Dear Editors and Reviewers,

The authors, above all, would like to thank the editor and the reviewers for their comments to help to improve the manuscript. We have now carefully reviewed and addressed all of comments which we hope meet with approval, with revisions to the manuscript shown in red. The primary corrections in the paper and the responds to the reviewer's comments are as flowing:

Responses to Reviewer's Comments:

Reviewer #1:

One technical comment:

Line 629 in references change "Rasmus, T., John, L." to "Tonboe, R., J. Lavelle"

Response: Thank you for the valuable comments. We have revised to 'Tonboe et al. (2016)' (Line 150, 154, 509 and 717-718). We have revised to 'Aaboe et al. (2021)'(Line 158, 509 and 524-526). We also have checked all referneces through the manuscript. We supplied the some references (Line 556-557, 625-627, 645-647, 649-651, 699-702, 715-716, and 734-736).

Reviewer #2:

I'd like to thank the authors for the extensive edits, added work, and detailed response made to all the reviews. This is my second time reviewing this manuscript, so my comments will primarily focus on the edits made since the last review as well as the author's response to the reviews, but I will also include some additional comments that I realized when reading it a second time. Unfortunately, even with the extensive edits and comments provided by the authors, I still have concerns that should be considered before publication.

The authors have since the original submission changed the processing slightly to include not-a-number values when sea surface height anomalies (SSHA) were not available (instead of providing a value of 0), as well as used the 15 lowest (instead of 9) points within a 25-km segment as a measure of the sea level within leads (SSHA). This has improved their statistics when comparing with reference data (Operation IceBridge data) for both sea ice freeboard and sea ice thickness estimates. They have produced additional studies in response to reviews, where they have assessed waveform parameters/features to use for discriminating between leads/floes instead of their current methodology. However, this did not yield better results than their current methodology (and have therefore not been included in the manuscript). The authors have amended their uncertainty estimation, which now presents more feasible estimations, and provided a more extensive discussion on the limitations of their methodology and impacts hereof.

General comments

I am happy to see the positive impact that the change in processing has shown in your comparison with OIB, however the statistics are still not a positive for HY-2B freeboard nor thickness estimates as the CryoSat-2 estimates. This, again, tells me that the processing chain of AWI (lead/floe discrimination, TFMRA50 re-tracker) is likely the way forward even with the different footprint and altimeter specifications of HY-2B compared with CryoSat-2. I do recommend the authors to investigate using waveform parameters for lead/floe discrimination as well as applying a re-tracker commonly used for sea ice (such as TFMRA50), however I understand that it will require quite the effort. If this is not possible, I think it is crucial that the manuscript clearly reflects (and mentions) the limitations of the methods and data used in this study. Below I have mentioned some suggestions for implementing this.

Response: Thank you for the valuable comments. The processing chain of AWI (lead/floe discrimination, TFMRA50 re-tracker) provide a good reference for HY-2B L1 data. This will be the way of HY-2B forward.

I think it is important to highlight that this study is a feasibility study. What I mean, is that you are in fact not truly assessing the retrieval ability of HY-2B for Arctic sea ice

thickness, since you are not using re-trackers commonly used, or designed, for sea ice–in fact, you use observations provided by an ocean-preferred re-tracker. As such, your results are also limited by this. I therefore propose you change the title of your manuscript to reflect this. Suggestions could be: "Feasibility of retrieving Arctic sea ice thickness from the Chinese HY-2B Ku-band radar altimeter" or "Assessment of Arctic sea ice thickness retrieval ability of the Chinese HY-2B Ku-band radar altimeter: a feasibility study". Also, I think it is crucial that you state the purpose of this study in the end of the introduction, highlighting that you are aiming to investigate whether HY-2B can be used for sea ice, but that you are limited to already provided higher-level products, and that it is not within the scope of the study to derive a freeboard product using your own re-tracker from the HY-2B product.

Response: Thank you for the valuable comments. We have revised the title to "Feasibility of Retrieving Arctic Sea Ice Thickness from The Chinese HY-2B Ku-band Radar Altimeter". We have revised 'With the continuous development of China's Marine Dynamic Environment Satellite (Haiyang-2B, HY-2B), the HY-2B satellite can be used to observe polar sea ice' to 'With the continuous development of China's Marine Dynamic Environment Satellite, the feasibility of using the HY-2B satellite to map the polar sea ice must be explored.' (Line 72)
We have also supplied the limitation of this study. 'It is important to note in this study, however, that we are aiming to investigate whether HY-2B can be used for sea ice, but that we are limited to already provided higher-level (SGDR) product, and that it is not within the scope of the study to derive freeboard product using own re-tracker from the HY-2B SGDR product.' (line 72-75).
We have also revised 'In this study, we preliminarily tried to use HY-2B radar altimeter to retrieve reliable Arctic sea ice thicknesses. However, the shortcoming of this work is that we did not accurately distinguish between floes and lead. We did not re-track the SGDR products since they have been re-tracked using the Brown model.' to 'However, we are aiming to investigate whether HY-2B can be used for sea ice, but that we are limited to already provided higher-level (SGDR) product, and that it is not within the scope of the study to derive freeboard product using own re-tracker from the HY-2B SGDR product. The deficiency of this work is that we did not accurately distinguish between floes and lead.' (line 484-486).

I'm happy to see a study using the waveforms and classifying the waveforms using pulse peakiness (PP), however slightly confused to see that it was in fact not satisfactory. Based on your response, I see some aspects that could potentially explain this: (1) PP can be calculated in different ways (that is to say, sometimes they are tweaked in different studies). I cannot see how you have calculated PP, but I do find it exceptionally odd that HY-2B is not able to use it to classify by–whereas all other radar altimeters have in fact successfully used this. Therefore, it may be due to how you have calculated PP; (2) For HY-2B we do not yet know which PP thresholds to use. You have tried several PP thresholds (not sure what the selection of thresholds were based on) but choosing high PP's (which would normally yield a classification as lead)

resulted in almost no lead observations. This might be a result of the already low data coverage, which you also suggest, but I do propose that for the next time you look at this, that instead of pre-defined PP's, you choose a selection of waveforms and make a statistical analysis to derive the PP thresholds. I am aware that you state this is work for future studies, however it will unfortunately be those studies that have the true value then.

