

General Comments

This paper examines the future increase/decrease of SSWs due to anthropogenic forcing in a large ensemble of models (12) from the CCMI database. Previous studies have had opposing views on this with some finding an increase in the number of SSWs whilst others have found a decrease. To check the robustness, they use three different commonly-used SSW criteria and conclude that they do not find a significant increase in the future number of SSWs over the course of the 21st century. I find the results to be interesting in that, by using a much larger number of models than previously used, the overall number of SSWs will remain approximately the same. The paper is well-written and the topic is well discussed, hence I suggest only minor corrections which I list below.

Specific Comments

Lines 51-52; not just due to anomalously large injections of wave activity from the troposphere, but can also be due to the resonance of wave activity such as that first described in Plumb (1981) and shown by Esler and Scott (2005). I would either cite this second mechanism, or just change the wording in your current sentence to be less definite.

Line 53-54; Small point, but not all SSWs can impact the troposphere (e.g., Gerber et al. 2010, GRL and Hitchcock and Simpson 2014). I would instead just add the word 'can' into the sentence to change it from meaning all as it is currently: 'SSWs can also impact the...'

Line 151; which criteria are absolute and which are relative? Can you somewhere distinguish between the two – from what I can see, the U6090N and WMO are absolute and ZPOL is relative.

Line 184-185; at which confidence level do the models simulate statistically significant differences between the SSW duration in the past and in the future? Can you make a comment here on the 90% level? Perhaps just say how many models would become significant if this level was used. My guess is that the HADGEM3-ES and MRIESM-1r1 would be significant at close-to the 95% level as the error bars are nearly separated.

Line 191; the eddy heat flux ($v'T'$) is what you plot right? This is a proxy for the injection of wave activity. Can you make this clearer? Further, I gather from figure 3 that you use 100hPa (I think this should be included here in the text also), but given the recent paper by de la Camara et al. (2017) and Birner and Albers (2017), who suggest that 100hPa is not an ideal surface to use as it is already in the bottom of the vortex, how sensitive are you results to the level of choice? Do you get more significant results if you use a slightly lower level?

Section 2.2 and Line 197; I would be interested to see what the changes between the number of splits and displacements are between the past and the future. From figure 3 it appears that the wave 1 and wave 2 forcing through 100hPa doesn't change too much between each period and so the number of splits and displacements may not change too much either. But given the relatively short length of the paper as it is, this would be an interesting addition.

Technical Comments/Grammatical Errors

Line 56; on → at

Line 82; The recent paper by Kim et al. (2017) which you cite later may be a good citation here.

Line 111; Could you clarify this sentence to say whether both reanalyses extend back to 1979, or that both extend back to before 1979 (and if the latter, then which year: 1960?)

Line 125; Is the Polar Cap area weighted? I think it should be and it would be good to include a sentence here saying so.

Line 191; 'in the course' → 'during the course'. Also, aHF100 in the text should correspond to the figure title of HF100

Line 193; None of the individual models show significantly different results?

Line 209; 'no statistically significant changes'

Line 220; Can you give references to the figure which shows this? Figure 1?

Line 221; 'across' → 'using'

Line 260; 'in the last years' → 'in recent years'

Line 261; 'metrics' → 'metric'