Reviewer comments RC1

The authors introduced a new climate data record of sea ice drift vectors from 1991-2020, which covers both Arctic and Antarctic. This provides good continuity data for long time series observation of sea ice motion in polar regions, and is useful for quantifying the response of sea ice motion to climate change. The overall organization of this manuscript is clear and the methods are well described in detail. However, the comparative analysis of the product with other products in the manuscript is relatively vague and there is a lack of elaboration on the areas in which the product could be applied. After refining the above deficiencies, I consider this article would be more in line with the main purpose of the *Earth System Science Data*. Some improvements could be made, as listed below.

Thank you for reviewing our manuscript and for your comments. We answer below.

Major comments:

1. The introduction describes relatively little about the importance of sea ice drift, although much of it is well known. For readers unfamiliar with the field, what is the need to produce sea ice drift products? What can we apply sea ice drift products for? Please emphasize this in the introduction.

This is a good point. We might have taken this for granted. We will add more context in the introduction.

2. Usually buoy data are considered to have a certain level of authenticity and accuracy, and the reason for not choosing buoy data as one of the input sources in the section on multi-source merging (Section 3.4) needs to be elaborated more than just emphasizing the difference from existing sea ice motion products (L285).

We agree this requires elaboration. Our choice was deliberated but is not justified. We will add a paragraph to cover this discussion in section 4.1 and we might remove the sentence L285.

3. The lack of comparisons with existing sea ice motion products would be relatively strange. For users of the data, a general overview comparison with widely used products is necessary. It is also useful to have a visual overview of the advantages of this new CDR product.

See our combined reply to your next point.

4. After reading the whole manuscript, my main concern is, what exactly are the advantages of the new sea ice drift CDR product? This needs to be highlighted and detailed, but there is no summary paragraph in the introduction or in section 4.3 (Discussion, known limitations and outlook), which is not convincing. According to the manuscript, the new CDR has a coarse spatial resolution of 75 km and a time length of 1991-2020, which is not as fine

as the products provided by NSIDC and IFREMER in terms of time length and spatial resolution.

We cover several of your points (spatial resolution, time length, spatial extent, and more) in our section 4.3, most of the time in comparison to NSIDC or IFREMER. Your comment however made us realize that we do not cover the temporal frequency aspects (daily for the new data, 3-days for IFREMER, 1 week for NSIDC) and we will add a subsection in 4.3.

We agree that at present we do not summarize this comparison, and that this could be useful for a user. We will thus add a summary table at the end of section 4.3. To improve readability we will also gather our text describing what could be improved towards the next version of our data in a new section, so as to focus 4.3 on the comparison to other data sources.

In this data-description manuscript, we report on an extensive validation against buoy trajectory data, and chose not to perform a quantitative intercomparison to other sea-ice drift data records like IFREMER or NSIDC, if this is what you call for with “The lack of comparisons with existing sea ice motion products would be relatively strange.”

We indeed do not believe such initial data-description papers should hold quantitative inter-comparisons to existing products. We think the user community deserves better, and that product intercomparisons are best led by authors / teams independent from the data producers. Otherwise there is always the risk that the selection of reference data, metrics, reprojection, etc… favor one’s product. The community is better served with independent and transparent product intercomparison studies, reported in a separate manuscript (e.g. Sumata et al., 2014; Sumata et al., 2015; Wang et al., 2022).

We note that neither the manuscript introducing the IFREMER data record (Girard-Ardhuin and Ezraty, 2012), nor that introducing the NSIDC data record (Tschudi et al., 2020) held an intercomparison to other products.

Sumata, H.; Kwok, R.; Gerdes, R.; Kauker, F.; Karcher, M. Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data. J. Geophys. Res. Ocean. 2015, 120, 5285–5301.

Sumata, H., Lavergne, T., Girard-Ardhuin, F., Kimura, N., Tschudi, M. A., Kauker, F., Karcher, M., and Gerdes, R. (2014), An intercomparison of Arctic ice drift products to deduce uncertainty estimates, J. Geophys. Res. Oceans, 119, 4887– 4921, doi:10.1002/2013JC009724.

Wang, X.; Chen, R.; Li, C.; Chen, Z.; Hui, F.; Cheng, X. An Intercomparison of Satellite Derived Arctic Sea Ice Motion Products. Remote Sens. 2022, 14, 1261. https://doi.org/10.3390/rs14051261

To summarize our answer to your comment: in addition to our extensive validation against buoy trajectories, we will strengthen the comparison to NSIDC and IFREMER in terms of main characteristics (in section 4.3), but we suggest not to perform and report on a quantitative intercomparison against other satellite-based products in this paper.

5. Section 4.3.4 mentions that for the free-drift model, the authors' method does not capture the multi-year interdecadal variability of the relationship between wind and sea ice motion vectors, which is a non-negligible problem for summer sea ice motion. I would like to know how this problem is handled by other products that have been applied besides the mentioned solution by Brunette et al., (2022). You only seem to mention that the NSIDC product use a constant wind-ice-ocean transfer coefficient.

To our knowledge, the NSIDC product is the only other CDR providing summer sea ice drift. IFREMER stops during summer. KWOK was winter only (and is no longer updated). We will investigate and write a short summary of our findings re: summer sea-ice drift.

