

Interactive comment on “Global simulation of tropospheric chemistry at 12.5 km resolution: performance and evaluation of the GEOS-Chem chemical module (v10-1) within the NASA GEOS Earth System Model (GEOS-5 ESM)” by Lu Hu et al.

Anonymous Referee #2

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Review of GMDD 2018-111

This paper presents an extraordinary and highly valuable piece of work. It is well written and succinct, most unusual given the scope of work involved. It is publishable as is, but given the unique nature of developing a 12.5-km gridded chemistry run, the authors should add just a bit more of information to address some of the obvious questions.

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(1) Comment on the lack of improvement in vertical resolution (L72) while jumping up the horizontal. For example, ECMWF now uses L91 or L137 with high-res models.

(2) You talk of a 300 s heartbeat step across the physics and dynamics, but what is the exchange time with the chemistry and how fast is the chemistry updated for tracer transport? There is some confusion in Sect 3.1 about the chemistry time step.

(3) As one gets to 10 km, there are a number of issues that arise in our standard treatment of the continuity equations. Presumably the large-scale transport adjust u, v, w such that the hybrid coordinate system is maintained when it passes to the chemistry? This creates a problem when doing convection and scavenging of tracers in the chemistry package because convective mass fluxes are balanced by u, v flows – especially when one gets to 10 km scales. If the chemistry package does convective upward fluxes and then balances with downward flow to balance the coordinate system, then there is a false downward transport in the column, and the results will have increasing errors as the resolution gets smaller. This really should be discussed, explained what is actually done (this is not here), and then assess possible errors from the formulation. To be correct one has to have the large-scale and convective w 's done together, since convection involves neighboring columns. For example, some CTMs allow the u, v step to leave the air mass in each layer that may not be according to the hybrid coords and then the convection in the single column corrects it back to the std levels. If you did this typical operator split, then say so. If not, note it.

(4) The approach taken here for assessing the surface ozone (air quality) simulation is very disappointing and this is the only area that needs a substantial redo. First, they build a 12 km model and then go back to the old-fashioned way of correcting for bad simulations of air quality that may be needed in 2 deg models (i.e., only compare with 4 pm values). Further, with G5NR-chem there should be no need to compare only with "background sites" – the polluted regions should be included in the comparisons. (There are reasons to remove of the "traffic sites" in the EU data, but eliminating all urban data gives you the wrong comparison. The idea that you compare with only

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afternoon ozone dates back to models that could not really do realistic surface ozone – this model should be able to compare with the true AQ data and for all hours. You have all the simulated data (since this is save for an OSSE) and so it is straightforward and essential that a more serious and more accurate comparison with surface ozone be done. There are numerous better publications, including multi-model evaluations, please start with one of those. For example, you could be using the Schnell gridded surface ozone and doing the comparisons as in the Schnell 2015 ACP paper Figure 1 – It would be good to see how the seasonal and diurnal cycles differ between the 2 chemistry models. Figures A3 and A5 are very interesting – presumably this includes all 24-hr data?

(5) The concept of tropospheric burden depends a lot on the resolution of the tropopause structures. It would be very useful to discuss (show may be too much here) how the G5NR-Chem model reproduces the folds around (above and below) the jet where the STE takes place. Is the tropospheric mass the same in both chem models at 0.1 and 2.0 degrees? STE flux is a very important model diagnostic, and since you are running Linoz in both models it is trivial to evaluate from the flux needed (presumably archived) to reset the O3strat in the boundary layer. Such a simple monthly, hemispheric value would help greatly in understanding differences between the two models.

(6) Another problem with increased resolution is the need to connect neighboring columns for the radiation and photolysis. In terms of radiation, the direct sunlight in one column often passes through and is scattered or absorbed by a neighboring columns. The overhead ozone column is often not the effective ozone column. The effect of neighboring (unknown) columns is very important for clouds. I doubt that the model is able to considers this effect, but it should be noted as this may be a case where increasing resolution may actually be worse. Related to this, it is also important to described how the Fast-J code treats clouds and aerosols. This later is an easy fix.

(7) The authors have not really shown off the capability of a 12-km global chemistry

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model. Particularly disappointing the comparisons with data from sondes or surface sites where the resolution of the G5NR is dumbed-down to compare with the GEOS-chem reference run at 222 km [I think this is what saw]. It is important to see what the 222 km result from G5NR looks like when comparing with GEOSchem, but then to see the better structures and better match when using 12-km results.

Minor: p.6 l.22: 'a ... OSSEs ...' singular or plural? p.9 l29: what is code availability for G5NR-chem?

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