Economic Development, Rebel Mobilization, and Civil War Onset

Helge Holtermann

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Department of Political Science
University of Oslo

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Oslo, 18 January 2013

Introduction

Introduction

Purpose

Civil war is predominantly a "problem of the poor" (Sambanis 2002:216). While extremely rare in high-income countries, it is quite commonplace in poorer parts of the world (Figure 1). This pattern may partly be explained by the fact that civil war harms the economy, but scholars largely agree that the more important reason is that civil war is more likely to begin in poorer countries (Collier et al. 2003: 53). Underpinning this view, a low level of economic development, as measured by GDP per capita, has been found one of the strongest determinants of civil war onset in cross-national studies (Hegre and Sambanis 2006). Yet, while there is considerable agreement that economic development is linked to a decreased likelihood of civil war onset, there is no agreement about what causal pathways underlie the association.

Several pathways are conceivable partly because economic development has historically tended to go hand in hand with social and institutional change (Polanyi 1944; North, Wallis, and Weingast 2009). This makes it difficult to identify which parts of the "economic development complex" (Lipset 1959:71) are causally related to intrastate peace. Two accounts have been especially influential in the recent literature. The first, which I refer to as the *economic opportunity cost account*, holds that poverty creates financial opportunity for large-scale armed rebellion because rebel combatants must be paid, and the cost of rebel labor is lower where people have meager opportunities in the regular labor market. The second, which I refer to as the *politico-military opportunity account*, posits that poor countries are more prone to civil war because they tend to have weak states, a rural settlement structure, and poorly developed infrastructure, which makes it possible for insurgents to carve out pockets of control where they can mobilize the resources needed to fight a war. Other accounts have also been suggested. Grievance arguments posit that poorer countries tend to have more deprived and frustrated populations – due to poverty, discriminatory governance, or other conditions – that can more easily be mobilized for rebellion. Finally, institutional arguments hold that

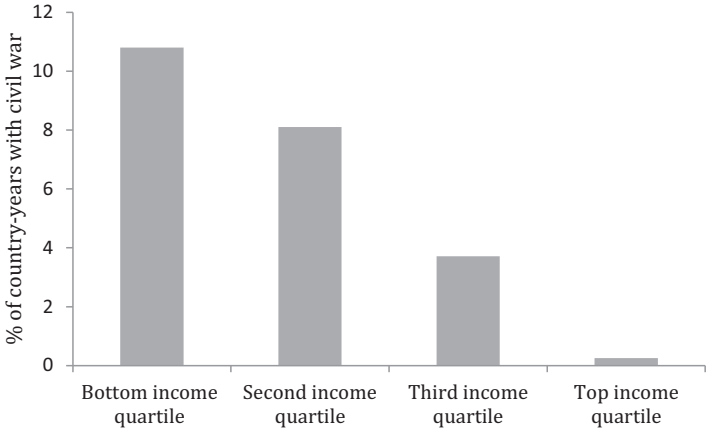

Figure 1. Incidence of Civil War by GDP per Capita Quartiles, 1980-2009
Note: The figure shows the percent of years between 1980 and 2009 with civil war for four income categories. The country income quartiles are based on GDP per capita (PPP) in constant 2005$ (WDI 2010). Civil war is defined as an internal conflict with > 1000 annual battle-deaths and based on UCDP data (Harbom and Wallensteen 2010).

poor societies are typically marked by personalized and dispersed power, and violence is restrained by fragile elite agreements that can easily break down during crises.

When I started this dissertation project, some empirical research had looked at these arguments, but much remained in terms of identifying the important causal variables and the processes and mechanisms that link them to civil war onset. The most widely noted finding in the cross-national civil ware literature was therefore still poorly understood.

Research Questions and Outline

The main purpose of the dissertation is to contribute to our understanding of why poorer countries are more prone to civil war than wealthier ones. More precisely, it seeks to shed light on *which important causal variables, processes, and mechanisms underlie the observed association between lower levels of economic development and a higher likelihood of civil war onset*. To clarify, causal variables are observable attributes of a certain unit of analysis that affect the likelihood of an outcome – civil war onset in this case. Processes can be defined as sequences of events leading to some macro-level outcome. The onset of civil war depends on several processes, like organization-building, mobilization, and bargaining. Mechanisms tell us how and why a variable, in a given context, contributes to a

particular outcome. They say, for instance, how actors influence each other, how their beliefs are formed, and how they make their choices.[1]

These are big questions, and the dissertation does not attempt to assess every possible argument that might help answer them. It focuses mainly on the politico-military opportunity and economic opportunity cost accounts, since they are most prominent in the quantitative literature. Following these accounts, I give most attention to the processes of insurgent mobilization and expansion after the initiation of a violent political conflict with the government. These are not the only relevant processes for civil war onset. Still, they are important, because rebel organizations rarely start out with the capacity to give effective military resistance to the state (Sambanis 2004a:267).[2] The articles relate to different parts of the overarching question, and fill different gaps in previous research.

Article I explores how within-country variations in economic development relate to civil armed conflict onset. Previous global studies looking at how economic development relates to civil armed conflict onset have used measures aggregated to the country level. Yet, some of the explanatory variables that might underlie the development-civil war association, like poverty and accessibility, tend to vary within countries. Other possibly relevant variables, like central state capacity and political institutions, do not. If any of the first set of variables were important, we should expect that civil wars tend to break out in less developed areas within countries. This article assesses this implication using a global, geographically disaggregated design.

In *Article II*, I test more specific implications of the politico-military opportunity and the economic opportunity cost accounts at the country level. In previous cross-national research, GDP per capita has been used as a proxy for very different variables: state capacity and reach (Fearon and Laitin 2003) or economic opportunity cost of rebelling (Collier and Hoeffler 2004). Yet, little evidence has been provided to support either interpretation. This article develops more specific indicators of state reach and economic opportunity costs, and assesses whether these variables could plausibly be underlying the development-civil war association.

While cross-national analyses of civil war onset are useful for identifying plausibly relevant explanatory variables, they offer little insight into the processes leading up to civil war. In order to grasp these processes and their links to development-related conditions, I collect and analyze fine-

[1] This conceptualization is closely related to that of Falleti and Lynch (2009:1143, 1147).
[2] I discuss exceptions to this in the concluding section.

grained data from the Maoist insurgency in Nepal, which turned into a civil war claiming more than 13,000 lives.

Article III uses empirics from Nepal to develop theory about how and where insurgencies over central government emerge and spread within a country over time. A general proposition is that the dynamics of insurgency depend on the relative military capacity of the belligerents. I derive several hypotheses from this starting point. Most relevant for the overall dissertation, I suggest that rebel activity should tend to emerge in less accessible areas when the rebels are militarily far weaker than the state. This follows from politico-military opportunity accounts. However, I hypothesize that the relationship between inaccessibility and insurgency is a contingent one: as the rebels gain military strength, inaccessibility should become less important, because stronger rebels have less need to hide from state forces.

Article IV takes a deeper look at the processes and micro-level mechanisms of the politico-military opportunity account. The key to the survival and growth of insurgency is the ability of rebels to carve out controlled "base areas", according to prominent politico-military opportunity accounts. The main reason, they suggest, is that control spurs local collaboration by making threats of punishment and promises of protection credible. Yet, even in remote areas, it is very difficult for militarily weak rebels to establish control, since a stronger state can transfer forces to rebel-affected zones. How, then, can weak rebels survive and gain strength? This article explores this puzzle through a fieldwork-based study of insurgency processes in a hamlet within the epicenter of the Maoist insurgency in Nepal.

The articles are described in more detail later in this introduction. Below, I turn to defining central concepts. Next, I provide a broad review of the existing literature, also discussing new studies appearing over the course of the dissertation work. Then I briefly present the overall mixed-methods design of the dissertation and outline each article. Some overall conclusions are then drawn by looking at the findings of all the articles in combination. Finally, I suggest some directions for future research.

Central Concepts

Civil War and Insurgency

There is no clear consensus about what constitutes a civil war. Most definitions agree that it involves armed combat within the boundaries of a recognized sovereign entity between government-affiliated forces and one or more non-state organizations (Gleditsch et al. 2002:618-619; Kalyvas 2006:17; Small and Singer 1982:210). Yet, these criteria do not unequivocally distinguish civil wars from other forms of collective violence. For this, additional, disputed criteria are needed, which typically concern the scale of violence, the degree of organization of the non-state actors, and the level of violent resistance given by the non-state actors (Sambanis 2004b:815-816). In practice, a major distinction between operational definitions is the choice of fatality thresholds. The much-used UCDP/PRIO armed conflict dataset separates between "minor armed conflicts", which claim between 25 and 999 yearly deaths in battles, and "wars", which see at least 1,000 yearly battle-deaths. This dissertation, like many other studies, does not use such a strict definition of civil war. *Article I* includes all UCDP/PRIO internal armed conflicts but sometimes refers to them as civil wars. *Article II* focuses on larger-scale armed conflicts, since those are most relevant for the two specific arguments it assesses. Accordingly, it uses a higher threshold of violence in its operationalization of civil war onset. The articles based on empirics from Nepal also refer to civil war as a condition of intensive political violence, but they do not rely on a strict operational definition, since they focus on the continuous process by which insurgency grows and spreads over time.

Insurgency can be defined as an armed rebellion in which the rebels rely primarily on irregular warfare, tending to avoid decisive battles with government forces and rather engaging in hit-and-run attacks using light weapons (Kalyvas and Balcells 2010). The dependent variable in *Article III*, *insurgency onset*, is defined as the beginning of rebel activity that includes lethal violence in an area within a country.[3]

The State, State Capacity, Reach, and Control

Most scholars agree that *the state* basically consists of a set of administrative, policing, and military organizations formally headed by a government with internationally recognized authority within a limited territory. However, some argue that the concept must be reserved to organizations in which

[3] A low-level threshold of five days of lethal insurgent violence within a half-year period is chosen, since I am not concerned with the intensity of violence, but the beginning of armed rebel activity.

the formal government *in fact* controls the state apparatus, is relatively autonomous from societal groups, enjoys a monopoly of (legitimate) violence, and implements decisions through an impersonal, rule-based bureaucracy (Chabal and Daloz 2006:226-245; Nettl 1968). These scholars follow Weber in emphasizing "empirical statehood", as opposed to "juridical statehood", which merely necessitates international recognition (Jackson and Rosberg 1982). I apply the broader juridical conceptualization. Rather than distinguishing between degrees of statehood or "stateness", I conceptualize important differences between states mainly in terms of different state *capacity* or *reach*. Both concepts are related to what Mann (1984:113) calls infrastructural power: "[…] the capacity of the state to actually penetrate civil society, and to implement logistically political decisions throughout the realm."[4]

For some purposes it can be useful to distinguish between different types of "infrastructural" state strength. A state's economic, coercive, and administrative capacity need not fully overlap (Evans, Rueschemeyer and Skocpol 1985:352), and its power may not be equally distributed across its territory. It may have high central capacity, but limited reach into rural areas. State reach, in turn, consists of two aspects: First, the presence of state agents and institutions throughout its inhabited territory, and second, the ease by which the state can project power throughout its territory. The opportunity for the state to project power to a specific area is referred to as "accessibility" in what follows.

I follow Kalyvas' (2006:111) definition of control as "the extent to which actors are able to establish exclusive rule on a territory", as well as his operationalization, which focuses on the military presence of an armed organization and its ability to prevent other armed organizations from operating in an area. Importantly, the term refers to local military power and not the degree to which organizations actually obtain collaboration by the civilian population (which is to be explained).

[4] This concept of capacity is separated from the state's autonomy from society. Mann (1984:113) refers to the latter as "despotic power": the extent to which state elites can make decisions without routine consultations with civil society groups. Clearly, it is possible to have high infrastructural power but low despotic power, as in most present-day capitalist democracies.

Existing Arguments

Arguments that are potentially relevant for explaining the association between poverty and civil war abound, and all of them cannot be discussed here. This overview concentrates on four types of arguments that are prominent in the literature.[5]

Economic Opportunity Cost of Rebelling

After the Cold War, many scholars and journalists argued that the nature of war has changed, and in the contemporary "new wars", rebels tend to be hardly distinguishable from criminals (Kaplan 1994; Mueller 2004). Simultaneously, some economists built general models of rebellion and civil war based on the idea of "rebellion as crime", in which greed rather than grievance motivates rebel combatants (Collier 2000; Grossman 1999). Later influential work by Paul Collier and colleagues abandons the assumption that rebels are always motivated by greed, and rather explains civil war by pointing to what makes large-scale armed rebellion *financially feasible* (Collier and Hoeffler 2004; Collier, Hoeffler, and Rohner 2009). In this framework, poverty makes rebellion viable mainly because it reduces the economic opportunity cost of becoming a rebel combatant.[6] In Collier and Hoeffler's (2004:659) words, "[r]ecruits must be paid, and their cost may be related to the income foregone by enlisting as a rebel". A central implicit assumption is that individuals freely decide whether to spend efforts on fighting or production based on maximization of expected private economic utility. When opportunities in the regular labor market are low, the supply of rebel labor increases, which in turn reduces the costs of forming and maintaining a rebel army.

One objection to this argument has been that poverty would reduce the cost of hiring soldiers for the government as much as for the rebels, given the economic recruitment logic (Fearon 2008). If the ratio of belligerents' forces determines the potential for civil war, the level of income might therefore not matter. However, it could be that rebel organizations are more sensitive to labor costs than governments, since they often use more labor-intensive technologies of warfare. Moreover, government revenues might increase more than proportionately with income levels, thereby indirect-

[5] Additional arguments are discussed in a review of the literature on poverty and conflict that I have co-authored with Håvard Hegre (Hegre and Holtermann 2012).

[6] Notably, in the latest version of the theory, the authors suggest that military feasibility of rebellion could provide an additional link between low levels of economic development and civil war, since poor countries tend to have lower state capacity and control (Collier, Hoeffler, and Rohner 2009:4).

ly benefitting the government (Collier 2000:849). The argument therefore does not appear to be easily dismissed on logical grounds alone.

Politico-Military Opportunity for Insurgency

Other influential accounts of the poverty-civil war nexus highlight the political and military opportunities for oppositional groups to challenge the government through insurgency. Fearon and Laitin (2003) argue that rebels, because of their initial military inferiority to the state, must be able to hide from state forces while mobilizing. Whether this is feasible depends foremost on the state's police and military capabilities and the reach of state institutions into rural areas, they argue. Where states have a weak presence, the rebels can establish pockets of control, or base areas, which helps them mobilize resources and hinder information flows to state agents (Fearon and Laitin 2003:76).[7] This argument builds on Kalyvas' (2006) theory of control and collaboration during civil war, which holds that most people learn to prioritize security over other values in wartime.[8] Control can therefore effectively spur collaboration by giving credibility to threats of sanctions against individual defectors and promises of protection of collaborators.[9]

Kocher (2004) applies a similar logic, but argues that the opportunity for insurgency depends primarily on settlement patterns rather than state capacities. Only in rural societies where a large proportion of people live in scattered settlements can weak insurgents assume control over significant settlements, which allows them to extract information, manpower and material resources from local populations, he argues. Fearon (2008) similarly emphasizes that "social terrains" characterized by rural settlements poorly connected by roads and communication networks favor insurgency.

Grievances and Rebel Support

Mass discontent is often claimed to be a central ingredient in civil wars (Gurr 1970; Regan and Norton 2005). Although what sparks grievances may vary according to cultural norms of justice and

[7] Fearon (2008) argues that the predominantly agricultural economy of poor countries also favors rebellion, since immobile assets tied to the land are possible to tax through house-to-house visits, the typical insurgent method of taxation. Boix (2008), on the other hand, argues that abundance of immobile assets increases the chance of political violence by increasing the benefits of violent expropriation and the stakes of political contestation.

[8] Kalyvas' theory was made known through articles and working papers years before the publication of his book in 2006.

