



## ***Interactive comment on “Coupling global chemistry transport models to ECMWF’s integrated forecast system” by J. Flemming et al.***

### **Anonymous Referee #1**

Received and published: 4 September 2009

#### General:

The authors report on the results of coupling three different CTMs to the ECMWF IFS system using the OASIS coupler. This approach allows for the assimilation of satellite atmospheric composition measurements into the IFS model in a straightforward manner. This provides an extension of the IFS model which will be of much use for the atmospheric chemistry and climate modelling community, allowing e. g. for air quality forecasts as part of weather forecasts.

The paper is technical in nature, going into some detail about the way the coupling is performed. I have got a problem with the attribution of some of the tendencies found for CO and O<sub>3</sub>, which the authors describe as the effect of "diffusion" when to me the trends look much like the result of the PBL schemes operating on the tracers. Section 2

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



should go into slightly more detail how these transport-related processes are handled. It can be made more explicit that the PBL schemes, convection and diffusion, as far as their effects on composition are concerned, are all part of the CTMs. These are all parameterized processes, and there is ample scope for differences between the 3 CTMs regarding these processes, particularly regarding convection and the PBL schemes. Considering the shape of the CO tendencies, almost certainly the differences in the trends are the result of differences in the height of the PBL.

I don't understand the rationale of why a particular set of compounds is chosen for assimilation and not others which space-borne observations are also readily available for. This needs to be motivated more clearly.

The authors use 6 or 7 surface stations to assess realism in their model. Either the focus should be changed to become exclusively tropospheric. Or some stratospheric diagnostics should be included, such as a map of total ozone compared to TOMS or other total ozone data, or some profiles of ozone compared to ozone sonde data or satellite ozone profiles. I understand that the validation of the CTMs has been done elsewhere, but still I find the selection of diagnostics a little unbalanced. SO<sub>2</sub> and CH<sub>2</sub>O are also assimilated but no diagnostic is shown of these compounds. I recommend explicitly limiting the scope of the paper to the troposphere, and drawing up a separate paper about validation of the coupled system in the stratosphere.

Figure 5 (three panels), 6, 7, and 8 are included but not mentioned in the text. I suggest to either refer to the figures in the text or remove them.

I recommend publication of the paper in GMDD subject to revisions.

Specific comments:

p. 765, line 25 ff: Atmospheric chemistry of course comprises a lot more species, particularly short-lived ones such as the HO<sub>x</sub> family, O, O(<sup>1</sup>D), methane etc. I imagine that the chemistry schemes also simulate these species as they are needed for pre-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

dicting ozone. This should be expanded explicitly. Regarding methane, it is certainly also observed from space. Why does the system not predict and assimilate methane? The same goes for  $\text{N}_2\text{O}$ ,  $\text{HCl}$ ,  $\text{ClO}$ ,  $\text{BrO}$ , the CFCs, and possibly more species that are routinely observed from space.

p. 773, line 28ff: A one-hour timestep is however routinely used in some CTMs. Some of these models do not transport  $\text{NO}$  and  $\text{NO}_2$  together and simply live with the fact that the  $\text{NO}/\text{NO}_2$  transition jumps discontinuously with the terminator. For most, that's no problem. If you use a one-hour timestep, inaccuracies around the terminator are part of the trade-off for faster turn-around. Transporting them together simply pushes the discontinuity into the ratios  $\text{NO}/\text{NO}_x$  and  $\text{NO}_2/\text{NO}_x$  that need to be computed within chemistry.

p. 775 line 10: Unclear sentence.

p. 776, line 21: I don't believe that diffusion is the main player here. I guess you have localized emissions of  $\text{CO}$ , but wide-spread dry deposition to the surface. A difference should be made between "diffusion" (which is usually kept minimal and is there to keep the model stable) and the PBL scheme. The PBL scheme may work a bit like a vertical diffusion operator but in reality is not just diffusion. So I think the tendency to increase  $\text{CO}$  concentration near the top of the PBL and decrease it close to the surface is because of the PBL schemes operating. Diffusion would also not make much difference between day and night whereas the PBL scheme does. The difference between the different CTMs presumably originates in their different PBL schemes. Can you derive the PBL height from the CTMs to compare? Probably they differ from each other and from the IFS PBL height.

p. 776, L28: It's pretty obvious that  $\text{CO} + \text{OH}$  (the dominant gasphase sink for  $\text{CO}$ ) does not operate at night. I would leave out the nighttime plot (panel 4 of figure 3).

p. 777 L25: Again you mean to say that the PBL scheme does not impact ozone too much (which I doubt). The difference w. r. t.  $\text{CO}$  may be that sources of  $\text{O}_3$  (as a

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



secondary pollutant) are not as localized as those of CO, which possibly means that the maximum at the top of the PBL is not as pronounced.

p. 777 L27: I would replace NO with NO<sub>x</sub> in this sentence. This is a cycle involving NO to NO<sub>2</sub> and NO<sub>3</sub> conversion.

Figure 3: Should the caption read "due to surface and aircraft emissions, dry deposition and diffusion" for the left 2 panels? In the text, much is made of the effect of diffusion which is however not listed as a process in the caption.

Figure 6" Which station does "STC" stand for? Where is Neumayer (NEU) in the CO plot?

---

Interactive comment on Geosci. Model Dev. Discuss., 2, 763, 2009.

**GMDD**

2, C217–C220, 2009

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper