Chapter 13: Exploring the Resource-Civil War Nexus

Prepared for T. David Mason and Sara McLaughlin Mitchell eds.
What Do We Know About Civil War?

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Since Paul Collier and Anke Hoeffler (1998) suggested nearly twenty years ago that economic incentives were a major determinant of civil wars, the exploration of possible links between natural resources and conflict has grown dramatically. Given the prevalence of intrastate violent conflicts in the post-Cold War world, interest in this possible relationship has grown all the more, leading to major investments in research by the World Bank and other international organizations. As it has grown, however, scholars have collectively accumulated a mass of conclusions that sometimes contradict one another. This lack of consensus makes it difficult to extract solid implications, with a majority of researchers finding a positive link between resources and conflict, but a sizable minority either finding no relationship or a negative (conflict-reducing) role.[1] The growing divergence of conclusions warrants some stock-taking for two reasons. First, the question of whether or not there exists a "resource curse" is a major one in the social sciences and its scope extends well beyond the study of civil war to tracing the determinants of long-term economic development, the prospects for democracy, and broader political stability. Second, this is a widely important substantive question in international politics. Just within one "resource," in the global oil sector, there are at least fifty states that can be classified as major oil producers (Ross 2012, 20-21). One-quarter of the world's independent states falling into the resource-rich category after considering just one non-renewable resource merits close attention by both scholars and policy makers for analytic and prescriptive reasons.

[1] Full disclosure: this author generally comes down in the conflict-reducing camp.

This chapter outlines the state of current research on the resources-civil war linkage. I would note up front that it is an intentionally non-technical review of the research. There are a number of excellent but more technical compliments to this chapter, among them Ross (2014), Koobi et al (2014), and Humphreys (2005). Also, I focus primarily, although not exclusively, on fuel resources—in particular oil and natural gas (hereafter simply "oil"). I do so because oil is the most important non-renewable commodity resource in the world, accounting for the vast majority of commodity trade globally.[2]

The chapter proceeds as follows. First, I outline the origins of the hypothesis that oil would increase the likelihood of violent intrastate conflict. Second, I explore the various linking mechanisms that have been theorized to tie oil wealth to conflict. Third, I discuss briefly how scholars typically *measure* oil wealth and conflict—after all, a hypothesis cannot be tested until we find a way to measure the concepts in it. Fourth, I summarize the current debate over whether national- or sub-national level research is the more appropriate level of focus, highlighting where results differ as a function of this decision. Finally, I suggest some of the more promising avenues for future research, noting in particular the valuable new contributions stemming from locational data on oil fields. To summarize my conclusions briefly, the jury is still out on whether resources are a direct determinant of violent civil conflict or whether they are a result of weak state institutions along *with* such conflict. As a result, the weak states variant of the resources and conflict thesis is less compelling than the possibility that aspects of resource wealth—in particular its location in regions populated by excluded or dominated minorities—might conditionally shape the likelihood of violent conflicts erupting.

[2] In this broader research program, scholars also address the conflict implications of extractive sectors such asa minerals, diamonds, timber. More expansively we see exploration of still more commodities: water, agricultural production, fisheries etc.

Origins of the Thesis that Oil Breeds Conflict

The end of the Cold War brought, among other things, a fairly large number of civil wars into sharp focus for both the international and scholarly communities. Conflicts that had often been folded into manifestations of superpower rivalry or whose ideological meta-narratives (Kalyvas 2001) had been only lightly questioned lost that cover with the collapse of the Soviet Union. At the same time, a number of new conflicts within the former Soviet Union and outside it broke out in the first half of the last decade of the 20th century. These conflicts took place historically outside the Cold War, pulling back a convention to ascribe loose left-right division to violent internal conflicts and demanding a new set of explanations. A number of them—in Angola, Algeria and the Republic of Congo among others—resonated with the apparent lessons of Iran's 1979 revolution and Nigeria's Biafra war to generate a sense that resource-rich countries might be more prone to civil war outbreak than others. This was particularly true in sub-Saharan Africa, where a full third of the civil wars of the 1990s took place (Ross 2004, 47).

At the same time, countries that were both resource-rich and either politically unstable, autocratic or economically stagnant continued to proliferate. Civil wars in Algeria, Liberia, Sierra Leone and Angola, to take just a handful, were tied to those countries' ample diamond and oil wealth. Authoritarianism and oil were tied together, too, in an important article by Ross (2001). Economic variants of the resource curse accompanied these political ones. The economic resource curse was centrally about "Dutch Disease," a dynamic in which a booming resource sector (e.g. the discovery of North Sea oil resources in the Netherlands) inflates a country's currency value, pricing its agricultural and manufacturing sectors out of global competitiveness and later undermining growth and diversification (Sachs and Warner 2001; Frankel 2010). Nigeria's trajectory from strong agricultural producer to heavy agricultural importer between independence and 1990 illustrates this: as the country's oil sector grew, it drew investment away from agriculture so completely that Nigeria must import the majority of its produce.

