

## ***Interactive comment on “Extreme events in total ozone over Arosa – Part 1: Application of extreme value theory” by H. E. Rieder et al.***

**Anonymous Referee #2**

Received and published: 2 July 2010

The authors analyse the Arosa total ozone time series by using for the first time extreme value theory to interpret the distribution of extreme events.

Although I acknowledge the need to study the distribution with new methods I disagree the way this new method is implemented and reject the paper.

I criticize the technical way thresholds are determined and interpreted. As usual in statistics several time periods are defined and their relevant properties are compared. According to page 12772 these periods are 1927-1959 (anthropogenically and volcanically unperturbed period) and 1960-2008 (anthropogenically and volcanically perturbed period). The corresponding annual averages of total ozone can be seen in Fig. 1.

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I disagree with this choice and naming, although it is triggered by our knowledge of anthropogenic emissions and volcanic eruptions. I agree that the first period seems to be anthropogenically and volcanically unperturbed. The second period seems to have in fact three subdomains. A first subdomain ranging until the early 1970ies seems to be in fact still unperturbed, the second subdomain until the late 1990ies seems to be increasingly perturbed, and the third subdomain seems to be on a more or less stable high level of perturbation, which is also somewhat similar described later in the text. One can't say that the second period is in any way homogeneous, i.e. that it is equally anthropogenically and volcanically perturbed. The authors don't claim that - and a better description is given later, but the simple naming suggest simple conditions. I come back to this choice later.

According to their new method the authors determine thresholds for extreme low (ELO) and extreme high (EHO) total ozone events for both periods and for each calendar month and present them in Fig. 8. On page 12776 the authors write:

"The monthly means of the two time series show a larger difference than the corresponding thresholds for low and high total ozone. This shows that thresholds for ELOs and EHOs depend only weakly on the selected period, so changes in the frequency of ELOs and EHOs cannot be attributed to a particular choice of reference period. Although the differences between the thresholds are quite small they differ slightly among the seasons, for example there is a larger difference in the EHOs thresholds in spring and in the ELOs thresholds in spring and late fall and winter (which might be a signal of the overall influence of anthropogenic ODS)."

The way the data are presented in the Figure may easily lead to the wrong conclusion given by the authors. The values of the monthly mean values are given as single symbols. The values of the thresholds are not given by symbols but instead just connected by straight lines. Most parts of the lines do show considerable gradients. Optically, lines with considerable gradients seem to be located much nearer than the corresponding relevant monthly values indeed do. In trying to overcome this optical ef-

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fect I conclude that most differences of the thresholds of different periods are within 0.5 - 1 times the difference of the corresponding means. In some cases these differences are even greater than the mean differences, e.g. in August and September, may be in July (EHO), and even in November (ELO). I agree, that most threshold differences are smaller than the mean differences. However, I disagree, that the thresholds depend only weakly on the selected period. Using the word "weakly" would require an order of magnitude difference in the signal.

Therefore, I disagree with the statement: "... so changes in the frequency of ELOs and EHOs cannot be attributed to a particular choice of reference period." When other periods would have been chosen, which follow more the overall trend of the data, e.g. a high ozone period 1927-1970, an increasing perturbed period 1971-1991, and a low ozone period 1992-2008, it would become even more obvious that the threshold definitions of ELOs and EHOs are dependent on the periods and therefore any changes in their frequencies. This is the weak point of the study! The method claims to be objective at this point but isn't. It cannot be overcome by the fact that some of the results seem to describe the data better than former approaches. Results cannot justify the method.

I have no clue how to overcome the situation given the available data. Nevertheless, I am sure the authors will continue to work on this topic. Therefore, I add some minor comments:

Looking at Fig. 1 statements like "The results show an increase in ELOs and a decrease in EHOs during the last decades." in the abstract are not surprising.

On page 12769 numbers like  $9.83 \times 10^{-6}$  seem to suggest a precision which does not exist. Reducing the number of significant digits would help.

On page 12770 I would omit equation (1) since it is misleading (mathematically wrong). The message of the paragraph before is rather simple, can be even shortened in text, and doesn't need the equation.

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Page 12771, line 23: Term (3) isn't an equation. (Same for page 12772, line 4.)

Page 12771, line 25: What does the brackets around index  $i$  mean?

Page 12776, lines 6-8; Table 1: The standard errors of the shape parameter  $u$  (too low) are lower than those for  $u$ .

Page 12778, lines 14-18: One would be interested to know, whether the predicted mini-holes coincide with the observations or the number of mini-holes can be predicted only.

Figures: Enlarging many labels (numbers and letterings) would enhance easy readability.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12765, 2010.

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