

While the quality of this manuscript has improved, the authors have not adequately responded to my initial review (Reviewer #1). After responding to the following concerns, I think the paper will be suitable for publication in ACP.

General comments

- The paper is excessively wordy and there are grammar errors. I urge the authors to use less words whenever possible and en dashes in their compound adjectives to enhance readability and check their grammar.
- There needs to be more consistent use of terms and abbreviations. For example, sometimes the authors use “RL” and sometimes “residual layer”
- The abstract should be treated as separate from the paper and the abbreviations and terms should be re-defined.
- “pairs of flights” needs to be introduced as early as the abstract. It’s not a given that pairs of flights means night and following morning.
- More information on the regressions should be given. The authors regress ozone on x, y, and z, and then calculate the partial derivatives? What is the error and amount of variability explained by the regression? How many data points go into the regression?
- Again, I urge the authors to shorten and clarify their discussion of nitrate, as it is hard to follow.
- It would be very useful for the reader if section names were a bit more detailed (e.g., articulated findings, or objectives).
- I still find it challenging to interpret Figure 9 with the topography on the map. I urge the authors to reconsider including the topography on this figure.

Line-by-line comments

Line 82: Insert “, which is ” before known more generally

Line 102-130: This introductory paragraph on the Fresno Eddy is extremely long and still seems out of context. Please better contextualize this discussion.

Line 115: Many readers may not know what a Froude number is. Please briefly define

Line 114-5: “act as a barrier to the jet” is not clear; please rephrase

Line 115: By “eddy feature” do the authors mean the eddy? Please clarify in text

Line 140-2: This is helpful to the reader, but it seems quite strange to have this description without a prior introduction of the study in the introduction

Line 148: typo

Line 149: “ozone problems” is too colloquial

Line 150-5: This paragraph would benefit from a sentence introducing that the authors are going to start talking about modeling. The authors need to more directly state that models don’t capture the nocturnal circulation motivates their study in the text.

Line 151: “—“ should be “-“; please check elsewhere that the authors use of “—“ vs. “-“ is correct

Line 157-69: This is too long and the motivation from daytime studies is a bit convoluted. I brought this up previously but I don’t feel like the authors sufficiently addressed my concern (or convinced me that the discussion is necessary). Can the authors simply say that most studies focus on the day, and thus our understanding of the nocturnal ozone budget and mixing on ozone air pollution more generally is limited?

Line 171: Why are there quotes on depletes?

Line 175: Please give the audience context for “broader dataset” - what dataset are the authors using? Also, please say the goal of this analysis here.

Line 177: “bolster” has a negative connotation in my opinion

Line 198: "lab" -> "laboratory"

Line 200: I'm not sure that it's ok to cite papers in preparation

Line 231: I'm not sure that it's to cite papers that have been submitted

Line 233-4: "If time permitted on ... , we typically completed ... or flew ..."

Line 236: "Residual Layer ozone project" has not been defined or acknowledged previously, please revise; "ground tracks" seems colloquial; please give acronyms for the sites that are used on the plot in the caption (as well as which network a given site is a part of). Why are only some of them labeled on the figure? Please label them all. Additionally, please move the label closer to the "x" - not always easy to tell which "x" goes with which label

Line 250: please cut "aforementioned"

Line 252: the objective aims to use? "to address this objective, we use a method ..."

Line 263: "the flight volume" is a bit colloquial - please rephrase

Line 269: where is the storage term in equation 1?

Line 272: it would be helpful if the authors had a line here saying something like "in the following sections, we detail the methods for estimating the terms in equation 1"

Line 276-298: Why isn't this paragraph its own section (to estimate h)?

Line 290: I think it would be clearer to state "late night and morning flight pairs"

Line 303: I think it is confusing to say that O₃, NO₂ and NO₃ are grouped together for Ox here. The authors clarify that their definition of Ox is different from that conventionally used in the following lines but I think some general restructuring of this part would help with clarity.

