

Reviewer #2: The authors address the topic of parameter sensitivity for simulating Antarctic sea ice, an important topic, and demonstrate some parameters which are particularly important. This is a valuable piece of work that is worthy of publication.

However, I must disagree with reviewer #1 that the paper was well-written. I may not have the requisite expertise to fairly review the paper, but I found the manuscript to be incredibly hard to follow, the Methodology was poorly explained and in particular I was not sure how to interpret many of the figures. I've attached an annotated PDF but here are some general comments that I think the authors should address:

Thank you for your constructive comments. We have revised the manuscript by improving its clarity and readability. Please see below our response (blue text) to each specific comment point by point.

1. Methods - I'm not very cognizant of Emulator techniques, and I struggled to understand many of the results because I wasn't sure how to interpret most of the metrics. I think some concise and clear description of what the metrics indicate, why different emulators are employed etc would be helpful

We have reformulated the methods section to make it clearer and more concise.

- In section 2.2 we rephrase the process in Fig. 1, clarifying more clearly what each metric means and why the GP emulator should be used. The methods in each section below correspond to one of the steps in Fig. 1.
- Section 2.3 was separated from section 2.2 of the original manuscript to specifically describe the method of sampling the 18 model parameters.
- Section 2.4 has been made smoother and more concise to highlight that the most critical step is the selection of the covariance function in the GP software, and the sentence in the original manuscript about how the software is calculated has been removed to avoid diverting the reader's attention.
- The notation in section 2.5 on the first term on the left in Eq. (6) has been represented more specifically, which now reads (Line 176): "where the left-hand-

side term is the change or dadt (also referred to specifically as dC/dt and dV/dt for changes in SIC and SIV respectively)". And these two notations are unified throughout the text and the figures.

2. Figures. There are a lot of figures, many of which are buried in the Supplemental Material. I think where an entire paragraph or more is devoted to a figure, it cannot be fairly described as Supplemental. There seemed to be no logical reason why some figures are in the ms and some in the Supplement. I don't think the Authors have given sufficient thought on what their key messages are, and what figures are required to convey that message (and more importantly which are not). I suspect that the ms will get much more impact with fewer but well-explained figures, appropriately placed in the main text.

We have moved Figs. B4 and B5 from the original manuscript to the main text (now Figs. 4 and 6) as suggested by the reviewer, given that the text uses two paragraphs to describe the two figures. In addition, we have split section 3.2 into two subsections (3.2.1 and 3.2.2) and have started section 3.2 with a paragraph that briefly summarises the results that this section seeks to present. The headings in section 3.2.1 (Model ensemble mean and standard deviation) summarise exactly the information expressed in Figs. 3 to 6. Subsequent to the above changes, the reasons for placing the figures in the main text and in the appendix respectively should now no longer confuse the reader.

At this point, we believe that the main text contains all the key information we have intended to convey. However, precisely because of this, it seems impossible to put fewer figures in the text, as the removal of any one figure would make the study incomplete.

3. Parameter names; the authors consistently refer to the model variable name (or symbol as per Table 1 header) for each parameter (e.g. Cd_{ice}) rather than a more physically meaningful long name. For anyone not intimately familiar with SI3 this makes it really hard to follow, without frequently referring back to Table 1. Since the authors focus on only 4-5 parameters, perhaps be a little kinder to the reader and use long names ('Description' in the Table 1 header) in the text.

All parameter names appearing in the main text have been replaced with long names, especially the key ones, and we have additionally appended symbols to the long names to make it easier for the reader to follow and correspond to the figures. For example,

- Lines 383-385: ... Fig. 9 shows that in summer the $RMSE_{SICB}$ may also be sensitive to the minimum floe diameter for lateral melting parameter (rn_{dmin}) and the magnitude of the damping on salinity (rn_{deds}) ...
- Lines 434-435: Consequently, in contrast to the reduction of snow thermal conductivity (rn_{cnd_s}) which inhibits ice freezing ...
- Lines 456-458: With the exception of snow thermal conductivity (rn_{cnd_s}), ice-ocean drag coefficient (rn_{cio}) and air-ice drag coefficient (Cd_{ice}), whose physical effects have been elucidated, the adv term in the winter SIV budget is also sensitive to ice strength (rn_{pstar}) (Fig. 12a).

4. Sub-sections. It took me a while to work out the aim of some of the subsections. Some slightly more meaningful sub-headings would be helpful, and maybe an opening sentence (e.g. in the last section we showed which parameters the model is most sensitive to, we now explore the optimal values of those parameters to match the observed budget..)

Thanks for the good idea to add some link-up sentences at the beginning of each section. We checked heading of each sub-section and added the opening sentence accordingly to improve the readability of the entire manuscript, e.g.:

- Section 3.2 (Lines 242-247): The diagnostics of the SIC and sea ice thickness of the model ensemble in the last section show that In this section we first calculated the SIC budget and SIV budget for the ensemble of 449 model runs by applying the same approach as for the calculation of the observed SIC budget (cf. Fig. B1), and then ...
- Section 3.3 (Lines 333-336): Based on the results of the last section, the area integrals of adv and res in the SIC (and SIV) budget and the $RMSE_{SICB}$ are used as the metrics to assess the sensitivity of the model's sea ice budget to 18 parameters in this section ...

