

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

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Received and published: 20 March 2013

Anonymous Referee 1

Comment 1

Compared to the literature that is available and deals with the LIA the introduction is very short and does not set the stage for the paper. The reader should be informed about the general relevance of oceanic processes for the North Atlantic climate and the influence of the volcanic

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forcing. Moreover, authors should motivate why the EMIC is an appropriate tool to address their specific question of interest, especially given the fact that there are readily simulations available with comprehensive AOGCMs including internal variability with data that are freely accessible [<http://esgf-data.dkrz.de/esgf-web-fe>]. In the current form I see no reason why this study should outperform AOGCM studies previously carried out. The present setup is just a replication of studies that have already been carried out. As such they should at least be addressed and the results of the EMIC, given the first- order-assessment of the generation of atmospheric forcing fields, should be critically assessed. I would also expect a more detailed description and discussion of the basic influence of volcanic eruptions on climate, specifically in the North Atlantic realm. There is a broad band of literature from empirical and modelling studies describing the effect of volcanic outbreaks on climate including ocean circulation (cf. Zanchettin et al., 2012, Timmreck, 2012; Robock, 2000 and earlier studies of Stenchikov et al., 1998 and Kirchner et al. 1999 related to Pinatubo) Also articles addressing the sensitivity to wind and thermal forcing on the variability on the Atlantic meridional overturning, the Atlantic multi-decadal oscillation and their potential impacts on ocean circulation should be mentioned (Hunt et al., 2012). This would then also allow to introduce potential internal climate variability as one factor inducing anomalous climatic periods.

We thank the reviewer for the suggestion of additional references that greatly helped to improve the manuscript. In a recent model intercomparison of complex coupled climate models over the last millennium by Fernández-Donado et al. (2013) the authors found little agreement between the models in the spatial distribution of simulated temperature changes during the MCA - LIA transition (compare their Fig. 7) and concluded: “Thus, either internal variability is a possible major player in shaping temperature changes

through the millennium or the model simulations have problems realistically representing the response pattern to external forcing.”

In our view, these results indicate that the mechanisms behind the MCA-LIA transition are still not fully understood. Due to their reduced computational demand EMICs can help to explore possible mechanisms. Recent studies utilizing EMICs to investigate climate effects of the last millennium are e.g. Sedlacek and Mysak (2009), Goosse et al (2012) and Eby et al. (2013).

We disagree with the reviewer that our study is just a replication of already published work. We explore the shortcomings of the less complex atmosphere by applying wind-stress reconstructions based on the NAO time series by Trouet et al. 2009. Sedlacek and Mysak (2009) follow a similar approach with the EMIC UVic limited to the last 500 years. We extend such an approach to the last millennium and additionally perform a stochastic method with 10 ensemble members.

The aim of such an approach is not to achieve highest accuracy in the individual reconstruction, which indeed would be inappropriate to perform with an EMIC, but rather to perform simulations covering a large uncertainty range. The main finding of our study, the existence of a coupled ocean sea-ice mechanism in our EMIC model, is independent from the individual reconstruction. We emphasize that the regime shift found in our model simulations improves the agreement with reconstructions.

Comment 2

The author state the CLIMBER was used in several study but especially for the oceanic component but only little is done to justify the usage of the model to test their hypotheses. i.e. providing difference maps between modelled and observed SSTs, sea ice or salinity – surface ocean currents are also important fields for the characterization of oceanic processes in the model world – when the plots for observational periods are not provided at

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least a paragraph addressing these points or including the results of related studies should be mentioned.

The EMIC model Climber 3α contains an oceanic component based on the GFDL MOM-3 code with 24 vertical layers. A detailed model description including comparison with observations is presented in the Montoya et al. 2005 paper for surface sea temperatures and salinities (their Fig. 12) as well as sea-ice extent (their Fig. 23). Their Fig. 17 shows the surface ocean currents and chapter 3.3.2 discusses the model results in comparison with observations.

Climber 3α performs well in reproducing large-scale characteristics of ocean and sea-ice dynamics. On the regional scale its coarse resolution and the simplicity of its atmospheric component limit its performance. For our study region, this is the case for the overflows over the Greenland-Scotland Ridge, where the coarse resolution leads to an overflow over the Denmark strait that is larger than suggested by observations. Furthermore, due to the coarse resolution of our model the SPG does not extend to the Labrador Sea, which is of course a limitation for the model capability to reproduce observed dynamics in this region.

