From Mountains to Movements:

Dissent, Repression and Escalation to Civil War

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Abstract

Recent analyses of civil war have suggested that these events grow out of lower-level conflict dynamics involving state repression and political dissent. Unfortunately, existing work has either indirectly measured escalation or considered only one argument in isolation of others. Examining 149 countries from 1976 to 1999, we develop new measurements for lower-level conflict and explore alternative explanations for conflict escalation within rural, urban and combined contexts. Our results disclose that there is no single theory of escalation to civil war but rather there are several, which explain different civil wars. This research has significant implications for how we understand and examine the origins of large-scale conflict.

Where do civil wars come from? While existing research has improved our understanding of onset in numerous ways (e.g. Collier and Hoeffler 2002; Reynal-Querol 2002; Elbadawi and Sambanis 2002; Fearon and Laitin 2003),[1] perhaps no insight has been as significant as the acknowledgement that large-scale conflict grows out of lower-level state-dissident interactions and that the key to understanding civil war onset lies in identifying the escalatory process that leads from one form of contention to another (e.g., Fearon and Laitin 2003; Sambanis and Zinn 2005). This innovation is particularly important because it: 1) diminishes the current reliance upon essentially invariant structural characteristics to guide theoretical explanations and empirical investigations (e.g., mountains, gdp per capita and levels of democracy) and 2) points toward an important factor long-highlighted within literature more concerned with protest, insurgency and state repression (e.g., the movements of distinct political actors in competition with one another). To date, our understanding of escalation is quite limited; either arguments have been indirectly assessed (e.g., Fearon and Laitin 2003) or only one type of escalation has been considered (e.g., Sambanis and Zinn 2005). The current paper is directed toward shedding light on this topic.

Specifically, we identify and analyze three escalation arguments which we view as not competitive but alternative paths to civil war. Within one, the "*inflammation*" hypothesis, civil war emerges when state repression prompts dissidents to increase their rebellious efforts (e.g., Sambanis and Zinn 2005). Here, prior repressive activity enrages citizens who subsequently increase dissident behavior to the level of civil war. Within another, the "*incapacity*" hypothesis, large-scale conflagration emerges when despite dissident behavior political authorities are unable to apply significant levels of state repression. Here, repressive behavior is initially low, challengers perceive a weakness in state coercive power (which reduces the potential costs of collective action), and in this context, dissidents increase their rebellious efforts to civil war. Within a third, the "*ineffectiveness*" hypothesis (derived from the "Insurgency" argument [e.g., Fearon and Laitin 2003]), large-scale conflagration emerges when governments apply coercion and dissidents simply outlast or outfight states. Here, repressive behavior is initially high, subsequent dissident activity is high, challengers perceive a weakness in state coercive power

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[1]See Sambanis (2002) for a good review.

(despite sustaining costs), and in this context those in opposition to authority increase their rebellious efforts, resulting in civil war.

To date, research has not explicitly considered these arguments. Consideration of these lower-level conflict dynamics is important for it allows us to assess an alternative way in which political-economic context influences civil war. For instance, some analyses suggest that the effects of repression and dissent on civil war are conditioned by structure (e.g., Fearon and Laitin 2003). If this is the case, then models of onset need to be more specifically tailored to estimate this type of relationship by including interaction terms. Alternatively, it is possible that contextual factors exert some influence on civil war onset in and of themselves distinct from their relationship with lower-level conflict - a proposition that is implicitly maintained within existing research, which employs political, economic and demographic factors as explanatory factors. We consider these empirical propositions and examine them explicitly.

Our investigation begins with an overview of the civil war literature. In the second section, we discuss the deficiencies with this work and outline a new approach to the examination of onset - Escalation. The third section provides the data and methodological technique employed for statistical analysis. Our results are provided in section four. From the examination, we find that there are distinct paths to civil war. For example, inflammation, incapacity and ineffectiveness each prove useful for understanding civil war onset in the context of urban, rural and general contexts. Cognizant that we may only be able to understand the origins of large-scale conflict in terms of this more complicated empirical picture, our findings compel us to revisit the civil war literature. We address these issues within the conclusion (section five).

1. Alternative Approaches to Civil War Onset

In the last ten years, at least sixty quantitative studies of civil war have appeared - dramatically outnumbering the output of the previous three decades. By almost any measure, the work here has been impressive. Over a relatively brief period of time, major scholars have received large grants, penned widely-cited articles, and produced important books that explore how civil wars begin (e.g. Fearon and Laitin 2003; Reynal-Querol 2002; Sambanis and Zinn 2005), how they differ across various dimensions (e. g., Azam 2002; Hegre et al. 2001) and how they end

(e.g., Licklider 1995; Sambanis 2000; Walter 2002). We concern ourselves, in this article, with the first category.

At present, most explanations for onset are structural in orientation (animated by rationalist foundations). Here, diverse system characteristics are believed to hold the key to understanding when large-scale conflagrations erupt. Despite this common theme, however, different researchers utilize distinct approaches to develop, examine and interpret the findings relevant to their models. Three, in particular, have dominated the discussion.

In what is generally considered the oldest approach - referred to as the "Grievance" model, civil wars take place in an effort to redress past injustice (e.g., Muller 1985; Huntington 1996; Gurr 2000; Sambanis and Zinn 2005). Here, political, economic and cultural structures create specific inequities: e.g., access to power (because of autocracy) and economic inequality (because of income concentration). To redress these inadequacies, individuals engage in civil war.

Empirical support for this argument is mixed to negative. Employing diverse structural characteristics (e.g., political democracy), some studies find support for this argument but only with regard to specific groups (e.g., Gurr 2000) or subsets of civil wars (e.g., Sambanis 2000). Others, however, find no support at all for the hypothesis (e.g., Collier and Hoeffler 2002; Fearon and Laitin 2003).[2]

In contrast to "Grievance," the argument referred to as "Greed" maintains that civil wars take place in an effort to take advantage of particular political-economic situations (e.g., Collier and Hoeffler 2002; Collier and Sambanis 2002; Elbadawi and Sambanis 2002). Specifically, two elements are highlighted: 1) incentives and 2) opportunities.[3] The basic logic is again fairly straightforward.

First, there is the motivation (or incentive) for rebellion. This emerges when challengers believe that they can profit from seizing or seceding from the state (i.e., after dividing expected loot or tax revenue among organizational members) and/or when they believe that they are able to sustain themselves through the period of confrontation with political authorities by effectively

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[2]Not all have been as exclusive in their focus. For example, Hegre et al. (2001) argue and support the claim empirically that grievances alone are not sufficient to explain civil war; rather, they maintain that it is also necessary to consider opportunities (e.g., the type of regime in place). This represents a distinct approach within this research tradition.

[3]Some do not particularly care for the Greed-Grievance distinction at all (e.g., Berdal and Malone 2000).

paying for dissident activity (i.e., hiring rebels away from supporting the state and/or remaining neutral). Second, there is the opportunity for rebellion (Collier and Hoeffler 2002 3). This involves two factors: 1) the expected cost of dissent (the "financing to procure arms and rebel labor" [Elbadawi and Sambanis 2002, 309] as well as the amount of potentially forgone economic opportunity for rebels lost by joining insurgent campaigns) and 2) the perceived strength of the government (e.g., the ability to counter domestic challengers).