[9] Although coercion and protection is emphasized, Kalyvas (2006:124-131) suggests five additional mechanisms by which control may produce collaboration: socialization and information monopolies "mechanically" shaping allegiances; credibility of rule in the short and long term; the provision of benefits to collaborators; monitoring and population control; and self-reinforcing dynamics.

rightful entitlements (Gurr 1970:13), it is plausible that various conditions typical for poor countries often produce discontent and frustration. Some suggest that poverty directly spurs discontent (Gurr 1970:131), while others point to factors like patron-client structures associated with local exploitation and social immobility (Richards 2005), governmental favoritism and discrimination of ethnic or regional constituencies (Cederman, Weidmann, and Gleditsch 2011), and a general shortage of public goods provision (Taydas and Peksen 2012).

The most common objection to grievance accounts of rebellion stems from Olson's (1965) free-rider problem of collective action: even if people support a rebel movement, they have incentives not to participate if such participation is costly, their participation is unimportant for the outcome of the struggle, and the gains from rebellion are public goods (Tullock 1971). Yet, the free-rider problem does not apply if grievances cause emotions like aggression or rage, which make people behave irrationally (Gurr 1970:326). Also, politicized grievances could make people *enjoy* taking part in rebellious action, which amounts to a private good (Wood 2003). Moreover, participation in rebellion may not *necessarily* be costly in economic (Popkin 1988) or security terms (Kalyvas and Kocher 2007); this depends on individual and local circumstances and the strategies of belligerent organizations.

Institutional Sources of Inter-Group Violence

Another category of arguments holds that economic, political, and social institutions typical for poor countries can give both incentives and opportunities for rivaling elites to resort to political violence. Such arguments come in various forms. North, Wallis, and Weingast (2009) hold that economic development tends to go hand in hand with the transition to an "open access" order characterized by rule of law, open access to organizations, and a state monopoly of violence. Under this social order, violence is avoided through state deterrence. Societies that have not made this transition, "natural states", are built around limited access to organizations, which underpins the creation of economic rents to elites within a "dominant coalition".[10] In such systems, control of coercive means is dispersed, and violence is controlled through fragile elite agreements. Yet, there is always a risk of civil

[10] This order may co-exist with formal institutions of democratic participation and elements of legal-rational bureaucracy, according to the authors. They suggest that only about 25 countries are open access orders today (North, Wallis, and Weingast 2009:xii).

war; especially during political or economic crises, when the terms of the agreements must be renegotiated.[11]

Mousseau (2012) makes a related argument that poorer countries are more prone to civil war because they tend to have a "clientelist" economy, marked by reciprocal and personalized exchange, rather than a "market-capitalist" economy, with impersonal and contractual exchange. A clientelist economy produces a strong separation of in-groups and out-groups and a politics of rent-seeking for one's own group, he argues. A market-capitalist economy, by contrast, makes citizens interested in general prosperity, peace, democracy, and impartial enforcement of laws. Insurgency cannot happen in market-capitalist economies, he argues, because of preferences for peace as well as insurmountable collective action problems. In clientelist economies, on the other hand, insurgency can occur because military capacity is dispersed among groups and strong patron-client relations within groups remove collective action problems.

While such theories may point to important conditions underlying the long-lasting peace in most wealthy democracies, they are relatively vague on the dynamics and micro-level mechanisms leading to civil war in countries lacking a consolidated market economy or rule of law. In Mousseau's theory, which is most explicit, some assumptions are also questionable: in particular that war readily follows from elite-level decisions and that there is no collective action problem of rebellion within social groups.

Existing Evidence

Various empirical literatures touch upon the question of why poorer countries are more prone to civil war than wealthier ones. First, there is a large body of cross-national quantitative studies of civil war onset. Second, there is a rapidly growing field of quantitative studies using spatial and temporal variation *within* countries to explore conflict processes. And third, there are studies on mobilization and collective action during armed conflict, which tend to rely on surveys or qualitative methods. I discuss relevant studies within each of these literatures and point to important research gaps and methodological shortcomings.

[11] Relatively similar arguments are made by Bates (2001, 2008), Fjelde and de Soysa (2009), and Keefer (2008).

Cross-National Studies of Civil War Onset

The questions motivating this dissertation derive mainly from the cross-national literature on the determinants of civil war onset. Although their definitions of civil war onset vary somewhat, most such studies treat civil war onset as a dichotomous variable coded by the country-year and use pooled cross-sectional time-series regression models to estimate how a range of explanatory variables relate to the likelihood of onset.[12] The temporal scope varies, but very few studies go further back than the end of World War II.

One of the strongest associations found in these studies, as already noted, is that between a higher GDP per capita and a lower propensity for civil war onset. GDP per capita has been interpreted as a proxy for different variables, however. In an influential study, Collier and Hoeffler (2004) posit that it captures the economic opportunity cost of rebelling. They suggest two additional indicators of economic opportunity cost – male secondary school enrollment and economic growth – and find both to be associated with a lower risk of civil war. Yet, GDP per capita remains negatively associated with the risk of civil war when controlling for these two variables (Fearon and Laitin 2003; Thyne 2006), which could suggest that it is capturing something else.

Fearon and Laitin (2003), in contrast, interpret GDP per capita mainly as a proxy for state capacity and reach. They test several other indicators of state weakness and opportunity for insurgency: newly independent state; political instability; mountainous terrain; "anocracy" (regimes that are neither completely autocratic nor democratic); population; noncontiguous territory; and oil dependency. They find all these variables to be associated with a higher risk of civil war onset.[13] Although this supports their theory, these measures are relatively distant proxies of central concepts like counterinsurgency capacity and state reach into rural areas. Moreover, GDP per capita remains a strong negative determinant of civil war onset when controlling for these indicators.

Other indicators of state capacities have been tested in recent cross-national studies. Several of them are based on state revenues and spending (Hendrix 2010). None of the revenue-based measures are found to be robustly linked to the likelihood of civil war onset across studies, however, and GDP per capita remains linked to civil war onset when controlling for these measures (Taydas

[12] The explanatory variables are usually lagged by one or a few years to reduce endogeneity concerns, and conflict history variables are included to reduce temporal dependency.
[13] The finding that anocracies are more prone to civil war has later been questioned on the grounds that political violence enters as a coding criteria for this regime category in the Polity scale (Vreeland 2008).

and Peksen 2012; Fjelde and de Soysa 2009).[14] Fewer studies have tested measures focusing on counterinsurgency capacity. One very rough measure, the number of state military personnel, has been found negatively associated with civil war onset (Hegre and Sambanis 2006; Mousseau 2012). Still, GDP per capita remains negatively associated with civil war onset when controlling for it.

There has also been some research into how "social terrains" relate to the risk of civil war. Several studies look at population density and geographical dispersion, but they have not found a robust linear association between these variables and civil war onset when controlling for GDP per capita (Buhaug and Rød 2006; Collier and Hoeffler 2004; Fearon 2005). The proportion of the population living in urban areas, on the other hand, appears to be more strongly linked to civil war onset. Kocher (2004) finds that controlling for this variable, GDP per capita is no longer negatively associated with civil war onset. Settlement structure might therefore be one variable underlying the development-civil war association. The role of roads and communication networks for civil war onset has been less studied. In a geographically disaggregated, cross-sectional study from Africa, Buhaug and Rød (2006) find that higher road density was associated with lower risk of armed conflict over territorial issues, but had no association with the risk of conflict over central government.

The role of institutional factors has been investigated in several recent studies. It is well established that the development-civil war association is not mainly due to a tendency for poorer countries to have less democratic formal institutions (Hegre et al. 2001).[15] A few recent studies suggest that contract enforcement and protection of private property may be more important. Fjelde and de Soysa (2009) use the ratio of non-currency money to the total money supply ("contract-intensive money") as an indicator of contract enforcement. They find this to be strongly related to a lower risk of civil war onset, and that the GDP per capita association with civil war onset is considerably reduced when controlling for it. Mousseau (2012) tests a related variable, the extensiveness of life insurance contracts, which he interprets as a measure of the degree to which the economy is based on market-capitalist exchange. He finds this to have a strong negative association with civil war onset, and that the GDP per capita is no longer related to the likelihood of civil war onset when controlling for it. De Soysa and Fjelde (2010) test two other institutional indicators: the "quality of government", which is based on expert assessments of corruption, law and order, and bureaucratic quality, and

[14] Fjelde and do Soysa (2009) find some evidence that higher government spending as a share of GDP is related to a lower risk of civil war onset, however; especially in regimes with high constraints on the executive.
[15] Collier and Rohner (2008) find evidence suggesting that the effects of (formal) democracy and GDP per capita on the risk of civil war may be contingent, however. Increasing income levels, they find, is more strongly associated with reduced risk of civil war among democracies than among autocracies.

"economic freedom", which captures contract enforcement, the openness of markets, and government non-interference in the economy. When controlling for both the "quality of government" and "economic freedom", they find that GDP per capita is no longer negatively associated with civil war onset. Generally, these studies provide support for the idea that societies with reliable third-party enforcement of contracts are less prone to civil war, and that this might help explain the poverty-civil war association. Still, the findings are consistent with a politico-military opportunity account, since states that credibly enforce contracts are likely to have considerable administrative and policing capacity, which facilitates effective counterinsurgency.

Grievances were largely dismissed as explanations for cross-national variation in civil war risk by early studies on the basis that individual income inequality, democracy, and ethno-linguistic diversity were not found associated with civil war (Fearon and Laitin 2003; Collier and Hoeffler 2004). Subsequent studies challenge this inference. Some argue that inequalities among individuals are not as relevant for armed conflict as "horizontal" inequalities between culturally defined groups, since armed conflicts are often structured around identity groups (Stewart 2008). Several articles show that exclusion of ethnic groups from state executive power is related to a higher risk of civil war (e.g., Wimmer, Cederman, and Min 2009; Cederman, Weidmann, and Gleditsch 2011). Socio-economic horizontal inequalities have also been found associated with a higher likelihood of internal armed conflict (Østby 2008). Although this suggests that group-level grievances matter, GDP per capita is still associated with civil war onset when controlling for horizontal inequalities and ethno-political exclusion (Wimmer, Cederman, Min 2009; Cederman, Weidmann, and Gleditsch 2011). Another grievance argument is made by Taydas and Peksen (2012), who posit that governments can reduce grievances, and thereby also the risk of civil war, through welfare spending. They find that spending on education, health, and social security goods decreases the risk of civil war, whereas higher military spending and total government spending do not. When controlling for welfare spending, the GDP per capita-civil war association is weakened, but still significant in most models.

While the above-mentioned studies use pooled time-series cross-sectional models, some recent studies have taken other approaches. Djankov and Reynal-Querol (2010) use fixed effects models to test within-country temporal covariation between economic development and civil war incidence, using new estimates of GDP per capita dating back to 1825.[16] They find that GDP per capita is no longer significantly associated with civil war when adding country fixed effects. This

[16] Data exists back to 1825 for only 6 countries, however. Back to 1850, there is data for 27 countries.

may suggest that it is not income levels per se that reduce the risk of civil war. Further, using a sample of ex-colonies and pooled models, they find that GDP per capita no longer has a significant negative coefficient when controlling for European colonial settlement. Their interpretation is that "historical phenomena […] jointly determine income evolution and conflict" (Djankov and Reynal-Querol 2010:1035). However, it is not clear how European colonial settlement prevents civil war down the road. It could, for instance, be related to infrastructural or institutional developments that in turn affect the risk of armed conflict.

Several studies take an instrumental variable approach to get around the problem of possible endogeneity of income and growth to political instability and violence.[17] Miguel, Satyanath, and Sergenti (2004) use rainfall variation as an instrument for economic shocks in Africa, and find that decline in rainfall is associated with a higher risk of internal armed conflict onset. However, Ciccone (2011) shows that this finding is an artifact of their operationalization of rainfall shocks. Consistent with this, Theisen, Holtermann and Buhaug (2011/2012) find no association between droughts and civil war onset in Africa, using a geographically disaggregated design. International commodity prices have recently been used as an instrument for economic growth, but also for this variable different studies seem to reach different conclusions (Brückner and Ciccone 2010; Bazzi and Blattman 2011). Generally, then, although short-term economic decline has been found associated with a higher risk of civil war in several studies (Dixon 2019:715), there is no firm consensus about the direction of causality.

To sum up, the cross-national literature on civil war onset has established quite clearly that the poverty-civil war association is not only due to poorer countries having smaller armies, more autocratic formal institutions, or more vertical inequality. Various institutional differences, including whether governments credibly and impartially enforce contracts, may plausibly form part of an explanation. So could social terrains, which may capture state reach and opportunity for insurgency. Whether differences in economic opportunity costs may help explain the development-civil war association is less clearly established.

Shortcomings

While the cross-national literature has yielded considerable insights, there are also significant gaps and methodological problems. First, several central explanatory variables have been proxied by

[17] The problem is more acute for economic growth than for the level of income, which tends to change very slowly.

vague and theoretically distant indicators. One such variable is the economic opportunity cost of rebelling. Arguably, the most relevant indicator for this concept would be income levels, but none of the previously tested indicators, like GDP per capita and male secondary schooling, measure this. Further, some single indicators of social terrains have been tested, but these do not fully capture the broad concept of state reach. This variable can better be measured by a composite index involving both social terrains and infrastructure. These measurement problems are addressed in *Article II*, which is described below.

A second problem concerns spatial overaggregation. Previous cross-national studies compare country averages, even though insurgencies often take place only in limited parts of a country and factors related to economic development tend to vary within countries. This could result in measurement error. Moreover, some hypotheses that could help us understand the development-civil war association cannot be tested using country-level designs, like whether armed conflicts tend to begin in poorer or wealthier areas *within* countries. *Article I* contributes to filling this gap by using a geographically disaggregated design to study the links between economic development and the likelihood of civil armed conflict onset.

There are additional problems related to much of this literature. First, development-related variables could be partly endogenous to civil war, although this problem is more acute for rapidly changing variables, like economic growth and political instability. More fundamentally, there are problems related to the lack of a clear agreed-upon definition of civil war (Sambanis 2004b). Moreover, by looking at civil war onset as a discrete occurrence, these studies are not able to capture the often lengthy processes leading up to this outcome. While cross-national studies are useful for identifying general patterns and sorting out more and less *plausible* explanatory factors, other designs offer more leverage for analyzing the processes and mechanisms posited by theories of civil war onset.

Subnational Quantitative Studies of Conflict Processes

In response to limitations of cross-national quantitative studies of civil war onset, scholars have increasingly turned to subnational analyses of wartime processes. While such studies are unable to fully assess the causes of armed conflict, they can be useful for assessing the micro-foundations of theories of civil war onset. Several of the theories described above concern rebel mobilization and growth during armed conflict. Since data on rebel mobilization are difficult to obtain, many subna-

tional quantitative studies have attempted to derive implications from these theories about the spatial or spatiotemporal patterns of violence we should see within countries during conflict.

Many such studies look at the association between local prewar socio-economic, geographic, or demographic conditions and the level of violence during armed conflict. They tend to come to different conclusions about the role of poverty, which is sometimes interpreted as a proxy for economic opportunity cost (Do and Iyer 2010) and sometimes for grievances (Murshed and Gates 2005). Even for particular cases, like the Maoist insurgency in Nepal, studies have not arrived at a consensus. Some studies find that poorer areas saw more violence (Do and Iyer 2010; Murshed and Gates 2005) while others find no such association (Acharya 2010; Nepal, Bohara, and Gawanda 2011). In a study from Liberia, Hegre, Østby, and Raleigh (2009) find that fighting actually tended to be more intense in wealthier areas. Somewhat similarly, Berman et al. (2011) find that areas with higher unemployment rates tended to see less insurgent attacks against government forces in Afghanistan, Iraq, and the Philippines. Studies also come to different conclusions about the role of accessibility and infrastructure, central variables in the politico-military opportunity account. For Nepal, studies find that remote areas with few roads and rough terrain tended to see more violence (Acharya 2010; Do and Iyer 2010), but for 14 Central African countries, Raleigh and Hegre (2009) find that "war events" (mainly fighting) were more frequent in areas connected by roads.

