Response to Reviewer 1

We greatly appreciate the reviewer's effort to provide valuable and constructive comments to improve our manuscript. We faithfully addressed the reviewer's comments below. For clarity, our responses are colored in blue.

Major Comments:

1. The framing of the title, abstract, and introduction is too general.

Currently, the first parts of the manuscript give the impression that this work mainly aims to understand the impacts of the two SSWs using the SNAPSI experiments. However, in my reading, the paper is not simply asking whether SSWs affect the surface, as some previous studies have already addressed this question. Rather, the more specific and interesting question is whether two contrasting SSW cases have different or similar forced downward responses and dynamical pathways when the stratospheric state is controlled. This is an important result, because it provides clearer evidence that the observed difference between the two events may mainly reflect tropospheric internal variability rather than fundamentally different stratospheric downward pathways. I therefore think this key question should be stated much more clearly in the Introduction, also the title of this paper should be modifed to better reflect it.

I also suggest moving Fig. 1, or a simplified version of it, to the Introduction. Fig. 1 mainly establishes the observed contrast between the 2018 and 2019 events. This is important motivation for the study, but it is less a new result. Introducing the two cases with this figure would help the reader understand early on why these two events are being compared.

Related to this, there is some repetition between the Introduction, Section 2.1, and Section 3.1. The Introduction discusses the general issue of SSW surface impacts and previous nudging studies; Section 2.1 again introduces SNAPSI and the two cases; and Section 3.1 again establishes the observed contrast between the two events. I suggest streamlining these sections: use the Introduction to introduce the two cases and the key scientific question, keep the Methods focused on the experimental design and diagnostics, and start the Results with the SNAPSI-based comparison.

Thank you for the constructive suggestion. Following the reviewer's suggestion, we have restructured the manuscript. The overview of the 2018 and 2019 SSW (Fig. 1) is moved to the introduction to provide

research motivation and avoid repetition (L40–47). The key objective of the study is also clarified in the Introduction by stating that the aim is to identify whether the contrasting downward influences of the two SSWs stem from the stratosphere or tropospheric internal variability (L73–78). Additionally, the title has been modified to " Surface impacts of Sudden Stratospheric Warmings (SSWs): Similar dynamical pathways of 2018 and 2019 SSWs in stratosphere-nudging experiments" to better reflect these main findings. Section 2 now focuses on the experimental design, while Section 3 begins with the SNAPSI-based comparison.

2. The nudging logic and tendency interpretation should be clarified earlier.

This is especially important for this manuscript because the later analysis relies on detailed KE diagnostics and tendency decompositions. At present, some of the relevant explanations appear later in the interpretation of Figs. 8–11, but they are somewhat buried there. I suggest at least adding a brief explanation in the Methodology of what tendency terms are available from the model output, which terms are diagnosed as residuals, and how direct nudging tendencies, parameterized tendencies, and resolved eddy-flux responses should be interpreted. This would provide the necessary basis for understanding the later diagnostics and for distinguishing the directly imposed nudging response from the subsequent dynamical adjustment of the model atmosphere.

Thank you for the comment. In the revised manuscript, data availability for each model is clearly described. We have also clarified how diabatic heating and zonal drag are derived, and how nudging and parameterized tendencies are isolated (L161–169). We have improved the physical interpretation of the KE equation by describing how it diagnoses the ageostrophic meridional circulation that acts to restore thermal wind balance when disrupted, and how it distinguishes the circulation response driven by directly imposed nudging forcing from that induced by subsequent dynamical adjustments (L156–158).

Specific Comments:

L123: Please clarify what “these terms” refers to.

Replaced with “these derived diabatic heating and zonal drag terms”.

L133–142: As noted in my major comment 1, the contrasting impacts of the 2018 and 2019 SSWs have already been introduced several times. I suggest integrating this part, together with Fig. 1, into the Introduction.

See the response to major comment 1.

Figs. 1 and 2: The grey triangles are not very visible. I suggest changing them to another colour or making them more prominent.

Both figures are improved.

L187: Please specify that "the absence of SSW-related signal in the observations" refers to the 2019 SSW.

Replaced with "the absence of an SSW-related signal in the SSW2019 observations".

L205–209: The transition to the next section is too general and does not fully reflect the core question. The analysis is not simply about how SSWs drive tropospheric anomalies, but about diagnosing the dynamical pathway of the forced response and comparing whether this pathway differs between the 2018 and 2019 cases. I suggest revising this sentence accordingly to better reflect the storyline.

We have revised the text by emphasizing the objective of the study.

L212–217: The statement here seems too strong, as the conclusion is drawn from Fig. 5, where the geopotential height response has been standardized. I suggest either softening statements such as "only in the stratosphere" and "instead of a time-lagged direct downward propagation" or also checking the results showing the unstandardized decomposition.

