The Cryosphere Discuss., 7, C629–C634, 2013
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Interactive comment on "High resolution 900 yr volcanic and climatic record from the Vostok area, East Antarctica" *by* E. Yu. Osipov et al.

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Received and published: 22 May 2013

Comments on scientific merit

The discussion paper presents a 900-year volcanic record that also contains some information on climate variations during this time period at Vostok. Volcanic records of similar coverage and detail have been obtained from ice cores drilled in several other locations in Antarctica. This record (VR), the first of this type (based on sulfate measurement, rather than on ECM) from Vostok, represents a large central East Antarctica region from which few such records exist. This work used methodology that is very similar to one used in the studies that produce the other ice core records. The authors credit previous work by other researchers on this topic by making appropriate references. The conclusions here are similar to those from the other studies, although

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some results are tied to the unique local (Vostok) settings. The authors offer some preliminary analysis on the implications of the accumulation rate and seasalt deposition histories contained in this record.

Comments on technical description

The description of the methodology in this paper is ambiguous or incomplete in several places. This may be due to the fact that similar methodology has been used and described in other ice core studies. However, detailed and accurate description of the various aspects of ice core analysis, data analysis and synthesis, and dating procedures is necessary in order that readers can assess the quality and validity of the work and the conclusions. I point out below a few places where more specific or detailed description is needed. First, the depth intervals of the four cores and snowpit samples listed in Table 1 appear to be those analyzed for this work. But it is no clear what the length is for each of the three cores. I think both information (length of core and depth interval analyzed and used for this paper) should be provided. Second, although measurement of ionic concentrations of ice cores with ion chromatography has become somewhat routine in ice core research, the minimization and elimination of contamination during sample preparation and analysis is still crucial and must be verified in each lab and assured on a daily basis. The description of sample preparation and analysis (Page 4, lines 4-15) provides no information on quality control of the analysis concerning contamination. At a minimum, the description should contain information on the results of procedure blanks during the analysis. Third, four sets of ice core and snowpit samples were used in this work. Since these sets of samples covering different time periods were used, they must be linked together to cover the entire period of 900 years continuously. How are they linked? This is not, but should be, described in the paper. The authors may have used the same volcanic event in two cores (for example, Long Island (1660) in 5G and VK-07, Krakatoa in VK-07 and VKT-55 in Table 2) to link the two cores by determining an overlap between the two cores; but this is not mentioned or described in the paper. In the case of an overlap, the overlapping part of which core

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is retained to construct the continuous profile (Fig. 3) without overlap is not explained in the paper. Fourth, the non-volcanic background sulfate or nss-sulfate concentration for each core is to be calculated after the removal of the high (presumably volcanic) concentrations in the core. This is not stated (Page 5, lines 20-23). In addition, the range or standard deviation of the background sulfate or nss-sulfate depends strongly on what samples with high sulfate concentrations have been removed. The very high volcanic signal detection threshold for 5G (421 microgram/L in Fig. 3) seems to suggest that the standard deviation was calculated without the removal of the samples with high sulfate concentrations. This implication can be avoided with a specific description of how the average and standard deviation of the non-volcanic background are calculated. The oscillations in Na concentrations (Fig. 2) were considered by the authors to be annual and used to date the 2-m VK-55 snowpit by annual layer counting. Given the sampling resolution in the snowpit (2-3 cm per sample, Page 4, line 6) and the snow accumulation rate of approximately 6-7 cm per year, the temporal resolution in the snowpit is 3.0 samples per year (Page 4, line 15). At this resolution, one is required or obligated to count each Na concentration oscillation (high-low-high) as a year, which is what appears to have been done with the data in Fig. 2. This interpretation of annual layers does not allow the possibility that an oscillation may be the result of analytical error or fluctuation of the measured concentration. (Actually, the fluctuation in the concentration due to analytical error is more than a possibility; the same measurement carried out twice will yield two different numbers.) Many researchers have previously pointed out that, in order for annual cycles or oscillations to be reliably identified, at minimum of 5 (preferably 8 or more) measurements in a year's accumulation must be made. I suspect that the Na concentration oscillations in Fig. 2 are not annual and that, unless the temporal sampling resolution is 1 cm or less per sample, annual layers cannot be reconstructed. Furthermore, due to the extensive mixing of surface snow down to a few centimeters below the surface, the layering of seasonal snow at Vostok, where average annual accumulation is a few centimeters, must be irregular and, as a consequence, annual layers cannot be reconstructed for any time period of more than

