

**No Representation without Taxation? Rents, development  
and democracy**

Michael Herb  
Georgia State University  
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## **Abstract**

It is widely thought that oil and democracy do not mix. Rentier states, it is argued, need not tax their citizens, thus breaking a crucial link between citizens and their governments, and dimming the prospects for democracy. In this paper I examine the link between rentierism and democracy using a cross-regional dataset. I pay particular attention to the possibility that there are both positive and negative effects of rentierism on democracy. I do not find consistent support for the notion that there is a net negative effect of rentierism on the prospects that a country will be democratic. I do find that democracy scores in the surrounding region are strongly correlated with a country's own democracy score.

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## **Introduction**

Natural resource windfalls would seem to be a boon for poor countries, reducing the burden of taxation, creating a middle class, and paying for social services. In the view of most scholars, however, natural resource bounties are more a curse than a blessing. In the literature on rentier states it is argued 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: 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 an pathbreaking and long overdue contribution, tested the thesis using a large-*n*, cross-regional data set. Ross finds that “the oil-impedes-democracy claim is both valid and statistically robust, ... oil does hurt democracy.” This, he says, helps to “vindicate” the rentier state theory.<sup>1</sup> In this article I revisit this issue, and come to a more ambivalent conclusion concerning the theory. My results differ, first, because I use a new dataset that directly measures rentierism. Second, I use different methods to analyze the data. Third, I take a different tack in dealing with a phenomenon little discussed in the literature on 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 the degree to which it is counterbalanced by the negative consequences of rentierism. The issue has

received scant attention in the rentier state literature (though the issue is engaged in Ross's article and in a study of Congo by John Clark).<sup>2</sup> The issue is important not only in understanding 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 via the "Dutch disease" and other mechanisms.<sup>3</sup> Political consequences include characteristic patterns of state formation, the prevalence of corruption, the lack of democracy, and so forth.<sup>4</sup> Of these, I test the most prominent political claim found in the literature: 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 generally measured as the share of natural resource exports as a percentage of GDP. Rentierism, by contrast, is the percentage of rents in government revenues. Beblawi's definition of rents is widely followed: rents (1) come from abroad, (2) accrue to the government directly and, (3) "only a few are engaged in the generation of this rent (wealth), the majority being only involved in the distribution or utilisation of it."<sup>5</sup> The third point requires some elaboration: it is not only that a few people produce the wealth, but that the wealth is the result of a windfall that is very largely independent of any efforts made by citizens of the rentier state.<sup>6</sup> Table 1 lists the world's rentier states in the period from 1972 to 1999.

[Table 1 about here]

The term 'rent' appears, with a different meaning, in a separate literature which deals with

the consequences of rent-seeking in the domestic political economy by influential actors. To heighten the potential for confusion, rent-seeking is sometimes identified as an attribute of rentier states. I do not here deal with rent-seeking as such.

The argument that rentierism harms democracy has been advanced in a number of case studies and theoretic pieces by, among many others, Lisa Anderson, Jill Crystal, Dirk Vandewalle and Giacomo Luciani.<sup>7</sup> The causal mechanisms underlying the claim that rentierism harms democracy are of three main sorts (for a more extensive discussion, see Ross's 2001 article). The first concerns how the state collects revenue: rentier states need not tax (or need not tax much). This, it has been argued, "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."<sup>8</sup> The second causal mechanism concerns how the state spends revenues: rentierism, it is argued, 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."<sup>9</sup> A third causal mechanism focuses on society, not the state: oil revenues change the class structure of society. This can stymie democracy by preventing changes in class structure that usually lead to democracy.<sup>10</sup>

Critics have registered several objections to the thesis that rentierism obstructs accountability. Okruhlik argues that, in Saudi Arabia, the regime's efforts to buy off dissent failed, generating more of it.<sup>11</sup> Second, the venerable tie between taxation and representation in European history may provide far less support for the rentier state theory than is often supposed.<sup>12</sup> Third, it has been suggested that citizens have many reasons to want to hold their

rulers accountable even if they are not taxed.<sup>13</sup> Finally, some have raised doubts concerning the empirical robustness of the theory, especially because there are rentier democracies outside the Middle East.<sup>14</sup>

### **The Lipset thesis and the rentier state theory**

In a well-known 1959 article Lipset argued that development is correlated with democracy, and the Lipset thesis has proven to be one of the more durable empirical findings of comparative politics.<sup>15</sup> The Lipset thesis has important implications for the argument that rents prevent democracy, because rentierism often leads to a sharp increase in metrics of development: per capita GDP rises, as do levels of education, urbanization, energy usage, and so forth. It is possible, however, that the ‘development’ caused by rents has a much weaker effect on democracy than does the development caused by other sources of wealth.

