Response to the RC1 #2 comment on “How well do the regional atmospheric and oceanic models describe the Antarctic sea ice albedo?” by Verro et al.

Thank you to anonymous Reviewer #2 for the comments, which helped identify and resolve some issues in the analysis and contributed to improving the overall quality of the article. We hereby respond to the comments point by point.

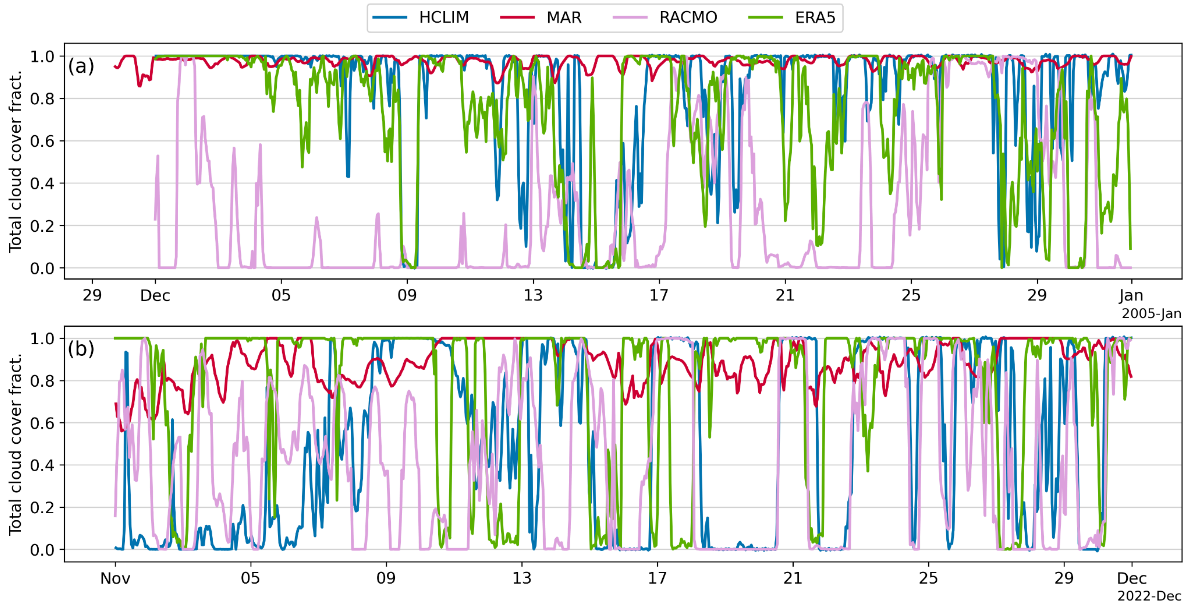
Major comments

1) While I can accept the decision to defer cloud impacts on sea ice albedo to future studies, it is on this point that the manuscript should be clearer. First, the typical difference between the clear and cloudy sky albedo should be noted, using e.g. Key (2001) as a reference. Then, if all data sources in the study are indeed clear-sky only, it should be made very clear how the models are coerced to only provide albedos consistent with clear-sky conditions.

We acknowledge that we should have taken more time to discuss the role of clouds, even if a detailed analysis is reserved for future studies.

We set out to test the modelled albedos against observations and satellite products as is. The albedo time series shown Fig. 4 shows the measured albedo during both cloudy and cloudless conditions, while the high-resolution satellite products were selected on cloudless days.

