No Representation without Taxation?

Rents, Development, and Democracy

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Natural resource windfalls would seem to be a boon for poor countries. They reduce the burden of taxation, create a middle class, and pay for social services. In the view of most scholars, however, natural resource bounties are more a curse than a blessing. Studies of rentier states argue that democracy is unlikely or even impossible without taxation, which had a crucial role in the emergence of representation and democracy in European history. Skeptics, however, argue that the negative effects are exaggerated and that the lack of democracy in the Middle East and elsewhere can be traced to other factors. This debate is important because many natural resource exporters are found in the developing world. And the theory speaks directly to a pressing issue: why is the Middle East so resistant to democratization.

While the effects of natural resource wealth on democracy has been the subject of much discussion, only recently has Michael Ross, in a pathbreaking and long overdue contribution, tested the thesis using a large-n, cross-regional dataset. Ross finds that "the oil-impedes-democracy claim is both valid and statistically robust, ... oil does hurt democracy." This conclusion helps to "vindicate" the rentier state theory.[1] This article revisits this issue and comes to a more ambivalent conclusion. The results differ, first, because a new dataset that directly measures rentierism is used. Second, different methods are used to analyze the data. Third, a different tack is taken in dealing with a phenomenon little discussed in studies of natural resources and democracy: in richer rentier states, rents create a larger middle class, pay for schoolteachers, increase per capita GDP, and drive up other measures of development. As Lipset observed, development is correlated with democracy. But it is far from clear that rent-induced development has a positive effect on democracy, what magnitude it might be, and to what degree it is counterbalanced by the negative consequences of rentierism. The issue has received scant attention in studies of the rentier state (though it is engaged in Ross's article and in a study of Congo by John Clark).[2] The issue is important not only in analysis of the causal mechanisms underlying the rentier state theory, but also in a multivariate test of the theory, where different ways of dealing with this issue have an important impact on the results.

The Rentier State Theory

A growing body of work is concerned with the political, economic, and social consequences of dependence on natural resource exports. Economic consequences include the frustration of economic growth through the "Dutch disease" and other mechanisms.[3] Political consequences include characteristic patterns of state formation, the prevalence of corruption, and the lack of democracy.[4] Of these, this article tests the most prominent political claim, that rents harm a country's chances of being democratic.

Rentierism and natural resource dependency are not the same thing, though in practice they are highly correlated. Natural resource dependency is measured as the share of natural resource exports as a percentage of GDP. Rentierism, by contrast, is measured by the percentage of rents in government revenues. Beblawi's definition of rents is widely followed. Rents come from abroad and accrue to the government directly, and "only a few are engaged in the generation of this rent (wealth), the majority being only involved in the distribution or utilisation of it."[5] The third point requires some elaboration. Not only do a few people produce the wealth, but the wealth is the result of a windfall that is very largely independent of any efforts made by citizens of the rentier state.[6] Table 1 lists the world's rentier states in the period from 1972 to 1999.

The term "rent" appears, with a different meaning, in studies that deal with the consequences of rent-seeking by influential actors in the domestic political economy. To heighten the potential for confusion, rent-seeking is sometimes identified as an attribute of rentier states. I do not deal here with rent-seeking.

The argument that rentierism harms democracy has been advanced in case studies and theoretically by, among many others, Lisa Anderson, Jill Crystal, Dirk Vandewalle, and Giacomo Luciani.[7] The causal mechanisms underlying the argument that rentierism harms democracy are of three main sorts, concerning state revenues, state expenditures, and society. First, rentier states need not tax (or need not tax much). Freedom from levying taxes "release[s] the state from the accountability ordinarily exacted by domestic appropriation of surplus....[T]he state may be virtually completely autonomous from its society, winning popular acquiescence through distribution rather than support through taxation and representation."[8] Second, rentierism increases the capacity of the state to both buy off and to repress opposition. These two mechanisms, together, often are thought to produce a "rentier social contract" in which "the state provides goods and services to society...while society provides state officials with a degree of autonomy in decision-making."[9] Third, oil revenues change the class structure of society. Democracy is stymied when oil revenues prevent changes in class structure that usually lead to democracy.[10]

Critics have registered several objections to the argument that rentierism obstructs accountability. Okruhlik points out that in Saudi Arabia the regime's efforts to buy off dissent failed, generating more of it.[11] Second, the venerable tie between taxation

Table 1 Rentier States, 1972–1999

		Rentier period	Average degree of rentierism in rentier period	Per capita GDP 1990	Democratic intervals
Middle East	Kuwait	Entire period	88%	13,114	
	Qatar	Entire period	87%	16,986	
	UAE	Entire period	84%	19,648	
	Oman	Entire period	81%	7,879	
	Saudi Arabia	Entire period	80%	7,174	
	Bahrain	Entire period	59%	8,879	
	Libya	Entire period	58%	n/a (middle income)	
	Iraq	Entire period	n/a	3,205	
	Iran	Entire period	55%	3,392	
	Algeria	Entire period	53%	2,777	
	Yemen	From 1990	46%	1,979	
Sub-Saharan Africa	Nigeria	Entire period	71%	995	1979-1983
	Botswana	From 1983	64%	2,284	Entire period
	Angola	From independence	62%	678	
	Equatorial Guinea	From 1996	58%	n/a (poor)	
	Congo (Brazzaville)	From 1980	57%	2,211	1992-3
	Gabon	Entire period	50%	3,958	
	Guinea	1986-91	50%	767	
Latin America	Venezuela	Entire period	61%	6,055	Entire period
	Trinidad & Tobago	1974-1984	53%	7,764	Entire period
	Ecuador	1982-1993	47%	2,755	1979-
	Bolivia	1988-1991	43%	1,658	1982-
S.E. Asia	Brunei	Entire period	n/a (extreme rentierism)	n/a (rich)	
	Indonesia	To 1989	49%	1,974	
Pacific	Nauru	Entire period	n/a (extreme rentierism)	n/a (middle income)	Entire period
	Kiribati	1995-1998	47%	n/a (poor)	Entire period

