Resource Wealth is an Amplifier, not a Curse: Evidence from Country-Level Effects of Exogenous Variation in Resources Endowments

Yu-Ming Liou
Georgetown University

Paul Musgrave
Georgetown University
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Yu-Ming Liou and Paul Musgrave
yl254@georgetown.edu and rpm47@georgetown.edu
Department of Government
Georgetown University

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Abstract

Is there a resource curse? We apply a new tool, the method of synthetic controls, to test old claims, that large oil endowments retard democratization and economic growth. To directly estimate the impact of resource wealth, we take advantage of the 1973 OPEC oil embargo, which caused a massive price spike that transformed previously marginal oil-producing countries into resource-reliant states. By estimating the counterfactual experiences of those countries, we find that there is little evidence of either a political or an economic resource curse. Nevertheless, resource wealth does affect policy outcomes. Leaders use resource wealth to pursue their policy preferences with greater vigor, a finding consistent with a theory of policy substitution that predicts greater variance in the choices of resource-rich regimes. Because resource wealth affords leaders greater policy autonomy, we conclude that resource wealth is not a curse but an amplifier.

Word Count: 8,471 words

Keywords: Q3, Oil, Oil Price, Resource Poor, Resource Rich, Resources
Does endowing a state with natural resources curse it? For almost a generation, political scientists and economists alike have usually answered “yes.” Economists believe the curse lowers growth rates; political scientists contend that it slows democratization. Yet some scholars contend that there is no curse. Indeed, Haber and Menaldo (2010a) assert that “roughly twice as many countries have been blessed by resource booms as cursed by them.”

Ideally, scholars would resolve this debate by assigning some countries at random to receive massive endowments of resource wealth. If the treated states democratized more slowly or their economies grew less quickly than a control group, then our confidence in the resource-curse hypothesis would be strengthened. If not, then we would side with the skeptics. Although researchers cannot conduct such an experiment, we argue that the OPEC oil embargo prompted by the October 1973 Yom Kippur War provides us with the next best thing: an exogenous treatment with analogous effects. Oil prices quadrupled in real terms within weeks of the embargo, leaving countries with hitherto marginal oil industries suddenly awash in oil earnings.

We use the method of synthetic controls to test if these newly oil-rich countries became less democratic or grew more slowly. We find no strong evidence supporting either hypothesis. In our political test, only two countries became significantly less democratic, while another became much more democratic. In our economic test, no country became significantly poorer, while one country became significantly richer. We argue instead that resource wealth amplifies leaders’ autonomy over policy choice, consistent with a theory of policy substitution. Accordingly, we argue that resource wealth can lead to the consolidation of democratization and increased economic growth. Unlike other scholars, we do not believe that institutions alone determine those outcomes. We must instead know something about the preferences of the regime to accurately predict the behavior of resource-endowed states. We supplement our quantitative tests with qualitative evidence to demonstrate that this is the pattern we observe.
Is resource wealth a curse or a blessing?

The resource curse literature is extensive and productive. Although new scholarship (e.g. Rudra and Jensen (2011)) has extended the debate’s scope, the central arena for testing the curse remains whether and how variations in oil wealth affects regimes.

Broadly speaking, scholars are divided into two camps. Those in the first camp, whom we term “oil pessimists,” believe there is a resource curse. Ross (2001) argues that resource wealth tends to make states less democratic because governments freed from bargaining over taxation with their populations have little incentive to secure popular consent or expand the franchise. Moreover, such states invest resource wealth in more effective repression, slowing or reversing democratization. Many researchers have reached similar conclusions. (Wantchekon 2002; Jensen and Wantchekon 2004; Gassebner, Lamla and Vreeland 2009; Aslaksen 2010; Tsui 2011; Ramsay 2011) Further work delivers similarly bad news on other fronts: resource wealth may exacerbate civil wars, erode women’s rights, and spur interstate conflict. (Ross 2006, 2008; Colgan 2010) In economics, much work has been inspired by Sachs and Warner (1995), including Goldberg, Wibbels and Mvukiyehe (2008), who find a negative relationship between resources and economic growth among individual U.S. states.

Members of the second camp, whom we term “conditionalists,” argue that the effects of natural resource wealth are contingent on other factors, particularly institutions (for instance, democracies may manage resource wealth better than non-democracies). Some in this camp even contend that resource wealth may even be a blessing. Using a long-term panel design, Haber and Menaldo (2011) conclude that in many cases there is evidence not of a resource curse but of a resource blessing. Haber and Menaldo (2010b) find that resources are “neither a curse nor blessing” in Latin America, broadly congruent with Lederman and Maloney (2007) and Dunning (2008). Morrison (2009) concludes that nontax government revenues (from both aid and natural resources) reduce the probability of regime transitions, a finding similar to Smith (2004). Humphreys (2005) argues that resource wealth does not prolong civil wars or cause their inconclusive resolutions. In economics, Robinson, Torvik
and Verdier (2006) propose that countries with accountable and competent governments will benefit from resource booms. Similarly, Alexeev and Conrad (2009) demonstrate that a windfall that lowers a country’s long-term growth rate could still leave it wealthier overall.

**Resource wealth, leader preferences, and policy variance**

We build on a theory of foreign-policy choice by Clark, Nordstrom and Reed (2008) to suggest another way that resource wealth affects policy outcomes. Clark et al argue that leaders vary not only in available resources but also in their preferences over foreign policy strategies. Given idiosyncratic variance in preferences, the more resources are available to leaders, the more variance their choices exhibit. We suggest that similar dynamics are at play when states receive resource windfalls. Accordingly, we put forward a broader conceptualization of what leaders seek to accomplish than most extant theories. Our distinction is similar to that between Mayhew (2004)’s and Kingdon (1977)’s models of politicians’ behavior. Whereas Mayhew believes that representatives are single-minded seekers of reelection, Kingdon proposes that they act to satisfy their constituents, maximize their influence in Washington, and enact good public policy, making reelection not a goal in itself but a means of achieving other ends. We propose, analogously, that retaining office is unlikely to be the sole objective of any leader. Instead, tenure in office should be desirable at least in part because it allows incumbents to effect policies they prefer. The comparison is imperfect: a congressman who loses an election does not fear he will also lose his life. Still, we argue that it is clear that even dictators have policy preferences over more dimensions than just extending their tenure in office.

