Natural Hazards
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Discussions

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Interactive comment on "Improving early warning of drought-driven food insecurity in Southern Africa using operational hydrological monitoring and forecasting products" *by* Shraddhanand Shukla et al.

Anonymous Referee #2

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This paper applies a recently developed hydrological forecasting and monitoring system (NHyFAS) to drought early-warning in Southern Africa. This forces a large-scale hydrological model whose parameters depend on global datasets, with 1) observation data and 2) a multi-ensemble forecast. These forcings input in the hydrological model provide monitoring and forecasting hydrological metrics that are then correlated with crop yields to assess their performance as early-warning signals of drought in Southern Africa. Rootzone soil moisture (RZSM) is used as the main hydrological variable for both monitoring and forecasting. With harvest starting in March, authors use moni-

toring variables available in early Dec, Jan, Feb, Mar (i.e. up to 3-4 months in advance) and monitoring in early Nov and Jan (i.e. up to 4-5 months in advance). Authors test the efficiency of these RZSM products, first on the 2015-16 drought event (with dramatic repercussions on the prices of staple foods) and then on the whole 1982-2018 period (36 years). They show that the proposed forecasting products could have forecast the food availability crisis in Southern Africa in 2015-16 up to 4-5 months before the next harvest starts. They then go on to show that if products are in the lower tercile, there is a high confidence that crop yields will be below average months in advance. Their conclusion is that the proposed products will improve early warning systems of low water-food availability.

The paper's results are interesting, very relevant to this journal and timely, at a time when such early-warning systems for drought conditions are viewed as a priority in Africa (see Nature https://www.nature.com/articles/d41586-019-02760-9). Yet, the text is marred by unstated assumptions, the lack of comparison with existing early-warning systems, and the absence of rationale to explain the results' performance. In particular:

1) The work provides evidence that the proposed products correlate with crop yields, but as the authors know, correlation is not causation. Authors should discuss evidence in the literature of what key variables the forecasts pick up (ENSO maybe?), or alternatively, what supplementary work is needed to establish causation, and therefore, credibility for the products their propose. 2) Other forecasting systems for the area are evoked (Sheffield et al 2014, the African Flood and Drought Monitor (lines 103-104)), why not compare results with those obtained with other products? A justification should be provided in the introduction. 3) If forecasting systems are unavailable, authors link food security crises with El Niño. So that's a simple, well-established indicator (the ENSO index) whose predictive power could easily be compared with that of the RZSM-based products. 4) This journal is an interdisciplinary forum around natural hazards such as droughts and not a hydroclimatology outlet, so authors should make their methods more accessible. A figure of the workflow could help, and so could extra ex-

planations along some of the acronyms. 5) Likewise, justifications for the selection of the key variable (RZSM) or of the forecast ensemble, among others, should be provided to help the paper to be understandable by a larger audience.

I would advise a careful, rigorous revision accounting for the remarks above and where at the minimum, the products' performance should be compared with that of ENSO. If the products work mainly because the forecast ensemble picks up the state of the ENSO index, is there added value to that work:

There is no mention of model/ code / processed data availability for this study: all data sources are the raw data that was used into NHyFAS.

Some detailed comments:

Abstract: it should be made clear in there that the RZSM products are derived from the new NHyFAS. It reads like that they are not.

lines 39-43: authors aren't obligated to show a graph (also that is helpful) but they should cite references.

Line 137: the choice of RZSM as a hydrologic variable of interest makes sense but a rationale should be provided for it being the main (or indeed only) variable of interest in this study. What justifies not using other variables.

Figure 3, and commentary lines 230-250: this seems needlessly confusing. My understanding is that Fig 3 shows the correlation of crop yield with three monitoring-based products whereas the text touts the superiority of the forecasting-based product on all three as early as November. The latter, as well as one of the three monitoring products, includes RZSM, and the distinction is not always clear on first read. Besides, back-and-forth with Figure 4 doesn't make the reading easy either. Could it be a good idea to 1) include the forecasting-based product on Figure 3 to provide a striking visual of why the proposed product is better, and 2) separate comment on Figure 3 from that of Figure 4.

lines 246-249: this should be clarified and explained in Section 2.

Line 266: why the lower tercile? Please justify.

Interactive comment

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