Review for “Thermodynamic and dynamic drivers underlying extreme central Arctic sea ice loss”, by Zhenlin Li, Fei Huang, Jian Shi, Ruichang Ding and Shumeng Zhang, manuscript submitted to The Cryosphere.

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General comments

The study conducted by Li *et alii* investigates events of major sea ice concentration decrease in the Central Arctic. To do so, they use an Empirical Orthogonal Function decomposition of standardized sea ice concentration (SIC) anomalies, and use a set of complementary analyses, sea ice budget, temperature tendency analysis, sea ice dynamic decomposition and Rossby wave source identification, to determine the drivers of the two dominant modes of the SIC anomalies. The topic is of interest, though already extensively studied, and the study could contribute to the overall understanding of how sea ice in the Arctic evolves, in particular for the next few decades, during which the Central Arctic sea ice will become more and more vulnerable. Moreover, the large range of complementary analyses to determine the sources of sea ice loss are at first read enticing.

Unfortunately, the methods at the heart of those analyses and the data used to conduct them raise some serious concerns and I therefore have doubts as to whether they can support the claims from the authors. But more importantly, the study never addresses the potential role of the ocean to drive the ELSEs, while it is now considered as the first driver of sea ice loss. Moreover, it does not seem to disentangle the trend from the EOF modes, while it is likely hidden in the two dominant modes.

I do my best to detail my concerns below and to support them convincingly. I suspect addressing them properly will require a significant amount of work, including a total change of the methodology used. It should also lead to a significant change in the results.

Major comments

In a nutshell:

- Using reanalysis data to close the temperature and sea ice budget should not be done
- The temperature tendency equation does not include the heat flux coming from "below" (sea ice or ocean), while this can be the first order driver
- The ocean is never considered in the drivers of sea ice loss
- Sea ice concentration trends are not removed from the anomalies before conducting the EOF decomposition and are not discussed neither, which is a clear lack
- There might be some important between sea ice divergence and velocity divergence
- Many of the suggested causalities are not supported by the results and could be the other way around
- Many important methodological precisions are missing

Two major concerns are related to the methods and data used in the budgets (Methods sections 2.4 and 2.5 and Results sections 4 and 5).

Reanalysis data fluxes should not be used to close a budget

An important caveat of reanalysis products is that they are not physically consistent. Indeed, when assimilating observational data into the model state, spurious fluxes are introduced, breaking the conservation of some properties, including momentum and mass: in reanalysis, "the system state estimate can undergo jumps, implying implicit non-physical sources, and rendering very difficult the physical interpretation of the time-evolving state. Methods have been employed to smooth out the discontinuities over finite times, but still leaving artificial imbalances in the solution." (Wunsch & Heimbach, 2007). While reanalysis products provide the best estimate of the state of the climate, they should not be used to calculate budgets, as they cannot physically close them. A better, physically-consistent alternative to reanalysis would be State Estimates products, but those are costly to compute (e.g. ASTE, Nguyen et al., 2021) and typically not available for the kind of investigations conducted here.

Unfortunately, this study relies on reanalysis products, ERA5 and JRA55 to calculate a temperature budget, and PIOMAS to close a sea ice budget. This is a major issue, especially considering that the most important terms of the budgets (diabatic heating for the temperature; thermodynamics for the sea ice) are calculated by making the assumption that those budget are closed and that the residuals therefore correspond to the wanted term. Uncertainties are difficult to evaluate in reanalyses, and so it remains unknown whether using those data while significantly alter the results on the spatial and temporal scales considered here. But in doubts, I believe we have to make the assumption that the unphysical flux produced by data assimilation might not be negligible. Therefore, the method proposed here to evaluate the thermodynamical and dynamical contributions to sea ice loss is not sound. Note that this is a bit less worrisome for the sea ice budget, as the dynamical term is actually estimated by observations and not a reanalysis, but the thermodynamical term should still include not only the "real" thermodynamics but also a (hopefully small) spurious term related to the correction of the sea ice thickness by assimilation of observations into the PIOMAS H_{eff}.

