Comment on acp-2022-304

Anonymous Referee #1

Referee comment on "Contributions of meteorology and anthropogenic emissions to the trends in winter PM$_{2.5}$ in eastern China 2013–2018" by Yanxing Wu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-304-RC1, 2022

This paper presents a MLR statistical attribution of the 1985-2018 PM2.5 trends in three megacity clusters in China, using visibility data as proxy for pre-2013 PM2.5 data. It finds a large meteorological (non-emission) contribution to the trend, and argues that previous MLR analyses of the 2013-2018 trend using the actual PM2.5 data starting in 2013 and attributing the trend to emissions are not robust. The paper makes some good points about the difficulty of sorting out meteorological effects when interpreting short (post-2013) trends. However, I believe that it may be (1) flawed in its reconstruction of the 1985-2018 PM2.5 record which is the basis for most of the argumentation, (2) mistaken in claiming that attribution of recent PM2.5 trends to emissions is not based on mechanistic knowledge, and (3) annoying in belaboring trivial statistical points that are well known to any trained scientist. I don't think that this paper is publishable in ACP in current form. Specific comments are below.

- The 'bulge-2013' feature in Figure 1 (line 88) anchors much of the argumentation in the paper but it is very weird. It seems caused by the switch from the visibility proxy to the actual PM2.5 data in 2013. The methods are buried in Supplementary Material. Is this 'bulge-2013' seen in the consistent long-term satellite AOD data record? I think that the authors would have to show that it is present in the AOD data in order to have credibility.
- What is the 'emission' in Figure 1? Of what species?
- Line 91: the Mao et al. 2019 reference which is intended to provide support for the authors' meteorological attribution of the trend is in fact grey literature involving some of the same authors.
- Line 132, etc.: the mechanistic meteorological connection of ASI to PM2.5 is not clear, and as the authors point out any meteorological variable with a suitable long-term trend would do the trick. But there is in fact a strong mechanistic argument for emissions to be related to PM2.5 (line 154), and there is strong independent evidence that Chinese emissions have decreased over the 2013-2018 period (emission inventories, satellite data). To claim that the connection of PM2.5 to emissions has no mechanistic support strikes me as obviously wrong. In fact the authors cite Chen et al. 2019 in demonstrating the mechanistic connection in WRF-CMAQ but argue that the analysis is flawed because it did not consider the effect of the bulge-2013 (line 158). As pointed out above, I am very suspicious of this bulge-2013.
- There is a lot of trivial stuff about the non-mechanistic basis of statistical models,

correlation not implying causality, more years increasing the credibility of the model, etc., that is repeated again and again and does not rise above the level of a basic course in statistics.