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Interactive comment on “Theoretical implication of reversals of the ozone weekend effect systematically observed in Japan” by A. Kannari and T. Ohara

Anonymous Referee #2

Received and published: 3 December 2009

This paper proposes a theoretical explanation of the spatial and temporal reversals of the ozone weekend effect (OWE). Beyond the theory based on their advection-reaction model, the objective of the paper is then to discuss the usefulness of the observation of reversals of the OWE for determination of ozone formation regimes, as stated in the introduction. This is quite vague and it is finally not very well highlighted.

In the first part of the paper (section 2), authors are presenting the large Japanese data set and propose a detailed statistical analysis of the observations of such reversals of the OWE. This part is interesting and obviously deserves publication. In the second part (section 3) they present the theoretical explanation based on the advection-

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reaction model. This is based on previous publications from the same authors (Kannari, 200a,b and Kannari and Ohara, 2009) which are available in Japanese only. It is then very difficult to really assess the additional new information included in this paper. Finally section 4 is dedicated to the discussion. This latter part is quite deceiving and prevent the publication of the paper in its current version. Besides, the paper is way too long, not easy to read, and several figures and tables are not straightforward to understand (Fig 10 is the worse one).

General comments :

Therefore, I would recommend publication only after major revisions as those proposed by referee #1, have been addressed. Additionally, I would suggest to focus more on the data analysis and further highlight these new findings. Section 4 should be entirely modified to further clarify and discuss this phenomenon in the light of emission control policies. For example, it would have been appreciated to have a kind of “validation” of the theoretical conclusions. Besides the good and valuable statistical analysis, I wonder if a few case studies could be extracted and further detailed to compare model and observations. For example, having figures 8 and 9 also based on measurements would be particularly interesting. At the end, Table 4 is giving nice informations. Such results should be further detailed and highlighted (by commenting them earlier maybe). Equivalent summary for Osaka-Kyoto would give further confidence in the conclusions.

Minor comments :

Is figure 5 absolutely necessary ? The two parts of the figure are identical and the message behind it is not surprising enough to be shown. The relative sentence in the text page 12933 should be sufficient.

Table 3 is described before table 1 and 2.

CBIV is not defined.

page 12941 : It would be nice if authors could clearly answer the question “Is it the

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case indeed ?” after the sentences lines 9-10 and 15-16.

page 12942 : If the last paragraph lines 10-14 is so important, it should be written in the conclusion (section 5).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 12927, 2009.

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