Review of "Feature-based comparison of sea-ice deformation in lead-resolving sea-ice simulations" by Nils Hutter and Martin Losch

General comments

This manuscript presents a set of analyses for feature-based comparison of sea ice deformation. The authors address a useful evaluation, which has been required for some time. The comparison includes a) detecting Linear Kinematic Features (LKF) and b) measuring some of their geometrical characteristics. The sea ice deformation of high-resolution Arctic simulations and an LKF data-set derived from the RARDARSAT Geophysical Processor System (RGPS) are used. The manuscript contains information about the lifetimes and growth rates of LKFs detected from RGPS. The authors suggest the feature-based comparison as an effective substitution for the scaling analysis.

Although the feature-based comparison is well suited for the journal, the used algorithms, and methodology need to be further clarified before the manuscript paper can be accepted for publication. In addition, it is necessary that the objectives of the research be clearly described. Furthermore, the text is wordy and the writing misses conciseness. Authors should clearly describe the originality of their methods in relation to the published ones and avoid reporting style. I outlined some major points, which I would like the authors to consider. I would strongly encourage them to conduct the suggested analyses. It would be constructive if the literature review could contain all relevant researches and projects.

Major Comments

1- There is ambiguity in recognizing the novelty of this study. In addition, the manuscript does not explain explicitly the points of providing such comparison. Thus, I inferred that either the authors aim to introduce a new framework for evaluating the numerical results or try to assess the performance of visco-plastic rheology used in a very high-resolution experiment. I review the manuscript for both aspects as follows. The authors are highly encouraged to consider them for the revision.

   a) If the manuscript aims to introduce a new feature-based comparison in sea ice dynamics:
      I- The idea of comparing LKF detected from RGPS and numerical fields and all introduced algorithms in this manuscript have been already published (e.g. Levy et al. 2008, Wang et al. 2016, Hutter et al. 2019, Linow and Dierking 2017, Hutter et al. 2018).
      II- Additional analysis is required to show that the skeletons of LKF could represent the spatial characteristics of LKF.
      III- Although a significant part of the paper is devoted to explaining detecting LKF as an object-based approach, the authors avoid using an object-based comparison. The argument stating that a direct comparison should be avoided due to chaotic dynamics of sea ice is not sufficiently convincing. Is the evaluation of the detecting algorithm (section 3.2, Hutter et al. 2019) not an object-based comparison? Object-based verification of precipitation (e.g. Wernli et al. 2008) is an example that is

applied for chaotic fields. Authors are encouraged to benefit from the advantage of the introduced object-based detecting algorithm.

IV- Again, due to the lack of specific question for the comparison, I assume here that the main goal of developing a comparison framework is assessing the performance of the sea ice models using visco-plastic rheology in a very high-resolution configuration. For such simulations, distribution of LKF (leads in this case, which are controlled by tuning the ice strength parameterisations) is important. However, an analysis, which measures the spatial distribution of LKF, is missing. In addition, it is not clear why comparing the intersection angles and computing scaling characteristics are not sufficient. It is useful to know which physical processes or performance of which numerical schemes are linked to the number of LKF.

V- The authors state that computing scaling characteristics is an old approach and is not appropriate for evaluating the simulated deformation features (line 20 of the second page). Nevertheless, a significant part of the paper is devoted to explaining the scaling analyses and their results. The does not establish useful links between scaling characteristics, and spatial characteristics of LKF. Furthermore, the results of scaling analyses in this paper do not provide new insights into the sea ice dynamics. I suggest removing all these sections unless the authors could emphasize on the positive contribution of the scaling analyses. In this case, I encourage the authors to apply spectral analysis that might be more appropriate for computing the scaling properties of sea-ice deformation (Hutching et al. 2011).

b) If the purpose of the evaluation is an assessment of a specific configuration of the sea ice model.

1- The horizontal resolution of the coupled sea ice-ocean model is pushed to high resolution (~2 km) to resolve much more LKF. However, to reduce the computational costs, the oceanic component of the model has only 16 vertical layers. The authors argued that such configuration is rational since the main purpose of the study is focusing on sea ice processes. In contrast, other configurations of coupled ocean-sea ice models use much more vertical layers to resolve the halocline circulation in the Arctic. For example, Spall (2013) used 30 layers with 50 m thickness on the upper 500 meters and the configuration of Mu et al. 2018 has 50 vertical layers. In addition to resolving the halocline circulation, it is well understood that the number of vertical layers might affect vertical mixing. Liang and Losch (2018) show that vertical mixing affects the vertical heat and salinity exchange. Consequently, they influence directly the sea ice states such as concentration and thickness. Thus, the formation, density, number and all spatial characteristics of LKF might be affected. Thus, the authors should conduct the following analyses.

i- Discuss whether the current configuration could resolve the corresponding oceanic circulations or not. What are their driving force and their temporal and spatial scale? What type of mixing parameterization is used?

ii- Compare the main characteristics of the sea ice in the current configuration with the sea ice state of a configuration with comparable horizontal resolution and more vertical layers and/or with any available product, e.g. EUMETSAT OSI SAF for ice concentration. To perform a reasonable comparison, In my opinion, the authors should provide the contours of ice thickness and sea ice strength for sea ice concentration more than 50 % and 85 % for all 12 months of the year.

2- According to the first comment, the title of the manuscript is very general.

3- Two different sea-ice simulations are performed. The manuscript does not explain the scientific reason for designing these two simulations. Thus, it is difficult to evaluate the selected comparison methods.

4- It is argued (Section 4.2.2) that the shape of LKF is scale invariance. This statement is rather subjective. Hutter et al. (2019) showed the LKF detection algorithm terminates detecting LKF when there is a directional change compared to the orientation of the last 5 pixels. Further, they introduced a new starting point. In addition, closed contours are first divided into several segments. The probability that the reconnecting algorithm combines such segmented features is thus questionable. It means that the algorithm might not be able to detect linear features with high curvature. Overall, I speculate the introduced "number and length of LKF" are not truly spatial features of the LKF and are more and less subjective quantities.

5- The enhanced horizontal resolution does not necessarily increase the prediction skill (e.g. Mass 2002). A fair analysis discussing position error, double penalty, etc is missing. The analysis should show that the 2 km is a rational horizontal resolution. When the horizontal resolution increases, the objective verification scores might be degraded, although more useful information on a smaller scale is generated. Are deformation of both simulations interpolated into a similar 12.5 km grid? If so why is high-resolution simulation necessary for explaining a new comparison approach?

6- It is very practical if the authors could tell that how many operators did repeat the seven visual detections of the LKF within one single RGPS image (Linow and Dierking, 2017). To better understand the optimization in detecting LKF explained by Hutter et al. (2019), the evaluation of section (3.2) is highly recommended to be revisited. Try to compare again the uncertainty of the LKF detecting algorithms using a quantification mechanism so that they were comparable with the intrinsic accuracy of the hand-picked lines (Linow and Dierking, 2017).

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