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The Resource Curse and its Potential Reversal

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Summary. — Several recent papers suggest that the negative association between natural resource intensity and economic growth can be reversed if institutional quality is high enough. We try to understand this result in more detail by decomposing the resource measure, using alternative measures of both resources and institutions, and by studying different time periods. While an institutional reversal is present in many specifications, only ores and metals interacted with the ICRG measure of institutional quality consistently have a negative growth effect but a positive interaction that turns the curse around when institutions are good enough.

Key words — natural resources, minerals, fuels, resource curse, property rights, institutions, economic growth, development

1. INTRODUCTION

Even if it seems clear that there is a robust negative relationship between a country's share of primary exports in GDP and its subsequent economic growth, it seems equally clear that there are plenty of exceptions to this general pattern. [1] In the recent past natural resources have been positive for economic growth in countries such as Australia, Botswana, Canada, and Norway, and historically there are also many examples of resource led growth. [2] As Frederick van der Ploeg (2011) notes in a recent overview, "*the interesting question is why some resource rich economies [.] are successful while others [.] perform badly despite their immense natural wealth*."

Recent work by Mehlum, Moene, and Torvik (2006) suggests that the answer lies in differences in institutional arrangements across countries. When institutions are "grabber friendly" resources push aggregate income down, while resources under "producer friendly" institutions raise income. Similarly, Boschini, Pettersson, and Roine (2007) propose that the extent to which natural resources are good or bad for growth depends on their "appropriability" in two dimensions. First, natural resources do not, by themselves, harm growth, but become a problem in the absence of good institutions (*institutional appropriability*) and second, for some types of resources this problem is bigger than for others (*technical appropriability*). Both these studies find empirical support for the basic idea that resources can have positive effects on growth given that institutions are good enough, emphasizing the interaction effect between these variables. [3]

The main purpose of this paper is to analyze the interaction effect and its possibility to reverse the resource curse in greater detail. Starting from a basic regression of the type used in Mehlum *et al*. (2006), which focuses on interacting a broad resource measure (primary exports in GDP) with a composite measure of a particular dimension of institutional quality, we study to what extent we can add precision to their argument by decomposing the result with respect to (i) the types of resources, (ii) the measure of institutional quality used, and (iii) different time periods. We also discuss problems with the various econometric specifications that one could use to test the idea of an institutional reversal of the curse. In particular, it seems natural to consider using the panel structure of the data, especially to include country fixed effects. This, however, turns out to be problematic as there is not enough variation in the institutional measures over time and also because important level effects in institutional quality would be captured by the country fixed effect. The alternative that we instead explore is to use pooled OLS (and IV) regressions with time effects, including lagged values of both dependent and explanatory variables. This at least partly addresses some important concerns: First, time effects account for what previously was an omitted variable; second, including lagged values (of the variables of interest) reduces the endogeneity problems in the original specification; and, third, including the lagged value of growth itself accounts for the autoregressive properties of the growth process. [4] We use both 5-year averages and a yearly panel with different lag structures and discuss the relative merits of each.

The reasons for attempting to "unbundle" the resource curse, and in particular its reversal, can be found in previous research. With respect to *types of resources* it has been argued that the severity of the resource curse depends on the kinds of

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resources that are important in a country. In particular, what has been labeled "point-source" resources, such as plantation crops and minerals and fuels, have been suggested to be more problematic than "diffused" ones. The basic argument is that point-source resources, characterized by being more "centrally controlled", generate rents that are more easily appropriable.[5] An alternative argument is that they cause more societal division and weaker institutions, which in turn lead to lower growth.[6] Yet another related argument is that labor-intensive resources should be expected to have different effects compared to capital-intensive resources through their differences in the likelihood of causing conflict.[7] If it is the case that different types of resources contribute differently to the resource curse itself, it certainly seems interesting to see if this is also the case for its reversal. Throughout our analysis we are concerned with "extracted resource wealth" (rather than reserves) in the form of resource rents or, alternatively, resource exports. We discuss this in more detail in Section 2.1 below.

With respect to the composite *measure of institutional quality* (used in Mehlum *et al*. (2006) and in Boschini *et al*. (2007)) there are several reasons for trying to understand which parts of this are driving the result, as well as to what extent the interaction effect varies across types of institutional measures. It is important to note that our primary concern is not to compare "different measures of the same thing" but rather to see if different aspects of institutional quality play different roles in the potential reversal of the resource curse. We make use of two dimensions according to which the literature has discussed measures of institutional quality. One is the division between "rules" and "institutional outcomes". Many have pointed out that some often used measures of institutions, for example the ICRG measures of institutional quality, actually reflect actions (or restraint) by governments rather than actual rules constraining their behavior.[8] This distinction between rules and outcomes is the basic motivation in Andersen and Aslaksen (2008) who carefully study the resource curse under different constitutional arrangements, finding that the standard resource curse result is indeed different for presidential and parliamentary as well as for autocratic and democratic systems.[9] It is also a key distinction made by Brunnschweiler and Bulte (2008) who differentiate between institutions as "durable constraints" and "changeable policy outcomes". Both of these studies find that the resource curse result is sensitive to this dimension of institutional quality, which of course also suggest that this dimension is interesting to include in decomposition of the curse and its reversal.[10] The other institutional dimension we explore is that between "property-rights institutions", which protect citizens (and firms) against expropriation by the government, and "contracting institutions", which enable private contracts between citizens, studied in Acemoglu and Johnson (2005).[11] They find that when it comes to explaining long-run growth, only property-rights institutions seem to have a first-order effect, while contracting institutions matter only for the form of financial intermediation in the economy. With respect to the reversal of the resource curse this distinction may also be of interest especially when decomposing the resource side. For example, government involvement is typically more important for fuels and minerals than for agricultural products and food, suggesting that constraints on the government are more important in these cases.

Finally, the literature on the resource curse varies slightly in the periods of study. Depending on the exact question at hand, data availability places starting dates between the mid-1960s and the mid-1970s.[12] Looking at changes in the importance of certain resources (in particular fuels and minerals) as well as commodity prices over time this does not seem innocuous. Individual countries differ significantly in their resource intensity over precisely this time period (see Tables 7 and 8). Consequently, the choice of starting year (the point around which the importance of the resource is measured so as to minimize reverse causality) may have an effect on the results. Therefore, we systematically run our regressions over different time spans using a homogeneous country sample (as well as for unrestricted samples). All time periods end in 2005 and the start years vary from 1965 to 1984. As previously mentioned we also run regressions with pooled data (using five year averages, as well as yearly data) with time effects and lagged dependent and independent variables as regressors.

Our results show a number of interesting patterns. First, with respect to the differences across types of resources it seems that the resource curse, as well as its reversal, is mainly driven by the ores and metals component of primary exports. In the OLS specifications using the outcome based ICRG measure of institutional quality, ores and metals are negative for growth but for good enough institutions the curse is reversed by the positive interaction effect. The same is true when using resource rents data. Only the minerals component is consistently negative with a positive interaction effect reversing the curse in most specifications. The same is true for the panel regressions, across samples, and when excluding outliers.[13] The result is also present in the IV regressions but the results are less statistically significant due to weak instruments. The other components do exhibit some patterns and some results point to potentially interesting regularities, but in general they display much more variation in terms of robustness across specifications and time. Also changing institutional measures makes a significant difference and it is hard to find results that are robust across time and samples. Again interesting patterns are there in the data, but no results that survive across different specifications.

The rest of the paper is organized as follows. Section 2 presents the data in some more detail and also relates our approach to a number of frequently discussed issues regarding measurement and endogeneity and the interrelations between variables used. In Section 3 we present the basic empirical model and the results for aggregated as well as disaggregated resource data (exports as well as rents) when comparing outcome based to rule based measures of institutions (including robustness and IV results) and in Section 4 we do the same for contracting and property-rights institutions. In Section 5 we summarize our main results and discuss implications for further research.

2. ISSUES OF MEASUREMENT, MULTICOLLINEARITY AND ENDOGENEITY

In this section we describe the data we use and also discuss a number of issues relating to the measurement of natural resources, the choices of starting dates, as well as the concerns related to natural resources affecting institutional quality. Tables 9 and 10 contains descriptive statistics and cross correlation tables.

(a) *Resource data*

Our first main broad resource measure is the share of primary exports in GDP, *PrimExp,* taken from the World Bank's World Development Indicators (WDI). This is the measure used by Sachs and Warner (1995) and also a measure that has subsequently been used by many others studying the

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resource curse. To examine whether different types of resources have different effects we decompose *PrimExp* into its four main components: agricultural raw materials (*agri*), food exports (*food*), fuels (*fuel*), and ores and metals (*oresmet*). Our second main resource variable is the World Bank measure of "natural resource rents." By calculating the "unit rent" as the difference between the unit price of a good/commodity and the unit cost of extraction/production and then multiplying this by total production the measure tries to capture the potential value of resource production to the country. Total natural resources rents of a country are the sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents. These are also available separately allowing us to group them so as to roughly match ores and metals and fuels in our export data.

We agree with those who emphasize the importance of distinguishing between resource "endowments" (e.g., measured by proven reserves) and resource "dependency" (e.g., measured as production or exports).[14] We also agree that the latter is an endogenous outcome (e.g., Wright & Czelusta, 2004) but on the other hand there are arguments for this being the case for the former as well. Torvik (2009) and van der Ploeg and Poelhekke (2010) point out that measures of reserves are not necessarily exogenous either.[15] Countries with longer periods of being industrialized and better institutions are likely to have explored more and hence having found more of their actual reserves.

Regardless of these important aspects we think there are reasons for focusing on the share of primary exports in GDP and resource rents. First, we believe that these are appropriate measures for a number of theoretical settings. In models where a politician faces some trade-off between grabbing resources today or developing other parts of the economy in expectation of future gains, or where individuals, for example, choose to work in the (existing and dominant) resource sector rather than educating themselves, or where individuals can become "producers" or "grabbers" (as in Mehlum *et al*., 2006), it is the share that resources make up of the economy at the point of deciding that matters.[16] Measures of reserves (which are arguably more exogenous) or measures of geography or geology would not be appropriate from this perspective. Second, our focus is on decomposing different dimensions of the interaction between resources and institutions. To do this we want to have a homogenous measure over time for as many countries as possible, and for this purpose, export shares in GDP and resource rents are the best available measures.[17]

(b) *Data on institutional quality*

Our data on institutional quality aims at capturing two dimensions of institutions; first to distinguish between rules and outcomes, second to distinguish between property-rights institutions and contracting institutions. Figure 1 illustrates how the measures we use roughly can be classified according to the two dimensions of "institutional quality".

	Property Rights	Contracting
Rules	PolityIV: Polity score, as well as constraints on the executive	
Outcomes	ICRG: Composite measure, as well as five individual subcomponents	Djankov et al (2003): index of legal formalism World Bank: number of procedures and procedural complexity

Figure 1. *Dimensions of "institutional quality".*

The source for our main outcome measure is the International Country Risk Guide (ICRG) data base, which in total contains a total of 22 variables in three subcategories: political, economic, and financial risk, starting in 1984.[18] These data have been extensively used in the literature on the effects of institutions and, in particular they are the data underlying the composite measure of "institutional quality" used in Mehlum *et al*. (2006) and Boschini *et al*. (2007).[19]

Our main rule-based measures of institutional quality come from the Polity IV data set (Marshall & Jaggers, 2002). The Polity IV measure of democracy reflects the extent to which the three essential, interdependent elements are adhered to: (1) the presence of institutions and procedures through which citizens can express effective preference about alternative policies and leaders; (2) the presence of institutional constraints of the exercise of power of the executive; and (3) the guarantee of civil liberties. As pointed out by Glaeser *et al*. (2004) the fact that parts of the measure are concerned with how various rules are "adhered to" moves it closer to an outcome based measure, but nonetheless they agree that "Polity IV makes the greatest attempt at measuring the political environment rather than [.] choices" (Glaeser *et al*., 2004, p. 276). This is in particular true for the measure of "constraints on the executive" (*exconst*), which is also the preferred measure for this dimension of institutions in Acemoglu and Johnson (2005).[20]

Finally we also try contrasting contracting institutions (CI) and property-rights institutions (PI) as in Acemoglu and Johnson (2005). They define contracting institutions as the rules and regulations governing contracting between ordinary citizens, for example, between a creditor and a debtor or a supplier and its customers, while property-rights institutions regulate the relationship between ordinary private citizens and the politicians or elites with access to political power. There are econometric problems (mainly due to weak instruments) in our context that make it difficult to follow the approach in Acemoglu and Johnson (2005) so we adapt their approach slightly. The data are, however, the same, i.e., the index of formality in legal procedures for collecting on a bounced check from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2003) and the index of procedural complexity (as well as the number of procedures) originally from *Doing Business in 2004* from the World Bank (2004).

