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Do Natural Resources Fuel Authoritarianism? A Reappraisal of the Resource Curse

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Abstract: A large body of scholarship finds a negative relationship between natural resources and democracy. Extant cross-country regressions, however, assume random effects and are run on panel datasets with relatively short time dimensions. Because natural resource reliance is not an exogenous variable, this is not an effective strategy to uncover causal relationships. Numerous sources of bias may be driving the results, the most serious of which is omitted variable bias induced by unobserved, country-specific and time-invariant heterogeneity. To address these problems we develop unique historical datasets, employ time-series centric techniques, and operationalize explicitly specified counterfactuals. We test to see if there is a long-run relationship between resource reliance and regime type within countries over time, both on a country-by-country basis and across several different panels. We find that increases in resource reliance are not associated with authoritarianism. In fact, in many specifications we generate results that suggest a resource blessing.

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Introduction

A substantial political economy literature argues that economic and fiscal reliance on petroleum, natural gas, and minerals helps create and perpetuate authoritarian political regimes. The genesis of this idea can be found in Mahdavy (1970), who noted that petroleum revenues in Middle Eastern countries constituted an external source of rents directly captured by governments, thereby rendering them unaccountable to citizens. Other scholars then built upon Mahdavy (1970) to postulate a general law about natural resource rents and authoritarianism. Luciani (1987), for example, avers that: "The fact is that there is 'no representation without taxation' and there are no exceptions to this version of the rule." Huntington (1991: 65) then popularized this idea: "Oil revenues accrue to the state: they therefore increase the power of the state bureaucracy and, because they reduce or eliminate the need for taxation, they also reduce the need for the government to solicit the acquiescence of the public to taxation. The lower the level of taxation, the less reason for publics to demand representation."

The idea that there is a causal relationship between natural resource reliance and authoritarianism underpins a broad and influential literature. This includes a plethora of country case studies, policy papers produced by multilateral aid organizations, popular books on world politics and economics, and articles in the mass media that make sweeping claims, such as the existence of a "first law of petropolitics" (Friedman 2006). The view that natural resources and democracy do not go together is often coupled with parallel literatures that find correlations between natural resources and slow economic growth or the onset of civil wars. Taken together, these three literatures have given rise to the stylized fact that there is a "resource curse."

Beginning with a seminal paper by Ross (2001), numerous scholars have employed cross-country regression frameworks to examine the hypothesis that oil, gas, and minerals cause authoritarianism. Although the details vary, the vast majority of the literature produces results that are consistent with the hypothesis (e.g., Wantchekon 2002; Jenson and Wantchekon 2004; Smith 2007; Ulfelder 2007; Papaioannou and Siourounis 2008; Goldberg, Wibbels, and Myukiyehe 2008; Askalen 2009; Ramsey

2009; Ross 2009). A considerably smaller literature either finds against the hypothesis (Herb 2005), or finds that the effect of natural resources on regime type is conditional on other factors (Dunning 2008).

The researchers who find evidence that ostensibly supports the resource curse have not yet provided compelling tests of the hypothesis that natural resources cause authoritarianism. Neither, however, have the skeptics produced compelling results to the contrary. The fundamental issue is that the resource curse is about a dynamic, time-series process that requires the specification of a counterfactual: the discovery, production, and export of natural resources is hypothesized to distort a country's regime type, putting it on a different path of political development than it would have otherwise followed. The empirical tests that have been used to test the resource curse hypothesis, however, do not tend to employ time series centric methods, nor specify counterfactual paths of political development. Instead, they tend to compare resource-reliant countries to resource-poor countries.

When using observational data there is, of course, a big difference between finding a correlation between two variables and demonstrating that the relationship is causal. It is particularly problematic to infer causality when the correlation is produced by a technique that primarily exploits variance between countries. It would not take lengthy argumentation to demonstrate that there are fundamental differences between countries, and that these differences may be correlated with both the dependent and independent variables that researchers are introducing into their regressions. This is an inconvenient, but ubiquitous, feature of observational data when country-years are the unit of analysis. It implies that, unless a researcher is certain that the dependent and independent variables are uncorrelated with countries' unobserved differences, it is not appropriate to estimate regressions that pool the data or employ random effects. There is a strong likelihood that the results generated by such approaches will be driven by omitted variables that are time-invariant and country-specific.

This problem besets much of the resource curse literature. To put it concretely, the assumption behind the majority of the regressions in the resource curse literature is that had Venezuela not become oil reliant, it would have developed the same political institutions as Denmark, controlling for other covariates. It is hard to believe, however, that endemic, time-invariant institutions that are not captured

by covariates such as GDP per capita, and the population share that is Muslim, do not differentiate these countries. Moreover, these persistent, unspecified differences define the possible set of political institutions, and the possible set of economic sectors, which emerge and survive (Acemoglu et. al. 2008). This includes the resource sector. As some researchers have pointed out, a country's resources, whether measured as stocks or flows, are not exogenous: they are determined by underlying legal and cultural institutions (e.g., David and Wright, 1997; Norman 2009).

There are any number of factors that might jointly determine resource reliance and authoritarianism. Permit us to provide just one example. Rulers who have inherited inveterately weak states tend to have pressing fiscal needs and short time horizons; they may therefore *choose* to search for resources and/or extract them at high rates today to obtain the rents needed for political survival, rather than save those resources for tomorrow. Indeed, as Manzano and Monaldi (2008) point out, world oil reserves happen to be concentrated in precisely those countries with weak state capacity—and as any number of case studies have shown, weak state capacity *preceded* the discovery of oil or other minerals in those countries (e.g., Haber, et. al. 2003). Given that countries' underlying institutions are also correlated with their regime types (Acemoglu et. al. 2008), it is likely that inveterately weak state capacity jointly determines authoritarianism and high levels of resource reliance.[1] Unfortunately, there is no consensus metric to operationalize "state capacity" across countries and time, let alone a metric that is exogenous. Moreover, there are likely to be several such unobserved factors that confound correlations between resource dependence and authoritarianism. The implication, we hope, is clear: lest the results be biased by omitted variables, time invariant, country specific factors have to be expunged.

There are a number of techniques available to control for unobserved country heterogeneity, but one technique in particular—looking at variance within countries over time—gives researchers the

[1] This is also true of country populations, the denominator usually used to normalize resource reliance. As Culter et. al. (2006) and Soares (2007) show, a country's persistent institutions determine the size and rate of growth of its population, even after controlling for its GDP.

flexibility to simultaneously address other factors that may also produce biased estimates. The core of our approach is to employ time-series centric methods that evaluate the long-run effect of resource reliance on regime types. We carry out this analysis using both a country-by-country time-series approach, as well as a dynamic panel framework with country fixed effects. In order to do this, we construct original datasets whose time-series dimension extends back to the period before countries became reliant on natural resources: our panel covers 1800 to 2006 and includes 168 countries. To ensure that our results are robust we construct four different measures of natural resource reliance and employ the two most popular measures of regime type used in the literature. In order to fully exploit the time series dimension of the data and avoid generating spurious correlations we: diagnose the stationarity characteristics of both our resource reliance measures and regime type; perform cointegration tests to know if there is actually a structural relationship between these variables; and employ error correction mechanism models to estimate their long-run, dynamic behavior.

Focusing on the relationship between natural resource reliance and regime types within countries over the long run also allows us the flexibility to address other issues that may confound causal inference. First, if there are good theoretical priors about factors that may condition the effect of an independent variable on the outcome of interest, the regressions need to go beyond simply estimating the average effect. One must model those conditional effects—not assume that both the direction and magnitude of the coefficient is uniform across countries and time. Do natural resources always give rise to authoritarianism, or only under sometimes? To answer this question we employ split-sample techniques. We group countries by their level of per capita income, income inequality, threshold levels of resource reliance, time periods, and regions and then estimate separate regressions on those subsamples.

Second, another common problem in drawing causal inferences is the specification of the counterfactual outcome. What would have happened had a particular country not been exposed to the treatment variable of interest? One technique that researchers use to address this problem is a difference-in-differences estimator. Focusing on variance within countries over time also allows us to employ such an approach, but we differ from typical applications: we develop a technique that is suited for estimating

the effect of a continuous treatment variable. First, we specify the counterfactual path that a resource-reliant country's regime type would have followed in the absence of those resources, on the basis of the path followed by the non-resource reliant countries in its geographic region. Second, we compare that counterfactual path to the actual path. Third, we see whether any divergence between the actual and counterfactual paths of political change correlates with increases in resource reliance. If one wanted, for example, to specify the counterfactual path that would have been followed by oil and gas rich Kazakhstan had it not discovered those resources, the best approximation would be the other Central Asian Republics that have not emerged as major resource producers (e.g., Uzbekistan)—but which share Kazakhstan's history of repeated invasions and occupations, as well as broad geographic and cultural characteristics.

Last, researchers have to be certain that their results are not biased by reverse causality. Do natural resources fuel authoritarianism, or is it the other way around? Might it be the case that the only economic sectors that yield rates of return high enough to compensate for expropriation risk in authoritarians states are oil, gas, and minerals, thereby engendering resource reliance (Haber 2006)? We therefore create several instruments based on countries' proven oil reserves that have both time series and cross sectional variance in order to estimate instrumental variables regressions with country fixed effects.

When we address all of these potential sources of bias we find that there is not a causal relationship between natural resources and authoritarianism. In fact, simply controlling for unobserved unit heterogeneity by looking within countries over time makes the well-known resource curse results disappear. Indeed, to the degree that we detect any statistically significant relationships that survive our battery of specifications designed to improve causal inference, they point to a resource blessing: increases in natural resource income are associated with increases in democracy. The weight of the evidence indicates that scholars might want to revisit the idea of a general law known as the resource curse.

II. Literature Review

We are not the first researchers to have noted that the techniques employed in the resource curse literature may yield biased results. Indeed, resource curse researchers have become increasingly aware of the problems of drawing causal inferences from observational data. They have, however, attempted to

mitigate these problems in a piecemeal fashion. In addition, even the most sophisticated attempts to date to address sources of bias individually do not always reflect econometric best practice.

Aslaken (2009) provides the best attempt to date to address unit heterogeneity bias by employing a dynamic panel model. Her approach, however, introduces a range of new problems that she does not adequately resolve. First, because the time dimension of her dataset (1972-2002) is only 30 years, she has to be concerned about Nickell Bias (correlation between the lagged dependent variable(s) and the unit fixed-effects). She therefore employs a Generalized Methods of Moments (GMM) System approach. Her estimation strategy is to introduce a one-year lag of the dependent variable and independent variables, as well as the typical instruments: the lagged levels of the lagged dependent variable and its lagged differences. This is problematic on a number of grounds. She chooses this "Dead Start" model without empirically verifying whether this particular dynamic structure (one period lags of the dependent and independent variables) is warranted. This decision potentially imposes invalid restrictions on the structure of the data that may bias the results (Debouf and Keele, 2008). Second, although a System GMM estimator is designed to estimate models with data in levels that are highly persistent, this is not a license to neglect the evaluation of the time series properties of the data. In particular, Askalen does not evaluate whether her data are non-stationary—even though high persistence strongly suggests unit roots—and then take the proper steps to estimate relationships in light of this fact. Third, as Bun and Wendmeijer (2007) have shown, the System GMM estimator suffers from a weak instrument problem, making results unreliable. Finally, when estimating regressions that are centered on "within variance," one has to be concerned about bias that may be introduced by measurement error. Aslaken mitigates measurement error by abandoning yearly data as the unit of observation. She instead employs five-year averages. Unfortunately, by compressing the time dimension of the data into only six periods, Aslaken foregoes the opportunity to model adequately the time-series relationship between oil and democracy.

Herb (2005) gains considerable traction on the specification of historically plausible counterfactuals for resource-reliant countries in order to better isolate the effect of resources on regime types. He reasons that resource-reliant countries would have been substantially poorer had they not found

oil, gas, or minerals, and that their lower GDP's would have caused them to be less democratic. He therefore estimates what their GDP would have been in the absence of these resources, and then estimates their level of democracy at those lower, counterfactual levels of GDP. This is, however, only a partial solution because it ignores the dimension of time. A more powerful approach is to specify the alternative political trajectories that resource-reliant countries would have followed in the absence of increasing resources, compare those counterfactual trajectories to their actual trajectories, and thereby control for the other changes that the resource-reliant cases underwent during exposure to those resources.

Dunning (2008) provides the best attempt to date to address the possibility of conditional effects. He theorizes that when a society has a highly unequal distribution of income, natural resource wealth permits democratization because elites do not fear redistribution by the enfranchisement of the poor; conversely, when the distribution of income is more equal natural resource wealth reinforces authoritarian regimes because leaders do not face demands for redistribution, and therefore can deploy the rents from resources to buy off or coerce opponents. He therefore introduces to the typical random effects specification with resource reliance as the independent variable, a measure of inequality and an interaction of inequality with resource reliance. These regressions, however, can be critiqued for employing a measure of inequality (the capital share of non-oil value added) that omits the oil sector. This potentially causes him to overestimate the share of income that is earned by labor in oil-rich countries that have undiversified economies (e.g., the Middle East). These regressions may therefore be picking up a fixed effect associated with undiversified oil economies. There are also other theoretically relevant conditional effects for which Dunning's seminal book does not search.