We expect leaders to spend to maximize their utility along both security and non-security dimensions. Resource-rich governments’ behavior is consistent with our argument. Although resource-rich regimes do not publish budgets for their secret police, we have strong reasons to assume that they invest massive amounts in security even beyond the obvious tools of
repression. During the Arab Spring, for instance, Kuwait gave each of its citizens 1,000 dinars ($3,500), while Saudi Arabia bestowed nearly $40 billion in job grants and subsidies on its citizens. (Kerr (2011)) Yet resource-rich regimes do not behave strictly like security-maximizers but spend lavishly on other priorities. In the 1970s, the Saudi monarchy turned its customs agency from a revenue source into a net drain on the treasury. The agency’s principal duty changed from taxing imports to prohibiting the import of “immoral” items—a goal not obviously connected to security. (Chaudhry 1997)

Because our theory incorporates multiple dimensions along which leaders will maximize their utility, we make different predictions about both outcomes and process than other models. Given idiosyncratic variation in regimes’ utility functions over multiple dimensions, preference functions that produce nearly-identical outcomes when resources are scarce will yield different policy choices when resources are plentiful. Moreover, states with small selectorates should exhibit more variance, if only because preference outliers are more likely in a large number of small samples. In other words, ideal-typical autocracies should, ceteris paribus, exhibit more variation in their policy choices than democracies as resources increase. Thus, as similarly-situated autocratic leaders enjoy the benefit of resource wealth, they should begin to choose bundles of policies that are more different than alike. Conversely, similarly-situated democratic governments with stable institutions should be able to use their windfalls to benefit the entire public while being subject to less extreme policy variance.

The result of these factors is easy to describe. As Clark et al write, states that have more resources “choose less on the basis of cost and more on the basis of preference.” Much like earlier writers on the rentier-state hypothesis, we agree that one effect of resource wealth on governments is that it frees them from negotiating how taxes are raised and spent; we simply assume that governments might spend these windfalls on goals in addition to survival. Consequently, we say that resource wealth amplifies the effect of existing preferences. If there are diminishing returns to spending on security, as resources increase we should begin
to observe more resources being spent on other dimensions.

**Identification Strategy**

The testable implications of the oil pessimists, the conditionalists, and our theory are clear. If the oil pessimists are correct, then endowing countries with resource wealth should lead them to become or remain more authoritarian, to experience slower economic growth, or both. If the conditionalists are correct, then resource wealth endowments might lead to greater democracy, increased wealth (which might mean faster growth, but might not), or some combination of the two. If we are correct, however, then we should expect to see substantial policy variance on non-security dimensions in a manner congruent with leaders’ pre-treatment preference functions. Thus, we can distinguish our theory and other conditionalists’ postulates from the oil pessimists by looking at outcome variables, while we can test our logic against conditionalist theories by examining policy processes.

We seek to test these theories using a research design based on the 1973 oil price shock. We first demonstrate the principal blind spots inherent in conventional studies. We discuss how different approaches proposed in the literature to cope with that flaw introduce new problems. We then establish that the 1973 oil price shock was both unexpected and exogenous for marginal oil producers. Finally, we argue that the method of synthetic controls offers a unique opportunity to test hypotheses about the local effect of resource wealth.

**The pitfalls of conventional analyses**

We agree with Herb (2005) and Haber and Menaldo (2011) that cross-sectional research designs capture between-country, not within-country, variation. If regimes that are resource-rich also happen to be authoritarian for reasons that have nothing to do with resources, then between-country measurements will nevertheless return a strong and negative association between authoritarianism and resource wealth. Since the theory of the resource curse makes
predictions about within-country variation, namely that endowing states with resources will cause those states to become less democratic and grow more slowly than they otherwise would, this implies that cross-sectional studies have a hard time distinguishing between a world in which resources made states authoritarian and one in which authoritarian states happened to possess resources.

Solving these problems is difficult. Haber and Menaldo (2011) propose to do so by employing a very long (as much as 200 years) observational window, which they argue allows them to observe treatment, and thus within-country variation, directly. We find this strategy unsatisfying. Many regimes characterized as “oil states” may have been resource-reliant even before petroleum mattered. Crystal (1995) establishes that Gulf Arab states were dependent on resource extraction (e.g., pearling) before the discovery of oil in the Arab peninsula. Similarly, pilgrim fees and other revenues derived from controlling Mecca and Medina were mainstays of the Saudi monarchy’s treasury long before the exploitation of oil. (Chaudhry 1997) Further, many oil-rich regimes’ initial and continued independence may itself have been affected by their possession of oil wealth. Colonial powers such as the United Kingdom may have delayed the timing and terms of independence of resource-rich countries. Great powers might be reluctant to see such countries conquered. Certainly, U.S. policy toward Kuwait and other Gulf states suggests that Washington has such concerns in mind. Accordingly, strategies that rely on long observational windows may be subject to unanticipated challenges to inference.

Finally, such a strategy requires that the same processes be at work throughout the entire duration of the study. The most obvious challenge to that assumption is that petroleum was not particularly valuable to governments until at least the mid-twentieth century. According to the Energy Information Administration, even in the United States, petroleum did not overtake wood as a source of energy until the mid-1910s or coal until about 1950. For authors who begin their observations in, say, 1970 or later, that is not a concern. For researchers using a century or more of data, it is a critical point. The history of petroleum suggests that
there was a radical disjunction in the structure of oil markets the early 1970s. From the Rockefeller era of the 19th century until the late 1960s, Western firms restricted competition and set prices in collaboration with Western governments. That world of stable prices and managed markets is fundamentally different from the contemporary world of price volatility and nationalized oil firms. (Yergin 1991; Maugeri 2006) The 1973 price shock, which affected both average price and variance, underlined the fact that the West no longer controlled oil prices; see Figure 1. Indeed, augmented Dickey-Fuller tests of oil prices since 1900 and global oil production since 1965 strongly suggest that both series are nonstationary. A more searching investigation of price trends for 26 primary commodities by Cuddington (1992) finds that only two commodities exhibit any structural breaks at all: Coffee in 1950—and oil in 1974.¹ Consequently, analyzing oil production and pricing across the October 1973 threshold on the basis that there is no major difference in the underlying data-generating process is an assumption that we feel is unjustified.

