

The illusory leader: natural resources, taxation and accountability

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Abstract This paper proposes and tests a mechanism through which natural resources can affect democracy. 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 micro-level data from public opinion surveys across 15 sub-Saharan countries, in addition to country-level data on natural resource rents. Results are robust to a range of alternative specifications. A supplementary analysis reveals that, consistent with the two-period model proposed, the effects are more acute closer to national elections.

Keywords Democracy · Political economy · Natural resources · Curses · Africa

JEL Classification D73 · O13 · O55

1 Introduction

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

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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 behavior 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. Brunnschweiler 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 (forthcoming) 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 (forthcoming), Cabrales and Hauk (2011) and Aslaksen (2010). There is also substantial evidence linking resource

wealth to increases in corruption. Arezki and Bruckner ([forthcoming](#)), 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 partly 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 behavior 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 hypothesize that resource rents lessen perceptions of 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 dependant 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-economy variants 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 organized as follows. First I contextualize the proposed relationships and present a simple model of leader behavior. I then discuss the estimation strategy and the data used for the analysis, before examining the results and finally offering some concluding remarks.

2 The political economy of the natural resource curse

The idea that natural resources affect leader behavior 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 realized.

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.

2.1 The rentier effect

I propose that dysfunctional leader behavior 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 1990). While there are many channels along

which this can occur (which I discuss in detail in Sect. 4.2), 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.

Ross draws upon two prior case studies to substantiate his characterization 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 formalize a very simple theory before offering an empirical test of its implications.

2.2 Theory

Although the contribution of this paper is primarily in its empirical findings, in this section I develop a simple model of rentier behavior that is a variation on Caselli and Cunningham (2009), in which the leader's utility is partially determined by a standard survival

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

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

³See IMF (2009: Table SA11) for an up-to-date version of this table.

function (i.e., the reduced-form probability of retaining power) that predicts how natural resource rents will affect behavior.⁴ In that paper, increased returns to power-preserving activities caused by high resource rents alter the behavior of leaders in accordance with the incentives produced by the survival function. The authors therefore explore the effects of resource rents on leader behavior 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 demobilized—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 τv , subject to survival of probability $\pi(\tau)$. The objective function is thus:

$$u = \alpha + \pi(\tau)[\alpha + \tau v], \quad (1)$$

with first-order condition,

$$\frac{du}{d\tau} = \pi'(\tau)[\alpha + \tau v] + \pi(\tau)v, \quad (2)$$

total differential,

$$d\alpha[\pi'(\tau)] + d\tau[\pi''(\tau)(\alpha + \tau v) + 2\pi'(\tau)v] = 0, \quad (3)$$

and comparative statics:

$$\frac{d\tau}{d\alpha} = -\frac{\pi'(\tau)}{\pi''(\tau)(\alpha + \tau v) + 2\pi'(\tau)v}. \quad (4)$$

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.

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

⁵Although I leave a detailed theory of this mechanism for future research, the channels through which 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 strongly 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.

⁶I thank an anonymous referee for clarifying this.

3 Estimation

I test the theory by analyzing 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 $\pi'(\tau) < 0$).

3.1 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 standardized, 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 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 minimized 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:

⁷Samples 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. (2005).

