Atmos. Chem. Phys. Discuss., 10, C8338–C8362, 2010
www.atmos-chem-phys-discuss.net/10/C8338/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of transported background ozone inflow on summertime air quality in a California ozone exceedance area" *by* D. D. Parrish et al.

D. D. Parrish et al.

David.D.Parrish@noaa.gov

Received and published: 6 October 2010

The authors are grateful to the referees for the time and thought that they put into their reviews and comments regarding our paper [Parrish et al., 2010] hereinafter referred to as *Petal.10*. The revised manuscript is significantly improved as a result of our response to some of their comments. We respond to other of their concerns below; formulating this response helped us to review and refine our analysis and further improve the paper.

At this point we urge the referees to carefully review their initial concerns in the light of our response and provide a positive judgment. We believe that this paper does address

a very important air quality issue in a substantive manner. This issue is particularly important with the present consideration of further lowering the O_3 standard for U.S. air quality (http://www.epa.gov/air/ozonepollution/actions.html#jan10s).

General Comments:

1) Both referees express confusion regarding the conceptual model that underlies our entire analysis. It is expressed in Eq (1) of *Petal.10*:

O_3 = O_3(background) + O_3(regional) + O_3(local). (1)

Our goal is simply to clearly communicate one concept: Measured O_3 concentrations at a surface site are influenced by transport of background O_3 from outside the region in which the site is located, production and loss processes characteristic of that region as a whole, and local production and loss processes characteristic of the very local environment of that particular site. Conceptually speaking, it is well known and widely accepted that background, regional and local influences all affect measured O_3 concentrations at any particular site. For example, the second paragraph of the draft Executive Summary of the HTAP 2010 Assessment (http://htap.icg.fz-juelich.de/data/ExecutiveSummary) states:

"O_3, PM, Hg, and POPs are significant environmental problems in many regions of the world. For each of these pollutants, observed concentrations or deposition at any given location can be thought of as composed of several different fractions, one of which is related to the intercontinental transport of anthropogenic emissions. Other fractions may be associated with natural emission sources or local and regional anthropogenic sources. Each fraction differs in terms of the emission sources that contribute to it, the temporal and spatial variability of the contribution, and the potential for and sensitivity to emission controls."

Eq (1) explicitly states a version of this thought in an equation that meets our needs. We must communicate this concept in an explicit, quantitative manner because our goal in this paper is to approximately quantify the first term of Eq (1), while being mindful of the potentially confounding effects of the second two terms. We do not intend to convey any meaning beyond this generally accepted understanding, and do not imply that we are able to separately quantify all three terms. However, it is critical that we keep this concept in mind when interpreting our analysis, and the terms in Eq (1) provide a convenient way to refer to the three influences. Our response to the next general comment below provides an excellent example of the importance of considering the influences of all three terms of Eq (1). We have clarified the discussion of the conceptual model in our revised manuscript.

2) Both referees express concern regarding the relatively low correlation coefficients for the relationship between O_3(background), as estimated by the sonde measurements, and the maximum daily 8-h average (MDA8) O_3 concentrations measured at surface sites. However, since the MDA8 O_3 is affected by multiple influences (three as formulated in Eq. (1)), the correlation with only one of the influences cannot be expected to be large. More quantitatively speaking, as noted by referee #1, the square of the correlation coefficient, r^2, can be taken as an estimate of the fraction of the variance of a dependent variable that can be explained by its dependence on an independent variable. Table 2 of *Petal.10* summarizes the correlation coefficients between the MDA8 O_3 concentrations measured at all the surface sites. For example, r = 0.70 for Tuscan Butte and Redding (see Fig. 2 of *Petal.10*), two surface sites in the North Sacramento Valley (NSV). This value for r implies that about 49% of the variance of the MDA8 O_3 at Redding can be explained by covariance of the Redding data with the Tuscan Butte data (although the cause of the covariance is certainly not established.) Therefore, this r value also implies that the remaining approximately 51% of the variance of the Redding data cannot be explained by influences common to the two sites. (This would be one approach to quantifying O_3(local) in Eq. (1), if one wished.) Thus, one can hope to capture only about one-half of the variance of MDA8 O_3 at a site by considering only the background and regional influences. The fact that the sonde data can capture approximately 50% (in some cases more) of this "non-local" or "region-wide" variance

(i.e. the variance caused by the first two terms of Eq (1)), and that those sonde data can capture approximately that fraction of the variance for all six inland surface sites are remarkable findings indeed. They are especially remarkable, since regional photochemical contributions (i.e. O_3(regional)), to which the sonde data are not sensitive, are often considered to be the dominant driver of regional scale ozone episodes. In summary, a careful consideration of the influences of all three terms of Eq. (1) indicates that the correlations discussed in this paper must be considered remarkably strong. We have incorporated some of this discussion into our revised manuscript.