Ramsey (2009) provides the most convincing attempt to address endogeneity bias by instrumenting oil income with out-of-region natural disasters, reasoning that if a tsunami hits Malaysia, for example, it increases oil income in the rest of the world's producers without affecting their regime type through any other channel. Several concerns, however, cast doubt on Ramsey's findings. First, he makes the strong assumption that his instrument both addresses endogeneity and unit heterogeneity bias, and therefore does not introduce country fixed effects. This assumption is particularly problematic

because a short-term shock to oil prices will likely be offset by an immediate increase in oil production by a few big producers with substantial excess capacity before any increase in oil prices materializes. In point of fact, Saudi Arabia, the world's largest producer, seeks as a matter of policy to create a stable world oil market by manipulating output to offset shocks. In short, Ramsey's instrument may be picking up a "big producer" fixed effect—a conjecture that is warranted given the fact that his instrument is rendered weak when the sample excludes the Middle East producers.

III. Research Design

Measuring Regime Types

Our primary measure of regime type is the standard measure of democracy employed in the resource curse literature—the Combined Polity 2 score, an index of the competitiveness of political participation, the openness and competitiveness of executive recruitment, and the constraints on the chief executive that is coded for every country in the world since 1800 (Marshall and Jaggers 2008). For simplicity, we refer to this measure as *Polity*. In order to make the regression coefficients easier to interpret, we normalize Polity to run from 0 (complete autocracy) to 100 (complete democracy). Some researchers have argued that democracy is best measured as a binary variable. We therefore also employ a widely used binary measure of democracy known as *Regime* (Przeworski et al. 2000). Our Regime measure extends from 1800 to 2002 (See Appendix on Sources and Methods).[2]

Measuring Oil and Mineral Dependence

We construct four different measures of resource dependence. We choose these measures by following precedents in the literature, but we go beyond the literature by expanding their coverage back in time (typically back to independence, but for some countries back to 1800 or 1900, depending on the variable). A full discussion of the sources and methods used to estimate these variables can be found in our separate appendix on sources and methods.

[2] This appendix is included in our submission to the journal. At time of publication we will post this appendix to the web.

The resource curse literature claims that the causal mechanism that links oil and minerals to regime types is the rents captured by governments from oil, gas, and mineral production, which allow them to become "rentier states" that are financed without taxing citizens. We therefore follow Mahdavy (1970) and Herb (2005) by constructing a measure of Fiscal Reliance on Resource Revenues, the percentage of government revenues from oil, gas, or minerals. For the sake of simplicity, we refer to this variable throughout the paper as *Fiscal Reliance*. Unlike Mahdavy (1970), who only covers a few years in the 1950s and 1960s for a small group of Mideast countries, and Herb (2005) who truncates his coverage to mostly the major producers during the period 1972-1999, we provide coverage of Fiscal Reliance from a country's first year of independence (or 1800) to 2006, allowing us to observe countries before and after they became oil, gas, or mineral producers.

There is one practical disadvantage to our time series approach to this measure: the retrieval and standardization of fiscal data extending back to the nineteenth century is not an enterprise characterized by economies of scale. We therefore truncate our coverage of Fiscal Reliance with respect to the number of countries by focusing on large producers that demonstrate variance in Polity (see appendix on sources and methods for details about the selection criteria). We code Fiscal Reliance for 18 countries: sixteen oil and gas producers and two of the world's major copper producers. The oil and gas producers are Mexico, Venezuela, Ecuador, Trinidad and Tobago, Nigeria, Angola, Indonesia, Iran, Algeria, Bahrain, Equatorial Guinea, Gabon, Yemen, Oman, Kuwait, and Norway. The copper producers are Chile and Zambia.

We also estimate regressions on Total Oil Income Per Capita (barrels produced, divided by population, multiplied by the real world price, expressed in thousands of 2007 dollars). For the sake of simplicity, we refer to this variable as *Total Oil Income*. Total Oil Income is a theoretically second-best metric compared to Fiscal Reliance: it measures the income earned by a country from crude oil, not the rents garnered by the government from that income. We employ it, however, for two reasons. First, it has emerged as standard measure in recent work on the resource curse (e.g., Dunning 2008; Aslaken 2009; Ramsey 2009; and Ross 2009). Second, it affords broad time series and cross-sectional coverage. Unlike the literature to date, which truncates coverage to the period since 1960, we begin coding in 1800

and cover 168 countries (104 display positive values) until 2006. Our first positive values are in 1861, just after the United States and Romania sank the world's first commercial oil wells.

We also develop two additional measures of resource reliance—*Total Fuel Income* (oil, natural gas, and coal, divided by population, expressed in thousands of 2007 dollars) and *Total Resource Income* (oil, natural gas, coal, precious metals, and industrial metals, divided by population, expressed in thousands of 2007 dollars). These measures are based on a measure frequently employed in the literature, the Hamilton and Clemens (1999) Mineral Depletion variable (e.g., Ulfelder 2007, Dunning 2008, and Aslaksen 2009). Our measures differ from theirs in multiple respects, the most salient of which is longitudinal coverage: we estimate our measures back to 1900, instead of 1960, as is standard in the literature (see Appendix on Sources and Methods for a more complete discussion).

Control Variables and Instrumental Variables

In the unrestricted specifications that follow we introduce a battery of variables to control for other determinants of regime type, such as per capita income, global and regional democratic diffusion effects, and civil war. We discuss those controls as we deploy them below. We also instrument for Total Oil Income with several measures based on oil reserves in order to control for possible endogeneities. We discuss those instruments when we deploy them below. For a discussion of the sources and methods used to develop the control and instrumental variables see our Appendix on Sources and Methods.

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 adduced by inspecting and graphing the data for the 168 countries in our dataset. Our goal is to group the countries according to whether they appear to be resource cursed, with an eye to biasing these findings in favor of the resource curse hypothesis. To group the countries, we take four steps. First, we decide whether they are resource reliant based on their fiscal reliance on resource revenues. We note that a poor, authoritarian government may obtain a significant share of its revenues from natural resources, even if the country produces trivial quantities of those resources in an absolute sense. Second, we set the threshold for resource reliance at a relatively low level:

an average of five percent during the period 1972-1999 (for the details see Appendix on Sources and Methods). This procedure yields a set of 56 resource-reliant countries. We note that our criteria exclude resource-rich, mature democracies (e.g., the United States, Canada, Australia, and Great Britain) while including authoritarian countries that produce trivial quantities of oil, gas, and minerals (such as Belarus, Tajikistan, Egypt, and Morocco). Third, we graph each country's Polity series and its Total Resources Income series; for the 18 countries for which we have Fiscal Reliance data, we also graph that series. Fourth, we group the countries by whether they appear to be blessed or cursed by resources.

We present the patterns revealed by this process in Table 1. Twenty-two of the countries appear to be resource blessed. This includes six countries that remained democratic after they experienced a resource boom (democratic means that Polity is 85 or above, following Gleditsch and Ward 2006); another eight that transitioned to democracy during a resource boom; two that were near-democracies (Polity was 80) before they experienced a resource boom, and remained at that level during the boom; and six that were autocratic before they experienced a resource boom, and then saw at least a one-standard deviation improvement in Polity (25 points, calculated from the "within" variation) during that boom.

In order to give readers a sense of what the data for these resource-blessed countries looks like, we present the graphs for Trinidad and Tobago, Mexico, and Angola (see Data Analysis Appendix for all 168 graphs). Figure 1 reveals that Trinidad and Tobago was democratic at independence, in 1962. Even though Fiscal Reliance and Total Resource Income increased dramatically in subsequent years—indeed, Trinidad has one of the highest levels of Resource Income Per Capita in the world—Polity continued to tick upwards, reaching the maximum score of 100 in the 1990s. Figure 2 reveals that Mexico had two distinct natural resource booms, the first running from 1900 to 1924; the second began in 1974 and is still ongoing. It is striking that when Mexico's first resource boom ended, after it had depleted its oil reserves given the technology of the time (Haber et. al. 2003), Polity did not increase, as predicted by the resource curse theory. Instead, Mexico saw the heyday of single party rule. It is also striking that Mexico's second natural resource boom was followed by democratization. In 2000, when the PRI lost its grip on power, Fiscal Reliance had increased four-fold since the 1960s (to 23 percent) and Total Resource Income

had increased six-fold, to $478 per capita. By 2006, when Mexico held a second free and fair election, Fiscal Reliance and Total Resource Income were even higher: 37 percent and $871 per person, respectively. Figure 3 presents the data for Angola. Although its Polity score does not reach the democracy threshold, Fiscal Reliance, Total Resource Income, and Polity are all increasing together.

A case can be made for a potential resource curse on the basis of the graphed data in only ten of the 53 countries. This includes two countries in which democracy failed during resource booms; two countries that were already autocratic, but became more so during a resource boom (based on a decrease in Polity of at least one standard deviation, as above); two countries that were autocratic and resource reliant, but which then democratized during a period in which their resource reliance declined; and four autocratic countries that became less so (based on the one standard deviation rule, as above) during a resource boom. We present the graph for the strongest case for a resource curse—Zambia—in Figure 4. Zambia was autocratic and heavily reliant on copper in the 1960s and early 1970s. Its copper revenues then steadily declined. By 1991, Zambia's fiscal reliance on copper revenues had plummeted to six percent. In that same year, its Polity score increased 16-fold, and remained relatively high afterward.

What are we to make of the remaining 21 cases? Two of them display no discernable pattern. The remaining 19 are cases that were autocratic *prior* to the discovery of natural resources, and remained autocratic after they experienced a resource boom. An aggressive interpretation would count these 19 as resource cursed, on the assumption that they would have democratized had it not been for their natural resource reliance. In that case, we would have 22 potentially resource-blessed and 29 potentially resource-cursed countries. However, grouping the countries in this way requires that one set aside a few inconvenient facts. Twelve of the 19 cases are clustered in a single geographic region of the world—the Middle East and North Africa (MENA)—that has a long history of tribally organized societies, foreign conquest (beginning with the Sassanid Empire, followed by the Ottomans, and ending with British protectorates), and authoritarian government. Indeed, most had been kingdoms, sheikdoms, or imamates for centuries before they found oil. Moreover, their neighbors, Jordan and Syria, share these same historical legacies, but importantly not their natural resource wealth—and they are not democracies either.

This suggests that resources were not the decisive factor shaping the political trajectories of the other 12. A similar pattern holds if we posit Yemen as the appropriate comparison: prior to its discovery of (quite modest levels) of oil and gas in 1980 it too was a long-lived autocracy. Much the same is true about the histories of the resource rich, former Soviet States of Central Asia (Kazakhstan, Turkmenistan, and Tajikistan)—and, as in MENA, their non-resource-reliant neighbors (e.g. Uzbekistan) are not democratic either. In short, unless one ignores the fact that these resource rich countries were autocratic well-before they discovered oil, and that their non-resource-reliant neighbors remained autocracies as well, the potentially resource-blessed countries outnumber the resource-cursed countries by a ratio of approximately two-to-one.

Country-by-Country Time Series Analysis

Do the patterns described above actually represent causal relationships? To gain traction on this question we must employ multivariate analysis. We begin with the theoretically most appropriate independent variable, Fiscal Reliance, and evaluate its time-series relationship with Polity on a country-by-country basis for 18 major oil and mineral producers. We note that the time-series variation displayed by both of these series is quite high (see Data Analysis Appendix for summary statistics).[3]

Unit Root and Co-integration Tests

The resource curse is a theory about variables that should be expressed in levels: higher levels of natural resource reliance within countries over time are purported to induce lower levels of democracy. In estimating time series regressions in levels, however, researchers must be certain that they are not drawing spurious inferences. In particular, they need to know whether 1) the series of interest are individually integrated (non-stationary in levels but stationary in first-differences) and, if so, if 2) they are together cointegrated. We therefore performed Augmented Dickey Fuller (ADF) unit root tests on Polity

[3] This appendix is included in our submission to the journal. We will later post this appendix to the web.

and Fiscal Reliance in both levels and differences, respectively (see data analysis appendix).[4] We find that only two of the 18 cases have Polity series for which we can reject the null hypothesis that the series is non-stationary in levels: Nigeria (at 10 percent) and Iran (at 5 percent). We can, however, reject the null hypothesis that the Polity series is non-stationary in differences for all 18 countries with the highest level of confidence. We also find that only three of the 18 cases have Fiscal Reliance series for which we can reject the null hypothesis that the series is non-stationary in levels with a high level of confidence (Bahrain, Algeria, and Zambia). In addition, we can reject the unit root hypothesis for Chile, but only at the ten percent level of confidence. When we first difference Fiscal Reliance, however, we find that the null hypothesis can be rejected for all 18 cases.

