

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-42

Anonymous Referee #1

Referee comment on "Revisiting the wintertime emergent constraint of the southern hemispheric midlatitude jet response to global warming" by Philipp Breul et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-42-RC1>, 2022

Revisiting the wintertime emergent constraint of the Southern Hemispheric mid-latitude jet response to global warming, by Breul et al

This paper revisits the wintertime emergent constraint on the Southern Hemisphere (SH) jet latitude, which relates the climatological jet position to the future jet shift in CMIP models. Specifically, previous work has argued that this constraint arises primarily as a result of a geometrical effect related to the fact that the wind anomalies in the zonal mean are anchored at the same location regardless of the climatological jet position. It is argued here that the climatological jet latitude differences across models are related to inter-model differences in the relative strength of the single Atlantic jet and the strengths of the two jets in the double jet Pacific structure. The authors argue that the zonal mean jet location is not very physically meaningful given that it is an average over these two distinct jet structures. I think this study is tackling an issue that needed to be resolved and I find the arguments somewhat convincing. I do think that some improvement on the connections between their toy model and the actual CMIP model behavior could strengthen the conclusions considerably. At the moment, the toy model is presented as being able to reproduce the relationships that are found in CMIP, but it might be nice if there were a way to connect the toy model to the behavior of individual models a bit more. I've made some suggestions along these lines below, but overall I think this manuscript is acceptable for publication after minor revisions.

General suggestions:

(1) Improving the linkage between the toy model and the behavior of the individual CMIP models. At the moment, the pieces of evidence for the authors argument are (a) there is no local connection between the jet position and jet latitude in the two longitudinal sectors separately and (b) the toy model can exhibit similar behavior to the CMIP models when random values are added to the amplitude of a_1 and a_2 . It's not totally clear to me what a_1 and a_2 represent but I'm assuming it's either the amplitude of the two pacific jets or the amplitude of one of the Pacific jets and the Atlantic jet (see comment below). Anyway, the piece that seems a bit missing is then linking this back to the behavior of CMIP. It seems like it should be possible to then show that there is a relationship between the CMIP zonal mean jet latitude and the amplitude of the relevant jets in a manner that is similar to the toy model. If possible, I suggest the authors

investigate whether these aspects can be tied together a bit better e.g., is the climatological latitude of the jet in each model highly correlated with the amplitude of the Pacific southern jet?

(2) I think the prior work of Bracegirdle et al 2013 doi:10.1002/jgrd.50153 deserves some discussion. They showed that the emergent constraint holds in the Pacific sector and is kind of there, albeit weaker, in the Atlantic and the Indian ocean sector. The big difference here is probably that you are looking at the winter while Bracegirdle et al used the annual mean, but I think it could still be worth discussing this prior work and why your conclusions differ.

Minor comments by line number:

l20: It sounds a bit strange to first cite Simpson and Polvani 2016 and Breul et al 2022 in the context of studies that link the jet shift to annular mode timescale and then cite them in the next sentence to say that these studies couldn't find that constraint. Suggest removing Simpson and Polvani 2016 and Breul et al 2022 from the first lot of citations.

l40: What has motivated this choice of 22 models? Why not use all the models?

l46: "chose" --> "chosen"

l48: I think Kidston and Gerber (2010) used this quadratic method to define the jet latitude before Barnes and Polvani (2013) did. Suggest citing them instead.

l55: Simpson et al (2021) might be the more relevant one here since they used CMIP6, while Simpson and Polvani (2016) used CMIP5.

l56: This correlation is also quite a lot higher than was found for CMIP6 by Simpson et al 2021 (they found -0.57). This is probably due to the different models being used, but some motivation for not including some models should probably be given here.

l90: I'm not sure that "observed response" is the best phrasing here. It could be mixed up with the climate change signal or the climatological jet latitude in the observations. I think really you're referring to the CMIP behavior here?

l97 and 98: I may have missed it but I don't think you've defined a1 and a2. I'm assuming you're defining the amplitude of the three jets with a1, a2 and a3, but it's not clear to me which jets a1 and a2 correspond to.

199: Something's not right about this sentence "and the therefore also" is not making sense.