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Working Paper 08/78
January 2008

Economics Working Paper Series

Eidgenössische Technische Hochschule Zürich
Swiss Federal Institute of Technology Zurich

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January 2008

Abstract: In this paper we examine the claim that natural resources invite civil conflict, and challenge the main stylized facts in this literature. We find that the nature of causation between resource dependence and civil war is opposite to conventional wisdom. In particular, (i) civil war creates dependence on primary sector exports, but the reverse is not true, and (ii) resource abundance is associated with a reduced probability of the onset of war. These results are robust to a range of specifications and, considering the conflict channel, we conclude there is no reason to regard resources as a general curse to development.

Keywords: Civil war, resource abundance, resource dependence, greed versus grievance, resource curse.

JEL codes: Q34, O11, N40, N50

1. Introduction

The appreciation for natural resources as a driver of economic development has undergone a dramatic change in the past decades. While economists generally perceived an abundance of resources as advantageous until the 1980s, an influential empirical and theoretical literature emerged in the 1990s that reached rather opposite conclusions. The phrase "natural resource curse" was coined and, perhaps because of its paradoxical connotation, caught on in both academic and policy circles. The current literature distinguishes between no less than three different 'dimensions' of the resource curse: resources are associated with (i) slower economic growth, (ii) violent civil conflict, and (iii) undemocratic regime types.[1] Arguably, these different manifestations of the curse can be inter-related.

In this paper we focus on the nature of the relation between resources and civil war. Collier and Hoeffler (1998) offered a pioneering empirical contribution based on cross-section analysis, where among other things they found that resource dependence had a significant curvilinear effect on both the onset and duration of war. As a measure of resources they used the ratio of primary exports to GDP, a measure also popularized by Sachs and Warner (1997) in the parallel literature focusing on the relation between resources and economic growth. In a follow-up series of papers, Collier and Hoeffler demonstrated (i) that resources have an impact on some types of wars, but not on others (Collier and Hoeffler 2002, see also Reynal-Querol 2002); (ii) that resources are also significantly correlated with the onset of war in a panel-data setting (Collier and Hoeffler 2004); and finally (iii) that the main results are robust to employing alternatives measures of resource wealth (notably a measure of resource rents, see Collier and Hoeffler 2005). In addition, they consistently

[1] We cannot possibly do justice to the many papers in these three fields, but selected contributions include the following works. On economic growth, refer to Sachs and Warner (1997), Mehlum et al. (2005), but also Brunnschweiler and Bulte (2008) and the citations therein. On conflict, refer to Collier and Hoeffler (1998, 2004), Collier et al. (2007), Ross (2004), Lujala (2005), and to the special issue of the Journal of Conflict Resolution devoted to the topic. On regime type (and institutions more broadly), refer to Karl (1997), Ross (2001), Leite and Weidmann (2002), Jensen and Wantchekon (2004), and Bulte et al. (2005). Overview articles include Rosser (2006), and van der Ploeg (2008).

demonstrate the important role of low income and slow growth as drivers of conflict (see also Miguel et al. 2004).

The Collier and Hoeffler series of papers has proved influential and controversial, not least because of its focus on the *economic* roots of conflict. By now, the standard explanations of civil war as advanced by economists and political scientists are 'greed' and 'grievance.'[2] The rational choice paradigm considers civil war a special form of non-cooperative behavior, and the greed motive simply reflects opportunities for rebels (or rebel leaders) to enrich themselves, possibly by seizing resource rents. Grievance, in contrast, is rooted in a behavioral paradigm, and emphasizes relative deprivation, social exclusion and inequality (e.g. due to ethnic or religious divides, see for example Gurr 1970, and more recently, Regan 2003). In the context of resource-rich societies, grievance might be exacerbated by insufficiently compensated land expropriation, environmental degradation, inadequate job opportunities, and labor migration (e.g., Rosser 2006). Relevant for both the greed and grievance motive is that resource rents provide a potential source of funding for the start-up costs associated with initiating a rebel organization. The findings by Collier and Hoeffler support the greed perspective. Rebels, then, may be viewed as rational predators or, using terms with a less negative connotation, as entrepreneurs following up on a profitable opportunity. The theoretical underpinnings of this perspective may be traced back to Grossman (1991) and Hirschleifer (1995).

A small "cottage industry" has now emerged in economics and political science on the purported association between natural resources and civil war (Ross 2004a, Ron 2005), and the resource-war link is increasingly viewed as a stylized fact. However, this link, and in

[2] Ballantine (2003) has noted that the mix of greed and grievance can be particularly potent, and relevant as an explanation of the onset of war (see also Murshed and Tadjoeddin 2007). For an interesting discussion of the motives of rebels, and the interaction between opportunistic and ideological leaders (as well as its consequences for the 'type of war' that evolves), refer to Weinstein (2005). Ross (2004b) examines the greed and grievance motives, along with other possible conflict triggers, in a series of case studies. Other reasons why resources might be linked to conflict have to do with the probability of foreign intervention (Rosser 2006) and the probability of suffering from economic shocks (e.g. Collier and Hoeffler 2005).

particular the economic terms within which it has been couched, have not gone unchallenged (especially in the political science domain of the literature). The first series of challenges stems from doubts about the data and statistical analysis. Some authors point out that the relation between resources and conflict only exists for a subset of resources, but there is no agreement about which resources may be implicated. If resources are important for financing an insurgence, then arguably the "lootability" of resources is important. If so, many resources included in the standard dependence variable are irrelevant. Worse; many relevant resource revenues – such as those obtained by smuggling – are *not* included in the data. In contrast, if resource rents are a motive for rebellion (the prize that may be grabbed after seizing power), then a much wider range of rent-generating resources might be relevant.

The empirical evidence is mixed (e.g., Ross 2004a, 2006). Elbadawi and Sambanis (2002) question the robustness of the resource-war link. Ross (2003) finds little support for a link in general, but does argue in favour of a relation between conflict and 'lootable' resources such as alluvial diamonds and drugs (see also Olsson 2006, Snyder and Bhavnani 2005). Lujala et al. (2005) demonstrate that there is no relation between diamonds and conflict onset, but that lootable (alluvial) and non-lootable (i.e underground) diamonds have opposite effects on the incidence of conflict. De Soysa (2002), Lujala (2005), Fearon (2005), Ross (2006), and De Soysa and Neumayer (2007) emphasize the role of (legal) oil and mineral resource trading in explaining conflict, but this is disputed by Smith (2004) who focuses on regime instability. Finally, Humphreys (2005) suggests that it is dependence on agricultural production that matters. If the latter finding is robust, it would imply that social relations co-shaped by economic structure is a driver of conflict, not the mere presence of resource rents that can be grabbed. However, the agricultural angle is downplayed by Ross (2004a).