When the data series is integrated of order 1, as is the case here, there is a high probability that any correlation between them in levels is spurious (Granger and Newbold 1974). It is only when there is evidence of cointegration between non-stationary variables in levels (the data series capture a long-run, equilibrium relationship—permanent changes in the independent variable consistently drive the dependent variable to new levels) that we can be confident that their time-series correlation is structural.

We follow Kanioura and Turner (2005), who have developed a method to detect cointegration from the same regression used to model the long-run, dynamic relationship between variables expressed in levels.[5] We employ an Error Correction Mechanism (ECM) framework and conduct F-tests of cointegration on the lagged dependent and independent variables in levels.[6] The ECM models both the

[4] To choose the lag length of the dependent variable we use a standard t test. Our results, however, are robust to different lag selection methods, such as the BIC statistic—and to the inclusion of a time trend.

[5] See the Data Analysis Appendix for the traditional Engle and Granger (1987) two-step residual based cointegration tests based on the residuals from a regression in levels of Polity against Fiscal Reliance.

[6] Engle and Granger (1987) prove that if there is a linear combination of two non-stationary series that is itself stationary, then these series are cointegrated, and their long-run relationship can be estimated via an ECM. Based on this fact, Kanioura and Turner (2005) generate critical values (from a non-normal

long run, total impact on Polity made by a permanent change in the level of Fiscal Reliance (the coefficient on the Long Run Multiplier, the LRM), as well as any short-run effects. Moreover, it is an ideal way to estimate the dynamic relationship between Fiscal Reliance and Polity *regardless* of whether the series in levels are stationary or not: it does *not* impose *a priori* restrictions on the dynamic relationship between dependent and independent variables that are possibly invalid (DeBoef and Keele 2008). So, even if the data in levels are stationary, ECM is the best approach.

We therefore estimate a time-series regression that can be expressed as follows:

$$\Delta Y_t = \Delta Y_{t-1}\rho_0 + \Delta X_t\beta_1 + \Delta X_{t-1}\beta_2 + \ldots + \Delta X_{t-k}\beta_k + \delta(Y_{t-1} - X_{t-1}\gamma) + u_t \quad (1)$$

where Y is Polity and short-run changes in Y that take a year's time to elapse are captured by the coefficients on the differenced independent variable (Fiscal Reliance); and increases in X produce a change in Y that disrupts the long-term, equilibrium relationship between the level of X and level of Y. Therefore, Y will respond by gradually returning to the path traced by the level of X, registering a total change equal to γ. The δ term is < 0, and is the error correction rate: a δ proportion of this discrepancy (or "error") is corrected by a movement in the dependent variable each subsequent period. Therefore, the LRM is the total effect that an increase in Fiscal Reliance has on Polity spread over future time periods.[7]

In order to be certain that our results are not driven by the choice of the lag length of the differenced independent variable, or the addition of conditioning variables, we perform a set of experiments on each country, the results of which are reported in our online data analysis appendix. We begin with a simple bivariate ECM with no lags of Fiscal Reliance in first differences. We then sequentially add from one to five finite lags of Fiscal Reliance in first differences. Finally, we estimate a

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distribution) for a cointegration test based on the joint significance of the levels terms in a conditional ECM model. They show that this F-test has higher power than other popular cointegration tests.

[7] To calculate the standard error of the LRM of Fiscal Reliance we used the delta method because it is computed from the following ratio: (-1)*(Fiscal Reliance t-1/Polity t-1). We perform the Newey West adjustment with a one-year lag to correct for serial correlation if detected via a Lagrange Multiplier Test.

bivariate model with the number of lags of Fiscal Reliance in first differences selected by the minimization of the BIC statistic. We find that in the overwhelming majority of specifications the coefficient of interest—the LRM—has the "wrong" sign: it is positive, suggesting that permanent increases in Fiscal Reliance are correlated with increases in Polity. We also find that very few of the positive or negative coefficients on the LRM are statistically significant, while the Kanioura and Turner (2005) F-tests are rarely above the threshold required to suspect co-integration between Fiscal Reliance and Polity. In short, the bivariate regressions yield results that are inconsistent with the resource curse.

We then move beyond these bivariate regressions by adding conditioning variables. One might argue that increased reliance on natural resource income is correlated with rising GDP, and rising GDP drives democratization (Lipset 1959), or protects democracy (Pzeworski et al. 2000). We therefore include the log of Real Per Capita GDP. Because the ECM framework includes variables measured in levels and in differences, our regressions therefore also include the growth rate of GDP per capita, which addresses concerns raised by Gasiorowski (1995) that high growth promotes regime stability while economic crises catalyze regime transitions. One might also argue that increased democratization in resource-reliant countries is influenced by world or regional trends. We therefore control for democratic diffusion effects by adding two variables, following Gleditsch and Ward (2006): 1) the percentage of democracies in a country's geographic-cultural region; and 2) the percentage of democracies in the world. Finally, we control for an ongoing civil war with a dummy variable. We do not reproduce the coefficients on the control variables because of space limitations, but report the F-test on their joint significance in levels. We chose the number of lags of Fiscal Reliance in differences based on the BIC statistic.

Table 2 presents the results, which are inconsistent with the hypothesis of a resource curse. The results predicted by the theory would be that the series would be cointegrated and the LRM would be negatively signed and statistically significant. Eleven of 18 country time-series regressions, however, yield LRM's with the "wrong" (positive) sign, and two of these are statistically significant at the ten percent level or better. Of the seven that yield the predicted negative sign, none are statistically significant. Only three of these seven even suggest cointegration. The regressions also do not detect

negative contemporaneous short run effects of increases in Fiscal Reliance on Polity: only six of the 18 yield negative coefficients, none of which are statistically significant. We obtain similar results when we extend our search for temporary effects by introducing distributed lags of Fiscal Reliance in differences as indicated by the BIC statistic. The introduction of lags is called for in eight of the 18 cases. Of these higher order lags, five have the "wrong" (positive) sign, and two are significant at ten percent or better. Only three of the eight yields a statistically significant coefficient that is negative.

Panel Analysis of Fiscal Reliance

One might argue that our country-by-country regressions underestimate the negative relationship between Fiscal Reliance and Polity. Instead, pooling the data, and imposing a uniform slope on the LRM of Fiscal Reliance—albeit, while still assuming heterogeneous intercepts—may yield the predicted, negative coefficient (see Phillips and Moon 1999). One might also argue that time-series cointegration tests are low powered: they do not exploit the cross-section dimension, making it less likely to find an equilibrium relationship between Fiscal Reliance and Polity (Levin, et al. 1992). Finally, one might argue that panel estimators attenuate measurement error more effectively than time-series do (Baltagi 1995).

We therefore pool the 18 countries to generate a panel dataset. Before estimating panel regressions we perform a series of diagnostics on the data. We estimate unit-root tests via the Maddala-Wu-Fisher (1999) panel version of the ADF test (designed for unbalanced panels) in order to see if the data is non-stationary.[8] The panel unit root tests performed on the data in levels suggest that both Polity and Fiscal Reliance are integrated of order 1 (results available upon request). Therefore, we look for evidence of cointegration using ECM-based cointegration tests developed by Westerlund (2007) for panel data. We employ this approach for three reasons. First, it is the closest analogue to the Kanioura and Turner (2005) time series ECM approach. Second, it is designed to make it easier to detect cointegration

[8] We estimate each of the pooled ADF regressions with country and year fixed effects and White robust standard errors. The lags of the dependent variable are chosen via standard significance tests; the same goes for whether to include a linear time trend. The results are robust to choosing the lags via the BIC.

by virtue of the fact that it provides greater power than residual based panel tests. Third, it can be estimated with bootstrapped standard errors that are robust to cross-sectional dependence. Specifically, the Westerlund (2007) panel cointegration approach estimates country-by-country ECM regressions and then pools the information to produce two panel cointegration tests: the Panel Test t; and Panel Test a.[9] The null hypothesis is that the error-correction term (the lagged dependent variable in levels) is equal to zero for all countries. Failure to reject the null hypothesis therefore suggests that there is no long-run equilibrium relationship between Fiscal Reliance and Polity in the panel as a whole.

We then go on to actually estimate the ECM parameters of interest by running panel ECM regressions. The regressions include country fixed effects and year fixed effects; Driscoll Kraay standard errors are estimated to address non-spherical errors.[10] We specify the lag length of Fiscal Reliance in first-differences by choosing the BIC statistic with the lowest value.[11]

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[9] All models include bootstrapped standard errors to address cross-sectional correlation; a lead of Fiscal Reliance in first-differences to make Fiscal Reliance weakly exogenous, a lag of Fiscal Reliance in first differences; and a lag of the (differenced) dependent variable to eliminate serial correlation. Allowing these lags and leads to vary by country does not materially affect our results. We estimate all four of Westerlund's cointegration tests; for reasons of space we only report the test statistics for the two that are the most apposite to a panel approach. See the online data analysis appendix for the group mean tests.

[10] We do so to correct for heteroskedasticity, serial correlation (with a Newey West one lag adjustment) and contemporaneous correlation.

[11] Because the panel cointegration tests demand that there be no gaps in the time-series dimension, we linearly interpolate missing values for all variables (we only do so for the ECM panel regressions—for the conditional logit regressions and difference in differences regressions that we report further ahead, we do not use interpolated versions). We do not report various lag experiments where we add from one to five distributed lags of Fiscal Reliance in differences. They do not materially affect the main results and

The results of the cointegration tests are reported in Table 3, Panel A; the results of the ECM panel regressions are reported in Panel B. The resource curse theory would predict that the data series are cointegrated, and that the LRM is negatively signed and statistically significant. The results, however, point in the opposite direction. The Westerlund panel cointegration tests suggest that Polity and Fiscal Reliance are not cointegrated. In and of itself, this casts serious doubt on the resource curse hypothesis. Moreover, the ECM panel regressions consistently produce coefficients with the "wrong" sign, regardless of specification. In model 1, which is a bivariate specification, the coefficient on the LRM is positive but not significant. In model 2, we add the same conditioning variables that we used in the country-by-country regressions, and the LRM remains positive, and is now statistically significant at ten percent. In model 3, we introduce a lagged dependent variable instead of doing the Newey-West adjustment to control for serial correlation. In model four we use robust standard errors clustered by year instead of estimating Driscoll-Kraay standard errors to control for contemporaneous correlation. In model 5, we return to estimating Driscoll-Kraay standard errors, and again conduct the Newey-West adjustment, and re-estimate a bivariate regression that now employs the same set of observations as model 2. This specification ensures that the addition of controls with less data coverage did not artificially increase the statistical significance of the LRM. Our results are robust to all of these tests.

There are two ways to interpret the results in Table 3, neither of which is consistent with the hypothesis of the resource curse. A conservative approach would be to simply reject the resource curse, because the LRM has the wrong sign and Polity and Fiscal Reliance are not cointegrated. A more aggressive interpretation would be to argue for a resource blessing: the LRM has the wrong sign and is statistically significant at the ten percent level; and the coefficient on contemporaneous Fiscal Reliance in differences has a positive sign and is highly significant. Such an approach would discount the cointegration tests on the grounds that the data series in levels might only be locally non-stationary.

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are available in the data analysis appendix. We also experimented with the introduction of one to five finite lags sequentially, and these also did not materially affect the results (results available upon request).

Panel Analysis of Total Oil Income

A skeptical reader might argue that these regressions suffer from sample selection bias: they focus on an unrepresentative sample of the world's largest resource producers. We therefore substitute Total Oil Income, which covers the entire world since 1800, as the independent variable and re-estimate the regressions. The broad time series and cross-sectional coverage of Total Oil Income confers an additional benefit: we are able to run variants of our ECM regressions on split samples in order to search for possible conditional effects (time period; thresholds of resource reliance; region; per capita income at the time oil was first produced; and income distribution). We perform the same set of data diagnostics that were applied to Polity and Fiscal Reliance. We find that Polity and Total Oil Income are integrated of order one (unit root tests available upon request). We therefore again perform Westerlund's panel cointegration tests and estimate panel ECM regressions.[12]

We make one minor change to the presentation of the regressions: The Westerlund cointegration tests require that countries' time-series component have a minimum number of years, and our dataset now includes countries that do not always satisfy this requirement. Therefore, to make sure that the estimation of the ECM parameters is robust to dropping countries that are below these thresholds, we estimate the panel models twice—once on the truncated dataset used to conduct the cointegration tests, and a second time on the full dataset.[13] We also perform the same robustness checks as we did for the Fiscal Reliance

[12] Even though the BIC statistic indicates that no lags of Total Oil Income in differences are necessary, we run the Westerlund ECM panel cointegration tests with one lag of Total Oil Income (in differences) and a lag of Polity (in differences) to control for serial correlation. To reflect the lack of lags selected by the BIC, however, we also reran the Westerlund ECM panel cointegration tests without these lagged terms and it made no material difference to the results (see data analysis appendix).

[13] The BIC statistic indicates that no lags of Total Oil Income in differences are necessary. However, we ran experiments in which we introduced from one to five finite lags for all of the ECM panel regressions that follow. These specifications never materially affected the results (see data analysis appendix).

panel regressions (depicted in models 3, 4 and 5 of Table 3), and again our results are always robust. We therefore do not reproduce them here (see data analysis appendix).

