Do Natural Resources Fuel Authoritarianism?

A Reappraisal of the Resource Curse

Stephen Haber and Victor Menaldo

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Abstract: A large body of scholarship finds that there is a relationship between economic dependence on oil or minerals and authoritarianism. This finding is based, however, on pooled, time-series cross-sectional regressions without country fixed effects run on datasets that are longitudinally truncated. This is not an effective strategy to uncover causal associations. We therefore develop unique historical datasets, and employ time-series centric techniques, that allow us test for long-run relationships between resource reliance and regime type within countries over time. Our results indicate that increases in resource dependence are not associated with the undermining of democracy or less complete transitions from authoritarianism to democracy. We 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 properly using historical datasets that operationalize explicitly specified counterfactuals provides a better fit between theory and evidence.

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Introduction

Are countries with abundant oil or minerals cursed? Parallel literatures in political science and economics suggest so: economic dependence on oil or minerals is positively correlated with the onset of civil war (e.g., Fearon and Laitin 2003); slow economic growth (e.g. Sachs and Warner 1995), and authoritarian government (e.g. Ross 2001a). These ideas have had an impact well beyond the academy. The resource curse is taken as a self-evident truth at multilateral aid organizations (e.g., Harford and Klein 2005), presented as a robust fact in popular books on world poverty (e.g., Collier 2007), and is disseminated widely in the news media. Indeed, New York Times columnist Thomas Friedman (2006) has gone so far as to decree a "first law of petro-politics": the price of oil and the spread of political freedom are inversely correlated. The normative implications are non-trivial. Some researchers have suggested that developing countries might consider leaving their resources in the ground, in order to avoid their pernicious effects (e.g. Ross 2001b).

We question the theory, methods, and evidence supporting the contention that resource dependence is causally associated with authoritarianism. Our basic point is that the resource curse is about a historical 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 should therefore be evaluated using time-series centric data and methods. The extant literature, however, relies on pooled, time-series cross-sectional regressions without country fixed effects on data that is longitudinally truncated. The implicit theory underlying these regressions—even those that control for possible endogeneities with instrumental variables— is that authoritarian, resource dependent countries were on a path of political development that would have culminated in democracy. Nigeria could have followed the same trajectory as Denmark, had it not discovered oil.[1] Researchers are, in short, drawing inferences

[1] Merely adding regional dummies, per capita GDP, or the percent of the population that is Muslim, to mitigate unobserved heterogeneity, does not solve the fundamental problem: the implicit counterfactual to Nigeria is now a fictional Denmark with a lower GDP per capita and more Muslims located in Africa.

about processes that are purported to happen within countries over time from techniques that are primarily driven by the variation between countries.

We therefore go beyond the extant literature in three ways. First, we build historical datasets that allow us to observe time series variation within countries over time, typically going back to the period before a country became a major oil or mineral producer. Second, we employ econometric techniques centered on the dynamic modeling of time-series processes. Third, in order to mitigate possible measurement error, a salient concern when relegating attention to within country variation, we estimate our regressions using three different measures of resource dependence: fiscal reliance on oil or minerals; windfall profits from oil and minerals per capita; and total oil income per capita. We also run our regressions using three different measures of regime type: the polity score; a binary measure of democracy/autocracy; and a synthetic measure that estimates what a country's polity score would have been had it not developed its resource sector. This last variable allows us to draw explicit counterfactual comparisons.

The body of evidence that we develop and the set of methods that we employ allow us to control for confounding factors, and model relationships between variables, in ways that have not yet been accomplished in the literature. First, our data and methods allow us to determine if there are long-run relationships between resource reliance and 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 persistent institutional differences between countries. 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.

Regardless of how we measure the dependent and independent variable, or specify the regressions, we cannot detect a resource curse. We find no evidence at all that oil or mineral booms undermine democracies. Indeed, the evidence indicates the opposite: democracies are made more resilient by increases in their oil and mineral reliance. We also find that there are countries, such as Mexico, Chile, and Ecuador, which have transitioned from authoritarianism to democracy in the midst of oil or mineral booms. Even more striking, the actual histories of the countries typically cited as examples of the resource curse—such as Venezuela, Iran, or Turkmenistan – contradict the claim that their contemporary political institutions are caused by oil or mineral dependence. Venezuela, for example, may be governed by the self-styled revolutionary Hugo Chávez today, but it was governed by a series of dictators before oil was discovered in 1917, and then transitioned to democracy at the pinnacle of the growth of its oil industry—a fact that was pointed out by Karl (1997). Iran was authoritarian for more than a century before oil was discovered. Turkmenistan was born as an autocratic petro-state when the Soviet Union collapsed. But none of the other so-called "Stans" (Uzbekistan, Tajikistan, Afghanistan, Kazakhstan, Kyrgyzstan, Pakistan) are democratic either, even though only one of them, Kazakhstan, is a major oil or mineral producer. Much the same can be said about the history of other "resource cursed" countries. In short, when the most appropriate – historical – evidence is brought to bear, the tenet that oil and mineral reliance fuels authoritarianism fails.

The remainder of this paper proceeds as follows. Section Two reviews the literature on the political resource curse and discusses its methodological and theoretical flaws. Section Three presents a discussion of the historical datasets we develop and our research strategy. Section Four presents our results, depicting some of our findings graphically, providing diagnostics on whether the data series exhibit a long-run relationship, and conducting dynamic, panel data analyses. Section Five concludes, discussing the implications of our historical approach for a wider set of questions in comparative politics.

II. Review of the Literature, Methods and Theory

The idea that there is a causal connection between resource reliance and autocracy goes back to Mahdavy (1970: 466-67), who noted that petroleum revenues in Middle Eastern countries constituted an external source of rents 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 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 that are inconsistent with democracy soon became a central theme in the study of the Middle East and Africa (e.g., Hodges 2001; Vandewalle 1998). Huntington (1991: 65), generalizing from this literature, proclaimed a rentier effect across the 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 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 are extracted or exported.

Researchers therefore began to subject the hypothesis that oil and mineral reliance 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 2006); used instrumental variables to address reverse causality (Ramsey 2007); exploited variance at the sub-national level (Goldberg, Wibbels, and Myukiyehe 2008); and explored the effects of oil 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, this large-N literature finds an association between resources and authoritarianism.

The adoption of large-N approaches was not, however, a panacea. In the first place, Luciani's (1987) claim, "no representation without taxation," remained *the* core assumption of the theory that guided empirical work.[2] As Ross (1999: 313) points out, however, it is unclear why a ruler who seeks untrammeled power would be revenue maximizing when it comes to resource taxation, but revenue-satisfycing when it comes to the taxation of income, property, and consumption. The implication is that such a ruler must not be predatory, but complacent. If the ruler is complacent, however, then why would citizens not seek greater representation to commandeer more of the resource taxes for themselves? For that matter, why would they not seek greater representation in order to address non-taxation grievances?

In point of fact, the idea that there is "no representation without taxation" is at variance both with recent work in public choice economics and with the actual history of the spread of democracy: A ruler trading taxation for representation is only one of *several* paths by which representative governments arise, and these paths include resource reliant as well as non-resource reliant cases. One path is when rivalry between enfranchised and disenfranchised groups induces democratization from below (Conley and Temimi 2001), as occurred in oil-rich Mexico, for example. Another 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 when public goods become more highly valued than pork (Lizzeri and Persico 2004), as occurred in oil and gas rich Trinidad and Tobago. In short, as Herbst (2000) has argued, the idea that democracy develops as a result of rulers trading representation for taxation may only be true about Western Europe.