Ross assumes that oil wealth has the same positive effect on democracy as other sorts of wealth.<sup>16</sup> That is to say, 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 we think that Kuwait’s wealth has the same effect on the likelihood that it will be democratic. If we think that Kuwait’s wealth is less potent in this regard, we should compare Kuwait (in terms of wealth) to Jordan, perhaps, or Yemen or Djibouti.

Throughout the rentier state literature, and the wider literature on democratization, I am

not aware of any 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."<sup>17</sup> 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."<sup>18</sup> Lipset himself offers a similar view.<sup>19</sup>

[Table 2 about here.]

Yet this 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 contributed to a brief bout of democratization there. At the same time, 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."<sup>20</sup> 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 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.<sup>21</sup> 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. Put differently, Kuwait is compared to Canada, in terms of the positive effects of wealth on democracy. Since Kuwait is much less democratic than Canada, this leaves much of Kuwait's authoritarianism underexplained. This leads (as the analysis below suggests) to an exaggeration of the statistical significance of the oil export dependence variable.

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, we could find a different measure of development, one that is not affected by rent wealth. But in a country like Kuwait everything that is measurable, and which might serve as a proxy for development, has been affected by oil, from per capita electricity consumption, to education levels, to literacy.<sup>22</sup> 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. We cannot subtract rent or oil wealth 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 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 (or, more exactly, this would hold true, on average, for all rentier states). I thus posit a counterfactual world in which rentier states lack

rentier wealth. I estimate what their per capita GDP figures might be in this counterfactual world, and I replace standard per capita GDP figures with these counterfactual figures. This gets directly to the counterfactual at the heart of the rentier state theory. If we use this measure *in place of* standard per capita GDP, and if the measure of rentierism continues to be negative and statistically significant, we can conclude that 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 we have seen, we have theoretic reasons to believe that these effects could be simultaneously positive and negative.<sup>23</sup>

I derive counterfactual GDP figures by comparing rentiers to otherwise similar countries that lack abundant rents – I did not attempt to adjust per capita GDP for poor countries such as Nigeria or Angola. I selected comparison countries from the same region, usually neighbors: region is the most powerful predictor of democracy scores. I averaged the per capita GDP figures for these countries for each year, and I replaced standard per capita GDP figures for rentiers with these figures.<sup>24</sup> For the crucial Gulf Arab monarchies, I use an average of the per capita GDP of Jordan, Egypt and Yemen. The Gulf states, before oil, were far less developed than Jordan or 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: this provides a conservative bias. Another reasonable counterfactual would be Yemen alone. For sub-Saharan African countries, I used a regional average of non-rentiers. Elsewhere I used neighbors: I set Iran's per capita GDP to an average of Turkey (an optimistic counterfactual) and Pakistan (a more likely counterfactual).<sup>25</sup> Any possible calculation of counterfactual GDP requires major – perhaps heroic – assumptions. My defense is straightforward: without counterfactual GDP we cannot test the claim that rents harm democracy in a multivariate model. This is a claim that is

worth testing: it informs our understanding 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 the Lipset thesis. A brief survey of rentier states (Table 1) shows that most are authoritarian. This 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).

Following the literature, rentierism is rent revenue as a percentage of total government revenues:<sup>26</sup>

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

The equation used to measure natural resource dependency is similar:<sup>27</sup>

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

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 come to dominate government revenues. In 1996 Angola's government received a modest \$127 per capita in oil rents, but these constituted fully 86% of government revenues. In the same year, by contrast, the Norwegian government received, per capita, *19 times* more oil revenue than did Angola, but this oil wealth amounted to only 13% of government revenues.<sup>28</sup> 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.