Response: Thank you for the valuable comments. We used the equation (1) to calculate the PP value.

$$PP = \frac{\max(WF_i)}{\sum_{i=1}^{128} WF_i} \times 128 \tag{1}$$

where WF_i represents the echo power at range bin index i.

We tried to use low, middle and high PP values (3,7,10 and 15) to extract lead points, but choosing high PP's (which would normally yield a classification as lead) resulted in almost no lead observations, choosing low PP's resulted in sparse floe observations (this might be a result of the already low data coverage). So we chose to use PP value greater than 10 to extract lead points. We will choose a selection of waveforms and make a statistical analysis to derive the PP thresholds in the future studies.

There is very little data available by HY-2B. Why is that? It is based on the selected re-tracker where some restrictions/flags are applied? There must be applied some post-processing steps that remove the data. Could you provide paragraph explaining when in the processing this data is removed, and why it was removed? Surely, this should not be the case when a more appropriate re-tracker is used. Could you also provide a measure of how much coverage HY-2B has compared with CryoSat-2? (e.g., how many points within each grid cell when gridding).

Response: Thank you for the valuable comments. The HY-2B altimeter uses two different tracking modes: suboptimal maximum likelihood estimation (SMLE) and offset center of gravity (OCOG). The two tacking modes can exchange according to the observation surfaces. For areas with slower changes in terrain height, such as the ocean and large areas of flat sea ice, the SMLE tracking mode is used. For areas with more dramatic changes in topographic height, such as land and sea ice areas, the OCOG tracking mode is used. The HY-2B Level-2 altimetry products (SGDR products) we used do not have OCOG data. Different re-trackers correspond to different flags in HY-2B L1 data. The data of OCOG re-tracking is not stored in SGDR products, because researchers only consider using it for ocean research when making SGDR products.
According to your valuable comments, we have conducted a statistic of the number of points within each grid cell when gridding the HY-2B radar freeboard, and have compared with CS-2 over the common area, as shown in Figure 1. As the data points of

the HY-2B SGDR products without OCOG re-track data is sparse, the number of points in HY-2B grids are generally less than CS-2. The mode of the data points within HY-2B grid cell is 5, and the mode of the data points within CS-2 grid cell is 104. The cumulative probability of measuring points greater than or equal to 15 within HY-2B grid cell is 58.3%.

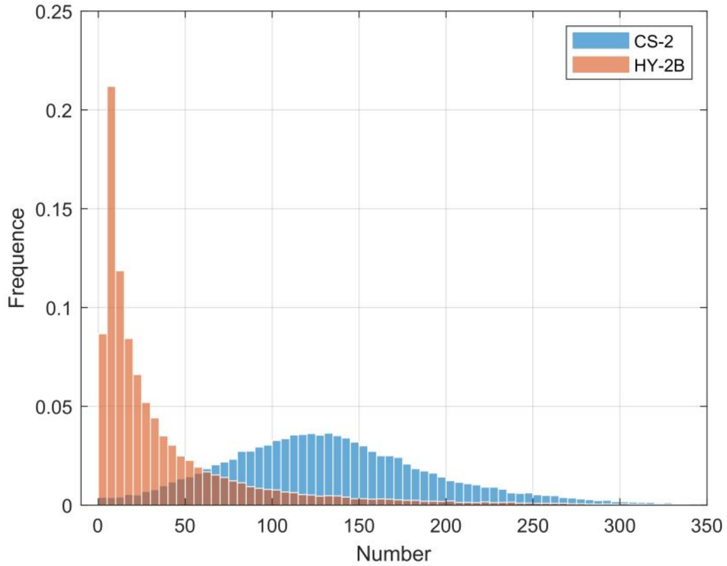

Figure 1. A histogram of the number of points within each grid cell between HY-2B and CS-2 during two cycles (from October 2019 to April 2020 and from October 2020 to April 2021), bin=5.

Your method of using 25 km segments and choosing leads based on the lowest 15 points within a segment still hasn't fully convinced me. As mentioned, several times throughout your study, this will have different impacts: higher freeboards during freeze-up, lower freeboards during spring, and a difference across first-year ice (FYI) and multi-year ice (MYI). So, essentially, you have a known bias due to your choice of methodology that you are not accounting for. I think it would be necessary to provide a measure (average, modal, median–take your pick) of how often this is the case, e.g., by providing a statistical value of how many points you usually have within a 25 km segment, as well as the standard deviation of this (or min-max range, quantiles–again, take your pick) to understand the variability. In truth, we do not know how often it is the case that your method will be impacted using the 15 lowest points.

Response: Thank you for the valuable comments. We have conducted a statistic of the number of points within each 25 km segment, as shown in Figure 2. The mode of the data points within each 25 km segment is 5. The cumulative probability of measuring points greater than or equal to 15 within each 25 km segment is 43.4%. Meanwhile, we have supplied a histogram of the standard deviation of radar freeboard within each 25 km segment, as shown in Figure 3. The mode of the standard deviation within each 25 km segment is 0.11 m.

