The wrong suspect. An enquiry into the endogeneity of natural resource measures to civil war.

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The wrong suspect

An enquiry into the endogeneity of natural resource measures to civil war[1]

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Abstract

This paper argues that natural resources in the past have been falsely identified as a cause of civil conflict onset. The idea that natural resources spur conflict has reached a certain degree of acceptance among scholars and policy makers with a number of quantitative studies concluding that natural resource abundant countries are more likely to experience a civil war than countries without such resources. However insights from the 'resource curse' literature suggest that measures of natural resources abundance conventionally used in these studies, can not be considered exogenous. Reversed causality and/or omitted political and economic variables may thus undermine the ability of these studies to prove a causal relationship between natural resource abundance and civil war. In this paper, I propose two more exogenous measures of natural resource abundance such as natural capital and subsoil capital. I identify those studies that find the strongest effect of natural resources on civil war (Collier and Hoeffler 2004, Collier Hoeffler and Rohner 2009) and replicate the exact same specifications, only changing the measure of natural resource abundance. Using the proposed more exogenous measures, I find no evidence that natural resource abundance leads to higher civil war risk. On the contrary, this paper provides some inconclusive evidence for the proposition that natural resource abundance may diminish civil war risk. Furthermore, I interact natural resource abundance with various measures of institutional quality, suggesting that the effect natural resources have on civil war might depend on quality of the institutions in the country at hand.

1. Introduction

"Diamonds are a guerrilla's best friend" (Malaquias 2001). This statement can be taken as an example of a popular conception: natural resources fuel civil war. Contributing greatly to this insight, are various quantitative studies into the causes of civil war, most prominently featuring Paul Collier and Anke Hoeffler (1998, 2004, Collier, Hoeffler and Rohner 2009). Many of these studies find a statistical relationship between the production or export of natural resources and civil conflict. Policy has followed: the Kimberly initiative intending to exclude 'conflict diamonds' from the market and the Extractive Industries Transparency Initiative (EITI) attempting to provide insight into how the proceeds from natural resources are spent, to name two. Diamonds in Sierra Leone and Liberia, timber in Cambodia, oil in Sudan, Aceh and Angola (Ross 2004b): it

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seems hard to argue that natural resources do not play some role in many conflicts. However, is there certainty about a *causal* link: does natural resource abundance indeed *cause* the onset of civil conflict?

In this paper, I will argue that the existence of such a positive causal link is doubtful. Recent insights from the 'resource curse' literature, investigating the link between natural resources and slow rates of growth, indicate that often used measures of natural resource abundance are endogenous to models of economic growth (Brunnschweiler 2008). The same could apply in the context of civil war. A high rate of natural resource exports as a percentage of GDP or a large quantity of natural resources produced can be a consequence of an anticipated war. Alternatively, these measures can be correlated to weak economic structures, low institutional quality or a government with a high discount rate, all factors that can plausibly be related to civil conflict. To address these endogeneity concerns, I will introduce two measures of natural resource abundance that are deemed more exogenous to conflict, natural and subsoil capital, both devised by the World Bank (2006).

My basic strategy then is to reproduce quantitative research finding a positive relationship between natural resources and civil war and replace the original measure of natural resource abundance with the presumably more exogenous ones. I will show that, when doing this, the relationship will either become insignificant or negative. Since Collier and Hoeffler (2004) and Collier, Hoeffler and Rohner (2009) find the strongest relationship between natural resource abundance and civil war, and their specification thus poses the 'toughest test' if one wants to prove that the relationship does not exist, a large part of my analysis will be based on their regressions. However, I will also use a model based on a major alternative specification by Fearon and Laitin (2003). Adding to this, I will interact natural resource abundance with institutions, providing some inconclusive evidence that in countries with weak institutions, natural resource abundance might increase the number of civil wars occurring.

The remainder of this paper is organized as follows. Section two will introduce the existing literature. Section three will discuss problems with often used measures of natural resource abundance and introduce various alternatives. Section four will present the results from my own analysis. Section five will discuss the results and provide some policy implications, before concluding in the final section.

2. Literature

This section will introduce relevant literature on the topic of natural resources and civil war, starting with various empirical analyses investigating the correlation between these two variables. Subsequently, some of the theoretical mechanisms that are thought to connect natural resource abundance and civil conflict are discussed.

2.1 Quantitative analyses

The overwhelming majority of authors investigating the empirical relationship between natural resource abundance and civil conflict estimate a logit model, with civil war onset as a dependent variable. A number of databases on civil war, including the those supplied by the Correlates of War project (Singer and Small 1994, Sarkees 2000) and the UCDP/PRIO Armed Conflict dataset (Gleditsch 2004), are used create a dichotomous variable indicating whether a civil war onset occurred in a given country in a given year. Ongoing wars are either coded as missing data (Collier and Hoeffler 2004) or as no new war start, controlling for incidence of war in the previous period (Fearon and Laitin 2003). Natural resource abundance, measured as natural resource exports as percentage of GDP (henceforth called sxp), is usually one of the explanatory variables in these models.

The most cited article of this type, is authored by Paul Collier and Anke Hoeffler (Collier and Hoeffler 2004). An updated version of this article was published in 2009 (Collier, Hoeffler and Rohner 2009). In both articles, the authors use 5-year periods as the unit of analysis. They arrive at the conclusion that natural resource abundance increases the likelihood of civil war and find this relationship to be U-shaped. The authors hypothesize that medium levels of natural resource abundance increase the chance of war, whilst very high levels of resource abundance generate so much revenue for the government that rebellion becomes infeasible.

A similar analysis, but with very different results is provided by Fearon and Laitin (2003). They construct their own war database, a compilation of various others, and work with 1-year periods in contrast to 5-year periods. Sxp and sxp^2 enter their regression insignificantly. A dummy variable for oil exports is both significant and positively related to civil war however. In explaining this difference, Fearon (2005a) critiques Collier and Hoeffler's choice to use 5-year periods, arguing that it results in inconsistent lag times (variables are lagged to the beginning of the period) and inconsistent treatment of quickly renewed wars. Furthermore, Fearon critiques Collier and Hoeffler for not imputing missing data and thereby accepting a large loss of observations. He also argues that there is no sound theoretical nor empirical reason to expect a quadratic relationship between natural resource abundance and conflict, and that a loglinear relationship is more appropriate. Fearon concludes that the relationship between natural resource abundance and civil war is fragile. If there is one, it is mainly due to oil.

Elbadawi and Sambanis (2002) similarly conclude that the Collier and Hoeffler analysis is sensitive to alternative coding of ongoing wars, data imputation, changing the control variables and using a random effects or probit model instead of a logit model. Humphreys (2003) also finds that both models lack robustness, as interchanging the measures of natural resource between them leads to substantial changes in their significance.

Since these analyses appear to be sensitive to changes in specification, Hegre and Sambanis (2006) list 88 possible explanatory variables and estimate a model for every possible combination of 4 of those. They then compute the chance that there is no relationship between an explanatory variable and civil conflict, given the distribution of the results for each variable. The null hypothesis of no relationship is not rejected for any of their six measures of natural resource abundance, using a definition of war that requires more then 1000 battle-related deaths over the duration of the conflict. If they lower this threshold to 25 deaths per year, oil exports as a percentage of GDP and sxp^2 do appear to be related to conflict.