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a few years, even if the temporal sampling resolution is better than 5 samples per year. The mixing of annual layers at Vostok is acknowledged by the authors: see Page 8, lines 2-5 and lines 9-10. Fig. 2 and the use of "annual layers" for dating and for annual accumulation rate calculation should be deleted.

Comments on data presentation and interpretation

1. I would like to see an explanation on why samples from three ice cores and one snowpit are used for this work. For example, VKT-55 was drilled in 2010 (Table 1), presumably from the 2010 surface, and the depth interval of 2.50-7.80 m was used in this work (Table 1). Why was the top 2.50 m of this core not used? How could VK-55 (0-2.00 m) fill completely (Fig. 3) in the time period covered by 0-2.50 m of VKT-55?

2. The average background sulfate concentration varies significantly among the four cores and snowpit samples (Page 5, lines 24-25): it is twice in VK-07 (235 microgram/L) as in VKT-55 (118). Could the authors offer any explanation about the large differences? The background sulfate concentration over a period of several hundred years usually does not vary that much in the same small local area. It appears that VK-07 is significantly different from the other cores (Fig. 3) in average background and in variability. Could it be that samples of VK-07 were analyzed differently from those of the other cores?

3. The larger number (33) of volcanic events in the Vostok record (Table 3) than those in the other similar records (PR, DC, SP, Table 4; Page 7, lines 6-14) should be explained; at least an attempt at explaining should be made. For example, many of the VR events (V4-V25) not present in the other records (Page 7, lines 18-19) are in the time period of 1620-1890, a period covered by VK-07 (Fig. 3, Table 4). This may be related to the high variability of sulfate concentrations in VK-07, as mentioned above.

4. The first paragraph on Page 9 is an explanation offered by the authors to explain the differences in volcanic sulfate deposition flux between Vostok and the other locations (PR, DC, SP). It should be immediately following Line 21 on Page 8.

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5. The discussion on the trend of snow accumulation rate (Pages 9-10, Fig. 6A) should acknowledge that the measurement uncertainty and natural variability of the accumulation rate are unknown or not estimated in this case. Without the knowledge of the uncertainty in the calculated accumulation rates, discussion on trend and temporal variations is speculative. For example, the difference between the low accumulation rate during 1883-1963 (20.8 mm) and that (22.5 mm) during 1815-1883 may not be significant, if the measurement uncertainty is larger than 1.7 mm.

6. Page 11, line 8: What does “greater” mean? Greater than what?

7. Page 11, lines 14-15: Is there an explanation for the much lower average Na concentration (12.6) during 1980-2010 than any other time period in this record? Please note the the period of 1980-2010 is covered entirely by VK-55.

8. Page 12, line 20: “Decreased” should probably be “Increased”.

9. The data in Fig. 5 are the same as in Fig. 4. The only difference is that data in Fig. 4 are for flux and the data in Fig. 5 are flux scaled or normalized against a common value in each core. Fig. 5 should be eliminated.

10. Data in Figs. 3 and 6 are on the same x-axis. These two figures can be combined. Because seasonal layering are not likely preserved at Vostok and annual layers are not likely resolvable, the insert to the top graph in Fig. 6 should be deleted.

11. Page 14, line 16: The journal for this paper is Journal of Glaciology, not Annals of Glaciology.

12. The title of Table 2 should clearly indicate that the dates in “Years in core” are assigned years to the volcanic signals, not years determined by the dating process for these cores.

13. For most of the volcanic events in Tables 3 and 4, it is very difficult to directly connect a volcanic event in the ice core record to a specific eruption. I suggest that the authors delete all of the names of the volcanoes (Column 2 in Table 3 and Column

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1 in Table 4), except those used in Table 2.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C629/2013/tcd-7-C629-2013-supplement.pdf

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