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

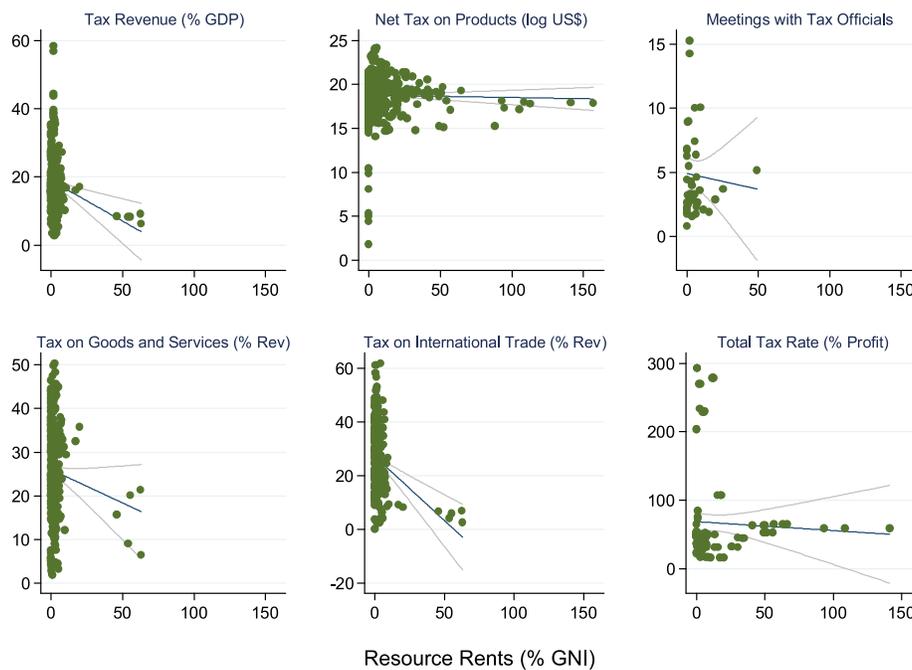


Fig. 1 Taxes and Natural Resources in sub-Saharan Africa, 1960–2009

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 labeled 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 directly. Alternative methods of capturing the tax burden are limited: 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 Fig. 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 *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-invariant 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 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 in Sect. 4.

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

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

¹⁰Wood 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

is particularly accurate as, although commodity 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:

$$T_{icst} = a_T + \beta R_{ct} + X'_{icst} \gamma_T + C'_{ct} \lambda_T + S'_{st} \psi_T + u_{icst} \quad (5)$$

and

$$D_{icst} = a_D + \varphi T_{icst} + X'_{icst} \gamma_D + C'_{ct} \lambda_D + S'_{st} \psi_D + e_{icst} \quad (6)$$

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.

3.1.1 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 behavior 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 maximize 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 centralized 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:

$$T_{icst} = \alpha_T + \beta R_{ct} + \theta E_{ct} + \zeta (R_{ct} * E_{ct}) + \lambda_T C_c + X'_{icst} \gamma_T + S'_{st} \psi_T + v_{icst} \quad (7)$$

access problem, one could argue that poor property rights and bad management could lead to overharvesting, which would decrease unit rents. To assuage 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).

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 Data

Table 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 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.

4 Results

4.1 External corroboration

Before turning to more formal analysis, I begin in Fig. 1 by supplementing the findings of Bornhorst et al. (2009) with external corroboration of the hypothesized 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 taxation¹²: 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 Fig. 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.

¹¹The Nigerian mean for 2001, 2003 and 2005 is 26.9 ($\sigma = 3.51$); the sample mean is 4.58 ($\sigma = 4.45$).

¹²These are defined in [Appendix](#).

¹³It 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.

Table 1 Demand for democratic accountability (Likert scale)

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	0.13	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
<i>Mean</i>		<i>0.09</i>	<i>0.09</i>	<i>0.28</i>	<i>0.55</i>	<i>3.29</i>
<i>Standard Deviation</i>		<i>0.04</i>	<i>0.03</i>	<i>0.06</i>	<i>0.07</i>	<i>0.12</i>

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 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	Toure*	-6.5
Mali	2005	284	3.34	0	0	Toure*	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
<i>Mean</i>		<i>1078</i>	<i>3.25</i>	<i>125.94</i>	<i>4.58</i>		<i>15.24</i>
<i>Standard Deviation</i>		<i>1031.91</i>	<i>0.19</i>	<i>149.55</i>	<i>4.45</i>		<i>7.24</i>

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 Sect. 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)

