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Interactive comment on "Improving the WRF model's simulation over sea ice surface through coupling with a complex thermodynamic sea ice model" *by* Y. Yao et al.

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Currently the regional climate models (RCM) and the operational numerical weather prediction (NWP) models applied simple sea ice scheme as boundary module. Any research activity toward better representation of sea ice scheme in RCM and NWP deserves attention and encouragement. I believe work done in this MS belongs to this category. Overall, I see descriptions of work are clear and easy to follow. However, the analyses of the results suffer various kinds of structural and technical problems and some proper physical interpretations are missing.

We thank the Anonymous Referee #2 for the comments. Your time and effort is highly appreciated.

Substantial revisions have been made to the manuscript according to the comments. The structure of the manuscript has been adjusted and more detailed information has been added to the manuscript. We have included more analyses on the physical interpretation for the results simulated in both offline and online simulations.

I can tell authors made a lot of review in this section, but still I would like to see authors provide a more explicit and clear motivation of this study, i.e. why do we need to improve sea ice scheme in WRF? What are the expected and added values of WRF overall?

We thank the referee for this helpful suggestion.

The WRF model has been widely used in polar researches but it only incorporates a simple sea ice scheme. Previous evaluations have found that this simplicity can lead to a problem of energy imbalance in simulating the sea ice. Such an energy imbalance would limit the application of WRF model for regional climate simulation over polar region. To made the WRF model more suitable for polar climate simulation, there is a need to improve the sea ice scheme in WRF. Due to the large number of WRF users, the development work on the WRF model would benefit a wide range of researches.

We have rephrased the presentation of the motivation of this study in the revised manuscript.

Section 2.3 presents setup for online simulations and a study domain is given as Figure 2, while Figure 1 is presented after Figure. 2. This is quite odd. I suggest authors revise the structure of the MS and make the text flow more smooth and logic.

We thank the referee for the suggestion.

We have adjusted the structure of the manuscript. The section on the setup for online *simulation has been fitted into the section on online simulation.*

Why Noah provided cold bias of ice temperature? Authors stated ”the snow depth simulated in Noah is considerably higher than the observation, which can lead to cold bias in the snow layer and upper part of sea ice” But what could be the physical reason behind this phenomenon? When snow layer is thick, it will introduce strong insulate effect so the ice temperature should be warmer.

We are sorry for the inappropriate statement here. The thicker snow would warm the sea ice, but this only showed small impact on the result of this simulation. We performed a sensitivity experiment in which the snow depth and surface temperature in Noah is specified with the SHEBA observation. The bias still exists in this simulation, implying that the snow processes do not play a role in the underestimation of sea ice temperature.

We've found that the cold bias in winter is mainly caused by the imbalance of energy in sea ice. Noah does not include the ablation and accretion processes and the sea ice thickness in Noah has to be specified. The imbalance of energy in sea ice happens when the ice thickness changes. Since Noah applies the sigma-coordinate for the grid system, the temperature at each sigma level will not change when the thickness changes. Ice temperature at a specific depth is derived from the interpolation between two sigma levels. When the ice thickness changes, the ice temperature at a specific depth changes because each sigma level will represent a different ice depth. As a result, the ice temperature at a specific depth would become colder when the ice thickness grows thicker and become warmer when the ice thickness becomes thinner.

The explanation of the cold bias in Noah has been added to the revised manuscript.

How snow is simulated in Noah and HIGHTSI? What kind of external forcing both models applied? Since manuscript targets geoscientific model development, some more modelling technical details should be given.

GMDD

8, C4105–C4112, 2016

We apologize for the ambiguous statement in the manuscript.

Noah uses a single layer snow and assumes its depth not to below 0.01 m. HIGHTSI has multiple layers for the snow and number of snow layers is 10 in this study. Noah fixed the snow density at 0.3 kg/m^3 and this lead to an overestimation of simulated snow depth in winter. HIGHTSI, on the other hand, can well simulate the snow depth.

The surface pressure, 10 m air temperature, humidity and wind speed, precipitation, downward longwave and shortwave radiation, and ocean heat flux from SHEBA are used to drive the two offline simulations.

We have added more detailed information on the snow simulation in both Noah and HIGHTSI. And a table summarizing the differences between Noah and HIGHTSI has been added in the revised manuscript. Details of the external forcing have been added and a paper describing the forcing dataset has also been cited in the revised manuscript.

I feel that early section "model setup" should be fitted here for better clarity.

We thank the referee for this suggestion.

The early section "model setup" has been fitted here.

Authors claim "Both WRF-Noah and WRF-HIGHTSI overestimate the sea ice albedo due to the simulation of a too early snow melting over sea ice and the lack of melt-pond effect in summer. While the empirical estimate of sea ice albedo in WRF-Noah is better tuned for the summer sea ice in the Arctic (Bromwich et al., 2009), the higher upward shortwave radiation in WRF-HIGHTSI results from the overestimation of sea ice albedo"

Are you sure about this? I don't quite understand why "overestimate the sea ice albedo due to the simulation of a too early snow melting over sea ice" When snow melts it will

GMDD

8, C4105–C4112, 2016

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trigger the positive feedback mechanism, i.e. melting will reduce the surface albedo and further enhance the melting. So too early snow melting will make the surface albedo smaller, why overestimate?

