

Peer Review

Review of: "Understanding the 2024 Summer Riots in the UK: Three Case Studies"

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I appreciated the aims of this paper, which has several merits associated with its rapid-response accounting of recent events, its effort to parse out key features of collective violence that comprised the wave of actions in the summer of 2024, the effort to triangulate among multiple available data sources, and the comparative orientation of the analysis focused on three cities. The questions that motivate this paper are important in terms of presenting a rigorously descriptive account of actions that are often painted with a singularly broad brush and offering analytic conclusions rooted in comparative assessments and oriented toward policy considerations.

In these respects, the design of this study is clearly presented, and many of the descriptive elements (including the maps that aid our visualization of protest routes and the locations of various constituencies) are arrayed and presented effectively. Clearly, the baseline findings here help to refute the accuracy and effectiveness of specific policy approaches that have, and remain likely to, follow from this sort of wave of far-right collective action and violence. I am particularly enthusiastic about the authors' recognition of the multiple constituencies in play in these and other instances of contention, and how the interplay among those groups helps to shape the overall contours of conflicts as they unfolded.

That said, in my view, this study would benefit from more rigorous engagement with the conceptual literature that presumably forms the basis for the analysis here. To take just a couple of examples, while the question of the mode of contention represented by these actions is important, I was surprised not to see the authors' conclusions about whether the actions in question should be considered protests, vs. riots, vs. targeted attacks, rooted more carefully within an explicit conceptualization of what constitutes each of these modes and how we know when a particular interaction is considered, e.g., a "protest." While the authors' conclusions seem reasonable (i.e., my comment here is not motivated by any skepticism

about their substantive conclusions), fundamentally it remains difficult to understand the conceptual basis on which those conclusions are offered. A similar comment might be offered around the question of the “spontaneity” of these actions. On what basis can/should this characterization be assessed, especially given the rapid transmission of (dis-)information via social media that we see in such cases in our contemporary milieu?

Likewise, while the parsing of the constituencies involved in these actions constitutes perhaps the most promising aspect of the analysis, the lack of rigorous comparative assessment of the relative involvement of the identified groups and their interplay (both within any given action and across the three city-cases examined here) makes it difficult to see the payoff of this component of the analysis. While the conclusion that the participants were not in fact “the same” seems compelling, I’m not sure if the counterfactual (i.e., that challenging parties are in fact homogeneous) is ever borne out in known cases of collective action. So, while the associated policy implications reported by the authors remain important even given this limitation, I would recommend they consider a more precise and rigorous way to assess (comparatively or otherwise) the roles and interplay of these various identified constituents if we are to understand how other groups present influence the catalyzing forces mobilized via right-wing social media influencers, etc.

Finally, I would have appreciated more from the authors on two additional aspects. First, the question of the role played by the police and other authorities in the trajectory of the actions in question. Much of the analysis seems to treat the police as a key source of data and, at least implicitly, as neutral reactive defenders of order. But a large literature on political repression and the policing of protest engages with the ways in which police actively (and often proactively, in terms of their mobilization in anticipation of protest actions, rooted in sometimes-faulty or otherwise biased intelligence on anticipated protest/violence) contribute to the structure and scale of contention. Second, while the front end of the paper highlights the rationale for the comparative efficacy of the multiple case-study approach (i.e., the cities examined here varied in terms of demographics, anti-racist culture, and proximate history of far-right mobilization), we would benefit from hearing more in the discussion and conclusions about how these oriented dimensions shaped (or didn’t) the patterns observed and presented in the descriptive accounts of each of the three cities. In other words, if some of the value of the analysis here is rooted in the comparison of three cases, readers will want to see some discussion that discusses findings comparatively rather than in parallel. I would offer the same comment about the temporal comparison (i.e., looking at similarities and differences between waves in 2024 vs. 2011), which, in my view, is severely

limited by the lack of parallel data/description presented regarding 2011, to a degree where I might recommend the authors consider cutting this aspect of the discussion altogether.

In sum, this paper has evident value in expanding our understanding of these recent waves of contention and what they tell us about the contours of far-right protest/violence and the efficacy of predominant policy responses to the same. But realizing this promise would require more rigorous engagement with the conceptual and comparative bases of the design and analysis.

Declarations

Potential competing interests: No potential competing interests to declare.