

Interactive comment on “On the effect of upwind emission controls on ozone in Sequoia National Park” by Claire E. Buysse et al.

Anonymous Referee #1

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This is a well-written manuscript, supported by ample and well-developed data analysis, that involves a topic of substantial interest. Thanks to the authors for preparing the work. A few minor suggestions and comments are provided simply for consideration.

General:

1) Am a little concerned about the conclusion regarding O₃ sensitivity to downwind *distance* from Visalia/SJV given that (if i understood correctly) that this finding is based on just the two sites in SNP (one ~ 10km downwind of the other). Two general concerns here:

a) Do the authors intend for readers to extrapolate this conclusion beyond these two specific SNP sites? Would a hypothetical 3rd site further downwind by 10-20 km from

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SJV be expected to have shown even greater responsiveness? At some point downwind, this conclusion presumably breaks down as areas become less and less influenced by SJV. If this conclusion is intended to be limited to these two SNP sites, maybe those parts of the paper that cite downwind distance as a factor in responsiveness could be revised to limit this conclusion to the SEQ1 and SEQ2 sites.

b) Is it possible, especially given complex flows in the region, that the elevation differences between these two sites is an equal or greater driver of the differential O₃ decreases in the area than distance?

2) A little more detail on the approach used to project future exceedances would be useful (Table 5). Clearly, the trend is assumed constant, but then did you also assume that the within-year variability would also stay the same?

Specific:

Figure 2: Information about mean vector flow would be more informative than mean direction. The paper notes the mean wind speeds on page 6, line 3.

Section 2: If possible, a schematic of the various flows and layers would be valuable.

Page 5, line 2: "during *the* ozone season"

Page 6, line 1: "Figure 2"

Page 7, line 18: The 8-hour ozone NAAQS is slightly more involved than described here. EPA defines a design value metric to determine an area's status relative to the NAAQS. For 8-hour ozone, the design value is the 3-year average of the 4th-highest max daily 8-hour ozone concentration at a site. Based on the description here, it's not entirely clear what metric was used for the trends in table 1. Clarification would be helpful.

Page 7, lines 21-26: My (limited) understanding is that statistics like W126 are typically calculated over a specified period (e.g., 3 months for W126 as discussed on page 8).

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Does this paper follow those conventions for the Table 1 trends? Either way, may want to clarify.

Page 8, lines 1 and 11: It's not immediately clear to me ... is the term "interannual variability" as used in the context of Table 1 referring to the year-to-year differences in these metrics (i.e., the standard deviation of yearly values over the 12-year period)? Or is it just referring to the trend itself as "interannual variability"? May want to clarify, especially if you mean the latter.

Page 9, line 21: The paper tends to reserve the use of the term "impacts" for O₃ impacts on plants and use the term concentrations when talking about non-plant effects (e.g., human health). This is fine, but of course both are impacts.

Table 5: Given the statement on page 10, lines 1-3 about the potential overly optimistic nature of the W126 metric relative to SUM0, why not include SUM0 in Table 5 instead of W126?

Table 5: Very minor It might make it easier on the reader if this table was reconfigured such that directional changes were consistent across the two metrics (i.e., lower numbers indicate improvement). May want to consider switching from # of days required for an exceedance to something like the inverse of that.

Table 5: Per an earlier comment, it's not clear to me how you could have a value > 92 days (e.g., the value of 107 listed for 9 ppm h in 2021) if the W126 metric is calculated over 3 months. Wouldn't that be a "never"?

Page 11, lines 13-14: May want to clarify these specific listed values in text are for SUM0.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-1006>, 2017.