

No Greater Representation with Taxation: Experimental Evidence from Ghana and Uganda on Citizen Action toward Oil, Aid, and Taxes

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Abstract

Seminal contributions to political economy argue that citizens will more readily engage in political action when they are being taxed, especially compared to when their governments receive nontax revenue from oil and aid. In part this tendency is said to enable the well-known resource curse and its aid equivalent. We perform two substantively identical experiments with behavioral outcomes on nationally representative subject pools in Ghana and Uganda to estimate citizens' likelihood of taking action to monitor spending of taxes compared to foreign aid or oil proceeds. Interestingly, sizable numbers willingly sign petitions and donate money in order to scrutinize both sources of revenue. However, we find that neither Ghanaians nor Ugandans are more likely to take action for tax revenues than for aid or oil. If anything, citizens more readily pay costs to monitor either aid or oil over taxes. Experimental results suggest that the key mechanisms by which taxation might cause representation – perceptions of greater transparency, reduced misappropriation risk, or improved public goods – do not seem to be operating in either Ghana or Uganda, which likely accounts for the lack of differential citizen action across revenue sources.

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1. Introduction

In 2006 the Ugandan government, working with London-based Tullow Oil, announced the discovery of major oil deposits along the shores of Lake Albert. The estimated 3.5 billion barrels of reserves promise to be a major source of nontax revenue for the government, with President Yoweri Museveni multiple times referring to it as “my oil” (Bariyo 2014, Salama 2014). Many worry that the anticipated oil boom will send the East African country, already beset by corruption, poor governance, and political repression, down the road of the “resource curse” that appears to plague many developing countries.

One year later across the continent, the American oil exploration firm Kosmos Energy, under contract from the West African nation of Ghana, announced in 2007 the discovery of an oil field off of the country’s coast containing up to 1.8 billion barrels. The find raised particular concerns that oil would come to curse the most economically developed and stably democratic country in the region. Yet many in the Ghanaian government and society were exultant at the prospect of reducing political pressure from taxes.

From a political economy perspective, natural resource wealth, such as that discovered recently in Uganda and Ghana, undermines good governance. Foreign aid may be similar (Morrison 2009, 2015). Oil and aid move the countries away from the “taxation causes representation” model in which citizen demands for democratization follow from government expropriation of tax revenues. The mechanism is straightforward: citizens feel the loss of tax money from their own pockets and are

therefore willing to pay costs to monitor the revenue and hold officials to account for its spending. Money collected from taxes is thus often assumed to be spent in ways that are less likely to face resource-curse problems because taxes are said to heighten citizen's attention to accountability and therefore make political elites more likely to provide public goods (Bates and Lien 1985, Ross 2001, 2004, Robinson, et al. 2006, Ross 2012, Paler 2013). Hence, it is assumed that there is a differential accountability effect: funds provided by tax revenues are much more likely to be monitored by citizens, which makes politicians more likely to face sanctions if the taxes are misused (Huntington 1991; Ross 2001, 2004).

In contrast to taxes, funds from oil and aid are believed to be “windfall revenues” that enable corruption, undermine governance, foster repression, prolong autocratic rule, and increase conflict (Bräutigam and Knack 2004, Djankov, et al. 2008, Smith 2008, Caselli and Cunningham 2009, Morrison 2009, 2015). Elites benefit from windfalls since they now have access to more funds that can be diverted to corruption and clientelism relatively free from citizen scrutiny. Citizens, on the other hand, suffer from windfalls, as money from nontax sources is said to undermine accountability, generate resources for repression, and buy citizen quiescence toward bad governance (Mahdavi 1970, Beblawi and Luciani 1987, Chaudhry 1997, Waterbury 1998).

If these arguments are correct, citizen demand for transparency and citizens' willingness to undertake costly political action to monitor spending should differ by revenue source. The psychological mechanism underpinning the taxation-causes-representation argument appears to be the endowment effect, well known in social psychology and behavioral economics (Kahneman, et al. 1990). When people feel

ownership of something, they tend to value it more dearly than when they do not possess it. This induces them to pay higher-than-expected costs to retain or recover it (Kahneman, et al. 1990). Presumably, this endowment effect extends to government funding (Sandbu 2006). Indeed, high-quality laboratory-in-the-field experimental evidence suggests that people appear to be significantly more likely to monitor and sanction governments when their endowments are taxed compared to when they benefit from windfalls alone (Paler 2013; Martin 2014). These findings corroborate many studies in social psychology and behavioral economics indicating that owners are willing to pay greater costs to retain or acquire goods than non-owners (Van Dijk and Van Knippenberg 1996; Van Boven et al. 2000; Morewedge et al. 2009).

It is possible, however, that real-world governance settings – as opposed to the controlled environment of the laboratory – may not activate the endowment effect for taxes compared to oil windfalls in the hypothesized manner. At least one of three mechanisms must function for citizens to take action to oversee taxes more than windfall revenues. First, taxation must be transparent; that is, citizens must note that taxes are being withdrawn from their earnings and perceive that they can track the revenue in ways that they cannot for windfalls. Second, taxes must be less susceptible to misappropriation: citizens should perceive a reduced risk that taxes will be hijacked by elites and spent for personal or political gain compared to windfall proceeds. Third, taxes should promote public goods provision; because taxation presumes a public-goods bargain between leaders and the people, elites should direct tax revenues more than windfalls toward welfare provision. If citizens perceive that none of these mechanisms function in their real-world governance settings, then we should not

expect them to be *more* willing to pay costs to monitor taxes compared to windfalls. Even if citizens show a general propensity to monitor government spending, we should not expect *differential* action for one revenue source over the other.

To test the effects of revenue source on citizen action toward monitoring, we enlisted native enumerators to administer substantively identical large-N experiments with attitudinal and behavioral outcomes to nationally representative samples of Ghanaian (n = 3,653) and Ugandan (n = 3,186) citizens. Respondents were randomized into one of four treatment conditions testing the between-subjects effects on attitudes and behavior of taxes compared to oil revenue, aid funds managed by the government, and aid channeled through non-governmental organizations.¹ In each condition, respondents heard a short vignette about government spending. The vignette included information on the amount (held constant) and source (randomly assigned) of additional revenue that will 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) and donate money to their choice of good-government non-profit organizations. Next, subjects were asked a battery of questions probing the hypothesized causal mechanisms that underpin the argument that taxation causes representation. Leveraging the between-subjects design that avoided priming subjects

¹ The introductory consent statement included information about the entity funding the experiment. We worried that giving respondents information about the funding institution would bias their responses. As such, in Uganda we randomized among the conditions: an introduction that mentioned the World Bank, one that mentioned the academic institutions we are affiliated with, and one that mentioned neither but activated a debriefing prompt at the conclusion of the survey. This allowed us to assess whether this bias exists. We found no evidence for this effect.

to think about alternative revenue sources, we asked them how transparent and accountable they believed 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.

