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Interactive comment on "Variability of daily winter wind speed distribution over Northern Europe during the past millennium in regional and global climate simulations" *by* S. E. Bierstedt et al.

Anonymous Referee #2

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General comments

This paper proposes to understand the physical driver of variability of the distribution of daily winter wind speed over Northern Europe. For this purpose, the authors use climate models simulations and reanalysis products. They use three global climate models simulations, two regional model simulations, one global climate model reanalysis and one regional climate model reanalysis. They focus on a few classical potential driver for wind speed: mean regional temperature, meridional temperature gradient and NAO index. From simple statistical analysis, they conclude that relationship between local wind speed and these three potential drivers is not consistent among the models, showing especially different behaviours between global climate models and regional ones. They also insist in the final part of the paper on the crucial role potentially played by land-use changes over the last millennium through surface roughness modifications.

The paper's topic is interesting and important for impact study. The results invite to much cautiousness when using model outputs, even though here, the number of models compared remain relatively low as compared to larger model ensemble from CMIP5/PMIP3 or CORDEX. Nevertheless, I have some concerns about the methodology used in the present study.

First of all, I find the description of the simulations not precise enough. External forcing are key elements of simulations, and as its stands the manuscript does not offer any clear way to estimate the forcing present in the simulation. Indeed, the manuscript quite surprisingly ends by arguing that land-use forcing differences among the simulations may strongly affect the results presented. The same can hold for other external forcings. Since there is no figure presenting them, we cannot really estimate this. Moreover, even the land-use change is very rapidly depicted in Fig. 7 with comparison of only two periods from a millennium simulation. This is not enough to prove anything: time series are necessary as well.

Secondly, and this may be related with my first point, I have an issue with the fact that all the statistics presented in the different figures are not provided for same time period, but only for available time period for each simulation. While I understand the authors cannot do very differently for short reanalysis, I think it is necessary to evaluate sensitivity of the results to the time period chosen to compute the correlations. Indeed, some time periods, like the recent one, can be strongly affected by diverse forcings, which may affect the robustness of any searched relationship. For instance, the change in land use, as stated by the authors in their final part, can strongly affect relation between temperature gradient and wind, while it is not temperature that causes the changes in wind but surface roughness. Hence, the authors should compare simulations on similar time period, and we need to know more clearly and quantitatively

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what are the variations of the forcings over this time period. For instance comparing 1655-1999 period for CCLM and 850-2005 period for ECHAM5 (his father) is not rigorous from my point of view.

These two points are strong enough to prevent publications of these results, since the conclusions may be modified when a correct processing of these two caveats (representation of the evolution of the forcing and comparison of correlation on similar time frame) will be achieved.

Moreover, even though the paper is generally well written, I think it lacks precision at several occasions. Please see my specific comments.

Specific comments:

- p. 1481, l. 6: I would add a “Indeed” before “Section 3” for logical clarity

- p. 1482, l. 1: Any reference to support the claim of the first sentence?

- p. 1482, l. 3: “change in temperature”. Where?

- p. 1482, l. 3-5: Paper by Cattiaux & Cassou (2013) shows that this is only true for SLP and less clear in altitude (Z500). Please discuss a little bit the results from Gillet and Fyfe (2013) in regard to Cattiaux & Cassou (2013).

- p. 1482, l. 7: Please remind the reader for which region you say that.

- p. 1482, l. 8-11: I also assume that the time length of available observations is a bit short to satisfactorily assess changes in extremes, which occur scarcely.

- p. 1483, l. 28: There is already a “Nevertheless” in the sentence before, so that this “However” sounds weird.

- p. 1484, l. 1-3: I also think that topographic feature may play a role within regional simulation as compared to coarse global models.

- p. 1484, l. 7-13: Please clarify here that you mainly differentiate global and regional

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climate models.

- p. 1484, l. 22: This first sentence is not very precise. What do you mean by “mean”? spatial or temporal?

- p. 1485, l. 28: There misses a space at “Roeckner et al.”

- p. 1486, l. 7: I think there is a capital for Earth. There is also a missing “”s” afterwards

- p. 1486, l. 21-25: More description of the forcing is necessary. What are the differences of forcings between all the simulations? I assume they may not use the same solar forcing, for instance between the very old run from ECHOG and the recent one from ECHAM6 (but maybe I’m wrong, please clarify).

- p. 1487, l. 6-8: Does the regional models also use the same land-use change interpolated at their grid scale? I assume so, but it is worth better clarifying that. Maybe a table describing the external forcing for each simulation could be useful. Moreover, I cannot find any precise information concerning the exact domain of the regional models, and we need this (can be indicated in one of the tables for instance).

- p. 1487,l. 26: Why do you say “also”, while you do not state before that former reanalysis use spectral nudging from von Storch. This does not sound logical. Please clarify logical connection.

- p. 1489, l. 1: This definition for the gradient is not very precise. Please give the exact boundary of the box used.

- p. 1494, l. 28: what is “(s.”?

- p. 1496, l. 19: Can you give a flavour of what this “underlying physical mechanism” is? (Please be more precise).

- p. 1499, l. 9: Fig. 6 appears before Fig. 5 I think.

- p. 1500, l. 9: What about other time period?

- P. 1500, l. 15-18: Indeed, but this is a strong caveat, that should be analysed in more depth.

- Fig. 1-4: Please use the same time period when comparing different correlations, at least to check sensitivity of the results to the time period considered. So: recent period when including reanalysis and no reanalysis for period before. Maybe, this will give the same results, but this will be more robust and rigorous (role of forcing, stationarity. . .) to evaluate this sensitivity.

- Fig. 5: The legend is not precise enough. Are any smooth applied? Remind the region for the average, can we have an idea of the forcing (I have the impression for instance that Samalas volcanic eruption forcing was far larger in ECHAM5 than in the other, or maybe this is just due to stronger dynamical response)?

-Fig. 7: Please show other period as well.

Reference:

Cattiaux, J., and C. Cassou (2013), Opposite CMIP3/5 trends in the Northern Annular Mode explained by combined local sea-ice and remote tropical influences, Geophysical Research Letters, 40 (14), 3682–3687. doi :10.1002/grl.50643

Interactive comment on Clim. Past Discuss., 11, 1479, 2015.