3) Both referees suggest some sort of additional event-based analysis be included in the paper. However, given the constraints of available data sets this really is not a productive approach for two reasons. First, as we discuss in the paper, the existing meteorological data sets are not adequate to provide more than a general indication of large scale flow patterns in a region of complex terrain, such as characterizes the northern California coast. They cannot resolve the small-scale features we believe responsible for vertical transport of air into the NSV. The recently completed CalNex 2010 field study and the ongoing associated fine scale meteorological modeling will provide a basis for such event-based analysis, but that analysis is not yet available and is far beyond the scope of this paper. Second, as the discussion in General Comment 2) emphasizes, the variance in observed surface O_3 concentrations is large and is affected by multiple factors. Thus, it is difficult to specify exactly define an "event". An event defined by high O_3 observed by a particular sonde launch may or may not affect a particular surface site depending upon a number of factors including large scale flow, meteorological conditions (e.g. cloud cover) that affects vertical transport, etc. Consequently, the statistical approach we have chosen is certainly appropriate, and is the only practical approach likely to be successful.

Point by Point Responses to Referee Comments:

(The referee comments are reproduced in italics; our response follows in plain text.)

Referee 1:

Pg. C6701

- *This analysis is an important step towards addressing the role of background in contributing to air pollution events in the Northern Sacramento Valley (NSV) region. I'm not convinced that the interpretation of background contribution to exceedances is well supported by the analysis, which requires extrapolating from relationships that only capture 25-50% of the observed variability. However, additional event-based analysis could provide this support (see specific comments).*

We address these comments in our response to General Comments 2) and 3) above.

- *I don't see where the paper addresses what is causing the observed variability in the sonde data but this seems key to the interpretation and conclusions.*

We did not address the cause of the observed variability in the sonde data. The causes are certainly complex, including long-range transport of pollution, a wide manifold of differing emission and photochemical histories in the air masses approaching the California coast, varying stratospheric influences, etc. Presumably global CTM modeling can potentially elucidate the causes, but a description accurate in space and time is presently beyond the capabilities of any existing model. However, those causes are not central to the interpretation and conclusions of this paper; for our purposes it is enough that this variability has indeed been observed and characterized by the sonde data (as well as other data sets). We have added a short discussion of the likely causes of the variability as contextual material for our results in the revised paper.

- *The conceptual model in Section 3.3 is confusing and I question if it is "required" as stated in the text (see below).*

We address this comment in our response to General Comment 1) above.

Pg. C6702

- *The paper clearly addresses a need from the policy community as discussed in Dr. Lashgari's short comment. I suggest either adding more analysis to support the conclusions or limiting the discussion to only those points clearly supported by the analysis, with discussion of what additional work is needed to provide conclusive evidence as to the implications for the role of background on NSV exceedances; the latter is already done to some extent in Section 7.*

Thank you for recognizing the importance of our paper. We see no additional analysis that we can usefully add. As discussed with regard to the specific comments below, we believe that all of our points are clearly supported by the analysis that we have presented. We have added a short discussion of needed additional work to Section 7 of the revised paper.

- *This correlation analysis is based on several assumptions: the sondes are measuring background levels; background is not produced over the continent or subsiding to the NSV in other air masses not sampled by the TH sondes; background is not lost during transport or mixing to the surface. This could be more clearly stated.*

We are assuming that the sondes are measuring background O_3 levels; this is a widely accepted approach (e.g., see Oltmans et al., 2008), and Appendix A1 further supports that assumption. We are also assuming that the air masses passing over Trinidad Head and sampled by the sondes are a representative sample of the air masses subsiding to the NSV; given the proximity of Trinidad Head to the NSV, the topographical isolation of the NSV, the variability of the large scale flow patterns (see Figs. A1 and A2 of *Petal.10*) that bring air predominately from the west but with substantial variability, we do not see how this assumption can be questioned – from where else could the subsiding air come? We are not assuming that background is not produced over the continent; such production would be included in the O_3(regional) term of Eq (1). Nor are we assuming that background O_3 is not lost during transport or mixing to the surface; we allow the correlations to reveal to us how much the background O_3 actually affects the measured surface O_3, but do not assume that it is 100% of the O_3 measured by the sonde.

Indeed, *Petal.10* do discuss this issue at the bottom of pg. 16250 in the context of the relationship of our results to the policy relevant background O_3. We have clarified our assumptions in the revised paper.

