The Big Oil Change: A Closer Look at the Haber–Menaldo Analysis

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Article

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Abstract

The claim that oil wealth tends to block democratic transitions has recently been challenged by Haber and Menaldo, who use historical data going back to 1800 and conclude there is no "resource curse." We revisit their data and models, and show they might be correct for the period before the 1970s, but since about 1980, there has been a pronounced resource curse. We argue that oil wealth only became a hindrance to democratic transitions after the transformative events of the 1970s, which enabled developing country governments to capture the oil rents that were previously siphoned off by foreign-owned firms. We also explain why the Haber–Menaldo study failed to identify this: partly because the authors draw invalid inferences from their data and partly because they assume that the relationship between oil wealth and democracy has not changed for the past 200 years.

Keywords

resource curse, natural resources, autocratic survival, democratization, nationalization

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Many studies have found that authoritarian countries with more oil wealth are less likely to transition to democracy. Haber and Menaldo (2011) challenge these studies, arguing they are tainted by reverse causality and omitted variable bias. Using new data on natural resource wealth for the years 1800-2006, and statistical models that control for country fixed effects and many other factors, they found no evidence to support the "resource curse" claim. They conclude that

> no matter how we look at the long-run data—including just making simple country-by-country graphs—we cannot find a systematic tendency that matches the concept of a resource curse. (p. 3)[1]

The Haber–Menaldo article has had a powerful impact on the resource curse debate, calling into question widely held beliefs about the politically malignant effects of petroleum wealth. In the first 18 months after its February 2011 publication, it was cited more than 100 times by other scholars, and featured in influential policy journals (Kenny, 2010), World Bank publications (Sinnot, Nash, & De La Torre, 2010; Barma, Kaiser, Le, & Viñuela, 2011), and prominent blogs as evidence that claims about the "resource curse" are false.[2] An earlier study by Gurses (2009) used a similar fixed-effects model and came to a similar conclusion.

Why do these findings differ so much from other recent studies, which conclude that there *is* a resource curse (Andersen & Aslaksen, 2013; Aslaksen, 2010; Cuaresma, Oberhofer, & Raschky, 2010; Fayad, Bates, & Hoeffler, 2011; Ramsay, 2011; Ross, 2012; Tsui, 2010)? We employ the Haber–Menaldo data and models and show that each side of the debate is partly correct: From 1800 to the 1970s—the period that dominates the Haber–Menaldo data set—there is no strong evidence of a resource curse. Yet, since the late 1970s—the period that is the focus of most other studies—oil wealth has strongly inhibited democratization.

The emergence of a resource curse—or more properly, a petroleum curse—in the late 1970s is consistent with a closer look at the history of the global oil industry. Although the Haber–Menaldo analysis began in 1800, no country produced economically significant quantities of oil before 1918.[3] Until the late 1960s, most of the rents generated by oil production in non-Western countries were captured by a handful of large, vertically integrated international oil companies—sometimes called "the Seven Sisters."[4] But in the 1970s, the industry was transformed by a wave of nationalizations and contract revisions that enabled the governments of host countries to seize control of these rents. We refer to this transformation as "the big oil change."

Theories of the "rentier state" were formulated, beginning in the mid-1980s, in response to the big oil change; the idea that the oil producers were afflicted by a "resource curse" began to circulate in the early to mid 1990s.[5] In both literatures, the central concern is what happens to a country's politics and economy when the state accumulates large resource rents—a condition that only became widely true in the 1970s. The Haber–Menaldo study, however, combines data from 175 years when governments did *not* typically capture most of these oil rents (from 1800 to about 1975), with data from about 30 years when they *did* capture them. The powerful antidemocratic effects of oil since the late 1970s are hence obscured by the weaker relationship between oil and democracy in the 1800-1975 period.

We use Haber and Menaldo's data and error-correction model (ECM) to illustrate this change. When we allow for a break in the effect of oil around 1980, we find that oil has strongly inhibited democratization in the postbreak period. This result holds in the presence of country and year fixed effects, and under a wide range of conditions: with each of the variables that Haber and Menaldo use to measure resource wealth, when we use a dynamic fixed-effects model in place of Haber and Menaldo's ECM, and when we use any year between 1982 and 1990 to identify the temporal break. We also show that when oil income is allowed to affect regime types over 3, 5, or 7 years, rather than a single year, these antidemocratic effects become much larger and emerge earlier.

Haber and Menaldo claim that their finding of "no resource curse" is also supported by other evidence—particularly their longitudinal analysis of 53 resource-reliant states. They report that most of these countries eventually became more democratic, even though they had been "treated" with resource wealth. This leads them to conclude that 45 of these 53 states were either "resource blessed" or unaffected by resource wealth, while just 8 of them might have been "resource cursed."

We show that Haber and Menaldo's inferences from these data are invalid: It is not possible to determine whether a given treatment (oil wealth) has had an effect by only observing countries that have been treated. To make valid inferences about a treatment, researchers must compare outcomes in the treated population to outcomes in a control population. Haber and Menaldo observe that countries treated with oil wealth have grown slightly more democratic over time, and interpret this as evidence against the resource curse. We show, however, that the oil states examined by Haber and Menaldo (the treated group) made much less progress toward democracy than the nonoil states (the control group). In other words, once we use the correct counterfactual, we observe that the "oil treatment" strongly inhibited democratization.[6]

Despite our criticisms, there is also much we admire about the Haber–Menaldo study. They have offered a smart and spirited challenge to a well-established (but not always rigorous) literature; they have gathered a large quantity of new historical data, and made their data public and transparent; and they have helpfully clarified some of the conditions under which resource wealth is *not* associated with less democracy. Yet, their study obscures the powerful effect that oil revenues have had on authoritarian rule over the last three decades—the period of greatest concern for most researchers and policy makers.

Our findings have important implications for the study of both the resource curse, and democratic transitions more broadly. Virtually all of the resource curse literature assumes that the malignant (or benign) effects of petroleum wealth have changed little over time; some studies draw explicit parallels between the influence of oil today and other resources in the past (e.g., Karl, 1997). More generally, studies of democracy have increasingly used historical data to identify the factors associated with democratic transitions (Acemoglu, Johnson, Robinson, & Yared, 2008; Boix, 2011; Freeman & Quinn, 2012).

A key assumption in most of this research is that the relationships between democracy and its correlates are stable over time; if this were true, data sets that cover longer periods of time should give scholars more leverage to identify these correlates. However, if causal relationships tend to change over time, the identification of short- or medium term causal effects in one era might tell us little about their salience in a different era. Our study shows that the causal effect of at least one important factor—petroleum wealth—on democratic transitions changed sharply from the 1960s to the 1980s, as the global distribution of petroleum rents shifted from firms to governments. We hope our analysis encourages scholars who use long data sets to study the resource curse, democratization, and other social and political phenomena to become more sensitive to changing historical patterns, including discontinuities in economic and political relationships.

