A review of Sensitivity of Northern Hemisphere climate to ice-ocean interface heat flux parameterizations

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1 Summary

This paper presents a study of the impact of three increasingly realistic parameterizations of the ice-ocean turbulent heat flux (ice-bath, 2-equation,3-equation) on the seasonality of Arctic sea ice (thickness and concentration) and the climate system in general, using four different models with increasing level of complexity (SIM, CICE, MPIOM, COSMOS). In the ice-bath model, the top ocean grid cell is simply fixed at freezing temperature. The 2-equation and 3-equation models have a linear dependency on the friction velocity and the temperature difference between ocean mixed layer and its interface with the ice. Their difference lies in the definition of the interface temperature: the freezing temperature of the top grid cell of the ocean or the freezing temperature of the water at the interface which depends on the ice bottom melting rate. Results show that the simulation of the seasonal cycle in sea ice thickness and concentration improves with the complexity of the model. Results also show that the spatial distribution of sea surface temperature is insensitive to the treatment of the ice-ocean turbulent heat flux in winter and summer, except at the ice edge in summer for the 3-equation model. In the Arctic Ocean, the 3-equation model leads to cooler deep waters and saltier waters over the whole column. This results in a stronger NAO and AMOC. Based on these results, the authors argue for the importance of a realistic parameterization of ice-ocean heat exchange.

The paper presents an insightful and detailed analysis of the impact of different treatment of the ice-ocean turbulent fluxes on Arctic climate simulation. The introduction, however, lacks details about the relevance of the study and how it fits in the context of previous studies, as well as with the presentation of the three parameterizations. The paper is generally well organized and clear but it should be proof-read for English grammar – particularly after the Introduction section. We recommend that the paper be accepted for publication after the comments below have been addressed.

2 Major comments

a Abstract: "...a similar turbulent heat-flux parameterization as (2) but with the temperature at the ice-ocean interface depending on ice-ablation rate". The general reader will not appreciate this sentence early in the paper, i.e. before having read the rest of the paper. An explicit reference to the three-equation model should be included here.

b Abstract: The abstract should report on all key results. As it stands, only results from Model (3) are reported and one wonders why option (1) and (2) were considered in the first place if they don't deserve a line in the abstract.

c l19, Introduction: The same Schmidt et al., 2004 reference is used for the simplest parameterization (fixed Tocn) and also for the most complex parameterization (3-eqs model). This is confusing. The contribution of Schmidt et al. is the system of 3-eqs to solve for the interface temperature, not the fixed Tocn at freezing point. Another (earlier) reference should be used for the fixed Tocn parameterization.

d The authors should describe how their work fits in with the existing literature in both the Introduction and Discussion. Tsamados et al., 2015 examines similar questions. How do the results of this study compare with those of Tsamados et al? What is learned from this study that was not known before?

e The background provided in the introduction is well written. However, the overall motivation for the study and its relevance to the field is not clearly stated. A discussion of how different climate models currently simulate ice-ocean heat fluxes would go a long way in addressing this point. This is stated at the end of the paper. It should also be stated earlier in the paper.

f The presentation of results from the ice-bath parameterization should be motivated given that the authors states that it is "incompatible with observations" (l.24)?

g l.45. The introduction should include a discussion of the different ways of calculating the ice-ocean heat flux for all three methods. This is only clarified in section 2. This should also come earlier in the paper, in section 1.

h l.170: Ideally, the experiments would be done with the same GCM, subsequently removing components to reduce the level of complexity. As it stands now, the CICE model has an ITD, when the GCM (COSMOS) does not. The same comment applies for the forcing that changes between models. This would facilitates the comparison of simulations with different model components. As it stands now, we are left wondering how much of the difference in behavior between models is due to the different model components and forcing rather than the ice-ocean turbulent flux parameterization. GCMs typically has this functionality. If COSMOS does not have this capability, it should be acknowledged, and this caveat should be mentioned.

i Section 2, l92-110: The discussion of the methods, the caveats related to each method, and how each method relate to previous studies should be streamlined. References should appear in parentheses for clarity and text that does not pertain to the Heat Flux Parameterisations should appear in other sections (e.g. Discussion).

j l.206. The temperature at the interface for SIM-icebath is described here; yet it is not used in ice-bath. Are T_f and $T_{interface}$ from Equ. (2) and (3) for the ice-bath and 2-equation parameterizations (respectively) always the same? Does T_f for the ice-bath parameterization also depend on salinity? If so, that would also make it a "2 equations" problem. The differences between each "temperatures" in all three parameterizationa should be described more clearly. A table with the formulas and temperatures used in each method would help clarify this issue.

k l.99 Why is the sensitivity of the model to the parameter R tested giventhat "R=35 best describes reality"?

l Section 4.1: This is where the authors examine the sensitivity of the choices described in Section 2 using an idealized model. A more detailed discussion of these results (along with additional figures) should be included in this section.

