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.

> *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.*

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.

> *This is a helpful comment, which prompted us to put more effort into better explaining the mechanisms and contributions of the work (see also our response to the next two comments). We address this comment in a number of ways. First, we considerably re-arranged and re-phrased the introductory sections of the paper. As a result, Introduction has a more focused feel, while the Background and Context section goes in-depth in building the case for the mechanisms and pathways in which agricultural harvest may be linked to the considered forms of conflict. Second, we considerably expanded the empirical section, particularly its mechanisms tests component, where we specifically focus on identifying the plausible mechanisms or, where possible, ruling out the alternative explanations of the estimated effect.*

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.

> *We thank the reviewer for this incredibly valuable comment. That resentment can possibly interfere with opportunity cost helped us to explain and justify some of the results that seemed puzzling or seemingly non-existent in the previous version of the manuscript. We incorporate the role of resentment in forming our theoretical expectations in the Introduction as well as*

*Background and Context. For example, on **p. 1, par. 2** we note: "Harvest may fuel conflict by presenting opportunities to extort wealth or incur damage on one's opponents—the predation or rapacity mechanism. It may amplify or mitigate conflict by changing the absolute as well as relative incomes of groups of individuals between agricultural and non-agricultural locations as well as within agricultural locations—the resentment mechanism. And it may reduce conflict because the potential actors of conflict are busy harvesting or the benefits from harvesting outweigh the costs incurred by forgoing this activity in favor of involving in conflict—the opportunity cost mechanism." In Table 1 (**p. 9**) we project the harvest-time effect on riots and protests as "ambiguous" due to the potentially offsetting effects of opportunity cost and resentment mechanisms. On **p. 9, par. 3** we state: "Social unrest, on the other hand, which is often triggered by negative income shocks, may be linked to agricultural harvest in rural areas. However, the relationship can be negative or positive. The opportunity cost mechanism would lead to fewer protests at harvest time, which can happen for at least two reasons. First, when people—potential protesters—are busy harvesting, they are unlikely to take part in protests as the opportunity cost of this form of conflict is high. Second, if there is a short period of time, during the calendar year, when people in rural areas are relatively better off compared to other times of the year or to people in urban areas, it is during or shortly after the harvest season, when the years' worth of income has been realized. Therefore, the harvest-time increase in income can mitigate social unrest in croplands relative to the urban, non-agricultural areas. At the same time, there may be a resentment mechanism that could lead to increased unrest: within agricultural areas, the harvest time increase in income inequality—between farmers and non-farmers—may amplify social unrest (e.g., Panza and Swee, 2023). The net effect, manifested through opportunity cost and resentment mechanisms, can be ambiguous."*

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.

*Further to our response to the previous comment, we try to motivate the expected impacts of the harvest time transitory shift in income and employment. In so doing, and particularly as we incorporate the resentment mechanism into the analysis, we think about harvest time changes in conflict that may be inflicted due to absolute vs relative changes in income. For example, on **p. 12, par. 1** we note: "During harvest time, while the grievances of farmers may decrease due to the realization of income, increase in their income relative to others (whether rural non-farmers or urban dwellers) lead to resentment and unrest between different groups (Panza and Swee, 2023)." We also try to disentangle these mechanisms empirically. For example, on **p. 31, par. 2** we state: "if our proposed opportunity cost and resentment effects are valid, we would expect a decrease-then-increase in conflict incidence over the harvest window. During the harvest season, the opportunity cost will dominate the effect, thus leading to a dip in protests and riots. Toward the end of the harvest window, resentment will become more prominent, thus resulting in an uptick in social unrest. Table 7 illustrates the estimated effects."*

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.

*Thank you for pointing us to this paper, which is super interesting. Our claim is that because we use short-term and transitory agricultural shocks, related to harvest, rather than permanent shifts in production practices (such as that studied by Iyigun et al., 2017), we likely do not pick up the effect of the same motives. We make note of this study, and explain our reasoning more carefully on **p. 6, par. 3**: "even though agricultural production and large-scale conflict can be inherently connected in the long run (e.g., Iyigun, et al., 2017), incidents linked to battles between incumbents and insurgents to take control of a territory, are less likely to be driven by, or related to, harvest-induced short-term and transitory shifts in agricultural employment and income"*

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.

*We agree. We made such connections throughout the paper. In the introductory paragraph, on **p. 1, par. 1** we note "mounting empirical evidence points to a connection between crop yields and conflict (Wischnath and Buhaug, 2014; Buhaug et al. 2015; Koren, 2018; Vestby, 2019; McGuirk and Burke, 2020), wherein crop yields serve as a likely mechanism connecting climate shocks with conflict (e.g., Burke et al., 2009; Hsiang et al., 2013; Dell et al., 2014; Crost et al., 2018; Koubi, 2019)." Furthermore, on **p. 2, par. 4** we note "we contribute to the literature on climate shocks and conflict (e.g., Burke et al., 2009; Hsiang et al., 2013; Dell et al., 2014; Crost, et al., 2018). We present empirical evidence that emphasizes the effect of growing season precipitation on conflict through its effect on the harvest of the most produced and consumed staple cereal crop in the region. Moreover, we show that climate shocks can have varying effects on different types of conflict that are likely governed by different mechanisms and thus respond differently to seasonal agricultural shocks."*

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.

