Atmos. Chem. Phys. Discuss., 3, S1298–S1302, 2003
www.atmos-chem-phys.org/acpd/3/S1298/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "An evaluation of the performance of chemistry transport models by comparison with research aircraft observations. Part 1: Concepts and overall model performance" *by* D. Brunner et al.

D. Brunner et al.

Received and published: 14 August 2003

We would like to thank the referee for the encouraging remarks and for the critical reading and very helpful suggestions.

The referee has raised the point (similar to referee #1) that also the point-by-point comparison between interpolated model values and observations has some limitations. The main question is how representative is an aircraft observation measured along a line in space with respect to a grid volume average as provided by the model.

We have added a few sentences to section 2.3 (Quantitative analysis of model performance using Taylor-diagrams) referring to this problem. The section now reads as

follows:

"The maximum attainable correlation is limited for instance by the fact that the model fields can not fully resolve all features of the 6-min averaged observation data. Representativeness errors are a further limitation. It is not clear how representative an observation averaged along a line-shaped aircraft track is with respect to a grid volume average. An estimate of this error would require additional information on the real variability of the concentrations inside a model grid volume. Apart from the mean value first and second moments of the trace gas distribution inside a grid box are sometimes available in a model depending on the type of advection scheme. This information could be used to improve the interpolation from the model grid to the aircraft track. However, for simplicity we have applied the same interpolation algorithm in all models which may only account for a first order linear variation in the spatial distribution of trace gas concentrations. In order to obtain a rough estimate of the influence of the above mentioned limitations on R0 we have investigated the correlation between modeled and measured temperature as discussed in Sect. 3.3. Instrument noise further reduces the maximum attainable correlation. To estimate this effect we added an artificial Gaussian noise to the point-by-point output of a particular model according to the stated instrument precision. The correlation between the original and the noisy model output then provides a measure for the influence of instrument noise on R0."

Furthermore, we have clarified how the skill score S is calculated and what the role of temperature is in this context by adding the following sentences to section 3.3. (Taylor diagrams):

"Isolines of skill scores S are shown in the figure as grey contours. The definition of S (see equation 2) includes an estimate for the maximum attainable correlation R0. A rough estimate of this value may be obtained by considering the correlation between modeled and observed temperatures represented by the dark blue labels in the figure. Model temperatures are expected to be fairly accurate because of the assimilation of temperature observations into the driving ECMWF model. The correlation is limited

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by representativeness errors, the lower spatial resolution of the models as compared to the 6-min averaged observations, and errors in the meteorological analysis. The influence of instrument noise on R0 was estimated for a few individual campaigns and instruments. It was found to be most of the time significantly smaller then the influence of the errors mentioned above because instrument noise is usually very small for the 6-min averaged observations. We have therefore neglected this influence here and used the same R0 for all trace species. This allows plotting the results for all different trace gases and different measurement campaigns in a single Taylor diagram with a single representation of skill score contours. However, this is not fully justified for instance for some of the OH and PAN measurements and for measurements of very low NO concentrations over remote regions as discussed in more detail in Brunner et al. (2003)."

Small issues:

Table 1, vertical transport and dispersion: We have already included information on the advection scheme (which also refers to vertical advection), the convection scheme, and the way the vertical wind component was derived. We think that this already provides the most relevant information on vertical transport. Further details for each individual model can be found in the publications referred to in the table.

Table 2: The table has been changed to show the emissions as used exactly in each model.

Instrument lag and response time: We have not considered this. My own experience with in-situ observations in the German SPURT project (Spurengastransport in the tropopause region), which includes a broad range of common measurement techniques such as tunable diode laser, chemiluminescence detectors, GC-ECD, UV and infrared absorption techniques, Lyman-alpha) time lags and differences in response times are typically well below 10 sec. The distance traveled by an airplane in 10 seconds is only of the order of 2 km, which is very small compared to the model grid size.

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10 sec are also small compared to the 6-min averaging period used for the aircraft data. Furthermore, response times and time lags are sometimes already accounted for in the observation data.

Model biases: The averages are taken over all observation samples and their interpolated model counterparts available for a given campaign, region and altitude range. Please note that the "average model bias" as defined in our paper does not represent an average over individual biases but rather a bias in averaged model values versus averaged observation data. It tells us how much does a model over- or underestimate the average concentrations over a given domain. This is now stated much more clearly in the text. Individual very low observation values (e.g. in NO), which may lead to very large amplitudes in the bias, are therefore not a problem. The averaged observed values used in the calculation of the bias were always well above the detection limit of the respective instrument. We have not considered to use a fractional bias because the bias as defined in the paper directly provides a value for how much an average model value deviates from an observed average.

NOx surrogate: The TM3 model was used because output of this model was available for the entire 4 year period and because the model has an advanced scheme for the online calculation of photolysis rates. We are aware of the problem that for large NOx:NO ratios errors in NO would be largely amplified. We have therefore used in all our calculations (biases, Taylor diagrams etc.) only observations for which the NO:NOx ratio is greater than 0.2. This efficiently excludes measurements at night and at high solar zenith angles. We have stated this now also in the manuscript. Using a photochemical steady-state assumption to derive NO2 from NO and ozone would indeed be a useful alternative but it would require quite a high effort (appropriate estimates for O3 and NO2 columns, aerosols, surface albedo) to get close to the accuracy of the method used in TM3. Also, this method would neglect the influence of peroxy radicals on the equilibrium state and it would be very difficult to estimate the probably significant influence of clouds.

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Technical trivia:

Section 2.1: - Removed redundant definition of CTMs and C-GCMs - Adapt changed to adopt as suggested. - HO2 has a subscript now

Section 2.2: - Reference to Figure 1 changed to Figs. 1a and 1b

Section 3.4: - Second reference to Fig. 10a changed to Fig. 10b - The discussion refers to NOx, not NO. We have added a line stating that NOx was calculated in the same way as in Sect. 3.2, i.e. from measured NO and the NO2:NO ratio of the TM3 model.

Fig. 8, 9 and 10: Changes made as suggested. Actually, in the first version of Fig. 10 the y-axis annotation was mistakenly NO instead of NOx. In the online version of the manuscript on ACPD this has already been corrected.

Improved grammar for better readability: We have put a significant effort in this point. We think the main issue was that we had a lot of very long sentences. Most of the amendments therefore are to the length of individual sentences.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2499, 2003.