Another major economic strand of the resource curse is persistently lower economic performance over time. Ross (2012) explains that on average, oil producing countries should have grown substantially faster than their oil-poor counterparts but instead barely broke even with them: it was as if the hundreds of billions of dollars in oil revenues accomplished nothing. As we shall see, slow growth and failed development are then centrally implicated in conflict outbreak. In short, the economics of the resource curse—stagnating non-resource sectors and slow growth—highlighted the material factors that could lower the perceived opportunity costs to rebelling.

Paul Collier and Anke Hoeffler (1998) inaugurated the political economy approach to civil war studies with a path-breaking effort to tease out a possible link between resources and conflict, and catalyzed a new greed-based approach to studying civil war onset: "War occurs if the incentive for rebellion is sufficiently large relative to the costs" (563). Pairing a utility model of rebellion with data capturing primary exports as a share of GDP, they found that resource wealth both increased the risk of civil wars and prolonged those that did break out. The logics were twofold for both center-seeking and secession wars. First, the possibility of capturing a state bringing in such immense revenue with virtually no social cost (as with taxation) was a strong incentive for leaders to organize, and for followers to join, rebellions. Second, the prospect of taking a resource-rich region to independence promised future benefits far outweighing the costs of a secessionist rebellion. The authors uncovered a positive correlation between primary commodity dependence and the onset of major intrastate conflict. This original finding underlays much of the subsequent research on civil war causes since.

Some aspects of this starting point are worth noting. First, Collier and Hoeffler conceptualized all primary commodities through the same lens: oil, palm oil, coffee, gold, and diamonds would be considered functionally the same, and presumed to have the same effect on the calculus of would-be rebels. As scholars pushed this research program forward, it became clear that this was a problematic assumption. Second, no apparent realization that commodity export dependence can be endogenous to political strife is evident. We have come to realize that countries undergoing civil war or a number of other forms of internal strife tend to see both foreign and domestic investment shrink as a function of shortened time horizons and uncertain stability and to see economic activity in general contract. This dynamic shrinks the size of the non-commodity sectors of the economy, thereby reducing the size of the overall GDP and boosting the size of the commodity revenue to GDP ratio. Third, Collier and Hoeffler found a non-linear (inverse U-curved) relationship between resource wealth and conflict, with the effect initially increasing to a threshold and then providing a stabilizing effect. This non-linear relationship harbored implications for future conditional analyses. Finally, the actual exploration of possible causal links between resources and conflict is thin and the theoretical framework for the original paper is entirely formal with no direct empirical inquiry as to causation. As a result, it left open questions of the causal chains that might be shown to tie resource wealth to the onset and duration of civil wars. The next section turns to this last issue: the exploration in subsequent research of causal links.

Theorizing the Mechanisms and Motives

Despite the somewhat coarse nature with which the greed-grievance dichotomy was originally spelled out (see Chapter 2, this volume), the theorized mechanisms linking resource wealth to civil conflict track fairly well along a grievance-greed continuum. It is important to keep in mind that the "greed" end of it has come to suggest a broader set of economic reasons more than simply economic gain—ranging from greed to basic needs provision or subsistence—but as outlined below, this range captures the set of mechanisms adequately. On the grievance end, we see two main lines of argument. The first is one related to the initial development of the rentier state theory (Mahdavy 1970; Beblawi and Luciani 1987; Delacroix 1980). This theory held that oil contributed directly to weak state capacity by obviating the need to build an effective extractive apparatus for collecting revenue. It was deeply influenced by European-derived theories of state formation that centered on the need to raise revenues to support standing professional armies during the formative centuries of nation-state building in Western Europe. The rentier state thesis of conflict is basically this: oil leads to weak state formation or to state decay. The concomitant lack of ability to collect revenues effectively leads to broader state decay, eroding public goods provision capabilities. This in turn generates the kinds of grievances that can lower the cost to benefit ratio of rebelling.

The weak state argument has been used in numerous ways both to link resources and conflict and as a standalone hypothesis in its own right. Fearon and Laitin (2003) use income per capita as a proxy for state strength and find, unsurprisingly, that it reduces the risk of civil war. They do not explore whether resource wealth has an independent effect on state strength as an intervening variable. What they do instead is to theorize that weak states are incapable of policing their territory—the classic Weberian state imperative—and thus are likely to fail at suppressing insurgents. Macartan Humphreys (2005) also theorizes this way, and finds that although oil appears to weaken state capacity, its main conflict-inducing effect is not through that mechanism. It is important to note that Humphreys does not model civil war onset with a state capacity indicator; I discuss this more below. Cullen Hendrix directly explores the state capacity thesis, employing factor analysis to explore fruitful strategies for making the broad concept operationally manageable (2010). Thies (2010) and Mitchell and Thies (2012) find, respectively that civil war weakens state capacity and that resource dependence is endogenous to ongoing conflict. Both of these cast doubt on the independent role of state capacity in fomenting conflict and on the indirect effect of resources influencing conflict by eroding state capacity. For the most part, the rentier state variant of the grievance mechanism is thought to shape the risk of center-seeking wars breaking out, both by weakening the state’s capacity to provide public goods and by reducing its ability to maintain social order or to quell rebellions.

