LIS
Working Paper Series

No. 566

Explaining the Development-Civil War Relationship

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July 2011

CROSS-NATIONAL
DATA CENTER
in Luxembourg

Luxembourg Income Study (LIS), asbl

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A revised version of the article is forthcoming in *Conflict Management and Peace Science*

Abstract

The vast majority of civil wars occur in economically less developed countries, as measured by GDP per capita. This paper assesses two suggested explanations for this: one emphasizing that poverty facilitates rebel recruitment due to lowered *economic opportunity cost* of rebelling, and the other highlighting that low accessibility and state capacity give *political and military opportunity* for organizing insurgency. I argue that the latter account is more powerful. Low accessibility is vital not only to rebel survival; it also enables rebels to obtain control over remote settlements, which facilitates the effective use of persuasion, coercion, organization, and economic rewards for mobilizing recruits and other resources. Although low economic opportunity costs can ease recruitment, it may not be essential if such tools are available. The argument is supported by a quantitative analysis covering 133 countries from 1989-2006. Countries experiencing civil war were distinguished more by low accessibility (measured by road density, telephone density and % urban of the population) than by depth of poverty (measured by the mean income of the poorest decile). Moreover, the negative association between GDP per capita and civil war risk disappeared when controlling for accessibility, but remained strong controlling for poverty.

1. Introduction

Civil war is predominantly a “problem of the poor”, as (Sambanis, 2002: 216) has put it. Cross-national studies have found a strong negative relationship between GDP per capita and risk of civil war onset (c.f., Hegre and Sambanis, 2006). Despite consensus on this empirical relationship, our understanding of its causes remains limited. Several different causal mechanisms are consistent with the finding, and GDP per capita has been interpreted as a proxy for widely different theoretical concepts. This paper examines two suggested explanations. The first stresses the economic opportunity cost of rebelling. Assuming that people decide to become rebels on the basis of short-term private economic considerations, it asserts that poverty should give a greater supply and lower cost of rebel soldiers, since the poor have less regular income to lose by rebelling (Collier, 2000; Collier and Hoeffler, 2004). The second rather emphasizes politico-military opportunity (Fearon and Laitin, 2003; Leites and Wolf, 1970). Since the state is usually militarily superior to the insurgents in the initial stages of conflict, the key for insurgents to survive through the first stage is to hide from state forces. This is typically possible in low-income countries where the accessibility of the state is limited due to less developed communication infrastructure, scattered settlements, and low state capacity. Inaccessibility also provide opportunities for rebels to carve out local control, which can effectively be used to mobilize resources.

Drawing on case study research, I argue that the politico-military opportunity account is more powerful than the economic opportunity explanation. Importantly, the latter does not take into account that the reasons for contributing resources to insurgents derive in large part from the *process* of insurgency, and that rebel organizations – if conditions allow – can manipulate not only economic incentives, but also security incentives and emotional and moral impetuses for participation. Although low economic opportunity costs can ease recruitment, it may not be essential if insurgents are able to gain control over remote settlements, which facilitates the effective use of coercion, organization, persuasion and economic rewards to mobilize recruits and other resources. The opportunity for rebels to do this depends mainly on accessibility and state capacity, not poverty. Moreover, beyond a certain level of state reach and capacity, there is hardly any military opportunity for insurgency.

I test the explanatory power of the two accounts on data for 133 countries in the 1989-2006 period. As a proxy for economic opportunity costs of potential rebels I use the income per capita of the poorest 10 % of society – the bottom decile income per capita (*BDI pc*). Accessibility is proxied by an index of road density, % urban of the population, and telephone density. The cross-national development-civil war relationship disappears when controlling for *Accessibility*, whereas it remains strong when controlling for *BDI pc*. I conclude that the politico-military opportunity account of the development-civil war relationship seems to be more powerful than the economic opportunity account.

2. Theory

2.1 The Economic Opportunity Explanation

The idea that low-income countries are prone to civil war because the poor have low economic opportunity cost of rebelling is often invoked (e.g. Sambanis, 2002). It is aptly formulated by Paul Collier and colleagues (Collier, 2000b; Collier and Hoeffler, 2004; Collier, Hoeffler, and Rohner, 2008).[1] They argue that a key step in explaining civil war is to account for the formation of a rebel army large enough to put up substantial resistance against government forces. Also, they point out that most (potential) rebel groups have strong financial constraints, and therefore, the ability to form an army may hinge upon the salary levels demanded by fighters. Poverty may thus facilitate rebellion because it makes rebel labor cheap: "[r]ecruits must be paid, and their cost may be related to the income foregone by enlisting as a rebel" (Collier and Hoeffler, 2004:659).[2]

Assuming for now that this economic recruitment mechanism is pervasive, is it true that poverty facilitates insurgency? A first challenge is that lower income levels should reduce labor costs not only for the rebel organization, but also for the government, which is competing with the rebels over skilled soldiers. However, as Collier (2000b) argues, the rebel organization is likely to be more sensitive to labor costs than the government. Not only does the government have greater financial resources; it may also conscript soldiers. Another objection is that poor societies may yield fewer assets for the rebels to loot or 'tax' in a more regularized way.[3] Although this is true, existing assets in low-income countries may be easier to tax for any ruler – rebel or government – because capital mobility is low (Fearon, 2008). Typically, the bulk of capital lies in land, which, in

[1] Note that the economic opportunity cost argument is part of their broader 'economic opportunity' or 'greed' theory of rebellion, which I have no ambition of assessing in its entirety here.
[2] In recent work, Collier and colleagues maintain that economic opportunity cost is vital for explaining the development-civil war relationship, but acknowledge that state capacity may also be part of the explanation (Collier, Hoeffler, and Rohner, 2008). No attempt is made at separating and empirically testing the importance of the two accounts, however.
[3] The ideal socio-economic conditions for financing a rebellion, following the 'greed' logic, might rather be one of high inequality: a poor segment to be targeted for recruitment and a rich segment to be targeted for financial resources.

contrast to financial assets or human skills, cannot easily be moved in response to tax increases.