That said, the direction of causation might be reversed: it is often argued that dependence on natural resource exports impedes economic growth.<sup>29</sup> Thus there may be a vicious circle at work: poor economies are more likely to be dominated by a single natural resource, and this in turn erodes the remaining economy, further increasing the economic importance of the resource. As the non-resource 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 non-rentier states has been anemic. Further, in regions where economic growth is common, such as Southeast Asia, some rentiers have escaped rentierism.<sup>30</sup> The “natural resource trap” may really be an Africa and Middle East trap.

## Data

I test the hypothesis that rents harm democracy with the following model:

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

My dataset includes all sovereign states with available data in each year from 1972 through 2000 (accounting for a one year lag, this yields 4746 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, I have at most 3332 observations in 144 countries over 28 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 I combined the Freedom House scores for Political Freedom and Civil Liberties to yield a 13 point scale from 0 to 12. I reversed the scale so that higher scores are more democratic.

There are no existing data sets that directly measure rentierism, so I consulted a number of readily available sources to construct my own, guided by Beblawi's definition of rentierism (see the Appendix for details).<sup>31</sup> I have not counted grant revenue as rent.<sup>32</sup> 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 cannot 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 close to the original intuition of the rentier state theory, and confounding issues are avoided.

I check my results using a second variable, net oil exports as a percentage of GDP (oil export dependence, henceforth): this is Ross's *Oil* variable, somewhat modified.<sup>33</sup> This variable has the important virtue of getting at a very closely related issue via an entirely different collection of data. As it turns out, rentierism and dependence on oil exports are highly correlated, with an adjusted R-squared of .82. I do not use the IMF's figures for nontax revenue as a percentage of current revenues, as these data are a poor measure of rentierism.<sup>34</sup>

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 US dollars, using purchasing power parity. I set the richer rentiers' per capita GDP figures to the means of non-rentier neighbors, or non-rentier regional averages, as described above. This was done yearly.

Previous studies of democracy have found that regional dummy variables or other controls for region are significantly correlated with democracy scores.<sup>35</sup> I follow Gasiorowski in constructing a measure of regional influences.<sup>36</sup> For each country in each year, I calculated the mean democracy score of all other countries in its region in that year. 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. I calculated regional averages using six regions: the Middle East, Europe and English-speaking North America, Latin America and the Caribbean, South Asia, East Asia, and Oceania.<sup>37</sup>

The percentage of Muslims in countries' populations changes little over time, and I used 1990 figures in most cases.<sup>38</sup> 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.<sup>39</sup>

Many rentiers that we are directly concerned with (Gabon, Qatar) have small populations. This is not at all irrelevant to how they got to be rentiers in the first place – a given amount of rent wealth produces more rentierism when divided amongst 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 is scarce

and they are not in the dataset used for the regressions, with the exception of a number of observations for Kiribati.

## Methods & 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 my analysis follows their lead. Beck and Katz argue that TSCS data suffer from three problems: autocorrelation, panel heteroscedasticity, and contemporary correlation.<sup>40</sup> PCSEs deal with the problems of panel heteroscedasticity and contemporary correlation. A test for panel heteroscedasticity in the pooled data was positive, as we would expect, suggesting that use of PCSEs is appropriate. The LDV deals with autocorrelation.<sup>41</sup>

[Table 3 about here]

Table 3 reports the results of the basic model (Equation 3), and three variations.<sup>42</sup> 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.<sup>43</sup> 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 4½ and 5½ times the expected change resulting from a standard deviation change in the rentierism variable, in the first year.<sup>44</sup> The model suggests that the democracy score of a Kuwait

without oil (past or present) would be a mere .086 points below its current value on the 13 point democracy scale.

