Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/nhess-2021-337-RC2, 2022

Comment on nhess-2021-337

Anonymous Referee #2

Referee comment on "A performance-based approach to quantify atmospheric river flood risk" by Corinne Bowers et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-337-RC2, 2022

This paper provides a framework (PARRA) to quantify Atmospheric River flood risk using performance-based submodules for Sonoma County, CA. The methodology is described in detail, and a case study from a 2019 AR event was investigated. The PARRA framework is very interesting and useful for providing a mean estimate of expected losses with uncertainty bounds. However, I have some concerns about the methodology and the way it was described in the manuscript. I also have some concern about the 2019 case study that was investigated – why choose a case study that the PARRA framework barely captures in the tail of its distribution? Why not show a case study that the PARRA framework captures much better? I have some comments below that can help improve the paper. I think the paper can be accepted after some minor revisions and clarifications.

General Comments

What would the PARRA framework provide a stakeholder as the estimated losses for the 2019 AR event? The results show an average loss of about $25 million (Figure 9a), but the actual cost was $91.6 million. This actual cost is covered in the tail of the distribution provided by PARRA but is far from the mean of this distribution. Do the authors consider this an accurate assessment? Some comments on how to interpret the results with their associated uncertainties, as well as how to interpret the acceptability of the results, would be helpful.

Specific Comments / Technical Corrections

Section 1 Introduction

Line 27: Pineapple express is not the only mechanism that brings ARs to California.

Line 56: "… understanding climatology of ARs", see Espinoza et al. 2018 and Massoud et al. 2019 who aimed to understand AR climatology in a global context.

- Espinoza, Vicky, Duane E. Waliser, Bin Guan, David A. Lavers, and F. Martin Ralph. "Global analysis of climate change projection effects on atmospheric rivers." Geophysical Research Letters 45, no. 9 (2018): 4299-4308.
- Massoud, E. C., V. Espinoza, B. Guan, and D. E. Waliser. "Global climate model ensemble approaches for future projections of atmospheric rivers." Earth's Future 7, no. 10 (2019): 1136-1151.

Section 2 Framework Description

Line 110: Is this theorem a version of Bayes theorem? How are they related?

Line 143: Initially it seems that the AR category score (1-5) is used as input in the PARRA framework. It isn't until later in the manuscript that it becomes clear that AR max IVT and duration are used. The authors should clarify this earlier in the paper.

Line 147: Some precipitation can be from non-AR sources. Is this considered for the calculation of the precipitation submodule in the PARRA framework?

Line 160: take out the word 'are'

Section 3 Case Study: Sonoma County

Line 235: Is there a citation that shows why WLS can be used to express the relationship between IVT/DUR and PRCP? This seems rather simplistic and not thorough enough to capture the estimated PRCP. According to Figure 4 there seems to be significant spread in these relationships. Perhaps the authors can explain why this choice was made.

Line 260: The mean of the distribution is way off here. Should this be a reason for concern? It seems that this methodology begins to break down for extreme AR events.

Line 266: "… a suitable representation of reality", this is a subjective acceptance criterion, and the authors should note it as so.

Line 282: Figure 6 is mentioned before Figure 5.

Line 332/Figure 5: The initial peak on Feb 26 is not captured. Can the authors provide some comments and reasoning behind this?

Line 339: This comment is applicable for this section and for other sections. There are several choices that need to be made by the user, such as the LISFLOOD parameters. This raises the question of the PARRA method's applicability to other locations. Does the whole framework need to be re-calibrated with local data for other local case studies?

Line 352: There is no information on which surrogate model was used, and what the accuracy or efficiency of that surrogate model is. In general, there is very little information on this emulation method or how it is used. How can other readers re-produce or build on this analysis if this critical information is missing?

Line 440: On correction factors - Again, this seems like a subjective fix for applying the PARRA framework in this region. How can the framework be applied elsewhere using this methodology? Although the framework seems to be useful for Sonoma County, how can the authors show that the methodology can still be efficiently applied for other locations? Some ideas that address this question can be helpful in accepting the PARRA framework as a generally usable framework.

Figure 9: The distribution just barely captures the observed event in its tail. As mentioned above, how is this result with its uncertainty reported to a manager or a stakeholder? What is the provided answer here?

Section 4 Results

Line 477: Equation 6 - Is this equation reported anywhere in the literature? Seems like another subjective criteria that the authors implement. There needs to be more information describing this choice.

Line 482: The AAL is an interesting concept to describe the average annual losses. However, it is known that this region experiences significant swings between wet and dry years. Is it feasible for the authors to calculate what the AAL is for wet vs dry years?

Line 487: What are the uncertainties around these estimates? Do the authors provide this?

Line 522: the word ‘the’ is duplicated

Line 524: ‘Expected benefits’ - See Massoud et al. 2018, who did a similar analysis for groundwater and investigated how changes to decisions in managing water resources can impact expected changes to groundwater storage. Studies like this are starting to populate the literature.

- Massoud, Elias C., Adam J. Purdy, Michelle E. Miro, and James S. Famiglietti. "Projecting groundwater storage changes in California’s Central Valley." Scientific reports 8, no. 1 (2018): 1-9.

Line 529: Take out the words ‘is of the 2019’.

Section 5 Discussion

Line 535: I would argue that these insights are helpful for planners, managers, and engineers, yet not so helpful for purely scientific investigation since many choices in the framework are purely subjective. I think it is important for the authors to make this clear throughout the paper.

Line 543: Another process that can matter here is the role of sequential ARs (i.e., multiple ARs occurring sequentially), something to consider for 'future directions'.

Line 557: Yes, but what did this do to the expected accuracy of capturing the relationships? The framework is trading potential accuracy and confidence for computational efficiency. This introduces even more uncertainty. The authors should state this.

Line 582: Component Model Alternatives - This is where some of the subjective choices of the framework can be replaced with more objective choices, and therefore can make the framework more sound for scientific analysis.

Line 589: the word ‘the’ is duplicated

Line 591: the word ‘underlying’ is duplicated

Section 6 Conclusions

Line 594: Is it possible/feasible to test another case study event that the PARRA framework accurately estimates the damages for? This can help show case the value of the PARRA framework even more than just showing the one case study from 2019 that was barely captured in the tail of the distribution.

Line 608: ‘… event fell within the expected probabilistic range …’, In the tail of the distribution. It was barely captured. The authors should be careful with how they communicate the accuracy of the provided result.

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-337/nhess-2021-337-RC2-supplement.pdf