

General comments

This manuscript presents a minimal bonded discrete element model for simulating sea-ice fracture and flow, with an emphasis on reducing particle-scale complexity and computational expense while retaining important emergent large-scale behavior. The authors use simple-shear experiments to establish relationships between particle-scale model parameters and bulk material properties, and subsequently apply the model to an idealized Nares Strait geometry to examine ice-arch formation and collapse. A particularly interesting aspect of the work is the demonstration that several distinct arching regimes can emerge from a relatively simple bonded-particle framework. The ability to systematically explore a large parameter space with a computationally inexpensive model is also a strength of the study.

I found the manuscript interesting and potentially useful for process-based studies of sea-ice dynamics. The authors are also appropriately transparent about many of the limitations associated with the deliberately simplified model physics. However, I have several concerns regarding the interpretation of the emergent bulk properties and the calculation and interpretation of stress. In particular, I think the manuscript needs to more clearly distinguish resolution-dependent properties of the discrete bond network from physical material-size effects, clarify the stress tensor formulation used for both calibration and analysis of the arch simulations, and more carefully support the interpretation of arch collapse as emergent compressive failure. I also think several aspects of the numerical setup, including the shear-periodic boundary treatment and resolution dependence of fracture geometry, need additional clarification. There are also several inconsistencies in equations, parameter definitions, units, and notation that currently make portions of the analysis difficult to follow.

I believe these issues are addressable, but because several affect the interpretation of the model calibration and the primary conclusions regarding the narrow-channel experiments, I recommend major revision.

Specific comments

1. Introduction and positioning relative to existing sea-ice models

In the first paragraph, the statement that continuum models cannot resolve lead-scale dynamics appears too broad. Some modern continuum sea-ice models, for example neXtSIM, are specifically designed to represent highly localized deformation and can resolve lead formation at sufficiently high spatial resolution. I suggest clarifying which particular processes or limitations the authors have in mind when contrasting continuum and discrete approaches, rather than implying that continuum formulations categorically cannot represent these dynamics.

In the second paragraph, the characterization of the SubZero model in the discussion of Manucharyan and Montemuro should also be revised. SubZero is a discrete-floe model in which individual floes are represented by discrete polygons that can evolve in shape; the floes are not held together by breakable bonds. This distinction is important here because the manuscript is specifically motivating a bonded DEM formulation.

2. Particle number, bulk mechanical properties, and interpretation of the statistical size effect

Section 3.1.1 interprets the decrease in macroscopic strength with increasing dimensionless system size $\Lambda = L/r_L$ as a statistical size effect analogous to Weibull failure. The physical domain remains fixed at 100 x 100 km while particle radius is changed from 2000 m to 111 m, resulting in systems ranging from 873 to 283,279 particles. The authors argue that increasing the number of particles introduces more possible locations for stress concentrations and therefore reduces the macroscopic strength. The manuscript

appropriately notes that, unlike classical Weibull theory, the disorder here arises from bond topology rather than random variations in individual bond strength.

However, changing particle size within a fixed physical domain also changes the underlying mechanical network. The number of bonds along a given physical distance, the number of parallel load paths, bond lengths, coordination and topology, and the geometry and number of possible force chains all change with particle number. In a simplified mechanical analogy, changing the resolution changes the number and arrangement of spring-like elements acting both in series and in parallel. Thus, even though the microscopic bond parameters remain fixed, the effective response of the aggregate need not necessarily remain unchanged.

I appreciate that Fig. 3b shows that the pre-failure shear modulus is approximately invariant with Λ , which provides useful evidence that the simplest form of this concern, namely that the material simply becomes more or less compliant as resolution changes, is not occurring. However, I am not convinced that invariance of the linear elastic modulus alone establishes that the reduction in peak strength is a Weibull-like statistical size effect rather than a consequence of resolution-dependent load sharing and failure within the discrete bond network. In fact, the authors themselves state that the effect originates from bond topology rather than variations in bond strength.

