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Interactive comment on "UV radiation below an Arctic vortex with severe ozone depletion" *by* B. M. Knudsen et al.

B. M. Knudsen et al.

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In the following the referee comments are repeated in bold.

General comment to both referees:

The main focus of the paper is neither UV model validation, nor to discuss UV measurements. It is to quantify the clear sky UV increases underneath a severely depleted vortex. This has never been done to our knowledge.

Referee 1

This manuscript discusses the effect of ozone depletion in the Arctic vortex on levels of UV irradiance during springtime, particularly April when the solar elevation is sufficient to provide appreciable UV. The effect of the vortex is modelled

using ozone data as the only variable from year to year, for clear sky days. The model is validated by comparison with UV measurements made in Tromso, which give excellent agreement considering the uncertainty inherent in the measurements, and model input data. However, the manuscript somehow loses focus by presenting only clear sky data yet discussing it alongside cloud information from every year. The most interesting results - the relative effects of cloud and ozone at these latitudes, have not been addressed.

We only present clear sky data because the analyzed cloud cover used here has certain shortcomings, and it is of interest to know how large an increase in UV can be expected underneath a severely depleted vortex in case of clear skies since UV-protection (e.g. sunscreen) might be necessary. The latter is now written in the paper.

There are several points that need to be clarified:

Abstract and Introduction - both imply work with radiation on vertical surfaces, yet the calculations and measurements in the paper are both for horizontal surfaces. Clarify eg P4681 l3 clear sky UV fields of erythemal irradiance on a horizontal surface. In addition, the manuscript does not make any comparison with summer levels in the body of the text. Remove summer levels and vertical surface comments from the abstract as they are misleading.

Done

L21 - Put this in context - what difference would 7DU make to the UV?

Done

P4682 l9 Definition of the UVI - the definition given is for the erythemally effective irradiance. The following sentence explains how this is then converted into a UVI - the two are not equivalent. Rewrite.

Done

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P4684 l21 There is a discussion about cloud that begins the results section, and figs 1, 2 and 5 both all indicate cloud cover. Please remind the reader that all UV data is calculated for clear sky conditions only. This is one suggestion for the place where this might be done. Were the calculations made for every day, regardless of cloud indications, for the reference years? Please clarify.

Done. In all Figure captions 'clear sky' is added where relevant.

Figure 5 This can be misleading at first glance. Panels a,b,c might be taken to imply that cloud is as important as ozone in determining absolute levels of UV (as indeed it is). However, the UV values are all calculated for clear skies (ozone-only dependent) and the high absolute UV (the UVI) in 1996 is due to the southerly extension of the vortex. Once again the presentation (and discussion) of cloud data alongside clear sky model output is confusing. What would be most instructive is the absolute UV under the cloudier skies of 1982 and 1997 compared to the clearer 1996.

We have added clear sky in the figure caption where relevant. The absolute UV would be very dependent on chance fluctuations in cloud cover.

P4685 l11,17 Clarify at these points, and in the captions for figures 6 and 7, that the vortex moves and at any location only the days when the location was inside / outside the vortex are considered in the analysis. Otherwise one might expect fig 7 to cover only the white part of fig 6b.

This has been clarified in the text at line 11.

Minor points: Fig 1 caption: squared should be squares Figs 2,4 It is not always easy to identify the black edge of the vortex as latitude and land lines are the same colour - could they be changed to white or some other hue? Fig 6 Both parts are labelled b.

The edge of the vortex is several times thicker than the land contours and latitude lines

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and we find them clearly distinguishable both in the print and online version. White could not be used since it is already in use. We think it is best to keep the figure as it is. All other suggested changes have been done.