Similar to research on Grievance, those examining Greed measure the diverse components of the model with diverse structural characteristics (e. g., per capita income, economic growth, educational attainment, primary commodity exports and population size), which simultaneously capture incentives and opportunities. Differing from the research focused on Grievance, however, "Greed" has received a great deal of empirical support. As found, results consistently disclose that the variables associated with this argument achieve statistical significance and they influence civil war onset in a manner consistent with the theory. Additionally, when compared to variables concerning Grievance, it is clear that Greed-related factors normally outperform them.

The third approach identified within the literature - referred to as the "Insurgency" model, advances a somewhat unique approach. Discussed by Fearon and Laitin (2003) this argument maintains that societal members hold a wide variety of grievances and desires derived from diverse political, economic and geographic factors. What accounts for civil war, however, in line with the Greed argument is the presentation of an opportunity. For example, according to Fearon and Laitin (2003), large-scale conflagration is most likely to emerge only when structural conditions are "right" - that is when "small, lightly armed bands practicing guerilla warfare from rural base areas" (Fearon and Laitin 2003, 79) can survive government efforts at counter-insurgency and protest policing.[4] Exactly when is this situation of state weakness most likely to occur (i.e., what is a "favorable" context)? Fearon and Laitin (2003, 79-82) argue that state repressive capability is undermined and challengers are favored when the economy is poor,

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[4]This is actually source of some confusion within Fearon and Laitin. At one point (2003, 79), repression in the form of restrictions on civil liberties is an indicator of grievance but at a latter point (2003, 80) repression in the form of counterinsurgency (e.g., search and destroy missions) is a measure of opportunity. Within the literature specifically focused on state repression, these are comparable.

population is high, there is large mountainous terrain, there is non-contiguous territory and political life is fragile.[5] Under these circumstances and only under these circumstances, can rural-based insurgency resist state repressive efforts, become a successful military strategy, and produce civil war.

Investigating the influence of diverse structural variables (almost all shared by the other approaches), Fearon and Laitin argue that they provide a great deal of support for their theoretical argument. This is in fact a major strong point of the research. Examining a wide variety of logit models, varying operationalizations (of civil war and "state failure" [Fearon and Laitin 2003, pages 84 and 87, respectively]), samples (all civil wars, "ethnic wars" and civil wars including empires [Fearon and Laitin 2003, 84]), methodological concerns (period and fixed effect [Fearon and Laitin 2003, 87]) and special conditions (e.g., the influence of foreign support and regional effects [Fearon and Laitin 2003, pages 86 and 87, respectively), the results consistently support Fearon and Laitin's argument. Under the favorable contexts described above, civil war onset is much greater. Additionally, similar to the research associated with Greed, when compared against variables associated with Grievance, the Insurgency argument tends to outperform this rival. Interestingly, there is no such comparison made to Greed. We discuss this further below.

2.   Limitations and a New Direction

While generally viewed in a favorable manner, there are two problems with existing literature which prompt us to be somewhat skeptical of the results and to look for insights elsewhere.

First, there is the Problem of Invariance. At present, the causal determinants within existing models don't change at all (e.g., the percent of the country that is mountainous), they change very slowly (e.g., per capita GDP) or they change rarely (e.g., the degree of political democracy). How can one predict change in conflict behavior from variables that are essentially invariant? This presents a major limitation in this area of research.

Second, we have the Problem of Observational Equivalence. Essentially, this concerns the

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[5]Another set of variables used to explain civil war onset derive from theories that Fearon and Laitin hope to falsify. Since some claim that civil wars are contingent on factors such as "ethnicity, discrimination, and grievances" (Fearon and Laitin 2003, 78), their models also include ethnic fractionalization, religious fractionalization, and democracy. Although there are no results provided, the authors also suggest that civil liberties restrictions were examined as well.

fact that the same variables are employed to operationalize diverse theoretical explanations and thus different arguments claim support from the same statistically significant variables. For example, both Greed and Insurgency focus on the impact of per capita GDP on civil war onset but the substantive meaning of the influence varies. In the case of the former, the statistical significance of the variable is taken as evidence that lower levels of forgone income prompt individuals to engage in civil war. In the case of the latter, the statistical significance of the variable is taken as evidence that incapable states are unable to control lower-level dissident challenges. These are very different explanations and there has been no attempt made to adjudicate between them.

Moving us in a direction out of this quandary, existing research points us to a distinct and, we believe better causal explanation which is, at once, variable across space and time as well as separate from the largely invariant structural characteristics: the Escalation of lower-level conflict to civil war. While prominently highlighting state weakness and per capita GDP, such an approach is clearly found but underdeveloped as well as underutilized within Fearon and Laitin. The basic logic of their argument viewed from the perspective of Escalation is straightforward.

In the "Insurgency Model," it is argued that civil wars emerge when a particular type of dissident behavior is allowed to thrive. When does this situation exist? We are told that insurgency grows and conflict is likely to escalate when a government's capacity to identify, capture and kill challengers is diminished (Fearon and Laitin 2003, 79-80); in short, insurgency escalates to civil war when repression is ineffective. Why does prior conflict matter? Precisely for the reasons that Fearon and Laitin (2003) argue when they repeatedly use the words "practicing" (2003: 79), "experienced" (2003: 82), "nascent" (2003: 80) or "active" (2003: 80) before mentioning rebel organizations and why the words "insurgency" and "warfare" are used, connoting a sense that they are ongoing (before and leading up to). Active rebels recruit before civil war, practicing rebels instigate others to protest before civil war, experienced rebels attack authorities while coordinating other efforts to attack the existing political-economy. Indeed, what is most important about the diverse insurgent strategies mentioned above is that in every case, pre-civil war behavior (below the threshold of civil war) is deemed important for understanding latter conflict behavior.

Unfortunately, Fearon and Laitin move away from the dynamics of repression and dissent when they attempt to operationalize their argument (focusing as we said earlier on GDP per capita and mountains), but it is worthwhile to stay with the Escalation argument and find a way to better examine it.

One important attempt in this direction is provided by Sambanis and Zinn (2005). In this work, it is argued that prior repressive action enrages citizens, prompting a shift in dissident tactics and/or compelling challengers to redouble their efforts at socio-political change. To examine their argument, the authors examine the direct effect of dissent (e.g., social movement organizations engaging in lower-level violent and non-violent activity) and repression (proxied by the imposition of direct rule, restrictions imposed on group autonomy and system type) on the onset of separatist war. They also examine the influence of repression on tactical shifts from non-violent to violent lower-level activity as well as the interactive effects of repression (proxied by anocracy) with non-violent dissident behavior. The results of this work show that prior repression and dissent increase the likelihood of civil war. Additionally, it is found that repressive behavior leads to a tactical shift in dissident strategies, which in turn prompts large-scale conflagration.

While a useful effort in more directly identifying the process that explains the emergence of civil war, there are several difficulties with this work which require additional consideration.