Whilst some thus refine empirical techniques, other authors critique the studies described above for lacking a formal theoretical basis. Caselli (2006) and Besley and Persson (2008) for example, both construct mathematical models in which natural resource abundance leads to more income available to the government to be distributed at its discretion. This increases the incentive for opponents to unlawfully take over the government in order to take control of this income. Besley and Persson (2008) refine this model by including institutions as a constraint on the extent to which the government can indeed spend proceeds from natural resources at will. Besley and Persson show that their model is confirmed by empirical data using a fixed effects model, with variation in the world price of resources as an explanatory variable. Their model lacks explanatory power in a cross-country setting however. They admit that their measure of natural resource abundance in this setting (an indicator for major oil producers) is likely to be endogenous to conflict.

Brunnschweiler and Bulte (2009) raise the same concern as to the endogeneity of resource dependence. They rightfully argue that a distinction should be made between natural resource dependence and resource abundance. While sxp is may be a good proxy of the former, the latter is better measured by a stock measure of natural resources such as those described in the next section. Brunnschweiler and Bulte furthermore advocate the idea that natural resource abundance is more exogenous to civil war as compared to resource dependence, as is also argued in this paper. They show a single regression indicating that natural resource abundance is negatively related to the chances of civil conflict onset, whilst natural resource dependence is unrelated to it, controlling for abundance. Although this is an interesting result, Brunnschweiler and Bulte do not explore the idea of using these measures of natural resource abundance to replace sxp further. Instead, they use stocks of natural resources, jointly with a number of other variables, to instrument for both natural resource dependence and GDP. Other instruments include trade openness as measured by imports plus exports over GDP, latitude and distance to nearest navigable river. Although various overidentification tests indicate that these variables are exogenous to conflict in their model, this can be contested. Brunnschweiler and Bulte conclude that thus instrumented natural resource dependence is unrelated to civil conflict onset.

The relationship between natural resource abundance and civil war is thus not established beyond any doubt. Although Collier and Hoeffler's analyses can be critiqued on many points, their specification does result in the strongest relationship between natural resource abundance and war. Ross (2004a) even goes as far as to remark that Collier and Hoeffler's specification might be biased such as to find a stronger relation. Using this specification thus seems the toughest test if one wants to claim that the relationship between natural resource abundance and conflict does not exist. This is exactly why I will use it as the basis for my own analysis. However, I will also use a variation on the Fearon and Laitin (2003) analysis as a robustness check.

Partly because of these seemingly unrobust results when using 'natural resources' as a single category and because some scholars claim that different types of natural resources have different effects on different types of war (Ross 2003, Le Billon 2004), another strand of research considers specific types of resources. Oil and diamonds, measured as the production per capita (as an absolute quantity or its corresponding $-value), are most often considered.

Humphreys (2005) for example, finds a positive significant relationship between the quantities of oil and diamonds extracted and the chance of having a civil war. Oil reserves are not robustly related to conflict in his analysis. Lujala, Gleditsch and Gilmore (2005) distinguish between primary and secondary diamonds, using dummy variables for the production of each type. They find that mainly secondary diamonds are related to the onset of ethnic war. By contrast Ross (2006), using the $-value of diamond production per capita, finds that only primary diamonds are related to conflict onset. Secondary diamonds can be related to separatist conflict in his analysis though. In the case of oil, Ross finds onshore oil production to significantly increase the likelihood of all types of war. Lastly, De Soysa and Neumayer (2007) use yet another measure of natural resource abundance: natural resource rents derived from fuel and mineral extraction, as percentage of GNI. They find that energy rents can be related to war, if this is defined as causing at least 25 casualties per year.

Although not the main point of this paper, some innovative research has been done on the influence of natural resources on other aspects of civil war. Examples of this are: war duration (Collier, Hoeffler and Söderbom 2004, Fearon 2005b), rebel recruitment (Weinstein 2005) and casualty rate (Lacina 2006). Another interesting development is the use of data at subnational level, using a 100 x 100 km grid (Buhaug and Rød 2006) or exploiting variation in the intensity of resource production between regions, coupled to world prices (Dube and Vargas 2009) .

2.2 Theoretical mechanisms

If a statistical relationship between natural resources and civil war were to be established, the question of mechanism still remains. How and why would natural resource abundance lead to civil war? Many theories exist.

A popular interpretation put forward by Collier and Hoeffler (2004) is *greed*. The greed mechanism comes in two variations. The presence of natural resources either encourages rebels to fight in order to benefit during wartime or it increases the 'prize' if the rebels manage to 'win' the state.

In their later article (2009) Collier and Hoeffler move away from the greed interpretation in favour of *opportunity*. Natural resources in this view do not motivate rebels, but they provide the financial means to make rebellion, however motivated, feasible.

An alternative mechanism is *weak governance*. Fearon (2005b) argues that governments that derive large revenues from natural resource exports need to raise less tax. This in turn, is presumed to decrease the accountability of the government to its citizens, which might fuel war against it. A smaller need to tax also decreases the need for the government to actually control its territory, making war more feasible. It is also possible that natural resources, which are usually associated with sizeable rents, lead to corrupt governance. (Lujala, Gleditsch and Gilmore 2005).

Low economic growth is another option (Collier and Hoeffler 2005). Sachs and Warner (2001, 1997) argue that a causal relationship exists between natural resource abundance and slow growth, the so-called 'resource curse'. A high rate of natural resource exports could drive up the exchange rate and the prices of non-tradable goods, rendering the manufacturing sector less competitive (a phenomena also called the 'Dutch disease'). Low economic growth might in turn translate into dissatisfaction with the government and/or lower the opportunity costs of being a rebel (Collier and Hoeffler 2005).

A related argument is, that natural resource abundance leads to a *low level of connectedness*. The extractive industry in this view has few backward or forward linkages to other sectors of the national economy, whilst natural resource abundance increases the competitiveness of imports as described above. The resulting lower level of internal trade is in turn associated with lower levels of 'social capital' and intercultural understanding. Both could increase the chances of civil war (De Soysa 2002).

Yet another possible mechanism is *grievance*. Natural resource abundance could lead to grievances among citizens in a number of ways. Extracting them could cause environmental damage (Ross 2004b), or the perception might exist that the proceeds are not fairly distributed (Humphreys 2005). Furthermore, governments might be inclined to take harsher pre-emptive action against separatism in resource-rich parts of their territory, angering citizens in the process (Ross 2004b) or the presence of natural resources might strengthen regional identities (Lujala, Gleditsch and Gilmore 2005).

Lastly, Ross (2004b) hypothesizes that *third party intervention* in support of challengers to the government, either by companies or foreign governments, is more likely in resource-rich countries.

As is evident, many mechanisms ascribing a causal role to natural resource abundance have been proposed.

