Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1220-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Impact of Future Emission Policies on Tropospheric Ozone using a Parameterised Approach" *by* Steven Turnock et al.

Anonymous Referee #2

Received and published: 21 March 2018

The manuscript by Turnock et al. presents an updated version of earlier work done by Wild et al. (2012), who constructed a parameterisation for calculating the response of tropospheric ozone to changes in precursor emissions (and methane abundances). The parameterisation reduces the need to run an ensemble of computationally expensive global models of atmospheric chemistry in order to explore the effects of different emission scenarios on the abundance of tropospheric ozone, and the need to run multiple model experiments to determine the influences of emissions from multiple source regions on individual receptor regions. As such, this is a tool for rapid assessment of alternative emission control policies, but does not replace global atmospheric chemistry models, since it does not represent the nonlinearities of tropospheric ozone chemistry, or the influence of future climate changes on tropospheric ozone. As an update to Wild

et al. (2012), I expect that this paper will be widely used.

Unfortunately I found much of the description of the method to be vague and confusing. This should be improved before the paper is published. Also, the authors could do more to compare the predictions of their parameterised ozone with actual simulations from global atmospheric chemistry models. More details are given below

Page 3, lines 5-7: Ignoring changes in future ozone due to climate change is an important limitation of this study. Here it would be appropriate to give a short summary of the expected changes in surface ozone due to climate change, to provide the reader with more information about this limitation.

Page 3, line 13: There appears to be a typo in the last sentence of this paragraph.

Page 4, line 5: Why is f replaced with g? Is this just a difference in terminology between Wild et al. (2012) and this study? Or something else?

Page 3, line 9: Where does "2f -g" come from? This appears to come out of nowhere.

Page 3, line 10: Much more explanation is needed here. Which model simulations? Which year? Why is the spatial extent of the titration regimes important? How is the magnitude defined? A lot of very important information appears to have simply been left out.

Page 3, line 14: It's fascinating to read that the response of surface ozone to methane, a well-mixed gas with a lifetime measured in years, could be similar to the response to NOx, which has a lifetime on the order of hours. In what way is the response "similar"? More explanation is needed here.

Equation 2: How are the coefficients in the calculation of g determined?

Page 8, line 40: There is no need to mention titration a second time.

Section 3.3.1: Can you compare the predictions of the parameterisation using the RCP scenarios with the actual global model runs done in the ACCMIP exercise? See Young

et al. (2013) for some examples. This comparison would really help the reader to understand more about how well the parameterisation is doing in comparison with the global models. Perhaps this comparison could be added to Fig. 5. Also, why isn't this part of Section 4? ("Future Surface Ozone Predictions")

Page 10, line 8: "successive emission increases" of what magnitude? In what succession?

Equation 3: The coefficients appear to have changed since Equation 2. Why? How are they calculated? This seems like something for Section 2.

Page 10, line 33: What are the "appropriate fields"?

Section 4.1: Can you compare the predictions of the parameterisation using the ECLIPSE scenarios with the actual global model runs done in the ECLIPSE exercise? See Stohl et al. (2015, DOI: 10.5194/acp-15-10529-2015) and work citing that paper for examples.

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