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Interactive Comment

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

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

A relatively new trend has been established over the last few years to dig into archives and look for early measurements of ozone. The reason is obvious, any sort of climatology requires a long term dataset, and "modern" measurements of ozone, especially those obtained with the now universally accepted spectroscopic methods, did not come about until the 1970s. This paper is a welcome addition, reporting on early measurements of ozone during the IGY in 1957/58 at Halley Station in the Antarctic. The authors present a quite extensive analysis on methods that were used, and in particular investigate potential problems that may raise doubts about the validity of the data.

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What they end up with is this: (1) ozone may have been lower in 1957/58 then nowadays. However, the difference is smaller than found elsewhere, and may just be within the uncertainty of the data. Hence no solid conclusions can be reached from these observations. (2) ozone depletions during austral spring are clearly observed. They conclude that ozone depletions therefore should be clearly a natural process driven by halogen chemistry. They go on by suggesting that if these data had not been forgotten then early speculation that ozone depletion might be partly pollution driven, especially by NOx chemistry, would never have been made. Here I have to disagree. The arguments for a role of anthropogenic pollution are based on speculation that Arctic (acidic!) haze may offer a particularly effective surface for the heterogeneous bromine activation. While there is growing consensus that the bromine activation is largely surface based, in the Arctic it is quite conceivable that Arctic Haze plays an additional role. I do agree that a role for NOx is difficult to see, and in fact it has not been considered very likely from the start. (3) a case of ozone depletion in mid July is observed. Here the authors go into a lengthy discussion to argue that these data are in fact valid. I have my doubts on some of the arguments. But more to the point, as indicated by the comments of Kaleschke to this paper, winter episodes in Polar regions are not uncommon. A quick look at the GAW database (http://gaw.kishou.go.jp/wdcgg.html) shows that they occur at all Antarctic coastal stations, and virtually every year (to some extent this is a matter of definition: how much of an ozone loss constitutes ozone depletion? A clear deviation of something like 10 ppbv from what is normally a very stable signal does qualify in my opinion). And ozone depletions in mid winter are also observed in the Arctic, although here they are mostly probably pollution driven (in this case this can be shown quite convincingly by the negative correlation with such pollution tracers as CO, NOy and soot). I believe this is an interesting topic for further study. Trajectory calculations in conjunction with satellite images should permit a better analysis than the speculative discussion of the IGY data. The very recent CHABLIS data, also obtained at Halley, should be an obvious choice, and data from other stations could be included to get a more balanced picture. I do agree with Kaleschke that the current discus-

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sion is premature and should be part of such a more detailed analysis including more "modern" data. Finally, I think the authors are too apologetic for reviving these data (" we should not discount the results merely because it was over 40 years ago (that the sensor was operated)", "the measurements had ... been forgotten"). I think there is a clear role to be played by older data, and their age should not by itself disqualify them. In fact, I am curious as to what other ozone data are lingering in old archives. When we looked at surface data from ozone sondes in the Arctic (Tarasick and Bottenheim, ACP,2, 197, 2002) the oldest data we could find were for 1967 but we sure would be interested in "forgotten" data from earlier times. A few minor comments:

* on page 3629, the bromine explosion mechanism (2): indicate what part is gas phase vs heterogeneous chemistry in/on snow and ice

* on page 3633, line 15: the choice of fitting factors is arbitrary and obviously based on making the curve fit the maxima of the Halley data. The text is somewhat unclear on this; just clearly say what is done.

* page 3636, line 3-4: I feel the connection with change in wind direction is tenuous and in fact a stronger correlation seems to exist with wind speed (visually anyway).

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