

Referee report for “Agricultural Shocks and Social Conflict in Southeast Asia”

Summary:

This paper argues that the seasonal nature agricultural work and income is a predictor of the temporal variation social conflict. Specifically, through the use of monthly data (2010-2022) on conflict in Southeast Asia, it brings to light the connection between weather-induced income shocks and (two different forms of) social conflict. The paper’s findings suggest that rural development programs ought to take into account the weather-conflict nexus in an agrarian context, with particular focus on the rice harvest months.

I found the manuscript extremely easy to read and applaud the author(s) for bringing this issue to light, and for applying it to a context less often studies (Southeast Asia). However, I have a few comments and concerns below.

Framing:

1. The authors correctly cited the large – and expanding – weather-conflict literature, which has recently highlighted several aspects about the relationship. One is that there are economic roots of conflict. This is by now well-documented and so the contribution of yet another case is probably small in my view. Second is the exploration of underlying mechanisms. To this end, the authors did a commendable job of discussing the greed vs grievances channels in the paper, but somewhat fell short of explaining carefully the theoretical as well as empirical challenges on sorting out channels (I will discuss this later in my next point), and how this paper actually contributes materially to our understanding of those channels.
2. On pp.1-3, the authors states that the income and conflict are fundamentally linked by greed (rapacity/predation) or grievance. While this is fine, they did not do an adequate job in explaining that grievance is itself possibly driven by opportunity cost (a rational response, relative to own past income) or by resentment (possibly irrational response, relative to the income of others). I suggest that the authors refer to Mitra & Ray (2014) and Panza & Swee (2023). Both papers do a great job of laying out why grievance could be either an opportunity cost or a resentment channel, which many in the earlier literature have failed to distinguish. The latter paper even provided a method to empirically sort between opportunity cost and resentment.
3. Related to above, the authors claimed, on p.2, that “protests and riots are often triggered by negative income shocks, and thus they are unlikely to relate to agricultural harvest, or if they are, the relationship should be negative”. But, it has already been established empirically that negative agricultural income shocks can in fact drive protests and riots (see Panza & Swee, 2023). Basically, the issue that I have is that harvest time can mean either positive or negative income shocks, depending on the harvest (weather), so the predictions in Table 1 are uncertain. In this regard, I encourage the authors to rethink whether using different forms of conflict can indeed

help one sort out the underlying mechanism behind weather-induced income shocks and conflict.

4. Relatedly, on p.3, the authors said that “incidents linked to larger-scale conflicts are unlikely to be driven by or related to agricultural income”. But the authors should look at a paper by Iyengar et al (2017) that convincingly connects historical (large-scale warfare) to agricultural shocks.
5. This paper will also benefit from being connected more closely to the climate literature (see Burke et al, 2009, and Dell et al, 2014, for example), to gain more salience.

Data & Methodology:

1. While the ACLED data does reflect decent amount of geographical variation in conflict, Figure 1 raises a concern about whether the spike around 2020-2021 is specific to any country, for example, Myanmar. If so, some subsample analysis (excluding Myanmar for instance) would be required to bolster the credibility of the main results.
2. This may be a minor point, but the deliberate omission of certain low-conflict countries may not be that desirable. This is because, some of those countries e.g. Laos or Timor-Leste may in fact be primarily agrarian while having systematically low levels of conflict, so they are in fact useful as “control group”. Countries that are mainly urban e.g. Singapore can be omitted since they won’t fit the agrarian story (but not because they don’t experience conflict).
3. The two-way fixed effects model can only account for time and cell (110km x 110km) fixed factors, but not time-varying cell factors. This means that lingering endogeneity issues remain. For example, if the overall economy experiences more frictions during harvesting months, and that these lead to heightened capacity for conflict, then the authors cannot interpret correctly the beta coefficient as being indicative of the harvest itself.
4. A minor point is that the main regression model as presented in equation (1) is not the same one they implement in the empirical analyses. The model uses “harvest_it” which the authors described as “the cell-specific harvest dummy” but from the tables I gather that the main regressor is actually the product of cropland area share multiplied and a harvest month dummy (so the regressor is 0-1, not 0 or 1).
5. While I understand that the authors are trying to use rice prices and rainfall to further examine the mechanisms, it must be said that while rainfall can be thought of as arguably exogenous (some may even question this, see Hsiang et al, 2013), price is almost certainly endogenous and so should not be used in the same way.

References:

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