Atmos. Chem. Phys. Discuss., 12, C10898–C10900, 2012
www.atmos-chem-phys-discuss.net/12/C10898/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Semi-empirical models for chlorine activation and ozone depletion in the Antarctic stratosphere: proof of concept" *by* P. E. Huck et al.

Anonymous Referee #2

Received and published: 21 December 2012

This is an enticingly simple and well written contribution. Nevertheless one would wish for a more (self) critical assessment of what earlier models provide and what this model provides. It compares itself to the Cariolle and LINOZ scheme; both models have been developed for a very different purpose compared to the OMD model introduced here (provision of seasonally varying global ozone fields for models versus explaining integrated quantities (e.g. OMD) with reanalysis data). The Cariolle scheme has been complemented with different approaches to mimic polar chemistry, e.g. an idealised tracer mimicking the impact of PSCs on ozone using characteristic time constants (Hadjinicolaou et al., 1997), which makes it suitable for data assimilation (e.g. Eskes et al., 2003) and idealised climate variability studies. A similar context exists for

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LINOZ. Both 'classic' schemes are certainly based on a fundamental understanding of global scale ozone chemistry as well, even though they do the 'fitting' very differently to the model introduced in this paper. Therefore I would suggest a number of changes before publication, which hopefully will make it easier to put the paper into context with existing work and will give a fairer balance of pros and cons for all schemes.

Introduction: What does the introduction mean? Process oriented validation might be helped by a bulk model? In how far reduces the work presented here uncertainty estimates? Isn't the main point that the authors provide a methodology to reconstruct existing timeseries, which may help our understanding of past variability and trends? I am not sure about the abilities of this model to predict future ozone on its own. Please clarify your main point.

Page 28453, line 10-16: Rewrite, mention pro and cons and possible extensions (please see general comment above), including global versus regional aspects of (parameterised) modelling.

Page 28454, line 20-23: Make it clear that the use of a-priory knowledge (e.g. age-of-air spectra) determines to a large amount the modelled result. It is fine to say that global models don't get it right, but to get chemistry right when you put age-of-air in a model as a prior condition is far easier.

Page 28456, line 21-23: Which CCM? Why do you believe the CCM is doing a good job? I would suggest a table for all acronyms to simplify life for the reader: for example FAP, FAS, MAC/sMAC, OMD, S, Fact (p 28458, line 25: What is it precisely?), etc.

Choice of temperature implies choice of water vapour (p. 28455, line 15) – not fixed in models and part of the bias problem! If one can choose the temperature threshold and the age spectrum, one can model the past well – in this respect this paper proofs that our conceptual understanding is correct; unfortunately it does not help to improve this two 'parameters' in a chemistry-climate model (see previous request about clarification how this contribution impacts on uncertainty estimates). Please contrast the reduced

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degrees of freedom in your model system and the far larger number of degrees of freedom in a CCM more clearly. This request relates also to your choice of limiter, because the model doesn't act on ozone but OMD with respect to an empirical minimum value (S=OMD/OMD150), which makes it fairly save to capture extreme depletion events.

Page 28460, line 28460: So why not omit the quadratic term and simplify? How simple could the model be to capture the past with confidence?

Page 28460, line 19-21: How? Wouldn't it be better to monitor the quantities and model the processes? Why would we like to rely on a statistical model?

Page 28461, line 1-2: How would one do this? Where are feedbacks considered (e.g. water vapour, changing age spectra, etc.)? Where would temperatures come from? From models that do not consider ozone fully. Stress lines 5-7 more!

Eskes et al. (2003), QJRMS, 129, p1663.

Hadjinicolaou et al. (1997), GRL, 24, p2993.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28451, 2012.