

Interactive comment on “Residual Layer Ozone, Mixing, and the Nocturnal Jet in California’s San Joaquin Valley” by Dani J. Caputi et al.

Anonymous Referee #1

Received and published: 9 October 2018

Caputi et al. is a thorough and well-designed analysis of the relationship between nocturnal turbulence and next-day ground-level ozone concentration in the southern San Joaquin Valley in California, one of the most polluted parts of the United States. This study presents much needed constraints on this relationship with a variety of observations - nocturnal airborne measurements, surface monitoring and sounder data, and reanalysis data. The authors construct an ozone (or Ox) budget for the nocturnal boundary layer using airborne observations, and use this to infer turbulent diffusivity. They explore some of the implications of their assumptions in constructing that budget (i.e., nitrate chemistry, the effects of recirculation on advection), and perform an error analysis of the budget, clearly stating that their calculations are quite uncertain.

Their budget-derived estimates of turbulent diffusivity are on the higher end of the

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few existing previous estimates for other parts of the world, atmosphere, &/or time of day, but largely within the bounds of previous estimates. The authors speculate that this could be due to the unique topography of the region. The authors also calculate mean eddy diffusivity independently from the Ox budget using TKE dissipation and the Brunt-Vaisala frequency. They do find that this other estimate suggests a much smaller mean turbulent diffusivity, but don't really discuss the implications of this finding. Their budget-inferred eddy diffusivities largely correlate to estimates of the bulk Richardson number, which is expected and builds confidence in at least the estimates' variation.

The authors find a correlation between nocturnal boundary layer eddy diffusivity and next day ozone concentration using airborne measurements and regional mean surface network observations, respectively. As this correlation is only based on 12 data points, they leverage observations from a long-term monitoring site in Visalia, CA that both a sounder and an ozone sensor. The authors find a correlation between overnight maximum lower-level jet speed and next-day afternoon ozone concentration at Visalia, building further support for their hypothesis that strong nocturnal turbulence influenced by the low-level jet depletes ambient nocturnal ozone, and leads to cleaner next-day conditions. The authors also suggest that more efficient nocturnal ozone dry deposition under strong turbulence further acts to deplete nocturnal ozone.

Although I do think this paper merits publication in ACP, I would like to see substantial revisions before publication. However, these revisions for the most part have to do with improving structure and clarity of the manuscript. As is, the paper pretty severely lacks cohesion. I found it challenging to understand the goals, results, and implications of most sections. The abstract, introduction, and conclusion focus on the relationship between nocturnal turbulence and next day ozone, but there is quite a lot of supplemental analysis investigating the assumptions going into the Ox budget calculation, the uncertainties with respect to the inferred eddy diffusivity, etc. These parts could be much better integrated with the rest of the text. A clear articulation of the goals of each section at the beginning of the section, and a more detailed roadmap of the in-

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investigation in the introduction could be helpful. I like that the results and discussion are combined, but in many sections there is no discussion of the implications of the results, and they are not discussed in the conclusions. There are several figures that are barely discussed and I urge the authors to reconsider whether they should be included in the paper. The paper would strongly benefit from a streamlining of the analysis.

Also, the order of the figures should be the order that they are mentioned in the text. I would also like to see more explicit referencing in the text to the figure that is being discussed. The figures could also be more publication-quality. I would like to see the information on the plots themselves also included in captions, acronyms spelled out in captions, explicit references to the data sources in the captions. For most of the plots, I would also like to see larger text and larger symbols. The jet color scheme used on many figures is hard to interpret. For the maps, I find the underlying image of topography distracting and not helpful. If the authors really want to show the topography then one map of only the topography would be sufficient.

For the correlation between afternoon maximum hourly ozone and overnight maximum lower level jet speed at Visalia (Figure 14), the correlation is quite low. The fitted line doesn't look like it's capturing the pattern well, and looks like it's strongly influenced by the one really high jet speed. If this data point is removed, how does the relationship change?

— Line-by-line comments.

Lines 56-57: Will the authors please include the point about dry deposition in a separate sentence? Also, the way the part about deposition is phrased too much does not really suggest that there is much uncertainty to this estimate, but there it is quite uncertain (see comments below)

Line 58: Would “more” be better than “stronger” here?

