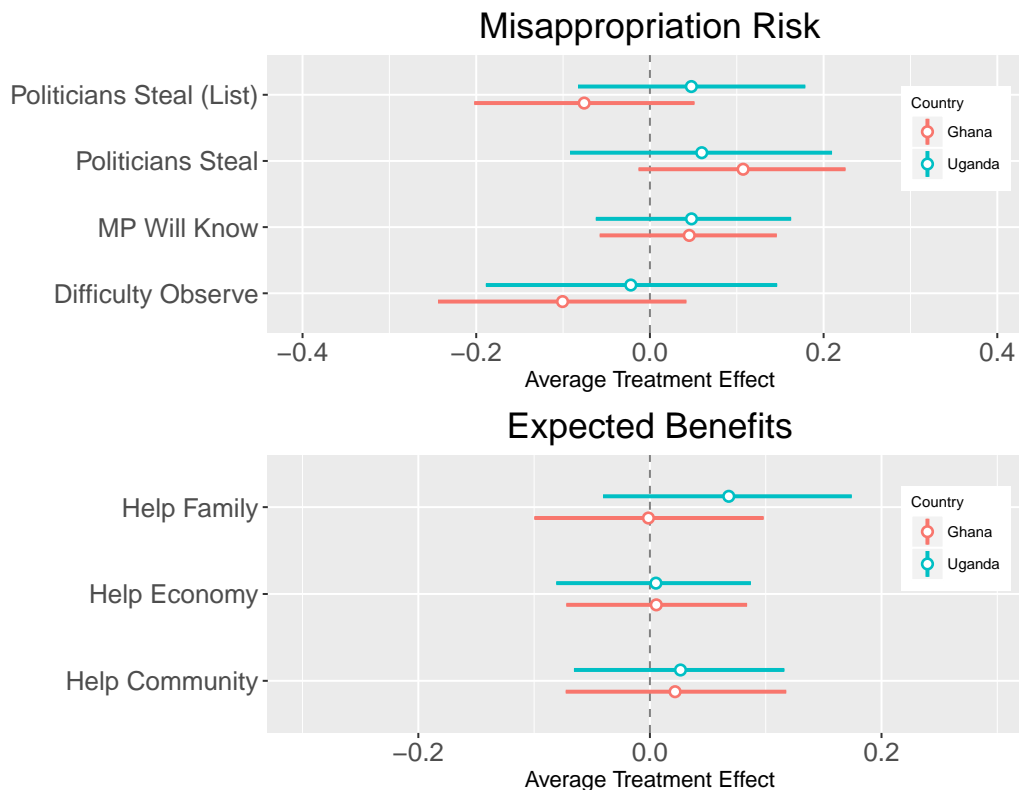
Figure 4: Tax Treatment Effects, Aid as Control. Effects in standard deviation units shown for transparency and misappropriation (top pane) and potential public goods benefit measures (bottom). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

Yet, the general lack of differences is not a function of a common belief in good behavior on the part of government actors: for example, in a separate item, an overwhelming majority (82 percent) of Ghanaian respondents reported that it was "very important" (the highest item in the scale) that the revenue be tracked, a proportion that is nearly invariant across treatments. Moreover, disaggregating the misappropriation measures demonstrates the high degree of cynicism that Ghanaians share with citizens in other African countries about elites' use of public funds for private gain: over 80 percent of the sample reported it was likely or somewhat likely their MP would know how the funds were spent, while 70 percent of the sample believed people like them would be unlikely or somewhat unlikely to learn how the money was spent. These quantities are similarly high in Uganda. In light of the absence of the differences we would expect according to the taxation-causes-accountability hypothesis, it remains an open question whether taxation leads to greater accountability in developing-country contexts.

6   Robustness Checks

We conduct three additional sets of analyses as robustness checks. We examine the subset who passed the manipulation checks to see if those who were treated still show null effects; we test whether our null effects derive from various subgroup effects that cancel each other out; and we explore whether prominent mechanisms may lead to indirect effects that are not null.

First, while enumerators delivered the prompt with special emphasis on the source of the revenue, not all respondents passed the post-treatment manipulation check. Thus, despite having received the treatment prompt, these respondents have not been "treated" insofar as they may not have adequately understood the critical piece of the prompt: the source of the additional revenues.[15] Because the treatment was a prompt rather than a more intense intervention, we crafted the manipulation check to be especially difficult: the test came many questions after the intervention and prompted respondents to recall the source of the new government revenue. Enumerators were instructed not to read any answer choices or give any assistance to respondents as they answered this question. Only an unassisted answer matching exactly the experimental source was coded as correct.

To see if non-compliers drive the intent-to-treat estimates towards a null result, we estimated the treatment effects for the subgroup of respondents who passed the manipulation check. Given the difficulty of passing the manipulation check in an open-ended prompt coming many questions after the intervention, passage rates were relatively high, averaging 70 percent across conditions and countries. However, subjects failed the manipulation check significantly more often for the tax condition, which may be one sign of how few people pay taxes in either country. There were thus significant selection effects across experimental conditions for correctly identifying the revenue source, and as a result it is not possible to make causal claims from these tests. Nonetheless, this restricted sample does pass balance tests for both the Tax-Oil and Tax-Aid comparisons. We report these results in Figures A.5 and A.6 in the appendix, with behavioral measures in the top panes and our theoretical willingness measures in the bottom panes. We also note that, if the most attentive subjects were those passing the manipulation check, the subgroup analysis should bias the findings in favor of the taxation-causes-accountability argument: those most attuned to taxes should be the subjects most willing to take action to monitor the use of tax money. The tax condition thus selects for a smaller, more exclusive group of subjects especially attentive to the mention of taxes. Yet

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[15]We reported results above for all subjects, which capture the effects of the intent to treat. We note that estimating the complier average causal effect (CACE) is not advised in this case because we did not employ a standard control condition; rather, we are comparing multiple treatment conditions against each other, so double-sided non-compliance is not symmetric.

as with the prior findings, most results are null with narrow confidence intervals suggesting precise estimation.

Second, our finding of null effects may not mean there are no causal effects if we are confounding different causal effects for different subgroups. Hence it is important to examine whether our results vary by subgroups that might experience the revenues in distinct ways. There may be subsets of respondents that are more able and willing to monitor and sanction tax-based revenues. We identify two important groups here, but examine many others in the appendix. Previous work suggests that wealthy, informed, income-tax-paying citizens are going to be most susceptible to the taxation leads to accountability effect (Bates and Lien 1985, Levi 1988). The logic is straightforward: because they are more likely to be paying income taxes, they should be both more likely to desire greater transparency as well as more likely to act on that desire. To capture these types of respondents, we created an indicator variable for those who reported paying both direct and indirect taxes and were urban (and thus more educated and wealthy, on average, than rural respondents). We classify these subjects as "high-type" respondents—that is, as people who were likely to have both the ability and desire to monitor tax-based revenues more closely than oil or aid money.

We also examined members of the opposition party in both countries since they may be most likely to feel that tax-based revenues will not be used to benefit them, and hence should seek greater transparency and stricter oversight of tax-based expenditures. We subset respondents by whether or not they self-identified as supporters of the governing party, which may proxy for expectations that the government will be more likely to spend revenues according to their preferences. We examined these two subgroups to learn if they can help us understand the differences or lack of them across the revenue sources, estimating ATEs for the same measures as above for both "low-type" and "high-type" respondents. In neither case do we see significantly different treatment effects for high- and low-type respondents.[16]

Taxpayers do not differ from non-taxpayers and opponents and supporters of the government do not differ from one another in terms of differential monitoring on taxes on our measures in response to experimental conditions. But might other factors condition reactions to these revenue sources? We investigated many other subgroups to learn if there were distinct causal reactions in effect leading to our null results. We examined respondents from oil regions and those not, those who were coethnic with the president, those who had high levels of trust in the government (as proxied by trust in the president), and those who had high levels of approval of central government's handling of a host

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[16]See Appendix D for a visualization and discussion of these results.

of public goods issues. These results are reported in the appendix in Tables B.6-B.9. In no case do we see significant subgroup effects after controlling for the false discovery rate. Critically, this holds even when testing for interactions between oil- and non-oil producing regions in both countries. Residents of oil-producing regions do not donate or sign the petition at differential rates, nor are they more likely to express a willingness to take actions such as contacting their local leaders or MPs. We do not think that our null effects on differential treatment result from combining groups who have opposing causal reactions to the revenue sources. All subgroup types in our representative samples seem to react the same way: they are willing to monitor or sanction at relatively high rates regardless of the source of revenue, arguably because all revenue sources are equally opaque, equally misappropriated, and equally likely to produce public goods.

Finally, some might argue that taxes could have an indirect effect on citizens' willingness to take action for greater government accountability. This may be especially plausible if subjects found our behavioral measures too costly to undertake at high rates but were nonetheless affected by the treatment in less concrete ways. While this possibility seems remote given the failure to find meaningful effects on our theoretical measures, it is nonetheless possible that such indirect effects exist. In order to examine some possible mechanisms by which such indirect effects could arise, we performed a mediation analysis on three particularly meaningful outcomes of interest: whether a subject donated to a transparency or good governance organization, how much they donated, and whether they signed the petition using their name.

