The Cryosphere Discussions

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Comment on tc-2022-84

Jean Sterlin (Referee)

Referee comment on "Arctic sea ice and snow from different ice models: A CICE–SI3 intercomparison study" by Imke Sievers et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-84-RC1, 2022

General comments

This paper presents results from the new SI³ model and the CICE model using NEMO4 as a modelling framework. The authors performed two simulations based on the same experimental design: one using CICE and the other using SI³. The simulations share the same initial conditions, atmospheric forcing, and lateral boundary conditions. This approach allows investigating the conceptual differences between CICE and SI³ and the impact of such differences on the representation of the Arctic sea ice. In this study, SI³ simulates a thicker sea ice cover than CICE in most the Central Arctic. The ice concentration from CICE is larger than the one from SI³ in the marginal ice zone. The authors noted substantial differences in the modelled snow thickness between the two simulations. Eventually, the authors propose mechanisms to explain the differences in the simulated sea ice properties.

The representations of the snow, albedo and form drag in the models are suggested as the origin of the differences between the two simulations. It is not clear from the manuscript how these parameterizations can explain the results. How do the snow, albedo and form drag parameterizations work in CICE and SI³? How can their implementations explain the differences in the sea ice properties? How do the results compare with other model studies? Many aspects of the sea ice models can explain the results presented here (see Tsamados et al. 2015 https://doi.org/10.1098/rsta.2014.0167 and Hunke et al. 2010 https://doi.org/10.1016/j.ocemod.2010.05.004). Most of these aspects have not been examinated by the authors. Therefore, I am not convinced by the conclusion of the manuscript.

The setup of the sea ice and ocean models is not clearly outlined in the manuscript. The authors should include a description of the namelist parameter for CICE and SI³ and add a

justification if they deviate from the standard namelists. I suggest including a table citing the sea ice parameterizations used by the two models. This table will help the reader understand the key differences between the two models. I also advise adding the reference namelists for CICE, SI³ and NEMO in the appendices.

The overall structure of the manuscript is clear. I advise adding a separate section for the experimental design in the method section. The abstract is clear and concise. The conclusions reported in the abstract are more assertive than the conclusion. Although the language is fluent, the manuscript needs a more precise and rigorous style. Commas are missing, acronyms are undefined, words are misspelt, sentences are ambiguous. I tried to correct as many errors as I could. There are likely many others left. I encourage the authors to improve the quality of the writing.

Specific comments

- The authors should discuss the tuning of the albedo in further detail, and ideally, they should assess the albedo value. The tuning of the albedo comes first in explaining the differences between the simulations. The manuscript does not tell whether the albedo schemes from CICE and SI³ or the tuning of the albedo causes the differences in snow and ice thickness. In addition, melt ponds and their relation with snow should be discussed as they play an important role in the surface albedo.
- The modification of the ice rheology to account for tensile strength is likely to impact the sea ice velocity and the ice thickness in the Arctic. Can the authors provide the reader with more information on the parameterization of landfast ice in the two models and the expected outcome of these modifications?
- The variable drag coefficients in the CICE run are different from the results shown by the modelling community. Moreover, it is unclear whether the differences in ice properties are caused by the variability of the form drag coefficients or by the differences between the constant drag coefficients used in the SI³ run and the mean values of the drag coefficients from the CICE run. Lastly, SI³ has a variable formulation of the atmospheric drag coefficients over sea ice. Why not using this formulation to compare against the parameterization of Tsamados et al. 2014?
- Did the authors consider sea ice drift products for the analysis, for instance, the data from the International Arctic Buoy Program or the Polar Pathfinder sea ice motion vectors? Ice motion is relevant because of the formulation of the drag coefficients and the parameterizations of landfast ice (tensile strength).
- The approach chosen by the authors to compare the two sea ice models is not entirely clear to me. Are the authors aiming for a fair comparison between the two models by selecting similar sea ice parameterizations? Or are the authors keeping the models as close as possible to their standard configurations with only minor changes to the namelist parameters? I feel that a fair comparison is needed as future modellers may choose between CICE and SI³ based on the conclusions of this study.
- Have the authors examined the effects of recent developments in the sea ice models, notably the floe size distribution in CICE and the parameterization of the lateral ice melt?

