

# The Missing Dimension of the Political Resource Curse Debate

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## Abstract

Given the methodological sophistication of the debate over the “political resource curse”—the purported negative relationship between natural resource wealth (in particular oil wealth) and democracy—it is surprising that scholars have not paid more attention to the basic statistical issue of how to deal with missing data. This article highlights the problems caused by the most common strategy for analyzing missing data in the political resource curse literature—listwise deletion—and investigates how addressing such problems through the best-practice technique of multiple imputation affects empirical results. I find that multiple imputation causes the results of a number of influential recent studies to converge on a key common finding: A political resource curse *does* exist, but only since the widespread nationalization of petroleum industries in the 1970s. This striking finding suggests that much of the controversy over the political resource curse has been caused by a neglect of missing-data issues.

## Keywords

quantitative methods, democratization and regime change, political economy, non-democratic regimes, political regimes

## Introduction

The debate over the existence of a so-called “political resource curse”—a negative relationship between natural resource wealth (in particular oil

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wealth) and democracy—has attained a high degree of methodological sophistication in recent years. It is thus surprising that contributors to this debate have not paid more attention to one of the most basic issues in the statistical analysis of cross-national political and economic data, namely, how to handle missing values. Data sets used to examine the political resource curse are often highly incomplete; indeed, I later show that levels of missingness in such data sets tend to be markedly higher than in other areas of quantitative comparative politics research. Nevertheless, analysts continue to deal with such values using the ad-hoc technique of listwise deletion—that is, to omit observations with missing values on any variable—which is known to produce inefficient and often biased statistical inferences in the typical conditions of applied empirical research.<sup>1</sup>

This article draws attention to the methodological problems caused by the use of listwise deletion in statistical analyses of the political resource curse and examines how addressing such problems through the best-practice technique of multiple imputation affects empirical results. I argue that, in addition to being inefficient, listwise deletion tends to produce biased inferences in such analyses because the distribution of missing values is not completely random. Most notably, authoritarian states are more likely to have incomplete data, causing listwise deletion to yield samples that are skewed toward the most democratic nations in the data set—a selection problem that I call *pro-democracy bias*. Multiple imputation, which has long been recommended by statisticians and is now emerging as the principal alternative to listwise deletion in many areas of the social and natural sciences, involves replacing each missing cell with multiple values based on information contained in the observed portion of the data set.<sup>2</sup> By utilizing all observed values, keeping incomplete observations in the sample, and preserving key relationships in the data, it both produces considerably more efficient inferences than listwise deletion and—crucially—avoids selection problems such as pro-democracy bias.

The reexamination reveals that multiple imputation alters the results of a number of prominent recent studies in ways that present a strikingly coherent, unified picture of the political resource curse. I focus on two important ongoing debates in the literature.<sup>3</sup> The first concerns the extent to which existing statistical evidence for a political resource curse is plagued by omitted variable bias and other sources of endogeneity.<sup>4</sup> I find that multiple imputation challenges Haber and Menaldo's (2011) influential conclusion that oil wealth is not negatively related to changes in democracy once we control for country fixed effects (and other sources of unobserved heterogeneity). Rather, it supports Andersen and Ross's (2014) claim that Haber and Menaldo's analysis *does* provide evidence of a negative relationship, but only after the widespread nationalization of petroleum industries in the 1970s, which enabled

governments to seize control of oil rents. In addition, it corroborates the results of Ramsay's (2011) instrumental variables analysis of the political resource curse—an alternative strategy for addressing endogeneity—which, consistent with Andersen and Ross's findings, indicates the existence of such a curse during a period that mostly coincides with or follows the wave of nationalizations.

The second debate concerns the effects of oil wealth on the stability of different types of political regimes. I find that multiple imputation casts doubt on evidence for Morrison's (2009) notable argument that oil and other nontax revenues reduce the probability of regime change in both dictatorships and democracies by enabling them to appease social groups that pose a threat to regime survival. Instead, it indicates that such revenues have “antidemocratic” effects, prolonging dictatorships but *destabilizing* democracies. Critically, however, such effects are once again restricted to the post-1980 period. These findings are substantiated by reanalyses of two important additional studies of the impact of oil wealth on regime stability: Ross's (2012) book “The Oil Curse,” whose results were already in line with the findings; and Wright, Frantz, and Geddes's (2015) study of the mechanisms of autocratic regime survival, whose results were only partially consistent with the findings but become fully consistent when re-estimated.

The reexamination thus provides remarkably congruent evidence that there *is* a political resource curse, albeit only during the relatively recent period in which governments have controlled most of the rents generated by petroleum production. These findings both shed new empirical light on the existence and scope conditions of the political resource curse and suggest that a major reason different studies of this issue have produced conflicting results is that they have overlooked the problem of missing data.

## **The Neglected Missing-Data Problem**

In this section, I provide a brief overview of the missing-data problem in statistical analyses of the political resource curse. The first part of the section discusses the specific methodological issues that arise when listwise deletion is used to deal with missing data in such analyses. The second part explains how multiple imputation can help to address these issues and hence improve the quality of statistical inferences.

### *Listwise Deletion and Pro-Democracy Bias*

Although scholars of the political resource curse are generally aware of the high proportion of missing values in their data sets—a trend that I

systematically document in the following section—they have paid surprisingly little attention to the potential methodological pitfalls of using listwise deletion to handle such values.<sup>5</sup> The performance of listwise deletion in analyses of the political resource curse can be evaluated in terms of three basic statistical criteria: bias, efficiency, and the ability to yield reasonable estimates of uncertainty (Graham, 2009). With respect to efficiency, listwise deletion is always wanting: By discarding information contained in incomplete observations, it results in higher standard errors, wider confidence intervals, and reduced statistical power. Although it fares better on the third criterion—estimated standard errors are generally valid—this advantage is offset by losses in efficiency (Allison, 2002).

The bias caused by listwise deletion is a more complex issue that rests on the *mechanism* by which data become missing. Scholars usually distinguish between three such mechanisms (Little & Rubin, 1987, 2002). Data are (a) *missing completely at random* (MCAR) if the probability that a given value is missing does not depend on any information in the data set, (b) *missing at random* (MAR) if it depends on observed data only, and (c) *missing not at random* (MNAR) if it depends (at least in part) on missing data.<sup>6</sup> Listwise deletion produces unbiased inferences only when the MCAR assumption holds—that is, when omitting incomplete observations leaves a random sample of the data. Under MAR or MNAR, deleting such observations produces samples that are skewed away from units with characteristics that render them more likely to have missing data.

How do data on the political resource curse become missing? A first, crucial point is that the MCAR assumption is unlikely to be satisfied in any area of comparative politics or political science more generally. As Cranmer and Gill (2013) note, “It is difficult to think of a situation in political science, other than a computer malfunction, that would result in missing values being entirely unrelated to *any* attribute or political phenomena, observed or unobserved” (p. 429). Indeed, in the next section, I show that the MCAR assumption—which, unlike the MAR and MNAR assumptions, can be tested in practice—is violated in every study I reanalyze. In general, therefore, listwise deletion will tend to produce biased inferences in analyses of the political resource curse.

If missingness in data sets on the political resource curse is not completely random, what is it caused by? Although the answer will vary from one data set to another depending on its specific range of countries, years, and variables, one factor that tends to be important in almost all cases is a state’s *political institutions*.<sup>7</sup> Empirical studies have found that democracies are more likely to release policy-relevant socioeconomic data to the public and to international organizations than dictatorships (controlling for income and other variables; Edwards, Coolidge, & Preston, 2011; Hollyer, Rosendorff, &

Vreeland, 2011; Ross, 2006).<sup>8</sup> One potential explanation for this difference is that democratic leaders have stronger incentives to adhere to popular demands for transparency because they can be removed from power (Hollyer et al., 2011). Another is that democracies depend less on strong economic performance for their legitimacy and are thus less concerned about revealing the true state of the economy. It is also conceivable that democratic institutions embody norms of transparency and accountability that politicians externalize in their interactions with the international community.<sup>9</sup> Regardless of the precise causal mechanism, measures of democracy are likely to be strongly related to the pattern of missing data.

The upshot is that applying listwise deletion to data sets on the political resource curse will often give rise to a selection problem that might be called *pro-democracy bias*. Because authoritarian states typically report fewer data, listwise deletion will tend to produce samples in which the most democratic nations in the data set are overrepresented. The effect of this bias on estimates of the strength of the negative relationship between democracy and natural resource wealth will depend on the particular range of countries and years covered by the data set. In some data sets, observations with lower levels of democracy will have higher levels of resource wealth, causing inferences based on listwise deletion to understate the strength of this relationship. In other data sets, less democratic observations will have lower levels of resource wealth, producing the opposite effect. In either case, as shown in the next section, pro-democracy bias has significant implications for our empirical understanding of the political resource curse.

