



A CLOSER LOOK AT OIL, DIAMONDS, AND CIVIL WAR

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■ **Abstract** Studies of natural resource wealth and civil war have been hampered by measurement error, endogeneity, lack of robustness, and uncertainty about causal mechanisms. This paper develops new measures and new tests to address these problems. It has four main findings. First, the likelihood of civil war in countries that produce oil, gas, and diamonds rose sharply from the early 1970s to the late 1990s; so did the number of rebel groups that sold contraband to raise money. Second, exogenous measures of oil, gas, and diamond wealth are robustly correlated with the onset of civil war. Still, these correlations are based on a small number of cases, and the substantive effects of resource wealth are sensitive to certain assumptions. Third, petroleum and diamond production lead to civil wars through at least three different mechanisms. Finally, the only resource variable robustly linked to conflict duration is a measure of “contraband,” which includes gemstones, timber, and narcotics.

INTRODUCTION

Dozens of studies since 1998 have scrutinized the effects of natural resource wealth on conflict.¹ Many find the onset or duration of civil war linked to two commodities—petroleum and diamonds. Yet these studies have not been fully persuasive, for four reasons.

First, they typically use natural resource measures that are imprecise and rely on datasets marked by missing data and measurement error. Many use figures from the World Bank’s World Development Indicators (WDI), whose data on mineral exports are almost invariably misinterpreted. Others rely on dummy variables that offer crude distinctions between “oil exporters” or “diamond exporters” and all other states. Any inferences drawn from these data should be treated cautiously.

The second problem is that most studies use “natural resource” variables that may be endogenous to conflict. Scholars typically measure a country’s resource wealth by dividing its oil (or other mineral) exports by its gross domestic product

¹Most of those written before mid-2003 have been reviewed (Ross 2004a); this article focuses on more recent studies.

(GDP). This “resource exports to GDP” measure was originally developed by Sachs & Warner (1995) and later adopted by Collier & Hoeffler (1998) and many others—including, regrettably, me (Ross 2001a).

Unfortunately, this variable opens the door to two problems. The first is reverse causality: civil wars might cause resource dependence by reducing the size of a country’s nonresource (i.e., manufacturing) sector, leaving its resource sector—which is location-specific and cannot easily depart, and may be confined to secure enclaves—the major force in the economy by default. The second is spurious correlation: both civil war and resource dependence might be independently caused by an unmeasured third variable, such as poor property rights or the weak rule of law. A state where the rule of law is weak might be unable to attract investment in its manufacturing sector and hence would depend more heavily on resource exports; it might also face a heightened risk of civil war through a different process. The result could be a correlation between resource dependence and civil war, even though neither factor would cause the other.

The third problem has been robustness. Many econometric studies find that some measure of oil or diamond wealth can be tied to the onset or duration of civil war (Collier & Hoeffler 1998, 2004; de Soysa 2002; Hegre, unpublished manuscript; Reynal-Querol 2002; Buhaug et al., unpublished manuscript; Fearon & Laitin 2003; Fearon 2004; Lujala, unpublished 2002 manuscript; de Soysa & Neumayer, unpublished manuscript; Humphreys 2005; Lujala et al. 2005), but others have found these correlations to be weak or nonexistent (Elbadawi & Sambanis 2002, Smith 2004, Regan & Norton 2005).

Two recent studies underscore the robustness problem. Hegre & Sambanis (unpublished manuscript) examine 88 proposed correlates of civil war onset to see which are robust to changes in the specification of the civil war model. Neither of their “resource” measures (oil exports as a fraction of GDP, fuel exports as a fraction of merchandise exports) passes the test, although the oil-exports-to-GDP variable is “marginally robust.” Sambanis (2004b) looks at whether a series of potential civil war correlates, including a dummy variable for major oil exporters, is robust to alternative definitions of civil war. He finds that the oil-exporter dummy is significantly linked to civil war onset in about one third of the estimations and is generally uncorrelated with civil war duration.

The fourth problem is the failure to determine the causal mechanisms that link mineral wealth to war. Different scholars offer different theories: mineral wealth could foster conflict by funding rebel groups (Collier & Hoeffler 2004), weakening state institutions (Fearon & Laitin 2003, Snyder & Bhavnani 2005), making the state a more attractive target for rebels (Fearon & Laitin 2003), facilitating trade shocks (Humphreys 2005), making separatism financially attractive in resource-rich regions (Le Billon 2005a, Collier & Hoeffler 2005), or through other processes (Ross 2004b, Humphreys 2005). But due in part to the aforementioned problems—measurement, endogeneity, and robustness—and due in part to a shortage of data, we have not been able to tell which mechanism (or mechanisms) is correct. Because each causal mechanism implies a different set of policy interventions, getting the mechanism right is critical.

This paper seeks to put the natural resource–civil wars literature on a more solid footing by addressing these four problems. The first section below describes the temporal pattern of civil wars in petroleum-rich and diamond-rich states between 1960 and 2002. A second section reviews recent trends in the study of natural resources and civil war. A third section constructs more accurate and exogenous measures of oil, diamond, and other mineral wealth, building on major advances by Hamilton & Clemens (1999), Gilmore et al. (2005), Humphreys (2005), and Lujala et al. (unpublished manuscript). A fourth section explains the models I use to explore the links between these variables and the onset of civil war, the duration of civil war, and the causal mechanisms behind these relationships. A fifth section presents the results of these tests, and the final section summarizes the key findings and highlights topics for further research.

The paper has four main findings. First, the likelihood of civil wars in countries that produce oil, gas, and diamonds rose sharply from the early 1970s to the late 1990s. So did the number of conflicts in which insurgents raised funds by selling contraband resources. Second, exogenous measures of oil, gas, and diamond wealth are correlated with the onset of civil war, and these correlations are robust along several dimensions. But several cautions are warranted: these correlations are based on a small number of civil wars—29 in petroleum-rich states, and 12 in diamond-rich states—which should make us cautious in our inferences. And the effect that natural resources have on a country’s conflict risk depends on how the revenues affect the rest of the economy: If new oil or diamond wealth is productively invested and leads to a substantial rise in GDP, the benefits of a higher income can offset the detriments of resource extraction.

Third, petroleum and diamond production increases a state’s civil war risk through at least two mechanisms: by fostering insurgencies in resource-rich regions, and through a process linked to trade shocks. But these two mechanisms do not seem to fully account for the resource–conflict relationship—indicating that one or more additional mechanisms are also valid. Finally, the only resource variable robustly linked to conflict duration is Fearon’s (2005) measure of contraband, which includes gemstones, timber, and narcotics. I argue, however, that it is not yet clear that contraband funding actually causes longer conflicts.

TEMPORAL PATTERNS

Between 1960 and 2002, there was a steady rise in the number of conflicts, and the risk of conflict, in petroleum-rich and diamond-rich countries. I classify countries as “petroleum-rich” if they produce at least \$100 per person (in constant 2000 dollars) in rents from oil, gas, or coal. In 1999, there were 28 states that crossed this threshold, ranging from Uzbekistan (\$111 per capita) to Kuwait (\$7422 per capita).

As Figure 1 suggests, the wars of the petroleum-rich countries can be divided into two periods: 1960–1973, when they occurred at a rate of slightly below one per year, and 1974–2002, when their rate was ~ 4.9 per year.

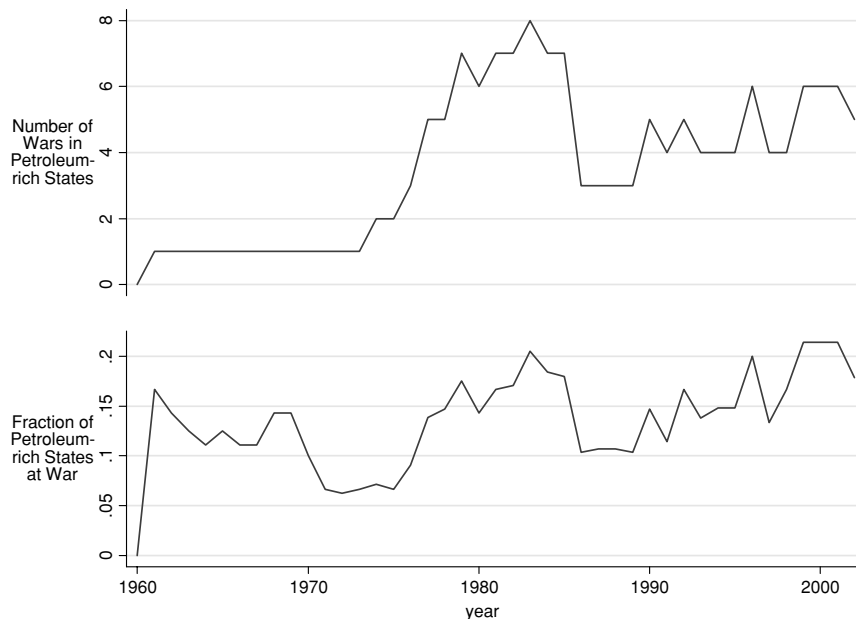


Figure 1 Civil wars in states producing at least \$100 per capita in petroleum rents (constant 2000 dollars), 1960–2002.

This jump in the number of wars after 1973 was caused by two factors. One was a rise in the number of petroleum-rich states, due to both rising prices and the geographical spread of petroleum extraction. In 1973, there were 15 petroleum-rich states; by 1980, there were 42. The second and more worrisome reason, however, was an increase in the civil war rate among the petroleum-rich countries, which rose from 0.067 in 1971–1975 to 0.18 in 1981–1985 as new wars broke out in Angola, Indonesia, Iran, Peru, and South Africa. After dropping between 1985 and 1995, the annual civil war rate rose to .184 from 1995 to 2002.