¹Accounting for timing of measurement, Cuddington’s 1974 is equivalent to our 1973.
Another strategy that has been employed is to use resource stocks or the discovery of resource endowments as a treatment (e.g. Tsui (2011)). This is subject to objections on two points. First, the same processes affecting existing reserves also affect discovery, since firms are more incentivized to explore when prices are high. Moreover, researchers cannot trust measures of existing reserves to identify resource endowments. Measurements of countries’ oil reserves are not simply counts of hydrocarbons in the ground. The energy firm BP (2011) defines proved reserves as “those quantities that geological and engineering information indicates with reasonable certainty can be recovered in the future from known reservoirs under existing economic and operating conditions.” BP’s definition, like those used by the CIA, the Energy Department, and other authorities, demonstrates why proved reserves may rise or fall even without any extraction, technological innovation, or discovery. For instance, the International Energy Agency argues that the doubling of estimated reserves in the Middle East and North Africa between 1984 and 2004 was due not to discoveries or technological enhancements. Instead, the IEA attributes the increase to a combination of oil companies’ nationalization, exempting them from strict S.E.C. accounting regulations, and OPEC rules, setting production quotas based partly on members’ unaudited reports of their own reserves.

A related debate concerns how to measure resource wealth. Early studies measured oil income as a fraction of GDP. However, as Brunschwiler and Bulte (2008) argue, “a negative correlation between this variable and growth could mean that resources lead to slower economic growth . . . [or] that poor economic development policies—leading an economy to become dependent on its primary exports—dampen growth.” They find that the resource curse disappears when they instead use World Bank estimates of the discounted expected value of resource rents. Herb (2005) creates counterfactual income series for resource-endowed countries based on similar countries. He concludes that there is no consistent support for the rentier-state thesis. Even Ross (2009) concedes that his earlier measure of oil wealth was flawed, although his revised tests continue to support the rentier effect. Because we believe that the critical element in whether a state is endowed with significant amounts of
resources is the amount of revenue it can generate from that endowment, we turn to Haber and Menaldo (2011). Their extensive primary-source research has yielded a new variable, *Fiscal Reliance*, which directly measures how much of a state’s revenues come from resource wealth. As we discuss below, we prefer this measure because it directly taps what the rentier-state hypothesis states is crucial: the degree to which states depend on nontax revenues from natural resources. Consequently, we use their measure to identify treated countries.

The exogeneity of the 1973 shock

The 1973 oil price shock transformed oil industries in countries that had previously been marginal producers into major sources of state revenue. In 1970, the Mexican government raised essentially zero income from resources. By 1980, oil accounted for two-fifths of its revenues. In Gabon, per capita income from oil peaked at more than $10,000 (in 2007 dollars) in 1979—approximately 20 times the country’s *entire* real per-capita GDP in 1965. The situation was similar for several other countries, as we discuss below.

The price shock was both exogenous and unexpected for marginal oil producers. Darmstadter and Landsberg (1975) show that 1960s-vintage energy forecasts about the 1970s produced by firms, governments, and institutions like the OECD were consistently too conservative. Although a tiny spot market had emerged in the early 1970s, prices remained comparatively stable and low, despite the large differentials that could have been exploited by speculators with foreknowledge of a price increase. More important, marginal producers played no role in bringing about the sudden price increase. Politics drove the embargo. It was the result of Arab oil-producing states’ reaction to the Yom Kippur War. Although some Middle Eastern governments had long advocated using the “oil weapon” against Israel, efforts along those lines in 1956 and 1967 had foundered quickly, not least because Saudi Arabia was reluctant to participate. By the spring of 1973, however, the Saudis’ attitude toward the Israelis was hardening. (Lenczowksi (1975) suggests that this was partly because King Faisal wanted to make a pilgrimage to an Arab-ruled Jerusalem.) Accordingly,
President Nixon’s decision to give Israel billions of dollars of weapons during the October war prompted Riyadh to wholeheartedly support an embargo. Even though OPEC then included members from outside the Middle East—including Algeria, Ecuador, Indonesia, and Nigeria—a group of six states made the crucial decisions. On October 16, the “Gulf Six”—Saudi Arabia, Kuwait, Iran, Iraq, Qatar, and the United Arab Emirates—voted to raise the price of Saudi oil by 70 percent, from $3.011 to $5.119 per barrel. The full membership of OPEC approved the increase in November. In December, the Gulf Six, now officially empowered as an OPEC ministerial committee, more than doubled the price of Saudi oil to $11.651—an increase of about 300 percent over pre-October prices. (Lenczowksi 1975)

The record makes two points clear. First, although oil-producing countries had long been interested in forming a cartel, no such agreement had ever stuck because there was no way to overcome the problems of collective action. Anti-Israeli and anti-American sentiment spurred by the war alleviated that problem. Second, the decision to impose the embargo was made by a handful of OPEC members. Even within that group, Saudi Arabia exercised especial influence. States outside the Gulf Six were neither in a position to make policy nor to ramp up production in anticipation of such a surge, meaning the price shock was exogenous with respect to these countries.

