Atmos. Chem. Phys. Discuss., 6, S7064–S7070, 2007
www.atmos-chem-phys-discuss.net/6/S7064/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparing atmospheric transport models for future regional inversions over Europe. Part 1: Mapping the CO_2 atmospheric signals" *by* C. Geels et al.

C. Geels et al.

Received and published: 6 March 2007

First we want to thank the reviewer for the positive comments and for recommending publication. We repeat the reviewer's statements below and comment on them:

General comments by reviewer: This paper presents CO2 simulations (essentially over Europe) from 5 models of different construction and resolution. The simulations are compared with each other and to some extent with existing measurement data. The goal as stated in the title is to build knowledge about model behavior to serve as guidance for how inverse modelling techniques should be used to make quantitative estimates of regional European fluxes of carbon dioxide. The paper makes clear headway towards this goal but falls short of addressing issues like station representativity, model

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adequacy and measurement strategies thoroughly. Nevertheless, the paper is clear and well written and is a useful contribution which merits publication and attention. My main concern is related to the logic utilized in arriving at the four main recommendations listed in the abstract. The authors appear to be attracted towards finding traits in the model simulations that are similar (and by their logic thus “robust”). That the model results are similar need not mean that the results are correct depictions of the real atmosphere. Secondly, the features of the model simulations that are similar are probably related to the least questionable of regional aspects of the system and will thus lead to minimal new knowledge when later de-convolved in an inverse study. The authors also have a general attraction towards “higher resolution” as a solution to several problems. Higher resolution is only useful (for regional CO2 flux quantification) if we have ADEQUATE knowledge about the physics such that the increased resolution does not introduce more degrees of freedom in the model. We can actually end up knowing less despite acquiring better correspondence between data and simulation.

Answer: We agree that the issues related to for example station representativeness are very important, but also a very complicated problem that have been the main subject of many papers. The uncertainties related to flux fields estimated by inverse methods are certainly related to the insufficient coverage of CO2 observations as well as the representativness of these stations. The issues raised by the reviewer are very interesting, but beyond the scope of this paper. The main purpose of this paper is, however, to compare existing models against each other and against existing CO2 measurements in order to assess the uncertainties related to the use of different models. It is true that just because the model results are similar it does not mean that they are correct depictions of the real atmosphere, but similar model results is a strong indication that some of the features and processes of the real atmosphere is captured correctly. In the inversions we just want to have the transport as correct as possible in order to assess the surface exchange as precise as possible. The reviewer is right on emphasising that a higher resolution might require better description of the physics in the model. This is a general problem related to the application of atmospheric models and

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is also beyond the scope of this paper. However, we know that higher resolution gives a better representation of the driving meteorology as well as a better representation of e.g. the surface topography and if proper physical parameterizations are used then the transport and mixing of e.g. CO2 are better simulated than the coarse scale models.

Specific comments by reviewer: Page 3712 line 22 “Atmospheric transport integrated over all CO2 surface sources and sinks”. This is the key statement. The real atmosphere “integrates” over all spatial and temporal scales. Our models are simplifications of reality with finite time steps, coarse grids and approximate (or incomplete) physics. Our source and sink functions are, likewise, approximate at best with yet other resolutions in time and space than the transport model. The instantaneous measured value at one sampling location is made up of all real effects “integrated”. We are all attracted to a conceptual idea that the data are composed of a “background” signal with superimposed “local” effects. The real atmosphere is, however, full of idiosyncrasies with constantly changing mixing scales in space and time. We can here even discuss what we mean with “integrate”. As argued above it is the sum of all effects prior to the occasion of measurement; as implicitly argued in the paper it may be more appropriate to consider “integration” as mixing. As sources and sinks influence a volume of air that air “parcel” acquires a new composition. Over time this air is mixed with other air and the mixtures composition is then a volume weighted average of the mixed air masses. When the mixture is completely homogeneous the information regarding the (idealized) two air masses different histories has been erased. It will then be impossible to arrive at anything but an average (or integrated?) CO2 flux for the areas of exchange of the two air masses. But the surface exchange is continuous and the mixing is a continuous process. Sometimes the atmosphere is kind and we have some sort of physical boundary (like the ITCZ) that encloses or simplifies the problem at least for a limited time or area but most of the time we must utilize all our knowledge about the full history of the air being sampled. Page 3713 line 13. Observation sitesˇE Based in preconceived ideas of what is “regional” and “local”.

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Answer: We agree with the reviewers general comments on the complexity of the real atmosphere and that the term “integration” in this case should be written as “mixing”. It will, however, not be appropriate to include detailed description or definition of the term in the paper and we will for now stick to the term “integration” as this is commonly used in this context. We also agree that the “definition” of “regional” and “local” observation sites are not very appropriate, but that have traditionally been the terms applied in the scientific community.