Technical corrections

In the co-author list: Aalborg University,A.C. -> Aalborg University, A.C
line 3 CICE -> the Los Alamos sea ice model (CICE); SI3 -> Sea Ice modelling Integrated Initiative (SI³); It is worth mentioning the NEMO framework as well: Nucleus for European Modelling of the Ocean (NEMO)
line 3 "In the this study two commonly" -> "In this study, two commonly"
lines 7-9 These lines in the abstract are more assertive than the conclusion. Please try to harmonize them.
line 9 "forcing is equal": The meaning of "forcing" appears to change along with the manuscript. Here, do you mean the atmospheric forcing? Or the same model setup?
line 11-23 Try to keep one idea per paragraph to ease the reading.
line 13 "toward seasonal the variability" -> "toward seasonal, the variability"
line 15 You should make a clear distinction between "forecast" and "research" activities.
line 17 I somewhat disagree: In-situ data are also commonly used to validate sea ice models. See the International Arctic Buoy Programme (IABP). Moreover, the term "model systems" is too general in this context. Weather balloons are an important aspect of the validation of climate models, for example.
line 20 "sea ice extend has retreated" -> "sea ice extent has decreased"
line 21 Hindcasts do not aim to predict past changes.
line 21-22 I don't get the last two sentences of the paragraph. What do you mean?
line 24-34 You should improve this paragraph by citing previous work examining the effects of initialisation and atmospheric forcing. These are not limited to Dirkson, Day, and Wang.
line 24 "These differences" recall elements discussed in the previous paragraph, but which differences are you referring to? Some people browse through manuscripts by reading the first sentence of the paragraphs. Try to present the main idea of your paragraph in the first sentence to ease the reading.
line 25 Can you make an exhaustive list of the external parameters? The word parameter may be misleading. Could it be the initial conditions and the boundary conditions (lateral boundary conditions, atmospheric forcing, etc.) of the model? These are external as they do not depend on the model algorithm.
line 26 Explain what is the atmospheric forcing here. Wind stress (momentum) is missing. Radiation supplies heat. Consider using "heat, mass, and momentum", or even better, do a list of the forcing terms of the model (SW, LW, latent and sensible heat, wind stress, solid and liquid precipitations in case of NEMO).
line 28 low difference in what quantity?
line 28-29 Is that from Wang et al. as well?
line 30 "when cold starting a model run the initial SIC" -> "when cold starting a model run, the initial SIC"
line 31 Some use sea ice reanalyses to prescribe the initial conditions. SI³ offers such a possibility (see namelist parameter nn_iceini_file).
line 31 We don't know from the sentence if the initial sea ice concentration and thickness are set as constant or if the atmosphere and ocean are constant.
line 35 These internal parameters should be described in the method section. Can you provide an explicit definition of internal parameters, and then give some examples relevant to your study?
line 36 discretized -> parameterized
line 36-37 Kiss et al. (2020) refer to a group of authors: Kiss et al. (2020) compare ... and find that ...
line 38 girds -> grids
line 38 such as
line 38 "Another example of internal cause for resulting": I don't understand this sentence
line 40 "an a" -> "and an"?
line 39-40 unclear
line 42 To what problem?