We feel that we addressed this issue in our manuscript as it is, but we agree to be more detailed on the issue, since our model shortcomings within the region of interest are important when evaluating the model outcome. We also commented on this issue in the discussion section of the original manuscript (see p. 6208 l. 15 ff.)

We modified the model description paragraph as follows:

“Due to the coarse resolution, the SPG circulation does not extend to the Labrador Sea and therefore subpolar convection is limited to the central Irminger basin.”

“CLIMBER-3 α was found to reproduce large-scale characteristics of the global climate system. On the regional scale, the coarse model resolution and the simplified atmosphere limit its capabilities. For a comprehensive discussion of the model performance and comparison with observational data see Montoya et al. (2005).

Due to the coarse resolution, the SPG circulation does not extend into the Labrador Sea and therefore subpolar convection is limited to the central Irminger basin. The overflows over the Denmark strait are too strong resulting in an overestimated ice export by the East Greenland Current. Winter sea-ice extent over the Labrador Sea is found to exceed observations. “

Comment 3

p. 6203, ll. 14ff.: The authors should state due to construction the variability of the atmospheric circulation is restricted by the bandwidth of the NCEP/NCAR data for the 2nd half of the 20th century. I still do not understand why the authors do not use wind fields of available studies carried out with comprehensive GCM studies – most of these studies are freely accessible in the context of the PMIP3 simulations and would circumvent most problems related to the bandwidth of potential NAO states and also implicitly include internal climate variability. Moreover the influence of the atmospheric circulation was also previously studied for present day climate with more complex models – some of these studies could for instance be mentioned in this chapter

We feel it is important at this point to clarify again the scope of our study: Our aim by using an EMIC is not to perform individual runs with high accuracy (for which we are indeed limited by the simplified atmospheric dynamics of our model), but rather to

perform ensemble studies comprising a larger uncertainty range and to explore basic mechanisms explaining the MCA-LIA transition. This would not be possible with an assessment based on existing runs with AOGCMs. The stochastically constructed wind-stress fields comprise an uncertainty range for possible wind-stress fields. This ensemble exhibits a considerable dynamical spread (compare Fig. 2). However, the wind-stress ensemble spread alone cannot explain the MCA-LIA transition in the Mann et al. (2009) reconstruction, but a coupled sea-ice ocean mechanism is identified as a possible origin of this transition. Besides the short-comings of the method e.g. due to the limited band-width as discussed in our manuscript on page 6204 L 3 ff. this conclusion is based on proxy reconstructions combined with a statistical method and can thus be seen as a complimentary approach to the ones mentioned by the reviewer.

Comment 4

p. 6204, l.25: Why do the authors – while using volcanic forcing for the spin down – not use the Crowley 2012 data set from 850 AD onward ? Also the reader needs a lot of information of the Mengel et al. 2012 study to understand the exact experimental setup. In this context it would be important to summarize the most important settings with respect to the hypotheses the authors would like to address with their study. For instance I do not understand what ‘repeated NCEP wind field variability’ means. I guess the authors perform a perpetual forcing with the NCEP data with selected years or the whole 60 yr time series. Another point is that volcanoes also influence atmospheric circulation. As the wind forcing and the volcanic forcing are independent from one another this could, especially in the North Atlantic region also have effects (cf. mid- winter warming after large tropical eruptions) and could partly offset the direct cooling effect of large tropical eruptions.

We thank the reviewer for this comment and modified the corresponding paragraph:

“During the spin-up time, repeated NCEP wind-field variability was applied as in Mengel et al. 2012 and the volcanic forcing mirrored from the years 1000-1150 was applied for the 850-1000 period.”

To:

“During spin-up, the applied wind-stress anomalies are based on the NCEP/NCAR 1948-2009 wind-stress time series. To avoid discontinuities the 1948-2009 period is combined with reversed 2009-1948 time series (Mengel et al. 2012). The resulting 122-year time series is cyclic and continuously applied during spin-up. The volcanic forcing mirrored from the years 1000-1150 was applied for the 850-1000 period.”