Minor comments:

1. “The first/second part of the period” appears several times in the manuscript, e.g., L631, L632 and L640. It would be more appropriate to clarify the period in which the product showed a large bias when reminding users of the precautions (Section 4.3.4).

We will revise. This was also pointed out by the 2nd Reviewer.

2. L222 “The model is expected to be less valid in winter than in summer, and less in the Arctic than in the Antarctic, because of neglecting internal sea-ice stresses.” This statement is a bit difficult for me to understand, please explain it in more detail.

We will revise and potentially split it into two sentences.

3. L412 I tried to find the Appendix B but couldn't.

We were referring to Appendix B of Stoffelen 1998. We will check with the editorial team if our citation format (Stoffelen, 1998, Appendix B) could be written differently.

4. L508 Given that the multi-oi summer sea ice drift product is obtained by merging only wind-driven product, their RMSEs are exactly equal rather than very similar by comparing Tables 7 and 6.

Indeed. We will revise this sentence.

Reviewer comments RC2

Review of

A Climate DAta Record of year-round global sea ice drift from the EUMETSAT OSI SAF

by

T. Lavergne and E. Down

Summary:

This paper introduces a long needed alternative to the NSIDC sea-ice motion data set based on satellite microwave radiometer observations. Albeit shorter, i.e. for 1991-2020, the description of the data set itself, how it has been derived from a well-established high-quality inter-sensor calibrated microwave brightness temperature data set and ERA5 near-surface wind speed data input to a free-drift sea ice model, and how the data have been evaluated with buoy observation is very good and provides a quite comprehensive insight into this new data set. The data set is available for both hemispheres at daily temporal resolution. While the data set is year-round only the winter months are based solely on satellite observations. Summer months data are solely based on free-drift sea ice model forced with ERA5 near-surface winds tuned to summer sea-ice motion derived from the latest generation of satellite microwave radiometer observations. Transition months are a blend. An essential detail to know is that the product contains sea ice displacement together with position and temporal information; the product does not contain readily derived sea ice drift velocity. The data set itself follows latest CF-conventions, is very-well documented in the netCDF files' global attributes and contains a useful set of flag values. The data are accessible via tools such as ncview and the Climate Data Operators (cdos), documenting their usability.

We thank the reviewer for their positive appreciation of our CDR and manuscript. We will address the sea-ice displacement / velocity aspect (among others) later in this review.

General comments:

GC1: There are a few aspects detailed in my specific comments that require a bit more information and/or rewriting parts of the text. To this belongs usage of different frequency channels for SSM/I / SSMIS versus AMSRX, the transition of sea-ice model based sea ice velocity to sea ice displacement, and an even more comprehensive motivation of and emphasis on the fact that the product contains displacement along the x- and y-axis of the EASE grid used - which makes an easy uptake and usage of the product not straightforward for quite a fraction of users. Also not clear is why on the one hand summer sea ice motion is derived from the free-drift sea-ice model but on the other hand the summer sea ice motion data are considered good enough for the required tuning.

GC2: I am not convinced that the discussion added regarding apparent differences in the bias been buoy and satellite (or multi-oi) data is mature enough to be kept in the form as is in this manuscript. What I am missing instead and was hoping that it would be touched somewhere in the discussion is the consideration of the free-drift assumption during summer. How "accurate" are the summer sea ice displacement estimates for sea ice concentrations below, say, 80%, or for almost closed sea ice conditions?

Both noted. We will address these issues as replies to your Specific comments below.

Specific comments:

L72: It seems you decided to use the near-90 GHz channels for your product because of their comparably fine resolution. But what about the weather influence at this frequency which is known to be considerably larger than at the other channels offered by SSM/I or

SSM/IS?

In fact this is contrasting what you write later-on about AMSR-E/2 where you explicitly state that you are NOT using the 89 GHz channels because of the larger atmospheric influence. This inconsistency needs to be clarified.

We agree this needs to be clarified. We will add a short section *2.1.3 Choice of microwave frequency* to explain why we used near-90GHz imagery for SSMIs and 36.5GHz imagery for AMSRs. Shortly, the choice of microwave frequency is a compromise between the level of details in the imagery (mostly driven by spatial resolution) and the stability of the imagery from one day to the next (better with channels that are less sensitive to the atmosphere). There are no perfect channels for sea-ice drift on these missions: this is the result of a compromise.

L92: "free drift" requires sea ice concentrations below about 80%. What is the uncertainty / bias in case this constraint is not met? Is this discussed and if yes in what context?

We agree this should be discussed in our manuscript, e.g. in section *3.3 Summer sea-ice motion : wind-driven free-drift model*. It is correct that the free-drift equation is derived from the general momentum balance equation under specific assumptions, including that internal sea-ice stresses are negligible, which can only happen if SIC is not too high (e.g. below 80%). In our case however, we use the equation of the free-drift model only as a parametric formula for sea-ice motion, and we tune the parameters A, θ and Uwg against data. The values for these parameters are not derived from the free-drift equation theory (involving the Nansen and Rossby number, the Coriolis constant, etc…). Because we only use the free-drift equation as a model to fit data, the assumption is that our tuned parameters will compensate for the theoretical limitations of the free-drift model (to the best of their capacity). We will add some text along those lines in section *3.3 Summer sea-ice motion : wind-driven free-drift model*.