A few studies have begun to explore how rebellion or violence spreads over time within a country (O'Loughlin and Witmer 2010; Schutte and Weidmann 2011). Do and Iyer's (2010) study of the spread of rebellion in Nepal is especially relevant, since it focuses on the role of development-related factors. They analyze how pre-insurgency district-level factors are related to the likelihood of early insurgency onset, operationalized as the year a district reaches 100 cumulated fatalities. Poverty is found to be the most important predictor of early insurgency, while roads or terrain did not play any role.

Buhaug (2010) takes a different spatial approach. Using global data on the location of armed conflict onset collected in this dissertation project, he analyzes how prewar conditions affect both the risk of conflict breaking out in a country and where fighting occurs within a country. He argues that the location of armed confliict depends on the relative capacity of the belligerents: if the rebels are facing a stronger government, they are likely to focus activities in areas farther from the capital. Most of the findings support the argument: the conflict is more likely to begin in the periphery of countries with a high level of economic development, an effective state bureaucracy, and militarily weak in-

surgents. But seemingly contrary to the argument, a smaller government army also predicts fighting in the periphery.

Shortcomings

While these studies give important insights, there are also problems associated with them. In particular, studies of the intensity of violence have limitations for assessing theories about mobilization and collective action. First, the intensity of violence is a poor proxy for rebel mobilization or activity. During civil war, low levels of violence in an area need not indicate low rebel activity; rather, it could stem from complete rebel control and monopoly of violence (Kalyvas 2008). Second, most of these studies do not take into account that armed conflicts are dynamic processes, and fighting may move from one place to another over time. In a highly disaggregated study of selective violence during the Greek civil war, Kalyvas (2006) finds that the pattern of control was the most important explanatory factor, and that local income levels had different associations with violence in different time periods. Rebel activity in one place may also not only be a consequence of local circumstances, as is sometimes assumed; it can spread through diffusion or rebel projection of power from other areas.

Looking at how insurgent activity spreads over time has greater potential for giving insights to the role of local factors in armed conflict. Still, existing studies, like that of Do and Iyer (2010) for the Nepalese case, have considerable limitations. Although this study takes a step towards capturing conflict dynamics, it does not include time-varying explanatory variables. The model thereby rests on the questionable assumption that insurgency onset in a district is independent of conflict processes in nearby areas and developments at the macro-level. *Article III* presents an analysis of the spread of insurgency in Nepal that addresses this and other limitations of the study.

Studies of Mobilization and Collective Action

Most of the theoretical accounts of why poorer countries are especially prone to civil war focus on rebel mobilization. The focus is not surprising: many rebel groups begin as militarily weak, and in order to fight a war, they must mobilize considerable human and material resources from the domes-

tic population.[18] The micro-foundations of these accounts give quite different predictions for what mobilization dynamics we should observe. The most obvious difference lies in the motivations of rebel participants. While the economic opportunity cost account emphasizes private economic motivations, grievance accounts highlight emotional or idealistic motivations rooted in perceived injustice. Politico-military opportunity accounts are more agnostic towards personal motivations, but suggest that most people will not do things that strongly hamper their security.[19]

Not many general conclusions can be drawn from the vast literature discussing motivations behind various forms of collaboration with rebel organizations. The most apparent insight is that expressed motivations vary considerably, usually also within specific organizations or localities. In a survey of ex-combatants in Sierra Leone, a clear majority of the RUF rebels said they had been abducted, and very few mentioned other reasons for joining (Humphreys and Weinstein 2008). Survey data from Colombia suggest a variety of motivations for joining rebel as well as paramilitary groups; among other things, material gain, ideology, revenge, fun and adventure, and power (Arjona and Kalyvas 2012). Viterna (2012) finds that women guerillas in El Salvador followed different paths to recruitment: some joined for political reasons, often early on; others joined reluctantly when experiencing a material or security crisis related to government repression; while some joined for adventure or retribution after being persuaded by guerillas. Similarly, rebel collaborators without full-time engagements have been found to express different motivations, including emotional or moral reasons stemming from perceived injustice and political support of the rebels (Wood 2003), fear and security considerations (Kriger 1992; Vines 1991), and self-interest in gaining access to public or private goods provided by rebel organization (Popkin 1988; Young 1998).

Generally, these studies suggest that motivations vary and that they often relate as much to conflict processes as to prewar conditions. This may challenge some grievance accounts, which assume that discontent derives predominantly from structural conditions. It also does not fit well with the economic opportunity cost account, which holds that people generally emphasize economic considerations and that prewar poverty levels determine people's opportunity cost of rebelling. Some dismiss rebel statements, however, since rebels may have reasons to conceal selfish motivations

[18] Some rebel organizations gain considerable material resources as well as fighters from external sources (Byman et al. 2001). Yet, even so endowed organizations, if they are of a considerable size, depend on local supplies of food, shelter, information, and often, part-time collaborators and new recruits.

[19] Since rebel combatants often comprise a tiny minority, these accounts tend to focus more on collaboration by noncombatants than on recruitment of full-time combatants.

(Collier 2000). They rather suggest focusing on who the rebels are and what they do to learn about their motivations.

Few existing surveys allow for comparing the backgrounds of rebel combatants with those of noncombatants. Yet, evidence from unsystematic comparisons as well as from the available surveys appears to suggest that rebel combatants tend to be poorer and less educated than the population average (Humphreys and Weinstein 2008:447; Viterna 2006:10; Weinstein 2007:114; Wood 2003:222).[20] The most certain finding is that rebel (as well as paramilitary) combatants tend to be young, and usually male (Arjona and Kalyvas 2012:151-153; Humphreys and Weinstein 2008). Some studies also find that rebel combatants, especially in the initial phase of insurgency, tend to be drawn from political or social activist groups linked to the rebel cause (Petersen 2001:101-129; Viterna 2006:19). Moreover, several studies point to recruits often having family or friendship ties to rebels before joining (Arjona and Kalyvas 2012:160; Hart 1999:209), and often having relatives or friends harmed or killed by the opposing armed group (Arjona and Kalyvas 2012:164; Wood 2003:223-230). Generally, however, rebel combatants and collaborators appear to come from a variety of backgrounds, although there is a tendency for some groups to be overrepresented.

According to the politico-military opportunity account, the political and military context plays an important role in mobilization. Consistent with this, Arjona and Kalyvas (2012) find that rebel combatants in Colombia tended to come from localities with a guerilla presence. Similarly, Elizabeth Wood suggests that rebel participation in El Salvador was highly unlikely where the government had a strong presence (Wood 2003:212).

Shortcomings

The large literature on mobilization and collective action during armed conflict, which is far from fully covered here, is highly insightful. Still, important gaps remain. Few studies have been able to assess the micro-foundations of politico-military opportunity theories, for instance, which emphasize the role of local control for mobilization. This would require careful tracing of control and mobilization over time at the local level, which is usually not provided in micro-level studies. *Article IV* contributes to filling this gap through an in-depth qualitative study of insurgency processes in a hamlet of Nepal.

[20] As in other political organizations, however, the leadership tends to be highly educated (Wickham-Crowley 1992:23-25).

Design and Contents of the Dissertation

The dissertation uses a mixed-methods approach to deepen our understanding of the links between economic development and civil war. The articles focus on different levels of analysis, ranging from countries to individuals, and use different methods: statistical methods, qualitative methods, or a combination of the two. The obvious advantage of integrating diverse designs and methods to study an overarching question is that every approach has its strengths and weaknesses, and tends to leave some questions open that can better be answered by other approaches.

The dissertation includes two quantitative cross-national studies. They help establish general patterns, and point to some factors that could plausibly underlie the development-civil war association. However, cross-national designs have limitations for analyzing how the explanatory variables are linked to the complex outcome of civil war. To further explore this, I study the dynamics of the Maoist insurgency in Nepal. This case has several advantages: First, the country displays considerable spatial variation in development-related factors, which gives leverage for comparisons. Second, the case is well suited for exploring the micro-foundations of the politico-military opportunity account, since initial conditions fit the theory well, and we would therefore expect mechanisms to adhere to the theory.

The first Nepal study uses a mixed-methods approach to analyze the emergence and spread of insurgency over time. Most importantly, this allows me to explore whether the role of local factors in insurgency processes is contingent on the relative military capacity of the belligerents. The other article provides an in-depth analysis of mobilization processes in one hamlet of Nepal. Through careful process-tracing and comparison over time, I am able to explore the micro-foundations of politico-military opportunity theory, which has not been thoroughly investigated before. I present each article in brief below.

Article I: It's the Local Economy, Stupid! Geographic Wealth Dispersion and Conflict Outbreak Location[21]

This article constitutes, to my knowledge, the first global study of the association between subnational income variations and the onset of civil armed conflict. Some of the explanatory variables suggested to underlie the negative association between economic development and civil war onset at the country level, like poverty and state presence, tend to vary within countries; other suggested explanatory factors, like central government capacity and national-level institutions, do not. If any of the first set of variables were important, we would expect to find that civil wars tend to break out in less developed areas of countries. By implication, we should see a stronger association between income levels and the outbreak of civil war when using subnational data than country-level data.

We use new data coded by myself on the location of civil armed conflict onset, defined as the place of the first recorded battle-death in the conflict. An inclusive definition of armed conflict is chosen, with a low 25 annual battle-deaths threshold.[22] Using Geographic Information Systems (GIS), the conflict onset data were merged with spatially disaggregated data on economic production and other variables into a grid cell structure with a resolution of 0.5 x 0.5 decimal degrees. The main advantage of this design is the spatial disaggregation on a global scale; the main disadvantage is the restricted temporal scope. The economic data exist only for the year 1990. We therefore look at how "local" economic production ("gross cell product per capita") in 1990 relate to the risk of seeing onset of a civil armed conflict in the following ten-year period (1991-2000).[23]

We find that areas seeing onset of conflict tend to be somewhat poorer than the national average, although the association between negative income deviations and conflict onset is not statistically significant at conventional levels. For the least developed countries, however, the results suggest that *wealthier* areas see a higher likelihood of conflict onset.[24] This may suggest that the role of local or subnational poverty or state reach may be contingent on macro-level factors related to economic development, like state capacity. Moreover, we find that areas with more forested and mountainous terrain see a higher risk of conflict onset, as expected from politico-military opportunity theories. On

[21] This article is co-authored with Halvard Buhaug, Kristian Skrede Gleditsch, Gudrun Østby, and Andreas Forø Tollefsen and published in the *Journal of Conflict Resolution* (Buhaug et al. 2011).
[22] This could be problematic for assessing some accounts of the development-civil war association. However, this article is explorative, and does not test any of the above theories in particular.
[23] The term "local" is used in an untraditional sense, since we are not looking at particular places, but relatively large, arbitrarily defined grid cells.
[24] The small number of armed conflict onsets in the sample makes this inference relatively uncertain, however.

the other hand, conflict onset is more likely in areas closer to the capital, which appears to contradict such theories. This could be related to the inclusion of low-intensity armed conflicts like coup attempts, however, which are outside the scope of standard politico-military opportunity accounts.

Generally, the study suggests that the development-civil war association is at least partly rooted in subnational conditions. However, there are several limitations to the study. First, the limited temporal variation makes estimates relatively uncertain. Second, the study cannot say much about the processes leading to civil war and how development-related factors are linked to them. Third, the finding that conflicts tend to begin in areas with lower levels of economic production is consistent with several theoretical interpretations. The data needed to construct more precise indicators for the different causal variables that GDP per capita may proxy for are currently not found at a disaggregated level for the entire world. To construct such indicators and test their impact on the risk of civil war I turned to a country-level design.

Article II: Explaining the Development-Civil War Relationship[25]

This article assesses cross-national, quantitative implications of the two most influential accounts of the development-civil war association: the economic opportunity cost and the politico-military opportunity accounts. As discussed above, existing cross-national studies provide little evidence to assess which of these interpretations of the development-civil war association are most plausible. I develop more precise indicators of the explanatory variables of the two arguments, which allow for assessing this question. Following the economic opportunity cost logic, the poorest should be the first to rebel since they have the least to lose. Since rebel organizations need only a tiny fraction of the population as full-time rebel soldiers, the income opportunities of the poorest segment of society should, according to this logic, be the most important determinant of labor costs for rebel organizations. I therefore use the income of the poorest 10 percent of the population ("bottom decile income") as a proxy for economic opportunity costs. The data for this indicator were compiled from two survey databases, the Luxembourg Income Study (2009) and the World Bank's (2008) "Povcalnet". I focus on one central variable in the politico-military opportunity account: state reach. This variable is

[25] This article is single-authored and published in *Conflict Management and Peace Science* (Holtermann 2012).

measured by an additive index of three indicators: the density of roads and telephone lines and the percentage of people living in urban areas.[26]

I test the impact of these variables on the risk of civil war onset in a sample covering 133 countries from 1989 to 2006.[27] The state reach indicators are all found to be strong determinants of the risk of civil war onset, and controlling for the state reach index, GDP per capita does not have any significant association with civil war onset. "Bottom decile income", on the other hand, is not significantly associated with the risk of civil war onset and the GDP-civil war relationship remains intact when controlling for it.

These findings suggest that the association between low levels of development and civil war is more plausibly related to low state reach and social terrains favoring insurgency than to the depth of poverty. The interpretation I suggest is that rebel mobilization and survival depends more on the opportunity for rebels to carve out control than on low economic opportunity costs. Where politico-military conditions allow insurgents to establish control, they can effectively apply several tools – persuasion, coercion, organization and economic rewards – to mobilize resources from local populations. Although poverty may ease recruitment under such conditions, it may not be necessary or even important. Moreover, where the state has firm control, the risk of capture or death is likely to deter insurgent participation irrespective of economic considerations.

There are caveats to these inferences, however. The findings are made uncertain by the limited sample size and the relatively high correlation between bottom decile income and state reach. Moreover, even if the statistical results were indisputable, other causal accounts could be imagined. Lack of infrastructure could, for instance, be associated with grievances, and might offer rebel organizations opportunity to gain collaboration by providing public goods. Moreover, the mechanisms suggested and their links to structural factors are not sufficiently established by the existing case study literature. To further explore this, I turned to a case study of the Maoist insurgency in Nepal.

[26] These indicators probably also capture central state capacities to some extent, but they are too distant proxies to say anything with certainty about, for instance, state military or administrative capacity.
[27] Civil war onset is coded if there are more than 500 battle-related deaths in a year *or* if there are more than 25 deaths in a year and cumulative deaths over the three subsequent years reach 1,000.

Article III: Relative Capacity and the Spread of Rebellion: Insights from Nepal[28]

This article analyzes the Maoist insurgency in Nepal (1996-2006) in order to build theory about the emergence and spread of insurgencies seeking to overthrow the central government. While this article has a broader theoretical aim, it also sheds light on the links between development-related factors and civil war. The central argument builds on politico-military opportunity theories, and posits that insurgency tends to be a highly coordinated phenomenon in which belligerent organizations strongly shape local processes. In consequence, conflict dynamics become contingent on the overall relative military capacity of the belligerents: as an initially weak rebel organization develops militarily, the role of local conditions for the prospects of rebellion will change due to shifts in rebel priorities as well as the appearance of new tools of influence. If this argument is correct, it means that not much can be learned by looking at associations between prewar local conditions and the intensity of violence for the entire conflict, as several previous studies of Nepal's Maoist insurgency have done. It also challenges Do and Iyer's (2010) study of the spread of insurgency in Nepal, which assumes stationary effects of all explanatory variables.

The empirical analysis combines qualitative and quantitative approaches. I use qualitative data to identify key processes underlying the spread of insurgency in different phases of the conflict. This analysis draws on my own interviews from three districts as well as other studies, reports, and Maoist party documents. I find that the rebels relied considerably on pre-existing ties to populations in political strongholds to mobilize in the beginning. After a couple of years, mobilization in these areas allowed them to expand activities to nearby districts, mainly through movement of guerillas and political cadres. Over time, diffusion mechanisms also helped spread the rebellion. Some people for instance became inspired to rebel after hearing about Maoist achievements elsewhere. As the rebels managed to improve their military capacity, the processes of expansion changed. The Maoists began to transfer large groups of combatants across regions and coercion took a more prominent role in mobilization efforts. Moreover, they constructed alliances with other political movements where they lacked a political foothold.