First, there are multiple paths to escalation not are not considered. For example, Sambanis and Zinn (2005) explore the influence of the inflammatory influence of state repression on dissent - provoking subsequent dissident activity, but they ignore other influences of repression on dissent that could also result in conflict escalation. Previous reseearch has disclosed a wide variety of influences (e.g., Lichbach 1987; Gupta et al. 1993; Moore 1998). It is possible that repressive action is unable to diminish dissent, which, in turn, facilitates conflict escalation. It is also possible that despite attempts at political control authorities are simply unable to diminish the activities of challengers, which, in turn, facilitates conflict escalation. Finally, it is possible that different paths exist concurrently, explaining different civil wars.

Second, the measure for repression used by Sambanis and Zinn (2005) is indirect and not precise. As designed, it is assumed that direct rule and specific forms of restrictions on particular

political activities will have an influence on dissident activity. This actually pinpoints a major imbalance in Sambanis and Zinn (2005). Within their research, they consider actual dissident behavior but they do not consider whether actual repression influences this behavior. They simply explore interactions between dissent and the proxies for repression. Given the variability in how repressive behavior influences conflict (e.g., Lichbach 1987; Moore 1998), however, it makes sense that we be as precise and as comprehensive as possible with the measurement of relevant behavior. In short, one should consider a wide array of state coercive tactics that are widely acknowledged as such (e.g., Hibbs 1973; Poe and Tate 1994; Davenport 1995; 1999; Harff 2003), examining the influence that it wields on the behavior of challengers.

Third, it is possible that it is not just the type of dissent that matters but also the locale of this activity. For instance, there has been a large amount of effort extended toward understanding the relative balance of pre-civil war levels of rural-based activity - consistent with Fearon and Laitin (the so-called "Eastern model" of rebellion) as compared to urban-based activity (the so-called "Western model" of rebellion). While many have suggested that only one or the other would be found before the eruption of large scale conflict (e. g., Huntington 1968), there were others who have suggested that there would always be some combination present (e.g., Shugart 1989). Dix (1983, 284), for example, argues that

> it would be misleading to contend that the revolutionaries in the those countries won
> by moving from the countryside to the capital, as in the Eastern model. They did
> that, but to stop there obscures a vital part of the picture, namely, that probably as
> much, and perhaps more, of the revolutionary action took place in towns and cities
> as in the country-side.

An appropriate investigation of escalation would consider these factors.

Fourth, and finally, it is possible that political-economic context wields an influence that is conditional in nature as opposed to the current practice of assuming that influences are more direct in nature. For example, Fearon and Laitin (2003, 80) argue that

> "(m)ost important for the prospects of a nascent insurgency are the government's
> police and military capabilities and the reach of the government institutions into

rural areas. Insurgents are better able to survive and prosper if the government and military they oppose are relatively weak - badly financed, organizationally inept, corrupt, politically divided and poorly informed about goings-on at the local level."

This suggests that poorly-financed and bureaucratically incompetent states weaken a state's ongoing efforts at counter-insurgency.

The distinctions here are important. For example, if context has a direct and independent effect on civil war onset, then we just need to consider these variables on their own. If, however, context conditions/interacts with lower-level conflict, then there is an interactive dynamic that must be identified explicitly as it influences civil war. Because of the exclusive focus of existing researchers on diverse structural variables, however, these issues have not been explored.

2.1   The Logic of Escalation

In this research, our main point is to examine the influence of diverse escalation arguments - considering how lower-level conflicts grow into larger conflagrations. Here, we acknowledge that civil wars are not born - arising from nothing (i.e., from 0 to 1). Rather, they are born - arising from prior interactions between authorities and dissidents who are engaged in a contentious battle for the state, taking place in the streets, farms as well as the mountains.

Specifically, drawing upon insights within as well as outside of the civil war literature, we consider three distinct paths. In one argument, referred to as "inflammation" states provoke citizens when prior applications of state repression are met with increased dissent. States would be "incapable" when despite growing dissent coercive behavior is low.[6] Finally, repression is "ineffective" when the state initially applies high amounts of coercive behavior in the face of dissent but withdraws its application as dissent continues.[7]

The adoption of this approach is important because although repression and dissent play a vital role in all three arguments, the precise nature of that role varies considerably. For example, if repressive *inflammation* leads to civil war, then it is the objectives and behavior of

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[6]By this logic, states are "capable" when prior repressive activity is able to diminish subsequent dissident activity.

[7]Correspondingly, states are " effective" at applying repression when significant applications in the face of dissent decreasing subsequent challenges.

challengers along with the unintended consequences of state repression that are relevant to civil war onset. This compels one to think about the motivations of those that challenge the state (e. g., "cultural frames" in the social movement literature) and the unacceptability of coercive behavior for those who are subjected to that behavior (e.g., the normative framework adopted by the human rights movement). This also compels one to reflect about the type of mobilizing structures underlying resistance efforts that are not only undeterred by state repression but are provoked by them. Indeed, in cases of repressive inflammation coercion is not perceived as a cost to collective action (something that is generally assumed by rationalists). Rather, the argument is changed to reflect the fact that as individuals get more enraged by repression, they are more likely to engage violent conflict with the state. Tied to a victimization identify in these contexts, increased coercion functions more like a benefit than a cost (e.g., Zwerman and Steinhoff 2005)

In contrast, if *incapacity* leads to large-scale conflagration, then this suggests that civil war is associated with the inability of the state to apply repressive behavior. While this commonly brings to mind the rationalist conception of costs discussed in civil war research, this is very similar to the concept of "political opportunity" found in the social movement literature (e.g., McAdam 1996, 23). In this latter work, groups challenge governments when the broader political system appears to be vulnerable to dissident behavior. The social movement literature is also relevant here for several have argued that repression is one (and perhaps the best) measure for opportunity structure because of its direct impact on social movements and citizens (e.g., Della Porta 1995).[8]

Finally, if repressive *ineffectiveness* leads to civil war, then this tells yet another story about the genesis of large-scale political conflict. In this context, it is the state's inability to eliminate dissent and not their inability to apply repression that is of interest. This perspective leads us to a different understanding. Invoking the standard rationalist account about reduced cost and opportunities[9] but also investigations of conflict management (e.g., Kitson 1971), the

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[8]One could also associate this concept with research on political development and "state failure" for this work frequently discusses the idea that when political authorities are no longer able to function properly (in this case as the sole actors able to wield coercive power), domestic challenges are likely to emerge.

[9]This time in the face of cost but not insurmountable ones.

ineffectiveness argument highlights a repeated interaction between two actors, which provides a systematic updating of the group's chances of success. Here, when prior repression is unable to deter challenger's efforts, then it would be perceived that conflict escalation has a higher likelihood of being successful and effort would be extended toward this end. However, when prior repression decreased dissident behavior, then escalation would not be perceived as a viable strategy and no effort would be extended in this direction.

While we have discussed the three escalation arguments individually, it is important to understand that these are not necessarily competing with one another. Indeed, this would be another criticism that we have of existing civil war literature. To date, it has been assumed that one theoretical explanation underlies all civil wars. We do not accept this position. It is possible that different civil wars (either in the same country or in different countries) could be explained by different escalatory processes. We should at least allow for these possibilities within our statistical investigations. We move to this in the next section.

