

Northern Hemisphere Stratospheric Temperature Response to External Forcing in Decadal Climate Simulations

Abdullah A. Fahad, Andrea Molod, Krzysztof Wagan, Dimitris Menemenlis, Patrick Heimbach, Atanas Trayanov, Ehud Strobach, and Lawrence Coy

Reviewers' comments are in black, responses are in blue color

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A tracked change version of the manuscript is attached.

RC1: The study by Fahad et al. aims to explore the stratospheric temperature response to external forcing. To this end the authors analyze a series of 30-year perpetual time slice experiments with forcings corresponding to year 1992, 2000, and 2020 conditions. The authors motivate their study with a stratospheric temperature increase during 1992-2000 and decrease during 2000-2020. While I appreciate the authors aim to advance our understanding of stratospheric temperature trends and variability, I identify a series of shortcomings in the presented work.

We thank the reviewer for their insightful suggestions to improve the manuscript. We have revised the manuscript in accordance with the reviewer's comments, detailed below.

Specific Comments:

1) The time periods for trend analysis: the authors motivate their analysis with opposing trends between 1992-1999 and 2000-2020. I consider the first time period too short for a robust trend analysis. Similar positive sloping trends could be randomly identified e.g. at the end of the time series in Fig. 1b. Overall Fig. 1 highlights strong inter-annual variability in the temperature evolution over the Arctic polar cap. To provide further insights into the drivers of this variability – and its peak amplitudes – would be a worthy endeavor and I suggest the authors to focus their analysis on this rather than short-term trends or tendencies.

We agree that focusing on the 1992–2000 period for trend analysis may appear arbitrary given the large interannual variability. To address this, we have computed a 7-year running mean of polar stratospheric temperature over the entire 1981–2024 reanalysis period, which filters out much of the interannual variability. The results are shown in our new Figure 1. The longer, filtered time series reveals a clear cooling trend in the polar stratosphere throughout the period, except during 1992–2000, when temperatures increase dramatically. From this plot, it is evident that the end of the initial cooling period and the onset of warming occur in 1992, the peak of the transient warming is reached in 2000, and a renewed cooling trend begins after 2000 to 2020s. We believe that this filtered time series clarifies the rationale for selecting our analysis periods (1992–2000 and 2000–2020). Figure 1 has been updated accordingly, and the manuscript text has been revised to explain these changes more clearly.

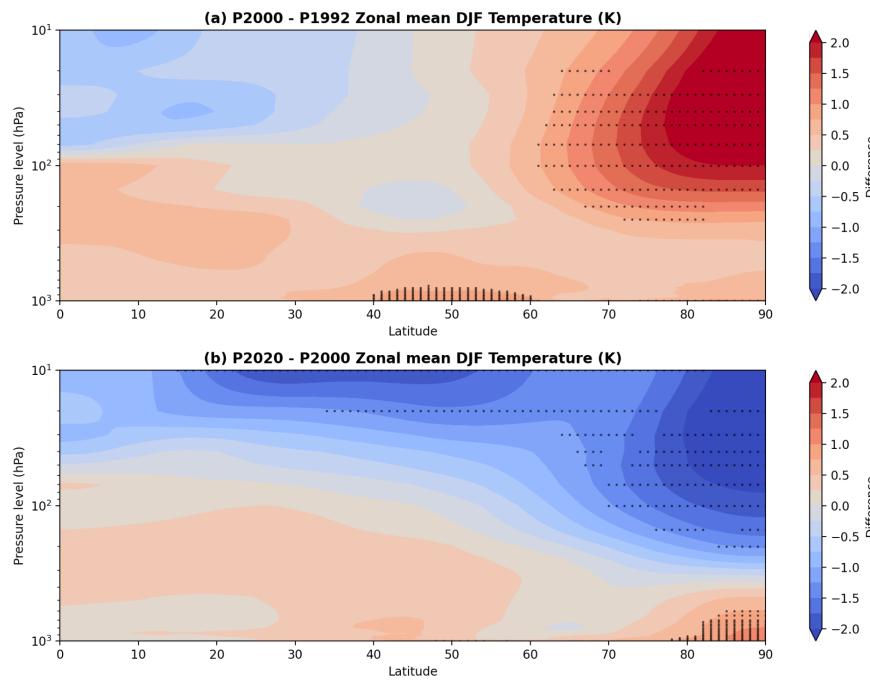
We have updated the following in the manuscript:

