

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

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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 nontax 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 costly political action to monitor spending should differ by revenue source. 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 taxation-causes-accountability and windfalls-cause-quiescence arguments are especially important in contemporary development de-

bates, 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. Respondents were randomized into one of three treatment conditions testing the between-subjects effects on attitudes and behavior of citizens toward taxes compared to oil revenue and aid funds managed by the government. 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 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 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. No indirect effects appear either 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 tentatively suggest that these results are due to the institutional environment in poor, developing countries. These findings should help to provoke a rethinking of the taxation-causes-accountability argument

in developing-country contexts, as well as its corollary that nontax 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, Michael Ross, and numerous others, forming an important foundational concept in theories of democratization. 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 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."

The lack of accountability in resource- and aid-rich states follows logically from these arguments: if citizens are not taxed, they will not insist on a say in government. Huntington 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" ([Huntington 1991, p. 65](#)).

As 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.”

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, 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 (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 governments 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).¹

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). But we emphasize that the promi-

¹Collier (2006) argues that aid revenues are more accountable than oil revenues, in part because donors pursue “governance conditionality”.

ment arguments reviewed above by Schumpeter, Huntington, North, Weingast, Bates, Tilly, 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 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 the effects of resources on governance are contingent on other factors (Dunning 2005, Haber and Menaldo 2011, Jensen and Wantchekon 2004).² 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, 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

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

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.

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 \(2001, p. 333\)](#) 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.

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, pp. 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.

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.

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. 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. 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 Uganda's government expenditure (Tierney et al. 2011). And, importantly, Ghana has been receiving revenues from oil since 2010, while Uganda's oil is not yet flowing in large amounts. 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 what they thought the effects of the funds would be and how they thought these funds should be spent. The amounts of these revenue streams were held constant across conditions in each coun-

try. All were based on best estimates of plausible future budget sources given publicly available information; thus, no deception was used in the experiment.³

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 giving them either Ghanaian cedis or 1,000 Ugandan shillings at the start.⁴ 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)⁵:

“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 vignettes 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 vignette 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 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.

³We 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 four possible pairs: oil versus taxes and aid versus taxes, respectively. The remaining comparisons are the focus of ongoing research and will be reported elsewhere.

⁴At 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.

⁵See Section D.1 in the appendix for information on the randomization protocol.

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 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 (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, 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 also queried them about the likelihood that the money would be used to supply public goods. Finally, questions probed whether 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.⁶ 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.⁷

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.⁸ 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, 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

⁶In 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.

⁷See Table A.3 in the appendix for results of balance tests designed to test successful randomization.

⁸See 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 makes increases the probability of obtaining significant results.

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 on a traditional test: that of 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 difference: 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 very 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 left 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

	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	0.48	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.

(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). Uganda is represented by the solid line and Ghana by the dashed one. The treatment effects are given in standard deviation units for all non-binary variables, such that the estimates can be interpreted as the change in standard deviation units in the dependent variable induced by the tax treatment relative to one of two reference groups, oil or aid.

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 ones 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. We find no support for Hypothesis 1 or Hypothesis 2 for both Ghana and Uganda.⁹

⁹Due 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.