Figure 2 show the temporal pattern of wars in states that produced at least \$1 per capita of diamonds.² There is a striking increase in these conflicts from the mid-1960s to the mid-1990s. This does not result from an increase in the number of diamond producers, but rather from an increase in the rate at which diamond producers were engaged in civil wars—which rose from 0.0625 (1 out of 16) in

²To capture the 12 wars that broke out from 1960 to 1999 in states that produced nontrivial amounts of diamonds, I must use the \$1-per-capita threshold. Because of the worldwide collapse in diamond prices since the 1960s, the number of states producing \$100 worth of diamonds per capita dropped from 12 in 1966 to just two in 1999. The \$1-per-capita threshold produces a fairly even number of diamond-producing countries (12 to 18) between 1961 and 1999.



Figure 2 Civil wars in states producing at least \$1 per capita (in constant 2000 dollars) in diamonds, 1960–2002.

1982 to 0.3125 (5 out of 16) in 1999. This implies that diamond production became more closely linked to conflict over time. If the sample is divided into two periods, diamonds are correlated with conflict onset after the Cold War (1986–1999) but not during the Cold War (1960–1985).

Figure 3 compares the pattern of civil wars in three categories of states: those that produced at least \$1 per capita of diamonds, those that produced at least \$100 per capita in fuel rents, and those that produced neither. The pattern of wars in the nonfuel/nondiamond states follows the path noted by other scholars—rising monotonically from 1960 to 1992 and falling sharply thereafter. The diamond-producing states followed a similar trend between about 1970 and 1992, but their conflicts peaked several years later and did not fall off so sharply. By contrast, the civil war rate among petroleum-rich states shows no obvious long-term trend, although the rate seems to increase slightly over time.

Figure 4 displays the number of ongoing conflicts in which rebel groups use contraband funding—a category that includes gemstones, timber, and narcotics.³ During the Cold War, the number of ongoing contraband conflicts rose from two to six. After 1988, it jumped to nine, and remained at nine or ten through the end of

³I have taken Fearon's (2004) coding of 17 conflicts with contraband funding and used Ross (2004a) to identify the years when this funding began.

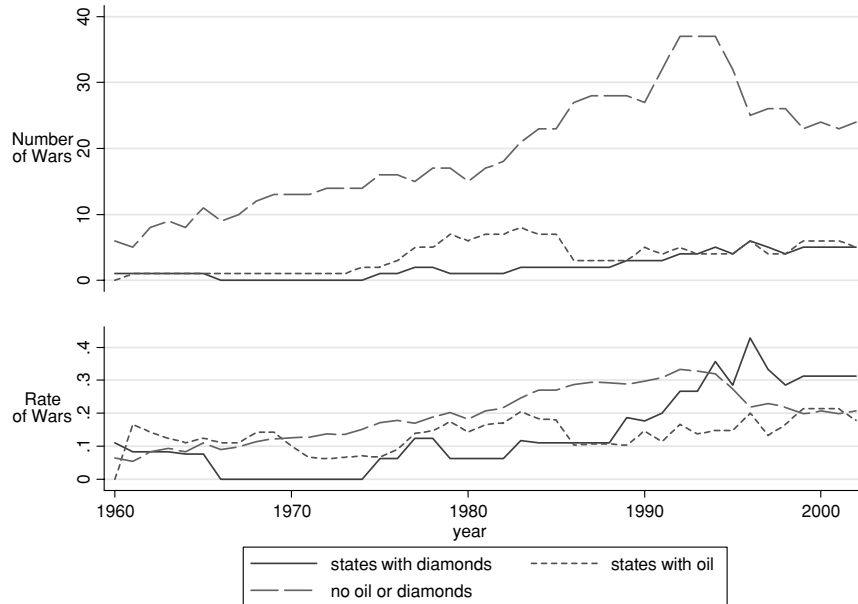


Figure 3 Civil wars in states with petroleum rents (>\$100 per capita), diamond production (>\$1 per capita), and neither petroleum nor diamonds, 1960–2002.

the millennium. While the overall number of civil wars fell after 1992, the fraction with contraband funding rose from 0.19 in 1988 to 0.32 by 1999.

The rise in the number of contraband wars had three causes. First, as Fearon (2004) points out, contraband conflicts tend to last an unusually long time, and they seem to begin more frequently than they end, so they accumulate over time. Second, existing rebel groups shifted toward contraband funding, particularly at two points: in the mid-1980s, when insurgents in Colombia and Peru began to take advantage of the narcotics trade; and at the beginning of the 1990s, when the end of the Cold War forced rebels in Angola and Cambodia to turn to gemstones (and in the case of Cambodia, timber) to replace their foreign funding. Third, contraband became a more common way to finance new conflicts once the Cold War had ended. Contraband helped fund seven of the 92 civil wars (7.6%) that began between 1945 and 1988, but eight of the 36 wars (25%) that began after 1988.

Some argue that the widespread belief in the late 1990s that natural resources were significant causes of civil war was merely the result of a selection bias: People were observing wars in a small number of resource-rich states but ignoring the absence of war in a larger number of resource-rich states. These data argue against that view. When the number of wars in the nonfuel/nondiamond states dropped after 1992, wars in the fuel- and diamond-rich states became a growing fraction of the world's civil wars. After the Cold War's end, insurgent groups turned toward contraband funding, which included the sale of gemstones, alluvial

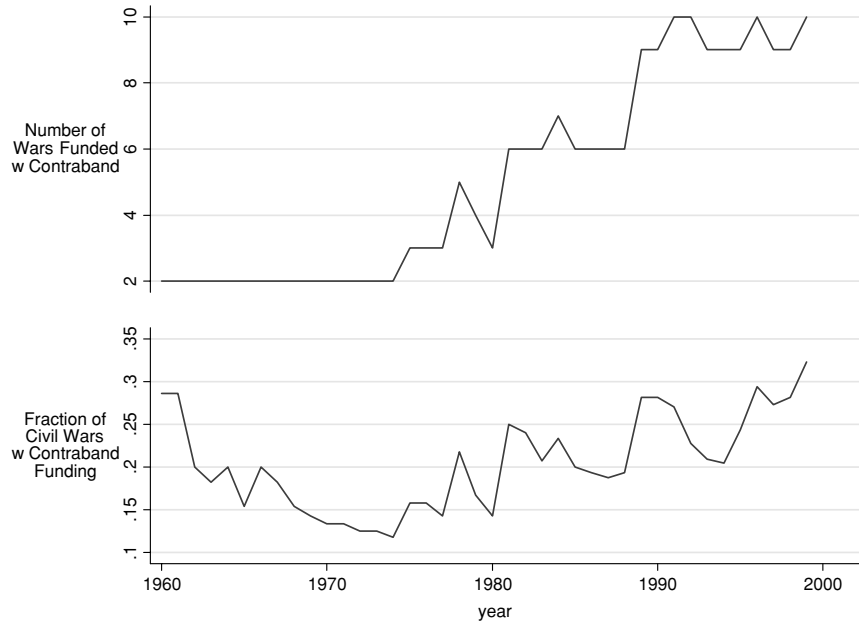


Figure 4 Civil wars funded by the sale of contraband (gemstones, minerals, timber, narcotics), 1960–2002.

minerals, timber, and narcotics. The heightened attention to natural resources and conflict, beginning in the late 1990s, reflected these trends.

TRENDS IN THE NATURAL RESOURCE–CIVIL WAR LITERATURE

The study of natural resources and civil war is part of the broader literature on civil wars and is hence influenced by many of its trends. These include the following:

- a convergence of economic and political science approaches to conflict (Collier & Hoeffler 1998, 2004; Sandler 2000; Ballentine & Sherman 2003; Sandler & Hartley 2003), plus a smaller backlash against these approaches (Arnson & Zartman 2005);
- efforts to bring together cross-national quantitative work with case studies (Laitin, unpublished manuscript; Sambanis 2004a; Collier & Sambanis 2005);
- a special concern for Africa, where a rising fraction of the world's civil wars are found (Elbadawi & Sambanis 2000, Herbst 2000, Azam 2001, Collier & Hoeffler 2002, Leonard & Strauss 2003)

- the development of better civil war datasets (Gleditsch et al. 2002, Fearon & Laitin 2003, Sambanis 2004b, Raleigh & Hegre 2005);
- convergence toward a “standard model” of civil war onset, based on the model introduced by Fearon & Laitin (2003);
- efforts to test the robustness of civil war models (Sambanis 2004b; Hegre & Sambanis, unpublished manuscript)
- a growing interest in postwar settlements and reconstruction (Doyle & Sambanis 2000, 2006; Stedman et al. 2002; Walter 2002);
- efforts to make models more predictive and relevant to policy makers (King & Zeng 2001a; Mack 2002; Ward & Bakke, unpublished manuscript; Ballentine & Nitzschke 2005).

Most of these trends are influencing the study of natural resources and civil war. Below I discuss three trends that have special importance: the improvement of natural resource measures, the use of geographical data, and the accumulation of more country case studies.

Better Natural Resources Data

In the past, natural resource wealth has been poorly measured. Some earlier studies—most importantly, the seminal Collier-Hoeffler (1998) work—used “primary commodity exports” (usually divided by GDP) to measure the impact of natural resource wealth on conflict. Several scholars have pointed out the drawbacks of relying on “primary commodity exports” as a way to measure the influence of natural resources: It lumps together a wide range of goods, including some that may influence conflict and others that may not; it omits data on diamonds and other gemstones, even though these appear to be salient; it focuses on exports, even though production might be a better measure of the availability of these resources; and it includes commodities that were first imported and then re-exported (Lujala, unpublished 2004 manuscript; Fearon 2005).

Other studies have measured specific commodities (usually oil) using a dummy variable that represents states that have crossed a certain export threshold (Fearon & Laitin 2003, Sambanis 2004b). This approach also has drawbacks, since it employs a dichotomous variable to measure a continuous phenomenon and relies on an arbitrary threshold to trigger the zero-to-one change.