(c) *Why examine different starting dates?*

As mentioned in the introduction previous studies of the resource curse, and in particular the ones concerned with the interaction between natural resources and institutional quality, have used a variety of starting dates for their analysis, ranging from the mid 1960s to the late 1970s. This has been driven by different reasons, mainly to do with data availability for whatever particular focus the respective study has had.[21] Here we explicitly address the possibility that results could be sensitive to the starting date of the analysis while keeping the sample of countries constant. The reason for this potential concern is easy to illustrate. Over the 1960s and 1970s especially the world oil and ores and metals sectors experienced both price shocks as well as major changes in terms of many countries rapidly becoming large exporters. This means that some countries that appear as resource dependent in 1980 (measured as resource exports in GDP) had very low, or in some cases no, resource exports in 1965. This in turn (as we will further discuss in the next section) means both that the

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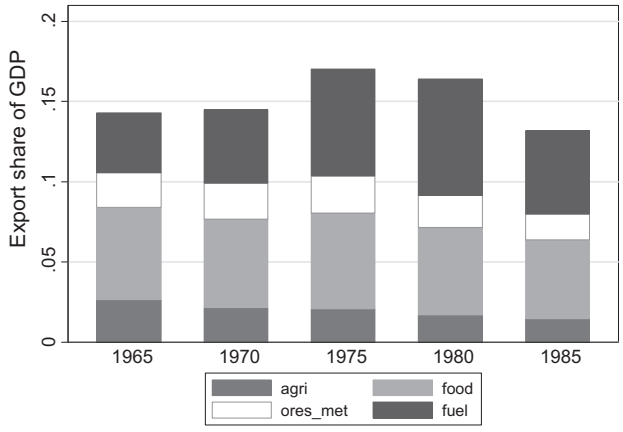

Figure 2. *Average export share in our sample of 75 countries of agricultural goods, food, ores and metals, and fuels in 1965, 1970, 1975, 1980 and 1985.*

importance of these resources as well as the countries rich in them vary considerably depending on the starting date.

Figure 2 illustrates how the mean export shares of GDP of the different resource types have varied from 1965 to 1985. The mean fuels export share of GDP almost doubled until 1980 after which it declined somewhat. The share for agricultural good and food shrunk over this period.

(d) *On the issues of endogenous institutions and natural resources affecting institutions*

Apart from measurement problems there are two key concerns when attempting to study the effects of natural resources, institutions, and their interaction, on economic growth. First there is the relationship between these two variables. It could be the case that extraction of resources, or the knowledge of the existence of extractable resources, affects the quality of institutions. Alternatively, institutional quality may determine the extent to which resources are searched for and extracted. In either case an econometric specification where these variables enter separately (and interacted) would be mis-specified and our institutions measure would capture part of the resource effect, or vice versa. Second there is the well-known problem of institutions being endogenously determined in the development process.

There are many studies that, in different ways, have addressed the question of resources affecting institutions. Over the very long run, Engerman and Sokoloff (1997, 2002) suggest that differences in natural endowments, in particular in the optimal scale of agriculture which in South America created landed elites, had a decisive impact on the development of institutions. However, Dell (2010) finds that within Peru, land concentration does not seem to be associated with worse economic performance over the long run (but rather the opposite). Over more recent periods Ross (2001) focusing on oil and Wantchekon (2002) focusing on primary exports find cross-country evidence of resource dependence being correlated with lower levels of democracy, Tsui (2011) finds evidence that discoveries of oil have negative effects on democracy over the long run.[22] However, in a comprehensive study Haber and Menaldo (2011) find that these results are, at least for oil, mainly driven by outliers and unobserved heterogeneity. There are also a number of papers that have used two-stage procedures to first estimate the effect of resources on institutions and find this to be the channel; resource dependence has a negative effect on institutions which in turn have a negative effect on growth (e.g., Isham, Woolcook, Pritchett, & Busby, 2005; Sala-i-Martin & Subramanian, 2003).

In the context of our study we do not expect to solve these problems but, in light of the above evidence, we have to deal with these concerns and we do this in a number of ways. First—as is rather standard in this literature—we measure resource dependence and institutional quality in the beginning of the period (or as early as possible) mainly so as to minimize the reverse causality, but also to avoid having developments in resources and institutions affecting each other in the period of study. The basic idea is to identify the importance of resources in the economy and the institutional quality at some starting point, and then to study the *subsequent* economic development (of course controlling for a number of things including initial level of development). It is also worth emphasizing that even if one believes (as we do) that resources can affect many aspects of institutional development, this does not preclude the possibility that institutional quality can be exogenous with respect to resources. Arguably many countries have made discoveries of resources, which have then become important in the economy, and claiming that the institutional quality at the point of discovery is independent of the resource is not the same as claiming that institutions are not affected by resources. In Tables 7 and 8 we provide listings of discoveries and production take-offs for some of the most resource rich countries in our data set. This listing shows that, while some countries have "always been resource rich" (and hence these resources may indeed have contributed to the institutions in the beginning of the period) there are also quite a few countries for which the resource dependence is a relatively recent phenomenon.

Second, we also run pooled OLS regressions with time effects and lagged values of both dependent and independent variables. This takes care of some of the endogeneity problems that remain in the cross sectional OLS regressions with different starting dates.

Third, we note that looking at the development of the institutional measures for which we have historical data (i.e., for the Polity measure) for a long period prior to the starting date of our analysis we see that resource rich (above average) countries in 1970 do not have a different average development of their institutional measure in the period 1945–1970, as compared to countries that are relatively resource poor (below average) in 1970.[23] Figure 3 shows these developments for the four measures of resource dependence in 1970 over the period 1945–2005.

These figures are not proof of anything except the fact that there is no evidence of systematic differences in institutional development *prior* to the starting period for countries with above and below average levels of resources in this starting period, which is of some interest for some of the concerns in the OLS framework.

Finally, we recognize that even if the above concerns would not be present, the problem of institutions being endogenous would still remain (see also Arezki & van der Ploeg, 2008). We therefore also use a multiple instrumental variables (IV) strategy, similar to the one in Acemoglu and Johnson (2005), with two sets of (different) instruments for the contracting and property-rights measures of institutional quality. We will discuss these in more detail in the separate sections where we introduce this below.

3. A ROBUST REVERSAL? MAIN RESULTS

In this section we first report our main results contrasting rules and outcome based institutional measures, for aggregate

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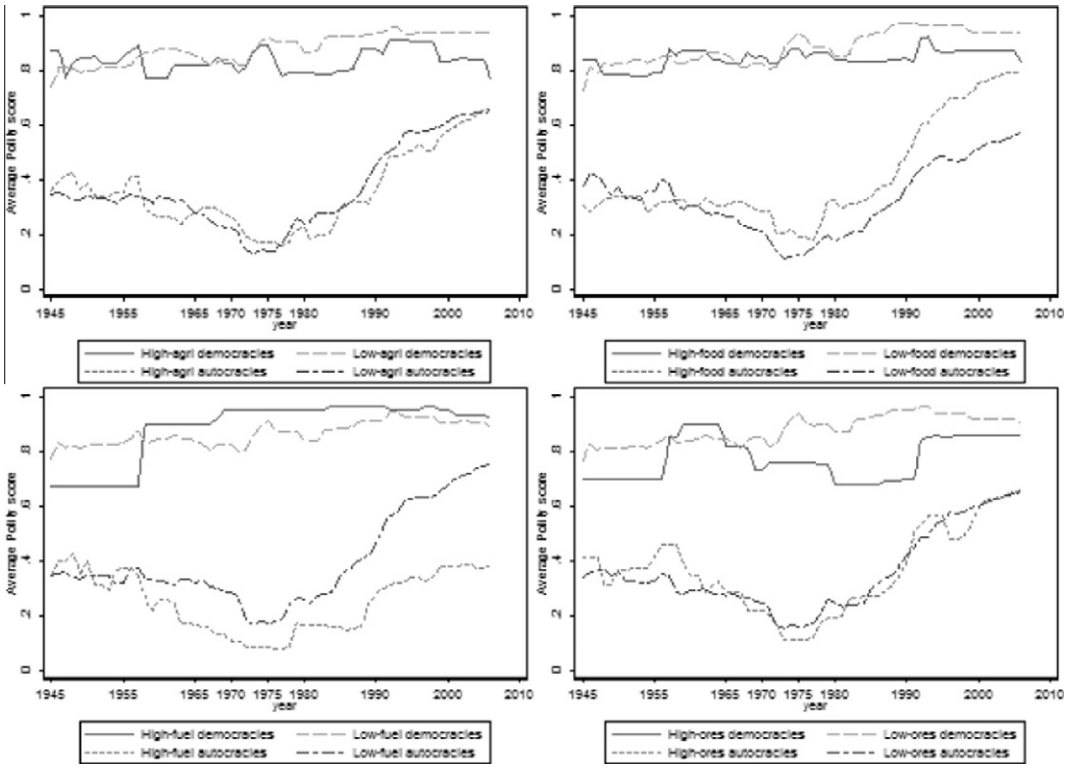

Figure 3. *Institutional development 1945–2005 in countries with above and below average resource intensity measured in 1970 (separately for the four main resource components).*

resources as well as for all resource components individually, using export as well as resource rents data, for different time periods and also for pooled OLS with lags and time effects. We then present the results from our IV-regressions. In Section 4 we present the same set of estimation output for a number of variations to the original specification, including influential observations and contrasting property-rights institutions and contracting institutions and also contrasting democratic and autocratic countries.

(a) *Basic econometric specification and sample*

The basic econometric specification is the same as in Mehlum *et al.* (2006) and in Boschini *et al.* (2007) that is:

$$growth_i = X_i'\alpha + \beta_1 Inst_i + \beta_2 NR_i + \beta_3(NR_i \times Inst_i) + \varepsilon_i$$

where growth is the average yearly growth rate of per capita GDP between varying start years and 2005 in the standard OLS. X is a vector of controls including initial GDP per capita level, period averages of trade openness (i.e., exports plus imports divided by GDP) and investment ratios, population growth, regional dummy variables for Sub Saharan Africa, Middle East and North Africa, and Latin America respectively and a constant. Inst is a measure of institutional quality that changes across specifications and NR is (a vector of) natural resource exports. For export data the four main subcomponents of primary exports are agricultural products, food, fuels, and ores and metals, while for rents the subcomponents are forestry, fuels, and minerals. Finally, NR × Inst is the interaction(s) between (the components of) natural resources and institutional quality.[24] Resources and institutions are all measured at the beginning of the period. Also to avoid that the resource measure is too dependent on individual year observations each "start year" is the average export share over a five year period as early as possible, e.g., when the regression is over the period 1965–2005 the value of resource exports in GDP at the start of the period is calculated as the average of this share for the years 1963–1967.

The pooled OLS has the same basic structure but now growth in a period t is a function of the same variables as above in $t-1$ and now also including lagged growth and time effects in X, that is

$$growth_{it} = X_{i,t-1}'\alpha + \beta_1 Inst_{i,t-1} + \beta_2 NR_{i,t-1} + \beta_3(NR_{i,t-1} \times Inst_{i,t-1}) + \varepsilon_i.$$

We run this regression both for a panel of 5-year averages (where if t = 1970-74, then $t-1$ = 1965–69, *etc.*) and also for a yearly panel with no averaging.

The basic result found in Mehlum *et al.* (2006) and in Boschini *et al.* (2007) is that β_1 is positive (good institutions are in themselves good for growth), β_2 is negative (the standard resource curse result), and that β_3 is positive, and, crucially, sufficiently positive so as to turn the negative effect from resources into a positive one *given* that institutions are good enough.[25] The main question posed in the following is: Can we understand more about this broad relationship by studying different dimensions of institutions, different components of resources, and different time periods?

Depending on the starting date the sample varies but to have a homogenous set of countries we require that there be data that enable us to run the regressions starting from 1965, 1975 and 1985. For resource exports this means a sample of 75 countries, while for the resource rents sample the number of countries is 86. The unrestricted samples (used later in the robustness section) contain between 90 and 107 countries. Countries are listed in Table 11.[26]

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(b) *OLS results*

In Table 1 we first present the basic results when contrasting the more outcome based composite measure of institutional quality, constructed from ICRG, to a more rule based democracy measure, Polity2 score, using primary export data as the resource measure. In columns 1–3, and 6–8 we vary the starting year from 1965–1985.[27] Columns 4 and 9 show results from the pooled regression using 5 year averages of the variables, including time effects and also using (one period) lags of the variables including lagged growth. Columns 5 and 10 show the results from the same regression but now using the yearly data and one year lags.[28]

The upper panel shows results when using the aggregate measure of primary exports to GDP.[29] Focusing first on ICRG (*Inst*, *primexpgdp* and *primXInst* in columns 1–5), the signs are as expected in every regression but only (weakly) significant in the yearly panel regression. In columns 6–10 the reversal looks somewhat more promising but the size of the coefficients indicate that to the extent that there is a reversal it is quite weak.

The lower panel of Table 1 shows the results when disaggregating primary exports into its four components: agricultural raw materials, food, fuels, and ores and metals. These results give a clear indication that most of the effect comes from the ores and metals sub component and the ICRG interaction. For all ICRG regressions except for the one-year panel the results indicate a reversal at the 1% level. The Polity2 results, however, are now more scattered.[30]

In Table 2 we present exactly the same regressions but now using resource rents as the measure of natural resources.[31]

Again the top part of the table shows results using the broad resource measure and the lower part the disaggregated effects from, in this case, forest, fuel, and mineral rents respectively. For ICRG there seems to be a reversal in the standard OLS but less clearly so in the panel specifications. Looking at the disaggregated effects it is clear that the result is mainly due to a clear reversal of the mineral rents component. In all regressions with different starting dates and for the five year panel the negative effect of mineral rents and the positive interaction effect are estimated at the 1% level. In the one year panel the point estimates are similar but less precisely estimated. Forest and fuels also show reversal in individual specifications but the clearest result is that for minerals. For Polity2 no clear results are to be found.

