

## Comment on esd-2021-29

Laurent Li (Referee)

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Referee comment on "Wind speed stilling and its recovery due to internal climate variability" by Jan Wohland et al., Earth Syst. Dynam. Discuss.,  
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This manuscript uses large-ensemble climate simulations to investigate changes of land surface wind speed. Climate simulations are from MPI Grand Ensemble in the CMIP5 framework: a long preindustrial control simulation, historical simulation, future scenarios RCP2.6, 4.5, 8.5, and an idealized simulation with CO<sub>2</sub> increase at 1% per year. The use of large ensemble provides the possibility to separate effects of external forcing from those of internal climate variability. The idealized simulation of 1% CO<sub>2</sub> increase is used to estimate the effect of CO<sub>2</sub>-induced global warming (called dynamical contribution in the manuscript), and to deduce a residual term attributable to other contributors (mainly considered as effect of land-use in the manuscript).

There are two main conclusions. The first one is on the response of wind speed to external forcing. It was shown that CO<sub>2</sub>-related global warming has a very small effect for wind stilling, but the historical land-use and future evolution of vegetation cover are the most important contributor for changes of wind speed. The second conclusion is that the internal climate variability plays a major role in explaining the ups and downs of wind speed at the time scale of decades and these decadal changes of wind speed are much larger than effects of external forcing, whatever its nature (CO<sub>2</sub> or land-use). These conclusions are in line of my expectations.

The manuscript is generally well written. I recommend its acceptance for publication in Earth System Dynamics. I have a few comments that might be useful for the authors to improve their manuscript with a few minor revisions. Some of them are useful for further discussions, so not mandatory to be incorporated in the manuscript.

1. For the decomposition of wind speed changes into a dynamical contribution and a residual term. I think the authors are well aware of the imperfections of this decomposition, but I am still a little unsatisfied by the ignorance of effects related to aerosols, land-sea thermal contrast (different rapidity of warming), and other non-CO<sub>2</sub> greenhouse gases.
2. For the methodology. It is not very clear how to deduce trends for the 20-y periods. This is a quite important issue, since the main conclusion of the manuscript is dependent on this procedure. Please also precise if results are robust or sensitive to any choices of tuneable or predefined parameters in the methodology.
3. The manuscript focuses on wind speed changes in Europe. How about other regions of the world? Asia? North America? Are there any coherent structures across the globe? It seems that observation (e.g. Zeng et al., 2019) also reveals important changes of wind speed in other parts of the world.
4. Can the authors comment on the origin of internal climate variability? At decadal scale,

the global ocean should be the main player. Are there any corresponding variations at global sea surface which can generate such internal variability?

5. Can the authors comment on the possible mechanism that land-use plays a major role of external driver for changes of surface wind?