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Oil and the 'new wars': another look at the resource curse using alternative data

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The thesis that natural resources cause conflict is criticized on the basis that resource dependence and conflict are caused by other underlying factors and that the relationship is endogenous. Using disaggregated resource rents on per capita basis, a measure likely to be less influenced by endogeneity, this study finds that oil, rather than other resources, influenced the onset of civil war between 1970 and 2013. Granger causality shows no relationship between resource dependence, measured as resource rents per gross domestic product, and measures of resource rents per capita. Moreover, in multivariate models of societal violence measured by the Global Peace Index (GPI), which capture aspects of 'new wars' witnessed during the post-Cold War era, oil rather than minerals is what matters for predicting societal insecurity defined more broadly than just the absence of war. These results are upheld across subcomponents of the GPI, such as measures of crime, the ease of access of small arms and light weapons, political instability, and the repression of human rights. If oil wealth is associated with these maladies, even if it might not always correlate with war, can it still be asserted that oil is not a curse?

Keywords: natural resource curse; civil war; repression; criminalized violence

> The resource curse is overwhelmingly an oil curse. Ross (2012, 1)

Introduction

The counterintuitive proposition that countries richly endowed with natural wealth do less well economically, politically, and socially relative to poorly endowed countries finds support in theory and empirical work in several disciplines, ultimately making up a body of literature termed the 'natural resource curse' (Frankel 2012; Morrison 2013; van der Ploeg 2011; Ross 2015). While economists argue that natural resources act as a hindrance against sound economic management and economic development (Sachs and Warner 1995), political scientists and other social scientists recognize that natural wealth affects the development of institutions that generate good governance and sociopolitical progress, *ceteris paribus* (Auty 2001; Beblawi 1987; Chaudhry 1997; Karl 1997; Ross 2001). Leaders of resource-rich states lack incentives to build institutions around taxation and the provision of public goods, which increases vulnerability to sociopolitical failure including open rebellion (Fearon 2005; Kaldor, Karl, and Said 2007; de Soysa 2002). While several propositions about natural resources and conflict have been made, including how resources directly invite loot-seeking rebellion (Collier and Hoeffler 2000; Ross 2004), several recent studies raise objections about the empirical validity of previous research on theoretical and methodological grounds (Alexeev and Conrad 2009; Brunschweiler 2008; Brunschweiler and Bulte 2009; Cotet and Tsui 2013). This study revisits the issue to provide new statistical tests utilizing new data on sociopolitical and institutional decay along various dimensions of societal insecurity referred to as the 'new wars' that are captured by the Global Peace Index (GPI) and several of its subcomponents (Institute for Economics and Peace 2014).[1]

The results are easily summarized. I use natural resource rents on per capita basis as a measure of resource wealth, which is less susceptible to bias from endogeneity compared with resource rents per gross domestic product (GDP). Using standard models of civil war and the most widely used civil war onset data for the years 1970–2013, I find that only oil associates positively with the onset of civil war, results supporting several previous studies using various other data and operationalization (Fearon and Laitin 2003; de Soysa and Neumayer 2007). Moreover, Granger causality analyses show that resource dependence measured as rents per GDP and resource abundance, measured as rents per capita, are causally unrelated, which boosts confidence in the results because resources measured in per capita terms are uninfluenced by factors

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that may affect dependence measures as resources per GDP. The results also show that oil rents, and to some extent gas rents, affect societal insecurity measured by the GPI, an aggregated measure of societal insecurity measured by open war and Political repression, criminalized violence, and state militarization. The results are robust to a battery of alternative specifications and support the proposition that oil is particularly troublesome because of the sheer scale of the importance of oil rents as unearned income for governments, which leads to the neglect of public goods, including the public goods of peace and societal security. The results survive a battery of standard robustness checks.

Theory, measurement and empirics of the resource curse

How the curse works

As Michael Ross has suggested, finance streams for states are akin to food for the human body as they shape the form and character of states and the outcomes generated in the economy, polity, and society (Ross 2012). Unearned income in the form of natural resource rents apparently do not lead to the classic Weberian tax state where institutions around taxation and the provision of public goods strengthen and diversify the financial sources of a state, which some suggest increase the independent 'political capacity' of states (Organski and Kugler 1980; Smith 2008). The political capacity of a state in turn produces development and peace, given that the diversification of public financial resources reduces the probability that the state is 'captured' by special interests (Kugler et al. 1998). Rents emanating from a single source, such as natural resources, tend to lead to states that are captured by narrow interests of actors reluctant to make democratic concessions or economic reforms because incumbents fear the loss of future rents (Acemoglu and Robinson 2012a). These states will tend to be weak due to lack of strong institutions of governance, which increases vulnerabilities and generates pathologies, among them the inability to provide public goods including the public goods of decent governance, justice, and peace (Barma et al. 2012). As a consequence, such states suffer political underdevelopment and vulnerability to insurgency, most importantly due to weak administrative capacity (Fearon 2005).

Many explanations focus on the incentives shaping corruption and rent-seeking behavior because of access to easy money in the form of unearned income and the development of oligarchic institutions (Bhattacharyya and Hodler 2010; Leite and Weidmann 1999; Pendergast, Clark, and Van Kooten 2011). Resource-wealthy states are also accused of providing lower levels of public goods in terms of education and health, measured in terms of health outcomes and as health spending (Cockx and Francken 2014; Gylfason 2001; de Soysa and Gizelis 2013). Finally, resource wealth generates weak institutions (anarchic institutions) where groups organize armed violence for capturing rents, making a resource-dependent state prone to violence because either state institutions are too weak to monopolize violence or because the resources themselves invite looting, which in turn finances costly rebellion (Collier et al. 2003; Fearon and Laitin 2001; Ross 2004; de Soysa 2002). Governments of resource-rich countries where income is unearned simply neglect their citizens and institutions that are needed for providing public goods (Papyrakis and Gerlagh 2004). This view has in fact loosely been referred to as the 'political Dutch disease.'[2]

The purely economic view of the natural resource curse is based on explaining the slower-than-average growth among resource-rich countries relative to resource-poor ones (Frankel 2012; Sachs and Warner 1995). Growth failure happens through numerous mechanisms, but the most prominent explanation is that resource booms raise the exporting country's real exchange rate above those of its trading partners, thereby affecting the exports of manufactures and causing the neglect of more dynamic sectors over time. The end result of this reverberates through the entire economy in terms of opportunities forgone for 'learning by doing' in the manufacturing sector. Others show that despite initial income gains when resources are found, long-term effects on income are negative, working through investments, particularly in the generation of knowledge and innovation (Papyrakis and Gerlagh 2006).