Overall, the results so far indicate that much of the resource curse is, in fact, driven by ores and metals, while the other resources seem to behave quite differently depending on specification and period. This suggests two things with respect to previous findings: (1) The resource curse may be present to different degrees for all types of resources across specifications but the main driver of it, and in particular of the reversal, comes from the interaction between ores and metals and institutional quality; (2) This result is clearer when using the

Table 1. *Time, resources, and institutions*

	ICRG					Polity				
	(1) 196505	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 196505	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	1.696	1.219	−0.988	1.593	−1.189	−0.832	−0.904	0.279	0.0440	−0.940
	(1.164)	(1.470)	(2.090)	(1.180)	(1.380)	(0.590)	(0.736)	(1.016)	(0.560)	(0.565)
primexpgdp	−6.658	−5.208	−8.811	−5.109	−8.355*	−6.131**	−4.976**	−3.147	−4.721***	−3.376*
	(4.039)	(4.582)	(10.372)	(3.368)	(4.970)	(2.468)	(1.963)	(4.909)	(1.604)	(1.974)
primX	7.121	7.016	15.127	5.159	15.60*	5.465	6.574**	2.708	4.801*	4.795*
	(6.952)	(8.225)	(15.835)	(6.689)	(8.157)	(3.314)	(2.507)	(4.280)	(2.455)	(2.739)
R^2	0.730	0.685	0.530	0.300	0.207	0.693	0.678	0.509	0.299	0.201
Inst	1.910	1.870	−0.091	0.296	−0.280	−0.950	−1.439*	−0.164	−0.718	−1.083
	(1.202)	(1.376)	(1.722)	(1.429)	(1.401)	(0.645)	(0.852)	(1.161)	(0.641)	(0.667)
agrigdp	−4.023	−1.498	7.634	−5.331	18.80	−9.216	−16.643*	−19.695	−0.339	7.404
	(10.453)	(17.914)	(23.202)	(17.90)	(14.13)	(8.544)	(8.851)	(22.721)	(10.23)	(11.36)
agriX	−8.317	−12.499	−29.813	7.040	−27.08	6.595	21.611	16.233	4.064	−9.154
	(15.240)	(26.669)	(39.769)	(26.40)	(30.59)	(10.849)	(15.875)	(32.277)	(12.93)	(14.69)
foodgdp	−11.519**	−4.889	−0.476	−6.330	−6.112	−9.089*	−8.577**	0.703	−7.235*	−8.609**
	(4.937)	(6.361)	(8.277)	(5.161)	(5.958)	(4.545)	(3.591)	(5.680)	(3.656)	(3.825)
foodX	10.896	5.224	6.157	7.902	11.42	8.593	14.300**	3.148	11.84**	13.07**
	(8.500)	(11.316)	(14.557)	(10.17)	(11.49)	(6.169)	(6.047)	(9.073)	(5.383)	(5.102)
fuelgdp	1.460	6.841***	10.194	−7.570	−10.70	−3.330*	−3.112*	1.719	−4.002**	−4.669**
	(1.532)	(2.312)	(8.595)	(5.468)	(6.461)	(1.851)	(1.582)	(3.867)	(1.688)	(2.100)
fuelX	−6.154**	−14.203***	−18.966	10.37	18.01	2.405	4.602*	0.702	2.769	5.814*
	(2.770)	(4.540)	(15.573)	(10.57)	(11.66)	(3.201)	(2.304)	(4.627)	(2.064)	(3.119)
ores_metgdp	−17.661***	−18.064***	−48.800***	−18.27***	−4.991	−11.984**	−9.206**	−12.620	0.380	−2.853
	(3.691)	(6.399)	(11.249)	(6.629)	(18.25)	(4.664)	(4.307)	(12.607)	(8.597)	(6.833)
oresX	35.931***	44.354*	124.347***	41.93***	20.08	10.268	3.632	1.502	−2.192	4.117
	(11.283)	(25.103)	(36.840)	(14.29)	(26.60)	(14.390)	(15.762)	(15.888)	(11.61)	(8.167)
R^2	0.787	0.773	0.711	0.301	0.201	0.734	0.756	0.628	0.292	0.199
N	75	75	75	488	1506	75	75	75	488	2919

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

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Table 2. *Time, resource rents, and institutions*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	2.864***	2.444**	1.012	3.555***	1.694	1.815***	1.073	1.556**	1.531**	0.526
	(1.014)	(1.213)	(1.188)	(1.101)	(1.138)	(0.592)	(0.700)	(0.749)	(0.595)	(0.579)
Rents	−14.233***	−15.176**	−18.634**	−3.565	−9.460*	0.736	−1.677	−0.032	−0.806	−1.952
	(5.147)	(6.561)	(7.130)	(5.749)	(5.078)	(2.398)	(2.527)	(2.511)	(1.689)	(2.614)
RentsXInst	27.928***	27.993**	37.070**	4.141	17.40*	−17.281**	−1.815	−3.748	−2.416	0.590
	(10.013)	(13.503)	(14.508)	(11.33)	(9.529)	(7.437)	(5.127)	(4.082)	(2.749)	(4.457)
R^2	0.750	0.716	0.637	0.279	0.188	0.700	0.631	0.579	0.272	0.186
Inst	3.396***	2.834**	0.432	3.831***	−0.912	1.328**	1.098	1.235	1.418**	0.570
	(1.168)	(1.371)	(1.394)	(1.334)	(1.200)	(0.587)	(0.932)	(1.090)	(0.670)	(0.700)
Forestry	2.600	−2.554	−65.785*	7.021	−40.55**	−5.335	−17.692	−32.572	−22.80	9.233
	(14.327)	(16.755)	(33.139)	(12.13)	(16.01)	(12.318)	(18.150)	(31.280)	(21.01)	(18.44)
ForestryXInst	−20.405	−24.036	60.336	−70.41	73.79*	−0.085	14.192	−12.948	16.02	−29.22
	(27.162)	(43.919)	(66.283)	(42.50)	(37.18)	(26.211)	(35.273)	(52.346)	(34.15)	(24.58)
Fuel	−16.992	−6.296	−10.706**	−5.250	−6.759	1.449	−0.447	1.515	−0.0708	−2.274
	(15.586)	(7.779)	(4.650)	(5.430)	(4.280)	(2.207)	(2.575)	(2.927)	(1.881)	(2.960)
FuelXInst	32.710	12.247	23.705**	7.373	10.51	−16.584*	−2.467	−4.504	−4.726*	0.873
	(28.411)	(14.973)	(10.238)	(11.28)	(8.995)	(9.385)	(4.810)	(4.186)	(2.781)	(4.591)
Minerals	−28.389***	−46.140***	−52.344***	−44.51***	−27.47	−14.629**	−6.860	−20.217	−17.93**	−15.35***
	(5.923)	(8.051)	(17.302)	(10.22)	(17.13)	(7.154)	(10.101)	(16.022)	(8.166)	(5.149)
MineralsXInst	80.561***	117.749***	119.018**	86.94***	57.19*	11.844	−26.816	1.222	2.015	7.877
	(21.197)	(27.855)	(48.419)	(29.18)	(29.45)	(17.882)	(20.170)	(19.129)	(11.99)	(9.882)
R^2	0.765	0.762	0.717	0.395	0.202	0.724	0.671	0.642	0.383	0.195
N	86	86	86	502	1834	86	86	86	502	2992

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

outcomes based ICRG measure for institutions instead of the rules based Polity2 measure.

There are several ways of displaying the result and also many different questions that could be answered using the regression results above, but one obvious key question is: "What is the average growth effect across different levels of institutional quality for a country rich in ores and metals?" To answer this we have used the relevant coefficients and their variance and covariance to generate the marginal growth effects across different levels of institutional quality including confidence bands (see Figure 4).

The two upper diagrams contrast the close-to-zero effect from aggregate resource exports with that of ores and metal exports. The two lower diagrams show similar corresponding effects for resource rents and mineral rents, respectively. The slope of the ores and metals exports and mineral rents effects is much steeper suggesting that for these resources the impact on growth varies much more across institutional quality.

(c) *IV results*

A key theme in much of the work on the relationship between institutions and economic development has been the problem of disentangling the causal effect from the former on the latter.[32] There are a number of reasons (omitted variables, errors-in-variables, and, in particular, a potential simultaneous causality between institutional quality and economic growth) to believe that our institutional measures are correlated with the error term. One possible way of finding the causal effect from institutions on growth may in this case be to use instrumental variable techniques. This possibility depends crucially on the validity of the instrument(s). In order for an instrument to be valid, it needs to fulfill both the criteria of instrument relevance (in our case that it is sufficiently correlated with institutions) and of exogeneity (that the instrument is uncorrelated with the error term, i.e., the instrument has no partial effect on growth once institutions are controlled for). In our context, it is important to note two things. First, even if an instrument has been considered "good" in general it is not certain that this is the case in a particular sample (that is, possible violations of instrument validity always need to be considered). Second, the validity of an instrument is likely to change depending on specification chosen. While some Z may be a valid instrument for X (say, institutions) when analyzing its effect on $Y1$ (say, log GDP per capita), the validity considering the effect from X on $Y2$ (say, economic growth) may be quite different. The exogeneity of the instrument may change depending on whether the different omitted variables (i.e., the error term) are related to the instrument or not. Hence, we would not be surprised if our results varied between specifications also when instrumenting for institutions.[33] The reason for pointing these things out is that there are a number of well-known studies, such as La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997), Hall and Jones (1999) and Acemoglu, Johnson, and Robinson (2001) that have found good instruments for institutions and successfully used these to disentangle the causal effect of institutions on long-run development. These instruments are clearly obvious candidates in our setting too but, for the above mentioned reasons, we cannot assume that they will work equally well.

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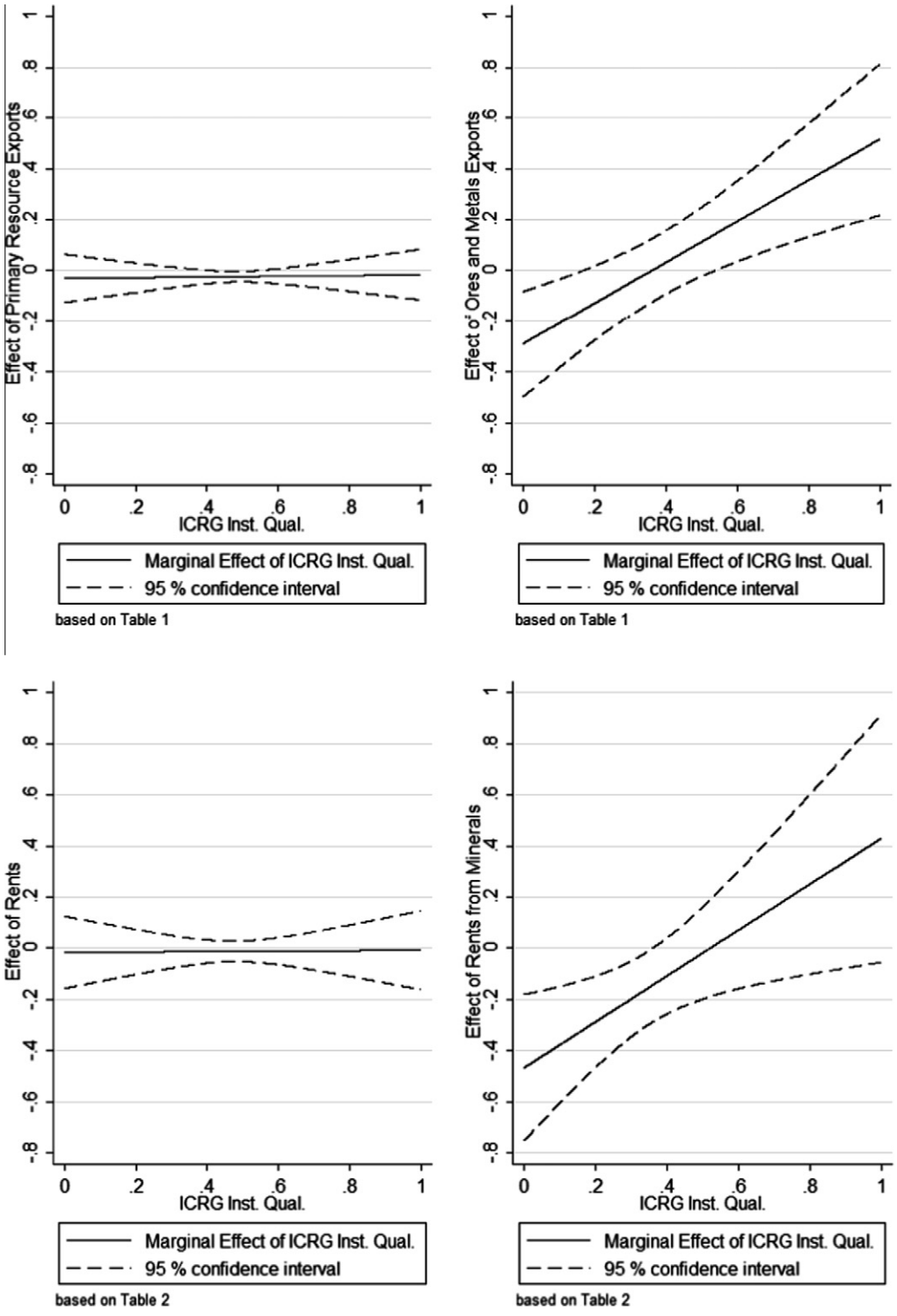

Figure 4. *Marginal growth effects for resource rich countries across institutional quality. (Top panel contrasting resource exports and ores and metals exports. Bottom panel contrasting resource rents and mineral rents).*

Having tried multiple available instruments, we in the end use *latitude* and the share of the population speaking a European language at home (*eurfrac*). We also add to the instrument set *British legal origin* and the interaction of *latitude* and *resources*. [34] For varying start years as well as for the pooled samples, we run regressions both on our first specification, using *exports* as well as *rents* as the resource measure, and also a specification where we include all four sectors of natural resources and but only the interaction of ores and metals and rents, respectively with institutions. [35] In the interest of saving space we limit the tables to only including the IV regressions for the broad measure of resource rents and using the

mineral rents interaction. The results for primary exports and ores and metals are similar.

