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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial, temporal, and vertical variability of polar stratospheric ozone loss in the Arctic winters 2004/05–2009/10" *by* J. Kuttippurath et al.

Anonymous Referee #2

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Review of "Spatial, temporal, and vertical variability of polar stratospheric ozone loss in the Arctic winters 2004/05–2009/10" by Kuttippurath et al.

The paper presents a comprehensive overview over Arctic stratospheric ozone losses for the most recent five winters. Losses are derived from observations and a lagrangian CTM is used to model the evolution of the chemistry through these winters. It's main strength is its comprehensiveness and detailed analysis of chemical mechanisms in a very quantitative way. This makes the paper a valuable contribution although nothing really new is reported, besides giving an update to the record of ozone loss quantifications for the recent years.

The approach is sound (with the exception of one specific issue I have with the initialisation, see below) and the paper is well written and organised. The figures are all clear and well selected. The paper should clearly been published in ACP after some revisions.

page 14678, line 15: Do the particles sediment and if so, how?

page 14680, line 26: To me the peak loss appears to be substantially larger in 2005 (dark red color, exceeding 1.6ppm) than that in 2010 (only yellowish color, barely exceeding 1.2). So I can't follow the statements in these lines.

page 14681, line 14: Any bias between ECMWF ozone and MLS ozone will show up as ozone loss or production in this calculation. Because this is so, it is crucial to assess the differences between the initialistion fields for ozone and the MLS fields at that time very carefully. If the differences are negligible the analysis is fine but that has to be shown. Note that negligible here means very small compared to the calculated ozone losses, which for many winters and altitudes are in the order of 10% or so. I.e. the difference between the initialistaion field and MLS ozone needs to be below one percent or so, to be smaller than 10% of the ozone loss signal. If the differences are not negligible a correction needs to be applied. Actually Figure 3 shows that in most winters "ozone loss" is already present right at the beginning of the simulation, which is particularly large in 2009/2010. This apparent loss is due to the bias between ECMWF ozone and MLS and the calculations need to be corrected for that. It is important that such a correction considers the downward transport during the winter. So just substracting the profiles of initial ozone differences will not work.

page 14681, line 21: The "large losses" in the observations are partly caused by the bias issue described above. This is particularly pronounced in 2010 and that explains why the model / observation differences are so large for that winter: The "observations" largely overestimate the loss during this winter. Note that the initial "loss" (which is just the bias) is in the order of 0.8 ppmv (the light blue color at the left edge of the lower left

panel of Figure 3). That initial offset propagates down with the subsidence inside the vortex and explains about half of the "observed loss" at the end of the winter.

page 14682, line 8: Since the absolute differences in ozone, particularly those right at the beginning of the calculation, are really important, a plot of the differences should be shown. Otherwise it is hard to see the differences due to the strong vertical gradient of ozone.

Sections 5.2.4. and 6.3.1 I generally agree with the comments by Jens-Uwe Groß about comparisons between ozone loss results from various techniques. But Doing a thorough comparison with any one of the previous studies here would go beyond the scope of the paper and would be a study of its own. I think clearly mentioning the caveats and otherwise leave the comparisons unchanged should be sufficient. I just can't see any other practical solution here.

Page 14686, lines 10-15: Comparing ozone loss rates in terms of loss per sunlit hour is very tricky. It depends a lot on using exactly the same definition for what a "sunlit hour" is. Stricter definitions (e.g. those based on a smaller sza cutoff) result in substantially larger ozone loss rates. Changing the cutoff from 95 deg to 90 deg can change the derived ozone loss rates in terms of these different sunlit hours by nearly a factor of two under some conditions. So some words should be said on the definition of a sunlit hour in this work and care should be taken that this definition is in agreement with that used in Frieler et al..

Page 14690, lines 2-7: Hm, since the ClO-O cycle (which is a halogen catalyzed cycle) is the second most important cycle at these altitudes and contributes up to 20-55% I think saying that "the halogen catalysed cycles play little role" is somewhat misleading.

Page 14691, line 16: Calling 350-850K the total column is a bit sloppy.

Table 3: The date for which the estimate is given should also be given in the table, particularly because it is so different for the von Hobe et al. estimate. Also major

deviations in the domain and time covered could perhaps be printed in bold or italics, to make reasons for deviating loss estimates more obvious. This would include 400K as lower limit in Singleton et al., 460K as upper limit in von Hobe et al. and 7 March as date in von Hobe et al.

Page 14694, line 7: I can't see the basis for this statement. Feng et al. reported larger losses for the partial column. Also Rex et al. note that in 2005 a substantial part of the column loss took place in the lowest part of the 350-550K region, i.e. below 400K and also significant contributions from below 380K. That region is not included in the studies that report smaller values for the partial column, with the only exception of the present work.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14675, 2010.