Responses to referee #2

Dear Reviewer:

You offers strong criticism of the paper and does not review all of it. We believe we can answer all of the criticisms stated.

The paper is not unusual in that it considers interannual variability in satellite altimetry of ice thickness, in the context of climatic forcing. These kinds of studies are widely published(Kwok, 2018; Kwok and Cunningham, 2015; Tilling et al., 2015). While there are of course important caveats to the use of these data, e.g. snow loading, these published studies demonstrate that it is widely accepted that satellite altimetry may be used to study variability in sea ice thickness.

We think the satellite data do show a real signal of anomalously thin ice, that deserves investigation. We accept of course that there are important uncertainties in the data, particularly caused by interannual variability in the snow loading. The revised paper will more fully present these uncertainties and consider them in the discussion of the conclusions.

It should be noted, as suggested by the reviewer, we agree that it is not rigorous to discuss trends and minima of sea ice thickness with the limited data. The value of the paper is more in revealing the underlying effects that cause the ice thickness/volume anomaly in 2011. So, we removed the texts related to the minimum and trend of sea ice thickness in the manuscript.

Qinghua Yang

On behalf of all the co-authors

General Comments:

1） First off, I disagree strongly with the statement that the ice thickness decline slowed down during CS2, there is no evidence of this, especially given the use of snow depth climatology (see Mallett et al. in TCD).

Response: *As mentioned by Kwok (2018) : "In the satellite record, the five ICESat years seem to have captured the steep declines in thickness (especially the sharp decrease in thickness after the record setting years of 2005 and 2007); the thinning seems to have slowed in theCS-2 years." However, as suggested by the reviewer, we agree that it is not rigorous to discuss the slow down ice thickness trends with the limited data and CS2 uncertainties of sea ice thickness. In addition to that, this is not the central claim of our paper. Our paper mainly focusses on the 2011 anomalies, not the change in thickness trend. The two are linked of course, but our work is really about 2011. So, we removed the text related to the trend of sea ice thickness in the manuscript.*

2） All your statements about CS2 thickness variability should be stated with a caveat in that this assumes no interannual changes in snow depth, except as represented by first-year vs. multiyear ice. Thus, because this paper hinges on the 2011 minimum sea ice thickness as measured by CS2, then you will have to address the use of snow climatologies in these thickness estimates which are assimilated into your model.

I'm also concerned about the lack of discussion on snow cover in general which plays an important role on thermodynamic ice growth as well as the timing of when bare ice and melt ponds form.

Response: *We agree with the reviewer that the lack of snow data and the uncertain radar penetration into snow is a significant weakness of any product based on CS2. The observational CS2 uncertainties of sea ice thickness contain contributions that are associated with speckle noise, sea-surface height estimation, snow depth and densities of ice and snow (Ricker et al., 2014). CS2 data have relatively large errors over the thin ice area, while SMOS has smaller error, and vice versa for the thick ice area (Ricker et al., 2017). So we replaced the data with CS2SMOS for consistency to compare the daily behavior of sea ice* *thickness and volume from October 2010 through April 2020 and calculated the uncertainties in SIT as shown in the Figure S1 below. About the contribution of snow on CS2 uncertainties of sea ice thickness, the CS2 use the snow climatologist in thickness estimates. As shown by Fig.5 in* Mallett et al.(2020)*, there was no obvious snow anomaly contributions to sea ice thickness in the Central Arctic in October 2011.*

We also believe that the sampled radar signal is real, and that we broadly believe it's interpretation as an ice thickness trend, as shown by Mallett et al.(2020)*. We would also note in general that these ice thickness data have been used in many high-profile previous studies*(Kwok, 2018; Min et al., 2019; Ricker et al., 2017b; Tilling et al., 2015)*, so the snow thickness issues do not in general prevent us from studying ice thickness anomalies.*

Following your comments, we added a discussion on the uncertainty of CS2SMOS to the manuscript: The combined Cryosat-2 and SMOS satellite data (CS2SMOS) data from AWI is also used to compare the daily behavior of sea ice thickness and volume from October 2010 through April 2020 and calculated the uncertainties in SIT (Ricker et al., 2017a). In addition, Systematic errors as associated with the lack of interannual variability in the Warren snow climatology (Warren et al., 1999) or due to variable snow penetration will increase the uncertainty of altimetry-based thicknesses (Ricker et al., 2014). The snow data with more realistic variability and trends has wide implications for thickness variability in marginal seas (Mallett et al., 2020). The SMOS retrieval can contribute valuable information, especially in regions with uncertain snow depth estimates.