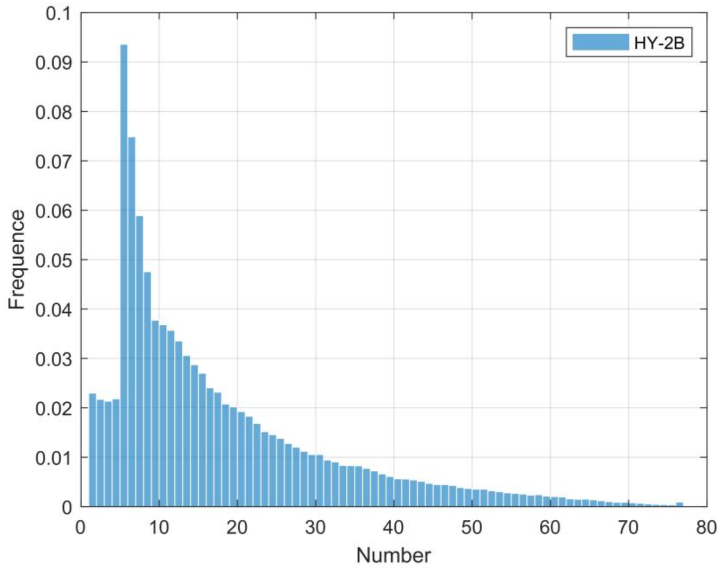

Figure 2. A histogram of the number of points within each 25 km segment during two cycles (from October 2019 to April 2020 and from October 2020 to April 2021), bin=1.

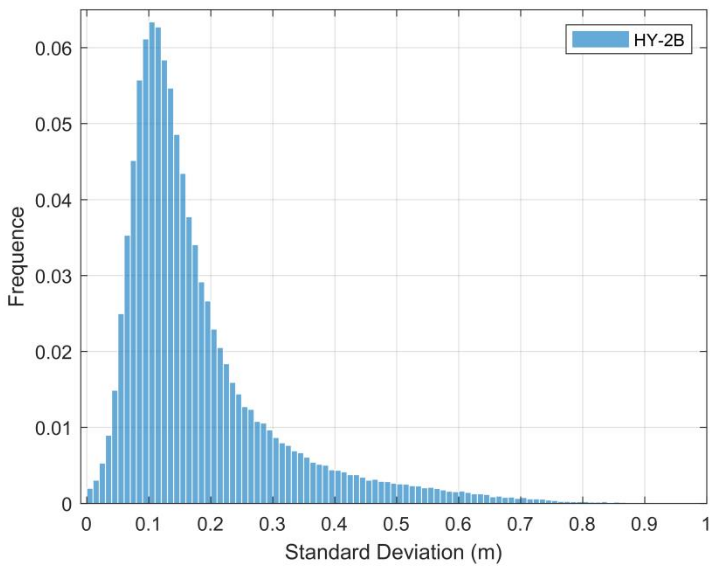

Figure 3. A histogram of the standard deviation of points within each 25 km segment during two cycles (from October 2019 to April 2020 and from October 2020 to April 2021).

Furthermore, your method ensures a lead point every 25 km (if 15 points within a segment). Is this 'enough'? Here, I am considering the fact that HY-2B's footprint is 1.9 km across-track. In the study of Tilling et al. (2019), they compared CryoSat-2 and EnviSat, and saw that a lead-to-floe echo distance for EnviSat ranged from 0-20 km (average 11.3 km)–a satellite with a larger footprint, whereas for CryoSat-2 it ranged 0-4 km (mean of 1.0 km)–a satellite with comparable footprint. Somehow one lead observation every 25 km seems low.

Response: Thank you for the valuable comments. We have used the average of the 15

lowest points as the sea surface height anomaly within each 25 km segment, but this does not necessitate that there is only one lead point within each 25km segment. This is just an estimation method, because there are too few data points in the HY-2B SGDR product to use conventional waveform characteristics to extract the true lead, so we have used the lowest point method to estimate sea surface height anomaly (the elevation of lead) within each 25 km segment. By comparing the lowest points of different values, it is found that the average of 15 lowest points have the best results. According to Figure 1, we have found the cumulative probability of measuring points greater than or equal to 15 within HY-2B grid cell is 58.3%. According to Figure 2, we have found the cumulative probability of measuring points greater than or equal to 15 within each 25 km segment is 43.4%. Therefore, it is enough for HY-2B SGDR product that at least contain 15 data points within 25 km segment to estimate sea surface height anomaly.
We have supplied the sentence in th revised manuscript. 'The cumulative probability of measuring points greater than or equal to 15 within each 25 km segment is 43.4%. Therefore, it is enough for HY-2B SGDR product that at least contain 15 data points within 25 km segment to estimate SSHA.' (Line 383)

Figure 1 in response to Reviewer#1 suggests that something odd is going on due to the limited lead observations identified using the selected PP threshold, since you are simply using a nearest neighbor interpolation–which makes me wonder; how come you use this interpolation? Many other studies use either linear or cubic interpolations, making the interpolation dependent on several points rather than just one. Furthermore, do you have a limit on how far away points are allowed to be from a lead observation, e.g., 200 km, which other studies have required.

Response: Thank you for the valuable comments. Firstly, when the spatical range of floe exceeds the spatical range of lead, the interpolation algorithms of matlab (cubic and linear) cannot interpolate. Secondly, we didn't have a limit on how far away points are allowed to be from a lead observation. Even if we have a limit on how far away points are allowed to be from a lead observation, e.g., 200 km, we cannot get the data through cubic and linear interpolations because the sptical range of floe exceeds the spatical range of lead within each 200 km segment. Compared with Figure 1 in response to Reviewer#1 (PP value greater than 10), we used PP value greater than 5 to obtain more SSHAs, as shown in Figure 4. We also compared the result of three interpolation algorithms (nearest, linear and cubic). All radar freeboard values interpolated by cubic and linear interpolation algorithms are nan.