The results are presented in Tables 3 (for resource rents) and 4 (for mineral rents) with tables labeled (a) showing the second stage and (b) showing the first stage results. Qualitatively we obtain the same results as in our OLS regressions but the precision of our estimates becomes lower. [36] Looking at our first-stage results (Tables 3b and 4b), this is not surprising. While in all cases but one the instrument set passes the Hanson *J*-test for the overidentifying restrictions (using 5%, Ovid in Tables 3a and 4a) and the instrument set is jointly significant in the first stage regressions (IVpval in Tables 3b and 4b), the *F*-value as well as the Shea partial R-squared for the institutions regression (ShInst) and for the interaction first stage regression (ShResInst) are low in almost all specifications, indicating that our instruments are weak. When looking at the interaction result for the broad resource measure (*rentsXinst*) in Table 3a this is only significant in one of the eight (second stage) regressions run. The model sum of square is even negative in one case (column 8) evidencing the bad model prediction under instrumentation in this particular sample. The results are again somewhat clearer in the preferred specification when interacting institutions and *mineral rents*—shown in Table 4a—but the results lose precision (and the model yields a negative r-squared for two of the samples when using polity as the measure for institutions). [37] This seems to be the case in the pooled specifications with lags as well.

It is not obvious what conclusions to draw from these results. Results become less precise and are not significant at conventional levels in many of the specifications and in this sense the hypothesis that the interaction effect may reverse the negative impact from resource dependence should be rejected. But this result is found in a setting where the instruments are weak, which makes the interpretation less clear than if the same was found with good instruments, and also the point estimates (to the extent we allow ourselves to interpret these) remain in favor of the hypothesis of a possible reversal. Arezki and van der Ploeg (2008) draw the conclusion that the finding of a reversal of the curse, using primary exports, no longer holds when instrumenting for institutions. The first stage results reported in their Table 4b are very similar to ours and when it comes to the interpretation we would agree. We do not, however, think that the same applies to the results for mineral rents (or ores and metals exports) which again illustrate the importance of decomposing the effects. Our overall conclusion is that instrumenting for institutions in the type of setting we examine is problematic but that the results are still weakly supportive of the interpretation of an institutionally driven reversal of the curse for mineral rents extraction (and ores and metals exports).

Table 3a. *Instrumenting for institutions, natural rents specification: Second stage*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	6.766**	6.389	5.812	7.117**	10.500***	3.131*	4.279	−10.377	4.492	−0.139
	(3.174)	(4.872)	(5.777)	(3.308)	(3.306)	(1.770)	(2.911)	(16.456)	(2.745)	(3.426)
RentsXInst	11.875	29.285	40.784	−3.506	3.364	−27.715	−15.944	101.284	−15.022	22.354
	(14.197)	(48.537)	(35.939)	(29.444)	(16.525)	(24.326)	(20.451)	(120.014)	(17.738)	(24.194)
Lngdppc	−1.172***	−1.259***	−1.230**	−1.086***	−1.256***	−0.661***	−0.847**	1.131	−0.796**	−0.226
	(0.338)	(0.382)	(0.586)	(0.373)	(0.330)	(0.229)	(0.407)	(2.185)	(0.357)	(0.422)
Inv	0.137***	0.140***	0.153***	0.025	0.084***	0.227***	0.195***	0.151	0.061*	0.104***
	(0.033)	(0.043)	(0.052)	(0.022)	(0.024)	(0.052)	(0.055)	(0.110)	(0.032)	(0.026)
Mopen	0.004	0.004	0.001	0.005	0.002	−0.001	−0.004	−0.004	−0.001	0.005
	(0.005)	(0.006)	(0.007)	(0.004)	(0.004)	(0.005)	(0.007)	(0.017)	(0.005)	(0.004)
Ssa	−2.517***	−2.831***	−2.400**	−3.133***	−2.545***	−1.437**	−1.700**	−1.240	−1.955***	−1.907***
	(0.426)	(0.548)	(1.056)	(0.572)	(0.535)	(0.704)	(0.823)	(1.974)	(0.632)	(0.502)
Lac	0.510	0.310	0.776	−0.119	0.395	0.179	0.197	−2.489	−0.668	−1.627**
	(0.755)	(0.883)	(0.958)	(0.641)	(0.514)	(0.897)	(1.385)	(2.375)	(0.721)	(0.727)
Mena	0.200	−0.159	0.583	−0.164	−0.093	0.448	0.731	0.651	0.335	0.164
	(0.583)	(0.703)	(0.855)	(0.574)	(0.506)	(0.662)	(0.919)	(2.115)	(0.757)	(0.734)
Pop.growth	−0.171	−0.254	−0.473	−0.160	−0.855	−0.162	−0.191	−0.359	−0.130	0.020
	(0.148)	(0.324)	(0.314)	(0.199)	(1.161)	(0.165)	(0.274)	(0.754)	(0.162)	(0.814)
Rents	−5.094	−14.455	−18.550	−0.652	−0.397	3.807	3.147	−34.778	2.124	−8.299
	(7.024)	(24.239)	(17.533)	(14.465)	(8.358)	(5.598)	(6.232)	(42.290)	(6.156)	(8.765)
Constant	4.100***	5.312**	5.633**	6.129***	2.563*	0.653	2.361	−3.131	4.192***	0.212
	(1.393)	(2.127)	(2.574)	(1.808)	(1.551)	(1.351)	(1.872)	(6.616)	(1.277)	(1.209)
R^2	0.702	0.656	0.559	0.286	0.142	0.673	0.501	−2.180	0.237	0.155
N	86	86	86	413	1510	86	86	86	413	2659
Jpval	0.029	0.047	0.035	0.073	0.002	0.177	0.236	0.574	0.089	0.009
ShInst	0.137	0.074	0.090	0.107	0.110	0.107	0.069	0.029	0.035	0.027
ShResInst	0.424	0.043	0.196	0.126	0.342	0.098	0.071	0.019	0.045	0.029
Ovid	0.431	0.258	0.189	0.076	0.282	0.144	0.253	0.892	0.151	0.081

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

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Table 3b. *Instrumenting for institutions, natural rents specification: First stage*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
First stage institutions regression										
eur_frac	0.100*	0.104**	0.098**	0.099***	0.081***	−0.097	−0.021	0.129	0.144***	0.126***
	(0.050)	(0.048)	(0.046)	(0.017)	(0.009)	(0.142)	(0.091)	(0.093)	(0.044)	(0.017)
Lo_british	0.039	0.044	0.033	0.020	0.017***	0.344***	0.289***	0.173***	0.136***	0.103***
	(0.031)	(0.032)	(0.031)	(0.013)	(0.006)	(0.068)	(0.070)	(0.065)	(0.028)	(0.011)
Latitude	0.002	0.002	0.002	0.003***	0.002***	0.004	0.002	−0.002	−0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.000)	(0.004)	(0.004)	(0.003)	(0.001)	(0.001)
latX	0.026	0.007	0.010	−0.000	−0.001	−0.032	−0.025	−0.018	−0.006	−0.012***
	(0.019)	(0.013)	(0.009)	(0.005)	(0.002)	(0.041)	(0.026)	(0.021)	(0.011)	(0.004)
Fval	3.579	2.552	2.465	12.871	38.760	7.226	4.792	3.294	15.681	59.522
IVpval	0.010	0.046	0.052	0.000	0.000	0.000	0.002	0.015	0.000	0.000
First stage institutions ∗ natural rents regression										
eur_frac	0.002	0.004	0.005	0.004*	−0.003**	0.003	0.011	0.010	0.012**	0.008***
	(0.004)	(0.005)	(0.004)	(0.002)	(0.001)	(0.008)	(0.010)	(0.013)	(0.005)	(0.002)
lo_british	0.001	0.008**	0.004	0.001	0.001	0.021***	0.033**	0.027***	0.016***	0.014***
	(0.003)	(0.004)	(0.003)	(0.001)	(0.001)	(0.006)	(0.013)	(0.010)	(0.004)	(0.002)
Latitude	−0.001***	−0.000	−0.000	−0.000**	−0.000***	0.001	0.000	−0.000	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
latX	0.012***	0.001	0.005***	0.002	0.003***	−0.010*	−0.009	−0.000	−0.009***	−0.008***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.006)	(0.007)	(0.005)	(0.003)	(0.001)
Fval	5.876	1.977	5.273	4.555	23.568	3.349	1.961	2.513	7.956	32.929
IVpval	0.000	0.107	0.001	0.001	0.000	0.014	0.109	0.049	0.000	0.000

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

4. A ROBUST REVERSAL? FURTHER ALTERATIONS

(a) *Heterogeneous sample sizes*

As mentioned, differences in research findings could be driven by variations in sample or time periods studied. In the previous section we let the starting period vary from 1965 to 1985 in our cross-section estimations. In this subsection we allow the sample to vary over time periods so that it is as large as possible given a certain starting date. Tables 5a and 5b report our basic and preferred specifications with a sample varying from 92 to 116 countries.

Comparing the results for the respective time periods we find them to be quite similar. Some new results appear, some of the previous ones disappear, but the main result, i.e., that the ores and metals (mineral rents) component of resources is mostly significantly negative with a positive interaction effect, remains intact.

(b) *Influential observations*

Some countries are well known for being extremely dominated by natural resources. Since our resource measures are related to the overall size of the economy (as export shares of GDP) some countries may have disproportionately large effects on our results. Using a formalized procedure, we identify influential observations using the DFITS index when estimating the equation in Section 3.2. Observations with a DFITS index larger than the absolute value of $2\sqrt{k/n}$ (where k is the number of independent variables, including the constant, and n the number of observations) are excluded from the sample.

Table 6 reports the results and the countries excluded from the sample for each time period. The most interesting result is that when excluding influential observations the results for mineral rents actually become stronger. As shown in Table 6b the negative effect for mineral rents is significant in all specifications (and at the 1% level for all ICRG specifications) and the interaction when using ICRG is positively significant everywhere (and at the 1% level for the three most important specifications). [38]

(c) *The panel dimension*

Apart from using the pooled panel data as in Section 3 we also tried exploiting the panel dimension more fully by including country fixed effects. As expected this washes out all results involving institutions in this setting as there is simply not enough (within country) variation, and in particular we do not have enough countries with good institutions turning bad. What is likely to be an important institutional level effect between, say Norway and Nigeria, is not captured using country fixed effects and institutions separately because all of Norway's positive institutional quality is captured by the country fixed effect. For completeness, we also estimated the effects in both a "difference GMM" and "system GMM" linear dynamic growth model without obtaining any results of interest. [39]

(d) *Contracting versus Property-rights institutions*

As suggested by Acemoglu and Johnson (2005) there are reasons to distinguish between "property-rights institutions", which protect citizens (and firms) against expropriation by

Table 4a. *Instrumenting for institutions, mineral rents specification: Second stage*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	5.203	6.010	4.813	6.265	1.948	0.817	7.331	−2.706	−0.587	5.702*
	(3.643)	(3.788)	(3.983)	(11.939)	(13.215)	(1.260)	(5.160)	(5.536)	(2.918)	(3.218)
rentsminX	77.023**	82.564*	141.459**	31.088	399.898	48.981**	−286.009	264.085	183.276	−122.322
	(33.149)	(45.444)	(57.812)	(421.437)	(558.253)	(20.328)	(183.706)	(272.711)	(147.663)	(175.461)
Lngdppc	−1.057***	−1.196***	−1.259**	−1.221	−0.651	−0.486***	−1.343*	−0.449	−0.456*	−0.836***
	(0.388)	(0.410)	(0.485)	(1.098)	(1.039)	(0.159)	(0.692)	(0.475)	(0.266)	(0.295)
Inv	0.133***	0.111***	0.113**	0.026	0.077***	0.152***	0.277***	0.037	0.012	0.149***
	(0.033)	(0.036)	(0.045)	(0.020)	(0.026)	(0.057)	(0.097)	(0.122)	(0.037)	(0.039)
Mopen	0.008*	0.009*	0.011**	0.009	0.008	0.003	0.012	0.011	0.007	0.003
	(0.004)	(0.005)	(0.005)	(0.011)	(0.008)	(0.005)	(0.011)	(0.009)	(0.006)	(0.003)
Ssa	−2.469***	−2.928***	−2.550***	−3.111***	−2.660***	−1.990***	−0.698	−2.603*	−2.670***	−1.037
	(0.413)	(0.447)	(0.673)	(0.554)	(0.538)	(0.725)	(1.529)	(1.379)	(0.843)	(0.673)
Lac	0.139	0.076	0.261	−0.191	−1.000	−0.952	1.785	−1.137*	−1.630**	−0.299
	(0.879)	(0.851)	(0.817)	(2.254)	(2.119)	(0.609)	(2.165)	(0.652)	(0.635)	(0.786)
Mena	−0.063	−0.350	0.084	−0.588	−0.648	−0.296	1.332	−0.550	−0.968	1.794
	(0.618)	(0.628)	(0.700)	(1.178)	(0.862)	(0.754)	(1.683)	(1.449)	(1.171)	(1.328)
Pop.growth	−0.213	−0.217	−0.557**	−0.265	−2.069	−0.144	−0.392	−0.991	−0.376	−0.582
	(0.133)	(0.194)	(0.211)	(0.381)	(2.224)	(0.149)	(0.394)	(0.685)	(0.241)	(0.753)
rentsfor	−3.593	−12.029	−38.206*	−24.783	−2.674	−3.579	−14.605	−4.601	−9.572	7.349
	(9.064)	(10.563)	(20.400)	(17.334)	(10.179)	(8.069)	(20.100)	(31.629)	(11.943)	(8.003)
rentsfuel	1.518	0.564	1.807	−1.569	−0.009	−0.235	3.829	0.060	−2.922	1.231
	(2.069)	(1.635)	(2.128)	(2.134)	(2.678)	(2.699)	(5.351)	(3.269)	(2.014)	(1.801)
rentsmin	−25.533**	−31.901*	−54.511**	−18.825	−155.823	−31.217***	62.998	−108.241	−94.105	36.868
	(11.601)	(16.091)	(20.793)	(143.968)	(216.230)	(9.979)	(48.580)	(79.231)	(66.065)	(66.999)
Constant	4.210***	5.499***	7.397***	7.955***	3.546**	2.350	1.778	7.887*	6.427***	−0.508
	(1.200)	(1.522)	(2.215)	(1.970)	(1.619)	(1.690)	(3.241)	(4.623)	(1.950)	(1.763)
R^2	0.745	0.725	0.680	0.308	0.130	0.688	−0.384	−0.089	0.075	0.105
N	86	86	86	413	1508	86	86	86	413	2657
Jpval	0.000	0.000	0.000	0.027	0.002	0.005	0.300	0.554	0.106	0.002
ShInst	0.101	0.087	0.087	0.008	0.007	0.161	0.064	0.154	0.032	0.035
ShResInst	0.595	0.420	0.522	0.007	0.004	0.432	0.050	0.104	0.015	0.011
Ovid	0.300	0.273	0.056	0.057	0.386	0.321	0.417	0.647	0.119	0.044