The Temperature tendency does not account for the ocean or sea ice

Equation (1) in section 2.4 equates the temperature tendency to the advection, the adiabatic heating and the diabatic. The tendency, the advection and the adiabatic heating are computed using the ERA5 reanalysis (see above for a major caveat of using this data for a budget). The last term, arguably the most important (I regret the authors did not show the comparison of all the terms), is estimated by considering that it is equal to the residuals of the budget. This could be true if 1. the reanalysis could be used to close the budget (I have argued above that it is not the case) and 2. if it was the only term missing. Unfortunately, I believe that the heat flux at the surface is not accounted for, in this equation. Indeed, sea ice or ocean are important heat sources or sinks and are therefore likely to provide an important heat flux. In equation (1), it is implicitly in the residual, but the text describing the equation makes me think that the authors are not aware of it: "The diabatic heating rate can also be directly estimated by summing the large-scale condensation heating rate, convective heating rate, vertical diffusion heating rate *[this would be the sensible heat flux between atmosphere and ocean/sea ice]*, solar radiation heating rate *[did the authors account for upward solar radiation proportional to the albedo of the surface? it seems not]*, and longwave radiation heating rate *[another heat flux for which sea ice or ocean need to be accounted for, but with no mention of it in the text]* based on the JRA-55 datasets." This is particularly worrisome as this study focuses on sea ice, but the equation is never used to link temperature tendency to sea ice! And in some cases (including during ELSEs), we can expect this flux to be the first order driver of the temperature tendency. Therefore this budget is not closed and, unless I missed something fundamental in the methods, what the authors consider as the diabatic heating is actually not the diabatic heating alone.

Because of those two major concerns, the results described in this study cannot be fully trusted. Many of those results are overall consistent with the scientific literature (e.g. the dominating importance of the thermodynamics over the dynamics in the sea ice budget, Le Guern-Lepage & Tremblay, 2023 or the importance of the "diabatic term" in the temperature tendency, over the other terms). But some other results are a bit at odds, to the best of my knowledge, e.g. the prominent importance of latent heat flux, which is rather supposed to be one or two orders of magnitude smaller than radiative and sensible heat fluxes (note that this could actually be related to another methodological issue, see last major comment).

What is the role of the ocean in the sea ice loss?

This says it all. The ocean is a complete blind-spot of this study, while it now explains over half of the sea ice melt in the Central Arctic (e.g. Carmack et al., 2015, Oldenburg et al. 2024).

Are trends of sea ice concentration included in the EOF modes?

In the preprocessing steps before decomposing the sea ice concentration into EOFs, the sea ice anomalies are computed by removing the climatology. But no trend seems to be removed. Considering the major changes that the sea ice is undergoing in the Arctic, I would expect the trend to be the dominant mode of the EOF decomposition. The authors first briefly mention that indeed the first mode of the non-normalized anomalies "spatially manifest as significant SIC anomaly signals along the Arctic marginal seas and the edge of the central Arctic" (l. 220). The authors claims this is due to the summer signal; my guess is that this should also include the overall trend. The authors then normalize the anomalies to give equal weights to other seasons. But I would not expect this normalization to remove the trend. Yet, I am surprised to not see any mention of it when analysing the EOF decomposition of the standardized anomalies. Is that because it only appears in the third or higher order mode? Or because the method does indeed remove the trend? Or because the trend is actually not a major mode of variability? If the latter, this would be a major result that should be discussed. If not, I suspect it should be hidden somewhere and needs to be analysed and discussed. Moreover, in general, EOF decomposition studies tend to first remove the trend. I believe this needs to be done here as well. Note that this is not straight-forward for sea ice concentration, as this typically leads to sea ice concentrations above 1 at the beginning of the period of interest, and that a trend needs to be computed for each day-of-year (e.g. Richaud et al., 2025 for an example of day-of-year trend calculations for atmospheric variables).