Many studies have relied on data on fuel exports and nonfuel mineral exports from the WDI, but these data contain hundreds of observations that are misleading, or at least easily misinterpreted. For example, they identify 127 countries where fuel exports constituted at least 1% of merchandise exports for one or more years from 1965 to 1997. Yet according to Hamilton & Clemens (1999), only 85 states actually produced petroleum domestically during this period, and according to the annual reports of the U.S. Geological Survey (USGS), only 39 of them exported it. The rest either exported petroleum products that were made exclusively from imported oil, exported fuel products other than petroleum (such as coal and peat),

or were transshipment points for fuel products from other countries. A substantial amount of the WDI data on fuel exports is also missing—including data for oil-rich states such as Angola, Congo-Brazzaville, Equatorial Guinea, Gabon, Iran, Iraq, Libya, Nigeria, Qatar, Saudi Arabia, and the United Arab Emirates.

Fortunately, scholars have developed far better measures of both fuel and non-fuel mineral wealth. Humphreys (2005) has compiled data on the volume of oil production and oil reserves between 1960 and 1999; Lujala et al. (unpublished manuscript) have assembled data on the geographical location, date of discovery, and date of first production of all oil and gas fields in 119 countries; and Hamilton & Clemens (1999) calculated the rents generated by the production of oil and a wide range of other minerals for most countries between 1970 and 1999. The Hamilton & Clemens dataset is especially noteworthy; many scholars hypothesize that mineral resources lead to economic and political problems because they generate rents, and these data are enabling scholars to test their claims with greater precision (Stijns, unpublished manuscript; de Soysa & Neumayer 2005; Collier & Hoeffler, unpublished manuscript).

Studies that employ these new datasets tend to support the claim that a country's oil exports are correlated with its civil war risk. Humphreys (2005) shows that a country's oil production per capita is positively linked to its conflict risk; de Soysa & Neumayer (unpublished manuscript) report that oil and other fuel rents are linked to some conflict measures but not others; and Lujala (unpublished 2004 manuscript) finds evidence that onshore oil production is linked to civil war but offshore production has no impact.

Some scholars have also created datasets on the production of diamonds, covering the volume of diamond production (Humphreys 2005), the value of diamond production (Olsson, unpublished manuscript), and the location, type, date of discovery, and date of first production (Gilmore et al. 2005). Lujala (unpublished 2002 manuscript) has also produced a dataset on other types of gemstones.

These studies have backed the claim that diamonds and conflict are connected, although they differ on important details. Humphreys (2005) finds that the volume of diamond production (measured per capita) is positively associated with the likelihood of civil war onset—both within Africa and more generally. Surprisingly, Humphreys also shows that diamond wealth tends to produce shorter wars, by facilitating military victories by one side or the other.

Lujala et al. (2005), using the Gilmore et al. (2005) diamond database, explore the impact of two types of diamonds (“primary” diamonds, which are extracted from deep-shaft mines and are generally controlled by large firms and governments, and “secondary” diamonds, which are near the surface and are commonly mined by small teams of unskilled workers) on two types of conflict (ethnic and nonethnic conflict). They find (a) that the production of diamonds, indicated by a dummy variable, has little effect on nonethnic conflict but a pronounced effect on ethnic conflict; and (b) that primary diamonds seem to reduce both the prevalence and likelihood of ethnic wars whereas secondary diamonds increase the prevalence and likelihood of ethnic wars.

Employing Geographical Data

A second trend is the growing use of geographical data to explore more finely graded links between natural resources and conflict. A handful of international relations scholars have already drawn on Geographical Information Systems (GIS) data to investigate such topics as international integration and the transnational spread of conflict and democracy (Gleditsch 2002; Murdoch & Sandler 2002). But the use of GIS data has special importance in the study of natural resources and civil wars—both of which tend to be spatially clustered within countries, and hence, amenable to subnational geographical analysis. Scholars associated with the Center for the Study of Civil War at the Peace Research Institute Oslo (PRIO) have taken the lead in developing these new datasets, which cover the location of diamonds (Gilmore et al. 2005), other gemstones (Lujala, unpublished 2002 manuscript), and oil and gas deposits (Lujala et al., unpublished manuscript).

Several studies have already combined these data with information on the location of “conflict zones” in countries with civil wars (Raleigh & Hegre 2005) to produce important new findings on the natural resources–civil war correlation. Buhaug & Gates (2002) show that the presence of mineral deposits tends to expand a conflict zone; Buhaug et al. (unpublished manuscript) find that the presence of “lootable” natural resources (including alluvial diamonds, other gemstones, and alluvial gold) inside a conflict zone tends to produce longer conflicts; Lujala et al. (unpublished manuscript) show that the presence of oil and gas in a conflict zone tends to prolong wars for control of the government; and Buhaug & Lujala (2005) demonstrate that when we move from country-level to conflict-level data, the relationship between gemstones (including diamonds) and conflict duration becomes both substantively and statistically more significant.

These findings are broadly consistent with others. Fearon (2004) reports that the presence of contraband resources (including gemstones and narcotics) is associated with conflict duration. Lujala et al. (2005) show that secondary diamonds are linked to longer conflicts. Ross (2004b) suggests that alluvial gemstones have lengthened recent conflicts in Afghanistan, Angola, Congo-Kinshasa, Liberia, and Sierra Leone by providing rebel groups with funding.

Case Studies

A third trend is the production of a large number of country case studies that explore the resource–conflict relationship. Cross-national regressions can tease out correlations among variables, but these variables are often poorly measured and only hint at the underlying processes that produce civil war. Recent case studies have brought us much closer to the causal dynamics of the natural resources–civil war relationship.

Many of the country case studies are in edited volumes (Peluso & Watts 2001, Ballentine & Sherman 2003, Pugh & Cooper 2004, Arnson & Zartman 2005, Collier & Sambanis 2005, Le Billon 2005b); dozens of others are in articles, working papers, dissertations, and book chapters. These studies vary widely in

their motivation. Some authors are interested in commenting on, modifying, or refuting the claims of the initial Collier-Hoeffler model (Samset 2002, Collier & Sambanis 2005, Pearce 2005); some develop theoretical frameworks of their own (Peluso & Watts 2001, Lowi 2004). Some focus on the relationships between mining companies and local peoples (Bury & Kolff 2003, Frynas 2003, Gore & Pratten 2003). Most rely on qualitative data, although several use survey or other types of quantitative data (Angrist & Kugler 2005; Humphreys & Weinstein, unpublished manuscript) or employ formal models (Olsson & Fors 2004).

Part of the value of these studies is the attention they give to underexplored dimensions of conflict. Weinstein (2006), for example, links a country's natural resource base to the intensity of a civil war's violence. Based on an analysis of conflicts in Peru, Mozambique, and Uganda, he argues that when countries have fewer lootable resources, they are more likely to have well-disciplined rebel groups that use violence strategically. Countries that have more lootable resources (including narcotics) are more likely to have opportunistic rebel groups that use violence indiscriminately.

Virtually all of these case studies affirm that natural resource wealth is connected to violent conflict at the country level—sometimes as a source of finance, other times as a source of grievance. Often they suggest alternative causal mechanisms and complex interactions between resources, inequality, ethnicity, grievances, violence, and the actions of governments and extractive firms. They also tend to stress the importance of historical and sociological processes that are specific to the country or region. Collectively they imply that some version of the natural resource–civil war link is valid within certain countries, even if they cannot tell us whether it is valid cross-nationally.

MEASURING MINERAL WEALTH

Despite recent advances, statistical studies of the resource-conflict issue have suffered from the use of natural resource measures that are both imprecise and endogenous to civil war. In this section, I develop more precise and exogenous measures of mineral wealth, which in a later section I test with a variety of civil war measures.

Mitigating Endogeneity

The most commonly used measure of resource wealth—resource exports as a fraction of GDP—may be endogenous to conflict. There is good reason to think that conflict, or the anticipation of conflict, affects a country's manufacturing sector more than its resource sector. Manufacturing plants are relatively easy to move from one country to another, whereas mining operations are not. Industrial facilities also tend to be located near population centers and are more susceptible to disruption; extractive industries often function in enclaves or remote regions, making them easier to secure. If this is true, then the anticipation of conflict should

reduce a country's GDP (the denominator) more than its resource exports (the numerator)—thus producing a higher resource-exports-to-GDP ratio.

Some studies have tried to avoid this problem by instead relying on dummy variables to indicate the presence or absence of a given resource sector (e.g., Fearon 2004, Lujala et al. 2005, Regan & Norton 2005). But the use of dummy variables carries a high cost, since these measures contain no information about the value of the resources produced.

An alternative solution—first employed by Humphreys (2005)—is to replace the “resources-to-GDP” measures with “resources-per-capita” measures. There is no good rationale for measuring natural resource production (or exports) as a fraction of GDP. A high resource-to-GDP ratio may indicate great mineral wealth, but it can also indicate a weak nonmineral economy, which could be caused by a civil war itself or by an omitted variable that is correlated with civil war. If extracting and selling mineral wealth makes civil war more likely—the claim that some scholars in this subfield advance—it should be apparent when we measure the value of resource production per capita. If natural resource wealth is only harmful when other conditions prevail—such as a weak nonresource economy, or low per capita incomes—these interactions should be modeled explicitly instead of smuggled into an omnibus resource variable.

Because the resources-per-capita measure is largely unaffected by activity in the nonmineral economy, it can help mitigate the endogeneity problem. It may still be biased in more subtle ways, although the direction of bias is unclear. Poor countries might be more likely to exploit their natural resources because they have low labor costs, or place a lower value on environmental protection; this could create a false positive correlation between resources-per-capita and civil war. Alternatively, poor countries might be less likely to extract natural resources if they lack the requisite capital and infrastructure or suffer from low-quality government; this would create a false negative correlation between resources-per-capita and civil war.

