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# *Interactive comment on* "The 1816 'year without a summer' in an atmospheric reanalysis" *by* Philip Brohan et al.

**Anonymous Referee #2**

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Review of

'The 1816 'year without a summer' in an atmospheric reanalysis.

by P. Brohan et al.

Recommendation: major revisions

This manuscript presents a reanalysis of daily weather in the year 1816 using the Ensemble Kalman Filter data assimilation system already used for the NCEP 20C reanalysis. For this study daily SLP values from 12 stations in Europe and occasional ship observations are assimilated. The analysis is focused on Europe, because only there the atmospheric states in the reanalysis are constrained enough by the observations to provide reasonable skill. The reanalysis is mainly validated against temperature



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Discussion paper

observations from Geneva. It shows that the circulation anomalies over Europe and the volcanic forcing from Tambora both contribute to negative summer temperature anomalies over Europe.

There is some evidence provided that the reanalysis has skill over Europe, and showing that this can be achieved by assimilating only the low number of pressure records available at the beginning of the 19th century is a finding that in principle justifies publication. However the manuscript is clearly premature for several reasons: i) the research question is not well defined, ii) the validation is quite limited and unsystematic, iii) the results shown are incomplete, iv) there is a substantial lack of conceptual clarity with respect to the interpretation of the results, as well as v) an element of overselling the relevance of the study. Given that many of the authors are very experienced I found this a bit surprising. I thus think that major revisions followed by another round of reviews are required before the manuscript can be considered for publication.

Specific comments:

L5, the statement on spatial and temporal resolutions does not make sense as tree-ring reconstructions are local (temporal resolution is indeed a limitation) and climate simulations have a typical spatial resolution on the order of 100km, which is sufficient to investigate spatial patterns on sub-continental scale, and a temporal resolution of about 30 min. Admittedly resolution and skillfull scale are not the same, but this needs to be clarified.

L18/19, statement on predictability ('if something similar were to happen next year would we be able to predict it') is fundamentally wrong for two reasons: i.) the volcanic forcing is not predictable, and ii.) one cannot conclude from the successful capture of circulation anomalies by data assimilation, which obviously uses observations, that the circulation anomalies can be either deterministically predicted in the sense of an initial value problem, or successfully simulated as the part of the response to a forcing.

L34-41, it is not convincing to restrict the discussion to Geneva. There should be a

comprehensive review of what is known on European-wide temperature and pressure anomalies over Europe in 1816 AD from both proxy-based reconstructions as well as standard, forced GCM simulations.

L51, in the 20C reanalysis SSTs, sea ice cover and assimilated pressure are all taken from observations and are thus dynamically consistent (apart from the errors they include). The climatological SST and sea ice cover used in the reanalysis can be expected to be dynamically inconsistent with the assimilated pressure observations for 1816. I think it is unclear whether this inconsistency leads to substantial problems. A thorough analysis might help to shed light on this question but might be beyond the scope of this paper. However, the text should include at least a short discussion of this issue.

L69-71, the statement 'the sequence and location of individual weather events (highs and lows)' is not sufficiently supported by the results shown. First of all some information about the temperature observations that have been used for Fig.1 should be given. Second, the dates for Fig.1 are hard to read and are in January. Given that the focus of the paper is on summer temperature anomalies it is not consistent and not informative to show an example for winter. Moreover, selecting just a few days does not qualify as a sound validation. The revised version should include a comprehensive validation of temperature anomalies at all locations for which temperature records are available, using the entire year, including a seasonal breakdown and specific statements on the skill in summer. Potential skill measures are correlations and RMSE. The analysis should include a comparison with a suitable reference simulation without data assimilation e.g. standard forced PMIP3 simulations, or running the data assimilation system with forcing but without assimilation of pressure observations. In addition to the skill measures the timeseries should be shown for several locations, not only for Geneva (Fig.2). When calculating skill measures comments on how the ensemble is dealt with should be made, so it becomes clear whether the ensemble mean has been used or scores for individual ensemble members have been averaged.

L91-99, The discussion of the signal of the volcanic forcing and the interpretation of Figs. 3-5 are unlogical for at least two reasons:

i.) One should distinguish between local thermodynamic forcing and forcing of large-scale circulation anomalies. The relative contributions of the two over Europe are a priory not clear. It is in principle conceivable that the forced signal over Europe is mainly dynamical, in which case including the forcing in the simulation might not increase the skill as the forced signal would be included through the assimilation of the pressure observations. This seems not to be the case, but nevertheless the possibilities need to be discussed in a conceptually sound way to guide the analysis of the results.

ii.) It is also a possibility that the temperature anomalies over Europe are a combination of a thermodynamic response to the volcanic forcing and of a temperature response to random, unforced pressure anomalies. Even in this case on might get smaller analysis increments if the volcanic forcing is included, as the individual forecasts of chaotic, quasi-random variability can be expected to be better if an atmosphere with more realistic radiative properties is used.

The circulation anomalies for summer should be shown, if the authors come to the conclusion that they might be due to the volcanic forcing the arguments need to be made clear, a comparison with the forced circulation response in PMIP3 simulations should be made, and potential forcing mechanisms that might lead to this circulation anomaly should be discussed.

L114-117, this paragraph mixes the question of prediction (see comment on L18/19) with the question of impact modelling.

L118-125, The attribution question is interesting but the discussion is not clear. This paragraph should be either deleted or improved such that the line of argument is explained more precisely.

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