The next section of this article uses simple graphs and cross-tabs to illustrate our argument that oil wealth only began to inhibit democratic transitions after the 1970s. Section Two suggests this was probably caused by the wave of nationalizations that swept the oil-producing world in the 1970s. In section Three we begin our analysis of the Haber–Menaldo results, focusing on their longitudinal analysis of resource-rich countries; we explain why their inferences from these data are logically flawed. We replicate Haber and Menaldo's core statistical results in section Four and show how they are altered once we

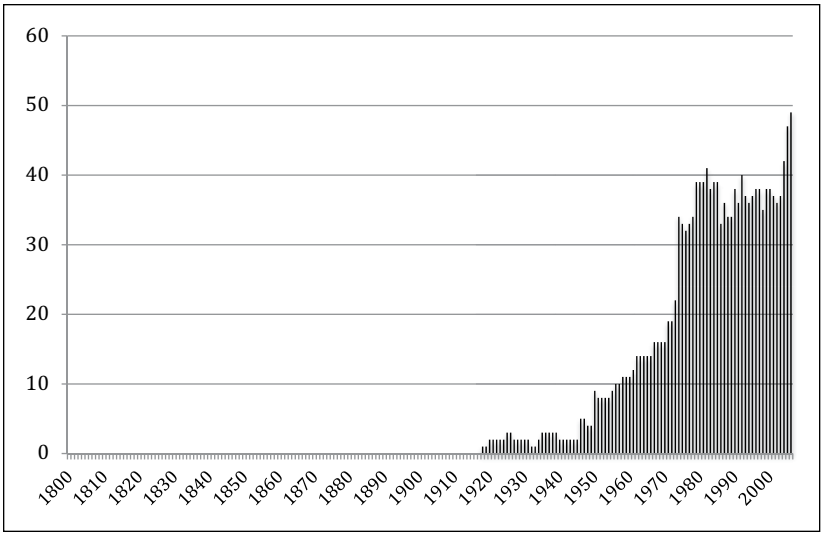

Figure 1. Number of significant oil producers, 1800-2006.
Source: Data from Haber and Menaldo (2011).

account for a temporal break in the effect of oil in the late 1970s or early 1980s. Section Five shows that these results are robust. Our article concludes by reflecting on the implications of the "big oil change" of the 1970s, and the methodological issues raised by the Haber–Menaldo study.

Looking at Oil and Democracy Over Time

Most studies of the resource curse rely on data sets that begin in 1960 or 1970; Haber and Menaldo construct a data set that goes back much earlier—for some of their variables, to 1800. They argue that this unusually long time-series allows them to identify the long-run equilibrium relationship between natural resources and regime type.

Yet, most of the years in the Haber and Menaldo data set are uninformative: Between 1800 and 1860, no country produced a single barrel of oil, and until the 1940s, only a couple of countries—chiefly the United States, Venezuela, and Mexico—produced economically significant quantities. Figure 1 shows that the number of countries producing significant quantities of oil—which we define as US$100 per capita (in constant 2007 dollars)—over the 207 year period covered by their data. In 2006, there were 49 such

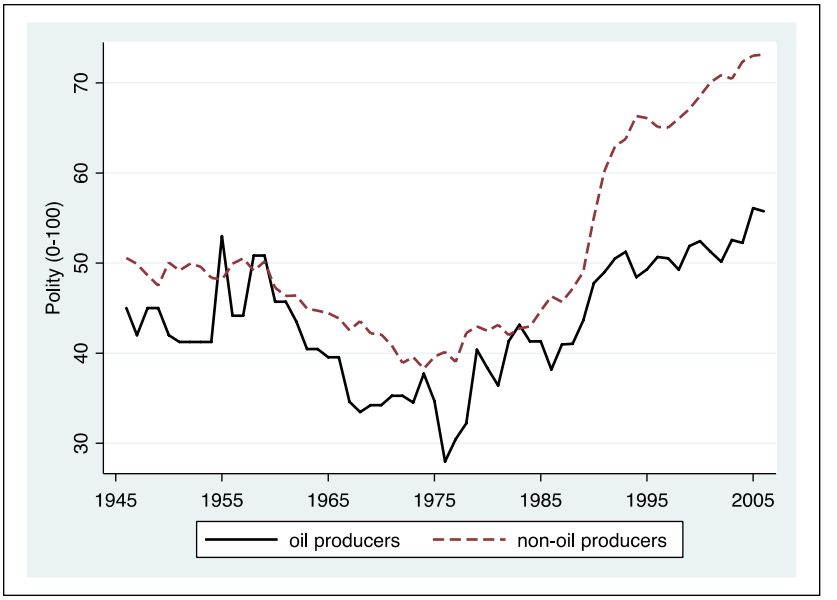

Figure 2. Polity scores of current oil and nonoil states, 1945-2006.
The solid black line shows the mean polity score (on a 0-100 scale) of all countries with at least US$100 in oil income per capita; the broken line shows the polity scores of all other countries that are included in the Haber–Menaldo data.

countries. Yet, no country crossed this modest threshold before 1918; as late as 1949, there were just four significant oil producers.

Moreover, using a very long time-series—even when time-series and cross-national observations are pooled, as in about half of the Haber–Menaldo models—has an important drawback: It can open the door to misleading inferences, if the relationship between the independent and dependent variables has changed over time.

There is good reason to suspect that the relationship between oil and regime types has indeed changed over time. In Figure 2, we plot the mean polity scores of the oil-abundant countries and compare them with all other states.[7] While the polity scores of the oil countries were slightly below the score of the nonoil countries for most of this period, they seem to diverge sometime in the early 1980s. Both sets of countries became more democratic after 1976, but the nonoil countries moved further and faster toward democracy.

Of course, this figure may be suspect because the composition of both sets of countries changes over time: More countries are becoming sovereign,

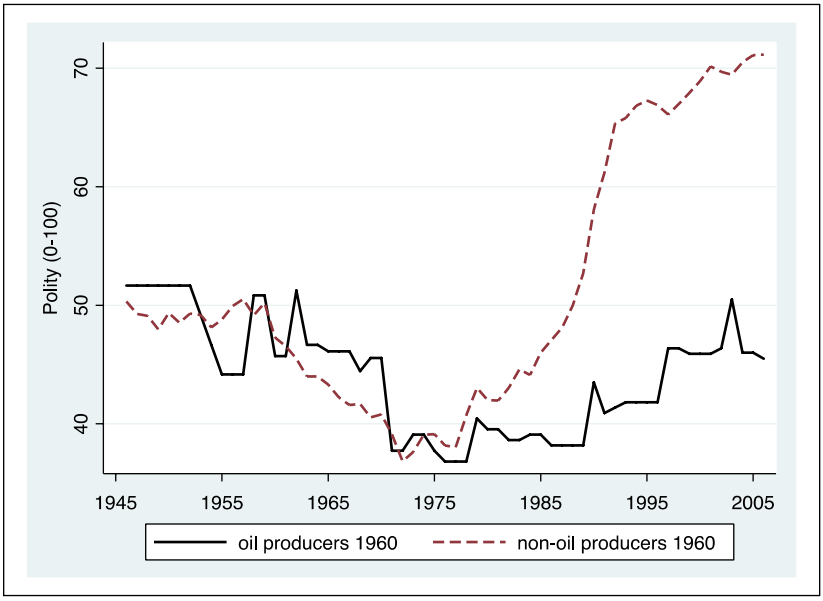

Figure 3. Polity scores of 1960 oil and nonoil states, 1945-2006.
The solid black line shows the mean polity scores of the 11 countries that in 1960 produced at least US$100 in oil income per capita; the broken line shows the polity scores of the 107 other countries that were sovereign in 1960 and are included in the Haber–Menaldo data.

increasing the membership of both groups; and countries shift between "oil abundant" and "not oil abundant" when they discover or run out of oil, and when global oil prices rise or fall. Figure 3 shows a simple way to circumvent this problem: It compares the polity scores of two constant groups of countries—the 11 states that produced at least 100 constant dollars per capita in oil income in 1960, and continued to be significant producers throughout this period, and the remaining 107 states that were also sovereign in 1960.[8] Now the divergence between the two lines is sharper and appears to start in the late 1970s. In the online appendix, we demonstrate that the results do not change if we move our base year from 1960 to 1970 (Figure A1), or if we raise the threshold for identifying "oil producers" from US$100 to US$500 dollars (Figure A2).[9]

We can also see the temporal break with simple cross-tabulations, by comparing the rate of democratic transitions (still using the Haber–Menaldo data) among countries that produce at least 100 dollars in oil income, compared with everyone else (Table 1, top half). Over the whole 203 year period

Table 1. Annual Likelihood of a Democratic Transition (%).

	Without oil	With oil	Difference
All oil producers (oil income > US$100 per capita)			
1800-2002	1.23	1.58	0.35
1940-1980	1.67	1.74	0.07
1981-2002	3.70	1.56	−2.14[a]
Major oil producers (oil income > US$500 per capita)			
1800-2002	1.26	1.22	−0.04
1940-1980	1.66	1.90	0.24
1981-2002	3.51	0.74	−2.78[a]

[a]Significant at 5% in a two-tailed *t* test.

covered by the Haber–Menaldo data (row 1), there is no statistically significant difference between the oil producers and the nonoil producers. The similarity of these two numbers is the basis of much of the Haber–Menaldo analysis.