Fuel and Nonfuel Mineral Rents

To measure fuel and nonfuel mineral rents, I use the data produced originally by Hamilton & Clemens (1999), which is now updated annually by the World Bank. Hamilton & Clemens gathered data on the global price of 14 types of minerals (oil, gas, hard coal, lignite, bauxite, copper, iron, lead, nickel, phosphate, tin, zinc, gold, and silver) and subtracted the extraction cost per unit for each producing country. This calculation yields an estimate of the country-specific rents created by each unit extracted. They then multiplied this figure by the quantity of minerals that each country extracts, to produce an estimate of the country's annual mineral rents. Their data now cover all countries from 1970 to 2002.

The Hamilton & Clemens data are not without problems. They ignore country-to-country variations in the quality—and hence, the value—of the minerals produced; extraction costs are based on estimates for a single year and are assumed to be constant (except for inflation) over time; when no data on extraction costs

are available for a particular country, they use extraction costs for a neighboring country; and the entries for nine countries are incorrect, according to the exhaustive reports of the USGS. Nevertheless, the Hamilton & Clemens data are remarkably complete and open the door to more careful tests of the resource-conflict puzzle.

I divide these rent data into two categories: rents from fuel minerals (oil, gas, hard coal, and lignite) and rents from nonfuel minerals (all other). I also extend the data for oil rents back to 1960, taking oil production figures for 1960–1969 from Humphreys (2005), data on oil prices from the World Bank, and data on extraction costs from Hamilton & Clemens. Finally, I cross-check the Hamilton & Clemens production data with the annual country reports of the USGS and correct entries for nine countries.

I then divide the resulting figures for fuel rents and nonfuel rents by population, to produce the variables *Fuel rents per capita* and *Nonfuel rents per capita*. To explore the differences between onshore and offshore oil, I draw on the PETRODATA dataset (Lujala et al., unpublished manuscript), which has dummy variables indicating whether a country produces onshore petroleum and whether it produces offshore petroleum. I code an additional eight petroleum-producing countries that are absent from the PETRODATA dataset in the same fashion, using reports from the USGS and the US Energy Information Agency. My final dataset shows that between 1960 and 2002 104 countries produced oil, gas, or coal onshore, and 55 produced oil or gas offshore. Forty-six countries produced both. I then interact the *offshore* and *onshore* dummy variables with *Fuel rents per capita* to produce two new variables: *Fuel onshore per capita* and *Fuel offshore per capita*. In countries with both onshore and offshore production, there are no available data on how much petroleum is produced through each route; consequently, all rents are attributed to both onshore and offshore production.

Diamonds

To measure diamond production, I begin with Humphreys' (2005) dataset on the annual quantity of diamonds produced, by country, since 1960. I supplement this with diamond production data from the USGS for five additional diamond producers not covered by Humphreys (Burma, Gabon, India, Indonesia, and Swaziland). The final dataset shows that 28 countries produced natural diamonds for one or more years between 1960 and 2001.

If all of these countries produced diamonds of equal value, then the quantity of production would be closely correlated with the value of production. Unfortunately, the price of diamonds per carat varies by a factor of more than eight, ranging from the industrial diamonds of Ghana (\$25 a carat in 2001) to the high-quality gemstones of Namibia (\$215 per carat in 2001). The quantity of diamonds produced is only a rough indicator for the value of diamonds produced.

To calculate the value of diamond production, I estimate the price per carat of diamonds (in constant dollars) for each country and year, beginning with the country-specific international diamond prices for 2001 reported in trade journals

and extrapolating backward using a historical index of diamond prices. I multiply the quantity of production by the country-specific diamond price to estimate the total value of diamond production for every country-year. I divide this figure by the country's population to create *Diamond production per capita*.

Many studies suggest that primary diamonds, which are mined from kimberlite shafts through a capital-intensive process, tend to have different effects on conflict than secondary diamonds, which are scattered over alluvial plains and can be extracted by small teams of artisanal miners (Le Billon 2001, Ross 2003, Lujala et al. 2005). The "conflict diamond" dataset (Lujala et al. 2005) includes dummy variables that indicate whether or not a country produces primary diamonds and whether or not it produces secondary diamonds.⁴ I interact these dummy variables with my *Diamond production per capita* variable to generate two additional variables, *Primary diamonds per capita* and *Secondary diamonds per capita*.

MODELS OF RESOURCES AND CIVIL WAR

The tests I describe below are based on those in earlier quantitative studies of civil war (Fearon & Laitin 2003; Fearon 2004, 2005; de Soysa & Neumayer, unpublished manuscript; Humphreys 2005; Lujala et al. 2005). They are distinct in four ways: The independent variables of interest are the new resource measures described above; I extend several datasets to cover longer periods; I carry out more robustness tests; and I carry out several new tests to help distinguish among causal mechanisms.

Dependent Variables

There are three carefully coded datasets on the incidence of civil war: the Fearon-Laitin dataset, the Sambanis dataset, and the PRIO/Uppsala dataset. I test my resource variables with each.

Each dataset divides civil wars into subcategories. Fearon & Laitin (2003) categorize civil wars in two ways: They distinguish between wars for regional independence and wars for control of the central government, and they distinguish between ethnic and nonethnic wars. I look at the impact of resource wealth on each type of conflict.

Sambanis (2004b) uses two methods of coding civil wars. Version A is coded 1 in the year a war begins, and all subsequent observations are dropped for the country until the war ends; if a second civil war begins before the first has ended, it is not recorded. Version B includes observations for all country-years and codes

⁴This dataset indicates that natural diamonds were produced in Mali, Thailand, and the United States between 1960 and 2000. I could find no evidence to support this in the USGS Minerals Yearbook or the *Mining Annual Review* produced by the *Mining Journal*. According to the USGS, the United States produces only synthetic diamonds and salvaged or recycled diamonds from jewelry or industrial equipment.

every civil war onset, even if the country is already experiencing a separate civil war. I test the resource variables with both versions.

The PRIO/Uppsala dataset (Gleditsch et al. 2002) classifies conflicts by size, ranging from minor conflicts that generate as few as 25 battle-related deaths per year to major conflicts that produce at least 1000 deaths per year. I look at the impact of mineral resources on all conflicts collectively (including minor, intermediate, and major) and on major conflicts only.

In all, I test the impact of my resource variables on nine different measures of civil wars: five from the Fearon-Laitin data, two from the Sambanis data, and two from the PRIO/Uppsala data. Because my resource data are available only back to 1960 (or 1970 for *Nonfuel rents per capita*), I use only the post-1960 data from each source.

Control Variables

Following Fearon & Laitin (2003), I use ten control variables in my model: GDP per capita, the log of population, the fraction of a country covered by mountainous terrain, ethnic fractionalization, religious fractionalization, democracy, and dummy variables for states with noncontiguous territories, states that are newly independent, states that have recently experienced major changes in regime type, and states that had civil wars in the previous year.

Method

When a dependent variable is dichotomous, it is normally appropriate to use a logit or probit estimator; but if the dependent variable measures the occurrence of a “rare event,” standard logit or probit estimators produce biased coefficients. Because civil wars are rare events—between 1960 and 1999 the Fearon-Laitin dataset identifies just 90 civil war onsets in 5436 country-years—I use the “rare events logit” estimator developed by King & Zeng (2001b), employing software written by Tomz et al. (1999).

Another concern is serial correlation. Previously, scholars have used two strategies to correct this. Fearon & Laitin (2003) take the year of civil war onset as their dependent variable and code all subsequent war years as zeroes; they then control for the presence of war in the prior year. Beck et al. (1998) recommend controlling for the number of years since the end of the last civil war and introducing a set of cubic splines. I try both methods and find the results are virtually identical.

Robustness

I test the robustness of the models in five ways:

- To evaluate whether any results are sensitive to the way that civil wars are defined and coded, I test my variables on nine different measures of civil war, which are drawn from three datasets.

- To make sure my findings are not sensitive to the choice of estimation technique, I use both the Fearon-Laitin approach of controlling for prior wars and the Beck et al. method of including a variable for peace years and cubic splines.
- To see whether natural resource wealth is masking regional effects, I add a series of regional dummies.
- To see if the model is robust to alternative model specifications, I drop each of the control variables one at a time.
- To determine the models' sensitivity to influential observations, I rerun them after dropping the most influential countries from the dataset.

To simplify the presentation of results, I display only the models that use the Beck et al. correction and summarize the robustness checks in the text.

Causal Mechanisms

Most scholars claim that natural resource production is linked to the onset of civil war through one of the five mechanisms described below. [Humphreys (2005) discusses several other possible mechanisms.] In the first two hypotheses, resource wealth influences rebels' motivations, giving them incentives to begin either a national civil war or a war of secession. In the remaining three, resource wealth enhances rebels' opportunities by providing financing or weakening the state.

RESOURCE WEALTH ENCOURAGES NATIONAL CONFLICTS BY INCREASING THE VALUE OF THE STATE AS A TARGET Because the production of minerals—particularly oil—tends to swell the state's coffers, governments in resource-rich countries may provide more attractive targets than governments in resource-poor countries (Englebert & Ron 2004, Fearon 2005, Le Billon 2005a). This hypothesis applies only to national civil wars, in which the rebels aim to capture the state. If insurgents wish to establish their own state, they should be indifferent to the size of the government's resource revenues.

This mechanism implies that the natural resource variables that generate substantial state revenues—*Fuel rents onshore*, *Fuel rents offshore*, *Nonfuel rents*, and *Primary diamonds*—should be correlated with national civil wars.