We do agree that using the Crowley 2012 dataset from 850 AD would have been a better option than the mirrored volcanic applied during spin-up. Still, we would expect the differences to be marginal, since their reconstruction exhibits no period of exceptional volcanic activity during 850 AD -1000 AD (compare Crowley et al., their Fig. 7).

Paleo-reconstructions as well as GCM studies suggest a positive NAO state as a dynamical impact of tropical volcanic eruptions possibly leading to the winter warming mentioned by the reviewer (e.g. Fischer et al. 2007, Shindell et al. 2004). Our EMIC atmosphere is not capable to reproduce these effects as we state on P 6203 L 11:

“Aerosol forcing is only considered in its direct impact on the TSI, since our atmospheric component lacks a sufficient representation of other climate impacts of aerosol release.”

However, since our atmospheric forcing is based on a paleo-reconstruction, this method does to some extent account for the discussed NAO-type response. Reviewer

2 also commented on this issue, please find the modifications below in our response to Question 1 of Reviewer 2.

Comment 5

6205, ll. 11.: In my opinion this statement is not a proof of concept but rather a disproof of concept as already slight changes could lead to pronounced changes and hence I assume that the following model results are very sensitive to the tuning of specific model parameters – authors should state in greater detail the implications of this sensitivity. The clustering of volcanic outbreaks is also evident prior to the onset of the LIA, including even more pronounced eruptions for instance around 1259 and the subsequent decades. Why does ocean circulation not already respond in these earlier times ?

We do agree with the Reviewer that the terminus “proof of concept” might be misleading in this context and we deleted the sentence

P 6205 L 15:

“Therefore, the timing of this transition should not be over-interpreted but rather be seen as a proof of concept.”

and added the following paragraph:

“The actual timing of the transition should not be over-interpreted, since its very sensitive to minor changes in the North Atlantic freshwater budget and also exhibits a considerable ensemble spread (compare Fig. 2). Yet, our results indicate that a transition in the SPG circulation regime might have been triggered during the onset of the LIA.”

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Thereby, we cannot rule out that such a transition could have also been triggered around 1259 and subsequent decades as suggested by the reviewer. Identifying the timing of the transition is beyond the scope of our study, but we would like to emphasize that the possibility of such a transition during the last millennium could help to understand observed climatic changes between MCA and LIA.

As shown in Mengel et al. (2012), the oceanic response to atmospheric forcing in our model performs better compared to observations, when the system is close to its internal threshold. To test this hypothesis, we performed additional experiments with a 5 mSv FW offset and the results show a better match with the reconstruction as discussed with the manuscript. We cannot rule out that this sensitivity is a model artifact, but a multi-stable SPG circulation regime has also been identified in other climate models including AOGCMs (e.g.. Schulz et al. 2007, Born et al 2012).

Comment 6

p. 6206, ll. 7 ff: The authors only use one possible reconstruction of the TSI – how would other scalings of the TSI potentially affect the evolution of the SPG, i.e. how sensitive does the model react on different scalings ?

Review studies by Gray et al. (2010) and Lockwood (2012) discuss an emerging consensus between different reconstruction methods with regard to TSI scaling with variations being close to the one used in our study. Among recent published studies, the reconstruction presented by Shapiro et al. (2011) shows considerably higher scaling. Issues connected to this reconstruction approach are discussed e.g. by Schmidt et al. (2013), and in Feulner (2011). Model results comparing different TSI reconstructions are presented, where the Shapiro reconstruction leads to temperature variations beyond the reconstructed uncertainty band.

We therefore do not see the need to perform a dedicated sensitivity study with higher

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TSI variability, but we have added the following paragraph to the result section:

“These conclusions are, however, based on the TSI reconstruction applied; the result may look different for TSI reconstructions showing larger variability.”

Comment 7

6206, l. 10: How is the volcanic forcing included in the atmospheric response – as much as I could understand the atmospheric forcing is based on a stochastic reconstruction based on NCEP data

This remark is similar to the comment 4 and to comment 1 by the second reviewer. We do agree that the statement on this in the original manuscript is not clear and strongly modified the respective paragraph, see our response to comment 4 and to comment 1 by the second reviewer below.