Line 63: “infer” instead of “measure”

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Line 73: I find “occasion” as a verb to be non-intuitive

Lines 96-99: Will the authors refer to the stable layer as the NBL for consistency? This part is quite dense, especially for readers not fluent in boundary layer meteorology

Lines 101-104: I'm not seeing why the last two sentences are needed here. I would urge the authors to be as concise as possible here, again for readers not as fluent in BL meteorology

Line 110: Replace the “is” in “is important” to “is likely important”. Also, both is plural, so “is” should be “are”

Lines 112- 128: I struggled with this paragraph, which feels out of place. It's not clear why the authors start to talk about the Fresno Eddy. One option would be to move this paragraph to Section 3.3. Another option would be to more clearly direct the reader as to why they are introducing it (i.e. that it challenges their analysis). Also, will the authors please briefly introduce ozone production potential?

Lines 129-140: I find this paragraph a bit awkward, especially the first sentence with the term “acknowledge”. It seems like this sentence should be followed by a discussion of assumptions made, but this does not seem to be the case. The authors then proceed to mostly talk about daytime conditions, then say nitrate chemistry and dry deposition cannot be ignored. Why even talk about daytime? I would suggest saying that the focus of this work is nighttime and previous work has focused on daytime. The discussion of daytime doesn't feel meaningful, and it's confusing for the reader. Also, I'm confused about the point of mentioning nitrate chemistry and dry deposition here in this way. Do the authors examine these processes in detail later on? Perhaps framing it like that would help.

Line 152: Does “this ozone difference” refer to the day-to-day difference in ozone concentration? Please specify

Line 157: Do the authors average over a large area? The limitations would only be

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overcome if so, right?

Line 161: Does “in this area” refer to Taiwan, or SSJV?

In general, the introduction is pretty dense. I feel that most readers would find some sort of schematic useful.

Lines 194-196: Do the authors think that their “somewhat arbitrary” cutoff has a substantial influence on their results?

Lines 199-204: Again, do the authors think that this assumption has a substantial influence on results?

Lines 241-242: Seems like this sentence is unnecessary

Line 247: Please cut “tracked by”, it’s confusing. The ultimate fate of nitrate? Please specify

Line 259: Please specify the field site and time examined in Padro 1996

Line 271: “A blend of these three methods” is too vague. Please specify the method

Line 290: Do the authors mean that NO₂ and O₃ are by far the dominant species of Ox? Please specify

Lines 319-386: This is a lot of information. I found this section very confusing and long-winded. Will the authors please break this paragraph up? It would be helpful if the authors stated the goal of this analysis upfront and more clearly stated what the assumptions are, the bases for making them, and how they feed into calculating the net reaction of R1-R6 as a constant multiple of R2.

Lines 323-234: But the authors just said that their airborne measurements are supported by the ground-level measurement network? What is the measurement network used? Do the authors not trust that it provides values that should be regionally representative?

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Lines 327: This “channel of NO₃” meaning R6?

Line 330: What are the “VOC reactions in our analysis”? So does this finding mean that the authors ignore R6?

Lines 319-330: So what’s the conclusion here? It looks like the authors are finding a basis for including R6, but also a basis for not including R6.

Line 344: “Out of respect for” should be “Based on”

Line 348: Can “channel” be “reaction”? I find “channel” confusing and a bit colloquial

Line 352: Why is temperature shown in Figure 5 if it is not discussed? Also, in the caption of Figure 5 the acronyms of the airports should be spelled out.

Table 2: What do the authors mean that values may not match literature values? How is the extrapolation and valley average done? It seems like this info should be somewhere in the paper or supplementary material.

Lines 390: Will the authors better explain what the linear regression here is for, and how it is done?

Line 403: What is the similar environment? Please specify

Line 404: I don’t think the authors have specified yet that the SSJV is an agricultural region.

Line 405: What’s the basis of using these papers, over other ozone deposition papers? Half of these papers are not listed in the references list. There are also additional papers on CODE (California Ozone Deposition Experiment) that the authors may find helpful - for example, Massman 1994, Padro et al. 1994, Grantz et al. 1997. The authors should specify whether they are looking at an average of the CODE sites, or one in particular (there is a vineyard, cotton field, ...).

Line 409: Will the authors at least spell out that 2.5 cm/s is likely much higher than the

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deposition velocity for NO₂ should be, and perhaps cite some previous work here?

Line 410: Is the vertical flux divergence used in the last term or the last two terms?

Lines 412-3: Will the authors better explain what the linear regression here is for, and how it is done?

Lines 423-4: By surplus of Ox do the authors mean where Ox indicated by the purple line is greater than Ox indicated by the black line? Please specify this. Also please specify in the caption which of the terms have been inferred (and refer to section on calculation) and which have been observed.

Line 429: How is the error propagation calculated? At least refer to Section 3.2

Table 3: What exactly is the error estimate? At least refer to Section 3.2

Line 433: Please cut "Another way to frame . . . NBL"

Line 434: Please cut "Further". (In my opinion, doing this and the above suggestion would make this part more digestible).