RESOURCE WEALTH INCREASES THE VALUE OF SOVEREIGNTY IN MINERAL-RICH REGIONS Resource wealth may help motivate separatist movements by increasing the perceived benefits—or reducing the apparent costs—of sovereignty in resource-rich regions (Collier & Hoeffler 2005).

If this is true, we should observe a correlation between separatist civil wars and *Fuel rents onshore*, *Nonfuel rents*, and *Primary diamonds*. Because offshore oil and gas are less likely to be claimed by secessionist movements, we would not expect a relationship between *Fuel rents offshore* and separatist civil wars. Several

scholars also imply that *Secondary diamonds* is unlikely to be associated with separatist conflicts (Le Billon 2001, Ross 2003, Collier & Hoeffler 2005).

RESOURCE WEALTH HELPS FUND REBEL ORGANIZATIONS Collier & Hoeffler (1998, 2004) note that natural resources (along with agricultural commodities) are easy targets for rebel predation, since unlike industry, natural resources produce rents and cannot be easily relocated. According to these authors, incipient rebel organizations engage in resource predation to fund the start-up costs of their insurgency. This process could apply to all types of commodities; even if insurgents cannot extract or market them by themselves, they can extort money from those who can.

This argument implies that the resources to which rebel groups can gain access—those labeled *Fuel rents onshore*, *Nonfuel rents*, *Primary diamonds* and (in particular) *Secondary diamonds*—should be correlated with all types of conflict. Because offshore oil is far more difficult for rebel groups to exploit, we would not expect a link between *Fuel rents offshore* and conflict.

RESOURCE WEALTH CAUSES CONFLICT BY WEAKENING THE STATE Middle East scholars have long suggested states that rely on nontax revenues are too weak to manage the economy and resolve social conflicts (Mahdavy 1970, Beblawi 1987, Crystal 1990). Fearon & Laitin (2003, p. 81) modify this line of reasoning and speculate, “Oil producers tend to have weaker state apparatuses than one would expect given their level of income because rulers have less need for a socially intrusive and elaborate bureaucratic system to raise revenues.”⁵ Others have argued that secondary diamonds (Snyder & Bhavnani 2005) and narcotics (Gates & Letzkian 2004) have similar effects: Their production weakens the state, which increases the risk of civil war.

If they are correct, then *Fuel rents onshore*, *Fuel rents offshore*, *Nonfuel rents*, and *Primary diamonds* should all be linked to all types of conflict, since they all generate large government revenues. If Snyder & Bhavnani are correct, *Secondary diamonds* should be associated with all types of conflict.

⁵Fearon (2005) attempts to test this idea by exploring the statistical relationship between fuel exports (measured as a fraction of GDP) and a measure of “government observance of contracts” derived from investor surveys. After controlling for income, he shows, states with more fuel exports are also more likely to repudiate contracts with investors. It is not clear, however, that contract repudiation is a good indicator of state strength, or more importantly, of the state’s ability to deter civil war. Moreover, there are other ways to explain the correlation between oil exports and contract repudiation. Multinational petroleum firms are more likely to have their government contracts repudiated than are other multinational firms, since they are less able to transfer their operations to other countries (because hydrocarbon deposits are location-specific), and they must make large up-front investments before they can realize any income. Hence, governments have strong incentives to abrogate these contracts once firms have made irreversible investments in oil and gas development—resulting in a pattern of expropriations that Vernon (1971) called “the obsolescing bargain” (see also Kobrin 1980, Minor 1994).

RESOURCE WEALTH LEADS TO CONFLICT THROUGH TRADE SHOCKS The price of minerals is unusually volatile, making mineral producers unusually susceptible to trade shocks (Reinhart & Wickham 1994). Both Humphreys (2005) and Blattman (unpublished manuscript) suggest that trade shocks might make resource-rich states more susceptible to civil war. If this is true, a measure of trade shocks—which I explain below—should be significantly linked to the onset of civil war and should reduce the size and significance of the resource variables.

Civil War Duration

Mineral resources may also affect the duration of wars, through one (or more) of three mechanisms. First, resource wealth could lengthen a conflict if it provides funding to the weaker side, helping it equalize the balance of forces; conversely, it could shorten a conflict by providing revenues to the stronger side, boosting its military capacity and bringing a quicker victory. Second, mineral wealth could lengthen conflicts by providing combatants with opportunities to get rich that would be absent in peacetime. By making war profitable, it would reduce incentives to bargain for peace (Sherman 2000, Addison et al. 2002, Collier et al. 2004). Conversely, resource wealth could shorten conflicts by offering combatants economic opportunities they can realize only in peacetime. Finally, resource wealth could make separatist civil wars last longer by reducing the credibility of any government commitments to regional autonomy (Fearon 2004).

To test the resource-duration hypothesis, I use a hazard model and employ the Fearon (2004) dataset and model as a template. The dependent variable is now the duration, in years, of civil wars once they begin. The independent variables of interest are the same resource measures described above, except now I take their mean values over the duration of the conflict.

The control variables are those identified by Fearon (2004): dummy variables for wars linked to coups or revolutions, for Eastern Europe, for states with non-contiguous territories, and for wars between governments and ethnic minorities in peripheral territories. Fearon also includes a resource-related dummy variable, called *Contraband*, that identifies conflicts in which the rebel group sells illicit drugs or mineral resources. He finds that *Contraband* is associated with longer conflicts.

The Fearon dataset includes 128 wars. Because 33 of them ended before 1960, when my dataset begins, my own estimations cover 95 wars or fewer. Twenty-five civil wars were still ongoing in 1999, the last year in the dataset, and are hence right-censored. To correct for this, I use two types of estimations: a Weibull analysis and a Cox proportional hazards model. The Cox approach may be somewhat preferable because it does not assume any specific distributional form for the unobserved duration data. In any case, the two models produce essentially the same results.

RESULTS

Table 1 shows a series of estimations that employ rare events logit and the Beck et al. (1998) correction for duration dependence. All of the right-hand-side variables are lagged one period.

The first model (column 1) replicates the original Fearon-Laitin model, which includes their dummy variable for major oil exporters. (To make it comparable to the other estimations, I limit the sample to the 1960–1999 period.) In models 2 through 10, I replace this dummy variable with four of my resource variables: *Fuel offshore per capita*, *Fuel onshore per capita*, *Primary diamonds per capita*, and *Secondary diamonds per capita*. For each model, I use a different measure of civil war. In models 2 through 6, I use the Fearon-Laitin codings (all wars, national wars, separatist wars, ethnic wars, nonethnic wars); in models 7 and 8, the Sambanis A and B codings; and in models 9 and 10, the PRIO/Uppsala codings for major wars and for all wars.

Fuel Rents and the Onset of Civil War

One of the two fuel-rents variables—*Fuel onshore*—is linked to the onset of conflict in all models. *Fuel offshore* is associated with an increased risk of national and nonethnic conflicts but a reduced risk of smaller conflicts. A Wald test rejects the hypothesis that *Fuel onshore* and *Fuel offshore* are jointly insignificant. In other tests, I found no evidence that *Fuel rents* is more likely to cause conflict in poor countries than wealthy ones. I also found that the log of the *Fuel rents* variables, and their squared terms, fit the data less well than the untransformed *Fuel rents* variables.

The link between *Fuel onshore* and civil war onsets is fairly robust. When I repeat these regressions using the alternative estimation procedure, the results are virtually identical. In model 11, I add a series of regional dummy variables to the baseline specification; the *Fuel onshore* coefficient drops by ~30% but remains marginally significant. To see how sensitive it is to model specification, I drop each of the control variables, one at a time. *Fuel onshore* remains significant at the 0.05 level in all models except the one in which *GDP per capita* is dropped.

Still, civil wars are rare events, and civil wars in resource-rich states are especially rare. If the two most influential observations are dropped from the dataset—the wars in Iran in 1978 and 1979—*Fuel onshore* loses its statistical significance, although the coefficient rises slightly. If Iran remains in the dataset, but the next most influential country—Russia, which had civil wars in 1994 and 1999—is dropped, *Fuel onshore* remains highly significant and the coefficient changes little.

The impact of *Fuel rents* on the likelihood of conflict is substantial. Its magnitude depends partly, however, on how a rise in *Fuel rents* affects GDP. When the values of the other regressors are held at their means, between 1960 and 2000 a