The second broad strand of grievances has to do with inequitable distribution of the goods that resource revenues can provide. Particularly in states geographically divided along ethnic lines, weak states are predisposed to favor some regions and groups versus others (Wimmer 2012). Ethnic favoritism makes this starker, raising the likelihood of the emergence of a sense of relative deprivation by have-not groups. Recent research at the sub-national level has engaged this dynamic with promising new data and detail, and as I discuss below, has helped to put empirical richness into this particular strand of the grievance thesis while providing more nuanced understanding of the interplay between the economic incentives in natural resources and the grievances that state management of them can catalyze.

The broad set of economic incentives for both rebel leaders and for potential followers – what has come to be simply called the greed thesis—was nicely spelled out in two edited volumes on the topic (Berdal and Malone 2000; Collier and Sambanis 2005). The contributors to the Berdal and Malone book mostly devoted themselves to fleshing out both macro- and micro-variants of economic explanations, often going beyond the original hypotheses laid out by

Collier and Hoeffler. This first set of chapters established a number of durable war economy equilibria—situations likely to arise in civil war settings that powerful actors might find it profitable to sustain. A second set of chapters proposed international remedies for this growing set of wartime economic incentives.

Ross (2004) explored causal mechanisms inductively with a sample of thirteen civil wars from the 36 that took place during the 1990s. On one hand, it is worth noting that the other twenty-three civil wars appeared to have had little to do with resources. On the other hand, Ross finds in the resource-linked civil wars evidence for a causal onset effect in just two, with no evidence to bolster the looting mechanism and little to support a grievance hypothesis. In assessing duration, however, Ross finds some strong support for the looting hypothesis: in ten of thirteen of the conflicts, looting appeared to have kept them in motion. This is fully in line with the first set of chapters in Berdal and Malone, which developed a number of rich accounts of wartime economic orders that go well beyond a simple "looting" model and explored a ride range of ways that actors might profit from the continuation of conflict. Ross' additional insight is that, while rebel actors may not originally be motivated by prospects for material gain, their understandings and preferences can shift during the conflict itself, so that what may not have been a powerful motive at the outset can become one later.

In addition, Ross found evidence of two previously understudied mechanisms: foreign intervention and what he termed "booty futures." In the latter, rebels bargain with the future value of extraction rights on territory they hope to conquer (58).[3] Perhaps most importantly, in assessing the effect of resources on conflict, Ross finds multiple causes at work in the resource

[3] For example, in advance of the American-British invasion of Iraq in 2003 Iraqi Kurds began making public statements indicating that they would view partnerships with American and British oil companies favorably in a post-Saddam Iraqi political economy.

curse: no single mechanism appears in more than nine of thirteen cases, suggesting multiple pathways. He concludes that "this multiplicity of causal linkages…may help account for the analytical muddle, and contradictory findings, of earlier studies" (2004b, 62)

The contributors assembled by Collier and Sambanis (2005) explored fifteen different civil war case studies to assess the utility of Collier's and Hoeffler's greed and grievance mechanisms in explaining the outbreak and unfolding of each case. The general conclusion emerging from this collection of cases was essentially that motives tend to be more complex than a single "greed" or "grievance" lens can capture. Indeed, as the editors note, "case studies offer a more textured and nuanced view of civil war and show that the distinction between "greed" and "grievance" in the CH model should be abandoned for a more complex model that considers greed and grievance as inextricably fused motives for civil war" (2).

Edward Aspinall's analysis (2007a; 2007b) of the evolution of Acehnese identity and incorporation of the region's oil and gas reserves into that narrative illustrates nicely how resource wealth can be woven into aspects of both economic motives for gain and of anti-state grievance. In this case, he argues, the Free Aceh Movement, (GAM, for *Gerakan Aceh Merdeka*) built a narrative of economic deprivation of Aceh's rightful gains from its oil. This became a powerful part of a broader identity frame of resistance to the Indonesian government, particularly for urban and more educated Acehnese. Moreover, Aspinall is careful to distinguish the role that the resource narrative played among different strata of Acehnese society. Less well educated and rural Acehnese GAM recruits and supporters were much less influenced by it, largely joining instead for reasons related to direct experience of state violence and family lineage in past rebellions. This qualified role of Aceh's resource wealth in the broader narrative is an important corrective to macro-accurate but micro-inaccurate accounts such as that provided by Kell (1995).