In the second place, the techniques that were employed in the large-N literature—which primarily exploit variance across countries, rather than within countries over time—introduced a mismatch between

[2] Researchers initially suggested two other transmission channels: oil and minerals were claimed to generate autocracy by allowing dictators to finance a repressive state apparatus; and were claimed to retard modernization, and hence prevent the so-called prerequisites of democracy from emerging. Recent work (e.g. Ross 2008, Ramsey 2008) affords these other channels little importance.

the theory and empirical strategies used to test it. The hypothesis that resource wealth fuels authoritarianism implies that the discovery of oil or minerals sent countries down an alternative path of institutional development. Specifically, it implies three possible counterfactuals: 1) Autocracy X would have become democratic, had it not found oil or minerals; 2) Democracy Y would have remained democratic, instead of lapsing into autocracy, had it not found oil or minerals; and 3) Democracy Z would have made the transition from autocracy faster, had it not found oil or minerals. The methods employed in the literature do not, however, allow researchers to focus on these counterfactuals. Rather, they pool countries together, treating them as identical units, and estimate the variance across them. The problem is that country-years are not isomorphic: Switzerland is not Angola without oil; endemic, 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. Herb (2005) reasons that resource dependent countries would have been substantially poorer had they not found oil or minerals. He therefore calculates the income gain contributed by countries' resource sectors, and then estimates their level of democracy at these lower, counterfactual levels of GDP. His results indicate that the net negative effect of oil and minerals on democracy is nil. Dunning (2008) takes another important step: he introduces country fixed effects in an analysis where polity scores are regressed against per capita windfall profits from oil in a panel of 17 Latin American countries between 1960 and 2001, and finds a positive relationship between the level of resource reliance and level of democracy.[3] Finally, Ross (2001:

[3] Dunning interprets this result as support for his theory about a conditional resource curse: oil is associated with authoritarianism, except in societies with highly skewed distributions of wealth. In those inegalitarian societies, oil reliance promotes democratization, because elites can distribute oil rents among the masses and not fear demands for the redistribution of their own wealth and therefore lose their incentive to resist democracy. There are several reasons why we do not find Dunning's argument compelling, however. The first is that there is reason to believe that the results of his country fixed

341,fn 58), tests the resource curse hypothesis on a global panel of countries between 1972 and 1997, and finds that the statistical significance of the negative impact of oil and mineral exports as a share of GDP on democracy is high when the data are pooled, but that the coefficients lose significance if country fixed effects are introduced. The question is: why?

The answer is that the failure to control for countries' fundamental, time-invariant institutions may allow for the cross-sectional results to be confounded. As Norman (forthcoming) argues, resources, whether measured as stocks or flows, may themselves be endogenous to a country's underlying legal,

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effects regressions on the Latin American panel may be spurious. When focusing on the data's longitudinal variation, it is crucial for the series to be either stationary or, if non-stationary, co-integrated (see Granger and Newbold 1974; Phillips 1986). In Table 2, we find that windfall profits from resources and Polity are not stationary and not co-integrated for a global panel between 1970 and 2006. Moreover, when estimating so-called Bounds Co-integration tests (see Pesaran et al. 2001) across individual country time-series for Chile, Ecuador, Mexico and Venezuela, going back to these countries' independence, we fail to find evidence that there is a relationship in levels between these variables (results not reported, but available upon request). The second is that Dunning also runs regressions on a global panel that includes an interaction between oil rents and income distribution; in this set of estimations he does *not* control for country fixed effects. As we have already argued above, these results may be driven by omitted variable bias. The third is that Dunning's theory has a clear and easily testable implication that is not empirically supported: we should expect countries that are oil rich, and that experience increasing inequality over time, to democratize. The oil rich countries of the former Soviet Bloc, particularly Russia itself, are an ideal natural laboratory to test this hypothesis. Thus far, however, the experiences of formerly communist, oil rich nations that have experienced the greatest increases in inequality during the transition to a market economy have not borne out this prediction. Russia's creeping authoritarianism is the quintessential example.

political, and cultural institutions.[4] David and Wright (1997) make this point in explaining why the United States, which does not have a particular favorable geologic endowment, became a leading resource producer. McSherry (2006) and Smith (2007) suggest something similar when they state that the blame for bad policy choices and autocracy may rest with a country's pre-oil institutions, not the oil discoveries themselves. Engelbert (2000) gets to the heart of the matter in his comparison of democratic and prosperous Botswana and autocratic, impoverished Congo: reliance on resources cannot explain the differences between them because both are mineral rich; their colonial institutions instead explain the differences in development outcomes.

The genealogy of the theory that oil or mineral wealth fuels authoritarianism appears, in short, to be an example of what Tversky and Kahneman (1973) call the availability heuristic—where a prediction about the frequency of an event is based on how easily a prototypical example can be brought to mind. The more vivid the example, the more certain is the prediction. The first generation of scholars who worked on the resource curse were Middle East specialists, a region of the world suffused with autocratic countries, on the one hand, and vast oil resources, on the other. Against this background, they understandably were led to impute oil as the cause of autocracy: it was an obvious characteristic shared by many of the cases they studied. They therefore inductively formulated a theory based on what they perceived were prototypical examples: Saudi Arabia and other Persian Gulf monarchies. Although a later generation of scholars sought to move beyond case studies via large-N techniques, they accepted the theory uncritically, taking it as a premise of their analyses. As a result, even though the sequence of cause

[4] The level of resource production, resource exports, or resource rents can be driven by *political* decisions: governments that have inherited inveterately weak institutions may have short time horizons and pressing fiscal needs. Rulers may therefore *choose* to exploit resources today, rather than save them for tomorrow. A similar logic affects a country's population, the denominator usually used to compute resource reliance: 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 GDP (see Culter et al. 2006, Soares 2007).

and effect is critically important in making a case for the hypothesis that oil or mineral dependence either undermines extant democracies, or prevents and protracts transitions to democracy, they did not formulate appropriate counterfactuals. Nigeria, for example, was not compared to itself before it became an oil producer, nor was the trajectory of its political institutions compared to that of other African countries whose economies were not dependent on petroleum or minerals. Rather, Nigeria was pooled with every other country in the world, including such improbable counterfactual cases as Belgium, Canada, and Japan. The results of these pooled regressions seemingly confirmed the theory; correlations were therefore deemed sufficient to infer a causal connection between natural resources and autocracy.

III. Research Design

The key to any meaningful analysis of the resource curse is to specify the right counterfactual: what would a resource dependent country have looked like had it not discovered oil or minerals? In order to operationalize this approach 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 fitful; oil and mineral sectors go through booms and busts; and governments modify the output of oil and minerals, as well as adjust the tax rates they impose on those resources. In other words, history –not scatter plots – provides the best way to tell the story. We therefore build historical datasets, draw explicit counterfactual comparisons, graph the trajectory of political institutions against resource reliance over the long run, and employ dynamic, time series-centric analysis—autoregressive distributed lag panel models.[5]

[5] There are both advantages and disadvantages to availing this panel data strategy instead of individual country time-series regressions. Besides the obvious advantage of practicality when N is large (as is the case with our dataset), panel analysis has two additional advantages. First, 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 individual time-series (Baltagi 1995). The disadvantage is that the panel framework

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 the constraints on the chief executive—from the Polity IV dataset (Marshall and Jaggers 2008). For simplicity, we refer to this measure as the *Polity Score*. We center our analysis on the Polity Score because it is the standard measure of democracy/autocracy employed in the resource curse literature, and because it is coded for each country in the world from independence to 2006. In order to make the regression coefficients easier to interpret, we normalize the Polity Score to run from 0 (complete autocracy) to 100 (complete democracy).

Some researchers have argued the democracy is best measured as a binary variable. We therefore also employ, as a robustness check, an electorally-based, binary measure of democracy known as

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imposes a common slope of the independent variables' effects across all countries. It is therefore important to determine whether the assumption of parameter homogeneity is unduly strong. One approach is to employ Pesaran et al.'s (1999) pooled mean group estimator, which allows for slope heterogeneity in the short-run effect across panels. We cannot avail this framework, however, because it requires that the variables be stationary or co-integrated. As we discuss below, our data series do not have these qualities. A second approach is to separately estimate co-integration tests and regressions for each country series. We thus applied this procedure for each of the 17 countries for which we have all three independent measures of resource dependence—fiscal reliance on oil and minerals, per capita gross oil rents, and per capita windfall profits from oil and minerals—and produced results that are materially similar to our panel framework results. They are available upon request.

