The effect of oil and diamonds on democracy; is there really a resource curse?

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Abstract

This thesis examines the effect of natural resources on the level of democracy in a set of countries. The main model is a fixed effects regression model, where the focus is on within-country variation over time. The effect of different resources is investigated, namely the effect of oil, diamonds and agriculture. Furthermore, a distinction is made between two broad types of resources, diffuse and point resources, to explore whether the effect on democracy is similar or different. Criticism in existing literature on the presence of the resource curse is taken into account. Production data on natural resources is used and not the common variable 'resource exports-over-GDP', the latter being flawed. A possible endogeneity problem is taken into account, as well as the persistence of democracy over time. I find evidence for a resource curse of oil on democracy. It is present in different model specifications, such as models with either fixed effects or a lagged dependent variable. There only seems to be very weak evidence for a negative effect of point resources on democracy, compared to the effect of diffuse resources. It is argued that this might be due to the geographic concentration of these types of resources, which enables governments and elite groups to capture resource rents.

Keywords: Natural resources, democracy, resource curse

Until the late 1980s, the general view in the literature was that natural resources were a blessing for developing countries. However, after the 1980s economists and political scientists started to challenge this conventional wisdom. Existing recent studies (e.g. Sachs and Warner (1997) and Ross (2008)) have established a cross-country relationship between natural resources on the one hand and economic growth, conflict and democracy on the other hand. This new literature suggests that natural resource abundance is associated with poor economic performance, civil wars and low levels of democracy (Rosser 2006). Nowadays researchers have accepted the idea of a negative relation between these three factors and natural resource abundance, and refer to such outcomes as 'the natural resource curse'. Some may even call it a 'stylized fact'. However, while some claim that natural resource abundance tends to destabilize democracies, slow down economic growth and initiate conflicts, others (e.g. Brunnschweiler and Bulte, 2008) look at these issues critically and come to rather different conclusions. The focus in this thesis will be on the relation between natural resource extraction, proxied by production data, and level of democracy.

The statistical association between natural resources and democracy has been a topic of interest in modern economic and political theory. For example Ross (2001) was among the first to look at the causal relation between natural resource wealth and democracy. In Ross (2001) he found that the "the oil-impedes-democracy claim is both valid and statistically robust, …oil does hurt democracy". However, in Ross (2008) his conclusion was somewhat altered and less strong. In this more recent paper he suggests that "oil wealth strongly inhibits democratic transitions in authoritarian states" and that "oil has no overall affect on the survival of democracies, but may weakly encourage democratic break-down in low-income states". In his earlier work (Ross, 2001), he explained the causal mechanism between natural resources and low levels of democracy by using the so called 'rentier state theory'. The rentier state theory asserts that governments whose revenues consist largely of natural resource profits ('rents') are unlikely to be democratic. Due to high rents in the government budget, governments are able to have low tax rates, relieving pressures for accountability (section I explores the mechanisms behind the resource curse). Moreover, they are able to oppress opposition with high spending on internal security (Brunnschweiler and Bulte, 2008[a]).

However, the theory of the resource curse does not go unchallenged. Recent research done by Herb (2004) and Brunnschweiler and Bulte (2008[b]), among others, showed that the evidence of the resource curse might not be very robust. Herb (2004) revisited the same issues addressed by Ross (2001) using a new dataset, different variables (including natural resources rents) and different methods, and arrived at more ambivalent results concerning the relation between democracy and natural resources. He does not find consistent support for the notion that there is a net negative effect of rentierism on democracy. According to Herb (2004), "the statistical relationship between rentierism and authoritarianism is weak and perhaps absent". Although in Ross (2008), as mentioned above, Ross himself admits that the earlier result is hampered by poor data due to which he makes an effort to improve the data and find more robust results.

A recent paper by Horiuchi and Waglé (2008) also criticizes the conclusions made by Ross. Their statistical analysis (although their analysis can easily be criticized) suggests that the empirical evidence supporting the resource curse might not be robust at all. One of their arguments is that the country's regime type is mostly determined posterior to the availability of natural resources. When taking into account historic levels of democracy, the most oil-abundant countries and least democratic today were also not democratic well before oil discoveries, and conversely countries that were democratic before, or at the time of oil discovery, have remained democratic (Horiuchi and Waglé, 2008). Coinciding with this argumentation, also Smith (2004) claims that because institutions (linked to political regimes) usually pre-exist resource discoveries, the effects of the rents are likely to be dependent upon this prior institutional variation[1].

The search for robust results in the resource curse literature is ongoing. This also applies for the "political regime perspective" of the curse. Until now this has been analyzed by political scientists. However, political regimes (e.g. democracy versus autocracy) have real economic implications, and therefore should also be of interest to economists. Hence, the question rises what would happen to Ross' "oil-impedes-democracy claim" if we would use the criticism in the recent literature, made by, among others Brunnschweiler and Bulte (2008) and Horiuchi and Waglé (2008), and apply it to a new resource curse model.

The objective of this thesis is to examine the existence of the resource curse. The main research question is whether a resource curse can be found in the data, and if so, whether it is robust to different model specifications. Another question to be answered is whether the resource curse can be found for all natural resources, or whether it seems to affect only specific (types of) resources.

In this thesis evidence for a resource curse for oil is found. The resource curse seems to be present in both a model with levels of democracy, as well as a model explaining changes of democracy. The resource curse hypothesis for oil holds for both fixed effects model and models where a lagged dependent variable is included. For the second research question, whether the resource curse affects only point resources, very weak evidence is found. The difference in effect on democracy of point and diffuse resources is argued to be due to the geographic concentration of these types of resources. However, the results for this hypothesis do not seem to be robust.

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[1] In light of historic institutional variation, there is the influential work of Acemoglu, Johnson and Robinson (2000), where they also refer to the adverse role of resource rents; resource rents induce institutions suited to extracting the rents rather than the provision of public goods.

The thesis proceeds as follows. The first section is about the theory and mechanisms behind the natural resource curse, linked to democracy. In section II the empirical challenges in the existing literature are addressed, and the approach to these problems in the thesis is presented. The third section provides a description of the data. Section IV presents the econometric model and in section V the main empirical results are presented. Section V also contains robustness checks on the main model and section VI concludes. The appendix further contains information on the data and its sources.

I. Mechanisms and theory

According to Rosser (2006) there are four main explanations in the literature to explain the adverse effect of natural resources on democracy. The first, and probably most popular explanation, reflects a state-centred perspective and is referred to as ‘rentier’ state theory, as mentioned earlier in the introduction. Supporters of rentier state theory are for example Ross (2001) and Jensen and Wantchekon (2004). According to Rosser (2006) the theory suggests that “…natural resource wealth hinders democracy because governments in resource-rich countries are able to use government spending and low taxes to reduce pressure for democratisation”. In practice, this means that since governments of resource-rich countries do not need taxes to for the provision of public goods, accountability of the government to the population is reduced. This causal mechanism seems to be relatively popular since there is some empirical support for this mechanism, for example from Ross (2008).