What I mean by this is that scholars who proceed in this vein sometimes neglect to explore the salience of resources and their monopolization by central governments as a reason for participating in rebellion. Aspinall, by contrast, does exactly this, asking a wide array of former GAM supporters and fighters about the role of resources and finds that a small, mostly urban and highly educated subset were convinced by the resource narrative. I return to this theme in the conclusion.

Among other things, the insights afforded by Aspinall's extensive ethnographic research in Aceh provide a research design link to a prominent research program in civil war studies: that focused on why ordinary people are willing to incur extreme risk to themselves and their families to participate in a rebellion. Drawing such links can facilitate broader inquiry that normalizes resource-related rebellions in a fuller set of civil conflicts. To take one non-resource related example, Elizabeth Wood's (2003) work on the civil war in El Salvador exemplifies this ecumenical approach to theorizing and explaining participation as a dual function of both material and non-material considerations. Since resource issues are of course a powerful additional layer on top of what are already complex motivating factors in the choice to join a rebel group, our limited vistas of what this kind of micro-qualitative research can uncover should prompt more inquiry along the same lines.

An additional methodological cautionary note from the Collier-Sambanis case study war project is the danger of spurious correlation. As Sambanis puts it, in a number of important cases "the narratives in this volume show that those natural resources were neither a motive for the war nor a means to sustain rebellion" (309). In others, as in the Democratic Republic of the Congo, it was not resource wealth per se but its concentration in the country's east, where ethnically dominant regional groups threatened secession and the resource-poor but national dominant west felt there was no alternative but to exercise substantial state repression. This conditional relationship between ethnicity, inclusion/exclusion and resources turns up again in Ross' chapter in the volume on Aceh, as well as in more recent econometric work discussed below.

Capturing the Concept of "Resource Wealth: Measurement Issues and Levels of Analysis

Scholars who explore the onset of civil wars quantitatively face several measurement choices. The initial generation focused exclusively at the national level, drawing on publicly available indicators. The first widely used continuous measure of natural resource wealth, drawing on the Sachs and Warner (1995) and Collier and Hoeffler (1998) precedents, was to take commodity export revenues as a share of GDP. This measure captured the relative economic dependence of a country—thus of both its government and of its populace—on the resource sector. Capturing the concept this way became the standard not just for civil war studies but also for work on the effect of oil on regime type and durability (Ross 2001; Smith 2004, 2007; Morrison 2009).

Measuring it this way, however, created problems of endogeneity. Poorer countries—less industrialized, more dependent on agriculture, and with a smaller non-oil economy—looked more *resource dependent* even if they were not necessarily more oil-*abundant* due to their baseline GDP being smaller overall. Another problem with this measure was that by focusing only on revenues derived from the export of oil, it biased the indicator against economically diverse countries that consumed most of what they produced. The United Stated, for example, has been one of the world's largest volume oil producers for the last four decades, but as the world's largest economy, it consumes nearly all of its produced oil. In the last two decades a number of increasingly well-governed and economically diversified countries—Brazil is one notable example—have tapped massive new oil and gas reserves and expanded their economies so far beyond just commodities that their resource revenues to GDP ratios would look relatively small. In short, while earlier indicators provided a useful way to think about resource dependence, they are less useful for measuring abundance.

A bevy of studies not specifically focused on resource wealth as the explanatory variable of choice have employed different dummy variables to capture one aspect or another of the concept—among them OPEC membership, oil exports more than 50 percent of total exports, oil more than 25 percent of GDP and a host of others. These were more problematic by far than the oil export revenues to GDP measure. Today, for example, OPEC countries make up only a quarter or so of the major oil producers in the world. Moreover, we have no good analytical reason to believe that arbitrary threshold points such as 25 or 50 percent demarcate a point at which oil suddenly begins to have political importance. Finally, coding country years as "1" if oil exports comprised more than half of total exports told us nothing about the importance of exports in a country's economy. A large, prosperous country with a sizeable domestic consumer market would have a smaller export to GDP ratio than a smaller, equally prosperous country (such as Norway) but this figure would not tell us as much as we would want to know to think about the empirical importance of oil wealth.

Subsequently, scholars (Humphreys 2005; Ross 2012) constructed a new measure, based on fuel income per capita. This indicator, not dependent on a GDP denominator for its magnitude, solved the problem of being endogenous to a country's level of development and provided a consistent, easy to measure standard. It also captures the conceptual half of resource wealth that we think of as abundance—the total amount of wealth per person that accrues to a nation's economy from the production and sale, abroad or at home, of natural resources. It does not tell us (Smith 2014) how relatively important the fuel income per person is in differing contexts, however, and this point raises the issue of thinking about two kinds of oil wealth: dependence and abundance. Ratio measures with GDP in the denominator capture the former, oil income per capita the latter.

