

Conservation of heat and mass in P-SKRIPS version 1: the coupled atmosphere-ice-ocean model of The Ross Sea

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We thank the Reviewer for all the time and effort put into the review of our manuscript and are pleased with their positive and constructive comments. Please find the response to each of the comments below. The reviewer's comments are displayed in **bold text**, replies are shown in normal text, text from the original manuscript is shown in **blue**, and proposed changes to the manuscript are shown in **red**.

Review of Conservation of heat and mass in P-SKRIPS version 1: the coupled atmosphere-ice-ocean model of The Ross Sea” by Alena Malyarenko, Alexandra Gossart, Rui Sun and Mario Krapp.

Summary: The authors present a new version of a coupled regional model of the Ross Sea region in which the coupling between the atmosphere and ocean components now conserves heat and mass, in contrast to previous versions of such models. Overall I find this to be a very valuable contribution to the modelling of polar regions and recommend publication after addressing the relatively minor comments below.

1 General comments:

Overall, while the results presented by the authors are compelling, they are somewhat oversold. I have highlighted some specific instances below but in general the authors refer to their new version of the model as being “better” or “more accurate” with no (by design) quantification of what this means. The authors note that they do not spin up the model as they are not attempting to simulate the Ross Sea in this paper, but rather aim only to highlight the technical advancements of the model. I commend the authors for noting this explicitly, but it then makes it difficult to understand what they mean by “better”. I recommend checking instances of such language (many of which I have highlighted below) and either clarifying what the authors mean by “better” or “more accurate”, or rephrasing.

We thank you for this comment, and will make sure the language has been checked to rephrase such instances.

My second general comment is on organization. It is quite muddled where various parts of the technical changes the authors have made to the model are within the paper. Some are in the "Methods", some in the "Experiments" and some in the "Results" sections. In particular, I found it odd and difficult to understand why Section 3.1.2 "Scaling by the sea ice concentration mask" is in the results section. Surely the description of the contrasting methods by which this scaling is done should be in the Methods, and then only the results contrasting the two shown in the Results section? As I have noted below, I also found it difficult in this section to understand what the contribution of the authors is in this section. See below for details.

We thank you for this comment, which is addressed in the minor comments below.

2 Minor comments:

Line 18: Maybe "mass balance" would make more sense here than "mass gain"?

Thank you, we will replace "mass gain" by "mass balance".

For example, the Southern Ocean is the main source of moisture for the Antarctic continent (van Wessem et al, 2018; Agosta et al, 2019) and it controls the mass gain of land-locked ice and thus global sea level (e.g. Holland et al., 2020; Golledge et al., 2015; Krinner et al., 2007).

For example, the Southern Ocean is the main source of moisture for the Antarctic continent (van Wessem et al., 2018; Agosta et al., 2019) and it controls the mass balance of land-locked ice and thus global sea level (e.g. Holland et al., 2020; Golledge et al., 2015; Krinner et al., 2007).

Lines 30-36: Would be worth discussing in this section about the fact ESMs don't have coupled ice sheets or ice shelf cavities, but that regional models can.

Line 38-39: "their global physics are not optimised for polar areas". What does this mean? Can you provide specific examples? I also looked at the Agosta et al., 2015 paper that is cited but it also just makes this claim with no substantiation or reference. More detail needed.

Thank you for the two remarks above. We decided to address them together. First, as described by Smith et al. (2021), GCM are typically run at low resolution and can not represent all the processes. For example, refined spatial patterns of accumulation and melt processes at the ice sheets - atmosphere/ocean interface can not be represented in GCMs. Which leads to static ice sheet boundaries and heavy parametrization, limiting the inclusion of ice sheet/ice shelves cavities models into GCMs. This implies that the physics of ice and the ice-ocean and ice-atmosphere interactions are usually not accurately represented in GCMs.

Secondly, the coarse resolution of GCM limits them in the representation of local scale and regional features (e.g., the Antarctic Peninsula) and the parametrization of physical processes occurring at finer resolution prevents the adequate representation of regional and local scale phenomena, especially in complex areas (Bozkurt et al., 2021).