A second explanation of the adverse effect, which coincides with the state-centred perspective, suggests that natural resource wealth enable governments to have relatively high spending on internal security (Ross (2001) and Jensen and Wantchekon (2004)). By having a strong military force, governments can limit the possibilities of political opponents to organize themselves and challenge the government, and oppress the population. According to Ross (2008), this ‘repression’ effect does not seem to be very robust. In Collier and Hoeffler (2009), a general model linking natural resource wealth to levels of democracy is developed. According to their theory, in addition to the ‘repression’ effect, resource rents could be used to oppress and buy out competing political parties, but also to directly buy votes to stay in position (Collier and Hoeffler, 2009).

The third argument focuses on the link between civil war and political regimes. The central idea is that natural resource wealth makes it possible for particular regimes to remain in power, which results in opposition groups pursuing power through extra-constitutional means (e.g. rebellion and war). In the end, this could result in a dictatorship of the parties, depending on the outcome of the war (Jensen and Wantchekon, 2004).

Fourth, Ross (2001) argues that natural resource wealth hinders democracy by preventing the social and cultural changes that facilitate democratisation (e.g. education) and calls it the ‘failed modernization’ effect. Again in Ross (2008) he claims that this mechanism does not appear to be valid, when further examining the causality.

Next to the causal mechanisms proposed to explain the adverse effect of natural resources, it might be interesting to also look at the literature that explores which types of resources have these adverse effects. For example in Lujala (2003) the importance to classify sub-categories of natural resources is emphasized. In her research, Lujala mentions that the impact on economic growth and conflict might be different for different resources. A relevant difference might be ease of exploitation and investment requirements. Furthermore, the nature of natural resources might play a role: "…if the commodity flow [of the resource] can be subject to sanctions, the outside powers may have greater influence on the side holding access to the resource" (Lujala, 2003). However, previous literature (e.g. Lujala et al. 2005 and Ross, 2002) mostly focuses on the different effects of natural resources on conflict and economic growth, but not so much on democracy. When it comes to democracy, aspects such as controllability of rents might play a bigger role than 'lootability'[2], the latter often mentioned in the conflict literature (e.g. Ross 2002). Especially 'rentier' state theory implicitly assumes that governments have complete control over the resource rents. It is therefore interesting to distinguish between point resources and diffuse resources, where it is likely that point resources due to their geographic concentration are easier to control than diffuse resources. Lujala et al (2005) state that "the concentration [of a point resource] makes it easier for the government to control the mining site and the revenue flows; revenues from primary diamonds (which is a point resource) are more likely to accrue to the government, which may make the government more corrupt and repressive, but will also enhance it's capacity for defending itself".

By contrast, revenues from diffuse resources such as agriculture are less likely to be captured by elite groups or the government, making the government more dependent on taxation of the population to obtain resource rents, thus increasing the accountability of the government.

Another aspect possibly influencing government control is the extent to which a resource is a strategic resource. Strategic resources are characterized by rareness, the availability of substitutes and whether deposits are located in a few countries, according to Lujala (2003). When a resource is globally rare, the international community may be reluctant to pose sanctions on these countries and they may be held less accountable for how resource revenues are used or how they are reported in state budgets. In this thesis, both the effect of specific resources (e.g. oil, diamonds and agriculture) as well as the effect of different sub-categories of natural resources (diffuse and point resources) are examined.

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[2] Ross (2002) defines lootability as the ease of resource extraction and transportation. In his paper, he identifies alluvial gemstones, timber and agricultural products as lootable resources.

II. Empirical challenges

In the current literature, there seems to be an ongoing discussion about the existence of the resource curse and the results of research seem to be ambiguous. Arguably, part of the ambiguity regarding the empirical results is caused by the fact that the data used in most of these analyses have serious shortcomings. A well known shortcoming is the common resource variable, also used by Ross (2001) and Sachs and Warner (1997), which is primary exports of natural resources divided by a measure of national income (e.g. GDP). Brunnschweiler and Bulte (2008) state that, essentially, this is a measure of export dependence, not abundance (of for example oil rents). Concerning oil, it measures the amount of oil exports of a country, ignoring the amount of oil domestically produced and consumed. For large economies using a lot of oil domestically, such as the US, this might be a serious drawback. According to the theory, extracting oil is harmful because of the revenues it generates, and revenues can come from both domestic and foreign sales. Therefore data on oil production per capita is used in this thesis to better proxy the revenue flows coming from the oil sources. Another drawback of the oil-exports-to-GDP ratio, according to Ross (2008), is that this measure contains bias in both its numerator and denominator, since they are influenced by other variables of interest. For example, if two countries export the same amount of oil, the ratio will be larger in poorer countries, so when this oil-exports ratio is used, one is also measuring the size of a country's economy. These drawbacks may create a false correlation with level of democracy and autocracy. When using a different proxy for resource wealth, as done by Brunnschweiler and Bulte (2008) and Herb (2004) among others, one comes to different conclusions concerning the resource curse.

In order to address the problem of omitted variable bias, one could include fixed effects in the model, or a lagged dependent variable. With fixed effects, unobserved time invariant country specific effects are taken into account. When omitted unobservable variables are expected to be time-variant, including a lagged dependent variable in the model could be a method of taken these effects into account (Angrist and Pischke 2009). For example Horiuchi and Waglé (2008) choose to include both a lagged dependent variable and fixed effects in their natural resource model. In their work, current cross-country differences in political regimes are attributed to historic events. When they control for previous levels of democracy (by including a lagged dependent variable in the estimation) Horiuchi and Waglé do not find a significant effect of oil rents on democracy. Although the idea of Horiuchi and Waglé of taken into account previous levels of democracy (and persistency of institutions) seems very plausible, the implementation in their model might be flawed and causes their findings of a insignificant effect of oil on democracy. For example, they argue that their data on oil rents might not show sufficient within-country variation to actually find any significant effect on democracy. They aggravate this potential problem by using relatively long time periods (three steps from 1970 till 2006) in their model and thereby averaging out most of the variation. And to further complicate matters, data on oil rents is plagued by the fact that it is skewed; there are only a few big oil producers relative to a large number of non-oil producers. Furthermore, in their panel regression they use only two time observations and one could argue that the fixed effects estimator is biased when including a lagged dependent variable in a panel with a relatively small amount of time observations, in this case T=2 (Nickell, 1981). The direction of the bias depends on different aspects, such as the sign of the regressor and whether the regressor is exogenous (Nickell, 1981). Angrist and Pischke (2009) acknowledge the problem presented by Nickell (1981) and argue that it might not be such a good idea to both include fixed effects and a lagged dependent variable in a model. Although there are possible solutions to the problem, for example using an Anderson-Hsiao estimator or the estimator suggested by Arellano and Bond (1991), Angrist and Pischke (2009) argue that the cure may be in some cases worse than the "disease", i.e. these suggested solutions also have their flaws. Their practical guideline to address the problem is to look at estimations from both a model with fixed effects and one with a lagged dependent variable. According to them, these are the boundary cases of the effect of interest, and the true value will likely lie in between.