A second challenge concerns the debate about the economic perspective on (potential) rebels as the key decision maker.[3] Many analysts favour explanations based on politics over explanations based on economics (e.g., see Auty 2004, Humphreys 2005, Snyder and Bhavnani 2005, Dunning 2005, for arguments based on state strength). According to this view, resource-rich economies have a greater propensity to suffer from weak and unaccountable leadership, and are therefore unable or unwilling to diversify the economy and deliver public goods associated with economic prosperity to "buy the peace". Alternatively, resource riches may invite (military) dictatorships and generally oppressive regimes, resulting in genuine grievances among a share of the population (Fearon and Laitin 2003, Ron 2005). It is not always easy to distinguish between the various mechanisms linking resources to war (but see Humphreys 2005 and Ross 2006). For example, while the level of income may serve as a proxy for the opportunity cost of rebel activity, entering in an economic analysis, it is equally plausible to argue that it proxies for the "effectiveness of the state" in delivering public goods (e.g. Fearon and Laitin 2003). Regardless of the exact mechanism, the politics view builds a natural bridge between the different dimensions of the resource curse mentioned above – regime type, growth and conflict – and implies a re-appraisal of the earlier political science literature on the resource curse (e.g. Karl 1997 on the rentier effect).[4]

Is this literature moving towards a consensus? If so, the pace of convergence is very slow. While Collier and Hoeffler (2005) acknowledge the importance of governance and political structures, and discuss the "looting versus provision of public goods" dilemma faced

[3] Recent literature also contains contributions supporting the rational-rebel or greed explanation. For example, Lujala (2005) finds that the exact location of resource endowments (i.e. in the war zone, or not) matters for conflict duration, and that the onset of conflict is affected by onshore but not by offshore oil extraction. These results suggest that the effect of resources on rebels' opportunity and incentives may be more important than the effect on state revenues and capacity.

[4] One issue that should be addressed in this literature is collective action dilemma. If justice is a public good, then fighting to restore it will suffer from free rider problems. To better understand this, arguably it makes sense to explicitly introduce group formation and a sense of identity into the analysis. Also, see Herbst (2000) for a treatment of incentives and rebel motivations.

by incumbent political leaders in resource-rich societies, in their most recent work they again emphasize rebel decision-making (Collier et al. 2007). Employing the most recent data, they argue in favour of military and financial 'feasibility' as key concepts. Conflict will emerge whenever it is feasible (i.e. whenever profitable opportunities for violence exist, the rebel niche will be filled), and the exact motivation is of less importance. But, as argued by Murshed and Tadjoeddin (2007: 14), this almost amounts to rephrasing the initial greed argument – "the basic arguments and empirical evidence are much the same as before." Also, the relation between the nature of conflict (if any) and different types of resources is far from understood. Finally, complicating matters further, there is a literature that implicates resource *scarcity*, rather than abundance, as a driver of violent conflict (e.g. Homer-Dixon 1999). Scarcity is linked to conflict via two mechanisms: it may trigger marginalization of powerless groups by an elite scrambling for resources, and it could have a debilitating effect on processes of social and economic innovation (resulting in an 'ingenuity gap'). In the words of Hirschleifer (1995: 44): "As Malthusian pressures depress per capita incomes, it comes to a choice between fighting and starving." For a formal model analyzing this issue in theory, refer to Grossman and Mendoza (2003).

We return to the scarcity-abundance dichotomy later when discussing our own results. For now we simply note that in light of these outstanding controversies, and the obvious relevance of the research subject for the lives and prospects of millions of people, it is easy to predict that the resource-war link will remain a lively field of enquiry for economists and political scientists in the future.

The contribution of this paper is twofold. First, we explore the nature of the causal link between resources and the onset of war. [5] As mentioned above, most analyses are based on the ratio of primary exports to GDP. While the literature refers to this variable as both a

[5] We do not consider the relation between resources and conflict duration or intensity. For more work on these issues, refer to Doyle and Sambanis (2000), Ballantine (2003), Ross (2003, 2004b), and Fearon (2004).

measure of resource dependence and abundance, it is evident that it is at best an imperfect proxy for the latter. Resource-rich countries that have also developed other industries may not be dependent on primary exports. Similarly, resource-poor countries might nonetheless depend on primary production if they host only few alternative economic activities. Our key concern is that the common resource variable may be *endogenous* with respect to conflict. Similar observations have been made by De Soysa (2002), Dunning (2005), Lujala (2005), Lujala et al. (2005), Ross (2006), and Murshed and Tadjoeddin (2007), but heretofore this issue has been circumvented by using alternative measures of resource wealth in empirical analyses such as production or reserve data (which present their own endogeneity issues), without however exploring the implications of a positive relationship between resource dependence and conflict. A similar concern applies to analyses based on scaled resource rents (rents divided by national income), as in Collier and Hoeffler (2005) and De Soysa and Neumayer (2007). If investments in sectors other than the resource-agricultural complex are depressed by either past, present or (the shadow of) future conflict, then the nature of the resource-war link could run from conflict to resource dependence, rather than the other way around. Investment in manufacturing requires a stable politico-economic environment, and one may hypothesize that the resource sector is less sensitive to mounting tensions or outright conflict than other sectors.[6] Reasons for this may include limited linkages with the rest of the economy, the sector's orientation towards international markets, and its inability to relocate. Moreover, as argued by Ross (2004a: 338): "using lagged independent variables does not eliminate the danger of reverse causality: civil wars can be preceded by years of low-level violence that drives off manufacturing firms, producing a high level of resource dependence before the violence actually begins." In this paper we explore the endogeneity issue directly by instrumenting for resource dependence in a two-stage procedure.

[6] Indeed, Lujala (2005) even mentions that (the shadow of future) conflict might increase resource extraction (and hence: dependence) as it gives incumbent leaders an incentive to increase extraction to finance investments in the military, or for personal gain.

Second, we add a novel proxy for resource abundance – a stock variable capturing the discounted value of the future flow of resource rents – in our empirical analysis, and explore the relation between resource wealth and the onset of conflict anew. Our variable naturally captures much of the interpretations associated with the alleged resource-war link. For example, to the extent that conflicts are triggered by greed (fighting over future resource rents), our variable better captures the essence of what is at stake than measures of current dependence.

Our main findings turn received wisdom upside down. We find that resource dependence is indeed an endogenous variable in conflict regressions, and that properly accounting for this endogeneity removes the statistical association between dependence and conflict. In a follow-up regression we demonstrate, not surprisingly, that a country's history with respect to war and peace is a significant determinant of resource dependence – clenching our main result. Moreover, we find a significant *negative* relationship between resource abundance and the onset of war, possibly because of an income effect, suggesting that the label "resource curse" seems misplaced. Resource-rich countries have on average a *lower* propensity to enter a civil war, but countries that do end up with civil strife (possibly resource-poor ones) will experience increasing dependence on the primary sector.

2. Data and Empirical Strategy

We now outline our empirical procedure and present the most important data. Following up on the empirical strategy by Brunnschweiler and Bulte (2008), we first explore the determinants of resource dependence, extending the analysis of Collier and Hoeffler (2004) to a panel dataset of nine 5-year periods between 1960-2004, similar to Collier et al. (2007).[7] We then proceed by analyzing the impacts of resource dependence as well as abundance on

[7] Fearon (2005) demonstrates that the results of Collier and Hoeffler are not robust to using annual panel data. However, it is not evident that annual data better capture the potentially "slow dynamics" that may trigger the onset of war.

the propensity of conflict to start. Our main resource abundance stock variable may directly affect the probability of conflict (through rebellion motives), but the influence may also be indirect through the level of resource dependence or income. We explicitly distinguish between such direct and indirect linkages.