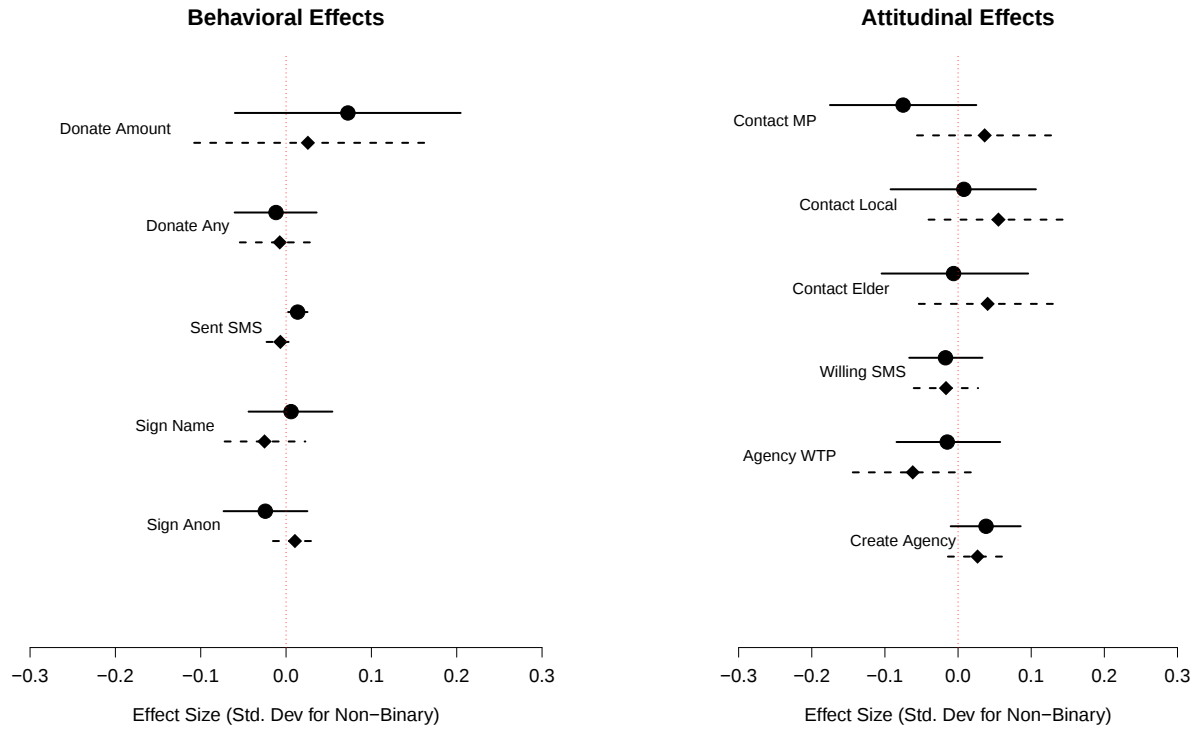


Figure 1: Main Tax Treatment Effects, Oil as Control. Effects in standard deviation units shown for both behavioral (left pane) and attitudinal measures (right pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence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

