

Interactive comment on “Evaluation of Anthropogenic Emissions and Ozone Pollution in the North China Plain: Insights from the Air Chemistry Research in Asia (ARIAs) Campaign” by Hao He et al.

Anonymous Referee #2

Received and published: 18 June 2019

This manuscript focuses on the comparison of ground/airborne measurements and satellite retrievals of a variety of air quality relevant trace gases with CMAQ model products. The authors attempt to adjust emissions in CMAQ based on column model/satellite ratios, thus improving the modelled ozone agreement with ground/airborne measurements. The dataset described in the manuscript is certainly novel, but there are enough omissions in the analysis that would prohibit its publication in its current state. I recommend the manuscript to be rejected, while encouraging the authors to resubmit after the address of the below issues.

C1

Major comments:

-The improvement in ozone agreement is quite modest, to the point I am interested as to how significant it is. How does this compare with the various uncertainties in measurements and the EI? What ratios would be required to force greater agreement? It seems there would be many more sensitivity studies needed to try to evaluate this. As is, this suggests to me that the ozone discrepancy cannot be explained by a simpler large scale emissions adjustment and resolving it would require a more intensive study of the VOC chemistry.

-A prevalent assertion of the authors is that discrepancies between model and observations are driven by reduced emissions, but the authors do not address any validation of the 2010 emissions. It is difficult to claim difference is due to emissions reduction without evidence that CMAQ and observations, airborne or satellite, agreed in the first place. Consistent, yes, but not necessarily suggestive or conclusive. The authors do not cite other studies to support this. Language should be adjusted throughout the manuscript to reflect that the adjustments needed to improve the model agreement are inherent, a component of which is consistent with emissions reduction.

-Line 296-306: the EF technique is not very well described here. How were the cutoffs that the authors state (R^2 , altitude, etc) justified? A fuller description of the technique would help significantly.

-Line 296-306: Since these come a variety of emission sources, a simple mean seems simplistic to describe the population. In looking at Fig 6, there seem to be multiple overlapping populations contributing to the mean. A broader discussion of these populations seems warranted.

-Line 277-293: Where do the Y12 NO_y measurements come from? There is no description in the instrument section. Without the details of the measurement, this entire discussion of reactive nitrogen is not possible to evaluate.

C2

-Sect 3.3: It seems there is should be a before/after version of this analysis: one with the baseline CMAQ model (which is to my understanding what is used for this analysis) and with at least the CMAQ_all case. That seemed to be where the analysis was heading, and without it, the manuscript feels incomplete.

Minor comments:

-Fig 1 a probably unnecessary, label fig 2 more clearly

-Fig 3: change zeros to O

-Fig S1 & S3: these have very similar captions, though the difference is clearer from the manuscript. I would edit the captions to highlight the differences

-Line 251, "We observed isolated plumes. . .": Where are these plumes with respect to the surface layer structure? What about the secondary plumes at 800-1200 m? The authors need to add context on the atmospheric structure to describe the transport impact of these plumes. Perhaps a summary plot of potential temperature as well?

-Line 259, "In summary. . .": This sentence was somewhat confusing, is unclear whether authors mean exclusively east-west gradients observed or both north-south and east-west.

-Fig 7: highlight here the aircraft campaign area, is difficult to eyeball where it is using fig 1b

-Fig 7 & 8: having both the differences and ratios seems redundant. As ratios are used in the later analysis, the authors should keep those figures and get rid of the difference figures, with some minor rewording in section 3.2.

-Fig. 9: it is not clear to me from the discussion the need for both panels A & B

-Fig 10: these are difficult to evaluate with the high variability. Perhaps a difference plot between observed and modeled, or ratios of the same, in order to clarify the improvement

C3

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-248>, 2019.

C4