

Reviewer #1 (REMARKS to AUTHOR(s)):

The author proposed a 2D kinetic Ising model (IM) to simulate Arctic sea ice dynamics on a large length scale. The IM is an idealized model in statistical mechanics, and the author modified this model by introducing continuous spin values between -1 and 1 to describe the mixture between ice and water. The classical Metropolis-Hastings algorithm is used for time evolution, and the author modified this algorithm by introducing a factor to account for the inertia of spin value changes. The parameters of the IM are estimated based on observations, and many observed features are well captured by the IM.

This paper applies statistical physics to an important problem in climate science, so it should be of great interest to both communities. Therefore, I can recommend the publication of this paper after the following minor issues are addressed.

Abstract:

1. The length scale being modeled in this paper should be clearly stated. Arctic sea ice has multiple length scales, and it should be stressed that this paper focuses on a large scale.

EW: Revised accordingly

2. It should be clarified that the continuous spin values are between -1 and 1. By default, a continuous variable can take values between -infinity and infinity, which is unphysical.

EW: Revised accordingly

Section V.A:

1. In Table 1, the last row should be I instead of J. The fluctuations of the parameters I, J and B<sub>0</sub> are quite regular and well explained, but the fluctuations of the parameters B<sub>x</sub> and B<sub>y</sub> are much wilder and not explained. If unexplained, these fluctuations may imply overfitting. A related question is whether the estimated parameters are consistent across multiple runs of the dual annealing optimization method, especially for B<sub>x</sub> and B<sub>y</sub>. If not, then the probability density functions of the estimated parameters should be provided.

EW: Corrected table 1 last row to I.

2. It would be useful to discuss the critical temperature for this version of the IM and whether the estimated IM temperature is below or above this critical temperature. This will provide valuable insights into the role of environmental noise on the time evolution of Arctic sea ice.

EW: You are absolutely right that critical IM temperature will provide valuable insights on environmental noise to the sea ice evolution. However, critical IM temperature is out of scope for this specific research which focuses on the kinetic evolution only. In fact, critical temperature of a 2D Ising lattice with continuous spin value will be a very interesting problem itself.

Reviewer #2:

### General Comments

This paper deals with the Ising model with continuous-valued spin configuration for modeling the Arctic sea-ice concentration field. A Monte-Carlo-based optimization technique is used to find optimal parameters to match the final concentration data. The objective function employs an 'inertia' factor, punishing drastic changes between subsequent configurations in the dynamics.

The paper demonstrates the applicability of the Ising model to sea-ice dynamics, which is interesting and promising. Once fitted against the initial and final configuration, the model shows excellent agreement between the simulation and actual data for the intermediate time steps. While the paper is generally well written, the tone is often expository and will benefit from making some statements more technical to appeal to the target audience. Some statements are missing relevant citations.

Overall, this is an interesting paper and deserves publication in Journal of Applied Physics after quite a bit of revision and attending to the points below. The idea to use a continuous spin Ising model to describe the values of the sea ice concentration field which takes values between 0 and 1, rather than a binary model, is novel and powerful. The introduction needs to be augmented to better describe up front what is assumed, what is fit, and so on, in other words, what are the inputs and outputs of the model. What would happen if you started with known initial conditions from sea ice data, and then explored different scenarios of evolution for different parameter regimes, regardless of what the actual evolution looked like?

### Specific Comments

1. The first sentence of the paper, "The rapid loss of the Arctic sea ice over the past four decades has been an alarming phenomenon that points to drastic global warming, a serious challenge that calls for collective actions by the entire humankind," and much of the Introduction, is inappropriate for a publication in a professional physics journal.

EW: revised the first sentence

2. On page 3, "... the feedback loop effect may occur, i.e., less reflection and more absorption of solar energy, leading to even more ice loss and further global warming." It should be stated that this is usually called "ice-albedo feedback."

EW: revised as suggestion

3. Eq 1: It is unclear why the interaction  $J_{ij}$  cannot be negative (i.e., antiferromagnetic)? Is it enforced in the method, or does it just come out of the optimization process?

EW: You are right that  $J_{ij}$  is negative for antiferromagnet materials. For the research in this paper, it is natural that the area surrounding ice will be more likely to freeze, and area surrounding water will tend to melt. So  $J_{ij}$  is expected to be positive with intuitive understanding. In this paper, this is the

result coming out of the optimization process instead of being enforced, matching our expectation. We revised the paper accordingly on page 3 and also for explanations of table 1 on page 9.

