

## Response to Critiques and Avenues for Future Research

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**Mobilization and Conflict in Multiethnic States**, by Manuel Vogt, Oxford University Press, 2019, 296 pp., £59.00 (hardcover), ISBN 9780190065874.

I would like to start my response by expressing my profound gratitude to the three commentators for their in-depth engagement with my book, their generous comments, and the rich variety of thought-provoking challenges and critiques. Their contributions urge me to refine the theoretical and empirical implications of the book in novel ways. Responding to all of their excellent observations would go beyond the scope of this short essay, but I will address what I identify as the most fundamental arguments, clustered in three main themes, according to which I will structure my response. The first part will address the comments of the reviewers that relate to key definitions and case classifications. The second part focuses on challenges to the theoretical argument, including alternative explanations. Finally, the third part addresses unresolved questions in the book identified by the reviewers, which open up promising avenues for future research.

First, the reviewers raise questions about both the definition of ethnicity and the classification of colonial settler states. With respect to ethnicity, Şener Aktürk argues that my definition conflates ethnicity with other social categories, especially religion, which “runs counter to the Weberian definition of ethnicity as subjective belief in common descent or blood kinship” (2), and that Islamic and Christian political movements are usually supraethnic. I would argue that my definition of ethnicity is in line with the one used in the majority of the civil war and ethnic politics literatures, going back to Horowitz (1985) and Gurr (Gurr et al. 1993), which identifies a variety of supposedly innate human traits as possible visible foundations of a group’s subjective belief in common descent, including language, religion, and phenotypical features (often summarized in the term “race”) (see, e.g., Horowitz 1985, 51–52). Thus, while politicized religious identity can indeed be supranational (as in contemporary transnational Islamist groups), in many instances, religion takes on a decidedly bounded and locally grounded ethnic character, with members of religious groups attached to each other by a subjective belief in common descent very much in the Weberian sense. This certainly seems to be true for Palestinians in Israel, Protestants and Catholics in Northern Ireland, and many other groups considered ethnic (i.e., ethno-religious) groups in the book. (Few people would probably disagree with the description of the Israel-Palestine and Northern Ireland conflicts as *ethno-religious*.) And while ethno-religious groups often extend beyond national borders, so do ethno-linguistic groups, such as the Kurds or the Basques.

With respect to the definition of colonial settler states, both Killian Clarke and Aktürk question the book’s classification of Israel. It is of course true that the very birth of Israel as a state was based on the immigration of Jewish settlers into a territory predominantly inhabited by Arab Muslims. Moreover, this state became independent under the political, economic, and cultural dominance of these settlers. The reason why the book does not count Israel as a colonial settler state is that Jewish settlement mostly occurred in the context of colonial rule by *another* colonizer: the British Empire,

whereas those states classified in the book as colonial settler states experienced colonial settlement (overwhelmingly) by the actual colonial rulers themselves.

This subtle distinction is crucial – not only because the British colonizers left, but also because it shaped the type of ethnic cleavage between the non-British groups that stayed in the former colony *after* independence. It is probably fair to say that the British rulers exploited the Jewish-Arab division, playing one group against the other, in a very similar way as in what I term “decolonized states” such as Sri Lanka or Uganda. Thus, once the British colonizers left, Arab-Jewish group relations in Israel arguably resembled more closely the ethnic cleavage type of decolonized states than that of the countries classified as colonial settler states in the book, such as Guatemala. Still, while the key empirical findings remain unaffected by the classification of Israel, the book could have indeed discussed the implications of its theoretical argument for this crucial case in more depth.

While the case of Israel might constitute a somewhat idiosyncratic phenomenon, the example of Algeria, which Clarke and Aktürk also refer to, raises a more systematic issue with the book’s definition of colonial settler states. Algeria experienced relatively extensive European (in this case, French) settlement – and more extensive than was typically the case in what the book defines as “decolonized states” – yet became independent under the self-rule of the formerly colonized people. (Kenya might be another example on the British side.) As Clarke points out, in such cases as Algeria (as well as Ireland), violent resistance by the subordinated groups did occur as part of the independence struggle. (The Haitian revolution is a particular case of slave emancipation.) Thus, they contradict my argument that, as stratified societies, colonial settler states are more likely to experience peaceful than violent mobilization by historically marginalized groups – but because my very definition of settler colonies excludes cases where anti-colonial resistance by the indigenous population was successful, these instances of violent resistance are not picked up in the book’s analysis.

