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Interactive Comment

Interactive comment on "Ozone variability in the troposphere and the stratosphere from the first six years of IASI observations (2008–2013)" *by* C. Wespes et al.

Anonymous Referee #2

Received and published: 10 December 2015

This manuscript describes the temporal evolution of ozone in the stratosphere and troposphere from 2008-2013 using observations from the IASI instrument on MetOp-A. Regressions are performed for zonal mean ozone in four atmospheric layers from the troposphere to the upper stratosphere, with the time series fit to constant, linear, and annual harmonic terms as well as known geophysical drivers of ozone variability. This investigation is well done in general but fails to account for lags between the geophysical quantities such as QBO and ENSO and ozone variability in non-local regions. Following this analysis, the authors make a fairly convincing argument that the density of the IASI measurements allows for trend analysis even over short timescales, though the comparison between FTIR instruments and IASI should be more carefully

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Interactive Discussion

Discussion Paper

done. The paper includes supplementary information with a model analysis aimed at quantifying how much the tropospheric ozone signal seen by IASI is "contaminated" by stratospheric ozone. It is unclear to me, however, that their approach quantifies how much of the tropospheric ozone that IASI sees is stratospheric due to the "smearing" of the averaging kernels (what they are trying to quantify) versus stratospheric air that has actually been transported into the troposphere. Given the important results presented here and the overall quality of the work, I recommend publication in ACP once the following comments have been addressed.

Abstract

Page 27576, Line 3: Should there be a "the" before MetOp-A?

Page 27576, Line 5: "time development" is a little awkward – "temporal evolution" would be better

Page 27576, Lines 15-19: The attribution of the trends is somewhat overstated in the abstract as compared to the paper. Perhaps "which is consistent with other studies suggesting a turnaround for stratospheric O3 recovery" and "possibly linked to the impact of decreasing ozone precursor emissions" would be more appropriate.

Introduction

Page 27577, Line 7: Suggest replacing "present" with "undergoes".

Page 27578, Lines 17-18: The wording is difficult to follow. I suggest "…by the possibility of using IASI measurements to discriminate O3 distributions…."

Section 2

Page 27579, Line 16: Given IASI's 9:30 / 21:30 overpass time, I would not expect it to be as sensitive to changes in precursor emissions as instruments with afternoon overpass times. Have you used a model to assess this and what implications the overpass time might have in terms of quantifying the true trend associated with decreases in

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precursor emissions?

Page 27581, Lines 8-17: Please see comments on the supplementary material. It is unclear to me whether the model analysis the authors performed actually quantifies how much of the tropospheric ozone that IASI sees is stratospheric due to the "smearing" of the averaging kernels (what they are trying to quantify) versus stratospheric air that has actually been transported into the troposphere.

Section 3

Page 27581, Lines 20-21: Why just ODS-driven trends here? The authors are not specifically talking about stratospheric ozone, and in fact address tropospheric ozone trends driven by precursor emissions. Also, in both instances in this sentence, "trend" should be preceded by "the" or should be plural.

Page 27582, Lines 9-14: It would be very helpful if the authors could provide more detail here. Why was the time lag for the autocorrelation of the residuals assumed to be 1 day or 1 month? Given the lifetime of ozone, might it not be longer? What method was used to correct the coefficient estimates by accounting for this autocorrelation?

Page 27582, Lines 14-15: "robust" would be more accurate than "adequate"

Page 27583: In the analysis using geophysical variables, the zonal wind at 10 and 30 hPa are used to represent the QBO. However, it is the QBO shear rather than the zonal wind itself that strongly affects the zonal distribution of ozone, which responds primarily to the anomalous QBO thermal wind circulation cell driven by the zonal wind gradient. While the temporal evolution of the shear is generally consistent with the temporal evolution of the zonal wind itself, there can be important differences when the descent of a particular QBO phase is delayed or occurs faster than usual. The authors may want to compare the time series of the shear to that of the individual wind components to determine whether using the shear time series might make an important difference in their results.

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Page 27583, Line 17: What is meant by “both components” here?

Page 27583, Line 27: “there is a” is needed before “predominance of easterlies”

General comment on Section 3: For many of the geophysical variables considered here, there may be important lags between the geophysical quantity and the ozone response in particular parts of the atmosphere. Lower stratospheric ozone in midlatitudes, for example, does show a QBO signal, but it lags the QBO winds in the tropics by a few months. Likewise, midlatitude ozone does not respond within a month to ENSO changes in the tropics. Optimizing the regressions including the possibility of time lags may be too involved for this paper, but the authors should at least acknowledge that they may be underestimating the role of geophysical variables in regions outside the location of the geophysical quantity for QBO, ENSO, NAO, and AAO.

