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Interactive comment on “Ozone pollution over the Arabian Gulf – role of meteorological conditions” by L. Smoydzin et al.

Anonymous Referee #2

Received and published: 8 May 2012

This paper presents results from WRF-Chem simulations of 3 five-day episodes over the Arabian Gulf, showing high ozone concentrations. These are discussed in terms of the Shamal and in terms of land-sea breezes. Additional simulations are used to evaluate the impact of reducing NO_x, VOC, or both on ozone concentrations along the coast.

Major Comments:

Unfortunately there are no comparisons with any measurements, except for the radionsonde profiles. These are shown for 6 days along with WRF results, but the model performance is not discussed.

The paper over-extends in drawing conclusions. For example the abstract states “A

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general finding from our simulations is that extreme pollution events . . . can occur regularly over the Arabian Gulf.” There are 15 days of simulations, 5 per year for 2009, 2010 and 2011, and no evaluations with data whatsoever. It therefore seems that one cannot make such a claim based on the results presented.

The episodes are chosen to be 14-18 July for each of 3 years, and are then compared with each other. There is no reason to think that these may be comparable with each other: 3 similar episodes should have been selected for that. On the other hand, if instead of matching episodes a representative sample was sought, then many more days would have had to have been selected. As it is, the choice of episodes seems both limited and arbitrary.

A mechanism similar to Houston is postulated whereby a polluted plume is vented out to sea and then returns in the afternoon. However, this is neither clearly demonstrated nor illustrated. There is no indication of how frequent this mechanism might be. Please see RM Banta et al., “A bad air day in Houston,” BAMS 2005.

There is very minimal discussion of emission inventories. This should be dealt with in much greater detail, including emission maps of NO_x, and discussion of speciation profiles of VOC’s – presumably there are very different profiles for example between the coastal area sources and the oil producing industrial sources off-shore? Fig 5a shows the VOC emissions but in a manner that makes it very difficult to figure out what is going on and how the major source types compare. Pg. 6346, ln 17-22 touch on this issue as an after thought, but it should be given greater consideration up-front.

Fig. 7 is only mentioned in passing. It should be discussed in more detail if it is to be included at all. The discussion of NO_x vs. VOC limited regimes is too short: shouldn’t there be a clearly distinction of which areas are NO_x-limited and which are VOC, and how this relates to section 4.3.

Section 4.3 is too short and not rigorous enough. The discussion of the “A” cases is done in 3.5 lines, and Table 1 incorrectly states that these are “increased” emissions. If

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this were to be retained, it would be nice to see a table with results for different domains and quantitative increases / decreases in ozone. As it is, the fact that there are so few simulations and no evaluations of the results means that it is premature to do scenario analysis at this time.

While there may not be much data to evaluate an ozone model, there is much more data available for meteorology. This study could have used this in combination with a greater number of WRF simulations. Results could then have been presented showing frequency of occurrence of the Shamal and of sea-breezes. This would be on a firm foundation of data. The link to ozone would then be a more tentative section with a view to recommending necessary measurements.

Minor Comments:

There are several instances of general and vague comments that could be made more precise. For example:

Pg 6332, ln 24: “NO_x concentrations . . . are exceptionally high.” Compared with what?

Pg 6333, ln 9: “humidity is often high”

Pg 6343, ln 8: “for the year 2011” (vs. 5 days of it.)

Pg 6333, ln 5: Would the long range transport of precursor species be an important factor to explore here (and in the discussion when this point is reiterated.)

It would be helpful to see a map with WRF domains and locations discussed in the text. Shouldn't the shallow inversion merit a figure of its own?

The discussion about high venting in Riyadh could be interesting if supported by some evidence and more discussion, but as it is it should be left out rather than being squeezed in as an aside.

Pg. 6347, line 8-12: This seems like it is another example of over-reach – I'm not sure I saw how this was supported by the results in the paper.

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Fig. 1 is hard to see. There is no mention of the vectors in the caption, and no scale for them. Especially for the bottom panels, it would make sense to display fewer vectors.

Fig. 3: Is maximum ozone at 12:00? Maybe these should be summarized, classified or averaged somehow?

Fig. 8: A more rigorous study could easily have looked at 1-3 months for each of the three years and then shown average results. These could then have been discussed in greater length in the text.

There are instances of run-on sentences, eg. pg 6332, ln 10-13.

[Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6331, 2012.](#)

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