### *Addressing the Problem With Multiple Imputation*

How can multiple imputation address the shortcomings of listwise deletion in analyses of the political resource curse? Multiple imputation involves three key stages.<sup>10</sup> First,  $m$  values are imputed for each missing cell, with variation across values reflecting uncertainty about the correct imputation model. Imputed values are independent draws from a posterior distribution of the missing data conditional on the observed data, typically derived from a parametric model that assumes the complete data are multivariate normal.<sup>11</sup> Second, each of the  $m$  complete data sets is analyzed. Due to the separation between the imputation and analysis stages, complete-data methods can be applied to each data set, making this a straightforward task. Finally, the  $m$  separate point estimates are combined into one using the “Rubin rules” (Rubin, 1987). These rules state that the pooled point estimate is equal to the average of the  $m$  estimates, while its variance is equal to a weighted sum of the estimated variances within and between the  $m$  data sets.<sup>12</sup>

Multiple imputation is substantially more efficient than listwise deletion because it (a) utilizes rather than discards data in incomplete observations and (b) allows analysts to incorporate extra information into the imputation model by including variables that are not in the analysis (“auxiliary variables”). Multiple imputation also performs at least as well as listwise deletion on the third criterion mentioned earlier as it reflects uncertainty about imputed values and thus yields valid estimates of standard errors. This is a major advantage over ad-hoc “single” imputation methods such as mean substitution, zero imputation, and linear interpolation. These methods produce downward-biased standard errors because they treat imputed values as “knowns” rather than probabilistic estimates.<sup>13</sup> They can thus be legitimately accused of “making up data”—a common misconception about multiple imputation. The goal of multiple imputation is in fact to *preserve* important features of the existing data (such as means, variances, and covariances) while capturing the uncertainty inherent in missing-data prediction.

Can multiple imputation address selection problems such as pro-democracy bias? Unlike listwise deletion, multiple imputation is unbiased when data are MAR as well as MCAR. Under MNAR, however, multiple imputation cannot avoid bias: Because missingness depends (to some extent) on missing values, observed data alone do not provide the basis for a valid imputation process. Strictly speaking, real data are almost always MNAR, with missingness depending in part on observed data and in part on missing data (Graham, 2009). Critically, however, multiple imputation is not seriously biased under MNAR if missingness is strongly related to observed data and thus approximates MAR (Collins, Schafer, & Kam, 2001; Graham, Hofer, & MacKinnon, 1996; Schafer, 1997). The key question is thus: How *much* does missingness depend on observed data?<sup>14</sup> Obviously, this is not possible to directly measure because we do not actually have access to missing data. Nevertheless, if the data set contains one or more variables that are highly correlated with missingness, it is reasonable to assume that multiple imputation will perform almost as well as under (pure) MAR. If there are no such variables, however, the technique may result in major bias—although, importantly, still no more than listwise deletion in almost all cases.<sup>15</sup> Indeed, even in this worst case scenario, multiple imputation is still the preferable of the two strategies due to its superior efficiency.

The implication is that multiple imputation can help to mitigate pro-democracy bias so long as variables that measure or are correlated with democracy are included in the data set—which, of course, is always the case in analyses of the political resource curse.<sup>16</sup> The precise extent of these gains in bias reduction will vary as a function of two factors. The first is the proportion of incomplete observations in the data set. The higher this fraction, the

greater the extent to which more democratic observations will be overrepresented in samples produced by listwise deletion and thus the greater the gains from keeping all observations in the sample through multiple imputation. The second is the number of variables in the data set that are related to missingness. Although all data sets on the political resource curse contain at least one such variable—that is, a measure of democracy—higher numbers increase the degree to which missingness is related to observed data and thus lower the bias resulting from multiple imputation.

## Reexamining Previous Findings

The preceding discussion suggests that in analyses of the political resource curse, multiple imputation typically offers major gains in efficiency and bias reduction over listwise deletion (and almost never performs worse than it). This section investigates how substituting multiple imputation for listwise deletion affects the results of a number of influential empirical studies on the topic. I begin by providing an overview of missing-data patterns in these studies and describing the specific steps by which I implement multiple imputation. The rest of the section presents the results of the reanalysis in detail.

### *Scope and Implementation*

I re-estimate the results of seven empirical studies on the political resource curse (see Table 1). These studies were selected for four reasons. First, most of them were conducted recently—all but one were published after 2008—and can thus be said to represent “state-of-the-art” knowledge on the topic. Second, despite their novelty, they are widely cited in the academic literature and in many cases have also received considerable attention in policymaking circles.<sup>17</sup> Third, and closely related, they have each introduced significant theoretical and/or empirical innovations that have influenced subsequent studies and advanced our understanding of the political resource curse, as discussed in more detail later.<sup>18</sup> Finally, they contribute to arguably the two most prominent ongoing debates in the literature, namely, those concerning (a) whether there is statistical evidence for a political resource curse once we properly control for sources of endogeneity, and (b) how oil wealth affects the stability of different types of political regimes.

An examination of the studies’ data sets and main empirical analyses reveals two important trends.<sup>19</sup> First, as shown in Table 1, they suffer from a severe missing-data problem. On average, almost one-quarter of the cells in the data sets are missing, with the figure exceeding 30% in three studies. The use of listwise deletion excludes more than 40% of eligible observations from

**Table 1.** Overview of Data Sets in Reanalyzed Studies.

Study	Main empirical analysis (%)					Polity effect on inclusion		
	Missing cells (%)	Cases omitted	Existing data lost	Countries missing	Years missing	MCAR	Sign	Significance level
Ross (2001)	33.37	41.84	37.21	26.75	15.63	X	+	***
Morrison (2009)	32.65	78.52	69.84	44.57	32.56	X	+	***
Haber and Menaldo (2011)/Andersen and Ross (2014)	30.83	67.08	67.08	44.64	0.48	X	+	***
Ramsay (2011)	2.97	22.77	21.79	5.73	0.00	X	+	***
Ross (2012)	27.12	12.58	5.05	5.06	3.75	X	NA	
Wright, Frantz, and Geddes (2015)	13.89	27.08	18.51	18.57	7.58	X	NA	
Average	23.47	41.65	36.58	24.22	10.00			

MCAR = missing completely at random; Polity effect on inclusion = the results of a logistic regression in which the dependent variable is a dummy for whether a given observation is included in a study's main empirical analysis and the regressor is a variable recording Polity scores (Ross, 2012 and Wright, Frantz, and Geddes, 2015 are excluded because their analyses are restricted to either democracies or dictatorships).  
 \*\*\*p < .01.



the studies' analyses, causing the loss of 37% of available observed values.<sup>20</sup> As a result, a relatively large proportion of eligible countries in the data sets—almost one quarter on average—are not just underrepresented but entirely omitted from the analyses.<sup>21</sup> Naturally, levels of missingness and data loss vary across the studies, and are relatively modest in the case of Ramsay (2011) and Ross (2012). Nevertheless, it is noteworthy that in the majority of studies, at least 27% of cells in the data set are missing, 27% of eligible observations are excluded from the analysis, 22% of available observed data are lost, and 19% of eligible countries are completely discarded.

Is the missing-data problem in these studies *unusually* severe? To answer this question, I conducted a systematic review of missingness in a large sample of recently published comparative politics studies. Specifically, I sought to obtain the data set of every cross-national quantitative study published during the 3-year period 2013-2015 inclusive in the *American Political Science Review*, the *American Journal of Political Science*, *Comparative Political Studies*, the *Journal of Politics*, and *World Politics*.<sup>22</sup> Through searches of online data repositories and authors' personal websites, I was able to acquire 114 of the 231 data sets, a full list of which is provided in Table A1 of the online appendix (along with individual values of the statistics described below).<sup>23</sup> On average, 14% of cells in the data sets were missing, more than 40% lower than the equivalent figure for the six political resource curse data sets (23%). Moreover, 61% of observations in the former data sets had at least one missing value (a rough proxy for how much information will be lost due to listwise deletion), compared with 90% in the latter.<sup>24</sup> This evidence strongly suggests that the missing-data problem in studies of the political resource curse is indeed more severe than in other areas of comparative politics (while indicating that such areas would also benefit from adopting more sophisticated missing-data methods).

The second trend is that the data sets are considerably better suited to multiple imputation than listwise deletion. The standard "Little's MCAR test," which evaluates a null MCAR hypothesis that observed variable means for subgroups of observations sharing the same missing-data pattern do not differ from expected population means based on maximum likelihood estimates, indicates that the MCAR assumption is not satisfied in a single case (full test results are reported in Table A2 of the online appendix).<sup>25</sup> Furthermore, missingness is strongly related to observed data. All the data sets contain a variable recording Polity scores, the standard measure of democracy in the literature. Moreover, the sample included in each study's main analysis has a substantially higher mean Polity score than the sample excluded from it (see Table A3).<sup>26</sup> Although we cannot compare absolute means across studies because they are based on alternative versions of the

Polity data set and are scaled in different ways (as discussed in more detail below), the included sample means are on average 97% higher than the excluded ones. A student's *t* test shows that in each case, these differences are statistically significant at the 1% level.<sup>27</sup>

To more rigorously assess the extent to which missingness is related to democracy, for each study, I estimated a logistic regression model in which the dependent variable is a dummy variable indicating whether or not a given observation is included in the main analysis and the regressor is a variable recording Polity scores. As indicated in Table 1, the coefficients on the latter variable are positive and significant at the 1% level in all studies.<sup>28</sup> It is also important to note that in each data set, there were several other variables that had positive and significant coefficients when added to the regression model (including income, economic growth, public spending, and trade). In sum, any bias resulting from multiple imputation is likely to be small.