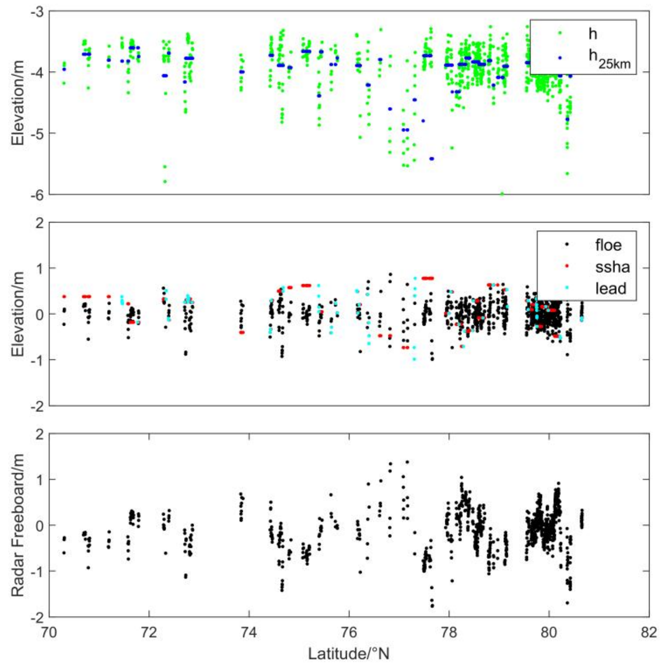

Figure 4. A sample of the HY-2B elevation profile obtained for of track number 14418 on April 4, 2020. The green points in panel (a) are the relative elevation (h) values; the blue points in panel (a) are the h25km values, defined as the 25-km running mean of h; the black points in panel (b) are the modified relative elevation (hr) values; the red points in panel (b) are the sea surface height anomaly (SSHA) values **using nearest interpolation algorithm**; the azury points in panel (b) are lead points extracted by PP values greater 5; and the black points **in panel (c) are the radar freeboard values.**

In response to GC2 for Reviewer#1, you present a comparison with TFMRA50 product–which is not available in the SGDR product. How come you have not used this sea-ice specific re-tracker for the study instead of the SGDR? Please provide some justification/explanation for the choice of this re-tracker in the manuscript.

Response: Thank you for the valuable comments. Firstly, the range item in SGDR products has been re-tracked using the Brown model, so we did not re-tracked again using TFMRA when retrieving the radar freeboard. Secondly, we have used an implementation of the TFMRA to estimate the range to the main scattering horizon for each waveform based on SGDR product. But we didn't obtain satisfactory results, as shown in Figure 5. Finally, in response to GC2 for Reviewer#1, we have supplied a more reasonable result of **HY-2B L1 product** using TFMRA 50. We would like to explain that a more reasonable sea ice freeboard product can be obtained using L1 product instead of SGDR product. The processing chain of AWI (lead/floe discrimination, TFMRA50 re-tracker) provide a good reference for HY-2B L1 data. But it is difficult to obtain reasonable radar freeboard using SGDR products since it has sparse measurement points and the error of interpolation SSHA is larger than the method of 15 lowest points.

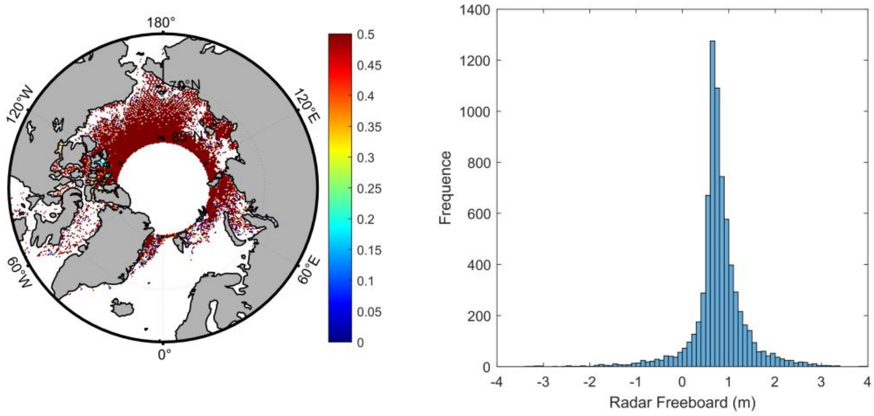

Figure 5. The retrieval of Arctic radar freeboard using TFMRA 50 and the average of 15 lowest points. Left: the spatial distribution of HY-2B radar freeboard in April 2019; Right: the histogram of HY-2B radar freeboard in April 2019.

In response to GC2 from Reviewer#2, you conclude that the assumption of Ku-band penetrating to the snow-ice interface still holds based on former studies. However, more recent studies (Stroeve et al. 2020, 2022; Nab et al. 2022) have questioned this (with good reason). Since we do not currently have methods that can take into account this change of scattering horizon within the snowpack, I'd say the assumption is fair (and still widely used), however I do think that the topic warrants a discussion which is currently not given in the manuscript.

Response: Thank you for the valuable comments. We have supplied the discussion of scattering horizon within the snowpack in the revised manuscript. We have revised 'We assumed that the radar pulses penetrate through any snow cover on floes and scatter from the snow-ice interface, which has been shown in laboratory experiments where the snow cover on sea ice is cold and dry (Beaven et al., 1995; Tilling et al., 2017). Despite some evidence that the scattering horizon migrates as temperature rises (Willatt et al., 2010), Tilling et al. (2017) did not observe any bias in their thickness retrieval when compared to year-round ice draft data, and so they thought that the impact of this effect was not significant.' to 'Several studies have found that radar freeboard uncertainty also pertains to inconsistent knowledge on how far the radar signal penetrates into the overlying snow cover (**Nandan et al., 2020; Willatt et al., 2011; Willatt et al., 2010; Drinkwater, 1995**). The general assumption is that the radar return primarily originates from the snow–sea ice interface at the Ku-band. While this may be applicable to cold, dry snow in a laboratory (**Beaven et al., 1995**), scientific evidence from observations and modeling indicates this assumption may not be valid even for a cold, homogeneous snowpack (**Nab et al., 2023; Nandan et al., 2020; Willatt et al., 2011; Willatt et al., 2010; Tonboe et al., 2010**). Morever, field campaigns have revealed that the dominant radar scattering actually occurs within the snowpack or at the snow surface rather than at the snow–ice interface (**Stroeve et al., 2020; Willatt et al., 2011; Willatt et al., 2010; Giles et al., 2007**). Since we do not currently have methods that can take into account this change of scattering horizon within the snowpack, we have assumed that the radar pulses penetrate through any snow cover on ice floes and scatter from the snow-ice interface.' **(Line 220-230)**

Also, based on several review comments, the uncertainty estimation procedure has been refined, and the results look more promising. However, when comparing with other studies (Ricker et al. 2014, Landy et al. 2020), the uncertainties estimated in this study (for both CS-2 and HY-2B) are in the lower range, which warrants a discussion. Also, consider separating your uncertainty estimates into ranges for MYI and FYI (like Ricker et al., 2014), since you also mention that you see different results depending on ice type.