A number of recent studies (Basedau and Lay 2009; Lederman and Maloney 2007, Dunning 2008) have found both oil abundance and dependence to be statistically significant and substantively important for predicting civil war onset, although sometimes in the opposite direction. Given the analytical differences between the two dimensions of resource wealth, and their contradictory effects, best practices at least in preliminary empirical analysis would suggest employing a measure for each.

A third set of studies attempt to move beyond income from oil and gas to reserves and in some cases pursue an instrumental variables approach.[4] Cotet and Tsui (2013) use data on oil discoveries, the value of oil reserves, and on natural disasters in producing countries. Their goal in developing and employing alternate measures to fuel income is to sidestep the endogeneity problems inherent in production value-based measures. Here, in contrast to the "resource curse" thesis, they find no relationship between oil and conflict, after controlling for country fixed effects.[5] They do find that oil-rich countries spend more on defense, which may provide an explanation for the lack of an effect, but in essence their conclusion is that omitted variables specific to countries matter much more than the fact of national-level oil wealth.

[4] When scholars suspect that explanatory (causal) variables are correlated with a model's statistical error term, they sometimes employ an instrumental variables (IV) approach, which are simply indicators related to the explanatory variables but not to the error term. In political economy research, many factors such as resource wealth are prone to this problem, hence the increasing commonness of IV analysis.

[5] Fixed effects analyses are common in political economy research when scholars suspect that the units of analysis—especially countries—may have time-invariant traits not accounted for in the set of variables included but that might be causally important. Concretely, a scholar asks, "are there things about Nigeria (or any other country) that could influence the dependent variable but that we haven't identified or measured?" Fixed effects models account for these potential omitted variables.

Brunnschweiler and Bulte (2009) similarly attempt to attenuate the risk of endogeneity by using per capita reserves and production figures as proxies for resource abundance. Their conclusions are further still from the conventional wisdom: resource abundance is strongly associated with less risk of conflict onset, and resource dependence is not only not a cause of conflict but appears to be a systematic effect of it. In other words, as discussed above, the effect that conflict has on non-resource economic productivity is strongly negative enough to depress overall GDP, which by reducing the size of the denominator, produces the statistical appearance of "resource wealth." As mentioned above, Mitchel and Thies (2012) find similar endogeneity to exist between conflict and resource production.

Levels of Analysis

For much of the two decades during which serious comparative work was ongoing in this research program, it was focused at the national level, as discussed above with regard to measurement. The question in cross-country research effectively asked whether countries rich in natural resources were more likely to suffer conflict than their resource-poor counterparts. Indeed, the strongest book-length case for the resource curse (Ross 2012) is sub-titled "Petroleum Wealth and the Development of *Nations*" [my italics]. There are multiple reasons for this. First, both development economics and the political science sub-fields of comparative and international politics were for a very long time all focused squarely on national states as the units of analysis. Most of the outcomes of interest for scholars across these disciplines therefore manifested at that level, and with the study of civil wars and of the effect of resources on their onset, it was a natural extension to continue the nation focus. Second, it was equally the case that until relatively recently, we lacked data at the sub-national level for enough states to conduct representative comparative research. The Correlates of War Project, and then the Armed Conflict Data project,[1] both originally measured conflict only at the national level, as did the Polity and Freedom House regime datasets, World Bank, International Monetary Fund, and Penn World Tables data projects, meaning that not only our dependent variables (wars) but our independent and control variables too were captured at the national level. Finally, nearly all of the original body of theory we have to guide us in exploring the politics of resource wealth points directly to national states as the main locus of causality.

Rentier states, national-level corruption, ethnic favoritism, and similar theoretical frameworks all rely on the presumption that states are the main targets in town, both for scholarly inquiry and for the potential rebels seeking either to capture the center or to exit from it. Is this a problem? To the extent that national-level data cloud potential sub-national dynamics in the resource-conflict relationship, yes. To the extent that national-level data are arguably plagued by causal identification problems that could be addressed with more fine-grained data, again yes. These two arguments are at the center of critiques of past research (see Ross 2014 and Koubi et al 2014 for summaries). Two trends in conflict research data may offer partial solutions to these problems. First, research on variation across space during civil wars (for example Straus 2006; Kalyvas 2006; Balcells 2010) helped to focus the attention of scholars on why some parts of countries during civil war were so much more violent than others. In each of these cases—Rwanda, Greece, Spain—the civil wars under discussion have not been argued to have been shaped by resource wealth. Rather, each of these conflicts turns out to have evinced important variation across even very small national territories, opening a new line of inquiry. And, the increasing turn to sub-national exploration of patterns of violence during civil wars has in turn helped to reshape the contours of research specifically focused on the resource link. It has in part been motivated by scholars convinced that the national-level confusion of findings is a function of sub-national variation.

