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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Antarctic ozone loss in 1989–2010: evidence for ozone recovery?" *by* J. Kuttippurath et al.

Anonymous Referee #2

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The manuscript presents a decadal time series of the polar ozone losses in the Antarctic winter/spring obtained using the passive tracer method. Analysis utilizes data from 12 ground based stations and several satellite instruments. The work is in essence an extension of their previous paper "Estimation of Antarctic ozone loss from ground-based total column measurements" by J. Kuttippurath, F. Goutail, J.-P. Pommereau, F. Lefèvre, H. K. Roscoe, A. Pazmiño, W. Feng, M. P. Chipperfield, and S. Godin-Beekmann; Atmos. Chem. Phys., 10, 6569-6581, 2010, which was limited to the years (2005-2009). The extension to decadal time scale improves the details of the annual variability and gives better picture on timing of the beginning, peaking and ending of the polar loss periods including useful visualizations and gives hints for the recovery discussion. The trend analysis included in this work is also an extension compared with the predecessor paper. They use by now already standard statistical model which

applies so called explanatory variables that help in attributing the observed changes to physical and chemical causes. This type of statistical models has been extensively used since late 2000's for global, NH and Arctic data. For the Antarctic research they have not been used regularly because the dynamical proxies that work so well in NH and Arctic have smaller role in a stable and extremely cold Antarctic stratosphere. To get a better insight to the ozone layer recovery issue they use a piece wise linear trend estimator or EESC for spring time total ozone data with tipping point at 1997. The third element of the of the work includes speculation of whether or not and when "the first signs of recovery" can be seen from the Antarctic ozone data analyses.

The Antarctic ozone is of course one of the most widely covered subject in geophysics and consequently, it is very difficult to find and present new aspects on it. The main novelty here is the extension of Antarctic ozone loss time series to two decades and clearly the work advances the knowledge on the details of polar ozone losses time series. On the other hand, general conclusions are quite as expected from the previous literature. Structurally, the manuscript organization is well done and description of the methods sufficient as well as referencing to previous literature. I leave the specific comments on the use of English language to specialists or native speakers but in my mind rephrasing expressions and changing wording at places would improve the readability. In addition to language improvements I would like to draw attention (at least) to the following points in case the manuscript will be published.

Detailled comments:

Personnally ,I do not like the abbreviation GB (= gigabyte). "ground based" is short enough expression to write.

At the bottom of page 4 reference should be to(Kuttippurrath 2010 a) and not 2010 b (which deal with Arctic loss profiles).

On the top of page 5 ; the Brewer instrument precision 0.5 % is unrealistic. Use later more realistic references to Brewer accuracy in the field work. In polar monitoring it

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is also essential to mention the model Brewer Mk II , Brewer Mk III, etc, which should be readily available from the data source, since the performance in high SZA is significantly different in single and double monochromator versions.

Ch 2.2 is devoted to satellite instruments which are listed together with their accuracies and biases, algorithm versions, time periods and other details. A table would help following and shortening the discussion here.

page.8 ,ch 2.4, 2nd sentence on the initialization of the ozone field I did not quite catch, could this be re-phrased, please. Also note that in here you speak about difference between passive tracer and measured ozone , (in this order), but in following parenthesis vice verca: " (ground based ozone – model tracer)". You should really clearly define the concept of loss because your sign convention seems to vary (in the table positive and in the figures negative, in the text usually positive, this should be unified. The same with loss rates.

Page 10 , 5th row from bottom ".. GB ozone is shown to be about 32 %..". Should be probably ozone loss and I would prefer minus sign when referring to the loss.

Page 15. 5th row from bottom; ".. show about 130-145 DU." Is about

Page 17, A personal view again but I would prefer subscripts over superscripts in coefficients of the trend model (because the superscript represents usually exponent).

Page 19 top : "...the ozone reduction in the Antarctic dominates the halogen loading .." Shouldn't this be vice verca?

Chapter 4.4.3

Page 19. Here again the sign convention is confusing and this time in speaking of trends from different sources. Check and unify.

Page 19 and 20 . Somewhat confusing that first on page 19 last paragraph starting at the 2nd row they state that "...are significant at 85% confidence intervals but not

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significant at 95% confidence intervals." but next on page 20; "..our diagnosis yield a positive trend at 95% confidence intervals.." Actually the whole discussion here with layered (i)-structure should be written more clearly.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10775, 2012.