Review of tc-2021-223 : *Large-scale sea ice motion from Sentinel-1 and the RADARSAT Constellation Mission* by Howell and co-authors.

This manuscript describes a new processing setup for monitoring sea-ice motion at the pan-Arctic scale by taking advantage of satellite imagery from five C-band SAR satellite missions (Copernicus Sentinel-1 A&B, and the three missions from RCM). A first batch (10 months) of S1+RCM sea-ice motion data is prepared and evaluated qualitatively at pan-Arctic scales and regionally in the Canadian Arctic Archipelago. A comparison to two existing large-scale sea-ice motion dataset (from NSIDC and OSI SAF) reveals that the new S1+RCM data generally retrieves faster drift regimes, as well as more vectors in regions with intermediate concentrations and close to land.

The paper provides a description of the processing setup, and conveys well the main message that the recent availability of operational SAR missions opens a new era for large-scale sea-ice motion mapping. The paper is convincing and can be published with some more work.

As I see it, two weak points of the manuscript at this stage are 1) the lack of dedicated quantitative validation of the new S1+RCM drift vectors, namely against trajectories from on-ice drifters, 2) the lack of a stand-alone Discussions section where the choices and assumptions made in the new processing setup and its uncertainties are justified and discussed.

Major comments:

Validation against buoys:
The paper would be much stronger with a dedicated validation against buoy data at the pan-Arctic scale. Validation against buoy data is the de-facto standard for documenting the accuracy of sea-ice drift datasets (e.g. OSI SAF, NSIDC, Kwok 1998, etc…). In your case it would be particularly useful because validation of RCM SIM vectors (and thus geo-location, resolution, speckle) have never been assessed. You could also check the assumptions built into your uncertainty model (e.g. the scaling of the uncertainty on velocities by Delta_t, see discussion below). I strongly suggest that a dedicated validation against buoy data is conducted and reported here, but leave it to the Editor to decide if this major revision is required or not.

Sea-ice motion technique:
Section 3.2 is missing some details to fully characterize the processing. Some of the missing elements are:
* how old are the scenes allowed to be before they are not taken in the stack of scenes?
* In Fig. 5: we see that IMG1xIMG2, IMG2xIMG3, IMG3xIMG4, etc… are processed for SIM, but what about IMG1xIMG3, IMG1xIMG4, etc…? Considering these overlaps would dramatically increase the number of retrieved vectors and the sampling in the temporal domain. Please indicate if these additional overlaps are processed for SIM and, if not, add a discussion/justification why they were not considered (e.g. in a Discussions section).
* In Fig. 3: it is clear and well justified that S1 and RCM scenes are processed on their own (before the merging step). Are SIM vectors processed within the S1 and RCM missions? E.g. S1a with S1b, RCMa with RCMc, etc… Please add this information.
* Fig 6 a) gives the impression that S1 has a complete coverage of the dark blue region at least once on every week. Is it really the case, or are there weeks were S1 leaves some holes in the weekly coverage? Could these [0-1] average density be in a different color to better appreciate the weekly coverage? Same for b).
* L184: what is the justification for the cap at minimum 12 hours?
* starting L183: it is not immediately clear that you average the velocity vectors instead of the displacement vectors. Please clarify.

Uncertainty parametrization:
Section 3.3 presents an approach to uncertainty characterization. To date, there are no established procedures or algorithms for attributing per-pixel uncertainties to sea-ice motion vectors, yet alone for merged/averaged vectors like the S1+RCM ones. It is commendable that the authors design such an uncertainty model, but a discussions of the choices made would strengthen the paper.

In the absence of dedicated validation against buoys, the authors chose to base their uncertainty estimates on earlier results by Komarov and Barber (2014). However, these were obtained for a limited number of RADARSAT-2 scenes. Please discuss why the RADARSAT-2 results can be extended to RCM and S1.