Some implications of the arguments are then tested statistically, using district-level Cox duration models of insurgency onset, defined as the beginning of rebel activity that includes lethal vio-

[28] This article is single-authored and currently under review in an academic journal.

lence.[29] I argue that as the rebels gain military strength relative to the government, they become less dependent on (i) pre-existing local networks for mobilization; (ii) inaccessible areas for hiding; and (iii) proximity to areas with rebel presence for projecting power. We should therefore see that these conditions increased the likelihood of insurgency onset in the early phase of rebel weakness, but that their effect gradually declined over time, as the rebels gained capacity relative to the state. I use the percentage of district votes for the UPF, the precursor of the Maoist party, in the 1991 elections as a proxy for pre-existing local networks. Accessibility is measured using an equally weighted index of rough terrain, road density, and distance from the district headquarters to Kathmandu.[30] Proximity to areas with rebel presence is proxied by a dummy marking whether any adjacent districts already had seen insurgency onset at a particular time. I test the hypotheses using interaction terms between these conditions and the time since the launch of the insurgency as well as two measures of relative military capacity. The first relative capacity measure is the number of rebel full-time combatants as a percentage of government forces; the second is a moving average of daily government force fatalities over six-month periods. The results support the hypotheses: while pre-existing networks, inaccessibility, and proximity to insurgency-affected areas were strong determinants of onset in the beginning, their effect faded over time and with increases in rebel capacity relative to the state.

I test several other variables as well, which I do not have strong reasons to expect will have a temporally contingent effect. To assess the role of diffusion through mass media I include the percentage of the population with radios, which I find robustly and positively associated with the likelihood of insurgency onset. As an indicator for poverty, I use the Human Development Index (HDI), which is an equally weighted index of life expectancy, educational attainment, and GDP per capita. HDI is negatively associated with insurgency onset, but it is not statistically significant in most models, thereby providing only partial support for economic opportunity costs or poverty-related grievances being important for insurgent activity.[31]

[29] Onset is coded if there were at least five days of insurgent killings within any half-year period in a district, based on violent events data from INSEC (2010). The main results are robust to changes in this operationalization.
[30] "Accessibility" is measured somewhat differently here than "state reach" in *Article II*. Since the latter focuses on factors that could plausibly relate to both economic development and the reach of the state, it excludes rough terrain. The "accessibility" index excludes urbanization, since this is not a good measure of accessibility on the district level in Nepal. Nearly all districts were predominantly rural, and tiny differences would therefore get considerable weight in the index. Telephone density is excluded mainly for simplification, since the index hardly changes when including this indicator.
[31] It is somewhat difficult to disentangle poverty from state reach also in this study, however. HDI could indicate greater state presence in addition to economic wellbeing. Further, it is relatively highly correlated with the accessibility index ($r = 0.60$).

Overall, the study suggests that where and how insurgency spreads over time depends upon the relative military capacity of the belligerents. This insight may not be very surprising, but it is rarely taken into account in empirical studies. Politico-military opportunity accounts, for instance, suggest that inaccessibility generally favors insurgency (e.g., Fearon and Laitin 2003:80). Yet, this rests on an assumption that the insurgents are far weaker than the state, which does not always hold across conflict periods or in the initial phase of every insurgency. Further, the study shows the importance in this case of a factor that is ignored in most studies of the Maoist insurgency in Nepal and in the cross-national literature: rebel pre-existing networks and political support. The last article provides more insights to the role of this factor, and particularly its importance relative to patterns of territorial control.

Article IV: How Can Initially Weak Insurgent Groups Grow? Lessons from Nepal[32]

The final article looks more deeply into the processes of mobilization and growth of insurgency. It takes politico-military opportunity arguments as a starting point. Building on Kalyvas' (2006) theory, these arguments hold that local control is the key determinant of collaboration by local noncombatants during armed conflict, and that collaboration in turn is essential to rebel survival and growth.[33] If this is so, how can rebel groups that are far weaker than the state gain strength to begin with? Fearon and Laitin (2003) and Kocher (2004) suggest that even if a state is stronger than the rebels, it may be too weak to fully control the entire polity, thereby leaving open politico-military spaces where the rebels can carve out base areas. However, since a very weak rebel group can hardly spread its influence widely, the state should be able to deny the rebels of base areas by transferring forces to rebel-affected zones. A simple politico-military opportunity account therefore seems insufficient for explaining the growth of weak rebel groups.

This study explores this puzzle by carefully tracing the patterns of control and mobilization in a hamlet within the core area of the Maoist rebellion in Nepal. It focuses on the period from the initiation of insurgency in 1996 up to 2002, when the country was undoubtedly in a state of civil war. Nepal's Maoist insurgency is well suited for exploring the micro-foundations of politico-military opportunity theory. The country's weak state, rural settlement structure and difficult terrain make it a highly plausible, or perhaps even a "most-likely" (Eckstein 1975:118), candidate for a politico-

[32] This article is single-authored.
[33] This is more clearly stated in Kocher (2004) than in Fearon and Laitin (2003), who also focus on other factors, such as the skills of counterinsurgent forces. Still, they explicitly draw on Kalyvas' theory.

military opportunity explanation of civil war onset. This gives reason to expect that the theory's suggested causal processes would be in operation, and, if not, that the theory is imperfectly specified.

I use Kalyvas' (2006) detailed theory of control and collaboration during civil war as a starting point.[34] He argues that collaboration is largely a result of local control, mainly because control allows an actor to credibly threaten punishment of defectors and promise protection of collaborators. Kalyvas separates between "full control", under which one actor completely prevents the opposing actor from operating in the area; "dominant control", under which one actor has forces stationed in the area, while the opponent must operate clandestinely or only sporadically enter the area; and "parity", where no actor is markedly stronger than the other. Under full control, he predicts no collaboration with the weaker actor. Under dominant control, a minority of highly committed individuals is predicted to collaborate with the weaker actor, whereas the majority collaborates with the dominant actor. Under parity, similar levels of collaboration with each side are expected (Kalyvas 2006:197, 205).

The analysis makes use of various qualitative approaches. First, I use the congruence method (George and Bennett 2005:181) to establish whether Kalyvas' predictions about collaboration under different control zones hold in this case. Second, I draw on comparisons over time, particularly between periods under dominant *government* control and periods under dominant *Maoist* control. Third, I use process-tracing (George and Bennett 2005:205-213) to identify mechanisms of collaboration. The primary source of information is 32 in-depth interviews from Rolpa by the author, which included Maoist combatants and cadres, non-combatant villagers, and leaders of the other major political parties.

In consistence with my argument, I find that even if the state had scant presence in the field site before the insurgency, the rebels were not allowed to gain control easily. After the launch of insurgency, the government rapidly set up new police stations and assumed dominant control. In contrast to Kalyvas' predictions, collaboration with the rebels was more widespread than with government agents in the period of state dominance, and recruitment of rebel full-timers higher than recruitment to counterinsurgent forces. This helped the rebels slowly gain strength during this period, which resulted in the police being forced out of the area in early 2000. In the following period of

[34] Notably, it is unclear whether Kalyvas believes the theory is applicable to the early phase of insurgency that I focus on. He restricts the theory to civil war, but his broad definition of civil war makes it difficult to assess at what point an insurgency becomes a civil war. The theory is still a useful starting point since it is more detailed than related accounts, and has inspired politico-military opportunity theories of civil war onset.

dominant rebel control, collaboration was in agreement with Kalyvas' theory. Nearly all opposition faded, everyone contributed in cash or kind to the rebels, and many joined as full-timers.

I identify two main reasons for these outcomes. First, the Maoists' strong local pre-existing organizational and social networks spurred rebel collaboration and recruitment, often in spite of considerable personal risk. The specific mechanisms connecting networks to collaboration were difficult to establish, but several mechanisms are likely to have operated, including solidarity, norms of reciprocity, and social incentives. Second, inept counterinsurgency and the government's inconsistent law-and-order approach benefited Maoist mobilization. Importantly, many Maoist supporters were arrested on trumped-up charges and later released by the courts. Provided with grievances and often with ties to insurgents, many of those arrested joined the guerillas upon their release. The police also tended to rely on a few locally disliked anti-Maoist strongmen as informants, which led to false denunciations. Further, the police often engaged in coercive information gathering, which caused resentment and reduced civilians' opportunity cost of joining the guerillas.

Although the study points out limitations of military control, it does not suggest that control is unimportant for the prospects of insurgency. If the government had been able and willing to establish full control in these areas, the outcome might have been different. Limited state resources combined with scattered settlements and difficult terrain also forced the police to spread its forces thinly, which made them vulnerable to ambush. Still, the analysis suggests that the processes underlying the growth of weak rebel groups may be more complex than what is suggested by prominent politico-military opportunity accounts. Moreover, it points to the role of networks and to counterinsurgent policies and practices, not just capacity, as important topics for further research.

Conclusion

The dissertation's contributions to answering the overarching question are best recognized when seeing the articles in combination. In the following, I discuss the inferences to be drawn from the articles' findings. I also show how the insights from the Nepal studies help make sense of observations in the cross-national studies. Next, I sum up the contributions of the project to the study of civil war, before suggesting directions for future research.

Discussion of Main Findings

The overarching aim of this dissertation was to enhance our understanding of why civil war occurs disproportionately in countries with low levels of economic development. Several accounts had been suggested at the outset of the project, but much remained in terms of empirically identifying the causal variables, processes, and mechanisms producing this pattern. I have used various approaches to help fill these research gaps. First, cross-national, quantitative analyses were carried out to assess what causal variables can plausibly be driving the global pattern. Second, I analyzed insurgency processes in Nepal in order to develop theory about *how* and *under what conditions* development-related conditions are connected to civil war onset.

A central conclusion is that the state's reach throughout the polity is an important variable underlying the development-civil war association. I have provided cross-country evidence showing that countries with greater state reach have a significantly lower chance of seeing onset of civil war, and when controlling for state reach, GDP per capita is no longer associated with civil war onset (*Article II*). Further, I have shown that in the Nepalese conflict, areas less accessible to state agents were more likely to see insurgency begin in the early years of the conflict (*Article III*). The Nepal study also suggested that the role of accessibility and state reach are contingent on the relative military capacity of the belligerents, however. In the last phase of the Nepalese conflict, when the Maoists had become able to offer considerable military resistance to the state, accessibility no longer mattered for where the insurgency spread. This makes theoretical sense: when the rebels do not need to hide from state forces, the main advantage of inaccessibility disappears.

Most studies of civil war onset or insurgency processes do not take into account the contingent role of state reach. This also applies to the cross-national analyses of *Article I* and *Article II*, which preceded the quantitative Nepal study.[35] Reconsidering some observations in these studies that deviate from a simple politico-military opportunity account gives credibility to the modified account, however. *Article I* found that armed conflicts tend to begin in wealthier, rather than poorer, areas within the least developed countries. Since wealthier areas tend to be more accessible for state agents, this seemed puzzling from a politico-military opportunity perspective. Yet, the puzzle disappears when taking into account that accessibility only matters under asymmetry of military power: in

[35] Even if this contingent relationship had been theorized when these analyses were carried out, it would have been extremely difficult to model this using such designs. I discuss this below.

the poorest countries, the state is likely to have so low capacity that even very weak rebel groups may not need to hide from state forces.

Article II pointed to some outliers of civil war onset in countries of fairly high levels of state reach and economic development (*Article II*, Figure 1). The Balkan civil wars in the 1990s constitute a considerable proportion of them. The poor fit of these conflicts is not puzzling when considering that most of them were not highly asymmetric from the initiation because of the disintegration of the Yugoslav state (SIPRI 1992:408; Woodward 1995:250-264).[36] Hence, these cases do not seem to challenge the core claims of politico-military opportunity theory after scope conditions have been properly defined. Other cases, like the civil wars in Israel/the Palestinian Territories and in the United Kingdom/Northern Ireland, are more difficult to account for in a politico-military opportunity framework. Anomalous cases are found for most social science theories, however, and the relationship between state reach and civil war onset is only a probabilistic one: although unlikely, insurgency can occur under difficult politico-military conditions.

My qualitative study from Nepal (*Article IV*) provides insight into the limitations of politico-military opportunity arguments. Prominent scholars have emphasized that low state reach allows insurgents to carve out pockets of control, which is essential for their survival and growth. I show, however, that low state reach does not necessarily make it possible for weak insurgents to carve out control over significant settlements to begin with, since the state can move forces to insurgency-affected areas. Moreover, it is possible for rebel groups to gain strength even under dominant state control if they have strong pre-existing local networks, which provide powerful spurs to collaboration and make it more difficult for state agents to exploit the benefits of control.

While this study shows that control is not the only factor influencing the prospects of insurgency, it does not dispute that it plays an important role. Nepal's Maoist rebels greatly enhanced their mobilization after gaining control. Control allowed them to credibly threaten punishment of defection, as emphasized in other politico-military opportunity accounts. However, it also made them better able to organize villagers and shape their political beliefs, which gave additional spurs to collaboration.

Scattered settlements, lack of infrastructure, and limited state resources helped the Maoists eventually gain control. These factors forced the government to scatter its police forces thinly, which

[36] A possible exception is the Kosovo conflict, where the Kosovo Liberation Army was militarily much weaker than the Serbian forces from the beginning (SIPRI 1999:50-62). Yet, support for the Kosovo Liberation Army by Albania and pressure against the Serbs by NATO relatively soon provided a counterbalance.

made them vulnerable to rebel raids. Another possible reason for the counterinsurgency failure in the area could be that transferring coercive forces does not weigh up for the lack of long-lasting administrative and police presence. If state institutions were more firmly rooted in the area, the police may perhaps have avoided relying on coercive intelligence gathering and associating themselves with unpopular anti-Maoist activists.

The dissertation does not find much support for the economic opportunity cost account, on the other hand. Collier and Hoeffler's (2004) argument implies that the poorest should be the first to join a rebel army, and that the income opportunities of this segment should be an important determinant of the feasibility of rebellion and thereby the risk of civil war onset. This implication does not hold up well against the evidence. *Article II* finds that the risk of civil war onset in a country is not robustly related to the income of the poorest decile of society, and that GDP per capita remains associated with civil war onset when controlling for the variable. The studies from Nepal also do not lend much support to this account. While there was some tendency for insurgency to begin earlier in areas of lower socio-economic development, this association was not robust when controlling for accessibility. Moreover, in the initial few years, the poorest segment of society seemed to be *underrepresented* among insurgent combatants; living at the subsistence level, they could not afford to spend their time on unpaid activism.

This does not necessarily imply that economic opportunity costs are generally unimportant for the decision to become a full-time rebel. There are several other reasons why poverty may not be essential for explaining the potential for insurgency and civil war. First, recruits may not be the most important shortage for many rebel groups. Limited supplies of arms and food often restrain the demand for full-time participants. Part-time participation may therefore be an equally important resource. Because such participation can be combined with regular work, income levels are not likely to be important for the decision to join. Second, poverty could also make it easier for the state to recruit combatants or to buy information services from noncombatants (Berman et al. 2011). Third, when politico-military conditions do not favor insurgency, security considerations are likely to outweigh economic reasons to rebel for the simple reason that money has little value if you are dead. Finally, as indicated above, if the rebel organization is not able to pay its soldiers, too low incomes may actually inhibit rebel activity since people living at the margins of subsistence must work to feed their families.

A more sophisticated economic argument might still be plausible. It could be that rebel organizations often have abundant volunteers, and therefore screen them for desired skills. If that is the case, the economic opportunity cost of rebelling for people with some education could be what matters the most for building a strong rebel army (Bueno de Mesquita 2005; Kavanagh 2011). While this argument cannot be dismissed, my study from Nepal suggests that those joining the insurgency in the important initial period tended to do so mainly for non-economic reasons.

The articles do not provide as much direct evidence pertaining to grievance arguments. My findings do not dismiss that grievances play a role, but they suggest that a direct causal chain from poverty to grievances to rebel collective action and civil war is not likely to be important. The Nepal case study suggests that the formation of grievances is often contingent on political processes. In this case, the strongest grievances arose from processes during the conflict; not the least, indiscriminate violence and unlawful behavior by the police, combined with the framing efforts of Maoist political entrepreneurs. Poverty and low government public goods provision did play a role in the formation of grievances and support for the Maoist rebellion, but such factors were insufficient for making most people participate in the rebellion.