Some of the models include cloud-modified albedo (MetROMS-UHel, NEMO, MAR), while others do not (RACMO, HCLIM, ERA5). We have added modelled cloud coverage timeseries to Appendix A, and expanded Sect. 4.1:



“We have included total cloud coverage of the models in Fig. A3. There are considerable differences in cloud cover over the ISPOL and Marsden campaign sites between the models, which can only partly be explained by differences in resolution. The snow and ice albedo can be, on average, 4–6% higher under cloudy conditions compared to clear skies (Key et al., 2001). However, only MetROMS-UHel, MAR, and NEMO account for the effect of clouds in their sea ice albedo parameterizations, whereas HCLIM, RACMO, and ERA5 do not include this effect. ”

Furthermore, we have expanded on the secondary effects on the albedo, such as cloudiness and SZA:

“Surface albedo over Polar sea ice is complex, and this study focused only on first-order effects, excluding factors such as the cloud and solar zenith angle dependence of surface albedo, which themselves can be 10% (Key 2001, Gartner and Sharp (2010), Jäkel. 2023). In study cases characterised by significant variation in surface types, such as during the spring/summer season, it is primarily uncertainties in the parameterisation of these surface types that influence the modelled surface albedo, rather than cloud effects (Jäkel. 2023). The effect of using different surface types can lead to a 20–30% difference in albedo, such as when shifting from bare ice to snow-covered ice, as seen in the case of MAR (Fig. 4b). Using a snow albedo parameterization that is not suitable for polar regions can result in differences exceeding 30%, as demonstrated by HCLIM (Fig. 2). While future research should include a more comprehensive evaluation of cloud impacts, as explored by Jäkel et al. (2023) and Foth et al. (2023), there is still room for improvement by refining the aspects of albedo discussed in this paper. “

2) Additionally, For the satellite data, the S2 and LS9 data are evidently clear-sky, though I would appreciate clearer details on the clear-sky atmospheric correction necessary to provide the surface reflectances (yes, actually these data are nadir-view directional snow reflectances – they can well be a good estimate for the view-integrated albedo, but you should be clear on the distinction). And for CLARA-A3, I think that the data there are available for various illumination conditions – which did you use here?

Thank you for your comment. We clarify below the atmospheric correction methods applied to the satellite datasets and the type of albedo used from CLARA-A3:

- Sentinel-2: We used the Level-2A product, which provides surface reflectance (bottom-of-atmosphere) already corrected for atmospheric and topographic effects using the Sen2Cor processor. This includes corrections for aerosol optical thickness (AOT), water vapor, and terrain effects, based on Look-Up Tables generated via libRadtran and adapted from the ATCOR software (Richter et al., 2006). The baseline aerosol model is rural/continental, with atmospheric profiles selected according to scene location and climatology (Main-Knorn et al., 2017).

- Landsat 9: Surface reflectance was derived following the method described in Traversa et al. (2021), using the 6S radiative transfer model (Vermote et al., 1999). Inputs included satellite geometry, date/time/location, a subarctic winter atmospheric model, a continental aerosol model, and visibility or AOT at 550 nm.

- CLARA-A3: As stated in the manuscript, we used the blue-sky albedo product, which represents a weighted combination of black-sky (direct) and white-sky (diffuse) albedo, based on the estimated clear-sky fraction. This choice provides a more realistic approximation of surface albedo under typical illumination conditions.

We acknowledge the importance of distinguishing between nadir-view directional reflectance (as provided by S2 and L9) and hemispherical albedo (as in CLARA-A3). While directional reflectance can serve as a proxy for albedo under certain conditions (e.g., over snow), we have taken care to interpret and compare these datasets accordingly.

3) 4.4 – it's not clear how many drone flights contributed to the histograms in Fig 6? If from multiple days, what was the day-to-day albedo variability in the drone data? Were the flights made always over the same survey grid? Was the weather clear or cloudy – that would also change the observed snow albedo.

The histograms were done during a single flight over each surface type. The measurements were taken between 30 and 50 m over thick ice (~2.1 m, CS) and between 30 m to 70 m altitude over thin ice (~1.2 m, NIS), which are shown on the map on Fig. 1.

We have added more explanations on the drone measured albedo:.

“Figure 6a-b shows the probability distributions of the albedo measured from a drone flying a single vertical profile at an altitude between 30 and 50 m over thick ice and another profile between 30 m to 70 m altitude over thin ice with patchy snow cover. The flight lasted ~10 min, without significant changes in solar zenith angle and cloud conditions.”

