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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and temporal changes of the ozone sensitivity in China based on satellite and ground-based observations" *by* Wannan Wang et al.

Anonymous Referee #1

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In their manuscript "Spatial and temporal changes of the ozone sensitivity in China based on satellite and ground-based observations", Wannan Wang et al. report on a study using OMI NO2 and HCHO columns to investigate where in China photochemical ozone production is NOx limited, and where it is VOC limited. In contrast to earlier studies using similar approaches, the classification here is based on linking satellite columns with in-situ surface ozone observations to empirically determine thresholds for the HCHO to NO2 ratio. The method is applied to data from China for the years 2016 to 2019 to investigate changes in photochemical ozone regime. Measurements from the lockdown phase in spring 2020 are included as a test case on the effect of strongly reduced NOx emissions.

The manuscript is clearly written and fits well into the scope of ACP. The results are not really surprising but add information on a relevant topic in particular in view of what the best approach is to reduce ozone levels in China. I have however concerns with the basic approach taken to identify the NOx and VOC limited regimes, which is the key point of this study and not trivial. In my opinion, this needs to be better explained and justified before this manuscript can be accepted for publication in ACP.

Major comment

The one new thing in this study is that the different ozone chemistry regimes are defined from measurements alone and not from models which explicitly determine it for each location. This is an interesting approach but I do not see strong justification for it in the manuscript. What the authors use in Figure 1d is a display of monthly mean ozone surface concentrations as a function of the ratio of formaldehyde to NO2 columns. While it is tempting to look for a maximum in the ozone curve and define this as the separation between NOx and VOC sensitive domains, this is not necessarily justified. The FNR determines the *change* of ozone levels in reaction to a change in NOx or VOC concentrations, but not the total ozone concentration itself (as can clearly be seen in the graph). An argument can be made that by limiting the analysis to the highest values, we are indeed looking at local ozone chemistry and assuming that everything else remains unchanged, absolute ozone levels should reflect ozone production but more justification is needed to make this approach convincing. I think that adding the threshold lines in Figure 1c may help to make the argument.

Thresholds are defined from this figure in a manner not clear to me but this is of course the key question: What are the correct thresholds, are they the same throughout China, are they valid in all seasons / meteorological conditions, do they change over time as emission patterns change? None of this is discussed and this needs to be added.

Minor comments

At some point in the manuscript, a short discussion of the problems in using columnar data instead of surface concentrations is needed, as well as of the question, in how far monthly averages are representative of the highly variable concentrations and the strongly non-linear NOx-O3 chemistry.

L111: Add that quoted resolution for OMI is at nadir

L137: Which product is used in the DESCO algorithm – the QA4ECV product?

L150: Please add some basic information on which type of instrument is used for the ground-based data, which measurement principle is applied and if there are possible cross-sensitivities

L168: If the model is to be used in any quantitative sense, then much more information on initialisation, VOCs used, inclusion of heterogeneous reactions etc. is needed.

L219: Sentence is not clear to me but probably touches on my main point of concern: That the way the thresholds are derived is oversimplified and not necessarily valid for all locations in China

L303: Up to this point, only summer values have been discussed and used. Now, the method is suddenly used for winter values which is problematic and needs at least to be acknowledged and discussed

L358: Here (and earlier), the assumption Is made that VOC emissions have not decreased (much) during the lockdown which would deserve a bit of discussion, in particular as a significant number of studies related to the signature of the lockdown in pollution in China is already available in the literature.

L363: I guess that something should also be said about the availability of the data produced in this study

Figure 1: This figure needs to be revised in several ways:

1. the same colour scheme and scale should be used for model and observations

2. the figures showing observations should not just plot the data on top of each other as this prevents readers from seeing many of them. Instead, a “heat map” type display would be more appropriate showing the binned data

3. I’d suggest to include the threshold ratio lines also in panel 1c

Figure 2: I don’t see the need for the inserted maps in particular as they depict disputed areas without relevance for the study.

Figure 2: Add in caption that HCHO columns are from OMI

Figure 3: Explain arrows in caption

Figure 5: Looking at panels (b) and (c) it is clear that the ozone changes in China are not linked in a simple way to the NOx changes: Ozone decreased very much in Neijiang in spite of very moderate NOx decreases while it increased significantly in Guangzhou where NOx emissions remained basically constant. In Beijing, a reduction in NOx emissions by a factor of 2 from 2016 to 2018 had no effect on surface ozone but further reduction then lead to an increase in O3. This needs more discussion in the text.

Figure 6: Define time periods in caption

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