

Response to reviewers after first review

July 2, 2025

We thank both reviewers for their time and for their useful comments and suggestions. Our responses below are in blue.

Reviewer 1:

This paper presents a detailed sensitivity study on the influence of a non-normal flow rule in the viscous-plastic (VP) sea-ice rheology on the simulation of linear kinematic features (LKFs) in Arctic sea ice. The authors implement the concept of a plastic potential and non-normal flow rule introduced by Ringeisen et al. (2021) into the CICE sea ice model and conduct a comprehensive series of pan-Arctic simulations based on this implementation. However, the central result of the study is that the peak of the probability density function (PDF) of the simulated intersection angles between conjugate LKFs remains at 90° , whereas observational data show a peak around 45° . This finding is counterintuitive, as Ringeisen et al. (2021) demonstrated that using a non-normal flow rule enables the simulation of smaller intersection angles that align more closely with observations. This discrepancy raises a fundamental and scientifically important question: what is the underlying cause of this difference? Answering this question would be interesting and valuable for the community. Instead of systematically addressing this question, the authors limit themselves to a purely parametric sensitivity analysis. As a result, parts of the paper take on the character of a technical documentation of the non-normal flow rule implementation in CICE, without achieving the intended effect, namely more realistic intersection angles. In my view, the innovative contribution of this paper is therefore limited. Since the discrepancy between the idealized tests in Ringeisen et al. (2021) and the CICE implementation remains unresolved, the overall validity of the presented results is questionable. In my view, it is entirely possible that the peak in LKF intersection angles at 90 degrees arises from numerical details specific to the CICE implementation, and that a careful reproduction of the setup proposed by Ringeisen et al. could yield a peak at smaller angles. The authors' explanation, that this discrepancy is due to a grid effect, appears vague and insufficiently analyzed. Moreover, this hypothesis is not in line with previous studies, such as Hutter et al. (2022) and Hutter and Losch (2020).

We think our manuscript focused too much on the intersection angles and

did not convey enough that, beside the intersection angles, a non-normal flow rule is a very interesting and useful model capability. Indeed, the plastic potential allows to optimize the deformations while maintaining the same yield curve. Similarly, the yield curve can be modified to optimize simulated landfast ice and sea ice drift with little impact on the deformations. We have modified the abstract, some of the introduction and conclusions to emphasize these clear advantages of the plastic potential.

I therefore recommend that the manuscript place greater emphasis on the core scientific question: Why does the CICE implementation yield different results from the idealized tests by Ringeisen et al.? A targeted and transparent validation of the implementation is essential in this regard. For these reasons, I recommend a major revision.

We include a supplement to the revised version of the manuscript. This supplement describes the numerical experiments that were conducted to validate the implementation of the plastic potential and non-normal flow rule. In our initial response to the reviewers we implemented a similar experiment as the one of Ringeisen et al. (2021). For this final response, however, we spend some time implementing the Dirichlet condition for the velocity and were therefore able to implement the uni-axial compression experiment of Ringeisen et al. (2021). We also find that the intersection angles vary when changing e_F and e_G and that the simulated angles follow closely the theoretical Arthur angles. Moreover, we also confirm that the numerical solution is viscous-plastic (i.e. states of stress are inside or on the yield curve) even when e_G is not equal to e_F . We are confident that our implementation is correct and that it follows the one proposed by Ringeisen et al. (2021).

Giving a complete explanation why the non-normal flow rule with $e_G < e_F$ does not lead to more realistic LKF intersection angles in pan-Arctic experiments is beyond the scope of this paper. In the mean time, we think that our study is sound and could inspire other researchers to further investigate this.

Major comments:

1) The authors write: “We don’t think it is related to the use of a C-grid and a VP rheology by Ringeisen et al. (2021) as opposed to the B-grid with EVP dynamics used here.” To validate this claim and, more importantly, to demonstrate that the implementation performs as intended, I suggest reproducing the idealized test case from Ringeisen et al. within CICE and first evaluating whether their results can be replicated. This would confirm that the basic implementation is correct and that differences in numerical treatment—such as C-grid vs. B-grid or the choice of solver—do not, as the authors suggest, contribute to the observed deviations in the results.

