Answers to the reviewers #1

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.

We thank the anonymous referee #2 for his review. Please find our detailed answers below.

We would like to point out that we do not recommend to replace the scaling analysis, rather than complement it.

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.

   The manuscript describes the application of a recently introduced method (Hutter et al., 2019) to a comparison between satellite-based remote sensing data and two numerical model simulations. In doing so, we introduce a new evaluation frame-work and use the two simulations as examples. Thereby, we indirectly also assess the performance of these simulations. We do not see how these two aspects could be split and, therefore, clarified the objective of our study:

   The objective of this paper is to establish a feature-based evaluation of sea-ice deformation in lead-resolving sea ice simu- lations. We apply the LKF detection and tracking algorithm of Hutter et al. (2019) to two different sea-ice simulations with a horizontal grid-spacing of 2 km, of which one uses an Ice Thickness Distribution (ITD). We compare the extracted LKFs to an LKF data-set derived from RADARSAT Geophysical Processor System (RGPS) deformation data (Hutter et al., 2019) with respect to their Pan-Arctic distribution (density and orientation), their spatial properties (length, curvature, and intersection angle), and their temporal characteristics (persistence and growth rates). **In addition, we test which conclusions about the properties of LKFs can be drawn from a spatio-temporal scaling analysis of sea ice deformation (following, e.g.**

Rampal et al., 2016; Hutter et al., 2018). By analyzing two different model simulations, we study how changes to the model physics, in our case the explicit ridging processes in an ITD model, affect the simulated LKFs, and how the different analysis methods pick up that difference. **With our analysis we test whether the ice strength parameterization of the ITD model, which mainly depends on the thinner ice classes, accelerates lead formation by a faster feedback between deformation, ice thickness, and ice strength as suggested in Hutter et al. (2018).**

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).

    Other studies have addressed the question how to compare LKFs in observations and simulations: Coon et al. (2007), Levy et al. (2008), and Mohammadi-Aragh (2018) presented metrics that provide one score that quantifies the agreement of the entire field of LKFs. While such comparisons can be added to a cost function in model tuning easily or applied to assess forecast skill, these metrics do not provide insights into the characteristics of LKFs fields, like spatial distribution and persistence, that are important to model the interaction of atmosphere, ocean, and ice along LKFs. Wang et al. (2016) compared lead densities in a model to satellite observations, but no further characteristics, especially no temporal, are deduced.

    We apply the recently introduced method (Hutter et al., 2019, extension of Linow & Dierking, 2017) to detect LKFs in two model simulation and in RGPS. With this analysis, we can explore spatial and temporal characteristics in detail. To our knowledge, such an extensive analysis has not been done before.

    We added the following sentence to make reference to these earlier studies:

    Various metrics for evaluating LKFs as discontinuities in the deformation fields have been suggested, but they all provide only a summary of agreement with a reference in a single score for the entire LKF field (Coon et al., 2007; Levy et al., 2008; Mohammadi-Aragh et al., 2018).

    ii. Additional analysis is required to show that the skeletons of LKF could represent the spatial characteristics of LKF.

    Kwok (2001) first defined LKFs: "*Quasi-linear features of the scale of kilometers to hundreds of kilometers can be observed in the high-resolution deformation fields of the sea ice cover ... They appear as sharp discontinuities separating regions of uniform ice motion. … Here, we refer to them as linear kinematic features (LKFs).*" This definition states that LKFs are quasi-linear and their length is considerably larger than their width. This makes an abstraction of these features to skeletons possible to describe their overall shape such as length, curvature, orientation, etc. (Banfield, 1992; Van Dyne and Tsatsoulis, 1993; Van Dyne et al., 1998, Linow & Dierking, 2017, Hutter et al., 2019). Only the information about the width of the LKFs is lost by using the skeletons. However, the Lagrangian deformation data with a resolution of 12.5km used in our study does not allow to reliably retrieve the width of an LKF anyways. Thus, no information is lost by representing LKFs by their skeletons.

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.

We avoid a direct comparison of deformation features, because a perfect initialisation of the model would be needed for such a comparison due to chaotic dynamics of sea ice. In numerical weather forecasting, sophisticated data assimilation methods are applied to obtain an optimal model initialisation, which makes object-based comparison of short-term precipitation patterns possible. In case of sea ice simulations, knowledge about initial weaknesses in the ice are needed to predict short-term sea-ice deformation and evaluate such predictions with a object-bases one-by-one comparison. In our study, we focus on long-term sea ice simulations without data assimilation and therefore are not able to compare deformation features observed from satellite and modeles one-by-one, but study long-term statistics of both fields.

