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Discussions
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# *Interactive comment on* "Impact of assimilating a merged sea ice thickness from CryoSat-2 and SMOS in the Arctic reanalysis" *by* Jiping Xie et al.

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Thank the review for the constructive comments. We would like to answer the points one by one as follows.

General comments The authors address the impact of assimilating merged sea ice thickness data into an operational system in order to improve challenging and important forecasts of sea ice thickness in the Arctic Ocean. They use the CS2SMOS data for this purpose and perform one year (March 2014-March 2015) Observing System Experiment (OSE) with the TOPAZ system. The assimilation of CS2SMOS data assimilation improves the modelled sea ice thickness when compared to CS2SMOS and independent data and, to a less extent, the modelled sea ice drift. Compared again to



CS2SMOS merged data, the total sea ice volume also is improved and, as expected, no improvement of sea ice extent but without noticeable degradation. A quantitative impact of the observational network with the Degrees of Freedom for Signal (DFS) approach shows the dominant source of information of the sea ice thickness in the central Arctic Ocean.

The paper is comprehensive and shows the positive impact of assimilating the merged CS2SMOS ice thickness product in reanalysis mode. I recommend publication with minor revisions considering the points below, and especially parts of the work with in situ analysis. Even if it has been already found by Mathiot et al. (2012) with a different approach and data, the authors should highlight more the important finding of improvement of the system (SIT and SIV) outside the observed period, e.g. in the abstract for instance. -A: Thank you for this suggestion. We have tried to better emphasize some key results of our study in the abstract about the formulation of the observation error about reliability and about sea ice volume.

The impact analysis with independent data deserves the use of all available data. In situ sea ice thickness data from the "Unified sea Ice Thickness Climate Data Record" such as Air-Em data (http://psc.apl.uw.edu/sea_ice_cdr/Sources/airborne_em.html) are available within the time period of these experiments. This wouldn't need any re-run of these experiments and may reinforce assessments.

-A: We have downloaded the data and performed a validation. We prefer not to include it because we feel it is inconclusive. The data coverage is very small and the variability in the observation is much larger than the misfit between the two OSE runs as shown in Fig. B.

Fig. 1 (shown at last) Left: the locations of SIT observed by AIR-EM during this experimental time period. There are 8 points from 5th April 2014 to 7th April 2014 near the Chukchi Sea. Right: measurements presented as daily means than their standard deviations (black) collocated with the two model runs (blue triangles and red stars)

Note that we have complemented the paper with the validation against the WHOI ULS data as recommended by the first reviewer. The validation for that data set is consistent with the rest of the analysis.

The impact onto the sea ice drift is low but need better relative quantification. And comparisons with independent IABP drifting buoys deserve a better methodology, suggested below. I'm not a native English-speaker but I think the English should be corrected in few parts mentioned below. -A: We have been through the paper and tried to improve the English and the formulation.

1 Introduction Page 3 line 63: replace by "...and the use of reanalyses..." -A: Corrected, thank you

Page 3 line 83: replace by "...with high accuracy the sea ice freeboard ..." -A: Corrected, thank you

Page 3 line 85: replace by "...because of approximations..." -A: Corrected, thank you

The context in the introduction part should include the recent work made by Mu et al. (2018) using the same data in their LSEIK filter. -A: That paper was already cited in discussion but we agree it is important to refer to it in the introduction.

It is added P4 L107-110: "The Ensemble Kalman Filter has previously been demonstrated for assimilation of SIT data (Lisæter et al., 2007) or freeboard data (Mathiot et al., 2012) or C2SMOS data Mu et al. (2018)."

Page 4 line 114-115: add recent paper of Uotila et al. (2018) in their intercomparison of reanalysis including TOPAZ in polar oceans. Uotila et al. (2018): An assessment of ten ocean reanalysis in the polar regions, Climate Dynamics. Doi:10.1007/s00382-018-4242-z. -A: The paper is cited at Line 74.

Page 4 line 120: replace by "...too thick ..." -A: Corrected, thank you

Page 5 line 123 : " ...and for reanalysis (Chevallier et al., 2016, Uotila et al., 2018)



Chevallier et al. (2016) : Intercomparison of the Arctic sea ice cover in global ocean sea ice reanalyses from the ORA-IP project, Climate Dynamics, doi: 10.1007/s00382-016-2985-y -A: Thanks, it is changed. As P5 L124-127: "Similar biases for SIT have been reported for other Arctic coupled ocean-ice models (Stark et al., 2008; Johnson et al., 2012; Schweiger et al., 2012; Yang et al., 2014; Smith et al., 2015) and for reanalyses (Uotila et al, 2018)."