- *The intercept is likely very sensitive to the fit used but the derived number is critical to the conclusions drawn regarding the contribution of local vs. background, so some discussion of this uncertainty is needed.*

In general the uncertainty of the intercept of a linear regression is difficult to accurately define, because as the referee notes, the intercept is very sensitive to the fitting criteria and the data used in the fitting procedure. However, if the axes for any linear fit are transformed to an origin that is centered on the average of the x and y data, then the intercept is zero regardless of the fit (e.g. see Figs. 6 and 12 of *Petal.10* where all fits go through a common point, which is in fact the average of the x and y data. It is the uncertainty of the transformation back to the original axes with origin at O_3 = 0 for x and y that gives the largest contribution to the uncertainty of the intercept. However, if a slope of unity is specified (as we do in our quantitative analysis) the intercept is numerically equal to the average of y minus the average of x, and the uncertainty of the intercept is equal to the uncertainty in the differences of the averages. (This discussion is related to the specific comment P 16242 L4-5 that is further discussed below.) For our purposes we only make use of the average difference between the two data sets being fit, which is equal to the uncertainty of the mean difference between data sets. The 95% confidence limits of these intercepts (i.e. the confidence limit of the mean differences) are given in the annotations in Figs. 6 and 12 and in Table 3 of *Petal.10*. We have added a short discussion of this uncertainty to the revised paper.

- *More evidence is needed to support the idea that the sondes don't include a "regional" component, particularly since some of the trajectories in Figure A1 show an occasional continental influence at the sonde location.*

We agree that more evidence is needed, and we are preparing a manuscript for

submission that provides such evidence. Here, we do note that on the relatively few occasions that trajectory segments pass over portions of the continent before arriving at Trinidad Head does not necessarily indicate continental influence. This is because the trajectories remain in the free troposphere, and are generally isolated from boundary layer influence. Preliminary results of the analysis being that provides additional evidence were presented in a poster at the Fall 2009 AGU meeting (see Abstract A41D-0136). The poster that resulted from this abstract is now posted at http://esrl.noaa.gov/csd/pubs/acpd_10_16231to16276_2010/refereeresponsefile.pdf. The manuscript presently being prepared fully describes this analysis. In summary, MOZAIC profiles at Los Angeles and Portland, a flight conducted by the NASA DC-8 off the coast of California in June 2008, and the sondes and aircraft flights at Trinidad Head all agree that the boundary conditions coming ashore from the Pacific along the western coast of the U.S. are not significantly affected by a North American "regional" component.

- *The contribution of background to exceedance events is limited to a discussion of averages. The correlations, though statistically significant, are only explaining at best 20-50% of the variance at the NSV sites (Table 3). In Figure 12, the highest events at Tuscan Butte and Redding are not correlated with the sondes; similarly the highest sonde values fall off the best-fit line. What are the trajectories on these days? Some analysis for specific exceedance events could provide more compelling evidence for the conclusions drawn here.*

Yes, the contribution of the background must be limited to averages, as this is a statistically based study. The relatively low correlations are addressed in our response to General Comment 2) above. A transport-based study of the highest events at Tuscan Butte and Redding and/or based on the highest sonde values might be illuminating. We would certainly expect a wide variety of events including episodes dominated by transport of particularly high O_3(background) and episodes dominated by particularly large contributions of O_3(regional) and /or O_3(local). This variety of episodes is consistent with points lying off of the best-fit line noted by the referee. However, our goal is not to investigate a variety of episodes; rather we aim to provide as complete a picture as possible using the broadest statistical sample possible from the 12 years of sonde launches. Further, as discussed in our response to General Comment 3) above, we expect any transport-based study that relies solely on the available large-scale trajectory calculations to be severely compromised by the inability of the trajectories to capture the important small-scale transport features.

- *The trajectory analysis should be included in the main paper.*

Given the limited use of the trajectory analysis in this paper, we prefer to leave it in the Appendix.

Pg. C6703

- *The temporal offset for the highest correlations between the sondes and surface sites is first interpreted as suggesting a direct transport pathway but this is not strongly supported by the trajectory analysis. P16247 L5-11 discuss that direct transport is not required to explain the observed correlations but then how does one explain the time offset? This could be reconciled if there are times when direct transport is occurring and other situations where the correlation represents a large-scale influence – this could probably be addressed by examining specific events.*

Given typical wind speeds at 1-2 km altitude in summer, what is the time scale for transport from TH to the NSV sites? Showing a map of summer wind climatology at 2km and possibly also at the surface would be very helpful; e.g. from the North American Regional Reanalysis (http://www.emc.ncep.noaa.gov/mmb/rreanl/). NARR data may also address the need discussed on L2-5 of P16251.