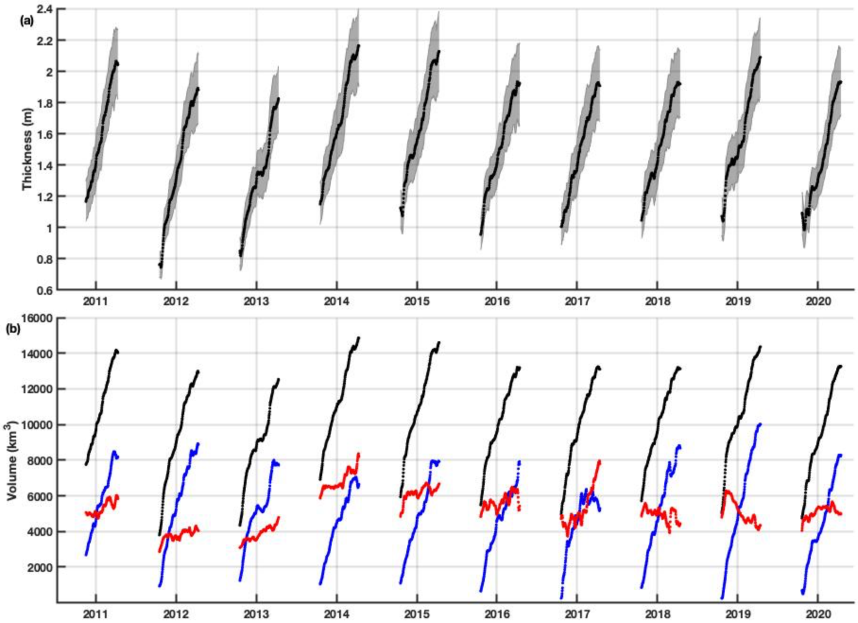

Figure S1: Daily behavior of sea ice thickness and volume based on CS2SMOS dataset from October 2010 through April 2020. (a) Mean sea ice thickness within area of actual ice coverage. (b) Total(black)， first-year(blue) and multiyear (red) sea ice volumes within Arctic basin. The mean sea ice thickness is computed within the area of actual ice coverage bounded by the gateways into the Pacific (Bering Strait), the Canadian Arctic Archipelago, and the Greenland (Fram Strait) and Barents Seas.

3） Does the model not simulate any snow? The entire description of the modeling framework is too vague for this study. While references are given to the CMST model, you need to at least include some basic information such as resolution, atmospheric forcing data, etc. The entire methods section is weak and not suitable.

Response: *Regarding the description of the model, note this was deliberate as we were citing an earlier paper, but of course it is fine to add more details here. So we refined this description as: We apply this methodology to a well-validated sea ice thickness and drift dataset (the Combined Model and Satellite Thickness data, CMST), which was generated by the MITgcm ice-ocean model with CryoSat-2, SMOS sea ice thickness and SSMIS sea ice concentration assimilated (Mu et al., 2018). Both the ocean and sea ice model are discretized on an Arakawa C grid* *with a grid spacing of 18 km. In the ocean model, there are 50 unevenly spaced layers in the vertical direction. The atmospheric ensemble forecasts of the United Kingdom Met Office (UKMO) Ensemble Prediction System (EPS; Bowler et al., 2008) available in the THORPEX Interactive Grand Global Ensemble (TIGGE) are used to drive the ice-ocean model. The thermodynamics of sea ice use a one-layer, zero-heat capacity formulation (Semtner Jr, 1976; Parkinson &Washington, 1979) and the snow thickness is a prognostic variable following Zhang et al. (1998). The CMST thickness data cover both the cold seasons and the melting seasons for the period of October 2010 to December 2016. The CMST has been already quantitatively evaluated against observations by previous studies (Mu et al., 2018; Min et al., 2019; Min et al., 2021), demonstrating an accurate performance in simulating the real sea ice drift and thickness.*

4） I also found the conclusions drawn often not supported by the data. In fact, the largest amount of ice in terms of area was not lost in 2011, and since you are further arguing that the ice was thinner, there is no way that you had the largest volume loss.