Note, that in the mentioned object-based comparison in the method evaluation (Section 3.2, Hutter et al., 2019) different algorithms are applied to the same input data, which makes this comparison possible. However, this is completely different from comparing satellite observations to model output.

We changed the text to make the importance of model initialisation more clear:
The emergence of deformation features, which can be identified as leads and pressure ridges, calls for a proper evaluation of model simulations against observations. This is challenging because ice mechanics are non-linear and chaotic. A direct comparison of deformation fields bears similar issues as comparing eddy resolving ocean model simulations to high-resolution satellite observations (Mourre et al., 2018). Therefore, it should not be attempted if accurate initial conditions (e.g. obtained by data assimilation) are not available. Still, proper LKF characteristics in sea ice models are important in the context of Arctic climate.

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.

The comparison framework presented in our study is not limited to sea ice models using the VP rheology, but can be applied to any simulation that resolve LKFs as localized deformation rates. To present this method, we use two very high resolution VP simulations.

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.

The reviewer argues in the previous statement that the distribution of LKFs is important. We agree, and we show the spatial distribution of LKFs in Section 4.1.2 “LKF density” and discuss the temporal distribution of LKFs in Section 4.1.1 “Number of LKFs”. Scaling characteristics and intersection angles of LKF provide useful insight into the deformation physics that can be used to modify the rheology. In our study, however, we present two simulations that show similar scaling characteristics and intersection angles, but different number of LKFs and different LKF lifetimes. These differences will have an impact once such sea ice models are used in coupled climate modeling. The point of the paper is that we should not restrict ourselves to very few diagnostics.

It is useful to know which physical processes or performance of which numerical schemes are linked to the number of LKF.

In the study, we show that applying an ITD model increases the number of simulated LKFs (see Sections 4.1.1, 4.1.2. and 5). In Section 5, we discuss that the implementation of a damage parameter will also have a positive effect on the number of simulated LKFs.

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).

The phrase “for evaluating the simulated deformation features themselves” is misleading and we removed it. We never meant to say that the scaling analysis is "old" and "not appropriate" (and we don't use these exact words anywhere). We mean "individual features" and rephrase the sentence to

"While scaling characteristics give some insight into the underlying material properties of sea ice, their interpretation with respect to individual deformation features is not straightforward (Bouchat and Tremblay, 2017; Hutter et al., 2018). ... A comprehensive description of individual deformation features requires their detection to extract statistics such as density, orientation, intersection angle, and persistence."

The scaling analysis is interesting here, because it does provide useful information about LKFs, but it also provides a direct comparison to previous work (e.g. Girard et al., 2009; Rampal et al. 2016; Rampal et al., 2019). We cannot use it here to differentiate between the two simulations, because both simulations reproduced the observed scaling of RGPS data. This is important as failure to do so in previous coarse resolution VP models has been used to argue against their use (e.g. Girard et al 2009). The higher moment analysis is the first successful match of a VP-model to RGPS, to our knowledge.

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

    More vertical ocean layers may affect the ice thickness, but the main driver of sea ice thermodynamics is the atmospheric forcing. It is beyond the scope of this manuscript to demonstrate that. In Figure 1 (of this response), we compare the performance of both simulations with respect to sea ice volume and extent. In this comparison period between November to April, both simulations show reasonable sea-ice volume and extent, such that we evaluate the misfit in spring and fall as non-essential to our analysis (For a full discussion see Answers to reviewer #2). In addition, the LKF densities presented for RGPS in Figure 5a (of the manuscript) do not vary strongly between the thin first-year ice and thick multi-year ice. Thus, we assess that the misfit of sea ice volume presented in

Figure 1, does not impact significantly the numbers and distributions of simulated LKFs.