We run three different regression equations – a resource dependence equation, an income equation, and a conflict regression equation. The former two regressions are important to assess whether resource dependence *RD* (denoted by *sxp,* calculated as primary exports divided by GDP) is a proper exogenous variable in conflict regressions, as implicitly assumed in empirical work until now. Specifically, we try to unravel the determinants of resource dependence as follows:

(1) $RD(ti) = a_0 + a_1 * conditioning\ variables(ti) + a_2 * RA(ti) + e.$

RA measures resource abundance in period *t* in country *i*, and is included to account for the notion that resource-rich economies may have a comparative advantage in exporting primary products. We use estimates of the net present value of rents (in USD per capita) of a country's total natural capital stock, taking the natural logarithm of the value in order to reduce the influence of outliers (*lnnatcap*). The aggregate measure includes subsoil assets (fuel and non-fuel minerals), cropland, pastureland, timber and non-timber forest resources, and protected natural areas. We also consider two disaggregate measures, focusing on mineral resources and land (crop- and pastureland, protected areas, and forest resources) separately. All these *RA* estimates are taken from extensive studies by the World Bank (1997, 2006). Drawbacks of the variables are their limited country and time coverage: they are available for 98 countries, and only two years (1994 and 2000).[8] It can be argued that resource abundance is a relatively persistent variable. Moreover, we are confronted with a more practical problem, as the World Bank did not start collecting the relevant data before

[8] We chose to use the 1994 values for the periods until 1999, and the 2000 values for the last period from 2000-2004. All our results are robust to using the earlier data throughout the sample period.

the 1990s. However, in order to test the robustness of our results we also use alternative measures of resource abundance, namely per capita estimates of the value of total reserves of fuel and non-fuel minerals (including industrial diamonds) in 1970 from Norman (2007), and the per capita production and reserves of oil from 1960-1999 from Humphreys (2005). Both datasets cover a wider range of countries, bringing the sample coverage up to 115 and 118 countries, respectively, albeit at the cost of omitting all non-mineral natural resources from the analysis.

How "exogenous" are our resource wealth measures? We argue that the data on natural resource wealth are likely to be relatively independent of local issues (including conflict intensity), and therefore exogenous for our purpose. The (fuel and non-fuel) mineral deposits have been well explored and estimated due to the potentially large profits they represent, and also thanks to the involvement of large multinational firms who operate with little regard for local political or technological conditions.[9] Of course, our resource abundance data are not perfect, as the present value of rents is not completely invariant with respect to policies, and exploration and exploitation efforts may to some extent be determined by the level of development. But we believe they are less prone to the policy endogeneity which plagues export-based and rent-based dependency measures; and less subject to technology standards which influence production levels (see also Brunnschweiler

[9] Around 90% of known oil and gas stocks are controlled by national companies, but "…because of the enormous capital and technological resources necessary to exploit minerals, foreign oil companies became the dominant internal actors in all oil exporters […] The complexities of the international market, the continued need for foreign investment and technology, and their links to other powerful actors mean that these companies still retain significant power even after nationalization." (Karl 1997: p.55) Moreover, foreign mineral companies have been known to get involved in production even if local political and regulatory conditions were unstable or deteriorated to the point of open conflict. An example is Shell's long-standing involvement in oil production in Nigeria despite violent conflict, and its willingness to enter into arrangements with both warring parties to ensure some level of production continuity (Zalik 2004). Nevertheless, a counterfactual is lacking and we don't know to what extent these companies would have been involved in a different political setting).

and Bulte 2008). In addition, they offer the great advantage of potentially covering all natural resources in the estimation, which to the best of our knowledge is unique to this dataset.[10]

Equation (1) thus captures the fact that resource dependence may be influenced by both the biophysical context (resource abundance), and by the institutional framework and the policy choices it generates (government system and trade openness). As mentioned above, since we don't wish to rule out *a priori* that something similar applies to our income variable, we also run a series of regressions akin to (1) but with income *I* (i.e. the log of GDP per capita at the start of each period, *lngdp*) as the dependent variable.[11]

The main conditioning variables serving as exogenous instruments for *RD* and *lngdp* are average openness to trade over the previous 5-year period (*openness*); a dummy variable for a presidential-type system of government;[12] latitude; percent of land area in the tropics; and distance to the nearest coast or navigable river (*distcr*).

Our main challenge is to examine the impact of resource dependence (*RD*) and abundance (*RA*) on the propensity of conflict to start, both directly and indirectly (for the case of abundance). To this end, we compare the results of pooled instrumental variable (IV) probit and panel data IV regression analyses:

(2) $$warstart(ti) = b_0 + b_1 * conditioning\ variables(ti) + b_2 * RD(ti) + b_3 * I(ti) + b_4 * RA(ti) + v,$$

[10] There is one earlier paper that used our resource abundance data to gauge the relation between resources and the onset of civil war, and that is De Soysa (2002). Our study is different for a number of reasons: (1) De Soysa focuses on the post-cold war era (1989-1999) and does a cross-section analysis, whereas we focus on the 1960-2004 time frame and do a series of panel regressions; (2) De Soysa applies a 25 battle death per year threshold, and we mainly focus on the COW dataset with a cutoff value 1000 battle deaths; and (3) in all regressions De Soysa combines total resources abundance and mineral resource abundance (and sometimes an oil exporter dummy as well) which likely introduces multicollinearity issues. Finally, we explicitly analyze the endogeneity of the resource dependence variable, and also note a disconnect between the main empirical results as displayed in De Soysa's Table 1 (with negative coefficients for log(mineral resources 1990)) and the accompanying discussion on p.409: "*the evidence supports the position that mineral wealth abundance fuels greed-motivated conflict*…"

[11] Collier et al. (2007) take the endogeneity of income into account and find that its coefficient more than doubles in size after instrumenting, but decide to ignore it in their core specification as it does not qualitatively affect their other main variables of interest..

[12] This argument is based on seminal contributions by Persson and Tabellini (2003, 2004). See Andersen and Aslaksen (2006) and Brunnschweiler and Bulte (2008) for a more in-depth discussion of how constitutional rules may influence economic outcomes, and the relative economic importance of the primary sector in particular.

where *warstart* is a dummy that takes on zero value if conflict did not occur during the period and a value of one otherwise. In our main estimations, we follow Collier and Hoeffler and treat ongoing conflicts as missing observations so as not to confuse the onset of war with its duration, adding the number of peaceful years as a control variable. We base our definition of civil war on the Correlates of War (COW) database by Gleditsch (2004), which considers all organized military conflicts with at least 1000 battle-related deaths, of which at least five percent must be inflicted by the weaker party. Furthermore, at least one party in the conflict must be the national government. This definition gives us a potential sample of over 160 countries and up to 93 conflict episodes between 1960-2004. In the robustness tests, we also try coding ongoing conflicts as zero and introduce a dummy variable for conflict in the previous period, following the methodology of Fearon and Laitin (2003). Furthermore, we consider an alternative definition of conflict based on the Armed Conflict Database (ACD) compiled by Gleditsch et al. (2002). The ACD classifies conflicts according to two main dimensions, the first being location and participants: 1. extra-systemic conflicts, 2. interstate wars, 3. intrastate wars, and 4. internationalized intrastate wars, of which we include all but interstate wars. The second dimension is the level of violence: both minor conflicts (more than 25 battle-related deaths per year, at least 1000 deaths over the entire conflict) and major conflicts (more than 1000 battle-related deaths per year, analogous to the COW classification) are included, bringing the potential coverage in our sample to 149 conflict episodes. Further details on all variables and sources are given in the Appendix.