To implement multiple imputation, I use Honaker, King, and Blackwell's *Amelia II* program in R, the most widely used multiple imputation software in political science.<sup>29</sup> My implementation strategy follows the recommendations of the *Amelia II*'s software manual, with a few small exceptions (Honaker, King, & Blackwell, 2011). First, as *Amelia II*'s expectation-maximization algorithm sometimes fails to converge when applied to large data sets, if the total number of variables exceeds 100, I exclude from the imputation model all auxiliary variables that fail to meet the following two requirements: (a) they have a correlation of  $r \geq 0.5$  with at least one analysis variable or at least one specially created dummy indicating whether observations for a given analysis variable are missing, and (b) less than 25% of their values are missing (as highly incomplete variables provide information at greater cost in terms of model size).<sup>30</sup>

Second, I do not round off imputed values for categorical variables to the nearest discrete number, as this practice has been shown to produce biased parameter estimates.<sup>31</sup> For the same reason, I also avoid applying logarithmic, square root, and logistic transformations to heavily skewed variables to normalize their distributions.<sup>32</sup> I add only three features to the imputation model: (a) a sequence of third-order time polynomials to better model smooth temporal variation within cross-section units, (b) lags of the dependent and key explanatory variables (or leads if they are already lagged) because data for one period tend to be highly correlated with data for the previous (and subsequent) period, and (c) a ridge prior of 1% of the total number of observations, which addresses computational problems caused by high levels of missing data and multicollinearity.

Finally, I conduct more than the five imputations generally recommended by *Amelia II*'s software manual (Honaker et al., 2011). Recent statistical

research has shown that conducting only five imputations frequently results in unacceptably weak statistical power, wide confidence intervals, and large Monte Carlo standard errors (i.e., errors across repeated runs of the same imputation process; Bodner, 2008; Graham, Olchowski, & Gilreath, 2007; White, Royston, & Wood, 2011). To avoid these problems, I set  $m$  equal to the average missing-data rate of all variables in the imputation model.<sup>33</sup>

### *Does Oil Really Hinder Democracy?*

I begin the reexamination by revisiting the findings of Ross's (2001) seminal article, "Does Oil Hinder Democracy?"—one of the most influential studies in all of comparative political economy—which launched the debate over the political resource curse.<sup>34</sup> To test the hypothesis that oil hinders democracy, which was first proposed by Middle East specialists in the 1970s but previously examined only in a handful of case studies, Ross analyzed pooled data on 157 countries in the period 1966-1997 using a feasible generalized least squares model. The dependent variable *Regime* is measured using Polity98 scores (rescaled to run from 0 to 10), whereas the key explanatory variable *Oil* is measured as mineral-based fuel exports as a fraction of GDP. In addition, the model contains a lagged dependent variable, year dummies, and a host of control variables. The main results, reproduced in Table 2, are consistent with the political resource curse hypothesis: The coefficient on *Oil* is negative and statistically significant at the 10% level in all three specifications.<sup>35</sup>

Although most variables in the analysis have relatively complete data, 40% of values for *Oil* are missing. Consequently, listwise deletion restricts us to using just 58% of eligible observations and discards 37% of observed data. This results in the complete exclusion of 42 of the 157 countries in the data set. Importantly, observations included in the analysis differ systematically from those excluded, possessing higher levels of both *Regime* and *Oil*. These differences are illustrated graphically in the upper-left panels of Figures 1 and 2, which plot the distribution of *Regime* and *Oil*, respectively, for included (black line) and excluded (gray line) observations. In each case, the distribution of excluded observations is concentrated at lower levels of the variable than the distribution of included observations. The implication is that autocracies that produce little or no oil—cases that do *not* provide evidence for the political resource curse hypothesis—are underrepresented in the analysis. This may cause it to overestimate the strength of the negative relationship between *Oil* and *Regime*.

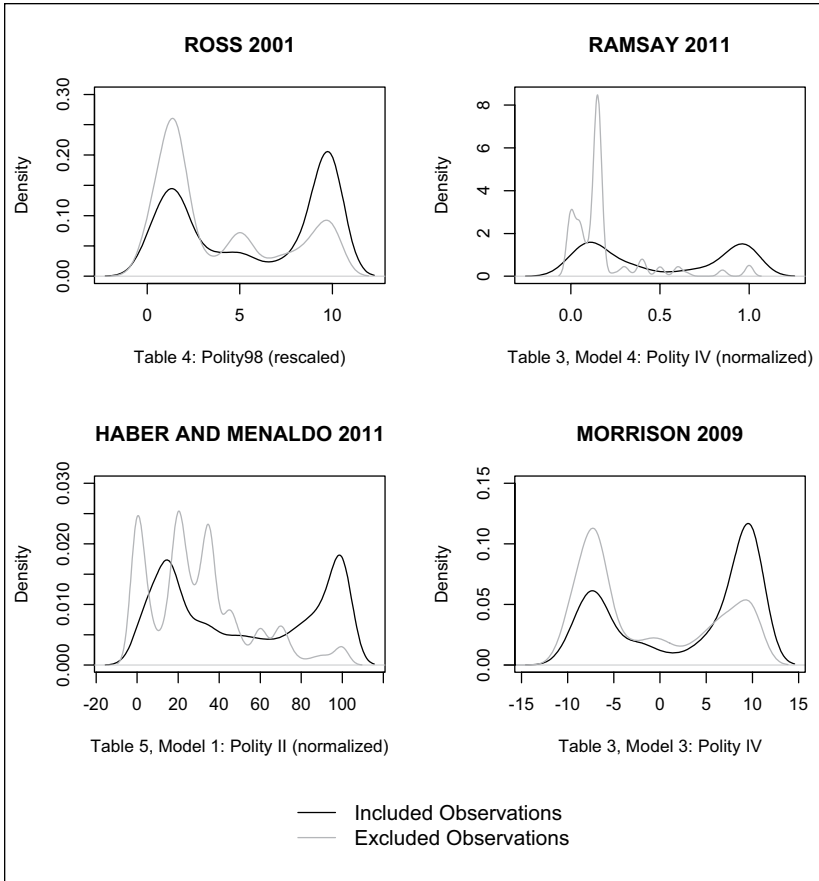
Indeed, when the excluded observations are incorporated into the analysis through multiple imputation—which enables us to use 59% more observed values and almost twice as many observations—support for the

**Table 2.** Ross (2001): Resource Wealth and Democracy (Table 4 in Original Study).

	(1)		(2)		(3)	
	Replicated	Imputed	Replicated	Imputed	Replicated	Imputed
Regime (lagged)	0.267*** (0.020)	0.261*** (0.017)	0.239*** (0.020)	0.263*** (0.016)	0.242*** (0.020)	0.264*** (0.016)
Oil	-0.030*** (0.005)	-0.007** (0.003)	-0.016*** (0.005)	-0.003 (0.003)	-0.011** (0.005)	-0.003 (0.003)
Minerals	-0.043*** (0.008)	-0.012* (0.006)	-0.028*** (0.007)	-0.007 (0.006)	-0.028*** (0.007)	-0.007 (0.006)
Income (log)	0.920*** (0.100)	-0.000 (0.000)	0.712*** (0.124)	-0.000 (0.000)	0.684*** (0.126)	-0.000 (0.000)
Islam	-0.019*** (0.002)	-0.015*** (0.002)	-0.011*** (0.003)	-0.011*** (0.002)	-0.015*** (0.002)	-0.012*** (0.002)
OECD	1.395*** (0.302)	3.892*** (0.269)	1.469*** (0.310)	3.458*** (0.262)	1.517*** (0.312)	3.483*** (0.262)
Large	0.429 (0.297)	-0.556*** (0.161)				
Mideast			-2.792*** (0.427)	-1.272*** (0.271)		
SSAfrica			-1.524*** (0.174)	-0.938*** (0.150)	-1.446*** (0.174)	-0.873*** (0.148)
Arabian Peninsula					-3.439*** (0.525)	-1.223*** (0.291)
Constant	-3.313*** (0.736)	2.980*** (0.233)	-1.081 (0.780)	2.841*** (0.196)	-0.934 (0.796)	2.802*** (0.196)
N	2,181	3,750	2,181	3,750	2,181	3,750

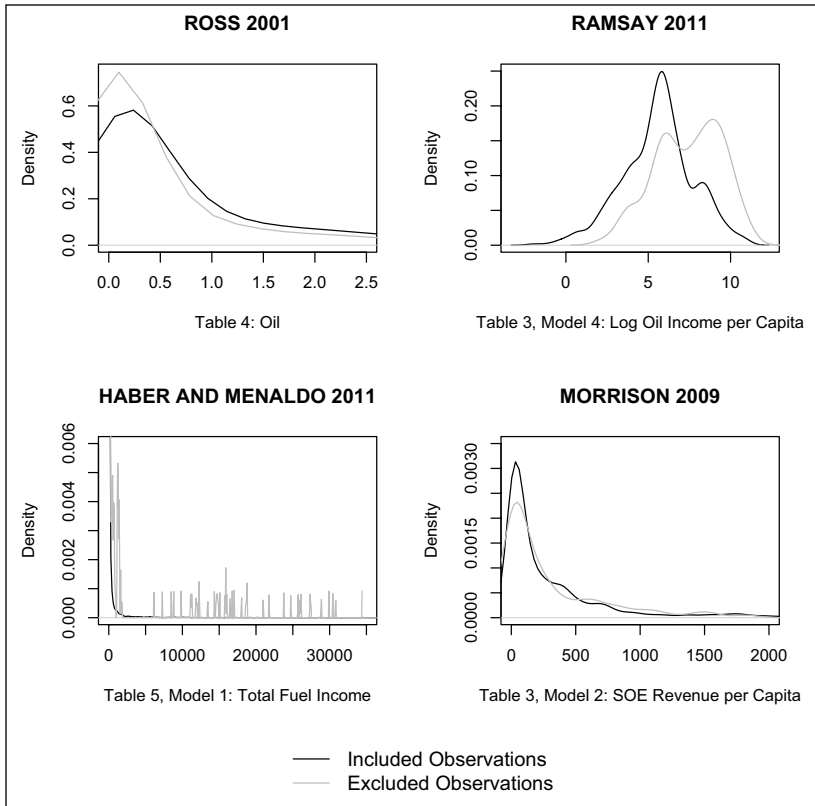
Dependent variable is democracy (measured by Polity98 scores rescaled from 0 to 10). All right-hand-side variables are lagged 5 years. Year dummies estimated but not reported. Feasible generalized least squares regressions corrected for first-order autocorrelation with standard errors in parentheses. OECD = Organisation for Economic Co-operation and Development. \* $p < .1$ . \*\* $p < .05$ . \*\*\* $p < .01$ .

political resource curse hypothesis all but disappears. In Model 1, the coefficient on *Oil* shrinks to just one fifth of its original size, although remains significant at the 5% level. In Models 2 and 3, which include region dummies, the coefficient halves again and loses significance. In other words, once we control for the unobservable, time-invariant characteristics of an oil producer's region—or what Ross (2001) concisely calls “the region's history” (p. 345)—we can no longer be confident that *Oil* has a strong negative relationship with *Regime*.



