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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future changes in surface ozone over the Mediterranean basin in the framework of the Chemistry-Aerosol Mediterranean Experiment (ChArMEx)" *by* Nizar Jaidan et al.

Anonymous Referee #1

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MS No.: acp-2017-553 Future surface ozone over the Mediterranean Basin using the ACCMIP outputs of 11 models as predicted for 2000, 2030 and 2100 and based on four representative concentration pathways was carried out. Each of these four pathways results are compared to measure surface ozone datasets. This study serves as an important contribution to the ChArMEx Project and as such, deserves its publication in ACP. However, few of the interpretations given by the authors explaining the discrepancies obtained between the simulated concentrations as compared to the present and future reference periods are expressed in a too hypothetical manner and ought to be

based on a more scientific basis based on further references of similar studies. Consequently, I support its publication after treating the following comments. Major comments: 1) L205-215: The results obtained by NMB and CVR models as regarded to the model's overestimate of surface O3 are clear. However, some more explanations are necessary (e.g., sensitivity analyses to VOCs emissions) 2) L195: Please discuss the possible reasons for the model's overestimating surface O3 3) L235: Please discuss the possible reasons for the best performance obtained by CMAM while comparing surface O3 over the Mediterranean Basin and ACCMIP simulations. The rational for referring to Parrish et al. (2014) in this context is not clear. 4) L298: I don't see the point in discussing the ACCMIP inter-model variability of total VOC species since, as the authors mentioned, biogenic emissions are not specified and the VOC module differs from one model to another. The contribution of biogenic VOC (BVOC) emissions to regional ozone formation is significant. Over most of world's regions, the leading BVOC species, isoprene, reacts rapidly with hydroxyl radicals to form ozone (Pierce et al., 1998). The high ratio of BVOC to total VOC emission makes BVOC emissions a significant contributor to regional ozone concentrations. Estimations of BVOC emissions are subject to large uncertainty and are not well estimated by proxy concentrations (i.e. formaldehyde) observed from space. Hence, estimates of isoprene emissions by different biogenic emission models can vary more than a factor of two (Shim et al., 2005). 5) L182-188: The explanations for the high SD values over the Ligurian Sea and around Marseille are not clear. For example, do Vautard et al. (2007) give some evidences indicating that the stagnating conditions are the reason? What about the high SDs over the south-eastern parts of the Mediterranean Basin? 6) L160: I wonder, whether is not it obvious that a comparison of models to surface O3 averages for such long periods (8 to 16 years and for 1990-2010) is expected to give relative elevated correlations?

References to Major comments: T. Pierce, C. Geron, L. Bender, R. Dennis, G. Tonnesen, A. Guenther: Influence of increased isoprene emissions on regional ozone modeling, J. Geophys. Res. Atmos., 103 (D19) (1998), pp. 25611–25629. C. Shim, Y. Wang, Y. Choi, P.I. Palmer, D.S. Abbot, K. Chance: Constrain-

ing global isoprene emissions with Global Ozone Monitoring Experiment (GOME) formaldehyde column measurements, J. Geophys. Res. Atmos., 110 (D24) (2005) https://doi.org/10.1029/2004JD005629.

Minor comments: 1) L153-154: The maximum observed concentration recorded in July as compared to the ACCMP models August average for 1990-2010 contradicts the statement made by the authors in line 154. 2) L175: Please replace "Northern Africa" by south-east Mediterranean Basin. 3) L377: Please supply further explanations for the term "highly dynamical". 4) L389: The sentence: "..This shows that a positive correlation between changes in CH4 and surface O3 can exist" is not clear, please elaborate.

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