Another problem in the natural resource literature, next to possible omitted variable bias described above, is a reversed causality problem of natural resources. Both these issues are endogeneity problems, a notorious phenomenon in economic literature. Brunnschweiler and Bulte (2008) acknowledge the endogeneity problems in their paper, linking natural resources to economic growth (Brunnschweiler and Bulte, 2008[b]) and conflict (Brunnschweiler and Bulte, 2008[c]). They show that the endogeneity problem is indeed present and by using a different and perhaps more exogenous proxy for resource wealth variable, provided by the World Bank, they show that the resource curse disappears. Furthermore, instead of applying a conventional regression with the resource dependence variable assumed to be exogenous, they use an instrumental variable approach and show that there is no relation between resource dependence and slow economic growth and conflict. Coming back to the reversed causality problem; when linking oil production to level of democracy, countries with low democracy levels might have higher oil production due to the nature of the government to extract the rents and remain in position, which is only possible on a non-democratic environment. Formulated differently; because the country has a low level of democracy, the government controls oil production, hence determining the extraction rate of natural resources and thus the revenue flows going to the government. Also rent seeking behaviour in non-democratic countries might drive up oil production.

In this thesis some of the general problems mentioned earlier are addressed by, first of all, using panel regressions. Previous studies (e.g. de Soysa, 2002) mostly show cross-country regressions (non time variant), which focus on between country variation and are notoriously plagued by omitted variable bias. Focusing on within country variation would be a good way to tackle the omitted variable bias problem (Acemoglu et al, 2008) and could thus be a valuable addition to the existing literature linking natural resource wealth to democracy. This is done by investigating models with either fixed effects, or a lagged dependent variable, following up on the approach of Angrist and Pischke (2009) to compare both possibilities. Second, in stead of the common exports-to-GDP ratio, data on production of natural resources is used to better proxy the revenue flows coming from the natural resource. I will refer to this variable as “natural resource extraction” throughout the thesis. Last, the problem of possible endogeneity is discussed and addressed, although unfortunately no suitable instrument is found for natural resource extraction.

III. Data and Descriptive Statistics

A cross-country panel is constructed for the model, with nine five-year periods, the first period from 1960 until 1964 and the final period from 2000 until 2004. Since the panel is constructed from other existing datasets, some of the variables are averages over the five year period, where others are observations every fifth year. The dataset from the paper by Acemoglu, Johnson, Robinson and Yared (2008) is extensively used. In their paper on income and democracy, they explore the statistical association between level of income (measured by GDP) and level of democracy. Following up on earlier work done by Barro (1996), they show that introducing country fixed effects to the model, the significant relation between GDP and democracy disappears. Acemoglu et al. (2008) use non-averaged data and argue that this is preferred since 'averaging introduces additional serial correlation, making inference and estimation more difficult'.

The first and main measure of democracy is the per country Polity index from the Polity IV dataset. The variable measures a regime on a scale from minus ten (institutionalized autocracy) to plus ten (institutionalized democracy). It is a composite from the Polity's Democracy and Autocracy indices for each country. To be more precise; it is the difference between the two scores. Both indices range from zero to ten and both are scores based on competitiveness of political participation, the openness of and competitiveness of executive recruitment and constraints on the chief executive (Acemoglu et al, 2008).

When checking for robustness, the polity variable is replaced by a different measure of democracy, which is the Freedom House Political Rights Index. This index is derived from a checklist of questions such as whether there are free and fair elections, whether those who are elected rule, whether there are competitive parties or other political groupings, whether the opposition plays an important role and has actual power and whether minority groups have reasonable self-government or can participate in the government through informal consensus (Freedom House, 2004). For the regressions, both indices are transformed to lie between zero and one, where one corresponds to the most democratic set of institutions. This allows for easier comparison between both indices. Furthermore, both indices are taken from the dataset used by Acemoglu et al. (2008) and are thus non-averaged data.

As a proxy for natural resource abundance, production data on different natural resources from the dataset by Humphreys (2005) is used. In this dataset, data on oil production (total and per capita), oil reserves, diamond production and agricultural production (see data appendix A for detailed definitions and sources). Using these proxies for natural resource abundance differ from the commonly used natural resource variable, namely natural resource exports over GDP. As mentioned earlier, this variable would introduce bias (e.g. not measuring domestic sales) in any natural resource model, and therefore using production data will hopefully be an improvement compared to earlier research. Production would be a clear proxy to the extraction of natural resources in a country and thus indirectly a proxy to the actual resource rents flowing from a resource to elite groups and government. The data also allows for examining the effect of natural resource reserves on democracy. Reserves data are more a proxy of potential resource rents (e.g. oil resources available but not yet extracted). Since governments may have different strategies and optimal extraction rates (e.g. at a sustainable rate or snatching as much as they can) they may value future rents in different ways. It therefore might be more interesting to look at the current resource rent flows because these are the actual flows available for government, influencing decisions on tax rates and military expenditure. Furthermore, one has to be very cautious when using data on oil reserves. An article in the Energy Economist of January 8, 2008, points out that reported oil reserves have experienced dramatic increases in the past due to strategic behaviour of the OPEC countries after oil quota negotiations, and consequently: “If the 285 million barrel upward revisions of OPEC reserve estimates mid-1980s were just paper barrels, then OPEC reserves are overstated by almost 50% and world reserves are overstated by over 25%” (Williams, 2008). This means that the oil reserve data may not be the best data to use in the main estimations and therefore the focus will be on the production data. When checking for robustness, the production data will be replaced by the oil reserves data to check whether the results hold for an alternative measure of oil wealth.

The data on diamond production is taken from both the dataset by Humphreys (2005) and Lujala et al. (2005). Humphreys provides data on per capita diamond production, where Lujala et al. provide dummy variables about the production of primary and secondary diamonds. By using an interaction between the variables one can distinguish between the countries that have only secondary diamond production (e.g. Angola), only primary diamond production (e.g. Botswana) or production of both types of diamonds (e.g. the Democratic Republic of Congo).

The set of countries in the sample is divers and includes countries from all continents, both developing and developed countries, oil and non-oil producers and diamond producers (see data appendix for a list of the countries in the sample). The main model is estimated from a sample of 120 countries. An effort is made to keep the sample as equal as possible during the different regressions, in order to facilitate comparison and to avoid problems with sample selection bias. However, the sample may vary slightly due to the inclusion of different variables in the regressions. Table 1 provides a data summary of the sample of the main model, as well as the sample for the yearly panel.

Table 1. Data summary

Variable	**Mean**	**Std Deviation**	**Min.**	**Max.**
Sample for fixed effects oil model, **5 yearly panel**				
Polity Score	0.57	0.38	0	1
Freedom House Index	0.54	0.36	0	1
GDP per capita	8.19	1.07	5.77	10.41
Log of Population	9.21	1.47	6.14	14.05
Per capita oil production	0.01	0.05	0.00	0.71
Per capita oil reserves	0.08	0.33	0.00	13.08
Share of agriculture in GDP	22.48	15.69	0.19	74.32
Per capita diamond production	0.10	0.89	0.00	12.68
Primary diamonds production	0.07	0.78	0	12.43
Secondary diamonds production	0.02	0.15	0	2.45
Number of countries	**120**	**Years**	**1960-2000(4)**	**T=9**
Sample for fixed effects oil model, **yearly panel**				
Polity Score	0.55	0.38	0	1
Freedom House Index	0.53	0.36	0	1
GDP per capita	8.14	1.05	5.64	10.38
Log of Population	9.18	1.46	6.06	14.04
Per capita oil production	0.01	0.05	0.00	0.74
Per capita oil reserves	0.08	0.37	0.00	13.46
Share of agriculture in GDP	23.12	15.73	0.19	78.02
Per capita diamond production	0.09	0.81	0.00	13.31
Primary diamonds production	0.08	0.80	0	13.31
Secondary diamonds production	0.03	0.13	0	4.80
Number of countries	**134**	**Years**	**1960-1999**	**T=40**

A first glance at the data provides some information about the patterns that can be found in the data. Following up on the idea of Horiuchi and Waglé (2008) to graph the polity scores of the dataset over time, figure 1A and 1B below show the result. However, different from the graph presented in their paper, in this case the data is divided into oil (1B) and non-oil (1A) producers, in order to get a clear image of the pattern, where oil producers are defined in the data as having “non zero” and “non-missing” oil production. I find a similar pattern in the data to the pattern Horiuchi and Waglé present: it seems that in general, most countries have become more democratic since most observations are above the diagonal. In this first glance there seems to be little difference between oil and non-oil producers concerning the movement towards democracy over time. In fact, there are actually less countries under the diagonal (i.e. moving towards less democracy over time) in the figure for the oil-producers.