**Figure 1.** Polity scores of observations included in and excluded from main empirical analysis.

Since the article’s publication, there have been a large number of follow-up studies that seek to address potential problems with its empirical analysis, generating a lively debate over the validity of the original finding. Do the conclusions of these more recent studies hold up when their results are re-estimated using multiple imputation? The rest of the section examines attempts to deal with two important issues: (a) endogeneity, which occurs when the explanatory variable is correlated with the error term: and (b) failure to distinguish between the effects of oil wealth on dictatorships and democracies.



**Figure 2.** Oil wealth of observations included in and excluded from main empirical analysis.

SOE = state-owned enterprise.

### *The Conditional Effects of Oil on Democracy*

There are three potential sources of endogeneity in Ross's analysis: (a) measurement error with respect to *Oil*; (b) reverse causation between *Regime* and *Oil*, for instance, if oil prices respond to political changes or oil exports depend on income (because poorer countries consume less oil domestically), which is, in turn, influenced by regime type (Ross, 2008, 2012); and (c) perhaps most importantly, omitted variable bias resulting from a failure to properly control for unobserved country-specific heterogeneity.

In an innovative recent study, Ramsay (2011) attempts to address all three possibilities by using an instrumental variables approach, which involves

replacing the endogenous variable with an “instrument” that is correlated with it but uncorrelated with the error term (and thus does not directly affect the dependent variable).<sup>36</sup> Specifically, Ramsay uses out-of-region natural disasters as an instrument for the total value of oil production per capita (rather than oil exports as a fraction of GDP), on the grounds that such events cannot influence regime type other than through their effect on the price of oil.<sup>37</sup> Analyzing panel data on 48 oil producers in the period 1968-2002 using two-stage least squares regression, Ramsay finds evidence of a political resource curse. As shown in Table 3, the predicted values of *Log Oil Income per Capita* have a negative and significant relationship with (normalized) Polity IV scores. Unlike before, however, the results are robust to multiple imputation: Although marginally declining in size, the coefficient on *Log Oil Income per Capita* remains negative and significant in all four replicated models.

What explains this difference? Ramsay’s data set, in contrast to Ross’s, has a low rate of missingness, with more than 97% of cells observed (and 91% for the instrument *Log Out of Region Disaster Estimates*). As a result, listwise deletion omits a more modest 23% of eligible observations and 22% of observed values. This ensures that differences in levels of democracy and oil income between included and excluded observations (see the upper-right panels of Figures 1 and 2, respectively) are less likely to seriously bias inferences produced by listwise deletion.

Ramsay’s findings, however, have not ended the debate due to doubts about the validity of his instrument. As Haber and Menaldo (2011) point out in an influential study, global oil price shocks are usually met with offsetting increases in production by major exporters, who have a policy of stabilizing prices. Thus, the instrument may be picking up a “big producer” fixed effect (and thus be correlated with the error term). To properly control for country-specific factors, Haber and Menaldo estimate an error-correction model with country fixed effects using new data on 163 countries from 1800 to 2006.<sup>38</sup> The results do not support the political resource curse hypothesis: Oil income per capita and government reliance on resource revenues have a *positive* relationship with changes in Polity IV scores (normalized to run from 0 to 100; see Table 4).<sup>39</sup>

Haber and Menaldo’s analysis has been challenged by Andersen and Ross (2014), who argue that it supports a conditional version of the political resource curse hypothesis that posits a negative relationship between oil wealth and changes in levels of democracy only *after* 1980. Indeed, when they add an interaction term between *Total Oil Income* and a dummy for the post-1980 period to the core fixed effects models, its coefficient is negative and significant (see Table 4).<sup>40</sup> Andersen and Ross’s explanation is that oil

**Table 3. Ramsay (2011): Two-Stage Least Squares: Polity IV Scores on Oil Income (Table 3 in Original Study).**

	World oil producers' political freedom measure							
	Polity IV (1)		Polity IV (2)		Polity IV (3)		Polity IV (4)	
	Replicated	Imputed	Replicated	Imputed	Replicated	Imputed	Replicated	Imputed
Log oil income per capita	-0.633*** (0.206)	-0.685** (0.290)	-0.356*** (0.077)	-0.276*** (0.049)	-0.356*** (0.077)	-0.278*** (0.049)	-0.357** (0.167)	-0.242*** (0.083)
Log GDP per capita			0.361*** (0.056)	0.310*** (0.041)	0.355*** (0.053)	0.308*** (0.040)	0.355** (0.155)	0.263*** (0.083)
GDP growth					-0.012*** (0.004)	-0.009*** (0.003)	-0.012** (0.005)	-0.008*** (0.003)
Polity at entry							-0.001 (0.371)	0.186 (0.209)
Constant	2.803*** (0.836)	3.272*** (1.238)	-1.031*** (0.206)	-0.799*** (0.148)	-0.961*** (0.200)	-0.737*** (0.142)	-0.962** (0.438)	-0.626*** (0.219)
	Log oil revenues per capita (first stage)							
	(1)	(2)	(3)	(4)				
Log out of region disaster estimates	0.178*** (0.056)	0.154*** (0.050)	0.245*** (0.055)	0.296*** (0.044)	0.244*** (0.054)	0.297*** (0.044)	0.117** (0.056)	0.154*** (0.048)
Log GDP per capita			0.733*** (0.055)	0.827*** (0.046)	0.710*** (0.054)	0.814*** (0.046)	0.937*** (0.040)	1.000*** (0.033)
GDP growth					-0.040*** (0.010)	-0.042*** (0.009)	-0.027*** (0.008)	-0.032*** (0.008)
Polity at entry							-2.090*** (0.173)	-2.314*** (0.162)
Constant	0.077 (1.245)	1.207 (1.118)	-7.127*** (1.417)	-7.992*** (1.129)	-6.888*** (1.406)	-7.742*** (1.114)	-5.051*** (1.353)	-5.330*** (1.133)
N	1,379	1,680	1,277	1,680	1,267	1,680	1,267	1,680

Dependent variable is level of democracy (measured using normalized Polity IV scores). Two-stage least squares regressions with Newey–West heteroskedastic and autocorrelation-consistent standard errors in parentheses.

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



**Table 4.** Haber and Menaldo (2011): Error-Correction Models; Andersen and Ross (2014): Replication of Haber–Menaldo ECM (Table 2 in Original Study).

	(1)		(2)		(3)		(4)	
	Replicated	Imputed	Replicated	Imputed	Replicated	Imputed	Replicated	Imputed
Polity in	-0.087***	-0.058***	-0.087***	-0.059***	-0.109***	-0.058***	-0.110***	-0.059***
Levels <sub>t-1</sub>	(0.008)	(0.005)	(0.008)	(0.005)	(0.024)	(0.005)	(0.023)	(0.005)
Total Oil	0.055***	-0.045	0.048**	-0.024				
Income <sub>t-1</sub>	(0.019)	(0.033)	(0.024)	(0.031)				
Total Oil			-0.147***	-0.203***				
Income <sub>t-1</sub> ×			(0.054)	(0.062)				
Post-1980								
Δ Total Oil	-0.020	-0.063	-0.059**	-0.103**				
Income	(0.021)	(0.049)	(0.027)	(0.052)				
Δ Total Oil			-0.048	-0.047				
Income ×			(0.037)	(0.065)				
Post-1980								
Log(per Capita	-0.279	0.212	-0.273	0.253*	0.845	0.122	0.324	0.095
Income) <sub>t-1</sub>	(0.326)	(0.138)	(0.324)	(0.137)	(0.827)	(0.177)	(0.809)	(0.177)
Civil War <sub>t-1</sub>	0.065	-0.020	0.057	-0.030	1.435	-0.068	1.607	0.010
	(0.458)	(0.346)	(0.458)	(0.346)	(1.330)	(0.351)	(1.331)	(0.360)
Regional	0.025***	0.029***	0.025***	0.027***	0.012	0.032***	-0.002	0.029***
Democratic	(0.007)	(0.005)	(0.007)	(0.005)	(0.023)	(0.007)	(0.024)	(0.007)
Diffusion <sub>t-1</sub>								
Global	0.058*	-0.010	0.059**	-0.005	-0.050	-0.011	-0.076*	-0.000
Democratic	(0.030)	(0.033)	(0.030)	(0.033)	(0.039)	(0.032)	(0.042)	(0.033)
Diffusion <sub>t-1</sub>								
Δ Log(per	1.289	1.425	1.199	1.355	-3.698	1.071	-3.391	1.016
Capita	(1.772)	(1.271)	(1.774)	(1.271)	(3.771)	(1.422)	(3.800)	(1.425)
Income)								
Δ Regional	0.375***	0.370***	0.375***	0.370***	0.168*	0.371***	0.167*	0.372***
Democratic	(0.071)	(0.056)	(0.071)	(0.056)	(0.085)	(0.055)	(0.086)	(0.055)
Diffusion								
Δ Global	-0.277**	0.015	-0.277**	0.016	0.113	0.012	0.120	0.021
Democratic	(0.111)	(0.116)	(0.111)	(0.116)	(0.126)	(0.115)	(0.125)	(0.115)
Diffusion								
Fiscal					0.023	0.005	0.047**	0.013
Reliance <sub>t-1</sub>					(0.018)	(0.012)	(0.021)	(0.012)
Fiscal Reliance <sub>t-1</sub>					0.049**	0.036	0.042**	0.039
× Post-1980					(0.022)	(0.036)	(0.020)	(0.036)
Δ Fiscal Reliance							-0.057***	-0.027**
							(0.021)	(0.011)
Δ Fiscal Reliance							0.004	-0.015
× Post-1980							(0.026)	(0.026)
Constant	6.989***	1.025	6.959***	0.790	2.443	1.623	6.986	1.835
	(2.349)	(2.004)	(2.343)	(1.998)	(6.624)	(2.209)	(6.850)	(2.205)
N	10,195	17,206	10,195	17,206	1,132	17,206	1,132	17,206

