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Essays on the Political Economy of Development

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Thesis submitted to Trinity College, University of Dublin in fulfilment of the requirements for the degree of Doctor of Philosophy (Ph.D.)

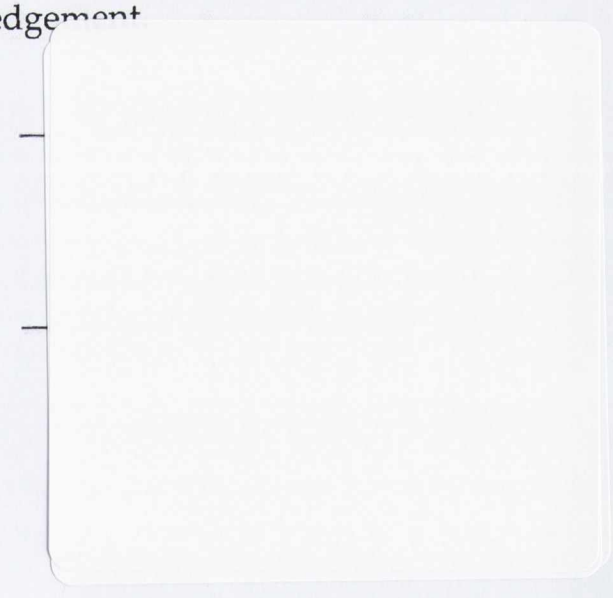
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Summary

The core of this thesis explores three issues at the intersection of political and development economics.

In Chapter 2, I demonstrate how ethnic divisions can undermine the provision of an important public good: teacher attendance. In the presence of weak formal institutions—such as those found in many less developed countries—teachers face a lower likelihood of punishment for absenteeism. In these settings, other forms of local collective action are often required to produce public goods and to prevent free-riding. However, a growing literature has shown that local collective action outcomes are often adversely affected by ethnic divisions. I identify the impact of a new measure of ethnic divisions on teacher absenteeism using two datasets: one collected by the World Bank from random unannounced school visits in Uganda, and another collected from over 20,000 respondents to the Afrobarometer survey in 16 sub-Saharan African countries. I incorporate constructivist theories of ethnicity into the measure by allowing the effect of diversity to vary by the salience of ethnic identification in each district. I find that, at high levels of ethnic salience, a one standard deviation increase in ethnic diversity increases the observed absenteeism rate in Uganda by between 3.8 and 9.3 percentage points, or 0.08 and 0.21 standard deviations. In the multi-country survey data, the same change increases perceived absenteeism by 0.08 standard deviations. At low levels of ethnic salience, ethnic diversity has no positive effect on absenteeism in either dataset. I provide suggestive evidence that social capital in the form of within-school teacher networks, rather than community-level monitoring, may explain the findings. The results offer one explanation for why substantial recent investment in education does not seem to be leading to improved test-score outcomes for children in many ethnically diverse countries. The analysis also has implications for the measurement of ethnic divisions.

In Chapter 3, I propose and test a mechanism through which natural resources can affect democracy in less developed countries. I posit that, in the presence of high natural resource rents, leaders lower the burden of taxation on citizens in order to reduce the demand for democratic accountability. The theory is corroborated using Afrobarometer public opinion surveys across 15 sub-Saharan countries, in addition to country-level data on natural resource rents and other indicators. I find that the results are robust to a range of alternative specifications and tests for competeting hypotheses. A supplementary

analysis reveals that, consistent with the two-period model proposed, the effects are more acute closer to national elections. The study is published in a refereed journal.

In Chapter 4, I present an analysis that was undertaken in collaboration with my supervisor, Pedro Vicente, and Daniel Kaufmann, of Brookings Institution. In a change from the previous chapters, we present a channel through which politico-economic factors in rich countries can directly affect outcomes in less developed countries. We analyse preferences for government allocations of Official Development Assistance (ODA) using new data on respondents surveyed in 24 OECD donor countries from 2005 to 2008 by Gallup International, in association with the World Bank Institute. First, we characterise supporters of foreign aid. We posit and corroborate empirically that preferences for government donations to foreign countries are negatively determined by the level of taxes an individual is obliged to pay in a proportional system. We also show that, at the country-level, inequality and national income increase support for foreign aid. Second, we analyse the democratic aid deficit—the difference between the level of ODA desired by citizens and the level actually donated by their government—and propose that the monetary influence of private-interest groups on policymakers extends the gap between government policy and citizens' preferences. We validate this assertion using data on lobbying from the World Economic Forum's Global Competitiveness Report. Finally, we use information on the private charitable donations of citizens to dismiss a 'crowding out' story as a competing hypothesis.

I conclude the thesis in Chapter 5.

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Chapter 1

Introduction

This thesis contains three essays which address separately the role of public institutions in the development of the world's poorest countries. In the first substantive chapter, I show how the provision of a basic public good, primary school teacher attendance, can be affected by local ethnic diversity in poor African countries. In the second analysis, I use a similar dataset to identify a channel through which rents from the extraction of natural resources can decrease political accountability. In the final chapter, I show how private interests in donor countries can influence the level of foreign aid allocated by governments to poor countries.

In an influential volume, Banerjee and Duflo (2011) compellingly demonstrate the potential for well-designed government interventions to provide cost-effective public goods that are essential for the development of human capital in poor countries. However, Besley (2007) and Collier (2007, 2009), amongst others, explain that the conditions necessary for the formation of effective public institutions are often absent in the very countries that need them most.

In the first analysis, I show how this can present unique problems for primary education in ethnically divided African districts. In the absence of well-functioning public institutions, teachers are less likely to be formally sanctioned for absenteeism. This may explain why the rate of teacher absenteeism is over five times higher in Uganda than it is in New York. Or why, in India, it is two and a half times higher than the rate of absenteeism for private sector factory workers. In such settings, informal collective action methods are particularly important to ensure the provision of public goods and to prevent free-riding. An expanding literature has shown that these local collective action methods are more likely to fail in ethnically diverse areas than in homogeneous areas. This poses a particular threat to the provision of education in Africa, the most ethnically diverse continent in the world.

With this in mind, I ask if local ethnic divisions can explain variation in teacher absenteeism rates in sub-Saharan Africa. I use two datasets to identify the relationship: one based on almost 100 random, announced visits to primary schools in Uganda; and another based on Afrobarometer opinion surveys of over 20,000 individuals in 16

countries. I develop a new measure of ethnic divisions, consisting of an interaction term between district-level ethnic diversity and the district-level average of a survey question which measures the salience of respondents' ethnic identity relative to their national identity. I also construct a school-level alternative in the Ugandan data. The term is created in response to growing empirical support for 'constructivist' theories of ethnicity, which stress that diversity is only salient in the context of certain political and historical factors, and that it otherwise does not lead to the collective action failures and political cleavages with which it is commonly associated.

I measure teacher absenteeism with observed data collected by the World Bank at Ugandan schools, and with multi-country data on survey respondents' perceptions of teacher absenteeism at their local school. I find that, at a high level of ethnic salience, a one standard deviation increase in ethnic diversity increases absenteeism by between 3.8 and 9.8 percentage points, or 0.08 and 0.21 standard deviations. At low levels of salience, I find no positive effect.

Having established a robust link between ethnic divisions and absenteeism, I provide suggestive evidence for a channel of causality in the form of social capital between teachers. Neither parent teacher associations, school inspections, the ethnicity of teachers nor the self-described strictness of head teachers appear to have any effect on observed absenteeism in Uganda. Rather, I find that the extent to which teachers socialise together accounts for almost all of the effect of ethnic diversity. The finding is consistent with theoretical and empirical evidence of the effect of ethnic diversity on group formation and social participation from Alesina and Ferrara (2000). Whether it reflects the possibility that teachers face more credible sanctions from members of their social network for not turning up to school—consistent with Miguel and Gugerty (2005) and Habyarimana et al. (2007, 2009)—or the possibility that they simply show more altruism toward their own group members (Becker 1957, 1974; Hjort, 2012) is an open question for future experimental research.

The chapter makes three noteworthy contributions to the literature. Firstly, it deepens significantly our understanding of teacher absenteeism, which is found by Chaudhury et al (2006) to be widespread in poor countries, and by Duflo et al. (2012) to have substantial effects on the educational attainment of children. Secondly, I introduce a new, district-level measure of ethnic divisions which expressly accounts for the relevance of ethnic identification; and, thirdly, I show evidence that social capital amongst teachers is a more likely channel of causation than official sanctioning institutions or the ethnic status of teachers. Moreover, the results leave room for policy responses in the form of either addressing the salience of ethnic identification or harnessing the potential of social networks to incentivise attendance by teachers at public primary schools in poor countries.

The second chapter of substance has been accepted for publication at a peer-review journal (McGuirk, forthcoming). In it, I present a channel through which public institutions can deteriorate further in poor, resource-rich countries. Specifically, I propose that, as resource rents increase, political elites in these countries lower the burden of taxation on citizens in order to reduce the demand for democratic accountability. I corroborate this by combining Afrobarometer data on tax enforcement and democracy with World Bank data on natural resource rents.

I use a two-stage instrumental variables approach in the empirical analysis, where survey-based measures of the demand for democratic accountability and of perceived tax enforcement are the dependent and endogenous variables respectively, and the resource rents variable is the exogenous instrument. The credibility of the identification strategy relies substantially on the inclusion of controls for country fixed effects in the specifications, made possible by the repeated nature of the cross-sectional data, which contain over 50,000 observations taken in 33 survey rounds across 15 countries from 2001-2006. I find that an increase in resource rents lowers perceived tax enforcement, which itself significantly determines the demand for accountability by around a third of a point (both variables are measured on four-point scales).

The finding contributes to the growing case for a political explanation of the natural resource curse, which purports that, in countries with weak institutions, resource boons often result in adverse outcomes for democracy. To my knowledge, it is the first study that tests empirically for 'rentier' state behaviour using micro-level data.

The final analysis shows how public goods in the form of foreign aid can be affected by politico-economic factors in rich donor-countries. It is the product of a collaborative effort between my supervisor, Pedro Vicente, and Daniel Kaufmann of Brookings Institution. Using rich new data on the attitudes of donor-country citizens towards Overseas Development Assistance (ODA, i.e., official aid from their home country government), we first establish the within-country determinants of individual support for aid. We present a simple median-voter model which predicts that, in a proportional tax system, support is decreasing in income. We corroborate it empirically using micro-level data on almost 90,000 respondents in 24 OECD countries. We also prove empirically a country-level implication of the theory: that average support for aid is increasing in inequality, measured as the ratio of the mean to the median income level.

We then extend the model to incorporate the influence of special interest group politics (Grossman and Helpman, 1996, 2001) on the final level of aid that governments decide to allocate overseas. We propose that the top income agent can influence actual aid through 'buying' lower levels from the policy maker. We provide suggestive evidence for this behaviour using data on the pervasiveness and effectiveness of private monetary influences on political decision-making based on mangers' responses to

an anonymous firm-level survey carried out for the World Economic Forum's Global Competitiveness Report. Finally, we present an analysis of private aid donations in order to rule out a 'crowding out' competing hypothesis.

I present each chapter in order, before offering a brief conclusion in Chapter 5.

Chapter 2

Teacher Absenteeism and the Salience of Local Ethnic Diversity: Evidence from African Districts

Despite the unprecedented expansion of primary school access over the past decade, standardised test results in many less developed countries reveal that basic numeracy and literacy skills are not improving.[1] Evidence from Chaudhury et al. (2006) and Duflo et al. (2012) strongly suggests that teacher absenteeism may be significantly contributing to this observation. The former study found that 19% of teachers were absent during unannounced visits to nationally representative samples of schools in Bangladesh, Ecuador, India, Indonesia, Peru and Uganda in 2002 and 2003,[2] while Duflo et al. (2012) show that a reduction of the absenteeism rate in Udaiper, India, from 36% to 18% led to an improvement in test scores of 0.17 standard deviations.

In this analysis, I show that ethnic divisions at the district and school levels are associated with significantly higher rates of teacher absenteeism. I design a new measure of ethnic divisions to capture what are sometimes called the 'evolutionary' and the 'constructivist' components of ethnic identity. Traditional measures of ethnic diversity, or 'ethnolinguistic fractionalisation', are based on the composition of ethnic groups in a given area. This is to a large extent the product of long-term cultural drift, itself caused by historical settlement duration (Ashraf and Galor, forthcoming; Ahlerup and Olsson, 2012) and geographic variability (Michalopoulos, 2012). However, the extent to which this diversity is manifested in collective action problems or political cleavages depends on the salience of ethnic identification, which can vary across countries and over time due to nation-building policies (Miguel, 2004), political competition (Posner, 2004; Eifert et al., 2010) or other historical and contextual factors (Glennerster et

[1] See Uwezo (2011, 2012) for East African cases, and Pratham (2006) for Indian cases.

[2] This is a conservative estimate. The inclusion of 'tea-drinkers'—teachers who were present but were not teaching as scheduled—increases the figure from 25% to almost 50% for India alone.

al., 2012). In order to capture these 'constructivist' conditions, I create a district-level term that represents ethnic divisions by interacting ethnic diversity with the salience of ethnic identification. The diversity component is based on a Herfindahl concentration index of Afrobarometer survey respondents, while the salience component is based on respondents' propensity to identify themselves along ethnic lines rather than national lines.

I combine this new measure of ethnic divisions with data on (i) observed teacher absences collected from random visits to almost 100 schools in 10 Ugandan districts;[3] and (ii) perceived teacher absenteeism amongst over 20,000 survey respondents in 16 African countries. At high levels of ethnic salience,[4] I find that an increase of one standard deviation in local ethnic diversity increases the rate of observed teacher absenteeism in the Ugandan dataset by 9.3 percentage points, or 0.21 standard deviations. In the multi-country survey data, the comparable increase in perceived teacher absenteeism amongst respondents is 0.08 standard deviations.[5] At low levels of ethnic salience, however, I find no positive effect of ethnic diversity on absenteeism in either dataset: in the multi-country data there is no significant effect; while in the Ugandan data there is a significantly negative effect of 6.7 percentage points, which I suggest may be explained by residential sorting. In addition, I find that including the diversity component alone, and by implication ignoring the role of salience, would lead to the erroneous conclusion that ethnicity does not have any effect on absenteeism.

I also replicate the Ugandan analysis using a school-level alternative to the district-level measure of ethnic divisions. In each of the 94 schools, head teachers are asked to estimate the shares of the three most commonly spoken mother tongues amongst pupils in that school. I use this information to construct a measure of ethnic diversity, and interact it with the salience measure from the Afrobarometer. Across a broad range of specifications, the results are consistent with those reported above.

The main findings of the study contribute to a growing literature on the negative association between ethnic diversity and the local provision of public goods in sub-Saharan Africa (Miguel and Gugarty, 2005; Habyarimana et al., 2007, 2009) and elsewhere (Alesina et al., 1999, Vigdor, 2004). This association has particularly acute consequences for countries that lack the strong formal institutions required to implement government policies—like India, Uganda and most of the 16 countries under analysis—where informal collective action methods at the local level are required in-

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[3]These data were collected in a project led by Habyarimana (2010) and Chaudhury et al. (2006).

[4]High (low) ethnic salience is defined as the mean plus (minus) one standard deviation.

[5]This is a subjective measure based on a four-point scale, hence there is no meaningful percentage point change to report for comparison.

stead to provide public goods and to prevent free-riding. To illustrate, Chaudhury et al. (2006), show that teachers in their sample of less developed countries are rarely sanctioned formally for not attending school. In India, for example, only one head teacher in a sample of nearly 3,000 public schools reported a case in which a teacher was dismissed for absenteeism, despite an absenteeism rate of 25%; in the Ugandan data, the comparable figure is just under 1.5% of head teachers, despite an absenteeism rate of 28%.[6] Indeed, as the authors venture (pp. 93):

> ...the mystery for economists may not be why absence from work is so high, but why anyone shows up at all. For many providers, the answer must be that important intrinsic and non-pecuniary motivations - such as professional pride or concern for the regard of peers - affect attendance decisions

The literature suggests that, in this clear absence of formal organisation, local efforts to produce public goods, such as teacher attendance, will be affected by ethnic divisions.[7]

Of course, the characterisation of ethnic divisions that I present brings with it considerable challenges for the empirical estimation strategy. Ethnic diversity is not a random accident, nor, especially, is the salience of ethnic identity. To account for the potential effect of omitted variables on teacher absenteeism, I construct an extensive set of controls for inclusion in the econometric models. For the Afrobarometer sample, I include controls for a wide set of respondent- and district-level characteristics, as well as fixed effects for regions, ethnicity, and pre-colonial ethnic boundaries. I also control for the temporal and spatial proximity to recent armed conflict events and fatalities by combining information on the location coordinates of each district in the Afrobarometer with those in the Armed Conflict Location Event Database (ACLED). In addition, I show that the inclusion of controls for endogenous sorting (based on pre-colonial ethnic boundaries) and historical settlement patterns have no effect on

[6] Accordingly, absenteeism is rarely as grave an issue either in countries with strong formal institutions or in private sector industries with high monitoring: administrative data from New York school districts in the mid-1980s revealed a teacher absenteeism rate of around 5% (Ehrenberg et al 1991); while the Indian Ministry of Labour Industrial Survey 2001-2002 shows that absenteeism amongst factory workers is 10.5%, despite the existence of rigid labour laws. As I note above, the rate of teacher absenteeism in India is estimated (conservatively) by Chaudhury et al (2006) to be 25%.

[7] Revisiting the Chaudhury (2006) data with this in mind, it is interesting to note that Bangladesh has only the fourth highest absenteeism rate, despite being the second poorest of the six countries. This is likely to be at least partially explained by the fact that it has by far the most homogeneous population, as measured by both ethnolinguistic and cultural diversity from Fearon (2003). Absenteeism in Bangladesh is lower than in Indonesia, which is over twice as wealthy but three times more diverse.

the model in the presence of such an extensive set of fixed effects. For the Ugandan teacher-level dataset, I can control comprehensively for teacher characteristics, as well as institutional and location-based covariates based on the specification of Kremer et al. (2005), who use an almost identical dataset in their study of the determinants of teacher absence in India.[8]

An additional methodological challenge inherent in the multi-country study is the reliability of subjective assessments of teacher absenteeism. While it is likely to be measured with some stochastic error, Olken (2009) suggests also that an error component may be systematically correlated with ethnic diversity. He finds that people in ethnically diverse villages tended to overestimate significantly the level of corruption in a road-building project in Indonesia. The implication for this study is that ethnic diversity may be associated with disproportionately negative perceptions of public goods delivery in general, as a result in part of feedback mechanisms over time. Although the obvious mitigation of this concern lies in the replication with objective data from Uganda, I also run a large set of falsification tests to show that the results are highly unlikely to be driven by this systematic bias. These include testing for the effects of ethnic divisions on perceptions of other aspects of school quality and of national-level governance issues, as well as holding school-level characteristics constant in order to analyse variation in the error component alone. I offer an explanation for why the mechanism at play in the Indonesian setting is unlikely to be applicable in this context.

Having robustly established the link between ethnic divisions and teacher absenteeism, I take a tentative step towards establishing the channels of causation that may explain the relationship. This is facilitated by the richness of the Ugandan data, which includes information on teacher-level characteristics as well as school-level infrastructural and management characteristics. In simple terms, ethnic diversity can affect cooperation through its impact on the credibility of social sanctions (Miguel and Gugerty, 2005; Habyarimana et al., 2007, 2009), or through a 'taste' for discrimination between coethnics and non-coethnics (Becker, 1957, 1974; Hjort, 2012). This can affect in turn the formation or the effectiveness of local monitoring institutions, such as parent teacher associations, or other social networks which may ultimately impact a teacher's decision to attend school. Beyond this general effect on institutions, however, these channels may also have heterogeneous effects that depend on the ethnicity of the teacher. Coethnics may be informally sanctioned by parents or other teachers, whereas 'outsiders' may not face the same credible threats. Or, if the taste mechanism is at play, outsider teachers may simply care less about non coethnic students, parents or other

[8]Indeed both are component datsets for the six-country Chaudhury et al (2006) study. As such, they are based on very similar methodologies. The Ugandan dataset is analysed separately by Habyarimana (2010).

teachers.

Using this framework, I examine the effect of various social and monitoring institutions on absenteeism, both in general and conditional on whether or not a teacher is an outsider. I show that neither parent teacher associations, school inspections nor the sanctioning history of head teachers can explain the main result. Instead, I find that the strength of social networks betweens teachers in a school is the only intermediate variable that determines absenteeism while significantly diminishing the direct effect of ethnic divisions. Theoretical and empirical evidence from Alesina and La Ferrara (2000) support the claim that group formation and social participation is less likely in heterogenous communities. Moreover, I find that the effect of these social networks on absenteeism does not depend on ethnic divisions, though whether or not it operates through credible sanctioning threats or other-regarding preferences (i.e., discriminatory altruism) amongst social groups is left for future experimental research.

This paper makes at least three contributions to the literature. First, I provide new evidence on a significant determinant of teacher absenteeism that can partially explain such high rates in ethnically divided areas. Second, I introduce a new measure of ethnic divisions that is consistent with the heterogenous effects of ethnic diversity on a variety of outcomes found in the literature. Moreover, the 'constructivist' component of the measure leaves room for a policy response. Finally, I present suggestive evidence that ethnic divisions do not affect absenteeism through formal sanctioning institutions, but may in fact operate through their impact on social group formation amongst teachers within schools, which in turn influences attendance decisions.

The paper is organised as follows. In the next section I discuss briefly the analysis of ethnic diversity in the literature, including methodological challenges. I then introduce the data and discuss measurement issues, before presenting the main estimation results for both the multi-country and Ugandan analyses. Finally, I offer a potential explanation for the results by testing for channels of causality before concluding.

2.1 Analysing Ethnic Diversity

Scholars have long highlighted the deleterious impact of ethnic diversity on economic and political development, particularly in poor and institutionally weak countries (Easterly and Levine, 1997; Alesina et al., 2003; Alesina and La Ferrara, 2005). The cross-country evidence that characterised the early stages of the literature have been complemented since by a series of micro-level studies that have made progress in uncovering the channels through which ethnic diversity affects certain outcomes.

Of particular importance for this study are analyses that increase our understand-

ing of how divisions lead to collective action problems in sub-saharan Africa. Miguel and Gugerty (2005) provide evidence that parents in Kenya contribute to school funding more in homogeneous areas due to the credible threat of social sanctions for non-cooperation. In a seminal paper, Habyarimana et al. (2007, 2009) provide laboratory evidence for this social sanctioning channel amongst residents of Kampala, Uganda. It is an especially compelling explanation in these settings, where people are more reliant on within-group networks to organise the provision of public goods that effective governments would otherwise provide.[9] However, Hjort (2012) also points to a 'taste' based discriminatory mechanism, showing that floriculture plant workers in Kenya weigh the utility of coethnics ahead of non-coethnics. He finds that non-coethnics were even willing to incur a cost to display this discrimination amid the heightened ethnic tensions associated with the 2007 presidential election.

This wave of micro-level studies has also shed light on the conditions under which ethnic diversity may not have the expected effect on certain political and economic outcomes. Miguel (2004), for example, shows that ethnic diversity has heterogenous effects on school funding and water well maintenance in districts that straddle either side of the Kenya-Tanzania border. In Kenya, moving from a homogeneous area to one with a mean level of diversity lowers local school fundraising by 25%; whereas in the neighbouring Tanzanian district, the same change has no significant effect. These contrasting outcomes are put down to the well-known nation-building efforts made in post-independence Tanzania, characterised, amongst other policies, by the promotion of one common language (Kiswahili) and a strong emphasis on national unity throughout public school curricula. In Kenya, if anything, the opposite course was followed by a succession of politicians who were demonstrably willing to use ethnic diversity as a vehicle for their own political ends.[10] These divergent policies are set against a background of broadly similar colonial and historical characteristics, providing an empirical basis, also found in Hjort (2012), for the suggestion that the salience of ethnic diversity is politically malleable.

Direct evidence of this is presented by Eifert et al. (2010), who show using Afrobarometer data that the salience of ethnic identity, measured as the likelihood that respondents identify themselves along ethnic lines when faced with an open question on self identification, increases significantly with the proximity of competitive elections. Posner (2004a) also finds variation in the salience of ethnic cleavages across political contexts. Like Miguel (2004), he exploits the arbitrary determination of a na-

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[9]La Ferrara (2003) also analyses the role of kin groups in the functioning of informal credit markets in Ghana.

[10]This culminated in a wave of violence following the disputed 2007 presidential elections.

tional border—this time between Zambia and Malawi—and observes that the Chewa and Tumbuka groups are more likely to perceive each other as allies in Zambia than they are in neighbouring villages on the Malawian side of border, where they view each other with considerable antagonism and are less likely to inter-marry. This is explained by the political landscape in each country: in Zambia, neither group is large enough to form the basis of a viable political coalition; whereas in Malawi, by contrast, they each form large political blocs that vie for power.

Another example of the importance of context when analysing the effects of ethnic diversity comes from Glennerster et al. (2012), who find that variation in ethnic diversity within Sierra Leone does not affect the provision of local public goods. This is despite being a poor and highly diverse country that has recently experienced major civil conflict. The authors explain the result by documenting Sierra Leone's unique combination of colonial history, tribal organisation and language composition which together prevent the collective action failures one may expect to find in such settings.

2.1.1 Methodological Concerns: Measurement

Taken together, these studies highlight the incontrovertible role for 'constructivist' explanations of ethnic identity, which stress the importance of context and time in shaping both the formation and salience of ethnic identification. So what, if any, are the implications of these accounts for the measurement of ethnic diversity? Early cross-country studies used a measure of ethnolinguistic fractionalisation that was calculated using the Herfindahl concentration formula on a dataset compiled by Soviet anthropologists in the *Atlas Narodov Mira* (1964). In a pointed critique, Laitin and Posner (2001) bemoan its once ubiquitous use in the cross-country economic literature, noting, for example, that it is akin to using the rate of inflation in 1945 as a measure for a country's prosperity today. This is because, firstly, it is a static measure of a changing phenomenon. Identities and cultures change over time in response to economic and political climates. They cite, for example, the reorganisation of identities in Somalia since independence, where Isaaqs and Hawiyes would once have considered themselves part of a shared linguistic group. Today, Isaaqs conspicuously differentiate their speech in an attempt to justify attempts at secession. Second, the group categories on which the measure is based may have no discernible meaning in the context of political or economic cooperation. Ethnic identities have multiple dimensions in every country, and there is no way for a researcher to know ex-ante which ones are salient in which contexts. A third point rests with the concept of salience itself. As the literature above shows, ethnic identities in general may have very few implications for cooperation

in some countries, while having a significant effect in others, be it at the dyadic level (Posner, 2004a) or in general (Miguel, 2004).

In response to these and similar critiques, several researchers have compiled new measures that incorporate better the multi-faceted nature of ethnic diversity. Alesina et al. (2003) create a new index that includes linguistic and religious fractionalization; Laitin (2000) and Fearon (2003) create measures that incorporate the concept of linguistic 'distance' between groups; Posner (2004b) develops an index based on groups who engage in political competition, called the PREG index (for Politically Relevant Ethnic Groups); and Baldwin and Huber (2010) highlight the importance of accounting for economic inequality between groups.

In this paper, I use a new measure of ethnic *divisions* that overcomes the main pitfalls listed above. It consists of an interaction term between district level ethnic diversity, measured by applying the familiar variant of the Herfindahl concentration formula on the self-reported ethnicities of Afrobarometer respondents, and a district-level average of the respondents' answers to a question on the salience of their ethnic identity compared to their national identity. It is based on data that is concurrent with the outcome variable; it allows the subjects to choose their own ethnic identity; and it explicitly accounts for salience. A crucial added advantage is that it is measured at the district level, which allows me to control for regional (and, by implication, country) fixed effects throughout the analysis. The main implication, in light of the literature outlined above, is that it measures *relevant* diversity, and is thus a more accurate tool for identifying the types of problems that are synonymous with local heterogeneity in Africa and elsewhere.

2.1.2 Methodological Concerns: Estimation

While these constructivist findings have clear implications for the appropriate measurement of ethnic divisions, they also point to the need for a more careful approach to identifying valid estimation strategies. The ethnic divisions interaction term I use in this analysis is likely to be endogenous to a multitude of political and economic outcomes through factors as diverse as colonial history and current day political competition. This calls for more comprehensive econometric specifications than those typically found in much of the early literature, which generally treats ethnic diversity as an exogenous phenomenon. Moreover, recent contributions from Ashraf and Galor (forthcoming), Aherlup and Olssen (2012) and Michalopoulos (2012) have provided empirical bases to 'evolutionary' theories, which describe diversity as a function of long term settlement patterns. Specifically, Ashraf and Galor (forthcoming) and Aherlup and

Olssen (2012) show that the duration of human settlement is a significant determinant of modern day diversity across countries. This is owing to cultural drift that happens over time in response to the need for peripheral groups to provide their own public goods. Michalopoulos (2012) provides more evidence of cultural drift, this time due to geographic variability—such as the variation in soil quality—which led to the development of non transferable human capital and, eventually, the formation of new linguistic groups.

Taken together, these strands of literature necessitate the inclusion of an extensive set of political, economic, geographic and historic controls in the analysis. I describe my data and estimation strategy in Sections 2.3 and 2.4 respectively.

2.2 Data

I use two main sources of data in the analysis. The first dataset comes from the 2005 round of Afrobarometer, a series of nationally representative surveys based on standardised interviews of a random sample of either 1,200 or 2,400 individuals in 16 sub-Saharan African countries: Benin, Botswana, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, and Zambia.[11] The second dataset comes from Habyarimana (2010), and is a constituent dataset of the Chaudhury et al. (2006) survey of teacher absenteeism. It consists of data from two visits to each of almost 100 schools in 10 Ugandan districts. In each school, up to 20 teachers are selected at random from the roster, and their attendance is recorded at each visit. In addition, a rich set of characteristics for each teacher—present or otherwise—is recorded, as well as information on the head teacher, the school's facilities, its pupils and its structures of governance and management.

2.2.1 Data: Ethnic Divisions

Afrobarometer multi-country sample

For the Afrobarometer sample, I measure ethnic divisions as the product of ethnic salience and ethnic diversity. The measure for ethnic salience is recorded as the district level mean of the following survey question:

[11] I omit Cape Verde and Zimbabwe, as those respective samples do not have information on ethnicity and certain individual characteristics necessary for the analysis.

> Let us suppose that you had to choose between being a [Ghanaian/Kenyan/etc.] and being a _____ [respondent's ethnic identity group]. Which of these two groups do you feel most strongly attached to?

I ascribe a value of 1 to respondents if the answer is "only [group]" or "more [group]," and 0 if the answer is "equal," "more [group]" or "only [group]".[12] In Table 2.1, I present some external validation that the question is in fact measuring the concept of salience that I discuss in the previous section. Recall that Posner (2004a) found Chewas and Tumbukas to be salient adversaries in Malawi, but not in Zambia. This was due to the political landscape in each country, as Chewas and Tumbukas were each large political groups vying for power in Malawi, whereas in Zambia they were too small to form the basis of any competitive coalition. In the top panel of Table 2.1, we can see that Chewas and Tumbukas are significantly more likely to identify themselves along ethnic lines in Malawi than they are in Zambia. While this is an imperfect test for their animosity towards each other, it is nonetheless illustrative of the fact that political competition can lead to salient sub-national identification.

Table 2.1: Ethnic Salience in the Literature

Mean Ethnic Salience			
Posner (2004) Test			
	Malawi	Zambia	Difference
Chewa	0.31	0.05	0.26***
Tumbuka	0.16	0.07	0.09*
Miguel (2004) Test			
	Tanzania	Kenya	Difference
	0.06	0.16	0.10***

In the second panel, we also see consistency with the findings from Miguel (2004). Recall again that ethnic diversity had adverse effects on local collective action in Kenya, but not in Tanzania. This was due to serious nation-building efforts in Tanzania that were designed to inculcate a sense of common national identity ahead of sub-national ethnic attachments. Accordingly, ethnic salience is two and half times higher in Kenya than it is in Tanzania, despite similar levels of ethnic diversity and comparable colonial and precolonial backgrounds in both countries.

[12]Using the full five-point scale instead of this dichotomous interpretation does not qualitatively change the results. This is also the case when "equal" takes on a value of 1.

I measure ethnic diversity using the following Herfindahl concentration formula:

$$ELF_d = 1 - \sum_{g=1}^{n} s_g^2,$$

where s_i is the share of self-reported ethnic group $g \in (g...n)$ in each of the 1207 sample districts d. It reflects simply the likelihood that two randomly drawn individuals in a district d report different ethnicities. In addition to the 2005 Afrobarometer sample, I include respondents to the 2008 round in order to increase the power of the Herfindahl statistic. The median district sample size for the variable is 47.[13]

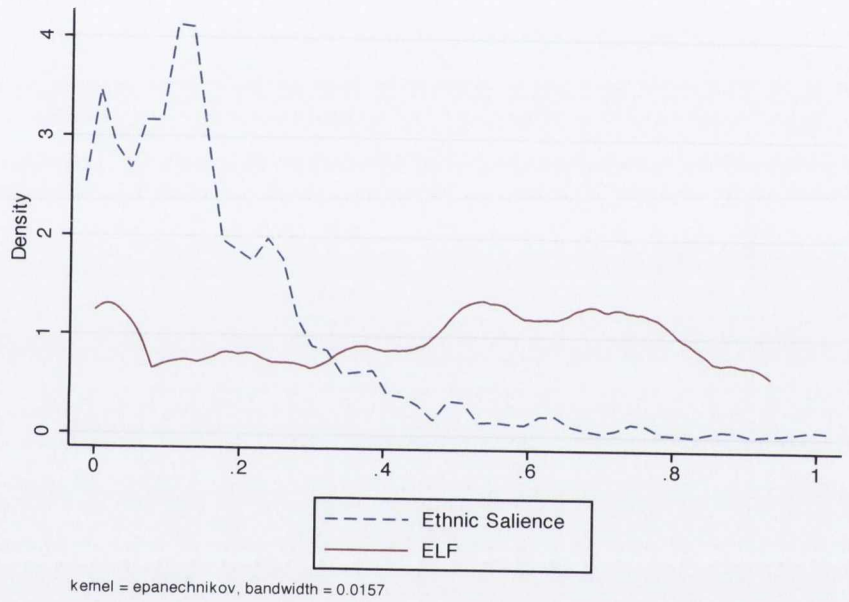


Figure 2.1: Kernel Density Functions: Ethnic Salience and ELF, Multi-Country

I present non-parametric density functions of both interaction components in Figure 2.1, and a scatter plot of their country mean values in Figure 2.2. A cursory look at the scatter plot reveals very clearly the importance of accounting for both components in the analysis.

