What Is Civil War?

CONCEPTUAL AND EMPIRICAL COMPLEXITIES OF AN OPERATIONAL DEFINITION

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The empirical literature on civil war has seen tremendous growth because of the compilation of quantitative data sets, but there is no consensus on the measurement of civil war. This increases the risk of making inferences from unstable empirical results. Without ad hoc rules to code its start and end and differentiate it from other violence, it is difficult, if not impossible, to define and measure civil war. A wide range of variation in parameter estimates makes accurate predictions of war onset difficult, and differences in empirical results are greater with respect to war continuation.

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Advances in the empirical literature on civil war depend critically on the collection and refinement of data on civil war occurrence. Most civil war lists rely heavily on the Correlates of War (COW) project.[1] Since the first COW list was published, there has been little peer review of COW coding rules. Within the walls of the COW project, there has been substantial debate on how to improve data quality. That debate, however, has not benefited from open scholarly discourse.[2] Given the project's preeminence, many large-*N* studies use COW data unquestioningly or limit themselves to making only minor changes to COW data.[3] Currently, about a dozen research projects

1. Singer and Small (1972, 1994), Small and Singer (1982), Sarkees and Singer (2001), and Sarkees (2000).
2. Personal communication (July 31, 2001) with Stuart Bremer, founding member of the Correlates of War (COW). COW recently initiated an online forum for public debate, but the forum is rarely used.
3. Walter (2002) and Collier and Hoeffler (2001) use COW data without making any modifications. Mason and Fett (1996) make only minor changes.

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have produced civil war lists based on apparently divergent definitions of civil war, but there is less pluralism here than one might think. Most projects do not conduct original historical research and depend heavily on COW. The result may be replication of errors due to the original COW coding rules and uncertainty about whether different definitions generate different results.

I take a close look at the operational definition of civil war and review several coding rules used in the literature and analyze their implications for our understanding of civil war. To make the discussion concrete, I refer to specific cases. This exercise generates insights into our ability to measure civil war and distinguish it from other forms of political violence. Drawing on those insights, I propose a new coding rule and offer a new civil war list. I then measure the impact of differences across coding rules by regressing the same civil war model on 12 different versions of war onset and prevalence. Although it may be impossible (and some would argue undesirable) to arrive at a consensus on a single definition and measurement of civil war, it is important to know if our understanding of civil war is affected by different coding rules. My analysis shows that some of our substantive conclusions are sensitive to differences in coding rules, whereas others are remarkably robust.

Significant differences across civil war lists are mainly due to disagreement on three questions: What threshold of violence distinguishes civil war from other forms of internal armed conflict? How do we know when a civil war starts and ends? How can we distinguish between intrastate, interstate, and extrastate wars? Answers to these questions are not only relevant for the purposes of accurate coding, but they also reveal the degree to which we share a common understanding of the concept of civil war.

I discuss these issues as a way to explore what differences across coding rules can tell us about the meaning of civil war. I do not offer new theory that helps distinguish civil war from other forms of violence.[4] Rather, I want to see if available operational definitions (coding rules) allow us to measure and analyze civil war as a distinct category with a "natural history" (cf. McAdam, Tarrow, and Tilly 2001), an "ontology" that is different from that of other forms of political violence (cf. Tilly 1978). Thus, I implicitly accept here the premise that civil wars are different from other violence and consider if the coding rules we have at our disposal are sufficient to make a clear empirical distinction between civil war and other violence.

I find that it is not possible to arrive at an operational definition of civil war without adopting some ad hoc way of distinguishing it from other forms of armed conflict. Although a core set of "ideal" cases of civil war may exist, too many cases are sufficiently ambiguous to make coding the start and end of the war problematic and to question the strict categorization of an event of political violence as a civil war as opposed to an act of terrorism, a coup, genocide, organized crime, or international war.[5] In the end, it may be difficult to study civil war without considering how groups

4. I offer such a theoretical discussion in a book-length manuscript, currently in progress. For a discussion of why we should distinguish civil war from other political violence, see Sambanis (2004).

5. Proceeding theoretically, rather than empirically, Tilly (2003, 14) makes a similar argument, stating that civil war does not have a distinct causal logic. It is a form of "coordinated destruction"—a typology that includes various other forms of political violence that generate salient "short-run damage" and are perpetrated by highly coordinated actors (the two dimensions that describe Tilly's typology of violence).

in conflict shift from one form of violence to another, or it may be profitable to analyze political violence in the aggregate, rather than cut across that complex social phenomenon with arbitrary definitions. This article is a first step in exploring that idea by providing an analytical review of existing coding rules that highlights the difficulty of accurately defining and measuring civil war. It also improves currently available coding rules at the margin, so as to produce a civil war list that is more consistent with the core elements of most working definitions of civil war.

HOW WOULD WE KNOW A CIVIL WAR IF WE SAW ONE?

In their seminal study *Resort to Arms*, Small and Singer (1982, 210) defined a civil war as "any armed conflict that involves (a) military action internal to the metropole, (b) the active participation of the national government, and (c) effective resistance by both sides." The main distinction they drew between civil (internal or intrastate) war and interstate or extrastate (colonial and imperial) war was the internality of the war to the territory of a sovereign state and the participation of the government as a combatant. Civil war was further distinguished from other forms of internal armed conflict by the requirement that state violence should be sustained and reciprocated and that the war exceeds a certain threshold of deaths (typically more than 1,000).

This definition is deceptively straightforward. It is, I will argue, difficult, if not impossible, to develop an operational definition of civil war without adopting some ad hoc coding rules to distinguish civil wars from other forms of political violence and accurately code war onset and termination. First, it is often difficult to distinguish extrastate from intrastate wars: for example, the Russian civil war in Chechnya in the 1990s might be considered as a war of decolonization, similar to Cameroon's war of independence in 1954.

Second, it is unclear *what degree of organization* is required of the parties to distinguish a civil war from one-sided, state-sponsored violence. In some cases, a functional government has ceased to exist, but we still code a civil war (e.g., Somalia after 1991). In other cases, the government may be fighting a war by proxy using militias in seemingly disorganized intercommunal clashes (as in Kenya's Rift Valley from 1991 to 1993), but most would not classify such a case as a civil war. Elsewhere, rebel organizations are indistinguishable from criminal networks or ragtag militias.

Third, if we focus on a numerical threshold of deaths to identify wars, how do we deal with the problem of unreliable reporting and incomplete data? Even with reliable data, should termination be coded only on the basis of significantly reduced hostilities, or should we also focus on discrete, easily coded events, such as peace treaties?

Fourth, given that violence during civil war is typically intermittent, how do we determine when an old war stops and a new one starts? And how can we distinguish the end of a civil war from the beginning of a period of politicide, terrorism, or other form of violence?

The COW project has provided a valuable public good to the profession by coding cases of war, but it has not offered much guidance on how to answer these difficult questions. It may also have caused some confusion. One source of conceptual confusion is the lack of clarity on the threshold of deaths used to distinguish civil war from other violence. Small and Singer (1982, 213) used an annual death threshold of 1,000 deaths in their early coding efforts, although they later seem to have abandoned that criterion. Many authors still operate under the assumption that the COW project uses an annual battle-death criterion.[6] This uncertainty stems from the fact that COW coding rules have changed at least three times. The first major change was the adoption of an annual death criterion for civil wars in *Resort to Arms*, whereas no such requirement existed for international wars in *The Wages of War, 1816-1965* (Singer and Small 1972). Then the annual death criterion for civil wars was relaxed (Singer and Small 1994), but it was still required for extrasystemic wars, although now deaths incurred by nonsystem members were counted (Singer and Small 1994, 5).[7] In a third set of changes, Sarkees (2000, 129) and Sarkees and Singer (2001) classified some extrasystemic wars as civil wars while abandoning the annual death criterion for extrastate wars.[8] It is unclear why the definition of *extrasystemic war* kept changing or if the new rules were used to recode all armed conflicts in empires and colonies throughout the period covered by COW.[9] More important, if data on annual deaths had been collected to code civil wars according to the old (annual) criterion up to the 1994 revision, why would COW researchers not have made use of those data, instead of losing much of that information by using a binary variable denoting the onset and termination of civil war? To fully evaluate the research that went into compiling the COW lists, one needs access to COW's raw data and coding rules.[10]

COW's efforts to constantly refine its data and improve its coding rules are admirable. But this process raises the following question: were the new rules consistently reapplied to historical data? When the annual death criterion was abandoned, how did COW coders determine the beginning and end of a civil war? Did they code the first year with 1,000 deaths as the onset of the war?[11] Or did they code the start at the year that the cumulative death toll surpassed the 1,000 mark? An example that demon-

6. See Walter (2002), Gleditsch et al. (2002, 617), and Licklider (1995, 682).

7. According to Singer and Small (see codebook, 1994), interstate system membership is defined in terms of "certain minimal criteria . . . at least 500,000 total population and either diplomatic recognition by at least two major powers or membership in the League of Nations or United Nations." Extrasystemic wars are fought between "a nation that qualifies as an interstate system member . . . [and] a political entity that is not an interstate system member." They are subdivided into imperial and colonial wars. I refer to *extrasystemic* and *extrastate* wars interchangeably, as they both refer to colonial and imperial wars.

8. As I was working on final revisions of this study, I received an e-mail message from Professor Sarkees (March 15, 2004), stating that the 1,000 battle-death threshold has never been abandoned in the COW project and that poor editing of the Sarkees (2000) article gives readers this mistaken impression. Several members of the COW team had read early versions of this study, and they had never corrected me. Moreover, I have referred to codebooks and publications from COW that counter Sarkees's recent claim.

9. A concern is that information on deaths of colonial subjects might have been systematically worse in empires than information on deaths of insurgents in nation-states, particularly in the post-1945 period.

10. *The Wages of War, 1816-1965* and *Resort to Arms* include appendices on included and excluded wars, but little, if any, explanation is given for exclusions. Most cases were dropped because they did not meet the death threshold (Small and Singer 1982, 330).

11. This may be the implicit COW coding rule, according to a communication with Stuart Bremer (July 31, 2001). However, Gleditsch et al. (2001, 16) note that "any conflict coded by COW as having more than 1,000 battle-deaths overall is recorded as a civil war for all years (even years of inactivity and years before the cumulative death toll reached 1,000)."

strates potential errors in the application of the new rules is the Algerian civil war in the 1990s. This war, which some data sets code as starting in 1992, is omitted from Singer and Small (1994), even though five other wars that started in 1992 were included (thus revealing that their coding extended through 1992). Perhaps the war had not caused more than 1,000 deaths in 1992 (it actually had), but the revised COW data set, which goes up to 1997 (Sarkees and Singer 2001), includes the Algerian war with a 1992 start date. Because the coding rules were the same in the two COW data sets, unless this was a coding error in the 1994 version, the coded start of the war in the new version suggests that the war reached the 1,000 death mark only *after* 1992, and the start of the war was then back-coded to the start of the violence in 1992.

The cumulative death criterion added few wars to the COW list. For the period from 1816 to 1979, 13 wars were added to the 1994 list (107 wars were included in the 1982 list). Only 5 of these 13 wars had been included in Appendix B of *Resort to Arms* (Small and Singer 1982), and 3 of these 5 were excluded because of lack of system membership of the participants or because the violence was characterized as a massacre. Thus, shifting to the cumulative death criterion added only 2 wars to the list.[12]

The cumulative death criterion introduces some problems. First, it is harder to know when to code the start of the war. If we code it the first year the killing begins, then we will not be able to study violence escalation because the outbreak of minor violence will be subsumed in the period of "civil war." One research group that tries to avoid this problem is Gleditsch et al. (2001, 2002), who code a "war" when they count more than 1,000 deaths in a single year and do not code the beginning of the war during the first year with more than 25 deaths. But their data set does not have a rule for coding war termination, so we do not know precisely how to code their wars in a way that is compatible with other data sets that use the cumulative death criterion. The problem is created by the so-called "intermediate" armed conflict category, in which there are more than 100 but fewer than 1,000 deaths in a given year. If we had a conflict with, say, 600 people killed in the first year, then 3 years with virtually no deaths, and then another year with 600 deaths, this might qualify as a civil war according to the cumulative death criterion but would be coded as two distinct events of intermediate violence in the Gleditsch et al. (2001, 2002) data set.[13] The annual death threshold solves this

12. See, again, Sarkees's comments noted earlier. She argues that COW never abandoned the 1,000 annual death threshold. But this raises new questions. For example, the Uppsala data set and the related data set by Gleditsch et al. (2002), which use an annual death threshold of 1,000 to code civil war, should have few differences from COW lists if COW used an annual threshold. But, as I show later, the differences between these data sets are large. Take the example of Cambodia: Singer and Small (1994) code two civil wars, one from 1970 to 1975 and another from 1979 to 1991. Gleditsch et al. (2002) and Strand, Wilhelmsen, and Gleditsch (2003) code a first war in 1967, a second war from 1970 to 1975, a third war in 1978, and a fourth in 1989 (these all have different "conflict sub-IDs" in the Strand, Wilhelmsen, and Gleditsch data set; hence, they are considered as different "cases" or war starts).

13. The data set by Gleditsch et al. (2001, 2002) has undergone many revisions (one current and four old versions can be found online at http://www.prio.no/cwp/armedconflict [accessed June 22, 2004]). Later versions have addressed (although not entirely) the problem of coding war onset/termination by assigning a "conflict ID" and "sub-ID" to each conflict and considering conflicts to be different if or when they switch from an intrastate to an interstate war (and vice versa), when the parties and issues are different, or when there are more than 10 years with fewer than 25 deaths per year. However, it is still not clear to me if a "same" conflict that switches from "war" to "minor" and back to "war" should be considered a single "war" for the purposes of comparison with war lists that use a cumulative death criterion.

problem because an end to the war would be coded whenever violence drops below 1,000 deaths. But this creates the opposite problem of coding too many war starts in what is essentially the same conflict, if levels of violence fluctuate widely.

One way around these problems is to stop trying to code and analyze civil war as a distinct phenomenon and, instead, to code levels of violence along a continuum (by country and year). Wars might then be identified as periods with many violent spikes, in addition to other characteristics of the violence, depending on the authors' theoretical argument (e.g., the degree of organization of the parties, the presence of two-way violence, etc.). The analysis would then try to explain political violence first and, by refining the theory, explain why violence takes different forms.