Dependent variable is change in democracy (measured by Polity IV scores normalized to run from 0 to 100). Country and time fixed effects error-correction regressions with Driscoll–Kraay standard errors (estimated with Newey–West adjustment with one lag length) in parentheses. Separate country and year intercepts not reported. ECM = error-correction model.

\*p < .1. \*\*p < .05. \*\*\*p < .01.

wealth only acquired antidemocratic properties following the widespread nationalization of petroleum industries during the 1970s, which transferred oil rents from international petroleum companies (most notably the so-called “Seven Sisters”) to governments.<sup>41</sup> This argument is consistent with the popular “rentier state” theory of the political resource curse, according to which oil rents alleviate social pressures for democratization by enabling governments to reduce taxes and increase spending on patronage (Beblawi & Luciani, 1987; Mahdavy, 1970; Ross, 2001).

Haber and Menaldo’s data set is highly incomplete, with 31% of cells missing (and more than 50% for one fifth of the variables). Missing values for several variables are filled in using linear interpolation, which produces biased standard errors (see Note 9) as well as biased point estimates when—as is common in comparative politics—variables change smoothly over time or exhibit sharp deviations from a trend (Honaker & King, 2010). Listwise deletion is applied to the remaining variables, resulting in the omission of 67% of eligible observations and the same proportion of observed data. Interestingly, excluded observations have lower levels of democracy but *higher* levels of resource income than included ones (see lower-left panels of Figures 1 and 2, respectively).<sup>42</sup> Specifically, data on 33 oil-producing autocracies are omitted, including every major producer that has never democratized. This suggests that the analysis suffers from the opposite selection problem to Ross’s: By excluding stable oil-rich autocracies, it is biased *against* finding a negative relationship between oil wealth and democracy.

The combination of this selection problem, the high proportion of missing data, and the use of two biased methods for handling such data ensures that multiple imputation alters the results substantially. As shown in Table 4, in Haber and Menaldo’s original analysis, the positive coefficient on *Total Oil Income* loses significance and turns negative, while the coefficient on *Fiscal Reliance*, which was not significant in the first place, shrinks to less than one quarter of its original size.<sup>43</sup> In Andersen and Ross’s modified model, the coefficients on the interactions between these variables and the post-1980 dummy remain negative and significant, increasing by almost 50% in the case of *Total Oil Income*. The coefficients on the two variables themselves, meanwhile, drop out of significance, with that on *Total Oil Income* also turning negative. Interestingly, the latter change might be seen as even more consistent with the logic of Andersen and Ross’s conditional argument, because governments would presumably derive at least *some* rents from oil wealth even before petroleum industries were nationalized (for instance, through corporate taxes and royalties).<sup>44</sup> Finally, it is important to note that the results are also consistent with those of Ramsay’s analysis, although the latter did not explicitly test a conditional version of the political resource curse

hypothesis: Since Ramsay's panel begins in 1968, almost every year covered by the analysis is either during or after the wave of nationalizations.

### *Is Regime Type a Conditioning Factor?*

The second issue with Ross's analysis is that it fails to distinguish between two possible scenarios: (a) Oil wealth reduces the likelihood that dictatorships become democracies, and (b) oil wealth increases the likelihood that democracies become dictatorships (Ulfelder, 2007). One of the most notable attempts to disentangle these possibilities has been made by Morrison (2009), who argues that sources of nontax revenue such as oil make *both* dictatorships and democracies more stable by enabling them to pursue fiscal policies that appease social groups who pose a threat to regime survival.<sup>45</sup> In other words, oil does not have antidemocratic properties per se, but only stabilizing properties. Using a data set on 184 countries in the period 1960–2002, Morrison tests this hypothesis by logistically regressing a binary measure of regime change on nontax revenue per capita and (separately) its three components, the largest of which is state-owned enterprise (SOE) revenue, the main form of oil rent for governments.<sup>46</sup> The results bear out the hypothesis: As shown in Table A1 of the online appendix, the coefficients on all measures of nontax revenue, including SOE revenue, are negative and significant.<sup>47</sup>

Morrison's data set is highly incomplete, with one third of cells missing (and one third of variables possessing a missing-data rate of more than 70%). Listwise deletion discards 79% of eligible observations and 69% of observed data, resulting in the complete exclusion of a remarkable 82 countries. Similar to the previous case, included observations have lower levels of democracy but higher levels of SOE revenue than excluded ones (see lower-right panels of Figures 1 and 2, respectively). Unlike before, however, the excluded data comprise a large number of oil-producing states that underwent regime change *away* from democracy (and relatively few stable autocracies). Indeed, shifts toward autocracy in a remarkable 60 oil producers—including nine of the 20 largest today—are omitted from the analysis.<sup>48</sup> The results are thus likely to overestimate the stabilizing effects of oil rents and thus nontax revenues more broadly.<sup>49</sup> Perhaps unsurprisingly, therefore, when the analysis is re-estimated with imputed data, the coefficients on both the overall measure of nontax revenue and its SOE revenue component cease to be significant and fall to zero in the two core models (Models 1 and 2; see Online Appendix Table A4).<sup>50</sup>

Although these results signal the absence of a strong negative relationship between nontax revenue and regime instability, they do not distinguish between dictatorships and democracies. In Table 5, therefore, I split the

**Table 5. Morrison (2009): Nontax Revenue's Effect on Regime Instability (Table 3 in Original Study) with Split Samples.**

	Democracies (Polity score $\geq 7$ )						Dictatorships (Polity score $\leq 6$ )					
	(1) SOE revenue		(2) Nontax revenue		(3) Pre-1981		(1) SOE revenue		(2) Nontax revenue		(3) Pre-1981	
	LD	MI	LD	MI	LD	MI	LD	MI	LD	MI	LD	MI
SOE Revenue per Capita <sub>t-1</sub>	-0.001 (0.002)	0.000* (0.000)		0.000 (0.000)	-0.001** (0.000)	0.000*** (0.000)	-0.001** (0.000)	0.000*** (0.000)			-0.002 (0.002)	-0.000 (0.000)
GDP per Capita Growth	-0.111** (0.058)	-0.008 (0.018)	-0.115** (0.058)	-0.011 (0.025)	-0.059*** (0.022)	-0.011* (0.007)	-0.059*** (0.021)	-0.011* (0.007)	-0.007 (0.021)	-0.056*** (0.021)	-0.131*** (0.050)	-0.020** (0.008)
GDP per Capita ( $\ln_t - 1$ )	-0.141 (0.322)	-0.395** (0.167)	-0.096 (0.460)	-0.302* (0.181)	0.353** (0.155)	0.291*** (0.103)	0.353** (0.155)	0.291*** (0.103)	0.235** (0.109)	0.172 (0.072)	0.634 (0.399)	0.211* (0.120)
$\Delta$ % Population Urban	0.886* (0.509)	0.326 (0.386)	0.994* (0.535)	0.298 (0.385)	-0.003 (0.294)	-0.500*** (0.244)	-0.003 (0.294)	-0.500*** (0.244)	-0.528** (0.286)	0.072 (0.263)	-1.214* (0.682)	-0.635** (0.265)
Ethnolinguistic Fractionalization	3.263** (1.288)	-0.961 (0.812)	3.346** (1.354)	-0.899 (0.819)	-1.175 (0.864)	-0.268 (0.479)	-1.175 (0.864)	-0.268 (0.479)	-0.263 (0.562)	-0.250 (0.480)	1.252 (1.187)	0.189 (0.586)
Population Density ( $\ln_t - 1$ )	0.247 (0.150)	0.209 (0.165)	0.227 (0.235)	-0.034 (0.168)	-0.151 (0.156)	0.157* (0.089)	-0.032 (0.100)	0.157* (0.089)	-0.069 (0.096)	0.162* (0.096)	-0.024 (0.238)	0.174* (0.090)
Past Regime Instability	-0.150 (0.161)	-1.150*** (0.290)	-0.146 (0.169)	-1.144*** (0.287)	-0.049 (0.387)	-0.082* (0.061)	-0.049 (0.154)	-0.082* (0.154)	0.049 (0.060)	-0.384** (0.155)	0.095 (0.160)	-1.027*** (0.234)
Regime Age	-0.197 (0.210)	-0.049 (0.031)	-0.195 (0.213)	-0.049 (0.031)	-0.049 (0.044)	-0.071*** (0.055)	-0.049 (0.044)	-0.071*** (0.055)	-0.073*** (0.014)	-0.372*** (0.057)	-0.487*** (0.112)	-0.087*** (0.018)
Spine (1)	0.000 (0.001)	-0.000 (0.000)	0.000 (0.001)	-0.000 (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.001*** (0.000)	0.002*** (0.000)	0.000** (0.000)
Spine (2)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	0.000 (0.000)
Nontax Revenue per Capita <sub>t-1</sub>												
Constant	-4.063** (1.828)	3.766** (1.492)	-4.479 (2.868)	3.076* (1.654)	-2.925** (1.373)	-1.531* (0.860)	-2.925** (1.373)	-1.531* (0.860)	-1.492 (1.113)	-1.189 (0.923)	-4.367 (3.224)	-0.665 (0.992)
N	928	2,313	922	2,313	907	5,252	907	5,252	886	5,252	301	2,726