L134/135: "in particular ... and 24 UTC" --> I have a problem understanding this. Every point in the polar hemisphere is covered by a number of satellite overpasses every day - those closer to the poles more often. I can imagine that for some regions there are overlapping overpasses at (roughly, don't count me on the full hours please), for instance, 4 UTC and 6 UTC and then later on at 15 UTC and 17 UTC (i.e. four overpasses in total) whereas there are other regions where these overpasses occur at 9 UTC and 11 UTC and later on at 20 UTC and 22 UTC. Does your weighing scheme mean that in the first case overpasses at 15 UTC and 17 UTC would given more weight than the other two (because of being closer to 12 UTC) while in the second case it would be the overpasses at 9UTC and 11 UTC? Doesn't this result in a preference of using TB values always from the same time of the day since overpass times are rather stable - within certain bounds? It seems to me that this results in regions where gridded TB values are predominantly either from the ascending overpasses or from the descending ones. Is this the intention?

Your understanding is correct. The spatial patterns of the mean observation times from these satellite missions are rather stable, but not constant because the orbits do not repeat exactly (in terms of UTC hours). You can see the result of the patterns on Figure 4, and note that the

differences of observation time from one day to the next (right-most panel) is not uniformly 24h especially at lower latitudes.

L136-138: "we compute ... is useful ..." I have two comments here.

- 1) Does the mean observation time use the same weighing as introduced above? If not my first case from the previous comment would result in 10:30 UTC and the second case in 15:30 UTC - which would not reflect the time of those swaths that has been given more weight than others.

Yes. We will modify the sentence to read: “In addition to the daily gridded TB, we compute the mean observation time for each pixel in the gridded image using the same temporal weighting”.

- 2) You state that the users of the CDR and your own evaluation activities would need this mean time. I agree. You might want to state for what and hereby stress that your CDR does not include any speed information but is solely about the displacement.

Yes. We can do that. For example: “This mean observation time can be useful for the users of the CDR to compute sea-ice velocity vectors (Sect. X.X) or compute model-equivalent displacement vectors, and we use it when collocating the CDR with buoy data (Sect. 3.6)”.

From your several comments on this topic we will add a dedicated section (Sect. X.X) to discuss the sea-ice displacement / velocity topic.

- This puts the question in my mind whether, when doing the consistency checks of your data set, investigated whether the time difference between two consecutive displacement maps provides a reasonable ice motion speed estimate when used to compute the velocity.

In each product file, we add variables holding the start and end observation times of the displacement vectors, so that users can compute the actual duration of the displacement (and hence the velocity).

L142/143: The influence of this Laplacian filter seems to be quite delicate because on the one hand it removes gradients in the intensity while on the other hand it is supposed to enhance patterns. As written this seems contradictory and I invite you to provide sample information about how such a gradient might look like and where it comes from (i.e. what is its cause), and how one can discriminate between a gradient and a pattern.

Thank you. We use the term “gradient” for large-scale changes in the image intensity level, as can typically be caused by atmospheric effects such as water vapour. We use “patterns” for short-scale variations of the image intensity level that are stable in time and can be tracked by the motion algorithm. We will add some text explaining better what we mean here.

L145: "rotated onto the EASE2 ..." --> It is inherently recognizable that you thereby switch from the meteorological convention of directions to the "grid world" and are only interested in the components along the x- and y-directions of the grid.

I guess this is the right moment to comment that: If you would have decided to provide a sea-ice motion instead of a sea-ice displacement product, then this step would have been obsolete. I haven't found yet the motivation why you prefer to derive a sea-ice displacement product. I fear that the uptake of such a product by the user community won't be straightforward - particularly in light of the NSIDC sea ice motion product providing motion in x- and y-direction of the grid they used and the modificed version of this product offered by ICDC which comes up with the u- and v-component of the ice motion in meteorological notation - an, to my opinion, much more handy product as it immediately represents the action of cyclones on the sea ice. In your product, one does not only need to play around with the time information to derive the motion (and with that to be at the same level as the NSIDC product) but one also needs to be very careful in the interpretation of the data as a positive x-displacement means eastward drift just north of Svalbard, southward drift in the Laptev Sea, and westward drift north of Bering Strait ... complicated ... and error-prone for a user.

This is a very good comment, and we must address it in the revision of the manuscript (and potentially in a later version of the CDR). We foresee the following changes will be implemented in the revised manuscript:

- We will add a new section (Sect X.X, possibly 4.2) to justify why we are focused on the dX and dY components of the displacement vectors (in short, because they are what the motion algorithm primarily retrieves) and why we refrain from giving easy access to velocities or other derived quantities such as speed and direction (in short, because we want to nudge the user to realise what satellite-derived sea-ice drift products really are, and because some transforms actually introduce pseudo-biases). This will borrow text from today's Sect. 3.6.3 "Validation Metrics" but make it more general.
- We will explain to the users how they should proceed for computing u/v velocities (in the meteorological convention) from our dX and dY. They do not need to rotate the dX and dY components: they can use the lat1 and lon1 variables from the product file. This will be a useful addition to the manuscript.
- We will provide a (python) notebook example in the accompanying github repo to demonstrate the conversion from dX,dY (km) to u,v (m/s) and refer to the notebook from the text.

