

Dear Jan Sodoge and co-authors,

we appreciated the opportunity to jointly review your manuscript. Please find our (by Kerstin Stahl and Kathrin Szillat) comments attached below.

Kerstin and Kathrin

Contribution and General Assessment

The presented study aims to improve the "understanding of **drought impact dynamics** during increasingly frequent multi-year drought periods". The present work analyzed the patterns of socio-economic drought impacts during two single-year events (2003 and 2015 in Germany) and one multi-year drought event (2018-2022 but excluding 2021). The study used a dataset from media-sourced drought impact statements (DIS) for that analysis. The main novel contribution (and method) introduced is the DIP (drought impact pattern). The way of analysing drought impact statements is new and we found it generally suitable and interesting for an NHESS readership and really enjoyed reading the paper. The methods appear to be valid, but we do see some need for clarifications. The manuscript is well written and mostly logically presented. Some sections where we see need for improvements are pointed out in the comments below. Also, we find the conclusions to require some more precision; in particular the title's promise to have uncovered 'unique dynamics' should be toned down and better adjusted to the generalisation that can really be drawn from this one multi-year drought in comparison with (only) two single year events.

Major Comments

1) The title is inaccurate. An assertive title claiming to have uncovered (generally) unique dynamics of multi-year droughts (Plural) is a very strong statement that is not supported by the very limited case study and analysis of only one multi-year drought vs two single year events in only one country. This title suggests a much more globally applicable discovery which has yet to be shown. The title has to be adapted.

2) The three aims are somewhat different in their levels of analysis depth and their interdependence. 3 relates to 1 and 2 and is hence not independent. Also we wonder why only land cover? There are many vulnerability studies around that have shown different sensitivity and adaptive governance aspects besides land cover to matter for drought impacts. The aim must be phrased more as one possible example and needs to be discussed in light of other factors - with suitable acknowledgement of those other controls that have already been shown.

3) The method section was challenging to fully follow for several reasons. It needs improvement to appeal to a wide readership.

a) the description is partly difficult to follow without reading the paper by Sodoge 2023. A bit more on that data would be useful - maybe just an example of a DIS?

b) the structure of the method section is in parts not clean and logic. Some important aspects e.g. in the unnumbered lead text under 2.2 then come up again later etc. This needs considerable editing so that everything is in logical order and only in one place under a clear subheading or clearly introduced paragraph.

c) the DIP clustering is not sufficiently specified. Add equation for the Euclidean distance mentioned somehow in passing in 123. We could not fully follow what exactly went into the Ward algorithm and the elbow method - what exactly is 'height' in Fig. A5?. This algorithm also has some disadvantages that need to be discussed.

4) The correlation with Google Trends: is this really an independent validation dataset? Does it not use the very same media articles that were used to assemble the DIS dataset? If this is the case it cannot not be used for this purpose.

5) The results section contains many different interesting results. These are difficult to appreciate, however, due to a lot of discussion already mixed in. There is a reason why a strong separation between pure results and discussion is generally recommended: it is much easier for readers to see the results of THIS study and hence they will receive much more and more clear appreciation. Interpretations based on literature in particular need to be moved to the discussion section in many places (e.g. lines 132 - 245 contains more discussion than results; or 290ff - either phrase this as a hypothesis to test (methods!) or discuss in the discussion - here it clearly waters down a clear picture of the results and takes the attention off THIS study's achievement).

6) The discussion (and preferably also the analysis, but this may be asking too much) lacks a more quantitative and thorough consideration of the uncertainty, e.g. of the limited sample of events, of the DIS classification (shown with independent data, but are their alternatives to those as well?) and its propagation into the derived DIP patterns? At least a theoretical elaboration, e.g. based on the numbers of correctly/incorrectly classified DIS and the DIP splits or so is needed to stake the limitations. Also the limitation in the base data from Sodge et al., 2003 need to be considered more here (esp. given that we don't learn so much about it in this manuscript). Overall the discussion section could be more clearly structured and should contain more specific aspects (see comment about moving discussion wrongly placed in results).

Specific comments

1. line 23 missing separator (visualization techniques statistical tests)
2. line 62 sounds like those refs only looked at ag. and for., but in fact they looked at more - could be phrased more precisely - overall there is a lot of overcitation with lists of papers the exact contribution of which to the argument is not sufficiently clear - I suggest to weed this out a bit and make the referencing a bit more specific. Otherwise it is not useful and in fact often wrong!
3. line 70 grammatically it should read either "period from 2018 to 2020 "or "period between 2018 and 2020" or "the period of 2018-2020"
4. line 72 land cover is related (singular required)
5. line 75 "Germany-wide"?
6. 75ff three aims and beginning of methods partly redundant - I would find it easier to merge the two at the end of the intro and directly start with 2.1 Data under 2.
7. 86f this is an example of where perhaps a bit more info is required on the data set. Fires are in general mostly ignited by careless people ...a bit more on assumptions/definitions etc. for the reader to get a feeling for the level of determination in the data would be appreciated.
8. 98 'agriculture in Berlin' - in a city? Is this really a convincing example?
9. 132 multi-year drought events? only one event was analysed, so this should be singular. Also see major comment.
10. 140ff define p and i separately and clearly, rel abundance p for each category i?
11. 160 these equations likely do not concur with Copernicus rules for mathematical notation. Define symbols according to those and then use those in equations.
12. 176 Phrasing statistical testing as hypotheses would not only be correct and more clear but also save long somewhat difficult to read text. Suggest to improve.
13. 265ff The placement of the figures is not great for readers and even a bit confusing as Figs 6 and 7 belong together and not Figs 5 and 6.

14. 314 Why is the link not intuitive? There have been so many news on wilting city and park trees in those years that for example these waterbag initiatives started... Urban trees are most affected. There should be literature on it - we suggest to take a look at the actual text of a few DIS. In fact this would be really helpful overall - cite from your data. This is the great advantage of having text data providing the explanation - it doesn't have to be found through correlations with uncertain land cover classification (to put it provocatively) but it is right there to read.
15. 337 This sentence is a bit misleading. The presented work doesn't demonstrate that (you did it in the Sodoge et al. 2023 paper referred to), but how so-derived impacts can be further processed (in theory the analysis could have been done with other impact data as well). The more precise such sentences are the better will be the further use (and) impact of the study!
16. 409 The conclusion is good. But the last few sentences need to be revised with respect to our criticism regarding the title. Also, did the study really demonstrate HOW land cover controls impacts? Or so far mostly 'THAT' with some suggestion for the HOW that lead to the 'more context specific' conclusion?
17. Figures:
18. Fig 1 has very small font sizes and we wonder if it is really needed? The Caption "overview of methods..." suggests a bit more illustration on methods rather than just naming them. At least a conceptual illustration for the three lower boxes as well would be useful then to really see added value (a schematic cluster tree etc.)
19. Fig. 3 suggest to make this a full page (maps are too small)
20. Fig. 4 ID - was this defined as a term?
21. Fig. 8 Font sizes are too small
22. Some minor mistakes in Table A.2 (missing commas)
23. Figure A.2 (peak is in August)
24. The list of references misses doi's. Check the journal's style guide.