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Review of tc-2021-80: The MOSAiC Drift: Ice conditions from space and comparison with previous years

Anonymous Referee #1

Referee comment on "MOSAiC drift expedition from October 2019 to July 2020: sea ice conditions from space and comparison with previous years" by Thomas Krumpfen et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-80-RC1>, 2021

Review of

The MOSAiC Drift: Ice conditions from space and comparison with previous years

by Krumpfen, T., et al.

Summary: This paper reports on the physical sea-ice conditions (drift, coverage, leads and deformation, ice thickness and snow depth) as observed by satellite remote sensing sensors during the October through end-of-July drift of the MOSAiC observatory. The paper attempts to put these observations into context a) with the meteorological conditions - aka 2m air temperature, surface air pressure and near-surface wind speed - and b) with the conditions typical along the same but hypothetical drift track of the period 2005 until 2019. The paper is very descriptive but contains a wealth of different pieces of information. The paper provides intendedly little (critical) discussion or interpretation of the observations made. The paper provides little to no outlook about what next steps are already underway to i) better evaluate / improve the products shown or to ii) merge (some of) the products for improved understanding of geophysical interaction processes.

The paper gives access to the satellite remote sensing products obtained, covers an impressive set of parameters, and puts the observations made into a first wider context; it is hence a useful contribution to the scientific community.

I have two general comments (see below), followed by a number of specific comments and a few typos / editorial remarks.

General Comments:

GC1: My main general comment / concern about the content goes towards the imbalance in the degree of detail with respect to differences between the 50 km radius disc and the 100 km radius disc. For sea-ice thickness this is discussed at length - including hypothesis about why this difference of 4% is observed. For snow depth, one of the key parameters for freeboard-to-thickness conversion, this is not discussed. Neither are the differences in the lead fraction discussed nor the differences in the sea-ice concentration. I can understand that sea-ice thickness is an important parameter. However, in the light of the time series of the various parameters shown and in the light of the retrieval uncertainties in sea-ice thickness, I see other features that might be more worth to be put upfront / discussed in a bit more detail in such an overview paper. What explains the observation that despite an average 2m air temperature of -20degC throughout December sea-ice thickness did not increase? What explains the observation that apparently sea-ice thickness growth accelerated from January to February, staying at a high level in March, reaching a plateau first half of April despite continued -20degC 2m air temperatures? How different would results for the years 2005-2019 look if the parameters would have been retrieved along the reconstructed hypothetical drift tracks shown in Fig. 8?

My believe is that this overview / first synthesis paper would benefit from a few more overarching considerations and/or opening questions to be answered in forthcoming papers to be put at the end. I find the lack of opening doors for the work to come the weakest part of this manuscript.

GC2: My second general comment is a follow-up to the degree of detail offered for the sea-ice thickness part. Given the retrieval uncertainties of sea-ice thickness it might be a very good idea to transparently communicate these uncertainties. Putting the observed L2P and/or CS2SMOS observations into context with earlier observations calls to at least note that uncertainties exist. I am not asking to discuss these at length. But comments like: "Observed snow depths (in situ!) agree well with Warren et al. (1999) snow depth climatology used in the sea-ice thickness retrieval ... " Do they? or "Sea-ice thickness retrieval at the border between first-year and multi-year ice is not trivial because the ice type determines values of key retrieval parameters snow depth and ice density ... " --> How solved here? or "SMOS sea-ice thickness products have yet received little evaluation for sea-ice conditions encountered during October/November ..."? or "The observed difference between observed and 10-year mean sea-ice thickness could potentially be explained by a mismatch between actual and climatological snow depth ... "? I guess all I ask for is to be a bit more specific at certain places and to be a bit more critical about the data merged together in this overview paper.

Specific Comments:

Title: Any reason why you did not choose to write "physical sea-ice conditions"?

L26: I suggest to note what you mean by "previous years" and later on, on which years the climatological mean is based.

L35: I suggest to check how safe it is to come up with such a detailed number when speaking about the ice thickness which is known to have a substantial uncertainty. Is 4% within the retrieval noise? This refers back to GC1 and GC2.

L61: I suggest to provide a motivation for the length and years included in the reference period. It is neither a classical climatological period - aka 30 years in mid-latitudes, nor does the length resemble any other common period.

