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Atmospheric Chemistry and Physics Discussions

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

H. E. Rieder et al.

harald.rieder@env.ethz.ch

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This is the reply of H. E. Rieder on behalf of the authors to the two major comments of Referee 2 (marked by R2). A detailed response including also the minor comments will follow. The referee acknowledges the need of new methods to study the distribution of total ozone but disagrees with the applied method. He/she rejects the paper based on two major points both related to threshold selection:

1.Disagreement with the periods used for threshold selection for extremes in low (ELOs) and high (EHOs) total ozone

2.Disagreement with the comparison of thresholds for EHOs and ELOs with monthly mean values presented in Figure 8 of the manuscript.

Concerning point 1 the referee states:

R2: "I criticize the technical way thresholds are determined and interpreted. As usual in statistics several time periods are defined . . . 1927-1959 (anthropogenically and volcanically unperturbed period) and 1960-2008 (anthropogenically and volcanically perturbed period). . . . 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 pertubation, 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. . . . 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."

The referee suggests three subdomains for the second time period, the first ranging until the early 1970s, the second one until the late 1990s and a third thereafter. Later he/she mentions three different time periods, namely 1927-70 for the unperturbed time period, 1971-1991 perturbed and 1992-2008 for the "low ozone period". These statements reveal that it is not simple to derive significant time periods for an on inter-annual time scale highly variable time series. We had decided to split the time series in 2 parts

(P. 12772, L. 14): “Starting from the 1970s, anthropogenic emission of ozone depleting substances (ODS) had a major influence on the Earth’s ozone layer, and one might argue that appropriate threshold values for natural extremes in total ozone must be based on measurements prior to 1970. Moreover, total ozone is also influenced by changes in quantities such as the aerosol loading of the Earth’s atmosphere. The Northern Hemisphere aerosol climatology (e.g. Sato et al., 1993) shows some smaller peaks in the 1960s. From these two “constraints” total ozone data at Arosa is considered as anthropogenically and volcanically unperturbed before the 1960s. As the aim of this study is to address the influence of short-term and long-term processes (including long-term trends) on the variation of extremes, analyzing an anthropogenically and volcanically unperturbed period (1927–1959) and an anthropogenically and volcanically perturbed period (1960–2008) is from the authors point of view a solid way to address ozone changes within two clearly different regimes.”

We agree that the periods used for the determination of the thresholds are important and need careful testing and want to clarify that we did not ignore this. Therefore we refer on P. 12773 to the supplementary material of the article providing an extensive discussion on the drawbacks of threshold estimation based on detrended datasets or on choosing a higher number of sub-periods. From the comments of R2 it remains unclear whether he/she paid attention to the supplementary material. In Section 1.2 we analyzed two periods (1927-1969 and 1970-89) which are very close to the ones suggested by the referee (1927-70 and 1971-1991), and also 1980-1999, which is strongly influenced by the eruptions of El Chichón and Mt. Pinatubo (for winter and spring). Results for thresholds selected based on these choices of time periods are compared to those presented in the main body of the article.

Furthermore, we do discuss the sensitivity of the thresholds for different time periods in Section 1.2 of the supplementary material: “One might argue that threshold estimation should be based on various shorter time periods, because a lot of inter-annual to decadal variability is visible in the Arosa total ozone time series (see Fig. 1 in the main

paper). We decided not to use different sub periods, first for statistical reasons as a sufficient amount of data is necessary for the threshold selection process, and second because this would lead to a subjective a priori decision on the properties of the time series (similar to detrending of the time series, described in Sect. 1.1 of the supplement). However, for discussion of the stability of threshold estimates, thresholds for low and high total ozone have also been estimated for three 20-year periods (1930-49, 1970- 89 and 1980-99) and an extended “unperturbed” period (1927-69)...“

Finally, when the reviewer states R2:

“One can’t say that the second period is in any way homogeneous ... The authors don’t claim that - and a better description is given later, but the simple naming suggest simple conditions“, this leaves us puzzled, as R2 first makes a strong statement, but then qualifies it himself/herself. While we maintain that this is a perfectly arguable choice, it was not our intention to choose a “simple naming” in order to suggest “simple conditions”, and therefore we will improve our wording here to avoid the impression obtained by the reviewer.

Concerning point 2 the referee states:

R2: “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. ... 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 effect 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."

We apologize if our Figure 8 has been misleading, but respectfully disagree with the conclusions drawn by R2. The fact that for August, September and November there is comparable or slightly larger difference in the monthly threshold values than in the mean values does not perturb the general validity of the analysis (because the main effects ODS, volcanoes and dynamics are found in winter to spring). We will add an additional table to the manuscript (see Table 1 in the attachment) to avoid all kinds of optical illusion. For further clarification we added here also a black and white line plot of Figure 8 where also mean values are interpolated to daily values (see Figure 1 in the attachment). However, we want to point out that an interpolated version of mean values (or deviations from those) was never used in mini-hole or mini-high analysis. The small sensitivity of thresholds on time period is also confirmed by the small differences among the thresholds based on various time periods presented in Figure S2 of the supplementary material.

Finally, we want to clarify that the dependence of the thresholds on time periods was described as "weakly" not only based on the comparison with mean values but also on the inter-threshold comparison given in Figure S2. We also wonder in this context about the strict definition of "weakly" given by the referee ("one order of magnitude difference in the signal"), as from our point of view "weakly" describes a quantitative difference (on average a factor of 1.4), which allows such statement.

We hope that this response helps clarifying the main points made by R2.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C5012/2010/acpd-10-C5012-2010-supplement.pdf

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