According to some, not all natural resources produce a resource curse, particularly when it comes to predicting armed conflict (Le Billon 2001; Lujala 2009; Ross 2012). Point-source resources, such as mineral wealth, are easily captured; therefore, mineral extraction should be most problematic if looting wealth was the mechanism from natural resources to war. However, there does not seem to be a clear indication that mining activity in terms of mineral extraction alone matters (Ross 2004; de Soysa and Neumayer 2007). Natural resources, particularly those concentrated geographically, may invite separatist wars, such as the case in Biafra and South Sudan. Separatist wars, however, have raged in countries such as Ethiopia, Spain, and Sri Lanka where ethnicity rather than access to natural resources is what has mattered. Neither does it seem that the connection from natural resources to conflict works through autocratic regimes. Considering all the evidence, Ross (2012) concludes that it is oil that matters because it tends to have several features that make it prone to the curse.

First, oil tends to dominate an economy due to demand abroad and high prices so that it bestows unusually large finance streams for governments. Thus, the importance of the rent matters because these governments become independent of taxpayers (citizens) as a result, leading to

neglect of public services and other goods that ensure economic and social progress. Second, oil prices have fluctuated in a way that governments have borrowed big in boom years and faced the negative consequences in bust years. Third, the nature of its extracting process allows secrecy around revenue streams, affecting governance and corruption, perhaps fuelling mass-scale grievances and prompting economic decline. Fourth, oil rulers suppress the rights of women, which hurt social and economic progress.

Indeed, some argue that the oil wars emblematic of the crisis in Iraq exemplify how oil-rich countries suffer chronic instability and violence (Kaldor, Karl, and Said 2007). Kaldor uses the term 'new wars' to capture this chronic instability that is made up of a cocktail of weak or failed states, internal war, external war, terrorism, human rights violations, and violence against civilians witnessed in many parts of the world (Kaldor 2013). Such wars stand in stark contrast to the proxy wars of the Cold War period, when the determinants of the outbreak of violence and its longevity were heavily influenced by great powers (Kaldor, Karl, and Said 2007). According to Kaldor, Karl, and Said (2007, 3–4), oil-exporting states are prone to suffer a 'rent-seeking conflict cycle' that explains violence as a result of weakly institutionalized states. These post–Cold War 'new oil wars' are different from the old wars of the Cold War period when geopolitics among great powers ensured some internal stability. Indeed, the only instance when the two superpowers ever came close to a nuclear war during the Cold War was during crises in the Middle East.

In the post–Cold War world, rent-seeking and weak states have led to the outbreak of violence. Since resource extraction requires a modicum of stability, violence, criminality, terrorism, and human rights repression may co-exist without complete breakdown, and private, often criminalized networks with transnational links are often the perpetrators of violence. These are the defining features of the new wars as opposed to the old wars, which had clearly defined battle lines between states and opposition over issues such as control of the territory or populations. Indeed, as some have suggested, these new conflicts are the 'remnants of war' in an age of post-heroic warfare (Mueller 2004).

This study, thus, will utilize data that capture civil war in terms of open warfare between states and rebels and focus on newer data that also capture aspects of the new wars during the post–Cold War period. These alternative data on internal insecurity capture violence and political disarray in terms of armed conflict and crime, terrorism, and political repression, even if full-scale rebellion might be absent. The originators of the GPI explicitly try to capture aspects of what Johan Galtung termed 'negative' and 'positive' peace, where negative peace is simply the absence of violence and positive peace refers to a more autonomous (organic) peace without militarization and repression (Galtung 1969).

Measuring the curse

How to measure the influence of resources on the economy is not conceptually clear, or straightforward. Dependence on natural resources is assumed to be because of an abundance of these resources, which in turn shapes the incentives and motives of policy makers (Karl 1997; Sachs and Warner 1995). However, a measurement of dependence does not necessarily reflect abundance since a country with no industry that has even a limited quantity of resources at its disposal would still show a high ratio of primary commodities to GDP, or total exports (Fearon 2005; de Soysa 2002). Likewise, even an industrialized country with a very expensive resource, such as oil and gas, could show a dependency ratio similar to that of a very poor country exporting a cheap resource, such as agricultural produce. Yes, the influence of resource rents for a government is best measured with GDP as the denominator because it captures the influence of the rents, but since GDP is influenced by many other factors, including conflict, this measure is found to be wanting.

Rents over GDP as the key independent variable measuring resource dependence thus potentially suffers from endogeneity bias in studies of natural resources and civil war (Brunschweiler and Bulte 2008, 2009). Since GDP is the denominator of resource dependence (resources/GDP), and the denominator is affected by conflict, resource dependence increases with conflict, and not the other way around. However, conflict, or conditions that cause it, can also impact the numerator, the rate of extraction of resources, which makes the question whether or not conflict can induce dependence an empirical question that is fairly critical. As Ross (2015) suggests, there is evidence suggesting that bad environments do not boost extraction relative to other economic activity but that it actually slows where institutions are weaker. Thus, the question of how exactly resource abundance and dependence relate to each other is critical to the choice of the measure used in studies of civil war. This study will use Granger causality analysis to examine this issue empirically and to justify the use of resources per capita as a good indicator of resource abundance.