Response**:** *Although the loss of sea ice area in 2011 was not the largest, the loss of sea ice thickness from October 2010 to September 2011 was also one of the important factors affecting the loss of sea ice volume. We have examined the whole satellite record, which includes pan-Arctic SIT snapshots from ICESAT (2003-2008) and CryoSat-2 (2011–2020) satellite datasets. We have now added a new figure to the Supplement of the revised version of our manuscript (as shown in the Figure S2 below) and compared the sea ice volume, area and mean sea ice thickness based on the ICESat (2003–2008) and CryoSat-2 (2011–2020) satellite datasets. That clearly shows that the volume loss and thickness loss are both largest.*

This conclusion is consistent with several published studies. We argue that the fact that Kwok (2018) *has a paper published discussing Arctic sea ice volume in 2011 hit the lowest record from 0ct. to Nov. between 2003 and 2018 in the same Arctic basin shows that i) the satellite data are considered worthy of studying and ii) this individual event is worthy of studying. As mentioned by* Tilling et al.(2015) *in Nature Geoscience : "It is notable, for example, that the record minimum Arctic sea ice extent of September 2012 was accompanied by thicker autumn ice in this region* *than in previous years, demonstrating that decreasing ice extent does not necessarily result in a proportionate decrease in ice volume". Although only the data from 2010 to 2014 were available, Tilling et al. (2015) showed that the total loss of sea ice volume from autumn 2010 ($9.03*10^3 km^3$) to autumn 2011 ($7.86*10^3 km^3$) was the largest in the five years (2010-2014) (as shown in the Table S1 below). As suggested by reviewer, the total ice extent lost in 2011 was not the largest, but the ice volume loss was. This is not inconsistent; it just shows that there was an abnormal loss of sea ice thickness from October 2010 to September 2011.*

We disagree that our study does not add value, and we believe that there was thinner ice in 2011, and that the factors are discussed. We are happy to address any specific issues that the reviewer would like to raise that concern these points.

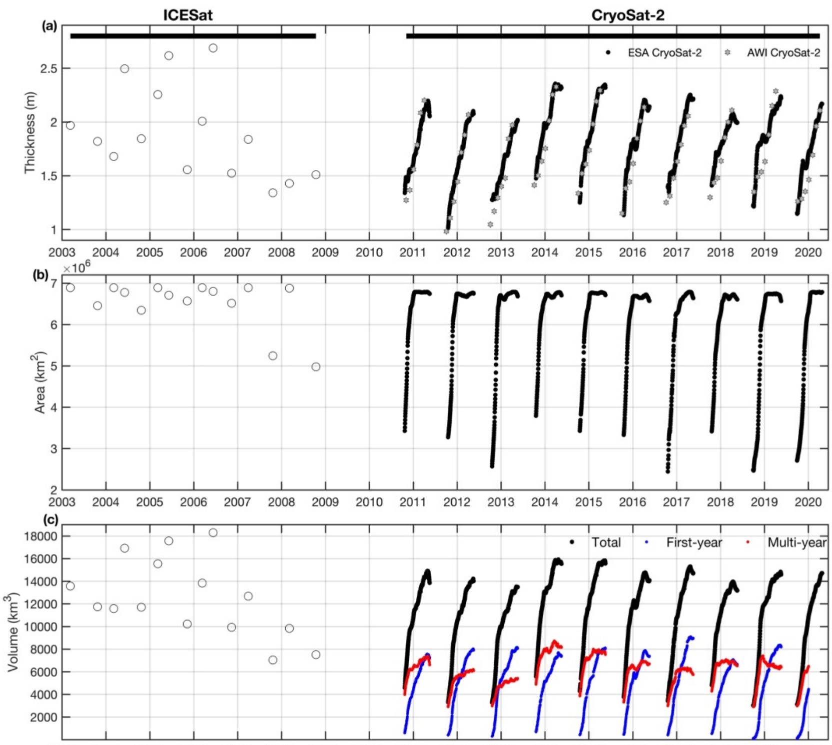

Figure S2: Interannual changes in sea ice volume, area and thickness based on the ICESat (2003–2008) and CryoSat-2 (2011–2020) satellite datasets. (a) Mean sea ice thickness within area of actual ice coverage. (b) Total sea ice area (cumulative area of actual ice coverage) within Arctic basin. (c) Total(black)，first-year(blue) and multiyear (red) sea ice volumes within Arctic basin. Arctic basin volume and area is computed within the bounded by the gateways into the Pacific (Bering Strait), the Canadian Arctic Archipelago, and the Greenland (Fram Strait) and Barents Seas.

