

## BOOK REVIEW

Michael Wahman. *Controlling Territory, Controlling Voters: The Electoral Geography of African Campaign Violence*. New York: Oxford University Press, 2024. 272 pp. \$90. Hardcover. ISBN: 9780198872825.

It is a great compliment when colleagues thoughtfully engage with your work. I am especially humbled as the four reviewers in this review forum, Adrienne LeBas, Nathalie Letsa, Meshack Simati, and Megan Turnbull, are some of the most interesting scholars working on topics related to election violence and political geography in contemporary Africa. When writing this book, I tried—I fear unsuccessfully—to emulate the excellence that characterizes their work. The four reviews published in this forum have similarities but vary in their perspectives. These variations reflect the diverse perspectives of the four reviewers on African electoral politics. Given the diversity of the reviewers, I am delighted to see that they all saw contributions in the book, suggesting that it could be useful to multiple audiences.

The book attempts to bridge scholarship on African electoral geography and electoral manipulation and violence. While a growing literature has acknowledged the strategic nature of election violence and explored subnational variations, this literature has adopted a mainly behaviorist and micro-level lens. Instead, in *Controlling Territory, Controlling Voters*, I highlight a more sociological understanding of voting behavior in Africa. I argue that space is not simply a stand-in for other geographically clustered variables, such as ethnicity. Voters form their expectations about elections locally, and violence has the potential to fundamentally shift local electoral environments. In African democracies shaped by localism, violence is a spatial strategy rather than a strategy targeted at individuals to shape political behavior.

The argument presented here is not a simple causal story where violence produces geographic strongholds or where the clustering of voter preferences gives rise to violent electoral manipulation. Instead, these processes are mutually reinforcing. The lack of electoral nationalization in Africa is a product of regionalism, geographic clustering of segmental cleavages, electoral system design, and clientelistic considerations, but violence is used to reinforce such geographically polarized patterns in environments where parties compete to mobilize fundamentally contested regional cleavages.

I put a fair amount of focus on rural politics in the book. This is for several reasons. First, rural spaces are often neglected in the election violence literature because violent episodes are under-reported and more often low-scale. Second, in both Malawi and Zambia, rural mobilization primarily decides elections. LeBas questions how the argument travels to more urban settings. However, the

territorial aspect of elections is also vital in urban areas, as the case of Lusaka clearly illustrates. Territoriality does not necessarily have to operate at the constituency level but can also entail restricting the democratic space at a more granular level—such as a neighborhood, compound, or market.

While the argument in the book has broad applications for the African context, where elections are generally structured around regional cleavages and where most elections are conducted under similar electoral rules (a combination of Single Member District parliamentary elections and national presidential ballots), I am also careful to lay out some of the scope conditions. Many of the reviewers' comments are related to applying the argument outside the two primary cases of my book. I believe the argument carries weight in other African and non-African cases (as I also illustrate in a final empirical chapter, where I apply the argument to the cases of Kenya and Zimbabwe), but not necessarily every other case. For instance, I would not expect the theory to hold in systems where party competition is more nationalized, or parties are less focused on mobilizing strongholds in favor of winning swing districts. However, the form of geographically polarized electoral system that I recognize and define in the book has become the predominant electoral system in the African context.

One of my main contentions with previous literature on election violence is its inability to differentiate different electoral contexts and over-generalize its predictions across vastly different cases. Any theory on election violence as a type of electoral manipulation has to be in close conversation with theories on electoral mobilization. Voters are mobilized in fundamentally different ways—targeting different types of cleavages and using varying mobilization strategies—across varying contexts. If violence is strategic, it will be tailored for particular forms of mobilization.

The reviews also highlight some of the limits of the book's scope. The main preoccupation with the book is to explain the function of election violence in geographically polarized electoral systems. The central focus is not to explain why violence escalates to higher levels in some elections but remains more constrained in other contests. Partly, this is a consequence of the case selection and my focus on two cases with the relative absence of high-scale violence but where election violence has nevertheless played an important role in electoral competition.


However, the book's last chapter provides some important insight into this issue that can be explored in future work. It has often been assumed that the absence of high-scale violence is indicative of relatively few incentives for violent manipulation. I disagree with this conclusion. Rather, low-scale violence is often preferable if actors can affect elections without engaging in high-cost, high-scale violence. Nevertheless, in some contexts, violence is likely to escalate due to underlying grievances or limited control over those perpetrating violence. A considerable volume of literature has focused on such grievances, may they be insecure property rights or historical processes of ethnic marginalization. Even without deep grievances, the book shows that violence can be used strategically and effectively to distort democratic competition.

Lastly, it is true that I emphasize parties as the main actors in electoral violence. It is important to note that Zambia and, even more so, Malawi have

relatively weak political parties. Party volatility is considerable, and many party functions are decentralized and performed by local candidates affiliated with national parties. However, I here use the term “party” widely to refer to all the supporters and candidates affiliated with the political party. Parties generally gain violent capacity by attracting local elites with adequate financial resources to organize violence on behalf of the party.

Turnbull raises the important question of whether stronger or weaker political parties will likely lead to more or less violence. These questions have been central to some recent important work by authors such as Hanne Fjelde and Niloufer Siddiqui. My book suggests that some organizational strength is necessary for parties to perpetrate violence outside their own strongholds (see the comparison between PF in Zambia and PP in Malawi). However, violence can also escalate when violence specialists contracted by political parties perpetrate violence.

All reviewers point to interesting extensions of my argument and further questions to be studied using the framework provided in the book. I look forward to exploring many of these questions in my future work and cannot thank the reviewers enough for further sparking my interest in this topic. I hope the book will also inspire others to study the electoral geography dimensions of election violence and probe the ways in which election violence affects the quality of democracy in regionalized African elections.

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