Sea ice divergence is not the same as Helmholtz divergent term

In section 2.5, the sea ice dynamical term is decomposed into a advective and diverging term (eq. 3). Then in section 2.6, an Helmholtz decomposition of the (sea ice) velocity field is done, computing the divergent and rotational term. The text leads to think that the diverging term of eq. 3 and that of eq. 7 are equivalent. This is not the case and was (still is) very confusing to me. Those kinds of Helmholtz decompositions are typically done in rheological studies, but this is not the case here. Moreover, the text gives the impression that since dynamics can be decomposed into advection and divergence, and since the velocity field can be decomposed into divergence and vorticity, then the advection is equivalent to the vorticity: "By decomposing the standardized SIM fields into divergence (Fig. 9a and b) and vorticity (Fig. 9c and d) components, it can be observed that the sea ice advection primarily drives the anomalies in sea ice motion" (l. 312). This is obviously not the case. I do not understand why the advection was not directly computed, or if it was, why it wasn't shown and relied upon, rather than going through the rotational/vorticity. In any case, the Helmholtz decomposition does not bring anything to the study and I would suggest to drop it.

Many of the assertions are not supported by the analyses

"The above analysis suggests that local surface temperature anomalies caused by local diabatic heating anomalies are the important factors in the formation of EWSM and PASM." (l.243): I do not see how this sentence is supported. It suggests that atmospheric (surface) temperatures drive the two EOF modes found in the study, on the basis that the spatial patterns of the composite temperature match the EOF mode patterns. But it could very well be (and I would guess likely is) the opposite, with the ice pattern driving the temperature. This is one example, amongst many, of a causal link claim made by the authors that could very well be the other way around. And that reversed causality is never explored or mentioned. Other examples include l. 285-286, l. 317-318, l.330-332, l.341-342, l.343-345 (list not exhaustive). Moreover, some other claims in the Conclusions and Discussions section do not seem to be really demonstrated in the paper: "Under thermodynamic dominance, both the EWSM and the PASM trigger water vapor and cloud feedbacks to sustain and enhance their development" (l.386-387). Section 4.2 does discuss this but does not provide any result to prove it and Figure 7 just gives some vague (not convincing to me) spatial coherence between the different metrics. Same with l.394: "the convergence and divergence of SIM exert a minor positive contribution to the EWSM": I could not find any substantial result in the main text that support this.

Proving the causal link is complex, requires using some causality methods (e.g. Liang-Kleeman), and seems outside the scope of the study. Nonetheless, the claims of the authors are a bit too assertive to my opinion, and a more nuanced view on the direction of the links needs to be taken into account.

Many important aspects of the methodology are missing

The description of the methodology at the moment does not allow to reproduce the results. For example, the temperature tendency description (section 2.4) does not mention if this is for surface (2m) temperature, atmosphere-integrated temperature), boundary layer temperature or else. See also above for the lack of description of the trends of sea ice concentration, and other variables as well, if any trend is accounted for. The calculation of the climatology is not sufficiently described (see also minor comments on a suggestion to smooth it). None of the units of the terms are ever given (if they had been, it would have become obvious that the different "divergent" terms are not the same). The temporal threshold for the detection of ELSEs is not given. Looking at the figures, there might not be any, which then is a potential point of improvement of the study (see minor comment). Finally, references are missing for nearly all important equations used in the method.