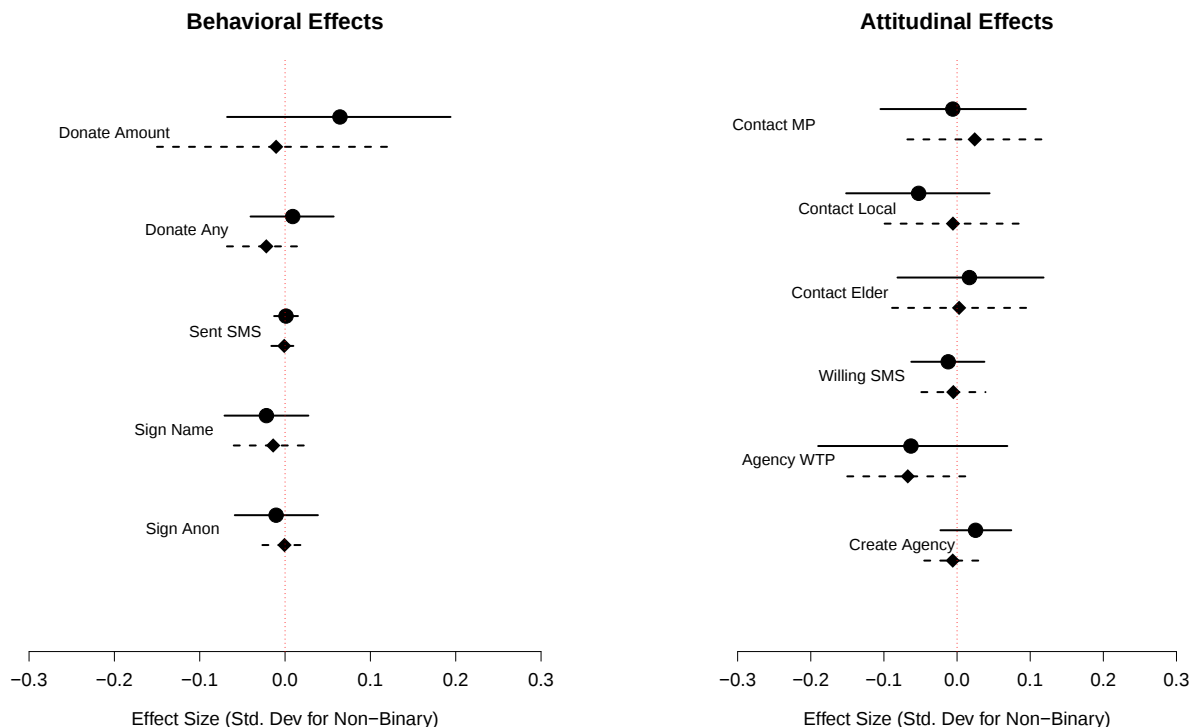


Figure 2: Main Tax Treatment Effects, Aid as Control. Effects in standard deviation units shown for both behavioral (left pane) and attitudinal measures (right pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in *ri* package. Dashed lines represent estimates from Ghanaian subjects, solid lines from Ugandan subjects.

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).¹⁰ The right 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. 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.

We also presented three additional empirical implications that flow from the taxation-causes-

¹⁰The equivalent in Ghana was the district chief executive.

accountability hypothesis. First, according to Hypothesis 3 citizens should realize they are being taxed and can 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.

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 to 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 difference between tax and non-tax revenues in these key areas.