Table 4, Specifications 1 and 2 (on the cointegration truncated dataset and the full dataset, respectively) present our base model—and the results are inconsistent with the resource curse hypothesis. Instead of the negative sign on the LRM predicted by the resource curse, the LRM is positive and significant at the one percent level. Moreover, there is some evidence that the series are cointegrated: Panel Test t strongly indicates cointegration; Panel Test a fails to reject the null. A conservative interpretation of these results would be a rejection of the resource curse hypothesis. A more aggressive interpretation, that would discount Panel Test a, would be that there evidence for a resource blessing.

Conditional Effects

One explanation of this surprising finding is that the resource curse is perhaps a result of recent geo-strategic developments. Perhaps it only exists in the post-1973 period, when dramatic increases in oil prices gave significant leverage to oil producing countries that allowed them to nationalize their oil industries, become price setters, and deploy the resulting windfalls to make their governments accountability-proof. Moreover, 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 time by truncating the dataset to 1973-2006 (Table 4, models 3 and 4). The findings are even more surprising: not only does the LRM continue to have the opposite sign predicted by the resource curse, but it is of even larger magnitude than in the base model and it remains significant at the one percent level. The cointegration tests yield similar results to the base models (column 1).

One might also argue that our base regression underestimates the negative effects of Total Oil Income on Polity. One might imagine that increases in Total Oil Income affect a major producer, such as Venezuela, much more than they affect a minor producer, such as Belize. One might also imagine that increases in Total Oil Income only affected Venezuela's Polity Score negatively once it became a major producer in the 1940's, but had no effect before that. In other words, is there a range of country-year observations in which increases in Total Oil Income above a critical threshold drives decreases in Polity?

We therefore split the dataset into three groups: all observations above the mean of Total Oil Income of all countries; all observations above the mean of Total Oil Income for oil producing countries only; and all observations that are at least one standard deviation above the mean of Total Oil Income for all countries. The cut-off point for each group is: $338; $971; and $2,954, respectively.

Table 4, models 5 and 6, present the results produced by the first cut-off—and these are inconsistent with the resource curse hypothesis. The LRM has the wrong (positive) sign. Models 7 and 8 present the results produced by the second cut-off—and, again, the results are inconsistent with the hypothesis. Model 7 does produce the predicted negative coefficient on the LRM and one of the panel cointegration tests weakly suggests cointegration. The LRM, however, is far from statistical significance. Moreover, the results in model 7 appear to be driven to by the fact that the cointegration tests require that 20 out of 27 countries be dropped because they have less than 21 observations. When we re-estimate the same regression on the full sample of 27 countries in model 8, the LRM switches signs. The difference between models 7 and 8 suggests that there is perhaps some limited range of countries in which one can detect the predicted negative relationship between Total Oil Income and Polity. We therefore re-estimate the same regressions on the third cut-off. The results are displayed in model 9—and are again inconsistent with the resource curse hypothesis: at very high levels of oil production, the long run relationship between Total Oil Income and Polity is positive and significant at the ten percent level. We cannot perform the cointegration tests because there are insufficient observations at this high level of resource reliance. In short, the evidence is not consistent with the hypothesis that the resource curse is conditional on high levels of per capita oil rents.

Perhaps it is the case that oil only has negative effects in particular geographic/cultural environments? In order to test this hypothesis we group countries by region and estimate regressions on those regions where we would expect to find a resource curse: Africa, Latin America, the Middle East and North Africa, Central Asia and Eastern Europe, and Southeast Asia. The results, presented in Table 5, are inconsistent with the hypothesis: only one of the eight models produces an LRM with the predicted (negative) sign, and it is far from statistically significant. One region, Latin America, produces a highly

statistically significant, positive LRM. Panel Test t suggests cointegration (at the ten percent level). Moreover, the magnitude of the effect is large: for every increase of $1,000 in Total Oil Income, Polity increases 23 points. This supports Dunning's (2008) finding of a resource blessing in Latin America.

Does the evidence also support Dunning's theory about why Latin America has a resource blessing: in regions where income is unequally distributed there should be a resource blessing; in regions where income is more equally distributed there should be a resource curse? We test this hypothesis in Table 6, models 1-4. We measure income inequality using the same metric as Dunning (2008), the capital share of non-oil value added in GDP, and split the data into three groups: countries with equal distributions of income (below the mean), countries with unequal distributions of income (above the mean), and countries with very unequal distributions of income (one standard deviation above the mean).[14] Models 1 and 2 of Table 6 do not support the hypothesis that there is a resource curse at low levels of inequality. The LRMs both have the wrong sign, although they are far from significant. Models 3 and 4 do suggest, however, that there is a resource blessing at high levels of inequality: the coefficient on both LRMs is positive and highly significant, while the Panel Test t suggests cointegration at a high level of confidence. One would therefore think that the resource blessing is even more pronounced at very high levels of inequality. When we estimate the regressions on this sub-sample, however, the positive coefficient on the LRM is far from statistically significant, although there is still evidence of cointegration (see data analysis appendix). This result may be a product of the fact that, with the exceptions of Indonesia and Nigeria, there are few major oil producers among the set of highly unequal countries. Taken as a whole, the results are inconsistent with the hypothesis of a resource curse at any level of inequality, but they do provide some evidence in support of a conditional resource blessing.

[14] In order to make sure that our coding is robust, we also employ a second measure of inequality, the Gini coefficient on incomes in the manufacturing sector. Our regression results are not sensitive to the choice of measure, and thus we only reproduce the results from the first measure here (see appendix on sources and methods for a discussion of the measures; see data analysis appendix for robustness tests).

One might argue that the resource curse is conditional on the level of economic development at the time that oil is discovered: countries with high per capita incomes will be immune to the pernicious effects of oil rents, while countries with low per capita incomes will be cursed. In order to test this hypothesis we split our dataset into three subsamples: rich countries (above the mean of GDP per capita of the set of non-oil producers when oil was first exploited in the producing country); poor countries (below the mean); and very poor countries (one standard deviation below the mean).[15] The results are reported in Table 6. Not surprisingly, they indicate that increases in oil rents have no impact on Polity in rich countries (models 7 and 8). They do not, however, support the hypothesis that increases in oil rents curse poor countries. In fact, models 5 and 6 show that, among poor countries, increases in Total Oil Income are associated with *increases* in Polity at the five percent level of confidence. Moreover, the magnitude of the LRM is non-trivial: for every $1,000 increase in Total Oil Income, Polity increases by eight points. When we split the sample still further, to very poor countries, the magnitude of the LRM almost doubles and its statistical significance increases (results not shown, see online data analysis appendix). Whether one wants to argue for a resource blessing in countries that are poor when they discover oil depends on how much weight is put on the cointegration tests (which do not produce statistically significant results). At the very least, however, one can reject the resource curse hypothesis.

Regime Type as a Binary Variable

Some researchers claim that regime types are best measured as binary variables (e.g., Przeworski et al. 2000). We therefore turn to a dynamic, conditional fixed effects logit regression with Regime as the dependent variable. This estimation technique allows us to calculate separate estimates for those countries observed as democratic and those observed as autocratic—and then see whether they switch

[15] In order to make sure that our results are robust, we also code countries on the basis of their average levels of GDP, rather than the level at the time that oil was first produced. Because the results are not sensitive to this coding choice, we do not report these results (see data analysis appendix).

regime type as a result of increased resource reliance. This estimation strategy also allows us to continue to 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. The Regime variable codes autocracies as "1"; the conditional transition probabilities are estimated via the following functional form:

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

where $\Lambda(\cdot)$ is the logistic cumulative distribution; α is the intercept term for country *i* and depicts the fact that the country fixed effects are potentially correlated with variables in **X** (although these coefficients are not actually estimated); **X** is a (n×k) matrix of n observations on *k* explanatory variables; β is a vector of estimated parameters that indicate the effects of the covariates on the probability of a 1 at time *t* given a 0 at time *t-1* and ρ is the estimated coefficient on the lagged dependent variable; 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 $\mathbf{X}_{it}$); **v** is a time 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. The **v** term implies that time indicators are also included (except for one), represented by the heterogeneous intercepts in vector λ.[16] The first set of coefficients evaluates the hypothesis that *oil undermines democracy*; the addition of these coefficients and their respective interaction terms evaluates the hypothesis that *oil prevents democratization*. 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;

[16] A country that never experiences a regime change is dropped: countries that do not switch from one state to another do not contribute information to the optimization of the log-likelihood function.

subtracting the product of this addition from 1 identifies the impact of resource reliance on the odds of democratic transition.[17] Robust standard errors clustered by country address serial correlation.

We present the results in Table 7. Model 1, specification 1 estimates the effect of increases in Total Oil Income on countries that are observed in any year as democratic from 1818 to 2006. The coefficient on Total Oil Income (in t-1) tells us the effect of an increase in Total Oil Income within countries over time on the probability that those countries will become autocratic. If increases in Total Oil Income 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. Model 1, specification 2, estimates the effect of increases in Total Oil Income on countries that are observed in any year as authoritarian. Here the resource curse would predict a negative coefficient: as Total Oil Income increases, authoritarian countries should be *less* likely to transition to democracy. Once again, our results yield the opposite result: the coefficient is positive and statistically significant at the 5 percent level. In Model 2, we re-estimate these regressions on the post-1972 period. These results provide even less support for the resource curse: democracies are *less* likely to break down as Total Oil Income increases (5 percent level of significance); autocracies are *more* likely to transition to democracy as Total Oil Income increases (10 percent level of significance). In short, when we substitute a binary measure of democracy for Polity, the evidence does not support the hypothesis of a resource curse but instead provides some evidence of a resource blessing.

Difference in Differences

By focusing on variance over time within countries, we have addressed the problem of time-invariant omitted variable bias. To put it concretely, we are implicitly comparing Venezuela to itself over time in order to see whether increases in its resource reliance explain lower levels of Polity, controlling for the effects of higher GDP per capita and possible democratic contagion effects from other countries.

[17] To calculate the z-statistics for the coefficients that gauge the probability of transitions from autocracy to democracy we use the Delta Method since we are calculating the statistical significance of the addition of a linear term and its interaction with the lagged DV.

One might argue, however, that Venezuela might have democratized even faster, or more fully, had it not developed an oil-based economy. The key to addressing this issue is the specification of a more powerful counterfactual than the before-and-after comparison implied by our ECM regressions. Producing such a counterfactual requires us to ask a question of the following type: what would Venezuela's Polity have been today had it not been earning oil rents since the 1917?

This counterfactual Venezuela does not, of course, exist; but we can observe the political trajectory of a set of countries that were broadly similar to Venezuela, in terms of history, geography, culture, level of economic development, and degree of democratization *before* Venezuela became increasingly reliant on oil, but which did not *subsequently* become major oil producers. That set of countries is the other nations of Latin America that did not become resource reliant. We therefore return to using Polity as the dependent variable but now transform it: we net out the difference in Polity between oil-producing countries and a synthetic, non-resource-reliant country that is represented by the average polity score of the non-resource countries in the oil producing country's geographic/cultural region (our procedure for identifying the non-resource reliant countries can be found in our online appendix on sources on methods). We refer to this variable as *Net Polity.* This transformation allows us to see if the yearly differences in the changes in Polity between treatment and control groups are a function of changes in the dose of oil, after controlling for the same set of covariates as in the previous regressions.

Our approach is therefore a refinement of a typical difference-in-differences model that captures the treatment effect with a dummy variable. We run an OLS model with the following functional form:

$$\Delta \mathbf{Y}_{it} = \Delta \mathbf{X}_{it}\beta + \mathbf{n}_i\varphi + \mathbf{v}_t\lambda + \mathbf{u}_{it} \quad (3)$$

where $\mathbf{Y}$ is a (n×1) vector of observations on the dependent variable; Δ is the first-difference operator; $\mathbf{X}$ is a (n×k) matrix of n observations on k explanatory variables; β is a (k×1) vector of parameters, $\mathbf{n}$ is a country fixed effect potentially correlated with variables in $\mathbf{X}$, $\mathbf{v}$ is a year fixed effect potentially correlated with variables in $\mathbf{X}$ and $\mathbf{u}$ 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 $\mathbf{n}$ and $\mathbf{v}$ 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). We employ the same control variables as our earlier regressions, and estimate Driscoll Kraay standard errors with a Newey West adjustment with one lag length. Because the data is differenced we do not worry about cointegration.[18]

We present the results in Model 1 of Table 8. The Total Oil Income coefficient is negative, but far from statistically significant. One might argue that the reason for lack of significance is endogeneity: for example, perhaps countries that are transitioning toward democracy pump more oil than they did under autocracy because the new regime needs to placate voters' demands for public goods?

