Do Natural Resources Fuel Authoritarianism?

A Reappraisal of the Resource Curse

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Date of this Draft: December 12, 2007

Abstract: Is there a relationship between natural resource dependence and authoritarianism? In order to answer this question we develop unique datasets that allow us to focus on within-country variance in resource dependence and regime types. Our results indicate that resource dependence is not associated with the undermining of democracy, the persistence of authoritarianism, or less complete transitions to democracy. Our results are at variance with a large body of scholarship that finds a negative relationship between natural resource dependence and democracy in cross section. We therefore subject those cross-sectional results to a battery of standard diagnostics, and find that the results reported in that literature are very fragile—and the source of that fragility is the use of cross-sectional data to address a question about change over time. We suggest that when researchers are testing theories about processes that take place within countries over time, assembling time-series datasets designed to operationalize explicitly specified counterfactuals is a better match between theory and empirics than regressions centered on the cross-sectional analysis of longitudinally truncated data

Research support was provided by the Stanford University President's Fund for Innovation in International Studies, the Vice Provost for Undergraduate Education, the Social Science History Institute, and the Institute for Research in the Social Sciences. We also thank Nikki Velasco, who not only helped conceptualize the construction and standardization of the data sets, but also coordinated an exceptionally talented team of undergraduate research assistants: Aaron Berg, Nicole Bonoff, Pamela Evers, Joanna Hansen, Meryl Holt, Sin Jae Kim, Gabriel Kohan, Ruth Levine, Aaron Polhamus, Diane Raub, Jennifer Romanek, Eric Showen, Daniel Slate, Anne Sweigart, and Hamilton Ulmer. Michael Herb generously shared his insights on data sources and methods with us. Earlier drafts of this paper were presented at the Social Science History Workshop at Stanford University and the Turning Oil Wealth into Development conference organized by the Center for Democracy, Development, and the Rule of Law of the Freeman Spogli Insititute for International Studies at Stanford University. We thank Ran Abramitzky, James Fearon, Miriam Golden, Avner Greif, Michael Herb, Pauline Jones-Luong, Ross Levine, Francisco Monaldi, Michael Ross, Paul Sniderman, William Summerhill, Nikki Velasco, and Gavin Wright for their helpful comments on an earlier draft.

Introduction

What effect does oil and mineral abundance have on democracy? Broadly speaking, there are three possible answers to this question: oil and minerals are bad for democracy; oil and minerals are good for democracy; and oil and minerals have no effect on democracy one way or the other.

The view that oil and mineral abundance has negative effects on democracy can be found in a broad case study literature that links petroleum to authoritarianism (see Mahdavy 1970, Beblawi 1987, Chaudhary 1994, Van de Walle 1994, Karl 1997, Ross 1999, Gardinier 2000). The "Resource Curse" view articulated in these case studies receives considerable support from studies that use large-n techniques. Ross (2001), for example, finds that a high ratio of oil and mineral exports to GDP is cross-sectionally correlated with lower levels of democratic governance. Wantchekon (2002), using similar cross-sectional techniques, finds that "a one percent increase in resource dependence as measured by the ratio of primary exports to GDP leads to nearly an eight percent increase in the probability of authoritarianism." Ramsey (2006), using instrumental variable techniques, obtains similar results. Jenson and Wantchekon (2004) find that resource abundance is a powerful determinant of autocracy in Africa.

The view that oil and mineral wealth has positive effects on democracy is articulated in a much more recent, and smaller, body of literature. Jones Luong and Weinthal (2006) argue that the effects of oil on democracy are conditional upon the ownership structure of a country's petroleum industry: when petroleum industries are privately owned, oil exerts a positive impact on democracy. Dunning (2007) also argues that natural resources have a conditionally positive impact on democracy. He finds that when there are sizable natural resource rents and the distribution of income is highly unequal, economic elites are less likely to resist democratization.

The third view, that an abundance of oil and mineral resources has no effect on democracy, is articulated by Herb (2005). He points out that any argument about the impact of natural resource rents on regime types requires the specification of a counterfactual: what would a resource dependent country look like had it not found resources? Herb calculates how much poorer resource dependent countries would have been had they not developed their natural resource sectors, and then estimates their level of democracy at these counterfactual levels of GDP. He finds that the net, negative effect of resource dependence on democracy is negligible. Goldberg and Wibbels (2007) reach a similar conclusion via an analysis of data on U.S. states.

The purpose of this paper is to adjudicate among these three views.[1] We follow Herb (2005), and frame the question as a counter-factual: in the absence of natural resource dependence, would countries' regimes have looked all that different? Before they discovered oil and minerals, were today's authoritarian, resource dependent countries just as autocratic? Are some of today's democracies actually democratic *because* of increases in resource dependence over their history? Did the discovery and development of natural resources really have any effect on the direction, magnitude, or timing of subsequent changes in countries' regime type?

Posing the question in this way requires that we employ methods that have not been used in the literature to date. The extant literature largely relies on the analysis of datasets that have short time spans, typically from the 1970s to 2000. Reliance on this time frame has two methodological implications: 1) researchers are initially observing countries *after* they have been oil or mineral exporters for quite some time and; 2) there is not enough temporal variation in the data to allow for the estimation of fixed effects regressions, in which potentially confounding, time-invariant differences between countries (culture, geography, history) are controlled for.[2] As a consequence, researchers employ pooled, cross sectional regression techniques; but these

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[1] Although we realize that several different channels are hypothesized to link oil and mineral dependence to regime types (Ross, 2001: 327-28), our goal is not to examine these mechanisms. We instead assess whether there is a first-order effect of economic dependence on natural resources on regime types. If there is no first-order effect, the channels are irrelevant.

[2] In the case of resource dependence this is an especially germane concern because country specific, time invariant institutions partially determine both the numerator and denominator of many of the resource dependence measures that are commonly used in the literature, as well as a country's regime type. Whenever GDP or GNP is in the denominator, this problem is especially acute, as Acemoglu et al. (2005) have shown.

methods come at a cost: researchers are forced to draw causal inferences about processes that are purported to happen within countries over time on the basis of static variation between countries. The theory underlying these cross-sectional regressions is that, after holding the covariates associated with regime type constant, authoritarian, resource dependent countries were on a path of political development that would have led to the same outcome as that obtained in democratic, non-resource-dependent countries: Venezuela could have followed the same path of institutional development as Denmark – if not for its discovery, extraction and export of oil.

Overcoming this limitation in the extant literature requires an approach to evidence that does not treat countries as homogenous units, but that focuses on longitudinal change within countries over time. We therefore build historical datasets that allow us to observe countries before they became major oil or mineral producers. These datasets allow us to estimate the impact of increasing oil or mineral dependence on countries' regime types. Our longitudinal approach to evidence also allows us to posit explicit counterfactual cases to the resource dependent countries (countries or groups of countries that were broadly similar to the resource producers, but were not themselves resource dependent). We can then see if the subsequent institutional development of the non-resource dependent countries was substantially different than that of the resource dependent ones.

The results of our analysis are neither consistent with a "Resource Curse" or "Resource Blessing" view: we find that economic dependence on oil or minerals has no effect, one way or the other, on regime types. This is not to say that one cannot identify particular cases in which an authoritarian leader has used revenues from the natural resource sector to maintain himself in power. It is to say, however, that the evidence does not support generalizable, law-like statements about the effect of resource rents on regime type.

We take it to be our responsibility to not only produce a substantive result about the resource curse, but to also account for how a large and distinguished body of research could come to another view. To find out, we reproduce the results reported in that literature and then subject those results to standard diagnostics. We find that the cross-sectional approach to evidence in those analyses imposes big costs: outliers can drive regression results; omitted variable bias can give rise to spurious inferences; and results can be driven by the metric of resource dependence one chooses.

Hypothesis Specification:

The Resource Curse contains three implicit hypotheses: 1) natural resource wealth *undermines* democracies; 2) natural resource wealth *impedes* democratic transitions; and 3) natural resource wealth *delays* (*or protracts*) democratic transitions. Each of these hypotheses implies a different counterfactual.

The first hypothesis, that natural resource wealth undermines democracy, implies that had they not found natural resources, today's authoritarian, resource-dependent states would have remained democratic. Operationalizing this hypothesis is a straightforward enterprise; the appropriate counterfactual case is that same resource dependent country before it became resource dependent. If we find that democracies remained equally democratic after they began to develop their natural resources, *or* if we find that authoritarian states remained as authoritarian after they found natural resources, then it is difficult to sustain the claim that natural resource wealth undermines democracy.

The second hypothesis, that natural resource wealth impedes democratic transitions, implies that had they not become major resource producers, countries that were authoritarian before they developed their natural resource sectors would have become democratic. Operationalizing this hypothesis is a somewhat less straightforward enterprise because there are two appropriate counterfactuals. The first counterfactual is that same resource-dependent country *before* it became a major resource producer. If we find that authoritarian states became democratic *after* they became dependent on oil or minerals, it will be difficult to sustain the claim that resources impede democratization.

What are we to make, however, of cases that were autocratic both before and after they became resource dependent: Does the stability of authoritarianism imply that natural resource wealth blocked a democratic transition? Answering this question requires researchers to imagine a counterfactual country that was the same as the resource dependent country in all respects, except that the resources were not found. Obviously, such an imaginary country does not exist and cannot be observed. In order to operationalize this counterfactual we therefore assume that a resource dependent country would have followed the same path of democratization as the other countries in its same geographic/cultural region (e.g., Latin America, Sub-Saharan Africa, the Middle East and North Africa) which were not resource dependent. We then compare the trend (or lack of one) toward democracy of the resource dependent country against the trend (or lack of one) of its region's non-resource-dependent countries. If we find *both* that the resource dependent country deviated negatively from its regional trend, and that the deviation from trend correlates with increasing resource dependence, we can infer that oil or minerals impeded democratization. Conversely, if we find *both* that the resource-dependent country deviated positively from the regional trend, and that the deviation from trend correlates with increasing resource dependence, we can surmise that oil or minerals promoted democratization.

