Do Natural Resources Fuel Authoritarianism?

A Reappraisal of the Resource Curse

Stephen Haber and Victor Menaldo

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Abstract: Is there a relationship between economic dependence on oil or minerals and authoritarianism? In order to answer this question we develop unique historical datasets that allow us to focus on within-country variance in resource dependence and regime types, test for long-run relationships between resource reliance and regime type, and estimate dynamic panel models. Our results indicate that dependence on oil and minerals is not associated with the undermining of democracy or less complete transitions to democracy. Our results are at variance with a large body of scholarship that finds a negative relationship between oil or mineral dependence and democracy using pooled, time-series cross-sectional techniques centered on the variation between countries using data substantially truncated with respect to time. We surmise that the reason for this discrepancy is that countries' underlying institutions jointly determine their resource reliance and regime types.

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Introduction

Are countries with abundant oil or minerals cursed by their resource wealth? Parallel literatures in political science and economics suggest that they are: economic dependence on oil or minerals, according to these literatures, is positively correlated with the onset of civil war, slow economic growth, and authoritarian government. These ideas about a "resource curse" have had an impact well beyond the academy. They are taken almost as self-evident truths at multilateral aid organizations such as the United Nations and the World Bank (e.g., Harford and Klein 2005), are presented as robust facts in popular books on the alleviation of world poverty and inequality (e.g., Collier 2007), and are disseminated in the news media as a consensus finding of social science research (e.g., Rosenberg 2007). Indeed, New York Times columnist Thomas Friedman (2006) has gone so far as to decree a "first law of petro-politics," in which the price of oil and the spread of political freedom are said to be inversely correlated. The normative implications are obvious and non-trivial. Some researchers have even suggested that developing countries might consider leaving their resources in the ground, in order to avoid their pernicious effects (e.g. Ross 2001b).

In this paper we question the evidence and methods supporting the idea that economic dependence on oil or minerals is causally related to authoritarianism.[1] Our basic point is that the resource curse is about a time series process—the discovery of oil or minerals is hypothesized to send a country down a different path of institutional development than it would have followed otherwise—and thus the theory should be

[1] We note that there is a parallel literature in economics that questions the evidence and methods supporting the link between oil or minerals and slow growth (Wright and Czelusta 2004; Lederman and Maloney 2007, 2008; Manzano and Rigobon 2007). That literature proceeds along somewhat different empirical lines than our time-series approach.

evaluated using time series evidence and methods. The extant literature, however, relies on pooled time series cross-sectional regressions without country fixed effects estimated on datasets that are considerably truncated with respect to time. The implicit theory underlying these regressions—even those that control for possible endogeneities with instrumental variables— is that, after holding the covariates associated with regime type constant, authoritarian, resource dependent countries were on a path of political development that would have led to the same outcome as that obtained in democratic, non-resource dependent countries: Venezuela could have followed the same institutional trajectory as Denmark – if it had not discovered, extracted and exported oil.[2] Researchers are, in short, drawing causal inferences about processes that are purported to happen within countries over time from a technique that is primarily driven by the variation between countries.

We therefore build upon and go beyond the extant literature in four ways. First, we build long-run historical datasets that allow us observe countries both *before* and *after* they became major oil or mineral producers. Second, we employ econometric techniques that are centered on the dynamic modeling of time-series processes. Third, in order to mitigate possible measurement error in the dependent and independent variables, a salient concern when relegating attention to within country variation, we estimate our regressions using three different measures of oil and mineral dependence and three different measures of regime type. Fourth, we draw explicit counterfactual comparisons.

[2] Researchers realize, of course, that the regressions have this quality, and they therefore add control variables, such as regional dummies or the initial level of per capita GDP, to mitigate the problem of unobserved heterogeneity. But, this step does not really solve the problem: the implicit counterfactual to Venezuela is now a fictional Denmark with a much lower per capita income located in Latin America.

The body of evidence that we develop and the set of methods that we employ allow us to control for factors, and model relationships between variables, in ways that have not been accomplished in the literature to date. First, our data and methods allow us to determine if there are persistent effects of resource shocks on regime types. Second, they allow us to determine if resource shocks influence regime types with a time lag. Third, they allow us to control for long-run differences across countries in terms of their fundamental institutions. The ability to control for time-invariant heterogeneity is particularly critical, given the fact that historically-persistent institutions may jointly determine both countries' regime types and the numerators and denominators typically used to compute measures of resource reliance.[3]

Regardless of how we measure the dependent and independent variables, specify the regressions, or frame the counterfactual comparisons, we cannot detect the relationship between oil or mineral dependence and authoritarianism posited by the resource curse. In fact, in some specifications we detect short-run benefits from increases in resource reliance on democratization. These results do not suggest that there may not be specific cases in which a dictator successfully used oil or mineral revenues to

[3] The numerators are either measures of resource production, resource exports, or the rents extracted from resources. The levels of all three of these metrics are often driven by *political* decisions made by governments that have inherited inveterately weak institutions: these governments may have short time horizons and pressing fiscal needs, causing them to exploit resources today rather than save them for tomorrow. This is even the case when the numerator is proven oil and mineral reserves: governments that inherit weak fiscal and political institutions may have strong incentives to frantically search for new resources. Recent examples include Sudan's and many sub-Saharan African countries' willingness to allow China to leave no rock unturned within their borders in the search for untapped oil. A similar logic affects the denominators used to compute resource reliance. One recent improvement made in the literature is to normalize resource wealth by population rather than GDP, in order to avoid introducing simultaneous causation running from regime types to income levels (see Ross 2006, 2008; Dunning 2008). Even this innovation, however, does not resolve the problem of unobserved heterogeneity: a country's persistent institutions may jointly determine its regime type and the size and rate of growth of its population –even after controlling for its level of income (see Culter et al. 2006, Soares 2007).

consolidate his hold on power. It is to say, however, that the evidence does not support the notion of generalizable laws—even conditional ones.

The remainder of this paper proceeds as follows. Section Two reviews the literature on the political resource curse, discusses the logical flaws in its underlying theory, and provides a critical discussion of the empirical strategies employed in the literature to date. Section Three presents a discussion of the historical datasets we develop and the methods we employ to analyze them. Section Four presents our results, providing both graphical analysis of individual cases, as well as panel data analysis. Section Five concludes and discusses the implications of the kind of time series approach to evidence that we employ here for a wide variety of questions in comparative politics.

II. Literature Review

The idea that there is a causal connection between economic dependence on oil or minerals and authoritarian government goes back to Mahdavy (1970: 466-67), who noted that petroleum revenues in Middle Eastern countries constituted an external source of rent that was directly captured by governments. He hypothesized that: "A government that can expand its services without resorting to heavy taxation acquires an independence from people seldom found in other countries… In political terms, the power of the government to bribe pressure groups or to coerce dissidents may be greater than otherwise." What Mahdavy advanced as a hypothesis to be tested was soon recast by other researchers as a robust fact. Consider, for example, the shift in tone in Luciani (1987): "Democracy is not a problem for allocation states…The fact is that there is 'no representation without taxation' and there are no exceptions to this version of the rule."

The idea that oil begets rentier states which are inconsistent with democracy soon became one of the major organizing themes of the literature about authoritarianism in the Middle East and Africa (Skocpol 1982; Anderson 1987; Beblawi 1987; Luciani 1987; Davis 1987; Crystal 1989, 1990; Chaudry 1994, 1997; Van de Walle 1994; Khan 1994; Yates 1996; Clark 1997, 1998; Gardinier 2000; Dillman 2000; Hodges 2001; Bellin 2004; Vandewalle 1998, 2006). Huntington (1991: 65) went so far as to generalize the lessons from this case study literature to the entire developing world.

One of the obvious weaknesses of this literature was that its case study approach limited its ability to draw clear causal inferences. As Herb (1999: 256) put it: "…the lack of democracy is noted, oil is proffered as the culprit, 'no representation without taxation' cited as the mechanism, and the matter is closed."Another obvious weakness was that any argument made about a causal relationship between oil abundance and authoritarianism also had to be true for hard rock minerals: both are extracted using capital and technology intensive methods, are sold on an international market, and can be directly taxed by governments as they come out of the ground or are exported.

Researchers therefore began to subject the hypothesis that oil and mineral abundance are associated with authoritarianism to tests against large-N datasets (Barro 1999; Ross 2001a; Wantchekon 2002; Jenson and Wantchekon 2004). Over time, this large-N literature has grown increasingly sophisticated: researchers have developed better proxies for oil and mineral reliance (Ross 2008); employed techniques to control for possible reverse causality (Ramsey 2008); exploited variance at the sub-national level (Goldberg, Wibbels, and Myukiyehe 2008); and explored the effects of oil wealth on the durability of authoritarian regimes using survival analysis or dynamic probit regressions

(Smith 2004, 2007; Ulfelder 2007; Ross 2008; Papaioannou and Siourounis 2008). While the specific findings vary from study to study, the literature finds an association between oil or mineral reliance and authoritarianism.

The adoption of large-N frameworks did not, however, resolve an underlying weakness of the theory, which is that the mechanism that putatively links oil or mineral dependence to authoritarianism—the development of rentier states—was rooted in a logical fallacy: The idea that there is no taxation without representation *does not* logically imply that there is no representation without taxation. [4] In point of fact, as both research in public choice economics and in the history of democratization show, a ruler trading taxation for representation is only one of *several* paths by which representative governments arise—and those specific examples of those alternative pathways include countries that are highly dependent on oil or minerals. One alternative path to democratization is a rivalry between enfranchised and disenfranchised groups that induces democratization from below (Conley and Temimi 2001), as occurred in oil-rich Mexico, for example. A second path is when economic elites are split, and the ruling elite extends suffrage strategically to advance its interest against rival elites (Llavador and Oxoby 2005), as occurred in nitrate-rich Chile in the nineteenth century. A third path is when political elites split, and agree to democratize in order to avoid violence (Bardhan 1993), such as occurred in Colombia. A final path, is that democratic institutions develop

[4] Researchers have also suggested two other transmission channels, but tend to afford them less importance. In some early versions of the theory (e.g., Ross 2001a) oil and minerals were claimed to generate autocracy by allowing dictators to finance a repressive state apparatus. Later work by Ross (2008), suggests little empirical support for the existence of this mechanism. In other versions of the theory, (e.g., Ross 2001a) reliance on natural resources is said to have an indirect effect on regime types by retarding modernization and, hence, preventing Lipset's (1959) prerequisites of democracy from emerging – a large middle class, an educated population and higher living standards.

and thrive when public goods become more highly valued than pork (Lizzeri and Persico 2004); in the case of Trinidad and Tobago, public goods financed by oil rents achieved this status and helped sustain democracy. Indeed, as Herbst (2000) has pointed out, the idea that democracy develops because rulers trade representation for taxation appears to be completely inapplicable to the developing world. The historical record indicates that it may not even be applicable to most Western European democracies.[5]

The adoption of large-N frameworks—specifically statistical methods that primarily exploit variance across countries, rather than within countries over time—introduced an additional weakness into the literature: a mismatch between the theory and the empirical strategies used to test it. The hypothesis that oil or mineral wealth fuels authoritarianism implies that the discovery of oil or minerals sent countries down different paths of institutional development than they would have followed otherwise, in the absence of resource discoveries. Specifically, the resource curse implies three possible counterfactuals: 1). Authoritarian country X would have become democratic, had it not found oil or minerals; 2). Democratic country Y would have remained democratic, instead of lapsing into authoritarianism, had it not found oil or minerals. 3). Democratic country Z would have made the transition from authoritarianism even faster had it not found oil or minerals. The methods of analysis employed in the literature do not, however, allow for the direct observation of these inflection points in countries'

[5] The canonical case that resource curse theorists seem to have in mind about rulers conceding representation for taxation—England after the Glorious Revolution—is not, in fact, an example of the phenomenon. The British Crown, as North and Weingast (1989) make clear, did not trade anything in exchange for revenues—it demanded forced loans and confiscated property to fund foreign wars. It was only *after* a protracted and bloody civil war that a coalition of nobles and citizens forcibly imposed constraints on the executive in order to prevent the crown from plundering their wealth in the future. Higher tax revenues were only an incidental, unintended consequence of the strengthened legislature and judiciary – a byproduct of more secure property rights – that considerably lagged increased representation (Stasavage 2003).

paths of institutional development. Instead, the extant methods pool all countries together, implicitly treating them as identical units, and then look for variance across the countries. The problem with this approach is that country-years are not isomorphic: Denmark is not Venezuela without oil; endemic and time-invariant institutions differentiate these countries; and those institutions constrain the possible set of political institutions, and the possible set of economic sectors, that can emerge and be sustained.

Some researchers have begun to acknowledge these problems. Norman (forthcoming) makes the point that resources, whether they are measured as stocks or flows, may themselves be endogenous to a country's underlying legal, political and cultural institutions. McSherry (2006) and Smith (2007) suggest that the blame for bad policy choices and authoritarian political structures may lie with a country's pre-oil institutions, rather than with the oil discoveries themselves. Engelbert (2000) gets to the heart of the matter in his comparison of democratic and prosperous Botswana and authoritarian and impoverished Democratic Republic of the Congo: economic reliance on natural resources cannot explain the differences between the two, because both are mineral rich; the differences are a product of their colonial institutions.

The key to any meaningful analysis of the resource curse is to specify the right counterfactual: what would a resource reliant country have looked like had it not discovered oil or minerals? One important step in this regard was made by Herb (2005), who reasoned that resource dependent countries would have been substantially poorer had they not found oil or minerals, and would therefore have obtained lower levels of democracy—under the modernization theory assumption that the growth of income catalyzes social and political changes that result in democratization. He therefore

calculated the per capita GDP loss from eliminating a country's oil or mineral sector, and then estimated its level of democracy at the counterfactual level of GDP. His estimates indicate that the net, negative effect of resource dependence on democracy is negligible. One drawback to Herb's approach is that, following the traditions in the resource curse literature, it employs pooled time series cross sectional regressions without country fixed effects on a data set that is truncated with respect to time—in this case 1972 to 1999. That is, it does not compare countries to themselves, before and after they found resources, but rather infers what the time-series process would have looked like on the basis of cross-sectional evidence.

