Comments from Referee #2, followed by the authors' responses

Referee #2 - In their manuscript 'WIce-FOAM 1.0: Coupled dynamic and thermodynamic modelling…', Rutger Marquart and coauthors describe an OpenFOAM-based sea ice model designed to study the effects of sea ice type ('ice floes' versus 'grease ice'), spatial orientation and arrangement of ice floes, and wave forcing (wave length and direction) on the net (domain-averaged) viscosity of the ice. The model considers two ice types with two different, prescribed rheology models, at ice concentration of 100%. The dynamic part of the model is coupled with a thermodynamic model, so that nonlinear coupling between ice dynamics and thermodynamics can be studied.

The manuscript is very well and clearly written, the figures illustrate well the topics discussed and the results obtained. The assumptions and limitations of the model and of the simulations are clearly stated. I find the results very interesting and relevant, as they might contribute to better understanding of sea ice rheology in the marginal ice zone, and thus to better parameterizations for sea ice models. In my opinion, after some rather minor corrections, the manuscript is worth publishing in GMD.

Referee #2 - Line 90: 'As a result, interactions between ice floes are represented as continuous, churning contact of varying intensity rather than brief, forceful impacts. This justifies the exclusion of ice floe failure and fracture.'

It justifies the exclusion of failure and fracture due to floe-floe collisions, but not fracture in general, due to, e.g., ice convergence, shear deformation or bending of floes on waves. Maybe it's worth formulating more precisely?

Authors' response - We thank the referee for highlighting this important point. In the revised manuscript, we will clarify that our approach justifies the exclusion of failure and fracture due to floe-floe collision, but not fracture processes in general as pointed out by the referee.

We will revise the sentence accordingly to avoid confusion with other fracture mechanisms such as those driven by convergence, shear or wave-induced bending. The sentence in the updated manuscript will read: 'As a result, interactions between ice floes are represented as continuous, churning contact of varying intensity rather than brief, forceful impacts. This justifies the exclusion of ice floe failure and fracture due to floe-floe collisions. However, we acknowledge that other potential fracture mechanisms are also excluded, such as those driven by ice convergence, shear deformation, or wave-induced bending.'

Referee #2 - Lines 101-103: I'd consider moving the references from line 101 to 103, just before equation (3). Equation (2) is quite obvious and doesn't require references.

Authors' response - We agree with the referee. In the revised manuscript, the references have been moved from line 101 to line 103, and the references for Equation (2) have been removed.

Referee #2 - Line 138: 'apical plane'?

Authors' response - The apical plane refers to the upper surface of the ice exposed to the atmosphere (i.e., the ice–air interface). To improve clarity, we have clarified this in the text as 'upper (apical) surface'. We also recall that the basal plane refers to the lower surface of the ice, which is in contact with the ocean. We will clarify this in the revised manuscript.

Referee #2 - Lines 193-194: 'For the ice floes, the one-dimensional thermodynamic model in the z-direction, developed by Tedesco et al. (2009), is applied to OpenFOAM cells associated with ice floes to simulate thermodynamic variations in snow and ice thickness.' Does it mean that the ice floes in cells associated with grease ice do not grow thermodynamically? And vice versa, how is the growth of grease ice treated in cells classified as ice floes?

Earlier, line 87 states that 'cells are classified according to the predominant ice type - either ice floes or grease ice'. But what does it really mean? And, first of all, why is this classification necessary? Is the information on the surface area fraction covered with a given ice type not enough?

Authors' response - We apologise for the lack of clarity. Frazil/grease ice also grows thermodynamically, as shown in Figure 2b. In our model each computational cell is treated as being entirely occupied either by ice floes or by grease ice. The classification of a cell as an 'ice floe' or 'grease ice' cell is determined by the dominant ice type within that cell, based on the initial condition field derived from the SAR image. During the simulation, no transitions between ice types occur. We will modify the sentence to clarify that thermodynamics acts in both ice types.

We note that using the surface area fraction of each ice type as an alternative would not be compatible with the limitations of the Volume-of-Fluid (VoF) method employed. In the VoF framework, a scalar field α represents the ice type: $\alpha = 1$ corresponds to an ice floe, $\alpha = 0$ to grease ice, and intermediate values represent the interface, which does not have a direct physical meaning. Therefore, for both numerical stability and physical consistency, it is preferable to initialise each cell with a single ice type.

Referee #2 - Line 254: 'we can interpolate the thermodynamic model'. Is 'interpolate' the right word here? What exactly is interpolated?

Authors' response - We agree with the referee that 'interpolate' is not the right word, as it could cause confusion. In the updated manuscript, we will remove this term and state that the thermodynamic model results at the hourly timescale are updated to a time step smaller than the hourly frequency of the forcing functions, which allows for the coupling between the two models. We will add this clarification to the revised manuscript.

Referee #2 - Lines 338-339: 'The viscosity is locally affected by the propagating wave, since the rheology of the grease ice and ice floes is described as a function of thickness.' Is it really the thickness that's responsible for the observed differences in viscosity? And not the different form of the two rheology models? In other words, if the thickness of ice floes and grease ice was the same, would viscosity remain unaffected?