Suppose, however, that we find a salutary effect between increasing resource dependence and democracy: as dependence on oil or minerals increased over time, the resource-dependent country was able to narrow the difference between its level of democracy and that of its non-resource dependent peers – or even surpass it. Does this mean that the resource dependent country democratized as fast or as completely as it could have without resources? Operationalizing this third hypothesis—that natural resource wealth delays or protracts democratic transitions—requires that we specify counterfactual cases with considerable precision. We therefore draw upon techniques developed in the social sciences to approximate experimental setups in the natural sciences (Pearl 2000; Gelman and Hill 2007). These so-called matching methods are based on the calculation of propensity scores that allow researchers to identify pairs of cases in which the non-random nature of the assignment to the treatment group is countervailed against by neutralizing the confounding factors that make assignment to the treatment group more likely. In an ideal world, we would employ this matching approach by: 1) building datasets on oil and mineral producers that extend back to the period before they discovered oil or minerals; 2) collecting data on a wide range of covariates for all countries in the world covering the same years as the datasets for the oil and mineral producers; and 3) generating propensity scores that match each natural resource producer with a non-resource producing control case across each time period of observation.

Unfortunately, we face constraints imposed by the available data. We can, however, approximate these matching techniques by identifying a single relevant control case for each oil or mineral producer based on similarities between their economic, political, and social structures in the period immediately prior to the discovery of natural resources in the treatment case. That is, we assume that the history of the matched, control case represents the path of institutional development of the resource-dependent case, had that resource-dependent country not discovered resources. For example, when Venezuela's discovered oil in 1917 it was little different from neighboring Colombia (which did not discover petroleum until 1977, and even then found it only trivial quantities).[3] Both countries were poor, racially stratified, and politically authoritarian. In fact, Venezuela and Colombia were a single country at the time of their independence from Spain; they only went their separate ways in 1830. Thus, we posit Colombia as a counterfactual to Venezuela without oil. If we observe that Colombia and Venezuela did not follow dramatically different paths of institutional development after Venezuela found petroleum, it would be difficult to sustain the claim that oil exerted a meaningful, independent effect on Venezuela's political institutions.

[3] From 1977 to 2003, average net Colombian petroleum exports accounted for less than two percent of GDP, and the average revenue accruing to the government from oil accounted for less than one percent of total government revenue.

Case Selection

In order to be able to draw causal inferences about the relationship between resource dependence and regime types we must select cases carefully. Simply showing that a country historically had a high level of resource dependence and an autocratic government demonstrates a correlation between the two variables, but it leaves the question of causality open: resource dependence could have caused autocracy; autocracy could have caused resource dependence; or some third (unobserved) factor could have caused both resource dependence and autocracy. We therefore apply three criteria in selecting cases for study. First, the country in question must have a sufficiently large oil or mineral sector to judge it "resource dependent." As a cutoff, we require a country to have oil or mineral exports that have historically averaged at least six percent of GDP.[4] Second, we need to be able to observe time-series variance both in a country's regime type and in its degree of resource dependence. Third, both of these variables' data series must extend back to the period before the country became resource dependent. This means that we must exclude from our analysis cases, such as Saudi Arabia or Kuwait, that were major oil or mineral producers before they became independent countries. It also means that we must exclude cases from our analysis, such as Oman, whose regime types have not varied since independence. Finally, we must exclude from analysis cases, such as Zambia or Angola, where a systematic lack of data prevents us from constructing time-series on resource dependence with adequate coverage. One might lament the need to drop these cases, but both as a matter of econometrics and a matter of logic their inclusion is not justified: without at least some time series variance on both the independent and dependent variables, it is not possible to make causal claims about time series processes.

These restrictions still leave us, however, with a set of ten major resource exporters: nine oil producers and the world's major copper producer. The nine oil producers are Mexico,

[4] We note, however, that the statistical results that follow are not sensitive to specifying lower cutoff points.

Venezuela, Ecuador, Chile, Norway, Nigeria, Iran, Syria, Algeria, Yemen, Oman, Iraq, and Libya.[5] As a group, they accounted for 29 percent of the world's oil exports in 2000. The major copper producer is Chile, which controls one-third of the world's output.[6] One might worry that our restrictions have yielded cases that are potentially unrepresentative and, thus, that our time-series results on the relationship between resource dependence and regime type cannot be extrapolated to the history of other resource producers. Later in this paper we estimate country fixed effects regressions on a panel of the entire world from 1961 to 2000. These panel regressions are performed on all countries that export any amount of natural resources greater than zero, and that exhibit variation over time in their level of resource exports. The results we obtain from this longitudinally truncated panel are consistent with those we obtain from our case-by-case, time-series regressions. We therefore do not think that our restrictions have introduced selection bias.

Measuring Regime Types

We measure regime types by the *Combined Polity Score* (for simplicity we refer to this measure throughout this paper as the *Polity Score*) for several reasons. The Polity Score is the standard measure of democracy/autocracy employed in the Resource Curse literature, as well as in the field of comparative politics more broadly. In addition, the Polity Score is measured for each country in the world going back to its first year of independence through 2003. The Polity

[5] We note that for Yemen, we merge the regime type and resource dependence data for North Yemen (before 1990), with the data for Yemen (after 1990). This allows us to observe Yemen as a single, seamless time-series since North Yemen's independence from the Ottoman Empire in 1918 (North and South Yemen united in 1990).

[6] We note that three of our cases, Chile, Nigeria, and Algeria, had already discovered natural resource wealth prior to independence. However, they did not become major producers of those resources for at least another decade. Therefore, although we cannot observe their regime types before they found natural resource wealth, we can observe them before they really "developed" that wealth. Nigeria provides a case in point: it achieved independence in 1960, but it first began to export oil in 1958. Nevertheless, its level of exports was so low that, at independence, oil only accounted for one percent of government revenues. Moreover, taxes and royalties on oil would not account for more than 20 percent of government revenues until 1970.

Score is an index that measures the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive (see Gurr and Marshall 2005, pp. 15-6). Following conventions in the literature, and in order to make the regression coefficients easier to interpret, we normalize Polity Scores to run from 0 (complete autocracy) to 100 (complete democracy).

Measuring Oil and Mineral Dependence

We measure resource dependence as *Fiscal Reliance on Resource Revenues*. It is calculated as the Percentage of Government Revenues from Oil and the Percentage of Government Revenues from Minerals, following Herb (2005). Our fiscal reliance data series cover Mexico from 1821 to 2003, Venezuela from 1821 to 2003, Ecuador from 1830 to 2000, Chile from 1818 to 1999, Norway from 1814 to 2000, Iran from 1800 to 2000, Yemen from 1926 to 2000, Syria from 1944 to 2000, Nigeria from 1960 to 2003, and Algeria from 1962 to 2000.[7]

We focus on this measure for both practical and theoretical reasons. As a practical matter, using government treasury reports and other serial documents allows us to estimate government revenues from oil or minerals, as well as total government revenues, back to independence. Developing other measures that are commonly used in the Resource Curse literature, such as the ratio of windfall profits from oil or minerals to GDP, or the ratio of windfall

[7] Yearly total revenue data from resources (including taxes, royalties and dividends for state-owned petroleum companies), as well as total government revenues, is usually taken from each country's treasury department. We supplement these sources with data from Herb (2005), the International Monetary Fund's (IMF) *Government Finance Statistics Yearbook* (various years), state-owned oil company annual reports, and secondary sources. For Venezuela, our data is from: Departamento de Hacienda Venezolano (various years) and Ministerio del Poder Popular para la Energía y Petróleo (2004). For Mexico: Wirth (1985); PEMEX (various years); Gobierno de los Estados Unidos Mexicanos (1980); INEGI (1991) and IMF (various years). For Ecuador: Herb (2005). For Nigeria: Amu (1982) and Adeoye (2006). For Norway: Royal Norwegian Ministry of Finance (2005) and Herb (2005). For Chile: Ministerio de Hacienda Chileno (various years) and Mamalakis and Reynolds (1965). For Iran: Bank Markazi Iran (various years); Central Bank of the Islamic Republic of Iran (various years); Herb (2005) and Askari et al. (1982). For Syria: Syrian Government (various years) and Herb (2005). For Algeria: Secretariat D'etat au Plan (various years) and Herb (2005). For Yemen: Herb (2005).

profits from oil or minerals per capita (Ross 2006), would be impossible unless we knew the costs of production by country and by year going back to the first year in which that country produced resources in quantity. To our knowledge, no one has developed such measures before 1960.

As a theoretical matter, our measure of resource dependence – Fiscal Reliance on Resource Revenues—captures the extent to which natural resource rents are taxed or directly absorbed by regimes. These revenues are hypothesized in the literature to affect a regime's ability to repress the population, buy off opponents, and sever its need to trade popular representation for citizens' tax contributions (Ross 2001), therefore allowing it to remain politically unaccountable and, thus, autocratic. Moreover, Fiscal Reliance on Resource Revenues implicitly controls for differences in tax rates and in the extraction costs of oil and minerals across countries and across time. To be sure, this measure of resource dependence may miss oil or mineral revenues that are available to rulers but are not captured via taxation, royalties, or dividends from state-owned oil or mining enterprises. We recognize this potential shortcoming. Later in this paper we will estimate a series of regressions on pooled data from the 1960s and 1970s to the present. As we will discuss in that section, Fiscal Reliance on Resource Revenues *does not* perform much differently in those regressions from other popular measures of resource dependence that are not centered on the resource rents that enter states' coffers.

Testing Hypothesis One: Natural Resources Undermine Democracy

As a first step in assessing the hypothesis that natural resource dependence undermines democracy, we estimate a series of separate time-series, OLS regressions on the relationship between Fiscal Reliance on Resources and Polity Scores for each resource-dependent country.

