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The Cryosphere Discussions

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

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We thank the reviewer for taking the time to give such detailed criticism. They make many valid points, but they seriously misrepresent our results on a crucial point: When comparing analyses of sea-ice area and extent they mistakenly relabel the time intervals, then use this mistake to support their argument that "the results depend critically on what metric of sea ice cover is employed". We think this remark is wrongly predicated, unfair, and should be withdrawn. The claim that "the story in this submission completely changes whenever a different dataset is used" is correspondingly unfair and overstated.

The key comments (in *italics*) and our replies to them are listed below:

Comment 1. *The analysis method is not necessarily suited to the datasets*

Response. We deliberately start with the simplest model and with methods designed to detect, and in some cases anticipate, bifurcations – so the methods are at least suited to the scientific questions being addressed.

1a. *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.*

Response. The paper by Agarwal studies long-range power-law correlations and fractal scaling. By comparing MF-DFA and MF-TWDFA, Agarwal et al state the following: "We found that the crossovers for time scales of 2 years or longer would not have been captured by MF-DFA because of large-amplitude fluctuations in this range."

By employing DFA-indicator in our work, we study short-term correlations in interval 10-100 time units. This means that the objects of our study and the study of Agarwal are different. The reviewer can compare the curve for moment q=2 in Agarwal's figure 5d (reproduced below), with the following curve calculated using the convential DFA used for DFA-indicator, see the attached figure.

One can see that the modification introduced in TW-DFA is irrelevant for the DFA-indicator, because the improvement it provides appears in the asymptotic scales, whereas the DFA-indicator is calculated in short-range scales 10-100.

1b. *The potential analysis method - the center point of this analysis... was recently developed by the authors of this study, and it is relatively untested in its effectiveness...*

Response. We have tested the method on a range of artificial (model-generated) data and paleo-data, such that in our view it is not "relatively untested".

The reviewer states elsewhere that "the methodology employed in this study is far from established for detecting bifurcations in general 1D ODEs", but they do not mention Fig.2 of our Physica A paper (a bifurcating series), which clearly they have read:

http://www.sciencedirect.com/science/article/pii/S0378437111006534

There are also example tests in the Climate Dynamics paper:

http://www.springerlink.com/content/n08331082327722v/

where Fig.2 shows test of the data with detection success rate, and in the Climate of the Past paper:

http://www.clim-past.net/6/77/2010/cp-6-77-2010.html

with its Figure 2 showing a further test on data with changing numbers of underlying states.

In the latter paper (Figs.3,4) the method is also able to identify the same inferred paleoclimatic bifurcation using two completely different proxy data sets (oxygen isotopes and calcium in dust). Moreover, there is a recent manuscript by our Dutch colleagues:

http://www.clim-past-discuss.net/8/4269/2012/cpd-8-4269-2012.pdf

using a different method to detect the same inferred bifurcation in paleo-data.

1c. *...the authors cite one recent study where they "rigorously blind-tested" the method... 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.*

Response. The use of the word "rigorously" was perhaps an over-statement in response to criticisms received elsewhere, but still it is notable that we have blind-tested our method - there are many published analyses of geophysical data using new methods that have not been blind-tested.

The effects mentioned by the reviewer are due to imposed periodicites (sine waves added to the Langevin equation), and the additional detected state is the result of the nonstationarities of partial periodicities in subsets of the data. The defining equations are shown in page 492 of the PhysA paper:

http://www.sciencedirect.com/science/article/pii/S0378437111006534

We identified the nonstationarites and modified the Langevin equation accordingly. It was not done "intuitively", but was rather based on empirical observations.

1d. *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.*

Response. The applicability of the method may be limited, but the inference of a bifurcation in Greenland ice-core data (which undoubtedly represents a complex multi-dimensional system), has recently been confirmed with a more sophisticated multi-dimensional analysis technique:

http://www.clim-past-discuss.net/8/4269/2012/cpd-8-4269-2012.pdf

The stochastic model used for potential analysis is an approximation of the system dynamics based on probability density function. It is obvious that various dynamical system may have similar pdfs, and it is crucial to use various time scales for analysis in order to capture the actual dynamics. Moreover, the same dynamical trajectory can be produced by various models, and the sample 8 in Physica A paper was reproduced in our simulations with very similar properties

- though using a very different model. This does not mean that the potential analysis is not applicable, because it still provides useful information about the dynamics and shows whether the simulated data has the same properties as the test data.

Moreover, sample 9 of Physica A paper demonstrates the ability of the method to distinguish between potential and non-potential behaviour in the data, i.e. whether the probability density at various time scales vary the number of states/modes.

Furthermore, the modelled and test data have equivalent potential portraits, which means that the evolution of the probability densities is similar.

Comment 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.*

Response. The results do NOT depend on the switch between sea ice 'area' and 'extent' datasets, the reviewer is misled by making a mistake in shifting the time axis between these datasets. They DO however change when using the equivalent sea ice extent, but we are less enthusiastic about the pertinence of this dataset than the reviewer, because it is hypothetical data (the areas refer to a world with no northern continents). This dataset has much greater variability than actual sea-ice extent or area, which could then mask / over-ride the relatively modest signals of switches between summer ice-cover attractors that we hypothesise we have detected in the real data.