REGIME. Originally created by Przeworski et al. (2000) and coded between 1950 and 1990, REGIME has been extended by Cheibub and Ghandi (2004) from 1946 to 2002.[6]

A 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 producing 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 country in its same geographic/cultural region. Specifically, *Counterfactual Polity* is a resource producing country's polity score minus the average Polity Score of the non-resource dependent countries in the resource producing country's geographic region.[7] Decreases in the magnitude of this variable over time bespeak divergence between a resource dependent country and its region's average Polity Score, while increases represent convergence.

Measuring Oil and Mineral Dependence

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

[7] 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, with few exceptions, countries that export oil and minerals worth more than five percent of GDP also obtain more than five percent of government revenues from those same resources. In the handful of cases where both measures of resource reliance are unavailable, implying that they are trivial producers, we verify that they produce no, or extremely modest amounts, of petroleum from our dataset on per capita oil production and produce no, or extremely modest amounts of minerals from the Central Intelligence Agency's *World FactBook*.

Researchers are often confronted with important tradeoffs when testing hypotheses with observational data centered on the time dimension, therefore requiring the compilation and standardization of historical data. They can focus on a measure of the independent variable that provides the greatest validity – the best test of the theory under consideration – but be constrained by the fact that its coverage is truncated due to the prohibitive cost of gathering and generating reliable historical data. Or, they can focus on the measure of the independent variable that is the easiest to code, and thus provides the broadest coverage, but be constrained by a weaker fit between this measure and the theory being tested. Where a researcher should fall on this "validity-coverage" curve is not self-evident – especially since the choice may have the potential to drive results. In order to address this issue we employ three different measures of oil and mineral reliance that are used in the extant literature, moving from the one with the theoretically best fit but worst coverage to the one with the theoretically weakest fit and best coverage; explain how these variables are generated; and discuss the advantages and disadvantages of each measure. We then show, in the next section of the paper, that our results are not arbitrarily determined by the measure of resource reliance: all the measures of the independent variable yield similar results.

The causal mechanism that links oil and minerals to regime types is assumed to be the rents captured by governments from the production of oil or minerals. Thus, the first measure of resource dependence we employ is *Fiscal Reliance on Resource Revenues*, the percentage of government revenues from oil or minerals. These revenue streams include taxes and royalties paid by either privately-owned or state-owned oil and mining firms, as well as dividend payments or direct transfers paid to the government by state-owned firms. This measure provides a direct test of the hypothesis that increased revenues from resources induces or protracts authoritarianism by allowing governments to finance themselves without taxing citizens. In fact, it is the measure employed by Mahdavy (1970) in his seminal article on "The Rentier State." Mahdavy's coding of fiscal reliance was far from systematic (he only coded one or two scattered observations per country for ten Middle Eastern countries, leaving him with a total of 15 observations). Other researchers have coded fiscal reliance for individual countries over time (e.g. Haber, Maurer, and Razo 2003). Herb (2005) undertook a systematic coding of this variable for 124 countries on an annual basis covering the period 1972-1999. We code this variable back in time, typically to a country's first year of independence, as well as forward in time to 2006.[8] This means that we can observe countries before and after they became major oil or mineral producers.[9]

There is one practical disadvantage to our time series approach to this measure: the retrieval and standardization of idiosyncratically organized fiscal data from the annual reports of central banks, treasury ministries, and statistical offices extending back to countries' independence is not an enterprise characterized by economies of scale. We therefore truncate our coverage of Fiscal Reliance with respect to the number of countries. We do so by applying three criteria: 1) a country had oil or mineral revenues equal to at least five percent of total government revenues between 1972 and 1999, based on Herb (2005); 2) we are able to obtain uninterrupted volumes of the serial publications that contain countries' fiscal data; and 3) those records allow for the identification of oil and mineral revenue streams. Seventeen major resource exporters meet these criteria: fifteen oil producers and two of the world's major copper producers. The oil producers are Mexico, Venezuela, Ecuador, Trinidad and Tobago, Nigeria, Angola, Indonesia, Iran, Algeria, Bahrain, Yemen, Oman, Saudi Arabia, Kuwait, and Norway. Together, they accounted for 45 percent of the world's oil output in 2006. The copper producers are Chile, the world's most important producer (35 percent or world output in 2005), and Zambia, the world's tenth most important producer (3 percent of world output).

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[8] We use Herb's data series to fill in missing values when primary sources were not available, and thank him for sharing his data set with us.

[9] One concern is that by relying on central bank or treasury records we are potentially omitting resource rents available to rulers but *not* captured via taxation, royalties, or dividends from state-owned oil or mining enterprises. We recognize this potential shortcoming; and we review, where possible, the records of state-owned oil or mineral companies in order to include off-budget revenues from oil or minerals.

One might worry that our restrictions have yielded cases that are potentially unrepresentative and, thus, that our 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 our analysis is immune to this concern. The first is that many of the major petroleum and copper producers that we have excluded from coverage are now robust democracies—such as the United States, Canada, Australia, and Great Britain—that have achieved the highest possible Polity Score. 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 authoritarianism, making it easier to find evidence for the relationship predicted by the resource curse. The second reason is that we employ 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.

As a robustness check we employ a second measure, *Per Capita Windfall Profits from Resources*. It is the value of oil, gas, coal, hard rock minerals, and precious metals produced, minus the costs of production and the opportunity cost of capital, converted to constant 2007 U.S. dollars, divided by population, with all data retrieved from the World Bank's *World Development Indicators*. This measure was developed by Hamilton and Clemens (1999), and variants of it have been employed in the resource curse literature, such as Ross (2006), Ulfelder (2007), Dunning (2008), and Aslaksen (2008). Our data series includes 146 countries, of which 104 exhibit positive values in any given year. This measure has two disadvantages as compared to Fiscal Reliance: it does not control for differences across countries and time in terms of the rents actually accruing to government via the taxation of petroleum and minerals; and it is truncated with respect to time because the first World Bank estimates of the components needed to construct this measure are not available until 1970. Thus, while Fiscal Reliance runs from a country's first year of independence to 2006, this measure only runs from 1970 to 2006.

While we cannot measure windfall profits on oil and minerals before 1970, we can measure one of its components, total income from the production of crude oil (barrels produced multiplied by the real world price), back to a country's first year of independence—and thus employ this variable as an additional robustness test. In doing so, we follow a long-line of scholarship. To begin with, as a first step in pushing Windfall Profits from Oil back to 1960, Ross (2006) and Dunning (2008) code total income from petroleum. [10] Country-specialists have also generated versions of this measure on a country-by-country basis when studying the history of the oil industry within countries over time (e.g., Haber, Maurer, and Razo, 2003). Finally, Resource curse researchers also code and run regressions on this variable directly, usually normalized by population, including Humphreys (2005), Humphreys and Sandbu (2007), Ramsey (2007), Dunning (2008), Aslaken (2008), Colgan (2008), Brambor (2008), and Ross (2009).[11]

We therefore code 165 countries (of which 104 display positive values) from their first year of production or their first year of independence, whichever is earlier, to 2006. Our first positive observations on the volume of output are displayed in 1857, when the first commercial well was sunk in Romania, though we do not have data on prices until 1861. Total oil income per capita has obvious disadvantages that make it theoretically inferior to both Fiscal Reliance and Windfall Profits: it does not include rents from natural gas, coal, and non-fuel minerals; does not reflect differences across countries and time in petroleum extraction and refining costs; and does not reflect differences across countries and time in terms of the rents actually accruing to government via the taxation of petroleum. It does, however, offer one attractive feature. The volume of oil output and oil prices can be easily coded, on a

[10] They do so by coding oil output in real dollars for pre-1970 country-year observations from standard sources, assume a constant opportunity cost of capital, and extrapolate the production costs for the pre-1970 country-years on the basis of post-1970 data. Ross (2006) codes back to 1960 using this technique, while Dunning does so back to 1946, but only estimates regressions on the 1960-2004 part of the series. We thank him for sharing his data series with us. We experimented with this approach, but found that imputing pre-1970 capital and production costs from post-1970 data required a strong set of assumptions that introduced a significant probability of measurement error—a salient concern in time series analysis. Ross (2009) reaches a similar conclusion.

