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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of differences in large-scale circulation output from climate models on the regional modeling of ozone and PM" *by* A. M. M. Manders et al.

Anonymous Referee #1

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This paper presents an analysis of the impact of different meteorological conditions derived from global climate models on future climate projections using a regional chemistry transport model.
Quantifying the uncertainties of climate simulations and pointing out subsequent uncertainties on future air quality predictions is highly important and therefore this paper fits within the scope of ACP and is certainly worth publishing.
However, I find parts (especially of the 'results' chapter) confusing and I had often difficulties to follow the authors line of arguments. I have the impression that parts of this article are written slightly careless and some results are not described precisely enough. From my point of view the article could be significantly improved by

thoroughly rephrasing the results chapter and to clearly point out the major findings. A few scientific conclusions in this article raise my concern and I would recommend publication only after having addressed the following issues:

General comments:

My major concern is related to a lack of statistics showing the significance and robustness of the results.
The authors present almost solely average plots of certain quantities but draw several times the conclusion that the frequency of events was increased/decreased (I give an example below, e.g. section 3.2.1). How do they get to these conclusions? The figure in the supplement is the only one showing at least a standard deviation for some stations. Though a frequency analysis would be the appropriate analysis method. What causes the standard deviation - extreme events or a difference in the frequency of the occurrence of certain events? Were extreme events filtered for the average plots?

One of the main objectives of this article is to point out differences between global model meteorological fields and the impact of the related uncertainties on air quality predictions. However, the authors do not show or mention a single time if the differences are even statistically significant. I highly recommend to add at least a simple significance test to the analysis of the model simulations.
The authors write several times sentences similar like the following: *'For RLE ECHAM, the difference in concentration between future and present-day is smaller than the interannual variability in North Europe and about equal to the interannual variability in South Europe*.
Though, I have difficulties seeing immediately how the authors come to this conclusion. Therefore I would suggest to show the interannual variability!

Vast parts of the discussion focus on the uncertainties of the regional model even though this is not the objective of the article. I would suggest to focus the discussion more on the objective as given in the abstract: *.... The present study illustrates the impact of this uncertainty on air quality*

Specific comments:

Section 2.1:
I am a bit surprised by a (rather long) timestep of 15 minutes for a horizontal resolution of 0.44°. Don't you get interferences with the CFL criterion?

Section 2.2:
Could the authors add a reference for MACC?
I find the structure of this section a bit confusing. Could the authors shift the description, which gas phase mechanism and which aerosol module they are using to the beginning of the section (right after mentioning which species are included)?
A reference for CBMIV and EQSAM is missing.

Section 3.1:
This section is a summary of Manders et al. 2011 and therefore it does not belong to the chapter 'results'.
As the objective of this article is not a future climate projection but an assessment of climate projection uncertainties I do not necessarily expect the authors to present a more in depth analysis of the hindcast for proving a general good performance of the applied model system. However, when comparing model simulations driven by ERA-interim data with ECMWF analysis data - can't we expect a good correlation, simply because these two data sets are not fully independent? Shouldn't the model simulations (the hindcast / 'present day climate') be evaluated against a totally independent data set (e.g. the CRU data set)?

Section 3.2.1:
Can the authors explain in more detail how they come to the conclusion that a southward shift of the average low pressure centre leads to a more frequent occurrence of stagnant weather conditions?
How can you draw a conclusion from the AVERAGE (difference) in the mslp on the FREQUENCY of the occurrence of certain weather conditions? Does the southward shift of the low pressure centre in RLE-MICRO result from more frequently occurring southward shifts of the low pressure system (compared to RLE-ERA) or were 'extreme events' responsible for the difference in the average mslp? Were extreme events somehow filtered when calculating the average mean sea level pressure?

p.12255, l. 24: Maybe this sentence can be rephrased as using the expression 'overestimate' might give the wrong impression that RLE-ERA is closer to reality than RLE-MICRO.

p. 12255, l.21: ECHAM instead of ECHAM5 ?