As stated in our response to reviewer 2, implementing the plastic potential

of Ringeisen et al. (2021) is incredibly simple; it requires minor modifications to the code. We have revised our code and confirm it is consistent with the plastic potential formulation of Ringeisen et al. (2021). The reviewer is invited to read the supplement section where we validated our implementation and overall reproduced the results of Ringeisen et al. (2021).

2) “For VP experiments with a normal flow rule, Hutter and Losch (2020) compared the mean orientation of LKFs with the grid axis and could not find a clear correlation. For the SIREx project, Hutter et al. (2022) made the qualitative argument that the peak of the PDF at 90° is not caused by LKFs aligned with the grid as models with an unstructured grid also lead to a peak at 90° . Our analysis, involving the computation of the intersection angle for all the conjugate pairs, clearly indicates a tendency of simulated LKFs to be aligned with the grid.” This is already the second aspect in which the results of this study seem to differ from existing work. To relate the observed behavior to a grid effect, a more in-depth investigation, as suggested in point 1, is required.

We agree that our presented results seem to contradict previous studies. At the same time, we want to highlight that this study is the first thorough quantitative analysis of the alignment of simulated LKFs with the grid axes and that the methodology differs from the ones in previous studies. Hutter and Losch (2020) binned all LKFs in 200×200 km boxes, computed the orientation distribution, and visually compared its mode(s) with the grid orientation. In this comparison, they could not find a distinct Arctic wide correlation between the mode and the orientation of the grid axes. Note that the orientation of an LKF contributing to the distribution within a box is defined as the mean orientation of segment of the LKF that is laying within the box. In doing so, the orientation of the entire LKF contributes to the analysis. Also, all LKFs, conjugate and non-conjugate ones as well as new and old LKFs, were averaged.

As already written in our manuscript, Hutter et al. (2022) only made the qualitative argument that the peak at 90° is also found for models with unstructured grids and therefore likely not linked to the orientation of the grid axes, without further analyzing the orientation of LKFs in relation to the numerical grid used in the simulations. In our quantitative analysis, we only consider newly formed LKFs of conjugate pairs. Also, the orientation is measured directly at the intersection point. In doing so, we omit the effect that advection and different type of deformations can have on the orientation of LKFs. In our specific analysis, we find that LKFs of conjugate pairs tend to preferentially orient themselves along the numerical grid axes in a region close to the intersection point. In Fig. 1 and 2 of the manuscript this characteristic is also observed showing a preferred orientation at the intersecting points and bended LKFs with varying orientation away from the intersection point. Compared to previous studies, the presented analysis therefore offers new insights about the formation of fractures in VP-EVP models.

We added more details in the revised manuscript to explain why we draw a different conclusion than Hutter and Losch (2020) and Hutter et al. (2022).

3) In line 351, the authors write: “As demonstrated in other studies, we find that the elastic-viscous-plastic (EVP) rheology with a normal flow rule (i.e., $e_G = e_F$) leads to a PDF of intersection angles (for conjugate pairs) with a peak at 90° . Using a non-normal flow rule with the elliptical yield curve, as introduced by Ringeisen et al. (2021), does not remedy this problem. Either with $e_G < e_F$ or $e_G > e_F$, the peak is still at 90° .” In my opinion, this study cannot claim general validity of its findings, as it remains unclear what the cause of the observed failure is. In particular, it has not been convincingly ruled out that the observed effect may be due to details in the implementation. From my perspective, it is still possible that the effect observed by Ringeisen can also be reproduced in pan-Arctic simulations, provided that the setup is carefully implemented.

See our responses above and the supplement section of the revised manuscript.

Minor comments:

1) Line 32: “It is expressed with the use of a plastic potential: the post-failure deformations are normal to the plastic potential.” This sentence is extremely technical for an introduction and difficult to understand. Some readers may not know what it means that post-failure deformations are normal to the plastic potential. It should either be reworded and better explained or supplemented with a sketch or equation that clarifies the meaning.

We refer to the papers of Ringeisen if the reader wants to have more details. We don’t think we need to provide a schematic as Ringeisen et al. (2019) and (2021) already include nice illustrations.

2) Lines 31–40: These are too technical and difficult to digest for an introduction. Please revise and invest more effort in explaining the technical terminology.

We are aware that our article is a bit niche and not accessible to all the researchers in the field. However, we refer to many published articles to support the introduction of the concepts and equations. This is our approach to make sure the article is not too long.

