

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 flagging 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 approach—while better than 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) 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, however. 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 of reported conflict incidents. But the point is well taken. In this version of the manuscript, we include all the available countries—save Brunei, Timor-Leste, and Singapore, so, eight countries in total—in the region. The results have remained qualitatively the same.

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 connections with income

shocks due to price variation. Moreover, because the size of the croplands and the times of the harvest, that we use in the analysis, are fixed, we mitigate the endogeneity issue alluded to by this comment. 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."

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.

We agree this wording was confusing. We addressed this (and similar) issue(s) by considerably rewriting and rephrasing the paper.

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.

We used "explosions" as a short-hand for "explosions/remote violence" as defined by the ACLED Project—the source of the conflict data in our study. So, in our analysis, we combine this conflict category with "battles." These two conflict categories share similarities insofar as they both tend to be manifestations of a large-scale military clash, often between the state and insurgents. We make note of this in the text and use the label "battles" as a catch-all term for the form of conflict combining these two conflict categories.

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.

We changed the caption of the table and clarified its purpose as instructed.

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

This is very true!

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.

We no longer apply price data in the analysis. See also our response to the first major issue.

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.

We now 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."

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.

Thank you. We rephrased this text. On p. 13, par. 3 we note "Our study covers more than 70,000 unique incidents. This excludes incidents for which exact locations are unknown, and they are thus arbitrarily attributed to the nearest known site, typically a provincial capital (such locations are recorded with the geo-precision code 3 in the database)."

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 sowing seasons.

Thank you for this comment, which nudged us to better explain our identification strategy. We quoted the relevant paragraph in our response to the third major issue.

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).

As noted in our previous responses, we resolved the issue raised in this comment by abandoning the prices from the analysis. To re-iterate, we believe this was the right thing to do, considering the role that the region plays in price formation on the global rice market.

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.

In revising the manuscript, we made a conscious choice to focus on changes in the conflict that stem from the harvest time changes in income and employment. To that end, we decided to give up the breadth in favor of the depth of the analysis. So, from the earlier version of the manuscript, we discarded the last regression and the related discussion that linked small-scale conflicts with large-scale conflicts. Such an analysis, while interesting in and of itself, felt out of context. As a result, we are also no longer facing the issue that was referred to in this comment.