These static regressions in levels have the following functional form:

$$\text{Polity}_t = \alpha_0 + \mathbf{B}_1\mathbf{X}'_{t-1} + u_t \qquad (1)$$

where α is the intercept, **B** is a vector of parameters to be estimated and always includes the coefficient for Fiscal Reliance on Resource Revenues$_{t-1}$ (the one-period lag of the Percentage of

Government Revenues from Resources to Total Government Revenues), **X** is a matrix of covariates, and u is the error term. Heteroskedasticity and autocorrelation consistent Newey West standard errors are estimated for each country time-series.

If oil or mineral rents undermine democracy, even fledgling ones, then we would expect negative and statistically significant coefficients on the Fiscal Reliance variable: as the percentage of government revenues from oil or minerals increases, Polity Scores should fall. The results presented in Panel A of Table 1, indicate quite the opposite. In only one of the ten cases (Nigeria), does the regression produce the predicated negative coefficient at a statistically significant level (for Syria the coefficient is also negative, but not significant). Eight of the ten cases (Mexico, Venezuela, Ecuador, Norway, Iran, Yemen, Algeria, and Chile) yield Fiscal Reliance coefficients with the "wrong" (positive) sign. In six of these cases, the positive coefficient is statistically significant.

One might argue that these regressions do not control for time varying factors that might be responsible for pushing countries' Polity Scores up over time and that are also correlated with their level of Fiscal Reliance. Fiscal Reliance, according to this view, might be proxying for an unidentified determinant of democratization. For example, perhaps the positive, statistically significant coefficients on Fiscal Reliance for Mexico, Venezuela, Ecuador, Iran and Yemen are spurious because their resource dependence is trending upwards at the same time that Per Capita Income is trending upwards. Thus, the (real) negative impact of oil or mineral dependence on democracy may be masked by the positive effect on democracy exerted by increasing wealth over time (the so-called modernization effect; see Lipset 1959). Similarly, one might argue that there has been steadily increasing international pressure for authoritarian countries to democratize since the fall of the Berlin Wall, and that this pressure creates a trend in the data that may be confounding the results. We therefore control, in Panel B of Table 1, for these possibilities by adding the *Log of GDP Per Capita*, as well as linear and quadratic time trends, to equation (1).[8] The addition of these variables does not, however, materially affect the results. Only three of the ten countries (Nigeria, Iran, and Algeria) display the predicted, negative sign, but none of these coefficients are statistically significant at conventional levels.[9] In seven of the ten cases, the coefficient on Fiscal Reliance has the "wrong" (positive) sign and in three of those cases (Venezuela, Ecuador and Yemen) the positive coefficient is significant at conventional levels.

Another potential objection to these results is that our regressions employ a one-year lag structure, which might not provide enough time for Polity Scores to adjust to increases in Fiscal Reliance on Resources. We therefore re-estimated the regressions employing a single five year lag, a single ten year lag, a five year, distributed lag structure, and an infinitely distributed lag structure. The results of these regressions indicate that our results are not sensitive to the lag structure imposed on the data. We therefore do not reproduce them here.

A different objection to these regressions is that the Polity Score data series may be non-stationary, which would increase the probability that a spurious longitudinal correlation will be induced between Fiscal Reliance on Resources and regime type. This suspicion is confirmed by Dickey Fuller Tests conducted separately on each country series: we *cannot* reject the null hypothesis that the natural resource producers' Polity Scores have a unit root. [10] To deal with the

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[8] For Ecuador and Chile, GDP Per Capita is from OXLAD (2007) and is measured as 1970 Purchasing Power Parity US dollars. For Mexico and Venezuela it is from OXLAD (2007) and from the World Bank Development Indicators (2007) and is measured in current local currency units. For Norway it is from Maddison (2003) and is measured as 1990 International Geary-Khamis dollars. For Iran, Syria, Algeria, and Yemen it is from Fearon and Laitin (2003) and is Real Per Capita Income (chain index) measured in 1985 US dollars. For Nigeria it is from Heston et al. (2004) and is Real Per Capita Income (chain index) measured in 1996 US dollars.

[9] Estimating the regressions without the time trends does not change the qualitative results on fiscal reliance. It does, however, increase the magnitude of the coefficients on the log of GDP per capita.

[10] Keeping in mind that the critical values for reaching statistical significance with a Dickey Fuller Test lie in the negative range, for Mexico's Polity series we cannot reject the hypothesis that it follows a random walk with drift (a z-statistic of 3.4 is returned with a p-value of 1.0); for Venezuela's Polity series we cannot reject the hypothesis that it follows a random walk with drift

non-stationarity of the Polity data, we therefore estimate a new series of regressions using first-differenced data. Besides making each data series stationary, first-differencing also de-trends the data and (usually) eliminates serial correlation. Therefore we estimate regular OLS regressions with White robust standard errors to deal with heteroskedasticity.

When data is first differenced it is particularly important to specify the proper lag structure. We therefore experimented with single one-year lag, single five year lag, five-year finitely distributed lag, ten-year finitely distributed lag, and rational, infinitely distributed lag models. We also estimated models in which we added a lagged dependent variable, in order to account for possible correlation in the changes experienced in Polity from year to year. The material results of these different dynamic models do not vary. Hence, we report the results from a finitely distributed five-year lag model.

We estimate regressions with the following functional form:

$$\Delta \text{Polity}_t = \alpha_0 + \Delta\delta_0 \text{Fiscal Reliance}_t + \Delta\delta_1 \text{Fiscal Reliance}_{t-1} + \Delta\delta_2 \text{Fiscal Reliance}_{t-2}$$
$$+ \Delta\delta_3 \text{Fiscal Reliance}_{t-3} + \Delta\delta_4 \text{Fiscal Reliance}_{t-4} + \Delta\delta_5 \text{Fiscal Reliance}_{t-5} + \Delta u_t \qquad (2)$$

where Δ is the first-difference operator, α is the intercept, δ_0 is the immediate change in Polity Score due to the one-unit change in Fiscal Reliance at time t, δ_1 is the change in Polity Score one period after the temporary change, and so on; and u is the error term.

Table 2 presents our regression results. The coefficient of foremost interest in the regression table is the *Long Run Propensity* (LRP), which is the sum of the coefficients on the five lags of Fiscal Reliance plus the coefficient on Fiscal Reliance in *t*. For example, a coefficient

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(a z-statistic of -0.81 is returned with a p-value of 0.21); for Norway's Polity series we cannot reject the hypothesis that it follows a random walk with drift (a z-statistic of -1.0 is returned with a p-value of 0.16) and for Iran's Polity Series we cannot reject the hypothesis that it follows a random walk with drift (a z-statistic of -0.007 is returned with a p-value of 0.50). As for the remaining resource producers, although we cannot say with statistical certainty that their Polity Scores exhibit a drift, they nonetheless follow a Random Walk. For Ecuador a Dickey Fuller Test returns a z-statistic of -2.2 with a p-value of 0.21; for Chile a z-statistic of -1.7 is returned with a p-value of 0.45; for Nigeria a z-statistic of -2.27 with a p-value of 0.18 is returned; for Syria a z-statistic of -2.31 with a p-value of 0.18 is returned; for Algeria a z-statistic of -1.68 with a p-value of 0.44 is returned and for Yemen a z-statistic of -2.01 with a p-value of 0.28 is returned. In short, the Polity series for the ten countries is non-stationary.

of -1.0 implies that a *permanent* one percentage point increase in Fiscal Reliance five years ago implies a long run fall in the Polity Score of one percentage point. Chi-square tests on the joint-inclusion of the fiscal reliance variable and its lags for each country time-series reveal that none of the coefficients on the LRP's are statistically significant (see F-tests in Table 2). These results are unaffected by the inclusion of Log of GDP Per Capita (results are not shown). In short, the regressions that deal with the unit-root problem in the Polity Score series by first differencing produce results that are inconsistent with the hypothesis that natural resources undermine democracy; they do not reveal any association between changes in resource dependence and changes in Polity Scores.

Testing Hypothesis 2: Natural Resources Impede Democratic Transitions

One simple way to evaluate the hypothesis that natural resources block democratic transitions is to compare countries' Polity Scores before and after they developed their natural resource sectors. Visual inspection of the data reveals that there is a set of countries that were authoritarian before they developed their natural resource sectors, but that nonetheless democratized during a period of increasing resource dependence. These countries include Mexico, Ecuador, Venezuela, Chile and Nigeria. The increase in Polity Scores for some of these countries, we hasten to add, was not trivial. Mexico's Polity Score grew from 0 in 1976 (when its oil boom began), to 80 in 2003, our last year of observation. Venezuela's Polity Score grew from 0 in the 1910s, when it started exporting oil, to 90 between 1969 and 1991. Even with a fall in its Polity Score, to 60 in 2003, as a consequence of Chavismo, the long-run trend is positive. Ecuador's Polity Score grew from 10 in 1971, when it first started exporting oil, to 90 in 1999, our last year of observation. Nigeria transitioned to democracy during the oil boom of the late 1970s (achieving a Polity Score of 80 in 1979), although a military coup ended its democratic experiment in 1983. How are we to know that Mexico, Venezuela, Ecuador, Chile and Nigeria might not have achieved higher levels of democracy if they lacked resources or had been less fiscally reliant upon those resources?

Visual inspection of the data also reveals that, despite upward and downward changes in Polity Scores, there is a set of countries that were, for all intents and purposes, authoritarian before and after they developed their natural resource sectors. These countries include Iran, Yemen, Syria and Algeria. How are we to know that those countries might not have democratized had they not developed their resource sectors? In order to answer these questions, we need to pose a slightly different counterfactual: we have to imagine how democratic our resource dependent countries would have become had they not found and developed their natural resource sectors.