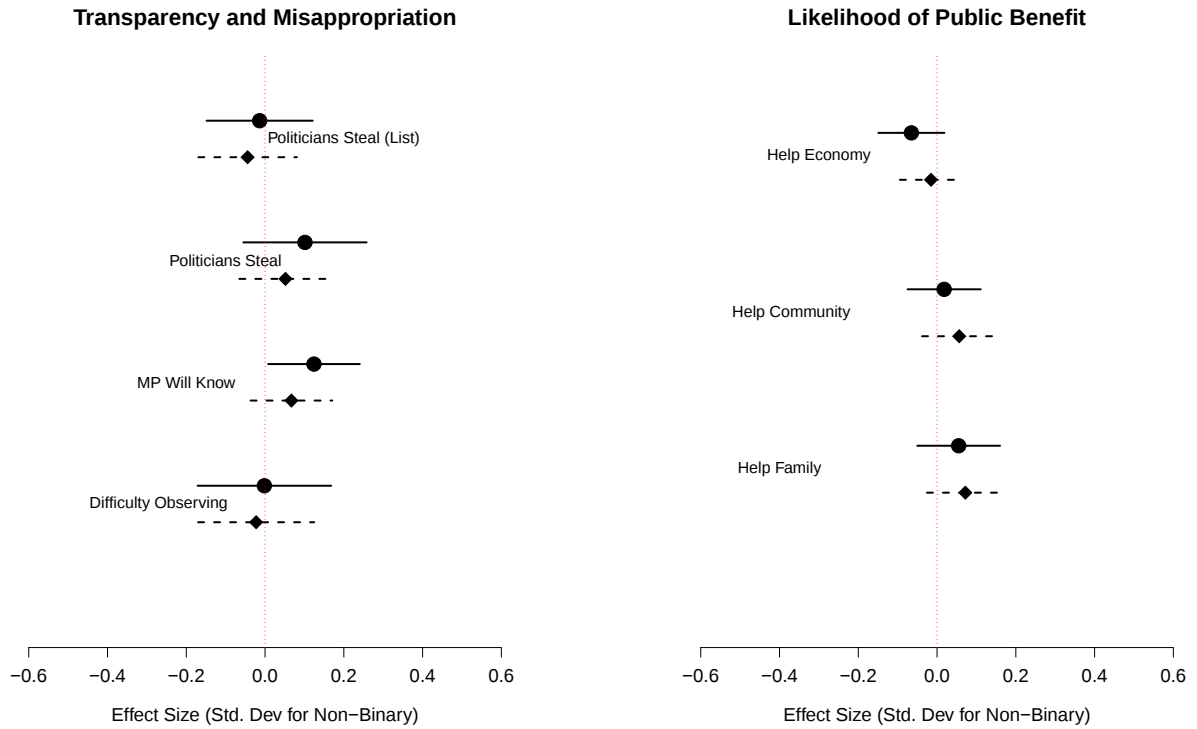


Figure 3: Tax Treatment Effects, Oil as Control. Effects in standard deviation units shown for transparency and misappropriation (left pane) and potential public goods benefit measures (right pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package. Dashed lines represent estimates from Ghanaian subjects, solid lines from Ugandan subjects.

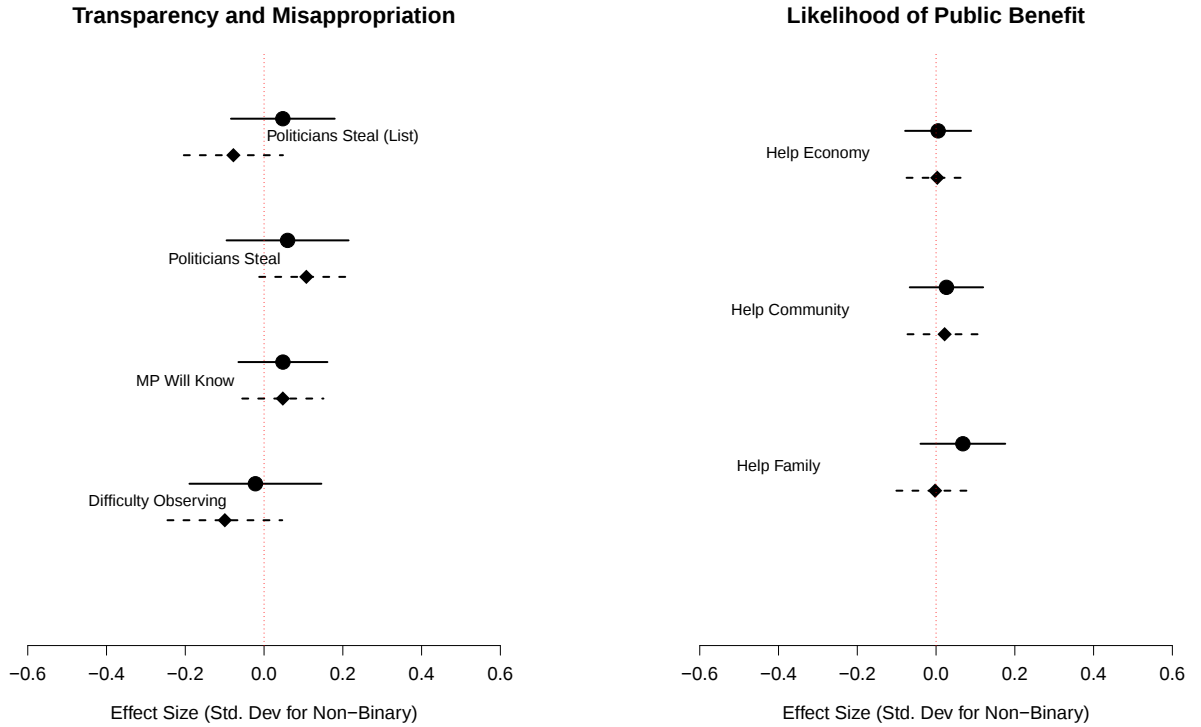


Figure 4: Tax Treatment Effects, Aid as Control. Effects in standard deviation units shown for transparency and misappropriation (left pane) and potential public goods benefit measures (right pane). Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in *ri* package. Dashed lines represent estimates from Ghanaian subjects, solid lines from Ugandan subjects.