There are two primary channels through which indirect effects are likely to occur. First, subjects may make inferences about the benefit that is likely to accrue to the public or to their community according to the revenue source. An endowment-effect type story would suggest, for example, that subjects will infer a greater likelihood of personal or communal benefit from tax revenues relative to oil and aid revenues because of the greater accountability pressures that is assumed to accompany tax-based expenditures. To test for this indirect effect, we modeled as mediators the survey items that asked subjects about the likelihood that their community, family, or the economy as a whole would benefit from the new revenue.

Second, subjects may be more willing to monitor oil or aid-based revenues out of a concern that such revenues will be misused at higher rates. To test for indirect effects from this channel, we modeled the items from the misappopriation index as mediators. Tables C.10 and C.11, available in the appendix, report a selection of estimates of the Average Causal Mediation Effect (ACME) in both Ghana and Uganda for the Tax-Oil comparison (e.g. tax-based revenue as the treatment condition

and oil-based revenue as the control condition). Similar results hold for the Tax-Aid comparison. In neither case do any of the mediation effects approach significance.[17]

7   Discussion

In this section we discuss possible objections to our claims and findings. Our most notable result is that the reported findings are not consistent with what we might expect if different revenue sources cause variation in citizen monitoring: there was no greater willingness to take action to increase the likelihood of transparency or accountability among those who received the tax treatment relative to the oil and aid treatments. Respondents in both countries are, in general, willing to take political action to monitor and sanction spending behavior of politicians, but not differentially. That is, they are equally willing to act in favor of better governance and more public goods, just not differently according to revenue source.

A skeptic may ask what theory we are examining and whether we are testing it fairly. Our project focuses on the argument that taxation causes representation, one made both in seminal works (Bates and Lien 1985, Huntington 1991, Levi 1988, North and Weingast 1989, Robinson, Torvik and Verdier 2006, Ross 2001, 2004, Schumpeter 1991) as well as more recent studies (Martin 2014, Paler 2013, Ross 2012). This claim is often connected to the resource curse, which argues that non-tax revenues cause great political (and economic) damage to countries at least in part because the impetus to monitor and sanction elected officials for the misappropriation of government revenues is thought to be weaker than with taxes. Nevertheless, we emphasize that our project does not test all hypotheses suggested by the broader resource curse phenomenon that posits multiple causal mechanisms involving macro political and economic processes unsuited to an experiment on individual citizens. Rather, this article explores the single resource-curse implication that windfalls cause citizen quiescence relative to taxes.

Nor does this study test the endowment effect, which is the psychological bias that has recently been proposed as a micro-level mechanism driving the (macro-level) resource curse (Martin 2014, Paler 2013). Discussion of the endowment effect was absent from earlier studies on publics' willingness to monitor and sanction governments. Instead, this literature suggested that, in a rational choice framework, the source of the differential effects is the costs to taxpayers caused by the misuse of tax funds not spent on public goods. Taxes are a critical part of the social contract citizens make

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[17]We use a standard battery of demographic controls to model the value of the mediator, but our results are robust to more extensive controls that include partisan affiliation and levels of political knowledge. All ACME estimates are produced using the `mediation` package in R.

with their government in return for public goods. As such, the failure to use tax revenues to provide public goods is considered by citizens as an abrogation of that contract.

Thus, in line with seminal contributions and consistent with our pre-analysis plan, we assumed that citizens would be more willing to pay greater costs to monitor and sanction the government when taxes were in play than when non-tax revenues such as aid and oil were invoked. Our results suggest that, counter to our expectations and existing theory, the public does not react differentially to these revenue sources. Yet our results also show that publics do care and will pay costs to take action when they think government funds are being diverted from public-goods provision. Social desirability bias might elevate levels of these actions, but our list experiment suggests little of such bias and any bias should operate across the revenue sources similarly. It simply appears that subjects are equally willing to take actions to monitor public money whether the revenues come from taxes, aid or oil. Contra expectations, there is no differential willingness to act in the case of taxes.

Why do we find that citizens are both willing to act and yet not at greater rates for taxes compared to windfalls? First, it could be related to our tax treatment. Many Ghanaians and Ugandans do not face tax obligations, especially direct income ones. According to the Afrobarometer survey, only 48 percent of Ghanaians and 24 percent of Ugandans believe they are obligated to pay income tax, and the proportions are still only 72 percent and 35 percent, respectively, for general sales tax. [18] So, perhaps subjects' relative inexperience with taxes makes them equally willing to pay costs to monitor all revenue sources. However, as we show, even those who are much more likely to pay taxes react the same to the three revenues sources, suggesting that experience with taxation does not account for the lack of differences.

Second, it could be that the treatment we gave subjects was too weak and failed to elicit the taxation-as-representation effect. We did not provide voluminous information to subjects but simply told them a certain sum of revenue was coming to the government from a particular source and that the money was intended for public goods. This approach has the dual advantage of being both clear and realistic. The revenue source was stated in a straightforward manner, and the vast majority of subjects understood it and could recall it in the manipulation check. The taxation-as-representation literature cited above suggests that identification of the revenue source as taxes should be sufficient to provoke the expected effect. Yet we reiterate that subjects who passed the manipulation check also did not act to monitor taxes in greater proportions than aid or oil.

Moreover, actual petition and fund-raising drives in these countries have a very similar setup to

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[18]See Afrobarometer Data, Ghana and Uganda, Round 5, 2011-2013, available at http://www.afrobarometer.org.

our research design: citizens visit door-to-door or stop their fellows on the street, provide information, and ask for support. We also did not see significant differences in the type of people who were willing to take action and donate. So the design—especially the behavioral outcomes—provides relatively high ecological validity. Moreover, the charge that the treatment is too weak does not square well with the rest of our findings. That is, we do see that many in the public are willing to take action and to pay costs to monitor their government's use of revenues—no matter the source. So all conditions were strong enough to elicit costly monitoring behavior. There is simply no differential effect.

Potentially, low levels of generalized willingness to act (or to state such a willingness) may be creating "floor effects," making the detection of statistically meaningful (but substantively small) differences more difficult. However, we find very little evidence for floor effects; in fact, precisely the opposite is the case. We report in Table 1 the mean proportion of respondents who are willing to engage in both actual and hypothetical monitoring or sanctioning across experimental conditions. Consistent with other work (see e.g., Paler 2013), Table 1 reveals that respondents in both Ghana and Uganda are generally willing to act in order to monitor a given revenue source or, in the case of the Contact measures, sanction its misuse.

A further criticism could be raised that, had we invoked the endowment effect for taxes by providing a windfall and then taxing it—compared with simply providing a windfall equal to the after-tax amount—we could have obtained a reaction that was different for taxes. We acknowledge this likelihood and point to well-designed studies both using this approach and finding some significant effects (Martin 2014, Paler 2013). However, we also worry that such an intervention may not reflect real-world conditions well in our research context. Income taxes are not relevant for the majority of subjects in Ghana and Uganda, and the indirect taxes they do experience obscure the actual amounts paid in ways that likely fail to invoke the endowment effect. Yet, in our study, even income taxpayers did not evince behavioral differences between treatment conditions, suggesting that the explanation for the null result lies elsewhere.

Our interpretation of the null findings focuses on the institutional contexts in low-income developing countries: in environments of low information, high corruption and a limited tax-base, there may be little reason for respondents to believe that the source of government revenues matters. Our results suggest that respondents generally view these different revenue sources as equally opaque and equally susceptible to corruption. If transparency is low and misappropriation risk high, it will be equally difficult to discover if funds have been misused across revenue sources. Likewise, in the presence of high corruption, the source of a revenue stream is unlikely to make a difference in citi-

zens' ability to benefit in purely welfare terms. As Waterbury (1994) noted, the relationship between revenue sources and political accountability is complex and there may be many intermediate factors that shape the impact that taxes and resource rents have on politics.

However, citizens in such poor countries may be willing to act because any resource their country possesses may acquire endowment effects. In a sense, the endowment effect may operate for all three types of sources, a finding seen in recent work (Author 2017). Oil revenues are often seen by citizens as their collective legacy to effect a better future, and therefore not to be squandered. The rhetoric surrounding the discovery and extraction of oil in both Uganda and Ghana has emphasized that the revenue resulting from oil exploration is a national good and should be treated as such. The public awareness and perception of these funds as being critical for development (and being intended for that purpose) may counteract the predisposition to value revenues raised via taxes more highly. And citizens may likewise view aid as another resource given to them to improve their lives and again one to be valued and monitored (Author 2016).

