

We would like to thank reviewer #1 for providing insightful comments on our submission to ACP. As stated in our response to reviewer #2, we are aiming to better partition the methodology and results. We are also making some revisions such that the goals of the study are articulated towards the beginning.

Most of the figures are being adjusted to improve quality and legibility. In particular, we plan to adjust the terrain to be greyscale, higher resolution, and less distracting. However, we believe the terrain in each figure is important because flow characteristics in California are highly influenced by it. We will use larger text for fonts, as requested.

We would like to report that removing the single outlier point of the MDA8 vs. LLJ correlation does not substantially affect the results ($r^2 = 0.167$).

Line-by-line comments:

Lines 56-57: Done.

Line 58: Done.

Line 63: Done.

Line 73: Changed “occasion” to “are associated with”.

Lines 96-97: Done.

Lines 101-104: We have removed these last two sentences.

Line 110: Done.

Lines 112-128: Added “The complex nocturnal wind patterns in the SSJV contribute to the challenges of understanding and forecasting ozone pollution in our study region” to the beginning of the paragraph. Also changed “ozone pollution potential” to simply “ozone pollution”.

Lines 129-140: Here we are discussing the context of the scalar budget equation in general, although I do understand why discussing daytime studies in detail might be confusing. The discussion about advection is relevant for both daytime and nighttime scalar budgets, but we changed the sentence regarding daytime photochemical production to “Studies performing daytime scalar budgets of ozone (Conley et al., 2011; Lehning et al., 1998; Lenschow et al., 1981; Trousdell et al., 2016) have shown that chemical production is important, and similarly, we expect the chemical loss of ozone to be important at night.”

Line 152: Yes, changed to “the aforementioned ozone difference”.

Line 157: 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.

Line 161: Changed to “ozone problems in southern Taiwan”

Lines 194-196: Changing the cutoff will result in different TKE values, but the night to night variability should not be affected by this. The TKE analysis is mostly supplemental to the main thesis and would not change our conclusions. This is an issue that arises in any stable boundary layer study.

Lines 199-204: The similarity relationships are employed as a best approximation and we acknowledge that the uncertainty in our TKE estimates are high. Again, we do not use the TKE estimates for anything critical to our conclusions.

Lines 241-242: Removed.

Line 247: Changed to “then computed by the reaction ..., and the ultimate fate of nitrate will affect...”

Line 259: Done.

Line 271: Changed to “all three of these methods were used in tandem.”

Line 290: Changed to “as these are by far the dominant species of O_x.”

Lines 319-386: To clarify the aim of this paragraph, we added “Thus, determining the dominant loss of nitrate is crucial for our analysis” to the end of the previous paragraph (line 318). We started a new paragraph on line 327 (“There is a further question), 347 (“With longer lifetimes”), and 355 (“Given the obvious”).

Lines 323-324: Changed to “However, both the ground network and aircraft observations may be biased high to the regional average because of their proximity to...”

Line 327: Yes.

Line 330: We have clarified that we mean VOC reactions in Table 2. For these calculations, we are only considering the VOC channel of nitrate loss (R5) in order to answer the question of whether or not R5 is important.

Lines 319-330: As stated in line 355, we conclude that R6 should not be ignored. We added this statement in line 330 for clarity.

Line 344: Done.

Line 348: Done.

Line 352: The temperatures of Figure 5 are later referenced in lines 480-482.

Table 2: 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. We added this to the text.

Line 390: It is our opinion that the linear regression was concisely summarized here.

Line 403: Specified that this study was done in a flat grass field.

Line 404: Replaced “these agricultural regions” with “the SSJV”.

Line 405: Corrected the reference list to include Meszaros et al. (2009) and Pederson et al. (1995). We found the Lin et al. (2010) reference to be the most helpful in that it summarized past estimates in Table 3, and it specifically focused on nocturnal dry deposition values.

Line 409: Done.

Line 410: Discussed in our response to reviewer #2.

Lines 412-413: Changed to “A linear regression through the 20 m resolution vertical O_x profile is used to determine dO_x/dz (for the last term in equation 1) in the upper...”

Lines 423-424: Discussed in our response to reviewer #2.

Line 429: Done.

Table 3: Done.

Line 433: Done.

Line 434: Done.

Line 438: Yes. Changed to “ O_3 is less than NO_2 by...”

Line 445: 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.