Line 438: Do the authors mean NO₂ is less than O₃ by 10-20 ppb here?

Line 445: There should be an introductory sentence here, instead of starting with a specific component's error calculation.

Line 455-6: I would cut the term "conservative". What basis do the authors have for this value judgement? It seems little, especially in terms of the ozone deposition velocity

Section 3.3: This section is confusing because the authors say that the presence of Fresno Eddy could be problematic for their analysis. Then, they say that the predominant circulation during their flights is similar to Fresno Eddy, but then they say any recirculation has a minimal impact on their results (lines 492-3). A lot of the analysis on Fresno Eddy could be cut. . .especially because it's found to be irrelevant. This would help with clarity and flow. Additionally, can the authors split Section 3.3 in two?

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One section on Fresno Eddy, and one on the low-level jet?

Lines 468-72: Are Zhong et al. 2004 describing the Fresno Eddy conditions, or other prevailing conditions? Please specify.

Line 473: The authors need to more clearly specify that they are suggesting there are Fresno Eddy conditions during their flights.

Lines 480-2: I don't really know what the takeaway here is.

Figure 7: What is shown in the background of the plots? It's hard to see the yellow and light blue colors on top of the grey. I recommend using a different color scheme and/or thicker lines.

Line 494: I would repeat the hypothesis more in full here (i.e., the effects of the nocturnal jet on the next day's ozone levels; "contribute to the variability of ozone" is a bit vague).

Line 494-5: Again, "explored some of the meteorological factors that are absent from the current literature" is vague. Further, why would the authors only explore unexplored factors?

Line 498: "in 100m bin space" is too colloquial

Lines 506-525: This paragraph is confusing. The authors should state up front what they are investigating here.

Line 506: Explicitly say which thresholds correspond to strong and weak jets

Line 506: What is "it" here? The trough? Please specify

Line 512: Why the mention of Fresno Eddy here? Are the authors trying to attribute eastward trough to Fresno Eddy not happening? Please clarify

Line 516: What are "those" conditions?

Lines 516-526: It seems like this should be a paragraph on it's own, and better linked

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with the mention around Line 512 of Fresno Eddy. Referring to “LLJ” generally in this paragraph here is particularly confusing because in the preceding lines the authors were talking about weak vs. strong LLJ.

Lines 522-527: I’m not exactly sure why the authors feel the need to compromise here.

Lines 523-524: Previously the authors had said the Fresno Eddy and the LLJ are not the same thing, but here the authors seem to be referring to them interchangeably.

Lines 527: What is in addition to synoptic forcing?

Lines 532: High temperature could also decrease deposition through stomatal pores

Line 534: -> With the NARR climatology.

Figure 9: A map showing the difference in 2m air temperature for stronger vs. weaker LLJ may be more effective. Hard to see the contours. Or maybe just cut the elevation map, and color by temperature contours.

Figure 11 is never referenced, but I think it should be on Line 545. Figure 11 is interesting, but very tangential, and I think the figure and the short discussion of it should be cut.

Line 551: “Another look at . . .” is not a very helpful way of introducing what the authors are doing here. What are the authors trying to investigate here? Also, what is overnight layering?

Section 3.4 What’s the rationale for including the discussion of Figure 12 in the previous section, as opposed to at the beginning of this one? Seems like it would flow better in Section 3.4.

Line 562: “several previous studies examining different parts of the world”

Line 567: Will the author please make it more clear that their hypothesis is stated on lines 564-5?

Line 566: Specify regional mean ozone from monitoring stations in a

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certain network Lines 568: Here are the authors examining ozone at the monitoring stations or measured on the aircraft? Please specify Line 574: Why would the relationship be strongest for MDA8? How much stronger is the relationship for MDA8 vs. max hourly, 24 hour average? If it’s a lot stronger, is MDA8 roughly representing ozone at the same hours each day? Examining this could be insightful.

Also, why is this relationship stronger for MDA8 than that observed during the fumigation period?

Line 578: It would help the reader to briefly restate the hypothesis. Lines 580-3: Wait, why not MDA8 here?

Figure 13 and 14: Please be consistent in terms of ozone on the y vs. x axis.

Lines 593-5: Why would R_b be 0 at night? This doesn’t make much sense to me. Is this stated in the Padro 1996? R_b is not included in Padro 1996 Figure 4. In Massman [1994] R_b is estimated to be nonzero for the CODE vineyard. I recommend specifying that not only R_a is modeled in Massman [1994] but R_c is too (it’s not a residual of observed v_d and estimated R_a and R_b). Then I might just say here that modeled R_a and R_c are similar at night and R_b is unknown, rather than zero. It’s also important to note that this is only one way of estimating R_a (u/u_*^2) and estimates at night are likely highly uncertain. Lines 600-3: How would taking changes in R_a into account in the budget calculation change the eddy diffusivity estimate?

