

Review of “Commodity Price Shocks and the Seasonality of Conflict.”

This paper offers several interesting contributions, primarily: (i) the focus on seasonal food-based violence as opposed to annual variations, and (ii) disaggregating violence by actors – focusing on militias – and (iii) by type, emphasizing civilian victimization. Below I make several theoretical and empirical suggestions I think can help in strengthening the paper.

1. An alternative explanation to food-related seasonality is that the weather conditions during this period are better as to facilitate fighting. Wars often happen in late spring and summer, which overlaps with harvest season. The fact that only one type of actor seems to increase violence suggests this is not the case (more on that below), as do the inclusion of month fixed effects and the reliance on an interaction, but I think it would be useful to acknowledge this issue from a theoretical perspective as well, and state exactly why it is unlikely that violence increases due to seasonal issues other than harvest-related impacts.
2. On a related note, is the harvest season variable constant across the entire period for each grid cell, or has it been shifting over time (e.g., due to climate change)? Again, it would be interesting – and worth emphasizing – if harvest seasons has been shifting, as it helps to illustrate the robustness of the results. If harvest season is constant, then I wonder if this serves – in effect – as a substitute for grid fixed effects within the interaction term.
3. The focus on political militias: I think the emphasis on violence by political militias is an especially appealing part of this paper. However, I think that this emphasis should be given greater primacy in the theoretical, empirical, and discussion sections. In particular:
 - a. The section on pg. 6 is unnecessary and includes several inaccuracies about violence by rebels and identify militias (many of which are incredibly violent). Identity militias are also the ones often implicated with food-related conflicts, e.g. due to cattle rustling (see e.g., Rockmore 2012; Maystadt and Ecker 2014). In general, it feels like the authors are reading too much into the ACLED categorization. I would suggest to simply begin the theory section by explaining why political militias are important, and then discuss their motivations for violence over food, while keeping only the relevant sub-figure in figure 1 (or just report figure 1 in the appendix). I would also move Tables 1 and 2 to the appendix. To account for concerns some might have about other actor, the authors can write at the beginning of the section something like: “more comprehensive comparisons across food-motivated violence by different armed actors is provided in the supplemental appendix.” But I think that highlighting the distinctions and importance of political militias is sufficient and would make for a ‘punchier’ and more precise argument.
 - b. On a related note, the authors state that: “In general, political militias represent a category difference in violence, compared with other types of conflict actors. First, temporally, political militias’ violence often takes place outside of civil wars episodes. Second, political militia violence increases as a regime transitions from authoritarianism...” (pg. 7). The same is true for identity militias (which, according to ACLED, include, e.g., civil defense forces, tribal militias, some criminal organizations, etc.). Political militias are distinct in other ways, which the authors do highlight, but not in these two.
 - c. Similarly, the statement that, “First, because they are generally linked to the state (or elements within the state), they do not seek control territory on their own, and thus do not establish long-term control of, or long-term extraction of resources from territory” (pg. 9) is also inaccurate. Political militias may indeed – and often do – seek control if the

opportunity arises for them to expand their influence beyond that of “contractors,” depending on organizational agendas (e.g., Aliyev 2016).

- d. The statement “By contrast, rebel groups and identity militias stage attacks more frequently in non-crop-producing regions” (pg. 11) is not supported by Table 2 (61.6% and 61.2%, respectively). Again, I would suggest moving this table to the appendix, as it raises questions about what might be going on with formal state forces (who attack civilians in crop producing areas at almost the same rates as militias).
 - e. Finally, while the authors report limited anecdotes in support of the role of political militias, I would recommend providing some more substantive case-based evidence to help the readers visualize and understand these dynamics.
4. The dependent variable. The focus on violence against civilians is not only important from both research and policymaking perspectives, but also shows a welcomed awareness of the authors to the difference in the motivations of perpetrators across different types of violence, which diverges from the tendency to combine all types together as ‘conflict’ found in much of the extant research. I would hence encourage the authors to be more explicit about this point and add more detail to the discussion on pgs. 2-3 – right off the bat, explain why violence against civilians is important for the theory (it is used to facilitate looting or reduce production activity), and that it is more likely to arise as a result of food shocks, similarly e.g., to urban riots, but unlike ‘standard’ armed warfare between combatants.
- a. Semantics: in line with this stronger emphasis on political violence, I would also suggest changing the name of the DV (and the reference in the title) to “political violence.”
5. Empirics:
- a. ACLED data: the use of ACLED, especially considering the focus on militias, is in line with much of the research. However, ACLED suffers from **massive** reporting bias, where the number of reports (and hence incidents) greatly increases over time, although there are also variations across different periods that are not time linear. Including month and year FEs is one way of accounting for this, but I would also suggest adding a robustness model with time splines that take a more flexible functional form.
 - b. On a related note, some studies suggest that the GED is often preferred to ACLED for understanding subnational violence patterns, at least partly due to these issues (e.g., Eck 2012). I am not sure if it would be possible to subset GED violence by actor – maybe using text analysis – but at the very least reporting two robustness model that use (i) all one-sided violence events and (ii) one-sided violence by nonstate actors from the GED could help to illustrate the findings are robust to the reliance on ACLED. The PITF (Schrodt and Ulfelder 2016) is another potential option.
 - c. I think that the authors should include at least one-month DV lag in their models, considering the persistence of violence over time in many of these locations.
 - d. Related to point 2 above, with the inclusion of gid, month, and year fixed, is there sufficient variation on the harvest season variable to ensure the data is not being overfitted or that the authors capture ‘fixed’ effects?
 - e. Building on these two points, I would suggest the authors will do the following in Table 3: (i) in line with point (3) above, focus all the main models in Table 3 on violence by political militias; (ii) then, add each harvest month X shock interaction sequentially (+2, +3...+11) to each model, while including DV lag and temporal controls. (iii) In the appendix I would also suggest reporting the underlying constitutive term coefficients.

- f. I would then suggest that what is currently Table 3 will be reported as a robustness set of analyses that compares the impact of price shocks on violence over each post-harvest month across groups to highlight the fact the violence during the first three months after harvest during an agricultural shock is only robust for political militias.
 - g. Figure 4 is really useful. Again, in line with the political militias focus, I would suggest reporting these figures twice, once only for political militias, and again for all conflicts.
6. Discussion section:
- a. Rebel violence: I would qualify or revise the statement “By contrast, rebel groups, who are primarily attacking state forces, would not see any seasonal change in their ability to harm their opponents,” (pg. 21) considering that rebels may often still raid civilians in breadbasket territories (see e.g., Hultman 2009; Koren 2019).
 - b. The point that “Specifically, in developing countries with high dependence on agricultural production, attacking civilians during harvest season can be a useful way of damaging opponents' support base when they are at their most vulnerable – the time of the year when they are (in theory) realizing the bulk of their income for the year” (pg. 22) is very important. While the authors do not show this is the case empirically, I think this point is worth discussing in more detail, considering such violence represents a different set of dynamics, which can explain raids but not necessarily food appropriation (see Linke and Ruether 2021; Koren 2019). It is also possible such violence can also arise later in the year to destroy granaries or simply remove civilians from the land so again, I think this point merits more discussion.

References:

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