By way of preview, Ghanaians and Ugandans are, with few exceptions, no more likely to take political action to monitor or encourage transparency of funds for tax-based revenue than for oil or aid money. Where significant differences do exist, they suggest that, if anything, respondents are less willing to monitor public spending from taxes than oil or aid. The confidence intervals bounding the lack of difference are relatively small, suggesting precise estimation – even with greater statistical power, any statistically significant effect would likely be very small substantively. These findings are robust to numerous alternative specifications, including scrutiny of subgroup effects on the wealthiest and best educated segments of the population (vs. poorest and least educated) and of supporters and opponents of the government..

Citizens' unwillingness to take greater action to monitor taxes appears to result from taxation's failure to activate any of the hypothesized causal mechanisms differently than oil revenue or foreign aid. Ghanaians and Ugandans see few significant differences among sources of government revenue with respect to their accountability and transparency, ability to be misappropriated by elites, or likelihood of being spent according to citizen preferences for public goods. We emphasize that the evidence presented here cannot rule out alternative mechanisms that might lead to the resource curse or to governance problems due to aid windfalls, such as Dutch disease, government capture by special interests, or the provocation of violent conflict, among others. However, the findings in two very different African countries are informative

about the prominent argument that citizens will more likely take action to monitor taxes compared to windfalls. The evidence suggests that the conditions necessary for endowment-type effects that would lead to greater accountability for tax-based spending are not operative.

The 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 randomly assigned revenue conditions. These findings should help to provoke a rethinking of the “taxation causes representation” argument and its corollary that nontax revenue causes citizen quiescence through the causal channel that the different revenue sources provoke different citizen behavior. In what follows we situate the study in the relevant literature, develop hypotheses, describe the research design, and present the data and findings.

2. Theory and Existing Work

When leaders of developing countries structure their economies around the extraction of export-based natural resources, such as crude oil or precious minerals, many have argued that a series of negative economic and political consequences follow (Ross 1999, 2001, Mehlum, et al. 2006, Collier 2007, Humphreys, et al. 2007). Several mechanisms may be in play. Resource-rich economies may contract Dutch disease in which the strengthened currency from booming commodity exports undermines local

production (Cordon and Neary 1982). Special interests may indulge in runaway rent-seeking (Tornell and Lake 1999). And violence may result from armed competition over the prize (Humphreys 2005).

Importantly for this study, the literature also holds that citizens are significantly less motivated to monitor and sanction the mismanagement of natural resource windfalls compared to taxes they pay out of their own pockets (Ross 2001, 2004, 2012; Robinson, et al. 2006; Morrison 2015). This phenomenon, at least in part, is believed to enable the “resource curse” 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 and investment in human capital decreases (Bulte, et al. 2005, Dunning 2005). The value of public office suddenly spikes, causing leaders to increase internal security and spend more to buy public support (Caselli and Cunningham 2009; Robinson, Torvik, and Verdier 2006; Ross 2001). Thus, such governments often become more autocratic and more prone to corruption and clientelism (Collier and Hoeffler 2005, Robinson, et al. 2006).

Foreign aid may be similar. 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, Humphreys 2005, Djankov, et al. 2008, Smith 2008, Morrison 2009). 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). Corruption and clientelism are also associated with foreign aid, as government officials have been known to use foreign funds for political and personal gain (Svensson 2000, Knack 2001). These and other problems with the receipt of foreign aid appear to parallel the problems of over-reliance on resource exploitation. Critically, natural resource and aid windfalls relieve leaders of the need to finance government through taxation (Beblawi and Luciani 1987; Mahdavi 1970).

As potentially important as the other causal pathways may be, it is this last mechanism that draws our attention in this study. It intersects one of the most important ideas of political economy, namely that taxation causes representation. The intellectual history of the idea runs through Joseph Schumpeter, Barrington Moore, Samuel Huntington, Douglass North and Barry Weingast, to Charles Tilly, Robert Bates, Michael Ross, and numerous others, forming a bulwark of democratic theory. Its essence was perhaps first and 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 relatively 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 is more able to flee despots, so leaders are forced to bargain with citizens in an exchange of public goods for tax payments. Given their credibility problems in enticing creditors to finance their militaries or in persuading citizens to pony up taxes,

successful leaders offered a say in government in return, thus tying their hands against future coercion. The ability to raise revenues to repay sovereign debts concomitantly requires the enlistment of citizen representatives who know where the gold is buried in order to levy taxes effectively. These mechanisms thus connect taxation to democratization. This is the basic argument articulated in various forms by Schumpeter (1918), North and Weingast (1989), Tilly (1990), and Bates (2009). It is similar to Barrington Moore's (1966) argument that only after a broad base of bourgeois taxpayers becomes sufficiently invested in government can stable democracy thrive.

As Bates and Lien (1985, 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) 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" (307, cited in Martin 2014).

The inverse of taxation causes representation thus logically follows budgetary windfalls from natural resources and foreign aid: if citizens are not taxed, they will not demand a say in government. As Paler (2013) states, quoting Huntington (1991), "By now it is almost a truism in political science that the 'lower the level of taxation, the less reason for the public to demand representation'" (Huntington 1991, 65). Hence follows the strong suspicion that windfall revenues are problematic at the micro-foundational level. As Ross (2001, 332) notes, '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.”

Tax revenues are said to increase citizens’ demands for information and government accountability and to make elites less able to divert resources to clientelism or corruption (Mahdavi 1970, Beblawi and Luciani 1987, Chaudhry 1997, Waterbury 1998, Ross 2004, Collier and Hoeffler 2005, Devarajan, et al. 2011). Moreover, higher taxes should increase scrutiny of governments and elicit more active citizens and more accountable elites; hence to the extent that windfall revenues make taxes less onerous than they otherwise would be, they will have the ascribed negative effects. Of course, it may also be the case that taxation leads not to representation but to angry citizens bent on overthrowing the regime, even if the government is democratic. Taxation may therefore not lead to democratization so much as regime change (Morrison 2015).

However, some scholars have questioned the inevitability of the resource curse and claimed that the effects of resources on governance are contingent on other factors (Jensen and Wantchekon 2004, Dunning 2005, Haber and Menaldo 2011).² This debate remains unresolved even in the context of large-N country-year studies. As Ahmadov (2013) points out, examining “29 such studies that in total report 246 empirical estimates of the impact of resources, they...range from negative through no association

²Alexeev and Conrad in a recent econometric study (2009) even claim that natural resources like oil are strongly positive for economic growth and have no negative effect on political institutions.

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 (Moss et al. 2008, Djankov et al. 2008, Busse and Gröning 2009, Finkel et al. 2007, Tavares 2003, Knack 2004, Wright 2009, Bueno de Mesquita and Smith 2013).