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 is a coefficient for the LDV that approaches one.<sup>45</sup> The problem occurs when (1) there is severe autocorrelation in the error term of the model when it is run *without* the LDV and (2) there is autoregression in an independent variable (typically not a problem in itself). Not surprisingly, these are characteristics of the data used in this study, which include a separate observation for every country *in every year*. This "pooling" of the data allows us to capture information about change over time, and by increasing the number of observations it tends to make it more likely that we will find statistical significance. But pooling leads to autocorrelation because there is typically little change, from one year to the next, in a country's levels of democracy, rentierism, and so forth. 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 of Mexico 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.<sup>46</sup> Nonetheless, Achen argues that the LDV can "squash" the effects of the other variables.<sup>47</sup>

I ran several alternate models to address Achen's problem. First, I lengthened the time interval between observations.<sup>48</sup> This 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.<sup>49</sup> The cost is a loss of observations, though this 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.<sup>50</sup>

Second, I transformed the dependent variable 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.<sup>51</sup> 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).<sup>52</sup> 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, I used what might be called a "layered" five year lag: 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.<sup>53</sup> This is true 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.<sup>54</sup> 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.<sup>55</sup> 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.<sup>56</sup> The fixed effects model adds a dummy variable for each country (but one), allowing a separate intercept for each country in the analysis. This 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.** I checked my results 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, which is bizarre. Second, a distinction is drawn between regime change with regulated transfers of power and regime change via "forceful seizures of power." Authoritarian regimes with *regulated* transfers (typically including monarchies) are punished with a deduction of two points.<sup>57</sup>

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, does achieve 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 13 point scale. The effect of a standard deviation change in the region variable was about 4 times as much. (I rescaled the POLITY measure to make coefficients comparable with the Freedom House models.) This is the strongest evidence in support of the notion that oil harms democracy found in this analysis. 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 about here.]

**Additional Robustness Tests.** I tried several other model specifications to further test the robustness of my findings. First, rentier states tend to be small, and it is possible that this confounds the results. To test for this I added the natural log of population as a variable. However the results changed little and the variable was little correlated with democracy scores. Second, I coded Israel as being in the Middle East for the calculation of regional democracy score means. Separately, I also dropped the Kiribati observations. 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, I ran the regressions 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 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 this is the case I first removed all observations with a standard per capita GDP above \$2500 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 rentiers, where the positive effects are presumably weaker. Second, I multiplied income 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 we revise the measure of rentierism 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. In this article I have attempted to measure the net effect of these two countervailing forces. I did this by employing a counterfactual measure of development in which I set the level of development of the richer rentiers to a level we would expect in a counterfactual world in which they lack 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 do not find consistent support for the thesis that rentierism has a harmful net effect on democracy scores. I have offered an partial explanation for this in the observation that the definition of rentierism assures that rentier states will be drawn largely from amongst the world's poorer states, and hence those that we would expect to be more authoritarian. But neither do my findings emphatically refute the thesis. As we have seen, 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 we cannot dismiss the possibility that rentierism harms democracy, it is clear that it has a smaller substantive impact than region, Muslim share of the population, or income.

The rentier state theory is not solely about the claim that rents harm democracy: it is also about the failed promise of natural resource wealth. Our initial intuition is that the net effect of natural resource bounties should be positive. My results provide no evidence that they are, in terms of democracy. I thus confirm one of the original intuitions of the rentier state literature, which is that rent wealth fails to deliver the benefits we would expect. 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, say – cannot expect to benefit much in terms of democracy if it begins to export oil. Yet neither is rentierism 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 either. 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. My findings here call into question the signature thesis of the rentier state literature, but in no way call into question the need for a rentier state theory – that is, for a theoretical framework for understanding 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 (viz., authoritarianism, rent-seeking, corruption, economic stagnation) are characteristics of both rentiers and their otherwise similar neighbors. Rentiers tend to be located in regions where states, rentier or not, typically suffer from generally unsatisfactory political outcomes (the Middle East, sub-Saharan Africa). 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 explaining 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 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](http://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 the EIU 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. In this, I consulted the IMF's Country Reports; statistics on mineral and oil exports and statistics 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 the Fall of 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 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 a substantial amount of rentier income (or might be), 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 I lacked per capita income figures for an observation, 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 to sometimes 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, and so forth. Specific data on rent, or per capita GDP, was 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.

[Table 5 (Summary statistics) about here.]

## Endnotes

<sup>1</sup> Michael L. Ross, "Does Oil Hinder Democracy?" *World Politics* 53 (April 2001): 356-7.

<sup>2</sup> John Clark, "Petro-Politics in Congo," *Journal of Democracy*, 8 (July 1997), 62-76.

<sup>3</sup> 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).

<sup>4</sup> See for instance Terry Lynn Karl, *The Paradox of Plenty: Oil Booms and Petro-State* (Berkeley: Univ. of California Press, 1997).

