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Discussions

Interactive comment on "Tropical drought risk: estimates combining gridded vulnerability and hazard data" *by* Alexandra Nauditt et al.

Anonymous Referee #2

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Review of "Tropical drought risk: estimates combining gridded vulnerability and hazard data", Nauditt et al.

This manuscript proposes a method to map drought risk in small to medium sized tropical basins, using mainly globally available gridded datasets. The objectives the paper sets out to are of interest, and the need for well-developed methods to assess drought risk to support water management decisions is clear. However, despite these objectives and needs, I feel the scientific merit and method development presented to be weak, with many of the results appearing to be somewhat trivial. It is also somewhat unclear what the role is of the four cases presented. On the one hand the paper presents a method to assess risk, and then presents these results of the method applied to each of these cases. However, the paper does not explore the strengths and

weaknesses of the proposed method, critically reflecting on limitations or in places simplistic assumptions, and does not employ the four cases to underpin such a critical assessment through for example a comparative assessment. Such comparative assessment that goes beyond an enumeration of the results could add some merit to the paper.

I have several concerns.

First the concept of risk that the authors present is somewhat confusing. In line 54 the concept of risk; constituting hazard, exposure and vulnerability is presented. That concept I agree to, and also aligns to the concept of risk commonly used in drought (and flood) risk assessments. See for example the recent World Bank guidance on drought risk assessment: https://reliefweb.int/report/world/assessing-drought-hazard-and-risk-principles-and-implementation-guidance. In the rest of the paper it would, however, seem that exposure and vulnerability are used interchangeably. Indeed there is some discussion on this around line 100 of the paper, but I fail to understand how in the context of the method presented these can be simply interchanged. I will discuss this later when exploring some of the characteristics of the indicator.

Another inconsistency is the importance of infrastructure, which is introduced as an important contributor to drought risk. In the abstract it is noted that this is related to water infrastructure. However, in other parts of the paper it would appear that this is road infrastructure (see Table 2). The source of the data is also unclear. In table 2 it is noted to be the CIESIN data, but this is the gridded population of the world dataset, which to the best of my understanding does not contain data on infrastructure. Also the data has a resolution of 30-arc seconds, which is about 1 km at the equator. This raises several issues on scale, as the classification classes proposed in Table 4 suggests several categories at scales lower than 1 km, which if the scale of the data used is on the order of $\sim$ 1 km means that there is insufficient resolution to support such a detailed classification. The resulting map of the Upper Magdalena basin, where only major roads seem to be considered, shows that this leads to a resulting map of the contribution to vulnerability that is either very low, or very high. The scale of the data used for the Tempisque is, however, quite different, and there seems to be a very high infrastructural density. What is curious though is that the population maps of the Magdalena shows that the City of Bogotá is located within the basin (which is confirmed by the coordinates), which has a dense road network commensurate with a major city of $\sim$10 Million inhabitants. The comparison of these two maps suggests there are some major scale issues in the underlying data and what these represent. This would raise some major questions on what the overall index represents and how this can be compared between basins.

I also have some major issues with the structure of the index, which I think has major flaws. I will consider first the vulnerability index and then the hazard index. For the vulnerability index five factors are considered. However, several of these would appear to be highly correlated. For example population density and GDP. The maps for the Magdalena show these to align almost perfectly. Also the density of infrastructure is closely correlated, as it is somewhat trivial that there are more roads in densely populated cities. This means that thee of the five factors considered to contribute equally may well have a very high correlation, and thus dominate the result. In several analyses of vulnerability indices, techniques such as PCA may be applied to reduce the dimension of the variables considered, which may well be useful here. The authors also note in the paper that they find a central tendency of the index, with little evidence of severity category 1 or 5. This is I think primarily due to the trivial nature of the indicator. A simple thought experiment illustrates this. Imagine a basin with a pristine forest area, untouched by humans without any infrastructure or crops or livestock. This would result applying table 5 in values of 1,1,1,5,5 respectively, and therefore a value of the vulnerability indicator of 2.6. So what does that mean? A fully natural area has a vulnerability of 2.6? I agree that this may well be due to the equal weighting of categories, which immediately raises the question of why that choice was then made. The resulting maps show there is indeed little resolution to the index. It is also not clear in Table 4 how the classes chosen are motivated, and indeed validated. These seem

to be somewhat arbitrary.

