The Cryosphere Discuss., 6, C1394–C1400, 2012
www.the-cryosphere-discuss.net/6/C1394/2012/
© Author(s) 2012. This work is distributed under
the Creative Commons Attribute 3.0 License.

The Cryosphere
Discussions

Interactive comment on "A recent bifurcation in Arctic sea-ice cover" *by* V. N. Livina and T. M. Lenton

Anonymous Referee #1

Received and published: 4 September 2012

This paper uses statistical methods to look for evidence of bistability in Arctic sea ice observations. The paper reports that the observations imply a single high ice cover state during most of the record, but that the presence of a 2nd stable state with low ice cover is detected from 2007 onwards, implying a bifurcation. The paper paints a picture of the sea ice cover as a noisy system that samples both high and low ice cover states after 2007.

This is a welcome addition to previous studies on this topic which have relied on the results of physical models rather than observations. It addresses a topic of broad interest, all the more so in light of the current conditions in the Arctic this month. The paper is extremely clear and well written. However, the paper is riddled with serious issues that draw every step of the story into question, as discussed below.

(1) The analysis method is not necessarily suited to the datasets.

The paper uses an analysis that draws primarily on a "potential analysis". It also draws on the traditional ACF and variance indicators of stability in a noisy system, as well as DFA, to look for "early warnings" of an abrupt shift associated with the loss of stability of an underlying state in a time series.

The authors are encouraged to examine the recent paper by Agarwal et al. (doi:10.1098/rspa.2011.0728), who found that using temporally weighted DFA (TWDFA), rather than standard DFA, was important for capturing the long-term scaling behavior in sea ice cover datasets. The authors use standard DFA on sea ice cover datasets.

The potential analysis method - the center point of this analysis - applies to systems evolving in a 1D potential well with white noise forcing, i.e., systems described by a 1D Langevin equation [eq (1) in paper]. The point of the potential analysis is to determine the underlying potential well from the noisy time series. This methodology was recently developed by the authors of this study, and it is relatively untested in its effectiveness, although the authors cite one recent study where they "rigorously blind-tested" the method. That study is highly relevant to the paper at hand, so I'll summarize it here.

In Livina et al. (2011a), Ditlevsen sent Livina & Lenton 9 sample time series, each with 4000 points. Livina & Lenton were tasked with (blindly) identifying the equations used to generate each sample. The first 7 samples were generated with a simple 1D Langevin equation (which their analysis assumes) with or without periodic forcing; all 7 of these samples were generated using a double-well potential (representing two stable states) and had white noise forcing and sometimes periodic forcing with parameters that differed between the samples. Livina & Lenton then used potential analysis, along with ACF and DFA analyses, to examine the samples, and they guessed what equation had generated each sample based on visual inspection of their results as well as qualitative intuition. In 6 of the 7 samples, they correctly identified that it was generated with a Langevin equation with a double-well potential and whether there was periodic forcing, although the parameter value estimates were not accurate. It is noteworthy to mention that their potential analysis contour plots in that paper appear to robustly show 3 stable states in samples 4, 6, and 7, which is erroneous, and that the plots also appear to show a number of states that varies in time in many samples (especially samples 1 and 5), but Livina & Lenton were able to intuitively identify these as spurious aspects of the analysis. The last 2 samples sent by Ditlevsen were from different equations, namely a 3D chaotic system and a system with an imposed drift and a jump when a threshold is reached. Of these, the chaotic system was misidentified and the drift/jump system was identified by visual analysis of the time series with virtually no assistance from the quantitative analysis methods. It is worth noting that the method thus failed for the only system it was tested on with more than one degree of freedom, suggesting a limited applicability to actual physical systems. The 2011a paper was a highly interesting study, but two points need to be highlighted in the context of the present study: (i) despite the title of the 2011a paper mentioning the detection of "bifurcations in time-series data", none of the time series data analyzed in that study actually had a bifurcation; and (ii) the method was only demonstrated to be powerful (with a 6/7 success rate) when applied to samples generated with 1D Langevin equations.

With this in mind, the methodology employed in this study is far from established for detecting bifurcations in general 1D ODEs, not to mention complex spatially continuous (PDE) systems such as the Arctic climate. Such a connection could be possible (cf. center manifold theory), but with only the present knowledge in hand it requires a bold leap of faith. In the interest of providing a complete review, however, I will take this leap of faith in what follows.

(2) The results depend critically on what metric of sea ice cover is employed, and the analysis completely fails when equivalent sea ice extent is used.

The main point of the paper is summarized in Fig. 3, which shows the results of the analysis applied to daily sea ice area data. Although there are many regions in Fig.

3b where the analysis seems to imply 2 states (green) before 2007, they are rather patchy, whereas starting in 2007 there is more pervasive green shading. Fig. 3b does not unequivocally show a transition from red (1 state) to green (2 states) in 2007, but the plot is certainly suggestive of such a transition. Fig. 3c is even more clear: the reconstructed 1D potential for each 4-year interval is single-well except in 2008-2011, where it is clearly double-well. I note that the caption says, "In the penultimate interval 2004–2007 a second state starts to appear", when in fact there appears to be no evidence for bimodality in Fig. 3c for 2004-2007 (there is almost bimodality in 1996-1999, but not in 2004-2007). The actual PDFs in Fig. 3d are less clear, with 2008-2011 appearing to indicate 3 rather than 2 states.