A rentier period is one in which rent typically makes up at least 40% of government revenues (see Luciani, p. 72). Per capita GDP is in constant 1985 $US (Iraq data is for 1987). Data on democracy is from Michael Bernhard, Timothy Nordstrom & Christopher Reenock ("Economic Performance, Institutional Intermediation, and Democratic Survival," *Journal of Politics*, 63 [August 2001], 775-803) and Freedom House.

and representation in European history may provide far less support for the rentier state theory than is often supposed.[12] Third, it has been suggested that citizens have many reasons to want to hold their rulers accountable, even if they are not taxed.[13] Finally, some have raised doubts concerning the empirical robustness of the theory, especially because there are rentier democracies outside the Middle East.[14]

Lipset's Thesis and the Rentier State Theory

In 1959 Seymour Martin Lipset argued that development is correlated with democracy, and his thesis has proven to be one of the more durable empirical findings of comparative politics.[15] It has important implications for the argument that rents pre-

vent democracy, because rentierism often leads to a sharp increase in metrics of development: per capita GDP rises, as do levels of education, urbanization, and energy usage. It is possible, however, that the "development" caused by rents has a much weaker effect on democracy than development caused by other sources of wealth.

Ross assumes that oil wealth has the same positive effect on democracy as other sorts of wealth.[16] For example, oil wealth has the same positive impact on democracy in Kuwait that other sorts of wealth have on democracy in Canada. Kuwait is authoritarian because other factors, including the separate negative effects of rentierism, hammer Kuwait's democracy scores back down. He measures the two countervailing effects and concludes that the negative effect dominates. Yet, if oil wealth does not push Kuwait's democracy scores up so far, then perhaps rentierism does not have such a large effect in pushing democracy scores back down. In the real world, Kuwait's wealth should be compared to that of Canada only if Kuwait's wealth has the same effect on the likelihood that it will be democratic. If Kuwait's wealth is less potent in this regard, Kuwait should be compared (in terms of wealth) to Jordan, Yemen, or Djibouti.

Throughout studies of rentier states and, more widely, of democratization, there is no explicit defense of the idea that rent wealth has the same democracy-promoting effects as wealth generated in a productive economy. Huntington's view is typical. He writes that "broad-based economic development involving significant industrialization may contribute to democratization but wealth resulting from the sale of oil (and, probably, other natural resources) does not."[17] Inglehart, similarly, writes that "only so far as [wealth] brings appropriate changes in social structure and political culture does it enhance the viability of democratic institutions.…[S]uch nations as Saudi Arabia, Kuwait and [Libya] are quite wealthy, but neither their social structures nor their political cultures seem favorable to democracy."[18] Lipset himself offers a similar view.[19]

This argument perhaps goes too far. Rent wealth induces something that looks like development, in some respects at least (see Table 2). At least one case study suggests that oil wealth has some positive effects. John Clark in his study of Congo (Brazzaville) argues that oil wealth created a white collar middle class and that this class contributed to a brief bout of democratization there. However, Clark also notes oil's negative effects, weighs the two, and concludes that, "[o]n the whole, [Congo's] oil wealth slightly increases its chances of becoming democratic."[20] In short, it seems unlikely that oil-induced development has no positive effects on democracy but also implausible that it has the full positive effects of other sorts of wealth.

Measuring the Net Effect of Rent Wealth on Democracy

These issues are manifested in complex ways in a regression model. Ross, as is standard, uses per capita GDP to control for the level of development. To find the net impact

Table 2 Some Indices of Development

	High income OECD, or U.S.	Kuwait	Saudi Arabia	Egypt
Per capita GDP (1995, PPP, US$)	23,370	22,050	11,340	2,950
Adult female literacy, 1997	98.6	77.4	62.9	40.7
Daily newspapers per 1,000 inhabitants, 1996	*212*	377	59	38
Scientific and technical journal articles per 100,000 residents, 1997	50.5	9.6	3.2	1.8
% of 1990 female labor force employed in agriculture:	4.6	0.5	12.0	52.0
in industry:	18.0	2.0	6.1	10.2
in services	77.2	97.6	81.9	37.5
Infant mortality rate (per 1,000 live births), 1997	6.5	14	26	66.1

Sources: World Bank *World Development Indicators* CD-ROM, 2001; UNESCO (for female literacy figures for richer countries). United States figures in italics.

of oil on democracy, he determines the impact of an oil windfall on two independent variables, per capita GDP and oil export dependence. Working from the coefficients of the two measures, he then calculates the net effect, which is negative.[21] Yet the per capita GDP measure does not distinguish between oil wealth and other sorts of wealth and thus assumes that all types of wealth have the same effect on democracy scores. Kuwait is compared to Canada in terms of the positive effects of wealth on democracy. Since Kuwait is much less democratic than Canada, much of Kuwait's authoritarianism remains underexplained. As the analysis below suggests, the statistical significance of the oil export dependence variable is therefore exaggerated.

This mixture of two sorts of wealth in the per capita GDP variable poses thorny problems. Two solutions suggest themselves but ultimately do not work. First, a different measure of development, one that is not affected by rent wealth, could be used. But in a country like Kuwait everything that is measurable and might serve as a proxy for development, from per capita electricity consumption, to education levels, to literacy, has been affected by oil.[22] There are no statistics that could serve as a measure of development that have not been confounded by oil wealth.

A second approach, while also initially attractive, also does not work. Rent or oil wealth can not be subtracted from existing per capita GDP figures, thus "unmixing" the two types of wealth. The effect of oil (or other rents) on the economies of the richer rentier states is transformative, not additive. The non-oil economy that Kuwait might have had without oil is no longer there. Oil destroyed it.