Equation (2) allows us to distinguish between different interpretations of the resource-war link. If resource dependence (or abundance) indeed excerbates the risk of conflict, then coefficients b_2 (or b_3) should take on a significant and positive value. To gauge the total effect of resource abundance on conflict, one needs to account for both the direct effect (as measured by b_3) and the indirect effect via dependence or income (as obtained by combining

a_2 and b_2). In contrast, for negative values of b_2 and b_3, or for sufficiently large offsetting indirect effects, the conventional interpretation of the resource-war link needs to be revised.

The conditioning variables include a wide set of explanatory variables used in the literature: the annual GDP per capita growth over the previous period (*growth*); the years of peace since the last conflict (or WWII) as mentioned above; the social fractionalization measure (*socialfrac*) from Collier and Hoeffler (2004); the (log of) population at the beginning of the period (*lnpop*); the percent of mountainous terrain (*mountain*) from Fearon and Laitin (2003); and a dummy variable for former French colonies in sub-Saharan Africa during 1960-1999 (*frafcol*, see Collier et al. 2007). We also add a political regime measure from the Polity IV project (*polity*), ranging from -10 (strong autocracy) to 10 (strong democracy), as well as the proportion of young men aged 15-29 in the total population (*youngmenpop*, taken from various issues of the UN Demographic yearbook), to reflect the potential recruitment base for rebel movements (see Collier et al. 2007). *RD* is estimated using (1).

<< Insert Table 1 about here >>

Table 1 shows the descriptive statistics of the main variables. Per capita GDP and GDP growth are both considerably lower at the start of a conflict episode, in accordance with one of the standard economic explanations for violent conflict. Resource dependence and abundance are also lower during conflict episodes, which is counterintuitive given the substantial evidence in previous research showing that natural resources increase the risk of conflict. However, the finding is consistent with the observation in Collier et al. (2007).

3. Regression Results and Robustness Analysis

We now present our empirical results. In the first two columns of Table 2 (Panel A) we derive results that are very similar to those of Collier et al. (2007), Fearon (2005) and others. Specifically, we obtain a concave relationship between resource dependence and the onset of conflict. However, we find that the logarithmic specification highlights this correlation most clearly and robustly, and in what follows we use the log of the share of primary exports in income as our key resource dependence variable. The coefficients for the other variables are also in line with previous research. We find a negative correlation between the onset of war on the one hand, and (logged) income, income growth, the duration of peace, and the French colony dummy on the other hand. In contrast, there is a positive correlation between (logged) population size and the propensity of war. The percentage of young men, the variable measuring mountainous area, and social fractionalisation appear to have no correlation with civil war, which is also consistent with many earlier studies.[13] We don't find any effect of the political system on the onset of civil war (*polity*), although the coefficient has the positive and somewhat counterintuitive sign frequently observed in earlier studies. Possibly this variable picks up conflicting factors: autocratic regimes may be better able to repress potential insurgents, and democratic regimes may be better at buying the peace through provision of public goods and catering for the median voter (see Azam 2001 for a treatment). However, weakly democractic regimes (i.e. with low positive values on the polity scale) may also be more subject to potentially destabilizing political changes.

In column 3 we introduce our aggregate resource *abundance* variable. It enters with a significant and negative coefficient – countries with more abundant natural capital appear to have a *lower* probability of becoming engaged civil war. This result also holds when we run a logit estimation adjusted for the "rare event" nature of civil war, as shown in Table 4,

[13] For example, young men and mountains are included as proxies for "feasibility" in Collier et al. (2007), but in that study also do not appear to be robustly related to the onset of conflict.

column (1). If correct and robust, this result turns the "resource curse" interpretation upside down. Upon inspecting the effects of introducing resource abundance on the other variables, two effects stand out. First, the resource dependence variable (*lnsxp*) is no longer significant when controlling for abundance. Second, both the significance level and coefficient value of the income variable are attenuated. Specifically, income is now only significant at the 10% level (and not at the 1% level as before), and the value of its coefficient drops from -0.39 to -0.25. This could be indicative of collinearity between income and resource abundance. Moreover, in light of our earlier arguments, there is reason to suspect that resource dependence may be endogenous, and similarly, income may also be endogenously determined. For this reason we proceed by simultaneously instrumenting for resource dependence and income in a series of follow-up regressions.

In columns (4)-(6) we instrument for income and resource dependence using a series of instruments. We apply a pooled two-step ivprobit model, where the two endogenous variables are estimated in the first stage by a simple linear regression, while the second stage uses a probit approach to determine the probability of the onset of war. The exogenous instruments for dependence and income include *openness*, *presidential*, *latitude*, *distcr*, and *tropics*. Note that we now also include resource abundance as an exogenous instrument, as the second-stage results and test statistics clearly indicate that abundance has no significant direct effects on the onset of conflict after instrumenting for dependence and income (implying the effects of abundance are indirect).[14] First stage regression results for these six exogenous instruments, matching the regressions in columns (4)-(6) in Panel A, are provided in Panel B of Table 2. Results for resource dependence are reported in columns (1)-(3), and for income in columns (4)-(6) of Panel B. The three IV probit specifications that are shown

[14] If included in the second-stage specification (i.e. not as exogenous instruments in the first stage only), the total natural capital and land variables both enter with a negative sign with p-values of 0.762 and 0.578, respectively. Subsoil has a positive sign and a p-value of 0.480. Identification tests for all resource abundance measures show that they are proper exogenous instruments.

correspond to three different resource abundance variables in the first stage. In specification (4) (columns (1) and (4) of Panel B) we use the aggregate resource abundance variable, encompassing both subsoil wealth and land-based resources. In contrast, specification (5) (columns (2) and (5)) is based on subsoil wealth only (fuel and non-fuel mineral resources), and specification (6) (columns (3) and (6)) is based on land (sum of forest land, crop- and pasture land, and protected areas).

The first stage results are consistent with intuition and show that our instruments are strong.[15] Resource dependence is a positive function of all three measures of resource abundance (consistent with the comparative advantage argument), openness to trade, a presidential dummy, and tropical area. Next, income is a positive function of resource abundance, openness and latitude, and negatively affected by presidential systems, distance to navigable rivers and the coast, and tropical location. While the positive correlation between resource abundance and income might appear counterintuitive in light of the resource curse literature relating resources to slow growth, it is fully consistent with results reported in Brunnschweiler and Bulte (2008) and is arguably in line with a positive income effect due to resource wealth. Finally, we note that resource abundance does not enter the second stage significantly in the IV estimations but is a strong exogenous instrument in the first stage (confirmed by the test results), suggesting that the effects of resource wealth on the risk of conflict are in fact indirect.

