“How realistic are air quality hindcasts driven by forcings from climate model simulations?”

by G. Lacressonnière et al.

Comments

Simulations of future changes in air quality provide an interesting new application for multiscale chemistry-transport models. During the last years, a multitude of relevant studies appeared in the literature. However, these studies rely on climate model simulations for providing the meteorological forcing, and their performance is difficult to assess. The present paper aims to help discriminating between the concept of air quality modeling in climate studies and forecast or hindcast studies, and in this respect, is most welcomed. However, there are some shortcomings which need to be taken into consideration, before it merits publication.

Specific comments

1. Introduction: The authors refer to previous relevant articles but they do not clarify if the results of these studies are related purely to climate change or emissions change. So the introduction needs practically to be put under a more thorough structure and perspective. Also, there are a number of recent relevant publications which are not referred at all. See for example (Andersson & Engardt, 2010, JGR, doi: 1029/2008JD011690; Huszar et al., Climate Research, 2012, doi: 10.3354/cr01036; Katragou et al., JGR, 2011, doi:10.1029/2011JD015899; Juda Rezzler et al., Climate Research, 2012). There is also a recent evaluation study by Zanis et al., Atmospheric Environment, 2011, doi:10.1016/j.atmosenv.2011.09 with a similar concept to that of this paper comparing long-term air quality simulations forced with reanalysis or GCM fields. An update of the literature with relevant work for Europe is needed.

2. The authors claim that they want to study the impact of different climate forcing on air quality. In the 1st experiment (ANALY), the chemistry transport model MOCAGE is driven by reanalysis and then by ARPEGE climatology (INT). The comparison of these 2 runs would have provided an idea of how the hindcast (driven by reanalysis) can be different from the control (driven by the climate model ARPEGE) in case the model set up had been the same. However, if I understand correctly, different resolutions are used for the two models configuration. Thus, the differences between ANALY and INT simulations do not show only the impact of reanalysis vs control, but also the differences of a coarse vs fine resolution meteorological forcing on air quality.

3. 2003 is mentioned to be omitted from the analysis as it is considered to be a climatic anomaly. This needs to be better justified. Climate extremes are part of the present-, and even more importantly for the future-climate, thus potential climate-air quality models should be assessed for their potential to capture such cases, as well.

4. Page 2088, line 21: Organic and Nitrate aerosols are not taken into account, which is an important drawback for the assessment of PM.

5. Page 2094, lines 15-26: The authors claim that temperature change is the most important factor that determines the ozone change. In what physical sense? Due to temperature dependence in reaction rates? I think this is an oversimplification. The authors should consider that anticyclonic anomalies lead to positive temperature anomalies but at the same time also lead to positive anomalies in incoming solar radiation and more stagnant conditions with longer residence times of air masses over certain areas which favour ozone production. In general, circulation changes is an important factor that is not referred at all.

6. Page 2096, lines 3-4: The authors state that for all pollutants the differences are primarily due to PBL height differences. This process is more important for primary pollutants while for secondary pollutants the situation is more complex. Furthermore, especially for ozone which has also a source from above the situation is even more complex.

7. Page 2097, lines 20-21: According to the authors, the changes in isoprene are mainly attributed to dynamical processes of the Boundary layer. Following the Guenther approach to calculate biogenic emissions it is sensible that changes in temperature and solar radiation are at least as important.

8. The effect of the lateral and top boundary conditions is not discussed at all. Especially for ozone, these play a very important role (e.g. Stratosphere-Troposphere Transport, intercontinental transport etc). In general a paragraph should be added with the limitations of the modeling approach.

9. Another important point is that the statistical results are presented but they are not discussed or explained in a physical sense.

10. The Section 3.2.3 is not clear to understand. Please provide more clarifications for the methodology used.

Minor/technical comments

1. The abstract needs to be revised as it is too generic. It should focus more on the basic findings of the work performed.

2. In the abstract are mentioned 6 year of simulations while in the manuscript the time slice 2004-2008 appears to be selected. The inconsistency needs to be corrected.

3. 12 figures and 12 Tables for a single paper may be a little bit more than a reader can follow. I tend to think that some filtering will be necessary. The description of the presented material could be more concise and more focus could be given on the analysis of physics and chemistry, their linkages and the implications.

4. The writing (language) does not always meet the required standards. Several paragraphs need to be more carefully rewritten.