[11] We thank Humphreys for sharing his data set with us.

yearly basis, from publicly available sources (websites, online databases, CD-ROMS, yearbooks, and periodicals) produced by government agencies, international organizations, producer's associations, and industry trade journals, which means, in turn, that this variable affords considerable longitudinal and country coverage.[12]

Control Variables

Due to the time-series properties of the data (discussed below), the dependent variable is first-differenced across all of the OLS models that follow. One might therefore be concerned that our regressions are primarily picking up changes in Polity Scores that occur when countries move from very low Polity levels to higher levels that are far short of what constitutes a full-fledged democracy. Therefore, across our unrestricted specifications, we introduce a dummy variable coded as "1" when the year-to-year change in Polity is one in which the threshold of what constitutes a "coherent democracy"—a Polity Score of 85 or above on the normalized scale, following Gleditsch and Ward (2006)—is surpassed. This variable detects whether the changes in Polity of the highest magnitude systematically occur when countries move between Polity Scores below the coherent democracy threshold (or, alternatively, if the biggest changes occur when countries' Polity Scores move beyond this threshold).

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). Following Gasiorowski (1995) we also include the Growth Rate of *GDP Per Capita*, on the assumption that that high growth promotes regime stability while negative (or slow) growth catalyzes regime transitions.

Finally, we also hold democratic diffusion effects constant in the unrestricted specifications that follow. We do so by controlling for regional and world trends in democratization. Following Gleditsch and Ward (2006), we add two variables to the regressions: 1) the percentage of democracies in a country's

[12] For a discussion of the sources and methods used to code this variable see Appendix A.

geographic-cultural region; and 2) the percentage of democracies in the world (with democracy measured as a score of 85 or higher on the normalized Polity Score).

IV. Data Analysis

Before diagnosing the time series properties of our data, and reviewing the results of several multivariate analyses, we first report some basic patterns by inspecting and graphing the data. Before walking through these patterns, we note that the variables' "within" variation is summarized in Table 1. We also note that this exercise yields some very intriguing surprises.

Hypothesis One: Natural Resources Undermine Democracy

Of the 165 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 coherent 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 measures of resource dependence. 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 (see Figure 1) illustrate the general pattern: even during a period in which rents increased dramatically (60 to 70 percent of government revenues derived from oil and gas), Trinidad's Polity Score 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 became significant oil producers. Figure 2 graphs the Nigerian data. In 1966, when a coup felled democracy, oil accounted for only seven percent of government revenues, and gross oil rents were only $34 per person. 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 the price of oil that occurred in 1973, which ushered in Nigeria's petroleum boom. One would also have to account for the fact that Nigeria's Polity Score has been trending upwards since 1998: since then oil rents have skyrocketed.

Hypothesis Two: Natural Resources Impede Democratic Transitions

We do not find compelling evidence in favor of the hypothesis that oil or minerals impede democratization based on visual inspection of the data. In fact, 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. These cases include Ecuador, Chile, Peru, Venezuela, and Mexico. Figure 3 graphs the data for Mexico. The increase in Mexico's Polity Score from 20 in 1976, to 90 since 2000, captures the country's transition from a long-lived authoritarian regime to a multiparty democracy. What is particularly striking is that this occurred during a petroleum boom. During the heyday of one party rule in Mexico—the 1950s and 1960s—oil typically accounted for less than five percent of government revenue, and total income from petroleum typically amounted to only $35 per capita (in 2007 dollars). In 2000, when the PRI lost its grip on power, oil accounted for 23 percent of government revenue and total income from petroleum had jumped to almost $385 per capita. By 2006, when Mexico held a second free and fair election, the percentage of government revenues derived from oil, and as well as total income from oil, stood at historic highs: 37 percent and $694 per person, respectively.

The second surprising pattern revealed by visual inspection of the data is that there is a set of countries that were highly authoritarian *prior* to the period in which they became significant oil or mineral producers 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, Gabon, and Angola. Figure 4 graphs the data for Angola. The Polity Score, Fiscal Reliance, Per Capita Windfall Profits from Resources, and Total Petroleum Income 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 that have persistently low Polity Scores and that are highly reliant on oil. Many of these countries, however – as Herb (1999) has pointed out with respect to the monarchies of the Persian Gulf (Kuwait, Saudi Arabia, Oman) – had authoritarian political structures in place for decades *before* the first drop of oil ever flowed. In addition to the "oil monarchies," other examples include Iraq, Libya, and Equatorial Guinea. The number of countries actually born as autocratic "petro-states," in which one might *potentially* argue that oil fundamentally conditioned the new nation's political institutions, is quite small. Of the 165 countries in our dataset, only six—Qatar, the United Arab Emirates, Bahrain, Azerbaijan, Kazakhstan, and Turkmenistan—fall into this group.

There is a final set of countries whose data display a pattern that potentially accords with the resource curse: when resource dependence is low, the country has a high Polity Score; conversely, when resource reliance is high, the country exhibits a low Polity Score. The number of such cases, is, however, exceedingly small. We can identify only two: Indonesia and Syria.

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 series: do the data have the same mean, variance and co-variance over time? The second is the specification of the correct lag structure, in order to model dynamics fully and correctly.

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 many countries' Polity Scores. We therefore 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 for each resource dependence measure and for Polity, with separate diagnostics for each distinct time period covered by our resource dependence measures. For example, because Fiscal Reliance covers 17 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 only has coverage between 1970 and 2006, we check to see if Polity is stationary during this period and across the set of countries covered. Table 2, Panels A and B, presents the results. 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 makes each of them stationary.

The non-stationarity of most of our series means that unless the series are co-integrated, 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.[13] We again apply the Maddala and Wu (1999) augmented Dickey-Fuller tests, and report the results in Table 2, Panel C. Because these unit root tests are conducted on the aforementioned regressions' residuals, the null hypothesis is that the variables are *not* co-integrated. All of the tests on the non-stationary series produce insignificant results, implying that three is not a long-run relationship between countries' Polity Scores and their resource dependence. In and of itself, this result casts very serious 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, we estimate a specification in first differences.[14] Despite the lack of co-integration, however, our regressions should still account for both

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[13] 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.

[14] 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

short and long-term effects made by changes in resource dependence on changes in Polity. We thus adopt an Autoregressive Distributed Lag (ARDL) framework in first differences.[15] In order to select the right number of lags of Polity, we choose a vector autoregressive model with the lowest AIC statistic; to select the right number of lags of resource dependence, we choose the number of lags that minimizes the AIC statistic. Because we introduce country dummies into the regressions, the coefficients on the independent variables represent a cross-country average of the longitudinal effect.[16]

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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.

[15] We are forced to 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.

[16] Although first differencing the data 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, which provide 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

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 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 $\text{GDP})_t$), or at some lag of both variables, (Δresource reliance measure_{t-m} X Δlog(Per Capita $\text{GDP})_{t-m}$). 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).[17]

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given year are estimated by introducing year dummies (also, since robust standard errors clustered by country are not robust to contemporaneous correlation between panels, year dummies address this issue).

[17] 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

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 captures the total changes in Polity due to a permanent change in resource reliance at an indeterminate time in the past.[18]

Empirical Findings

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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, it is not necessary to go 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.

[18] 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, 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.

We begin with a set of regressions in which resource dependence is proxied by Fiscal Reliance on Resources. 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. 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 lagged independent variable are positive. To the degree that any variables yield a statistically significant result, it is the coefficient on the sixth lag of the independent variable – but it, too, is positive.[19] One might be tempted to argue that our results are an artefact of our (very) long time series. Column 2 of Table 3 therefore truncates the estimation to the 1950-2006 period. This move has no material effect on the results.