Figure number: 1, supplementary figure S1

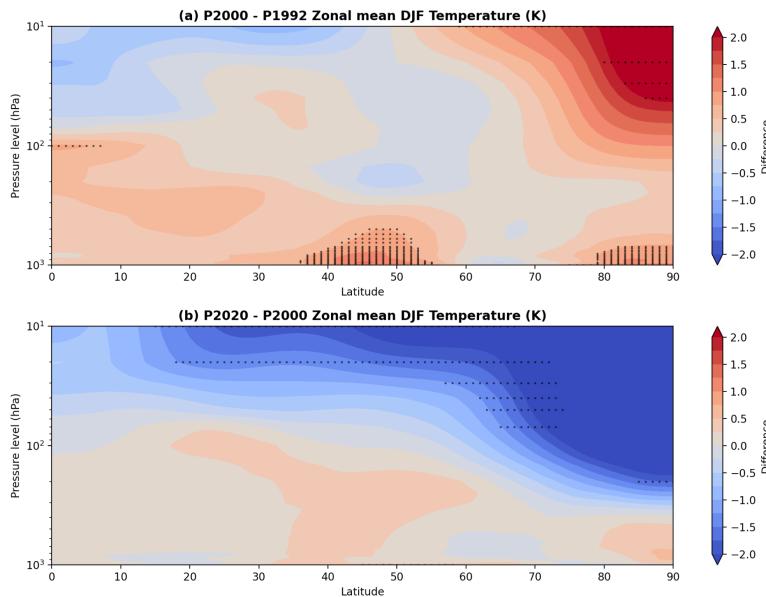
Text Line numbers: lines 2-7, lines 78-85, lines 165-173.

2) Simulation design and ensemble size: the authors investigate 30-year perpetual simulations with 1992, 2000, and 2020 conditions. Each year of these simulations is treated as single ensemble member that are pooled for composite analysis. Generally, I consider this ensemble size too small to derive robust conclusions especially, given the large inter-annual variability in NH polar cap stratospheric temperature. Commonly single forcing studies focusing on stratospheric temperature effects have utilized 100 year (+) plus integrations. How robust are the findings against sub-sampling? How different/overlapping are the polar cap temperature distributions across these 30-year sets? How would the results change if a bootstrap analysis is applied?

We agree that a 30-member ensemble size can be small for understanding some aspects of the stratospheric response under climate change; however, our 30-member “perpetual” ensemble plus a 10-member “transient” ensemble shows robust agreement of NH polar vortex warming from 1992 to 2000 and cooling from 2000 to 2020. We used a t-test on the 30-member ensemble and applied stippling to indicate 95 % significance for the mean differences (all figures in the manuscript that include mean-difference panels use this convention). Additionally, as suggested, we performed a nonparametric bootstrap significance test using $\alpha = 0.05$ (95 % confidence) and 1,000 bootstrap samples to assess whether the mean difference between the two datasets is statistically significant without assuming normality. We still found the NH winter polar vortex temperature increase between 1992 and 2000 to be significant. Next, we randomly subsampled the 10-member ensemble five times and applied the same bootstrap test; even the least significant subsample shows warming from 1992 to 2000 at 95 % confidence. Figures are shown below, and we have added this description to the manuscript text (*Line numbers: 158-162*).



R.Fig. 1: Same as Figure 3 in the manuscript, except stipplings are based on the bootstrapping method at a 95% significance level.



R.Fig. 2: Same as Figure 3 in the manuscript, except based on randomly sub-sampled (10-member) and the stipplings are based on the bootstrapping method at a 95% significance level.

3) How robust are the findings against the starting years selected?

We have analyzed starting years 1992, 1993, 1994, and 1995, and the conclusions from the 10-member transient experiments remain unchanged: the polar stratosphere warms significantly during that period (95% confidence, Student's t-test). Figure 1 (updated) in the manuscript shows the mean trend of the transient experiments along with their spread, where the observed warming trend and model trend remain similar for different starting years nearby.

4) How does the vortex state and stability in these integrations compare with the observational record? And how different is it within and across the ensembles?