The book’s relatively strict definition of colonial settler states – states that became independent under the continuing rule of the European settler colonizers (or their descendants) – has the advantage that it entails relatively objective classification criteria, without the need for arbitrary thresholds of the degree of settlement and/or debatable decisions about how far back in time one goes to identify colonial settlement. And while the cases of Algeria and Ireland would indeed seem to contradict the book’s argument, the existence of counter-examples as such does, of course, not undermine the general validity of a probabilistic argument. (The book also discusses other exceptions, e.g., in Latin America.) Moreover, to the extent that the colonized indigenous people adopted the languages and religions of their colonial oppressors, *all* European overseas colonies constituted stratified societies *during colonial rule*. Thus, a comprehensive consideration of this issue would require a systematic analysis of all anti-colonial movements in those European overseas colonies that are classified as decolonized states in the book, rather than just the violent cases. In other words, all periods of anti-colonial mobilization *before* independence of decolonized states would need to be counted as observations that fall into the category of colonial settler states. Interestingly, only fourteen of the book’s sample of decolonized states (less than a fourth) achieved independence by (mainly) violent means. This seems to be in line with the book’s fundamental argument about ethnic mobilization in stratified societies. Hence, although such an analysis is beyond the scope of the book, its theoretical argument could in principle be applied to anti-colonial movements within the realm of European overseas colonialism.

Second, the commentators raise a number of thoughtful challenges to the book’s theoretical argument, highlighting other important factors that could explain the observed patterns of violent or non-violent ethnic mobilization. Both Belgin San Akca and Aktürk point to the important role of the international environment and external support for non-state actors in conflict escalation, albeit in slightly different ways. While San Akca suggests that the lack of external support from third-party states could explain the observed differences in armed conflict between colonial settler states and decolonized states, Aktürk acknowledges that “the absence of an ethnic kin state” (60) in the case of historically marginalized groups in the colonial settler states is part of the book’s argument about distinct opportunity structures. Yet, he emphasizes that foreign support can emanate from other

sources than ethnic kinship. He argues that geopolitics and, in particular, US hegemonic influence in Latin America are responsible for the dearth of armed ethnic rebellions in the colonial settler states category.

I am somewhat skeptical of this explanation. For much of the time period covered in the book's empirical analysis, US governments were almost exclusively concerned about the threat of communist revolutions (certainly much more than about ethnic-based rebellions). In fact, the US *sponsored* an indigenous uprising in Nicaragua to undermine a revolutionary leftist government in Nicaragua. And yet, despite the massive resources invested in countering communism, the US was unable to prevent the occurrence of armed leftist uprisings from Central America to the Southern Cone. (US interventions did influence the outcome of those conflicts, but that is not the focus of the book's analysis.) If the US could not prevent the onsets of those conflicts it was most concerned about, it seems implausible that the dearth of armed ethnic rebellions in colonial settler states, even in Latin America, was due to US protection.

On a more general level, however, I do agree with San Akca and Aktürk that, overall, the book does not give much attention to the international and geopolitical environment. It only briefly discusses such factors as alternative explanations, and the statistical analyses use rather crude empirical proxies to control for them. This is mostly a function of scope, and I think that a more embedded approach to the domestic and international dimensions of the opportunity structure for violent and non-violent ethnic mobilization constitutes a promising avenue for future research.

In addition, San Akca points out that the colonial settler states gained independence much earlier than the decolonized states, suggesting that institutional consolidation, as well as lower salience of ethnicity in contemporary politics, could explain the former's lower rates of armed ethnic rebellion. Relatedly, Aktürk argues that religious homogeneity in the Americas is responsible for the lack of violent conflict in the colonial settler states category. Due to a lack of global data for many crucial variables (including lower-level armed conflicts) in the 19th and early 20th centuries, it is difficult to replicate the book's statistical analyses for the immediate post-independence period of the colonial settler states.

However, using data on major intrastate wars from the Correlates of War project (Sarkees and Wayman 2010), the book does provide a descriptive comparison of the rate of armed ethnic rebellions in the colonial settler states and decolonized states that includes the former's immediate post-independence phase (93–94). This comparison reveals that the colonial settler states already had a significantly lower rate of such rebellions in their first years of independence when their political institutions could hardly be considered consolidated. It is even questionable whether political institutions in today's Honduras (where indigenous people make up a larger percentage of the population than the Basques in Spain) or Bolivia (with an indigenous majority) are significantly stronger than in, say, contemporary Côte d'Ivoire or the Philippines.