Yet, 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 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.

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 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.

¹¹ Because the treatment was a vignette 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.1 and A.2 in the appendix, with behavioral measures in the left panes and our theoretical willingness measures in the right 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

¹¹We 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.

for a smaller, more exclusive group of subjects especially attentive to the mention of taxes. Yet 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 a “high-type” respondent—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 examine 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 examine these two subgroups to see 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.

Figures 5-8 visualize the results of analysis identical to that in the previous section for each of these subgroups for the Tax-Oil and Tax-Aid comparisons with the left pane for Uganda and the right for Ghana. In Figures 5 and 6 the dotted lines represent estimates from those respondents who were “low-type,” while solid lines represent those who were “high-type.” In Figures 7 and 8, the dotted lines represent opposition party supporters and the solid ones represent supporters of the governing party. Figures 5 and 7 show the treatments effects for taxes relative to oil, and Figures 6 and 8 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.

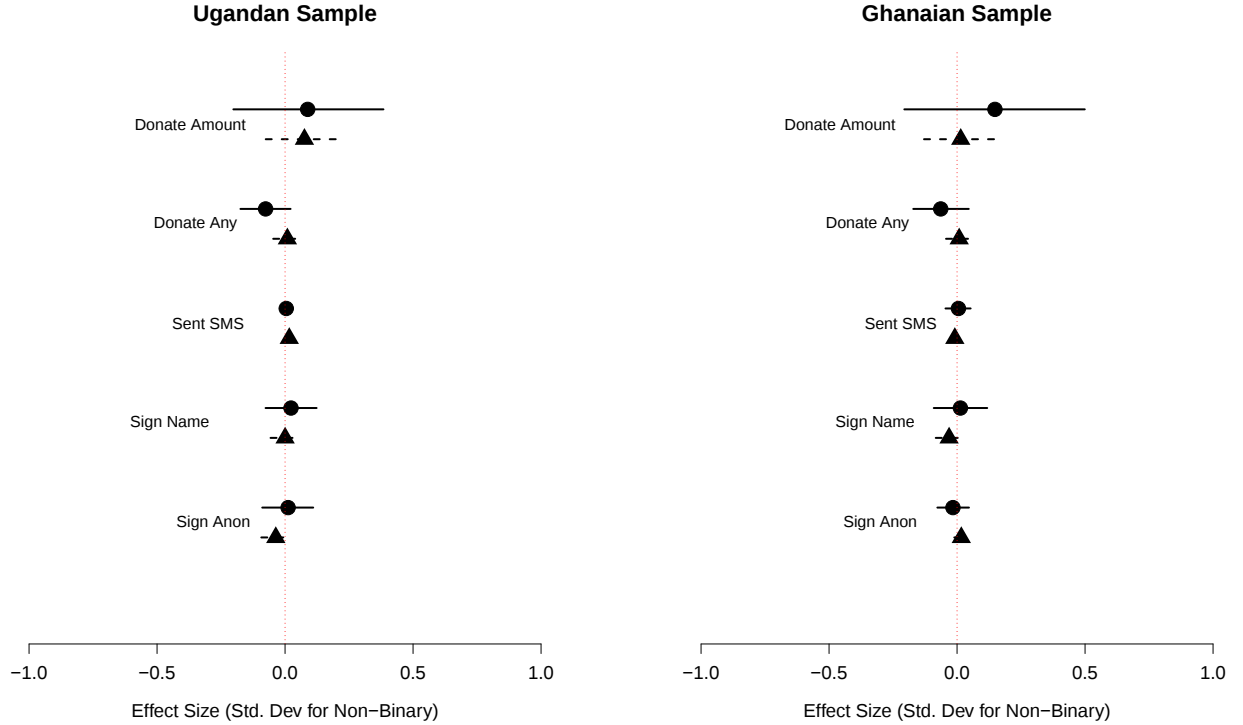


Figure 5: High-Type vs. Low-Type Respondents, Tax-Oil. Solid lines represent “high-type” respondents, while dashed lines represent “low-type” respondents. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