Response: Thank you for the valuable comments. The uncertainties of CS-2 are provided by the AWI sea ice thickness product. We have supplied a discussion in the revised manuscript. "However, the uncertainties estimated in this study for CS-2 and HY-2B are in the lower range when comparing with other studies (Ricker et al. 2014, Landy et al. 2020). This is because we just make a statistics of uncertainty over the common area for both CS-2 and HY-2B. Other studies do the statistics of CS-2 uncertainty with the upper limitation range of 88° N. In addition, Landy et al. (2020) also considered the following principal sources of systematic uncertainty: (i) partial wave penetration into the snowpack on MYI, for instance, due to metamorphic snow features; (ii) partial penetration into the snowpack on FYI, for instance due to brine wicking-induced snow basal salinity; and finally (iii) sea ice surface roughness. And they revealed sea ice surface roughness as a key overlooked feature of the conventional retrieval process (Landy et al. 2020). It is important to note that these key uncertainties limit the accuracy of the radar-based freeboard retrieval, which then propagate into the freeboard-to-thickness conversion." **(Line 436-444)**
We have supplied our uncertainty estimates for FYI and MYI, as shown in Table 1 and 2 (Table 9 and 10 in the revised manuscript). Here, we have used the monthly average grids of sea ice density and sea ice density uncertainty provided by AWI product. So we have updated the figure 12.
We have revised to '**from 0.61 m to 0.74 m**.' **(Line 22-23, 434-435 and 480)** We have revised to 'The sea ice thickness uncertainties over MYI are greater than over FYI for HY-2B and CS-2.' **(Line 426-427)** We have revised to 'Table 9: Mean sea ice freeboard uncertainties of HY-2B and CryoSat-2 on FYI, MYI and total sea ice.' **(Line 876)** We have also revised to 'Table 10: Mean sea ice thickness uncertainties of HY-2B and CryoSat-2 on FYI, MYI and total sea ice.' **(Line 876)**

Table 1. Mean sea ice freeboard uncertainties of HY-2B and CryoSat-2 on FYI, MYI and total sea ice.

Unit: m	Oct 2019-April 2020						Oct 2020-April 2021					
	HY-2B			CS-2			HY-2B			CS-2		
	FYI	MYI	ALL	FYI	MYI	ALL	FYI	MYI	ALL	FYI	MYI	ALL
Oct	0.025	0.028	0.027	0.026	0.028	0.028	0.021	0.027	0.025	0.026	0.028	0.028
Nov	0.019	0.028	0.022	0.021	0.027	0.023	0.018	0.026	0.022	0.020	0.026	0.023
Dec	0.020	0.030	0.023	0.021	0.028	0.023	0.018	0.029	0.022	0.020	0.028	0.023
Jan	0.019	0.029	0.022	0.021	0.028	0.023	0.018	0.028	0.021	0.020	0.027	0.022
Feb	0.021	0.033	0.024	0.022	0.030	0.024	0.019	0.032	0.023	0.021	0.030	0.024
Mar	0.022	0.036	0.025	0.023	0.033	0.025	0.021	0.036	0.025	0.022	0.033	0.025
Apr	0.023	0.037	0.025	0.022	0.033	0.024	0.022	0.039	0.025	0.022	0.034	0.024
mean	0.021	0.032	0.024	0.022	0.030	0.024	0.020	0.031	0.023	0.022	0.029	0.024

Table 2. Mean sea ice thickness uncertainties of HY-2B and CryoSat-2 on FYI, MYI and total sea ice.

Unit: m	Oct 2019-April 2020						Oct 2020-April 2021					
	HY-2B			CS-2			HY-2B			CS-2		
	FYI	MYI	ALL	FYI	MYI	ALL	FYI	MYI	ALL	FYI	MYI	ALL
Oct	0.81	0.58	0.67	0.45	0.51	0.49	0.80	0.56	0.61	0.46	0.47	0.47
Nov	0.64	0.68	0.65	0.44	0.50	0.46	0.62	0.66	0.64	0.40	0.45	0.42
Dec	0.69	0.73	0.70	0.44	0.51	0.46	0.61	0.72	0.65	0.47	0.54	0.49
Jan	0.66	0.76	0.69	0.48	0.54	0.50	0.63	0.73	0.66	0.51	0.54	0.52
Feb	0.71	0.80	0.73	0.55	0.58	0.56	0.67	0.77	0.70	0.57	0.58	0.57
Mar	0.71	0.88	0.74	0.63	0.65	0.63	0.70	0.84	0.73	0.63	0.64	0.63
Apr	0.71	0.88	0.73	0.68	0.77	0.69	0.68	0.87	0.71	0.68	0.72	0.69
mean	0.70	0.76	0.70	0.52	0.58	0.54	0.67	0.74	0.67	0.53	0.56	0.54

Finally, I tried retrieving the data again. It was indeed necessary to create an account to access the FTP server, so I was not able to look at the data used for this study without creating a user (the user was not activated within deadline of this review). I suggest you write, in the data availability section, how to properly retrieve the data as you wrote in the response to reviews (especially since the website is not available with an English translation, thus limiting the potential user pool) to help other users get a hold of this data.