The following text and a new Figure 2 are added to the updated manuscript to show the vortex state comparing reanalysis to the GEOS-MITgcm simulations, demonstrating the fidelity of the simulations. (Line numbers: 132-142)

"The GEOS-MITgcm coupled model transient simulations reproduce the mean state and variability of the polar vortex reasonably well as compared to reanalysis. The geopotential height at 10 hPa from the 10-member ensemble mean for January over 1992–2020 shows a similar mean state and location of the NH polar vortex during winter compared to reanalysis (MERRA-2: 1992–2020) (Fig. 2a,b). The 30-year mean January momentum-flux variance ($U'V'$) associated with the vortex wind jet, calculated from sub-monthly fields, further shows that the stability and variability of the vortex core are simulated reasonably well in the GEOS-MITgcm compared to the reanalysis (Fig. \ref{vortex} c,d). The model, however, produces a bit weaker mean geopotential height (~ 4km higher at the core) and momentum flux variance (weaker ~ 25 m²/s² near Greenland) compared to MERRA-2, suggesting that the simulated vortex may be a bit more resilient under extreme wave forcing. This could be a result of a lack of higher vertical resolution in the stratosphere. However, the close agreement in both mean height structure and eddy flux confirms that the model faithfully represents the polar vortex's stability and natural variability."

5) In analogy to comments 2+4), how different are the EHF (and stationary and transient terms) within these integrations? How robust are the findings against bootstrapping?

Figure 5, Figure 6, and supplementary Figure S1 show the mean differences in eddy heat fluxes. The figures are stippled to indicate 95 % significance, computed using a two-sided difference-of-means t-test on the 30-member ensemble. The bootstrapping method, with 1 000 iterations at the 95 % confidence level, yields similar results.

6) The study is based on ensembles obtained with a single model, which does not particularly well align in terms of variability (trends and their significance) with reanalysis data (as shown in Fig. 1). I would suggest including in a revised manuscript additional models to corroborate the results.

We agree that the previous Figure 1 was somewhat confusing for interpreting the variability and trends between the reanalysis transient years and the 10-member ensemble-mean transient experiment. We have replaced it with a new Figure 1, in which a 7-year running mean, applied to minimize internal variability, shows behavior very similar to that of the 10-member ensemble-mean polar-vortex temperature. This result holds for both smaller and larger running-mean windows, although variability increases with smaller windows and is further smoothed with larger ones. A comparison with similar perpetual experimental designs is beyond the scope of this study but will be explored in future work. For the transient experiment, we have examined both high-top and low-top historical CMIP simulations and discussed the results in the manuscript (Fig. 8).

7) The authors refer several times to supplementary figures, which I could unfortunately not find enclosed in the preprint or linked to a research square.

Uploaded a single PDF with supplementary figures attached at the end.

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RC2: The paper investigates the recent decadal variations of the polar stratospheric temperature during boreal winter (DJF) by analyzing the last 30 years of reanalysis datasets, simulations of a 1-degree ocean-atmosphere coupled GCM (GEOS-MITgcm) and CMIP6 historical simulations. Two types of simulations are performed with the coupled GCM, some are forced with time-evolving CO₂, ozone and aerosols from 1992 to 2020 and called transient simulations. Some others have constant concentrations of CO₂, ozone and aerosols related to a given year and called "perpetual year" simulations. The authors detect a positive trend in polar stratospheric temperature from 1992 to 2000 and negative trend from 2000 to 2020 in both the ensemble mean of the transient simulations and reanalysis. They also show that the "2000 perpetual year" simulations have higher stratospheric temperature than the "1992" and "2020 perpetual year" simulations. The setup of the "perpetual year" simulations is interesting to investigate the stratospheric temperature changes during the last decades.

However, no physical interpretation of the results is provided. The heat fluxes budget show that wave propagation and breaking does play a role. But there is no interpretation of why waves behave as they do for the different experiments. So the paper does not provide any direction of why 1992, 2000, 2020 generated different behaviors in the waves.

We agree with the reviewer that we have not provided an end-to-end physical explanation of the response to CO₂ and ozone trends that results in the cooling–warming–cooling behavior in the polar stratosphere. We have, however, offered a physical interpretation of the proximate cause of this behavior. We have shown that the warming during 1992–2000 is related not to radiative

effects, low-frequency modes, or volcanic influences, but to dynamical processes. We have traced the dynamical heating to the meridional eddy heat flux and the associated differences in wave activity. As noted in the summary section, the remaining physical interpretation, explaining why the wave activity changed during the warming period, remains the subject of ongoing studies.

We have added the following caveats and texts in the manuscript in response to these questions.
(Line 320-328)

I have another major concern related to the detected trends. Computation of trends is quite strange when analyzing 8 consecutive years from 1992 to 2000. I think these trends for this small period are not significant (in particular ERA5 in Fig.1b or high-top simulations in Fig.8b).

