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Comment on acp-2022-781

Anonymous Referee #2

Referee comment on "What is the cause(s) of ozone trends in three megacity clusters in eastern China during 2015–2020?" by Tingting Hu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-781-RC1, 2023

This paper analyzes the causes of the 2015-2020 surface ozone increases over three megacity clusters in China (BTH, YRD, PRD) and concludes that increasing Western Pacific Subtropical High (WSPH) conditions are responsible for the ozone increase rather than changes in emissions.

I found the paper difficult to read because it is so chatty, hand-waving, and qualitative and weak in its argumentation. Its central thesis that the ozone trend is driven by the WSPH is in my opinion unsupported and flies in the face of ample literature showing that ozone trends in China over the past decade are anthropogenically driven including as evidence (1) removal of meteorological influence using statistical models, (2) broadening of the ozone season, (3) surge of ozone following the Covid shutdown, (4) consistency with model ozone increases when using anthropogenic emission trends as input. The paper largely ignores this literature, but if it is to make the contrary claim that the ozone trend is in fact driven by meteorology rather than emissions it either needs to refute or show consistency with these different strands of evidence. It does not.

The proposed evidence for a WSPH driver of ozone trends is in my opinion very flimsy. The first piece of evidence proposed is that ozone pollution episodes are becoming more regional, but (a) this does not imply a meteorological trend (witness the US in the 1980s when the same phenomenon was observed), (b) it could reflect the well-known broadening of the ozone season in China (a big weakness of this paper is not resolving the seasonal variation of ozone). The second piece of evidence proposed is the correlation of ozone with temperature and SSR, combined with the trends in these meteorological variables over 2015-2020, but (a) this correlation with meteorological variables is well known, (b) past studies have removed statistically the influence of meteorology on the ozone trend, as is very standard practice.

So I don’t think that this paper should be published in anything close to current form. It could be used to argue wrongly against the urgency for China to decrease VOC emissions, and it will waste the research community’s time in having to debunk it. One interesting

result in this paper is the apparent regionalization of ozone pollution in China, which I don't think has been discussed before. That could provide the basis for a paper but it would need to be better demonstrated.

Specific comments:

Line 65: are only sites with complete records for 2015-2020 used? Otherwise the analysis would be biased by expansion of the network.

Line 83: I didn't see the criteria for separating clean and polluted sites in Table 1.

Line 125: text and captions don't match what is actually shown in Figures 5-7. Comparing just two years of data (2017 vs. 2015)as a trend indicator is obviously bad – of course the difference between two individual years could be meteorologically driven.

Line 136, elsewhere: a big weakness of this paper is not resolving the ozone data by season as is standard practice. In particular, it is not clear to me that this regionalization of ozone could not simply reflect the broadening of the ozone season that has been reported in previous papers.

Line 141: why jump to an attribution to weather? This is characteristic of the weak argumentation throughout this paper. Same thing in line 151 – why would the regionalization of BTH and YRD be very unlikely to be driven by emissions?

Line 184: no one is arguing that the increase in ozone is driven by decreasing NO titration (OK, maybe in winter, but ozone is low then anyhow). The argument is that it is driven by NOx emission decreases under VOC-limited conditions.

Line 187: past studies have attributed the ozone increase to PM2.5 decrease only for summer. Again, the paper would need to resolve its analysis by season.

Line 241, elsewhere: there is nothing new in the attribution of ozone pollution episodes to the WSPH. The lengthy discussion of this attribution is just routine.