By ignoring the salience of ethnic identities, researchers would erroneously conclude that highly heterogeneous Lesotho is more likely to suffer the consequences of salient ethnic divisions than relatively homogeneous (at the district level) Nigeria. This does not take into account, however, the cultural closeness of groups in Lesotho nor the

[13] There is no measure of teacher absenteeism in the 2008 sample.

history of ethnic violence in Nigeria, which may have contrasting effects on collective action. I argue that these are captured by the measure of ethnic salience.[14]

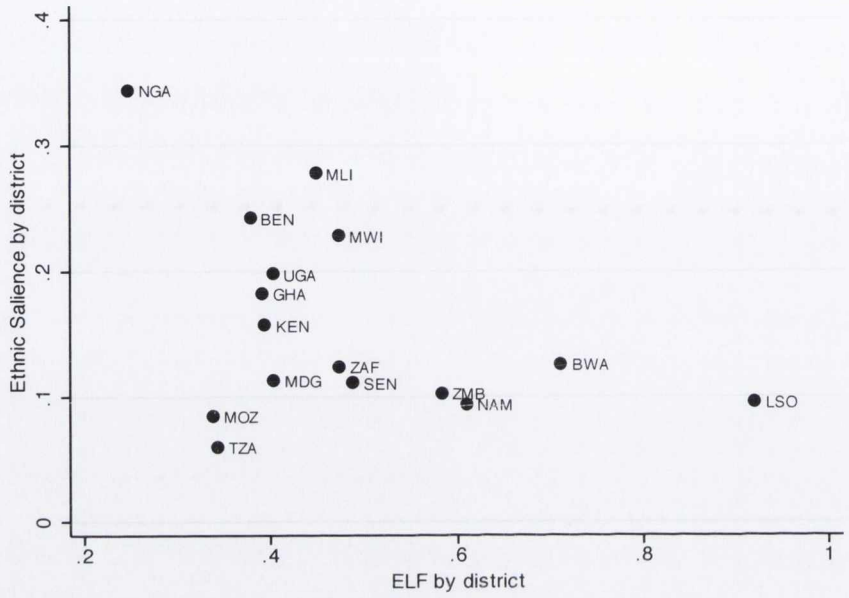


Figure 2.2: ELF and Ethnic Salience By District, Country Means

While I have thus far pointed to the clear need for including a 'salience' component of ethnicity in my measure of ethnic divisions, it is worth remembering that, in the context of local collective action, it is of course necessary to include a measure of diversity as an interaction component, for it is unlikely that highly salient ethnic identification will lead to adverse public goods outcomes within a homogeneous community.

Ugandan school visits

In the Ugandan sample, I use two measures of ethnic diversity. First, I match the district level Afrobarometer measure to all ten districts covered by the survey, namely: Arua, Bugiri, Bushenyi, Jinja, Kamuli, Kisoro, Luwero, Mpigi, Tororo and Yumbe. As this gives only ten data points, I also construct a value of ethnic divisions for each of the

[14]This may be also reflected in the degree of residential sorting in each country. In Figure 2.6 in the appendix, I plot ethnic salience against diversity at the country level rather than the aggregated district level measure in Figure 2.2. The large difference between ELF at the district and country levels for Nigeria—and the relatively minute difference between them for Lesotho—suggests that coethnics may sort into districts in countries with high ethnic salience. This again highlights the importance of accounting for both diversity and salience, where otherwise highly diverse districts could be misinterpreted as highly divided districts.

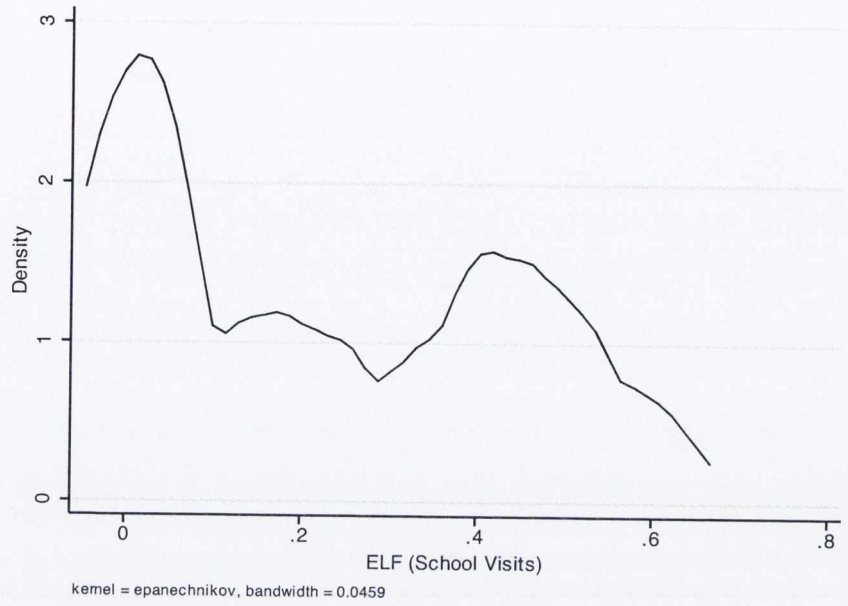


Figure 2.3: Kernel Density Function: ELF in Uganda (School Visits)

94 schools in the sample. During the first random visit to each school, head teachers were asked to list the three most commonly spoken mother languages amongst the pupils, and to estimate the corresponding share of pupils for whom this is the case. Using these shares, I create another Herfindahl-based concentration index for each school, and interact it with the district level mean ethnic salience from Afrobarometer.

The kernel density function for school-level diversity is estimated in Figure 2.3. All equations described in Section 2.5 are estimated using both measures, which have a correlation coefficient of 0.48.

2.2.2 Data: Teacher Absenteeism

In the multi-country Afrobarometer dataset, I base the measure of teacher absenteeism on responses to the following survey question:

> Have you encountered any of these problems with your local public schools during the past 12 months: Absent teachers?
>
> 0=Never, 1=Once or twice, 2=A few times, 3=Often, 7=No experience with public schools in the past twelve months, 9=Don't Know,

I code the responses on a four point scale from 0 to 3, omitting respondents who choose the remaining categories. This lowers the potential sample size from 21,598 to 14,100. Descriptive statistics are presented in Table 2.2, showing mean values for ELF_d, district level ethnic salience, and a selection of individual and village level variables for each response category of the question.

As is frequently the case when variables are based on subjective opinions, the major concern in this part of the analysis is the potential for systematic bias caused by non-random measurement error. While it is likely that the survey question picks up at least some noise, so that $TA_{id} = TA^*_{sd} + u_i$, where TA_{id} is subjective teacher absenteeism reported by individual i in district d, TA^*_{sd} is actual teacher absenteeism at school s, and u_i is stochastic measurement error, the danger is that $TA_{id} = TA^*_{sd} + u_i + v_i$, where the error component v_i is correlated with ethnic diversity. If this is the case, the observed coefficient that describes the relationship between the outcome variable and ethnic divisions may be driven by the error component, rather than a fundamental association between ethnic divisions and true absenteeism. Olken (2009) suggests that this should be treated as a genuine concern. He finds that people in ethnically heterogeneous villages in Indonesia are more likely to overestimate the level of corruption associated with a road building project than villagers in homogenous areas.

Table 2.2: Descriptive Statistics

Teacher absence:	Never	Once/twice	A few times	Often	Full Sample
ELF_d	0.48	0.44	0.43	0.44	0.46
	(0.30)	(0.29)	(0.27)	(0.26)	(0.29)
Ethnic Salience	0.15	0.17	0.18	0.18	0.17
	(0.15)	(0.17)	(0.17)	(0.16)	(0.16)
Urban	0.35	0.39	0.38	0.32	0.38
	(0.48)	(0.49)	(0.49)	(0.47)	(0.47)
Village facilities: school	0.78	0.80	0.81	0.82	0.80
	(0.41)	(0.40)	(0.39)	(0.39)	(0.40)
Village facilities: water	0.53	0.52	0.47	0.43	0.51
	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
Village facilities: electricity	0.52	0.58	0.52	0.47	0.54
	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)
Village facilities: health	0.46	0.50	0.52	0.47	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Village facilities: sewage	0.23	0.28	0.24	0.20	0.24
	(0.42)	(0.45)	(0.42)	(0.40)	(0.43)
Respondent characteristics					
Hardship	7.51	8.27	9.20	10.45	8.69
	(5.66)	(5.41)	(5.52)	(5.79)	(5.90)
Age	38.25	36.53	35.18	35.98	36.53
	(14.72)	(14.27)	(13.14)	(13.58)	(14.76)
Male	0.48	0.51	0.52	0.54	0.50
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Post-primary education	0.49	0.50	0.48	0.44	0.63
	(0.49)	(0.50)	(0.50)	(0.50)	(0.48)
Observations	6,755	2,521	2,791	2,033	21,598

Urban indicates the percentage of respondents surveyed in urban areas; Village facilities indicates the percentage of respondents surveyed in villages containing a school, piped water, electricity, a health clinic and a sewage system, respectively; Hardship is a composite variable ranging from 0-30, where 0 indicates that respondents never go without food, water, medical care, cooking fuel, a cash income, and school supplies like fees, uniforms or books, and 30 indicates that they always do. Post-primary education is the average of a dummy variable indicating that respondents have recieved any form of post-primary education.

He explains the findings by suggesting that feedback mechanisms over time have caused people in diverse areas to be wary of corruption in public projects, which in turn increases their scrutiny of public funds. However, the means through which scrutiny led to lower corruption in ethnically diverse villages for that particular project is linked to the disproportionately high rate of attendance at 'monitoring meetings' that were provided by the central government. In the absence of this exogenous facilitation of scrutiny, it is unclear whether or not diverse communities would cooperate better

than homogeneous villages to minimise corruption. In any case, I include a wide set of falsification tests below that directly address this concern, and find it to be an unlikely driver of results.

Ugandan school visits

The measure of teacher absence in the Ugandan dataset is more straightforward. Enumerators recorded a teacher as absent if she was not present to teach a class that she was scheduled to teach. Over two visits, they collected the information of 20 randomly selected teachers from the school's roster. In the cases where schools have less than 20 names on its roster, the enumerators collected information on all teachers.

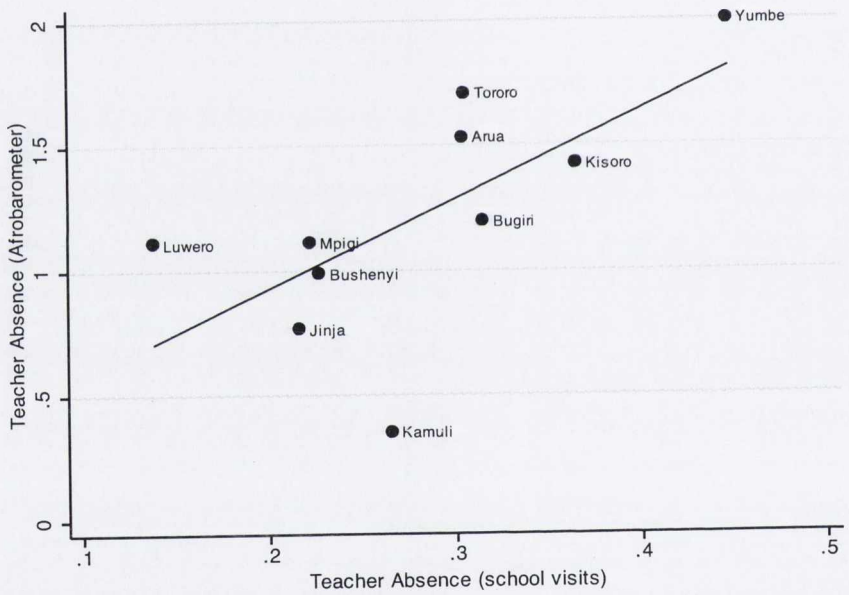


Figure 2.4: Absenteeism in Uganda: Afrobarometer vs. School Visits

In Figure 2.4, I plot the district mean values of each absenteeism variable. On the y-axis is the mean value for the four-point Afrobarometer scale; on the x-axis is the district mean for a teacher-level dummy variable indicating that a teacher was absent during a random school visit. I also include a linear fit, which confirms that the Afrobarometer measure contains significant information on actual teacher absenteeism. This provides some evidence for the validity of the multi-country analysis, which itself allows for a general interpretation of the results across 16 sub-Saharan African countries.

2.3 Estimation: Afrobarometer Multi-Country Sample

I begin the estimation section with a focus on the multi-country Afrobarometer survey data. As I note above, the two main challenges in this section relate to the potential endogeneity of salient ethnic divisions to teacher absence rates due to omitted variable bias, and also the possibility of a correlation between ethnic divisions and the error component of the dependent variable. The basic equation I estimate takes the following form:

$$TA_{idr} = a + \Psi \sum_{i=1}^{n} \frac{ES_{id}}{n_d} + \lambda ELF_d + \beta(\sum_{i=1}^{n} \frac{ES_{id}}{n_d} * ELF_d) + \gamma X_{id} + \delta V_{id} + \eta R_r + e_{id}, \quad (2.1)$$

where TA_{idr} is teacher absenteeism reported by individual i in district d and region r; ES is ethnic salience; X is a vector of individual controls; V is a vector of village-level controls; and R represents regional fixed effects for 184 regions in 16 countries. It is estimated using least squares with standard errors adjusted for two-way clustering within ethnicity groups and within districts (Cameron et al., 2011).[15] This method produces standard errors that are higher than those clustered by either ethnicity or districts alone. The individual controls include measures for age, age squared, level of education, gender, employment status, physical health, mental health, religion, individual ethnic salience,[16] access to food, water, medicine, fuel and income, as well as indicators for ownership of three assets: radio, television and a vehicle. The village level vector controls for whether or not a village—which is a sub-district level unit with a modal sample size of 8 respondents—contains each of the following services or facilities: a school, piped water, a sewage system, a health clinic, electricity, a police station, a post office, recreational facilities, a place of worship, community buildings and a tarred or paved road.

There is good reason to believe that this set of covariates controls for many of the potential sources of endogeneity that I explore in Section 2.2. In particular, the control for regional level fixed effects at a stroke controls for country-level colonial and pre-colonial historical factors (Glennerster et al., 2012), post-colonial national policies (Miguel, 2004) and current day political competition (Eifert et al., 2010, Posner, 2004), as well as the country-wide effects of macroeconomic policies that are associated with

[15]The Stata command for this estimator is 'cgmreg'. Although I treat the dependent variable linearly throughout the analysis, the results remain qualitatively unchanged when I treat it as a categorical variable and run an ordered probit model, and also when I merge all non zero responses and estimate a probit model

[16]This can be interpreted as controlling for the effects of an individual's deviation from the district mean value of ethnic salience. Omitting it does not have a significant effect on the results.

ethnic diversity (Easterly and Levine, 1997; Alesina et al., 2003). Moreover, the rich set of individual and village level controls accounts for variation in individual and local wealth, health and economic factors that could otherwise plausibly affect our interpretation of the coefficient of interest, β.

2.3.1 Results

In Figure 2.5, I present an illustration of the output from the most comprehensive specification described in this section.[17] I use a re-centering method to hold ethnic salience constant at a low level, which I define as the mean value plus one standard deviation (or 0.004), and at a high level, defined as the mean plus one standard deviation (or 0.33). The slope of ethnic diversity (ELF_d) at the low level (grey) is -0.14, and is statistically indistinguishable from zero; whereas the slope at the high level (red) is 0.29, or 0.26 standard deviations of perceived teacher absenteeism. The p-value for the interaction effect, which describes the statistical significance of the difference between the two slopes, is 0.005.

The results show clearly the importance of conditioning on salience when trying to ascertain the effects of ethnic diversity at the local level. In the absence of salience, ethnic diversity has no significant effect on teacher absenteeism.

In Table 2.3, I present the regression output for the most basic specification. In column (1), I omit the village level controls. While the independent effects of ethnic diversity (ELF_d) and ethnic salience do not affect perceived teacher absenteeism at traditional levels of statistical significance, their interaction has an impact that is both economically and statistically significant.

In column (2), I add the village level controls described above. Although this inclusion increases the precision of the main estimate, while simultaneously reducing bias by controlling for potentially endogenous factors, it also lowers the sample size from 13,468 to 12,240. In column (3), I highlight the importance of including the interaction term in the model. Its omission would lead us to erroneously conclude that it is only ethnic salience that reduces teacher absenteeism within a district, rather than a combination of salience and ethnic diversity. In column (4), I present the most naïve representation of ethnic divisions, that is, ethnic diversity with no account for salience whatsoever. Again, though common in the literature, this measure fails to capture at

[17] Specifically, I use the output from a specification based on Table 2.5, column (6), but without the pre-colonial fixed effects, which I describe in the next sub-section. The effect of diversity for any level of salience can be found simply by plugging in the desired level of salience to the relevant regression output function. Figure 2.5 is a graphical representation of a function $TA_{idr} = 1.71 - 0.151(ELF_d) + 1.33(\sum_{i=1}^{n} \frac{ES_{id}}{n_d} * ELF_d) + \Psi \sum_{i=1}^{n} \frac{ES_{id}}{n_d} + \gamma X_{id} + \delta V_{id} + \eta R_r + e_{id}$, where $\sum_{i=1}^{n} \frac{ES_{id}}{n_d} = 0.33, 0.004$. The black dots represent observations plotted by ELF_d on the x-axis and TA_{idr} on the y-axis.

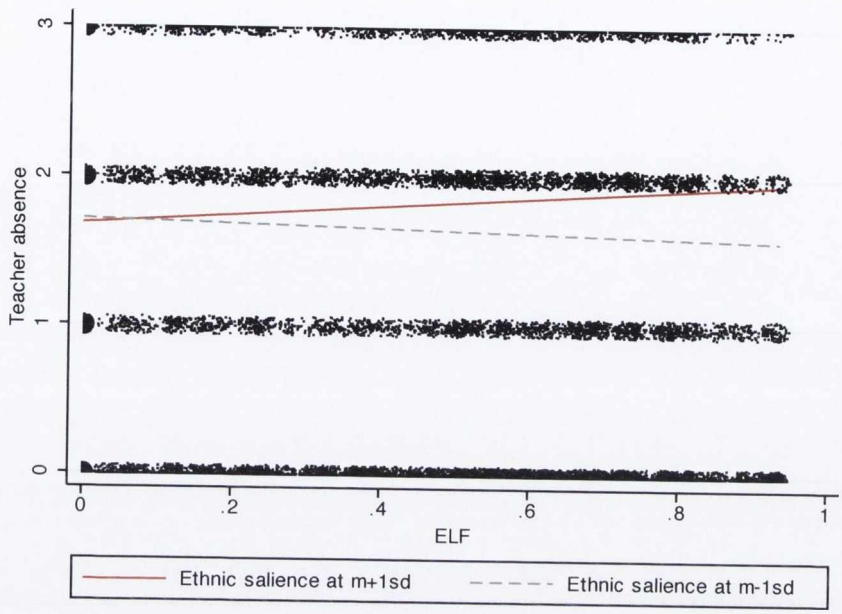


Figure 2.5: Teacher Absence and Ethnic Divisions

all the significant effect of ethnic divisions on the outcome.

Table 2.3: Ethnic Divisions and Teacher Absenteeism

	(1)	(2)	(3)	(4)
	Teacher absence: Afrobarometer			
ELF * Ethnic salience	1.008**	1.193***		
	(0.403)	(0.425)		
ELF	-0.002	-0.066	0.140	0.125
	(0.107)	(0.115)	(0.085)	(0.084)
Ethnic salience	-0.082	-0.073	0.319*	
	(0.166)	(0.178)	(0.187)	
Village controls	No	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	13,468	12,240	12,240	12,330
Number of clusters	318/1234	318/1234	318/1234	318/1234
R-squared	0.177	0.176	0.175	0.174

Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

For the remainder of this section, I test the robustness of the association by (i) controlling for an additional range of factors that are plausibly correlated with ethnic di-

visions and teacher absenteeism; (ii) conducting a set of falsification tests to ensure that the results are not being driven by an error component in the measurement of the dependent variable; and (iii) presenting alternative specifications to ensure that the results are not driven by particularities in the survey sample.

Controlling for Observables: Cultural and Institutional Persistence

Nunn and Wantchekon (2012) use Afrobarometer data to show how historical events can affect current behaviour through culture, or, more specifically, the intergenerational transmission of norms within ethnic groups. They show that members of ethnic groups that were historically targeted by the slave trades have lower levels of trust in institutions and other people today. This is due to the nature of the slave trade, which often rewarded trickery and dishonesty by sparing from slavery those who provided other people for export. This led to a profusion of distrust amongst the kin of those who were sold for export, which in turn developed into a cost-saving heuristic which evidently survived over time. The implication for this analysis is simple: certain ethnic groups may display common traits that could affect the salience of their ethnicity and the development of local institutions.

In Table 2.4, I show how the omission of ethnicity level covariates can lead to a biased estimate of β. The inclusion of three such variables in column (1) changes the size and statistical significance of the coefficient. The first variable is the natural log of the number of slave exports taken from the respondent's ethnicity group divided by the size of the area which it historically inhabited. It is taken directly from Nunn and Wantchekon (2012), who use Murdock (1959) to link current day ethnic group names to their pre-colonial ancestral groups. Historical slavery exports may affect current day social capital, which could be manifested in more salient sub-national identities and lower levels of local cooperation. The second variable is taken from the *Ethnographic Atlas* (Murdock 1967) and coded by Nunn and Wantchekon (2012). It captures the political sophistication of each ethnicity's corresponding pre-colonial ancestral group by measuring the number of hierarchical layers in its power structure. This organisation of power could itself persist over time within ethnic groups to reflect better local coordination today. The third variable is a proxy for each ethnicity's historical wealth by indicating whether or not there was a city within its pre-colonial boundaries in 1400. It is taken from Chandler (1987) and again coded by Nunn and Wantchekon (2012), and is intended to reflect the probability of colonial plunder, which may negatively affect the persistent quality of institutions within groups over time.[18]

[18] Acemoglu et al. (2001) provide some evidence on the colonial origins of institutional quality.

Table 2.4: Historical Controls

	(1)	(2)	(3)
	Teacher absence: Afrobarometer		
ELF * Ethnic salience	1.027**	1.306***	1.193**
	(0.474)	(0.465)	(0.533)
ELF	-0.018	-0.165	-0.153
	(0.129)	(0.116)	(0.134)
Ethnic salience	-0.049	-0.078	-0.075
	(0.184)	(0.202)	(0.229)
Slave exports	-0.025		
	(0.033)		
Pre-colonial juris. hierarchy	-0.061**		
	(0.026)		
Existence of city in 1400	0.091*		
	(0.047)		
Pre-colonial FE	No	No	Yes
Ethnicity FE	No	Yes	Yes
Village controls	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Observations	10,772	12,240	11,593
Number of clusters	318/1234	318/1234	318/1234
R-squared	0.185	0.204	0.228

Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Slavery enters the model with no statistical significance, while the sophistication of pre-colonial institutions and the indicator for pre-colonial wealth enter significantly with the expected signs. Taken together, these results suggest that controlling for ethnicity level variation in the response is a necessary step in establishing the robustness of the main finding. Accordingly, I include ethnicity fixed effects in column (2) and for the remainder of the analysis. I code ethnicity by country, in order to account for the variation in ethnic salience within ethnic groups that spill across country borders, as in Posner (2004a).

Although clearly a necessary inclusion, ethnicity fixed effects are not a sufficient means of controlling for variation in unobserved historical factors. Michalopoulos and Papaioannou (forthcoming) show that pre-colonial factors—in this case the jurisdictional hierarchy measure from column (1)—can also have persistent effects through

local institutions. Indeed Nunn and Wantchekon (2012) present evidence of this channel that is independent of the cultural persistence discussed above. Both studies use information on the pre-colonial boundaries of ethnic groups from Murdock (1959) to link historical factors to current outcomes. In the case of Nunn and Wantchekon (2012), this is facilitated by recording the geographic coordinates of each Afrobarometer district in order to determine the ethnic group that inhabited the corresponding area in pre-colonial era. I use this information to create vector of fixed effects that control for historical factors that vary at this level of pre-colonial settlement areas.[19]

Controlling for Observables: Conflict, Sorting and Settlement

I now consider three more factors that could plausibly affect the main result. First, I address the possibility that armed conflict is associated with salient ethnic divisions and with perceived teacher absence. Montalvo and Reynal-Querol (2005) provide empirical evidence for the link between ethnic polarisation and conflict, building on a seminal contribution from Horowitz (1985). It is not too far-fetched to consider that ethnic conflict and ethnic salience are related; nor is it implausible to suggest that teacher attendance—or indeed perceptions of teacher attendance—could be affected by local conflict.

To account for this, I turn to the Armed Conflict Location and Event Dataset (Acled), which contains data on over 60,000 fatal and non-fatal incidents of conflict throughout Africa, parts of Asia, and Haiti from 1997 to 2012.[20] The dataset also includes the geographic coordinates of each incident, which I use to measure its geodesic distance from the centroid of each Afrobarometer district.[21] I combine various levels of spatial and temporal proximity to the number of armed conflict events and to the number of fatalities associated with each event,[22] and find that only the number of conflict fatalities within 1 year and 1 kilometre from the centroid of each district has a significant impact on perceived teacher absenteeism. In columns (1) and (2) of Table 2.5, I show that the inclusion of both this measure and the corresponding measure of conflict events (fatal and non-fatal) has no qualitative effect on the main result.

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[19]Not every area could be linked to a precolonial group. I thus present results with and without these fixed effects for the remainder of these estimations.

[20]More information on the Acled dataset is available online at www.acleddata.com

[21]Sangnier and Zylberberg (2012) also combine Afrobarometer and Acled data using location coordinates.

[22]Specifically, I included all possible combinations between 20km, 10km, 5km and 1km, and 10 years, 5 years, 2 years, 1 years and six months.

Table 2.5: Conflict, Sorting and Settlement

	(1)	(2)	(3)	(4)	(5)	(6)
	Teacher absence: Afrobarometer					
ELF * Ethnic salience	1.299***	1.192**	1.307***	1.184**	1.344***	1.201**
	(0.462)	(0.532)	(0.463)	(0.527)	(0.481)	(0.529)
ELF	-0.157	-0.149	-0.154	-0.128	-0.176	-0.138
	(0.116)	(0.134)	(0.111)	(0.131)	(0.117)	(0.134)
Ethnic salience	-0.076	-0.077	-0.079	-0.077	-0.092	-0.067
	(0.201)	(0.228)	(0.200)	(0.227)	(0.205)	(0.226)
Conflicts within 1km & 1 year	-0.010	-0.008		-0.008		-0.008
	(0.010)	(0.007)		(0.007)		(0.007)
Fatalities within 1km & 1 year	0.005**	0.004***		0.004***		0.004***
	(0.002)	(0.001)		(0.001)		(0.001)
Share of historical migrants			0.025	0.049		0.052
			(0.057)	(0.066)		(0.068)
Distance to Addis Ababa (km)					-0.000	-0.000
					(0.000)	(0.000)
Latitude					0.010	0.013
					(0.023)	(0.026)
Distance to sea (km)					-0.000	-0.000
					(0.000)	(0.000)
Pre-colonial FE	No	Yes	No	Yes	No	Yes
Ethnicity FE	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,240	11,593	12,240	11,593	12,044	11,593
Number of clusters	318/1234	318/1234	318/1234	318/1234	318/1234	318/1234
R-squared	0.204	0.228	0.204	0.228	0.204	0.228

Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Although potentially captured by the covariates already presented in the model, I include a measure of historical sorting in columns (3) and (4). Residential sorting amongst ethnic groups, as I discuss above, may reflect traits that could impact collective action outcomes. For example, it is possible that individuals from different ethnic groups who have no animosity toward outsiders may coexist in districts, which itself may perpetuate long-term cooperation and better local institutions. To measure sorting, I go back to the information on pre-colonial boundaries in Murdock (1959). I define historical assimilation as the percentage of current district residents whose ethnic ancestors were based elsewhere during the pre-colonial era. Adding this variable to the model has no discernible effect on the main results.

Like sorting, it is likely that correlates of the 'evolutionary' sources of ethnic diversity are already controlled for by the regional and pre-colonial fixed effects in the model. To see if this is the case, I include three proxies for human settlement that are discussed by Alehrup and Olsson (2012) and Michalopolous (2012) in columns (5) and (6). I first use the geodesic distance from each district to Addis Ababa, which is also shown by Ashraf and Galor (forthcoming) to correlate highly with the duration of human settlement and, in turn, genetic diversity. In addition, I include distance to the equator (measured in degrees of latitude) and distance to the sea, which reflect two theories of early human migration patterns from East Africa between 150,000 and 200,000. As I mention above, I also include a binary variable for whether or not each village contains a tarred or paved road, which could be interpreted as a proxy for the ruggedness of land. None of the variables have a significant impact on the model.

Falsification Tests

In this section, I address the potential effects of non-classical measurement error in the dependent variable. To recount, Olken (2009) shows that survey respondents in heterogenous Indonesian villages overestimated corruption in a road project more than those in homogeneous areas. The author suggests that this was caused by a higher level of scepticism in diverse villages that may have been triggered through a feedback mechanism from corruption in previous projects. As a result, community meetings designed to facilitate local monitoring and oversight of the road project were 22% more highly attended than meetings in homogeneous areas, which in turn led to a lower level of actual corruption. The implication for this study is that respondents from ethnically divided areas may simply write-off the quality of all public services and collective action outcomes without regard to the true measure. This could lead to an upwardly biased β coefficient.

For the first set of falsification tests, I examine the impact of ethnic divisions on responses to alternative survey questions. In addition to absent teachers, respondents are also asked in the Afrobarometer survey whether they have encountered six other problems related to their local public schools in the previous 12 months. They are: "services are too expensive," "poor conditions of facilities," "overcrowded classrooms," "poor teaching," "lack of textbooks and other supplies," and "demands for illegal payments." The questions are framed in exactly the same manner as the way in which the dependent variable is framed, and all are sequenced together. If it is the case that residents of ethnically divided areas have a common proclivity to overstate problems with public goods provision, and if that is driving the main result of this section, then we should

observe a similar effect of the interaction term on all—or at least some—of the other six variables.

In Table 2.6, I present the effects of ethnic divisions on all seven variables, each normalised to have a mean value of 0 and a standard deviation of 1 for comparison.[23] Using the most comprehensive specification presented thus far (from Table 2.5, column (6)), I show that district-level ethnic divisions only have a significantly positive effect on teacher absenteeism.

Table 2.6: Falsification: School Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Perceived school problems (normalised)						
	Teacher abs.	Expensive	Facilities	Crowding	Teaching	Materials	Bribes
ELF * Ethnic salience	1.073**	-0.105	-0.575	-0.131	0.127	-0.725*	0.383
	(0.473)	(0.382)	(0.374)	(0.389)	(0.375)	(0.387)	(0.395)
ELF	-0.123	0.012	0.235**	0.064	0.070	0.098	-0.138
	(0.119)	(0.098)	(0.096)	(0.101)	(0.110)	(0.101)	(0.108)
Ethnic salience	-0.060	0.206	0.223	0.051	0.044	0.390*	0.096
	(0.202)	(0.168)	(0.202)	(0.207)	(0.161)	(0.211)	(0.206)
Spatial controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-colonial FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,593	12,071	11,589	11,634	11,479	11,754	11,568
Number of clusters	318/1234	318/1234	318/1234	318/1234	318/1234	318/1234	318/1234
R-squared	0.228	0.266	0.294	0.265	0.255	0.259	0.230
Intra-country corr.	0.077	0.092	0.122	0.118	0.103	0.088	0.074

Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

This comprehensively rules out the possibility that a common error component of all seven measures is significantly correlated to ethnic divisions. The tests also suggest that, whatever is the channel through which ethnic divisions ultimately affect teacher absenteeism, it does not apply to the other outcomes. The possible explanation for this may be reflected in the final row of the table, which shows that five of the six additional outcomes have higher within-country correlations than teacher absenteeism, albeit by small margins. This indicates that the organisation of these outcomes may take place

[23]This does not affect the interpretation of the test results.

at a more centralised level than teacher attendance.[24] As I discuss in the introduction, absenteeism is rarely sanctioned by official sources, and is thus highly likely to be affected by more local factors.

The next set of falsification tests follows a similar line of reasoning to a set presented by Olken (2009). It is based on a simple premise: if people in ethnically divided areas systematically overstate corruption, or any other metric representing the quality of public goods or collective action, then perceptions of common, national-level indicators should significantly differ between divided and undivided areas. If they do not overstate these measures, their responses should not significantly differ from those in undivided areas, as both groups are assessing the same phenomenon.

Table 2.7: Falsification: Country-Level Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Perceptions of country-level institutions					
	Corruption		Trust		Gov. performance	
	Government	President	Ruling party	Opposition	Corruption	Education
ELF * Ethnic salience	-0.093	-0.452	0.511	0.137	-0.452	0.081
	(0.386)	(0.343)	(0.403)	(0.358)	(0.391)	(0.273)
ELF	0.053	0.056	-0.102	0.023	0.141	-0.068
	(0.094)	(0.084)	(0.105)	(0.083)	(0.103)	(0.087)
Ethnic salience	-0.147	0.010	0.032	0.004	-0.247	-0.121
	(0.165)	(0.130)	(0.180)	(0.186)	(0.158)	(0.159)
Spatial controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-colonial FE	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity FE	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,324	13,943	16,595	16,126	15,857	17,051
Number of clusters	318/1234	318/1234	318/1234	318/1234	318/1234	318/1234
R-squared	0.221	0.266	0.294	0.167	0.226	0.257

Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

I run this test using six variables that measure responses to questions about national-level governance. The results are presented in Table 2.7. In columns (1) and (2), the dependent variables measure perceived corruption in the offices of government and the president respectively; in columns (2) and (3) the dependent variables measure

[24]The only outcome with a lower intra-country correlation, the demand for illegal payments, is positively affected by ethnic divisions in a specification with no pre-colonial fixed effects (not reported).

the trust held by respondents in the ruling party and in the main opposition party respectively; and in columns (5) and (6) the dependent variables measure respondents' assessment of the manner in which the government is handling corruption and education respectively. All six dependent variables are measured on four point scales.

In the case of every dependent variable, the responses of individuals in ethnically divided (or indeed ethnically diverse or ethnically salient) districts does not differ from the responses of those in undivided areas. It provides further evidence that the effect of ethnic divisions on perceived teacher absenteeism cannot be explained by this source of measurement error.[25]

In Table 2.8, I present the final set of tests for the robustness of the results to non-classical measurement error in the dependent variable. I first test the hypothesis that only minority group members in each district have higher perceptions of teacher absenteeism, and that this is the reason for the main results. I do this by including a binary variable that indicates whether or not an individual is a member of a non-modal group within their district. The result, shown in column (1), indicates that minority group members are not driving the main findings.

In columns (2) and (3) I present the results of a test which attempts to hold the true level of teacher absenteeism constant, and therefore analysing only the variation in the individual error component. Recall that $TA_{id} = TA^*_{sd} + u_i$. If I hold TA^*_{sd} constant by controlling for school fixed effects, I can then observe the relationship between ethnic salience and the error component only.

As there are no school-level fixed effects in the dataset, I instead use village-level fixed effects. The village (or "primary sample unit") is the most granular level above the individual in the Afrobarometer. It has a modal sample size of 8 respondents. To the extent that all 8 refer to the same school in these surveys, controlling for village fixed effects will allow me to uncover the statistical relationship between the error component of the dependent variable and an assortment of individual characteristics.

In column (2) I provide strong evidence that individual ethnic salience is not related to the individual error component of the dependent variable. In column (3) I include an interaction term between individual salience and district level ELF. Al-

[25]Readers familiar with the Afrobarometer surveys will be aware of several questions that probe respondents for their opinion on a multitude of political and social issues. I chose questions that were most likely to elicit views on strictly national-level characteristics that plausibly affect individuals in diverse and homogeneous areas equally. Nevertheless, I could have presented alternative variables in each of the three categories shown in the Table 2.7 that arguably fit the criteria. Under corruption, respondents were also asked about judges and magistrates; under trust, respondents were also asked about parliament, the president, the national elections commission and courts of law; and under government performance, respondents were also asked about crime, health, water and food. The only question for which respondents in divided areas answered differently to others was on the government's handling of crime, which itself is likely to be influenced at least partially by local factors.

though the inclusion of village fixed effects implies that district-level ethnic diversity is held constant, this is still a strictly better test than the one presented in column (2), as the interaction term gives a closer approximation to the true district-level interaction effect in a test without fixed effects. Again, the results suggest that there is no significant relationship between ethnic divisions and the error component of the dependent variable.

