Review of egusphere-2025-748

Summary and General Comments

This is a well-written case study of the 2002 stratospheric sudden warming event in the southern hemisphere, referred to in the paper as SSW02. Given its extremely unusual nature, SSW02 has attracted attention in the literature for many years. The core contribution of the present study is to examine the role of in situ-excited planetary wave 2 in causing SSW02, as distinct from zonal wavenumber 2 waves propagating up from the troposphere.

I think this study merits prompt publication, albeit after what are likely to be major revisions. The single largest concern I have about the current version of the work is that the conclusions lean on the upper stratospheric and lower mesospheric fields of a single reanalysis product (MERRA-2). But how reliable are those, really? A recent study [1] of an NH SSW event found that MERRA-2 had problems above ~60 km and the only satellite instrument used by MERRA-2 above that altitude (Aura MLS) didn't launch until 2004. It's one thing to argue that the GCM underlying a reanalysis should be able to spread information from lower altitude upward, but the arguments made in the present paper involve apparent downward propagation of both waves and the zonal jet from higher altitudes. This would seem to create a greater need for high-altitude data.

Unfortunately I am not clear on how good the observing system for this part of the atmosphere was in September 2002. A recent whole-atmosphere reanalysis [2] starts in 2004 and used 6 satellite instruments, only one of which (TIMED SABER, not used in MERRA-2) was active in 2002. A technical report about the MERRA-2 observing system [3] warns of large biases in the lower mesosphere of the model, although it also suggests that the reanalysis state at those altitudes should be influenced by (if nothing else) channel 14 of the AMSU-A which was carried on three NOAA satellites active in 2002. In contrast, the ERA5 description paper [4] refers to the mesosphere as "observation-free" and warns of associated artifacts in the ERA5 product. However, ERA5 is also assimilating AMSU-A channel 14.

Within the framework of the present manuscript, I think the best way to address this issue is to repeat at least some of the major analyses using a different reanalysis product to see how robust the results are. It would be fine to skip the statistical significance calculations (about which more below), in which case the amount of new data that needs to be downloaded will not be large. It's not obvious to me which reanalysis should be used for the comparison—ERA5 is actually available in two versions (5 and 5.1) for the early 2000s for reasons laid out in [4] and an associated ECMWF technical report [5]. (Note that the technical report includes some figures showing SSW02 in several different ECMWF reanalyses.) Perhaps one of the Japanese reanalyses would be appropriate instead. If the two reanalyses agree, great! If not, I think the study is still worth publishing as a demonstration of the limits of existing reanalysis for studies of this event and as a prompt for future research.

Specific Comments By Line Number

24-25 One of your references (Newman and Nash 2005) attracted a (2014!) comment claiming that there was an SH SSW in 1972 and citing some related works—do those works need to be cited and/or your claim modified?

26 "breakdown of the polar vortex during midwinter"→the event took place on 25 September, but to me at least SH winter means JJA. So I would classify the event as happening in spring.

27 I don't quite understand what is meant by "sparse mountainous".

78 The MERRA-2 model top is actually at 0.01 hPa (not 0.1 hPa), although I gather you are using the pressure level data that ends at 0.1 hPa.

78-79 Sentence reads as though MERRA-2 ends at the end of 2023, which is not correct. What you mean is that you're using the 44 years 1980-2023 to calculate the characteristic magnitude of variability used to contextualize the size of SSW02.

84 Make sure to double-check this equation to make sure it has correct factors of cos(latitude)

94 This equation and subsequent discussions of BT-BC instability involve derivatives with respect to "y"—this would make sense for theoretical analyses on the QG beta plane, but you're analyzing real data on the sphere. You should either explain what you mean by "y" or just rewrite this equation, references to q_y, etc. in the physically correct spherical geometry.

96-97 This is an allusion to the way in which the flow is satisfying the Charney-Stern-Pedlosky criterion for instability, right? Maybe you should say that directly. It might also be interesting to point out that this isn't necessarily the most common way to satisfy the CSP criterion—see discussion in [6].

101 The cos(latitude) superscript appears to be a typo.

101-107 These equations use both f and f_0—the generalization of QG theory to the sphere is not trivial, and (assuming these equations are all written correctly) you should clearly state what you have chosen for f_0 and (in my opinion) say a few words to remind readers about why you are using both f and f_0.

115 Just for clarity maybe you should state that k is the nondimensional zonal wavenumber.

122 "the PNJ weakened dramatically by more than 100 m/s"—is the weakening really *that* large? I'm not seeing it in the figure.

124 "statistically significant at the 99% confidence level"—in my opinion "statistical significance" is an odd concept to apply to a case study of a single event that is well-known to be both a) real and b) unusual. In this context there is no "null hypothesis" being tested, you're just trying to quantify how extreme SSW02 was. I think you should drop the language here and elsewhere in the paper about "statistical significance" and "confidence intervals" and just state that the various colored dots represent <0.5th/<2.5th/>97.5th/>99.5th percentile as appropriate.

126 Are we supposed to be able to see the claimed upward-propagating signal in the figure? I also don't understand how this claim is supposed to related to the downward propagation of the easterly wind from the mesosphere (or even if the claims are supposed to be related).

Figure 2a This figure might be easier to understand if you also had a supplementary figure showing the full geopotential height field at 10 hPa on these same dates, instead of just the GHP. Figure 7 does sort of do this, but for somewhat different levels and times.

Figure 3 This figure seems to be (in the preprint at least) a raster graphic. I think you should try to have this figure (and all others) published as vector graphics, to enable zooming in to view the many details.

