

Interactive comment on “Exploiting the weekly cycle as observed over Europe to analyse aerosol indirect effects in two climate models” by J. Quaas et al.

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Response to Referee 1

We would like to thank the reviewer for their constructive and helpful comments. [...] In general the paper is well written and concise, if a little short on detail (model processes included, observations used etc). The summary section repeats a lot of the results and could be shortened in order to allow more details of the models/observations to be put in earlier sections.

We added information for both, observations and models (particularly brief descriptions of the pertinent parameterizations) to the Methods sections of the revised manuscript.

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It would also be good to comment on the wider implications of the work and whether the understanding could be improved by additional/different simulations.

We think the main result is that one has to be cautious to interpret weekly cycles in cloud, precipitation or radiation quantities as evidence for aerosol indirect effects, especially when the comparison to a model or the statistical significance tests might be questionable. As for future approaches, we think that this type of analysis will become even more useful when even longer time series of large-scale high-quality satellite data become available. This statement is added as a last paragraph in the revised manuscript.

Specific points

Method

Is it a problem that the different observational and model data sets used apparently have very different lengths? [...]

This is indeed a very valid comment. In the revised version, we have shortened the periods analysed for the surface SO₂ and SO₄ concentrations, as well as for the DWD meteorological data, to the period 1 January 2000 (EMEP data) and 1 April 2001 (DWD data) to 31 December 2006, a time period directly comparable to the one by the satellite data. The later starting date for DWD data was now chosen since the method to obtain daily averages changed from this date on, and we wanted to avoid using two different methods. Please find in additional response a discussion of how the results change.

It is also not explained why the 10 year simulation was analysed as two 5 year simulations.

The reason was to use a simulation length similar to the (satellite) observations

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(admittedly in the previous manuscript version this choice was inconsistent with the longer periods used for the surface observations). Also, the sentence was misleading, we have re-formulated it.

Results

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I would like to have seen more discussion of why the weekly cycle in aerosol is buffered so dramatically compared to SO₂ both in the real world and in models.

We think that this is due to two reasons. Firstly, SO₄ is produced from SO₂ through chemical reactions in the atmosphere, and SO₄ variability is thus delayed and smoothed out, and secondly, variability in environmental conditions and oxidant concentrations is also contributing to the SO₄ variability. We have added a few sentences to explain this in the manuscript.

[...] Why is the amplitude of the weekly cycle in CDNC different by a factor of 2 between Terra and Aqua? [...]

We don't see a plausible reason for this large difference. We rather think that this reflects the uncertainty in this result in the observations. A sentence on this has been added to the manuscript.

Technical issues:

In figure 1 it is often very difficult to distinguish between the light and dark green of the two HadGEM simulations.

Thank you very much for the advice. We modified the colours. Also, your comment motivated us to split the figure into four separate ones, in which we hope the curves are easier to distinguish and better comparable.

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