Table 2.8: Falsification: Other Tests

	(1) Teacher absence	(2) Teacher absence	(3) Teacher absence	(4) Community meetings
ELF * Ethnic salience (district)	1.348**			-0.102
	(0.549)			(0.365)
ELF	-0.175			0.075
	(0.139)			(0.112)
Ethnic salience	-0.110			0.139
	(0.636)			(0.232)
Minority	0.023			
	(0.597)			
ELF * Ethnic salience (individual)			0.830	
			(0.734)	
Ethnic salience (individual)	0.045	0.064	0.728	-0.098**
	(0.053)	(0.047)	(0.719)	(0.039)
Village FE	No	Yes	Yes	No
Spatial controls	Yes	N/a	N/a	Yes
Pre-colonial FE	Yes	N/a	N/a	Yes
Ethnicity FE	Yes	Yes	Yes	Yes
District controls	Yes	N/a	N/a	Yes
Individual controls	Yes	Yes	Yes	Yes
Region FE	Yes	N/a	N/a	Yes
Observations	11,000	12,974	12,974	17,359
Number of clusters	318/1234	289	289	318/1234
R-squared	0.225	0.396	0.396	0.255

Standard errors in column (1) and column (4) are adjusted for two-way clustering within ethnic groups and within districts. Standard errors in column (2) and column (3) are adjusted for clustering at the ethnicity level. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Finally, I show that the explanation put forward by Olken (2009) for the link between ethnic diversity and the overestimation of corruption is not applicable in this context. Recall that, in Indonesia, attendance rates at community oversight meetings were significantly higher in heterogeneous communities, which led to lower levels of

actual corruption. A key part of that story lies in the fact that these meetings were facilitated as part of the nationwide road building project. It is unclear that heterogeneous communities would have monitored the project as effectively in the absence of this exogenous provision of community fora. I show in column (4) that, in this sample, members of ethnically divided communities do not attend community meetings more frequently than those in relatively undivided communities.[26]

Other Robustness Checks: Sample Issues

In this sub-section, I present a final set of robustness tests for the multi-country analysis. In column (1) of Table 2.9, I use district-level averages for all variables to show that missing values for some covariates are not affecting the estimation of β.

In column (2), I run the analysis on a sub-sample of respondents who might be expected to have a better grasp of true teacher absenteeism TA^*_{sd}: mothers. Case and Ardington (2006) show using panel data that maternal deaths had a substantially more negative effect on a wide range of schooling outcomes than paternal deaths amongst a sample of Zulu children in South Africa, while paternal deaths had a larger impact on other socio-economic factors. This could be reflective of a higher maternal involvement in children's schooling. In such a case, the absence of a significant interaction effect in a sub-sample of women between the ages of 25 and 50 would cast some doubt on the validity of the results, as one would expect mothers to have a more accurate response to the question on perceived teacher absenteeism. The results show that the interaction effect is even larger, perhaps indicating that the rest of the population sample underestimates true absenteeism.

In columns (3) and (4), I provide evidence that the results are not driven by ELF measures that are calculated from small sample sizes, and thus have little statistical power. Indeed, as the table shows, the results are more robust for districts that have above-median sample sizes than districts with below-median sample sizes. This may have implications for establishing the channels of causation, which I discuss in Section 6.

[26]Respondents are asked if they attend community meetings. The dependent variable ranges on a four point scale from "[n]o, would never do this" to "[y]es, often"

Table 2.9: Robustness: Sample Issues

	(1)	(2)	(3)	(4)
	Teacher absence: Afrobarometer			
	District average	Women 25–50	District sample size < Med.	District sample size > Med.
ELF * Ethnic salience	1.218**	2.549***	0.657	3.668*
	(0.564)	(0.816)	(0.610)	(2.101)
ELF	-0.126	-0.263	-0.142	-0.121
	(0.194)	(0.220)	(0.157)	(0.472)
Ethnic salience	0.089	-0.807**	0.025	-0.933
	(0.219)	(0.356)	(0.220)	(1.157)
Spatial controls	Yes	Yes	Yes	Yes
Pre-colonial FE	Yes	Yes	Yes	Yes
Ethnicity FE	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	20,600	3,611	5,499	6,094
Number of clusters	318/1234	274/1149	254/1036	264/200
R-squared	0.868	0.335	0.266	0.260

Pre-colonial fixed effects, ethnicity fixed effects, and individual controls are included as district-level means in column (1). Standard errors are adjusted for two-way clustering within ethnic groups and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

In summary, this section presents robust evidence that the statistical association between ethnic divisions and perceived teacher absenteeism in the Afrobarometer multi-country dataset is neither caused by omitted variables nor an error component in the dependent variable. In the next section, I extend the results to a sample using objective data on recorded absenteeism in Uganda.

2.4 Estimation: Ugandan Dataset

Kremer et al (2005) and Habyarimana (2010) analyse the determinants of teacher absenteeism using sub-samples of the Chaudhury et al (2006) data collected during random, unannounced school visits in 2002 and 2003. Here, I use an almost identical empirical specification in order to establish the relationship between the probability of a teacher's absence and ethnic divisions in Uganda. The basic estimation equations

take the following form:

$$TA_{jsd} = a + \Psi ES_d + \lambda ELF_d + \beta_d(ES_d * ELF_d) + \gamma X_{jsd} + \delta S_{sd} + \eta T_{tdm} + e_{id}, \quad (2.2)$$

and

$$TA_{jsd} = a + \Psi ES_d + \lambda ELF_s + \beta_s(ES_d * ELF_s) + \gamma X_{jsd} + \delta S_{sd} + \eta T_{tdm} + e_{id}, \quad (2.3)$$

where ES_d is $\sum_{i=1}^{n} \frac{ES_{id}}{n_d}$ from the Afrobarometer sample, ELF_d is the district-level ethnic diversity, and ELF_S is school-level linguistic diversity, as described in Section 3.1. Teacher-level characteristics, represented by X_{jsd}, include gender, age, marital status, education, place of birth, employment rank, experience, contract status, union status, and career training; S_{sd} is a set of school level characteristics, comprised of controls for institutional features, such as the existence of parent teacher associations (PTAs), access and the quality of its facilities; T_{tdm} is three sets of fixed effects for the time, day and month of the visit. All covariates are described in more detail in the notes beneath Table 2.10. I estimate each equation using linear probability and probit estimators, given that the dependent variable is now binary.

2.4.1 Results

I present in Table 2.10 the estimation results for equations (2) and (3). In columns (1) and (2) I omit school-level characteristics, which are added in columns (3) and (4). All four specifications are estimated with a linear probability estimator. In each model, the interaction effect is large and significant. To illustrate, a one standard deviation increase in district-level ethnic diversity at a high level of ethnic salience (again defined as the mean plus one standard deviation) raises the probability of a teacher not turning up to a scheduled class by 9.3 percentage points. At a low level, the same increase of diversity actually decreases the probability of absence by 6.7 percentage points. An even larger interaction effect is present in the school-level diversity data: a one standard deviation increase in ELF_s at a high level of ethnic salience increases the probability of absence by 3.8 percentage points; while at a low level of salience the same change in diversity decreases the probability of absence by almost 17.7 percentage points.

In columns (4) and (5), I present results from the probit model. Ai and Norton (2003) highlight the dangers of misinterpreting the effects of an interaction term in non-linear models. They show how the marginal effect of an interaction term can have a different magnitude, sign and level of statistical significance than the true cross-partial

derivative.[27] The results of their corrected method, shown in the table, support the linear results.[28]

It is possible that these symmetrical effects, i.e., the negative effects of district- and school-level diversity on absenteeism where salience is low, reflect two forms of sorting mechanisms that are described respectively by Glennerster et al. (2012) and Miguel and Gugerty (2005). The first case concerns residential sorting, whereby certain coethnics choose to live in clusters. By implication, the remaining individuals—those who do *not* cluster by ethnicity—form more diverse communities that are likely to have low levels of ethnic salience. Glennerster et al. (2012) find that 'non-sorters' in diverse areas tend to have higher levels of educational attainment, which I find in the analysis to be strongly linked with lower absenteeism. Moreover, we can see from Figures 2.2 and 2.6 that Uganda has one of the highest rates of sorting amongst the entire 16 country sample: the mean level of ethnic diversity by district is less than half the mean level of ethnic diversity for the country as a whole.

The symmetrical effect at the school-level may be further compounded by a more straightforward sorting mechanism described in Miguel and Gugerty (2005), whereby good schools (which are likely to have teachers who attend class) attract pupils from a wider radius, leading to a positive link between diversity and, in this case, teacher attendance. It is interesting to note also that teacher absenteeism is lower in homogeneous areas where people express a high sense of common ethnic identity, as implied by the significantly negative independent effect of ethnic salience. Again, sorting may explain the finding, as homogeneous communities formed from deliberate sorting may be better equipped for the type of collective action needed to provide local public goods than other homogeneous communities.

In any case, the impact of ethnic diversity on teacher absenteeism at a high level of ethnic salience is positive and significant in all specifications. Whether measured at the district level or at the school level, the effects of ethnic diversity on the probability of a teacher being absent from class is significantly higher in districts where individuals are more likely to identify themselves along ethnic lines.

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[27] I explain this issue in more detail in the appendix of this chapter.

[28] The Ai and Norton (2003) method requires that I drop the fixed effects for the time of day from the model. Making the same adjustment to the linear model produces quantitively similar results, supporting the interpretation presented above. The remaining marginal effects are taken from the full model.

Table 2.10: Ethnic Divisions and Teacher Absenteeism: Ugandan School Visits

	(1)	(2)	(3)	(4)	(5)	(6)
	Teacher absence: school visits					
					Probit	
					Ai & Norton (2003) Interaction effect	
ELF (District) * Eth. salience	4.410***		4.115***		2.36**	
	(1.477)		(1.429)		(1.019)	
ELF (School) * Ethnic salience		4.901***		5.253***		2.43**
		(1.288)		(1.343)		(0.979)
					Marginal effects	
ELF (District)	-0.818***		-0.766***		-0.744	
	(0.272)		(0.287)		(0.463)	
ELF (School)		-1.327***		-1.393***		-1.542**
		(0.338)		(0.349)		(0.722)
Ethnic salience	-2.229***	-2.145***	-1.966***	-2.127***	-1.392**	-1.644**
	(0.454)	(0.446)	(0.462)	(0.529)	(0.596)	(0.701)
Teacher demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes	Yes	Yes
Teacher employment characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	No	No	Yes	Yes	Yes	Yes
School and location controls	No	No	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,686	1,686	1,594	1,594	1,400	1,400
Number of clusters	94/10	94/10	94/10	94/10	83	83
R-squared	0.248	0.253	0.252	0.256	0.223	0.230

Teacher demographic controls include: gender, age, marital status, a dummy variable indicating completion of A-levels (high school final exams), and a dummy variable indicating place of birth (this district or another district); Teacher's rank is a categorical variable indicating the following ranks: deputy head, head of department, permanent teacher, private teacher, temporary teacher, volunteer teacher, and 'other'. The omitted category is head teacher. Teacher's employment characteristics include: duration of teaching career; duration of tenure at current school, and dummy variables indicating full-time status, membership of a union, and attendance of a teacher training program in the previous year. Institutional controls include dummy variables indicating the existence of a Parent Teacher Association (PTA), a categorical variable indicating the time lapsed since the last meeting, and dummy variables indicating an official inspection in the previous six months and the existence of a local means of recognition for good teachers. School and location controls include a set of dummy variables to indicate the existence of the following facilities: covered classrooms, non-dirt classroom floors, a toilet/latrine, drinking water and electricity; as well as the pupil-teacher ratio, average education levels of parents, and dummy variables indicating that the school is public, that it practices multi-grade teaching, whether it is within five kilometres of a paved road and whether it is in a rural location. Column (5) and (6) present marginal effects from a Probit regression. Standard errors in columns (1) to (4) are adjusted for two-way clustering within schools and within districts. Standard errors in column (5) and column (6) are adjusted for clustering at the school level. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

2.5 Testing for Channels of Causality

In this section, I make a preliminary attempt to identify the channel or channels through which ethnic divisions affect teacher absenteeism using the Ugandan teacher-level data. As I discuss in the introduction, ethnic diversity can affect local cooperation in different ways. First, the credibility of the threat of social sanctions is lower in ethnically diverse communities (Miguel and Gugerty, 2005; Habyarimana et al., 2007, 2009). In the absence of well functioning formal institutions, coethnics often rely on each other for public goods, credit or other services. Because of this, the costs of uncooperative behaviour towards a coethnic are likely to be higher than the costs of uncooperative behaviour towards a non-coethnic. In the case of teacher absenteeism this could have any or both of two consequences: (i) 'outsider' (or non-native) absent teachers may not take the threat of sanctions from native (i.e. non-coethnic) parents, communities or head teachers credibly; and (ii) diverse communities may lack the capacity for coordination needed to develop institutions of oversight for all teachers, such as effective parent teacher associations or other effective means of sanctioning. Any of these channels could be driving the association between ethnic divisions and teacher absenteeism.

A second mechanism regards the possibility of taste-based discrimination towards non-coethnics. Coethnics may have a higher concern for the utility of each other than for the utility of non-coethnics (Becker, 1957, 1974; Hjort, 2012). This could result in teachers in homogeneous areas having a higher preference for increasing the wellbeing of pupils and parents than teachers in ethnically divided areas, or 'outsider' teachers. Again, this would explain higher absenteeism rates in ethnically divided schools or districts.

I test for the existence of these channels in the next two sub-sections using data on sanctioning institutions and teacher ethnicity. I begin by analysing the effects of non-coethnicity on the probability of absence, which could reflect either channel. I then look at the effects of sanctioning institutions, both on average and conditional on non-coethnicity, which will allow for a disentangling of the stories.

2.5.1 Teacher Coethnicity

In this sub-section, I test the hypothesis that native teachers are less likely than 'outsider' teachers (or non-coethnics) to be absent from a class that they are scheduled to teach. If this is the case, it may be reflective of either channel: natives may view the threat of sanctioning more credibly; or natives may have a higher degree of altruism (or 'other-regarding' preferences) toward children and parents. If I find no evidence

of a statistically significant relationship, it suggests that the 'taste' mechanism can be rejected as a unique explanation for the link between ethnic divisions and teacher absenteeism.

I use two variables to measure whether or not a teacher is native. The first is a dummy variable indicating whether or not a teacher speaks the local language natively. The second is a dummy variable which indicates that the teacher was born in the local county. In all specifications, I include as a control a categorical variable for where the teacher currently lives: the included categories are the local county and the local district; the omitted category is the local village.

In Table 2.11 and Table 2.12, I present the results using the district level and school level measures of ethnic diversity respectively. Firstly, it is important to note that the interaction effect representing ethnic divisions is large and statistically significant in all specifications, ruling out the possibility that the full effect is explained by teacher-level ethnicity. In columns (1) and (2) of both tables, I show that native teachers are no more likely to be absent than outsiders. This indicates that teachers do not make decisions about attendance based on discriminatory altruism toward coethnics. In columns (3) and (4), I test for the heterogeneous effects of coethnicity between ethnically divided and undivided districts/schools by including a triple interaction term. This is to ensure that the statistically insignificant average effects presented in columns (1) and (2) are not masking significantly contrasting effects across districts or schools. The intuition is that non-native teachers in otherwise homogeneous areas may have selected into the district or school because they are not discriminatory. I also include triple interactions between the teacher's current home and the ethnic division components as controls. As all four specifications show, it is unlikely that non-local teachers are absent more than local teachers either in divided or undivided districts and schools.

Table 2.11: Channels: Teacher Coethnicity (ELF_d)

	(1)	(2)	(3)	(4)
	Teacher absence: school visits			
ELF (District) * Ethnic salience	4.132***	4.393***	4.089**	3.879**
	(1.517)	(1.462)	(1.775)	(1.593)
ELF (District)	-0.866***	-0.814***	-0.746**	-0.666**
	(0.295)	(0.289)	(0.357)	(0.306)
Ethnic salience	-2.252***	-1.995***	-1.794***	-2.017***
	(0.505)	(0.475)	(0.656)	(0.552)
Native lang.	0.009		0.216***	
	(0.031)		(0.073)	
Native birth		-0.041		-0.086**
		(0.034)		(0.041)
Live: this county	0.040	0.027	-0.038**	-0.003
	(0.045)	(0.030)	(0.015)	(0.028)
Live: this district	-0.029	-0.052**	0.039	-0.036
	(0.021)	(0.024)	(0.037)	(0.042)
Native lang. * ELF (District) * Ethnic salience			0.329	
			(1.027)	
Native birth * ELF (District) * Ethnic salience				-1.604
				(2.008)
Live: this county * ELF (District) * Ethnic salience			-3.313***	0.320
			(1.253)	(0.639)
Live: this district * ELF (District) * Ethnic salience			0.552	1.085
			(0.939)	(1.073)
Teacher demographic controls	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes
Teacher employment characteristics	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
School and location controls	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	911	1,594	911	1,594
Number of clusters	94/10	94/10	94/10	94/10
R-squared	0.428	0.260	0.438	0.262

Models in column (3) and column (4) include all component double interaction terms. Omitted categories are Born: this parish/city and Live: this parish/city. Standard errors are adjusted for two-way clustering within schools and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 2.12: Channels: Teacher Coethnicity (ELF_s)

	(1)	(2)	(3)	(4)
	Teacher absence: school visits			
ELF (School) * Ethnic salience	5.521***	5.613***	5.897***	5.503***
	(1.698)	(1.365)	(2.009)	(1.579)
ELF (School)	-1.381***	-1.459***	-1.332**	-1.354***
	(0.471)	(0.350)	(0.539)	(0.392)
Ethnic salience	-2.505***	-2.163***	-2.318***	-2.274***
	(0.513)	(0.527)	(0.577)	(0.629)
Native lang.	0.010		0.114***	
	(0.031)		(0.029)	
Native birth		-0.042		-0.026
		(0.036)		(0.031)
Live: this county	0.043	0.029	-0.001	0.008
	(0.044)	(0.028)	(0.034)	(0.041)
Live: this district	-0.036	-0.056**	0.051*	-0.054**
	(0.022)	(0.024)	(0.031)	(0.027)
Native lang. * ELF (School) * Ethnic salience			-1.040	
			(1.069)	
Native birth * ELF (School) * Ethnic salience				1.094
				(1.608)
Live: this county * ELF (School) * Ethnic salience			-0.825	0.537
			(1.096)	(0.981)
Live: this district * ELF (School) * Ethnic salience			0.970	0.398
			(1.040)	(0.875)
Teacher demographic controls	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes
Teacher employment characteristics	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
School and location controls	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	911	1,594	911	1,594
Number of clusters	94/10	94/10	94/10	94/10
R-squared	0.433	0.266	0.442	0.268

Models in column (3) and column (4) include all component double interaction terms. Omitted categories are Born: this parish/city and Live: this parish/city. Standard errors are adjusted for two-way clustering within schools and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

2.5.2 Sanctioning Institutions

The possibility remains that ethnic diversity may affect teacher absenteeism through its impact on either the effectiveness of sanctioning institutions or on the credibility of those institutions' threats in the eyes of 'outsiders' only. In Table 2.13 I show the effects of parent teacher associations, school inspections and the previous sanctioning behaviour of head teachers on teacher absenteeism. In columns (1) and (2), I present models with no sanctioning variables for comparison. In columns (2) and (3), I include a dummy variable for the existence of a parent teacher association, a categorical variable to indicate its last meeting (omitted variable is "this month"), and a dummy variable to indicate that an inspection by the education ministry had occurred within the previous six months. In columns (5) and (6), I include a dummy variable to indicate that the head teacher has previously sanctioned a teacher for absence by either dismissing, suspending or transferring her. I present this in a separate set of specifications due to the obvious likelihood of endogeneity, as head teachers in schools with no absenteeism are not required to sanction teachers.

The first point to note from the table is that the average effects of these institutions do not represent likely candidates for the channel of causation from ethnic divisions to teacher absenteeism. Across all six columns, the main interaction effect is positive, significant and stable, providing indirect evidence that the sanctioning variables are not associated with ethnic divisions.

The other point to note is that the temporal proximity to the most recent PTA meeting is the only one of these variables that seems to reduce absenteeism significantly, perhaps reflecting the functionality and effectiveness of the association. There is evidence also that the sanctioning history of head teachers is significantly endogenous.

In Table 2.14, I present the effects of these sanctioning institutions conditional on whether or not a teacher is native by language (columns (1) and (2)) or by birth (columns (3) and (4)). To recount: the threat of sanctions by PTAs, local ministry officials or head teachers may be only credible to coethnic teachers, as non-coethnic teachers may be reliant on other groups for public goods. If this is the case, the lack of significance in the average effects presented in Table 12 may be masking significant heterogeneous effects.

Again, in all four specifications, the main interaction effect is positive, significant and stable. There is suggestive evidence that the effect of head teachers sanctions has a significantly negative effect on absenteeism for the case of natives by birth, providing some support for the hypothesis. It is not enough to explain the main results, however.

Taken together, these results indicate strongly that ethnic divisions do not affect teacher absenteeism though either the credibility of social sanctions or differential al-

truism of teachers towards coethnics.

Table 2.13: Channels: Sanctioning Institutions

	(1)	(2)	(3)	(4)	(5)	(6)
	Teacher absence: school visits					
ELF (District) * Ethnic salience	4.034***		4.115***		4.084***	
	(1.421)		(1.429)		(1.385)	
ELF (School) * Ethnic salience		5.057***		5.253***		5.367***
		(1.433)		(1.343)		(1.180)
ELF (District)	-0.731**		-0.766***		-0.753***	
	(0.301)		(0.287)		(0.277)	
ELF (School)		-1.316***		-1.393***		-1.436***
		(0.364)		(0.349)		(0.308)
Ethnic salience	-1.834***	-1.925***	-1.966***	-2.127***	-1.957***	-2.146***
	(0.462)	(0.521)	(0.462)	(0.529)	(0.436)	(0.466)
PTA			0.030	0.028	0.043	0.049
			(0.103)	(0.102)	(0.102)	(0.097)
Last PTA meet: last month			0.008	-0.029	0.005	-0.037
			(0.119)	(0.107)	(0.118)	(0.107)
Last PTA meet: in last six months			-0.131	-0.153*	-0.133	-0.159**
			(0.087)	(0.080)	(0.086)	(0.080)
Last PTA meet: in last year			-0.180**	-0.183**	-0.184***	-0.190**
			(0.071)	(0.074)	(0.072)	(0.074)
Last PTA meet: more than year ago			-0.070	-0.065	-0.077	-0.076
			(0.084)	(0.086)	(0.087)	(0.087)
Recent inspection			0.028	0.050	0.030	0.056
			(0.048)	(0.046)	(0.047)	(0.044)
Head teacher has sanctioned					0.066	0.106**
					(0.065)	(0.052)
Teacher demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes	Yes	Yes
Teacher employment characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes
School and location controls	Yes	Yes	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,600	1,600	1,594	1,594	1,594	1,594
Number of clusters	94/10	94/10	94/10	94/10	94/10	94/10
R-squared	0.249	0.253	0.258	0.263	0.258	0.264

Standard errors are adjusted for two-way clustering within schools and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 2.14: Channels: Sanctioning Institutions by Coethnicity

	(1)	(2)	(3)	(4)
	Teacher absence: school visits			
ELF (District) * Ethnic salience	3.820***		3.962***	
	(1.478)		(1.448)	
ELF (School) * Ethnic salience		5.576***		5.397***
		(1.504)		(1.379)
ELF (District)	-0.931***		-0.721***	
	(0.263)		(0.297)	
ELF (School)		-1.440***		-1.452***
		(0.421)		(0.359)
Ethnic salience	-2.133***	-2.541***	-1.895***	-2.119***
	(0.517)	(0.475)	(0.472)	(0.512)
Last PTA meet: in last month	-0.092	-0.085	-0.033	-0.074
	(0.163)	(0.155)	(0.107)	(0.094)
Last PTA meet: in last six months	-0.196**	-0.186***	-0.171*	-0.197**
	(0.078)	(0.069)	(0.092)	(0.084)
Last PTA meet: in last year	-0.635***	-0.629***	-0.200***	-0.205***
	(0.117)	(0.088)	(0.072)	(0.075)
Last PTA meet: more than year ago	-0.216***	-0.226***	-0.096	-0.093
	(0.061)	(0.074)	(0.087)	(0.087)
Recent inspection	0.088	0.128	0.013	0.039
	(0.071)	(0.081)	(0.051)	(0.046)
Head teacher has sanctioned	0.116	0.155**	0.045	0.088*
	(0.106)	(0.069)	(0.059)	(0.047)
	Native by language		Native by birth	
Native	0.024	0.036	0.033	0.046
	(0.038)	(0.037)	(0.090)	(0.091)
Native * Recent inspection	-0.016	-0.021	-0.088	-0.108
	(0.041)	(0.047)	(0.106)	(0.106)
Native * Head teacher has sanctioned	-0.091	-0.089	-0.159*	-0.157**
	(0.117)	(0.092)	(0.092)	(0.079)
Native * Last PTA meet: in last month	-0.085**	-0.074	0.189	0.183
	(0.042)	(0.050)	(0.167)	(0.170)
Native * Last PTA meet: in last six months	0.080	0.036	-0.044	-0.033
	(0.130)	(0.126)	(0.151)	(0.142)
Native * Last PTA meet: in last year	-0.036	-0.046	-0.089	-0.099
	(0.058)	(0.055)	(0.084)	(0.080)
Native * Last PTA meet: more than year ago	0.267*	0.240	0.162	0.158
	(0.158)	(0.157)	(0.114)	(0.107)
Teacher demographic controls	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes
Teacher employment characteristics	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
School and location controls	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Observations	911	911	1,594	1,594
Number of clusters	94/10	94/10	94/10	94/10
R-squared	0.431	0.435	0.271	0.277

Standard errors are adjusted for two-way clustering within schools and within districts. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

2.5.3 Social Networks Between Teachers

While the dominant theories behind local collective action failures do not appear to explain teacher absenteeism through the effectiveness of local sanctioning institutions or through teacher ethnicity, there is one more intermediate variable that can shed light on the mechanism: social capital amongst teachers within a school.

Alesina and La Ferrara (2000) provide theoretical and empirical evidence that group formation and social participation is lower in ethnically diverse areas. It is plausible that teachers who have fewer social ties to their colleagues will feel less obliged to attend school solely in order to prevent their colleagues from having to manage unattended pupils.

To explore this possibility, I use a measure of social capital amongst teachers in each school that is based on answers to the following survey question given by the head teacher of each school:

> When was the last time that the teachers in your school socialized with each other outside of school hours, gathering for a meal or party for example?

I let "never" equal 0, and all other responses equal 1. The responses almost split the sample evenly: 54% percent of head teachers report that teachers in their school never socialise together, while 46% report that they do.

I present the average effect of this measure—which I name *Social*—in columns (1) and (2) of Table 2.15. Moving from a school where teachers never socialise to one in which they have socialised at least once in the memory of the head teacher decreases the probability of a teacher's absence by between 11.3% and 12.2%. The inclusion of this variable in the model eliminates entirely the significance of the ethnic divisions using the district level measure, while also reducing substantially the effect of ethnic divisions as measured at the school level. This implies that ethnic divisions increase teacher absenteeism primarily through their effects on social networks between teachers within schools.

In columns (3) and (4), I include triple interaction terms comprised of *Social* and the main ethnic divisions interactions. The results show that the effect on absenteeism of active social networks between teachers is not significantly different in ethnically divided areas. In columns (5)-(8), I examine whether or not these effects depend on the ethnicity of the teacher. I find no significant heterogeneous effect when I use the language-based variable as a proxy for coethnicity. However, when I define a native teacher as one who was born in the immediate area, I find that the interaction term accounts entirely for the effect of social participation on absenteeism. Locally native

teachers in schools where colleagues socialise have lower rates of absenteeism than teachers who were born elsewhere.

Table 2.15: Channels: Teacher Social Networks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teacher absence: school visits							
	Average effect		Triple interaction		Coethnicity interaction			
ELF (Dist.) * Eth. sal.	2.140		0.473		0.225		2.121	
	(1.790)		(2.162)		(1.654)		(1.774)	
ELF (Sch.) * Eth. sal.		3.346*		1.985		3.788**		3.315*
		(1.726)		(2.668)		(1.754)		(1.717)
ELF (Dist.)	-0.277		0.519		0.038		-0.280	
	(0.362)		(0.429)		(0.459)		(0.358)	
ELF (Sch.)		-0.892**		-0.595		-0.787*		-0.882**
		(0.412)		(0.698)		(0.470)		(0.410)
Eth. sal.	-1.404***	-1.573***	-0.842	-1.267**	-1.531***	-2.145***	-1.401***	-1.571***
	(0.489)	(0.497)	(0.784)	(0.512)	(0.318)	(0.232)	(0.483)	(0.493)
Social	-0.122**	-0.113**	-0.375**	-0.191	-0.132***	-0.025	-0.084	-0.078
	(0.049)	(0.052)	(0.152)	(0.137)	(0.086)	(0.065)	(0.072)	(0.274)
Social * ELF (D) * Eth. sal.			-2.202					
			(2.405)					
Social * ELF (D)			1.452***					
			(0.377)					
Social * ELF (S) * Eth. sal.				-2.143				
				(3.557)				
Social * ELF (S)				0.481				
				(0.935)				
Social * Eth. sal.			-0.056	0.378				
			(1.009)	(0.581)				
Social * Native lang.					-0.056	0.051		
					(0.046)	(0.069)		
Native lang.					-0.043	-0.030		
					(0.048)	(0.047)		
Social * Native born							-0.123**	-0.116**
							(0.053)	(0.049)
Native born	-0.013	-0.014	-0.016	-0.016			0.006	0.004
	(0.042)	(0.043)	(0.042)	(0.044)			(0.037)	(0.035)
Teacher demog. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher rank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher employ. chars.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School, location controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time of Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,482	1,482	1,482	1,482	803	803	1,482	1,482
Number of clusters	94/10	94/10	94/10	94/10	94/10	94/10	94/10	94/10
R-squared	0.258	0.261	0.265	0.261	0.426	0.429	0.258	0.261

Standard errors are adjusted for two-way clustering within schools and within districts. ***Significant at the 1% lev
**Significant at the 5% level; *Significant at the 10% level.

Our ability to interpret these findings is limited without knowing the composition of the social groups to which the head teacher refers; or indeed without knowing the extent to which social capital is endogenous in the model.[29] The story may be that teachers who socialise together are altruistic towards each other, and have an added incentive to attend school either to prevent a colleague from having to work harder or simply to socialise more. Or it could simply be a manifestation of the social sanctioning mechanism at play, whereby absent teachers face ostracism or other forms of punishment from their social group.

What we can say with more clarity is that the association between ethnic divisions and active social networks amongst teachers is likely to play some role in our understanding of teacher absenteeism. Moreover, we can say with higher confidence that the role of ethnic divisions in either the formation or the consequences of monitoring institutions, like parent teacher associations, is inconsequential.

These tentative findings serve as a clear invitation for experimental research in the field, where innovative measures of social capital and social sanctions in particular could allow us to delve further into the relationship between ethnic divisions and teacher absenteeism.

2.6 Conclusion

In this paper I present robust evidence of a link between ethnic divisions and teacher absenteeism using district level data from two sources: a nationally representative series of random, unannounced school visits in Uganda; and a large opinion survey of citizens in 16 sub-Saharan African countries. The results are robust to a comprehensive set of individual, geographic, institutional and historical controls. I introduce a new measure of ethnic divisions which captures both ethnic diversity and the salience of ethnic identification. Using ethnic diversity alone, a practice common in the literature, would lead to a significant underestimation of the true effect. Across a range of specifications, and using various combinations of data, I find that ethnic diversity increases teacher absenteeism at high levels of ethnic salience; while, at low levels of salience, it either decreases teacher absenteeism or has no effect at all. I present suggestive evidence that the effect is unrelated to community monitoring institutions

[29] For example, it could be that high absenteeism creates animosity amongst colleagues, which may in turn reduce the likelihood of social participation.

such as parent teacher associations, and may be explained better by the effect of ethnic divisions on within-school teacher networks.

The results provide a basis for further experimental research in the field which could determine how social capital amongst teachers is affected by ethnic divisions, and how it ultimately affects attendance decisions. Moreover, the demonstrable malleability of ethnic salience leaves room for direct policy responses to collective action failures in ethnically divided areas, which could involve either fostering a common national identity or suppressing the attraction of ethnic electioneering. In all, this paper increases our understanding of high teacher absenteeism in poor, ethnically divided areas. In so doing, it points to new suggestions that could strengthen the link between educational investment and educational attainment.

Appendix A

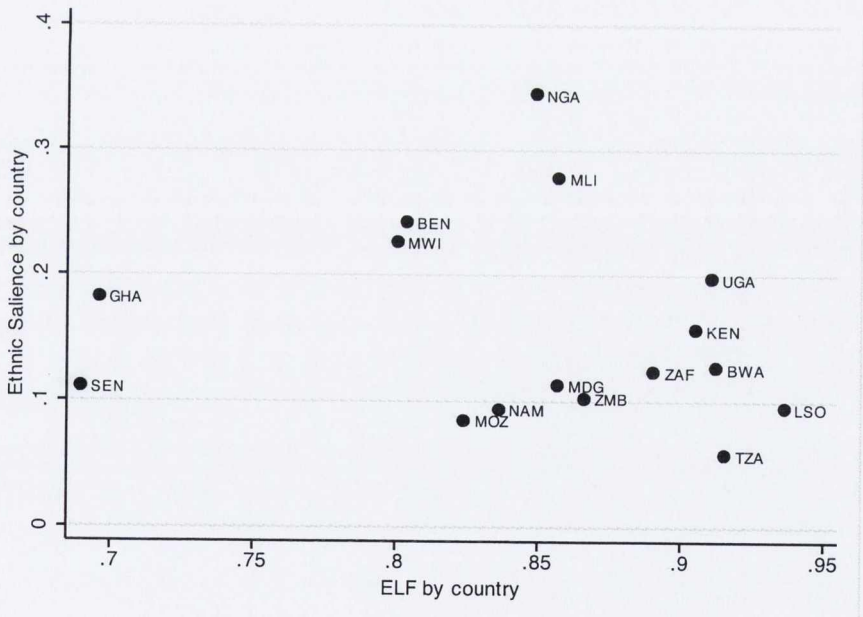


Figure 2.6: ELF and Ethnic Salience by Country

Appendix B

In order to estimate the interaction effects in the probit models presented in Table 10, I turn to Ai and Tobin's (2003) method of calculating the cross-partial derivative. They show that the marginal effect of an interaction term in non-linear models can have a different magnitude, sign and level of statistical significance than the true cross-partial derivative.

In a straightforward linear model of the form:

$$E[y|x1, x2, X] = \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + X\beta \tag{2.4}$$

where y is a continuous dependent variable and x_1 and x_2 are continuous independent variables, the interaction effect of x_1 and x_2 is simply β_{12}, provided that the vector of covariates X is independent.