It is important to note that if weak, developing-country governments are just as unaccountable for tax spending as for all other revenue types in the eyes of citizens, or citizens are equally unable to learn about any centralized form of spending regardless of its source, then we must reconsider whether this mechanism leads directly to the resource curse. All of these arguments presume that citizens know when they are being taxed and can follow the money from their pockets to government action.

Indeed, the psychological process at the heart of the taxation-causes-representation argument is the endowment effect (Kahneman, Knetch and Thaler 1990).³ In a social psychology-behavioral economics collaboration, Kahneman, Knetsch and Thaler found that subjects tended to overvalue objects (such as coffee mugs) that were in their possession compared to what disinterested subjects would offer in an open market. This, of course, violates expectations of rational action but nevertheless

has been replicated reliably in subsequent research. This has obvious implications for taxpaying. As Sandbu (2006, cited in Paler 2013) states,

“Tax payments are generally perceived as a cost that people have to pay out of their earnings, and so people have an incentive to hold the government accountable for how it spends their money. Natural resource wealth that is wasted or stolen, in contrast, is more likely to be perceived as a foregone gain, since it has never passed through the hands of the population and therefore has never been ‘earned’ or ‘possessed.’ The endowment effect implies that the motivation to hold the government accountable is less strong in the case of natural resource revenue than in the case of taxes.”

The endowment effect is closely related conceptually to the notion of loss aversion, a dynamic in which utility losses weigh significantly more than equivalent gains in people’s minds (Kahneman and Tversky 1979, Tversky and Kahneman 1991, though see also Morewedge et al. 2009).

The findings from experiments testing what Kahneman and Tversky call “prospect theory” are some of the most well-established in behavioral economics. Much of the evidence in favor of the endowment effect or loss aversion as the mechanism driving the taxation-causes-representation argument comes from highly controlled lab experiments in which citizens are exposed (sometimes more than once) to strong treatments that often include information, behavioral games, or both (see e.g. Paler 2013 and Martin 2014).⁴ While research of this kind is critical in uncovering what

⁴ Another issue concerns who pays what taxes. In many poor developing countries, few people pay any taxes. Moreover, a substantial part of the tax base may be paid indirectly.

mechanisms may be at play when citizens evaluate how (or whether) to monitor and sanction officials for financial mismanagement, these interventions, like most lab experiments, take place in artificial environments in which study conditions can be tightly controlled. This improves internal validity but often comes at the expense of external validity, or the ability to generalize from the findings to other populations and settings (see McDermott 2011). These studies are important in providing baseline expectations for behavior when the conditions (both implicit and explicit) of the study are met.

We caution, however, against generalizing too much from the laboratory findings to broader populations, particularly in the developing world, where geographical isolation, high levels of poverty, low levels of labor mobility, and poor communication infrastructure are the norm rather than the exception. Less educated, rural respondents – which, it bears noting, are a large proportion of total population in many less urbanized, developing countries – are unlikely to possess much meaningful information about budgets or government spending processes, much less the detailed information necessary to distinguish between different sources of revenue.

To the extent people may learn or hear about the absolute or relative value of a revenue source, whether it be taxes, oil or aid, it is likely to occur in a context in which action may be costly and its potential benefit unclear. Thus, in designing our intervention, we sought to craft a treatment that would resemble the kind of exposure about government revenue sources citizens are more likely to receive, and to offer them behavioral choices that are also closer to the kinds of actions they might reasonably take on political issues. Our goal in doing so is to bring greater realism or ecological

validity to the literature on the micro-foundations of the resource curse, and to complement the growing body of research that utilizes lab or lab-in-the-field experiments with a treatment that more closely approximates one citizens are likely to experience on a day-to-day basis.

3. Causal Mechanisms

For the taxation-causes-representation argument to function through the channel of citizen action, at least one of three different mechanisms must be in play. First, citizens must believe they can track the spending of tax revenue differently and/or that politicians are more accountable for taxes compared to resource or aid windfalls. Second, citizens must perceive that elites can less readily misappropriate taxes than natural-resource windfalls for personal or political gain. And third, citizens must judge that leaders will use taxes more than windfalls to provide public goods.

In terms of transparency and accountability, the bargain described by Bates and Lien (1985) referenced above requires that citizens know when they are being taxed and can follow the money from their pockets to the production of public goods. 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. It may seem obvious that citizens know when they are being taxed, but indirect taxes such as sales and value-added taxes may effectively confound taxes with the prices citizens pay on goods in formal transactions. And democracy may therefore

paradoxically be associated an increase in such obfuscated and regressive taxation (Timmons 2010). Critically, indirect taxes may not provoke the endowment effect in the same way as income taxes and therefore may fail to induce citizen scrutiny and sanctions (Paler 2013, 722). Indeed, both the Paler (2013) and Martin (2014) lab experiments give subjects money and then randomly assign subjects to pay taxes or not, which should invoke the endowment effect in a way that the indirect taxes of most developing countries may not.

As Manin et al. (1999, p.40) explain, accountability implies that citizens can “discern whether governments are acting in their interest and sanction them appropriately.” Our study examines whether the public will devote resources to monitor how a given revenue source is used by the government. Accountability requires that political leaders and publics believe that citizens will monitor and punish leaders if they misuse public funds, and the endowment effect implies that citizens will be especially interested in scrutinizing the money that came out of their own pockets in the form of taxes. Therefore, citizens should be more likely choose to monitor the revenue sources based, at least plausibly and in part, on perceptions of revenue transparency.

Turning to the risk of misappropriation, the taxation-causes-representation 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 resource windfalls should be significantly greater than for taxes. This is conceptually related to what Ross (2001) calls the “‘spending effect:’ oil wealth may lead to greater spending on patronage, which in turn dampens latent pressures for democratization” (333). The spending effect holds that leaders can more readily divert

resource windfalls to political ends than tax revenues. Resource wealth and foreign aid both increase the size of government budgets and, critically, enable less budgetary constraint, presumably due to lower citizen scrutiny (Morrison 2009, 2015).

Because the windfall revenues are not expropriated from citizens' earnings, individuals should be less inclined to monitor natural resource proceeds or aid money. After all, why monitor what you do not have to pay for? Indeed, windfalls may enable governments to reduce tax burdens at the same time as they improve public goods (Ross 2001; Morrison 2009, 2015). 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.

In terms of public-goods provision, again as argued in the taxation-causes-representation literature, 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, Ross 2004). Indeed, Ross (2004) holds that citizens will only tolerate taxes if the public revenue produces actual public goods. And his econometric analysis isolates the positive effects of taxation on democracy to just such circumstances: taxes lead to democracy only if they also improve public services. 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, 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.