Because production was minimal in all but a few countries before 1940, we think it is more enlightening to focus on the period between 1940 and 2002 (which is the last year in the Haber–Menaldo data containing their dichotomous measure of regime types). From 1940 to 1980 (row 2), oil producers and nonoil producers had democratic transitions at almost exactly the same rate; after 1980 (row 3), the nonoil producers became democratic at more than twice the rate of the oil producers. If we move the threshold for identifying oil producers up to US$500 (Table 1, bottom half), the patterns are even stronger: Since 1980, the nonoil producers have been 5 times more likely to transit than the oil producers.

In short, a visual inspection of the historical data and simple cross-tabulations suggest that the democracy paths of oil producers and nonoil producers diverged sometime in the late 1970s or early 1980s.

Historical Change in International Oil Markets

Why did this occur? Most theories of the resource curse argue that oil helps prolong authoritarian rule because it generates large rents for the government, which the ruler can use to lower taxes and increase patronage and pork barrel spending (e.g., Luciani, 1987; Mahdavy, 1970; Ross, 2001). However, governments have not always been able to capture these rents. Before the 1970s, the global petroleum industry was dominated by a handful of vertically integrated companies that colluded to maintain control of world

supplies (Yergin, 1991). In all but a few countries, these firms owned or controlled the local subsidiaries that extracted and exported the host country's oil. Globally, they controlled the shipping and marketing of almost all of the world's petroleum and used both highly favorable contracts and transfer pricing to capture most of the rents for themselves (Hartshorn, 1962; Levy, 1982).

In the 1960s and 1970s, international petroleum markets were transformed by a series of closely related developments: Oil supplies began to grow tighter, as rising demand outpaced new discoveries; the major oil exporters of the developing world began to collude through the Organization of Petroleum Exporting Countries (OPEC); the Bretton Woods system of fixed exchange rates—which had helped keep prices stable—fell apart; and global energy markets were shaken by the 1973-1974 and 1978-1979 price shocks (Tetreault, 1985).

These events signaled—and helped precipitate—a profound shift in the relationships between international oil firms, and the governments of oil-rich countries. As economic historian Edith Penrose wrote in 1976,

> Exploration and production concessions granted in the early days have been repeatedly re-negotiated, invariably in favour of the countries; where the concessions covered a very large proportion of a country's drilling area, they have been reduced in size; stiffer regulations respecting drilling requirements, reservoir maintenance and similar matters have been introduced; and financial arrangements of all kinds have improved in favour of the countries. (p. 198)

Perhaps the most important change during this period was nationalization: Almost all oil-exporting countries in the developing world seized the assets of foreign oil companies. Some states, like Ecuador, Qatar, and the United Arab Emirates, established new state-owned enterprises to manage these assets; others, like Iraq, placed them under preexisting but moribund state-owned companies. From the 1960s to the late 1970s, the number of nationalizations rose, culminating in the 1973-1976 period (Figure 4). Even countries that were reluctant to use expropriation were able to strike better deals with foreign oil companies, who were anxious to placate resource nationalists. According to Kobrin (1980), "the net result was a revolutionary transformation of the international petroleum industry" (p. 17; also see Jodice, 1980; Minor, 1994; Tetreault, 1985; Victor, Hults, & Thurber, 2011; Yergin, 1991).

These events led to sharp changes in the finances of most oil-producing states: The size of government revenues grew dramatically; they became more fiscally reliant on their petroleum sectors; and instead of collecting taxes and royalties from foreign companies, governments could fund

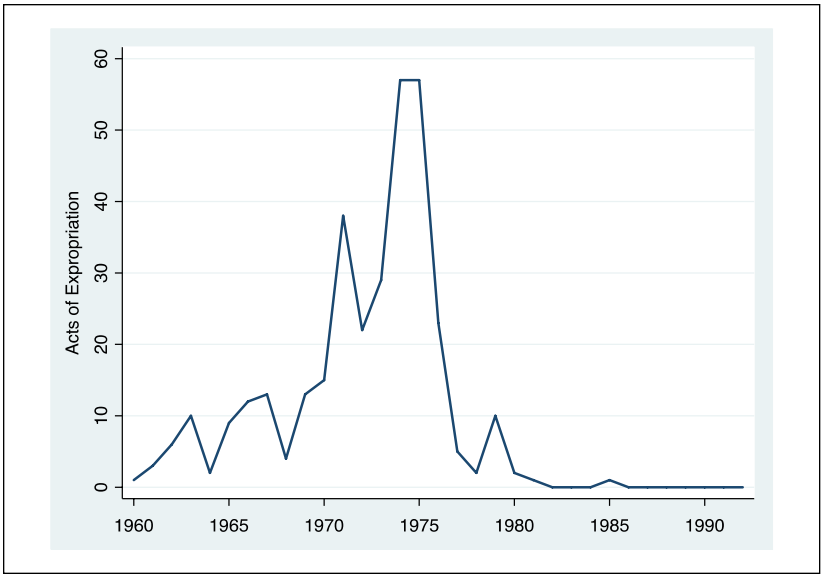

Figure 4. Acts of expropriation in the petroleum sector, 1960-1993.
Source: Kobrin (1980); Minor (1994).

themselves by selling oil through their national oil companies. Many rulers also used their national oil companies to gain greater control over the distribution of patronage, and to cloak these transactions in secrecy (Ross, 2012).[10] Collectively, these developments gave the governments of oil-producing countries far more influence over their economies and citizens. We think—though cannot prove, in the absence of more reliable nationalization data—that when the fiscal powers of autocratically governed oil producers passed a critical threshold in the mid to late 1970s, they became capable of withstanding the democratic wave that swept the rest of the authoritarian world in the 1980s and 1990s.

Most of Haber and Menaldo's statistical tests refute the hypothesis—which they attribute to the resource curse literature—that there is a long-run equilibrium relationship between a country's resource wealth in one year, and its democracy level the following year. Because they found no such long-run equilibrium between 1800 and 2006, they concluded there is no resource curse:

> A reader who accepts the results of the cointegration tests has to conclude that there is no resource curse, because they indicate that there is not a long-term equilibrium relationship between Fiscal Reliance and Polity. (p. 15)

Yet, if history matters, these variables *should not* be cointegrated. A long-run equilibrium would imply that the world of oil and politics has not changed over the past 2 centuries—and that the historic shift in the control of resource rents in the 1970s was politically inconsequential.

Haber and Menaldo's Longitudinal Analysis

We begin our examination of the Haber–Menaldo data by looking at their longitudinal analysis of 53 resource-reliant countries, which takes up about half of their study.[11] They explain their strategy by correctly pointing out that the resource curse is a theory about changes that occur within countries over time. As cross-national comparisons can be tainted by omitted variable bias, they argue, "it is best to employ evidence and methods designed to see whether that time-series process actually occurred" (p. 2).

In pursuit of this goal, they identify 53 "resource-reliant" countries and look for evidence that fluctuations in each country's resource income (or alternatively, its "fiscal reliance" on resource revenues) were followed by corresponding fluctuations in their Polity scores. They first conduct a "graphical analysis" of each country (meaning that they plot and visually inspect graphs) and report that resource wealth seems to be negatively correlated with Polity scores—as predicted by the resource curse—in just 8 of the 53 countries. They observe *positive* correlations—which they call evidence of a "resource blessing"—in 19 other countries, and report no conclusive pattern in the remaining 26 countries (pp. 5-11).

Haber and Menaldo follow this with a statistical analysis of the 18 countries for which they have collected data on "fiscal reliance," using ECM-based cointegration tests—first running tests on each country individually (pp. 11-14), then all 18 countries as a panel (pp. 14-18). They conclude that "no matter how one looks at the relationship between Fiscal Reliance and Polity, there is no evidence for a resource curse" (p. 15).

