The Cryosphere Discuss., 6, C1566–C1572, 2012
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The Cryosphere Discussions

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6, C1566–C1572, 2012

Interactive Comment

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

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Received and published: 14 September 2012

We thank the Reviewer for their constructive criticism which should help us improve the manuscript. Our responses to the comments are listed below:

> **Recommendation.** *I find the publication in its current form unsuitable for publication in The Cryosphere, primarily because the paperÆs argumentation is not fully convincing, in particular since it lacks geophysical interpretation.*
>
> **Response.** Statistical models cannot tell us about geophysical mechanisms. We tried to bring in current physical understanding of mechanisms in the Conclusions section, suggesting where and how a new regional attractor for low summer ice cover could arise. Clearly this geophysical interpretation was too short and can

be expanded. However, we believe it is too much to ask of one paper to do both detailed time-series analysis and geophysical modelling to explain the signals, especially as existing models have largely failed to consider or capture the possibility that there could be a threshold to partial (rather than complete) summer ice loss.

Comment 1. *A number of recent studies have examined the possible existence of an irreversible bifurcation and have concluded on physical grounds, or through the application of complex Earth-System models, that such bifurcation is unlikely to occur. While these publications are cited in the introduction, it is not discussed in detail as to why their findings are different from the ones presented here. Such discussion is however very desirable, since the results presented here are "only" derived from a purely statistical analysis of a 1-dimensional time series, which is supposed to describe the dynamical behaviour of as complex a system as the Arctic sea-ice cover. As such, some argumentation would be helpful as to why these results should be more convincing than the physical (or complex modelling) analysis that previous studies have employed.*

Response. The reviewer misunderstands what we are claiming. We are not saying that the bifurcation is irreversible, we are arguing that recently there have been switches between attractors on a seasonal timescale, and (implicitly) the recent sampling of a low ice cover attractor in summers could be stopped if we could reverse the overall climate warming of the Arctic (although of course that will be difficult) – see Fig. 10 for this conceptual interpretation. Furthermore, recent model studies have focused on whether near-complete summer ice loss is irreversible, whereas we suggest we have detected a bifurcation involving a step increase in partial summer ice loss. The two views can be reconciled in that they are asking different questions – firstly on the matter of (ir)reversibility, secondly on the matter of the extent of ice loss. We are not trying to claim that our results are more convincing, we are simply saying that the previous modelling studies missed the possibility of a bifurcation involving regional-scale ice loss with regular

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switches between states on seasonal timescales (because they were asking a different question).

Comment 2. *The statistical analysis presented here doesnÆt seem very robust. For example, if the interpretation of ACF and variance as given by the authors is correct, Fig. 4 c and g indicate that the ice cover was stabilizing prior to the 2007 extreme minimum. Fig 3 b, however, is interpreted as showing that in 2007 a new state emerged. This to me simply says that conclusions based on such time-series analysis are inconclusive, and that apparently the complexity of the Arctic sea-ice cover is not captured by the indicators (and/or the time series) presented here. In the text, this contradiction is circumnavigated by arguing that the old state wasnÆt de-stabilized, but that a "new state" simply was added to it. In geophysical terms, and from Fig.1, one might argue that such "new state" simply is related to a substantial loss of summer sea ice, with less loss of winter sea ice. Hence, the main conclusion of this paper could be re-written as "Arctic sea-ice loss is faster in summer than in winter in recent years", which is not really a new result.*

Response. We are arguing that the new state involves a greater loss of summer sea-ice (see Fig. 10), but the key point of the argument is that the seasonal cycle jumped in amplitude in 2007 (and hasn't recovered since) because summer ice-cover started sampling a new attractor. We believe this is an original interpretation and a new result. As we explain in the text the indicators of critical slowing down via rising autocorrelation only pertain to the stability of the attractor state a system is in, not the existence or stability of other states. Hence the rather puzzling apparent increase in stability leading up to 2007 does not necessarily contradict the subsequent appearance of a new attractor.

Comment 3. *It remains unclear after reading this paper what the "new state" of the Arctic sea-ice cover as discussed here means. Obviously, the loss of multi-year ice, the extensive reduction of sea-ice volume, the increasing melting at the ice bottom, etc. are all indications of changes in Arctic sea ice, but these things are all known from published* *literature. As such, the very complex time-series analysis here could be interpreted as a rather complex method to show that the Arctic sea-ice cover is changing rapidly at the moment. But I donÆt see what really is "new" about this, unless the authors provide some more insight into what this "new state" means geophysically. I am convinced that time-series analysis can provide deep insights into the functioning of the EarthÆs climate system, but such analysis should be backed up by physical understanding of the system. This is the more the case given the conflicting results that are obtained from various indicators as presented here.*

Response. We can be clearer about what the "new state" means - it is an attractor for lower sea-ice cover in summer (although the influence of this attractor can apparently extend into other seasons). It involves the loss of around a million square kilometres more ice than the previously 'normal' state of ice cover through the summer. What is new is this dynamical systems interpretation of the recent behaviour of the sea-ice, including highlighting the jump in amplitude of the seasonal cycle in 2007 and since (which Fig. 1 shows doesn't require "complex" time-series analysis), and associating this jump with the existence and sampling of a new attractor. We do not claim to have a full physical understanding of what is going on, but we can expand our discussion of the mechanisms that could contribute to an attractor for lower summer ice cover.