I think this interpretation could be strengthened with either additional analysis or a relatively simple mechanical test. For example, the authors could use a fixed physical domain with **one boundary held stationary and apply the same uniform wind stress across the ice toward that fixed boundary**, while varying particle number but keeping k_b , ϵ_c , packing fraction, and the other microscopic parameters constant. The fixed boundary would prevent rigid translation of the ice cover and allow internal stresses to develop through the bonded network. Comparing the resulting bulk stress-strain response across resolutions, both below failure and through the onset of fracture, would help distinguish a change in the effective mechanical response from a statistical failure-size effect. If the pre-failure response remains approximately resolution independent while the failure stress varies systematically with particle number, this would provide much stronger support for the interpretation advanced in Section 3.1.1. Conversely, if the bulk response begins to differ prior to failure, this would suggest that changing particle number is also changing the effective load sharing and mechanical behavior of the discrete bond network. At minimum, I suggest discussing this distinction more carefully and describing the fitted Weibull relationship as an empirical resolution-dependent scaling unless its connection to a physical statistical size effect can be established more directly.

A related question is whether changing particle size also changes the geometry of macroscopic failure. Because fracture occurs through the discrete bond network, changing resolution changes not only the number of potential failure sites but also the set of available fracture pathways. Do the resulting macroscopic fracture angles and crack geometries converge as particle size decreases, or are they sensitive to resolution and bond topology? Some characterization of fracture orientation across the different values of Λ would help demonstrate that the observed fracture patterns are not primarily discretization artifacts.

3. Simple-shear methodology and stress calculation

The simple-shear experiments provide the basis for much of the subsequent model calibration, so I think a few aspects of this methodology require additional clarification.

First, I am having difficulty reconciling the stress expressions in Eqs. (7) and (9) with the quantities subsequently calculated and plotted. Equation (7) is introduced as the macroscopic shear stress, but as written the right-hand side appears to contain an interaction force multiplied by the scalar Euclidean distance between particle centers. Equation (9) similarly defines a tensor σ_{ij}^p , but the displayed right-hand side again appears to involve the interaction force multiplied by the scalar separation distance. It is therefore unclear how the individual σ_{xx} , σ_{yy} , and σ_{xy} components subsequently used in Eq. (10) are obtained from Eq. (9).

If these equations are intended as shorthand for the full force-branch-vector/virial stress expression, I suggest writing the tensor expression explicitly and then showing how the scalar shear stress in Eq. (7) is obtained from the appropriate component. This is important because these stresses underpin the calculated effective shear modulus, peak shear strength, principal stresses, and ultimately the interpretation of the arch simulations.

I also think the boundary conditions used in these calibration experiments should be described more completely. Section 2.2.1 describes the simple-shear domain as doubly periodic. Under simple shear, however, the upper and lower boundaries have a relative velocity, so conventional periodic boundary conditions would not appear to be directly compatible with the imposed shear. Is the model using a shear-periodic/Lees-Edwards-type boundary condition, a deforming periodic simulation cell, or some other remapping procedure? It would also be useful to clarify how bonds that span the periodic boundary are handled as the periodic images become displaced under shear. Since these experiments provide the calibration for the effective shear modulus and strength used throughout the remainder of the manuscript, both the stress calculation and the implementation of the shear boundary conditions should be described sufficiently for the calibration to be reproduced.

Finally, there is an index ambiguity in Eq. (9), where i appears to denote the neighboring particle in the summation while also appearing as one of the tensor indices in σ_{ij}^p . I suggest using separate indices for particle neighbors and coordinate components.

4. Interpretation of system-scale compressive failure

One of the more interesting conclusions of the manuscript is that the model can exhibit macroscopic compressive failure even though bonds cannot fail in compression. The authors interpret arch formation as tensile failure because σ_1 decreases during the first fracture event, and arch collapse as compressive failure because the magnitude of σ_2 decreases during collapse. This emergent behavior is subsequently emphasized as an important capability of the minimal model.