Following our earlier discussion, we not only identify distinct escalatory arguments but we also highlight distinct geographical domains focusing upon situations where conflicts are essentially urban-based, rural-based or both. Why are such distinctions important? There are several reasons.

First, it is useful to investigate the importance of different locales to gauge the robustness of the escalation arguments. If these explanations are only useful in explaining dynamics within one context, then this would need to be factored into our discussions/analyses of civil war onset. Second, explicit consideration should be given to the strategies adopted by political challengers and the states they interact with is crucial because this has relevance for existing theory. For example, Fearon and Laitin's insurgency model focuses exclusively on the activities of rebels and authorities in a rural context (i.e., as they fight in the mountains). This approach ignores, however, the role played by students, workers, indigenous people, ethnic minorities and ordinary citizens throughout the country in question who engage in resistance efforts - in other words, this approach ignores a more encompassing view of all individuals who, prior to civil war, challenge authorities. These differences are important because it may not only be the behavior of rebels in the countryside that is of interest. In the period immediately preceding civil war, it may be

the overall level of dissent in the central locales of the polity (the urban centers) that are of the utmost importance. Alternatively, it may be the overall level of ungovernability throughout the society writ large (i.e., within both the rural and urban environments) that produces a context favorable to large-scale conflagration. A useful investigation of onset would allow for both.

Before addressing these issues further, it is worthwhile to consider a concrete historical example. Such an exercise is useful for it highlights the relative importance of diverse structural characteristics relative to the dynamics of lower-level conflict behavior. Toward this end, we discuss Nicaragua.

2.1.1 Understanding the Nicaraguan Civil War of 1978 In trying to explain the Nicaraguan case, one insurgent group - the Sandinista National Liberation Front or the FSLN and its relationship with the state has consistently received the bulk of the attention.

Composed of armed revolutionaries, the FSLN came into being on July 23, 1961 to oppose the dictatorship of the Somoza regime. The tactics of this rebel organization were compatible with the Fearon and Laitin description of insurgency. As found, the FSLN engaged in numerous military-style attacks against the regime, including border raids in Ocotal (near the Honduran border) and attacks in San Carlos (near the Costa Rican border) and in Managua over the period from 1974 to 1977. Most notably, the FSLN stormed the National Palace during a session of congress in 1978. Approximately 1500 members of the existing government were taken prisoner and held hostage for the exchange of 59 political prisoners (e.g., Central Intelligence Agency 1977; Ortega 1979).

One can argue that the structural factors consistently highlighted by previous civil war literature played a role in increasing the capacity of the FSLN relative to the Nicaraguan authorities. For example, terrain appeared to matter a great deal as an extensive Maribios mountain range (the central cordillera reaching about 8000 feet) and series of 25 volcanoes divided the country in two. On one side, towards the Pacific Ocean, there was Managua (the capital city), the largest concentration of population, and the flatlands (or open plains). On the other side, towards the Atlantic Ocean/Caribbean Sea, there was Bluefields (a small coastal city), sparsely populated, and with extensive tropical forests. In line with existing literature,

the geographic seclusion offered by the mountain range, volcanoes and forests as well as the easy access to neighboring Honduras proved to be extremely important for undermining government police and military capabilities and the reach of government institutions into rural areas (Fearon and Laitin 2003, 80). However, the mountains existed before there was a Nicaragua and hardly explain why the civil war began in 1978. To explain the onset of the civil war, we turn to the dynamics of Nicaraguan lower-level conflict behavior.

Before the large-scale conflagration of interest, the FSLN was engaged in a protracted but small guerilla campaign against the existing regime (e.g., Ortega 1979), who responded to the attacks with very precise tactical strikes on the people of Matagalpa, Masaya, Leon, Esteli, and Chinandega (Gentile 1989). In this earlier period, most of the population was not involved, which provided a major part of the explanation as to why there was no civil war before 1978: the battle was contained and society was still ?governable" (del Cid 1979). Mass resistance did eventually become more widespread, destabilizing the regime and facilitating the FSLN's challenge. In January 1978, while Somoza was out of the country, the death of Pedro Joaquin Chamorro Cardenal, a publisher of Nicaragua's most popular newspaper La Prensa and a tireless reformist who consistently struggled within the existing political system, provoked mass protests among students, workers, farmers, and professionals throughout the country. Even before 1978, however, students and the other protest groups had been slowly increasing their resistance to the regime (Wheelock 1986). Moreover, Indians in Masaya and Monimbo - near Managua - began to increase their anti-regime activity, further challenging authorities and providing a breeding ground for resistance (Central Intelligence Agency 1977).

The expansion of resistance beyond the guerrilla insurgent organization in the rural context is what those in the field of social movements refer to as a growth in the ?conflict cycle" (e.g., Porta 1995; Tarrow 1993; Tilly 1978). According to this work, an increase in internal war would affect many things (Hodges 1986). The rise in contentious politics in both rural as well as urban environments signaled to the regime that it lacked popular support; widespread dissident activity prompted the regime to increase its repressive behavior even further - now targeting more of the population and giving people less of an opportunity to insulate themselves from the state-societal conflict (Mason and Krane 1989); the rise in contentious politics signaled

to the FSLN that widespread dissatisfaction with the regime existed and that its insurgency would likely receive indirect (if not direct) support from the population; increased resistance prompted moderation in the FSLN's platform in an effort to acquire new members and build a broader coalition; the rise in contentious politics led numerous actors in the country who were previously unsupportive of resistance, regime change, and/or the FSLN to move in these directions; and, finally, increased resistance convinced numerous actors outside of the country that the existing regime lacked popular support and that the challenge to the authorities was both legitimate and likely to succeed.

In short, focusing on the military and predatory aspects of the key insurgent organization, without considering the wide-spread resistance manifest in general conflict behavior, misspecifies the etiology of the Nicaraguan civil war. While the FSLN was engaged in a battle with authorities in the rural setting, it was not until peasants, Indians, workers, and everyday citizens voted with their feet, fists, and firebombs throughout the country that civil war began. The ?onset" of the civil war must therefore be traced to a country made ungovernable by the interaction of authorities and challengers.

Although we do not have the space to provide a thick description of the many Nicaraguan internal wars, Figure 1 offers some key stylized facts: five distinct time-series of conflict behavior that run ten years before the onset of the 1978-1979 civil war. With the exception of violations of political rights, which seem, along with the standard civil war variables, to remain relatively stable throughout the period, all of our measures of ongoing internal wars spike the year of the civil war. Dissident conflict - riots and anti-government demonstrations - and state conflict - political sanctions and violations of civil liberties were precursors of civil war.

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This figure supports four important points. First, some of the variables highlighted by existing civil war scholars do not vary over time. Second, the only ones that do vary over time (population and economic development) likely provide cues to societal and political actors that that the moment was appropriate to act. Third, if the structural constants and variables discussed by civil war scholars were important, it was only because they refracted ongoing state-dissident interactions. Finally, consistent with the escalation perspective, the mobilizations and

countermobilizations eventually joined together and produced the Nicaraguan civil war. The civil war was indeed an outgrowth of long-standing political struggles.

Now, clearly we do not argue that the escalatory process discovered in the Nicaragua case apply to all civil wars; indeed, we expect that distinct combinations will be important for understanding different large-scale conflagrations. Rather, this was used to sensitize us to the importance of lower-level conflict dynamics in diverse parts of a country and the role that this plays in the escalation to civil war.