3) Move Section 2 to the appendix. The presentation of the concepts is currently very isolated, making it completely unclear how they relate to the equations that describe the rheology. For example, in line 60: Which system of equations are you referring to? Please specify clearly.

This is just a brief introduction about the plastic potential and yield curve. We prefer to keep section 2 here. People familiar with the standard VP rheology

will easily recognize these equations. We have clarified line 60 in the revised manuscript and now refer to the constitutive and momentum equations.

4) In addition to the equations describing the rheology or yield curve in the standard VP model and in Ringeisen’s version, graphical representations would be helpful for better understanding the key parameters e_F , e_G , and e . In the current presentation, it is very difficult for readers from the Cryosphere community, who may not be familiar with all of Ringeisen’s previous work, to follow the argument.

We refer to the papers of Ringeisen if the reader wants to have more details.

Line 65: Please explicitly state the constitutive equation.

We don’t think this is needed as it is given in many papers (e.g. Hibler 1979).

Line 77: “With the non-normal flow rule, η is now equal to $e_G^{-2}\zeta$.” – Why? Please explain.

This is from the derivation of Ringeisen et al. (2021). We have clarified the text about this.

5) Line 136: Where does this choice come from? Why is e_G set to $e_F/1.5$, e_F , or $1.5e_F$? Please justify.

The e_F values were chosen to match the values used in previous studies (e.g. Dumont et al. 2009). Setting e_G to either $e_F/1.5$, e_F , or $1.5e_F$ ensures that the experiments are consistent for the different e_F and that a sufficiently large range of values are tested.

6) Line 155: “A kernel value of seven is used for the detection algorithm.” – Why 7? What does it mean? Please explain.

We refer here to the kernel width used to determine the orientation of LKF at each grid point. A rotating 1-d kernel of size seven centered at a LKF pixel is used to determine the orientation perpendicular to the LKF by maximizing the standard deviation of total deformation within the kernel. This kernel size should be chosen to roughly represent the width of LKFs in the data. To make clearer what kernel size we are talking about, we moved this sentence two paragraphs below, where we discuss the morphological thinning and orientation of the LKF.

7) Lines 168–173: What is α ? What does “morphological thinning” mean? Please include a sketch to make this clearer.

The morphological thinning is associated with the LKF detection algorithm of Hutter et al. (2019). This part of the algorithm ensures that a detected pixel only belongs to a single LKF. We do not think we need to add anything as we refer to this paper which describes the method and includes a schematic.

8) Line 176: What do you mean by “conjugate fault lines”?

We don’t use this expression in the revised manuscript. We now use the expression pairs of conjugate LKFs or just conjugate LKFs (i.e., LKFs that form simultaneously under compressive stresses).

9) Line 177: What exactly is the vorticity being analyzed?

We follow the work of Hutter et al. (2022) by using the simulated sea ice vorticity deformation to determine if the LKFs are pairs of conjugate LKFs.

10) Lines 177–182: This section is extremely technical and hard to understand. Either explanatory sketches should be added, or the content should be moved to the appendix.

We understand that our paper might not be accessible to all the sea ice community as it targets a small and specialized audience. In order to keep the paper concise we don’t have the choice to refer to other scientific articles instead of explaining many complicated concepts related to sea ice rheology.

11) Figure 1a: To me, the structures shown look like numerical instabilities. It is possible that the modification proposed by Ringeyisen increases the stiffness of the system of equations, making it harder to solve. While 900 iterations are considered sufficient in the standard EVP model, they may be inadequate for the modification investigated here. I therefore recommend conducting tests with smaller time steps and/or more iterations to check whether sharper structures can be resolved and whether the 900 EVP subcycles are indeed sufficient here.

It is indeed stated in Ringeyisen et al. (2021) that the non-normal flow rule leads to a more difficult numerical convergence. This is also what we observe in our idealized numerical experiments described in the supplement section. For the pan-Arctic experiments, 900 EVP subcycles is already a lot (the default is 240 in CICE) and quite demanding in terms of computational time.

12) Line 366: “Note that our conclusions remain the same whether the implicit approach (VP) in CICE or the explicit one (EVP) is used.” There are numerous publications, including some by Lemieux, that show that ten Picard steps are insufficient to achieve a convergent solution. Therefore, it is possible that the observed effect is due to the inadequate accuracy of the approximation solutions in both setups (EVP and Picard).