I think the distinction between failure occurring while the system is under compression and failure caused by compressive loading needs to be made more carefully. A decrease in the magnitude of compressive stress during a fracture event demonstrates stress release, but it does not necessarily establish that compression itself caused the bonds to fail. This is particularly important because individual bonds can only fail according to the tensile-strain criterion.

Could the authors examine the local stress or bond-strain state immediately prior to bond failure during arch collapse? For example, it would be useful to know whether bonds are actually failing locally in tension as the larger-scale arch is under compression, perhaps as a consequence of stress redistribution or geometric instability. Such an analysis could make the emergent system-scale failure mechanism substantially clearer.

A principal-stress-space representation might also be informative here. For example, plotting trajectories through (σ_1, σ_2) space for the no-arch, unstable-arch, stable-arch, and no-fracture simulations could potentially show whether the different regimes occupy distinct portions of stress space or cross a recognizable system-scale stability boundary. I would hesitate to call such a boundary a constitutive failure envelope given the simplicity of the particle-scale rheology, but an arch-stability/regime diagram in principal-stress space could provide a useful physical interpretation of Fig. 8.

5. Dependence of the channel regimes on $k_b \epsilon_c$

The collapse of the 200 narrow-channel simulations onto $k_b \epsilon_c$ is presented as one of the principal findings of the paper. The manuscript interprets this product as a proxy for bond/ice strength and reports progression from no arch to unstable arch, stable arch, and no fracturing as $k_b \epsilon_c$ increases.

I think this result is interesting, but the interpretation could be explored more carefully. Section 3.1 already establishes that macroscopic shear strength scales with approximately $k_b \epsilon_c / h$. Thus, finding that the regime boundaries approximately follow the product $k_b \epsilon_c$ is perhaps not entirely independent of the preceding calibration.

More importantly, k_b and ϵ_c do not play identical mechanical roles. Changing k_b changes the elastic stiffness, whereas changing ϵ_c changes the strain at failure. Therefore, two systems with identical values of $k_b \epsilon_c$ may have identical nominal bond strengths but substantially different stiffnesses and failure strains.

Figure 8e provides good evidence that $k_b \epsilon_c$ predicts the categorical arch regime across different combinations of k_b and ϵ_c . However, because k_b and ϵ_c independently control elastic stiffness and failure strain, respectively, I think it would be useful to clarify how far this collapse extends beyond regime classification. For simulations with similar $k_b \epsilon_c$ but substantially different individual values of k_b and ϵ_c , **are quantities such as arch-formation/collapse timing, stress evolution, or fracture morphology also similar?** For the unstable-arch cases in particular, the shading in Fig. 8e already represents the time between arch formation and collapse. **A direct plot of $k_b \epsilon_c$ versus time to collapse could therefore provide a useful quantitative test of whether the product also controls the dynamics within this regime.** If a clear relationship emerges, this would strengthen the interpretation of $k_b \epsilon_c$ as a broader system-scale control parameter. Conversely, substantial scatter, particularly if associated systematically with different combinations of k_b and ϵ_c , would suggest that the individual elastic and failure properties remain important even when their product is similar.

6. Calibration equations and resolution correction

There appears to be an inconsistency between Eq. (8) and the subsequent resolution-correction expression. Equation (8) gives $\tau_{\max} = 0.35 k_b \epsilon_c / h$, while the expression on the following page for adjusting ϵ_c^* uses $0.25 k_b$ in the denominator. If the latter expression is derived by combining the Weibull scaling with Eq. (8), I would expect the coefficient to remain 0.35. This should be checked because the resolution-correction relationship is presented as practical guidance for configuring the model at different particle sizes.

7. Environmental force balance in Eq. (4)

In Eq. (4), is the environmental force term intentionally formulated without a Coriolis contribution? If so, I suggest stating this explicitly and briefly explaining why Coriolis acceleration can be neglected for the experiments considered here, particularly given the intended application to sea-ice dynamics. The p, i notation in this equation should also be formatted consistently as a subscript.