ESMs that are part of the Coupled Model Intercomparison Project (CMIP) experiments, have coupled global ocean, atmo-

sphere, land and sea ice models (Meehl et al., 1997), but their spatial resolution is rather coarse. In addition, their global physics are not optimised for polar areas (Agosta et al., 2015), leading to various performances in the Arctic and Antarctic. Although there is a general improvement since previous experiments, the CMIP6 (Eyring et al., 2016) models still represent an ocean surface that is too warm and too fresh for the Southern Ocean, with a too small annual sea ice extent (Beadling et al., 2020). The models struggle to represent realistic sea ice cover (Mohrman et al., 2021) and show a wide spread in the different terms of sea ice formation/dissipation, leading to large uncertainties in the sea ice budget across the different models (Li et al., 2021; Roach et al., 2020).

ESMs that are part of the Coupled Model Intercomparison Project (CMIP) experiments, generally have coupled global ocean, atmosphere, land and sea ice models (Meehl et al., 1997). However, the global atmosphere and ocean models that make up ESMs are not optimized for polar areas (e.g. Hines et al., 2008, Azaneu et al., 2014) and polar versions of these models are developed to represent processes specific to these regions. In addition, the spatial resolution of ESMs is rather coarse, which prevents them from representing local or regional-scale processes. For example, Smith et al., (2021) raises the fact that accumulation and melt at the ice-ocean-atmosphere interface have refined spatial patterns that can not be represented in GCMs. And this leads to static ice boundaries and heavy parametrization of these processes, limiting the inclusion of refined ice sheet or ice shelf cavity models into GCMs. Therefore, ice-ocean and ice-atmosphere interactions are usually not accurately represented into GCMs.

In addition, the parametrization of processes occurring at higher resolution in GCMs physics limits them in the representation of local scale and regional features (e.g., the orography and associated local processes of the Antarctic Peninsula, Bozkurt et al., 2021), indicating that the global physics of GCMs are not optimised for polar areas (Agosta et al, 2015; Bozkurt et al., 2021), leading to various performances in the Arctic and Antarctic. Although there is a general improvement since previous experiments, the CMIP6 (Eyring et al., 2016) models still represent an ocean surface that is too warm and too fresh for the Southern Ocean, with a too small annual sea ice extent (Beadling et al., 2020). The models struggle to represent realistic sea ice cover (Mohrman et al., 2021) and show a wide spread in the different terms of sea ice formation/dissipation, leading to large uncertainties in the sea ice budget across the different models (Li et al., 2021; Roach et al., 2020).

Line 57: Table reference is broken

We thank you for highlighting this. This is an error as no table is planned to be inserted here, we will remove the reference to the table.

Line 98: NCAR is the National Center for Atmospheric Research (American spelling of center and not plural).

Thank you, we will change the text accordingly.

It is a non-hydrostatic mesoscale atmospheric model developed by the NCAR (National Centres for Atmospheric Research) for both atmospheric research and operational forecasting applications.

It is a non-hydrostatic mesoscale atmospheric model developed by the NCAR (National Center for Atmospheric Research) for

both atmospheric research and operational forecasting applications.

Line 107: “and has a closer snow water equivalent reproduction” closer to what? What is it reproducing?

The improvements made to the model led to a better representation of a series of snow surface properties and fluxes in the surface snow cover. One of them is a representation of the snow water equivalent (the amount of liquid water present in the snow) that is closer to the reality. We agree that the sentence is somewhat incomplete and propose to rephrase as follows:

It improves the representation of surface fluxes, surface meltwater production, percolation and retention/refreezing in the snow layers and surface runoff, and has a closer snow water equivalent reproduction with an improved diurnal cycle of the snow skin temperature.

It improves the representation of surface fluxes, surface meltwater production, percolation and retention/refreezing in the snow layers and surface runoff, and has a refined snow water equivalent reproduction (closer to reality) with an improved diurnal cycle of the snow skin temperature.