A second problem with the cumulative-death criterion is that it could simply lead us to code as civil wars many small conflicts that slowly accumulate deaths. In effect, this criterion creates inherently right-censored data, making it hard to code war termination. By consistently applying the cumulative death criterion, Sarkees and Singer (2001) could code as a civil war any minor conflict or terrorist campaign that causes 25 deaths per year for more than 40 years. Fearon and Laitin (2003, 76) attempt to correct this with a rule that 100 deaths must occur every year *on average* in an ongoing war. But, in combination with the 1,000 aggregate-deaths threshold, this creates another logical problem. They would not code as a civil war a conflict that caused 900 deaths over 9 years, but they would code a conflict that caused 1,000 deaths over 10 years.[14]

A useful example to consider is Northeast India (Nagaland), where Fearon and Laitin (2003) code an ongoing civil war since 1952. I was not able to find evidence that many (say, more than 100) deaths per year occurred in armed conflict there from 1952 to 1961. According to Gleditsch et al. (2002, appendix) and Small and Singer (1982, 339), there was no war or intermediate violence during any year of the conflict in Nagaland. This case illustrates not only the problem of how to code war termination with the cumulative threshold but also the related difficulty of how to handle several chronologically overlapping insurgencies in the same country. Combining regionally concentrated insurgencies in India's Northeast states may be reasonable and would probably satisfy the aggregate-death threshold in the period considered by Fearon and Laitin. But a strict application of the cumulative-death rule in such cases is problematic, given that in other countries, chronologically and even geographically overlapping insurgencies are often treated as separate conflicts. How to distinguish between these rebellions is not always easy. Burma, Chad, India, Ethiopia, and Zaire (in the 1960s) are all countries that pose difficulties in distinguishing among various rebellious groups and periods of violence.[15] In the absence of a clear standard of how to handle such complicated cases, a rule of thumb should be to code a "civil war" in countries with many overlapping insurgencies when the violence escalates markedly and not at the start of low-level hostilities. In the case of India, this means that if we were to com-

14. Moreover, that 100 is the *average* number of deaths per year means that a conflict with thousands of deaths in the first year can be considered ongoing, even if annual deaths after the first year fall to near zero.

15. For example, Gleditsch et al. (2002) and Strand, Wilhelmsen, and Gleditsch (2003) distinguish between the Serb and Croat rebellions in Bosnia, but most others combine the two insurgencies into a single "Bosnian war" event. At least five separate rebellions were ongoing from 1960 to 1965 in Zaire (now Democratic Republic of the Congo), and all data sets typically combine these events into a single civil war.

bine the rebellions in the Northeast states, a civil war should be coded as starting in the 1980s, when violence escalated in Assam, Tripuras, and Manipur.

Despite these problems, a cumulative-death threshold becomes more defensible if it is combined with a criterion that military activity be sustained for the duration of the conflict. Both Sarkees and Singer (2001) and Fearon and Laitin (2003) apply this rule, but it is unclear how sustained military activity is best defined. To focus on an average number of deaths per year reintroduces the problems of counting annual deaths. If we can accurately count 100 deaths per year, we might be able to accurately measure higher numbers of deaths and be better off using these numbers as our dependent variable. We might instead count battles, requiring at least one battle per year between the same parties. But perhaps counting "battles" would create a definitional problem more severe than counting deaths, and again we would need an arbitrary "number-of-battles" threshold, which would exclude cases of low-level insurgency in wars of attrition where battles are not easily distinguishable from terrorist activity (as in the case of Peru's civil war).

To reach a balance between the pros and cons of the absolute versus annual thresholds, we must consider a few issues related to how we measure war magnitude:

1. What level of violence qualifies as civil war?
2. Should this be an absolute or relative level?
3. Should we only count battle deaths or also civilian deaths?

ARRIVING AT AN ABSOLUTE THRESHOLD OF DEATHS TO CODE A CIVIL WAR

A characteristic of civil war that distinguishes it from other forms of violence is that it causes large-scale destruction. A high threshold of deaths can set wars apart from riots, terrorism, and some coups (although not necessarily from pogroms or genocide). But there is nothing inherently "right" about the 1,000-deaths threshold used in the literature, and strict application of that threshold can cause us to drop several cases that satisfy all other characteristics of civil war. A range of 500 to 1,000 deaths could, in principle, be equally consistent with a common understanding of civil war as an event that causes major destruction. Using a range rather than a single cutoff point may work better, given the highly skewed distribution of civil war deaths. In 145 civil wars that started between 1945 and 1999, the mean number of deaths is 143,883 (with a standard deviation of 374,065), and the median is 19,000. Despite the high average, 11 conflicts have caused fewer than 1,500 deaths, and some barely reached 1,000 deaths. But these cases—for example, the Taiwanese revolt in 1947, the Dar ul-Islam rebellion in 1953 in Indonesia, or the fighting in Croatia after its independence in 1992—satisfied most or all of the other criteria for civil wars: they were fought by well-organized groups with political agendas, challenging the sovereign authority, and violence was reciprocal. Given the poor quality of our data (recall how hard it was to count the number of dead after the Twin Towers attack on September 11, 2001, in New York and consider how much harder it would be to measure civil war deaths in Angola in 1990 or Tibet in 1951) and the skewed distribution of deaths, we should use a more flexible coding rule, such as a range of deaths, instead of the 1,000-death threshold.[16] We could *provisionally* code a war onset at the year that we count 100 to 500 deaths and keep the event as a civil war if coders count more than 1,000 deaths in total within 3 years of onset. This incorporates the sustained fighting rule because events with low deaths in the first year would be coded as wars only if armed conflict is sustained and causes 1,000 or more deaths within a 3-year period. The value of this coding rule is that it could easily be applied to back-code events by integrating existing databases that have information on lower level armed conflicts.

FOCUSING ON THE RELATIVE MAGNITUDE OF VIOLENCE

Another problem with the absolute threshold is that it does not reflect the conflict's magnitude as well as a relative (per capita) measure would. If we added a per capita deaths measure to our coding rules, we would be less likely to miss armed conflicts in small nations that produced few deaths but were nonetheless dramatically important for the history of those countries. A 1-year conflict that killed 500 people (the lowest number in the range suggested above to code war onset) in a country with half a million inhabitants (the smallest population size allowed by most coding rules) would amount to deaths at the level of 0.001 of the population. A conflict of equal magnitude in a country of 20 million people would have caused 20,000 deaths and would have been coded as a civil war in all data sets. We could use the 0.001 threshold as a benchmark, and wars that do not exceed the 500 to 1,000 deaths mark could still be classified as civil wars if they meet the per capita deaths criterion.

An example that illustrates the need for greater flexibility in applying death thresholds is the Dhofar Rebellion in Oman (1965-1976). This was an insurgency waged by an ethnically organized rebel group (the Dhofar Liberation Front [DLF]) against the Sultan of Oman. The rebels recruited mainly from the Qara, a small ethnic group living in the mountains of the Dhofar province, and the group presented itself as Communist-Maoist. Young potential recruits were sent to school to receive political indoctrination (Price 1975, 7; Connor 1998, 156-57). There was an element of Islamic fundamentalism in the group's ideology until mass defections of Islamist soldiers after the 1970 Bait Ma'ahshini massacre (Connor 1998, 159). Some scattered terrorism took place in the populous north, but fighting was largely confined to mountainous areas. The rebels were supported by the People's Republic of Yemen in their campaign to control the mountains (Price 1975, 3). The pattern of armed conflict and the organization of the rebellion are consistent with a common understanding of civil war, but this case is typically excluded from civil war lists because of a low death count, even though, in per capita terms, this conflict caused more damage than many others that are typically included as civil wars.

The 1,000-deaths criterion may lead us to include more cases of civil wars in large countries, if more populous countries can more easily produce several insurgencies that can cause high levels of deaths. Ethnic rioting in Nigeria or state repression and

16. In my data set, I include cases that may fall just short of the 1,000 mark and identify them as potentially ambiguous cases.

popular dissent in China is much more likely to generate large numbers of casualties than a coup in Fiji or Cyprus. This may be one reason that a variable controlling for a country's population is among the most significant and robust explanatory variables in models of civil war onset. Consider the omission from most data sets of the Greco-Turkish war in Cyprus that started in 1963. This war was excluded by Singer and Small (1972, 398; Small and Singer 1982, 340) due to insufficient death count. The conflict caused around 1,000 deaths and certainly met all other criteria for civil war: there were battles between organized military units, involving the government against rebel militias, and parties with local recruitment and a clearly articulated political agenda. Even if only 500 deaths had occurred in Cyprus in that period, this would have amounted to 0.001 of the population. A war with the same intensity in a country with 100 million inhabitants would have caused 100,000 deaths—a massive tragedy that would have been coded in all data sets.

A per capita measure would capture these cases, but we would also need to preserve a relatively high absolute threshold not to run the opposite risk of selecting too many small conflicts in small countries (or dropping "relatively" small wars in larger countries). Creating a per capita measure is difficult and labor intensive. What we must *not* do is compute per capita deaths in conflicts that have already been selected by COW or other projects on the basis of an absolute-death threshold. Rather, coding must be based on primary research to measure deaths on a per capita basis in all countries with any political violence. A starting point might be the Gleditsch et al. (2001) list of minor armed conflicts. So far, no death figures are available for those conflicts, and we know only that they have caused more than 25 deaths per year and less than 1,000 for the duration of the conflict. Getting better data for those conflicts might facilitate the construction of a per capita measure. An alternative (and less costly) approach is to code per capita deaths for a shorter period (e.g., from 1980 onwards), allowing us to construct a data sample based on the combination of absolute and per capita death measures (post-1980) and another sample using only the absolute threshold. We could then estimate a model on the two samples of the post-1980 period and compare the results. We could then do a sort of out-of-sample test by estimating the same model on each sample period to see if the model's predictions are consistent across periods using either sample. If the results are not influenced by our coding rules, then going through the trouble of coding a per capita measure might not be necessary.

THE EFFECTIVE RESISTANCE CRITERION AND MEASURING BATTLE DEATHS VERSUS TOTAL DEATHS

The third crucial question is whether we should code battle deaths or also include civilian deaths due to the war. The battle-deaths criterion is the legacy of the COW project and usually refers to military deaths (the measure was initially used to measure deaths in interstate wars). Small and Singer (1982, 213) claim that they counted "civilian as well as military deaths" in civil wars, but it is not clear if they have, in fact, coded civilian deaths due to rebel attacks. Civilian deaths must be counted in death totals.[17]

17. This position is gaining ground: see Sarkees and Singer (2001, 12) and Valentino, Huth, and Balch-Lindsay (2001).

Civilians are targeted in civil war and are disproportionately affected by humanitarian disasters created by combatants to hold civilian populations hostage and gain control of territory.[18] One might also consider counting refugees and internally displaced persons as a measure of the human cost of the war (cf. Doyle and Sambanis 2000).

To get an accurate measure of civilian deaths, we must also determine if deaths due to politicide that occur in and around civil war should be counted. Small and Singer (1982, 215) counted deaths due to "acts of massacre committed . . . during the course of a civil war." That seems reasonable, but a harder question is, how do we handle deaths due to acts of massacre that flank periods of civil war? Should we code a war as having started if civilian massacres occur for a period (say, 1 year) and military confrontation ensues with deaths that exceed 1,000 in the next year? What about the reverse situation, where a civil war gives way to civilian massacres? Distinguishing periods of civil war from periods of massacres clearly is very hard, as the case of Cambodia illustrates. Most data sets code civil war onsets in 1970 and 1978 in Cambodia but no war from 1976 to 1977, which is a period that corresponds to civilian massacres (the killing fields). Small-scale battles between the Khmer and Vietnamese-backed troops did take place near the Thai border in that period, but it is unclear how many people died as a result of armed conflict. Hence, most data sets code this as a period of politicide that is distinct from the civil war. Yet, if we could have established that 100 or so people had been killed on the side of the stronger party between 1976 and 1978, we would have coded an ongoing war in that period (especially in data sets using the cumulative-death criterion and where effective resistance is not measured as a percentage of the total number of deaths).

In light of these difficulties, a conservative strategy is to count deaths due to massacres that occur right before or after the war as war-related deaths. The main difficulty is to find evidence of effective resistance. According to Small and Singer (1982, 214-15), effective resistance implies that the stronger side should suffer at least 5% of the casualties of the weaker side. Fearon and Laitin (2003) relaxed this coding rule and measured effective resistance by 100 state deaths. They do not, however, specify if these deaths must occur in the first year of the war or cumulatively throughout the war. Without data on the distribution of state deaths throughout the war, we are left rudderless in trying to distinguish periods of "civil war" from periods of "politicide" or "genocide." (Could we say with certainty that 50 government troops were not killed in the jungles of Cambodia in 1976 and another 50 in 1977?) Some find the distinction easier to make if massacres occur before a "civil war" starts, as in the case of the 1966 massacres in Nigeria, which are typically thought of as separate from the Biafran civil war, which started in 1967.[19] It might also be easier to distinguish massacres from civil war if the massacres occur after a formal end to the war is reached (i.e., a peace treaty). But if the war peters out and mostly one-sided violence starts (as in the 1984-1987

18. It might even be desirable to count civilian deaths that are indirectly the result of the war as, for example, in the case of war-related starvation deaths in Ethiopia and Somalia. However, this is a slippery slope, as it is unclear how far one could trace the health effects of civil war (see Ghobarah, Huth, and Russett 2003 for such an effort).

19. In May-July 1966, massacres of 30,000 Ibos in the Northeast took place, including massacres during the Northern leaders' coup and murder of Ironsi and Ibo leaders. Biafra achieved de facto independence by the end of 1966, and war started in 1967.

massacres in Ndebele, Zimbabwe), the distinction is harder to make, particularly because war may restart within 1 or more years as a result of such massacres.