Page 5 line 138: "strongly" really? -A: The ocean observations are used to update the state vector of ocean and sea ice observation are also used to update the ocean state, so it can be considered as a strongly coupled data assimilation of ocean and sea ice.

The following sentence and reference were added to avoid confusion with the "strength" of the assimilation's pull towards observations: See P5 L142-145: "TOPAZ4 uses strongly coupled data assimilation of ocean and sea ice - Meaning that sea ice observation will impact also the ocean and vice versa (see Penny et al., 2017; Kimmritz et al., 2018) - with a flow dependent assimilation method."

Penny, G., Akella, S.R., Frolov, S., Fujii, Y., Karspeck, A., Peña, M., Subramanian, A., Tardif, R., Wu, X., Anderson, J., Kalnay, E., Kleist, D.T., and Todling, R.: Coupled Data Assimilation for Integrated Earth System Analysis and Prediction : Goals , Challenges , and Recommendations. Technical Report, https://ntrs.nasa.gov/search.jsp?R=20170007430, 2017. Kimmritz, M., Counillon, F., Bitz, C.M., Massonnet, F., Bethke, I. and Gao, Y., 2018. Optimising assimilation of sea ice concentration in an Earth system model with a multicategory sea ice model. Tellus A: Dynamic Meteorology and Oceanography, 70(1), 1435945, https://doi.org/10.1080/1600870.2018.1435945, 2018

Page 6 line 159: Chassignet -A: Corrected, thank you

Page 6-7-8: the description of the assimilation system in TOPAZ in paragraph 2.2 could may be shortened by giving relevant references to Sakov and Oke (2008), Xie et al. (2017), Evensen (1994) for the Kalman Gain, etc ... -A: We tried to shorten this part, skipping the model details without direct relation to the manuscript. However, it is very hard to shorten the data assimilation section as the Kalman gain is for example used for the DFS Section, the observation error for the representativity error part, the innovation term in many places. We have therefore kept this Section unchanged.

Page 8 line 224: Details of assimilated data in Table 1 doesn't give much information for the scope of this paper, you may remove it. -A: The table has been removed

Page 8 line 225: "superobed"? -A: We have provided a definition of the term superobed: As P8 L233-236: "All measurements are retrieved from http://marine.copernicus.eu, and are quality controlled and superobed – i.e. all observations falling within the same grid cell are averaged and the observation uncertainty is reduced accordingly (Sakov et al., 2012)."

Page 8 line 231-232: sentence to be rephrased -A: The sentence has been rephrased: P8 L238-239: "Similarly, the sea ice drifts during the last 2 days of the assimilation cycle are assimilated from OSI-SAF."

Page 8 line 241: rephrase "…is on the low side". -A: We have rephrased the sentence: See P8 L247-249: "As such, we expect that this observation error is only accounting for a part of the real error and misses both the sensor errors and the model-related representation errors."

Page 8, end of paragraph 2 and paragraph 3: this part should be more explained, the initial observed error used in the sensitivity assimilation experiment is the one from CS2SMOS? It seems that you use Desroziers to inflate observations errors but Desroziers could deflate initial errors; rephrase the methodology please. Authors attribute to SMOS the presence of discrepancies in Desroziers diagnostics in Figure 2, but what is the source of abrupt changes in Desroziers diagnostics above 3m thickness of sea ice? -A: We have tried to clarified this section. We only use the Desroziers method as an indication to qualify the choice of observation error (can be either larger or lower than the initial obs error). In the present experiment (Fig 2) the chosen er-



rors are always larger than the Desroziers diagnostics, as a precautionary choice. We suspect that the discontinuities above 3 meters are due to the fact that there are fewer CryoSAT2 observation or model values in certain intervals of ice thickness values. These abrupt changes were another motivation for not applying the Desroziers diagnostics blindly, but more like a lower bound. The paragraph has been re-ordered to make this point obvious.

