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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General comments:

Overall, the paper is well written and is an easy read, but there are some fundamental issues that must be addressed before this paper can be published. In general, there are numerous, rather bold statements, that need to be substantiated. Most things are overstated in the manuscript, and the rudimentary analysis done in Section 3.4, past and future exceedances, is completely unacceptable for any paper that is going to be published in ACP, or any other scientific journal. As far as overstating goes, the first sentence of the abstract is simply not correct:

Abstract. Sequoia National Park (SNP) experiences the worst ozone (O3) pollution of any national park in the U.S.

My response to the first sentence of the abstract is: NO – if you look at the NPS ozone data for all of their sites, you find that Joshua Tree is actually the worst, Sequoia and Kings Canyon is comparable at best. Even though they are only using data through 2012, the following is still relative and the patterns remain the same: Acadia and Joshua Tree recently reported the highest ozone levels in 2017 for all NPS sites, and Yosemite also beat out Sequoia and Kings Canyon for 2017. Moreover, Dinosaur National Monument has wintertime ozone levels can greatly exceed what is observed at Sequoia and Kings Canyon. My point, the information disseminated throughout the manuscript must be conveyed accurately, and not overstated. Simply changing the sentence to "some of the worst" is all that is needed, but these types of statements are too common throughout the manuscript.

Additionally, the authors don't really convey any new information – one of their main points, that the transport of NOx is more important than the transport of ozone to the sites in the park is something people already know and understand for this area. How else would you manage to get higher levels in the park if the precursors were not being transported out of the source region photochecmically processed along the way?

Additionally, it is stated in a couple places in the manuscript that emission controls are optimized for the hottest days in the summer, so the policies that have been implemented are not optimized for decreasing springtime ozone, when it is cooler. This is a rather bold and cavalier statement to make without providing any type of information on what the policies are, and how the seasonal differences in temperature affect the emissions control strategies. In my opinion, there needs to be a substantial discussion, that pulls in what the policies are, and how the overall emissions are affected by these seasonal temperature differences to justify their statement that these polices are less effective in the springtime during cooler weather. My guess is they are trying to rehash the points about temperature dependence as as described in Pusede et al. (2015); however, they have stated that it's the emission control strategies that aren't optimized, so this means diving into the SIPs and seeing what and how emissions were/are controlled and correlating this to the seasonal temperature changes. The authors beat on policy not being appropriate for the seasonal changes, so this needs to be addressed. In particular, what part of the SIPs are not effective for the springtime emissions and how can they demonstrate this? What would be done differently to improve the effectiveness of the emission control policies to improve springtime ozone?

Moreover, the authors discuss how precursor emission controls have been less effective at reducing O3 concentrations in SNP in springtime, yet, there is no mention or discussion about other factors that may be influencing springtime ozone. For example, how do the springtime chemistry and dynamic processes of the widely observed springtime maximum of ozone in the Northern Hemisphere mid latitudes influence ozone levels in this region? Are these processes influencing what the authors are referring to as less effective emission controls during the spring? Also, it's not actually clear in the paper how the authors get to the conclusion that "…precursor emission controls have been less effective at reducing O3 concentrations in SNP in springtime…".

Finally, the term "trend analysis" is used quite a bit in the paper; however, it would be useful if they included a figure of the full time series of ozone data from the sites, the annual 8-hr 4th high, a table of annual basic statistics to help set the stage for the analysis. What is presented is rather "thin" – the reader needs to be provided more information in order to better evaluate what is presented…which is very little. This is even that much more important for Sect. 3.4 – the authors should, at minimum, show the simple regression that was used to come up with the values in Table 2. I personally think this section should be removed or done in a much more rigorous manner, but the authors need to show how these values were derived.

Specific comments: P1, L21-22: If you are referring to the whole area (re Sierra Nevada forests), then you should use the 4 letter NPS designation for the site, SNP should be referred to as SEKI, as the measurements are representative of Sequoia and Kings Canyon NPs.