Minor comments

- Many equations are not referenced, such as the temperature tendency equation, the Helmholtz decomposition, the diabatic heating rate calculation, etc. I know those are classic equations, but they can take alternative forms depending on the field of interest, and therefore a quick reference towards other papers using those equations in the same way would be relevant.
- Sea ice observation data: why only start in 1989? Sea ice concentration and motion data are available starting in 1979, which would give another decade of precious data on an else relatively short time series. This would give a more robust analysis.
- Climatology calculation: the methods are not very explicit on the way the climatological mean is computed. It seems to be simply the mean of each day of the year, I suspect over the whole time series. A justified choice of the baseline would be good: 1979-2007 would avoid the recent decline period; or on the contrary only take the last 30 years to have the most recent behaviour, or the whole period? See Smith et al. (2025) for an in-depth discussion of why baseline are important. On top of that baseline aspect, it is conventional to smooth out the climatology when using daily data, by using a window around the considered day-of-year, yielding the advantage of increasing the sample size (see the MHW field of research, e.g. Hobday et al., 2016). This does not seem to have been the case in this study, when looking at Fig. 1.d).

- ELSE definition: there does not seem to be any temporal threshold on the detection of the ELSEs. In other word, a sea ice concentration below the 1.5 standard deviation threshold for 1 day would count as an ELSE, as would an event lasting a year. No discussion is brought on that aspect, and it seems to me that this requires some thinking and a different definition could yield different results. Considering the different temporal scales of the atmosphere, the ocean and the sea ice, the choice of the temporal threshold would give more or less weight on events that are likely to influence larger scale dynamics. I would recommand to filter out events shorter than a specific threshold, to be justified (e.g. 10 days? 1 month?). This would likely change the results in section 3.3: why is the 2008 event included as a significant one for the PASM mode, but not 2003?

- On the same aspect of ELSEs definition, I was very surprised to see that 2012 is not included in the list of ELSEs that are related to the EWSM and PASM modes, while it is the observational record of sea ice low. Considering its spatial pattern, I would expect it to be maybe in a PASM positive phase, but also with some EWSM negative contribution (wild, vaguely educated guess ;)). That does not seem to match Fig. 2, and I am curious as to why it is not in phase opposition with 2007 and why it is not more prominent. The study should at the very least discuss this aspect, considering the importance of 2012 in the recent sea ice evolution. Similarly, 2007 is also a very important sea ice low event: a couple of sentences on how it fits in the EOF modes story would be valuable.

- The text only mentions the first two modes, which seem to explain 33 % of the total variance. This means the other modes are also important. It seems that the authors would like to discuss the other modes in another study, but I think it would be important to at least mention how quickly the explained variance decreases with the other modes. Moreover, the fact that the first two modes explain "only" 33 % of the total variance of sea ice concentration standardized anomalies seem to indicate that the EOF decomposition might not be the best approach to explain the variability of sea ice. A discussion on this aspect seems important.

- Figure S7 shows the composite differences of the standardized anomalies for the different heat fluxes. First, decomposing the radiation flux into solar (shortwave) and thermal (longwave) radiation would be valuable. Second, no description of how those composite differences and anomalies are computed, leaving the reader guessing the methodology. Is that the standardized anomalies of each flux? Or is it the composite anomalies of the fluxes but for the EOF modes of the standardized anomalies (of sea ice)? No units, no labels are given on the colorbar to help me decide. But I suspect it is the first option, looking at the colorbar values. If that's the case, it means each flux is normalized by its own standard deviation. But then we certainly cannot compare those fluxes together! The absolute value of the latent heat flux could be (and likely is, to the best of my knowledge) orders of magnitude smaller than the absolute value of the radiative fluxes: we have no information on that aspect in this manuscript. If that is the case, the claim that "Fig. S7 indicates that latent heat flux anomalies are prominent in the formation of the EWSM and PASM" (l.249-250) could be wrong, unfortunately.

- On a related topic, the reader misses critical information to understand how the SIM anomalies are standardized: are U&V standardized by the total velocity SD, or by U SD for U and by V SD for V? I suspect the first option, as the second would not make any sense and would prevent any comparison, but some weird patterns on Fig. 8 (e.g. on the Siberian Shelves for 8.b) cannot take off my mind that the second option might be used. Please provide the necessary details.