If this is the case, we should see a combination of high stated and actual willingness to monitor across all government revenue sources, but no differences between them in terms of their transparency, misappropriation risk, or potential benefit. Table 1 demonstrates the first point, that of high baseline willingness to act, while Figures 2-4 in the previous section suggest minimal differences in willingness to act, actual behavior, perceptions of transparency, misappropriation risk, or potential benefit across the revenue sources. In fact, despite large institutional differences between the two countries, the propensity to undertake action or to express a desire to do so is quite similar. As Table 1 reveals, the share of respondents who undertake our behavioral measures or who express a high likelihood of undertaking other political action is close to 50% for both countries. Ugandans were, in general, more likely to engage in our behavioral outcomes and also more likely to report that they would take other forms of political action. However, given Uganda's relatively higher level of corruption and lower level of development, these differences are not as large as one might expect.

8   Conclusion

Using behavioral measures of willingness to monitor elected officials for the misuse of revenues from taxes, oil, and aid, we have sought to understand when citizens choose to monitor some sources of spending and not others. While existing literature argues that a unique feature of taxation—the loss aversion associated with the appropriation of earned income—can produce strong incentives to hold politicians accountable for tax-based spending (Martin 2014), we sought to understand whether

taxes led to differential political action compared to other actual revenue sources such as foreign aid and oil in a setting that more closely approximated the information stream and political actions available to most sub-Saharan Africans, particularly in contrast to controlled laboratory settings.

We conducted substantively identical large survey experiments in two developing countries to determine whether differential revenue sources might, in fact, lead to greater willingness to monitor and enforce greater transparency for non-tax forms of spending, including oil and aid, which are often captured by elites. In general, our results suggest that citizens do not take greater action to monitor taxes over aid or oil. Moreover, they do not perceive greater transparency, misappropriation risk, or propensity toward public goods for one source over another. We explored many subgroups and found no significant alternative explanations for our null results among them. And we investigated a number of indirect causal paths that might show a differential effect for taxes but likewise found no evidence for them.

Our tentative answers to why we do not see taxes leading to greater action for accountability rest on two ideas that require further research. First, citizens in such poor countries may imbue non-tax resources with endowment effects just as they do taxes, leading them to be willing to take action at similar levels. All forms of revenue may be seen by citizens as important sources of public goods that can improve their lives. Second, the institutional environment may affect their ability to differentiate taxes from non-tax revenues. The use of obfuscating value-added taxes and very low reliance on income taxes for government revenue may both be factors. In addition, the degree to which political institutions within a country enhance transparency about revenues, curb corruption and clientelism, and allow for punishment of political elites may be critically important mediating factors. In institutional environments that fail to do these things, citizens may not differentiate among revenues both because they believe any source can be diverted away from its intended ends and they desperately want the public goods that could be provided. Further research should look into these ideas. In this sense, non-tax revenues thus may produce just as much, or as little, representation as do taxes.

Also notably, despite significant contextual differences between Uganda and Ghana, we do not find many differences in our results across countries. Ghana is significantly richer, more urban, more democratic, and more dependent on oil rents than Uganda, which is much more aid dependent. But in neither country do tax revenues seem to prompt citizens to demand or pay the costs for greater monitoring and accountability compared to windfalls. Our data from the two countries increase confidence in our null results about the lack of differential willingness to monitor taxes compared

to non-tax revenues from oil exploration and foreign aid. For many poor developing countries then, taxes and non-tax revenues may have similar effects on citizens and their willingness to monitor their governments.

In sum, Ghanaian and Ugandan citizens, when prompted about large revenues coming to their government, care enough about public spending to be willing to monitor and pay costs to keep their governments from diverting these funds from public goods provision—no matter the revenue source. We hope in future research to pinpoint the exact reasons behind such citizen action. But our results provide a more optimistic picture of non-tax revenues. They may be less of a curse than is conventionally believed, at least in the minds of citizens who experience their effects. Of course, our results do not rule out other channels by which aid and resource revenues might constitute a development "curse," but they do suggest that taxation revenues compared to aid or oil appear to cause no greater citizen demands for accountability in developing countries.

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APPENDIX: Taxation without Representation? Experimental Evidence from Ghana and Uganda on Citizen Action toward Taxes, Oil, and Aid

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A Randomization Inference Results

In the paper, Figures 1 and 2 directly test our first two hypotheses about differential action in favor of taxes. Figure 1 shows the tax treatment relative to the oil control and Figure 2 shows the tax treatment relative to the aid control. We asked citizens to undertake various costly actions, and thus our measure reflects the nature and intensity of their preferences. The top panes of Figures 1 and 2 report the treatment effects across experimental conditions on our primary outcomes of interest: the rate with which subjects sign anonymous or named petitions stating they would like for a new agency to be created to monitor revenues from either taxes, oil, or aid through government (`Anon. Petition` and `Named Petition`, respectively), whether they sent an SMS stating as much (`Sent SMS`), whether they donated to a third party transparency organization (`Donated Any`) and, if they donated, the amount (`Donated Amount`). Treatment effects reported in the following tables are on the original scale of the outcome measure and are not standardized unless otherwise noted. For illustration purposes, the donation amount measure is standardized to more closely match the scale of the remaining variables.

We also included several measures designed to capture respondents' willingness to both monitor the treatment source as well as sanction its misuse. Specifically, we asked respondents whether they desire an independent monitoring agency to be created to monitor spending from the revenue (`Create Agency`), how much in taxes they would be willing to pay monthly to fund it (`Agency WTP`), whether they would be willing to send an SMS to their MP stating their support for the agency (`Willing SMS`), and how likely the respondent was to contact their local village elder, LC3 Chairman (roughly equivalent to a US mayor), or MP in the event money from the source was misused (`Contact Elder`, `Contact LC3`, and `Contact MP` respectively).[1] The bottom panes of Figure 1 and Figure 2 report the treatment effects on these measures, with positive average treatment effects (ATEs) for respondents implying that the tax treatment induced more action, as hypotheses 1 and 2 imply. Index variables are created for each of the four major families of outcomes by taking the average of the non-missing values for all items comprising the index. Index variables are reported first, followed by the variables used to create the index.

Results presented below include the estimates used to produce Figures 1 and 2 in the paper. Also included are figures identical to Figures 1 and 2 except analysis is conducted only on those who passed the manipulation check. Figures A.5 and A.6 show the results from our main test for the group that passed the manipulation check. Tables A.1 and A.2 report treatment effects as estimated

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[1]The equivalent in Ghana was the district chief executive.