In their carefully considered analysis, Brunschweiler and Bulte (2008) show that a measure of primary commodity exports to GDP does not predict the onset of civil war when endogeneity is accounted with instrumental variables techniques, and that an abundance of natural resources measured as stock of natural capital per capita is favorable to peace. They discard the resource curse as a 'red herring' and conclude that abundant resources may in fact produce peace. Others use oil discovery and reserves data and come to similar conclusions (Cotet and Tsui 2013). However,

because of the use of disparate measures that may or may not properly capture arguments about unearned income and the resource curse, it is quite hard to evaluate all these dissenting findings fully. While accepting the argument that the measure of primary commodity exports to GDP may suffer from endogeneity bias, many aspects of the studies showing that resources are endogenous to conflict raise several other questions.

First, the instruments they use might be challenged on conceptual grounds, and the question of valid instruments is a minefield even when it comes to well-accepted instruments capturing the quality of institutions, such as European settler patterns (Acemoglu, Johnson, and Robinson 2001; Albouy 2012). For example, Brunschweiler and Bulte (2008) used total trade to GDP as an instrument as part of a set of instruments. However, it is quite well established that total trade is affected by the dependent variable, civil war (Magee and Massoud 2011). Thus, much of the findings hinge on the validity of the instruments, which are generally hard to come by. Indeed, correcting for the problem of weak instruments, some have reversed the finding of Brunnschweiler and Bulte's (2008) conclusions on resources and growth already, which also raise doubts about the findings on conflict (van der Ploeg and Poelhekke 2010).

Second, most of the studies discussed also use disparate specifications and estimating strategies. Most include peace years, which capture the years of peace since the last conflict, which should be a strong proxy for all factors that make a country conflict prone, accounting for omitted variables. If conflict causes dependence on natural resources then the brevity of peace should capture much of the effects of conflict on natural resource exports and account for a major portion of the arguments suggesting that resource dependence is endogenous to conflict.

Including several variables that may in fact be highly correlated, such as wealth, economic growth, and regime type that capture institutional quality together with the history of peace, can obfuscate any relationship between resources and conflict and make the interpretation of results extremely difficult (Achen 2005; Schrodt 2014). In many ways, it might be unsurprising that a significant effect is washed out in the same way that any effect of smoking on death is washed out if one controls for heart disease and lung cancer.[3] In other words, conflating confounding and intervening variables makes interpretation of findings extremely difficult and raises the possibility of dismissing an underlying causal effect, particularly working through institutions or other unobserved channels (Ray 2003; Schrodt 2014). The studies mentioned above all dismiss the resource curse because they fail to find a significant effect after controlling for a variety of factors through which resource wealth may influence societal insecurities that may curse a country's prospects despite the absence of open conflict.

Third, the use of resource stock rather than flows may be subject to the same endogeneity concerns as that of the denominator effects of GDP in studies using rents per GDP. For example, countries at peace, such as the wealthier countries, are more likely to have the institutional and commercial capital required for exploration and new finds of natural capital; that is, Norway is likely to know more about what resources are under the ground than would Bangladesh (Frankel 2012; Ross 2015). This might mean that development matters in predicting higher natural capital stock rather than the other way around.[4] Fourth, these studies, which find no effect of natural resource rents on conflict, also suggest that states are likely to repress conflict through higher levels of political repression, while concluding that there is no resource curse and that it may be a 'red herring' as if repression should not matter as a conflict outcome (Brunschweiler and Bulte 2008; Cotet and Tsui 2013). It is in fact quite reasonable to assume that like open-armed conflict, political repression and other societal insecurities due to the 'threat' of violence are likely to degrade a country economically, socially, and politically by suppressing investment, draining human capital, and depleting institutions and finances through high spending on militarization and repression (Acemoglu and Robinson 2012).

Taking these concerns into account, particularly criticism surrounding the problem of endogeneity, this study will use new data on civil violence and societal insecurity to test the effect of natural resource dependence on internal violence. Specifically, I use natural resource rents per capita to avoid the issue of omitted variables affecting the denominator (GDP) in measures of resource dependence. Rents per capita is a good measure of natural resource abundance (Ross 2012). However, as discussed earlier, the speed of extraction of resources determined by other factors, such as conflict, may affect the measure of rents per capita as well. Thus, I first use Granger causality tests to examine the direction of causality between resource dependence measured as rents per GDP and resource abundance measured as rents per capita. These analyses will be followed by reexamining the association between natural resource rents per capita and the onset of civil war using standard data and models of conflict. Additionally, I test the effect of resources on societal insecurity including and exclusive of civil war measured by the GPI, an alternative measure of societal peace. As Schrodt (2014) suggests, using alternative data in civil war studies is one way forward rather than crunching the same old numbers in search of new answers.

Data and methods

The natural resource rents data are taken from the World Bank's World Development Indicators (WDIs).[5] Rents are defined as unit price minus the cost of production

times the quantity produced (World Bank 2011). I then divide total rents by total population for expressing in per capita terms. I assess the effects of natural resources disaggregated into oil rents, gas rents, and mineral rents separately on the outcome variables.[6] The World Bank's rents data show that oil rents per capita and oil rents GDP explain roughly 55% of the variance between them ($p < .01$), which suggests that dependence does not necessarily mean abundance. Moreover, the data show that oil rents dominate an economy compared to rents from other sources among countries with a population of over 1 million. For example, for my entire sample of 147 countries determined by the availability of the GPI data, the average of oil rents per GDP is 6.9% with a maximum of 66.6%, while the average of mineral rents per GDP is 2.2% with a max of only 54% and that of the gas rents is 1.6% with a max of 69%, respectively. This shows that oil's average is more than triple that of average mineral rents per GDP and dominates in terms of importance to an economy (see Table A1, and Table A2 for summary stats). I lag the main independent variables of interest by one year to minimize the effect of simultaneity.