Returning to the core mechanism, how important are economic opportunity costs likely to be for the choice of rebelling? Economic concerns plausibly play *some* role in many people's choice of becoming full-time rebels or not. Surveys of rebel combatants have been performed in too few cases to conclude on any general rebel socio-economic profile. Still, the existing evidence indicates that the poor tend to be overrepresented among rebel as well as government forces (Arjona and Kalyvas, 2006; Humphreys and Weinstein, 2008). This fits with the economic opportunity cost argument.[4]

However, in civil war, economic concerns are often trumped by other concerns, as the rich case study literature suggests. In several cases, like in Zimbabwe (Kriger, 1992), Sierra Leone (Humphreys and Weinstein, 2008), or Guatemala (Stoll, 1993), many rebel participants emphasize security reasons for participating, due to threats by the government, the rebels, or both. In some cases, like in El Salvador, many highlight emotional or moral engagement (Viterna, 2006: 20-21; Wood, 2003: 231). Other reasons, like fun or adventure (Arjona and Kalyvas 2006) or status (Kriger, 1992: 115) are also found. In an exceptional case of economic motivations figuring more prominently, Arjona and Kalyvas (2006) report that one fifth of Colombian guerillas said they joined

[4] It could be partly spurious, however, caused by the rebel organization focusing mobilization efforts in the poorer periphery, not because it is poor, but because this is where government presence is the weakest.

due to the promise of material goods. Still, the overall pattern is one of rebels emphasizing motivations other than the economic.[5]

Collier (2000a) argues for skepticism towards rebels' statements, since talk is cheap and rebels have incentives to construct a narrative of grievance rather than greed. Still, there is one strong reason to believe rebels' statements: most rebel organizations put great resources into shaping non-economic incentives and impetuses for participation. Why would so many rebel organizations prioritize methods like persuasion, coercion, organization, and network building if political beliefs and preferences, social incentives, and security incentives were not important for participation? This points to a key limitation of the economic opportunity cost account: its ignorance of the *politics* of resource mobilization. Despite the tempting analogy, rebel organizations are not like firms recruiting employees in the market; they can use tools to shape people's beliefs about the context of their choice, as well as mold the actual context itself – thus affecting people's perceived incentives in terms of profit, security, or social esteem. The availability of these tools, so essential for mobilizing all types of resources, depends less on poverty than on state reach and capacity.

2.2 The Politico-Military Opportunity Explanation

The other prominent account of the poverty-civil war association emphasizes that low state reach and capacity create political opportunity for insurgency. This theory is more

[5] A large project involving 21 case studies designed to assess and expand Collier and Hoeffler's economic opportunity theory found that the suggested economic mechanisms were often not important. Forced recruitment was highlighted as one important mechanism ignored by the economic model (Sambanis, 2004a).

process-oriented: it explains the poverty-civil war association mainly by the opportunity for dissident organizations to effectively apply mobilizational tools and survive government repression, rather than by any pre-war characteristics of the population, like poverty. In Fearon and Laitin's (2003:76) words, "[w]here states are relatively weak and capricious, both fears and opportunities encourage the rise of would-be rulers who supply a rough local justice while arrogating the power to 'tax' for themselves and, often, for a larger cause." They emphasize how rebel organizations – under the right circumstances – can mobilize resources by shaping the beliefs and incentives of local populations, and that weak state governments often apply inept and indiscriminate counterinsurgent measures that further fuel the insurgency.

The typical features of poorer countries giving politico-military opportunity for insurgency are not only weak military and police capacity (Herbst, 2004), but more generally a low penetration of state authority throughout the polity. Shortage of roads and communication infrastructure and a scattered, village-based settlement pattern make information as well as state control more costly in low-income countries. This gives opportunity for building up an armed oppositional organization in the periphery, even where the state is militarily superior (c.f., Buhaug, 2010).[6] Without knowing where the rebels are, the state cannot take advantage of its initial military superiority (Kalyvas, 2006: 174; Leites and Wolf, 1970: 33). This is amply shown in the counterinsurgency literature, which points to information-gathering abilities as the most important determinant of counterinsurgent effectiveness (Lyall and Wilson, 2009; Lyall, 2010).

[6] Note that although most insurgent groups are initially very small (Sambanis 2004: 267), in a substantial minority of civil wars since 1945, state power had almost broken down by the start of insurgency, making the rebel-government power balance more symmetric from the outset (Kalyvas and Balcells 2010:423).

The infrastructural weakness of the state is not only important for rebel survival; it also provides a chance for rebels to carve out military and political control over peripheral settlements, which gives them powerful tools for mobilizing resources. An organization's use of coercion, provision of collective goods, organization, and indoctrination all become more effective in the context of control, for at least three reasons:

First, certain *threats* are only credible if the threatening organization has more local control than its rival. This is perhaps particularly important for obtaining information, and hindering information flowing to the rival. Informing the locally weaker organization involves a great risk of sanction (Kalyvas, 2006). Moreover, coercion may be cost-effective for mobilizing resources that require virtually universal compliance, like control of information: it is cheaper to punish a few defectors than to pay a majority of compliers (Oliver 1980:1363-64). Control also improves insurgents' opportunities for *rewarding cooperation*. A variety of collective goods can be provided in return for the local population's cooperation. Protection against government violence (Mason, 2004), provision of land (Wood, 2003), or the breaking of chains to powerful landowners (Popkin, 1988) can hardly be provided without ample local control. As Skocpol (1982:366) argues, "[i]t is hard to imagine the successful institutionalization of such social exchange between peasants and revolutionaries except in places and times unusually free from counterrevolutionary state repression." Finally, control facilitates *organization* and *persuasion* by making it possible to enlist the local population into insurgent-affiliated organizations, like youth groups, farmer cooperatives, unions, and militias (Tse-Tung, 1967:105-108). These organizations provide arenas for indoctrination, encourage part-time labor efforts, and enhance intelligence collection. Moreover, they may facilitate collective action by placing insurgent leaders in the center of networks, and create stronger ties through more regular face-to-face interactions (Petersen, 2001:61-75).

In sum, the politico-military opportunity account is better able to capture the multiple and varied motivations for contributing to insurgency, and how these motivations can be shaped by the belligerent organizations. Although it is only a partial explanation of any civil war, this account plausibly provides core insights into why poor countries are more prone to civil war than wealthier ones. I next assess the degree to which the two accounts find support in cross-national quantitative evidence.

2.3 Empirical Implications

I use large-N cross-national data to test the power of the two explanations. Previous cross-national studies have used GDP per capita as a proxy for both the central variable in the economic opportunity account – economic opportunity costs (Collier and Hoeffler, 2004), and the central variables in the military opportunity account – accessibility and reach of the state (Fearon and Laitin, 2003). This offers little help in determining each account's explanatory power. I construct more specific cross-national proxies than GDP per capita for the central variables of the two accounts, and I find that these new proxies have considerable independent variation, which gives leverage for statistical analysis.

