What do We Know as a Field about the Causal Determinants of Ethnic Conflict?

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What do We Know as a Field about the Causal Determinants of Ethnic Conflict?

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ABSTRACT This essay makes a provocative case against the study of ‘ethnic conflict’ as a special subset of political conflict. It pushes against the ‘silo-ing’ of conflict studies that exists in which ‘specialists’ study ‘civil war’, ‘counter-insurgency’, ‘ethnic conflict’, ‘terrorism’, etc. It calls for theories and hypothesis tests drawn from frameworks of political conflict that can explain everything from quiescence that is seen in contemporary Syria.

My reaction to the study of ‘ethnic’ conflict is that it is generally a bad idea. That is, I am concerned that despite the widespread recognition that ethnic identity is socially constructed, our continued use of the concept belies an essentialist approach that is ultimately counterproductive. Put plainly, by studying ethnic conflict as a ‘thing apart’ we will obscure more than we will illuminate. Why?

Let us begin generally and ask: ‘Why and how does political conflict among humans occur?’ The most simple answers are: over a difference of interests, and via the collective use of coercion by one group against another. Studies of human conflict thus must (implicitly or explicitly) address two issues: (1) what interests divide groups of people and (2) are both groups willing to mobilize to defend/press their claims? To those familiar with the literature circa the 1970s it will be apparent that I am strongly influenced by the work of both Ted Robert Gurr (who emphasized conflict of interest) and Charles Tilly (who emphasized collective action). This position is valuable because of its claim to generality: quiescence induced by repression can be explained, as can harmony (if it exists), as well as the full range of observed dissident >= state coercion between those two poles.

I am unaware of alternative theoretical first principles that can also account for the full scope of human political conflict. When we focus our attention on a subset of mobilized conflict, such as ethnic conflict, we draw attention away from the full range of human political conflict, and that is more likely to be counterproductive than productive.

Is it necessarily harmful to argue that there is a subset of human conflict that is mobilized over an ethnic cleavage? I argue that it is unless we explicitly note that we are...
studying such a subset, and include a positive (rather than implicit) statement explaining why doing so is intellectually advantageous. Yet, to focus on ethnic conflict is even more likely to be counterproductive than will be a focus on, say class conflict, or partisan conflict. Why would this be so? The difficulty is the prevalence of Manichaean stories across human societies coupled with the tendency of politicians, historians, reporters, and pundits to latch on to the ‘ancient hatreds’ trope to explain mobilization across an ethnic cleavage (see Moore, 2015; and chapter 8 of Pinker, 2011 for discussion). Were we to conduct our research absent these two historical realities, then the ethnic category would be no more a problem than, say class, or partisanship. But we cannot conduct our research outside of that reality.

What Has Been the Most Useful Development Within This Literature over Time and More Recently and Why?

The most valuable development has been the growth of the constructivist challenge to the so-called primordial approach to understanding ethnic conflict. More specifically, forthcoming work from some of the people working with the Minorities at Risk project (i.e., Birnir et al., 2016; Birnir, Fearon, Laitin, Wilkenfeld, & Gurr, 2015) is addressing the heretofore unexamined problem of latent versus mobilized groups. This work moves us closer to viewing ethnic cleavages as one of the many potential cleavages over which mobilization might take place.

As valuable as this has been, however, see my response to the first question for an argument about why this development is not fully satisfactory.

What Has Been the Least Useful Development Within This Literature over Time and Recently and Why?