4. Experimental Design

The experimental design and data analysis plan was registered with EGAP prior to researcher access to the outcome data. For this experiment, we drew samples of Ghanaian ($n = 3,653$) and Ugandan citizens ($n = 3,186$) that are nationally representative in most respects except that we over-sampled districts that are nearest the sites of oil exploration. The selection of the two countries was done to maximize differences in political system, economic outlook, and experience with different sources of revenue 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 (Polity IV 2014, Freedom House 2014). 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, *World Development Indicators*). Aid comprises roughly 16 percent of Ghana's annual budget compared to aid's 86 percent share of Ugandan's government expenditure (Tierney et al. 2011). And, importantly, Ghana has been receiving revenues from oil since 2010 but Uganda's oil is not yet flowing. Consistent results across the two disparate settings should increase the study's claims on external validity.

In interviews with the samples of Ghanaian and Ugandan citizens, we presented a randomly assigned statement about the source of significant public funds and then asked subjects a series of questions about what they think the effects of the funds will be and where they think these funds should be spent. These revenue streams were all

plausible future budget sources given publicly available information and thus no deception was used in the experiment.

The survey first asked questions covering a wide array of standard demographic characteristics. These included ethnicity, age, gender, education, employment, income, quality of life, access to public services, government effectiveness, media consumption, political knowledge, political interest, political activity, political trust, party affiliation, perceptions of corruption, and perceptions of clientelism.

After the battery of demographic questions, we randomly assigned subjects to receive a statement about revenues from one of four sources: oil receipts, domestic taxes, aid flows through government, or aid flows through NGOs. Randomization of treatment assignment allows us to uncover systematic differences in subject responses across conditions. We incentivized respondents to take the survey giving them either 6 Ghanaian cedis or 1,000 Ugandan shillings at the start; in part we did this so we could ask them to donate (parts of these) sums to NGOs as a behavioral outcome at the end. We tested the effects of the information 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.

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, 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 permuted-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 treatment conditions are as follows (with differences highlighted in boldface):

Oil Condition: “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]. 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].”

Tax Condition: “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 **taxes on wages and purchases that will be paid by all [Ghanaians/Ugandans]. 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].”

Aid Condition: “As part of this survey, we are also providing important information to Ugandans about finances in Uganda. In next few years, **government agencies** of Uganda will receive at least five trillion shillings. This money will come from **aid given by foreign governments. This money will become part of the Ugandan government budget. Lawmakers and the President** are supposed to use the money to improve the lives of Ugandans.”

NGO Condition: “As part of this survey, we are also providing important information to Ugandans about finances in Uganda. In next few years, **non-governmental organizations** of Uganda will receive at least five trillion shillings. **This money will come from foreign governments and go directly to non-governmental organizations, not to the central government.** Non-governmental organizations are supposed to use the money to improve the lives of Ugandans.

5. Survey and Key Outcomes of Interest

Following the experimental condition text, subjects were asked a series of questions about how transparent spending financed by the new revenue source was likely to be; 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); and whether subjects would be willing to pay taxes to finance a transparency agency to monitor the new spending or to contact local or national elected officials in the event the new revenue was misused.

Subjects were then given the opportunity to voice their support for a proposal to create an independent agency to track the new revenue source, and allowed to sign a petition either anonymously or by in their actual name that would later be sent to their constituency MP informing him or her of 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 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

answered affirmatively and zero otherwise. The prompt for the donation measure was as follows:

“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.”

After the money was donated, enumerators recorded the amount in the survey software. In addition to spot-checks, field managers verified the amount of money given out each night and confirmed that it matched reported totals.

The behavioral measures were designed with two principles in mind. First, in order to make sure that 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 but little more, 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 and frugally (all subjects were later reimbursed for the costs of their texts but this intent was not conveyed

during the survey). Finally, with the ability to pocket up to 6 cedis or 1,000 shillings by refusing to donate altogether, 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. It is also worth noting that the sum of 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 in both samples.

6. Data and Method

The sample contains a total of 3,653 observations in Ghana and 3,186 in Uganda, which were both collected using an area-probability sample designed to achieve national representativeness. Data collection in Uganda began in May of 2014 and ended in June 2014. In Ghana the study began in March of 2015 and concluded in April of 2015.

To enhance the validity of our estimates, we blocked treatment assignment on the primary sampling unit. In a randomization inference framework, the practical effect of blocking regardless of the estimator 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 with which treatment and control groups are unbalanced along key covariates. The selection of the primary sampling unit (PSU) as

the block exploits 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. Blocking at the PSU increases the probability with which our sample is balanced with respect to a range of macro-level political variables that might affect political behavior. Examples include the partisanship and quality of the respondent's Constituency Member of Parliament (MP), the amount of political corruption, the collection (or lack thereof) of local taxes, and the responsiveness of local politicians.

There are also advantages to blocking in the estimation of treatment effects, where the inclusion of block fixed-effects has been shown to increase efficiency. However, because of the well-known bias of the fixed effects in experimental settings (see. e.g. Aronow and Middleton 2011; Freedman 2008), we focus here on the unadjusted results, though our results are substantively unchanged even under adjustment.⁵ We report results below for all subjects, which captures the effects of the intent to treat. To test robustness, we also report results for the subgroup of subjects who passed the manipulation check verifying that the experimental condition was

⁵ Ideally, we would also explore the extent to which treatment effects vary by our blocking unit, the constituency. However, while investigating the reason for between-constituency differences—to the extent they exist at all—is the subject of ongoing work, they are of secondary importance for two reasons. The first is a matter of data availability: constituency-level data of interest, such as variables measuring respondents' experience with corruption, the strength and tenure of the ruling party, and ethnic composition are not widely available. The second is substantive: while between-constituency differences may be of interest to those studying the resource curse, the differential willingness of citizens to take costly action to monitor and sanction elected officials depending on the revenue source is ultimately a local phenomenon. As such, our primary focus here is discovering whether accountability pressures from *within* the constituency would differ by revenue source.

received. We note here 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. Because the treatment was a vignette rather than a more intense intervention, we crafted the manipulation check to be especially difficult: the test prompted respondents to recall the source of the new government revenue, and 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.

In terms of covariate balance, as expected, the modified permuted-block randomization algorithm was successful in randomizing respondents into equally sized treatment groups within our primary sampling unit, the polling station. Because any imbalance on measured pre-treatment covariates can be adjusted for using regression methods, conventional balance tests (despite their frequency) are ultimately of little value (Imai, King and Stuart 2008). However, given that covariate adjustment via regression has been shown to produce bias of unknown direction (but of small magnitude in large samples) under the Neyman-Rubin Causal Model (Freedman 2008), we present in Table 1 a series of conventional difference-in-means tests for a range of covariates that could predict how likely a respondent is to take, or express the desire to take, political action to monitor and sanction officials for mismanagement of government revenue. As is clear from Table 1, there is no significant imbalance on key covariates in the Oil-Tax and Aid-Tax comparisons.

[Table 1 Here]

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. First, and perhaps most importantly, 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, 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.