4. Allowing  $\sigma_i = 0$  in the model introduces the possibility of ‘noninteraction’ (coinciding with  $J_{ij} = 0$ ) with any lattice site with 50% ice. Although it is numerically unlikely for any lattice site to achieve zero value exactly, theoretically, it is not ruled out. This poses a theoretical challenge which should be addressed.

EW:  $\sigma_i = 0$  is possible even though numerically extremely unlikely. This is a challenge for the original binary Ising model, however, in the context of this paper, it has a natural physical explanation, i.e. the cell comprises exactly 50% water and 50% ice. The impacts from water and ice to an adjacent cell offset each other, so the net effect is neutral. So this  $\sigma_i = 0$  case is not a problem for the study in this paper.

5. P4 l10: Is the energy same as the Hamiltonian  $H$  in Eq 1? Or is it the argument of the exponential in Eq (5)? Please clarify.

EW:  $H$  in Eq (5) is the same as Eq(1) in section II.A. The total energy required for a spin flip is  $\Delta E = H_v - H_\mu + I|\sigma'_i - \sigma_i|$ , which consists of two parts: the system Hamiltonian change plus the inertia effect. We added more clarification in the paper now.

6. P6: The methodology will benefit from a little more explanation. It is not clear in what order the simulation and the optimization are carried out. I propose adding a subsection to the beginning of Section IV to outline the methodology and how Subsections IV.A - IV.E are tied together.

EW: Thanks for the advice. Added a paragraph in section IV to outline the methodology.

7. Error analysis: An error plot showing the simulated and actual data would help demonstrate the results better. Section IV.E mentions l1-based optimization, but how small is the tolerance? The author provides the comparison of some useful statistics such as extent and coverage. In addition to the visual similarities (in the ‘eyeball norm’) described in the figures 3, 4, 6, and 7, a pointwise (or lp) error plot would be useful to demonstrate the difference.

8. Extrapolation: Can the author comment on the extrapolation abilities of the model? For example, once the best-fit parameters are obtained for 2022, to what extent can it be recycled for 2023 or 2021?

9. A methodological concern: the paper could use a comparison with a simpler model (say, “vanilla Ising”) in order to make the present results compelling. i.e., given a four parameter simplification of this model, are the fits just as good, or only slightly worse? If so, then the conclusion isn’t that the inertia term is needed– it’s that the standard Ising model (as-is) can already describe seasonal variation in sea ice extent. That would also be an interesting result, but it would be fundamentally different from the central claim of this paper, which is that the inertia term is important. It may well be, but it’s not yet compellingly argued. Comparison is necessary to show improvement.

10. "Use of language" concerns: quite a few instances of opinion and emotional language in the text.
11. Lack of citations for "obvious" claims.
12. The form of the inertia term is a little unmotivated– why this and not something else?
13. Its acknowledged in the paper that a different repetition number could have led to different parameter fits; this is potentially problematic, as it introduces an "almost" degree of freedom.
14. Issues of notational clarity in a few places; see below, but especially the  $\sigma' i$  (which is actually misleading and not explained until much later in the text) and the use of the word "energy." It also looks like more clarity is needed with regard to the normalization of the probability density. It may be normalized within the code, but as stated in the manuscript, it's pretty clearly wrong.
15. A few typos/grammatical issues; see below.
16. "The fact that the year 2023 has witnessed the most sizzling summer on record and the hottest year in history adds even greater severity to such urgency." Facts should be cited, and "sizzling summer" feels too informal here– great for an essay, less great for technical writing.
17. Ising model or Ising Model? Within the text I think it should be Ising model. Capitalization is a little inconsistent throughout the first couple of paragraphs before changing to just "IM."
18. "Onsager identified that there exists a critical temperature  $T_c = 2.27 \text{ J/kB}$  when the phase transition happens in a 2-D IM. Later studies of IM in higher dimensions have been closely associated with various developments in advanced 20th-century physics and mathematical theories, including the transfer matrix method, quantum field theory, mean-field theory, etc." I assume that the 2.27 J/kB figure comes from either Figure 6 or Figure 7 of the Onsager paper cited, but I don't see the number stated anywhere; is this the correct paper reference for this claim? It would also seem proper to cite the applications in transfer matrix method, etc.
19. "a major climate change indicator that is of significant environmental, economic, and social significance" Needs citations.
20. "Moreover, the Arctic ecosystem is directly impacted by the change in sea ice coverage, which, for instance, threatens the lives of polar bears and walrus who rely on sea ice for hunting and breeding." Citations needed.
21. "Jij is usually positive, meaning that adjacent spins are inclined to maintain the same value to achieve low energy." ...What does "usually" mean here?
22. "The probability of each spin flip depends on whether such a flip increases or reduces energy" Energy has never been defined here; she's almost certainly referring to the Hamiltonian, but it would be nice for that to be made explicit
23. "mathematically the probability is determined by  $\exp(-\beta (H_\nu - H_\mu))$ , where  $H_\nu$  and  $H_\mu$  represent the Hamiltonian of the system before and after the flip. It can be easily seen that a higher IM temperature leads to more thermal fluctuations and greater randomness in the spin value distribution, while a lower IM temperature shows less fluctuations." I buy what she says about IM temperature T and its influence on the variance... but are we sure that the expression she cites as