L183-189: Since your product is a sea ice displacement it would be good to provide a number for the maximum displacement in addition to the 0.45 ms-1. That way a reader can go back to the product and eventually check how far away a particular value in the displacement maps is from this maximum displacement value.

We will consider this. But since the actual duration of the displacement varies from place to place and during the day, it is not clear a single number is useful. We argue that the user should rather compute the speed associated with a displacement vector and compare it to 0.45 ms-1.

L202-203: "value less than rho = 0.5" --> It seems that this is the first time you mention a threshold for the correlation. How about in all the other cases, i.e. before the rogue vector

filtering: Is there a minimum value of rho which needs to be exceeded or will also displacement vectors with a rho = 0.35 (as an example) make it into the product?

We will add more information about the correlation thresholds for the other cases. All this information is in the product ATBD, but we can bring some of this here.

L218: "... during summer" --> I suggest to somewhere in this paragraph add information that surface melting reduces the identification of patterns in the observed TBs that are required for the tracking; one classical example applying to the Arctic is the loss of the radiometric difference between FYI and MYI. Without this additional information a reader might think this lower accuracy is merely due to melt pond coverage.

Good idea, we will add such sentence.

L224: I note that you switch from displacement (during winter) to velocity (during summer) here.

Indeed. We will add a sentence to specify that the free-drift equation is introduced as velocities, but that we apply it to yield displacements in the product files.

L234-236: I have two questions here:

- 1) You stated that during summer the CMCC algorithm is not suited well to derive ice motion (displacement vectors). But here you use maps of ice motion vectors from July derived nevertheless with the CMCC for the tuning. This needs to be explained better as it seems contradictionary.

Agreed. This will be stated either here, or in the discussion section related to the summer sea-ice motion bias (Sect. 4.3.4).

- 2) You state that you use data of years 2002 through 2020. Did you perform the tuning individually for every year? Or did you average over data of all June, all July, and all August months of the years 2002-2020, respectively, to arrive at one set of tuning parameters per month?

We do the parameter tuning per months, using all available years in the tuning. In the end we obtain one set of parameter for January (using all January months from 2002 through 2020), one set of February (using all February months from 2002 through 2020). We agree this is not well explained and we will clarify in the text.

L240-242: I agree that during the earlier decade of the CDR the sea ice covered more area during summer and I welcome the thought to be able to apply the wind-driven model also in the early decade of the CDR. However, I am wondering whether the extrapolation isn't potentially causing artefacts that negative influence the tuning, and whether without this extrapolation the tuning wouldn't be as good as with. After all, all you need is a representative set of conditions relating sea ice displacement observed to ERA5 near-surface wind vectors and I would assume your AMSRX period is long enough for this purpose.

The tuning itself is not influenced by the extrapolation. We first perform the tuning in areas where we have enough sea-ice drift vectors, then perform an extrapolation towards the areas where no tuning could be performed. That being said we could have used different gap-filling strategies than the extrapolation from neighbours, e.g. filing the gaps with fixed mean parameter values. We have too strategies to avoid serious impact by extrapolation artefacts: 1) we performed visual inspection of the parmeter maps (there are only 12 maps, for 2 hemispheres); 2) we have a different status_flag for vectors derived from the extrapolated parameters.

We will add a sentence or two about the extrapolation and its potential artefacts.

L243: "small radius" --> consider providing a value. How small?

Yes. The value is in the ATBD but we can bring it in the paper.

L247 / Fig. 2: It might be a trick of my eyes but when I compare the map with the legend I would say that the turning angle is merely between -25 and -40 degrees to the left; I can hardly see any values around (just) -20 degrees.

L256 / Fig. 3: Also here I would be inclined to see mostly larger values than 20 degrees. Table 3 actually confirms my view - also the one noted in L247.

We will double-check our colour scales and text. The mean values reported in Table 3 indeed tend to confirm your impression.

L256: "with generally thinner sea ice" --> I am wondering whether the overall lower SIC and with that better match between the free-drift assumption and the actual sea ice conditions doesn't play also a role here - perhaps even more than the thinner sea ice?

This might well be the case and we will add this as a possible explanation.

Figures 2 and 3:

- If possible I would have these figures to extend across the entire text width such that one does not need to zoom 200% or more to see what is in the figures in detail.

Agreed. We will enlarge them for the revised manuscript, and hopefully they will also be bigger in the final edited file.

- Are these figures showing an individual year or are we looking at a July mean for 2002-2020?

They show the July parameters, obtained from using all July data in the period 2002-2020 (see our answer to your earlier point).

- You might want to explain the white disc in the center of the maps shown in Figure 2.

Yes, we will. This is the imprint of the polar observation hole. The interpolation / extrapolation discussed earlier will fill this region so that the free-drift parameter can be applied there as well.

