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Atmospheric Chemistry and Physics Discussions

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

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Author's response to referees comments on manuscript acp-2012-686

We thank the referees for their constructive and helpful suggestions which have improved the quality of this work; we have provided our responses to the reviewers' comments and we have done our best to address each of the points as detailed below.

Note: All reviewer comments in italics and marked with apostrophes at the begining and the end of each comments; all responses by the authors in normal font

Anonymous Referee 1

'1. The title should contain that has been studied only a summer situation. This should

be corrected.'

Response: The title of the manuscript "Modeling air pollution in Lebanon : evaluation at a suburban site in Beirut" was changed and replaced by "Modeling air pollution in Lebanon : evaluation at a suburban site in Beirut for July 2011" as recommended by the reviewer.

'2. The weather situation should be explained in more detail.'

Response: The weather situation is now explained in greater detail, including a brief description of general topography, regional climate and weather characteristics at the site during the measurement period.

'3. The comparison of results is made, in a meaningful way, on the work of Matthias et al 2008, referred essentially as described in the manuscript is the area North Sea coastal region. That has characteristics for climate and topography/terrain very different of the coast under study in the East Mediterranean, would be much more convenient comparison with other studies done on the coast of Israel, and also the contrast to the eastern coast of the Iberian peninsula.'

Response: As recommended by the reviewer, the comparison of the model performance for NO2, CO and O3 was made to another study done in the Western Mediterranean basin over the Iberian Peninsula (Jiménez-Guerraro et al., 2008). The levels of O3 and NO2 are comparable and the MFB values for NO2, O3 and CO are also of the same order of magnitude. Although there have been measurement campaigns conducted in Israel, we have not identified any air quality modeling studies.

'4. It would also be desirable, it would have been an analysis of the model results in vertical, especially take in consideration the recirculation mesoscale processes due to land-sea breezes and land-sea. They have been widely studied in the Mediterranean coasts, and its association with air pollution probemas.'

Response: We agree with the reviewer that it is desirable to perform an evaluation of

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the model results in the vertical direction and to investigate meteorological processes such as land-sea breeze, mountain-valley, winds and the urban heat island. However, measurements for the PBL height, meteorology and pollutant concentrations aloft are not available. Therefore, this kind of evaluation could not be done. In the manuscript, we highlighted this point in the prospects and recommendations are now made regarding the need for an evaluation of the model results in vertical when measurements become available.

'5. The additional material is not necessary, since it is only the numerical definition of statistical indicators used, and are abundantly described in the literature.'

Response: The additional material has been removed from the manuscript.

Minor comments

'1 Introduction The Middle East region is characterized by a diverse landscape including elevated terrain, semi-arid and Saharan deserts OUTBREAKS, sea shores and vast plains.'

Response: These sentences have been rewritten.

Anonymous Referee 2 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 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

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issue with the ocean, the D2 domain should be extended westbound.'

Response: We have now clarified in the manuscript that the domain D3 was not used in the air quality simulations because an emission inventory for the Middle East region is not yet available. However, this domain was used in the meteorological modeling in order to reduce the influence of the boundary conditions used for the meteorology, which are available with a spatial step of 100 x 100 km. Regarding the extension of the D2 domain west bound, we do not think that this extension would have any significant effect on the modeled results because westbound of the domain, there are no major anthropogenic or biogenic emission sources except for some sea salt emissions and marine traffic emissions.

'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 NOx 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).'

Response: Simulation A4, where O3 boundary conditions were halved, was indeed used as the best simulation and, therefore, we presented the model performance evaluation for that simulation. We agree that this presentation is misleading and we now present the model performance for both simulation A1 (initial boundary conditions) and simulation A4 (O3 boundary conditions halved) without referring to simulation A4 as the 'best' simulation. The numbers used in past to justify this decision (page 29584,

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middle) are different from the numbers presented in Table 3 because they are from different locations in the modeling domain D2. The results presented in Table 3 and discussed at the beginning of the section 4.1 (p29583) correspond to the July 2011 field campaign, whereas the results presented in the middle of section 4.1 (p29584) were obtained during a measurement campaign in the summer of 2004 in a location in the city of Beirut very close to the sea (33.88 N, 35.48 E). The location of the urban site of the 2004 measurement campaign has been added in the text.

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)?'

Response: Section 2.3, second paragraph The physics options used in this study, which represent cloud microphysics, radiative transfer and transfer of heat, moisture, and momentum between the surface and the atmosphere, have been described in the literature (Skamarock et al., 2008; Borge et al., 2008; Carvalho et al., 2012). In this study, we tested various options for the Planetary Boundary Layer (PBL) scheme, because it has an important effect on surface wind fields, we also tested the urban canopy model because it affects the heat anthropogenic flux were tested. This has been clarified for other physics parameterizations, no tests were performed because it was assumed that they have less effect on the results. For the simulations presented in the manuscript, only the PBL scheme and the presence of an Urban Canopy Model (UCM) were tested. The same physics options were used for simulation M1 through M5, except for the PBL scheme and the UCM. The results of model evaluation obtained during this study were compared to other meteorological model evaluation conducted in other regions and suggest satisfactory performance.