3 How to measure natural resource abundance?

If we want to establish whether natural resource abundance has any causal effect on civil conflict, we have to make sure we measure it correctly. Ideally, a measure of natural resource abundance should be exogenous to war. This section will argue that commonly used measures are not exogenous and will subsequently introduce various alternatives.

3.1 Endogeneity problems

As outlined in the previous section, commonly used measures of natural resource abundance are: natural resource exports as percentage of GDP (sxp), the amount of resources produced (in absolute quantity or its $-value) and natural resource rents, measured as the value of the total production net of extraction costs. Can these measures be considered exogenous?

A first concern might be reversed causality. This concern is especially valid for sxp. As it is a ratio, its value is influenced by the denominator: GDP. If a war seems to be imminent, manufacturing companies often pull out of the country at hand, decreasing GDP. Companies extracting natural resources however, are more location-specific and cannot move as easily. Thus: the anticipation of war might cause a higher level of sxp, instead of the other way around. As a civil war is often preceded by years of low-level violence, simply lagging sxp (as is done in many of the analyses described) may not adequately address this concern (Ross 2004a). Using some measure based on natural resource production or rents seems less subject to this problem. However, if the government involved attempts to compensate for the declining tax revenue received from manufacturing companies by speeding up the pace of natural resource extraction, similar concerns again arise (De Soysa 2002).

Secondly, there is the possibility of omitted economic variables. Again, sxp is especially vulnerable to this. We should bear in mind that sxp does not actually measure natural resource *abundance*, but rather natural resource *dependence* (Brunnschweiler 2008). Natural resource dependence could plausibly be a characteristic of countries with a weak economic structure: they could be underdeveloped in general (Elbadawi and Sambanis 2002) and/or their institutional environment is not fostering a home-grown manufacturing sector, nor does it attract one from abroad (De Soysa 2002). If less developed countries are indeed more prone to war, as is often suggested, this could lead to a spurious correlation between sxp and civil war onset. Although all authors control for GDP and/or GDP growth, one might wonder whether this adequately controls for economic structure. Production- or rents-based measures represent natural resource *abundance* more adequately, but they are not free from omitted variable concerns. To illustrate this: imagine a developed and a developing country, both having an equal amount of natural resources. It is possible that in the developing country, in which costs to unskilled labour are lower and environment regulations and opportunities in alternative sectors are fewer, a higher absolute level of natural resource production will be observed. On the other hand, since the developed country will have more infrastructure, physical and financial capital, the bias could also work in the opposite direction (Ross 2006).

Furthermore, it is important to remember that the rate of natural resource extraction is to some extent a *choice*. As the national government usually controls the stocks of many types of natural resources and is entitled to give out contracts to exploit them, it can influence the pace of exploitation. This opens the door to potentially omitted political variables. Governments with a high discount rate are typically inclined to exploit natural resource stocks at a fast pace, making 'fast money' at the cost of future generations (Easterly 1999). One could also imagine that corrupt governments will choose to encourage activities that generate large rents, typically natural resource extraction. More general: governments that have less of an interest in sound (economic) policy, plausibly the governments that are more at risk of rebellion, are likely to exploit the national stock of natural resources faster. This again could lead to a spurious correlation between all measures of natural resource abundance and civil conflict.

In sum: commonly used measures of natural resource abundance suffer from various endogeneity problems. Therefore, I will propose some alternatives in the next paragraph.

For completeness, I should mention some other critiques. Sxp is said to exclude diamonds and drugs, while including re-exports of natural resources. It is also considered to be overtly broad and unable to distinguish between different types of resources (Humphreys 2005). Whilst production-based measures are not subject to those critiques, they are usually only available for narrow categories of resources. This makes it difficult to determine whether there is a relationship between natural resource abundance in general and civil conflict.

3.2 Two alternatives found in the 'resource curse' literature

Two alternative measures derive from the 'resource curse' literature. This type of quantitative research tries to establish whether there is a negative relationship between natural resource abundance and economic growth. Using sxp as a measure of natural resource abundance, one would arrive at this conclusion (Sachs and Warner 1997, 2001). However, Brunnschweiler (2008) argues that this is due to the endogeneity problems described above. When she uses the value of the stock of natural resources per capita, the observed relationship between natural resource abundance and growth becomes either insignificant or positive. It would thus be interesting to see if a similar radical change would result from using this measure in the context of conflict.

More precisely, the measures used by Brunnschweiler are natural and subsoil capital (World Bank 2006): the net present value of the flow of income derived from a stock of natural and subsoil resources over an assumed lifespan, in $ per capita. Subsoil capital includes the discounted flow of income from stocks of oil, gas, coal, lignite, bauxite, copper, gold, iron, lead, nickel, phosphate, silver, tin and zinc. Natural capital includes all of these and adds income derived from forests, cropland and pastureland.

As is apparent, natural and subsoil capital are not expressed as a percentage of GDP. Nor are these measures influenced by current decisions regarding the pace of exploitation. This is because only *stocks* are considered and their assumed life span does not vary by country. Essentially, an identical discount rate is imposed on all countries (the only exception to this is the lifespan of forests). This solves many of the endogeneity concerns identified. I argue that since they are more exogenous, natural and subsoil capital can be preferred over more commonly used measures of natural resource abundance.

Obviously, no measure is flawless. One could argue that present stocks of natural capital are not influenced by decisions on the *current* rate of exploitation, but that *past* exploitation rates do influence present stock. If past exploitation rates are in turn somehow related to present conflict, this could pose a problem. But as long as we are willing to assume that past exploitation rates are more weakly related to present conflict then the present exploitation rate, natural and subsoil capital can still be preferred over production-based measures.

Another disadvantage of the World Bank (2006) measures is that they are only available at one point in time (2000). Thus, all variation over time is lost. However, Fearon (2005a) remarks that 75% of the variation in natural resource abundance is a result of variation across countries, whilst variation within countries only accounts for 25% of the variation. Thus, the main source of variation seemingly remains untouched. Furthermore, if we want to explain past conflict with a more recent estimation of natural resource stocks, we have to assume that recent stocks are sufficiently correlated to past stocks. This assumption does not seem unreasonable.

Finally, although natural and subsoil capital lack the precision of production-based measures of specific resources, they do enable us to investigate the relationship between natural resource abundance in general and civil war.

In section four, I will use these arguably more exogenous measures to replace the original ones in various models explaining civil war onset. To my knowledge, natural and subsoil capital are rarely used in the context of civil conflict, the exceptions being De Soysa (2002) and Brunnschweiler and Bulte (2009). In the first article however, older estimates are used (World Bank 1997), available for considerably fewer countries. De Soysa's analysis is also limited to the years 1989-1999 and it defines war as resulting in at least 25 deaths a year, a low threshold. Brunnschweiler and Bulte mainly use natural and subsoil capital as instruments for sxp, along with other variables that are potentially not exogenous, rather then including them in the main regression. This paper will use natural and subsoil capital in a number of influential specifications, in a cross-section set-up (which is arguably more appropriate since natural and subsoil capital are time-invariant) and in interaction with institutional quality.