TABLE 1 Logit analyses of civil war onset, 1960–1999^a

	1 ^b	2 ^b	3 ^c	4 ^d	5 ^e	6 ^f	7 ^g	8 ^h	9 ⁱ	10 ^j
Income	-0.305 (3.91) ^k	-0.315 (4.16) ^k	-0.395 (3.05) ^k	-0.161 (2.04) ^m	-0.302 (3.56) ^k	-0.387 (3.04) ^k	-0.210 (3.33) ^k	-0.219 (3.48) ^k	-0.170 (4.93) ^k	-0.123 (2.11) ^m
log (population)	0.224 (3.15) ^k	0.224 (3.11) ^k	-0.002 (0.02)	0.484 (3.35) ^k	0.269 (3.19) ^k	0.159 (1.39)	0.196 (2.95) ^k	0.193 (3.18) ^k	0.332 (9.19) ^k	0.318 (5.95) ^k
log (% mountainous)	0.208 (2.33) ^m	0.197 (2.25) ^m	0.254 (2.34) ^m	-0.048 (0.26)	0.133 (1.29)	0.115 (1.01)	0.227 (2.78) ^k	0.165 (2.30) ^m	0.169 (3.31) ^k	0.294 (3.30) ^k
Noncontiguous	0.253 (0.82)	0.425 (1.26)	-0.605 (0.75)	1.268 (2.27) ^m	0.582 (1.51)	0.089 (0.13)	0.005 (0.01)	0.268 (0.94)	0.510 (2.88) ^k	0.520 (1.71)
New state	2.369 (3.80) ^k	2.394 (3.84) ^k	2.332 (2.79) ^k	2.389 (1.96)	2.412 (3.42) ^k	2.686 (2.87) ^k	0.434 (0.89)	1.120 (2.59) ^k	-0.704 (1.48)	-0.856 (0.80)
Instability	0.721 (2.91) ^k	0.770 (3.05) ^k	0.781 (2.14) ^m	0.966 (2.02) ^m	0.542 (1.77)	1.239 (3.36) ^k	0.866 (3.45) ^k	0.696 (3.14) ^k	0.327 (2.10) ^m	0.566 (2.15) ^m
Democracy	0.026 (1.39)	0.019 (0.98)	0.015 (0.50)	-0.001 (0.03)	0.010 (0.44)	0.023 (0.71)	0.019 (1.03)	0.007 (0.42)	0.015 (1.49)	-0.010 (0.53)
Ethnic fraction	0.224 (0.47)	0.300 (0.65)	-0.220 (0.32)	0.417 (0.56)	0.686 (1.23)	0.404 (0.57)	0.444 (1.02)	0.591 (1.40)	1.068 (4.04) ^k	0.484 (1.06)
Religious fraction	0.275 (0.42)	0.145 (0.23)	0.861 (1.00)	-0.173 (0.15)	1.004 (1.28)	-1.303 (1.75)	0.852 (1.56)	0.724 (1.45)	-0.614 (1.98) ^m	-0.451 (0.88)
Peace years	0.106 (0.87)	0.101 (0.84)	0.071 (0.43)	0.065 (0.33)	0.082 (0.58)	-0.003 (0.02)	-0.236 (2.18) ^m	-0.108 (1.32)	-0.124 (2.25) ^m	-0.115 (1.24)

Oil exporter	0.785 (2.66) ^k	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Fuel onshore	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Fuel offshore	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Primary diamonds	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Secondary diamonds	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Observations	5188	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177	5177

^aRobust z statistics in parentheses. All regressions include three cubic splines and a constant; all variables are lagged one period. Estimations done with Stata 8.0.

^bIn models 1 and 2, the dependent variable is *Civil war onset* (Fearon-Laitin coding).

^cIn model 3, the dependent variable is *National civil war onset* (Fearon-Laitin coding).

^dIn model 4, the dependent variable is *Separatist civil war onset* (Fearon-Laitin coding).

^eIn model 5, the dependent variable is *Ethnic civil war onset* (Fearon-Laitin coding).

^fIn model 6, the dependent variable is *Nonethnic civil war onset* (Fearon-Laitin coding).

^gIn model 7, the dependent variable is *Civil war onset* (Sambanis coding A).

^hIn model 8, the dependent variable is *Civil war onset* (Sambanis coding B).

ⁱIn model 9, the dependent variable is *All civil war onsets* (PRIO coding).

^jIn model 10, the dependent variable is *Major civil war onset* (PRIO coding).

^kSignificant at 1%.

^mSignificant at 5%.

country with no fuel rents had a conflict risk of 0.92%. A country that produced \$100 in oil rents (about the level of New Zealand, Colombia, or Nigeria) had a risk of 0.99%, whereas a country with \$1000 in oil rents (about the level of Venezuela, Iraq, or Gabon) had a risk of 1.8%—about double the risk of a similar country with no petroleum.

If a country discovers a new oil field, however, the baleful effects of *Fuel rents* may be at least partly offset by the beneficial effects of a boost in GDP per capita. (New oil does not always increase a country's GDP per capita. Between 1970 and 1999, Nigeria's oil industry generated \$231 billion in rents, while GDP per capita fell from \$264 to \$250). If a rise in *Fuel rents* from zero to \$1000 leads to a \$1000 rise in GDP per capita, a country's net conflict risk will increase from 0.92% to 1.32%—still a rise of >40%, but much less than it would be without the GDP offset. If the new fuel revenues are productively invested in the economy and lead to a \$2150 per capita increase in GDP, then the harmful effects of oil will be fully offset by the benefits of greater wealth.

Diamonds and the Onset of Civil War

If a consolidated measure of diamonds per capita is placed in Table 1's models 2 through 10, it never approaches statistical significance. But if diamonds are separated by type, a strong pattern emerges: *Primary diamonds* is associated with the onset of conflict in eight of the nine models in Table 1, and *Secondary diamonds* is correlated with civil war in the ninth, which covers separatist conflicts only. The two variables are jointly significant in a Wald test. Once again, there is no evidence that diamond production is more hazardous in poor countries than in rich ones, or that log or squared terms improve the fit of the diamond measures.

The association between *Primary diamonds* and civil war is quite robust. The alternative estimation procedure yields identical results. Adding regional dummies has little effect on the coefficient or significance of *Primary diamonds*. When each control variable is dropped, one at a time, *Primary diamonds* never loses statistical significance.⁶

Still, civil wars in diamond-producing states are quite rare, which should make us exceedingly cautious about generalizations. Of the 90 civil wars that began between 1960 and 1999, only 12 took place in countries that produced diamonds in nontrivial quantities (Table 2). Of these 12, only seven happened in countries that produced primary diamonds: four in the Democratic Republic of Congo, two in Russia, and one in South Africa.⁷ *Primary diamonds* remains statistically significant if Russia and South Africa are simultaneously dropped from the dataset.

⁶The division between primary and secondary diamonds, however, is somewhat fragile. After 1985, secondary diamonds seem to grow more salient for conflict than primary diamonds, owing to the outbreak of war in four African states that produced secondary diamonds only: Liberia (1989), Sierra Leone (1991), Angola (1992), and Central African Republic (1996).

⁷Five additional civil wars occurred in two countries that produced tiny amounts of natural diamonds: Indonesia (three conflicts) and India (two conflicts).

TABLE 2 Civil wars in diamond-producing states, 1960–1999 (in order of the value of diamonds produced per capita in the year of onset)^a

Country	Onset	Diamonds per capita	Conflict type	Diamond type
Dem. Rep. Congo	1960	1528	ambiguous	both
Dem. Rep. Congo	1977	374	national	both
South Africa	1983	372	national	both
Angola	1975	178	national	secondary
Liberia	1989	132	national	secondary
Angola	1992	39	separatist	secondary
Sierra Leone	1991	35	national	secondary
Central African Rep.	1996	13	national	secondary
Dem. Rep. Congo	1998	13	national	both
Russia	1999	8	separatist	both
Dem. Rep. Congo	1996	6	national	both
Russia	1994	4	separatist	both
Indonesia	1975	0.05	separatist	secondary
India	1982	0.04	separatist	both
Indonesia	1965	0.02	separatist	secondary
India	1989	0.01	separatist	both
Indonesia	1991	0.01	separatist	secondary

^aThe year of onset and conflict type are from Fearon & Laitin (2003). Diamonds per capita is the per capita value of diamond production, in constant 2000 dollars, in the onset year.

If the Democratic Republic of Congo alone is dropped, however, *Primary diamonds* loses statistical significance.

Diamond production can somewhat increase a country's conflict risk, but somewhat less than oil production. When the values of the other variables are held at their means (and oil production is zero), a country with no diamond production had a conflict risk of 0.91% between 1960 and 2000; a country with a relatively high level of diamond production (\$170 per capita, about the level of Namibia in the late 1990s) had a conflict risk of 0.98%. A new discovery of diamonds, however, would be less damaging if it simultaneously raised GDP. If \$170 per capita in primary diamonds were suddenly unearthed, and it led to a rise in GDP per capita of \$170, the conflict risk would rise to just 0.93%; if it lifted GDP per capita by \$270, the net impact of the new diamond wealth would be zero.

Resource Wealth Increases the Value of the State as a Target

If resource wealth causes civil wars by giving insurgents a greater incentive to capture the state's assets, we should see national conflicts associated with the

four resource variables that produce large state revenues: *Fuel rents onshore*, *Fuel rents offshore*, *Primary diamonds*, and *Nonfuel rents*. National conflicts are indeed linked to the first three variables, although not the fourth.⁸

There are three possible versions of this state-as-target hypothesis: that all types of government revenue have the same conflict-inducing effects; that all nontax revenues (which may be subject to less pressure for accountability) have conflict-inducing effects; or that only resource revenues have this effect, owing to some undefined special characteristics.

If the first claim is true, then other types of government revenues should also increase the likelihood of conflict. To test this, I add to the model an additional variable called *Government share*, which is the government's share of real GDP and represents the wealth held by the government relative to the rest of the economy. It is not significantly correlated with any of the five Fearon-Laitin civil war measures, nor the two PRIO civil war measures, using either estimation procedure. It is, however, positively correlated with Sambanis's B civil war measure and is significant at the 0.10 level, but only with one of the two estimation procedures (using the *Lagged war* variable in place of *Peace years* and no cubic splines). Even in this model, though, its inclusion has little effect on the substantive or statistical significance of the resource variables.

If the second claim is true, then all types of nontax revenue—not just oil and diamond wealth—should increase the danger of civil war. To find out if this is so, I add to the model a variable called *Nontax revenue*, which measures all forms of nontax revenue as a fraction of the government's total revenues. It is not positively correlated with any of the civil war measures. It is however, negatively correlated with the onset of major conflicts in the PRIO dataset, when using one of the two estimation procedures (including the *Peace years* variable and the cubic splines).

In short, my tests offer partial support for the state-as-target hypothesis. The connection between three of the four resource variables (*Fuel rents onshore*, *Fuel rents offshore*, *Primary diamonds*) and national civil wars is consistent with this mechanism. But the two broader versions of this claim—that all government revenues, or nontax revenues, are linked to conflict—appear to be untrue. If this mechanism is correct, only revenues from diamonds and hydrocarbons seem to encourage rebel groups to displace the government—an odd pattern that begs for further inquiry.