Returning to Panel A, the test statistics at the bottom provide clear support for the idea that income and resource dependence are jointly endogenous, and that instrumenting for these variables is necessary to obtain unbiased estimates of the causal relationships running from dependence and income to the onset of conflict. The most important results are

[15] The over-identification test results for specification (6) are borderline significant. However, upon removing the presidentialism variable, the statistic jumps up to 0.53 without qualitatively affecting any of the other coefficients or standard errors. For consistency we have chosen to present the results using all six exogenous instruments in every specification.

threefold. First, the peace variable is no longer significant. Instead, first-stage results presented in Panel B show that peace is strongly correlated with both lower resource dependence and higher per-capita incomes, lending some support to the argument advanced by other researchers that dependence may be endogenous to conflict, since the primary sector is more location-specific than the manufacturing industry and less likely to flee the country in case of (threatened) war (see e.g. Ross 2004).

Second, higher incomes again attenuate the risk of conflict (see also Miguel et al., 2004, Collier et al. 2007). Indeed, its coefficient has nearly tripled with respect to before and is consistently highly significant. Third, there is no evidence of a negative causal relation running from dependence to conflict. After we instrument for dependence, the effect vanishes, which suggests that the correlation between these variables was in part driven by the impact of conflict on the composition of exports and income – shifting in favour of primary commodities. Columns (4)-(6) indicate that this is true for our aggregate abundance measure, for subsoil resources, and also for land.[16]

This brings us to our main result. Resource-rich countries are not generally "cursed" in the sense that they run a greater risk of being torn by civil wars. Indeed, the opposite seems to be true: resource wealth has a positive and significant effect on income, and this in turn reduces the risk of war. Our results therefore support the hypothesis of Homer-Dixon (1999) that resource scarcity may be a trigger of conflict. While some analysts have argued that scarcity might be especially relevant in the context of land-based resources (it has been viewed a key driver of conflicts in several countries, including Rwanda and Burundi – e.g., see Andre and Platteau 1998), our findings suggests that the mechanism may be more general, and relevant for a broad range of resources. As far as the magnitude of this effect is concerned, we find that increasing resource abundance by one standard deviation reduces the

[16] We have also experimented with using an alternative resource dependence variable. Specifically, we have used log(mineral exports) capturing fuels, ores and metals, and find the exact same results: (instrumented) dependence is not significant, and there is a positive indirect effect of abundance via income.

risk of war from 7.1% to 6.7%, or a 4.5% reduction in risk (evaluating variables at the sample mean). Finally, there is no evidence to believe that resource dependence causes civil wars. Instead, our findings support an opposite causal relation, running from conflict to resource dependence.

Robustness analysis

To what extent are these results robust to alternative model specifications? In Table 3 we provide the second-stage results of a series of representative examples (first-stage results for income and dependence are omitted to save space, but proved very similar to those shown in Table 2, Panel B). While the significance of specific control variables varies from one specification to another (e.g., fractionalization, polity, duration of peace and the French colony dummy), it is clear that our main results are very robust.

In column (1) we first explore the consequences of introducing additional control variables commonly used in conflict regressions (e.g. Fearon and Laitin 2003, Ross 2006). Including measures of political instability, a dummy for newly independent states or for countries with non-contiguous territories, and income inequality does not affect any of our results: resource dependence is still not significantly different from zero, and abundance lowers the risk of the onset of conflict via an income effect. The same applies when we add period or decade dummies (the latter is shown in column (2)): both variations confirmed the common finding that the risk of conflict has increased over time, spiking during the 1970s and early 1980s, and again over the course of the 1990s – although the end of the Cold War itself (captured by a dummy variable for periods after 1990 in alternative specifications) proved insignificant. In column (3), we introduce regional dummies for Latin America and Sub-Saharan Africa; again, the main coefficients of interest remain robust to these controls.

In columns (4)-(6) we use alternative abundance variables to the ones provided by the World Bank, and demonstrate that our results are no artefact of our abundance measure. The alternative datasets have somewhat wider coverage than the World Bank dataset (data limitation arguably being its greatest shortcoming), increasing the number of observations as well as of the included conflict episodes. However, they concentrate solely on fuel and non-fuel minerals, ignoring all other types of natural resources. In column (4) we use a new mineral reserves dataset constructed by Norman (2007). First-stage results (not shown) again indicate a positive and significant relation between abundance and income, and the second-stage results confirm that income negatively affects conflict risk – the same indirect channel as identified and discussed above. Moreover, using this new abundance variable to instrument for resource dependence again shows that *lnsxp* is no longer significant. In columns (5) and (6) we use, respectively, per capita oil production and reserve data from Humphreys (2005). While we are not confident that the production data are fully exogenous, it is comforting to observe that our main first and second stage results are confirmed.

<< Insert Table 3 about here >>

Results from further robustness tests are reported in Table 4. Column (2) reports results from a panel regression while instrumenting for income and resource dependence, using the same data as before. In contrast to earlier IV probit analyses, we have now estimated a linear equation. While ideally we would do a panel probit regression and instrument for income and dependence, the routine is not available for such a regression. Regardless, we find the main results are robust with respect to the nature of the estimation method. Again, resource abundance has an attenuating indirect effect on the risk of war, via income, and resource dependence has no statistical effect at all.

In column (3) we adjust our approach of tackling ongoing conflict, coding it as "0" instead of missing, and adding a dummy variable for conflict in the previous period instead of peace years to avoid confusing war onset and duration. While including a dummy for ongoing conflict does not affect any of our main results from the first and second stages, it does compromise significance levels of some of our control variables (population size, fractionalization, mountainous territory), although there is little change in the magnitude of the coefficients. In the first stage, violent conflict in the previous period still increases resource dependence (the coefficient of the dummy variable is significant at the 7% level), while it does not significantly affect income levels.

Columns (4) and (5) represent a radical departure from our earlier regressions, as we now switch to another dependent variable – the conflict dummy from the Armed Conflict Database (ACD). The obvious difference with our earlier "onset of war" variable, taken from the Correlates of War (COW) dataset, is that the ACD variable captures more violent incidents. Specifically, the ACD dataset takes 25 battle-deaths per year as the threshold value above which an incident is counted, and the COW variable only includes violence with an annual death toll exceeding 1000. Arguably, violent incidents with a number of casualties close to the 25 threshold level do not represent war as commonly understood (Collier et al. 2007), which is probably why many analyses on the resources-war link are based on the higher 1000 battle-deaths data (e.g., Collier and Hoeffler, Fearon and Laitin 2003; see Ross 2004 for a survey). Nevertheless, it is interesting to see if small-scale violence responds to the same triggers as civil war.

Columns (4) and (5) suggest it does not. In column (4) we report results using the two-step ivprobit routine as above with ongoing conflicts coded as missing, and in column (5) we again code it as "0" and introduce a dummy variable to capture previous conflict. Regardless of the specification, we find the results using the ACD data are different from the ones for the COW dataset. First-stage results still suggest that previous episodes of conflict significantly increase resource dependence. Moreover, while it is still the case that resource abundance has an important indirect and attenuating effect on the onset of violence via income, we now also find that resource dependence enters significant and positive.[17] With other words, resources – or possibly the distribution of resource rents – do not seem a reason for full-blown war, where issues related to grievance (e.g. economic underdevelopment) appear to be more important; but they may trigger more small-scale conflicts.[18]

Since resource abundance increases both income and dependence in the first stage, and since income and dependence have opposite effects in the second stage, the net effect of abundance on small scale violence is ambiguous. Upon combining results from the first and second stages, we can assess the net effect of resources on violence. Specifically, a one-standard deviation increase in resource abundance decreases the risk of all levels of conflict via higher incomes by 0.27%, and simultaneously increases the risk via increased resource dependence by 0.20%. The net effect of resource abundance, therefore, continues to be a reduction in the probability of conflict, although the limited magnitude in fact suggests that resources are not a major determinant of conflict in any event. A robust finding of this study is that resource abundance attenuates the risk of conflict, and labelling resources a curse for development appears misleading in this light.