Dunning (2008) takes another important step in the specification of appropriate counterfactuals by introducing country fixed effects in his regressions of polity scores on per capita windfall profits from oil in 17 Latin American countries covering the period 1960-2001.[6] These regressions capture how increases in resource reliance affect polity scores within countries over time, and they indicate that reliance on oil has conditional effects on regime types: when a country has a high level of inequality, increased oil reliance exerts a positive effect on democratization; but when a country has a low level of inequality, increased reliance on oil exerts a negative effect.[7] There is a reasonable

[6] In a panel of the entire world, he continues the tradition in the literature of pooling all countries together. We also note that Ross (2001: 341,fn 58), who tests the resource curse hypothesis on a global panel of countries observed between 1972 and 1997, notes that the statistical significance of the negative impact of oil and mineral exports as a share of GDP on democracy disappears if country fixed effects are introduced.

[7] Jones Luong and Weinthal (2006), on the basis of case study evidence, also make a conditional argument about the resource curse: when petroleum industries are privately owned, oil exerts a positive impact on democracy; when publicly owned, a negative effect. Their case study approach does not, however, permit them to control for a possible endogeneity running from regime type to the ownership structure of the oil sector;: nationalizations usually occurs – and are sustained – in autocratic regimes. Similar methodological issues also mean that they cannot

likelihood, however, that Dunning's reported results are spurious. When estimating time-series regressions it is crucial that the series are either stationary – which means searching for unit roots if the data is in levels – or, if non-stationary, that they are co-integrated (see Granger and Newbold 1974; Phillips 1986).[8] Work we have done with the same measures Dunning uses, and which we discuss later in this paper, indicates that the series in levels are not stationary but *not* co-integrated.

III. Research Design

We frame the resource curse hypothesis as a counter-factual: in the absence of the discovery of oil or minerals, would countries' regimes have looked all that different? In order to answer this question, we focus on longitudinal change within countries over time. A dynamic, within country approach is ideal because the variables of interest exhibit considerable temporal variation, and that variance is not always monotonic: democratization may be either discontinuous or fitful, while oil and mineral sectors go through booms and busts, and countries both modify the amount of natural resources they choose to produce and change the way they tax natural resources. In other words, history –not scatter plots – provides the best way to tell the story.

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control for pre-existing institutions that may jointly determine autocracy and state-owned enterprises.

[8] There is also an issue of the validity of the independent variable in terms of the theory being tested—an issue that we hasten to add affects most of the literature, with the exception of Herb (2005). The causal mechanism that links oil to regime types is assumed to be the rents captured by the government from the production of oil, but the most popular variable employed captures total rents produced—some of which are captured by the government and some of which accrue to the rest of society. How the rents are divvied up depends, crucially, on the type of taxes and royalties that are levied, as well as on the tax rate and the enforcement of direct taxes on resources such as corporate taxes on oil and mining companies. These factors not only vary across countries, but they vary within countries over time.

We therefore build long-run historical datasets from primary sources, draw explicit counterfactual comparisons, graph the trajectory of political institutions against resource reliance over time, and employ dynamic, time series-centric econometric techniques—most particularly autoregressive distributed lag panel models. We employ cross-section time-series data instead of country-by-country time-series for two reasons. First, we are better able to detect whether the variables of interest have unit roots and, by extension, exhibit a long-run relationship (are co-integrated). This is because the augmented Dickey Fuller tests used to identify the stationarity of the data is more powerful in the panel context (see Levin and Lin 1992; Quah 1994). Second, panel estimators have been shown to militate against measurement error more effectively than time-series regressions (Baltagi 1995).[9]

Measuring Regime Types

Polity Score

Our primary measure of regime type is the Combined Polity 2 score—an index that measures the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive (see Gurr and Marshall 2005, pp. 15-6)—from the Polity IV dataset. For simplicity, we refer to this measure throughout this paper as the *Polity Score*. We center our analysis on the

[9] One could argue that the assumption of parameter heterogeneity implied by our cross-country time-series framework is unduly strong. We note that if we run individual time-series co-integration tests and regressions for seventeen of the largest oil and mineral producers in the world (see below), we attain materially similar results. We will discuss some of these results below, when we reevaluate Dunning's (2008) evidence for a "resource blessing" – i.e., that high levels of resource reliance in highly unequal societies is favorable to democracy. We also note thatwe do not employ the Pesaran and Shin (1999) pooled mean group estimator, which allows for slope heterogeneity in the short-run effect across panels, because this framework requires that the data be stationary.

Polity Score because it is the standard measure of democracy/autocracy employed in the resource curse literature, as well as in the field of comparative politics more broadly. In addition, the Polity Score is measured for each country in the world going back to its first year of independence, through 2006. Following conventions in the literature, and in order to make the regression coefficients easier to interpret, we normalize these scores to run from 0 (complete autocracy) to 100 (complete democracy).

Binary REGIME

The second measure we employ is the binary measure of democracy originally created by Przeworski et al. (2000) for the period 1950-1990, popularly known as *REGIME*. It is an "electoral" measure of democracy that is coded as a "1" if: (1) the executive is directly or indirectly elected; (2) the legislature is directly elected; (3) there is more than one party; and (4) control of the executive alternates between different parties under the same electoral rule. REGIME has been extended by Boix and Rosato (2005) from 1800 to 1999 and by Cheibub and Ghandi (2004), from 1946 to 2002.[10]

Counterfactual Polity

One potential concern with both of these measures is that they do not measure a country's level of democracy/autocracy relative to what it *might have become* had it not discovered oil or minerals. In order to address this concern, we develop a third measure of regime type—*Counterfactual Polity*. We construct this variable by assuming that, if a resource dependent country did not develop its resource sector, it would have obtained the same level of democracy as that achieved by the typical non-resource dependent

[10] We use the Cheibub and Ghandi (2004) data set, but fill in missing observations observations for some country years during the 1946-2002 period by using Boix and Rosato (2005).

country in their same geographic/cultural region. We therefore calculate, for each year, a synthetic, hypothetical Polity Score for each resource reliant country and then calculate how much that country's observed Polity Score differs from its hypothesized Polity Score. Specifically, *Counterfactual Polity* is the average Polity Score of the non-resource dependent countries, computed by region, and then subtracted from the resource dependent country's Polity Score.[11] Decreases in the gap between the hypothesized and observed Polity Scores over time bespeak convergence between a resource dependent country and its region's average Polity Score, while increases represent divergence.

Measuring Oil and Mineral Dependence

Fiscal Reliance on Resource Revenues

The first measure or resource reliance we employ is *Fiscal Reliance on Resource Revenue*s, the percentage of government revenues from oil or minerals. These revenue streams include taxes and royalties paid by oil or mineral companies, as well as dividend payments or direct transfers from government-owned oil and mining enterprises. This measure has both theoretical and practical advantages. The theoretical advantage is that, by capturing the extent to which natural resource rents are directly absorbed by regimes, this measure provides a direct test of the resource curse hypothesis that increased

[11] We specify a country as being non-resource dependent if its level of fiscal reliance on oil and minerals during the period 1972-1999 as measured by Herb (2005) was less than five percent. We code those few cases not treated by Herb (2005) on the basis of their ratio of oil and mineral exports to GDP (a ratio of less than five percent is coded as non-resource dependent). We note that, when we compare the countries for which we have both measures, our method produces few false negatives: countries that export oil and minerals worth more than five percent of GDP tend to obtain more than five percent of government revenues from those same resources. Finally, in the handful of cases where both measures of resource reliance are unavailable, we verify that they produce no, or extremely modest amounts, of petroleum from our dataset on per capita oil production. We also verify that they produce no, or extremely modest levels, of minerals from information in the Central Intelligence Agency's *World FactBook*.

revenues from resources induce or protract authoritarianism by allowing governments to operate autonomously from citizens. The practical advantage is that this measure has generous longitudinal coverage. By retrieving and standardizing countries' fiscal statistics from the annual reports and retrospective series of central banks, statistical offices and treasury ministries, we are generally able to construct this measure from independence to 2006. In most cases, this means that our measure extends back to the period before countries became major oil or mineral producers.[12]

There is one practical disadvantage to this measure, however: the retrieval and standardization of idiosyncratically organized fiscal data from historical sources is not an enterprise characterized by economies of scale. We therefore have to truncate our coverage of *Fiscal Reliance* with respect to the number of countries. We do so by applying three criteria: 1) a country had to have oil or mineral revenues equal to at least five percent of total government revenues during the period 1972-1999, based on data from Herb (2005); 2) we had to be able to obtain uninterrupted volumes of the serial publications that contain countries' fiscal data; and 3) those records had to be transparent enough to allow for the identification of oil and mineral revenue streams, as well as total government revenues.

Seventeen major resource exporters met these criteria: fifteen oil producers and two of the world's major copper producers. The fifteen oil producers are Mexico, Venezuela, Ecuador, Trinidad and Tobago, Nigeria, Angola, Indonesia, Iran, Algeria, Bahrain, Yemen, Oman, Saudi Arabia, Kuwait, and Norway. As a group, they accounted

[12] One concern is that by relying on central bank or treasury records we are potentially omitting oil or mineral revenues that are available to rulers but *not* captured via taxation, royalties, or dividends from state-owned oil or mining enterprises. We recognize this potential shortcoming, and try to mitigate it by reviewing, where possible, the records of state-owned oil or mineral companies in order to detect off-budget government expenditures.

for 45 percent of the world's oil output in 2006. The two copper producers are Chile, which is the world's most important producer (accounting for 35 percent or world output in 2005), and Zambia, which is the world's tenth most important copper producer (accounting for 3 percent of world output).

One might worry that our restrictions have yielded cases that are potentially unrepresentative and, thus, that our time-series results on the relationship between *Fiscal Reliance* and regime type cannot be extrapolated to the history of other resource producers. There are two reasons why we doubt this to be true. The first is that many of the major petroleum and copper producers that we have excluded are now robust democracies that have achieved the highest possible democracy score across the popular measures of regime type.[13] Therefore, if there is sample selection bias engendered by our incomplete coverage of fiscal reliance on resource revenues with respect to countries, it is likely to run in the direction of the resource curse, making it easier to find a relationship between natural resources and authoritarianism. The second reason is that we develop two other measures of resource reliance (see below) for panels covering the entire world, and they yield regression results that are not materially different from those we obtain with Fiscal Reliance on Resource Revenues.

Per Capita Gross Petroleum Rents

[13] These countries include, but are not restricted to, the United States, Great Britain, Canada, and Australia. The United States is a particularly poignant case: from the middle of the nineteenth century until 1975, when it was finally passed by Saudi Arabia, it was the world's leading producer of petroleum. It remains the world's third largest producer of petroleum, accounting for seven percent of world output. From the 1870s until 1910 the United States was also the world's leading mineral producer (David and Wright 1997). To this day it remains the world's second most important producer of copper, accounting for 7.5 percent of world output—following only Chile, which is also democratic.

The second measure we employ is *Per Capita Gross Petroleum Rents*, which we compute as the output of oil in barrels, multiplied by the world price of petroleum in constant 2007 U.S. dollars, divided by population.[14] Petroleum production for every country-year was obtained from yearbooks produced by petroleum consulting companies, producers associations, and government agencies. This measure has two main advantages. The first is longitudinal coverage: our observations start with the first year of a country's independence and extend to 2006. With the exception of a few producers in the Middle East and Central Asia, this means that we can observe countries before they became major oil producers. The second is that we do not confront issues of country selection: this variable covers 166 countries—virtually every country in the world. Of these, as many as 94 produce oil in any given year. The chief disadvantages of this variable are that it does not include rents from non-fuel minerals; and does not reflect differences across countries and time in petroleum extraction and refining costs.[15]

Per Capita Windfall Profits from Natural Resources

In order to mitigate potential problems of measurement error in Gross Oil Rents Per Capita we develop a third measure, *Per Capita Windfall Profits from Natural Resources*. This measure is calculated from data retrieved from the World Bank's *World Development Indicators*. It is the value of the oil, gas, coal, hard rock minerals and precious metals produced, minus the costs of production and the opportunity cost of

[14] This measure, as well as any of the measures normalized by population that we employ, is measured in thousands in the regressions that follow in order to facilitate interpretation.

[15] To some extent, this problem is mitigated by the fact that the decision to produce each marginal unit of oil is, at least in the medium run, a function of the rents available to producers: if production costs are too high relative to the world price, countries cut back on output, which is implicitly incorporated into this measure.

capital, converted to constant 2007 U.S. dollars, divided by population. This measure, originally developed by Hamilton and Clemens (1999), has two clear advantages over Gross Oil Rents Per Capita: it includes hard rock minerals as well as oil; and it implicitly controls for production costs.[16] Like Gross Oil Rents Per Capita its coverage is global: our data set includes 146 countries, of which 104 exhibit positive values for windfall profits from oil or minerals in any given year. This measure does, however, have an obvious disadvantage: it is considerably longitudinally truncated; the first World Bank estimates of the components of this measure are not available until 1970. Thus, while our other measures of resource reliance run from a country's first year of independence to 2006, this measure only runs from 1970 to 2006.

Control Variables

The unrestricted specifications that follow include several control variables. Following modernization theory – increasing wealth drives democratization (Lipset 1959), or at least protects it (Pzeworski et al. 2000) – we include the log of Real Per Capita GDP. We take this data from the Penn World Tables, version 6.1, and update it to 2006, using data on the rate of economic growth from the World Bank Development Indicators (2008). We also include the Growth Rate of *GDP Per Capita*, following the widely held belief that economic performance (high growth) can lead to regime stability and economic crises (negative growth) can catalyze regime transitions (e.g., Gasiorowski 1995). Finally, in order to hold democratic diffusion effects constant, we control for regional and world trends in democratization, as well as the possible contagion effects of

[16] It also provides an advantage over other measures that can be developed from online data, such as oil and mineral exports as a percent of GDP, in that it measures production that is both consumed domestically and exported (Ross 2008).

nearby democratic transitions. Following Gleditsch and Ward (2006), we add three variables to the regressions: the percentage of democracies in a country's geographic-cultural region (with democracy measured as a score of 85 or higher on the normalized Polity Score); the percentage of democracies in the world; and a dummy variable coded "1" for country-years in which a democratic transition occurs within the country's region.