Referee 2

With some exceptions the manuscript is well written and the methods used are up to date. However, the findings are not very new. This is not well discussed and regarded in this manuscript. Most of the results are based on model results that confirm earlier findings, for which the references are missing (e.g. to be found in WMO ozone assessments). The new experimental results are sparse and they are not well related to the rest of the manuscript. The purpose of their presentation does not become clear for the reader: The UVSPEC model has been validated with more suitable spectroradiometric measurements and the validation has been extensively described in the scientific literature. If the purpose of the presentation is the introduction of this type of measurement, then more detail should be given on the experimental setup.

See general comment above. In the introduction references to UV increases in the Arctic are now given: (WMO, 1999; Herman et al., 1996). A reference is now given to validation papers: (see Mayer and Kylling, 2005). If further references are needed please give exact references: which assessment and what page number.

The last statement of the abstract is not scientifically valid. The authors seem to assume that all dynamical changes are non-anthropogenic. This is not justified. On the contrary, climate changes are considered to cause changes in the dynamical behaviour of the atmosphere. This may also be the case for the occurance of ozone mini-holes. It is therefore strongly suggested to remove the last statement of the abstract.

We refer here to the dynamical contribution in 1982, which must be non-anthropogenic. The sentence has been rephrased. Further we have added in the description of Figure

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5: The UV increase in 1982 is our best estimate of the maximum non-anthropogenic dynamical contribution to the increases in 1996 and 1997. In 1996 and 1997 there may be a larger dynamical contribution than in 1982, but this could be due to anthropogenic influence.

Introduction: The authors state: "The worst effect of ozone depletion is arguably to be below a severely depleted vortex during springtime, where substantially elevated levels of UV radiation are expected. This can happen in Antarctica and southern parts of South America (Pazmiño et al., 2004) and in the Northern Hemisphere (NH) north of about 50 N.". This statement is very questionable, both with respect to the biological impact (which might be higher for lower sza) and with respect to atmospheric sciences (why should there be a limit at 50 N?). It is suggested to remove these statements or give references.

We did write 'arguably' since the truth of this statement depends on the viewpoint. It is true that a person staying at a certain location will experience the largest possible UV increase relative to climatology under clear sky conditions when a severely depleted vortex passes. We have changed the sentence by writing high northern latitudes instead of north of 50N.

Page 4681, first line: the argumentation concerning the eye is misleading. For the eye the radiance from certain directions is the relevant quantity, not the irradiance on a vertical plane. Furthermore vertical planes are not used any more in the following.

This is written in the ACIA report with references to 3 papers. Please give references to the contrary if possible.

Page 4681, line 4: there is a newer reference for UVSPEC available with Kylling as a coauthor

This is now included

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Page 4681, line 11: the albedo can be increased up to 100%, not only 95%

According to Tanskanen (2004) the increase is up to 95%

Page 4682, line 14: why should the UV with sza greater than 75 be harmless? There is no known biological lower limit, and such radiation might still be relevant at high latitudes.

In our calculations it is necessary to set a limit to avoid biologically less relevant UV changes to overshadow biologically highly relevant UV changes

Page 4682, line 27: there is no reason given why an albedo of 36-44% was used. Actually it might be too low in the UV, which might also be one of the causes for the small discrepancy of 6% between measurement and model results.

This albedo comes from both the climatology and ECMWF - the sentence is rephrased

Page 4686, line 2: the finding that UV is increased in the Arctic is not new. Please give references (e.g. as summerized in EC reviews or WMO ozone assessment).

The quantification of the UV increases underneath a depleted vortex is new to our knowledge. In the introduction references to UV increases in the Arctic are now given (WMO, 1999; Herman et al., 1996). If more references are needed please specify exact references (which assessments and page numbers)

Page 4686, line 5: the impact on mid-latitudes is not a theory, but has been observed already. The results are published, e.g. in Seckmeyer et al.: New Maximum UV Irradiance Levels Observed in Central Europe, Atmospheric Environment, 1997.

We were thinking about dilution of mid-latitude ozone after vortex break-up. We have added a reference to the Seckmeyer paper together with a sentence describing it.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4679, 2005.