Dear Qiang,

Please find attached our revised manuscript and the difference file.
Responses to the reviewers’ comments can be found concatenated after this letter.

Best regards,
Nicolas Mokus

Response to RC1's report

The already good manuscript has been improved following Reviewers' suggestions. I personally enjoyed the addition of Sec 2.6. I will leave here few minor comments for potential consideration by the authors.

1 Minor comments

1.1

It would be great if SWIIFT will find its way in coupled ocean-sea ice models in which waves are also accounted for. For this to be feasible, the computational time of the SWIIFT module should be small. Within this context, it would be appreciated a comment on the computational cost of the algorithm, e.g. what is the CPU time for the example in Sec 2.6? What is the computational cost of SWIIFT relative for example to Swell fracture in Horvat and Tziperman 2015?

Thank you for this relevant comment. If our ultimate goal would indeed be for our results to percolate into global climate models, we are not for the moment thinking of running SWIIFT alongside (as a module) these large scale models. We designed it primarily as a tool to get a better grasp of a physical process and derive associated parametrisations, in particular at a fine scale (that of floes) and when it comes to time evolution (something Horvat and Tziperman (2015) cannot do, as it considers instant fracture along a full grid cell). For instance, we can use SWIIFT to follow the progress of the breakup front. Comparison with observations of this process is our current goal.

Because SWIIFT resolve time-evolution and fine spatial scales, it is unfortunately slow. It is also consequence of, on the first hand, the sought convergence in fracture patterns which requires a short timestep; and, on the second hand, the systematic approach to fracture lookup, which requires a first discretisation of the hypothetical fracture locations (with a resolution high-enough to detect local maxima and avoid aliasing of the expected free energy) and as many local minimisations as pairs of local maxima were found. It can be accelerated through several axes:

- Tweaking the termination condition of the minimisation algorithm. The default condition of the SciPy function we use is 1×10^{-5}, which means we seek convergence in floe lengths of the order of $10\,\mu\text{m}$, which is quite unnecessary.
- Moving from local to global optimisations. Several methods exist, either stochastic or deterministic. The limitation of either is that finding the true global minima is never guaranteed, which can be an acceptable tradeoff when running an ensemble of simulations with added noise.
- Parallelising the fracture search. This can be done either by parallelising the local minima search on a single floe, or parallelising fracture search across several floes.

The first point is trivial beyond some eventual sensibility testing, and we have started to work on the second point. As illustration, the first 20 s of the simulation presented in Sect. 2.6 take 41 minutes to run; the first 9 s (1050 timesteps) of these take only thirty-one seconds, as the conditions for fracture are not met (the wavefield is not energetic enough for the elastic energy of the initial floe to exceed the fracture energy) and, as a result, no fracture search is conducted.

However, when it comes to deriving parametrisations for the coupling of our wave–ice interaction model to large scale models, an obvious method we are considering would the preliminary building of a table predicting some floe size distribution statistics out of wave and ice conditions, as in Roach et al. (2018). These were then used to train neural networks to considerably reduce the associated overhead (Horvat and Roach 2022).

1.2

In the intro, where the MIZ response to storms is discussed, I leave a couple of references that the authors can consider adding (Vichi, 2022, TC, *`https://tc.copernicus.org/articles/16/4087/2022/tc-16-4087-2022.html`* ***; Cavallo et al, Comms Earth Env,*** *`https://www.nature.com/articles/s43247-025-02022-9`* ***).***

Thank you for these suggestions. We added the reference to Cavallo et al. We appreciate the other reference, but we think its topic (identifying the marginal ice zone from satellite products) is not directly implementable into our discussion and would require introducing the broader context, which would lengthen the text.

1.3

2.4.2 "Wave state" sounds unusual, the preferred forms are "sea state" or "wave properties".

Thank you for the suggestion. We changed the Section title to "Sea state".

1.4

2.6 L351, I think a better and clearer phrasing would be "wave height $H_S = 0.5\,\mathrm{m}$ and peak period $3.84\,\mathrm{s}$, ...", since the H_S and peak period can be prescribed in dependently of each other.