IV. Data Analysis

Hypothesis One: Natural Resources Undermine Democracy

There are occasions in social science research when descriptive information and simple graphs are enough to cast serious doubt on a hypothesis—and this is one of those occasions. Before diagnosing the time-series properties of our data, and reviewing the results of several multivariate analyses, we first conduct some simple tests by inspecting and graphing the data. Of the 164 countries for which we have data, 33 enter the data set with a Polity Score that meets Gleditsch and Ward's (2006) criteria for a stable democracy (a Polity Score of at least 85 on our normalized scale). Of those 33 countries, seven display non-trivial values on *any* of our three measures of oil and mineral reliance. Of those seven resource reliant democracies, five—Australia, Botswana, Trinidad and Tobago, Jamaica, and Papua New Guinea—remained democratic. The data for Trinidad and Tobago, displayed in Figure 1, illustrate the general pattern: even during a period in which petroleum rents increased dramatically, and 60 to 70 percent of government revenues derived from oil and gas, the country's Polity Score actually ticked up.

One might be tempted to argue that the remaining two cases—Malaysia and Nigeria, which are oil producers, and which underwent periods of authoritarianism—provide evidence consistent with the resource curse. The problem with this reading of the

facts is that democracy broke down in these countries *before* they ever became significant oil producers. Figure 2 graphs the Nigerian data, and it makes clear that oil accounted for only seven percent of government revenues, and gross oil rents were only $36 per person, in 1966, when a coup ended democracy. In order to make a convincing case for the resource curse, one would have to believe that the military officers behind the 1966 coup foresaw the run-up in world oil prices in 1973 that ushered in Nigeria's petroleum boom. In addition, one would also have to account for the fact that Nigeria has been trending towards greater democracy since 1998. Between 1998 and 2006, its fiscal reliance on petroleum grew by 40 percent, its gross rents from oil per capita more than tripled, and its windfall profits from oil increased almost five-fold.

Hypothesis Two: Natural Resources Impede Democratic Transitions

As with hypothesis 1, we begin our analysis by visually inspecting the data; and present some representative graphs that plot the longitudinal relationship between our three measures of resource reliance and a country's Polity Score. We do not find compelling evidence in favor of the hypothesis that resources impedes democratization.

One of the first features of the data that jumps off the page is that there is a set of countries that were authoritarian *prior* to the exploitation of oil or minerals, and which then democratized during a period in which their oil or mineral sectors boomed. Such cases include Ecuador, Chile, Peru, Venezuela, and Mexico. To provide a sense of what the data looks like, Figure 3 graphs the data for Mexico. The increase in Mexico's Polity Score from 20 in 1976 to 90 in 2000 captures the country's transition from a long-lived authoritarian regime to a multiparty democracy. What is particularly striking about the increase in Mexico's Polity Score is that it was coterminous with the rebirth of the

Mexican petroleum industry, which had been stagnant since the late 1920s. During the heyday of one party rule in Mexico—the 1950s and 1960s—income from oil typically accounted for less than five percent of government revenue, and real gross rents from oil typically amounted to only $35 per capita. In 2000, when the PRI finally lost its grip on power, oil income accounted for 23 percent of government revenues and real gross oil rents had jumped to almost $400 per capita. By 2006, when Mexico held a second free and fair election, the percentage of government revenues derived from oil, and real gross rents from oil, stood at historic highs: 37 percent and $865 per person, respectively.

The second surprising pattern revealed by visually inspecting the data is that there is a set of countries that were highly authoritarian *prior* to the exploitation of oil or minerals and which, while they did not fully democratize, saw at least a twofold increase in their Polity Scores during the period in which their resource sectors grew rapidly. These cases include Chad, Iran, Egypt, Yemen, Algeria, and Angola. Figure 4, which graphs the time-series relationship between our measures of resource reliance and Polity for Angola, provides a representative example. The Polity Score, Fiscal Reliance, Real Gross Rents from Oil Per Capita, and Real Windfall Profits from Oil and Minerals Per Capita all trend together, and the trend is a monotonic, secular increase.

The third pattern that emerges is that there is a group of countries, most of which are clustered around the Persian Gulf, that have persistently low Polity Scores and that are highly reliant on oil. The majority of these countries, however – as Herb (1999) has pointed out with respect to Kuwait – had authoritarian political structures in place for decades *before* the first drop of oil ever flowed. Other examples include Saudi Arabia, Iraq, Libya, Oman, and Equatorial Guinea. This suggests that the number of countries

born as autocratic "petro-states," in which one can claim that oil fundamentally conditioned the new nation's political institutions, is actually quite small: candidates include Qatar, the United Arab Emirates, Bahrain, Azerbaijan, and Turkmenistan.

There is a final group of countries whose data display a pattern that accords with the resource curse: when resource dependence is at low levels, irrespective of the way in which it is measured, the country has a high Polity Score; conversely, when resource reliance is high, the country exhibits a low Polity Score. An example is Indonesia, whose data we graph in Figure 5. The number of such cases, however, appears to be exceedingly small. Besides Indonesia, potential candidates include Syria and Gabon.

Multivariate Analysis

When the central objective of the estimation approach lies with the data's time-series processes, two issues must be addressed before moving to regression analysis. The first is the stationarity of each data series: do the data have the same mean, variance and co-variance over time? The second is specifying the data's correct lag structure. One might imagine that political actors and institutions do not automatically react to changes in resource dependence. One might also imagine that changes in resource dependence come in two flavors; and each has a differential effect on political actors and institutions. Changes in resource reliance may be transitory, if they are a product of a short-lived increase in the price of oil, for example, or permanent, if they arise from a technological innovation that improves resource extraction. It is important to model these dynamics.

Unit Root and Co-integration Tests

Our graphed data (not all of which we reproduce here) indicate that the various measures of resource dependence tend not to be mean-reverting—there is an upward trend. This is also the case for several countries' Polity Scores. The implication is that the data are non-stationary. Therefore, we first apply a series of diagnostics to determine whether the data are non-stationary and, if they are indeed not stationary, whether they are co-integrated. We conduct a series of Maddala and Wu (1999) based augmented Dickey-Fuller tests for unbalanced panel data (based on Fuller 1932), and we do so for each resource dependent measure and for Polity, with separate diagnostics for each distinct time period covered by our resource dependence measures. For example, because Fiscal Reliance covers seventeen countries between 1800 and 2006, we check to see if Polity is stationary during this time period and across these particular countries. Conversely, because Windfall Profits on Oil or Minerals Per Capita has global coverage, but is truncated to the years 1970-2006, we check to see if Polity is stationary during this time period and across the countries covered by this variable. Table 2, Panels A and B, presents the results (Table 1 presents summary statistics for the variables in the dataset). The null hypothesis is that the data are non-stationary.

The tests performed on the data in levels (Panel A) indicate that, in the majority of cases, both the dependent and independent variables are non-stationary. The sole exception is Per Capita Gross Oil Rents. The augmented Dickey Fuller tests in Panel B of Table 2 show that the series that are not stationary are integrated of order 1: first differencing the data makes these series stationary.

The non-stationarity of most of our series means that unless the series are cointegrated, it is inappropriate to estimate regression in levels. To discover if Polity is

co-integrated with the non-stationary measures of resource dependence, we test for unit roots in the residuals of static models in which Polity is regressed against Fiscal Reliance and Per Capita Windfall Profits, respectively.[17] We again apply the Maddala and Wu (1999), augmented Dickey-Fuller tests. Since these unit root tests are conducted on the aforementioned regressions' residuals, the null hypothesis is that the variables are *not* co-integrated. The results for these co-integration tests are documented in Table 2, Panel C. All of the tests on the non-stationary series produce insignificant results. This means that there is no evidence for a long-run relationship between countries' Polity Scores and their resource dependence. Failure to find evidence for a long-run relationship between the level of democracy and the degree of natural resource dependence casts strong doubt on the claim that resource reliance is *causally* associated with autocracy.

Regression Specification

Because the diagnostics described above reveal that our series are either non-stationary and not co-integrated, or are a mix of stationary and non-stationary series, the implication is that we should estimate a specification in first differences.[18] Despite the lack of co-integration, however, our regressions should still account for both short and long-term effects made by changes in resource dependence on changes in Polity. We

[17] Granger (1981) argues that two or more non-stationary time series that become stationary after first-differencing may have linear combinations that are stationary *without* differencing. If such a relationship in levels exists, these non-stationary variables are said to be co-integrated. The existence of a stationary, long-run relationship between series that are individually non-stationary justifies an evaluation of the relationship in levels between non-stationary variables.

[18] See Beck and Katz 2004: 26; Kittel and Winner 2005, footnote 10; Wooldridge 2006: 652-53). We note that An Error Correction Model (ECM) is *not* appropriate in these situations because, though the dependent variable is first differenced in an ECM, and thus stationary, the introduction of covariates measured in levels may lead to spurious relationships when they are not stationary.

therefore adopt an Autoregressive Distributed Lag (ARDL) framework in first differences.[19] In order to select the right number of lags of Polity, we choose a vector autoregressive model with the lowest AIC statistic. In order to select the right number of lags of resource dependence, we choose the number of lags that minimizes the AIC statistic. Since we introduce country dummies into the regressions, the coefficients on the independent variables represent a cross-country average of the longitudinal effect.[20]

Specifically, we run a model with the following functional form:

$$\Delta \mathbf{Y}_{it} = \Delta \mathbf{X}_{it}\beta + \mathbf{n}_i\varphi + \mathbf{v}_t\lambda + \mathbf{u}_{it} \quad (1)$$

where **Y** is a (n×1) vector of observations on the dependent variable, **X** is a (n×k) matrix of n observations on *k* explanatory variables. Variables included in **X** include the one year lag of the dependent variable, as well as higher order lags of the DV, as selected by the AIC statistic; and the contemporaneous value of the measure of resource reliance

[19] First-differencing significantly affects the interpretation of the results. Most crucially, we forfeit the information about the *degree* of reliance on minerals and oil and retain only the information about its *change*. This implies that we are relegated to making inferences about the effect that the size of the change in resource reliance has on the size of the change in regime type. Conversely, the resource curse implies that autocracy, observed during any particular year, is not necessarily related to short-run fluctuations in resource reliance – in or before that year – but to the *accumulation* of resource reliance since the discovery of resources. We stress that we have ruled this type of relationship out, however, via the co-integration tests described above.

[20] Although first differencing the data expunges country fixed effects, and therefore controls for countries' unobserved, time-invariant heterogeneity, we also include country dummies. The country dummies control for systematic cross-country differences in the annual changes in Polity; in other words, for country-specific time trends (see Kittel and Winner 2005: 280; Daveri and Tabellini 1997: 26). We also note that, across our models, AR(1) serial correlation is eliminated by introducing one or more lags of the DV – according to Arellano Bond serial correlation tests (see Arellano and Bond 1991) – higher order serial correlation is not always eliminated (detected via the same Arellano Bond tests). Therefore, across our models we estimate robust standard errors clustered by country. Robust standard errors clustered by country are estimated via a variant of the Huber/White/Sandwich technique and yield estimates that are robust to both groupwise and within-country heteroskedasticity, as well as correct coverage in the presence of *any* arbitrary correlation structure among errors within the country panels (Williams 2000: 645). Shocks that are common across countries in any given year are estimated by introducing year dummies (also, since robust standard errors clustered by country are not robust to contemporaneous correlation between panels, the year dummies address this issue).

used, as well as lags of resource reliance as selected by the AIC statistic: **X** includes Δregime type measure$_{t-1}$ through Δregime type measure$_{t-k}$, as well as Δresource reliance measure$_t$ and Δresource reliance measure$_{t-1}$ through Δresource reliance measure$_{t-m}$. Also, in some specifications, contemporaneous values of several control variables, and in some cases their values at relevant lags, are also included. Finally, in some specifications the interaction of the measure of resource reliance used and log(Per Capita GDP) are also entered into the equation, either contemporaneously (Δresource reliance measure$_t$ X Δlog(Per Capita GDP)$_t$), or at some lag of both variables, (Δresource reliance measure$_{t-m}$ X Δlog(Per Capita GDP)$_{t-m}$). Meanwhile, β is a (k×1) vector of parameters, **n** is a country fixed effect potentially correlated with variables in **X**, **v** is a year fixed effect potentially correlated with variables in **X** and **u** is a (n×1) vector of disturbance terms that are unique to each country and assumed to be possibly heteroskedastic and correlated within countries. Both **n** and **v** imply that a dummy variable for each country in the data set (except for one) are included in the equation; and a year dummy for each year in the panel data set (except for one) are also included. Heterogeneous intercepts are estimated by country and year (the φ and λ vectors, respectively).[21]

[21] Because of the terms discussed above, Equation (1) depicts an ARDL(p,q) with p equal to the number of lags of Polity that are selected by the AIC statistic and q equal to the number of lags of the measure of resource dependence selected. Moreover, Equation (1) is a rational distributed lag model with an infinite lag. Because country dummies are also estimated, there is the concern that bias is induced via the correlation between the lagged dependent variable(s) and the individual effects (see Nickell 1981). The bias's severity is a function of the sample size and the magnitude of the autoregressive coefficient and decreases as T grows. Monte Carlo evidence uncovered by Judson and Owen (1999) show that a fixed effect estimator performs as well or better than alternative approaches that use instrumental variables or a methods of moments (GMM) approach to address this bias. Because T is always greater than 30 across our regressions, we do not think it necessary to move beyond the Ordinary Least Squares context. We note, however, that after running a series of so-called Arellano Bond, difference GMM dynamic panel data regressions, in which first differencing the data expunges the country fixed effects and the lagged dependent variable(s) are instrumented with all of their available lags, materially similar results are returned.

Several values hold interest: the Impact Multiplier (the coefficient on the contemporaneous value of resource dependence); the coefficients on the lags of resource dependence; and the Long-run Multiplier (the total, long-run effect). The Impact Multiplier is the immediate change in Polity due to a one-unit, temporary change in resource dependence. The coefficients on any of the lags of resource dependence are the delayed changes in Polity that occur after a temporary, one-unit change in resource dependence. Finally, the Long-run Multiplier depicts the total changes in Polity due to a permanent change in resource reliance at an indeterminate time in the past.[22]

Empirical Findings

We begin with a set of regressions in which resource dependence is proxied by Fiscal Reliance on Oil or Minerals. We present the results of a base specification of this regression in Table 3, Column 1. If there is a resource curse, we would expect to find a negative coefficient on the Long Run Multiplier. We would also expect the majority of the coefficients on the lagged independent variable to be negative, with many of them significant. The theoretical predictions on the Impact Multiplier are somewhat ambiguous, but one would probably not expect to find systematic positive coefficients.