We agree that focusing solely on the 1992–2000 period for trend analysis can be seen as arbitrary. However, examining the centered 7-year running mean of polar-stratosphere temperature over the entire 1981–2024 reanalysis period, thereby minimizing interannual variability, reveals a clear cooling trend in the polar stratosphere, except during the 1992–2000 interval, when temperatures increase dramatically. This is the rationale for examining the warming trend during 1992–2000. We selected 1992, 2000, and 2020 to investigate and compare the end of the initial cooling period (1992), the peak of the transient warming (2000), and the recent resumption of cooling at the end of the time series (2020). We have updated Figure 1 to reflect this rationale and revised the manuscript text to explain these changes more clearly.

We have updated the following in the manuscript:

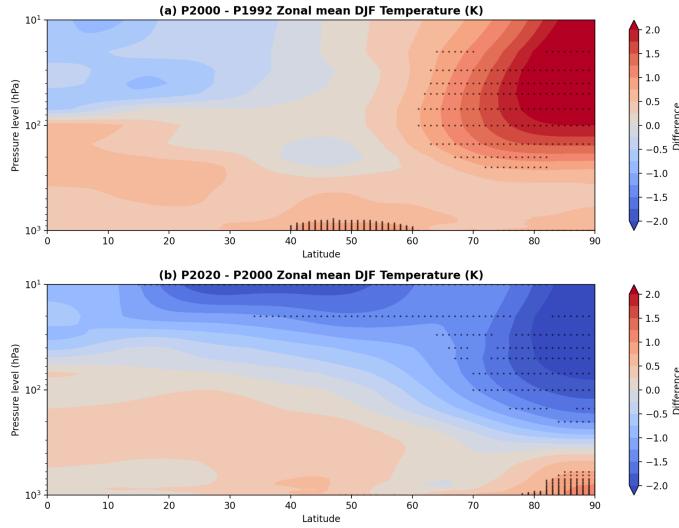
Figure number: 1, 8, supplementary figure S1

Text Line numbers: lines 2-7, lines 78-85, lines 165-173.

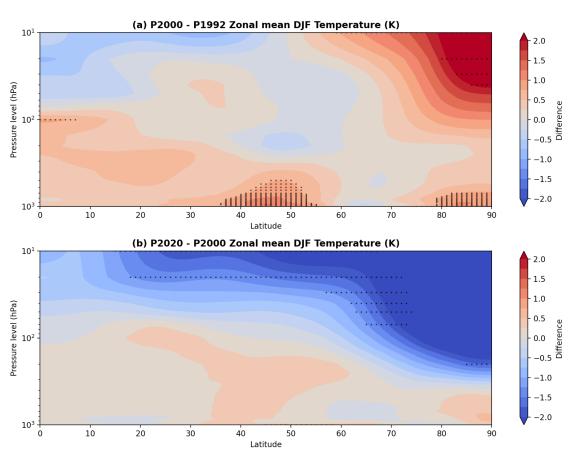
Also from a statistical significance point of view, it would be important to show the spread of the 30 members in Fig.1a to see if the ensemble means are significantly different.

We have shown the spread of the ensemble with a shaded line in the updated fig. 1. We used a t-test on the 30-member ensemble and applied stippling to indicate 95 % significance for the mean differences (all figures in the manuscript that include mean-difference panels use this convention). Additionally, as suggested, we performed a nonparametric bootstrap significance test using $\alpha = 0.05$ (95 % confidence) and 1,000 bootstrap samples to assess whether the mean difference between the two datasets is statistically significant without assuming normality. We still found the NH winter polar vortex temperature increase between 1992 and 2000 to be significant. Next, we randomly subsampled the 10-member ensemble five times and applied the same bootstrap test; even the least significant subsample shows warming from 1992 to 2000 at

95 % confidence. Figures are shown below, and we have added this description to the manuscript text. (Line numbers: 158-162).



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R.Fig. 2: Same as Figure 3 in the manuscript, except based on randomly sub-sampled (10-member) and the stipplings are based on the bootstrapping method at a 95% significance level.

To conclude, even though the perpetual year simulations are interesting, the paper does not provide any interpretation of the different behaviors of the three years 1992, 2000, 2020 and there is a strong lack of significance tests. How do the various changes in CO₂, ozone and aerosols influence the waves and residual mean circulation ? Such a question should be addressed in the paper but this is not the case. Therefore I recommend rejection of the paper even though I must admit there is some potential for publication in the future but with an entirely revisited paper including deeper analysis of the simulations and an interpretation of CO₂/ozone/aerosols effects.