Therefore, the distribution of the drone-based albedo data did not include day-to-day variability, and cloud conditions were unchanged during the flights. Furthermore, we have added the mean values of the drone measurements to the sea ice albedo time series of Fig. 4.

4) 4.5. – you can calculate the mean SZA over the study areas as you know the S2 and LS9 overpass times; a first-order estimate for the albedo effect assuming non-melting snow would then easily be available from a lightweight albedo parameterization such as that of Gardner and Sharp (2010), please provide the assessment in the text as a yardstick for the reader.

For L9: SZA = 70.8°, overpass time: 20:21:00.7699389Z, and for S2: SZA = 67.3°, overpass time: 20:05:29.024Z. As suggested, we have estimated the albedo effect of SZA from Gadner and Sharp (2010) and have provided the yardstick albedo effects in the text:

“We can estimate the effect of solar zenith angle on snow and ice albedo between the two dates using parameterisation from Gartner and Sharp (2010). Assuming pure, dry fresh snow with an albedo of 0.9 and a solar zenith angle of 0^0, the albedo reduction due to the solar zenith angle at the Landsat 9 overpass (SZA = 70.8^0) is 0.038. For the Sentinel 2 overpass (SZA = 67.3^0), the corresponding albedo effect is 0.036. For ice and snow with lower albedo values, the effect is more pronounced -- for example, sea ice with an albedo of 0.5 experiences albedo effects of 0.1, for both SZA = 70.8^0 and 67.3^0. However, the difference in albedo between the two observation dates remains small, on the order of 0.01 or less.”

5) Figs 10-12: It is my understanding that CLARA-A3 (not 3A as in some legends) albedos are either 5-day or monthly means, yet here the text refers to CLARA-A3 products from a specific day. Did you recompute your own daily version based on provided raw data?

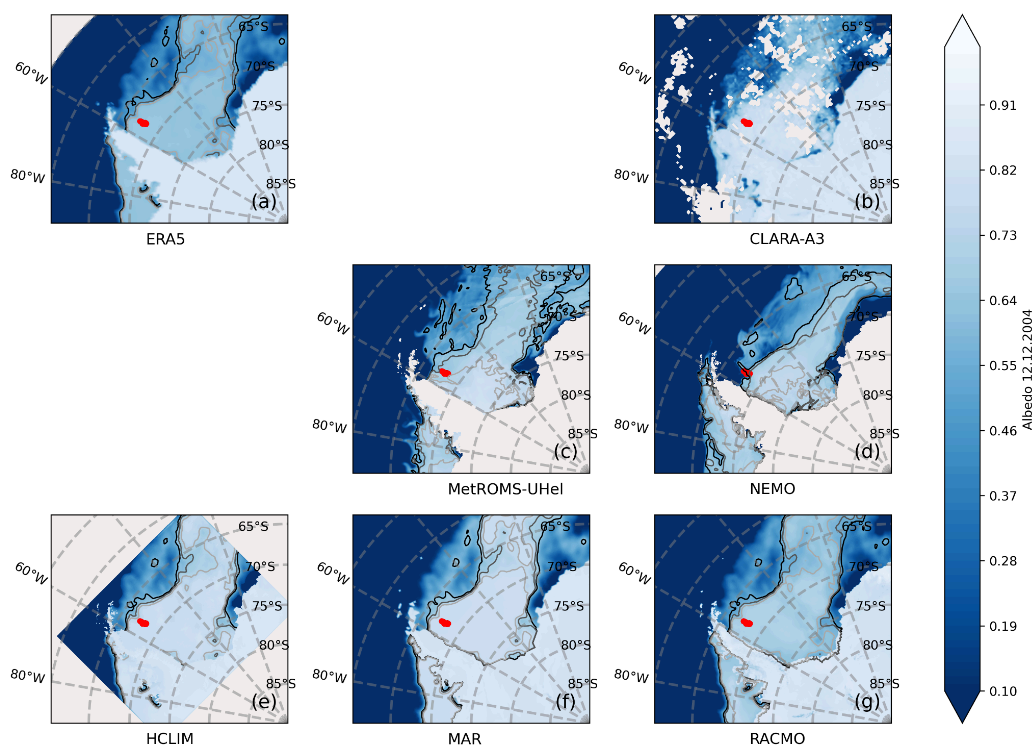
Thank you for your observation. You are correct that the CLARA-A3 SAL dataset provides surface albedo products as either monthly means or 5-day means, depending on the product type. In our case, we used the polar monthly mean product, and the dates mentioned in the text refer to the central date of the monthly averaging period, as defined in the file naming convention. We did not recompute any daily albedo values from raw CLARA-A3 data.