We therefore adopt an instrumental variables approach to evaluate this hypothesis before we continue on in the difference in differences framework. We construct a dataset on proven oil reserves for virtually every oil producer in the world on an annual basis from 1943 to 2006, and use it to generate three instruments in levels: *Reserves*; *Reserves per Surface Area*; and *Total Reserves in the Region* (see online appendix on sources and methods). We then estimate a generalized method of moments (GMM) two-stage instrumental variables regression with country and year fixed effects.[19] We treat Total Oil Income in first differences as potentially endogenous, and therefore instrument it with Reserves, Reserves per Surface Area, and Total Reserves in the Region. All three instruments enter the first stage of the regression as independently and jointly significant as determinants of Total Oil Income (in first differences). This stage also includes all of the control variables employed previously (results not reported because of space constraints). The independent variable of interest in the second stage (Model 2

[18] First differencing controls for countries' unobserved, time-invariant heterogeneity; yet we also include country dummies to address heterogeneity in Polity's annual changes (see Kittel and Winner 2005: 280).

[19] While heteroskedasticity tests reject the hypothesis that the error term is homoskedastic, an Arellano Bond serial correlation test upholds the hypothesis that there is no AR1 correlation. We therefore perform a GMM two stage instrumental regression, instead of a regular two-stage least squares, with a weighting matrix estimated by an Eicker-Huber-White robust covariance estimator.

of Table 8) is the predicted values of Total Oil Income (in first differences) from the first stage regression. The dependent variable is Net Polity (in first differences). The instruments are valid according to a Hansen J-test of the over-identifying restrictions (see bottom of Model 2), which means that we cannot reject the null hypothesis that the instruments are exogenous.

Model 2 suggests that changes in Total Oil Income are not endogenous to changes in Net Polity: the difference in Sargan C-test strongly indicates that we *cannot reject* the null that Total Oil Income is exogenous (see bottom rows of Model 2). Therefore, although the sign on Total Oil Income (in differences) in the second stage of the regression is negative and significant at the 10 percent level, there is no justification for using instrumental variables. In fact, if we drop the instrumental variable approach, and run a regular, static OLS regression on the same subsample as Model 2, we obtain a result that is nowhere near statistically significant (see Data Analysis Appendix). In addition, if we employ the instrumental variables approach on subsamples that are truncated with respect to time, we again *cannot reject* the null that Total Oil Income is exogenous. Moreover, in these specifications, the second stage of the regression now produces coefficients on Total Oil Income that are either far from statistically significant or have the wrong (positive) sign (see Data Analysis Appendix).

Perhaps the results discussed above, which are not consistent with the hypothesis of a resource curse, are a function of the fact that our models only capture the instantaneous impact of changes in Total Oil Income on changes in Net Polity? What if the changes in Net Polity induced by changes in Total Oil Income are spread out over a period of several years? We therefore estimate a rational, infinitely distributed lag model as an Autoregressive Distributed Lag model (ARDL) in first differences following Wooldridge (2006: 638) in order to calculate the total change in Net Polity made by a change in Total Oil Income. Specifically, **X** in equation (3) now includes the one-year lag of the (differenced) dependent variable and a lag of (differenced) Total Oil Income to calculate the total change made by Total Oil Income on Net Polity, with the standard errors of this coefficient computed via the Delta Method.

`Table 8, model 3 presents the results of the full panel, and it provides no evidence in favor of a resource curse. The coefficient on the immediate impact of Total Oil Income continues to be negative,

but far from significant. The total change distributed over all periods, however, is positive and statistically significant at the one percent level. We then searched for possible conditional effects under which changes in Total Oil Income effect changes in Net Polity by employing the same split sample techniques that we used in the panel ECM approach: we estimate all regressions on subsamples of the dataset split by time period, oil income thresholds, region, income distribution, and economic development.

None of these regressions produce results that are consistent with the resource curse, which is to say a statistically significant negative coefficient on the Total Change Made by the Change in Total Oil Income. Table 8, models 4-7, presents only those results in which the coefficient on the Total Change Made by Total Oil Income is significant at five percent or better (the rest of the results are available in our data analysis appendix, as are results for static models). Of the 15 conditional effects regressions we estimate, only one (Sub-Saharan Africa) produces the predicted negative coefficient—and that result is far from statistically significant. Fourteen of the 15 regressions produce coefficients with the wrong (positive) sign, and of these seven are statistically significant at the one percent level, while an additional two are significant at ten percent. To the degree that any of the regressions produce a statistically significant, negative coefficient on the Immediate Impact of Changes in Total Oil Income (Total Oil Income in t), only two reach the ten percent level. Moreover, these two negative coefficients are eclipsed by positive coefficients of greater magnitude on the lagged value of Changes in Total Oil Income that are statistically significant at the one percent level. In short, the regressions rule out even a short-run resource curse, even if the effect of oil reliance on Net Polity is conditioned by other factors.

With so many positive and statistically significant coefficients on the Total Change Made by Total Oil Income, one may wonder if there is a resource blessing. The answer depends on how much one weighs the statistical significance of coefficients versus their magnitude. An emphasis on statistical significance would indeed suggest a resource blessing. The small magnitude of the positive coefficients, however, would suggest that if there is a resource blessing, it is negligible.

Robustness Tests: Total Fuel Income and Total Income from Fuel and Metals

One might argue that our measure of resource reliance, Total Oil Income, leaves out important sources of the rents generated by the production of other fuels and minerals, and that if we accounted for the income from those additional sources we would find evidence for a resource curse. We therefore re-estimate the all of the difference-in-differences regressions presented above, but substitute Total Fuel Income (oil, natural gas, and coal) and Total Resource Income (oil, natural gas, coal, precious metals, and industrial metals) for Total Oil Income. The results do not overturn our regressions on Total Oil Income, and thus we do not report them here (they are available in our Data Analysis Appendix). The sign and magnitude of the coefficients of interest remain positive. The one difference that we pick up is that the coefficients of interest are of somewhat less statistically significant—though they still achieve significance of 10 percent or better.

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 search for a host of conditional effects. 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 civil war onset. In studying these processes, however, comparativists often rely on

datasets with a limited time dimension and employ pooled regression techniques that treat countries as homogenous units. These methods increase the risk that correlations will be mistaken for causation.

The research design that we adopt in this paper also goes beyond a concern with the typical sources of bias that bedevil researchers' ability to draw causal inferences from observational data. Even when there is no obvious danger that country fixed effects are correlated with the independent and dependent variables of interest, the approach pursued in this paper is valuable. When a hypothesis is not about static differences between countries, but about the complex changes that take place within countries over time, deploying historical datasets provides a better fit between theory and evidence.

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Figure 1: Trinidad and Tobago