Part of the confusion here seems to be in the definition of "direct". Our hypothesis, simply stated, is that the sondes characterize background O_3, which does have a large-scale horizontal spatial dimension. *Petal.10* cite Liu et al. (2009), who derive a 500 to

1000 km spatial dimension. This sonde-characterized background is transported to each of the fairly widely spaced surface sites. However, this transport cannot be (and does not need to be) direct point-to-point transport from directly above Trinidad Head to a particular surface site. Rather there is a general movement of the characterized background from above Trinidad Head inland with the prevailing winds, followed by a mixing of this background to the surface. The maximum correlation between the surface data and the sonde data occurs after a time delay determined by time required for these two transport events. This time delay is only partially determined by the typical wind speeds; the time for the mixing to the surface, which has strong diurnal variation, must also be considered. Again, examining specific transport events is not possible as discussed in the response to General Comment 3) above.

- *P 16242 L4-5, how do the intercepts compare with the more straightforward averages of the datasets?*

This question is answered in exactly the lines referenced: "The intercepts of these fits give a measure of the offset of the data sets from each other; they are numerically equal to the average difference between the MDA8 O_3 at the two sites." We have clarified the discussion in this regard by changing those words to ". . . they are mathematically identical to the average difference . . .".

P16242 L6-8. What about large-scale conditions conducive to ozone production or destruction at both TH and the NSV sites? Same question for P16243, end of Section 4.1; in addition, is the 0.8 h offset meaningful given the time resolution of the datasets? P16251 suggests this is speculative without a driving mechanism yet the regional nature of synoptic weather influencing pollution has been demonstrated for the eastern USA: (3 references.)

We are not clear what the referee is questioning here. The referenced lines are accurate – the dependence of the correlation on the time shift between data sets does indeed "provide information regarding the transport of ozone within the region" even in the presence of "large-scale conditions conducive to ozone production or destruction." Of course, the effects of transport and ozone production or destruction would have to be appropriately separated. However, this issue does not affect any of the analysis we present. We discuss the 0.8 $\pm$ 0.4 h offset only as being close to 0, so, no, we are not arguing that it is significantly different from 0. We do agree that the regional nature of synoptic weather influences pollution, and would certainly agree that influence is important in the NSV. This influence is explicitly included in the O_3(regional) term in Eq. (1). However, such regional effects are confined to the boundary layer, and are not expected to influence the O_3(background) term in that equation, since that is determined by sonde measurements from above the MBL.

Pg. C6704

- *Abstract L10-12 seems inconsistent with the discussion on the bottom of P16250 that indicates these approaches are not comparable. Where is the attribution to hemispheric scale transport in the next sentence supported by this analysis?*

It is clear that the background O_3 derived by *Petal.10* is higher on high O_3 days. This clearly conflicts with the model calculations that find that the policy relevant background decreases during pollution episodes. As discussed on P16250 there are important differences between the background O_3 derived by *Petal.10*, and the policy relevant background from models, but the differences cannot be large enough to account for this conflict. The marine air coming ashore, which provides the background O_3 derived by *Petal.10*, certainly reflects O_3 transported between continents.

- *P16237 L20-22. This statement should be supported by a quantitative assessment such as a correlation coefficient for the seasonal cycles. The seasonal cycle in the sonde data (Fig 4) has a springtime peak which seems closer to the MBL sites than the inland sites.*

The sonde data do have a springtime peak, but overall they have a broad spring summer maximum. The correlation coefficients that the referee requests are now included

in the revised manuscript, and they do support our statement. The lines in question have been modified to read: "Interestingly, the seasonal cycle defined by the sonde measurements above the MBL (1-2.2 km average) is more similar to that at the inland surface sites (e.g., r = 0.72 and 0.71 at Lassen NP and Redding, respectively) than that within the MBL (r = 0.25 at Trinidad Head)."

- *Section 3.2. Could ship emissions also be contributing to the low values measured at the MBL sites?*

Ship emissions could be increasing or decreasing low O_3 values measured at the MBL sites, but this would not affect the analysis of *Petal.10*. Regardless, papers that have investigated the photochemistry of ship plumes (e.g. Chen et al., 2005) indicate relatively modest O_3 formation from ship emissions in the MBL. Further, the poster available at http://esrl.noaa.gov/csd/pubs/acpd_10_16231to16276_2010/refereeresponsefile.pdf indicates similarly low MBL O_3 concentrations far from shore, outside of the major shipping lanes.

- *Section 3.3. P16239. I see the value of separating an ozone measurement into contributions from processes operating on different scales, but in practice the attribution is ambiguous, as noted later in the paper. For example, although diurnal cycles occur locally (L11 p. 16239) and in part reflect local emissions contributing to ozone production, these chemical processes are operating in tandem with the growth and decay of the boundary layer (and the mountain valley flows illustrated in Figure 11), which mix "background" and "regional" ozone into surface air at the same time as local chemical production is likely highest.*

We do not disagree with the statements by the referee.

L10-11. Are these terms "responsible for synoptic scale variability" or rather varying consistently across sites on synoptic scales?

