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Climate of the Past Discussions

Interactive comment on "A volcanically triggered regime shift in the subpolar North Atlantic ocean as a possible origin of the Little Ice Age" *by* C. F. Schleussner and G. Feulner

Anonymous Referee #2

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The paper by Schleussner & Feulner aims at exploring the respective role of natural external forcings (volcanic and total solar irradiance) and internal mode of variability, namely NAO and AMO/AMOC, in explaining the onset of the Little Ice Age. To address this issue they developed a modelling approach relying on the CLIMBER-3alpha model to perform idealised experiments imposing either external forcing scenario alone and/or North Atlantic wind field anomalies scaled upon NAO modern day probability distribution. The distribution of wind-field variability is first evaluated using an inverse transform sampling against NCEP 1948-2009 observations. Assuming a stationarity in the links between mean state, NAO index and wind variability over the 60-years period of observations, they were able to reconstructed a wind-field variability time series for the

1049-1995 period by scaling the statistical distribution on the NAO index reconstructed by Trouet et al (2009). Such approach is intended to overcome the fact that the atmospheric component of the model does not reproduce internal variability. Such approach is quite interesting since it allows performing many sensitivity experiments and tests the possible impact of a reconstructed NAO index (Trouet et al) within a model dynamical framework. The low model resolution in the ocean especially over the Nordic Seas and the statistical-dynamical atmosphere might constitute a limitation for any thorough process oriented study. However such model allows testing easily many scenarios concerning the climate response to external radiative forcing and the ocean dynamics first order response to transient changes in atmospheric variability - even though such results shouldn't be over interpreted and taken only as sensitivity tests. The paper is very well written and the presented results will contribute significantly to the current efforts to understand the climate processes underlying major climatic transition that occurred during the last millennium. However the authors need to significantly improve the paper, as there are many important issues to clarify or to be corrected before publication. I've listed bellow my main comments and criticism to be addressed before publications:

1. My first comment concerns the consistency between the imposed wind-field anomalies and the atmospheric state and transient changes when applying volcanic forcings. The direct radiative impact of stratospheric volcanic aerosols is known to influence both the vertical temperature gradient between the surface and the stratosphere and the gradient between equator to the pole within the stratosphere itself. Such impact modulates significantly the transient state of the atmosphere including the NAO and wind fields over North Atlantic regions. The authors didn't discuss whether applying a reconstructed-NAO scaled wind-field anomaly was consistent with the simulated wind field itself as a response to the applied volcanic forcing. This could be an interesting piece of information to quantify how the applied wind anomalies is consistent with the initial wind field response to volcanic forcing as simulated by the model. This is a relevant question especially since the authors state in section 3 that "Our method also incorporates atmospheric variability induced by external forcing, because imprints

of both TSI changes and volcanic eruptions have shaped our reconstructed NAO time-series" while they stated in section 2.2 that their procedure is insufficiently representing past extreme NAO years or persistent phase shift due to the short period of instrumental observations. Both statements seems to be contradictory and these issues should be clarified.

2. The authors show that adding an extra 5mSv to the constant 15mSv offset to their model simulations induce a change in the timing for the SPG spin-up with a switch occurring around the LIA onset as described in most climate records. Since the aim of the present paper is to explore the relative role of external forcing vs internal variability modes, it looks to me that such sensitivity of CLIMBER-3 model to freshwater offset (either constant 15mSv or with an additional 5mSv) in the Nordic seas is used here to tune the model so that the transition toward LIA occurs at a time matching the reconstructions rather than giving meaningful "real" physical processes explaining that climate transition. The authors should be more careful when discussing these issues and give more justifications on the applied constant 15mSv off-set in their simulation labelled "NO-OFFSET". As it is stated, I found it quiet difficult to understand why applying any-offset or additional off-set at all in the context of exploring the underlying physical processes leading to the LIA transition.

3. Based on figure 1, the authors state that the AMOC is greatly sensitive to the SPG changes and sea-ice extent in the Nordic Seas. My first comment concerns the confusion the authors make with AMO and AMOC. They switch from one term to the other throughout the paper and use either AMO or AMOC as if they were exactly the same thing but this is not correct. These are very different climate modes and the links between both is still a matter of investigations in the climate community. If the authors want to discuss both then they should add time series of AMOC to each figures in addition to that of AMO and discuss differences and similarities since these are non-trivial scientific questions. As for the response of AMO shown in figure 1, it doesn't look to me that it is very sensitive to the sea-ice scenario since for both "No-Offset"

and "5mSv Offset" sensitivity experiments, while the sea-ice trajectories are drastically different, the overlapping blue and red curves showing the AMO transient changes in Fig.1e. illustrate a quite similar response in both experiments. It looks like the AMO (which is a SST index) response is rather dominated by the imposed radiative forcing shown in panel (a) of Fig1, while sea-ice seems to evolve in pace with the SPG. In that sense a time series for the AMOC (in Sv for the Atlantic Meridional Circulation) would be useful to see how the SPG dynamical response and Sea-Ice transient changes influence the Atlantic Meridional Circulation (AMOC) dynamics and intensity.

4. Concerning the role of each volcanic forcings in the simulated transient response of SPG, sea-ice and AMO, again I don't understand the choice made by the authors to impose either a constant volcanic forcing, a 15 or 5-years long forcing. They discuss the outcomes of such experiments respectively to previous modelling study that revealed the impact of isolated and decadally spaced eruptions. Imposing a constant volcanic forcing invokes a totally different climate forcing, a totally different radiative impact and climatic processes altogether. A constant or even a 15-years long volcanic forcing is rather relevant for Pre-Quaternary mega-volcanic eruptions. If the aim of these sensitivity experiments is to test whether volcanic forcing alone similar to that occurring during the last millennium or during the LIA onset can induce a persistent sea-ice/SPG shift, they should design dedicated sensitivity experiments consistent with the short-lived nature, pacing and intensity of the volcanic forcing of that period. A constant or even a 15-years long forcing can't be used as a relevant analogue to what occurred during that transition. The real question concerns the time persistence of a single 3-years long volcanic forcing and the transformation of these short lived forcings into a long term climatic response. Imposing a constant forcing raises a whole different question and relies on different climate processes that do not apply to decadally or even sub-decadally spaced eruptions.

5. Still in the discussion section, the authors discuss the AMOC response while showing AMO time-series so that the cascade of processes (involving the volcanic forcing,

increases sea ice extent, a spin-up of the SPG) influencing the AMOC response can't be discussed from any results or analyses presented in the paper so far. AMO is not equal to AMOC. In addition to the inappropriate sensitivity experimental design (with a constant or 15-years long forcing) the AMOC response is not displayed so it can't be discussed and the AMO response can't be discussed respectively to previous study discussing the response of AMOC.

Interactive comment on Clim. Past Discuss., 8, 6199, 2012.