The regression results, however, yield coefficients with the "wrong" signs. The Long Run Multiplier, the Impact Multiplier, and the majority of the coefficients on the

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[22] The Long-run Multiplier is calculated by setting the dependent and independent variables at their long-run values for all *t* and then finding the change in the long run value of the dependent variable with respect to the long run value of the independent variable. See Wooldridge (2006): 638. Since the Long-run multiplier is non-linear function of the estimated coefficients, its standard error is computed via the Delta Method. Because of the correlation in changes in resource reliance at different lags, it is often hard to gain precise estimates of the individual coefficients for each lag. Therefore, for each distributed lag model we also calculate an F-test on the hypothesis that the contemporaneous value of resource reliance and its lags are jointly statistically significant.

lagged independent variable are positive. To the degree that any variables yield a statistically significant result, it is that the coefficient on the sixth lag of the independent variable – yet it is positive.[23] One might be tempted to argue that our results are an artefact of our (very) long time series. Perhaps the resource curse is a post-World War II phenomenon only. Column 2 of Table 3 therefore truncates the estimation to the 1950-2006 period. This move has no material effect on the results.

One might also be tempted to argue that the positive coefficients we obtain are the product of concomitant increases in GDP per capita that accompany the creation of economies that experience increases in their resource dependence. Specification 3 therefore adds controls for the log of GDP Per Capita and the Growth Rate of GDP Per Capita. One might also argue that the positive coefficients we obtain are the product of the fact that there has been a world-wide trend towards increased democracy. We thereforecontrol for the possibility of contagion effects in specification 3 with three variables, following Gleditsch and Ward (2006): the percentage of countries in the world that are democratic, the percentage of countries in a resource producer's geographic region that are democratic, and a dummy for a democratic transition in a resource producer's geographic region. If anything, these controls strengthen our earlier results: the coefficient on the Long Run Multiplier is now positive and significant at the 90 percent confidence level.

A sceptical reader might argue that there are conditional effects. For example, she might suggest that resource dependent countries with low per capita incomes (such as Equatorial Guinea) might be affected by the resource curse, while wealthy resource

[23] For clarity of presentation, we do not report the coefficients on every lag, but only the last lag, and any before the last lag that comes up as statistically significant in any specification.

dependent countries (such as Canada) might be immune. We therefore add interactions of Per capita GDP and Fiscal Reliance on Oil or Minerals in specification 4 of Table 3 and calculate the marginal effects of Fiscal Reliance at the 25th, 50th, and 75th percentiles of Per Capita GDP. The results are inconsistent with this conditional resource curse view: the coefficients on the lower values of GDP Per Capita have the wrong sign; and none of the coefficients are statistically significant. An even greater sceptic might argue that the conditional effects occur with a lag. We therefore interact per Capita GDP in t-6 and Fiscal Reliance on Oil or Minerals in t-6 (we choose t-6 because it is the one lag of Fiscal Reliance that is statistically significant across our regressions). This step, reported in specification 5, increases the statistical significance of the marginal effects of Fiscal Reliance as GDP Per Capita increases. Nevertheless, we still cannot obtain the negative coefficient that one would expect at the 25th percentile of GDP (nor at lower values of Per Capita Income that we do not report).

A diehard advocate of the resource curse might still insist that the resource curse is actually a result of recent geo-strategic developments. She might argue that the dramatic increase in oil prices after 1973 gave significant leverage to oil producing countries, allowing them to become price setters and deploy the resulting rents to make their governments accountability-proof; knowing that uninterrupted access to oil trumps any concern about democracy. She might also point to the fact that increasing numbers of oil producing governments nationalized their oil industries in those same decades, giving them greater control over oil rents. We therefore test the hypothesis that the resource curse is conditional with respect to this particular time period by truncating the

dataset to the period 1973-2006. This experiment, reported in specification 6 of Table 3, has no material effect on any our results.

One might argue that the results in Table 3 are a product of sample selection bias, because our panel dataset with Fiscal Reliance is truncated with respect to the number of country cases. We therefore substitute Gross Oil Rents Per Capita, whose coverage is global, as the independent variable in Table 4, and re-run all of the regressions. The material results do not change appreciably when we substitute this new independent variable. Five the six specifications yield Long Run Multipliers with the wrong (positive) sign. We never detect statistically significant coefficients on the lagged independent variable. In fact, in some specifications the coefficients on the lagged independent variable are positive and statistically significant. There is only one specification that produces a result that is at all consistent with the Resource Curse—specification 1, which is run on the entire length of the panel and includes none of the conditioning variables. In this specification, the Long Run Multiplier is negative (but far from significant) and the Impact Multiplier is negative and significant. This result is curious, given the non-results in the other regressions. We therefore split the sample to the pre-1950 period and the post-1950 period, and allow the AIC to choose the most appropriate lag structure for each sub-sample. When we do so, we find that the Long Run Multipliers are positive in both specifications, while the Impact Multiplier is positive and highly significant in the pre-1950 period (results not shown), and negative in the post-1950 period. It is, however, not statistically significant (see Table 3, specification 2).

As a robustness check we re-estimate all the regressions run in Tables 3 and 4, substituting Windfall Profits from Oil and Minerals Per Capita as the independent

variable. The results, reported in Table 5, are not materially different from the results in Tables 3 and 4. All the specifications yield Long Run Multipliers that have the wrong (positive) sign. The lagged independent variables are consistently positive. Although the Impact Multiplier is negative in specification 1, and significant at the 90 percent level, once we introduce control variables (specifications 2 through 4), the sign of the Impact Multiplier turns positive.

Robustness of Results: Democracy as a Binary Variable

Several researchers claim that regime types are best modelled as binary variables instead of continuous ones (see Przeworski et al. 2000: chapter 1). We therefore re-run the regressions on the Gross Oil Rents Per Capita and Windfall Profits from Oil and Minerals Per Capita as dynamic conditional fixed effects logit regressions with a binary measure of democracy, REGIME. Because we need to include the Per Capita GDP and the Growth Rate of GDP Per Capita, the dataset is truncated to post-1950 observations.[24] The estimation technique we adopt affords many advantages. First, it allows us to calculate separate estimates for those countries observed as democratic and those observed as autocratic—and then see whether they switch regime type as a result of increased resource reliance. Second, we can include the independent variables in levels. Third, departing from dynamic probit based approaches without country fixed effects (e.g., Ross 2008), we can control for time-invariant heterogeneity between countries.

A dynamic conditional logit model can estimate a first-order Markov chain transition process between different states over time, where the probability distribution of y_{it} for observation i at time t is modelled as a function of i's prior state at previous time

[24] We do not run these logit regressions on the dataset on Fiscal Reliance on Oil or Minerals because there is not enough switching from one regime type to the next with only 17 countries.

periods, t -1,…, t-T. Specifically, if we invert REGIME, so that autocracies are coded as a "1", we can evaluate the conditional transition probabilities, while expunging country specific fixed effects, via the following functional form:

$$\Pr(y_{it} = 1 \mid y_{it-1,} \mathbf{X}_{it-1}) = \Lambda[\alpha_i + \mathbf{X}_{it-1}\beta + y_{it-1}\rho + \xi(y_{it-1}*\mathbf{X}_{it-1}) + \mathbf{v}_t\lambda + \mathbf{u}_{it}] \quad (2)$$

where $\Lambda(\cdot)$ is the logistic cumulative distribution; α is the intercept term for country i and depicts the fact that the country fixed effects are potentially correlated with variables in **X** (although these coefficients are not actually estimated); **X** is a (n×k) matrix of n observations on k explanatory variables; β is a vector of estimated parameters that indicate the effects of the covariates on the probability of a 1 at time t given a 0 at time t-*1* and ρ is the estimated coefficient on the lagged dependent variable; meanwhile, the effects on the probability of a 1 at time t given a 1 at time *t-1* are given by $\beta + \xi$ (the coefficients on the interactions between y_{it-1} and $\mathbf{X}_{it}$). Meanwhile, **v** is a year fixed effect potentially correlated with variables in **X** and **u** is a (n×1) vector of disturbance terms that are unique to each country and assumed to be possibly heteroskedastic and correlated within countries. Finally, **v** implies that a dummy variable for each year (except for one) are also included, represented by the heterogeneous intercepts in vector λ.[25]

The first set of coefficients evaluates the hypothesis that *oil undermines democracy*; and the addition of these coefficients and their respective interaction terms evaluates the hypothesis that *oil prevents democratization*. Therefore, the coefficient on the measure of resource reliance (un-interacted with the lagged dependent variable) is the effect of resources on the likelihood that a democracy will revert to authoritarianism. Conversely,

[25] We note that if a country did not experience a regime change during the time period under consideration, the estimation procedure drops the panel's observations because countries that do not switch from one state to another do not contribute any information towards the optimization of the log-likelihood function.

because the addition of this coefficient and its interaction term represents the effect of resource reliance on the likelihood that an autocracy will remain autocratic, if we subtract the product of this addition from 1 we can identify the impact of resource reliance on the odds of democratic transition.[26]

We present the results in Table 6. Specification 1 models the effect of increases in Per Capita Oil Rents on countries that are observed in any year as democratic. Per Capita Resource Reliance t-1 tells us the effect of an increase in Per Capita Oil Rents on the probability that those countries will become autocratic. If increases in resource dependence are associated with the breakdown of democracy, the coefficient should have a positive sign. Our results, however, tell the opposite story: the coefficient is negative, although not significant. Specification 2 models the effect of increases in Per Capita Oil Rents on countries that are observed in any year as authoritarian. Here the resource curse would predict a negative coefficient: as Per Capita Oil Rents increase, authoritarian countries should be *less* likely to transition to democracy. Once again, however, our results yield the opposite result: the coefficient is positive (although not significant).

We repeat these operations in specifications 3 and 4, substituting Windfall Profits Per Capita as the independent variable. We can now no longer use Cheibub and Gandhi's (2004) version of the Przeworski et al (2000) REGIME variable. This is because between 1970 and 2002, the years for which Cheibub and Gandhi provide data coverage, most countries exhibit a strictly monotonic trend in Windfall Profits Per Capita and, thus, the

[26] To calculate the z-statistics and p-values for the coefficients that gauge the probability of transitions from autocracy to democracy, we use the Delta Method because we are calculating the statistical significance for the addition of a linear term and its interaction with the lagged DV (Alternatively, a Wald test of the hypothesis that $\beta + \xi = 0$ returns the same results). All z-statistics are derived from robust standard errors clustered by country to address serial correlation and heteroskedasticity (see Beck et al. 1998).

maximum likelihood estimation fails because convergence does not occur. If we extend the dataset to 2006, however, the dominant pattern in Windfall Profits is no longer secularly monotonic. We therefore construct a binary measure from 1970 to 2006 derived from Polity (following Gleditsch and Ward 2006). Specification 3 models the effect of increases in Windfall Profits Per Capita on countries observed as democratic. Once again, the regression produces a result with the wrong sign. Specification 4 models the effect of increases in Windfall Profits on countries that are observed as autocratic. It produces a coefficient with the predicted (negative) sign, but it is very far from statistical significance.

Multivariate Analyses of Hypothesis III

One potential criticism of our analyses so far is that, because they estimate country fixed effects, they do not take into account countries whose Polity Scores, or binary democracy measure, do not vary over time (see Ross 2008). Another potential criticism of our tests is that they only measure countries against themselves through before and after comparisons. They therefore do not measure countries against what they could have become during the period in which they have resources had they never developed their oil or mineral sectors.

In order to address these concerns, we developed Counterfactual Polity, which measures the gap between the Polity Score of a resource dependent country and the average Polity Score of the non-resource dependent countries in their respective regions. Therefore, it exhibits time series variance even if the resource dependent country's Polity Score does not vary. Counterfactual Polity also addresses the criticism about measuring countries against what they could have become. The logic is that a resource dependent

country could have obtained the average Polity Score of the non-resource dependent countries in its geographic region.

Unit root and co-integration tests

We conduct the same diagnostic tests on Counterfactual that we did on Polity in order to choose the correct functional form for the regressions. We begin with augmented Dickey-Fuller tests to investigate whether the data is stationary (see Table 2, Panels A and B). These test indicate that Counterfactual Polity is non-stationary, except for the particular sub-period 1970-2006, when it is included in a regression against Windfall Profits on Oil or Minerals. To identify if there is co-integration between Counterfactual Polity and our resource dependence measures we again apply Maddala and Wu (1999), augmented Dickey-Fuller tests on the residuals of a regression in which Counterfactual Polity is regressed against resource dependence. The results of these co-integration tests are shown in Table 2, Panel C. In only one of these specifications (Panel C, Column 2) do we find evidence of a long-run relationship: Counterfactual Polity and Fiscal Reliance on Resource Revenues appear to be co-integrated.

Regression Specification

Our diagnostic tests indicate that the regressions of Counterfactual Polity against Gross Oil Rents Per Capita and against Windfall Profits Per Capita should be estimated using the ARDL framework in first differences we introduced earlier. Our tests also indicate that it is possible to model the relationship between Counterfactual Polity and

Fiscal Reliance on Oil or Minerals under this same ARDL framework, but with a slight twist: via an Error Correction Model (ECM) interpretation.[27]

The ECM interpretation makes three improvements to the purely first-differenced ARDL model. First, it allows us to model the relationship in levels between Polity and resource dependence. Second, the ECM not only allows for the evaluation of both short and long run impacts of changes in the independent variable, it also allows the estimation of the speed at which the long-run equilibrium between the variables is restored – or "corrected" – after a short-term shock. We therefore estimate a regression that can be expressed with the following functional form:

$$\Delta Y_{it} = \Delta Y_{it-1}\rho_0 + \ldots + \Delta Y_{it-k}\rho_k + \Delta X_{it}\beta_1 + \Delta X_{it-1}\beta_2 + \ldots + \Delta X_{it-k}\beta_k + \delta(Y_{it-1} - X_{it-1}\gamma) + n_i\varphi + v_t\lambda + u_{it} \quad (3)$$

where short-run changes in Y that take a year's time to elapse are captured by the coefficients on the differenced independent variables; increases in X produce a change in Y that also disrupts the long-term equilibrium relationship between the level of X and Y and subsequent (lagged) changes in Y are conditioned by deviations from the long-run equilibrium. Y will respond by gradually returning to this relationship, registering a total change equal to γ. Specifically, when $Y_{it-1} > X_{it-1}\gamma$ (the coefficient on the lagged independent variable in levels is negative), then Y has overshot the equilibrium in the

[27] In the case of non-stationary variables that stationary after first-differencing, such as Counterfactual Polity and Fiscal Reliance, Engle and Granger (1987) have shown that if there is a linear combination of the non-stationary processes that is itself stationary, call it $y_{it} - \gamma x_{it}$, then y_{it} and x_{it} are co-integrated with parameter γ and their time-series relationship can be depicted through an ECM framework. Furthermore, the latter is equivalent to an ARDL approach (see DeBoef and Keele 2008). These innovations are made possible by simply adding the stationary variable identified above, $y_{it} - \gamma x_{it}$ to equation (1). More specifically, lags of each constituent part are added, so that the model now also includes y_{it-1} and γx_{it-1} and the equation (excluding control variables for simplicity). DeBoef and Keele also argue that an ECM framework is the most valid way to model the relationship between *stationary* time series when complex dynamics at work.

previous period and the error correction term works to push Y back towards the equilibrium by inducing negative changes in subsequent periods at a rate determined by δ. Alternatively, when $Y_{it-1} < X_{it-1}\gamma$ (the coefficient on the lagged independent variable in levels is positive), then Y has remained below the equilibrium in the previous period and the error correction term induces positive changes in subsequent periods. The δ term is < 0 and is the error correction rate; so that a δ proportion of this discrepancy (or "error") is corrected by a movement in the dependent variable each subsequent period. Finally, if $Y_{it-1} = X_{it-1}\gamma$, the dependent and independent variable are in a long-run equilibrium.