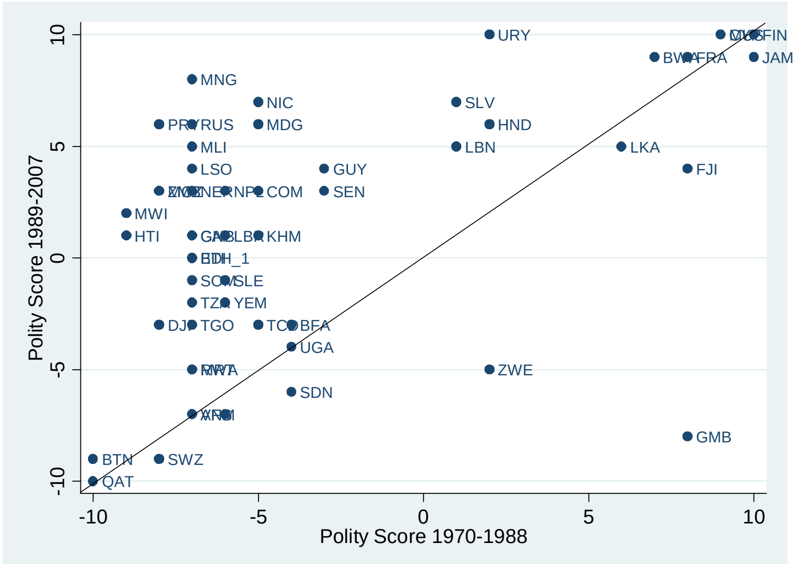

Figure 1A. Scatter plot of average polity scores for non-oil producers

Notes: The line in the graph is not a regression line, but a diagonal. Observations above the diagonal indicate that the average Polity IV Score for the period 1989-2007 was higher compared to the average Polity IV Score for the years 1970-1989 and when positioned under the diagonal, visa versa. Observations are marked with the country codes developed by the World Bank. Non-oil producers are defined as having zero oil production in the data.

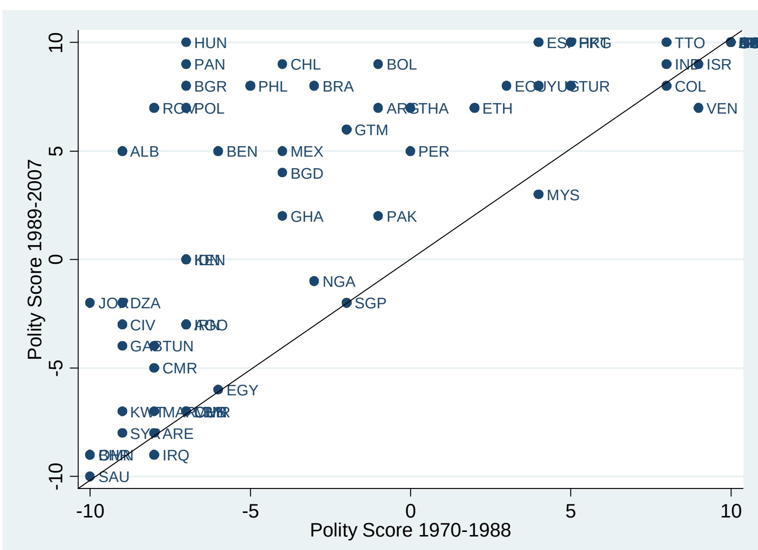

Figure 1B. Scatter plot of average polity scores for oil producers

Notes: see notes to Figure 1A. Oil producers are defined as having non-zero and non-missing oil production in the data.

IV. Econometric model

Consider the following econometric model, which will be the basis for the majority of the estimations:

$$p_{it} = \alpha_i + \mu_t + (\gamma p_{it-1}) + \lambda y_{it-1} + x'_{it-1}\beta + \varepsilon_{it} \qquad (1)$$

Where

p_{it} is the democracy score for country *i* in period *t*,

α_i is the country specific fixed effect,

μ_t is a year dummy,

p_{it-1} is the lagged value of the democracy score,

y_{it-1} is natural resource extraction variable for country *i* in period *t-1*,

x_{it} are controlling variables for country *i* in period *t*,[3]

and

ε_{it} is the idiosyncratic error term.

The main variable of interest is the lagged value of the lagged natural resource extraction variable y_{it-1}. Its corresponding parameter λ measures the effect of natural resource extraction on democracy. Natural resource extraction will be proxied by production data of different types of natural resources (oil, diamonds and agriculture).

The fixed effects α_i capture all time-invariant country specific elements (reducing the potential omitted variables problem) and the time effects μ_t capture the common shocks (or common trends) to the democracy score of all countries. The error term ε_{it} picks up other omitted factors for all *i* and *t,* and is assumed to be IID.

The lagged value of the dependent variable on the right-hand side, denoted between brackets, is included to capture persistency in democracy. Acemoglu et al. (2008) argue that the lagged dependent variable captures mean-reversion of dynamics; "the tendency of the democracy score to return to some equilibrium value for the country". This variable is not included in the models with fixed effects since the standard fixed effects estimator is said to be biased (Nickell, 1981), when T is small and fixed. However, when the number of time periods in the sample are very large (i.e. when $T \rightarrow \infty$) the fixed effects estimator becomes consistent (Nickell, 1981). Unfortunately, the literature provides little practical guidance about when exactly T is large enough

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[3] some of the controlling variables x_{it} are lagged values, if it has practical meaning to prefer the lagged value over the current value of t.

for OLS to become consistent. In the 5-year panel, T=9 it is likely that the results are biased when the lagged dependent is included, if one considers T=9 to be "small". Therefore, as mentioned earlier, the choice is made to follow up on the practical guidelines of Angrist and Pischke (2009) to not to include both in a single model, but to compare the estimations of two different models.

A second econometric model that will be used to estimate the effect of natural resources on democracy is as follows:

$$\Delta p_{it} = \mu_t + (\gamma \Delta p_{it-1}) + \lambda \Delta y_{it-1} + \Delta x'_{it-1}\beta + \Delta \varepsilon_{it}, \tag{2}$$

where all the parameters are corresponding to the parameters in (1) and the Δ prefix denotes the change from one year to the next, i.e. $\Delta p_{it} = p_{it} - p_{it-1}$.