- Section 4.1 (Lines 416-418): Several parameters have been identified in Sections 3.3 and 3.4 as having a significant impact on the simulated SIC and SIV budgets in the Southern Ocean. In this section we present how these parameters specifically act on the SIC and SIV budget by looking at the impact of parameter changes on the cumulative distribution function (CDF) in the PAWN method.
- Section 4.3 (Lines 502-504): The previous sections have shown the sensitivity of the simulated sea ice budget to parameters and there are a number of parameter sets that are recommended (Table 3), in this section we provide further insight into how these parameter sets perform in terms of other metrics.

Please also note the supplement to this comment:

<https://gmd.copernicus.org/preprints/gmd-2022-170/gmd-2022-170-RC2-supplement.pdf>

Line 18: "sensitivity" change to "sensitive"

Revised.

Line 20: change "better quality of" to "optimised"

Revised.

Line 37: remove "Whereas"

Removed.

Line 56: "at time", something missing here?

Yes, we have modified it to "at a time".

Line 70: the switch from writing about emulators to the purpose of this paper is quite abrupt. I would suggest adding a sentence that clarifies the relevance of the text on emulators to this paper

The new text reads (Line 73): "In this paper, our research objective is to quantify the

sensitivity ... in a coupled ocean-sea ice model by constructing a GP emulator, ..." to clarify the relationship between the emulator and our research objectives: the emulator is our key method for achieving this sensitivity experiment.

Line 101: "the other frequencies are" suggest changing to "...and 3 hours for all other surface boundary conditions."

Done.

Line 102: maybe add comment on whether this includes Antarctic ice mass loss (which of course is most relevant for Antarctic sea ice)

The added text reads (Line 102): "The continental discharge rates followed the climatological dataset of Dai and Trenberth (2002) and do not include ice mass loss in Antarctica."

Line 106: This sentence needs some work, the phrasing is a little odd and I don't quite understand what is being stated.

This sentence has now been reorganised as (Lines 108-110) "To investigate the sensitivity of sea ice budgets, we selected 18 parameters and determined their uncertainties (Table 1), which cover a number of important processes in sea ice modelling, such as ice/snow physical properties, ocean mixing and eddies, and ice-ocean/air-ice interactions." to make it more fluent and easier to follow.

Line 109: "elicited" change to "selected"?

Done.

Line 116: I struggle with this section, it needs a bit of editing to make it more generally comprehensible

This section has been largely reformulated. We tried our best to make it clear.

Line 200: comment on underestimation of divergence?

We agree that an overall overestimation of sea ice velocity observations by 5% may lead to a relative underestimation of divergence in the model results, however, we do not have sufficient evidence to suggest that this relationship is robust. For instance, even when calculated using NSIDC sea ice drift data (Tschudi et al. 2019), which has been reported to be underestimated, the model results are still underestimated compared to it (Nie et al., 2022), and the reason for the model underestimation of divergence is still under investigation.

Line 246: Spatial differencing always produces a 'noisy' output, even using the 3x3 grid cell smoothing that the authors applied to the velocity data. I would be hesitant to pay much heed to relatively small scale features such as this, which are just as likely to be artifacts of the observations

Thanks for your thoughts, we agree with the reviewer that the satellite-derived observation would contain some noise, even with the 3x3 grid filter. However, we are not sure if it would be good to consider this part of the convergence to be noise and accordingly unimportant. Because a) the observed convergence and divergence in the marginal ice region are almost interleaved, i.e. they are comparable in scales, rather than the convergence having a smaller scale (Fig. B1); b) as a comparison, there is also significant sea ice convergence in marginal sea ice regions even with a much smoother 7x7 grid filter (Holland and Kimura, 2016; their Fig. 3). Therefore, we would prefer to keep the original text.

Line 253: There's a whole paragraph devoted to this figure, so why is it stuffed into the appendices?

We agree that moving Fig. B4 from the appendix to the text would be a better choice, as we have already devoted a whole paragraph to the figure. We have now modified it and also moved Fig. B5 from the Supplementary Material into the main text.

Line 264: I can't help but think that this should be in the appendices and fig B4 - which shows the sensitivity of these terms to parameter changes - in the main text

We have followed the reviewer's suggestion to move Fig. B4 to the text, and to change the title of section 3.2.1 to "Model ensemble mean and standard deviation" to better summarise the information in Figs. 3 to 6.