However, an equivalent interpretation does not apply to non-linear models. Consider a probit model similar to the above example, where y is now a dichotomous dependent variable, whose conditional mean is:

$$E[y|x1, x2, X] = \Phi(\beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + X\beta) = \Phi(u). \tag{2.5}$$

The marginal effect of the interaction term $x_1 x_2$ is

$$\frac{\partial \Phi(u)}{\partial (x_1 x_2)} = \beta_{12} \Phi'(u), \tag{2.6}$$

which is meaningless in the context of this study, where x_1 can be substituted for ELF and x_2 can be substituted for ES_d. The interaction effect, that is, the cross-partial derivative of the expected value of y, is given instead by:

$$\frac{\partial^2 \Phi(u)}{\partial x_1 \partial x_2} = \beta_{12} \Phi'(u) + (\beta_1 + \beta_{12} x_2)(\beta_2 + \beta_{12} x_1) \Phi''(u). \tag{2.7}$$

I calculate this, and the corresponding standard errors, using the 'inteff' Stata command developed by the authors.

Below, I show the interaction effects and z-statistics for the estimates presented in columns (5) and (6) respectively. The corresponding marginal effects are also presented for comparison. The plot of the z-statistics shows that every observation significantly different from zero is positive.

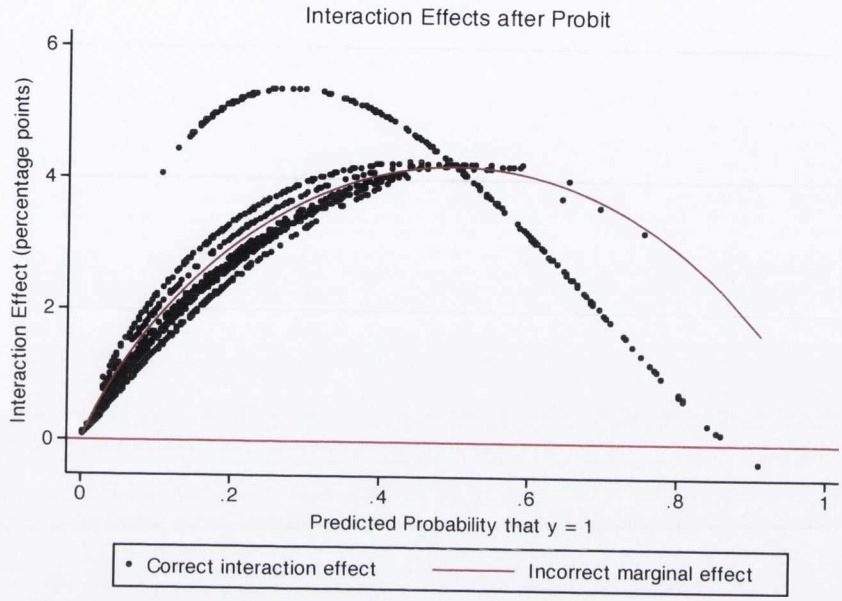


Figure 2.7: Interaction Effect With ELF at District Level, Probit

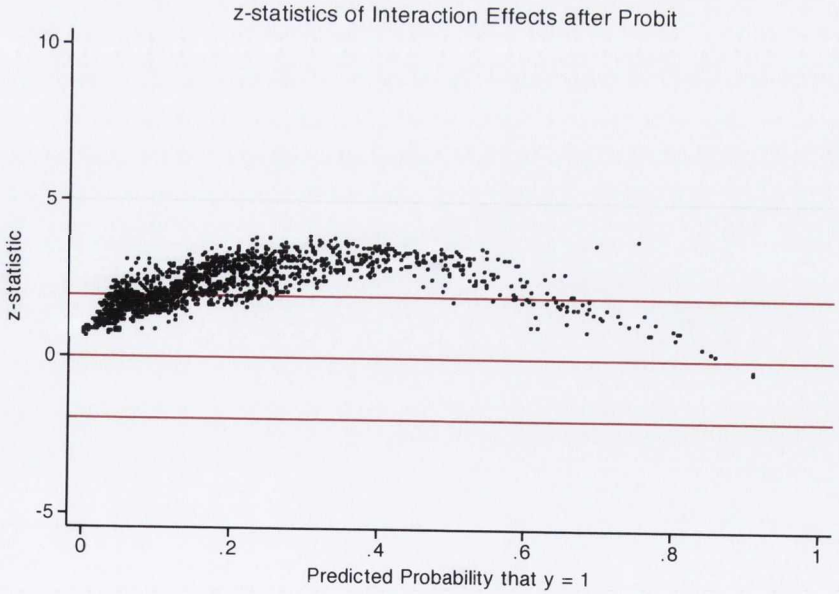


Figure 2.8: Z-statistics for Interaction Effect with ELF at District Level, Probit

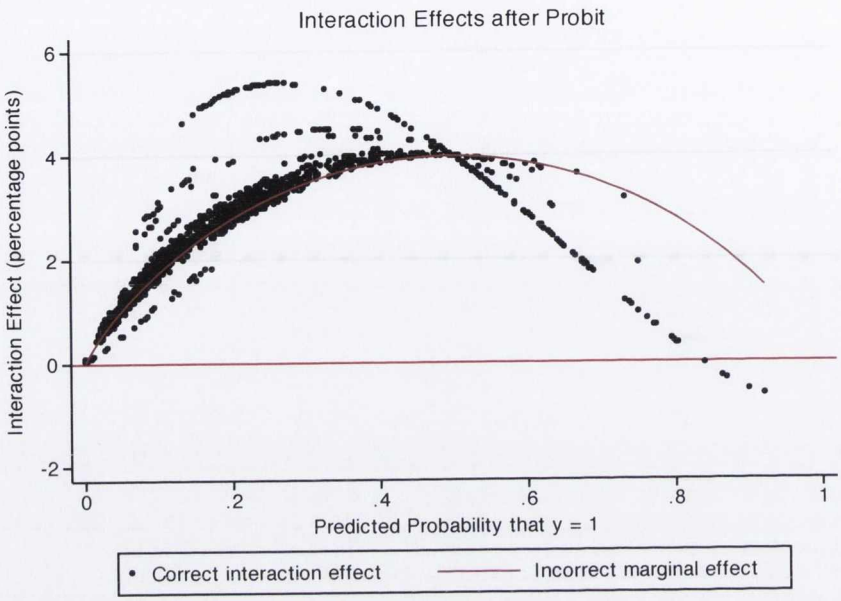


Figure 2.9: Interaction Effect with ELF at School Level, Probit

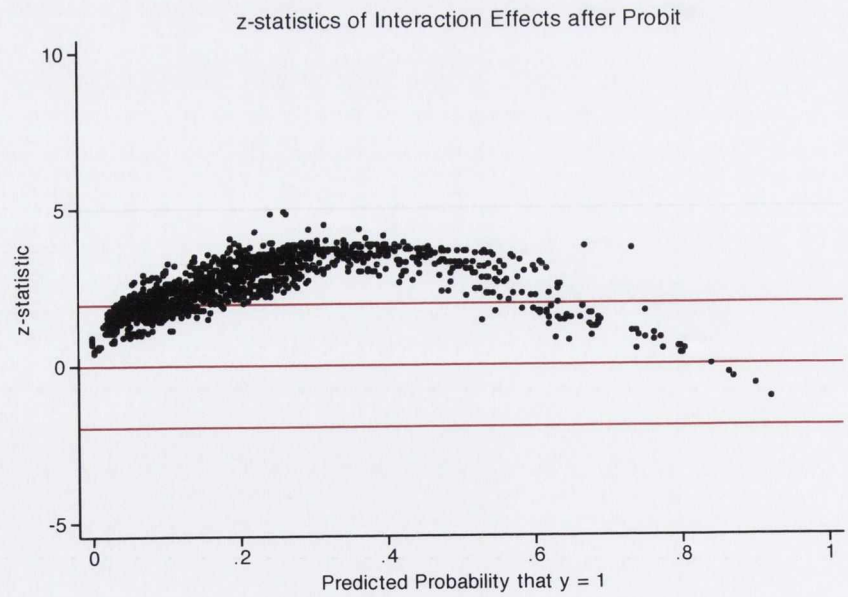


Figure 2.10: Z-statistics for Interaction Effect with ELF at School Level, Probit

Chapter 3

The Illusory Leader: Natural Resources, Taxation and Accountability

From 1965 to 2000, cumulative oil revenues in Nigeria amounted to $350 billion at 1995 prices. In 1970, per capita GDP was $245, and the poverty rate, measured as the share of the population subsisting on less than $1 a day, was around 40%. Oil revenues per capita were $33. In 2000, per capita GDP was the same; the poverty rate was just under 70%. Oil revenues per capita were $325 (Sala-i-Martin and Subramanian, 2003).

Sachs and Warner (1995) show that resource-rich countries grow more slowly than others; the importance of understanding why, how and when this occurs is clear. The extensive literature on the effects of natural resource wealth, surveyed amongst others by van der Ploeg (2011), points to a generally accepted view that the extent to which countries benefit from natural resources varies considerably; in some cases it can lead to a boon, in others a curse. In particular, countries with strong institutions tend to benefit more than those with weaker ones, and natural resources are negatively correlated with a number of governance and democracy-related outcomes. In this paper, I propose and test a mechanism linking larger rents from natural resource exploitation with decreases in demand for political accountability through a reduced burden of taxation on citizens.

The finding contributes to a growing body of literature specifically addressing the relationship between natural resources and a variety of institutional features. Earlier theoretical treatments of natural resources focused more on their economic implications. One notable phenomenon was the Dutch Disease, whereby natural resource booms lead to an appreciation of the real exchange rate, reducing competitiveness in more productive export sectors and, as a result, total factor productivity growth (Corden and Neary, 1982; Krugman, 1987). Models of rent-seeking are also used: Torvik (2002) shows how allocative distortion can reduce growth when entrepreneurs depart from productive activities to engage in rent-seeking, and Tornell and Lane (1999) show how, in economies containing a concentration of powerful groups and a lack of institutional strength, revenue shocks reduce growth rates.

Subsequent largely empirical studies have tended to support the latter argument for the existence of heterogeneous outcomes. Cross-country observations from, e.g., Mehlum et al. (2006) suggest that a political mechanism is at play: the impact of natural resources on growth varies with the strength of institutions. The finding supports political-economy explanations of resource effects posited by Robinson et al. (2006), who show how resource booms create dysfunctional state behaviour in the presence of bad institutions—a proposition observed by Sala-i-Martin and Subramanian (2003).

Recent evidence on the *economic* resource curse—whereby natural resource booms directly lead to lower GDP—is by and large mixed. Brunnshweiler and Bulte's (2008) failure to support the hypothesis using revised measurements of resource abundance, which is argued to be more exogenous than a resource dependence measure, is consistent with findings by Alexeev and Conrad (2009), whereas Cotet and Tsui (2009) find that oil wealth increases population size, but has no other direct effect on income. Van der Ploeg and Poelhekke (2010), though directly challenging Brunnschweiler and Bulte, also fail to find a direct effect of resources on growth, before identifying an effect operating through revenue volatility in previously turbulent economies. They conclude that a failure to account for heterogeneous effects may lead one erroneously to conclude that there is no effect of resources on growth. Bruckner (2010), on the other hand, finds a strong negative relationship using a purchasing power parity (PPP) corrected measure of resource dependence. Arezki and Van der Ploeg (2011) find that a country's stock of natural resource capital has a significantly negative effect on per capita income, especially in the absence of good institutions, good policy and trade openness.

On the other hand, evidence of a deleterious effect of natural resources on governance and institutions is widespread, notwithstanding an important finding by Alexeev and Conrad (2009), which suggests that there may be no relationship between resource abundance and institutions. Tsui (2011) argues that his conflicting findings are due to a more exogenous resource wealth measure based on oil discoveries. He finds that discovering 100 billion barrels of oil lowers a country's democracy level by almost 20 percentage points below trend after three decades. Interestingly, the estimates are less precise when oil abundance is measured in per capita terms (as it is in Alexeev and Conrad), suggesting that politicians are more concerned with the level rather than the per capita value of oil wealth. Tsui's results add to evidence from earlier studies by Barro (1999) and Ross (2001), who also find that oil impedes democracy. These are complemented by, inter alia, Ramsey (2011), Cabrales and Hauk (2011) and Aslaksen (2010). There is also substantial evidence linking resource wealth to increases in corruption. Arezki and Bruckner (2011), Bhattacharyya and Hodler (2010) and Busse and Groning (2011) all make the observation using country-level data.

In truth, Van der Ploeg's observation that "[c]ross-country and panel data results are sensitive to changing the sample period, the sample of countries, or the definition of various explanatory variables" is hard to dispute. For this reason, particular importance should be attached to quasi-experimental micro-level analyses that exploit within-country variation where confounding variables are held constant. Vicente (2010) shows that an oil discovery increased corruption in the forms of vote-buying, suborning customs agents, and influencing the allocation of scholarships in the West African island country of São Tomé e Príncipe. Brollo et al. (2010) use a regression discontinuity design to show that a 10% windfall to Brazilian municipalities raises corruption by 17-24%, while also raising the incumbent's likelihood of staying in power by 7% and decreasing the fraction of its opponents in possession of a college degree by 7%. Finally, Caselli and Michaels (2011) show that greater spending on public goods and services in oil-rich Brazilian municipalities leads to little or no increase in social transfers, public goods provision, infrastructure and household income. This anomaly is party explained by a corresponding increase in allegations of illegal activities on the part of city mayors.

This paper specifically contributes to the literature on the political effects of natural resources at the micro-level. I propose that, in the presence of large resource rents, political elites reduce the burden of taxation on citizens in order to reduce accountability. The mechanism is consistent with theories presented by Robinson et al. (2006) and Caselli and Cunningham (2009), who explain that natural resource rents alter the behaviour of the elites by raising the value of being in power, leading to an increase in resources spent on power-preserving activities and a misallocation of resources in the rest of the economy. Although a reduction in accountability is purported to lead to lower growth in resource-rich democracies (Collier and Hoeffler, 2009), an analysis of such an economic impact is beyond this paper's remit.

To test the proposition, I use a cross-country representative household survey sample of over 50,000 observations taken in 33 survey rounds across 15 nascent democracies in sub-Saharan Africa from 2001-2006. I use data on the demand for democratic accountability—which I interpret as the strength of respondents' preferences for regular, open and honest elections—and on perceived tax enforcement, which is used to measure the tax burden on citizens in the absence of data on tax payments at the individual level and of aggregate data on tax revenues for the sample country-years. Data on annual natural resource rents are at the country level. I hypothesise that resource rents lessen perceptions of the stringency of tax law enforcement, which in turn reduces the demand for accountability. As the latter relationship is likely to exhibit endogeneity, the identification strategy lends itself to an instrumental variable (IV) two-stage least squares approach, where the demand for accountability is the depen-

dant variable, tax enforcement is the endogenous regressor, and a measure of resource rents is used as the exogenous instrument. I include country fixed effects to control for time-invariant country-level correlates. That the data are at the individual level facilitates the estimation of the second, inherently micro-level, relationship, as well as the inclusion of numerous demographic and economic variables to control for otherwise unexplained variation in the dependent variable.

I find clear and significant evidence in support of the hypothesis. Increases in resource rents lower perceived tax enforcement, which itself is a significant predictor of the demand for accountability (a one point increase in perceived tax enforcement raises the demand for accountability by around third of a point. Both are measured on four-point scales). Moreover, suggestive evidence is given in support of a negative relationship between a variety of aggregate tax measures and resource rents, providing external corroboration. The results are robust to tests of various competing hypotheses suggested by the literature, as well as alternative measures of the instrumental and dependent variables, the exclusion of outliers and a range of alternative specifications.

In addition to the primary estimation strategy, I explore a testable implication of the theory based on the political business cycle (Nordhaus, 1975): that the effect of rents on perceived tax enforcement is more acute closer to an election, or, phrased alternatively, that the effect of election proximity on perceived enforcement of the tax laws is significant in the presence of large resource rents. This is supported by the data—the manipulation of tax enforcement in order to preserve power is a phenomenon that expressly concerns resource-rich countries. Overall, the results are consistent with political explanations of the natural resource curse, in which resource rents are purported to affect the decisions of the political elite by raising the returns to holding onto power.

The paper is organised as follows. First I contextualise the proposed relationships and present a simple model of leader behaviour. I then discuss the estimation strategy and the data used for the analysis, before examining the results and finally offering some concluding remarks.

3.1 The Political Economy of the Natural Resource Curse

The idea that natural resources affect leader behaviour is central to Robinson et al. (2006). Resource booms increase the value of being in power and also provide politicians with the means to influence elections. Where checks and balances are weak, the political incentives generated by the boom will result in a misallocation of resources in the economy. The nature of these incentives is the key to determining the extent to which the potential benefits of the boom will be realised.

Empirical evidence is cited above in support of the effect of resources on democracy and corruption. Large returns to staying in power can be observed in the form of a larger probability of coups d'états and civil conflict (Collier and Hoeffler, 2004; Fearon, 2005; Lujala, 2010; Dube and Vargas, 2010) and in higher military spending (Cotet and Tsui, 2010). Taken together, the evidence clearly shows that perverse political incentives are fostered by resource booms, and adverse effects of this are more likely to be manifested where institutions are weak.

3.1.1 The Rentier Effect

I propose that dysfunctional leader behaviour in the presence of resource rents can be observed as attenuations in citizens' tax burdens, effected with a view to decreasing the demand for accountability in order to stay in power.

The proposition is derived from studies of *rentier* states. Rentier states are defined by Mahdavy (1970: 428) as those "that receive on a regular basis substantial amounts of external rent." Beblawi (1990) adds in that regard the conditions that rent accrues directly to the government, that it is paid by foreign actors, and that few are engaged in its generation. It is a concept originally applied to oil rich middle-eastern states and is strongly believed to undermine democracy. The general *rentier effect* is defined by Ross (2001) and Herb (2005) as the alleviation of government accountability resulting from the greater availability of direct inflows of rents. The sale of natural resources represents one potentially significant source of such a rentier effect (Anderson, 1987; Crystal, 1995). While there are many channels along which this can occur (which I discuss in detail in the robustness section), there appears to be no evidence conclusively linking government rents to citizens' demand for accountability, either directly or indirectly.[1]

The *rentier* channel that I propose is described by Ross (2001: 332) as follows:

> "[...] governments use their oil revenues to relieve social pressures that might otherwise lead to demands for greater accountability [...] 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."

[1] Ross (2001: 332) notes that the state of the literature on rentier behaviour is perhaps best captured by Chaudhry (1997: 187): "theories of the rentier state far outstrip detailed empirical analysis of actual cases."

Ross draws upon two prior case studies to substantiate his characterisation of the rentier effect. Crystal (1990) observes that the governments of Kuwait and Qatar were made less accountable to the traditional merchant class by oil discoveries, and Brand (1992) argues that decreases in foreign aid and remittances in 1980s Jordan led to greater demand for political representation.[2] Ross himself offers tentative evidence of the mechanism, using cross-country data to link, via tax receipts, oil dependence with democracy index scores. A more suggestive finding is presented in another paper by Ross (2009), where oil income is strongly correlated with a less favorable view of democracy amongst World Values Survey respondents.

It is clear that the first 'link' in the mechanism—that between resource rents and taxation—is much more amenable to empirical estimation, a fact reflected in the literature. Bornhorst et al. (2009) offer the most recent and detailed cross-country evidence of an offset between natural resource revenues and revenues from other domestic sources. They find that a 1% increase in the former lowers non-resource revenues by about 0.2%. Although a reduction in public scrutiny of government is mentioned as a possible consequence, the authors refrain from identifying a specific explanation for the offset. Collier (2006: 1484) also discusses the relationship, referring to IMF data that, on average, shows no discernible difference in government expenditure as a percentage of GDP between resource-rich and resource-poor African countries.[3] His explanation is that "the governments of oil economies do not spend more, they tax less."

Although the second 'link'—between taxation and accountability—appears not to have fallen under empirical scrutiny, it is a concept that has still received recent treatment outside of the *rentier* context: a book by Brautigam et al. (2008) discusses at length the role of taxation in the formation of democratic states. It is argued that taxation is an integral part of the social contract that increases representation in and scrutiny of government. However, recent attention from World Bank policy-makers (Devarajan et al., 2010) suggests that it is in resource rich countries that this issue is causing most concern. Below, I formalise a very simple theory before offering an empirical test of its implications.

---

[2]The link between rents, taxes and accountability is also discussed at length by, inter alia, Herb (2005) and Moore (2004).

[3]See IMF (2009: Table SA11) for a more recent version of this table.

3.1.2 Theory

Although the contribution of this chapter is primarily in its empirical findings, in this section I develop a simple model of rentier behaviour that is a variation on Caselli and Cunningham (2009), in which the leader's utility is partially determined by a standard survival function (i.e., the reduced-form probability of retaining power) that predicts how natural resource rents will affect behaviour.[4] In that paper, increased returns to power-preserving activities caused by high resource rents alter the behaviour of leaders in accordance with the incentives produced by the survival function. The authors therefore explore the effects of resource rents on leader behaviour under various assumptions of survival determination. For example, the 'repressive leader' emerges when survival is determined by repressive spending; the 'fatalistic leader' emerges when survival is negatively determined by resource rents, and so on.

Here, I extend the rentier effect—the reduction of taxation by a leader to keep citizens politically demobilised—by proposing that it increases the probability of survival for a leader of a resource-rich country. This is based on the assumption that the scrutiny provoked by taxation diminishes the prospects of reelection for a leader with access to external rents and who may be engaged in resource embezzlement or patronage.[5] It follows that the reduced-form probability of survival π is a function of taxation τ, where $\pi'(\tau) < 0$ and $\tau \in [0, 1]$. The leader's consumption in the first period is financed by exogenous rents α. The leader's consumption in the second period is financed by α plus tax revenue based on non-resource GDP $\tau\nu$, subject to survival of probability $\pi(\tau)$. The objective function is thus:

$$u = \alpha + \pi(\tau)[\alpha + \tau\nu], \tag{3.1}$$

with first-order condition,

$$\frac{du}{d\tau} = \pi'(\tau)[\alpha + \tau\nu] + \pi(\tau)\nu, \tag{3.2}$$

total differential,

$$d\alpha[\pi'(\tau)] + d\tau[\pi''(\tau)(\alpha + \tau\nu) + 2\pi'(\tau)\nu] = 0, \tag{3.3}$$

---

[4] As in Caselli and Cunningham (2009), the term 'leader' is broadly construed as the political elite.

[5] Although I leave a detailed theory of this particular mechanism for future research, the channels through with this may lead to a lower probability of survival could be more vigorous political competition (access to rents) or voter punishment (embezzlement or patronage.) This is supported by Smith (2004), who finds that oil booms and oil wealth are strongly and negatively associated with regime failure and antistate protests consisting of peaceful demonstrations, riots and strikes.

and comparative statics:

$$\frac{d\tau}{d\alpha} = -\frac{\pi'(\tau)}{\pi''(\tau)(\alpha + \tau\nu) + 2\pi'(\tau)\nu}. \tag{3.4}$$

By the second-order condition, the second term in brackets in (3) must be non-positive. It follows that the denominator in (4) is non-positive. Since the numerator is negative by definition, it must be true that $\frac{d\tau}{d\alpha} < 0$.

It is important to note that this prediction is conditional on the assumption that survival is determined by taxation. Moreover, it is a relationship that can be derived from a wide range of theoretical specifications: Becker and Mulligan (2003), using a classic model of interest-group politics, show that an exogenous increase in government revenue can reduce other taxes, and Tsui (2010) finds a similar result in a model of competition for political leadership.[6] Nevertheless, the theory provides a framework from which we can clarify an empirical approach.

3.2 Estimation

I test the theory by analysing two specific propositions: first, that resource rents decrease the extent to which citizens are taxed ($\frac{d\tau}{d\alpha} < 0$); and, second, the assumption above: that the extent to which citizens are taxed affects their demand for democratic accountability (i.e., a test of the reduced form $\pi'(\tau) < 0$).

3.2.1 Identification

To investigate the nature of these relationships, I use data collected in rounds 1.5, 2, 2.5 and 3 of the Afrobarometer, a series of standardised, nationally representative public opinion surveys conducted in nascent sub-Saharan democracies.[7] The sample of 50,755 is drawn from 33 surveys conducted between 2001 and 2006 in 15 countries: Botswana, Cape Verde, Ghana, Kenya, Lesotho, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda and Zambia. Since I include country fixed effects in the analysis, the sample is restricted to countries in which the questions of interest were asked in more than one round. As in Eifert et al. (2010), the loss of data (in this case two countries) is more than compensated for by the methodological benefits of including country fixed effects. Additional constraints are imposed by the

[6] I thank an anonymous referee for clarifying this.

[7] Samples are drawn from a multi-stage stratified, clustered sampling procedure; sample sizes are sufficient to yield a margin of error at the 95% confidence level. Further information on the Afrobarometer is provided by Bratton et al. (2004).

absence of the relevant questions in the 12 first-round surveys.

The two questions of interest ask respondents to attribute a subjective score to a statement or question. The dependent variable comes from a standard question designed to gauge respondents' preferences for democratic, accountable governance:

> I would like to hear your views about how this country is governed. Which of the following statements is closest to your view? Choose Statement A or Statement B.
>
> A: We should choose our leaders in this country through regular, open and honest elections.
>
> B: Since elections sometimes produce bad results, we should adopt other methods for choosing this country's leaders.

The respondents are probed for the strength of their opinion; their responses are coded on a four-point scale ranging from "agree very strongly with A" (4) "*to* agree very strongly with B" (1).[8] The significant advantage of this question is that the variation in respondents' subjective concept of elections is minimised by the inclusion of the "regular, open and honest" qualifier and, most importantly, by the country fixed effects framework, which controls for time-invariant country-specific characteristics, such as colonial history, ethnic heterogeneity and level of economic and institutional development. In addition, one can safely assume that the alternative choice—the "other methods"—is unlikely to capture any preference for democratic accountability, especially given that colonial or autocratic rule constitute the most salient alternative methods of governance for most Africans.

The taxation variable is based on individuals' subjective assessment of income tax enforcement:

> How likely do you think it would be that the authorities could enforce the law if a person like yourself did not pay a tax on some of the income they earned?

Values are labelled on a four-point scale, ranging from "not at all likely" (1) to "very likely" (4). The variable is used to measure the extent to which leaders tax the citizenry

[8] I recode responses so that the variable *increases* with the demand for accountability.

directly. Alternative methods of capturing the tax burden are scarce: measures of individual tax payments are not included in the survey; the availability of data on tax revenues for the sample country-years is severely limited (I use an extended dataset in Figure 3.1, discussed below), as are data on tax rates, though I would argue that the latter two are less appropriate measures than the one used, given that they are aggregated to the country level. This variable appears to capture well the individual tax liabilities of each respondent, as its intra-country correlation is lower than most other individual variables (0.04 compared to 0.05 for age, 0.07 for economics hardship, and 0.16 for education - only occupation at 0.01 is lower, aside from gender, of course), although it is likely that it also captures some country-level enforcement efforts, in which case distinguishing between a conscious drop in enforcement and a type of 'fiscal laziness' is not possible. I would argue, however, that the low intra-class correlation, the consistency of the literature and the theoretical motivation combine to establish the primacy of a *rentier* explanation. Again, concerns that initial country-level institutional variation influence the results are mitigated by the inclusion of country fixed effects in the econometric model.

Nevertheless, and as with many survey-based studies, significant methodological issues remain. As answers to the above questions are given in a specific context, it is necessary to control for potentially confounding factors that could influence responses. With this in mind, I include controls for characteristics of the interview (whether other people were present during questioning) and of the interviewer (age, education, gender, rural-urban background). As mentioned, time-invarient contextual factors that are correlated with the country in which the interview takes place are controlled for, as are survey round-specific features. I also include a control for the proximity of the survey to a national election, together with a linear time trend.

In addition to context-specific variation, subjective survey responses are also prone to bias. It is plausible that one's inclination to *express* a preference for democratically accountable governance—or indeed to convey the impression that income tax is weakly enforced—varies somewhat according to both the social norms of the country and to suspicions of the enumerator's affiliation. For example, a downward bias in the responses could be expected where such opinions are frowned upon. The possibility of this is assuaged by both the confidential and private manner in which the Afrobarometer is conducted and by the independence of the enumerators, who were not affiliated with the respondents' national or local government or any political party. That the presence of other people during interviews is controlled for will also reduce the possibility of bias. Where the bias is symptomatic of a nation's social norms, the country fixed effects will hold these differences constant. I also control for respondents' attitude towards the ruling party by exploiting a question on political support and

matching it with the concurrent incumbent (shown in Table 3.2) as that is likely to bias responses on the demand for accountability. Similarly, I control for the sector in which respondents work (formal or otherwise) in order to account for heterogeneous exposure to taxation. As is standard in studies of this nature, respondents' demographic and socioeconomic characteristics (including age, economic status—measured using an index comprised of respondents' access to food, water, fuel for cooking and healthcare during the preceding year—education, gender and rural-urban background) are also entered as independent variables.

As these data stem from repeated cross-sections rather than from a panel, it is possible that sampling variation accounts for some of the changes observed between rounds. However, the combination of large nationally representative surveys (mean survey size is 1538.03 respondents) and the consistency of the sampling methodology used by Afrobarometer across rounds indicate that this may not be a significant problem. Nevertheless, I include controls for survey rounds (1.5, 2, 2.5 and 3) in the estimations.

Endogeneity is another concern in a survey-based analysis such as this. It is possible that either one's demand for accountability may have an influence on his or her perception of tax enforcement, or that other factors simultaneously affect both responses. In such a case, the reported relationship would be spurious. To overcome this, I instrument the potentially endogenous tax variable with the exogenous resource rents variable. This approach is in line with the theory, and allows a clear test of the proposed links. Of course, the validity of the approach is dependent on the satisfaction of the usual restrictions: that the instrument is directly related to the endogenous variable and is independent of the error term. I argue that this is the case below.

The measure of resource rents is taken from the World Bank's World Development Indicators (WDI). It was initially developed by Collier and Hoeffler (2009), who define rents as the difference between the price of a resource and its cost of extraction.[9] They then multiply the unit rent by the total volume extracted. Rents are included for a variety of resources and are then divided by GDP. The resources used in this paper are crude oil, natural gas, coal, bauxite, copper, iron, lead, nickel, phosphate, tin, zinc, gold, silver and wood.[10] The measure is particularly accurate as, although commodity

[9]The measurements are based on sources and methods from Kunte et al. (1998).

[10]Wood rents, coded as net forest depletion in the WDI, are calculated as the product of unit resource rents and the excess of roundwood harvest over natural growth. It has been pointed out that as forests have an open access problem, one could argue that poor property rights and bad management could lead to overharvesting, which would decrease unit rents. To assauge the potential resultant endogeneity of the variable, I check that results are robust to the exclusion of the wood component in the rents variable (unreported).

prices vary over time but are constant across countries, extraction costs vary over time *and* across countries. It thus precisely captures the value for which cruder resource-revenue measurements were hitherto used as proxies. In addition, specifications are tested using per capita rent measures, which I discuss below.

The main specification is as follows: I use an IV (two-stage least squares) method to estimate the causal links between resource rents, perceived tax enforcement, and the demand for democratic accountability of individual respondent i living in country c taking part in survey round s during period t. I express the two-stage relationship as:

$$T_{icst} = a_T + \beta R_{ct} + X'_{icst}\gamma_T + C'_{ct}\lambda_T + S'_{st}\psi_T + u_{icst} \tag{3.5}$$

and

$$D_{icst} = a_D + \varphi T_{icst} + X'_{icst}\gamma_D + C'_{ct}\lambda_D + S'_{st}\psi_D + e_{icst} \tag{3.6}$$

where T is the measure of (perceived) tax enforcement; R is resource rents; X is a vector of individual covariates including age, economic status, education, gender, rural-urban background and interview controls; C is a vector of country-related controls, namely country dummies and an election proximity variable; S is a vector of temporal controls, comprised of a linear time trend and survey round dummies; D is the demand for democratic accountability; and φ explains the relationship between T and the dependent variable D. Throughout the analysis, β and φ are the coefficients of interest, where β is predicted to be negative and φ positive.

Rents, Taxes and Survival

In addition to the main analysis, I explore another testable implication of the theory using data on the proximity of electoral competition. As outlined above, the role of the 'survival function'—the reduced-form probability of retaining power—is a key concept in the political explanations for resource curse mechanisms. As elections are likely to determine the survival or defeat of leaders, I propose that dysfunctional leader behaviour in the presence of high resource rents is likely to intensify as elections draw nearer. The proposition is supported by Block (2002), who identifies fiscal and monetary political business cycles in Africa. These are based on the assumption that leaders are opportunistic, and thus try to maximise their chances of political survival (Nordhaus, 1975). While this is challenged in certain contexts (e.g., situations involving ideological leaders), it is generally held that this is an appropriate framework for studying African political economy owing to that continent's combination of relatively uninformed voters and highly centralised political power structures.

I test this proposition by introducing an election proximity model to the analysis. I hypothesize that the effects of an approaching election on tax enforcement will be negative as resource rents increase. This is tested by including a simple interaction variable between rents and election proximity in the following:

$$T_{icst} = \alpha_T + \beta R_{ct} + \theta E_{ct} + \zeta(R_{ct} * E_{ct}) + \lambda_T C_c + X'_{icst}\gamma_T + S'_{st}\psi_T + \upsilon_{icst} \qquad (3.7)$$

where E is election proximity, defined as the absolute value in months between a survey round and the most proximate national elections – parliamentary or presidential. I follow Eifert et al. (2010) by multiplying the values by -1 for ease of interpretation: elections are closer as the variable *increases*. As such, I expect ζ to have a negative sign, signifying that, in resource rich countries (i.e., as rents become more important as a revenue source), leaders reduce tax enforcement as an election approaches.

3.2.2 Data

Table 3.1 gives a summary of the main dependant variable used in the analysis, showing the percentage of responses attributed to each of the four points on the Likert scale for all 33 survey rounds, together with means and standard deviations. In Table 3.2 I present more sample statistics, including mean tax enforcement scores and resource rent data used in the IV estimations. Two potentially problematic issues are evident here: first, some countries (Namibia, Nigeria and South Africa) are represented by three survey rounds in the sample, whereas other countries appear twice; and, second, Nigeria has particularly high resource rent levels that could bias the overall results.[11] To mitigate the first problem, I use population weights (defined as $\frac{1}{n_c}$, where n_c is the total number of observations from that country) to control for over-representation. For the second, I exclude Nigerian data from the analysis as a robustness check.

[11]The Nigerian mean for 2001, 2003 and 2005 is 26.9 (σ = 3.51); the sample mean is 4.58 (σ = 4.45).

