

Reply to the public justification

Sven Karsten

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We thank the reviewers and editor for further suggestions that greatly improve the manuscript. In the following each of the remaining issues is addressed by first repeating the remark in italics, followed by our reply and the changes performed for a revised manuscript in red.

“1. Missing conclusion regarding the added value of the exchange grid

You have added a paragraph in the conclusion insisting on the flexibility of the exchange grid approach. However, I think that one important conclusion, i.e. that calculating the fluxes on an exchange grid does not improve the results compared to calculating them on the ocean grid, at least when the ocean is much finer than the atmosphere as in your case, should also be stressed.”

We added two sentences to the conclusions (lines 389-392) stating that there is no apparent improvement for mean sea surface temperatures and that the impact of the differences in the 95-th percentile is still unknown.

“2. Main first comments of Referee # 1.

I think you did not considered these comments that are :

• 1/ « In this coupling scheme, there is the assumption that even above surfaces of variable temperature, the atmosphere is homogeneous, on the scale of a model grid box, from the first level of the model to the top. This is a very strong assumption, and in fact a priori false. You only have to look at the clouds above a free water-sea ice front to that at least the planetary boundary layer may have very different vertical structure within one atmosphere grid box. »”

We apologize that we have overseen these comments in the first revision. To the first comment, we now add some sentences to lines 172-174 in section 2.3.1 and to the second comment, we modify the text in section 2.3.2.

“• 2/ « The transfer coefficients ch and cm are sent by the atmosphere (Fig. 4). They are not calculated by the flux calculator. It is quite possible in reality to have stable air above the ice and unstable air above the open water. This yield very different transfert coefficients above the two surfaces. This should lead to very different flux compare to what the flux calculator computes. Note that if the calculations of these coefficients involve iterations in the atmosphere, this can indeed be complicated to compute them in the flux calculator. »

Referee # 1 wrote : « This (sic) two limitations should be explained to the reader, with if possible some descriptions of the potential impact, and technical constraints that explained the chosen algorithm. » and I don't see anywhere where this has been done.

Also the method does not separate scales. It treats an ice front separating an ice-covered area from an area of open water (large-scale heterogeneity) in the same way as ice fractured by leads (small-scale heterogeneity).

Please add something to address these remarks.”

We elaborate on the insensitivity of the transfer coefficients to the surface heterogeneity in the beginning and the end of section 2.3.2 (starting from line 204) in the revised manuscript.

“3. Paragraphs lines 112-15 and lines 226-228 The sentence « The calculation of some fluxes cannot be reasonably implemented via bulk formulas that take only surface fields as input » sounds awkward to me, as does the sentence « do not only simply depend on surface fields » line 227 as noticed by Referee # 2.

Please consider rephrasing lines 112-113 more clearly with something like "However, the calculation of some fluxes does not depend only on surface fields and therefore are out of scope of the flux calculator capabilities. In particular, ..."

On line 114, consider changing "will still be calculated" with "are calculated".

Please consider rephrasing lines 226-228 as: "As stated above, the downward radiation fluxes $\phi_{rad}(x,y,t)$ (i.e. shortwave and longwave radiation) do not depend only on surface fields; they are thus entirely calculated by the atmospheric CCLM model and then passed through the flux calculator to the ocean model." ”

We change the mentioned lines as suggested in the revised manuscript.

"4. Paragraph lines 239-242 The first and last sentences of this paragraph are redundant. Please consider merging them into "Note that all the presented formulas for fluxes might be changed, e.g. to involve different methods, within the flux calculator source code without further changing the model codes." Also, I think you should take the opportunity there to stress the fact that this is not true for the transfer coefficients ch and cm , which are calculated and sent by the atmosphere, see my remark 1. above."

We change the mentioned lines as suggested and stressed the particular role of the transfer coefficients in the end of Sect. 2.3.2 *Flux formulas* in the revised manuscript.

"5. Paragraph lines 257-261 With your latest modifications, this paragraph does not read well. Please consider rephrasing for something like: "In the atmosphere, radiation fluxes are updated every hour; the timestep for the physics is 150s and is internally further subdivided to account for fast modes in the dynamics. The ocean model time step is 600s. The time step of the coupling between the two models is also set to 600s to temporally resolve strong wind gusts (Davis and Newstein, 1968). An investigation on the impact of different coupling time step sizes is planned for future work.""

We change the mentioned lines as suggested in the revised manuscript.

"6. Reference to the appendix For clarity, change the references to the appendices using the word "appendix" and not "Sect." and give the number of the appendix. For example, line 89, "Sect. B" should be "Appendix B" and at line 192 "Sect. D in the Appendix" should be "Appendix D2"."

We change all cross references to the appendix using the word "appendix" in the revised manuscript.

"7. Line 153 Consider changing « ... is employed, however, ... » for « ... is employed; however, ... »"

We replace the comma by the semicolon in the revised manuscript.