Contributions

The most important contribution of the dissertation relates to the main research question of why poorer countries are more prone to civil war than wealthier ones. To sum up, it provides new evidence suggesting that poverty *per se* is not the main factor explaining the association. More important factors are low state reach and accessibility, which provide opportunity for a rebel organization challenging a militarily stronger state to survive and grow. The dissertation also further specifies this politico-military opportunity account: First, state reach and accessibility are unlikely to be important when the rebels have considerable capabilities relative to the state to begin with. Second, local military control does not necessarily trump all other factors in generating collaboration in the initial phase of armed conflict. These theoretical specifications help account for several observations left unexplained by previous versions of politico-military opportunity theory.

The dissertation also provides more general insights into rebel mobilization and civil war. Many previous subnational studies of conflict dynamics have assumed that the role of various structural conditions is constant over time. *Article III* shows that this assumption is untenable: the conditions favoring rebellion, and the mechanisms driving it, may change over the course of a conflict.

The study also contributes methodologically by showing how such contingency of local factors can be incorporated in statistical models of conflict dynamics.

The dissertation also contributes to the literature on the Maoist insurgency in Nepal. While previous quantitative studies have argued that economic factors were key drivers of the rebellion, my findings suggest that pre-existing Maoist networks, a factor ignored by nearly all such studies, were far more important in the initial phase. I also provide new qualitative data and analysis that helps shed light on the growth of the insurgency in Rolpa district.

Finally, the project has involved collecting data that are of use beyond this study. Most importantly, I have collected data on the location of onset for all civil armed conflicts in the UCDP dataset from 1946 to 2006. Somewhat relevant for the dissertation, Theisen, Holtermann, and Buhaug (2011/2012) use these data to study the association between droughts and civil war onset in Africa. We find no such association, which is consistent with the conclusion that poverty is not a key determinant of civil war onset. The data are also integrated in the publicly available PRIO-GRID dataset (Tollefsen, Strand, and Buhaug 2012).

Future Research

Variables that are potentially relevant for explaining the development-civil war relationship abound, and this dissertation has not assessed all of them. For instance, it has not carefully investigated the role of institutional factors, which a few recent cross-national studies suggest are important. I therefore cannot exclude that patron-client structures, weak rule of law, or a dispersion of military power within the state may help explain the development-civil war association. Future cross-national analyses of civil war onset that include such factors as well as indicators of state reach could perhaps provide further indication of their relative importance. Yet, this approach has limitations. It is difficult to take into account that the role of state reach is contingent on the relative military capacity of potential rebels and the state, for example. To properly do this, we would need information on the military capacity of potential rebel groups before armed conflict begins, which, of course, is not easily available.

New approaches may hold more promise. Civil war onset is a complex outcome produced by several different processes. This dissertation has focused mainly on rebel mobilization and survival after the onset of hostilities. Yet, to reach a complete understanding of civil war, we also need to ask why violent rather than nonviolent forms of contestation are chosen by dissident groups and state

actors in the first place and why negotiated settlements are not reached short of war. The prospects for insurgent mobilization and survival surely influence these processes: if dissidents expect to be crushed if they turn to violence, they are not likely to do so. But choices of violence depend on other factors as well, and require study in their own right. Even if structural factors make insurgency feasible, this does not fully explain why it is chosen. The rebels and the government could take such factors into consideration when bargaining over political settlements (Walter 2009a). By disaggregating these processes, analyzing each step, we may gain a more complete understanding of how various factors affect the likelihood of civil war. A few studies have begun to do this for "ethnic" or separatist conflicts (Öberg 2002; Walter 2009b), but much more theoretical and empirical work along these lines is needed.

Mobilization processes during armed conflict have been more studied, but gaps remain also in this field. This dissertation suggests that pre-existing factors, including accessibility and social networks, can be important in the initial phase of asymmetric conflict. More temporally disaggregated, comparative research would be useful for testing this claim and further specify the mechanisms underlying it. Moreover, my studies from Nepal point to the importance of counterinsurgency practices, and not only capacities, for understanding rebel mobilization. Studies have begun to look at the effect of various counterinsurgency policies on rebel activity (e.g., Berman, Shapiro, and Felter 2011; Lyall 2009; Kocher, Pepinsky, and Kalyvas 2011). Why various counterinsurgency policies are made and how these are turned into practice has been much less studied, and are important avenues for future research.

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Article I

It's the Local Economy, Stupid! Geographic Wealth Dispersion and Conflict Outbreak Location

Article II

Explaining the Development–Civil War Relationship

Article III

Relative Capacity and the Spread of Rebellion: Insights from Nepal

Article IV

How Can Initially Weak Insurgent Groups Grow? Lessons from Nepal

Appendices

Appendix to Article I

"It's the Local Economy, Stupid! Geographic Wealth Dispersion and Conflict Outbreak Location"

Table A1: Country Descriptive Statistics

Country	GWno	No. cells	min GCPpc	max GCPpc	mean GCPpc	s.d. GCPpc	GDPpc
USA	2	4863	5559.4	62044.88	30985.35	15584.24	27951.31
Canada	20	8030	0	67800.31	22436.01	8099	23049.89
Bahamas	31	44	15334.62	15633.02	15412.89	59.51	15403.68
Cuba	40	78	4023.37	4113.67	4090.24	16.48	4084.48
Haiti	41	17	1587.59	1588.01	1587.91	0.1	1587.96
Dominican Republic	42	29	2712.91	2717.21	2716.39	1.11	2716.97
Jamaica	51	10	4807.54	4936.29	4863.53	43.6	4863.69
Trinidad and Tobago	52	7	6595	6637.61	6613.24	22.75	6595.5
Barbados	53	2	11858.8	11858.8	11858.8	0	11858.8
Mexico	70	825	594.25	33001.28	7962.98	3821.21	7868.15
Belize	80	11	4341.98	4494.62	4387.61	56.29	4371.64
Guatemala	90	43	3148.65	3854.3	3528.09	191.34	3598.29
Honduras	91	46	735.63	7974.84	2447	1275.86	2268.55
El Salvador	92	9	3472.75	3647.09	3536.64	61.07	3504.4
Nicaragua	93	52	1543.36	2235.69	1834.3	155.06	1853.42
Costa Rica	94	26	4868.43	5344.51	5126.56	159.21	5158.22
Panama	95	47	4458.49	5780.97	4691.21	295.73	4705.43
Colombia	100	396	1400.92	21701.32	3957.93	2842.45	4244.74
Venezuela	101	331	330.18	63451.18	8898.31	8593.57	8392.46
Guyana	110	71	1096.31	1232.53	1194.85	44.29	1226.74
Surinam	115	56	3576.55	23078	7990.16	2922.25	4990.92
Ecuador	130	107	1995.16	25793.59	5939.79	5419.86	4302.63
Peru	135	452	303.17	746957.96	9564.6	37561.53	4044.36
Brazil	140	2932	758.56	147577.42	7248.76	10219.78	6298.46
Bolivia	145	368	1423.17	4205.09	2643.28	700.72	2567.17
Paraguay	150	141	816.53	3814.6	3186.94	709.51	3558.25
Chile	155	430	48.87	87201.82	10174.78	16662.87	5562.68
Argentina	160	1169	922.64	15431.56	4568.54	2441.8	5939.16
Uruguay	165	75	5875.5	6247.69	5975.91	78.96	5980.07
United Kingdom	200	217	84.77	57917.5	19192.09	9434.53	20206.92
Ireland	205	54	6804.4	43923.92	15434.68	8956.6	14721.11
Netherlands	210	23	17019.65	25556.95	22463.23	2062.55	23076.84
Belgium	211	18	15975.45	26340.93	20661.28	4030.58	22827.39

Country	GWno	No. cells	min GCPpc	max GCPpc	mean GCPpc	s.d. GCPpc	GDPpc
Luxembourg	212	1	37040.48	37040.48	37040.48	NA	37040.48
France	220	292	14342.61	36661.56	19043.18	2825.68	22414.48
Switzerland	225	16	26182.42	32613.86	29975.47	1393.2	30492.72
Spain	230	272	10125.6	22746.84	16201.2	3328.82	17148.5
Portugal	235	58	6207.9	20564.67	10870.34	3019.91	12635.26
GFR	260	135	15510.81	33513.16	22985.64	3751.85	24225.99
GDR	265	65	10446.26	21691.72	15118.86	2404.19	16717.47
Poland	290	174	3674.23	11875.06	6857.78	1907.73	6765.39
Austria	305	40	16934.72	29987.27	21232.88	3593.36	23317.24
Hungary	310	45	5833.09	14650.29	9559.59	2141.36	10819.1
Czechoslovakia	315	62	6908.58	17096.07	11257.54	2564.87	12311.17
Italy	325	200	2890.24	77335.23	22337.03	12688.08	21065.6
Malta	338	2	12835.57	12835.57	12835.57	0	12835.57
Albania	339	16	3115.25	3115.85	3115.61	0.24	3115.66
Serbia (Yugoslavia)	345	129	2730.78	24694.71	8257.98	5542.75	6927.58
Greece	350	122	6665.62	217621.67	29541.02	31280.39	18782.94
Cyprus	352	12	10806.49	66660.58	28103.13	19699.39	16625.64
Bulgaria	355	50	4261.63	13478.87	6476.44	2159.1	6261.12
Rumania	360	116	2481.02	26366.32	7728.26	3892.57	7133.25
Russia (Soviet Union)	365	15010	0	366949.32	14537.64	10335.8	8740.53
Finland	375	276	13075.65	35748.47	18239.41	3202.14	20058.64
Sweden	380	359	17243.79	28111.99	20764.26	1457.91	21731
Norway	385	334	14477.72	36734.85	19513.1	3103.01	24270.05
Denmark	390	51	15163.39	28847.32	19383.03	2655.55	21189.64
Iceland	395	126	6300.37	22816.22	19260.73	4442.06	22355.63
Cape Verde	402	16	1419.55	1426.18	1424.86	1.47	1425.05
Guinea-Bissau	404	13	515.96	541.94	531.65	7.42	533.19
Equatorial Guinea	411	12	1701.59	1874.84	1799.21	71.31	1794.23
Gambia	420	3	879.33	880.34	880	0.59	880.18
Mali	432	428	8.7	2989.75	749.64	511.69	665.94
Senegal	433	75	1088.26	1175.38	1150.7	15.92	1156.7
Benin	434	40	637.34	1070.5	793.49	140.1	907.91
Mauritania	435	403	736.31	10319.34	1754.85	1149.33	1527.76
Niger	436	402	98.56	4814.16	1414.07	1119.65	599.02
Côte d'Ivoire	437	113	1349.76	1450.45	1416.04	9.32	1415.27
Guinea	438	85	782.23	818.35	796.37	4.65	795.49
Burkina Faso	439	87	438.25	785.77	658.27	57.71	647.43
Liberia	450	37	2298.26	2305.81	2302.91	2.45	2304.09
Sierra Leone	451	30	536.85	537.61	537.33	0.19	537.41
Ghana	452	85	590.25	1002.07	765.41	102.47	743.29

Country	GWno	No. cells	min GCPpc	max GCPpc	mean GCPpc	s.d. GCPpc	GDPpc
Togo	461	19	728.46	741.33	734.58	4.59	737.33
Cameroon	471	156	1178.81	4681.49	1774.98	615.47	1804.08
Nigeria	475	312	699.51	242808.66	1780.95	13704.33	1020.22
Gabon	481	94	9703.92	16038.67	13638.64	1019.02	11892.6
Central African Rep.	482	202	328.34	1429.06	573.92	138.47	696.63
Chad	483	391	450.8	28338.5	749.43	1437.38	776.48
Congo	484	112	2391.29	2513.55	2477.73	34.13	2504.94
DR Congo (Zaire)	490	763	370.4	5966.71	452.1	220.53	476.67
Uganda	500	80	27.48	796.57	481.96	141.91	471.18
Kenya	501	192	161.46	12890.33	1392.95	1807.7	1250.19
Tanzania	510	316	306.4	1156.9	697.27	211.44	646.29
Burundi	516	11	420.85	425.84	422.95	1.9	422.51
Rwanda	517	9	589.82	589.86	589.84	0.02	589.84
Somalia	520	241	144.94	5073.7	727.04	419.72	626.35
Djibouti	522	9	2593.45	2607.14	2601.52	5.11	2604.79
Ethiopia	530	424	193.33	1041.76	423	91.31	436.24
Angola	540	435	864.65	237724.83	3006.43	16001.42	1939.22
Mozambique	541	302	92.69	1323.43	375.73	215.15	353.21
Zambia	551	250	534.08	1639.09	781.89	177.88	964.99
Zimbabwe	552	135	1143.56	3723.31	2566.7	432.02	2442.73
Malawi	553	39	534.08	534.8	534.66	0.16	534.74
South Africa	560	483	1259.74	220862.25	11633.95	21317.95	6702.35
Namibia	565	307	260.07	40640.24	7150.68	7763.57	3660.66
Lesotho	570	12	1339.9	1510	1462.18	58.14	1475.58
Botswana	571	205	1472.33	14329.54	3851.85	2832.03	5756.12
Swaziland	572	5	4054.17	4133.28	4069.99	35.38	4059.04
Madagascar	580	255	684.98	988.09	779.86	75.06	830.35
Comoros	581	5	998.46	1108.42	1045.54	57.75	1057.98
Mauritius	590	6	4887.63	5055.53	4999.57	86.68	5054.08
Morocco	600	242	1049.72	9702.66	2451.63	1482.38	2354.09
Algeria	615	870	2831.84	664972.84	9628.79	45361.06	4495.18
Tunisia	616	72	3101.69	4029.36	3406.5	255.34	3408.46
Libya	620	655	8140.33	565393.81	20925.83	57746.65	14556.5
Sudan	625	840	247.86	2690.41	687.01	341.6	833.6
Iran	630	644	820.08	135110.44	12687.64	22254.17	5590.54
Turkey	640	368	1071.98	11271.61	4392.45	2021.97	5045.3
Iraq	645	169	3207.56	12642.54	5393.45	1347.37	5994.97
Egypt	651	388	81.8	3062677.95	131378.63	471607.33	2897.16
Syria	652	72	857.12	7578.02	3411.84	1809	2489.11
Lebanon	660	5	5654.44	5655.47	5655.21	0.45	5655.39

Country	GWno	No. cells	min GCPpc	max GCPpc	mean GCPpc	s.d. GCPpc	GDPpc
Jordan	663	34	2720.81	2805.39	2782.91	23.22	2802.45
Israel	666	15	10996.46	24576.05	16284.87	4678.12	14512.8
Saudi Arabia	670	722	485.61	1150064.78	21744.59	95645.72	17375.84
Yemen	678	168	988.77	10447.09	1554.37	1400.87	1587.84
Kuwait	690	9	4083.56	90978.58	35279.4	28385.03	21921.89
Bahrain	692	1	11125.74	11125.74	11125.74	NA	11125.74
Qatar	694	9	26447.99	571824.91	244440.7	232588.2	34084.83
United Arab Emirates	696	33	33281.55	33446.81	33388.08	45.03	33422.64
Oman	698	128	3993.86	12501.26	11099.43	2032.06	12158.17
Afghanistan	700	255	213.91	51847.65	2136.03	4749.98	1062.9
China	710	3880	133.11	3133.64	930.03	245.31	980.39
Mongolia	712	741	222.03	10569.03	2377.79	1378.97	1830.53
Taiwan	713	23	7513.78	8613.66	8526.8	221.01	8570.96
PR Korea	731	65	924.52	4121.5	2010.96	702.87	1779.99
R Korea	732	60	5400.38	19110.91	9675.97	2480.22	9484.29
Japan	740	273	4008.96	430549.89	33046.22	32816.02	22897.6
India	750	1197	464.63	8351.56	1188.51	895.15	1051.46
Bhutan	760	12	1501.37	1502.39	1501.73	0.31	1501.74
Pakistan	770	336	620.26	5487.5	1444.79	629.2	1550.61
Bangladesh	771	55	502.7	941.93	635.79	109.53	624.8
Myanmar (Burma)	775	265	209.52	392.68	284.86	21.93	299.21
Sri Lanka	780	37	682.11	3435.55	1812.05	575.49	1778.13
Maldives	781	20	380.21	12337.49	4404.55	2812.14	3471.11
Nepal	790	53	602.97	647.31	627.88	7.11	626.73
Thailand	800	196	1037.13	12333.9	2720.99	2257.27	3752.64
Cambodia	811	64	260.77	1651.66	965.2	210.46	1006.78
Laos	812	79	752.45	1078.95	859.21	58.37	888.15
DR Vietnam	816	138	211.68	1595.36	670.91	233.78	754.92
Malaysia	820	144	2150.61	9626.13	5360.21	1261.94	5792.17
Singapore	830	1	21238.64	21238.64	21238.64	NA	21238.64
Brunei	835	4	41490.44	44330.17	43620.24	1419.86	44315.41
Philippines	840	236	859.93	4367.44	1686.56	629.33	2170.77
Indonesia	850	1047	0	105377.88	4513.98	10467.3	1772.96
Australia	900	3038	833.22	45573.84	20316.08	4711.22	20592.54
Papua New Guinea	910	250	943.46	3618.18	1108.94	364.79	1285.37
New Zealand	920	190	12058.86	23458.15	15796.46	1755.74	16027.24
Solomon Islands	940	52	739.02	5791.13	2958.78	1689.48	1835.21
Fiji	950	25	1534.3	10603.9	4735.74	2576.75	3237.69