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

the government, and "contracting institutions", which enable private contracts between citizens. The institutional measures in the previous section (and in most of the previous studies of the role of institutions in the resource curse) are measures of property rights institutions.

To look at the potential contrasting effect from contracting institutions we have run the same specifications as in Section 3, first using broad composite measures and then decomposing these into their main components, but now including both property rights institutions (as above) contracting institutions, and their respective interactions. As our measure of contracting institutions we have used the index of formality in legal procedures for collecting on a bounced check from Djankov *et al*. (2003) and the index of procedural complexity (as well as the number of procedures) originally from The World Bank (2004).[40] As mentioned in the introduction, we do not expect these two measures simply to be different proxies of the same thing. This is evidenced by a fairly low correlation of the measures (varying from 0.26 to 0.47 depending on the sample).

The overall results indicate that contracting institutions have no impact on the reversal of the resource curse. There are occasional significant interaction effects but nothing consistent. The inclusion of contracting institutions sometimes changes the size and significance of coefficient estimates for property rights but in general, the estimates relating to the property rights are similar to the previous results. Standard errors are however typically increased. The results are perhaps not very surprising, in particular when considering ores and metal exports. These are typically industries where state involvement is high and the actions of governments have a more direct impact on the performance of these sectors than through their function in ensuring the contracting opportunities between citizens.

Finally, even though our measure of contracting institutions comes from the very end of the sample period (2004), we do not attempt to instrument for the endogenous variables in this setting. As we saw in Section 3, the instruments we use are found to be weak. Using the same set of (four) instruments for our (four) endogenous regressors would yield an underidentified equation with no reliability in terms of regression output.

(e) *Splitting the sample into democracies and autocracies*

The fact that some interaction effects become negative for certain resources in some specifications may at first seem

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Table 4b. *Instrumenting for institutions, mineral rents specification: First stage*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
First stage institutions regression										
eur_frac	0.094*	0.093*	0.083*	0.099***	0.081***	−0.126	0.020	0.151*	0.153***	0.128***
	(0.050)	(0.049)	(0.049)	(0.018)	(0.009)	(0.145)	(0.097)	(0.088)	(0.046)	(0.017)
Lo_british	0.051	0.047	0.048	0.022*	0.023***	0.305***	0.297***	0.181**	0.138***	0.110***
	(0.031)	(0.030)	(0.031)	(0.013)	(0.006)	(0.079)	(0.072)	(0.069)	(0.028)	(0.011)
Latitude	0.004*	0.003	0.003	0.003***	0.003***	−0.000	0.001	−0.003	−0.000	0.001**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.000)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
latX	0.040	0.048*	0.052	−0.002	−0.006***	0.140**	−0.116***	−0.179**	−0.018***	−0.020***
	(0.026)	(0.025)	(0.035)	(0.002)	(0.001)	(0.070)	(0.041)	(0.085)	(0.007)	(0.003)
Fval	6.234	7.203	4.518	15.063	60.720	9.335	5.519	3.770	18.575	92.335
IVpval	0.000	0.000	0.003	0.000	0.000	0.000	0.001	0.008	0.000	0.000
First stage institutions ∗ rents from minerals regression										
eur_frac	0.001	0.001	0.000	0.004***	0.002***	−0.004	−0.002	0.002	0.002	0.002***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
lo_british	0.001*	0.001	0.001	−0.000	0.000	0.003	0.006*	0.004	0.002	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.003)	(0.003)	(0.002)	(0.001)
Latitude	−0.000	0.000	−0.000	0.000	0.000*	−0.000	0.000	−0.000	−0.000**	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
latX	0.011***	0.010***	0.013***	−0.000	0.000***	0.023***	−0.006**	0.007	−0.000	−0.000
	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)	(0.005)	(0.003)	(0.010)	(0.000)	(0.000)
Fval	64.113	35.263	17.907	4.984	9.485	6.527	2.913	0.898	4.495	17.314
IVpval	0.000	0.000	0.000	0.001	0.000	0.000	0.027	0.470	0.001	0.000

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

strange. This, however, happens for polity only and when thinking about what "worse institutions" (lower polity score) for this measure implies it is going not only from consolidated to partial democracy, but also moving further down the score to "stable autocracies". In terms of attracting investments in certain resource industries, it is not at all impossible to imagine that "full autocracy" is better than partial democracy. To study this possibility we split the sample into democratic countries (Polity score above 0) and autocratic countries (Polity score below 0) and re-run the regressions above.

The results are interesting and in line with the hypothesis that more autocratic states may be better at turning some resources into growth than are less autocratic (but still autocratic) states. When looking at the sample of democratic countries the interaction effect is always positive (but not always significant) for ICRG as well as Polity. For autocratic countries, however, the interaction effect is sometimes negative. When disaggregating with respect to the resources it turns out that this effect is due to forest rents and mineral rents contributing more to growth the more autocratic the country is in some specifications. Results are similar when instead using the resource measures primary exports and its disaggregation. The result is not robust but still gives an indication of the possibly different results across different institutional measures.

5. CONCLUDING DISCUSSION

Recent research, both theoretical and empirical, has found that having large shares of natural resource exports in the economy is problematic only if institutional quality is too poor. In countries with sufficiently good institutions resource dependency instead contributes positively to economic growth (Mehlum *et al.*, 2006, and Boschini *et al.*, 2007). In this paper we have explored to what extent we can get a more detailed understanding of this result by studying three dimensions of this broad finding. First, we have looked at the four main components of primary exports: agricultural raw materials, food exports, fuels, and ores and metals, as well as components of the aggregate measure of resource rents: forestry, fuels, and minerals. Second we have used different measures of institutional quality, distinguishing between more outcome based measures (ICRG) and more rule based measures (Polity), but also tried exploring if there are differences across functional aspects of institutions, specifically when contrasting institutions regulating the political elite (property rights institutions) and institutions regulating contracts between citizens (contracting institutions) and also contrasting democracies and autocracies. Finally we have looked at the effect of studying different time periods and also explored the time dimension of data using pooled versions with time effects and lags.

In some respects our study can be viewed as an ambitious robustness check of the previous result, but we would like to emphasize that there are also more conceptual reasons for studying differences across types of resources and institutions. As has been stressed in previous work, resources are, for example, not homogenous in terms of capital (or labor) intensity, they are not comparable in terms of technological requirements for their extraction or production, nor are they equally "suitable" for rent-seekers and corrupt politicians, *etc*. Furthermore, institutional measures are in some cases to

Table 5a. *Time, resource exports, and institutions, unrestricted sample*

	ICRG					Polity				
	(1) 196505	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 196505	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	2.515**	3.040**	1.121	2.963***	0.992	0.389	0.502	0.940	0.551	0.109
	(1.247)	(1.220)	(1.513)	(1.112)	(1.317)	(0.759)	(0.749)	(0.852)	(0.475)	(0.531)
primexpgdp	−5.937**	−3.656	−3.818	−5.505**	−2.328	−6.034**	−3.816**	−0.388	−2.925*	−0.496
	(2.384)	(2.724)	(8.711)	(2.747)	(3.511)	(2.335)	(1.718)	(3.640)	(1.543)	(1.831)
primX	5.975	2.113	3.015	5.132	4.125	3.234	3.243	−3.408	1.813	0.0872
	(4.560)	(4.138)	(11.991)	(4.855)	(5.712)	(3.545)	(2.853)	(3.692)	(2.359)	(2.532)
R^2	0.711	0.641	0.534	0.263	0.265	0.673	0.579	0.511	0.278	0.208
Inst	3.544***	3.395**	0.603	1.948	1.685	0.434	0.700	0.790	0.298	0.107
	(1.328)	(1.300)	(1.452)	(1.270)	(1.388)	(0.776)	(0.879)	(1.008)	(0.515)	(0.577)
Agrigdp	−0.159	3.648	−6.670	2.038	13.21	−9.429*	−11.938	−31.980	5.613	12.12
	(8.109)	(14.064)	(22.341)	(17.26)	(11.46)	(5.291)	(7.959)	(22.179)	(8.431)	(9.266)
agriX	−13.989	−13.476	0.531	4.583	−17.77	7.308	15.909	18.073	2.997	−10.70
	(12.877)	(21.659)	(35.285)	(26.14)	(24.78)	(7.303)	(14.766)	(29.993)	(10.75)	(11.74)
Foodgdp	−7.122**	−2.448	−0.359	−4.101	−1.437	−6.743	−3.905	3.697	−4.866	−4.732
	(3.448)	(3.799)	(6.924)	(4.316)	(5.036)	(4.753)	(3.601)	(5.777)	(3.735)	(3.841)
foodX	3.873	−2.282	0.301	2.044	0.115	4.080	1.693	−2.302	3.871	4.549
	(6.147)	(7.385)	(10.008)	(6.687)	(8.010)	(5.622)	(5.369)	(7.784)	(4.213)	(4.903)
Fuelgdp	1.819	4.760*	7.354	−5.003	−3.087	−2.615	−2.085	2.959	−3.252*	−0.815
	(2.772)	(2.459)	(6.892)	(3.706)	(5.173)	(2.186)	(1.653)	(4.038)	(1.653)	(2.164)
fuelX	−6.688	−10.373***	−12.883	4.457	4.431	0.361	1.884	−0.005	1.335	0.355
	(4.918)	(3.375)	(8.963)	(6.085)	(8.521)	(3.880)	(2.520)	(5.002)	(2.154)	(3.313)
ores_metgdp	−14.144***	−12.859**	−27.632***	−14.54***	−7.365	−9.474***	−6.014**	1.299	−1.633	−2.161
	(1.839)	(5.071)	(8.510)	(4.639)	(7.448)	(2.910)	(2.353)	(2.613)	(2.012)	(2.347)
oresX	31.021***	25.264	47.059***	26.66***	11.74	3.088	0.909	−20.207***	−1.886	−0.117
	(4.510)	(18.649)	(14.139)	(9.598)	(12.21)	(6.221)	(10.879)	(5.513)	(6.398)	(5.060)
R^2	0.753	0.701	0.658	0.284	0.261	0.710	0.608	0.617	0.283	0.208
N	94	102	90	654	2165	92	105	89	708	4144

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

be seen as proxies for the same thing but in other cases they capture different aspects of the "rules of game". Finding systematic differences across these dimensions can help inform or distinguish between competing hypotheses about why "good enough institutions" can make a difference for the effect that resource intensity has on economic growth.

Focusing first on specifications where we use alternative measures of property rights institutions (ICRG and Polity), our findings suggest that the basic result that good institutions can reverse the negative effect of resources is not very robust when resources are measured as primary exports in GDP. Regardless of using institutional measures from ICRG or Polity we find that in most specifications the coefficients have the expected signs (i.e., negative effect from resources, positive for institutions and positive for the interaction) but these effects, and in particular the interaction effect, are in many cases only weakly significant. This finding remains when using an IV approach, and hence, confirms the result in Arezki and van der Ploeg (2008) that the institutional reversal of the resource curse is not very robust when using the broad measure of primary exports.

However, we do find that there remains one, relatively strong empirical regularity with respect to the institutional reversal. This is that good institutions measured using "institutional quality" from ICRG turn the resource curse around in countries rich in ores and metals, as measured by both mineral rents as well as ores and metals exports. The result is not universally true across all specifications but it is largely robust across different time periods, for pooled regressions including time effects and lagged variables, and when controlling for outliers. In fact, in the specification using mineral rents and excluding outliers, the negative coefficient for mineral rents and the positive coefficient for interaction effect are statistically significant at least at the 5% level in all of our specifications (and at the 1% level in our most preferred panel specification). It also remains when instrumenting for institutions (though as we have pointed out there are problems with finding good instruments in this context). This is interesting in relation to many studies of the "original resource curse" that emphasize the difference between various types of resources, and in particular, stress problems in countries rich in minerals, or in some cases countries rich in the broader category of "point source" resources (rather than resources in general).[41] Our results suggest that countries rich in ores and metals are indeed the ones with the largest negative effects from the resource, but also that they are the ones where institutional quality really makes a difference for the outcome.