Line 335: are 30 ppb of O₃ and 20 ppt of NO₃ hypothetical values for SSJV? Please clarify in the text

Line 338: which surface air quality network? I asked this previously; please specify in the text.

Line 343: Will the authors directly link this nitrate lifetime with the implications for ozone here?

Line 373: cut "obvious"

Line 376: cut "and best accounts dominant."

Line 378: cut "very"

Line 379: cut "highly"

Line 283: ampersand should not be used here after Table 2

Line 392: give acronyms used in figure in figure caption

Line 395: "2nd" -> "second"

Line 403: "would be" -> "are"

Line 410: "the 1-second Ox data"

Line 414: this is not a sentence

Line 420: the authors' field campaign or that of Padro? Please clarify in the text

Line 422: does Padro conclude this or do the authors infer this? Please clarify in the text

Line 426: cut "purposefully"

Line 436: cut "on any given night"

Line 436-7: "likely accounts ... in Ox"

Line 461: Why is this worth noting? Is this observed in a figure? Otherwise seems extraneous to include this.

Line 481: Why is uncertainty in deposition computed in this way? It would only make sense to me if the authors are considering a deposition flux here. Do the authors mean the deposition flux (rather than the deposition velocity) here? If so, please specify.

Line 489: can the authors refer the reader to where they did this previously (e.g., the section)?

Line 501: In the level? Cut level?

Line 504: Zhong et al. (2004)

Line 508: Cut "It is noted that"

Lines 512-4: Can the authors more closely link with line with the previous finding (i.e., that this is additional evidence supporting a minimal influence of advection)

Line 520: "is"-> "are"

Line 524: Cut "that is"

Line 525: Define acronym

Line 528: "To analyze variability of the jet strength" does not give me much insight as to what the authors are trying to do here. Please more clearly lay out the goal. Also, in the following paragraph, will the authors please refer to the figures that they are referencing more.

Line 535: "where"-> "that showed that"

Line 539: "were" -> "was"

Line 542: Cut "thing"

Line 543: Please clarify in the text what the authors mean by essentially

Line 541-552: This entire paragraph needs to be re-worked for clarity

Line 550: By optimal, do the authors mean the best for good air quality? Please revise

Line 555: Refer to Figure 9?

Line 560-2: Why is this worth noting? What is the implication of this finding? Please include in text

Line 575: "50% of daytime values during convective conditions"?

Line 576: "TKE increases"

Line 578: "air pollution problem" is too colloquial

Line 580: Again, what is higher ozone pollution potential?

Line 583-5: Suggestion to break this into two sentences. "relative validity" doesn't make much sense

Line 590: Is the growth entraining into the RL? Suggest re-phrasing

Line 592: Instead of saying "were in the predicted direction" can the authors just say the direction of the relationship?

Line 596: "we explored"

Line 600: "is"-> "are"

Line 604: "This" is confusing here, because the authors were just talking about the outlier

Line 614: "is neglected", "combining an estimate of aerodynamic resistance"

Line 618: "the"

Lines 619-20: "The difference in U10 ... assuming an average U10 of ..."

Line 626: "will need to"-> "should"

Line 635: "for oceans and the free troposphere"

Line 642: cut "where"

Line 643-4: So do the authors use the median or the average...?

Line 648: "is"=>"are"

Line 664-5: sentence is too colloquial

Line 693: cut "a lot"

Line 693-4: "the observations of elevated mixed layers may be"

Line 695: "to confirm that this is not the case, we examine"

Line 698: "they"

Line 700: What are the implications of this finding?

Line 704: Mention ozone?

Line 705: again, please change “air quality problems”

Line 707: correlations between what and both Richardson number and ozone? Specify

Line 713: the context of high ozone episodes is hardly discussed in the text

Line 717: “next-day ozone”?

