**The roots of ethnic cleansing in Europe**, by H. Zeynep Bulutgil, New York, Cambridge University Press, 2016, 234 pp., 8 b/w illus. 12 tables, $99.99 (cloth), ISBN 9781107135864

# Response to critiques and new research questions

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I would like to start this response essay by expressing my deep thanks to the three reviewers for their excellent comments and critiques. As I discuss below, their contributions not only drive me to further refine the theoretical and empirical implications of the book but they also highlight important future research questions that should be of interest for scholars of ethnicity and mass violence.

I organize the rest of this response in three sections. The first focuses on the comments and questions of the reviewers that relate to the extent to which the argument of the book travels to certain contexts. The second section concentrates on the potential competing explanations that the reviewers allude to in their comments. Finally, I briefly summarize the research questions that arise from the debate in this symposium that are not explicitly dealt with in the book but remain important for our understanding of ethnicity, mass violence, and ethnic cleansing.

The reviewers raise questions about the applicability of the theory from three perspectives: geographical, temporal, and institutional. On the geographic front, Akturk suggests that the argument of the book is specifically applicable to contexts with a "monoethnic regime" and therefore would have difficulty travelling to parts of Africa, Asia, and Western Europe, where according to him this type of set-up is not common. Similarly, Mylonas suggests that the argument of the book is specifically applicable to contexts where a significant part of state elites is driven by a homogenizing imperative along ethnic lines. Unlike Akturk, Mylonas agrees that the argument applies to Europe in general but, like him, raises questions of applicability in the contexts of Africa and Asia. Both reviewers also question whether the argument would travel to South America, though as I argue below, the basis of the critique, in this case, is different.

Before responding to these questions, two points of clarification are in order; one on the assumption of the book about dominant ethnic groups, the other on the definition of a dominant ethnic group. First, the argument of the book does not presume that the state elite as a whole is driven by the desire to ethnically homogenize the population. Instead the book is explicitly based on the argument that the members of the dominant ethnic group (including those who become leaders) significantly vary in their underlying preferences on the treatment of non-dominant ethnic groups, with some willing to provide significant rights, others willing to use softer tools of assimilation such as public education, and yet others preferring to remove the group from the territory of the state. The book discusses several empirical examples from Central and Eastern Europe with significant ethnic cleavages to make the point that those that belong to the last category are typically politically weak. To understand the causes of ethnic cleansing, we need to figure out why the distribution of different types varies temporally and across different countries.

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Second, the book conceptualizes a dominant ethnic group as one that predominates in the state apparatus, which could be due to indirect reasons such as the priority given to their language in the public sphere or due to explicit preference for the group in the laws of the country.

Having made these points of clarification, let me now move on to the different geographical contexts that the reviewers refer to. First, on the question of Western Europe, I would argue that the theory in the book applies to this region. Assuming we define Western Europe as territories not covered by the four large empires in Central and Eastern Europe (Russia, Austro-Hungary, Prussia, and the Ottoman Empire), this region would include France, Spain, most of Italy, the Benelux countries, Scandinavia, and Britain. Historically, with the possible exception of Belgium, all these countries adopted a system with a clear dominant language/dialect, which allowed the speakers of this language/dialect to play a leading political role in the public sphere. It is also fairly well established that the elites in the more heterogeneous Western European states such as France and Spain went through periods during which they made efforts to homogenize their populations, though with varying levels of success (Carr 1966; Weber 1976). Given the existence of a clear and specific dominant ethnic group in most Western European countries, the argument on the non-repetitiveness of ethnicity and the impact of territorial conflict on ethnicity are both applicable to this region. As Akturk cites, the book actually provides a potential explanation for the lack of ethnic cleansing in places such as the German-speaking regions of France after WWII that is based on the existence of robust non-ethnic cleavages and the political parties that emphasized such cleavages.

The other geographical regions that come up in the reviewers' questions are parts of Asia and Africa, where the dominant political status may belong to shifting alliances of several groups rather than one persistent group that is easy to identify. I would make two points about this critique, the first one is an observation about Asia, the other is a summary of the point I make in Chapter 6, which focuses on post-colonial Africa. First, looking over the general set-up in Asia, while there might be contexts in which it is difficult to identify a clear dominant group, I would question whether the general trend is fundamentally different from Europe. In fact, in a significant number of the countries in this region, there exists a clear ethnic and/or religious group that dominates the political system.[1] It is, however, true that shifting alliances of multiple groups are closer to the norm in post-colonial Africa and this raises questions about the applicability of the argument in this context. As I argue in Chapter 6, however, the argument might still provide insights about Africa in two respects. First, while clear dominant groups are harder to identify in Africa, as long as the coalition of dominant groups varies from state to state, territorial wars might still have a radicalizing impact on the dominant groups in Africa by changing the existing political hierarchy. Second, the argument of the book gives us insights about why we observe a relatively small number of ethnic cleansing cases in post-colonial Africa, especially given the high ethnic heterogeneity of this context. Basically, the infrequency of intra-state territorial conflict might account for the relative non-existence of ethnic cleansing in this region. As the book discusses, it is possible that patterns of territorial conflict within civil wars in Africa actually result in localized cases of ethnic cleansing carried out by non-state militant actors. Such cases, unfortunately, lie outside the empirical scope of the book but are important for providing a complete answer to the reviewers' questions on the applicability of the argument in this context.

