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Interactive comment on "ICON-ART 2.1 – A flexible tracer framework and its application for composition studies in numerical weather forecasting and climate simulations" *by* Jennifer Schröter et al.

Anonymous Referee #1

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The manuscript presents a number of unique features of the implementation of tracers in the ICON-ART model, including a flexible method of specifying tracers and their properties in a separate XML file, that is made possible by the use of object oriented programming in Fortran. In addition to the description of the implementation, results from a short simulation of the 2002 Antarctic vortex split using a simple stratospheric chemistry scheme and the Linoz parameterized ozone is presented. Results from a 30-year simulation are also discussed, comparing a simulation with interactive ozone using the Linoz parameterization with a focus on the impacts on dynamical variables

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such as winds, temperature and age of air.

While I am sympathetic to the goals of model documentation supported by GMD, I find Sections 2.1 through 2.3, and particularly section 2.2.1, to be much too technically oriented – to the point of seemingly like a users guide rather than a scientific article. While I believe the discussion of particular aspects of object oriented programming in Fortran 2003 and the ways this can be linked with XML to control model configuration would find an audience of interested readers, the detailed discussion of the XML input files seems much too specific. I would strongly urge the authors to find a way to more generally discuss the way the model has been structured and the advantages you find using XML with Fortran 2003 and remove some of the specific examples of code that are included. As discussed in the particular comments below, there are also a number of areas where the discussion of the model results does not seem clear and this should be improved. With a few improvements the manuscript could provide a nice example of a modern approach to numerical model design and illustrate the flexibility of the modelling infrastructure with a couple of examples.

Page 2, Lines 2 – 7: Nothing scientific in this comment at all: the first paragraph has two completely separate ideas stuck together (non-hydrostatic and seamless prediction) and is a very difficult start for the reader.

Page 2, Lines 31-32: I might suggest changing the text from 'field, like large eddy simulations, numerical weather predictions and climate simulations ICON-ART can run with the different existing physics configurations.' to 'field, like large eddy simulations, numerical weather predictions or climate simulations, ICON-ART can run with different existing physics configurations.'

Page 4 Lines 6-7: Should the phrase '...not have any impact on other tracers of the (thermo-)dynamical of the simulated system.' be '... not have any impact on other tracers or the (thermo-)dynamics of the simulated system.'

Page 13, Lines 5-16: Age of air derived from a linearly increasing tracer is fairly widely

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used and I do not think all the details are required of the implementation. But I would also note that equation (4) is a bit difficult to understand. Is the first term on the right-hand side (7 x 86400.0 x 365.2425) the initial value assigned to the age tracer everywhere in the model?

Page 15, Lines 5 – 12: Looking at Figure 4, there is considerable ozone production (negative loss) occurring in the Linoz simulation and the authors note this here: ‘Inside the polar vortex, on 1 October 2002, we model negative ozone loss for both simulations. The chemical tracer in both simulations is increased with respect to the passive one. The increase is higher in the Linoz simulation than in the extended Chapman cycle. This implies that temperatures in that region are not low enough to trigger the heterogeneous destruction of ozone in the Linoz scheme. Outside the polar vortex, mainly on 25 September, high values of ozone loss can be observed for the Linoz simulation but not for the extended Chapman cycle. This is also caused by the difference in addressing heterogeneous destruction. Within the Linoz scheme, the loss term has been triggered and we can observe additional ozone loss. This feature is missing for the extended Chapman cycle chemistry.’ From Figure 4, the production in the Chapman chemistry seems to be quite less, if not close to zero, but Figure 3 shows that total column ozone within the vortex in the Linoz simulation is actually considerably lower than in the Chapman simulation. I admit one is total column and the other is ozone at 50 hPa, but the differences in the chemical change in ozone at 50 hPa do not seem to agree with the differences in total column ozone in the two simulations. Can the authors provide some reasons why total column ozone inside the vortex is lower while chemical change at 50 hPa suggests it should be higher? I am also curious about the ozone loss that occurs outside the vortex in the Linoz simulation and that is mentioned in this section as well. Is it possible to add the 195K isotherm on to Figure 4 to help the reader unambiguously see where the PSC-chemistry parameterization is active? While I can imagine some filaments of the original vortex having temperatures below 195K, I am curious why the two lobes of the original vortex are not cold enough to activate the ozone loss parameterization.

Page 17, Lines 3-4: For the control simulation that uses specified ozone from Cionni et al. (2011) in the radiation, what water vapour is used for radiation? Is it the water vapour that crosses the tropopause, but with no addition from methane oxidation?

Page 19, Lines 9 – 14: The standard deviation of ozone is discussed here and the point that the variability in the Cionni et al. dataset shows a pattern very similar to the ozone contours themselves is made. The Cionni et al. ozone dataset was made using a standard climatological ozone with a temporal trend given by regression to an idealized reactive chlorine (Cly). I think what you are showing in the standard deviation is just the long-term trend in ozone itself – the evolution of the trend around the long-term mean for the 1980 – 2009 period. While the Linoz ozone actually has year-to-year variability but as it is constructed I don't think it has any long-term trend. This comparison does not seem very valid. And that brings up another point that since Linoz is designed for a particular time period, particularly the PSC parameterization, is it correct to use it over the full period from 1980 when chlorine loading will have evolved considerably? I might suggest restricting the comparison to 1990-2009 when the effects of ODSs are more fully realized.

Page 20, Line 5: 'In the southern hemisphere winter, ICON-ART reaches temperatures of about 200 K.' This seems to be like a significant warm bias that would have serious impacts on PSC processes. How does this statement agree with the findings shown in Figure 9 that the feedback simulation seems to be biased cold compared to ERA-Interim?

Page 20, Lines 7-8: Is the comparison reversed in the statement 'In the vertical region around 50 hPa, the difference between ERA-Interim and the feedback simulation is about 15 K to 20 K in the tropics and below 8 K in the southern hemisphere.'

Page 22, Lines 2-3: On the comparison of zonal winds to ERA-Interim the authors state 'Here, the ERA-Interim shows values up to 25 m s−1 higher than in the ICON-ART simulation. Between 30âŮę N and 60âŮę N latitude above 20 hPa the sign of

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the differences changes. Here, we observe stronger zonal wind speeds than in ERA-Interim.' The finding of stronger zonal winds, particularly around the southern hemisphere stratospheric jet, does not seem to fit with the cold temperatures found the ICON-ART feedback simulation that are shown in Figure 9. I think the negative difference means the winds in ICON-ART are stronger.

Page 23 – the caption on Figure 10 does not seem to match with what is shown in Figure 10.

Page 24, Lines 2-3: It is difficult to see the difference in water vapour entering the stratosphere that is mentioned by 'Due to lower tropical tropopause temperatures in the feedback simulation, less water can enter the lower stratosphere.' because Figure 12 is the monthly anomaly to the annual mean. A quick mention of the difference in the annual mean in the lower stratosphere would help.

Page 25, Figure 12 – Do you have any idea why the maximum in the annual cycle in the feedback simulation around 80 hPa is so broad, stretching from May to October. The broad annual cycle in the bottom right corner of the feedback plot is also difficult to reconcile with the very narrow maximum in the top left corner.

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