4 Results

This section presents the results from re-estimating models by Collier and Hoeffler (2004), Collier, Hoeffler and Rohner (2009) and Humphreys (2005) replacing the original measures of natural resource abundance with the ones described in section three.

4.1 Collier and Hoeffler (2004)

Table 2 column 1 replicates the logit regression presented in Collier and Hoeffler (2004) table 5 column 3. The dependent variable is conflict onset: coded as‘1’ if a war started in a 5-year period in a given country and ‘0’ otherwise. Ongoing wars are coded as missing data, unless an ongoing war ended and a new one started in the same country and period. A war is defined following Singer and Small (1994) as an organized military action internal to a state to which the national government is party, resulting in at least 1000 battle-related deaths per year, with each party to the conflict incurring at least 5% of the casualties. For a detailed description of the control variables, I refer to Collier and Hoeffler (2004). The only difference between the original analysis and the one presented here, is that I use robust standard errors, taking into account possible heteroscedasticity and/or autocorrelation within panel data.

Table 2

	(1)	(2)	(3)	(4)
Sxp	37.0716***			42.6033***
	(10.5647)			(11.0933)
Sxp^2	-69.2696***			-78.0838***
	(22.3228)			(23.5757)
Natural capital		-0.0000		
		(0.0001)		
Natural $capital^2$		-0.0000		
		(0.0000)		
Subsoil capital			0.0000	
			(0.0001)	
Subsoil $capital^2$			-0.0000	
			(0.0000)	
Post cold war dummy	-0.8730	-0.9217	-0.8860	-1.2948
	(0.8222)	(0.7592)	(0.7488)	(0.9508)
Male secondary schooling	-0.0288**	-0.0086	-0.0132	-0.0229
	(0.0136)	(0.0166)	(0.0164)	(0.0168)
GDP growth (t-1)	-0.0455	-0.0390	-0.0396	-0.0192
	(0.0533)	(0.0642)	(0.0615)	(0.0644)
Peace duration	-0.0003	-0.0023*	-0.0022	-0.0004
	(0.0013)	(0.0013)	(0.0014)	(0.0015)
Mountainous terrain	0.0054	0.0041	0.0033	0.0110
	(0.0104)	(0.0104)	(0.0105)	(0.0118)
Geographic dispersion	-4.0317***	-2.7158*	-2.8384**	-4.9182***
	(1.4503)	(1.4225)	(1.3749)	(1.5441)
ln Population	0.9272***	0.4013***	0.3993***	1.1295***
	(0.2562)	(0.1445)	(0.1502)	(0.2544)
Social fractionalization	-0.0008**	-0.0001	-0.0001	-0.0008**
	(0.0003)	(0.0004)	(0.0003)	(0.0004)
Ethnic fractionalization	0.0412**	0.0170	0.0172	0.0397*
	(0.0187)	(0.0171)	(0.0172)	(0.0215)
Religious fractionalization	0.0148	-0.0189	-0.0154	0.0051
	(0.0185)	(0.0208)	(0.0200)	(0.0214)
Polarization	-25.2763	-0.6202	-1.9408	-27.5816
	(15.8550)	(13.4866)	(13.9283)	(17.7793)
Ethnic dominance	2.0202*	0.2982	0.4458	2.3759*
	(1.1253)	(0.9143)	(0.9416)	(1.2814)
Democracy	-0.0177	-0.0258	-0.0142	0.0007
	(0.0604)	(0.0732)	(0.0748)	(0.0648)
Income inequality	0.0252	0.0397	0.0362	0.0397
	(0.0291)	(0.0271)	(0.0275)	(0.0338)
Observations	479	450	450	450

Robust standard errors in parentheses. Dependent variable: war onset.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

As we can see from column 1, natural resource exports as percentage of GDP and its square are significant at 1% in the original analysis, suggesting an inverse U-shaped relationship. In column 2, sxp and sxp^2 are replaced with natural capital and its squared term. The relationship observed earlier looses its significance completely. In fact, the coefficient on natural capital is very close to 0. Entering subsoil capital as a measure of natural resource abundance similarly shows no significant relationship between resource and war. Using the natural log of either two variables equally gives similar results (not shown).

Although replacing sxp only results in a marginal loss of observations, one might still be concerned that this loss of significance is an artefact of a different sample composition. Therefore, I ran the original regression on the same sample as used in columns 2 and 3. As shown in column 4, this does not cause sxp to loose significance. In fact, the size of its coefficient increases somewhat. Sample composition does not seem to drive the loss of significance.

Replacing sxp with the alternative measures also has consequences for other variables in the analysis. As we can see, a number of them looses significance. Only geographic dispersion and the natural logarithm of population size appear to remain consistently significant. Experimenting with dropping various control variables or step-wise deletion of insignificant controls does not cause either subsoil or natural capital to become significant at any meaningful level.

Table 2 shows us that using another, more exogenous measure of natural resource abundance, can have large consequences for the relationship observed. With both exogenous measures, no evidence can be found for a positive significant relationship between natural resource abundance and civil conflict.

Since natural capital and subsoil capital are time-invariant, as are many of the control variables and the measures of institutional quality I will use later, the time dimension seems to add little to this new analysis. Therefore, I have converted the data into cross-sectional data. As a rule, I took the average of the available data for a given country. The only exceptions are social and religious fractionalization: since these only change at one point in time, I took the 1960 value, analogous to ethnic fractionalization. The dummy for the post cold war period and the peace duration variable (days since the last war) were dropped, as these are variables that can only be included in analyses with time variation. Lastly, income inequality was dropped, allowing more countries in the sample. Including it would lead to a radical loss of observations, while changing the coefficients of interest very little.

An advantage of the cross-section analysis is that I can construct multiple dependent variables. This allows us to investigate the effect of natural resource abundance on various aspects of civil war. One possible dependent variable is a dummy indicating whether a war started in a given country at any time between 1960 and 1999. Alternatively, the *number* of wars occurring in the same period could be used. I construct both variables twice, once using the list of wars printed in Collier and Hoeffler's article and once using an updated version of the Singer and Small dataset they use (Sarkees 2000). The variables differ considerably, both because the set-up of the dataset has been changed (see Sarkees 2000 for details) and because Collier and Hoeffler seem to have made additions to the original dataset (Elbadawi and Sambanis 2002). Using the updated dataset, I can also obtain the number of days a country was at war between 1960-1999, the average number of days per war, the total number of casualties and the average number of casualties per war. As most of these dependent variables are no longer dichotomous I use linear regression instead of logit regression, except in the case of both dummy variables corresponding to war starts.