Thus, the effective-resistance criterion cannot help us establish with certainty the difference between civil war and politicide, and we would need to use a *sustained* effective-resistance criterion to make a clear distinction. Just as Fearon and Laitin (2003) require 100 deaths per year to code an ongoing civil war, we could require 10 state deaths per year to distinguish a civil war from a politicide.[20] But this type of arithmetic seems inappropriate, not least because it trivializes and imposes a petty discipline on the complex violence that occurs during civil war. What if insurgents suffering, say, 1,000 deaths per year can kill 60 soldiers or police officers in one year, none in the next year, and then 5 per year for the next 8 years (for a total of 100 in 7 years)? Would this be any more or any less of a civil war than a conflict in which 20 soldiers are killed per year for 5 years? The problem of counting effective resistance each year might be avoided if we require 100 deaths per year *on average* for the duration of the conflict, but this would obviously require a higher threshold of the state deaths criterion and would lead us to reclassify some civil wars as politicides or genocides.[21] What if we forget about the question of distribution of state deaths over time? Should an absolute criterion of 100 state deaths be the single most important factor in deciding whether to classify an insurgency as a civil war if the insurgency meets all other criteria for a civil war? An example is the Gamaat Islamiya (Islamic Group) and Islamic Jihad's insurgency in Egypt from 1992 to 1997. According to one source, around 1,200 people were killed over 5 or 6 years, which meets the cumulative death criterion.[22] The rebels were organized and had a political ideology. Guerilla insurgency was sustained, and attacks were directed against police, security forces, government ministers, tourists, and the Coptic minority. Whether we code this case as terrorism or civil war currently rests on whether the state suffered 100 or more deaths. This seems a weak criterion on which to base the classification of any case.[23]

Another difficult case is Argentina's "dirty war," in which Harff (2003) codes a politicide from 1976 to 1980, Fearon and Laitin (2003) code a civil war from 1973 to 1977, Singer and Small (1994) do not code a civil war, and Gleditsch et al. (2002) code a war in 1975 and "possibly" in 1976 and 1977. Official statistics from the Argentine military cite 492 deaths (including civilian officials) due to "terrorist attacks" from 1969 to 1980.[24] Guerillas suffered deaths starting in 1969 until 1972 (more than 100 per year), and it is hard to know how many people were killed between 1973 and 1977, the period sometimes coded as a "civil war." Total deaths, including guerillas and civil-

20. The 1,000 cumulative-death criterion divided by the 100 deaths-per-year rule gives a 10-year window for minor conflicts to be labeled civil wars. Dividing the Fearon and Laitin (2003) 100 state-deaths effective-resistance rule by the same 10-year window yields a 10 state-deaths-per-year rule.

21. Also, averaging state deaths might not resolve the problem, if all or most deaths occurred early in the conflict.

22. See my online supplement for information on this and all cases discussed in the text.

23. The Kenyan "shifta" war in the 1960s is another such case that no data set includes as a civil war, most likely because of the difficulty of finding information on the effective-resistance criterion. I found sources listing dozens of killings of police and military but no clear evidence of more than 100, although the number and apparent intensity of battles described in several sources suggest that there was effective resistance.

24. See a more detailed discussion and a list of sources in the online supplement.

ians, in that period seem not to have reached 1,000, particularly if we do not count deaths after the coup of 1976, which brought a new regime to power, and so an end to the "war" could be coded in 1976. The new regime engaged in a massive purge of suspected guerillas and their supporters; starting in 1976, the new regime killed an estimated 9,000 to 30,000 people in the period from 1976 to 1983. During that period, deaths incurred by the state were few, so we could not code an ongoing civil war after the 1976 coup. In the absence of clear rules on how sustained violence and effective resistance must be coded over time (i.e., how many state deaths must occur per year), it is unclear if and when we can code this conflict as a civil war or rather a low-level insurgency, combined with a coup, and followed by a politicide.

This brief discussion should reveal that most of the coding rules currently used to measure civil war are somewhat ad hoc and that the problem is often exacerbated by the low quality of data on deaths due to armed conflict. There is large room for measurement error here, and such error will make it harder to establish empirically the differences between civil war and other forms of political violence. Thus, in the presence of these problems, one might argue that coding rules should never be applied too strictly and that, when we are faced with an ambiguous case, we should err on the side of caution, including such cases while making it possible to identify them at the analysis stage. In the civil war list that I have compiled by applying a new set of coding rules that try to address the issues raised here, I have included ambiguous cases but have flagged them both in the data set and in a supplementary document that explains the coding for each case. This allows analysts to make their own decisions about which conflicts to drop or explore further to confirm that they are accurately coded.

CLASSIFYING AND ANALYZING EXTRASYSTEMIC WARS

Another issue that sometimes accounts for coding differences across data sets is the difficulty of how to code extrastate (colonial, imperial) wars. I argue that extrastate wars may reasonably be considered to be different from other civil wars and excluded from civil war lists.

The concept of a territorial state is central to the definition of civil war but creates some problems in the classification of the so-called extrastate (or extrasystemic) wars. The extrasystemic-wars category in COW included colonial and imperial wars. Imperial wars were defined as wars between "system member[s] versus independent nonmember[s] of the interstate system" (Singer and Small 1972, 382). Colonial wars were defined as wars between a "system member versus an ethnically different, nonindependent, nonmember of the interstate system" (Singer and Small 1972, 382). Extrasystemic wars were classified as a distinct category due to a conceptual distinction between wars that are "peripheral to the center of government (or the metropole)" and therefore qualitatively different from wars that take place within the core territory of the metropole (Sarkees 2000, 126). This was a normative definition because it interpreted the nature of the relationship between governments and dependent or independent regions within the state's territory. This normative element stands in contrast to mechanistic definitions of civil war (such as the death threshold). Moreover, a consideration of power relationships was absent from the definition of interstate wars, which were defined entirely based on a territorial conception of the state. Over time, "the emphasis on the state member as a territorial entity necessitated a re-thinking and ultimate abandonment of the metropole [and periphery] distinction" (Sarkees 2000, 127), leading the COW project to redefine its coding rules and reclassify many extrasystemic wars as civil wars.

Identifying extrasystemic wars is more complicated as a result of the way the COW coding rules evolved. Initially, coding rules for extrasystemic wars were different from rules for intrastate war: "whereas an interstate war which qualified on the basis of battle deaths would be included if the total fatality figure for the protagonists on each side reached the 1000 mark, the member *itself* (including system member allies, if any) had to sustain 1000 battle fatalities in order for the extrasystemic war to be included" (Singer and Small 1972, 36), and "if the war lasted longer than a year, its battle deaths had to reach an annual average of 1,000" (Small and Singer 1982, 56; see also Sarkees 2000, 129).

Eventually, that criterion was relaxed, and nonsystem member deaths were taken into consideration. But could we assume that we would get an accurate count of colonized people's deaths in uprisings against the metropole given the imperial powers' contempt for human life in their colonies? If data measurement problems for extrasystemic wars are systematically different from data problems in other civil wars, then combining the two categories may bias analyses.[25] The Mau-Mau (1952-1955) rebellion in Kenya, for example, was excluded from COW because it caused only 591 deaths on the side of the United Kingdom (Singer and Small 1972, 397). The war was added to the 1994 and 2000 updates of COW as a result of counting Kenyan deaths, but how many other such wars failed even to appear in the original "excluded-wars" list in the first COW publications? One such example is the Rwandan revolution (against Belgium), which Fearon and Laitin (2003) code as a war between 1956 and 1961, but this case does not appear in any COW list. A lingering concern with extrastate wars, therefore, is if the original coding rules have hampered our ability to compile a list according to the current definition without having to engage in painstaking and resource-intensive historical research.

Another important question is if extrasystemic wars should be coded as taking place in the metropole (i.e., in the territory of the system member) or in the territory of the nonsystem member. Given our territorial conception of the state (and of civil war), it seems clear that the war should be coded in the territory of the metropole, but this is not established practice across the literature. Gleditsch et al. (2001), for example, code extrasystemic wars under colonized states. Doyle and Sambanis (2000) and Licklider (1995) code some extrasystemic wars as civil wars (where the country was under trusteeship, as in the case of Namibia, or in the case of the war in Zimbabwe/Rhodesia from 1972 to 1980, given Rhodesia's independent status). Other scholars have committed more obvious errors in coding, for example, Bangladesh's war of independence in 1971 as taking place in Bangladesh rather than Pakistan (Leitenberg 2001), when it is

25. This may be an empirical question because governments everywhere have an incentive to underreport the number of their subjects that they kill.

clear that Bangladesh did not exist as an independent state until after the conclusion of that war. In Sarkees and Singer (2001), the intention seems to be to code extrastate wars as having taken place in the territory of the system member.

This brings up another difficulty. On one hand, extrastate wars that are coded as taking place in colonized states will have to be dropped from quantitative analyses because data for the "usual suspect" explanatory variables (e.g., democracy, gross domestic product [GDP]) are typically not available for dependent territories. On the other hand, if the war is coded as taking place in the territory of the system member, then explanatory variables have to be adjusted for the whole empire. Estimating average GDP per capita or the level of ethnic fractionalization for entire empires would be a very difficult task and could only be done by taking shortcuts that might seriously compromise the quality of the data. Consider also that averaging GDP per capita over entire empires will have the effect of turning highly developed countries, such as France or England, into middle- to low-income countries. If GDP per capita is used as a proxy for state strength, then averaging GDP values for empires will have the effect of underestimating state capacity in the metropoles (although it may accurately capture state capacity with respect to the rest of the empire). But distance from the metropole and military technology are important missing variables here that could explain the metropole's reach.[26]

Empires were uniquely autocratic regimes, in which subjects lived under different forms of government, and one argument for setting extrastate wars apart is that the legal structure of empires prevented the articulation of colonized peoples' demands (voice) and left them with rebellion as their only option. Might right-hand side controls for the level of democracy or autocracy capture this pressure to rebel in colonies? Consider the example of France, which, according to Fearon and Laitin (2003), had several civil wars from 1945 to 1960. France scores as a "deep" democracy over the relevant period in the Polity IV database (Marshall and Jaggers 2000). However, a standard notion of democracy is that it provides checks and balances to resolve disputes peacefully and that it affords the right of political representation to individuals and groups. But this notion does not apply to the periphery of an empire where disputes are resolved despotically, even when they are resolved by consensus in the metropole. Fearon and Laitin attempt to correct for this by using polity scores for empires that are weighted by the share of the colonized subjects to the population of the metropole, thereby reducing the polity score of countries, such as France or England. But it is not clear to me that the combinations of regime characteristics that the Polity IV database uses are even applicable to cases of foreign domination or empire. "Watering down" France's, Britain's, or Belgium's democracy score in this way is likely to make these countries seem like so-called "anocracies" (i.e., regimes scoring in the middle of the Polity range). But one could argue that as far as colonial subjects were concerned, they were living in autocracies, whereas citizens in the metropole were living in a democracy. Thus, considering France or England as anocracies is dou-

26. Moreover, the waves of decolonization wars in the 1950s and 1960s were all related to systemic changes that affected entire groups of countries in similar ways. This, at a minimum, calls for the use of appropriate controls for systemic trends.

bly wrong, and the meaning of an anocracy is here inconsistent with the theoretical hypotheses that scholars usually test by using the anocracy measure.[27]

Perhaps all these problems can be addressed by adding appropriate right-hand side controls to the regressions by denoting, for example, if a country possessed colonies. Regional inequality, for example, may be more significant in explaining political violence in a world full of empires than in a world composed mainly of nation-states.[28] But even trying to measure inequality in the British empire or in the Belgian Congo is likely to be systematically more difficult than measuring it in the average nonimperial nation-state. If so, estimates based on the variables with systematic coding error for empires are likely to be biased. Thus, abandoning the distinction between *metropole* and *periphery* may have been ill-advised. At its core, the earlier COW distinction highlighted that some peripheries were constitutionally excluded from the political process, which presented insurmountable obstacles to the peaceful resolution of political disputes with the metropole.

Despite these arguments, in many respects, extrastate wars are indeed similar to civil wars: rebels are mostly locally recruited, the groups have a political agenda, and the governing authority is involved in the fighting. Thus, analysts could add these wars to their civil war lists, but doing so would necessitate adding appropriate controls to the statistical model, as outlined above. Yet, only colonial wars should be considered for inclusion.[29] Imperial wars of annexation do not meet the territoriality rule because the nonsystem member's territory was never part of the metropole's territory prior to the war. Wars in East Timor, Tibet, or the Western Sahara could be considered civil wars because (a) the fighting that started during a war of annexation continues after the territory is annexed by an imperial power, and/or (b) the annexation is de facto accepted by the international community.[30]

CODING CIVIL WAR

Drawing on the preceding analysis, I propose an operational definition of civil war that resolves some of the problems we have encountered so far. Parts of my coding rule are new, and parts are based on other, widely used coding rules. I provide a list of civil

27. One hypothesis (Hegre et al. 2001) is that anocracies are prone to violent rebellion because they are neither autocratic enough to preempt or crush rebellion nor democratic enough to resolve conflicts peacefully. Many imperial powers were both autocratic enough (in their colonies) and democratic enough (in their metropoles).

28. A common notion of empire is that it uses the periphery to extract resources with no regard for equality; this may also be true in some nation-states, although there is likely to be a systematic difference in the degree of regional inequality between empires and nation-states with the same level of democracy at the metropole.

29. Wars of decolonization may be combined with lists of secessionist wars because they are likely to share some of the same causal logic. However, the difficulties associated with the measurement of key variables in empires (see above) would still apply.

30. Such cases in my data set include Morocco (Western Sahara), Indonesia (East Timor), and Israel (West Bank and Gaza). Excluded is, for example, the Malaysian Insurgency, because the level of violence after independence was very low, although deaths during the phase of decolonization were high (see online supplement). If there is an intervention by another state to prevent annexation of the territory, then this should be considered an interstate conflict, even if local parties join the fighting.

wars that conforms to the coding rule proposed here and later use the new civil war list in an empirical analysis. Extensive notes that justify the coding of each case are available in a supplement posted online.[31]

An armed conflict should be classified as a civil war if

(a) The war takes place within the territory of a state that is a member of the international system[32] with a population of 500,000 or greater.[33]
(b) The parties are politically and militarily organized, and they have publicly stated political objectives.[34]
(c) The government (through its military or militias) must be a principal combatant. If there is no functioning government, then the party representing the government internationally and/or claiming the state domestically must be involved as a combatant.[35]
(d) The main insurgent organization(s) must be locally represented and must recruit locally. Additional external involvement and recruitment need not imply that the war is not intrastate.[36] Insurgent groups may operate from neighboring countries, but they must also have some territorial control (bases) in the civil war country and/or the rebels must reside in the civil war country.[37]
(e) The start year of the war is the first year that the conflict causes at least 500 to 1,000 deaths.[38] If the conflict has not caused 500 deaths or more in the first year, the war is

31. Go to http://pantheon.yale.edu/~ns237/index/research.html#Data. Note that this coding rule is more demanding than most of the others. Sometimes, the information to code each case with certainty is not available, so I identify those cases in the notes. I usually err on the side of caution and include ambiguous cases, identifying them in the data set so that researchers can decide if they want to include them or drop them from the analysis.

32. This includes states that are occupying foreign territories that are claiming independence (e.g., West Bank and Gaza in Israel and Western Sahara in Morocco). A strict application of this coding rule could drop those cases in which the international community (through the United Nations) rejects the state's claims of sovereignty on the occupied territories.

33. We could include countries after their population reaches the 500,000 mark or, from the start of the period, if population exceeds the 500,000 mark at some point in the country series. If a civil war occurs in a country with population below the threshold, we could include it and flag it as a marginal case. Cases of civil war close to the 500,000 mark are Cyprus in 1963 (578,000 population) and Djibouti in 1991 (450,000 population). The per capita death measure would allow us to relax the population threshold.

