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Climate of the Past Discussions

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Interactive Comment

Interactive comment on “Multi-periodic climate dynamics: spectral analysis of long-term instrumental and proxy temperature records” *by* H.-J. Lüdecke et al.

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Tying in with the comment previously made by Oliver Bothe, I’d like to elaborate on the observed periodicity in the instrumental temperature record exclusively from a climate physics point of view. On page 4501, line 12, the authors explicitly state: “The cause of the periodicities is unknown”. This worded, the authors literally thwart half a century of research which has been trying to isolate the volcanic climate forcing (e.g. Mitchell (1961), Lamb (1970)). In fact, Hansen et al. (1981) already showed that the global temperature trend can only be successfully reproduced in climate model simulations if one considers the historic volcanic forcing strength (see Fig. 5 in their paper). Only recently, Mann et al. (2012) suggested that the volcanic forcing might still be underestimated as far as tree-ring reconstructions are concerned. Hence, given that known forcing mechanisms might explain the observed periodicity to a large extent, the entire premise of the manuscript (namely the spectral analysis) could potentially be undermined, rendering it useless after all. Subsequently, I am trying to highlight why I think this could the be case.

While it is abundantly clear that Volcanoes indeed produce abrupt climate responses on short time scales (Robock, 2000), it is still debated to what extent multi-decadal volcanic effects play a role in past temperature variations (Porter (1981) already discussed this issue thirty years ago). Based on the work of Delworth et al. (2005), Gleckler et al. (2006a) and Gleckler et al. (2006b), Randell et al. (2007) concluded: "It is possible for one large volcano or a series of large volcanic eruptions to produce climate responses on longer time scales, especially in the subsurface region of the ocean".

Since then, e.g. Stenchikov et al. (2009), Gregory (2010), Zhong et al. (2010), Miller et al. (2012), or Zanchettin et al. (2012a) further strengthened the notion that there likely is a long-term component identifiable in the global surface temperature record. Due to the rapidly induced negative Ocean heat content anomaly in the wake of a strong volcanic eruption, a positive Ocean surface flux anomaly is expected to persist even after the radiative forcing has diminished (returned to conditions equal those prior to the eruption). According to Stenchikov et al. (2009), it takes several decades until the corresponding radiative imbalance between ocean and atmosphere is entirely overcome. During that period the global surface temperature anomaly is slightly negative (see Fig. 2a in their paper). On top of that, atmospheric and oceanic anomalies may persist for 20-25 years (Zanchettin et al., 2012a).

Schneider et al. (2009) focused on regional impacts of volcanic eruptions in general, whereas Fischer et al. (2007) looked at the European climate response to tropical volcanic eruptions over the last half millennium in particular (with specific regard to the potential NAO response). This brings me back to the 6 instrumental records presented

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in the manuscript. Although Fischer et al. (2007) only discuss short-term effects, they find a significant continental scale summer cooling in the first two years after an eruption. It is accompanied by warmer and wetter conditions during winter (pos. NAO) in Northern Europe. Clearly, the volcanic imprint can be identified. Schneider et al. (2009) showed that the combined effect (summer and winter) is still negative in both seasons over Europe.

To further illustrate my point, in Fig. 1 I plotted the instrumental temperature record of Prague, Hohenpeissenberg, Vienna, Paris, Munich, and Stockholm (instead of Kremsmünster whose data are not at my disposal). Find juxtaposed to the 6 temperature records, the NH volcanic aerosol forcing and the monthly mean sunspot numbers (with running mean). I omitted further temperature averaging, but applied a simple lowess smoothing to filter out higher frequencies. The impact of the volcanic dust upon the individual temperature records can clearly be seen in the composite image. No statistics required to visually identify the potential temporal correlation. The author's notion that some "intrinsic system dynamics" might have driven the observed changes is unfounded and contradicted by the available literature as pointed out before. Not to mention, that external forcings are widely regarded as the controlling factor for the global surface temperature (Stott et al., 2000).

The scientifically interesting part for the authors would now be to prove or disprove the long-term impact, particularly during the volcanically active phase in the 19th century. Are there cumulative effects as Miller et al. (2012) suggest for an earlier period? Is the Little Ice Age a product of the combined effect of volcanic and solar forcing signals as proposed by Free and Robock (1999) or Crowley et al. (2008)? Are changes in the AMOC strength triggered by volcanic eruptions as discussed by Otterå et al. (2010) and Iwi et al. (2012)? Are strong eruptions relevant to modulate the PDO-AMO phase as demonstrated by Zanchettin et al. (2012b)? These are the questions I expect to be discussed when the instrumental record of the past 250 years is explored.