Moreover, while it is true that in stratified societies, where ethnic groups tend to be organized as ethno-classes, ethnic mobilization might be "hidden" within class-based political movements, luckily, the data on (post-World War II) civil conflict used in the book provides a relatively nuanced coding of the ethnic dimensions of a given conflict. This allowed me to include conflicts in the analysis that did not feature explicit ethnic demands on the part of the insurgents, but nevertheless exhibited a systematic component of ethnic recruitment. The fact that all results remain robust when using this alternative, broader ethnic civil conflict variable (84), together with the finding that the colonial settler states exhibit significantly *higher* levels of *non-violent* ethnically based protest (91), makes me confident that the observed differences in violent ethnic conflict between colonial settler states and decolonized states are not simply due to low salience of ethnicity in the former type of states.

With respect to religious homogeneity, in particular, it is important to note that both in the book and elsewhere (Bormann, Cederman, and Vogt 2017), I find evidence that linguistic divisions are at least equally prone to violent conflict as religious divisions. Also, the growth of evangelical churches in Latin America has broken the Catholic Church's religious monopoly on the continent, decisively

affecting ethnic mobilization in countries such as Ecuador, where the indigenous movement has become divided along the same denominational divisions that divide the dominant European-descendant ethno-class (as described in chapter 8). Thus, I would argue that the crucial factor is not religious homogeneity but the integration of the oppressed groups into the cultural system of the oppressors more generally, along both linguistic and religious lines, that makes violent ethnic conflict less likely in stratified societies – which is very different from the case of Israel (as well as [Northern] Ireland), where the marginalized group has preserved its own religion and language, distinct from that of the settlers. As Malcolm X put it, “The greatest miracle Christianity has achieved in America is that the black man in white Christian hands has not grown violent. It is a miracle that 22 million black people have not risen up against their oppressors – [...] that a nation of black people has so fervently continued to believe in a turn-the-other-cheek and heaven-for-you-after-you-die philosophy” (Malcolm X and Haley 2001, 349)!

Besides their cultural integration, historically marginalized groups in stratified societies typically also become integrated into the political-ideological system of the dominant ethno-class, such that the latter’s ideological divisions (e.g., communism versus liberalism) impose themselves on these marginalized groups (and their movements), as, for example, in Guatemala (described in chapter 8). The book only briefly touches on this issue (35), also because a lack of systematic data on the ideological outlooks of ethnic group representatives (e.g., ethnic organizations) makes an empirical analysis of the extent and effect of ideological divisions difficult.

Finally, Clarke questions the implications of the book’s argument in terms of what I call the “titular nation states.” Rather than being “cross-pressured in terms of the structural conditions for conflict” (44), he expects them to exhibit a particularly high proneness to violent ethnic conflict because of the strong grievances caused by long-standing ethnic hierarchies combined with ethnic segmentation. However, as I argue in the book, “stronger motivations do not automatically entail more extreme forms of contention” (4). The causal mechanisms emphasized in the book’s argument suggest that the high degree of group hierarchization generally undermines historically marginalized groups’ mobilization capacity and, more specifically, prevents challenges against the center of state power – while, simultaneously, the high level of segmentation allows even generally weak groups to mount an armed challenge to state power in limited, peripheral geographic areas. As a consequence, armed ethnic rebellions are more frequent in titular nation states than in colonial settler states and when they occur, they tend to be of a secessionist nature (which is also what the empirical results indicate). In this sense, Clarke is right to observe that the book’s argument privileges the mobilization capacity and opportunities variables over grievances in its explanation of the *form* of ethnic conflict (violent vs. non-violent).

In the last part of this response essay, I will address four important topics and questions, which, as the three commentators highlight, remain unresolved or somewhat neglected in the book but point to promising avenues for future research: the occurrence and degree of one-sided violence, temporal dynamics in mobilization in the form of changes from one form to another, the relevance of race, and the political salience of ethnicity in stratified societies if/when established hierarchies are toppled.

Both Aktürk and Clarke point out that by focusing on “collective action undertaken by organized nonstate actors” (8), the book overlooks one-sided violence against civilians, especially violence perpetrated by the state, and, more generally, structural violence – what Clarke calls “racial capitalism and institutionalized discrimination” (2) – on the part of the dominant group. While the book refers to the omnipresence of violence in the colonial settler states (as well as high incarceration rates) in the concluding chapter, it is true that the analytical focus is entirely on the (violent or non-violent) actions of non-state challengers to state authority. In this sense, the book follows a long-standing tradition of political scientists who, as Christian Davenport (2007, 1) remarked fifteen years ago, “have paid far more attention to the evils done *against* governments [...] than to the evils done *by* presidents, the police, military, secret service, national guard, and death squads against those within their territorial jurisdiction” (emphasis added).