3.   Data and Methods

To operationalize and test the different escalatory arguments identified above, we employ data from a number of different sources, including Banks' (2001) Cross-National Time- Series Data Archive, Gibney's (2005) Political Terror Scale, and the replication dataset provided by Fearon and Laitin (2003) - which we view as being the state of the art within the civil war literature.[10] Immediately, after comparing across the databases one is able to discern that there are some differences between our sample and the Fearon and Laitin study. For example, due to the coverage of the Political Terror Scale, our analysis only covers the years 1976-1999[11] and includes 149 countries. This differs from the Fearon and Laitin research because within their study there are 12 additional countries. Due to the shortened time frame, we also lose 58 civil war onsets.[12]

While we would have liked to examine the different escalation arguments within exactly the same sample as one of the pre-eminent analyses within the civil war literature, there is no reason to believe that our study would be inappropriate. First, despite the differences in sampling, the unconditional distribution of civil war onsets remains relatively similar (e.g., onsets are 1.68% of the observations in the Fearon and Laitin study and 1.7% in ours). Second, no scholar in the civil war literature discusses nor demonstrates empirically that either temporal or spatial

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[10]We obtained these data from the authors at the following url: http://www.stanford.edu/ group/ethnic/publicdata/publicdata.html. We refer the interested reader to their article (Fearon and Laitin 2003) for a more detailed discussion.

[11]We use Poe and Tate's (1994) back-coding of the Political Terror Scale to fill in where Gibney's is missing.

[12]There are 111 in the Fearon and Laitin study and 53 in ours.

influences exist; consequently, it should not matter where one truncates time or space. [13]

3.1 Civil War Onset

As with most in the field, civil war onset is our dependent variable, which we take from Fearon and Laitin's replication data. In the interest of space and parsimony of presentation, we focus specifically on overall civil war onset, but our basic argument and statistical model could easily be applied to the different operationalizations of civil war offered within the literature (e.g., ethnic civil war or the onset measures provided by the Correlates of War, Collier and Hoeffler as well as Sambanis).

3.2 Repression, Guerrilla War and Dissent

To operationalize the concepts of inflammation, incapability, and ineffectiveness, we employ three different variables - repression (the Amnesty International version of Gibney' s Political Terror Scale), guerrilla war (a dummy variable representing the presence of guerrilla war in the previous period taken from Banks Cross National Time Series Data Archive) and a variable for increased dissent that is coded 1 if either riots or anti-government demonstrations (both taken from Banks CNTS Data Archive) occurred. We combine these variables in various ways to represent distinct state-dissident interactions.

In order to offer an operationalization that makes sense, we needed to create three new variables: years of high repression in the previous 4 years (a 4 or 5 on either the Amnesty International or State Department Political Terror Scales), years of dissent in the past 4 years (1 or more riots or anti-government demonstrations) and years of guerrilla war in the previous four years. These refer to the number of occurrences of the behavior of interest between $t-5$ and $t-1$. For instance, China had high repression from 1989 to 1992. So, in 1990, we coded China as having one year of previous repression in the past four. In 1992, we coded China as having 3 years of previous repression (1989, 1990 and 1991). We use these as follows:

Inflammation refers to a situation where states escalate their actions prompting the escalation of dissident action. This suggests that states should have been employing high

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[13]This is true assuming that the sample size provides sufficient degrees of freedom for the inferences made.

repression longer than dissidents have been dissenting and both should have exhibited increased action for some time. So, we coded inflammation as 1 if:

- Years of high repression is greater than years of dissent (urban or rural) & years of dissent is greater than 1.

Incapacity refers to a situation where dissident action is increased, but state activity cannot increase to meet it. Here, dissent increases and states continue to employ low levels of repression. Incapacity is coded 1 if:

- Years of high repression equals zero & years of dissent is greater than 1.

Ineffectiveness refers to a situation where states try increased repression, but find that it is ineffective in the face of continued dissent. Thus, they will decrease their efforts in defeat. Ineffectiveness, then, is coded as 1 if:

- Years of high repression is less than the years of dissent (urban or rural) & years of dissent is greater than 1.

Figure **??** shows how we divide the dissent-repression space to create our indicators.[14] These three different schemes, giving rise to nine variables (Inflammation, Incapacity, and Ineffectiveness with both urban dissent [riots and demonstrations] and rural dissent [gurrilla war] and both rural and urban strategies) these will be the primary focus of our study of civil war onsets. Through a comprehensive assessment of the various ways that political authorities could be challenged, we thus provide a thorough investigation of the different circumstances that facilitate large-scale conflagration.

3.3   Other Exogenous Variables

To assess the importance of different contextual variables we also use the structural characteristics highlighted by previous civil war scholars. There are, however, a couple of notable

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[14]Ideally, we would consider sequencing (like Moore [1998]), but we feel that the gross level of temporal aggregation makes the sequencing a relatively unreliable indicator of the actual sequence of events between dissidents and states. If we had daily or weekly event data, this would make much more sense, but to suggest dissidents at the end of 1990 are responding to something states did in the beginning of 1989 (a situation that could occur with the current level of aggregation) seems somewhat unrealistic.

exceptions. We drop the "new state" variable in Fearon and Laitin (2003) from our analysis as it has very little variation in this sample. The complete list of variables employed in this analysis are provided in Table 2.[15]

[Insert Table **??** about Here]

3.4   The Sampling Scheme

To begin our investigation, we acknowledge that we contend with the same problem that all people who attempt to study civil war in a large-N statistical model confront. Namely, there are very few civil wars as opposed to non-civil wars (53 vs. 3024 in our sample). This makes it very difficult to predict any positive events. Further, it has not been our experience that those events given the highest probability of being a 1 (though still less than .5) are likely to be civil war events. This leaves us with two options - 1) use all cases and simply focus on statistical significance, which is practiced by the majority of scholars in this area, or 2) use a (rigorously chosen) sub-sample of the data and focus on prediction. Differing from existing research, we choose the latter, as suggested by King and Zheng (2004). This is made even more appealing given the demonstration by Ward and Bakke (2005) that on not-so-rare occasions adding statistically significant variables actually can decreases the predictive capacity of the model.

Specifically, we implement a density case-control design where for every civil war onset in the dataset, we randomly draw two non-onsets from that same year.[16] This gives us a sample of 212 cases. We then use the Rare Events Logit algorithm provided by King and Zheng (2001) in the Zelig package in **R**.[17] As King and Zheng suggest, with the rare events correction, we can report quantities of interest from the logit regression, such as first differences and simulated predicted probabilities.

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[15]Fearon and Laitin (2003) and Sambanis and Zinn (2005) have argued that anocracies may be more civil-war prone than regimes occupying the extremes of regime type. To allow for this possibility, we include Polity22 in our models. The general finding of these authors is upheld.

[16]This is similar to the approach adopted by Harff [2003] in the context of genocide.

[17]We use the following arguments: case.control="weighting" and tau=0.017.