L220: is it really the case that the uncertainty of the velocity is the uncertainty of sea-ice displacement divided by the Delta_t ? This should be better justified. For example take Fig. 5 in Lavergne et al. (2021): the RMSE of displacements increases slightly with time-separation Delta_t but not at a rate where twice the time-separation, twice the RMSE in displacement. The RMSE in displacement is controlled by the image "sharpness" and the presence of trackable features. Longer time separation can lead to larger decorrelation, but it is not intuitive that the uncertainty of a drift velocity from a pair of images with Delta_t = 12h is half that from a pair of images with Delta_r = 24h. If you keep this approach, please discuss and justify it more in the text.

The scaling of individual uncertainties into the merged uncertainties by Eq. 5 has some merit. Consider explaining in the text why such a parametrisation was chosen: what are the expected outcome in case, e.g. only a small temporal fraction of the week was covered by SAR scenes, etc… This will help the reader in addition to the description of the terms.

The value of the MCC on its own is not necessarily a good indicator of the vector quality (e.g. Hollands et al. 2015). Intuitively, a sub-window with no features (uniform grey) will match perfectly (MCC=1) with another grey sub-window, yet there will be no peak. Please discuss if the value of the MCC is a good metric of quality.

T. Hollands, S. Linow and W. Dierking, "Reliability Measures for Sea Ice Motion Retrieval From Synthetic Aperture Radar Images," in *IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing*, vol. 8, no. 1, pp. 67-75, Jan. 2015, doi: 10.1109/JSTARS.2014.2340572.

Comparison to NSIDC and OSI SAF:
Fig. 16 you compare the products in terms of absolute velocities, while the products deliver velocities along x and y components. In Lavergne et al. (2010) we note that: "The transformation from vector components to total velocities is strongly non linear and is known to create artificial pseudobiases that might hinder valuable conclusions to be drawn [Stoffelen, 1998, Appendix B]". Did you try to compare the products in terms of x/y components and did you find similar results?

Stoffelen, A. (1998), Towards the true near-surface wind speed: Error modeling and calibration using triple collocation, *J. Geophys. Res.*, **103**, 7755–7766.

L364-365: the 25km resolution of the NSIDC product is mainly oversampling (the original PMR input data have a coarser resolution, see Table 2 of the NSICD V004 User Guide).

Other:
L243-245: it is not intuitive to me that such a seasonal cycle as shown Fig 8 is expected. During summer melt, the ice is thinner and more free to move when pushed by winds (smaller internal stress). In fact Rampal et al. (2009) Fig 4.c find a different seasonal cycle from IABP buoys, with a ramp-up during summer and a maximum in September. Please discuss.

Minor comments:

L49: The dataset based on passive microwave indeed have coarse resolution, that rather are in the range (50 – 100 km) than (12-25 km) as stated here. The OSI SAF is ~60km like the data from IFREMER/CERSAT, Kwok's is ~100km. NSIDC's 25km grid results from oversampling (see e.g. Table 2 of the NSIDC V004 User Guide).

L75: Did you use the multi-sensor OSI SAF product (multi-oi) or the single-sensor products (from AMSR2, SSMIS, etc…) Please provide this information.

Fig 14: the labels and legends are hardly readable. Please enlarge the text.

Conclusions: with "swath-to-swath" approach, SIM from passive microwave now achieves sub-daily temporal resolution (Lavergne et al. 2021). This will be extremely difficult to reach consistently and pan-Arctic from SAR constellations alone. Maybe the complementary of SIM estimation from SAR and "swath-to-swath" PMW would deserve a mention in the Conclusions.

Editorials:

L15: delete "able to be"

L18-19: OSI SAF, without "-" (in long form and acronym).

L49: "trade-off with respect to" → "drawback of" or "limitation of".

L73: replace "2020" with "this period".

L86: here and later in the section: "coarser spatial resolution levels". Consider changing "levels" with "images" for clarity.

L90. Lowest resolution → coarsest resolution

L135. "at _least_ 32,000 km2"

L250. Based _on_ the weekly image….