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

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Response: Page 29577, line 22 The ratio SVOC/POA represents the ratio between semi volatile organic compounds (SVOC) and primary organic aerosols (POA). This ratio derived from measurement campaigns conducted for several emission sources (e.g., Robinson, A.L., Donahue, N.M., Shrivastava, M.K., Weitkamp, E.A., Sage, A.M., Grieshop., A.P., Lane, T.E., Pierce, J.R., and Pandis, S.N, 2007. Rethinking organic aerosols: Semi-volatile emissions and photochemical aging, Science, 315, 1259-1262). It was found that combustion of diesel emits SVOC and POA with a ratio of 5 for conditions estimated to be representative of current European emission inventories (EI-POA). This ratio was used by Couvidat et al. (2012), who found that it provides the best performance results when compared to ambient organic aerosols. However and during this study, the impact of this ratio was not tested.

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

Response: Page 29578, line 4 In air quality modeling, pre-simulations are typically conducted a few days before the starting date of interest simulations to ensure that most of the concentrations corresponding to the initial conditions are eliminated and thus have negligible influence on the modeling results.

'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?'

Response: Section 3.1 Concerning the availability of meteorological data, we identified meteorological data from another site, however, the measurements were not accurate and appeared unrealistic for wind speed (constantly less than 0.5 m/s, thereby suggesting a problem with the instrumentations). Measurements are performed at the Beirut International Airport (BIA), but we did not have the permission to access these data. Accordingly, we mentioned in the future prospects and recommendations that the model should be evaluated against measurements at many sites in future campaigns.

'Section 3.3: Since models are supposed to diverge, does this mean that 12h spinup

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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.'

Response: Section 3.3 In fact a twelve hour simulation as a spin-up was used in several earlier meteorological studies for model evaluation (Kim, 2011 ; Carvahlo et al., 2012). We believe that 12 h spin-up is sufficient and that the problem in this study is not due to the spin-up period rather than to the effect of initial conditions. In particular, initial conditions have a spatial grid spacing of 100 km, compared to our modeling domain of 1 km resolution covering an area of 30 km by 40 km. Therefore, one grid cell of these initial conditions covers the entire D2 domain. Generating these initial conditions each two days leads to biases than in a long simulation where these initial conditions are generated only once at the beginning of the simulation. There is little divergence in the meteorological simulations because they are rather well constrained by the boundary conditions. Concerning the correlation of a long run between the first half and the second half of the simulation using simulation M3 as an example, the wind speed correlation varies between 0.6 and 0.65, surface temperature correlation varies between 0.92 and 0.89, and humidity correlation varies between 0.29 and 0.31. Therefore, there is no significant variation in the correlation coefficients.

'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?'

Response: Page 29583, lines 13-14 The four mentioned VOC, these are model surrogate species which include the majority of the measured species (xylene, toluene, monosubstitued and polysubstitued aromatics, trimethylbenzene, ethyltoluene, pinene, isoprene, and sabinene). Of course, there are many VOC species, which are missing, however, VOC measurements are necessary limited to a number of species. Since the VOC measurements include anthropogenic and biogenic species, we believe that they provide useful information concerning different VOC emission sources.

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‘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.’

Response: Section 4.1 For the O3 diurnal variation, the explanation has been expanded and we now mention that the second peak observed on some days is due to fluctuations in the measurements of O3 during a wide peak period. In addition, this second peak could be related to the diurnal variation of road traffic when the vehicles emissions decrease non significantly between 10 am and 11 am (Waked et al., 2012 ; Waked and Afif, 2012) and therefore, O3 formation would decrease. Although this diurnal profile was included in the model, the difficulty of the model to reproduce wind directions accurately on some days could explain this discrepancy.

‘Page 29587, line 5: How is this reconstruction of PM2.5 is being made?’

Response: Page 29587, line 5 The formula which was used for the calculation of the PM2.5 values follows the IMPROVE protocol. It has been added in the manuscript. (PM2.5 = 1.8*OC + EC + 1.375 * sulfate + 1.29 * nitrate + 1.8* Cl + 2.2 *Al + 2.49 *Si + 1.63* Ca + 2.42 *Fe + 1.94 * Ti).

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

Response: Page 29588, line 13 We now state in the future prospects and recommendations that a model evaluation with data aloft should be addressed especially for the PBL height in order to improve the choice of the appropriate PBL scheme.

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.’

Response: Section 5 The title of the section 5 has been changed to include future prospects.

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

Response: Table 3 Simulation details in Table 3 are now mentioned.

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

Response: Figure 4 The USJ site is now marked on the map in Figure 4 (a red dot on the map) and indicated in the caption.

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

Response: Supplement The supplement was eliminated because according to reviewer 1 there is no need for this table since these statistical indicators are very well described in the literature.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C13736/2013/acpd-12-C13736-2013-supplement.pdf

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