See P9 L273-282: "In Fig. 2, the diagnosed observation errors from Desroziers et al. (2005) are larger than the mapping error included in CS2SMOS, but still do not account for biases in the CryoSAT2 and SMOS observations. The CS2SMOS mapping error is particularly low for sea ice below 0.5 m: about 4 times lower than the uncertainties obtained by error propagation in the SMOS processing chain (used in Xie et al. 2016), which would make the assimilation of SMOS SIT too strong. The Desroziers diagnosed errors generally increase with ice thickness, although they vary unrealistically for SITs above 3 m, possibly due to low counts of either modelled or observed ice thickness in certain thickness ranges."

Page 9: The estimated observation error (eq. (6)) is pretty large compared to initial CS2SMOS error, how this minimum threshold of 50cm has been chosen, with short sensitivities tests?

-A: The following text was added: As P9 L286-295: "At low SIT, the resulting values are slightly higher than those used in Xie et al. (2016) and comparable to the Desroziers diagnostics. At SITs of 1.5m, for which SMOS and CS2SMOS overlap, the added correction is comparable to reported differences between the two satellites: about 20 cm in the Beaufort Sea and 1 meter in the Barents Sea, see Table 3 in Ricker et al. (2017). Tilling et al., (2018) show that the standard deviations between the CryoSat-2 and independent measurements are between 30 and 70 cm depending of the source of observation and increase with ice thickness (their Figure 16). It should be noted however that the processing of CryoSat2 data differs in CPOM and AWI's algorithms."

3 OSE runs and validations Page 10 line 296: "…is set to 0.1 m, …" -A: Corrected, thank you.

Page 10 line 307: "…is the total number of time steps …" -A: This was unclear, we use : "where L is the total number of assimilation cycle over the study period"

Figure 1: locations of IMB buoys are hardly detectable, use another colorbar? How coastal areas are defined? -A: We have revised the figure with a new colormap and add an enlarged panel.

3.2 Validation against CS2SMOS and innovation diagnostics Page 11: line 328: "…have all been improved" but how much is the improvement? -A: The overall quantitative improvement is given with text on the figure.

Line 335:"…but is thickened in the Test run." But once again how much? The assimilation of CS2SMOS in the Test run definitely show improvements but Figure 3 doesn't help to know how much and where are these changes compared to CS2SMOS? 2D maps differences with CS2SMOS in Figure 3 (replacing mean values) will help to better quantify these regional changes and better understand the Figure 4. -A: The quantitative improvement is given in the statement below: See P12 L366-370: "The averaged SIT in the Test run around the North pole (>80°N), is increased from 1.3 m in the Official run to 1.6 m, which is closer to CS2SMOS by 43%. In the marginal zones of the East Siberian Sea, the Laptev Sea, and the Kara Sea, the SITs in the Official run is too thin, but is thickened in the Test run."

For Figure 3, we prefer showing the SIT than the difference because it helps relating the change to the observation sources of SMOS and CryoSat-2. We have added more isolines to better highlight the impact.

The text is also changed at P11, L355-361: "In the Official run, the thick sea ice to the north of the CAA is underestimated but thickens slightly in the Test run: the 3 m SIT isoline covers a wider area, in better agreement with the observations. The areas



of thinner sea ice north of the Barents Sea, west of the Kara Sea, and the coast of the Beaufort Sea, which were too thick in the Official run, have all been improved also shown by reduced area delimited by the isolines of 1 m or 2 m SIT in the Test run."

Page 11 line 349: replace by ".. is not significantly impacted by…" -A: Corrected, thank you.

Figure 3: Color bar of SIT s unreadable, Put row and columns titles instead of yellows labels which are hardly readable. -A: Thanks the figure clarity have been improved.

RMSE or RMSD?: RMSD or RMSE are used all along the paper (text and figures), please be consistent. -A: Thank you. This has been corrected (RMSD).

Considering the Figure 3 in March 2015, how do you explain in Figure 4 the bias in Official run vanishes with time? Error compensations? -A: Yes indeed error compensations. As shown in Figures 3, 5, 6, and 7, the overestimation in the Beaufort Sea plays an important role to balance the underestimation in north of CAA and Greenland. Clearly, the biases in the Arctic marginal seas like in the Beaufort Sea always vary and may have a strong seasonality and interannual variability.