We do not appreciate the distinction that the referee is attempting to make here. We have changed the wording to "responsible for the synoptic scale variability of O_3"

L12-14. I don't follow the distinction in regional versus local processes; isn't "regional" the aggregation of "local" processes, or synoptic meteorology creating conditions that influence chemistry and deposition similarly?

The important distinction between "local" and "regional" processes is that O_3 at two sites within the NSV would have similar regional contributions, but potentially quite different local contributions. We do not disagree with the referee's description of the source of the regional contribution. However, it is important to realize that O_3 can have a regional contribution from photochemical and deposition processes, not just from transport of background O_3; hence our inclusion of the O_3(regional) term in Eq (1).

L24-25 and P16240 L8-9 further confuse the argument since any measurement includes the sum of background+regional+local as shown in Eq (1), and it is argued later in the paper that MDA8 does in fact contain a local component (P16241 L24-25).

Yes, unfortunately MDA8 does indeed include a substantial local component, but it is the best available approximation for the sum of the O_3(background) + O_3(regional) contributions. We have added some further discussion of this qualification to our revised manuscript.

- *P16245 L21-26 The correlation coefficients are small (r<0.3), so is this discussion necessary? Similarly on P16248 L20-27, why is it NSV character rather than general continental outflow? I'm not convinced this discussion is necessary.*

Thank you for the suggestion. This discussion has been removed from the revised manuscript.

Pg. C6705

- *P16246 L2 The offset looks like up to 10-20 ppb; is this "modest"?*

Within the context of the conventional thinking, which considers surface ozone at con-

tinental sites to be primarily due to photochemical production over the continent, the 10-20 ppbv contribution from continental influences is indeed modest. This wording has not been changed.

- *P16246 L14-15. It would better support this argument to show differences in, for example, mixing depths in winter versus summer for this region (perhaps from the NARR product mentioned above).*

We do not believe that the seasonal differences are simply related to differences in mixing depths. Rather we atttribute the seasonal differences to differences in the mountain-valley heating that we believe drive the transport as discussed with reference to Fig. 11 in *Petal.10*. A full resolution of these issues await detailed transport modeling as we discuss in our response to General Comment 3) above.

- *P16249 L17 What is the weighting used in the averaging and why? I'm not convinced that the correlations support the interpretation of the slope as a measure of air mass origin. How does this explain a slope above 1 at Redding?*

The slope at Redding is 1.10±0.39 (95% confidence limit). Hence, this slope is statistically equivalent to 1. There is no need to explain a slope above 1; it is consistent with a statistical distribution about a value of 0.88±0.13, which we take as the best estimate of the slope. The weighting used in the average is the inverse of the square of the confidence limits given in Table 3; such weighting is conventional when averaging numbers with differing uncertainty. The slope is indeed a measure of the air mass origin. Let us imagine a situation where the only influence on O_3 measured at a surface site was the transported background O_3. If that background came solely from transport of air sampled by the sonde, then the slope would be exactly 1 since the sonde and the surface site would measure identical air. If the background air came from some other source that was uncorrelated with the air sampled by the sonde, then the slope would be 0. A consideration of mixing indicates that a 50:50 mixture of two such sources of background air would yield a slope of 0.5.

- *P 16250 Would the faster removal processes at the NSV sites relative to Tuscan Butte also be accompanied by a larger chemical production? Would there be larger chemical loss of both locally produced ozone and transported background at these sites?*

The referee's questions are not clear to us. No, there would not be larger chemical production. Removal processes have less influence at Tuscan Butte because that elevated site is expected to be representative of a deeper boundary layer or receive air from above a shallow nocturnal boundary layer; thus nighttime O_3 at Tuscan Butte remains higher longer into the afternoon and evening hours than at the valley floor sites. In the revised manuscript the explanation has been changed to: "It is interesting to note that the largest net regional plus local contribution occurs at the most rural site (Tuscan Butte), not because the average peak O_3 is much higher (see Fig. 3), but rather because O_3 remains near the peak longer, evidently due to a lesser influence of removal processes at the rural, elevated terrain environment of that site."

- *Modeling work addressing the second "implication" on P16252 was recently published in ACP, with the first author on this manuscript as a co-author, yet it is not cited anywhere in the text.*

Good point; we have added a reference and a brief discussion of this work to the revised manuscript.

- *Table 3. Do the MDA8 values occur at the same time of day at all the sites? Why is the intercept for the slope of unity better than the intercept of the bivariate slope?*

The MDA8 values occur at various times of day at each site depending upon meteorological conditions, and the average time of day varies between sites as suggested by the varying diurnal cycles illustrated in Fig. 3 of *Petal.10*. This variability undoubtedly contributes to the relatively low correlation coefficients given in Table 3, but does not confound the results of our statistical analysis. As discussed above in our responses to the third comment on pg. C6702 and the second comment on pg. C6703, the slope of unity gives an intercept that is numerically equal to the average O_3 difference between

the two data sets, and the same information would be derived from the intercept of the bivariate slope if the axes were transformed properly.