In order to operationalize this counterfactual, we assume that, if our resource dependent countries had lacked resources, they would have obtained the same level of democracy as the non-resource dependent countries in their same geographic/cultural region (Latin America, Sub-Saharan Africa, and the Middle East/North Africa). We therefore calculate the average Polity Score for the non-resource dependent countries in each region, and then subtract that value from the resource dependent country's Polity Score. This data series therefore allows us to compare the trend in the democratization of each of our resource dependent cases against the trend for its geographic/cultural region, and to evaluate whether increased fiscal reliance on resource revenues is systematically related to deviations between these two trends.[11] Therefore, we estimate regressions with the following functional form:

$$\text{Polity Resource Exporter}_t - \text{Average Polity non-resource exporters}_t = \alpha_0 + \mathbf{B}_1\mathbf{X}'_{t-1} + u_t \quad (3)$$

[11] For Nigeria, that means that we include all of the forty four countries in Sub-Saharan Africa except those whose oil or mineral exports have totaled more than six percent of GDP over their history. The excluded countries are Mauritania, Guinea, Liberia, Togo, Gabon, Zambia, Congo Brazaville, Congo Kinshasha, Namibia and Angola. For Iran, Syria, Algeria and Yemen that means that we only include the few Middle Eastern and North African countries that are not major oil exporters *vis-à-vis* the 6 percent threshold. That means that the Polity Score average of non-oil producers includes Turkey, Tunisia, Morocco, Sudan, Jordan and Egypt. We note that we drop the case of Norway from this exercise because it obtained a Polity Score of 100 (the highest possible value) over fifty years before it found any oil.

where α is the intercept, **B** is a vector of parameters to be estimated and includes the coefficient for Fiscal Reliance$_{t-1}$ (the one-period lag of the Percentage of Government Revenues from Oil or Minerals to Total Government Revenues), **X** is a matrix of covariates and u is the error term. Heteroskedasticity and autocorrelation consistent Newey West standard errors are estimated for each country time-series.

We present the results of this counterfactual exercise Table 3, Panel A. The results do not unequivocally support the hypothesis that natural resource dependence impedes democratization. Four of the nine cases—Mexico, Nigeria, Iran, and Syria—produce the statistically significant, negative coefficients predicted by the hypothesis that natural resource dependence impedes democratization. Chile produces a negative coefficient, but it is not significant. Four other cases—Venezuela, Ecuador, Algeria, and Yemen—produce opposite results: statistically significant, positive coefficients, suggesting that increasing resource dependence induced them to either narrow or surpass the gap between their level of democracy and that of their non-resource dependent counterparts within their respective geographic/cultural regions.

As in Table 1, however, the results displayed in Panel A of Table 3 could be confounded by the omission of important time-varying factors that one might think would produce spurious positive coefficients. To address this concern, Panel B of Table 3 includes the Log of GDP Per Capita, as well as linear and quadratic time trends, to control for time varying factors that may be trending in the same direction as Fiscal Reliance. Once we add these control variables, however, the coefficients for Nigeria, Mexico, Syria, and Chile actually switch signs. This means that only one of our nine cases, Iran, now yields the predicted negative coefficient, but it is no longer statistically significant. Moreover, three of the eight cases with the "wrong" (positive) coefficients, Venezuela, Ecuador and Algeria, are also statistically significant. In short, once these robustness checks are performed, we do not find any evidence that as fiscal reliance on resource revenues increased over time in our resource dependent countries, their democratization trends deviated negatively from that of countries that were similar to them, except for the fact that they lack resource wealth.[12]

Testing Hypothesis 3: Oil Delays (or Protracts) Democratic Transitions

Our data series indicate that five of our ten cases—Mexico, Nigeria, Venezuela, Ecuador, and Chile—underwent periods of democratization after they began to develop their natural resource sectors. Some of these countries, such as Mexico and Chile, are currently vibrant democracies. How do we know that these five countries would have become *even more* democratic, or would have transitioned to democracy faster, had they not developed their resource sectors? As we explained earlier, assessing this hypothesis requires us to specify a set of counterfactual paired comparisons that were similar to these resource dependent countries in terms of their underlying economic, social, and political institutions on the eve of the discovery of natural resources. In the language of the experimental sciences, our treatment cases are the natural resource exporters. Our control cases are a set of countries that were broadly similar to these natural resource exporters before the "treatment" (in this case the production of oil or minerals on a large-enough scale to export them) was first applied.[13]

Justifying the paired comparisons

The control case for Venezuela is Colombia.[14] During the colonial period these nations were part of a single administrative entity, and after obtaining independence from Spain in 1811, they were a single country called Gran Colombia. They did not go their separate ways until 1830.

[12] Estimating the regressions with GDP per capita alone, i.e., dropping the time trends, does not qualitatively affect the results.

[13] Because of the astronomically high price of petroleum in recent years, some of our control cases may currently produce oil in modest amounts. Nevertheless, none of them exhibited net petroleum exports that averaged even just one percent of GDP during the period of observation.

[14] From 1977 to 2003, average net Colombian petroleum exports accounted for less than two percent of GDP, and the average revenue accruing to the government from oil accounted for less than one percent of total government revenue.

During the nineteenth century, both countries had similar economic, social, and political structures: slow-growing economies based on the export of coffee, sugar, and cattle; mestizo populations in the highlands and African populations on the coast; and politically unstable, poorly-funded governments. Neither country succeeded in creating stable democratic institutions.[15]

The control case for Ecuador is Peru. During the pre-colonial period, both were part of the Inca Empire, sharing a common language and common political and administrative institutions. For most of the colonial period, they were part of a single administrative unit, the Viceroyalty of Peru. Their political and economic geographies determined broadly similar patterns of development after independence from Spain in 1822. Both were highly stratified societies, with an urban white elite and an indigenous and *mestizo* underclass. Both were also largely highland societies, in which high transportation costs bedeviled the creation of a national economy and a national state. To the degree that either was linked to external markets, it was through products found in the relatively unpopulated lowland areas: bananas in Ecuador and guano (in the nineteenth century) in Peru.[16] Both countries were characterized by endemic instability and authoritarianism. In fact, on the eve of Ecuador's discovery of oil in 1972, both were governed by military juntas.

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[15] On the eve of Venezuelan oil exports in 1916, Venezuela's GDP per capita was $107, while Colombia's was $246; Venezuela had 2.7 million inhabitants, while Colombia had 5.6 million inhabitants; Venezuela had 25 percent of GDP in agriculture and 11 percent of GDP in manufacturing while Colombia had 48 percent of GDP in agriculture and 8 percent of GDP in manufacturing. Meanwhile, Venezuela's (un-normalized) Polity Score was -6 and Colombia's Polity Score was -5. Data for population, GDP per capita and economic structure is from OXLAD (2007); data for agriculture and manufacturing is percent value added.

[16] On the eve of Ecuadorian oil exports in 1972, Ecuador's GDP per capita was $367, while Peru's was $550; Ecuador had 6.2 million inhabitants, while Peru had 13.6 million inhabitants; Ecuador had 30 percent of GDP in agriculture and 18 percent of GDP in manufacturing while Peru had 18 percent of GDP in agriculture and 21 percent of GDP in manufacturing. Meanwhile, Ecuador's (un-normalized) Polity Score was 0 and Peru's Polity Score was -7. Data for population, GDP per capita and economic structure is from OXLAD (2007); data for agriculture and manufacturing is percent value added.

The control case for Mexico is Brazil. In the nineteenth century, both were slow growing economies hampered by high internal transport costs (Summerhill 1997). Both experimented with limited democracy in which the suffrage was tightly restricted, and both had powerful local elites that often operated with considerable latitude from the central government. During the last decades of the nineteenth century, both underwent similar processes of market integration via the construction of railways; both industrialized rapidly, protected by high tariff walls; and both built stable, yet decidedly non-democratic, political regimes. Thus, at the time of Mexico's first oil boom (1905-1924), Brazil is the appropriate counterfactual.[17]

At the time of Mexico's second oil boom (1977 to the present), Brazil remains the appropriate counterfactual. Before that oil boom got underway, Mexico and Brazil were both extremely protectionist, concentrating their efforts on building national manufacturing industries. Both were also ruled by autocratic regimes: the PRI in Mexico and a military junta in Brazil. While they were hardly identical, these two regimes did have some striking similarities: they maintained the trappings of democracy, with "elected" bicameral legislatures housed with political parties licensed and severely restricted by the ruling party, in Mexico's case, and by the military, in Brazil's case.[18]

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[17] On the eve of Mexican oil exports in 1905, Mexico's GDP per capita was $258, while Brazil's was $116; Mexico had 14 million inhabitants, while Brazil had 19 million; Mexico had 27 percent of GDP in agriculture and 6 percent of GDP in manufacturing while Brazil had 45 percent of GDP in agriculture and 13.2 percent of GDP in agriculture. Meanwhile, Mexico's (un-normalized) Polity Score was -9 and Brazil's Polity Score was -3. Data for population, GDP per capita and economic structure is from OXLAD (2007); data for agriculture and manufacturing is percent value added except for Brazil, which is from 1905 and is raw output as a percentage of GDP (from Pinheiro, et al. 2001).

[18] On the eve of Mexican oil exports in 1976, Mexico's GDP per capita was $974, while Brazil's was $729; Mexico had 62 million inhabitants, while Brazil had 108 million; Mexico had 10 percent of GDP in agriculture and 24 percent of GDP in manufacturing while Brazil had 8 percent of GDP in agriculture and 30 percent of GDP in manufacturing. Meanwhile, Mexico's (un-normalized) Polity Score was -6 and Brazil's Polity Score was -4. Data for population, GDP per capita and economic structure is from OXLAD (2007); data for agriculture and manufacturing is percent value added.