Table 3 summarizes the results from running a similar specification as in table 2 on the thus created cross-sectional data. Subsoil capital is not significantly related to any of the eight dependent variables (panel C). Natural capital is equally unrelated to all aspects of civil war, except the total and average number of days at war. It is significant and *negatively* related to the latter two variables (panel B). This suggests that natural resource abundance actually shortens civil war duration, a result that has been obtained before by Humphreys (2005). He argues that natural resource abundance can facilitate military victory if the benefits accrue to the strongest party to a conflict. However, the same pattern cannot be found using subsoil capital, so this result is not very robust. The coefficients on both squared terms are consistently very close to zero, providing little evidence that a quadratic relationship exists. Since this has also been highly criticized by other authors, both on theoretical and empirical grounds (Fearon 2005a, Fearon and Laitin 2003, Humphreys 2005) and because the inclusion

Table 3

PANEL A	(1) War dummy	(2) War dummy	(3) # Wars	(4) # Wars	(5) War duration	(6) Avg duration	(7) Casualties	(8) Avg casualties
Sxp	8.4355	9.5083	4.3789*	4.4297**	3,506.6741	1,127.1339	51,485.5318	28,196.2452
	(7.4263)	(7.5884)	(2.5089)	(2.1033)	(5,989.3269)	(3,388.2371)	(232,463.3070)	(114,593.9888)
Sxp²	-21.5510	-23.7569	-8.7144*	-8.7516**	-8,742.2005	-4,570.2156	9,318.4346	-39,227.0868
	(15.6865)	(16.1745)	(4.6496)	(3.8978)	(11,104.3394)	(6,310.2049)	(430,963.0100)	(213,397.6328)
Observations	118	118	118	118	108	115	109	116
R-squared			0.258	0.282	0.198	0.188	0.164	0.163

PANEL B								
Natural capital	-0.0001	-0.0001	0.0000	0.0000	-0.1321*	-0.0903**	-1.1739	-0.9049
	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0774)	(0.0417)	(3.0119)	(1.1997)
Natural capital²	0.0000	0.0000	-0.0000	-0.0000	0.0000	0.0000*	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0000)
Observations	97	97	97	97	90	94	91	95
R-squared			0.263	0.279	0.232	0.220	0.214	0.192

PANEL C								
Subsoil capital	0.0002	0.0001	0.0000	0.0000	-0.0970	-0.0677	-1.6746	-0.9068
	(0.0002)	(0.0002)	(0.0000)	(0.0000)	(0.0858)	(0.0459)	(3.2893)	(1.2947)
Subsoil capital²	-0.0000	-0.0000	-0.0000	-0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0000)
Observations	97	97	97	97	90	94	91	95
R-squared			0.269	0.294	0.215	0.194	0.215	0.191

Standard errors in parentheses. Dependent variables: (1) war occurrence 1960-1999 (Sarkees 2000) (2) war occurrence 1960-1999 (Collier and Hoeffler 2004) (3): number of wars 1960-1999 (Sarkees 2000) (4) number of wars 1960-1999 (Collier and Hoeffler 2004) (5) total number of days at war 1960-1999 (6) average number of days per war (7) total number of war casualties 1960-1999 (8) average number casualties per war. Controls for male secondary schooling, GDP growth, mountainous terrain, geographic dispersion, ln population, social, ethnic and religious fractionalization, polarization, ethnic dominance and democracy are included.

*** p<0.01, ** p<0.05, * p<0.1

of the squared term does not affect the results meaningfully, I will not use it in the subsequent analysis. The strong relationship between sxp and civil war only partly survives the conversion to cross-section: it is only significant (on a lower level) using the number of wars as a dependent variable (panel A).

This exercise shows that within this cross-section dataset, no positive significant relationship can be found between natural and subsoil capital and any aspect of civil war.

Another interesting question is whether institutions matter for the effect natural resource abundance has on civil conflict. It is possible that natural resource abundance increases the likelihood of civil war occurrence in countries with low institutional quality, while decreasing it in countries with 'good' institutions. The same basic pattern has been suggested in the context of the 'resource curse' (Mehlum, Moene and Torvik 2006). In order to investigate this, I expand the cross-sectional dataset with four different measures of institutional quality: average protection against expropriation (Acemoglu, Johnson and Robinson 2001), the legal structure and security of property rights (Fraser Institute 2008), government effectiveness (Kaufmann, Kraay and Mastruzzi 2008) and overall institutional quality by Mehlum Moene and Torvik (2006), henceforth called MMT IQ. All measures score institutional quality on some scale, with a higher value indicating better institutions. When appropriate, I compute the average of the available scores over the relevant period (1960-2000). Since institutions can be assumed to be endogenous to civil war, I instrument for institutional quality using two different instruments: colonial settler mortality rate, provided by Acemoglu, Johnson and Robinson (2001) and the latitude of the country centre (Gallup, Sachs and Mellinge 1999).

Table 4 reports the results from four linear IV models, interacting subsoil capital with all four measures of institutional quality, using colonial settler mortality as an instrument. The same controls as used in table 3 are included. The dependent variable is the number of wars in the period 1960-2000 according to the updated version of Singer and Small. As we can see at the bottom, the use of settler mortality leads to a large loss of observations, since it is only available for a limited number of ex-colonies. In practice, this also means that results are now comparing the effect of natural resource abundance among developing countries, as most developed countries (not being ex-colonies) are dropped from the sample.

Bearing this in mind, let us look at the results. The coefficient on subsoil capital interacted with institutional quality is negative and significant at 5% in each of the four regressions shown in table 4. This would indicate that a natural resource abundant country with 'good' institutions is expected to have significantly fewer civil wars then a similarly resource abundant country with 'bad' institutions. Subsoil capital by itself has a positive and significant coefficient in three of the four regressions, (the negative coefficient in column 2 is due to the fact that countries can be assigned negative scores on government effectiveness). This would basically mean that in a country with 'bad' institutions, resource abundance does lead to an increased number of civil wars. Institutions enter the regressions with a surprising positive, but insignificant, coefficient.

Table 4

	(1)	(2)	(3)	(4)
Subsoil capital	0.0026**	-0.0001**	0.0032**	0.0027**
	(0.0011)	(0.0001)	(0.0013)	(0.0011)
Interaction	-0.0051**	-0.0008**	-0.0007**	-0.0004**
	(0.0021)	(0.0003)	(0.0003)	(0.0002)
Measures of institutional quality:				
IQ MMT	0.1928			
	(3.7609)			
Government effectiveness		0.0300		
		(0.5860)		
Property rights			0.0249	
			(0.4862)	
Average protection against expropriation				0.0144
				(0.2814)
Observations	49	49	49	49
R-squared	0.431	0.431	0.431	0.431

Standard errors in parentheses. Dependent variable: number of wars 1960-1999 (Sarkees 2000).
Controls for male secondary schooling, GDP growth, mountainous terrain, geographic dispersion, ln population, social, ethnic and religious fractionalization, polarization, ethnic dominance and democracy are included.
*** p<0.01, ** p<0.05, * p<0.1

In order to give an impression of the magnitude of this result, consider a country endowed with the mean amount of subsoil capital and otherwise mean characteristics. If this country has the lowest score in the sample on MMT IQ, the model predicts that it experienced about 3,3 civil wars in the period 1960-1999. If the country has the mean score on MMT IQ, only 0,45 wars are predicted.

Addressing the issue of robustness, I can report that very similar results to those presented in table 4 are derived replacing subsoil capital with natural capital. I arrive at equally similar outcomes when using war occurrence as a dependent variable, and either war occurrence or the number of wars according to Collier and Hoeffler's list. That said, these results do seem sensitive to changing the instrument for institutions. Instrumenting for institutions using latitude generally results in insignificant coefficients on natural and subsoil capital and their interaction terms. As latitude is more widely available then settler mortality, I also ran the same regression on a sample identical to that used in table 4, again resulting in insignificant coefficients.

