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Interactive comment on "Nonlinearity in the Tropospheric Pathway of ENSO to the North Atlantic" *by* Bernat Jiménez-Esteve and Daniela I. V. Domeisen

Anonymous Referee #3

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This paper analyzes the tropospheric impacts of ENSO on the North Atlantic region, focusing on nonlinearities regarding the amplitudes of the events, and asymmetries comparing El Niño and La Niña phases. To do so, they use different idealized simulations with a simplified model in which stratospheric winds are nudged to climatological values to shut down the stratospheric ENSO pathway. General comments: This study extends that from Jimenez-Esteve and Domeisen 2019 who studied the nonlinearities of the ENSO teleconnections to the North Pacific. The authors use similar idealized experiments in both papers except that they shut down the stratospheric pathway by nudging the stratospheric winds to the climatology in the present study. For the reader's interest, it would have been easier to have one single paper on the asymmetries of the

tropospheric ENSO pathway and make the paper more self consistent and not having the reader go back and forth between papers. I detail below some of my concerns with comments to improve the paper, making it more self consistent and complete. Thus, I think the paper is appropriate for publication in Weather and Climate Dynamics after the authors address the comments below. I feel the discussion of mechanisms on the origins of the asymmetries, etc, is reduced to references to Jimenez-Esteve and Domeisen (2019). This is why in a few places I ask the authors to add more information to clarify certain aspects in addition to their reference to Jimenez-Esteve and Domeisen (2019). There are many references to Jimenez-Esteve and Domeisen (2019) but results from both papers are not really compared or discussed. Indeed, the comparison of the results could give us additional information not discussed in this study. The differences in the North Pacific between simulations in Jimenez-Esteve and Domeisen (2019) and the present manuscript must be related to the stratosphere (e.g. comparison of Figs. 2 and 5 in both papers). These differences and possible explanations should be discussed further not only in the Pacific (as it is done in Jimenez-Esteve and Domeisen (2019)) but also in the Atlantic. I also recommend plotting fig. 5 in the same projection as in Jimenez-Esteve and Domeisen (2019) for easy comparison. I see differences in the Atlantic region already comparing Fig.5 of both papers that need to be discussed. As figure 4 in Jimenez-Esteve and Domeisen (2019) compared their modeling results to reanalysis data, here a similar comparison should be made when possible, Figure 2 to 4 (or some of them) from Section 3 and figures 5 and 7 for asymmetries. Similarities and differences between model and reanalysis would give hints about how realistic are the modeling results when comparing the signals in the Pacific and about the relative role of the tropospheric and stratospheric pathways when comparing the signals in the Atlantic Ocean. Several studies have pointed out differences in the timing of the teleconnections in the Pacific and Atlantic Oceans (e.g. early vs late winter). How different are the responses and the non-linearities and asymmetries if we look at individual months or early vs late winter instead of DJF means? No need to show figures but add a sentence in the manuscript. Regarding wording, I find con-

fusing the use of ‘nonlinearity’ in certain places particularly in relation to ‘asymmetry’. I recommend using nonlinearity for differences in the response regarding the magnitude of the events, and asymmetry for the differences between ENSO phases. Indeed, this is the way it was used in Fig. 5 in Jimenez-Esteve and Domeisen (2019) while here it is not. I find this confusing.

Minor comments: L. 30. I believe Bell et al. (2009), Cagnazzo and Manzini (2009) and Ineson and Scaife (2009) are the first ones to discuss the stratospheric pathway in connection to North Atlantic surface impacts. Please add the references. L.35-50. All this paragraph reads too long considering that the stratosphere is not the main focus of the study. I would shorten it and move it to line 75 to connect to the paragraph previous to the last one of this section. L. 53-L.70. The description of mechanisms is confusing. The authors start saying that they focus on the NP downstream effect. Which one is that of the ones described later on? Perhaps listing then as first, second, etc would help. L. 71-75. Which one is the mechanism used in the study? L.107. Please remove ‘As in Jimenez-Esteve and Domeisen (2019)’.. it adds confusion. L.111. When mentioning here the four spatial patterns, please refer to figure 1a. L. 177. This first sentence is not very clear. Indeed the authors analyze these simulations throughout the paper. So the sentence can be improved to focus more on this particular section. Please substitute ‘while relaxing. . .’ by ‘ by relaxing . . .’ L.180. Here and throughout the paper, are the results similar if we look at individual winter months instead of DJF averages? Do we see differences between early and late winter in the teleconnection and asymmetries? (see my general comments above). L.185 Following my comment about asymmetries vs linearity above, I think asymmetry should be used here. L. 232. Can the authors argue about why the response in temperature in EN over Europe is the opposite from a negative NAO? However, for LN the response is as expected, right? Section4 . Perhaps the title would be more appropriate as ‘Spatial distribution of the asymmetry and non-linearity response to the ENSO. . .’ or something similar. For a better comparison with Jimenez-ESteve and Domeisen (2019) please replot the figure with the same polar projection and add the same ‘phase asymmetry’ and ‘single phase

nonlinearity’ to the figure. L. 254. Where do we see in Fig. 5 that the asymmetry denotes a stronger AL/PNA for moderat EN than moderate LN? Individual phases are not shown here. L. 257 ‘EN compared to LN (not shown), and the strong . . .’ L.266. Note also than in observations, the strongest EN winters are not accompanied by SSWs. L. 270. Note that the impact over Europe is linear (there is no signal in figure 5d). However, there is some positive signal in Fig. 5d in Jimenez-Esteve and Domeisen (2019), does this mean that the non linearity in that case came from the stratosphere? This is the type of comparison/discussion that needs to be included. L.298. and paragraph above. Can perhaps the authors explain a bit more about the origin of the nonlinearities (mechanism) here? L. 312. Can the authors include reanalysis data in Figure 7? Similar to the scatterplots in fig. 4 of Jimenez-Esteve and Domeisen (2019)? The comparison would give us a hint also on the role of the stratosphere. . . L. 19. Fig. 4 in Jimenez-Esteve and Domeisen (2019) do not show convection directly. Can the authors elaborate their argument a bit more here? L. 385. I also see a dipole for strong LN in Fig. 2d. L.388-390. I understand it might be difficult to answer, but the authors should discuss and elaborate on why moderate LN forcing has a stronger impact than moderate EN forcing or why there is a saturation effect for LN and not for EN? How all of this compares to observations? L. 392. Where is the sentence ‘. . .although the stratosphere may contribute when it is active’ from? L. 400-410. I find this discussion too long for something not directly related to the paper, as there is no focus on the stratospheric nonlinearities. Please make it shorter.

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