

Interactive comment on “Decadal Trends and Variability in Intermountain West Surface Ozone near Oil and Gas Extraction Fields” by Ying Zhou et al.

Anonymous Referee #2

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General Comments: The paper presents and analyzes surface level ozone over the U.S. Intermountain West for periods ranging from five to more than twenty five years, depending on the site. The writing is generally quite good, with few mistakes in grammar and usage. The authors have done a reasonable job of collecting, organizing, and analyzing data, but the presentation is quite heavy on details and not tightly focused enough for the average reader. That is to say, there is a small community that will benefit from the thorough work presented, but manuscript is full of site acronyms and numbers of all kinds, making it difficult to extract the conclusions the authors deem to be the most important. Unfortunately, I don't have an easy solution to fix this situation, but I encourage the authors to consider revising the paper in such a way as to

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de-emphasize the minute details and instead zero in on the most important take away points.

To enlarge upon this point, the impression this reader gets from the abstract is things are complicated and there are many factors that contribute to ozone concentrations and trends. Even within a moderately coherent region, trends vary from site to site. These variations are “likely”, but not definitively linked to O&NG and other activities. I am not necessarily finding fault with the work and the findings, but the paper comes across as suggestive, rather than conclusive. As the authors reflect on the significant body of work they carried out, are there two or three (or four) key points they would like the scientific community and policy makers to learn from this work? I find the abstract too diffuse to pick out these points.

The sentence on lines 40-43 is a case in point. First of all, it is a difficult sentence to read and understand due to the construction, and the “while” clause in particular. Second, is this nuanced finding important enough to be in the abstract? Are the authors suggesting emissions controls predicated on the phase of the AO? Is that in any way practical, either from a scientific or a regulatory perspective?

Specific Comments: A problem with the paper that is easier to fix is the methodology for determining whether the trends they compute are “significant”. The authors perform a linear fit to the various data sets, then use the p values to impute the significance of the trends. The p value actually is a measure of how well the linear model fits the data. If the data were monotonically decreasing in a quadratic or exponential manner, the p value would be “poor” for the linear fit, but the trend would be undeniable. They need to use an appropriate statistical test for trends in the data if they want to evaluate the trend. The use of a Mann-Kendall test is a much more general way to do this. Of course, the authors use the slope from the linear model as their “trend estimate”, which is probably okay if Mann-Kendall shows that there is a significant trend, but this slope doesn’t have any real meaning if the trend itself is not significant. This should not be a difficult fix – and it may not change their results much, but it may. In any case it needs

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to be done properly!

Lines 173-175: To help put the study in perspective, the authors should report the decadal mean A4DM8HA for the Denver-Boulder and Salt Lake City metropolitan areas here.

Lines 184-187: Is this sentence important to the paper? It is a very difficult sentence to digest, and it is not clear if there is any significance to the finding. If it is significant, take some time and explain it clearly. If not, omit it.

Lines 236-37: Presumably this sentence refers to the CANY site only, although the text changes between site, basin and county get very confusing. The linear model computes a negative slope (decreasing trend), but the p value indicates the linear model does not fit the data well enough to fit the author's criteria. I wonder if a Mann-Kendall test would indicate the presence of a trend? My gut feeling is yes.

Line 423 and 889: Figure 10a-e is difficult to read. First of all, it would be helpful to the reader if in the title of panels a-e, there were an indication of whether the measurements took place in the summer or the winter. Example: "NACHTT(winter)". Second, the authors should indicate on each panel (a-e again) the part of the day that was sunlit. Admittedly, over the course of each campaign the length of day varied. One way to indicate this would be to lightly shade the period between the earliest sunrise and the latest sunset, then use a slightly darker shading between the latest sunrise and the earliest sunset.

Line 460-75 and 889: Figure 10f is presented with inadequate explanation. What do "NO_x scaling factors" refer to? (How are they defined?) What is the base case VOC scenario? Where did it come from? Is it the same VOC mix and concentrations for all simulations? How does one interpret the sensitivity curve?

Lines 434-35: It seems unlikely to me that the mixing hypothesis is the only possible reason that the model and observations disagree! Either propose alternate possibili-

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ties, or qualify the favored hypothesis. (Or back it up.)

Line 812: Figure 4 – panel b of this figure needs to be better explained to the reader. A simple sentence like the following added to the figure caption would be sufficient: “Yellow color indicates regions where NO_x has increased between 2005-2010 and 2011-2015, and blue color indicates regions where NO_x has decreased.”

Line 848: Figure 7 is too small to be useful. Perhaps a single example here, with better explanation, and move the rest to the supplement (also much bigger!). Explain in the figure caption or in the text the physical interpretation of the different color areas on the maps and cross sections.

Technical Corrections and/or Suggestions: Line 74: Suggest adding “winter” between “four” and “O₃”.

Line 76: Suggest adding “winter” between “6-day” and “high”.

Line 128: Add “of” between “upwind” and “O&NG”.

Line 137: Suggest deleting “the analysis of” – or similar rewording.

Line 189: Cooper et al., 2014 is not in the reference list.

Line 269: Please reword the passage “two U.S. largest”.

Line 273: Add “and” before “reached”.

Lines 410-411: This sentence is confusing. “Stronger” than what?

Line 454: NFR was used and defined back on line 78. I suggest eliminating the acronym and spelling out Northern Front Range all four times that it is used in the manuscript.

Supplement:

Table S1: What is the meaning of the shadings? Please describe.

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Section S8.2: Equation E1 – I find it odd that $j(\text{NO}_2)$ does not go to zero in the absence of radiation. How can that be?

Table S8: Acronyms need to be spelled out.

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