Similar doubts can be raised for the hazard indicator. The authors note that the widely applied assessment of drought at monthly time scales is flawed for tropical catchments. One reason for this is given in Line 70: A (sic) few days without rainfall might lead to a severe precipitation deficit that can affect cattle grazing and rain-fed agricultural. I find this somewhat suggestive, and it is not further substantiated. I agree that for some crops the occurrence of e.g. dry spells, which is the more commonly used term in literature of a sequence of dry days during the wet season, may have significant impact on yield. But generally this would be more than just a few days, often a dry spell is considered in excess of 5 days, or sometimes 10. I would argue that this would depend very much on the crop, and local conditions such as soils. In the hazard indicator, a division is then made of a short duration and a long duration event, which indeed also considers longer periods; so how does that reflect back on the argument of the need for an assessment at daily scale. However, it would appear to me that the selection of the length of period is somewhat arbitrary, and the same thresholds are applied for all four cases. I would think this should depend somewhat on the variability of the climate? I think it would be good to explore the distributional properties of a climate. I would expect the distribution of the climate of the Tempisque and to be quite different than the Upper Magdalena, with the latter having a much lower coefficient of variation. The same holds I am sure for the other catchments though I am myself less familiar with their climate regimes. A similar discussion can be extended to the hydrological indicators. These are considered across different periods to the meteorological indicators, but again choices made seem somewhat arbitrary. There is no consideration of autocorrelations, which for discharges during low flow periods would be expected to be quite high, in particular in large basins such as the Upper Magdalena, and lower in small basins such as the Tempisque. Given these strong autocorrelations, it is unclear to me if there may be some form of double-counting (or are all short duration events that coincide with a long duration event removed?). All these details on the construction of the index would need to be clarified.

It is also unclear to me how anthropogenic influences are taken into account. If I understand correctly, the hydrostreamer approach used distributes the hydrological outputs of a global hydrological model given the temporal resolution of a gauge, which may be influenced by the operation of a hydropower station. Does this then translate to the same distribution (temporally) upstream of the gauge, and therefore perhaps upstream of the reservoir? That is not clear to me, and raises questions to how representative that then is of upstream drought hazard? Also the index does not consider temperature (evaporation), which in the drier basins may have an important impact.

Other remarks on the method are logically on the equal weighting of the constituent parts of the indicator. The sensitivity of these weights is not explored anywhere in the paper. I realise that the authors suggest that in all four basins local experts have corroborated the results. However, I do think that it is very unclear what that corroboration actually constitutes. Was some methodological approach chosen to validate results found? What benchmarks were used? Were local data on e.g. impacts used? I also do have many much more detailed remarks, where there are minor flaws in writing, style and presentation. Units are not always correct (check Table 4, cropland and population columns), and at times quite suggestive claims are made. For example, in line 135 the authors claim that: Available discharge observations data in the study regions (Figure 1) do not allow 135 to display the spatial variability in hydrological behaviour. However, in the Upper Magdalena they report to have 46 stations in a 49382 km2 basin. This translates to a density of one station per 1000-2000 km2. I would argue this is very reasonable, if not even reasonably high. The Muriaé has a similar density, it is a little lower for the Tempisque and indeed much lower for the Srepok. There are many other such remarks that are made by the authors that seem somewhat suggestive.

Concluding, I think at face value the paper seems to present an interesting analysis, but when digging a little deeper there are many methodological issues, and in my opinion raises more questions than it answers. My recommendation would therefore be to not consider this suitable for publication in its current form as it lacks a well-developed

scientific analysis.

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