As Keele, et al. (2012, p. 486) note, one of the principal advantages of this approach is that the resulting test of significance is an intuitive quantity, one that is often precisely what the political science experimenter seeks to approximate with traditional parametric tests: a p -value that represents "the probability that the result observed among his or her specific set of experimental subjects can be explained away by the chance constitution of the treatment groups under one allocation of treatment."

While our sampling strategy was designed to be nationally representative (and thus to maximize external validity), this so-called *exact* test ensures the internal validity of reported estimates for any sample, even one generated non-randomly, without recourse to a parametric distribution.⁶ Although the flexibility in choosing a substantively meaningful test statistic is another advantage of randomization inference, we focus here on a traditional test: that of a difference-in-means between the reference and comparison groups.⁷

7. Analysis

In terms of descriptive statistics, we note that subjects were, on average, willing to undertake political actions to promote budget transparency and monitor government spending. 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, 48.0 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)

⁶ While not relevant for the present study, randomization inference is also assumption-free with respect to the behavior of treated units, and does not require what, after Rubin (1986), is often called the "stable unit-treatment value assumption" (SUTVA), or non-interference between units. This assumption is often violated in practice and is especially untenable the more intense the treatment becomes.

⁷ We also conducted the analysis with the Horvitz-Thompson estimator. The results were substantively and statistically unchanged.

cedis and 386 (of 1,000) shillings, which both constituted sizable shares of their windfalls for participating in the study.

But do we see differences in citizens' willingness to take action to monitor and sanction leaders across the revenue sources? We asked citizens to undertake various costly actions, and thus our measure reflects the nature and intensity of their preferences. Figure 2 reports 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 oil or taxes, oil, aid through government or aid through NGOs (*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*). All treatment effects are shown in standard deviation units, with 90-percent confidence intervals shown with error bars and 95-percent confidence intervals shown with spikes. As is clear from the figure, there are few meaningful differences between the tax and oil conditions on the outcomes, with the sole exception that Ugandan subjects were significantly ($p = .087$) more likely to sign the anonymous petition in the oil condition compared to the tax condition. This result provides evidence against the taxation causes representation argument, though only for the least costly behavioral outcome. But this difference does not survive a multiple-testing correction by controlling the false discovery rate (FDR) at an alpha of 0.05. 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. This is true for both Ghana and Uganda.⁸

[Figure 1 Here]

We also included several measures designed to capture respondents' hypothetical willingness to both monitor the treatment source as well as sanction its misuse. Specifically, we asked respondents whether they would like an independent monitoring agency to be created to monitor spending from the new 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). Figure 2 visualizes the treatment effects on these measures, with ATEs again representing positive treatment effects for respondents receiving the tax treatment. Like the results for our behavioral measures, we see little differential willingness for respondents in the tax condition relative to those in the oil, aid, or NGO conditions for either Ugandan or Ghanaian subjects. Again, there is a single exception: Ugandan subjects proved significantly ($p = .057$) more willing to contact their local councilperson in the NGO treatment compared to the tax condition. Once more this

⁸ Due to the large number of tests, we also implement multiple-testing corrections by controlling the false discovery rate (FDR) at an alpha of 0.05 for all individual tests. The effect of the FDR correction is negligible given the strongly null results. We note those places where results survive the correction.

evidence goes against the taxation-causes-representation argument. But yet again the result does not survive the multiple testing correction.

[Figure 2 Here]

8. Robustness Checks

We also conduct two additional sets of analyses as robustness checks. First, while enumerators were trained to deliver the vignette with special emphasis on the source of the revenue, not all respondents passed the post-treatment manipulation check. Thus, despite having received the treatment vignette, these respondents have not been “treated” insofar as they may not have adequately understood the critical piece of the vignette: the source of the additional revenues. This may be particularly true for poor, rural respondents, who may have limited knowledge of budgetary processes and limited information about government spending.

To account for the possibility that non-compliers are driving the intent-to-treat estimates downwards towards a null result, we also 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 and several minutes after the intervention, passage rates were relatively high – averaging 70 percent across conditions and countries. Alas, subjects failed the manipulation check significantly more often for the tax condition than for the oil, aid, or NGO conditions. Thus, there were significant selection effects across experimental conditions for

correctly identifying the revenue source, so we report the subgroup analysis here with considerable caution as we weigh alternative estimation procedures going forward.

We 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-representation 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. But this is not what we learn from the analysis. As with the prior findings reported, most results are null with narrow confidence intervals suggesting precise estimation. However, Ghanaian subjects who passed the manipulation check were significantly more likely to donate money to monitor oil ($p = .077$) and aid ($p = .048$) compared to taxes. And Ugandan citizens correctly identifying the revenue source were significantly more likely ($p = .024$) to pay to monitor NGO aid compared to taxes. However, these results again do not survive multiple-testing corrections.

[Figure 3 Here]

Second, despite the general pattern of negative treatment effects for the tax condition, there may be a subset of respondents for whom the endowment effect as conventionally understood operates. Existing work suggests that wealthy, informed, income-tax-paying citizens are going to be most susceptible to the endowment effect. Because they are 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).⁹ We classify these subjects as a "high-type" respondent—that is, as people who were likely to have both the ability and desire to act in the way consistent with what the endowment effect would predict. It may also be that those who believe with a higher probability that tax-based revenue will not be used to benefit them, such those who are supporters of the opposition party, would seek greater transparency and stricter oversight of tax-based expenditures. We examine these two subgroups to see if they can help us understand the differences or lack of them across the revenue sources. We thus estimated ATEs for the identical measures as above for "low-type" and "high-type" respondents. We also 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 responsive to their concerns or more likely to spend revenues according to their preferences.

[Figure 4 Here]

⁹ For Uganda, information on tax-payer status was not available, requiring us to proxy for tax-payers by considering the top quartile of an index measuring wealth. As such, the results for Uganda and Ghana are not directly comparable in this particular instance. However, to the extent that taxpayers are also wealthier, the use of the proxy measure for the Uganda comparison should serve only to make the estimates noisier than they might otherwise be. We thank Marc Beissenger for the suggestion to identify the subset of respondents most likely to exhibit endowment-effect type behavior.

Figure 4 visualizes the results of analysis identical to that in the previous section for each of these subgroups for the Oil-Tax, Aid-Tax, and NGO-Tax comparisons. The dotted lines represent estimates from those respondents who were "low-type" or opposition-party supporters, respectively. Solid lines represent those who were "high-type" or supportive of the governing party. The results suggest no evidence that a positive (that is, endowment-type) effect exists for any set of 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 that we would expect in an endowment-effect framework. Yet controlling the false discovery rate at conventional levels using the Benjamini-Hochberg procedure eliminates even those few results that are marginally significant.

