

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.

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The paper in its present form is clearly written and could be published after revisions

At this stage of the review process, I would like to add a few points and suggestions to be carefully addressed by the authors:

1. Since the focus of the paper is on the response of ozone to possible forcing processes, I wonder why so little is said about the role of heterogeneous chemistry. There is growing evidence that the increase in ozone is related to the increase in HO₂ due to the reduced scavenging of HO₂ by aerosols (PM) more than a change in the J-values.

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The paper highlights the change in the $J(\text{NO}_2)$ due reduced PM concentrations, but does not provide the quantitative response associated with the change in heterogenous processes. It would be important to discuss this aspect, even if no specific simulation of this effect has been done. 2. A more convincing discussion must be provided regarding the role of the boundary layer, the changing meteorology (e.g., average cloudiness, precipitation, etc.) and the surface deposition processes. Some of these may not be explicitly treated in a box model, but should be discussed with appropriate references. 3. There should be a discussion about the processes that have changed HOx chemistry (which affects the ozone production and loss) and this includes, for example, the HONO and formaldehyde photolysis.

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