4. Discussion and Conclusions

According to both the economics and political science literature, natural resources tend to magnify the risk of civil war. While there are debates about the types of resources that are most prone to enhance the propensity of conflict, and about the nature of the mechanism

[17] We have repeated this analysis using subsoil and land-based resource abundance variables, and the qualitative results are identical to the ones for the aggregate abundance variable on which the first stage results of Table 4 are based. Note that the presidential dummy variable, tropical location and distance to the coast or navigable river proved weak instruments in the specifications using the ACD dataset and were therefore dropped, leaving three strong exogenous instruments.

[18] In his survey article, Ross (2004) shows that the influence of resources on conflict also depends on the conflict level considered, with the 25-death threshold apparently delivering more consistently negative natural resource effects. Fearon and Laitin (2003) and Ross (2006) further break down the types of conflict and find different effects of resources on ethnic and non-ethnic, and regional separatist and nationwide civil wars.

linking resources to conflict, the resource-war link is often viewed as a stylized fact. Indeed, it is commonly treated as one of the dimensions of the paradoxical resource curse perspective – the view that more of a good thing may be bad for development. Resource wealth is nowadays associated with slow growth, bad governance and greater risk of conflict (see Rosser 2006 for an overview of all dimensions).

In this paper we qualify this interpretation, and indeed turn received wisdom upside down. We find evidence of a link between resource wealth and the onset of conflict, but demonstrate it runs opposite to the usual perspective. Resource wealth, via an income effect, *lowers* the probability of conflict, and especially of the onset of a major conflict. Moreover, we find no evidence of an across-the-board link running from resource dependence to civil war. Instead, the opposite is true – conflict-torn societies become dependent on natural resources, which arguably is hardly a paradox. These findings corroborate earlier results by Brunnschweiler and Bulte (2008) in the domain of resources and slow growth, who also distinguish between abundance and dependence. Their main results in the domain of growth echo the ones about conflict above. Our findings are also consistent with Homer-Dixon's view that resource scarcity – rather than abundance – may drive conflict. Interestingly, it appears as if the mechanism is not only relevant for land-based resources – triggering a Malthusian-type of scramble for subsistence resources – but also for subsoil resource wealth.[19]

Our extensive array of alternative specifications supports the idea that the various resource-war linkages as identified in this paper are robust, particularly when we consider major conflicts with a threshold of at least 1000 battle-related deaths per year. When we include smaller conflicts (starting at 25 battle-related deaths per year and 1000 deaths

[19] Our cross-national analysis questions whether there is a general tendency for resource-rich countries to be more conflict-prone, but we do not deny that natural resources played an important role within certain countries.

overall), resource abundance still has a net conflict-reducing effect, although it does increase the probability of conflict via the resource dependence channel.

While we believe the distinction between resource dependence (a flow variable) and abundance (a stock variable) is valuable, and that the proper treatment of endogeneity and reverse causality is essential, we do not view this as the definitive treatment on resources and conflict. Our preferred abundance variable is only available for up to 98 countries, and while it presents a nice combination of industrial and developing countries, we cannot rule out that the dataset suffers from selection bias, as the limited country coverage eliminates several conflict episodes from our main estimations. Moreover, while we believe our abundance variable to be largely exogenous to conflict, by its nature it displays very little variation over time, and the dataset moreover offers only two observations so far. This introduces restrictions on the type of econometric techniques that may be used. We view this as an inevitable consequence of analyzing resource abundance and tackling the endogeneity problem that compromises earlier work on time-variant flow variables. Nevertheless, we appreciate that our treatment of abundance in the core regressions represents an extreme perspective (indeed: the opposite extreme of the annual export, production or rent variables featuring in many panel studies). Importantly however, our main results appear robust with respect to time-varying oil production and reserve data, which also cover a wider range of countries and (conflict) episodes.

Two final remarks are in order. First, following most of the earlier work based on resource dependence, we have excluded some of the most contested resources. In particular, secondary (‘lootable’) diamonds are often implicated as a driver of conflict, but do not feature in our abundance variables. This is mainly for practical reasons – diamonds are also not included in the commonly used primary exports ratio variable, and therefore it appeared inappropriate to include diamond reserves as an instrument. Nevertheless, it is conceivable that (lootable) diamond reserves have a direct effect on the onset or duration of conflict (e.g. Ross 2006), and we don't wish to downplay that possibility. Second, we have been silent on the exact mechanism linking resource abundance and higher incomes to a reduced risk of the onset of civil war. One interpretation is that the income of potential rebels is higher (or that the quality and quantity of public goods provided by the government improves), raising the opportunity cost of rebellion. A competing hypothesis will be that resource rents enable incumbent rulers to more effectively oppress rebellion. Our results therefore do not settle the greed versus grievance view on the resource-war link (even if the nature of the link is reversed). Analyzing this in more detail is left for future work.

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Appendix

Distance to nearest coast or navigable river (distcr) Measured in km, taken from the Geography Dataset by Gallup et al., available from the Harvard Center for International Development at http://www.cid.harvard.edu/ciddata/ciddata.html.
Former French African colony (fafcol) The dummy takes a value of one for the periods 1960-1995 for the following countries: Benin, Burkina Faso, Cameroon, CAF, Chad, Rep. of Congo, Côte d'Ivoire, Djibouti, Gabon, Guinea, Madagascar, Mali, Mauritania, Niger, Senegal, Togo. Based on Collier et al. (2007).
Income and growth Per capita real GDP at the beginning of the period and annual growth rates of real GDP per capita during the previous period are based on the Penn World Tables (PWT) 6.2.
Inequality Period Gini coefficient, primarily based on household-level income surveys for the entire population (in some cases only consumption-based data were available). Missing years filled in with country mean and controlled for with a dummy variable. Taken from UNU/WIDER (2007) dataset.
Instability Political instability measure, defined as a change >2 in the Polity2 indicator during the previous three years, updated from Fearon and Laitin (2003).
Latitude Absolute value of latitude of a country on a scale of zero to one. From La Porta et al. (1999).
Mountainousness Percent of total land area covered by mountains, taken from Fearonand Laitin (2003).
Natural resource abundance Data on total natural resource wealth, subsoil (fuel and non-fuel mineral) wealth, and land wealth (cropland, pastureland, timber and non-timber forests, and protected areas) was taken from World Bank (1997, 2006). The measures estimate the net present value in USD per capita of rents from the resources. See World Bank (1997) for more details). Estimates of per capita oil production and reserves are taken from Humphreys (2005), while the value of fuel and non-fuel minerals in 1970 (in USD) is taken from Norman (2007) and scaled by population (see below).
New State Dummy variable with one indicating a newly independent state (first two years of independence). Taken from Fearon and Laitin (2003).
Non contiguous Dummy variable indicating whether a state has non-contiguous territories. Taken from Fearon and Laitin (2003).
Openness Average openness (imports plus exports over GDP) during the previous period, based on PWT 6.2.
Peace Number of years since the last civil war, or since the end of WWII.
Polity Polity2 indicator from the Polity IV dataset. This measures a regime on a scale from -10 (institutionalized autocracy) to +10 (institutionalized democracy).
Population Total population at the beginning of the period, taken from PWT 6.2.
Presidential system dummy Takes a value of one if the chief executive is directly presidential or a strong president elected by an assembly, and zero if parliamentary. Based on Beck et al. (2005).
Primary commodity exports over GDP/ Resource dependence (sxp) Updated from Collier and Hoeffler (2004) with data from the World Development Indicators (WDI). The measure includes exports of foods and beverages, raw agricultural materials, fuels, and ores and metals.
Social, ethnic and religious fractionalization Measures of fractionalization are taken from Collier and Hoeffler (2004). They range from 0 (totally homogeneous) to 1 (completely heterogeneous).
Tropics Percent of land area in the geographical tropics, taken from the Geography Dataset by Gallup et al. (see above).
Warstarts The main measure is updated from Collier and Hoeffler (2004) with data from the Correlates of War (COW) database by Gleditsch (2004). The alternative measure is based on the Armed Conflict Database (ACD) and is compiled by Gleditsch et al. (2002).
Young men in total population This gives the proportion of young men aged 15-29 in the total population in percent. The data is taken from various UN Demographic Yearbooks.