- You might want to explain the white fringe around the coastlines.

Yes, we will.

- Please add units for the turning angle and the under-ice ocean current; I note in this context that you could harmonize the name between the title underneath the panel and the caption.

All good suggestions, we will implement them.

L271/2 72: "we always compute two ... of the month" --> please try to give more details here. There are many ways how to interpret your writing:

1) You only compute two sea-ice motion fields at the very end / beginning of the month, i.e. June 30 vs. July 1, July 31 vs. Aug. 1

2) You compute two fields for every day of the summer months and then interpolate linearly between maps of June 1 and July 1, June 2 and July 2 and so forth.

3) Only within a certain period of time you blend the two motion fields, e.g. June 26 with July 1, June 27 with July 2, ..., June 30 with July 5.

Indeed, this requires a clarification. To ensure a smooth transition across the month transitions, we always compute two sea-ice velocity fields based on the parameter files from the "bracketing" months. These two velocity fields are then blended by linear interpolation, weighted by the difference between the simulation date and the mid-month dates of the parameter files. For example, for a free-drift simulation for June 21st, the two bracketing months are June (16th) and July (16th).

We will clarify this in the text around L271, but also when we introduce the monthly parameter files at the end of Sect. 3.1.1 “Tuning the free-drift model”.

L273: "sea-ice mask" --> could you remind the reader where you take the sea ice mask from?

Yes. We can note that we take it from the OSI SAF Sea Ice Concentration CDR v2 and cross-ref to Sect. 2.3.

L280: "gapfill whole winter days ..." --> While I can understand that you blend shoulder month ice motions and solely use the wind-driven product during summer I have my difficulties to understand why you gapfill the satellite product with these model data during winter, a time when the free-drift assumption often does not hold. This seems to degrade the quality of the CDR during winter. Yes, you do provide a flag for these grid cells but I would assume that 90% (at least) of the users see a gapfilled product and don't care what the source is and whether they should potentially exclude these grid cells because these are not based on the satellite observations.

Users have different levels of expertise, and different levels of applications. It is a difficult task to address them all with a single CDR. Our approach has been to provide a gap-free CDR as an entry point for all (non-expert) users with a multi-source / merged dataset. There we use the quality flags, and higher uncertainties, to warn the user. We also write user documentation and this publication. Given that sea-ice motion is very variable from day to day, mostly due to wind forcing, we think it makes more sense to use the free-drift model than other interpolation methods (e.g. a linear interpolation from neighbouring days). We underline that winter free-drift estimates are only to fill whole days at a time (not grid cells here or there which are interpolated from neighbouring vectors). We will try to strengthen this message in the text.

As we already note, expert users can access the flags and throw out the winter free-drift data, and can use the time series from individual satellite missions to do their own application.

L297: "drift vector" --> just because it strikes me in this moment: I am not sure the reader fully understands why sometimes "drift" and sometime "motion" is used. I am wondering whether, for the sake of clarity, you at some point in the introduction specify what you mean by motion and drift, and that these two terms can be used interchangably. Ideally, though, you switch to "displacement" where you refer to a distance traveled by the sea ice (as is actually the variable in the product) and to "velocity" where you refer to the speed with which the sea ice displacement has taken place (as is the output of the wind-driven model). I could imagine that this could avoid confusion reading the paper. At least I needed to re-assure myself whether in a particular passage you were writing about the displacement or velocity when using drift or motion.

This is a good point. We use motion and drift interchangeably and will make it clear in the introduction. Displacement and velocities are not the same thing and we will also note it early in the text. We might even create an Appendix “Terminology and disambiguation” if we find that these explanations (together with those why we prefer dX/dY to u/v) take too much space.

L300/301: "adjusted to ... Sect. 3.5.)" --> I did not find a sufficient description about how this adjustment is carried out; Section 3.5 is rather short as well.

Noted. We also agree that Sect. 3.5 will be extended.

L314 / Section 3.4.3: What I am missing in this section is how the wind-driven model ice motion vectors = velocities will be transferred to displacement vectors so that these can be merged with the satellite product.

This is a good point. We actually use the free-drift model to generate maps of sea-ice 24h-mean Eulerian velocity vectors, then multiply them by 24h to obtain the displacement vector. This results in an Eulerian displacement while the satellite-based displacement vectors are Lagrangian displacements. We will attempt to clarify this in the new Sect X.X where the choice of dX/dY over u/v is justified.

L319-322: "In the spring month, ... nominal at the end." --> I am wondering whether it would make sense to share typical nominal values of the uncertainties with the reader here. It

would be good to learn whether this ramping begins at 0.5 km, 5 km or even closer to the 10 km stated. I also note that in case of the wind-driven product the status - at this point of writing - is that these are sea ice velocities and hence a standard deviation of 10 km does not apply properly.

This is a good point. At this stage, the reader does not know what is the typical scale of the satellite- or wrind-based uncertainties since these derive from the validation results that are introduced later in the manuscript. We will do as you suggest and mention typical values here.

L326: "The wind-derived motion field ..." --> interesting to see the effort you carry out to have a smooth transition across the shoulder months from winter to summer and back to winter while here there seems to be no further effort to establish a smooth transition - at least not from your writing; perhaps this is in the ATBD?