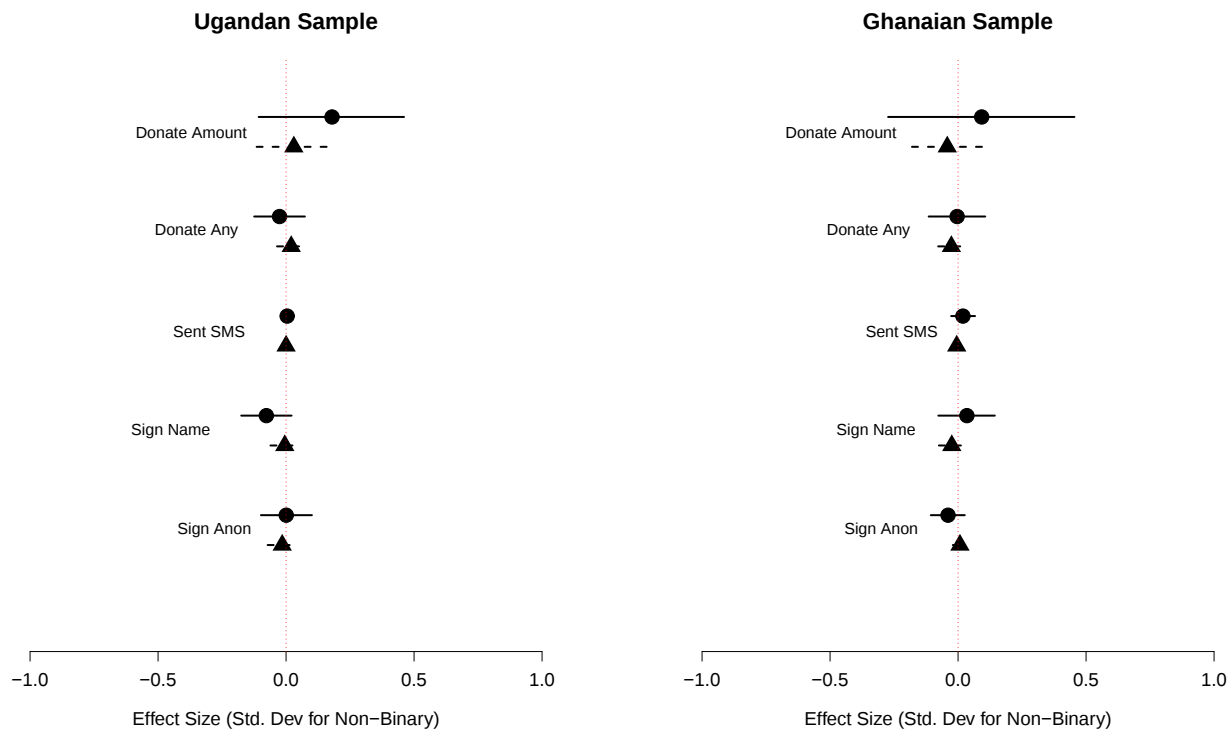


Figure 6: High-Type vs. Low-Type Respondents, Tax-Aid. Solid lines represent “high-type” respondents, while dashed lines represent “low-type” respondents. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