Dependent variable is regime instability (a dummy for whether a regime changes in a given year). Logistic regressions with standard errors clustered by country in parentheses. Replicated results for Model 3 are not provided because there are not enough observations in the original data set to estimate the model. SOE = state-owned enterprise; LD = listwise deletion; MI = multiple imputation.

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

sample by regime type, focusing solely on *Nontax Revenue per Capita* and *SOE Revenue per Capita*. Surprisingly, in the sample restricted to democracies (Models 1 and 2), the coefficients on both variables are nonsignificant under listwise deletion. Under multiple imputation, the coefficient on *SOE Revenue per Capita* becomes *positive* and significant, implying that oil undermines regime stability in democracies. The coefficient on *Nontax Revenue per Capita* remains nonsignificant and negative, however, suggesting that the other two components of the variable do not destabilize democracies.<sup>51</sup> As expected, in the sample restricted to dictatorships (Models 4 and 5), the coefficients on the two variables are negative and significant under listwise deletion. In line with earlier results, these estimates are robust to multiple imputation (although the coefficients become slightly smaller). Critically, as shown in Models 3 and 6, the key findings are still subject to the earlier temporal condition: When the sample is restricted to the pre-1981 period, the coefficient on *SOE Revenue per Capita* is nonsignificant (with the same sign) in both democracies and dictatorships.<sup>52</sup>

To examine the robustness of these findings, I reanalyze the results of two even more recent studies of the impact of oil wealth on regime stability that use alternative measures and data sources. The first is Ross's (2012) important book, "The Oil Curse," which separately analyzes regime change in dictatorships and democracies using a new data set covering 174 countries from 1960 to 2000. The results, displayed in Table 6, are consistent with those reported above: Oil income per capita has a negative and significant effect on the probability of a dictatorship democratizing (Panel 1) and a positive and significant effect on the probability of a democracy failing (Panel 2)—with the key exception of the period 1960-1979.<sup>53</sup> These results are virtually unchanged by multiple imputation, in large part due to the low level of missingness in the sample: Only 8% of data on *Oil Income* are missing, whereas the remaining variables in the analysis have an average missing-data rate of 11%. As a result, listwise deletion omits only 13% of eligible observations and 5% of observed values.<sup>54</sup>

Ross's (2012) explanation for these results is that oil wealth stabilizes *incumbents* rather than regimes, which in the case of democracies involves giving them sufficient resources to dismantle institutional constraints by "buying off" citizens with lower taxes and increased public spending (pp. 75-77).<sup>55</sup> An interesting implication of this argument is that poor democracies will be more prone to failure because their institutions tend to be weaker and their citizens easier to buy off. Indeed, as shown in Panel 1 of Table 6, the coefficient on *Oil Income* ceases to be significant when the sample is restricted to countries with a GDP per capita above 5,000 (Model 3), but remains significant—albeit only at the 10% level—when restricted to countries below

**Table 6.** Ross (2012): Transitions to Democracy, 1960-2006 (Table 3.7 in Original Study); Transitions to Authoritarianism, 1960-2006 (Table 3.9 in Original Study).

	(1) Dependent variable: Transition to democracy (dummy)											
	(2)		(3) 1960-1979		(3) 1980-2006		(5)		(6)			
	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.		
Income (log)	0.332* (0.171)	0.158* (0.084)	0.406 (0.254)	0.262 (0.389)	0.124 (0.134)	0.322 (0.197)	0.256 (0.168)	0.315 (0.194)	0.225 (0.162)	0.340* (0.202)		
Regime Duration (log)	-0.046 (0.181)	-0.214** (0.101)	-0.122 (0.303)	0.151 (0.343)	-0.438*** (0.141)	-0.132 (0.193)	0.01 (0.181)	0.049 (0.152)	0.387* (0.229)	0.506** (0.197)		
Oil Income (log)	-0.179** (0.079)	-0.126*** (0.04)	-0.078 (0.114)	0.074 (0.174)	-0.129** (0.059)	-0.202** (0.091)	-0.292*** (0.092)	-0.432*** (0.111)	-0.197** (0.09)	-0.372*** (0.115)		
Oil Income (log) x Latin America												
Prior Democracy												
Economic Growth												
Muslim Population												
N	3,507	4,239	1,297	1,715	2,210	2,524	3,507	4,239	3,422	4,239		

	(1) Dependent variable: Transition to authoritarianism (dummy)											
	(2)		(3) Below 5,000		(3) Above 5,000		(5)		(6) 1960-2002			
	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.	Rep.	Imp.		
Regime Duration (log)	-0.342** (0.169)	-0.344** (0.164)	-0.240 (0.164)	-0.260 (0.159)	-0.892** (0.452)	-0.786 (0.555)	-0.417* (0.242)	-0.367 (0.237)	-0.280 (0.209)	-0.320 (0.206)		
Income (log)	-0.717*** (0.144)	-0.704*** (0.135)	-0.580*** (0.177)	-0.568*** (0.163)	-2.480*** (0.651)	-1.835*** (0.692)	-0.689*** (0.172)	-0.693*** (0.174)	-0.963*** (0.218)	-0.948*** (0.201)		
Oil Income (log)	0.121** (0.0564)	0.123** (0.056)	0.113* (0.0630)	0.123** (0.061)	0.129 (0.174)	0.133 (0.183)	0.0949 (0.117)	0.098 (0.117)	0.242*** (0.0752)	0.252*** (0.077)		
N	2,673	2,910	1,301	1,421	1,372	1,481	728	764	1,945	2,146		

Explanatory variables are lagged 1 year (with the exception of Prior Democracy). Panel 1 shows logistic regressions with robust standard errors in parentheses. Five-year period dummies not reported. Panel 2 shows rare-event logistic regressions with robust standard errors clustered by country in parentheses. Rep. = Replicated; Imp. = Imputed.  
\*p < .1. \*\*p < .05. \*\*\*p < .01.

this threshold (Model 4). As before, these results are robust to multiple imputation, with the coefficient in the latter model becoming significant at the five% level. Hence, the antidemocratic effects of oil appear to be contingent upon income as well as time period.

The second study is Wright et al.'s (2015) analysis of the effects of oil wealth on autocratic regime survival. Wright, Frantz, and Geddes seek to distinguish between two mechanisms by which oil wealth could enhance the stability of dictatorships: (a) lowering the chances of democratization (as indicated by the above results), and (b) reducing the risk of transition to subsequent dictatorship. They use an innovative empirical strategy that also enables them to separate the between-country effects of oil wealth (i.e., the effects of cross-national variation in average levels of oil wealth) from the within-country effects (i.e., the effects of over-time changes in oil wealth in a single country) by conditioning the marginal effects of covariates on country means for explanatory variables.<sup>56</sup> They apply this strategy to a sample of 280 autocratic regimes in 140 nations from 1945 to 2010, using Haber and Menaldo's data on oil income per capita. The results, displayed in Table 7, indicate that oil income has a negative impact on the overall likelihood of autocratic regime failure (captured by the coefficient on  $\overline{Oil}_{t-1}$ ) but only negative *between-country effects* ( $\overline{Oil}_i$ ) on the likelihood of democratization and *within-country effects* ( $\overline{Oil}_{dev}$ ) on the likelihood of autocratic transition.<sup>57</sup>

Wright, Frantz, and Geddes's data set contains a moderate proportion of missing values: 7% of cells are missing, with analysis variables possessing an average missing-data rate of 11%. Because almost 20% on values on the three dependent variables are missing, however, listwise deletion discards 25% of eligible observations and 20% of observed data, causing 32 countries to be entirely omitted from the analysis.<sup>58</sup> When the results are re-estimated using multiple imputation, oil income continues to have a negative impact on the overall probability of autocratic regime failure, consistent with the reanalyzed results of the previous two studies. Interestingly, however, the within-country effects of oil income become negative and significant in the case of democratic transitions and remain so in the case of autocratic transitions. The between-country effects, meanwhile, become slightly weaker in the former but considerably stronger in the latter, only just exceeding the 10% significance threshold in both cases.<sup>59</sup> Hence, there is fairly strong evidence that oil income has negative within-country *and* between-country effects on the likelihood of *both* democratization and autocratic transition. Once again, however, this finding only holds for the post-1980 period: When the sample is restricted to the Years 1945 to 1980, the coefficients on  $\overline{Oil}_i$  and  $\overline{Oil}_{dev}$  cease to be significant (or close to the 10% threshold) in every model.