Line 719: “11 out of 12 days WHEN ozone concentration exceeded 100 ppm over Visala were preceded”

Line 722: the ozone reservoir where? Please specify in text

Line 723-4: suggestion to separate this into two sentences

Below I copied and pasted some of my initial reviews (black), along with the authors' response (green), and my response (black, bold). I ask that the authors also respond to these comments.

Line 157: Do the authors average over a large area? The limitations would only be overcome if so, right?

Response: The scalar budget technique we present covers a large swath of the SSJV, and thus the terms in the budget equation can be taken as averages of the entire region for which the budget is performed.

Will the authors more clearly articulate in the text, somewhere close to the beginning, that they are examining a large area of the SSJV? This should be closely linked with the authors' introduction of the Fresno Eddy.

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

Response: changed to “Combining those measurements with an estimated 0.2 cm s^{-1} nighttime dry deposition velocity of ozone at night in the SSJV (Padro, 1996), we can indirectly estimate K_z .”

My interpretation of Padro 1996 is that they examine several field sites in the SSJV - which one do the authors examine? Please specify in the text

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

Response: Changed to “all three of these methods were used in tandem.”

A “blend” / “in tandem” is too vague. How do the authors combine them? Please specify in the text

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.

Response: We found that often, the measurements in the studies were taken in specific areas such as crop fields. Since the aim of this analysis was merely to get a reasonable estimate, we used our meteorological knowledge to estimate whether a valley-averaged concentration may be slightly higher or lower than what was reported in the study.

Changes made:

The measurements in some of the studies above were taken in specific crop fields. Since the aim of this analysis was merely to obtain an order of magnitude estimate, we predicted whether a valley-averaged

concentration may be slightly higher or lower than what was reported in the study. Thus, values here may not exactly match literature.

I think back-of-the-envelope calculations are fine here, but the authors need to describe the method. Their description is too hand wavy. Somewhere in the text the authors should describe the land use characterization of the SSJV to give context to the several references to agriculture (e.g., is only a little of the SSJV agriculture?)

Line 403: What is the similar environment? Please specify

Response: Specified that this study was done in a flat grass field.

Now it needs to be more clear that this is a land use type (or climate?) representative of the SSJV.

Lines 423-4: By surplus of O_x do the authors mean where O_x indicated by the purple line is greater than O_x 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.

Changes made:

The dashed profiles show the expected profile that would have been observed on the morning flight if only advection (blue), chemical loss (green), or both advection and chemical loss (red) processes were occurring. The observed morning O_x (magenta) is inferred to exceed the predicted morning O_x (red) due to the vertical mixing term in the scalar budget equation.

Figure 6. O_x profiles from 2016-06-04 overnight analysis, NBL height (green line), and lower bound to vertical mixing gradient (yellow line). The solid lines are observations and the dashed lines are inferred.

Ok, but now it is not exactly clear why Figure 6 is included in the paper. What should the reader be taking away from this snapshot figure? Please better integrate this figure and the discussion of it into the text.

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

Response: Added "Here we estimate the uncertainty for each term in the budget equation, as well as the ultimately calculated eddy diffusivities." as an introductory sentence.

In my opinion "ultimately calculated" leaves room for confusion. Please rephrase

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

Response: As addressed in some of the following comments, we have attempted to clarify our discussion of the Fresno Eddy and where it fits in to this work. We firmly believe that a clear discussion of the

Fresno Eddy is absolutely necessary to retain because it is constantly referred to in air quality discussions of the SJV, but not clearly understood. It is a major conclusion of the paper that we sample and describe the Fresno Eddy in a new and better way, which we believe can help illuminate future studies. We have tried to clarify the discussion where possible, but maintain that the low-level jet is *part and parcel* of the Fresno Eddy, therefore separating the two into distinct sections in the manuscript only perpetuates the misleading distinction.

I still think the discussion of the Fresno Eddy feels tangential. I urge the authors to better articulate “It is a major conclusion of the paper that we sample and describe the Fresno Eddy in a new and better way, which we believe can help illuminate future studies” in their paper (upfront, and in the conclusions).

