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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating organic species with the global atmospheric chemistry general circulation model ECHAM5/MESSy1: a comparison of model results with observations" *by* A. Pozzer et al.

Anonymous Referee #2

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Summary: The authors present a comprehensive comparison of their "Earth System" model with observations. Their model must probably count as state-of-the-art, yet their results are a bit ambiguous regarding how realistically the model represents the budget and distribution of organic trace gases. I think they give a fair reflection of their results; I could not find any major points where I would disagree with the authors. At a first read I was a little stunned that the authors went into a full discussion of their model results without properly introducing the model chemistry, photolysis, dry and wet deposition etc. I realize that a full description of the model would make the paper considerably

longer and such a description is given elsewhere (on the web), but having no however cursory description of their isoprene chemistry, their photolysis etc strikes me as rather bold. With these caveats in mind I recommend publication of the paper in ACP.

Detailed points: Abstract: I think the fact that the model simulates isoprene (albeit very poorly) should be mentioned in the abstract.

Section 2: A reference for anthropogenic emissions is given; how about biogenic emissions? All coupled, online?

Section 3.1, line 22: I thought a Taylor diagram visualized two statistical quantities, correlation and standard deviation. Which "three" are you referring to?

Section 5.2: I have my doubts that the revised reaction coefficient will do enough to reconcile the model profiles of C_3H_6 with observations. An inspection of the profiles suggests that a decrease of the loss rate by a factor of two might be sufficient to close the gap for some stations, but given that most of the loss will be near the surface near the warm end of the temperature range displayed in figure 16, the revised temperature dependence will not nearly yield that. Also figure 15 suggests that for the two European stations the agreement is much better than for the rest, indicating that perhaps the (anthropogenic?) emissions elsewhere are not quite right. Another factor might be, as you noted elsewhere, that the convection is not transporting matter as effectively into the upper troposphere as it should do.

Section 6: You mention that there is a substantial temperature bias over Amazonia due to errors in the hydrological cycle. I appreciate that you're not nudging hydrological variables, but still I thought that nudging meteorological variables should eliminate or at least keep within acceptable limits this error. Also I don't understand how this problem can exist in the nudged version of E5M1 but not in the free-running version. Is it that you need a cold bias of surface temperatures to keep hydrology in check to prevent run-away surface heating in Amazonia?

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Also in this section, line 1, youu state that annual production of isoprene in 1998 is 580 TgC. but then, for the years 1999-2004, you have just over 300 TgC/year. Did you apply the ad-hoc scaling to 50% to those years but not to 1998? If so, please make this explicit. If not, why does 1998 produce so much more isoprene than later years?

Figure 17: I find this picture hard to believe. I thought that regions emitting isoprene are relatively well-known, so either figure 17 is unrepresentative of global conditions, or isoprene sinks are very poorly understood. I gather that for ozone your model does not do so badly, so perhaps isoprene is not so important for climate feedback purposes? Do you have the option in your model to replace on-line with off-line emissions (taken from a database) to see whether perhaps it's the on-line coupling which is to blame?

Section 7.1: H_2O_2 is one of your stronger species. My comment on fig. 19 is that the only region where there is some cause for concern is North America (the bottom left panel), where E5M1 overestimates H_2O_2. This means that there must be to much HO_x in that region. How does that square with your results for CO and other HO_x-related species for North America?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 127, 2007.