Authors' response - We agree with the referee that the form of the rheology is responsible for the observed differences in viscosity. If both materials had the same thickness, their viscosities would still differ because their rheologies are fundamentally different. What we aim to emphasize in lines 338-339 is that, within each rheological framework, viscosity also depends on the local ice thickness. The propagating wave modifies the ice thickness field, which in turn affects the viscosity according to the thickness-dependent terms in Eqs. (15) and (18). Hence, the local variations in viscosity shown in Fig. 6 are a combined effect of the wave-induced thickness changes and the distinct rheological behaviour of the two ice types.

We will add this clarification to the revised manuscript.

Referee #2 - Line 369: 'the intercept at 0% represents the viscosity of grease ice' – which seems to be zero in Fig. 9. I think it requires a comment.

Authors' response - We agree with the referee that this requires an additional comment, as it may appear that the viscosity at 0% ice floes is zero, which is not the case. The viscosity at 0% ice floes (100% grease ice) is approximately $440\,\mathrm{kg\,s^{-1}}$, as also indicated in the equations shown in the top-left corner of Fig. 9. This small value is not easily visible in the figure because the Y-axis is scaled in $10^8\,\mathrm{kg\,s^{-1}}$.

We will add this clarification to the revised manuscript.

Referee #2 - Lines 369-370: 'The north-south orientation describes a linear relationship (see the equation in Fig. 9).' This sentence is unclear. Orientation of what? A relationship between what?

Authors' response - This refers to the north-south orientation of the incoming wave. The relationship is between sea ice viscosity and the percentage of ice floes in the domain. We will add this clarification to the revised manuscript.

Referee #2 - Lines 376-377: 'Based on the linear relationship presented in Fig. 9, we assume that the model resolves the smaller scales of the heterogeneous field, allowing us to extract properties at larger scales.' Please explain why/how exactly the obtained relationship justifies this assumption.

Authors' response - We acknowledge that the sentence is unclear.

In the revised manuscript we will explicitly mention the strong scale invariance in this sentence as follows: 'Based on the inclusion of smaller scale processes that we assume realistic, the emergence of the linear relationship presented in Fig. 9 and the strong scale invariance of the mean viscosity of sea ice, we are confident that our results can be used to extract properties at larger scales as further discussed in Section 4.'

Referee #2 - I find the results presented in Figs. 13 and 14 quite remarkable. The contribution of the bridge connecting the two floes to the total model surface area is very minor, well below 1%, presumably closer to 0.1% (as far as I can estimate it from the plots in Fig. 13), but its influence on the area-averaged viscosity, seen in Fig. 14, is at the level of 1%. To me, this suggests that the net viscosity is indeed very sensitive to the orientation of tiny (in terms of surface area fraction) 'ice elements', presumably much more so than Fig. 9 may suggest – as, in the 'real' case, different contributions from many elements with different orientations cancel out. (All this might be related to the discussion around line 510 in the manuscript?). This would mean that in situations with strong anisotropy of the ice cover very small changes e.g. in wave propagation direction may lead to significant changes in net viscosity.

Another interesting thing in Fig. 14 is that the panel (b) is qualitatively different from the other three: In this case, the viscosity increases with wave period – as it does in the cases shown in Fig. 7, but unlike in those in Fig. 14 a,c,d. What is the explanation for this behavior? I think this fact should be commented upon.

Authors' response - We are pleased that the referee appreciated the test cases and their results, which indeed required a large amount of work and interpretation. We fully agree with the referee on the role of geometry, which contributes to the novelty of the present manuscript. One of the key outcomes of this study is the influence of the orientation of narrow connections relative to the incoming wave. The domain-averaged sea ice viscosity is very sensitive to these narrow connections, even though they represent only a small fraction of the surface area. Demonstrating this effect was precisely the purpose of the test cases.

The main difference between Fig. 14(a) and Fig. 14(b), and the resulting order of the curves, arises from the orientation of the bridge with respect to the incoming wave. When the bridge is perpendicular to the wave (Fig. 14(a)), both floes (including the bridge) behave more as a rigid body, making the system less sensitive to the wave period. In contrast, when the bridge is parallel to the wave direction (Fig. 14(b)), the floes can move more independently, creating a higher strain rate in the bridge and, consequently, a lower viscosity. Clearly, when the angle is changed to 45° (Fig. 14(c) and (d)), the floes and the bridge again move more as a rigid body rather than as two independent bodies, with the outcome being more similar to Fig. 14(a).

We will add this comment to the revised manuscript.

Referee #2 - Lines 486-488: 'We also observed changes in the magnitude and oscillatory behaviour of the mean shear viscosity that are indicative of resonance at smaller scales that can be propagated to the kilometre scales.' Is there a possibility that the oscillations are related to the model setup (e.g., the periodic domain and the fact that the wavelength is adjusted to the domain size) and/or numerics (numerical schemes used etc.)?

Authors' response - We observe oscillations in most of the sea ice viscosity curves, which appear to be more pronounced at smaller wave periods (see Figs. A1 and A2). We would exclude issues with numerical convergence and stability of the scheme since these oscillations are independent on the time step. Our comment on the resonance was made to include a possible influence of the domain periodicity and wave lengths, and we will be more explicit about it in the revision. However, no oscillations are observed when the bridge is oriented perpendicular to the wave direction (Fig. 14(a)), suggesting that the orientation of the narrow connections also plays an important role in the resonance. We will add this consideration in the revised version.

Referee #2 - Line 497: 'in t = 24h'. Meaning after 24h?

Authors' response - We agree with the referee. In the revised manuscript, we will update the text to 'at $t = 24\,\mathrm{h}$'.