This comment drew attention to a mistake on our part: the monthly mean satellite product was indeed compared to the daily values of the date in the CLARA-A3. We have changed the Figures 10-13.
The spatial patterns of sea ice albedo changed little, especially over the Ross Sea during November 2022, but it does not change any of the conclusions drawn.

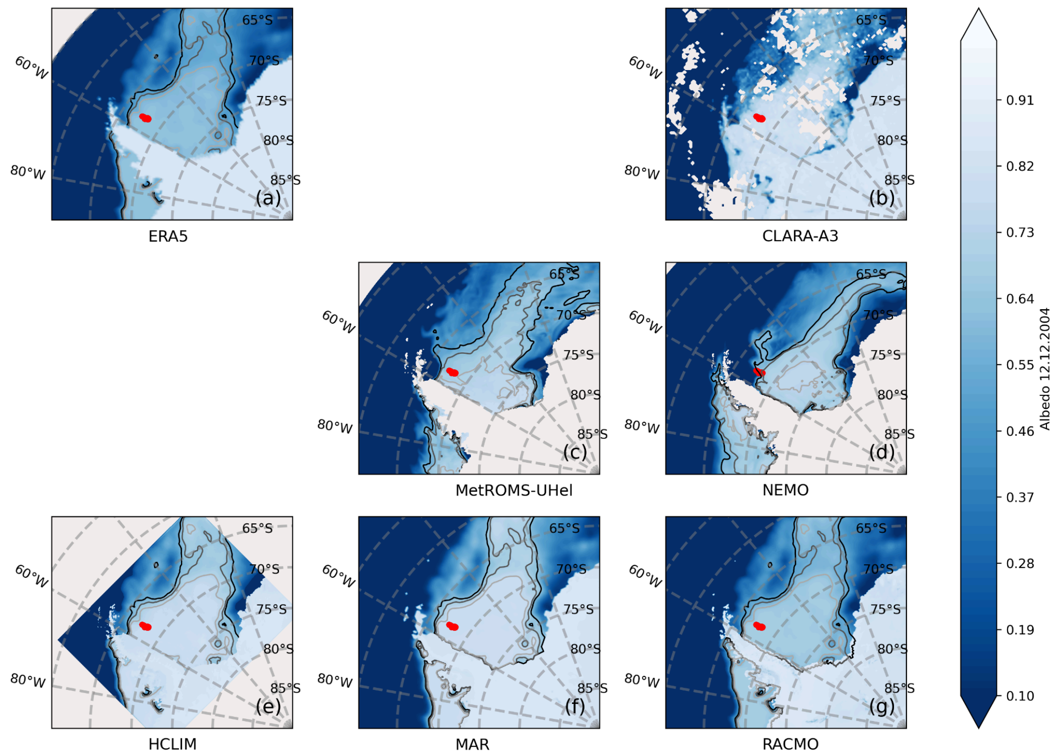
We added this information to the text in line 204: “Furthermore, the CLARA-A3 blue-sky albedo monthly product (dates mentioned in the text refer to the central date of the monthly averaging period, Karlsson et al., 2023), which has a coarser resolution of 25 km, …”

We have made a further clarification in the caption in the Figures 10, 11: “Monthly mean ERA5 (a) reanalysis and CLARA-A3 (b) satellite albedo products from over the Weddell Sea domain as reference for model validation, and corresponding monthly mean albedo maps from MetROMS-UHel (c), NEMO (d), HCLIM (e), MAR (f) and RACMO (g) models.”