Reviewing these results, there is some inconclusive evidence that natural resource abundance decreases conflict occurrence in countries with 'good' institutions, while increasing it in countries with 'bad' institutions. If this is the case, this can also partly explain why Collier and Hoeffler (2004) find such a strong and positive relationship between natural resource abundance and conflict. As explained in section three, their measure (sxp) to some extent conflates natural resource abundance and institutional quality. Again, imagine two countries with a similar amount of natural resources, one with 'bad' institutions and one with 'good' institutions. The value of sxp for the first country will be higher then for the latter. To the extent that natural resource abundant countries with bad institutions indeed experience more war, this can lead to a positive correlation between sxp and civil conflict.

However, bearing in mind that this result is not robust to changing the instrument for institutions, one might argue that it is only valid for a sub-sample of ex-colonies or developing countries. This may well be true. However, explaining variation in war occurrence between developing countries seems just as interesting, if not more, as explaining variation between developed and developing countries. And within a sample of developing countries, settler mortality can be preferred over latitude as an instrument. Latitude explains very little variation in institutions in this sample, resulting in a first stage R^2 far below levels generally considered acceptable.

4.2 Collier, Hoeffler and Rohner (2009)

Using more exogenous measures of natural resource abundance causes the conclusions from the Collier and Hoeffler 2004 analysis to change quite dramatically. However, Collier and Hoeffler have published a new version of their analysis in 2009 together with Dominique Rohner, one they claim improves their 2004 one. Therefore, it is interesting to see if using natural and subsoil capital in this new analysis has the same impact.

The 2009 analysis has the same setup as the 2004 one: it is again a logit model, using 5-year periods and the same coding rules for civil war. It is based on a different war database however, by Gleditsch (2004). This defines war as resulting in at least 1000 battle-related deaths over the course of the entire conflict, as opposed to per year. The dataset the model is run on has expanded: an extra 5-year period is added (2000-2004), the number of countries included has increased and some previously missing data has been retrieved. In order to further minimize the amount of observations deleted, Collier and Hoeffler choose to impute missing data on some control variables. Lastly, they change their set of explanatory variables, most prominently by adding the percentage of young man in the population and a dummy for former French colonies in Africa.

Table 5 presents the results using this new specification, which can be compared to those in table 2. Column 1 is the result obtained by Collier, Hoeffler and Rohner (again the only difference being the use of robust standard errors). Sxp and sxp^2 are significant at a considerably lower level then in the previous analysis (10%). Column 2 and 3 show that this relationship again turns insignificant using natural and subsoil capital and that similarly small coefficients are obtained. The only difference is that subsoil $capital^2$ is significant at 10% level, but with subsoil capital itself being insignificant, this result seems hard to interpret.

In this analysis, considerably more observations are lost when replacing sxp with natural and subsoil capital. Running the original analysis on exactly the same reduced sample is not possible in this case though, as no sxp values are available for 35 observations that do have values for natural and subsoil capital. The results obtained by also omitting these 35 observations are presented in column 4. The fact that sxp and sxp^2 enter this regression insignificantly, indicates that we cannot exclude the possibility that (part of) the loss of significance is due to changes in sample composition.

Looking at the control variables, we can see that mountainous terrain gains significance upon replacing sxp with natural and subsoil capital, while the French African colony dummy looses it. Comparing columns 2 and 3 with column 4 however, indicates that this could again by due to changes in sample composition. Interestingly, democracy is significant in column 3. This could indicate that sxp has been proxying for some aspect of democracy. This would confirm my hypothesis that sxp to some extent captures institutional quality. However, sample composition could equally be causing this.

All and all, replacing sxp with natural and subsoil capital in both the new and the old analysis results in a very similar picture: no positive significant relationship can be found between these more exogenous measures of natural resource abundance and civil conflict.

As with the Collier and Hoeffler 2004 analysis, I subsequently went on to convert the 2009 data into cross-sectional data and interact natural and subsoil capital with institutional quality. The results mirror the ones obtained before. No proof for a positive relationship between natural and subsoil capital and war can be found in a cross-section analysis. A consistently negative and significant coefficient on the interaction term is obtained, coupled with a positive and significant coefficient on subsoil and natural capital. Again, using latitude as an instrument for institutions leads to in an aberrant result. Since these results are so similar, they are not presented here, but can be obtained from the author.

Table 5

	(1)	(2)	(3)	(4)
Sxp	7.1495*			4.7999
	(3.9831)			(5.0865)
Sxp²	-14.5810*			-9.4272
	(7.7718)			(11.6577)
Natural capital		0.0000		
		(0.0001)		
Natural capital²		-0.0000		
		(0.0000)		
Subsoil capital			0.0001	
			(0.0001)	
Subsoil capital²			-0.0000*	
			(0.0000)	
ln GDP	-0.2323*	-0.3445*	-0.4437**	-0.3607**
	(0.1206)	(0.1924)	(0.1801)	(0.1462)
GDP growth	-0.1477***	-0.0969*	-0.0915*	-0.1205***
	(0.0346)	(0.0558)	(0.0535)	(0.0442)
Post cold war dummy	-0.1349	-0.2139	-0.2096	-0.0342
	(0.3710)	(0.4364)	(0.4273)	(0.4459)
Previous war dummy	-0.0818	0.0355	0.0666	-0.1376
	(0.4879)	(0.5125)	(0.5103)	(0.5671)
Years since previous war	-0.0579***	-0.0441***	-0.0420**	-0.0540***
	(0.0154)	(0.0165)	(0.0168)	(0.0176)
Former French African colony	-1.2032**	-0.7685	-0.7162	-0.8593
	(0.6046)	(0.6246)	(0.6326)	(0.6898)
Social fractionalization	2.1734***	1.9105**	1.7676*	1.8951**
	(0.7820)	(0.9291)	(0.9351)	(0.9662)
Proportion of young men	12.4933	18.2281**	18.4066**	18.1814**
	(7.9723)	(8.2574)	(8.2181)	(8.4871)
Population	0.2764***	0.1810**	0.1566**	0.2097**
	(0.0856)	(0.0743)	(0.0765)	(0.1063)
Mountanous terrain	0.0110	0.0188***	0.0185**	0.0190**
	(0.0074)	(0.0072)	(0.0075)	(0.0082)
Democracy	0.0123	0.0888	0.0976*	0.0800
	(0.0452)	(0.0567)	(0.0582)	(0.0586)
Observations	1063	863	863	828

Robust standard errors in parentheses. Dependent variable: war onset.
*** p<0.01, ** p<0.05, * p<0.1

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4.3 An alternative specification

I have shown how natural and subsoil capital are consistently insignificant in explaining civil war onset using Collier and Hoeffler's specifications. As emphasized in section two however, one major critique directed at these kind of analyses is that they are not robust to changes in specification. As a robustness check, I therefore also show some results based on the most cited alternative (Fearon and Laitin 2003). More precisely, the analysis used is that of Humphreys (2005), who uses the same data on the dependent and control variables as Fearon and Laitin. The only difference is that a rare events logit analysis[2] is used instead of an ordinary logit model. As emphasized, Fearon and Laitin (2003) find no significant relationship between sxp and civil conflict.