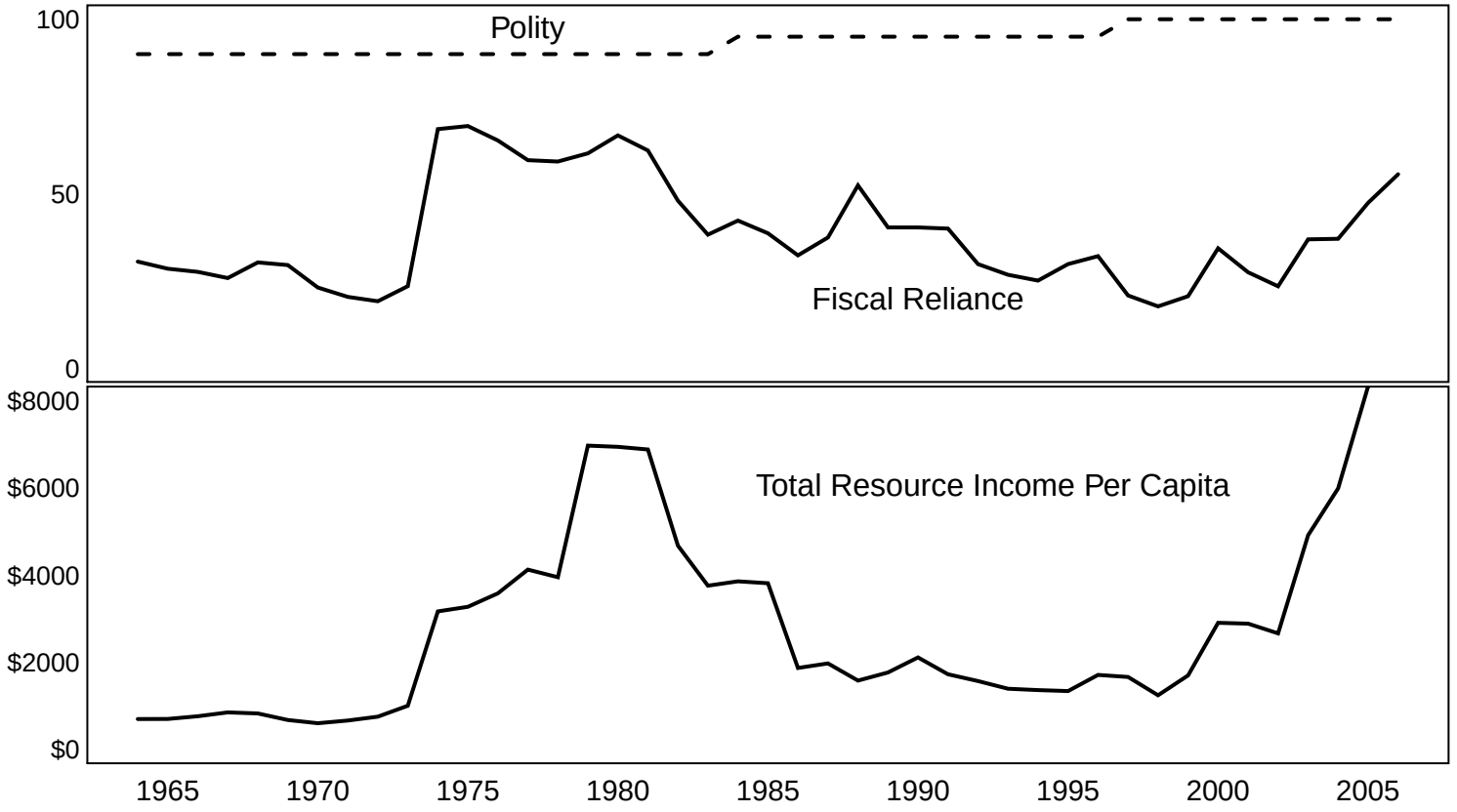

Figure 2: Mexico

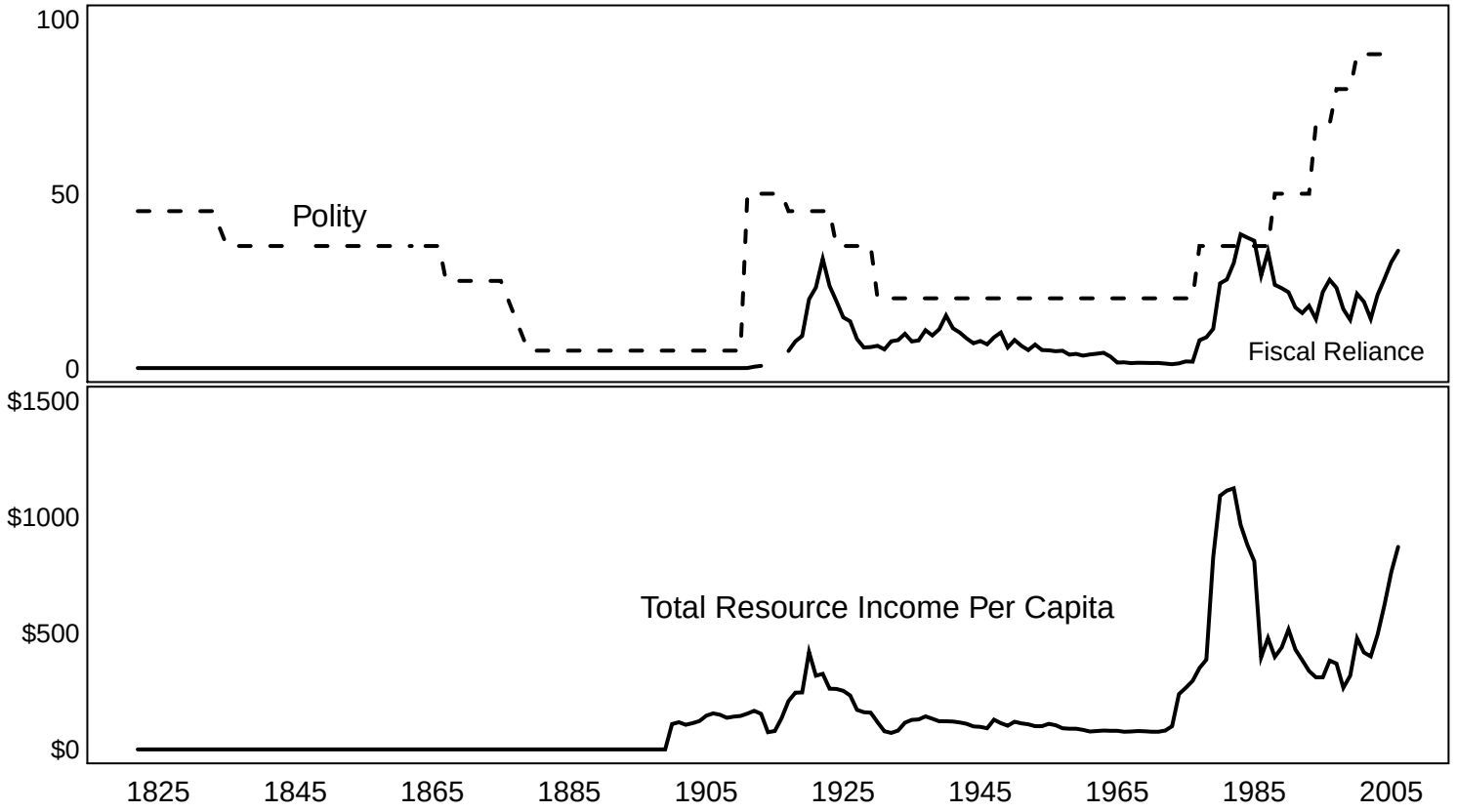

Figure 3: Angola

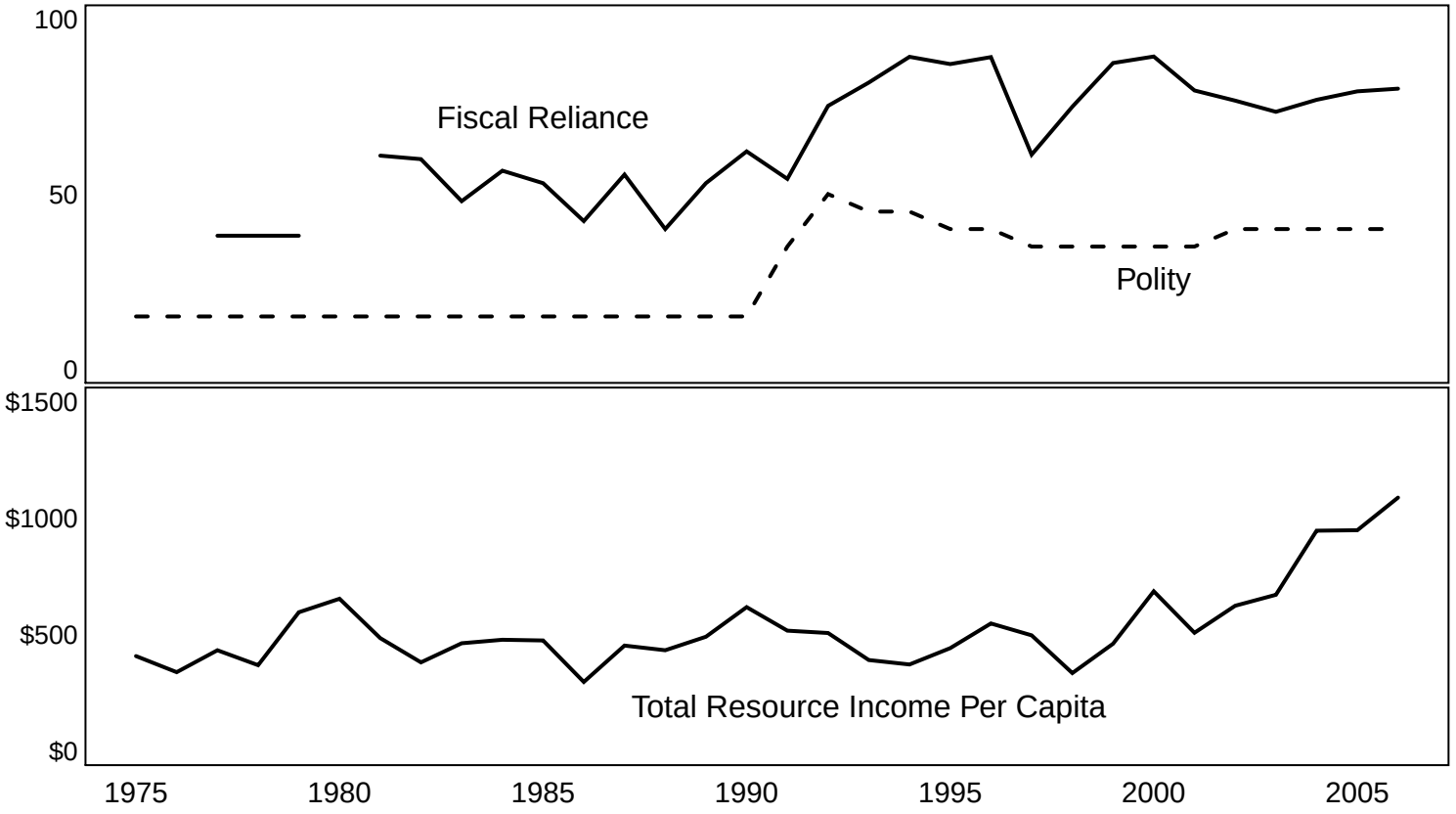

Figure 4: Zambia

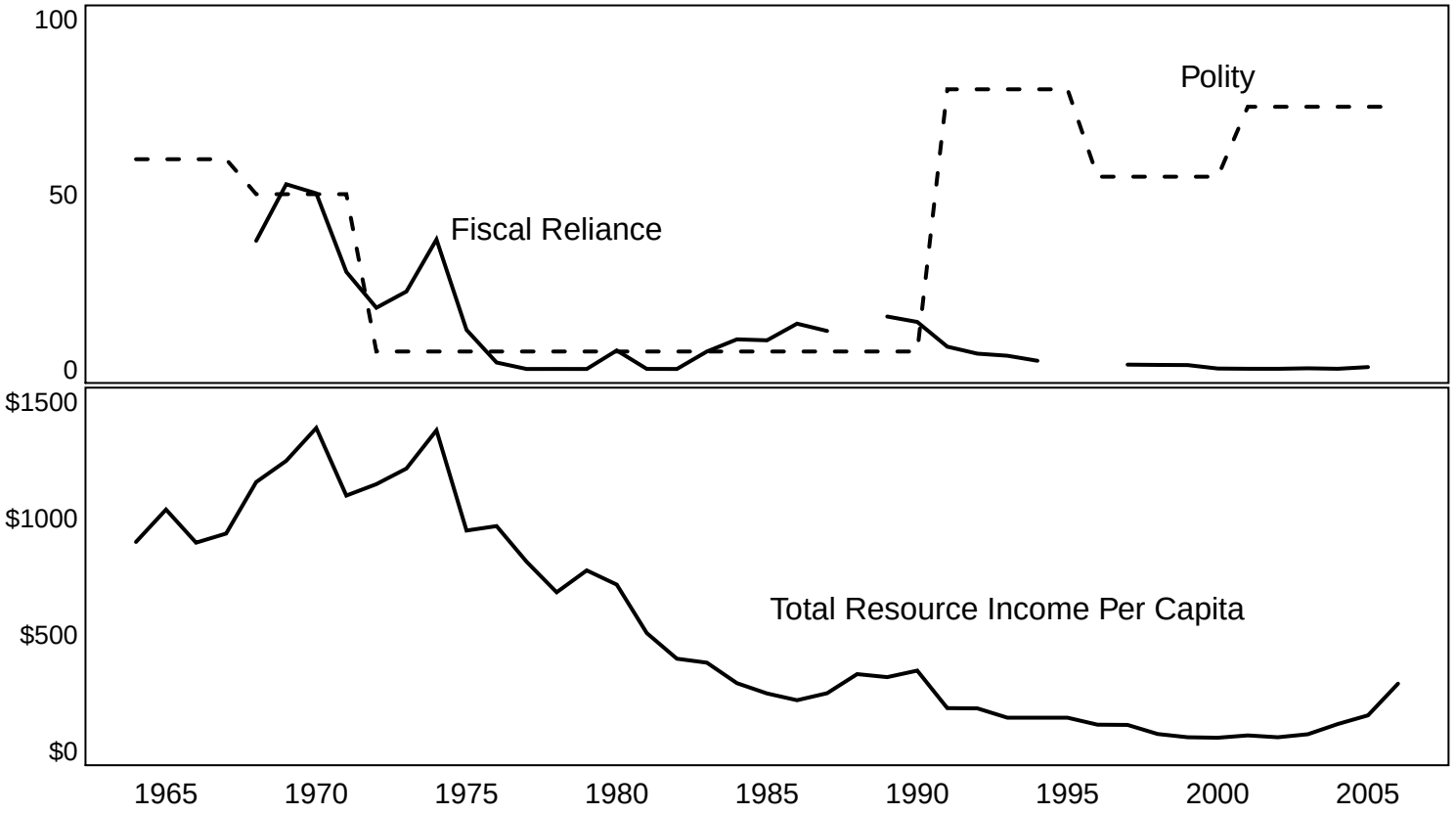

Table 1. Potential Patterns of Resource Blessings and Curses

Polity refers to normalized Combined Polity Score (0 to 100)

Panel A: Potentially Resource Blessed Countries			
Remained Democratic During a Resource Boom	*Democratized During a Resource Boom*	*Remained at Threshold of Democracy Polity=80 During Resource Boom*	*Polity Increased by at Least One S.D. During Resource Boom*
Jamaica	Botswana	Estonia	Algeria
Lithuania	Ecuador	Namibia	Angola
Netherlands	Mexico		Iran
Norway	Mongolia		Kyrgyzstan
Papau New Guinea	Peru		Niger
Trinidad & Tobago	Russia		Tunisia
	Ukraine		
	Venezuela		

Panel B: Potentially Resource Cursed Countries			
Democracy Fails During a Resource Boom	*Polity Decreases by One S.D. During a Resource Boom*	*Democratizes After Resource Boom Collapses*	*Polity Increases by One S.D When Resource Boom Collapses*
Belarus	Azerbaijan	Bolivia	Dem. Rep. of Congo
Malaysia	Congo	Indonesia	Guinea
			Liberia
			Zambia

Panel C: Neither Blessed Nor Cursed	
Variance in Polity, But No Identifiable Pattern in the Data	*Country is Autocracy Before Boom, and Remains So Afterwards*
Chile	Bahrain
Nigeria	Cameroon
	Egypt
	Equatorial Guinea
	Gabon
	Iraq
	Kazakhstan
	Kuwait
	Libya
	Mauritania
	Morocco
	Oman
	Qatar
	Saudi Arabia
	Tajikastan
	Turkmenistan
	United Arab Emirates
	Vietman
	Yemen

Table 2. Error Correction Models (ECM) and Co-integration Tests for the relationship between Polity and Fiscal Reliance (F.R.), 18 Major Oil and Copper Producers

t-statistics in brackets

	Polity's Speed of adjustment (Polity t-1)	Long-run Multiplier for Fiscal Reliance (F.R.)	F-test of Co-integration and stat. significance	Short-run Effect for F.R. in year t	Largest short-run effect at higher lag	At what lag?	Total # of lags of Δ Fiscal Reliance	BIC Statistic for lags of Δ F.R.	F-test on control variables in levels	Observations	Rsquared
Trinidad and Tobago	-0.229 [1.90]*	**-0.029 [0.41]**	**1.83**	-0.006 [0.31]			0	-3.409	2.1	42	0.25
Mexico	-.122 [2.00]**	**0.049 [0.08]**	**2.15**	0.037 [0.22]			0	207.661	2.82**	107	0.09
Venezuela	-.085 [2.07]**	**0.676 [1.68]***	**2.17**	0.046 [1.42]			0	176.295	1.59	122	0.15
Ecuador	-.212 [2.37]**	**-0.063 [0.07]**	**3.02**	-0.117 [0.50]			0	254.076	0.48	66	0.19
Chile	-0.102 [1.88]*	**0.924 [2.28]****	**1.91**	0.07 [1.51]			0	304.96	1.2	140	0.14
Norway	-0.049 [1.73]*	**0.322 [0.31]**	**1.49**	-0.014 [0.12]			0	186.192	0.83	168	0.05
Nigeria	-0.418 [2.94]***	**-0.112 [0.18]**	**4.44***	0.037 [0.09]	-.714 [2.69]***	1	5	225.553	2.54*	41	0.59
Angola	0.078 [0.14]	**2.87 [0.14]**	**0.23**	0.068 [0.33]	0.304 [2.11]*	2	2	93.501	1.17	23	0.56
Zambia	-0.683 [3.64]***	**-0.105 [0.32]**	**8.55*****	-0.479 [1.77]	-0.448 [2.41]**	3	3	104.005	6.36***	23	0.82
Gabon	-0.169 [1.55]	**-0.189 [1.55]**	**1.23**	0.063 [1.06]			0	78.529	1.25	46	0.35
Algeria	-0.653 [2.31]*	**-1.386 [1.37]**	**12.27*****	0.153 [0.59]	-0.829 [3.67]**	3	5	84.51	8.45***	22	0.95
Equatorial Guinea	-0.729 [8.21]***	**0.007 [0.21]**	**41.9*****	0.639 [3.77]***	1.088 [6.49]***	1	4	70.3	5.29***	28	0.96
Iran	-0.450 [2.29]**	**0.117 [0.11]**	**2.87**	-0.054 [0.16]	0.187 [0.56]	4	4	200.126	1.18	38	0.43
Yemen	-0.203 [1.74]*	**0.381 [1.08]**	**1.82**	0.055 [0.56]			0	144.159	0.91	53	0.17
Kuwait	-.434 [3.25]***	**-0.523 [1.12]**	**5.62****	0.054 [0.31]			0	80.05	4.09***	41	0.43
Bahrain	-.499 [1.64]	**-0.244 [0.75]**	**2.39**	-0.039 [0.82]	0.104 [0.64]	2	3	32.22	3.70**	27	0.62
Oman	-.194 [1.61]	**0.167 [0.72]**	**1.34**	0.016 [0.024]			0	90.53	0.5	50	0.16
Indonesia	-.143[1.63]	**0.837 [0.83]**	**1.38**	0.175 [0.87]	0.125 [0.69]	1	1	210.71	2.32*	60	0.21

***significant at the .01 level; **.05 level; *.10 level; Newey West standard errors with 1 lag adjustment estimated to address serial correlation detected for Angola, Chile, E.G., Iran, Nigeria, and Yemen; For the critical values for the ECM F-test of co-integration we used Kanioura and Turner (2005: Table 1, p. 267) for the hypothesis that Polity t-1 + Fiscal Reliance t-1 = 0. To calculate the standard error of the LRM of Fiscal Reliance we used the delta method, since it is computed as follows: (-1)*(F.R. t-1 /Polity t-1). The control variables included, but not reported, in both levels and differences are: Per Capita Income; % Democracies the Region; and % Democracies in the World; dummy variable for ongoing civil war also included.

