

Response to reviewers (reviewer comments in black, authors in red)

To Reviewer #1

We made the technical corrections and removed the phrase “continue to” in our conclusions concerning the need, or lack thereof, of in-situ production from energetic electrons. I should note, that Siskind et al (2015) reached the same conclusion for the 2009 SSW event and Shepherd et al (2014) reached the same conclusion for the 2006 SSW event. It does seem like the beginning of a pattern. However, the third sentence of the paragraph discusses alternative scenarios where energetic electron precipitation would be important and why the present simulations may not be of complete general applicability.

To Reviewer #2

Motivated by the reviewer’s comments, we looked further at the question of where the transport originates. We have added new text on page 6 concerning the possible occurrence of longitudinally-isolated occurrences of transport from above 90 km into the lower mesosphere and document that we did not find any. I do believe this discussion strengthens our argument. Based upon that, we continue to be very reluctant to add another figure on w^* as the evidence continues to support our argument that transport from above 90 km is not relevant to this event. And even if descent from above 90 km had been occurring in specific longitude sectors, w^* would not be an appropriate diagnostic as it is, by definition, a zonal mean variable.

Responses to other comments (reviewer comments in black, authors in red)

P4, line 15: In fact, the SD-WACCM simulations of the same 2012/2013 event in Orsolini et al. (2017, cited) were also made with both $Pr=2$ and $Pr=4$. Their conclusion was that the high diffusion run did enhance the transport of NO in the mesosphere, where GWs break, but not in the LT.

We added mention of Orsolini’s work. While they may differ in the specific details of where the transport was perturbed, both their work and Hendrickx are consistent in arguing against our use of higher diffusion in our simulations for this event.

P14, line 20): the authors argue that the equatorward transport is stronger in WACCM-X than what is inferred from SOFIE or ACE. It is mentioned “as discussed in the context of Figure 8...” Shouldn’t Figs 13 and 14 be rather mentioned here? Or do the authors refer specifically to the WACCM-X equatorward meridional velocities in Fig 8 between 1.0 and 0.1 hPa (incidentally, similar to NAVGEM) ? Please clarify.

Yes, good point, thank you. We’ve added mention of those two figures.

Typos/English/Wording

P2, line 7: (Langematz and Tully, 2018) corrected

P2, line 24 (also P3, line 6): I wonder if the term “long range transport” is the best choice of terminology since it has a connotation of long-range (e.g., pollutant or aerosol) horizontal transport, esp. for readers of ACP. Here it is meant deep vertical transport across atmospheric layers. We added the word “vertical” to clarify.

P5, line 15: remove “bf” before Figure 1 fixed

P17, line 12: Smith-Johnsen **corrected, sorry for the misspelling**

Caption of Fig 4: Indicate that stars and diamonds taken from Orsolini et al. (2017) are from SMR satellite observations. **done**

Caption of Fig 2: remove “This tracks” **done**