Section 4

Page 27587, Line 16-18: Why don’t we see ozone depletion in the Southern hemisphere stratosphere?

Page 27587, Lines18-19: There appears to be a difference between the Northern and Southern hemispheres in terms of how similar the 10 km panel is to the 19 km panel. Is this the case? What is the reason for the asymmetry?

Page 27587, Lines 10-26: What is the source of the noise at high latitudes?

Page 27588, line 3: It would be much clearer to say “color contours” rather than “color scale”

Page 27588, lines 8-14: High midlatitude lower stratospheric ozone values in 2010 have been linked to the combined Easterly shear QBO and El Nino (Neu et al., Nature Geosci., 2014), and the failure to reproduce them with the regression may also be due to 1) the use of the zonal wind as a QBO proxy rather than the shear in the zonal wind and / or 2) the failure to account for lags between the QBO and ENSO and the response of midlatitude stratospheric ozone.

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Page 27589, Lines 20-21: Is it clear that the spring maxima solely reflect STE, or is there also a seasonal dependence of the averaging kernels that makes the lower atmosphere more sensitive to the layers above during spring?

Page 27591, Lines 5-14: The QBO results for the midlatitudes may differ substantially if a lag were considered. The authors should at least acknowledge that by using zonal winds rather than shear and not considering a lag, they likely underestimate the QBO's importance in regions not directly impacted by the QBO winds.

Page 27591, Lines 15-22: The negative ENSO coefficient in the tropical UTLS is consistent with results from Neu et al., Nature Geosci., 2015.

Page 27592-27593, Lines 24-1: I don't understand what the percentages refer to here.

Page 27594, Line 14: need an "increase" in "reports a factor of two in"

Section 4.3.3: I am not sure I agree with the conclusions presented here. The authors show that total column trends from FTIR data and from IASI over different time periods do not agree and use this to argue that the sampling of IASI provides confidence in the determination of trends. However, to actually reach this conclusion, they would need to show that trends between IASI sub-sampled at the same times and locations as the FTIR measurements are consistent with those from the FTIRs (i.e. non-significant) for the overlapping period of the data (2008-2012). Only by doing so can they demonstrate the advantage of IASI sampling and show that it explains the difference between IASI and the FTIR measurements.

Tables and Figures

Table 2: Caption is very unclear – it should say "based on daily (top values) and monthly (bottom values)..."

Supplementary Material

Lines 19-21: Why use constant emissions, particularly when the goal is investigate

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variations in ozone with time? Also, it is not clear whether the emissions are truly constant or simply have no interannual variability.

Lines 30-32: Are representative averaging kernels used, or scene-dependent averaging kernels?

Lines 37-40: This goes back to the previous question – if the averaging kernels underestimate the stratospheric influence on the partial column this could produce a bias.

Lines 47-48: One might expect the model to perform better in the southern hemisphere since the fire emissions do vary with time, but it isn't clear that this is the case. Why not?

Section S.3: Given the large difference between the model and IASI, are the IASI averaging kernels (which must depend on ozone amount) still applicable to the model profiles? Also, the authors are using the difference between total ozone and the tagged ozone in MOZART (with the IASI averaging kernels) as a measure of the stratospheric influence on tropospheric ozone as seen by IASI. However, this term includes both stratospheric air that has been transported to the troposphere and the "smearing" of the stratospheric signal into the troposphere by the averaging kernels. I understood the point of this analysis to be to understand how much the IASI tropospheric column is "contaminated" by stratospheric ozone due to the averaging kernels rather than to understand how much stratospheric ozone matters for tropospheric ozone variability. In this case, wouldn't a better measure be the difference in the difference between total ozone and tagged ozone with and without the IASI averaging kernels?

Line 76: The sentence beginning "This method allows..." should be reworded.

Line 110: Again, I am confused as to whether the emissions have no variability at all or simply no interannual variability.

Lines 120-122: I don't think it's clear from this analysis that the decrease in tropospheric ozone is "much more important than what we estimate from IASI". The emissions-driven portion of it may be, but tropospheric ozone is a mixture of the emissions-driven ozone and ozone that enters through STE, and the authors have not convincingly shown that the “true” trend in this mixture of air is significantly larger than that observed by IASI (because they have not fully separated the influence of STE and the averaging kernels).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27575, 2015.

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