- *Figures 4 and 5. Why not use the MDA8 here for consistency? These figures can be more effective by combining them, using the same scale on 2 panels side by side with all the surface data in one panel, and the sonde data in the other.*

For presenting seasonal cycles, we felt it was more important to use a consistent time window for all sites than to use the MDA8, which is more useful later. We prefer to keep the two separate figures as Fig. 4 is intended to show the comparison between the marine and continental environments, and Fig. 5 is intended to show the vertical dependence of the seasonal cycle of O_3 in the marine environment. We have however modified the figures so that they are in identical formats.

- *Is Figure 7 necessary in addition to Table 2?*

We have had some difficulty in clearly communicating the correlation procedure that we are using in this paper, and have found that we need a visual presentation of the time offset correlation. Figure 7 is necessary for this purpose.

- *Section A2. Could fires also explain the CO enhancements? Wouldn't the CO background of 100-130 ppb include some influence from U.S. emissions?*

Fires in Asia could explain the CO enhancements, and we have not ruled this out. As we discuss North American fires are seldom transported to the west and recirculated to the continent, so generally we do not believe that they are of North American origin. Trajectory analysis, which we have not carried out, would be required to definitely establish the origin of the CO enhancements. The CO background of 98 $\pm$ 12 ppbv presumably does include a contribution from U.S. emissions that are circulated zonally, as the CO lifetime is long enough for CO to circle the globe at northern mid-latitudes. Such issues complicate clearly defining what is meant by "background". This issue does not confound our analysis as we conceive "background" as that transported ashore from the Pacific, excluding recirculated North American emissions.

Technical corrections

- The term "marine air" is confusing since it can be interpreted as implying a certain composition of the air mass, such as the marine boundary layer air discussed. Consider rephrasing to clarify, for example, "inflow of air from the marine troposphere" on P16233 and elsewhere.

When we first use the term "marine air" we now define our meaning in the revised manuscript.

Pg. C6706

- The citation to Fiore et al. 2002 for PRB O_3 concentrations considered by EPA in the NAAQS process should instead be: (reference)

We have included both references in the revised manuscript.

- The discussion on the top of P16236 should refer to Table 2.

We disagree here. Table 2 deals with the correlations of MDA8 O_3 between sites and with the sondes, which is not addressed until pg. 16241.

- P16236 and elsewhere, clarify that "impacted" refers to local pollution sources or drop the phrase.

The use of impacted in this context has been removed throughout the revised manuscript.

- P16237. Typo: tow -> two

Corrected

- P16240 L5 MBL -> PBL or continental BL?

MBL has been changed to convective boundary layer

- Table 2. State which years are included in this analysis.

All available coincident data are included in the analysis. This is now stated in the revised manuscript.

- Figure 1 should include a color scale for topography and some indication of the horizontal distance between sites (e.g., adding lat/lon boundaries).

We have added latitude and longitude tics to the boundaries. We do not have a color scale for the topography elevation, but have added a semi-quantitative description to the figure caption.

- Figure 6. What is the dotted line?

The dotted lines indicate the standard linear regressions. They are now identified in the revised figure caption.

- Figures A1 and A2 should include the TH and NSV site locations.

The site locations are now indicated in the revised figures.

Referee 2:

Pg. C6762

- The errors described below on pages 16239 and 16240 are critical and must be corrected prior to publication.

We disagree with the referee. These are not errors, as discussed in our response to General Comment 1) above, and the page 16240 comment below.

Pg. C6763

- Pg 16233, Line 14: Prevailing winds are westerly in the free troposphere, especially during winter and spring. They show much greater variability at the surface.

The statement has been modified to read "...the prevailing winds in the lower free troposphere are westerly . . ."

- *Pg 16235, line 18: What time are sondes usually launced? This is important for correlation analysis.*

The sondes are generally launched during the three-hour period before noon, local time. However, the launch time is not important since the correlation analysis is based upon the interpolated MDA8 O_3 from the surface sites. This effectively removes all diurnal dependence from the analysis.

- *16236, line 12: I assume winter is DJF. Please state.*

Your assumption is correct. We now state this.

- *16238, line 25: This is an odd statement. The Eureka site is clearly an urban site.*

We do not understand the referee's concern here. We agree with the referee that Eureka can be considered an urban site, and do state ". . .at the less exposed and more polluted Eureka station located in a town." Parrish et al. (2009) discuss the large continental influences at Trinidad Head, probably one of the least impacted North American continental coastal sites possible. No change has been made in this discussion.