We have softened the statement. Here it should be noted that the result does not change when anomalies are not standardized. Figure R1 illustrates the unstandardized geopotential height differences for each SSW case. As in Fig. 5, differences in temperature contribution exhibit weak negative differences in the troposphere rather than positive differences.

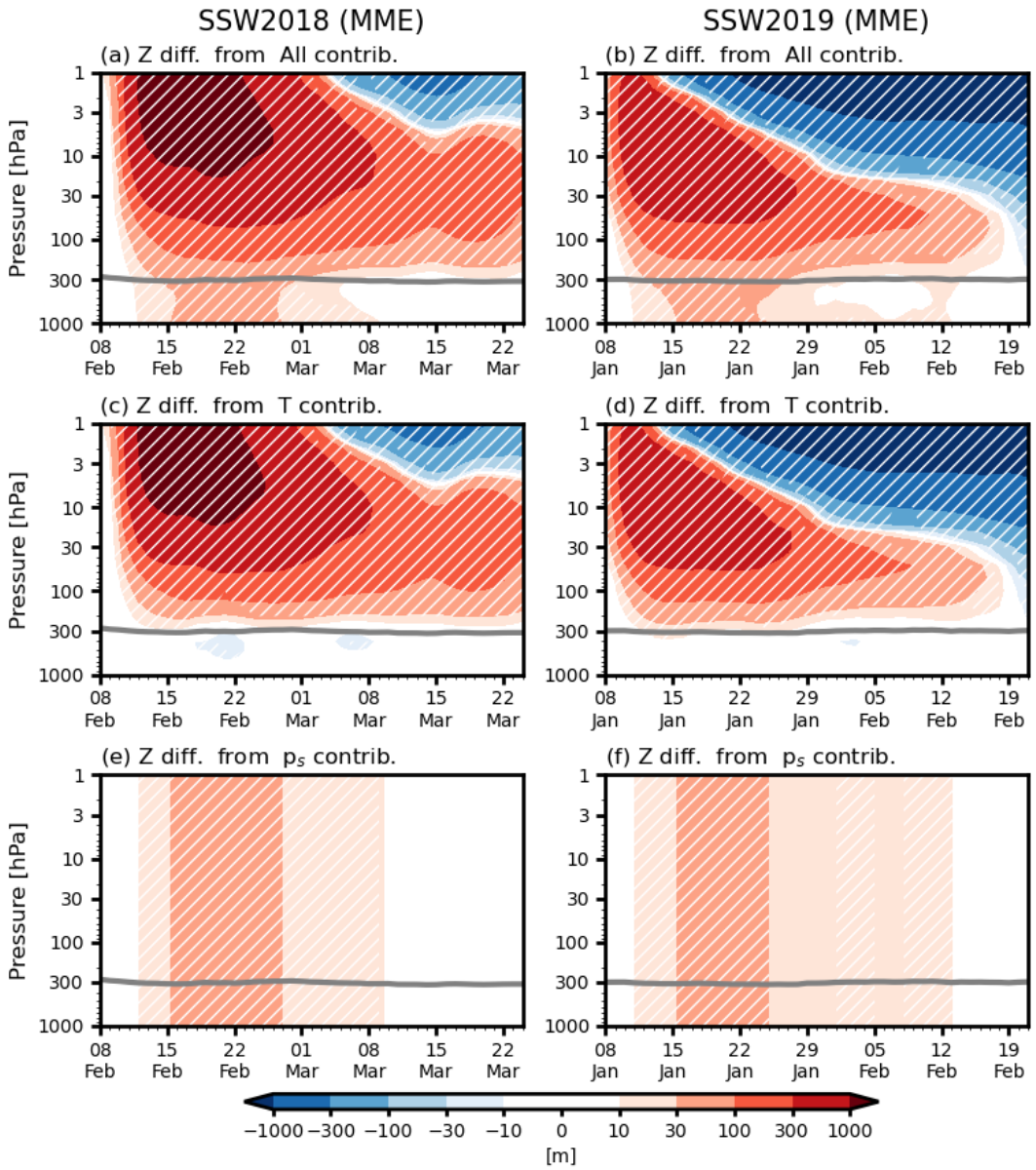

Figure R1. Same as Fig. 5, but for unstandardized geopotential height differences.

L273–276 / Fig. 9: Please clarify how the direct nudging tendency is separated from the parameterized zonal wind tendency. It would be useful to state whether these tendencies are directly available from the model output or diagnosed indirectly (as in my major comment 2).

We have clarified in the Methods section how nudging and parameterized tendencies are isolated.