Table 7 presents the results of these estimations, repeating the order of specification from Table 3. The Resource Curse would predict that Counterfactual Polity and Fiscal Reliance on Oil or Minerals will tend to revert back to their long-run relationship: over time higher fiscal reliance will lead to lower Counterfactual Polity. Capturing the process by which these two variables remain in equilibrium requires three terms: the short run shocks (represented in our model as the first-differenced versions of Fiscal Reliance); the error correction term, which is the rate at which the equilibrium is restored (represented in our model by the lagged dependent variable in levels); and the Long Run Multiplier, which is the total effect that an increase in Fiscal Reliance has on Counterfactual Polity, spread over future time periods. The resource curse would predict negative coefficients for the first-differenced Fiscal Reliance terms, as well for the Long Run Multiplier.[28] Meanwhile, whether there is a resource curse or a resource blessing, the error correction term should be negative since it depicts the restoration to equilibrium.

[28] We constructed Counterfactual Polity so that its values *shrink* as the average Polity Score for non resource producers becomes *greater* than the resource producers' Polity Score.

The coefficients in Table 7 do not yield the predicted signs. The coefficients for the short-run changes in Fiscal Reliance and for the Long Run Multiplier are positive in every specification. There also appears to be a positive short run effect (at the sixth year lag) that enters as significant in some specifications. That is, regardless of how we truncate the data set or add conditioning variables, we cannot find a result that is consistent with the resource curse. None of the specifications that look for the marginal effects of Fiscal Reliance at different levels of Per Capita Income (columns 4, 5, and 6) produce the predicted results either: poor countries that become increasingly resource dependent do not see a decline in Counterfactual Polity.

As we did for our regressions on Polity, we repeat the regressions on Counterfactual Polity substituting in Gross Oil Rents Per Capita. Because the latter series is not cointegrated with Counterfactual Polity we revert to the ARDL framework in first differences that we employed in our regressions on Polity. The results of these regressions are presented in Table 8, and are not materially different from the regression that used Fiscal Reliance as the independent variable. Some of the independent variables that capture short run effects (lagged Gross Oil Rents) are negative and significant, but they are matched by even larger positive and significant coefficients on other lagged terms. For example, in specification 1 the contemporaneous effect of a change in Gross Oil Rents on Counterfactual Polity is negative, as is the effect of a change in Gross Oil Rents on Counterfactual Polity with a four year lag. The effect of a change in Gross Oil Rents on Counterfactual Polity with a one year lag is, however, positive, significant, and of large magnitude relative to the other lags. The net effect is captured by the Long Run

Multiplier, which is positive. In fact, the LRM is positive in every specification, and is almost always significant at the 95 percent level.

Table 9 repeats this process, substituting Windfall Profits from Oil and Minerals Per Capita as the independent variable. If anything, these results cast even stronger doubt on the notion of a resource curse. Indeed, the regressions produce results that are consistent with an argument for a resource blessing: the Long Run Multipliers are always positive and significant; the independent variable lagged one period is always positive and significant, and it dwarfs any negative coefficients revealed in any other year.

V. Conclusion

We have developed new datasets to analyze the relationship between oil or mineral dependence and regime types within countries over the time. We observe countries prior to their becoming resource dependent, and evaluate whether increasing resource dependence over time affected their regime type – both relative to their level of democracy before resource dependence and relative to the democratization experiences of countries that were similar to them save for resource dependence. Our results indicate that natural resource dependence does not undermine democracy, preclude democratic transitions, or protract democratic transitions. Our evidence also does not support the view that there are conditional resource curse effects that are systematic—at least to the degree that these conditions are captured by differences in per capita incomes. This is not to say that there may not be cases in which natural resources contributed to the maintenance of an authoritarian regime in a particular time and place—indeed, it would be surprising if this *never* happened. It is to say, however, that the evidence does not

support generalizable, law-like statements about the impact of natural resources on regime types.

The implications of our analysis are clear. It is not the case that oil or mineral abundance causes countries to become autocratic. It is also not the case that oil or mineral abundance, in the presence low GDP, causes autocracy. Rather, autocracy, as well as economic reliance on oil or minerals, is often jointly determined by a country's fundamental institutions.

Our results have significant methodological implications. Researchers in comparative politics are intensely interested in explaining processes that occur within countries over time, such as the rise of the welfare state, transitions to democracy, and the onset of domestic conflict. In studying these processes, however, comparativists have tended to rely on datasets that have limited longitudinal coverage. They therefore employ pooled regression techniques, treating countries as homogenous units. Our results suggest that when the theory in question is not about static, cross-sectional differences between countries, but about changes that take place within countries over time, assembling and using long-run, time-series datasets designed to operationalize explicitly specified counterfactuals is a better match between theory and empirics than regressions centered on the cross-sectional analysis of longitudinally truncated data.

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Figure 1
Trinidad and Tobago, Resource Reliance and Polity, 1960-2006

