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Atmospheric Chemistry and Physics
Discussions

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 #3

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General overview

This manuscript describes a statistical analysis of ozone variability in the troposphere and the stratosphere based on 6 years of IASI/METOP daily global observations (2008-2013). Therefore, a multivariate regression model is used over different vertical levels and latitudinal bands. The variability is first decomposed using specific geophysical drivers (provided by proxies for solar flux, QBO, ENSO, NAO/AAO), as well as the fitting of constant, linear and harmonic terms, the later with 1 year (Brewer-Dobson) and 1/2 year (solar insolation) periods.
This analysis shows that the fitting procedure allows a generally good representation

of observed O3 variability and an attribution of the associated drivers – to some extent. Once these contributions to the observed variability identified, the trends during the analysed time period have been studied. Results are evaluated through comparisons with similar analysis based on FTIR surface-based observations and model predictions (supplemental material).

Overall, this paper presents a careful analysis, very complete and it is generally well written.
It shows that IASI observations can be used for trend analysis, and that its unprecedented spatial and temporal coverage provides significant additional information compared to surface networks.
Therefore, I think this manuscript is worth publishing in ACP. However, a few points should be considered (see main comments) before publication. Some figures could also be improved (see technical comments). Specific comments on the text is also provided.

Main comments

The motivation for the choice of conducting a multivariate regression to understand the variability observed is not very clearly explained. I guess its main purpose is to be able to eventually extract trends from the full signal, is that right? Section 4.2, providing the detail of each fitting parameter, is missing conclusion comments to guide the reader through the interest of all this work on large datasets. For example: what amount of variability is explained by each process included/fitted. Can it be a way to efficiently analyze controlling processes without using model simulations?

The authors have chosen to provide an analysis of several altitude levels, chosen according to the IASI sensitivity profiles (provided by the averaging kernels).

In Section 2 the IASI retrieval, the AK and associated DOFs are discussed, as well as the correlations between the observed concentrations and the a priori information used as an initial constrain. Although this contribution is around 20-30Also, in Figure 1 the averaging kernels for the selected layers show significant overlap, so that all levels are not independent. This is mentioned throughout the paper but it would really be helpful to provide information on the vertical correlations between vertical levels in IASI. The authors discuss this problem in the supplemental material but I think that their are 2 effects combined in their discussion: the natural influence of the stratosphere on the tropospheric levels through STE, which has to be accounted for to understand observed variability of tropospheric O3 from any type of measurement; and the smoothing from the observational system used here. It is the later that is particularly critical because it may cause artefacts in the trends compared to what would be obtained using in situ observations for example. I guess it would be quite easy to evaluate this effect using the MOZART simulations and the stratospheric O3 tracer with and without applying the IASI AK. Another related question: could correlations in the observations be mistaken for dynamical processes (strat-trop exchanges) in the trend analysis? Because this part is confusing, the fact that tropospheric ozone from IASI can be used for trend analysis is still somewhat questionable after reading this paper... I think this could be easily improved.

Regarding the general performance of the fitting procedure: the figure (Fig. 4) is hard to read. Later in the study, the residuals are shown to actually be quite large (Figs 9 and 10): does this mean that significant processes are not considered? The performance should be better described and discussed at the beginning of section 4.2, with a dedicated figure.

Another point that needs to be improved: throughout the paper, the authors attribute enhanced lower tropospheric ozone to ozone production from anthropogenic emis-

sions. But there are not only anthropogenic emissions that will contribute to lower tropospheric production, biogenic emissions and fires, for example, will also emit significant O3 precursors. This attribution to anthropogenic activities is not really demonstrated. The comparisons to MOZART-4 simulations in the supplemental material is not that convincing: constant anthropogenic emissions are used, but with daily fire emissions. For the 'realism of anthropogenic emissions', the authors should provide some numbers on previous model evaluations against surface networks. Are there more uncertainties on anthropogenic emissions than on natural emissions? What about stratosphere-troposphere exchanges: is it well simulated by models? Has it been evaluated?

My final main comment is that the discussions generally lack quantitative comparisons to other studies. It is generally written that there is a 'good agreement' in trends but it would be interesting to provide orders of magnitude: maybe with a final table comparing results depending on the method chosen?