*This is an incredibly valuable comment. We addressed it in a number of ways. To begin, we changed the dependent variable from the count of incidents to the incidence of conflict. As we note on **p. 25, par. 1** "By transforming the count variable (conflict incidents) to a binary variable (conflict incidence), we mitigate the effect of influential observations manifested through surges in forms of conflict incidents during the Myanmar war." Importantly, after such transformation, our estimates related to battles and, especially, violence against civilians remained qualitatively and quantitatively similar. Perhaps expectedly, the estimated effects related to protests and riots attenuated toward zero. In the Appendix, we present the results of the regressions using conflict incidents as the dependent variable, with and without observations from Myanmar in the years 2021-2022 (**Appendix Tables B3 and B4**). This latter set of results, in particular, aligns well with the main results of the study. More broadly, we performed two sets of sensitivity checks wherein we omitted from the sample one country at a time and one year at a time end re-estimated the regressions. In the **Appendix Figures A3 and A4** we summarize the results of this exercise. Again, while the signs of the estimated effects are largely comparable across the regressions, Myanmar 2021-2022 incidents seem to be driving the results. This is not surprising, considering that Myanmar contributes to nearly half of all incidents in the sample. So, we note the caveat and on **pp. 25-26** provide a condensed but detailed summary of the case of Myanmar and its role in the current analysis.*

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).

*In the current version, we include Laos in the analysis, thus only omitting Brunei, Singapore, and Timor-Leste. In the text, we justify this on **p. 13, par. 2**: "We exclude Brunei, Singapore, and Timor-Leste because they are small and/or not agriculturally dependent countries and because the ACLED coverage for these three countries is from 2020 onward only."*

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.

*This is a valid comment. In the paper, we partly circumvent the raised issue by using the fixed sizes of the croplands, and times of the harvest. In the text, we clarify this on **p. 20, par. 3**: "The identifying assumption in Equation (1) is that the treatment variable is exogenous to conflict. This assumption may seem tenuous because conflict may affect production through factors such as abandoned plots and missed or mistimed harvests and planting seasons. As a result, a lower agricultural output may be the consequence of the change in conflict rather than its cause. However, in our study, we do not apply production data that would vary yearly and instead use cropland area and harvest months, which are location specific and fixed over time. Such an approach, admittedly driven by data limitations, mitigates the issue of reverse causality." In the following paragraph, on **p. 21, par. 2**, we also note that "We also include contemporaneous rainfall, which varies over time and across space, in the regression in an attempt to address, at least to an extent, remaining endogeneity issues. This allows us to control for the direct impact of weather on conflict, for example, if excessive rainfall reduces the mobility of troops or makes protests and demonstrations somewhat untenable." These steps are taken to alleviate endogeneity issues, but to the extent that this is an observational study, we cannot be completely sure that the estimated effect is indeed what it is intended to be. But what we can do is scrutinize the main results through a set of robustness and sensitivity checks, which we do. In our response to an earlier comment, we noted some of these checks. In addition, we perform a specification check where we vary the fixed effects of the model. We present the related specification chart in **Appendix Figure A5**. We also perform a falsification check by scrambling the harvest seasons and re-estimating the regressions. **Appendix Figure 6** presents the results of this exercise where we observe no substantial impact when the "wrong" harvest seasons are randomly assigned to the croplands, suggesting that the estimated results are not merely coincidental.*

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).

*Thank you. We fixed the issue. We also re-labeled the treatment variable. The main regressor (unless otherwise stated) is the indicator for the cropland interacted with the indicator for the harvest season, which we denote by $s_{itm} = cropland_i \times harvest_{tm}$, where the subscripts indicate cell (i), year (t), and month (m). So, on **p. 20, par. 2** we describe: "The treatment variable, $s_{itm} = cropland_i \times harvest_{tm}$, is the product of the cropland binary variable and*

the harvest binary variable. cropland, which is fixed over time, equals one if more than 10,000 hectares of land is used for rice production in the cell (IFPRI, 2019) and equals zero otherwise. harvest, which is cell specific, equals one when the period of observation is the harvest month and equals zero otherwise."

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.

> *We resolved this issue by dropping the price-related analysis/discussion from this version of the manuscript. In the original version of the manuscript, we used international prices. Specifically, we used the export prices (fob) of Thai rice. But upon further deliberation, during the revision process, our approach—while not as bad as using local prices—was far from perfect. Because the region consists of the world's biggest exporters of rice, it might not be sensible to argue, with sufficient confidence at least, that export prices are not a function of any ongoing socio-political issues in the region. To that end, the endogeneity concern rightly pointed out in your comment, does not (and cannot) be addressed. At least, not to the extent that it would make us comfortable going with that line of argument. By no longer estimating the price-related effect did not weaken the paper, however. In fact, we believe this change has strengthened the focus of the research, where we now put more emphasis on the internal validity of the results.*

References:

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