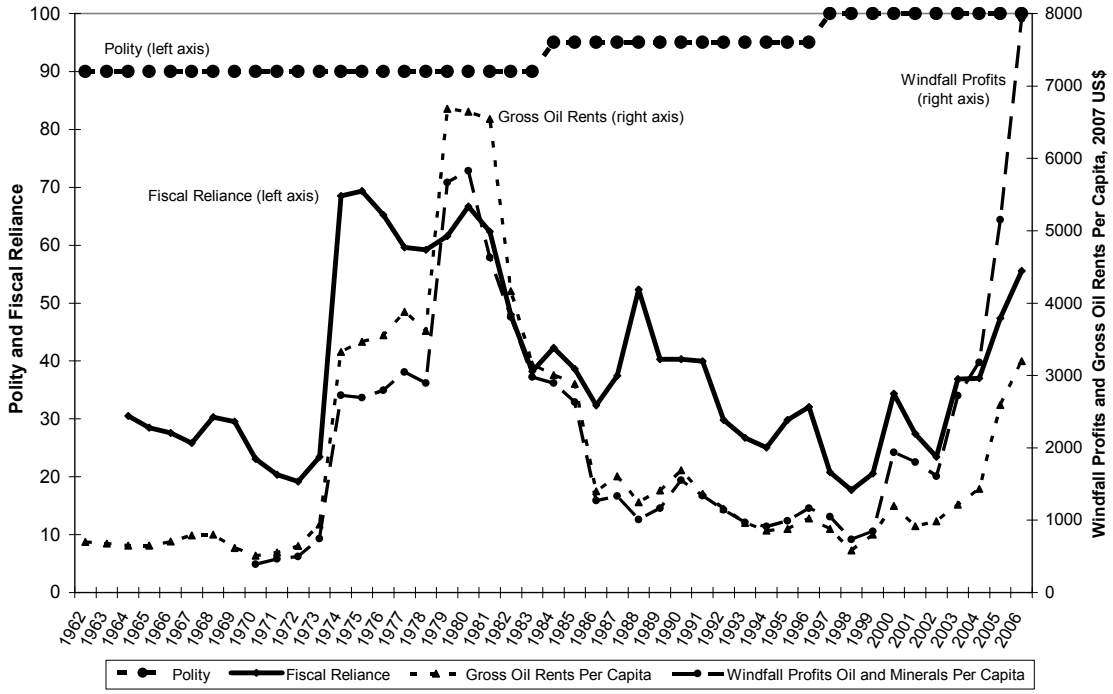

Figure 2
Nigeria, Resource Reliance and Polity, 1960-2006

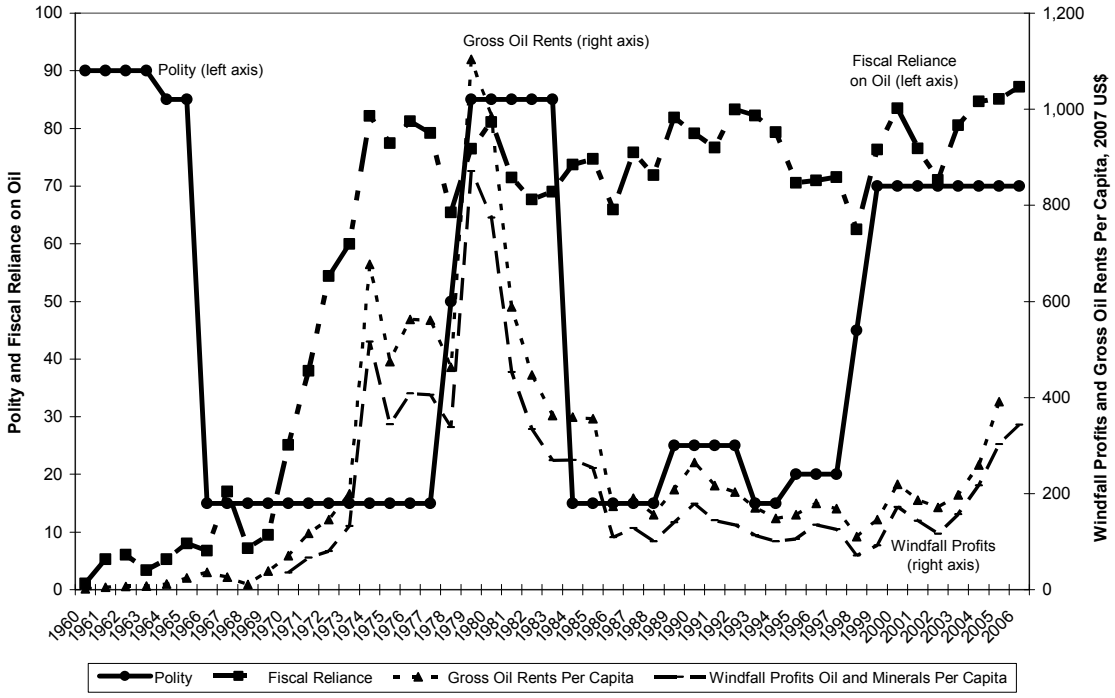

Figure 3
Mexico, Resource Reliance and Polity, 1822-2006

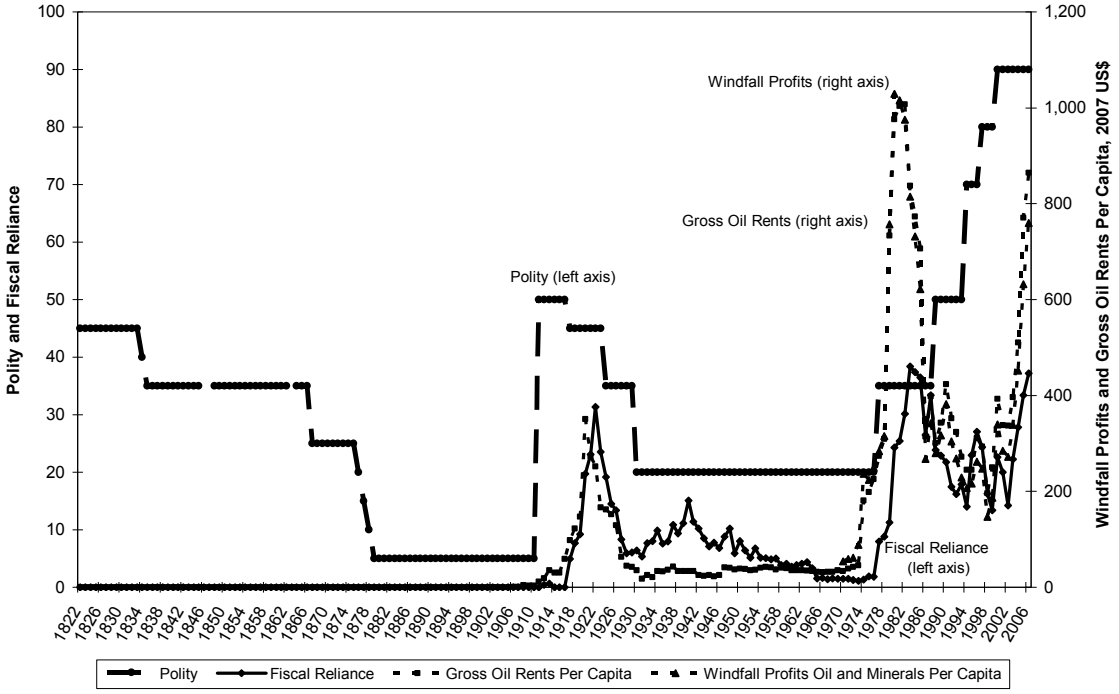

Figure 4
Angola, Resource Reliance and Polity, 1970-2006

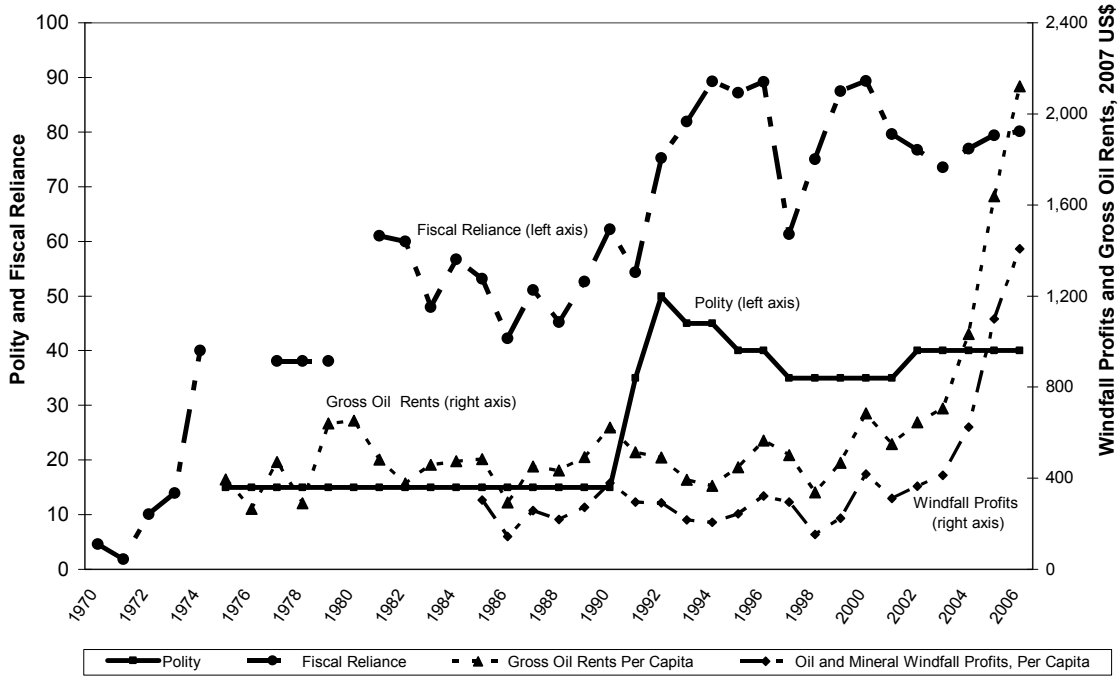

Figure 5
Indonesia, Resource Reliance and Polity, 1945-2006

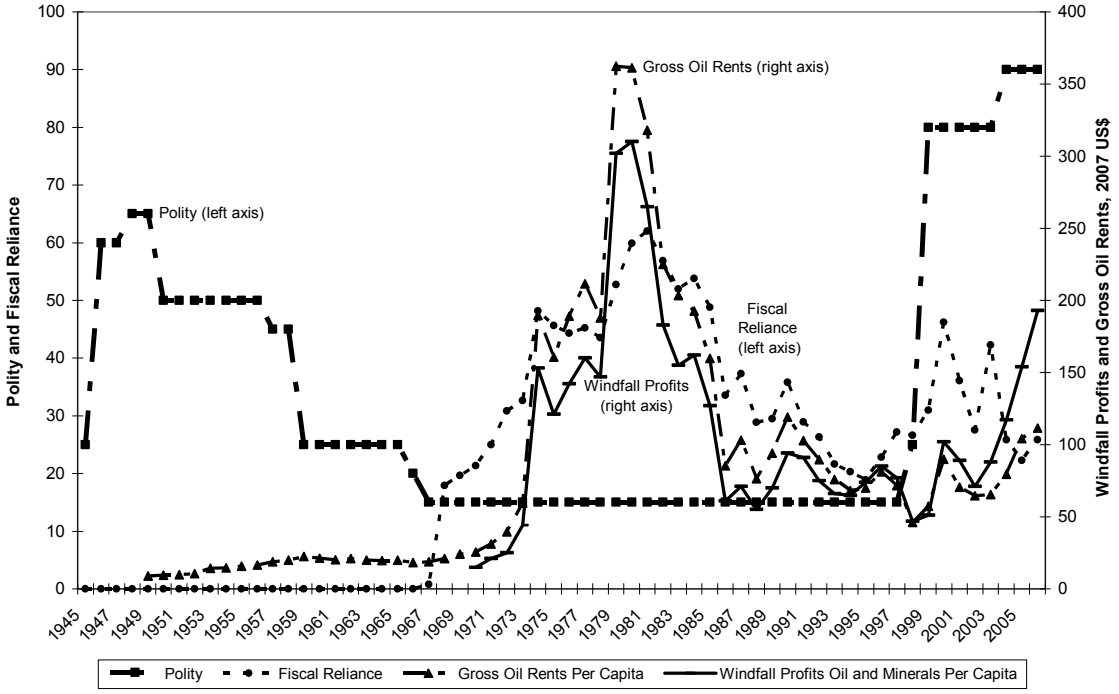

Table 1. Summary Statistics of the "within" variation for Dependent Variables and Co-variates included in Regression Models

Summary Statistics for Resource Dependence and Regime Type measures

Panel A

	1800-2006: Fiscal Reliance as independent variable			*1776-2006: Gross Oil Rents as independent variable*			*1970-2006: Windfall Profits as independent variable*		
	Fiscal Reliance	*Polity*	*C.F. Polity*	*Gross Oil Rents*	*Polity*	*C.F. Polity*	*Windfall Profits*	*Polity*	*C.F. Polity*
Mean	21	34	-6.664	$3,083	46	-1.763	$677	56	-0.673
Minimum	0	0	-60	0	0	-83.947	0	0	-80.25
Maximum	97.8	100	85	$79,093	100	95	$6,286	100	91.667
Standard Deviation	21.4	22.937	18.389	1675	24.475	18.742	2157	20.242	15.769
Resource measure panel statistics	**observations**	**# of countries**	**Mean of T**	**observations**	**# of countries**	**Mean of T**	**observations**	**# of countries**	**Mean of T**
	1654	17	97	14111	164	86	4695	153	31

Panel B

	ΔFiscal Reliance	*ΔPolity*	*ΔC.F. Polity*	*ΔGross Oil Rents*	*ΔPolity*	*ΔC.F. Polity*	*ΔWindfall Profits*	*ΔPolity*	*ΔC.F. Polity*
Mean	0.324	0.354	-0.073	$7.83	0.237	-0.136	$16	0.688	-0.088
Minimum	-37.68	-70	-70.623	-$22,822	-95	-89.515	-$18,623	-75	-77.5
Maximum	67.857	75	66.379	$46,942	80	76.666	$3,691	80	73.462
Standard Deviation	5.802	6.531	7.674	1000	7.505	7.433	1433	8.883	8.627
Resource measure panel statistics	**observations**	**# of countries**	**Mean of T**	**observations**	**# of countries**	**Mean of T**	**observations**	**# of countries**	**Mean of T**
	1610	17	95	13879	164	85	4529	152	30

Summary Statistics for Control Variables (observed over Gross Oil Rents panel)

Panel C

	Per Capita GDP	*Econ. Growth Rate*	*% Democ. World*	*% Democ. Region*
Mean	$5,981	2%	24%	25%
Minimum	$256	78%	0%	0%
Maximum	$37,304	-46%	49%	100%
Standard Deviation	3069	6.34	10	21

ΔPer Capita GDP	*ΔGrowth Rate*	*Δ% Dem. World*	*Δ% Dem. Region*
$1,329	0.111	0.3	0.4
-$3469	-75	-6	-67
$3,570	110	6	67
324	7.977	1.385	3.368

Fiscal Reliance is Fiscal Reliance on Resource Revenues; Polity is Polity 2 Score normalized to run from 0 to 100; C.F. Polity is Counterfactual Polity: Polity - Polity Score of non-resource producers' average Polity Score in the country's geographic-cultural region (see text for coding critera); Gross Oil Rents is Per Capita Gross Oil Rents (see text for construction); Windfall Profits is Per Capita Windfall Profits on Resources (see text for construction); Per Capita GDP is Real Per Capita GDP; Econ. Growth Rate is the yearly rate of growth of GDP Per Capita; % Democ. World is the percent of democracies in the world in year t; % Democ. Region is the percent of democracies in the region of the world of each country by year; Δ is the first-difference operator. Summary statistics for resource dependence measures only for country-years in which Polity Score values are not set to missing; summary statistics for Polity only for country-years in which the relevant resource dependence measure is not set to missing; summary statistics for control variables only for country-years in which Polity Scores are not set to missing and Gross Oil Rents are not set to missing.

Table 2. Maddala and Wu (1999) unit root and co-integration tests (Augmented Dickey Fuller Tests for unbalanced panel datasets)

Unit Root tests: null hypothesis is that the data follows a Random Walk without Drift

Panel A

	1800-2006: Fiscal Reliance as independent variable			*1776-2006: Gross Oil Rents as independent variable*			*1970-2006: Windfall Profits as independent variable*		
	Fiscal Reliance	*Polity*	*C.F. Polity*	*Gross Oil Rents*	*Polity*	*C.F. Polity*	*Windfall Profits*	*Polity*	*C.F. Polity*
Chi-square statistic	40.96	11.841	38.706	335.707	182.091	325.941	105.613	275.425	704.289
p-value	0.192	0.999	0.266	0.02**	1	0.459	1	0.862	0
# lags included	16	8	6	20	16	9	10	5	5
trend included?	YES	YES	NO	YES	YES	NO	YES	YES	NO
stationary?	**NO**	**NO**	**NO**	**YES**	**NO**	**NO**	**NO**	**NO**	**YES**

Panel B

	ΔFiscal Reliance	*ΔPolity*	*ΔC.F. Polity*	*ΔGross Oil Rents*	*ΔPolity*	*ΔC.F. Polity*	*ΔWindfall Profits*	*ΔPolity*	*ΔC.F. Polity*
Chi-square statistic	143.83	136.39	270.901		519.258	1505.264	345.437	467.064	
p-value	0***	0***	0***		0***	0***	0.041**	0	
# lags included	15	7	5		15	8	10	4	
trend included?	NO	YES	NO		YES	NO	YES	YES	
order of integration	***I*(1)**	***I*(1)**	***I*(1)**	***I*(0)**	***I*(1)**	***I*(1)**	***I*(0)**	***I*(1)**	***I*(0)**

Co-integration tests: unit root test on the residuals of a two-way fixed effects model

Panel C

	Polity and Fiscal Reliance	*C.F. Polity and Fiscal Reliance*	*Polity and Windfall Profits*
Chi-square statistic	17.01	47.239	310.422
p-value	0.993	0.065*	0.271
# lags included	15	6	10
trend included?	NO	NO	NO
Co-integration?	**NO**	**YES**	**NO**
Estimation strategy	**ΔARDL**	**ECM**	**ΔARDL**

***significant at the .01 level; **.05 level; *.10 level

Unit-root and co-integration tests are Maddala-Wu-Fisher versions of Dickey Fuller tests. This test combines the results of the country by country unit root tests, each with p-value Pi, and yields the following test-statistic: -2Σlog(Pi), distributed chi-square with 2N degrees of freedom (see Maddala and Wu 1999: 636). Time trend included in unit root tests when it was significant at conventional levels; no series found to follow a Random Walk with drift. Fiscal Reliance is Fiscal Reliance on Resource Revenues; Polity is Polity Score; C.F. Polity is Counterfactual Polity; Gross Oil Rents is Per Capita Gross Oil Rents; Windfall Profits is Per Capita Windfall Profits on Resources; Δ is first-difference operator; *I*(n) denotes that data series is integrated of order n; # of lags of the dependent variable included in the test are selected by the Akaike Information Criterion. The AIC choose lag length p to minimize log(SSR(p)/n)+(p+1)2/n, where SSR(p) is the sum of squared residuals for the Vector Autoregression Model (VAR) with *p* lags and *n* is the number of observations. ECM = Error Correction Model; ΔARDL = Autoregressive Distributed Lag model in first differences. Co-integration tests are augmented Dickery Fuller tests on the residuals from a regression of Regime Type against Resource Dependence measure. Because this test relies on no cross-country correlation between observations, we run each regression with year dummies *and* use Driscoll-Kraay standard errors, which are robust to contemporaneous correlation and serial correlation (see Driscoll and Kraay 1998). We also include country dummies to control for unobserved heterogeneity. For the co-integration tests, if we reject the Ho that the spead between both variables is non-stationary, this evidences that the data-series are co-integrated.

Table 3. Autoregressive Distributed Lag Panel Data Model in First Differences

Dependent Variable is ΔPolity (Polity 2 Score normalized to vary from 0 to 100)

	ARDL (1,15)	ARDL (1,6)	ARDL (1,6)	ARDL (1,6)	ARDL (1,6)	ARDL (1,6)
	1	***2***	***3***	***4***	***5***	***6***
	1800-2006	1950-2006	1950-2006	1950-2006	1950-2006	1973-2006
ΔPolity t-1	0.059	0.055	0.027	0.026	0.026	0.027
	[1.45]	[1.66]	[0.91]	[0.92]	[0.85]	[0.73]
# of Fiscal Reliance lags included	*15*	*6*	*6*	*6*	*6*	*6*
AIC statistic for # of lags chosen	6.603	7.719	7.664	7.668	7.824	8.039
F-test for all lags of Fiscal Reliance	**175.23**	**1.46**	**1.73**	**1.38**	**1.57**	**1.84**
p-value	**0**	**0.25**	**0.1711**	**0.278**	**0.216**	**0.147**
Long-run Multiplier: total change due	**0.354**	**0.349**	**0.33**	**0.328**	**0.331**	**0.337**
to a permanent change in Fiscal Rel.	**[1.38]**	**[1.55]**	**[1.86]***	**[1.82]***	**[1.64]**	**[1.53]**
ΔFiscal Reliance on	**0.002**	**0.009**	**0.02**	**0.019**	**0.04**	**0.066**
Natural Resources	**[0.06]**	**[0.36]**	**[0.68]**	**[0.48]**	**[0.96]**	**[1.20]**
ΔFiscal Reliance t-6	**0.157**	**0.16**	**0.147**	**0.147**	**0.152**	**0.15**
	[2.19]**	**[2.23]****	**[2.31]****	**[2.29]****	**[2.10]***	**[1.99]***
ΔFiscal Reliance t-15	**0.04**					
	[0.64]					
ΔFiscal Reliance X Δ Log(Per				**0.015**		
Capita GDP)				**[0.08]**		
ΔFiscal Reliance X Δ Log(Per					**1.202**	**1.089**
Capita GDP) t-6					**[1.22]**	**[1.18]**
Marginal effect of Fisc. Rel. at				**0.019**	**0.141**	**0.133**
25th Percentile of F.D. Log(PCGDP)				**[0.46]**	**[1.86]***	**[1.66]**
Marginal effect of Fisc. Rel. at				**0.018**	**0.128**	**0.167**
50th Percentile of F.D. Log(PCGDP)				**[0.44]**	**[1.58]**	**[2.28]****
Marginal effect of Fisc. Rel. at				**0.019**	**0.208**	**0.19**
75th Percentile of F.D. Log(PCGDP)				**[0.59]**	**[2.83]****	**[2.57]****
ΔLog(Per Capita GDP)			3.777	3.641	3.023	6.329
			[0.57]	[0.50]	[0.41]	[0.68]
ΔLog(Per Capita GDP) t-6					1.241	1.931
					[0.18]	[0.26]
Δ% Growth of GDP Per Capita			-0.013	-0.013	-0.016	-0.018
			[0.19]	[0.19]	[0.21]	[0.21]
Δ% Democracies in the Region			0.01	0.01	-0.007	-0.043
			[0.12]	[0.12]	[0.08]	[0.43]
Δ% Democracies in the World			-1.743	-1.742	0.173	0.525
			[0.98]	[0.98]	[0.48]	[0.99]
Democratic Transition in Region			25.336	25.34	25.208	29.608
Dummy			[3.22]***	[3.23]***	[3.36]***	[3.39]***
F-test for country dummies	54593.12	697.93	1002.05	11568.01	82000	50957.67
p-value	0	0	0	0	0	0
F-test for year dummies	5.83	4.88	7.57	8.6	28.57	8.62
p-value	0.0004	0.0014	0.0001	0	0	0
Arellano Bond AR(1) serial	0.64	0.07	0.1	0.11	0.09	0.62
correlation test	0.5239	0.9466	0.924	0.914	0.931	0.532
Observations	1226	577	567	517	567	431
R-squared	0.16	0.16	0.24	0.25	0.24	0.27