We agree with the reviewer that the vertical resolution is a very important aspect of an ocean model. Especially the circulation of the Atlantic Water depends strongly on vertical mixing and vertical grid spacing (to the extent that it affects numerical mixing in the vertical, e.g. Spall 2013). But we do not (and will never) use the present model configuration with only 16 vertical layers for studies of the Arctic Ocean interior. For sea ice dynamics and thermodynamics, the ocean surface is important (apart from the much more important atmospheric forcing fields, which to 1st order determine the ice extent), because it exerts stress (dynamical forcing) and because it imposes a heat flux (thermodynamic forcing). Our analysis is restricted to the ice covered part (mostly western Arctic in the winter months), where RGPS data are available. In this region and at this time, the impact of vertical mixing on the ice extent and surface temperature is practically independent of changing vertical mixing coefficient over 2 orders of magnitude (at least in the simulations of Liang and Losch 2018, where the grid spacing is 18km). The ice thickness does decrease with more vertical mixing in Liang and Losch (2018), but not by more that 50cm, except for a small area near the Canadian Arctic Archipelago. This is well within the general uncertainty of sea ice thickness estimates, and especially sea ice model biases.

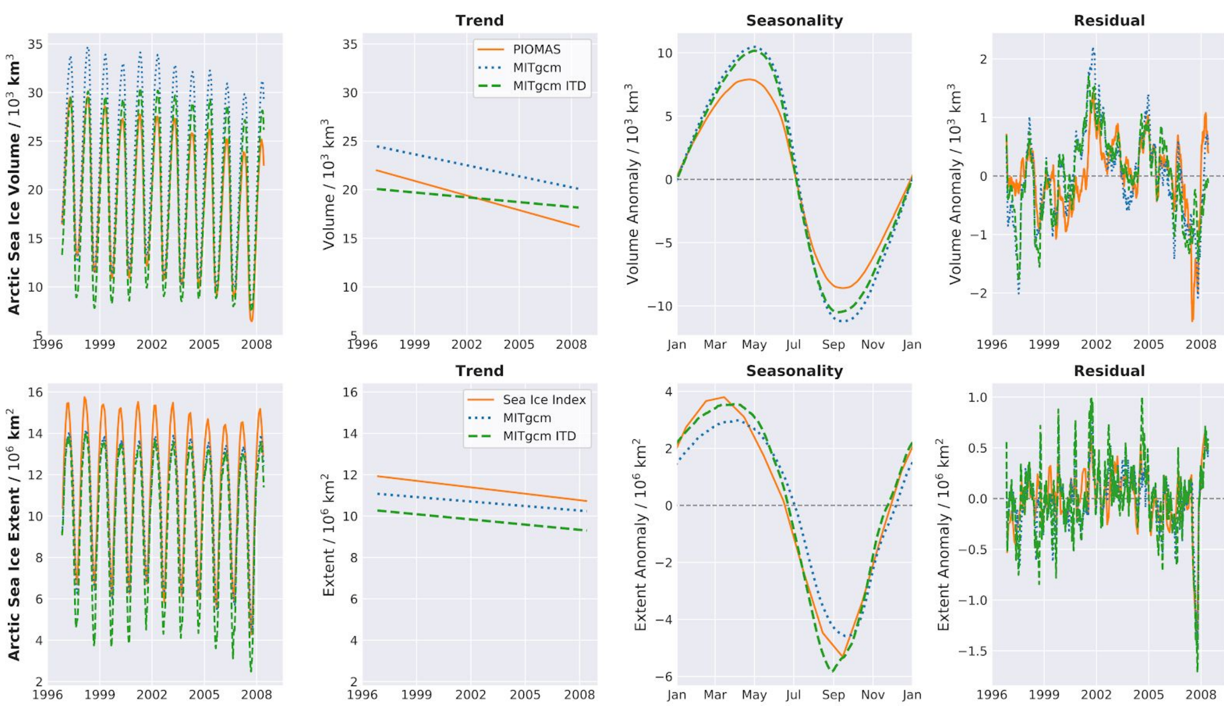

Figure 1: (top row) Comparison of Arctic sea ice volume in both model simulations used in our study to the PIOMAS model given as a time series over the entire RGPS period (1996 to 2008) and separated into

a linear trend, seasonality and residual. **(lower row)** Same as upper row but for the Arctic sea ice extent from NSIDC. For a full description of the plots see Ungermann & Losch (2018).

