

Review of “Drought impact in the Bolivian Altiplano agriculture associated with El Nino Southern Oscillation using satellite imagery data”

The authors of this manuscript blend a variety of in situ and satellite-based data to assess the impact of drought on crop yield in the Bolivian Altiplano. Overall, I found the paper to be well written, with excellent figures. However, I think the paper lacks direction, with the aims and objectives rather unclear. Looking at the conclusion, the main outcome of the paper is to demonstrate the link between ENSO and agricultural drought in the Bolivian altiplano, but it is not apparent from the introduction that this will be the main subject of the work. Overall, I think a more comprehensive introduction is needed both to establish the main purpose of the manuscript, and to place the work in the context of other such efforts to use satellite-based data for drought risk management. As other reviewers have pointed out, the authors could use the introduction to establish a theoretical framework of risk (e.g. risk = hazard * vulnerability * exposure) and drought (meteorological, agricultural, and so on). Readers unfamiliar with this part of the world may also benefit from a discussion of the ENSO phenomenon, the mechanisms through which it may cause droughts, and the challenges of predicting its likely effects.

Section 2.1 (Ground data and satellite imagery) would make more sense as two sections describing (i) meteorological data; and (ii) land surface data (i.e. NDVI and LST). Additionally, I would suggest separating the methods section, and providing a short overview at the start of this new section of the overall approach.

The satellite air temperature data used in this study has a resolution of 0.5 degree, which is relatively coarse compared to other input data, especially considering the variability in elevation in the altiplano. Could the authors clarify whether this data was downscaled using a lapse rate?

MODIS data would provide LST and NDVI at a much higher spatial resolution (500m versus 1/12 degree) but for a shorter period (2000-present versus 1981-2015). The authors should justify their preference for temporal coverage over spatial resolution. My concern here would be that at such a coarse resolution the agricultural signal may be much smaller than the signal from natural vegetation, and hence the analysis may not adequately account for aspects of on-farm water management such as irrigation, mulching, and crop selection – or indeed not growing crops at all and seeking off-farm work. Perhaps this isn't an issue for this study; regardless, the authors should allay the readers concerns.

To add weight to the argument presented by the manuscript, perhaps the authors could consider showing yield data for the study period for the crops in question? This would go some way to establishing the link between drought and agricultural risk.

In the conclusion, lines 7-9, I think the authors may be confusing risk with hazard exposure, and socio-economic vulnerability with risk. This is one reason for establishing the theoretical framework for risk/hazard/vulnerability/exposure at the outset (see previous comment above).

In the Conclusion, the authors state that “Through empirical research with climate variables on the local scale our approach can enable a proactive approach to disaster risk management against droughts.” I think this requires elaboration – it's not actually clear how the work in this manuscript could contribute to such a system. The main result of the paper appears to be to show that El Nino years are associated with more severe droughts in the Bolivian Altiplano. This comes back to the lack of clarity about the purpose of the manuscript at the outset.