Robust t statistics clustered by country in brackets

included but not reported; year dummies included but not reported; # lags of dependent variable to include based on the minimization of AIC statistic; only 1 lag selected, which fully eliminates AR(1) and higher-order serial correlation; # lags of Fiscal Reliance to include calculated based on minimization of AIC statistic; all lags of Fiscal Reliance included but only relevant lags reported; Long-run Multiplier (LRM): (contemporaneous Fiscal Reliance + lags of Fiscal Reliance)/(1-coefficient on lagged dependent variable); standard error for LRM calculated via "Delta Method"; marginal effect of Fiscal Reliance conditioned by ΔLog(Per Capita GDP) in interaction models calculated at different percentiles of ΔLog(PCGDP); standard error for marginal effects calculated via "Delta Method."

Table 4. Autoregressive Distributed Lag Panel Data Model in First Differences

Dependent Variable is ΔPolity (Polity 2 Score normalized to vary from 0 to 100)

	ARDL (9,4)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)
	1	***2***	***3***	***4***	***5***	***6***
	1778-2006	1950-2006	1950-2006	1950-2006	1950-2006	1973-2006
ΔPolity t-1	0.012	-0.009	-0.039	-0.039	-0.039	-0.032
	[0.71]	[0.37]	[1.83]*	[1.83]*	[1.82]*	[1.68]*
# of Gross Oil Rents lags included	*4*	*5*	*5*	*5*	*5*	*5*
AIC statistic for # of lags chosen	6.861	7.27	7.092	7.092	7.093	7.152
F-test for all lags of Gross Oil Rents	**1.58**	**0.62**	**1.11**	**0.99**	**1.11**	**0.98**
p-value	**0.16**	**0.712**	**0.357**	**0.431**	**0.359**	**0.442**
Long-run Multiplier: total change due	**-0.02**	**0.175**	**0.139**	**0.152**	**0.139**	**0.138**
to a permanent change in Oil Rents	**[0.28]**	**[1.20]**	**[1.10]**	**[1.11]**	**[1.13]**	**[1.06]**
ΔPer Capita Gross Oil Rents	**-0.058**	**-0.042**	**-0.061**	**-0.054**	**-0.057**	**-0.042**
	[2.39]**	**[0.77]**	**[1.00]**	**[0.97]**	**[0.89]**	**[0.67]**
ΔPer Capita Gross Oil Rents	**0.013**	**0.084**	**0.104**	**0.101**	**0.1**	**0.103**
t-1	**[0.52]**	**[1.60]**	**[2.00]****	**[1.88]***	**[2.04]****	**[1.96]***
ΔPer Capita Gross Oil Rents	**0.001**	**-0.011**	**-0.036**	**-0.034**	**-0.04**	**-0.037**
t-4	**[0.07]**	**[0.17]**	**[0.58]**	**[0.55]**	**[0.63]**	**[0.59]**
ΔPer Capita Gross Oil Rents		**0.076**	**0.036**	**0.038**	**0.033**	**0.026**
t-5		**[0.66]**	**[0.34]**	**[0.34]**	**[0.30]**	**[0.24]**
ΔGross Oil Rents X Δ Log(Per				**-0.253**		
Capita GDP)				**[0.55]**		
ΔGross Oil Rents X Δ Log(Per					**0.115**	**0.028**
Capita GDP) t-1					**[0.22]**	**[0.05]**
Marginal effect of Oil Rents at				**-0.053**	**0.1**	**0.102**
25th Percentile of F.D. Log(PCGDP)				**[0.95]**	**[2.03]****	**[1.94]****
Marginal effect of Oil Rents at				**-0.06**	**0.102**	**0.103**
50th Percentile of F.D. Log(PCGDP)				**[1.00]**	**[2.01]****	**[1.95]****
Marginal effect of Oil Rents at				**-0.066**	**0.105**	**0.104**
75th Percentile of F.D. Log(PCGDP)				**[1.00]**	**[1.88]***	**[1.85]***
ΔLog(Per Capita GDP)			-0.36	-0.336	8.958	18.222
			[0.11]	[0.10]	[0.56]	[0.94]
ΔLog(Per Capita GDP) t-1					-9.723	-19.061
					[0.65]	[1.03]
Δ% Growth of GDP Per Capita			0.01	0.01	-0.084	-0.19
			[0.38]	[0.38]	[0.55]	[1.01]
Δ% Democracies in the Region			0.261	0.261	0.261	0.277
			[3.32]***	[3.32]***	[3.32]***	[2.97]***
Δ% Democracies in the World			-0.092	-0.092	-0.093	0.109
			[0.29]	[0.29]	[0.29]	[0.39]
Democratic Transition in Region			32.708	32.708	32.704	31.215
Dummy			[11.36]***	[11.36]***	[11.36]***	[9.78]***
F-test for country dummies	16000000	13000000	160000	130000	370000	68138
p-value	0	0	0	0	0	0
F-test for year dummies	15000000	2.64	2.58	2.58	2.59	2.21
p-value	0	0	0	0	0	0.001
Arellano Bond AR(1) serial	0.13	-0.1	0.22	0.22	0.23	-0.95
correlation test	0.894	0.92	0.828	0.828	0.821	0.341
Observations	12037	5435	5376	5376	5376	4109
R-squared	0.05	0.06	0.22	0.22	0.22	0.24

Robust t statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; country dummies included but not reported; year dummies included but not reported; # lags of dependent variable to include based on the minimization of AIC statistic; all lags of dependent variable included but only first lag reported; # lags of resource dependence to include calculated based on minimization of AIC statistic; all lags of resource dependence included but only relevant lags reported; Long-run Multiplier (LRM): (contemporaneous resource dependence + lags of resource dependence)/(1-coefficient on addition of lags of dependent variables); standard error for LRM calculated via "Delta Method"; marginal effect of resource dependence as conditioned by ΔLog(Per Capita GDP) in interaction models calculated at different percentiles of ΔLog(PCGDP); standard error for marginal effects calculated via "Delta Method."

Table 5. Autoregressive Distributed Lag Panel Data Model in First Differences

Dependent Variable is ΔPolity (Polity 2 Score normalized to vary from 0 to 100)

	ARDL (5,3)	ARDL (5,3)	ARDL (5,3)	ARDL (5,3)
	1	***2***	***3***	***4***
	1970-2006	1970-2006	1970-2006	1970-2006
ΔPolity t-1	0.008	-0.023	-0.023	-0.022
	[0.35]	[1.24]	[1.24]	[1.19]
# of Windfall Profits lags included	*3*	*3*	*3*	*3*
AIC statistic for # of lags chosen	7.312	7.159	7.159	7.159
F-test for all lags of Windfall Profits	**2.03**	**0.92**	**0.88**	**1.04**
p-value	**0.094**	**0.454**	**0.477**	**0.387**
Long-run Multiplier: total change due	**0.012**	**0.211**	**0.218**	**0.226**
to a permanent change in W.P.	**[0.19]**	**[1.36]**	**[1.35]**	**[1.46]**
ΔPer Capita Windfall Profits	**-0.054**	**0.002**	**0.007**	**0.006**
on Natural Resources	**[1.84]***	**[0.04]**	**[0.11]**	**[0.09]**
ΔPer Capita Windfall Profits	**0.004**	**0.043**	**0.049**	**0.051**
t-3	**[0.24]**	**[0.67]**	**[0.63]**	**[0.78]**
ΔWindfall Profits X Δ Log(Per			**-0.18**	
Capita GDP)			**[0.27]**	
ΔWindfall Profits X Δ Log(Per				**-0.236**
Capita GDP) t-1				**[0.31]**
Marginal effect of Windfall Profits at			**0.009**	**0.115**
25th Percentile of F.D. Log(PCGDP)			**[0.13]**	**[1.66]***
Marginal effect of Windfall Profits at			**0.003**	**0.109**
50th Percentile of F.D. Log(PCGDP)			**[0.06]**	**[1.50]**
Marginal effect of Windfall Profits at			**-0.001**	**0.122**
75th Percentile of F.D. Log(PCGDP)			**[0.01]**	**[1.33]**
ΔLog(Per Capita GDP)		-0.403	-0.395	22.84
		[0.09]	[0.09]	[0.88]
ΔLog(Per Capita GDP) t-1				-24.1
				[0.97]
Δ% Growth of GDP Per Capita		-0.009	-0.009	-0.241
		[0.29]	[0.29]	[0.97]
Δ% Democracies in the Region		0.264	0.264	0.264
		[2.68]***	[2.68]***	[2.68]***
Δ% Democracies in the World		0.144	0.143	0.145
		[0.50]	[0.50]	[0.51]
Democratic Transition in Region		31.499	31.45	31.489
Dummy		[9.02]***	[9.02]***	[9.02]***
F-test for country dummies	9162.72	26505.02	150000	29419.99
p-value	0	0	0	0
F-test for year dummies	1.95	1.92	1.92	1.91
p-value	0.004	0.006	0.006	0.006
Arellano Bond AR(1) serial	-1.03	-1.58	-1.58	-1.54
correlation test	0.303	0.113	0.113	0.125
Observations	3998	3822	3822	3822
R-squared	0.06	0.23	0.23	0.24

Robust t statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; country dummies included but not reported; year dummies included but not reported; # lags of dependent variable to include based on the minimization of AIC statistic; all lags of dependent variable included but only first lag reported; # lags of resource dependence to include calculated based on minimization of AIC statistic; all lags of resource dependence included but only relevant lags reported; Long-run Multiplier (LRM): (contemporaneous resource dependence + lags of resource dependence)/(1-coefficient on addition of lags of dependent variables); standard error for LRM calculated via "Delta Method"; marginal effect of resource dependence as conditioned by ΔLog(Per Capita GDP) in interaction models calculated at different percentiles of ΔLog(PCGDP); standard error for marginal effects calculated via "Delta Method."

Table 6. Determinants of Transition from Democracy to Autocracy and from Autocracy to Democracy
(Dynamic Conditional Fixed Effects Logit Transition Model, First-order Markov Chain)

Dependent Variable is Binary Measure of Democracy (coded 1 if regime is autocracy and 0 if regime is democracy)

	Model 1		*Model 2*	
Measure of Democracy used	***REGIME***		***Polity binary***	
regime transitioning *from*	Democracy	Autocracy	Democracy	Autocracy
regime transitioning *to*	Autocracy	Democracy	Autocracy	Democracy
Measure of Resources used	***Gross Oil Rents***		***Windfall Profits***	
	1950-2002		1970-2006	
Per Capita Resource Reliance	**-1.588**	**0.357**	**-1.332**	**-0.891**
t-1	**[1.42]**	**[0.43]**	**[0.86]**	**[0.43]**
Log(Per Capita GDP)	-0.243	0.746	-1.235	1.083
t-1	[0.36]	[1.16]	[1.05]	[0.77]
% Growth of GDP Per Capita	-0.081	-0.051	0.027	-0.036
t-1	[2.55]**	[1.91]*	[0.92]	[1.50]
% Democracies in the Region	-0.015	-0.005	-0.033	0.035
t-1	[0.86]	[0.26]	[1.38]	[1.55]
% Democracies in the World	-0.171	0.18	0.001	0.209
t-1	[1.75]*	[0.06]*	[0.02]	[3.12]***
Democratic Transition in Region	-0.482	1.767	-14.581	dropped
t-1	[0.43]	[2.50]***	[27.93]***	
Year dummies	Yes		Yes	
Pseudo R-squared	0.764		0.783	
Observations	2556		1681	

Robust z statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; year dummies included but not reported; REGIME is electoral measure of democracy as developed by Przeworski et al. (2000) and coded and expanded by Cheibub and Ghandi (2004) and Boix and Rosato (2001); Polity binary is coded as "1" if the Polity Score is less than 6 on the -10 to +10 Combined Polity 2 scale and coded "0" if it is 7 or more. Polity-based measure of democracy used in Model 4 instead of REGIME because there is not enough within-country variance for Per Capita Windfall Profits; between 1970 and 2002 most countries exhibit a monotonic trend and, therefore, convergence fails to occur when REGIME is the dependent. variable.