The best approach to the problem comes from recalling the counterfactual argument at the heart of the rentier state theory. If the gods of geology had not seen fit to put oil under the sands of Kuwait, it would be more democratic than in fact it is. This statement would hold true, on average, for all rentier states. Thus, this article posits a counterfactual world in which rentier states lack rentier wealth. Their per capita GDP figures in this counterfactual world are estimated, and standard per capita GDP figures are replaced with these counterfactual figures. This procedure gets directly to the counterfactual argument at the heart of the rentier state theory. If this measure is used in place of standard per capita GDP, and if the measure of rentierism continues to be negative and statistically significant, then rentierism has a harmful net impact on democracy scores. Of course, it is also possible that the rentierism variable would have a positive impact on democracy scores. With the use of counterfactual income, the rentierism variable is now the only measure of the effects of rent wealth. As pointed out above, there are theoretical reasons to believe that these effects could be simultaneously positive and negative.[23]

Counterfactual GDP figures were derived by comparing rentier states to otherwise similar countries that lack abundant rents. No attempt was made to adjust per capita GDP for poor countries such as Nigeria or Angola. Comparison countries were selected from the same region, usually neighbors, since region is the most powerful predictor of democracy scores. The per capita GDP figures for these countries were averaged for each year, and standard per capita GDP figures for rentier states were replaced with these figures.[24] For the crucial Gulf Arab monarchies, an average of the per capita GDP of Jordan, Egypt, and Yemen was used. The Gulf states, before oil, were far less developed than Jordan and especially Egypt. All three countries' per capita GDP figures are inflated by oil-driven economic growth in other parts of the Arab world, and Yemen exports some oil, providing a conservative bias. Another reasonable counterfactual example would be Yemen alone. For sub-Saharan African countries, a regional average of nonrentier states was used. Elsewhere, neighbors were used. Iran's per capita GDP was set to an average of Turkey (an optimistic counterfactual example) and Pakistan (a more likely counterfactual).[25] Any possible calculation of counterfactual GDP requires major, perhaps heroic, assumptions. My defense is straightforward: without counterfactual GDP the hypothesis that rents harm democracy can not be tested in a multivariate model. This hypothesis is worth testing. It informs explanations of the problems of democratization in important parts of the world.

Poverty, Democracy, and Rentierism

Before proceeding to the regression analysis, it is useful to emphasize a second insight concerning the rentier state theory that emerges from a focus on Lipset's thesis. A brief survey of rentier states (Table 1) shows that most are authoritarian. This conclusion should not be surprising. The standard definition of rentierism ensures that rentier states are drawn from among states more likely to be authoritarian because they are more likely to be poor (but for rent wealth).

Rentierism can be measured by rent revenue as a percentage of total government revenues.[26]

$$\text{Rentierism} = \frac{\text{Rent revenues}}{\text{All other revenues} + \text{Rent revenues}} \quad (1)$$

The equation used to measure natural resource dependency is similar.[27]

$$\text{Oil export dependence} = \frac{\text{Net oil exports}}{\text{GDP}} \quad (2)$$

The denominator is crucial. The effect of a rent windfall on the standard measure of rentierism is mostly a function of a country's level of development. In a poor country a relatively minor sum of rent can dominate government revenues. In 1996 Angola's government received a modest $127 per capita in oil rents, but these results constituted 86 percent of government revenues. In the same year, by contrast, the Norwegian government received, per capita, nineteen times more oil revenue than Angola, but this oil wealth amounted to only 13 percent of government revenues.[28] It is virtually impossible that a rich, productive country of any substantial size could become a rentier (see Table 1). In a sense, poverty causes rentierism.

Nevertheless, the direction of causation might be reversed. It is often argued that dependence on natural resource exports impedes economic growth.[29] Thus, there may be a vicious circle at work. Poor economies are more likely to be dominated by a single natural resource, and this dominance in turns erodes the remaining economy, further increasing the economic importance of the resource. As the nonresource part of the economy shrinks, the prospects for democracy decline also. On balance, however, it is likely that poverty does more to cause rentierism than the other way around. Most rentiers are in parts of the world where economic growth in nonrentier states has been anemic. Further, in regions where economic growth is common, such as Southeast Asia, some rentiers have escaped rentierism.[30] The "natural resource trap" may really be an African and Middle Eastern trap.

Data

The hypothesis that rents harm democracy can be tested with the following model.

$$\begin{aligned}\text{Democracy Score}_{i,t} = {} & a_1 + b_1(\text{Democracy Score}_{i,t-1}) \\ & + b_2(\text{Rentierism}_{i,t-1}) \\ & + b_3(\text{natural log of counterfactual per capita GDP}_{i,t-1}) \\ & + b_4(\text{Muslim share of population}_i) \\ & + b_5(\text{Mean democracy score for other countries in region}_{i,t-1})\end{aligned} \quad (3)$$

The dataset includes all sovereign states with available data in each year from 1972 through 2000. With a one year lag, this dataset yields 4,746 possible observations. Data for democracy scores, the Muslim share of the population, and the regional democracy score means are complete for all sovereign states in this period. After accounting for missing data in the measures of rentierism and income, there are at most 3,332 observations in 144 countries over twenty-eight years.

The dependent variable is Freedom House's democracy score. The data series starts in 1972 and covers the universe of sovereign states. Following convention, the Freedom House scores for Political Freedom and Civil Liberties were combined to yield a thirteen point scale from zero to twelve. The scale was reversed so that higher scores are more democratic.

No existing datasets directly measure rentierism, so a number of readily available sources were consulted to construct one, guided by Beblawi's definition of rentierism (see the Appendix for details).[31] Grant revenue was not counted as rent.[32] The underlying idea of the rentier state theory is that power follows money and that rent wealth releases rentier rulers from accountability to their people. Those who buy the oil sold by rentier rulers rarely constrain how these rulers spend their profits. Grants are different. Former American possessions in the Pacific, for example, could expect less American generosity if they ceased to be democratic. Second, much grant income is earmarked to a greater or lesser degree and can not be spent at the ruler's discretion. It thus provides only some (hard to measure) fraction of the benefit of natural resource rents. These theoretical issues create difficult data collection problems as well. Since most donors are democracies, including grants would probably introduce a bias against finding significance for the measure of rentierism. At a minimum, the analysis would need to be done both with and without grants. The opposite is not true. Without grant income the analysis hews closely to the original intuition of the rentier state theory, and confounding issues are avoided.