TABLE 1. DESCRIPTIVE STATISTICS

Means	Sample	Peaceful episodes	Conflict episodes
Warstart	0.071	0	1
s.d. (obs)	*0.257 (1311)*	*0 (1218)*	*0 (93)*
Years of peace since last conflict	28.851	29.725	17.41
s.d. (obs)	*15.177 (1311)*	*14.834 (1218)*	*15.056 (93)*
GDP per capita (USD) (in logs)	8.235	8.344	7.473
s.d. (obs)	*1.135 (1211)*	*1.128 (1062)*	*0.823 (74)*
GDP growth in previous period	1.418	1.647	0.017
s.d. (obs)	*3.38 (1130)*	*3.103 (995)*	*4.928 (64)*
Total primary export share of GDP	0.165	0.176	0.142
s.d. (obs)	*0.183 (1311)*	*0.191 (1190)*	*0.116 (86)*
Total natural capital (USD per capita, in logs)	8.441	8.497	7.868
s.d. (obs)	*0.951 (821)*	*0.964 (733)*	*0.630 (41)*
Oil production per capita	0.0418	0.047	0.0142
s.d. (obs)	*0.2845 (1104)*	*0.2845 (943)*	*0.0376 (77)*
Oil reserves per capita	0.7513	0.8574	0.1512
s.d. (obs)	*6.8045 (1119)*	*7.3567 (955)*	*0.5467 (80)*
Value of minerals in 1970 (USD per capita)	27.5116	31.0021	5.0909
s.d. (obs)	*159.0941 (1230)*	*170.112 (1072)*	*11.0884 (76)*

Table 2. Civil war onset

Panel A	(1) probit	(2) probit	(3) probit	(4) ivprobit	(5) ivprobit	(6) ivprobit
sxp	3.291					
	(1.75)*					
sxp^2	-3.498					
	(1.00)					
lnsxp		0.24	0.161	0.0065	-0.0135	0.0294
		(2.40)**	(1.23)	(0.03)	-0.05	(0.11)
lnnatcap			-0.308			
			(-2.21)**			
lngdp	-0.374	-0.389	-0.254	-0.783	-0.834	-0.778
	(3.40)***	(3.49)***	(1.80)*	(3.38)***	(2.88)***	(3.09)***
growth	-0.0719	-0.0715	-0.108	-0.0809	-0.103	-0.0821
	(3.09)***	(3.08)***	(2.83)***	(1.99)**	(1.89)*	(1.97)**
lnpop	0.160	0.174	0.159	0.165	0.164	0.171
	(2.93)***	(3.06)***	(2.19)**	(1.63)	(1.25)	(1.52)
socialfrac	-0.0001	-0.0001	-0.00004	-0.00001	-0.00004	-0.0001
	(1.06)	(1.10)	(0.74)	(1.25)	(0.42)	(1.24)
polity	0.0029	0.0039	0.0172	0.0401	0.0375	0.0393
	(0.25)	(0.33)	(1.12)	(2.13)**	(1.65)*	(1.93)*
peace years	-0.0208	-0.0201	-0.0173	-0.0100	-0.005	-0.0101
	(3.97)***	(3.85)***	(2.77)***	(1.40)	(0.51)	(1.38)
frafcol	-0.685	-0.701	-0.362	-0.696	-0.566	-0.690
	(1.99)**	(2.04)**	(0.91)	(1.79)*	(1.02)	(1.74)*
youngmenpop	-0.0058	-0.0066	-0.0044	-0.0054	-0.0155	-0.0047
	(0.52)	(0.59)	(0.35)	(0.39)	-0.67	(0.34)
mountain	0.0051	0.0051	0.0076	0.004	0.00775	0.0045
	(1.41)	(1.41)	(1.73)*	(0.79)	(1.15)	(0.83)
Res. abund. var.			lnnatcap	lnnatcap	lnsubsoil	lnland
Observations	865	865	696	688	505	676
Pseudo R^2	0.21	0.21	0.210			
Log likelihood	-176.0	-175.9	-116.8			
Wald exogeneity p				0.0144	0.0581	0.0344
Joint endogeneity p				0.0068	0.0508	0.0147

Notes: The dependent variable is *warstart*. Second-stage results shown for IV probit estimations in columns (4)-(6). p-values given for Wald test of exogeneity and joint endogeneity test (the latter performed in linear regression). Absolute value of z-statistics in parentheses. *, **, *** statistically significant at 10, 5, and 1 percent levels, respectively. All regressions include an intercept (not shown).

Table 2 (continued - first stage results from ivprobit estimations)

Panel B	(1) *lnsxp from spec. (4)*	(2) *lnsxp from spec. (5)*	(3) *lnsxp from spec. (6)*	(4) *lngdp from spec. (4)*	(5) *lngdp from spec. (5)*	(6) *lngdp from spec. (6)*
lnnatcap	0.3563			0.4207		
	(12.68)***			(16.38)***		
lnsubsoil		0.1578			0.1759	
		(11.91)***			(14.01)***	
lnland			0.2529			0.3402
			(8.10)***			(11.81)***
openness	0.0091	0.0091	0.0081	0.0047	0.001	0.0042
	(9.22)***	(8.00)***	(7.67)***	(5.20)***	(0.89)	(4.31)***
presidential	0.1863	0.2586	0.1607	-0.1698	0.1156	-0.2147
	(2.46)**	(3.20)***	(2.04)**	(2.46)**	(1.51)	(2.95)***
latitude	0.4361	1.4337	0.4176	0.7971	1.9033	0.6665
	(1.35)	(4.46)***	(1.24)	(2.69)***	(6.24)***	(2.14)**
distcr	-0.000002	-0.0001	-0.00004	-0.0005	-0.0008	-0.0005
	(0.03)	(1.05)	(0.49)	(5.93)***	(7.75)***	(5.96)***
tropics	0.8762	1.0476	0.8434	-0.4235	-0.3058	-0.4668
	(6.93)***	(7.97)***	(6.47)***	(3.67)***	(2.45)**	(3.88)***
peace years	-0.0053	-0.0051	-0.0058	0.0072	0.0135	0.0069
	(2.97)***	(2.64)***	(3.18)***	(4.42)***	(7.34)***	(4.09)***
Adj R2	0.57	0.69	0.53	0.75	0.74	0.73

Notes: Panel B shows 1st stage results for all exogenous instruments from the two-step IV probit analyses, plus the coefficient for years of peace. The overidentification tests for specifications (4), (5), and (6), are respectively: (Sargan N*R-sq test 0.1397, Basmann test 0.1442), (0.4381, 0.4503), (0.0985, 0.1018). Absolute value of t-statistics in parentheses. *, **, *** statistically significant at 10, 5, and 1 percent levels, respectively. All regressions include an intercept (not shown).