Line 116: “as is ordinarily the case”

Yes, we will correct this. In addition, Reviewer 2 asked to add some details, so the sentence will be changed:

The preprocessing of the input files was conducted as is ordinary the case,

The preprocessing of the WRF input files followed the standard procedure (ungrib.exe - metgrid.exe - real.exe, Wang et al., 2019),

Section 2.3: "Model coupling" seems like the wrong title for this section. Maybe "Model domain" would be better?

We agree and will rename the section title accordingly.

Line 121-122: When combining an acronym with a reference in parentheses, the parentheses are not needed around the reference also. Can be done in latex with `\cite {Hill2004}` or `\citep [ESMF][]{Hill2004}` or similar.

Thank you, this detail was overlooked in the first submission. We will change the citation when combining acronyms and references in the new version of the manuscript.

Line 179: “we compare the models performance” against what benchmark? You mentioned earlier that you were not spinning up the model since you are not aiming to simulate the Ross Sea realistically, which is fine, but then you should mention here what you mean by performance.

We agree that the wording was not ideal. We propose to rephrase as follows:

Lastly, we compare the models performance over a month-long simulation.

Lastly, we compare the results of the different model set ups over a month-long simulation.

Line 185: 41st not 41th

Yes, this will be corrected.

and then imported into MITgcm at the 41th min.

and then imported into MITgcm at the 41st min.

Line 199: Should this say “, reflecting different emissivity coefficients.”? Unclear what coefficients this is talking about.

Thank you, we will correct this in the new version of the manuscript.

At that time, longwave and shortwave net heat fluxes show more homogenous differences, reflecting different coefficients.

At that time, longwave and shortwave net heat fluxes show more homogenous differences, reflecting only different emissivity coefficients and albedo values.

Line 205: Albedo spelled incorrectly

Thank you, we will correct this in the new version of the manuscript.

the differences in the upward shortwave radiation are a result of conflicting albedo calculations between the two models,

the differences in the upward shortwave radiation are a result of conflicting albedo calculations between the two models,

Section 3.1.2: I found this section confusing, as I am not sure what the result is here. Are the authors:

- **Contrasting two existing ways the model does this scaling and commenting on which is best? If so, make this clearer and explicitly say which is better and why.**
- **Comparing the existing way of doing the scaling with a new method the authors developed? If so, make it clearer that you are saying the existing way is deficient in some way that you are fixing**
- **Comparing two new options that the authors have developed? If so, as in option (a) make this clearer and explicitly say which is better and why.**

and

Figure 5. This way of describing what is shown in each panel was confusing to me. Would suggest to describe panels (a), (b) and (c) in order. Also, should X_{WRF} at the end of the caption be X_{PWR} ?

We decided to collate those two comments and address them at the same time.

We agree that the original section 3.1.2 can be more explicit. We will make sure this is stated clearly. In addition, and in response to you second general comment, we will move section 3.1.2. to 2.5. and adapt it accordingly.

We also agree that it would be more convenient to describe the panels in Figure 5 in order and propose to revise the paragraph as follows. And yes, it should be X_{PWR} and not X_{WRF} at the end of the caption.

We initially directly import the flux components from PWR into MITgcm using the variables available from the PWR output files. The import of surface fluxes in the exf package is prohibited when using the sea ice package in a standard version

