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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence from IASI of a speeding up in stratospheric O_3 recovery in the Southern Hemisphere contrasting with a decline in the Northern Hemisphere" *by* Catherine Wespes et al.

Anonymous Referee #1

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In their manuscript, Catherine Wespes and coworkers investigate stratospheric ozone changes from 10 years of global IASI satellite measurements. This is an important topic and the availability of IASI measurements over a sufficiently long period provides now an important new data set with high spatial and temporal coverage. The study is performed carefully with the required statistical rigor. The manuscript is well written and I recommend publication in Atmos. Chem. Phys. after consideration of the following comments (which in particular for the two general comments may require more than just minor corrections):

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Discussion paper

I have one general comment that relates to the simultaneous use of the linear trend with a VPSCxEESC proxy in the multi linear regression: How well can the linear O3 trend be determined at high latitudes (in winter/spring) when part of this change (through the EESC factor) is already included in the MLR? The combined effect of EESC and linear trends in polar regions is briefly addressed at L.690, but I could not find a discussion on the effect on the trends. This is in particular important in the light of the strong statements made: "To the best of our knowledge, these results represent the first detection of a significant recovery in the stratospheric and the total O3 columns over the Antarctic from one single satellite dataset."

I have to say that I am skeptical about the robustness of the speeding up of the trends in recent years, given that these trends are evaluated over really short periods only. Although the authors have done the analysis with statistical rigor, linear trends over periods as short as 2 years (2015-2017) are prone to changes in atmospheric dynamics and circulation (or other factors) that may not be perfectly captured by the MLR proxies. I (strongly) suggest that the authors consider a more careful wording in the conclusions and abstract, stating the evidence for the speeding up of the trends, but also the inherent uncertainties.

Specific comments:

L.72: Is this true for both hemispheres, or only NH?

L.83: "sensitive" does not seem the right word here. Sensitive to what?

Section 2.2: It would be good to have an explicit formula for the MLR included here, in addition to the reference to eq. (1) in Wespes et al. (2016).

L.210: A few more words on the GEO and PV proxies would be helpful. Although L.372 states that their contribution is generally small, their use in ozone trend studies is not common practice, so some reference to their purpose and how and why they improve the fit is justified. Are these proxies lat/lon dependent?

L.357++: SF: energetic particle precipitation (solar protons and also electrons) can also lead to enhanced ozone destruction in the MUSt through NOx catalysed cycles. The main effect of a solar proton event in the MUSt is actually to decrease O3 (and only to second order to decrease O3 destruction).

L.380++: EPF: I am surprised that the correlation of IASI O3 with EPF is small at low latitudes: Weber et al. (2011) note a rather strong anti-correlation between tropical total ozone and extra-tropical EPF.

L.474: suggestion “N.H. mode” -> NAO

L.480: Just as a note: It may also be that large O3 changes impact on the AAO

L.514: “if the influence of ENSO on stratospheric O3 measurements has been reported”: the word “if” seems a bit out of place here as clearly the influence of ENSO on stratospheric O3 has been reported in the cited studies.

Technical corrections

L.233: “EFP” -> “EPF”

Additional Reference:

Weber et al., The Brewer-Dobson circulation and total ozone from seasonal to decadal time scales, Atmos. Chem. Phys., 11, 11221–11235, 2011.

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