Both Akturk and Mylonas also wonder whether the argument would be applicable to South America. Here the basis of the question is different. Although these states have identifiable dominant groups, the groups in question are the same across different countries (the

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white/mestizo descendants of Spanish/Portuguese colonizers), which means that the dominant groups are repetitive rather than non-repetitive in this context. As the book discusses, the causal mechanism based on territorial conflict and salience of ethnicity would not be applicable in this context. One general prediction for this region as a whole would be that, controlling for other factors such as the importance of non-ethnic cleavages, ethnic cleansing should be less likely for the non-dominant groups in this continent.

Akturk also raises the important question of whether the argument in the book would account for pre-nineteenth century cases of ethnic cleansing. Given that ethnicity as a politically relevant category did not really emerge until the mid-nineteenth century, I believe it would be anachronistic to talk about "ethnic cleansing" before this period even if groups that were targeted are coded as "ethnic" today. For ethnic cleansing to occur, the perpetrators should also see and define their victims as ethnic groups, not as representatives of another category such as a class or a religious group. Based on this understanding, I would readily admit that the argument might not account for cases of religious cleansing in the medieval period (such as the deportation of Jewish and Moorish populations from Spain in the fifteenth and sixteenth centuries). I would, however, humbly suggest that some of the intuitions that follow from the book might be potentially useful in understanding these cases. For example, it would be interesting to study the divisions within the main religious group (e.g. Catholics in Spain) and how the political disagreements within this group affected the policies towards the religious minorities. After all, it was typical for medieval kingdoms to have competing court factions with different preferred policies on a variety of issues. Of course, until the sixteenth century, there was only one dominant religious group (Catholics) across different countries of Europe and hence the part of the argument on territorial conflict would not apply to this period. However, it is possible that some of the cases of religious cleansing in post-sixteenth century Europe (of course, we do not yet have an inclusive list) might be explained by territorial conflict during the religious wars between the Protestant and Catholic states.

Finally, Mylonas also suggests that the argument of the book is only applicable to contexts with democratic or quasi-democratic systems. I have two comments on this suggestion. First, a number of the cases studied in the book (e.g. Ottoman Empire, Austro-Hungary, and inter-war Poland) show that it is possible to have meaningful competition among political parties or factions in regimes that we would not define as democratic even using the most minimalist procedural definition of democracy. Second, for the more totalitarian contexts in which competition among factions is largely rooted out, the argument makes a clear prediction. In these types of contexts, even the smallest possibility that a minority group might cause security issues can easily prompt ethnic cleansing because the obstacles in the form of competing political parties with alternative political concerns does not exist. The typical example for this type of context is the Soviet Union under Stalin (discussed in the book) but it might also apply to other countries in specific periods (for example, the Ottoman Empire under the radical faction of the Committee of Union and Progress between the end of the Second Balkan War and the end of WWI). One extension of this argument is also that even when there is political competition in the core of the country, colonial populations that are completely excluded from participating in this system would also be particularly vulnerable. This extension would explain why colonial populations such as the Herero and Nama in Namibia were promptly targeted with genocide when they revolted against the German colonizers.

The reviewers also articulate a number of explanations that might be alternative or complementary to the argument of the book. I have identified five such suggestions: (1) The possibility of an ideological rather than geopolitical path (Akturk). (2) Political

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legitimacy based on a multi-ethnic constitutive story (Roshwald and Mylonas). (3) Theory of "brewing time" (Roshwald). (4) Alternative obstacles to ethnic cleansing (Roshwald). (5) Security-based and revisionism arguments (Mylonas). I consider each of these alternatives and how they relate to my argument in this order.

