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Interactive comment on "Coupling between the North Atlantic subpolar gyre vigor and forest fire activity in northern Scandinavia" *by* Tine Nilsen et al.

Anonymous Referee #2

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General comments ——————

The manuscript by Nilsen et al. is proposing to evaluate to what extent variations in the North Atlantic subpolar gyre might influence atmospheric conditions over northern Scandinavia and contribute to drought and associated risk in forest fire. For this purpose, the authors based their work on a three-member ensemble of last millennium simulations from MPI-ESM climate model. They focus both on summer and winter droughts, since both of them can be of importance for the onset of fire in this region as suggested by recent observations. They do not find any statistically robust linkages between extreme drought and vigor of the subpolar gyre (SPG), leading to reject the

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hypothesis they were testing, based on these model simulations. On the opposite, they suggest more robust linkages of droughts with winter NAO and little ice age conditions.

This is an interesting study and hypothesis to be tested. Nevertheless, it was pretty clear that interannual drought extremes (which is what the authors are looking at in the end) would not be necessarily related with SPG variations, since very few literature is proposing such a link. On the opposite, variability in the atmospheric weather regimes was clearly a more natural candidate, so that it is a bit surprising to spend so much time in the manuscript on a hypothesis that was clearly not straightforward. While I am fine with papers showing "negative" results, they need to be well-substantiated by former literature stating the existence of a link which is maybe not robust. Here, this is not really this approach, since the authors seem to choose arbitrarily a hypothesis; they show it does not work, but they do not try an alternative hypothesis to end up with a story that really brings sufficient new scientific knowledge.

This caveat is very well illustrated by the fact that the paper which is proposing a model analysis, with potentially a lot of analysis given the number of fields available, is in the end proposing only two kinds of figures (Fig. 2 and 3 being just the replicate of Fig. 1, and the same for Fig. 5 and 6, replicate of Fig. 4). In fact, Fig. 1-3 and 4-6 can be clearly combined into two figures. Indeed, considering all the three members together can be done within the same analysis of extreme, since they are supposed to reproduce different occurrences of the same climate conditions (only differing by their internal variability, i.e. noise and extreme). Indeed, for leading an analysis of extreme events it is important to consider a large enough sampling, but there is no reason to separate the three members.

On top of this major point, which prevent publication from my point of view, since the manuscript and the analysis are clearly not mature enough, I have a number of specific comments that are explained below.

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Specific points: ——————

- l. 32: “high predictive potential”. Of what? Can you please be more specific?

- L. 37-38: “Weaker circulation…” the statement is only true in winter according to Moreno-Chamaro et al. (2017) at least. It would be interesting to insist on this.

- L. 47: remove “testing” after hypothesis.

- L. 74-78: Since you are focusing on drought, it will be useful to better know how is the land surface hydrology is represented. This could be key for soil moisture content and thus drought conditions.

- L. 86-87. The MDC is a soil moisture index, but apparently it is only based on atmospheric temperature and precipitation, while hydrology within the soil and interaction with vegetation might play a role. I assume soil moisture is a variable from the climate model, so I am wondering why the authors do not directly use this variable, which is more representative of the exact processes at play in the model (e.g. hydrological).

- L. 119-124: Since the author are mainly interested in oceanic impact on the atmosphere, I am wondering why they do not directly use ocean heat transport here, which might combine AMOC and SPG transport, but also Ekman part, and might be more physically enlightening, and offering a better process-based understanding of what is going on in their simulation (i.e. more objective and physical approach).

- L. 132: AMJJA or JJA: why is it changing depending on the member?

- L. 135-139: it would be interesting to also see the response of sea ice in order to provide a more complete description of the conditions associated with the droughts. Other variables might be considered to have a more complete view of the processes at play. In fact, the main hypothesis to contradict is: drought conditions are driven by stochastic interannual variability in the atmosphere. Given what is shown afterwards it is not clear that it does not hold. See next comment.

- L. 163-179: When looking at Fig. 1-3, it appears that the only robust oceanic signal preceding the droughts is the heat flux anomaly. Indeed, SST and streamfunction conditions are really not consistent among the different members. Then, this might suggest that the main processes that play a role (apart from chaotic variability in the atmosphere) is the release of heat by the ocean (whatever the process leading to it, e.g. increase of wind, anomalous SST, etc.). Then, it is unclear if it actually leads to the atmospheric conditions over the North Atlantic leading to drought (it can be just a necessary condition, on top of particular atmospheric circulations), or if it is the combination of the specific atmospheric conditions and anomalous ocean heat fluxes the few months before (and what about during the event?) that leads to the drought conditions. More cross-correlation analyses for instance will be helpful to correctly decipher the processes at play (possible if you leave aside the SPG hypothesis on which it is not clear why you focus). Having a more objective approach would have benefited the study and could have offered a clearer view of the processes at play for drought conditions.

- L. 180: "proposed mechanistic coupling": not clear to me what this refers to. Can you please be more explicit? I feel that there is indeed a lack of mechanistic approach in this study, or at least too superficial.

- Discussion section: I am wondering here why you focus on interannual variability, while 10-year drought conditions might have had stronger linkages with the SPG (e.g. your hypothesis). Is it because such low frequency variability in drought have little influence on fire conditions?

- L. 193: "is likely": This word has a strong meaning in climate science due to IPCC influence. Here I do not feel your demonstration was sufficient to use such a wording since I see no clear demonstration of the subsequent proposed processes, nor supporting references.

- L. 199: "consistent oceanic heat loss": can you specify where?

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- Table A1: there is a weird “40” in the second row on the right handside.

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