line 44 Is the albedo calculated from the radiative forcing?
line 45 What is skill?
line 46 Not clear. Arctic sea ice is highly variable
line 47 "the external and the internal parameters and forcing" : forcing is an external parameter if I understand well. Maybe exclude the term "forcing" to avoid confusion. Consider removing the extra "and" from your list.
line 47 Reality and ideal are words subjects to interpretation. Does realistic mean as close as possible to observations? Or is it a comprehensive representation of sea ice processes?
line 48 biases
line 48-49 Unclear. Do you mean: "only the external forcing can be set to the same"? Or "study the internal parameters only"?
line 50 You need to rephrase your scientific question to be clear: what do you want? At this stage, I don't understand what type of variability you are referring to. Line 50 interferes with line 66. It is better to formalize the scientific question in one paragraph.
line 55 It would be nice to introduce NEMO, CICE + Icepack and SI³ before the scientific question: why do you select these models?
line 56 Unclear: did you couple CICE to NEMO because CICE can be coupled to an ocean model or run in stand-alone mode?
line 56 Try to better highlight the novelty of your work. Why is it important to compare CICE and SI³ using the same modelling framework?
line 58-65 Should be addressed before the scientific question.
line 59 "ensures": albedo doesn't ensure anything
line 60 "During the last year" -> "During the last years". Snow has always been an important topic for the climate community.
line 63 "understanding". You were talking about observations. What are the new understandings?
line 64 Is there more and more snow?
line 66-79 Merge this paragraph with the one line 50.
line 65 Have you considered sea ice drift datasets for your analysis? The data from the International Arctic Buoy Program or the Polar Pathfinder sea ice motion vectors? Ice motion is relevant because of the formulation of drag and the ice rheology (isotropic tensile strength).
line 70 "two" -> "2" and "section" -> "Section"
line 71 "whereas" introduces a contradiction
line 71 do you present the differences between the simulations or between the two models?
line 77 Please define NEMO and the ocean model OPA. Define the acronyms (NEMO, OPA, ERA5, GLORYS, TPXO). This section lacks some essential elements. For instance, "OPA is a finite-difference, hydrostatic, primitive-equation model". It is common to think of NEMO as an ocean model. However, NEMO is a framework for ocean modelling.
line 89 SI³ and CICE have many aspects in common besides the EVP. Some aspects depend on the model setup. Please discuss these common points between CICE and SI³ (ITD, rheology, thermodynamics, advection scheme, etc.).
line 90 Coupled to OPA using NEMO as a modelling framework.
line 91 multi-category for the ITD? Snow or ice layers for the thermo-dynamics?
line 93 The description of the model runs could be put in a separate section "Experimental design" as they are needed in both Sections 2.1 and 2.2. In the new section, you could describe the horizontal and vertical grid of the model, the forcing method (CORE-II ?), the time period of the runs, how many runs, etc. Sections 2.1 and 2.2 would describe the ocean and the sea ice models, respectively. Although your manuscript refers to the study of Hordoir et al. (2022), it is worth recalling the essential points of your experimental design. In the manuscript, I assumed CICE and SI³ standed for simulations ran with CICE and SI³. I don't think this is stated anywhere in the manuscript. CICE and SI³ refer to the models and the simulations. It is sometimes confusing.
line 94 Are two years enough to spin up the models? Do you expect a spun-up state for the ocean or only the sea ice?

line 98 The acronym of SI³ needs to be defined before
line 102 which which -> which
line 102 Can you list the different surface types? Do you consider the contribution from melt ponds or do you adjust the ice albedo to account for puddled effects on sea ice?
line 106 Icepack vertical column model should be cited in this section.
line 107 Change in thickness due to mechanical processes (ridging and rafting)?
line 110 "flow size" -> "floe size". The formulation of Tsamados depends on many other variables: SIT, concentration and volume of deformed ice, floe draft, or melt pond edge height to name a few. It may not be necessary to do an exhaustive list of these variables. Please state the main hypotheses used to construct this parameterization and what form drag contributions are accounted for. Are the atmospheric and ocean stabilities taken into account by CICE in your model setup?
line 111 The reference paper for the form drag formalism is Tsamados et al. (2014). Are we referring to the same work?
line 112 "Delta-Eddington multiple scattering parameterization" should appear somewhere in the description of the scheme
line 112 It seems that you are simulating melt ponds in CICE. How are they parameterized in CICE and SI³? Do they contribute to the atmospheric form drag in CICE?.
line 113 "different processes" is vague. Can you explain what are the differences between the Shine and Henderson parameterization?
line 125 with the help.
line 127 independence to what?
line 134 "time step" -> "period"; time step is more appropriate for models
line 139-140 12.5km -> 12.5 km
line 140 81.5N -> 81.5 °N
line 140 Garnier et al. should be cited at the beginning of the paragraph, ideally.
line 142 Sentence is not clear.
line 144 Stronger pronounced -> more pronounced?
line 146 These differences are small?
line 152 Atlantic sector of the Arctic?
line 154 remove "Here"
line 154 ice edge: how do you define it? Is it a 15% sea ice contour line? Other metrics? Or do you evaluate the ice edge approximately by looking at the figures?
line 154-155 This sentence is very hard to read
line 145-156 Section 3.1 is very hard to read. It is not always clear which simulations or observations you are comparing together. Please quantify and provide estimates of the differences between the two simulations and between the simulations and the observations.
line 158 The monthly SIT from the simulations were interpolated
line 159 was -> were
line 160 CS2SMOS is not an optimal interpolation. It is a data record. I don't understand why the optimal interpolation method prevents using CS2SMOS at latitudes higher than 88N.
line 160 not .. nor -> neither .. nor
line 161 figure -> Figure
Figure 2: define in the caption Sep and Mar. Same for the other figures of the manuscript.
line 163 From Figure 4, the CS2SMOS shows greater sea ice volume than the simulations in early winter for 3 out of 9 years.
line 166 CS2SOMS -> CS2SMOS: check all the acronyms. (e.g. LEGOS). CS2SMOS does not control the sea ice volume. Do you refer to seasonal changes in sea ice volume or changes in sea ice production?
line 168 add commas whenever necessary to avoid ambiguous sentences
line 169 as a reference
line 173 central Arctic -> Central Arctic; is -> are
line 174 more ice concentration?; stronger pronounced -> more pronounced

