I appreciate that the revised manuscript now states more explicitly that this study focuses on classifying SSW types. This seems to address my earlier concern well. I also think the consistency between ERA5 and CESM2-WACCM, including the parts related to SSWs and downward propagation, is a strength of this study. This consistency suggests that those aspects of type-dependent differences are not merely dependent on a few specific cases, but that there are indeed robust distinctions between types. From this perspective, I do see several statements throughout the manuscript noting that results are consistent in CESM2-WACCM. It would be helpful if the manuscript could more clearly remind the reader why this consistency between ERA5 and CESM2 is meaningful.

My main remaining concern is that several places still read as if coincidences or composite-mean features are being mechanistically confirmed. Of course, given that the surface temperature responses differ across types, it is appropriate to describe how the temperature anomalies for each type arise through differences in the NAM evolution or circulation structures. This aligns with examining mechanisms of downward influence. However, as your first sentence of the Abstract suggests, a "downward propagation mechanism" of SSWs generally refers to the full chain of processes—from precursors, to SSW onset, to downward propagation, and to the subsequent surface influences. In the current manuscript, the mechanism remains somewhat unclear from the pre-SSW stage through the tropospheric circulation differences. At present, it seems clearer how the unique circulation patterns are linked to different surface temperature responses, while the mechanism leading to those pre-SSW differences is less well established.

Results expected from the type definition

Also, I still notice a tendency in the main text to overstate results that are essentially expected given how the types are defined. My point is not that composites should not be shown or discussed, but rather that outcomes that follow naturally from the definition should not be framed as discoveries. For example, if a CP type were defined as cases where Niño4 is stronger than Niño3, one would not frame it as a "finding" that Niño4 SST is higher for the CP type.

For example, in explanation of Figure 3, the text states: "To compare the possible different impact of DWs on the near surface, the composite t2m anomalies in the 40-day intervals before and after the SSW onset are shown in Fig. 3. In both ERA5 and CESM2-WACCM simulations (Fig. 3a-d), the cold anomalies appear over different regions during the SSW for DWs and the NDW."

As I have emphasized, these impact differences are inevitable because the types are defined based on impacts. This phrasing could lead readers to interpret the result as unexpectedly large or newly revealed. A more straightforward framing such as "the temperature impacts are naturally well

separated by the type classification" would be more appropriate. In particular, the phrase "possible different impact" is difficult to justify here, because different impacts are expected by definition.

Emphasis on detailed differences between the types

1. The statement around Line 25 "NDWs show relatively weaker impact on the troposphere, which is primarily related to the weaker stratospheric disturbance amplitude" reads as if the amplitude difference is being treated as a deterministic driver of type differences. In other words, does this imply that when an SSW is strong it will always propagate downward, or vice versa? Please consider revising this sentence to avoid that implication.

2. Some of the detailed descriptions in the Figure 1 section read as though the type separation is mechanistically rooted in those pre-SSW characteristics. If the statistical test was conducted against the full wintertime distribution, I suggest modifying the approach to directly test differences among the SSW types themselves. That is, without considering non-SSW cases, please test whether the differences among the SSW groups shown here are statistically significant. For example, are the pre-SSW stratospheric signals for BOTH and NDW truly significantly different? Since the pre-SSW signals also appear less evident in CESM2-WACCM, it would be helpful to test the difference explicitly, or to show additional evidence (e.g., a scatter plot of cases using a NAM averaged between lag# and lag# at level#) to confirm how well these groups are separated. I also suggest stating in the text that these pre-SSW signals are not clearly seen in CESM2-WACCM.

3. For Figure 7, the precursor patterns appear not always consistent between observations and the model. Yet authors still explain the details. I think the discussion should focus more on the common features. Given that the analysis pathway is presented as a sequence addressing the overall mechanism (precursors → pre-SSW NAM → NAM propagation → tropospheric circulation/PV → surface impacts), it seems important to demonstrate whether the differences between types are statistically significant, for the precursors as well (Figures 7, 8, 9, and 10). The goal here is not to contrast SSW vs. non-SSW conditions, but to emphasize differences among types given that SSWs have already occurred. In that sense, estimating within-type variability (e.g., standard deviations around the composites) and testing differences among type composites would help describe these results as type-specific characteristics. This would help the manuscript more convincingly claim not only a useful classification but also type-dependent dynamical behaviors. My intention is not to reject the overall framework, but to encourage the text to focus on the most meaningful and statistically supported signals.