The control case for Nigeria is Tanzania. Both share common colonial histories: both were governed by Britain under a set of similar administrative institutions. Both had similar pre-independence economies centered on cash crops: coffee in Tanzania and cocoa and palm oil in Nigeria. Finally, Tanzania is one of a few countries in Africa that, like Nigeria, has a sizable Christian and sizable Muslim population.[19]

The control case for Chile is Argentina. Both share common colonial histories: each was a backwater of the Spanish empire. They are geographically contiguous and both became independent at the same time: in fact, Chile's independence was the product of an invasion launched by newly-independent Argentina. Both had small indigenous populations and were sparsely populated; that is, until the end of the nineteenth century, when both experienced considerable European immigration. Both countries began their existence as sovereign nations as producers of agricultural products (but neither was a major exporter of agricultural goods until well after independence). However, while Chile was blessed with considerable deposits of nitrates and copper, Argentina was not.[20]

Multivariate Analysis

To discern the treatment effect of oil (and minerals, in the case of Chile) on countries' regime types we estimate a regression in which we pool each treatment case with its relevant control case. These regressions have the following functional form:

$$\Delta \text{Polity}_{it} = \beta_0 + \gamma \text{Resource Producer}_i + \beta_1 \Delta \text{Resource Exports}_{it\text{-}1} + \ldots + \Delta u_{it} \qquad (4)$$

[19] On the eve of Nigeria's oil export boom in 1960, Nigeria's GDP per capita was $567, while Tanzania's was $303; Nigeria had 40.8 million inhabitants, while Tanzania had 10.2 million inhabitants (Heston et al. 2004); Nigeria had a .28 Labor to Land ratio, while Tanzania had a .20 Labor to Land ratio (Lusigi and Thirtle 1997). Nigeria's (un-normalized) Polity Score was 8 and Tanzania's Polity Score was -7.

[20] On the eve of Chile's independence in 1818 and its first nitrate exports as a sovereign nation, Chile had 1.8 million inhabitants, while Argentina had 1.7 million inhabitants (Mitchell 1998); this data is from 1865 for both countries. Chile's (un-normalized) Polity Score was 10 and Argentina's Polity Score was also 10.

where Δ is the first-difference operator, Polity is the Polity Score, β_0 is the baseline, the country that serves as the control case for the oil exporter, γ is a parameter to be estimated, *Resource Producer* is a dummy variable that always takes a value of "1" for the resource producing country and "0" otherwise, *Resource Exports* is a dummy variable that takes a value of "1" *only* when the resource producing country exports petroleum or minerals and "0" otherwise (because resource exports are perfectly multicollinear with the Resource Producer dummy in the case of Nigeria and Chile, Resource Producer is omitted from those regressions) and u is the error term.

First-differencing of equation (4) allows us to take a quasi-experimental approach. We can sidestep the dilemma of non-random assignment, in which the timing of resource discoveries and their exportation is not fully exogenous. Specifically, first-differencing expunges the country-specific, unobserved factors that may engender selection bias: countries' unobserved factors may be systematically-related to whether we observe them export natural resources in year *t*. Indeed, governments most in need of revenue may step up their efforts to discover natural resources and/or rev up resource exports. Moreover, because the data is first-differenced, including a dummy variable for the resource producer captures the unobserved, country-specific, *time-varying* heterogeneity that is not explicitly modeled in this first-differenced equation. This is particularly important in the case in Ecuador, and during Mexico's second oil boom, when the Resource Export dummy is coded as a "1" late in the sample and, thus, this variable could unduly proxy for country-specific time trends such as increasing trade openness.

We can now safely apply a difference-in-difference interpretation to the data: the difference in the average changes in polity between the resource producing country and the non-resource producing country *during the period on which the resource producer exports oil or minerals*. The coefficient of interest is the Resource Export dummy. For example, a coefficient of 1.0 would imply that exporting oil produced a one-percentage point cumulative increase in the treatment country's Polity Score during the period in which it is "exposed" to oil.

Our results are presented in Table 4. Four of the five specifications produce coefficients with the "wrong" (positive) sign. To the degree that the regressions yield any statistically significant results (at the ten percent level), the result is positive: in the Mexico-Brazil regressions, exporting oil is associated with *an increase* in Mexico's Polity Score after controlling for time-trends specific to Mexico. None of these results are affected by controlling for per capita income (results not shown). In short, the results indicate that Polity Score changes are not affected by exporting oil or minerals.

Reconciling our Results with the Resource Curse Literature:

We take it to be our responsibility to not only account for how things happen, but to account for how a large and distinguished body of research could come to another view. To find out, we reproduce the results reported in the resource curse literature and then subject those results to standard diagnostics. We find that the cross-sectional approach to evidence in those analyses imposes big costs: outliers can drive regression results; omitted variable bias can give rise to spurious inferences; and results can be driven by the metric of resource dependence one chooses.

Are the Conventional Results Sensitive to How Resource Dependence is Measured?

We begin by estimating a series of cross-sectional regressions on the relationship between resource dependence and Polity Scores, in which we vary the measure of resource dependence. Because we are now concerned with differences between countries in terms of their resource dependence and level of democracy, we include a set of control variables with significant cross-sectional variation to capture differences between countries that could confound the results. These controls are standard in the Resource Curse literature.[21] One might imagine

[21] We chose these variables following Ross (2001): 341. These include: log(Per Capita Income), which we get from Heston et al. (2004); the *Percentage of the Population that is Muslim* (which we get from Fearon and Laitin 2003); and a dummy for *High Income OECD Countries* (as

that changes in resource dependence are not immediately reflected in a country's Polity Score. Furthermore, Achen (2000) has shown that the explanatory power of an independent variable lagged one year is suppressed in a dynamic model in which the dependent variable is also lagged one year. We therefore estimate all of the regressions with a one-year lag, a single five-year lag, and as a five-year, finitely distributed lag model. The results are not materially sensitive to the lag structure chosen. We therefore report only the results from the one year and single five year lag models.

We estimate a pooled, cross-sectional regression with the following functional form:

$$\text{Polity}_{it} = \alpha_0 + \alpha_1\text{year}_2 + \ldots + \alpha_T\text{year}_T + \Phi\text{Polity}_{it\text{-}1} + \beta_1\text{Resources}_{it\text{-n}} + \mathbf{B}_2\mathbf{X}'_{it\text{-}1} + \text{u}_{it} \quad (5)$$

where Polity is the Polity Score of country i = 1,…, N, at time t = 1,…, T_i, α is the intercept for the first observed year, year_1, α_t is a parameter to be estimated for each year, which is a dummy variable that assumes unique year intercepts, $\text{Polity}_{it\text{-}1}$ is country *i's* Polity Score lagged one period, Φ is a parameter that taps the resilience (adjustment) of a country's regime type against shocks, β_1 is a parameter to be estimated, *Resources* is our measurement of resource dependence (lagged) and conceptualized as a shock driving Polity Scores to new levels, **B** is a vector of parameters to be estimated, **X** is a vector of covariates that are conceptualized as a series of shocks driving Polity to new levels lagged one year.

Table 5 presents the results. While the lag structure does not matter, the choice of variable does. When resource dependence is measured as either *Fuel Exports as a Percentage of GDP* or *Mineral Exports as a Percentage of GDP*, following Ross (2001) resources are strongly associated with lower Polity Scores (see specifications 1 and 5). When resource dependence is measured as the *Percentage of Government Revenue from Resources* (a measure that includes government revenue from both oil and minerals), which we take from Herb (2005), resources continue to be associated with lower Polity Scores. However, the magnitude of the coefficient

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defined by the World Bank). We also follow Ross (2001) and lag the dependent variable; we note, however, that if we instead estimate static models with an AR(1) structure, or adjust the standard errors via the Newey West technique, we obtain qualitatively similar results.

falls by nearly half (see specifications 2 and 6). When resource dependence is measured as either Windfall Profits from Oil over GNP, or Windfall Profits from Minerals over GNP, following Hamilton and Clemens (1999), only the oil measure comes up as statistically significant: dependence on minerals is no longer associated with lower polity scores (see specifications 3 and 7). Finally, when resource dependence is measured as either Per Capita Windfall Profits from Oil or Per capita Windfall Profits from Minerals, following Ross (2006), both measures produce a coefficient of zero, and neither is statistically significant.

Are the Results in the Literature Sensitive a Product of Outliers?

The regressions reported in Table 5 suggest an additional area of concern with the results reported in the literature. A *Shapiro-Wilk W Test* of skewness indicates that five of the seven measures of resource dependence are highly left-skewed. These skewed measures exhibit a statistically significant, negative association with countries' regime types; this suggests that outliers may be driving the results. Inspection of the data corroborates this suspicion: a few countries, such as Trinidad and Tobago, Brunei, and Qatar, have ratios of Fuel Exports to GDP that are an order of magnitude above the mean; the same is true of Zambia and Bahrain when it comes to the ratio of mineral exports to GDP. This indicates that a fundamental assumption of OLS regressions may be violated: the non-normal distribution of the data suggests that the residuals are not normally distributed. The extreme skewness of the resource variables measured in levels is also likely exacerbating heteroskedasticity.

In order to minimize the adverse effects of outliers we log the independent variables that come up as significant in our regressions in Table 5.[22] (We do not log Per Capita Windfall Profits from Oil or from Minerals because the unlogged coefficients are zero and are not

[22] Following conventions in the literature, we add .01 to all zero values before taking logs.

statistically significant).[23] We present the results of a one-year lag model and a single five-year lag model in Table 6. We note that we also estimated a five year distributed lag model that produced the same qualitative results, and thus we do not reproduce those results here. While the results are not sensitive to the lag structure we impose, they are highly sensitive to logging the data. In fact, almost all of the standard results (as replicated in Table 5) are no longer statistically significant at conventional levels once we log the data. When we use a one-year lag structure, only one of the five measures of resource dependence yields a statistically significant, negative result: the ratio of Windfall Profits from Oil to GNP. When we use a single five-year lag structure, none of the five measures of resource dependence yield statistically significant results at conventional levels (two of the five are significant at ten percent). When we use a five-year distributed lag model (results not shown), none of the long-run propensity coefficients of the logged resource dependence variables (the sum of the coefficients on the five lags, plus the coefficient on the variable measured in t) are statistically significant at conventional levels. One of the coefficients (the ratio of Mineral Exports to GDP) is significant at ten percent. In short, when we transform the data in order to control for the impact of outliers, almost all of the conventional results disappear.