Resource Wealth Increases the Value of Sovereignty in Mineral-Rich Regions

There is good support for the hypothesis that resource wealth heightens the likelihood of civil war by increasing the value of sovereignty in mineral-rich regions.

⁸*Nonfuel rents* may not achieve statistical significance—here and elsewhere—in part because all observations for 1960–1969 are missing. I revisit the issue of *Nonfuel rents* in the conclusion.

TABLE 3 Separatist movements in petroleum-producing states, 1960–1999 (in order of fuel rents per capita in the year of onset)

Country ^a	Year	Region/Movement	Fuel rents per capita	In region? ^b
Iran	1979	KDPI (Kurds)	1926	Yes
Iraq	1961	KDP, PUK (Kurds)	547	Yes
Russia	1994	Chechnya	409	No
Russia	1999	Chechnya II	343	No
Azerbaijan	1992	Nagorno-Karabagh	223	No
Angola	1992	FLEC (Cabinda)	171	Yes
Indonesia	1991	GAM (Aceh)	77	Yes
Indonesia	1975	E. Timor	74	No
Croatia	1992	Krajina	50	Yes
China	1991	Xinjiang	36	Yes
India	1982	Sikhs	22	No
Turkey	1984	PKK (Kurds)	16	Yes
Indonesia	1965	OPM (West Papua)	13	No
Bosnia	1992	Rep. Srpska/Croats	13	Yes
India	1989	Kashmir	12	No
Nigeria	1967	Biafra	10	Yes
Yugoslavia	1991	Croatia/Krajina	6	Yes
Pakistan	1973	Baluchistan	3	Yes
Pakistan	1971	Bangladesh	3	Yes
Morocco	1975	Polisario	3	No
Bangladesh	1976	Chittagong Hills	0.52	Yes
United Kingdom	1969	IRA	0.04	No

^aThese 22 states produced petroleum at the time that a separatist conflict began (according to the Fearon-Laitin codings).

^bThis column indicates whether petroleum was produced in the separatist region.

Both *Fuel onshore* and *Nonfuel rents* are correlated with the onset of separatist conflicts. The *Fuel offshore* variable is not correlated with separatist conflicts, which is also consistent with this hypothesis: Separatist movements should be less likely to claim sovereignty over offshore petroleum than onshore petroleum.⁹

A closer look at the data also supports this hypothesis. Table 3 lists all of the post-1960 separatist conflicts in the Fearon-Laitin dataset that took place in

⁹Two notable exceptions are the conflicts in Angola's Cabinda enclave and Indonesia's Aceh province. In both cases, however, the offshore petroleum rigs have substantial onshore processing facilities in the separatist region.

petroleum-producing states. In 13 of these 22 conflicts, the separatist region contained at least some of this petroleum wealth.

Yet the *Primary diamonds* and *Secondary diamonds* variables do not fit well in this explanation. In all other tests, *Primary diamonds* is correlated with conflict, and *Secondary diamonds* is not correlated with conflict; but for separatist civil wars, the reverse is true. The correlation between *Secondary diamonds* and separatist conflict contradicts arguments made by Le Billon (2001, 2005a), Collier & Hoeffler (2005), and most explicitly, Ross (2003).

A look at the separatist wars in diamond-producing states suggests that this mechanism is not at work. Between 1960 and 1999, there were eight separatist conflicts in diamond-producing states (Table 2); five of them occurred in Indonesia and India, which produce only trivial quantities of diamonds. Diamond wealth was not found in any of the eight separatist regions.

Although this mechanism helps explain the correlation between petroleum and conflict, it does not explain the link between diamonds and conflict.

Resource Wealth Helps Finance Rebel Organizations

There is partial support for the proposition that resource wealth facilitates civil wars by funding rebels. Civil wars are correlated with *Fuel onshore* but not *Fuel offshore*; offshore oil and gas deposits afford rebel groups fewer extortion opportunities. Yet *Secondary diamonds*, which measures the most lootable resource—and hence is the most likely to contribute to rebel finance—is uncorrelated with civil war onsets in eight of the nine models.

This remains the most controversial causal mechanism, and it has been challenged by several studies (Fearon 2005, Arnson & Zartman 2005). Ross (2004b) finds little evidence at the case study level to support this claim.

Resource Wealth Weakens the State

There is partial support for this mechanism. If resource wealth leads to civil war by weakening the state (or perhaps by weakening society), both *Fuel onshore* and *Fuel offshore* should have identical conflict-inducing qualities for all types of conflict. Conflict is more consistently tied to *Fuel onshore* than *Fuel offshore*, but we should not draw strong conclusions from this pattern.

If this mechanism is correct, we might also expect to see other forms of non-tax revenues producing a “weak state” and a heightened civil war risk. Yet, as noted above, a *Nontax revenue* variable is not positively correlated with civil war onsets, and in one specification, it is negatively correlated with civil war onsets.

If Snyder & Bhavnani (2005) are correct that secondary diamonds tend to weaken the state’s capacity to maintain order, we should see *Secondary diamonds* linked to more types of conflict—not just separatist conflict. This link is not found, although as noted above, the distinction between *Primary diamonds* and *Secondary diamonds* in the model is somewhat fragile.

Still, there is some evidence for the state-weakness mechanism. It may be the best way to explain the correlation between *Secondary diamonds* and separatist conflicts, since these diamonds are never found in the separatist regions and produce little revenue for the government. It may also account for link between *Fuel offshore* and national, non-ethnic conflicts. Finally, the state-weakening process may occur through a more complex route than the ones I test for here, although I have explored one such route elsewhere (Ross 2001b).

Resource Wealth Leads to Conflict Through Trade Shocks

There is partial support for the mechanism of trade shocks. Some of the evidence in its favor may be tainted by endogeneity, however, and hence should be treated cautiously.

If this hypothesis is true, both *Fuel onshore* and *Fuel offshore* should have identical conflict-inducing effects, since both types of petroleum render the state susceptible to price shocks. *Fuel onshore* is more robustly linked to civil war than *Fuel offshore*, although this could be a statistical artifact.

To further probe this mechanism, I introduce a new variable, *Oil shock*, in which the change in the real international price of petroleum over the previous two years interacts with fuel rents per capita. Unfortunately, this variable is endogenous to conflict: Turmoil in oil- and gas-producing states tends to produce price shocks. To reduce this problem, I measure the change in oil prices from January 1 in year $t - 2$ to January 1 in year t . Because civil wars almost invariably begin after January 1 in the year of onset, *Oil shock* represents changes in the price of oil before conflict commences. This additional step does not, however, eliminate the endogeneity problem. Oil prices also reflect the anticipation of conflict in petroleum-exporting states, and hence may be driven up by early signs of unrest.

When *Oil shock* is added to models 2–10 it performs relatively well. The absolute value of *Oil shock*—which treats both positive and negative shocks as equally hazardous—performs even better: It is significantly linked to civil war (at the 0.05 level) in five of the nine models. I separate positive oil shocks from negative ones to test this further. Both positive and negative shocks are correlated with the likelihood of civil war onsets in about half of the models. Only negative shocks are linked to separatist conflicts (e.g., Iraq 1961, Iran 1979, Azerbaijan 1992, Angola 1992, and Russia 1994 and 1999), whereas only positive shocks are connected to national conflicts (e.g., Zimbabwe 1972, Argentina 1973, Angola 1975, and Peru 1981). Although negative shocks may be endogenous to conflict, positive shocks should not be.

One way to interpret these results is as follows. Perhaps negative shocks foster separatist insurgencies by producing discontent in the resource-rich region while weakening the state's finances and hence its ability to repress dissent. Negative shocks are also linked to the fall of authoritarian governments, which could also lead to the rise of separatism (Przeworski et al. 2000; Acemoglu et al., unpublished manuscript.).

The outbreak of a large-scale separatist insurgency in Indonesia in 1999 appeared to follow this pattern. A large negative shock (the Asian economic crisis) simultaneously weakened the central government, led to the fall of an authoritarian ruler (Suharto), and produced enormous proindependence protests in a petroleum-rich region (Aceh). Within a few months, a dormant secessionist movement reappeared and achieved unprecedented military success (Ross 2005).

A different process might connect positive shocks to national civil wars. If the state-as-target hypothesis is correct, positive shocks could make the government a more attractive target for insurgents. At the same time, it could lead to institutional breakdown within the government, which could weaken its ability to maintain order (Ross 2001b).

Resource Wealth Lengthens Civil Wars

Table 4 presents the results of the hazard models that estimate the effect of natural resource measures on the duration of civil wars. The first column (model 1) replicates the Fearon (2004) study; the variable of interest, *Contraband*, is significantly linked to longer conflicts.

In model 2, I replace *Contraband* with my four resource variables, using a Cox model; none is statistically significant. Model 3 contains the Lujala et al. (2005) diamond variables; these, too, are not statistically linked to conflict duration. I repeat these tests using a Weibull model and find the results unchanged.

The only resource variable that is linked to longer conflicts is Fearon's original *Contraband* variable. Although it is a dummy variable, it is probably better suited to test the claim that natural resource wealth makes conflicts longer. Whereas my own resource variables measure the production of minerals in an entire country, *Contraband* identifies resources under the control of the rebel group. It also captures a broader range of funding sources; whereas I measure the production of fuel, nonfuel minerals, and diamonds, *Contraband* also covers narcotics (which have funded rebel groups in Peru, Colombia, and Burma), timber (which has funded insurgents in Indonesia and Cambodia), and gemstones other than diamonds (which have funded rebellions in Afghanistan and Cambodia).

The association between *Contraband* and conflict duration is quite robust. The results change little when I use alternative estimation procedures (Cox and Weibull), add a series of regional dummies, or drop each right-hand-side variable in turn. The substantive and statistical significance of *Contraband* is essentially unchanged when the three longest-running conflicts where contraband was present (Burma, Colombia, and India) are dropped from the sample.