Consequently, our identification strategy is straightforward. Like Gelb (1988), we posit that windfall gains from oil transformed several countries into resource-reliant states. The 1973 price spike was massive: Karl (1997) observes that for oil-exporting countries “the transfer of wealth in 1973 and again in 1980 produced greater revenues than those available to them over the entire past century.” By exploiting this shock, we argue that we have a better chance at avoiding the factors that confounded earlier studies and providing estimates of the local treatment effect of resource wealth on the newly treated regimes. Our strategy is similar to Luong and Weinthal (2010), who exploit the sudden independence of post-Soviet states to see how variations in political institutions affect the extraction of natural resources. However, we address the resource curse more directly, investigate a greater diversity of treated
regimes, employ a methodology designed to generate precise counterfactuals, and examine regimes that did not emerge from the collapse of the Soviet Union—which may itself have been endogenous on oil price changes. (Gaidar 2007) Moreover, we take advantage of a new method that allows us to estimate the impact of resource treatments directly.

The method of synthetic controls

The method of synthetic controls, developed by Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010), offers an effective strategy for coping with the problems earlier studies have faced. In general, synthetic controls should be considered when there is both a discrete treatment and insufficient data or variation to support the use either of traditional observational methods or of matching methods for causal inference—exactly the situation for our research question.

The method is best-suited to a discrete treatment that affects only one or a few units in a largely untreated population. Given data for a sufficiently long period before and after an intervention, the method uses data from a universe of similar units to create a virtual “control unit,” weighted both by country and predictor variable, that is as identical as possible to the treated unit in the period before a hypothesized treatment. The researcher then evaluates the differences between the synthetic control unit’s predictions and the observed unit’s actual behavior to see if the intervention actually yielded a meaningful effect. Thus, using synthetic controls allows us to estimate the local individual treatment effect of an intervention.

More formally, the synthetic control method takes information from $J$ units (the donor universe) and one unit suspected to have been treated (which we refer to as the “observed” unit). We will evaluate these units over a period of $T$ time periods (where $1 \leq T_0 < T$ is the number of pre-intervention periods and $T_1 = T - T_0$ is the number of post-intervention periods). Let $Y_{\text{control}}$ be a $(T \times J)$ matrix that contains the values of the outcome of interest for the donor universe and $Y_{\text{obs}}$ be a $(T \times 1)$ vector that contains the same information for the treated unit. The donor universe provides inputs to $W = (w_1, \ldots, w_J)'$, a $(J \times 1)$
vector of nonnegative weights \( \left( \sum_{i=1}^{J} w_j = 1 \right) \). For each time period, we also have a \((K \times 1)\) vector \( \mathbf{X}_{\text{obs}} \) and a \((k \times J)\) matrix \( \mathbf{X}_{\text{control}} \), representing the pre-intervention characteristics on \( K \) predictor variables for each unit in the donor universe and the treated countries. If we take these matrices across the entire pre-treatment period, then we choose vector \( \mathbf{W}^* \) to minimize the distance \( ||\mathbf{X}_{\text{obs}} - \mathbf{X}_{\text{control}}\mathbf{W}|| \); the method uses \( ||\mathbf{X}_{\text{obs}} - \mathbf{X}_{\text{control}}\mathbf{W}|| = \sqrt{(\mathbf{X}_{\text{obs}} - \mathbf{X}_{\text{control}}\mathbf{W})'\mathbf{V}(\mathbf{X}_{\text{obs}} - \mathbf{X}_{\text{control}}\mathbf{W})} \), where \( \mathbf{V} \) is a \((k \times k)\) symmetric and positive semidefinite matrix that weights the linear combinations of the variables in \( \mathbf{X}_{\text{control}} \) and \( \mathbf{X}_{\text{obs}} \).

In other words, \( \mathbf{W}^* \) defines the combination of donor units that most resemble the treated unit before the intervention period. Consequently, the nearer \( \mathbf{Y}_{\text{obs}} \) and \( \mathbf{W}^*\mathbf{X}_{\text{obs}} \) in the pre-treatment period, the nearer the synthetic control \( \mathbf{Y}_{\text{synth}} \) will be to the observed \( \mathbf{Y}_{\text{obs}} \).

We can now use this information to discern the difference between the synthetic control unit’s prediction and the observed behavior of the treated unit— an interpretation similar to a standard difference-in-difference design. (Hinrichs 2010) That is, we want to estimate \( \mathbf{Y}_{\text{obs}} - \mathbf{Y}_{\text{synth}} \) in the post-treatment period, where \( \mathbf{Y}_{\text{synth}} \) is the vector of the predicted values of the outcome of interest. Accordingly, we define \( \mathbf{Y}^*_{\text{synth}} = \mathbf{Y}_{\text{control}}\mathbf{W}^* \), thereby using the country weights optimized from the pre-treatment training period. Abadie, Diamond and Hainmueller (2010) present a fuller exposition of the underlying mathematics, while Abadie, Diamond and Hainmueller (2011) describe the software necessary to implement the method.

Comparing the synthetic control unit to the actual treated unit is insufficient for hypothesis testing. Following the practice in Abadie, Diamond and Hainmueller, we use placebo tests to determine whether the effects of the posited treatment are limited to only the putatively treated case or are common to other, untreated cases. Much like a placebo in a medical experiment, we run the same procedure on every country in the donor universe. If the gap between the outcomes of the putatively treated case and the other countries is sufficiently large, our confidence in the evidence is increased; if not, then our confidence is accordingly lowered. Moreover, we can compute a simple measure of whether the outcome we observe is due to chance or not by assuming the ordinal position of each unit is determined by a
random walk and then calculating

\[ p = \frac{\text{Ordinal}_{\text{synth}}}{N} \]  

(1)

where \( \text{Ordinal}_{\text{synth}} \) is the ordinal rank of the synthetic control and \( N \) is the number of countries in the placebo test (\( J + 1 \), to include the country of interest itself). For obvious reasons, this is a one-tailed test, and is meant to be more suggestive than probative.

