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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global chemical weather forecasts for field campaign planning: predictions and observations of large-scale features during MINOS, CONTRACE, and INDOEX" *by* M. G. Lawrence et al.

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We are very appreciative of Dr. Horowitz's thorough review, positive rating, and many constructive comments. The revised manuscript will incorporate almost all of the suggestions. Here are point-by-point responses (sorted by section, as done by the referee, then numbered within each section).

Abstract:

The correlations are concentrations (although one gets the same for deviations from the mean), while the RMS differences are for deviations from the mean; this will be clarified in the text.

1. Introduction

1) will be added

2) I disagree on this point a bit. The chemical weather forecasts currently do rely heavily on the meteorological forecasts, and use climatological data for several processes, particularly anthropogenic emissions. However, the response of the chemical system through reactions, photolysis, dry deposition, and other processes is dynamic rather than climatological. In this sense, the chemical weather forecasts are not qualitatively different from meteorological forecasts, which also depend to some degree on climatological data (e.g., land use and biosphere types determining albedo and surface heat fluxes, sea surface temperatures, etc.). More sophisticated climate and NWP models include dynamic representations for these, just like future chemical weather forecast systems will include dynamic representations of various types of emissions.

3) Continental scale is meant and will be added; the possibility of using cloud-resolving models is mentioned later in the introduction.

2. Forecast model system setup

1) only industrial and biomass burning emissions are included, not secondary production from VOC oxidation or oceanic emissions; this will be noted in the text

2) This was in the appendix, will be pulled forward into the main text; thanks for the tip on the new name.

3) This was in an earlier version (before it was shortened), and will be reinstated (in the appendix).

4) The text will be modified to indicate that this worked for the three specific campaigns discussed here. It is worth noting on the side that the feedback regarding FLEXPART received from the ITCT flight planning team was that the updated forecasts every 6 hours was the biggest advantage of the FLEXPART forecasts compared to the others, which were available only once or twice a day. Andreas was a bit surprised about this

ACPD

2, S771–S776, 2002

Interactive Comment

(me, too), because the forecasts did not seem to change so much from one to the next, but obviously for the exact flight planning it was an advantage to have forecasts available based on the most recent meteo forecast. Since that is based on ITCT, I'll leave it out of this paper, but it will certainly be something for the community to discuss in ongoing forecast system improvements.

3.2 Overview of the forecast applications...

1) The following text will be added: "In addition to the flights listed in Table 3, MATCH-MPIC forecasts played a secondary role in the planning of several other flights, especially during INDOEX, when the chemical weather forecasts were untested and more weight was normally placed on the traditional forecast products. Several flights were also guided by other criteria to which the forecast system described here did not apply, such as intercomparison flights between aircraft, and aerosol observations."

2) This is a very good question to raise. I have thought of two metrics to demonstrate that the MINOS plumes are not part of the background variability (for CONTRACE the enhanced mixing ratios in the plume and the evolution of the plume seen in both MATCH-MPIC and FLEXPART is clear enough that I don't think this is much of an issue). First, I have recomputed the correlations for the 1 and 3 August flights, but limiting the altitude range to only above 4 km (as done for the 8 August flight already); these are now included in the summary table, and they show that the correlation is still good for the smaller zig-zags in the MT and UT, and is not solely driven by the surface to UT vertical gradient. Second, the mean CO levels in the UT for these three flights far exceeds the mean for all 14 flights averaged together. This was already shown and discussed in Lelieveld's MINOS Science paper; I refer to that analysis in support of the diagnosis of these events as plumes, and feel that, along with the time series analysis presented for the model output, this is a much stronger demonstration of the point that the model can successfully guide flights into intercontinental pollution transport events.

3) thanks, corrected

ACPD

2, S771–S776, 2002

4) 6S was indicated by the met forecasts; the text will be modified to make this clearer.

5) I'll add the following to the main text: "note that in both models the convection is re-diagnosed based on the basic meteorological data, and is not taken directly from NCEP; see the appendix for further information", and to the appendix: "in particular, the temperature and humidity fields are ``destabalized" by advecting them over a timestep before using them to diagnose the moist convection and vertical diffusion (for further details about this procedure the reader is referred to the previous MATCH papers cited in the main text)." I would like to see an extensive discussion of this in the literature, but I don't think this paper is the right place to go into more detail than this.

6) I made this into a generic statement; I think it is better not to pick on any one particular future campaign as an example, since it is hard to know at present how to favor one over the others.

3.3 The Asian monsoon plume during MINOS

1) Thanks for making clear that this wasn't clear; this also relates back to the comment (above) on additional metrics for the plume diagnosis. I have carefully rewritten the paragraph (starting at line 11, p. 1558) to make the extent of the plume and the comparison with the models clearer. I liked your suggestion of including the ovals on the figure, but when I tried them out I found they mainly cluttered up the picture and made it confusing to read, so I'm going to leave them off and hope the text is now clear enough.

2) I have decided to cut this discussion from this section (to compensate in part for text added elsewhere). While it is interesting to think about, as the referee points out, it is a rather tentative signal, and it is not central to the discussion.

(3.4 CONTRACE)

1) This will be reworded approximately as follows: "Both indicate a clear enhancement in CO from North America over Scandinavia and northern-central Europe (MATCH-

ACPD
2, S771–S776, 2002

MPIC shows the total CO, which was particularly useful in the flight planning for indicating that the signal should be easily observable within instrumental uncertainty). The qualitative resemblance between the forecasts at 500 hPa, despite the widely differing model formulations, provided a considerable degree of confidence for the flight planning." It is actually very difficult to compare the two models quantitatively, since FLEXPART only tracks particles for 20 days, and thus has no background level of CO due to North American emissions.

4. Conclusions

1) This is a good point; the text will be reworded accordingly, and will cite the new Kasibhatla et al. (2002) study showing a possible 35% underestimate of Asian CO emissions in current datasets.

2) Thanks for the positive judgement of the discussion! I'm glad if this point comparing chemical and meteorological weather forecasts gets across (see also the response to the related comment above). I will mention the climatological sources in the conclusions; I assume the rest of the comment was reflecting on what was found to be good in this discussion, since the points about the fire count assimilation and the satellite tropospheric column (or profile) assimilations are already in the manuscript.

Appendix

1) I have not yet done a careful evaluation of the modeled water vapor field, only quick comparisons to ECHAM and CCM (it compares qualitatively well). This is something I've wanted to do for a while, and have finally hired someone who will start on this project in January.

2+3) These are all good comments and will be worked into the text.

4) The RMS equation (it is the differences of the deviations from the means) is sort of in the table caption already, but I will add the full equation to make it clearer.

5) Good idea, done.

ACPD

2, S771–S776, 2002

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ACPD

2, S771–S776, 2002

Interactive Comment