

Interactive comment on “Modeling air pollution in Lebanon: evaluation at a suburban site in Beirut” by A. Waked et al.

Anonymous Referee #2

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The paper by Waked et al. describes the design and application of a regional model centered over Lebanon, which also captures the greater area of the Middle East, Turkey, Egypt, and the eastern Mediterranean. Due to lack of sufficient measurement sites, the model evaluation only occurs at a suburban site in Beirut, but an attempt of comparing with other regional models of similar design is being made. Although the manuscript is in general very well written, there are two major points that need to be addressed before considering publication to ACP.

Major comments

In parts of the manuscript it is mentioned that the outer domain of the regional model, D3, is not being calculated online. For example, in page 29577 line 25 is mentioned that the boundary conditions of the D2 domain are fixed. This makes the D3 domain

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useless. Further, the D2 domain is then very small and is heavily impacted by its boundary conditions, as mentioned in the manuscript. Although I do understand the authors' argument (page 29584, top) that there are no emission inventories of that fine resolution over Middle East, dropping completely the D3 domain is not a solution. If the D3 domain is not being used, it should be completely eliminated from the discussion. If it is used, then the text should be greatly modified, in order to clarify how it is used. As a last remark on that, the D1 domain is extremely close to the boundary conditions; since there is no anthropogenic emissions-related issue with the ocean, the D2 domain should be extended westbound.

Another major issue is that the authors modified the O₃ boundary conditions towards the direction that improves the comparison with measurements (page 29583, middle). O₃ levels is closely linked with the NO_x and VOC levels; modifying O₃ alone so close to the measurements site is not any kind of improvement, it is only a trick. I do understand that one can perform a sensitivity experiment like this, but the authors use that simulation as their base case used throughout the manuscript (page 29584, lines 18-19). Finally, the numbers used to justify this decision (page 29584, middle) do not agree with the numbers presented in Table 3. If the authors insist in using simulation A4 as their standard, they will have to present more convincing evidence on their choice. In addition, the change on the boundary conditions has to be stated clearly in the manuscript, especially when stating that the model performs well (e.g. page 29588, lines 8-9).

Minor comments

Section 2.3, second paragraph: Please be more specific on what the physics options are. For example, what are their degrees of freedom, how much the different selection options affect the results, whether there are any recommended values, if there are why deviate from them, etc. Which of these options were finally selected for the M3 simulation (section 3.5)?

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Page 29577, line 22: what does the value of 5 mean? Please say more about it and how much its selection impacts the results.

Page 29578, line 4: "eliminate the effect of initial conditions". What effect?

Section 3.1: I am really surprised that a whole country has no meteorological data available to be used for a basic model evaluation. There is no meteorological service in Lebanon?

Section 3.3: Since models are supposed to diverge, does this mean that 12h spinup is not enough? How does the correlation of a long run look like at the first half vs. the second half of the simulated period? It appears likely that the problem mentioned at the end of the section can be fixed by a longer spinup.

Page 29583, lines 13-14: The VOCs mentioned here are only 4, how about the others? Is there any evidence that these 4 are representative for the whole mixture of VOCs over Lebanon?

Section 4.1, last few lines: Please expand the discussion on the diurnal variation of O₃. Also discuss about the possible explanation of the presence of the second peak on the 2nd, 6th and 8th of July.

Page 29587, line 5: How is this reconstruction of PM_{2.5} is being made?

Page 29588, line 13: The future work should also focus on the boundary conditions tuning.

Technical corrections

Section 5 is not only conclusions; it contains a great deal of future work. Consider changing the title of the section, or splitting it in two.

Table 3, column 1 should mention a bit of the simulation details, e.g. base, NO_x/2, VOC/2, O₃/2.

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The USJ site should be marked on the map on Fig. 4.

The table of the supplement is very low quality; it appears to be a direct scan from a handbook, or something similar.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29571, 2012.

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