According to Haber and Menaldo, this within-country analysis helps solve the problem of omitted variable bias:

> 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. (p. 23)

Yet, their reasoning is flawed: Their analysis can only tell us the *conditional probability* that a country with more (or less) resource wealth will democratize; the important question, however, is whether resource-rich

countries are more likely, or less likely, to democratize than similar countries without resource wealth.

The fallacy can be described in experimental terms: It is not possible to make valid inferences about the effects of any treatment without comparing the treated group with a control group. Haber and Menaldo compare countries "treated" with natural resource wealth to themselves over time, instead of comparing them with countries without resource wealth. Because they find that when countries are "treated" with resource wealth, they do not become less democratic, they mistakenly infer that the treatment had no effect. However, if they compared the resource-producing countries with the control group—the nonresource countries, which happened to grow substantially *more* democratic after the 1970s—they would have observed that the "resource treatment," and in particular oil, has had a profound antidemocratic effect.[12]

We can illustrate our point with a simple graph. In Figure 5, the solid line represents the mean Polity score of Haber and Menaldo's 34 oil-reliant states (using their 0-100 scale) between 1945 and 2006.[13] If we only observe these oil-reliant states—which were being collectively treated by large revenue windfalls in the 1970s—Haber and Menaldo appear to be correct: These countries became collectively more democratic after the mid-1970s, which at first glance looks like evidence against the notion of a resource curse. This appears to be consistent with their classification of these states as more often "blessed" than "cursed" by oil wealth.[14]

Yet, we cannot make inferences about the effects of a treatment until we observe the control group—the nonoil countries, whose mean Polity score is shown in Figure 5 by the broken line. Notice that there are two differences between the two lines: The *level* of democracy is much lower in the oil states; and since around 1980, the gap between the two groups has widened.

Haber and Menaldo's within-country analyses might be more informative if they wanted to test the claim that *changes* in resource reliance led to *changes* in Polity scores. However, as Haber and Menaldo themselves note, the resource curse is not a theory about changes, but about levels:

> The resource curse is a theory about variables expressed in levels: Higher levels of natural resource reliance are purported to induce lower levels of democracy; lower levels of natural resource reliance within countries over time are purported to induce higher levels of democracy. (p. 11)

Conceivably, democracy levels in Haber and Menaldo's oil-reliant states might be lower due to omitted variable bias—which is why we prefer to look at oil abundance instead of oil reliance, and generally favor the use of country-level fixed effects in regressions. It is harder to identify, however, an

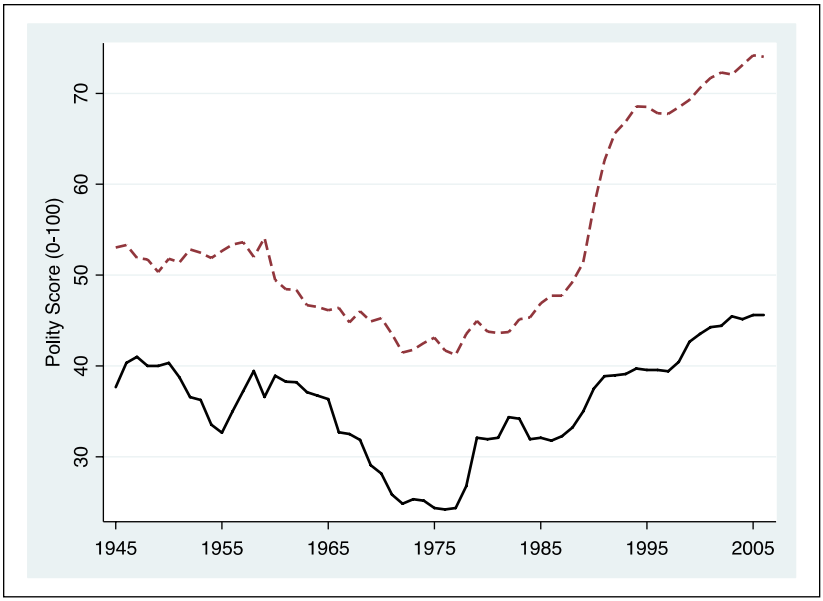

Figure 5. Polity scores of oil-reliant and non-oil-reliant countries, 1945-2006. The solid black line shows the mean polity score (on a 0-100 scale) of the 34 oil-producing countries that Haber and Menaldo identify as "resource-reliant"; the broken line shows the mean polity score of all other countries.

omitted variable that might explain why the oil and nonoil states diverged after the 1970s.

Accounting for Change in the Haber–Menaldo Model

Haber and Menaldo's findings are significantly altered once we account for the historic changes of the 1970s. We use Haber and Menaldo's data and ECMs to illustrate.

In Table 2, column 1, we replicate Haber and Menaldo's core ECM (found in their Table 5, column 1), in which their independent variable is Total Oil Income (measured in thousands of constant U.S. dollars per capita), their dependent variable is a country's normalized Polity score (ranging from 0 to 100), and their data cover 163 countries from 1800 to 2006. We use their data and their controls, including time and country fixed effects, a lagged measure of Polity to capture endogenous dynamics (or error correction), and measures

Table 2. Replication of Haber–Menaldo ECM.

	(1)	(2)	(3)	(4)
Dependent variable	Δ Polity	Δ Polity	Δ Polity	Δ Polity
Model	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE
Sample	FULL	FULL	FULL	FULL
Polity in levels $t-1$	−0.087*** (0.008)	−0.087*** (0.007)	−0.109*** (0.021)	−0.110*** (0.021)
TOI $t-1$	**0.055*** (0.019)**	**0.048** (0.024)**		
TOI $t-1$ × Post-1980		**−0.147*** (0.052)**		
ΔTOI	−0.020 (0.021)	−0.059** (0.027)		
ΔTOI × Post-1980		−0.048 (0.036)		
Log (per capita income) $t-1$	−0.279 (0.319)	−0.273 (0.317)	0.845 (0.734)	0.324 (0.718)
Civil War $t-1$	0.065 (0.448)	0.057 (0.448)	1.435 (1.181)	1.607 (1.181)
Regional democratic diffusion $t-1$	0.025*** (0.007)	0.025*** (0.007)	0.012 (0.021)	−0.002 (0.021)
Global democratic diffusion $t-1$	0.058** (0.029)	0.059** (0.029)	−0.050 (0.035)	−0.076* (0.037)
Δ Log (per capita income)	1.289 (1.734)	1.199 (1.736)	−3.698 (3.349)	−3.391 (3.371)
Δ Regional democratic diffusion	0.375***(0.070)	0.375*** (0.070)	0.168** (0.076)	0.167** (0.076)
Δ Global democratic diffusion	−0.277** (0.109)	−0.277** (0.109)	0.113 (0.112)	0.120 (0.111)
FR $t-1$			**0.023 (0.016)**	**0.047** (0.018)**
FR $t-1$ × Post-1980				**−0.057*** (0.019)**
Δ FR			0.049** (0.020)	0.042** (0.018)
Δ FR × Post-1980				0.004 (0.023)
Observations	10,195	10,195	1,132	1,132
R^2	.0976	.0977	.168	.173
Number of groups	163	163	18	18
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

ECM = error-correction model; DKSE = Discroll–Kray standard errors; TOI = Total Oil Income; FR = Fiscal Reliance; FE = fixed effect. The polity score is normalized to run from 0 to 100. DKSE in parentheses. Constant term not reported.
*$p < .1$. **$p < .05$. ***$p < .01$.

of income per capita, population, civil war, and the regional and global diffusion of democracy. Like them, we use Discroll–Kray standard errors to account for spatial heterogeneity.[15] Our results match theirs precisely: As they report, the coefficient on the Total Oil Income variable has a positive sign and is statistically significant. They interpret this as implying that there is no resource curse, and maybe even a resource blessing.