Collier and Hoeffler use three indicators for economic opportunity cost: GDP per capita, male secondary school enrollment, and economic growth. I argue that these are all unsatisfactory indicators of potential rebels' foregone income. First, they do not measure income.[7] Second, they are too broad.[8] Rebel soldiers are not randomly drawn from the population. Recent micro-level studies suggest that most rebels come from poor families (Humphreys and Weinstein, 2008; Arjona and Kalyvas, 2006). Also according to the economic opportunity theory, the poorest should be the first to enlist as rebel soldiers, as they have the least to lose economically. The national level of economic opportunity costs of potential rebels should therefore be closely related to the income level of the poorest class of citizens. Accordingly, I suggest using the mean income (or expenditure) of the lowest income decile, bottom decile income per capita (*BDI pc*), as a cross-national indicator of economic opportunity costs (see Section 3.1 and Appendix, Section 1 for details).

I have emphasized three factors that make counterinsurgency difficult in poor countries: lack of state reach and control; a rural, scattered settlement structure; and poor communications. The first is the most difficult to measure. Often-used cross-national state capacity indicators, like indicators of tax capacity and bureaucratic quality, do not cover this concept well.[9] Tax data says little about the state's reach because in most developing countries the main bulk of taxes are levied on trade rather than on personal

[7] Collier and Hoeffler confusingly use the term "mean income per capita" for GDP per capita. GDP measures production, not income or consumption (c.f. Deaton 2005).
[8] Male secondary school enrollment has the advantage of being focused on young men, who are overrepresented in most rebel groups. However, school enrollment is probably not a good indicator of the economic opportunity cost of rebelling for these young men.
[9] Hendrix (2010), in a recent theoretical and empirical review of state capacity measures used in conflict research, concludes that the most promising indicators are based on tax data and on surveys of bureaucratic quality.

incomes, and custom taxes do not require extensive administrative reach and capacity (Fauvelle-Aymar, 1999: 392).[10] The Political Risk Services Group's measure of 'bureaucratic quality' does not capture my concept of state reach either, since it includes no indicator of the degree to which state administration encompasses the entire population.[11] I instead suggest roads per capita (*road density*) as an indicator of the state's reach of its population. It may serve as a proxy not only for the reach of administrative institutions, but also for the state's opportunity to project military force.[12] Herbst (2000) emphasizes the importance of roads for the broadcasting of state power in Africa. He argues that the low density of roads, which effectively cuts large areas off from the capital, is at the heart of most African states' weakness. Fearon and Laitin (2003: 80) also emphasize that poor countries may be more prone to civil war because their terrain is less "disciplined" by roads.

The second variable, settlement structure, can be proxied by the percentage of the population living in urban areas (*% urban*). The third variable, communication infrastructure, can be proxied by the number of telephone lines per 1000 inhabitants (*telephone density*). Telecommunication infrastructure is clearly related to the flow of information over distance, and probably also to the state's ability to find out about

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[10] Illustrative of the inadequacy of simple taxation indicators is that Angola because of its oil production should have greater state capacity than the average OECD country judging by its tax-to-GDP ratio.
[11] Its indicators include how regular and meritocratic recruitment is, how insulated the bureaucracy is from political pressure, and the ability to provide services during government changes (Knack, 2001).
[12] Clearly, *road density* is not a perfect measure of state reach. I therefore avoid concluding strongly about the role of this particular variable, but rather focus on the composite measure of how accessible the population is to state agents, where road density is one of three components.

insurgents' whereabouts.[13] It may also capture some aspect of state capacity, as it is usually strongly regulated by governments.

None of these three indicators, *road density*, *% urban*, and *telephone density*, sufficiently capture how large a part of the population is easily accessible to the state. Accessibility can rather be seen as a combination of the three indicators. A formative index is therefore appropriate.[14] I construct a simple additive index where the indicators are given equal weights. The indicators were standardized by their minimum and maximum values in the sample before calculating the index.[15] *Road density* and *Telephone density* were logged before standardizing because it is reasonable to assume that they have a decreasing marginal impact on accessibility. The index is defined as the mean of the three standardized variables:

$$Accessibility = \frac{\text{std}(road\ density) + \text{std}(\%\ urban) + \text{std}(telephone\ density)}{3}$$

The two accounts each have one simple implication that can be tested using cross-national quantitative data. According to the economic opportunity account, *GDP pc* is strongly related to the risk of civil war because it acts as a proxy for economic

[13] In the last few years of my sample, the use of mobile telephones could plausibly make telephone lines less important. It is only from the year 2000 that mobile subscribers reach an average of more than 10 percent for the countries in my sample. For 2000-2006, telephone density is still likely to be a decent measure since it is quite closely related to mobile subscription density (r = 0.80).

[14] Unlike the reflective scale, the formative index does not presume a particular pattern of correlation between the indicators. Neither does it presume that the indicators are similarly related to consequences of the latent construct. An important assumption is that all formative indicators are included in the model (Diamantopoulos and Winklhofer, 2001). This is difficult to assess. It may be a problem here if, for example, some part of state control and capacity is not captured by the component variables.

[15] $\text{Std}(x) = \frac{\text{actual } x \text{ value} - \text{min } x \text{ value}}{\text{max } x \text{ value} - \text{min } x \text{ value}}$

opportunity costs. If this account holds, we should therefore observe cross-nationally that:

$H_{Ec.Op.}$: *The statistical relationship between GDP per capita and the risk of civil war is significantly weakened when controlling for BDI per capita.*

The military opportunity account, on the other hand, suggests that *GDP pc* is a proxy for how accessible populations are to the state. If that account holds, we should see that:

$H_{Mil.Op.}$: *The statistical relationship between GDP per capita and the risk of civil war is significantly weakened when controlling for accessibility.*

3. Empirical Analysis

3.1 Data and Operationalizations

Data on *BDI* - the income of the poorest 10 percent - come from household surveys, not national accounts.[16] For low- and middle-income countries, I primarily use survey data reported in the World Bank's Povcalnet (World Bank, 2008a). Povcalnet gives information on mean income per capita converted to PPP 2005US$ and its distribution in deciles. It covers 467 surveys from 114 low- and middle-income countries for 1980-2004. For most high-income countries, I use data from the Luxembourg Income Study (LIS)

[16] Using GDP per capita instead of survey data would have added more country-year observations to the sample. The main reason not to do this is that income in the poorest countries is underestimated in GDP per capita since only a small proportion of their production is registered in the national accounts. See Appendix, Section 1.

(2009). Their public database has survey information for 23 high-income countries.[17] Survey data for Serbia and Montenegro is taken from the UNU/WIDER WIID2c (2008) database.

The temporal extent of the sample is limited by the available survey data. The number of surveys is very low in the 1980s, and increases rapidly in the early 1990s. There is a trade-off between retaining as many country-year observations as possible and getting as reliable information as possible. Taking this into account, I delimit the sample to the 1989-2006 period. I choose 1989 as the starting year because 1989-1993 are particularly valuable years to include as they saw many civil war onsets, and because the BDI country time-series can be stretched back to that point without risking severe measurement error. The average number of years from 1989 to the first survey in a country is 2.9 years. I do not go beyond 2006 because I lack data both for BDI and some other explanatory variables for these years. The number of countries included in the sample is also limited by data availability. I only include the 133 countries with population over 500,000 and with at least one survey observation.