m Section 5: One question that keeps coming up in the readers mind while reading the results section is: did the model become more realistic as a result including a more realistic ice-ocean heat flux? Sometimes, especially in complex models, fixing one thing can often expose other problems, perhaps related to model tuning. With respect to this, the authors might consider describing how the results using the most realistic ice-ocean flux parameterization compare with observations. Sea ice thickness is tricky to measure, but Labe et al., 2018 could be a good start (They compare CESM, which uses CICE, to PIOMAS data). For the ocean surface, a qualitative comparison could be made to Peralta-Ferriz and Woodgate 2015, which is a nice study looking pan-Arctic ocean observations over the past 30 years.

n l218 and lines below. l.218 should read "difference in annual mean ice thickness increases with friction velocity". The same form should be used for l220 and other instances that appear below: e.g. "... the larger the deeper our mixed layer is". The same comment for "mixed-layer and atmospheric temperature", and again for "ice concentration and ice thickness".

Line 225: Do not word “explicable” should not be used in this context. Use instead: “This is explained by the fact that..”

3 Minor comments

a l.9, abstract. ‘The most realistic representation”. This is vague. It should reference parameterization (1), (2) or (3) above.

b l.49-51. grammatical errors.

c l.53-57. This sentence should be broken into more than one sentence for clarity.

d l.71. T_{mix} is defined as the ocean temperature. The vertical level (e.g. first layer) should be stated since the temperature is not constant in the mixed layer of CICE.

e l.80. The freezing-point equation for seawater should be written explicitly.

f l.100. typo: salt and heat are transported almost equally efficiently.

g Section 3: There are several instances where numbers appear in the text or within equations without references. The references should be added.

h l.121. T_s is the ICE surface temperature. This should be defined. T_{bot} should also be defined.

i l.116. The acronym MPIOM should be defined when it first appear.

j l.127: A reference for k_i should be given. Is this measurement really precise up to two decimal places?

k l.130 The Notz et al. and Maykut and Untersteiner references should appear above on line 129, i.e. before Fsw nd Fother is introduced.

l (5), l.130,l.131, l.135. The number in the equations should be replaced by symbols; the ”$\times$” should be removed and the numerical values should appear in the text, for the sake of clarity: (eg. 5.67×10^{-8} would be σ)

m l.126. The bulk freezing temperature of the ice should be defined.

n l.130. Numbers should be added for equations that appear after (5).

o l.147: “the so-called CCSM3 set-up”. This is not a commonly know term. The set-up should simply be defined. There are a lot of acronyms in the paper that are used and not defined. Acronyms should be defined when they first appear.

p l.153 Is the mixed-layer salinity held constant in all the models? This would have an impact on the freezing temperature, and therefore on the heat flux parameterizations. This should be discussed (though not necessarily in the methods section).

q l.154 What climatological dataset is being referred to exactly? The reference to the dataset should be included here. The time period over which the climatology was calculated should also be stated.

r l.166: The ice distribution parameter must be described. The paper should be stand-alone.

s l.188, l.193: “We start with” is used twice.

t l.191: ”which is set to either 35 or 70”. This is redundant as the simulations are listed above.

u l.192: ”no more changes from one year to the next.” What were the initial changes?

v l.202: Why is the mixed-layer warming more slowly in SIM-2eq and 3eq? Is it that the ice cover is lost earlier when the sun is higher over the horizon? This should be stated.

w l.208: The word “faster” should be used instead of “stronger” when describing ”ice melt”.

x Section 4.2: This subsection consists of a single paragraph that is approximately one page long. It should be broken into several paragraphs for clarity.

y l.240. The student’s t-test should be more clearly described. Is it testing that the results from two different parameterizations are significantly different?

z Figures: Units and labels should be included on all figures.

Fig.1. Units are missing.

Fig.3-4. A larger font size for longitudes and the colorbars should be used.

Fig.6-7. Units are missing for depth on the y axis.

Fig.6-7. The seabed should appear in grey or a different color to differentiate it from zero in the water.

Table 1: The parameter R should be defined/included in the Table caption. Currently, it is only defined later on l.192

References:

- Tsamados, M., Feltham, D., Petty, A., Schroder, D. & Flocco, D. Processes controlling surface, bottom and lateral melt of Arctic sea ice in a state of the art sea ice model. Philos. Trans. R. Soc. A 17, 10302 (2015).
- Labe, Z., Magnusdottir, G. & Stern, H. Variability of Arctic sea ice thickness using PIOMAS and the CESM large ensemble. J. Clim. 31, 3233–3247 (2018).
- Peralta-Ferriz, C. & Woodgate, R. A. Seasonal and interannual variability of pan-Arctic surface mixed layer properties from 1979 to 2012 from hydrographic data, and the dominance of stratification for multiyear mixed layer depth shoaling. Prog. Oceanogr. 134, 19–53 (2015).

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