

Interactive comment on “A long term study of polar ozone loss derived from data assimilation of Odin/SMR observations” by Kazutoshi Sagi and Donal Murtagh

Anonymous Referee #2

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General comments

In their manuscript, Sagi and Murtagh present assimilated ODIN SMR observations to derive Arctic and Antarctic stratospheric chemical ozone loss for the period 2002 to 2013. Monitoring polar stratospheric ozone and attributing changes to chemistry and transport is an important topic within the scope of Atmos. Chem. Phys. By providing a decadal perspective using a sophisticated assimilation scheme this study significantly adds to what is known so far. I suggest publication in Atmos. Chem. Phys. after consideration of some comments given below.

The manuscript is generally well written, but contains a number of typos and slightly awkward phrases. I have indicated some suggested correction but in addition suggest

C1

that the authors carefully double-check before submission of the revised manuscript.

The outline of the manuscript can be slightly improved in two ways: (a) the abstract is too long it should be focused on the new and most important findings. (b) I suggest to move the discussion of ozone depletion in subsections 4.4.1-4.4.3 into a new section 5.

Specific comments

p1, l14: Not fully clear why you primarily compare to Sonkaew et al.? Are SCIAMACHY data particularly useful here or is this one of a few studies that span many years?

p1, l16: "544GHz ozone loss" is jargon to be avoided. Better write "Ozone loss derived from the 544GHz measurements..." or similar.

p1, l17: which year does this refer to?

p1, l20 and following: You name three classes of chemical ozone loss in the Northern Hemisphere, but the following sentences do not clearly state what these three classes are. There is "cold type loss" in cold winters with maximum loss below 500K, "warm winter loss" at 700K, but the third class is not mentioned. Instead the discussion is on the SH. Please state this more clearly.

p2, l16: Winter 2010/11 was cold, but was it really "obviously different from other Arctic winters"?

p3, l3: suggestion: "Konopka et al. (2007) showed for the Northern Hemisphere winter 2002/2003 that above 600K ..."

p4, l1: This study uses ECMWF operational analyses. Can you briefly motivate why operational analyses rather than ERA-Interim re-analyses have been used here? For a multi-year study ERA-Interim may be more appropriate?

p.4, l2: "normally" -> "approximately" (?)

C2

Figures 1 and 2: Wouldn't it be better to have the two channels shown for the same ozone profile? What exactly do the error bars for the ozone profile contain? Only the the statistical error?

p6, l32: The reference to Lait (1994) should be for modified PV, the reference for the vortex edge should be Nash et al. (1996).

p8, l11: Are the different averaging kernels between 544 and 501GHz considered in the assimilation process, in addition to the measurement precision?

p9, l7: The sentence "Thus, ..." does not refer to the previous sentences, as one may expect, but introduces the conclusion to use only 544GHz. This should be better stated explicitly.

p10, l15: It makes a lot of sense to consider the period 1 December to 14 March, but the argument that this is the best to compare with the other studies starting at 1 January does not make much sense.

p12, l5: "We do not find any clear problem due to the assimilation technique": Maybe you want to phrase more positively?

p15, l11: Another study that may be referred to here (and the discussion on p16) is Kieseewetter et al. (2010) who used assimilation of SBUV data to investigate inter annual variations of Arctic and Antarctic ozone in the mid- and lower stratosphere: Kieseewetter et al., A long-term stratospheric ozone data set from assimilation of satellite observations: High-latitude ozone anomalies, J. Geophys. Res., 115, D10307, doi:10.1029/2009JD013362.

p15, l4: "although similar": similar to what?

p16, l8: what exactly does it mean to "neglect the direct effect of solar radiation"?

p18: I suggest to better separate the discussion of chemical ozone loss and long-term changes due to a possible acceleration of the BDC.

C3

Table 1: The maximum loss of -59DU by 28 March 2011 is much smaller than the inferred loss of almost 120 DU as given by Sinnhuber et al. (2011) or other studies. Can you comment?

p19, l6: an additional explanation may be that by inspection of Fig 8 the loss in 2002 occurs at low altitudes where the density is larger. Would be good if you could give a quantitative explanation (e.g. by providing partial column losses over different altitude regions). In particular for the following sentence "Another reason could be an error in the vertical descent...!"

p19, l8: This is the first time that heating rates from SLIMCAT are mentioned. Should of course be introduced in Section 2. (Reference needed!) The discussion of the uncertainties due to heating rate interpolation should be expanded and maybe moved to Section 2.

p20, l1: If the regression slope makes less sense, I suggest to remove and just use mean ozone as a reference. These details detract here from the main points.

p23, l7: The largest uncertainty is probably due to the still short time period?

Minor corrections

Throughout the manuscript the usage of "the" in many places does not follow standard practice. Sometimes "the" should be omitted, sometimes it is missing. I suggest that a native speaker carefully checks again for a revised version of the manuscript.

p1, l12: "Hemispheremore" -> "Hemisphere more"; "50% of the difference found": do you mean: "a difference of 50% found"? ; "parts per million volume" -> "parts per million by volume"

p1, l15 "using a SCanning..." > "using the SCanning..."

p3, l7: remove redundant phrase "This paper contains the following sections."

p3, l32: "quality of the transport is preserved": maybe better say "quality of the transport

C4

is improved" or "tracer gradients are better preserved"?

p11, l25: Something wrong with the PV units. Typically 1 PV unit = 10^{-6} K m²/kg/s, so Kelvin seems to be missing here?

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