P1, L25-26: The reference cited in this sentence does not make the statement that it Sequoia is the most ozone polluted park in the U.S. – please ensure that you accurately represent what a reference says, period.

Sequoia National Park (SNP) is a unique and treasured ecosystem that is also the most ozone-polluted national park in the U.S. 25 (Meyer and Esperanza, 2016).

P2,L7-9: Revise the following sentence – reads awkwardly: On multi-decadal timescales, O3-resistant plants may thrive over O3-sensitive species, system-level dynamics that would maintain forest productivity and carbon storage, but would induce changes in ecosystem composition (Wang et al., 2016).

P3, L3: there are additional references that should be included regarding the W126 metric

P3,L16; technically, the NPS started measuring ozone in the early 1980s, not the late 1980s. Shenandoah NP started in 1983 and Sequoia and Kings Canyon NP – Lower Kaweah started in 1984.

P4, L25: Beginning a sentence with "Due to" is grammatically incorrect. The Chicago Manual of Style suggests using "due to" when you can replace it with "attributable to," but not when you could use "because of"; if a sentence starts with "due to", it is most likely incorrect. Therefore, please revise.

P4, L30-31: "strongly" in "strongly temperature-dependent" really should be defined here – a counter to this statement is that in the Uintah Basin, during snow cover cold periods, ozone levels are usually higher there than in the Sequoia and Kings Canyon National Parks, yet the temperature is significantly lower. Temperature is only one factor, not the only factor. Moreover, high ozone episodes have occurred as early as March, but high ozone starting in the spring is fairly typical, so I would change "summer" to "spring" or through the fall. Ozone levels in Sequoia and Kings Canyon are comparable in April and September.

P5, L4: "due to" is inappropriate here – it is a result of the Mediterranean climate.

P5, L21: First off, there isn't a methods or experimental section – more information needs to be provided. This get to the more important point that you state that all data are provided by CARB, when in fact, they are not. The data are served up by CARB on their website, but the NPS data is provided by the NPS on their data page, which also gets uploaded to AQS, which is the main repository that houses all national ozone data – it is the single main repository. CARB either serves up the data directly from AQS or in their own database, where they have either obtained the data directly from the NPS or AQS. So, it should be clearly stated who is the proprietor of the data and where it was obtained from. These have been merged into one item, and what is disseminated in this sentence is incorrect.

P7, L15: The NAAQS for ozone is an 8 hour average value; it is the annual 4th highest daily maximum 8 hour average ozone concentration, averaged over 3 consecutive years (the design value) – this must not exceed 70 ppb. So, what you are saying is repetitive and not conveyed properly. What you should say is the annual 4th highest daily maximum 8 hour average, or the DM8HA or DM8A, not the "8-h O3 NAAQS".

P7, L16: Why are trends only reported as a percent change? It would be more useful to include the ppb per yr trend in the table, along with the percent change. Moreover, why is only the 8-hr daily max being listed? I'm assuming it's the annual 4th high daily max 8-hr average, but it's not stated in the text. Please clarify.

P7, L16: This sentence is not correct – see previous comment. "The 8-h O3 NAAQS is a human health-based metric computed as the maximum unweighted daily 8-h average O3 mixing ratio." As for SUM0 – you need to say why it's called SUM0 – as in, why is it a "0"?

P8, L1-11: For this paragraph, only percentages are reported – it is absolutely necessary to include what the corresponding values were on ppb and ppm hrs for W126 and SUM0. For this to have value to ecosystem and plant effects folks, numbers, not

percentages, are needed.

Section 3.4 You state that you "predict future O3 levels in the context of protective threshold"; however, it is not stated how your do this in this section – please provide necessary information and figures.

"Future exceedances are computed assuming individual daily indices continue to decline at the 2001–2012 rate and are projected from 2011–2012 values." Is this a reasonable assumption? I'm not convinced this is the case. There is ozone data beyond 2012 at these sites, so does the rate of decline hold true? The fact that you are predicting future ozone levels off of this would suggest it should be evaluated.