An important difference between Humphreys' specification and that of Collier and Hoeffler is, that he uses 1-year periods instead of 5-year periods. The dependent variable is a '1' or a '0' depending on whether a war started in a given country in a given year. Ongoing wars are also coded differently: as ongoing wars mean no new war start, they are coded as '0'. A dummy is added to control for whether a war occurred in the previous year, thus capturing ongoing wars. The coding is based on an alternative war database, compiled by Fearon and Laitin (2003). A conflict is considered a war if it results in more then 1000 deaths over its entire course. The explanatory variables include many variables also included in Collier and Hoeffler (2004), adding a dummy for non-continuous territory and a measure of political instability. All explanatory variables are lagged one period.

In this specification, I add natural or subsoil capital and their squared terms, as proposed by Collier and Hoeffler. Table 6, column 1 and 3 report these results. Both natural and subsoil capital enter insignificantly. Although the coefficient on both squared terms is significant, it has the 'wrong' sign, as an inverse U-shape would imply a negative coefficient. And again this result is difficult to interpret in the absence of significant coefficients on natural and subsoil capital.

Because the regressions fail to give evidence for an inverse U-shaped relationship and because Fearon (2005a) strongly argues for a loglinear relationship, I also add the natural logarithm of both natural and subsoil capital. Column 4 again indicates that there is no significant relationship between subsoil capital and war onset. The natural logarithm of natural capital however, is significant and *negatively* related to the chance of civil war, as shown in column 2. This would indicate that civil war risk is *smaller* in natural resource abundant countries.

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[2] King and Zeng (1999a, 1999b) and Tomz, King and Zeng (1999)

Table 6

	(1)	(2)	(3)	(4)
Natural capital	-0.0001			
	(0.0001)			
Natural capital²	0.0000**			
	(0.0000)			
ln Natural capital		-0.2418**		
		(0.1004)		
Subsoil capital			-0.0000	
			(0.0001)	
Subsoil capital²			0.0000**	
			(0.0000)	
ln Subsoil capital				-0.0407
				(0.0464)
War previous year	-0.8678**	-0.8728**	-0.8285**	-0.8578**
	(0.3986)	(0.3973)	(0.3939)	(0.3975)
GDP	-0.2206***	-0.2184***	-0.2900***	-0.2530***
	(0.0783)	(0.0744)	(0.0854)	(0.0800)
ln Population	0.2745***	0.2844***	0.2737***	0.3038***
	(0.0830)	(0.0838)	(0.0820)	(0.0904)
ln % Mountanous	0.2364**	0.2320**	0.2359**	0.2252**
	(0.1050)	(0.1034)	(0.1073)	(0.1057)
Noncontinuous	0.3970	0.3974	0.4007	0.4041
	(0.3906)	(0.3930)	(0.3842)	(0.3934)
Instability	0.8682***	0.8663***	0.8738***	0.8828***
	(0.2956)	(0.2954)	(0.2932)	(0.2927)
Democracy	0.0218	0.0219	0.0257	0.0227
	(0.0220)	(0.0223)	(0.0214)	(0.0221)
Ethnic fractionalization	0.6387	0.6472	0.5564	0.6342
	(0.5309)	(0.5318)	(0.5361)	(0.5286)
Religious fractionalization	0.2114	0.1812	0.3286	0.2316
	(0.6958)	(0.6934)	(0.6691)	(0.6997)
Observations	3970	3970	3970	3970

Robust standard errors in parentheses. Dependent variable: war onset.
All explanatory variables are lagged one period.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

It is perhaps not surprising that natural and subsoil capital are insignificant in a model that did not find any relationship between natural resource abundance and civil war to begin with. However, it does indicate that my results are not sensitive to changing between these two specifications, whilst other results are proven to be exactly that (Humphreys 2003). It is also interesting that in one case, a negative relationship between natural resource abundance and civil war is found, indicating that resource abundant countries could be 'blessed' instead of 'cursed' (analogous to Brunnschweiler 2008).

5 Discussion and policy implications

From this paper, four important results stand out. First, endogeneity is a very real concern if one wants to establish what effect, if any, natural resource abundance has on civil war risk. Secondly, no proof can be found that more exogenous measures of natural resource abundance are positively related to the chances of civil war onset. Thirdly, the effect of natural resource abundance on war in a given country might depend on its institutional quality, although the evidence for this is inconclusive. And finally, equally based on inconclusive evidence: natural resource abundance might actually *decrease* the chance of civil conflict. If we take these results at face value, what implications do they have for scientific research and policy? This section will elaborate on this question.

5.1 Addressing endogeneity

As Hegre and Sambanis (2006) remark, endogeneity issues are often ignored in the literature on civil war (and they continue to ignore them themselves). If anything, this paper has proven that we cannot afford to do so. In order to draw sensible conclusions on the causes of civil war, possible endogeneity has to be taken into account, as not doing so could lead to a spurious correlation between natural resource abundance and civil war onset. Since this type of research has drawn a great deal of attention from policy makers and a number of policy initiatives is based on the proposition that natural resource abundance is related to war risk, this seems especially undesirable.

Furthermore, failing to correct for endogeneity can have a wider impact on models intended to explain civil war occurrence. As paragraph 4.1 shows most clearly, changing the measure of natural resource abundance can substantially influence the significance of other variables. If sxp indeed partially captures institutional factors, one would expect that its use underplays the importance of institutional quality in causing civil war. This remains a theoretical expectation however, as it is not shown conclusively in the empirical research presented in this paper.

The need to address endogeneity is accompanied by a need for more and better exogenous variables. Natural and subsoil capital, in my opinion, come a long way in being exogenous and are very recommendable for use in future research. However, they are only available at one point in time, so attempting to expand the availability of these measures seems valuable.

Searching for more and better instruments for particular types of natural resources is another promising strategy. In order to find them, one could look at the geological circumstances under which certain resources are formed. Oil for example, one of the resources I consider most prominently in need of a valid instrument, tends to form in areas gradually sinking, in which lots of sediments are deposited. So the rate of subsidence of the soil could possibly explain occurrence of oil. Some measure of indigenous soil type could equally instrument for oil and various other types of resources. As diamonds are associated with certain types of rock (mainly kimberlite and lamproite deposits) and volcanic activity, the presence of these deposits or of volcanoes could be exploited as an instrument.

Thus, endogeneity should not and does not have to be ignored. A number of promising options to address this problem are available.

5.2 Natural resources: an undeserved reputation

Using exogenous measures of natural resource abundance, there is no evidence that it is positively related to civil war risk, except perhaps in countries with 'bad' institutions. If we take this to be true, it challenges a number of claims made in the literature and the effectiveness of various policy interventions.