by randomization inference for both Uganda (Columns 1 and 3) and Ghana (Columns 2 and 4), and for both the Tax-Oil and Tax-Aid comparisons (Columns 1-2 and 3-4, respectively). As in the paper, positive point estimates represent an increase associated with the tax treatment relative to the aid or oil treatments. Finally, Table A.3 shows the results of our balance test that verified that randomization was successful, producing nearly identical covariate distributions between the treatment conditions.
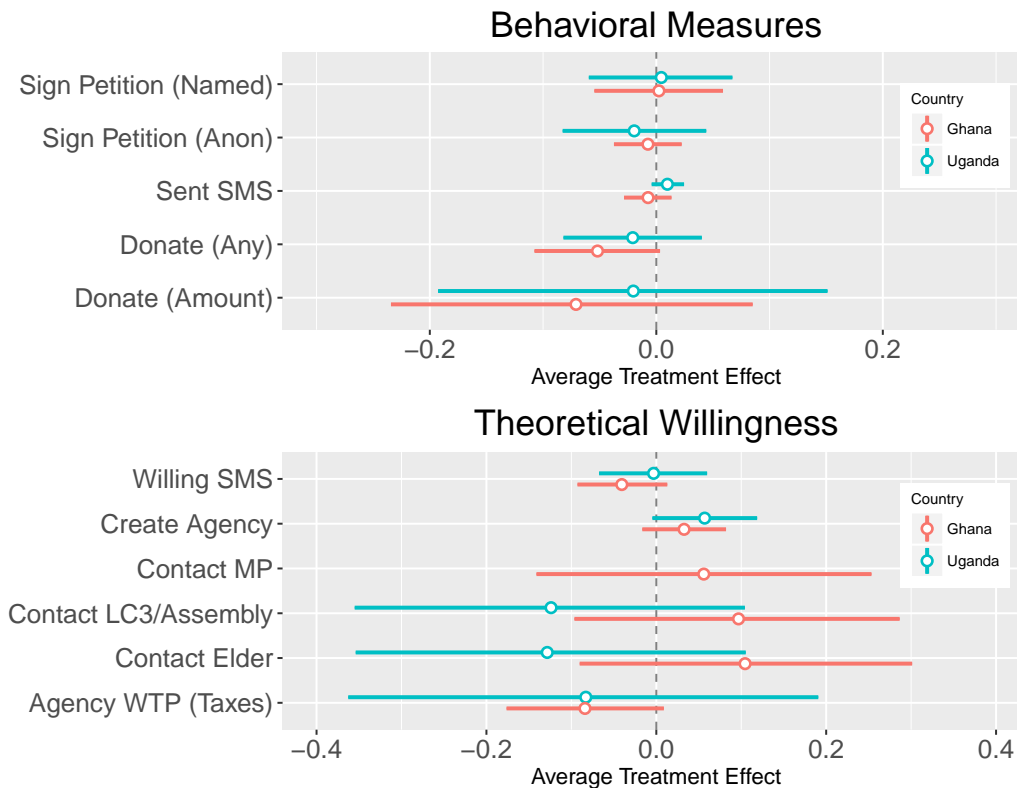
Figure A.5: Main Tax Treatment Effects, Oil as Control, Manipulation Pass Sample. Effects in standard deviation units shown for both behavioral (top) and attitudinal measures (bottom pane). 95% confidence intervals shown for both Uganda and Ghana. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.
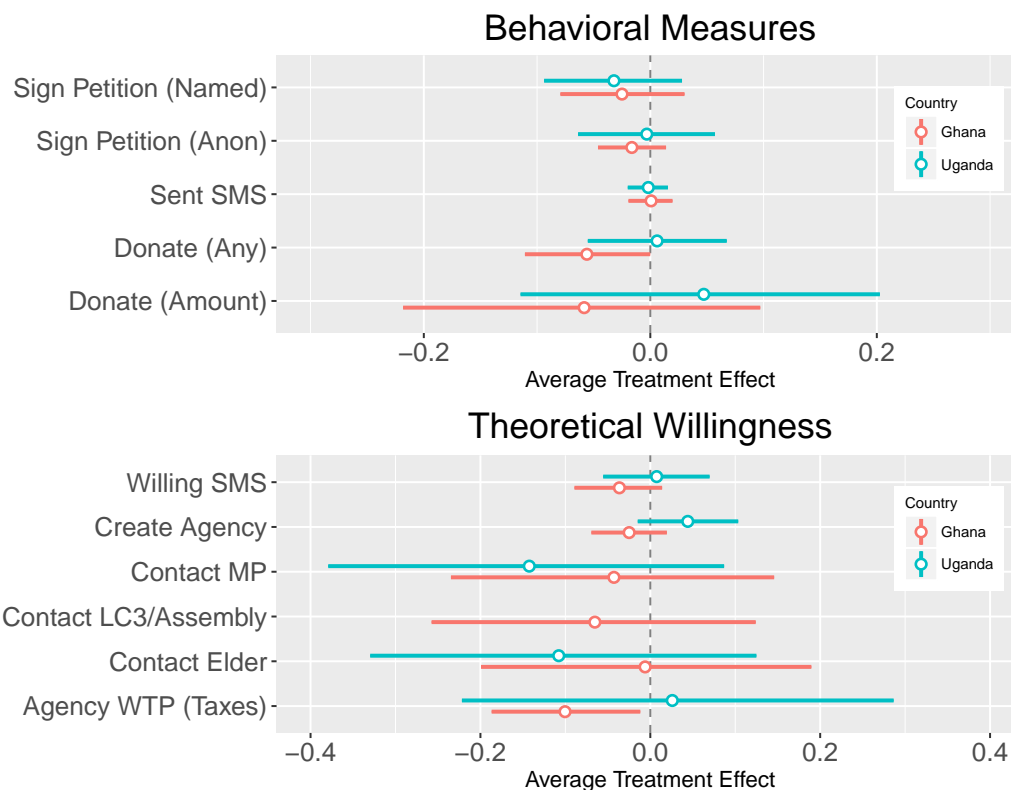
Figure A.6: Main Tax Treatment Effects, Aid as Control, Manipulation Pass Sample. Effects in standard deviation units shown for both behavioral (top pane) and attitudinal measures (bottom pane). 95% confidence intervals shown for both Uganda (solid lines) and Ghana (dashed lines). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

	Tax-Oil		Tax-Aid	
	Uganda	Ghana	Uganda	Ghana
Behavioral Index	0.021	−0.010	0.053	−0.041
	(0.662)	(0.414)	(0.853)	(0.192)
Sign Petition (Anon)	−0.024	0.010	−0.010	−0.001
	(0.151)	(0.816)	(0.347)	(0.442)
Sign Petition (Named)	0.006	−0.025	−0.022	−0.013
	(0.611)	(0.142)	(0.173)	(0.314)
Sent SMS	0.014	−0.007	0.001	−0.001
	(0.980)	(0.175)	(0.493)	(0.506)
Donate (Any)	−0.012	−0.007	0.009	−0.022
	(0.304)	(0.403)	(0.663)	(0.170)
Donate (Amount)	17.419	0.038	15.463	−0.015
	(0.849)	(0.640)	(0.823)	(0.434)
Willingness Index	−0.002	0.036	0.001	−0.010
	(0.478)	(0.777)	(0.512)	(0.418)
Create Agency	0.038	0.026	0.025	−0.007
	(0.945)	(0.904)	(0.856)	(0.348)
Agency WTP (Taxes)	−0.107	−0.057*	−0.025	−0.080**
	(0.168)	(0.056)	(0.414)	(0.013)
Willing SMS	−0.017	−0.016	−0.012	−0.004
	(0.231)	(0.225)	(0.332)	(0.419)
Contact Elder	−0.011	0.069	0.030	0.004
	(0.450)	(0.798)	(0.630)	(0.519)
Contact LC3/Assembly	0.014	0.094	−0.094	−0.008
	(0.572)	(0.879)	(0.156)	(0.457)
Contact MP	−0.140*	0.064	−0.011	0.042
	(0.061)	(0.785)	(0.454)	(0.694)

*p < .1; **p < .05; ***p < .01

Table A.1: Behavioral and Willingness Measures, Randomization Inference. Columns 1 and 3 report the Ugandan results while Columns 2 and 4 Report the Ghanaian estimates. Two-tailed p-values in parentheses. None of the significant results survive a multiple testing correction.

	Oil-Tax		Aid-Tax	
	Uganda	Ghana	Uganda	Ghana
Misappropriation Index	0.069	0.087	0.0003	0.037
	(0.919)	(0.964)	(0.494)	(0.790)
Difficulty Observe	−0.002	−0.024	−0.022	−0.101*
	(0.501)	(0.366)	(0.394)	(0.088)
MP Will Know	0.124	0.063	0.048	0.045
	(0.984)	(0.885)	(0.788)	(0.807)
Politicians Steal	0.101	0.049	0.060	0.107
	(0.903)	(0.792)	(0.777)	(0.958)
Politicians Steal (List)	−0.014	−0.042	0.048	−0.076
	(0.405)	(0.250)	(0.777)	(0.109)
Benefit Index	0.040	0.024	0.015	0.041
	(0.781)	(0.689)	(0.617)	(0.795)
Benefit Family	0.055	0.073	0.068	−0.001
	(0.855)	(0.928)	(0.895)	(0.485)
Benefit Community	0.018	0.057	0.026	0.022
	(0.645)	(0.885)	(0.725)	(0.679)
Benefit Economy	−0.065*	−0.016	0.005	0.006
	(0.058)	(0.357)	(0.564)	(0.565)
Benefit Country	0.096	−0.028	−0.049	0.056
	(0.898)	(0.317)	(0.259)	(0.825)

*$p < .1$; **$p < .05$; ***$p < .01$

Table A.2: Misappropriation Risk and Potential Benefit Measures, Randomization Inference. Columns 1 and 3 report the Ugandan results while Columns 2 and 4 Report the Ghanaian estimates. Two-tailed p-values in parentheses.

	Ghana		Uganda	
	Aid-Tax	**Oil-Tax**	**Aid-Tax**	**Oil-Tax**
Covariate	p-value		p-value	
Age	0.638	0.396	0.468	0.489
Employed	0.379	0.543	0.839	0.454
Female	0.789	0.621	0.583	0.866
Rural	0.587	0.905	0.899	0.898
Education	0.098	0.753	0.378	0.63
Coethnic Pres	0.464	0.479	0.691	0.619
Trust Pres	0.777	0.889	0.513	0.795
Gov Supporter	0.559	0.515	0.802	0.903
Held Pol Position	0.467	0.807	0.207	0.469
Voted	0.236	0.804	0.082	0.042
Gov Corrupt	0.311	0.223	0.811	0.949

Table A.3: Balance Test. Estimates obtained using conventional difference in means estimator. The few significant results do not survive a multiple testing correction. Null results are consistent with successful randomization.

B Robustness Test: Regression with Block Fixed-Effects

All regression specifications include block fixed-effects as well as the covariates reported in Table A.3. Given the efficiency gains from adjustment, this is an especially conservative robustness check as it increases the probability of obtaining significant results. These results demonstrate that the null findings presented in the paper are not an artifact of the estimation method. Also included are regression estimates of heterogeneous treatment effects for a variety of subgroups. The vast majority of these effects are not significant at conventional levels, and those few that are do not survive a multiple testing correction. Logistic regression is used for all binary variables (e.g. `Anon Petition`, `Named Petition`, `Sent SMS`, `Donated Any`, `Willing SMS`, `Create Agency`) and conventional OLS is used for all remaining measures. Reported estimates are the coefficients from these regressions with (classical) standard errors in parentheses. Like the randomization inference results, the dependent variables of interest are in the rows, with the comparison arranged by country and group in the columns.