- Divergence: "There is an out-of-phase relative variation of D between the Eastern and Western Hemispheres composited for EWSM mode" (l. 315). As already mentioned in the major comments, I am very confused by the use of sea ice divergence in the sea ice (thickness) budget and in the Helmholtz decomposition; there is also a third "divergence" used in this study, defined by eq. 6 and written DF. This is not the same divergence as the other two, since it uses SIC, instead of H_{eff} for the sea ice budget divergence and no sea ice for the Helmholtz decomposition. Yet, it does not seem to be distinguished in this section. I suspect that the DF and the ice thickness divergent term might be similar, but they would still show some differences.

- The whole section 6 on the Rossby wave source and teleconnection seems out of place. It does not use the same approach (why not use the composite differences as done for all other aspects?), it is not clearly linked to the EOF modes, Fig. 11 only shows the Pacific side of the Arctic (why not the rest) and its contribution to new scientific knowledge is not obvious to me (I believe the Pacific role in the Rossby wave generation is well known and that the spatial pattern of the T-N wave activity flux has also already been documented extensively). Therefore, I do not understand the contribution of this section to the general scientific knowledge. But I admit that this is a bit far from my domain of expertise and I might simply not be knowledgeable enough to understand its significance. I let the other reviewer(s), the editor and the authors judge on that aspect.

- Oceanic discussion (l. 413-435): This is the first and only time that the ocean is considered in the story. Unfortunately, the Sverdrup balance is not valid for the Arctic (e.g. Timmermans & Marshall, 2020), and the inflow of warm Atlantic water is governed by a complex set of atmospheric and oceanic interactions. Moreover, the Sverdrup balance is brought up in an attempt to explain discrepancies between Fig. 5b and S10e. The issue is that those are not showing the same thing at all! Fig. S10e shows surface temperature anomalies (in K) while Fig. 5b shows the diabatic rate (which should be in K/s); comparing that latter figure to the temperature tendency anomalies could be done, but not to the actual temperature anomalies. Hence, we should not expect a similarity between Fig 5b and S10e. Regarding the oceanic heat transport in the Arctic, a good source of information are Docquier & Koenigk (2021) and Docquier et al. (2021). Check also Polyakov et al. (2023). Many other papers investigate the role of heat inflow into the Arctic and would be a much more reliable and convincing source of explanation of Atlantification than the proposed hand-wavy Sverdrup balance that is not applicable.

Suggestions, technical details and typos

Text

- Use of “vorticity” (e.g. l. 313), “rotational” (e.g. l. 154) and “nondivergent” (e.g. Fig. 9) names for the same term: please pick one term and stick to it. Same for divergent vs. irrotational.
- l.10: remove “First,”
- l.14: “which highlight common characteristics of sea ice variations.” We could argue that this is not really the case, and that concentration anomalies are simply a convenient metric to observe, but that ice thickness would be much better to really understand sea ice variation.
- l. 28, “approximately twice the global average”: the most recent estimates rather suggest 3 to 4 times (e.g. Rantanen et al. 2022)
- l. 33, “This rapid warming in the Arctic has led to a significant decline trend in Arctic sea ice ”: one could argue that the decline in Arctic sea ice has led to the rapid warming, more than the opposite (albedo positive feedback). Please nuance this sentence.
- l. 47: Voosen (2020) seems like a journalistic piece, not a scientific article. While it is an interesting one, please provide a peer-reviewed paper. Moreover, I could not find any part of that (short) piece that talks about a dynamic and thermodynamic coupling... was that LLM-generated?