34. This should apply to the majority of the parties in the conflict. This criterion distinguishes insurgent groups and political parties from criminal gangs and riotous mobs. But the distinction between criminal and political violence may fade in some countries (e.g., Colombia after 1993). "Terrorist" organizations would qualify as insurgent groups according to this coding rule, if they caused violence at the required levels for war (see other criteria). Noncombatant populations that are often victimized in civil wars are not considered a "party" to the war if they are not organized in a militia or other such form, able to apply violence in pursuit of their political objectives.

35. Extensive indirect support (monetary, organizational, military) by the government to militias might also satisfy this criterion (e.g., Kenya during the Rift Valley ethnic clashes), although here it becomes harder to distinguish civil war from communal violence. In some cases, where the state has collapsed, it may not be possible to identify parties representing the state because all parties may be claiming the state, and these conflicts will also be hard to distinguish from intercommunal violence (e.g., Somalia after 1991).

36. Intrastate war can be taking place at the same time as interstate war.

37. This weeds out entirely interstate conflicts with no local participation. The Bay of Pigs, for example, would be excluded as a civil war because the rebels did not have a base in Cuba prior to the invasion. Some cases stretch the limits of this definitional criterion—for example, Rwanda in the late 1990s, when ex-FAR (Rwandan Army Forces) recruits with bases in the Democratic Republic of the Congo engaged in incursions and border clashes against government army and civilians. If this is a civil war, then so is the conflict between Lebanon-based Hezbollah and Israel (assuming the other criteria are met)..

38. This rule can be relaxed to a range of 100 to 1,000 because fighting might start late in the year (cf. Senegal or Peru). Given the lack of high-quality data to accurately code civil war onset, if we do not have a good estimate of deaths for the first year, we can code the onset at the first year of reported large-scale armed

coded as having started in that year only if cumulative deaths in the next 3 years reach 1,000.[39]

(f) Throughout its duration, the conflict must be characterized by sustained violence, at least at the minor or intermediate level. There should be no 3-year period during which the conflict causes fewer than 500 deaths.[40]

(g) Throughout the war, the weaker party must be able to mount effective resistance. Effective resistance is measured by at least 100 deaths inflicted on the stronger party. A substantial number of these deaths must occur in the first year of the war.[41] But if the violence becomes effectively one-sided, even if the aggregate effective-resistance threshold of 100 deaths has already been met, the civil war must be coded as having ended, and a politicide or other form of one-sided violence must be coded as having started.[42]

(h) A peace treaty that produces at least 6 months of peace marks an end to the war.[43]

(i) A decisive military victory by the rebels that produces a new regime should mark the end of the war.[44] Because civil war is understood as an armed conflict against the government, continuing armed conflict against a new government implies a new civil war.[45] If the government wins the war, a period of peace longer than 6 months must persist before we code a new war (see also criterion k).

conflict, provided that violence continues or escalates in the following years. Note that in the data set, I also code the start/end month, where possible. In some cases, my coding rules can be used to identify the start month (e.g., in cases where the war causes 1,000 deaths in the first month of armed conflict). But in most cases, the month only indicates the start of major armed conflict or the signing of a peace agreement, which can give us a point of reference for the start or end of the war, respectively.

39. This rule also suggests when to code war termination if the 3-year average does not add up to 500. In such a case, we can code the end of the war at the last year with more than 100 deaths unless one of the other rules applies (e.g., if there is a peace treaty that is followed by more than 6 months of peace).

40. This criterion makes coding very difficult because data on deaths throughout the duration of a conflict are hard to find. However, such a coding rule is necessary to prevent one from coding too many war starts in the same conflict or coding an ongoing civil war for years after the violence has ended. Three years is an arbitrary cutoff point but is consistent with other thresholds found in the literature. The data notes (see online supplement) give several examples of cases in which the coding of war termination has been determined by this criterion. A more lenient version would be a 5-year threshold with fewer than 500 deaths.

41. This criterion must be proportional to the war's intensity in the first years of the war. If the war's onset is coded the first year with only 100 deaths (as often happens in low-intensity conflicts), then we would not be able to observe effective resistance in the first year of the war if we defined effective resistance as 100 deaths suffered by the state.

42. This criterion distinguishes cases in which insurgent violence was limited to the outbreak of the war and, for the remainder of the conflict, the government engaged in one-sided violence. A hypothetical example is a case in which insurgents inflicted 100 deaths on the government during the first week of fighting, and then the government defeated the insurgents and engaged in pogroms and politicide for several years with no or few deaths on the government's side. If we cannot apply this rule consistently to all cases (due to data limitations), then periods of politicide at the start or end of the war should be combined with war periods. This implies that civil wars will often be observationally equivalent to coups that are followed by politicide or other such sequences of different forms of political violence.

43. Treaties that do not stop the fighting are not considered (e.g., the Islamabad Accords of 1993 in Afghanistan's war; the December 1997 agreement among Somali clan leaders). If several insurgent groups are engaged in the war, the majority of groups must sign. This criterion is useful for the study of peace transitions but may not be as important if researchers are interested in studying civil war duration, for example.

44. Thus, in secessionist wars that are won by the rebels who establish a new state, if a war erupts immediately in the new state, we would code a new war onset in the new state (an example is Croatia from 1992 to 1995), even if the violence is closely related to the preceding war. A continuation of the old conflict between the old parties could now count as an interstate war, as in the case of Ethiopia and Eritrea, which fought a war between 1998 and 2000 after Eritrea's successful secession from Ethiopia in 1993.

45. This criterion allows researchers to study the stability of military victories. Analysis of the stability of civil war outcomes would be biased if we coded an end to civil war through military victory only when the victory was followed by a prolonged period of peace. This would bias the results in favor of finding a positive correlation between military outcomes and peace duration. This criterion is important for analyzing war recurrence but not necessarily war prevalence.

(j) A cease-fire, truce, or simply an end to fighting can also mark the end of a civil war if they result in at least 2 years of peace.[46] The period of peace must be longer than what is required in the case of a peace agreement because we do not have clear signals of the parties' intent to negotiate an agreement in the case of a truce/cease-fire.[47]
(k) If new parties enter the war over new issues, a new war onset should be coded, subject to the same operational criteria.[48] If the same parties return to war over the same issues, we generally code the continuation of the old war, unless any of the above criteria for coding a war's end apply for the period before the resurgence of fighting.

Using these coding rules, I have coded 145 civil war onsets between 1945 and 1999 (2.08% of 6,966 nonmissing observations). Without coding new war onsets in countries with already ongoing civil wars, the number of civil wars is 119 (1.93% of 6,153 nonmissing observations). Out of these cases, 20 are "ambiguous"—that is, they may not meet one or more of the coding rules.

DIFFERENCES IN THE CODING OF CIVIL WAR AND THEIR SUBSTANTIVE IMPLICATIONS

The discussion thus far has established some of the sources of disagreement among civil war lists. Disagreements over the coded year of *onset and termination* of civil war may matter for the inferences drawn when we analyze civil war onset, duration, or recurrence using different data sets. The extent of disagreement over the coded year of civil war *onset* is apparent in Table 1a-d, which presents correlations between war starts during the period from 1960 to 1993—a period covered by most data sets.[49] The unit of analysis is the country-year. The dependent variable is civil war onset, a binary variable. All years of no war are coded equal to 0. There are two versions of coding war starts: in version (a), I code a 1 when a civil war starts and drop observations of ongoing war in that country until the war ends. Thus, if another war starts in the same country while another war is ongoing, we would not consider it. In version (b), I code a 1 whenever a war starts, even if another war is ongoing. Country-years with no new war starts are coded 0, and in this way, we end up with more war starts.[50]

46. Peace implies no battle-related deaths or, in a lenient version of this criterion, fewer deaths than the lowest threshold of deaths used to code war onset, that is, fewer than 100 deaths per year.

47. These situations are different from those in which there is no violence as a result of armies standing down without a cease-fire agreement, which would fall under criterion (f).

48. These incompatibilities must be significantly different, or the wars must be fought by different groups in different regions of the country. For example, we would code three partially overlapping wars in Ethiopia (Tigrean, Eritrean, Oromo) between the 1970s and the 1990s. New issues alone should not be sufficient to code a new war because there is no "issue-based" classification in the definition of civil war. We could apply such a rule if we classified civil wars into categories—for example, secessionist wars versus revolutions over control of the state. In addition to having new issues, most parties must also be new before we can code a new war onset.

49. See the online supplement for a summary of the definitions and operational criteria used by major data projects. The period was selected to include the Collier and Hoeffler (2001) data set (1960-1999) in the analysis, given the prominence of that study in the literature. Most of the other data sets start in 1945. I compare results from a smaller number of data sets covering the entire post-1945 period.

50. Collier and Hoeffler (2001), Hegre et al. (2001), and Sambanis (2001) use version (a). Fearon and Laitin (2003) use version (b).

(text continues on p. 835)

TABLE 1
Correlations among Civil War Lists, 1960-1993

	a. Version (a) of War Onset (3,198 Observations)										
	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001)*	*Fearon and Laitin (2003)*	*Leitenberg (2000)*	*Regan (1996)*	*Doyle and Sambanis (2000, Extended)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	warst1a	warst2a	warst3a	warst4a	warst5a	warst7a	warst8a	warst9a	warst10a	warst11a	warstnsa
warst1a	1.00										
warst2a	0.96	1.00									
warst3a	0.82	0.83	1.00								
warst4a	0.74	0.75	0.71	1.00							
warst5a	0.42	0.46	0.52	0.57	1.00						
warst7a	0.69	0.70	0.70	0.70	0.54	1.00					
warst8a	0.60	0.66	0.56	0.66	0.46	0.59	1.00				
warst9a	0.70	0.70	0.66	0.66	0.46	0.67	0.59	1.00			
warst10a	0.69	0.69	0.80	0.68	0.48	0.72	0.55	0.72	1.00		
warst11a	0.76	0.76	0.75	0.79	0.53	0.77	0.61	0.77	0.85	1.00	
warstnsa	0.74	0.74	0.73	0.83	0.51	0.80	0.62	0.72	0.83	0.88	1.00

b. Version (b) of War Onset (3,503 Observations)

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001)*	*Fearon and Laitin (2003)*	*Leitenberg (2000)*	*Regan (1996)*	*Doyle and Sambanis (2000, Extended)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	warst1b	warst2b	warst3b	warst4b	warst5b	warst7b	warst8b	warst9b	warst10	warst11b	warstnsb
warst1b	1.00										
warst2b	0.92	1.00									
warst3b	0.83	0.83	1.00								
warst4b	0.57	0.55	0.62	1.00							
warst5b	0.37	0.45	0.46	0.40	1.00						
warst7b	0.58	0.61	0.61	0.58	0.49	1.00					
warst8b	0.51	0.55	0.49	0.50	0.37	0.56	1.00				
warst9b	0.74	0.70	0.68	0.52	0.40	0.58	0.51	1.00			
warst10	0.69	0.67	0.80	0.60	0.41	0.61	0.46	0.69	1.00		
warst11b	0.67	0.66	0.73	0.68	0.45	0.63	0.50	0.66	0.85	1.00	
warstnsb	0.64	0.64	0.68	0.69	0.44	0.69	0.51	0.64	0.75	0.77	1.00

(continued)

TABLE 1 (continued)

c. Correlations among Lists of Civil War Prevalence (3,997 Observations)

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Extended)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	atwar1	atwar2	atwar3	atwar4	atwar5	atwar7	atwar8	atwar9	atwar10	atwar11	atwarns
atwar1	1.00										
atwar2	0.94	1.00									
atwar3	0.88	0.90	1.00								
atwar4	0.75	0.76	0.82	1.00							
atwar5	0.62	0.69	0.70	0.68	1.00						
atwar7	0.67	0.70	0.73	0.75	0.65	1.00					
atwar8	0.63	0.67	0.66	0.70	0.59	0.70	1.00				
atwar9	0.62	0.66	0.64	0.66	0.53	0.70	0.62	1.00			
atwar10	0.70	0.73	0.77	0.74	0.64	0.76	0.66	0.77	1.00		
atwar11	0.69	0.73	0.75	0.78	0.66	0.78	0.67	0.77	0.94	1.00	
atwarns	0.69	0.74	0.77	0.79	0.66	0.83	0.69	0.75	0.87	0.87	1.00

d. War Onsets and Prevalence in Major Data Sets, 1960-1993

Data Set	*Number of War Starts*	*Country-Years at War*
Singer and Small (1994)	61	353
Sarkees and Singer (2000)	73	411
Collier and Hoeffler (2001)	70	460
Licklider (1995)	58	505
Gleditsch et al. (2001)	83	330
Fearon and Laitin (2003)	79	663
Regan (1996)	116	764
Doyle and Sambanis (2000)	100	700
Sambanis (current study)	102	691

NOTE: Years for which the dependent variable is coded missing (due to lack of state independence or other reason) in my data are excluded from this list. Some authors code civil wars in those years. Leitenberg (2000) is excluded from this table because his civil war list ends before the end date for the comparison period.

As is evident from Table 1a,b, the correlation between most pairs of civil war onset lists is low. Among the lowest correlations (0.42 and 0.46) are those between Gleditsch et al. (2001) and the two versions of the Correlates of War data set. These correlations are even lower with version (b) of the dependent variable (0.37 and 0.45, respectively).[51] The correlation between Gleditsch et al. (2001) and my data set is 0.44 [version (b)], and the correlation with Fearon and Laitin (2003) is 0.69 [version (b)]. The most highly correlated civil war lists are the two versions of the COW data set.[52]

The correlation between *war prevalence* in any two lists would be higher if one compared wars that started within the same 2- or 3-year period (see Fearon and Laitin 2003 for such a comparison). But that would be a comparison of war *lists*, not war *onsets*. A difference of 2 to 3 years in the coding of war onset is significant because values of right-hand side variables, such as economic growth or political instability, would be immediately influenced by war, which would in turn influence the empirical results. In Table 1c, I correlate civil war prevalence (combined onset and duration) in the different lists. If two lists included different war starts but the war overlapped for most of its duration, then the correlation between the two databases would be higher than in Table 1a,b. Indeed, the correlations between pairs of lists are now higher (e.g., between Gleditsch et al. 2001 and COW2, it rises to 0.69). Thus, there is more disagreement on the question of war onset and termination than there is over whether a war happened at all.

However, there is still considerable disagreement about which armed conflicts should be classified as civil wars. Many wars are coded in only one out of a dozen data sets. Table 1d lists the number of war starts and total country-years of war in the different lists from 1960 to 1993.[53] The highest number of war starts (116) is found in Regan's (1996) list and the lowest two in Licklider's (1995) list (58) and Singer and Small's (1994) list (61).

