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Climate of the Past Discussions

Interactive comment on "Tree ring effects and ice core acidities clarify the volcanic record of the 1st millennium" *by* M. G. L. Baillie and J. McAneney

Anonymous Referee #1

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The paper submitted by Baillie and McAneney deals with the impacts of volcanic eruptions on tree growth and compares dendroclimatic data with ice core records for the first millennium of the common ere. Over the last three decades, ice cores have been widely used by glaciologists and climatologists to establish more firmly the history of explosive volcanism and to quantify the sulphate emissions ejected into the Stratosphere by large volcanic events.

The present submission suggests that the volcanic signals preserved in the Greenland (Dye3, GRIP, NGRIP) and Antarctic (Law Dome, WDC06A) ice core chronologies have been wrongly dated by seven years for the period before 700 AD. The authors have indeed observed that several major volcanic signals recorded in ice cores in the 6th and 7th centuries do not coincide with any pronounced cooling as suggested in tree-

ring records, as it should be normally expected given the massive concentration of sulphate aerosols deposited in the ice cores. They notice that the problem can be solved if ice core dates are shifted forward in time by 7 years, so that they match the tree-ring records. The paper also aims at showing the value of tree-ring chronologies to complement, improve and refine the chronology of global volcanism.

The results presented are interesting as such and certainly fit well with the scope of Climate of the Past. However, and based on the comments listed below, major revisions will be needed before the paper can become acceptable for publication.

General comments:

1. When reading the manuscript for the first time, one may really wonder whether the findings presented are indeed original enough to be published in CP. Indeed, several of the volcanic acidities studied in the present work (AD 522, 536, 541, 574, and – to a lesser extent – AD 628 and 681) have already been investigated and published by the lead author in Geophysical Research Letters (Baillie, 2008) and Antiquity (Baillie, 2010). What is the added value of this paper, and in what do the results presented here differ from previously published work? This is not clear at all in the paper as it stands now. One may recognize that the authors use new ice core records of volcanism from Greenland and Antarctica, but is it really novel enough to deserve publication in CP? In order for the paper to become acceptable, the authors will need to highlight more clearly what is really new in this study.

2. One may also regret that the authors mainly restrict their analysis to the period AD 500-700. The NGRIP, NEEM, Law Dome and WDC06A ice cores all show prominent sulphate peaks around 230, 258, 425, 741, 748, 811 (Plummer et al., 2012; Sigl et al., 2013) which should have been included in the analysis, as the title of the paper clearly refers to the first millennium and not to a small subperiod (as described in the paper). The findings of Baillie and McAneney would be more convincing had the authors demonstrated that eruptions were also misdated in the 3rd, 5th, 8th or 9th

centuries, as otherwise their 7-yr shift could not withstand a more critical review.

3. On p. 1812, line 28 to p. 1813, line 1, the authors state that "[. . .] the so called Eldgjà acidity around AD 933+/-1 may also be wrongly dated by several years [. . .]". It would be nice if the authors could elaborate on this Does it mean that the acidity date should also be moved by 7 years (i.e. AD 939-940)? And if so, how do the authors explain the contradiction with work of Stothers (2000) who hypothesized, based on historical archives, that the Eldgjà likely occurred in AD 934. At the same time, however, the authors would be more in line with McCarty and Breen (1997), as well as, Oman et al. (2006) who placed the event in AD 939. Clearly more details are needed here.

4. The growth ring series of Bristlecone pines have proven to be a valuable tool to document the climate response to volcanic eruptions. However, as the authors correctly note, Bristlecone pines are not a perfect archive of past volcanism. At some occasions the cooling generated by large volcanic eruptions was not pronounced well enough to induce the formation of frost rings, such as aftere.g. the 1783 Laki, 1816 Tambora, and the Novarupta-Katmai events. In that respect, one may believe that the findings presented in the paper would be more robust if one would use - along with the Bristlecone pine frost rings chronology - another tree-ring chronologies, such as the Torneträsk MXD chronology (441-2008) or the Nscan MXD chronology (-200-2010), recently published by Esper et al. (2012) and freely available at: http://www.blogs.uni-mainz.de/fb09climatology/publications-jan-esper/

On p. 1813, lines 3 to 15, the authors mention an eruption that could have been responsible for the dry fog observed in Constantinople in AD 797. However, this event does not seem to have induced substantial cooling in the Great Basin (USA), since no frost rings can be found in Bristlecone pines. Can the authors find evidence of cooling in the MXD chronologies from Scandinavia for this eruption? And what would this mean for the spread of impacts and possibly regarding the source of the eruption? 5. Did the frost rings found by the authors in the Bristlecone pine frost-ring chronology for the 7th century coincide with significant cooling in the MXD Scandinavian chronologies?

One might ask this question as the reasons for cooling might be other than volcanic, even more so if only one chronology is used and the signal is not analysed in a more hemispheric context.

Technical comments:

1. The paper is well written but rather poorly structured. The reviewer strongly encourages the authors to add at least a section entitled "Methodology" in which they define more clearly the methods used to identify the misdated volcanic signals. They could also use this section to introduce in more detail the ice core datasets employed in this study.

2. Furthermore the authors are asked to revise the titles of certain sections which are not always relevant (especially the section 2 entitled "Analysis").

3. The paper contains no figures at all, only three tables which are not easy to read (especially the table 2). The reviewer is asking the authors to think of better ways to convey the key message of the paper?

4. p. 1802, lines 17 to 18: The authors state that Jones et al. (2013) "assessed the worst 20 individual years in the last 7500 years using MXD in tree rings from Northern Fennoscandia [. . .]". Please note that Jones et al. also used TRW data since the longest MXD chronology "only" covers the last 2000 years.

5. p. 1806, lines 7 to 8: The authors refer to "[. . .] the now famous AD 536 global environmental event (Barras, 2014)" and cite a publication in New Scientist. This does not seem to be the appropriate reference in this context, and I suggest to use Arjava (2006), Baillie (2004), D'Arrigo et al. (2001), Gunn (2001), Oppenheimer (2011) or Stothers (1984) instead.

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