Response: Thank you for the valuable comments. Please click the red button in the upper right corner to switch to the English interface, as shown in Figure 8. We have supplied the description of data availability in my revised manuscript (Line). "If you haven't registered before, you'll need to create an account to access the FTP server at this website (https://osdds.nsoas.org.cn/register). Then, you can enter your account and password to log in to the official website to access the FTP folder with SDGR HY-2B data using filezilla (ftp://osdds-ftp.nsoas.org.cn/). The SGDR HY-2B data can also be accessed through https://osdds.nsoas.org.cn/MarineDynamic/." (Line 499-502)

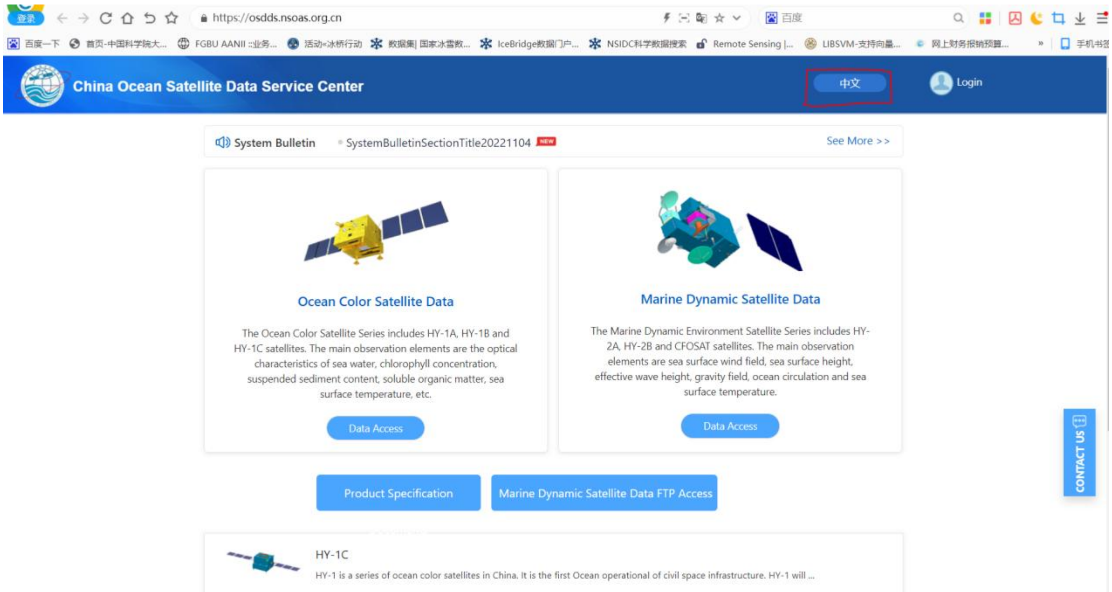

Figure 8. The website of HY-2B satellite.

Please check again that all references are properly written. E.g., line 144 (new manuscript) should be 'Tonboe et al. (2016)' not 'Rasmus et al. (2016)'.

Response: Thank you for the valuable comments. We have revised to 'Tonboe et al. (2016)'(Line 150, 154, 509 and 717-718). We have revised to 'Aaboe et al. (2021)' (Line 158, 509 and 524-526). We also have checked all referneces through the manuscript. We supplied the some references (Line 556-557, 625-627, 645-647, 649-651, 699-702, 715-716, and 734-736).

Specific comments (references to line numbers in the new manuscript)

I highly encourage the authors to look over the text again. With the introduction of a several new paragraphs, there are several places where it could benefit from some proofreading.

Response: Thank you for the valuable comments. We have removed the sentences, 'Compared to conventional radar altimeters, CS-2 can achieve monthly observations of the Arctic with a coverage range of 88°N/S' (Line 112), 'It should be noted that the HY-2B SSHAs are slightly lower than those of the CS-2, the elevations of the floes are slightly higher than the CS-2, so the HY-2B radar freeboards are higher than those of the CS-2' (Line 300).

We have revised 'Fig. 10 shows comparisons of the HY-2B and CS-2 sea ice thicknesses with the IS-2 sea ice thicknesses from October 2019 to April 2020 and from October 2020 to April 2021, respectively.' to 'Fig. 10 shows comparisons of the HY-2B and CS-2 sea ice thicknesses with IS-2, respectively.' (Line 362-363)

We have revised 'the mean of these SSHA standard deviations' to 'the standard deviation of these SSHAs' (Line 394).

Line 15-17. Your methodology has yet to be described, yet you mention specifics about how it is done. I suggest generalizing this sentence.

Response: Thank you for the valuable comments. We have revised 'In spring, more of the lowest 15 points within 25 km segment are likely to originate from floes, while more points may originate from leads in early winter.' to 'A spring segment likely have more floe points than an early winter segment.' (Line 18) We also have supplied the description of methodology. 'Here, we derive radar freeboard by calculating the difference between the relative elevation of floe obtained by subtracting mean sea surface height (MSS) and sea surface height anomaly (SSHA) determined by an average of 15-lowest points method.' (Line 13-15)

Line 17-20. Could you include a short sentence of how CryoSat-2 compare with OIB, for a perspective?

Response: Thank you for the valuable comments. We have supplied the description of how CS-2 compare with OIB. 'The correlation between HY-2B (CS-2) sea ice freeboard retrievals and OIB values is 0.77 (0.84), with a root mean square error (RMSE) is 0.13 (0.10) m and a mean absolute error (MAE) is 0.12 (0.081) m. The correlation between HY-2B (CS-2) sea ice thickness retrievals and OIB values is 0.65 (0.80), with a RMSE is 1.86 (1.00) m and a MAE is 1.72 (0.75) m.' (Line 19-22)

Line 22. I'm happy to see that the abstract has been shortened. Perhaps, you could include a short line on future work or current limitations for your work with HY-2B observations.

