Atmos. Chem. Phys. Discuss., 6, S2017–S2019, 2006
www.atmos-chem-phys-discuss.net/6/S2017/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Polar tropospheric ozone depletion events observed in IGY" *by* H. K. Roscoe and J. Roscoe

H. K. Roscoe and J. Roscoe

Received and published: 21 July 2006

We have now revised our manuscript in the light of comments by Jan Bottenheim as Referee, by Lars Kaleschke intended as a Referee, and the Editor.

Both Kaleschke and Bottenheim felt we should consider splitting the paper into two, one concerning the measurements, and the other concerning the interpretation of the midwinter ozone loss in terms of the dark reaction with major additional material concerning other apparent dark depletion events. We are reluctant to do so because the lack of explanation of the midwinter ozone loss would call into question the whole data set in the minds of some readers. When we first brought the data set including winter ozone loss to the attention of Andy Rankin he commented that the 1958 workers had obviously made a serious error, and Eric Wolff was also doubtful about the data. It is essential to make the case for the dark reaction to convince such sceptics, who might

otherwise discount the whole data set, not just the winter part.

Note that the ozone loss in midwinter at Halley (76°S) was almost total for a long period (less than 4 ppbv for over 7 days, less than 2 ppbv for 1.5 days), when winds were often so light that there was negligible chance of such ozone-poor air having originated from the nearest sunlit regions of the Weddell Sea over 1000 km to the north. Hence it is essential to invoke the dark reaction to provide an explanation.

This is by contrast to the episode at Neumayer (71°S) in June 1982 mentioned by Kaleschke. Then, ozone fell to 4 ppbv for only one hourly-mean point, mostly hovering near 8 ppbv (links from http://gaw.kishou.go.jp/wdcgg.html). These amounts would be typical of air that has suffered near-total ozone depletion, then travelled some distance being diluted by neighbouring ozone-rich air - as at Halley in August 1958. Maps of potential frost flowers in June 1982, kindly transmitted by Kaleschke, show significant areas of potential frost flowers in sunlit areas of the Weddell Sea in June 1982, less than 500 km north of Neumayer. Hence it is not essential to invoke the dark reaction to explain the Neumayer data. The same is true of other years at Neumayer, where there is one isolated day of 8 ppbv in late July 1984, other isolated days of 4 ppbv in late July 1986 and early July 1992, and no more.

The website http://gaw.kishou.go.jp/wdcgg.html shows no daily values below 20 ppbv in winter at McMurdo (78°S), whereas there are frequent ozone-loss episodes below 5 ppbv for one or more days in September, as at Halley in 1958. At Syowa (69°S), the only other coastal Antarctic site measuring surface ozone, there are no daily values below 15 ppbv in any June or July. Hence we beg to differ from Bottenheim - coastal stations in Antarctica do not show lots of depletion events in the dark, merely the few above at Neumayer.

However, the Referees are correct that a serious study of the Neumayer events of winter ozone loss is overdue, preferably with complete trajectory analyses and modelling studies. CHABLIS data at Halley would naturally help. But these are data sets whose

ACPD
6, S2017–S2019, 2006

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validity even the most sceptical reader would not challenge. We say again, it is essential to discuss the dark reaction at Halley in 1958 or some readers would dismiss the whole data set as erroneous. The Referees may not do so, but there are others in the scientific community who would - hence the need for our phrases “We should not discount the results merely because it was over 40 years ago”, etc.

Kaleschke comments that sea ice extent was greater in the 1950s and so there would be less open water. However, there is not necessarily a correlation of less open water with greater sea ice extent, only with colder surface temperatures, and they were not colder in the late1950s at Halley. Furthermore there were vibrant colonies of Emperor Penguins near Halley and at Precious Bay in the late 1950s, so open water must have existed nearby in midwinter. Finally, the wind conditions in early July 1958 make it almost impossible that such a sustained and near-total ozone loss could have arisen over 1000 km to the north, undiluted in transit.

Bottenheim corrects us that the early suggestions of a role for pollution in Arctic ozone depletion events involved reactions on sulphuric acid aerosol, not man-made NOx, and we have revised the paper accordingly.

Finally, in response to a comment by Kaleschke and by the Editor, the title is revised; in response to comments by the Editor, various minor technical corrections are implemented, and the discussion of the salinity of brine surrounding frost flowers on p3640 is clarified; and in response to comments by Bottenheim, gas phase and heterogeneous reactions are distinguished and the statement about fitting factors is now clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3627, 2006.