One of the strongest new lines of inquiry to have emerged in the broader civil wars research program in the last five to ten years has been the proliferation of sub-national analyses versus national-level ones (see Chapter 15, this volume). Considering the growing improvement of data quality at the sub-national level, and the volume of non-resource focused research on violence during civil wars (for example Balcells 2010; Kalyvas 2006) this is a welcome direction for research. In particular sub-national research promises to help develop answers to two sets of questions. First, do ethnic minorities or sub-national regions launch violent challenges to governments more often when the resource reserves are located within their regions? Second, do we see sub-national variation within national settings that might help to explain the inconsistency of country-level research?

In addition to these problems is the reality that all of the above-mentioned measurement strategies focus only at the national level. If the effects of resource wealth on conflict accrue *only* there, this could be a reasonable strategy. At this point, however, many recent studies have shown systematic sub-national and micro-level dynamics at work, pointing strongly to the need for careful analysis below the level of national states. These dynamics take two broad forms: rebel recruitment through lootable resources (individual level), and regional inequities and resource location (group/regional level). I address these in order below.

Rebel Recruitment and Lootable Resources

Any rebellion must recruit and retain fighters to be viable. A central question related to resources and recruitment is lootability—put simply the ease with which rebel organizations can seize and allocate the resources themselves. Weinstein (2007) points to coca cultivation in Colombia as one source of easily looted wealth. Secondary (the so-called “blood”) diamonds are another—because they require no logistically expansive mining, rebels who hold the territory in which they are located can make use of them. More recently, our awareness that ISIS has developed an expansive smuggling network for selling Syrian oil from territory it controls (through Turkey), along with Mexican cartel looting of oil in the northern parts of the country, suggest that we should devote further inquiry to the issue. This prospect could, like the primary versus secondary diamond bifurcation, create two tracks of argument about oil reserves and micro-level conflict dynamics.

At the level of rebel organizations, scholars have theorized that resource allocations shape both rebel groups themselves and the kinds of recruits who are attracted to the movement. Weinstein (2007) illustrates how groups with greater resource endowments tend to more effectively attract opportunistic recruits and to become more loot-driven themselves. Buhaug et al (2009) find that rebel movements originating in mineral-rich regions can sustain much more durable violent conflicts with the governments they challenge. Gates (2002, 115) models a dynamic in which “loot-seeking groups generally possess more resources than other types of rebel groups.” While this latter logic leans toward the circular, it fits into a broader line of inquiry seeking to uncover the extent to which rebel leaders benefit from having access to resources they can offer to potential recruits.

Regional Inequities in Resources

Since many countries at risk for civil war are also deeply divided along regional, ethnic or religious lines, the prospect for resources affecting those cleavages has also produced a growing research program. One line of argument suggests simply that rebels—ethnic or otherwise—located in resource-rich regions are more likely to rebel, and more likely to succeed when they do. Related to this, the development of the Minorities at Risk,[2] and then the Ethnic Power Relations projects enabled the analysis of ethnically charged conflicts at the group level. This made it possible to analyze center-seeking civil wars distinct from separatist ones, and also to explore the role that political exclusion plays in group mobilization against the state. These trends helped to push research forward by encouraging scholars to ask whether resource location too might be a promising direction, in essence allowing us to explore not whether a country was resource-rich, but where resources are produced across its territory. Hunziker and Cederman (2012) for example, find a strong difference in the conflict proclivities of ethnic minority regions based on a) whether or not they enjoy meaningful access to political authority and b) whether or not their regions are home to resource reserves. Sorens (2011) finds that while mineral riches in an ethnic minority region discourages center-seeking conflicts, it enhances the risk of separatist ones by providing a base for thinking about post-independence economic sustainability. This is in line with Ross's (2003) analysis of the independence narrative of the Free Aceh Movement in Indonesia, whose leaders looked to nearby Brunei as a model of oil-funded small country success. Oyefusi (2008) similarly finds a strong positive relationship between the size of the oil sector in Nigerian communities and the willingness of individuals to participate in rebellions.

It is reasonable to ask simply whether the large number of different measures used to capture the concept of "resource wealth" might be driving increasingly divergent results. The answer to that question is of course impossible to know unless we could convince all of these scholars to use the same indicators. But there are some important implications of the measure debate for future research. The first is that, while more challenging, national-level analysis based on cross-country data is not by any means a dead end. Instead we find scholars working diligently to craft measures that deal with endogeneity problems, constructing research designs that account for the uncertain but inevitably missing variables in explaining the onset of civil conflict, and in general taking careful steps to improve the quality of data and the reliability of results. It is worth noting that the current uncertainty in cross-country research is by no means unique in political science and political economy research. One could say the same about the development-democracy nexus, and as a result, I would caution against arguing for a shift away from national-level research simply because causal identification is challenging. Rather, as those who argue for a resource curse and those who argue against it continue to accumulate findings, it would be well worth trying to bridge the disparity of conclusions with explicit efforts to isolate a smaller number of measures of resource wealth. This if nothing else would allow for genuine knowledge accumulation around a consistent set of indicators and would make it possible to focus on the other differences of specification, design and analysis. Another strategy, one I detail more in the conclusion, is to pair cross-country aggregate data analysis with structured qualitative comparisons arguably better suited to teasing not just causality but the mechanisms underpinning them. Despite the clear accomplishments of the World Bank case study war project (Collier and Sambanis 2005), we have seen too little of this multi-method research.