- l. 51: “Arctic sea ice concentration” or “extent”
- l. 54: Sticker et al. (2025) would be a good, recent addition here
- l. 61: Hoffman et al. (2025) would be a good, recent addition here
- l.63, “ the reduction of Arctic sea ice is spatially heterogeneous, which is attributable to the spatial variation of thermodynamic and dynamic processes driven by atmospheric and oceanic circulation. [. . .] The trend of sea ice reduction is notably significant in the marginal seas along the Eurasian and the North American coast, while the sea ice from north of Greenland and Canadian Arctic Archipelago to the pole remains relatively stable”. I agree that atmospheric and oceanic circulation play an important role in generating spatial variability, but not those described by the text. The difference between shelves and northern part is simply due to astronomical considerations (less solar radiation close to the pole than further south, e.g. Maksym 2019)... Please rephrase.
- l.71, “the perennial sea ice in the central Arctic has begun to undergo extreme reductions in recent years”: “recent” is subjective, but it has been a few decades by now, so I would remove the “begun [. . .] in recent years”
- l.76, “This is because the sea ice in the central Arctic region is predominantly multi-year thick ice, and the absolute value of sea ice variation in winter and spring is relatively small”. It is also (and maybe primarily) because the winter and spring sea ice extent is strongly geographically constrained by the surrounding continents, and that there is therefore less degree of freedom (e.g. Maksym, 2019). Please rephrase.
- l. 179-186: This is a good introduction, though maybe a bit too detailed. You could shorten to only highlight the relevant definition.
- l.187, “climatological SIC”: is that annual mean? seasonal? day-of-year? Maybe worth considering a time-varying definition. See also minor comment for smoothing suggestion, to have a more statistically robust definition. If it is annual (as Fig 1 seems to suggest), it needs some discussion, as it will induce a significant ELSE detection bias between winter and summer.
- l.193, “which validates the rationality of the definition of the central Arctic in this study”: except for JAS, for which the probability of SIC¿90 % represents a small fraction of the Central Arctic. See above suggestion to use a time-varying definition.
- l.225-226: please provide references for those EWSM and PASM: are those names yours? Or do they come from other studies? Do they match other studies?

- l. 241: how is the spatial correlation computed? Why is it only computed for air temperature and not for the diabatic heating, heat fluxes, etc? Also, see major comments: correlation is not causation.

- l. 334, “Considering that the most pronounced EWSM signal occurs during the winter of 2002 [...]”

- l. 454-455, “ The unpresented EOF3 and EOF4 in this study are primarily related to anomalous temperature advection from mid-latitudes, with significant increases in dynamic contributions.”: I don’t really understand the distinction made between dynamic and thermodynamic, then. To me, the advection of heat would lead to a change in thermodynamics, not dynamics, which would rather be controlled by momentum fluxes, not heat fluxes. So this sentence seems self-contracting to me.

Figures

- All: please provide labels + units on all colorbars and don’t hesitate to also add title above the different panels to make sure the reader can quickly understand what they are looking at. Try to be consistent and homogeneous between figures, keeping the same latitude boundaries, projections, row/column orientation, etc.

- Fig. 1: The colormap for panels a and b is not sequential, consider using another one; please mask regions where there is no ice, instead of plotting the 0% SIC. Panel e (and left panels in figure 2) are great! I would suggest making those a bit wider to see better.

- Fig. 3: this figure is a bit confusing because at first glance, it is not clear that adding advection and divergence leads to the Dynamics term. Please consider stacking them to reducing their width and adding transparency to make it more obvious that there are only two terms and that you decompose one of them into two.

- Figures 6 and 9: I would suggest to transpose the figures, putting the EWSM and PASM as rows instead of columns, to match the other figures (e.g. Fig 7 and those in SI). Keeping the same convention would allow the reader to be able to skim through and compare the figures in a more intuitive way.

- Figs. 10 and 11: Why change the projection? Other figures use a North Polar Stereo projection while 10 and 11 are cylindrical (?).

- Fig. 11: Why not plotting the whole hemisphere? At least for a) and d).

- Fig. 12: Great schematic! Not sure it is very colorblind-friendly, but I like it. I am unfortunately not convinced by the content, because of all the reasons detailed in the major comments...

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