Are the Results in the Literature a Product of Omitted Variable Bias?

One of the reasons why outliers are able to have a large impact on the results from pooled OLS regressions is because the samples that are conventionally used are truncated with respect to time. (We have used those same 1972-1999 time periods to produce our results, above). Relying on longitudinally truncated datasets comes at an additional cost: the amount of temporal change on the independent variable of interest—resource dependence—is systematically underestimated,

[23] Histograms of the unlogged versions of these variables indicate severe left skew and the bottom row of Table 6 reports the Shapiro-Wilk W tests for skewness. Histograms of the logged versions suggest a distribution approaching normality; Shapiro-Wilk W tests confirm this.

increasing the likelihood of measurement error. This is why researchers tend to pool the data and focus on countries' cross-sectional variation.

The use of pooled regression techniques, however, introduces the problem of omitted variable bias. In order to mitigate that problem we re-estimate the regressions reported in Table 5 with country dummies.[24] Similar to the separate time-series analyses we conducted earlier on each of the resource dependent countries, our regressions now pick up the impact of changes in resource dependence on Polity Scores within countries over time. To control for changes common to each country during any given year, we also include year dummies.[25]

One might argue that problems of longitudinal truncation will reduce the statistical significance of a country-fixed effects regression. Fortunately, we are able to address this problem with respect to two often used measures of resource dependence—the Percentage of Fuel Exports to GDP and the Percentage of Mineral Exports to GDP—by extending the series back to 1961. One might also argue that the results of a country-fixed effects regression will be highly sensitive to the lag structure imposed on the data. We therefore estimate all regressions using a single one-year lag, a single five-year lag, and a finitely distributed, five-year lag model.

We estimate a dynamic, fixed effects regression with the following functional form:

$$\text{Polity}_{it} = \alpha_0 + \alpha_1\text{year}_2 + \ldots + \alpha_T\text{year}_T + \Phi\text{Polity}_{it-1} + \beta_1\text{Resources}_{it-\mathbf{n}} + \mathbf{B}_2\mathbf{X}'_{it-1} + \nu_i + u_{it} \quad (6)$$

where Polity is the Polity Score of country $i = 1,\ldots, N$, at time $t = 1,\ldots, T_i$, α is the intercept for the first observed year, α_t is a parameter to be estimated for each year, where "year" is a dummy variable that assumes unique year intercepts, Polity_{it-1} is country *i's* Polity Score lagged one period and Φ is a parameter that taps the resilience (adjustment) of a country's regime type

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[24] The fixed effects estimator we employ transforms the model by subtracting out the time series means of each variable for each country.

[25] Including fixed effects in a dynamic model such as this one (in which the dependent variable is lagged) is especially important in order to avoid dynamic panel bias, in which the lagged dependent variable is unduly attributed with explanatory power (higher inertia) that belongs to the country fixed effects.

against shocks, β_1 is a parameter to be estimated, Resources is our measurement of resource dependence lagged one year or five years and conceptualized as a shock driving Polity to new levels, **B** is a vector of parameters to be estimated, **X** is a matrix of covariates that are conceptualized as a series of shocks driving Polity to new levels, each lagged one year, ν_i is a country-specific and fixed unobserved effect and u is the error term. The dependent variable, independent variables and error term are time-demeaned to remove ν_i. Therefore, a within-estimation of the data is performed.

We present the results of the single one-year and single five-year lagged models in Table 7. Each of the specifications employs a different measure of resource dependence: the Percentage of Fuel Exports to GDP; the Percentage of Mineral Exports to GDP; the Percentage of Government Revenues from Resources to Total Government Revenues; the Percentage of Windfall Profits from Oil to GNP; and the Percentage of Windfall Profits from Minerals to GNP.[26] We also estimated similar regressions on the relationship between Polity Scores and Per Capita Windfall Profits from Oil and Per Capita Windfall Profits from Minerals. We do not report those results because they produced coefficients of zero (that were not statistically significant) in a pooled OLS setting. They also produced coefficients of zero (that were not statistically significant) when country fixed effects are included. We repeat the same steps in Table 8, only that there we employ a five-year, finitely distributed lag structure.

Regardless of how we measure resource dependence, and regardless of the lag structure we employ, all of the negative and statistically significant results obtained in the pooled OLS regressions (see Table 5) disappear when country fixed effects are included. Many coefficients actually change signs. Moreover, the coefficients on the Percentage of Windfall Profits from Oil to GNP and the Percentage of Windfall Profits from Minerals to GNP are not only positive in every specification, they either approach conventional levels of statistical significance (see

[26] The results of an F-Test for jointly including country dummies in each of these regressions are highly significant (see Table 7), thereby justifying a country fixed effects approach.

specification 3, Table 7), or attain statistical significance (see specification 6, Table 7, specification 3, Table 8).

Does the Evidence Point to a *Resource Blessing*?

One might be tempted to read the results on the Percentage of Windfall Profits from Oil to GNP and the Percentage of Windfall Profits from Minerals to GNP as suggesting that there may be a resource blessing. Specifications 3 and 6 of Table 7, as well as specification 3 of Table 8, indicate that as countries become more dependent on the rents from oil and minerals over time, they become increasingly democratic.

We caution, however, that just as it is dangerous to draw inferences about a Resource Curse from regressions that are sensitive to small changes in specification and functional form, it is dangerous to draw inferences about resource blessings without first addressing possible sources of bias, spurious correlation, or reverse causality. The preeminent concern with dynamic, fixed effects regressions of the sort conducted above is Nickell bias, a byproduct of the time-demeaning of the data that can induce spurious correlations between the dependent variable and the regressors.[27] Moreover, as we pointed out before, when focusing on longitudinal variation one must also be concerned about unit-roots, as non-stationary time-series can also induce this type of spurious correlations.[28] Because the estimated coefficient on the lagged Polity Score is close to unity in each of the estimated regressions that use Windfall Profits from either Oil or Minerals to measure resource dependence, there is reason to believe that the Polity Score series has a unit-

[27] Nickell (1981) showed that for a finite T, the within group estimator is biased due to the correlation between the demeaned, lagged dependent variable and the demeaned, idiosyncratic error term. It is also inconsistent. Indeed, Judson and Owen (1999) find a bias equal to 20 percent of the coefficient of interest when T is as large as 30.

[28] Essentially, when the mean, variance and co-variances of a variable change over time the random errors in period 1 are not, on average, cancelled out in later periods.

root.[29] Finally, as Ross (2006) and Haber (2006) have pointed out, regressions that use GNP or GDP as components of the independent variable that measures resource dependence might suffer from reverse causality: a country's regime type may partially determine either the numerator, the profits it can extract from resources, or the denominator, its Gross National and/or Domestic Product.

To deal with Nickell bias, Arellano and Bond (1991) have developed a Generalized Method of Moments (GMM) estimator in which the equation is first-differenced to eliminate the unobserved, country-specific heterogeneity. A salutary byproduct is that the data is made stationary by first-differencing.[30] More importantly, to obviate Nickell bias, Arrellano-Bond (difference) GMM introduces instrumental variables that are uncorrelated with the idiosyncratic errors. These are the available lagged values, in levels, of the lagged dependent variable, the predetermined independent variables and the endogenous variables.[31] To address concerns about the possible endogeneity between regime type and resource dependence, we can avail the (difference) GMM framework to instrument Windfall Profits from Oil as a Percentage of GNP and Windfall Profits from Minerals as a Percentage of GNP. The exogenous variables in the model are not instrumented and are instead included in the estimation only as differences.[32]

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[29] This suspicion is confirmed by an Augmented Dickey-Fuller Test, which indicates that we *cannot reject* the null hypothesis that the data is non-stationary (specifically, that it follows a Random Walk with drift).The chi^2 statistic is 299.766 with a P value of 0.883.

[30] First-differencing also de-trends the data. We still include year dummies in order to capture any un-modeled shock that may jointly affect all of the countries in year *t*. This has an added advantage: the robust estimates of the standard errors assume no correlation across units in the idiosyncratic errors; year dummies make this assumption more likely to hold.

[31] Predetermined variables are variables in which the error term in *t* has a feedback on future values of the variable variables; endogenous variables are variables that are potentially correlated with both past and present errors.

[32] This approach rests on the assumption that there is no-second order correlation in the first-differenced residuals – although first-order correlation in the idiosyncratic errors is not a problem (Arellano and Bond, 1991, pp. 281-82). In Stata, an Arellano Bond test for second-order autocorrelation is automatically conducted, along with a test-statistic for whether the instruments are valid. We use the Windmeijer's correction with two-step robust standard errors (the only

We therefore estimate an Arrellano-Bond (difference) GMM regression with the following functional form:

$$\Delta \text{Polity}_{it} = \alpha_0 + \alpha_1 \text{year}_2 + \ldots + \alpha_T \text{year}_T + \Phi \Delta \text{Polity}_{it\text{-}1} + \mathbf{B}_1 \Delta \mathbf{X}'_{it} + \mathbf{B}_2 \Delta \mathbf{W}'_{it} + \Delta u_{it} \qquad (7)$$

where Δ is the first-difference operator, $\mathbf{B}_1$ and $\mathbf{B}_2$ are vectors of parameters to be estimated, **X** is a vector of exogenous covariates and **W** is a vector of endogenous covariates, the country specific effects, ν_i, have been wiped out by first-differencing, $\Phi\Delta\text{Polity}_{it\text{-}1}$, ΔWindfall Oil_t ...ΔWindfall $\text{Oil}_{t\text{-}5}$, ΔWindfall Minerals_t ...ΔWindfall $\text{Minerals}_{t\text{-}5}$, lags, and $\Delta\text{income}_{it\text{-}1}$ are included in **W**, and instrumented with their second and higher-lagged levels.