the probability density on configuration space really sums to unity? It seems like either there are a lot of constraints going on that aren't stated, or there's a missing prefactor. Of course, her claim about the variance is insensitive to a missing prefactor. However, "greater randomness" is essentially meaningless. I think she means "greater variance" .

24. "less" should probably be "fewer"

25. "Most studies of the IM focus on binary values of the spins" , this should be cited; feels impossible to know without a very comprehensive literature review

26. I have the same concern as above re: the normalization of Equation (5).

27. Re: the inertia factor  $I$ , I would like to better understand why it's included this way– i.e., why does it live in the argument of the exponential? Why is this assumption made, and not some other?

28. "In summary, the novelty of our IM is twofold: we introduce to the classical IM the continuous spin values and an inertia factor." This is a bit misleading; continuous spin values were already considered, the novelty of this IM is in the inertia factor. References [31]-[33] describe continuous spin values in the IM.

29. "not excessively too long" There's some redundancy here.

30. " $\beta$ , the inverse Boltzmann temperature, is set to 1 without loss of generality." It appears that something is then not being considered carefully with the normalization of the probability density.

31. "Generate another random variable  $\sigma' i$  between -1 and +1" So  $\sigma' i$  is the "attempted change" ; this should be mentioned much earlier. Also, what is the distribution used to generate the  $\sigma' i$  ? The distribution used to generate these will influence the results by skewing which states are accepted first by the algorithm.

32. "In this case, another random variable  $r$  between 0 and 1 is generated" ...I think she means uniformly random here; it would make sense given what she's trying to have the algorithm do.

33. "This specific repetition number is an intuitive pick," You can't really just pick things out of intuition - more needs to be said and justified.

34. ... "the fitted parameter values [...] might vary with different repetition numbers." Yes, exactly! This feels like a significant issue that should be addressed; is it true that you could get nice-looking results just by tweaking the repetition numbers? If so, there's a hidden tuning parameter here.

35. "Euclide" → Euclidean.

36. "Finally, we fit the values of parameters [...] to maximize of the similarity measure" It seems like it should be shown what happens when  $I$  isn't included at all, in order to establish that this really is an improvement; on the other hand, if you include an extra parameter, of course you can fit things better, so it really needs to be a significant improvement in goodness of fit in order to justify the inclusion of  $I$ ; exactly what constitutes "significant improvement" can be made quantitative.

37. "succeeding" → "successive" is probably better.

38. “J” in the last row of the table should be “I” , I think.

39. Figure 4 is very impressive! It might be better to have Figure 3 and 4 closer together (same page?) though; it’s a little tedious flipping back and forth to get to see the main “wow” result from this paper.

40. re: Figure 5, it would still be nice to see how well “vanilla Ising” does here, without the extra parameter– it isn’t necessarily surprising that having an extra parameter makes fits better.

41. General question: it looks like at least J and I are pretty similar between Table I and Table II! How different are the other parameters, quantitatively? If they’re all very consistent, this would be a nice story.

42. Figure 12, same concern; does vanilla Ising do this just as well, or do we need the inertia term?

43. “Many scientists are concerned that the effect of Arctic sea ice decline on global warming will intensify as the sea ice loss continues” Citations would be good here.

44. “It is exciting and inspiring to see that the 100-year-old classical Ising model has the potential to bring enormous power towards climate change research and other applied science studies.” This sentence feels a little fluffy, it has the wrong “flavor” for a journal article, and can be re-worded to have a similar spirit.