Table A2: Summary Statistics for Full Sample of Cells

Variable	N	Mean	St. dev.	Min	Max
Ln GCP pc	55707	8.71	1.36	2.16	14.93
Ln GPD pc	55707	8.7	1.22	5.86	10.7
Ln infant mortality rate	53164	5.42	1.01	2.94	7.9
Positive income deviations	55707	0.59	12.54	0	1067.91
Negative income deviations	55707	-0.13	0.18	-1	0
Least Developed Country	55707	0.06	0.23	0	1
Ln distance to border (+1)	55707	1.02	0.7	0	2.79
Ln distance to capital (+1)	55707	7.03	1.07	2.51	8.98
Capital city	55707	0	0.05	0	1
Forrest cover (%)	55707	40.53	42.09	0	100
Mountainous terrain (%)	55707	23.66	36.43	0	100
Ln Population	55707	9.74	2.98	2.33	17.09
No. of cells (*1000)	55707	5132.75	5747.25	1	14518

Table A3: Summary Statistics for Case Control Sample of Cells

Variable	N	Mean	St. dev.	Min	Max
Ln GCP pc	342	8.59	1.33	5.28	11.86
Ln GPD pc	342	8.63	1.24	5.86	10.24
Ln infant mortality rate	330	5.5	1.01	2.94	7.42
Positive income deviations	342	0.25	0.83	0	11.6
Negative income deviations	342	-0.14	0.2	-0.95	0
Least developed country	342	0.08	0.27	0	1
Ln distance to border (+1)	342	1.02	0.72	0	2.71
Ln distance to capital (+1)	342	6.93	1.13	3.13	8.78
Capital city	342	0.02	0.14	0	1
Forest cover (%)	342	37.53	41.92	0	100
Mountainous terrain (%)	342	23.6	36.54	0	100
Ln Population	342	10.17	2.97	2.52	15.85
No. of cells (*1000)	342	4808.02	5524.2	1	14518

Table A4: Estimates for Country Level Data, Model 3 Including Fearon and Laitin (2003) Control Variables

	Model 1			Model 2			Model 3		
	Coef.	SE	Z	Coef.	SE	Z	Coef.	SE	Z
Intercept	3.482	1.239	2.809	4.592	1.731	2.653	0.519	4.711	0.11
Ln min. GCP pc	-0.708	0.183	-3.869	-0.636	0.199	-3.192	-0.857	0.289	-2.959
Ln GDP pc				-0.206	0.218	-0.948	-0.410	0.508	-0.807
LDC * ln max. GCP pc							1.662	1.304	1.275
Cell variance scaled							2.839	2.133	1.331
Ln No. of cells							-0.418	0.305	-1.371
Ln Population							0.574	0.288	1.994
Ln mountainous terrain							0.246	0.208	1.185
Non-contiguous							1.725	0.933	1.849
Oil-producing state							0.774	0.852	0.908
New state							-13.907	1455.4	-0.01
Instability							-0.197	0.846	-0.233
Polity II							0.126	0.059	2.117
Ethnic fractionalization							1.854	1.093	1.697
Religious fractionalization							0.851	1.503	0.566
N	149			149			132		
Log likelihood	-63.66 (df=2)			-63.20 (df=3)			-46.38 (df=15)		
LR Chi-Square	19.56 (df=1)			20.47 (df=2)			46.24 (df=14)		
AIC	131.31			132.40			122.76		

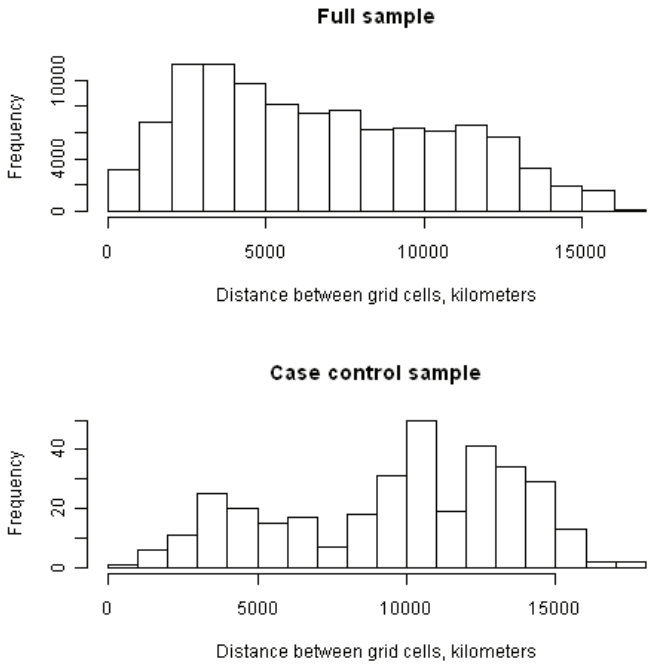

Figure A1: Distribution of Average Pairwise Cell Distances across Full Sample and Case Control Sample

Appendix to Article II

"Explaining the Development–Civil War Relationship"

Constructing the Economic Opportunity Cost Indicator, BDI per Capita

Not all the choices underlying the economic opportunity cost indicator, Bottom Decile Income (BDI) per capita, could be thoroughly explained in the article. First, the main reason for using household surveys rather than national accounts data to measure income is that incomes in poorer countries tend to be underestimated in national accounts data mainly due to their larger informal sector. My data also suggests that this is the case. The ratio of survey income to GDP clearly declines with economic development. An OLS regression of the ratio of survey income to GDP on GDP pc (logged) shows that the ratio decreases by between 52-34 % with 95 % confidence, when moving from the 10^{th} to the 90^{th} percentile of GDP pc.

Another question concerns which bottom quantile of the income distribution should be used. Whether to use the bottom decile (10%) or the bottom quintile (20%) can hardly be determined by theory. This choice is inconsequential, however, because mean bottom decile income is almost perfectly correlated with mean bottom quintile income in the survey data ($r = 0.993$).

The "mean income" estimate in the World Bank survey data is based on consumption expenditure for 2/3 of the observations and income for the rest. Income and consumption expenditure is clearly not equivalent. However, when looking at the income of the poor, it appears not to matter much whether income or expenditure is measured. Income has a higher mean value than expenditure, but is also less equally distributed. Chen and Ravallion (2004:13) assessed this by comparing surveys of both income and expenditure distribution for 27 countries. They found only a small and statistically non-significant difference between their poverty headcount measures based on income and those based on expenditure. I consider the risk of error from using all the data to be considerably lower than the costs in terms of precision and potential bias of excluding the third of the sample which has data on income, but not expenditure.

The number of countries included in the sample is limited by data availability. I only include countries with at least one original BDI pc survey observation in the main sample. My sample then contains 133 out of the 157 independent countries in the 1980-2006 period with a population above half a million.[1] BDI pc values could be tried imputed for countries without any

[1] The population restriction follows most other studies of civil war. Countries with less than half a million inhabitants will have a very small chance of experiencing a number of battle-deaths high enough to meet the civil war definition's threshold.

Table A1. List of Countries Excluded from the Sample by Region

Region	Countries Excluded
South & East Asia	Afghanistan
	Fiji
	Japan
	Korea, People's Rep.
	Myanmar
	New Zealand
	Singapore
Middle East & North Africa	Bahrain
	Cyprus
	Iraq
	Kuwait
	Lebanon
	Libya
	Oman
	Saudi Arabia
	Syria
	United Arab Emirates
Sub-Saharan Africa	Eritrea
	Liberia
	Mauritius
	Somalia
	Sudan
	Zimbabwe
Latin America	Cuba

original surveys using some other variables, like GDP per capita, as predictors in a statistical imputation model, but I consider the probable loss of reliability from this to be higher than the efficiency gains from a larger sample. Also, such an approach would bias the predicted BDI values towards the other explanatory variables of interest used in the imputation model, like GDP pc, roads and % urban. The countries excluded from the sample due to lack of data are listed in Table A1. Exclusion from the sample appears not to be systematically related to the risk of conflict onset: the percentage of country-years with an armed conflict onset is only slightly higher in the excluded countries (2.20 %) than in the included countries (1.75 %), and the difference is not statistically significant ($p = 0.45$).

The availability of survey data also places limits on the temporal extent of the sample. Figure A1 shows the total number of country survey observations by year for the 133 countries included, and a three-year average of this number. The number of surveys is low for most of the 1980s, but rises sharply in the beginning of the 1990s. Figure A2 shows the mean of the number of years before each country's first survey observation by starting point. If the sample started in

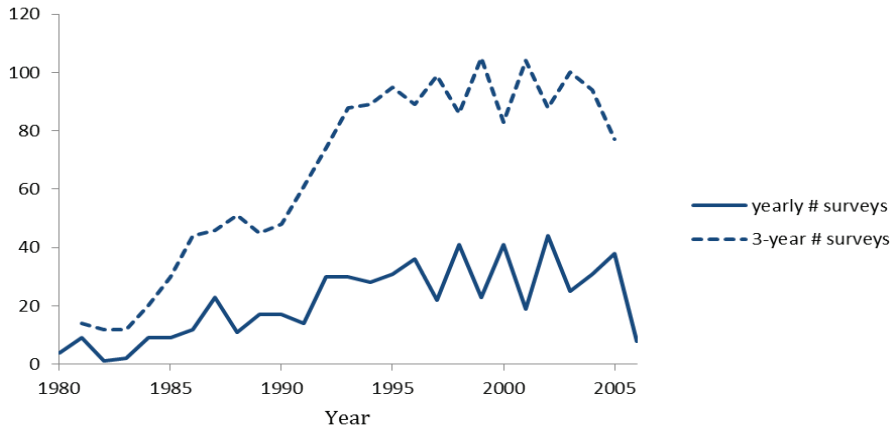

Figure A1. Number of Country Survey Observations over Time in the Sample

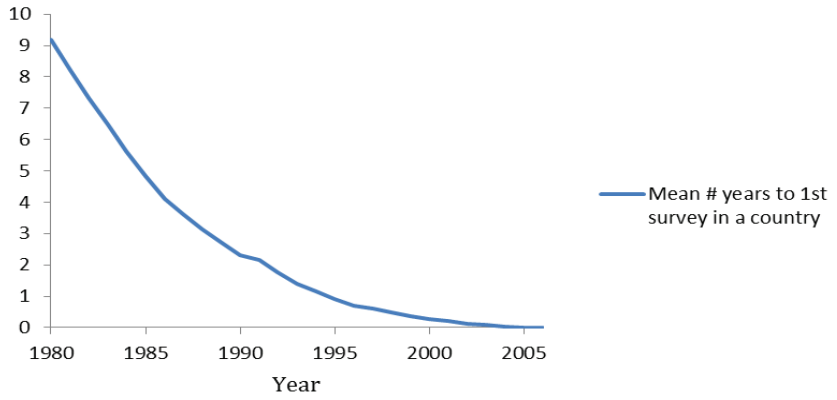

Figure A2. Mean Number of Years to the First Survey in the Countries

1980, there would be an average gap of almost 10 years before each country's first survey; starting in 1990, the average gap would decline to 2.3 years. One would have to extrapolate most time series far back to include all years from 1980. On the other hand, starting as late as the mid-1990s, one would lose much valuable information. Since civil war onset is a rare event, onset-years provide more valuable information than non-onset years. The years 1989-1993 have many onsets, whereas the years before 1989 have very few onsets (Figure A3). This, together with the fact that there is a great increase in surveys from around 1990, leads me to restrict the sample to the 1989-2006 period. I do not go beyond 2006 because I lack data both for BDI pc and some other explanatory variables for these years.

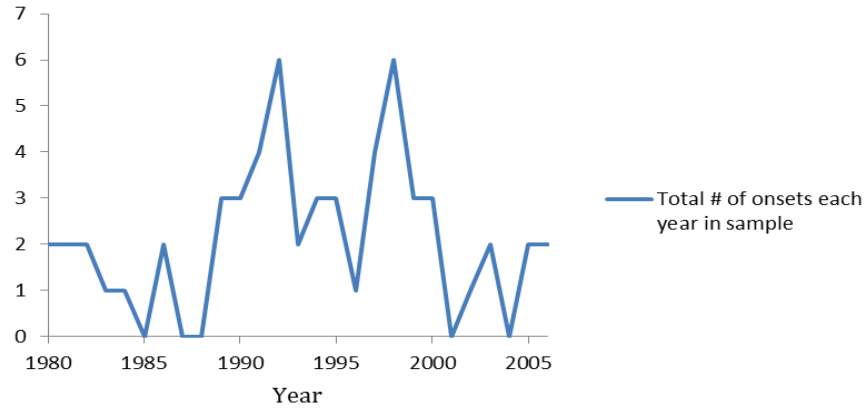

Figure A3. Total Number of Onsets by Year in the Country Sample

The time-series still have holes when limiting the sample to the 1989-2006 period. The mean number of years with surveys for the countries in the sample is 3.7, and 16 countries have only one survey observation. Clearly, values in the time-series must be imputed to facilitate a statistical analysis. Two imputation methods stand out as the best candidates. The first is linear interpolation and the second is using growth rates of national accounts data (household expenditure or GDP) to extend the time-series. The second may be recommendable if there are many years between the observations and BDI follows the same time trend as these national accounts measures. The mean number of years between two surveys is 2.7, and 37 countries have more than five years between two surveys, 15 countries have more than seven years between two surveys, and 4 countries have more than 10 years between two surveys. Looking at some of the countries with long intermittence between surveys suggests that interpolation may still be the best alternative, as BDI pc sometimes does not follow the trend of GDP pc. Guinea, Rwanda, and Sierra Leone are examples of this. I therefore choose interpolation between surveys as the first imputation method, and turn to national accounts data as an alternative to linear extrapolation when interpolation is not possible. BDI pc is log-transformed before interpolating.

In the next step, I use the existing BDI pc observations as reference points, and extend the time-series using the growth rate of private household consumption expenditure pc (PPP) from national accounts (World Bank 2008). I thus assume that the Lorenz curve has not shifted, and

Table A2. 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
Observations, total	2323
Missing after imputation	6

that growth rate of national accounts private consumption expenditure per capita changes in parallel with the survey mean income/expenditure.[2]

In the third step, for observations lacking data on private consumption expenditure I use the growth rate of GDP pc (PPP) to estimate missing BDI observations. I primarily use data from the World Bank (2008) on PPP-adjusted GDP pc 2005US$. For states that have experienced border changes, I use the estimates of "Real" (PPP-adjusted) GDP pc from Gleditsch (2002).[3] After this step, only six observations (and no onsets) are left missing.[4] Table A2 shows the number of BDI observations imputed with each method.

