

Interactive comment on “Spatial distribution and temporal trend of ozone pollution in China observed with the OMI satellite instrument, 2005–2017” by Lu Shen et al.

Anonymous Referee #1

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This paper seeks to quantify surface ozone across China using the SAO OMI tropospheric column ozone product. While I appreciate this effort to quantify such a relationship, the current analysis has not demonstrated a clear and convincing link between the lower/mid-tropospheric OMI retrievals and day-to-day ozone variability at the surface. I realize that the authors are trying to find some signal in OMI that reflects ozone at the surface, but the degrees of freedom are so small, and the sensitivity to surface ozone is so weak, that there's no real way to distinguish between the signal that comes from the surface and that which comes from 800 or 700 hPa. As presented, the relationship is more likely due to weather pattern variability causing ozone at the surface and in the free troposphere to vary in tandem. Far more work is required, including a

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thorough evaluation of the OMI product against extensive IAGOS aircraft observations across mainland China, South Korea, Taiwan and Hong Kong. The additional analysis required to convince me that OMI can provide a meaningful evaluation of surface ozone across China goes beyond a standard major revision. My recommendation to the editor is that the paper be rejected to allow the authors adequate time to conduct additional product evaluation. If the expanded analysis can indeed demonstrate sensitivity of OMI to surface ozone then the authors will have the basis for a new manuscript which will make a valuable contribution to ozone monitoring across East Asia.

Further comments:

When I read the title and abstract I was under the impression that the authors had made a new breakthrough regarding the detection of surface ozone using OMI. It seemed like the instrument could actually detect ozone at the surface and the detection was so good that daily ozone variability at any given surface site could be determined with a precision of ± 10.7 ppb. But when I read the full paper I learned that this is not the case.

The premise that lower/mid-tropospheric OMI ozone retrievals are closely associated with surface ozone is not shown in a convincing manner. The initial correlation of: $[O_3] = 8.9 \Delta\Omega + 15.8 \pm 10.7$ appears to be driven entirely by the latitudinal gradient of ozone at the surface and in the mid-troposphere. Just because surface and OMI ozone have similar latitudinal gradients, when averaged over several years, does not mean that the mid- to lower troposphere can tell us how ozone varies at the surface from day to day. A better test of the relationship is to focus on a narrow latitude range. This is done in Figure 3, for daily observations, where we can see that the correlation is very low above five urban regions. For the region of Beijing the correlation is only $R=0.27$ which corresponds to an r-squared value of 0.07, which means that the variation in OMI only explains 7% of the ozone variability at the surface. The best case is made by the Wuhan region, but even here $R=0.53$, which means OMI only explains 28% of the surface ozone variability. Figure 3 shows that OMI is only weakly correlated with

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surface ozone and provides no convincing argument that the retrieval is sensitive to surface ozone. The weak correlation is probably just due to weather patterns causing surface and lower to mid- tropospheric ozone to vary in tandem.

In a related comment, do the authors think that any correlation between ozone at the surface and ozone in the lower/mid troposphere is linked because of similar photochemical processes, or is the correlation just a coincidence due to meteorology? For example we know that in southern China the ozone at the surface varies strongly with the strength of the summertime Asian monsoon. When transport is from the south then the relatively clean air masses from the tropical Pacific bring air that is low in ozone, both at the surface and in the lower-mid troposphere. But when the monsoon winds weaken, mid-latitude air is allowed to move back into the region of southern China, bringing higher ozone to the lower and mid-troposphere. At the same time, the flow of clean air from the south also ceases at the surface, allowing ozone to build up in the polluted air masses from mainland China. Under this scenario ozone in the mid-troposphere is correlated with ozone at the surface even if the two layers are isolated from each other by strong temperature inversions.

OMI ozone could be compared to long-term ozone monitoring sites in rural areas which would be a better comparison than the urban data from the new Chinese monitoring network. It would be very helpful to see time series of daily OMI values (when available) and corresponding surface observations from the following sites: Mt Tai – data can be obtained from Prof. Likun Xue, Shandong University [Sun et al., 2016] Hok Tsui – located on the south coast of Hong Kong, data can be obtained from Prof. Tao Wang, Hong Kong Polytechnic [Wang et al., 2017] Shangdianzi – see Ma et al., 2016 LongFengShan – located in northeastern China. Contact Dr. Xiaobin Xu at the China Meteorological Administration: xiaobin_xu@189.cn LinAn – Near Shanghai, Contact Dr. Xiaobin Xu at the China Meteorological Administration: xiaobin_xu@189.cn Xi-angGeLiLa – in south central China, Contact Dr. Xiaobin Xu at the China Meteorological Administration: xiaobin_xu@189.cn

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Ozone at the surface and in the mid-troposphere varies greatly with transport pathway and abrupt changes in air masses, and recent studies have shown that ozone in China varies with meteorology [Pu et al., 2017; Zhao et al., 2018]. The authors are aware of this phenomenon as their previous work has explored the impact of climate variability on ozone. Therefore I'm surprised that the authors didn't first explore how surface ozone across China varies with meteorology, such as surface temperatures (or temperature at 850 hpa) [Pusede et al., 2015], or with the height of the 500 hPa surface [Reddy et al., 2016], both of which correlate quite well with surface ozone. The authors should first determine the correlation between surface ozone and meteorology, and then compare these results to what they find from OMI ozone. Does OMI give more information on surface ozone than basic meteorology? Given that reanalysis data are available for all of China under all weather conditions (no cloud screening) I would think that the meteorology would perform better than OMI. If OMI performs less well than meteorology, is there any reason to use OMI to try to predict surface ozone, when meteorological analyses are available everywhere and at all times?

Another necessary analysis is to see if in situ observations of ozone in the mid-troposphere are correlated with surface ozone. I realize that the authors did look at ozonesonde profiles above Hong Kong, but they are not very frequent and they don't tell us anything about ozone in other parts of China, especially in the highly polluted North China Plain. The IAGOS program has hundreds of profiles above East Asia since 1995. As shown by Ding et al. [2005] and by Gaudel et al. [2018] ozone in summertime in the boundary layer is much greater than ozone in the mid-troposphere. The difference is due to very strong ozone production in the boundary layer, versus distant source regions for ozone in the mid-troposphere. If the authors conducted a transport study for ozone in the mid-troposphere they would find that very little of the air in this layer comes from the surface of China. Probably 80-90% of the mid-tropospheric above China air has either been in the mid-troposphere for days, or it comes from the boundary layer far upwind of China. The authors can freely access hundreds of commercial aircraft profiles of ozone and carbon monoxide above mainland China, Hong

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Kong, Taiwan and South Korea from the IAGOS database. They can then apply the OMI averaging kernel to the profiles and determine the relationship between IAGOS ozone in the mid- and lower troposphere to ozone at the surface. Does IAGOS ozone in the mid-troposphere correlate with ozone at the surface? Is the correlation any better than when surface ozone is correlated with meteorology? Then compare the IAGOS relationship to the OMI relationship. Does OMI perform any better than IAGOS?

Figure 5 shows surface ozone trends across China which were derived from the OMI ozone product. The strongest trends are in the far north of China and in the far south of China. Based on the summer OMI trends (2005-2015) reported by the Tropospheric Ozone Assessment Report in supplementary Figure S-24 of Gaudel et al. [2018], OMI has a strong trend across southern China but no trend across northern China. Therefore I don't understand how Figure 5 can show trends across northern China. It would be helpful to include a map that shows the OMI trends across China.

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