Results were checked with a second variable, net oil exports as a percentage of GDP (oil export dependence, henceforth). This variable is Ross's oil variable, somewhat modified.[33] It has the important virtue of getting at a very closely related issue through an entirely different collection of data. As it turns out, rentierism and depen-

dence on oil exports are highly correlated, with an adjusted R-squared of .82. The IMF's figures for nontax revenue as a percentage of current revenues were not used, as these data are a poor measure of rentierism.[34]

Per capita GDP figures are drawn from the Penn World Tables 5.6 and are extrapolated forward into the 1990s (and occasionally backward in time) using World Bank data from the 2001 World Development Indicators CD-ROM. Figures are in constant 1985 U.S. dollars, using purchasing power parity. Richer rentiers' per capita GDP figures were set to the means of nonrentier neighbors or nonrentier regional averages, as described above. This procedure was done yearly.

Previous studies of democracy have found that regional dummy variables or other controls for region are significantly correlated with democracy scores.[35] Gasiorowski was followed in constructing a measure of regional influences.[36] For each country in each year, the mean democracy score of all other countries in its region in that year was calculated. This measure avoids a proliferation of dummy variables, reflects changes over time, and includes information on countries otherwise excluded from the regressions because of missing data. Regional averages were calculated using six regions: the Middle East, Europe and English-speaking North America, Latin America and the Caribbean, South Asia, East Asia, and Oceania.[37]

The percentage of Muslims in countries' populations changes little over time, and 1990 figures were used in most cases.[38] Previous studies have found Islam to be correlated with democracy. While I do not think that there is an immutable authoritarian or democratic "essence" to Islam, the ideological and cultural currents that are common to the Islamic world may well have an effect on democracy scores.[39]

Many rentiers (Gabon, Qatar) have small populations. Their size is not at all irrelevant to how they became rentiers in the first place, since a given amount of rent wealth produces more rentierism when divided by fewer people. If there is ever a justification for dropping the world's smallest countries from statistical analyses, it is an especially hard argument to make in a study of rentierism. While there are several democratic rentiers among the micro-states of the Pacific (see Table 1), data on their economies are scarce, and they are not in the dataset used for the regressions, with the exception of a number of observations for Kiribati.

Methods and Results

Beck and Katz propose the use of OLS (ordinary least squares) with PCSEs (panel-corrected standard errors) and a lagged dependent variable (LDV) to deal with the various problems created by the pooling of data for countries in TSCS (time-series cross-sectional) datasets. My basic model (Equation 3) and analysis follow their lead. Beck and Katz argue that TSCS data suffer from three problems: autocorrelation, panel heteroscedasticity, and contemporary correlation.[40] PCSEs deal with the

problems of panel heteroscedasticity and contemporary correlation. A test for panel heteroscedasticity in the pooled data was positive, as would be expected, suggesting that use of PCSEs is appropriate. The LDV deals with autocorrelation.[41]

Table 3 reports the results of the basic model (Equation 3) and three variations.[42] Rentierism is not statistically significant, nor is oil export dependence when used in place of rentierism. The coefficients for the rentierism and oil export dependence variables increase sharply when counterfactual per capita GDP is replaced with standard per capita GDP, and the oil export dependence variable achieves significance.[43] The coefficients for counterfactual income are larger than those for standard income, suggesting that removing rent wealth improves the fit of the measure of development. The expected change in democracy scores resulting from a standard deviation change in the counterfactual income and region variables are, respectively, about four and one-half and five and one-half times the expected change resulting from a standard deviation change in the rentierism variable in the first year.[44] The model suggests, for example, that the democracy score of Kuwait without oil (past or present) would be a mere .086 points below its current value on the thirteen point democracy scale.

Table 3 Results of the Basic Model (Equation 3)

	Rentierism / counterfactual per capita GDP	Rentierism / per capita GDP	Oil / counterfactual per capita GDP	Oil / per capita GDP
Democracy Score (LDV)	**.927 (.015)** **0.000**	**.928 (.014)** **0.000**	**.910 (.016)** **0.000**	**.911 (.016)** **0.000**
Rent as a % of government revenues	-.095 (.089) 0.283	-.214 (.124) 0.085		
Net oil exports as % of GDP			-.195 (.144) 0.175	**-.537 (.214)** **0.012**
Counterfactual per capita GDP (natural log)	**.095 (.040)** **0.016**		**.144 (.038)** **0.000**	
Per capita GDP (natural log)		**.084 (.037)** **0.023**		**.136 (.040)** **0.001**
Regional democracy score mean	**.042 (.013)** **0.001**	**.044 (.013)** **0.001**	**.054 (.012)** **0.000**	**.055 (.012)** **0.000**
% Muslim in population	**-.180 (.072)** **0.013**	**-.185 (.072)** **0.010**	**-.187 (.073)** **0.011**	**-.195 (.076)** **0.010**
Constant	-.417 (.263) 0.113	-.340 (.242) 0.160	.771 (.245) 0.002	**-.713 (.254)** **0.005**
Countries / Years	144 / 27	142 / 27	138 / 27	135 / 27
Observations	3332	3282	2534	2450

The standard error (in parenthesis) follows the coefficient, with the P-value below, in bold type if P<.05.

Christopher Achen has criticized Beck and Katz's approach, arguing that the LDV can "falsely dominate" the regression and "suppress the legitimate effects" of the other independent variables. A visible symptom of this problem is a coefficient for the LDV that approaches one.[45] The problem occurs when there is severe autocorrelation in the error term of the model when it is run without the LDV and there is autoregression in an independent variable (typically not a problem in itself). Not surprisingly, the data used in this study, which include a separate observation for every country in every year, exhibit these characteristics. This "pooling" of the data makes it possible to capture information about change over time, and by increasing the number of observations it tends to make it more likely that statistical significance will be found. But pooling leads to autocorrelation because there is typically little change from one year to the next in a country's levels of democracy and rentierism. In a substantive sense the autocorrelation is caused by omitted variables, specific to individual countries, that lead to little change in variables over time. Mexico's democracy scores in 1985 resemble those in 1984 and 1986 in no small part because of things specific to Mexico not captured by the other independent variables in the model. The LDV acts as a control for these omitted variables, and thus it is not surprising that the coefficient of the LDV is substantial and highly significant.[46] Nonetheless, Achen argues that the LDV can "squash" the effects of the other variables.[47]