I need to impute many missing values for the country time-series for 1989-2006 (see Appendix, Section 1 for a thorough description of the imputation procedure). The mean number of surveys for the countries in the sample is 3.7, and 16 countries have only one survey observation. I choose interpolation between the survey years as the first step of imputing missing values. In the next step I use the existing BDI survey observations as

[17] It is particularly well suited because it allows me to apply the equivalence scale used in Povcalnet – household per capita.

reference points and extend the time-series using the growth rate of private household consumption expenditure per capita (PPP) from national accounts (World Bank, 2008b). I thus assume that the Lorenz curve does not shift and that the growth rate of national accounts expenditure per capita is similar to that of the survey mean income/expenditure.[18] In the third step, for observations lacking data on private consumption expenditure I use the growth rate of GDP per capita (PPP) to estimate missing BDI observations. After this step, only six country-years (and no onset observations) are left missing.[19] Table 1 gives an overview of the number of BDI observations that are imputed with each method. BDI per capita is log-transformed since it is likely to have a decreasing marginal impact on the risk of civil war.

For *road density*, I use data on the total length of roads per capita from Canning (1998). It combines information from the International Road Federation, the UN, and national sources. I fill in missing observations where possible using World Development Indicators (WDI) data (World Bank, 2008b). Unfortunately, the data series end between 2000 and 2002 for most countries. I lag the last value in each country's time-series for the last 2-5 years. This should not cause discernible error, as the variable changes slowly over time.[20] For *% urban*, the percentage of the population living in urban areas, I use data from WDI (World Bank, 2008b), which is based on national reports. There is little information on their definitions and coding criteria. Kocher (2004: 58) explored this by browsing through the various countries' census classifications. He found that a settlement

[18] This imputation method is used by Chen and Ravallion (2004) for the same survey data. Household consumption expenditure per capita (PPP) is correlated r = 0.90 with BDI. The same correlation is found with GDP per capita (PPP). All variables are logged.
[19] The missing values include five years in Turkmenistan's time-series and Yugoslavia, 1989.
[20] The average annual change is 1.1 %.

is counted as urban if it has more than ca. 2000 inhabitants.[21] The variable has full coverage for the sample.[22] *Telephone density*, the number of telephone lines per 1000 inhabitants, is also taken from WDI (World Bank, 2008b) and has full coverage for the sample.[23]

Gross domestic product per capita (*GDP pc*) is measured in PPP-adjusted constant 2005 USD and taken from WDI (World Bank, 2008b). For states that have experienced border changes, I use the estimates of "Real" (PPP-adjusted) GDP per capita from Gleditsch (2002).[24] The variable is log-transformed since it is reasonable to assume that it has a decreasing marginal impact on the risk of civil war.

Civil war is a disputed concept, but there is wide agreement about some core traits. It is a conflict between a government and one or more domestic armed oppositional groups within a sovereign polity involving violent confrontation of a considerable scale (Kalyvas, 2006: 5; Sambanis, 2004; Small and Singer, 1982: 210). This minimal definition leaves many hard choices for operationalization. I take the UCDP/PRIO Armed Conflict Dataset as my starting point. They define an armed conflict as "an incompatibility that concerns government or territory or both where the use of armed force between two parties results in at least 25 battle-related deaths. Of these two parties, at least one is the government of a state" (Gleditsch et al., 2002: 619). The UCDP/PRIO

[21] Somewhat different thresholds were used. It is unknown how much this affects reliability.
[22] I recalculate the values for states that have experienced border changes since 1989 based on the current state boundaries. This is also done for *telephone density*. WDI only gives values based on present-day states (e.g., 1989 values are given for Russia, not for the Soviet Union).
[23] The time-series were interpolated. This should not be problematic, as the variable changes slowly over time.
[24] I also use PWT 6.3 "Real" GDP per capita estimates (Heston, Summers, and Aten, 2009) for a few country-years missing in WDI 2008.

dataset has the major advantage over most other war lists of being updated to the end of my sample period. It also allows for choosing between different fatality thresholds. It distinguishes between *minor conflict,* having between 25 and 999 battle-related deaths in a year, and *war*, with at least 1,000 battle-related deaths in a year. Strand (2006) has coded armed conflict *onset* based on these data; one *armed conflict onset* version with a low threshold of at least 25 annual battle-related deaths, and a *war onset* version requiring more than 1,000 battle-related deaths in at least one year of the conflict. Whereas 25 annual battle-related deaths do not qualify as large-scale violence – an important trait of civil wars – 1,000 annual battle-deaths may be too strict. It excludes some conflicts displaying most characteristics of civil wars, like the one in Croatia after its independence in 1992. In my sample, there are only 26 civil war onsets using this operationalization, whereas there are 101 armed conflict onsets using the low-threshold operationalization.

I choose a somewhat lower and more flexible fatality threshold, as suggested by Sambanis (2004: 829-830). Onset of civil war is coded in the first year a conflict causes more than 500 battle-related deaths. If it causes less than 500 deaths (but at least 25 deaths) in its first year, that year is still coded as an onset if cumulative deaths over the following three years reach 1,000.[25] The conflict must have at least two years of non-activity before a new onset is coded.[26] Since Sambanis' dataset is not updated beyond 1999, I use the UCDP/PRIO conflict data with the corresponding PRIO battle deaths

[25] I also test the robustness of the results to this choice by using an onset variable where onset is coded in the year when cumulative deaths over the last three years reach 1,000.
[26] Years with less than 25 battle-related deaths are coded as years of non-activity. I also check the robustness of the results using a version using a 4-year intermittency period.

dataset for coding the variable (Lacina and Gleditsch, 2005). There are 48 civil war onsets in my sample using this definition. Although this is my preferred operationalization, I check the robustness of the results using Strand's (2006) operationalization with a threshold of 1,000 deaths in at least one single year of the conflict.

3.2 Descriptive Analysis

The utility of statistical analysis depends on the degree of independent variation between the central independent variables, *accessibility*, *BDI pc*, and *GDP pc*. If they cannot be empirically distinguished, statistical analysis can only tell us that *GDP pc* may be a proxy for both *accessibility* and *BDI pc*. Table 2 shows the correlations between the variables. *GDP pc* and *accessibility* are quite highly correlated (r = 0.91), but there is more independent variation between *GDP pc* and *BDI pc* (r = 0.86) and even more between *accessibility* and *BDI pc* (r = 0.81). This independent variation is, as shown below, enough to give significantly different relationships between these variables and the risk of civil war onset.