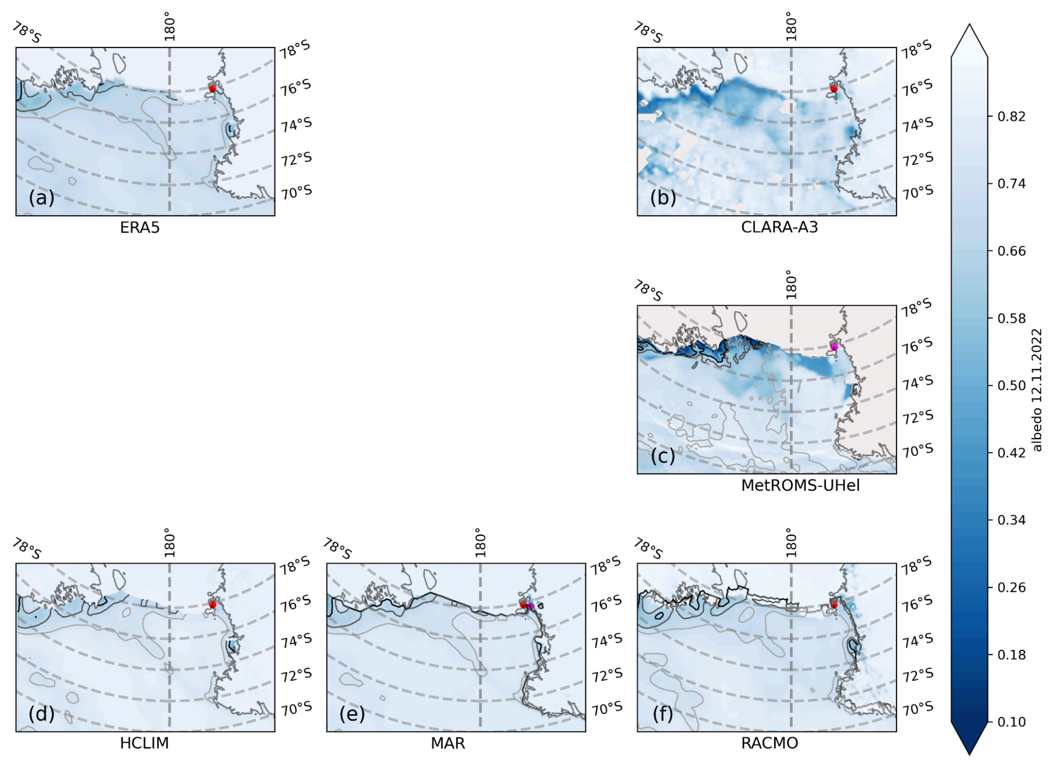
Old Fig. 10:



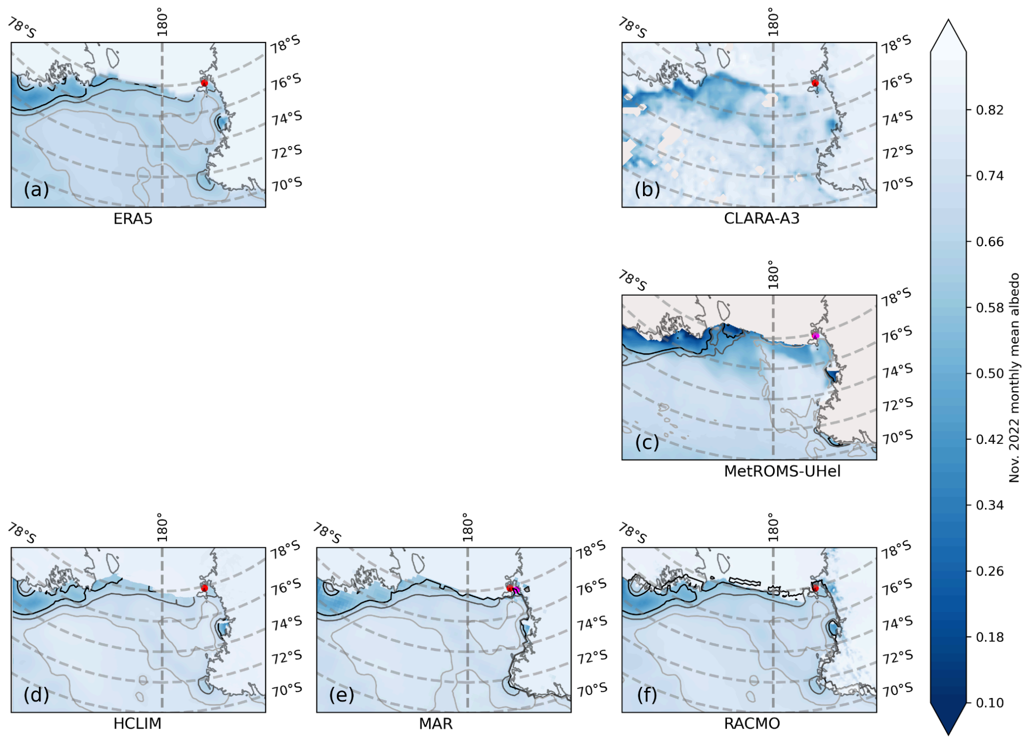
Updated Fig. 10:


65°S
70°S
75°S
80°S
85°S
60°W
80°W
(a)
ERA5
(b)
CLARA-A3
(c)
MetROMS-UHel
(d)
NEMO
(e)
HCLIM
(f)
MAR
(g)
RACMO
Albedo 12.12.2004
0.91
0.82
0.73
0.64
0.55
0.46
0.37
0.28
0.19
0.10

Old Fig. 11:


78°S
180°
76°S
74°S
72°S
70°S
(a)
ERA5
(b)
CLARA-A3
(c)
MetROMS-UHel
(d)
HCLIM
(e)
MAR
(f)
RACMO
albedo 12.11.2022
0.82
0.74
0.66
0.58
0.50
0.42
0.34
0.26
0.18
0.10

Updated Fig. 11:



We have fixed the “3A” typos in legends and in text.

324: 20 cm of snow is considered thin? From the optical (albedo) viewpoint, 10 cm is typically enough to effectively make the snowpack optically semi-infinite. The text does refer to this effect (for both ice and snow), but it would be nice to have quantified estimates here for typical depths required – and for the authors to consider if any of the results are affected. Also, while melt ponds are rarely encountered over Antarctic sea ice, it would be nice from the completeness viewpoint to recall that melt pond albedo is also not uniform, but depends on the depth of the pond and the properties of the underlying ice. Several appropriate references exist highlighting this effect.

The snow thickness of 0.2 m is considered thin mainly in comparison with the snow thickness observed over the Weddell Sea. But the reviewer is right, 0.2 m is sufficient to make the snow optically semi-infinite. We therefore corrected one sentence, and added one additional sentence to clarify this point:

“The snow on top of the 1 to 4 m thick land-fast ice in the McMurdo Sound had reportedly relatively thin (~0.21 m, CS), or patches of thin (~0.02 m, NIS) snow on top. Snow thickness variability in the observed range between 0 and 0.2-0.3 m has a big impact on the albedo. For densely packed, fine-grained snow, the snow layer becomes semi-infinite—meaning that further increases in depth no longer affect snow reflectance—at a depth of approximately 0.10 m"

Minor comments:

131: A +/-1% measurement uncertainty sounds very high for field conditions. Is this a manufacturer estimate?