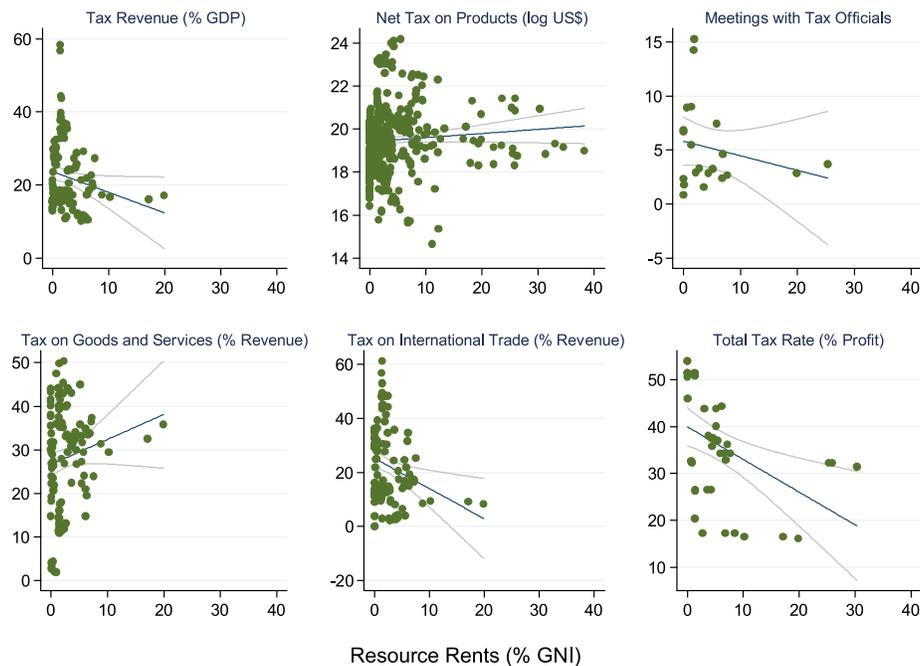


Fig. 2 Taxes and Natural Resources in Sample Countries, 1960–2009

Regarding the second case, the glossary of tax variable definitions in [Appendix](#) 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.

4.2 Instrumental variable estimates: rents, taxation and accountability

Table 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 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 causal; 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 ($F = 95.02; 83.79$).

¹⁴That 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-

Table 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*** (0.006)	0.0647*** (0.0061)			0.1274* (0.0717)	0.3731*** (0.0910)
Rents			-0.0363*** (0.0031)	-0.0308*** (0.0032)		
<i>Controls</i>						
Individual	No	Yes	No	Yes	No	Yes
Interview	No	Yes	No	Yes	No	Yes
Election Proximity	No	Yes	No	Yes	No	Yes
Observations	46832	45270	47957	46276	46832	45270
R^2	0.023	0.033	0.061	0.067		
1st Stage F-stat.			95.02	83.79		

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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

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.

4.2.1 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 modernization effect. Although these

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.

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 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 behavior, 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 organization 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.

(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 nonmembers 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)

¹⁵See Messiant (2001) for a detailed analysis of the organization.

Table 4 Alternative mechanisms (Ross 2001, and Egorov et al. 2009)

	2SLS			
	(1)	(2)	(3)	(4)
Dependent Variable:	Dem a/c			
Instrument:	Rents			
Tax	0.3462*** (0.0871)	0.3484*** (0.0925)	0.4286*** (0.0901)	0.3853*** (0.0906)
In 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
<i>First Stage</i>				
Rents	-0.0234*** (0.0042)	-0.0309*** (0.0032)	-0.031*** (0.0033)	-0.0289*** (0.0032)
F-stat.	31.13	90.09	90.41	79.44

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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

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 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 organized or supported by the state.

Auxiliary regressions of each variable on resource rents show that none of them are likely to constitute channels through 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—findings that 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 behavior 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 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 hypothesized 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—

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

¹⁷The measure is increasing in physical integrity rights from 0–7.

¹⁸Durability 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).

current physical integrity decreases the demand for accountability regardless of the age of a regime.

(iv) *The modernization effect* The final major channel explored by Ross (2001) is the modernization 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 modernization, 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 modernization 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 4 shows that, curiously, press freedom (measured by the same [100—Freedom House score] variable used by Egorov et al. 2009) is associated with *lesser* demand for democratic accountability. These results are robust to the inclusion of democracy and other potentially correlated variables in Columns (3), (4) and (5) in Table 5. This may suggest that citizens are well aware that their media are biased and that, moreover, 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 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

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 behavior 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.

