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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of summer O_3 in the Madrid air basin with the LOTOS-EUROS chemical transport model" *by* Miguel Escudero et al.

Anonymous Referee #2

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The article presents a detailed case study of how a state-of-the-art chemistry-transport model (Lotos-Euros) captures ozone air pollution episode, making use of an intense field campaign previously published in ACP. The presentation quality of the article is outstanding and the scientific discussions, both in terms of atmospheric processes and modelling challenges, is of very high level. Therefore, I recommend publication after the following points are addressed.

General Comments:

Over the past few years, the majority of model evaluation papers have been focused on long term simulations (at least annual), for statistical robustness considerations. But

this tendency comes at a cost: that few authors analyzed in details air pollution episode case studies. In that context, the article from Escudero et al. is a welcome initiative.

My only major comment would be that I somehow disagree with the authors that model evaluation is only useful to build confidence in the tools: it is also essential to guide their development. The detailed analysis presented here could thus be more conclusive in pointing specific issues that would deserve higher priority in future model development, i.e. being more specific that pointing to "future research" (as stated p26). Taking the example of vertical resolution, the two configurations tested here are somewhat extreme: going from 5 to 70 layers, which is presumably not realistic in long term simulations used in policy support or air quality forecasting. It would have been useful to know to what extent the 5 layer model captures the episode processes discussed in 3.2, and how a tradeoff could be found.

Specific comments:

P2L12 : why using plural here? only O3 is a secondary air pollutant

P5L14: the air pollution regimes (REC/SAD/NAD) should be introduced here and related to synoptic meteorological situations.

P6L10: indicate the range explored in terms of O3 dry deposition velocities. Was this impact assessed on the basis of free-tropospheric total ozone burden as in Stevenson et al. or rather surface ozone?

P7L2: what is the reference year for emissions used?

P9L5: the worsening of correlations when increasing resolution has been documented as "double penalty" in the field of meteorological forecast. It would be worth discussing in more detail the issue here

P12L2: it is indeed frustrating that NO2 is not included in the detailed validation. It would also be interesting to see some basing meteorological validation, not clear why WRF outperforms IFS and there could be compensation of errors

P16L25: how do you explain the local minima of O_3 around 4km agl?

P18, Fig 7: what is driving the sharp horizontal convergence of NO_2 between 12 and 18UT?

P20L15-20: this section deserves further discussion in light of Querol et al. 2018 (Section 4), where they analyze the likelihood of impact at the ground of the STE event in relation with diurnal PBL variability.

P21L9: The model still produces a peak in the mid/late afternoon and the shift of the O_3 daily maxima is biased by the spatial distribution of stations, which miss the plume that has been advected NW at 18UT.

Figure 6, 8, 10: those figures are very nice and comprehensive. The only missing information is modelled O_3 time series. Although model and observations are compared in the in 3.1.1 and 3.1.2, I am missing a visual comparison of time series. The vertical cross sections are difficult to read and would benefit from being consistent with Fig 7&9 (i.e. both could extend to 7km to allow discussing the stratospheric intrusion). The color legend of O_3 for the vertical cross section should be consistent with the maps (and the label corrected: it is probably ppb, not ppm).

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