Line 278: should really change the figure captions to dV/dt , not $dadt$, ideally change fig 3 to dC/dt ($dadt$ is a bit sloppy)

Thanks for your suggestion, we have replaced " $dadt$ " with " dC/dt " or " dV/dt " when it has a specific indication, both in the main text and in all the related figures, e.g.:

- Line 176: where the left-hand-side term is the change or $dadt$ (also referred to specifically as dC/dt and dV/dt for changes in SIC and SIV respectively).
- Lines 249-251: As can be seen in Fig. 3, the spatial pattern characteristics of the ensemble mean of dC/dt and adv for each season are generally consistent with observations. The magnitudes of the model ensembles of dC/dt and adv are significantly larger due to the fact that the observed ice drift has some missing values and the dC/dt term ...
- Figs. 3 to 7.

Line 285: Note that since SIV is a conserved term (unlike SIC), the 'residual' can be correctly called the 'thermodynamic' contribution.

Thank you for the clarification, we have now added a sentence to clarify it (Lines 295-297): "The residual term, which equals the thermodynamic contribution as SIV is conserved, still has the largest standard deviation as it retains the deviations of the other terms."

Line 287: insert '...and time...' after 'area'

Added.

Line 291: numerically, the dynamic terms of SIV ($adv + div$) should be exactly zero when integrated over the Southern Ocean. (not necessarily for SIC, since it's not a conserved quantity)

We agree, and we believe that the possible reason for the sum of the area integrals of adv and div to be zero for SIC is that in the Southern Ocean, where sea ice is close to free drift, the non-conservation due to sea ice ridging/rafting (r in Eq. 5) is negligible relative to the dominant role of thermodynamics (f in Eq. 5), as revealed by Uotila et al. (2014) and Holland and Kimura (2016).

We have reformulated this sentence and it now reads (Lines 305-308) "For SIV this is because these two processes do not change the total amount of sea ice, and for SIC this also holds approximately, considering that in the Southern Ocean sea ice is close to free drifting and the non-conservative nature of SIC due to ridging can be neglected (Uotila et al., 2014; Holland and Kimura, 2016)."

Line 292: This isn't correct. It's true that the Southern Ocean integral of the the dynamic SIV term should numerically be zero. BUT those terms still effect the thermodynamic term. Consider a completely static icepack with no velocity at all - it will have a much lower thermodynamic freeze/melt rate because divergence/advection will not maintain areas of open ocean (e.g. polynyas). Numerically though the net dynamic term is the same as a realistic representation, i.e. zero

Thanks for the correction and the reasonable example. Since numerically the spatial integrals of adv and div do cancel each other out (and thus the integrals of dadt and res are almost identical), the sensitivity analysis only for res and adv does not affect the conclusions of our study. We have modified this sentence to make it flow more logically with the preceding text (Lines 308-309), "Therefore, when studying the effects of model parameter uncertainty on sea ice budgets in the following sections, it is only necessary to only use the area integrals of res (or dadt) and adv (or div)."

Line 305: insert 'net' after 'of'

Revised.

Line 329: how do these thermodynamic parameters have an influence on the dynamic term?

The thermodynamic and dynamic processes are interdependent, one example would be a case given by Uotila et al. (2014) "where the ice melt decreases the sea-ice concentration and thickness, and consequently results in a faster moving sea ice, which in turn affects the divergence and advection."

Line 331: 'PWAN' change to 'PAWN'

Fixed.

Line 338: Although you've already defined the term in line 330, it's really hard to remember what all the parameter names represent, so I would advice using the long names each time as well

We are now using long names throughout the text and have added a short-hand symbol at the end of the long name to enhance readability.

Line 353: "perfectly" change to "well"

Revised.

References:

- Holland, P. R. and Kimura, N.: Observed concentration budgets of Arctic and Antarctic sea ice, *J. Clim.*, 29(14), 5241–5249, doi:10.1175/JCLI-D-16-0121.1, 2016.
- Holmes, C. R., Holland, P. R. and Bracegirdle, T. J.: Compensating Biases and a Noteworthy Success in the CMIP5 Representation of Antarctic Sea Ice Processes, *Geophys. Res. Lett.*, 46(8), 4299–4307, doi:10.1029/2018GL081796, 2019.
- Nie, Y., Uotila, P., Cheng, B., Massonnet, F., Kimura, N., Cipollone, A., and Lv, X.: Southern Ocean sea ice concentration budgets of five ocean-sea ice reanalyses, *Clim. Dyn.*, <https://doi.org/10.1007/s00382-022-06260-x>, 2022.
- Tschudi, M. A., Meier, W. N., and Scott Stewart, J.: An enhancement to sea ice motion and age products at the National Snow and Ice Data Center (NSIDC), *Cryosphere*, 14, 1519–1536, <https://doi.org/10.5194/tc-14-1519-2020>, 2020.

Uotila, P., Holland, P. R., Vihma, T., Marsland, S. J. and Kimura, N.: Is realistic Antarctic sea-ice extent in climate models the result of excessive ice drift?, *Ocean Model.*, 79, 33–42, doi:10.1016/j.ocemod.2014.04.004, 2014.