

A review report on “The TOMCAT global chemical transport model v1.6: Description of chemical mechanism and model evaluation” by Monks et al., 2017.

General

The study presents a comprehensive description and validation of a tropospheric chemical mechanism currently used in the TOMCAT global chemical transport model. The current description is an update to the original scheme of Chipperfield et al. (2006) and references therein. However, in view of recent scientific updates in chemical mechanisms addressing isoprene recycling under low NO_x conditions as well as RO₂/HO₂/ROOH formation in remote regions in the EMAC (Lelieveld et al., 2016), the MCMv3.3.1 (Jenkin et al., 2016) and GEOS-Chem (Fisher et al., 2016), my main concern here is what are the scientific benefits of such (updated) chemical mechanism, which does not cover any of these known biases. Also, this updated chemical scheme is unfortunately based on outdated chemical scheme (MIM), which was replaced by MIM2 and most recently by MIM3 (Lelieveld et al., 2016). The authors are also using the most outdated version of MCMv3.1 instead of the most recent versions of MCMv3.3 and MCMv3.3.1? I think that this is a serious issue since the authors are aiming to document and publish a reduced chemical mechanism that is literally based on outdated schemes..?

It is certainly important to document a chemical scheme that is used for scientific research, but only if there are important updates that warrant publication, **compared to the original scheme**, which is not the case here, at least not demonstrated. The study does present a comprehensive validation of the chemistry scheme but without a sufficient justification for the scientific benefits, especially given the recent updates in isoprene and HO_x chemistry in the last few years (see above), none of which are implemented here. I wished also to see a long simulation experiment (e.g., 20 years) to see if the model can capture, e.g., methane trend and growth rates, CO and O₃ inter annual variability, especially given the large biases in OH, CO, O₃ and VOCs.

Some Specific Comments

P1 L 10-12: Authors may also show the mean global OH concentration along with the range of multimodal ACCMIP values and MCF-based estimates.

P1 L13: Would this bias result from using a biased/different water vapor verticals profiles, photolysis profiles?

P4 L 7-10: It is unfortunate that the authors decided to use the old MIM version (Pöschl et al., 2000), which has been updated several times, with most recent versions MIM2 (Taraborelli et al., 2012) and MIM3 (Lelieveld et al., 2016) addressing important update in Isoprene and HO_x chemistry.

P4 L13-14: Since the authors aim at documenting this scheme, could the authors further elaborate why does the extended chemistry increase the burdens of the mentioned species? Would be also very interesting to see some scientific discussions and justifications compared to the original scheme.

P4 L24: Again, The users are using the most outdated version of MCMv3.1 instead of the most recent versions of MCMv3.3 and MCMv3.3.1? I think that is

a serious issue since the authors are aiming to publish a chemical mechanism that is literally based on outdated schemes?

P6 L3: Change “cheap” to “efficient”. Is there is any quantitative analysis that support the author’s claim that the option “TOMCAT-GLOMAP” is computationally efficient? Compared to what?

P9 L 12: Fig. 3, Why OH levels are very high over the sea southeast and southwest of India?

OH vertical profile does not seem typical, e.g., compared to Spivakovsky et al. (2000). Although the authors mentioned this earlier but some discussions are needed here to address this difference?

P10 L7: OH from TOMCAT is still too high in the Arabian Sea to the coast of India, not related to ship traffic as in Voulgarakis et al. (2013), artifacts?

P11 L5-15: Although the authors discussed the higher OH levels near the surface as opposed to other models, the issue still not corrected, which, as the authors mentioned, affect e.g., model calculation of methane lifetime. I think whether this issue is related to the driven H₂O profiles, photolysis rate calculations, or the underestimated anthropogenic emissions (CO is a main OH sink near the surface), it has to be corrected, otherwise, how this new scheme can be used for comparison with other models, which see high OH near 600 hpa for know reasons (see e.g., Spivakovsky et al., 2000).

References

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