Reviewer 1

Reviewer #1: Reviewer Report, "Agricultural Shocks and Social Conflict in Southeast Asia"

Recommendation: Revision (on balance, more minor than major)

I found a lot to like in this piece, though there are several issues I'd like to see resolved/clarified before recommending publication - though to be clear, in terms of scope, subject matter, and novelty in terms of clarifying the complex incentives created by seasonal abundance/scarcity in agriculturally-dependent societies, I think it is definitely an eventual candidate for publication.

*We appreciate your time and are truly thankful for your thoughtful and very constructive comments and suggestions. We addressed all issues raised in your report. In what follows, we reiterate your points followed by our comments summarizing the relevant changes made.*

My big outstanding issues are three:

1. Clarifying the use of price data. It is never mentioned at what level it's collected/modeled, which calls into question some of the identifying assumptions that are intended to address endogeneity concerns (see below in my point about pg. 11).

*This is a crucial point, and we thank you for raising it. In the original version of the manuscript we used international prices, indeed. Specifically, we used the export prices (fob) of Thai rice. But upon further deliberation, during the revision process, our strategy, while better than using local prices, was not perfect. Because the region consists of the worlds biggest exporters of rice, it might not be sensible to argue, with sufficient confidence at least, that export prices are not a function to any ongoing socio-political issues in the region. To that end, the endogeneity concern, rightly pointed out in your comment, does not get addressed. At least, not to the extent that it would make us comfortable going with that line of argument. So, we decided to exclude the price-related analysis/discussion from this version of the manuscript. This has not weakened the paper, we firmly believe. In fact, we believe this change has further refined the focus of the research.*

2. The exclusion of Laos in particular needs to be justified and theoretically motivated. At present, it is lumped in with several smaller and/or not agriculturally dependent countries like Brunei and Singapore; this doesn't make sense considering its economy is more similar to those of its neighboring countries that are included. The inclusion/exclusion of cases needs to be better theoretically justified.

*Originally, we excluded Laos (and a couple of other countries) because there were only a handful reported conflict incidents. But the point is well taken. In this version of the manuscript, we include all the available countries (ten countries in total) in the region. The results have remained unchanged.*

3. "A common pattern that emerges in both tables is that when the value or the volume of the harvest increases, the intensity of all forms of social conflict decreases (or at least do not increase)." I just don't see (unless it's by assumption) that you can reject the plausible mechanism that the reduction in social conflict made the volume and value of the harvest increase absent some clarification of the price variable (if it's a global price, then the exogeneity to specific conflict events for a widely grown staple crop make sense; if it's a very local price, not so much). This point is seemingly a narrow empirical one, but I think it is important for demonstrating the degree to which market signals can be thought of as exogenous or endogenous to proximate (conflict) environment.

*By omitting the prices and related analysis and discussion from the paper, we address much of this concern. We rephrased the remaining text to ensure that the channel is more volume-based rather than value-based, as we no longer observe/make connection with income shocks due to price variation. For example, [need to quote stuff from the paper once finalized]*

Best of luck in revision!

Minor points:

\*Pg. 4, 7-9: "This finding likely conflates the rapacity mechanisms of conflict with grievance and opportunity cost mechanisms of conflict." This wording is unclear; does the author mean the two effects cancel one another out? Please clarify.

\*Pg. 4, 29: "battles and explosions". During the time of an explosion? Does the author mean literal explosions, like bombs and mines and the like? Please clarify.

\*Pg. 8, 16-27: Please consider relabeling the table "Actors and conflict seasonality: theoretical expectations" to clarify that this would be the short-hand go-to table for reference the pieces theoretical conjectures.

\*Pg. 9: "Nasi Bungku Brigade" is a pretty sweet punk band name.

Pg. 11: The specific modeling choices may render this question immaterial, but at what resolution do you have price data? Is it at the national level? The regional level? The dynamics being discussed here strike me as being pretty localized in some instances and not in others. It's important the price data match as closely as possible the level of spatial/temporal resolution at which the theory operates.

Pg. 12, 11-14: "Moreover, there are very few incidents observed in Brunei, Laos, Singapore, and Timor-Leste, and we omit these countries." This omission needs some defense on theoretical grounds, not just empirical ones. I can buy that rice harvesting isn't a big tax on the average Singaporean's time, but I would 100% expect these dynamics to apply to Laos unless the Lao political opportunity structure simply isn't permissive enough. That is, this needs to be either framed in terms of a theoretically-motivated scope condition or these examples need to be included in the analysis.

\*Pg. 12, 23-28: "This excludes incidents with the geo-precision code 3 in the database, as the exact locations of such incidents are unknown and they are arbitrarily attributed to the nearest known site, typically a provincial capital." The reader (including yours truly) may not know what precision code 3 means, so please spell out even parenthetically.

Pg. 18, 40-45: "The identifying assumption in Equation (1) rests on the premise that the treatment variable, which is the product of the cropland area fraction and harvest month, is exogenous to conflict observed across locations." This strikes me as a pretty tenuous assumption, given what we know about how conflict affects food production via abandoned plots and missed harvests and/or sewing seasons.

Pg. 22, 41-47: "A common pattern that emerges in both tables is that when the value or the volume of the harvest increases, the intensity of all forms of social conflict decreases (or at least do not increase)." I just don't see (unless it's by assumption) that you can reject the plausible mechanism that the reduction in social conflict made the volume and value of the harvest increase absent some clarification of the price variable (if it's a global price, then the exogeneity to specific conflict events for a widely grown staple crop make sense; if it's a very local price, not so much).

Pg. 27, 7-12: "This is to be expected—the battles and explosions usually involve, directly or indirectly, the state. As a result, there is less policing elsewhere, which among other things, results in more crime and less order in the region." OK, but this could also be evidence of other mechanisms, like socialization into violence as a legitimate means of addressing grievances.

Reviewer 2

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 Iygun et al (2017) that convincing connects historical (large-scale warfare) to agricultural shocks.

5. This paper will also benefit from being connected more closely to the climate literature (see Bourke 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.

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