The main dependent variable in the first multivariate analysis of resources and conflict is the standard civil war onset data taken from the Uppsala Conflict Data Project (Gleditsch et al. 2002). These analyses cover the period from 1970 to 2013 (the availability of rents data determine the start period of the analyses). A civil war is defined as a contest between a rebel group and a government that has led to at least 25 battle-related deaths in a single year. Alternatively, I also use the GPI, which captures aspects of the new wars as outlined above. While resource rich states might maintain peace through political repression, a fact that is well supported in the empirical literature, it would be dangerous to conclude that countries do not suffer a resource curse simply because open armed conflict is suppressed (Basedau and Lay 2009; de Soysa and Binningsbø 2009). The GPI captures many aspects of insecurity and militarization. These data are constructed precisely for reasons of capturing the level of general insecurity measured by 22 variables along three main dimensions – actual domestic and international conflict, societal safety and security, and level of societal militarization between the years 2008 and 2013.[7]

The 22 variables making up the GPI are assigned weights from 1 to 5, with internal conflict dimensions making up 60% of the total while external conflict dimensions sum up to 40% of the total. The expert panel decided on the weighting based on the influence of domestic peace on personal security, the higher frequency of domestic insecurity, and because domestic insecurity often fuels external conflict (Institute for Economics and Peace 2014). One weakness in the single-dimensional civil war variable is that it does not capture multiple conflicts going on in a country simultaneously, or the severity of that particular conflict. The GPI rates a country also according to severity based on deaths and the number of people displaced, which is a good proxy for capturing the degree of violence against civilians. The rest of the GPI captures crime, repression, militarization, political instability, terrorism, as well as the degree of external conflict. According to the data in my sample of countries, Denmark, New Zealand, Finland, and Switzerland head the index of peace while Somalia, Sudan, Iraq, and Afghanistan are at the bottom.[8] I find a bivariate correlation of $r = -0.60$ between the GPI (peace) and the Uppsala Armed Conflict Data (25 battle deaths and above threshold), which is quite high, but civil war incidence only explains roughly 36% of the variance of the overall GPI index, which confirms further that peace is a lot more than the absence of open-armed conflict.

I test the effect of resource dependence on subcomponents of the GPI as well, largely because the GPI also captures external conflict unrelated to our discussion. Thus, I isolate the level of crime in society by testing the effects of resources on the prevalence of homicide and serious crime. I also test the degree of militarization in society by assessing the degree of ease with which small arms and light weapons can be obtained as well as the degree of political repression of human rights. I also assess political instability, which is defined as the likelihood of an irregular change in government due to coups or mass political upheaval. The data on crime, terrorism, ease of access of small arms, and political instability are collected by the Economist Intelligence Unit and are based on quantitative data on homicide rates and the like as well as qualitative assessments made on the basis of perceptions of crime by inhabitants as well as expert judgments on questions such as the ability to purchase firearms and the likelihood of irregular regime change. These variables proxy a government's ability and will to monopolize the use of force and provide the public good of peace and societal security. If the natural resource curse works to weaken states because of government neglect of basic public goods, then we would observe the effects of rents independently of other factors, controlled in the model, on crime, political instability, and political terror, which are all features of a weak administrative regime living in the shadow of large-scale dissent.

I keep our models simple and results easy to interpret by controlling for four relevant variables.

$$\mathrm{DV}_{it} = \phi_i + \beta R_{it} + \beta Z_{it} + \lambda_t + \omega_{it}, \qquad (1)$$

where DV_{it} is our dependent variable, GPI and its components and R_{it} is our resource rents variables. The vector Z_{it} captures the effects of the basic control variables I include per capita income, which is a catch-all variable for the level of development and the administrative capacity of a state. Richer individuals have higher opportunity costs

for joining armed groups and richer countries have greater taxable wealth and administrative capacity to deter insurgency. Importantly, per capita wealth also proxies for good institutions necessary for development and thus reduces resource dependency. It is also one of the most robustly related factors to peace (Ward, Greenhill, and Bakke 2010). One might say that resources are problematic if the net effect of resources positively predicts conflict over and above that of per capita income. I also include population size, which is also robustly related to armed conflict, most likely due to diminishing state weakness in population size and because it can be a confounding factor in terms of large countries possessing large amounts of resources and the reverse (Hegre and Sambanis 2006; Ward, Greenhill, and Bakke 2010). These variables are obtained from the World Bank's WDI website.[9]

Importantly, I include a count of the years of peace since the last outbreak of a civil war with over 25 battle-related deaths, which captures aspects related to rebellion-specific capital in a country and accounts for endogeneity between conflict and the denominator (GDP) in our main independent variable – the rents per GDP measure.[10] The brevity of peace is important since GDP growth is affected by war and low growth influences resource dependence. Data on the incidence of civil war since 1946, which I use to generate the peace years, are obtained from the Uppsala Conflict Data Project's website.[11] Following others, I enter regime type in the form of democracy, taking the value 1 if the polity2 score in the POLITY IV data is above 6 and 0 if not and a separate variable for full autocracy taking the value 1 if the polity2 score is below −6 and 0 if not. The reference category is anocracy, which falls between −6 and 6 on the polity scale and is thought to influence the risk of civil war (Ward, Greenhill, and Bakke 2010).[12] The regime variables are in the model because they potentially confound the association between rents and conflict since resource wealth also determines regime type and regime type may in turn determine conflict (Ross 2001). I use this basic model on all the dependent variables, since per capita income and regime type are often used to explain outcomes such as crime, militarization, political terror, and violence. While we do know that fixed or relatively slow changing aspects such as culture may determine crime, militarization, and political instability, they do not determine the availability of natural resources.