- *16239: I find this "conceptual model" very strange. I understand "O_3 (background)", especially the marine background, where surface deposition is relatively minor. But the separation between "local" and "regional" is artificial and not useful. Ozone is not an inert gas that can be divied up this way. The factors are not independent. For example net ozone production (dO_3/dt) is a function of NOx and O_3, which partly explains its non-linear behavior. The model is not only unrealistic, but also not necessary for their analysis or results.*

This comment is addressed in our response to General Comment 1) above.

- *16240, line 12: I cannot understand why one would take an hourly dataset, throw out 70% of the data and then interpolate between the remaining points to regenerate*

an hourly dataset. I can see no physical basis for doing this. While it is true that the nighttime data is highly influenced by local deposition and removal, it is a huge assumption to say that the nighttime data can be interpolated between successive 8-hour averages. This very odd interpretation of the hourly data can be completely avoided. The authors only need to examine the correlation of the sonde data with the maximum daily 8-hour average. The sonde data may or may not show a lag. This is the key analysis that the authors must show for their results to be believable.

The physical basis for our treatment of the data is quite direct. During the late morning through the mid-afternoon when the convective boundary layer (CBL) is active and well developed, surface O_3 measurements are representative of the planetary boundary layer (PBL). When the BL collapses in the late afternoon, as the referee notes, local effects strongly deplete the near-surface O_3 in the shallow nocturnal boundary layer (NBL), but O_3 above the NBL is not strongly affected. Hence, it is only during daytime that surface measurements are regionally representative, and the MDA8 O_3 is a representative measure of the regional O_3 concentrations. Further, MDA8 O_3 changes relatively slowly on a day-to-day time scale as illustrated in Fig. 3 of *Petal.10*; the correlation of MDA8 O_3 with that on the preceding day is high ($r \approx 0.7$-0.8) as can be seen for Tuscan Butte and the surface measurements at Trinidad Head from their autocorrelations illustrated in Fig. 7 of *Petal.10*. Hence the linear interpolation of successive MDA8 O_3 concentrations at a given surface site provides a good representation of the hourly variation of the regional O_3 concentrations.

This interpolation of the MDA8 O_3 concentrations to hourly data is required for a precise determination of the time lag between data sets, which is an important part of our analysis. Examination of the correlation of the sonde data with the individual MDA8 O_3 data cannot provide this information. However, it is possible to perform the time-lag correlation analysis with the raw hourly data. Figure 1 below compares these correlations with the interpolated MDA8 O_3 for two NSV sites, including Tuscan Butte shown in Fig. 8 of *Petal.10*. Besides greater variability, the two correlations are not markedly different at the Tuscan Butte site, where nighttime O_3 concentrations are minimally impacted by loss processes, but at the more polluted, lower altitude Redding site using the interpolated MDA8 O_3 data is particularly important. Both correlation analyses at both sites agree that the NSV sites lag the sonde data; however, the interpolated MDA8 O_3 data provide much more accurate and precise determinations of the time lags.

- *Line 27: I do not believe the lag times calculated from this massively modified hourly data are believable.*

As discussed in our response to the preceding comment, the analysis we have chosen is well justified, both physically and mathematically. The lag times derived for the six sites in Table 3 present a consistent picture of the transport from Trinidad Head inland.

- *16242, line 0-5: I don't understand why the slope is fixed. Let the data speak for themselves.*

As discussed in the paper and in response to a comment by Referee 1 above, fixing the slope at unity guarantees that the intercept is numerically equal to the average difference between the MDA8 O_3 at the two sites. We have clarified the discussion in this regard.

Pg. C6764

- *16244, line 3: The sondes are launched weekly, so I do not believe you can get an autocorrelation that is smaller than one week. This section is poorly explained.*

This is a good point. There were two 4 to 6 week summertime periods when the sondes were launched on a nearly daily schedule. This is now stated in the revised manuscript. This fraction of the sonde data, along with linear interpolation between successive sonde data points allows the autocorrelation determination on a time scale less than a week, and indeed even less than daily.

- *16245, line 1-2: This weak correlation is important. It indicates a clear problem with the method of using the interpolated one hour data. It is a big red flag that the surface*

data and the sonde data in the boundary layer show such a poor correlation.

This weak correlation is not indicative of a problem. It is indicative of the difficulty of separating the various influences that determine the MDA8 O_3 concentration at any continental surface site. The relatively small correlation coefficients are discussed in our response to General Comment 2) above.

- *Line 15: The lag times of 20-30 hours is not consistent with transport. Based on a very rough view of the trajectories, I guess the transport time to be 6-12 hours at most.*

The reviewer is evidently considering only the first step in the transport process: the time for air to move from above Trinidad Head to above the surface site. As discussed by *Petal.10* and in response to a comment by Referee 1 above, other time scales are involved as well. It will be interesting to see if the detailed transport modeling discussed in General Comment 3) above will be able to reproduce these lag times.