Table B.4 and Table B.5 replicate the main randomization inference results with regression analysis. While the point estimates cannot be directly compared between the two approaches due to the use of adjustment and, in the case of binary variables, the logistic link function, the general pattern of statistical significance remains unchanged regardless of the approach. Tables B.6-B.9 report a series of interaction results. Dependent variables are in the rows as in other tables, with the reported coefficients representing the additive effect of the treatment when subjects belong to the subgroup of interest (given in the column) relative to those subjects not in that group. Index variables are created for each of the four major families of outcomes by taking the average of the non-missing values for all items comprising the index. Index variables are reported first, followed by the variables used to create the index. The `Sent SMS` variable is not included in Tables B.6-B.9 because the small number of sent texts makes heterogeneous effect estimation using block fixed-effects infeasible.

	Oil-Tax		Aid-Tax	
	Uganda	Ghana	Uganda	Ghana
Behavioral Index	0.023	−0.014	0.059	−0.059
	(0.048)	(0.043)	(0.048)	(0.043)
Sign Petition (Anon)	−0.140	0.160	0.001	0.014
	(0.119)	(0.191)	(0.119)	(0.187)
Sign Petition (Named)	0.032	−0.185	−0.081	−0.131
	(0.116)	(0.113)	(0.115)	(0.112)
Sent SMS	0.922*	−0.142	−0.226	0.061
	(0.521)	(0.289)	(0.397)	(0.302)
Donate (Any)	−0.023	−0.041	0.106	−0.146
	(0.122)	(0.112)	(0.122)	(0.111)
Donate (Amount)	15.623	0.025	13.974	−0.064
	(15.763)	(0.096)	(15.955)	(0.095)
Willingness Index	−0.012	0.063	0.030	−0.005
	(0.047)	(0.044)	(0.047)	(0.044)
Create Agency	0.199*	0.124	0.213*	−0.086
	(0.120)	(0.127)	(0.120)	(0.130)
Agency WTP (Taxes)	−0.054	−0.042	0.001	−0.087**
	(0.109)	(0.037)	(0.108)	(0.036)
Willing SMS	−0.130	−0.118	−0.027	−0.038
	(0.123)	(0.121)	(0.123)	(0.120)
Contact Elder	−0.045	0.127	0.029	0.030
	(0.085)	(0.078)	(0.085)	(0.078)
Contact LC3/Assembly	0.018	0.119	−0.067	−0.009
	(0.087)	(0.077)	(0.087)	(0.076)
Contact MP	−0.141	0.106	0.013	0.043
	(0.089)	(0.077)	(0.089)	(0.077)

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B.4: Behavioral and Willingness Effects, Regression Results. Columns 1 and 3 report the Ugandan results while Columns 2 and 4 Report the Ghanaian estimates. Standard errors in parenthesis. Reported estimates obtained using block fixed-effects as well as standard battery of demographic covariates. All binary outcome measures modeled using logistic regression, with effects representing the log of the factor change in the likelihood of taking the given action holding all other variables constant. None of the significant results survive a multiple-testing correction.

	Oil-Tax		Aid-Tax	
	Uganda	Ghana	Uganda	Ghana
Misappropriation Index	0.070	0.070	−0.029	0.052
	(0.047)	(0.043)	(0.047)	(0.043)
Difficulty Observe	−0.027	−0.030	−0.078	−0.062
	(0.079)	(0.067)	(0.079)	(0.067)
MP Will Know	0.134**	0.045	0.027	0.023
	(0.059)	(0.048)	(0.059)	(0.048)
Politicians Steal	0.138*	0.059	0.073	0.123**
	(0.081)	(0.057)	(0.081)	(0.056)
Politicians Steal (List)	−0.024	−0.070	0.011	−0.087
	(0.064)	(0.059)	(0.064)	(0.060)
Benefit Index	0.033	0.030	0.028	0.042
	(0.046)	(0.041)	(0.046)	(0.041)
Benefit Family	0.037	0.074	0.061	0.001
	(0.051)	(0.046)	(0.051)	(0.046)
Benefit Community	0.027	0.076*	0.052	0.039
	(0.045)	(0.043)	(0.045)	(0.043)
Benefit Economy	−0.063	−0.002	0.011	0.009
	(0.041)	(0.038)	(0.041)	(0.038)

*p < .1; **p < .05; ***p < .01

Table B.5: Misappropriation Risk and Potential Benefit Effects, Regression Results. Columns 1 and 3 report the Ugandan results while Columns 2 and 4 Report the Ghanaian estimates. Reported estimates obtained using block fixed-effects as well as standard battery of demographic covariates. All binary outcome measures modeled using logistic regression, with effects representing the log of the factor change in the likelihood of taking the given action holding all other variables constant. Standard errors in parenthesis. None of the significant results survive a multiple-testing correction.

	High Type	Gov Supporter	Oil Region	Female	High Trust	Coethnic Pres	High Approval
Behavioral Index	−0.160	0.073	−0.099	0.139	0.089	0.093	−0.171
	(0.125)	(0.120)	(0.150)	(0.103)	(0.127)	(0.175)	(0.112)
Sign Petition (Anon)	0.127	−0.067	0.251	−0.301	−0.100	0.055	0.112
	(0.255)	(0.246)	(0.308)	(0.211)	(0.259)	(0.359)	(0.228)
Sign Petition (Named)	−0.113	−0.182	0.172	−0.253	0.025	0.017	−0.018
	(0.257)	(0.248)	(0.316)	(0.213)	(0.261)	(0.367)	(0.230)
Donate (Any)	−0.424*	0.284	−0.215	0.155	0.222	0.019	−0.439*
	(0.256)	(0.245)	(0.311)	(0.211)	(0.259)	(0.366)	(0.228)
Donate (Amount)	−15.836	−18.102	8.659	40.339	−1.693	44.947	−12.667
	(40.484)	(39.170)	(46.919)	(33.298)	(41.001)	(53.584)	(36.895)
Willingness Index	0.135	−0.057	0.233	0.165	−0.051	0.255	0.061
	(0.122)	(0.118)	(0.147)	(0.101)	(0.124)	(0.171)	(0.109)
Create Agency	−0.090	−0.187	0.339	−0.159	0.003	0.587	0.261
	(0.264)	(0.253)	(0.316)	(0.218)	(0.267)	(0.387)	(0.238)
Agency WTP (Taxes)	−0.193	−0.285	0.208	0.181	0.054	0.143	−0.060
	(0.276)	(0.267)	(0.336)	(0.228)	(0.281)	(0.368)	(0.245)
Willing SMS	−0.009	−0.247	−0.166	0.088	−0.062	0.436	−0.332
	(0.260)	(0.251)	(0.322)	(0.215)	(0.264)	(0.377)	(0.233)
Contact Elder	0.237	−0.131	0.387	0.263	−0.126	0.176	0.396**
	(0.223)	(0.215)	(0.268)	(0.184)	(0.226)	(0.310)	(0.199)
Contact LC3/Assembly	0.363	0.047	0.199	0.342*	−0.041	0.245	0.125
	(0.223)	(0.214)	(0.267)	(0.184)	(0.226)	(0.309)	(0.199)
Contact MP	0.162	0.115	0.155	0.333*	−0.318	0.324	−0.015
	(0.233)	(0.225)	(0.281)	(0.192)	(0.237)	(0.323)	(0.209)

*p < .1; **p < .05; ***p < .01

Table B.6: Heterogeneous Treatment Effects, Ugandan Estimates, Tax-Oil Comparison. Estimates obtained using regression with block fixed-effects and a standard battery of demographic characteristics. Reported estimates are interaction effects that test whether the effect of the Tax treatment varies across potential moderators, including whether the respondent had experience paying taxes (High Type), was a government supporter (Gov Supporter), resides in an oil region (Oil Region), is female, reported high levels of trust in the government (Trust High), was a coethnic with the sitting president (Coethnic Pres), and approved of recent central government performance (High Approval). Standard errors in parentheses. Logistic regression used for binary variables, which are italicized. Because few respondents actually sent a text and many of our subgroups of interest are relatively small, the number of respondents who had both sent a text and belonged to the subgroup made estimation of this effect infeasible. We thus drop the measure here.