We compared our horizontal surface fields and vertical sections of temperature and salinity through the Central Arctic to previous simulations with 50 layers (as in Mu et al 2018), but a 4km horizontal grid spacing and find that grossly the vertical structure is similar between the models, but indeed the vertical gradients of temperature and salinity in the 16 layer model are not as strong as in the 50 layer model, as expected (Fig. 2 and 3 of this reply). The surface fields, however, are not significantly different (Fig. 4 of this reply). It was important to us that the surface ocean reproduces some of the observed small scale eddy activity as an additional forcing for the sea ice dynamics, and indeed the fronts and eddies in the 2km surface fields are better resolved than in coarser simulations, see attached figures:

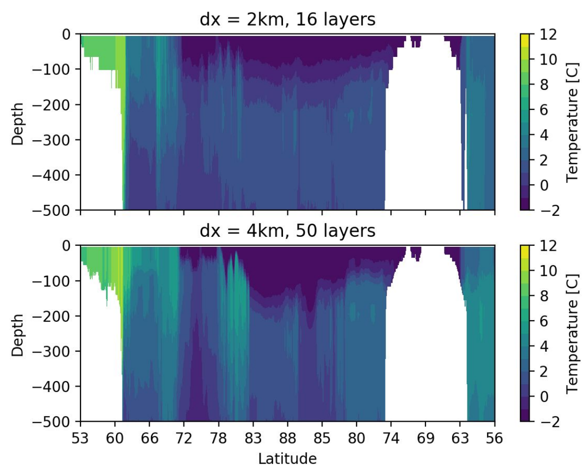

Figure 2: Temperature transect of the Arctic ocean along prime meridian **(upper)** in the 2km configuration with 16 vertical layers used in our study and **(lower)** in a 4km configuration with 50 vertical layers (Mu et al., 2018).

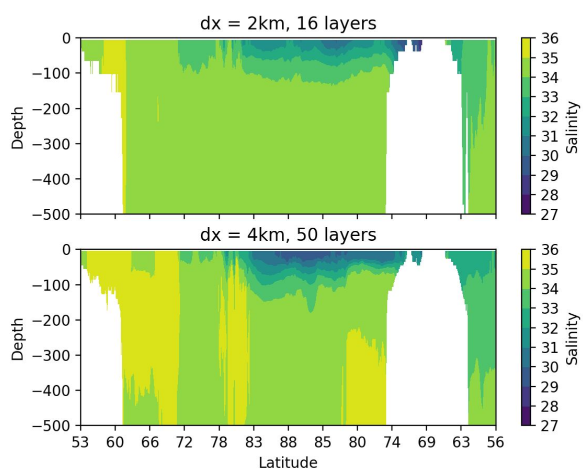

Figure 3: Same as Fig. 1 but showing salinity

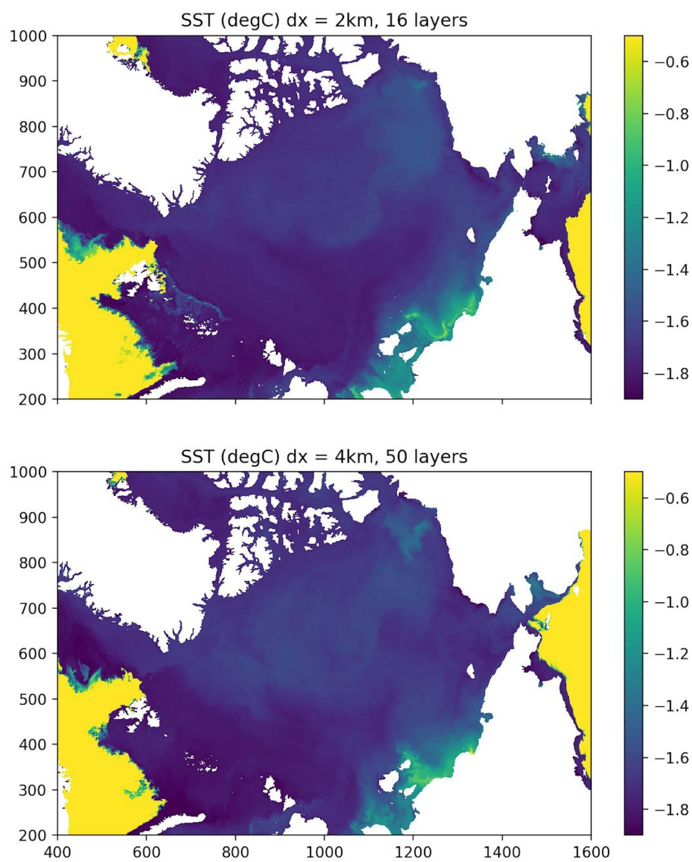

Figure 4: Sea surface temperature in the Arctic Ocean **(upper)** in the 2km configuration with 16 vertical layers used in our study and **(lower)** in a 4km configuration with 50 vertical layers (Mu et al., 2018).

