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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increasing ozone concentrations in marine boundary layer air inflow at the west coasts of North America and Europe" *by* D. D. Parrish et al.

Anonymous Referee #1

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This is an interesting paper with some valuable analysis, despite the deficiencies of the data set. However, it is clearly not the last word on the subject, although the authors seem to be trying very hard to make it so.

This is the principal difficulty I have with the paper, that its strident tone will cause the reader concern about a possible lack of objectivity. Whether they intend to or not, the authors come across as quite offended by Oltmans et al. [2008], and seem to be trying to disparage that work. I was curious enough about this to re-read Oltmans et al. [2008] to see what was so irksome about it, and I'm still puzzled. While that work, like the present one, also deals with data sets that are either of limited duration or compromised quality, the analysis is thorough and sophisticated and the basic conclusion (no trend) is presented simply, with an air of "Isn't that interesting?" (or surprising, given that one might well expect to see a positive trend, as Parrish et al. have found). However, Parrish et al. state (p. 13852, l. 9-10) that their purpose is to "evaluate the suggestions of Oltmans et al. that the previously derived trends do not represent the marine troposphere." (I cannot find such a statement in Oltmans et al.) and claim (l. 13-14) that they show "that the data sets and analyses on which Oltmans et al. [2006, 2008] based their suggestions have serious shortcomings." In fact, they show no such thing; the Oltmans et al. analysis is, if anything, more sophisticated, and although the Yreka data set (which they devote two pages to criticizing) is certainly questionable, the Olympic Park one is equally so. Moreover, their trend results for Yreka are substantially the same as those of Oltmans et al..

The result of this is that the authors do themselves a grave disservice: as their work is presented now, I'd bet dollars to doughnuts that many readers will close the paper and say "I don't believe it". The sceptical reader will have several reasons for concern, which I note below.

p. 13852, l. 15: Lassen Volcanic Park is described as a site that receives "direct inflow of marine air", despite the fact that it is much further inland than the Yreka site.

p. 13861, l. 27-30: "It should be noted that simply selecting a northwest wind direction window as proposed by Oltmans et al. (2008) is not adequate for isolating marine air." I can find no such suggestion in Oltmans et al. [2008]. In fact they use calculated backtrajectories to define the history of an air parcel, an approach that Parrish et al. dismiss (p. 13873, l. 6-9; also p. 13877, l. 2-4). While backtrajectory calculations have their limitations, the authors present no evidence that their use of local wind direction and speed is somehow superior at defining air parcel history. Their use of higher wind speeds would tend to select for the large-scale flow, and would therefore tend to mimic the results of a trajectory-based approach, but that is not to say it would be better, or even as good. That being said, however, an analysis of the trajectory-based

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approach based on the tracer data from ITCT 2K2, as they have done for their wind speed selection method, would be enlightening.

p. 13866, l. 12-16: This is a strange statement. Normally in trend analysis one deseasonalizes data in order to avoid biases introduced by missing data. An alternative is to specify that each averaging period contain a certain minimum number of valid data points for an average to be computed. It is not clear that Parrish et al. do this, and in fact their analysis rejects so much data that representativeness may be a serious issue.

Section 3.4: I am uncomfortable with an analysis that rejects so much of the data (see Table 4), based on meteorological conditions. How do we know that the remaining data are representative? The authors really should address this 8211; it would be much more convincing if they could also achieve approximately the same results without such severe data filtering.

p. 13873, l. 3-14: Despite their suggestion later in the paper (p. 13875, l. 22-24) that an offshore marine site will be needed to "fully characterize ozone concentrations in the MBL of the eastern North Pacific", the authors are dismissive of the Channel Islands site proposed by Oltmans et al. [2008]. They do this on the strength of their statement that "complex circulation patterns" along the coast near San Francisco are unresolved in the NCEP reanalysis. This is a surprising claim.

p. 13852, l. 14-18; also p. 13878, l. 1-6: The authors attempt to bolster their conclusions with "preliminary results" that are not presented. It seems to me inappropriate to draw conclusions based on results that are not shown, and are admittedly incomplete.

p. 13879-13880: The grand synthesis of the present trend results with the famous results from Arkona and Montsouris is peculiar, especially since it is missing the surface ozone data from about 1000 other stations in the US, 200 in Canada, 150 in Europe, etc.. I'm not sure what the point is, either. The comparison with models, as suggested in the following and final paragraph, would be much better done with the actual data, rather than the severely filtered set used here. Transport models are now capable of simulating much more complex variations than long-term trends (see, for example, Sudo, K. and H. Akimoto, Global source attribution of tropospheric ozone: Long-range transport from various source regions. J. Geophys. Res. 112: D12302, doi:10.1029/2006JD007992, 2007.) and will need real data in order to challenge them.

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