of MITgcm, because the fluxes need to be calculated according to the sea ice mask. In the coupled set up, the sea ice mask is coordinated between PWRF and MITgcm, so the fluxes can be directly used in the sea ice packages, without any mask mismatch. We thus remove this error flag from `seaiice_model.F` (in MITgcm). This standard import way (used in SKRIPS) is referred to as Direct Import in Figure 4 (b), where the path of a variable X is shown. X refers to any component of the heat flux: latent, sensible, shortwave and longwave net fluxes. In the Direct import, X_{PWRF} is imported as is into the MITgcm `exf` package. However, we find out that the flux components have already been scaled in PWRF to account for mixed grid cells, partially occupied by sea ice. This occurs mid-timestep (`module_surface_driver.F` in PWRF, Figure 4 (a), X_{scaled}) and the model output contains these scaled variables. This implies that when MITgcm receives the variable and accounts for the presence of sea ice in its own routines, the heat fluxes are scaled twice, thereby affecting the amount of heat gained or spent over the sea ice areas. To tackle this issue, we have developed a new technique (referred to as Separate Fluxes), whereby the separate heat flux components are captured in PWRF over the sea ice and over the ocean separately (Figure 4 (a), X_{ice} and X_{ocean}). In the Separate fluxes approach two variables are now imported for each heat flux component: X_{ocean} becomes the `exf` variable; and X_{ice} is used instead of the recalculation of fluxes (`solve4temp.F` in MITgcm) to determine the amount of heat available for sea ice growth/melt (`F_ia` variable).

2.5. Scaling by the sea ice concentration mask

The development of P-SKRIPS includes the import of the individual heat and mass flux components from PWRF into MITgcm. The import of surface fluxes in the external forcing package (`exf` package) is prohibited when using the sea ice package in a standard version of MITgcm, because the fluxes need to be calculated according to the sea ice mask. We have set up the two models with the same grid and the same ice-ocean mask between PWRF and MITgcm, so the fluxes can be directly used in the sea ice packages, without any mask mismatch. We thus remove this error flag from `seaiice_model.F` (in MITgcm).

This standard import way (used in SKRIPS) consists in directly importing the PWRF output variable (X_{PWRF}) to MITgcm. When X_{PWRF} refers to any component of the heat flux: latent, sensible, shortwave and longwave net fluxes or evaporation, it is scaled to account for mixed grid cells, partially occupied by sea ice in the model cell in PWRF (Figure 4 (a)). This occurs mid-timestep (`surface_driver.f90` in PWRF) and the PWRF model output contains these scaled variables. The Direct Import variable path in Figure 4 (b) illustrates how this scaled X_{PWRF} is then imported as is into the MITgcm `exf` package. This implies that when MITgcm receives the variable and accounts for the presence of sea ice in its own routines, the variables are scaled twice, thereby affecting the amount of heat gained or spent over the sea ice areas. In the P-SKRIPS version, we exchange the four heat flux components between the models for physical consistency. To tackle the double-scaling issue, we have developed a new technique (referred to as Separate Fluxes), illustrated in Figure 4 (c), whereby the separate heat flux components are captured in PWRF over the sea ice and over the ocean separately (X_{ice} and X_{ocean}). In this new approach two variables are now imported from PWRF to MITgcm, for each heat flux component: X_{ocean} becomes the `exf` variable; and X_{ice} is used instead of the recalculation of fluxes (`solve4temp.F` in MITgcm) to determine the amount of heat available for sea ice growth/melt (`F_ia` variable). This ensures that the fluxes are calculated and scaled only once between the two models.

Line 217: What is the exf package? Should give a brief explanation before talking about it.

Thank you, the exf package refers to the external forcing package in MITgcm and its description can be found in the documentation: "The external forcing package, in conjunction with the calendar package (cal), enables the handling of real-time (or "model-time") forcing fields of differing temporal forcing patterns. It comprises climatological restoring and relaxation. Bulk formulae are implemented to convert atmospheric fields to surface fluxes. An interpolation routine provides on-the-fly interpolation of forcing fields an arbitrary grid onto the model grid."

We have added the full name "external forcing package (exf package)" the first time we mention it in the manuscript.

The import of surface fluxes in the external forcing package (exf package) is prohibited when using the sea ice package in a standard version of MITgcm

Line 268: "In that regard, the land ice meltwater runoff is captured in PWRP." I am not sure what this sentence means. Please clarify.

Apologies, this will be rephrased to improve the clarity of the message:

In that regard, the land ice meltwater runoff is captured in PWRP.

For that reason, we capture the land ice meltwater runoff and send it to MITgcm.

Line 269: Comma needed on "i.e.,"

Thank you. This will be adapted.