Figure A4 shows the time-series of Spain as an example, where there are four original survey observations – for 1980, 1990, 1995, and 2000. Between these surveys, interpolation is used to estimate missing values. For the remaining years, 2001-2006, the growth rate of household consumption per capita is used to extend the time-series. The imputed and original BDI pc values are shown by different symbols, whereas household consumption pc is shown with a line. We can see that BDI follows household consumption quite closely over time, giving confidence to the imputed values.

An alternative to extrapolating the BDI values using national accounts data is *multiple imputation*: building a statistical model to estimate the missing values (Honaker and King 2010). I perform an out-of-sample prediction test to evaluate the relative performance of these two methods. I select the 9 countries with most BDI survey observations (more than 7) and existing observations in 2005 or 2006. I then remove their observations for 2000-2006 from the sample, and try to predict these values using my method of extending the time-series and multiple imputation. I construct a multiple imputation model and use Amelia II to estimate the missing values

[2] 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.
[3] I also use PWT 6.3 "Real" GDP per capita estimates for a few country-years missing in WDI (2008) (Heston, Summers, and Aten 2009).
[4] The missings are 5 years in Turkmenistan's time-series and Yugoslavia, 1989.

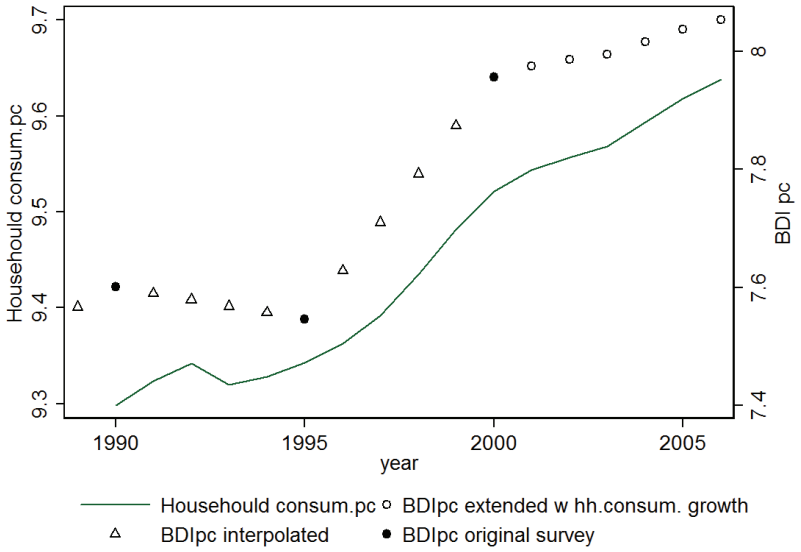

Figure A4. Spain BDI per Capita Time-Series Scatter Plot

based on this.[5] I impute five datasets and calculate the mean BDI value for these five datasets. The results suggest that extending the time-series using national accounts data works considerably better than the multiple imputation model. The extended time-series BDI and the true (interpolated) values correlate r = 0.91, whereas the correlation between the multiply imputed BDI and the true values for is a low r = 0.35 (N=47).[6] The mean percentage error (in either positive or negative direction) is 2.6 % for the extended BDI and 6.8 % for the multiply imputed BDI.

[5] I tried several different models. The best one, which I settled for, included these variables: BDI, GDP pc (ln), Accessibility index, Population (ln), proximity to war, civil war onset, household consumption pc (ln), infant mortality rate (ln), Polity 2 (democracy/autocracy scale), Oil production value pc, region dummies, country dummies, and year dummies with three polynomials.

[6] Looking only at the original survey observations without interpolating between them gives quite similar correlations. R=0.85 for extended BDI and r = 0.38 for multiply imputed BDI (N=20).

Tables of Coefficients from Regressions in the Article

Table A3. Coefficients from Regressions Underlying Figure 2 in the Article

	(1)	(2)	(3)	(4)	(5)	(6)
Ln GDP pc	-0.48***					
	(0.14)					
Ln BDI pc		-0.25				
		(0.17)				
State reach			-3.25***			
			(0.95)			
Ln Road pc				-0.46***		
				(0.14)		
%Urban					-0.03***	
					(0.01)	
Ln Tel pc						-0.27***
						(0.09)
Ln Population	0.40***	0.40***	0.38***	0.33**	0.39***	0.41***
	(0.13)	(0.14)	(0.14)	(0.16)	(0.12)	(0.15)
Proximity of	1.40***	1.57***	1.44***	1.62***	1.45***	1.45***
war	(0.43)	(0.44)	(0.43)	(0.42)	(0.42)	(0.44)
Constant	-7.46***	-9.78***	-9.14***	-9.58***	-9.98***	-11.13***
	(2.15)	(2.31)	(2.33)	(2.91)	(2.00)	(2.52)
Observations	2321	2321	2321	2321	2321	2321
Onsets	48	48	48	48	48	48
log pseudo LL	-202.44	-207.42	-201.85	-205.50	-202.48	-202.91

Robust standard errors in parentheses. * p < 0.10; ** p< 0.05; *** p< 0.01
Dependent variable is civil war onset (UCDP/PRIO data, Sambanis threshold) in all models.

Table A4. Coefficients from Regressions Underlying Figure 3 in the Article

	(1)	(2)	(3)
Ln GDP pc	-0.88***	-0.12	
	(0.26)	(0.34)	
Ln BDI pc	0.59*		0.46*
	(0.35)		(0.26)
State reach		-2.54	-5.11***
		(2.26)	(1.30)
Ln Population	0.39***	0.38***	0.35**
	(0.13)	(0.14)	(0.15)
Proximity of war	1.40***	1.42***	1.50***
	(0.44)	(0.42)	(0.45)
Constant	-7.64***	-8.64***	-10.41***
	(2.30)	(2.50)	(2.71)
Observations	2321	2321	2321
Onsets	48	48	48
log pseudo LL	-200.41	-201.79	-200.23

Robust standard errors in parentheses. * p < 0.10; ** p< 0.05; *** p< 0.01
Dependent variable is civil war onset (UCDP/PRIO data, Sambanis threshold) in all models.

Table A5. Coefficients from Regressions Underlying Figure 4 in the Article

	(1) Sambanis onset; Fearon-Laitin controls	(2) Sambanis onset; Amelia BDI imputa-tions	(3) Sambanis onset; 4 yr intermit	(4) Sambanis onset; late coding	(5) UCDP/PRIO civil war onset
Ln BDI pc	0.77***	0.75**	0.69***	0.41*	0.32
	(0.219)	(0.335)	(0.234)	(0.237)	(0.308)
State reach	-7.49***	-6.48***	-5.85***	-3.98***	-4.06**
	(1.607)	(1.681)	(1.240)	(1.230)	(1.988)
Ln Population	0.25**	0.34**	0.31**	0.28*	0.03
	(0.111)	(0.155)	(0.148)	(0.144)	(0.133)
Proximity of war	0.86	1.57***	1.36***	2.00***	1.53**
	(0.584)	(0.456)	(0.486)	(0.478)	(0.612)
New state (dummy)	3.28***				
	(0.808)				
Noncontiguous state (dummy)	0.91**				
	(0.375)				
Ln %Mountainous terrain	0.13				
	(0.110)				
Democracy	0.02				
	(0.029)				
Instability (dummy)	-0.04				
	(0.442)				
Ethno-linguistic fractionalization	1.91**				
	(0.752)				
Religious fractionalization	0.07				
	(0.689)				
Prior war (dummy)	-0.18				
	(0.519)				
Oil dependency (dummy)	1.34				
	(0.944)				
Constant	-10.71***	-11.33***	-10.86***	-9.78***	-5.38**
	(2.322)	(2.879)	(2.668)	(2.535)	(2.274)
Observations	2,224	2,321	2,321	2,321	2,321
Onsets	47	48	39	47	25
log pseudo LL	-178.2		-173.6	-197.2	-127.9

Robust standard errors in parentheses. * p < 0.10; ** p< 0.05; *** p< 0.01

Table of Coefficients from Additional Tests

Table A6. Coefficients from Additional Regression Models

	(1) Sambanis onset, BDI-Africa interact.	(2) Sambanis onset, State reach-Africa interact.	(3) Sambanis onset, excluding Ethiopia
Ln BDI pc	0.53**	0.63***	0.44
	(0.251)	(0.223)	(0.278)
State reach	-4.94***	-5.69***	-4.84***
	(1.447)	(1.560)	(1.534)
Africa (dummy)	0.19	0.91	
	(0.650)	(0.727)	
Ln BDI pc*Africa	-0.10		
	(0.509)		
State reach*Africa		2.61	
		(2.930)	
Ln Population	0.37**	0.38***	0.35**
	(0.149)	(0.145)	(0.153)
Proximity of war	1.53***	1.61***	1.49***
	(0.445)	(0.461)	(0.472)
Constant	-11.42***	-11.71***	-10.42***
	(2.653)	(2.581)	(2.851)
Observations	2,321	2,321	2,303
Onsets	48	48	48
log pseudo LL	-200.0	-199.6	-190.4

Robust standard errors in parentheses. * p < 0.10; ** p< 0.05; *** p< 0.01

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Appendix to Article III

"Relative Capacity and the Spread of Rebellion: Insights from Nepal"

Time Plots for Key Variables

Figures A1-A3 show how the values of onset versus non-onset districts change over time for the variables that I hypothesize should have a temporally contingent effect. The onset observations are marked by dots, the running mean of the onset observations by the short-dashed line,[7] the running mean of the non-onset observations by the long-dashed line, and the standard deviation of the remaining observations in the sample is shown by the solid line. The figures show that, as hypothesized, the early onset districts tend to have considerably higher levels of *UPF support*, lower *accessibility*, and a higher proportion have *nearby insurgency* than the average non-onset district. For all the variables, the difference between onset and non-onset districts declines over time, and from around 2001-2002 there is no, or almost no, difference. The figures also address the concern that the variance in the sample may decline over time, as districts see onset (failure) and drop out. For *UPF support*, the variance declines somewhat over time, but there is still considerable variance left at the time of the last onset observation. For *accessibility*, the variance is almost constant over time. For nearby insurgency, in contrast, variance steeply declined in the first half of 2002, and by mid-2002 all remaining districts had at least one insurgency nearby. This is not a considerable problem for testing my hypothesis of temporal contingency, since by early 2002 the divergence between onset and non-onset districts had already disappeared.

[7] The running means are calculated using locally weighted scatterplot smoothing (LOWESS) with a bandwidth of 0.8.

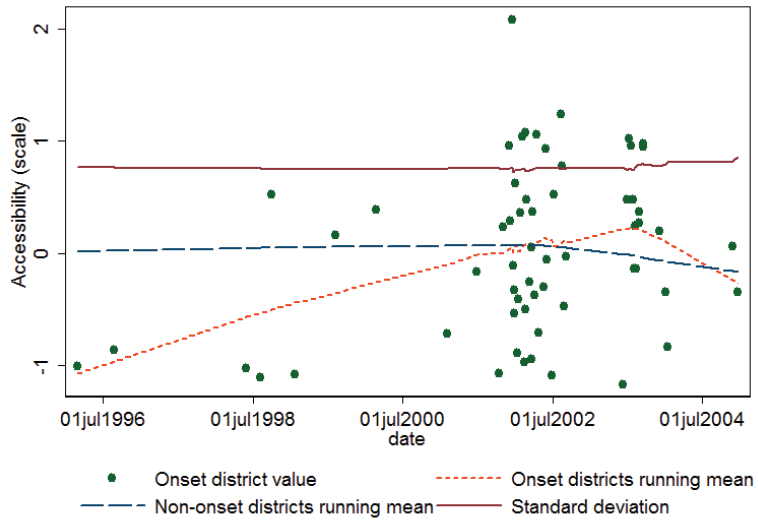

Figure A1. Variance and Onset and Non-Onset Means of *Accessibility* over Time

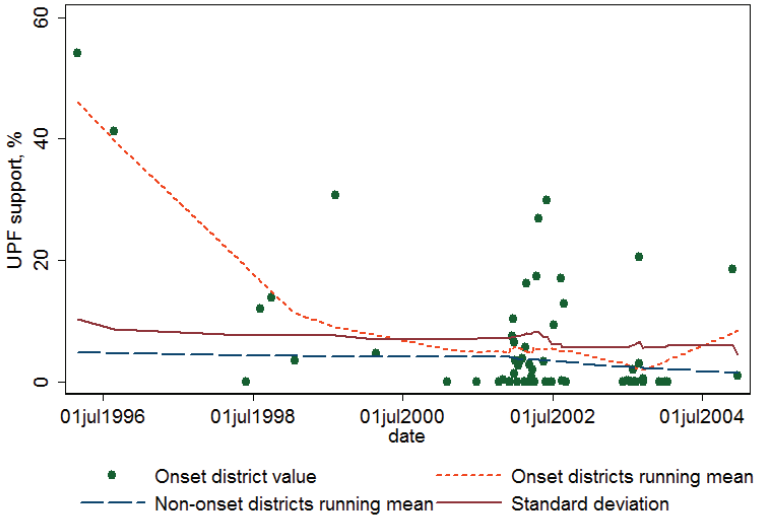

Figure A2. Variance and Onset and Non-Onset Means of *UPF Support* over Time

Table A1. Rebel-to-Government Troops, Government Sources

Year	*Gov. estimate*	*Mean estimate (inter-/extrapolated.)*	*Source*
1996	Missing	200	Commander of Maoist PLA's Eastern Division, "Ananta"; cited in Ogura (2008:13)
1997	Missing	350	Maoist Central Committee member; cited in Onesto (2005:92)
1998	2300-3450	2875	Police estimates; referred to in Manchanda (1998)
1999	5000-6000	5500	Police estimates; referred to in Singh (1999)
2000	Missing	6500	
2001	5000-10000	7500	Government officials; referred to Miglani (2001)
2002	Missing	8000	
2003	Missing	8500	
2004	Missing	9000	
2005	9500	9500	Colonel Victor J.B. Rana, RNA press conference, May 2005; referred to in ICG (2005:8)
2006	Missing	10000	

Robustness Tests

Below, I show results from models using an alternative *rebel-to-government troops* measure primarily based on government sources. Table A1 lists the government information that is used for this variable. Since government estimates are missing for several years, I use Maoist sources for the initial two years and linear inter- and extrapolation for the rest. The final values are listed in the 'Mean estimate' column.

Tables A2-A4 shows results from Cox analyses using different operationalizations of the dependent variable as well as one additional measure of relative capacity: *rebel-to-government troops* based on estimates from government sources. The models of Table A2 use a slightly lower threshold for onset: 3 (instead of 5) days of insurgent killings in a half-year period. Most of the central variable coefficients are quite similar. The largest difference is that the accessibility interactions with time and rebel-to-government troops become weaker. The interactions with *rebel-to-government troops* based on data from government sources (fourth column) give results very similar to the other relative capacity measures.[8]

In Table A3, insurgency onset is operationalized as at the first time a district experiences at least five days of insurgent *or government* killings. The signs of the main variables remain similar, but results become somewhat more dependent on the relative capacity measure employed. The three first *nearby insurgency* interactions become even more strongly negative,

[8] The coefficients are smaller, but the range of this variable is larger than the other relative capacity measures, implying that the difference of moving from a low to a high quantile on the variable is relatively similar.

whereas the interaction with the second rebel-to-government troops measure becomes slightly weaker, although still negative. For *accessibility,* the interactions with *time* and two of the relative capacity measures are clearly positive (but only significant for the last measure), whereas it is almost zero for the first *rebel-to-government troops* measure.

In Table A4, insurgency onset is operationalized as at the first time a district experiences at least ten conflict-related fatalities (all victims and perpetrators included). Results for *UPF support* are largely similar, and strongly significant. The *nearby insurgency* coefficients become more strongly dependent on time and relative capacity using this onset measure. The influence of *accessibility*, on the other hand, becomes less consistently dependent on time and relative capacity. Two of the relative capacity interactions with accessibility are positive, as hypothesized, but one becomes negative (although not significant).

