Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-385-RC1, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Rapid increase in summer surface ozone over the North China Plain during 2013–2019: a side effect of particulate matters reduction control?" *by* Xiaodan Ma et al.

Anonymous Referee #1

Received and published: 4 September 2020

Overview

The paper deals with the rapid increase in summer surface ozone over the North China Plain during 2013–2019 and the hypothesis that this decrease would be a side effect of reduction control of atmospheric particulate matter is examined. I would suggest publication of the paper, after the issues raised below are addressed.

General comments

I think that there are some key measurements missing in order to undertake a proper review of this manuscript. The most important is the lack of hourly in-situ NO2 measurements from the same stations providing the hourly ozone measurements, so that

to be able to check if the ozone increase is directly related to the corresponding NO2 decrease of surface concentrations The reason is that it is already known that for most urban stations the sum NO2+O3=Ox, called also potential ozone is constant (e.g. Kley et al., 1994; Kalabokas et al., 2000), so that any NO2 decrease is directly related with an exactly equivalent increase in ozone (in ppb) through reaction with ozone (NO titration), which is very rapid. The presented data of total NO2 column might provide some indication on that, but it is expected to be less efficient than in-situ measurements.

In addition, the issue of tropospheric ozone and its subsequent influence to the boundary layer and surface background ozone concentrations should be also taken into account. In relation to that, in my opinion, a weak point of the paper is that the levels of measured surface ozone are mainly related to the photochemical ozone production over the examined region of China. On the other hand, the issue of background ozone levels and their variability within the boundary layer and the free troposphere are not (or very little) discussed. For this purpose, I think that it would be quite helpful to take into account a relatively recent extended review paper on tropospheric ozone on global scale, including SE Asia which is one of the most important global tropospheric ozone hotspots (Gaudel et al, 2018, Elem Sci Anth, 6: 39. DOI: https://doi.org/10.1525/elementa.291 and also references therein). From my perspective and based on my expertise of analyzing ozone episodes in the Mediterranean region, I would just point out that the possibility of vertical ozone transport in the troposphere influencing the boundary layer and surface ozone values (a major factor in the Mediterranean, especially in its eastern part during summer but also in its western part during spring) is not mentioned in the manuscript and so all measured ozone is considered to be produced by local photochemistry from precursor pollutant emissions emitted in China only. This might not be always the case, especially during the May-September period when the tropospheric influence to the boundary layer gets its maximum height while at the same time the tropospheric ozone maxima are observed during the same period of the year, with subsequent influence to the boundary layer and surface ozone values depending on the prevailing synoptic meteorological conditions.

In relation to the above, tropospheric vertical ozone measurements over China (e.g. Ding et al., 2008; Zhang et al., 2020) would be needed for a thorough assessment together with tropospheric satellite ozone data. In addition, synoptic weather patterns might influence greatly the tropospheric as well as the surface ozone concentrations (e.g. Kalabokas et al., 2013; Kalabokas et al., 2017) and this issue is not discussed.

Overall, I think that the submitted paper presents some interesting data and ideas regarding the recent increasing trend of surface ozone in China but I think that the above described missing information is essential for a proper review of this manuscript.

Specific comments

Page 2, lines 216-220: This is reasonable as higher NO/NO2 levels increase the ozone destruction in urban and semi urban stations, through NO titration.

Page 9, lines 235-236: This applies to stations with low NO emissions in their surroundings. As mentioned before in most urban and semi urban stations, the NO titration is the controlling factor.

Page 14, lines 379-386: This in fact reflects the preponderant role of NO titration. Lower NO emissions destroy less ozone, which in most stations is originated from the tropospheric/boundary layer background.

Supplement: I would suggest plotting also the average diurnal profiles of pollutants (O3, PM2.5) per season, at least for spring and summer.

Technical comments

Page 26, line 665 (Fig. 3): PBLH (g, h).

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-385, 2020.