Comment 4. *The robustness of the results presented here hinges crucially on a robust removal of the seasonal cycle. This is for example in particular the case for the DFA indicator, which samples a period of 10 to 100 days - hence the primary signal of the DFA indicator will stem from slight shifts in the seasonal cycle.*

Response. It is true that the deseasonalising of the data can crucially influence the results. We propose to illustrate this with a model time series in the revision.

As Peter Ditlevsen's comment shows, what is considered the normal seasonal cycle affects what appears as a shift to bistable behaviour. Underneath it all however there appears to be a fundamental jump in the amplitude of the seasonal cycle around 2007. We interpret this in terms of the preceding behaviour being the norm and a new attractor starting to be sampled. We welcome alternative dynamical systems interpretations, but none of the comments offer one.

Comment 5. *The Center Manifold Theorem, close to a bifurcation point, allows for the reduction of even high-order dynamical systems reduces to a low-order system. This argumentation is, however, circular in that it must first be assumed that a bifurcation point is being approached, which then allows one to treat the high-order system as low-order, which then allows one to detect the bifurcation. It seems well possible that the system isnÆt approaching a bifurcation, hence canÆt be treated as low-order, hence one doesnÆt learn much by deriving a bifurcation for a possible low-order system. Additionally, it is not clear how the authors can be sure that the thus constructed 1-D time series can be described by a linear AR(1) model with, by definition, Gaussian noise.*

Response. Our approach is to start with the simplest model to interpret a data set. We do not claim it is the definitive model. Note however that we do have some past success taking the same simple approach to 1-D time-series of complex systems. In Climate of the Past 6(1): 77-82 (2010), we used 1-D potential analysis to detect a bifurcation in Greenland ice-core records. Recently, using a higher-dimensional approach, the same bifurcation has been detected by Cimatoribus et al.:

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

This suggests that despite its limitations, the 1-D methodology can be useful in studying higher-dimensional systems.

If it is "well possible" there is not a bifurcation and a new attractor being sampled then what is the explanation for the sudden and persistent jump in the amplitude of the seasonal cycle, associated with recent summers? In revising the paper we can reframe our explanation as a hypothesis, allowing that there may be other

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explanations for the behaviour, and inviting further work to come up with such explanations.

Comment 6. *Some more discussion as to the use of a daily time series would be necessary. Such focus on daily values on the one hand increases the length of the time series, on the other hand, given the low degree of freedom at such short time intervals, there will be significant short-term auto correlation of that time series. I was wondering how this was taken into account in this analysis - and if indeed it is possible to derive additional information on the dynamical behaviour of the system by focussing on such comparably fast time sampling.*

Response. We spent quite a bit of time aggregating the data to different time resolutions, for example to check consistency between the ACF and DFA indicators. However, we felt the paper was already overloaded with figures, so did not include any on this. Unfortunately, annual averaging of the time series (e.g. to compare with the recently published historical reconstruction) cannot provide sufficient statistics as it would contain only 34 data points.

Comment 7. *There is very little discussion as to the implications of the short length of the time series for the conclusions found here. A 30-year long time series of as complex a system as Arctic sea ice with decadal fluctuations, trends, a strong seasonal-cycle all contribute to very complex dynamics that canÆt fully be captured within such short time series. Given these limitations, how robust are the results found here?*

Response. We can add more discussion on this, but obviously we can only work with what data we have. We made an attempt in the paper to include longer timescales by analysing the annual reconstructed ice extend since 1870. Unfortunately the annual resolution of this dataset doesn't allow us to address the hypothesis of bifurcation and seasonal switching arising from the high resolution data, nor does it allow proper inter-comparison between them. Regarding trends, the quadratic downward trend in the 30-year dataset is clear, and looking at the historical reconstruction it started somewhat earlier around 1970, but it is well removed by our detrending, and we don't see any clear residual decadal fluctuations.

Comment 8. *p.2624, l.5: Estimates of sea-ice area don't necessarily use the 15% cut-off. Some do, some others don't.*

Response. We will correct this in the revision.

Comment 9. *I am not aware of a "standard convention" for removing the seasonal cycle of sea ice. Where does this come from?*

Response. We can rephrase this, but we saw the approach used in the Cryosphere Today project and by other groups/publications. The up-to-date sea-ice area data is deseasonalised with base 1979-2008, i.e. the first 30 years of data. Given our analysis it might make sense to shorten this averaging interval slightly to stop before 2007. We actually experimented with different averaging intervals but just picked what we saw being publicly used and widely reproduced. In the revisions we will reconsider this and its affect on the results.

Overall, we plan to rephrase the claim of a bifurcation in 2007 as a hypothesis, and examine whether this hypothesis is supported by our analysis.

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