When analyzing a rare event like the onset of civil war, the most valuable information is found in the onset cases. Figure 1 shows deviation from mean *BDI pc* and *accessibility* for the country-years with civil war onsets from 1989-2006, with the number of onsets in parenthesis for countries with more than one onset.[27] The line shows the fitted values from a regression of *accessibility* on *BDI pc* for the entire sample, including the non-

[27] For countries with more than one onset, the average values of *BDI pc* and *Accessibility* for the onset years are shown. I chose not to plot every country-year with onset, since very many labels would be hidden.

onsets. The onset cases above the line have a higher *accessibility* than would be expected from their *BDI pc* values and the cases below the line have lower *accessibility* than expected from *BDI pc*. The graph clearly shows that onset cases tend to have relatively lower *accessibility* scores than *BDI pc* scores.[28] In total, 37 out of 48 onset country-years have lower *accessibility* than predicted from their *BDI pc*. Most countries with civil wars in this period, like Sri Lanka, Yemen, Nepal, Ethiopia, and Rwanda, are relatively poor, but what really sets them apart from the average country is their remarkably low accessibility. This may indicate that a*ccessibility* is more strongly related to the risk of civil war than is *BDI pc*. I next show this statistically using regression analysis.

3.3 Regression Analysis

I test the hypotheses using pooled regression analysis of my cross-national time-series data for 133 countries from 1989 to 2006. The observations are pooled because the explanatory variables change relatively little over time, making within-country comparison futile. Standard errors are clustered on countries and estimated using a robust "sandwich" estimator.[29] The cross-national design is chosen because we don't have disaggregated data for the variables of interest with good enough spatial and temporal coverage to allow for large-N testing. Aggregating to the national level may cause measurement problems since civil wars may take place in a limited area of a country which does not closely resemble the country-mean values on the explanatory variables

[28] Plotting *GDP pc* against *BDI pc* gives a quite similar figure, whereas a figure with Accessibility and GDP per capita shows the onset cases laying much closer to, and more equally distributed around, the regression line. This indicates that both *GDP pc* and *accessibility* have a stronger relationship to the risk of civil war than *BDI pc* has, and that *GDP pc* and *accessibility* are closely related, as expected from the military opportunity account.

[29] I use a general estimating equation model with a logit link function and a robust "sandwich" estimator, specifying the correlation structure as independent (Zorn 2001).

(Buhaug and Lujala, 2005). However, there is reason to believe that the aggregation problem is not overwhelming. Gleditsch et al. (2010) find that the global relationship between development and the risk of civil war holds when using geographically disaggregated measures. Also, they find that civil wars arise both in relatively wealthy and relatively poor areas of a country, indicating that country-level aggregates do not yield a strong systematic bias.

Quantitative conflict studies often include a host of control variables in every regression model. This is likely to be problematic here because of the limited sample size. Including many controls would reduce the degrees of freedom and further increase the risk of accepting a false null hypothesis. Excluding some of the often-used control variables may increase the risk of omitted variable bias, but I would argue that the risk is only significant if there are strong theoretical reasons to expect that a confounding variable is omitted.[30] I therefore only include the two most significant standard control variables: total *population* (logged) and *proximity of war*.[31] Population figures are taken from WDI (World Bank, 2008b).[32] *Proximity of war* is a decay function of the time since the last year of civil war in the country, or, if the state has not experienced any civil war, since independence (Raknerud and Hegre, 1997: 393).[33]

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[30] Including a host of control variables on a weak theoretic basis has its own pitfalls. We do not know whether they reduce or rather increase omitted variable bias (Clarke, 2005), and they make results more difficult to interpret (Ray, 2005).

[31] GDP per capita is seen as a variable of theoretical interest in my models. I also include one robustness check where I use all the variables in Fearon and Laitin's (2003) first model as controls (see Figure 5).

[32] For historical states or states that have undergone large border changes I use Kristian Gleditsch's (2002) population estimates.

[33] I use the high-threshold UCDP/PRIO definition of civil war as a basis for this control variable because of the difficulty of making a complete civil war list based on the Sambanis definition with the UCDP/PRIO data. The difference would be relatively small, and the variable has very little influence on the coefficients

First, I look at the relationship between the central independent variables and civil war onset by estimating several separate logit models that include only one variable of interest and controls for *population* and *proximity of war*.[34] Figure 2 shows the risk ratio (RR) for the central independent variable of each separate model. The risk ratio is here defined as the ratio of the probability of onset when the central *x* is at the upper quartile to the probability of onset when the central *x* is at the lower quartile, with control variables held at their means.[35] A higher *GDP pc* is strongly related to a lower risk of civil war. Its RR estimate of 0.41 means that countries in the upper quartile of *GDP pc* have an estimated 59 % lower probability of civil war onset than countries in the lower quartile of *GDP pc*, holding *population* and *proximity of war* at their mean levels. The 95 % confidence interval around the estimate, shown by the line, is well below a risk ratio of 1 (no risk difference). The estimated relationship between *BDI pc* and the risk of civil war onset is weaker and much less certain. The RR mean estimate for *BDI pc* is 0.69 and it is not significantly different from 1 at the 95 % level (the confidence interval spans a RR of 1). The *accessibility* index, on the other hand, has an estimated risk ratio of 0.40, and is clearly significant. *Telephone density* and *% urban* have a risk ratio in the same range (RR=0.44 and RR=0.41 respectively). *Roads density* has a somewhat higher risk ratio (RR=0.61), but it is clearly significantly different from 1.

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of interest whether I use the UCDP/PRIO low or high threshold. The half-life of the decay function is set to eight years.

[34] Regression coefficients for the models are found in the Appendix, Table A3.

[35] Risk ratio estimates are made using simulations with Zelig (Imai, King, and Lau, 2006).

The Model with *BDI pc* also has significantly worse fit to the data than the models with *accessibility* and *GDP pc*. This can be seen by the area under the ROC curve (AUC), which is smaller for the *BDI pc* model (AUC = 0.746) than for the *accessibility model* (AUC = 0.776) and the *GDP pc* model (AUC = 0.771). This difference between the *BDI pc* model and each of the other two models is significant at the 5 % level. The AUC values of the *GDP pc* and *accessibility* models, on the other hand, are not significantly different (p = 0.48).