9. Discussion

Broadly speaking, there are several results worth noting. The first is that the reported findings were consistent with what we might expect in cases where respondents see little difference between different revenue sources: there was little greater willingness to take action to increase the likelihood of greater transparency among those who received the tax treatment relative to the oil, aid, and NGO treatments. If anything, subjects were more willing to take action to monitor nontax revenue over taxes. Our results suggest that, as discussed below, regardless of the existence of an endowment-type effect in experimental settings, respondents generally view these different revenue sources as equally opaque, equally corrupt, and equally targeted to special interests. Put more optimistically, respondents 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, just not differently according to revenue source. To the extent there exists any pattern in the data at all, there is some evidence that so-called “high-type” citizens are more likely to pay costs to monitor oil compared to taxes, opposite the predictions of taxation-causes-representation arguments.

It is possible that low levels of generalized willingness to act (or to state such a willingness) are creating floor effects, making the detection of statistically meaningful (but substantively small) differences more difficult than it might be otherwise be. To account for this possibility, we report in Table 2 the mean proportion of respondents who are willing to engage in both actual and hypothetical monitoring or sanctioning across experimental conditions. We find very little evidence for floor effects. In fact, precisely the opposite is the case. Consistent with other work (see e.g. Paler 2013), Table 2 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.

[Table 2 Here]

Our explanation for the null results is straightforward: in environments of low information, high corruption and a small tax-base, there is little reason for respondents to believe that the source of revenue matters. If transparency is low and misappropriation risk high, it will be equally difficult to discover if funds have been

misused (even if such a case is likely) across revenue sources. Likewise, in the presence of high corruption, the source of a revenue stream is unlikely to make a difference in citizens' ability to benefit in purely welfare terms. If this is the case, we should see a combination of high stated and actual willingness to monitor *across* revenue sources, but no differences between them in terms of their transparency, misappropriation risk, or potential benefit. Table 2 demonstrates the first point, that of high baseline willingness.

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 (*Transparent*), 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 (*Rent*). 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 (*List Exp*). Additionally, 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*).

Figure 5 reports the results of basic difference-in-means tests for these measures of transparency, misappropriation risk and potential benefit. Consistent with our expectations, we find that respondents perceive few differences between sources in

terms of transparency, misappropriation risk, or potential welfare benefit. Ugandans thought that their MPs were significantly more likely to know about tax money compared to the anticipated oil proceeds ($p = .036$) and aid through NGOs ($p = .015$), but these results again are not robust to multiple testing corrections. On the other hand, Ghanaians perceived a significantly greater probability ($p = .092$) that tax revenues would be misappropriated compared to aid money received by the government, and Ugandans thought taxes were significantly more likely ($p = .004$) to be misappropriated than aid through NGOs. The latter result does survive the multiple-testing correction.

Thus, there is little evidence supporting any of the three posited mechanisms underlying the taxation-causes-representation argument through the channel of citizen action. Figure 6 reports the same tests for the subgroup of subjects passing the manipulation check in which some of the results strengthen, especially regarding the lower misappropriation risk of aid through NGOs, but most others attenuate. All told, we find little evidence that citizens are more inclined to monitor taxes over nontax revenue, nor do the results suggest that any of the three posited mechanisms are operating as expected.

[Figure 5 Here]

[Figure 6 Here]

Yet, it is worth noting that the general lack of differences is not a function of a general 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, in addition to the mean, is nearly invariant across treatments. Moreover, disaggregating the risk 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.

10. 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 and why citizens may choose to monitor some sources of spending and not others. While existing literature demonstrates that a unique feature of taxation—the expropriation of earned income after the fact—can produce stronger incentives to hold politicians accountable for tax-based spending, we sought to understand whether other factors might lead to a preference for monitoring other revenue sources in a setting that more closely approximated the information stream and political actions available to most sub-Saharan Africans compared to controlled laboratory settings.

Emphasizing the mechanisms of transparency and misappropriation risk of existing streams and the potential benefits of different revenue sources, we conducted

two large survey experiments to determine whether differential beliefs over these features of spending sources might, in fact, lead to greater willingness to enforce greater transparency for non-tax forms of revenue, including oil and aid, that are often captured by elites. In general, our results suggest that where citizens do not perceive greater transparency, misappropriation risk, or propensity toward public goods for one source over another, it is not necessarily the case that they should prefer to monitor tax expenditures.

The extent to which this is driven by a feature common to many developing countries—the heavy use of the value-added tax and lesser reliance on income taxes for government revenue—is beyond the scope of this study, but may prove a fruitful area for future research. It may indeed be that the combination of low priors on government efficacy and widespread belief in systemic corruption means that citizens are equally pessimistic about the extent to which *any* revenue that passes through the central government will be well-spent. Where this is the case, it is difficult to see why non-tax sources of revenue should be any more likely to contribute to poor economic and political performance. Also notably, despite significant contextual differences between Uganda and Ghana, we do not find many differences in our results. Ghana is significantly richer, more urban, and more democratic than Uganda. But in neither country do tax revenues seem to prompt citizens to demand or pay the costs for greater monitoring and accountability. Our data about the two countries increases confidence in our null results about the differential willingness to monitor of taxes compared to windfalls.

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	Ghana			Uganda		
	Aid-Tax	Oil-Tax	NGO-Tax	Aid-Tax	Oil-Tax	NGO-Tax
Covariate	P-Val	P-Val	P-Val	P-Val	P-Val	P-Val
Age	0.638	0.396	0.971	0.468	0.489	0.904
Employed	0.379	0.543	0.169	0.839	0.454	0.427
Female	0.789	0.621	0.304	0.583	0.866	0.840
Rural	0.587	0.905	0.979	0.899	0.898	0.851
Education	0.098	0.753	0.871	0.378	0.630	0.132
Coethnic Pres	0.464	0.479	0.545	0.691	0.619	0.810
Trust Pres	0.777	0.889	0.755	0.513	0.795	0.185
Gov Supporter	0.559	0.515	0.698	0.802	0.903	0.819
Held Pol Position	0.467	0.807	0.956	0.207	0.469	0.240
Voted	0.236	0.804	0.579	0.082	0.042	0.065
Gov Corrupt	0.311	0.223	0.060	0.811	0.949	0.939

Table 1: 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.