We present the results in specification 4 of Table 8. There is one coefficient of interest: the LRP (the sum of the coefficients on the five lags plus the coefficient of the Windfall Profits measure variable in *t*). For example, a coefficient of -1.0 implies that a permanent one percentage point increase in fiscal reliance five years ago implies a long run fall in Polity Score of one percentage point. The regressions indicate that the potential resource blessing we detected in the dynamic fixed effects regressions run above is spurious: although both the coefficient on the LRP on Windfall Profits from Oil and the coefficient on the LRP on Windfall Profits from Minerals are positive, they are no longer statistically significant. The results are not sensitive to the lag structure: we obtain similar results if we employ just a one-year lag or a single five-year lag. In short, we cannot conclude that there is a Resource Blessing – anymore than we can conclude that there is a Resource Curse.

CONCLUSION

We have adjudicated among three views about the relationship between natural resource dependence and regime types: that natural resource dependence fuels authoritarianism (and

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exception is model 2, where, because of missing data, we only employ a one-step GMM estimator). We note that the Hansen tests never reject the validity of the over-identifying restrictions used in the GMM models and that the second-order auto-correlation of the residuals is always rejected.

precludes democratization); that natural resource dependence favors democratization; and that natural resource dependence is unrelated to regime type.

To do so with an eye to proper causal inference, we developed new datasets to analyze the relationship of resource dependence and regime types within countries over the time. We observe countries prior to their becoming resource dependent, and evaluate whether increasing resource dependence over time affected their regime type – both relative to their level of democracy before resource dependence and relative to the democratization experiences of countries that were similar to them save for resource dependence. Our results indicate that natural resource dependence does not undermine democracy, preclude democratic transitions, or protract democratic transitions. Nor do they indicate that democratization is universally promoted by natural resource dependence.

The results of our longitudinal analyses contradict a large body of scholarship that relies on pooled regression techniques. We wonder why this was the case, and therefore replicate those results and subject them to a series of standard diagnostics. We find that pooling the data comes at a very large cost: omitted variable bias produces spurious inferences; outliers drive regression results; and results are sensitive to the measure of resource dependence on which the researcher chooses to focus.

Taken together, the analysis of long-run time series data and the reexamination of the extant cross-sectional findings indicate that regime types are *not* determined by the presence or absence of natural resource wealth. This is not to say that there may not be cases in which natural resources contributed to the maintenance of an authoritarian regime in a particular time and place—indeed, it would be surprising if this *never* happened. It is to say, however, that the evidence does not support generalizable, law-like statements about the impact of natural resources on regime types. Our evidence also does not support the view that there are conditional resource curse effects that are systematic—at least to the degree that these conditions are captured by differences in resource dependent countries' Polity Scores at the time of resource discovery and development. Some of our time-series cases, such as Chile, Mexico, Ecuador, and Venezuela, had extremely low Polity Scores at the time of their independence from Spain—but they subsequently developed their natural resource sectors and democratized. One of our time series cases, Nigeria, had a relatively high Polity Score right after independence from Great Britain—but it subsequently developed its oil sector and failed to consolidate democracy.

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

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Table 1
Time-Series Regressions in Levels, Dependent Variable is Resource Exporter Regime Type
Polity Score normalized 0-100

Panel A

	Mexico	Venezuela	Ecuador	Norway	Chile	Nigeria	Iran	Syria	Algeria	Yemen
	1821-2003	1821-2003	1830-2000	1821-2000	1818-2000	1960-2003	1800-2000	1944-2000	1962-2000	1926-2000
Fiscal reliance t-1	0.942	1.115	1.528	2.534	0.158	-0.32	0.073	-0.25	0.05	0.109
	[3.52]***	[11.12]***	[20.26]***	[2.36]**	[0.82]	[2.49]**	[2.61]**	[1.62]	[1.27]	[5.45]***
Observations	164	158	170	184	110	41	144	40	33	74

Panel B	Mexico	Venezuela	Ecuador	Norway	Chile	Nigeria	Iran	Syria	Algeria	Yemen
	1900-2003	1920-2003	1900-2000	1900-2000	1900-2000	1960-2003	1947-1999	1946-1999	1962-1999	1947-1999
Fiscal reliance t-1	0.552	0.403	0.497	0.825	0.104	-0.067	-0.189	0.322	-0.036	0.34
	[2.14]**	[7.79]***	[6.70]***	[1.95]*	[0.58]	[0.48]	[1.65]	[1.90]*	[1.36]	[2.29]**
Log(Per Capita Income)	-5.326	-20.43	13.234	-56.328	188.191	94.062	32.947	54.928	-7.445	9.196
t-1	[10.68]***	[6.75]***	[1.46]	[3.78]***	[8.82]***	[5.25]***	[2.44]**	[2.32]**	[1.16]	[2.98]***
Linear time trend	-0.865	-0.027	-2.809	4.04	-3.713	-3.821	-4.568	-6.503	0.182	-0.784
	[2.55]**	[0.03]	[7.56]***	[7.02]***	[4.55]***	[1.73]*	[3.59]***	[3.96]***	[0.36]	[1.90]*
Quadratic time trend	0.015	0.025	0.01	-0.008	0.003	0.072	0.074	0.06	0.006	-0.008
	[4.36]***	[2.90]***	[9.23]***	[3.02]***	[0.95]	[1.67]*	[3.85]***	[5.77]***	[0.63]	[1.15]
Observations	96	68	100	132	67	41	38	38	32	43

t statistics in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%; constant estimated but not reported.

Note: Newey West autocorrelation and heteroskedasticity consistent standard errors used to adjust for AR(1) detected via Arellano Bond AR(1) test, with # of lags included for each time-series: n-1.

Table 2
Long-Run Time-Series Regressions for Major Oil and Mineral Exporters

Dependent Variable is first-differenced Polity Score (all regressors are also first-differenced)

	Five Year Finitely Distributed Lag Model									
	Mexico	Venezuela	Ecuador	Nigeria	Norway	Chile	Iran	Syria	Algeria	Yemen
D. Fiscal Reliance	0.026	-0.037	-0.275	-0.587	-0.026	0.381	-0.03	1.153	0.086	0.046
on Oil or Minerals	[0.17]	[1.38]	[0.82]	[1.43]	[1.03]	[1.50]	[0.49]	[1.09]	[0.77]	[0.42]
LRP	**0.258**	**-0.027**	**-0.852**	**0.544**	**-0.104**	**0.698**	**0.265**	**0.72**	**-0.041**	**-0.317**
F-Test on joint significance of regressor	**0.8**	**0.75**	**1.13**	**0.5**	**0.3**	**0.64**	**0.25**	**0.21**	**0.91**	**0.03**
P-Value on F-Test	**0.57**	**0.61**	**0.35**	**0.8**	**0.94**	**0.7**	**0.96**	**0.96**	**0.51**	**0.99**
Observations	152	134	164	31	177	73	131	17	22	62

* significant at 10%; ** significant at 5%; *** significant at 1%; constant estimated but not reported.

LRP is the Long Run Propensity: the sum of the coefficients on the the five resource dependence lags and its coefficient in t.
All regressions are estimated using robust standard errors.
Adding controls for per capita GDP does not materially affect the results.

Table 3
Time-Series Regressions in Levels

Dependent Variable is Deviation of Resource Exporter Polity Score from Average Polity Score of non-resource exporters.

Panel A

	Mexico	Venezuela	Ecuador	Chile	Nigeria	Iran	Syria	Algeria	Yemen
	1822-2003	1821-1999	1830-2000	1818-2000	1960-2003	1800-2000	1944-2000	1962-2000	1926-2000
Fiscal reliance t-1	-0.457	0.75	0.751	-0.222	-0.322	-0.15	-0.168	0.256	0.168
	[1.98]**	[6.33]***	[17.25]***	[1.17]	[2.31]**	[4.68]***	[3.12]***	[6.90]***	[5.65]***
Observations	165	157	170	110	41	144	40	33	74

Panel B

	Mexico	Venezuela	Ecuador	Chile	Nigeria	Iran	Syria	Algeria	Yemen
	1900-2003	1920-2003	1900-2000	1900-2000	1960-2003	1960-1999	1946-1999	1962-1999	1947-1999
Fiscal reliance t-1	0.261	0.453	0.276	0.035	0.021	-0.016	0.241	0.196	0.289
	[1.42]	[4.45]***	[3.66]***	[0.23]	[0.15]	[0.10]	[1.32]	[4.00]***	[1.78]*
Log(GDP/C) t-1	-4.858	-27.446	61.586	154.203	90.571	25.664	64.118	9.053	16.024
	[22.00]***	[5.17]***	[2.12]**	[6.93]***	[4.34]***	[1.51]	[2.37]**	[0.53]	[2.77]***
Linear time trend	-0.206	1.296	-1.518	-2.038	-2.802	-4.007	-5.531	-0.236	0.76
	[2.19]**	[0.86]	[2.22]**	[11.86]***	[1.14]	[2.85]***	[2.93]***	[0.16]	[1.30]
Quadratic time trend	0.005	0.017	0.005	-0.007	0.036	0.066	0.045	0.005	-0.041
	[4.49]***	[1.17]	[3.12]***	[2.58]**	[0.76]	[3.10]***	[3.58]***	[0.20]	[5.91]***
Observations	96	68	100	67	41	38	38	32	43

t statistics in brackets; constant estimated but not reported, * significant at 10%; ** significant at 5%; *** significant at 1%;
Newey West autocorrelation and heteroskedasticity consistent standard errors used to adjust for AR(1) detected via Arellano Bond AR(1) test, with # of lags included for each time-series: n-1.