Again, in terms of computational efficiency, it is too demanding to use too many Picard iterations in these realistic pan-Arctic simulations. Note that the time step also has to be considered with respect to the numerical convergence. Our simulations with 10 Picard iterations and $\Delta t=3$ min have a better convergence than the ones of Lemieux and Tremblay (2009) with 10 Picard iterations and $\Delta t=30$ min. We don't think that the too wide simulated intersection angles are due to the numerical convergence.

Reviewer 2:

This is a review of the manuscript entitled "Impact of non-normal flow rule on linear kinematic features in pan-Arctic ice-ocean simulations." The study discusses the effect of allowing the rheology used in the CICE large-scale sea ice model to deviate from the normal flow rule, which is normally used to determine the direction of deformations when sea ice undergoes plastic deformation. It is largely motivated by a previous study that showed that using a nonnormal flow rule resulted in more realistic linear kinematic features (LKFs) in a more idealized setup. In particular, it had been shown to significantly improve the model's ability to reproduce realistic intersection angles of conjugate faults. Here, the authors show that this is not the case. They discuss the potential reasons for this negative result compared to the more idealized study and suggest (with some supporting arguments) that these intersection angles may be largely constrained to follow the model grid axes, resulting in intersection angles often close to 90° in the case of the grid used here. Despite this negative result, they show that using a non-normal flow rule has other effects on LKF properties, which could make its use a convenient way to tune LKF characteristics in large-scale sea ice models.

The manuscript is well-written and clear. The scope is well-defined, and the analysis is sound and well-supported. The topic is perhaps a bit niche, but it aligns with previous studies published in the journal and is of interest to the sea ice modelling community targeted by The Cryosphere. The suggestion that the peak at 90° partly results from the alignments of LKFs with the grid axes is sound. It may not be fully demonstrated here (as mentioned), but the authors have gathered enough elements to point to this cause in their results and discussion. The argument is strong enough to motivate the community interested in sea ice deformations to investigate the numerics instead of only focusing on the physics. Additionally, the fact that the non-normal flow rule can be "tweaked" to tune sea ice deformations properties is an interesting result for the sea ice modelling community. Therefore, I support the publication of this manuscript in The Cryosphere. I do not have comments that I consider major, but I would recommend the minor revisions below:

We thank the reviewer for these positive comments.

General comments:

Given that one of the main results is that using a non-normal flow rule can be a convenient way to tune certain LKF properties independently, the authors may consider commenting on the implementation in CICE. Does it require substantial modification to the code? Does it significantly impact computational time?

This is a very good suggestion. The implementation is actually incredibly simple. Indeed, only small modifications to the calculation of the shear (η) and bulk (ζ) viscosities are required. With the standard VP rheology, $\eta = e^{-2}\zeta$ where e is the ellipse ratio and $\zeta = P/2\Delta$ with $\Delta = [D_d^2 + e^{-2}(D_t^2 + D_s^2)^2]^{1/2}$. For the non-normal flow rule, e^{-2} is simply replaced by e_G^{-2} for computing η while e^{-2} in Δ is replaced by e_F^2/e_G^4 . There is therefore no impact on the computational time. We have added the following text at the end of section 2:

‘As only small modifications to the definitions of η and Δ are required, the implementation of the non-normal flow rule for the VP rheology with an elliptical yield curve is straightforward. There is also no impact on the computational efficiency.’

I sometimes found the order in which the figures are referenced a bit confusing. For example, it appears that Figure 4 is referred to after Figures 5 and 7, and Figure 2 before Figure 1. This has little impact in the preprint, since all figures are at the end, but the authors may consider reordering the figures for the final version.

We added a new figure showing the mask used for the analysis and the orientation of the grid axes. We have also moved Fig.4. It is now Fig.9 in the revised manuscript. All figures are now referred to in the correct order.

Specific comments:

L213: I think “of” is missing (or something else)

This has been corrected.

L337/338: I find this sentence a bit unclear.

We have rephrased this sentence.

L340/350: I find the ideas in this paragraph a bit hard to follow, I think it would benefit from a bit of rewriting (especially the sentence starting with “But clearly..”)

We have rephrased and clarified the content of this paragraph in the revised manuscript.

Best regards,
Guillaume Boutin

Thanks again to both reviewers.

Jean-Francois Lemieux