

Interactive comment on* “Evidence from IASI of a speeding up in stratospheric O₃ recovery in the Southern Hemisphere contrasting with a decline in the Northern Hemisphere” *by Catherine Wespes et al.

Anonymous Referee #2

Received and published: 21 May 2019

First review of manuscript entitled “Evidence from IASI of a speeding up in stratospheric O₃ recovery in the Southern Hemisphere contrasting with the Decline in the Northern Hemisphere,” by C. Wespes et al.

This manuscript describes ozone trends and variability in the IASI record from 2008 through 2017. A multiple linear regression model is used to isolate various natural sources of variability from the anthropogenic trend caused by CFCs, represented over this time period as a linear function. IASI measures ozone in four broad vertical ranges. Here the middle to upper stratospheric layer and the lower stratospheric layer are ex-

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amined, as well as the total column ozone. This manuscript is largely an update of Wespes et al., 2016 but includes 4 more years of data.

The manuscript is well written, though there are occasions where the wording is confusing, likely due to language issues. The analysis is well structured and easy to follow, and the work is well referenced. The figures are clear and informative. However I suggest a major revision is needed because I question the results shown in Figure 12 on the ozone change rates, and the authors make some strong overall conclusions based on this analysis (including the title of the manuscript). If these issues can be addressed I believe the manuscript is a useful contribution to the ozone trends body of work and worthy of publication.

Major Comment: My primary comment concerns the analysis and conclusion that the ozone response to CFCs is changing in time. The authors base this conclusion on a series of linear fits over varying time periods, which show sharper trends (both positive and negative) in the most recent data relative to trends in the record from earlier start points. The series of trends is computed after the sources of natural variability, as fit over the full IASI time period to the most relevant proxies, are removed. Nevertheless there will still be variability in the time series that has not been perfectly captured by the regression model. If that variability has autocorrelation on a longer scale (months), a tendency for the data to be high or low at the beginning or end of the record, which might actually be due to uncaptured noise, will disproportionately affect the trend. If this is the case, such a variation at the end of the record will have successively more influence as the fit period gets shorter, as the end point of each fit is the same. If I understand correctly, the associated uncertainty plots in Fig. 12 tell us that each trend is different from zero trend at the 95% level, but that does not mean that the trend fit over the last 2 years is different from the trend fit over the last three years or last 4 years at the 95% level. For example in the SH high latitude LST the initial trend is ~ 1 DU/yr with uncertainty of say 0.25 DU/yr (difficult to tell exact numbers from the contour plots) and the final trend is approaching 2.5 DU/yr with an uncertainty of close to 1.5

DU/yr, meaning the initial and final trends are not statistically significantly different or only barely so, depending on the exact numbers.

I believe a more appropriate approach would be to fit trend segments over the same length of time, with varying start and end points. The authors could compare the time evolution of trends over 2-yr segments, 3-yr segments, 4-yr segments and longer. The 2-yr segments would be the trend fit from 2008-2009, 2009-2010, 2010-2011, . . . , 2015-2017. 3-yr segments would be 2008-2010, 2009-2011, . . . , 2014-2017 and 4-yr 2008-2011, 2009-2012, . . . , 2014-2017, and so on. In this way both the start and end point will vary, and each fit has the same length, such that the uncertainty is similar across the fits. If the results show consistent changes in time in the fit trends that are greater than the inherent uncertainty, this would indicate a change may be taking place. As the segments get longer (4-yr +) the change in trend will be less from segment to segment, but so will the uncertainty threshold that must be met to show significant change. So the authors can check for consistency in the trends within each segment length vs. time and consistency between 2-yr, 3-yr, 4-yr etc. . . segment results to determine if there is a shift in the ozone change rate.

I also believe showing some example time series of the data being fit, after the other variations have been removed, would be very useful in this particular analysis.

Finally, when doing this analysis, is the VPSC term also removed, or is this term considered part of the ozone response to CFCs and thus left in the time series? Similarly, in reference to the jump in the data in September 2010, although this may be small relative to the full trend, does this jump influence the results of the time dependent trend analysis shown in Fig. 12, or has it's effects been removed before fitting these trends?