Looking at the other resource components of primary exports the results are more sensitive across specifications. In most instances the signs of the effects are the expected ones but the significance is often quite low and no coherent pattern emerges across specifications. For example, forestry rents appear to have effects similar to those found in minerals but only for some specifications.

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Table 5b. *Time, resource rents, and institutions, unrestricted sample*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	3.037***	3.354***	2.420*	3.771***	1.602	1.645***	0.846	1.646**	1.181**	0.656
	(1.034)	(1.102)	(1.283)	(1.038)	(1.371)	(0.554)	(0.673)	(0.667)	(0.479)	(0.467)
Rents	−8.149	−6.075	−5.968	−0.458	−2.272	0.030	−1.811	0.499	1.805	−0.582
	(5.491)	(4.365)	(5.496)	(4.001)	(4.706)	(2.459)	(2.418)	(2.657)	(1.191)	(1.510)
RentsXInst	10.773	5.120	6.993	−2.817	4.046	−13.604**	−3.857	−6.946	−6.699**	−0.159
	(11.419)	(6.281)	(9.864)	(7.053)	(8.222)	(6.843)	(5.292)	(4.231)	(3.130)	(2.833)
R^2	0.704	0.680	0.511	0.248	0.200	0.688	0.609	0.543	0.250	0.187
Inst	4.205***	4.226***	2.529*	3.828***	−1.617	0.992*	1.032	2.084***	1.682***	0.405
	(1.169)	(1.402)	(1.447)	(1.268)	(1.668)	(0.519)	(0.776)	(0.762)	(0.476)	(0.579)
Forestry	10.810	0.682	−7.356	7.039	−43.95**	−7.978	−4.247	7.378	6.650*	14.77*
	(15.041)	(16.414)	(48.508)	(11.92)	(17.65)	(9.386)	(11.011)	(6.952)	(3.940)	(8.392)
ForestryXInst	−53.279	−53.121	−43.261	−43.36	107.1***	22.315	−11.572	−50.239	−29.34**	−14.80
	(35.098)	(40.077)	(91.532)	(36.57)	(35.09)	(20.103)	(24.380)	(34.075)	(13.63)	(17.25)
Fuel	7.222	−0.119	−0.109	0.224	1.787	0.933	−0.752	1.603	1.667	−1.270
	(8.245)	(4.511)	(4.753)	(3.546)	(4.489)	(2.202)	(2.380)	(2.959)	(1.598)	(2.504)
FuelXInst	−12.956	−2.172	0.114	−3.322	−4.907	−14.040	−1.631	−5.151	−4.829*	1.576
	(13.135)	(6.234)	(8.401)	(5.949)	(7.581)	(9.126)	(4.433)	(4.029)	(2.733)	(4.118)
Minerals	−29.206***	−25.325***	−44.695***	−32.39***	−22.03	−13.911**	−8.833	−20.373	−13.91**	−9.012*
	(5.559)	(8.167)	(14.749)	(9.517)	(14.46)	(5.915)	(8.601)	(12.776)	(6.884)	(4.711)
MineralsXInst	82.930***	32.235	68.565**	46.94*	27.87	11.571	−11.872	8.535	−0.146	4.293
	(20.205)	(30.368)	(30.439)	(24.97)	(30.43)	(14.743)	(10.155)	(14.629)	(6.771)	(5.811)
R^2	0.739	0.724	0.604	0.313	0.211	0.722	0.643	0.593	0.366	0.194
N	95	97	107	596	2439	97	100	116	686	4204

Notes: Dependent variable is growth. Robust standard errors in parentheses.
All regressions include the controls listed in the text (not shown). See text for details.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

Table 6a. *Outliers, using DFITS, resource exports data*

	ICRG					Polity				
	(1) 196505	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 196505	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	1.022	1.438	0.022	0.846	−0.425	−0.382	−0.286	1.266*	0.141	−0.224
	(1.132)	(1.345)	(1.392)	(0.911)	(0.858)	(0.478)	(0.569)	(0.636)	(0.408)	(0.371)
Primexpgdp	−8.596*	−3.328	−2.937	−3.234	−8.290***	−3.624	−3.967***	−0.131	−2.661**	−0.864
	(4.500)	(4.011)	(4.551)	(2.556)	(2.503)	(2.346)	(1.118)	(1.668)	(1.121)	(0.957)
PrimX	10.499	4.847	5.690	2.589	14.85***	1.632	4.425	0.382	3.553	0.630
	(7.619)	(7.069)	(7.449)	(4.625)	(4.699)	(3.246)	(3.343)	(3.067)	(2.275)	(1.623)
Observations	69	68	71	0.425	0.339	66	67	69	0.437	0.306
R2	0.765	0.679	0.628	395	1754	0.741	0.780	0.691	391	2828
Inst	1.463	2.320**	0.182	0.189	−1.635*	−0.364	−0.392	1.537**	−0.468	−1.180***
	(1.140)	(1.101)	(1.490)	(0.969)	(0.903)	(0.461)	(0.519)	(0.595)	(0.507)	(0.370)
Agrigdp	1.717	12.950	−29.187	−4.019	9.640	−0.585	−9.385	−23.409	−1.285	9.913*
	(9.682)	(9.464)	(31.535)	(9.719)	(9.689)	(7.274)	(6.609)	(15.397)	(6.731)	(5.518)
AgrigdpX	−12.750	−32.899*	29.008	5.618	−11.06	−11.152	9.010	14.893	5.362	−13.40*
	(16.036)	(17.571)	(48.049)	(15.27)	(19.48)	(10.093)	(13.709)	(21.703)	(10.31)	(7.568)
Foodgdp	−7.860*	−4.916	−1.732	−2.559	−9.470**	−2.759	−2.330	1.421	−6.326**	−4.722*
	(4.386)	(3.405)	(5.187)	(3.640)	(3.890)	(3.180)	(3.361)	(5.145)	(2.939)	(2.705)
FoodX	11.260	6.087	7.230	1.962	14.61**	6.439	6.076	−1.285	8.774**	7.350**
	(8.362)	(5.469)	(9.383)	(6.543)	(6.703)	(4.715)	(4.387)	(7.313)	(4.035)	(3.490)
Fuelgdp	0.339	7.021***	2.746	0.979	−7.678***	−1.915	0.453	4.027*	−2.195*	−2.257*
	(7.917)	(1.981)	(6.085)	(3.210)	(2.915)	(1.176)	(1.564)	(2.307)	(1.173)	(1.171)
FuelX	−2.667	−16.633***	−1.381	−6.221	12.94***	10.926*	−5.237**	−3.715	0.178	3.042*
	(13.145)	(5.124)	(11.496)	(6.053)	(4.868)	(5.581)	(2.224)	(3.727)	(2.221)	(1.774)
ores_metgdp	−22.240***	−14.212***	−10.830	−26.31***	−5.226	−12.078	−15.868***	−8.745	−16.08***	0.497
	(7.346)	(3.995)	(13.017)	(6.868)	(10.42)	(7.939)	(3.773)	(9.116)	(5.311)	(4.420)

oresX	44.171***	18.821	6.792	50.94***	17.78	12.838	28.976**	−6.427	24.28**	5.343
	(14.062)	(14.353)	(37.009)	(14.52)	(17.32)	(14.645)	(12.563)	(12.948)	(9.443)	(6.455)
Observations	68	64	64	392	1410	62	61	66	391	2756
R^2	0.817	0.881	0.725	0.436	0.363	0.799	0.843	0.735	0.445	0.319

Notes: Dependent variable is growth. Robust standard errors in parentheses. All regressions include the controls listed in the text (not shown). The regressions drop influential observations as identified by DFITS. Dropped countries in upper panel are, in turn: Column (1): DZA, KOR, LBR, LBY, OMN, TTO; Column (2): CYP, DZA, KOR, LBR, LBY, SAU, TTO; Column (3): DZA, IRL, LBR, LBY; Column (6): DZA, GAB, KOR, LBR, LBY, NIC, OMN, SAU, TTO; Column (7): CHL, DZA, KOR, LBR, LBY, OMN, PHL, TTO; Column (8): CHL, DZA, IRL, KOR, LBR, LBY; Dropped countries in lower panel are, in turn: Column (1): DZA, GAB, KOR, LBR, LBY, NIC, OMN; Column (2): CHL, CYP, DZA, GHA, IRL, KOR, MYS, NIC, OMN, SAU, TTO; Column (3): CHL, COG, DZA, GHA, IRL, KOR, LBR, LBY, MLI, MYS, NZL; Column (6): CHL, CIV, DZA, GAB, JAM, KOR, LBR, MYS, NIC, OMN, PAN, TTO, VEN; Column (7): CHL, CYP, DZA, IRL, JAM, KOR, LBR, MYS, NIC, NZL, OMN, PHL, SAU, TTO; Column (8): CHL, COG, DZA, IRL, KOR, LBR, LBY, MLI, MYS. For the panel regressions the procedure is the same, but we chose not to list all combinations due to space constraints; See text for details.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 6b. *Outliers, using DFITS, resource rents data*

	ICRG					Polity				
	(1) 197005	(2) 197505	(3) 198505	(4) 5-Year panel	(5) Yearly panel	(6) 197005	(7) 197505	(8) 198505	(9) 5-Year panel	(10) Yearly panel
Inst	1.207	1.288	−0.722	1.671**	−0.425	1.091***	0.721	0.880	0.740*	−0.224
	(0.825)	(0.879)	(0.938)	(0.686)	(0.858)	(0.354)	(0.462)	(0.560)	(0.374)	(0.371)
Rents	−24.130***	−18.367***	−22.912***	−8.074**	−8.292***	−0.251	−2.423**	−2.775	−1.374	−0.861
	(3.142)	(4.783)	(8.243)	(3.978)	(2.503)	(1.638)	(1.063)	(1.879)	(1.075)	(0.957)
RentsXInst	41.061***	32.402***	44.641***	12.02	14.85***	−17.503***	3.843	0.422	−1.393	0.625
	(6.550)	(10.543)	(16.368)	(7.257)	(4.699)	(5.962)	(8.062)	(2.777)	(2.008)	(1.623)
Observations	80	76	75	454	1754	76	73	75	457	2828
R2	0.828	0.815	0.697	0.411	0.339	0.759	0.739	0.709	0.411	0.306
Inst	1.541	0.936	−1.091	1.993**	−1.311*	0.710*	0.462	0.674	0.509	0.213
	(1.147)	(1.035)	(1.095)	(0.906)	(0.768)	(0.422)	(0.629)	(0.912)	(0.462)	(0.341)
Forestry	−21.601*	−19.393	−58.083*	−2.923	−25.31***	−19.356	−5.737	−22.801	−15.87	16.82**
	(12.922)	(13.239)	(29.360)	(14.23)	(9.763)	(12.088)	(11.586)	(28.133)	(12.69)	(7.605)
ForestryXInst	17.354	27.127	74.428	−8.776	51.93**	6.279	12.403	7.302	14.75	−29.52**
	(24.456)	(33.988)	(51.266)	(29.86)	(24.46)	(22.179)	(25.686)	(40.596)	(19.30)	(12.34)
Fuel	−33.650	−6.659	−11.649***	−1.932	−4.238	−0.193	−2.567***	0.313	−0.363	−0.0475
	(39.835)	(7.138)	(3.801)	(3.662)	(2.857)	(1.662)	(0.931)	(2.446)	(1.119)	(0.948)
FuelXInst	63.886	7.224	23.126***	1.449	7.367	19.122	11.866*	−0.670	−1.477	−0.201
	(74.648)	(14.742)	(7.938)	(6.767)	(5.245)	(24.382)	(6.552)	(3.661)	(2.127)	(1.746)
Minerals	−29.032***	−38.094***	−57.812***	−33.79***	−34.64***	−23.127***	−10.483**	−24.485*	−25.94***	−9.796*
	(6.851)	(10.804)	(17.597)	(6.007)	(9.403)	(5.886)	(4.735)	(13.543)	(6.819)	(5.642)
MineralsXInst	63.842**	70.323**	111.668***	54.56***	73.40***	28.463	−13.053	37.757	18.36	9.251
	(30.089)	(29.807)	(36.513)	(17.53)	(19.55)	(27.860)	(9.043)	(34.572)	(11.85)	(8.851)
Observations	78	76	75	456	1743	73	72	75	457	2819
R^2	0.839	0.782	0.787	0.441	0.340	0.849	0.797	0.702	0.433	0.310

Notes: Dependent variable is growth. Robust standard errors in parentheses. All regressions include the controls listed in the text (not shown). The regressions drop influential observations as identified by DFITS. Dropped countries in upper panel are, in turn: Column (1): BWA, DZA, GNB, IDN, OMN, ZMB; Column (2): BWA, DZA, GUY, IDN, IRL, KWT, LBR, NIC, OMN, SAU; Column (3): BWA, CHN, COG, DZA, GNB, IRL, JOR, KWT, LBR, MYS, NOR; Column (6): BWA, CHL, CHN, DZA, GNB, GUY, IDN, IRN, JAM, OMN, TTO, VEN, ZMB; Column (7): BWA, CHL, CHN, DZA, GAB, GUY, IRN, KOR, KWT, LBR, NIC, OMN, TTO, VEN; Column (8): BWA, CHL, CHN, COG, GNB, IRL, IRN, JAM, JOR, KWT, LBR. For the panel regressions the procedure is the same, but we chose not to list all combinations due to space constraints; See text for details.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Finally, our examination of differences when using measures of "contracting institutions" instead of "property rights institutions" gives a clear message; the former seems to play no role in changing the effect of natural resources on growth. The interaction effect between contracting institutions and resources is basically never significant. This suggests that it is the direct involvement of governments in the resource sectors that determines the potential reversal of the resource curse, rather than the government in its role as enabling contracting between private parties in society.

