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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future climatic drivers and their effect on PM_{10} components in Europe and the Mediterranean Sea" *by* Arineh Cholakian et al.

Anonymous Referee #1

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The paper presents a wealth of data on future climate, based on several climate simulations, emission scenarios, and boundary condition scenarios, for a domain covering Europe and the Mediterranean Sea. The data are analyzed in detail, with attention to all contributors to PM10 (PM2.5) and the impact of the different drivers. The analysis is done for the European continent, for the Mediterranean basin and two smaller subdomains covering parts of the Mediterranean area. The problem with the paper is that there is nearly too much information, and that it is difficult to remain focused on the main results. A major point of criticism is that the Mediterranean domain, and more so for the subdomains, are far smaller than the European domain. It is not completely fair to contrast them, and within Europe, different regimes could be found as well (e.g.

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EUROCORDEX regions). Also , the Mediterranean are is not even fully covered by the model and significantly affected by the boundary conditions. Nevertheless, the approach as such is not wrong, if one is clear on the limitations, and I would encourage the authors to motivate the choices more clearly. Below, detailed comment is given.

Major comments

Abstract: No discrimination was made in the conclusion for the European area and for the Mediterranean area, whereas a lot of attention was given to the differences in the main text. Add a few sentences on the major differences. Dust is not an anthropogenic emission, so only long-range transport and boundary condition effects can be studied in the present set-up.

P2 38-51: Here the motivation of the choice for Europe and Mediterranean is made, but could be improved. North-eastern Europe and Mediterranean are mentioned as hotspot, but in the next sentence Europe as a whole is mentioned. The relation to Charmex is only mentioned, not made in the paper. Should this come back in the conclusions?

P3 It would be good to note explicitly that meteorological drivers are correlated, it is not possible to fully separate them. They are driven by circulation patterns. This is analyzed in the SI but could be taken into account more in the main text at several instances. It should also be mentioned here that the choice of global circulation model driving WRF has an impact on the results, for the same RCP scenario, a different GCM may give the same global temperature change but different seasonal /regional impacts.

P3 l24 Since there may be nonlinear effects, the simulation with all drivers changed at once will not be the sum of the individual scenarios with one driver changed. This notion could be added to the text.

P6 l 85 Reference to EEA is not precise enough: which document/web page?

P9 I do not understand the approach towards rain episode: unit in Fig 2 is number of hours (per year), but the number of episodes is mentioned in the text. I would call it total duration of rain if I understand your definition correctly (l 57-59). Is the threshold set to exclude drizzle? Number of rain events is something else.

P10 l68 Is there a main message from these correlations that should be mentioned here? Maybe conclusions that are used in the interpretation of the PM10 relationships to metereorological drivers?

P13 This section is difficult to follow. First relative changes due to climate change for EUR are discussed, then absolute concentrations for the Mediterranean and EUR are presented. For MED the impact of climate change is not presented, whereas this is the main subject of the paper. Transition to p14 is a surprise. Figure 8 and 9 are just mentioned without further explanation here. Why do you show them?

P14 Section 3.4: this section is quite long and repeats many well-known relations between PM10 and meteorology. Could you highlight the interesting parts (difference in distribution for sulfate) by reducing the description of the non-surprising parts? (e.g. nitrate analysis). I miss a short description of the behavior for the primary components PPM/POA/BC to start with.

P16 l36: but PBL and wind speed are highly correlated themselves, so this is no surprise.

P17 l59 I would say increase in concentration, not production, as you analyze concentration which is a results of production/chemistry, transport and deposition. How confident are you in the SOA scheme/literature, given the evolution in SOA parameterizations over the past 10 years?

P20 l90-96 think that changes in wet deposition are far more relevant than changes in RH and T for mineral dust, as it is hardly takes up water, and dust is dominated by inflow through the boundary of the model domain.

P20 l 5-9 Precipitation is difficult to model accurately, also for present-day conditions.

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The analysis does not provide an indication of rain intensity/frequency, only total number of hours and total amount, as far as I understood

P21 not only accumulated effects but also taking into account nonlinearities/compensating effect.

P25 l2 Focus: EUR and MED get more or less same attention in the paper.

P27 l 30 But also secondary aerosols are relatively sensitive to advection, as it takes time to form them, but there are more processes involved.

P28 For sea salt emissions, I expect that changes in wind speed/circulation patterns and wet deposition will dominate over changes in salinity and density. Given the still large uncertainties in sea spray emission parameterizations, I would consider the relation between temperature, sea level and salinity that is mentioned irrelevant here and leave it out. There is a large difference between sea spray emissions in the Mediterranean and in the Baltic sea due to the far lower salinity of the latter, but I would by no means expect such a drastic change in salinity for the Mediterranean. Eventually sea level rise could give an effect on total area covered by the sea. Other land use changes are also not addressed in the paper, and they may have more impact (BVOC emissions, deposition) without being mentioned in the conclusions.

Minor textual comments

P1 14 dependency on temperature/humidity

P1 22 large number of components, with different origins and different behavior with respect to . . .

P2 28-29 formulate more precisely

P2 30 positive/negative: use enhance or reduce

P2 52 possible future changes

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P3 20 composition (instead of content)

P6 l70: Land use does not change in the simulations (and leave out the last sentence)

P12 section numbers 3.4 and 3.1

P13 Figure 7 shows the PM10 concentrations and concentration changes for all different scenarios and subdomains, as well as the contributions of all the different PM10 components.

P13l 25 An interesting results is that sulfate concentrations show an increase. . .

P17 56 Our simulations are consistent with these results

P21 Typo in caption fig 10, 3rd line.

P22 l 51 climate effects were a few percent. I would not call them negligible, they are small but still visible

P24 l 77 COV not explained, this is first instance

P24 l 80. To be presented later: next paper or next section?

P27 l 32 In contrast (instead of on the contrary)

P27 l 34 emission changes show larger effects on non-dust and non-sea salt PM10 and PM2.5 components than changes in boundary conditions and climate conditions

P27 l 37 further investigation

P27 l 6 is dominated by dust

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