Table 5 Additional controls

	2SLS				
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Dem a/c				
Instrument:	Rents				
Tax	0.4247*** (0.0897)	0.5546*** (0.1064)	0.5825*** (0.2227)	0.4149*** (0.0579)	0.3826*** (0.0881)
Polity	-0.0627*** (0.0109)	-0.2009*** (0.0237)	-0.2274*** (0.0350)	0.0071** (0.0035)	-0.0617*** (0.0106)
Durable		-0.0425*** (0.0054)	-0.0338*** (0.0107)	-0.0005 0.0024	
Polity*Durable		0.0037** (0.0016)	0.0111* (0.006)	0.00004 (0.0025)	
In Gov			0.0544 (0.1014)	0.0426*** (0.0078)	0.0168 (0.0417)
Group			0.0413** (0.017)	-0.0522*** (0.0151)	0.0509*** (0.0151)
Physical Integrity			-0.0623*** (0.0201)	-0.0484*** (0.0101)	-0.07*** (0.0157)
Press Freedom			-0.0153** (0.0060)	0.0053*** (0.0052)	-0.0228*** (0.0039)
Country FE	Yes	Yes	Yes	No	Yes
Observations	43112	45270	35440	35440	36549
<i>First Stage</i>					
Rents	-0.0350*** (0.0035)	-0.044*** (0.0088)	-0.03*** (0.0053)	-0.009*** (0.0023)	-0.0350*** (0.0049)
F-stat.	99.42	25.40	32.07	15.37	51.28

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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

4.2.2 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 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 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.

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 6 Robustness checks

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

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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 5

It has been noted that the first stage may itself be endogenous. The suggested mechanism was 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 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 dichotomize the dependent variable by grouping each half of the four-point scale into a single point in order to facilitate the estimation of the relationship using IV Probit (Table 7, Column 1) and linear probability models (Column 2). The resultant marginal effects shown in Table 5 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:

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

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

Table 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*** (0.1662)	0.1487*** (0.0352)	0.5907*** (0.1088)	0.8440*** (0.1124)	0.3734* (0.2241)	0.3011*** (0.0402)
Democracy Controls	No	Yes	No	Yes	No	Yes
Observations	45270	36549	44490	38707	40746	33351

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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 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 Likert scale are bundled into single points (0/1) to create binary variables. Coefficients reported represented marginal effects (dy/dx)

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 range in results in Table 7 reinforce the original findings.

Finally, it could be reasonably claimed 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.

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

Table 8 Models using per capita rents measure

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

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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 5

4.2.3 Per capita rents

The measurement of natural resources is a point of considerable contention in the literature. Alexeev and Conrad (2009) and Brunnschweiler 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 shares of GDP, is endogenous to export policies and domestic consumption; and (ii) because they are generally used in GDP growth regressions, where they are 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 characterized by the relative importance of resource rents in the economy.

Both views have compelling elements, and, in Table 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 incidence is an intuitive one.²⁴ Finally, if the original measure is endogenous to political development operating through the denominator, its use

²⁴In that the 'substantial' (Mahdavy 1970: 428), or 'sufficient' (Ross 2001: 332) resource rents required to elicit rentier behavior 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).

Table 9 Rents, taxes and elections

	OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	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
Obs.	46968	39131	46968	39131	46968	39131
R ²	0.065	0.068	0.065	0.065	0.067	0.07

Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: 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 5, together with the Law control from Column (6), Table 6

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 (0.7018), perhaps due to the relative homogeneity of this sample compared to the global samples more commonly used in the literature.

4.3 Rents, taxes and survival

Table 9 reports the findings of (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 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.

5 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 behavior 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

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.

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.

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Appendix: Macro tax variable definitions (source: World Bank)

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 labor taxes) after accounting for deductions and exemptions as a percentage of profit.

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