Response: Thank you for the valuable comments. We have supplied the future work in the revised manuscript. 'We will reprocess the HY-2B L1 data to obtain more reliable polar sea ice thickness products.' (Line 24)

Line 59. "corrections" -> "processing steps"

Response: Thank you for the valuable comments. We have revised 'corrections' to 'processing steps'. (Line 61)

Line 67. "The overall difference (…) AWI data is" -> "They noted the average difference (…) AWI data to be "

Response: Thank you for the valuable comments. We have revised 'The overall difference between the HY-2B radar freeboard estimates and the AWI data is 0.088±0.057 m.' to 'They noted the average difference between Haiyang-2B (HY-2B) radar freeboard estimates and AWI data to be 0.088 ± 0.057 m.' (Line 69-70)

Line 69. "The radar freeboards are generally higher for HY-2B than CS-2" -> "They

generally observed higher radar freeboards for HY-2B than CS-2".

Response: Thank you for the valuable comments. We have revised to 'They generally observed higher radar freeboards for HY-2B than CS-2.' (Line 70-71)

Line 70. "(…), the HY-2B satellite can be used to observe polar sea ice" -> "(…), the feasibility of using the HY-2B satellite to map the polar sea ice must be explored". Please, also present limitations of your study (using already provided and re-tracked data rather than applying your own re-tracker etc.).

Response: Thank you for the valuable comments. We have revised 'With the continuous development of China's Marine Dynamic Environment Satellite (Haiyang-2B, HY-2B), the HY-2B satellite can be used to observe polar sea ice' to 'With the continuous development of China's Marine Dynamic Environment Satellite, the feasibility of using the HY-2B satellite to map the polar sea ice must be explored.' (Line 72)
We have also supplied the limitation of this study. 'It is important to note in this study, however, that we are aiming to investigate whether HY-2B can be used for sea ice, but that we are limited to already provided higher-level (SGDR) product, and that it is not within the scope of the study to derive freeboard product using own re-tracker from the HY-2B SGDR product.' (Line 72-75).

Line 84-85. Is "take into account the observations of sea ice" part of its main mission? I suggest rephrasing this sentence for clarity.

Response: Thank you for the valuable comments. We have revised to 'Its main mission is to monitor and survey the marine environment, obtain a variety of marine dynamic environmental parameters, including sea surface winds, wave heights, sea surface heights, sea surface temperatures and other elements as well as the parameters of polar sea ice.' (Line 87-90)

Line 88. Please provide a description of which stage the satellite is currently at and when the stages have started/ended.

Response: Thank you for the valuable comments. The design life of HY-2B is 5 years. Generally, it will enter 168 days of repeated orbit in the later stage of operation. The specific time will be determined according to the satellite operation. We have supplied the description of the current stage of satellite. 'Currently, the repeat cycle of HY-2B is 14 days.' (Line 93)

Line 90. Waveforms have not been mentioned yet. Consider rephrasing this for clarity.

Response: Thank you for the valuable comments. We have removed this sentence. (Line 95)

Line 96-97. Are the tracking modes named after the different re-trackers (OCOG, SMLE) or does HY-2B have to tracking modes based on surface, that tracks with different re-trackers? Consider rephrasing this, while also including a reference to what the "Brown" model is.

Response: Thank you for the valuable comments. We have supplied a reference to explain Brown model. 'The SGDR products contain waveform data and have been re-tracked using the Brown model (Zhang et al., 2022).' (Line 101-102)
We have also revised 'In addition, the HY-2B altimeter uses two different tracking modes: suboptimal maximum likelihood estimation (SMLE) and offset center of gravity (OCOG). The two tacking modes can exchange according to the observation surfaces.' to 'The HY-2B altimeter will switch between suboptimal maximum likelihood estimation (SMLE) tracking mode and offset center of gravity (OCOG) tracking mode according to terrain changes. The SMLE tracking mode is suitable for areas with slower changes in terrain height, such as ocean and large areas of flat sea ice. The OCOG tracking mode is used for areas with dramatic changes in topographic height, such as land and sea ice areas.' (Line 102-105)

Line 114. Include which version (baseline) of ESA CryoSat-2 product you are using.

Response: Thank you for the valuable comments. We have revised to 'We mainly used the level-2 (L2) along-track data published by the ESA (processor baseline-D) and the monthly average products published by the AWI.' (Line 119-120)

Line 196. MSS defined (by acronym) later than first used (used in MSS data section).

Response: Thank you for the valuable comments. We had already defined the MSS in line 146. We have revised 'mean sea-surface (MSS)' to 'MSS'. (Line 199)

Line 199. You mention residual error–be careful with terms like error/uncertainties. Consider using a different term. Also, this is the first time you are mentioning anything about this residual "error". Could you describe it more?

Response: Thank you for the valuable comments. We have revised 'residual error' to 'estimation error' (Line 201, 203). We have revised 'The estimation error may be caused by the errors of orbit determination and different tracking algorithm.' to 'The estimation error does not include the modeled portion of the sea surface height, but includes all the unexplained static and time-varying components of the sea surface as well as noise introduced by our estimation process including the errors of orbit determination and different tracking algorithm (Kwok et al., 2007).' (Line 201-203)

Line 207. Has SSHA been defined yet?

Response: Thank you for the valuable comments. We had already defined 'SSHA' (Line

79).

Line 209-210. "Since the (…)" -> consider rephrasing this or write the limitations in the introduction already, and then highlighting here, that you are simply using already provided elevations in the SGDR product.

Response: Thank you for the valuable comments. We have supplied the limitation of this study. 'It is important to note in this study, however, that we are aiming to investigate whether HY-2B can be used for sea ice, but that we are limited to already provided higher-level (SGDR) product, and that it is not within the scope of the study to derive freeboard product using own re-tracker from the HY-2B SGDR product.' (line 72-75).
We have revised 'we did not re-track it again in this study.' to 'we are simply using the range terms from the satellite to the ground already provided in the SGDR product.' (Line 214-215)

Line 232 (Section 3.2). Aren't these results? Consider moving them to the result section.

Response: Thank you for the valuable comments. We have moved the section 3.2 to section 4.1 (Line 249-267). We have supplied the description. 'Firstly, we compared the parameters involved in the retrieval process with those in the CS-2 L2 along-track data released by the ESA.' (Line 244-245)
We have also revised to 'In this section, we described the sea ice thickness retrieval method applied for the SGDR data of the HY-2B pulse-limited radar altimeter in detail.' (Line 187-188) We have removed the '3.1 Retrieval process'.