This econometric model is different from (1) in the sense that the variables are now expressed in terms of "change over time", whereas (1) expresses the variables in levels, e.g. level of oil production and level of democracy. Model (2) is also known as a "first difference" model, since it expresses the variables in a change from period t-1 to t, i.e. the first difference. Differencing removes the country specific fixed effect and is therefore a well-known estimation model for panel data (Angrist and Pischke 2009). This model has another advantage. In case the parameters in model (1) are not stationary over time, e.g. they have a unit root, the results might be driven by a spurious regression. First differencing changes possibly non-stationary variables into stationary variables, in case of a unit root. If the similar results can be found in a first differenced model, non-stationarity of variables should not be a problem; it is not a spurious process that drives the results.

In practice, it will be interesting to look at both a model expressed in levels and a model expressed in changes, since natural resource extraction, proxied by the production data, can possibly have an effect on either the level of democracy or a change in democracy or both.

V. Fixed Effects Estimates

A. Main results

The empirical results are now presented. In the first column of table 2 a fixed effects model is estimated, including yearly time dummies, a constant[4] and only lagged oil production as a regressor on level of democracy. The coefficient for oil production is negative and highly significant, which indicates the presence of a resource curse. In column 2, two other regressors as control variables[5] are added, namely (the log of) real GDP per capita and (the log of) population size. The lagged level of GDP is used in order to reduce possible endogeneity problems. There seems to be no effect of these two variables on level of democracy, which is consistent with the earlier study of Acemoglu et al. (2008). The oil production variable is reduced in size and significance once controlled for GDP per capita and population size, but there remains evidence of a resource curse. The same procedure for two other types of natural resources is repeated, namely for diamonds and agriculture. In the case of lagged diamond production, it looses significance once accounted for GDP and population size. The effect of agriculture on democracy in both regressions seems zero; the beta coefficients are very close to zero, both with very small standard errors. From the first glance at the data, there seems to be evidence of a resource curse for oil only.

Looking at the magnitude of the λ coefficients, it seems that the negative effect of oil in column (2) is a lot bigger than the effect of diamonds in (3), namely -0.356 and -0.014 respectively. This does not come as a surprise since the rents in oil are likely to be much larger compared to rents from diamonds, due to the difference in amounts of resources. Oil is traded in large quantities and diamonds in much smaller quantities. However, the coefficient for oil in itself seems to be rather large. A 1 percent increase in the oil rents per capita will decrease the level of democracy with approximately 0.3, which is quite a lot on a scale between 0 and 1. The effect is seems large due to the fact that the oil production variable is measured per capita. A 1% per capita increase

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[4] I included a constant, although it might not be necessary in addition to the country fixed effects and time dummies. I included it to show the difference between a model with just natural resource abundance and a model including GPD and population as controls. The significance of the constant disappears when controlling for GDP and population.

[5] I ran these regressions with several control variables that are time and country specific, such as level of education, military spending by the government and country tax rates. Oil production remained significant in the case of tax rates. Education level didn't have an effect on democracy but the resource curse lost its significance in this regression. The same goes for military expenditure. However, these control variables reduced the sample size drastically due to lack of data on certain countries and years, making comparison between different regressions impossible (in the case of education, the sample size was reduced to 90 countries and for military expenditure, T was reduced to 3). These results therefore likely have sample selection bias. I thus chose only to include the control variables of which I have a reasonable amount of data. The model with tax rates is reported in the robustness section.

in reality likely corresponds with a rather large increase in actual production, especially in the case of a large population. In practice, the estimated effect would mean going from the level of the Dominican Republic (which has a Polity Score of 0.9 in 2000) to the level of Djibouti (Polity Score of 0.6 in 2000), or going from Kyrgyzstan (0.35 in 2000) to Oman (0.05 in 2000). Equivalently, the effect could also be interpreted as a change over time, for example the change in level of democracy in Mexico from the year 1980 (Polity Score of 0.3) to the year 1995 (Polity Score of 0.7).

Brunnschweiler and Bulte (2008) remark the following about existing analyses of the resource curse: "A key problem is that the resource variable used in cross-country regression models is endogenously determined" meaning that any resource variable itself is not invariant with respect to economic growth, conflict or the level of democracy – biasing existing empirical research. As mentioned earlier, oil production therefore might be endogenous to level of democracy (e.g. governments determine the level of oil production and less democratic governments might use this power strategically to remain in position). Using an instrumental variables approach could deal with endogeneity for the oil production model. Unfortunately, experimenting with a wide variety of possible instruments (e.g. oil prices[6,] oil discoveries, natural disasters etc.), did not produce an instrument with any explanatory power. It seems very difficult to find a source of exogenous variation that can explain oil production in a country[7]. Therefore lagged levels of oil production, to at least consider endogeneity problems, are used in the models throughout the thesis.

[6] The time dummies will control for yearly shocks, such as demand shocks, filtering out the price effect of oil. I tried using an interaction between a country specific, time varying variable (e.g. openness to trade) and the oil price, but unfortunately also this instrument did not have any explanatory power.

[7] I choose not to report the empirical results of the IV approach since using a bad instrument will do more harm to the model than it will do good, making it very difficult to interpret these results.

Table 2. Fixed effects regression results using Polity IV data and Natural resource abundance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Dependent variable is Polity IV Score							
GDP per capita $_{t-1}$		-0.025		-0.022		-0.084	-0.072
		(0.050)		(0.052)		(0.059)	(0.061)
Log of Population		0.009		0.023		0.087	0.088
		(0.056)		(0.057)		(0.087)	(0.089)
Oil production $_{t-1}$	-0.356***	-0.326**					-0.244*
	(0.124)	(0.140)					(0.141)
Diamond production $_{t-1}$			-0.014**	-0.013			-0.013
			(0.006)	(0.008)			(0.009)
Agricultural production $_{t-1}$					-0.003	-0.004	-0.004
					(0.002)	(0.003)	(0.003)
Constant	0.478***	0.594	0.498***	0.457	0.600***	0.660	0.566
	(0.021)	(0.548)	(0.025)	(0.571)	(0.094)	(0.974)	(1.009)
Observations	784	784	781	781	646	646	646
Countries	120	120	119	119	115	115	115
R-squared within	0.25	0.25	0.25	0.25	0.24	0.25	0.25
R-squared overall	0.07	0.02	0.08	0.02	0.20	0.00	0.01
F test	8.57	7.22	8.81	7.30	6.81	6.08	5.20

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For all regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

Table 3 investigates whether the model holds when the panel is replaced by a yearly panel. In stead of taking the average over periods of 5 years (T=9), the data is now altered to a yearly panel and thus the amount of time observations (T=40) is drastically increased. The results in table 3 are remarkably similar to the results reported in table 2. Once again, the resource curse appears for oil production. The negative effect of diamond production disappears when controlled for GDP per capita and population and the effect of agriculture on democracy seems to be zero.