None of this is to say that cross-country research, either econometric or small-N comparative historical, is dead or on the way out. Because resource revenues are overwhelmingly owned by national states,[3] and because states are the most frequent arbiters of who gets exploration and production contracts as well as the last line of responsibility, national governments will continue to play a central practical role in determining the future of the politics of resource wealth. Accordingly, problems with national-level data such as endogeneity or the likelihood of disparateness stemming from sub-national variation, are ones that we ought to tackle to improve, not ones we ought to use to justify ending, cross-country research. This is simply to say that we need to address squarely the data and theoretical problems that have challenged cross-national research in the past.

A very promising avenue of research, as I suggested with reference to cross-country studies, is the exploration of conditional relationships between sub-national resource wealth and conflict. For example, rather than simply asking whether oil-rich regions rebel more, Hunziker and Cederman (2012) ask whether oil-rich regions that have been excluded from political power are more likely to rebel than oil-rich regions that are included. Similarly, Østby, Nordås and Rød (2009) find that the presence of oil fields in ethnic regions only makes those regions more likely to rebel when they are relatively economically deprived compared to the national average. The slow but steady erosion of monotonic findings at the country level suggests strongly that as more scholars pursue research below the national level, more important conditional relationships are likely to emerge. Moreover, in the same way that conditional institutional quality-resource linkages led to "institutions, not resources" conclusions (see for example Brunnschweiler 2008; Menaldo 2014), and as we develop better ways of capturing sub-national political dynamics, we may well discover that they are similar to country-level ones. In short, while at this time scholars are finding strong sub-national relationships between resources and conflict, ten to fifteen years ago exactly the same thing would have been true about country-level relationships and the important point is that we are early in our empirical understanding of the sub-national dynamics.

Looking Forward

As the volume and quality of research on the relationship between resources and conflict has expanded, so too has the discord in conclusions. While acknowledging that there are multiple views on why this is the case, my sense is that it is normal in social science. “Civil wars” are big events and conceptually complicated ones. Measuring civil war itself is a debated topic and the standard threshold of 1,000 battlefield deaths raises questions about why 999 would be substantively different than 1,001. Notwithstanding that, there are a number of areas in which it seems most fruitful to encourage future research to push forward. In this concluding section, I outline five main priorities of focus: the direction of causality in the institutions-resources nexus, endogeneity concerns, measurement choice, levels of analysis and the promise of multi-method research. Two—the question of whether resource wealth is a product of, or a cause of, weak institutions and the issue of multi-method inquiry—could be called meta-theoretical and research design level issues, respectively. The other three are essentially concept and measure questions.

One conceptual area that stands out—both in terms of links to broader questions in comparative politics and political economy—as needing closer consideration is the relationship between resource wealth and state capacity (or institutional quality). Although Humphreys (2005) concluded that the weak states mechanism was more consistent with the empirics than others linking resources to conflict, it was inferred rather than directly explored. And, subsequent research has increasingly suggested three things. First, resource wealth does not appear to have any direct weakening effect on state capacity or on the quality of institutions. Ross (2012) in fact finds a small but significant strengthening effect of oil wealth on institutional quality, and Smith (2012) finds the same in a sample of Southeast Asian countries. In short, the net effect of resources often seems to enhance, not undercut, government performance.

Second, several recent studies have concluded that it is institutional quality that determines resource “wealth” rather than the other way around, via two processes. Brunnschweiler and Bulte (2009) found that countries with weaker institutions tend not to adopt economic policies that encourage diverse growth and development. As a result, the resource sector’s share of the total GDP increases, effectively making “resource wealth” endogenous to prior institutions. In line with this, Menaldo (2014) demonstrates that rulers in countries with weak institutions tend to turn to resource sector development to compensate for their inability to accomplish broader development. Hence, there are really two mechanisms at work, both of which plausibly boost the size of the resource sector. Moreover, the established effects of conflict in increasing a country’s resource dependence, plus the frequency of repeated conflicts in war-prone countries, suggest a further endogeneity effect. In a research program in which it is very often taken as a given that resources are granted a priori by nature—and by definition exogenous to the political and social worlds due to their natural occurrence—this new insight is among the most important in moving forward. If this is the case, the scholarly community ought to cease advising policy makers on how to combat the resource curse and instead focus on improving the quality of institutions and battling corruption.