"If past decreases in O3 continue over the next two decades, we predict no exceedances of the 8-h O3 NAAQS at SEQ2 by 2021 in springtime and by 2031 during O3 season, no exceedance of the 9-ppm h W126 threshold by 2021, and no further exceedances of 5- and 7-ppm h thresholds by 2031."

Following suit, more information needs to be provided to make such a bold statement when using such a rudimentary method. How much is NOx going to go down? How are large scale circulation patterns (e.g., PDO, ENSO, etc.) going to change and influence what is being transported in? What about the different climate futures? There are an array of emissions scenarios that can lead to significant differences in what you are inaccurately and inappropriately conveying here. Also, climate change - This section either must be expanded upon significantly or simply removed from the paper. As an example that contradicts your statements about ozone exceedances, the following is pulled directly from Val Martin et al. (2015) for Sequoia and Kings Canyon. Here, the report the actual values using different climate futures in order to assess what the ozone and W126 values will be. According to their rigorous analysis, both ozone and W126 values will exceed the current NAAQS level of 70 ppb and W126 values will also increase, and be well above the 5-9 ppm hr range.

Summer MDA-8 Ozone (ppb)

2000 (Baseline): 71.3, 2050 (RCP4.5): 72.9, 2050 (RCP8.5): 73.8

O3 W126 (ppm-hr)

2000 (Baseline): 46.0, 2050 (RCP4.5): 50.6, 2050 (RCP8.5): 53.2

Val Martin, M., C. L. Heald, J.-F. Lamarque, S. Tilmes, L. K. Emmons, and B. A. Schichtel How emissions, climate, and land use change will impact mid-century air quality over the United States: a focus on effects at national parks, Atmos. Chem. Phys., 15, 2805–2823, 2015, www.atmos-chem-phys.net/15/2805/2015/.

P9, L30: “O3 reductions predicted by W126 are almost twice those of SUM0.” What does this statement mean? How are ozone reductions predicted by W126 or SUM0? Both of these metrics are determined from ozone levels – how are these used to predict ozone reductions?

P10,L2: Regarding the following statement: “. . .W126 likely provides an overly optimistic representation of past and future trends in O3 impacts in SNP.”, this is a rather bold statement to make to summarize the paragraph, yet you provide no hard evidence of this – there is nothing in this section that supports this statement. Please address this in a more rigorous manner.

P10, L9: For the following statement: “. . .leading to policies not optimized to decrease O3 in cooler springtime conditions.”

Please elaborate on this point - this needs to be shown quantitatively. How large or small of a difference are you suggesting? What are the policies? How are they not optimized for the cooler springtime conditions? What could/should be done to address this policies in order to optimize them for the springtime?

P10,L15: Regarding the following “Third, aircraft observations collected in the direction of daytime upslope flow from the SJV to Sierra Nevada foothills reveal substantial decreases in NOx concentrations relative to isoprene, a key contributor to total organic reactivity (e.g., Beaver et al., 2012).” You are consding the 2001-2012 time frame, how

representative is this single day? Can this be put in to greater context?

P10, L18: For the following sentence: “This implies that PO3 in Visalia and SNP is differently sensitive to emission controls, with SNP more responsive to NOx emissions control than Visalia.”

This is only one aspect of the issue, the other is that you are sitting in a source region, so the regime you are in is different; also, there is mixing and dilution that occur with transport, so this is another major factor - it’s not simply response to emissions controls. This needs to be addressed and put into context.

P11, L15-16: “. . .day due the mixing. . .” please fix this sentence, and it would be best not to use due to. . .

As it currently stands, the data disseminated in the tables is not very useful, especially Table 2. What would be better to provide in Table 2 are the projected DM8HA values in ppb and the W126 values in ppm hrs, along with their corresponding #s of exceedances per year. However, the method used for this work is not suitable for providing any type of reasonable predicted value. As for Table 1, actual values should be included along with the percent change.

In summary, before this paper is worthy of being published, there are significant issues that must be addressed.

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