Although synthetic controls and many varieties of matching are similar in some ways, matching is not appropriate in exactly the situations where synthetic controls are most desirable. Matching methods are necessarily cross-sectional, while synthetic controls use panel data. More problematic, by using matching (particularly exact matching), we would assume unit homogeneity where none exists. This is often ignorable when there are large and similar enough samples. Even if any individual counterfactual claim is implausible, researchers could justifiably say that since the treated and untreated populations are equivalent before treatment on average that any observed difference between populations constitutes the average treatment effect. Similarly, in propensity score matching, the researcher relies upon the propensity score to stochastically balance the covariates in the treated and the untreated populations. However, in small samples (common in international relations, comparative politics, and state-level American politics), this assumption is likely to be violated. Unlike synthetic controls, matching requires a rough balance to exist between the characteristics of the treated and the untreated populations. No such balance exists in the universe we study.\(^2\)

The counterfactual outcome we test using the method of synthetic controls is not whether the states we investigate would have democratized without the 1973 price shock. Rather, it is whether they would have democratized if they had not benefited the windfall increase in the value of their petroleum production and reserves but had instead been without any

\(^2\)The process for constructing synthetic controls is akin to constructing propensity scores for matching, but synthetic controls are more flexible than standard PSM methods since both unit and covariate weightings are allowed to vary over each unit. In contrast, in standard propensity scores, unit weights are constrained to be either 0 or 1, and units are either matched or discarded. (Hirano, Imbens and Ridder 2003)
such industry. We believe that our research design allows us to evaluate the counterfactuals posited by the contending theories in a more direct way than previous studies. As the statistician John Tukey advised, “far better an approximate answer to the right question … than the exact answer to the wrong question.” Using the synthetic control method, for the first time we believe that we can begin to estimate the answers to the correct question.

Testing the resource curse theory

Our research design focuses on eight countries: Algeria, Ecuador, Gabon, Indonesia, Mexico, Nigeria, Norway (for GDP models only), and Trinidad and Tobago. We emphasize that this set represents the universe of treated countries for which we can conduct empirical investigations. Focusing on these eight countries allows us to directly investigate both outcomes and processes for each country. Table 1 displays the magnitudes of the oil shocks affecting each country, as well as the results of Augmented Dickey-Fuller tests suggesting that the trends for oil income, production, and fiscal reliance are nonstationary.

We identify the countries that the 1973 price shock transformed into resource-reliant states using Haber and Menaldo’s data on fiscal reliance on oil, gas, and minerals for 17 major oil producers. Their data is the best available on fiscal reliance. Proper identification requires us to exclude countries that do not receive a clear treatment. Table 2 summarizes our reasons for excluding different countries. We exclude countries that did not significantly increase their fiscal reliance on oil before and after the shock. We exclude countries that became independent after the treatment, as well as those that only had a brief period of independence before 1973. We also exclude countries for which it is impossible to build cotenable counterfactuals since they have extreme scores on the Polity scale, including both hereditary monarchies (Polity score of -10) and consolidated democracies (+10). We discuss the exclusion of consolidated democracies below. Hereditary monarchies appear to be

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3They are Mexico, Venezuela, Ecuador, Trinidad and Tobago, Nigeria, Angola, Indonesia, Iran, Algeria, Bahrain, Equatorial Guinea, Gabon, Yemen, Oman, Kuwait, Saudi Arabia, and Norway; they also collect data on two copper producers, Chile and Zambia.
Table 1: “Oil shock” describes the change in percent for oil income per capita (Oil Income) and each country’s fiscal reliance (Fiscal) between the pre-treatment (1965–73) and post-treatment (1974–85) period. We also report Production to illustrate the difference between simple increases in hydrocarbon production and actual resource wealth endowment. ADF reports MacKinnon’s approximate p-value for Augmented Dickey Fuller Tests (with trends) for each component over the period 1965-2009.

<table>
<thead>
<tr>
<th>Country</th>
<th>Oil Income</th>
<th>Fiscal</th>
<th>Production</th>
<th>ADF</th>
</tr>
</thead>
<tbody>
<tr>
<td>ALGERIA</td>
<td>334%</td>
<td>141%</td>
<td>24%</td>
<td>0.63</td>
</tr>
<tr>
<td>ECUADOR</td>
<td>6,200%</td>
<td>1,987%</td>
<td>488%</td>
<td>0.76</td>
</tr>
<tr>
<td>GABON</td>
<td>880%</td>
<td>1,176%</td>
<td>109%</td>
<td>0.61</td>
</tr>
<tr>
<td>INDONESIA</td>
<td>823%</td>
<td>253%</td>
<td>95%</td>
<td>0.74</td>
</tr>
<tr>
<td>MEXICO</td>
<td>1,760%</td>
<td>1,160%</td>
<td>299%</td>
<td>0.86</td>
</tr>
<tr>
<td>NIGERIA</td>
<td>892%</td>
<td>262%</td>
<td>95%</td>
<td>0.44</td>
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<tr>
<td>NORWAY</td>
<td>44,110%</td>
<td>36,862%</td>
<td>56%</td>
<td>0.98</td>
</tr>
<tr>
<td>TRINIDAD</td>
<td>529%</td>
<td>122%</td>
<td>28%</td>
<td>0.77</td>
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thoroughly different than other countries, even those with Polity scores of -9 (consolidated autocracies), not least because all four of these countries were once British colonies or protectorates, are neighbors, and relied on oil long before 1973. Even if they were not excluded on other grounds, we believe it would be impossible to construct meaningful controls for them since they would be compared with the experiences of states that did not have a pre-modern form of political organization. Note that we exclude some countries on multiple grounds even though failing any one test is sufficient for exclusion.

Table 2: Summary of reasons to exclude countries identified by Haber and Menaldo as highly fiscally reliant from our treatment universe.

<table>
<thead>
<tr>
<th></th>
<th>No Oil In Period</th>
<th>Independent Too Late</th>
<th>Hereditary Monarchy</th>
<th>No Variation in Fiscal Reliance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Angola</td>
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<td>Bahrain</td>
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<td>Chile</td>
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<td>Eq.Guinea</td>
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We must also specify a donor universe. Because the synthetic control method will use information from the donor universe to construct the synthetic control, the choice of countries to include must be theoretically motivated. It is far better for us on both research-design and methodological grounds to have a universe that maximizes the information it contains instead of trying to have the largest possible universe. We are principally worried about introducing baseline bias into the control group by including pre-treated countries. We begin by excluding significant oil-producing countries and countries listed by Haber and Menaldo as being resource-dependent. We also omit countries for which we do not have consistent
information on the covariates (population, real per capita GDP, government share of GDP, and Polity scores). We are also concerned about minimizing interpolation bias by including countries who appear to be uninformative controls. Consequently, we omit all Communist countries, which were very unlikely to exhibit variation on the dependent variable for reasons exogenous to domestic factors. We also exclude South Africa and Singapore. Singapore is simply too rich for its political development. We consider the apartheid regime in South Africa so different from other countries in the risks to its survival and the structure of its economy that it did not appear theoretically relevant.