Table 7. Autoregressive Distributed Lag Panel Data Model, Error Correction Framework

Dependent Variable is ΔCounterfactual Polity (see text for construction)

	ARDL (3,6) *1* 1800-2006	ARDL (3,6) *2* 1950-2006	ARDL (3,6) *3* 1950-2006	ARDL (3,6) *4* 1950-2006	ARDL (3,6) *5* 1950-2006	ARDL (3,6) *6* 1973-2006
Long-run Effects						
Fiscal Reliance t-1	0.011	0.045	0.026	0.053	0.075	0.007
	[0.51]	[1.20]	[0.58]	[1.00]	[1.37]	[0.08]
Long-run Multiplier: total effect due to	0.195	0.314	0.184	0.379	0.475	0.042
a permanent change in Fiscal Reliance	[0.49]	[1.21]	[0.61]	[1.07]	[1.43]	[0.08]
Fiscal Reliance t-1 X Log(Per				-0.023	-0.025	0.008
Capita GDP t-1)				[1.18]	[1.21]	[0.27]
Marginal effect of Fisc. Rel. at				0.035	0.055	0.015
25th Percentile of Log(PCGDP)				[0.78]	[1.19]	[0.23]
Marginal effect of Fisc. Rel. at				0.15	0.034	0.022
50th Percentile of Log(PCGDP)				[0.37]	[0.80]	[0.38]
Marginal effect of Fisc. Rel. at				0.001	0.019	0.027
75th Percentile of Log(PCGDP)				[0.02]	[0.43]	[0.48]
Log(Per Capita GDP) t-1			3.091	4.395	5.991	5.287
			[1.61]	[2.11]*	[2.61]**	[1.42]
% Growth of GDP Per Capita			-0.106	-0.144	-0.071	-0.531
t-1			[0.38]	[0.40]	[0.17]	[0.71]
% Democracies in the Region			0.028	0.032	0.018	0.013
t-1			[0.68]	[0.78]	[0.34]	[0.19]
% Democracies in the World			-0.075	-0.101	-0.08	0.022
t-1			[0.33]	[0.43]	[0.29]	[0.10]
Short-run Effects						
Counterfactual Polity t-1	-0.057	-0.142	-0.14	-0.139	-0.158	-0.169
(Error Correction)	[2.49]**	[4.28]***	[4.14]***	[4.05]***	[4.47]***	[3.85]***
ΔFiscal Reliance on	0.023	0.017	0.048	0.048	0.076	0.095
Natural Resources	[0.66]	[0.34]	[0.99]	[0.86]	[1.34]	[1.73]
Number of Fisc. Rel. lags included	6	6	6	6	6	6
AIC statistic for # of lags chosen	6.895	7.827	7.738	7.744	7.892	8.103
F-test for all lags of Fisc. Rel.	2.46	2.13	1.84	1.76	1.21	1.38
p-value	0.071	0.1	0.148	0.165	0.351	0.279
ΔFiscal Reliance t-6	0.127	0.119	0.133	0.138	0.126	0.128
	[1.73]	[1.62]	[2.49]**	[2.57]**	[2.12]*	[1.89]*
ΔFiscal Reliance X Δ Log(Per				-0.023		
Capita GDP)				[0.08]		
ΔFiscal Reliance X Δ Log(Per					0.768	0.712
Capita GDP) t-6					[0.81]	[0.73]
ΔLog(Per Capita GDP)			8.967	12.483	6.26	51.739
			[0.30]	[0.36]	[0.15]	[0.82]
ΔLog(Per Capita GDP) t-6					-3.147	-3.036
					[0.36]	[0.35]
Δ% Growth of GDP Per Capita			-0.077	-0.114	-0.041	-0.473
			[0.27]	[0.32]	[0.10]	[0.67]
Δ% Democracies in the Region			-0.565	-0.566	-0.567	-0.635
			[6.91]***	[7.08]***	[7.13]***	[5.37]***
Δ% Democracies in the World			-2.334	-2.397	-2.604	-0.542
			[1.08]	[1.14]	[1.20]	[0.98]
Democratic Transition in Region			27.372	27.322	26.8	31.528
Dummy			[4.08]***	[4.08]***	[4.00]***	[4.18]***
F-test for country dummies	4132.97	229.85	229.85	327.35	54.42	3288.76
p-value	0	0	0	0	0	0
F-test for year dummies	50.11	20.59	20.59	35.5	5.82	100.68
p-value	0	0	0	0	0.001	0
Arellano Bond AR(1) serial	0.02	0.07	-0.03	-0.07	0.01	0.1
correlation test	0.983	0.945	0.973	1.07	0.989	0.923
Observations	1411	577	567	567	517	431
R-squared	0.23	0.23	0.34	0.34	0.35	0.37

Robust t statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; country dummies included but not reported; year dummies included, not reported; # lags of dependent variable to include calculated based on the minimization of AIC statistic; all lags of D.V. included but not reported; # lags of Fiscal Reliance to include calculated based on minimization of AIC statistic; all lags of Fiscal Reliance included but only relevant lags reported; Long-run Multiplier (LRM): (coefficient of Fiscal Reliance t-1)/(1-coefficient on LDV); standard error for LRM calculated via "Delta Method"; marginal effect of Fiscal Reliance as conditioned by Log(Per Capita GDP) in interaction models calculated at different percentiles of Log(PCGDP) standard error for marginal effects calculated via "Delta Method."

Table 8. Autoregressive Distributed Lag Panel Data Model in First Differences

Dependent Variable is ΔCounterfactual Polity (see text for construction)

	ARDL (9,4)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)	ARDL (7,5)
	1	***2***	***3***	***4***	***5***	***6***
	1778-2006	1950-2006	1950-2006	1950-2006	1950-2006	1973-2006
ΔCounterfactual Polity t-1	-0.005	-0.026	-0.048	-0.048	-0.048	-0.043
	[0.29]	[1.13]	[2.35]**	[2.35]**	[2.34]**	[2.28]**
# of Gross Oil Rents included	*4*	*5*	*5*	*5*	*5*	*5*
AIC statistic for # of lags chosen	6.841	7.224	7.07	7.07	7.07	7.121
F-test for all lags of Oil Rents	**9.42**	**8.15**	**7.97**	**7.52**	**8.01**	**7.65**
p-value	**0**	**0**	**0**	**0**	**0**	**0**
Long-run Multiplier: total change due	**0.137**	**0.358**	**0.301**	**0.306**	**0.342**	**0.316**
to a permanent change in Oil Rents	**[1.66]***	**[2.44]****	**[2.11]****	**[2.11]****	**[2.38]****	**[2.17]****
ΔPer Capita Gross Oil Rents	**-0.15**	**-0.125**	**-0.169**	**-0.166**	**-0.177**	**-0.172**
	[4.36]***	**[1.58]**	**[1.90]***	**[2.14]****	**[2.05]****	**[2.01]****
ΔPer Capita Gross Oil Rents	**0.329**	**0.559**	**0.516**	**0.515**	**0.541**	**0.535**
t-1	**[4.12]*****	**[5.79]*****	**[5.47]*****	**[5.29]*****	**[5.66]*****	**[5.53]*****
ΔPer Capita Gross Oil Rents	**-0.133**	**-0.154**	**-0.147**	**-0.147**	**-0.115**	**-0.108**
t-4	**[3.45]*****	**[1.90]***	**[1.85]***	**[1.86]***	**[1.34]**	**[1.26]**
ΔPer Capita Gross Oil Rents		**0.063**	**0.059**	**0.06**	**0.067**	**0.064**
t-5		**[0.54]**	**[0.53]**	**[0.53]**	**[0.59]**	**[0.55]**
ΔGross Oil Rents X Δ Log(Per				**-0.1**		
Capita GDP)				**[0.12]**		
ΔGross Oil Rents X Δ Log(Per					**-0.903**	**-0.735**
Capita GDP) t-1					**[1.50]**	**[1.18]**
Marginal effect of Oil Rents at				**-0.166**	**0.547**	**0.542**
25th Percentile of F.D. Log(PCGDP)				**[2.18]****	**[5.69]*****	**[5.57]*****
Marginal effect of Oil Rents at				**-0.168**	**0.521**	**0.52**
50th Percentile of F.D. Log(PCGDP)				**[1.95]****	**[5.50]*****	**[5.38]*****
Marginal effect of Oil Rents at				**-0.171**	**0.499**	**0.503**
75th Percentile of F.D. Log(PCGDP)				**[1.72]***	**[5.20]*****	**[5.09]*****
ΔLog(Per Capita GDP)			1.276	1.286	13.199	19.7
			[0.41]	[0.41]	[0.89]	[1.14]
ΔLog(Per Capita GDP) t-1					-12.309	-17.981
					[0.90]	[1.11]
Δ% Growth of GDP Per Capita			0.013	0.013	-0.106	-0.171
			[0.56]	[0.56]	[0.77]	[1.04]
Δ% Democracies in the Region			-0.235	-0.235	-0.235	-0.212
			[3.41]***	[3.41]***	[3.41]***	[3.01]***
Δ% Democracies in the World			-0.554	-0.554	-0.556	-0.318
			[1.78]*	[1.78]*	[1.79]*	[1.17]
Democratic Transition in Region			32.002	32.002	31.997	30.432
Dummy			[12.56]***	[12.56]***	[12.56]***	[10.91]***
F-test for country dummies	16000000	90139.58	120000	120000	290000	230000
p-value	0	0	0	0	0	0
F-test for year dummies	15000000	2.15	1.46	1.46	1.46	0.93
p-value	0	0	0.04	0.04	0.04	0.577
Arellano Bond AR(1) serial	-0.17	-0.26	-0.17	-0.17	-0.15	-1.31
correlation test	0.862	0.793	0.868	0.867	0.88	0.189
Observations	11973	5435	5376	5376	5376	4109
R-squared	0.04	0.05	0.19	0.19	0.19	0.2

Robust t statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; country dummies included but not reported; year dummies included but not reported; # lags of dependent variable to include based on the minimization of AIC statistic; all lags of dependent variable included but only first lag reported; # lags of resource dependence to include calculated based on minimization of AIC statistic; all lags of resource dependence included but only relevant lags reported; Long-run Multiplier (LRM): (contemporaneous resource dependence + lags of resource dependence)/(1-coefficient on addition of lags of dependent variables); standard error for LRM calculated via "Delta Method"; marginal effect of resource dependence as conditioned by ΔLog(Per Capita GDP) in interaction models calculated at different percentiles of ΔLog(PCGDP); standard error for marginal effects calculated via "Delta Method."

Table 9. Autoregressive Distributed Lag Panel Data Model in First Differences

Dependent Variable is ΔCounterfactual Polity (see text for construction)

	ARDL (9,3)	ARDL (9,3)	ARDL (9,3)	ARDL (9,3)
	1	***2***	***3***	***4***
	1970-2006	1970-2006	1970-2006	1970-2006
ΔCounterfactual Polity t-1	-0.018	-0.043	-0.043	-0.042
	[0.74]	[2.22]**	[2.22]**	[2.19]**
# of Windfall Profits lags included	*3*	*3*	*3*	*3*
AIC statistic for # of lags chosen	**7.246**	**7.132**	**7.132**	**7.132**
F-test for all lags of Windfall Profits	**12.15**	**10.54**	**10.76**	**10.4**
p-value	**0**	**0**	**0**	**0**
Long-run Multiplier: total change due	**0.433**	**0.541**	**0.526**	**0.55**
to a permanent change in W.P.	**[4.18]*** **	**[3.52]*** **	**[3.15]*** **	**[3.52]*** **
ΔPer Capita Windfall Profits	**-0.09**	**-0.094**	**-0.105**	**-0.105**
on natural resources	**[2.44]** **	**[1.00]**	**[1.27]**	**[1.14]**
ΔPer Capita Windfall Profits	**0.449**	**0.659**	**0.664**	**0.681**
t-1	**[4.19]*** **	**[6.23]*** **	**[6.11]*** **	**[6.30]*** **
ΔPer Capita Windfall Profits	**0.087**	**0.011**	**0.009**	**0.001**
t-2	**[1.69]***	**[0.15]**	**[0.13]**	**[0.01]**
ΔPer Capita Windfall Profits	**0.158**	**0.183**	**0.168**	**0.194**
t-3	**[4.83]*** **	**[1.46]**	**[1.06]**	**[1.55]**
ΔWindfall Profits X Δ Log(Per			**0.412**	
Capita GDP)			**[0.39]**	
ΔWindfall Profits X Δ Log(Per				**-1.086**
Capita GDP) t-1				**[1.32]**
Marginal effect of Oil Rents at			**-0.108**	**0.689**
25th Percentile of F.D. Log(PCGDP)			**[1.35]**	**[6.32]*** **
Marginal effect of Oil Rents at			**-0.096**	**0.658**
50th Percentile of F.D. Log(PCGDP)			**[1.05]**	**[6.12]*** **
Marginal effect of Oil Rents at			**-0.086**	**0.633**
75th Percentile of F.D. Log(PCGDP)			**[0.81]**	**[5.73]*** **
ΔLog(Per Capita GDP)		2.089	2.068	24.246
		[0.47]	[0.47]	[1.02]
ΔLog(Per Capita GDP) t-1				-22.881
				[1.03]
Δ% Growth of GDP Per Capita		-0.002	0	-0.221
		[0.01]	[0.01]	[0.99]
Δ% Democracies in the Region		-0.192	-0.192	-0.191
		[2.56]**	[2.56]**	[2.56]**
Δ% Democracies in the World		-0.209	-0.207	-0.211
		[0.75]	[0.74]	[0.76]
Democratic Transition in Region		30.19	30.189	30.182
Dummy		[9.60]***	[9.60]***	[9.61]***
F-test for country dummies	130000	5650.75	4062.92	24199.29
p-value	0	0	0	0
F-test for year dummies	0.8	0.98	0.98	0.98
p-value	0.772	0.507	0.507	0.504
Arellano Bond AR(1) serial	0.32	-0.6	-0.61	-0.57
correlation test	0.749	0.546	0.544	0.569
Observations	3865	3702	3702	3702
R-squared	0.07	0.2	0.21	0.2

Robust t statistics clustered by country in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%; intercept included but not reported; country dummies included but not reported; year dummies included but not reported; # lags of dependent variable to include based on the minimization of AIC statistic; all lags of dependent variable included but only first lag reported; # lags of resource dependence to include calculated based on minimization of AIC statistic; all lags of resource dependence included but only relevant lags reported; Long-run Multiplier (LRM): (contemporaneous resource dependence + lags of resource dependence)/(1-coefficient on addition of lags of dependent variables); standard error for LRM calculated via "Delta Method"; marginal effect of resource dependence as conditioned by ΔLog(Per Capita GDP) in interaction models calculated at different percentiles of ΔLog(PCGDP); standard error for marginal effects calculated via "Delta Method."