Section 3.5: It would be helpful if the authors introduced the goal of their analysis in this section upfront.

Line 607: Why should the authors values be comparable to Banta et al. 2006 and Lenschow et al. 1988? Please specify. Line 610: Did Banta et al. try to remove buoyancy waves? Line 610-1: Why? What is the implication of this finding? Line 624: “lower end of the range inferred from the Ox budget”. It would be helpful here if the authors re-stated the range of eddy diffusivities that they infer from the Ox budget.

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Line 626: “our estimates inferred from the Ox budget” Line 631: “similar turbulent environment to ours” ? Line 634: Specify here that the Lenschow et al. 1988 eddy diffusivity from the lower half of the NBL is the most comparable Lines 645-9: To me flow is better if the order of these two sentences is flipped Line 636: “variability in the reported values” Lines 640-4: What’s the point of this analysis? Because this relationship is expected, does this build confidence in the authors’ estimate of eddy diffusivity (at least the variability in eddy diffusivity)? If so, it should be explicitly stated. Lines 658-9: Briefly, why would the unstable layers have to extend upward beyond the NBL depth? Line 659-60: Why is this more likely? What’s the implication of this? Lines 663-4: Briefly, how would they contribute to overnight mixing?

Figure 16: Why is only 50 m shown? The authors say they examine thickness of 50m and 100m. It is challenging to interpret this plot. Another color scheme, and a zoomed-in map would be better. Also, the font size should be increased. It would be helpful to indicate the location of the Tehachapi pass on the map.

Lines 668-9: Where is this shown? Also, “seen”-> “observed” Line 669: What finding? Line 674-5: It might be more clear to state that the figure does not support the hypothesis that the authors outlined on lines 671-2. Also where is this figure? It would be helpful if the authors specified that it is not shown. Line 675-6: How does this fit into the above discussion? What are the implications of this finding?

Line 687: Cut “slightly” Line 689: “A limitation of our study” Line 690: Cut “being conducted”. Also what do the authors mean by pairs? Do they mean morning and evening flights? I would specify this. “pairs” is non-intuitive. Line 690: What demonstrates? Specify what “it” is. Line 691: Seems strange to mention that the authors demonstrate something “within the context of high ozone episodes” when ozone hasn’t been mentioned yet in the conclusion. On a similar note, the authors haven’t noted in the conclusion that there was a particular focus strategy of the flights, so it’s strange to mention it. It’s helpful for the reader if the conclusion can really stand alone from the rest of the text. Line 692: Specify where the soundings and surface monitoring data

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are from (locations, networks) here Line 692-3: Specify the implication of this finding (tie back to hypothesis) Line 694: What do the authors mean “although in the former analysis”? In the analysis of soundings and surface network data? This could be more clearly articulated, and it should be directly stated that this is not found in the airborne measurements. Line 695-6: “is an important link that may have consequential implications for modeling studies and policy making” is vague and verbose. I think the authors’ findings are important for modeling and policy, but this sentence doesn’t do much to convince me of it. Line 697: Introduce Visalia Line 698: “infer” -> “determine” Line 701: Spell out that reduced aerodynamic resistance means more efficient transport to surfaces where ozone can deposit Line 704: It would be good to articulate that this may be why the correlation between night turbulence + next day ozone may not always be high. Line 704: “Airborne measurements from flights over Bakersfield, CA showed . . .” Lines 704-6: Spell out the implication of this finding Line 706: In what study? Trousdell et al. 2016? If so, the subject should not be “we”, it should be “they” or better, Trousdell et al. (2016) Lines 704-10: I’m not quite following why the discussion of Trousdell et al. 2016 is relevant for the conclusions of this paper. Lines 711-2: “illustrated”-> “suggested”; “which consequently has impacts for”-> “and thus likely impacts” Lines 712-5: But what exactly is so uncertain about nitrate, and why will it affect ozone? There should be a line stating that the authors haven’t measured nitrate on their flights, and how/why this leads to uncertainty in their analysis. The authors should re-introduce alpha, and why it’s important. I really like how the authors have spelled out that nitrate measurements (specifically the lifetime) are needed in future nocturnal airborne measurement campaigns. Are there any other measurements or techniques that their analysis suggests doing or developing would reduce uncertainty?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-854>, 2018.

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