Table 3. Specification tests

	(1)	(2)	(3)	(4)	(5)	(6)
lnsxp	0.137	0.0115	0.126	-0.114	0.162	0.142
	(0.57)	(0.05)	(0.46)	(0.41)	(0.58)	(0.50)
lngdp	-0.915	-0.837	-1.368	-0.817	-0.710	-0.716
	(3.55)***	(3.42)***	(4.17)***	(3.67)***	(3.06)***	(2.91)***
growth	-0.0734	-0.0786	-0.0846	-0.0553	-0.101	-0.0977
	(1.74)*	(1.85)*	(2.00)**	(2.16)**	(3.35)***	(3.20)***
lnpop	0.159	0.164	0.0876	0.0890	0.202	0.192
	(1.48)	(1.58)	(0.81)	(0.97)	(2.17)**	(1.98)**
socialfrac	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(1.45)	(1.37)	(0.72)	(1.71)*	(1.73)*	(1.69)*
polity	0.0413	0.0481	0.0551	0.0196	0.0253	0.0227
	(2.07)**	(2.41)**	(2.70)***	(1.31)	(1.57)	(1.41)
peace years	-0.0043	-0.0119	0.0011	-0.0134	-0.0113	-0.0117
	(0.55)	(1.53)	(0.13)	(2.23)**	(1.93)*	(1.97)**
frafcol	-0.798	-0.762	-0.349	-0.877	-0.827	-0.850
	(1.90)*	(1.94)*	(0.82)	(2.38)**	(2.17)**	(2.22)**
youngmenpop	-0.0077	-0.0071	0.0007	-0.0043	-0.0065	-0.006
	(0.55)	(0.50)	(0.051)	(0.35)	(0.54)	(0.49)
mountain	0.0062	0.0040	0.0033	-0.0019	0.0035	0.0028
	(1.18)	(0.79)	(0.62)	(0.37)	(0.70)	(0.54)
instability	0.249					
	(1.08)					
new state	0.360					
	(0.47)					
non contiguous	0.269					
	(0.83)					
inequality	-0.0147					
	(1.24)					
dum70s		0.664				
		(2.46)**				
dum80s		0.500				
		(1.68)*				
dum90s		0.534				
		(1.83)*				
ssa			-1.652			
			(2.88)***			
latam			-0.563			
			(1.56)			
Res. abund. var.	lnnatcap	lnnatcap	lnnatcap	minerals pc	oil prod. pc	oil res. pc
Observations	676	688	688	814	808	810
Wald exogeneity p	0.0075	0.0222	0.0006	0.0352	0.0946	0.104
Joint endogeneity p	0.0003	0.0065	0.0001	0.0583	0.1089	0.1001

Notes: All estimations are IV probit; only second-stage results shown, with *warstart* as the dependent variable. The exogenous instruments for *lnsxp* and *lngdp* in the 1st stage are average openness during the previous period, a presidential dummy, latitude, percent land in tropics, distance to nearest coast or navigable river, and a natural resource abundance measure. The natural resource abundance variable in columns (1)-(3) is taken from the World Bank; the per capita value of fuel and non-fuel minerals in 1970 in column (4) is based on data by Norman (2007); per capita oil production and reserves data in columns (5) and (6), respectively, are from Humphreys (2005). Absolute value of z-statistics in parentheses. *, **, *** statistically significant at 10, 5, and 1 percent levels, respectively. All regressions include an intercept (not shown).

Table 4. Robustness tests

	(1)	(2)	(3)	(4)	(5)
Dep't var.	*COW*	*COW*	*COW with ongoing conflict*	*ACD*	*ACD with ongoing conflict*
	relogit	xtivreg	ivprobit	ivprobit	ivprobit
lnnatcap	-0.596				
	(2.02)**				
lnsxp	0.277	0.0052	0.007	0.600	0.478
	(0.83)	(0.26)	(0.032)	(2.79)***	(2.51)**
lngdp	-0.449	-0.0730	-0.817	-0.734	-0.590
	(1.67)*	(4.37)***	(3.71)***	(3.71)***	(3.36)***
growth	-0.199	-0.0086	-0.0651	0.0011	-0.0004
	(2.28)**	(2.47)**	(1.71)*	(0.037)	(0.015)
lnpop	0.308	0.0153	0.155	0.366	0.267
	(1.65)	(1.69)*	(1.60)	(3.88)***	(3.29)***
socialfrac	-0.0001	-0.00001	-0.0001	-0.0001	-0.0001
	(0.75)	(1.74)*	(1.46)	(2.16)**	(2.17)**
polity	0.0345	0.0039	0.0336	0.0348	0.0214
	(1.05)	(2.24)**	(1.82)*	(2.09)**	(1.40)
peace years	-0.0299	-0.0014		0.0007	
	(2.61)***	(2.11)**		(0.13)	
previous conflict			0.0405		-0.183
			(0.16)		(1.09)
frafcol	-0.394	-0.0691	-0.753	-0.123	-0.0667
	(0.48)	(2.19)**	(2.01)**	(0.50)	(0.29)
youngmenpop	-0.0014	-0.0004	-0.0044	-0.0087	-0.0044
	(0.06)	(0.28)	(0.34)	(0.82)	(0.45)
mountain	0.0154	0.0009	0.0036	0.0035	0.0017
	(1.70)*	(1.84)*	(0.75)	(0.88)	(0.47)
Observations	696	688	728	649	736
Wald exogeneity p			0.0171	0.0019	0.0079
Joint endogeneity p			0.0319	0.0115	0.0322

Notes: The dependent variable is *warstart* according to two different datasets, the Correlates of War (COW) by Gleditsch (2004) and the Armed Conflict Database (ACD) by Gleditsch et al. (2002). Column (1) shows a basic regression performed with rare events logit, corresponding to Table 2, column (2). Column (3) shows our core specification (corresponding to Table 2, column (4)) in a linear panel IV regression. Columns (3) and (5) code ongoing conflicts as "0" instead of missing, and include a dummy variable for conflict in the previous period. Only second-stage results are shown. The exogenous instruments for *lnsxp* and *lngdp* in the first stage are average openness during the previous period, absolute latitude, and total natural resource wealth *lnnatcap* in columns (3)-(4), and additionally a presidential dummy, percentage of land in the tropics, and distance to the nearest coast or navigable river in columns (1)-(2). Absolute value of z-statistics in parentheses. *, **, *** statistically significant at 10, 5, and 1 percent levels, respectively. All regressions include an intercept (not shown).

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