line 175 It is very hard to read this sentence: an area is not a month and cannot be close to a thickness.
Figure 4 put the labels in the legend in upper case. Consider removing the title and changing the y-axis label to display "Total sea ice volume". This will allow to expand the figure vertically.
line 179 The SIT from CICE
line 180 over all -> overall
line 181 Figure 6
line 182 CICE is not thicker than SI³
line 188 data is the plural of datum, hence should be followed by "were interpolated"
line 190 centimeter?
line 191 mode of the snow thickness pdf
line 194 which season?
line 195 I'm not sure noisy is an appropriate term to describe a pdf. Maybe secondary modes?
Figure 7 On the legend, please indicate Ku/Ka instead of KUKA
line 205 Figure 9.
line 207 Snow blue and Ice turquoise are confusing to me because they look like color names. Can you say "the blue and turquoise curves correspond to the snow and ice volumes, respectively"?
Figure 9 Can you change "Difference Total Area" to "Difference in the Arctic"? Change "central Arctic" to "Central Arctic"
line 228 In the title, do you mean atmosphere-ice drag?
line 228 The spatial distribution of the drag coefficients is rather different from the other modelling studies (see Tsamados et al. 2014, Martin et al. 2016, Castellani et al. 2018, Chikhar et al. 2019). In these studies, the drag coefficients decrease toward the open ocean in most of the Arctic. In Figure 10, the variable drag coefficients from CICE appear to increase in most of the peripheral seas of the Arctic during September. In all three mentioned studies, the drags are maximum along the northern coast of Greenland and the Arctic Archipelago all year round. This characteristic is poorly seen in Figure 10. I suspect important issues related to the sea ice characteristics simulated by CICE. The scheme of Tsamados et al 2014 allows examining the contributions associated with ridges, floe edges, melt ponds and surface skin. Could you use this decomposition technic to present the different contributions from the CICE run? Are your results reasonable compared to the other modelling and observational studies?
line 230 Flow size? Please avoid copy-paste. See comment on line 110.
line 231 This -> These. You should indicate what is the mean values of the drag coefficients simulated by CICE.
line 235 Canada: be more specific. Canada is the second-largest country in the world.
line 236 the both the?
line 239 The drag coefficients act on the turbulent heat and momentum fluxes, which are only a part of the external forcing. Can you explain what you mean by the vulnerability of sea ice?
line 241 Could it be that there are still more uncertainties in the prescribed atmospheric state and the formulation of CICE/SI³ than there are differences between CICE and SI³? You could discuss this result in the light of atmospheric reanalysis evaluations in the Arctic (e.g. Lindsay et al., 2014: https://doi.org/10.1175/JCLI-D-13-00014.1). The modelled sea ice thickness is highly sensitive to the atmospheric and oceanic forcing (Hunke et al., 2010: doi:10.1016/j.ocemod.2010.05.004) and CICE and SI³ have benefited from several collaborations between the two modelling communities (see developments on the EVP from Elizabeth Hunke and Sylvain Bouillon, for example). Can you discuss these elements?
line 242-243 I don't get the meaning of this sentence.
line 247 CMIP models are fully-coupled Earth system models. Here, the model setup is forced by a prescribed atmospheric state. This limits model biases. I'm not sure the comparison to CMIP model is fair within this context. I suggest comparing your results to model studies forced by an atmospheric state based on CICE and SI³/LIM3, if possible.