Given that the book clearly stakes out the scope of its analytical focus early on, I think the limitation in terms of the actual analysis is justifiable. However, there is of course also a discursive dimension to this question, and I agree that there is a risk that the terminology employed and the discursive focus on non-state actors – and, therefore, the neglect of state violence – could be interpreted as a portrayal of stratified societies as much more harmonious than they really are, which was certainly not the intention of the book. Hence, I acknowledge that the book could have done more (for example, in the theoretical chapter) to remind readers that, of course, “repression and structural violence are – apart from divide-and-rule strategies – the main instruments of the dominant ethnoclasses to uphold the ethnic hierarchies in ranked ethnic systems” (189). On the empirical front, a systematic analysis of one-sided state violence (and other outcomes of social violence, such as homicides) across different types of states certainly constitutes an important avenue for future research.

Another fruitful extension of the book’s theoretical and empirical contributions relates to the temporal dynamics of ethnic mobilization. In particular, Aktürk raises the question what explains changes from non-violent to violent mobilization (or vice versa) and the timing of such changes. The theoretical (chapter 3) and empirical (chapters 5 and 7) consideration of actors, in the form of ethnic organizations, goes in the direction of answering this question. In settings where the structural conditions for ethnic group violence are present (as, for example, in Sub-Saharan Africa), as such actors representing politically excluded groups increase in strength (in the form of electoral support), the risk of violent conflict increases – but this obviously raises the follow-up question why and under what conditions such actors gain strength in the first place. While this additional analysis would have arguably been beyond the scope of the book, more recent co-authored work examines processes of ethnic conflict escalation in more depth, showing that a radicalization in political demands (often as a result of organizational fragmentation) increases the risk of armed ethnic conflict (Vogt, Gleditsch, and Cederman 2021).

Next, Clarke observes that, following “the norm in the ethnic politics literature” (3), the book subsumes different types of ethnic identity markers under the common term of “ethnic,” and asks whether racial differences might have distinct implications for ethnic mobilization compared to linguistic and religious divisions. Again, this is an issue that the book briefly refers to in various parts, but does not exhaustively (or prominently) discuss. For example, the description of the colonial settler states’ ethnic cleavage type in chapter 2 does make explicit reference to the relevance of race: “The ideology of race developed during European colonial expansion played a powerful role in keeping the colonial hierarchies intact” (33), and “the racial boundaries have outlasted colonialism and continue to uphold the group hierarchies in these states” (34). In fact, one of the conclusions of the book, touched on in the final chapter, is precisely that while “most conflict researchers define ethnicity in terms of boundaries, rather than the specific ‘contents’ of ethnic identity,” the book’s findings indicate that “different colonial heritages – and the corresponding ethnic cleavage types – are associated with specific ethnic identity markers” and that “in contrast to the stratifying force of race, linguistic and religious segmentation of ethnic groups increases the risk of violent ethnic conflict” (184). The book also concludes that “racist ideologies continue to simmer below the surface of many democratically organized political systems” and that, citing Goldberg (2006, 338), race has been “buried alive” (184).

Yet, a more systematic empirical comparison of racial differences with linguistic and religious divisions is hampered by the difficulty of objectively “measuring” racial differences and degrees of racial integration/segmentation – which is not (or to a much lesser extent) the case for language and religion. It is arguably much easier to record overlaps in the languages that ethnic group members speak and in the religious denominations they belong to than identifying individuals’ overlap in phenotypical features. (From a methodological point of view, I would also argue that for the purpose of measuring cultural integration, based on observable [differences in] cultural practices, indicators of linguistic and religious overlap are more appropriate than a hypothetical racial overlap indicator.) Furthermore, the omission of a detailed empirical analysis of race in the book, in favor of more

descriptive references to racial hierarchies, also has the normative advantage that the book avoids the pitfall of trying to “objectively” measure and reify racial categories.

Finally, these considerations lead me to Aktürk’s fascinating question whether ethnicity would become politically irrelevant in stratified societies if the historically subordinated ethnic groups achieve equality. My hypothetical answer would be yes. It is a hypothetical answer, however, simply because overcoming the colonial legacies that form the backbone of today’s ethnic cleavage types has proven to be very difficult in reality. For example, even in post-Apartheid South Africa, referred to on the last pages of the book, profound socio-economic inequality between the privileged European descendants and the majority of the black African population persists. I would dare to predict that only a major geopolitical occurrence/shift – similar in its extent and consequences to the European conquest of the American continent – would offset the legacy of centuries of European colonialism and fundamentally change the entrenched global racial hierarchies.

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