The GPI data come in the form of indexes that have a range of different values. However, the GPI and the subcomponents of crime, militarization, and political instability are all continuous variables that are normally distributed, which allows me to estimate standard ordinary least squares models accounting for Huber–White corrected standard errors that are robust to heteroscedasticity and serial correlation by clustering on country units (Wiggins 1999). I also compute time dummies but we do not control for country heterogeneity due to a small T, which would lead to Nickell bias (Nickell 1981). In any case, I am interested in the cross-country variance between resource-rich and resource-poor states on the outcome variables.

To determine the direction of causality between resource rents per GDP and resource rents per capita, I use a dynamic model of Granger causality (Granger 1969). Accordingly, once the past influence of y has been accounted for, the variable x is said to 'Granger cause' a variable y if the past values of x help explain y above and beyond the past values of y (Engle and Granger 1987). I follow Dreher, Gassebner, and Siemers (2012) to account for Granger causality in a panel setting covering around 180 countries during the 1970–2013 period, as

$$y_{it} = \sum_{j=1}^{v} \psi_j y_{i,t-j} + \sum_{j=1}^{\rho} \xi_j x_{i,t-j} + \delta_i + \zeta_t + \omega_{it}, \quad (2)$$

where the parameters are denoted as: ψ_{it} and ξ_{it} for country i during the year t, and the maximum lag length is represented by ρ. Rather than choosing a certain lag length based on Akaike Information Criteria, I compute and report all relevant statistics for one, two, three, four, and five lags in order to assess the sensitivity of the results to the choice of the common lag-order. Note that considering more than five lags could lead to the problem of degrees of freedom. I choose this method because assessing the exact lag length does not have a theoretical basis. While δ_i are unobserved individual effects, ζ_t are unobserved time effects. ω_{it} denotes the error term. Under the null hypothesis, the variable x is assumed to not Granger cause y, while the alternative hypotheses allow for x to Granger cause y after controlling for past influence of the variable y. Note that the joint F-statistic is used to gauge the joint significance of resource dependence and resource abundance and vice versa.[13]

Results

I begin with the Granger causality analyses of natural resource dependence measured as resource rents per GDP and resource abundance measured as the natural log of rents per capita.

The rule of thumb for gauging the presence of causality is if F is greater than 10. The results of all the Granger tests are displayed in Table 1. As seen there, there is very little evidence to suggest that resource dependence causes resource abundance and vice versa. In the first test, resource dependence at 1 lag shows causality of resource abundance ($F = 10.5$) but the association is negative. These results suggest that there is no clear causal relationship either way between rents per GDP and rents per capita.

Next, I turn to the new analyses of civil war onset using the disaggregated rents per capita as the main variables

Table 1. Granger causality: the relationship between resource rents per capita and resource rents per GDP.

Joint *F*-statistic	Lag 1	Lag 2	Lag 3	Lag 4	Lag 5
Oil rents per GDP → Oil rents per capita	10.5***	7.2***	5.6***	3.6***	5.4***
Oil rents per capita → Oil rents per GDP	0.28	2.1	2.9**	2.6**	2.6**
Mineral rents per GDP → Mineral rents per capita	0.46	0.18	0.9	0.79	0.62
Mineral rents pc → Mineral rents GDP	1.1	3.7**	3.1**	2.2*	1.86*
Gas rents GDP → Gas rents pc	0.28	3.3**	2.5*	3.0**	4.76***
Gas rents pc → Gas rents GDP	0.65	0.82	1.4	1.6	2.1*

Notes: Country and time fixed effects included.
***$p < .01$.
**$p < .05$.
*$p < .1$

of interest. As seen there, oil rents per capita has a statistically significant positive effect on the onset of civil war (Table 2).[14]

Neither gas nor mineral rents show any significant association with the risk of onset of civil war, although gas comes very close to being statistically significant. Since the interpretation of logit coefficients is somewhat unwieldy, I compute the substantive impact of oil rents by computing a model prediction holding all variables at their mean values and then re-computing this prediction again with oil rents set at the maximum value. The average prediction of the model increases by 259% when oil rents are increased to the maximum value. Setting oil rents to 0 and then increasing by the maximum value increases the average prediction of the model by 338%. Comparatively, increasing by the full value of per capita income (or the level of development) reduces the average prediction by 558%, which suggests that oil wealth's impact is not trivial. In columns 4–6, I enter the growth rate of GDP per capita, which has a strongly negative effect on the risk of civil war, suggesting that oil's effect on the risk of civil war is net of growth-related factors. Oil, thus, may have important direct and indirect effects on the risk of civil war.

Table 3 presents results of the new analyses of the effects of the aggregated and disaggregated measures of natural resource rents per capita on the GPI and subcomponents of the index that capture aspects of societal insecurity.[15]

Table 3 displays the first set of results, which is the overall index and political instability estimated separately. As seen there, again it is oil that correlates positively with overall insecurity measured by the GPI. A standard deviation increase in oil rents per capita increases the risk of societal insecurity by 12% of a standard deviation of the overall GPI score. An increase in income per capita by a standard deviation reduces societal insecurity by 36% of a standard deviation of the GPI score, which is 3 times the impact of oil, but one that again suggests that oil's impact is hardly trivial. In columns 4–6, when political instability is assessed, both oil and gas show statistically significant effects, while mineral rents remain insignificant. In this case, a standard deviation increase in oil rents increases the risk of political instability by 19% of a standard deviation of political instability.