L94: Since you used the version 4 product you could in principle also cite the TC paper from 2020.

L96: Was this data set also assessed in the work of Sumata et al.? In this case I recommend to also include a respective reference.

L98: CERSAT summer-time ice motion is missing for good reason. I guess it would be fair to mention that in June/July NSIDC ice motion is nothing else than based on NCEP wind - supported with buoy motion (if present).

L99: "Sea ice is traced forward ..." It might make sense for clarity that the sea ice at the starting position of the CO is traced forward in time. You are not looking at the entire Arctic Ocean. It might also make sense to be just a little bit more specific in terms of how this tracking is applied. A) How large is the parcel traced forward? B) Do you interpolate the gridded ice motion information onto the location of the ice parcel? How?

L99: Why 1996 as the starting year here?

L100: "if ice concentration at" --> "if the sea-ice concentration at"
Question: Which sea-ice concentration product do you use? What is its grid resolution? Is it sufficient that the sea-ice concentration drops below 50% once or does it need to be a number of consecutive days?

L106 / L110: I am confused with respect to the Fig. 2 and Fig S1 in the supplementary material. These appear to be identical. It should be either or. This applies to some of the subsequent figures as well.

L124/125: You write about "uncertainties" and "bias". I suggest to make sure that the reader understands whether your "uncertainties" are in fact the bias (or the accuracy) or whether you also include precision (or the standard deviation around the true SIC value) into this expression. If there is, e.g., a notorious underestimation of SIC during summer due to melt conditions, i.e. a negative SIC bias, then there is possibly little chance to reduce the bias because neighboring grid cells are not independent and melt processes are typically larger scale phenomena.

L128: "multi-parameter retrieval" --> that is based on data of which sensor?

L145: "at least 50 data points" --> Perhaps add: "within the specified search radii".

L146: "as very few orbits ..." --> You could perhaps add that because of this there won't be any direct inter-comparison between SIT measurements taken at the CO and CS-2 observations ... ?

L149: "also north ... 88°N" --> While it is of course good to have this opportunity to "fill" the pole hole it might be fair to state that any SIT values north of 88degN that you retrieve and show along the track of the CO are basically the result of an informed extrapolation. Any gradients in SIT that might occur north of the latitudinal limit are not represented.

L159: At the end of this subsection I note that you provided information about the uncertainty for the sea-ice concentration product and in some sense also for the drift data (ok, for the drift track) but not yet for the SIT data used. I guess it wouldn't hurt to write a few lines about SIT retrieval uncertainty here - ideally you'd also state already the issue that a merged snow depth products needs to be used which is partly based on an (outdated) climatology which prevents adequate mapping of the inter-annual SIT variability.

L167: Where does the ice type information come from?

L168/169: "estimate of the uncertainty" --> This sounds perfect. Please then provide an estimate of the respective values. Perhaps a range would do it? Or some percentage values?

L169/170: "Snow depth currently ..." --> Perhaps, for similar studies in the future, it might make sense to revive the SMOS-based snow depth estimation approach by Maass et al. ?

L172: "Here we present ..." --> Hmmm ... given the fact that the grid resolution of the snow depth product is 25 km ... how do you realize this? Isn't this basically one grid cell?

L177: How is a lead frequency defined at daily temporal resolution (L184)? Could it be that lead frequency is a monthly product? In addition, what does a frequency of, e.g. 0.1 mean? Does this mean that a lead was identified at the respective location on about 3 days within the respective month?

A related question: Does it make a difference whether a lead occurs at a specific location on 3 consecutive days or on 3 arbitrarily distributed days within the respective month?

L186: Is 10 km the grid resolution (pixel size) of the lead product? Please add this information.

L192: Sea-ice drift and deformation fields are offered at the same spatial resolution? Does this imply that the grids (the one where the motion is provided and the one where the deformation is provided) are shifted with respect to each other by half a grid cell?

L202: "... standard deviations." --> This is the standard deviation of the deformation parameter (i.e. convergence or divergence or shear) averaged for each individual 5km-radius-circle areas? Or is this referring to the standard deviation derived over the 61 circle array? Or, to ask it in a different way: occurs the definition of a strong deformation event at the scale of the 5km-radius circles or at the scale of the gridded deformation, i.e. 1.4 km?

L220: I suggest to add the grid resolution of the ERA5 data used by you.