What if the surprising results in specifications 1 and 2 are driven by resource-rich dictators who, feeling an increased sense of security in the wake of a commodity price spike, undertake cosmetic constitutional changes to enhance their legitimacy? One might argue that these token "reforms" are qualitatively different from a transition to democracy—even though the magnitude of the changes in the former case may register as quantitatively larger than the changes in the latter case (there is more "ground to cover" when starting from a very low Polity Score). Therefore, in specification 3 we introduce a dummy variable called *Coherent Democracy Threshold*; it picks up whether there is a fundamental difference between changes in Polity below the 85 point threshold and those that surpass it.

One might also be tempted to argue that the positive coefficients we obtain in specifications 1 and 2 are the product of concomitant increases in GDP per capita that accompany resource booms. Specification 3 therefore adds controls for the log of GDP Per Capita and the Growth Rate of GDP Per

[19] 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.

Capita. Finally, one might argue that the positive coefficients we obtain are the product a world-wide trend towards increased democracy. We therefore control for the possibility of contagion effects in specification 3 by adding variables for the percentage of countries in the world that are democratic and the percentage of countries in a resource producer's geographic region that are democratic. If anything, the addition of all of these controls strengthens our earlier results: the coefficient on the Long Run Multiplier not only remains positive, but is now 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 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 25^{th}, 50^{th}, and 75^{th} 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 thus interact per Capita GDP in t-6 and Fiscal Reliance on Resources 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 do not obtain the negative coefficient that one would expect at the 25^{th} percentile of GDP (nor at lower values that we do not report).

A diehard advocate of the resource curse might 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 nationalize their oil industries, become price setters, and deploy the resulting rents to make their governments accountability-proof. At the same time, the strategic importance of these countries meant that they were not under international pressure to democratize. 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 Windfall Profits from Oil and Minerals Per Capita as the independent variable in Table 4, and re-run all the regressions. The material results do not change appreciably when we substitute this new independent variable. 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), its sign turns positive.

As an additional robustness check we re-estimate all the regressions run in Tables 3 and 4, substituting Total Income from Petroleum Per Capita as the as the independent variable. The results, reported in Table 5, are not materially different from the results in Tables 3 and 4. Five of the six specifications yield Long Run Multipliers with the wrong (positive) sign; and 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; the Impact Multiplier is positive and highly significant in the pre-1950 period (results not shown), and negative in after 1950 but not statistically significant (see Table 3, specification 2).

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 rerun the regressions on Windfall Profits from Oil and Minerals Per Capita and Total Income from Petroleum Per Capita as dynamic conditional fixed effects logit regressions with the REGIME variable. Because we need to include Per Capita GDP and the Growth Rate of GDP Per Capita, the dataset is truncated to post-1950 observations.[20] 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 2009), 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 periods, *t* -1,…, *t*-T. 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:

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

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**

[20] 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.

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.[21]

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, the addition of this coefficient and its interaction term represents the effect of resource reliance on the likelihood that an autocracy will remain autocratic; and if we subtract the product of this addition from 1 we identify the impact of resource reliance on the odds of democratic transition.[22]

We present the results in Table 6. Specification 1 models the effect of increases in Total Income from Petroleum Per Capita on countries that are observed in any year as democratic. Per Capita Resource Reliance t-1 tells us the effect of an increase in Total Petroleum Income Per Capita 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 Total Petroleum Income Per Capita on countries that are observed in any year as authoritarian. Here the resource curse would predict a negative coefficient: as Total Petroleum Income Per Capita increases,

[21] A country that did not experience a regime change is dropped: countries that do not switch from one state to another do not contribute information towards the optimization of the log-likelihood function.

[22] 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).

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 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. Not only does the coefficient have the "wrong" sign, it is statistically significant at the 99 percent confidence level. Specification 4 models the effect of increases in Windfall Profits on countries that are observed as autocratic. It, too, produces a coefficient with the "wrong" sign, although it is not statistically significant. The implication of these results is that an increase in resource dependence makes a democracy less likely to breakdown, but provides no such protection to autocracies.

Multivariate Analyses of Hypothesis III

One 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 2009). Another criticism is that our analyses only measure countries against themselves; they do not measure countries against what they could have become had they never exploited their resources. In order to address these concerns, we now use Counterfactual Polity as the dependent variable; it measures the gap between the Polity Score of a resource producing country and the average Polity Score of the non-resource dependent countries in its geographic region.

We conduct the same diagnostic tests on Counterfactual Polity that we did on the Polity Score 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 tests 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.

Our diagnostic tests indicate that the regressions of Counterfactual Polity against Per Capita Windfall Profits from Resources and against Total Income from Petroleum 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 Resources under this same framework, but with a slight twist: via an Error Correction Model (ECM) interpretation. That means adding the variables in levels to the right-hand side.[23]

The ECM interpretation makes two improvements to the 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

[23] 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}.

variables is restored – or "corrected" – after a short-term shock. We therefore estimate a regression that can be expressed as follows:

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

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 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.

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. Whether there is a resource curse or a resource blessing, the error correction term should be negative since it depicts the restoration to equilibrium.

The coefficients in Table 7 do not yield the predicted signs. The short-run changes in Fiscal Reliance and the Long Run Multiplier are positive across specifications. 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 evidence consistent with the resource curse. None of the specifications that look for the marginal effect of Fiscal Reliance at different levels of Per Capita GDP (columns 4, 5, and 6) produce the predicted results either: poor countries that become more resource reliant do not diverge from their counterfactual Polity Score.

As we did when Polity was the dependent variable, we repeat the regressions on Counterfactual Polity with Per Capita Windfall Profits from Oil and Minerals and then Total Petroleum Income Per Capita. Because these series are not co-integrated with Counterfactual Polity, we revert to the ARDL framework in first differences. Although we do not document the results of these regressions to conserve space, they cast even greater doubt on the resource curse: when resource dependence is measured as Windfall Profits from Oil and Minerals, the LRM is always positive and significant; when resource dependence is measured as Total Income from Petroleum, the LRM is uniformly positive and almost always statistically significant..

V. Conclusion

We have developed new variables that allow us to analyze the longitudinal relationship between countries' resource dependence and their regime type. We observe countries prior to becoming resource reliant, and evaluate whether increases in resource rents affected their political development – both relative to themselves before resource dependence and relative to the democratization experience of countries that were similar to them, save for resource dependence. Our results indicate that oil and mineral reliance does not undermine democracy, preclude democratization, or protract democratic transitions. We note that these results hold even when we calculate the conditional effects at different levels of per capita GDP. This is not to say, of course, that there may not be specific instances in which resource rents help sustain a dictatorship. It is to say, however, that there is a big difference between pointing to these instances and codifying a universal law.

The implications of our analysis extend beyond the literature on the resource curse. Researchers in comparative politics are intensely interested in explaining processes that occur within countries over time, such as industrialization, the rise of the welfare state, the centralization of taxation, transitions to democracy, and the onset of civil war. In studying these processes, however, comparativists often rely on datasets with limited longitudinal coverage. They therefore employ pooled regression techniques, treating countries as homogenous units. The danger of such an approach is that correlations can be mistaken for causation. We suggest that when the theory in question is not about static, cross-sectional differences between countries, but about complex changes that take place within countries over time, assembling and properly using historical datasets designed to operationalize explicitly specified counterfactuals provides a better fit between theory and evidence.