For our political model, we exclude states that do not meaningfully control their domestic political systems, such as Lesotho and Swaziland. We also exclude all countries that are consolidated democracies (Polity scores of 10). These states are not informative controls since they are practically guaranteed to exhibit no variation on the dependent variable. It is exceptionally rare for any state that has ever attained a score of 10 to revert to a non-democratic state. (Epstein, Bates, Goldstone, Kristensen and O’Halloran 2006) Of the 31 countries in our sample that attained a maximum Polity score of +10 at any point between 1960 and 2000, only Malaysia’s coding fell after it attained a +10. The remaining countries—more than 40 of them—still leave us a large basis upon which to derive the synthetic controls.

For our economic model, we specify donor universes by stratifying countries on income levels. For Algeria, Ecuador, Gabon, Mexico, Norway, and Trinidad and Tobago, our donor universes comprise countries with a real GDP per capita in 1970 of not more than 50 percent greater and not less than 50 percent smaller than each country. Because Nigeria and Indonesia have such small and similar real per-capita GDPs in 1970 ($1,476.62 in 2010 USD for Nigeria and $1,235.85 for Indonesia, compared to the next poorest countries, $3,175.70 for Ecuador and $6,190.26 for Mexico), we include all countries with per-capita GDPs under $2,500 in their donor universes.
Model and Data

Consistent with extensive previous research, we view democratization as a function of countries’ political development, economic development, and human capital. As has become standard in the literature, our dependent variable is the Polity measurement of regime type, revised for time-series analysis.\(^4\) Because Polity scores are the best predictor of subsequent years’ Polity scores, we lag this variable twice in each model. Many scholars have posited a relationship between economic growth and democratization. (Boix and Stokes (2003); Geddes (1999); Kennedy (2010).) To measure economic development, we employ the Penn World Tables’ Real GDP Per Capita. To measure states’ existing degree of government involvement in the economy, we use the Penn World Tables Government Share of GDP variable, much like Haber and Menaldo (2011) and Herb (2005). We also include Population to capture any effects of scale. Finally, we view education as important in its relationship to democratization, a position dating back at least a century. (Dewey 1916; Lipset 1959; Barro 1999; Glaeser, Porta, de Silanes and Shleifer 2004) We use Alvarez, Cheibub, Limongi and Przeworski (1996)’s Education, defined as the cumulative years of education of the average member of the labor force, to measure human capital. Real GDP per capita and government share of GDP are measured across the entire optimization period; education, which is slower to change, is measured at 1968 levels.

Our economic growth model is a simple two-factor model with controls. Our dependent variable is now Real GDP Per Capita; we again lag this variable twice during the optimization procedure. We retain Population, again measured for each year, and Education, again measured in 1968. We discard Government Share of GDP in favor of another Penn World Tables variable, Investment Share of GDP, which we view as a more reliable guide to strictly economic outcomes. In the models we present here, we do not include Polity in the

\(^4\)The 10-point autocracy score is subtracted from the 10-point democracy score, yielding a −10 to 10 variable. The “Revised Combined Polity Score,” as compiled by the Quality of Government Institute, modifies this combined score further by treating cases of foreign rule (normally coded as −66) as missing cases, cases of anarchy (normally coded as −77) as zero, and cases of “transition” (normally coded as −88) as prorated across the span of the transition. (Teorell et al 2011)
optimization procedure, although our results are robust to its inclusion.

The synthetic controls procedure will optimize the fit of the synthetic control unit across the pre-treatment period (1965–73) and then compare the outcome of the synthetic and the observed units in the post-treatment period (1975–85). We chose these periods to afford us as many years as possible before the treatment year while also maximizing the number of countries in the donor universe (a consideration affected by the waves of decolonization in the early 1960s). The long periods before and after the treatment allow us to build a synthetic control unit that closely approximates the treated unit and to trace effects that may require several years to appear.

Results

One of the advantages of the synthetic control method is that the interpretation of results can be done graphically and intuitively. We discuss the results of the the economic models. Interested readers can see the output of the synthetic control method’s weighting matrices for the eight countries of interest we present here, as well as robustness checks, in the supplementary information. Figure 2 displays separate results for the eight countries in the economic model test group.

Readers should examine how closely the synthetic control approximates the performance of the observed country in the pre-treatment period. Ideally, every plot would look like Mexico (Figure 2c), where the two lines are practically indistinguishable; a line such as Indonesia’s (Figure 2g) demonstrates a less satisfactory fit. We provide these plots comparing the observed outcomes for the country of interest with the synthetic control unit to explore both the magnitude of the posited effects as well as to provide a more intuitive illustration of how the method works. No clear trend toward lower economic growth emerges in these countries. Mexico (Figure 2c), Ecuador (Figure 2d), and Norway (Figure 2h) all demonstrate both an exceptionally close fit in the optimization period and higher incomes in the post-
Figure 2: Comparison of per capita real GDP for observed units (solid lines) and synthetic counterfactuals (dashed lines). Vertical dashed line represents 1973.

(A) Nigeria

(B) Trinidad

(C) Mexico

(D) Ecuador

(E) Algeria

(F) Gabon

(G) Indonesia

(H) Norway
treatment period, while the results for Gabon (Figure 2f) and Trinidad (Figure 2b), which are also good fits in the pre-treatment period, are more equivocal.