In column 2, we add terms interacting their Total Oil Income variables with a dummy for the post-1980 period, which takes the value 0 for the years 1800 to 1980 and 1 for the years 1981-2006.[16] The term interacting Total Oil Income with the post-1980 period has a negative coefficient and is statistically significant at the 1% level in a two-tailed *t* test. In columns 3 and 4, we repeat this exercise using Haber and Menaldo's Fiscal Reliance variable in place of Total Oil Income. We consider these tests less informative because the sample is limited to the 18 countries for which Haber and Menaldo collected data, and their Fiscal Reliance variable fails to capture much of oil's "rentier effect."[17] We nonetheless observe a similar pattern. After replicating

Table 3. ECM Using Different Temporal Breaks in the Coefficient, and in the Overall Effect, of TOI.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Δ Polity	Δ Polity	Δ Polity	Δ Polity	Δ Polity	Δ Polity	Δ Polity	Δ Polity
Model	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE
Year of post-year dummy	1976	1978	1980	1982	1984	1986	1988	1990
TOI $t-1$	0.096***	0.076***	0.048*	0.022	0.011	0.018	0.025	0.034
	(0.035)	(0.027)	(0.024)	(0.023)	(0.024)	(0.023)	(0.022)	(0.022)
TOI $t-1$ × Post-year dummy	−0.061	−0.038	−0.147***	−0.236***	−0.304***	−0.291***	−0.270***	−0.217***
	(0.037)	(0.035)	(0.052)	(0.072)	(0.066)	(0.065)	(0.062)	(0.059)
Net post-year effect of TOI $t-1$	**0.036**	**0.038**	**−0.099**	**−0.214****	**−0.292*****	**−0.273*****	**−0.245*****	**−0.183****
	(0.029)	**(0.034)**	**(0.062)**	**(0.085)**	**(0.081)**	**(0.078)**	**(0.073)**	**(0.072)**

ECM = error-correction model; DKSE = Discroll–Kray standard errors; TOI = Total Oil Income. The polity score is normalized to run from 0 to 100. DKSE reported in parentheses. The regressions in the table reproduce the regression in Table 2, column 4, but with different entry years to identify the breaks in the coefficient and the effect of TOI $t-1$ (TOI $t-1$). All regressions thus include the full battery of controls, including country and year fixed effects. In "TOI $t-1$ × Post-Year dummy," "Year" is indicated in the header of the columns (Year of the Post-Year dummy). "Net Post-Year effect of TOI $t-1$" refers to the estimated effect of the 1 year lagged level of Total Oil Income on the consequent 1 year change in Polity during the period of the Post-Year dummy; a negative effect is an indication of a negative long-run relationship between the two variables. The coefficients of the "Net Post-Year effect of TOI $t-1$" and the associated standard errors are estimated by employing Pre Year + 1 instead of the Post-Year dummies (e.g., a Pre-1981 dummy instead of a Post-1980 dummy, and so on) interacted with the TOI $t-1$ and ΔTOI variables, and is then given by the coefficients and the standard errors of the uninteracted TOI $t-1$ variable. For convenience, only the main coefficients of interest (and their associated standard errors) are reported in the table.
*$p < .1$. **$p < .05$. ***$p < .01$.

the original Haber–Menaldo model in column 3, we add the interaction terms in column 4; the sign on the coefficient is negative and statistically significant at the 1% level.

These results have two important limitations: The selection of 1980 as the break point might seem arbitrary and there is an important difference between identifying a temporal break in the oil (or fiscal reliance) coefficient and demonstrating that oil's net impact on democracy becomes negative in the postbreak period. While the interaction terms in Table 2 show evidence of the temporal break, to calculate the net effect of oil or fiscal reliance after 1980, we must simultaneously account for the effect of the interaction term, the direct measure of oil or fiscal reliance, and the change in these variables, and then calculate the appropriate standard error.[18]

We address both concerns in Table 3. Here, we show the results of the same ECM as displayed in Table 2, column 3, but use every other year from 1976 to 1990 to specify the temporal break. We also display the net effect of Total Oil Income on Polity following the break. The models include all of the controls displayed in Table 2, column 3, including the country and year fixed effects.

Beginning in 1976, the interaction term has a negative coefficient; beginning in 1980, this coefficient becomes statistically significant; and beginning in 1982, the net effect of oil becomes statistically significant and negative, and remains so for all subsequent break points in the table.

The Haber–Menaldo specification uses a 1-year lag, which may bias downward the substantive effects of oil on democracy. Haber and Menaldo are in effect asking whether a country's level of oil revenues, or annual changes in its revenues, lead to changes in its regime type from one year to the next. Many earlier studies consider the effects of oil over a longer period. We think the focus on the 1-year lagged effects of resource wealth is mistaken for two reasons.

First, political institutions change slowly: Many recent studies find that they are shaped by historical factors from decades or even centuries before (see, for example, Acemoglu, Johnson, & Robinson, 2001; Treisman, 2011). We would not expect short-term fluctuations in revenues to immediately cause fluctuations in regime types. Haber and Menaldo partly address this issue, as in some specifications, they allow the *differenced* resource variables to enter with two or several lags. However, they never allow the *level* of the Polity score—the error-correction mechanism, which accounts for the long-run relationship between the levels of oil and democracy—to enter with longer lags.

Second, it overlooks the fiscal practices of the oil-rich countries. Most of them have stabilization funds, or sovereign wealth funds, which they use to smooth out annual fluctuations in their revenues (see, for example, Davis, Ossowski, Daniel, & Barnett, 2003).[19] This allows them to buffer their annual budgets—and hence their political institutions—from the revenue shocks that Haber and Menaldo focus on. Changes in revenues are more likely to affect government expenditures, and hence political pressures, over the medium term.

Table 4 is identical to Table 3, except we move from 1-year lags to 5-year lags on all of the right-hand side variables and consider the effect on the 5-year change in the Polity score. Notice that the net effects of oil income on democracy grew almost fivefold, and become statistically significant several years earlier. This suggests that the marginal impact of a shift in the level of oil income per capita on the level of democracy accumulates over several years.

Figure 6 displays these results graphically, using the 5-year lag specification. The vertical axis shows the net effects of Total Oil Income on Polity in a given year, while the horizontal axis indicates the year of the break in each regression. While the antidemocratic effects of oil become statistically significant around 1980, they reach their maximum impact in the mid-1980s.

Table 4. ECM Using Different Temporal Breaks in the Coefficient, and in the Overall Effect, of Total Oil Income.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Δ5 Polity	Δ5 Polity	Δ5 Polity	Δ5 Polity	Δ5 Polity	Δ5 Polity	Δ5 Polity	Δ5 Polity
Model	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE	ECM DKSE
Year of post-year dummy	1976	1978	1980	1982	1984	1986	1988	1990
Total oil income $t-5$	0.252** (0.101)	0.203** (0.081)	0.049 (0.068)	−0.056 (0.058)	−0.080 (0.058)	−0.032 (0.062)	0.019 (0.068)	0.058 (0.069)
TOI $t-5$ × Post-year dummy	−0.364** (0.150)	−0.359*** (0.115)	−0.535** (0.214)	−1.037*** (0.277)	−1.374*** (0.186)	−1.257*** (0.194)	−1.073*** (0.184)	−0.955*** (0.167)
Net post-year effect of TOI $t-5$	**−0.089 (0.132)**	**−0.157 (0.134)**	**−0.486* (0.252)**	**−1.093*** (0.309)**	**−1.453*** (0.222)**	**−1.284*** (0.237)**	**−1.042*** (0.223)**	**−0.893*** (0.211)**

Note: ECM = error-correction model; DKSE = Discroll–Kray standard errors; TOI = Total Oil Income. Same specification as in Table 3, but with differences and lags of 5 years instead of 1. See Table 3 for details. *$p < .1$. **$p < .05$. ***$p < .01$.