We have changed the text as follows: As P13 L395-399: "The bias in the Test run converges to 0 and fluctuates around that level but this is likely not the influence from the assimilation as the bias in the Official run also converges to 0 during that time, albeit slower. This is likely due to the compensation of seasonal and regional errors. "

Page 13 line 383: "The RMSE (RMSI?) stabilizes at a value close to 0.4m". From Figure 4, the RMSI, and total uncertainty, seem to grow with time and with the number of assimilated observations, how do you explain that? -A: The observation error is also included in the total uncertainty. This uncertainty is zero in open water. We have now added the observation errors to the plot. Although the observation number is increasing, their accuracy decrease with the amplitude of the thickness and the newly frozen areas change from zero uncertainty (open water) to at least 50 cm of uncertainties.

Page 12 lines355-360: too long sentence, please rephrase it. -A: Thank you. We have rephrased the sentence: See P13 L391-395: “After the observations resume in the end of October 2014, the SIT RMSD is comparable between the two runs but the bias is slightly lower in the Test run. There is large spike in the bias and RMSD for both systems that relates to an inaccuracy of the CS2SMOS observations (see Section 4.2). After the spike, the RMSD and bias in the Test run are lower than in the Official run.”

Page 12 line 364: “The innovation statistics”? Rephrase the entire sentence please. -A: This is done: At P13 L403-404: “The innovation statistics taken at each assimilation time are used to evaluate how well our data assimilation system is calibrated.”

Page 12 line 375: remove “Then” -A: Then has been removed.

Page 13 line 383: RMSI or RMSE? -A: Corrected to RMSD

3.3 Validation against independent SIT observations From Figure 1 2013F and 2014B buoys seem to be located in the Canadian Basin (Beaufort Gyre), the fact that assimilating CS2SMOS improves the system in this area outside the observed period is an important finding. -A: Thanks.

Page 13 line 409: is the assimilated SIT really “pulled back” to the observations? Not clear. -A: Indeed. We have corrected this statement: As P14 L444-446: “When CS2SMOS is assimilated again in the fall 2014, the Test run initially overestimates slightly the SIT measured at the buoy compared to that in the Official run but is slowly improving as data is assimilated.

Page 15 line 455: the large spread of scatterplots explains low values of R2 (give definition) in Figure 7 and then a weak significant linear regression, this should be commented. -A: The text has been modified as follow As P17 L533-537: “The Test run shows improved linear correlations to the observation. The offset at the origin is reduced (0.52 m instead of 0.93 m) and the slope is closer to 1 m than in the Official run. The linear correlation in the Test run is slightly increased as indicated with the



correlation squared R2. There is still a lot of spread that explains why the correlation is on the low side.“

Figure 6: the two bottom plots are nearly undistinguishable, replacing Test run plot by differences between Test and Officialruns would be more helpful. -A: Thank you for this suggestion. We have added one panel to show the SIT differences in two model runs in the revision.

Figure 7: Lighten encapsulated text in the box and put it in the legend instead. Lines of linear regression are dotted or solid? -A: Thanks, this has been corrected as suggested. Linear regressions are now dashed.

4.1 Impact on the sea ice drift Addressing the impact of SIT assimilation onto the sea ice drift certainly is worthwhile and use of satellite measurements together with in situ data clearly assesses the results. But do we need such a long section by reminding classical equations such as the 2D momentum, total mass of ice and the conservative law. Please refer to adequate papers such as Hibler for example and shorten this section. -A: Thank you for this suggestion. In the revision, we have shortened the section and listed the factors that affect the sea ice drift.

Figure 8: Idem Figure 3, color bars are hardly readable (use more ticks) and put row and columns titles instead of small black labels which also are hardly readable. -A: The figure is updated according to the suggestion.

Figure 8: It is true that differences are pretty weak and could be found by only modifying the air-ice drag for instance. However, different ice thickness patterns could impact ice drift patterns, a plot showing Official and Test runs differences could highlight differences in large scale ice drift patterns. If patterns have no differences, just mention it. -A: We have tried to improve this section as suggested:

So the related illustrations are changed as P19 L598-606: “The RMSD of sea ice drift speed in two days is reduced by about 0.1-0.2 km in April 2014 and February 2015 for the whole Arctic, which corresponds to a reduction of less than 5% of the RMSD. However, near the North Pole (north of 80°N), the reduction of drift RMSDs is more important, by about 0.4-0.5 km. In December 2014 and February 2015 it is about 8-9% of the error in the Official run. Near the North Pole, the averaged SIT in March 2015 (Fig. 3) is about 10% thicker in the Test run than in the Official run. The impact is more important there than in the rest of the Arctic and well in line with the sensitivity found in ON14."