Specific comments

Introduction:
P. 27577, l. 21: Why are there warnings? Only because too many unknowns or are their specific reasons?
P. 27578: 1st paragraph: It would be helpful to provide a few numbers for trends identifies in previous studies. Are the signs consistent? What were these studies based on?
Observations at what resolution? etc.

Section 2:
Cf. main comment: a better evaluation of vertical correlations is the observations is

required. Also provide information on the a priori used for the retrieval.
The last sentence is disturbing and does not really help the reader… It would be good to confront each result to the identified sources of uncertainties in the discussion parts. 30 to 60

Section 3:
Define ODS
l.12-13, P.27582: I am not an expert in this type of statistical analysis but I just don't get what this means. Maybe a little bit more explanations would be helpful.

Table 1 and corresponding text: the source of the data used should be detailed for all proxies: are they from model simulations? reanalyses? observations? Sentence btw P. 27584-P. 27585: Would be good to detail briefly why harmonic and linear trends are appropriate for these effects, since these are among the main targeted features of the analysis.

Section 4:
Also, the discussion should include reminders about expected uncertainties (that had to be kept in mind). For example P. 27586, last paragraph about the results in the tropics: what about the large contribution from the a priori? Does it matter in this discussion of the variability?

Several mention of the impact of anthropogenic emissions that has not been demonstrated: what about other sources? (Cf. comments above)

Section 4.2: Cf comments above about the residual.
A lot of the processes have already been identified in the previous description of the O3 variability. This description could be a lot shorter, avoiding repetition. More

importantly, it would be helpful to clearly explain why the regression is performed and what we learn with the coefficients plotted, what we learn in terms of O3 variability.

P.27589, last sentence: this statement seems really strong and is not well demonstrated. MOZART4-GEOS5 has probably already been compared to surface observations of O3 and NO2 in many regions, very probably providing a better evaluation of the quality of the anthropogenic emissions. Authors could provide a few numbers from the literature here.

P.27590, l. 13-14: Unless I missed it, this is the 1st mention of these numbers regarding the performance of IASI. This should be in section 2. And in this section, the authors should explain if the findings are still relevant considering this uncertainty (bias in this case).

It would be helpful to add a short conclusion at the end of section 4.2 to clearly explain how/if the multivariate regression approach provided original information, or if it is mainly used to get rid of other factors than the trends the authors are aiming at identifying.

Section 4.3.1: Results only discussed here for the US layer. What about other layers? Similar conclusions (I guess) or less critical to have daily obs? What about previous studies cited throughout the paper: what temporal resolution are they using?

Section 4.3.2: Provide numbers for trends obtained in the literature (cf general comment above).

Section 4.3.3: Are trends consistent if the full IASI dataset or IASI at the FTIR location is used? Hence: do we really need such high coverage to conclude? For regions where trends are insignificant if inferred from FTIR: same result if IASI used only when FTIR observation is available?

Conclusions:
P. 27598, l. 19: ‘reasonably independent’ is too vague considering that it is critical. It needs to be better evaluated.
Last sentence: I have not really seen this conclusion clearly appearing in the results.
P. 27599: I do not really agree with the conclusion on anthropogenic emissions as well since it is not really demonstrated here, and not referenced.

Supplemental material

- Information on the O3 tagging technique should be briefly provided in the S.1 section.
- I do not agree with the conclusions provided.
- 1: I do not see how the authors conclude that it’s a problem due to anthropogenic emissions.
- 2: The contribution from STE exchange does not really help with the problem of IASI’s vertical resolution (Cf. comment above). A comparison of simulated contributions with and without smoothing would provide some indication on both aspects.

Technical corrections

P. 27576, l. 2: on the METOP-A platform

P.27578, l.1: STE after ’exchange’.

P.27578, l. 8: define GCOS.

P.27582, l. 20: with a period...

P.27586, l. 11: late winter

P. 27600: Crevoisier et al.

Table 3: why ‘Feb.-Oct’ and ‘Oct-Apr’? What do blank lines correspond to? (only ‘-‘)

Figure 2: titles are too small.

Figure 4: No (a) and (b) in this Figure... The lines for the partial columns are hard to read, I suggest adding a specific figure. Small black titles in the figures are hard to read, real titles would be clearer.

Figure 6: Color scale could be adjusted for the bottom plot since magnitudes change for each plot.

Figure 7: in the legend, specify that the legend for the colors is provided in the top-right panel.

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