We show the results of each country’s placebo test in Figure 3. We represent the gap between the synthetic control unit and the observed country’s outcome as a thick black line. The light gray lines represent the gap between each placebo country’s synthetic control unit and its observed outcome. (In other words, since France is in the donor universe for Norway, one of the gray lines in Norway’s placebo plot represents the outcomes of a synthetic control analysis for France.) The placebo test gives us a sense of how confident we should be in interpreting the gap between the synthetic control unit and the observed country’s performance as meaningful or merely due to chance. Thus, Figure 3h demonstrates that Norway’s economic growth during the post-treatment period was indeed exceptional. No other country even comes close to replicating this pattern. By contrast, when we examine the performance of Mexico (Figure 3c) and Ecuador (Figure 3d), our confidence that the pattern from the country-level plots is meaningful is decreased. The high p-values on both countries’ performance suggests that the gap between their synthetic control and their observed outcomes is due largely to chance. Similar arguments hold a fortiori for the other countries in this universe.

Overall, there is little support for the oil pessimists’ position. None of the countries we observe display a significant negative trend in their per-capita GDP. By contrast, Norway clearly outperforms its control, with a per-capita GDP nearly $4,000 higher in 1985 (in 2007 dollars) than it would have been without the oil shock. Although we are cautious in interpreting such point predictions, the magnitude of that gap suggests that Norwegians really were made better off because of the treatment.

Figure 4 shows the results of the country-level analysis for political outcomes. (As noted above, we exclude Norway because it was a consolidated democracy.) The synthetic procedure closely matches the observed country in each case except for Nigeria (Figure 4a) and Ecuador (Figure 4d). Even those cases actually lend support to the model’s performance,
Figure 3: Placebo plots for economic tests showing the difference between the outcomes for observed and the synthetic units for each country. The country of interest is represented by a thick black line; donor countries are shown in light gray. Vertical dashed line represents 1973.

(A) Nigeria, $p = 0.27$

(B) Trinidad, $p = 0.28$

(C) Mexico, $p = 0.34$

(D) Ecuador, $p = 0.48$

(E) Algeria, $p = 0.46$

(F) Gabon, $p = 0.43$

(G) Indonesia, $p = 0.16$

(H) Norway, $p = 0.05^*$
since the irregularities are easily explained. The Biafra war made Nigeria more autocratic than had been expected in the mid-1960s. Ecuador’s Polity scores are high during the late 1960s, but constitutional rule was need by President José María Velasco Ibarra himself in an autogolpe, or “self-coup,” in June 1970. (This suggests that the synthetic control’s estimate of the nature of political institutions in Ecuador during the late 1960s may have been more accurate than the Polity measure.)

What stands out are the results for Nigeria, Ecuador, Mexico (Figure 4c), and Algeria (Figure 4e). Ecuador appears much more democratic—a nearly 15-point swing on the 21-point Polity scale during most of the post-treatment period—than the synthetic control suggests it should have been. Nigeria displays a similar gap during most of the Second Republic (1979–83), with a Polity score nearly 12 points higher than its synthetic control. By contrast, both Mexico and Algeria are less democratic than predicted. In Figure 5, we compare these results with each country’s placebo tests. We see that the results for Mexico, Ecuador, and Algeria are indeed significant at conventional levels. Had we stopped the clock in 1980 instead of 1985, Nigeria, too, would be significantly more democratic than predicted. Even in this case, Nigeria is no less democratic at the end of the period than it is predicted to be—evidence against the claim that oil wealth made it authoritarian. Given the strength of the oil pessimists’ claims, the lack of any trend is yet more evidence against their theory.

Preferences and Autonomy in Policy Processes

So far, we have presented evidence inconsistent with the pessimists’ position. Although we believe that the contribution that our tests make is valuable in itself, we proceed by comparing the predictions that our theory makes with those made by other conditionalist theories. In particular, as we explained above, we contend that our theory is better able to explain how resource wealth amplifies leaders’ ability to act on their preferences over several dimensions of policy choices. If regimes converged on one preference function or policy bundle after treatment, it would be evidence against our claims; if not, it is evidence
Figure 4: Comparison of Polity scores for observed units (solid lines) and synthetic counterfactuals (dashed lines). Y-axes are on a common scale.

(A) Nigeria

(B) Trinidad

(C) Mexico

(D) Ecuador

(E) Algeria

(F) Gabon

(G) Indonesia
**Figure 5:** Placebo plots for political tests.

(A) Nigeria, $p = 0.44$

(B) Trinidad, $p = 0.43$

(C) Mexico, $p = 0.07^+$

(D) Ecuador, $p = 0.05^*$

(E) Algeria, $p = 0.09^+$

(F) Gabon, $p = 0.32$

(G) Indonesia, $p = 0.36$
against strict institutionalist theories. Accordingly, we return to the four countries that displayed significant results in our models (Norway, Ecuador, Mexico, and Algeria), as well as Nigeria, which would have been significantly more democratic if we had chosen to end our observational window before 1983. We focus on these countries because they most clearly illustrate the dynamics we describe above. Tellingly, four of these countries entered the treatment period as authoritarian or semi-authoritarian. Yet the nature and preferences of these governments’ leaders explains the subsequent divergence in their policy choices neatly.

Although Mexico became less democratic, in keeping with the pessimists’ predictions, we argue that a closer examination of its experience instead supports our claims about policy autonomy. The PRI never behaved like a purely authoritarian regime rewarding a minimum winning coalition. It was a large coalition of the poor, not a narrow coalition of the upper and middle classes. Sustaining such a coalition was expensive: “The economic costs of sustaining an oversized coalition also rise as the one-time windfalls of the initial seizure of power are used up . . . .” (Magaloni 2006) Unsurprisingly, the coalition was interested in continuing to prove private and club goods for its supporters, even though the strategy was failing by the early 1970. Oil wealth reversed that decline. The PRI used oil largesse to underwrite a rapid expansion of government spending, both for public works and social spending for the poor. (Krauze 1997) Oil wealth allowed the PRI not only to hold onto power but also to pursue its leftist and redistributionist goals. Had the shock taken place later, when the PRI’s decline was more advanced, or had the PRI been less leftist, we would have expected to see different policies enacted.