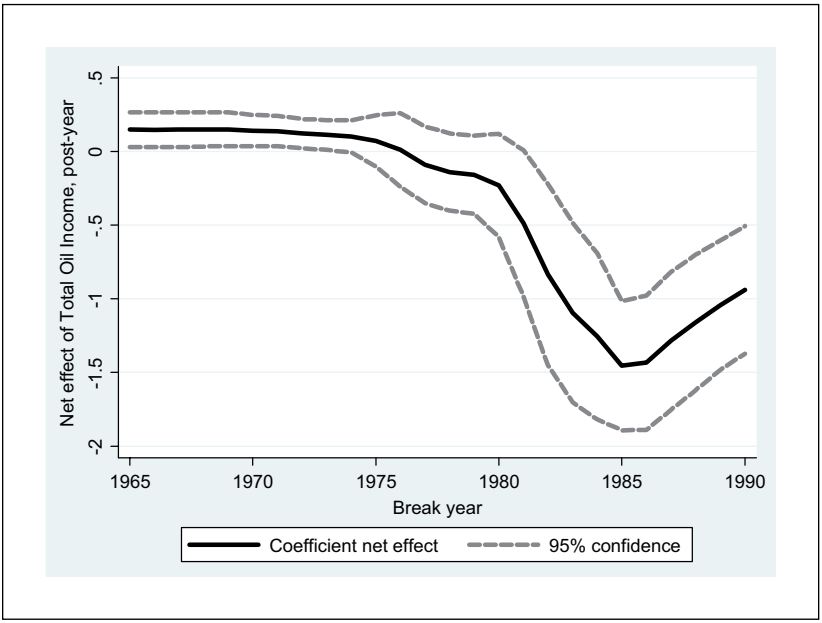

Figure 6. Net effect of total oil income on polity in error-correction model.
Based on the Haber and Menaldo error-correction model with a 5-year lag, the dashed lines represent the 95% confidence interval. See Table 3 for a full explanation of how the net effect is calculated.

Table 5. Dynamic Fixed-Effects Model—Total Oil Income.

	(1)	(2)	(3)	(4)
Dependent variable	Polity	Polity	Polity	Polity
Model	OLS FE	OLS FE	OLS FE	OLS FE
Sample, lag-length (s)	Full, s = 1	Full, s = 1	Full, s = 5	5-year avgs., s = 1
Polity in levels $t - s$	0.917***(0.008)	0.917*** (0.008)	0.595*** (0.033)	0.983*** (0.004)
TOI $t - s$	**0.050** (0.020)**	**0.047** (0.019)**	**0.121 (0.076)**	**0.042*** (0.015)**
TOI $t - s$ × Post-1980		**−0.121** (0.051)**	**−0.508** (0.244)**	**−0.098** (0.042)**
Log (per capita income) $t - s$	−0.347 (0.435)	−0.348 (0.433)	0.423 (1.974)	−0.399 (0.261)
Civil War $t - s$	0.009 (0.476)	0.002 (0.477)	−1.450 (1.748)	−0.897* (0.541)
Regional democratic diffusion $t - s$	0.007 (0.009)	0.007 (0.009)	0.041 (0.043)	−0.009 (0.006)
Global democratic diffusion $t - s$	0.123 (0.077)	0.124 (0.077)	0.441 (0.288)	−0.220 (0.218)
Observations	10,196	10,196	9,546	2,194
R^2	.891	.891	.570	.979
Number of countries	163	163	161	163
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

OLS = ordinary least squares; FE = fixed effects; TOI = Total Oil Income. The polity score is normalized to run from 0 to 100. Standard errors in parentheses, clustered at the country level. "5-year avgs." refers to the sample where the full sample is collapsed into a sample of consecutive 5-year averages. Constant term not reported.
*$p < .1$. **$p < .05$. ***$p < .01$.

As we move away from the 1976-1990 range in either direction, the break becomes more difficult to identify: Before 1976, it disappears entirely, and after 1990, it remains statistically significant but becomes weaker, as the number of postbreak observations becomes smaller.

Even though we use the Haber–Menaldo models for heuristic purposes, we think a more plausible way to estimate the relationship between resource wealth and Polity scores—while still using the Haber–Menaldo data and controls—would be to drop the restriction that the resource and democracy variables are cointegrated (a restriction that, according to Haber and Menaldo, is generally violated) and use a dynamic fixed-effects model.

We follow this strategy in Table 5, keeping all of the Haber–Menaldo controls, including both country and year fixed effects.[20] With a 1-year lag, the interaction term carries a negative sign and is now statistically significant (column 2); moving to a 5-year lag makes the term's absolute value more than four times larger (column 3). As we have dropped the error-correction framework, we can also collapse the data into 5-year panels (column 4). Even though this leaves us with just five panels in the post-1980 period, the coefficient remains negative and significant at the 5% level.

In Table 6, we repeat this exercise using the Fiscal Reliance measure and Haber and Menaldo's sample of 18 countries. The results are similar: The post-1980 interaction term has a negative sign and is statistically significant

Table 6. Dynamic Fixed-Effects Model—Fiscal Reliance.

	(1)	(2)	(3)	(4)
Dependent variable	Polity	Polity	Polity	Polity
Model	OLS FE	OLS FE	OLS FE	OLS FE
Sample, lag-length (s)	Full, s = 1	Full, s = 1	Full, s = 5	5-year avgs., s = 1
Polity in levels $t - s$	0.896*** (0.027)	0.895*** (0.028)	0.462*** (0.137)	0.937*** (0.022)
FR $t - s$	**0.014 (0.017)**	**0.040* (0.022)**	**0.182* (0.091)**	**0.033 (0.022)**
FR $t - s \times$ Post 1980		**−0.061** (0.027)**	**−0.191* (0.100)**	**−0.055** (0.025)**
Log (per capita income) $t - s$	1.145 (0.959)	0.558 (0.845)	2.128 (4.752)	0.309 (0.894)
Civil War $t - s$	1.413 (0.993)	1.603 (0.991)	2.841 (2.922)	−0.620 (1.553)
Regional democratic diffusion $t - s$	0.005 (0.034)	−0.010 (0.029)	0.037 (0.141)	−0.021 (0.024)
Global democratic diffusion $t - s$	−0.017 (0.049)	−0.032 (0.053)	−0.490 (0.339)	−1.621 (3.008)
Observations	1,138	1,138	1,069	244
R^2	.881	.881	.552	.972
Number of countries	18	18	18	18
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

OLS = ordinary least squares; FE = fixed effects; FR = fiscal reliance. The polity score is normalized to run from 0 to 100. Standard errors in parentheses, clustered at the country level. "5-year avgs." refers to the sample where the full sample is collapsed into a sample of consecutive 5-year averages. Constant term not reported.

* $p < .1$. ** $p < .05$. *** $p < .01$.

with the 1-year lag (column 2), 5-year lag (column 3), and with 5-year panels (column 4).

The substantive effects of oil income on democracy in this model are large.[21] Taking the dynamic fixed-effects model with a 5-year lag (Table 5, column 3) as our baseline, and using the 1980 temporal break, a one standard deviation rise in total oil income (US$2,618) leads to a one point reduction in the Polity score (using Haber and Menaldo's 0-100 scale) over a 5-year period, and a 0.6-point reduction over the long run, once Polity has stabilized.

When we move the temporal break to 1984, the effects are much larger—a drop of 4.7 points over 5 years, and a drop of 2.9 points in the long run. At the sample mean of the Polity score (=45.7), our estimates imply that a one standard deviation rise in total oil income is associated with a 10.3% reduction in Polity in the short run and a 6.3% reduction in the long run.

Our results are further confirmed by the R^2 statistics. We illustrate this in Figure 7, which plots the *within-country* R^2 figures for the dynamic fixed-effects model in the post-1945 sample, and with a 5-year lag.[22] Without oil, the R^2 figure is .46; adding Haber and Menaldo's *Total Oil Income* variable lifts it very slightly to .4605. When we interact the oil measure with different temporal breaks, the R^2 rises from .4609 (when the break is 1976) to .4634 (when the break is 1984), and then declines from 1986 and onward.