Table 4. Treatment Effect of Resource Exportation on Regime Type

Dependent Variable is the first-differenced Polity Score normalized from 0 to 100

	Venezuela Colombia 1830 to 2003	Ecuador Peru 1830 to 2003	Mexico Brazil 1821-2003	Nigeria Tanzania 1961 to 2003	Chile Argentina 1825 to 2003
Resource Exports	0.822	0.412	1.129	-0.307	0.41
	[1.07]	[0.37]	[1.85]*	[0.54]	[0.38]
Resource Producer	-0.471	0.826	-0.294		
	[0.56]	[1.02]	[0.87]		
Constant	0.355	-0.613	0.294	0.214	0.245
	[0.42]	[0.95]	[0.87]	[1.55]	[0.26]
Observations	340	336	332	77	346
R-squared	0.001	0	0.01	0.02	0

robust t statistics in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
All specifications robust to using Driscoll-Kraay standard errors to control for contemporaneous correlation.

Table 5: Cross-sectional OLS Regressions on Pooled Sample of the World
Dependent Variable is Polity in Levels (0-100)

Robust t statistics in brackets

	One Year Lag Structure				Single Five Year Lag Structure			
	(1)	(2)	(3)	(4)	(5)	(7)	(6)	(8)
	1972-1999	1972-99	1970-1999	1970-1999	1972-1999	1972-99	1970-1999	1970-1999
Polity Score *t-1*	0.946	0.948	0.946	0.953	0.952	0.944	0.942	0.941
	[108.08]***	[103.30]***	[126.21]***	[117.39]***	[120.74]***	[91.81]***	[104.61]***	[87.38]***
Fuel Exports over GDP	**-0.05**				**-0.053**			
	[2.66]* **				**[2.59]** **			
Mineral Exports over GDP	**-0.072**				**-0.06**			
	[2.45] **				**[2.35]** **			
Percent of Government Revenues from Oil and Minerals		**-0.029**				**-0.028**		
		[2.71]* **				**[2.16]** **		
Windfall Profits from Oil over GNP			**-0.05**				**-0.062**	
			[2.28] **				**[3.17]*** **	
Windfall Profits from Minerals over GNP			**-0.007**				**-0.004**	
			[0.34]				**[0.21]**	
Per Capita Windfall Profits from Oil				**0**				**0**
				[0.07]				**[1.15]**
Per Capita Windfall Profits from Minerals				**0**				**0**
				[0.49]				**[0.52]**
log(Per Capita Income) *t-1*	1.522	1.216	1.194	1.004	1.273	1.217	1.276	1.11
	[4.03]***	[3.56]***	[3.51]***	[3.10]***	[3.56]***	[3.16]***	[3.25]***	[2.72]***
Percentage Muslim	-0.023	-0.02	-0.02	-0.016	-0.018	-0.026	-0.022	-0.025
	[2.47]**	[2.43]**	[2.76]**	[2.48]**	[2.23]**	[2.88]***	[2.92]***	[2.92]***
High Income OECD dummy	-0.272	-0.089	0.521	0.6	-0.226	-0.127	0.303	0.788
	[0.46]	[0.14]	[0.88]	[1.11]	[0.39]	[0.19]	[0.45]	[1.28]
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2063	2415	3044	2376	1933	2020	2560	2013
Number of countries	122	113	132	101	107	111	132	100
R-squared	0.97	0.96	0.96	0.97	0.97	0.96	0.96	0.96
W test (for skewness) on oil	0.477	0.841	0.592	0.233				
z statistic	16.57***	14.16***	17.78***	18.022***				
W test (for skewness) on minerals	0.367		0.303	0.18				
z statistic	17.09***		19.17***	18.18***				

* significant at 10%; ** significant at 5%; *** significant at 1%; Note: W test (for skewness) for Percent of Govt. Rev. from Oil and Minerals is listed under W test on oil.
Constant and year dummies estimated but not reported. Regressions estimated with Driscoll-Kraay Standard Errors.

Table 6: Is the Resource Curse a Product of Outliers?
Dependent Variable is Polity in Levels (0-100)

Robust t statistics in brackets

	One Year Lag Structure, Logged Variables			Five Year Lag Structure, Logged Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
	1972-1999	1972-99	1970-1999	1972-1999	1972-99	1970-1999
Polity Score *t-1*	0.95	0.951	0.947	0.954	0.947	0.943
	[116.88]***	[107.60]***	[126.21]***	[135.27]***	[95.19]***	[102.41]***
Log (Fuel Exports over GDP)	**-0.037**			**-0.12**		
	[0.44]			**[1.28]**		
Log (Mineral Exports over GDP)	**-0.088**			**-0.158**		
	[0.8]			**[1.92]***		
Log (Percent of Gov't Revenues from Oil and Minerals)		**-0.066**			**-0.04**	
		[0.83]			**[0.48]**	
Log (Windfall Profits from Oil over GNP)			**-0.141**			**-0.147**
			[2.10]**			**[1.74]***
Log (Windfall Profits from Minerals over GNP)			**0.071**			**0.08**
			[0.97]			**[1.11]**
log(Per Capita Income) *t-1*	1.224	0.941	1.081	1.009	0.923	1.114
	[3.77]***	[3.09]***	[3.49]***	[3.23]***	[2.72]***	[2.90]***
Percent Muslim	-0.025	-0.024	-0.022	-0.021	-0.03	-0.027
	[2.63]***	[2.80]***	[3.27]***	[3.14]***	[3.39]***	[3.58]***
High Income OECD dummy	0.288	0.329	0.826	0.721	0.356	0.675
	[0.61]	[0.53]	[1.73]*	[1.52]	[0.52]	[1.13]
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2063	2415	3044	2090	2020	2433
Number of groups	122	113	132	102	111	132
R-squared	0.96	0.96	0.96	0.97	0.96	0.96

* significant at 10%; ** significant at 5%; *** significant at 1%
Constant and year dummies estimated but not reported. Regressions estimated with Driscoll-Kraay standard errors.

Table 7: Country Fixed Effects Regressions on Sample of the World to Control for Unobserved Heterogeneity
Dependent Variable is Polity in Levels (0-100)

Robust t Statistics in brackets

	One Year Lag Structure			Single Five Year Lag Structure		
	(1)	(2)	(3)	(4)	(5)	(6)
	1961-1999	1972-1999	1970-1999	1961-1999	1972-1999	1970-1999
Polity Score *t-1*	0.902	0.876	0.878	0.901	0.864	0.865
	[63.24]***	[42.07]***	[57.55]***	[57.55]***	[34.98]***	[44.17]***
Fuel Exports over GDP	**0.035**			**-0.023**		
	[1.45]			**[0.95]**		
Mineral Exports over GDP	**0.04**			**0.064**		
	[0.54]			**[1.03]**		
Percent of Govt. Revenues from		**-0.009**			**-0.013**	
Oil and Minerals		**[0.33]**			**[0.61]**	
Windfall Profits from Oil			**0.042**			**-1.101**
over GNP			**[1.90]***			**[1.57]**
Windfall Profits from Minerals			**0.119**			**0.1**
over GNP			**[1.95]***			**[2.53]****
log(per capita income) *t-1*	0.646	-0.303	-0.651	-0.663	-0.081	0.016
	[0.65]	[0.47]	[1.19]	[0.61]	[0.10]	[1.01]
Country Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2551	2415	3044	2175	2020	2560
Number of countries	122	113	132	107	111	132
R-squared	0.97	0.96	0.96	0.97	0.96	0.96
F-test for Country Fixed Effects	18.35	11.02	23.61	17.05	5.93	801.91
P-Value	0	0	0	0	0	0

* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions estimated using Driscoll Kraay standard errors.
Country dummies, year dummies and constant estimated but not reported.

Table 8: Country Fixed Effects Regressions on Sample of the World
Dependent Variable is Polity in Levels (0-100)

Five Year Finitely Distributed Lag Model

Robust t statistics in brackets

	Country Fixed Effects Model			Arrellano-Bond GMM
	(1)	(2)	(3)	(4)
Polity Score *t-1*	1961-1999	1972-99	1970-1999	1972-99
	0.899	0.862	0.863	0.875
Fuel Exports over GDP	[46.26]***	[34.45]***	[43.38]***	[28.27]***
	-0.019			
Mineral Exports over GDP	[0.42]			
	-0.158			
Percent of Govt. Revenues	[1.11]			
from Oil and Minerals				
Windfall Profits from Oil over GNP				
			0.019	0.104
Windfall Profits from Minerals over GNP			[0.40]	[1.73]*
		-0.064	-0.22	-0.22
		[0.80]	[1.08]	[0.72]
LRP for Oil Dependence				
F-Test on Joint Significance 5 lags	**0.008**	**-0.05**	**0.077**	0.08
P-Value	**1.01**	**0.59**	**3.56**	0.95
LRP for Mineral Dependence	**0.42**	**0.74**	**0.003*** **	0.46
F-Test on Joint Significance 5 lags	**0.336**		**0.115**	0.027
P-Value	**0.62**		**1.48**	1.05
	0.714		**0.19**	0.394
log(Per Capita Income) *t-1*	-0.977	0.396	-1.379	1.255
	[0.64]	[0.41]	[1.72]*	[0.19]
Country Dummies	Yes	Yes	Yes	No
Year Dummies	Yes	Yes	Yes	Yes
Observations	1791	1918	2540	2260
Number of countries	94	110	131	129
R-squared	0.97	0.96	0.96	

LRP is the Long Run Propensity Score: the sum of the coefficients on the the five resource dependence lags and its coefficient in *t*; Note: the LRP for the Percent of Govt. Revenues from Oil and Minerals is reported under the LRP for Oil Dependence.
Country dummies, year dummies and constant estimated but not reported.
Models 1-3 estimated with Driscoll Kraay standard errors and Model 4 with robust standard errors.