First, Akturk introduces a distinction between geopolitical arguments (in which he also includes mine) and ideological arguments. He also believes that the latter would be better at accounting for cases such as the Holocaust or colonial genocides such as the Hereros. I have already touched upon how my argument relates to colonial genocides above and the book spends some time on the admittedly outlier case of the Holocaust. Theoretically, however, I do not completely agree with Akturk's clear distinction between geopolitical and ideological paths to ethnic cleansing or his categorization of the book's argument. One of the main points that the book and my other work make is that often there is not a single way to assess whether a minority group would constitute a security threat, even when the members of the group in question collaborate with a rival state during a war (see also Bulutgil 2017). The ideological priors of the leaders shape the type of evidence they focus on as well as how they interpret the information they receive. International relations scholars typically deal with this situation by invoking the argument that it is the perception of threat rather than actual threat that matters. But they rarely offer a separate theory of where the "perception" comes from. My book, on the other hand, suggests a theory of why different leaders might perceive and interpret the same threat differently based on the extent to which they value non-ethnic political issues. Put another way, the argument in the book combines geopolitical and ideological elements to account for ethnic cleansing.

The second potential theoretical challenge, raised by Roshwald as well as Mylonas, is the idea that there might be different sources of legitimacy in different states with some, such as the Austro-Hungarian Empire, having a constitutive story that is based on the ability to bring together or peacefully arbitrate between different nationalities. A similar theory has also been suggested by Scott Straus in his recent book that links foundational narratives (not so different from "constitutive stories") to account for why some cases escalate into genocide and others do not in sub-Saharan Africa (Straus 2015). I have three thoughts on this line of argument, one is an empirical observation and the others are theoretical points. First, even in the Austro-Hungarian Empire where there was noteworthy institutional tolerance for multilingualism, there were groups that enjoyed a higher political status than others. For example, within most of the Austrian part of the Empire, German remained the main language in which public administration was conducted. This rule applied to Bohemia and Moravia, where Czech rather than German was the majority language, and efforts to put Czech on an equal footing with German through the Badeni Laws in 1897 failed due to a strong German backlash. The dominant status of the Magyar language was even more entrenched in the Hungarian part of the Empire, especially in areas such as Slovakia. Thus, territorial revisions could still be expected to have a significant impact on the members of the dominant group, Germans in the Austrian part, and Magyar speakers in the Hungarian part.

Second, if the alternative theory is that tolerant multi-ethnic constitutive stories, rather than non-ethnic cleavages, are the main obstacle against ethnic cleansing, then a number of questions need to be answered before we can test this argument. To begin with, how do we know or identify a "constitutive story" when we see it? The answer to this question might be somewhat more straightforward for regions such as Africa, where the immediate post-colonial period emerges as the prominent candidate. But what would be the relevant period for identifying the foundational or constitutive narrative for a state such as the Ottoman Empire? Would it be the initial period of formation in the early fourteenth century? The

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period after the capture of Istanbul in the mid-fifteenth century? The period right after Tanzimat in 1839? Or after the takeover by the Committee of Union and Progress in 1908? Depending on the answer to these questions, our assessment of the constitutive story argument would vary greatly because, until the mid-nineteenth century, the Ottoman Empire boasted a relatively tolerant system toward its Christian minorities. The point here is that to test this argument as an alternative we first need an exogenous definition that allows us to identify the "constitutive story" or "foundational narrative" without taking into account the minority policies of the state. To continue, we need to consider the possibility that the dominant "constitutive" narrative that one observes at a given point in time, as well as the change overtime in this narrative, might be at least partially driven by the factors that are highlighted in my book such as the salience of non-ethnic political issues and/or territorial conflict during wars. As I show in the book as well as my other work, in the Ottoman case both factors played a substantial role in the radicalization of the leadership.

The third potential alternative is Roshwald's thought-provoking notion that elites that are educated in a relatively tolerant state such as post-Habsburg Central Europe need some "brewing time" to be infected by the more exclusionist notions of nationalism. Roshwald bases this suggestion on the empirical observation that, with the exception of post-Ottoman territories, states in Central-Eastern Europe actually did not use ethnic cleansing until WWII. Once again I have an empirical and a theoretical response to this idea. On the empirical side, I agree with Roshwald that WWI stands in contrast to WWII in that it did not result in as many cases of ethnic cleansing. There is, however, a relatively simple explanation for this difference that relates to how these wars were fought. Especially on the Western Front, WWI was a trench war in which the frontlines remained relatively stable. On the Eastern front between the Ottoman Empire and Russia, the front lines moved back and forth, and therefore there were cases of ethnic cleansing and genocide (Bulutgil 2017). WWII also included significant movement of frontlines back and forth as well as the military occupation of large swaths of territory. In this respect, one could argue that WWI was not only different from WWII but it also differed from the Balkan Wars and the Greco-Turkish War of 1919–1921, both of which included shifting frontlines and resulted in ethnic cleansing. On the theoretical side, as Roshwald himself recognizes, whether and how the Central European leaders actually turned to exclusionary ideas about ethnic minorities can largely be explained by the experiences during the German occupation during WWII. In fact, as the book argues, in Czechoslovakia as well as Poland, politicians with diverse views on the treatment of German and Ukrainian minorities at the beginning of the war only converged on ethnic cleansing during the course of the war. Put otherwise, what Roshwald calls "brewing time" seems to entail the very causal mechanisms that I highlight in the book.