Let's start with the literature. First of all, there is no *a priori* reason for countries to lament being resource abundant. There is no proof that natural resources unavoidably form a 'curse' of some kind, causing conflict. I was not able to find evidence for a 'resource trap' (Collier 2008), leaving countries unable to escape recurrent conflict, solely because they are resource abundant. Inconclusive evidence can be found for the proposition that a combination of natural resources and bad institutions could cause any of these things, but further research seems necessary in this field.

Related to the above, doubt is shed on other negative effects natural resource abundance is said to have. A number of the mechanisms described in paragraph 1.2 rely on these effects, claiming that natural resource abundance leads to weak governance, low economic growth, low internal connectedness or third party intervention in a conflict. If we are willing to assume that these are critical factors in explaining war, this paper presents no proof that natural resource abundance is related to any of these negative effects in a full sample of countries.

A third often heard conclusion is that civil war is caused by 'greedy' people, motivated by economic gain (Collier and Hoeffler 2004). Obviously, it is impossible to assess the motivation of actors with this type of analysis. However, in as far as natural and subsoil capital and instrumented diamond production adequately measure the scope of potential gain, more potential gain does not necessarily mean a higher chance of civil war.

Finally, the relationship between natural resource abundance and civil war is an often cited argument in the debate on whether civil conflict is a function of motivation or feasibility (opportunity to mobilize financial resources). Natural resources are said to provide evidence for the latter, as the proceeds are argued to enable rebels to fund their activities regardless of motivation. The analysis presented here does not exclude the possibility that rebels or governments are using funding derived from natural resources during wartime. However, funding from natural resources does not proof to be a constraining factor. This appears to mean that proponents of the feasibility theory have one less argument.

Although the evidence from interacting natural resource abundance with institutions is somewhat weak, how could we interpret the finding that natural resource abundance *does* increase civil war risk in countries with 'bad' institutions? One plausible interpretation could be that when natural resources are extracted by irresponsible governments, certain grievances arise: dissatisfaction with how the proceeds are spent, environmental damage, preventive strikes against rebellious populations, stronger regional identities without civil rights enabling people to express them. Another possibility is, that resource extraction in 'bad' institutional environments leads to squandering of the proceeds and thus lower growth. Alternatively, a low quality of institutions might imply worse government control over its territory making rebellion, somehow encouraged by natural resources, more feasible. Which, if any, of these interpretations is correct remains a topic for further research.

Numerous policy initiatives based on the proposition that natural resource abundance increases civil war risk have been suggested. How are they affected by these conclusions?

The Kimberly initiative, attempting to ban 'conflict diamonds' from the market, is one of the best known policies in this field. As mentioned before, this paper has not excluded the possibility that warring parties derive finance from natural resources, given that there is a conflict. If this has been proven to be the case, cutting off these finances may well be a feasible strategy. This is a subject for separate evaluation. However, since this analysis finds no proof that natural resource abundance is a critical factor in financing or motivating war *starts,* the believe that the Kimberly initiative will *prevent* wars from breaking out is doubtful.

Another often cited initiative is the Extractive Industries Transparency Initiative (EITI). This is intended to give civilians more insight into how the proceeds of natural resources are spent and/or to spend these on social programmes (Humphreys 2005). This type of initiative seems promising if it succeeds in improving institutional quality *and* if resource abundant countries with bad institutions indeed run more risk of civil war. However, its workings are limited to a very specific kind of institutions: those rules and regulations covering the spending of natural resource proceeds. If a broader range of institutions influences the effect natural resource abundance has on civil war, a broader campaign to improve institutional quality might be required.

Other options named in the literature are smoothing the shocks in the prices of natural resources (Collier and Hoeffler 2005) and diversification (Humphreys 2005). If shocks are indeed positively related to the onset of conflict, no evidence is found that natural resource abundance leads to more shocks. This line of policy thus does not seem very fruitful. In respect to the diversification strategy: to the extent that this intends to decrease the *dependence* on a single source of revenue, one could hardly argue that this is an unwise strategy irrespective of civil war risk. However in this context, there seems to be no reason to lament having large stocks of resources, nor to refrain from using them in a sustainable fashion.

5.3 Blessing instead of curse?

The inconclusive finding that natural resource abundance might actually decrease the likelihood of civil war (whether or not depending on institutional quality), contrasts with many other conclusions the conflict literature, although it is not unprecedented (Brunnschweiler and Bulte 2009). How could it be explained?

As mentioned, a number of studies has found that natural resource abundance might have a positive effect on economic growth, possibly depending on institutional quality (Brunschweiler 2008, Mehlum, Moene and Torvik 2006). The most likely interpretation thus seems to me that natural resource abundance has some positive effect on the economy. Since nearly all studies control for level and/or growth of GDP, this positive effect must be explained by some other variable. Perhaps natural resource abundance increases government revenue, which might also lessen the need to tax other productive activities. Alternatively, it might provide employment, increase the amount or quality of infrastructure or attract foreign investors improving a countries image abroad. A more cynical explanation for the negative relationship between natural resource abundance and civil war risk may be that the proceeds from natural resources strengthen government military power, making rebellion less feasible. However, as this result is not particularly robust, all these possible mechanisms remain speculative.

6 Conclusion

"Since very few of the papers in the literature on civil war deal with the issue of endogeneity, we also ignore it…" (Hegre and Sambanis 2006). In this study, I have shown that we cannot afford to fail to address issues of endogeneity in this way. I have also shown that the existence of a causal relationship between natural resource abundance and civil war becomes doubtful if one does address it. Natural resources to me seem wrongfully accused of single-handedly causing the onset of civil conflict.

In this paper, I have described how many quantitative studies find a relationship between all or some natural resources and conflict onset. I have singled out the specifications that find the most significant relation (Collier and Hoeffler 2004, Collier, Hoeffler and Rohner 2009), because they pose the toughest test if one wants to prove that there is none. The measure of natural resource abundance used in these studies, can arguably be considered endogenous. Any relationship between natural resource abundance and civil war observed could be due to reversed causality and/or omitted variable bias, most omitted variables being weak economic or political institutions.

In order to test this, I proposed two more exogenous measures of natural resource abundance: natural capital and subsoil capital. I then replicated the selected analyses while replacing the original measure of natural resource abundance with the more exogenous ones. These measures could not be shown to be positively related to the chance of civil war onset, nor to any other aspect of civil war. This result is robust in two major competing specifications. In some cases, natural resource abundance even seemed to decrease the chances of conflict onset. Lastly, some inconclusive evidence was found that in countries with low quality institutions, resource abundance might increase conflict risk.

These results have implications for scientific research and policy. Natural resources might falsely be regarded as a 'curse' of some kind, or rebels unjustly classified as 'greedy'. The effect of institutional factors on civil war might be underplayed. Some of the proposed policy interventions might not be as effective as thought in first instance. And finally, endogeneity problems are not to be ignored, but should be addressed.

From this paper, two points can then be taken. First, continuing the search for exogenous measures of natural resources is necessary and potentially fruitful. Secondly, scientific researchers and policy makers alike should consider the possibility that natural resource abundance is not a cause of civil conflict onset and that they thus have been chasing the wrong suspect.

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