Table S1. Table 1 in Tilling et al. (2015).

Table 1 | Average CryoSat-2 Arctic sea ice volume (10^3 km^3) for autumn (October–November) 2010–2014 and spring (March–April) 2011–2014.

Year	Volume (MYI)		Volume (FYI)		Volume (total)	
	Autumn	Spring	Autumn	Spring	Autumn	Spring
2010–2011	5.34 ± 0.69	7.64 ± 0.94	3.69 ± 0.59	17.99 ± 2.44	9.03 ± 1.28	25.63 ± 3.37
2011–2012	3.75 ± 0.56	5.72 ± 0.71	4.11 ± 0.63	19.57 ± 2.66	7.86 ± 1.19	25.29 ± 3.36
2012–2013	3.70 ± 0.48	6.23 ± 0.80	4.05 ± 0.62	18.20 ± 2.53	7.75 ± 1.10	24.43 ± 3.32
2013–2014	6.95 ± 0.82	9.63 ± 1.12	3.99 ± 0.61	16.96 ± 2.29	10.94 ± 1.43	26.59 ± 3.41
2014–2015	6.18 ± 0.73	-	4.08 ± 0.62	-	10.26 ± 1.34	-

To estimate uncertainties in monthly sea ice volume, we account for uncertainties in the sea ice density, snow density, snow depth, sea ice area, sea ice concentration, and for spatial variations in the measurement of sea ice freeboard. The autumn and spring uncertainties are the averaged uncertainties of their corresponding months.

5） The manuscript suffers from many of these types of inconsistencies, and vague statements without supporting evidence. Sadly I cannot recommend this paper for publication. It does not add any value to our understanding of processes in the Arctic, nor does it accurately portray the factors contributing to the "supposedly" anomalous thin ice in 2011.

Response: *We believe the manuscript is not inconsistent, and that we are happy to address any examples of inconsistency that the reviewers raise. We believe there is plenty of evidence in our paper and in the literature that the 2011 anomaly is real. The dynamic and thermodynamic processes leading to the dramatic sea ice thickness loss are described in Sect. 3. We have performed a detailed investigation of the 2011 anomaly, and we think this clearly does add value to our understanding of Arctic processes.*

Some specific line comments

1) Line 84: I don't believe you are using any method to track ice age, you are using a known data product and it should be stated as such.

Response: *We refined this description as: We also use the weekly sea ice age for the Arctic Ocean introduced by Fowler et al. (2003) and described further by Maslanik et al. (2007), Tschudi et al. (2010), Stroeve et al. (2011) and Tschudi et al. (2020). In the Fowler et al. (2003) approach, the method used to estimate sea ice age involves Lagrangian tracking of sea ice from week-to-week using gridded ice motion vectors.*

2) Line 89: you should also be aware if ice is advected towards the coast in the ice age

product, the ice is lost (i.e. is transported onto land) so there will be a bias.

Response: *Following your advice, we changed this sentence as: Note that motions are not retrieved near coasts, because motion retrievals near the coast are unreliable due to the effects of mixed land and ice/ocean grid cells (Tschudi et al., 2019). Thus, the sum of FYI and MYI is slightly less than the total amount of ice.*

3) Line 90: you are not estimating anything here, instead you are using data from ERA5, unless this statement pertains to anomalies but then you need to specify how the anomalies are computed (i.e. relative to what reference period).

Response: *We agree that our explanation was not sufficient, so we refined this description as: In this study, to quantify the thermodynamic impact on the ice thickness budget, we estimate sea level pressure (SLP), 10 m wind speed, surface radiation fluxes, and albedo anomalies by subtracting the 6-year mean (from October 2010 to September 2016) for each month, derived from monthly ERA5 atmospheric reanalysis data from the European Centre for Medium-Range Weather Forecasts (ECMWF)(Copernicus Climate Change Service (C3S), 2017; Hersbach et al., 2020) .*

4) Line 113: I don't follow why you are only using a 6-year mean, you have a longer time-series and you should use it.