**Table 7. Wright, Frantz, and Geddes (2015): Oil Income and Autocratic Regime Survival (Table 1 in Original Study).**

	(1) Full				(4) Full				(6) Full			
	All years		Pre-1981		All years		Pre-1981		All years		Pre-1981	
	LD	MI	LD	MI	LD	MI	LD	MI	LD	MI	LD	MI
<b>(A) All regime failures</b>												
$Oil_{t-1}$	-0.121*** (0.038)	-0.117* (0.064)	-0.076 (0.050)	-0.087 (0.065)	-0.111*** (0.040)	-0.115 (0.073)	-0.057 (0.049)	-0.085 (0.068)	-0.071** (0.035)	-0.104 (0.075)	0.002 (0.049)	-0.068 (0.069)
$\overline{Oil}_t$					-0.166 (0.127)	-0.129* (0.067)	-0.204 (0.143)	-0.100 (0.117)	-0.290** (0.135)	-0.151** (0.071)	-0.307** (0.156)	-0.101 (0.117)
$\bar{y}_t$									14.90*** (1.369)	5.593*** (2.013)	13.859*** (1.352)	3.763** (1.682)
N	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848
<b>(B) Democratic transitions</b>												
$Oil_{t-1}$	-0.153*** (-0.054)	-0.121* (0.068)	-0.171 (0.106)	-0.093 (0.073)	-0.224*** (-0.07)	-0.125 (0.078)	-0.215** (0.108)	-0.097 (0.076)	-0.169*** (0.100)	-0.111 (0.079)	-0.061 (0.100)	-0.077 (0.077)
$\overline{Oil}_t$					0.107 (-0.155)	-0.101 (0.067)	0.158 (0.293)	-0.056 (0.124)	-0.038 (0.186)	-0.118* (0.070)	-0.026 (0.293)	-0.061 (0.125)
$\bar{y}_t$									24.107*** (3.552)	6.225** (2.894)	23.035*** (3.333)	3.958* (2.336)
N	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848
<b>(C) Autocratic transitions</b>												
$Oil_{t-1}$	-0.055 (0.054)	-0.110 (0.067)	-0.043 (0.067)	-0.081 (0.068)	-0.014 (0.054)	-0.107 (0.076)	0.001 (0.068)	-0.078 (0.071)	-0.023 (0.049)	-0.108 (0.078)	-0.006 (0.066)	-0.080 (0.072)
$\overline{Oil}_t$					-0.280** (0.141)	-0.135** (0.065)	-0.347** (0.144)	-0.106 (0.114)	-0.138** (0.145)	-0.138** (0.065)	-0.355** (0.155)	-0.101 (0.114)
$\bar{y}_t$									19.877*** (2.708)	4.260* (2.564)	19.285*** (2.777)	3.715 (2.369)
N	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848	4,138	5,675	2,167	2,848

Dependent variable is the likelihood of regime failure in Panel A, transition to democracy in Panel B, and transition to autocracy in Panel C. Logistic regressions with standard errors clustered by country in parentheses. Time dependence polynomials, calendar time polynomials, and control variables (GDP per Capita, Civil War, Neighbor Democracy) included in all models but not reported. LD = listwise deletion; MI = multiple imputation.

\*p < .1. \*\*p < .05. \*\*\*p < .01.



In sum, multiple imputation does not support the claim that oil wealth enhances regime stability in democracies as well as dictatorships. Nor does it support studies that have found no relationship between oil wealth and democratic stability (Al-Ubaydli, 2012; Caselli & Tesei, 2016; Ulfelder, 2007; Wiens, Poast, & Clark, 2014). Rather, it suggests that oil wealth has antidemocratic effects, reducing the likelihood of regime change in dictatorships—both within and across countries—and increasing it in democracies. However, consistent with the findings of the previous subsection, such effects only occur in the post-1980 period (and may also be restricted to poor democracies).

## Conclusion

Given the methodological sophistication of the ongoing debate over the political resource curse, it is puzzling that scholars have paid such little attention to the basic statistical issue of how to deal with missing data. This article has highlighted the methodological problems associated with the most common strategy for analyzing missing values in the political resource curse literature—listwise deletion—and shown that addressing these problems through the use of multiple imputation causes the results of a number of notable recent studies to converge on a key common finding: A political resource curse *does* exist, but only since the wave of petroleum industry nationalizations in the 1970s, which provided governments with greater access to the rents from oil production. This striking finding suggests that much of the disagreement over the political resource curse has been caused by a neglect of missing-data issues.

The finding is significant from a theoretical perspective, strengthening claims by Ross, Andersen, and others (e.g., Boschini, Pettersson, & Roine, 2012; Luong & Weinthal, 2010; Snyder & Bhavnani, 2005) that the capacity of states to capture oil rents—a capacity that has varied considerably over time—is a critical determinant of whether oil wealth adversely affects democracy. Such arguments, in turn, contribute to an important broader research agenda that seeks to better understand the scope conditions of the political resource curse (for an overview, see Ross, 2015). This line of research has mostly focused on the conditioning effects of a state's initial regime type, which, as shown in the third section, are substantially weaker than commonly believed. Developing integrated theories of the political resource curse that can reconcile and illuminate different findings regarding scope conditions—such as Ross's (2012) recent theory of incumbent empowerment (see third section)—is an essential task for future research.

The results of the reexamination also have important implications for methodological practices in the political resource curse literature. Beyond the obvious implication that statistical analyses of this issue should adopt a more

sophisticated approach to handling incomplete data, they suggest that scholars should generally be more explicit about how many missing values their data sets contain, how they deal with such values, and what consequences this choice may have for the validity of their inferences. The present situation is somewhat perverse: The studies that are least likely to provide such information are those using missing-data methods that require the most justification. The statistical and substantive importance of the changes brought about by multiple imputation suggests that the gains from adopting such practices could be considerable.

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### **Notes**

1. For instance, 27 of the 29 empirical studies included in Ahmadov's (2013) recent meta-analysis of the political resource curse use listwise deletion as their primary missing-data method. The remaining two studies use another ad-hoc and typically biased technique (linear interpolation in one case and a combination of linear interpolation, mean substitution, and zero imputation in the other). Worryingly, more than half of the studies—all of which use listwise deletion—are not explicit about how they deal with missing data.
2. According to van Buuren (2012), multiple imputation is "now accepted as the best general method to deal with incomplete data in many fields" (p. 25). For statistics on the rapid growth of the applied literature on multiple imputation in recent decades, see pp. 27-28.
3. As discussed in the third section, these debates emerged in response to Ross's (2001) classic study, "Does Oil Hinder Democracy?" which presented the first cross-national statistical analysis of the political resource curse. I begin the reexamination with this study, finding that multiple imputation undermines its central finding that oil wealth (measured as fuel exports as a fraction of GDP) is negatively associated with levels of democracy.

4. In the political science literature, the term “endogeneity” is sometimes used to refer more narrowly to reverse causation between the dependent and explanatory variables. I use the term in the traditional econometric sense, that is, to refer to situations in which the explanatory variable is correlated with the error term (of which reverse causation is merely one instance).
5. Efforts to address the problem of missing data have mostly focused on compiling new data sources, triangulating between existing ones, and identifying alternative treatments. Although such efforts are highly valuable, they must be accompanied by the adoption of modern missing-data methods (such as multiple imputation) to avoid the statistical problems described in this section.
6. More formally, if  $Z$  denotes a data set with an observed portion  $Z_{obs}$  and a missing portion  $Z_{mis}$ ,  $M$  denotes a matrix of the same dimensions as  $Z$  in which cells have a value of 1 if missing and 0 otherwise, and  $\varphi$  denotes parameters from the joint distribution function of  $Z$ , missing completely at random (MCAR) can be expressed as  $p(M|Z_{obs}, Z_{obs}) = p(M|\varphi)$ ; missing at random (MAR) as  $p(M|Z_{obs}, Z_{mis}) = p(M|Z_{obs}, \varphi)$ ; and missing not at random (MNAR) as  $p(M|Z_{obs}, Z_{mis}) = p(M|Z_{obs}, Z_{mis}, \varphi)$ .
7. Other possible determinants of missingness include a state’s size, economic system, bureaucratic capacity, investment inflows from foreign oil companies, and integration into global energy markets. The importance of these factors may vary by time period, making the latter another potential determinant. For instance, levels of missing data are likely to be higher for communist states during the Cold War, newly independent states in the 1960s, and almost all oil producers before the emergence of global energy markets in the 1990s.
8. Interestingly, Ross (2006) argues that high-income autocracies are less likely to release such information than low-income ones because they rely less on the financial support of international organizations such as the International Monetary Fund and the World Bank, which demand data in return for assistance. For evidence to the contrary, however, see Lall (2017).
9. Similar claims have been made about democratic norms of peaceful conflict resolution and compromise in the international relations literature on the democratic peace.
10. Multiple imputation was first proposed by the statistician Donald Rubin in the late 1970s and further developed with collaborators over the next decade (Little & Rubin, 1987; Rubin, 1976, 1977, 1987; Rubin & Schenker, 1986).
11. Although real data obviously do not always conform to multivariate normality, this model has been found to perform well even in the presence of violations (Rubin & Schenker, 1986; Schafer, 1997).
12. That is, for a given quantity of interest  $\beta$  (say, a regression coefficient),
 
$$\hat{\beta} = \frac{1}{m} \sum_{i=1}^m \hat{\beta}_i \quad \text{and} \quad \text{var}(\hat{\beta}) = W + \left(1 + \frac{1}{m}\right)B, \quad \text{where} \quad W = \frac{1}{m} \sum_{i=1}^m \text{var}(\hat{\beta}_i) \quad \text{and} \\ B = \frac{1}{m-1} \sum_{i=1}^m (\hat{\beta}_i - \hat{\beta})^2.$$