3.5   The Models

Underlying the Rare Events Logistic Regression model, is a simple logistic regression model, that is weighted by a function of the absolute risk level in the population. Thus, we get predicted probabilities and first differences in essentially the same way we would in a run-of-the-mill logit model. The basic model employed here is as follows:

$$Pr(Y = 1|\mathbf{X}, \mathbf{b}) = f(b_0 + b_1\text{Log(GDP/Capita)}_{t-1} + b_2\text{Log(Population)}_{t-1} + b_4\text{Oil}+$$
$$+ b_3\%\text{Mountainous} + b_5\text{Polity2}_{t-1} + b_6\text{Instability} + b_7\text{EthnicFrac} \qquad (1)$$
$$+ b_8\text{ReligiousFrac} + b_9\text{Dissent : Repression} + \varepsilon)$$

where $f()$ is the logit link function, the coefficients $\mathbf{b}$ are the rare-events logit bias-corrected coefficients and "Dissent:Repression" refers to the various types of state-dissident interactions mentioned above - inflammation, incapacity, ineffectiveness.

In an effort to explore arguments advanced but not tested in Fearon and Laitin, we estimate models that allow structural characteristics to change the effect of state-dissident interactions. We do this in two different models. First, in the spirit of Fearon and Laitin's work, we acknowledge that various structural characteristics may create an environment that is "favorable" for insurgency but detrimental to counter-insurgency. Here, we look at structural variables on the favorable side of their mean. For instance, low GDP/capita (signaling low state capacity) is favorable for insurgency but detrimental to counter-insurgency. Instability is also favorable for insurgency as is a high (greater than average) percentage of mountainous terrain. We generate a new variable that captures the number of structural characteristics that are favorable for insurgency. Presumably as more of these amass in the presence of dissent, civil war should become relatively more likely. Second, as a test for robustness, we also multiply conflict by the Log of GDP/capita, realizing that this particular structural variable seems to be particularly relevant in other empirical studies of civil war. As economic characteristics in particular are used to proxy opportunity (i.e., state- dissident interactions) as well as motivation (e.g., the amount of lootable resources that the state holds), by explicitly considering lower-level conflict behavior we will be better able to gauge the influence of GDP/capita as a motivational factor

in large-scale conflagrations.

Within this research, we take a somewhat different approach to the interpretation of our analysis. On the suggestion of Ward and Bakke (2005), we focus on prediction rather than on statistical significance. As such, we evaluate the utility of each variable by what it adds to the model predictions, all else equal. In the interest of space, the coefficient tables will be omitted from the paper, but are available from the authors upon request.

4.   The Empirical Findings

First, we must identify some baseline characteristics of our investigation. Figure **??** shows how the various structural characteristics effect the number of correctly predicted onsets. The "base" model refers to one where all of the structural characteristics are present, but no conflict variables are present. Panel (a) of Figure **??** shows that taking any of the structural variables out reduces the number of correctly predicted positives (onsets). However, taking them out allows us to correctly predict more non-onsets (as shown in panel (b) of Figure **??**). Although we must be cognizant of overall model performance, we are concerned principally with predicting positives. If we only wanted to predict non-onsets, we would just use a constant and would get all of the non-onsets right.

Plots like those in Figure **??** will appear below, so it is probably worth explaining how they were generated. Each solid box and set of accompanying whiskers represents a different model, in this case the base model with the identified variable removed. To get the distribution of correctly predicted civil war onsets, we use the `sim()` function accompanying `Zelig` in **R**. This function essentially takes 1000 draws from the k-dimensional multivariate normal distribution of coefficients. For every set of coefficients (for every draw from this distribution), it multiplies that vector by the matrix of observed variables, creating a predicted value ($\mathbf{Xb}$ for each observation). It then calculates the probability that each observation equals 1:
$Pr(Y = 1|\mathbf{X}, \mathbf{b}) = [1 + \exp(-\mathbf{Xb})]^{-1}$. This produces a 1000×212 matrix of predicted probabilities. Each column represents the 1000 sampled predicted probabilities for a single observation. To get the distribution of correctly predicted observations, we count the number of times in

each row that the predicted probability agrees with civil war onset.[18] The solid box marks the median of this distribution and the whiskers mark the 25th and 75th percentiles of the distribution.

In substantive terms, this is an interesting contrast to Fearon and Laitin's (2003) model. They suggest that certain of these variables - namely GDP/capita, population, mountainous terrain, oil exporter, and instability. Further, some variables were insignificant - democracy, ethnic and religious fractionalization and non-contiguous territory. One would expect that taking significant variables out of the model would reduce its predictive capacity and removing statistically insignificant variables from the model would have no effect. Figure ?? shows that this is not true. In our study, taking out any of the variables has almost exactly the same effect. Further, taking any one of those variables out allows the model to predict more non-onsets correctly. This is further evidence for the assertion that statistical significance has no necessary relationship to predictive power.

4.1   Investigating Direct Influences

In our first examination, we consider the effects of adding conflict variables to the base model. Figure ?? shows the effects of adding conflict variables. On average the effect is minimal when added individually. Only inflammation in the urban context shows any gain over the base model at all and that only results in getting one more onset right (as shown in panel (a) of Figure ??). Panel (b) of Figure ?? shows that ineffectiveness in the urban context helps us predict more onsets correctly, but it appears that this is mostly attributed to a better prediction of non-onsets.

However, underlying these statistical results (that are seemingly very similar) are somewhat different outcomes. While the median of properly predicted values is around 9 for the base model (i.e., the median of the distribution of correctly predicted onsets given the sampling distribution of the coefficients is centered around 9), it only gets two of them consistently right (more times than not) given the simulated predicted probabilities - Russia 1994 and Angola 1992. The obvious question is, do we gain anything interesting by adding in the conflict variables. The

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[18]The number of times the $Pr(Y = 1|\mathbf{X}, \mathbf{b}) > 0.5$ and onset = 1 plus the number of times that $Pr(Y = 1|\mathbf{X}, \mathbf{b}) < 0.5$ and onset = 0.

answer is a resounding YES! We find that the various contentious interactions inform us about different civil war onsets; in other words, there are distinct paths to civil war.

4.1.1 Urban Contention   In the urban environment, knowing that the contentious interactions are of an inflammatory nature (by our definition having more repression than dissent, but at least two years in the past four of each) helps us predict civil war onsets in Russia (1999), the Democratic Republic of Congo (1996) and Ethiopia (1992). Knowing that the government appears to be ineffective with respect to urban dissent (by our definition more protest than repression but positive values of both) helps us predict the civil war onsets in Iran (1979), China (1991) and India (1982). Understanding urban dissent and repression allows us to predict 6 civil war onsets that could not be predicted by Fearon and Laitin's model.

We would be remiss if we didn't look at the places where we predicted onsets when there were none? The results here are equally interesting. If we suspect that inflammation is causing civil war, then the model would suggest we need to look at Burundi (1996) and Bangladesh (1994), in which case we would immediately find out that these countries are both already engaged in civil wars at these times. The incapacity model would suggest looking at Colombia (1978), which is also involved in a civil war at this time. Finally, the model employing urban ineffectiveness would suggest consideration of Argentina (1978) and the Philippines (1991). The Philippines at this time is involved in a civil war. In 1978, Argentine had just ended a 4-year long civil war. The conclusion here is that our attention would not be wasted on any of these countries as they were almost all involved in civil war already.