Response: *Because the CMST sea ice thickness and drift dataset covers both the cold seasons and the melting seasons only for the period of October 2010 to December 2016.*

5) Line 125: I don't follow why you get enhanced winter melting in this region. I don't believe your residual term is entirely made up of thermodynamic processes, and I do not believe you have anomalous freshwater flux during this time. Where is the evidence for this? I think you are stretching your interpretations too far without the physics supporting these statements. What were your ocean and atmospheric temperatures in that region during that time?

Response: *The combination of figures 2 and 3 in the paper (Fig2d and Fig3d) show that north-east of Greenland, in the climatology there is freezing to the north, and then melting to the south, as the ice is advected south into a less cold climate. In*

2011 this climate is cooler, so freezing extends further south, but the ice advection is faster, so there is more melting south of the freezing line. This leads to a 'dipole' in thermodynamic anomalies, in figure 3d.

As shown in Fig S3, the mean air temperature and sea surface temperature from October 2010 to April 2011 show this cold climatic anomaly north-east of Greenland.

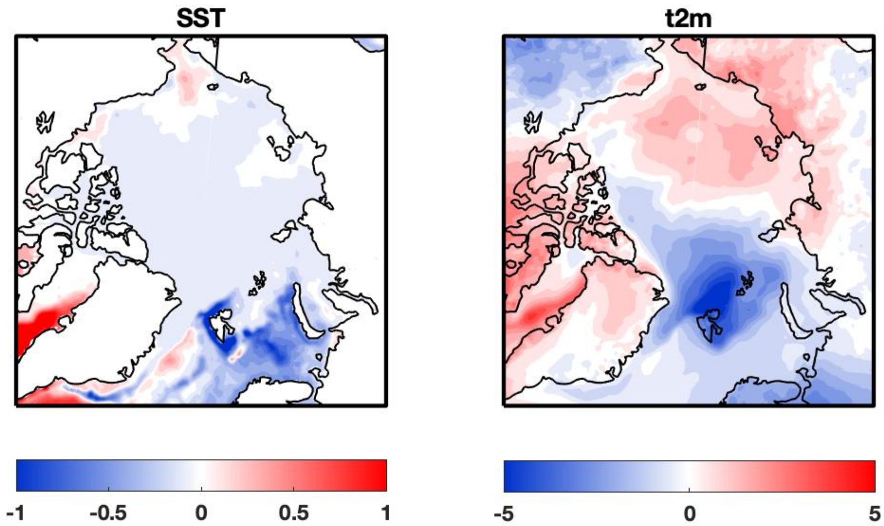

Figure S3: Mean sea surface temperature (SST) anomalies and mean air temperature at 2m (t2m) anomalies from October 2010 to April 2011.

6) Line 127: this is nothing new, divergence will result in thermodynamic ice growth that acts as a stabilizing feedback and there are many references the authors could cite about negative feedbacks. Further, the thickness of the ice to start the growth season also plays an important role in this feedback process, and none of this is discussed. There are also two recent papers suggesting that the thermodynamic ice growth may be slowing, one by A. Petty (GRL) using climate model simulations and one by J. Stroeve (TC) using CS2 data in CICE.

***Response**: We are simply claiming that we found that analyzing the budget anomalies in the context of the mean budget (figures 2 and 3) is helpful as it clarifies exactly how the atmospheric wind and thermodynamic forcing modifies the ice growth here. Compensation between dynamics and thermodynamics is of course expected, but we believe it is extremely informative to see the exact patterns and rates of thermodynamic and dynamic contributions to overall anomalies.*

Following the comments, we have added the discussion on the feedback between *dynamics and thermodynamics: The thickness of the ice to start the growth season also plays an important role in this feedback process. Stroeve et al. (2018) and Petty et al. (2018) highlighted the importance of the negative winter growth feedback mechanism—thinner ice grows faster than thicker ice due to its decreased insulation. Thus, although the summer sea ice in 2011 is rapidly declining, the negative feedbacks over winter allow for recovery following low summer's sea ice thickness.*

7) Line 132: you cannot simply state that increased melt was driving by atmospheric temperature net surface heat flux and other variables. That is vague and uninformative. There are numerous factors that play a role in melt, including the timing of ice retreat and opening of leads/open water areas between the ice floes. You should at least try to quantify the relative contributions.