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Appendix A: Estimating Total Oil Income

There are numerous publications that report data on petroleum output (see American Petroleum Institute 1997). These sources do not, however, all employ the same accounting methods; thus, they do not report the same level of oil output for any given country-year. These sources also do not share identical country or longitudinal coverage. Therefore, when researchers splice these series together they may be unwittingly introducing measurement error, a serious concern when the analysis is centered on the data's variation over time. Although some researchers (e.g., Humphreys 2005) take the average of all observations across each available source, this solution may actually be counterproductive: it may introduce unwanted noise if the sources employ widely different coding criteria. We therefore mitigate measurement error through two steps. The first is that we minimize the number of sources we use to construct any single country series. Indeed, we always set out to use a single source, with a consistent accounting method, for each country series. We are able to satisfy this rule for the vast majority of country series. The second is that, whenever feasible, we employ the *Oil and Gas Journal*, the petroleum industry's leading trade journal. With both broad country and temporal coverage, this source thoroughly documents its estimation methods based on field-by-field estimates of daily output for each country-year. Our series are therefore constructed by using the "Oil and Gas Journal Database" (OGJD), which covers 1970 to 2006, the *Oil and Gas Journal Databook* (OGJB), which covers 1983 to 2006 (but which includes minor producers not covered by OGJD), and individual issues of the *Oil and Gas Journal* (OGJ), for all other years. The OGJ began publication in 1902; but, fortunately, the American Petroleum Institute (1971—hereafter API 1971) used the OGJ as one of two sources in estimating petroleum output for the period 1857 to 1967. We therefore rely on API 1971 for pre-1967 figures, and verify that OGJ and API 1971 provided similar observations for overlapping country-years. For some blocks of years during the Cold War era, neither OGJ, OGJB, OGJD, nor API 1971 provide output estimates for some former Soviet Bloc countries. We therefore rely upon the major competing industry trade journal to the OGJ, *World Petroleum* (hereafter WP) for those country-years. Some minor producers of petroleum that have recently come on line (e.g., Belize) are not covered by OGJ, OGJB, OGJD or WP. In addition, the

OGJ, OGJB, OGJD, and WP only begin coverage of some countries that had been part of the USSR (e.g., Uzbekistan) several years after they gain independence. We therefore code the data series for these countries from the country reports of the United States Energy Information Administration website (USEIA). Because USEIA does not begin coverage until 1980, and because we want to make certain that our country coverage is as complete as possible, we verify the first year of output for these countries by using the International Energy Agency's, *Oil Information* (various years), International Energy Agency, *Energy Statistics of Non-OECD Countries* (various years), and International Energy Agency, *Energy Statistics and Balances of Non-OECD Countries* (various years). Countries that are not fully covered by USEIA (e.g. Sweden), or that produce only trivial amounts of oil, so they do not appear in USEIA (e.g. Moldava), are coded from these sources. We take a similar approach to estimating population, employing as few sources as possible (ideally only one) for each country series. We base our estimates on the United Nations (2008); World Bank (2008); Cross National time Series Data Archive; Maddison (2003); and Mitchell (2003).