Table 3. Fixed effects results using yearly panel data on Polity IV Score and natural resource extraction

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Dependent variable is Polity IV Score						
L.GDP per capita		-0.027		-0.028		-0.050
		(0.049)		(0.050)		(0.057)
Log of Population		-0.027		-0.012		0.141
		(0.103)		(0.103)		(0.119)
Oil production	-0.336***	-0.318**				
	(0.100)	(0.130)				
Diamond production			-0.012**	-0.009		
			(0.005)	(0.006)		
Agricultural production					-0.002	-0.002
					(0.002)	(0.002)
Constant	0.511***	1.215	0.510***	1.084	0.599***	-0.170
	(0.023)	(1.134)	(0.023)	(1.159)	(0.065)	(1.339)
Observations	3907	3907	3907	3907	3188	3188
Countries	134	134	134	134	129	129
R-squared within	0.21	0.22	0.21	0.21	0.20	0.21
R-squared overall	0.06	0.00	0.06	0.00	0.16	0.00
F test	3.05	3.24	3.04	3.28	2.83	2.93

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For all regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

In the next table, table 4, the consequence of introducing a lagged dependent variable in the model is explored. As mentioned earlier, proponents of including a lagged dependent variable in the model (Acemoglu et al., 2008 and Horiuchi and Waglé, 2008) argue that it is necessary to capture persistency in democracy and to take into account the tendency of democracy to revert to a mean. Table 5 presents a model for the 5-yearly and yearly panel where a lagged dependent variable on the right hand side is included. Following up on the guidelines presented by Angrist and Pischke (2009), the results of a model with a lagged dependent can be compared to the fixed effects results that were found in the previous table. The estimated effect of oil on democracy in the lagged dependent model is much smaller, and maybe even more realistic, than the effect found in the fixed effects model. According to Angrist and Pischke (2009), these are boundaries of the true effect, which is likely to be in between these values. Interestingly, there seems to be a negative effect of oil extraction on the level of democracy in both cases.

Table 4. Fixed effects results using yearly panel data, and 5-yearly panel data on Polity IV Score and oil production, including a lagged dependent variable (*t*-1)

	(1) b/se	(2) b/se	(4) b/se	(5) b/se
Panel	Yearly panel		5-yearly panel	
Dependent variable is Polity IV Score				
Polity IV Score $_{t-1}$	0.949***		0.747***	
	(0.006)		(0.030)	
Oil production $_{t-1}$	-0.040***	-0.318**	-0.137*	-0.327**
	(0.013)	(0.130)	(0.078)	(0.144)
GDP per capita $_{t-1}$	0.012***	-0.027	0.051***	-0.029
	(0.002)	(0.049)	(0.011)	(0.053)
Log of Population	-0.001	-0.027	0.002	0.001
	(0.001)	(0.103)	(0.005)	(0.069)
Constant	-0.075***	1.215	-0.311***	0.988
	(0.021)	(1.134)	(0.083)	(0.720)
Observations	3854	3907	745	783
Countries	134	134	120	120
R-squared within		0.22		0.25
R-squared overall		0.00		0.01
Adjusted R-squared	0.95	0.21	0.76	0.24
F test	6650.96	3.24	470.27	7.14
Fixed effects	NO	YES	NO	YES

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For all regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

Table 5 presents the results of the first difference model, where the focus is on oil production. The results are similar to the fixed effects results. The estimated effect is actually quite similar to the size of the effect in the previous table. One has to keep in mind that the OLS estimator in column (3) and (6) is not consistent due to the inclusion of the lagged dependent variable, as mentioned earlier. It seems that especially the 5-yearly panel, where the lagged dependent is significant, suffers from some bias, since the effect of the change in oil production is suddenly somewhat bigger (it seems overestimated). Unlike the model expressed in levels, where the lagged dependent was significant for the yearly panel, in the first differenced model, the lagged dependent variable is not significant and the estimated effect of oil is similar to the previous models[8]. The fact that the lagged dependent variable is not significant here does not come as a surprise. In many cases, regimes do not change drastically every year, so the step of just one year earlier is probably too small a step to explain current regime changes.

The sign of the lagged dependent variable is interesting in this case. It is negative, indicating that a change in the previous period leads to less change in the following period. It describes the tendency of regimes towards stability.

Since the results in the first differenced model are similar to the results when looking at levels of the variables, it can be concluded that potential problems with stationarity

[8] Nickell (1981) points out that when $T \rightarrow \infty$ the OLS estimator becomes consistent. Therefore models with a very large T suffer less from bias due to the lagged dependent, than models with a small amount of time observations. This effect can also be noticed in the result. The estimations in the yearly panel seem less affected by the lagged dependent variable than the estimations for the 5-yearly panel.

do not determine the results. In case of a unit root, these results seem not be driven by a spurious process between oil extraction and democracy.

During the estimation of the first difference model, also a model with the change in democracy on the left hand side, the levels of all the variables on the right hand side and a lagged dependent variable on the right hand side was tested. This model takes on the form of an error correction model, since the estimated effect of the lagged dependent variable was negative. Such a model focuses on the long term relation between the variables of interest. The results in this model are very similar to the result found previously; there was a negative effect of oil extraction on the change in democracy. However the interpretation of such a model is not straightforward, furthermore it does not allow for fixed effect and since the results are similar I decide not to report these results here (they are available upon request).

Table 5. Differenced regression results using Polity IV data and oil production

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Panel		5 Yearly panel			Yearly panel	
Dependent variable is change of the Polity IV score						
Change in oil production $_{t-1}$	-0.271**	-0.315**	-0.405***	-0.261**	-0.291***	-0.240**
	(0.122)	(0.132)	(0.134)	(0.104)	(0.111)	(0.100)
Change in GDP per capita $_{t-1}$		0.008	-0.016		-0.003	-0.003
		(0.053)	(0.058)		(0.034)	(0.035)
Change in log of Population $_{t-1}$		-0.032*	-0.012		-0.083	-0.087
		(0.017)	(0.023)		(0.100)	(0.101)
Change in polity score $_{t-1}$			-0.210***			-0.022
			(0.049)			(0.022)
Observations	737	651	614	3830	3723	3673
Adjusted R-squared	0.05	0.04	0.08	0.01	0.01	0.01
F test	5.77	4.52	5.47	1.79	1.70	1.72

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For all regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

In the following section, the focus will be on the effect of diamonds on democracy, and it is tested whether there is a difference between the effects of point and diffuse resources on democracy.

When splitting up the diamond production data into primary and secondary diamond production (also called kimberlite and alluvial diamonds resp.), a difference between the effects of both resources on democracy would be expected. Primary diamonds occur in underground rock formations and mining them requires large investment costs and risks, whereas secondary diamonds are easier to find and can be exploited with simple tools such as shovel and sieve (Lujala et al. 2005). Hence primary diamonds are defined as a point resource, whereas secondary diamonds are defined as a diffuse resource. According to Lujala et al. (2005) “revenue from primary diamonds accrues to the government and puts it in a stronger position to buy support, defend the state and crush possible uprisings”. Table 6 reports the findings, once this is put to the test. As the case with oil production, working through the same mechanisms, there might be an endogeneity problem with diamond production as well (section II).

However, possible endogeneity might vary across diamond types. It is difficult to determine beforehand which types of diamonds are more sensitive to endogeneity. Governments and elite groups might have easy access to primary diamonds, and may determine their production level, whereas rebel groups are likely to have their share in production of secondary diamonds.