Additional Comments:

Can the authors say more about the difference between fitting a daily record and a monthly mean record? I know this was addressed in the 2016 paper, but I am particularly interested in the error analysis. Is the daily autocorrelation similar to the monthly

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autocorrelation? For long-term trends, the uncertainty is more impacted by correlations in the residual on longer time scales rather than day to day variations. Is the lag-1 autocorrelation term used to scale the uncertainty similar when considering daily data and monthly data?

Although I appreciate not wanting to add too much to the paper, I think it would help the reader to repeat the basic equations defining the multivariate model in this paper. At different times three different papers are referenced for equations concerning the model. I think it would be easier to just include all relevant equations in this paper, including the normalization equation.

Very little is said about the seasonal cycle, though the model description includes terms for the annual and 6-month harmonics (pg 5.). Can the authors comment on the seasonal cycle, and particularly do they see the seasonal cycle interacting with EPF and VPSC, which are both also correlated and look very seasonal in nature. Similarly on the interaction between EPF and VPSC, in Fig. 7a in the NH high latitudes the ozone variability explained by the proxies for EPF and VPSC are similar and well above the variability of the actual IASI ozone. Is this another way of showing that the two terms falsely depict variability that isn't in the actual data, but that variability cancels when the terms are added? Have the authors tried fitting to one or the other of the terms, rather than both terms? Particularly in the Austral Spring, where the authors believe the VPSC signal is real, is the amplitude of that signal sensitive to whether or not EPF and/or the seasonal cycle are fit?

Can the authors discuss comparisons between IASI total ozone and other sources of satellite total ozone measurements? It is difficult to compare trend values presented here with previous studies (Weber et al for example) because of the different time periods fit, and zonal mean vs high spatial resolution gridded trends. Have IASI total ozone trends been directly compared to trends from any of the other total ozone satellite records? It would be very useful to also see how the data themselves compare in total ozone, either through reference to previous work or in a comparison plot in this

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manuscript.

Can the authors address how the seasonal averages are constructed? In particular, the authors specifically investigate the JJA trends over the South Pole and Antarctica, but it appears from Fig. 4a there is very little is any coverage in the deep winter at polar latitudes, but that coverage increases with latitude towards the equator. Are the JJA averages for each grid point made with any available data, or is a threshold set, and does the coverage vary with latitude in the polar regions in Figure 10 and 11?

Detailed Comments/Language/Typos

The use of the absolute value signs around the trend values was a bit confusing. I can see this when talking about the amount of time needed to detect a trend of $|x|$ DU yr⁻¹ because this can be a positive or negative trend, but in other cases the authors state the trend is positive or negative, and in that case it is unclear why the absolute value designation is needed. For example on page 15, the absolute value bars are not needed in lines 561 and 564. In line 591, is this a positive trend of 1.5 DU/yr or do you mean positive or negative? If the authors do not mean to say this value can be positive or negative, I would suggest removing the absolute value bars and just stating positive or negative (such as in line 594, positive is stated so the bars can be removed, to me at least the bars imply positive or negative).

L12 should this be > 25hPa or < 25hPa? Since the units are in hPa I suggest it is < as in 25 hPa and lower pressures.

L34 in a lesser -> to a lesser L41 introduce O3 after ozone L43 gas. In the stratosphere . . . L45 for regulating -> to regulate L45 introduce chlorofluorocarbons here, at first use of CFCs L47-48 suggest These latter are the origin of the massive -> CFCs cause L46-54: In general, I don't think the timing is correct is this introduction to the phase out of the CFCs. At the time the Vienna Convention was ratified, and the MP for that matter, it was not yet proven that CFCs were the cause. The Vienna Convention was ratified based on the theory that CFCs could cause ozone destruction; I don't believe

the Farman paper was even released yet. All this to say, even though this is just an introductory paragraph I think it is important to be precise on the history, the implication in the wording is that the ozone hole was discovered first and everything else was a reaction to that discovery.