We hope that we have addressed these questions already in our responses to your previous comments above.

Major concerns:

I) About detected trends. In fig1.b, interannual variability is very large compared to the trends. The results of the detected trends for the first period (1992-2000) might strongly change if the year 2000 is included or not. Same thing in Fig.8b, by removing or adding a year for the high-top simulations, the sign of the detected trends might change.

We agree that focusing solely on the 1992–2000 period for trend analysis can be seen as arbitrary. However, examining the centered 7-year running mean of polar-stratosphere temperature over the entire 1981–2024 reanalysis period, thereby minimizing interannual variability, reveals a clear cooling trend in the polar stratosphere, except during the 1992–2000 interval, when temperatures increase dramatically. This is the rationale for examining the warming trend during 1992–2000. We selected 1992, 2000, and 2020 to investigate and compare the end of the initial cooling period (1992), the peak of the transient warming (2000), and the recent resumption of cooling at the end of the time series (2020). We have updated Figure 1 to reflect this rationale and revised the manuscript text to explain these changes more clearly.

We have updated the following in the manuscript:

Figure number: 1, 8, supplementary figure S1

Text Line numbers: lines 2-7, lines 78-85, lines 165-173.

II) Significance tests. In addition to I) about trends, we do not know if the difference between the 30-year ensemble means are significantly different. Please show all the members, or least add the max and min among all members.

We have performed a two-sided t-test to assess the significance level. All figures showing the difference in means are stippled at the 95% significance level, using 30-member ensembles as samples. In addition, as suggested, we have conducted bootstrapping and sub-sampling significance tests, which are more stringent and still show consistent, statistically significant warming.

We have updated Figures 1, 7, and 8 to show the ensemble spread for both transient and perpetual experiments.

III) Section 3.2 is entitled "dynamical mechanism of forced change" but I do not see any interpretation of why the waves could change their propagation and breaking as function of the different forcings. So in my opinion, there is no proposed mechanism to explain the observed changes

in temperature and waves propagation. All the figures from 2 to 7 are consistent with each other but this only provides a part of potential mechanisms to explain the changes.

See our response to your previous similar comments.

IV) Simulations. The authors mention several times that the changes cannot result from low-frequency modes of interannual variability but they do not show any evidence. Line 102, the initial states are said to be uncorrelated but which variables have been looked at ? SSTs ? Also it is not clear to me if the 30 initial states are the same for the P1992, P2000 and P2020 simulations.

A new Fig. 8 shows that both the 10-member ensemble simulations and the perpetual experiments have initial states that differ from the observed IPO index phase. Although the ensemble mean (bold thick line in Fig. 8 minimizes the variability of the IPO index, we still observe a statistically significant polar vortex warming trend in our experiments. The large spread of IPO phases also reflects that our initial conditions were randomly chosen from previously spun-up simulations.

We have added text on choosing initial conditions and influence of low frequency modes on text: line numbers: 258-269

V) Abstract. I found the abstract not well written. It should first mention decadal variations in reanalysis and results of the transient simulations before describing the "perpetual year" simulations. Analysis of heat fluxes cannot be called "analysis of the temperature budget (line 13) since a full temperature budget would require to show the diabatic terms too.

We have updated the abstract as suggested. Our temperature budget terms included all terms, including diabatic terms. However, only significant term changes are discussed in the manuscript (mentioned in the text: Line 13).

VI) Wording. In some sentences, the text contains too strong statement. For instance, line 249 "high-top simulation" is said to be "very similar to reanalysis and GEOS-MITgcm simulations". I do not think the trends of the first period exist in reanalysis (Fig.1b) or high-top simulation (blue curve in Fig.8b) and that the curves resemble to each other.

We have updated the figures and text to reflect where is the similarities between the CMIP GISS run and GEOS-MITgcm and reanalysis.

Updated Fig. 1, 8

Line number: 283-290

Another example is in line 283-284 "The opposing polar stratospheric NH temperature trends in the two periods examined here are only significant during boreal winter". Why is the word "significant" used here ? In Fig.1 I do not see any significance tests.

Text updated

VII) Supplementary information. I was not able to download it from the web sites.

Uploaded a single PDF with supplementary figures attached at the end of the manuscript.