2a. *I note that the caption says, "In the penultimate interval 2004û2007 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.*

Response. There is a shallowing of the potential on one side in Fig. 3c for 2004-2007 and in Fig. 3d for 2004-2007 some bimodality is clearer - so we can rephrase this accordingly. Fig. 3d 2008-2011 does indeed suggest three states, which we refer to as "a separation of modes" in the text, but can expand on this.

2b. *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).*

Response. This comment is based on a mistake and should be withdrawn. The referee wrongly refers to the last 3-year interval (2007-2009) of sea-ice extent data in Fig. A2 (which is clearly labelled), as the 4-year interval 2008-2011, and then goes on to argue that it doesn't agree with 2008-2011 in the sea-ice area analysis. In fact the two datasets give very consistent results when one does not make this time-shifting mistake! In Fig. A2c the 2007-2009 interval shows one stable state and a degenerate state next to it, the corresponding histogram in Fig. A2d also shows some bimodality. Clearly the intervals cannot be directly compared with Fig. 3 as they are overlapping but 2007-2009 in Fig. A2 gives a result that is intermediate between 2004-2007 and 2008-2011 in Fig. 3 - which is why we claim the results are consistent! The reviewer uses their mistake to support their argument that "the results depend critically on what metric of sea ice cover is employed". We think this comment is wrongly predicated, unfair, and should be withdrawn.

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2c. *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.*

Response. Clearly results (like beauty) are in the eye of the beholder! We think there is still a distinct transition in 2007 particularly in the summer-autumn analysis, but it seems to overlap into the other seasons as well.

2d. *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...*

Response. Yes, the increase in amplitude is widely discussed, but we emphasize that it increased abruptly and persistently in 2007, suggestive of non-linear dynamics. Eisenman suggests that the increase in amplitude of the seasonal cycle is caused by the existence of large Northern Hemisphere land masses continuing to constrain fluctuations in winter ice area, whilst summer area has now shrunk largely within the land masses. We offer a different hypothesis to explain why the change in amplitude is abrupt; namely that the sea-ice is sampling a new, lower ice-cover attractor in the summers.

We are less enthusiastic about the pertinence of the equivalent extent dataset than the reviewer, because it is hypothetical data (the areas refer to a world with no northern continents). This dataset has much greater variability than actual sea-ice extent or area, which could then be masking the relatively modest signals of switches between summer ice-cover attractors that we hypothesise we have detected in the real data.

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2e. *their claim in the title/abstract/conclusion that they have robustly detected a bifurcation*

Response. We do not claim in the title, abstract or conclusion that our result is "robust". We use the phrase "suggesting a bifurcation" in the Abstract. Clearly we need to tone down our presentation.

2f. *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.*

Response. These two series have different temporal resolutions, the historic reconstruction is annual data, so it would be meaningless to look for bi-stability in it that we think manifests on a sub-annual timescale. As for the difference in variance trends, inter-annual variability is a completely different thing to variance at a daily resolution. Furthermore, differing window lengths are used in the two analyses. In revising the paper we can do some data aggregation and attempt a more direct comparison, but it is probably simpler to remove the analysis of historical data as it is causing confusion.

2g. *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.*

Response. The area and extent datasets give very consistent results – it is only the 'equivalent extent' that behaves differently – and it describes a hypothetical world. We cannot agree that hypothetical area fluctuations should be prioritized over real ones in analysis of the sea-ice. The analysis may fail because the much greater fluctuations in 'equivalent extent' simply mask what are then relatively subtle signals in predominantly summer area cover that we claim to have detected.

Comment 3. *The dataset seems too short for this type of analysis.*

Response. Of course we would all love a longer dataset, but what is important about the sea-ice is that the dataset is significantly longer than the timescale of memory in the system, and the resolution of the data set is significantly higher than this memory timescale. Hence it should be a good candidate for the detection of bifurcations.

3a. *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?*

Response. The referee makes a good point, but the quantification of success rate we quote also comes from a model where there are autocorrelations. We can look at this in more detail in revising the paper.

Comment 4. *The suggestion of a new approaching bifurcation is not well grounded... 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.*

Response. The referee is right to flag this up as a speculative remark at the end of the paper. They make a good point regarding the indicators and we will tone down accordingly.

Comment 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 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.

Response. We already appreciate these points about the physics, and only intended Fig. 10 as a "Schematic" cartoon. Our hypothesis is that the new low ice cover attractor is predominantly a summer phenomenon. Hence it is appropriate to discuss the ice-albedo feedback as a possible contributor to separating attractors. We think Fig. 10 captures a conceptual idea but admit its limitations.

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

Response. We plan to reframe our idea as a "hypothesis" which is then tested with the various methods and data sets. The title and abstract will be adjusted accordingly, as will the general tenor of the text.

Minor comment. *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.*

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Response. Minor gaps in the data sets used were interpolated using an averaged seasonal cycle.

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