Please note that there are also realistic sea ice simulations that are coupled to a simple “slab-ocean” with coarse and non-reactive ocean circulation (e.g. Rampal 2016)

1. 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?

   The model configuration in Section 2.3.1 includes grid resolution, bathymetry, lateral boundary conditions, initial conditions, surface forcing (even with time resolution and spatial resolution), and a reference to Nguyen et al (2011) for the configuration of the ocean model (mixing, etc.) Nguyen et al. (2011) focussed on the ocean circulation. We use the same ocean parameters, i.e. the vertical mixing scheme is KPP (Large et al 1994) and a modified Leith (1996) scheme in the horizontal. The ocean circulation will not be the same as in Nguyen et al 2011, because our horizontal resolution is higher, our vertical resolution is lower, our forcing is different, and even our initial conditions are different. However, the ocean is only interesting to the extent to which it drives the ice (mostly dynamics), i.e. the short scale eddy driving stress forcing (see also previous answer). Discussing details of the ocean circulation is well beyond the scope of this paper (assessing the statistics of LKFs in two different sea ice formulations in comparison to RGPS). We don't even find it appropriate in this journal. We added the following statement Section 2.3.1:

   The resulting ocean circulation has not been evaluated in detail, but the wind-driven surface circulation is plausible with strong mesoscale activity, the surface temperature and large scale sea ice distribution follow the prescribed surface forcing as expected. The main role of the ocean model is to provide dynamic bottom boundary conditions to the sea ice model.

2. 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 our knowledge, there are no sea-ice ocean simulations at this very high horizontal resolution publicly available that have more vertical layers and a similar simulation period. Therefore, we use satellite products and reanalysis to assess the performance of the two simulations (Figure 1 of this answer).

   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.

This would mean 96 plots, for which there is no space in the manuscript. Therefore, we prefer to compare Arctic wide properties in the more compact way above.

It’s not clear, what we should compare ice thickness and ice strength to, because we do have either observational data available for this comparison, nor numerical model simulations at similar resolution.

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

The title of our manuscript is “Feature-based comparison of sea-ice deformation in lead-resolving sea-ice simulations”. We do not agree that it is too general: In our study we compare the spatial and temporal characteristics of deformation features present as LKFs in deformation fields. This comparison is based on the detection of single LKFs, which is summarized in the title as “feature-based comparison of sea-ice deformation”. We compare deformation features extracted from satellite observations (RGPS) and two sea ice simulations that resolve leads by using a very high grid spacing and the VP rheology. These simulations are referred to as “lead-resolving sea-ice simulations”.

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.

The comment is not clear to us. We use two simulations with different levels of sophistication that are reflected mainly in the ice strength parameterisation. We show, how they compare to remote sensing data and draw some conclusions about the dynamics. To be more clear, we added a sentence to the objectives of the paper to highlight why we chose to change the ITD and the ice strength parameterisation, which is also further discussed in Section 5.:
With our analysis we test whether the ice strength parameterization of the ITD model, which mainly depends on the thinner ice classes, accelerates lead formation by a faster feedback between deformation, ice thickness, and ice strength as suggested in Hutter et al. (2018).

4. It is argued (Section 4.2.2) that the shape of LKF is scale invariant. 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.

The reconnection instance in the LKF detection algorithms prefers to reconnect segments that show little difference in the orientation in accordance to the definition of LKFs given above. Nevertheless, it will reconnect segments up to an upper limit of 35° for the difference in the orientation, if no better matching segments are present. By consecutively reconnecting small segments, LKFs with higher curvature can be detected (we note that this will only happen for larger features that span multiple pixels). Linow & Dierking (2017) studied the curvature of LKFs from hand-picked data and found low curvatures with close to linear LKFs. This shows that are no high curvature LKFs that the detection algorithm could miss.

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.

We do not understand what this statement is based on, as it is not linked to the curvature discussed above. We require more information to give a better answer.

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?

   Numerical weather prediction as in Mass et al, is a different subject: the dynamical system is completely different (based on Navier-Stokes/Primitive Equations and probably data assimilation); we do not evaluate predictive skill, but dynamical properties of the model. We do not claim, that 2km increases any skill. It just leads to more LKFs. Lower resolutions do not exhibit these features and hence a feature based analysis method could not be applied.

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).

   All of these comments relate to already published papers with a completed peer review. Repeating previous work is beyond the scope of the manuscript.

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