We apologize for the error here. It should be "the simulation of a too late snow melting" due to the underestimation of the downward radiation. The too late snow melting caused the overestimation of the albedo.

This statement has been revised. The word "early" has been changed to "late".

In section 2.2 authors started "the sea ice albedo scheme used in HIGTHSI is the same as the "CCSM" scheme used in the Community ICE Model (CICE) model (Collins et al., 2006). This scheme empirically estimates the albedo based on surface temperature, surface air temperature, snow cover and ice thickness" It seems to me that HIGHTSI applied quite advanced surface albedo scheme. Here you claim such albedo scheme overestimates the sea ice albedo?

Yes, the CCSM albedo scheme used in this study does not include the input of depth of melt pond, thus it only considers the effect of melt pond implicitly through its relationship with surface temperature.

We have added the relevant demonstration of melt pond treatment by the CCSM albedo scheme to the manuscript.

Why apply a prescribed ice thickness for WRF-HIGHTSI? Is this because you run the WRF-HIGHTSI for 2D domain? What happen if you let HIGHTSI to calculate the ice thickens or is it even possible to do like this?

We are sorry for the unclear presentation here. The WRF-HIGHTSI does calculate the ice thickness change. And the integral interpolation method which is energy conservative is also utilized at each integration step. These are the advantages of HIGHTSI and they are included in the WRF-HIGHSI to better resolve the energy balance in sea ice.

However, HIGHTSI only considers the ablation and accretion processes and may give rise to a biased result when neglecting the effect from lateral flux of ice mass. To reduce this bias, a further step that will correct the simulation of thickness with PIOMAS data is performed.

We have added the above explanation in the revised manuscript.

A prescribed ice thickness in RCM or NWP models is part of the current simplicity and this is a challenge needs to be solved in model development. The current work gives me an impression that WRF-HIGHSTI has a limited value toward big improvement of this aspect even though HIGHSTI is capable to simulate the ice thickness. Can authors provide any recommendations on how to improve this part in the future WRF-HIGHTSI development?

We agree with the referee on the point that the treatment of ice thickness is an important issue in current RCM and NWP models. Sensitivity experiments with different sea ice treatments were performed in this study. Based on the results from the sensitivity experiments, we have found that the HIGHTSI can represent the tendency of thickness variation. But the result is dependant on the initial guess of the sea ice thickness, especially for the perennial sea ice. For seasonal ice , the performance could also be limited if dynamic process played a role.

We have addressed the above explanations in the revised manuscript. For the future work, we recommend the development of an air-sea-ice coupled modelling system which can include the ice dynamic processes and interactive ocean component.

In various parts of the MS authors specifically argued the importance of ice dynamics to the change of sea ice thickness. What is the role of ocean? How important the ice-ocean interaction can affect the sea ice thickness?

We thank the referee for the comment.

GMDD

8, C4105–C4112, 2016

The ocean heat flux can influence the basal accretion and ablation processes of sea ice. And the ocean can influence the drift of sea ice. Ice-ocean interaction is important for a realistic simulation of sea ice thickness. For example, the sea ice thickness over Bering Sea can be significantly influenced by the ice-ocean interaction. Previous researches have found that the dynamic and thermodynamic processes have opposite signs in the thickness tendency and nearly cancel each other.

We have added the relevant presentations on the role of ocean in the revised manuscript.

In one model experiment (THERM), where sea ice thickness was calculated by HIGHTSI, what was the ocean boundary condition applied in this model experiment setup? Why WRF-HIGHTSI in this case can provide smaller bias for seasonal sea ice but larger bias for the annual ice thickness? A better air-ice coupling should make the atmosphere boundary layer (ABL) forcing more accurate for ice modelling, so I expect HIGHTSI yields better ice thickness

We thank the referee for the comment.

Ocean heat flux from PIOMAS is used as the ocean boundary condition.

Besides the calculation by HIGHTSI, the simulation of sea ice thickness also relies on the initial guess that derived from the empirical estimation. For seasonal sea ice, the initial guess is close to the observation in autumn but becomes smaller than the observation in winter. THREM could provide a better result than PARAM due to the introduction of thickening trend during winter. For perennial sea ice, the initial guess is already thicker than the observation. The thickening trend introduced by HIGHTSI would further enlarge this positive bias. Thus a better simulation of perennial sea ice thickness might be possible in HIGHTSI if a more realistic initial guess is provided.

The above explanations for the bias in thickness simulation have been added to the revised manuscript.

GMDD

8, C4105–C4112, 2016

The figure presentations are in general good, but the figure captures can still largely improved, e.g. in Fig. 7 (left) (mid) (bottom) expression is difficult to understand.

Captions have been revised for Fig. 5 and Fig. 7 in the revised manuscript.

Figure 2 shows the model domain, while the results in Figure 5 and 7 have apparently a different domain why?

We apologize for the error.

The error in Figure 2 has been revised in the revised manuscript.

Authors applied a lot acronyms, it would be better to specify them in a Table.

We thank the referee for the suggestion.

We have added a table summarizing the acronym and setup of each sensitivity experiment in the revised manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10305, 2015.

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8, C4105–C4112, 2016

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