I ran several alternate models to address Achen's problem. First, the time interval between observations was lengthened.[48] This procedure gets to the source of the problem: democracy scores change more from one three year period to the next than from one year to the next.[49] The cost is a loss of observations, though the result may better represent the actual amount of information in the underlying data. A three year lag also provides a reasonable amount of time for changes in the independent variables (including rentierism) to affect democracy scores. The basic findings did not change. In the models with counterfactual per capita GDP, rentierism and net oil dependence did not achieve statistical significance, though they sometimes did with standard income figures. The other independent variables typically achieved significance; region always did.[50]

Second, the dependent variable was transformed by subtracting the democracy score at t-1 from the democracy score at time t, yielding a model that predicts change in the level of democracy rather than its level.[51] First differencing fully addresses Achen's problem: there is no LDV. The results are virtually identical to those of the Beck and Katz model; the exception is the lagged democracy score variable, which now has a far more modest coefficient (-.07).[52] First differencing the independent variables and adding them to the right hand side of the model did not produce statistical significance in the rentierism or oil export dependence variables or their first differences.

Third, what might be called a "layered" five year lag was used. All independent variables (including the LDV) were lagged five years, but observations were made at

yearly intervals, following Ross. Oil export dependence achieves significance, and rentierism almost does, but the model produces a very large amount of first order autocorrelation in the errors even with the LDV.[53] There is autocorrelation even when first differencing the dependent variable. In this case the layered lag induces autocorrelation where there was little to start with. The autocorrelation is caused by the fact that, when layering the lags, sequential observations span most of the same years.[54] In a certain sense, sequential observations include much of the same information. Pooling raises serious questions about the independence and exchangeability of observations. Layering makes the situation worse.[55] If a longer lag is needed, a longer time interval should be used.

Finally, there is an argument for using a fixed effects model for Equation 3. An F-test suggests that it is necessary, and the model addresses some of the problems with pooling discussed above.[56] The fixed effects model adds a dummy variable for each country (but one), allowing a separate intercept for each country in the analysis. It is a hard test, but not equally for all variables. The regional democracy mean retains a substantial coefficient and strong statistical significance. The other variables are insignificant.

Polity Data Results were checked using the POLITY measure found in the Polity IV dataset in place of the Freedom House democracy scores. I prefer Freedom House scores to Polity scores because of the Polity dataset's odd bias against monarchies—and rentier states are disproportionately monarchies. The bias is found in the construction of the composite POLITY index. One point is deducted from the index if a country has a monarch who rules. Thus, only monarchies can receive the lowest score in the POLITY measure. Qatar in 1999 is scored as more authoritarian than Hitler's Germany, a bizarre result. A distinction is also drawn between regime change with regulated transfers of power and regime change through "forceful seizures of power." Authoritarian regimes with regulated transfers (typically including monarchies) are punished with a deduction of two points.[57]

When Polity democracy scores are used in place of Freedom House democracy scores, the rentierism variable remains statistically insignificant. The oil export dependence measure, however, achieves significance (P = .39) in a model that includes counterfactual income (see Table 4). The expected effect of a standard deviation change in the oil export dependence variable is .04 on a thirteen point scale. The effect of a standard deviation change in the region variable was about four times as much. (I rescaled the POLITY measure to make coefficients comparable with the Freedom House models.) This evidence is the strongest in this analysis in support of the notion that oil harms democracy. Ross, it should be emphasized, used both the Polity dataset and a measure of oil export dependence in his study. Use of the Polity democracy scores did not have a similar effect on the findings for the models with the rentierism measure, which saw little change.

Table 4 Results with Polity Democracy Scores

	Rentierism	Oil
Polity Democracy Score	**.938 (.011)** **0.000**	**.935 (.012)** **0.000**
Rent as a % of government revenues	-.113 (.101) 0.265	
Net oil exports as % of GDP		**-.289 (.140)** **0.039**
Counterfactual per capita GDP (natural log)	.069 (.037) 0.061	**.110 (.031)** **0.000**
Regional democracy score mean	**.045 (.016)** **0.005**	**.047 (.013)** **0.000**
% Muslim in population	-.128 (.088) 0.146	-.059 (.086) 0.493
Constant	-.304 (.229) 0.183	**-.650 (.179)** **0.000**
Countries / Years	128 / 27	123 / 27
Observations	2794	2241

The standard error (in parenthesis) follows the coefficient, with the P-value below, in bold type if P<.05.

Additional Robustness Tests Several other model specifications were tried to further test the robustness of the findings. First, rentier states tend to be small, and it is possible that size confounds the results. To test for size the natural log of population was added as a variable. However, the results changed little, and the variable was little correlated with democracy scores. Second, Israel was coded as being in the Middle East for the calculation of regional democracy score means. Separately, the Kiribati observations were dropped. The coefficients for the rentierism and oil export dependence variables rose modestly from the results reported in Table 3 and did not achieve significance. Third, the regressions were run on only those observations for which there is data for both rentierism and oil export dependence. The results for the two measures tended toward convergence. Simply dropping Botswana—a democratic diamond exporter which is a rentier state but not an oil exporter—noticeably increased the coefficient of the rentierism variable.

Rich Rentiers, Poor Rentiers Both rich and poor countries score high on the rentierism measure (see Table 1). But it is possible that the effects of rentierism on democracy appear more clearly among poorer rentiers (because there are no countervailing positive effects of rent wealth) or richer rentiers (where the countervailing effects might predominate). To see if either is true, first all observations with a standard per capita GDP above $2,500 were removed from the dataset. The rentierism and oil export dependence measures were very far from significant; the coefficients even changed signs. Region was significant. The ill effects of rentierism do not appear clearly in poor ren-

tiers, where the positive effects are presumably weaker. Second, income was multiplied by the share of rent in government revenues to create a variable with high values for richer rentiers (where rent wealth has a widespread effect on society), low values for poorer rentiers, and zero for other countries. This interaction variable had a negative and small coefficient and failed to achieve statistical significance. The impact of rentierism on democracy does not appear any more clearly when the measure of rentierism is revised to emphasize the potential positive effects of rent wealth on democracy.

