Comment on egusphere-2022-519
Marcello Vichi (Referee)

Referee comment on "A quasi-objective single buoy approach for Lagrangian coherent structures and sea ice fracture events" by Nikolas O. Aksamit et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-519-RC3, 2022

This manuscript proposes a diagnostic to measure stretching and compression in sea ice based on Lagrangian trajectories by individual buoys. This is demonstrated by the application to three datasets. There are no doubts that this contribution deserves publication, especially because the authors have demonstrated some limitations of some of the alternative methodologies. I have not found any major issue in the theory, and the results are convincing. I praise the authors for having ported this methodology to sea ice drift, and for clarifying some issues with the existing estimators of sea-ice state from buoys.

However, prior to publication, I would suggest a few changes that may strengthen the applicability of this methodology to sea ice, as well as generalise some of the results. Addressing these points should not involve major work, since they are mostly related to structure and additional explanations.

General comments

- It is not completely clear to me how fractures should be captured by this diagnostic, since we do not deal with a classical stretching-compression feature in a continuum. Fractures are highlighted as identifiable features at the beginning of the results (L168, L215 and in other sentences). Shear is not necessarily assimilated to a fracture. The authors should clarify this concept from the introduction, especially because one of the examples is fully dedicated to fracture identification.
- This manuscript would benefit from a more unified description of the examples, to remove any inference of cherry-picking (L147-165). This is all done in the method
section, but the rationale of the choice is not discussed. There is a major focus on the role of storms in setting ice conditions, but the choice of the examples is more varied, especially with the inclusion of fractures. This diversity is appreciable but may be confusing, since there is an expectation that this diagnostic application is to identify the presence of synoptic events. This seems to be the case in the first two examples, but then it is not summarised in the discussion/conclusion. The authors may consider to better frame the applicability of the method with a more general introduction.

- The authors have made available the code that should putatively reproduce the results presented in this manuscript. However, this is not attainable. The code is the same referenced by Haller et al. (2021), which was meant to compute TSE for ocean applications. It cannot be used to compute the TSE for sea-ice buoys. In collaboration with a PhD student in the group, we implemented the numerical computation of TSE for individual buoys from eq. 4-5, and we found some ambiguities in the choice of the discretization stencil that would affect the results. This is normal with numerical discretizations, but as it stands, a reader would not be able to implement the method and obtain the same results.

- This method is alternative and superior to the use of buoy clusters and polygons. This is clearly demonstrated in the results and appendices, but not explained in the introduction. The Method section is somewhat explained the other way around, with the existing methods described at the end, but invoked earlier. I honestly struggled to follow it, and I would recommend some restructuring, especially for readers who are not fully aware of the underlying mathematical concepts.

- Following up from the previous point, my main question is how different this method is from the single particle dispersion applied in Rampal et al. (2009) and other literature referenced in the manuscript. I am not sure this is addressed in the manuscript. The authors state at L98 that there are limited Lagrangian alternatives to compare to, but this comparison is not presented.

- The introduction to the Result section at L176-L182 is quite problematic and needs a thorough revision. These paragraphs are more akin to the Method section. The choice of the frequency of analysis is based on the synoptic scale, but then the same method is applied to the last example involving fractures, which may be related to internal ice stresses rather than storms. There is no sensitivity analysis on how TSE is affected by the choice of the sampling window, as well as the granularity of the source data. For instance, the authors say they have linearly interpolated to hourly frequency, but there is no justification for this choice. This is especially important when using the IABP data that have highly varying frequencies.

I have some more specific points related to this section that should be addressed in the revised method section:

- Not clear how the 3-day window would “balance the high temporal resolution of TSE” (artificial, since it is linearly interpolated to 1 hr), “while dampening influence of measurement noise and sub-daily oscillations”. Maybe it’s just the English, but I do not understand what the authors mean. There are known sub-daily oscillations and they will be captured in the Lagrangian estimation of velocity (Gimbel et al., 2012).

- Noise is mentioned several times in these paragraphs but never quantified (also in results, e.g. L207). What do the authors mean by noise? Inertial oscillations are not noise, they are signal.

- I do not understand why TSE should always precede significant storm events, and why this should depend on the choice of the 3-day window. Indeed, in Fig. 1 there are few cases in which TSE This is maybe where having the code or showing the discretization...
of the TSE computation would help. My understanding is that the TSE is computed over a rolling window, so, as long as the window is not larger than the scale of a storm (up to 3-5 days), it would detect the feature. This argument is used throughout the presentation of the results (e.g. L196) but not made fully explicit. Also, it is not clear how a storm is defined and shown in Fig. 1 (when the core of the cyclone is the closest to the buoy location? E.g. Vichi et al., 2019, for an example from Antarctic sea ice, or maybe when the MSLP is lower than a certain threshold). My question is whether the authors think the stretching-compression is enhanced when the storms approach the buoy location. And, maybe, after the passage (as reported by Itkin et al., 2017), due to wave-induced breaking, sea ice goes into free-drif state which indicates weaker LCS. The authors should make an effort to interpret this important feature.

- The authors state they have made a sensitivity analysis on this (L178-179) but this is not presented in the results

- Finally, but this is just a minor point, I would advise the authors to briefly discuss the possible application of this method in Antarctic sea ice, and maybe give a recommendation on what would be the best approach.