Comment 8

p. 6207, l 8 ff: In the context of changes in AMOC authors cite a study suggesting that the increase in sea ice reduces ocean convection – more important here would be how the mechanism in CLIMBER works and the authors should be careful in mixing results between different hierarchies in climate models because the controlling processes and time scales might be different.

As shown in Fig. 1 (see below), the sea-increase in our sensitivity study leads to an immediate reduction of oceanic heat release over the region of interest in the Nordic

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Seas (less negative values mean less heat release) and to a substantial weakening of deep convection in the same region. As a consequence, we report a weakening of the overflows subsequently leading to the AMOC reduction as discussed in the manuscript.

To clarify between the mechanisms in Climber 3α and other studies we modified the paragraph:

6207, l 8

“This increase in sea-ice cover reduces ocean heat release and thereby hinders convection (Marotzke, 2012).”

to

“This increase in sea-ice cover reduces ocean heat release and thereby hinders convection in our model.”

Comment 9

p. 6208, l. 5 ff. The fact that the EMIC model results agree and/or disagree with other modelling results should be viewed in terms of physical considerations. This way it can be interpreted as a coincidence that results are consistent or inconsistent.

The paragraph p. 6208 l 8 ff. contains a discussion of our results compared to the Zhong et al. (2012) model study and we also suggest reasons for the different model dynamics in both models. Unfortunately, we do not have access to the diagnostic output of the Zhong et al study and therefore lack the basis for a more detailed discussion. We modified the following paragraph:

p. 6208 l. 15:

“We cannot rule out that this result is due to the displacement of our sub-polar convection site from the Labrador Sea to the central Irminger Sea, but recent results of a high resolution model study support our finding of an anti-correlation of AMOC and SPG dynamics (Zhang et al., 2011b).”

to

“We cannot rule out that this result is due to the displacement of our sub-polar convection site from the Labrador Sea to the central Irminger Sea. A recent high resolution model study also reports an anti-correlation of AMOC and SPG dynamics (Zhang et al., 2011b) depending on the strength of the GSR overflows.”

Comment 10

Figure 1: Please separate forcings from model results.

Following the suggestion of the reviewer, we separated the original Fig. 1 into one figure showing the forcings and a second one depicting the model results.

Anonymous Referee 2

Comment 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.

This contradiction the reviewer points at with very good reason is a result of the unclear first statement and we thank the reviewer for this remark.

It is, of course, not our intention to claim that our stochastic reconstruction method is capable of capturing the physical impact of volcanic eruptions on the North Atlantic wind-field. This is far beyond the scope of our modeling approach, but the statement made in the manuscript is misleading on this. This comment is similar to remarks made by the first reviewer, see our response to reviewer 1 comment 4 above.

We changed the following paragraphs in our manuscript:

L240:

*"Our method also incorporates atmospheric variability induced by external
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forcing, because imprints of both TSI changes and volcanic eruptions have shaped our reconstructed NAO time- series”

To:

“Paleo-reconstructions as well as GCM studies suggest a positive NAO state as an dynamical impact of tropical volcanic eruptions possibly leading to a European winter warming (e.g. Stenchikov et al., 1998; Kirchner and Stenchikov, 1999; Shindell, 2004; Fischer et al., 2007;). Within methodological limitations, the NAO reconstruction by Trouet et al. (2009) thus incorporates effects of volcanic eruptions and changes in the total solar irradiance. Restricted by our statistical approach as discussed above, these imprints are also present in our reconstructed wind-stress time series.

Deleted L252:

“Since the atmospheric response to volcanic forcing is incorporated in the wind-field reconstructions, we can conclude that the direct sea ice-oceanic response is of major importance to Northern Hemisphere climate variability over the last millennium.”

Added L178:

“Our purely statistical method is not capable to capture the physical processes behind NAO variations.”

Comment 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

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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.

As the reviewer points out, the paragraph describing the experiment could be understood, as if we were applying a constant freshwater offset as in the Mengel et al. 2012 sensitivity study. “The offset applied in their model study is 15mSv In addition to the experiment described above, we present results for runs with a smaller constant 5mSv offset.”

This is misleading, since we performed our experiments without such an additional freshwater offset. However, the response of the subpolar gyre to prescribed wind-stress forcing in our model Climber 3 α has been found to be sensitive to such a freshwater offset in the Nordic Seas as described in Mengel et al. 2012. Based on this finding, we also performed ensemble simulations with a freshwater offset of 5mSv and find the SPG to increase sensitivity to the applied external forcing.