Conclusions

Many scholars suggest that democracy requires taxation. Yet rent wealth not only reduces the need to tax, it also induces changes that resemble the sort of economic and social development that have elsewhere been associated with democracy. I have attempted to measure the net effect of these two countervailing forces by employing a counterfactual measure of development in which the level of development of the richer rentier states was set to a level that would be expected in a counterfactual world in which they lacked rent wealth. This technique avoids the pitfalls that are encountered when using standard per capita GDP as a control for level of development in a study of the political consequences of rentierism.

I did not find consistent support for the thesis that rentierism has a harmful net effect on democracy scores. A partial explanation for this result is that the definition of rentierism assures that rentier states will be drawn largely from among the world's poorer states and hence those that would be expected to be more authoritarian. However, the findings do not emphatically refute the thesis. TSCS data pose thorny statistical problems. Data issues warrant caution; the dataset is rife with missing observations. Models run with a combination of Polity democracy scores and the measure of oil export dependence achieve weak statistical significance at conventional levels. Most important, the sign on the rentierism coefficient is negative across a variety of model specifications. Better data, or a longer time series, might more clearly resolve the issue in one direction or the other.

The effect of other variables emerges with much more clarity. The regional democracy score mean is quite clearly an important determinant of democracy. Ross, too, notes the importance of region in his conclusions. Of the independent variables, a single standard deviation change in the regional democracy score variable typically had the largest effect on democracy scores, and the rentierism or oil export dependence variables the smallest. Thus the findings are perhaps best thought of in a comparative sense: while the possibility that rentierism harms democracy can not be dismissed, it is clear that it has a smaller substantive impact than region, Muslim share of the population, and income.

The rentier state theory not solely hypothesizes that rents harm democracy. It also emphasizes the failed promise of natural resource wealth. Intuitively, the net effect

of natural resource bounties should be positive. The results provide no evidence that they are in terms of democracy. One of the original intuitions of studies of the rentier state—that rent wealth fails to deliver the expected benefits—is thus confirmed. At the same time, however, it is important to keep this idea distinct from the notion that rent wealth is an outright curse. A prospective rentier—Chad, for example—can not expect to benefit much in terms of democracy if it begins to export oil. Yet rentierism is also not likely to snuff out its (probably modest) democratic prospects. When examining the various ways in which the promise of oil wealth is squandered in rentier states, it is crucial to keep in mind that there is often little reason to think that political outcomes in the absence of rent wealth would have been very good. Rent wealth does not make countries better governed, but neither is it a curse.

More broadly, nothing here supports the notion that rentier states are somehow the same as other states. Rentierism is a distinct condition, and rent-induced development is a puzzle. The findings here call into question the signature thesis about the rentier state but in no way call into question the need for a rentier state theory, that is, for a theoretical framework to explain the distinctive economic, political, and social consequences of rent wealth. Case studies are an important part of the rentier state theory but should be designed to address the criticism to which the theory is most vulnerable: that the putative negative consequences of rentierism— authoritarianism, rent-seeking, corruption, economic stagnation—are characteristics of both rentier states and their otherwise similar neighbors. Rentier states tend to be located in regions (the Middle East, sub-Saharan Africa) where states, rentier or not, typically suffer from generally unsatisfactory political outcomes. Case studies, too, might also benefit from considering the possibility that the effects of rents are mediated decisively by other variables, so that overall outcomes of rentierism vary sharply across cases. Thus, for example, the political systems of the Gulf monarchies could not be as they are without oil, but oil itself could not replicate the key aspects of their political systems elsewhere.

Finally, it should be emphasized that, for many scholars, the core issue is to explain the persistent authoritarianism of the Arab world. Regional democracy scores emerge quite clearly from this study as the clearest and strongest predictor of democracy scores. Oil wealth should not provide an excuse to dismiss the possibility of democracy in the Middle East or a reason to avoid looking closely at regional factors that can better explain its persistent authoritarianism.

Appendix: Measuring Rentierism

I constructed the rentierism measure from several readily available sources. The IMF publishes staff country reports for most sovereign states, and these reports cover

most of the 1990s. Where rentier income is important, the IMF reports generally break it out in government revenues in a separate category. These reports can be found at www.imf.org/external/country/index.htm. The Economist Intelligence Unit sometimes details major sources of government revenue in its country profiles and its earlier quarterly economic reviews. In some cases it gives budget figures rather than outturns. The IMF's *Government Finance Statistics* sometimes provides useful data. I consulted electronic publications issued by national governments when they were linked from the IMF's web site. Finally, in a few cases information was gathered from readily available books on various countries.

In constructing the dataset, countries first were examined for any plausible source of rentier income. I consulted the IMF's country reports, statistics on mineral and oil exports and on the share of nontax revenue in total revenues from the World Bank's *World Development Indicators*, the United States Geologic Survey's Minerals Yearbook (online at http://minerals.usgs.gov/minerals/pubs/country/, accessed in fall 2001), and United States Energy Information Administration statistics. Countries with no obvious source of rentier income in these and other sources were presumptively coded as having no rentier income at all. In most cases this coding was done only after looking at IMF staff country reports; small poor countries received more attention than populous rich countries. A more extensive examination was done for countries with a potential source of rentier income. Where it seemed clear that there was (or might be) a substantial amount of rentier income but no specific figures could be found, observations were left blank. Missing observations were filled in with data from the immediately adjacent year, if available. If per capita income figures for an observation were lacking, I made no effort to collect data on rentierism.

In practice, it appears that rentier income derives from a limited number of sources: petroleum, minerals (especially diamonds, phosphates, and copper), investment income from previous years' exports of oil or minerals, fishing license revenue when paid by foreign vessels or countries, and canal passage fees. Oil is most important, by far. Mineral exports are often less profitable. Mineral exports appear sometimes to compose a substantial part of the economy while generating substantial rents for the government only sporadically, if at all. There were a few other assorted sources of rentier wealth, mostly from the exploitation of sovereign prerogatives by states with exceedingly modest revenues otherwise: flagging foreign vessels, selling passports, licensing Internet domain names. Specific data on rent, or per capita GDP, were missing for most countries with the odder varieties of rentier income. Sometimes government income from the export of agricultural (or other organic) products have a whiff of rent about them. In these cases, however, it is very difficult to sort out the relative share of productive activity and rent windfalls in government income, so proceeds from agricultural products were not counted as rents. For summary statistics, see Table 5.