(i.e. the amount from that timestep minus the amount from the previous timestep)

(i.e., the amount from that timestep minus the amount from the previous timestep)

Line 279: "Drygalski Ice Tongue"

Correct.

Thus, we are missing meltwater runoff from the Drygalsky Ice Tongue

Thus, we are missing meltwater runoff from the Drygalski Ice Tongue

Figure 8e: This difference in precipitation, which is explained by the authors as being due to the snow being double counted, is very large. However, I find the explanation for this confusing. They say that in SKRIPS, the total precipitation is the sum of RAINNCV and SNOWNCV. If I understand correctly, the issue is then that SNOWNCV is a component of RAINNCV already, and so adding them double-counts SNOWNCV. If this is correct, the wording needs to be changed to make this more clear. I think my main confusion stems from line 314, where it reads:

"...and, therefore, is accounted for twice in the snowfall term."

Shouldn't this say something like:

"...and, therefore, is accounted for twice in the total precipitation.?"

Since the issue is that the snowfall itself is counted twice, not that something is counted twice within the snowfall term?

Yes, we agree that it is not clear and the wording will be adapted in the new version of the manuscript.

However, the latter is defined as a component of the non-convective precipitation, which encompasses all species (rain, graupel and snow and ice) and, therefore, is accounted for twice in the snowfall term

However, the latter is defined as a component of the non-convective precipitation, which encompasses all species (rain, graupel and snow and ice) and, therefore, is accounted for twice in the precipitation term

Relatedly, since this difference is so big, is this an issue in existing models that the authors have identified? If so, I would suggest making more of this since it surely has a large impact to have double the precipitation that it should.

Thanks, we will add more details in the text.

However, the latter is defined as a component of the non-convective precipitation, which encompasses all species (rain, graupel and snow and ice) and, therefore, is accounted for twice in the snowfall term. In the P-SKRIPS, the components are added individually and the time step non convective precipitation term is ignored.

However, the latter is defined as a component of the non-convective precipitation, which encompasses all species (rain, graupel and snow and ice) and, therefore, is accounted for twice in the precipitation term. It is likely that over the course of the winter season, additional accumulation of snow over the sea ice affect the heat transfer through that layer, and in spring it will lead to increased freshwater flux into the surface of the ocean. In the P-SKRIPS, the components are added individually and the time step non convective precipitation term is ignored.

Line 337: “This is an improvement...”. An improvement over what? Be more clear about what you are comparing to here.

Thank you. We will refine the text to:

One-month test experiments for the January and August of 2016 have been carried out and show that the P-SKRIPS approach homogenises the ocean and atmosphere calculations. This is an improvement and is needed for balanced long term simulations, especially in coastal polynyas.

One-month test experiments for the January and August of 2016 have been carried out and show that the P-SKRIPS approach homogenises the ocean and atmosphere calculations. This is an improvement to pre-existing ocean-atmosphere coupling in regional climate models, and is needed for balanced long term simulations, especially in coastal polynyas.

Line 340-341: “Snow cover changes happen in PWRP. Its multi-layered snowpack and complex representation of snow on sea ice makes PWRP the better model to represent...”.

This is new, right? If I understand correctly in the previous version of the model, snow cover changes were done in both MITgcm and PWRP and so were inconsistent. Make it clear that this is new, and a change from the old way of doing it. You say it makes PWRP the “better model”. As with my comment above, be clear about what it is better than specifically.

Yes, the previous version of the model did not exchange the snow cover, and both models made it evolve independently, and so

were inconsistent. We will clarify both this and "better model" in the new version of the text:

The formation and advection of sea ice stay in MITgcm. Snow cover changes happen in PWRP. Its multi-layered snowpack and complex representation of snow on sea ice makes PWRP the better model to represent the snow/ice-atmosphere interactions.

