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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

Anonymous Referee #2

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In this publication, the authors use statistical methods to examine the possible emergence of a "new state" of the Arctic sea-ice cover from time-series analysis.

Recommendation In principle, an analysis as provided by this paper is an interesting addition to the ongoing debate regarding a possible bifurcation behaviour of Arctic sea ice. Nevertheless, I find the publication in its current form unsuitable for publication in *The Cryosphere*, primarily because the paper's argumentation is not fully convincing, in particular since it lacks geophysical interpretation. The paper might become suitable for publication once it has undergone a major revision

General comments The following comments should be addressed by the authors in a revised version.

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.

2. The statistical analysis presented here doesn't 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 wasn't 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.

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

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a rather complex method to show that the Arctic sea-ice cover is changing rapidly at the moment. But I don't 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 Earth's 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.

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.

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 isn't approaching a bifurcation, hence can't be treated as low-order, hence one doesn't 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.

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.

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 can't fully be captured within such short time series. Given these limitations, how robust are the results found here?

Minor comments

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

p.2624, l.13: I am not aware of a "standard convention" for removing the seasonal cycle of sea ice. Where does this come from?

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

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Interactive Comment