Specific comments

L35-37 This sentence requires some references. These references come later in the manuscript, but I think a brief introduction on the Lagrangian coherent structures would be of aid to the sea-ice experts that are less familiar with them. LCS are more common in ocean dynamics and less in the sea ice.

L42-49 This paragraph relies on the previously published papers by Haller et al. I acknowledge the value of those papers to provide the mathematical background for this application. They may not be entirely approachable by a variety of scientists interested in applying this diagnostic further, possibly not noticing the limits of applicability. The authors make the implicit assumption that they are directly applicable to sea ice. I am aware that this is partly addressed later and in Appendix A1 (see my comments below), but I would suggest an earlier introduction to the concept.
L57 and L77: some ambiguities in the use of the notation. Is the trajectory symbol in italic and bold? Than it should be consistent throughout the manuscript

L69-72 This is a major assumption, and since it has been verified in the realm of ocean currents in the cited paper, it is likely to be acceptable with sea ice drift. However, current detection from space is more accurate and less prone to the resolution issue of sea-ice drift retrieval. I would recommend the authors to bring back this issue of the . I also have a few issues with the “verification” in A1, which, given the many uncertainties, I would rather call this process “assessment of the main assumption”. The choice of the period will define the maximum speed of the Lagrangian velocity. There is no explanation in A1 on how the Lagrangian velocity has been computed, nor on the period used for this analysis. In the caption of Fig. A1 it only says “50 days of sea ice trajectories in 2017”. This distribution will certainly change with different years, regions, etc. None of this is included in the presented analysis.

L78-79 I would suggest the authors to reiterate the concept of quasi-objective diagnostics at this point of the manuscript

L89 Eq 7-9 have not been introduced yet, as well as the concept that this method is alternative to the use of buoy clusters and polygons. I would suggest the authors make clear from the beginning of the Method section that there are existing alternatives and this is complementary to them.

L97 Maybe “nature” is missing in this sentence

Eq. 6 These equations are presented in discretized notation, but this is not done for the
TSE. I understand that this method is more classical and it is somewhat obvious, but it would still require some definition of what u and v are. I noticed it when discussing with MSc and Phd students that struggled to understand the notation.

L124-125 Any method with more than one buoy, or with buoys covering a larger regional expanse where constellations are less reliable, will be affected by the signal-to-noise ratio. I understand this may be more of relevance to the Southern Hemisphere.

Fig. 1 Please explain what the dashed line in panels a and c represents. I would also recommend to use the same range in the Y axis of panels b and d, to better compare late winter with spring.

L212 Please report the frequency of the MOSAiC buoys and if the same

L225 January 14 is not very visible in the figure with the current choice of ticks. Also, the authors state that it is more extreme in TSE. More extreme than what?

L236 this reference to Copernicus data has a non-existent DOI

L220-239 The April example has been chosen because of SAR images available before and
after the event. Maybe this could be mentioned at the beginning of the paragraph. The interpretation of the coloured points is not given in this section, and the reader is left with a sense of incompleteness. Also, there are no letters in the panels of Fig 2 and the colormap choice does not clearly show negative and positive values. This latter comment applies to all the figures with colorbars. This colormap is not colorblind friendly.

L242 I am not sure LKF is spelt out in the text. Also this sentence is quite obscure. Does it mean that the subjective choice of the clusters (L217) would change the results?

L246 Please explain the meaning of “previously neglected periods”

Sec 3.5 Please indicate how many buoys have been used, what is the range of sampling frequency and why the interpolation frequency is now 6-hourly. I wonder if the 3-day window is justified in this context, and if yes, it should be justified. LKF are rather random events, not necessarily linked to the synoptic scales

L269-270 Please explain what previous behaviour means here. This does not seem to be shown. Only points from the peak period are presented. The fact that TSE is positive and apparently saturated, it may mean that the event has already started, but this is not clear.

L271 Bank Islands
L271-272 The buoys in the south show a clear stretching-compression cycle during this period but no evident fracturing. Maybe the authors can comment further on what kind of feature is being detected by the method, and whether it is realistic.

L300-303 The English can be improved. I also think this is a rather bold statement, given that this diagnostic is only related to sea ice dynamics. It can be associated with other data to obtain further insights in the coupling. The points after this sentence do not critically assess the required improvements as indicated in the sentence.

L314 Correlations? Do the authors mean approximations?

L331 of TSE signals

L336 I would argue with this statement. I think this is what the methodology would allow. The results show very promising applications of this method, but the interpretations are still in a preliminary phase.

Figures in Appendix: they should all be renumbered (not S2 but A2).
Appendix A2

Please explain if the rotation is done with the same angle. I would suggest first to discuss the flow field, and shift panel A2c to panel A2a. The buoy locations could also be added on that field, and the reader would see that they are meant to approximate the whole field and not local regions. I would also suggest to limit the X-axis of A2b to the range between 0 and 50, to make it more realistic. The convergence is indeed rather rapid, but it is not clear where it does happen

Appendix A3

This is another excellent example, but it is not adequately generalised in the text. The point is very well made, but the implications are not completely clear to the reader less interested in the mathematical formulation. The chosen flow is rather peculiar (a locally divergent flow, probably less relevant in sea-ice dynamics) and one may argue that this conclusion cannot be generalised to any kind of flow.

References