4.1.2 Rural Contention   Results here were actually quite different and interesting. In this case, inflammation (increased behavior both in terms of guerrilla war and repression) do not help predict any civil war onsets. Incapacity, continual low repression if the face of heightened dissent, helps predict civil war onsets in Zimbabwe (1983) and South Africa (1983). Ineffectiveness (more guerrilla war than repression, but positive values of both) helps predict civil wars in Cambodia (1978) and Nicaragua (1978).

As was discussed above, these models also suggest looking at some cases where no onsets were

found. Here, though inflammation in the rural context did not yield any positive predictions, it did yield a false positive in one country - India (1997-1998), which was involved in civil war for both years. Rural incapacity generated false-positives for four countries: Colombia (1978), the UK (1993), Spain (1978, 1983) and Malaysia (1978). Of these, both Colombia and the UK are identified as being involved in civil wars at the time. Neither Spain nor Malaysia experience a civil war onset over the entire period under investigation. Similarly, the model employing rural ineffectiveness generates false-positives in four countries: Argentina (1978), Ethiopia (1978-1979), Burma (1987) and the Philippines (1991). Of these, only Argentina is not involved in a civil war at the predicted year and as noted above, Argentina is just exiting a civil war in 1978.

4.1.3 Both Urban and Rural Contention   Here, we generate fewer new predictions. The model using both urban and rural inflammation (that is high dissent and guerrilla war as well as high repression) helps predict civil war onsets in Somalia (1991) and El Salvador (1979). Neither incapacity or ineffectiveness in the urban *and* rural context help generate any new positive predictions.

All three models - inflammation, incapacity and ineffectiveness - help generate false-positives, though. Ineffectiveness suggests that Turkey (1996), India (1998), Burma (1999) and El Salvador (1992) - all should have civil war onsets in the identified years. However, all are involved in civil wars in the identified years, they just started before the times mentioned. Incapacity generates the same false-positives as in the strictly rural context. Similarly, ineffectiveness generates a subset of the same false-positives - namely Argentina (1978) and the Philippines (1991), both of which have been discussed above.

Including different forms of contentious interactions in different contexts (urban, rural, and both) has proven very useful. These variables were able to predict the onset of 14 civil wars that were not accounted for by Fearon and Laitin's model. Furthermore, of the 12 countries (in various years) that were identified as likely to see a civil war onset when they did not, 10 were currently involved in civil wars and one was just exiting a civil war the year before. Only Spain and Malaysia were identified as likely to experience a civil war, yet had no ongoing or recent civil war experience.

What is most interesting, enlightening and novel, however, is the fact that different types of contention in different settings predict different civil wars. The hypotheses do not compete in a conventional sense - that is to say that one of these schemes would be a good predictor of civil wars and the others would fail at the same task. In fact, they are all quite complementary in their ability to predict different civil war onsets.

4.2 Investigating Interactive Influences

As discussed above, we propose two different multiplicative models - one where we use a variable that counts the number of structural characteristics that are "favorable" for insurgency[19] and one using log(GDP/capita) as the structural factor most likely to condition the effect of conflict.[20] For the model including favorable conditions, we exclude all of the other structural variables; the idea is that all of the important information is captured in the variable denoting favorable conditions.

4.2.1 Favorable Conditions
Here, we consider the effect of conditions favorable for insurgency. We assume that as the number of favorable conditions grows and conflict exists, that the probability of civil war onset increases. The predicted civil war onsets are largely the same as from the non-multiplicative model. The only real difference is in the case of Inflammation, where in all contexts, Somalia (1991) and El Salvador (1979) are correctly predicted. In the urban context, we add Haiti (1991) and in the rural context, we add Uganda (1993) and Nicaragua (1981). We falsely predict 8 countries, but all of them, with the exception of Argentina (1981), are involved in an ongoing civil war in the years identified. Incapacity and ineffectiveness generate no new predictions and, in fact, generate essentially the same missed cases, so from the point of prediction, there is no interaction effect. In sum, Fearon and Laitin's general argument about the conditioning influence of context is not supported but, for the wrong reasons, their assumption of an independent influence is supported.

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[19]Specifically, conditions are thought to be "favorable" in the following situations: log(GDP/capita) less than its mean, log(population) greater than its mean, polity2 between -5 and 5 (inclusive), % of mountainous terrain above its mean, ethnic and religious fractionalization both above their means, oil exporter= 1, instability = 1 and non-contiguous terrotiry = 1.

[20]Again, tables of coefficients are available, but the focus here is on prediction.

These results perfectly illustrate the point of Ward and Bakke - that statistically significant variables may add nothing to the predictive power of the model. In fact, in all three models of urban conflict, the coefficients of the conflict variables are significant. Table **??** shows the conditional coefficients for urban conflict and repression interacting with favorable conditions. In two of these models, they are headed in the opposite direction predicted by the theory - that is as the number of conditions favorable for uncertainty increases, the probability of a civil war onset actually decreases. The failure to increase predictive capability (at least prediction of the ones) is in part a function of this phenomenon pointed out by Ward and Bakke and in part probably a function of the fact that the hypothesized direction was wrong. This could be a function of the sparsity in some of the cells of the joint distribution of favorable conditions and conflict. Further investigation of this point is necessary.

4.2.2 GDP and Conflict   As stated above, GDP/capita is almost always a significant variable in comparative research. The civil war onset literature is no exception to this general regularity. There is, however, no shortage of theoretical explanations for this relationship. GDP signifies anything from lootable resources to state capacity. We see confirmation for two different theories in the empirical work here. First, in the presence of ineffectiveness and inflammation, GDP/capita actually increases the probability of civil war onset. When governments are incapable, GDP/capita actually lowers the probability of civil war.

This latter finding is a bit strange, but is illuminated by the predictions from the model. Most of the false positives in the urban incapacity model have no civil war experience - Dominican Republic, Hungary, Czechoslovakia, Bolivia, Spain and Poland. These are all countries that may have allowed a certain level of urban dissent without resorting to repression either due to tolerance or incapacity. This highlights one problem with this operationalization of our theory. Incapacity also looks a lot like tolerance and it is somewhat difficult to disentangle the two.

The GDP/capita interaction is most helpful for the urban inflammation model. This model correctly predicts 6 onsets not predicted by the Fearon and Laitin model - Russia (1999), Haiti (1991), Democratic Republic of Congo (1996), Ethiopia (1997), Sri Lanka (1987), and El Salvador (1979). While this model also falsely identifies six onsets, every country is involved in a

civil war at the identified time. In the rural context, the inflammation - GDP/capita interaction allows us to identify the civil war onset in Somalia (1991) and Nicaragua (1981) with many of the same false-positives, but similarly all are involved in civil wars at the times identified.

5. Conclusion

Within this research, we set out to advance understanding of civil war onset by identifying and competitively evaluating three hypotheses of conflict escalation from lower-level repression and dissent to large-scale conflagration. To date, prior research has neglected these explanations in favor of more structural explanations; mountains have been preferred to movements of distinct political actors. Specifically, three escalation hypotheses are considered. The first argument - "inflammation" - maintains that civil war emerges when state repression enrages citizens; the second - "incapacity" - maintains that civil war emerges when authorities cannot use repression; and, the third - "ineffectiveness" - maintains that civil war emerges when authorities use repression poorly.

