

## ***Interactive comment on “Hydroclimate variability in Scandinavia over the last millennium – insights from a climate model-proxy data comparison” by Kristina Seftigen et al.***

### **Anonymous Referee #1**

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The manuscript presents a novel reconstruction of soil moisture availability for Southern Scandinavia and compares it with CMIP/PMIP models and a regional temperature reconstruction. The key questions are, if the two proxy reconstructions and the GCM output show similar climate variability and patterns and to investigate possible drivers of hydroclimate variability in the study regions. While general patterns appear to be similar, there are considerable mismatches at most time scales and the authors raise the issue, that multi-decadal hydrological variability might be underrepresented in the GCMs, with substantial impact on future projections of hydrological extremes.

The paper is well-written, comprehensive and I recommend it to be published. It raises important questions about the ability of GCMs to represent regional hydroclimate cor-

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rectly and the discrepancy between proxy reconstructions and GCM output is a crucial problem to assess the performance of both reconstructions of hydroclimate as well as GCMs to predict future developments.

In general I find some formulations too strong and not fully supported by the presented analysis. This is in particular the case for formulations of over/underestimation and biases in the model output in comparison to the SPEI reconstruction. When comparing two non-perfect representations of a variable these terms are in my view only justified if one also includes observational data or if there is compelling evidence that one of the two is a better representation of physical reality. At most parts of the manuscript suggests that the proxy reconstruction is an accurate representation and the discrepancy is mainly due to an inability of the GCMs to reproduce these features. A better way would be, to just state a difference in variability, possibly followed by an assessment of both representations. The authors state in lines 561-562, that it is not possible to attribute the disagreements between the reconstruction and the models to one side. This is in disagreement with the rest of the paper, which blames the models. A reasonable formulation is found in lines 544-547 and I would have liked to see similar remarks earlier in the text.

This can probably be easily resolved by reformulating certain statements. In the current form, I found it widely irritating while reading. Some examples of statements which I find too strong (there might be others) are:

- l. 19-20, l. 315-318, l. 460-465 and l. 532-533: I would like to see a critical assessment of the variability of the SPEI reconstruction and a deeper comparison to the expected time-scale depended variability, also from observational data. Just comparing the model output to the reconstruction is in my view not enough to conclude a bias on the model-side.

- l. 22-23, l. 520-522: The formulation implies, that a positive correlation on multi-decadal scales is also found from observational evidence. This is not provided in the

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manuscript, but is crucial to determine if the mismatch between proxies and models is mainly due to GCMs deficiencies. Thus, much of the paragraph I. 518-526 is formulated with a bias towards a correct representation in the proxy reconstruction. The fact, that the multi-decadal variability is much stronger in the reconstructions can have many reasons (some of which are also discussed in the manuscript), but to claim a bias in the models from this fact alone is a bit strong.

### Specific Questions

- I. 215-216: It is not immediately clear, which period is the validation and which is the calibration period.
- Fig 3: It would be nice to include (at least for the high-frequency part) observations into this plot, to be able to judge both the proxy and the model performance. In Fig. 3 (d), adding the markers to the legend would make it more intuitive to read, especially for monochromatic prints.
- Fig 4.: I found it a bit confusing that the order of the columns does not correspond to the order in which the variables are discussed in the text. I would recommend adjusting the order accordingly.
- Repeatedly the formulations seem to imply that hydroclimate and temperature/precipitation are independent variables which one can "contrast" or "compared" (e.g. I. 227). While the first is rather a combination of the two and thus one is not comparing them, but rather investigating, which factor is more dominating.
- As a reader who is not familiar with SPEI it is hard to follow what this variable does and what its dependencies are. I would have liked to see the formula that is used in this study, possibly in the Supplementary Material.
- I. 235-236: While the two time series are coming from different data sets they might still share common signals and might not be totally independent. Given the rather low  $r^2$  of 0.2, one could also argue that the low-frequency variability of the SPEI in-

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dex, which is a combination of precipitation and temperature, is simply dominated by the temperature component, which would lead to similar multi-decadal variability with ScandT14.

- l. 309/310: A reference to the Supplementary Material Sec. S1 could strengthen this claim, even though it only applies to the inter-annual time scale.

- Sec. 4: I found the title misleading, as the only external forcing discussed is volcanic eruptions, while other forcings like solar variability are not mentioned. Please revise the title.

- l. 432 ff: I found this paragraph a bit confusing. While it begins with comparison of temperature and precipitation the results are about temperature and SPEI. As SPEI is a mixed variable, which also includes temperature, it is not clear, how one can draw a connection to a T-precip relationship. In general, it seems like SPEI and precipitation are used interchangeable here, which they are not.

- l. 448 ff: Can you quantify the relationship between ScandH17 and ScandT14? While the multi-decadal co-variability is clearly visible by eye this is not the case for the inter-annual values. In both cases it would be nice to have quantified values (including significance).

- Sec./Fig. S1: I'm a bit lost here. How can the annual mean be overestimated if all monthly means are underestimated?

#### Technical Corrections

- 387 [...] superior [to] TRW [...]

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