

Kalyvas, S. N. (2006). *The Logic of Violence in Civil War*. Cambridge, UK: Cambridge University Press.

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Stathis Kalyvas's *The Logic of Violence in Civil War* addresses four interrelated questions: What explains the variation of violence in civil war? Why are civil wars so demonstrably brutal? What is the causal direction between preexisting loyalties and violence? And how do macro-level causes of the war relate to the pattern of violence on the ground? Kalyvas's answers spring from two related key intuitions based on fieldwork in Greece: "First, local dynamics [are] of fundamental importance and, second, violence [appears] to be less the result of powerful political identities and deep divisions and more their cause" (pp. 14-15). Thus, in line with the current understanding of cleavage identities as fluid and context dependent, Kalyvas calls into question the conventional wisdom that the causal arrow in coercive violent conflict runs from preconflict social divisions to violence. Instead, he proposes a theory of selective violence; a privatization of the politics of civil war through the role of denunciation motivated by personal conflicts.

Kalyvas argues that civil wars are unconventional or irregular wars, which alter the nature of sovereignty in a fundamental way. In an irregular war, territorial sovereignty varies by zones between two rival actors, and boundaries between controlled and twilight areas are blurred and fluid. Each actor's objective is to establish control over contested areas, and as the war evolves, control determines the level of collaboration by the population. Acquisition of control, in turn, is determined by military effectiveness, which itself is undermined by the cost and chaos of civil war. The question in civil war, thus, becomes how to use available military capacity for coercion to the maximum advantage.

Kalyvas posits that political actors have two coercive options. The first—selective violence—personalizes targeting. Actors' abilities to use selective violence are limited, however, by the requirement of collaboration, which, in turn, requires some control. Alternatively, authorities can use indiscriminate violence, which involves collective targeting. Kalyvas argues that random violence cannot reliably induce compliance and may be counterproductive, because it does not provide a clear structure of incentives for noncollaboration and may even produce incentives for defection to insurgents. The exception is when there is a significant imbalance of power between the two actors and the gathering of information for selective violence is costly.

In turn, although many innocent people suffer because of the difficulty and cost in verifying collaborators' information, selective violence is a highly effective deterrent strategy, because it creates the perception that political actors can monitor and sanction behavior. There are many motivations for denunciations (including the personal), but their supply is restricted by the likelihood of retaliation. Building on these ideas, Kalyvas develops a game theoretic argument on the probability of denunciations with

respect to civilians' association with a particular political actor and that political actor's relative control of territory, with the objective of predicting the political actor's use of selective violence. In sum, the equilibria suggest that the number of denunciations of fellow civilians has a concave curvilinear relationship with a change from incumbent to insurgent control of territory. Defections to the camp of either political actor, in turn, are positively associated with that actor's control of territory. Consequently, taking into account the costs and benefits of using violence, Kalyvas conjectures that political actors have an incentive to use selective (homicidal) violence only in areas where they have secure but incomplete control to deter potential defectors and consolidate their control of the area. Conversely, indiscriminate violence tends to be used where one political actor is hegemonic and is perpetrated almost exclusively by the actor that has no control in the area; it is surprising that there will be little or no violence of either kind in zones of control parity.

It is these theoretical insights that constitute the heart of the book. For example, as correctly pointed out by the author, these predictions contradict the multitude of theories suggesting that violence is most intense in the most contested areas. Furthermore, this argument is capable of explaining and predicting many of the previously puzzling spatial and temporal variations in civil war violence. Clearly, the model simplifies reality, and Kalyvas articulates some reservations about this. In particular, he points to the problems in the assumption of individual ability to evaluate risk and reduction of the reality of a civil war to a two-actor interaction. Kalyvas's model makes other assumptions that readers will quibble with. Examples include the assumptions that political actors are naturally reluctant to endogenize revenge cycles and that potential civilian denouncers have an exclusive relationship with either incumbent or insurgent organizations despite operating in an environment where these organizations compete for public loyalties. The lack of political actor response following a high number of denunciations in a fully controlled zone (p. 222) might have other explanations than the political actor's simply deciding to disregard these denunciations. For example, due to defections, all civilians initially associated with either side may have sufficient access to a secure political actor in controlled zones to credibly counter accusations. Similarly, subsequent evidence about fence-sitting and dual identities in contested areas suggest that the assumption of exclusive association is troublesome, particularly alongside the prediction of increasing defection to the winning side in increasingly controlled areas. In my opinion, however, contrary to there being a flaw in the model, these contestable assumptions point to possible fruitful venues of extension made possible by the very fact that the form selected makes the chosen assumptions transparent.