Table 3.1: Demand for Democratic Accountability

Country	Year	Agree Very Strongly with A	Agree with A	Agree with B	Agree Very Strongly with B	Mean Score (1-4)
Botswana	2003	0.13	0.08	0.28	0.51	3.14
Botswana	2005	0.05	0.09	0.3	0.56	3.1
Cape Verde	2002	0.06	0.17	0.33	0.44	3.22
Cape Verde	2005	0.08	0.1	0.25	0.57	3.41
Ghana	2002	0.04	0.05	0.27	0.64	3.31
Ghana	2005	0.04	0.04	0.34	0.58	3.28
Kenya	2003	0.05	0.05	0.21	0.69	3.16
Kenya	2005	0.03	0.07	0.26	0.64	3.16
Lesotho	2003	0.17	0.14	0.34	0.35	3.52
Lesotho	2005	0.12	0.11	0.2	0.57	3.2
Malawi	2003	0.14	0.06	013	0.67	3.46
Malawi	2005	0.34	0.03	0.03	0.6	3.35
Mali	2003	0.09	0.07	0.33	0.51	3.54
Mali	2005	0.06	0.08	0.45	0.41	3.28
Mozambique	2002	0.08	0.1	0.41	0.41	3.31
Mozambique	2005	0.07	0.08	0.31	0.54	3.3
Namibia	2001	0.05	0.08	0.24	0.63	3.16
Namibia	2003	0.04	0.13	0.31	0.52	3.51
Namibia	2006	0.07	0.21	0.26	0.46	3.31
Nigeria	2001	0.05	0.08	0.27	0.6	3.37
Nigeria	2003	0.07	0.1	0.3	0.53	2.87
Nigeria	2005	0.1	0.14	0.26	0.5	3.16
Senegal	2003	0.11	0.09	0.3	0.5	3.46
Senegal	2005	0.02	0.06	0.46	0.46	3.54
South Africa	2002	0.07	0.09	0.32	0.52	3.22
South Africa	2004	0.08	0.09	0.29	0.54	3.31
South Africa	2006	0.06	0.08	0.36	0.5	3.38
Tanzania	2003	0.12	0.09	0.28	0.51	3.33
Tanzania	2005	0.06	0.03	0.22	0.69	3.51
Uganda	2002	0.11	0.06	0.18	0.65	2.89
Uganda	2005	0.05	0.06	0.22	0.67	3.21
Zambia	2003	0.15	0.08	0.19	0.58	3.27
Zambia	2005	0.05	0.07	0.32	0.56	3.39
Mean		*0.09*	*0.09*	*0.28*	*0.55*	*3.29*
Standard Deviation		*0.04*	*0.03*	*0.06*	*0.07*	*0.12*

Notes: A = We should choose our leaders in this country through regular, open and honest elections; B = Since elections sometimes produce bad results, we should adopt other methods for choosing this country's leaders. "Agree with Neither," "Don't Know" and refusals all treated as missing values. These values collectively account for 1407 observations in the total sample of 50755.

Table 3.2: Sample Country Statistics

Country	Year	GDP pc	Tax	Rents pc	Rents /GNI	Party	Electoral Proximity
Botswana	2003	3976	3.36	190.8	2	BDP	16
Botswana	2005	4336	3.52	332.7	3	BDP	-7
Cape Verde	2002	1269	3.13	0	0	APICV	-16
Cape Verde	2005	1357	3.42	0	0	APICV	10
Ghana	2002	264	3.42	58.8	6	NPP	-21
Ghana	2005	290	3.49	59	5	NPP	-3.5
Kenya	2003	404	3.19	11.8	1	NRC*	-8
Kenya	2005	426	3.58	13.4	1	NRC*	27.5
Lesotho	2003	437	2.93	28.8	2	LCD	-9
Lesotho	2005	453	3.67	15.3	1	LCD	18.5
Malawi	2003	134	3.2	5.7	1	UDF	12.5
Malawi	2005	138	3.51	6.2	1	UDF	-13.5
Mali	2003	278	2.94	0	0	Touré*	-6.5
Mali	2005	284	3.34	0	0	Touré*	22
Mozambique	2002	270	3.31	5.1	1	FRELIMO	27.5
Mozambique	2005	312	3.17	31.5	5	FRELIMO	-6.5
Namibia	2001	2067	2.82	0	0	SWAPO	-28
Namibia	2003	2194	2.97	0	0	SWAPO	14.5
Namibia	2006	2603	3.19	232	4	SWAPO	-15
Nigeria	2001	370	3.11	317.2	26	PDP	19.5
Nigeria	2003	394	3.04	299	23	PDP	-6
Nigeria	2005	438	3.08	459	30	PDP	19.5
Senegal	2003	492	3.21	0	0	SOPI*	-19
Senegal	2005	522	3.43	0	0	SOPI*	17
South Africa	2002	3128	3.09	279.2	4	ANC	18.5
South Africa	2004	3302	2.71	311.2	4	ANC	-6
South Africa	2006	3570	3.11	358.4	4	ANC	-22.5
Tanzania	2003	296	3.17	18.2	2	CCM	29
Tanzania	2005	321	3.62	31.5	3	CCM	4.5
Uganda	2002	266	3.28	37	5	NRM	-18.5
Uganda	2005	291	3.46	35.2	4	NRM	10
Zambia	2003	329	3.3	29.1	3	MMD	-16.5
Zambia	2005	348	3.48	106	10	MMD	14
Mean		*1078*	*3.25*	*125.94*	*4.58*		*15.24*
Standard Deviation		*1031.91*	*0.19*	*149.55*	*4.45*		*7.24*

Notes: GDP pc is measured in 2000 US$. Tax is the mean score on a four point scale (1-4) given by respondents in each survey round to the question shown in Section 3.1. Rents/GNI is natural resource rents expressed as a percentage of GNI. Rents pc is constructed using this measure and GNI per capita. Data on GDP, resource rents and GNI are all taken from the World Development Indicators. Electoral Proximity is number of months between the survey and the most proximate national elections, with a negative sign indicating that the nearest election is in the past. Party is the ruling party and the party of the president, except for * where it denotes the name of the associated coalition (NRC; SOPI) or a ruling independent president (Touré).

3.3 Results

3.3.1 External Corroboration

Before turning to more formal analysis, I begin in Figure 3.1 by supplementing the findings of Bornhorst et al (2009) with external corroboration of the hypothesised link between natural resource rents and taxation using cross-country data. Taking data from the WDI across sub-Saharan African countries from 1960 to date, I present the linear fit (with 95% confidence interval) of natural resource rents and six measurements of aggregate taxation:[12] tax revenue as a percentage of GDP; net taxes on products; the average number of meetings between firms and tax officials per year; taxes on goods and services as a percentage of government revenue; taxes on international trade as a percentage of government revenue; and the total tax paid by businesses expressed as a percentage of profits.[13] With varying levels of statistical significance, each plot is suggestive of a negative relationship. In Figure 3.2, I restrict the sample to include the 15 countries used in the main analysis of this paper only, again from 1960 onwards. Again with varying levels of confidence, the expected relationship holds in all but two cases: net taxes on products and taxes on goods and services. In the first case, the apparent anomaly can be explained by the fact that the tax measure is for indirect taxes, which can be reasonably assumed to be less salient than direct taxes. This would not be expected to affect the demand for democratic accountability. As a result, the negative relationship with resource rents is not necessarily required to corroborate the theory. Regarding the second case, the glossary of tax variable definitions in the appendix to this chapter shows that this measure includes taxes on the production and extraction of minerals, as well as taxes on the profits of governmental monopolies. The positive relationship between this variable and resource rents is thus hardly a confounding observation. It should be noted, however, that aggregated tax data, especially in less developed countries, are highly unreliable, and should only be used to inform more detailed analyses.

3.3.2 Instrumental Variable Estimates: Rents, Taxation and Accountability

Table 3.3 reports the main results of the paper. In column (1) we see the positive and significant relationship between tax enforcement and the demand for democratic accountability. Column (2) shows that the relationship is robust to the inclusion of

[12]These are defined in this chapter's appendix .

[13]It should be noted that business tax liabilities are not necessarily based on profits in every country, and this may drive the observed relationship if the basis for taxation in these countries is correlated with resource rents.

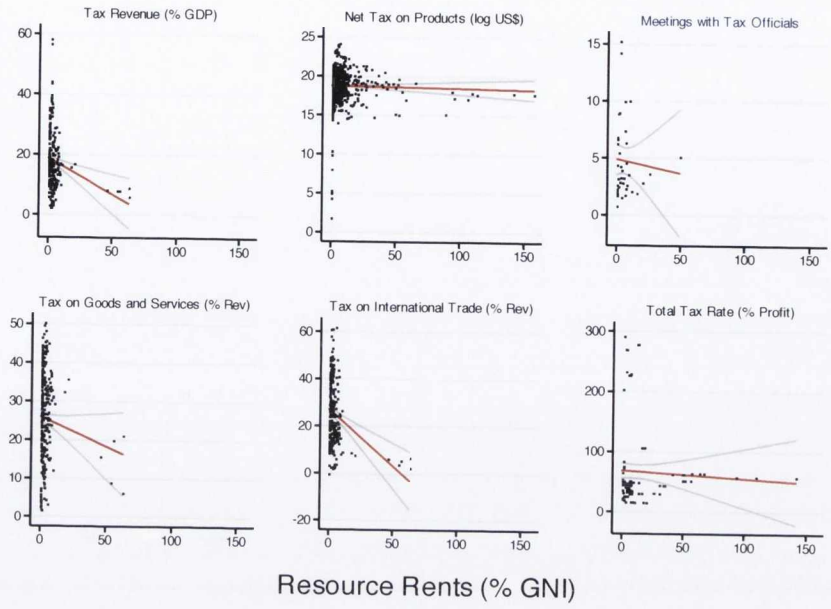


Figure 3.1: Taxes and Natural Resources in Sub-Saharan Africa, 1960-2009

individual, interview, time and country-related controls. However, as discussed, this relationship is likely to be endogenous.

Columns (3) and (4) show the first stage results. Consistent with the theory, increases in resource rents are found to reduce the level of tax enforcement. Again, the results are stable when controls are added. The IV 2SLS results presented in columns (5) and (6) suggest that the relationship between tax enforcement and the demand for democratic accountability is robust; a one-point increase on the four-point tax enforcement scale raises the demand for accountability by over a third of a point on an equivalent scale when controls are included, and by around an eighth of a point in their absence.[14] In both cases, the hypothesis that the instrument is weak can be rejected (F = 95.02; 83.79).

[14]That the instrument is measured at the country level would generally necessitate special treatment of the standard error. However, recalling the formula for calculating cluster robust standard errors for 2SLS, a non-zero intra-class correlation of the second stage residuals is a condition for clustered standard errors to be higher than robust standard errors. In contrast to most typical settings for discussions and treatments of clustering, the data here are repeated cross-sections at the cluster level with unrepeated units. I thus include country fixed effects in the 2SLS, and the intra-class correlation of residuals is zero. As a result, robust standard errors are reported throughout the paper. As a robustness check, I calculate clustered standard errors without country fixed effects. The results are unaffected by this.

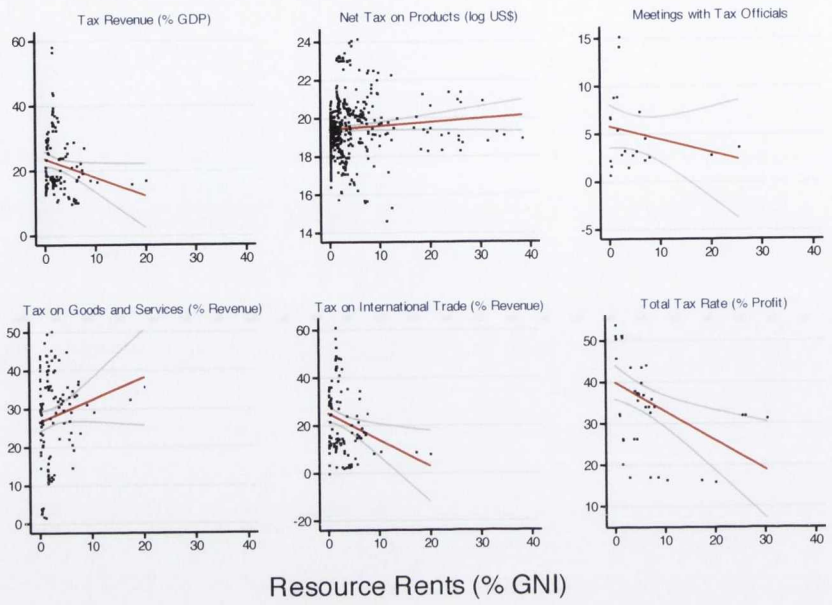


Figure 3.2: Taxes and Natural Resources in Sample Countries, 1960-2009

Table 3.3: Rents, Taxes and the Demand for Democratic Accountability

	Least Squares (1)	Least Squares (2)	1st Stage (3)	1st Stage (4)	2SLS (5)	2SLS (6)
Dependent Variable	Dem a/c	Dem a/c	Tax	Tax	Dem a/c	Dem a/c
Instrument:					Rents	Rents
Tax	0.0655***	0.0647***			0.1274*	0.3731***
	(0.006)	(0.0061)			(0.0717)	(0.0910)
Rents			-0.0363***	-0.0308***		
			(0.0031)	(0.0032)		
Individual controls	No	Yes	No	Yes	No	Yes
Election Proximity	No	Yes	No	Yes	No	Yes
Observations	46832	45270	47957	46276	46832	45270
R-squared	0.023	0.033	0.061	0.067		
1st Stage F-stat.			95.02	83.79		

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for a linear time trend, country fixed effects and survey round fixed effects. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

The results in Columns (5) and (6) are subject to the suitability of the rents variable as a valid instrument. Although the premise that it is weak can be rejected, the validity of the other implicit assumption—that the instrument is independent of the error term—has to be investigated.

Robustness: Alternative Mechanisms

As mentioned above, the literature offers alternative channels through which resource rents may affect citizens' demand for democratic accountability. Primacy must be attached to an income channel: any effect of rents on income could confound the results either through democracy's role as a normal good (where as increase in an individual's income raises his or her demand for it) or through an 'acquiescence' effect, whereby income makes a citizen more content with the incumbent government. As I state above, the individual nature of these data allows me to control comprehensively for these essentially micro-level effects. This is confirmed by the statistical insignificance of coefficients for GDP per capita and GDP growth variables when I include them in the model (unreported). Ross (2001, 2009) proposes a battery of other mechanisms: the (other) rentier effects (namely the spending effect and the group formation effect); the repression effect; and the modernisation effect. Although these are suggested as mechanisms through which resource rents may affect *democracy*, they can each easily be extended to affect the demand for democratic accountability. Thus, by omitting variables that capture these channels, there is a risk that the instrument is correlated with the error term, rendering the reported results spurious.

Below I address each channel individually. My approach has two main components: I control for the potentially confounding effect; and I discuss the direction of the potential bias. A framework for the analysis of the latter is given as follows: as tax enforcement is negatively correlated with resource rents and positively associated with the demand for accountability, i.e., $(\delta T/\delta R) < 0$ and $(\delta D/\delta T) > 0$, where T, R and D are tax enforcement, resource rents and the demand for democratic accountability respectively, any competing channel that exhibits a potentially significant positive (negative) first stage and negative (positive) second stage can be interpreted as a confounding one, as its omission may result in an upwardly biased coefficient for tax enforcement.

(i) The Rentier Effect: Spending

Ross (2001, 2009) suggests that governments in resource-rich countries may pacify the electorate by increasing spending and lowering taxes. To control for this, I include the log of government consumption expenditure (taken from the WDI) in both stages. Column (1) of Table 3.4 shows the results: tax enforcement remains largely unaffected, and government expenditure appears to have no significant effect on the demand for accountability. Although this suggests that the spending effect is not a symptom of rentier behaviour, it is nonetheless important to consider the external validity of this finding: it may not exist in the 15 countries under analysis in this sample, but it is

widely believed to exist in the better-off Middle-East resource-rich countries. Indeed Ross (2009) cites a rather blunt African case of rentier spending: the Eduardo dos Santos Foundation (FESA) in Angola—a philanthropic organisation under the personal control of the president.[15] These cases notwithstanding, I fail to find a spending effect in my sample. This, again, is consistent with Table SA11 in IMF (2009), an earlier version of which forms the basis of Collier's claim quoted above.

Table 3.4: Alternative Mechanisms (Ross, 2001, and Egorov et al., 2008)

	(1)	(2)	(3)	(4)
Dependent Variable:	Dem a/c			
Instrument:	Rents			
Tax	0.3462***	0.3484***	0.4286***	0.3853***
	(0.0871)	(0.0925)	(0.0901)	(0.0906)
ln Gov	-0.2421			
	(0.0342)			
Group: Religion		0.0299**		
		(0.0125)		
Group: Association		-0.0377**		
		(0.0165)		
Group: Union		-0.0273**		
		(0.0136)		
Group: Community		0.0262**		
		(0.0124)		
Physical Integrity			-0.1078***	
			(0.014)	
Press Freedom				-0.0304***
				(0.0035)
Observations	38707	44332	45270	45270
First stage				
Rents	-0.0234***	-0.0309***	-0.031***	-0.0289***
	(0.0042)	(0.0032)	(0.0033)	(0.0032)
F-stat.	31.13	90.09	90.41	79.44

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for individual and interview characteristics, country fixed effects, survey round fixed effects, election proximity and a linear time trend. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

[15]See Messiant (2001) for a detailed analysis of the organisation.

(ii) The Rentier Effect: Group Formation (or The Civil Society Effect)

Another possible feature of rentier states is the systematic suppression of independent civil society groups that could incubate movements for democratic reform. Although Ross (2009: 21) states that "this argument must remain speculative" due to the lack of cross-national data on civil society groups, I can test for this effect using the sub-national Afrobarometer dataset.

I create four binary group variables based on responses to the following:

> Now I am going to read out a list of groups that people join or attend. For each one, could you tell me whether you are an official leader, an active member, an inactive member, or not a member:

Four groups are cited: "religious group"; "trade union or farmers association"; "professional or business association"; and "community development association". I code individuals who are members of any such groups as taking on a value of 1, and non-members as 0.

The variables captures both types of groups that are purported to have a benign effect on the emergence of democracy: social groups (Putnam 1993) are (loosely) represented by religious and community development associations; while economic classes (Moore, 1966) are more accurately represented by unions and farming, professional, or business associations. In any case, all groups reasonably can be assumed to serve the function posited by the theory.

Column (2) of Table 3.4 shows that, while membership in these groups is positively correlated with individuals' demand for accountability, its inclusion has little effect on the tax enforcement result. I find also that membership in different groups can have diverging effects on the dependent variable. Religious and community group affiliation each increase the demand for accountability by about 0.03 points, whereas membership in professional or business associations and trade union or farmers' associations decreases it by around 0.04 and 0.03 points respectively. This indicates that groups associated with one's occupation may be implicitly organised or supported by the state.

Auxiliary regressions of each variable on resource rents show that none of them are likely to constitute channels though which natural resources affect the demand for accountability, although religious group membership exhibits a small positive relationship that does not affect the main results.

(iii) The Repression Effect

Ross (2001) and Cotet and Tsui (2010) find that oil-rich (and, in the case of the latter, nondemocratic) countries spend unusually large fractions of their GDP on military outlays. These findings are consistent with a repression effect, whereby the governments of oil-rich governments counter popular uprising by mobilizing domestic forces.[16] However, Ross (2009) questions the proposition, linking military spending in oil-rich countries to external and internal threats from neighbors and terrorist groups respectively (Middle East) and military patronage to protect governments from coups (Iran, Venezuela, Gabon). Ross tests the repression effect using a new, more appropriate measure from Cingranelli and Richards (2008) called Physical Integrity Rights,[17] which gauges the annual incidence of torture, extrajudicial killing, political imprisonment and disappearances that are attributable to the government. This, it is argued, is behaviour significantly more reflective of political leadership engaged in domestic repression. He finds no statistical link between this and oil income.

I replicate this test in column (3). It is reasonable to predict that repressive government activity is likely to stoke popular demands for democratic accountability rather than soothe it. Including this measure thus should either strengthen the original results (if domestic repression is linked with resource rents) or leave them unaffected (if it is not), while physical integrity rights should be negatively correlated with the demand for accountability. This is indeed what I find: the omission of this control variable had in fact been imposing a downward bias on my main result. In accordance with this, I find a negative correlation between resource rents and physical integrity rights (unreported).

It may be the case, however, that where repression through intimidation does not necessitate *concurrent* violence, regimes could arrive at the point where the threat of violence is viewed as credible by committing to it in previous games. I address this in two ways. First, I make use of a question in the Afrobarometer survey that allows me to control for this 'latent' oppression. Respondents are asked the following:

> In this country, how often: Do people have to be careful of what they say about politics?

Answers vary from "Never" to "Always" on a four-point scale. The results suggest that this latent repression, measured by the fear of free speech, reduces respondents' demand for democratic accountability: moving from "Never" to "Always" lowers the

[16] Bueno de Mesquita and Smith (2009) propose a similar hypothesis.

[17] The measure is increasing in physical integrity rights from 0-7

dependent variable by 0.1 points (p = 0.00). The inclusion of the variable does not affect the main result of the analysis. As a final test, I check for heterogeneous effects of physical integrity. Under the assumption that latent oppression is fostered by previous violence, it could be hypothesised that the effect of physical integrity varies according to the duration of a regime: whereas high physical integrity (that is, low violence) in a relatively new regime indicates that a regime is not oppressive, the same is not necessarily the case in an old regime—violence in the earlier part of its rule may be sufficient to quell current discontent, even if physical rights are now upheld. I can test for this by including an interaction term between Physical Integrity Rights and the current regime's durability.[18] I find no statistical relationship - current physical integrity decreases the demand for accountability regardless of the age of a regime.

(iv) The Modernisation Effect

The final major channel explored by Ross (2001) is the modernisation effect, which holds that resource-rich countries are less likely to undergo certain socioeconomic changes that can lead to more accountable governance. Whereas Ross (2009) rejects the hypothesis using cross-country measures of socioeconomic modernisation, I again make use of the individual-level data by controlling for three key variables in the standard empirical specification throughout the study: rural /urban background; educational attainment; and whether or not the respondent is working in the formal sector. I find that only educational attainment is significantly correlated with the demand for accountability. Moreover, the result for tax enforcement exhibits no statistically significant change in the absence of those variables. Again, I run an auxiliary regression of education on resource rents with and without vectors of individual, time and country controls—in each case I find a small positive effect, effectively rejecting a modernisation channel.

(v) The Media Effect

Egorov *et al.* (2009) show that the media are less free in oil-rich democracies. If free media increase the demand for accountability, then the main finding here is at least partially spurious. Column (4) in Table 3.4 shows that, curiously, press freedom (measured by the same [100 - Freedom House score] variable used by Egorov et al., 2009) is associated with *less* demand for democratic accountability. These results are robust to

[18]Durability is defined in Polity IV as the number of years since the most recent regime change (defined by a three-point change in the Polity score over a period of three years or less.)

the inclusion of democracy and other potentially correlated variables in columns (3), (4) and (5) in Table 3.5. This may suggest that citizens are well aware that their media are biased and that governmental influence triggers a demand for reform. In any case, a media effect, whereby less media freedom reduces the demand for democratic accountability, can be comprehensively rejected.

(vi) The Democracy Effect

That the political environment affects the demand for democratic accountability is an established hypothesis (O'Donnell and Schmitter 1986; Turner 1993; and Kostadinova and Power 2007 offer a recent analysis). To account for this, I include the familiar Combined Polity measure (taken from the WDI) as a control in column (1) of Table 3.5.[19] As expected, the demand for accountability is lower in more democratic environments. The negative relationship between the polity measure and resource rents is borne out by a larger coefficient on tax enforcement, confirming the expectation that its omission was putting downward pressure on the main result. Finally, in column (2), I include a control for the stock of democracy: an interaction of country's polity score with its current regime durability. The tax coefficient remains qualitatively unchanged, though, again, its magnitude is increased significantly.

Having investigated the individual effects of each potentially confounding variable above, I show in column (3) that the results are robust to the simultaneous inclusion of all control variables. As six of them vary at the country-level, it is necessary to show that the results withstand the absence of country fixed effects. Column (4) does this. Finally, as the most conservative specification - and arguably the most relevant - I enter the variables included in column (5) in all specifications that employ *Democracy Controls* hereafter.[20]

Other Robustness Checks

Another potential concern is that the variation of the resource-rent measure is merely capturing a trend effect within each country. To check this, I include a country-specific linear time trend in column (1) of Table 3.6. The results are not weakened. However, the inclusion of a country-level trend variable presents collinearity issues in a model that already includes country fixed effects, survey round fixed effects, two variables

[19]Combined polity ranges from strongly autocratic (-10) to strongly democratic (+10).

[20]I also test for overidentifying restrictions using measures of foreign aid revenues as a second instrument, the rationale for which being that aid represents an external flow of resources to leaders, and is thus likely to elicit behaviour analogous to that predicted by larger resource rents. Although I find that the aid and resource rent instruments are jointly valid, results are omitted due to concerns about strength.

that vary at the country level (election proximity and resource rents) and a linear time trend. I thus follow Eifert et al. (2010) in favoring a linear time trend to control for these effects.

Table 3.5: Additional Controls

			2SLS		
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:			Dem a/c		
Instrument:			Rents		
Tax	0.4247***	0.5546***	0.5825***	0.4149***	0.3826***
	(0.0897)	(0.1064)	(0.2227)	(0..0579)	(0.0881)
Polity	-0.0627***	-0.2009***	-0.2274***	0.0071**	-0.0617***
	(0.0109)	(0.0237)	(0.0350)	(0.0035)	(0.0106)
Durable		-0.0425***	-0.0338***	-0.0005	
		(0.0054)	(0.0107)	0.0024	
Polity*Durable		0.0037**	0.0111*	0.00004	
		(0.0016)	(0.006)	(0.0025)	
ln Gov			0.0544	0.0426***	0.0168
			(0.1014)	(0.0078)	(0.0417)
Group			0.0413**	-0.0522***	0.0509***
			(0.017)	(0.0151)	(0.0151)
Physical Integrity			-0.0623***	-0.0484***	-0.07***
			(0.0201)	(0.0101)	(0.0157)
Press Freedom			-0.0153**	0.0053***	-0.0228***
			(0.0060)	(0.0052)	(0.0039)
Country FE	Yes	Yes	Yes	No	Yes
Observations	43112	45270	35440	35440	36549
First Stage					
Rents	-0.0350***	-0.044***	-0.03***	-0.009***	-0.0350***
	(0.0035)	(0.0088)	(0.0053)	(0.0023)	(0.0049)
F-stat.	99.42	25.40	32.07	15.37	51.28

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for individual and interview characteristics, country fixed effects, survey round fixed effects, election proximity and a linear time trend, unless otherwise stated. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

A number of other robustness checks were carried out. I allude above to the potential problems that may arise due to the inclusion of Nigeria in the analysis, namely that its considerable resource rents may bias the overall results. In columns (2) and (3) we see that results are robust to the exclusion of Nigerian data.

Table 3.6: Robustness Checks

	2SLS (1)	2SLS (2)	2SLS (3)	OLS (4)
	CTT	Excl. Nigeria		Law
Dependent Variable:	Dem a/c	Dem a/c	Dem a/c	Tax
Instrument:	Rents	Rents	Rents	
Tax	0.6163***	0.3482***	0.3826***	
	(0.0621)	(0.0905)	(0.0881)	
Democracy controls	No	No	Yes	
Linear time trend	No	Yes	Yes	
Country time trend	Yes	No	No	
Observations	45270	38707	36549	
First Stage				
Rents	-0.0408	-0.0238***	-0.0350***	-0.0305***
	(0.9633)	(0.0041)	(0.0049)	(0.0035)
Law				-0.486***
				(0.1088)
F-stat.		34	51.28	

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for individual and interview characteristics, country fixed effects, survey round fixed effects, election proximity and a linear time trend, unless otherwise stated. Democracy controls are those shown in column (5) of Table 3.5. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

It has been noted that the first stage may itself be endogenous. The potential mechanism is via the rule of law, a deterioration of which could lead to lower GDP (and hence larger values for resource rents, given the variable's denominator) and a lower capacity to enforce tax collection. I show in column (4) of Table 3.6 that the first stage result is practically identical when a rule of law control is added to the specification, suggesting that it is exogenous to the first stage relationship.[21]

An added concern is that the linear specifications used thus far may not fit the data, and that, given the limited nature of the dependent variable, an alternative estimator should be tried. I thus dichotomise the dependent variable by grouping each half of

[21]The 2SLS is also robust to this: the tax coefficient is 0.3106 (p=0.00); the first-stage F-statistic = 66.76.

the four-point scale into a single point in order to facilitate the estimation of the relationship using IV Probit (Table 3.7, column 1) and linear probability models (column 2). The resultant marginal effects continue to corroborate the theory.[22]

Furthermore, the results may be conditional on the operation of the specific dependent variable used to proxy the demand for accountability in this analysis. To check this, I replicate the estimation using an alternative Afrobarometer question that could reasonably be expected to capture the demand for democratic accountability:

> There are many ways to govern a country. Would you disapprove or approve of the following alternatives?
>
> Elections and Parliament are abolished so that the president can decide everything.

As with the original measurement, I recode the variable so that the strongest level of demand for accountability (i.e., disapproval of one-man rule) is at the higher end of the four-point scale used to measure attitudes towards the statement. The variable is particularly appropriate as a substitute for the original measurement given that it also provides respondents with the option of forgoing democratic elections, albeit in favor of a defined alternative. The results in Table 3.7 reinforce the original findings.

Table 3.7: Alternative Dependent Variables

	IV Probit (1)	2SLS (2)	2SLS (3)	2SLS (4)	IV Probit (5)	2SLS (6)
Dependent Variable:	Dem a/c (1/0)	Dem a/c (1/0)	Reject OMR	Reject OMR	Reject OMR (1/0)	Reject OMR (1/0)
Instrument:	Rents	Rents	Rents	Rents	Rents	Rents
Tax	0.4558***	0.1487***	0.5907***	0.8440***	0.3734*	0.3011***
	(0.1662)	(0.0352)	(0.1088)	(0.1124)	(0.2241)	(0.0402)
Democracy controls	No	Yes	No	Yes	No	Yes
Observations	45270	36549	44490	38707	40746	33351

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for individual and interview characteristics, country fixed effects, survey round fixed effects, election proximity and a linear time trend, unless otherwise stated. Democracy controls are those shown in column (5) of Table 3.5. Dependent variables in columns (1), (2), (5) and (6) are dichotomous variations of the respective main measurements, where the higher and lower two points on the scale are bundled into single points (0/1) to create binary variables. Coefficients reported represented marginal effects (dy/dx.) ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

[22]Specifications including democratic controls were unable to be estimated using maximum likelihoood. They are instead treated as linear probability models.

Finally, it could be claimed reasonably that the effects of perceived tax enforcement on the demand for democratic accountability are not homogeneous—some people may be affected differently than others. In this context, it is perhaps most likely that individuals with higher levels of educational attainment are more sensitive to the effect. I thus test for heterogeneity across the ten levels of education in the survey, ranging from no formal education (0) to post-graduate level (9). Although I find significant differences for some categories, the effects are not concentrated at either end of the distribution: the effects for people with primary education (3), some high school education (4), post-secondary education excluding university (6), and university education (8) are significantly larger than the effects for those with no formal education.[23] Homogeneous effects across economic status could not be rejected ($p = 0.27$), nor could those for the rural-urban background of respondents ($p = 0.65$). I conclude that the effects of tax enforcement on the demand for accountability are largely homogeneous.

Per Capita Rents

The measurement of natural resources is a point of considerable contention in the literature. Alexeev and Conrad (2009) and Brunnchsweiler and Bulte (2008) argue in favor of per capita measures of resource abundance in addition to GDP shares, as (i) the traditional measure, resource exports as a share of GDP, is endogenous to export policies and domestic consumption; and (ii) it is generally used in GDP growth regressions, where it is more obviously endogenous due to the fact that the dependent variable and the denominator of main explanatory variable are related directly. Sachs and Warner (2001), on the other hand, argue in favor of using GDP shares as resource-rich states are characterised by the relative importance of resource rents in the economy.

Both views have compelling elements, and, in Table 3.8, I show that the main results of the paper are qualitatively unchanged (indeed they are strengthened) when I use a per capita measure. However, it is important to note that, while no resource measure can truly be ideal, the original measure used here—[(unit price−unit cost)∗volume extracted]/GNI]—is significantly less endogenous to policy choices and institutional conditions than the export-share measures traditionally used in the literature. Furthermore, as I am not using the measure in a growth regression, it is less problematic than it would be in the context of the Brunnschweiler-Bulte and Alexeev-Conrad papers mentioned above. Moreover, the Sachs and Warner view that resource importance is to be interpreted as its dominance in a country's economy rather than its per capita in-

[23]The coefficients are 0.22 ($p = 0.00$), 0.12 ($p = 0.09$), 0.18 ($p = 0.06$) and 0.36 ($p = 0.01$) respectively.

cidence is an intuitive one.[24] Finally, if the original measure is endogenous to political development operating through the denominator, its use is likely to produce a lower bound on the paper's main results, as political development will be negatively associated with both the resource rents measure and, as I show in Table 5, the demand for democratic accountability. This is confirmed in Table 8: the per capita measure results in larger coefficients. Thus, I believe that the original measure is both a more suitable and prudent choice. In any case, as I mention above, the results are robust to either measure, and the variables are highly correlated (0.70), perhaps due to the relative homogeneity of this sample compared to the global samples more commonly used in the literature.

Table 3.8: Models Using Per Capita Rents Measure

	2SLS			
	(1)	(2)	(3)	(4)
Dependent Variable:	Dem a/c	Dem a/c	Reject OMR	Reject OMR
Instrument:	Rents pc	Rents pc	Rents pc	Rents pc
Tax	0.5960***	0.4239***	0.7216***	0.8816***
	(0.1035)	(0.0878)	(0.1255)	(0.1159)
Democracy controls	No	Yes	No	Yes
Observations	46276	36549	44490	36040
First stage				
Rents pc	-0.0008***	-0.0011***	-0.0008***	-0.0011***
	(0.0001)	(0.0002)	(0.0001)	(0.0002)
F-stat	43.38	41.2	41.41	40.62

Robust standard errors in parentheses. All models include population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country) and controls for individual and interview characteristics, country fixed effects, survey round fixed effects, election proximity and a linear time trend, unless otherwise stated. Democracy controls are those shown in column (5) of Table 3.5. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

[24]In that the 'substantial' (Mehdavy 1970: 428), or 'sufficient' (Ross 2001: 332) resource rents required to elicit rentier behaviour are more likely to be salient relative to other central revenues and outlays, and hence the size of the economy, than the size of the population. This is supported by Tsui (2011).

3.3.3 Rents, Taxes and Survival

Table 3.9 reports the findings of Equation (3.7). I include six specifications, varying with the inclusion of controls and in the measure of election proximity: *Absolute Proximity* is the closeness of the nearest election in absolute months; *Election Year* signifies that an election is approaching within 12 months; and *Next Election Proximity* denotes the number of months to the next election. For the latter, it is necessary to include a squared term and a squared interaction to capture the nonlinearity that would be generated from the fact that there may be a post-election effect that would be captured by larger values resulting from fixed term lengths.[25] In five of the six specifications the interaction term is significant, and in of all of those estimates we find the predicted result: tax enforcement is reduced in resource-rich countries as elections become more proximate.[26,27] As rents increase, the rational leader disengages his citizenry by lowering tax enforcement.

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[25] A look at data on the proximity of previous and future elections in each country-year sample reveals that, on average, countries follow 60-month, or five-year, terms (the standard deviation is less than five months). This implies that, should there be any post-election effect, the result would be biased toward zero. In effect, each month towards the next election is one month away from the previous one. Accordingly, I find no statistically significant effect when I run this model.

[26] A negative and significant coefficient for *Rents*Next Election Proximity*^{2} indicates that a post-election effect exists, i.e., the 'resource specific' effect of next election proximity strengthens as the previous election becomes more distant in time.

[27] Using per capita rents, I find statistical significance in three specifications. In all cases the coefficient is in the predicted direction.