L236/Fig. 6: I suggest to avoid that data of the surface air pressure and data of the 2m air temperature overlap each other for the individual years. I would also make sure that the respective y-axes cover the full range of every parameter shown.

L238: "+10°C" --> Do you mean that air temperatures as high as +10degC were observed? Or do you refer to the temperature anomaly? As a meteorologist I'd always see the unit Kelvin used for a temperature anomaly to avoid misinterpretations. Hence: "up to 10K, not shown"

L243: Sloppy wording. Please specify what you mean by "stormy conditions".

L246/Fig. 7: I would make sure that the respective y-axes cover the full range of every parameter shown.

L248: "the raw on-board ..." --> Why is this? Because of the different anemometer heights? Didn't you correct the wind speed measurements to 10 m height? If not why not? Since you have mostly stable conditions it might be a straightforward thing to do and then you might be able to make a more quantitative statement about the ERA5 wind speed quality.

How about wind direction?

L249: change unit to K.

L249-251: "especially taking into ..." --> I suggest to be a bit more explicit here. I'd say there are two effects to take into account. The ship heats the air aloft and might cause higher air temperatures at the measurement height than in the free atmosphere. And at the same time the measurements are taken substantially further away from the surface and hence the near-surface inversion of the temperature might not be captured well.

L253/253: "Given the fact ..." --> This is one way how to close this paragraph. Another way would be to note that during MOSAiC a large set of air-temperature measurements along vertical profiles was taken which is (presumably) going to be used in a forthcoming study to more quantitatively assess ERA5 2m air-temperature data against the MOSAiC observations - including the ship-based data.

L265/266: "The interannual variability ..." --> How do you know? Can you exclude that ice thickness / ice-surface structure due to deformation doesn't have a similarly large effect?

L272: "97%" --> This number is based on which data set?

L279: "was with 99.5%" --> A good moment to cite papers such as Kwok, 2002 or Andersen et al., 2007, referring to the high-concentration Arctic average sea-ice concentration values.

L282/283: I possibly missed this: How did you compute the distance to the ice edge? What was your reference point?

L285: "stayed higher" --> Please be more specific. Was it 99% ... 95% ...?

L286/287: "We can see ... Fig. 6" --> I recommend to mark this period in Fig. 6 for better clarity.

I am wondering what the effect of the increase in wind speed might have had on the physical snow and sea-ice properties relevant for its microwave remote sensing?

L287/288: "liquid water ..." --> Please provide 1-2 references to back up your statement about this important process.

L288-290: "These surfaces ... used here." --> It might not hurt to also back up these statements by a reference.

L293: "which takes" --> I know that work and I suggest to be more careful in the writing: "which takes" --> "which attempts to take"

L294: Fig. 9: What I am missing at this point is that Fig. 9 indicates that sea-ice concentrations dropped below 90% in a number of previous years - just to put your observation into a wider temporal context.

L296: Just as a reminder: I am not sure you wrote how you derived that distance. And, of course, given the way you use this information in this manuscript, I am wondering why you did not exclude the Russian marginal seas after freeze-up rightaway.

Figure 12: I suggest to add to the caption that 50km SIT estimates are missing in case there were not enough available L2P data within the 50km radius disc.

Table 1: I suggest to add a column in Table 1 which provides the average number of valid data that was used to compute the mean, IQR and IDR.

L304: "period between ..." --> I assume that this is the period when the 100km radius of the disc centered at the CO first / last intersects with the observational data gap at the pole? Or did you in fact use L2P CS2 observations until the CO traveled across that border? In that case there is a phase before / after where the number of valid L2P decreases / increases. How was this solved in detail? In a way I don't fully understand your writing in the context of Fig. 12 ... showing continuous 100 km sea-ice thickness data.

L311: "was consistently" --> "was, on average, consistently" ... just to better comply with the observation that there are quite some locations where the 100km radius disc SIT is smaller than the 50km radius disc one.

L323-325: I have difficulties to understand this. I would have thought that, as you wrote, the L2P data provide an accurate "point" measurement, or better, a suite of such measurements ... covering the full sea-ice thickness range down to, say 0.2 m. When there is not thick enough ice, there won't be any measurement. Hence, thin ice is excluded and an average might in fact be biased high. CS2SMOS in contrast fills that gap by including thin ice thickness measurements, hence adding the thin tail to the ITD; because of this I'd expect that CS2SMOS is providing thinner average sea-ice thickness values than L2P CS2 data. Why is it the other way round here - according to your writing?

