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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The paper proposes an interpretation of the reversals in the "ozone weekend effect" (called here OWE) depending on distance from city centre, meteorology and VOC reactivity. It proposes an analysis of a large measurement dataset as well as a model restitution of this phenomenon. In terms of understanding of ozone formation, but also as a preliminary step for the establishement of emission control policies, this constitutes a relevant issue for atmospheric chemistry, and for this journal.

The observation of the OWE (change in the mean ozone value between sunday and weekdays) and its reversal (the sunday/weekday ratio above or below 1) are deeply investigated here. In particular, the osbervation of the weekend effect versus the ozone

percentile rank is very interesting and brings a quantitative view of the importance of this effect and its variability around the value of 1 close to large urban areas. This is why publication is recommended. Then, this phenomenon is discussed with an advection reaction model, first by the mean of isopleth diagrams, then by the lagrangian simulation of the evolution of air masses leaving the city at different times. In this part, it is much less clear what is the new information brought by this study to the atmospheric chemistry and pollution research field. Many things should be mentioned, cleared or detailed. Also, the text is not always easy to read. This is why major revisions are recommended. The following remarks are presented here so as to guide the authors to enhance their paper in view of publication.

At the end of part2, it is mentioned that many works have been conducted on the spatial reversal of the OWE, and mainly its dependance on meteorology. The authors should make clear here what is their contribution then : an application to a specific area? a first new quantification of this phenomenon (distance to city centre, intensity of the reversal...)? A more detailed investigation of this phenomenon? New links to ozone percentiles? In this version of the manuscript, it is not sure whether this study brings or not fundamental elements to the understanding of this phenomenon. And the structure of the paper becomes confuse, as we don't know what is important in the results.

The presentation of the isopleth diagram and the visualization they provide of the reversal in the OWE is too long. Isopleth diagrams are well known and the authors should inferr more rapidly what kind of new quantitative information these diagrams bring to the understanding of pollution events in this (or in such an) area. Only qualitative information is given here, on the fact that a different reduction in VOC and NOx can bring "point B" in a NOx-limited regime. This is somehow expected and new information would be to quantify this phenomenon in terms of intensity, location of the reversal, or to interpret it in terms of geographical ozone control by emissions around the urban area.

Part 3.4 brings quantitative information on the link between percentile of ozone and distance of the reversal. However, this part is a little confuse (which sentences refer to model, to measurements...?) and the tables and figures are not fully described, the values and results from the tables and figures are not discussed, and only final conclusions - thus, difficult to follow - are drawn. In particular, the complex Figure 10 should be described before it is interpretated.

The same remarks can be made for part 4. A long time is spent in the explanation of the ozone formation regimes which is well-known, and could be shortened, especially because only general ideas about regimes and only few examples are presented (case ot ETH, FORM...) and no general tendency is presented and discussed, except at the end of 4.1. In this last part, many values are given, but the only conclusion is that many parameters (VOC reactivity, solar intensity...) play on the chemical regime, which is too general. In the same way, the conclusion of Part 4.2 could be discussed within the scope of effective emission control, and not only general comments should be made ("release time (...) is earlier in the day for more remote points compared with points closer to the source"). Especially because this part is untitled "Applicability to the real world".

Part 4.3 finally brings some interesting quantitative elements, as the mean distance from the source of the regime transition depending on locations. But the mention that "the above inference is made for specified meteorological conditions and a specified initial VOC/NOx ratio" prevents from using this information. Can authors provide any idea of the representativity of this result? If it is meaningless, then the paragraph is meaningless.

If the authors can make clearer their contribution and highlight their results in the frame of ozone formation and emission control, but also better support the representativity of the model studies and results, then the work should be published. Before, major revisions are proposed.

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