

Interactive comment on “Resolving ozone vertical gradients in air quality models” by Katherine R. Travis et al.

Anonymous Referee #2

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General comments

I think that the issue that this paper raises, the modeling of vertical gradients of ozone, is interesting and potentially important. However, there are too many shortcomings to the modeling to give a definitive analysis of the issue. An obvious shortcoming is that the lowest model level is 60 m while the measurements are at 10 m. Most other air quality models use much finer grid resolution near the surface. Furthermore, I strongly object to using different PBL schemes for meteorology and chemistry. Not only has the PBL height been reduced for the chemistry but the K_z is also reduced and non-local term eliminated for the proposed correction. This is unjustified. If the same scheme does not give realistic results for both meteorology and chemistry, it should not be used for combined meteorology and air quality modeling. The other corrections and

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adjustments, such as reducing NO_x emissions, that are made in order to get better results also call into question the validity of the study and its findings.

Specific comments

P2 In17-19: The statement: “we showed that the NO_x National Emission Inventory (NEI) from the US Environmental Agency (EPA, 2015) was too high by 30-50 %.” is much too strong. All they should say is that this model with its many demonstrated errors better predicts ozone and NO_x with a 30-40% emission cut. I do not believe that it's been proven that the NEI is over predicting NO_x emissions by this amount.

P3 In6-7: I don't understand how the mixing depth (h) can be defined by Kh values when according to Holtslag and Boville (1993) the Kh is a function of h which is defined by bulk Richardson number. Also, if the mixing depth is 40% too high, then this problem should be diagnosed and fixed. It is not reasonable to “correct” this in Geos-Chem. The result would be inconsistencies between chemical concentration profiles and meteorological profiles which would lead to many errors in the chemical simulation including incorrect advection, errors in temperature and humidity especially above the GEOS-Chem h but below the GEOS h. I think that such “corrections” in AQ modeling should not be acceptable practice.

P3 In20-22: More explanation and justification should be provided for the corrections and adjustments to PBL, emissions, and chemistry. The reader should not have to look up these other papers to know what was done. The mixing height correction needs much better explanation. Even after reading Zhu et al. (2016) it is not clear how or if the Kh values were adjusted after the 40% mixing height correction. The fact that GEOS overpredicts mixing height so much indicates significant errors in surface fluxes or air temperature or winds which could adversely affect the AQ simulation. The emission adjustments are also not sufficiently explained or justified. Over predictions of NO_x concentrations do not necessarily indicate emission over prediction, especially when the meteorology simulation has such large errors. The isoprene corrections are

not explained at all. I think that these adjustments are very questionable. The fact that you get better results is not sufficient justification. It is likely that you are adding errors to compensate for other errors.

P3 In33: More needs to be said than just “simply explained by numerical diffusion”. An AQ model that cannot get the high end of ozone distribution is not very useful.

P4 In 7-10: Were model values for u^* , L , V_d , R_a used in the 10 m calculations or the “typical” values given in the text? In any case, this technique has its limitations and uncertainties that should be noted since ozone is not an inert tracer. The ozone profile between 60m and 10m is affected not only by deposition flux but also chemical reactions with NO_x and VOCs that usually have the opposite gradients from ozone. It would be preferable to run both the meteorology and chemical models at finer vertical resolution near the ground so that the model explicitly simulates 10m concentrations.

P4 In4-18: The large underprediction of cloud cover is another significant deficiency in this model. This, combined with the large overpredictions of mixing height, suggest that meteorology model is not sufficiently realistic for modeling boundary layer air quality.

P7 In1-2: This statement is way too strong considering the other errors in the model system!

P7 In9: Is the grey shading defined by model or observations?

P7 In 12-15: There is asymmetry in the Hotslag&Boville93 scheme for potential temperature. However, how this is applied to chemical concentrations is not explained.

P7 In22-24: Removing the non-local term actually removes the asymmetry which contradicts the findings of Wyngaard and Brost and others.

Section 6: Another possible reason for the poor modeling of the ozone gradient could be that the model underpredicts the ozone concentrations above the mixing layer. On both days the ozone profile increases throughout the PBL and above the PBL. Having high concentration above the PBL to entrain would tend to increase concentrations in

the upper part of the PBL. The greater gradient from the “corrected” GEOS-Chem is simply due to decreased mixing from an arbitrarily reduced K_z profile.

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