There are no further efforts in that case. We could have re-used a smooth transition in case of winter gaps but the risk would have been to degrade high-quality satellite-based winter drift vectors with lower-quality wind-based vectors around the gaps. We will note that this is an abrupt process, with no transition.

L333/334: "the grid spacing between vectors is 75 km" --> After the abstract this is the first time the grid spacing of the product is mentioned. This oversight should be corrected at an appropriate place in the methods section.

Yes. This will be added for example at the end of Sect 3.2.1 The continuous maximum cross-correlation method.

L349/350: "that increase ... to 12 UTC" --> this seems to be the part you are refering to from L300/301 but I don't understand what you are doing here. The method is not clear quantitatively and it is also not clear what you mean by "where the drift period of the single-sensor product is far from 12 UTC to 12 UTC". Clarification required.

Yes. This will be clarified and extended. In the ATBD we have a graph and a formula, but we did not want to bring this level of detail in the manuscript.

L362: Why this step? A buoy that reports no displacement over a certain time period seems to be stuck with the sea ice it sits on somewhere. Removing those records between t_1 and t_2 with t_1 = begin of being-stuck-period and t_2 = end of being-stuck-period complicates to generate a product with certain temporal resolution, doesn't it? It is like removing SIC values in a SIC maps where the values to not change from 100% (or 0%).

What happens if one wants to get the ice drift information for that particular buoy for a day that lies between t_1 and t_2? I assume that date might not be found then.

This should have been formulated differently. Our test was to capture unphysical data along the buoy trajectories, e.g. when data records showed flickering between fixed positions. Our test would not have discarded physically realistic slow-moving buoys. We will clarify.

L363/364: Does this work for buoys that have very variable drift speeds, e.g. being set out on moderately fast drifting sea ice in the Beaufort Sea end of summer, then being incorporated into perennial sea ice for 2 years with very slooooooooooow movement and then entering the transpolar drift stream being exported through Fram Strait? Would particularly the observations in the latter region mostly being discarded? Showing examples might help.

At the side: How do you adequately compute the standard deviation of the discplacement when you, as described in the previous step, remove positions where the buoy (safely) did not move?

We agree that this test could have removed trajectories with high variability during the lifetime of the buoy. We chose 3 times the standard deviation by inspecting the trajectories that were detected and concluded that this was an effective threshold to mostly detect rogue positions and not physically realistic trajectories. In future versions we might rather adopt a test based on a moving average and standard deviation.

We will add a sentence or two to state how these quality-control checks can be improved in future versions of the validation exercise.

L386: Is this "40 km" triggered by the grid spacing of the product of 75 km? Presumably you want to have reference and product vector to begin in the same grid cell and 40 km is then your approximation of half the grid-cell width of 37.5 km parallel to the x- or y-direction, right?

The spatial collocation uses the nearest-neighbour technique, so we are already ensured that the reference and product vectors are from the same grid cell. The additional threshold of 40 km is to bring a symmetry in the collocation to discard the corners of the grid cells. We could have used half the grid size (37.5 km) indeed.

Figure 4: I suggest to add in the caption that the start and stop times shown in the two left columns are given relative to 12 UTC? Otherwise I don't understand what the negative time denotes.

Agreed.

L448/449: I am suggesting that - in a future release of this CDR - you switch to 2-sigma uncertainty values to be in line with the GCOS requirements.

This is noted. However most users will naturally use uncertainties provided in a product as 1-sigma, which is why we might keep it this way. But it is indeed important to keep in mind that GCOS's requirements are expressed as 95%-probability (2-sigmas) when comparing our uncertainties or validation results to the GCOS requirements.

Figure 6:

- I note that the x-axis notation is showing "validation data" while earlier in the text when describing the validation you used the term "reference data". I suggest to stay consistent throughout the manuscript.

We will review the text and figure text and ensure consistency of the notations.

- Is there any pressing reason why the logarithmic scale shown changes with respect to the total range shown between the different panels? If this was on purpose you could note in the caption that the ranges differ. If this was not on purpose you could consider harmonizing the ranges.

We will note in the caption that the ranges differ.

- There is this small notion "All Flags" in every panel. Would you mind to state in the caption and/or the text what you are refering to here?

Yes. We will either mention it in the caption or remove that mention.

- You could mention in the caption that these are full-mission results.

Yes, we will do this.

Note that these comments apply to the following figures of the similar kind.

Noted, thank you.

L483: "10-15 km resolution" --> Is it really the native resolution of the satellite imagery which counts here? Isn't it rather the 75 km grid spacing that is used?

It is indeed the native resolution of the satellite imagery we want to point out here. We think it is remarkable that the RMSE against buoy trajectories results in sub-image-pixel accuracy.

L525/526: "One exception seems to be ..." --> What does this mean and/or what do you want to state with this sentence? One could ask the question why this is not confirmed by dY and/or what went wrong with dX here?

We do not have an explanation for this different behaviour of the F14 dX RMSE. We wrote the sentence as a description of the figure. We might remove the sentence as it does not bring new information.

