

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-394

Anonymous Referee #1

Referee comment on "Stratospheric Nudging And Predictable Surface Impacts (SNAPSI): a protocol for investigating the role of stratospheric polar vortex disturbances in subseasonal to seasonal forecasts" by Peter Hitchcock et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-394-RC1>, 2022

The manuscript outlines a set of protocols for multiple but standardized global climate modeling experiments to study the stratosphere-troposphere coupling under the umbrella called the Stratospheric Nudging And Predictable Surface Impacts (SNAPSI). The authors describe and outline an intercomparison modeling experiment to study the role of the Arctic and Antarctic stratospheric polar vortices in sub-seasonal to seasonal forecast models.

I appreciate that the authors have concerns for a nudging the stratosphere to the full observed state including eddies rather than the proposed zonally symmetric state. Still, I am concerned that only nudging to the zonally symmetric observed state may omit important stratospheric information or forcing on or coupling with the troposphere. I don't have a suggested solution but do want to raise the concern.

In Figure 2, I suggest that the plot of the observations be made consistent with the forecast pots? I found it hard to compare between observations and the forecasts.

Line 227 Not sure that I agree with the statement: "Comparisons between the nudged and control ensembles will provide a clear means of assessing the stratospheric pathway at play for those teleconnections that are active during the selected case studies." There could be multiple forcings in play and nonlinear interactions that would make attribution complicated.

3.1 Why are only temperature and zonal winds provided from ERA5. I would have thought to include geopotential height and meridional winds as well?

Lines 256-258 This is a difficult balance to strike nudging the stratosphere towards observations without throwing the whole model simulation out of whack. I can understand imposing no nudging below 90hPa that accelerates to full nudging at 100hPa but I don't believe that we fully appreciate the importance and role of the lower-stratosphere separate from the mid-stratosphere in stratosphere-troposphere coupling. In fact, I believe that the lower- and mid-stratosphere could influence the troposphere somewhat independently. I am concerned that by imposing now nudging in the lower stratosphere will dampen the full influence of the stratosphere on the troposphere. One idea that I would suggest considering is applying the limit of the nudging to different levels.

Lines 340-343 – I felt that the discussion about the MJO and its possible influence on the NAM and Northern Hemisphere weather is an unnecessary distraction almost like “having your cake and eating it.” The paper is about stratospheric influence and stratospheric nudging so why introduce that the MJO is needed to simulate the correct weather? I think better to leave tropical forcing and guidelines for modeling experiments to study tropical forcing for another paper.

Lines 361-365 I don't disagree that the tropospheric NAM response in 2019 was quite different than the tropospheric NAM response in 2018 to the stratospheric polar vortex split. However just by looking at Figures 3 and 5 it is not that obvious to me. I wonder if a different comparison might better highlight the difference.

Lines 376-379 I agree that the tropospheric response differences to the SSWs in 2018 and 2019 are interesting and is worthy of model experiments. But again I do question introducing into the discussion the MJO and tropical forcing. Almost makes the role of the stratosphere seem like noise rather than a signal and therefore could be ignored. My opinion is to take out this mostly hand wavy discussion of the MJO and tropical forcing, which seems self-defeating in trying to motivate stratosphere-only sensitivity experiments.

Line 400 I do wonder why the September 2019 Austral minor warming was chosen over the September 2002 major warming? In fact the SAM was much more negative in October 2002 (I believe a record in fact) than 2019. If we assume that a reversal of the winds at 10hPa is necessary for the tropospheric response, how do you justify including a case where the winds never reverse? I do believe that the September 2019 austral polar vortex disruption is interesting but seems like not a good fit for the framework of this study. The first two words in the Abstract are “major disruptions.” At a minimum justification of the choice is needed.

Line 471 – I am surprised by the data being embargoed initially. Seems counterproductive to me.