P-SKRIPS also splits the processes related to sea ice between the two models. The formation and advection of sea ice stay in MITgcm. The snow cover changes were happening in both PWRP and MITgcm in the previous version of the model, leading to inconsistent snow on sea ice. In P-SRIPS, the snow cover changes only happen in PWRP, and are then sent to MITgcm. Its multi-layered snowpack and complex representation of snow on sea ice makes PWRP more suitable to represent the snow/ice-atmosphere interactions.

Line 354: parameterization -> parameterizations

This will be corrected.

In summer, large differences in shortwave fluxes are directly linked to the calculation of the albedo, which varies between the model components as they have different parameterization of dry/wet snow.

In summer, large differences in shortwave fluxes are directly linked to the calculation of the albedo, which varies between the model components as they have different parameterizations of dry/wet snow.

Line 378: “While the atmosphere’s response to ...”

This will be corrected.

While the atmosphere’s response the extra energy is the formation of localized weather event

While the atmosphere’s response to the extra energy is the formation of localized weather event

Line 380: “The most accurate representation...”

I do not agree with the use of the word accurate here, since you are not evaluating the model against observations, so you have no basis on which to say it is more accurate. I would suggest saying “most physically consistent” or similar.

We thank you for this suggestion, that will be implemented in the new version of the manuscript.

The presented coupled model setup constitutes, to our knowledge, the most accurate representation of ocean/atmosphere/sea ice interactions for polar climates and is thus recommended for climate modelling in any Arctic and Antarctic region.

The presented coupled model setup constitutes, to our knowledge, the most physically consistent representation of ocean/atmosphere/sea ice interactions for polar climates and is thus recommended for climate modelling in any Arctic and Antarctic region.

As noted in the existing Discussion comment, the authors have not yet followed the journal guidelines on reproducibility, and need to do so before publication.

We are aware of this problem. We have provided all of the files that are under open access licences to the repositories: <https://doi.org/10.5281/zenodo.7739063> and <https://doi.org/10.5281/zenodo.7739059>. Unfortunately, the PWRP model (<https://doi.org/10.1029/2012J018139>) files are not publicly available for publishing and the decision is beyond our control. However,

the files can be obtained on the PWRF website upon request <https://polarmet.osu.edu/PWRF/>. We are also able to provide files to the reviewers for the purpose of reviewing this manuscript through the editor if needed. The data availability statement has been updated accordingly.

The developments and files required to set up and run the model presented in this paper are available at <https://github.com/alenamalyarenko/P-SKRIPS> and on Zenodo <https://doi.org/10.5281/zenodo.7297744>. The coupled model builds on the Scripps-Kaust model described in Cerovecki et al. (2022) and Sun et al. (2019). The base code can be found at https://github.com/iurnus/scripps_kaust_model/. ERA5 Reanalysis (0.25 Degree Latitude-Longitude Grid), generated by European Centre for Medium-Range Weather Forecasts are available at <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-pressure-levels> and <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-single-levels>. BSOSE data is available at <http://sose.ucsd.edu/>. The Bedmap 2 data can be downloaded from <https://www.bas.ac.uk/project/bedmap-2/>.

Our model code for the Ross Sea case is based on PolarWRF and MITgcm. The P-SKRIPS model code is an updated version of SKRIPS (<https://doi.org/10.5281/zenodo.7336070>) and can be found in the two directories due to size limits (<https://doi.org/10.5281/zenodo.7739063> and <https://doi.org/10.5281/zenodo.7739059>). The detailed instructions for converting WRF to PWRF (<https://doi.org/10.1029/2012J018139>) can be found within PSKRIPS-main folder. The short description of steps for PWRF is as follows: find PSKRIPS/Models/WRF_4.1.3 within the repository; obtain PWRF-4.1.3 modifications by email and merge the code locally; add P-SKRIPS modifications to PWRF; compile.

ERA5 Reanalysis (0.25 Degree Latitude-Longitude Grid), generated by European Centre for Medium-Range Weather Forecasts are available at <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-pressure-levels> and <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-single-levels>. BSOSE data is available at <http://sose.ucsd.edu/>. The Bedmap 2 data can be downloaded from <https://www.bas.ac.uk/project/bedmap-2/>.

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