Table 3. Panel Co-integration tests and Fixed Effects Estimation of Error Correction Models (ECM) for the Impact of Fiscal Reliance on Polity Score
Polity Score Normalized to run from 0 to 100
Robust t-statistics in brackets

	(1)	(2)	(3)	(4)	(5)
Westerlund ECM Co-integration Tests					
Panel Test t	-11.2	-9.8			-9.2
Robust p-value	0.28	0.54			0.52
Panel Test a	-14.9	-10.1			-10.5
Robust p-value	0.2	0.5			0.4
Panel FE ECM Estimation					
Type of standard errors estimated	*DKSE*	*DKSE*	*DKSE*	*RSE c/year*	*DKSE*
Serial Correlation correction technique	NW	NW	lag D.V.	lag D.V.	NW
Polity in levels t-1	-0.053	-0.107	-0.119	-0.119	-0.099
(Error Correction Term)	[5.30]***	[5.01]***	[4.79]***	[4.39]***	[5.25]***
Fiscal Reliance t-1	0.001	0.028	0.031	0.031	0.03
	[0.14]	[1.55]	[1.68]	[1.54]	[2.14]**
Fiscal Reliance	**0.027**	**0.261**	**0.258**	**0.258**	**0.309**
Long-run Multiplier (LRM)	**[0.14]**	**[1.82]***	**[2.01]***	**[1.84]***	**[2.51]****
∆Fiscal Reliance	**0.03**	**0.049**	**0.046**	**0.046**	**0.043**
	[1.54]	**[2.50]****	**[2.16]****	**[1.98]****	**[2.25]****
∆Fiscal Reliance t-1	**-0.018**	**-0.03**	**-0.036**	**-0.036**	**-0.036**
	[0.55]	**[0.86]**	**[1.00]**	**[0.92]**	**[1.06]**
Log(Per Capita Income) t-1		0.593	0.501	0.501	
		[0.81]	[0.73]	[0.67]	
Civil War t-1		1.477	1.854	1.854	
		[1.24]	[1.42]	[1.30]	
Regional Democratic Diffusion t-1		0.01	0.019	0.019	
		[0.50]	[0.92]	[0.85]	
Global Democratic Diffusion t-1		-0.06	-0.077	-0.007	
		[1.52]	[2.03]*	[0.21]	
∆Log(Per Capita Income)		-3.5	-3.468	-3.468	
		[1.07]	[1.01]	[0.93]	
∆Regional Democratic Diffusion		0.17	0.156	0.156	
		[2.41]**	[2.24]**	[2.05]**	
∆Global Democratic Diffusion		0.095	0.097	-0.076	
		[0.95]	[1.07]	[1.35]	
Country fixed effects	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Observations	1772	1121	1121	1121	1121
Number of groups	18	18	18	18	18
R-squared	0.13	0.17	0.18	0.18	0.16

* significant at 10%; ** significant at 5%; *** significant at 1%

Westerlund ECM Co-integration tests estimated with a lead of D.Total Oil Income to conform to weak exogeneity restriction; the estimation is performed with 1 lag of D.Polity and D.Fiscal Reliance, both to match the lag order selected by the BIC statistic and to conform to no serial correlation restriction; each Westerlund ECM Co-integration test run with the Bartlett kernel window width set according to 4(*T* /100)^2/9; each test performed with bootstrapped critical values for test statistics due to contemporaneous correlation between panel observations.

ECM Panel Regressions: DKSE refers to Driscoll Kraay standard errors; RSE c/year refers to Robust Standard Errors clustered by year; NW refers to Newey West AR1 adjustment, with a 1 lag max; lag D.V. refers to introducing a lag of D.Polity (omitted from table). LRM standard errors estimated using the Delta Method: -1(b(Fiscal Reliance t-1)/b(Polity t-1)). Separate country & year intercepts estimated but omitted from table; F-test on joint significance of country and year dummies always highly significant.

Table 4. Panel Co-integration tests and Fixed Effects Estimation of Error Correction Models (ECM) for the Impact of Total Oil Income on Polity Score

PolityScore Normalized to run from 0 to 100

Robust t-statistics in brackets (Driscoll Kraay standard errors estimated with Newey West adjustment with 1 lag of the dependent variable)

	FULL PANEL		Post Oil Shock: 1973-2006		obs.> avg. TOI, all		obs.> avg. TOI, only oil		obs.> 1 S.D.+ avg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Westerlund ECM cointegration Tests									
Sample	*Truncated*	*Full*	*Truncated*	*Full*	*Truncated*	*Full*	*Truncated*	*Full*	*Full*
Panel Test t	-28.6		-12.9		-5.4		-5.8		
Robust p-value	0***		0.04**		0.2		0.08*		
Panel Test a	-10.7		-2.1		-5		-9.8		
Robust p-value	0.4		0.16		0.64		0.2		
Panel FE ECM Estimation									
Polity in levels t-1	-0.085	-0.087	-0.138	-0.141	-0.084	-0.149	0.001	-0.129	-0.1
(Error Correction Term)	[11.12]***	[11.55]***	[7.99]***	[8.47]***	[2.81]**	[3.32]***	[0.06]	[2.13]**	[1.87]*
Total Oil Income t-1	0.054	0.055	0.142	0.144	0.022	0	0.012	0.016	0.034
	[2.88]***	[2.90]***	[6.96]***	[6.83]***	[1.56]	[0.01]	[0.90]	[1.12]	[2.79]**
Total Oil Income	**0.637**	**0.634**	**1.03**	**1.02**	**0.259**	**0**	**-8.444**	**0.13**	**0.342**
Long-run Multiplier (LRM)	**[3.03]*** **	**[3.06]*** **	**[7.42]*** **	**[7.59]*** **	**[2.16]***	**[0.01]**	**[0.06]**	**[0.97]**	**[1.84]***
ΔTotal Oil Income	**-0.018**	**-0.02**	**0.038**	**0.034**	**-0.01**	**-0.131**	**0.01**	**-0.087**	**0.017**
	[0.91]	**[0.97]**	**[1.33]**	**[1.15]**	**[0.76]**	**[1.96]***	**[1.02]**	**[2.20]** **	**[0.59]**
Log(Per Capita Income) t-1	-0.286	-0.279	-1.998	-1.979	-0.074	0.621	-0.028	0.015	-0.082
	[0.90]	[0.88]	[6.21]***	[5.94]***	[0.27]	[1.21]	[0.17]	[0.04]	[0.22]
Civil War t-1	0.077	0.065	-0.271	-0.296		2.169		4.444	3.509
	[0.18]	[0.15]	[0.46]	[0.48]		[1.60]		[1.04]	[2.52]**
Regional Democratic Diffusion t-1	0.024	0.025	0.048	0.053	-0.027	0.01	-0.2	-0.018	-0.092
	[3.21]***	[3.49]***	[3.75]***	[4.31]***	[0.62]	[0.38]	[1.31]	[0.84]	[1.81]*
Global Democratic Diffusion t-1	0.04	0.038	0.257	0.264	-0.002	-0.273	-0.013	0.233	-0.085
	[1.63]	[1.54]	[12.87]***	[12.73]***	[0.13]	[3.11]***	[1.46]	[2.09]**	[3.50]***
Δ Log(Per Capita Income)	0.809	1.289	-1.447	-0.595	2.025	-2.101	0.87	-0.555	-0.433
	[0.46]	[0.74]	[0.50]	[0.22]	[1.32]	[0.64]	[1.00]	[0.27]	[0.19]
Δ Regional Democratic Diffusion	0.378	0.375	0.462	0.479	0.005	0.277	-0.107	0.104	0.021
	[5.30]***	[5.37]***	[5.89]***	[5.26]***	[0.14]	[3.35]***	[1.23]	[1.27]	[0.40]
Δ Global Democratic Diffusion	-0.248	-0.244	0.769	0.71	-0.007	0.075	0.186	-1.256	-0.298
	[2.34]**	[2.34]**	[11.15]***	[7.68]***	[0.11]	[0.92]	[1.25]	[2.28]**	[4.54]***
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	9876	10195	4631	4970	438	919	274	511	290
Number of groups	139	163	138	163	11	42	7	27	14
R-squared	0.1	0.1	0.14	0.15	0.16	0.21	0.27	0.32	0.27

* significant at 10%; ** significant at 5%; *** significant at 1%

Truncated refers to the minimum # of observations required for each panel in order to run the Westerlund ECM Panel Co-integration tests given the number of leads and lags estimated. Specifically, these models are run estimated with a lead of D.Total Oil Income to conform to weak exogeneity restriction; 1 lag refers to the fact that the estimation is performed with 1 lag of D.Polity and D.Total Oil Income to conform to no serial correlation restriction; moreover, each Westerlund ECM Co-integration test run with the Bartlett kernel window width set according to 4(T/100)^2/9; each test performed with bootstrapped critical values for test statistics due to contemporaneous correlation between panel observations. LRM standard errors estimated using the Delta Method: -1(b(Total Oil Income t-1)/b(Polity t-1)). Separate country & year intercepts estimated but omitted from table; F-test on joint significance of country and year dummies always highly significant.

Table 5. Panel Co-integration tests and Fixed Effects Estimation of Error Correction Models (ECM) for the Impact of Total Oil Income on Polity Score

Polity Score Normalized to run from 0 to 100

Robust t-statistics in brackets (Driscoll Kraay standard errors estimated with Newey West adjustment with 1 lag of the dependent variable)

REGION	**LATIN AMERICA**	**AFRICA**		**MENA**	**CENTRAL ASIA & EASTERN EUROPE**		**SOUTHEAST ASIA**	
	(1)	(2)	(3)	(4)	(5)	(6)	(9)	(10)
Westerlund ECM cointegration Tests								
Sample	*Full*	*Truncated*	*Full*	*Full*	*Truncated*	*Full*	*Truncated*	*Full*
Panel Test t	-13.4	-5.8		-8.6	-5.9		-8.5	
Robust p-value	0.08*	0.08*		0.24	0.88		0.04**	
Panel Test a	-16.1	-9.8		-8.7	-12.1		-10.9	
Robust p-value	0.12	0.2		0.4	0.32		0.28	
Panel FE ECM Estimation								
Polity in levels t-1	-0.109	-0.144	-0.144	-0.136	-0.194	-0.187	-0.084	-0.082
(Error Correction Term)	[6.83]***	[6.60]***	[6.62]***	[4.62]***	[4.05]***	[5.00]***	[3.22]**	[3.16]**
Total Oil Income t-1	2.532	0.023	0.022	0.038	1.794	0.152	-3.319	1.628
	[3.64]***	[0.14]	[0.14]	[1.31]	[1.43]	[0.13]	[0.55]	[0.48]
Total Oil Income	**23.228**	**0.162**	**0.154**	**0.277**	**9.243**	**0.815**	**-39.329**	**19.767**
Long-run Multiplier (LRM)	**[4.34]*** **	**[0.14]**	**[0.14]**	**[1.38]**	**[1.51]**	**[0.13]**	**[0.54]**	**[0.47]**
ΔTotal Oil Income	**1.097**	**-0.376**	**-0.374**	**-0.081**	**0.869**	**-1.637**	**7.241**	**0.495**
	[1.77]* **	**[1.14]**	**[1.15]**	**[2.03]*** **	**[0.31]**	**[0.73]**	**[1.26]**	**[0.16]**
Log(Per Capita Income) t-1	-0.202	-1.236	-1.21	1.546	3.584	1.631	-0.487	-1
	[0.27]	[1.92]*	[1.88]*	[3.26]***	[1.26]	[0.75]	[0.25]	[0.52]
Civil War t-1	0.975	-0.28	-0.281	0.72	-0.484	-0.033	1.922	1.971
	[0.95]	[0.41]	[0.42]	[0.57]	[0.30]	[0.03]	[1.39]	[1.44]
Regional Democratic Diffusion t-1	-0.044	-0.022	-0.022	0.031	1.606	1.564	-0.327	-0.327
	[0.79]	[5.52]***	[5.61]***	[0.39]	[20.69]***	[20.71]***	[10.42]***	[10.04]***
Global Democratic Diffusion t-1	0.49	0.46	0.459	0.406	-2.77	-2.809	-0.916	-0.861
	[2.78]**	[6.91]***	[7.02]***	[3.42]***	[16.87]***	[21.37]***	[5.66]***	[5.49]***
Δ Log(Per Capita Income)	0.845	5.281	5.161	2.436	11.092	7.941	-4.822	-5.393
	[0.21]	[1.34]	[1.32]	[0.69]	[1.66]	[1.97]*	[0.63]	[0.71]
Δ Regional Democratic Diffusion	0.809	-0.007	-0.007	3.377	1.552	1.567	-0.688	-0.689
	[1.68]	[2.46]**	[2.47]**	[25.70]***	[20.56]***	[31.74]***	[16.30]***	[16.31]***
Δ Global Democratic Diffusion	0.854	0.22	0.22	0.24	-1.741	-1.785	3.683	3.731
	[3.01]***	[6.50]***	[6.60]***	[3.48]***	[19.97]***	[23.31]***	[15.82]***	[16.36]***
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1939	1864	1893	961	652	938	482	486
Number of groups	20	43	45	18	9	30	9	10
R-squared	0.14	0.15	0.15	0.19	0.44	0.38	0.18	0.18