Now that I have established that there is substantial variation in the coding of war onset and termination in most data sets, it is appropriate to ask if these differences have substantive implications. To answer that question in a tractable manner, I regress the same civil war model on all versions of the civil war onset variable and measure variation in parameter estimates. To isolate the differences that are due to the coding of the dependent variable, I use the same sources of data for the right-hand side variables and analyze the same set of countries over the same period (1960 to 1993, annual observations).[54] Thus, all differences in parameter estimates should be due to differences in the

51. Here I consider only wars from the Gleditsch et al. (2001, 2002) list. In the regression analysis later on, I add a second measure that included minor and intermediate armed conflicts from that list.

52. If we expand the comparison to the entire period from 1945 to 1999, the correlations are even smaller. My data set correlates with others as follows: 0.62 with Sarkees and Singer (2000), 0.34 with Gleditsch et al. (2001), 0.63 with Fearon and Laitin (2003), and 0.67 with Doyle and Sambanis (2000).

53. The number of observations across models differs in Table 2 because I drop observations of ongoing war. Thus, any differences are directly the result of coding war onset and continuation. In Table 4, the number of observations for the Collier-Hoeffler variable is slightly smaller because the lagged war variable is missing for the year 1960 since their data set starts in 1960, and we cannot know how they would have coded the preceding year. Singer and Small's (1994) data set has fewer observations because it does not code events in 1993, and Leitenberg's (2001) data set ends in 1990. Other models all have the same N.

54. Leitenberg's (2001) data set is an exception because it ends with the end of the cold war.

coding rules.[55] I also restrict the analysis to a smaller number of lists that cover the entire period from 1945 to 1999.

The model is very similar to those developed by Fearon and Laitin (2003) and Collier and Hoeffler (2001). Because these studies are by now well known, I do not discuss the theory behind the selection of variables. Briefly, civil war onset is expected to be less likely the higher the level of development, proxied by real per capita income (*gdpl1*).[56] Anocracies (*anoc2l1*), states at the mid-range in the Polity IV series, should have a higher risk of war as they are neither as effective as autocracies in repression nor as good as democracies in peaceful conflict resolution (Fearon and Laitin 2003; Hegre et al. 2001). States with political instability (*inst3l1*) and regime transition should be at higher risk of war onset.[57] Ethnic heterogeneity (*ef*) should increase the risk of war onset by pitting groups with different preferences against each other.[58] Competing theories expect a different relationship between this variable and civil war onset, and empirical results on the link between ethnicity and civil war are mixed. I control for population (measured by the natural log of population, *lpopnsl1*), which has been shown to be significant and positively correlated with war onset.[59] I also control for the percentage of Muslims in the population (*muslim*) as a proxy of part of the "clash-of-civilization" hypothesis and as a measure of religious division.[60] I control for economic growth, measured as annual percentage change in the level of real per capita income (*grol1*), because Collier and Hoeffler (2001) find this to be significantly and negatively correlated with war onset. I control for countries that are significant oil exporters (*oil2l1*).[61] Such countries are thought to be at higher risk of war for a number of reasons—the most commonly encountered hypothesis is that oil corrupts political institutions or that it generates incentives for secessionist war. I control for countries' mountainous terrain (*mtnl1*), following Fearon and Laitin, who view terrain as part of the technology of insurgency (mountains provide hideouts for rebels). I also control for a variable measuring time at peace since the last war (*pwt*). All right-hand side variables are lagged 1 year.[62]

55. A risk is that the degree of multicollinearity in the data may be different across civil war lists, influencing standard errors differentially. But this would be a result of differences in the coding of the dependent variable, so it does not pose a problem for the analysis.

56. I use data from Fearon and Laitin (2003) for this variable. According to civil war theories, higher state capacity should discourage rebellion or allow the state to repress it in its early stages. Higher development levels should discourage rebellion by raising the economic opportunity costs of violence.

57. I use the "Polity 2" series from the Polity IV data project, version 2002, to construct the anocracy and instability variables.

58. Ethnic heterogeneity was constructed by Fearon (2003) and is available in Fearon and Laitin's (2003) replication data set. This is a constant, so I do not lag it.

59. I used data from the World Bank and other sources to complete the population series. See online supplement for more information.

60. The source for this variable is Fearon and Laitin (2003).

61. This series has several differences from the respective series in Fearon and Laitin (2003), although the same underlying sources have been used (World Bank data). I discuss all differences in the online supplement.

62. Fearon and Laitin (2003) do not lag all variables (e.g., *oil* is not lagged) and use the value for the second period in the country series to fill in missing values at the start of the country series for those variables that are lagged.

This model captures the basic logic of prominent theories of civil war.[63] I did not include Fearon and Laitin's (2003) dummy variable for "new states" because this variable perfectly predicts the outcome when I lag all independent variables.[64] Moreover, it is not clear to me what that variable measures that is not already captured by the controls for "instability," "anocracy," and "income." "New" states are more likely to have a civil war than "old" states, as Fearon and Laitin found, but they are also more ethnically diverse than "old" states. Controlling for "new states" may artificially reduce the significance of ethnic fractionalization (*ef*). I say "artificially" because the fact that these states are "new" may well be causally linked to their high ethnic fragmentation. Most "new states" are former colonies with higher ethnic fractionalization than countries in which a nation-building process has reduced the effective level of ethnic fragmentation. It might therefore be the case that the "newness" of the state explains the high degree of ethnic fractionalization. Alternatively, looking further upstream in the chain of causality, the "newness" of the state may also be a consequence of demands for self-determination by ethnically distinct groups that a distant metropole had never attempted or succeeded to integrate into a single nation in the predecessor state.[65] Either way, if there is a causal connection between *new state* and *ef*, we cannot control for both in the regression, and *ef* is the more theoretically interesting concept.

We can now check if the correlations between these key variables and civil war are influenced by the coding of the dependent variable. In what follows, I estimate probit models corresponding to 12 different coding rules and two versions of the war onset variable and for overall war prevalence. I review these estimates, looking for changes in significance levels and very large changes in coefficient estimates (more than 1 or 2 standard deviations), because such volatility would have important policy implications in assessing the link between changes in the explanatory variables and changes in the probability of civil war onset. I present the results of these estimations in several tables.[66] Table 2 includes estimation results for 12 regressions using version (a) of the dependent variable, and Table 3 summarizes the range of these parameter estimates. Table 4 includes estimation results for 12 regressions using version (b) of the depend-

63. This is, of course, only one of several possible specifications of the model. I do not include some variables that others have found significant because I find the theoretical justification for including them problematic or because other variables included in the model are likely to capture much of what the excluded variables are measuring. For example, I do not include a control for noncontiguous territory because it is, by itself, not a good control of state strength without also controlling for military technology and ability to project military power. It may also be the case that stronger states only have the capacity to maintain noncontiguous territories. Although this certainly does not apply to each case, an equality of means test reveals that countries with noncontiguous territories have significantly higher average income ($5,855) than countries with only contiguous territories ($3,223). The t statistic for this test is –17.78.

64. For example, in my data set, there are only five war starts that occur in the second year of the country series and for which income is nonmissing, but all five are dropped due to missing values in the income growth variable.

65. A logit regression of "new state" on "ethnic fractionalization" (*ef*) yields a statistically significant and large coefficient (2.69) for *ef* (z value = 5.52) with 316 observations (I restricted the sample to the first two observations in each country series).

66. I do not report fixed-effects results because fixed-effects estimation is very sensitive to measurement error (hence we can expect the coding rule differences to influence the results). I present these results in the online supplement. There are important differences across war lists, even among variables that are robust in ordinary logit models.

(text continues on p. 840)

TABLE 2

Probit Models of Civil War Onset, 1960-1993

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al (2001) (Wars)*	*Gleditsch et al. (2001) (All)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Expanded)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
Variable	warst1	warst2	warst3	warst4	warst5	warst6	warst7	warst8	warst9	warst10	warst11	warstns
GDP	**–0.109**	**–0.114**	**–0.097**	**–0.075**	**–0.039**	**–0.047**	**–0.093**	**–0.069**	**–0.095**	**–0.098**	**–0.104**	**–0.073**
	(0.032)	(0.040)	(0.033)	(0.030)	(0.020)	(0.018)	(0.032)	(0.027)	(0.030)	(0.031)	(0.032)	(0.026)
GDP growth	–0.384	–0.237	0.493	0.564	–1.218	–0.560	–0.225	0.816	0.084	0.127	–0.015	0.179
	(1.015)	(0.898)	(0.810)	(0.760)	(0.798)	(0.542)	(0.787)	(1.026)	(0.721)	(0.686)	(0.702)	(0.565)
Instability	**0.339**	**0.321**	0.131	0.238	**0.263**	**0.228**	*0.239*	**0.382**	*0.235*	**0.281**	**0.328**	**0.297**
	(0.147)	(0.134)	(0.153)	(0.154)	(0.126)	(0.104)	(0.143)	(0.152)	(0.134)	(0.120)	(0.123)	(0.138)
Anocracy	0.212	*0.233*	0.199	0.234	*0.217*	**0.287**	*0.251*	0.203	0.082	*0.227*	*0.232*	*0.226*
	(0.141)	(0.121)	(0.133)	(0.147)	(0.130)	(0.089)	(0.136)	(0.125)	(0.128)	(0.124)	(0.125)	(0.127)
Oil exporter	0.274	0.222	0.180	0.188	0.204	0.170	–0.060	**0.371**	0.192	0.102	0.185	0.100
	(0.186)	(0.157)	(0.119)	(0.149)	(0.145)	(0.150)	(0.142)	(0.159)	(0.146)	(0.143)	(0.142)	(0.119)
Ethnic frac-tionalization	0.118	0.144	0.175	0.209	0.317	**0.524**	–0.006	0.281	*0.365*	*0.379*	0.261	0.290
	(0.225)	(0.206)	(0.200)	(0.217)	(0.258)	(0.180)	(0.210)	(0.213)	(0.198)	(0.214)	(0.200)	(0.208)
Population (log)	**0.102**	**0.090**	**0.104**	**0.113**	**0.111**	**0.090**	**0.109**	**0.108**	**0.116**	**0.105**	**0.093**	**0.076**
	(0.032)	(0.030)	(0.032)	(0.037)	(0.033)	(0.032)	(0.033)	(0.030)	(0.037)	(0.033)	(0.032)	(0.031)
Terrain	*0.005*	**0.005**	**0.006**	0.003	0.004	0.002	0.004	0.002	0.001	0.002	0.004	0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)

Percentage Muslim	–0.001	0.001	0.002	*0.003*	0.002	**0.003**	**0.003**	0.000	*0.002*	0.000	0.000	0.002
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peace duration	–0.003	–0.004	0.000	–0.006	**–0.012**	0.000	–0.002	–0.003	–0.005	–0.005	–0.003	–0.004
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Constant	**–3.844**	**–3.626**	**–4.014**	**–4.147**	**–3.987**	**–3.668**	**–3.903**	**–4.036**	**–3.898**	**–3.753**	**–3.595**	**–3.370**
	(0.563)	(0.527)	(0.582)	(0.718)	(0.587)	(0.506)	(0.574)	(0.553)	(0.615)	(0.599)	(0.599)	(0.563)
Observations	3,770	3,861	3,815	3,765	3,926	3,530	3,611	3,372	3,518	3,620	3,642	3,622
Log likelihood	–229.21	–263.55	–254.81	–224.91	–290.74	–435.29	–250.53	–238.11	–313.59	–324.87	–311.12	–312.66
Wald $\chi^2(10)$	67.38	75.69	72.15	73.59	102.21	82.62	89.31	48.72	55.92	68.53	63.48	67.96
Pseudo-R^2	0.0933	0.0995	0.0876	0.0944	0.0877	0.0719	0.0934	0.0849	0.0871	0.0906	0.0903	0.0741

NOTE: Coefficients (standard errors) are presented. Ongoing wars dropped after start. Bold indicates significance at .05 or higher; italics indicates significance at .10. GDP = gross domestic product.

TABLE 3
Summary of Parameter Estimates from Table 2

Model 1 Variables	*Observations*	*Mean*	*Standard Deviation*	*Minimum*	*Maximum*
GDP coefficient	12	–0.084	0.024	–0.114	–0.039
GDP *SE*	12	0.029	0.006	0.018	0.040
Growth coefficient	12	–0.031	0.550	–1.218	0.816
Growth *SE*	12	0.776	0.152	0.542	1.026
Instability coefficient	12	0.273	0.066	0.131	0.382
Instability *SE*	12	0.136	0.016	0.104	0.154
Anocracy coefficient	12	0.217	0.048	0.082	0.287
Anocracy *SE*	12	0.127	0.014	0.089	0.147
Oil coefficient	12	0.177	0.104	–0.060	0.371
Oil *SE*	12	0.146	0.018	0.119	0.186
Ethnic fractionalization coefficient	12	0.255	0.139	–0.006	0.524
Ethnic fractionalization *SE*	12	0.211	0.019	0.180	0.258
Population log coefficient	12	0.101	0.012	0.076	0.116
Population log *SE*	12	0.033	0.002	0.030	0.037
Terrain coefficient	12	0.003	0.002	0.001	0.006
Terrain *SE*	12	0.003	0.000	0.002	0.003
Muslim coefficient	12	0.001	0.001	–0.001	0.003
Muslim *SE*	12	0.001	0.000	0.001	0.002
Peace duration coefficient	12	–0.004	0.003	–0.012	0.000
Peace duration *SE*	12	0.005	0.001	0.004	0.006
Constant coefficient	12	–3.820	0.224	–4.147	–3.370
Constant *SE*	12	0.582	0.053	0.506	0.718

NOTE: Mean coefficients and standard errors are presented for each variable. GDP = gross domestic product.

ent variable. Table 4 models allow us to gauge the effect of war in the previous period (*warl1*) on the probability of a new war onset in the same country.[67] Table 5 summarizes the range of parameter estimates in Table 4.

One of the most robust variables is income per capita. *Gdpl1* is always highly significant, although its coefficient as reported in Table 3 varies considerably, ranging from –0.039 to –0.114. In Table 4, the results are similar, with a slightly larger range of variation. We can see in Tables 3 and 5 that the coefficient moves by slightly more than 2 standard deviations, depending on the coding of the dependent variable. But the relationship between income and civil war is extremely robust.

Growth (*grol1*) is never significant and switches sign in about half the regressions in Tables 2 and 4.[68] Other nonsignificant variables do not switch signs as frequently as growth, and the switching sign of growth may suggest a particularly strong

67. In Fearon and Laitin's (2003) data set, 14 wars start while another war is ongoing in the same country from 1945 to 1999. In my data set, that number is 29 and 17 for the period from 1960 to 1993. The *warl1* variable captures some of the time dependence of peace and war so it replaces the peacetime variable (*pwt*) that I use in Table 2 models.