Line 455-456: Done.

Section 3.3: 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.

Lines 468-472: Changed to “Zhong et al. (2004) uses a series of 915 MHz radio acoustic sounding systems to analyze low-level winds in the SSJV. Their Figure 4 shows that at night, ...”

Line 473: Changed to “...observations, suggesting the presence of a Fresno eddy during our flights.”

Lines 480-482: 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.

Figure 7: The color scheme used was the best one we could find in terms of readability. However, we have increased the resolution so that the arrows stand out better.

Lines 494-495: changed to “...variability of maximum daytime ozone concentration, we explored the synoptic patterns that are associated with differing strengths of the LLJ”.

Line 498: Changed to “averaged in 100 m vertical bins, ...”

Moved “To analyze variability ... (N=165 nights)” to the first sentence of the following paragraph (line 506) for better flow.

Line 506: Yes, corrected in text.

Line 512: Added “(and thus LLJ)” after Fresno eddy mention.

Line 516: Changed to “those synoptic conditions”.

Lines 516-526: We have made this a separate paragraph.

Lines 522-527: Changed to “As a provisional synthesis of these seemingly conflicting findings”

Lines 523-524: We are suggesting that the LLJ is the strongest branch of a Fresno eddy, thus a strong eddy will produce a strong LLJ. We have attempted to clarify this in the text.

Line 527: Changed to “In addition to the synoptic patterns discussed above”

Line 532: Thank you for pointing this out. We have added this.

Line 534: Done.

Figure 11: Removed along with the discussion of it.

Line 551: Section 3.4 now starts here. We removed figure 12 and instead added the TKE profile to figure 4, and reference that here.

Line 562: Done.

Line 567: Changed “the relationship” to “this relationship” for better flow.

Line 566: Done.

Line 574: Jin et al., JGR, 2013 suggests that the MDA8 occurs fairly consistently between 13 and 14 PST. For the 24 hour average ozone correlation with eddy diffusivity, $r^2 = 0.40$. I believe that the relationship is notably weaker for the fumigation periods due to slight variations in timing of the peak boundary layer growth rate.

Line 578: Changed to “Because this analysis consisted of only 12 flights, we decided to explore a larger data set that might support the hypothesis that a stronger LLJ reduces ozone the following day.”

Lines 580-583: The relationship with MDA8 was slightly weaker. We have added this in the text so that we are not biasing our results to only showcase the best correlations.

Figure 13 and 14: Done.

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.

Section 3.5: Done.

Line 607: Specified that these are studies of NBL turbulence.

Line 610: Banta et al. (2006) is a meta analysis of other studies. To the best of my knowledge, buoyancy waves were not removed.

Lines 610-611: 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.

Line 624: Done.

Line 626: Done.

Line 631: Done.

Line 634: Done.

Lines 645-649: Disagree with reviewer here because subjective turbulence should be mentioned before delving into it.

Line 636: Done.

Lines 640-644: Yes, the point of this analysis was to build confidence of our eddy diffusivity measurements. We have clarified this in the text.

Lines 658-659: changed to “as the unstable layers appear to be above the NBL where there is communication with the surface.”

Lines 659-660: 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 663-664: Absolutely unstable layers in the atmosphere promote the production of turbulence and thus vertical mixing.

Figure 16: We are making the requested adjustments to the figure. Only 50 m is shown in order to reduce the number of figures in this submission, and we did not believe the 100 m thickness plot added anything particularly useful.

Lines 668-669: We did not include this figure for sake of brevity.

Line 669: We are referring to the finding stated in the previous sentence. “Finding” has been removed for better flow.

Lines 674-675: Done.

Lines 675-676: 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.

Line 687: Done.

Line 689: Done.

Line 690: Yes and done. Also changed to “the study demonstrates”.

Line 691: Focus strategy of the flight restated in conclusion.

Line 692: Done.

Lines 692-693: Done.

Line 694: Changed sentence to “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.”

Lines 695-696: Adding a brief discussion of specific modeling and policy implications.

Line 697: Specified Visalia, CA.

Line 698: Done.

Line 701: Done.

Line 704: Done.

Lines 704-706: 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.

Line 706: Done.

Lines 706-710: 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.

Lines 711-712: Done.

Lines 712-715: We have followed these suggestions and are also stating that deposition velocity measurements of ozone using eddy covariance on future campaigns would be helpful.