

Interactive comment on “Persistence of upper stratospheric winter time tracer variability into the Arctic spring and summer” by David E. Siskind et al.

Anonymous Referee #1

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In this manuscript authors use SOFIE CH₄ and MLS (ClO and Ozone) data to show persistence in the upper stratospheric CH₄ that is linked to mesospheric downward descent and horizontal transport in the upper stratosphere. Authors also quantify its effect on the upper stratospheric ozone as ozone loss via ClO_x cycle is also modified by the background CH₄ concentrations. Then authors try to present 4 categories of this transport (a) weak wintertime downward descent, weak horizontal transport, (b) same as (a) but strong horizontal transport, (c) strong wintertime descent but weak mid-to-high latitude horizontal transport and (d) same as (c) but strong horizontal transport. They clearly show strong relationship between SOFIE CH₄ and MLS ClO as well as MLS O₃ and SOFIE CH₄. These results are supported by chemistry climate model

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WACCM nudged with NOGAPS-ALPHA data. Overall this is well written scientific manuscript that helps to improve our understanding about upper stratospheric/lower mesospheric chemistry and should be published in ACP, if authors can address some the comments listed below.

Major Comments: (a) I think authors have good understanding about various mesospheric/stratospheric dynamics but that is not well explained in the manuscript. Adding extra paragraph explaining differences between stratospheric and mesospheric dynamics should be included. This would help non-expert reader to get better understanding about background dynamics. My understanding is that mesospheric dynamics primarily driven by breaking of gravity waves that can propagate at higher altitudes whereas long planetary waves break at lower altitudes thereby driving stratospheric circulation (although nothing is completely decoupled, and both the circulation have horizontal and vertical component). But annual cycle in mesospheric and stratospheric circulation is out of phase. Also instead of descent, try to say “mesospheric descent”. And when you are discussing “spring-time wave activity”, I think you meant to say horizontal transport from low-to-high latitudes in the stratosphere.

(b) Main conclusions are primarily based on SOFIE data but it’s description is very short. These profiles must have been some validation against other instruments. Please include some references. Also give some information about the retrieval and instrument errors.

Minor comments: i) line 5: How do define “descent” and “spring-time wave activity”. These two are not exactly opposite terms.

ii) Line 28: it’s not just propagation; it is “breaking of waves” that is more important.

iii) Line 81: What about rewording it as “Validation of MLS CIO against . . . Is presented in Santee et al.,

iv) Line 90: Is it simple linear interpolation or you use information form SOFIE averaging

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kernel (which should be scientifically correct method).

v) Can you comment on problems in MERRA data

vi) Figure 1: Why 2012/13 is not there?

vii) Lines 160-165: What altitudes?

viii) Line 166: Also give altitude or 1.47 hPa

ix) Minor suggestion: Why you can't show HCl?. That would have help you present complete story. Do you think any sudden changes HCl trend in the NH can be explained with changes in CH₄ transport (Mahieu et al., 2014, Nature).

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1037, 2016.](#)

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