Roshwald also suggests the idea that there might be political obstacles against ethnic cleansing other than non-ethnic cleavages (such as class or religious-secular divisions) that divide the dominant ethnic groups. As examples, he cites concern for rule of law (including property rights) or for the international reputation of the state. I agree with Roshwald that these are indeed potential obstacles for ethnic cleansing. But I would suggest that they are not alternatives but rather extensions of the idea that non-ethnic concerns prevent ethnic cleansing. For example, a leader who cares more about upsetting the rule of law and creating precedence about arbitrary confiscation of private property is a leader who puts economic incentives and development over ethnic concerns. In fact, this would be the typical stance of a conservative/economically liberal politician who first and foremost focuses on the economic well-being of a country as a whole rather than preserving the

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ethnic hierarchy. According to the argument of the book, it is indeed this type of politician who stands as an obstacle against ethnic cleansing.

Finally, Mylonas also raises a number of theoretical questions about the relationship between the argument in the book and the security-based/geostrategic arguments. In answering his questions, I first refer to the security-based arguments in general and then I turn to Mylonas' own argument on revisionist states and exclusionary policies (Mylonas 2012). The main point of Mylonas' critique is that there is observational equivalence between the geostrategic arguments and the book's argument. I do not entirely agree with this assessment. The book highlights a number of ways in which the geostrategic arguments make different predictions than the argument in the book (on this point also see, Bulutgil 2015). The most obvious among them is that the security-based arguments fail to account for the large number of ethnic cleansing cases that take place right after rather than during wars. In these cases, the immediate war-related security concern has already ceased to exist, therefore, pure security-based arguments cannot on their own account for the outcome. Once again international relations scholars often argue that it is the perception of threat to long-term security that leads to ethnic cleansing. But then it is incumbent on the researcher to explain how and why this long-term security concern does not lead to ethnic cleansing during peacetime or at the very beginning of the war. The argument in the book answers this question by focusing on the dynamics during territorial wars that radicalize the dominant ethnic group.

Mylonas also suggests that there might be observational equivalence between the books' theory and his argument that revisionist states and outside aid for minorities in these states lead to exclusionist policies. There is clearly some overlap between the two arguments (as well as the other security-based approaches) in that they all predict that support for minorities, especially during wartime, increase the likelihood of ethnic cleansing.[2]

I also agree with the notion that we might observe the same patterns of radicalization regardless of whether the occupying state is a Great Power or a neighboring state (of course these two categories might overlap), though I would argue that the latter scenario is more common. I think a test of the two theories ought to include two steps. First, we need to establish a clear definition of what constitutes a revisionist state. It seems to me that if we were to define revisionist states as those that pursue the goal of substantially revising established territorial borders, a number of states that did use ethnic cleansing such as post-WWII Czechoslovakia, the Soviet Union, and Poland would not be coded as revisionist. This definition is somewhat different from that of Mylonas, which focuses on loss of territory (or increase of power) but depending on the extent of loss and the manner in which the war is lost, the elites might display war-fatigue and/or an aversion to violence that makes them committed to the established international borders. The most obvious example here would be post-WWII Germany, which most scholars would not code as revisionist.

A similar argument, based on war-fatigue, might also be made for the post-1923 elites in Turkey. Second, we also ought to consider the possibility that for the revisionist states, the two arguments are potentially compatible. In particular, it is worth asking what the underlying causal logic that connects revisionism to exclusionary policies is. If the reason the revisionist elites desire such policies is a result of their experiences during a recent war, then the underlying causal mechanisms that I outline in the book based on revenge and pessimism might be compatible with Mylonas' argument.

To conclude, let me briefly highlight a number of important research questions that arise from the exchange here that are not addressed in the book but that deserve attention in future

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research. First, to what extent is the argument of the book (as well as other arguments that focus on mass violence) useful in understanding pre-eighteenth century religious cleansing? Second, what is the relationship between non-ethnic cleavages and foundational narratives or constitutive stories? Finally, to what extent is the territorially non-repetitive nature of ethnicity (compared to other identity categories) a result of an underlying characteristic of linguistic categories or a result of deliberate state policy? These questions obviously go well beyond the goals of this exchange but raising them is the first step to providing answers.

## Notes

1. Some examples would include China, Japan, the two Koreas, Malaysia, Myanmar, and Sri Lanka.
2. Though they predict different underlying causal mechanisms and timing.

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