The authors also consider several other sea ice cover datasets, which is commendable on their part and important for accessing the robustness of this analysis. The figures indicate, however, that their analysis is anything but robust to changes in the ice cover dataset. Ice extent is a very similar measure to ice area - the two are normally used almost interchangeably. When their analysis is applied to daily sea ice extent (Fig. A2), there is no longer bistability in the reconstructed potential for 2008-2011 (Fig. A2c). Although there is a hint of possibly emerging bistability in 2008-2011 in the ice extent data, it is no more dramatic than the hint of emerging bistability that their analysis suggested for ice area during 1996-1999. In other words, the claim in the abstract that the authors "show that a new low ice cover state has appeared from 2007 onwards" does not appear to be supported by the sea ice extent data (appearing only for the sea ice area data).

When the authors plot daily ice area data only from summer-autumn or winter-spring of each year, there is no longer any clear onset of 2 states (Fig. 7). Instead, there are patches of green (2 states) throughout much of the record, with no substantial change occurring in the late 2000s.

It has been widely discussed that the amplitude of the Arctic sea ice area seasonal cycle has been increasing. This was addressed in Eisenman (2010), who argued that analyzing measures such as the sea ice area and extent causes errors associated with the shape of the Arctic coastlines. For this reason, the "equivalent sea ice extent" metric was introduced to account for the influence of land masses. When the authors apply their analysis to equivalent sea ice extent, the entire argument collapses: there is no evidence for bistability at any point in the observational record (Fig. A4b). The authors acknowledge this in the text - "our detection of a recent bifurcation in sea-ice cover could be (at least partly) a geographic property of the shrinkage of summer-autumn ice cover away from the continents facilitating larger fluctuations" - but this does not lead them to back down from their claim in the title/abstract/conclusion that they have robustly detected a bifurcation. This point is related to Ditlevsen's comment in this Discussion about the possibility of spuriously detecting emerging bistability due to changing seasonal cycle amplitude.

In addition to daily sea ice extent, area, and equivalent extent, the authors consider a reconstruction of annual sea ice extent from 1870. They do not consider evidence of bistability in this dataset, instead focusing on destabilization indicators. This dataset shows a steadily increasing variance during the 1979-present period of overlap with the daily observations (Fig. 6). In contrast, the daily observations of sea ice area (Fig. 4g) and extent (Fig. A3g) show a decreasing variance for most of this period.

In summary, the story in this submission completely changes whenever a different dataset is used, and it fails completely when the most sophisticated dataset (equivalent sea ice extent) is used.

(3) The dataset seems too short for this type of analysis.

The authors explain that the success rate of the potential analysis is 80% "when the window contains more than 400 data points (which in the case of daily sea-ice data corresponds to about 1.1 yr)". But the sea ice cover data has an autocorrelation time of about 1-3 months, implying that the entire dataset has only a couple hundred independent data points (i.e., effective degrees of freedom). This seems to imply that

if the authors would like to use this type of analysis to identify a changing underlying potential from one period to the next, each of the periods would have to be about twice as long as the entire dataset. Or is the potential analysis somehow able to mine more information out of the dataset than that implied by the number of effective degrees of freedom?

(4) The suggestion of a new approaching bifurcation is not well grounded.

The paper concludes, "The detected ongoing destabilization of the summer-autumn sea-ice cover suggests that a further bifurcation may be approaching." But Fig. 4c (ACF-indicator) indicates a destabilization since 2007 that is similar to the mid- to late-1990s (with the later period actually being more stable and showing a slower trend toward destabilization than the 1990s). In the 1990s, the record shows that this detected destabilization was followed by a decade-long period of steady stabilization, rather than a bifurcation. In other words, the text in the conclusion seems to imply that the recent destabilization is an unusual feature of the record, whereas the actual plotted results imply that it is not unprecedented in the 30-year record.

(5) The paper is inaccurate about the relevant physics.

This paper for the most part deals solely with statistics, but the physics causing the variability of the sea ice cover is addressed in the conclusions (Fig. 10). This discussion of the physics appears to be substantially out of touch with previous literature and knowledge.

The physics governing sea ice evolution varies substantially during the year. The standard textbook picture is that sea ice grows during winter due to the freezing of sea water at the base of the ice, whereas it melts during the summer primarily at the top surface of the ice. Similarly, the ice-albedo feedback operates in the summer, but not during the polar winter. In other words, even in the simplest sense, the evolution of sea ice is governed by very different equations in summer than in winter.

So based on our current understanding, viewing sea ice as a system that's being pushed back and forth seasonally in an otherwise constant dynamical system, as the authors do in Fig. 10, seems to completely miss the mark.

(6) The language - especially in the title and abstract - is incompatible with the uncertainty of the results.

Given that the authors are speculatively applying an analysis method meant for 1D stochastic ODEs to a single measure of a complex spatially continuous physical system, and that their analysis varies qualitatively when the details are changed such as using an alternative but largely equivalent observational dataset, one would expect the paper to have a very different message than it does. In this reviewer's opinion, a title like "investigations into the possibility of a bifurcation in Arctic sea ice observations" with an abstract describing the novel methods used and the varied results obtained for each dataset, would be appropriate for this analysis. Instead, the title and abstract claim that a bifurcation is concretely detected in 2007, telling a very different story than the actual results of the analysis. This disconnect between the uncertain and tenuous results of the analysis and the concrete language of the presentation should be bridged.

Minor comment:

The authors say "SMMR operated every other day in three months during the record, in 10/1978, 12/1987 and 1/1988", and they explian that they interpolate to daily resolution for these months. But there is actually only SMMR data for 3 days during 10/1978 and no data at all during 12/3/1987-1/13/1988. While perhaps not crucial for their analysis, the authors should clarify if they have interpolated over a 40-day continuous hole in the data onto a daily grid.

Interactive comment on The Cryosphere Discuss., 6, 2621, 2012.