Returning to the quote from Frederick van der Ploeg's (2011) recent overview on the interesting question being "*why some resource rich economies [.] are successful while others [.] perform badly despite their immense natural wealth*" we do not answer it but our results suggest that institutional quality does indeed seem to have an important role to play and especially in countries rich in minerals.

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NOTES

1. The negative relationship between the primary export share and subsequent growth was first established in a cross-section in Sachs and Warner (1995), and its robustness has been confirmed in, for example, Gylfason, Herbertson, and Zoega (1999); Leite and Weidmann (1999); Sachs and Warner (2001); Sala-i-Martin and Subramanian (2003). The robustness of this relation does not mean that there is a consensus about the existence of a "resource curse" as the views on how to measure resources and their impact on development have been much debated. See, in particular, Brunnschweiler and Bulte (2008), Alexeev and Conrad (2009), and also for example, Manzano and Rigobon (2007) and Lederman and Maloney (2007).

2. The point that resources have contributed positively to growth in the past has forcefully been argued by Wright (1990), David and Wright (1997), and Findlay and Lundahl (1999).

3. Note that the interaction effect introduced in these studies is not the same as controlling for institutional quality. This has been done in many previous studies, including, as pointed out by Mehlum et al. (2006) the study by Sachs and Warner (1995), without changing the negative relationship between primary exports and growth. Also see Torvik (2009) for more on this point.

4. IV regressions turn out to be problematic in our setting. We try using a multiple instrumental variables (IV) strategy, similar to the one in Acemoglu and Johnson (2005), with two sets of (different) instruments for the contracting and property-rights measures of institutional quality, respectively. However, in our data instruments turn out to be weak and consequently results are insignificant (but in line with our other results in terms of point estimates). We also study the development of institutional measures since 1945 for countries that have high and low resource dependence, respectively, two-three decades later (i.e. at the beginning of the periods we analyze) to see if there are any signs of institutional development being historically different across these groups of countries.

5. E.g. Sala-i-Martin and Subramanian (2003) and Boschini et al. (2007).

6. E.g. Auty (1997), Woolcook, Pritchett, and Isham (2001), Isham et al. (2005). Related arguments stressing differences in resource types are made in Sokoloff and Engerman (1997), Leite and Weidmann (1999) and Ross (1999). Sachs and Warner (2001), on the other hand, argue that the distinction is not very important.

7. Dal Bó and Dal Bó (2011) develop a model showing how positive shocks to labor-intensive industries diminish conflict, while positive shocks to capital-intensive industries increase it. Their theory receives empirical support from Dube and Vargas (2009) who contrast conflict propensities in coffee and oil intensive regions respectively in Colombia when income from the respective commodities fluctuate.

8. E.g. Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004). To capture a similar point Acemoglu and Johnson (2005) distinguish between economic and political institutions separating rules and regulations decided upon by politicians (economic institutions) from the rules that restrict the options available to politicians (political institutions). They also emphasize the *durability* of institutions compared with policy decisions (p. 174). Persson (2005) uses the term "structural policies" to separate regulations from more fundamental political arrangements such as constitutions (extensively studied in Persson & Tabellini, 2003).

9. More precisely, they find that the resource curse is present in democratic presidential countries but not in democratic parliamentary countries. They also find that being parliamentary or presidential matters more for the growth effects of natural resources than being democratic or autocratic. This underlines the results in Persson (2005) which suggest precisely that the form of democracy (rather than democracy vs. non-democracy) is important for the adoption of the structural policies that promote long-run economic performance.

10. As with distinguishing between "resource abundance" and "resource dependence", it is not our aim to argue for one over the other but rather to point out that the interpretation of the results depends on whether the institutional measure captures "rules" or "outcomes". For example, Egorov, Guriev, and Sonin (2009), and Guriev, Kolotilin, and Sonin (2008) gain important insights to the mechanisms through which the resource curse may operate by focusing on media freedom and government expropriation, respectively. Both of these measures are clearly government decisions rather than political institutions, but still they capture how natural resources can have different effects depending on the "institutional environment".

11. As noted in the opening paragraph of their paper this conceptual distinction is due to North (1981).

12. 1970 is the starting date in the seminal study by Sachs and Warner (1995). The main results in Mehlum et al. (2006) are for the period 1965–1990, while the main period of study in Boschini et al. (2007) is 1975–1998.

13. Clearly the size of the positive interaction coefficient cannot be immediately be given this interpretation but, as shown in Section 3 calculating the marginal effects bears out this point.

14. This distinction was first made in Stijns (2005) and later by Brunnschweiler and Bulte (2008)

15. Van der Ploeg and Poelhekke (2010) explicitly show that the value of subsoil assets are proportional to resource rents, and thus is also endogenous.

16. As pointed out in Boschini et al. (2007) measures of production to GDP may be even more appropriate since this comes closer to measuring what is "there to grab" at any given point in time. In this sense, the argument made in Alexeev and Conrad (2009) regarding the problem of magnifying the resource curse effect when measuring resources as share of GDP is not an issue in this paper. Unfortunately, this is difficult to test since production data are not available for all types of resources. However, as is shown in Boschini et al. (2007) results are relatively similar when using production and export measures for minerals. Related to the distinction between production and export measures is the point that for some resources, notably oil, production costs vary a lot across countries, see Tsui (2011), and also that taxation varies across countries (being important when, for example, measuring how big government incomes from resources are), see Haber & Menaldo, 2011 and references therein for more on this point. We are not able to take these things into account and again such data are not available for different types of resources.

17. A third reason for using the exports data is that we want to relate our results to the previous literature. As we believe that there are important insights to be found from the decomposition of the interaction effect we want to show that this based on similar data as used in the previous studies, rather than changing both data and method of analysis at once.

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18. The data are based on surveys and on perceptions of the situation in the country, which, apart from making it an outcome measure rather than a measure of the rules, makes it potentially vulnerable to biased assessments. There have been some changes in categories and in how data are presented but essentially these data are the same as used in Knack and Keefer (1995), Hall and Jones (1999), Acemoglu et al. (2001) and many others. We also use the previous version of the ICRG data set, which goes back to 1982 and has some differences in components, to confirm that there are no significant differences across these versions.

19. The institutional quality measure is an average of five variables: (1) The risk of expropriation (exprop) which evaluates the risk "outright confiscation and forced nationalization" of property; (2) The risk of repudiation of contracts by government (repud) which addresses the "possibility that foreign businesses or contractors face the risk of a modification in a contract taking the form of a repudiation, postponement, or scaling down"; (3) Rule of law (rule) which "reflects the degree to which the citizens of a country are willing to accept the established institutions to make and implement laws and adjudicate disputes."; (4) corruption (corrupt) for which lower scores indicate that "high government officials are likely to demand special payments" and that "illegal payments are generally expected throughout lower levels of government"; and (5) Quality of bureaucracy (burqual) where high scores indicate "an established mechanism for recruitment and training", "autonomy from political pressure", and "strength and expertise to govern without drastic changes in policy or interruptions in government services" when governments change. In the analysis below we focus on the standard average measure of institutional quality used in many previous studies. We have also analyzed the components the "institutional quality" measure individually, to see if there are any interesting differences to be found from different aspects of institutional outcomes. The basic result is that this does not seem to be the case. The results from are available from the authors upon request.

20. Clearly the direct measures of constitutions (being presidential or parliamentary, or elections being proportional or majoritarian) are the clearest measures of "rules" available, and, as Andersen and Aslaksen (2008) they also have important implications for understanding the resource curse result across countries. In this paper we limit ourselves to measuring versions of "institutional *quality*". The measure of "constraints on the executive" is in this respect the most "rule based" measure available.

21. Usually this implies that it is difficult to analyze differences across papers since not only the time but also the sample varies.

22. There are similar studies focusing on the effects on corruption. In cross-country settings Ades and Di Tella (1999) and Leite and Weidmann (1999) find that natural resources cause corruption, and in a recent paper Vicente (2008) finds evidence that oil discoveries in Sao Tomé and Principe lead to increases in the future value of office and therefore to an increase in corruption.

23. We do note, however, that, for example, in the period 1970–2005 there seems to be an effect of oil rich countries having less movement toward democracy (reflecting the results found in e.g. Ross, 2001, and Tsui, 2011). There are also some interesting developments across the food rich countries in the post 1970 period but these are not the focus of this paper. We have done this using 1965, 1975, and 1980 as starting dates as well but these do not reveal anything that is qualitatively different from what can be seen in the 1970-figures.

24. All controls are from the World Development Indicators (WDI). In difference to Boschini et al. (2007) we added a dummy for the Middle East and North Africa and also population growth. Results are in general not very sensitive to these additions. For more details on the choice of controls see Boschini et al. (2007).

25. The fact that the measures of institutional quality have been rescaled to a 0–1 measure allows for a direct comparison of the coefficients but the evaluation of the marginal effects requires calculation. We return to this when discussing the results.

26. Results using the unrestricted sample are reported in Section 4.

27. Note that doing so for ICRG means using 1984 data (the starting year for ICRG data) as starting values in 1965 and 1975 respectively. We do this for to be able to compare our results with previous work where this assumption has been made but would like to stress that the proper starting date using ICRG data cannot really be earlier than 1984.

28. The appropriate lag structure varies slightly between regressions but using the Akaike Information Criterion, AIC suggests that one year and sometimes 4th, 5th or 6th year lags is preferred. As we do not want to change lag structure across the regressions we use one year lags only but the results are very similar when adding more lags.

29. The tables present only the variables of main interest. An appendix, available on request, includes all point estimates.

30. We have tried multiple versions of these basic regressions with different starting dates, different panels, and lag structures. The displayed regressions show what we think is a fair representation of the various possibilities.

31. Note that the country coverage is different and somewhat larger for rents than exports. The results are, however very similar when using the 73 countries that make up the intersection between exports and rents data. See appendix table A5 for details.

32. For example, Glaeser et al. (2004) shows that the potential reverse causality (i.e. growth influencing institutions) is something that needs to be addressed. Likewise, Chong and Calderon (2000) performing Granger causality tests, find evidence for two-way causality between economic growth and institutions (using BERI and ICRG data for institutions), and in particular that "economic growth causes institutional quality in a much higher percentage than the opposite" (p. 78). Using measures of democracy as well as corruption Paldam and Gundlach (2008) find support for the 'Grand Transition'-view (income positively effecting institutions) over the 'Primacy of institutions'-view (institutions positively affecting growth in income).

33. A nice example of varying effects of this kind is Bardhan (2005, Table 5, p. 508) who in a levels specification (institutions and income level) uses four different dependent variables and three different country samples.

34. Results using the well-known instruments *population density in 1500* (82 countries in our sample) and *settler mortality* (56 countries in our sample) are available from the authors. Results are in line with the OLS but these instruments turn out to be very weak in our setting.

35. The specification with only one interaction is, when using for example ores and metal exports, of the form;

$$Growth_i = X_i'\alpha + \beta_1 Inst_i + \beta_2 agrigdp_i + \beta_3 foodgdp_i + \beta_4 fuelgdp_i + \beta_5 ores\_metgdp_i + \beta_6 (ores\_metgdp_i \times Inst_i) + \varepsilon_i$$

The reason for running this is that the other interactions are rarely significant and in the absence of more instruments we are limited to including one interaction only.

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36. Notice that the natural resource measure(s) enters both the first and second stage, as suggested by the results in Sala-i-Martin and Subramanian (2003). Natural resources are significant only in a few first stage estimations indicating that the indirect effect of natural resources via institutions on economic development is rather weak. However, when instrumenting ICRG, ores and metals are negatively correlated with institutions, suggesting a potential negative effect from those resources on institutions. Full results are available from the authors.

37. However, institutions and the interaction of institutions and resources (primary exports and ores and metals, respectively) are in general jointly significant, in particular in Table 4, as evidenced by Jpval (p-value for joint significance) in the tables.

38. Recall that ICRG is only available from 1984.

39. Results are available from the authors upon request.

40. Acemoglu and Johnson (2005, p. 957) argues that the advantage of the procedure measures is that they are explicitly about commercial transactions and may be more informative regarding the contracting institutions affecting firms while a potential advantage of the original legal formalism measure is that, because the amount involved is smaller, it may better approximate contracting institutions that are relevant for ordinary citizens. Since the resource curse is not primarily about the behavior of ordinary citizens, it thus makes sense to use the procedural measures and not the index of formalism. The procedural complexity index, 'proc index', measures substantive and procedural statutory intervention in civil cases in the courts. The original measure varies from 0 to 100 with higher values indicating more procedural complexity in enforcing a contract. To conform with our other institutional quality measures we have rescaled the variable into the [0,1]-range and reordered it so that a higher measure means less complexity.

41. Indeed, the 1993 book *Sustaining Development in Mineral Economies: The Resource Curse Thesis* by Richard Auty that coined the term "resource curse" was a study on mineral rich countries rather than countries rich in resources in general.

