Reviewer 2

This paper is a useful contribution because the authors have demonstrated the multifractal behavior the VP and a VP model modified to include damage can have (or in fact not have). While the model does not match observations there is discussion of how the model behaves that could help future investigators improve models of sea ice deformation. What is lacking is often a physical interpretation of the model behavior. While relationships are described mathematically, such as the differences in scaling exponents or departure from expected multifractal behavior, no context as to why this might be is given. Perhaps this is not possible to determine, however it would be useful insight.

Overall I feel this contribution is valuable and should be published. Though I would caution readers to consider if multifractals are the best metric to validate simulated deformation or distinguish models. There are places where the authors can strenghten their argument as to why it is important to reproduce observed scaling behavior for sea ice deformation or linear kinematic features. The contribution of this paper in describing the model behavior with the mathematical language of multifractals is helpful in the conversation.

General comments

The deformation of sea ice is shown to be coupled in space and time, such that the scaling relationship in each varies depending on the sampling in the other. This means that when comparing a model with observations you have to account for this time or space sampling difference. How do you ensure that you are comparing scaling relationships for the same time or space sampling in the model and observations?

These are important considerations for sea ice but are treated in great detail in other works. We include a brief discussion on this topic after line 30: "As discussed in Weiss, (2013), the scaling properties of sea ice, contrary to those of turbulence in fluid, have been shown to be coupled in space and time such that the value of one is influenced by the sampling resolution of the other, and also by its spacetime localization." For the second point, we direct the reviewer to section 4.3 for the method that we use to compute the scaling statistics. In this, we clearly state how we compute the scaling, for $\beta(T = 3 \text{ days})$ and $\alpha(L = 10 \text{ km})$, as stated in line 269 and 279 respectively.

It is a useful comment that even ad-hoc parameterization of heterogeneity in ice strength could improve representation of sea ice deformation. This jives with my personal experience where if I solve the VP model to full plastic equilibrium it is not possible to simulate LKFs unless you seed variability in ice strength randomly. Which is about as simple a parameterization as one can make! In the introduction a damage parameterization is introduced, and in the discussion this is compared to other parameterizations used in other models. It might help the reader to give a little information about how the parameterizations differ up front and why you choose to develop your own.

A VP model can simulate LKFs without seeding variability in ice strength (keeping the ice thickness and concentration constant). See for instance Ringeisen et al. 2019) who simulate conjugate pairs of LKFs with a fracture angle that is in accord with theory. When seeding "defects" in the ice, they show other LKFs linking defects together with a fracture angle that departs from theory. There appears to be something wrong in the way the reviewer has run their model. Girard et al (2011) were also not able to simulate spatial scaling with their VP model, in contrast with results from all other groups currently running a VP model (Bouchat et al. 2023). Our VP model is publicly available on github https://github.com/McGill-sea-ice/SIM, in case it can be useful to check on the reproducibility of results in the future.

We now introduce at line 162 the term as "plastic damage (hereafter referred to as damage for simplicity)", followed by the sentence: "In the context of sea ice and plasticity theory, plastic damage (hereafter referred to as damage for simplicity) can be accomplished by including strain weakening in the model that is independent of subsequent divergence and reduction of ice thickness and/or concentration (see for example Lubliner et al., 1989 for a simple model of plastic degradation). In recent years, more complex models have been developed that include (elastic-)plastic damage, notably in concrete (see for instance Luccioni et al., 1995; Jason et al., 2006; Voyiadjis and Taqieddin, 2008; Parisio and Laloui, 2017; Hafezolghorani et al., 2017; Friedlein et al., 2023; etc.) in which plasticity is taken into account in the damage variables, and the yield curve changes accordingly. Since elasticity is not taken into account in the VP model, we use a parametrization of damage that changes the yield curve of sea ice depending on its damage level extracted from its viscosity rather than its elasticity. Therefore, the damage that we present here, while intuitively based on previous literature, remains a parametrization of a more complex mechanism."

Specific comments

Abstract line 8: Grammatical error "an"

Corrected as suggested by the reviewer.

Line 14: Is "ilks" a good word to use here? This is a stylistic comment that you can take or leave: I found the first paragraph of the introduction did not really guide me to what the content of the paper would be about. In general I would suggest the introduction could be more focused.