Page 17 line 521: "...2 days ..." what this refers to? -A: We meant a 2-day sea ice drift trajectory, as in the OSI-SAF product.

Page 17 line 523: insert OSI SAF data reference instead than the Table 1. -A: Table 1 has been removed as suggested, but the reference to OSI-SAF data has been added. Lavergne, T., Eastwood, S., Teffah, Z., Schyberg, H., and Breivik, L. -A.: Sea ice motion from low resolution satellite sensors: an alternative method and its validation in the Arctic. Journal of Geophysical Research, 115, C10032, 2010. doi: 10.1029/2009JC005958, 2010.

Page 17 line 527: how much this 0.2-0.3 km/day represent compared to the mean value (give a percentage for example)? -A: The percentage of improvement is now given.

In consequent, we correct the related illustrations as P19, L590-598: "The RMSD of sea ice drift speed in two-days trajectories is reduced by about 0.1-0.2 km in April 2014 and February 2015 for the whole Arctic, which corresponds to a reduction of less than 5% of the RMSD"

Page 18 lines 544-560: IABP essentially sample locations of important ice flows areas such as Transpolar Drift Stream and Beaufort Gyre; Sumata et al. (2014) for instance made intercomparisons with OSI SAF and IABP and found relative agreement among these products. It would have been more appropriate to collocate (in time & space) both OSI SAF and experiments into IABP space to evaluate experiments



for IABP ice drift regimes. H. Sumata et al. (2014), An intercomparison of Arctic ice drift products to deduce uncertainty estimates, J. Geoph. Res., 119, p. 4887-4921, doi:10.1002/2013JC009724. -A: Our statement was unclear, we believe that the two datasets agree well with each other, as shown by the OSI SAF calibration report, but that the spatial coverage by IABP misses the largest signal near the North Pole. Thank you for the reference.

The statements are changed as P20 L634-637: "In Fig. 1 we can see that buoys north of 80°N are mainly found in the Eurasian Basin and sample poorly the region between the Transpolar Drift Stream and the Beaufort Gyre (Sumata et al., 2014), where the SID misfits are largest and where the model drift is too fast.

Figure 9: Meaning of "152/22329" in the top panel? -A:The text was corrected as follow: At P20 L621-622: "A total of 151 buoys are left from this selection, which provides 21,793 daily estimates of drift speeds." .

Page 18 lines 561-565: put these lines in the ice thickness validation context. -A: We agree that it would make more sense but the thickness validation from IMDB is too small to be meaningful and we prefer to keep it here in order to relate the drift updates with the thickness change.

4.2 Impact on the sea ice extent and volume in the central Arctic Page 20 lines 616-620: The spike end October-beginning November is related to SMOS measurements then? This Figure 11 should be more discussed in light of this event or removed. -A: Thank you for this comment. The figure provides the possible explanation to the spike in Fig. 4, 5, and 11, and helps us to understand the disadvantages about the CS2SMOS. We add more comments in the revision as P22 L695-699: "The weekly SIT innovation on the 2nd Nov reveals that the increase is largest south of the Eurasian Basin and around the Fram Strait. There, the SIT is thinner than 0.3 m on the 26th Oct and may suggest that the problem comes from the SIT measurement from SMOS."

Figure 10: to be corrected "...the test run (red-dotted)..." -A: Corrected, thanks

Figure 11: Put row and columns titles instead of small black labels which are hardly readable. -A: Thanks, the figure is changes as the suggestion.