Table 4 reports results of the effects of resource rents on the level of societal crime measured as the prevalence of homicides (columns 1–3) and serious crime (columns 4–6). Oil, alone, matters again for predicting the prevalence of homicides. When predicting crime, both oil and gas rents show statistically significant effects. Substantively, a standard deviation increase in oil rents per capita would increase homicides by roughly 20% of a standard deviation of homicide. This impact can be compared with a similar impact of 47% (roughly double) of a standard deviation increase in per capita income. A very similar magnitude of the relative impacts holds also for crime. If wealth captures both opportunity costs and the capacity of states to provide the public good of law and order, then resource wealth, particularly oil and gas, show negative effects independent of those of the catch-all variable of per capita income. Not only are oil's and gas's effects statistically significant, but their impacts seem also to be substantively significant.

The results of the effects of resource rents on societal militarization assessed as the ease of access of small arms and political repression of human rights are presented in Table 5. As seen there, oil and gas seem to have statistically significant effects on greater ease of access to small arms, results also supported by others who have assessed the influence of resource wealth on military spending by governments (Cotet and Tsui 2013). A standard deviation increase in oil rents per capita raises the accessibility of small arms by 24% of a standard deviation of the ease of access of small arms variable. Again, only oil seems to matter. The same is true for political repression (columns 4–6). Substantively, a standard deviation increase in oil rents raises political terror by 24% of a standard deviation of the Political Terror Scale. By and large, across all the models, the control variables show the expected results. Only democracy's positive effects on crime relative to negative and statistically significant effects for autocracy

Table 2. The effects of natural resource rents on UCDP civil war onsets, 1970–2013.

	(1) Onset	(2) Onset	(3) Onset	(4) Onset	(5) Onset	(6) Onset
Oil rents/pc(log) *t*–1	0.12**			0.10**		
	(0.05)			(0.05)		
Mineral rents/pc (log) *t*–1		0.02			0.02	
		(0.05)			(0.05)	
Gas rents/pc (log) *t*–1			0.10			0.08
			(0.07)			(0.07)
GDP per capita (log)	−0.45***	−0.32***	−0.39***	−0.40***	−0.29***	−0.35***
	(0.11)	(0.09)	(0.10)	(0.11)	(0.09)	(0.10)
Population total (log)	0.25***	0.31***	0.27***	0.31***	0.37***	0.33***
	(0.08)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
Democracy	−0.13	−0.29	−0.22	−0.17	−0.30	−0.24
	(0.31)	(0.31)	(0.31)	(0.31)	(0.31)	(0.31)
Autocracy	−0.49**	−0.43*	−0.39*	−0.47**	−0.41*	−0.38*
	(0.23)	(0.23)	(0.22)	(0.23)	(0.23)	(0.22)
GDP per capita growth rate				−0.06***	−0.06***	−0.06***
				(0.01)	(0.01)	(0.01)
Constant	−4.72***	−6.35***	−5.35***	−6.11***	−7.58***	−6.75***
	(1.43)	(1.30)	(1.50)	(1.46)	(1.32)	(1.54)
Countries	157	157	157	156	156	156
Observations	5479	5630	5546	5462	5613	5529

Notes: Robust standard errors in parentheses. Peace years and three natural cubic splines computed in all tests (not shown).
***$p < .01$.
**$p < .05$.
*$p < .1$.

are surprising at first glance (see Table 4). These results suggest that repressive measures might be acting as a deterrent against common crime. Democracy, however, has positive effects on the aggregated measure of societal security that includes armed conflict (internal and external), repression, terrorism, and societal militarization even if it may not correlate with civil wars. On balance, democracy is positive for societal security, even if it is less successful

Table 3. Effects of aggregated and disaggregated resource rents per GDP and rents per capita on societal insecurity, 2008–2013.

	(1) GPI	(2) GPI	(3) GPI	(4) Pol.inst	(5) Pol.inst	(6) Pol.inst
Log income per capita	−0.10***	−0.08***	−0.07***	−0.36***	−0.33***	−0.28***
	(0.03)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
Log population	0.04**	0.04**	0.04***	−0.08***	−0.07**	−0.06*
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Peace years	−0.01***	−0.01***	−0.01***	−0.01**	−0.01**	−0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democracy	−0.10	−0.14**	−0.16**	−0.51***	−0.59***	−0.66***
	(0.06)	(0.06)	(0.06)	(0.13)	(0.13)	(0.13)
Autocracy	−0.15**	−0.12	−0.10	0.26*	0.28**	0.38**
	(0.07)	(0.07)	(0.07)	(0.14)	(0.14)	(0.16)
Log oil rents per capita$_{t-1}$	0.02***			0.07***		
	(0.01)			(0.02)		
Log gas rents per capita$_{t-1}$		0.01			0.06***	
		(0.01)			(0.02)	
Log mineral rents per capita$_{t-1}$			0.01			0.01
			(0.01)			(0.02)
Constant	2.36***	2.20***	2.12***	6.77***	6.58***	6.08***
	(0.26)	(0.29)	(0.28)	(0.54)	(0.56)	(0.58)
Countries	147	147	149	147	147	149
Observations	771	769	828	771	769	828
R^2	0.593	0.579	0.576	0.684	0.668	0.651

Notes: Robust standard errors in parentheses; year dummies included.
***$p < .01$.
**$p < .05$.
*$p < .1$.

Table 4. Effects of resource rents per capita on level of homicides and criminality, 2008–2013.