	High Type	Gov Supporter	Oil Region	Female	High Trust	Coethnic Pres	High Approval
Behavioral Index	−0.099	0.090	−0.050	0.194*	0.046	0.035	−0.001
	(0.126)	(0.121)	(0.149)	(0.103)	(0.124)	(0.175)	(0.111)
Sign Petition (Anon)	0.090	0.006	0.411	−0.210	−0.349	−0.198	0.254
	(0.258)	(0.248)	(0.308)	(0.212)	(0.254)	(0.365)	(0.227)
Sign Petition (Named)	−0.444*	0.076	−0.058	0.080	−0.661**	−0.285	0.183
	(0.264)	(0.252)	(0.312)	(0.216)	(0.259)	(0.386)	(0.231)
Donate (Any)	−0.289	0.045	0.089	0.032	0.014	−0.175	−0.037
	(0.258)	(0.246)	(0.306)	(0.212)	(0.253)	(0.374)	(0.226)
Donate (Amount)	−8.688	27.275	−17.905	80.097**	21.530	36.199	15.609
	(41.554)	(40.028)	(47.453)	(33.483)	(40.933)	(52.792)	(37.295)
Willingness Index	0.039	−0.076	0.113	0.090	−0.096	−0.021	0.099
	(0.123)	(0.118)	(0.146)	(0.101)	(0.121)	(0.171)	(0.108)
Create Agency	−0.215	−0.126	0.411	−0.118	−0.068	0.167	0.149
	(0.268)	(0.254)	(0.312)	(0.219)	(0.261)	(0.398)	(0.238)
Agency WTP (Taxes)	0.108	−0.310	−0.296	0.348	0.104	0.430	0.001
	(0.277)	(0.265)	(0.333)	(0.228)	(0.273)	(0.359)	(0.241)
Willing SMS	0.144	−0.038	−0.163	−0.097	−0.222	0.572	−0.088
	(0.261)	(0.250)	(0.316)	(0.214)	(0.256)	(0.375)	(0.229)
Contact Elder	0.199	−0.165	0.070	0.172	−0.365*	−0.111	0.442**
	(0.224)	(0.214)	(0.264)	(0.184)	(0.220)	(0.311)	(0.197)
Contact LC3/Assembly	0.260	0.038	0.052	0.344*	−0.141	−0.410	0.081
	(0.225)	(0.215)	(0.263)	(0.184)	(0.220)	(0.311)	(0.197)
Contact MP	−0.151	0.071	0.100	−0.050	−0.056	−0.293	0.038
	(0.234)	(0.225)	(0.278)	(0.192)	(0.232)	(0.327)	(0.207)

*p < .1; **p < .05; ***p < .01

Table B.7: Heterogeneous Treatment Effects, Ugandan Estimates, Tax-Aid Comparison. Estimates obtained using regression with block fixed-effects and a standard battery of demographic characteristics. Reported estimates are interaction effects that test whether the effect of the Tax treatment varies across potential moderators, including whether the respondent had experience paying taxes (High Type), was a government supporter (Gov Supporter), resides in an oil region (Oil Region), is female, reported high levels of trust in the government (Trust High), was a coethnic with the sitting president (Coethnic Pres), and approved of recent central government performance (High Approval). Standard errors in parentheses. Logistic regression used for binary variables, which are italicized. Because few respondents actually sent a text and many of our subgroups of interest are relatively small, the number of respondents who had both sent a text and belonged to the subgroup made estimation of this effect infeasible. We thus drop the measure here.

	High Type	Gov Supporter	Oil Region	Female	High Trust	Coethnic Pres	High Approval
Behavioral Index	0.027	0.005	−0.076	0.171*	−0.170	−0.125	0.023
	(0.132)	(0.121)	(0.185)	(0.104)	(0.116)	(0.288)	(0.128)
Sign Petition (Anon)	−0.808	0.067	0.614	0.110	0.196	12.818	0.191
	(0.536)	(0.514)	(0.919)	(0.427)	(0.478)	(316.277)	(0.533)
Sign Petition (Named)	0.268	−0.119	−0.784*	−0.074	−0.268	−1.826***	0.027
	(0.292)	(0.262)	(0.456)	(0.224)	(0.250)	(0.683)	(0.277)
Donate (Any)	−0.260	−0.088	−0.198	0.255	−0.302	−0.594	0.147
	(0.287)	(0.266)	(0.479)	(0.228)	(0.254)	(0.645)	(0.281)
Donate (Amount)	0.305	0.200	0.015	0.420*	−0.233	1.468**	−0.064
	(0.282)	(0.294)	(0.333)	(0.246)	(0.275)	(0.730)	(0.299)
Willingness Index	−0.048	−0.034	0.098	−0.097	−0.034	−0.446	−0.023
	(0.133)	(0.123)	(0.188)	(0.105)	(0.117)	(0.293)	(0.130)
Create Agency	−0.415	−0.244	0.146	−0.365	0.133	−0.160	−0.110
	(0.342)	(0.307)	(0.608)	(0.258)	(0.286)	(0.715)	(0.326)
Agency WTP (Taxes)	0.022	0.019	0.111	0.137	0.055	0.039	0.027
	(0.106)	(0.102)	(0.143)	(0.087)	(0.097)	(0.236)	(0.106)
Willing SMS	0.107	−0.153	−0.345	−0.148	−0.319	−1.910**	0.427
	(0.292)	(0.289)	(0.406)	(0.253)	(0.278)	(0.791)	(0.303)
Contact Elder	−0.078	−0.040	0.056	−0.110	−0.107	−0.678	−0.020
	(0.235)	(0.215)	(0.329)	(0.185)	(0.206)	(0.511)	(0.227)
Contact LC3/Assembly	−0.120	−0.114	0.256	−0.119	−0.167	−0.516	0.137
	(0.231)	(0.213)	(0.325)	(0.183)	(0.203)	(0.505)	(0.225)
Contact MP	−0.149	0.029	0.273	−0.108	0.065	−0.556	−0.166
	(0.233)	(0.215)	(0.330)	(0.184)	(0.205)	(0.511)	(0.227)

*p < .1; **p < .05; ***p < .01

Table B.8: Heterogeneous Treatment Effects, Ghanaian Estimates, Tax-Oil Comparison. Estimates obtained using regression with block fixed-effects and a standard battery of demographic characteristics. Reported estimates are interaction effects that test whether the effect of the Tax treatment varies across potential moderators, including whether the respondent had experience paying taxes (High Type), was a government supporter (Gov Supporter), resides in an oil region (Oil Region), is female (Female), reported high levels of trust in the government (Trust High), was a coethnic with the sitting president (Coethnic Pres), and approved of recent central government performance (High Approval). Standard errors in parentheses. Logistic regression used for binary variables, which are italicized. Because few respondents actually sent a text and many of our subgroups of interest are relatively small, the number of respondents who had both sent a text and belonged to the subgroup made estimation of this effect infeasible. We thus drop the measure here.

	High Type	Gov Supporter	Oil Region	Female	High Trust	Coethnic Pres	High Approval
Behavioral Index	0.099	−0.030	0.058	0.113	−0.135	0.352	−0.012
	(0.134)	(0.119)	(0.186)	(0.104)	(0.116)	(0.307)	(0.131)
Sign Petition (Anon)	−0.949*	−0.325	1.031	−0.059	1.028**	0.022	−0.218
	(0.517)	(0.481)	(0.922)	(0.406)	(0.474)	(1.298)	(0.517)
Sign Petition (Named)	0.544*	−0.061	−0.662	0.180	−0.468*	−0.729	0.142
	(0.294)	(0.257)	(0.443)	(0.223)	(0.250)	(0.668)	(0.281)
Donate (Any)	0.188	−0.204	0.240	0.072	−0.224	0.587	0.024
	(0.289)	(0.259)	(0.459)	(0.224)	(0.252)	(0.744)	(0.283)
Donate (Amount)	−0.004	0.149	0.031	0.226	−0.159	1.757*	−0.097
	(0.289)	(0.281)	(0.338)	(0.238)	(0.269)	(0.914)	(0.301)
Willingness Index	0.089	−0.098	0.015	−0.002	0.063	−0.060	0.056
	(0.135)	(0.121)	(0.189)	(0.105)	(0.118)	(0.311)	(0.133)
Create Agency	−0.170	−0.086	−0.318	0.044	0.279	−0.406	0.120
	(0.353)	(0.307)	(0.691)	(0.262)	(0.295)	(0.814)	(0.332)
Agency WTP (Taxes)	−0.121	−0.056	0.040	0.091	0.223**	0.138	−0.026
	(0.108)	(0.100)	(0.141)	(0.086)	(0.096)	(0.251)	(0.109)
Willing SMS	0.122	−0.272	−0.063	0.190	0.076	−1.217	0.362
	(0.298)	(0.288)	(0.410)	(0.254)	(0.279)	(0.849)	(0.312)
Contact Elder	0.124	−0.171	−0.036	−0.093	0.111	−0.221	0.160
	(0.239)	(0.213)	(0.330)	(0.184)	(0.207)	(0.543)	(0.232)
Contact LC3/Assembly	0.177	−0.033	0.222	−0.093	−0.118	0.106	0.040
	(0.235)	(0.210)	(0.326)	(0.182)	(0.204)	(0.537)	(0.229)
Contact MP	0.161	−0.209	−0.048	0.054	0.037	0.259	−0.041
	(0.237)	(0.211)	(0.331)	(0.184)	(0.206)	(0.543)	(0.232)

*p < .1; **p < .05; ***p < .01

Table B.9: Heterogeneous Treatment Effects, Ghanaian Estimates, Tax-Aid Comparison. Estimates obtained using regression with block fixed-effects and a standard battery of demographic characteristics. Reported estimates are interaction effects that test whether the effect of the Tax treatment varies across potential moderators, including whether the respondent had experience paying taxes (High Type), was a government supporter (Gov Supporter), resides in an oil region (Oil Region), is female (Female), reported high levels of trust in the government (Trust High), was a coethnic with the sitting president (Coethnic Pres), and approved of recent central government performance (High Approval). Standard errors in parentheses. Logistic regression used for binary variables. Because few respondents actually sent a text and many of our subgroups of interest are relatively small, the number of respondents who had both sent a text and belonged to the subgroup made estimation of this effect infeasible. We thus drop the measure here.