In column 1 the diamond production is divided into primary and secondary diamond production and used as regressors on the level of democracy, including a constant, time dummies and fixed effects. Interestingly, a difference between primary and secondary diamonds is found supporting the idea of a resource curse due to the nature of the natural resource. Strangely enough, the size of the estimated effect of both types is diamonds seems to be similar (around 0.012) and only the standard error differs. Furthermore, the result doesn't seem to be robust to including control variables. Once controlled for GDP per capita and population size in column 2, although both controls are not significant, the effect disappears. The coefficient of the primary diamonds only differs slightly and the coefficient for secondary diamonds does not change at all, once controlled for GDP and population. The F-test actually shows that, although it is still significant (P-value of 0.0000), the controls do not improve the model at all. They might even cause additional problems (e.g. possible correlation between diamond production and GDP or endogeneity of GDP with democracy). Therefore based on these models, it is difficult to conclude anything about the existence of the resource curse for point resources[9].

Table 6. Fixed effects regression results using Polity IV data and primary and secondary diamond production

	(1) b/se	(2) b/se
Dependent variable is Polity IV Score		
Primary diamonds $_{t-1}$	-0.013***	-0.012
	(0.004)	(0.007)
Secondary diamonds $_{t-1}$	-0.012	-0.012
	(0.071)	(0.072)
GDP per capita $_{t-1}$		-0.018
		(0.056)
Log of Population		0.014
		(0.060)
Constant	0.499***	0.762
	(0.025)	(0.689)
Observations	764	764
Countries	118	118
R-squared within	0.24	0.24
R-squared overall	0.08	0.03
F test	7.21	6.00

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For both regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

[9] I also examined models of diamond production with a lagged dependent variable in stead of fixed effects, both for the yearly and 5-yearly panel. The results are very ambiguous and therefore I chose not to report them. The hypothesis of point versus diffuse resources does not seem to hold very well for different models.

There seems to be evidence of a resource curse of oil on democracy, but there is only weak evidence for the resource curse of primary diamonds. The hypothesis that the resource curse only exists for point resources may either be too simplistic, or the diamond production data is not suitable for showing a difference between both types of diamonds. For example, the distinction between point and diffuse resources may not be very clear-cut in some cases. Secondary diamonds can for example be found in very concentrated areas, whereas mining sites for primary diamonds can be very widespread. It could thus be likely that in some cases secondary diamonds do not have the characteristics of a diffuse resource and primary diamonds not of point resources.

Also likely is that other factors play a role in determining the difference between the effects of different resources. For example, diamonds and oil may differ in whether they are a strategic resource or not. Oil seems to perfectly fit the profile of a strategic resource; many economies are very dependent on oil, only a few countries produce it and there are no direct substitutes available (yet). Primary diamonds are less strategic, since secondary diamonds are substitutes and most economies do not depend on them. However, the weak evidence of primary diamonds do reflect the fact that geographic concentration may play an important role in the negative effect of natural resources on democracy.

B. Robustness

Table 7 investigates whether the results on the ‘oil curse’ are robust. Column 1 is the model as proposed in table 2. In column 2 the dependent variable is replaced (formerly measured as the Polity IV score) with an alternative measure of democracy, namely the Freedom House Index. In column 3 the oil production variable is replaced with a variable measuring oil reserves. Although the data on oil reserves data is criticized (see previous section), it might be useful to see whether the results hold with an alternative measure of oil wealth. The coefficient for oil reserves seems to be somewhat smaller compared to production, but it remains significant. In column 4 both alternative measures are used, and again the conclusion so far doesn’t change[10].

[10] I also checked whether my results hold when using alternative samples. For example, I excluded the high oil producers, former socialist countries and western countries. Except for excluding western countries, the results remain the same. When I look at different subsamples, I again drastically reduce the sample which makes it difficult to compare regressions and draw conclusions. I choose therefore not to report them here.

Table 7. Fixed effects results using alternative measures for democracy and oil

	(1)	(2)	(3)	(4)
	b/se	b/se	b/se	b/se
Dependent variable	Polity IV Score	Freedom House I	Polity IV Score	Freedom House I
GDP per capita $_{t-1}$	-0.025	0.018	-0.026	0.014
	(0.050)	(0.047)	(0.049)	(0.046)
Log of Population	0.009	-0.060	0.018	-0.058
	(0.056)	(0.055)	(0.056)	(0.054)
Oil production $_{t-1}$	-0.326**	-0.292***		
	(0.140)	(0.087)		
Oil reserves $_{t-1}$			-0.094***	-0.080**
			(0.032)	(0.033)
Constant	0.594	0.855	0.546	0.870
	(0.548)	(0.596)	(0.539)	(0.593)
Observations	784	802	781	799
Countries	120	120	119	119
R-squared within	0.25	0.12	0.26	0.13
R-squared overall	0.02	0.04	0.01	0.03
F test	7.22	5.69	7.42	5.05

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. For all regressions the F-test is significant on a 1% level. In all regressions year dummies are included.

Table 8 reports the results when I control for country tax rates in the model, where tax rates are measured as highest marginal tax rate on corporations, which are thus an indication of the tax environment in a country. According to rentier state theory, countries with high oil rents are less dependent on tax income, which reduces accountability to the inhabitants of a country, and therefore might reduce the level of democracy. Hence, it is interesting to see whether the oil results hold, once controlled for tax rates. Although the coefficient of the oil production variable is somewhat lower than in the main model, the results remain the same, also for diamond production and agricultural production.

During estimation, an interaction term between taxes and the production data was included in the models. This was done to test whether the natural resource curse may be different, given the level of taxes in a country. The link between natural resources and institutions is examined in existing literature from Mehlum, Moene and Torvik (2006) and Robinson, Torvik and Verdier (2006). The latter for example suggest that political processes are dysfunctional in the context of natural resource rents, but that through appropriate institutions this negative effect might be remedied. However, the question remains which institutions in particular are important in the context of natural resources.

Following up on the idea of the natural resource curse having a negative effect on democracy, given the 'level of institutions', I tested this effect with country tax levels. The results show that the effect 'through institutions' does not seem to be present in the data since the interaction terms were not significant in the models (not reported). However, it must be mentioned that the data on tax rates itself might be flawed and does not reflect the actual tax rates the voter is facing in every day life.

Further inspection of the tax data shows that corporate tax levels vary quit a lot among oil producers. Where some countries have, in line with rentier state theory, very low tax rates, others have very high corporate tax rates. This might be due to the different methods of collecting resource rents by governments. Where governments are directly owner of the oil producing companies, they do not have to raise high corporate taxes in order to collect the resource rents. In other cases, governments might not be the direct owner and collect the resource rents by imposing very high corporate taxes. Unfortunately, more appropriate data on country tax levels varying over time does not seem available; therefore I choose to look at corporate tax rates as a proxy of the tax environment in a country, although it might not reflect the actual tax environment in all cases.

Table 8. Fixed effects results including country tax rates

	(1) b/se	(2) b/se	(3) b/se
Dependent variable is Polity IV Score			
GDP per capita $_{t-1}$	-0.013	-0.006	-0.048
	(0.064)	(0.074)	(0.083)
Log of Population	-0.021	-0.002	0.120
	(0.116)	(0.127)	(0.152)
Country tax rate $_{t-1}$	0.030	0.029	0.029
	(0.020)	(0.020)	(0.020)
Oil production $_{t-1}$	-0.228***		
	(0.078)		
Diamond production $_{t-1}$		-0.005	
		(0.010)	
Agricultural production $_{t-1}$			-0.001
			(0.005)
Constant	0.964	0.740	0.093
	(1.146)	(1.374)	(1.524)
Observations	425	424	360
Countries	94	94	91
R-squared within	0.14	0.14	0.16
R-squared overall	0.00	0.00	0.04
F test	2.28	2.44	1.68

Notes: *,**, ***, statistically significant at 10%, 5% and 1% levels, respectively. Robust standard errors in parentheses. In (1) and (2) the F-test is significant on a 5% level, for (3) it is significant on a 10% level. In all regressions year dummies are included.