	(1) homi	(2) homi	(3) homi	(4) crim	(5) crim	(6) crim
Log income per capita	−0.53***	−0.41***	−0.42***	−0.45***	−0.43***	−0.33***
	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)
Log population	−0.15**	−0.12	−0.13*	0.01	0.01	0.03
	(0.06)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)
Democracy	0.36	0.09	0.11	0.49**	0.37*	0.23
	(0.26)	(0.27)	(0.26)	(0.19)	(0.19)	(0.18)
Autocracy	−1.03***	−0.84***	−0.76***	−0.73***	−0.70***	−0.52***
	(0.26)	(0.28)	(0.25)	(0.18)	(0.19)	(0.19)
Peace years	−0.01	−0.01	−0.01	−0.01**	−0.01**	−0.01**
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Ln oil rents per capita	0.11***			0.10***		
	(0.04)			(0.03)		
Ln gas rents per capita		0.01			0.09**	
		(0.05)			(0.04)	
Ln mineral rents per capita			0.06			0.04
			(0.04)			(0.03)
Constant	8.92***	7.89***	8.03***	5.92***	5.73***	4.94***
	(1.12)	(1.30)	(1.17)	(0.97)	(1.04)	(0.97)
Countries	147	147	149	147	147	149
Observations	775	773	832	771	769	828
R^2	0.350	0.311	0.330	0.426	0.399	0.379

Notes: Robust standard errors in parentheses; year dummies included.
***$p < .01$.
**$p < .05$.
*$p < .1$.

than autocracies for cauterizing crime and deterring armed rebellion. Democracies decrease human rights repression and political instability while both maladies increase among autocracies.

Robustness checks

I ran several alternative models to ascertain robustness of the main results. First, I test the effects of resources on the perception of criminality reported by citizens obtained from the GPI. The results on oil rents rather than the other two types of resources are upheld.[16] Next, I use the GPI's indicator for societal upheaval in terms of the likelihood of violent demonstrations, and oil rather than the other resources was again statistically significant. I also ran the basic model including growth rate of per capita income. Growth shows no effect on the GPI, and the effect of oil rents remains robust. Due to questions of endogeneity, I added the number of years a country has had continuous conflict before 2008 in addition to the basic years of peace since the last conflict. The results remain identical, and in some cases become statistically more significant when the length of conflict is added to the model.

Finally, I tried a dummy variable of mineral wealth taking the value 1 if mineral rents were larger than 10%, 15%, and 20% of GDP and 0 if not. At no point of these cutoffs did mineral resource rents show a statistically significant relationship to the GPI. In many ways, the results suggest that the resource curse is largely an oil curse in line with the findings of many others (Fearon 2005; Ross 2012; de Soysa and Neumayer 2007). It may very well be the dominance of oil relative to other taxable wealth, or the sheer importance of those rents, that distort the political economies of these countries. Moreover, studies that aggregate all resources are likely to find non-effects given the mixed nature of how rents may affect outcomes. Finally, the mineral rents data as collected by the World Bank could be tainted due to the addition of minerals such as phosphates and even scrap metal, which may account for the non-result of that variable. Future studies might usefully disaggregate the mineral rents data in order to see whether particular minerals matter.

I ran a barrage of additional tests of robustness by first computing the variance inflation factor scores to test for collinearity in the models. I found no evidence to suggest that multicollinearity is a problem in any of the models. Next, I test for influence points by assessing Cook's D scores. Dropping the estimated 32 data points that yielded Cook's D values above $4/n$ strengthened the result of oil's impact on insecurity considerably. The analysis dropped Israel, a non-oil-extracting country, which has little peace and shows some influence on the analysis together with Angola, an oil producer. The results, thus, are also robust to bias from unusually influential cases.

Table 5. Effects of resource rents per capita on the ease of access to small arms and repression of human rights, 2008–2013.

	(1) SALW	(2) SALW	(3) SALW	(4) repress	(5) repress	(6) repress
Log income per capita	−0.43***	−0.38***	−0.32***	−0.24***	−0.21***	−0.18***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
Log population	−0.06	−0.05	−0.04	0.21***	0.22***	0.22***
	(0.05)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)
Democracy	0.22	0.07	−0.03	−0.33**	−0.41***	−0.46***
	(0.19)	(0.18)	(0.18)	(0.13)	(0.13)	(0.12)
Autocracy	−0.24	−0.14	−0.08	−0.16	−0.12	−0.06
	(0.21)	(0.22)	(0.23)	(0.16)	(0.16)	(0.16)
Peace years	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ln oil rents per capita	0.10***			0.05**		
	(0.03)			(0.02)		
Ln gas rents per capita		0.05			0.02	
		(0.03)			(0.02)	
Ln mineral rents per capita			0.03			0.03
			(0.03)			(0.02)
Constant	7.50***	7.07***	6.60***	1.59**	1.34*	1.13*
	(0.89)	(0.93)	(0.91)	(0.64)	(0.69)	(0.64)
Countries	147	147	149	147	147	149
Observations	771	769	828	771	769	828
R^2	0.470	0.427	0.418	0.547	0.540	0.536

Notes: Robust standard errors in parentheses; year dummies included.
***$p < .01$.
**$p < .05$.
*$p < .1$.

Conclusions

There is now considerable disagreement over whether or not countries dependent on natural resources suffer a 'resource curse' in terms of political, social, and economic development (Morrison 2013). While proponents argue that resource rents shape governance in perverse ways because unearned income from natural resources leads to stunted state capacity, often leading to conflict, others suggest that the relationship between resource dependence and conflict is endogenous. Apparently, conflict (or conditions that cause it) shapes the dependence of states on natural resources. Many of these studies cannot be handily analyzed because they have used different conceptualizations of how dependence or abundance of natural resources is measured and the different ways in which empirical models have been handled. Moreover, many have used civil war onset as their conflict variable and suggest that because there is no open warfare, there must be no natural resource curse. In reality, open warfare could be suppressed through repression and state militarization, which might be equally disastrous or worse for sociopolitical development in the long run. I address the issue with alternative data, which capture aspects of so-called 'new wars' that may or may not mean open-armed warfare but capture everyday crime and violence, repression, terrorism, and societal militarization. To minimize bias from endogeneity, which plagues measures of resource dependence, I use natural resource rents in per capita terms, a variable that does not seem to be related causally to resource dependence, measured as rents as a share of GDP.