<sup>5</sup> 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.

<sup>6</sup> 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. 54-5.

<sup>7</sup> 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 *The Arab State*, ed. Giacomo Luciani (Berkeley: University of California Press, 1990), pp. 65-84; Dirk Vandewalle, *Libya Since*

*Independence: Oil and State-Building* (Ithaca: Cornell University Press, 1998).

<sup>8</sup> Anderson, p. 10.

<sup>9</sup> Quintan Wiktorowicz, "The Limits of Democracy in the Middle East: The Case of Jordan," *The Middle East Journal*, 53(Autumn 1999), 608.

<sup>10</sup> Jacques Delacroix, "The Distributive State in the World System," *Studies in Comparative International Development*, 15 (Fall 1980), 3-21.

<sup>11</sup> Gwenn Okruhlik, "Rentier Wealth, Unruly Law, and the Rise of Opposition: The Political Economic of Rentier States," *Comparative Politics*, 31 (April 1999), 295-315.

<sup>12</sup> Michael Herb, "Taxation and Representation," *Studies in Comparative International Development* (forthcoming Fall 2003).

<sup>13</sup> Michael Herb, *All in the Family: Absolutism, Revolution and Democracy in the Middle Eastern Monarchies* (Albany: State University of New York Press, 1999), 256-9.

<sup>14</sup> John Waterbury, "From Social Contract to Extraction Contracts: The Political Economy of Authoritarianism and Democracy," in *Islam, Democracy and the State in North Africa* ed. John P. Entelis (Bloomington: Indiana University Press, 1997), p. 153.

<sup>15</sup> Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *The American Political Science Review*, 53 (March 1959), 69-105; Ross E. Burkhardt and Michael S. Lewis-Beck, "Comparative Democracy: The Economic Development Thesis," *The American Political Science Review*, 88 (December 1994), 903-910.

<sup>16</sup> Ross, pp. 342-3.

<sup>17</sup> Samuel P. Huntington, *The Third Wave: Democratization in the Late Twentieth Century*.

(Norman: University of Oklahoma Press, 1991), 65.

<sup>18</sup> Ronald Inglehart, “The Renaissance of Political Culture,” *The American Political Science Review*, 82 (December 1988), p. 1219.

<sup>19</sup> 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.

<sup>20</sup> Clark, p. 74.

<sup>21</sup> 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. 342-3).

<sup>22</sup> Since we suspect that per capita GDP, when confounded by rent wealth, is not a “true” measure of development, we could look for another variable that is a better measure of development – that is, one that better measures how rents contribute to development. Female literacy comes to mind. Yet without an exact knowledge of how rent wealth increases female literacy figures, we cannot subtract the positive effects of oil wealth from the negative, arriving at the net effect.

<sup>23</sup> The rentierism measure that I use includes both rich and poor countries. Later in this article I discuss the results of an analysis done with a variable that excludes the poorer rentiers.

<sup>24</sup> It might be preferable to identify a pre-rent per capita GDP, then extrapolate forward using regional averages (for non-rentiers). Unfortunately, a clear pre-rent per capita GDP figure

is available for only one or two rentier states in the Penn World Tables 5.6 dataset.

<sup>25</sup> Comparison non-rentiers are in parentheses: Iraq (Egypt and Jordan); Libya (Egypt); Algeria (Morocco and Tunisia); Botswana, Congo Brazzaville and Gabon (average of non-rentiers in sub-Saharan Africa); Kiribati (average of non-rentier 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.

<sup>26</sup> Luciani, p. 72.

<sup>27</sup> Sachs and Warner, p.8; Ross.

<sup>28</sup> International Monetary Fund, *Staff Country Reports*.

<sup>29</sup> Sachs and Warner; Auty and Mikesell.

<sup>30</sup> Hal Hill, *The Indonesian Economy Since 1966* (Cambridge: Cambridge University Press, 1996), pp. 47-52.

<sup>31</sup> 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 is 0.16. The adjusted R-squared of the Muslim population percentage correlated with the regional democracy score is 0.28.

<sup>32</sup> 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.

<sup>33</sup> 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 is from the World Bank's *World Development Indicators* CD-ROM, 1999 and 2001.