L333-335: "The monthly sea-ice thickness ..." --> How do these differences relate to A) the retrieval uncertainty or error of the CS2SMOS product and B) to the standard deviation of the multi-annual average CS2SMOS sea-ice thickness?

L345: "good agreement ..." --> This is a rather unspecific statement. Which errors? What are the "expected uncertainties"?

L348: "Only after ..." --> Would it be possible to show precipitation in Fig. 6 along with the other three parameters? This would aid greatly in the credibility of the statements made in this sub-section.

L349-353: "After ... earlier." --> I'd say that these lines are not well backed up by in-situ observations and/or ERA5 data. I suggest to remove them. Interaction between snow properties and microwaves in the presence of snowfall (or other precipitation like freezing rain) and in combination with substantial variations in the near-surface air temperature as shown in Fig. 6 are very complex. There have been studies in the past demonstrating that adding a few centimeters of snow can have a profound influence on the brightness temperature. In addition, snow metamorphism (aka a change in grain size) might also have played a larger role than snow compaction.

My suggestion would be that you condense this paragraph to what we can see in the passive microwave snow depth estimates, ideally say something more specific about the in-situ observations and, which I would find of utmost importance given the relevance of snow depth for the CS2 sea-ice thickness retrieval, relate these observations to the Warren et al. (1999) snow depth climatology.

At the end you could then state that an immense measurement program took place during MOSAiC to make a step change in our understanding how microwaves react to varying snow conditions on sea ice.

L357: "The lead frequency is ..." --> You might want to put this information into the respective subsection of section 2.

L371-373: "Only in March ..." --> You could relate this observation also to the respective figure showing the anomalies in the atmospheric circulation which for March and April agree well with your results.

L384/385: "It is striking ..." --> Why? Perhaps remove the "striking" part and simply state that these events were not necessarily accompanied by a decrease in sea-ice concentration because of (... timing of lead opening vs. clear-sky image acquisition / near-surface air temperature determining freeze-over / intermittent change in wind direction - aka lead closing ...)

L390-392: "Moreover, we find ..." --> I am not sure I would compare the results of the entire MOSAiC drift with the results cited here because the geographical region the latter are representative of correspond to MOSAiC from May onwards. Perhaps you could relax your statement into that direction. In addition I recommend to add that the Oikkonen et al (2017) results are from the N-ICE2015 drift campaign.

L403/Fig. 17: Since it is not obvious from Fig. 17 that 60% of the events took place in Oct./Nov. you could add to the caption of Fig. 17 that vertical bars of successive events might overlap and look like one event ... or whatever you like to avoid the impression that 60% is a wrong number.

L404/405: It would have been really cool to be able to delineate this event also in Fig. 17. Why is it missing therein?

L411-417: How likely is it that the months long action on the floe has resulted in snow property changes that triggered faster melt?

Also, if I am not mistaken, then the sediment argument only can come into place when the snow cover is completely gone.

L445/446: "Significant changes ..." --> I suggest to make clear that these changes are i) artificial as provided by the sea-ice concentration product and ii) are concomitant with elevated but still below-freezing temperatures. The way written could easily be misinterpreted towards: "high air temperatures melted sea ice" by a non-expert.

L457: As the lead time series terminates end of April I suggest to delete "and summer".

Typos / Editorial remarks:

GC0: This is a general comment about the editing. I am wondering why the authors decided to write in mixed passive and active voice. I would find the paper easier to read if you'd have used active voice throughout.

L30: month --> months

L217: "temperature" --> I suggest to add "air" ; it might make sense in general to make clear that you are talking about air temperature and air pressure in the following (e.g. in L226) and hence be more specific in your writing.

L217: "derived" or simply "taken"?

L264: "westerly" --> Perhaps better: "westward"?

L276: "will" --> "we"

L280: "than the" --> "than during the"

L341: I note that the heading of the sub-section says: "Snow depth" but here you write about snow thickness. I suggest to use one term.

L380-382: "However ... dots." I suggest to move these two sentences further up in this paragraph, right behind the first sentence in L375.