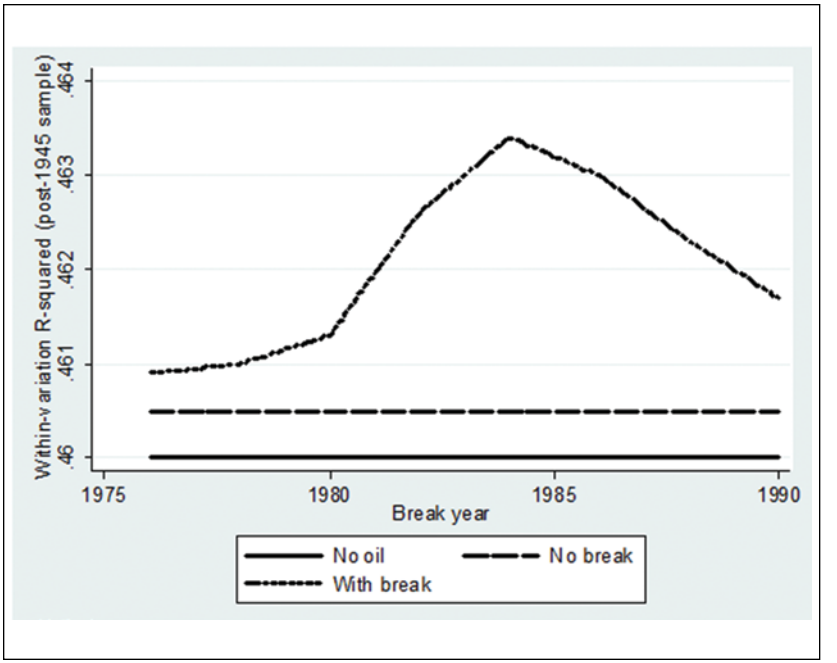

Figure 7. R^2 statistics for different models and break years, post-1945 sample.
Based on the dynamic fixed-effects model with a 5-year lag.

This suggests that at its maximum, oil explains 0.34% of the total within-country variation in democracy around the world, in the 1945-2006 period. Including the break boosts the explanatory power of oil approximately sevenfold.

It also implies that oil income has substantial within-country explanatory power. Assuming oil only begins to affect democracy after about 1984, its effects are limited to about 46% of the country-year observations in the 1945-2006 period.[23] About 25% of the countries post-1984 are consistent oil producers and hence susceptible to the resource curse. This means that oil's explanatory power is confined to about 12% of the country years in the sample, suggesting that within these post-1984 oil producers, oil alone explains about 3.0% of the total variation in their Polity scores. If the effects are further limited to the roughly two thirds of the oil-rich countries that are authoritarian—as suggested by several recent studies (see Endnote 6)—then oil income would explain about 4.5% of the total variation in Polity scores within the affected countries.[24]

In some regressions, the noninteracted measure of resource wealth (either Total Oil Income or Fiscal Reliance) still carries a positive sign, and is statistically significant, after the interaction term is included. Does this imply that, before nationalization, more oil led to more democracy?

We think this is unclear. In the large majority of countries before the 1970s, oil only played a minor role in the economy, as indicated in Figure 1. Hence, it is less plausible that the relationship between oil and democracy can be given a causal interpretation in this period; it might be spurious. The rich democracies invest much more in their extractive sectors than the rest of the world, which could mean that more democracy leads to higher levels of oil production, not the reverse (Ross, 2012). As a country's investments vary over time, they are not controlled for by country fixed effects. We consider this an unresolved puzzle for future studies.

Robustness

We have already shown that our results hold with each of Haber and Menaldo's measures of resource wealth; that they do not change when we use different years between 1980 and 1990 to specify the temporal break, or move from 1- to 5-year lags; and that they also hold in a dynamic fixed-effects model with all of the Haber–Menaldo controls, whether or not the data are collapsed into 5-year panels.

Our results are also unchanged if we vary the size of our lags: In the online appendix, we replicate our results with the ECM, using 3- and 7-year lags in place of the 5-year lags in Table 2. Nor do our findings change if we collapse our data into 3- or 7-year panels, instead of the 5-year panels we employ in the dynamic fixed-effects model in Table 4; in fact, moving from the 5- to the 7-year panel causes the coefficient on the interaction term to rise by about 20% in size and gain significance at the 1% level, while the coefficient on the noninteracted oil term drops in size by about two thirds, and loses significance at conventional levels. We display all of these results in Table A1 of the online appendix. If we further lengthen the panels to cover 10- or 20-year periods, it becomes more difficult to identify the temporal break, because we have too few data points after 1980 to make meaningful inferences.

Toward the end of their article, Haber and Menaldo use a new dependent variable, *Net Polity*, which represents the difference in Polity scores between oil-producing countries and other countries in the same region that are not reliant on oil or other resources. This allows them, they suggest, "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" (p. 23).

We note above that we consider these tests less informative, as the resource curse is a theory about the effects levels of, not changes in, oil wealth. Still, in the online appendix, we run an additional set of dynamic fixed-effects models, taking Net Polity as the dependent variable, including all of the Haber–Menaldo controls, and using either a 5-year lag or collapsing the data into 5-year panels. We run one set of tests with Total Oil Income as the key independent variable, and another set with the Fiscal Reliance measure. We find the same pattern as before: The post-1980 interaction term is always negative, and in three of these four tests, statistically significant (Table A2).

Some observers might worry that our results are biased by including the post-1980 interaction term without also including the post-1980 dummy by itself. This concern is unwarranted, as the models already include a full set of year dummies, and a post-1980 term will be perfectly collinear with them. Still, in the online appendix, we replicate Table 2 after adding a post-1980 dummy; the results are identical to those already reported in Table 2 (Table A3).

Finally, we show that in the dynamic fixed-effects model—like in the ECM—our results are similar when we move the temporal break to later years (A4). When the break is in 1976, the interaction term has a negative coefficient that is not statistically significant; in 1978, the coefficient gains statistical significance; and beginning in 1982, the net effect of oil income on democracy becomes negative and statistically significant, and remains so in each subsequent year.

All of our empirical models represent extensions and modifications of the Haber–Menaldo specification. An alternative way to investigate the timing of the break is to model the marginal effect of oil income as a function of time. In the dynamic fixed-effects model, this can be done by interacting the oil income variable with the time (year) variable, and, based on the resulting estimates, calculate in which year the marginal effect of oil income is predicted to change from positive to negative. This type of analysis is potentially sensitive to the specific modeling assumptions made—with respect to the structure of time dependence, and to the existence and the timing of trend breaks. Still, when we estimate different versions of this type of model—using the same range of lag structures and trend breaks that we use with the Haber–Menaldo models—we observe that the marginal effect of oil declines after 1960, and changes from positive to negative somewhere between 1975 and 1985—precisely in line with our other estimations.[25]

Conclusion

Our analysis finds that the antidemocratic effects of oil are only evident after the 1970s. We argue this reflects the global shift in the control of oil rents in

the 1970s, as governments renegotiated contracts with foreign oil companies, and often expropriated their assets. There is little to admire in the behavior of the major oil companies in the developing world in the 1940s, 1950s, and 1960s; yet, the resource nationalism that swept the resource-rich countries in the 1970s had its own perverse consequences, empowering autocrats and insulating them against the democratic waves of the 1980s and 1990s.

Our study complements recent efforts to better specify the conditions under which natural resource wealth leads to less democracy (see, for example, Clark, Poast, & Weins, 2012; Dunning, 2008; Morrison, 2009; Smith, 2004; Ulfelder, 2007). Several earlier studies argue that the resource curse is conditional on the initial regime type of the affected country; we suggest it is also contingent on the state's ability to capture resource rents, which was only widely true after the late 1970s. Our findings have broad implications for the study of the resource curse, which is often treated as a time-invariant truth—one that should apply equally to states in the 1950s and the 2000s.[26] They are also consistent with a recent study by Jones Luong and Weinthal (2010), which argues that oil wealth only leads to harmful outcomes when the government has a dominant role in the oil industry. We hope it helps point the way toward policy interventions that can counteract the resource curse: If it has been produced by government policies, it might be easier to fix.