4.3 Quantitative impact for the observational network The number of in situ data during these two months are pretty low, and more generally the years 2014 and 2015 have a pretty low number of CTD profiles compared to others years (see Behrens et al. 2018 https://www.earth-syst-scidatanet/10/1119/2018/essd-10-1119-2018.pdf for instance). Given the relative importance of in situ when these data are present (Figs 12 & 13 c)), does it mean that with a more homogeneous in situ network the CS2SMOS won't be the major source of information in the central Arctic? The DFS is an indication of the impact of one assimilated observation in regards to the others. But these observations are usually complementary to each other and give different sources of information, e.g. sea ice vs water masses or surface vs vertical distribution. -A: The DFS is an online metric that is for our entire state vector. As you mentioned profiles under sea ice are very few and as a consequence the ensemble spread is very large in the ocean. It should be added that comparatively the sea ice is better observed than the ocean underneath. In this context, if a homogeneous in situ observation network were made available it would clearly dominate initially because the ocean is currently nearly unconstrained, but after some time a more a balanced share will be expected. Note that at the height of the IPY there were no more than 10 profiles per week for the whole Arctic Ocean, so the prospect of a homogeneous coverage is still very remote. Similarly, we may except that the contribution of DFS for sea ice thickness will decrease with time as the realism of our sea ice thickness improves.

We have tried to hint to this idea in the discussion: See P25 L815-826: "However, the impact of SIT observations may vary with the evolution of the modelling and observing system. [...] Lastly, if a large number of in situ profiles were available below the sea ice, they would also compete with the SIT observations."

Page 20 line 640:"cannot exceed" -A: Corrected, thanks.



Page 21 line 653:"...are dominant close to the sea ice edge ..." -A: Corrected, thanks.

5 Conclusions and discussions We understand that the application of such developments is reanalysis and hindcasts experiments. According to 1) this merged data is not accessible in NRT (2) Cryosat-2 data is available in NRT (Tilling et al., 2016) (3) the recent work of Mu et al. (2018) (referenced in the paper) showing that "...the sea ice fields obtained by the joint assimilation of SMOS and CryoSat-2 data also have lower errors in thickness and concentration than those obtained from directly assimilating a statistically merged SMOS and CryoSat-2 sea ice thickness product." and 4) TOPAZ is the NRT Arctic Ocean operational system of CMEMS; it is surprising that assimilating CS-2 and SMOS together and separately is not a prospective of this work. -A: We agree with you and we have justified our choice in the conclusion:

See P26 L842-850: "An alternative to using the scheme CS2SMOS data would have been to assimilate the two data sets CryoSat-2 and SMOS SIT separately and let the EnKF merge them together rather than relying on optimal interpolation, as successfully demonstrated by Mu et al (2018). This would for instance avoid assimilating observations in places where they are the pure result of interpolation/extrapolation, but would not resolve the offset between the two satellites, which is arguably the most worrying issue as of the present state of the SMOS and CryoSat-2 data. The assimilation of the separate datasets will be attempted in the future when their consistency is further improved.

Page 23 line 737: "...which would reduce the IF of SIC" but also the IF of SIT because in situ and SIT are largely overlapped. -A: Thanks for this comment. The sentence was removed and replaced by as mentioned 4 questions ago.

Page 24 line757: "...as seen in Fig. 10". -A: Corrected, thank you.

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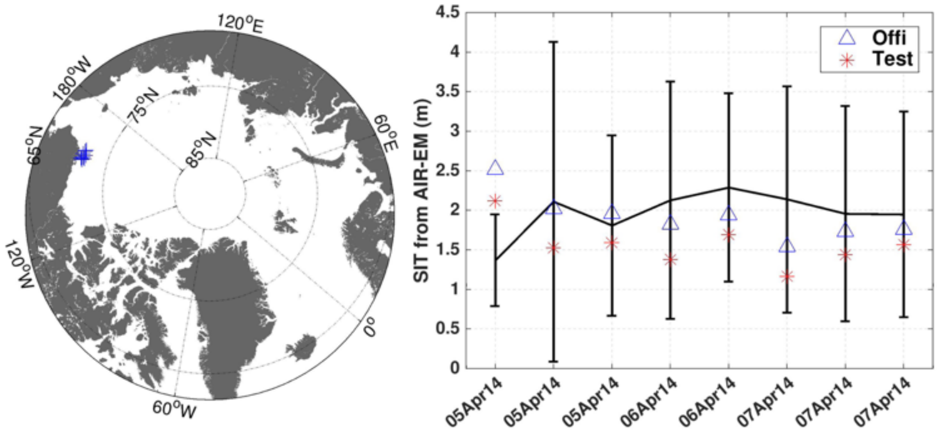
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**Fig. 1.**