In my point of view, it does not support their conclusion to argue for a similar periodicity in the Antarctic ice core record at all. It should be clear by now that this is almost inevitably connected to the very same global forcing mechanism and is hence all but surprising in phase with the instrumental record in Europe (Gao et al., 2008; Schneider et al., 2009). Bear in mind that the ice core data need to be treated with caution as it is difficult to calibrate their temperature against the instrumental record which introduces substantial uncertainties (e.g. Rhodes et al. (2012)). An interesting aspect to discuss here would be the magnitude of the Antarctic temperature departure, or in how far ice/ocean feedbacks may kick in after strong eruptions or a series of eruptions (Zhong et al., 2010; Miller et al., 2012).

To wrap up: Currently, I can't see how the submitted manuscript could possibly provide new insights which are of significant interest for the scientific community, without adding a paragraph which thoroughly discusses known natural forcing mechanisms (namely volcanic eruptions). As illustrated on the basis of the available scientific literature, thus far the presented work is at risk to be received as a mere curve-fitting exercise. While the authors certainly demonstrate a high level of technical skill, they fail at the same time to acknowledge the work done by multiple groups in the last decades. I strongly encourage them to put their results in a more physically based context of the climate system - something which I believe has necessarily to be done before it deserves publication in CP. Plenty of literature (not exhaustive) is given to start with. The other points raised by Oliver Bothe and Manfred Mudelsee are by no means of lesser merit. In particular, a hindcast assessment as suggested by Oliver Bothe could be included to increase the confidence in the proposed method.

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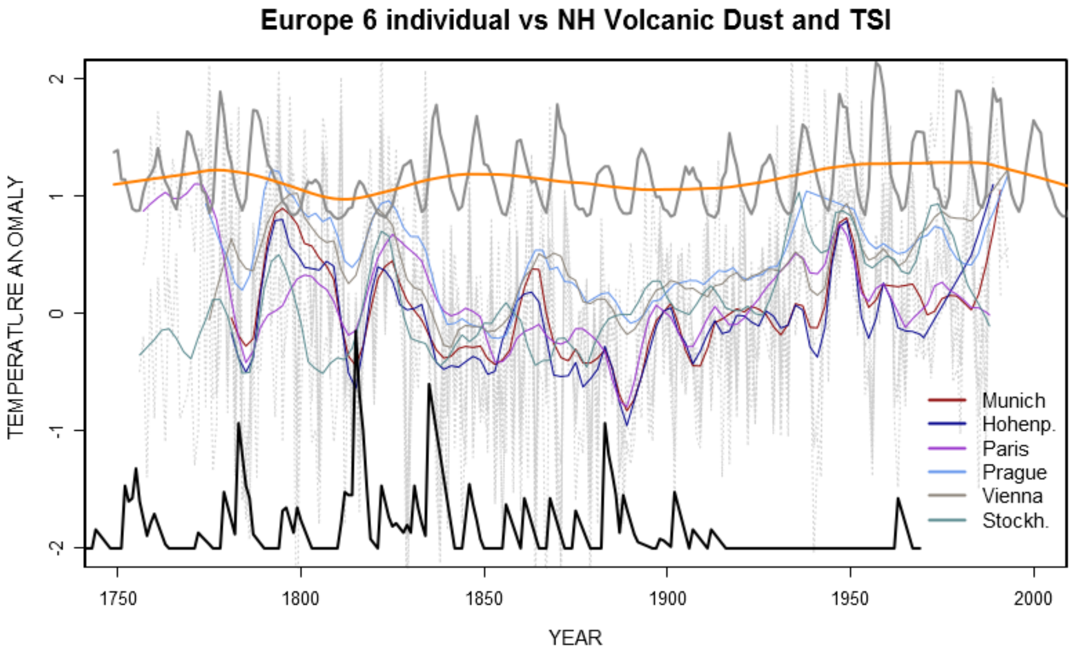

Fig. 1. Instrumental record of 6 European stations (see caption), the mean sunspot number (grey) with running mean (orange) and the northern hemispheric volcanic dust loading (black) for the period 1750-2000.

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