Figure 9: Consider enlarging the panels in this figure. It is almost impossible to see that MULTI-OI summer is identical to ERA5; you could used dashed lines for MULTI-OI so that ERA5 "shines" through. Where are the buoys?

Yes, the panels will be enlarged, both for the revised manuscript and for the edited version. We will consider using dashed lines for MULTI-OI. It was an error in our legend to indicate the buoys : we are showing the RMSE against the buoys so that we can't show the buoys independently.

L554: "the better of the SMMR period" --> What is this?

We will indicate the exact time period.

L569: "we compute drift vectors every six image pixels" --> you could emphasize that this applies to both x- and y-direction.

Yes, we will do this.

- Stupid question: Did you try to derive the same ice motion fields using a different set of center (?) 12.5 km grid cells, i.e. moved by 2 grid cells in x and 3 grid cells in y-direction?

We did not do this.

- The (?) I put behind center was to remind me that with 75 KM grid spacing there is no real center pixel because of the even number of 12.5 km grid cells per 75 km grid cell. A problem?

The center points of the 75km grid fall exactly on the center points of the 12.5km grid so there is no alignment problem.

L616: "our wind-derived ... in the northern hemisphere" --> I am not that much convinced about this partitioning into two distinct periods. Actually 4 of the 5 largest (by magnitude) biases fall into the AMSRX period. Also, while during the first decade positive biases are almost missing they occurred more often during the AMSRX period with values up to 0.3 km. I am therefore not convinced that the discussion as written and illustrated.

See the combined answer below.

L691/692: Again let me state that I am not that much convinced about emphasizing this first half / second half thing. Actually it is 11 years vs. 19 years, so the tuning is actually not just from one half of the 30 year CDR. I agree there are different biases but I don'T find the message is particularly clear. I would rather state the bias is quite variable and I am not sure whether all eventualities that might have caused this bias are adequately discussed. What do we know about potential trends in the ERA5 winds in the Arctic / Antarctic and/or how well (and with which data) has this parameter been evaluated in the polar regions?

For the two comments above : we will review our text and revise it with your input in mind. A bias or trend in ERA5 wind is indeed not something we can rule out. But in our revision we still want to keep the warning to our users to not use the summer data for trend analysis.

Another suggestion I have with respect to future plans is whether a two step validation process wouldn't perhaps be more promising. I am thinking of using the buoy derived ice displacement to evaluate SAR-based ice displacement vectors to have some spatial distribution of the reference data to compare the CDR with in a second step.

This is a good suggestion that might even help quantify the representativeness uncertainty between the buoy and the PMW sea-ice motion data. One issue with the SAR ice drift is that until recently with the availability of Sentinel 1A and 1B or RCM the repeat time of the imagery did not allow for many 24h drift vectors from SAR. It is not clear if we can use, e.g. ERS or Envisat SAR drift information from the 1990s and 2000s.

Typos / editoral comments:

L46: "mission" behind "AMSR-E" can be deleted.

Will do.

L62: It is correct that buoy data enter the product of Tschudi et al. However, this does only apply to the Arctic, not the Antarctic. You could consider adding this to your text.

Thanks. We will add this to the text. We might move this sentence following Reviewer 1's comment.

L78/79: Perhaps write a bit more about this data set? Are this gridded data? What is the temporal information?

We will expand. This is L1B data in swath projection, not gridded.

L88-90: Again it would be helpful to know how this data is provided (as swaths or as daily gridded data) and also information about the time sould be given.

We will expand. This is again L!B data in swath projection, not gridded.

L100: "full-resolution" means what?

This is the terminology used on the IABP website "Full Resolution Data (typically hourly)". We will add the same information "(typically hourly)" in our text.

L103-106: "In addition, ... Derocher 2020)." --> Would it make sense to the reader to put this information right behind the sentence dealing with the Arctic buoys ending in L101? All additional data sources given here are from the Arctic and none are from the Antarctic - unless I am mistaken.

It would make sense, yes.

L111: You could consider adding a short notion why the Fram Strait is excluded from the main validation and/or point to the respective later section wherein you deal with this issue.

Yes, we will point at the later section.

L121: "Theoterical" --> "Theoretical"

Will do

L214: "increase" --> "increased"?

Will do

L226: By "surface wind" you mean the 10-m wind?

This equation is quite general and can be expressed with both surface wind, geostrophic wind, and 10 m wind. The value of A and the turning angle would be different. We will consider change to 10 m wind since this is what we use in the end in this manuscript.

L247/248: "older and thicker sea ice" --> In L258 you rightly connect thicker sea ice with "a larger impact of neglected internal stresses". I believe it would be a good idea to mention this here as well.

Yes, we will do this.

L262: Please check the Greec letter for the turning angle. Also, there are two sentences beginnign with "Table 3 ..." which could possibly be merged?

We will change the Greek letter and avoid the repetition.

Table 3: Just a minor editoral remark: Please check how you want to (should) write the respective hemisphere ... Northern Hemisphere or northern hemisphere ... I guess there are rules but I am not aware of these at the moment.

We will check.

L285: Consider again adding the information that the NSIDC product uses buoys in the Arctic only.

Yes, we will.

