Comment on wcd-2022-34
Anonymous Referee #3

The manuscript does an excellent job of documenting over 50 years of research on stratospheric dynamics and variability. This is a broad focus and I think that this review strikes a nice balance between highlighting two of the subjects that are discussed in more depth (L37-40), SSWs and the QBO, and all of the other literature that is also encompassed by stratospheric dynamics and variability, e.g., the Brewer-Dobson Circulation, wave mean flow interactions, models and predictability, etc. These subjects are suitable for publication in this journal. I recommend publication following minor revisions.

I think it is probably impossible to include just the right amount of detail on every subject so that every reader is pleased. Me being one of those readers, below are my suggestions and questions that I hope can make some of the content of this manuscript more comprehensive. I do not advocate for any huge changes in the manuscript’s structure. Setting the sections up as 1) introduction, 2) extratropical stratosphere, 3) tropical stratosphere, 4) tropical-extratropical coupling, 5) models and predictability, and 6) influence on the troposphere makes sense.

L65: When considering the reverse influence of the extratropics on the tropics, perhaps a single sentence could be added citing work that shows a relationship between SSWs, the BDC, and subsequent changes in tropical convection via the BDC. These studies are mentioned later on in the manuscript (e.g., Noguchi et al. 2019).
L65: Consider citing Anstey et al. (2021) as their statistical analysis on the relationship between the QBO and SSWs reinforces this point.

L109-110: In an effort to be more comprehensive, consider adding something about ocean heat fluxes as a forcer of transient planetary waves. Garfinkel et al. (2020) considers this in addition to topography and land-sea contrast.

L166-168: Suggests removing “..is wave breaking ..” in this sentence. I generally think of wave-breaking as coinciding with wave activity flux convergence (probably with intermittent wave activity flux divergence too) and so the part of this sentence relating wave breaking to both acceleration and deceleration of the mean flow confuses me.

L223-224: Consider mentioning the study by Sjoberg and Birner (2014) on the vacillation cycles as a means of being more comprehensive. They corroborate these important results from Holton and Mass, but identify some key limitations of the 1976 modelling setup: https://journals.ametsoc.org/view/journals/atsc/71/11/jas-d-14-0113.1.xml?tab_body=fulltext-display

L256-257: I think the language stating the planetary waves may only promote deceleration of the polar vortex should be removed. Downward wave coupling events, in which the majority of a transient planetary wave propagates downward towards the troposphere rather than upward into the stratosphere, coincide with wave activity flux divergence and acceleration of the high-latitude stratospheric wind (e.g., Dunn-Sigouin and Shaw 2015).
L293-295: Would you please add some citations here to substantiate the point that radiative cooling is the most dominant process? I think this must be the case, but what studies (e.g., modelling, observational) are you thinking of when you write this?

L396-L308: While this section reads nicely as is, I think it could be expanded to include other mechanisms explaining transient planetary wave propagation. Consider mentioning linear interference, during which there is phase coherence between a transient wave and the climatological stationary wave (e.g., Watt-Meyer and Kushner 2015, mentioned in older work such as Holton and Mass 1976). Another mechanism to consider discussing is upscaling of synoptic scale wave activity to planetary scale wave activity (e.g., Boljka and Birner 2020).


Comments on section 2: I enjoyed this section. I think beginning with the zonal mean climate and progressing toward potential vorticity, which involves deviations from zonal symmetry (waves), and then progressing towards wind variability and SSWs makes sense structurally.

L380-382: Countering their being consensus on the role of extratropical waves modulating the QBO at that time, Hamilton et al. (2004) showed observational and modelling evidence that the QBO westerlies are zonally asymmetric. They attributed this response to equatorward propagation by the extratropical stationary wave. Shuckburgh et al. (2001) also showed evidence of horizontal wave propagation directly into the QBO westerlies. I think some of the earlier studies suggesting that extratropical planetary waves may influence the QBO could be mentioned. These are topical considering the current understanding of how QBO disruptions occur.


Figure 12: Rather than “E” and “W” as labels for eastward and westward, consider writing “eastward” and “westward.” I immediately assumed “E” meant easterly instead and if other readers make this mistake as I did, they will be wondering why the QBO-MMC looks reversed.

L428-436: Consider citing Yamazaki et al. (2020). By discussing a tropospheric pathway for the Holton-Tan effect, they have broadened the possible number of routes that the QBO can use to communicate with the polar vortex. The original HT mechanism, plus the QBO-MMC work, are very stratosphere focused, yet this new work is more troposphere focused (albeit the QBO’s influence must be communicated down to the troposphere from the stratosphere to initiate the teleconnection).

L510: type: “been” vs. “being.”

L629-630: There are some paper perhaps worth mentioning identifying some characteristics of SSWs that have a downward propagating response, White et al. (2019) and Cámara et al. (2019). This will help make this section more comprehensive.

