

Review of egosphere-2025-748 version 2

I thank the authors for their generally thorough address of the comments I made on their initial submission. There are several relatively minor comments I would still like to make, though, which I list by line number of the revised manuscript:

19-20: The sentence “Nonlinear wave–wave interaction drove early wave generation, while the latter played a role near the onset” is confusing. I’m not entirely sure what “the latter” refers to, and it is not clear how different “early” and “near the onset” are. Is the phrase “Nonlinear wave–wave interaction drove early wave generation” a reference to point (i) of the previous sentence, while “the latter played a role near the onset” is a reference to point (ii)? This should be rewritten.

101-103: This statement of the way the Charney-Stern-Pedlosky criterion is being satisfied is confusing. What you mean is “The criterion is met by Q_y changing sign within the domain” but I think the current version of the text could easily be misinterpreted as “The criterion is met by having $Q_y < 0$ somewhere in the domain” which is not a correct statement of the criterion. The text should be updated for clarity.

175: “sum of two terms” should be “sum of the two terms”. Doing another proofreading of the paper is probably warranted.

201: The listed publication year for the Jucker paper about E-P flux scaling is incorrect—it’s actually 2021.

204-206: The discussion in the Song et al. (2020) paper is helpful, but (correct me if I’m wrong) it doesn’t actually say anything about the sine and cosine tapering technique. I was not able to readily determine what was meant by this via a Google search, so some more explanation/appropriate citation to this specific signal processing technique is probably in order.

405: The phrase “the reason underlying the increasing frequency of SSWs with these easterlies, linked to climate change” is a little odd. Is this intended as a claim that the increasing tendency for enhanced stratopause wave driving to precede SSWs is a manifestation of radiatively forced climate change? Koushik et al. (2022) do not actually claim this, so it seems premature to make such an assertion. Alternatively if the intent is merely to suggest that there *may* exist a link to forced climate change, the language should be more tentative.

409-412: This claim should have a citation for it, and may be a bit exaggerated: yes, Aura MLS didn’t yet exist at the time in question but as I noted in my first-round review there should have been some AMSU-A channel 14 data. Looking at the text and Table 2 of the McCarty et al. 2016 technical report cited in my previous review, that channel was likely active on NOAA-16 and NOAA-17 during SSW02.