VI. Conclusions and discussion

The regression tables, presenting the effects of oil extraction on democracy, seem to show that there is a negative effect for oil, which is present in all the models. It holds for a model with the levels of democracy and oil production, as well as a first differenced model. The negative effect is present in a fixed effects model, as well as a model with a lagged dependent variable[11]. Including many different control variables does not change the result, neither does using different oil and democracy measures. From these results it seems that there is a negative effect present in the data that cannot be ignored.

Although the resource curse seems to be persistent in the oil data, for diamonds it is more ambiguous and event absent for the agricultural produce. When splitting up diamond production in primary and secondary diamonds, there seems to be hardly any evidence that the resource curse only exist for primary diamonds. The hypothesis was that primary diamonds, as well as oil, are considered to be a point resource and that governments and elite groups can easily control these resources and their rents, due to its geographic concentration. Point resources thus seem to be bigger threat to democracy than diffuse resources, such as agriculture and possibly secondary diamonds. However, the evidence for this doesn't seem to be robust. As mentioned earlier, it is possible that the data on diamond production is imperfect and the division between what is a point resource and diffuse resource is not clear-cut. The hypothesis is constructed to explain why we find different effects of different resources, but to that question I do not find an answer. It would be worthwhile to further investigate the difference between resources, and to be able to explain why oil is such a "special" resource.

Furthermore, in this thesis the problem of reversed causality of natural resource extraction on democracy is unfortunately not solved. Finding an instrument for oil production, or diamond production for that matter seems to be a difficult task and using a bad instrument only aggravates the potential problem. In the regressions lagged levels of production were used to address the endogeneity problem due to the lack of an instrument with enough explanatory power. However, this is not the best way of dealing with this potential problem. More research on finding a good instrument for oil production might be interesting for the discussion on the resource curse. It would also be interesting to further look at the endogeneity bias and estimate its magnitude on the results. This would allow taking it into account when interpreting the results.

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[11] The negative effect of oil production is not present when both fixed effects and a lagged dependent variable are included. However, in this case the OLS estimate is not consistent. Furthermore, fixed effects remove a lot of the bad and good variation (Angrist and Pischke 2009), and the lagged dependent does that as well. By including both, one "kills" the negative effect. It does not seem to be a wise thing to do. However, the negative effect of oil reserves does seem to, unlike the production data, "survive" a model with both fixed effects and a lagged dependent variable. From this I can only conclude that somehow there seems to be negative effect of oil present in the data that easily survives within in the boundaries of what is a "reasonable" econometric model.

Another point of discussion is about the appropriateness of using a panel regression on the resource curse. The mechanisms of how natural resources influence governments seem to be very complex. The rentier state theory is only a small part of possible explanations for the resource curse. Is it possible to capture these complexities with a panel regression? Furthermore, can we generalize our findings to all countries? It is very likely the influence of resource rents differ per case and per country and maybe a global regression will not give insight into these diverse mechanisms. Furthermore, the resource curse may only be existent given a certain characteristic of a country. In for example the work of Mehlum, Moene and Torvik (2006) the resource curse is linked to institutions and they look for a deeper understanding of natural resource curse. However, trying to explain why there are differences in the effect of natural resources in different countries, they unfortunately do not unbundle institutions. It might be helpful to investigate the natural resource curse case by case and then be able to explain the differences between Norway and Nigeria.

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Variable	Description	Source
Polity Composite Democracy Index, also referred to as Polity score	Data for 1960-2000 taken from the Polity IV data. The composite index is the democracy score minus the autocracy score. The original range is from -10,..10 but normalized here to 0-1. The score is not averaged over each 5-year period but the score is taken for each beginning year of the period. Countries for which data is are not available for 2000 are assigned the data for 1995	Taken from dataset by Acemoglu (2008), originally from: http://www.systemicpeace.org/polity/polity4.htm
Freedom House Political Rights Index, also referred to as Freedom House index	Data for 1970-2000 in Freedom House Political Rights Index, original range from 1,...7 but normalized to 0-1. Data for 1960 and 1965 taken from Bollen (2001), original range 0.00, 0.01,...1.	Taken from dataset by Acemoglu (2008), originally from: http://freedomhouse.org/ratin

		gs/, and Bollen (2001), "Cross National Indicators of Liberal Democracy 1950-1990" available on ICPSR
GDP per capita (chain weighted 1996 prices)	Data for 1960-2000 measured as log real GDP per capita (chain method in 1996 prices) from Penn World Tables 6.1	Taken from dataset by Acemoglu (2008), originally from: http://pwt.econ.upenn.edu/
Population	Log of total population in thousands	Taken from dataset by Acemoglu (2008), originally from: World Development Indicators, World Bank (2003)
Oil production per capita	Barrels per person per day, average for every five year period	Humphreys 2005
Oil reserves per capita	Thousand barrels per person	Humphreys 2005
Agricultural production as a share of GDP	Value of agricultural production in GDP, in percentages	Humphreys 2005, but originally from: Word Development Indicators, World Bank (2003)
Diamond production per capita	Carats per person	Humphreys 2005
Primary diamond production	Dummy variable which takes the value 1 if there is primary diamond production in the country and zero if otherwise, multiplied with the diamond production variable	Dataset from Lujala et al. (2005) but originally from Gilmore (2004) and Gilmore, Gleditsch, Lujala and Rød (2004)
Secondary diamond production	Dummy variable which takes the value 1 if there is secondary diamond production in the country and zero if otherwise, multiplied with the diamond production variable	Dataset from Lujala et al. (2005) but originally from Gilmore (2004) and Gilmore, Gleditsch, Lujala and Rød (2004)
Country tax rate	Data for 1960-2002, data refer to the top marginal tax rate on corporations, scaled to lie between 0 and 10. Different rates apply on non-resident/ foreign-owned companies. Provincial and local governments may levy additional taxes. In addition, the effective corporate tax rate may be higher due to the imposition of corporate level taxes on dividend or other distributions.	Available at the website: http://www.bus.umich.edu/OTPR/otpr/introduction.htm

Data appendix

A. Description of the variables

B. List of countries in sample

Algeria, Angola, Argentina, Armenia, Australia, Austria, Azerbaijan, Bangladesh, Belarus, Belgium, Benin, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Central African Republic, Chad, Chile, China, Colombia, Congo Democratic Republic, Cote d'Ivoire, Cuba, Cyprus, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Korea, S., Kyrgyzstan, Latvia ,Lesotho, Lithuania, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Moldova, Morocco, Mozambique, Namibia, Nepal, Netherlands, New Zealand , Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Romania, Russia, Rwanda, Senegal, Sierra Leone, Singapore, Slovakia, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syria, Tanzania, Thailand, Togo, Trinidad And Tobago, Tunisia, Turkey, Uganda, Ukraine, United Kingdom, United States, Uruguay, Uzbekistan, Venezuela, Zambia, Zimbabwe.