In multivariate models testing the effect of resource rents on conflict measured by the GPI, I find that oil rents predict higher conflict whereas mineral rents did not matter. These results were generally true across the dependent variables measuring crime, the ease of access to small arms and light weapons, political instability, and the repression of human rights. It is only the extraction of oil, and to some extent natural gas, that showed statistically significant effects on various measures of societal insecurity. These effects were independent of the effects of the catch-all per capita wealth measure, which acts as a proxy for state capacity and the opportunity costs of people for organizing violence as well as regime type and the history of conflict. The results suggest that oil wealth and dependence on it has deleterious effects on social development. Further research needs to isolate the exact mechanisms as to why oil, rather than other resources, matters. The fact that oil tends to dwarf other resources in terms of the sheer size of rents it produces relative to all economic activity for shaping a government and the political system is certainly a reasonable explanation for political Dutch disease. Future research might probe how external interference on geopolitical grounds sullies the politics of oil-rich states, a relatively under-researched area in large-*N* studies despite heavy speculation that foreign companies and great powers foment conflict in oil-rich states (Kaldor, Karl, and Said 2007).

Notes

1. See the appendix (Table A1) for a brief description. These data are derived from qualitative and quantitative sources largely based on data disseminated by the Economist Intelligence Unit, which are also assessed by country experts and others. See http://www.visionofhumanity.org/#/page/news/920.
2. The term 'Dutch disease' is used in the economic realm to explain slow growth following resource booms due to the rise in the real exchange rate of the resource economy relative to its trading partners. Among other things, price volatility of the resource, overenthusiastic borrowing, and the neglect of manufacturing lead to a dangerous cocktail of factors that crimp development, particularly when prices fall. The term stems from the poor performance of the Dutch economy after the discovery of natural gas in the late 1950s.
3. Many critique the so-called kitchen sink approach in many statistical studies of civil war, plus the commission of other 'deadly sins' (see Schrodt 2014). Others advocate simple models with limited (not more than three) independent variables so as not to cloud the analysis and for allowing easier interpretation of relevant results (see Achen 2005; Ray 2003).
4. Brunschweiler and Bulte (2009) anticipate the criticism but suggest that since their stock variable is uncorrelated with educational achievement, they could safely rule out endogeneity between peace and higher natural capital stock. However, they have stock data only for two points in time, which is a considerable weakness.
5. Accessed June 2015. http://databank.worldbank.org/data/. For a closer look at methodologies and data sources for calculating the rents, see World Bank (2011).
6. Since logging requires values above 0, I add $1 to the rents data for all countries before logging.
7. A list of the 22 variables making up the GPI is displayed in Table A1. For a full description of the data sources and methodology, see Institute for Economics and Peace (2014).
8. The GPI collects data on countries with over 1 million inhabitants.
9. Accessed June 2015. http://data.worldbank.org/data-catalog/world-development-indicators.
10. Along with the peace years, it is usual to enter three natural cubic splines to smooth the baseline hazard of the brevity of peace on an onset of civil war.
11. Accessed June 2015. I use the internationalized civil wars category. See http://www.pcr.uu.se/research/UCDP/.
12. The POLITY IV data are obtained from http://www.systemicpeace.org/polity/polity4.htm (accessed April 2015). It is noteworthy that using the full scale rather than dummies does not change the basic results.
13. The data and do files used to generate all results are available at http://folk.ntnu.no/indras/index/publishedarticles/DSRdata
14. The UCDP onset data are produced with 1-, 2-, 5- and 8-year gaps of peace before a new onset can be recorded. I present results for the 5-year hiatus. Note that the results remain basically the same for all onsets, regardless of the definition.
15. Note that higher values of the GPI denote greater insecurity.
16. Results from all the robustness tests will be made available on an online appendix.

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Appendix

Table A1. The variables and weights used in the construction of the GPI (2008–2013).

Indicator	Weight (1–5)
Internal peace	60%
External peace	40%
Perceptions of criminality in society	4
Number of internal security officers and police per 100,000 people	3
Number of homicides per 100,000 people	4
Number of jailed population per 100,000 people	3
Ease of access to weapons of minor destruction	3
Level of organized conflict (internal)	5
Likelihood of violent demonstrations	3
Level of violent crime	4
Political instability	4
Level of disrespect for human rights (Political Terror Scale)	4
Volume of transfers of major conventional weapons, as recipient (Imports) per 100,000 people	2
Potential for terrorist acts	1
Number of deaths from organized conflict (internal)	5
Military expenditure as a percentage of GDP	2
Number of armed services personnel per 100,000 people	2
Funding for UN peacekeeping missions	2
Aggregate number of heavy weapons per 100,000 people	3
Volume of transfers of major conventional weapons as supplier (exports) per 100,000 people	3
Military capability/sophistication	2
Number of displaced people as a percentage of the population	4
Relations with neighboring countries	5
Number of external and internal conflicts fought: 2003–2008	5
Estimated number of deaths from organized conflict (external)	5

Table A2. Summary statistics.

Variable	Obs	Mean	Std. Dev.	Min	Max
onset (5 year hiatus)	5630	0.025	0.155	0	1
civil war incidence	5630	0.168	0.374	0	1
GPI	828	1.997	0.443	1.19	3.5
instability	828	2.439	0.982	1	5
homicide	832	2.546	1.43	1	5
crime	828	2.688	1.113	1	5
access to SALW	828	3.089	1.06	1	5
repression	828	2.594	1.058	1	5
ln oil rents per capita	5426	2.131	2.639	0	10.389
ln gas rents per capita	5491	1.401	2.051	0	9.836
ln mineral rents per capita	5628	1.256	1.68	0	8.408
ln per capita income	5630	7.796	1.623	3.912	11.363
ln population total	5630	16.025	1.57	12.357	21.028
democracy	5630	0.403	0.49	0	1
autocracy	5630	0.247	0.431	0	1
peace years	5630	20.775	18.818	0	67