C Mediation Analysis

Subjects may be more willing to monitor oil or aid-based revenues out of a concern that such revenues will be misused at higher rates. To test for indirect effects from this channel, we modeled the items from the misappopriation index as mediators. Tables C.11 and C.10 report a selection of estimates of the Average Causal Mediation Effect (ACME) in both Ghana and Uganda for the Tax-Oil comparison (e.g. tax-based revenue as the treatment condition and oil-based revenue as the control condition). These estimates can be interpreted as the indirect effect of the treatment through the chosen mediator. Similar results hold for the Tax-Aid comparison. In neither case do any of the mediation effects approach significance.

The identifying assumption required for ACME estimates to be valid is that of sequential ignorability. In the context of mediation analysis, sequential ignorability requires that, conditional on covariates, the mediator is randomly assigned. We use a standard battery of demographic controls to model the value of the mediator, but our results are robust to more extensive controls that include partisan affiliation, and levels of political knowledge. All ACME estimates are produced using the `mediation` package in R. Index variables are created for each of the four major families of outcomes by taking the average of the non-missing values for all items comprising the index. Index variables are reported first, followed by the variables used to create the index.

	Help Family	Help Community	Politicians Steal	Difficulty Observe
Behavioral Index	0.010	0.008	0.001	0.001
	(0.298)	(0.382)	(0.878)	(0.832)
Sign (Anon)	0.010	0.009	0.001	0.001
	(0.240)	(0.358)	(0.898)	(0.830)
Sign (Named)	0.010	0.008	0.001	0.001
	(0.250)	(0.338)	(0.838)	(0.810)
Sent SMS	0.010	0.008	0.001	0.001
	(0.260)	(0.386)	(0.880)	(0.820)
Donate (Any)	0.010	0.008	0.001	0.001
	(0.278)	(0.358)	(0.882)	(0.852)
Donate (Amount)	0.011	0.008	0.002	0.001
	(0.262)	(0.282)	(0.848)	(0.832)

*p < .1; **p < .05; ***p < .01

Table C.10: Ugandan Estimates, Mediation Analysis, Tax-Oil Comparison. All estimates are the Average Causal Mediation Effect obtained with the `mediation` package in R. Each column represents one potential mediator, with each mediator's effect reported for the outcome measures of interest in each row. Positive effects represent increases associated for those receiving the Tax condition relative to the oil condition. Two-sided p-values in parentheses. The `Behavioral Index` variable is a simple weighted average of the non-missing behavioral measures for each respondent.

	Help Family	Help Community	Politicians Steal	Difficulty Observe
Behavioral Index	−0.005	0.002	0.004	−0.005
	(0.564)	(0.712)	(0.722)	(0.476)
Sign Petition (Anon)	−0.004	0.002	0.002	−0.005
	(0.612)	(0.716)	(0.814)	(0.410)
Sign Petition (Named)	−0.005	0.003	0.003	−0.005
	(0.534)	(0.666)	(0.806)	(0.446)
Sent SMS	−0.004	0.002	0.003	−0.005
	(0.598)	(0.716)	(0.752)	(0.384)
Donate (Any)	−0.005	0.002	0.003	−0.005
	(0.520)	(0.716)	(0.784)	(0.466)
Donate (Amount)	−0.004	0.002	0.003	−0.005
	(0.606)	(0.748)	(0.742)	(0.434)

*p < .1; **p < .05; ***p < .01

Table C.11: Ghanaian Estimates, Mediation Analysis, Tax-Oil Comparison. All estimates are the Average Causal Mediation Effect obtained with the `mediation` package in R. Each column represents one potential mediator, with each mediator's effect reported for the outcome measures of interest in each row. Positive effects represent increases associated for those receiving the Tax condition relative to the oil condition. Two-sided p-values in parentheses. The `Behavioral Index` variable is a simple weighted average of the non-missing behavioral measures for each respondent.