Table 5 Summary Statistics

	Observations	Mean	Std. Dev.	Min.	Max.
Freedom House democracy score	4746	5.94	4.13	0	12
POLITY democracy score	3957	5.90	4.65	0	12
Rentierism	3335	.103	.225	0	.97
Oil export dependence	2621	.049	.138	0	.93
Ln (per capita GDP)	3867	7.89	1.07	5.27	10.66
Ln (counterfactual per capita GDP)	4038	7.79	1.01	5.27	10.01
Regional democracy score mean	4674	5.97	2.76	1.77	11
Regional democracy score mean (Polity)	3957	5.89	3.11	.84	12
Muslim % of population	4746	.235	.351	0	1

NOTES

1. Michael L. Ross, "Does Oil Hinder Democracy?," *World Politics*, 53 (April 2001), 356–57.
2. John Clark, "Petro-Politics in Congo," *Journal of Democracy*, 8 (July 1997), 62–76.
3. Jeffrey D. Sachs and Andrew M. Warner, "Natural Resource Abundance and Economic Growth," National Bureau of Economic Research, Working Paper 5398 (1995); Richard M. Auty and Raymond F. Mikesell, *Sustainable Development in Mineral Economies* (Oxford: Oxford University Press, 1998).
4. See, for instance, Terry Lynn Karl, *The Paradox of Plenty: Oil Booms and Petro-States* (Berkeley: University of California Press, 1997).
5. Hazem Beblawi, "The Rentier State in the Arab World," in Giacomo Luciani, ed., *The Arab State* (Berkeley: University of California Press, 1990), pp. 87–88.
6. The concept derives from the usage of the term rent in classical political economy. Rents are not generated by productive human activity but instead by the scarcity value of natural endowments. David Ricardo, *On the Principles of Political Economy and Taxation* (London: John Murray, 1921), pp. 54–55.
7. Lisa Anderson, "The State in the Middle East and North Africa," *Comparative Politics*, 20 (October 1987), 1–18; Jill Crystal, *Oil and Politics in the Gulf: Rulers and Merchants in Kuwait and Qatar* (Cambridge: Cambridge University Press, 1995); Giacomo Luciani, "Allocation vs. Production States: A Theoretical Framework," in Giacomo Luciani, ed., *The Arab State* (Berkeley: University of California Press, 1990), pp. 65–84; Dirk Vandewalle, *Libya since Independence: Oil and State-Building* (Ithaca: Cornell University Press, 1998).
8. Anderson, p. 10.
9. Quintan Wiktorowicz, "The Limits of Democracy in the Middle East: The Case of Jordan," *Middle East Journal*, 53 (Autumn 1999), 608.
10. Jacques Delacroix, "The Distributive State in the World System," *Studies in Comparative International Development*, 15 (Fall 1980), 3–21. For a more extensive discussion of these three arguments, see Ross.
11. Gwenn Okruhlik, "Rentier Wealth, Unruly Law, and the Rise of Opposition: The Political Economy of Rentier States," *Comparative Politics*, 31 (April 1999), 295–315.
12. Michael Herb, "Taxation and Representation," *Studies in Comparative International Development* (Fall 2003).

13. Michael Herb, *All in the Family: Absolutism, Revolution and Democracy in the Middle Eastern Monarchies* (Albany: State University of New York Press, 1999), pp. 256–59.
14. John Waterbury, "From Social Contract to Extraction Contracts: The Political Economy of Authoritarianism and Democracy," in John P. Entelis, ed., *Islam, Democracy and the State in North Africa* (Bloomington: Indiana University Press, 1997), p. 153.
15. Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review*, 53 (March 1959), 69–105; Ross E. Burkhart and Michael S. Lewis-Beck, "Comparative Democracy: The Economic Development Thesis," *American Political Science Review*, 88 (December 1994), 903–10.
16. Ross, pp. 342–43.
17. Samuel P. Huntington, *The Third Wave: Democratization in the Late Twentieth Century* (Norman: University of Oklahoma Press, 1991), p. 65.
18. Ronald Inglehart, "The Renaissance of Political Culture," *American Political Science Review*, 82 (December 1988), 1219.
19. Seymour Martin Lipset, Kyoung-Ryung Seong, and John Charles Torres, "A Comparative Analysis of the Social Requisites of Democracy," *International Social Science Journal*, 45 (May 1993), 166.
20. Clark, p. 74.
21. The calculation works in the short run. It is assumed that a windfall's impact on GDP has no multiplicative effects. Thus, for example, subtracting all of Qatar's oil income leaves a very substantial residual GDP that is the consequence of previous years' oil exports, making it difficult to get at the counterfactual case of a Qatar that never had oil. Ross, pp. 342–43.
22. Since it is suspected that per capita GDP, when confounded by rent wealth, is not a "true" measure of development, another variable that is a better measure of development—that is, one that better measures how rents contribute to development—could be sought. Female literacy comes to mind. Yet, without an exact knowledge of how rent wealth increases female literacy figures, it is not possible to subtract the positive effects of oil wealth from the negative, arriving at the net effect.
23. The rentierism measure includes both rich and poor countries. See below for the results of an analysis done with a variable that excludes the poorer rentiers.
24. It might be preferable to identify a prerent per capita GDP, then extrapolate forward using regional averages (for nonrentiers). Unfortunately, a clear prerent per capita GDP figure is available for only one or two rentier states in the Penn World Tables 5.6 dataset.
25. Comparison nonrentiers are in parentheses: Iraq (Egypt and Jordan); Libya (Egypt); Algeria (Morocco and Tunisia); Botswana, Congo Brazzaville, and Gabon (average of nonrentiers in sub-Saharan Africa); Kiribati (average of nonrentier Pacific island states); Venezuela (Brazil). I reduced Trinidad's GDP by one-third. Some rentiers were not included in the regressions because of missing data.
26. Luciani, p. 72.
27. Sachs and Warner, p. 8; Ross.
28. International Monetary Fund, *Staff Country Reports.*
29. Sachs and Warner; Auty and Mikesell.
30. Hal Hill, *The Indonesian Economy since 1966* (Cambridge: Cambridge University Press, 1996), pp. 47–52.
31. There is little collinearity between the measures of rentierism or oil export dependency and other independent variables. The adjusted R-squared of the Muslim population percentage correlated with rentierism is 0.17, and with oil export dependency, 0.16. The adjusted R-squared of the Muslim population percentage correlated with the regional democracy score is 0.28.
32. More generally, I did not count as rent anything that creates dependence on a single foreign state. I include grants in the denominator, as government revenue.
33. I constructed this variable by subtracting oil imports from oil exports, then calculating the balance as a percentage of GDP. Negative values (indicating net oil imports) were set to zero. Twenty data points

