This paper documents the in-depth analysis of emergent linear deformation features from data sets of sea ice drift and deformation. The analysis is generally well documented and the results well presented. The results presented here will be very interesting to the field of sea ice rheological modelling and recommend it for publication after some minor corrections to make the methodology more understandable and repeatable.

My suggestions for the main corrections to this paper are as follows:

The first is based around figure 3. Can you add more detail, probably in the text, about the distance from the intersection point the intersection angle calculation is based upon. For figure 3a, the angle calculated at the distance of the label appears to be more acute that at a $1/5^{th}$ of this distance. This is even more pronounced for the 44 degree angle in the non-conjugate case. Is this distance a tuning parameter for the algorithm, can it be tuned, and is it a factor in the difference between the MOSAiC and RGPS data? Have variations to the distance from the intersection point been investigated, and do the final results of this study change with it?

The second is on the topic of reconstructing the yield curve. At the moment it is very difficult to understand exactly how this done from the description in the text. Adding more information is essential to allow this method to be repeatable, and also for the context of the results to be understood. This is true for the results in the main paper body, but even more so for those in appendix B2. I have given more precise comments for this section below, with the detailed minor comments.

L2 This is an awkward sentence as many rheological models don't even explicitly consider LKFs. A statement on emergent deformation features that are linear in nature will be more accurate.

L15 This sentence needs splitting or modifying with an 'or' instead of too many commas.

L18 It is not obvious how shear motion influences the halocline. Can you expand on this as it will be an interesting and relevant inclusion?

L24 Feltham (2005) is another good citation for granular flow.

L23 to L32 This is a good paragraph, but it ends on a note about the method of this paper. Consider splitting this last sentence in order to keep the writing coherent.

L41 Similar to the point above about the abstract, an extra description of what is a LKF in the context of a sea ice model will help.

L44 Keen et al. (2021) is a worthwhile inclusion in this list.

L 50 'phi' has not rendered correctly.

L39 and 55, MDA acronym is only used twice so it will be easier to read with the full term in both cases.

L60 this sentence is a little awkward with the citations next to the question mark, consider moving them to the first mention of the normal flow rule.

L 67 Can you be more explicit what is done beyond the work of Hutter et al. (2019 and 2022)?

L 105-110. This methodology is incredibly difficult to follow, and it is not at all obvious how LKFs are extracted beyond point 1. If this methodology is the same as Hutter et al. (2019), then it does not need to be described in detail. If any modifications have been made, then they need to be described better than is done here. Preferably with another figure showing how it all works.

L 117 a similar point to above, this sentence is filled with jargon and it is not all obvious what is done here. I see that the Hutter et al (2019) data is that which is mainly used, but the modifications made to the algorithms for the MOSAiC data need to documented in a way that is understandable and repeatable.

L 123 This method of calculating angles seem very sensible. Can extra sentence or two be added to explain how the vorticity relates to the principle, or most compressive stress direction? When comparing this description with figure 2, and later distributions, on a first read it seems unclear why there are obtuse examples. More description here will help explain this. For examples figure 2 contains no case for compression two directions, though this case will occur and can cause LKF features (see Heorton et al. 2018 for a model example). I assume the angle technique will account for this case too?

Figure 2, are the red arrows for deformation or stress?

L 140 (Heorton 2018) is relevant here too.

L 155 It is not obvious what Monte Carlo test is performed here to show the accuracy of the distribution. Can a citation for this test be added, or a description of how it is done? Is it in either (Clauset et al., 2009; Hutter et al., 2019) ?

L 165 A little aside on the method use will help here. Do you plot only the deformation rates from the pixels defined as part of a LKF?

L 169 Can a citation for the deformation regimes in these areas be added?

L 171 Is this reason for more shear speculative? If so can you state that this is an authors hypothesis. A similar issue can arise for the previous point. An extra figure that may help is a averaged map of the LKF deformation rates. This will back up this hypothesis and be a very interesting plot.

L 172 Is it possible to make this comparison when these two data have been normalized as stated previously?

L 176 My interpretation of 5d is that one of the data (I assume MOSAiC due to a previous figure) has a greater number of higher positive dilatancy angles. Is this what you mean?

Figure 5. Are the line colours the same as Figure 4? If so this needs repeating for easier readability. It is not immediately obvious how the normalization is achieved for the 5d. The legends I can see have some formatting and I think the decimal points are missing. This adds to the difficulty interpreting 5d. Will 5d benefit from no normalization?

Equation 4 what is tau_0?

L 208 what is a breaking index and i?

L 221 This functional form seems confusing for me, I understand the concept and it is sound, but should this equation read sigma_II = F_I(sigma_I)? Or F_I(sigma_I,sigma_II) = 0 (or a constant)? I guess this is a question of how the potential is defined.

L 226 PDF of what? Can you be more explicit on the exact angle data used?

L 228 can you define this origin in numerical coordinates? Is it sigma_I, sigma_II = 0?

L226 – 231 Is this method of curve reconstruction novel? If so it is very useful. Is it possible to include a more theoretical description of what is achieved? As in: using information on which angles to recreate the relation between principle stress components due to which assumptions. Is this done in the previous paragraph? I am currently finding it hard to link the two.

L 227 what assumptions are made for the from of F_I? How is F_I calculated from the data?
L 228 Which angles, intersection angles or internal friction angles? If intersection angles, why are these associated with the origin or invariant stress?

L 248 citation needs to be in parenthesis.

L 253 'We wonder' is this in relation to the work documented here or a comparison to future work? More defined language is needed.

L 319 The data availability needs to be a working url, not a paper citation. Does one exist from the other publication? If not, then it can not be claimed that the data is freely available.

Appendix B. This section really requires a short introduction. It is described earlier, but repeating the experiments and data used is needed in order to understand it in isolation.

L 345, it is not clear what has been done here. Have the angles been calculated from deformation plots or some other method? If the method in 3.4.2 is reversed then how is

figure B1 d made? As this method seems circular. Or are two methods used for the two pdfs in figure B1 c?

Additionally for figure B1, is the reasoning behind this appendix to show that the method can reconstruct a yield curve from deformation along intersection angles when using a model? If so then figure B1 is compelling, though can you comment on the larger difference for the e=2.0 case, and what context this has for the results in the main paper body?

[1]

Feltham, D.L. 2005. Granular flow in the marginal ice zone. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*. 363, 1832 (Jul. 2005), 1677–1700. DOI:https://doi.org/10.1098/rsta.2005.1601.

[1]

Keen, A. et al. 2021. An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models. *The Cryosphere*. 15, 2 (Feb. 2021), 951–982. DOI:https://doi.org/10.5194/tc-15-951-2021.

[1]

Heorton, H.D.B.S. et al. 2018. Stress and deformation characteristics of sea ice in a high-resolution, anisotropic sea ice model. *Phil. Trans. R. Soc. A*. 376, 2129 (Sep. 2018), 20170349. DOI:https://doi.org/10.1098/rsta.2017.0349.