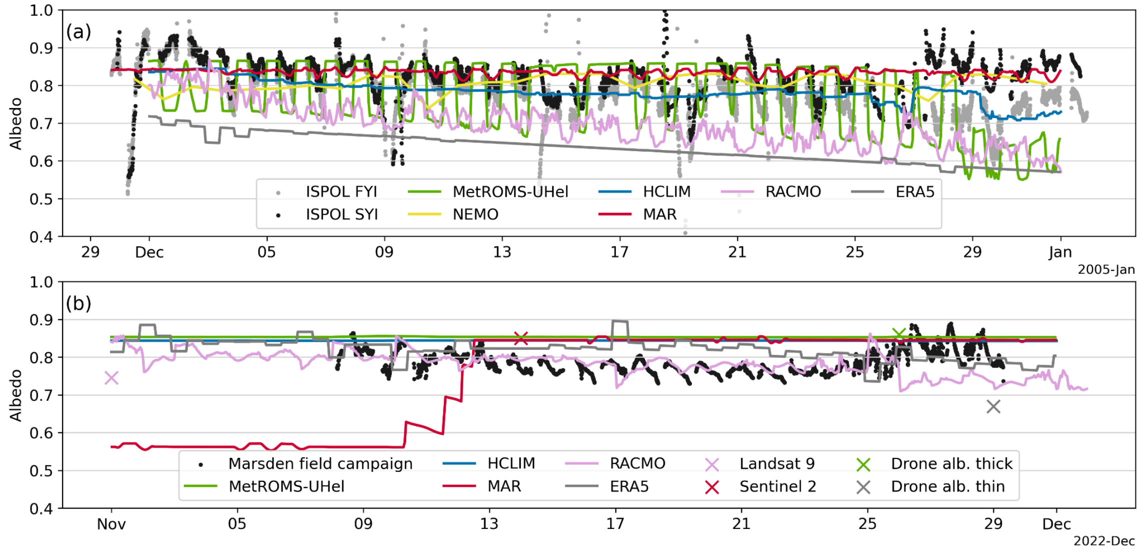
In fact, this albedo uncertainty is the lowest that can be possibly reached with the most accurate and well calibrated pyranometers, such as the Kipp&Zonen CMP22 used for this campaign. It corresponds to the sum of the different error sources as estimated by the manufacturer.

fig 3: legend gets lost in the subplot c, please consider moving it outside of plot area.

We have done so.

In addition,
the work done during the peer review process revealed a mistake in the analysis. We were using an older output version of MetROMS-UHe for Figs. 4 and 5 for the ISPOL case. The difference between the two model setups is minor with small changes in the input file of CICE, where default values were used for the newer run.

In our results, the behaviour of MetROMS-UHel at the end of the ISPOL campaign has changed, and we have made corrections in the paper. Firstly, the MetROMS-UHel has an abrupt lowering of sea ice albedo at the end of the month (around 29th of Dec), which was not present before.
Although the differences between the two model setups are minor, small discrepancies can accumulate over time, especially given that the model was run continuously from 1992, including about 16 years of simulation and spin-up by the time of the ISPOL case study.



Updated Fig. 4.

The value in Table 1 changes a little: 0.78 (0.08) → 0.77 (0.09)
In Fig. 5, the changes are not visible, and the snow height is not affected.

To address the comments here and those from Referee #1, we have added two additional figures to the manuscript compared to the previous version. As a result, the figure numbering in the revised version will differ from that referenced in this response.

We thank the reviewer again for the thoughtful comments, which have helped improve the clarity and quality of the manuscript. We hope that the revised version meets the reviewer's expectations, and we believe it is now significantly improved as a result of this review process.

With kind regards,
Kristiina Verro,
On the behalf of the authors.