Its continued existence is a stand-alone literature. During the early 1990s, when the constructivist approach to understanding ethnic conflict was clearly more than a little blip I imagined that it would be akin to Huntington’s (1968) book and the development school of comparative politics: I thought it would be the death of the study of ethnic conflict. Then scholarly reaction to the dissolution of Yugoslavia demonstrated to me the wisdom of Geddes’s (2003) Paradigms and sand castles essay, in which she argues that waves of Ph.D. theses (and their subsequent articles and books) that do empirical work driven by news headlines would wash away grand theories, setting the stage for grand theories to be erected to explain whatever came next in the news, which would then be followed by waves of empirically grounded Ph.D. work, etc. As such, I do not have any specific development to decry, but rather the entire field’s resurgence. This connects directly to my claim above about Manichaean stories: scholars reacted with frustration to the ‘ancient hatreds’ claims in news accounts, etc., yet rather than grounding the work broadly within the study of human conflict, they accepted the claim that ethnic conflict could be studied fruitfully as a thing apart and went to work explaining how the ancient hatreds account were unhelpful, misleading, etc. In so doing, however, they unwittingly and unintentionally reify the idea that ethnic conflict is fundamentally different from mobilized conflict over other cleavages, thus separating it from conflict processes generally.
What Remains to be Done or, if You Had 5 Million Euro to Engage in a Project, What Would You Do? What Is the Theory You Would Explore and Method and Sources You Would Employ?

I would spend the resources on the study of mobilization: what explains the cleavages over which mobilization takes place? To answer that question I recommend that we turn to the broad frameworks advanced by Gurr (1970) and Tilly (1978). Doing so would reduce the ‘silo’ problem in the field where ‘ethnic conflict’, ‘civil war’, ‘terror(ism)’, ‘state repression’, ‘non-violent protest’, and other ‘forms’ of political conflict are artificially separated from one another and studies in isolation (and apart from ‘routine’ political competition). The primary problem of ‘specialists’ who study certain ‘forms’ of political conflict is that it marginalizes the field, obscuring the centrality of coercion to the study of politics. The second, and important, shortcoming is that the field produces knowledge of ears, trunks, legs, and tails of the elephant as if there was no elephant. The problem, then, is not the existence of ‘ethnic conflict’ (or ‘civil war’, etc.) studies, but the dramatic imbalance between the number of researchers who study ‘forms’ of conflict in isolation from the whole and the few who pay attention to the existence of a whole.

Though a critic might argue that I am calling for grand theory at the expense of mid-range theory, I wish to challenge such a critique. My claim is that we do our research within a social context, and that our theories compete with non-theories and non-scientific claims of politicians, reporters, historians, etc. When our inquiry is about human conflict we will do well to consider the extent to which our work challenges misunderstandings that assist mobilization. One of the challenges of the ancient hatreds claim (and similar tropes) is that it operates with a self-fulfilling prophecy logic: if the modal person in a population sub-group believes that members of an out-group will mobilize along ascriptive, cultural, racial, and/or religious characteristic X, then the security dilemma logic kicks in and it is irrelevant whether that modal person identifies with characteristic X. She has an incentive to behave as if she identifies that way, and thus it is conceivable that mobilization along ethnic lines can occur in a situation where not even the ethnic entrepreneur identifies strongly with characteristic X. I am pessimistic that we can produce useful theory and evidence to illuminate this, and thus effectively counter the claims to ancient hatreds, etc., if we develop theories of ethnic conflict as a type of conflict apart from human conflict mobilized over other cleavages.

Ethnic conflict research, then, should proceed only to the extent that it explicitly places itself within a more general account of mobilization and in which it addresses why mobilization over an ethnic cleavage, rather than alternatives, occur.

Turning to methods, though I employ the research design and statistical methods in which I have developed some expertise, I am genuinely agnostic. Indeed, while I limit my research agenda to causal explanation, I have learned from scholars writing from critical and other ‘non-causal account’ perspectives. That said, I will focus on causal explanatory theory, and it seems to me that time-series case studies—both the observational type that relies upon multiple regression and natural experiments that seek to leverage ‘as if’ randomization to draw inferences (Dunning, 2012)—are dramatically underexplored. We have a strong penchant for cross-sectional comparison, even within the turn toward sub-national analyses. That turn is most welcome, as is the additional attention to the importance of spatial location. Yet time remains largely left out in the cold, and this is a weakness that we will do well to address.
Disclosure Statement

No potential conflict of interest was reported by the author.

Note

1. By ‘ethnic conflict’ I am referring to the panoply of collective political conflict where a major cleavage over which the dissidents and state involved is ‘ethnic’ (i.e. cultural, linguistic, national origin, racial, and/or religious). My usage thus combines ‘ethno political conflict,’ ‘ethno nationalist conflict’, and other distinguishable sub-categories into a single category.

References