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

Response: Here we are stating that Zhong et al. (2004) was presenting a climatological analysis of typical summertime conditions, while our flights were targeting periods of higher ozone, thus the synoptic and mesoscale conditions during our flights might be systematically different from climatological norms.

Ok, so can the authors more clearly state this rather than what they currently have (which feels tangential)?

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

Response: We have made this a separate paragraph.

Again, it seems like the authors have only responded to half of my concern.

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?

Response: Added suggested literature and stated that r_b is unknown and thus not included in this approximation. The average error of K_z due to the uncertainty of V_d is calculated to be $\sim 0.50 \text{ m}^2 \text{ s}^{-1}$, which is included in the original error propagation analysis.

Changes made:

Where r_a is the aerodynamic resistance, r_b is the viscous sub-layer resistance, and r_c is the surface (canopy) resistance. Figure 4 in Padro (1996) suggests that for ozone at night, $r_a \sim r_c \sim 250 \text{ s m}^{-1}$. r_b is likely non-zero (Massman et al., 1994) but will be neglected here because it is unknown.

Seems to me like it is important to spell out “The average error of Kz due to the uncertainty of vd is calculated to be ~0.50 m² s⁻¹, which is included in the original error propagation analysis” in the text close to this discussion

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?

Response: Specified that these are studies of NBL turbulence. Banta et al. (2006) is a meta analysis of other studies. To the best of my knowledge, buoyancy waves were not removed. While we were hoping that our TKE would have a relationship with ozone the following day, it is a very noisy measurement and we were also using many approximations to estimate it, as outlined in the paper.

Changes made:

Here we attempt to build confidence in the eddy diffusivity estimates by analyzing additional metrics of turbulence. We find that nocturnally and spatially averaged TKE in the NBL ranges from 0.35 and 1.02 m² s⁻², which is very comparable to values obtained in other NBL studies (Banta et al., 2006; Lenschow et al., 1988).

Can the authors please clarify in the text why they are mentioning that they did not remove buoyancy waves? I would suggest saying something like “differences between the studies may reflect Banta et al. 2006 removing buoyancy waves” if this is what the authors are implying

Please answer my question about the implication of the finding (now Lines 632-3

Line 659-60: Why is this more likely? What’s the implication of this?

Response: We are stating that although unstable layers are observed more frequently in urban areas compared to rural areas, we may have simply detected them more often there because the aircraft spends more time in urban areas. Hence, the apparent pattern of more unstable layers in urban areas could be insignificant.

Lines 663-4: Briefly, how would they contribute to overnight mixing?

Response: Absolutely unstable layers in the atmosphere promote the production of turbulence and thus vertical mixing.

Please incorporate the authors’ response into the main text

Line 675-6: How does this fit into the above discussion? What are the implications of this finding?

Response: This fits into the above discussion because we are showing the unstable layers appearing in the climatological averages of the 915 MHz profiler. The implications of this are that it lends some additional credibility to their existence.

Please incorporate the authors’ response into the main text

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 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 ...”

Response: Focus strategy of the flight restated in conclusion. The other requested changes have been made.

Changes:

A limitation of our study is the lack of sample size, with only 12 pairs of overnight and morning flights. However, we believe this study demonstrates the importance of synoptic and mesoscale features at night within the context of high ozone episodes, and the utility of this type of focused flight strategy where terms in the scalar budget equation are measured.