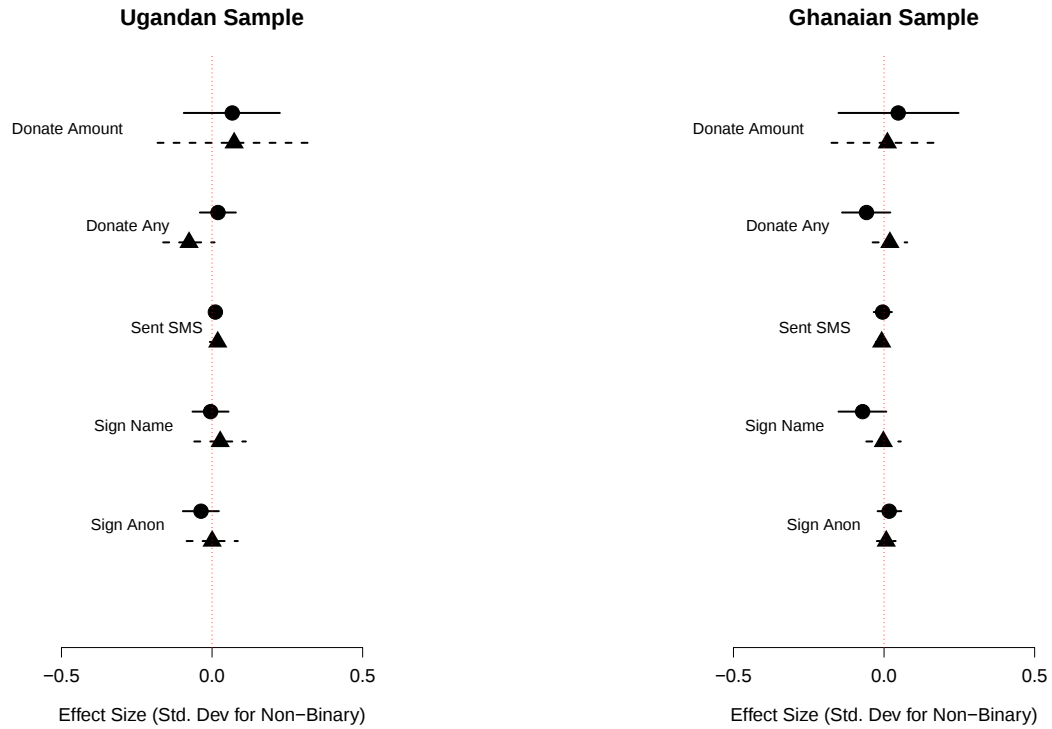


Figure 7: Gov Supporters vs. Non-Supporters, Tax-Oil. Solid lines represent respondents who self-identified as supporters of the governing party, while dashed lines represent opposition supporters. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in ri package.