68. Using a 3-year growth rate did not change this. By lagging growth, we lose the first two observations in the country series because we must lag *gdpen* by 1 year to calculate the growth rate (*gdpgro*), which cannot be calculated for the first year of the series. I coded one version of the growth variable, *grol1m*, where

(text continues on p. 843)

TABLE 4

Probit Models of Civil War Onset, 1960-1993

Variable	*COW 1994*	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (All)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Expanded)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	warst1b	warst2b	warst3b	warst4b	warst5b	warst6b	warst7b	warst8b	warst9b	warst10b	warst11b	warstnsb
GDP	**–0.126**	**–0.134**	**–0.092**	**–0.077**	**–0.056**	**–0.054**	**–0.098**	**–0.088**	**–0.104**	**–0.102**	**–0.105**	**–0.083**
	(0.035)	(0.039)	(0.029)	(0.028)	(0.023)	(0.016)	(0.029)	(0.030)	(0.026)	(0.029)	(0.028)	(0.026)
GDP growth	–0.295	–0.072	0.195	0.584	–1.087	–0.895	–0.351	0.431	–0.524	–0.010	–0.197	–0.170
	(0.914)	(0.791)	(0.716)	(0.738)	(0.804)	(0.560)	(0.774)	(0.896)	(0.539)	(0.574)	(0.626)	(0.529)
Instability	**0.316**	**0.348**	0.130	0.217	**0.284**	0.134	0.167	**0.393**	*0.196*	**0.258**	**0.315**	*0.227*
	(0.161)	(0.145)	(0.145)	(0.152)	(0.119)	(0.094)	(0.131)	(0.143)	(0.108)	(0.119)	(0.118)	(0.131)
Anocracy	**0.314**	**0.289**	*0.226*	0.215	0.204	**0.247**	*0.239*	0.170	**0.277**	**0.294**	**0.239**	*0.203*
	(0.139)	(0.127)	(0.121)	(0.143)	(0.132)	(0.082)	(0.124)	(0.123)	(0.123)	(0.114)	(0.114)	(0.112)
Oil exporter	**0.358**	*0.291*	0.194	0.222	*0.240*	**0.302**	0.111	**0.373**	*0.197*	0.139	0.221	**0.210**
	(0.180)	(0.155)	(0.130)	(0.156)	(0.141)	(0.116)	(0.140)	(0.153)	(0.111)	(0.145)	(0.142)	(0.106)
Ethnic frac- tionalization	0.095	0.157	0.295	0.260	0.391	**0.591**	0.084	0.075	*0.337*	**0.409**	*0.340*	*0.332*
	(0.212)	(0.199)	(0.203)	(0.222)	(0.277)	(0.169)	(0.188)	(0.228)	(0.197)	(0.202)	(0.186)	(0.190)
Population (log)	**0.090**	**0.083**	**0.094**	**0.129**	**0.143**	**0.133**	**0.131**	**0.101**	0.050	**0.093**	**0.099**	**0.097**
	(0.031)	(0.029)	(0.032)	(0.045)	(0.035)	(0.033)	(0.031)	(0.028)	(0.039)	(0.032)	(0.033)	(0.030)
Terrain	0.005	**0.006**	**0.006**	0.002	0.003	0.002	0.004	0.001	0.002	0.003	0.004	0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Percentage Muslim	–0.001	0.000	*0.003*	**0.003**	0.002	**0.002**	**0.003**	–0.001	0.001	0.001	0.001	0.002
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
War at (t – 1)	*–0.351*	**–0.351**	**–0.447**	**–0.677**	–0.005	–0.103	**–0.274**	*–0.371*	0.101	**–0.366**	**–0.414**	–0.168
	(0.192)	(0.176)	(0.200)	(0.236)	(0.169)	(0.107)	(0.124)	(0.215)	(0.158)	(0.130)	(0.138)	(0.121)

(continued)

TABLE 4 (continued)

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (All)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Expanded)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
Variable	warst1b	warst2b	warst3b	warst4b	warst5b	warst6b	warst7b	warst8b	warst9b	warst10b	warst11b	warstnsb
Constant	**–3.669**	**–3.575**	**–3.942**	**–4.520**	**–4.733**	**–4.353**	**–4.289**	**–3.747**	**–2.911**	**–3.678**	**–3.798**	**–3.769**
	(0.554)	(0.516)	(0.581)	(0.787)	(0.605)	(0.530)	(0.545)	(0.530)	(0.709)	(0.560)	(0.582)	(0.541)
Observations	4,045	4,179	4,099	4,178	4,179	4,179	4,179	3,782	4,179	4,179	4,179	4,179
Log likelihood	–248.10	–289.52	–266.58	–237.15	–334.42	–561.33	–303.22	–270.64	–443.69	–376.83	–353.05	–381.75
Wald $\chi^2(10)$	63.13	75.12	79.28	74.27	102.19	101.28	75.07	40.53	70.70	59.70	63.08	62.20
Pseudo-R^2	0.0936	0.1026	0.0863	0.0952	0.0908	0.0844	0.0838	0.0715	0.0817	0.0839	0.0896	0.0719

NOTE: Ongoing wars coded 0 if no new war starts. Coefficients (standard errors) are presented. Bold indicates significance at .05 or higher; italics indicates significance at .10. GDP = gross domestic product.

TABLE 5
Summary of Parameter Estimates from Table 4

Model 2 Variables	*Observations*	*Mean*	*Standard Deviation*	*Minimum*	*Maximum*
GDP coefficient	12	–0.093	0.024	–0.134	–0.054
GDP *SE*	12	0.028	0.006	0.016	0.039
Growth coefficient	12	–0.199	0.489	–1.087	0.584
Growth *SE*	12	0.705	0.137	0.529	0.914
Instability coefficient	12	0.249	0.085	0.130	0.393
Instability *SE*	12	0.131	0.020	0.094	0.161
Anocracy coefficient	12	0.243	0.043	0.170	0.314
Anocracy *SE*	12	0.121	0.016	0.082	0.143
Oil coefficient	12	0.238	0.080	0.111	0.373
Oil *SE*	12	0.140	0.021	0.106	0.180
Ethnic fractionalization coefficient	12	0.280	0.155	0.075	0.591
Ethnic fractionalization *SE*	12	0.206	0.027	0.169	0.277
Population log coefficient	12	0.104	0.026	0.050	0.143
Population log *SE*	12	0.033	0.005	0.028	0.045
Terrain coefficient	12	0.003	0.002	0.001	0.006
Terrain *SE*	12	0.002	0.000	0.002	0.003
Muslim coefficient	12	0.001	0.001	–0.001	0.003
Muslim *SE*	12	0.001	0.000	0.001	0.002
Lagged war coefficient	12	–0.286	0.212	–0.677	0.101
Lagged war *SE*	12	0.164	0.041	0.107	0.236
Constant coefficient	12	–3.915	0.493	–4.733	–2.911
Constant *SE*	12	0.587	0.081	0.516	0.787

NOTE: Mean coefficients and standard errors are presented for each variable. GDP = gross domestic product.

endogeneity problem with this variable. Growth is very sensitive to civil war, so getting the date of onset of civil war "wrong" in some data sets should influence the coefficient sign of this variable.[69] A possible explanation for the instability of growth is that, in the immediate postwar period, growth is typically very high, but these are also periods of high risk of war recurrence due to other influences. Thus, some periods of war recurrence might also be high-growth periods, whereas the general trend should be for growth to reduce the risk of civil war.

Political instability is mostly significant and always positive (see Table 2), although in two regressions (Collier and Hoeffler 2001; Licklider 1995) it is nonsignificant, and in another two cases, it is significant only at the .10 level. In Table 4, it appears to be

lagging is delayed by one observation, so this restores the second lost observation in each country series. In Tables 2 and 4, this adds only five observations, including one war (Rwanda in 1963). Using this measure instead of *groll* makes anocracy slightly more significant and terrain a little more significant in two regressions in Table 2 and *ef* a little less significant in Table 4 (Rwanda has a low level of *ef*, at 0.18). The results on growth do not change.

69. Given my concern with endogeneity here, I dropped the growth variable and reestimated all regressions. See versions of Tables 2, 3, 4, and 5 without growth in the online supplement. The results are very similar to the ones presented here.

less robust, being significant in only half of the regressions and nonsignificant in the other half.

Anocracy borders statistical significance in six regressions in Table 2 and is clearly significant only in Gleditsch et al.'s (2001) list, which combines all armed conflicts. Similarly, it is significant in only six out of the twelve regressions in Table 4. Its coefficient ranges from a low of 0.170 (Leitenberg 2001 list) to a high of 0.314 (Singer and Small 1994 list) in Table 4, and the range widens in Table 2 from a low of 0.082 to a high of 0.287.

Oil exports are basically nonsignificant in Table 2, although the picture is more mixed in Table 4, where it is significant in four regressions and borderline significant in another three.[70] Its coefficient varies wildly from –0.060 to 0.371 (Table 3; although it is less unstable in Table 5). This variable is not robust to changes in the coding of war onset.

Ethnic heterogeneity (*ef*) is positive and almost always nonsignificant with a large standard error, except for regression 6 (Gleditsch et al. 2001—all armed conflict) in Table 2. *Ef* is nearly significant (at the .10 level) in Regan's (1996) model (*warst9*), where again a number of additional wars are included because Regan uses a lower death threshold (200 deaths). In Table 4, *ef* is more often significant (in two models at the .05 level and in three more at the .10 level), although its coefficient has a wide range, from 0.075 to 0.591 (see Table 5). That *ef* is generally nonsignificant in Table 2 and much more significant in Table 4 may have something to do with those extra wars that we are able to include when we use version (b) of war onset.[71]

The coefficient of population (*lpopnsl1*) is quite stable and always positive and highly significant, except for model 9 in Table 4, where it is not significant. This interesting result seems to justify my earlier claim that the significance of population size may be an artifact of the high absolute threshold of deaths imposed to classify a civil war. In model 9, Regan's lower death threshold captures more small wars in small countries.

In both Tables 2 and 4, mountainous terrain (*mtnl1*) is relatively stable and is only significant in the Correlates of War and Collier and Hoeffler (2001) data sets. Mountainous terrain is a key variable in theories of civil war that emphasize the opportunity structure for rebellion (e.g. Fearon and Laitin 2003; Collier and Hoeffler 2001), but it is generally not statistically significant.

The percent of Muslims in a country (*muslim*) is marginally significant in two regressions and in another two at the .10 level (see Table 2) and becomes somewhat more significant in Table 4. There, we find it significant in the Fearon and Laitin (2003) model (*warst7b*) with a positive sign, despite the findings of these authors that religious division is nonsignificant in models of civil war onset. However, the period analyzed seems to matter, as later (see Table 6) we will find that the significance of

70. I use the variable *oil2*, lagged once (*oil2l1*). If I were to use Fearon and Laitin's (2003) *oill1* series (lagging it once), it would be significant in model 6 and, at the .10 level, in models 8 and 9 and nonsignificant in all other models, including some of those in which *oil2l1* was significant (COW 1994 and my data).

71. Countries with more than one war at the same time have slightly higher ethnic fractionalization. There are 26 such cases (of chronologically overlapping wars in my data set), and a means test for ethnic fractionalization shows a statistically significant 10-point difference between them and the other cases.

TABLE 6

Probit Models of Civil War Onset, 1945-1999

Variable	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (Wars)*	*Fearon and Laitin (2003)*	*Fearon and Laitin (2003)*	*Fearon and Laitin (2003)*	*Doyle and Sambanis (2000, Expanded)*	*Sambanis (This Study)*	*Sambanis (This Study)*	*Sambanis (This Study)*
	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
GDP	**–0.065**	**–0.079**	**–0.078**	**–0.095**	**–0.104**	**–0.099**	**–0.086**	**–0.090**	**–0.090**	**–0.096**
	(0.022)	(0.023)	(0.023)	(0.025)	(0.025)	(0.024)	(0.023)	(0.025)	(0.023)	(0.025)
GDP growth	*–0.962*	—	**–1.350**	–0.396	—	**–1.443**	–0.143	–0.504	—	**–1.043**
	(0.581)	—	(0.539)	(0.537)	—	(0.617)	(0.358)	(0.409)	—	(0.453)
Instability	**0.305**	**0.279**	**0.278**	**0.222**	*0.176*	*0.186*	**0.220**	**0.237**	**0.205**	**0.191**
	(0.095)	(0.088)	(0.089)	(0.101)	(0.096)	(0.097)	(0.099)	(0.096)	(0.092)	(0.093)
Anocracy	0.157	**0.244**	**0.214**	**0.265**	**0.332**	**0.296**	**0.315**	**0.244**	**0.266**	**0.275**
	(0.108)	(0.090)	(0.095)	(0.101)	(0.093)	(0.096)	(0.104)	(0.087)	(0.079)	(0.080)
Oil exporter	**0.294**	**0.310**	**0.301**	0.145	0.192	0.173	0.104	**0.261**	**0.300**	**0.268**
	(0.127)	(0.128)	(0.135)	(0.128)	(0.119)	(0.124)	(0.140)	(0.108)	(0.102)	(0.106)
Ethnic fractionalization	*0.417*	0.268	0.236	0.254	0.209	0.245	**0.429**	**0.339**	*0.336*	*0.304*
	(0.217)	(0.211)	(0.208)	(0.175)	(0.166)	(0.164)	(0.188)	(0.172)	(0.176)	(0.176)
Population (log)	**0.140**	**0.137**	**0.144**	**0.142**	**0.141**	**0.148**	**0.089**	**0.104**	**0.098**	**0.108**
	(0.028)	(0.025)	(0.027)	(0.029)	(0.029)	(0.030)	(0.031)	(0.025)	(0.026)	(0.026)
Terrain	0.003	0.002	0.002	0.003	0.003	0.003	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Percentage Muslim	0.000	—	—	0.002	—	—	0.001	0.001	—	—
	(0.002)	—	—	(0.001)	—	—	(0.001)	(0.001)	—	—
War at $(t-1)$	0.010	0.016	–0.007	**–0.309**	**–0.342**	**–0.364**	**–0.309**	–0.067	–0.031	–0.066
	(0.150)	(0.143)	(0.145)	(0.097)	(0.104)	(0.102)	(0.123)	(0.102)	(0.103)	(0.103)
Constant	**–4.670**	**–4.500**	**–4.571**	**–4.581**	**–4.472**	**–4.568**	**–3.716**	**–3.908**	**–3.799**	**–3.897**
	(0.487)	(0.447)	(0.474)	(0.504)	(0.489)	(0.514)	(0.527)	(0.459)	(0.460)	(0.468)

(continued)

TABLE 6 (continued)

Variable	*Gleditsch et al. (2001) (Wars)* *1*	*Gleditsch et al. (2001) (Wars)* *2*	*Gleditsch et al. (2001) (Wars)* *3*	*Fearon and Laitin (2003)* *4*	*Fearon and Laitin (2003)* *5*	*Fearon and Laitin (2003)* *6*	*Doyle and Sambanis (2000, Expanded)* *7*	*Sambanis (This Study)* *8*	*Sambanis (This Study)* *9*	*Sambanis (This Study)* *10*
Observations	5,893	6,092	6,051	5,893	6,092	6,051	5,893	5,893	6,092	6,051
Log likelihood	–445.83	–491.67	–479.81	–408.14	–456.90	–442.08	–477.98	–518.93	–557.83	–548.96
Wald $\chi^2(df)$	112.68	112.73	111.37	84.87	70.12	77.65	63.50	69.46	61.17	59.56
Pseudo-R^2	0.0910	0.0870	0.0943	0.0908	0.0897	0.0957	0.0789	0.0841	0.0789	0.0865

NOTE: Ongoing wars coded 0 if no new war starts. Coefficients (standard errors) are presented. Bold indicates significance at .05 or higher; italics indicates significance at .10. GDP = gross domestic product.

muslim disappears when we analyze the entire 1945 to 1999 period. But these new results suggest that newer wars (post-1960) may have some different characteristics from older wars (1945-1959) with respect to the role of religion.

