Overview

This paper posed an interesting scientific question regarding stratosphere-troposphere coupling, and addressed it with well-designed experiments. I think the authors' use of a more simplified atmospheric model was a good choice here, as these simplified simulations are able to capture most of the atmospheric dynamics relevant to their scientific interests. The experiments described in this paper explore the tropospheric response to the presence or absence of a stratospheric jet, in an idealized attempt to compare conditions that typically occur in wintertime (a strong stratospheric jet), and conditions that are common after a sudden stratospheric warming event (a much weaker or absent stratospheric jet). The authors explore a robust parameter space in their experiments, and they analyze a variety of stratospheric jet strengths, several different vertical structures of stratospheric winds, and the effects of surface friction. The authors find that when a stratospheric jet is absent, the tropospheric jet is located equatorward of where it is located in the presence of a stratospheric jet. The presence of surface friction enhances this response, with an even stronger equatorward shift of the tropospheric jet when surface friction is present and a stratospheric jet is absent. Finally, the authors note that the winds in the lower stratosphere are key for influencing the tropospheric response; when the wind anomalies are present only in the middle or upper stratosphere, the tropospheric response is weak.

Overall, I found the scientific question in this paper to be well-posed, the experiments to be well-designed, the authors' interpretation of results to be logical and interesting, and the paper to be well-organized and well-written. This manuscript is appropriate for publication in *Weather and Climate Dynamics*, and would make a good contribution to the scientific community. I recommend that this paper be **Accepted with Minor Revisions.** I have a few minor comments for the authors, and a few small suggestions that I think could strengthen this manuscript further, but I do not need to see this manuscript again. My comments and suggestions are presented in line-by-line format below.

Minor Comments

1. L17: "**The** troposphere and the stratosphere form a dynamically coupled system."
2. L20: Remove the word "maybe"; you could replace it with "Some of the most prominent stratospheric phenomena…"
3. L31-32: Replace "it can lead to periods with weak and equatorward shifted tropospheric jet stream" with "**a polar vortex break-down c**an lead to periods with **a** weak and equatorward shifted tropospheric jet stream".
4. L66-68:
5. L77: Your left quotation mark around "sub-vortex region" is backwards; if you're using LaTeX for your manuscript, you probably need to use the ` key instead of the ' key.
6. L89: Left quotation mark around "secondary cycles" is backwards; see above.
7. L145: Change "configuration" to "configuration**s**".
8. L146: Change "(slightly)" to "(**a** slightly)".
9. Lines 154-157: When I think about stratosphere-troposphere interactions in the context of SSWs, I often think about the role of planetary-scale waves (e.g., zonal wavenumbers 1-3) rather than the smaller-scale waves in your experiments. Since your results do seem to show

some sensitivity to wavenumber, do you think that larger waves would respond to your stratospheric perturbations in a similar way? Or do you think the behavior would be completely different?

10. Lines 177-178: I found the sentence starting with "However, especially the non-linear decay phase…" a bit confusing. Is this sentence trying to state that wave growth (first few days) is not substantially influenced by the presence of a stratospheric jet? If so, it might help to say that more explicitly. Also, is this result in opposition to some of the results you discuss in your introduction? My understanding is that Wittman et al. (2007) and perhaps Smy and Scott (2009) both saw changes in the growth rates of the baroclinic waves in their experiments, which seems to me to be different to what you saw. This could be a good place to refer back to these earlier works, and perhaps try to explain the discrepancies a bit. I realize you make this comparison a bit later in the paper (around lines 215-220), but I would consider moving this comparison a bit earlier.
11. Line 196, 198: Again, backwards quotation mark around "secondary cycles".
12. Figure 4: Overall, your figures were quite good; however, this figure was a bit confusing for me. We are comparing panels b-d directly to panel a, correct? That is, the shading indicates the difference between the black lines in panels b-d and the black lines in panel a? It might help to say that very explicitly in the caption. I don't think you need to change your figures necessarily (though adding a vertical line at day 6 could be nice but is not necessary), but I would suggest being very direct about what we're looking at, because it took me a couple of minutes to properly orient myself (and you have several other figures that follow this convention, so being painfully obvious about it the first time might be helpful).