13. They also frequently produce biased point estimates (for different reasons in each case; Little & Rubin, 2002).
14. As Graham (2009) argues,

Because all missingness is MNAR (i.e., not purely MAR), then whether it is MNAR or not should never be the issue. Rather than focusing on whether [multiple imputation's] assumptions are violated, we should answer the question of whether the violation is big enough to matter to any practical extent. (p. 567)

15. If the analysis model is a (correctly specified) regression of  $Y$  on  $X$ , data for  $X$  are MNAR, and missingness does not depend on  $Y$ , it is possible for listwise deletion to be less biased than multiple imputation. These conditions, however, are rarely satisfied in the real world (King, Honaker, Joseph, & Scheve, 2001; van Buuren, 2012).
16. Note that such variables are also correlated with the other potential determinants of missingness mentioned in Note 7. (The time-series index in the data set—normally *Year*—will capture temporal variation in the importance of these determinants and in missingness more generally.) Hence, multiple imputation will typically be capable of addressing a diverse range of selection problems.
17. The studies have a combined citation count of more than 4,000 on Google Scholar (search performed May 27, 2016). Three of the studies published after 2008—Haber and Menaldo (2011), Morrison (2009), and Ross (2012)—already have more than 300 citations each.
18. One obvious candidate for inclusion is Tsui's (2011) study of the impact of oil discoveries on long-term democratic development, which, together with Ramsay (2011), pioneered the use of instrumental variables analysis in the literature. This study is excluded because it uses multiyear averages of time-series variables—an ad-hoc and typically biased missing-data strategy (see previous section)—and thus contains a purely cross-sectional sample of 158 observations with almost no missing values.
19. Three of the data sets were obtained from online data repositories (Morrison, 2009; Ross, 2001, 2012), one from an author's personal website (Haber & Menaldo, 2011), and two through personal communications with authors (Ramsay, 2011; Wright, Frantz, & Geddes, 2015).
20. This is stark evidence of the inefficiency caused by listwise deletion. By preserving information in incomplete observations, multiple imputation utilizes an average of 93% more observed values in the data sets.
21. As shown in the sixth column, the proportion of eligible years excluded is fairly low, but still exceeds 15% in two cases.
22. I excluded studies on the political resource curse (of which there were two).
23. Fifty-six of the data sets were obtained from online repositories (in most cases, the Harvard Dataverse Network) and 54 from personal websites.
24. A small number of studies use multiple data sets. In such cases, I averaged these statistics across all data sets.

25. See Little (1988) for further details. The test was implemented using the *mcartest* command in Stata (Version 13.1), which, in some instances, required removing highly collinear variables from the data set.
26. If the samples vary across estimations models in the main analysis, I take an average of the different sample means. Ross (2012) and Wright et al. (2015) are omitted from Online Appendix Table A2 because their analyses are restricted to either democracies or dictatorships.
27. I separately calculate the gaps for each estimation model in the main analysis and combine the *p*-values using Fisher's method.
28. Ross (2012) and Wright et al. (2015) are again excluded for the reason mentioned in Note 26.
29. The *m* complete data sets generated by *Amelia II* are imported into Stata (Version 13.1), where analysis results are aggregated using the built-in *mi* command.
30. Similar rules have been proposed by Schafer (1997); White, Royston, and Wood (2011). The 100-variable threshold excludes the following four types of auxiliary variables, which are automatically omitted because they provide no extra information: (a) nonessential index variables, (b) items of composite variables, (c) dummies based on other variables, and (d) variables measuring data parameters such as means and variances.
31. This is because such values are typically not normally distributed around the cutoff point (for instance, 0.5 in the case of binary variables; Allison, 2005; Cranmer & Gill, 2013; Horton, Lipsitz, & Parzen, 2003).
32. Transforming skewed variables alters their relationship with other variables in the imputation model; in effect, it is equivalent to assuming that they have zero correlation with such variables (von Hippel, 2013).
33. A similar rule is suggested by van Buuren (2012).
34. The study has more than 2,500 Google Scholar citations and is listed as the most cited research article ever published in *World Politics* on the journal's official website (both searches performed May 27, 2016).
35. The replicated results differ marginally from the published results because the author made corrections to the data set before archiving it on the Harvard Dataverse Network.
36. Other innovative recent studies have sought to address the problem of reverse causation by conducting field experiments (de la Cuesta, Milner, Nielson, & Knack, 2015; Paler, 2013) and exploiting one-off exogenous shocks to oil income (such as the 1973 oil price spike; Liou & Musgrave, 2013). Alternative strategies for dealing with omitted variable bias include the use of extreme bounds analysis (Gassebner, Lamla, & Vreeland, 2013); synthetic controls (Liou & Musgrave, 2013); and, as discussed below, country fixed effects models.
37. As mentioned in Note 18, Tsui (2011) used an instrumental variables approach to examine the political resource curse at a similar time to Ramsay, using estimates of oil endowments as an instrument for oil discoveries (the key determinant of oil wealth). For more recent applications of this approach, see Cotet and Tsui (2013); Kennedy and Tiede (2013).

38. The model also includes time fixed effects, a lagged measure of Polity (for error correction), and a number of control variables. Driscoll–Kraay standard errors are used to account for spatial heterogeneity.
39. This relationship is statistically significant in the case of oil income per capita. For a similar finding, see Gurses (2009).
40. Aslaksen (2010) also finds support for the resource curse hypothesis in the presence of country fixed effects.
41. The Seven Sisters were Anglo-Persian Oil Company (now BP), Gulf Oil, Mobil (now ExxonMobil) Royal Dutch Shell, Standard Oil of California (now Chevron), Standard Oil of New Jersey (also now ExxonMobil), and Texaco (now a subsidiary of Chevron).
42. Figure 2 plots the distribution of total fuel income per capita, which contains elements of both *Total Oil Income* and *Fiscal Reliance*.
43. I do not focus on Haber and Menaldo's longitudinal graphical analysis (see pp. 5-11) because they stress that it is merely suggestive, not providing the basis for causal inferences (as it does not control for any potentially confounding factors).
44. Like Andersen and Ross, I place less weight on the *Fiscal Reliance* results because the data for this variable only cover 18 countries (and are thus almost entirely imputed in the reanalysis).
45. Specifically, they enable democracies to lower taxes on wealthy elites and dictatorships to increase spending on poor citizens.
46. The other two components are foreign aid and "other" nontax revenue, which mainly consists of government borrowing. The models also include a number of control variables.
47. Similar findings about oil revenues (rather than nontax revenues more broadly) have been reported by Dunning (2008); Smith (2004); and Tsui (2011).
48. These countries make up almost one third of the excluded observations. Stable autocracies, by contrast, account for just 8% (and oil-producing ones 6%).
49. Figure A1 in the online appendix indicates that levels of nontax revenue are also higher in excluded observations than included ones.
50. The coefficients on the other two components of nontax revenue, whose values are also higher in excluded observations than included ones, experience similar changes.
51. This difference is theoretically puzzling. One possible explanation is that it is easier to use oil and other state-owned enterprise (SOE) revenues to pursue anti-democratic ends because—unlike foreign aid and government borrowing—they are purely domestic and thus come with no international scrutiny or pressure.
52. Model 3 only displays the results based on multiple imputation because there were not enough observations in the original data set to estimate the model.
53. I exclude Model 1 from the original tables, which only includes control variables.
54. I do not compare levels of democracy and oil income in included and excluded observations because the sample is restricted to either dictatorships or democracies.
55. Similarly, a number of subnational studies have found that oil windfalls have pro-incumbent effects (Gervasoni, 2010; Goldberg, Wibbels, & Mvukiyeye, 2008; Mahdavi, 2015).

56. Most previous research solely examines between-country effects. A notable exception is Haber and Menaldo's study, which examines within-country effects in both dictatorships and democracies, although only in a subsidiary analysis whose results are reported in an online appendix. Wright, Frantz, and Geddes argue that Haber and Menaldo's choice of statistical model results in the exclusion of a large number of stable autocracies from the analysis and thus produces downward-biased estimates (a very similar selection problem to the one highlighted earlier).
57. I omit Models 2, 3, and 5 from the original table, which are restricted to autocracies that experience regime change (for the purpose of examining how excluding stable autocracies—like Haber and Menaldo's within-country analysis—affects the results).
58. As with Ross (2012), I do not compare levels of democracy and oil income between included and excluded observations because the sample is restricted to dictatorships.
59. The average  $p$ -value of the coefficients on  $\overline{Oil}_i$  is .13 in the case of democratic transitions (Panel B) and .16 in the case of autocratic transitions (Panel C).

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