

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

Anonymous Referee #1

Received and published: 2 May 2012

This paper describes some (boundary layer mixing, sea-breeze, synoptic, local wind system) but not all of the meteorological processes that affect ozone concentrations in the vicinity of the Arabian Gulf using the WRF-Chem model, and then uses the model to examine the sensitivity of ozone to VOC and NO_x emissions. While the subject matter of the paper is suitable for ACP, a number of issues affecting the conclusions that need to be addressed by the authors before the paper is published.

General Comments:

Most importantly, all of the conclusions drawn in the study are based solely on the model; only one figure compares observed and simulated quantities. A model is a useful tool to address emission control scenarios, but based cases need to be evaluated against observations to show that the model does an adequate job in simulating observed

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ozone and ozone precursors given the available emission inventory. While I understand that it is likely that less in situ data is available for this region than in other parts of the world (but I have a very hard time believing there is absolutely no data) to evaluate the model's performance, the authors need to state up-front and in the summary why virtually no data is used in this study. The authors also need to occasionally remind the reader throughout the text that their conclusions are being drawn from a model only, so that the reader understands that there is an element of uncertainty in these results. A preferable alternative is for the authors to find some alternative data to introduce into the paper. For example, there are satellite measurements that could be used (e.g. tropospheric NO₂ columns). A more thorough discussion regarding the performance of the parameterizations used in WRF from the literature needs to be discussed.

While I appreciate the authors using a fully-coupled model that simulates aerosols and their feedback processes, there does not seem to be a reason for running the aerosol model at all as described in the text. The authors could have simply run the model with the CBMZ mechanism by itself. It is possible that aerosols affect the local meteorology and photolysis rates, especially during high loading associated with dust events. Was dust not important for these cases?

While the paper does describe important meteorological processes, some important processes are omitted. Most importantly clouds. Were mostly clear skies observed during the cases presented? If not, how well did the model predict cloudiness and I assume then that the cloudiness affects photolysis rates and consequently ozone production.

Specific Comments:

Page 6332, line 7: Authors use nmol/mol here and elsewhere, but the authors should use ppbV which is more commonly used to express ozone concentrations.

Page 6332, line 9: Define “regularly”. This phrase is rather vague and means different things to different people. Similarly, a few lines later on line 13 “often” is used (which is

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also vague) to define how sea-breezes transport pollution in the afternoon. It is hard to gauge the importance of these events given the lack of specificity.

Page 6333, line 15: Change “elevated levels” to “higher altitudes”.

Page 6334, line 7: For “difficult to determine”, should the reader assume that there is a lack of data on the depth? Or is it just difficult to determine from the measurements? Please discuss.

Page 6334, line 25: The authors have picked one of many locations where the stable marine boundary layer has led to high ozone concentrations. Would be useful to include a few more instances to stress its importance globally.

Page 6335, lines 5-6: What is the basis for this statement? Is it based on modeling results, analysis of meteorological conditions, direct observations of ozone, or a combination?

Page 6337, lines 8-9: The authors mention that direct and indirect effects are used, but never discuss them again in the manuscript. Are these to effects important for the present simulation? Was there significant cloudiness and sufficient aerosols present to have an impact on clouds that feeds back into the ozone calculations? Since the paper’s focus is on meteorological processes, the potential feedbacks they mention are one of those processes. It is mentioned that dust outbreaks from the Shamals often occur – so are feedbacks associated with dust important for the simulations?

Page 6337, lines 19-20: the source of the initial and boundary conditions for the gas phase and aerosol species is mentioned, but what about for meteorology? This needs to be defined.

Page 6337, line 25: A list of trace gas emission species used is needed. Also a table that lists the total speciated emissions per year from the inventory over the region of interest (vicinity of the Gulf like in Fig. 5) would be useful, especially for the sensitivity simulations. For an air quality application it would also be useful to discuss how partic-

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ulate emissions from the inventory are used for the aerosol model; however, since no discussion is given for the predicted aerosol species perhaps that is not relevant.

Page 6338, line 2: The vertical grid spacing, especially near the surface, needs to be defined. How deep is the lowest grid cell, which most of the discussion focuses on?

Page 6338, line 2: Earlier it is mentioned that a 0.1 degree emission inventory is used, but the 4 km grid is smaller than 0.1 degree. So on the inner grid, increased detail will likely result from resolving the meteorology rather than the emissions? Is that correct? Some discussion is warranted on this point.

Page 6338, lines 12 – 18: It would be useful to have some observations of boundary layer depth to evaluate WRF.

Page 6334, line 16: The text should be changed to indicate that they are referring to simulated boundary layer depths. While the model results seem reasonable, there are no observations presented to verify the model.

Page 6334, lines 18 – 26: Again the statements regarding the sea-breeze are based on model results, but the text does not indicate that.

Section 3: Need to include how photolysis rates are treated. Are the photolysis rates influenced by aerosols? Is dust important enough that it significantly affected ozone production?

Page 6346, lines 22-23: Measurements are mentioned for the first time at the end of the paper. The authors should note there are no measurements earlier in the manuscript somewhere in the introductory material.

Page 6346, line 20-21: The authors mention the speciation of VOCs, but as stated earlier they should include a list of the species emitted for the present simulation.

Page 6347, line 1: Change “appear to occur regularly” to “appear to occur regularly in the model simulations.” Again the authors need to remind the reader where these

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findings come from.

Figure 1: Each case should be labeled to indicate conditions favorable or unfavorable for the Shamal.

Figure 2: It is difficult to differentiate between the observed and simulated profiles. Using color would help.

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