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APPENDIX

Table 7. *Top fuels and ores and metals export countries (as share of GDP) in 1975*

Most resource export intensive countries in 1975			
Country	Resource export share of GDP in 1975	Significant only after 1965	Comments on discovery/production take-off
Top fuel countries			
Brunei Darussalam	0880	No	1928 (prod starts in 1957) Today gas dominates
Qatar	0739	No	1940 (take-off early 1950s)
United Arab Emirates	0712	Yes	1930s (take-off late 1960s)
Kuwait	0686	No	1930s (take-off early 1950s)
Trinidad and Tobago	0681	No	1866 (expansion 1950s)
Saudi Arabia	0639	No	1936–38 (exp 1960s)
Libya	0572	No	1955–60 (rapid exp in 60s)
Oman	0479	Yes	1930s (no export pre 1967)
Gabon	0317	Yes	1956 (very small until mid 60s)
Nigeria	0276	Yes	1956 (take-off late 60s)
Algeria	0267	No	1890s (low prod until 1960s)
Venezuela, RB	0247	No	1867 (large production pre 1960)
Congo, Rep.	0181	Yes	Prod start 1957 but very low before 1972
Angola	0169	Yes	1955 (expansion in late 1960s)
Indonesia	0151	No	1885 (but not sign until 1960s)
Iran, Islamic Rep.	0139	No	Prod. since 1900, rel low 1960 sharp thereafter
Ecuador	0129	Yes	1967 (take-off early 1970s)
Iraq	0115	No	1920s
Tunisia	0076	Yes	Exploration start around 1965 (take-off thereafter)
Syrian Arab Republic	0076	Yes	Production began 1968
Netherlands	0073	Yes	Gas discovered starting 1948. Take-off late 1960s
Bolivia	0052	Yes	1930s but sign only since late 1960s (mainly gas)
Malaysia	0045	Yes	First drilling 1890s. Sign only after 1970
Kenya	0039	Yes	Explorations in 1960s and 1970s
Canada	0029	No	1947 (steady increase thereafter)
Norway	0024	Yes	1969 take-off in the 70s
Top ores and metals countries			
Liberia	0515	Yes	Iron ore in 1970s
Zambia	0400	No	Copper disc 1913, important pre 1960
Mauritania	0356	Yes	Iron ore, gypsum, copper
Suriname	0333	No	NonFerBaseMet, Alum (since 1920s)
New Caledonia	0267	No	Ferronickel (disc 1875)

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Papua New Guinea	0227	Yes	Copper, gold (mining since 1888) but major new discoveries in1970s and 1980s
Botswana	0221	Yes	Diamonds (disc 1969)
Guyana	0217	No	Bauxite (start 50s), diamond (disc 1900s)
Togo	0131	Yes	Phosphates
Bolivia	0130	No	Gold, Silver, Zinc, etc (long history of mining being important)
Chile	0123	No	Copper, Ferroalloys, Gold, Silver, etc (Long history of mining, peaked in 1970s)
Zimbabwe	0085	No	Copper, Iron ore, gold (peak production in the late 1970s)
Morocco	0078	No	Phosphates
Congo, Dem. Rep.	0075	No	Cobalt, copper, niobium, tantalum, diamonds, gold, etc. Mining expansion since 1950s
Niger	0056	Yes	Uranium (prod start 1970s)
Malaysia	0056	No	Tin, copper, iron ore,
Peru	0054	No	Long history of mining
Gabon	0054	Yes	Manganese (start early 1960s)
Jamaica	0048	No	Bauxite (start late 1950s)
Jordan	0046	Yes	Phosphates
South Africa	0043	No	Diamonds (since late 1860s)
Senegal	0042	No	Fertilizers
Iceland	0038	Yes	Aluminum
Ghana	0033	Yes	Gold, diamonds, bauxite (expansion in 1970s)

Table 8. *The development of export shares for countries that rank the highest in 1980 for fuels and ores and metals, respectively*

Rank of top fuel exporters 1980		Fuel exports as share of GDP			
		1965	1970	1975	1980
SAU	Saudi Arabia	0.49	0.47	0.64	0.58
KWT	Kuwait	0.60	0.55	0.69	0.56
LBY	Libya	0.51	0.57	0.57	0.56
TTO	Trinidad and Tobago	0.45	0.45	0.69	0.49
GAB	Gabon	0.08	0.18	0.32	0.45
OMN	Oman	0.04	0.51	0.48	0.40
COG	Congo, Rep	0.01	0.00	0.18	0.35
NGA	Nigeria	0.03	0.09	0.28	0.29
DZA	Algeria	0.12	0.14	0.27	0.29
VEN	Venezuela	0.24	0.21	0.25	0.23
IDN	Indonesia	0.04	0.05	0.15	0.18
IRN	Iran	0.20	0.19	0.14	0.13
TUN	Tunisia	0.01	0.03	0.08	0.12
NOR	Norway	0.00	0.00	0.02	0.12
ECU	Ecuador	0.00	0.01	0.13	0.11
MYS	Malaysia	0.02	0.03	0.05	0.11
SYR	Syrian Arab Republic	0.00	0.01	0.08	0.10
NLD	Netherlands	0.03	0.04	0.07	0.09
Rank of top ores and metals exporters 1980		Ores and metal exports as share of GDP			
		1965	1970	1975	1980
LBR	Liberia	0.35	0.45	0.52	0.37
BWA	Botswana	0.01	0.05	0.22	0.36
ZMB	Zambia	0.50	0.54	0.40	0.34
GUY	Guyana	0.20	0.24	0.21	0.21
PNG	Papua New Guinea	0.00	0.02	0.23	0.17
NER	Niger	0.00	0.00	0.06	0.17
TGO	Togo	0.05	0.05	0.13	0.11
BOL	Bolivia	0.10	0.14	0.13	0.11
CHL	Chile	0.10	0.10	0.12	0.10
ZWE	Zimbabwe	0.07	0.06	0.09	0.09
PER	Peru	0.06	0.06	0.05	0.07
JAM	Jamaica	0.08	0.07	0.05	0.07
SLE	Sierra Leone	0.05	0.04	0.02	0.06
ZAR	Congo, Dem Rep.	0.05	0.11	0.07	0.06
GAB	Gabon	0.15	0.10	0.05	0.06
MAR	Morocco	0.05	0.04	0.08	0.05
GHA	Ghana	0.02	0.03	0.03	0.05
JOR	Jordan	0.02	0.02	0.05	0.05

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Table 9. *Summary statistics for selected variables*

Variables	*N*	Mean	σ^2	Min	Max
avgr_wdi	1,182	1.347	3.608	−20.37	30.77
icrg_instqual	987	0.532	0.213	0.0521	0.989
polity2	1,116	0.502	0.375	0	1
primexpgdp	1,105	0.156	0.154	0.000414	0.930
agrigdp	1,105	0.0147	0.0252	0	0.193
foodgdp	1,105	0.0654	0.0819	0	0.930
fuelgdp	1,105	0.0482	0.127	0	0.921
ores_metgdp	1,105	0.0276	0.0784	0	0.727
agriXicrg	864	0.00753	0.0132	0	0.142
foodXicrg	864	0.0280	0.0350	0	0.272
fuelXicrg	864	0.0315	0.0819	0	0.739
oresXicrg	864	0.0139	0.0359	0	0.367
agriXpolity2	922	0.00850	0.0173	0	0.193
foodXpolity2	922	0.0333	0.0540	0	0.436
fuelXpolity2	922	0.0170	0.0502	0	0.721
oresXpolity2	922	0.0122	0.0333	0	0.364
lngdppc	1,190	7.466	1.541	4.086	10.77
minvest	1,169	22.87	8.371	4.507	83.05
mopen	1,203	76.52	44.68	2.086	358.7

Notes: Summary statistics for the main variables employed in the analysis.

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Table 10. *Cross-correlation statistics for selected variables.*

	avgr_ wdi	icrg_ instqual	polity2	primexpgdp	agrigdp	agriXicrg	agriXpolity2	foodgdp	foodX icrg	foodX polity2	fuelgdp	fuelXicrg	fuelX polity2	ores_ metgdp	oresXicrg	oresXpolity2
avgr_wdi	1															
icrg_instqual	0.1882*	1														
polity2	0.1383*	0.5502*	1													
primexpgdp	−0.1538*	−0.1731*	−0.2868*	1												
agrigdp	0.0657	−0.0476	−0.0548	0.2098*	1											
agriXicrg	0.1063*	0.2037*	0.0859*	0.1794*	0.9142*	1										
agriXpolity2	0.1031*	0.1743*	0.2798*	0.1287*	0.7983*	0.8720*	1									
foodgdp	−0.0591	−0.1520*	−0.022	0.3077*	0.2310*	0.1782*	0.1259*	1								
foodXicrg	−0.0047	0.1379*	0.1129*	0.2523*	0.2281*	0.2678*	0.1840*	0.8991*	1							
foodXpolity2	0.0242	0.0536	0.3940*	0.2117*	0.1048*	0.1235*	0.2220*	0.7734*	0.7982*	1						
fuelgdp	−0.1166*	−0.0539	−0.2566*	0.7190*	−0.0887*	−0.0856*	−0.1085*	−0.2203*	−0.2076*	−0.1861*	1					
fuelXicrg	−0.1001*	0.0232	−0.2226*	0.7022*	−0.0903*	−0.0714	−0.0934*	−0.2105*	−0.1838*	−0.1699*	0.9744*	1				
fuelXpolity2	−0.0382	0.0287	0.1389*	0.4190*	−0.0462	−0.0246	−0.0052	−0.1196*	−0.0896*	−0.0385	0.5794*	0.5657*	1			
ores_metgdp	−0.0761*	−0.1048*	−0.0936*	0.4221*	0.044	0.0538	0.0780*	0.0224	−0.0206	0.0161	−0.0808*	−0.0781*	−0.0512	1		
oresXicrg	−0.0265	0.0371	−0.0397	0.3886*	0.0847*	0.1245*	0.1472*	−0.0089	0.0088	0.0198	−0.0730*	−0.0616	−0.0394	0.9311*	1	
oresXpolity2	−0.0099	−0.0076	0.1878*	0.3102*	0.1303*	0.1686*	0.2602*	0.0731*	0.0637	0.1682*	−0.0865*	−0.0810*	−0.0004	0.6974*	0.6384*	1

Notes: To save space we do not report correlation statistic for control variables.

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Table 11. *The countries used in estimation*

Algeria (DZA)	Guatemala (GTM)	Oman (OMN)
Argentina (ARG)	Guinea-Bissau (GNB) [b]	Pakistan (PAK)
Australia (AUS)	Guyana (GUY)[b]	Panama (PAN)
Austria (AUT)	Honduras (HND)	Paraguay (PRY)
Bangladesh (BGD)[b]	Hungary (HUN)[b]	Peru (PER)
Bolivia (BOL)	India (IND)	Philippines (PHL)
Botswana (BWA)[b]	Indonesia (IDN)	Portugal (PRT)
Brazil (BRA)	Iran (IRN)[b]	Saudi Arabia (SAU)
Burkina Faso (BFA)	Ireland (IRL)	Senegal (SEN)
Cameroon (CMR)	Israel (ISR)	Sierra Leone (SLE)
Canada (CAN)	Italy (ITA)	South Africa (ZAF)
Chile (CHL)	Jamaica (JAM)	Spain (ESP)
China (CHN)[b]	Japan (JPN)	Sri Lanka (LKA)
Colombia (COL)	Jordan (JOR)[b]	Sudan (SDN)
Congo, D. REP. (ZAR)[b]	Kenya (KEN)	Sweden (SWE)
Congo,Rep. (COG)	Korea, Rep. (KOR)	Switzerland (CHE)
Costa Rica (CRI)	Kuwait (KWT)	Syrian Arab Rep. (SYR)
Côte d'Ivoire (CIV)	Liberia (LBR)	Thailand (THA)
Cyprus (CYP)[a]	Libya (LBY)[a]	Togo (TGO)
Denmark (DNK)	Madagascar (MDG)	Trinidad-Tobago (TTO)
Dominican Rep.(DOM)	Malawi (MWI)	Tunisia (TUN)
Ecuador (ECU)	Malaysia (MYS)	Turkey (TUR)
Egypt (EGY)	Mali (MLI)	United Kingdom (GBR)
El Salvador (SLV)	Mexico (MEX)	United States (USA)
Finland (FIN)	Morocco (MAR)	Uruguay (URY)
France (FRA)	Netherlands (NLD)	Venezuela (VEN)
Gabon (GAB)	New Zealand (NZL)	Zambia (ZMB)[b]
Gambia (GMB)[b]	Nicaragua (NIC)	Zimbabwe (ZWE)[b]
Ghana (GHA)	Niger (NER)[b]	
Greece (GRC)	Norway (NOR)	

Notes: The 73 countries without any note are included in both samples and thus have data for both resource measures for all periods starting in 1970, 1975, 1980 and 1985 (and 1965 for resource exports). Two countries marked with [a] lack data on resource rents for at least one start year from 1970 and are included in the resource exports data sample only ($N = 75$); 13 countries marked with [b] lack data on resource exports for at least one start year from 1965 and are included in the resource rents data sample only ($N = 86$). The main reason behind the smaller sample for resource exports is lack of resource export data for the start year 1985. The following countries are not in the final set only because of missing resource exports data for 1985 (years of missing data in parenthesis): BWA (1981–2000), ETH (1981–1992), GMB (1981–1994), GUY (1982–1996), HTI (1982–1987), IRN (1981–1996), NER (1982–1994), TZA (1981–1996), ZAR (1981–), ZMB (1981–1992).

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