Line 241-242. "(…), which may have been caused by the fact that not all points within the 25 km segment are leads" -> "(…), which may have been caused by the fact that not all points used to estimate the SSHA within the 25 km segments originate from leads".

Response: Thank you for the valuable comments. We have revised 'which may have been caused by the fact that not all points within the 25 km segment are leads' to 'which may have been caused by the fact that not all points used to estimate the SSHA within the 25 km segments originate from leads.' (Line 258-259)

Line 248. "totally overlapped" -> "fully coincident"

Response: Thank you for the valuable comments. We have revised 'totally overlapped' to 'fully coincident' (Line 266).

Line 249. Could you provide the exact time difference and overall spatial difference? If the difference is significant (in hours), perhaps a measure of drift might be provided as well, to imply how big of an impact drift may have in this comparison.

Response: Thank you for the valuable comments. The HY-2B track started to record at

18:05:48 on March 13, 2020, with the ending time at 18:58:02 on March 13, 2020. The spatial area of HY-2B track on Figure 4 (e) ranged from 97.7972° W-163.5410° W, 80.6271° N-63.3285° N. But the CS-2 track started to record at 15:10:52 on March 13, 2020, with the ending time at 15:20:54 on March 13, 2020. The spatial area of CS-2 track on Figure 4 (e) ranged from 123.0748° W-131.7198° W, 81.4975° N-70.5800° N. The time difference between HY-2B track and CS-2 track is as much as three hours. Considering the temporal differences between HY-2B track and CS-2 track indicates the sea ice drift likely impact the comparison between them. It is still a worthwhile question as to how big of an impact drift may have in this comparison.

Line 308-310. Consider combining these two sentences ("Except (…)") in the sentence starting in line 307: "The HY2-B sea ice thicknesses are thicker (…)" for clarity. Also, throughout the text, be aware of too many repetitions of practically the same text.

Response: Thank you for the valuable comments. We have revised 'These results are related to the accuracy of the extracted HY-2B SSHAs. Except in February, March and April, the monthly mean HY-2B sea ice thicknesses are thicker than AWI CS-2. The HY-2B modal thicknesses are thinner than AWI CS-2, except in December 2019, November 2020 and December 2020.' to 'The monthly mean sea ice thicknesses of HY-2B are thicker than CS-2 in early winter, while CS-2 sea ice thicknesses are greater than HY-2B in spring. The modal thicknesses of HY-2B are thinner than AWI CS-2, except in December 2019, November 2020 and December 2020. These results are related to the accuracy of the extracted HY-2B SSHAs.' (Line 318-321)

Line 331. What underestimation? It can be seen on the figures but has not been described in the text so far. Please include it.

Response: Thank you for the valuable comments. We have revised to 'The correlation between HY-2B and OIB is 0.65, with a RMSE of 1.86 m and a MAE of 1.72 m suggest that this underestimation of sea ice thickness could not only be attributed to sea ice freeboard but maybe also to snow depth or other parameters.' (Line 341-343)
We have also revised to 'The majority of the spread (shown by RMSE or MAE) in our HY-2B evaluation is caused by the underestimation of thickness over thick ice, which may have been caused by the fact that not all points used to estimate the SSHA within the 25 km segments originate from leads.' (Line 344-346)

Line 334. "The majority of the spread" -> "The majority of the spread (shown by RMSE or MAE)"–or however you see this spread, but link to it.

Response: Thank you for the valuable comments. We have revised to 'The majority of the spread (shown by RMSE or MAE) in our HY-2B evaluation is caused by the underestimation of thickness over thick ice, which may have been caused by the fact that not all points used to estimate the SSHA within the 25 km segments originate from leads.' (Line 344-346)

Line 339. The IS-2 snow freeboard is not subtracted from the AWI snow depths to obtain the sea ice freeboard. The AWI snow depths are subtracted from the IS-2 snow freeboards to obtain the sea ice freeboard. Please correct.

Response: Thank you for the valuable comments. We have revised to 'The AWI snow depths are subtracted from the IS-2 snow freeboards to obtain the sea ice freeboards.' (Line 351-352)

Line 342. Add which section this slower wave propagation correction has already been explained in, in the end of the sentence -> (see Section …).

Response: Thank you for the valuable comments. We have revised to 'To compare these values with IS-2 sea ice freeboard, we use AWI snow depth to perform a wave propagation speed correction for HY-2B and AWI CS-2 radar freeboard (see Section 3).' (Line 352-353)

Line 348. "In addition (…)"–consider rephrasing this sentence for clarity.

Response: Thank you for the valuable comments. We have revised 'In addition, the differences of measurement mode and footprint size maybe result the discrepancies between HY-2B and IS-2.' to 'In addition, the differences between HY-2B and IS-2 may be caused by inconsistent measurement modes and footprint sizes.' (Line 359-360)

Line 367. What is meant by a "larger" SSHA? I suggest rephrasing for clarity.

Response: Thank you for the valuable comments. We have revised 'As the table shows (Schemes 1-8), the mean deviation and MAE values first decrease and then increase with the gradual increase in SSHA, indicating that a larger SSHA does not necessitate a smaller mean deviation or MAE.' to 'As the table shows (Schemes 1-8), the mean deviation and MAE values first decrease and then increase with the gradual increase in SSHA, indicating that an increase in SSHA does not necessitate a linear reduction in mean deviation or MAE.' (Line 378)

Line 434. Similar to the changes for the abstract, consider generalizing this sentence since you are talking specifics about a methodology that has not been described in your conclusion yet.

Response: Thank you for the valuable comments. We have revised 'In spring, more of the lowest 15 points within 25 km segment are likely to originate from floes, while more points may originate from leads in early winter.' to 'A spring segment likely have more floe points than an early winter segment.' (Line 453-454)

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