Atmos. Chem. Phys. Discuss., 12, C1332–C1335, 2012
www.atmos-chem-phys-discuss.net/12/C1332/2012/

Atmospheric
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Discussions

Interactive comment on "OH reactivity measurements in a coastal location in Southwestern Spain during DOMINO" *by* V. Sinha et al.

Anonymous Referee #2

Received and published: 6 April 2012

The paper by Sinha et al. presents results of OH reactivity during a 3-weeks campaign in Spain. A careful analysis of the OH reactivity according to the air mass origins is performed. The limited number of VOC measured during the campaign does not allow the authors to compare the total OH reactivity against the sum of OH reactants reactivity. For this reason, Sinha et al. have oriented their discussion on ozone production regime. Overall, the paper is clearly written and presented.It brings a valuable contribution to the understanding of the photochemical regimes observed during the DOMINO campaign and suggests a new way to use the OH reactivity parameter. Therefore, at the end, it will deserve to be published in ACP. Nevertheless, I have some majors concerns with several sections which are not very informative or incomplete (introduction, diurnal cycle, comparison of two methods to distinguish ozone production regimes...see detailed comments below) and I would therefore recommend major revisions of the paper before publication in ACP.

Main comments

The introduction gives a good overview of the OH reactivity measurements in the world but nothing on previous measurements of pollutants in southern Spain and on the rationale of the campaign, what were the main objectives? As a lot of important parameters for OH reactivity (CO, VOC. . .) were not measured, what was the objective for measuring OH reactivity during this campaign? Except a few sentences (regarding the SO2 specific source), there is almost no reference to the other papers from the DOMINO special section and clearly more links should be made.

The experimental section of OH reactivity should be a bit more precise. The authors refer to previous papers, which is convenient for the method itself but more details about the measurements during this specific campaign are needed. For example, it is stated that “frequent tests with air samples of known OH reactivity were performed and excellent accountability for OH reactivity of the test samples was obtained”. Excellent is a nice word but rather vague. What was the range of reactivity investigated? What was the slope of the measured reactivity versus theoretical reactivity? What was the reproducibility of the measurements?...Regarding the role of relative humidity, how was the variation of the humidity in the air taken into account? How was determined the 5% attributed to the “error in matching the humidity of the ambient air exactly”?

The section presenting and discussing results is divided in three parts, a first section on general trends of OH reactivity, a second one on diurnal variations and finally a section on the ozone chemical regimes. The first section introduces the results of OH reactivity through figure 2. Some co-variations with formaldehyde are noted but not discussed. Why OH reactivity and HCHO co-variate on these specific periods and not on the others? In addition there seems to be some events with higher OH reactivity (for example from 24 to 27/11) but there are not commented. Are they only due to air mass origin changes? Rather than discussing further these influences, the authors prefer to examine diurnal variations of the OH reactivity and then of radical species, formaldehyde and sulfur dioxide. This whole section on diurnal profile is not very informative. Except the fact that usually all compounds follow the scheme that continental sector values > Huelva sector values > ocean sector values, there is no other conclusion. I'm not sure this is the best way to examine dependency of air mass origins and I'm wondering why the authors have chosen these specific compounds to compare with? (why not using in addition or instead the few VOCs which were measured during the campaign?). The following sections are focused on VOC and NOx regimes and these species could be presented as well in the first section. As a conclusion for this section, it could be either shortened if no additional information is available or discussed in more details if more results are presented.

The approach to determine the ozone production regime based on OH reactivity measurements is original and interesting as it gives a direct accessibility on the whole "reactive compounds loading" without knowing the contribution from individual VOCs. Nevertheless, I do not see the point to compare the two methods as at the end it brings a comparison between (almost) identical ratios (once with a threshold at 5 and another at 5.5).

Finally the last part compares two complementary methods for estimating the ozone production potential. The second method (based on measured HO2, RO2, NO) seems relevant here, although presenting a higher limit. It is questionable if the first method can be used for this dataset as it is "valid only for air masses where NOx is not the limiting reactant". In the previous section, the authors have shown that all continental air masses were NOx limited and therefore, if it is decided to keep the simulation of the ozone production with this method in Figure 6, the corresponding discussion and conclusion should be more justified (especially for continental air masses).

Minor comments

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Interactive Discussion

Discussion Paper

The current title is a bit vague and it is suggested to make it more precise.

Technical comments

On page 11, Song et al. 2011 and 2012 but it seems to be the same paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4979, 2012.

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Interactive Discussion

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