Figure 1:
Trinidad and Tobago, Oil and Mineral Reliance and Polity, 1901-2006

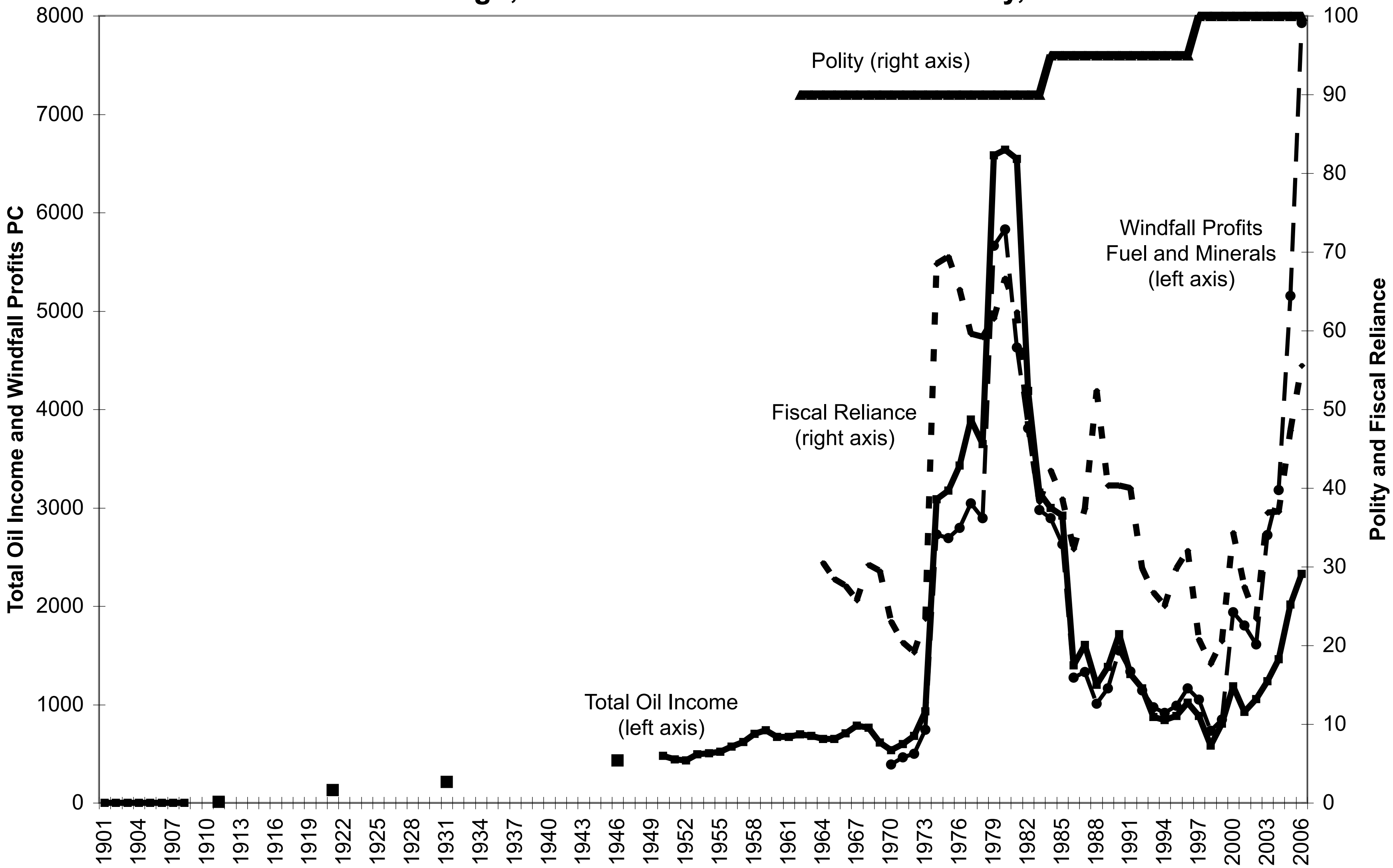

Figure 2:
Nigeria, Oil and Mineral Reliance and Polity, 1958-2006

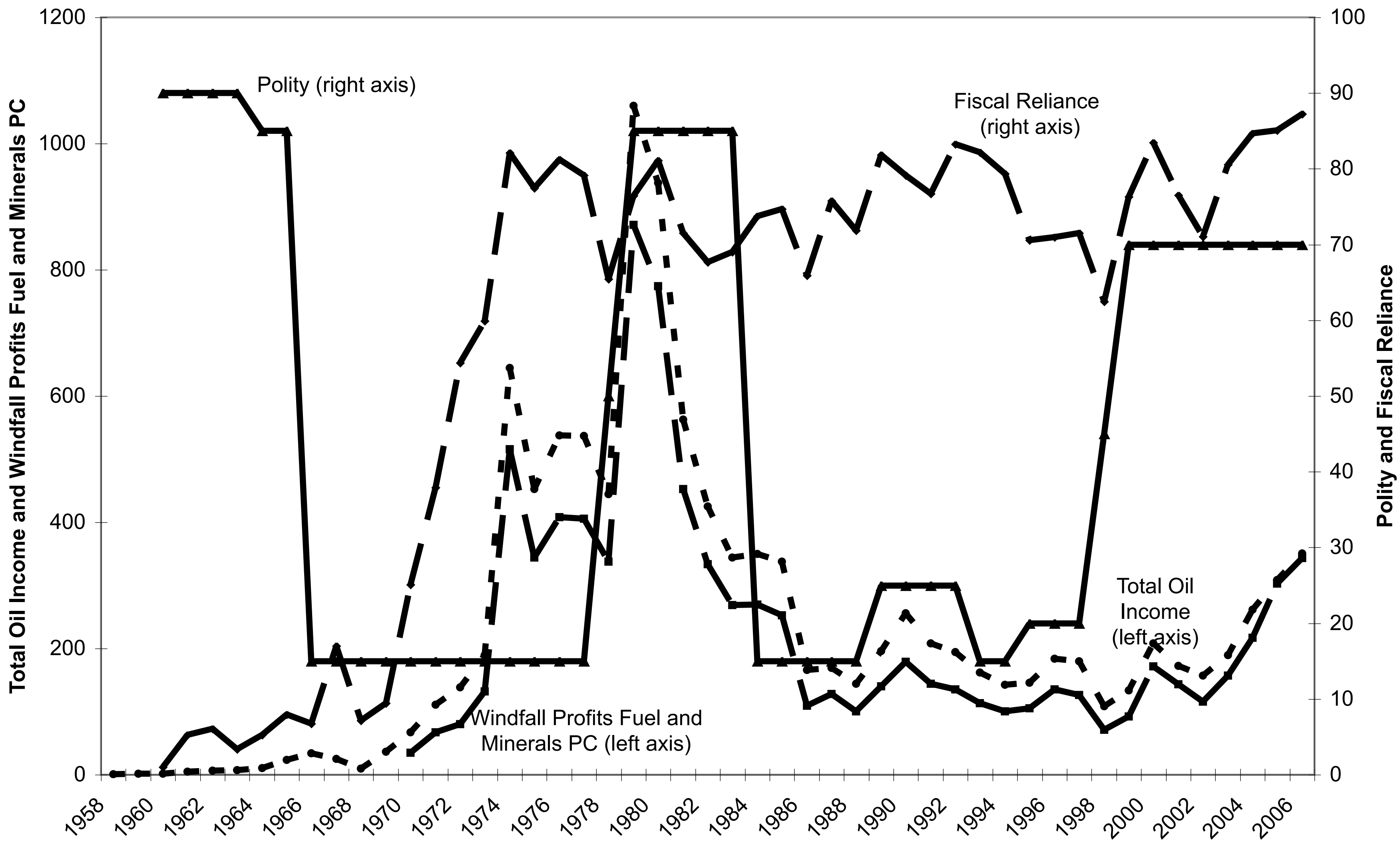

Figure 3:
Mexico, Oil and Mineral Reliance and Polity, 1822-2006

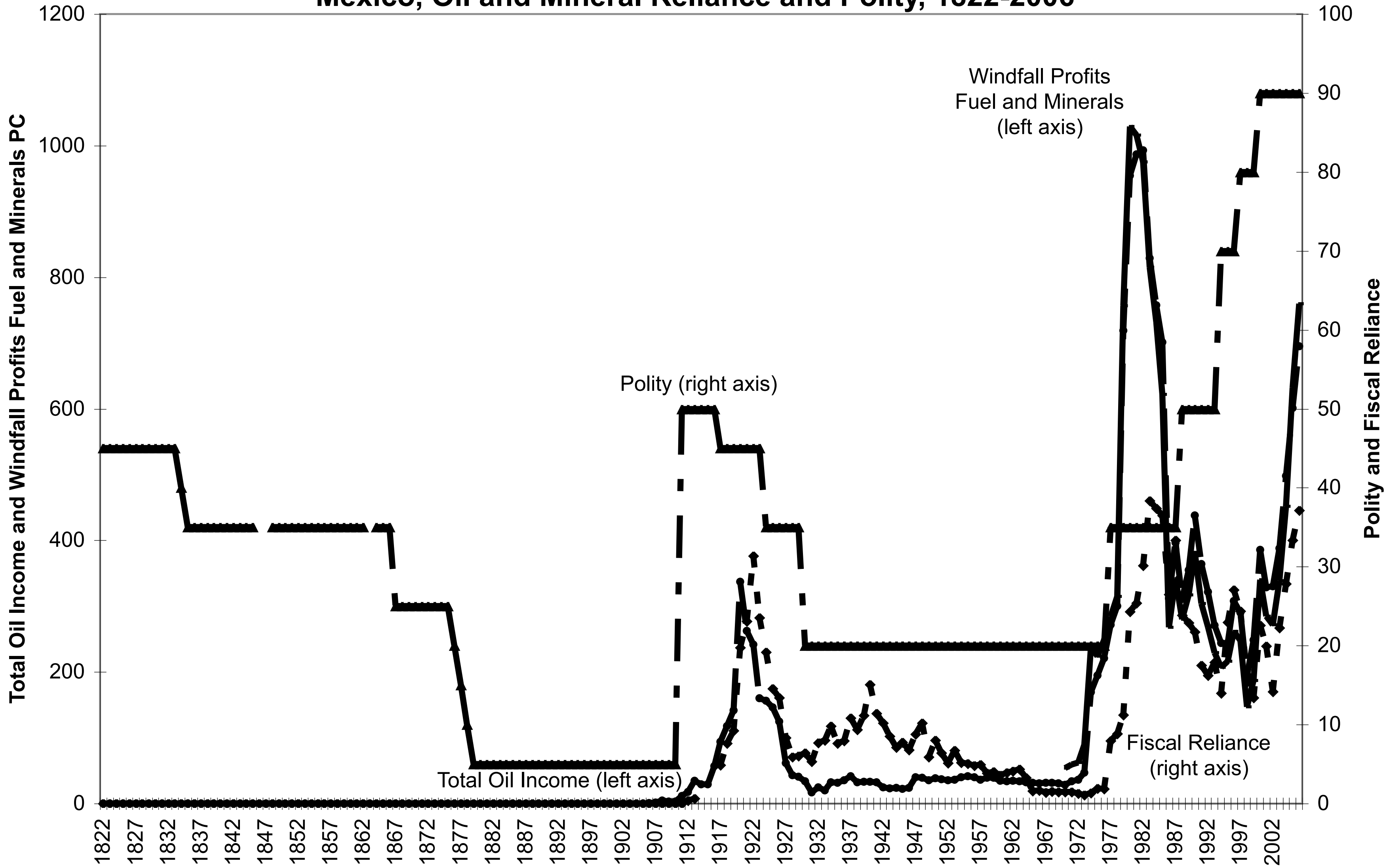

Figure 4:
Angola, Oil and Mineral Reliance and Polity, 1958-2006

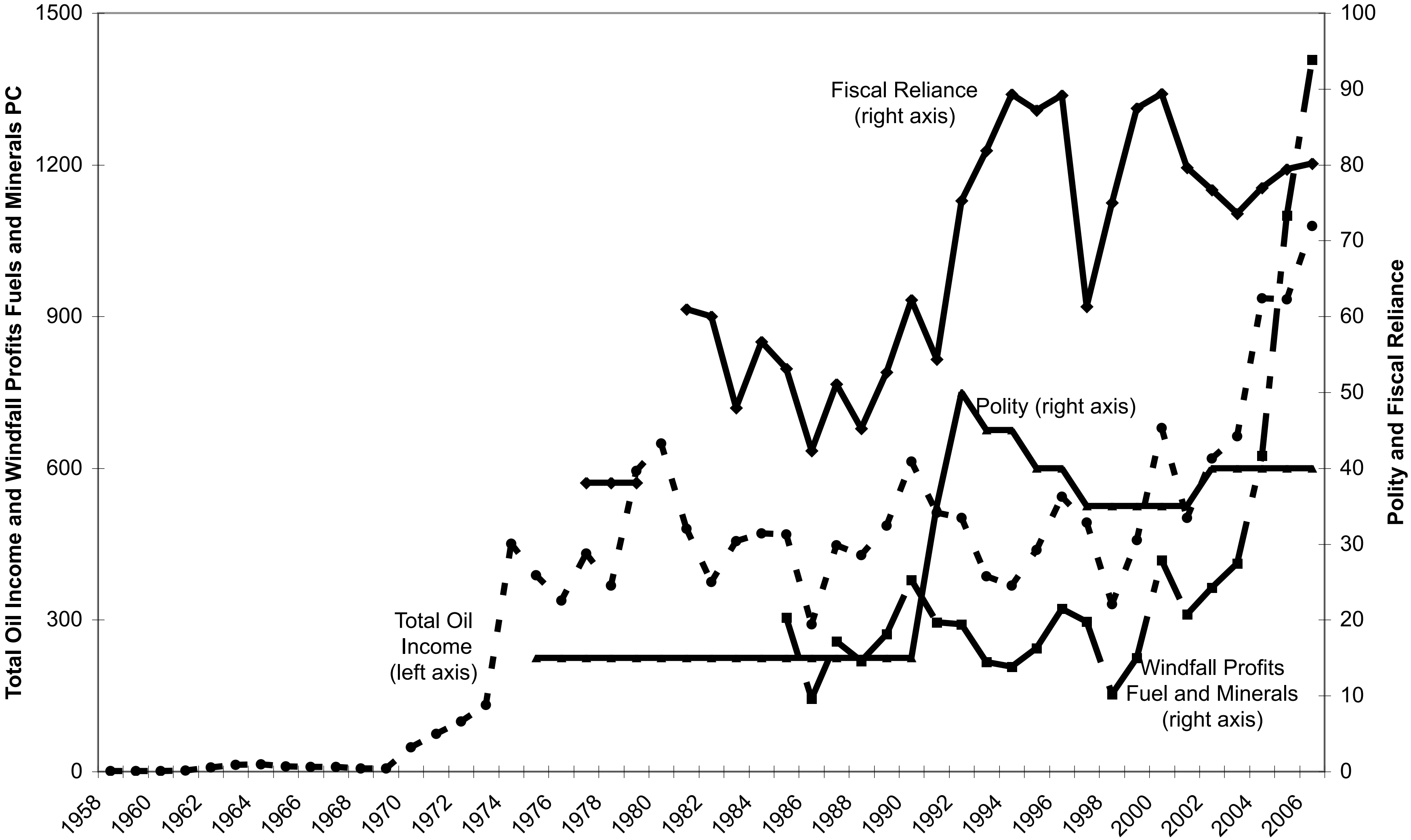

Table 1. Summary Statistics of the "within" variation for Dependent Variables and Co-variates included in Regression Models

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*	*ΔPer Capita GDP*	*ΔGrowth Rate*	*Δ% Dem. World*	*Δ% Dem. Region*
Mean	$5,981	2%	24%	25%	$1,329	0.111	0.3	0.4
Minimum	$256	78%	0%	0%	-$3469	-75	-6	-67
Maximum	$37,304	-46%	49%	100%	$3,570	110	6	67
Standard Deviation	3069	6.34	10	21	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

	1800-2006: Fiscal Reliance as independent variable			*1776-2006: Gross Oil Rents as independent variable*			*1970-2006: Windfall Profits as independent variable*		
Panel A	*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 ∆Polity (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
∆Polity 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]**
∆Fiscal 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]**
∆Fiscal 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]***
∆Fiscal Reliance t-15	**0.04**					
	[0.64]					
∆Fiscal Reliance X ∆ Log(Per				**0.015**		
Capita GDP)				**[0.08]**		
∆Fiscal Reliance X ∆ 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]****
Coherent Democracy Threshold			25.336	25.34	25.208	29.608
			[3.22]***	[3.23]***	[3.36]***	[3.39]***
∆Log(Per Capita GDP)			3.777	3.641	3.023	6.329
			[0.57]	[0.50]	[0.41]	[0.68]
∆Log(Per Capita GDP) t-6					1.241	1.931
					[0.18]	[0.26]
∆% Growth of GDP Per Capita			-0.013	-0.013	-0.016	-0.018
			[0.19]	[0.19]	[0.21]	[0.21]
∆% Democracies in the Region			0.01	0.01	-0.007	-0.043
			[0.12]	[0.12]	[0.08]	[0.43]
∆% Democracies in the World			-1.743	-1.742	0.173	0.525
			[0.98]	[0.98]	[0.48]	[0.99]
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 ∆Log(Per Capita GDP) in interaction models calculated at different percentiles of ∆Log(PCGDP); standard error for marginal effects calculated via "Delta Method."

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

Dependent Variable is ∆Polity (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
∆Polity 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]**
∆Per Capita Windfall Profits	**-0.054**	**0.002**	**0.007**	**0.006**
on Natural Resources	**[1.84]***	**[0.04]**	**[0.11]**	**[0.09]**
∆Per Capita Windfall Profits	**0.004**	**0.043**	**0.049**	**0.051**
t-3	**[0.24]**	**[0.67]**	**[0.63]**	**[0.78]**
∆Windfall Profits X ∆ Log(Per			**-0.18**	
Capita GDP)			**[0.27]**	