   Response: *The sea ice thickness budget contributions caused by thermodynamic processes in response to the driving climatic factors are described in Sect. 3.3. It is unfortunate that the reviewer didn't read section 3.*

8) Line 145: I do not believe your assessment of enhanced ice export out of Fram Strait from October 2009 to January 2010 as it doesn't really match with my own calculations from at least 1 December through end of January. It is actually the second lowest amount of volume flux through the Fram Strait.

   Response: *First, we did not calculate the sea ice export volume from the Fram Strait from October 2009 to January 2010 in this manuscript. The period we calculated is from October 2010 to September 2016, which is indicated in line 145. Second, in terms of the sea ice export volume from the Fram Strait during October 2010 to September 2011,* Min et al. (2019) *showed the same result by estimating the seasonal and interannual variations of Arctic sea ice volume flux through the Fram Strait from September 2010 to December 2016. Ricker et al. (2018) showed a similar result using OSI SAF ice drift in their Table 2. Although we used different datasets, the sea ice export volume in Jan-Mar 2011 also show statistically significant anomalies (as shown in the table below).*

Table S2. Table 2 in Ricker et al. (2018)).

Table 2. Monthly Arctic sea ice volume export through the Fram Strait in km^3 $month^{-1}$ computed with OSI SAF ice drift. Maximum and minimum values are denoted in italic and bold, respectively.

	Oct	Nov	Dec	Jan	Feb	Mar	Apr	Mean
2010–2011	–	−227	−275	−267	−**21**	−*540*	−279	−268
2011–2012	−164	−214	−354	−129	−381	−379	−487	−301
2012–2013	−203	−182	−187	−103	−163	−299	−318	−208
2013–2014	−215	−400	−231	−78	−195	−345	−452	−274
2014–2015	−200	−165	−373	−160	−425	−429	−354	−301
2015–2016	−52	−261	−275	−177	−352	−348	−310	−254

9) Line 177: More sea ice lost in 2011 than any other year? Again you haven't specified over what time-period this analysis is being done for, and it would be good for you to put this into the context also of total ice area lost. If I compute the total ice extent lost between the maximum and minimum for each year, the maximum loss in total ice extent during summer is 2012, not 2011. And in fact 2011 is not even the second highest amount. If you are also arguing that you had thinner ice in 2011, then there is no way that you had more sea ice lost in 2011. Since this is an incorrect statement I didn't finish reading the rest. The entire paper is currently flawed, making statements that are not supported by the observations or the data used

Response: *Thanks for this comment. We agree that our explanation could have been more specific, so we refined this description as: Compared with the 6-year mean (from May 2011 to September 2016), the sea ice thickness budget from May to September 2011 shows a negative anomaly, indicating that the loss of sea ice thickness increases during the season of sea ice retreat.*

As for the problem of volume loss of sea ice, we have replied in the second point of General Comments, that indeed 2011 did have the most ice loss, according to us and to other authors.

i) Although the loss of sea ice area in 2011 was not the largest, the loss of sea ice thickness from October 2010 to September 2011 was also one of the important factors affecting the loss of sea ice volume. *As mentioned by* Tilling et al.(2015) *in Nature Geoscience : "It is notable, for example, that the record minimum Arctic sea ice extent of September 2012 was accompanied by thicker autumn ice in this region than in previous years, demonstrating that decreasing ice extent does not necessarily result in a proportionate decrease in ice volume".*

ii) Kwok (2018) *has a paper published discussing Arctic sea ice volume in 2011* *hit the lowest record from Oct. to Nov. between 2003 and 2018 in the same Arctic basin. Although only the data from 2010 to 2014 were available, Tilling et al. (2015) showed that the total loss of sea ice volume from autumn 2010 ($9.03*10^3km^3$) to autumn 2011 ($7.86*10^3km^3$) was the largest in the five years (2010-2014) (as shown in the Table S1 below).*

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