    I also would suggest you might want to flip how you describe Figure 4--when I look at panels b-d in Figure 4, the first thing my brain thinks is "equatorward shift". But in the discussion beginning in Line 240, you talk about a poleward shift first, which took me a minute to comprehend. So maybe it would help to mention the equatorward shift without the stratospheric jet BEFORE you mention the poleward shift with the stratospheric jet. Again I don't think you need to change anything about the figure, just the order in which you discuss things.
13. **Discussion in Section 4**: When I was reading this section, I thought back to some of the experiments of Butler et al. (2010). The authors imposed a polar stratospheric cooling anomaly in a simplified model (not winds directly, as you do, but a stronger stratospheric jet was produced in response to the cooling). They tested several different heights of their temperature (and thus, wind) anomalies, and their results seem very complementary to yours--when the temperature anomaly was limited to the middle or upper stratosphere only, the tropospheric response was weaker or non-existent. I'd encourage you to check out their results (centered around their Figure 5), as I think this strengthens some of the points you're making in this section.

    To that end, zonal mean temperature anomalies for your experiments could be interesting, if you have them. My thinking is that it could be nice to more seamlessly link your work to some of the other idealized modeling work that simulates polar stratospheric variability with a temperature anomaly, instead of a wind anomaly (think Figure 4 but for temperature). This is just a thought though--I don't think it would really fit in well in your main manuscript but they could be a good supplemental figure, or even just for your own knowledge.

14. Line 314-15: Backwards quotation marks around "Set 1" and "Set 2".
15. Line 333: Remove "**does**" and change "increase" to "increase**s**".
16. Line 364: Change "worth to point out" to "**worth pointing out**".
17. Lines 365-367: Butler et al. (2010) (and maybe Polvani and Kushner (2002)) also show increases in eddy momentum fluxes near the tropopause in response to polar stratospheric cooling (and an increase of the winds, similar to your TS), so you could cite that here as well if you'd like.
18. L396: Change "weather" to "**whether**".
19. Lines 409-418: Another thing that occurred to me during your discussion of troposphere-only jet variability is that the jet response in many simplified and comprehensive models is dependent on its initial state--that is, jets that start farther equatorward tend to shift more in response to the same perturbation as a jet that starts closer to the pole (e.g., Barnes et al. (2010), Kidston and Gerber (2010)). Furthermore, the presence or absence of the stratosphere itself can have a large impact on the initial state of the jet (e.g., Wang et al. (2012)). Since the perturbations that lead to SSW events often come the troposphere, I suppose the point I am trying to make is that the background state can substantially modify the tropospheric jet response to atmospheric perturbations, and the background state of the troposphere can perturb the vortex in the first place. So at some point when thinking about SSWs it's necessary to zoom out and think about that troposphere-to-stratosphere pathway (not saying you need to change your analysis at all, but I think it is worth mentioning in the discussion at some point).
20. Line 445: Change "fiction" to "**friction**".
21. Lines 450-455: Is your explanation for the downward control essentially that changes in lower stratospheric winds (specifically, a weakening of the lower stratospheric winds on the flank of the polar vortex as generally occurs in response to a strong SSW event) drives a negative annular mode-like response in the troposphere? I'd recommend saying that very clearly and explicitly, since this is your last opportunity to concisely summarize your argument.

References

1. Barnes, E.A., D.L. Hartmann, D.M.W. Frierson, and J. Kidston (2010): Effect of latitude on the persistence of eddy-driven jets. *Geophys. Res. Lett.,* **37**, L11804.
2. Butler, A.H., D.W.J. Thompson, and R. Heike (2010): The steady-state atmospheric circulation response to climate change-like thermal forcings in a simple general circulation model. *J. Climate*, **23**, 3474-3496.
3. Kidston, J. and E.P. Gerber (2010): Intermodal variability of the poleward shift of the austral jet stream in the CMIP3 integrations linked to biases in 20th century climatology
4. Wang, S., E. P. Gerber, and L. M. Polvani (2012): Abrupt circulation responses to tropical upper-tropospheric warming in a relatively simple stratosphere-resolving AGCM. *J. Climate*, **25**, 4097-4115.