∆Windfall Profits X ∆ 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]**
Coherent Democracy Threshold		31.499	31.45	31.489
		[9.02]***	[9.02]***	[9.02]***
∆Log(Per Capita GDP)		-0.403	-0.395	22.84
		[0.09]	[0.09]	[0.88]
∆Log(Per Capita GDP) t-1				-24.1
				[0.97]
∆% Growth of GDP Per Capita		-0.009	-0.009	-0.241
		[0.29]	[0.29]	[0.97]
∆% Democracies in the Region		0.264	0.264	0.264
		[2.68]***	[2.68]***	[2.68]***
∆% Democracies in the World		0.144	0.143	0.145
		[0.50]	[0.50]	[0.51]
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 ∆Log(Per Capita GDP) in interaction models calculated at different percentiles of ∆Log(PCGDP); standard error for marginal effects calculated via "Delta Method."

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

Dependent Variable is ∆Polity (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
∆Polity 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]**
∆Per 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]**
∆Per 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]***
∆Per 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]**
∆Per 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]**
∆Gross Oil Rents X ∆ Log(Per				**-0.253**		
Capita GDP)				**[0.55]**		
∆Gross Oil Rents X ∆ 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]***
Coherent Democracy Threshold			32.708	32.708	32.704	31.215
			[11.36]***	[11.36]***	[11.36]***	[9.78]***
∆Log(Per Capita GDP)			-0.36	-0.336	8.958	18.222
			[0.11]	[0.10]	[0.56]	[0.94]
∆Log(Per Capita GDP) t-1					-9.723	-19.061
					[0.65]	[1.03]
∆% Growth of GDP Per Capita			0.01	0.01	-0.084	-0.19
			[0.38]	[0.38]	[0.55]	[1.01]
∆% Democracies in the Region			0.261	0.261	0.261	0.277
			[3.32]***	[3.32]***	[3.32]***	[2.97]***
∆% Democracies in the World			-0.092	-0.092	-0.093	0.109
			[0.29]	[0.29]	[0.29]	[0.39]
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 ∆Log(Per Capita GDP) in interaction models calculated at different percentiles of ∆Log(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	**-1.714**	**0.395**	**-7.333**	**0.893**
t-1	**[1.53]**	**[0.46]**	**[3.14]*** **	**[0.35]**
Log(Per Capita GDP)	-0.25	0.787	1.809*	1.083
t-1	[0.38]	[1.26]	[1.92]	[0.77]
% Growth of GDP Per Capita	-0.081	-0.047	-0.112	-0.036
t-1	[2.65]**	[1.75]*	[2.76]***	[1.50]
% Democracies in the Region	-0.016	-0.003	-0.03	0.035
t-1	[0.95]	[0.16]	[0.96]	[1.55]
% Democracies in the World	-0.165	0.18	-0.191	0.21
t-1	[1.70]*	[1.89]*	[3.34]***	[3.12]***
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)	ARDL (3,6)	ARDL (3,6)	ARDL (3,6)	ARDL (3,6)	ARDL (3,6)
	1	***2***	***3***	***4***	***5***	***6***
	1800-2006	1950-2006	1950-2006	1950-2006	1950-2006	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]
Coherent Democracy Threshold			27.372	27.322	26.8	31.528
			[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."