<sup>34</sup> 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.

<sup>35</sup> 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.

<sup>36</sup> Mark J. Gasiorowski, "Economic Crisis and Political Regime Change: An Event History Analysis," *The American Political Science Review*, 89 (December 1995), 882-897.

<sup>37</sup> Israel was included in the European region: the regional influence variable is meant to measure the effect of common culture, ideological currents, and so forth, 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.

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

<sup>39</sup> 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. These include Protestant and Catholic shares of the population and several measures of ethno-linguistic 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 ethno-linguistic fragmentation data, downloaded from [weber.ucsd.edu/~proeder/elf.htm](http://weber.ucsd.edu/~proeder/elf.htm) on February 20, 2002.

<sup>40</sup> Nathaniel Beck and Jonathan N. Katz, "Nuisance vs. Substance: Specifying and Estimating Time-Series Cross-Section Models," *Political Analysis*, 6 (1996), 4-5; Beck and Katz, "What to do (and not to do) with Time-Series Cross-Section Data." *The American Political Science Review*, 89 (September 1995), 634-647.

<sup>41</sup> 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 (1996, pp. 9-10, 28, 30-1).

<sup>42</sup> Data that makes possible the replication of my results can be found at my Web site, [www.gsu.edu/~polmfh](http://www.gsu.edu/~polmfh) and at the Inter-university Consortium for Political and Social Research (ICPSR), at [www.icpsr.umich.edu](http://www.icpsr.umich.edu).

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

<sup>44</sup> Beck and Katz, "Nuisance vs. Substance," pp. 28-9.

<sup>45</sup> 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; page 24.

<sup>46</sup> See Burkhart and Lewis-Beck, p. 905; Beck and Katz, “Nuisance vs. Substance;” Ross, p. 339.

<sup>47</sup> Achen, page 21.

<sup>48</sup> See, for example, Robert J. Barro, “Determinants of Democracy,” *The Journal of Political Economy* 107 no. 6 pt. 2 (1999), s158-s183.

<sup>49</sup> 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.

<sup>50</sup> 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 my Web site, [www.gsu.edu/~polmfh](http://www.gsu.edu/~polmfh).

<sup>51</sup> 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.

<sup>52</sup> 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.

<sup>53</sup> 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.

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

<sup>55</sup> Gary King, “Proper Nouns and Methodological Propriety: Pooling Dyads in International Relations Data,” *International Organization*, 55 (Spring 2001), pp. 498-9; Donald P. Green, Soo Yeon Kim, and David H. Yoon, “Dirty Pool,” *International Organization*, 55 (Spring 2001), 441-468.

<sup>56</sup> Nathaniel Beck, “Time-Series – Cross-Section Data: What Have We Learned in the Past Few Years?” *Annual Review of Political Science* 2001, 283-5; Londregan & Poole, pp. 11-13.

<sup>57</sup> Monty G. Marshall and Keith Jagers, *Polity IV Project: Dataset Users Manual* (College Park, MD: Center for International Development and Conflict Management, 2000). See pages 18 & 14 for the consequences when XRCOMP is 0.

**Table 1: Rentier states, 1972-1999**

		<b>Rentier period</b>	<b>Average degree of rentierism in rentier period</b>	<b>Per capita GDP 1990</b>	<b>Democratic intervals</b>
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.

**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	<i>212</i>	<i>377</i>	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.

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

**Table 4: Results with Polity Democracy Scores**

	Rentierism	Oil
Polity Democracy Score	<b>.938 (.011)</b> <b>0.000</b>	<b>.935 (.012)</b> <b>0.000</b>
Rent as a % of government revenues	-.113 (.101) 0.265	
Net oil exports as % of GDP		<b>-.289 (.140)</b> <b>0.039</b>
Counterfactual per capita GDP (natural log)	.069 (.037) 0.061	<b>.110 (.031)</b> <b>0.000</b>
Regional democracy score mean	<b>.045 (.016)</b> <b>0.005</b>	<b>.047 (.013)</b> <b>0.000</b>
% Muslim in population	-.128 (.088) 0.146	-.059 (.086) 0.493
Constant	-.304 (.229) 0.183	<b>-.650 (.179)</b> <b>0.000</b>
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  $P < .05$ .

**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