Reviewer: Page 3714 line 21. What do the authors mean with the words “transport variability” here? This sentence states the “ultimate purpose of the paper” but is unfortunately difficult to interpret.

Answer: We agree that this sentence is not clearly formulated, we are sorry about that and will change this for the final version of the paper. We simply mean that we want to estimate the variability across the model results due to the differences in the transport in the models - hence the word “transport variability”. The sentence is now changed to:” The purpose of this paper is to estimate the variability of the results given by a representative range of different models due to the differences in the description of transport in each model. Thereby a better understanding of how to use optimally the new continental CO2 data and models to reduce the uncertainties of land sources and sinks estimates can be obtained.”

Reviewer: Page 3716 section 2.1 A number of acronyms are introduced and well known to most of us in the field but an appendix with explanations may be appropriate. Some further details on the “zooming” and “nesting” methods used could also be considered.

Answer: It is true that more information and explanations on these technical terms could be given. We have, however, chosen to include references to papers where the models and hence the appropriate terms are described in more detail. This is necessary in order make room for the detailed discussion of the results.

Reviewer: Page 3721 line 3. The models are “referenced” to Mace Head. An explana-

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tion of how this is done would be helpful.

Answer: True, we will include an explanation and we will also mention the range across the models for the applied values at Mace Head.

Reviewer: Page 3721 line 19. “qualitatively similar”. It can be argued that this qualitative trait is there by design. Regardless of how “wrong” the models are in their physics anything but higher values over the continent and lower over the ocean given the fluxes as prescribed would have been shocking. This issue is profound since the choice of comparisons we make is rather arbitrary (for example page 3720 line 1, “occasional” high values are deemed unimportant).

Answer: The sentence (line 19) is just an introduction where features in figure 1 are described in general terms. We prefer not to include a philosophic discussion about the words emphasised by the reviewer.

Reviewer: Page 3725 The radiocarbon data are monthly mean values. How are the influenced by the “very local” contamination not included in the CO2 concentration data? The Heidelberg station is clearly very special with numerous local effects but also the only calibration point utilized in this paper for the entire European continent. This raises representativity issues.

Answer: We are not sure that we understand the question raised by the reviewer here. It is true that there will be an impact of “very local” contamination in the 14C observations that not will be included in the models (subscale processes and low spatiotemporal resolution of emission data). In order to avoid the local impact as much as possible the observations have been selectively subsampled according to site-specific “regional background” criteria based on wind speed and direction and we look at the daytime values of both observations and models. Only exception is Heidelberg where only night-time values are sampled in order to minimize very local contamination from traffic. References to papers where the sampling strategies are described are given in the paper. But we certainly agree that stations representativness is an issue here and we

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also mention that in the paper. 14C data from Schauinsland and Jungfraujoch are also included in the evaluation, so it is not true that Heidelberg is the only calibration point used.

Reviewer: Page 3730 paragraph beginning on line 22 ECMWF and NCEP have evolved from needs to predict weather. Mid-latitude weather prediction is to a large extent comprised of predicting frontal passages. It is reassuring that the synoptic scale is captured well by both these tools. But the CO2 concentrations are influenced by annual cycles on hemispheric scales, long term trends, synoptic variability, finer scale circulation features and other local effects. The present study side-steps the first two, does well on the synoptic scale and then has mixed results on the finer scales with most discussion centered on PBL variations and a little on topography. There are many effects on local to regional scales that intermittently play important roles. To arrive at a conclusion that mid-afternoon values a few hundred meters above ground maximizes the information content in the data is not apparent to this reviewer. The “signal” is weaker so even if the values are less variable we must decipher more from the smaller variability. The “representative” altitude for the “region” will vary with season and weather. The afternoon values may be “representative” of something we wish to interpret as “background” but are for example the weakest data when it comes to quantifying the night time respiration flux. And finally; it is concluded that mountain stations should be used with care. I dare say that the CO2 Mauna Loa data series has contributed more to our understanding of the global carbon cycle than all other stations combined. The series is comprised of hand picked afternoon (!) data from a mountain station.

Answer: We certainly agree that are numerous regional and local effects on long to short time scales that play important roles for the atmospheric CO2 concentration. We do, however, not conclude that mid-afternoon values maximizes the information content in the data. We attempt to find features in the observed CO2 field that the models captures and try to minimize the difference between measurements and model results by looking at mid-afternoon data.

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Reviewer: Figure captions 12 & 13 “of the scale” should be “off the scale”

Answer: Will be corrected. Thanks.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3709, 2006.