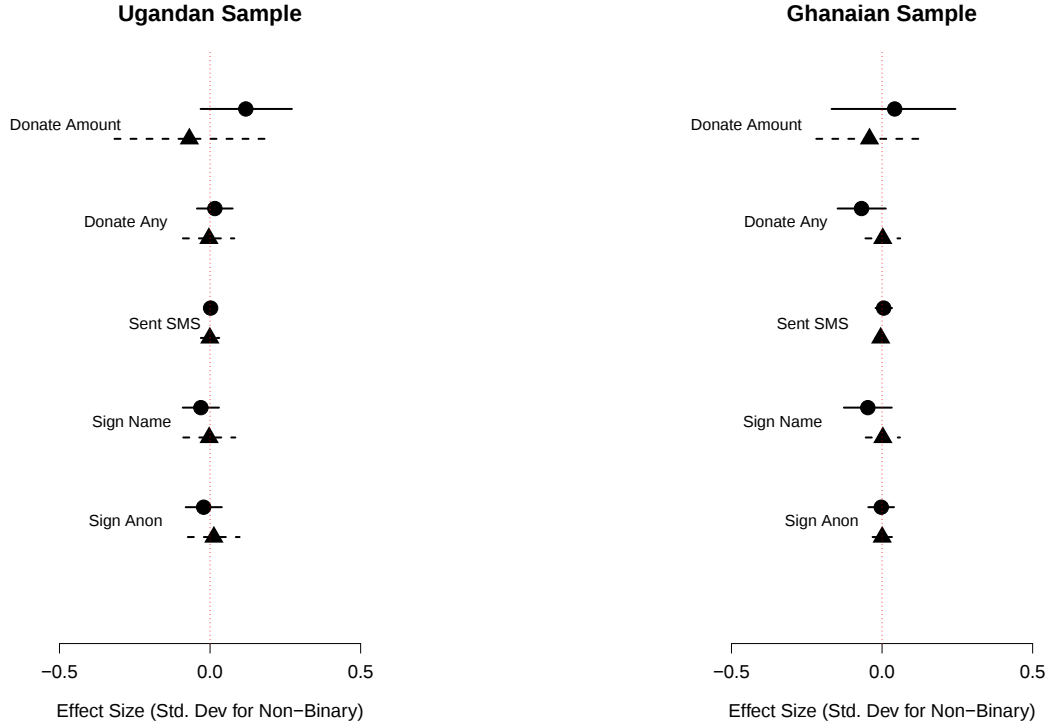


Figure 8: Gov Supporters vs. Non-Supporters, Tax-Aid. Solid lines represent respondents who self-identified as supporters of the governing party, while dashed lines represent opposition supporters. Estimates obtained using randomization inference with 10,000 draws to approximate the exact distribution. 95 percent Rosenbaum-style confidence intervals obtained via inversion of constant effects hypothesis as implemented in `ri` package.

Taxpayers do not differ from non-tax payers and opponents and supporters of the government do not differ from one another in terms of differential monitoring on taxes on our measures. But perhaps other factors condition reactions to these revenue sources? We investigated many other subgroups to see 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 of public goods issues. These results are all reported in the appendix in Tables B.6-B.9. In no case do we see significant subgroup effects. 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 types in our representative samples seem to react the same way: they do not want to monitor or sanction

differentially across the revenue sources since they do not perceive them as being different in kind.

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 effects, 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 misappropriation 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.¹²

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 varying revenue sources

¹²The 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.

cause differences 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. As pre-registered, 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 so-called resource curse, which argues that non-tax revenues cause great political (and economic) damage to countries precisely because the impetus to monitor and sanction elected officials for the misappropriation of government revenues is thought to be weaker for non-tax revenues. Our project does not directly test the resource curse or the endowment effect, which is the psychological mechanism 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 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, we assumed that citizens would be much 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 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 is just that they are equally willing to do this 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 differentially? First, it could

be related to our tax treatment. The vast majority of the Ghanaian and Ugandan public has never paid taxes, especially direct income ones. Very few people in either country are above the minimum income for tax payments, and few are in the formal market even for indirect taxes like the VAT. 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 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. 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 fundraising drives in these countries have a very similar setup to our research design: citizens stop their fellows on the street, provide information, and ask for support. So the design—especially the behavioral outcomes—provide 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 treatments 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 using this approach (Martin 2014, Paler 2013). However, we also worry that such an intervention may not have reflected real-world conditions well in our research context. Income taxes are not relevant for the vast 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 results focuses on the institutional environment 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 citizens' ability to benefit in purely welfare terms.

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. Oil revenues are often seen by citizens as their collective legacy to effect a better future, thus a national treasure belonging to all citizens, and therefore not to be squandered. The rhetoric surrounding the discovery and extraction of oil in both Uganda and Ghana have 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 fairly 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. Perhaps surprisingly, 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 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 (Paler 2013), 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.

We conducted substantively identical large survey experiments in two countries to determine whether differential revenue sources might, in fact, lead to greater willingness to 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 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. 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 the value-added taxes and very low reliance on income taxes for government revenue may be a factor. 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 affect this. 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. 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 from the two countries increases confidence in our null results about the 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” that have little or no relation to citizen action.

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