In general, these tests indicate that the contingency of *UPF support* and *nearby insurgency* on relative capacity is robust to changes in operationalization of insurgency onset and relative capacity. The influence of *accessibility* is consistently declining over time, but its dependency on relative capacity is somewhat less certain. Two out of the eleven estimates of interaction with relative capacity (from all tables) show nu such dependency.

Table A2. Cox Models Using Alternative Onset Threshold (3 Days of Maoist Killings in 6 Months)

Variables	(1) Interactions with time	(2) Interactions with rebel-to-gov. troops, Mil. Bal. source	(3) Interactions with gov. force fatalities	(4) Interactions with rebel-to-gov. troops, Gov. sources
Radio ownership, %	0.052**	0.051**	0.052**	0.052**
	(0.018)	(0.017)	(0.018)	(0.016)
UPF support, %	0.065**	0.062**	0.065**	0.045
	(0.023)	(0.021)	(0.021)	(0.025)
Nearby insurgency	1.110*	1.190*	1.132*	1.653**
	(0.540)	(0.577)	(0.574)	(0.524)
Accessibility	-1.276**	-1.059**	-1.066**	-1.035**
	(0.395)	(0.341)	(0.308)	(0.298)
UPF support x Time, years	-0.026**			
	(0.008)			
UPF support *x* Rebel-to-gov. troops, %		-0.015		-0.012**
		(0.008)		(0.003)
UPF support *x* Gov. force fatalities			-0.019	
			(0.010)	
Nearby insurgency x Time, years	-0.337			
	(0.271)			
Nearby insurgency x Rebel-to-gov. troops, %		-0.267		-0.131
		(0.262)		(0.083)
Nearby insurgency x Gov. force fatalities			-0.401	
			(0.373)	
Accessibility x Time, years	0.257			
	(0.153)			
Accessibility *x* Rebel-to-gov. troops, %		0.069		0.043
		(0.113)		(0.034)
Accessibility *x* Gov. force fatalities			0.093	
			(0.165)	
HDI	-5.977	-6.885*	-7.442*	-6.745*
	(3.632)	(3.498)	(3.558)	(3.341)
Communist mandates	0.060	0.063	0.079	0.051
	(0.166)	(0.158)	(0.160)	(0.150)
Tenants, %	-0.026	-0.020	-0.018	-0.020
	(0.016)	(0.017)	(0.017)	(0.017)
Magars, %	-0.030	-0.025	-0.021	-0.021
	(0.030)	(0.029)	(0.028)	(0.028)
Hill high-castes, %	-1.884*	-1.826	-1.868	-2.015*
	(0.942)	(0.937)	(0.989)	(0.902)
log Population	1.244**	1.237**	1.252**	1.310**
	(0.265)	(0.249)	(0.251)	(0.238)
Onsets (failures)	70	70	70	70
Districts	75	75	75	75
log pseudo LL	-206.8	-210.8	-210.6	-204.1

Onset threshold: Three days of insurgent killings within a half-year period. Coefficients shown. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table A3. Cox Models Using Alternative Onset Threshold (5 Days of Fatal Events in 6 Months)

Variables	(5) Interactions with time	(6) Interactions with rebel-to-gov. troops, Mil. Bal. source	(7) Interactions with gov. force fatalities	(8) Interactions with rebel-to-gov. troops, Gov. sources
Radio ownership, %	0.038*	0.038*	0.039*	0.043*
	(0.019)	(0.019)	(0.019)	(0.018)
UPF support, %	0.039*	0.038*	0.042*	0.020
	(0.017)	(0.016)	(0.017)	(0.019)
Nearby insurgency	0.121	0.051	0.104	0.401
	(0.426)	(0.443)	(0.438)	(0.425)
Accessibility	-1.055**	-0.876**	-1.008**	-0.977**
	(0.299)	(0.287)	(0.281)	(0.289)
UPF support x	-0.020**			
Time, years	(0.007)			
UPF support *x*		-0.020*		-0.010**
Rebel-to-gov. troops, %		(0.010)		(0.003)
UPF support *x*			-0.022*	
Gov. force fatalities			(0.010)	
Nearby insurgency x	-0.515*			
Time, years	(0.257)			
Nearby insurgency x		-0.570		-0.090
Rebel-to-gov. troops, %		(0.306)		(0.068)
Nearby insurgency x			-0.951**	
Gov. force fatalities			(0.302)	
Accessibility x	0.159			
Time, years	(0.114)			
Accessibility *x*		0.005		0.065*
Rebel-to-gov. troops, %		(0.102)		(0.030)
Accessibility *x*			0.140	
Gov. force fatalities			(0.139)	
HDI	-3.911	-4.636	-4.849	-4.623
	(3.834)	(3.788)	(3.818)	(3.736)
Communist mandates	-0.059	-0.034	-0.053	-0.070
	(0.099)	(0.098)	(0.098)	(0.099)
Tenants, %	-0.042*	-0.040*	-0.039*	-0.043**
	(0.016)	(0.017)	(0.017)	(0.016)
Magars, %	-0.012	-0.007	-0.006	-0.009
	(0.014)	(0.014)	(0.015)	(0.012)
Hill high-castes, %	-1.057	-0.928	-0.899	-1.344
	(0.807)	(0.807)	(0.846)	(0.857)
log Population	0.893**	0.906**	0.920**	1.023**
	(0.182)	(0.178)	(0.177)	(0.191)
Onsets (failures)	70	70	70	70
Districts	75	75	75	75
log pseudo LL	-206.8	-210.8	-210.6	-204.1

Onset threshold: Five days of insurgent or government killings within a half-year period. Coefficients shown. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table A4. Cox Models Using Alternative Onset Threshold (10 Fatalities in 6 Months)

Variables	(9) Interactions with time	(10) Interactions with rebel-to-gov. troops, Mil. Bal. source	(11) Interactions with gov. force fatalities	(12) Interactions with rebel-to-gov. troops, Gov. sources
Radio ownership, %	0.044*	0.041*	0.043*	0.046**
	(0.018)	(0.018)	(0.018)	(0.018)
UPF support, %	0.031	0.028	0.035*	0.006
	(0.017)	(0.015)	(0.015)	(0.021)
Nearby insurgency	-0.310	-0.327	-0.337	0.088
	(0.416)	(0.433)	(0.424)	(0.438)
Accessibility	-0.641*	-0.486	-0.680*	-0.634
	(0.324)	(0.310)	(0.326)	(0.348)
UPF support x	-0.027**			
Time, years	(0.009)			
UPF support *x*		-0.023*		-0.010**
Rebel-to-gov. troops, %		(0.010)		(0.003)
UPF support *x*			-0.029**	
Gov. force fatalities			(0.011)	
Nearby insurgency x	-0.508			
Time, years	(0.270)			
Nearby insurgency x		-0.626*		-0.131
Rebel-to-gov. troops, %		(0.308)		(0.077)
Nearby insurgency x			-0.685*	
Gov. force fatalities			(0.289)	
Accessibility x	0.130			
Time, years	(0.145)			
Accessibility *x*		-0.100		0.026
Rebel-to-gov. troops, %		(0.141)		(0.037)
Accessibility *x*			0.107	
Gov. force fatalities			(0.148)	
HDI	-9.990*	-10.705*	-10.476*	-10.321*
	(4.373)	(4.287)	(4.309)	(4.254)
Communist mandates	-0.080	-0.062	-0.070	-0.083
	(0.101)	(0.104)	(0.104)	(0.102)
Tenants, %	-0.014	-0.010	-0.011	-0.012
	(0.016)	(0.017)	(0.016)	(0.017)
Magars, %	0.010	0.013	0.015	0.012
	(0.014)	(0.014)	(0.014)	(0.012)
Hill high-castes, %	1.485	1.774*	1.585	1.084
	(0.833)	(0.884)	(0.888)	(0.906)
log Population	0.736**	0.798**	0.794**	0.893**
	(0.198)	(0.210)	(0.190)	(0.216)
Onsets (failures)	73	73	73	73
Districts	75	75	75	75
log pseudo LL	-214.4	-218.5	-216.6	-214.3

Onset threshold: Ten conflict-related fatalities within a half-year period. Coefficients shown. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table A5. Data Sources

Variable	*Source*
Onset of insurgency	INSEC (2010) event data
UPF support, %	Election Commission (1992)
Communist mandates	Election Commission (1992)
Magars, %	Sharma (2008)
Hill high-castes, %	Do and Iyer (2010)
Mean slope, %	CBS, WFP, WB (2006)
Forested, %	Do and Iyer (2010)
Roads	Do and Iyer (2010)
Distance from capital	http://www.distancefromto.net/
Tenants, %	Joshi and Mason (2008)
Radio ownership, %	UNDP (2004)
HDI	UNDP (2004)
Population	CBS (1991)
Government force fatalities	INSEC (2010) event data
Rebel-to-government troops	Government troops: International Institute for Strategic Studies (1996/1997-2006/2007). Sources for rebel troops listed in the text and in the appendix.

Table A6. Summary Statistics

	N	Mean	Std. Dev.	Minimum	Maximum
Dependent variable					
Insurgency onset	2678	0.02	0.15	0	1
Stable covariates					
Radio ownership, %	75	53.38	12.43	30.07	80.94
UPF votes, %	75	5.58	10.31	0	54.08
Accessibility	75	0	0.77	-1.38	2.33
HDI	75	0.45	0.07	0.30	0.65
Communist mandates	75	1.11	1.28	0	6.00
Tenants, %	75	11.76	9.29	0	43.24
Magars, %	75	8.17	10.61	0	50.90
Hill high-castes, %	75	0.41	0.22	0.04	0.85
log Population	75	12.16	0.85	8.59	13.42
Time-varying covariates					
Nearby insurgency	2678	0.60	0.49	0	1
Time, years	60	6.01	1.74	0.03	8.87
Gov. force fatalities, daily	60	1.78	0.97	0.00	3.17
Rebel-to-gov. troops, % (Source: Mil. Balance)	8	3.36	1.52	0.71	5.49
Rebel-to-gov. troops, % (Source: Gov. officials)	8	10.07	5.83	0.71	18.75

Note: For the time-varying variables, the onset operationalization in the article is used. *Insurgency onset* and *nearby insurgency* are coded for each remaining (non-failing) district at each failure date, giving 2678 observations. Stable covariates vary over districts (75). *Time* (fractions of years allowed) and *gov. force fatalities* vary over failure times (60); and the *rebel-to-government troops* measures vary over years (8; the sample begins with the first onset in February 1996 and ends with the last onset in December 2004).

Table A7. Correlations (for Variables in Table 1, Model 3 in the Article)

	1	2	3	4	5	6	7	8	9	10	11	12	13
Radio ownership (1)	1.00												
UPF support (2)	0.05	1.00											
Nearby insurgency (3)	0.15	0.09	1.00										
Accessibility (4)	0.09	0.10	-0.07	1.00									
HDI (5)	0.60	0.06	-0.07	0.60	1.00								
Communist mandates (6)	0.21	0.01	-0.13	0.32	0.40	1.00							
Tenants (7)	0.10	-0.14	-0.14	0.41	0.37	0.16	1.00						
Magars (8)	0.27	0.09	0.19	-0.09	0.20	-0.13	-0.37	1.00					
Hill high-castes (9)	0.30	-0.06	0.14	-0.39	-0.21	-0.07	-0.10	-0.08	1.00				
log Population (10)	-0.12	-0.07	-0.24	0.41	0.28	0.32	0.18	0.07	-0.12	1.00			
UPF X Reb-to-gov. Troops (11)	0.00	-0.10	0.05	0.01	0.01	-0.02	0.00	-0.02	-0.02	-0.02	1.00		
Nearby ins. X Reb-to-gov. Troops (12)	-0.02	-0.01	0.37	0.01	0.01	-0.05	0.00	0.01	0.01	-0.09	0.09	1.00	
Access. X Reb-to-gov. Troops (13)	0.02	0.01	-0.01	-0.01	-0.02	-0.04	0.01	0.02	0.01	0.00	0.06	-0.10	1.00

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Appendix to Article IV

"How Can Initially Weak Insurgent Groups Grow? Lessons from Nepal"

Table A1. List of Interviews

Nr. of interview	Interviewee(s), short description	Interviewee's place of origin
1	Journalist	Jungar, Rolpa
2	Congress Party district chairman, Rolpa	Rolpa (VDC unknown)
3	Student, then Maoist full-timer (PLA)	Rangsi, Rolpa
4	Student, then Maoist full-timer (PLA)	Madichaur, Rolpa
5	Man Bahadur Buddha Magar ('Bikalpa'): YCL India coordinator, then Maoist full-timer and district committee member, Rolpa, then district chairman, Rolpa	Gam, Rolpa
6	Rastriya Prajatantra Party (RPP) politician, Rolpa	Rolpa (VDC unknown)
7	CPN-UML district chairman, Rolpa	Jungar, Rolpa
8	7 members of Maoist Martyr's Family Society	Rolpa (various VDCs)
9	Civil society activist, Rolpa	Rolpa (VDC unknown)
10	Teacher, higher secondary school in Liwang	Ghodagaun, Rolpa
11	Peasant	Liwang, Rolpa
12	Teacher, lower secondary school in Kotgaun	Dhawang, Rolpa
13	Peasant, then Maoist part-time cadre	Madichaur, Rolpa
14	Shopkeeper/peasant	Kotgaun, Rolpa
15	Congress politician/landowner, fled during the war	Kotgaun, Rolpa
16	Teacher, lower secondary school in Madichaur	Tulsipur, Dang
17	Peasant	Jankot, Rolpa
18	Peasant	Madichaur, Rolpa
19	Student, then Maoist full-timer (PLA)	Madichaur, Rolpa
20	Peasant	Madichaur, Rolpa
21	Shopkeeper/peasant	Madichaur, Rolpa
22	Farmer/landowner	Madichaur, Rolpa
23	Laborer	Madichaur, Rolpa
24	Shopkeeper/peasant	Madichaur, Rolpa
25	Peasant	Jankot, Rolpa
26	Teacher, lower secondary school, Jankot	Jankot, Rolpa
27	Peasant	Jankot, Rolpa
28	Housewife	Jankot, Rolpa
29	Shopkeeper/peasant	Jankot, Rolpa
30	Peasant	Madichaur, Rolpa
31	Surender Thapa Gharti: Teacher; then Maoist full-timer (party wing), Rolpa	Madichaur, Rolpa*
32	Gulaf Magar: Student, then Maoist full-timer (PLA, then party wing), Rolpa	Madichaur, Rolpa*
33	Netra Bahadur Gharti ("Saddam"): Peasant and Maoist part-timer, then Maoist full-timer (party wing)	Jankot, Rolpa*

* Phone interviews December 2011-January 2012 with follow-up phone interviews December 2012.
For place of origin, the VDC name is given except for interviewees from Madichaur, who belong to Kotgaun or Jankot VDC.

Kalyvas' Protocol for Coding Control

Zone 1: Incumbent combatants permanently garrisoned in the village or within a one-hour radius; incumbent combatants and administrators operate freely during all times of day and night; no insurgent activity reported; insurgent clandestine organizations never set up or completely destroyed.

Zone 2: Incumbent combatants permanently garrisoned in the village or within a one-hour radius; incumbent combatants and administrators operate freely during all times of day and night; insurgent clandestine organizations operate inside the village; clandestine meetings take place; sporadic visits at night by insurgent combatants.

Zone 3: Incumbent combatants permanently garrisoned in the village or within a one-hour radius, but do not move freely at night; incumbent administrators usually do not sleep in their homes; insurgent organizers are active; meetings take place regularly at night; regular visits by insurgent combatants at night.

Zone 4: Insurgent combatant permanently garrisoned in the village or nearby; insurgent combatants and administrators operate freely during all times of day and night; incumbent clandestine organizations operate inside village, and/or clandestine meetings take place, and/or sporadic visits by incumbent combatants.

Zone 5: Insurgent combatants permanently garrisoned in the village or nearby; insurgent combatants and administrators operate freely during all times of day and night; no incumbent activity reported; insurgent clandestine organizations never set up or completely destroyed.

Note: Reproduced from Kalyvas (2006:421).

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