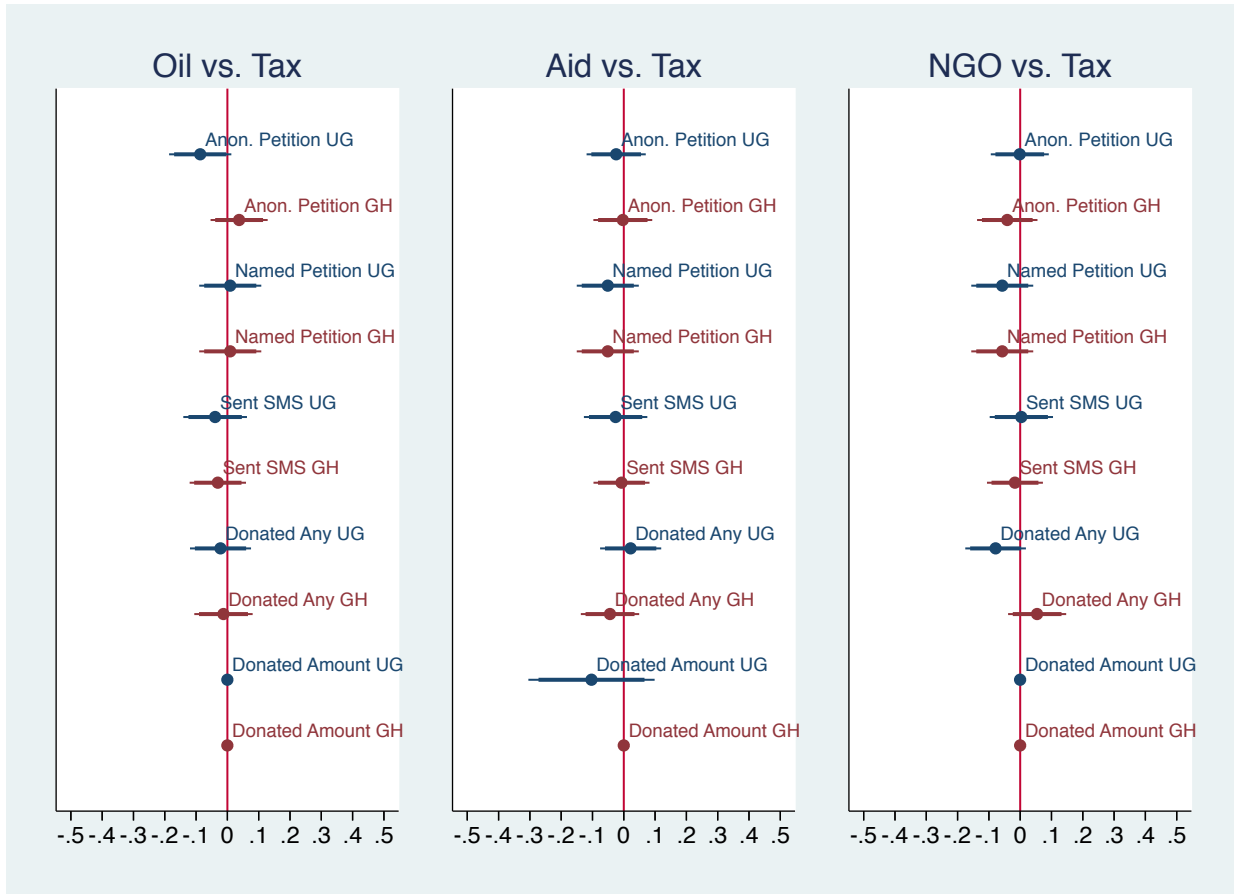


Figure 1: Main Treatment Effects on Behavioral Outcomes. Standardized effects shown for each behavioral outcome across experimental conditions. Confidence intervals are shown with error bars (90%) and spikes (95%).



Figure 2: Main Treatment Effects on Willingness Measures. Standardized effects shown for each behavioral outcome across experimental conditions. Confidence intervals are shown with error bars (90%) and spikes (95%).

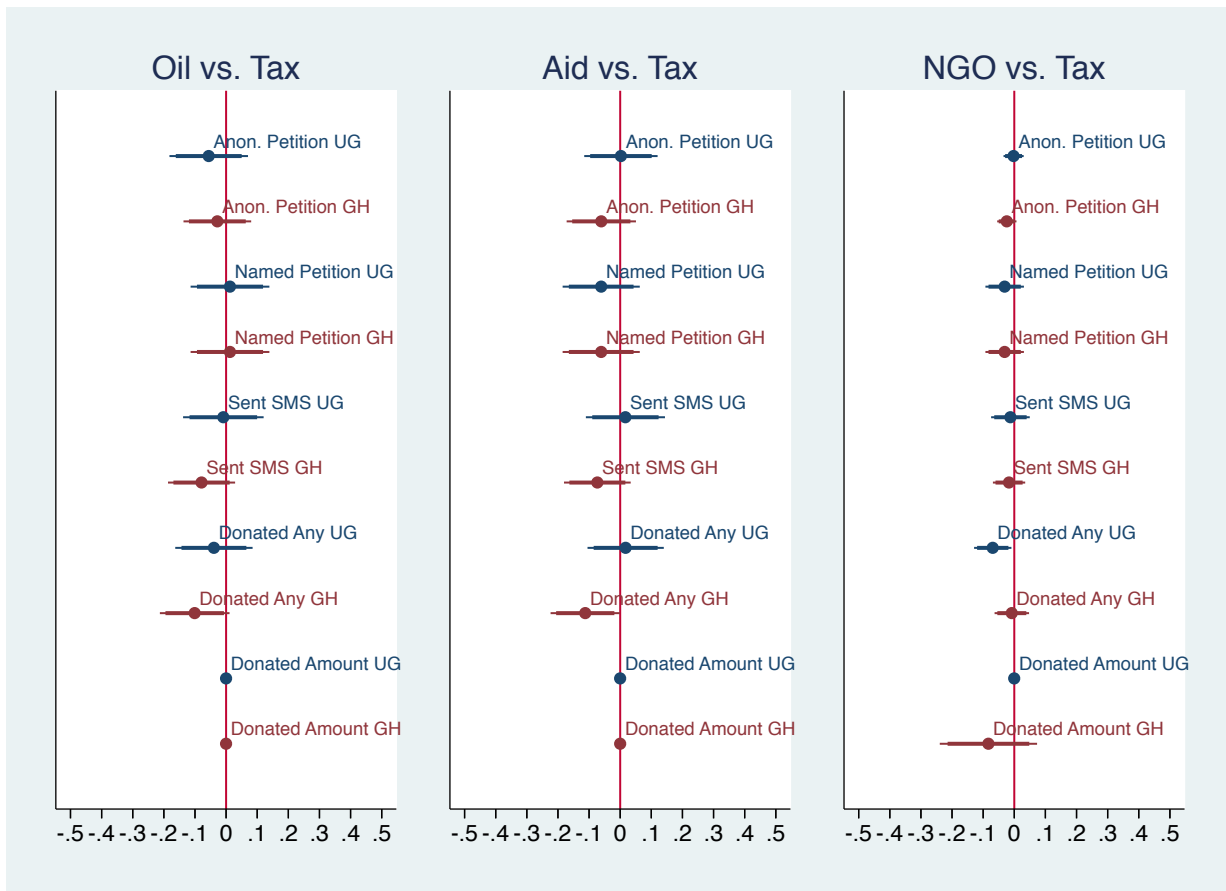
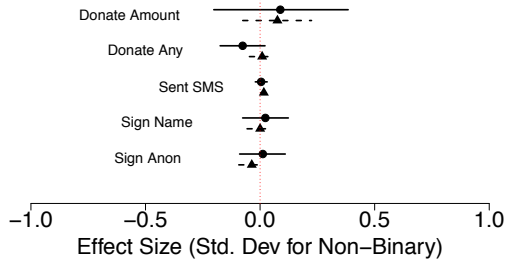


Figure 3: Treatment Effects for Subjects Passing Manipulation Check. Standardized effects shown for each behavioral outcome across experimental conditions. Confidence intervals are shown with error bars (90%) and spikes (95%).

