

## ***Interactive comment on “Lower tropospheric ozone over the North China Plain: variability and trends revealed by IASI satellite observations for 2008–2016” by Gaëlle Dufour et al.***

### **Anonymous Referee #2**

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This manuscript deals with an important question of current interest, i.e., the discrepancy of O<sub>3</sub> trends over China derived from different satellite sensors (UV IR), and surface measurements. Specifically, trends derived from the IASI instruments are negative, while those derived from ground stations are positive. The authors address the following issues: a) attribution of trends to both changes in emissions and meteorological parameters; b) examination of the stability and robustness of the trends derived from IASI and implications for derived trends. Overall, this is a strong manuscript. The authors have been very thorough in addressing the different issues involved, and their approach sets up a path for future data analyses and modeling efforts.

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A general comment is that, given the multiplicity of issues discussed, the Conclusions could be more detailed in terms of the specific questions addressed. These conclusions are dispersed in the different sections, but it would help if a summary is given as to, for example, conclusion on attributions, impact of retrieval parameters, comparison to sondes and surface stations, and issues of sampling. Most important, after this study, what is the status of the original question posed above?

Specific comments:

1. Abstract, line 24: Replace "as well" with "similarly"
2. Abstract: It is very descriptive of the different issues/approaches, but it seems to be missing an obvious conclusion (even if it is a "negative" one).

Page 2, line 7: Somewhere in the manuscript there should be more said about the potential discrepancy with MOZAIC data, particularly since the hypothesis is proposed that different regions of the troposphere are NO<sub>x</sub> or VOC limited. I presume that a lot of the MOZAIC data comes from higher altitudes in the LT, which would also be where IASI retrievals are most sensitive.

Page 4, Section 3.1: Discussion of the time series analysis should have some clarifications. References should be given for "Theil-Sen estimator" and "Mann-Kendall test." Use of certain terms, such as "climatological index" are unclear, and should be quantitatively defined through an equation, and not words.

Page 5, line 6: "amplitude", not "altitude"

Page 5, lines 11-12, and elsewhere: The authors note the change in O<sub>3</sub> in 2013, but it is not clear why this happens. If we do not have a reason it should be stated as an outstanding question.

Section 3.3, explicative variables: This is a very good discussion. One item that should be discussed in more detail is why the NO<sub>x</sub> abundances in the LT are not correlated with the O<sub>3</sub>. I would expect some correlation away from the surface?

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Page 7, line 10: "A period for which strong stratospheric intrusions..."

Figure 4 and Table 1: I found Figure 4 difficult to interpret. The residuals are, of course, all over the place, and impacts are sometimes difficult to see, for example, the impact of PV on residuals at beginning of 2003 (it's only one point!). The general trends become slowly apparent, but the large variations in residuals mask some of the impact of the different explicative variables. I find that Table 1 does a much better job of succinctly summarizing the results, which is harder to get from Figure 4. One suggestion for Table 1, etc., is that the same analysis be carried out for the attribution to the trends in the 2013-2016 period. Given that this period has a large impact on the overall trend, it would be interesting to see how the attributions work for this period.

Page 9, lines 5-6. "similar to the trend derived from the data with no filters'?"

Page 9, lines 22-25 and Figure 6. What is being plotted in figure 6, i.e., what are the units for the "AK stability" in the y-axis?

Page 10, line 4: Should read: THUS, the comparison of IASI-A and B...

Page 10, line 15: "The criterion on the time difference has been RELAXED

Page 12, line 8: "We consider daytime surface observations only ON the days

Page 12, line 10: "a recent study shows THAT the downward mixing..."

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