Our article partly refutes, and partly builds on, Haber and Menaldo's important study. We try to explain why Haber and Menaldo failed to observe the pattern we identify: because they made invalid inferences from their longitudinal analysis of resource-rich countries and because they paid insufficient attention to changes in the underlying conditions that give oil its antidemocratic properties.

Our analysis also highlights some of the pitfalls of Haber and Menaldo's approach to comparative analysis. After stressing the dangers of omitted variable bias, they suggest as follows:

> When a hypothesis is not about static differences between countries but about complex changes that take place within countries over time, long-run historical datasets provide a better fit between theory and evidence. (p. 25)

We agree with their criticism—the dangers of omitted variable bias and reverse causality are ubiquitous—but we think these dangers are widely recognized, and that Haber and Menaldo's proposed solution carries its own dangers.

Long-run historical data sets have intrinsic value, and we applaud Haber and Menaldo for their data collection efforts. However, it is not possible to make inferences about the effects of a treatment by only observing a treated population over time; the treatment group must be compared with a control

group, even if nonrandom assignment introduces the danger of omitted variable bias. Lengthening the period in which the treated group is observed does not change this.

It is also unwise to assume that the relationships among key variables—like oil wealth and political power—are fixed over time, without paying close attention to potential changes in the conditions that affect the causal relationship. Nor does it make sense to imply that theories about causal relationships, like the resource curse, are only valid today if they were also true in centuries past. Most meaningful claims in political science are probably specific to a set of historical conditions that change over time. Just because causal relationships are historically specific, it does not mean they are invalid.

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Notes

1. Although the term *resource curse* can refer to many different phenomena, we follow Haber and Menaldo in using this term to refer to the hypothesis that countries with greater oil wealth are less likely to transition from authoritarian to democratic rule. Like Haber and Menaldo, we focus on the effects of oil and natural gas—which make up more than 90% of the world minerals trade—rather than nonfuel minerals.
2. The Haber–Menaldo study was featured on the *Freakonomics* blog on April 4, 2011, and *The Monkey Cage* blog on August 23, 2011. The number of citations is from a "Google Scholar" search on April 10, 2012.

3. According to the Haber–Menaldo data, in 1918, Mexico became the first country to produce US$100 per capita worth of oil. By 2006, 49 countries were producing at least US$100 per capita of oil (constant dollars).
4. The seven companies were Standard Oil of New Jersey (later Exxon), Standard Oil of California (later Chevron), Anglo-Iranian Oil Company (later BP), Mobil, Texaco, Gulf, and Royal Dutch Shell.
5. The earliest modern study of the "rentier state" was published by Mahdavy in 1970, and focuses largely on Iran. Little else was written on the topic before the late 1987, when Luciani and Beblawi edited a seminal volume. The term *resource curse* dates to a study by Richard Auty in 1993, and was popularized by a 1995 article by Jeffrey Sachs and Andrew Warner.
6. There is another important reason that Haber and Menaldo fail to identify a resource curse: They decline to test the most credible version of the resource curse hypothesis, which is that when autocratic states collect a lot of oil revenues, they become less likely to transit to democracy (e.g., Andersen & Aslaksen, 2013; Clark, Poast, & Weins, 2012; Morrison, 2009; Papaioannou & Siourounis, 2008; Ross, 2009; Smith, 2004; Ulfelder, 2007).
7. Note we are not looking at country's *oil dependence* (meaning the ratio of oil exports to GDP), but its *oil abundance* (meaning the value of a country's oil and gas production divided by its population). As many scholars have pointed out, measures of oil dependence are biased upwards in poor countries, making it a poor measure of resource wealth (Dunning, 2008; Ross, 2009). We once again define "oil abundant" countries as those whose oil income exceeded US$100 dollars per capita (in constant 2007 dollars) in a given year.
8. The 11 oil producers are Bahrain, Canada, Gabon, Iran, Iraq, Kuwait, Qatar, Saudi Arabia, Trinidad, United States, and Venezuela.
9. Using 1970 as the base year yields 16 significant oil producers: Algeria, Bahrain, Canada, Gabon, Iran, Iraq, Kuwait, Libya, Oman, Qatar, Russia (Soviet Union), Saudi Arabia, Trinidad, United States, the United Arab Emirates, and Venezuela. Moving the threshold to US$500 in 1970 creates a group of 11 countries: Bahrain, Gabon, Iraq, Kuwait, Libya, Oman, Qatar, Saudi Arabia, Trinidad, United Arab Emirates, and Venezuela.
10. According to one study, expropriations raised the government's share of oil profits from 50% in the early 1960s to 98% by 1974 (Mommer, 2002).
11. These tests make up 24 of the 55 statistical tests reported in their tables, all of their 18 graphs, and about half of the article's written pages.
12. The experimental analogy highlights a further problem with the Haber–Menaldo analysis: The discovery and production of oil is not a one-time "treatment" that lasts for a short period, thereby allowing us to observe a country "before" and "after" treatment. Most countries in the Haber–Menaldo sample discovered oil or other minerals and remained significant producers through the remaining years in the data set, making it impossible to observe them post-treatment.
13. Recent studies find that oil wealth, but not other types of mineral wealth, are associated with less frequent democratic transitions (e.g., Ross, 2009). Conflating

oil producers with mineral producers may hence obscure oil's antidemocratic effects. Here and elsewhere, we use Haber and Menaldo's data, which they generously shared with us.

14. Among the 34 oil-producing states on their list, they classify 11 as "resource blessed," three as "resource cursed," and the remaining 20 as "neither blessed nor cursed (6)."
15. Haber and Menaldo's willingness to make their data and log file public is exemplary, and we thank them for their graciousness in answering our questions.
16. We expect that the beginning of the period in which oil impeded democratic transitions varied from country to country, depending on the relationships between governments and international companies. The transfer of rents often took place over a 5- or 10-year period, as governments gradually gained control over foreign assets, renegotiated or abrogated contracts, reorganized existing national oil companies or established new ones, and developed new regulations. This makes it hard to identify a single year when the salient dimensions of nationalization took place—which is why we decline to use country-specific measures of nationalization. For more on the nationalization process, see Kobrin (1980), Mahdavi (2011).
17. Prevailing theories of the resource curse suggest oil wealth can strengthen authoritarian regimes through at least two pathways: by allowing rulers to reduce taxation and to increase spending on patronage (Ross, 2001). Haber and Menaldo's Fiscal Reliance variable measures the first pathway but not the second as it does not tell us the absolute size of government's resource revenues—only their size relative to other revenue sources.
18. For a more precise description of how we do this, see the notes to Table 3.
19. They also tend to incur unusually large debt loads when prices fall, which further cushions their regimes against popular uprisings (Nooruddin, 2008).
20. Notice that the country fixed effects have different interpretations in the ECM and in the dynamic fixed-effects model. In the ECM, the regressand is the *differenced* Polity score, hence the country fixed-effects have the interpretation of average country-specific *changes* in the Polity scores over the sample period. In the dynamic fixed-effects model, the regressand is the *level* of the Polity score, implying that the fixed effects allow the *levels* of democracy to differ across countries for reasons not explained by the included set of covariates.
21. We hasten to add that we do not think this is the correct way to model the effects of oil: other studies show that oil income keeps autocracies from transiting to democracy but has no average effect on democracies (see Endnote 7). This implies that the effects of oil in the ECM and the dynamic fixed-effects models are biased toward zero, understating oil's true effects.
22. The R^2 approach is discussed in Western and Kleykamp (2004). In the online appendix, we show that using the full sample, going back to 1800, makes little difference (Figure A3).
23. The panel is unbalanced, and the post-1945 regression with a 5-year lag includes 6,978 observations in total, of which 3,242 are post-1984 observations.

24. In the online appendix (Figure A1), we show the same pattern emerges in the full 1800-2006 sample, and the substantive effects are only slightly reduced.
25. Results available from authors.
26. At least, by scholars who use cross-national quantitative methods. Scholars who carry out country-level quantitative analyses are often more sensitive to historical factors.

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