Response to the Editor

My impression is that this version of the paper is now closer to resolving the general concerns of the referees -- specifically that the stated conclusions have been too strong and cannot be justified either by logical argument or by the evidence available. (One aspect was the identification of physical mechanisms, the other was interpreting as important aspects of the different classes identified that essentially resulted directly from the definition.)

In looking through the paper myself I have identified below some points that need to be addressed -- ranging from typographical errors to lack of clarity or overstatement. Please can you address these specific points and check the whole paper again carefully -- making changes where necessary -- bearing in mind the general concerns of the referees. Your paper will be more valuable if it is free of errors and if what has been achieved is clearly stated. Please provide a clear list of the changes that you have made, with justification if needed.
Response: We thank the Editor for the careful reading of the manuscript and for the detailed and constructive comments. We have revised the manuscript accordingly, with particular attention to reducing overstatement, avoiding unsupported causal interpretations, and improving clarity and accuracy. Below we provide a point-by-point response to the specific comments.

I then expect to accept the paper for publication.

Detailed comments:

49: should be Haynes et al 1991. It was easy for me to spot that but please check all other citations/references carefully -- there are others where similar error have been made.
Response: Corrected. We have also carefully checked all other citations and references and corrected similar errors where identified. such as: “Rao et al., 2021” has been changed to “Rao and Garfinkel, 2021” in L143

69: why 'in observations' -- omit?
Response: The phrase “in observations” has been removed.

70: 'As a consequence, the tropospheric response signals vary in extent, area and scope.' -- this is an example of invoking causality in a vague manner. 'As a consequence of what' -- all we know is that the tropospheric response varies and -- from observations along we cannot be sure what the 'response' is.
Response: The sentence has been revised to remove causal wording and now reads:
“Accordingly, the tropospheric signals exhibit substantial differences in their extent, spatial coverage, and overall structure.”(L72-73)

82: 'dripping' -- many readers will not understand what this means -- later you refer to 'dripping paint' features in Baldwin and Dunkerton (2001) -- only then does it become clear what you mean.

Response: The first occurrence of “dripping” has been replaced with a clearer description (“negative”)

87: 'This study is mainly concerned with two questions: (1) What causes the inter-case difference in the DW influence on the troposphere although by definition the NAM during DWs shows downward propagation to the lower troposphere? (2) What can we learn from the flavor identification for DWs?' -- as the referees have commented -- you cannot directly address (1) -- you cannot find a 'cause'. Then you need to explain what you mean by 'the flavor identification' -- this is the first time you have mentioned the term. Then you need to include a sentence that makes it clear while you cannot answer (1) you believe that the 'classification' -- i.e. the 'flavor identification' -- why use 2 different terms for the same thing? -- provides useful information that may in due course help to answer (1).
Response: The term “flavor identification” has been deleted. An explanatory statement has been added to clarify that the classification provides a useful organizational framework that may help guide future studies, without claiming to identify causes.

“While the present analysis does not aim to establish a direct causal mechanism for these differences, it highlights robust and systematic contrasts among DW events. This identification provides a physically interpretable framework for organizing the diversity of DW events and offers useful information that may help constrain and guide future investigations into the mechanisms governing their tropospheric impacts.” (L93-97)

156: 'we further divide DWs based on the inland temperature anomalies' -- these are surface temperature anomalies? I don't believe that you have stated that explicitly.
Response: We have explicitly stated that the inland temperature anomalies refer to near-surface (2-m) air temperature.

270: 'random sample of 2000 winters' -- in WACCM presumably.
Response: We clarify that the climatological PDF (grey curve) is based on a combined pool of 2000 randomly sampled 60-day winter periods from both ERA5 and CESM2-WACCM.

566: 'The mean intensity of NDW events in terms of the NAM and the circulation anomalies in the stratosphere and troposphere are nearly half weaker than DW events.' -- this comes back to the criticisms of the referees -- to what extent does this difference results from the difference in definition of NDW and DW events? Make it clear what does and what does not.
Response: We have added a clarifying statement noting that the weaker intensity of NDW events partly reflects the event definition.

570: 'dipping' should be 'dripping'
Response: Corrected.

573: 'An isentropic vorticity analysis reveals that' -- that should be 'potential vorticity analysis'.
Response: Corrected.

635: 'Further, the distribution of various types of events (see Table 1) shows strong interdecadal variability in past decades. Whether this change is an internal climate variability or forced by global warming due to anthropogenic emission is still unknown, worth exploring in the future.' -- to be honest, the way in which the information is presented in Table 1 makes it very difficult to determine anything about interdecadal variability apart from the fact there is some.

Response: The paragraph has been removed, as Table 1 does not support a robust assessment of interdecadal variability.