Table 3.9: Rents, Taxes and Elections

	OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
	Tax					
Rents*Absolute Proximity	-0.0002*	-0.0059***				
	(0.0001)	(0.0007)				
Rents*Election Year			-0.0220**	-0.0185		
			(0.0090)	(0.0156)		
Rents*Next Election Proximity					-0.0035***	-0.0244***
					(0.0012)	(0.0021)
Rents	-0.0389***	-0.1225***	-0.0321***	-0.0325***	-0.0715***	-0.2838***
	(0.0054)	(0.1214)	(0.0033)	(0.0047)	(0.0154)	(0.0245)
Absolute Proximity	-0.0021***	0.0069				
	(0.0008)	(0.0017)				
Election Year			0.1103***	0.0474		
			(0.0280)	(0.0432)		
Next Election Proximity					0.0067**	0.042***
					(0.0154)	(0.005)
Next Election Proximity^2					0.0001	0.0007***
					(0.0001)	(0.0001)
Rents*Next Election Proximity^2					-0.0001***	-0.0004***
					(0.0000)	(0.0000)
Controls	No	Yes	No	Yes	No	Yes
Observations	46968	39131	46968	39131	46968	39131
R-squared	0.065	0.068	0.065	0.065	0.067	0.07

Robust standard errors in parentheses. Model includes population weights (defined as $\frac{1}{n_c}$, where $n_c$is the total number of observations from that country), and controls for individual and interview characteristics, country fixed effects, survey round fixed effects and a linear time trend, unless otherwise stated. Controls are those shown in column (5), Table 3.5, together with the Law control from column (6), Table 3.6. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

3.4 Conclusion

In this paper I identify a mechanism that supports a political explanation of the natural resource curse in developing countries. When and where resource rents increase, leaders lighten the burden of taxation on the citizenry as a means of rendering them more acquiescent to the current regime's policies. The findings are corroborated by supplementary analyses of testable theoretical implications. Moreover, the effect is

stronger closer to elections, suggesting that leaders of resource-rich countries adhere to the political business cycle.

In the main analysis I use household survey data from 15 sub-Saharan countries. The IV micro-level approach facilitates the identification of a specific channel through which the curse can operate. However, this approach also imposes a considerable constraint on the data, as country-level temporal variation is required when controlling for country fixed effects. The opportunity to exploit variation in natural resource data from more than 33 survey rounds would therefore be one way to reinforce the results. Another valuable addition to the literature would be made possible by the availability of data on tax payments at the individual level as an alternative to self-reported perceptions of tax enforcement.

Furthermore, it is important to note that this mechanism may play a role in a bigger story, namely that it may facilitate political malfeasance in the form of patronage politics and resource embezzlement. This is key to understanding whether or not this behaviour leads to an economic natural resource curse. As I mention in the main analysis, Collier and Hoeffler (2009) show that weak accountability in resource-rich democracies leads to a reduction in growth, suggesting that this is indeed the case. Analyzing the channel in this context would constitute a valuable extension, although the political implications of the results are interesting in themselves.

Turning to policy implications, a recent World Bank proposal (Devarajan et al., 2010) outlines a way in which the cycle identified in this paper can be moderated, namely by directly transferring resource revenues to citizens before taxing a proportion of it back. Though an assessment of this specific proposal is beyond the scope of this study, the findings I report in this paper reinforce the movement to strengthen accountability in resource-rich countries, as reflected by the emergence of international protocols such as the Extractive Industries Transparency Initiative and the Natural Resource Charter. Although the vital importance of democratic accountability as an accompaniment to electoral competition cannot be understated for any sub-Saharan African country, it should be noted that resource-rich countries are particularly prone to institutional deterioration as a result of citizen acquiescence purchased by a reduction in the tax burden—where checks and balances are most needed, they are perhaps at their most delicate.

Appendix

Tax Revenue

Tax revenue refers to compulsory transfers to the central government for public purposes. Certain compulsory transfers such as fines, penalties, and most social security contributions are excluded. Refunds and corrections of erroneously collected tax revenue are treated as negative revenue.

Net Tax on Products

Net taxes on products (net indirect taxes) are the sum of product taxes less subsidies. Product taxes are those taxes payable by producers that relate to the production, sale, purchase or use of the goods and services. Subsidies are grants on the current account made by general government to private enterprises and unincorporated public enterprises. The grants may take the form of payments to ensure a guaranteed price or to enable maintenance of prices of goods and services below costs of production, and other forms of assistance to producers. Data are in current U.S. dollars.

Meetings with Tax Officials

These figures show the average number of days firms spent in inspections and mandatory meetings with tax officials in the last two years.

Tax on Goods and Services

Taxes on goods and services include general sales and turnover or value added taxes, selective excises on goods, selective taxes on services, taxes on the use of goods or property, taxes on extraction and production of minerals, and profits of fiscal monopolies.

Taxes on International Trade

Taxes on international trade include import duties, export duties, profits of export or import monopolies, exchange profits, and exchange taxes.

Total Tax Rate

Total tax rate is the total amount of taxes payable by businesses (except for labour taxes) after accounting for deductions and exemptions as a percentage of profit.

Chapter 4

By The People? Foreign Aid and Donor-Country Democracy

In 2010, aid flows from Development Assistance Committee (DAC) donor countries totalled $129 billion—an unprecedented high.[1] In that light, it is interesting to note that despite the emergence of a well-publicised literature on the effectiveness of foreign aid in recent years, few studies have analysed attitudes to foreign aid from the perspective of donor-country citizens. We look at preferences for government aid donations using unique micro-level data taken from a survey commissioned by the World Bank Institute and collected by Gallup International. The dataset documents the aid preferences of over 235,000 individuals across 75 countries in 2005, 2006, 2007 and 2008. First, we make some observations on the patterns of aid support. Second, we propose and test a theory of support for official aid using a simple political economy model, whereby support is decreasing in the level of taxes an individual is obliged to pay in a proportional system and, by extension, is increasing in inequality at the country-level. We then extend the theory to incorporate private-interest lobby group influences, whereby the top-income agent can 'buy' less aid from the government, thus extending the gap between popular preferences and government activity. This is supported by tests using data on the strength of interest-group lobbying from the Global Competitiveness Report of The World Economic Forum. Finally, we incorporate private aid donations into the analysis as a robustness check. In doing so, we corroborate recent evidence that government crowding-out is incomplete, and that it operates mainly though reduced fundraising activities by charitable organisations; otherwise government donations have a crowding-in effect on individual private donations.

As we indicate above, the debate on the effectiveness of foreign aid is well documented. In short, a generation of macro-level studies on aid and growth has failed to establish a clear consensus. While some studies find positive average effects of aid on growth (Hansen and Tarp, 2001), many more identify heterogeneous effects: Burnside and Dollar (2000) famously find that aid has a positive impact on growth in the

[1] More DAC aid statistics are available on the OECD website at www.oecd.org/dac

presence of good policies; Boone (1996) finds that aid is generally absorbed by elites, though it can increase investment within small countries; Collier and Dehn (2001) and Collier and Hoeffler (2004) respectively show that aid can somewhat mitigate the deleterious effects of export-shocks and conflict on growth; whereas Dalgaard et al. (2004) find that the effectiveness of aid varies with climate conditions. However, these findings have been strongly challenged by, inter-alia, Easterly et al. (2004), Roodman (2007a) and Rajan and Subramanian (2008). Taken together, they uncover the familiar problems of reverse causality, omitted variable bias and measurement error when instruments are scrutinised. In some cases updated data is enough to confound the original findings. Review papers such as those by Roodman (2007b, 2008) broadly conclude that, given both the aggregate nature of the data and the difficulties inherent in estimating the relationship of interest, the macro-level literature is fundamentally limited. As a result, the focus has overwhelmingly turned to micro-level studies that purport to overcome these traditional pitfalls by using experimental or quasi-experimental methods to identify the specific effects of particular projects on well-defined outcomes.[2] Amongst hundreds of other findings, the literature has uncovered positive effects of medical interventions on health and educational outcomes (Miguel and Kremer, 2004) and of conditional cash transfer programs on consumption (Angelucci and DeGiorgi, 2009) and child development outcomes (Fernald et al., 2008).[3] In brief, the proliferation of these studies has diverted attention from the debate on aid and growth towards a more refined focus on the specific effects of individual projects. The results suggest that there remains a significant role for aid interventions—and aid research—in global development.

Analyses of aid supply tend to centre on the macro-level determinants of bilateral aid flows. Dudley and Montmarquette (1976) find that politically and economically strategic links appear to be the main drivers of aid. McKinley and Little (1979) broadly support this view, showing that US aid from 1960 to 1970 was more motivated by strategic interests than poverty. Maizels and Nissanke (1984) conduct a more nuanced test that reveals the 'recipient-need' rationale for multilateral aid allocations and the familiar 'donor-interest' approach to bilateral aid. Alesina and Dollar (2000) and Collier and Dollar (2002) reinforce the main thrust of the literature: political and strategic concerns mainly determine bilateral aid, and allocations would be more productive if poverty and policy played a larger role. It must be noted, however, that allocations

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[2]Some macro-level studies also overcome measurement problems by focusing on more refined measures of aid (Clemens et al. 2004) and outcome performance (Isham and Kaufmann, 1999).

[3]See Duflo et al. (2008) for an overview of these experimental methods. A more concise account of the strengths and weaknesses of randomised trials is given by Duflo and Banerjee (2009).

from multilateral institutions are not entirely immune from the political interests of their primary donors: Kilby (2009) finds that World Bank structural adjustment loan disbursements carry less conditionality for countries that share US voting behaviour at the United Nations; Dreher and Jensen (2007) find a comparable result for the case of IMF loans; and, in a similar vein, Dreher et al. (2009) identify a robust link between temporary UN Security Council membership and the number of World Bank projects allocated to a country.

In addition to this broad consensus, a number of auxiliary findings have emerged: Nordic countries are more likely than other donors to allocate aid on the basis of poverty (Alesina and Dollar, 2000); less populated countries receive more aid per capita (Trumbull and Wall, 1994; Burnside and Dollar, 2000); Cold War-era political support influenced donations (Lundborg, 1998), and its end precipitated a general switch from donor-interest to recipient-need models of allocation (Dollar and Levin, 2006; Lundborg, 1998); and France and the USA remain the most politically-motivated donors (Alesina and Dollar, 2000; Dollar and Levin, 2006), especially, in the latter's case, since the onset of the War on Terror (Fleck and Kilby, 2010).

Despite the convergence of these macro-level studies of bilateral aid supply to an apparently clear conclusion by the early 2000s, few studies have since explored the individual preferences of donor-country citizens for foreign aid. Chong and Gradstein (2008) and Paxton and Knack (2008) are the most similar in spirit to our analysis. The former use World Values Survey (WVS) data to show that, at the individual level, relative income is positively associated with preferences for foreign aid in probit regressions. At the country level, they find that income inequality negatively determines actual ODA disbursements. Paxton and Knack (2008) look at correlates of support for aid increases using WVS data together with an earlier version of Gallup International's Voice of The People survey. They identify correlations with largely attitudinal variables such as religiosity and positions on poverty, global affairs and institutions.

Our analysis contributes to the literature in several ways. Firstly, our unique Gallup International data provides us with a richer foundation for empirical analysis. Whereas Chong and Gradstein (2008) have two waves of WVS data (1995-1997 and 1999-2000) consisting of 10,000 observations, and Paxton and Knack (2008) have a single wave of WVS data (5,693 observations in nine donor countries) and a single wave of Gallup data (6,194 observations in 17 countries), we avail of a more recent four-wave dataset consisting of almost 90,000 respondents from 24 donor countries. This allows us to comprehensively control for year and country fixed-effects in our individual-level regressions. Furthermore, our measure for aid preferences is based on a wide-ranging scale which gives respondents the ability to choose their preferred levels of aid as a percentage of national income. Crucially, we also make use of a survey question which

measures each respondent's perception of current aid levels—something that, until now, was only proxied by actual levels of ODA as a control, an approach we later show to be extremely unsuitable due to a considerable 'aid illusion' effect.[4] This measure also contributes to the formation of a new index of aid preferences, which we call the Democratic Aid Level Preference Index (DALPI), whereby country-level measures of aid preferences are weighted by this 'illusion' effect to account for misconceptions of actual aid levels as a share of national income.

We also depart from the literature on certain other novel grounds. The nature of our aid variables allows us to study what we call the democratic aid deficit, defined as the difference between the level of aid desired by citizens and the level actually donated by their government. We note a wide variation in the size of this deficit, which we show is partly caused by the influence of monetary transfers on policymakers. The size and variation of this deficit also indicates that the use of ODA as a proxy for citizen preferences in country-level regressions is inappropriate. This may provide one explanation for why Chong and Gradstein (2008) identify a negative relationship between actual ODA and inequality, while we find that inequality is a positive determinant of *desired* aid. Our data also allow us to identify separately the direct and indirect effects of government foreign aid on private contributions through the use of our perceived aid variable. To our knowledge, this is the first test of this nature, particularly in the context of foreign aid.

On the theoretical side, we take our cues from the literature on philanthropy (see Andreoni, 2006) in assuming an altruistic utility function. This assumption is consistent with our treatment of foreign aid as a donor-country public good (first noted by Hochman and Rodgers, 1969, and Kolm, 1969), as in Lahiri and Schweinberger (2006). Grossman and Helpman (1996) and Acemoglu and Robinson (2006) provide the framework for our analysis of lobby influences and the democratic aid deficit. We build on recent contributions from Andreoni and Payne (2003, 2011a and 2011b) and Name-Correa and Yildirim (2012) in our robustness section on government and private aid interactions, whereby the effect of government aid funds on private donors is deconstructed into 'fundraising' and 'classic' channels.

Our paper is structured as follows: below we describe our Voice of The People data on attitudes to aid, after which we propose and test a theory of support for foreign aid at both the micro- and macro-level; we then incorporate our analysis of the democratic aid deficit and finally conclude.

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[4]Indeed perceived aid and actual ODA are negatively correlated.

4.1 Data

We restrict the sample to the 24 donor countries in which the survey was conducted, leaving 89,718 observations over the four years from 2005 to 2008 inclusive. Our main focus in the dataset concerns individual preferences for the percentage of national income devoted to overseas aid. Respondents were asked two questions:

> (i) What share of national income does your country actually give in foreign aid to help development / poverty alleviation in other countries?

and

> (ii) What percentage in your view should it give?

Respondents could choose one of 11 categories ranging in increasing intervals from 0 to higher than 25%. We take midpoints for all categories other than the minimum (0) and maximum (25) values. The responses are labelled 'perceived aid' and 'desired aid' respectively.

Table 4.1 shows the sample size for each survey round, together with scores for desired aid, perceived aid and country-level 'actual aid', measured by the OECD as the ODA share of Gross National Income (GNI), averaged over all rounds for each country. Figure 4.1, included at the end of the chapter, presents a visual accompaniment to the data. Some notable patterns emerge. First, people like aid. On average, desired aid as a percentage of GNI is around eight times higher than actual levels. Citizens of rich donor countries are prepared to support an attribution of 3.14% of GNI towards foreign aid—the average level actually attributed is 0.37%. However, people are also deluded: at 2.65% of national income, respondents' perception of official aid transfers is around seven times higher than the actual level. Nevertheless, it remains clear that, on average, citizens of DAC countries would support an increase in foreign aid.

Second, support for aid has been decreasing over time. In Figure 4.2, we can see that, in 2005, desired aid was over 1% higher than perceived levels; in 2008 they were almost identical. This effect is consistent across the distribution. Anti-aid 'absolutists', who we define as those who favour no transfers of aid, rose from 5.26% of the sample in 2005 to 9.41% in 2008; anti-aid 'relativists', who we define as those who believe aid should not exceed 0.2% of GNI, rose from 15.51% to 23.51%; and highly pro-aid respondents—those who favour transfers of over 1%—decreased from 58.62% to 50.78%. We find that this trend is robust to controls for many economic and other factors, perhaps reflecting a 'Gleneagles Effect': the result of a high-profile pro-aid public awareness campaign designed to influence the G8 meetings at Gleneagles, Scotland, in the summer of 2005. Actual aid levels have increased slightly as a percentage of GNI throughout this decline in support.

Table 4.1: Sample Sizes and Mean Values for Aid Variables by Country

	2005	2006	2007	2008	Desired Aid	Perceived Aid	Actual Aid
AUT	669	969	804	826	1.989	2.040	0.478
					(3.404)	(3.976)	(0.033)
CAN	1001	1000	1007	1012	4.684	3.345	0.313
					(5.999)	(5.483)	(0.023)
CZR	500	1000	1000	1000	1.511	1.995	0.116
					(3.001)	(3.825)	(0.005)
DNK	500	500	1118	1069	2.713	2.122	0.812
					(4.098)	(3.555)	(0.007)
FIN	1289	1249	1154	1267	1.321	0.763	0.423
					(2.574)	(1.712)	(0.029)
FRA	1003	1012	1000	.	3.618	2.959	0.440
					(5.001)	(4.819)	(0.042)
GER	500	505	500	503	5.600	4.406	0.368
					(6.451)	(6.139)	(0.008)
GRC	500	1000	1000	1000	2.849	1.684	0.181
					(5.308)	(3.955)	(0.020)
ICE	500	1018	1081	1084	2.849	0.986	0.317
					(4.143)	(1.986)	(0.103)
IRL	500	.	1020	.	5.957	3.671	0.507
					(7.062)	(5.943)	(0.061)
ITA	502	988	1000	1001	4.727	2.626	0.216
					(6.557)	(5.251)	(0.033)
JPN	2393	1203	1200	1200	3.453	5.755	0.234
					(5.208)	(7.395)	(0.046)
KOR	1515	1504	1001	1519	1.995	1.799	0.078
					(3.909)	(3.868)	(0.020)
LUX	582	528	504	532	2.735	2.642	0.890
					(3.880)	(4.016)	(0.067)
NLD	549	1000	1009	1031	2.944	3.596	0.809
					(4.509)	(5.231)	(0.006)
NOR	510	1008	1006	1000	2.040	2.120	0.912
					(2.849)	(3.462)	(0.031)
POL	908	1021	1048	1024	2.154	1.331	0.086
					(4.286)	(3.558)	(0.011)
PRT	520	1000	1000	1000	4.570	3.506	0.230
					(7.395)	(6.614)	(0.026)
SPN	500	1000	1000	500	3.893	1.544	0.350
					(6.310)	(3.739)	(0.056)
SWE	.	1000	1000	1000	2.322	1.558	0.977
					(3.718)	(2.747)	(0.037)

Table 4.1: Sample sizes and mean values for aid variables by country

	2005	2006	2007	2008	Desired Aid	Perceived Aid	Actual Aid
SWI	500	1000	1037	1005	3.983	3.330	0.407
					(5.432)	(5.148)	(0.026)
TUR	2036	2045	2015	2004	2.378	1.055	0.138
					(5.466)	(3.434)	(0.038)
UK	1031	1025	1000	1009	5.520	4.420	0.443
					(7.190)	(6.841)	(0.055)
USA	504	1022	1019	1000	3.190	7.516	0.184
					(5.589)	(9.316)	(0.022)
Mean	827	1026	1022	1027	3.142	2.65	0.371
					(5.21)	(5.082)	(0.264)
Observations	19012	23597	24523	22586	63024	55864	89718

Standard deviations are in parentheses. For Desired Aid and Perceived Aid, standard deviations are over time and across individuals; for Actual Aid, they are for the country-level average over time

Third, there is considerable variation between countries in Desired Aid, Perceived Aid and Actual Aid. Nordic governments appear to be the most generous in terms of actual donations: Sweden, Norway and Denmark, along with Luxembourg and the Netherlands, all allocate over 0.8% of GNI to foreign aid on average, confirming that they are different not only in terms of allocation criteria, but also in terms of allocation quantities. At the other end, Korea, Poland, the Czech Republic, Turkey and the USA are the only countries in the sample to donate less than a fifth of one per cent. That fact seems not to affect citizens' perceptions of their own government's generosity, however: US citizens believe they spend over 40 times more than they actually do on foreign aid. We see this graphically in Figure 4.3, where in the top two panels we show country averages of desired aid and 'aid illusion', measured as perceived aid divided by actual aid. Interestingly, the most generous countries also seem to be the least deluded. Four of the five least deluded countries—Sweden, Norway, Denmark and Luxembourg—are also four of the five largest donors. Irish citizens, meanwhile, are the most generous in terms of desired aid, with average preferences slightly higher than 6% of GNI. Germany and the UK are the only other countries to have average preferences of over 5%. The only countries where average desired aid is at 2% or lower are Finland, the Czech Republic, Korea and Austria.

However, we must exercise caution while interpreting this data. Panel 2 tells us that aid illusion varies significantly from country to country—a generous citizen, say, with an accurate grasp of national statistics may appear more parsimonious than the

deluded miser. In order to overcome this bias, we construct a weighted measure of support for aid called the Democratic Aid Level Preference Index (DALPI), defined as:

$$DALPI_c = DA_c^{Med} * \frac{ODA_c}{PerA_c} \tag{4.1}$$

where DA_c^{Med} is the median desired aid as a percentage of GNI in country c; $PerA_c$ is perceived aid, and ODA_c is actual aid.[5] The DALPI allows us to order citizens' democratic preferences for foreign aid while taking aid illusion into account.[6] The revised order is presented in panel 3, where we see Sweden, Denmark, Norway, Luxembourg and Iceland as the major illusion-adjusted foreign aid supporters in the sample, whereas Poland, Portugal, Korea, USA and the Czech Republic are the lowest supporters. In the next section we turn our attention away from a descriptive analysis of the data toward a more formal explanation of support for aid.

4.2 Support for Foreign Aid

4.2.1 Model

We propose a standard political-economy model to explain individual support for ODA. As mentioned above, we follow the literature on philanthropy in assuming altruistic utility functions (Andreoni, 2006). Kolm (1969) and Hochman and Rogers (1969) are credited with noting that the object of altruism, in this case foreign aid, can be considered a public good. Foreign aid donations create positive externalities for altruistic agents. Further, the utility derived from foreign aid is both non-rivalrous and non-excludable. Chong and Gradstein (2008) and Lahiri and Schweinberger (2006) follow the approach in this context.[7]

We begin with a standard median voter model in which we analyse a country with a continuum of altruistic citizens, where each citizen $i \in [0, 1]$ is assumed to have income y_i. Average income in the society is denoted by $\overline{y} = \int_0^1 y_1 di$. We hypothesise that

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[5] Again, we present means across the four sample years for ease of presentation.

[6] We take the median value of desired aid in order to reflect the democratic preferences of an electorate in the usual Median Voter Theorem setting of a direct pairwise vote with single-peaked preferences.

[7] Our theory differs from Chong and Gradstein (2008) on three grounds: they impose an iso-elastic functional form; they include private aid due to the 'warm glow' rationale; and they do not include a cost of taxation. They arrive at a prediction for the effect of an individual's income on their optimal tax that is dependent on the elasticity of substitution. If we incorporate private aid, we also arrive at predictions that depend upon ambiguous assumptions of functional form (available on request). We view the conditions as an empirical matter.

the political system of this economy determines a non-negative tax rate $\tau \in [0,1]$ proportional to income, the proceeds of which are distributed as foreign aid A to other countries. However, we assume it is costly to raise taxes: an aggregate cost from taxation distortions, $C(\tau)\overline{y}$, arises when the tax rate is τ. The function $C(.)$ has the following properties: $C(0) = 0$, $C'(.) > 0$, $C''(.) > 0$, and C is twice continuously differentiable.

The government then faces the following budget constraint:

$$A = \int_0^1 \tau y_i di - C(\tau)\overline{y} = (\tau - C(\tau))\overline{y}. \tag{4.2}$$

All individuals in this economy maximise the utility function $u(\hat{y_i}, A)$ (with $u_1(.)$, $u_2(.) > 0$; u is quasi-concave and twice continuously differentiable) where $\hat{y}_i$ is post-tax income $(1-\tau)y_i$. We can express indirect utility as

$$V(y_i \mid \tau) = u[(1-\tau)y_i, (\tau - C(\tau))\overline{y}]. \tag{4.3}$$

The preferred tax rate of i can easily be derived from maximizing unconstrained indirect utility (given that, under the properties of functions u and C, function V is strictly concave and twice continuously differentiable). The first order conditions are thus:

$$-u_i(.)y_i + u_2(.)(1 - C'(\tau))\overline{y} = 0 \tag{4.4}$$

if $\tau_i > 0$, and

$$-u_i(.)y_i + u_2(.)(1 - C'(\tau))\overline{y} \leq 0 \tag{4.5}$$

if $\tau_i = 0$.

Proposition 1 *The individually optimal tax rate function $\tau(y_i)$ is decreasing in y_i.*

Proof. Using the implicit function theorem for the case of $\tau_i > 0$, we know $\tau'(y_i)$ exists and is given by

$$\tau'(y_i) = -\frac{u_1(.)}{u_2(.)C''(\tau)\overline{y}} \tag{4.6}$$

From the properties of functions u and C, we can conclude that this derivative is strictly negative. ■

We simply model the political decision on τ as one subject to an open agenda (any individual can propose a new pairwise vote between any alternative and the winner from the previous vote) direct democracy. Using the Median Voter Theorem, provided that we have single peaked preferences (from strict concavity of function V), the outcome of the assumed political decision will be $\tau_{Med\{y_i\}}$, the individually optimal tax rate for the median income voter.

4.2.2 Empirical Analysis

The theory lends itself to simple empirical testing using both micro and macro-level variables. From a micro-level perspective, Proposition 1 can be tested with the following empirical specification:

$$DA_{ict} = \alpha + \beta y_{ict} + X_{ict}\gamma + C_c\Theta + T_t\mu + e_{ict} \tag{4.7}$$

where DA is desired aid for individual i in country c in year t; y is income, measured subjectively and coded relative to individuals in country c from 1 (bottom two quintiles) to 3 (richest quintile); X is a vector of individual covariates that includes age, gender, education, employment status, religion, ideology and aid illusion; and C and T are country and year dummies respectively. All individual covariates are presented in Table 4.2 by levels of desired aid. We expect the β coefficient to carry a negative sign, indicating that the individually optimal level of foreign aid, financed through tax revenues, is decreasing in income.

The results presented in Table 4.3 generally support the theory's prediction. In column (1) we see that, on average, respondents' preferred level of foreign aid is decreasing in relative income when controlling for age, gender, education and employment status. The inclusion of attitudinal and illusion controls (and the consequent loss of data) in column (2) weakens the results slightly, whereas the main specification in column (3), which incorporates country and year dummies, corroborates the theory comprehensively: income negatively determines preferences for tax-funded foreign aid.

A number of auxiliary findings in Table 4.3 are also interesting to note. Age and gender matter: young people and women are significantly more supportive of aid. The associations with education and unemployment presented in columns (1) and (2) are not robust to the inclusion of year and country fixed effects; nor, interestingly, are many of the links with religion. In the end, Roman Catholics, Protestants, 'other' Christians, Muslims and 'other' religions are the groups who exhibit significantly more support for foreign aid than non religious people. On the contrary, the results for ideology are robust across both specifications in which they are included: self-declared left wingers are significantly more supportive of aid then right wingers. Finally, and unsurprisingly, aid illusion leads individuals to support higher donations.

Table 4.2: Mean Values by Desired Aid

Desired Aid	N	Ideology L(1) - R(5)	Age (1-4)	Income (1-3)	Education			Employment			
					None / Basic	Secondary	High	Working	Unemp.	Not Working	Retired
0	4575	3.23	2.26	1.6	0.3	0.47	0.23	0.56	0.07	0.18	0.19
0.05	3344	3.22	2.26	1.68	0.22	0.52	0.26	0.64	0.05	0.16	0.15
0.15	4193	3.18	2.27	1.71	0.2	0.52	0.29	0.63	0.04	0.17	0.16
0.35	5680	3.11	2.29	1.81	0.16	0.52	0.32	0.64	0.47	0.15	0.16
0.75	11116	3.03	2.36	1.83	0.15	0.49	0.36	0.63	0.04	0.14	0.18
1.5	11983	2.94	2.32	1.85	0.13	0.49	0.38	0.63	0.04	0.15	0.17
2.5	6048	2.88	2.29	1.81	0.14	0.47	0.38	0.61	0.04	0.17	0.17
4	6398	2.84	2.25	1.84	0.12	0.47	0.4	0.61	0.04	0.17	0.18
7.5	5415	2.83	2.2	1.79	0.13	0.47	0.4	0.6	0.05	0.18	0.16
17.5	2563	2.82	2.03	1.7	0.18	0.5	0.32	0.57	0.06	0.23	0.13
25	1709	2.93	1.97	1.6	0.23	0.5	0.27	0.56	0.08	0.23	0.13
Full Sample	89718	3.00	2.26	1.73	0.21	0.49	0.3	0.59	0.05	0.19	0.17

Table 4.3: Sample Sizes and Mean Values for Aid Variables by Country

	(1)	(2)	(3)
	Desired Aid		
Income: Medium / Medium High	-0.138***	-0.038	-0.102*
	(0.051)	(0.056)	(0.055)
Income: High	-0.317***	-0.157**	-0.191***
	(0.062)	(0.067)	(0.067)
Age: 30-50	-1.008***	-0.934***	-0.859***
	(0.071)	(0.081)	(0.078)
Age 51-65	-1.219***	-1.229***	-1.181***
	(0.077)	(0.086)	(0.082)
Age 65+	-1.329***	-1.283***	-1.236***
	(0.105)	(0.116)	(0.112)
Male	-0.849***	-0.649***	-0.652***
	(0.046)	(0.051)	(0.049)
Education: Secondary	0.020	0.070	-0.012
	(0.069)	(0.078)	(0.077)
Education: Higher Level	0.237***	0.265***	-0.022
	(0.074)	(0.082)	(0.081)
Employment: Unemployed	0.561***	0.438***	0.161
	(0.127)	(0.139)	(0.137)
Employment: Not Working	-0.070	-0.015	0.067
	(0.075)	(0.085)	(0.082)
Employment: Retired	0.173**	0.189**	0.074
	(0.077)	(0.081)	(0.079)
Religion: Roman Catholic		0.971***	0.205**
		(0.071)	(0.081)
Religion: Russian / Eastern Orth.		0.758*	0.405
		(0.396)	(0.370)
Religion: Protestant		0.370***	0.187***
		(0.059)	(0.065)
Religion: Other Christian		0.760***	0.454***
		(0.098)	(0.124)
Religion: Hindu		2.452**	1.323
		(1.076)	(1.078)
Religion: Muslim		-0.142	0.815*
		(0.120)	(0.494)
Religion: Jewish		1.265**	0.830
		(0.557)	(0.558)
Religion: Buddhist		-0.355***	0.005
		(0.122)	(0.145)
Religion: Other		1.136***	0.329**
		(0.163)	(0.160)

Sample Sizes and Mean Values for Aid Variables by Country

	(1)	(2)	(3)
		Desired Aid	
Ideology: Left		-0.589***	-0.791***
		(0.137)	(0.133)
Ideology: Centre		-1.251***	-1.133***
		(0.136)	(0.132)
Ideology: Right		-1.355***	-1.293***
		(0.137)	(0.133)
Ideology: Very Right		-1.693***	-1.374***
		(0.163)	(0.157)
Illusion		0.060***	0.064***
		(0.002)	(0.003)
Year FE	No	No	Yes
Country FE	No	No	Yes
Observations	51,584	35,458	35,458
R-squared	0.017	0.128	0.193

Robust standard errors in parentheses. All models estimated by OLS. Omitted categories: Income: Low/Medium Low; Age: ¡ 30; Education: No Education; Only Basic Education; Employment: Working Full/Part Time; Religion: None; Ideology: Very Left Wing. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Turning to macro-level variables, the theory predicts that inequality (or, more specifically, the ratio of the mean to the median income) increases support for aid. This is because, in our framework, the extent to which the median voter earns less income than the country mean will positively determine her preferred tax rate (assuming a proportional system), and hence, the optimal level of foreign aid for that country's citizens.[8] We can also see from the first order conditions that desired aid is predicted to be a positive function of per capita GDP (which simply increases the government budget constraint).[9] We test these propositions with the following specification:

$$DA_{ict} = a + \delta I_c + \beta y_{ict} + \varphi Y_{ct} + X_{ict}\gamma + K_{ct}\Theta + G_{ct} + T_t\mu + e_{ict} \tag{4.8}$$

[8]It can be shown trivially from (4) that ${}^{d\tau}/_{d\frac{\overline{y}}{y_i}} > 0$ for the case of $\tau_i > 0$; and, by the median voter theorem, the optimal outcome is $i = i_{Med}\{y_i\}$

[9]In this case, $\frac{d\tau}{d\bar{y}} = \frac{(1 - C'(\tau))}{C''(\tau)\bar{y}} > 0$ so long as $C'(\tau) < 1$, which is a necessary condition for raising taxes.

where I is inequality, measured as the ratio of the mean to the median income (both taken from the OECD)[10] in country c; y and X are as above; Y is the logarithm of per capita GDP in 2000 US dollars; K is a vector of standard country-level controls that contains trade openness, growth and the logarithms of population and size, all of which are taken from the World Development Indicators; G is a vector of the six Worldwide Governance Indicator variables: Voice and Accountability, Stability, Government Effectiveness, Regulatory Quality, Rule of Law and Control of Corruption (Kaufmann, Kraay and Mastruzzi, 2010), and T is year. As the OECD provides unique 'mid 2000s' values of mean and median income for each country, we cannot include country dummies. The theory predicts that both δ and φ will be significantly larger than zero.

In Table 4.4 we see from the empirical results that the theory is broadly supported across a wide range of specifications. In column (1) we include no controls for governance; in columns (2), (3) and (4) we include pairs of controls for democracy, policy and institutions respectively; in column (5) we include all governance controls; whereas in column (6) we include only the principal component of the governance variables. Individual and other country controls, together with year dummies, are included in every regression. In all but one specification, inequality is shown to increase support for foreign aid. Similar results are obtained in Table 4.5 when we use the Gini coefficient (again measured by the OECD) as an alternative to the ratio of mean to median incomes. Figure 4.4 presents linear fits (with no controls) of country-level desired aid with both measures.

Our proposition on GDP is also strongly reinforced by the data—in nine of the 12 specifications the log of per capita GDP is found to have a significantly positive effect on desired aid. In most cases the coefficient is higher than one, indicating that, on average, a 1% increase in GDP leads to an increase in desired aid of larger than 1% of national income. Interestingly, Voice and Accountability and Control of Corruption appear to have strong positive effects on aid preferences, perhaps reflecting the effects of popular confidence in a government's ability to deliver aid efficiently.

Taken together, our results show that, within countries, those who earn higher incomes, and thus pay higher levels of tax, are likely to support government aid donations less than those who pay lower levels. By extension, support for aid is higher in more unequal countries. We also find evidence to support our proposition that desired aid is larger in countries that have higher levels of GDP per capita.

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[10]Data on real mean and real median incomes are available on in the *Income Distribution – Inequality section* (via *Social and Welfare Statistics ¿ Social Protection*) of the OECD's statistical website at http://stats.oecd.org.