The contrast with Algeria is clear. Much as in Mexico, a previous regime strategy of supporting a broad coalition was on the verge of failing when the oil price shock hit. The end of Algeria’s war for independence from France left the rebel Armée de la frontière as the country’s dominant political force. The patronage that sustained the military government was based upon the distribution of a windfall gain: the bien vacants, the homes and businesses abandoned by Europeans who had fled the country. That source had nearly been exhausted
by 1973, when oil revenues swelled the government’s coffers. These rents, larger and more sustainable than the old patronage, flowed through familiar channels to familiar beneficiaries. (Löwi 2009). Thus, much as the pessimists and the conditionalists alike would have predicted, the regime was able to use oil wealth to stifle democratization. What is important, however, is the contrast between Algeria and the other regimes. Algeria’s government behaved exactly as the oil pessimists’ model predicts because it simply does not appear to have had a consistent program besides survival.

In Nigeria, the military government poured money into a goal of becoming a leading African cultural, economic, and military power. It used its new revenues to launch an ambitious five-year plan aimed at diversifying the country’s economy and massively expanded the public sector, including the construction of a new capital city to ameliorate the country’s ethnic tensions. Successive military regimes invested huge sums in a pan-African cultural exposition, FESTAC ’77, which was an act of cultural diplomacy designed to challenge Senegal’s role as “leader of the new black world order.” (Apter 2005) Nor were the regime’s ambitions limited to seizing symbolic leadership: it supplied trucks and heavy artillery as well as medicine, food, and clothing to African nationalist movements in Mozambique, Angola, Namibia, and Zimbabwe. (Apter 2005) Most telling, the military government finally carried out their pledge to give up power. Our analysis suggests that blaming oil for the Second Republic’s failure may be exactly wrong. Oil wealth may have been a necessary condition for the establishment of civilian rule. Given the country’s poverty, the subsequent failure of democracy was no surprise.

In Ecuador, civil-military conflict dominated politics until the late twentieth century, pitting a reformist officer corps drawn from the urban middle class against the conservative landowning class. (Fitch 1977) Periods of military rule alternated with civilian rule, although politics was an elite affair even during civilian rule. In the late 1960s, the most recent cycle of a civilian government ousted by a military coup followed by civilian restoration had just taken place. In the early 1960s, a junta had launched ambitious land-reform schemes; both the
reforms and the junta faltered because of a drop in earnings from banana exports, heretofore one of Ecuador’s leading sectors. It was replaced by the septuagenarian conservative Velasco Ibarra, who had served as president four times earlier (and had been ousted by the army each time). Velasco Ibarra was distinctly more authoritarian than the junta he had replaced, dismissing Congress and the Supreme Court and assuming dictatorial powers in 1970. The military deposed him in 1972 in what seemed to be yet another repetition of the cycle.

So how did junta-ruled Ecuador become (by Polity standards) as democratic as France? If the oil pessimists are correct, then Ecuador must be a least-likely case for the triumph of democratization. Evidence about the motivations and policy choices of the regime resolves this puzzle. Ecuador’s unprecedentedly high levels of economic growth brought on by the oil shock allowed the junta to fully realize plans for import-substitution and other economic development programs that had been drafted in the 1950s by a yet earlier junta but abandoned because of a lack of financial resources. Simultaneously, the junta built up state capacity quickly, beginning a process that quintupled the size of the public sector payroll between 1974 and 1990. (Gerlach 2003) In its final act, the junta voluntarily disbanded itself and returned the government to civilian and democratic rule—thereby becoming the first South American military government to voluntarily return to its barracks. (Martz 1988) In other words, Ecuador democratized in part because oil wealth allowed a military but reformist government to do so, a finding that underlines the importance of regime preferences.

Finally, even though there was little chance that oil wealth could alter Norway’s regime type, the windfall nevertheless affected its policy choices. (Hodne 1983) Oslo even further expanded its generous welfare state, which in the early 1970s had seemed to be unsustainable. (Galenson 1986) Moreover, this policy reflected the Labour government’s longstanding ideological commitments. The party feared increasing wage inequality and environmental degradation, and acted accordingly. The phrase most often adopted to describe Norway’s oil policy was “go slow,” reflecting the often-stated aspirations of Norwegian cabinet ministers to build a sustainable foundation in contrast to the difficulties other Scandinavian welfare
states had begun encountering. Similarly, its principal initiative in foreign policy was to increase its development assistance even further to 1.3 percent of GNP, triple the OECD average. (Lind and Mackay 1979) The right-wing opposition noticed at the time, and accused the government of “using the oil revenues for its socialist agenda.” (Bayulgen 2010)

In each case, we find that leaders’ preferences matter along dimensions that existing theories do not consider important, suggesting that scholars have been looking in the wrong place for the effect of resource endowments. The pessimists’ predictions about outcomes appear to have been overstated. The conditionalist case appears stronger, but even here we observe that our theory comports better with the available evidence about how leaders chose to dispose of their newfound resource wealth. Institutional variation does not explain why military governments in Nigeria, Ecuador, and Algeria chose such different paths. Instead, resources amplified the effect of such leaders’ pre-existing preferences.

**Discussion and Conclusion**

We find no evidence for the standard view of the resource curse. Instead, the sudden endowment of regimes with resource wealth will simply amplify and focus the preferences and strategies of incumbent regimes. We contend that this pattern may explain the variance in resource-endowed regimes’ behavior. We recognize that our claims to inference are limited by examining only the local treatment effect of resource wealth. Furthermore, the limited number of countries in our sample may mean that the patterns we observe are the product of factors exogenous to our model. Nevertheless, we believe that the strength of our research design should mitigate such qualms.

Perhaps the largest implication of our work is for policymakers. If the effects of resource wealth are principally mediated by regime preferences, then the prospects that sincerely liberalizing governments in newly resource-rich countries (or new governments in resource-rich countries, such as Libya) can actually build democratic institutions that can last are
much brighter than political scientists have believed until now. On the other hand, should more radical governments come to power, they too will have much greater latitude to pursue any policies they would prefer.
References


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