Atmos. Chem. Phys. Discuss., 11, C2348–C2350, 2011
www.atmos-chem-phys-discuss.net/11/C2348/2011/


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Observations of the temperature dependent response of ozone to $NO_x$ reductions in the Sacramento, CA urban plume" *by* B. W. LaFranchi et al.

**Anonymous Referee #2**

Received and published: 27 April 2011

This paper describes the evolution of ozone in the Sacramento urban plume, and how this has changed in response to reductions in NOx emissions. The study uses observations from several locations downstream in the plume to relate photochemical ozone production to temperature and NOy (NOx). This observationally-based framework is shown to be robust alongside a detailed photochemical plume model, and conclusions are drawn regarding biogenic VOC control vs NOx emission control on past (7 years) ozone air quality in the plume and expected future air quality violations in the region. The topic is of relevance to ACP, and worthy of publication. The paper is very well written and the analysis is carefully designed and described. I recommend that it is published in ACP, once a number of specific comments have been addressed. These



are described below.

My only concern relates to whether the authors have sufficiently demonstrated that factors controlling ozone evolution in the plume that may be related to temperature, aside from biogenic emissions, can be neglected or considered unimportant. While the photochemical modelling part of the analysis goes a long way to reassuring the reader that the assumptions made are valid (since the modeled ozone changes shown in Fig. 3 have very similar relationships to those observed), the authors could improve the description of their analysis by addressing explicitly some of these issues.

Specifically:

- Is there any evidence to suggest changes in f(NO2) (i.e. fraction of NOx emitted as NO2) from vehicle emissions in California over the study period, due to e.g. changes in the vehicle fleet? Would such changes be expected to be reflected in your diagnosed P(Ox) from observations?

- The authors state that deposition losses for Ox are likely an order of magnitude slower compared with photochemical production. Some information on the assumptions used to derive this estimate would be helpful. A related question concerns the temperature dependence of dry deposition to vegetation. Can the impacts of high temperatures / drought on vegetative uptake of ozone, NO2, PAN etc be neglected when examining differences in P(Ox) between temperature regimes?

- What is the role of temperature-dependence of NOy chemistry in controlling relationships between temperature and diagnosed P(Ox)? e.g. the formation of PAN is very temperature sensitive. The authors argue that the chief VOC source in the region is biogenic VOCs, which are efficient in producing organic nitrates. Is there some trade-off therefore between the increasing BVOC source and PAN precursor source, but decreasing PAN stability with increasing temperature? If this type of effect is significant, the picture is more complex than temperature control on P(Ox) being driven by only changes in the biogenic VOC emission source. Perhaps a brief model experiment

could be used to test this.

Other comments:

- Equation 6 appears to be missing?

- Page 6276, Line 19: 'but still quite strong'. Please reword this, or be quantitative about the sensitivity (e.g. slope of relationship).

- Fig. 4 and associated discussion. The authors should better describe and justify the relationship between temperature and VOC reactivity and its applicability to this region / plume.

Typographical corrections

P6279, Line 7: 'lead' should be 'led'.

Fig. 4 caption: First sentence 'Temperature' shouldn't have capital letter.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6259, 2011.