- *16248, lines 0-5: I think the authors have the mechanism wrong. Air subsides around a high pressure center, but only to the top of the boundary layer. At this point getting the air into the BL requires growth in the afternoon boundary layer and entrainment of this free tropospheric air.*

The reviewer correctly describes some aspects of subsidence and boundary layer evolution in regions of flat, homogeneous topography. However, the NSV does not fit these criteria. Subsidence and boundary layer evolution in regions of complex, heterogeneous topography such as the NSV is not well understood, and is the subject of ongoing research. Myrup et al. (1983), cited by *Petal.10*, does show that during the afternoon hours negative vertical velocity (i.e. indicating subsiding air) continues all the way to the surface at Sacramento, a location much further removed from the complex topography than the NSV area considered in this study. The schematic description of *Petal.10* of the transport mechanism may not be correct in all details, but as we discuss, it is not presented as the final word in the transport description, and it does capture many aspects of the transport.

- 16249, lines 17-20: This entrainment fraction is not correct. The problem stems from the fact that "fraction of air" is undefined. All air has come from everywhere if you give it enough time. Without considering daytime photochemical production of ozone, it is impossible to get this "fraction of air" from the correlations alone.

The referee's assertion is not correct. There exists some definite, quantifiable flux of air that flows into (as well as out of) the NSV. This inflow of air carries the background O_3 that is the subject of this paper. The correlation of O_3 concentrations within the NSV with the background O_3 concentration (subject to the assumptions stated in the paragraph of *Petal.10* preceding the lines in question) does indeed provide the means to quantify the fraction of air flow that carries the background O_3 measured by the sondes. One of these assumptions is that O_3(regional) and O_3(local) are independent of O_3(background). These issues are discussed further in response to a comment by Referee 1 above.

- 16252, lines 18+: These summary statements are fairly reasonable.

Thank you. The goal of our paper is to provide quantitative support for these statements.

- Figure 2: I can not see the sonde data on this figure. Please revise.

The color of the sonde data has been changed in this and the other figures to be more easily seen.

- Figure 3: I don't get much from the winter data. It would be better to just include summer.

The winter data illustrate the very important effects of local influences; we have kept them.

- Figures A1 and A2 don't tell us much. They would be more useful if they showed ozone on the highest days.

As discussed in our response to General Comment 3) above, we agree that analysis based upon large-scale flow fields do not tell us much. They can only tell us that the air sampled by the sondes generally flows from east to west, but with large variability. Event based analysis must await more detailed transport modeling.

References:

Chen, G., et al. (2005), An investigation of the chemistry of ship emission plumes during ITCT 2002, J. Geophys. Res., 110, D10S90, doi:10.1029/2004JD005236.

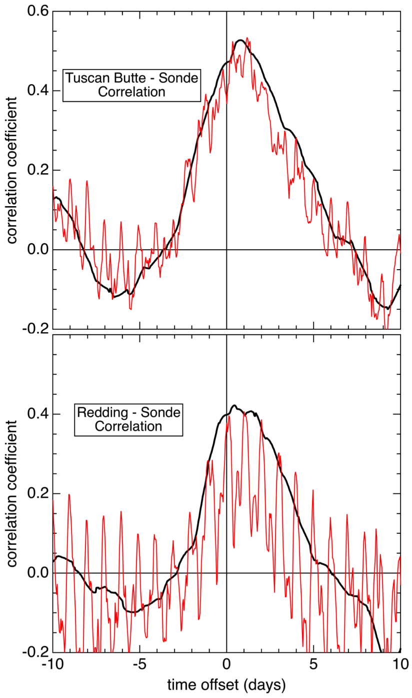
Oltmans, S.J. A.S. Lefohn, J.M. Harris and D.S. Shadwick (2008) Background ozone levels of air entering the west coast of the U.S. and assessment of longer-term changes, Atmos. Environ., 42, 6020-6038.

Parrish, D. D., K. C. Aikin, S. J. Oltmans, B. J. Johnson, M. Ives, and C. Sweeny, Impact of transported background ozone inflow on summertime air quality in a California ozone exceedance area, Atmos. Chem. Phys. Discuss., 10, 16231–16276, 2010.

Figure Captions

Figure 1: Correlation coefficient between summertime ozone at surface sites as a function of the time offset between data sets. Correlations are shown for both interpolated MDA8 O_3 (thick black lines) and raw hourly data (thin red lines). The time offset is referenced to Tuscan Butte and Redding in the upper and lower panels, respectively.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16231, 2010.



0.6
0.4
0.2
0.0
-0.2
correlation coefficient
Tuscan Butte - Sonde Correlation
Redding - Sonde Correlation
-10
-5
0
5
10
time offset (days)

Fig. 1.