D Heterogeneous Treatment Effects

Figures D.7-D.10 visualize the results of analysis identical to that in the previous section for high-
and low-type respondents for the Tax-Oil and Tax-Aid comparisons. Figures D.7 and D.9 show the
treatments effects for taxes relative to oil, and Figures D.8 and D.10 for taxes relative to aid. We
expect positive treatment effects for high types who pay taxes and are well educated, and none or
negative ones for low types. And we expect positives ones for opposition members and negative ones
for governing party supporters in both countries. The results suggest no evidence that a positive
effect exists for respondents with characteristics that would make them the most likely group to
monitor tax-based expenditures. The only treatment effects that approach conventional significance
are, in fact, in the opposite direction if the taxation-causes-accountability argument were to hold. Yet
controlling the false discovery rate at conventional levels using the Benjamini-Hochberg procedure
eliminates even those few results that are marginally significant.
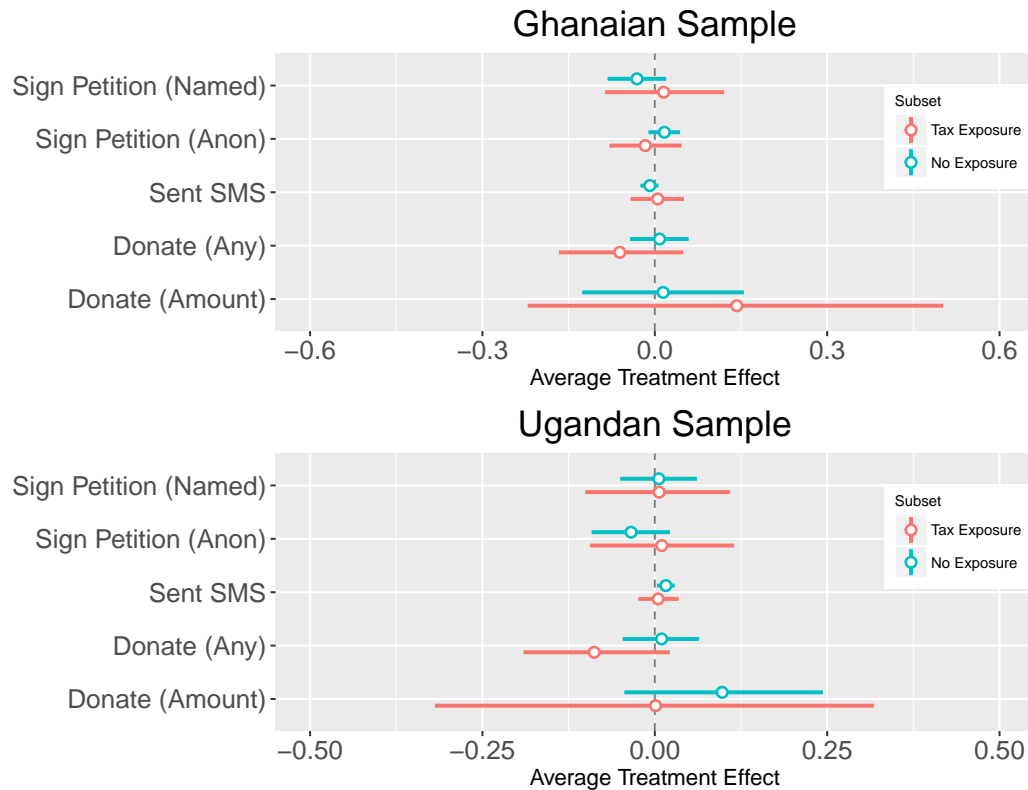
Figure D.7: High-Type vs. Low-Type Respondents, Tax-Oil. Estimates obtained using randomiza-
tion inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence
intervals obtained via inversion of constant effects hypothesis as implemented in ri package.
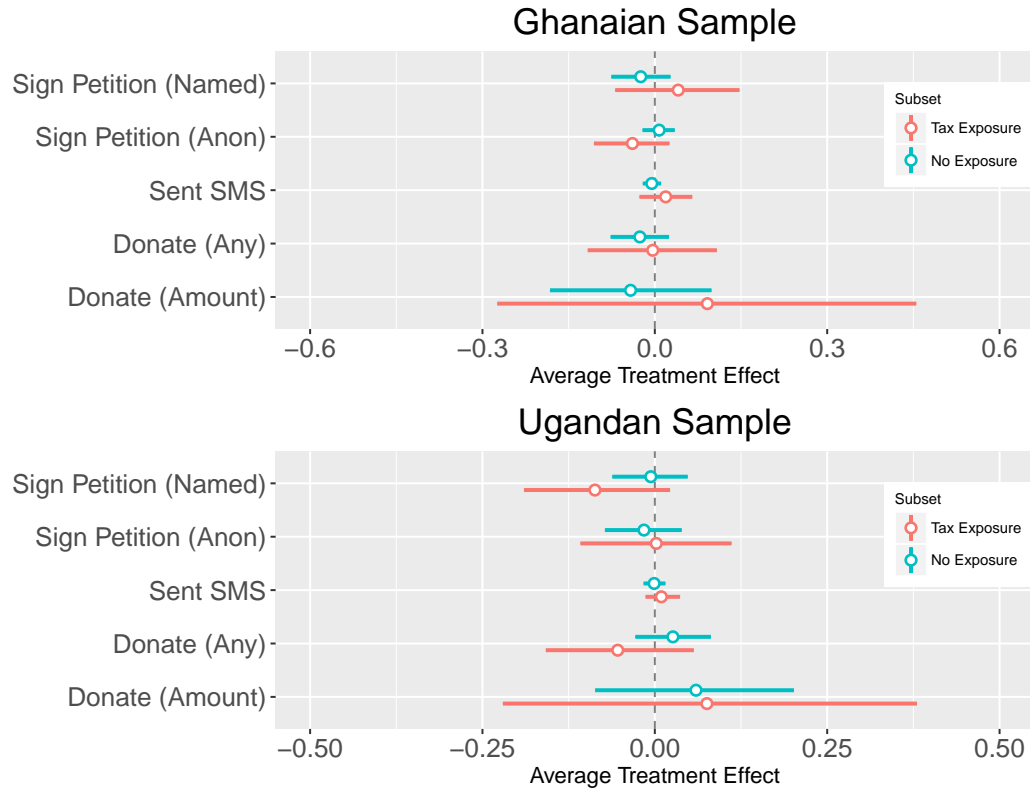
Figure D.8: High-Type vs. Low-Type Respondents, Tax-Aid. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.
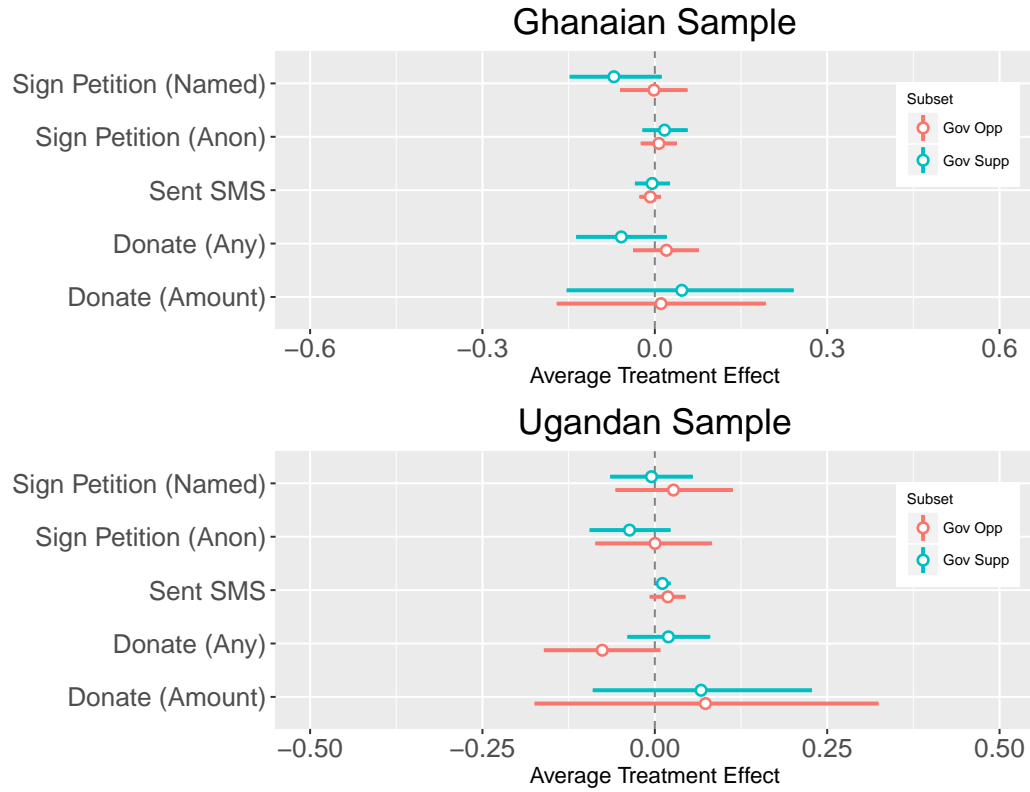
Figure D.9: Gov Supporters vs. Non-Supporters, Tax-Oil. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.
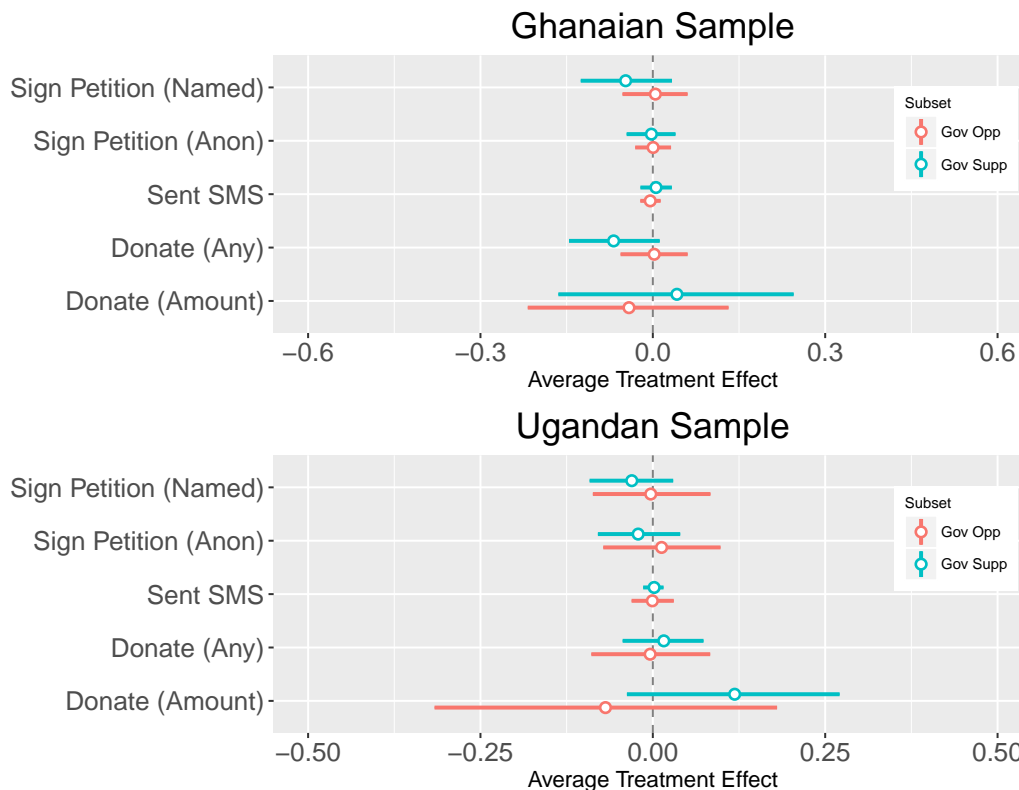
Figure D.10: Gov Supporters vs. Non-Supporters, Tax-Aid. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

E   Survey Language And Design

E.1   Assignment to Treatment

Subjects were randomly assigned to treatment and control conditions in which they were provided with information about government revenue. While simple randomization would not lead to biased estimates in expectation, the presence of non-trivial differences in respondent experience with local government at the constituency-level presented an opportunity to improve the efficiency of the differences estimator through the use of a block randomization algorithm. This algorithm was designed such that, within our primary sampling unit and thus at all higher-level geographic units—including, importantly, the constituency—there was perfect (or, when the number of respondents was not divisible by four, near-perfect) balance between our experimental conditions. The donation measure prompt was as follows:

E.2   Donation Prompt

"There are several organizations in [Ghana/Uganda] that work to make it easier for ordinary [Ghanaians/Ugandans] to see how development funds are spent. At the beginning of the survey, we gave you [6 cedis/1,000 shillings] to compensate you for the time it has taken to answer our questions. Now, we would like to know if you would like to donate to one of those organizations. You may choose to donate to [Action Aid Ghana/Uganda, Transparency International Ghana/Uganda, or IMANI, a research organization that analyzes government budgets, policies and initiatives/a third organization of your choosing]. If you would like to donate, please give me the amount of money you would like to donate and which organization you would like to donate to. If you do donate, your money will be used to help reduce corruption and improve the lives of ordinary Ghanaians/Ugandans."