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Interactive comment on "Climatology of Lyapunov exponents: The influence of atmospheric rivers on large-scale mixing variability" *by* Daniel Garaboa-Paz et al.

Anonymous Referee #2

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The paper provides an analysis of low tropospheric mixing (850 hPa) in terms of finite-time Lyapunov exponents computed from the European Centre for Medium-Range Weather Forecasts (ECMWF) Era-Interim dataset for the period 1974-2014. Two main results are provided. The first one links Lyapunov exponents to the baroclinic growth rate. The second result is a link between Lyapunov exponents and atmospheric rivers. The paper seems to have some potential, but I have difficulties in assessing its quality, because of the reasons discussed below.

First of all, I find the paper very short, in particular for the Results section. The result about the impact of Atmospheric Rivers (ARs) and mixing, which gives the title to the paper, takes 15 lines in the Results Section, and is then discussed in even less

lines in the Discussion. The other result, on the link between baroclinic instability and Lyapunov exponents, takes only a bit more space. For a reader like me, who is not a specialist in atmospheric processes but interested in more general subjects like geophysical mixing, it is very difficult to appreciate the importance of the results, as well as the motivation of some of the choices, like for instance the regions in the case studies. This objection I think is important for ESD, which promotes interdisciplinary research on the Earth system in general. Why comparing specifically Sahara-Morocco and the British Isles? Are they representative of other larger systems? How this result can be interpreted, or used in other studies? Does the link between baroclinic instability and Lyapunov exponent address a specific knowledge gap, or it is an incremental result, or a confirmation? What are the challenges in atmospheric science that can benefit from the results of this paper? The paper should be strengthened in all of its parts: in the Introduction, to motivate more the approach; in the Results, to motivate more the specific choices; and in the Conclusions, to discuss the possible larger impact of the results in terms of the challanges presented in the Introduction. For instance, some information are given about the relevance of Atmospheric Rivers (lines 15-20). Probably because of my (lack of) background, to me however is difficult to understand how the result of this paper specifically contributes to our understanding of the open questions related to ARs. Does this work really advocate as the main perspective the use of Lyapunov exponents for forecasting precipitations in some regions?

My second remark is methodological, and is about the choice of the pressure level (850 hPa). The manuscript mentions tropospheric mixing, but in fact only low tropospheric mixing is analysed. This fact rises some questions:

- What are the reasons behind the choice of the 850 hPa value? - As far as I understand, atmospheric rivers are not located in the low troposphere only. What are the arguments by which mixing at higher pressure levels can be neglected? What is the effect on the conclusions? - What are the limitations for studying baroclinic growth rates at 850 hPa only?

Summing up, I find the paper with some potential but I feel that the text should be extended, or if the format does not allow, at least consolidated. The presentation in particular should be improved, and aimed at establishing a more solid link between the motivations and the perspectives opened by the results. Regarding the analysis, the authors should also provide more arguments for the fact that their calculation is limited to 850 hPa, but the outcome used for discussing phenomena occurring in a region which a much larger vertical extension.

Other comments:

Convection: Convection can play a strong role at 850 hPa. How does convenction is taken into account, or what are the reasons for which it is neglected by the advection scheme?

Title: the subtitle highlights the influence of atmospheric rivers on large scale mixing variability, suggesting a causality (from ARs to mixing) which however is not clear to me in the results. In fact, by reading the manuscript, one has the feeling that the opposite may be also implied. The title should also take into account the fact that Lyapunov exponents are computed for the low troposphere only.

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