The larger set of RASS and ARB surface network data from Visalia, CA establishes a correlation between low level jet speed and the maximum 1-hour ozone the following afternoon for summertime months, further suggesting the link between nocturnal mixing and the following days ozone. Similarly, the correlations between the aircraft-estimated eddy diffusivities and MDA8 the following day also suggest that vertical mixing in the NBL plays an important role in determining ozone concentrations. In particular, we note that 11 of 12 days where the Visalia, CA ozone concentration exceeded 100 ppb was preceded by a low-level jet speed < 9 m/s. While we cannot determine a causal relationship between a strong low-level jet, stronger mixing, and reduced ozone pollution, we propose that a stronger LLJ leads to greater mixing, which helps deplete the ozone reservoir by bringing it into the stable boundary layer overnight. There it is subject to deposition to the surface, and that dry deposition rate may itself be partially modulated by the strength of the LLJ through reduced aerodynamic resistance resulting in more efficient transport to surfaces where ozone can deposit. Subsequently, when thermals begin to form after sunrise the following morning, there is less ozone to fumigate downward. While the correlation between nocturnal mixing and ozone the following day is not always strong, it is an important link that may have consequential implications for modeling studies and policy making. For example, our findings highlight the crucial need of models to capture the LLJ and Fresno eddy with sufficient resolution. Policy makers may consider putting more stringent emission limitations on days where synoptic and mesoscale patterns appear to favor a lack of overnight mixing.

Of course, in addition to nocturnal mixing, photochemical production of ozone as well as advection will play a major role in the ultimate daytime peak ozone levels observed, which may be why the correlation between nighttime turbulence and afternoon ozone is not always high. Airborne measurements from flights over Bakersfield, CA showed an average photochemical production as high as 6.8 ppb h⁻¹, with an

average advection of -0.8 ppb h^{-1} , though on any given day advection tended to be more comparable in magnitude to photochemical production (Trousdell et al., 2016).

Lines 704-6: Spell out the implication of this finding.

Response: We were mainly pointing this out to remind the reader that even though the advection term on average tends to be near zero, it can be large for any particular data point.

Changing “within the context of” → “for”, “establishes”-> “shows”, “the following days” -> “next-day”, “a lack of overnight”-> “weak nocturnal” would be helpful

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”

Response: Here we are reminding the reader that there is more to the picture than just vertical mixing of ozone at night, since afternoon ozone concentrations are influenced by advection and photochemical production.

Changes made:

In that study they have demonstrated that on days with very high ozone that pose hazards to human and agricultural health, the ozone abundance is dependent on elevated ozone in the mornings that serve to catalyze photochemical production through the afternoon. Future

modeling studies may directly investigate these factors, which may help elucidate the causal mechanisms of high ozone events.

We have also suggested that the fate of the NO_3 plays an important role in the nocturnal Ox budget chemical loss term, and thus likely impacts the following day’s maximum ozone concentration.

I find the discussion of Trousdell et al. 2016 tangential (and thus confusing for the reader). I agree that it is important to point out that photochemical production may lead to the weak correlation. This is could be spelled out concisely after “While the correlation between nocturnal mixing and ozone the following day is not always strong, ...”. On a similar note (in terms of re-structuring this section), I recommend cutting “it is an important link that may have consequential implications for modeling studies and policy making” because it is vague and wordy and the following sentences illustrate this point well.

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?

Response: We have followed these suggestions and are also stating that deposition velocity measurements of ozone using eddy covariance on future campaigns would be helpful.

Changes made:

We have also suggested that the fate of the NO_3 plays an important role in the nocturnal O_x budget chemical loss term, and thus likely impacts the following day's maximum ozone concentration. The loss of the nitrate radical at night can occur from N_2O_5 hydrolysis, reaction with VOCs, or a very rapid reaction with small NO concentrations, and there is considerable uncertainty regarding which reactions dominate without direct measurements of NO_3 . Thus, the lifetime of NO_3 can range from seconds to several minutes, which affects the chemical loss term in the scalar budget equation. It is thus crucial to measure the lifetime of NO_3 in future studies that analyze the NBL ozone or O_x budget. We also suggest more direct measurements of aerodynamic resistance and ozone deposition at the surface by eddy covariance in conjunction with future airborne studies.

Direct measurements of aerodynamic resistance are not really feasible at this point so I would recommend slightly rephrasing. Additionally, it's not really clear whether the authors want airborne ozone eddy covariance fluxes, or ground-based ozone eddy covariance fluxes.