Examining 149 countries from 1976 to 1999, our study reveals that of the different explanations for onset our results show that none of these explanations is sufficient by itself. Inflammation helps predict some civil wars, but so do incapacity and ineffectiveness. In fact, our results identify that through rural and urban strategies and through ineffectiveness, inflammation and incapacity - there are many ways states and dissidents can interact which result in civil war. Not only does this research shed light on hypotheses that were previously ignored or proxied with some structural variables, but it radically transforms the way the we think about these (seemingly) competitive theories of civil war onset. Indeed, by revealing that there are distinct paths to civil war it suggests that in the future our investigations must be more pluralistic in nature. In short, there is no silver bullet for understanding civil war; there is a magazine of different bullets that must be employed.

Considering this point for just a second, the point is rather far-reaching in nature. As designed, our research suggests that different escalatory processes can be equally likely to lead to civil war in different places. In general, social science research tends to try to predict some phenomenon as a function of a single process. Theories of political phenomena are considered

competing and in the end, the dominant theory - the one with the most empirical evidence - will be deemed the winner. Rarely do we consider the substantive predictions (i.e., which cases we get right or close to right). In the specific case of this research, we find that both inflammation and ineffectiveness lead us to predict roughly the same number of civil wars which would suggest (in the dominant paradigm) that each is an equally valid theory of civil war onset. Furthermore, it would send researchers off to collect more data that would help to forward one or the other of these as the dominant escalatory process. We find that these two processes are complementary rather than competing. The civil wars properly predicted by each model are distinct and to suggest that one escalatory process should tell us how all civil wars start is to privilege unduly parsimony and simplicity over accuracy.

On the other hand, there are a number of civil wars that seem to happen in times that are relatively pacific according to major databases of lower-level political conflict. As such, we are unlikely to predict these civil wars as a function of lower-level conflict escalation of any sort. Furthermore, the Fearon and Laitin model was also unable to predict these civil wars, so it is also unlikely that the current institutional model of civil war onset will be able to provide any leverage here either. This suggests that researchers should be looking for other processes that might lead to civil war, for other reasons why countries might erupt into civil war.

The current study not only has implications for theories of civil war onset but they also have implications for how we should study the topic. For example, to examine relationships We followed methodological advice that is on the cutting edge of conflict research. A case-control design coupled with a rare-events logit allow us to exit from the problem of too many zeroes that have plagued the study of civil war onsets. Ward and Bakke (2005) suggest that we should consider the predictive capacity of our models. We have taken this suggestion to heart and have evaluated the addition of conflict variables not on their statistical significance (which could be misleading in that they could actually worsen our ability to understand what is taking place), but on their ability to add to the predictive capability of the model. On these grounds, we have shown that these conflict variables offer considerable improvement.

Our research also suggests that, to the extent possible, every effort should be made to measure distinct components of a theory. This was important in this study because we found

that despite numerous analyses supporting the incapacity and ineffectiveness arguments as well as refuting the inflammation argument, when more direct measures were used (that more closely approximated what the theories described), there was in fact support for all hypotheses, but that each one identified different events.

Clearly, there is some need for further development of our work. As a way to improve upon this effort, it will be important to take up several areas. First, we should increase the number of countries and years examined in the study, recovering as much of the original Fearon and Laitin sample as possible. Second, we should attempt to identify and/or develop better indicators of pre-civil war dissident activity (including other forms of dissent as well as more refined indicators of guerilla warfare that address spatial dimensions). Our repressive indicators would have to reflect these nuances as well. Third, we should explore, in some detail, the cases that have emerged from our quantitative investigation to gauge the validity of our theoretical argument. Fourth, we should attempt to explain the variation in escalatory outcomes. For example, why would one state-dissident interaction lead to civil war in one context but not another.

Finally, echoing a point made by scholars within the area of civil war (e.g., Sambanis and Zinn 2005), our research leads us to conclude that work on large-scale conflict should more explicitly look to scholarship on lower-level conflict (e.g., human rights violation, state repression, genocide/politicide, guerilla warfare, protest and social movements). Invariably, our research forces us to acknowledge that we would not be able to understand civil war onset until we better understood the role played by lower- level conflict in the escalatory process. Indeed, the research provided here reveals that once one applies a more detailed analysis of lower as well as higher-level conflict behavior, many elements underlying research on civil war weaken (e.g., researchers are guided to pay less attention to invariant structural characteristics and more attention to the behavior of authorities and state challengers). To date, these two areas (the lower and higher levels of conflict) have been isolated from one another, but newer research suggests that this is inappropriate. In fact, we would go so far as to say that they key to comprehending the origins of civil war reside in better understanding lower-level conflict behavior as well as the processes that move political actors from these activities to larger- scale efforts. We must move from mountains to movements.

Table 1. Names and Definitions of Laitin and Fearon Variables

Variable	Definition
Previous War	Coded 1 if a country was involved in a distinct civil war in the previous period, zero otherwise.
GDP/capita$_{t-1}$	Gross Domestic Product per capita, lagged one period
Population$_{t-1}$	Natural logarithm of Total Population (World Bank) lagged one period.
% Mountainous	Natural logarithm of the estimated % of the entire land mass that is mountainous in nature.
Noncontiguous State	Coded 1 if the state has a territorial base separated by distance or water from the state's center, zero otherwise.
Oil	Coded 1 if greater than one-third of the country's export revenues came from fuel export in that year, zero otherwise.
New State	Coded 1 if the state is in its first or second year of independence, zero otherwise.
Instability$_{t-1}$	Coded 1 if the country experienced a change of greater than three points points in the polity index in any of the three years prior to the year in question, zero otherwise.
Polity2$_{t-1}$	-10 (autocratic) to 10 (democratic) scale of regime type from PolityIV. This measure is lagged one period.
Ethnic Fractionalization	The probability that any two individuals drawn at random from a country will be of the same ethnolinguistic group.
Religious Fractionalization	The probability that any two individuals drawn at random from a country will be of the same religious group.
Adopted from Laitin and Fearon (2003).	

Table 2. Conditional Coefficients of Conflict given Favorable Conditions

Favorable	Inflammation b/(se)	Inflammation Pr(Y=1)	Incapacity b/(se)	Incapacity Pr(Y=1)	Ineffectiveness b/(se)	Ineffectiveness Pr(Y=1)
1	5.341	0.649	2.117	0.069	16.769	1.000
	(0.001)		(0.056)		(0.000)	
2	4.693	0.590	2.290	0.118	13.587	0.999
	(0.001)		(0.049)		(0.000)	
3	4.046	0.530	2.463	0.195	10.405	0.998
	(0.001)		(0.037)		(0.000)	
4	3.398	0.468	2.637	0.305	7.223	0.975
	(0.001)		(0.025)		(0.000)	
5	2.750	0.407	2.810	0.443	4.041	0.717
	(0.001)		(0.024)		(0.000)	
6	2.103	0.349	2.983	0.590	0.859	0.139
	(0.001)		(0.029)		(0.000)	