Peace duration is significant only in a single regression (Gleditsch et al. 2001—*warst5*) in Table 2. We get a much more mixed picture in Table 4, where war in the previous period (*warl1*) is significant and negative in about half of the regressions and nonsignificant in the other half.[72]

Comparing the high- and low-death thresholds in the two regressions that are based on Gleditsch et al.'s (2001) data reveals important substantive differences between lower level violence and civil war. In model 6 (all armed conflict), we find that ethnic heterogeneity, percentage Muslim, and oil exporter status (in Table 4) are all very significant, whereas they become nonsignificant when we restrict the analysis to civil wars. Instability, by contrast, is significant only for civil wars and does not influence the risk of lower level violence significantly. This comparison is particularly informative because the same research team did the coding of both lists, and any differences in parameter estimates must be attributed to the death threshold. Also, these two regressions highlight the differences between the two versions of the dependent variable. Using version (a), instability enters significantly in both regressions 5 (civil war) and 6 (all armed conflict), and oil exports are nonsignificant. Using version (b), oil exports become significant for regression 6, and the coefficient for instability drops almost by half (see Table 4).

In Table 6, I ran the same regressions, restricting the number of civil war lists to four so that I could analyze the entire 1945 to 1999 period and use the most recent and well-documented data sets. Here, again, we observe that income and population are robustly significant, and anocracy becomes more significant (although not in regression 1). Instability is significant in most lists, though marginally nonsignificant, using Fearon and Laitin's (2003) list in regressions 5 and 6, where I use their coding scheme and do not lag the first observation in each country series. Anocracy is more robust than previously. Growth is now significant and negative in three of four lists, but only if we do not lag the first observation, which results in artificial starting values for each country series. Adding growth to regressions 3, 6, and 10 causes us to lose some observations, which reduces the significance of some variables (e.g., *ef* in regression 10). Although there is more agreement here than in the previous tables, we can also see some important differences in the results on ethnic fractionalization (now significant in two lists and borderline significant in regression 1 with Gleditsch et al.'s 2001 list) and with respect to oil exporter status, which is positive and significant in two out of four lists (Gleditsch et al. 2001; Sambanis 2004). Similarly, war in the previous period is significant and negative only in two out of four lists (Fearon and Laitin 2003; Doyle and Sambanis 2000). Other variables—percentage Muslim and mountainous terrain—are consistently nonsignificant.

72. It would have been interesting to explore the effects on war onset of interactions between this variable and other right-hand side variables but, because there are only a few instances of chronologically overlapping civil wars in the same country, these interaction terms might have had the effect of overfitting the model to a few observations.

The results presented in Tables 2, 4, and 6 suggest wide-ranging agreement on the robustness of income and population and cast doubt on the robustness of most other variables used in civil war models, especially when we consider the truncated period analyzed in Tables 2 and 4. The most significant differences are with respect to the impact of oil exports, ethnic heterogeneity, and war in the previous period. Overall, there seems to be agreement that the mountainous terrain variable, which is a significant variable in Collier and Hoeffler's (2001) model, is not robust to alternative measures of civil war. There is also no evidence of a robust association between civil war and the percentage of Muslims in a country. GDP growth is generally nonsignificant and may well be endogenous to levels of violence. If we restrict our analysis to the three most recent data sets—Gleditsch et al. (2001), Fearon and Laitin (2003), and Sambanis (This Study)—then we find more agreement among key variables. But when we extend the analysis back to 1945, we must also consider if there was something different about these earlier civil wars, particularly those that started before 1945 (a few civil wars are left-censored at 1945).

The significance of ethnic heterogeneity (*ef*) in some of the regressions in Table 6 is worth exploring further, given the prominence of that variable in the civil war literature. Both Fearon and Laitin (2003) and Collier and Hoeffler (2001) make strong statements against the significance of ethnic heterogeneity as a factor leading to civil war. I have shown that there is a very strong relationship between ethnic heterogeneity and an aggregate indicator of armed conflict and much less so with civil war. To the extent that violence escalates from minor to high levels, we should find ethnic fractionalization to be significant in a dynamic model of violence escalation.

But when we look at civil wars alone, why might *ef* not be significant in the Fearon and Laitin (2003) data set and analysis, given some of the results presented here? Lagging right-hand side variables makes a difference, as we saw in Table 6. Fearon and Laitin include in their analysis 10 wars that occur on the first year of the country series. Using this data and dropping those wars by lagging all explanatory variables in their replication data set brings *ef* very close to statistical significance (see online supplement). Those wars are dropped in my data set (except where I note that I replace missing observations at the start of each country series with values for the second year of the country series, as Fearon and Laitin do). For some variables (e.g., GDP), this approach to preserve observations makes sense and approximates what any imputation program would do. However, other variables, such as instability, economic growth, and anocracy, are harder to "impute" for the start of the country series, especially for new states.[73] Note also that among those dropped observations, the mean level of ethnic fractionalization for cases of war onset (using my civil war list) is higher than that for cases of no war. Thus, restoring those observations should not reduce the significance of *ef* in my data. Indeed, I followed Fearon and Laitin's approach to preserve those observations and recomputed the model in Table 6 (regressions 3, 6, 10). Ethnic fractionalization is nonsignificant in the Fearon and Laitin model, but it is still

73. In Fearon and Laitin's (2003) replication data set, for example, out of a total 966 country-years of political instability, only 4 were years of instability in a newly established state, which sounds implausible almost by definition. In fact, the instability variable, as defined by Fearon and Laitin (a greater than 2 change in the Polity scale) cannot be measured for a country's first year of independence.

significant using my data (it is marginally above the .05 level), so differences in coding rules influence the results on that variable. That said, *ef* seems very sensitive to the coding rules. Notice, for example, that by keeping 158 observations in regression 3 (by not lagging the first observation) using Gleditsch et al.'s (2001) civil war list, the coefficient for *ef* drops from 0.42 in regression 1 to 0.24. This suggests that the results on *ef* are fragile. But the question also hinges on whether we can use this arbitrary coding scheme to preserve those observations.

Another, perhaps more substantive, reason for dropping those few cases of war in the first year of the country series may be that several of them are left-censored. For example, the wars in Greece or the USSR all started before 1945, even though some data sets list them as starting on or after 1945. If violence was ongoing before that date, then it would have affected the right-hand side variables in 1945. Two of those cases—Greece and South Korea (also Philippines)—are particularly important for the nonsignificance of *ef* in the Fearon and Laitin (2003) model because they have very low *ef* scores. In sum, there may be something different about the wars that started during the few years after World War II. Most were communist insurgencies with no clear "ethnic" dimension, and the mean value of *ef* for countries at war from 1945 to 1949 was about half that of countries at war in the 1950s, 1960s, or any other decade up to 1999. Thus, losing those cases by lagging explanatory variables explains how *ef* comes close to significance using Fearon and Laitin's model and data.

I now turn to an analysis of civil war prevalence. Prevalence is defined as the union of onset and continuation of war, so the dependent variable is coded 1 for all periods of war. I assume that civil war is a first-order Markov process (i.e., there is no significant time dependence beyond the first period) and estimate a dynamic probit model by adding to the previous model a set of interaction terms between a variable denoting the prevalence of civil war in the previous period and all right-hand side variables. The estimated coefficients of the interaction terms can, after a minor adjustment, be interpreted as estimates of the relationship between the independent variables and war continuation, conditional on the occurrence of war in the previous period. The adjustment involves correcting the coefficient and standard errors of the interaction terms: for example, the coefficient of *gdpl1* with respect to war continuation is the sum of the two coefficients for *gdpl1* and *w1gdp*: (–0.124 + 0.293). The standard error is the square root of the variance of *gdpl1* plus the variance of *w1gdp* plus two times their covariance. The estimates in Table 7 are already adjusted in this way; thus, asterisks next to coefficient estimates for the interaction terms indicate significance with respect to *war continuation*. Estimates of the linear terms refer to civil war onset, as before.

The overall picture is one of substantial differences across civil war lists. Studying prevalence brings us a step closer to analyzing war duration, and it is not surprising, given the large differences in war onset and termination highlighted in Table 1a-d, that we should see differences in estimates of civil war prevalence.

GDP is again robust and negative in all regressions, although population now is not always significant with respect to war onset. Most variables (except GDP) are significant in only one or a few models, and instability is the most often significant (in 9 out of 12 models). Growth is not significant.[74]

74. Using the variable *grol1m* (instead of *grol1*) again adds only five observations and does not affect the results substantially.

(text continues on p. 853)

TABLE 7

Dynamic Probit Models of Civil War Prevalence, 1960-1993

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (All)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Expanded)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	atwar1	atwar2	atwar3	atwar4	atwar5	atwar6	atwar7	atwar8	atwar9	atwar10	atwar11	atwarns
GDP	**–0.126**	**–0.135**	**–0.098**	**–0.103**	**–0.061**	**–0.051**	**–0.100**	**–0.087**	**–0.108**	**–0.119**	**–0.120**	**–0.085**
	(0.038)	(0.042)	(0.031)	(0.034)	(0.024)	(0.017)	(0.033)	(0.031)	(0.032)	(0.034)	(0.033)	(0.028)
GDP growth	–0.425	–0.281	0.418	0.460	-1.187	–0.692	–0.307	0.720	0.106	0.121	–0.103	0.144
	(0.995)	(0.899)	(0.778)	(0.733)	(0.854)	(0.557)	(0.829)	(0.985)	(0.746)	(0.696)	(0.711)	(0.609)
Instability	**0.361**	**0.348**	0.122	*0.259*	**0.289**	**0.258**	*0.259*	**0.403**	**0.275**	**0.302**	**0.351**	**0.317**
	(0.149)	(0.136)	(0.157)	(0.154)	(0.130)	(0.106)	(0.141)	(0.155)	(0.139)	(0.121)	(0.124)	(0.140)
Anocracy	0.217	**0.241**	0.194	0.226	0.218	**0.307**	*0.262*	0.197	**0.087**	*0.235*	*0.239*	*0.231*
	(0.142)	(0.124)	(0.135)	(0.150)	(0.135)	(0.092)	(0.137)	(0.131)	(0.133)	(0.126)	(0.128)	(0.130)
Oil exporter	0.327	0.273	0.206	0.269	0.237	0.201	–0.051	**0.421**	0.215	0.152	0.230	0.119
	(0.206)	(0.178)	(0.127)	(0.172)	(0.164)	(0.156)	(0.147)	(0.187)	(0.168)	(0.153)	(0.156)	(0.129)
Ethnic frac-tionalization	0.033	0.062	0.130	0.077	0.257	**0.483**	–0.064	0.170	*0.385*	0.316	0.192	0.255
	(0.226)	(0.211)	(0.196)	(0.242)	(0.261)	(0.175)	(0.206)	(0.220)	(0.203)	(0.212)	(0.195)	(0.206)
Population (log)	0.053	0.043	**0.058**	0.038	0.054	**0.057**	**0.077**	0.028	**0.097**	**0.060**	*0.048*	0.044
	(0.033)	(0.032)	(0.029)	(0.031)	(0.034)	(0.031)	(0.032)	(0.031)	(0.038)	(0.029)	(0.029)	(0.029)
Terrain	0.004	*0.005*	**0.006**	0.003	0.003	0.002	*0.004*	0.002	0.001	0.002	0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Percentage Muslim	–0.001	0.001	*0.003*	*0.003*	0.002	**0.002**	**0.003**	0.000	0.002	0.000	0.000	0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

War(t – 1)												
• Muslim	–0.005	–0.003	–0.001	*–0.007*	–0.002	0.000	–0.003	0.001	0.002	–0.001	–0.002	0.000
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
War(t – 1)												
• GDP	0.169	0.147	0.033	*0.201*	**–0.197**	0.029	0.082	–0.003	*0.102*	**0.197**	**0.156**	0.055
	(0.139)	(0.110)	(0.099)	(0.111)	(0.066)	(0.053)	(0.068)	(0.058)	(0.055)	(0.069)	(0.067)	(0.048)
War(t – 1)												
• Growth	–0.021	–0.369	0.198	*1.395*	–0.451	*1.036*	1.200	0.419	0.240	–0.892	0.774	0.752
	(0.745)	(0.615)	(0.653)	(0.838)	(0.828)	(0.644)	(1.148)	(0.592)	(0.819)	(0.653)	(0.871)	(0.849)
War(t – 1)												
• Instability	**–0.533**	*–0.371*	–0.266	–0.164	–0.172	–0.152	–0.256	0.158	**–0.545**	–0.256	–0.209	–0.199
	(0.241)	(0.223)	(0.211)	(0.217)	(0.190)	(0.179)	(0.191)	(0.173)	(0.193)	(0.176)	(0.190)	(0.184)
War(t – 1)												
• Anocracy	0.077	0.144	*0.339*	0.115	*0.336*	0.019	0.112	**0.405**	0.086	0.169	0.078	0.133
	(0.238)	(0.215)	(0.193)	(0.194)	(0.175)	(0.189)	(0.200)	(0.154)	(0.242)	(0.173)	(0.176)	(0.182)
War(t – 1)												
• Oil	*–1.001*	–0.643	–0.508	–0.286	–0.312	–0.283	–0.088	–0.198	*–0.614*	–0.520	–0.603	–0.328
	(0.555)	(0.514)	(0.421)	(0.383)	(0.436)	(0.334)	(0.354)	(0.367)	(0.386)	(0.381)	(0.397)	(0.364)
War(t – 1)												
• Ethnic	**1.561**	**1.379**	0.761	1.001	**1.233**	0.306	**1.330**	*0.562*	0.382	0.600	0.608	0.377
	(0.717)	(0.587)	(0.541)	(0.772)	(0.484)	(0.443)	(0.553)	(0.370)	(0.462)	(0.474)	(0.460)	(0.494)
War(t – 1)												
• Population	**0.220**	**0.200**	**0.253**	**0.209**	**0.221**	**0.244**	**0.257**	**0.195**	**0.289**	**0.229**	**0.228**	**0.242**
	(0.043)	(0.043)	(0.039)	(0.045)	(0.040)	(0.036)	(0.041)	(0.030)	(0.039)	(0.036)	(0.036)	(0.036)
War(t – 1)												
• Terrain	0.000	0.000	0.000	0.008	0.005	0.005	0.002	0.004	0.006	0.005	0.001	0.003
	(0.006)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)