Next, I estimate models where two of the three central variables are included in addition to the two standard control variables.[36] Figure 3 shows risk ratio estimates, as defined above, for each of the variables in these models, holding all the other variables at their mean values.[37] In Model 1 (shown with black dots), *GDP pc* and *BDI pc* are included. The results speak strongly against the idea that the development-civil war relationship can be attributed mainly to a low *BDI pc*, or a low economic opportunity cost for the poor. The significantly negative *GDP*-civil war association actually becomes stronger when controlling for *BDI pc* (mean RR = 0.22), contrary to the economic opportunity hypothesis. The relative risk estimate of *BDI pc* is highly uncertain, with most but not all of the confidence interval stretching across *positive* RR values. The mean RR estimate is 3.06, meaning that the risk of civil war is tripled when *BDI pc* increases from the lower to the upper quartile, holding *GDP pc* at the mean. It is not statistically significant, however.

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[36] Regression coefficients for the models are found in the Appendix, Table A4.

[37] It would have been interesting to simulate effects when holding the other variables at other levels as well, but this would mean invoking counterfactuals very far from the actual data. Using the "WhatIf" software (King and Zeng, 2006) I found that the percent of observations 'nearby' the counterfactual sank considerably when holding the other central variables more than +/-10 percentiles from their means. All the models included here are based on counterfactuals with more than 10% of the data in the convex hull (see Stoll, King, and Zeng 2009).

In Model 2 (white circles), *accessibility* is included and *BDI pc* is excluded. The results are in support of the politico-military opportunity hypothesis. Controlling for *accessibility*, the relationship between *GDP pc* and civil war disappears. The close association between *GDP pc* and accessibility makes both their estimates highly uncertain. For both variables the confidence interval crosses RR = 1, meaning that they have no statistically significant relationship with the risk of civil war when holding the other variable constant. The mean of the RR estimates is 0.99 for *GDP pc*, whereas it is 0.57 for accessibility. However, we should not emphasize these point estimates much, given the great uncertainty surrounding them.

In Model 3 (squares), *GDP pc* is taken out and *BDI pc* is taken in together with *accessibility*. The relative risk for *BDI pc* is not significantly different from 1, but the mean of the estimates is strongly positive (RR = 2.32). *Accessibility*, on the other hand, has a strong and clearly significant negative relationship with the risk of civil war. The mean relative risk is 0.25, suggesting that countries in the upper quartile of *accessibility* has 75 % less chance of civil war than countries in the lower quartile, holding *BDI pc* at its mean level. This suggests that *accessibility* can plausibly account for the relationship between *BDI pc* and civil war onset, but not vice versa.

3.4 Robustness Checks

I test whether the results are robust to changes in control variables and operationalization of the dependent variable below. Figure 4 shows risk ratio estimates for *BDI pc* and *accessibility* from models in which they are both included.[38] When estimating the risk ratio for one variable, all other variables are held at their means. In Model 1, the battery of variables from Fearon and Laitin's first model (2003: 84) are included as controls: *new state* dummy, *noncontiguous state* dummy, *% mountainous terrain* (logged), *democracy* (Polity2, from -10 to +10), *instability* dummy (1 if >2 points change in Polity2 scale the last three years), *ethno-linguistic fractionalization*, *religious fractionalization*, *prior war* dummy (1 if civil war in the previous year), and *oil dependency* dummy (1 if oil exports >15% of GDP) (see Fearon and Laitin, 2003: 78-81 for details).[39] The results with these controls (shown with black dots) are even more supportive of my hypotheses. A higher *BDI pc* is significantly related to a higher risk of civil war, with a mean RR estimate of 3.33. *Accessibility* has a mean RR of 0.13; an increase in *accessibility* from the lower to the upper quartile gives an 87 % reduction in the risk of civil war, holding *BDI pc* and the control variables constant.

In Model 2 I test a different operationalization of the dependent variable. It uses my preferred onset definition (UCDP/PRIO's definition and data with Sambanis' death threshold), but increases the period of non-activity needed for a new onset to be coded to four years (from two in the original). The results are broadly similar to the ones in Model 3 of Figure 3, just even more clearly supportive of my hypothesis. *Accessibility* has a significant negative relationship to the risk of civil war controlling for *BDI pc*, whereas

[38] Regression coefficients for the models are found in the Appendix, Table A5.
[39] Since Fearon and Laitin's (2003) time-series stop in 1999, I recoded the variables that change over time to avoid losing much of the sample. The Oil dependency dummy is somewhat differently specified; they mark country-years in which oil exports exceed 1/3 of export revenues, whereas I mark country-years in which oil exports exceed 15% of GDP. The Democracy and Instability variables are recoded using updated data from PolityIV, 2007.

BDI pc in fact has a significant positive relationship to the risk of civil war controlling for *accessibility*.

In Model 3 I use an operationalization of the dependent variable where onset is coded in the year when cumulative battle-related deaths over the three last years reach 1,000 (in the original, onset is coded in the first year if the next three years lead to >1000 deaths). The positive risk ratio of *BDI pc* is no longer significant, but the main result remains stable: *accessibility* has a significant negative effect, whereas *BDI pc* does not.

Model 4 uses the civil war onset operationalization of Strand (2006) based on the UCDP/PRIO data (>1,000 battle-related deaths in at least one year of the conflict). This gives only 25 onsets in the sample (compared to 48 with the preferred onset operationalization). The estimates are therefore less certain, as seen from the wider confidence intervals. *Accessibility* still has a risk ratio below 1 which is (barely) significant and the mean risk ratio estimate of *BDI pc* remains positive.

4. Conclusion

This article has assessed two possible explanations of the development-civil war relationship: the *economic opportunity* account, suggesting that poverty facilitates the formation of a rebel organization since the poor have a lower economic opportunity cost of rebelling, and the *politico-military opportunity* account, suggesting that the inaccessibility of populations and the low reach of the state in developing countries give insurgents the opportunity to hide from the militarily mightier state forces as well as to establish local control which provides powerful tools for mobilizing resources. Based on the case study literature, I argued that the latter account is more powerful than the former. By focusing on a single pre-war characteristic of the population – poverty – the economic opportunity account largely ignores the *politics* of resource mobilization. Armed organizations use several tools – persuasion, coercion, organization, and economic rewards – to mold the incentives and allegiances of people. Although low economic opportunity cost might ease recruitment, it may not be necessary if insurgents can use such tools effectively. Their ability to do so depends foremost on accessibility and state capacity, rather than poverty. Adding to this, without the ability to hide, insurgents can hardly survive through the initial phase of mobilization.