Thank you for this comment. In their seminal paper, Pierson and Moskowitz introduced the Pierson–Moskowitz spectrum as depending on one parameter, the 19.5 m wind speed as measured by a given ship. Modern formulations used by various wave modellers express the spectrum parametrised by the significant wave height *or* the peak frequency (or the wind speed). There is thus a mapping between significant wave height and peak period, that can be expressed

$$\frac{\alpha g^2}{(2\pi)^4} = \frac{5}{16}\frac{{H_\mathrm{S}}^2}{{T_p}^4} \tag{1}$$

with $\alpha = 8.1 \times 10^{-3}$, following the guidelines of the International Towing Tank Conference (Stansberg et al. 2002).

This spectrum is generalised into the Bretschneider family, with two independent parameters. Unfortunately, there is a lack of precision in the literature when discussing these spectral formulations, so that the terms Bretschneider spectrum or two-parameter Pierson–Moskowitz spectrum may be used indiscriminately by different authors. Herein, we specifically used the one-parameter Pierson–Moskowitz spectrum Our phrasing thus convey our intended meaning: setting the significant wave height unambiguously sets the peak period. We added this precision to our manuscript.

References

Cavallo, S. M., M. C. Frank, and C. M. Bitz (Jan. 2025). "Sea ice loss in association with Arctic cyclones". en. In: *Communications Earth & Environment* 6.1. DOI: 10.1038/s43247-025-02022-9.

Horvat, C. and E. Tziperman (2015). "A prognostic model of the sea-ice floe size and thickness distribution". In: *The Cryosphere* 9.6, pp. 2119–2134. DOI: 10.5194/tc-9-2119-2015.

Horvat, C. and L. A. Roach (Jan. 2022). "WIFF1.0: a hybrid machine-learning-based parameterization of wave-induced sea ice floe fracture". In: *Geoscientific Model Development* 15.2, pp. 803–814. ISSN: 1991-9603. DOI: 10.5194/gmd-15-803-2022.

Pierson, W. J. and L. Moskowitz (1964). "A proposed spectral form for fully developed wind seas based on the similarity theory of SA Kitaigorodskii". In: *Journal of geophysical research* 69.24, pp. 5181–5190. DOI: https://doi.org/10.1029/JZ069i024p05181.

Roach, L. A., C. Horvat, S. M. Dean, and C. M. Bitz (2018). "An emergent sea ice floe size distribution in a global coupled ocean-sea ice model". In: *Journal of Geophysical Research: Oceans* 123.6, pp. 4322–4337.

Stansberg, C. T., G. Contento, S. Hong, M. Irani, S. Ishida, and R. Mercier (Jan. 2002). "The specialist committee on waves: Final report and recommendations to the 23rd ITTC". In: pp. 505–736.

Response to RC3's report

I want to thank the authors for their detailed answers. As stated in the previous review round, I am curious of how applicable this model will be to real world data in a broad range of conditions. I think that the discussion about what is mathematically vs. physically correct (not just at the scale of the laboratory or small scale experiments but also at the full scale of field realistic conditions), which we have had through this review round, is not simple and not settled out by the present work. But at the same time, I am aware that an exhaustive study comparing this model to field data would be too much work for including in this paper. Therefore, I think that the present work can be accepted as is: I believe it is strictly speaking mathematically correct given the assumptions that are made, as demonstrated by validation against idealized conditions in the laboratory, and I think it is acceptable to have it published as an attempt at modeling which mathematical correctness is validated in controlled conditions, with in-depth comparison and validation against full scale real world data planned for the future. I will follow developments in this regard with great interest, and I hope the authors can present such a work in the future, as, in my experience, the field of research about waves in ice attenuation is a good illustration that a model can be mathematically (and even physically) correct at one scale (for example the laboratory), but hard to transfer to the full scale due to changes in scaling and changes in the dominant physics. However, I am willing to recognize that this view is mine, and is likely quite heterodox, so I think that the authors should get the possibility to publish their findings and results, and get the possibility to check in the future if and how well this model applies also to full scale data.

We thank you for your understanding. We appreciate that the laboratory scale cannot necessarily be one-to-one transformed to field scale, due to numerous added complexities at the field scale (spatial heterogeneities, additional feedback processes between the ice, atmosphere and ocean, and so on). We believe an important step before addressing these complexities was to validate the model against a simple experiment, allowing to isolate specific physical processes and their model representation (here, the fracturing). However, even though it lacks formal validation, we believe for now the example of spectrally-forced, time-dependent fracture showed in the new Sect. 2.6 is a good illustration that our model can be run at a more geophysical scale. We now strive to conduct comparisons to field data.