* significant at 10%; ** significant at 5%; *** significant at 1%

Truncated refers to the minimum # of observations required for each panel in order to run the Westerlund ECM Panel Co-integration tests given the number of leads and lags estimated. Specifically, these models are run estimated with a lead of D.Total Oil Income to conform to weak exogeneity restriction; 1 lag refers to the fact that the estimation is performed with 1 lag of D.Polity and D.Total Oil Income to conform to no serial correlation restriction; moreover, each Westerlund ECM Co-integration test run with the Bartlett kernel window width set according to 4(T/100)^2/9; each test performed with bootstrapped critical values for test statistics due to contemporaneous correlation between panel observations. LRM standard errors estimated using the Delta Method: -1(b(Total Oil Income t-1)/b(Polity t-1)). Separate country & year intercepts estimated but omitted from table; F-test on joint significance of country and year dummies always highly significant.

Table 6. Panel Co-integration tests and Fixed Effects Estimation of Error Correction Models (ECM) for the Impact of Total Oil Income on Polity Score

Polity Score Normalized to run from 0 to 100

Robust t-statistics in brackets (Driscoll Kraay standard errors estimated with Newey West adjustment with 1 lag of the dependent variable)

CONDITIONED BY	LOW INCOME INEQUALITY		HIGH INCOME INEQUALITY		POOR COUNTRIES		WEALTHY COUNTRIES	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Westerlund ECM cointegration Tests								
Sample	*Truncated*	*Full*	*Truncated*	*Full*	*Truncated*	*Full*	*Truncated*	*Full*
Panel Test t	-11.9		-16.9		-18.4		-13.8	
Robust p-value	0.28		0***		0.2		0.4	
Panel Test a	-6		-6.5		-10.6		-12.8	
Robust p-value	0.52		0.52		0.64		0.48	
Panel FE ECM Estimation								
Polity in levels t-1	-0.078	-0.078	-0.155	-0.154	-0.093	-0.093	-0.067	-0.069
(Error Correction Term)	[4.93]***	[4.96]***	[9.75]***	[9.77]***	[7.56]***	[7.56]***	[5.87]***	[6.30]***
Total Oil Income t-1	0.005	0.005	0.184	0.184	0.753	0.774	0.029	0.027
	[0.20]	[0.23]	[4.81]***	[4.86]***	[2.18]**	[2.28]**	[1.12]	[1.04]
Total Oil Income	**0.058**	**0.066**	**1.188**	**1.198**	**8.076**	**8.303**	**0.433**	**0.391**
Long-run Multiplier (LRM)	**[0.20]**	**[0.23]**	**[4.89]*** **	**[4.95]*** **	**[2.35]** **	**[2.46]** **	**[1.14]**	**[1.06]**
∆Total Oil Income	**-0.034**	**-0.033**	**0.065**	**0.058**	**-0.218**	**-0.26**	**-0.001**	**-0.006**
	[1.41]	**[1.39]**	**[1.23]**	**[1.11]**	**[0.27]**	**[0.33]**	**[0.05]**	**[0.24]**
Log(Per Capita Income) t-1	0.084	0.064	-2.156	0.004	-0.168	-0.178	0.19	0.219
	[0.24]	[0.18]	[3.67]***	[2.03]**	[0.28]	[0.29]	[0.31]	[0.37]
Civil War t-1	0.218	0.25	-0.136	-2.141	-0.104	-0.105	-0.579	-0.475
	[0.24]	[0.28]	[0.20]	[3.70]***	[0.14]	[0.14]	[0.54]	[0.47]
Regional Democratic Diffusion t-1	0.038	0.038	0.04	-0.182	0.021	0.021	0.051	0.054
	[2.14]**	[2.26]**	[2.63]**	[0.26]	[1.60]	[1.60]	[2.58]**	[2.75]***
Global Democratic Diffusion t-1	0.099	0.009	0.227	0.04	-0.19	-0.19	0.055	0.047
	[3.08]***	[0.68]	[8.76]***	[2.67]***	[5.60]***	[5.59]***	[0.77]	[0.66]
∆Log(Per Capita Income)	-0.157	-0.011	-1.081	0.266	0.464	0.436	-0.161	0.235
	[0.06]	[0.00]	[0.23]	[6.96]***	[0.15]	[0.14]	[0.07]	[0.10]
∆Regional Democratic Diffusion	0.419	0.406	0.492	0.063	0.346	0.345	0.49	0.468
	[2.52]**	[2.47]**	[7.56]***	[0.01]	[3.78]***	[3.78]***	[2.91]***	[2.92]***
∆Global Democratic Diffusion	0.548	0.11	0.164	0.498	-0.725	-0.725	-0.073	-0.023
	[10.08]***	[1.94]*	[1.83]*	[7.81]***	[7.52]***	[7.52]***	[0.21]	[0.07]
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2589	2689	2739	2825	3039	3043	3423	3604
Number of groups	59	66	61	67	48	49	32	45
R-squared	0.13	0.13	0.14	0.15	0.12	0.12	0.11	0.12

* significant at 10%; ** significant at 5%; *** significant at 1%

Truncated refers to the minimum # of observations required for each panel in order to run the Westerlund ECM Panel Co-integration tests given the number of leads and lags estimated. Specifically, these models are run estimated with a lead of D.Total Oil Income to conform to weak exogeneity restriction; 1 lag refers to the fact that the estimation is performed with 1 lag of D.Polity and D.Total Oil Income to conform to no serial correlation restriction; moreover, each Westerlund ECM Co-integration test run with the Bartlett kernel window width set according to 4(T/100)^2/9; each test performed with bootstrapped critical values for test statistics due to contemporaneous correlation between panel observations. LRM standard errors estimated using the Delta Method: -1(b(Total Oil Income t-1)/b(Polity t-1)). Separate country & year intercepts estimated but omitted from table; F-test on joint significance of country and year dummies always highly significant.

Table 7. Determinants of Transition from Democracy to Autocracy and from Autocracy to Democracy

Dynamic, Conditional Logit Transition Models (First-order Markov Chain)

Dependent Variable is REGIME (coded 1 if regime is autocracy and 0 if regime is democracy)

Robust z statistics clustered by country in brackets

	Model 1 (1818-2002)		*Model 2 (1973-2002)*	
Regime transitioning *from*	Democracy	Autocracy	Democracy	Autocracy
Regime transitioning *to*	Autocracy	Democracy	Autocracy	Democracy
Total Oil Income	**-0.562**	**1.326**	**-3.517**	**2.396**
t-1	**[1.01]**	**[2.05]****	**[2.14]****	**[1.68]***
log(Per Capita GDP)	-1.777	1.71	0.711	-0.543
t-1	[5.37]***	[5.71]***	[0.7]	[0.56]
% Growth of GDP Per Capita	-0.021	-0.032	-0.107	-0.034
t-1	[1.24]	[1.64]*	[2.85]***	[1.45]
% Civil War	0.519	0.306	-0.508	0.58
t-1	[1.29]	[0.69]	[0.83]	[0.98]
Pseudo R-squared	0.83	0.83	0.77	0.77
Observations	5934	5934	1770	1770

* significant at 10%; ** significant at 5%; *** significant at 1%; (un-interacted) lagged dependent variable estimated but not shown.
Time fixed effects estimated for full period with dummies from 1970-2002 (pre 1969 period as baseline); results robust to using 5 dummies of forty year periods; time fixed effects for 1973-2002 period estimated with yearly dummies (1973 as baseline), results robust to using twenty temporal splines.

Table 8. Panel Fixed Effects Estimation, difference in differences models for the Impact of Total Oil Income on Net Polity

Net Polity calculated from Polity Scores Normalized to run from 0 to 100

Robust t-statistics (calculated with Driscoll Kraay standard errors) in brackets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	*Full*	*1943-2006*	*Full*	*Equal*	*Unequal*	*Poor*	*Wealthy*
Specification	STATIC OLS	STATIC IV GMM	ARDL OLS	ARDL OLS	ARDL OLS	ARDL OLS	ARDL OLS
Δ Net Polity t-1			0.015	0.092	-0.01	0.021	0.073
			[0.75]	[2.77]***	[0.39]	[0.80]	[3.32]***
ΔTotal Oil Income (immediate impact)	**-0.086**	**-1.093**	**-0.059**	**-0.035**	**-0.068**	**-0.253**	**0.024**
	[1.23]	**[1.72]***	**[1.14]**	**[0.55]**	**[1.61]**	**[0.38]**	**[0.57]**
ΔTotal Oil Income t-1			0.284	0.249	0.328	0.348	0.275
			[3.78]***	[4.00]***	[3.27]***	[0.80]	[2.52]**
Total Change made by			**0.229**	**0.236**	**0.257**	**0.97**	**0.323**
ΔTotal Oil Income			**[3.49]*****	**[2.79]*****	**[2.81]*****	**[0.12]**	**[3.16]*****
Civil War t-1	1.579	-0.35	1.064	-1.244	-0.229	-0.405	0.136
	[0.88]	[0.62]	[0.63]	[1.13]	[0.24]	[0.13]	[0.05]
ΔLog(Per Capita Income)	-0.474	5.176	-0.529	0.868	1.931	-1.301	-1.775
	[0.89]	[1.73]*	[1.18]	[0.37]	[0.40]	[1.41]	[1.19]
ΔRegional Democratic Diffusion	-0.127	-0.15	-0.127	-0.11	-0.12	-0.19	-0.149
	[3.12]***	[2.12]**	[2.74]***	[0.85]	[1.84]*	[2.53]**	[1.16]
ΔGlobal Democratic Diffusion	0.007	-0.619	0.018	-0.053	-0.505	-0.395	-0.018
	[0.10]	[4.86]***	[0.24]	[0.54]	[6.68]***	[3.85]***	[0.06]
F-test on instruments in first stage		8.53					
p-value		0					
GMM C statistic chi2		0.667					
(Difference in Sargan test of endogeneity)		0.414					
Hansen's J chi2 for instrument validity		1.14					
(Overriding restrictions test)		0.565					
Country fixed effects	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES
Observations	9909	7087	9783	2562	2682	2854	3509
Number of groups	163	159	163	66	67	49	45
R-squared	0.02	0.001	0.02	0.06	0.02	0.05	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%

Static Models run with Newey West AR1 adjustment with 1 lag of the dependent variable.
A battery of heteroskedasticity tests reject the hypothesis that the error term is homoskedastic; Arellano Bond serial corellation test fail to reject AR(1); thus, IV GMM (Instrumental variables Generalized Method of Moments) approach is taken (only second stage output shown) with D.Total Oil Income as potentially endogenous, instrumented with Proven Oil Reserves, Oil Reserves per Surface Area, and Total Regional Oil Reserves (all in levels), and with weighting matrix estimated by an Eicker-Huber-White robust covariance estimator. Results are robust to introducing Total World Oil Reserves as additional instrument; results are also robust to excluding any of the other instruments (each of these enters significantly as determinants of D.Total Oil Income in first stage regression). ARDL refers to Autoregressive Distributed Lag Model; Standard errors for the Total Change made by Total Oil Income estimated using the Delta Method: ((ΔTotal Oil Income t + ΔTotal Oil Income t-1)/(1-(ΔPolity t-1)). Separate country & year intercepts estimated but omitted from table; F-test on joint significance of country and year dummies always highly significant.