(continued)

TABLE 7 (continued)

	COW 1994	*COW 2000*	*Collier and Hoeffler (2001)*	*Licklider (1995)*	*Gleditsch et al. (2001) (Wars)*	*Gleditsch et al. (2001) (All)*	*Fearon and Laitin (2003)*	*Leitenberg (2001)*	*Regan (1996)*	*Doyle and Sambanis (2000, Expanded)*	*Doyle and Sambanis (2000)*	*Sambanis (This Study)*
	atwar1	atwar2	atwar3	atwar4	atwar5	atwar6	atwar7	atwar8	atwar9	atwar10	atwar11	atwarns
_cons	**–3.016**	**–2.842**	**–3.245**	**–2.898**	**–3.201**	**–3.095**	**–3.381**	**–2.683**	**–3.673**	**–3.026**	**–2.838**	**–2.873**
	(0.573)	(0.548)	(0.486)	(0.551)	(0.550)	(0.512)	(0.549)	(0.525)	(0.617)	(0.486)	(0.487)	(0.497)
Observations	4,045	4,179	4,099	4,178	4,179	4,179	4,179	3,782	4,179	4,179	4,179	4,179
Wald χ^2	1,050.19	1,169.87	1,270.07	952.18	988.45	1,037.77	1,240.66	1,701.62	851.45	1,147.56	1,139.25	1,598.91
Log likelihood	–337.62	–398.73	–375.13	–366.71	–431.94	–687.92	–371.13	–417.82	–483.08	–496.85	–476.10	–474.73
Pseudo-R^2	0.6996	0.6834	0.7199	0.7465	0.6134	0.6510	0.7887	0.7015	0.75	0.7201	0.7248	0.7308

NOTE: Coefficients (standard errors) are presented. Bold indicates significance at .05 or higher; italics indicates significance at .10. Coefficients and standard errors for all interaction terms have been adjusted to refer to war continuation only. GDP = gross domestic product.

The picture becomes much more unstable when we consider results with respect to war continuation in the shaded portion of Table 7. Here we see that the interaction between income and lagged war prevalence is often nonsignificant, but occasionally it is significant with switching signs: in Gleditsch et al.'s (2001) civil war list (*atwar5*), high income significantly reduces war continuation, whereas in the two models based on Doyle and Sambanis's (2000) data (*atwar10*, *atwar11*), higher income once a civil war is ongoing has the effect of increasing the risk of the war continuing. In Regan's (1996) and Licklider's (1995) data, the same relationship is borderline significant (at .10).

Instability while war is ongoing significantly reduces the risk of war continuation in two models (Singer and Small 1994; Regan 1996) and is nonsignificant in the rest.[75] Similarly, anocracy is significant in one model and borderline in another two but otherwise nonsignificant. Oil exports are generally nonsignificant. There is wide agreement that populous countries will have longer wars and no evidence that countries with significant Muslim populations or mountainous terrain will have longer civil wars.

Interesting results emerge again with respect to ethnic fractionalization (*ef*). The interaction term with *ef* is significant and positively correlated with war continuation in four regressions (e.g., in regression 7, using Fearon and Laitin's 2003 civil war list) and close to significant in one more. Thus, the results with respect to this variable are genuinely divided: about half the models would tell us that a very diverse country would have long wars, once a war actually started, whereas the other half would point to no statistically significant relationship between ethnicity and war duration. There is more support for the positive association between ethnic fractionalization and war continuation in Table 8, where I present prevalence results from the four civil war lists that span the entire period from 1945 to 1999, restricting the analysis to four civil war lists. Here, we see the same sort of substantial disagreement with respect to the effects of income on war continuation: in one model, the coefficient is significant and negative; in another, it is significant and positive; and in the other two, it is nonsignificant.[76] The more we push the data, the more the differences in coding rules will matter.

CONCLUSION

This article does three things. First, it demonstrates that there are substantial differences across civil war lists with respect to the coding of the onset and termination of civil war. Exploring those differences analytically reveals some conceptual confusion

75. I am not concerned here with the theoretical explanation of these results. How instability can reduce war duration is unclear. It may be that a regime transition toward significant democratization (which would be coded as instability) satisfies some of the rebels' demands and leads to war termination. The coding of the instability variable may be affected by the occurrence of war, but it is not my purpose here to resolve these questions of endogeneity.

76. The differences actually become greater if we do not lag the first observations in each country series (giving 6,051 observations). With respect to onset, growth is significant and negative in two out of four regressions, instability is significant in one regression, anocracy is significant in all regressions, oil exports are significant in one regression, and population is significant in one regression. With respect to continuation, there is no significant change from the differences noted in Table 8 (see the results in the online supplement).

TABLE 8
Dynamic Probit Models of Civil War Prevalence, 1945-1999

	Gleditsch et al. (2001) (Wars)	*Fearon and Laitin (2003)*	*Doyle and Sambanis (2000, Expanded)*	*Sambanis (This Study)*
GDP	**–0.073**	**–0.095**	**–0.102**	**–0.085**
	(0.025)	(0.027)	(0.026)	(0.025)
GDP growth	–1.006	–0.204	–0.187	–0.113
	(0.626)	(0.473)	(0.411)	(0.392)
Instability	**0.273**	**0.243**	**0.219**	**0.213**
	(0.108)	(0.108)	(0.103)	(0.109)
Anocracy	0.172	**0.291**	**0.274**	**0.294**
	(0.112)	(0.114)	(0.115)	(0.099)
Oil exporter	*0.276*	0.067	0.148	0.148
	(0.146)	(0.151)	(0.147)	(0.130)
Ethnic fractionalization	0.220	0.115	0.295	0.221
	(0.218)	(0.192)	(0.199)	(0.200)
Population (log)	**0.055**	**0.078**	0.034	*0.043*
	(0.027)	(0.027)	(0.026)	(0.024)
Terrain	0.003	0.003	0.001	0.002
	(0.002)	(0.002)	(0.003)	(0.003)
Percentage Muslim	0.001	0.002	0.000	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
War(t – 1) • GDP	**–0.153**	0.042	**0.111**	0.022
	(0.073)	(0.048)	(0.041)	(0.026)
War(t – 1) • Growth	0.108	0.288	–0.399	0.580
	(0.615)	(0.791)	(0.495)	(0.661)
War(t – 1) • Instability	–0.108	*–0.309*	–0.169	–0.040
	(0.184)	(0.178)	(0.153)	(0.166)
War(t – 1) • Anocracy	0.319	0.054	0.159	0.021
	(0.196)	(0.200)	(0.146)	(0.177)
War(t – 1) • Oil	–0.103	–0.042	–0.228	–0.087
	(0.323)	(0.291)	(0.302)	(0.284)
War(t – 1) • Ethnic	**1.004**	**1.052**	**0.700**	*0.639*
	(0.417)	(0.349)	(0.303)	(0.353)
War(t – 1) • Population	**0.211**	**0.265**	**0.197**	**0.226**
	(0.035)	(0.032)	(0.029)	(0.032)
War(t – 1) • Terrain	0.004	0.003	0.002	0.003
	(0.005)	(0.003)	(0.003)	(0.003)
War(t – 1) • Muslim	–0.001	–0.002	0.000	0.000
	(0.003)	(0.002)	(0.003)	(0.002)
_cons	**–3.159**	**–3.501**	**–2.689**	**–2.880**
	(0.436)	(0.448)	(0.429)	(0.412)
Observations	5,893	5,893	5,893	5,893
Wald χ^2	791.89	1,736.07	1,898.86	2,022.78
Log likelihood	–596.74	–548.11	–694.25	–671.34
Pseudo-R^2	0.5797	0.7731	0.6963	0.7185

NOTE: Coefficients (standard errors) are presented. Bold indicates significance at .05 or higher; italics indicates significance at .10. Parameters for all interaction terms have been adjusted to refer to war continuation only. GDP = gross domestic product.

about the meaning of civil war and can serve as the foundation for a theoretical discussion that establishes what civil war is and how it can be distinguished from other forms of political violence. Second, it proposes a new coding rule for civil war that attempts to sidestep some of the problems identified in other coding rules and offers a new list of civil wars that is based on this new coding rule. Third, it measures the substantive implications of differences in coding rules by formally comparing the empirical results that we get from a civil war model when we use 12 different rules to code civil war.

The quantitative literature on civil war reveals a remarkable degree of disagreement on how to code the onset and termination of wars, and the literature is fuzzy on how to distinguish among different forms of political violence. This implies the need for theorizing about civil war and then for proper measurement of the concept. Given the coding complexities I have identified, researchers should conduct robustness tests using different civil war lists and justify their coding decisions as transparently as possible. Differences in the coding of civil war have substantive implications, but perhaps not as large as one might have expected. The results presented here show that the estimated coefficients of most variables vary widely as a result of changes in the coded onset of civil war. In a few cases, the sign of the coefficient also changes, and significance levels also vary across data sets. Several variables are not robust to changes in the coding of civil war onset, most strikingly in the case of lagged war prevalence, ethnic fractionalization and oil exports, and, to a much lesser extent, anocracy and instability.

Predictions of when and where civil war might occur are likely to depend critically on which coding rule we use. More important, estimates of the substantive effects of policy interventions to reduce the risk of civil war by manipulating the level of income, political instability, or any of the other "manipulable" variables in the model will also vary widely, depending on the coding rule for war onset and termination.

At the same time, some variables are remarkably robust to coding differences. Income level and population size are very robust and significantly associated with civil war. That significance levels for these variables do not change much despite large differences in coding rules is likely because these variables change slowly over time. A problem that this highlights is that we cannot rely on them to make accurate predictions of the timing of war onset. Other variables—especially mountainous terrain, Muslim population, and economic growth—are consistently nonsignificant (with minor exceptions).

The results from models of war prevalence suggest that predictions of civil war duration will be even less accurate than predictions of civil war onset. There was greater instability of empirical results in the prevalence model, so analyses of civil war duration will be much more affected by differences in the coding rules. Robustness tests are therefore essential when analyzing war duration or termination.

Overall, civil war models seem to be good at identifying countries with long-term proclivities to civil war. But if the models are further developed to be able to predict the timing of war onset better, then we are likely to see coding differences in the dependent variable affect the parameter estimates more significantly (the changes we found in the coefficient sign of economic growth, which changes substantially over time, are an indication of this). Thus, differences in coding will begin to matter if the theoretical models "catch up" to the data by including more time-sensitive variables. As the theory develops more, it seems likely that analysts of civil wars will have to go back to the data to develop a higher degree of consensus on the meaning of civil war before they can produce accurate and credible predictions of where and when civil wars will occur.

That said, I have no clear answer on whether it is better to have a single definition or coding rule for civil war or if it is more beneficial to have differing definitions. On one hand, some standardization of our coding rules would reflect theoretical consensus on what civil war is, and homogeneity in measurement would rule out one possible source of difference in empirical results. On the other hand, that we have many coding rules suggests that the concept of civil war may mean different things to different people, and coding rules should reflect differences in subjective understandings of that concept. Analyzing the differences across coding rules can be instructive because these differences map out the space within which we might be able to find a shared understanding of civil war.

A substantive result from this analysis, and to which I must return, is that ethnic fractionalization may not be as nonsignificant a correlate of civil war as many scholars have argued. Its significance hinges perhaps too much on the coding rules for civil war, but ethnic fractionalization is clearly important in explaining a broad category of armed conflict that includes minor insurgency. This is an important result, considering how difficult it was to clearly distinguish between civil war and other forms and levels of violence. Ethnic fractionalization was sometimes significantly correlated to both war onset and war continuation, but these results were at times affected by the few wars that are dropped when we lag explanatory variables by 1 year. However, differences in coding rules seem to explain the nonsignificance of that variable in Fearon and Laitin's (2003) results. In other runs (see supplement), religious fractionalization and the size of the largest confession were significant and often dominated the effect of ethnic fractionalization.[77] Thus, ethnoreligious identity may have been written off too quickly as a correlate of large-scale armed conflict—even "civil war"—by many scholars.

Several other substantive results are worth noting. Economic growth may be endogenous to civil war, which implies the need for a different estimation strategy for models that include that variable. The political variables of anocracy and instability, which are central to some models of civil war, are sensitive to the coding rules but much less so if we examine the entire post-1945 period. The significance of the population variable may be tied to the high threshold of violence used to identify civil war and distinguish it from minor violence. Mountainous terrain—a key measure of the technology of insurgency in civil war in some models—is not robust to changes in the coding rules, and neither is the measure used to identify countries that are major exporters of oil. Peace duration or war in the previous year is also sensitive to changes in coding. Moreover, the significance of some of these variables is affected by whether observations of ongoing war are dropped (e.g., anocracy is much more consistently significant in Table 4 than in Table 2). Finally, the period analyzed influences some

77. For an analysis of the sensitivity of empirical results in the civil war literature to small changes in the specification of the model, see Hegre and Sambanis (2004).

results due to the small number of war starts: for example, instability is more consistently significant in Table 6 than in Table 4.

Despite these differences and difficulties, the conclusion from this study should not be that coding wars and analyzing them quantitatively is a futile exercise. Rather than abandon these efforts, I favor redoubling them by improving the coding rules, applying them transparently to the data, and studying the implications of differences across coding rules. The legacy of the Correlates of War project is that we now have at our disposal replicable data on civil war that we can analyze quantitatively to point to the theoretical and empirical complexities of defining and measuring civil war. This article suggests ways of building on that legacy.

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