Table 4.4: Ratio of Mean to Median Income and Desired Aid

	(1)	(2)	(3)	(4)	(5)	(6)
			Desired Aid			
Mean:Median Income	7.567*	8.014**	4.374	8.101**	5.608*	8.720**
	(4.328)	(3.033)	(4.166)	(3.878)	(3.054)	(3.870)
Log GDP	1.313**	0.523	1.217**	1.194**	0.997*	0.936
	(0.539)	(0.600)	(0.584)	(0.570)	(0.530)	(0.644)
Log Population	0.166	0.243	0.075	0.258	0.364	0.232
	(0.180)	(0.194)	(0.161)	(0.161)	(0.219)	(0.181)
Log Size	-0.116	-0.170	-0.092	-0.109	-0.119	-0.161
	(0.269)	(0.270)	(0.268)	(0.253)	(0.272)	(0.262)
Trade Openness	0.001	-0.001	-0.003	0.001	-0.001	-0.001
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Growth	-0.108	0.035	-0.076	-0.002	0.123	-0.076
	(0.121)	(0.108)	(0.113)	(0.113)	(0.103)	(0.122)
Democracy: Voice and Accountability		3.194***			4.166***	
		(0.753)			(0.935)	
Democracy: Stability		-0.912			-0.496	
		(0.695)			(0.682)	
Policy: Government Effectiveness			-1.363**		-0.936	
			(0.652)		(1.075)	
Policy: Regulatory Quality			1.947***		-0.321	
			(0.609)		(0.903)	
Institutions: Rule of Law				-2.687**	-2.713*	
				(1.096)	(1.514)	
Institutions: Corruption				2.037**	1.563*	
				(0.823)	(0.815)	
Principal component						0.199
						(0.191)
Observations	33,771	33,771	33,771	33,771	33,771	33,771
R-squared	0.155	0.164	0.159	0.161	0.171	0.157

Standard errors are clustered at the country level. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 4.5: Gini and Desired Aid

	(1)	(2)	(3)	(4)	(5)	(6)
	Desired Aid					
Gini	11.151**	9.118*	9.421	10.905**	4.384	11.284**
	(4.320)	(4.718)	(5.725)	(4.298)	(6.385)	(4.247)
Log GDP	1.389**	0.830	1.254**	1.287**	1.053*	1.171*
	(0.524)	(0.613)	(0.594)	(0.574)	(0.514)	(0.647)
Log Population	0.122	0.130	0.094	0.221	0.248	0.169
	(0.174)	(0.222)	(0.165)	(0.150)	(0.243)	(0.187)
Log Size	-0.209	-0.198	-0.191	-0.166	-0.111	-0.230
	(0.350)	(0.340)	(0.357)	(0.329)	(0.327)	(0.345)
Trade Openness	-0.005	-0.005	-0.006	-0.004	-0.003	-0.005
	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
Growth	0.014	0.090	0.004	0.128	0.119	0.038
	(0.165)	(0.158)	(0.167)	(0.157)	(0.154)	(0.160)
Democracy: Voice and Accountability		2.391**			3.603**	
		(1.027)			(1.466)	
Democracy: Stability		-1.043			-0.734	
		(1.019)			(1.082)	
Policy: Government Effectiveness			-0.477		-0.896	
			(0.970)		(0.900)	
Policy: Regulatory Quality			0.891		-0.045	
			(1.130)		(1.178)	
Institutions: Rule of Law				-2.906**	-2.618	
				(1.189)	(1.651)	
Institutions: Corruption				2.127**	1.435	
				(0.954)	(0.856)	
Scores for component 1						0.127
						(0.180)
Observations	29,025	29,025	29,025	29,025	29,025	29,025
R-squared	0.162	0.166	0.162	0.168	0.172	0.162

Standard errors are clustered at the country level. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

4.3 Special Interest Politics and the Democratic Aid Deficit

A particularly interesting advantage of this dataset is the opportunity it affords us to examine the democratic aid deficit, which we define as the difference between citizen preferences and government allocations, or

$$DD_{ct} = DA_{ct}^{Med} - ODA_{ct}. \tag{4.9}$$

Note that, in calculating the measure, we subtract official aid from the median value of Desired Aid rather than the mean. This follows from the Median Voter Theorem, which states that, if voters have single-peaked preferences, the median voter's preferred level of policy will be the chosen policy outcome. In effect, in a 'pure' democracy, we should observe $DA_{ct}^{Med} = ODA_{ct}$, i.e. $DD_{ct} = 0$.

An immediate point of interest, then, concerns the size (μ_{DD}=2.89%) and variation (σ_{DD}=1.53%) of the democratic deficit. The coefficient of variation for the illusion-adjusted alternative, that is

$$\frac{\sigma_{DD^a}}{\mu_{DD^a}}, \; where \; DD_{ct}^a = DD_{ct} * [\frac{ODA_{ct}}{PerA_{ct}}], \tag{4.10}$$

is 0.64, which, compared with the non-adjusted coefficient of 0.53, suggests that the significant variation in the democratic deficit is not simply a result of varying perceptions of aid. The extent of country-level variation in presented graphically in Figure 4.5, which shows the relative sizes of desired aid (blue) and actual aid (red) for each country. The red vertical line represents parity, i.e., $DD_{ct} = 0$. The ratio ranges from almost parity in Sweden to around 9:1 in Poland, Germany and Korea.

This illustration begs the question: what is causing this deficit? To answer this, we turn to the literature on special interest politics. We propose a simple extension of the theory using a Grossman and Helpman (1996, 2001) style adjustment to the government utility function, whereby the top-income-agent may transfer resources to the government in order to 'buy' lower aid (i.e., a lower tax rate τ).[11]

In this set-up, political power not only comes from an electoral mandate, but also from an individual's wealth and her marginal willingness to pay for changes in policy through bribes or campaign contributions. We assume that n agents can offer the policymaker a contribution function $\hat{\gamma}^j(.)$ in order to influence the choice of tax rate τ. The utility of an agent is

$$V^j(\tau) - \gamma^j(\tau), \tag{4.11}$$

where $V^j(\tau)$ is the utility derived by j from tax rate τ and $\gamma^j(\tau)$ represents the contri-

[11] A summary of these models is presented in Acemoglu and Robinson (2006)

bution paid to the policymaker.[12] The politician in power maximizes a utility function of the form

$$G(\tau) \equiv \gamma^l(\tau) + a \sum_{j=1}^{n} V^j(\tau), \tag{4.12}$$

where the first term is the monetary receipts of the policymaker from agent $l \in j$ and the second is the un-weighted sum of all indirect utility functions (utilitarian aggregate welfare), with a representing the extent to which the politician is concerned about aggregate welfare.

We assume the following sequence: (i) agents who wish to divert the policymaker away from imposing a tax rate τ that maximises utilitarian aggregate welfare offer a payment schedule; (ii) the politician chooses τ. The equilibrium of this game, composed of the contribution function $\hat{\gamma}^l(.) \in \hat{\gamma}^j(.)$ and the tax rate τ^*, has the following features: $0 \leq \hat{\gamma}^l(\tau) \leq V^j(\tau)$, which implies that agents do not make contributions that give them negative utility; the politician chooses the tax rate τ that maximises his welfare given contribution function $\hat{\gamma}^l(.)$, i.e.

$$\tau^* \in \arg \max_{\tau \in [0,1]} G(\tau \mid \hat{\gamma}^l(.)) \tag{4.13}$$

and there are no profitable deviations for agent l, implying that

$$\tau^* \in \arg \max_{\tau \in [0,1]} [V^l(\tau) - \hat{\gamma}^l(\tau)) + G(\tau \mid \hat{\gamma}^l(.))]. \tag{4.14}$$

The equilibrium can be simplified to the solution of the weighted sum of utilities, for which we only need to assume differentiable contribution functions. The first order conditions from the above two expressions respectively give:

$$\frac{d\gamma^l(\tau)}{d\tau} + a \sum_{j=1}^{n} \frac{dV^j(\tau)}{d\tau} = 0 \tag{4.15}$$

and

$$\frac{dV^l(\tau)}{d\tau} - \frac{d\hat{\gamma}^l(\tau)}{d\tau} + \frac{d\hat{\gamma}^l(\tau)}{d\tau} + a \sum_{j=1}^{n} \frac{dV^j(\tau)}{d\tau} = 0. \tag{4.16}$$

Combining these expressions obtains

$$\frac{d\hat{\gamma}^l(\tau)}{d\tau} = \frac{dV^l(\tau)}{d\tau}, \tag{4.17}$$

[12] For simplicity, agents cannot organise into groups. Relaxing this assumption does not change the results.

which gives us two important implications. First, it shows that l is willing to pay the marginal benefit for a change in the tax rate. As the largest beneficiary of a change in the tax rate in a proportional system is by definition the highest income earner, we can deduce that l is the top-income agent r.[13] Second, we can now substitute the first term in the policymaker's objective function and write the resulting tax rate from this game as the solution of

$$\arg \max_{\tau \in [0,1]} V^r(\tau) + a \sum_{j=1}^{n} V^j(\tau) = (1+a)V^r(\tau) + a(\sum_{j=1}^{a} V^j(\tau) - V^r(\tau)), \tag{4.18}$$

which implies that, to the extent that money can influence politics (i.e. as a decreases), the resulting level of aid chosen by the government will tend towards the richest agent's preferred rate. In our application, the rich prefer a lower τ (for reasons shown in Section 4.2), and hence a lower level of foreign aid donations A than the median voter's preferred choice. We are left with a clear empirical proposition: the influence of money on politics will directly decrease the level of aid donated by governments.

4.3.1 Estimation

To test this proposition, we turn to the Executive Opinion Survey (EOS) from the World Economic Forum's Global Competitiveness Report 2004-2005.[14] The EOS is a mail-based survey of executives from an average of 84 firms of widely varying sizes, sectors, and types of ownerships in 104 countries. We limit the data to the relevant donor countries in our analysis, and average responses to the questions on various forms of state capture at the country level, presented in Table 4.6.[15]

We argue that these variables proxy well for the influence of monetary donations, both legal and illegal, on policy outcomes in the sample countries. We propose that by lowering actual aid, these lobbying activities explain variation in the democratic aid deficit across countries. Figure 4.6 presents graphical corroboration. In a linear fit, all seven lobbying variable are clearly negatively correlated with actual aid.

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[13] We dismiss the influence of the bottom-income agent.

[14] Firms are stratified by size and sector. More information on the Executive Opinion Survey is available in Kaufmann and Vicente (2011) and on the official website at http://bit.ly/90RE67. The most recent sampling procedures are described on page 77 of the Global Competitiveness Report at http://www3.weforum.org/docs/WEF_GCR_Report_2011-12.pdf

[15] Country averages are presented in Table 4.7

Table 4.6: Lobby Variables

Lobby 1: Financial Dishonesty	Public trust in the financial honesty of politicians is:		
	Very high	1 2 3 4 5 6 7	Very low
Lobby 2: Bias	When deciding on policies and contracts, government officials:		
	Are neutral among firms and individuals	1 2 3 4 5 6 7	Usually favour well-connected firms and individuals
Lobby 3: Illegal Donations	How common are illegal donations to political parties in your country?		
	Never occur	1 2 3 4 5 6 7	Common
Lobby 4: Effect of Contributions	To what extent do legal contributions to political parties have a direct influence on specific public policy outcomes?		
	Little direct influence on policy	1 2 3 4 5 6 7	Very close link between donations and policy
Lobby 5: Budget Corruption	In your country, diversion of public funds to companies, individuals or groups due to corruption		
	Never occurs	1 2 3 4 5 6 7	Is common
Lobby 6: Bribery	In your industry, how commonly would you estimate that firms make undocumented extra payments or bribes connected with the following: Influencing of laws and policies, regulations or decrees to favour selected business interests		
	Never occur	1 2 3 4 5 6 7	Common
Lobby 7: Private Influence	How much influence do you think the individuals or firms with close personal ties to political leaders actually had on recently enacted national laws and regulations that have a substantial impact on your business?		
	No influence at all	1 2 3 4 5 6 7	Enormous influence

Source: Executive Opinion Survey (EOS), Global Competitiveness Report 2004-2005 (World Economic Forum).

Figure 4.7 highlights very clearly how well the theory is reflected in the data: when countries are split by the median value of actual aid (y-axis) and the mean lobbying score (x-axis), only four countries are marginally outside the two appropriate quadrants. The vast majority of countries have either a combination of clean politics and high foreign aid (e.g. Northern European countries), or a combination of malleable policy makers and low foreign aid (bottom right quadrant).

To formally test the theory, we use the following specification:

$$ODA_{ct} = a + \varphi L_c + \varsigma DA_{ct}^{Med} + K_{ct}\theta + G_{ct}\vartheta + T_t\mu + e_{ct} \tag{4.19}$$

where ODA_{ct} is actual aid for country c in year t, L is a lobbying variable and DA_{ct}^{Med} is the median value of desired aid in each country-year. As L is increasing in lobbying activity, we expect φ to be negative. In line with our theory, we also expect ς to be positive.

In Tables 4.8 to 4.11 we see broad support for the theory across various specifications. Table 4.8 presents the most rudimentary specification, which omits all potential correlates of desired aid other than the perceived aid control. We see that all seven lobby variables are strong negative predictors of actual aid. The evidence for the impact of desired aid is less convincing however, showing a significant positive effect in only three of the nine specifications.

In Table 4.9 we include country controls and find similar level of support for the influence of money on aid policy. We also see that the inclusion of these controls confounds any relevant effect of desired aid. To explain this, we propose that the impact of the median voter's preference on policy outcomes is dependent on the fluidity of democracy in any given country. It is reasonable to assume that the responsiveness of a government to its citizens is by definition a prerequisite for the median voter's preference to determine the level of aid chosen by the policymaker. This may extend beyond the case of lobbying as measured by our seven variables, and so in Table 4.10 we interact the median voter's preference with the Voice and Accountability variable from the World Governance Indicators.

This adjustment reveals a large and significantly positive effect of voter preferences in the presence of democracy. Perhaps unusually, the independent effects of desired aid are largely negative. These results are tempered somewhat in Table 4.11, which shows that, with added country controls, the interaction term is significant only in the specifications where lobbying is either absent, measured by private influence or measured by bribery.[16]

[16]The inclusion of the principal component of the other WGI governance variables does not signif-

Table 4.7: Mean Lobby Scores by Country, 1 (Low) to 7 (High)

	L1: Financial Honesty	L2: Bias	L3: Illegal Donations	L4: Effect of Contributions	L5: Budget Corruption	L6: Bribery	L7: Private Influence	Mean Lobby
AUT	4.000	3.930	2.940	3.450	2.240	2.160	2.940	3.094
CAN	4.990	4.520	3.480	3.900	3.180	2.520	3.880	3.781
CZR	5.850	4.920	4.960	3.980	5.230	3.630	4.800	4.767
DNK	2.160	2.520	1.440	2.040	1.480	1.370	2.950	1.994
FIN	2.270	2.610	1.920	2.290	1.390	1.480	3.000	2.137
FRA	5.030	3.950	3.850	3.710	3.070	2.550	3.990	3.736
GER	4.200	3.480	3.920	3.080	2.270	2.050	3.210	3.173
GRC	5.210	5.240	4.780	4.670	4.070	3.710	4.240	4.560
ICE	2.400	2.790	1.960	2.520	1.460	1.320	2.780	2.176
IRL	5.320	3.800	4.410	4.450	2.500	2.520	3.470	3.781
ITA	6.020	5.200	5.280	4.590	4.620	3.290	3.750	4.679
JPN	4.870	3.640	3.470	4.070	3.220	2.400	2.840	3.501
KOR	6.310	4.590	5.320	4.660	4.440	3.800	3.510	4.661
LUX	2.410	3.500	2.500	2.440	2.320	2.070	3.810	2.721
NLD	2.820	3.080	1.800	1.730	2.040	2.030	2.890	2.341
NOR	2.390	2.390	1.740	2.300	1.570	1.700	2.280	2.053
POL	6.470	5.870	5.240	5.090	5.330	4.450	4.670	5.303
PRT	4.780	3.980	4.440	3.700	2.950	2.510	3.680	3.720
SPN	4.390	4.250	4.410	4.120	3.050	3.020	3.390	3.804
SWE	3.450	2.680	1.900	1.950	1.700	1.600	4.120	2.486
SWI	2.940	3.410	2.300	2.990	1.860	2.070	3.380	2.707
TUR	5.710	5.110	5.220	4.920	5.060	4.220	4.620	4.980
UK	3.610	3.020	2.240	2.440	1.790	1.870	3.200	2.596
USA	4.130	3.990	3.270	4.550	2.490	2.770	4.090	3.613
Mean	4.239	3.853	3.450	3.485	2.889	2.546	3.562	3.432
S.D.	1.389	0.967	1.336	1.060	1.269	0.899	0.658	1.018

Table 4.8: Actual Aid and Lobbying

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Actual Aid				
Lobby Variable:	None	L1: Financial Dishonesty	L2: Bias	L3: Illegal Donations	L4: Effect of Contributions	L5: Budget Corruption	L6 Bribery	L7: Private Influence	L8: Principal Component
		-0.145***	-0.209***	-0.157***	-0.213***	-0.156***	-0.220***	-0.195***	-0.083***
		(0.016)	(0.022)	(0.015)	(0.018)	(0.018)	(0.027)	(0.038)	(0.009)
Desired Aid	0.081***	0.037**	0.020	0.033**	0.004	0.004	-0.007	0.038	0.009
	(0.025)	(0.018)	(0.018)	(0.016)	(0.020)	(0.019)	(0.019)	(0.023)	(0.017)
Perceived Aid	-0.025*	-0.016	-0.026**	-0.026**	-0.002	-0.029**	-0.023*	-0.024*	-0.021*
	(0.012)	(0.012)	(0.011)	(0.011)	(0.008)	(0.013)	(0.012)	(0.013)	(0.011)
Observations	90	90	90	90	90	90	90	90	90
R-squared	0.065	0.576	0.564	0.616	0.666	0.519	0.511	0.270	0.592

Robust standard errors in parentheses. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

In all specifications, however, the effectiveness and prevalence of monetary contributions to policymakers is a strong negative determinant of DAC government aid donations. Taking the principal component as the primary scale of lobby influence, we show that a standard deviation increase leads to a decrease in foreign aid of 0.72 standard deviations—a higher proportional impact than any other variable, including GDP growth, trade and democracy.

4.3.2 Robustness: Private Aid and Crowding Out

The inclusion of a measure for private aid contributions in the 2006 survey round allows us to test an alternative explanation for our lobbying result. If government aid fully crowds out private aid, it could be optimal for governments to reduce ODA in countries where there are high preferences for private aid donations. If these are the same countries in which lobbies most affect policy, then it may be the case that this channel explains our result. We test this by addressing two questions: (i) does government ODA fully crowd out private contributions to foreign aid? And (ii) do citizens exhibit higher preferences for private aid in countries with high lobby scores? Our analysis is facilitated by the following survey question:

> Over the past year, what share of your household income was spent on private aid/charities for helping in developing countries?

The answers are coded using the same scale as that used for the previous aid measures. We refer to the variable as 'private aid'.

Crowding Out

We begin by testing for crowding out. The traditional argument states that increased government expenditure on foreign aid reduces private donations, as people optimally withdraw their direct contributions once they are obliged to donate to charities through involuntary taxes (Warr, 1982, Bergstrom et al., 1986). However, empirical evidence from Andreoni and Payne (2011a) presents a more complex picture. Using a panel of US charitable organisations, they observe that government grants crowd out private donations by around 76%; that is, each marginal dollar received from the

icantly change the findings. They are omitted due to possible collinearity with our main explanatory variable. We use Voice and Accountability as our measure of democracy in light of an almost complete lack of variation in the Polity alternative for our sample countries.

Table 4.9: Actual Aid and Lobbying (with Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Actual Aid				
Lobby Variable:	None	L1: Financial Dishonesty	L2: Bias	L3: Illegal Donations	L4: Effect of Contributions	L5: Budget Corruption	L6: Bribery	L7: Private Influence	L8: Principal Component
		-0.127***	-0.185***	-0.150***	-0.165***	-0.150***	-0.194***	-0.093	-0.084***
		(0.022)	(0.030)	(0.019)	(0.025)	(0.025)	(0.043)	(0.071)	(0.012)
Desired Aid	-0.006	0.010	-0.006	0.016	-0.017	-0.021	-0.029	-0.011	-0.010
	(0.018)	(0.019)	(0.018)	(0.016)	(0.019)	(0.020)	(0.020)	(0.018)	(0.018)
Perceived Aid	-0.049**	-0.039***	-0.044***	-0.047***	-0.040***	-0.041***	-0.049***	-0.043**	-0.041***
	(0.019)	(0.014)	(0.014)	(0.012)	(0.013)	(0.014)	(0.016)	(0.019)	(0.012)
Log GDP	0.249***	0.052	0.052	0.031	0.145***	0.013	0.088	0.171**	0.015
	(0.059)	(0.052)	(0.053)	(0.048)	(0.049)	(0.056)	(0.064)	(0.075)	(0.048)
Mean:Median Income	-0.122	-0.262	-0.024	0.035	0.180	-0.299	-0.049	-0.118	-0.064
	(0.463)	(0.308)	(0.325)	(0.304)	(0.324)	(0.335)	(0.357)	(0.453)	(0.302)
Log Population	0.012	0.052**	0.041**	0.052***	0.044**	0.047**	0.062**	0.013	0.053***
	(0.026)	(0.020)	(0.020)	(0.018)	(0.020)	(0.020)	(0.024)	(0.025)	(0.019)
Log Size	0.003	0.008	0.017	-0.000	0.015	0.009	0.002	0.023	0.017
	(0.021)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.016)	(0.021)	(0.016)
Trade Openness	0.002***	0.002***	0.003***	0.003***	0.002***	0.003***	0.003***	0.003***	0.003***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Growth	-0.041**	-0.033**	-0.034***	-0.034***	-0.019	-0.030**	-0.020	-0.041**	-0.027**
	(0.017)	(0.013)	(0.013)	(0.012)	(0.013)	(0.014)	(0.015)	(0.017)	(0.013)
Observations	86	86	86	86	86	86	86	86	86
R-squared	0.600	0.720	0.742	0.768	0.763	0.710	0.684	0.615	0.748

Robust standard errors in parentheses. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 4.10: Aid, Lobbying and Democracy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Actual Aid				
Lobby Variable:	None	L1: Financial Dishonesty	L2: Bias	L3: Illegal Donations	L4: Effect of Contributions	L5: Budget Corruption	L6 Bribery	L7: Private Influence	L8: Principal Component
		-0.105***	-0.143***	-0.126***	-0.187***	-0.097***	-0.136***	-0.077*	-0.064***
		(0.022)	(0.027)	(0.021)	(0.027)	(0.025)	(0.043)	(0.044)	(0.012)
Desired Aid	-0.356***	-0.123*	-0.190***	-0.107	-0.105	-0.231***	-0.265***	-0.366***	-0.176***
	(0.078)	(0.071)	(0.071)	(0.069)	(0.066)	(0.080)	(0.075)	(0.068)	(0.066)
Democracy: V&A	0.306***	0.115**	0.126**	0.069	0.031	0.118*	0.099	0.230***	0.058
	(0.069)	(0.057)	(0.056)	(0.051)	(0.051)	(0.070)	(0.081)	(0.085)	(0.060)
Desired Aid * V&A	0.236***	0.093**	0.128***	0.085*	0.071	0.151***	0.171***	0.241***	0.120***
	(0.053)	(0.046)	(0.046)	(0.045)	(0.046)	(0.052)	(0.048)	(0.046)	(0.043)
Perceived Aid	-0.011	-0.013	-0.018	-0.021*	-0.001	-0.019	-0.015	-0.011	-0.016
	(0.011)	(0.012)	(0.012)	(0.012)	(0.008)	(0.014)	(0.012)	(0.011)	(0.011)
Observations	90	90	90	90	90	90	90	90	90
R-squared	0.496	0.601	0.610	0.629	0.673	0.560	0.554	0.519	0.611

Robust standard errors in parentheses. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 4.11: Aid, Lobbying and Democracy (with Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Actual Aid				
Lobby Variable:	None	L1: Financial Dishonesty	L2: Bias	L3: Illegal Donations	L4: Effect of Contributions	L5: Budget Corruption	L6 Bribery	L7: Private Influence	L8: Principal Component
		-0.108***	-0.163***	-0.145***	-0.157***	-0.134***	-0.153***	-0.064	-0.078***
		(0.028)	(0.036)	(0.027)	(0.032)	(0.036)	(0.055)	(0.070)	(0.016)
Desired Aid	-0.377***	-0.143	-0.141	-0.042	-0.150*	-0.166	-0.268***	-0.363***	-0.117
	(0.091)	(0.099)	(0.093)	(0.098)	(0.086)	(0.109)	(0.095)	(0.092)	(0.090)
Democracy: V&A	0.080	0.025	0.035	-0.025	-0.066	-0.052	-0.021	0.069	-0.036
	(0.079)	(0.070)	(0.080)	(0.064)	(0.075)	(0.086)	(0.088)	(0.082)	(0.073)
Desired Aid * V&A	0.242***	0.099	0.088	0.040	0.092	0.100	0.163**	0.232***	0.073
	(0.058)	(0.064)	(0.061)	(0.063)	(0.058)	(0.071)	(0.062)	(0.060)	(0.059)
Perceived Aid	-0.059***	-0.045***	-0.049***	-0.047***	-0.040***	-0.042***	-0.052***	-0.054***	-0.041***
	(0.018)	(0.014)	(0.014)	(0.012)	(0.013)	(0.013)	(0.016)	(0.018)	(0.012)
Log GDP	0.246***	0.081	0.075	0.040	0.156***	0.043	0.126*	0.193**	0.034
	(0.061)	(0.056)	(0.055)	(0.054)	(0.049)	(0.066)	(0.065)	(0.075)	(0.051)
Mean:Median Income	0.188	-0.127	0.087	0.012	0.108	-0.305	0.008	0.164	-0.083
	(0.463)	(0.317)	(0.367)	(0.309)	(0.334)	(0.354)	(0.381)	(0.452)	(0.314)
Log Population	0.064***	0.066***	0.057**	0.053***	0.048**	0.052**	0.075***	0.061**	0.057***
	(0.024)	(0.021)	(0.022)	(0.020)	(0.022)	(0.021)	(0.024)	(0.023)	(0.020)
Log Size	-0.013	0.001	0.009	-0.001	0.012	0.005	-0.006	0.002	0.014
	(0.019)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.021)	(0.016)
Trade Openness	0.002***	0.002***	0.003***	0.003***	0.002***	0.003***	0.002***	0.002***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Growth	-0.020	-0.026*	-0.027*	-0.034**	-0.019	-0.029*	-0.016	-0.022	-0.026*
	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)	(0.014)
Observations	86	86	86	86	86	86	86	86	86
R-squared	0.664	0.728	0.749	0.769	0.768	0.716	0.702	0.672	0.752

Robust standard errors in parentheses. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

government leads to a reduction in revenues from private donors by 76 cents. They decompose the crowding out of donations into two channels: the classic (direct) channel, as described above; and the fundraising (indirect) channel, through which government grants to charities reduce their fundraising activities, and, as a consequence, private donations. They find no evidence of classic crowding out. Rather, they find that indirect crowding out is around 80%, whereas the direct effect of government grants is a crowding in of 4%. In effect, a $1000 government grant to a charity causes individuals to donate a further $40, while simultaneously causing the charity to forego fundraising activities that would have yielded $800 of private revenues.[17] Furthermore, the crowding-out estimate is an upper bound, as it does not account for the fall in costs associated with less fundraising activities. Accounting for this leads the authors to revise the indirect effect from 80% to 66%, resulting in an overall crowding out effect of around 62%.

An explanation for these results is described by Andreoni and Payne (2011b) and, more formally, by Name-Correa and Yildirim (2012). Three main actors interact in a game for the allocation of foreign aid—the government, who spends tax revenues on foreign aid through charitable organisations; charitable organisations, who solicit funds from government and individuals for reallocation abroad; and individuals who pay taxes and also donate to charity (Andreoni, 2006; List, 2011).

Charities compete with each other for donations. Citizens are concerned with the effectiveness of their donation—or the 'quality' of the charity—on which they only have partial information. Individuals are also sensitive to the costs of donating, which can be reduced through fundraising activities by charities, such as soliciting donations in person or over the phone. Fundraising can also provide information that can attract donors. However, excessive fundraising is wasteful, and can potentially deter donors (Rose-Ackerman, 1982; Okten and Weisbrod, 2000).

Grants from government can have two possible direct effects on individual donations. The first, described above, leads to the withdrawal of private donations in response to the imposition of an involuntary tax to fund charities (direct crowding out). But given that individuals only have partial information on the quality of charities, the fact that one was successful in applying for government support may convey a positive signal that could provoke giving (Andreoni, 1998, 2006; Vesterlund, 2003). The empirical literature outlined above suggests that the signalling effect dominates any direct crowding out effect.

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[17]Similar results are found in Andreoni and Payne (2003) and (2011b). In the latter, richer data from a Canadian panel allow the authors to decompose further the sources of crowding out. The largest effect is attributed to reduced special fundraising activities, such as gala events or sponsorships. A significant effect is also attributed to a decline in grants from foundations and other charities.

Finally, individuals do not give unless they are solicited directly (Meer and Rosen, 2011; Andreoni and Rao, 2011; and Andreoni et al, 2011 provide evidence of this effect) and fundraising, all else equal, is an undesirable activity that charities would rather eschew in order to devote resources to their stated mission. Under these assumptions, Name-Correa and Yildirim (2012) predict that government grants should result in lower fundraising efforts and incomplete crowding out, i.e., a reduction in private donations of less than unity.

Estimation

We test this proposition using the following specification:

$$PrivA_{ic} = a + \psi ODA + \lambda PerA_{ic} + X_{ic}\zeta + K_c\theta + G_c\vartheta + e_{ic}, \tag{4.20}$$

where $PrivA_{ic}$ is the share of household income spent on private foreign aid in a 12-month period from 2005 to 2006.

We are in a position to offer a novel test of the theoretical predictions on the interactions between public and private aid, as outlined above. By including measures of both official aid and individuals' perceived aid, we are able to reasonably decouple the indirect and direct effects of government aid on private donations. The coefficient λ describes individuals' direct reaction to their perceived changes in government aid. A negative λ indicates that, as individuals perceive government funding for foreign aid to be increasing, they adjust their voluntary contributions downward, providing corroboration of the classic case. A positive λ would provide confirmation of a dominant crowding in effect, in which perceived government support of charities reduces informational asymmetries by providing individuals with a signal of quality, as in Andreoni and Payne (2011a, 2011b). A statistically insignificant λ may reflect a 'warm-glow' motive for giving. This occurs when individuals are not solely motivated by the welfare of the recipient. Rather, they derive utility directly from the act of donating aid, perhaps due to the enjoyment of gratitude or the relief of guilt.[18] To the extent that warm glow motives dominate purely altruistic ones, private donations are more likely to be independent of government aid.

We note the particular importance of our measure for Perceived Aid here, as its negative correlation with actual aid (-0.044) clearly indicates that the latter would be an inappropriate explanatory variable in a regression that seeks to identify individuals' *direct* reactions to changes in government aid. To our knowledge, this is the first study

[18] Andreoni (2006) ascribes the foundation of this type of model to Becker (1974), Cornes and Sandler (1984), Steinberg (1987) and Andreoni (1989, 1990).

of its kind to use such a measure. On the contrary, actual aid is an apt measure for our test of indirect crowding out. Charities are acutely aware of their own revenues from government; a negative ψ indicates that they reduce fundraising activities and solicitations for contributions in response to an increase in government grants, which in turn leads to less private donations. Our control for individuals' direct reactions allows us to be confident in this type of interpretation. If this effect exhibits full crowding out, i.e., if $\psi \leq -1$, then the possibility remains that the alternative hypothesis outlined above is explaining our result.

Other correlates

This specification also allows us a look at other individual correlates of private aid donations. Evidence from US tax returns by Randolph (1995) and Auten et al. (2002), amongst others, estimate permanent income elasticities of demand for all private charitable giving of between 0.87 and 1.14, although only 1.1% of all US private household charitable contributions are allocated to foreign aid (Andreoni, 2006). Using data from the Center on Philanthropy Panel Study (COPPS), List (2012) finds that charitable contributions as a percentage of household income is U-shaped. A combination of four explanations is offered for this observation: poor people donate significantly higher shares of their income to religious organisations; young, highly-educated individuals with low current incomes expect their wages to rise in future, and can thus afford to donate more; a large fraction of high-percentage donors are high-asset, retired members of society who contribute according to their accumulated wealth rather than their current income; and, at the other end of the income distribution, a very small number of top income earners donate exceptional amounts to charity.[19]

While we do not have the panel data necessary to advance this particular strand of literature, we can offer insights into whether or not these findings can are applicable to spending on foreign aid. We can observe the effect of religion and education on private donations using the above specification, while the third explanation seems to be given some weight by Figure 4.8, in which we can see a clear increase in the share of low income earners amongst those who contribute at the higher end of the distribution, suggesting that low-income, high-wealth individuals constitute a significant proportion of high-transfer donors. The categorical nature of our income variable limits somewhat our detection of the final effect.

[19]List cites a Bank of America "High Net Worth Philanthropy" study conducted by the Center for Philanthropy at Indiana University in 2005, which found that 56.5% of total donations were attributed to the wealthiest 2.3% of contributors.

Results: OLS and Tobit

We present the OLS estimates of specification (4.20) in Table 4.12. Our main focus in these models is on perceived aid and actual aid. The results in both specifications show that crowding out is incomplete: focusing on column (2), which includes a set of controls for country governance, we can see that the effect of a 1 percentage point increase in ODA as a share of GNI decreases private donations as a share of household income by 0.84 of a percentage point. The results also provide strong support for the theories of indirect crowding out and direct crowding in posited above. A percentage point increase in perceived aid increases private giving by almost a tenth of a percentage point. This implies that classic crowding out is not a plausible explanation for the negative effect of government aid on private donations, leaving the fundraising channel as the most probable explanation—charities decrease fundraising activities in response to increased revenues from the government, leading to a drop in private donations. However, when individuals perceive an increase in government spending on foreign aid, they view it as a positive signal, and accordingly increase their own donations.

Looking at the other correlates, we find no significant effect of income on private aid, though we do find some support for the propositions outlined by List (2011). Respondents in the highest education category (who are also younger than other respondents) spend a higher share of their income on aid. Recoding the religion variables to a single binary measure that indicates whether or not an individual is religious or not, we find that non-religious respondents donate a fifth of a percentage point less of their income to charities than others.[20] Decomposing this measure, we see that Protestants donate significantly more private aid than non-religious respondents, despite having preferences for lower publicly funded foreign aid in T3C2.[21] Muslims donate significantly more than any other religious group – most probably in the form of remittances. The coefficients on age provide support for a U-shape relationship, as those aged between 30 and 50 donate a significantly lower share of their income to foreign aid than those aged lower than thirty, and respondents over 65 donate significantly more than those between 30 and 65. On average, retired respondents donate less than full- or part-time workers, controlling for age. Political ideology does not affect private giving – left-wingers are no more likely to spend on foreign aid than right-wingers, despite preferring higher government spending.

[20] Unreported.

[21] This comparison is chosen as neither model includes country fixed effects.