The theory of selective violence speaks to spatial variation in the level and type of violence resulting from changes in control by political actors in subnational units. Showing the generalizability of this theory, Kalyvas first surveys anecdotal evidence from subnational units in civil wars throughout the world. Second, he subjects the predictions of the theory to rigorous qualitative and quantitative tests against data collected in interviews, judicial archives, local histories, ethnographies, agricultural

studies, research articles, and from secondary sources in one country—Greece. The first set of tests focus on the region of Argolid in southern Greece. The second set of tests use data from across the country.

Although the internal conflict in Greece involved a foreign occupation, the author makes a convincing case for why this conflict was a civil war. By and large, the results of the analysis of the two phases of the war in Argolid support the hypotheses about the type and level of violence used by incumbents and insurgents as a result of change in their control over territory. Furthermore, the analysis attempts to resolve the question of endogeneity while controlling for other factors. The results question conventional wisdoms such as beliefs about the distribution of violence between incumbents and insurgents, and the thick description accompanying the multivariate analysis casts light on local paradoxes such as why entire villages joined the Germans in 1944 when the war was being won by the Allies. Importantly, the qualitative evaluation of the predictions of the theory also highlights mispredictions and allows the author to speculate about their causes that may include positive reciprocity and revenge. The second set of tests examines the accuracy of the predictions in the region of Almopia, and finally, across a number of regions in Greece. The results are largely consistent with various predictions of the theory but also highlight the distinctive features of Argolid and draw attention to factors that were not as evident in the regional analysis.

Overall, this analysis is extremely well executed. Some points could, however, be further explicated to the advantage of future studies. For example, Kalyvas defines *civil war* as “armed combat within the boundaries of a recognized sovereign entity between parties subject to a common authority at the outset of the hostilities” (p. 17). According to this definition, however, every insurgent and terrorist group that perpetrates violence against the state—and thereby rejects the initial common authority monopoly on violence—is engaged in a civil war. Kalyvas clarifies that civil war “entails a de facto territorial division” (p. 18) but what this territorial division precisely entails is not explained further. Thus, when the author specifically discusses control (pp. 210-245), it theoretically and anecdotally ranges from none to full for both actors. Similarly, the different types of conflict discussed in the context of the privatization of the political presumably all qualify as civil wars. The precise onset of civil war is important, because in discussing what the author calls the “pathologies” in the preceding study of civil war, he posits that “war is a social and political environment fundamentally different from peace” (p. 38). Consequently, he argues that the treatment of civil war by many authors as “just a different kind of electoral process” (p. 38) is fundamentally mistaken. Although incentive structures in war unarguably differ from incentive structures in peace, it would aid future analysis of conflict situations if the author had helped operationalize this transition. To put my questions differently: When precisely does an insurgency become a civil war? And when do incentive structures change? Without such operationalization, it is difficult to surmise when Kalyvas’s theory will better predict patterns of violence than, for instance, a theory of electoral incentives (Wilkinson, 2004).

In sum, this book combines much of the best that comparative politics has to offer: conceptually clear and rigorous theorizing based on insights from extensive field work, and tested in a methodologically solid fashion on a wealth of quantitative and qualitative data. The conclusions constitute a major contribution to our understanding of violence in civil conflict. The theory generates a multitude of implications—only some of which the author has tested—and invites extensions that themselves will generate multiple testable implications. The subject matter is fascinating, and the author’s encyclopedic treatment of anecdotes from the literature on civil wars in general and a detailed history of one such war, makes the book a highly enjoyable read. In addition, the meticulous construction of the theory and explicit objectives laid out as reasons for the structure of the empirical analysis in addition to the detailed appendices will endear the book to students and instructors of the comparative method.

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Reference

- Wilkinson, S. (2004). *Votes and violence: Electoral competition and ethnic riots in India*. Cambridge, UK: Cambridge University Press.