Thank you for taking the time to review our manuscript. We have responded to your comments below.

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.

Thank you for bringing up this point. We have included an additional Appendix that mathematically relates trajectory stretching exponents to more classically studied rate-of-strain tensor diagnostics, including shear. There, we clarify that TSEs are not interchangeable with shear or divergence. That being said, the reviewer brings up a good point. TSEs spatially and temporally localize large stretching in the direction of the sea ice buoy trajectory. This does not guarantee a fracture either, but fracture is potential outcome from significant ice stretching and compression. We have reframed the manuscript so that this method is not a tool for identifying fracture, rather we use fractures as an external validation that TSEs have indeed identified significant stretching/compression (that actually led to factures). This is now clarified further in the abstract and throughout the text.

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

Thank you for the suggestion. The manuscript has been re-written and re-organized with a section devoted to the different experimental data sets. We have included a more general introduction and explanation of our intents. We have also further clarified this method is not fracture specific, nor is it synoptic event specific, but we are using synoptic events as they provide a useful timescale at which we can verify our identification of stretching/compression through storm analysis and remote sensing comparisons. We have improved the text to reflect this.

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.

Thank you for bringing this to our attention. We have now provided an example script "TSE_Buoy_Example.m" in the same repository, specifically for calculating TSEs from buoy trajectories. This function only requires latitude, longitude, and timestamp vectors. This script assumes a uniform temporal sampling as the GPS data were all resampled to uniform samplings in the three examples in the text, but leaves the actual discretization of the data up to the user's preference.

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.

Thank you. We have reorganized the manuscript to improve the flow and clarity of the topics and included an expository appendix section deriving TSEs.

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.

We have clarified the difference between our Lagrangian approach and the Rampal et al. (2009) dispersion rates. Pairwise dispersion relies on pairs of buoys, and is not a single buoy analysis like TSE. Their approach is a slightly modified version of relative dispersion commonly used in oceanography, with which TSEs have already been compared in previous studies. Rampal et al. also rely on manipulations and assumptions that we do not. This is also referenced in the new text.

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.

Thank you. The introduction to the results section have been changed and the referenced paragraphs have been moved to the methods section. We further explain our choice of Lagrangian timescale, and show the influence of variability in that choice.

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.

Thank you for bringing this to our attention. We now include a comparison with different TSE time scales in the Appendix. We have provided a better explanation of the choice of sampling frequency. We now explain how these choices were based on the datasets being analyzed, and other studies on the topic.

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)

Thank you, we have clarified and rewritten this section in connection with our new section on integration periods in the appendix.

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

Thank you. We have clarified what we mean by noise. There are clear artificial influences in some of the gps signals that cannot be attributed to inertial oscillations.

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.

Thank you. As mentioned above, we have provided a new code. TSE is a Lagrangian diagnostic evaluated over a certain length of a trajectory. To generate a TSE time series, a summation of instantaneous values is performed in a forward-looking fashion so that the TSE timeseries appears predictive. This is by design from dynamical systems. We have further explained this in the methods section.

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.

As TSE is computer for a specific section of a buoy trajectory, you are measuring stretching for that section. If the time window is larger than the scale of a storm, you will still be measuring all the stretching during the storm, but possibly include quiescent periods before and after the storm. This could result in a lower TSE value as we are normalizing by integration time, but this does not mean we cannot detect features at time scales smaller (or larger) than the window of computation. We have explained this further in the methods.

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.

Thank you. The storm definition came from previous N-ICE evaluations, including meteorological station data. This was not performed by us, and is referenced in the text, as is the subsequent analysis of sea-ice response by Itkin. We have expanded on the IABP example where the sea ice is transition towards a free-drift state to better explain these features.

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

Thank you for bringing this miscommunication to our attention. The cross-correlation you are referring to is not a sensitivity analysis, rather a quick validation of what is qualitatively presented. We do not think this computation warrants further validation as it is an observation supported by all the TSE peaks that precede stretching events discussed at length 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.

Thank you for the suggestion. We have added a small discussion on Antarctic sea ice. This is a venue some of the authors have been investigating, and may prove to benefit from TSE calculations.

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.

Thank you. We have modified the introduction to improve clarity and provide more references.