Table 4.12: Private Aid, Crowding In and Crowding Out

	(1)	(2)
	Private Aid	
Actual Aid	-0.528*	-0.840**
	(0.303)	(0.309)
Perceived Aid	0.089***	0.092***
	(0.017)	(0.017)
Income: Medium / Medium High	0.035	0.043
	(0.061)	(0.046)
Income: High	0.022	0.032
	(0.101)	(0.069)
Age: 30-50	-0.260**	-0.247**
	(0.109)	(0.106)
Age 51-65	-0.105	-0.093
	(0.110)	(0.118)
Age 65+	0.195	0.180
	(0.120)	(0.131)
Male	-0.071	-0.059
	(0.071)	(0.072)
Education: Secondary	0.140	0.129
	(0.090)	(0.085)
Education: Higher Level	0.398**	0.417***
	(0.147)	(0.138)
Employment: Unemployed	-0.027	-0.153
	(0.157)	(0.105)
Employment: Not Working	0.013	0.056
	(0.101)	(0.100)
Employment: Retired	-0.185**	-0.171**
	(0.086)	(0.079)
Religion: Roman Catholic	0.226*	0.102
	(0.113)	(0.118)
Religion: Russian / Eastern Orth.	0.125	-0.152
	(0.214)	(0.272)
Religion: Protestant	0.333*	0.273*
	(0.163)	(0.137)
Religion: Other Christian	0.529**	0.520**
	(0.247)	(0.201)
Religion: Hindu	-0.415*	-0.562**
	(0.213)	(0.208)
Religion: Muslim	1.295**	1.566**
	(0.484)	(0.653)
Religion: Jewish	0.546	0.510
	(0.863)	(0.829)
Religion: Buddhist	-0.419	0.117
	(0.327)	(0.181)

Table 4.12: Private Aid, Crowding In and Crowding Out

	(1)	(2)
	Private Aid	
Religion: Other	0.229	0.083
	(0.221)	(0.220)
Ideology: Left	0.094	0.062
	(0.109)	(0.093)
Ideology: Centre	-0.117	-0.078
	(0.103)	(0.106)
Ideology: Right	-0.046	-0.044
	(0.124)	(0.116)
Ideology: Very Right	-0.115	-0.087
	(0.176)	(0.171)
Log GDP	0.233	-0.055
	(0.285)	(0.319)
Log Population	0.025	-0.069
	(0.060)	(0.106)
Log Size	-0.002	0.017
	(0.089)	(0.070)
Trade Openness	0.003	0.003
	(0.003)	(0.002)
Growth	-0.166	-0.103
	(0.107)	(0.161)
Mean:Median Income	-0.230	-1.739
	(1.814)	(2.167)
Democracy: Voice and Accountability		1.566*
		(0.799)
Democracy: Stability		-1.202***
		(0.369)
Policy: Government Effectiveness		0.283
		(0.534)
Policy: Regulatory Quality		0.697
		(0.584)
Institutions: Rule of Law		-1.122*
		(0.607)
Institutions: Corruption		0.455
		(0.518)
Observations	8,492	8,492
R-squared	0.055	0.066

Standard errors clustered at the country level in parentheses. Omitted categories are Income: Low / Medium Low; Age < 30; Education: No Education / Only Basic Education; Employment: Working Full/Part Time; Religion: None; Ideology: Very Left Wing. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

We also run an alternative estimation to account for issues pertaining to functional form. Over 40% of respondents (7,288 of 18,117 in total) report donating no share of their income to foreign aid whatsoever. This necessitates a Tobit estimation to account for the censored data at zero. Running a Tobit regression on specification (4.20) gives us similar results to OLS, with a fundraising crowd out of -0.747 percentage points ($p = 0.04$) and a direct crowd-in of 0.096 ($p = 0$).[22] We note also that this estimation generates significantly positive coefficients for income: medium / medium high earners, on average, donate 0.18 ($p = 0.01$) more percentage points of their income than low / medium low earners, while the coefficient for the high group is 0.22 ($p = 0.02$). This is in contrast to our OLS results, which reveal no significant impact of income on private aid. Results on age, education, gender, religion and other controls are broadly in line with OLS results. Specifying an upper limit at 25—the highest value on the scale—does not change the results.[23]

Measurement, fundraising costs and Instrumental Variables

Before we settle on an interpretation of these results, there are a number of significant concerns which we must address. The first one is related to our measures for government and private aid. In Andreoni and Payne's analyses of crowding out (2003, 2011a, 2011b), both government grants and revenue from individual donors were measured in dollars. The reported crowding out result was thus simply obtained by observing the dollar-for-dollar effect of a marginal increase in government grants. In our case, however, ODA is measured as a percentage of GNI, while private aid is measured as a percentage of total household income. As the GNI of an economy is significantly larger than aggregated household incomes, the absolute magnitude of our result is biased upwards.

To account for this, we must multiply the ψ and λ coefficients by the average ratio of household income to GNI per capita in our sample. We turn to the OECD's *Society at a Glance* social indicators (OECD, 2011) to obtain a measure for household income in each of our sample countries. Specifically, we record the annual median equivalised disposable household income for each country in 2007, as the data does not exist for 2006. We then express this as a share of each country's corresponding GNI per capita (taken from the World Development Indicators), and find a sample mean of 0.599, with

[22] These are marginal effects.

[23] Tobit results are not reported.

a standard deviation of 0.074.[24]

Second, we recall from Andreoni and Payne (2011a) that, while a reduction in fundraising activities caused by government grants may indeed lower a charity's revenues, it also lowers its costs. In light of this, the authors lower their estimate of indirect crowding out from 80% to 66% (or by 0.175 of the initial effect). We interpret this with caution, as it is estimated from data on domestic charities in the USA, and does not necessarily describe the displacement of charity fundraising costs in our sample.

Accounting for the first source of bias adjusts our estimate for indirect crowding out to $-0.84 * 0.599 = -0.503$; and our estimate for the direct effect changes to $0.092 * 0.599 = 0.055$. Further adjusting the indirect effect to account for reduced fundraising costs gives a crowding out effect of $-0.503 * (1 - 0.175) = -0.415$.[25]

However, we argue that OLS estimates in this case are likely to be biased toward zero. It is easy to imagine cases where omitted variables such as natural disasters, famines or other humanitarian crises could be simultaneously influencing private aid, government aid and even charity fundraising. To account for this, we need an instrument for our actual aid variable—a determinant of government aid that is uncorrelated with the error term. Using panel data on charities, Andreoni and Payne exploited subnational variation in political representation in the US (2011a) and Canada (2011b) to arrive at the estimates reported above. As those were of a substantially higher magnitude than their initial OLS estimates, addressing this problem is particularly important in the context of this robustness check.

For our cross-country data, we instead look to our own results in Section 4.3 for an instrument, where we show clearly that lobbying activities have a substantially negative impact on government aid. We take this as a basis for our first stage regression, using the principal component of our vector of lobbying variables as an instrument. We assume that this variable is independent of the second-stage error term and is thus an appropriate instrument, noting that this specification includes controls for several country and individual level variables, as well as a comprehensive set of WGI governance measures.

We can also see from Table 4.11, that two other variables, the log of population and trade openness are potentially valid instruments, as they clearly influence official aid (perhaps reflecting the 'donor-interest' strategic motives uncovered in the literature on bilateral donations, as discussed in the first section of the chapter), while we argue they are unlikely to be correlated with the error term. Nevertheless, we present them only for robustness purposes; the lobbying variable is our main instrument. Our IV

[24] Both measures use current prices and PPPs.

[25] In the interest of prudence, we use our OLS estimate rather than our Tobit estimate as a base.

two stage least squares specification is as follows:

$$ODA_c = a_1 + IV_c\omega + X_{ic}\gamma_1 + K_c\theta_1 + G_c\vartheta_1 + u_{ic} \tag{4.21}$$

$$PrivA_{ic} = a_2 + \Psi_{IV}ODA'_c + X_{ic}\gamma_2 + K_c\theta_2 + G_c\vartheta_2 + e_{ic}, \tag{4.22}$$

where IV_c is a vector of our three instrumental variables, ODA'_c represents first stage predicted values, and the X_{ic} vector of individual controls now includes income, education and Perceived Aid.

Table 4.13: Private Aid and Crowding Out: Instrumental Variable 2SLS Estimation

	(1)	(2)	(3)	(4)	(5)	(6)
	Private Aid					
Actual Aid	-0.845**	-0.845***	-1.220**	-1.220***	-1.146**	-1.146***
	(0.340)	(0.221)	(0.503)	(0.260)	(0.552)	(0.295)
Perceived Aid	0.091***	0.091***	0.091***	0.091***	0.091***	0.091***
	(0.016)	(0.013)	(0.016)	(0.013)	(0.016)	(0.013)
Income: Medium / Medium High	0.042	0.042	0.044	0.044	0.043	0.043
	(0.045)	(0.064)	(0.045)	(0.064)	(0.045)	(0.064)
Income: High	0.032	0.032	0.026	0.026	0.029	0.029
	(0.070)	(0.081)	(0.071)	(0.081)	(0.068)	(0.080)
Education: Secondary	0.130	0.130	0.124	0.124	0.127	0.127
	(0.082)	(0.082)	(0.082)	(0.082)	(0.082)	(0.082)
Education: Higher Level	0.420***	0.420***	0.409***	0.409***	0.413***	0.413***
	(0.133)	(0.093)	(0.132)	(0.093)	(0.136)	(0.094)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes
Governance Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,492	8,492	8,492	8,492	8,492	8,492
R-squared	0.065	0.065	0.065	0.065	0.066	0.066
First stage coefficients and statistics						
Lobby PC	-0.197***	-0.197***	-0.197***	-0.197***	-0.197***	-0.197***
	(0.036)	(0.002)	(0.036)	(0.002)	(0.036)	(0.002)
Log Population	0.065	0.065***	0.065	0.065***		
	(0.071)	(0.004)	(0.072)	(0.004)		
Trade	0.004***	0.004***				
	(0.000)	(0.000)				
F-test of instruments	38.69	7183.93	15.35	5463.19	29.39	9240.29
	(0)	(0)	(0)	(0)	(0)	(0)
Overidentification test for instruments						
Hansen J statistic		6.740		0.259		
(p-value)		(0.034)		(0.611)		
Clustered s.e.	No	Yes	No	Yes	No	Yes

Robust standard errors clustered at country level in parentheses, unless stated otherwise. ***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

We present the results in Table 4.13. In columns (1) and (2), we see the IV results using all three instruments. The IV estimate of -0.845 is a modest increase (in absolute terms) on the OLS estimate. An advantage of having more than one instrument is the ability to conduct tests for overidentification, which can give us an indication as to whether our instruments are valid.[26] Here, the rejection of the overidentification test casts doubt on the joint suitability of the three instruments. When we drop the trade instrument in columns (3) and (4) we see a clearer picture: the coefficient is significantly larger at -1.220, while the lobbying and population variables are strong first stage predictors and do not appear to violate the exclusion restriction. The final two columns underline the consistency of our preferred instrument across all six specifications.[27] Our results on income, education and perceived aid are also notably consistent.

Table 4.14: Indirect Crowding Out

Estimator	Coefficient	Adjust: measurement	Adjust: measurement and fundraising
	ψ	$\psi * 0.599$	$(\psi * 0.599)(1 - 0.175)$
OLS	-0.84	-0.503	-0.301
IV $2SLS_1$	-1.146	-0.686	-0.566
IV $2SLS_2$	-1.22	-0.731	-0.603

As predicted, we are left with a larger crowding out effect once we use an IV approach. We present in Table 4.14 a summary of our nine estimates of indirect crowding out. We begin with our three basic estimates: OLS, IV1 using the lobbying variable, and IV2 using both log population and lobbying. Each coefficient is presented unadjusted, adjusted for scale, and adjusted for scale and displaced fundraising costs. Our preferred result of -0.566, where we use the fully adjusted IV1 estimate, suggests that crowding out is unlikely to be total.

[26] However, we are unable to run this with clustered standard errors, as our number of clusters (21 countries) is insufficient to calculate the robust covariance matrix. We thus present two columns for each specification: one with clustered standard errors, and another with robust standard errors and overidentification test statistics.

[27] Our lobby variable is our clearly preferred instrument, as we have no theoretical explanation for the first stage relationships exhibited by our other two instrumental variables—as mentioned above, we include them only as robustness checks.

Aid preferences in 'high lobby' countries

Despite showing that government aid is highly unlikely to completely displace private aid, we continue nonetheless to the second part of the robustness exercise to check whether or not the public appetite for private aid is particularly high in countries where the monetary influence on policy makers is high. Recall that, if crowding out is total, then it may be optimal for governments to withdraw ODA in countries with a high observable demand for private aid. If these are the same countries that score highly on our lobby scale, then it may be the case that this channel explains our main lobby result in Section 4.4.

In Figure 4.9, we show graphical evidence that countries with higher lobby scores are no more likely to exhibit a high demand for either government or private aid donations. We include both measures in case citizens mistakenly express their appetite for private aid while answering the survey question on desired ODA. Citizens in countries to the right spend higher shares of their household incomes on private aid contributions, while citizens in countries higher up the y-axis demand more government ODA.

Each observation is attributed a shade of grey to indicate the corresponding decile in the distribution of mean lobby scores. The darker the shade, the higher is the mean lobby score. We can see clearly that high lobby countries are found in 'low aid' areas at least as much as in 'high aid' areas. The corresponding correlation scores (Corr(private aid, lobbying) = -0.068; Corr(desired aid, lobbying) = -0.057) confirm that governments in fact observe a slightly lower demand for ODA and lower expenditure on private donations in high lobby countries.

Taken together, the results in this subsection give us confidence in our interpretation of the effects of lobby influence on ODA. We have shown that, where policy makers are influenced by money, they choose a level of foreign aid significantly lower than the preference of the median voter. Further, we can rule out an explanation wherein governments of high lobby countries lower ODA in reaction to high demand for private aid and the existence complete crowding out.

4.4 Conclusion

Using data from Gallup International, we observe that support for aid amongst citizens of OECD countries is, on average, higher than actual levels of ODA, notwithstanding a considerable 'illusion' effect. We show that, within countries, those who pay higher levels of tax are less likely to support government aid donations than those who pay lower levels. At the macro level, support for aid is higher in countries that have higher inequality and higher GDP per capita. We show that the democratic aid

deficit—the difference between citizens' desired levels of aid transfers and the level chosen by their government—is extended by private lobbying activities. Finally, we show that the 'crowding out' of individual voluntary contributions by government aid does not explain this result. Crowding out is incomplete, and the effect operates largely through reduced fundraising activities by charitable organisations—the direct effect is in fact positive.

As far as policy implications are concerned, the paper shows that those interested in increasing government-funded foreign aid donations should be concerned with publicising the current share of GNI that is spent on aid (in light of widespread 'illusion') and addressing the influence of private donations on policy-makers. Moreover, our final analysis suggests that a policy prescribed by Andreoni and Payne (2011a), whereby governments provide matching grants to charities in proportion to their private revenue, should be extended to charities operating abroad. This could assuage the effects of crowding out through the fundraising channel and increase the overall amount of aid sent to developing countries.

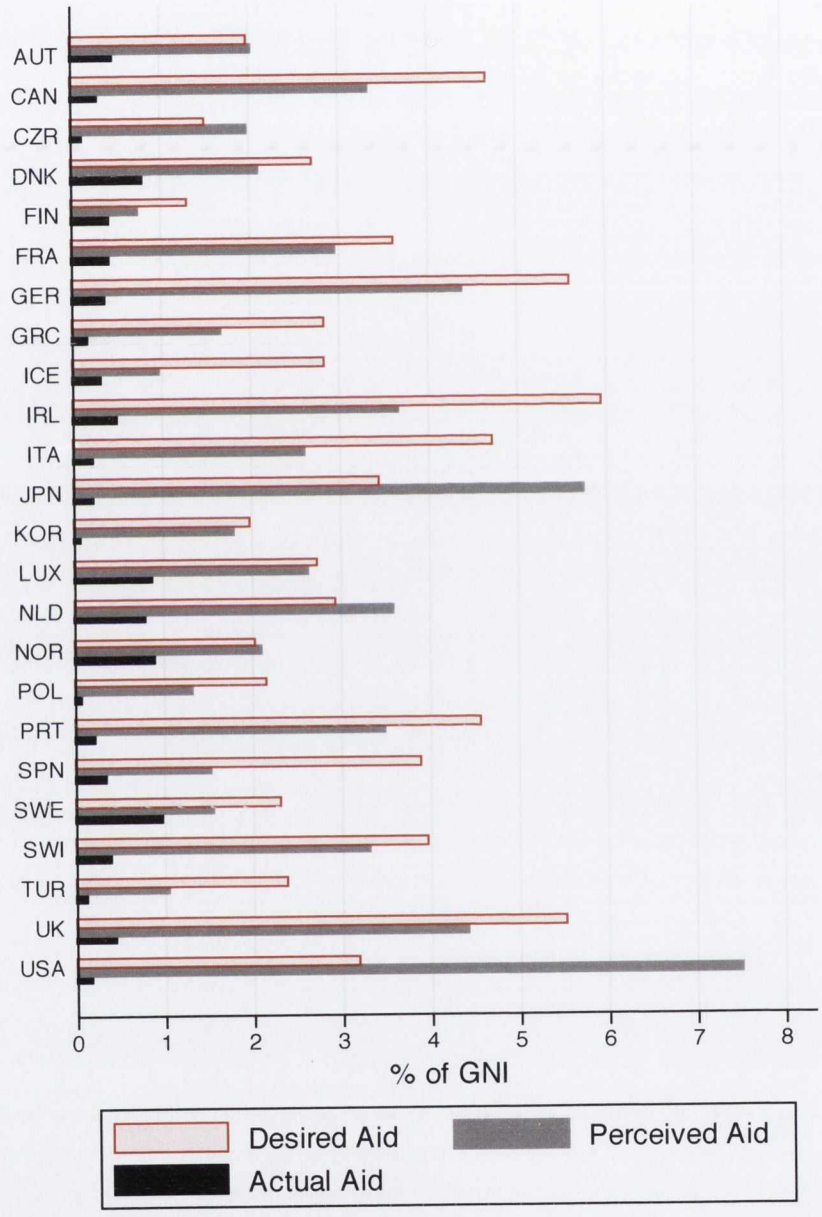


Figure 4.1: Sample Mean of Aid Variables by Country

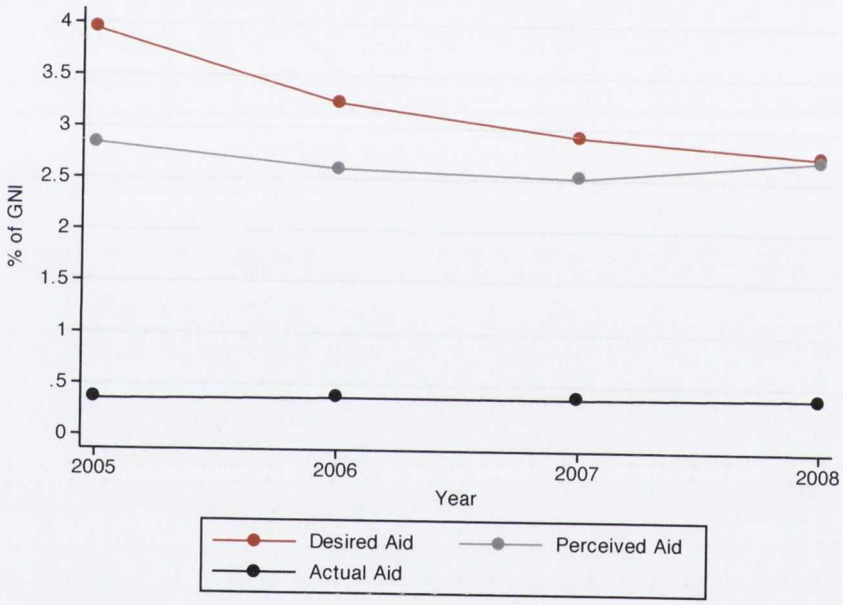


Figure 4.2: Sample Mean of Aid Variables by Year

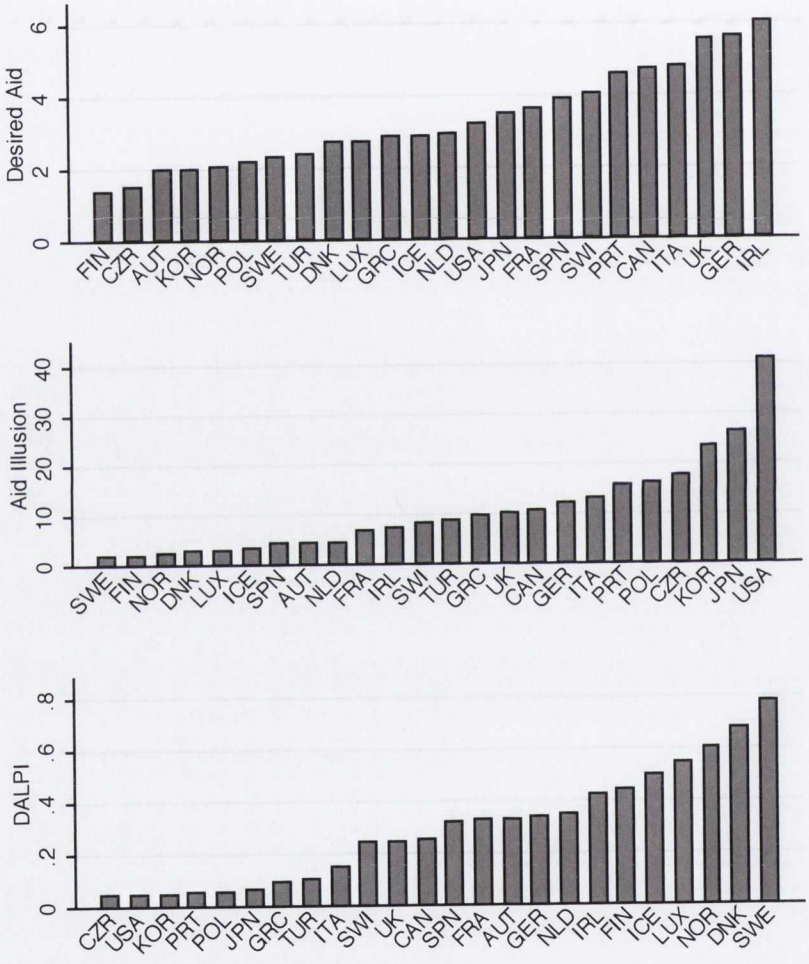


Figure 4.3: Desired Aid, Aid Illusion and DALPI by Country

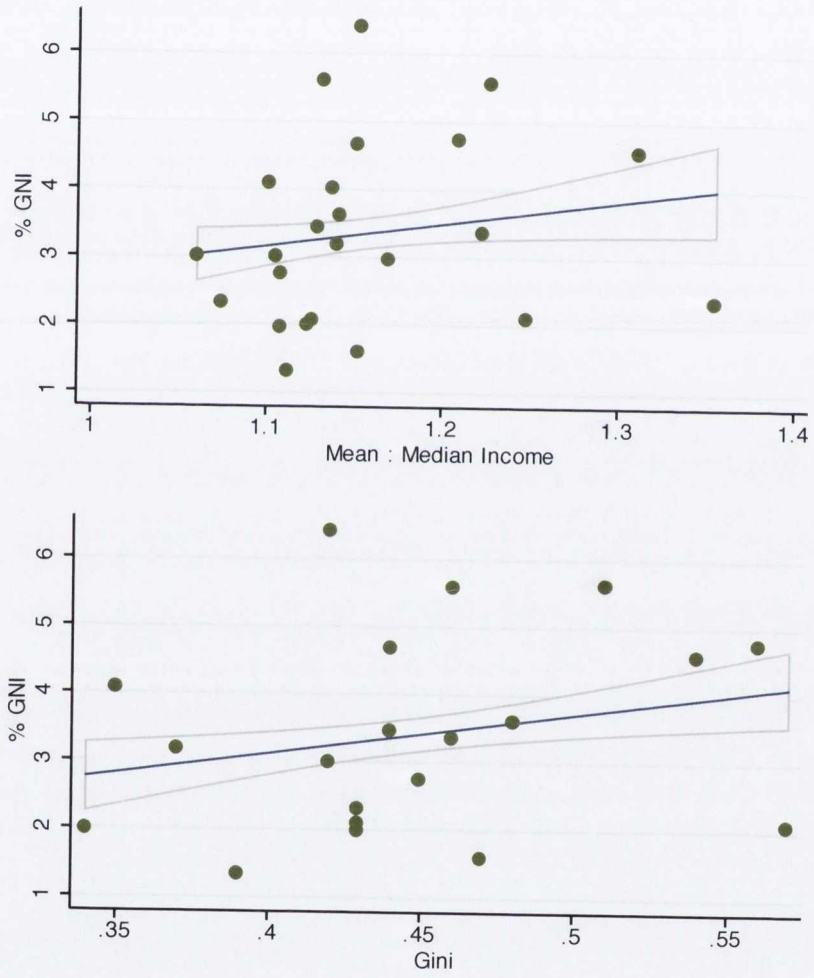


Figure 4.4: Inequality and Desired Aid (Country Means), Linear Fit and 90% Confidence Intervals

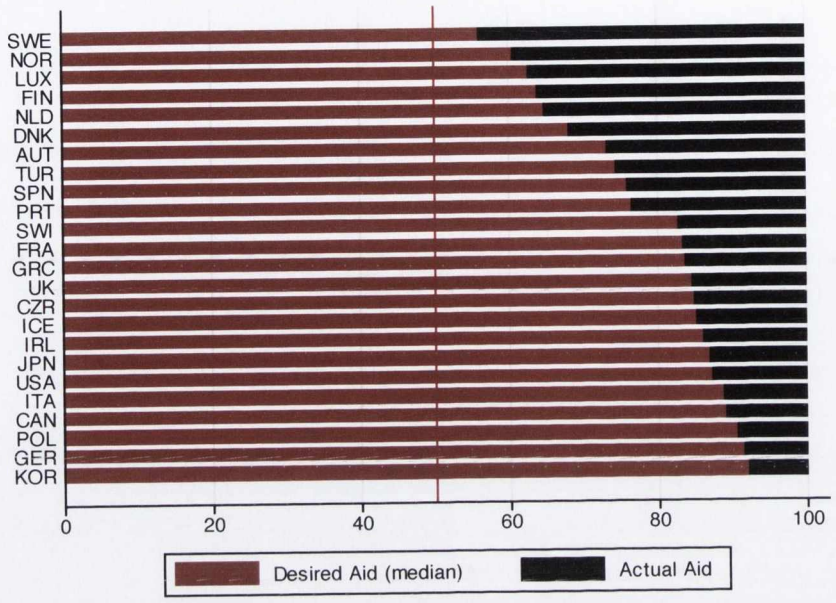


Figure 4.5: Democratic Aid Deficit

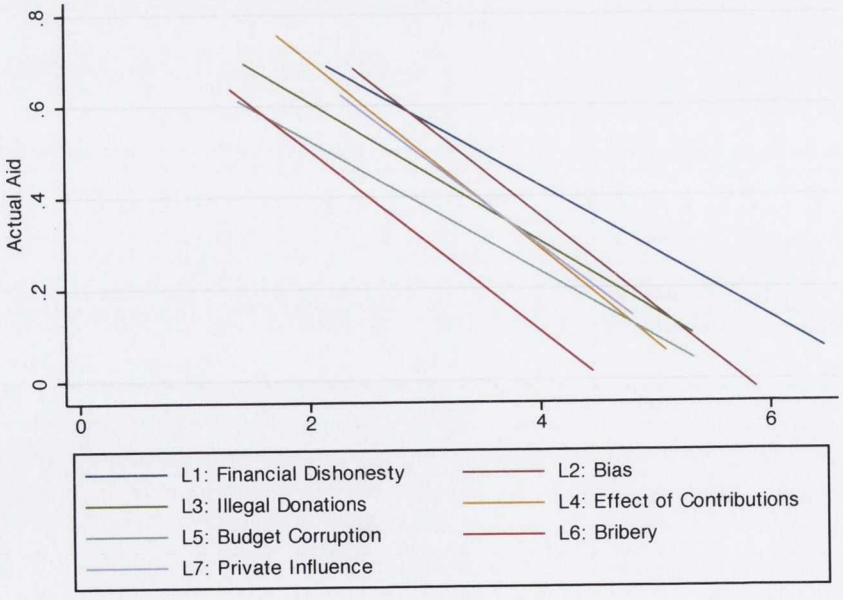


Figure 4.6: Lobbying and Actual Aid

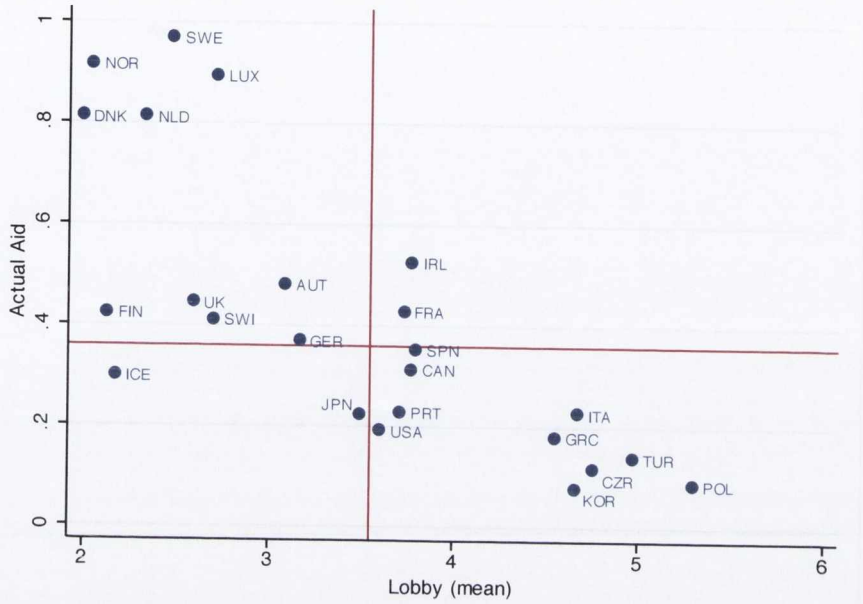


Figure 4.7: Actual Aid and Lobby Score by Country with Median Split

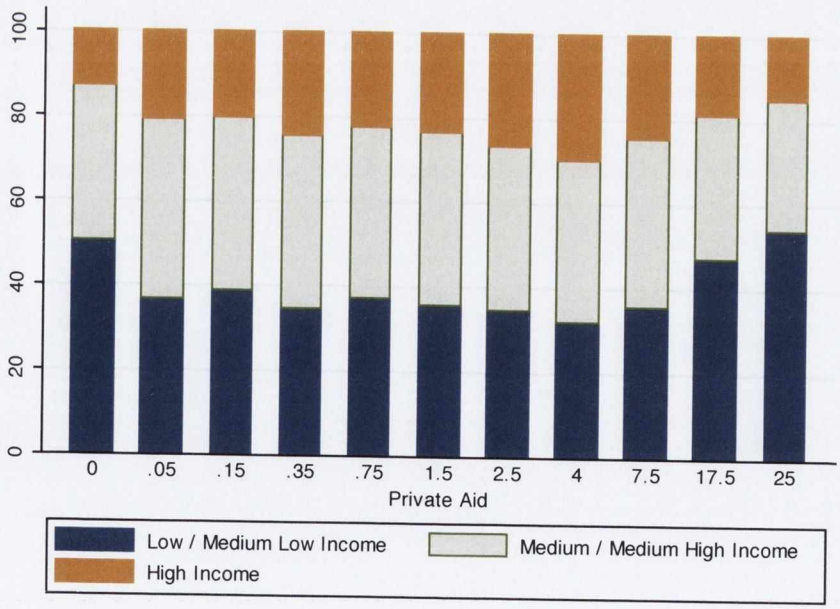


Figure 4.8: Share of Income Groups by Private Aid

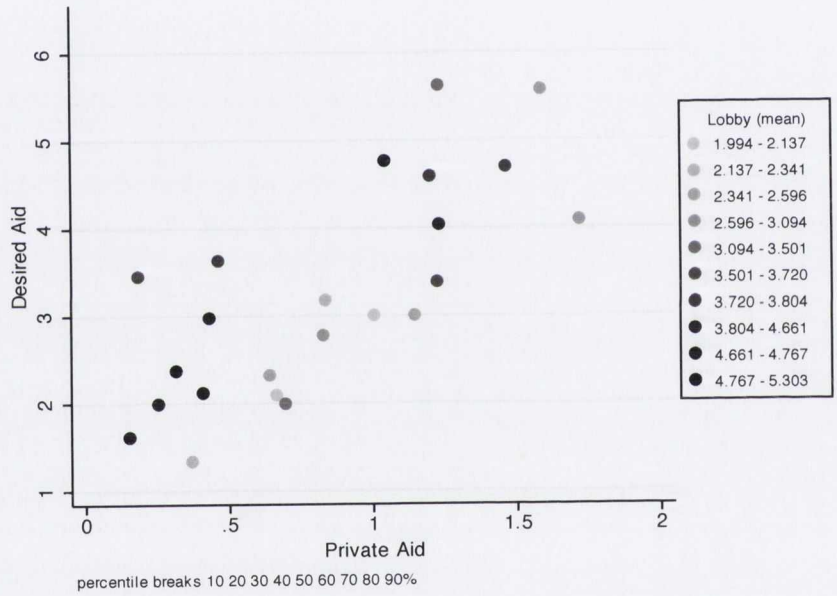


Figure 4.9: Private Aid and Desired Aid by Lobby Scores

Chapter 5

Conclusions

Many of the world's poorest countries lack well-functioning public institutions. As a consequence, the provision of public goods that are necessary for economic development is often susceptible to variation in a range of factors. In this thesis, I present cases where primary education, democratic accountability, and foreign aid are undermined by ethnic divisions, natural resource rents, and special interest politics respectively.

In the first substantive chapter, I show that salient ethnic diversity significantly increases teacher absenteeism in African districts. The analysis has several implications for the literature, for policy, and for future research challenges. First. It represents a significant contribution to our understanding of teacher absenteeism in poor, ethnically diverse countries, which have been shown by Chaudhury et al. (2006) to exhibit rates of over 25%. Second, I present a new measure of ethnic divisions, which expressly incorporates the empirical support for 'constructivist' explanations of ethnic identity by interacting district-level ethnic diversity with a survey-based measure of ethnic salience. Third, I provide evidence that neither community sanctioning institutions nor the ethnic status of teachers explain the relationship; rather, it is the strength of social networks amongst teachers that appears to be the relevant channel.

From a policy perspective, the results hold two main lessons. First, I show that a malleable aspect of ethnic divisions—the salience of ethnic identification—can adversely affect the provision of an important public good. As a result, policies that dampen the strength of sub-national identification may lead to higher primary school educational attainment. Second, the apparent channel of causation presents a significant opportunity for policy-makers to harness the role of social networks in their attempts to provide effective primary teaching, and perhaps other public goods too. Before we arrive at that point, however, future research must clarify exactly why teachers socialise less in divided areas, and why indeed this leads to lower absenteeism. This may be undertaken best in the field. Another area for future research lies in the determination of ethnic salience. Precisely why it varies, and, in particular, how that affects the provision of other public goods, the incidence of conflict, 'frozen' ethnic voting or a range of other political outcomes is an area that appears fruitful for research.

The second analysis demonstrates how the political elite in resource-rich African countries lowers taxation in order to quell the demand for democratic accountability. It has been cited in a World Bank policy proposal to directly allocate the benefits of natural resource extraction to citizens. My results suggest that the subsequent taxation of this income should increase public scrutiny of government behaviour. As with many studies of this nature, rich administrative data could improve significantly the identification of the relationship and of the precise channels through which it operates. Unfortunately for development economists, this is not often available in our countries of interest. Nonetheless, the study is, to the best of my knowledge, the first serious treatment of 'rentier' state behaviour that uses micro-level data.

The final chapter uses new data to shed light on how preferences for and allocations of official foreign aid to poor countries are generated in rich countries. While the empirical analysis would benefit from a longitudinal dimension to the data, the chapter still offers contributions to the literature and to policy. We find that richer citizens have preferences for lower levels of government-funded foreign aid. This can result ultimately in reduced allocations of aid in countries where rich agents can effectively 'buy' their preferred policy from a politician. Moreover, we provide novel support for recent advances in the literature which find no empirical basis to the claim that government aid crowds out private aid through the 'classic' channel. Instead, government aid appears to cause lower fundraising by charitable organisations. As a consequence, tying government grants to fundraising targets may result in higher aid flows to poor countries.

Understanding the causes and consequences of institutional dysfunction, from local collective action failures to the erosion of democratic accountability, is a challenge of first order importance to development economists. This thesis contributes to this pursuit by presenting three specific examples of how public institutions and the provision of public goods in some of the world's least developed countries can be so easily undermined.

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