Third, the exploration for and discovery of resource reserves is highly endogenous to politics and governance. Collier (2010) notes that in the developed world we estimate that 80 percent of actual reserves have already been discovered, with just 20 percent remaining. He notes further that the estimates are reversed for much of the developing world. The reason? Oil exploration firms facing limited asset mobility once “sunk” have been much more hesitant to commit to investing in unstable, poorly governed states than in stable, well-governed ones. The extent to which known reserves are thus a function of, rather than a cause of, state capacity provides yet a third compelling reason to think of resource wealth itself as an outcome to be explored, and as potentially a sub-outcome of state weakness alongside conflict. Thinking of it this way then would make the conflict-resources link seem less surprising. If it is the case that governments in command of weak states both tend to over-rely on resource sectors and fail in promoting economic diversification *and* to suffer more internal conflicts than others, scholars would do well to start conceptualizing resource dependence as a potential warning sign rather than strictly an independent variable.

Another line of promising future inquiry has to do with data. As data quality continue to increase at multiple levels, two major areas of potential scholarly gains appear most fruitful. One is the prospect of synthesizing national and sub-national research. Recognizing that this scope of inquiry is most likely to be book-length, or at least on the longer end of what journals in political science are generally willing to accept, it is the case that a growing consensus that, if there is a relationship between resources and the likelihood (or duration) of violent conflict, it is a conditional or non-linear one. This consensus appears to be emergent at both levels, and while I am sensitive to critiques of national-level data analysis for the reasons of difficulty in causal identification and in sorting out endogenous relationships, the substantive importance of continuing to explore dynamics at this level is simply too great to lose. However, to the extent that analysis of empirics at both levels is feasible—and I do not mean simply two levels of statistical data but a wider array of potential multi-method design options—we stand a much better change of nailing down robust conclusions (see for example Balcells and Justino 2014).

Another issue in need of attention as civil war scholars move forward on the resource angle is some consistency in measurement choice. There are three main clusters of indicators commonly employed as measures of resource wealth: resource abundance (resource income per capita, most commonly), resource dependence (resource income as a share of some measure of average income per capita), and a variety of efforts to instrument for resource wealth (known reserves, giant oil fields, proven reserves, etc.). Yet I catalogued more than one dozen separate measures including dummy variables for either OPEC membership or various thresholds of dependence (Smith forthcoming). Since it is the case in a number of recent studies that indicators capturing abundance and dependence either have opposite effects or varying ones, and since this broad concept of resource wealth has a number of dimensions, it makes sense that scholars ought to explore resource-conflict linkages using an array of measures. Given the wide availability and consistency of fuel income per capita, this indicator would seem the best for capturing abundance. I have argued elsewhere for employing rent leverage as the best measure of dependence. And while efforts to find instruments for oil wealth continue to be endogenous in some way to politics, non-income based measures ought to continue to play a role. As a result, best practices would argue for multiple measures, with one from each of the above mentioned three categories.

A final avenue for future research at the micro-level has to do with the dovetailing looting mechanisms in a host of case-driven studies. As conflict dynamics change so too might the balance of grievance to greed motives, affecting individuals over time but also the kind of individuals who join into conflicts at specific points in their duration. Aspinall’s in-depth analysis of joining dynamics during the Aceh conflict in Indonesia illustrates this point well. In the late 1990s and early 2000s, there took place a change in the most common motives of joiners. Immediately after the fall of the Suharto regime in Jakarta, the Free Aceh Movement (*Gerakan Aceh Merdeka*, or GAM) found itself with greatly expanded freedom to mobilize. As a result, in addition to political activists, GAM's ranks swelled with low-level criminals, for whom the payoffs of taking part at that moment far outweighed the costs. Beginning in 2001, however, with the declaration of a military emergency in the province, state coercion increased dramatically. The years following this change subsequently saw the defection from GAM of many if not most of the wave of opportunistic joiners.

Micro-focused research is most likely to continue to illuminate such dynamics as these. There has been relatively less ethnographic research on the resource link to conflict to compare to work such as that of Elizabeth Wood (2003), which holds much promise for sorting out just which motives appear most salient for individuals in deciding whether to participate in rebellions. And while we have seen valuable insights emerge from survey research (Humphreys and Weinstein 2008; Barron, Humphreys, Paler and Weinstein 2009), it is also likely that these more formalized, less ethnographically embedded research strategies may miss many of the honest and rich responses that more in-depth research might provide. This would argue for equal emphasis on the micro-qualitative side of civil war research. In short, a clearer focus on supporting the collection of quality ethnographic as well as quantitative data on the micro-dynamics of how resources shape conflict proneness could take us far in understanding the tough decisions that individuals make about whether or not to participate in rebellions.

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[1] Archived at http://www.correlatesofwar.org/ and http://www.pcr.uu.se/research/ucdp/datasets/, respectively.

[2] Including the Minorities at Risk data project here is not intended to minimize the selection bias problems that have been pointed out with it—i.e. sampling only groups that are discriminated against. Rather, I mention it here to note the real benefit for scholars of having access to publicly available group year level data.

[3] This is the case everywhere in the world other than in the United States.