Lastly, line 247 contradicts lines 244-245 where it is stated that the model outputs from this study are in good agreement with results from other modelling studies.
Figure 10 Do you compare the total drag coefficients (accounting for the boundary layer stability) or the neutral drag coefficients? Can you recall the values of the drag coefficients used for the SI³ run? Correct the spelling of the acronyms for the ocean (oce. or ocn.) and define the acronyms (Sep, Mar, Atm. Ocn. or Oce.) in the caption. I suggest that you use the same panel layout as the other figures: top row for March and bottom row for September.
line 250 Observations of melt ponds indicate a seasonal maximum in the melt pond area during mid-summer and refreezing in late August or early September (See Rosel et al., 2012: https://doi.org/10.5194/tc-6-431-2012). Figure 3 shows the September mean of the SIC. Thus, melt ponds should not impact the observed SIC in September. Usually, observational products have issues estimating sea ice quantities when the open water fraction is high or the sea ice thickness is small. Is this the case for OSISAF?
line 252-258 I don't understand
line 259 spatial
line 260 Figure 4 shows the seasonality of the sea ice volume. The sea ice volume is set by the balance between ice growth and ice losses. You haven't presented results on the sea ice growth or production
line 260 overall
line 260 CICE and SI³ do not grow in winter
line 263-266 What do you mean?
line 266 Figure 4 does not show the sea ice growth and melt. Did you perform an analysis of the sea ice growth and melt?
line 267 than what observed
line 268 Is it possible to check from the simulations this hypothesis? Is it reasonable for the models to melt the snow layer in summer? Can you check the summer evolution of the snow layer and their relation with the evolution of melt ponds, if any?
line 272-273 re-read the sentence
line 273 Can you provide a discussion on the representation of the snow in CICE and SI³? My first guess would be the tuning of the albedo. Melt ponds, if any, are likely to impact the snow layer properties at the end of summer. Hunke et al. (2010) showed that one can adjust the simulated ice thickness from CICE using multiple combinations of parameter values. The tuning of the ice thickness is likely to impact the properties of the snow layer. Can you provide arguments on the importance of the snow layer for explaining the differences between the simulations?
line 276-277 "Even though the models ... there are differences between the model": try to avoid this type of sentence.
line 284 It might help the reader to use the names of the regions instead of the numbers. Figure 1 could show these names for the reader unfamiliar with the Arctic.
line 289 Links -> links
line 291 "In the Lincoln (region just north off the cost of Greenland)": this is the second occurrence of Lincoln Sea in the paragraph (see line 288). This paragraph is very hard to read.
line 291 overall
line 292 Lincoln Sea.
line 293 The study of Tsamados et al. 2015 is more of a sensitivity study than a validation against observations study type. You cannot say that Tsamados et al. 2015 showed improvements in the simulated ice thickness when including form drag. Tsamados et al. 2014 use observations to validate aspects of the form drag scheme. See the links: https://doi.org/10.1098/rsta.2014.0167 and https://doi.org/10.1175/jpo-d-13-0215.1. You can also discuss the impact of form drag on the simulated ice concentration. The effects of the variable form drag coefficients on the ice thickness are less important in the ocean-ice model study of Castellani et al. (2018) than the ones shown by Tsamados et al. (2014, 2015) based on their stand-alone model setup.
line 295 extent? I'm not sure if you mean extent or extend. Please re-read to clarify the