158-159 This sentence reads to me as claiming that there were easterlies in 70S-50S, <10 hpa on 22 September, which does not actually appear to be true.

Figure 3a How is the orientation of the EP flux vectors selected in this and all other EP flux figures. This isn't a trivial issue [7].

170-173 Seems plausible, but did you actually check plots of separate q_y subterms to confirm?

173 The overlap isn't exact, though—is it worth commenting on why that might be?

193-194 The caption should clarify that it's the *zonal wavenumber 2* component of Z' that is being plotted in Figure 3c.

Figure 4 The main text about and/or caption of this figure need much more discussion of how the phase speed spectra are computed—e.g., doesn't a phase speed spectrum need to be computed over some finite-width time window? What was that window and how was it selected, particularly given that you are attempting to interpret short-term temporal variations of the diagnosed phase speeds. If the analysis follows some previous paper sufficiently closely, you could just cite that paper instead.

215 What "decreasing trend at 100 hPa"? (I don't see it in the figure.)

222 How sure are you about the 21 September date, given the finite width of the contour interval? (In other words, might the jet structure you describe have been visible on 20 September if the upper left panel of Figure 5 were redrawn with a lot more contours?)

226 Technically there was a very small region of easterlies on 21 September.

242 I don't really understand what is meant by "negative PW forcings". Also maybe you should include the figure(s) you refer to as "not shown" in the supplementary material?

246-247 I guess near the jet core the EPFD is at or below the 2.5th percentile value, but aren't more of the values actually below the 0.5th percentile?

278-279 If the "additional cyclone" is what I think it is, might not its location be better described as 90E-180E?

288-292 Are you sure about these claims? (I have a hard time seeing them in the figure.)

Figure 8 The figure panels contain a bunch of zeros that don't seem to be within (plotted) contour lines. What are they for? Maybe you should plot the associated zero contours?

306 Clarify that "increases" means "increases towards zero", assuming that is what is meant.

310-311 Are we supposed to be able to see EP fluxes going upwards at the bottom of the $n^2<0$ region?

318-319 You're arguing that the possibility of instability is sensitive to the zonally resolved—not just zonal mean—q_y, right? That doesn't necessarily seem impossible, but if you're thinking about the instability of *zonal wavenumber 2* doesn't this wave have to "feel" the PV gradient over a pretty zonally extended region (perhaps even all longitudes) precisely because its wavelength is so long?

320-322 Sorry, I don't understand what is substantiating this claim. (Maybe I just don't understand over-reflection well enough?)

Figure S1/Table S1 Thank you for including this information, but I think it's sort of weird to refer to 6 pages of plots as a single "figure". I think they should just be renumbered as 6 different figures.

352-354 Can you explain a bit more about how you are concluding that "in situ PW2 generation via instability may have played a more dominant role in approximately half of vortex-splitting SSW events than tropospheric wave forcing"?

356-358, 367-369 Maybe you should add a line to your abstract saying that you discussion the implications of your (SSW02-specific) results for our understanding of SSW dynamics in general.

364 What exactly do you mean by "not anomalous"? Surely the easterlies weren't exactly at their climatological mean values?

369-370 I imagine this will be tough to do with reanalysis, given the sparse and time-dependent nature of the observing system at some of the relevant altitudes. If this really is a forced response to climate change, hopefully it can be modeled robustly.

Additional Thoughts on Future Work

Regarding the long-term possibility of (much more challenging) additional work, unfortunately there doesn't seem to be much satellite data not already included in MERRA-2 or ERA5. However MIPAS on Envisat and SMR on Odin may have usable data—indeed, a MIPAS-based study of SSW02 was published nearly two decades ago [8]. (As explained in [2], the aforementioned SABER switches between observing 53S-83N and 83S-53N every 60 days and a quick look at the SABER data shows that a 53S-83N observing period began on 19 September 2002.) I am not sure if anyone has tried assimilating MIPAS or SMR data for this event.

References

[1] Liu et al. 2025, Dynamical Response of the Middle and Upper Atmosphere to the February 2018 Sudden Stratospheric Warming Revealed by MERRA-2 and SABER, JGR-Space Physics, https://doi.org/10.1029/2024JA033528

[2] Koshin et al. 2025, The JAGUAR-DAS whole neutral atmosphere reanalysis: JAWARA, Prog. Earth Pla. Sci., https://doi.org/10.1186/s40645-024-00674-3

[3] McCarty et al. 2016, MERRA-2 Input Observations: Summary and Assessment, GMAO Tech. Report, https://ntrs.nasa.gov/citations/20160014544

[4] Hersbach et al. 2020, The ERA5 global reanalysis, QJRMS, https://doi.org/10.1002/qj.3803

[5] Simmons et al. 2020, Global stratospheric temperature bias and other stratospheric aspects of ERA5 and ERA5.1, ECMWF Tech. Report, https://doi.org/10.21957/rcxqfmg0

[6] Vallis 2006, Atmospheric and Oceanic Fluid Dynamics: Fundamentals and Large-scale Circulation, Cambridge University Press, p. 265/equation 6.74, available via Google Books

[7] Jucker 2021, Scaling of Eliassen-Palm flux vectors, Atmos. Sci. Lettrs., https://doi.org/10.1002/asl.1020

[8] Wang et al. 2005, Longitudinal variations of temperature and ozone profiles observed by MIPAS during the Antarctic stratosphere sudden warming of 2002, JGR-Atmospheres, https://doi.org/10.1029/2004JD005749