Technical corrections

- **Variable definitions:** Section 2.1.2 is one example where variables are introduced in an equation and defined some time later. This occurs in several places. Please define variables immediately after the equation in which they first appear.
- **DEM abbreviation:** Introduction, Paragraph 4 spells out “discrete element model” after DEM has already been defined. Please check for other instances and use the abbreviation consistently after first definition.
- **Disks versus spheres:** Section 3.1 refers to “spheres,” but the model appears to use a two-dimensional representation. Should this be “disks” instead?
- **Figure 7b:** Section 3.2.2 refers to a “red line” in Fig. 7b, but it is not clear which feature is being referenced. Please identify the line more clearly and verify the figure/text correspondence.
- **Dimensionless system size Λ :** Section 3.1.1 defines $\Lambda = L/r_L$, explores values from approximately 50 to 900, and selects $\Lambda_0 = 300$. Table 1 instead lists $\Lambda_0 = 0.0033$, which appears to be $1/300$. Appendix C subsequently reports $\Lambda = 0.033$ for $r_L = 400$ m. These definitions/values should be reconciled throughout.
- **Bond and contact stiffness units:** k_b and k_c are introduced as spring stiffnesses with units N m^{-1} , consistent with Eqs. (1) and (3). However, Section 3.2.3 gives the k_b sampling range in Pa, and Section 4.4 gives k_c in Pa. These should presumably be N m^{-1} .
- **Damping coefficients in Table 1:** The table gives $\Gamma_b = \sqrt{m_s / k_b}$ and $\Gamma_c = \sqrt{m_s / k_c}$, whereas Sections 2.1.2 and 2.1.3 define them as $\Gamma_b = \sqrt{m_s k_b}$ and $\Gamma_c = \sqrt{m_s k_c}$. The table also gives the damping coefficients as dimensionless, although the force laws require dimensional damping coefficients. Please check both the expressions and units.
- **Timestep in Appendix C:** Section 2.1.4 and Table 1 give $\Delta t = 0.1 \sqrt{m_s / k_b}$. Appendix C instead gives $\Delta t = 0.1 \sqrt{k_b / m_s}$, which appears inverted and is dimensionally inconsistent with a timestep.
- **Particle separation notation:** Eqs. (1)-(3) use r for the distance between particle centroids, whereas Eqs. (7) and (9) use $\|x_p - x_i\|_2$. Consider explicitly stating $r = \|x_p - x_i\|_2$ when the latter notation is introduced and maintaining consistent notation where possible.
- **Velocity notation:** The manuscript uses u for particle/ice velocity in the momentum and drag equations, while the arch-classification discussion and associated quantities use v , e.g., $|v_y^{\text{up}}|$. Please clarify whether these represent the same velocity or distinct quantities and make the equations, text, and figure notation consistent.
- **Critical-strain range:** Section 3.1.1 states that ϵ_c is linearly sampled over $[5 \times 10^{-6}, 5 \times 10^{-8}]$. Please check the ordering of the bounds and verify the exponents.
- **Section 3.2.3:** The text states that the “symmetry about the line $k_b = \epsilon_c$ ” indicates dependence on the product $k_b \epsilon_c$. Since k_b and ϵ_c have different dimensions, an equality $k_b = \epsilon_c$ is not physically meaningful unless these are normalized coordinates. Consider referring instead to the slope/orientation of the regime boundaries in logarithmic parameter space, or explicitly defining normalized variables.
- **Section 3.2.4:** The phrase “where where compressive failure is possible” contains a repeated “where.”
- **Section 4.5:** “simulations with a higher contact stiffnesses” should be “simulations with higher contact stiffness” or “simulations with higher contact stiffnesses.”
- **References:** Several DOI entries appear to contain duplicated prefixes such as “https://doi.org/https://doi.org/”, and at least one uses “https://doi.org/doi:”. The reference list would benefit from a systematic DOI-formatting check.