sentence.
line 295 Barents Sea.
line 295 other models do not know about the overestimation of the ice extent in the Barents Sea and Eastern Greenland Sea.
line 297 At this point, I don't know if your model setups of CICE and SI³ are forced by a prescribed oceanic state, or if the ocean model from NEMO is coupled to SI³ and CICE. If the ocean model from NEMO is used, then you cannot argue that the ocean forcing of CICE and SI³ is identical.
line 299 SIS?
line 299 I would be careful with this type of sentence. Some of the parameterizations offered by CICE are still poorly constrained by observations. For instance, the form drag formulation of Tsamados et al. (2014) assumes periodically distributed squared floes to estimate the distances between floes. This has not been shown by observations, yet. On the other hand, the "simpler" form drag formulations from Lupkes et al. (2012) and Lupkes & Gryanik 2015 derive from observations. These parameterizations are available in NEMO.
line 302 please clarify what you mean by ocean forcing
line 305 Could you add a description of the landfast ice parameterizations in your manuscript. Can the tuning of the ice rheology impact the ice thickness in your simulations? In your setup of CICE, it appears that the isotropic tensile strength is null. In your setup of SI³, the isotropic tensile strength appears to be 0.2 (from the namelist), which is quite large compared to the default value of 0.05 suggested in SI³'s namelist. In recent works from Lemieux's team, the isotropic tensile strength is equal to 0.05 (Lemieux et al. 2018 https.://doi.org/10.1029/2018jc014080, Chikhar et al. 2019 https://doi.org/10.1080/07055900.2019.1694859) following the work of Hibler and Schulson (2000). The isotropic tensile strength has a significant impact on the sea ice velocities in the entire Arctic basin. This might explain some of the differences in SIT shown in Figure 6. Could you shed light on the parameterisations of landfast ice in your experimental design and discuss the impacts on the representation of sea ice?
line 316 larger
line 317 which ocean heat transport?
line 319 See comment on line 305. The ice rheology modifications to account for uniaxial and isotropic tensile strength concern all the ice cover.
line 324 Is your model configurations of CICE and SI³ in stand-alone mode?
line 329 only higher SI³ in March?
line 330 SI³ (Figure 3)?
line 334 is out of the scope
line 336 from -> form
line 337 form drag
line 325 I'm not sure you can say that. Are you using a prescribed oceanic state to force SI³ and CICE?
line 337 Castellani also showed that the sea ice thickness is sensitive to the choice of constant drag coefficients (see their Figure 13). Can you discuss the relative importance of the variable drag coefficients and the values of the constant drag coefficient in your simulations?
line 338-340 Albedo is a key parameter in your study that could explain many of your results. These two sentences are welcomed and I encourage you to investigate further the representation of the albedo in your two simulations.
line 341 I'm less knowledgeable on the parameterization of snow in CICE and SI³. Could you provide the reader with more insights on the current parameterizations and on how they could explain your results?
line 342 I am confused by the volume unit Mkm2. What is a special distribution of the differences?
line 345 Consider citing the work of Olivier Lecomte
line 346 Not really. 50% corresponds to the fraction of snow lost to the ocean during ridging and rafting events.

line 358 What is your definition of sea ice extent? NSIDC defines the sea ice extent as the ocean area with at least 15% sea ice.
line 359 I don't think you have presented results on the simulated ice production.
line 364-365 It is important to explain which of Castellani's results you are referring to: is it the effects of using variable drag coefficients or adjusting the constant drag coefficients? Please examine past results on the effects of form drag (Tsamados et al., 2014, Martin et al. 2016, Chikhar et al. 2019, etc.) to support your argument.
line 366 Albedo is important. Can you provide us with more insights on the tuning of the albedo, the schemes used by CICE and SI³, and the representation of the albedo in your model runs?
line 374 Not exactly the same, as it depends on the simulated ice area. I suppose you mean the same precipitation rates.
line 377 I'm not sure you can say that based on your results and your description of the models. It will always be true that better parameterizations will give better results. However, many aspects of the two models may cause the differences in sea ice and snow thickness you have reported. The study of Hunke et al. (2010) (https://doi.org/10.1016/j.ocemod.2010.05.004) and the one of Tsamados et al. (2015) (https://doi.org/10.1098/rsta.2014.0167) can help you investigate these aspects. Since then, the two models have benefited from new developments. See for example the implementation of a floe size distribution or the parameterizations of landfast ice. These developments are capable of leading to differences in the sea ice state between the two models.
Appendices What are the units of the figures? If time allows, add the ice drift vector field on the maps to visualize the Beaufort Gyre and the transpolar drift.