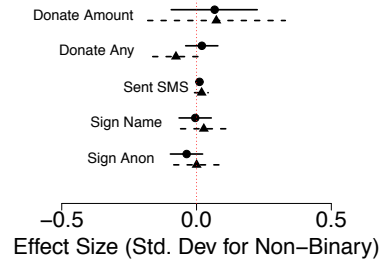
Oil vs. Tax

Ugandan Respondents

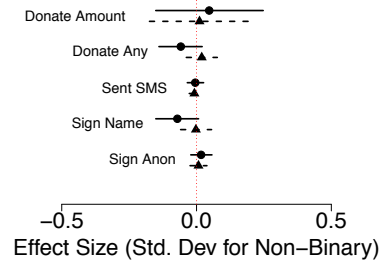
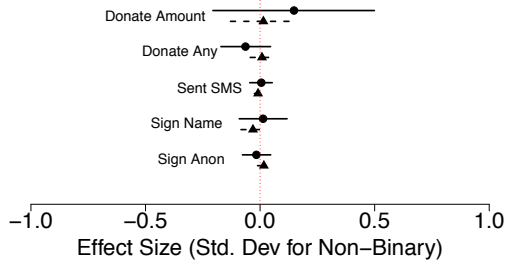
High vs. Low Type Respondents



Gov Supporters vs. Opponents



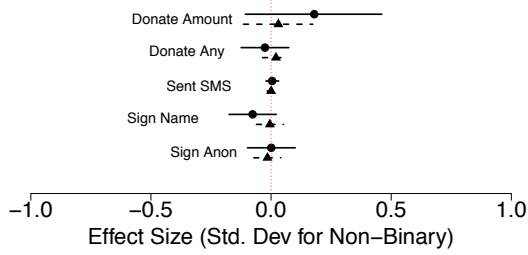
Ghanaian Respondents



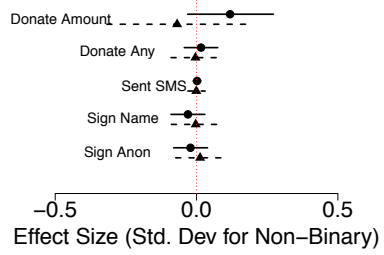
Aid vs. Tax

Ugandan Respondents

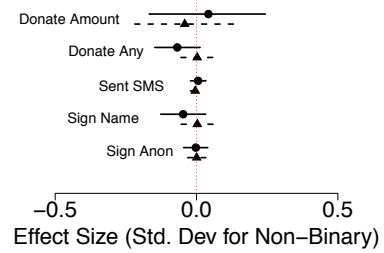
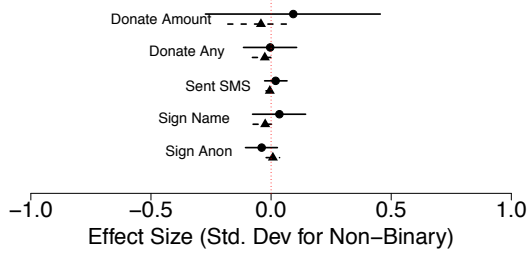
High vs. Low Type Respondents



Gov Supporters vs. Opponents



Ghanaian Respondents



NGO vs. Tax

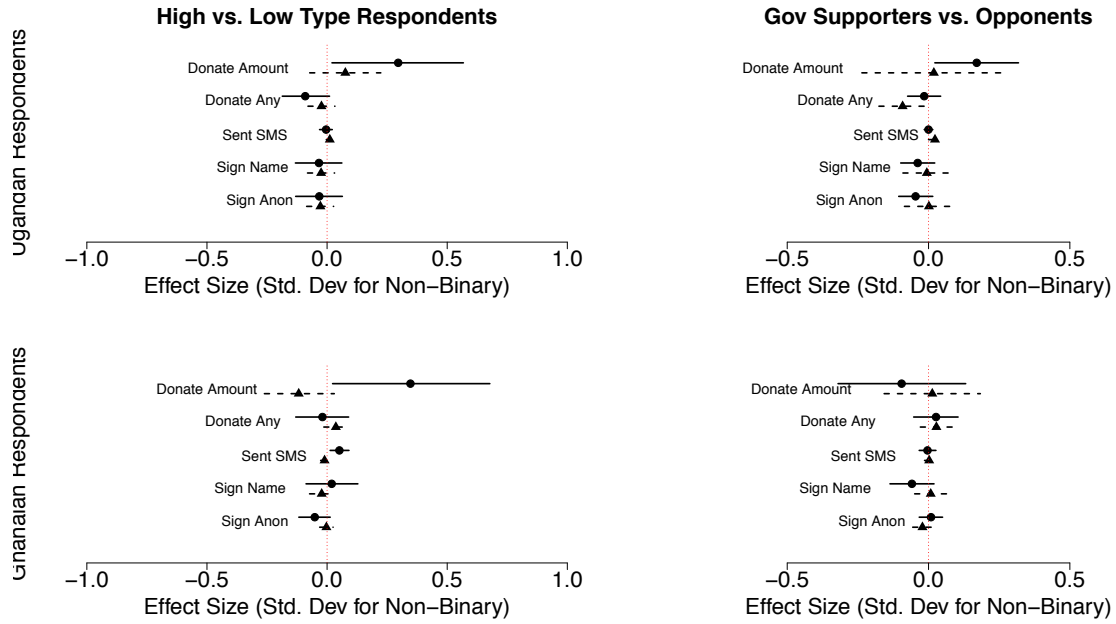


Figure 4: Heterogeneous Treatment Effects. Solid lines represent “high-type” and respondents who self-identified as supportive of the governing party, while dashed lines represent “low-type” and opposition supporters respectively. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

	Ghana		Uganda	
<i>N</i>	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	0.480	1833	0.575
Mean Amount Donated	2.10	Cedis	385.7	Shillings
Likely or Very Likely to Contact Village Elder	1628	0.460	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	0.350	1259	0.402

Table 2: Frequencies and Proportions of Subjects Taking Action and Expressing Willingness to Take Action to Monitor Revenue.



Figure 5: Main Treatment Effects on Mechanisms. Standardized effects shown for each outcome across experimental conditions. Confidence intervals are shown with error bars (90%) and spikes (95%).



Figure 6: Treatment Effects on Mechanisms for Subjects Passing Manipulation Check. Standardized effects shown for each outcome across experimental conditions. Confidence intervals are shown with error bars (90%) and spikes (95%).