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.

Thank you again for bringing this up. We seek to have this work accessible to a wide range of scientists and as such we have modified this introduction section to improve clarity and provide more references.

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

Thank you. This has been corrected.

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.

We have changed from verification to assessment and discussed the connection between the 50-day evaluation window and the IABP experiment. We have significantly expanded on the influence of slowly-varying conditions and how TSEs are derived using this assumption, both in the text and in a new appendix section.

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

Thank you, this has been clarified.

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.

Thank you. We have reorganized the development of the polygonal and single-buoy methods.

L97 Maybe "nature" is missing in this sentence

Nature has been added. Thank you.

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.

Thank you for pointing this out. These equations have been modified for clarity.

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.

Thank you for this clarification.

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.

Thank you for bringing this to our attention. We reference values exceeding 1d^{-1} in the text, but neglected to connect this with the dashed lines. This has now been mitigated in the text and in the caption. We have rescaled the y-axes as well.

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

This has been added to the text.

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?

Thank you, this figure and comparison has been improved.

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

Thank you. We have changed this reference to fit the standards of ASF and ESA (i.e. https://asf.alaska.edu/data-sets/sar-data-sets/sentinel-1/sentinel-1-how-to-cite/)

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.

Thank you. This before/after has been clarified. As well, we have changed the colormaps to a divergent colorscale when indicating positive vs negative values, and sought colorblind friendly linear maps.

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?

LKF is spelt out in the introductory paragraph of the new dataset section. Yes, the subjective nature of cluster choice does influence what is detected.

L246 Please explain the meaning of "previously neglected periods"

Thank you. This sentence has been expanded to

"In this scenario, the stretching and relaxation measured by TSE presents a clear correlation with material deformation of the ice and suggests TSEs may provide ice behavior insight during times when Green's theorem methods are not possible, such as when there are too few buoys or they are their orientation is inappropriate, and when array-based approaches have underestimated dynamic behavior."

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

We have further explained the sampling frequency and the Lagrangian calculation window in the new dataset and methods sections.

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.

We have changed this sentence to the following:

"All buoys in the free-drift region have high (red) TSE values and create another local maximum in the mean time series, further supporting this relatively significant stretching event in March and April when compared to times prior to and after these months."

L271 Bank Islands

Capitalization Corrected. Thank you

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.

Thank you. We have further expanded on the difference of buoys inside the free-drift and outside the free-drift zone.

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.

Thank you. We have rephrased this statement as follows

"The single-buoy quasi-objective trajectory stretching exponents (TSEs) identify dynamic sea ice events that are potentially significant in terms of understanding spatially and temporally varying sea ice deformation. As sea ice dynamics plays an important role in atmosphere-ice-ocean exchange processes, we find the further event-detection sensitivities possible with TSEs

are a valuable complement to common, polygon-based divergence, shear, and deformation approximations.”

L314 Correlations? Do the authors mean approximations?

Thank you for the question. TSEs do not measure fractures or break-up events, and thus the high trajectory stretching exponents are only correlated with the concurrent fractures and break up that we observed.

L331 of TSE signals

Thank you. Corrected.

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.

Thank you. We have improved this line to reflect your understanding.

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

The figures have been renumbered.

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

Thank you for the suggestions. Figure A2 has been adapted following your suggestions.

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.

Thank you for bringing this to our attention. We have removed this example from the appendices as we found it brought more confusion to the reader than clarity.

References

Gimbert, F., Marsan, D., Weiss, J., Jourdain, N.C., Barnier, B., 2012. Sea ice inertial oscillations in the Arctic Basin. The Cryosphere 6, 1187–1201. https://doi.org/10.5194/tc-6-1187-2012

Vichi, M., Eayrs, C., Alberello, A., Bekker, A., Bennetts, L., Holland, D., de Jong, E., Joubert, W., MacHutchon, K., Messori, G., Mojica, J.F., Onorato, M., Saunders, C., Skatulla, S., Toffoli, A., 2019. Effects of an Explosive Polar Cyclone Crossing the Antarctic Marginal Ice Zone. Geophysical Research Letters 2019GL082457. https://doi.org/10.1029/2019GL082457