We replaced "ilks of ice" with "countless variations of ice and snow". It is correct, the paragraph does not guide the reader to the content of the paper. The current uniform/direct/bare-bone scientific writing style was introduced in the 70s for the sake of uniformity between journals and discipline, but in doing so, we have lost some of the authors' perspectives and states of mind (read, for instance, earlier AIDJEX literature which was much more personal). We believe that something was lost in this transition. The editor has raised a similar issue. We decided to keep the paragraph, but we keep the suggestion in mind for the future.

Line 24: Typical floe sizes range from meters to 10s of kilometers. So saying floe size is 10km is factually incorrect. It is correct to mention that the data you are working with has this lower resolution, but incorrect to call it floe size.

This is correct. The sentence was changed from "ranging from floe size (10 km) to the size of the Arctic Basin" to "ranging from the smallest floes (meters) to the size of the Arctic Basin".

Line 30: "complex laws" This is overly general. I encourage you to be more specific. This is also where you could point out that unlike turbulence in water or air, the scaling relationships for sea ice deformation are coupled in space and time. There is also large differences in scaling exponents found for different years and seasons, so it would be good to comment on if you accounting for this or just using values found for a particular time period or region.

See the first paragraph of the general comments, we added a small discussion on this topic after line 30.

Line 34: Why should a model of Arctic sea ice simulate LKFs?

The reasons why a proper simulation of LKFs are important are stated in the previous paragraph. In this paragraph, we are coming back to the same idea and elaborate on the exact reasons. We now state all the reasons why a proper LKF simulation is important in the same paragraph when it is first mentioned.

Line 51: Missing full stop.

The reviewer is referring to: "Models that incorporate some of these recommendations include the Elasto-Brittle and modification thereof (EB, MEB, and BBM) [references], in which elastic deformations are followed by brittle failure, while larger deformations along fault lines following damage build-up are viscous." There is a period at the end of the sentence, please clarify.

Line 163: Sea ice can diverge and weaken (through reduced area) in the VP model even when LKFs are not present.

This is correct. We clarified that we are referring to weakening along LKFs. The new sentence reads: "weakens along an LKF only when sea ice divergence is present".

Line 205–210: I am curious, when you are creating a run with 10 random years how do you ensure there are not unphysical jumps in the wind forcing between years? Does this matter, given the spin up of ice drift is relatively fast compared to the wind speed change.

There are discontinuities at the end of each year. But the response time of sea ice velocities and thickness to changes in forcing is fast and the simulated sea ice deformation is not sensitive to exact sea ice thickness conditions (see for instance Bouchat and Tremblay, 2017 who simulated sea ice deformation for different years with different ice thickness distribution). The advantage of using random years is to avoid spinning up the model for a series of 10 years in a given phase of, say, the Arctic Oscillation or other low-frequency variability which would lead to a larger difference in the initial ice thickness field. This was clarified on line 238 of the revised manuscript.

Line 316: "soup-like" is a weird choice of word here. Also is there a missing label by the "2".

Corrected as suggested by the reviewer. We now use Figure 2d.

Line 320: "simulation" $\Longrightarrow$ "simulations"

We are referring here to only one simulation with no damage and increased shear strength ($e = 0.7$). The sentence was changed to: "as in the simulation without damage with $e = 0.7$".

Line 331: I believe the decay is log-linear not linear.

Correct. we are referring to a log-linear decay. This is clarified: "This shift results in a log-linear decay in the tail of the PDFs [...]"

Line 378: "morally" is a weird word choice here.

We agree. We replaced "being morally the same as that of RGPS" with "being the highest (0.14) and closest to that of RGPS".

Figure 3: It is not clear where the observations in the black line are from. Where do you describe the observations in the paper and how you calculate the deformation. Are the distributions for model and observations from the same time and region? It is only apparent later in the paper that you are only considering data from January 2002 (line 538). Which makes me think you need to improve details in your methodology.

Thanks for pointing this out. We now state in the Methods (section 4) that the simulated sea ice deformations are validated against RGPS data for January 2002 (following Bouchat and Tremblay 2017). This was clarified on line 230 of the revised manuscript:

"We calculate all metrics using simulated 3-day average sea ice velocities inside the SAR sea ice RGPS data in the region where an 80% temporal data coverage is present for the winters 1997–2008 [...]. The sea ice deformations are compared with observed sea ice deformations derived from RGPS three-day average ice velocities for January 2002. The results presented are robust to the exact choice of year.."

Data: I could not check code and data availably because links were not provided.

The code and data will be included when the paper is accepted for publication.