

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 #2

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The fundamental question of this type of studies is how to remove the effects of continental influence. The authors criticize Oltmans et al., (2008) for their use of long-range trajectories. While this criticism could be valid under some conditions, the authors approach to use local winds has its own limitations: the NW wind direction does not guarantee that an air parcel is not coming from the continental US. Perhaps possible correlation between long-range trajectories and local wind direction should be studied first.

They authors emphasize the similarity between trends at the Olympic N.P. site and the three other stations. But Figures 5 and 8 show that it is not the case. Figure

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8 demonstrates that the trends for NW winds >2 m/s are the same as trends for all winds. This suggests that the ozone concentration is also increasing for low wind speed (<2 m/s). In contrast, Figure 5a shows that ozone concentration for low wind speed at Trinidad Head in the 2000s is the same as at Redwood and Point Reyes in the 1990s. Therefore there should be no long-term trend in ozone concentration for low wind speed if the data from these 3 sites are combined into one data set. It also means that by lowering the wind threshold value, one can make the trend value lower for these three sites. So the choice for the threshold value is important, and hence it should be much better justified.

My last comment is related to the statistical approach. As reviewer #3 noted, the authors analyzed a very small subset of Olympic N.P. data (from 2 to 8%). It makes more sense to use a larger set of data, but add the wind speed to the statistical model. In that case it would be possible to evaluate the significance of the wind speed influence on the estimated trends.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13847, 2008.

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