Section 3.2.2:
The authors switched from present tense to past tense in the first sentences of this section. Can they consistently use present tense throughout the paper?

p. 12256, l.10: Actually the annual average daily maximum temperature is higher at ALL stations listed in S1 when using ERA compared to ECHAM.
l. 14: Can you add a reference from which you draw this conclusion?
Can you add a plot or table showing the interannual variability for all three scenarios?
l. 18-21 : 'Also the seasonal cycle is weaker.....'
The formulation of this conclusion is from my point of view misleading as it appears to

be valid for all of Europe even though only results for two stations are shown in Fig.4. I am also not sure if results from Madrid can be extrapolated to all of 'Southern Europe'. Again, I think it would be helpful to know the interannual variability for interpreting the results.

Differences in number of wet days: Is the number of wet days related to e.g. the number of frontal passages simulated differently in each global model? Or to the duration of cyclones over Central Europe, a feature that can be simulated differently in the global models?

Section 4.1 :

p.12259, l. 20: (Figs 7, S1): (Figs 7 and FigXXXX/Tab.XXXX in S(upplement)1) ?

Fig.7: I would find it convenient if the figure caption explained as precisely as the text in section 4.1. what I can see in the figure (e.g. 'June-July-August' instead of 'summer', 'average daily maximum' instead of 'average O_3 summer maximum'). Units are also missing.

I assume surface values were analysed? This should be mentioned somewhere!

p.12260, l3: presumably not only southern Europe, SOA formation should contribute to the PM load over large parts of Europe

Fig.8: Units are missing. The figure caption is confusing. Please write more precisely which differences exactly I can see in each panel.

p.12260, l19: 'Over sea....' This sentence seems incomplete. Or at least I don't

understand its meaning.

p.12260, l27: The patterns of differences...: Have you analysed a correlation between the number of calm days and PM concentrations?

p.12261, l22: 'magnitude' instead of 'amount' ?

p.12261, l38, 'nex' $\longrightarrow$ 'next'

Section 4.2

p.12263, l13: 'The reason is thats' $\longrightarrow$ 'The reason is that...'

l14: 'formation' rather than 'concentration' ?

How do you know that O_3 formation is VOC limited in the Netherlands and NO_x limited on the Iberian peninsula? As far as I understand, the authors assume that nitrate titration leads to a destruction of O_3 concentrations in Madrid - then they should clearly write that this is an assumption and not a result from their analysis.

How do you come to the conclusion that wind speeds are higher for temperatures around 12°C?

How well can you simulate a change in the mixing height with a very coarse (vertical) model resolution and only 5 model layers? Which parts of the results presented on p.12264 are based on assumptions and which parts are based on the analysis of you simulations?

'In Vredepeel, ammonium and nitrate concentrations are higher' $\longrightarrow$ are higher

than what?

Seasonal cycle of PM: How is this related to the seasonal cycle of emissions? And to what extent is it related to meteorology?

Section 5 :

'Two long-term...' $\longrightarrow$ 'Three long-term...' (ERA, ECHAM, MICRO)?

'Changes for PM10 are smaller than the interannual variability.' What exactly is the conclusion? Differences are (statistically) not significant?

p.12266, l.23: 'fot' $\longrightarrow$ 'for'

How do you come to the conclusion that ECHAM5 and MICRO are well-performing?

p.12268, l.14: '...between of meteorology' $\longrightarrow$ '...between meteorology'

It would be very helpful for the reader if the panels in all figures would be labelled with a), b), c) and it might be helpful to refer to the figures and corresponding panels in the text (e.g. Fig 3a) instead of Fig.3)
Figure S1: This is the only figure showing standard deviations to indicate the interannual variability. However, the quality of this figure is bad and should be improved (even though it is only part of the supplement). The labels are too small, the colours of the bars are too similar, standard deviations for the black bars are almost not visible.

Almost none of the figure captions contain units.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12245, 2012.