I carried out a test of cross-national implications of the two accounts using data for 133 countries from 1989-2006. I argued that the level of economic opportunity cost of rebelling can be proxied by the income of the poorest segment of society – the bottom decile income per capita (*BDI pc*) – since the poorest have the lowest foregone income and should therefore be the first to rebel according to the economic opportunity theory. The *accessibility* of the population to state agents, the central variable in the military opportunity account, was measured by an index composed of *road density*, the *% urban* of the population, and *telephone density*. I found that *Accessibility* was more closely associated with economic development than was *BDI per capita*. Moreover, *BDI per capita* could not account for the cross-national statistical association between *GDP per capita* and the risk of civil war, whereas *Accessibility* could. The results are robust to theoretically reasonable changes to model specification, dependent variable operationalizations, and imputation method. This suggests that although civil war is largely a “problem of the poor”, poverty *per se* is probably not the crux of the problem. State infrastructural weakness and a rural settlement pattern are likely to be more important for explaining the high prevalence of civil war in low-income countries.

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Tables and Figures

Table 1. Number of imputed BDI per capita Observations

Source / imputation method	N
Survey	495
Interpolation between survey observations	1038
Predicted using growth rate of household expenditure pc (national accounts)	491
Predicted using growth rate of GDP per capita, PPP (national accounts)	299
Total	2323
Missing after imputation	6

Table 2. Correlations (n = 2321)

	GDP pc (ln)	Access	BDI (ln)	Urban %	Roads pc (ln)	Telelin pc (ln)	Pop (ln)	Prox war
GDP pc (ln)	1.0000							
Accessibility	0.9136	1.0000						
BDI (ln)	0.8587	0.8141	1.0000					
Urban %	0.7900	0.9084	0.6872	1.0000				
Roads pc (ln)	0.6640	0.7504	0.6031	0.5064	1.0000			
Telelines pc (ln)	0.9018	0.9242	0.8141	0.7669	0.5923	1.0000		
Pop (ln)	0.0543	-0.0093	0.1063	0.0846	-0.2279	0.0378	1.0000	
Prox war	-0.2647	-0.2165	-0.2203	-0.2059	-0.1673	-0.1869	0.1510	1.0000
Onset	-0.0928	-0.1018	-0.0473	-0.0897	-0.0858	-0.0907	0.1099	0.1301

Table 3. Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
GDP pc (ln)	2321	8.278749	1.232466	5.492889	10.76928
Accessibility	2321	.5817043	.1826284	.1405593	.9458329
BDI (ln)	2321	6.029386	1.241918	3.630888	8.768187
Urban %	2321	49.76813	22.64077	5.28	97.2
Roads pc (ln)	2321	1.474552	.921082	-2.170322	3.909234
Telelines pc (ln)	2321	1.485339	1.900966	-4.037572	4.310293
Pop (ln)	2321	16.23731	1.442387	13.11547	20.98909
Prox war	2321	.2977075	.3774683	.0055243	1
Onset	2321	.0206807	.1423439	0	1

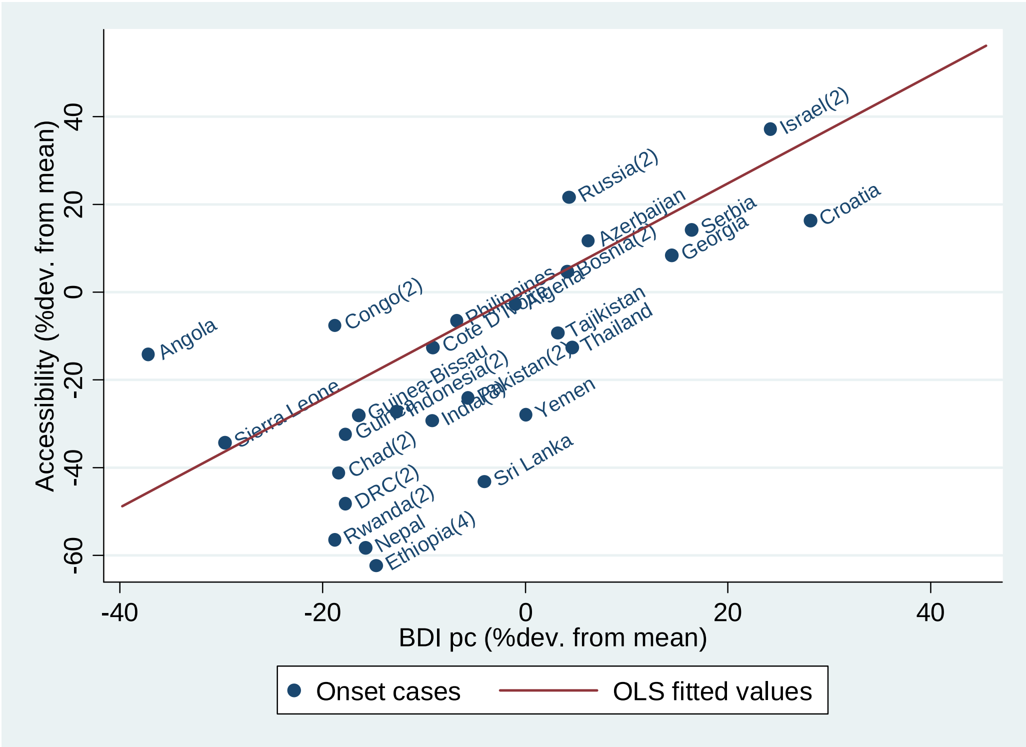
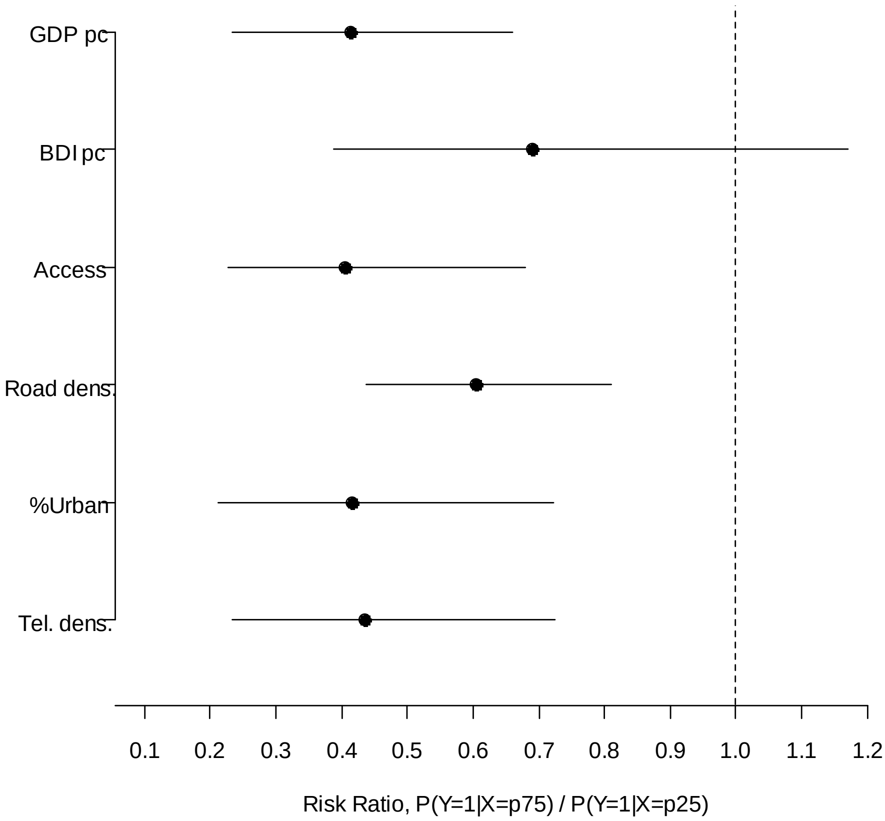


