Reply to 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

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
We do not compare our semi-empirical model to the Cariolle and LINOZ schemes but rather discuss these schemes to provide an historical context for the development of our semi-empirical approach. Our scheme is very different from the Cariolle and LINOZ schemes to the extent that it would not be possible to compare them i.e. they do not have comparable outputs.

; 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 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.
We note that it is not our intention to provide a detailed review and assessment of different parameterization schemes for polar ozone chemistry.

Introduction: What does the introduction mean?
Our intention was to develop models that are simple, yet interactive and based on physical and chemical processes in the stratosphere. In this proof-of-concept paper we introduce a very simplified version of a semi-empirical model for stratospheric ozone depletion.

Process oriented validation might be helped by a bulk model?
Applications such as process oriented validation might require more detailed formulation of the processes. In this paper we aim to highlight the conceptual approach that semi-empirical models based on this example could be developed for validation purposes.

In how far reduces the work presented here uncertainty estimates?
The work presented here does not intend to reduce uncertainty estimates; moreover, it provides a method to evaluate the relative source of uncertainties for ozone projections in the future. For example, with this model presented, ozone projections can be created looking at the full range of temperature simulations run for different greenhouse gas emission scenarios.

Isn't the main point that the authors provide a methodology to reconstruct existing time series, which may help our understanding of past variability and trends?
No, that is not the intended use of this model i.e. its primary use is not intended as a tool for attribution of past changes in ozone.

I am not sure about the abilities of this model to predict future ozone on its own. Please clarify your main point.
Reconstruction of existing time series was only performed to test the ability of the model to project OMD into the future. The reviewer is correct that the model on its own cannot be used for predicting future ozone. Knowledge of temperature and Cl_y is required for the model to project ozone.

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.
The Cariolle and LINOZ schemes are mentioned very much just in passing to provide an historical perspective for the development of our semi-empirical model approach. We are not in a position to be able to provide a review of the advantages and disadvantages of those two schemes, nor is that a goal of this

paper. We have added a few sentences to explain in greater detail the relation between the Cariolle and LINOZ schemes and the semi-empirical model approach described here.

Page 28454, line 20-23: Make it clear that the use of a-priory knowledge (e.g. ageof-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.
Cl_y and temperature are required *a priori* for this model to make OMD projections. This point has been clarified in the revised manuscript.

Page 28456, line 21-23: Which CCM? Why do you believe the CCM is doing a good job?
Two different CCMs (UMETRAC, NIWA-SOCOL) were used to examine the altitude dependence of the fit coefficients. This information is now included in the manuscript. We do not have enough measurement data available to examine this dependence on observations. The fact that the semi-empirical model fit coefficients are altitude independent in two different chemistry-climate models gives us some confidence (though not water tight proof) that equation (1) represents actual processes such that the fit coefficients represent sensitivities applicable across the full height range.

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.
Thank you for that suggestion. A table has been provided in the revised manuscript.

Choice of temperature implies choice of water vapour (p. 28455, line 15)
That is correct, the choice of the threshold temperature for PSC formation includes a specific assumption about water vapour concentrations.

– 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 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.
The semi-empirical model is not meant to compete with CCMs. It can provide uncertainty estimates because it is very fast and inexpensive to run and therefore a spectrum of input scenarios (i.e. different temperature scenarios) can be used by this model to provide a spectrum of ozone projections.

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?
Even though saturation effects lead in reality to a near-linear relationship, we have included the quadratic term because of the known quadratic dependence of ozone depletion on chlorine (Molina catalytic cycle).

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?
Simply what we meant here was that because the fit coefficients capture true sensitivities in the ozone chemistry system, and that these sensitivities appear to be altitude independent, simple experiments can be conducted whereby e.g. perturbed temperature time series are used to drive the semi-empirical model to examine how those perturbed time series would propagate through to a perturbation in the ozone mass deficit.

Page 28461, line 1-2: How would one do this?
One possible way to do this would be as follows: To use climate pattern-scaling approaches (e.g. Mitchell, T. D. (2003), Pattern scaling: An Examination of the Accuracy of the Technique for Describing Future Climates, Climatic Change, 60, 217-242) to generate stratospheric temperature fields from prescribed greenhouse gas emissions scenarios and an *a priori* ozone field. These would provide first guess temperature fields to the semi-empirical model. The effects of feedbacks between temperature and ozone would then be captured by using the semi-empirical model to update the ozone field and the temperature field. After some iterations convergence would be achieved such that there was internal consistency between the ozone and temperature fields. A stepwise approach through the years would generate ozone

projections for prescribed GHG emissions scenarios. We have not added this detail to the manuscript since it is untested and well outside the scope of the work presented in the paper.

Where are feedbacks considered
The way in which the feedback between temperature and ozone would be dealt with is discussed above.

(e.g.
water vapour, changing age spectra, etc.)?
The current construction of our semi-empirical model does not permit us to consider feedbacks between water vapour, changing age of air spectra etc.. These are considered to be second order effects that result in the model not explaining all of the unforced variability, but it is not our intention for the model to explain all sources of variability.

Where would temperatures come from? From models that do not consider ozone fully.
The stratospheric temperature fields could either come from an AOGCM (perhaps with prescribed ozone) or using a climate pattern-scaling approach as discussed above.

Stress lines 5-7 more!
We have added a sentence here in the revised manuscript.

Eskes et al. (2003), QJRMS, 129, p1663.
Hadjinicolaou et al. (1997), GRL, 24, p2993.