We clarified the paragraph:

“In addition to the experiment described above, we present results for runs with a smaller constant 5mSv offset.”

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Comment 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.

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We thank the reviewer for pointing out this issue and we do agree that we should show AMOC dynamics. Therefore, we added the dynamics of the AMOC to Fig 2, 3, 4 and 5. We modified the results section in the manuscript accordingly and discussing AMOC and AMO dynamics separately.

We find a persistent AMOC reduction as a consequence of the SPG spin-up (as in Mengel et al., 2012) of about 5 % during the LIA. This reduction leads to a SST and therefore AMO reduction and our separation between the ‘No FW Offset’ and the ‘5 mSv FW Offset’ scenario allows to clearly attribute the cooler AMO for the ‘5 mSv FW Offset’ ensemble during the 16th and 17th century to this oceanic changes, since the imposed radiative forcing is the same.

Comment 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 decadal 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

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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 decadal or even sub-decadally spaced eruptions.

On page 6207 L 18 we state:

“To test this hypothesis we prescribed JFM winter sea-ice coverage over the northern convection region (Fig. 4a grey box) to be fully covered as after volcanic eruptions in a control run with constant TSI and without prescribed atmospheric variability.”

In our understanding this paragraph describes the sensitivity experiments we performed and presented in Fig. 5 (old Fig.4) of our manuscript in a correct manner. As shown in Fig. 5a, volcanic eruptions lead, beside other profound impacts on the climate system, to a sea-ice increase in the Nordic Seas. We artificially prescribe the JFM sea-ice coverage as discussed in the following paragraphs, but did not impose any volcanic forcing.

For clarity, we added the following sentence before the paragraph:

“Our results suggest that sea-ice increase as a consequence of volcanic eruptions can trigger the SPG regime shift in our model and that this sea-ice forcing has to be persistent in time for longer than the imprint of a single volcanic eruption.”

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Comment 5

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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, increased 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.

We do agree with the reviewer that discussing the AMOC response without showing it is indeed inappropriate. Following the reviewers remarks above, we replaced the AMO and added the AMOC as an index in Fig. 5. We also clarified the nature of the forcing applied that to our understanding was misinterpreted by the reviewer.

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Figures

Fig.1: Results of the constant artificial sea-ice forcing run shown in Fig. 5 of the manuscript. The first panel shows the Nordic Sea sea-ice extend as in Fig. 5 of the manuscript. The second panel depicts the yearly heat flux over the the Nordic Sea region of interest from 40w:20 and 64:80N. Negative values indicate flux from ocean to atmosphere. The average Mixed Layer depth in February over the same region is shown in the third panel and the GSR overflows in the lower panel.

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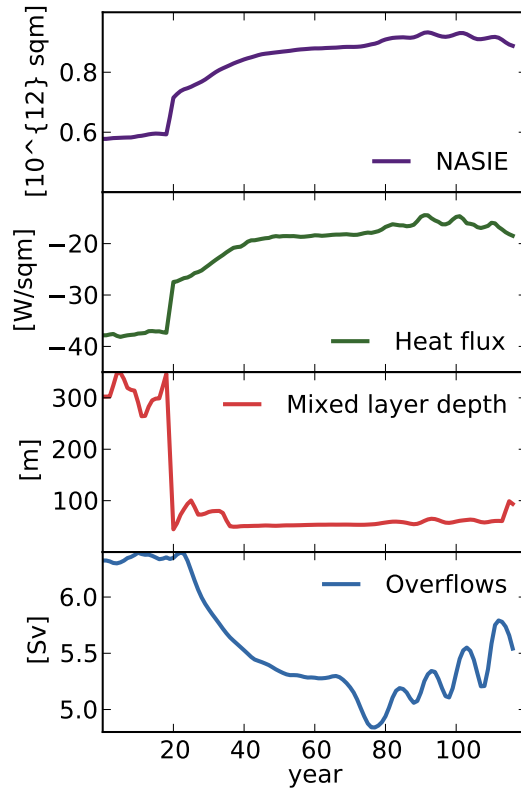


Fig. 1.

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