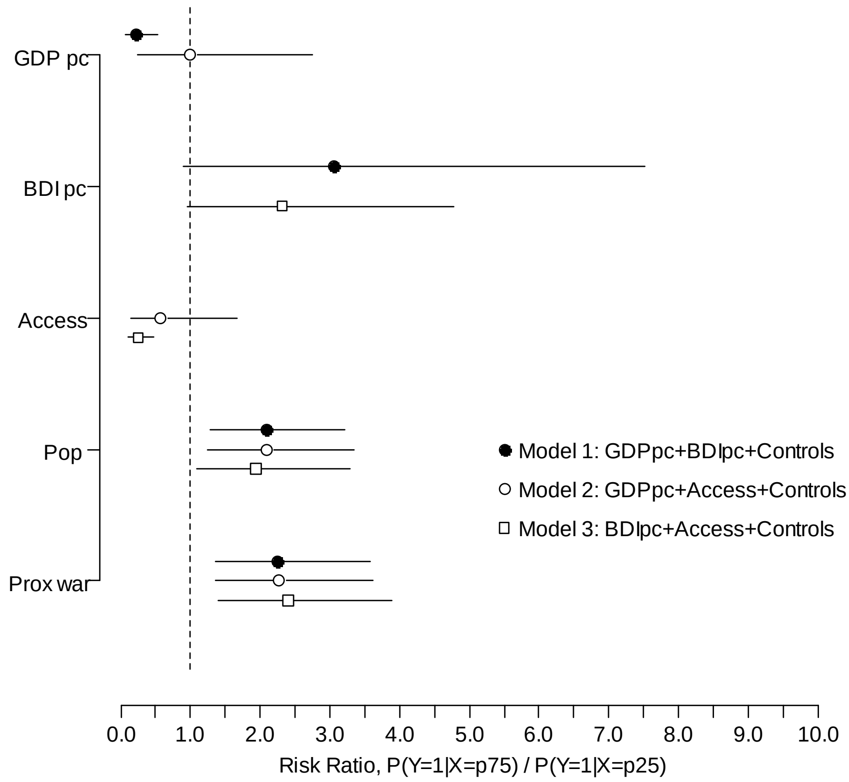
Figure 1. Deviation from Mean Accessibility and BDI per capita for onset cases, 1989-2006.

Figure 2. Risk ratio Estimates for Central Variables from Separate Logit Models



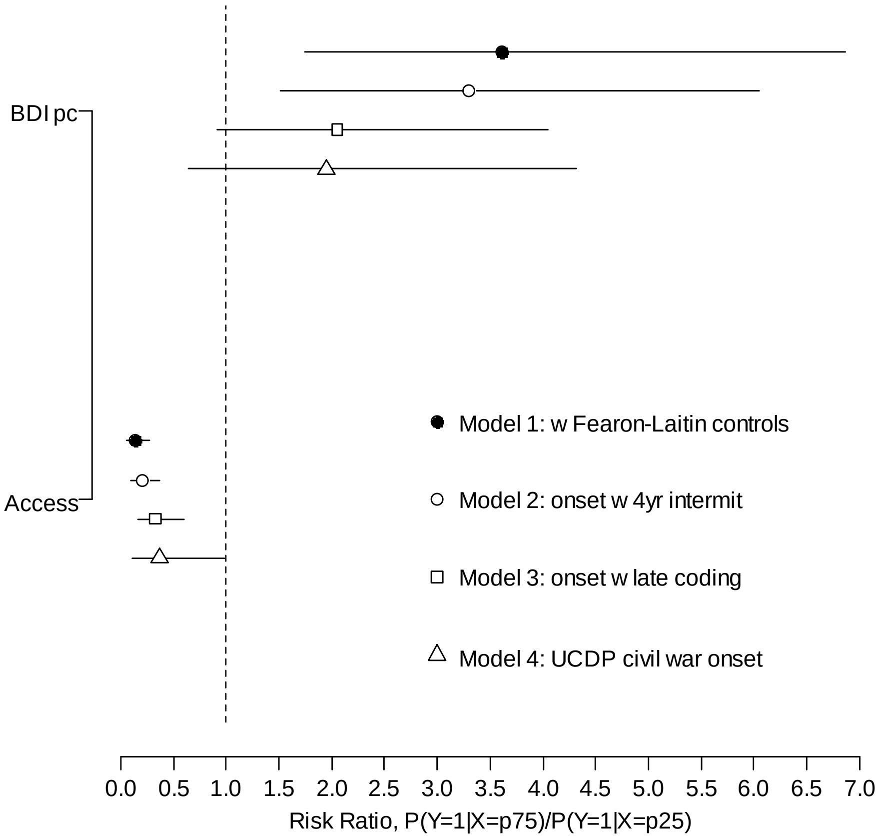
Note: Each estimate derives from a separate model including only the variable shown and two control variables, population and proximity of war. The horizontal axis shows the ratio of the probability of onset when the central *x* is at the upper quartile to the probability of onset when the central *x* is at the lower quartile, with control variables held at their means. The dots show the mean of the estimates and the lines show 95% confidence intervals.

Figure 3. Risk ratio estimates for all variables in three multivariate logit models



Note: The figure shows relative risk estimates for all the x variables in three models. The models are distinguished by the shape and color of the dots. The horizontal axis shows the ratio of the probability of onset when x_i is at the upper quartile to the probability of onset when x_i is at the lower quartile, with all other x variables held at their means. The dots show the mean of the estimates and the lines show 95% confidence intervals.

Figure 4. Risk ratio estimates for BDI pc and Access from 4 models



Note: The figure shows relative risk estimates for all the *x* variables in three models. The models are distinguished by the shape and color of the dots. The horizontal axis shows the ratio of the probability of onset when x_i is at the upper quartile to the probability of onset when x_i is at the lower quartile, with all other *x* variables held at their means. The dots show the mean of the estimates and the lines show 95% confidence intervals.