were dropped because they were manifestly in error, for the Bahamas, the UAE, Oman, and Kuwait. Data are from the World Bank's *World Development Indicators* CD-ROM, 1999 and 2001.

34. Many governments collect oil revenues as taxes. Nontax revenue includes all sorts of things, such as revenues from state-owned enterprises. As a result, the correlation (R-squared) between my measure of rentierism and nontax income is only 0.46; the correlation between oil export dependence and nontax revenue is 0.42. Given the existence of two far better measures, there is little profit from running the analysis using these data.

35. Burkhart and Lewis-Beck; Adam Przeworski, Michael E. Alvarez, Jose Antonio Cheibub, and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950–1990* (Cambridge: Cambridge University Press, 2000), p. 83, note 4; John B. Londregan and Keith T. Poole, "Does High Income Promote Democracy?," *World Politics*, 49 (October 1996), 1–30.

36. Mark J. Gasiorowski, "Economic Crisis and Political Regime Change: An Event History Analysis," *American Political Science Review*, 89 (December 1995), 882–97.

37. Israel was included in the European region; the regional influence variable is meant to measure the effect of common culture and ideological currents and not mere geographic propinquity. Former Soviet republics in Asia are not in the dataset as a result of their absence in the Penn World Tables; they compose a seventh region.

38. David B. Barrett, George T. Kurian, and Todd M. Johnson, *World Christian Encyclopedia: A Comparative Survey of Churches and Religions in the Modern World* (Oxford: Oxford University Press, 2001).

39. I considered several other variables based on the findings of previous studies but dropped them when they did not affect the results and proved insignificant statistically. They include Protestant and Catholic shares of the population and several measures of ethnolinguistic fragmentation. Data availability problems are severe for rentier states, making it difficult to use some other measures, such as inequality. I used Philip G. Roeder's ethnolinguistic fragmentation data, downloaded from weber.ucsd.edu/~proeder/elf.htm on February 20, 2002.

40. Nathaniel Beck and Jonathan N. Katz, "Nuisance vs. Substance: Specifying and Estimating Time-Series Cross-Section Models," *Political Analysis*, 6 (1996), 4–5; Nathaniel Beck and Jonathan N. Katz, "What to Do (and Not to Do) with Time-Series Cross-Section Data," *American Political Science Review*, 89 (September 1995), 634–47.

41. A Lagrange multiplier test indicates some remaining serial correlation, but the results were comparable to those found by Beck and Katz in their reanalysis of Burkhart and Lewis-Beck. In these circumstances, Beck and Katz recommend no further transformation of the data. Beck and Katz, "Nuisance vs. Substance," pp. 9–10, 28, 30–31.

42. Data that make possible the replication of my results can be found at www.gsu.edu/~polmfh and at the Inter-University Consortium for Political and Social Research (ICPSR) at www.icpsr.umich.edu.

43. There are more observations when using counterfactual GDP. When these observations are dropped, the coefficient for rentierism was -.111, and the coefficient for oil export dependence was -.174.

44. Beck and Katz, "Nuisance vs. Substance," pp. 28–29.

45. Christopher Achen, "Why Lagged Dependent Variables Can Suppress the Explanatory Power of Other Independent Variables," paper presented at the American Political Science Association annual conference, 2000, p. 24.

46. See Burkhart and Lewis-Beck, p. 905; Beck and Katz, "Nuisance vs. Substance"; Ross, p. 339.

47. Achen, p. 21.

48. See, for example, Robert J. Barro, "Determinants of Democracy," *Journal of Political Economy*, 107 (1999), s158–83.

49. Autocorrelation in the errors, in a model without the LDV, fell to between .77 and .84 (regressing the residuals against their lags). In the full model (with the LDV), the coefficient of the LDV fell to between .788 and .797.

50. I broke the data into three datasets, each containing observations at three year intervals. Complete regression results for this and other unreported analyses can be found at www.gsu.edu/~polmfh.

51. See, for example, David S. Brown and Wendy Hunter, "Democracy and Social Spending in Latin America, 1980–92," *American Political Science Review*, 93 (December 1999), 781.

52. There is little argument for removing the lagged democracy score altogether. It is, of course, no longer an LDV. And it makes sense that change in democracy scores might be related to the level of democracy scores; the variable is highly significant. When the variable is removed, the model loses coherence, with no variables achieving significance.

53. In a Lagrange Multiplier test, the lagged residual had a coefficient of .78, with a t-ratio of 67. Ross avoided this problem by using FGLS.

54. Specifically, the residuals are similar from one observation to the next, which produces autocorrelation.

55. Gary King, "Proper Nouns and Methodological Propriety: Pooling Dyads in International Relations Data," *International Organization*, 55 (Spring 2001), 498–99; Donald P. Green, Soo Yeon Kim, and David H. Yoon, "Dirty Pool," *International Organization*, 55 (Spring 2001), 441–68.

56. Nathaniel Beck, "Time-Series—Cross-Section Data: What Have We Learned in the Past Few Years?," *Annual Review of Political Science* (2001), 283–85; Londregan and Poole, pp. 11–13.

57. Monty G. Marshall and Keith Jaggers, *Polity IV Project: Dataset Users Manual* (College Park: Center for International Development and Conflict Management, 2000), pp. 18 and 14 for the consequences when XRCOMP is 0.