Although the correlation between *Contraband* and conflict duration is robust, the direction of causality is not yet clear. Access to gemstones and drugs might help insurgencies last longer, but longer-lasting insurgencies might also be more likely to sell contraband because they have more time to establish the production and trading networks they need to profit from drugs, timber, and gemstones. Longer-lasting conflicts might also be more likely to continue from the era when

TABLE 4 Hazard models of civil war duration^a

	1	2	3	4
Coup	1.021 (3.31) ^b	0.934 (2.35) ^c	0.907 (2.28) ^c	1.086 (3.49) ^b
Eastern Europe	1.057 (3.10) ^b	1.463 (3.46) ^b	1.576 (3.68) ^b	1.138 (3.34) ^b
Noncontiguous	0.386 (1.36)	0.222 (0.36)	0.285 (0.46)	0.823 (1.82)
Peripheral	-1.188 (3.35) ^b	-1.026 (2.34) ^c	-1.081 (2.47) ^c	-1.176 (2.90) ^b
Contraband	-1.044 (2.57) ^c			
<i>Fuel rents</i>		-0.000 (0.07)		
<i>All diamonds</i>		0.001 (0.84)		
<i>Fuel onshore</i>			0.002 (1.14)	
<i>Fuel offshore</i>			0.004 (1.23)	
<i>Primary diamonds</i>			0.021 (1.02)	
<i>Secondary diamonds</i>			-0.020 (0.97)	
<i>Pr. diamond dummy</i>				0.553 (0.81)
<i>Sec. diamond dummy</i>				-0.579 (1.17)
Observations	122	90	90	111

^aThe reported figures are hazard ratios. Absolute value of *z* statistics are in parentheses. The dependent variable is civil war length in years, using the Fearon (2004) dataset. The first five variables are dummy variables from the Fearon model.

^bSignificant at 1%.

^cSignificant at 5%.

contraband was less important (before about 1980) to the era when it is more important.

Indeed, many years of combat often elapse before insurgents begin to sell large quantities of contraband. In Afghanistan, it took four years; in Angola, 10 years; in Cambodia, 11 years; in Colombia, almost 20 years. We need more research on contraband financing before we can be sure that it prolongs civil wars.

COMPARISON WITH PREVIOUS STUDIES

The results in this paper are broadly consistent with previous studies; many researchers have found evidence that some measure of oil wealth tends to increase the likelihood that a civil war will begin (de Soysa 2002; Fearon & Laitin 2003; de Soysa & Neumayer, unpublished manuscript; Fearon 2005; Humphreys 2005). But in previous studies the link between the onset of civil war and the most common measures of oil wealth has been sensitive to the choice of civil war dataset and to the specification of the civil war model (Hegre & Sambanis, unpublished manuscript; Sambanis 2004b).

I address these concerns by using a more accurate way to measure a country's wealth from oil, natural gas, and coal, and showing it is robustly correlated with the onset of civil war and survives several sensitivity tests. I also find that onshore production is more robustly tied to conflict than offshore production, confirming a result first reported by Lujala (unpublished 2004 manuscript).

Earlier studies of diamonds reported somewhat different findings. Humphreys (2005) shows that diamond production increases the likelihood of conflict; Lujala et al. (2005) suggest that diamond production affects ethnic conflicts only, and that secondary diamonds increase the risk of ethnic war whereas primary diamonds decrease it.

There may be several reasons why these two studies produce different findings. One is that they use different categories for their analyses: Lujala et al. look separately at the impact of primary and secondary diamonds on ethnic and nonethnic conflict whereas Humphreys looks at the impact of all types of diamonds on all types of conflict. The two studies also measure diamond wealth in different ways. For Humphreys, the key independent variable is the volume of diamond production per capita; for Lujala et al., it is a dummy variable indicating whether or not a country was a diamond producer. Finally, they disagree about the data. Humphreys identifies 22 diamond-producing states whereas Lujala et al. identify 30.

I try to reconcile these findings by checking and correcting their data, combining their diamond measures, and converting the volume of diamond production into the value of diamond production. My results differ from those of both prior studies. Now the *Primary diamonds* variable—measured as the value of production per capita—is significantly and robustly associated with eight of the nine conflict measures, including both ethnic and nonethnic conflict. *Secondary diamonds* is linked to the ninth conflict measure, separatist wars. I note, however, that inferences about diamonds and conflict are based on a very small number of wars in diamond-producing states, which should make us cautious about drawing strong conclusions from these analyses.

Most previous studies on natural resources and conflict duration find that lootable natural resources—including secondary diamonds and other gemstones, other alluvial minerals, timber, and narcotics—are associated with longer wars (Buhaug et al., unpublished manuscript; Fearon 2004; Buhaug & Lujala 2005; Lujala et al., unpublished manuscript). Others find that primary commodities in

general (Collier et al. 2004), or oil in a conflict zone, also tend to prolong conflicts. Humphreys (2005), however, argues that both oil production and diamond production tend to reduce conflict duration.

My results are consistent with the prevailing view that when lootable goods are available to insurgents, civil wars tend to last longer. The effect seems to be large. In Fearon's dataset, when insurgents have access to contraband, the mean conflict length is 16.6 years; when they do not, the mean conflict length is only 7.5 years. I argue, however, that the direction of causality between contraband funding and conflict duration is still open to question. I also find no evidence that the production of hydrocarbons or diamonds at the country level is correlated with the duration of civil wars.

Prior studies have offered a wide range of hypotheses about the causal mechanisms that tie resources to conflict onset. I find evidence that more than two mechanisms are valid. The mechanism with the strongest support suggests that mineral wealth tends to foster separatist conflicts by increasing the perceived value of sovereignty in mineral-rich regions. This claim fits both the statistical evidence and the case study evidence. But this cannot be the only mechanism because it cannot explain the correlation between national conflicts and the *Fuel onshore* and *Primary diamonds* variables.

I find some evidence that trade shocks account for part of this correlation—and that negative shocks are associated with separatist conflicts and positive shocks with national conflicts. Yet even so, when the *Oil shock* variable is added to the model, it reduces the coefficient on *Fuel onshore* only slightly and leaves it statistically significant. If trade shocks matter, they seem to be only a small part of the story.

There is partial support for two other hypotheses: that resources weaken the state or increase its value as a rebel target. Yet I find no evidence that other types of government revenues—even nontax revenues—have similar effects. More work is needed to sort out the issue of causal mechanisms.

CONCLUSION

This paper has reviewed recent trends in the study of natural resources and civil war; emphasized the problems of measurement, endogeneity, robustness, and causality; and shown how more precise and exogenous measures of mineral production can help us overcome these problems. It finds that these improved measures of hydrocarbon and diamond production are robustly correlated with civil war onsets, but that only Fearon's measure of contraband is associated with conflict duration. It also documents the rise in the prevalence of civil wars in petroleum-rich and diamond-rich countries between 1960 and 2002, and the growing use of contraband funding. Finally, it presents evidence that oil and other minerals tend to foster conflict because they make independence more desirable for resource-rich regions; that trade shocks play a role in triggering both national and separatist conflicts;

and that at least one other mechanism ties oil and diamonds to the outbreak of civil war—particularly national civil wars.

It is important to remember, however, that civil wars are rare events, and civil wars in petroleum- and diamond-producing states occur quite infrequently. Between 1960 and 1999—a period with 5436 country-year observations—about 90 civil wars began. Twenty-nine of them occurred in states that produced at least \$100 per capita in petroleum and 12 in states that produced at least \$1 per capita in diamonds. Because these types of civil war are so rare, small changes in the data can alter the statistical significance of the minerals-conflict correlation. If the oil-rich country with the most civil wars (Russia) or the diamond-rich country with the most civil wars (Democratic Republic of Congo) did not exist, these correlations would lose statistical significance. The dependence of these correlations on a small number of rare events should make us modest in our claims about the resource-conflict link.

There are many unanswered questions about the resource wealth-conflict link. I have already discussed our need to better understand the causal mechanisms that explain the oil-conflict and diamond-conflict links, and to better establish the causal relationship between contraband commodities and conflict duration. Creative formal models (e.g., Aslaksen & Torvik, unpublished manuscript) can help clarify the logic behind these causal links.

We also need more work on the puzzle of nonfuel minerals. All of our theories about oil and conflict should also apply to other valuable minerals, but econometric tests only show a link between nonfuel mineral rents and separatist conflicts—and even that link is relatively weak. Is the nonsignificance of nonfuel minerals evidence of flawed data or of a flawed model? Is there something unique about petroleum that sets it apart from other minerals and gives it special conflict-inducing powers?

Finally, we need more clarity on the possible role of agriculture. Several earlier studies suggest that the export of agricultural commodities is unrelated to a country's civil war risk (Collier & Hoeffler 2005, Fearon 2005). Many others assume that the only primary commodities that matter are oil and gemstones. Three recent analyses, however, have challenged this view. Blattman (unpublished manuscript) suggests that in Latin America and Africa, the export of coffee, cotton, and (in poor states) cocoa is associated with higher rates of violent conflict. According to Moradi (unpublished manuscript), in sub-Saharan Africa, conflicts are more likely to break out in regions where cash crop processing facilities exist. And Humphreys (2005) finds that the likelihood of conflict is positively correlated with the share of agriculture in national income.

These findings are intriguing but difficult to interpret. The Blattman and Humphreys findings might be caused by the greater vulnerability of agriculture-dependent states to economic shocks; this would be consistent with several of the findings in this paper, and with a study by Miguel et al. (2004) that shows that economic shocks are strongly linked to civil conflict in Africa. More finely grained studies could help sort out what, if any, agriculture contributes to a country's civil war risk.

In just a few years, we have learned a great deal about the influence of natural resources on civil war. In the coming years, new avenues of research will almost certainly help resolve today's questions and generate new ones.

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