L292/293: "The mask over ..." --> It might make sense to move this sentence to behind the one ending in L290 with "86N)"?

Yes, we will move.

L325: "sometimes to used" --> "sometimes to be used"

Yes.

L335: "would never contains" --> "would never contain"

Yes.

L389-391: Here you use two times the expression "validation", in form of "validation vector" and in form of "validation data". The reader would possibly appreciate to have this new term explained or, alternatively, replaced by what applies: "product" or "reference".

Yes.

L402: "variables the" --> "variables of the"

Yes.

L404: "product" --> "products"

Yes.

L409: "he" --> "the"

Yes.

L410: "We do to not report" --> please check.

Yes. Should have been “We do not report”. But this text will be part of a new text Sect X.X discussing the dX/dY vs u/v choice.

L487: "that that" --> please check.

Will do

L497/498: "The wind-driven ..." --> Please check this sentence for plural "s": "products ... seems ... it extends ..."

Will do

Figure 7: What does the "(NONE)" in the first line of the text in every panel mean?

We will remove it, it does not hold any information in that case.

L598: "does ... results" --> "does ... result"

Will do

L653: "th" --> "the"

Will do

Editor Comment

Two reviewers both find benefits to this data product, but also substantial issues. I invite you to continue to the next round. At ESSD, this is responding to review comments without updating the manuscript. If responses are deemed reasonable, then revisions.

Thank you for this invitation. We prepared replies to reviews (see above).

I note that CF compliance is mostly met but there are still two issues according to my compliance checker, and there are several missing "Highly Recommended" and "Recommended" fields for ACDD compliance. Please fix these.

Thank you. As you know, we already went through a round of revisions of our metadata after your initial feedback earlier in March. We did then fix the ERRORs and took on board some of the Highly Recommended or Recommended fields for ACDD compliance, but not all.

For example, the *cchecker.py* compliance checker “highly recommends” standard_name for all variables, but some of the variables in our file have no registered standard name at CF. This is fine (in a CF sense): if a variable does not correspond to a registered standard name it should not use a standard_name attribute. Also, none of the possible ISO 19115-1 data

types seemed to apply as coverage_content_type. We also do not think vertical datum is required for our 2D surface product.

All in all we think that our set of metadata is extensive, and that users will find the information they need. It is also too late for us to amend the metadata now that the data record was accepted by the EUMETSAT data portal, and was uploaded there.

We hope you agree that our metadata are suitable for publication of the paper in ESSD.

Please change "1991 to December 2020" to "1991 through December 2020" (and similar phrasing elsewhere, e.g., "1979 *through* 2016" on L100) if the data includes December 2020. If it does not, then "1991 through November 2020".

We will correct this and similar phrasing.

I would also like to see the processing software workflow released that generated these products. When I download your software from Zenodo and try to run it, I get an error:

OSError: [Errno -90] NetCDF: file not found: b'https://thredds.met.no/thredds/dodsC/metusers/thomasl/SIDrift_CDR_v1pre/auxiliary_files/inv_params_osi455_nh_200301-202012_1day.nc'

Which is OK! I'm looking for *open* science, not *reproducible* science. That being said, the software only seems to be code for generating figures. Please share your processing software. The actual work. This would let anyone who has any methods questions not answered by your manuscript find out the answer for themselves (for example, how map projection errors are handled).

For the OSError: we moved the CDR to its official location now, and we will update the github code with the new location when submitting the revised manuscript.

Releasing the processing software is already in our todo list from our interaction with you in March. We will have the processing software ready for final paper publication.

All figures: Bigger fonts!

Yes. We also hope that the figures will be bigger in the final manuscript format.

Figure 6: Not necessary but I'm curious if you did both X and Y axes on +- log could the color scale then be linear? Would this presentation show anything different? Useful? Matplotlib has a "symlog" function for this type of display.

We will investigate your suggestion. We are not familiar with symlog.

Have you considered and propagated projection errors from your EASE2 grid?

We are not sure what you mean. The EASE2 grid and projection is fully defined and there are no propagated projection errors into the product. Both the satellite TB images and the sea-ice drift vectors are on the same EASE2 projection, so there are no uncertainties there. Please explain what you meant in more detail if our answer is not sufficient.

I don't think a recommended citation format is appropriate. Please remove. That's not your decision, that's the decision of the place where the citation is used.

We think this can be useful for a potential user. We agree that the publisher or citation managing tool will define a citation format (hence our suggestion to use doi2bib to get a BibTeX entry). But there are several other situations where one could want to cite the product, e.g. in a presentation, a project report, a grant proposal,.... without a strict citation format. If you agree we will write something like: “In case no citation format is prescribed, we invite you to cite the dataset as: ”. But we can also remove it if you are not convinced.

Beyond these issues I raise, please carefully consider comments from reviewers. In particular, I too am curious about the motion vs. displacement issue raised by R2. If you prefer displacement over motion for some reason, would it be possible to provide a 'derived' motion product? I am concerned about the complications and user-error issues raised by R2.

This comment by R2 was very relevant indeed and we will give it our full attention when revising the manuscript (see our answer to R2 for details). We will also publish a notebook so that users can directly generate the ‘derived’ product with u/v velocity convention.