L56 Suggest removing first phrase, and start sentence as A recovery from . . .

L59 This is decline of CFCs in the stratosphere, correct? L61 confirmed -> identified L67 polar region -> polar regions L68 No reliable estimates of long-term trend -> Statistically significant long-term recovery in total O3 column on a global scale has not yet been observed, likely because . . . L71 low -> lower L75 I believe there are other references here as well. Check Wargan, K., C. et al. Recent decline in lower stratospheric ozone attributed to circulation changes. Geophys. Res. Lett., 45, no. 10, 5166-5176, doi:10.1029/2018GL077406.

L81 controversy -> uncertainty L82 sensitive -> difficult L109 applied on -> applied to L110 remove 'of' L172 and contrasts with -> rather than L178-180 the effect of the jump is found small enough to explain the trend? I'm not sure what the authors mean here.

L192 In order to unambiguously -> In an effort to unambiguously (we try to separate unambiguously, but it is never perfect)

L209 of the mixing L270-272 I'm not sure what the authors are trying to say here. Including the equations would help here. There is already a seasonal cycle in the original model, so it is not clear how the seasonal terms are added. Is this the equivalent of 4 separate runs, one for each season? Equations would also clarify how the seasonal MLR is used after the annual MLR is run. L285-288 suggest for clarity not switching the order of the reported results, in L288 LSt goes first and in 291 MSt is reported first.

L302 counteracted -> counteracting (this may occur in other places as well in the text).

L 321 suggest adjusted signal of the proxies -> reconstructed proxies L333 shows up

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as a typical ... L347 MUST, (remove 'n') L360 records -> values L392 deployment -> formation L414 remove 'have' L460 in the case of prolonged ... L555 I do not see polar trends reaching 2.5 DU/yr in the MUST? The trends are positive in the NH pole but negative over Antarctica, and the scale only goes to 2 DU/yr.

L560 The authors call out the similarity between the MUST and LSt with both showing high positive trends at southern polar latitudes, but again at the pole the MUST trend appears negative, though the trends at southern high latitudes are positive. This description seems a bit confusing and doesn't seem to match Figure 8.

L596 an additional ~ 7 years L599 suggest The longer required measurement periods at high latitudes is due to the larger residuals in the regression fits (i.e. largest sigma e) at these latitudes (see Fig 4 a and b). L613 is there a reason the authors occasionally switch to DU per decade? If not, I suggest keeping DU per year. At first I could not understand why such a large value of 15 was used, then I saw it was DU per decade. L623-624 again it seems the increase in total ozone at high southern latitudes is dominated by the LSt result over the pole though both layers contribute in the latitude bands surrounding Antarctica, comparing to the results in Fig. 8.

L652 summer -> austral winter L674 over Antarctica (remove 'the') L686 what makes the negative trends here unrealistic?, It seems that the large positive trends off the coast of Antarctica have a similar detection length. I see that there is a bit more uncertainty in the fit in the negative trend region, but to say they are unrealistic requires more specific evidence, such as a time series showing the failure of the fit. I suggest the authors either provide more evidence or simply note that the area of higher negative trends is associated with a higher residual from the model. Could it also be something that is happening in the troposphere that is affecting the total ozone trend.

L696 Salomon -> Solomon

L705 This is just a suggestion, but to make the interpretation for the reader easier, could the authors provide the relevant IASI mean ozone values (or climatological values) so

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the readers can translate between DU/yr and % per dec when comparing results from other studies.

L766 suggest However, a longer period of IASI measurements is needed to unequivocally demonstrate a positive trend in the IASI record.

L775 additional measurements for the trend to be unequivocal. L781 suggest These results verify the efficacy of the ban on ozone depleting substances imposed by the Montreal Protocol and it's amendments throughout the stratosphere . . .

L788 and it likely -> which likely L807 in the near future L809 extent -> extend

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-206>, 2019.

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